Design and evaluation of an intervention to increase handwashing with soap after toilet use in Koumassi, Abidjan, Côte d’Ivoire: A cluster randomised trial

Maud Akissi Amon-Tanoh

Thesis submitted in accordance with the requirements for the degree of Doctor of Philosophy

LONDON SCHOOL OF HYGIENE & TROPICAL MEDICINE
University of London

FEBRUARY 2020

Funding details: No funding received
Self-funded research study

Department of Infectious Diseases Epidemiology
Faculty of Epidemiology and Population Health

Declaration by the candidate

I, Maud Akissi, Amon-Tanoh, confirm that the work presented in this thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the thesis.

Maud Amon-Tanoh

Abstract

Background: Diarrhoeal diseases are an important cause of morbidity and mortality in children under five years old in South Asia and Sub-Saharan Africa. Handwashing with soap after contact with faeces is a cost-effective way of preventing diarrhoea. However, handwashing with soap frequencies are low in many settings.

Aim: The study aimed to design and evaluate two interventions to increase handwashing with soap after using the toilet, in housing compounds in Koumassi commune, Abidjan, Côte d'Ivoire.

Methods: We randomly assigned 75 compounds to one of three arms in a 1:1:1 ratio. One arm received an intervention package comprising disgust-inducing handwashing messages, designed using the Theory of Normative Social Behaviour, and a handwashing station (TNSB); the second arm received only the handwashing station (HWS-only); the third arm served as a control group. The primary outcome was the proportion of occasions when hands were washed with soap after using the toilet, measured at the one-month and five-month post-intervention delivery. **Results:** One month post intervention, handwashing with soap somewhat increased from a baseline frequency of 3% to 9% in the HWS-only intervention group, and from 7% to 24% in the TNSB-based handwashing intervention group. In the control group, handwashing with soap changed little (from 6% at baseline to 5%). There was strong evidence (*P*<0.0001) that handwashing with soap frequencies varied between arms: HWS-only intervention versus control, OR=2.00 (95% CI: 1.03-3.90); TNSB-based intervention versus control OR=7.17 (95% CI: 3.91-13.12). Five months post intervention, the corresponding ORs were 1.01 (95% CI: 0.50-2.04) and 3.11 (95% CI: 1.62-6.00) respectively.

Conclusion: A handwashing intervention combining disgust-inducing messages with supply of a handwashing station was effective at increasing handwashing with soap after using the toilet. The provision of a handwashing station-alone had little impact. Future studies should investigate whether the same approach, when delivered via mass media, can have the same effect as the face-to-face delivery used in this study.

Acknowledgement

Having the physical and mental strength to complete this PhD, with all the difficulties that I encountered at almost every step of the process, would not have been possible without an exceptional support system. First and foremost, I wish to thank my supervisor, Simon Cousens, whose patience in this long and difficult journey was beyond remarkable. I was fortunate to be under your supervision, and the experience that I have gained under your guidance goes beyond my expectations.

I also wish to thank my secondary supervisor, Jim McCambridge for all his support, always giving me concise comments, and for having reviewed my entire thesis in such a timely fashion.

I thank my advisory committee (former and current members) Sandy Cairncross, Adam Biran, Patrick Nguipdop-Djomo, Wolf-Peter Schmidt and Maria Lapinski for their contribution.

I wish to thank Sharon Huttly for her support and the efficient and fair manner with which she dealt with my initial supervisory issues.

Thank you, Jenny Fleming, for all your support and for always being so kind.

I also thank Sia, from the IT department, for all his support.

Secondly, I wish to thank my family for all their support.

Merci Maman pour tout…pour tous les sacrifices que tu as faits et que tu continues de faire pour moi…pour nous.

Merci Papa pour tout…et pour avoir accepté la charge financière de mon doctorat, sans jamais questionner le temps exceptionnellement long que j'ai pris pour le terminer.

I wish to thank my precious little sisters, Magali…and Sonia, and my older brothers, Xavier and Stéphane.

Merci Abou et Amy pour tout votre soutien, et pour être toujours là pour moi.

To my favourite uncle, Tonton Réné, thank you so much for all our delicious lunch and for always being there for me.

Thank you Dédé and Tonton Mathieu, for having been close to missing your flight once, just to finish watching all of my intervention videos, and for all the enlightening conversations we have had around my research topic.

Thank you so much Eddine for all your support, for always being there for me, and for having taken the time to come to my fieldwork site to see my intervention. I am also grateful to Donald and you for having taken the time to review my poster for LSHTM's Poster Day.

Thirdly, I wish to thank my two exceptional fieldwork assistants Hermann Aka and Patrice Blon. I could not have managed the fieldwork phase as I did, without your valuable contribution.

Additionally, I thank my amazing intervention implementers, Christiane, Narcisse, and Marius. You contributed greatly to the success of the TNSB-based intervention. I also thank my hard-working fieldworkers from the 2012 pilot study to the actual trial (2014-2017).

I wish to thank my dear friends who have supported me throughout this long battle: Severine Frison, Francesca Cavallaro, Prince Antwi-Agyei, Sheillah Simiyu, Yume Imahori, Joel Francis, Louise Brown, and Lori Miller. Azie, Chantelle and Valerie, thank you so much for your support.

Thank you, Constance Pochard and Rudy Berol (*and Sam*), for having come to Côte d'Ivoire to visit me during my fieldwork, and for having come to my fieldwork site.

Thank you Anna Mädl for your support, and for having taken the time to come to my fieldwork site to see my campaign.

Monica, thank you so much for all your support, guidance, wise advice, always being there for me, despite your very busy schedule, and for coming to my fieldwork site to see my intervention.

Helena Carreira, Maria Peppa and Anu Jain, I could not have dreamt of spending my writing-up in another office than in LG21. You are the best PhD office mates I could have asked for. Thank you so much for your support. Helena and Rhea Harewood, I could never thank you enough for having proposed to proof-read my extensive thesis. "It's above me now."

À mes Philosophes Anonymes, Mildrid, Océane, Alassane et Ahmed, que dire de plus que MERCI… pour votre bonté de cœur et générosité, et pour tous ces beaux moments de partage.

To my dear uncle Tonton Daouda Thiam, who used to call me Doctor, regardless of me protesting as I had yet to finish my PhD. You sadly passed away before seeing me earn this title.

To Dr Jeroen Ensink, who sadly passed away during the course of my PhD. You told me, or words to that effect, that doing a PhD was not a sign of intelligence, but rather a sign of stubbornness. This could not have been truer in the context of my PhD journey. Thank you very much for everything you have done for me to be able to conduct my PhD within a supportive supervisory team.

This PhD is dedicated to my dear Mr Kamagate, who sadly passed away without seeing me finish my PhD, and finally start my professional career and life. You supported me from 2012, when we did the very first pilot study, up until December 30th 2018, when you suddenly passed away. This is besides the work we did together since 2009 in Grand-Bassam. This thesis is as much yours as it is mine

Table of Contents

Abstract 3

Acknowledgement 4

List of Acronyms 11

List of Figures 13

List of Tables 15

Chapter 1 - Introduction: Thesis structure and contribution of the candidate 19

1. Thesis structure 19
2. Contribution of the candidates to the thesis research 21

Chapter 2 - Background and Research Objectives 23

1. Diarrhoeal diseases 23
2. Handwashing with soap 26
3. Gaps in the literature 35
4. Study objectives 36

Chapter 3 - Effect of Behaviour Change Handwashing Interventions on Handwashing with Soap Practices in Low- and Middle-Income Countries: a Systematic review and meta-analyse of randomised controlled trials. 38

1. Systematic review and meta-analysis objectives 38
2. Methods 39
3. Data collection process and data items 42
4. Risk of bias assessment 43
5. Assessment of quality of evidence 44
6. Summary measures 44
7. Synthesis of results 45
8. Risk of publication bias 46
9. Additional analysis 46
10. Results 46

Discussion 82

Conclusion 88

Chapter 4 - Social Norms Theories and their Application to Handwashing 90

1. Social norms theory and the social norms approach 90
2. Theory of normative social behaviour (TNSB): an extension of SNT 95
3. Applicability of the TNSB to handwashing 98
4. Effectiveness of SNTs-based interventions on handwashing practices in LMICs 101
Discussion 108
Conclusion 109
Chapter 5 - Exploratory Handwashing Cross-Sectional Study in Abidjan, 2012 110
1. Study overview and objectives 110
2. Justification for the choice of study setting 111
3. Methods 112
4. Data analyses 124
5. Results 125
Discussion 140
Conclusion 146
Chapter 6 - Development and Psychometric Testing of Scales for Four Handwashing Norms-Related Constructs, in Abidjan, 2014 147
1. Development of four handwashing norms scales 147
2. Study objectives 148
3. Methods 148
4. Data analysis 153
5. Results 154
Discussion 160
Conclusion 163
Chapter 7- Interventions Rationale and Design 165
Part I: Interventions rationale 166
1. TNSB-based handwashing intervention 166
2. Handwashing station-only intervention 171
3. Ethical considerations 177
Chapter 8 - Interventions Delivery Methods 178
1. Interventions delivery methods 178

Chapter 9 – Methods: Trial Design and Conduct 187
Research questions 187
1. Trial design and overview of the study site 188
2. Participants........ 190
3. Procedures 196
4. Outcomes 207
5. Sample size 208
6. Statistical methods 210
7. Qualitative data analysis 214
8. Quality assurance and control 214
9. Data management........ 216
10. Financing and Insurance 216
11. Publication Policy 216
Chapter 10 - An Analysis of the Effect of the Interventions on Handwashing Practices ... 217
1. Baseline comparability 217
2. Effect of the interventions on handwashing practices 223
3. Post-hoc stratified analyses by age group of the intervention's effect on HWWS after toilet events 242
4. Masking assessment 245
5. Comparison of the assumptions used for the trial's sample size calculation and actual observed estimates. 245
Discussion 246
Conclusion 252
Chapter 11 - Process Evaluation of the TNSB-Based Intervention 253
1. Intervention implementation fidelity and participants' reactions to the intervention 253
2. Participants' intervention exposure and message recall 254
Discussion 260
Conclusion 260
Chapter 12 - Sustainability of the Handwashing Station 262
1. Handwashing initial location and maintenance strategy........ 262

2. Handwashing station sustainability assessment 264
Discussion 277
Conclusion 279
Chapter 13 - An Analysis of the Association of Norms-Related Constructs and Handwashing with Soap at Baseline, and of Changes in Norms-Related Constructs over Time and by Trial arm 281
1. Psychometric properties of the handwashing norms-related scales 281
2. Descriptive analysis of the handwashing norms-related constructs at baseline 284
3. A compound-level analysis of the association between HWWS after using the toilet and the perceived norms-related constructs at baseline 290
4. An analysis of the interventions' effects on the perceived norms-related constructs around HWWS after toilet use 293
Discussion 305
Conclusion 313
Chapter 14 – Discussion and Concluding Remarks 314
1. Summary of main findings 314
2. Reflections on the process of conducting this research 317
3. Recommendations for research, policy and practice 320
Conclusion 329
References 330

List of Acronyms

CENTRAL	Cochrane Central Register of Controlled Trials
CFA	Confirmatory factor analysis
CI(s)	Confidence interval(s)
CIE	Compagnie ivoirienne d'électricité
CRT	Cluster randomised trial
Dalys	Disability adjusted life years
DDQ	Daily drinking questionnaire
deff	Design effect
GSEM	Generalised structural equation modelling
HH	Household
HIC(s)	High income country/ies
HM	Harmonic mean
HW+	Handwashing coupled with other intervention(s) type(s)
HWDN	Handwashing descriptive norms
HWOE	Handwashing outcome expectation
HWIN	Handwashing injunctive norms
HWP	Handwashing publicness
HWS	Handwashing station
HWWS	Handwashing with soap
HWT	Handwashing treatment
ICTRP	International Clinical Trial Registry Platform
ID	Identity
INSAAC	Institut National Supérieur des Arts et de l'Action Culturelle
IQR	Interquartile range
ITT	Intention-to-Treat
LMICs	Low-and-middle-income countries
LSHTM	London School of Hygiene and Tropical Medicine
MDGs	Millennium Development Goals
MoH	Ministry of Health
OR(s)	Odd(s) ratio(s)
PRISMA	Preferred Reporting Items for Systematic Reviews and Meta-Analyses
RCT(s)	Randomised controlled trial(s)
RR(s)	Risk/Rate ratio(s)
SDG(s)	Sustainable Development Goals
SMD	Standardised mean difference
SNA	Social norms approach
SNT(s)	Social norms theory/ies
SE(s)	Standard error(s)

SODECI	Société de distribution d'eau de la Côte d'Ivoire
SR	Systematic review
TNSB	Theory of Normative Social Behaviour
UI	Uncertainty interval
UNICEF	United Nations Children's Fund
UR	Uncertainty range
U5MR	Under five mortality rate
WSA	Water and Sanitation for Africa
WHO	World Health Organization

List of Figures

Figure 2.1. Causes of death in children under five years old in 2018. Source: UNICEF [1] . 24
Figure 2.2. Mortality rate of children under five years old per 100 000 population due to diarrhoea in 2016. Source: Collaborators [7] 24
Figure 2.3. The 'F-Diagram' of faecal-oral transmission route, as per Wagner and Lanoix (1958). 26

Figure 3.1.PRISMA flow diagram 49
Figure 3.2. Risk of bias graph: reviewer's risk of bias judgements about each risk of bias item across all included studies (presented as percentages) 64
Figure 3.3. Reviewer's risk of bias judgements about each risk of bias item for each included study 64
Figure 3.4. Meta-analysis of the effect of behaviour change interventions on HWWS practices after faecal-related contact (by occasion, intervention motive and measurement method) 70
Figure 3.5. Meta-analysis of the effect of behaviour change interventions on HWWS practices before food-related contact (by occasion, intervention motive and measuring methods). 76
Figure 3.6. Meta-analysis of the effect of behaviour change interventions on HWWS practices after faecal contact and food-handling occasions combined (by occasion, intervention motive and measuring methods) 81

Figure 6.1.HWIN scale properties (unstandardized estimates) 159

Figure 7.1. Conceptual framework of the anticipated effect of the TNSB-based handwashing intervention on HWWS after using the toilet, through the intervention effect on the norms-related constructs 169
Figure 7.2. Conceptual framework of the anticipated handwashing station-only intervention effect on HWWS after using the toilet, both through the facility-alone and the unintended norms-related constructs effect 174

Figure 10.1. Trial flow diagram 219
Figure 10.2. HWWS after using the toilet pattern by trial arm and trial phase 229
Figure 10.3. HWWS after cleaning a child's bottom pattern by trial arm and trial phase 236

Figure 10.4. Effect of handwashing interventions on handwashing with soap after faecal contact, by occasion, intervention motive and measuring methods (*with PhD candidate's one-month follow-up point estimates*). 240
Figure 10.5. Effect of handwashing interventions on handwashing with soap after faecal contact, by occasion, intervention motive and measuring methods (*with PhD candidate's five-month follow-up point estimates*)........ 241

Figure 13.1. HWIN scale properties (unstandardized estimates) 283
Figure 13.2. Graph of the perceived handwashing descriptive norm scale items responses distribution at baseline. 286
Figure 13.3. Graph of the perceived handwashing publicness scale items responses distribution at baseline 287
Figure 13.4. Graph of the perceived handwashing injunctive norm scale items responses distribution at baseline 289
Figure 13.5. Scatterplot of the association between HWWS after toilet use and the perceived HWDN........ 291
Figure 13.6. Scatterplot of the association between HWWS after toilet use and the perceived HWP........ 291
Figure 13.7. Scatterplot of the association between HWWS after toilet use and the perceived HWIN 292
Figure 13.8. Perceived norms-related constructs around HWWS after toilet use in the control group by trial phase........ 302
Figure 13.9. Level of the perceived norms-related constructs around HWWS after toilet use in the HWS-only group by trial phase........ 303
Figure 13.10. Level of the perceived norms-related constructs around HWWS after toilet use in the TNSB-based group by trial phase 304

List of Tables

Table 3.1. Summary of characteristics of included handwashing interventions trials using health-motives 50
Table 3.2. Summary of characteristics of included handwashing interventions trials using non-health-motives........ 54
Table 3.3. Summary of characteristics of included handwashing interventions trials using both health and non-health motives 56

Table 5.1. Compound characteristics........ 128
Table 5.2.Households characteristics 129
Table 5.3. Age and sex distribution of compounds residents observed at key occasions 130
Table 5.4. Handwashing practices at key occasions observed by structured observations 130
Table 5.5. Odd Ratios of the association between handwashing and gender and handwashing with soap and gender 131
Table 5.6. Odd Ratios of the association between handwashing and age group and handwashing with soap and age group 132
Table 5.7. Respondents' self-reported occasions when they wash their hands with soap133

Table 6.1. Finalised, context-specific Likert-type response scale. The top row shows the conventional response categories in English. The second row shows the context-specific response expressions in French. The third row presents the response scale in Dioula, and the last row, the corresponding score for each response category. 151
Table 6.2. Finalised handwashing norms scales 156
Table 6.3. Age and sex distribution of respondents to the handwashing norms scales ... 157
Table 6.4. Handwashing social norms-related scales items distribution........ 158
Table 6.5. Matrix of inter-item correlations for each scale *(Spearman-Brown coefficient)* 159
Table 6.6. Summary of the psychometric properties of the HWDN, HWP and HWIN scales 159

Table 9.1. Anticipated and actual trial duration by activity........ 197

Table 10.1. Baseline characteristics of compounds, households and individuals by trial arm (August-September 2014)........ 220

Table 10.2. Baseline observed handwashing behaviours after toilet use and after cleaning a child's bottom by trial arm (August-September 2014) (Intention-to-treat) 222
Table 10.3. Characteristics of compounds clusters, households and individuals by study arm at the one-month follow-up round (September-November 2016) 226
Table 10.4. Characteristics of compounds clusters, households and individuals by trial arm, at the five-month follow-up round (January-March 2017) 227
Table 10.5. Observed handwashing behaviours after using the toilet, by trial phase and trial arm (Baseline: August-September 2014; One-month follow-up: September-November 2016; Five-month follow-up: January-March 2017) (Intention-to-treat) 230
Table 10.6. Unadjusted and adjusted analyses of the association between the interventions and handwashing with soap practices after using the toilet, by follow-up phases *(Intention-to-treat random effects logistic model)* 233
Table 10.7. Observed toilet occasions when hands were washed with soap and with water-only using the handwashing station, by trial phase (on-treatment) 234
Table 10.8. Observed handwashing behaviours after cleaning a child's bottom, by trial phase and trial arm (Baseline: August-September 2014 One-month follow-up: September-November 2016; Five-month follow-up: January-March 2017) (Intention-to-treat) 237
Table 10.9. Unadjusted analyses of the association between the interventions and handwashing with soap practices after cleaning a child's bottom (Intention-to-treat random effects logistic model) 239
Table 10.10. Age-group stratified analysis of observed HWWS practices after using the toilet, by intervention arm and follow-up point (Intention-to-treat) 243
Table 10.11. Age group-stratified odds ratios of the association between the interventions and HWWS practices after using the toilet (Intention-to-treat adjusted analysis) 243
Table 10.12. Comparison of the assumptions used for the trial's sample size calculation and actual observed estimates. 245

Table 11.1. Compound residents TNSB-based handwashing intervention exposure and key intervention messages recollection (seven months post initial intervention delivery-March-April 2017) 256
Table 11.2. Results of the vote to assess the intervention ability to trigger disgust in participants, by intervention delivery round 259

Table 12.1. Handwashing station location in the compound and residents chosen HWS maintenance 264
Table 12.2. Handwashing station sustainability assessment by study arm, at one-month follow-up (September-November 2016) 267

Table 12.3. Observed handwashing station maintenance status according to the type of arrangement chosen to replenish the HWS with water, at one-month follow-up (September-November 2016). 268
Table 12.4. On-treatment handwashing station sustainability assessment by study arm, at five-month follow-up (January-March 2017) 270
Table 12.5. Observed handwashing station maintenance status according to the type of arrangement chosen to replenish the HWS with water, at five-month follow-up 270
Table 12.6. Handwashing station sustainability assessment by study arm, and presence of intervention posters in the TNSB-based intervention group, at seven-month follow-up (March-April 2017) 275
Table 12.7. Observed handwashing station maintenance status according to the type of arrangement chosen to replenish the HWS with water, seven months post intervention. 276
Table 12.8. Description and results of comparable studies 280

Table 13.1. Matrix of scales inter-item correlation (Spearman-Brown coefficient) 282
Table 13.2. Summary of the psychometric properties of the HWDN, HWP and HWIN compared to the ones from the 2014 scales design study 283
Table 13.3. Age and sex distribution of respondents to the handwashing norms-related scales at baseline (August-September 2014) 284
Table 13.4. Norms-related scale items' response distribution at baseline by trial arm *(Intention-to-treat)* 285
Table 13.5. Mean and median scores of the perceived descriptive norm around handwashing with soap after toilet use at baseline (August-September 2014) 286
Table 13.6. Mean and median scores of the perceived behaviour publicness around HWWS after toilet use at baseline (August-September 2014) 288
Table 13.7. Mean and median scores of the perceived injunctive norm around handwashing with soap after toilet use at baseline (August-September 2014) 289
Table 13.8. Unadjusted analyses of the association between HWWS after using the toilet and the perceived norms-related constructs at baseline *(random effects logistic model) (Intention-to-treat)* 293
Table 13.9. Age and sex distribution of respondents to the handwashing norms-related scales at one-month follow-up (September-November 2016) 294
Table 13.10. Age and sex distribution of respondents to the handwashing norms-related scales at five-month follow-up (January-March 2017) 295
Table 13.11. Unadjusted analyses of the changes over time in the perceived descriptive norm, ... 297
Table 13.12. Norms-related scales items response distribution at the one-month and five-month follow-ups by trial arm *(Intention-to-treat)* 299

Table 13.13. Unadjusted analyses of the difference in the way respondents rated the norms-related scale items, by trial arm and trial phase 300

Chapter 1 - Introduction: Thesis structure and contribution of the candidate

This chapter details the thesis structure and the contribution of the PhD candidate to the work presented.

1. Thesis structure

This PhD thesis is organised in a book-style format with a single narrative. It is structured as follows:

Chapter 2 presents the background to the PhD research by defining diarrhoeal diseases, their main mode of transmission and the disease burden in children under 5 years of age. The child mortality rate in Côte d'Ivoire, and diarrhoeal disease burden in the country are also described. The chapter also presents handwashing with soap (HWWS) as a diarrhoeal diseases' prevention method, and the scientific evidence for its effect on reducing diarrhoeal diseases. The HWWS frequencies in Sub-Saharan Africa and Côte d'Ivoire are also reviewed. The low observed HWWS frequencies are discussed, and novel handwashing promotion strategies are described. The thesis rationale and objectives are also presented in this Chapter.

Chapter 3 presents a systematic review of behaviour-change interventions to promote HWWS in low and middle-income countries, comparing interventions by handwashing occasions, and based on intervention motives.

Chapter 4 presents the conceptual framework that informed the design of an intervention to promote HWWS in Abidjan, Côte d'Ivoire. It defines social norms theories in general, and then focuses on the theory of normative social behaviour (TNSB), as the thesis' conceptual framework. The TNSB is then applied to HWWS, to understand its low rate. This chapter also presents a literature review of the effect on handwashing practices of intervention studies that have used social norms theories (to at least some extent) in the design of their interventions.

Chapter 5 describes the results of a cross-sectional pilot study conducted in housing compounds in Koumassi commune, Abidjan, Côte d'Ivoire, in 2012, and which informed subsequent work. The study assessed the prevalence of HWWS after key opportunities, and the acceptability of handwashing stations (HWS) in this setting.

Chapter 6 details the process of designing and testing the psychometric properties of scales to measure four handwashing-related norms constructs, in housing compounds in Koumassi commune in 2014.

Chapter 7 outlines the process of designing the TNSB-based handwashing intervention and handwashing station (HWS)-only intervention.

Chapter 8 details the implementation of the interventions.

Chapter 9 describes the cluster randomised controlled trial design and data collection methods, as well as the statistical analysis plan.

Chapter 10 presents an analysis of the trial interventions' effects on HWWS practices after visiting the toilet and after cleaning a child's bottom, as well as the observed use of the HWS after toilet use.

Chapter 11 presents a process evaluation of the TNSB-based handwashing intervention, using data collected during and post-intervention delivery.

Chapter 12 describes the findings regarding the acceptability and sustainability of the HWS.

Chapter 13 presents an analysis of the TNSB-based intervention's effect on handwashing-norms related constructs. It also reports the psychometric properties of the handwashing norms related scales during the trial.

We conclude the thesis in **Chapter 14** by synthesising the research findings and their implications for the handwashing research field, and discuss the study's limitations. We also discuss the implications for policy and programming.

2. Contribution of the candidates to the thesis research

This PhD research was conducted under the supervision and guidance from the PhD candidate's lead supervisor (Professor Simon Cousens) and co-supervisor (Professor Jim McCambridge), and contributions from the advisory committee members. The candidate's supervisors provided critical feedback on all thesis drafts written by the PhD candidate.

I wrote the thesis research proposal, designed the data collection tools and designed and managed the fieldwork in Koumassi, Abidjan, Côte d'Ivoire. I also designed and conducted the pilot studies, with data collected by fieldworkers that I hired and trained. I managed the trial with the assistance of two Koumassi residents (Hermann Aka and Patrice Blon). I hired and trained the latter in 2014, to assist in the fieldwork study to validate the handwashing norms-related scales; and subsequently to assist me in managing the trial's fieldwork activities. I also hired and trained all trial data collection fieldworkers and the interventions implementers.

I designed the handwashing interventions. i wrote the scripts for the short video clips with the two fieldwork assistants. The dialogues were written with the latter and a brand manager (Anthony Kouakou) from a local beverage company, and a copywriter designer (Chrysostome N'Guessan) from a local communications company. i also hired a graphic artist (Olivier Kouamé) to design the video clips' storyboards, based on the scripts. The latter also designed the TNSB-based intervention posters. I hired a production company to produce the video clips and partook in the making-of the clips (production and post-production). I piloted all intervention components and the full intervention, with the support of the assistants.
I did not take part in data collection in the trial nor intervention implementation and process evaluation, as participants and fieldworkers were kept unaware (i.e. masked) of the research

objectives, to minimise the risk of bias. I wrote the data statistical analysis plan and conducted all statistical and qualitative analyses presented in this thesis. I also conducted the systematic review..

Chapter 2 - Background and Research Objectives

It is estimated that each year, 5.3 million children die before reaching the age of five years old, with the highest proportion of child deaths in Sub-Saharan Africa [1]. In 2015, the Sustainable Development Goals (SDGs), comprising 17 goals to be achieved by 2030, were adopted, as a follow-up to the 1990-2015 Millennium Development Goals (MDGs) [2]. Goal 3 of the SDGs aims to *"Ensure healthy lives and promote well-being for all at all ages."* [3]. This includes ending the preventable deaths of children under five years old, by reducing the under-five mortality rate (U5MR) below 25 per 1,000 live births [3]. Whilst some causes, such as for some non-communicable diseases, would require substantial means and sophisticated technologies to be addressed, others are entirely preventable using relatively modest resources [3]. One such disease is diarrhoea.

1. Diarrhoeal diseases

Diarrheal diseases account for approximately 15% of deaths in children in the 1-59 month (i.e. post-neonatal) period worldwide (Figure 2.1) [1, 4]. Globally, diarrhoea is the second leading cause of death in children under five years old, after pneumonia [1, 4]. An estimated 0.5 (95% Uncertainty Range (UR): 0.4-0.7) million deaths in children under five years old are attributable to diarrhoeal diseases [4], and Sub-Saharan Africa remains the region with the highest burden (Figure 2.2) [5, 6]. In 2017, diarrhoeal diseases were responsible for an estimated 27 million (95% Uncertainty Interval (UI): 23 million-31 million) disability adjusted life years (DALYs)[1], in Sub-Saharan Africa [5]. This is compared to an estimated 66,000 (95% UI: 60 000-72,000) DALYs in high-income countries (HICs)[5]. In terms of death, an estimated 291,000 (95% UI: 244,000-343,000) deaths in Sub-Saharan Africa are attributable to diarrhoeal diseases [5, 7]. This is compared to an estimated 800 (95% UI: 700-840) in HICs [5, 7].

[1] DALYs are the sum of years of life which were lost (YLLs), due to premature death, and years which were lived with disability (YLDs). In the case if Diarrhoea, DALYs reflect the disease's acute outcomes.

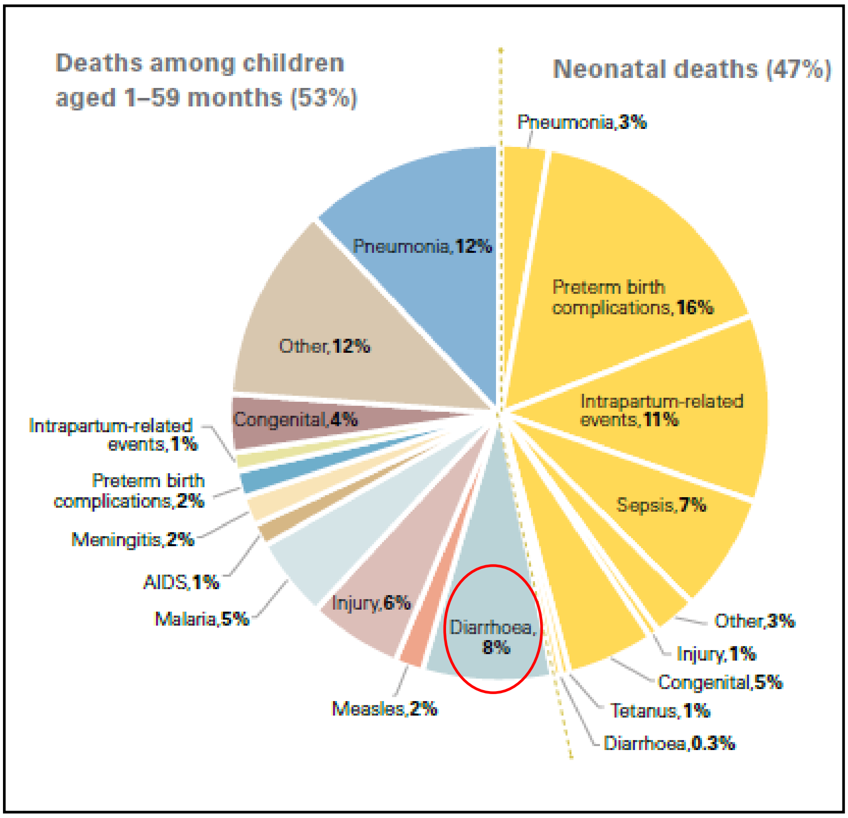


Figure 2.1. Causes of death in children under five years old in 2018. Source: UNICEF [1]

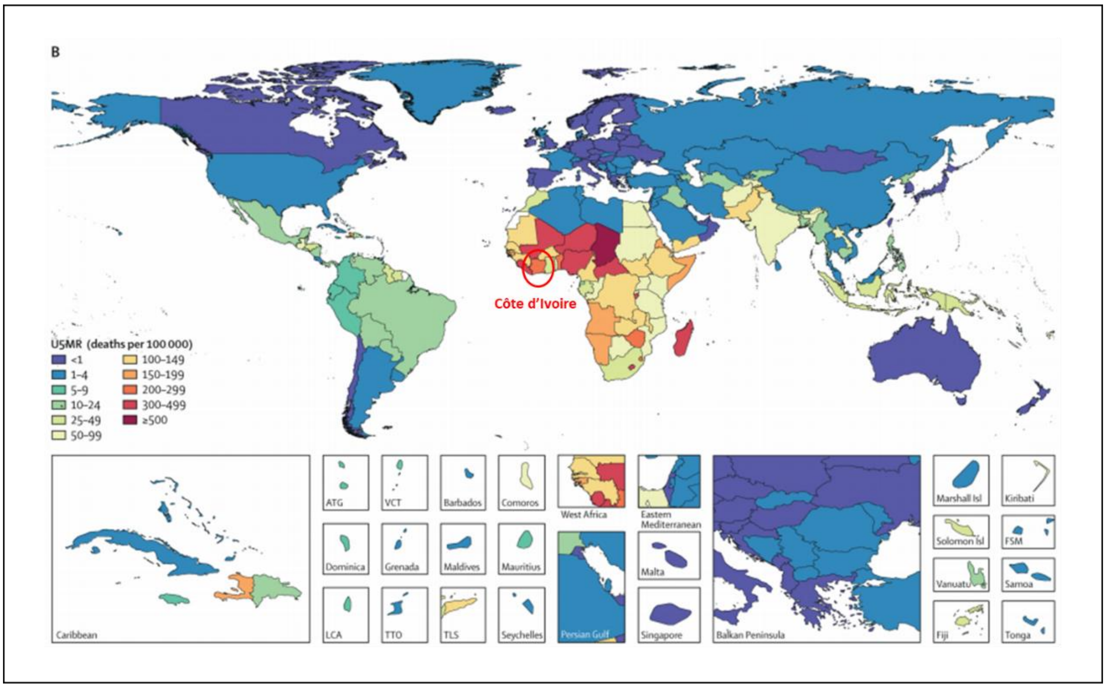


Figure 2.2. Mortality rate of children under five years old per 100 000 population due to diarrhoea in 2016. Source: Collaborators [7]

With an U5MR of 89 per 1,000 live birth, Côte d'Ivoire remains among the countries with the highest child mortality rates in the world [8], with diarrhoea as the third leading infectious cause of death in children under five years old [9]. It is responsible for an estimated 500,000 (95% UR: 300,000-800,000) DALYs [5] and 6, 000 (95% UR: 4,000-9,000) deaths in children under 5 years of age [5, 7].

1.1. Definition of diarrhoea, its main causes and modes of transmission

The World Health Organization (WHO) defines diarrhoea as:

> *"The passage of three or more loose or liquid stools per day (or more frequent passage than is normal for the individual) [10]*

Most episodes of diarrhoea are caused by infectious organisms, such as protozoa, bacteria, helminths and viruses [11], that have reached the gastrointestinal tract [10]. The main transmission route for these organisms is through ingesting water or food contaminated with faecal matter, or from person-to-person contact, due to poor hygiene [10, 12, 13]. This principal transmission mode is known as the faecal-oral transmission route [11]. Key risky behaviours that are linked to diarrhoea are the unsafe disposal of faeces, and the absence of handwashing with soap (HWWS) after coming into contact with faeces (e.g. after defecating or cleaning a child's bottom) or before handling food [12, 14]. Wagner and Lanoix's (1958)[2] 'F-Diagram' depicts the faecal-oral transmission paths (Figure 2.3). The risk of transmission of infectious diseases is often greater in urban than rural areas, due to high population density, and therefore greater level of interaction, and living proximity [16]. Additionally, poor living conditions and lack of safe water and sanitation access put economically disadvantaged urban communities at high risk of diarrhoeal diseases [16].

---

[2] As cited in 15. Scott, B., *WELL factsheet. Health impacts of improved household sanitation.* 2006.

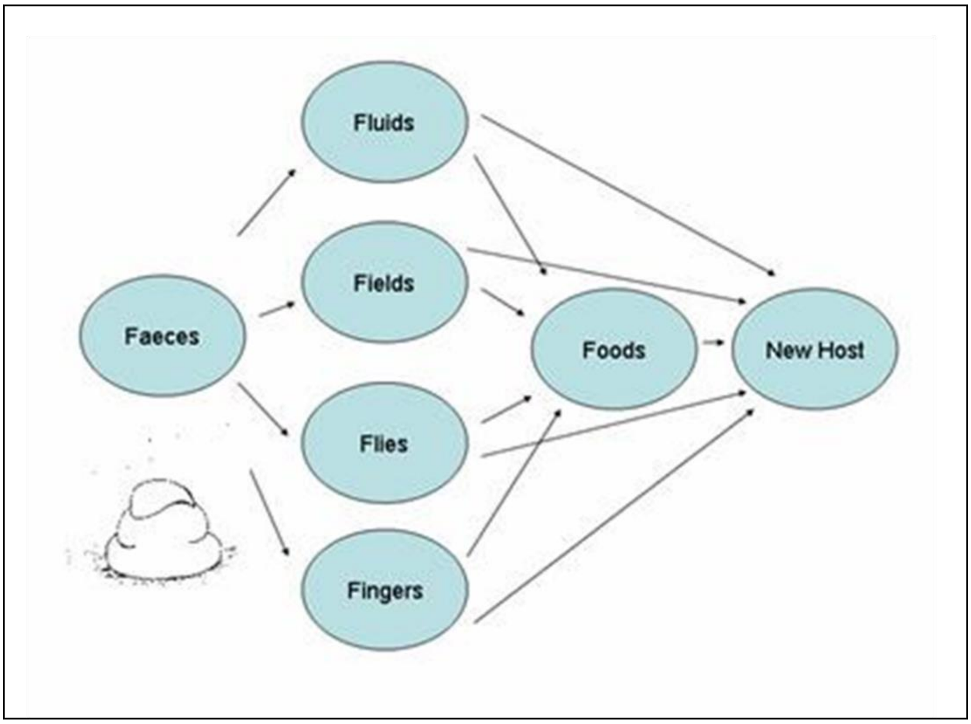


Figure 2.3. The 'F-Diagram' of faecal-oral transmission route, as per Wagner and Lanoix (1958)[3].

2. Handwashing with soap

2.1. Handwashing with soap for cost-effective prevention of diarrhoeal diseases.

Handwashing with soap, after coming in contact with faeces and before eating, is considered as potentially among the most cost-effective methods of preventing diarrheal diseases, by interrupting the faecal-oral transmission of pathogens responsible for diarrhoea [12, 17, 18]. Ejemot-Nwadiaro et al. (2015) conducted a systematic review assessing the effect on diarrhoea of interventions promoting handwashing in community and school settings and patients in hospital settings. [12]. Their review included individually randomised controlled trials (RCTs) and cluster randomised controlled trials (cluster-RCTs) [12].

A total of 22 studies were included in the review [12]. Of these, 10 were from HICs, and 12 from low and middle-income countries (LMICs) [12]. Twelve trials were cluster-RCTs conducted in schools or child day-care centres, and included 54,006 children [12]. Nine cluster-RCTs were

[3] As cited in 15. Ibid.

conducted in community settings, with 15,303 children [12]. The remaining trial was conducted in a hospital-based setting, and included 148 adults [12]. All interventions were composed of hygiene education messages [12]. Along with hygiene education, examples of other interventions components included were the provision of handwashing material (in seven trials), and handwashing behaviour change messages (in four trials) [12].

Ejemot-Nwadiaro et al. (2015) found that hygiene interventions including handwashing promotion could prevent around 30% of diarrhoea episodes (in schools or child day-care facilities), in both HICs (rate ratio (RR)=0.70; 95% CI: 0.58-0.85, 4,664 participants, from nine trials, high quality) and LMICs (RR=0.66, 95% CI: 0.43-0.99, 45, 380 participants, from two trials, low-quality) [12]. In community settings in LMICs, the authors found that handwashing interventions could also prevent about 30% of diarrhoea episodes (RR=0.72, 95% CI: 0.62-0.83, 14,726 participants, from eight trials, moderate quality evidence) [12]. However, the authors pointed out that six out of the eight trials were from Asia, with only one trial from Sub-Saharan Africa, and another from South America [12]. When restricting the analysis to studies which attempted to blind outcome assessors, Ejemot-Nwadiaro et al found a lower point estimate, compared to trials where outcome assessors were not blinded (RR=0.80, 95% CI: 0.67-0.94, from four trials, 11,656 participants). Ejemot-Nwadiaro et al. reported high heterogeneity between studies [19]. This pertained to handwashing material supply, intervention type and components, and study characteristics (blinding and cluster adjustment) [12].

Mbakaya et al. (2017) conducted a systematic review to identify handwashing intervention strategies in the reduction of infectious diseases more broadly, including diarrhoea and respiratory tract infections, in school children in developing countries [22]. A total of eight RCTs and cluster-RCTs were included in this review, including three trials which were also included in Ejemot-Nwadiaro et al.'s review [12, 22]. The sample sizes ranged from 398 to 44, 451 [22]. Mbakaya et al. found that handwashing could decrease the incidence of diarrhoea by 53% to 73% [22]. However, the authors reported that most of the studies lacked methodological rigour, making the findings uncertain [22]. Six studies were judged as providing low-quality, and the remaining two as high-quality[22]. The main study limitations were a lack of information on the random sequence generation methods and lack of blinding of participants and outcomes assessors [22].

Using the pooled estimate of a 48% reduction in severe diarrhoea reported by Cairncross et al. in 2010 [23], Greenland et al. (2013) estimated that 360 000 deaths could be prevented by HWWS at key points [17]. This reduction is higher than the estimate from the more recent review by Ejemot-Nwadiaro et al. reported above [12]. Greenland et al.'s figure may thus be an overestimate [17]. Nevertheless, the 30% reduction estimated by Ejemot-Nwadiaro et al. would still lead to a substantial reduction in the number of diarrhoeal disease deaths [12].

Wolf et al. (2018) conducted an updated systematic review assessing the impact of unsafe water, sanitation and hygiene (including handwashing) on diarrhoeal disease in children, in community and institutional settings [20]. Both RCTs and non-RCTs, and studies from HICs and LMICs were included[20]. A total of eight new sanitation studies and hygiene studies each, and 14 new water studies were added to the review [20]. This comprised a grand total of 73 studies (with 33 handwashing studies) included in the meta-analysis [20].

Wolf et al found that hygiene interventions were associated with a 30% reduction in childhood diarrhoea. Similar results were obtained when including all studies (RR=0.70, 95% CI: 0.64, 0.77), when excluding non-RCTs (RR=0.74, 95% CI: 0.65, 0.83), and when excluding studies in HICs (RR=0.70, 95% CI: 0.62, 0.78) [20]. This is comparable to the findings of Ejemot-Nwadiaro et al. However, when the lack of blinding was taken into account, Wolf et al found little evidence of an effect of hygiene interventions on diarrhoea (RR=0.90, 95% CI: 0.37 to 2.17) [20]. Wolf et al and Ejemot-Nwadiaro et al findings indicate that studies that have attempted blinding tend to estimate smaller to no hygiene interventions effect on diarrhoeal disease than those which did not.

Interestingly, the type of soap used for handwashing (i.e. plain soap vs. 'commercial' antibacterial soap) does not seem to significantly affect HWWS effectiveness in improving health (excluding healthcare settings) ([24-26]). De Witt Huberts et al. (2016) conducted an integrative review to assess whether adding anti-bacterial agents to handwashing products led to an increase in the health benefits of HWWS in developing countries [25]. Only four RCTs were identified (i.e. [27-30]), among which two were based on the same study (i.e.[28, 29]). Two of the identified trials were from a low-income country (Pakistan) (i.e. [27-29]), and the remaining one was from a HIC (the United States) (i.e. [30]). Aiello et al. (2008) had previously reviewed three of the identified studies (i.e. [27-29]) in a meta-analysis [24]. When looking at the results of studies investigating gastrointestinal illnesses, these latter found no evidence of a difference in the effect on health of using antibacterial soap compared to non-antibacterial soap (RR=0.99; 95% CI: 0.54-1.83, from two studies) [25]. Similar results were found for the combination of gastrointestinal and respiratory illnesses combined (RR=0.96; 95% CI: 0.71-1.30, from one study) [25].

2.2. Low rates of handwashing with soap after contact with faeces

Whilst diarrhoeal diseases can be prevented via HWWS after coming in contact with faeces and before handling food, findings from various studies show that HWWS frequencies in many settings are low [31]. Freeman et al. (2014) conducted a systematic review of global handwashing practices and health effects [31]. The authors only included studies that used structured observations to measure HWWS [31]. This is due to the social desirability bias attached to HWWS, leading to over-reporting of the practice when measured via self-reporting (e.g. interviewing someone and asking questions is likely to overestimate the frequency of handwashing) [32-35].

Forty-two studies were included in the review by Freeman et al. The authors found that, in Sub-Saharan Africa, HWWS prevalence after coming in contact with faeces ranged between 5% and 22% (13 studies from 7 countries) [31]. While structured observations are seen as a more reliable method of measuring HWWS practices compared with self-report, there is nevertheless the risk of a Hawthorne effect attached to the use of direct observations [36-39]. This occurs when the population under investigation modifies their behaviour (i.e. increasing or decreasing their usual practices) as a reaction to the presence of an observer, thereby introducing bias [36-39]. Nevertheless, the HWWS estimates reported by Freeman et al. 2014 are still very low, despite the risk of reactivity.

Wolf et al. (2018) conducted a study, using national survey data, to measure the presence of designated handwashing facilities, analyse the association between these facilities and observed HWWS, and derive country, regional and global-level HWWS frequencies after faecal contact [21]. Only studies using observed HWWS frequencies were included. The authors estimated that, in LMICs in the WHO African regions, the HWWS frequencies after faecal contact was 8% (95% CI: 5%-14%). This is within the range found by Freeman et al. (2014).

In Côte d'Ivoire, the United Nations Children's Fund (UNICEF) (2018) estimates HWWS practices after key opportunities to be lower than 4% [40], though it is unknown how this estimate was derived. Kumar et al. (2017) conducted a handwashing study analysing the proxy measures of handwashing practices in 51 countries, using demographic and health surveys (DHS) and multiple indicator cluster surveys (MICS) between 2010 and 2013 [41]. The authors reported that, in Côte d'Ivoire, 13% of households had water and soap observed at the dedicated handwashing location (N=9, 686). The presence of water and soap at the handwashing location is sometimes used as a proxy indicator of handwashing practices (e.g.[42-45]). Nevertheless, the presence of handwashing materials at the handwashing location does not guarantee that the supplies are used for handwashing at critical occasions [46]. The difference between the estimates of HWWS reported by UNICEF (2018) and Kumar et al. (2017) illustrates the difficulty of reliably measuring HWWS practices. Nevertheless, both estimates suggest that HWWS after defecation is uncommon in Côte d'Ivoire.

2.3. Factors influencing handwashing with soap practices

2.3.1. Health may not be a strong motivation for handwashing

Research suggests that health may not be a strong motivation for hygiene-related behaviour, and thus HWWS [29, 47-52]. For instance, Biran et al. (2009) conducted a study in rural India aimed at increasing HWWS through hygiene education and increasing germ-awareness [53]. The authors found that while germ awareness increased post intervention, there was no evidence of an effect of the intervention on HWWS at key occasions. Biran et al. (2009) reported that HWWS on key occasions did not change four weeks after the intervention in either the intervention arm (from 8% at baseline to 5% at follow-up) or the control arm (from 5% at baseline to 6% at follow-up) with P=0.35 [53]. In a formative research study conducted in Uganda, which collected baseline information on HWWS, 84% of adults knew of the importance and need to practice HWWS after using the toilet, as it pertains to disease prevention, but only 14% were observed doing so [54]. Given that not HWWS does not always produce illness, and when it does, the onset does not immediately follow the absence of HWWS, it is difficult for individuals to identify the association between the risk behaviour and the adverse outcome [55].

Whilst health does not seem to be a strong motivating factor for HWWS, a number of handwashing determinants have been identified. These include social norms [48, 56, 57], disgust [48, 58, 59], and environmental enablers (i.e. the presence of handwashing facilities and material) [51, 53, 60].

2.3.2. Social norms as a potentially key handwashing motivator

Social norms have been shown to be potentially significant influencers of handwashing-related behaviours. In a study assessing medical students' compliance to medical guidelines (including hand hygiene) in Thailand, Apisarnthanarak et al. (2006) found that among the many factors that explained students' non-compliance, observing other colleagues not complying was an important determinant [61]. Comparable findings were reported by Lankford et al. (2003) who found that students were less likely to comply with hand hygiene if peers were seen as non-compliers [62]. Similarly, in an experimental study in a college's public restroom aimed at testing the impact of increased self-awareness on handwashing behaviours, Munger & Harris (1989) found some evidence of a large effect of the presence of an observer on HWWS practices [63]. The authors reported that in the intervention group where an observer was present in the restroom with participants, 77% of participants washed their hands compared to 39% in the intervention/non-observer group (χ^2=8.78, P<0.05). Judah et al. (2009) conducted an experimental study aimed at pretesting the effectiveness of message domains at increasing HWWS after toilet use in a public restroom in the United Kingdom [64]. The authors reported that messages related to social norms and social status were the only message domains which were effective for both women and men, with a respective 11% and 12% relative increase in handwashing practices compared to the control group [64].

Thus, the presence of others, the awareness that one's behaviour can be scrutinised, and others' engagement in handwashing behaviour may have an impact on individual's handwashing behaviours.

2.3.3. Disgust as a potentially key handwashing motivator

There is a strong case for the emotion of disgust being used as a powerful motive when promoting HWWS. Hygiene behaviour, including HWWS, is motivated in a wide range of societies by the desire to remove things that are found disgusting [58]. Sensory cues that trigger disgust include materials that feel sticky, moist, damp, or hand-hot; but also the sight of things that resemble disgusting objects [58]. Materials perceived as being dirty such as rubbish, stained toilets or sheets, and bad smell also cue disgust [58].

As it pertains to handwashing, formative research in 11 countries found that hands are washed to remove things that cause discomfort or leave other things/people (i.e. surfaces, social interactions) dirty when touched [48]. Soap is only seen to be needed to remove 'visible' organic material perceived as dirty, and which is not easily removed by water alone, such as oily food or stains [48]. By contrast, germs are 'invisible' contaminants. For instance, after visiting the toilet, hands are usually not 'visibly' stained by faeces or urine. Hands can therefore appear to be clean. This could explain why rates of HWWS after toilet use are low, and thus, why health education models to promote HWWS at this critical occasion may be ineffective. Faeces appear as a particularly disgusting contaminant across societies [48, 54, 58].

Biran et al. (2014) conducted a handwashing behaviour change, village-level cluster-RCT in Southern Andhra Pradesh, India [65]. The study evaluated the effectiveness of a handwashing intervention using emotional drivers (i.e. disgust, nurture, affiliation and status) at increasing handwashing with soap at four key occasions (after defecation, after cleaning a child's bottom, before food preparation, before eating) [65]. This was compared to no intervention [65]. The authors found strong evidence of a large intervention effect on HWWS at key occasions. HWWS went from 1% to 19% in the intervention arm vs 2% to 4% in the control arm, at the 6 weeks follow-up (P=0.005) [65]. When restricting the analysis to possible faecal contact events (after defecation and after cleaning a child's bottom), the authors found weak evidence of a large effect of the intervention on HWWS after these occasions. HWWS increased to 28% in the intervention group, compared to 7% in the control group (P=0.18).

Guiteras et al. (2016) conducted a cluster-RCT in compound residents in slums in Dhaka, Bangladesh [66]. The study aimed to evaluate the effect of an intervention, designed using negative emotions (i.e. disgust at consuming human faeces), and social pressure (i.e. shame from being perceived to ingest human faeces), on handwashing with soap practices, among other study aims [66]. This intervention was compared to an intervention designed using conventional health messages (i.e. germ transmission through unwashed hands and how HWWS prevented illnesses), and a control/no handwashing intervention group [66]. Two months post-intervention delivery, Guiteras et al. (2016) found evidence of a small intervention effect on

HWWS practices after toilet use, when combining both handwashing treatment groups. [66]. HWWS practices went from 10% at baseline to 15% in the handwashing intervention groups combined, compared to a change from 10% to 11% in the control arm ($P<0.01$) [66].

2.3.4. Improving the physical environment to facilitate handwashing with soap practices

It is important not only to tap into the most relevant motives to increase HWWS practices, but also to ensure that the targeted behaviour is relatively easy to perform. In many low-income communities, HWWS is not a convenient practice. Indeed, although sinks in kitchens and bathrooms are the norm in developed countries, such facilities are commonly absent in economically disadvantaged communities [21, 48, 60].

While availability of water and soap are essential for the performance of HWWS, the presence of both these factors in communities may not be sufficient to trigger the behaviour. For instance, if an already busy mother has to go to fetch soap to bring it to a water source, after having used the toilet, the extra time needed to do so may act as a deterrent to HWWS [60]. In their 11-country formative research study, Curtis et al. (2009) reported that more than 95% of communities had soap available at household level, despite the low occurrence of HWWS (i.e. HWWS estimates ranging between 0% and 47%) [48]. However, to avoid being wasted or spoiled, soap was frequently kept out of reach [48]. By contrast, the authors found few households had handwashing facilities or a specific location to wash hands near the toilet [48]. Using the global database of WHO/UNICEF Joint Monitoring Programme for Water Supply, Sanitation, and Hygiene, Wolf et al. estimated that in LMICs in WHO's African region (38 countries included), the proportion of households with an observed handwashing facility with water and soap present was 18% [21].

In recent years, there has been an emergence of research suggesting that what is critical is for soap and water to be readily available together, ideally close to toilet areas or food preparation areas [43, 67, 68]. In that regard, it has been suggested that having 'enabling products', such as HWS would make handwashing more convenient [60]. For instance, Luby et al. (2009) conducted a study looking at household characteristics associated with HWWS in rural Bangladesh. The authors found that having water available at the handwashing site after visiting the toilet (OR=2.2, 95%CI [1.3, 4.0]), and having soap available at the handwashing site after visiting the toilet (OR=2.1, 95%CI [1.3, 3.4]) were both associated with HWWS after faecal contact [67]. Wolf et al. predicted that, after faecal contact, households with a dedicated place for handwashing with both water and soap present on site (including handwashing facilities) were approximately twice as likely to practice HWWS compared to households without such facilities (95% CI: 1.66, 2.39) [21]. HWS also provide a stable environment for HWWS to be performed [60, 68], and having a constant context where a behaviour repeatedly takes place is key for habit formation [69-72].

Biswas (2012) conducted a cluster-RCT aimed at assessing the uptake of HWWS or soapy water in a low-income community in Dhaka, Bangladesh [46]. One intervention arm was given cholera vaccine, water treatment and a handwashing intervention (Vaccine+handwashing treatment (HWT) group) [46]. The intervention included the provision of HWS and handwashing promotion to households [46]. Participants were encouraged to wash hands with soap after coming in contact with faeces and before handling food [46].The second intervention group only received the cholera vaccine (Vaccine-Only group), while the control group did not receive any intervention [46].

The author reported that the presence of water and soap or soapy water at handwashing sites in households from the Vaccine+HWT group increased from 22% at baseline to 60% post intervention ($P<0.001$), while no such increase was seen in the Vaccine-Only group or the Control group [46]. Biswas (2012) concluded that the provision of hardware to enable handwashing was successful at changing handwashing behaviour in this community [46]. However, as acknowledged by the author, there is no guarantee that water and soap at the HWS were indeed used for handwashing at key moments [46]. Additionally, given there was not an intervention group which received a handwashing promotion-only intervention (i.e. without a combination of HWS supply), it is difficult to assess whether the assumed increase in handwashing practices was due to the hardware provision or the handwashing promotion.

Similarly, Ram et al. (2017) conducted an RCT in rural Bangladesh aimed at increasing HWWS practices of mothers in the neonatal period after key events, including after coming in contact with faeces [45]. The intervention included both handwashing promotion messages and the supply of HWS with soap [45]. Ram et al. (2017) reported evidence of a large effect of the intervention on the presence of water and soap at the HWS. Whilst only 12% of participants were observed with both water and soap at the main HWS at baseline, two months post-intervention delivery, 39% of intervention households had HWS with water and soap present at all assessment visits, compared to 3% of control households (*P*<0.001) [45]. By contrast, the observed proportion of HWWS practices after coming in contact with faeces was 14% in the intervention group compared to 5% in the control group (risk ratio = 3.21 (95% CI: 1.42-6.30) [45]. There was thus a discrepancy between the large intervention effect observed regarding the proxy indicators of HWWS, and the more modest effect on actual HWWS practices. Post intervention, the HWWS frequency after faecal contact remained relatively modest in the intervention group, despite the size of the effect the intervention had on the presence of water and soap. As in Biswas' study [46], we cannot assess the effect of the HWS-alone on HWWS practices, given the absence of an HWS-only intervention group.

It could be hypothesised that without the internalisation of the importance of HWWS practice, the presence of handwashing facilities would not necessarily substantially increase HWWS without promotion. This hypothesis is supported by Wolf et al.'s study findings [21]. As part of their study, the authors conducted a meta-analysis of HWWS frequencies after probable faecal contact in HICs. Fifteen studies were included [21]. The authors reported that the results from the pooled estimate showed that the proportion of plausible faecal contact events followed by HWWS was 51% (95% CI: 43%-59%). The findings are comparable to that reported by Freeman et al. (2014) (i.e. HWWS frequencies ranging between 43% to 49% in HICs) [73]. However, Wolf et al. estimated that, in 2015, 95% of populations in HICs had access to a designated handwashing facility [21].

3. Gaps in the literature

In summary, diarrhoeal diseases remain among the leading causes of child mortality despite being preventable by means as cost-effective as HWWS after coming in contact with faeces, among other occasions. The highest burden of diarrhoeal disease mortality remains in Sub-Saharan Africa. HWWS frequencies are very low in this region of the world, perhaps as low as 4% in Côte d'Ivoire. Although health remains the most common message used to promote HWWS practices, some studies suggest that health messages may not be effective at motivating the practice, perhaps explaining the low rates observed globally. However, social norms, disgust and enabling technologies may be effective means of increasing handwashing.

Nevertheless, few studies have assessed the effectiveness of these alternative means in increasing HWWS practices in economically disadvantaged communities in LMICs, notably in Sub-Saharan Africa. To our knowledge, no study has explicitly used social norms theories in the design of interventions to increase HWWS practices in LMIC community settings, or measured the effect of the intervention on handwashing-related norms. Similarly, whilst disgust has been posited as a key handwashing motivator, very few trials have assessed the effectiveness of using disgust as a key intervention message to increase HWWS after coming in contact with faeces. Last, but not least, to our knowledge, no trial, in community settings in LMICs, has been conducted to assess the effect of supplying HWS-alone with soap on HWWS after faecal contact. In light of the rapid urbanisation taking place in developing countries, and given goal 3 of the SDGs, there is an urgent need for more intervention trials to assess the impact of non-health education-based HWWS interventions on HWWS practices in economically disadvantaged urban communities in LMICs. This PhD research aims to address the above gaps

4. Study objectives

The overall aim of this research is to contribute to our understanding of the effectiveness of non-health motivators, specifically disgust, and handwashing stations, in increasing HWWS practices after using the toilet (primary outcome) and after cleaning a child's bottom (secondary outcome)

The thesis' specific objectives are:

1. To develop a social norms-based behaviour-change handwashing intervention using disgust as a motive
2. To design handwashing social norms-related scales to measure handwashing norms-related constructs
3. To evaluate the effectiveness of a social norms-based intervention using disgust as a motive with provision of HWS with soap on HWWS practices after using the toilet (primary outcome) and after cleaning a child's bottom (secondary outcome)
4. To evaluate the effectiveness of the provision of HWS alone on HWWS practices after using the toilet and cleaning a child's bottom
5. To assess the impact of the interventions on social norms-related constructs
6. To assess the sustainability of HWS

The study was conducted in Abidjan, Côte d’Ivoire.

Chapter 3 - Effect of Behaviour Change Handwashing Interventions on Handwashing with Soap Practices in Low- and Middle-Income Countries: a Systematic review and meta-analyse of randomised controlled trials.

This chapter presents the results of a systematic review and meta-analyses of the effect of behaviour change handwashing interventions on handwashing with soap practices (HWWS), in low and middle-income community settings.

1. Systematic review and meta-analysis objectives

In light of research arguing that health may not be a strong motive for HWWS practices (e.g. [29, 47-51]), we conducted a systematic review (SR) and meta-analysis of the effect of behavioural change HWWS interventions on HWWS practices in low and middle-income country (LMIC) settings. The meta-analyses were first conducted by handwashing occasion and then by interventions' behaviour-change motives (e.g. health vs. non-health intervention motives). The aims of the systematic review were:

1. To assess the effect of behaviour change interventions on HWWS practices after key occasions in LMIC settings.
2. To compare the effect of behaviour-change handwashing interventions using health motives with those using non-health motives, such as emotions, on HWWS practices at key occasions in LMIC settings.
3. To assess whether the intervention's estimated effect depends on the outcome measurement methods (e.g. structured observation vs. self-report).
4. To examine whether the intervention's effectiveness depends on the particular handwashing occasion targeted.

To our knowledge, this is the first systematic review and meta-analyses assessing the effect of behaviour-change handwashing interventions on HWWS practices in LMIC settings, according to the motives used.

2. Methods

We used the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) guidelines [74] to report this systematic review. Appendix 3.1 presents PRISMA checklist. Inclusion criteria and methods of analysis were specified in advance.

2.1. Eligibility criteria

2.1.1 Type of studies

Only randomised controlled trials (RCTs), including cluster-RCTs were included in the review. No language, publication date or publication status restrictions were imposed.

2.1.2. Type of participants

The study population was individuals of any age living in LMIC settings (as per the World Bank's country classification [75]). Studies conducted in domestic settings (e.g. households) were included. We excluded studies conducted in health-care settings, such as hospitals, and targeting health practitioners. We did so as we were interested in handwashing to prevent diarrhoea as opposed to the transmission of microorganisms from healthcare practitioners to patients. Similarly, school-based intervention studies were excluded as the maintenance of water and soap at handwashing locations is often dependent on school staff (e.g. [76, 77]). We did include studies involving schools or clinics, if they also had intervention components implemented at household or community level.

2.1.3. Type of intervention

We included behaviour-change interventions aimed at increasing HWWS practices at key occasions, namely before food-related activities and after faecal contact-related activities. We restricted the systematic review to these occasions given that HWWS before eating and after faecal contact is considered potentially among the most cost-effective methods of preventing diarrheal diseases [12, 17, 18]. We included both handwashing-only interventions and handwashing interventions combined with other interventions such as water and sanitation interventions. There was no restriction on the methods used to promote handwashing practices (e.g. mass media or inter-personnel communication) or intervention components (e.g. videos, posters, pledging, discussions, handwashing equipment supply).

2.1.4. Control group

We had planned to include studies with control groups with no interventions, or control groups that had received an intervention unrelated to handwashing. However, we included one study (i.e. [78]) where the control group received a part of the handwashing intervention implemented in the intervention group. This is because the study was evaluating the effect of an interpersonal communication campaign against a background of mass media.

2.2. Outcomes measures

The primary outcome measures were:

1. The proportion of faecal-contact related occasions (i.e. after using the toilet and/or after defecating, and/or after cleaning a child's bottom) after which hands were washed with water and soap or antibacterial gel;
2. The proportion of food-related occasions (i.e. before cooking, and/or before eating and/or feeding a child) before which hands were washed with water and soap or antibacterial gel.

For the meta-analysis, we only considered studies which reported on changes in handwashing behaviour as a primary or secondary outcome. We restricted the meta-analysis to studies which reported direct measures of handwashing practices (i.e. structured observation and self-reports of handwashing practices). Studies that only reported proxy HWWS measures, such as the presence of water and soap at the handwashing location or microbiological analysis of hands-rinse samples were included in the systematic review but excluded from the meta-analyses. There was no restriction on the length of follow-up.

2.3. Search strategies to identify studies

To identify all studies that met our inclusion criteria, we chose to be as inclusive as possible in our search strategy. The search expression (Appendix 3.2) included terms (and synonyms) for behaviour type (handwashing and synonyms) and study design (RCTs).

2.3.1. Electronic searches

The search strategy was developed and conducted by the PhD candidate. The following databases were searched:

- Cochrane Central Register of Controlled Trials (CENTRAL) (from 1996 to August 16th 2018)
- MEDLINE (from 1946 to August week 2 2018)
- EMBASE (from 1947 to August 20th 2018)
- PubMed (from 1966 to August 18th 2018)
- LILACS (from1982 to August 20th 2018)
- CINHAL (from 1937 to August 22nd 2018)
- Global Health (from 1973 to August 22nd 2018)
- PsycINFO (from 1887 to August 20th 2018)
- International Clinical Trial Registry Platform (ICTRP) (in August 22nd 2018)

Appendix 3.2 presents the full electronic search strategies for PubMed, Cochrane CENTRAL, and Medline. These were the databases which returned the most hits (800, 619 and 541 respectively).

2.3.2. Reference lists and authors

We screened the reference lists of included studies and identified relevant systematic reviews, to find additional pertinent citations to be hand-searched. We also screened the 2002 to 2018 Summary of Randomised Controlled Trials in Developing Countries' list [79]. For the citations identified for which we could not find full-text papers or only found conference papers, we contacted the authors.

Two staff from the London School of Hygiene and Tropical Medicine's Library reviewed the search strategies.

2.3.3. Study selection

The PhD candidate exported the retrieved database search results to Endnote X8, and duplicates were removed. The remaining articles were exported to Mendeley to remove any duplicates which Endnote had failed to identify. The duplicate-free list of results was then exported back to Endnote. The PhD candidate screened the titles and abstracts of all identified articles to decide which to retrieve the full text for, applying the pre-defined inclusion and exclusion criteria. The full text of studies that appeared potentially relevant were retrieved for further assessment. Decisions on which studies to include were taken by the PhD candidate. In case of doubt, the PhD candidate consulted their supervisor. Studies published as conference presentations or posters which did not have full-text articles were excluded.

3. Data collection process and data items

The PhD candidate extracted study data using Excel forms created for this purpose, which they piloted on a sample of included studies. The studies were classified according to the behaviour change motives used for HWWS (e.g. health or non-health motives).

For all included studies, we extracted the following information: (1) country, (2) world region, (3) type of setting (i.e. urban, rural or both), (4) study design (RCT or cluster-RCT), (5) sample size, (6) cluster type (e.g. village or compounds), (7) inclusion criteria (8) type of intervention (e.g. HWWS promotion or HWWS promotion combined with other intervention types such as sanitation), (9) handwashing occasion targeted (e.g. after toilet use or before eating), (10) methods for measuring HWWS (11) length of follow-up (12), intervention population (e.g. adult or children and age), (13) whether the cluster design was accounted for in the analysis and (14) study duration.

On a separate form, inspired by a Cochrane systematic review [12], we extracted (1) the intervention's promotional activities/components, (2) intervention classification (e.g. health or non-health), (3) interventions motive(s) (e.g. health, disgust or social norms), (4) key handwashing occasion(s) (e.g. HWWS before cooking and/or after cleaning a child's bottom), (5) handwashing material provision (e.g. soap and/or handwashing station (HWS) supplied) and (6) intervention intensity. Interventions implemented daily over a minimum of two months were classified as high intensity. Interventions implemented at least once a week over a minimum of two months were classified as medium intensity. The remaining interventions were classified as low intensity.

3.1. Obtaining and confirming data from authors

We contacted authors whose interventions' descriptions did not allow us to determine which motive(s) the intervention predominantly relied on. For studies for which information was not provided by the authors, the PhD candidate made a first attempt at intervention-motives classification. The PhD candidate's supervisor then reviewed the intervention descriptions and any disagreements were discussed and consensus recorded. When feasible, we also contacted authors to have access to their trial datasets to be able to compute the studies' point estimates and confidence intervals.

4. Risk of bias assessment

The PhD candidate assessed the risk of bias in the included studies using the Cochrane Risk of Bias Assessment Tool (Version.5.1) [80]. We evaluated the following six domains: randomisation sequence generation, allocation concealment, blinding, incomplete outcome data, selective reporting, and other biases. As per the Cochrane assessment tool, our judgment was classified as 'high', 'unclear' (e.g. when the authors did not provide enough information or information was not clear) or 'low' risk of bias. Appendix 3.3 presents the Cochrane Risk of Bias Assessment Tool and criteria to judge risk of bias.

Double blinding of trial participants and outcome assessors is not possible in handwashing trials, due to the fact the intervention has visible components and no clear placebo [12]. We however noted when steps were taken to attempt to blind participants and/or outcome assessors to the purpose of the study. For incomplete outcome data, we assessed whether the authors had reported sufficient information on attrition and exclusion, if they provided reasons for attrition and exclusion, how this was dealt with in the analysis, and whether intention-to-treat (ITT) analysis was used, when relevant. We also looked at whether loss to follow-up was relatively balanced between intervention groups. Compared to individual RCTs, cluster-RCTs are more prone to chance imbalances between intervention groups due to the generally small number of clusters randomized [81]. We therefore assessed whether there were any major imbalances at baseline between intervention groups. We examined how these were taken into account in the analysis.

5. Assessment of quality of evidence

In order to judge the quality of the included studies and to interpret our findings, we used the GRADE approach [82, 83]. We created two tables of summaries of pooled estimates (i.e. one table for faecal-contact related occasions and one for food-handling related occasions). The results were also summarised according to the type of intervention motive. The tables were modelled on GRADEpro 2014 as used in another Cochrane systematic review [12, 84]. Studies that were not included in the meta-analyses were also separately summarised in tables, but point estimates were not reported.

The quality of evidence was then downgraded (when relevant) for each of GRADE's five domains: risk of bias, inconsistency, indirectness, imprecision, publication bias [82-84]. For each outcome, the level of evidence was downgraded by one level (indicating serious limitations), two levels (i.e. very serious limitations) or not downgraded in the absence of any reason to downgrade [83]. An overall grade was upgraded by one level, if the magnitude of effect was large (i.e. RR>2 [82, 85]), the number and size of the studies were not small (i.e. more than two studies, or with a minimum of 1000 households (HH)), and the ratio of the lower and upper bounds of the confidence intervals did not exceed four.

6. Summary measures

All studies included were compared qualitatively. We grouped studies according to intervention motive. As the primary outcome was dichotomous, risk ratios (RRs) were calculated. We chose RRs as these effect measures tend to be more consistent across studies compared to absolute effect measures [74, 86, 87]. For the meta-analyses, the data extracted consisted of count data and percentages per intervention group, RRs, 95% confidence intervals (CIs), standard errors

(SEs), and p-values. For the studies which did not report risk ratios, we used the reported data to compute the point estimates, SE and 95% CIs.

7. Synthesis of results

Data were analysed using STATA® 15. As some trials had more than one intervention arm, we combined the interventions arms, when handwashing promotion was part of the broader intervention received. We excluded outcomes for which results were not reported quantitatively.

When both observed and self-reported data were reported, we used the former to compute the point estimate of the intervention effect. For trials that did not account for clustering in their analysis, we made an approximate adjustment for cluster randomisation by assuming a design effect (*deff*) of 1.5 (which is within the range of *deff* reported or used in similar studies (e.g. [45, 65, 88-91]). We then multiplied the computed SEs by 1.22 (i.e. the square root of 1.5). We did so to ensure that the confidence intervals would not be overly narrow, and thus the precision of interventions' effects exaggerated [12, 92].

Appendix 3.4 details the methods used to compute the relevant estimates, when RRs were not reported.

We did not expect homogeneous effects across studies. This is because human behaviour is complex, and behaviour change tends to be dependent on psychological factors, social and environmental cues, among other elements [93]. It is thus difficult to predict how individuals will act in particular situations [93]. Additionally, most studies were conducted in different settings and used different interventions.

Summary RRs and 95% CIs were computed using random effects meta-analysis (DerSimonian and Laird method), given the expected heterogeneity among studies. To assess the degree of heterogeneity, we visually inspected forest plots, and assessed the I-squared statistic (I^2) [12]. We set a value of 50% to indicate a moderate level of heterogeneity, as per another Cochrane SR [12]. This method was preferred to the Chi-squared (χ^2) test, as this latter tends to have low power when the number of included studies is small, and excessive power when the number of included studies is large [74, 94, 95]. On the other hand, the I^2 statistic assigns a quantitative value to the variation beyond that which would be expected by chance [74, 94, 95]. The number of included trials per handwashing occasion was however quite small. Inferences about heterogeneity should thus be made with caution, as the I^2 statistic tends to have considerable uncertainty in such instances [74, 96].

8. Risk of publication bias

Given the small number of included trials per handwashing occasion, we could not assess publication bias.

9. Additional analysis

We had planned to investigate heterogeneity by conducting subgroup analyses looking at factors that could plausibly cause variability between studies (i.e. method of HWWS measurement, handwashing equipment provision, follow-up length, intervention intensity). However, due to the limited number of studies per handwashing occasion, and little variation in some of these factors, we only conducted subgroup analyses relating to outcome measurement method.

10. Results

10.1. Search results

The databases search yielded a total of 2,723 citations. After removing duplicates, titles and abstracts of 1,373 papers were screened. Of these, 1,338 papers were excluded as their titles and abstracts indicated that the studies did not meet the inclusion criteria. We examined the full-text of the remaining 35 papers resulting in the exclusion of a further 17 papers. Among these, two papers [97, 98] were excluded as they were conference presentations. Upon contacting the authors to ask for the full-papers, for one study (i.e. [98]), Pavani Ram kindly responded that the paper was not finalised yet. For the second paper, Stephen Luby kindly clarified that the final paper was Ram et al. (2017)'s paper (i.e. [45]). The paper was already part of the identified studies in the literature search.

The remaining 18 papers met the inclusion criteria and were included in the systematic review. One paper (Christensen et al. (2015) [99]) reported two distinct studies. This took the number of trials included to 19. As one study [66] had both a health-motive and a non-health-motive intervention trial arm, the study is reported once in each motive group. Two papers [42, 44] were follow-ups of a trial conducted in 2003 [100]. Among the 19 studies included, one was a report/non-peer reviewed paper [78]. We included one additional paper which had been identified previously during a non-systematic literature search, but which did not appear in any of the database searches. We are unsure why this was the case. This took the total number of included papers and trials to 19 and 20 respectively. We excluded three studies [42, 46, 101] from the meta-analyses, as the authors did not directly measure handwashing practices. Figure 3.1. shows the search results and screening process' flow diagram, according to PRISMA guidelines [74]. The characteristics of the excluded studies and reasons for exclusion are presented in Appendix 3.5.

10.2. Characteristics of included studies

Tables 3.1 to 3.3 present summaries of the characteristics of the included studies, by interventions' motives. Appendix 3.6 presents the detailed information of the included studies. Thirteen studies did not report whether their trials had been registered or not [42, 44, 46, 53, 65, 66, 78, 88-90, 101-103]. We had planned to compare interventions based on health-motives with those using non-health-motives. However, some of the included studies involved interventions which seemed to have been designed using both health and non-health motives. We therefore distinguished a third group of studies using a mixed-motives intervention.

10.2.1. Study design and length

Nineteen of the 20 included trials were cluster-RCTs, and one study was an individually randomised RCT [45]. For the cluster-RCTs, the randomisation units were described as villages [53, 65, 88, 99], communities or pairs of communities [101, 103], communes [78], wards [91], areas [90], neighbourhoods [42, 44], slums [102], compounds [46, 66, 89], households [104] and clusters of pregnant women [105]. Greenland et al.'s (2016) study clusters were health centres

[106]. We however included the study as the intervention targeted mothers rather than health practitioners. The intervention was also promoted via community events and radio messages, besides clinic-based activities [106].

Figure 3.1.PRISMA flow diagram

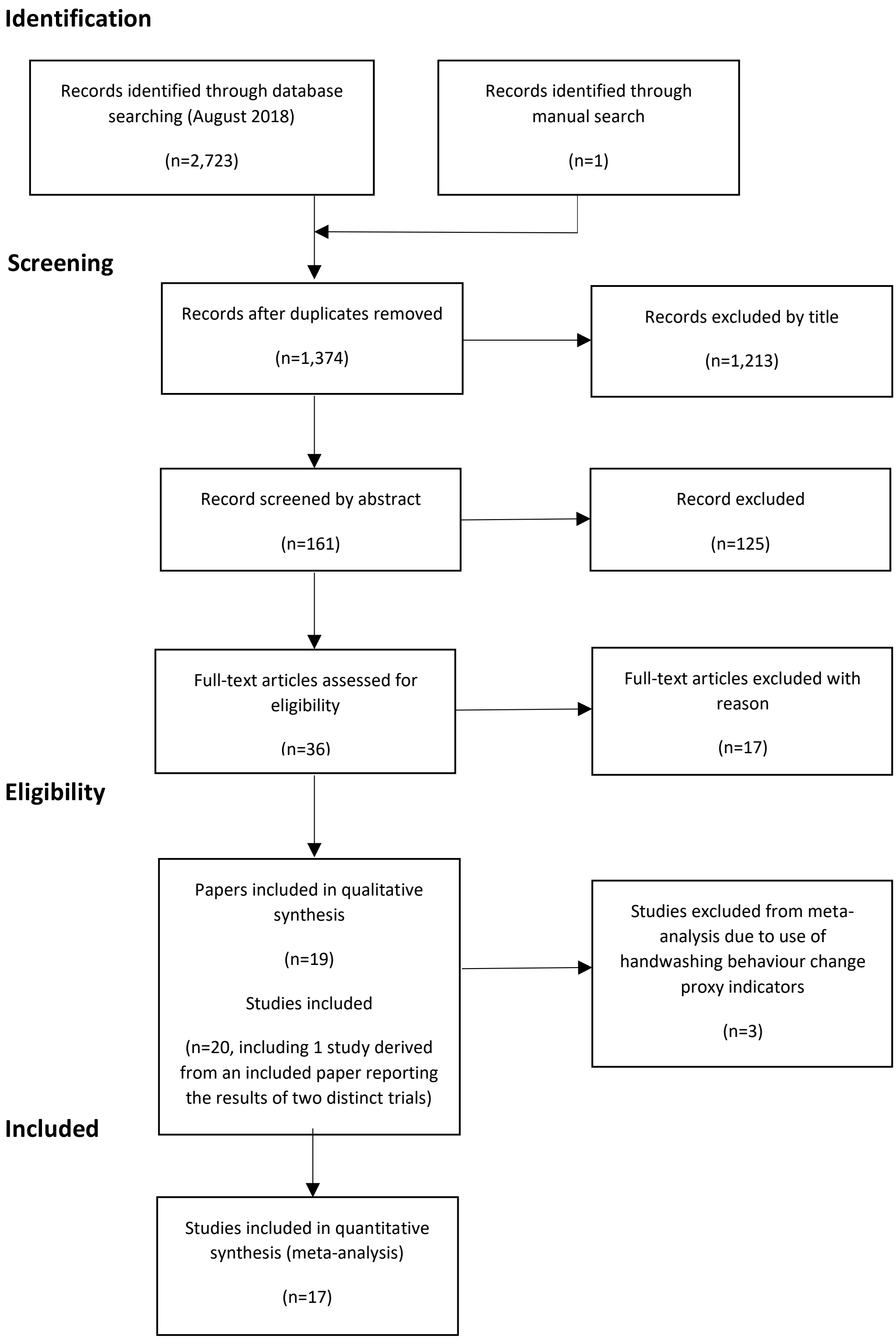


Table 3.1. Summary of characteristics of included handwashing interventions trials using health-motives

Source (study design)	Setting	Sample size	No of participants	Age range	Intervention	Comparison	Outcome (measuring methods)	Follow-up	Cluster design accounted for in the analysis
Biran 2009 [53] (cluster-RCT)	India (rural)	10 villages	288 households (HH)	Mothers and children aged 8 to 13 years old	Handwashing (HW)-only School and community-based events (e.g. discussions Glo Germ© demonstrations, games)	No intervention	HWWS after faecal contact and before eating/feeding (observation)	Within 6 weeks of intervention delivery	Yes
Biswas 2012 [46] (cluster-RCT)	Bangladesh (urban)	90 compounds	400 HH	Compounds residents (unspecified)	HW+[4] Compounds-based (e.g. flipcharts, cue cards and handwashing station (HWS) supply)	No intervention (control group) Vaccine-only intervention group	HWWS after defecation, cleaning a child's bottom and before eating (HW proxy indicators-only)	13 months	No
Bowen 2013 [44] (cluster-RCT)	Pakistan (urban)	28 neighbourhoods	461 HH	Women and 30 months<persons<96 months	HW-only and HW+ HH-based (e.g. videos, soap bar supply, pamphlets)	Supply of children's books and school stationary	HWWS after toilet use, cleaning a child's bottom and before cooking, eating and feeding (self-report)	5 years	Yes
Briceño 2017 [91] (cluster-RCT)	Tanzania (rural)	137 wards	724 HH	Child caregiver (unspecified)	HW-only and HW+ Social marketing (e.g. mass radio campaigns, HH visits, entertaining performances, provision of technical assistance to build HWS	No intervention	HWWS after faecal contact, and before handling food (observation)	>12 months	Yes

[4] *HW+* denotes handwashing interventions combined with other intervention types such as water treatment and sanitation.

Chase 2012 [78] (cluster-RCT)	Vietnam (rural and urban)	210 communes	600 HH	Mothers of children under five years old and other caregivers of young children, such as grandparents	HW-only Mass media campaign and interpersonal communication (e.g. TV ad, posters, intervention branded goods, HH-visits, market meetings)	Mass media campaign-only handwashing intervention	HWWS after faecal contact and before cooking and feeding a child (observation and self-report)	12 months	Yes
Friedrich 2018 [90] (cluster-RCT)	Zimbabwe (urban)	20 areas	600 HH	Child's primary caregiver (unspecified)	HW-only Caregivers targeted directly or indirectly (via children) or a combination of both HH, community and school-based events (e.g. small drama performances, HW self-monitoring calendars, health education)	No intervention	HWWS after faecal contact and before food handling occasions combined (observation)	3 months	Yes
Guiteras 2016 [66] (cluster-RCT)	Bangladesh (urban)	434 compounds	Unclear	Unspecified	HW+ Compounds-level events (e.g. flipcharts, health education, plastic bottle supplied with small detergent pack)	Water chlorine treatment intervention	HWWS after toilet use (observation)	2 months	Yes

Luby 2009 [42] (cluster-RCT)	Pakistan (urban)	28 neighbourhoods	577 HH	Women and 30 months<persons<96 months	HW-only and HW+ HH-based (e.g. videos, soap bar supply, pamphlets)	Supply of children's books and school stationaries	HWWS after toilet use, cleaning a child's bottom and before cooking, eating and feeding (HW proxy indicators-only)	18 months	Yes
Luby 2010 [89] (cluster-RCT)	Bangladesh (urban)	30 compounds	692 residents	Mothers (unspecified)	HW-only (soap arm and hand sanitiser arm) Compound-level intervention (e.g. stages of theory of change, discussions, posters, bar soap or hand sanitiser supply)	No intervention	HWWS after toilet use, cleaning a child's bottom and before cooking, eating and feeding (observation)	1 month	Yes
Nicholson 2014 [101] (cluster-RCT)	India (urban)	70 communities	2,155 HH	Children typically aged 5 with some aged up to 7	HW-only Social marketing campaign. Classroom and HH-based events (e.g. health education, soap supply, wall danglers, songs)	No intervention	HWWS after defecation, before handling food and during bathing (HW proxy indicators-only)	Over 6 months (not clearly specified)	Yes

Parvez 2018 [105] (cluster-RCT)	Bangladesh (urban)	Unclear	269 HH	Pregnant women in their second and third trimester (unspecified)	HW-only and HW+ HH-based events (e.g. discussions, songs, HWS supply with bottles and detergent sachets)	No intervention	HWWS after toilet use, cleaning a child's bottom and before cooking, eating and feeding (observation)	15 months	Yes
Ram 2017 [45] (RCT)	Bangladesh (rural)	229 HH	288 children	Primiparous women (unspecified)	HW-only HH-based events (e.g. Discussions, cue cards, supply of HWS with soap) - Maternal and neonatal health counselling	Maternal and neonatal health counselling	HWWS after faecal contact, before breast feeding and before food preparation (latter occasion excluded as not disaggregated from non-relevant occasions such as after respiratory secretion contact, and before umbilical cord care) (observation)	6 weeks	Not applicable, individually randomised
Stanton 1987 [103] (cluster-RCT)	Bangladesh (urban)	51 communities	1,923 HH	Families (unspecified)	HW+ HH and community-based events (e.g. discussions, demonstrations, games, posters) - Basic primary health care services	Basic primary health care services	HWWS before cooking (observation)	2 weeks	Unspecified Cluster sampling design taken into account when computing incidence rates of diarrhoea. Thus assumed for HW data also

Table 3.2. Summary of characteristics of included handwashing interventions trials using non-health-motives

Source	Setting	Sample size	No of participants	Age range	Intervention	Comparison	Outcome	Follow-up	Cluster design accounted for
Biran 2014 [65] (cluster-RCT)	India (rural)	14 villages	348 HH	Adults and children (unspecified)	HW-only School and community-based events (e.g. animated film, posters, intervention branded goods)	HWWS intervention at last follow-up	HWWS after faecal contact and before handling food (aggregated and disaggregated) (observation)	6 weeks, 6 months and 12 months	Yes
Burns 2018 [104] (cluster-RCT)	South Africa (urban)	229 HH	229 HH	Children aged 3 to 9 years old	HW-only - HH-based-events (e.g. supply of colourful soap with toy embedded inside) - Single standard hygiene and health lesson	- Supply of bar soap more colourful than usual soap, with a toy supplied alongside the soap bar. - Single standard hygiene and health lesson	HWWS before eating (observation and self-report)	6 weeks	Yes
Gautam 2017 [88] (cluster-RCT)	Nepal (rural)	8 villages	239 HH	Mothers (unspecified)	HW+ HH and community-based events (e.g. family drama, games, Glo Germ© demonstrations, intervention branded goods)	No intervention	HWWS before feeding a child and washing child's hands before child's eats	Approximately 4 months (45 days after completion of the 3-month period of intervention)	Yes

Greenland (2016) [106] (cluster-RCT)	Zambia (peri-urban and rural)	16 health centre-catchment areas	373 HH	Mothers of infants less than 6 months	HW+ Clinic and community-based events. Mass media campaign and face to face interaction (e.g. radio ads, demonstrations, discussions, video ads, intervention branded goods)	Standard care at clinics	HWWS after faecal contact and before handling food combined, and HWWS after faecal contact-only	7 months	Yes
Guiteras 2016 [66] (cluster-RCT)	Bangladesh (urban)	434 compounds	Unclear	Unspecified	HW+ Compounds-level events (e.g. flipcharts, Glo Germ© demonstration, plastic bottle supplied with small detergent pack)	Water chlorine treatment intervention	HWWS after toilet use (observation)	2 months	Yes

Table 3.3. Summary of characteristics of included handwashing interventions trials using both health and non-health motives

Source	Setting	Sample size	No of participants	Age range	Intervention	Comparison	Outcome (measuring methods)	Follow-up	Cluster design accounted for
Christensen 2015 [99]-Kakamega (cluster-RCT)	Kenya (rural)	29 villages	278 HH	Caregivers of children aged 4-16-month old (unspecified)	HW+ HH-based events (e.g. games, songs, visual aids, HWS supply with powder soap packs)	Child growth monitoring	HWWS after faecal contact and before handling food combined (self-report)	4 months	Yes
Christensen 2015 [99]-Bungoma (cluster-RCT)	Kenya (rural)	17 villages	63 HH	Pregnant women in their second or third trimester, and caregivers of 3-month-old children (unspecified)	HW-only HH-based events (e.g. games, songs, visual aids, HWS supply with powder soap packs)	Child growth monitoring	HWWS after faecal contact and before handling food combined (self-report)	4 months	Yes
Langford (2013) [99] (cluster-RCT)	Nepal (urban)	8 slums	88 HH (mother-infant pairs)	Mothers (unspecified)	HW-only Community and HH-based events (e.g. meetings, posters, bar soap supply)	No intervention	HWWS after toilet use, cleaning a child's bottom and before cooking, eating and feeding (self-report)	< 6 months	No

The majority of studies had interventions which were of low intensity [45, 53, 65, 66, 88-90, 99, 104]. In Luby et al.'s (2010) trial, the intervention's length was initially seven months [89]. However, due to a deviation from protocol during intervention implementation, the intervention was implemented again 10 months after the initial delivery, and for a month [89]. The authors' findings are based on the results of the second intervention implementation [89]. Four studies had interventions of medium intensity [42, 44, 46, 105]. The remaining three trials had interventions of high intensity (daily visits over a minimum of two months [91, 101, 106].

10.2.2. Settings and participants

The 20 included trials were conducted in Asia (N=14) and Sub-Saharan Africa (N=6) (Table 3.1.). In the former region, six studies were conducted in Bangladesh [45, 46, 66, 89, 103, 105], three in India [53, 65, 101], two in Pakistan [42, 44], two in Nepal [88, 102], and one in Vietnam [78]. In Sub-Saharan Africa, two studies were conducted in Kenya ([99]), and one study in each of Tanzania [91], South Africa [104], Zimbabwe [90] and Zambia [106] each. All studies were published in English.

Altogether, the included studies comprised a minimum of 9,868 households (HH), not counting two studies which did not (clearly) specify the number of households [66, 89] (Table 3.1.). Luby et al. (2010) reported a total of 692 compound residents in the trial, from 30 included compounds [89]. Guiteras et al. [66] reported the number of compounds (i.e. 424 compounds), but did not clearly report the number of households sampled. Most included trials were of small to medium size (i.e. less than 500 HH for the former, and between 500 to less than 1000 HH for the latter). Nicholson et al. [101] and Stanton et al.'s [103] trials were the only large trials, with 2,155 and 1,923 HHs respectively. These were all in the health motive group. The number of clusters ranged between 8 [88, 102] and 434 [66].

Study participants included adults and/or children, with children's ages not always specified (Table 3.1.). Four studies included mothers-only [42, 88, 89, 106]. Greenland et al. and Gautam et al. included mothers caring for infants less than 6 months and between three to 59 months respectively [88, 106]. Two studies were comprised of children-only [101, 104]. One of these included children aged between three and nine years old [104], while the other included children aged from five up to seven years old [101]. Three studies included both mothers and their children [53, 102] or women and children [44]. Biran et al. and Bowen et al. restricted the children's age range between eight and 13 years old, and between 30 and 95 months respectively [44, 53]. Biran et al.'s study involved adults and children but with no ages specified [65]. Stanton et al. included families, but did not provide any other specification [103].

Three studies were composed of child caregivers-only (primary or not) [90, 91, 99]. Christensen et al. (Kakamega study) restricted the study to the caregivers of children aged between four and 16 months old [99]. Chase et al. included both mothers of children under 5 years old and other caregivers of young children (including grandparents) [78]. Three studies [45, 99, 105] involved pregnant women. Christensen et al. (Bungoma study) and Ram et al. restricted their participants to pregnant women in their second or third trimester and caregivers of 3 months old children [99], and primiparous women [45]. Guiteras et al. [66] and Biswas et al. [46] did not specify who their intervention targeted. As the intervention was implemented at compound level [66] and household level [46], it is likely that they targeted compounds residents and household residents in general respectively.

10.2.3. Intervention

Appendix 3.7 gives the details of the interventions in each included trial, organised by intervention motive.

- **Types of handwashing interventions**

We identified three main handwashing intervention types (Table 3.1). Seven trials had standalone handwashing interventions [45, 53, 65, 99, 101, 102, 104]. Four trials had handwashing interventions which were combined with other interventions, such as sanitation, water treatment, food hygiene and diarrhoea control [66, 88, 103, 106]. Six trials had multiple handwashing intervention arms, with standalone handwashing intervention arms, and arms with handwashing interventions combined with other interventions [42, 44, 89, 91, 99, 105]. Friedrich et al. conducted a 3-arm trial in which the handwashing intervention either directly targeted caregivers, indirectly targeted them, or a combination of both [90]. Similarly, Chase et al. conducted a trial where handwashing promotion was either delivered via mass media and interpersonal communication (one arm), or via mass-media alone (second arm/control arm) [78]. Despite a handwashing intervention having been implemented in the control group, this study evaluated the effect of an interpersonal communication campaign against a background of mass media.

Nine studies delivered some form of intervention in the control group. Burns et al. (2018) provided the control group with soap, which was similarly packaged and with a toy, as in the intervention group [104]. However, the toy was not embedded in the soap bar, as in the intervention group [104]. Both arms also received a single hygiene and health lesson [104]. In the initial intervention implemented in Luby et al. (2006)'s trial for which Luby et al. (2009) [42] and Bowen et al. (2013) [44] conducted follow-up studies, the control group received a regular supply of children's books and school stationary (e.g. pencils, notebooks) [100]. This was to help with the child's education [100]. In Stanton et al. 's trial, each study arm received basic primary healthcare services from a community health worker [103].

In Greenland et al.'s study, the control group received standard clinic care [106]. Ram et al. provided neonatal and maternal health counselling in both the intervention and control arms [45]. In Guiteras et al.'s trial, all arms received a water treatment intervention [66]. As mentioned previously, in Chase et al.'s trial, the control group received one of the two intervention components delivered in the intervention group [78]. Although handwashing promotion was implemented in the control group, we still chose to include Chase et al.'s trial, because we could isolate the effect of the additional component in the intervention group.

All except four studies (i.e. [66, 88, 103, 104]) had messages which targeted both HWWS after faecal-contact and before food-related occasions. Guiteras et al. [66], Stanton et al. [103], Burns et al. [104] and Gautam et al. targeted HWWS after toilet use, before cooking, before eating, and before feeding a child and eating respectively.

Whilst the intervention messages and contexts varied between studies, many used similar intervention components. Generally, the intervention components entailed discussions, demonstrations, drama performances, songs, games, reminders (such as posters, cues cards and calendars), and intervention branded products (such as badges and stickers). Five studies used media components as part of their interventions (videos [42, 44, 78, 101, 106], radio ads [91, 101, 106]). Eleven studies provided soap as part of their interventions [42, 44, 45, 66, 89, 99, 101, 102, 104, 105]. In Luby et al.'s study, one study group received soap and another group waterless hand sanitiser [89]. Four studies provided both soap and handwashing stations [45, 99, 105]. Gautam et al. provided plastic buckets for handwashing, however, soap does not seem to have been supplied [88]. Briceño et al. provided technical assistance to build handwashing stations (i.e. tippy taps) [91].

- **Studies' intervention motives**

As stated previously, we organised the studies in three groups by interventions' motives (Table 3.1. and Appendix 3.7). There were 13 studies where health was the only or predominant intervention motive [42, 44-46, 53, 66, 78, 89-91, 101, 103, 105]. Nicholson et al. did not describe the intervention components involving social norms and disgust, while describing the health motive components [101]. We thus assumed that the dominant motive was health. Five studies only or predominantly had interventions with non-health motives [65, 66, 88, 104, 106]. Guiteras et al.'s study was classified in both the health and non-health motives intervention groups, as one study group received a health-motive handwashing intervention, and a second group received a disgust and shame-motivated handwashing intervention [66]. The remaining three studies were mixed health and non-health motives interventions [99, 102]. Two studies [65, 66] and six studies [42, 44, 53, 66, 89, 103] included arms using non-health-only and health-only motives respectively.

The interventions were seldom clearly described. The description did not always allow us to confidently classify the intervention into the different motives groups. This was the case for some of the studies where both health and non-health motives seemed to have been used [45, 91, 101, 105]. We contacted the authors for clarification. Only one corresponding author kindly responded (Aiden Coville (Briceño et al.'s trial [91]). The author's classification (health-motive) differed from the one initially made by the PhD candidate (non-health motive). The classification of the remaining three studies were done in consultation with the PhD candidate's supervisor.

The non-health motives used were disgust [65, 66, 88, 99, 101, 106], affiliation [65, 88], status [65], aspiration [91, 99], curiosity [104], social norms [99, 101, 102, 105], comfort [102], and shame [66]. Disgust and social norms were the non-health motives the most commonly used.

In Friedrich et al.'s trial, disgust was meant to be part of the intervention's motives, along with health [90]. However, due to a deviation from protocol during intervention implementation, only the health motive was used [90]. We classified nurture as a health motive given that, in the studies where the authors described it, nurture was defined as protecting and caring for one's infant [106], having a healthy child and mother's desire to care well for her child [45], and mothers safeguarding their baby's health and safety [105].

10.2.4. Outcome assessment

The majority of studies used structured observation to measure the outcome. Four studies, one in the health-motive group (i.e. [44]) and the three studies comprising the mixed-motive group (i.e. [99, 102]) used self-report as a direct measure of HWWS practices. Langford et al. used structured observation at baseline and self-reported data at both baseline and follow-up [102]. In Briceño et al., Burns et al. and Chase et al.'s studies, both self-reported and structured observation data were presented [78, 91, 104]. For the observation data in Chase et al.'s study [78], the authors only presented percentages and reported the results as being non-statistically significant [78]. In the same study, we noticed some inconsistencies between some of the numbers reported in the text compared to those in some of the graphs. In the non-health-motive group, all studies used direct observation to measure HWWS practices.

Upon request, Aiden Coville (in Briceño et al.'s trial [91]) kindly shared the link to their study's datasets. They also guided us to the relevant dataset and relevant variables to be able to compute the study point estimates. However, the way the data was structured did not allow us to compute cluster-adjusted estimates. Similarly, Biran et al.'s primary outcome was analysed as a pre-post handwashing practices comparison within study groups, by disaggregated handwashing occasions [53]. The secondary outcome was a between study groups comparison, by combined handwashing occasions [53]. Due to the validity issues attached to pre-post study designs [107], we chose to estimate the outcome of interest using the between study groups comparison data. As the point estimate's exact p-value was not reported, we contacted the authors. Wolf Peter-Smidt kindly provided us with the requested information.

In Friedrich et al.'s trial, given the information provided (i.e. proportions without count data), we chose to only compute the point estimates for the intervention group which directly received the intervention (see Table 3.1 and Appendix 3.7 for a description of the intervention). The authors only reported a single p-value which was for the comparison of the direct intervention with the indirect intervention (i.e. $P<0.001$ in favour of the direct intervention) [90]. Friedrich et al. (2018) also reported that there was no evidence of a difference between the control group and the indirect group [90]. We thus deduced that there was statistical evidence of a difference between the direct intervention and the control group. We used the single reported p-value to compute the point estimate, according to the procedures reported in Appendix 3.4. As the p-value was reported as an inequality , we assumed the p-value to be P=0.001 [108].

Christensen et al. (2015), Burns et al. (2018), Chase et al. (2012), Briceño et al. (2017) and Guiteras et al. (2016) used P<0.1 as indication of statistical evidence of an intervention effect [66, 78, 91, 99, 104], rather than the conventional P<0.05 threshold. Similarly, Stanton et al. indicated some evidence of intervention effect on HWWS practices, by reporting that P<0.05 [103]. However, when using the provided data to compute the point estimate, we found P=0.049, indicating weak evidence of intervention effect.

Eight studies measured other proxy indicators of handwashing practices (e.g. presence of water and soap at the handwashing location, collecting hand-rinse samples), along with directly measuring handwashing practices [44, 66, 78, 89-91, 99]. Three studies did not report any direct measure of HWWS practices [42, 46, 101]. Luby et al. (2009) used spot checks (i.e. presence of water and soap) of handwashing facilities, soap ownership, soap purchase and 'correct' handwashing techniques, as proxy indicators of handwashing practices [42]. Biswas also used spot checks of handwashing facilities. Nicholson et al. collected and weighed soap wrappers to assess soap consumption [101]. Two studies did not pre-specify their outcome measure [99].

For the trials with multiple handwashing intervention arms [42, 44, 89, 91, 99, 105], we combined the intervention arms to compute a single point estimate per handwashing occasion. The results obtained for the individual arms were comparable, except for Luby et al. (2010)'s trial [89]. In the latter study, the soap intervention appeared to have a larger effect than the hand sanitiser intervention [89]. For HWWS occasions in relation to toilet use, we made a distinction between studies measuring HWWS practices after toilet use-alone (i.e. HWWS toilet occasions excluding after cleaning a child's bottom) and HWWS after faecal contact (i.e. including HWWS after cleaning a child's bottom, in addition to after toilet use). In instances where handwashing occasions other than those linked to faecal contact or food handling (e.g. after sneezing, after handling trash) were measured along with the outcomes of interest, these were not taken into account when assessing the intervention effect.

All studies reported accounting for clustering in the analysis of the outcome measure except Langford et al. [102] and Biswas [46]. However, in general authors did not clearly report their statistical analysis for quantitative outcomes. Stanton et al. did not report the methods used to analyse their structured observation data [103]. We assumed that clustering was taken into account for the handwashing behaviour-change outcome, as the authors specified taking the cluster sampling design into account when computing incidence rates of diarrhoea for intervention and control groups [103].

Studies' follow-up times ranged from less than three months [45, 53, 66, 89, 103, 104], to between three months to six months [88, 90, 99, 102], to between six months to 12 months ([65, 78, 101, 106]), or greater than 12 months [42, 44, 91, 105]. Luby et al. (2009) [42] and Bowen et al.'s (2013) studies [44] involved 18 months and 5 years of follow-up respectively, of Luby et al. (2006)'s original trial conducted in 2003 [100]. Biran et al. (2014) had six-week, six-month and 12-month follow-up points [65]. However, the results were only partially reported at the six-month and 12-month follow-up points (with some data not shown but implied) [65]. Therefore, we chose to compute the point estimates for the six-week follow-up point only.

10.2.5. Risk of bias within studies

Figure 3.2 presents the reviewer's risk of bias judgements across all included trials, presented as percentages. Figure 3.3 presents the reviewer's risk of bias judgements for each included trial. Appendix 3.8 presents the reviewer's detailed risk of bias judgements for each included study. Overall, the studies were at high risk of bias and with evidence of heterogeneity, as expected. For all trials, data were collected at the same time period in the control and intervention groups. Five trials did not provide information on the methods used to generate random allocation sequences [46, 53, 78, 88, 102]. We judged all cluster-RCTs to be at low risk of selection bias. Lack of allocation concealment should not be an issue given that, in cluster-RCTs, clusters are usually randomised at one time point [80]. This was the case for all the included cluster-RCTs. We only judged Ram et al.'s individual-RCT [45] to be at high risk of allocation bias.

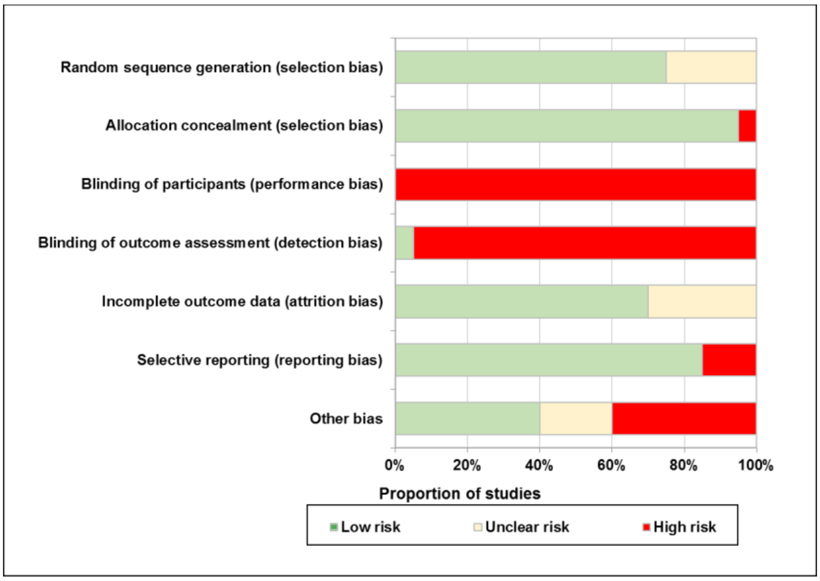


Figure 3.2. Risk of bias graph: reviewer's risk of bias judgements about each risk of bias item across all included studies (presented as percentages)

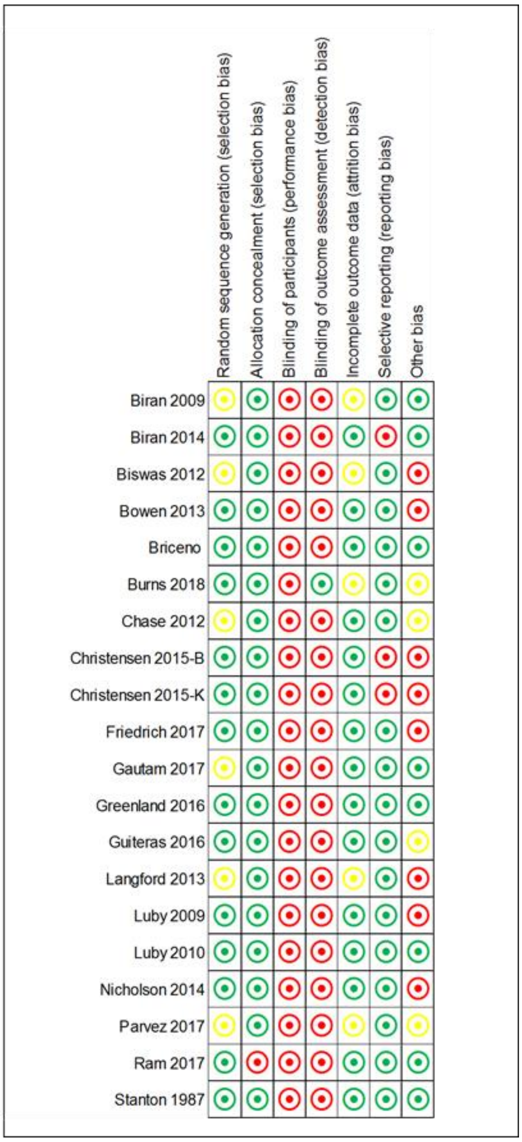


Figure 3.3. Reviewer's risk of bias judgements about each risk of bias item for each included study

All studies were classified as having high risk of performance and/or detection bias, due to the impossibility of totally masking handwashing interventions, as mentioned previously. There was also a general lack of effort to do so. In Friedrich et al.'s trial, due to Zimbabwe's medical research council procedures, it was mandatory to inform participants of the study's aim [90]. However, in six studies, efforts were made to mask participants and/or outcome assessors [53, 65, 66, 91, 101, 104]. Burns et al.'s trial [104] is the only one which we judged to be at low risk of performance bias, due to the use of a similar soap type in the control group compared to the one supplied to the intervention group. In Briceño et al.'s study [91] some of the information on performance bias was only reported in a World Bank report [109] and not in the published trial paper. Gautam et al. reported that outcome assessors were masked to the study objectives [88]. However, no information was provided regarding how this was achieved. The study was thus also judged to be at high risk of detection bias. None of the trials reported having masked their data collection tools.

The majority of studies used distinct teams to implement the intervention and assess the outcome measure. However, this was not the case for Luby et al. (2009, 2010), Bowen et al., and Langford et al.'s studies [42, 44, 89, 102]. In Stanton et al. (1987)'s study, it was unclear whether the outcome assessors and intervention implementers were distinct teams or not [103]. In Christensen et al. (2015)'s trial, both teams were distinct, however, outcome assessors and intervention implementers assisted each other on some of their respective activities [99]. Biswas [46], Parvez et al. [105], Langford et al. [102], and Burns et al.'s [104] trials were at unclear risk of attrition bias as little information was provided in that regard.

Three studies were judged as having a high risk of reporting bias [65, 99]. Biran et al. (2014) selectively reported the results at the six-month and 12-month follow-up points, but not at the six-week follow-up point as mentioned previously [65]. In Christensen et al.'s trials, the primary outcomes were not pre-specified [99]. Additionally, whilst several indicators were measured to assess handwashing practices (e.g. observation of visible dirt on mother's hands, dedicated handwashing location), as per the reported table, in the text, the authors only stated the results of one outcome to confirm intervention effectiveness (i.e. availability of soap for handwashing) [99].

Regarding other risks of bias, two studies were judged as having a high risk of confounding bias, due to imbalances at baseline in HWWS frequency [90] or covariates [91]. Friedrich et al. do not seem to have accounted for the reported baseline imbalances in the analysis. In Briceño et al.'s study, due to issues with the reliability of baseline data collected by the survey firm, baseline data collection was aborted [91]. The authors collected retrospective baseline data at the end of the trial, but could not retrospectively collect data on handwashing practices. The HWWS intervention group was more likely to have households with a connection to piped water and cement floor compared to the control group [91]. However, the authors took the baseline imbalances into account in their analyses [91].

Similarly, Luby et al. (2009) reported that a greater number of re-enrolled households had been initially randomised to the HWWS intervention and were more likely to own a refrigerator and television [42]. However, upon examining the in-table numbers, we judged that the difference was not important, and we did not judge the study at a high risk of confounding bias. In Burns et al. (2018)'s study, there were some baseline imbalances pertaining to the proportion of children who could not open the water tap [104]. However, the authors accounted for the baseline differences in their analysis [104].

Another source of bias identified was a high risk of social desirability bias or bias in favour of the intervention in six studies (i.e. [42, 44, 99, 101, 102]). Bowen et al. (2013), Christensen et al. (2015), Langford et al. (2013) and Luby et al. (2009) used self-report to measure HWWS practices [42, 44, 99, 102]. As stated previously, self-reports are prone to over-reporting of handwashing practices compared to observed data [32, 33, 110]. In Nicholson et al.'s trial, participants' soap was replenished upon presentation of empty soap wrappers. We judged these measurement methods to be at high risk of bias as participants might present empty soap wrappers, even in the absence of soap use for handwashing. Similarly, Biswas used the presence of water and soap at the handwashing location as a proxy indicator for HWWS practices. However, the presence of handwashing supplies at the handwashing location may not guarantee actual change in HWWS practices [46, 78], particularly in the absence of blinding. Luby et al. (2009) used several proxy indicators to measure handwashing practices (e.g. presence of water and soap at the handwashing location, handwashing technique, soap purchase, soap consumption) [42]. As intervention implementers and outcome assessors were the same team, there was high risk of social desirability bias, particularly for the outcomes that were self-reported.

Eleven studies did not report the methods used to calculate the HWWS behaviour-change outcome's sample size [42, 44, 46, 66, 78, 99, 101, 102, 104, 105]. Langford et al. did not calculate a sample size for their study [102]. However, they reported using an exhaustive sample [102]. This was quite small (88 HH of mother-infant pairs) [102]. In Christensen et al.'s trials, the sample sizes were also small (34 and 38 households) [99]. Chase et al.'s sample size calculation was based on detecting a decrease in diarrhoea [78]. The initial study which Luby et al. (2009) and Bowen et al.'s follow-ups are based on was also designed to detect an impact on diarrhoea [100]. Similarly, Nicholson et al.'s sample size calculation was based on detecting a between-group difference in the incidence of diarrhoea in children under five years of age [101]. Some of the studies may thus have been under-powered to detect intervention effects.

10.2.6. Effect of interventions

Appendix 3.9 presents the individual studies' findings, as reported, by handwashing occasions and by intervention-motives group and by handwashing occasion. We did not include the findings of the three studies where HWWS practices were not directly observed or self-reported. For trials where HWWS practices were directly measured, we summarised the data in forest plots (Analyses 3.1 to 3.3). Appendix 3.10 presents a summary of findings for the effect of handwashing interventions on HWWS practices, by handwashing occasions in general and by interventions-motives group, along with the GRADE quality of evidence. The findings from the three studies [42, 46, 101] that only used proxy indicators to measure HWWS practices are also summarised in separate tables.

Regarding the forest plots, some of the estimates presented contain multiple observations in the overall results. This is because some studies reported intervention effect on more than one handwashing occasion or/and had more than one handwashing intervention arm, against a single control. Whilst such results are treated as independent observations in the meta-analysis, they are not.

- Findings from studies only using proxy indicators for HWWS practices

Nicholson et al. [101] targeted HWWS practices after faecal contact and before food-handling occasions. Luby et al. (2009) [42] aimed to increase HWWS after defecation, after cleaning a child's bottom, before food preparation, before eating and before feeding a child. Biswas targeted HWWS after toilet use and before cooking [46]. Nicholson et al. [101] estimated at 45 g the median soap consumption per household per week in the control group, compared to 235 g in the intervention group (one trial in Asia, 2,155 HH, very low-quality) (Appendix 3.10, Section 3). The difference between groups was not subjected to a statistical test. The authors concluded that the observed intervention effect on the health outcomes may have been mediated by soap use [101]. This suggests that handwashing practices may have increased.

By contrast, Luby et al. (2009) [42] found that 18 months post-intervention delivery, there was no evidence of a difference between the handwashing intervention groups and the control group (Appendix 3.10, Section 3). This was the case for soap consumption (lowest P=0.16, in the handwashing-only intervention group), quantity of soap purchased (lowest P=0.40, in the handwashing-only intervention group) and soap ownership (lowest P=0.21, in the handwashing-only intervention group) [42]. There was however evidence of an intervention effect on the presence of water and soap at the handwashing location (P=0.001, in the handwashing-only intervention group), and proper handwashing technique (P<0.01, for hand rubbing and hand lathering techniques in both handwashing intervention groups) (577 HH, one trial in Asia, very low-quality) [42] (Appendix 3.9). The authors concluded that the initial improvement in handwashing practices had failed to be sustained [42].

Biswas [46] found that the presence of water and soap or soapy water at the handwashing location was around 30% higher in the intervention group including handwashing promotion (P<0.01) (Appendix 3.10, Section 3) compared to the control group and vaccine-only intervention groups, 13 months post handwashing intervention delivery (400 HH, one trial in Asia, very low-quality [46]).

- Intervention effects on HWWS after faecal contact-related occasions

We identified 10 studies [44, 45, 65, 66, 78, 89, 91, 102, 105, 106] which measured the effect of handwashing interventions on HWWS after faecal contact-related occasions (i.e. after using the toilet, after cleaning a child's bottom and both occasions combined). Only two studies were conducted in Sub-Saharan Africa (Tanzania [91] and Zambia [106]). We found strong evidence of heterogeneity between studies. Nevertheless, there is evidence that the interventions had positive effects on HWWS after both toilet use and cleaning a child's bottom. The small number of non-health and mixed motives studies prevent any reliable comparison of the effects of different motives.

When looking at the results in detail, overall the pooled estimate indicates strong evidence that the intervention effect on HWWS after faecal contact-related occasions was higher in intervention than control groups (RR=1.39, 95%. CI: 1.20-1.60 (*P*<0.001), at least 3,116 HH, very low-quality) (Figure 3.4). When restricting the analysis to observation data only, the HWWS estimate increased (RR=1.80, 95% CI: 1.32-2.46, eight studies, at least 2,567 HH, very low-quality). We found strong evidence of heterogeneity between studies (I^2=80% and I^2=81%, the latter restricted to observation data-only, *P*<0.001 for both).

When looking at intervention effects by intervention motive, all three groups found either strong evidence (health-motive and mixed-motive groups) or some evidence (non-health-motive group) of an effect of the intervention on HWWS practices after faecal contact-related occasions. In the mixed-motives group, the evidence is only based on a single small study. Therefore, in the forest plot, we did not present the overall result for this motive group. Similarly, in the non-health motive group, the evidence is based on three trials, including two which are small in size. The results for these two motives groups should therefore be interpreted with caution. The estimated magnitude of effect in the health-motive group (RR=1.57, 95% CI: 1.26-1.94 (*P*<0.001), seven studies [44, 45, 66, 78, 89, 91, 105], at least 2,307 HH, very low-quality) (Figure 3.4) is greater than in the non-health-motive (RR=1.38, 95% CI: 1.02-1.87 (*P*=0.04), three studies [65, 66, 106], at least 721 HH, very low-quality), and mixed-motive intervention groups (RR=1.14, 95% CI: 1.04-1.24 (*P*=0.005), one study [102], 88 HH, very low-quality). However, there is substantial overlap in the confidence intervals of the health-motive and non-health-motive groups.

When restricting the analysis to observation data only in the health-motive group, the magnitude of effect increased (RR=1.90, 95% CI: 1.29-2.78, six studies, at least 1,846 HH, low-quality) (Figure 3.4). The confidence interval still overlapped widely with that of the non-health-motive group. We found strong evidence of heterogeneity within the health-motives group of studies (I^2=80% and I^2=86% (the latter restricted to observation data), *P*<0.001 for both). In the non-health group, we found no evidence of heterogeneity between studies (I^2=0%, *P*=0.58). This may be due to the small number of studies (i.e. only three).

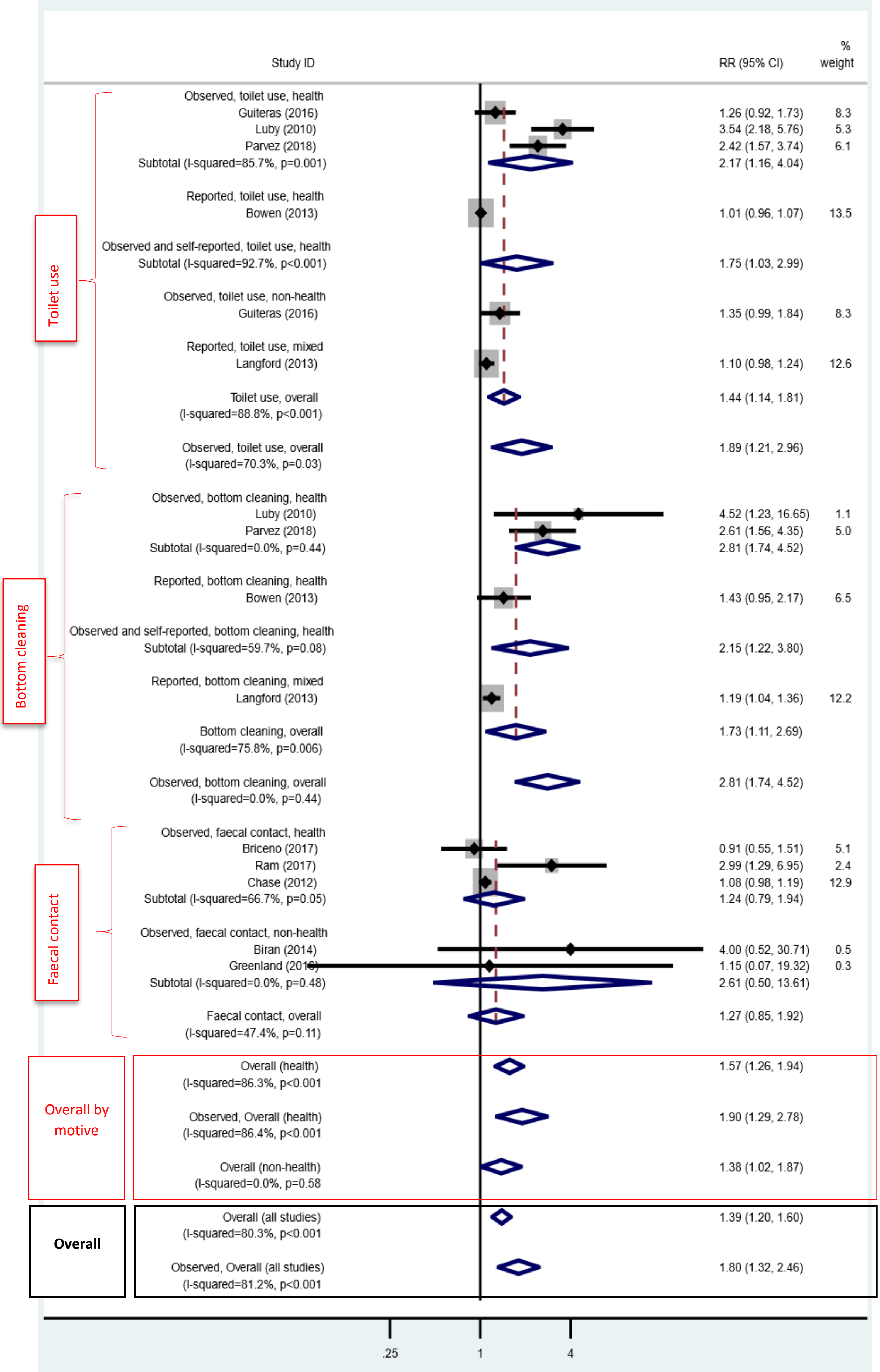


Figure 3.4. Meta-analysis of the effect of behaviour change interventions on HWWS practices after faecal-related contact (by occasion, intervention motive and measurement method).

- After using the toilet

Five studies (i.e. [44, 66, 89, 102, 105]) assessed the effect of the interventions on HWWS after toilet use. All studies were conducted in Asia. We found evidence that the interventions had positive effects on HWWS after using the toilet.

When taking a close look at the results, the pooled estimate shows strong evidence that the effect of the interventions on HWWS after using the toilet in intervention arms was higher than control arms (RR=1.44, 95% CI: 1.14-1.81 (*P*=0.002), at least 818 HH, very low-quality) (Figure 3.4). When restricting the analysis to observation data only, the estimate of the effect of the interventions increased (RR=1.89, 95% CI: 1.21-2.96, three studies, at least 269 HH, very low-quality). The evidence from this latter analysis is based on few studies which are small in size. The results should therefore be interpreted with caution. We found strong evidence of heterogeneity between studies (I^2=89%, *P*<0.001 and I^2=82% , the latter restricted to observation data-only, *P*=0.001).

When analysing effects by intervention motive, there were between one and four studies per intervention-motive group and per faecal-contact related occasion. The results should thus be interpreted with caution. There was some evidence of an effect of the interventions on HWWS after toilet use in the health-motive intervention group (RR=1.75, 95% CI: 1.03-2.99 (*P*=0.04), four studies [44, 66, 89, 105], at least 730 HH, very low-quality) (Figure 3.4). Although there was weak evidence of an effect of the interventions in the non-health-motive group (RR=1.35, 95% CI: 0.99-1.84 (*P*=0.06), one study (i.e. [66]), unclear number of HH, low-quality), the point estimate was consistent with an improvement. This was also the case in the mixed-motive intervention group, albeit with little evidence of an effect of the interventions (RR=1.10, 95% CI: 0.98-1.24 (*P*=0.11), one study (i.e. [102]), 88 HH, very low-quality). In both cases, the upper limit of the confidence intervals does not exclude the possibility of a substantial intervention effect. Additionally, all confidence intervals overlapped widely with each other.

When restricting the analysis to observation data only in the health-motive group, the point estimate increased (RR=2.17, 95% CI: 1.16-4.04, three studies, at least 269 HH, very low-quality) (Figure 3.4). The confidence interval still overlapped widely with that of the non-health-motive group. We found strong evidence of heterogeneity between studies in the health-motive group (I^2=93% and I^2=86%, the latter restricted to observation data, *P*<0.001 respectively).

❖ After cleaning a child's bottom

Four studies [44, 89, 102, 105] assessed the effect of the interventions on HWWS after cleaning a child's bottom. All studies were conducted in Asia. To summarise the key points, there is evidence that the interventions had positive effects on HWWS after cleaning a child's bottom. Only the health-motive and mixed-motives studies reported an effect of the interventions on HWWS after this occasion.

When looking at the results in detail, the pooled estimate shows some evidence that the effect of the interventions on HWWS after cleaning a child's bottom was higher in intervention than control groups (RR=1.73, 95% CI: 1.11-2.69 (P=0.02), at least 818 HH, very low-quality) (Figure 3.4). After restricting the analysis to observation data only, the estimated effect of the interventions increased (RR=2.81, 95% CI: 1.74-4.52, two studies, at least 269 HH, low-quality). The evidence from this latter analysis is based on only two studies which are of small size. The results should therefore be interpreted with caution. We found strong evidence of heterogeneity between studies (I^2=76%, P=0.006). However, when restricting the analysis to observation data only, there was no longer evidence of heterogeneity (I^2=0%, P=0.44), which could be due to the very small number of studies.

The results by motives shows that there was strong evidence of an effect of the interventions on HWWS practices in the health-motive group (RR=2.15, 95% CI: 1.22-3.80 (P=0.008), three studies [44, 89, 105], at least 730 HH, low-quality) (Figure 3.4) and some evidence of an effect in the mixed-motive group (RR=1.19, 95% CI: 1.04-1.36 (P=0.013), one study [102], 88 HH, very low-quality). The confidence intervals overlapped widely. When restricting the analysis to observation data only in the health-motive group, the pool of studies, and thus point estimate, were unchanged.

❖ After toilet use and cleaning a child's bottom combined

We identified five studies [45, 65, 78, 91, 106] which reported estimates of the effect of the interventions on HWWS after toilet use and after cleaning a child's bottom but did not distinguish between the two. Two studies were conducted in Sub-Saharan Africa [91, 106]. Generally, there was strong evidence of heterogeneity between studies. We found no evidence that the interventions had positive effects on HWWS after using the toilet and cleaning a child's bottom aggregated occasions. Only the health-motive and non-health motives studies reported an effect of the interventions on HWWS after this occasion.

The pooled estimate suggests that the estimate of the effect of the interventions on HWWS in trial arms which received the intervention was greater than control arms. There was however no evidence of an effect of the interventions (RR=1.28, 95% CI: 0.85-1.92 (*P*=0.24), at least 2,298 HH, very low-quality) (Figure 3.4). All studies used direct observation to measure HWWS practices. The magnitude of heterogeneity between studies was relatively moderate (I^2=47%, *P*=0.11).

Regarding the effect of the interventions according to motives groups, there was no evidence of an intervention effect in either of the two groups (health-motive group: RR=1.24, 95% CI: 0.79-1.94 (*P*=0.36), three studies [45, 78, 91], at least 1,577 HH, very low-quality (Figure 3.4); and non-health-motive group: RR=2.61, 95% CI: 0.50-13.61 (*P*=0.26), two studies [65, 106], 721 HH, very low-quality). However, the point estimates in both intervention groups were consistent with an improvement. All studies measured the effect using direct observation. The magnitude of heterogeneity between studies in the health-motive group was high (I^2=67%, *P*=0.05).

- Intervention effects on food-related occasions

We identified 11 studies [44, 45, 65, 78, 88, 89, 102-106] that evaluated the effect of the interventions on HWWS before food-related occasions. Only two studies were conducted in Sub-Saharan Africa (South Africa [104] and Tanzania [91]). We found strong evidence of heterogeneity between studies. However, we found evidence of consistent positive effects of the interventions on HWWS before food-related occasions. As in the case of faecal-contact-related occasions, there were too few non-health studies to draw a conclusion regarding the effect of the interventions depending on the motives used.

The pooled estimate shows strong evidence that the estimate of the effect of the interventions on HWWS before food related occasions was higher in intervention than control groups (RR= 3.82, 95% CI: 2.56-5.70 (*P*<0.001), at least 4,783 HH, very low-quality) (Figure 3.5). After restricting the analysis to observation data only, the estimated magnitude of effect was somewhat larger (RR=4.38, 95% CI: 2.43-7.90, nine studies, at least 4,234 HH, very low-quality). We found strong evidence of heterogeneity between studies (I^2=83% and I^2=75%, the latter restricted to observation data, *P*<0.001 respectively).

All intervention-motive groups found strong evidence of a large effect of the interventions on HWWS before food-related occasions. Similar to faecal-related contact, in the mixed-motive group, the evidence is only based on a single small study. We thus did not include the overall result for this latter group in the forest plot. In the non-health-motive group, the evidence is based on only three trials, including two which are small in size. The results should thus be interpreted with caution. In the mixed-motive intervention group, Langford et al. [102] found a substantially greater effect (RR=16.04, 95% CI: 2.10-122.77 (*P*=0.008), one study, 88 HH, very low-quality). The confidence interval was extremely wide. The second largest intervention effect was observed in the non-health-motive group (RR=5.95, 95% CI: 2.38-14.85 (*P*<0.001), three studies [65, 88, 104], 816 HH, very low-quality). The confidence interval was also wide. There was no evidence of heterogeneity between studies (I^2=7%, *P*=0.36), which may be due to the small number of studies. In the health-motive group, the magnitude of effect was RR=2.24 (95% CI: 1.58-3.19 (*P*<0.001), seven studies [44, 45, 78, 89, 91, 103, 105], at least 4,230 HH, low-quality).

When restricting the analysis to observation data only in the health-motive group, the point estimate increased (RR=3.87, 95% CI: 2.03-7.41, six studies, at least 3,769 HH, very low-quality) (Figure 3.5). The confidence interval overlapped widely with that of the non-health-motive group. We found strong evidence of heterogeneity between studies in the health-motive group (I^2=76% and I^2=77%, the latter restricted to observation data, P<0.001 respectively).

- Before cooking

Six studies [44, 78, 89, 102, 103, 105] measured the effect of the interventions on HWWS before cooking. All studies were conducted in Asia. The results showed positive effects of the interventions on HWWS before cooking. Only the health-motive and mixed-motives studies reported the effect of the interventions on HWWS before this occasion.

The pooled estimate shows that the estimate of the effect of the interventions on HWWS before cooking was higher in intervention than control groups (RR=2.84, 95% CI: 1.25-6.44 (*P*=0.012), at least 3,341 HH, very low-quality) (Figure 3.5). Luby et al. (2010) and Langford et al. found quite substantial effects of the interventions (RR=57.32, 95% CI: 1.89-1735.5 and RR=30.58, 95% CI: 4.37-214.07). When restricting the analysis to observation data only, the magnitude of effect increased (RR=6.17, 95% CI: 0.70-54.42, four studies, at least 2,792 HH, very low-quality). There was however little statistical evidence of an intervention effect (P=0.10). The confidence interval was quite large. We found evidence of substantial heterogeneity between studies in the health-motive group (I^2=79%, P<0.001, and I^2=70%, P=0.03, the latter restricted to observation data).

When conducting the analysis by intervention motive, there was only strong evidence of the effect of the interventions in the mixed-motives group (RR=30.58, 95% CI: 4.37-214.07 (P=0.001), one study [102], 88 HH very low-quality) (Figure 3.5). The point estimate was very large and the confidence interval extremely wide. Although there was little evidence of an effect in the health-motive group (RR=1.65, 95% CI: 0.91-3.02 (P=0.10), five studies [44, 78, 89, 103, 105], at least 3,253 HH, very low-quality), the point estimate was consistent with an improvement. In addition, the upper limit of the confidence interval does not exclude the possibility that the effect of the interventions could be substantial. When restricting the analysis to observation data only in the health-motive group, the remaining studies were the same as the ones observed when conducting the restricted analyses irrespective of intervention-motive group. There was substantial heterogeneity between studies in the health-motive group (I^2=66% and I^2=70%, the latter restricted to observation data, P=0.03 respectively).

- Before eating

We identified five studies (i.e. [88, 89, 102, 104, 105]) which measured HWWS before eating. One study was conducted in Sub-Saharan Africa (i.e. [104]). Generally, we found limited evidence of heterogeneity between studies. The results showed positive effects of the interventions on HWWS before eating.

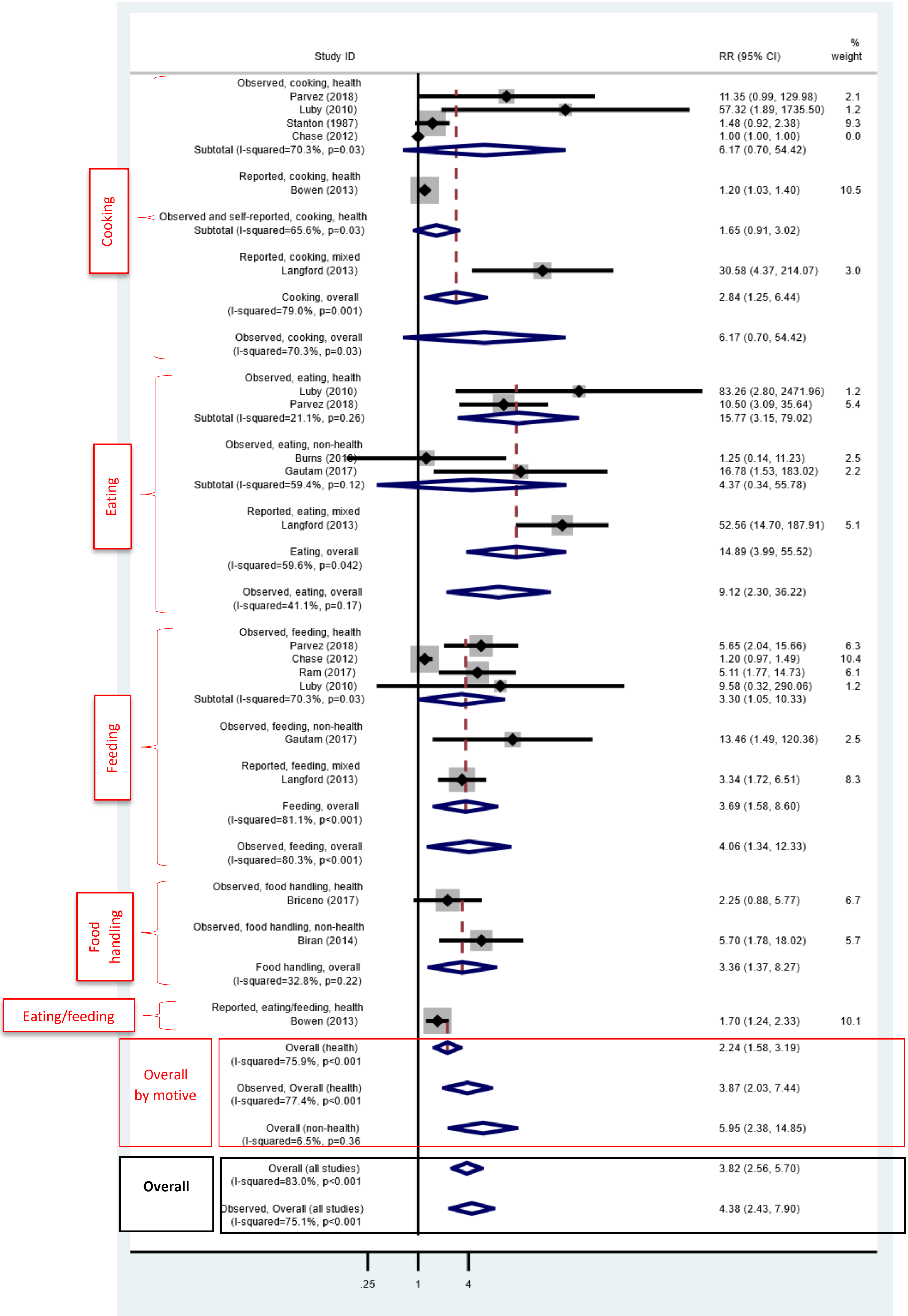


Figure 3.5. Meta-analysis of the effect of behaviour change interventions on HWWS practices before food-related contact (by occasion, intervention motive and measuring methods).

The pooled estimate indicates that the effect of the interventions on HWWS before eating was higher in intervention than control groups (RR=14.89, 95% CI: 3.99-55.52 (*P*<0.001), at least 745 HH, very low-quality) (Figure 3.5). The confidence interval was very wide. Similar to HWWS before cooking, Luby et al. (2010) [89] and Langford et al.'s [102] studies reported very large effect sizes, with very wide confidence intervals. After restricting the analysis to observation data, the magnitude of effect decreased (RR=9.12, 95% CI: 2.30-36.22, four studies, at least 657 HH, very low-quality). The confidence interval was still quite wide. We observed moderate heterogeneity between studies (I^2=60%, *P*=0.04). When restricting the analysis to observation data, there was relatively low heterogeneity I^2=41%, *P*=0.17. For both analyses, the evidence is based on studies which are small in size, and should thus be interpreted with caution.

When looking at the results by intervention motive, both the health-motive and mixed-motive intervention groups found strong evidence of an effect of the interventions on HWWS before eating (RR=15.77, 95% CI: 3.15-79.02 (*P*=0.001), two studies (i.e. [89, 105]), at least 269 HH, very low-quality (Figure 3.5) and RR=52.56, 95% CI: 14.70-187.91 (*P*<0.001), one study (i.e. [102]), 88 HH, very low-quality respectively). All studies used direct observation to measure HWWS practices in the health-motive group. We found little evidence of heterogeneity between studies in the health-motive group (I^2=21%, *P*=0.26), which may be due to the small number of studies. The point estimates were large in both groups, and with very wide confidence intervals. Whilst there was no evidence of an effect of the interventions in the non-health-motive group (RR=4.37, 95% CI: 0.34-55.78 (*P*=0.26), two studies [88, 104], 468 HH, very low-quality), the point estimate was consistent with an improvement. The confidence interval is, however, quite wide. The magnitude of heterogeneity between studies was moderate (I^2=59%). The evidence is only based on a maximum of two studies per intervention-motive group. Thus, the findings should be interpreted with caution.

- Before feeding a child

Six studies [45, 78, 88, 89, 102, 105] assessed the effect of the interventions on HWWS before feeding a child. All studies were conducted in Asia. We found positive effects of the interventions on HWWS before feeding a child. There was strong evidence of heterogeneity between studies.

The pooled estimate shows that the effect of the interventions on HWWS before feeding a child was higher in intervention than control groups (RR=3.69, 95% CI: 1.58-8.60 (*P*=0.002), at least

1,449 HH, very low-quality) (Figure 3.5). When restricting the analysis to observation data only, the magnitude of effect slightly increased (RR=4.06, 95% CI: 1.34-12.33, five studies, at least 1,361 HH, very low-quality). We found strong evidence of heterogeneity between studies (I^2=81% and I^2=80%, the latter restricted to observation data, *P*<0.001 respectively).

The analysis conducted by intervention-motive group showed strong evidence of an effect of the interventions on HWWS practices before feeding a child in the mixed-motive group (RR=3.34, 95% CI: 1.72-6.51 (*P*<0.001), one study, 88 HH, very low-quality) (Figure 3.5). In the health-motive and in the non-health-motive groups, there was some evidence of an effect of the interventions (RR=3.30, 95% CI: 1.05-10.33 (*P*=0.04), four studies [45, 78, 89, 105], 1,122 HH, very low-quality and RR=13.46, 95% CI: 1.50-120.93 (*P*=0.02), one study [88], 239 HH, very low-quality respectively). The point estimates in the mixed-motive and health-motive groups were comparable, and the confidence intervals overlapped widely. All studies in the latter motive group used direct observation to measure the point estimate. We found strong evidence of heterogeneity between studies in the health-motive group (I^2=81%, *P*<0.001). In the non-health-motive and mixed-motive groups, the evidence is only based on one study each. The findings should thus be interpreted with caution.

- Before handling food

We identified two studies [65, 91] which reported the estimates of the effect of the interventions on HWWS before handling food. The results should thus be interpreted with caution. One study was conducted in Sub-Saharan Africa [91]. The pooled estimate shows that the effect of the interventions on HWWS before handling food was higher in intervention than control groups (RR=3.36, 95% CI: 1.37-8.27 (*P*=0.008), 1,072 HH, very low-quality) (Figure 3.5). However, the confidence interval was wide. Both studies used direct observation to measure HWWS. We found no evidence of heterogeneity between studies (I^2=33%, *P*=0.22), which may be due to the small number of studies.

One study in the health-motive [91] and non-health-motive groups [65] each reported an effect of the interventions on HWWS practices before non-disaggregated food-handling occasions. Only Biran et al. (2014) [65] found strong evidence of an intervention effect on HWWS practices (RR=5.70, 95% CI: 1.79-18.11 (*P*=0.003), 348 HH, very low-quality) (Figure 3.5). The confidence interval was wide. In the health-motive group, although Briceño et al. [91] found weak evidence of an intervention effect, the point estimate was consistent with an improvement (RR=2.25, 95% CI: 0.88-5.77 (*P*=0.09), 724 HH, low-quality) (Figure 3.5). However, the confidence interval was also wide.

- Before feeding/eating

Bowen et al. [44] is the sole study which reported an intervention effect combining feeding and eating occasions together. The authors found strong evidence that the estimate of the effect of the intervention on HWWS before feeding/eating was higher in intervention than control groups (RR=1.70, 95% CI: 1.24-2.33 (*P*=0.001), 461 HH, very low-quality) (Figure 3.5). The intervention used a health-motive.

- Interventions effects on HWWS practices after faecal-contact and food-handling occasions combined

We identified four studies [53, 90, 99] which only reported the combined estimates of the effect of the interventions on HWWS after both faecal contact and food-related occasions. All the studies but one [53] were conducted in Sub-Saharan Africa. We found evidence of heterogeneity between studies. There was weak evidence of an effect of the interventions on HWWS after faecal contact and before food-related occasions combined. Only the health-motive and mixed-motives intervention groups measured HWWS at this occasion. There were too few studies to draw conclusions regarding the effect of the interventions depending on the motives used.

The pooled estimate shows that the effect of the interventions on HWWS was higher in intervention than control groups (RR=1.46, 95% CI: 0.94-2.29, 1,229 HH, very low-quality) (Figure 3.6). There was however weak evidence of an effect of the interventions (*P*=0.09). Nevertheless, the upper limit of the confidence interval does not exclude that there could be a substantial intervention effect. After restricting the analysis to observation data only, the estimated magnitude of effect was slightly larger (RR=1.75, 95% CI: 0.39-7.83, two studies, 888 HH, very low-quality), although there was still no evidence of an effect of the interventions. We found strong evidence of heterogeneity between studies (I^2=84% and I^2=94%, the latter restricted to observation data, *P*<0.001 respectively).

Two studies in the health-motive and mixed-motive intervention group each reported the effect of the interventions on HWWS practices after faecal contact and before food-related occasions combined. There was only strong evidence of an effect of the interventions in the mixed-motive intervention (RR=1.29, 95% CI: 1.15-1.45 (P<0.001), two studies [99], 341 HH, very low-quality) (Figure 3.6). We found no evidence of heterogeneity between studies in this group (I^2=0%, P=0.73). This may be due to the small number of studies. Although there was no evidence of an effect of the interventions in the health-motive group, the point estimate was consistent with an improvement (RR=1.75, 95% CI: 0.39-7.83 (P=0.47), two studies [53, 90]) 888 HH, very low-quality). We found strong evidence of heterogeneity between studies in the health-motive group: I^2=94%, P<0.001.

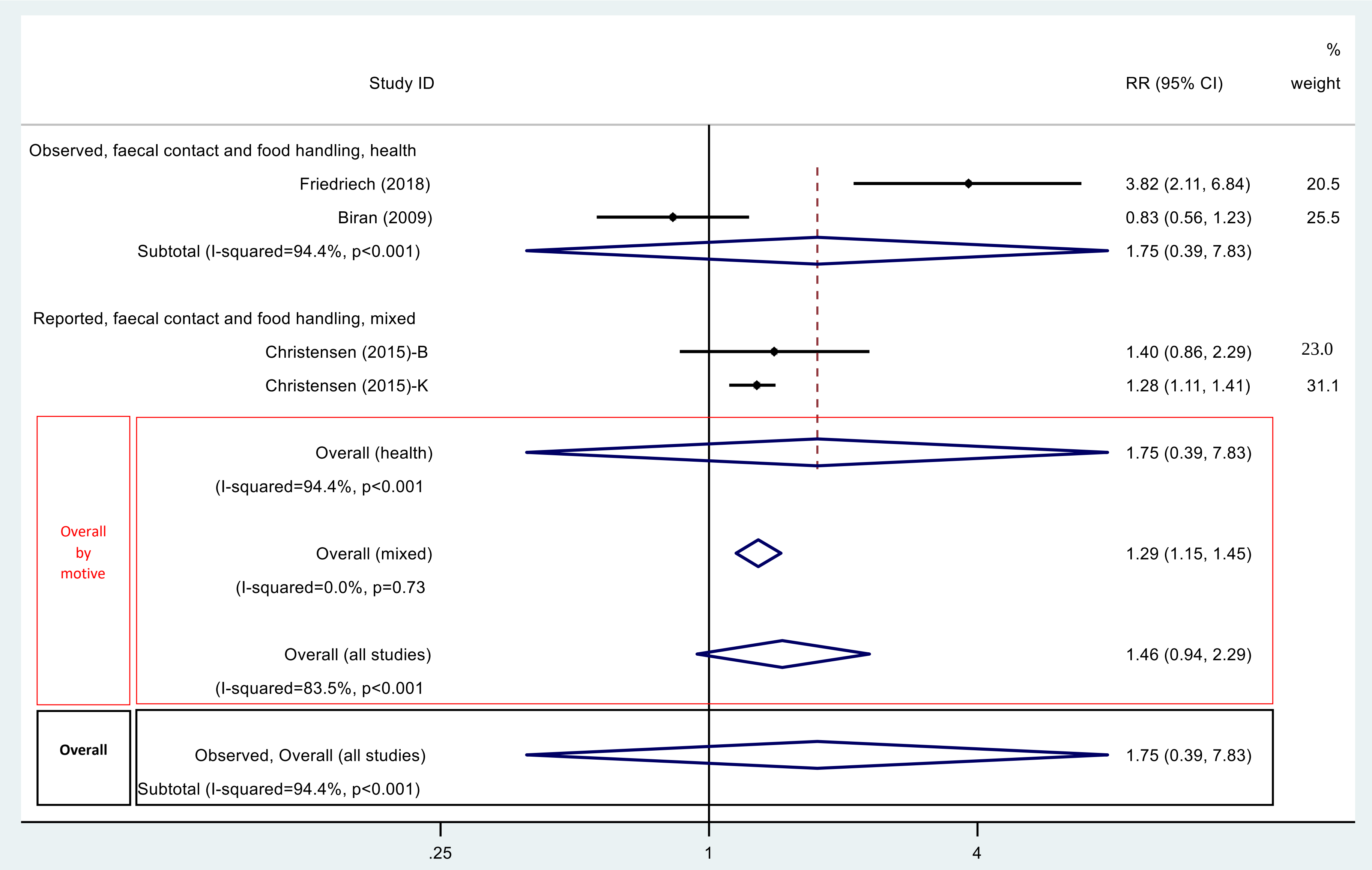


Figure 3.6. Meta-analysis of the effect of behaviour change interventions on HWWS practices after faecal contact and food-handling occasions combined (by occasion, intervention motive and measuring methods)

Discussion

This review included 20 trials conducted in LMIC settings that measured the effect of the interventions on HWWS after faecal-related contact and before food-related occasions. These included more than 9,868 HH. Most studies were conducted in Asia, with only six studies conducted in sub-Saharan Africa. We identified three different intervention-motive groups, as opposed to the two anticipated (i.e. health-motive and non-health-motive groups); the third group used a combination of health and non-health motives. There tended to be more and larger studies in the health-motive group, compared to in the non-health-motive and mixed-motive groups.

The studies were of low to very low quality due to risks of bias. For instance, all studies suffered from performance and/or detection bias. This is largely due to the nature of handwashing interventions, with intervention components visible, making it impossible to fully blind participants and trial personnel. Most studies made little attempt to minimise these biases. Examples of other biases the studies often suffered from were selection bias and attrition bias. Overall, there was substantial heterogeneity of results between studies. This is expected due to the complexity of human behaviour and the multiple factors that come into play [93]. It is thus difficult to predict behaviours from one setting to another [93]. Additionally, the interventions were implemented in different settings and contained different messages.

Nevertheless, many studies used similar intervention components (e.g. discussions, posters, intervention branded products). About half of the studies provided soap as part of their intervention, and about one third provided some sort of handwashing station or assistance to build such facilities. Generally, the intervention messages targeted both HWWS after faecal-related contacts and before food-handling related occasions.

In the meta-analyses, we only included studies which directly measured HWWS practices using observations or self-reported measures (17 studies), with most using structured observations. Structured observations remain the most reliable method to measure handwashing practices, despite the risk of a Hawthorne effect ("reactivity"), whereby participants tend to change their behaviours in the presence of an observer [32, 36, 37, 111]. While reactivity is a major concern in any study using structured observations, most dedicated studies have found that reactivity is limited (e.g. [33, 34, 36, 38]). Self-reported handwashing practices are also prone to bias [32, 33, 110, 111]. As handwashing is a socially desirable behaviour in most settings, individuals tend to over-report their practices [32, 33, 110, 111]. Nevertheless, both structured observations and self-report are likely to be more reliable than proxy indicators, which do not necessarily correlate with actual handwashing practices [48, 111].

Overall, we found modest evidence of an effect of the interventions on HWWS after faecal-related contact (RR=1.39, 95% CI: 1.20-1.60, 10 trials, 3,116 HH, very low-quality). We did not find any substantial difference in the effect of the interventions depending on whether the intervention motive used was health-based (RR=1.57, 95% CI: 1.26-1.94, seven studies), non-health-based (RR=1.38, 95% CI: 1.02-1.87, three studies) or a mix of health and non-health-based (RR=1.14, 95% CI: 1.04-1.24, one study).

When looking at disaggregated faecal-contact related occasions, there was modest evidence of an effect of the interventions on HWWS practices after toilet use (RR=1.44, 95% CI: 1.14-1.81, five studies) and after cleaning a child's bottom (RR=1.73, 95% CI: 1.03-2.99, four studies). We did not find evidence of an effect of the interventions on HWWS practices when the studies reported intervention effects without disaggregating toilet use and cleaning a child's bottom occasions (RR=1.28, 95% CI: 0.85-1.92, five studies). Nevertheless, the point estimate was consistent with that of HWWS after toilet use and after cleaning a child's bottom when both occasions were presented separately. This negative result may be due to low power.

We also did not find any substantial differences in the effect of the interventions on HWWS practices after each faecal-contact related occasion, depending on the intervention motive used. For instance, after toilet use, we found some evidence of an effect of the interventions on HWWS practices in the health-motive group (RR=1.75, 95% CI: 1.03-2.99, four trials). In the non-health-motive group and mixed-motive groups, there was weak evidence of an effect of the interventions, although the point estimates were consistent with an improvement (RR=1.35, 95% CI: 0.99-1.84, one trial, in the non-health-motive group; and RR=1.10, 95% CI: 0.98-1.24, one trial, in the mixed-motive group). The confidence intervals overlapped widely with that of the health-motive group. Due to the small sample size, the studies in the non-health motive [66] and in the mixed-motive group [102] may not have had sufficient powered to detect an intervention effect. Neither study reported a sample size calculation for the handwashing outcome.

The results of the meta-analyses seem to indicate that it is easier to change HWWS practices around food handling than it is around faecal contact. Overall, the magnitude of effect (RR=3.82, 95% CI: 2.56-5.70, eleven trials, 4,783 HH, low-quality) was substantially larger than that observed after faecal contact-related occasions.

Generally, due to either the small size of the trials in some intervention-motive groups [65, 88] or the low frequency of HWWS in the control group in some studies [89, 102, 105], we observed very large intervention effect sizes and wide confidence intervals. For instance, in Langford et al. [102], the frequency of HWWS before cooking in the control group was 2% (1 of 43 observations), compared to 71% (32 of 45 observations) in the intervention group, post-intervention delivery (RR=30.58, 95% CI: 2.85-328.46). Similarly, in Luby et al. (2010)'s trial, the HWWS frequency before eating was 0% (0 of 264 observations) in the control group, compared to 16% (98 of 626 observations) in the intervention group (RR=83.26, 95% CI: 2.80-2,471.96). Thus, the results per intervention motive are very imprecise for certain food-handling related handwashing occasions. It is thus difficult to assess whether one intervention-motive is more effective than the other to increase HWWS practices before food-handling related occasions.

When looking at disaggregated food-handling related occasions, we found strong evidence of an effect of the interventions on HWWS before cooking (RR=6.17, 95% CI: 1.25-6.44, six trials); before eating (RR=14.89, 95% CI: 3.99-55.52, five studies); before feeding a child (RR=3.69, 95% CI: 1.58-8.60, six trials); and before handling food (RR= 3.36, 95%CI: 1.37-8.27, two trials).

Four studies [53, 90, 99] only reported the effect of the interventions on HWWS after faecal contact and food-related occasions combined. There was weak evidence of an effect of the interventions on HWWS practices (RR=1.46, 95% CI: 0.94-2.29, 1,229 HH, very low-quality). Handwashing after faecal contact and food-handling occasions belong to two distinct handwashing domains. Thus, they may require different sets of strategies and motives to encourage behaviour change. This may explain why the magnitude of intervention effect on HWWS after faecal-contact related occasions was modest compared to that observed before food-handling related occasions. This echoes Luby et al.'s (2011) observation that strategies specific to handwashing before food preparation should be developed and evaluated, as this handwashing occasion is a fundamentally different context from handwashing after faecal contact [112]. Nevertheless, only one trial [88] out of the four [66, 103, 104] which had only targeted HWWS practices at occasions related to a single handwashing domain found evidence of an effect of the intervention. The discrepancy in the effect sizes of interventions on HWWS after faecal contact and before food-related occasions could also be due to the small number of observations for the latter occasion.

When conducting the analysis by intervention motive, as mentioned previously, there are too few studies to draw any conclusions regarding the relative effectiveness of different intervention motives on HWWS practices before food-handling occasions.

We conducted a subgroup analysis restricted to observation data only in order to assess whether there was a difference in estimated intervention effect depending on the measurement methods. The subgroup analyses systematically returned higher effect estimates in favour of the interventions for observed outcomes, compared to the main analyses combining observed and self-reported data, except for HWWS before eating. This appears, at first sight, to be in contradiction with handwashing studies showing the tendency of self-report of handwashing practices to be over-reported (e.g. [32, 33, 110, 111]). A possible explanation for this finding is that if substantial overreporting occurs in both arms this limits the scope to demonstrate an intervention effect. When relative risk measures are used, if over-reporting leads to a high frequency of HWWS in the control arm, this limits the magnitude of the RR.

In both Chase et al. [78] and Langford et al. [102]'s trials, in which both HWWS self-reported and observed measures were reported, the authors systematically found substantially higher self-reported HWWS estimates compared to the observed ones. In the former study, where the effect of the intervention at follow-up was measured using both methods, there was no evidence of an effect, irrespective of whether observations or self-report was used. The quasi-absence of HWWS practices before cooking and before eating in the control group, despite the use of self-report in Langford et al. [102]'s trial may indicate that some handwashing occasions are not as socially desirable as others, and thus not as prone to over-reporting. Nevertheless, reported handwashing frequencies may depend on whether the question is asked in an open-question format or prompts the response regarding specific occasions. Among the six studies [44, 78, 99, 102, 104] which used self-report, only Burns [104] used prompts. Langford et al. [102] did not specify how the question was asked.

To our knowledge, our review is the first to compare the effect of handwashing interventions on HWWS practices according to the type of behaviour-change motives used to design the interventions. We identified four reviews [12, 19, 111, 113] which reported the effect of the interventions on HWWS practices, among other outcomes. Watson et al. (2017) conducted a systematic review to assess the effect of handwashing interventions targeted at children on both health and behaviour change outcomes [111]. The authors included both RCTs and non-RCTs [111]. Six of the eight included studies were cluster-RCTs of school-based handwashing interventions [111]. The remaining cluster-RCT was Nicholson et al. (2014)'s trial [101], which we also included in our review.

Due to high heterogeneity between studies regarding intervention types and outcomes measure reported, Watson et al. [111] chose to only synthesise the results qualitatively. Similar to our findings, Watson et al. judged the included studies to be of low quality and with high risk of bias [111]. As different methods were used to measure handwashing practices (e.g. structured observations, soap consumption, self-report, and knowledge assessment), the authors found it difficult to directly compare studies results [111]. Nevertheless, Watson et al. [111] concluded that, whilst the interventions used different methods to change HWWS practices, there is not sufficient evidence to determine which approach is most effective at changing handwashing behaviour.

Willmott et al. (2016) conducted a systematic review and meta-analysis to assess the effectiveness of handwashing interventions in reducing absence due to illness, in children in educational settings [113]. Change in handwashing practices was a secondary outcome [113]. The review only included trials conducted in educational settings (e.g. schools, day care facilities) [113]. Wilmott et al. [113] also chose to only synthesise the results qualitatively. The authors concluded that the interventions might increase children's and staff handwashing knowledge, attitudes and behaviour.

Ejemot et al. (2008) conducted a systematic review to evaluate the effect of handwashing interventions on diarrhoeal episodes in adults and children [19] and subsequently published an updated review [12]. Handwashing behaviour change was a secondary outcome measure [12, 19]. Ejemot et al. (2008) found only one cluster-RCT [103] reporting handwashing behaviour-change outcomes [19]. This study is included in our review. In the updated review, Ejemot et al. (2015) included nine cluster-RCTs from LMICs, and conducted in community-based settings (excluding schools and health settings) [12]. Of these, three were trials that we also included in our review [101-103] that reported handwashing behaviour change outcomes [12]. We draw similar conclusions regarding the individual trial's findings as Ejemot et al. (2015). However, the authors did not conduct meta-analyses, as the outcome measurement methods were different [12]. This is in contrast with our study in which we restricted the outcome measures to only self-report or structured observations for the meta-analyses, but also conducted a sub-group analysis, restricted to observation data only.

Ejemot et al. (2015) judged all three trials together to be of high quality [12], whereas we considered the quality of evidence to be low to very low. The authors conducted an analysis restricted to blinded trials (i.e. if outcome assessors were blinded), and found a slightly smaller effect size, although the results continued to show statistical evidence of an intervention effect [12]. This is the reason Ejemot et al. (2015) gave to justify not downgrading the studies for risk of bias [12]. We also downgraded the trials for serious imprecision due to the methods used to measure the outcome (e.g. self-report or proxy measures) or very wide confidence intervals.

Limitations

This systematic review is not without limitations. The review was conducted by the PhD candidate alone. We cannot exclude the possibility that relevant reports were inadvertently excluded. Nevertheless, having more than one reviewer is most important when difficult judgements are needed to select or reject articles [74]. In this review, the inclusion criteria were relatively unambiguous.

The quality of the studies was generally low to very low, due to the significant risk of bias found in all studies, notably performance and detection bias. Some studies also suffered from poor reporting. The low quality of studies is in line with findings from previous systematic reviews of handwashing interventions in LMICs reporting studies with serious design limitations and poor quality of reporting (e.g. [12, 111, 113, 114]).

The interventions were not always described in a fashion that allowed us to determine the key behaviour-change motives used with certainty. This was particularly the case for studies which seemed to have used more than one type of motive. We thus cannot exclude the possibility that some of the trials' motives were misclassified. The estimates of the effect of different motives on handwashing practices at certain occasions may thus be unreliable. For the few studies which did not account for clustering, or which adjusted for clustering but reported their findings in a fashion which made it difficult to comprehend the clustered-adjusted data, the standard errors were computed assuming a *deff* of 1.5. This is comparable to the *deff* reported in similar studies (e.g. [45, 65, 88-91]). The assumed *deff* may thus have been too conservative or not conservative enough in certain trials. For these studies, the confidence intervals may thus be smaller or wider than they actually are.

Due to the small number of studies per handwashing occasion, we were unable to assess publication bias. However, given the number of studies which reported negative results in terms of statistical significance, we believe publication bias was not an important issue in this review. As mentioned previously, we expected high heterogeneity between studies. Nevertheless, we believe that there is value in examining what appears to be happening 'on average'. Additionally, our review only included studies from household/community settings, and the meta-analysis was restricted to directly measured handwashing outcomes (i.e. using self-report or direct observations). The questions this review aimed to answer, in the context of the broader literature, also warranted the conduct of meta-analyses.

Conclusion

This review found that handwashing practices after faecal-related contact appear to be more difficult to change compared to those before food-related occasions. Additionally, we were not able to confirm the claim that handwashing interventions designed using non-health motives are more effective at increasing handwashing practices than traditional interventions built on health models. Given the limitations of the included studies and small number of studies per handwashing occasion, the estimates of handwashing intervention effects found for each occasion and in each intervention-motive group should be carefully interpreted. Further research may produce different estimates. In future trials, efforts need to be made in order to mitigate the impossibility of fully blinding handwashing trials.

Ideally, intervention messages should be tailored to the specific handwashing occasion they aim to impact on. Similarly, when handwashing events are measured for more than one occasion, researchers should report estimates for each occasion separately, rather than presenting a single estimate combining various different handwashing occasions.

Based on the experience of conducting the systematic review, better reporting of HWWS studies is needed. Interventions need to be better described, including the key behaviour change motive used, as well as how the intervention was developed. More consistent reporting of results is needed. We would recommend risk ratios and confidence intervals, as well as exact p-values. This would facilitate the conduct of systematic reviews and meta-analyses, as well as making it easier to compare results between studies.

More trials evaluating the effect of handwashing interventions using behavioural motives other than health are needed. In the event that traditional health-focused handwashing interventions are indeed not very effective, it is key to identify and evaluate what type(s) of intervention(s) would be appropriate in order to contribute to a reduction in under five mortality. More trials need to be conducted in Sub-Saharan Africa, which is the world region with the largest burden of disability adjusted life years caused by diarrhoeal diseases [115].

Chapter 4 - Social Norms Theories and their Application to Handwashing

This chapter presents social norms theories and social norms approaches to behaviour change, including the theory of normative social behaviour which informed the trial's main intervention design. We also review evidence of the effect of interventions, designed with behaviour change mechanisms from social norms theories and approaches, on HWWS practices. The chapter is divided in 4 main sections:

1. Social norms theory (SNT) and the Social norms approach (SNA)
2. Theory of normative social behaviour (TNSB): an extension of SNT
3. Applicability of SNT and TNSB to handwashing
4. Review of the effectiveness of SNT-based interventions on handwashing practices in low- and middle-income countries

In the thesis, social norms theory (SNT) in the singular will specifically refer to social norms theory. Social norms theories (SNTs) in the plural will refer to the group/subgroup of theories developed by scholars to explain the relationship between norms and behaviours (such as the TNSB).

1. Social norms theory and the social norms approach

1.1. Definition of norms

Norms can be defined as the set of unwritten rules which govern behaviour, and generate social expectations about the 'proper' way to behave in particular situations [116-118]. While social norms usually develop in small, close-knit groups, they often spread well beyond the original group through communication [119-122]. Norms exist at both the collective and the individual level [119, 123-125]. Collective norms operate at the group, community or cultural level, and emerge through interactions among members within a social group or community [119, 123-125]. Lapinski & Rimal (2005) argue that:

> "At the collective level, norms serve as prevailing codes of conduct that either prescribe or proscribe behaviours that members of a group enact [119]."

However, because collective norms are informally codified and not explicitly stated, the interpretation of these norms can differ from person to person [123, 125-127]. Thus, measuring collective norms through aggregating data collected at the individual level is likely to be misleading [119]. Individuals' interpretation of collective norms is referred to as perceived norms [119, 125, 127, 128]. These latter exist at the individual level, and thus psychological level [119].

1.1.1. Descriptive norms

Descriptive norms, which are key in social norms theories (SNTs) and the social norms approach (SNA), refer to what is actually done [119, 127-129]. This norm exist both under the collective and the perceived norms [119]. At the collective level, descriptive norms could be evaluated by, for instance, assessing the media portrayal of a given behaviour [119]; while perceived descriptive norms could be assessed by, for instance, asking individuals whether they believe the majority of people around them engage in a given behaviour.

1.2. Social norms theory

Social norms theory states that the majority of people's behaviour is influenced by their perception of the behaviour of other members of their social group [130-133]. Individuals copy what they believe 'everyone else is doing' (perceived descriptive norms) [131], in order to fit into their social groups [131, 132]. SNT argues that individuals tend to misinterpret what the prevailing collective descriptive norms are [134]. Such misinterpretations often occur in relation to overestimated risk-related behaviours, and underestimated protective (healthy) behaviours [125, 127, 131].

For instance, in a study aimed at correcting Western Washington University students' alcohol and drug misperceptions in the United States (U.S.), Fabiano (2003) reported that, at baseline (1997), 89% of students believed that their peers drank heavily at least once a week [135]. However, the alcohol high-risk consumption levels, measured in 1993 and 1996, were both 34% [135]. Similarly, Pischke et al. (2015) conducted a study to assess European students' self-other discrepancies pertaining to use and attitudes towards use of tobacco [136]. The study also evaluated the association between personal use and approval of tobacco use, and perceptions of peer use and peer approval of tobacco use [136]. The study was conducted in Belgium, Denmark, Slovak Republic, Spain, Turkey and the United Kingdom [136]. The authors reported that 3,362 (75%) of 4,482 university students misperceived their peers to be more frequent users of tobacco than themselves [136]. It is worth pointing out that, in such studies, as the unhealthy behaviour is usually measured via self-reported measures, one cannot exclude the possibility that participants under-report their level of engagement in the problematic behaviour, given the negative connotation attached to it. The reported difference between actual and perceived behaviour may thus be overestimated.

Consequently, and in order to align with what they believe the collective norm is, individuals change their own behaviour to conform to their perceived norms [119, 134]. For instance, Perkins & Wechsler (1996) conducted a study among college students in the U.S., to assess the relationship between the perception of college students on norms around campus alcohol use and personal alcohol abuse [137]. The authors reported that there was a significant association between the strength of students perception of a lenient norm around drinking and personal alcohol abuse [137]. Similarly, in Pischke et al.'s (2015) study, respondents who believed that the majority of their peers smoked at least three times every week had 2.66 (95% CI: 1.90-3.73) times the odds of smoking for the same amount of time per week, compared to students who never smoked [136].

1.3. The Social norms approach (SNA)

The social norms approach (SNA) refers to interventions using SNTs in order to change risky behaviours [127]. The SNA predicts that correcting individuals' misperceptions of the prevailing descriptive norms, by revealing the true 'healthy' descriptive norm, will result in most individuals refraining from engaging in a given risky behaviour [126, 134, 138]. Thus, the SNA to behaviour change relies on giving feedback of a normative nature, in order to correct misperceptions that negatively influence behaviour [134]. By being made aware of the actual 'healthy norm', and realising the discrepancy between this latter and their unhealthy behaviour, individuals should feel encouraged to align their behaviour to the healthy norm [126, 138].

For instance, Perkins & Craig (2002) conducted a study to evaluate an intervention aimed at reducing alcohol use among students at Hobart and William Smith colleges, in New York, U.S. [131]. The authors used a mixture of posters, an interactive website and electronic media to change the misperceptions that college students had about alcohol use on their campus [131]. Examples of normative messages used to change the perceived descriptive norm were:

> *"The majority of Hobart and William Smith drink 2 days or less per week or do not drink at all"*, or *"83% of students NEVER drive in an alcohol impaired condition during the academic year* [131]."

One year post-intervention delivery, the perceived percentage of heavy drinkers (i.e. 5 or more drinks at residence hall gatherings, bars, and parties) went from 70% to 55% ($P<0.001$) [131]. The actual percentage of students drinking heavily weekly in a row went from 47% to 39% [131]. Additionally, the perceived average number of drinks consumed by all students at parties or bars went from 7 to 6 ($P<0.001$); whilst the actual average number of drinks consumed by all students at such settings went from 5 to 4 ($P<0.05$) [131].

Berkowitzs (2005) points out that, whilst the SNA aims to correct *misperceptions* regarding an already existing *collective healthy* norm around a given behaviour, this is in contrast with many public health campaigns using social norms in their behavioural change efforts [134]. Indeed, public health campaigns using social norms commonly intend to change existing *unhealthy collective* norms around a given behaviour [134]. We could add that, in public health, individuals' perception of the descriptive norm around a given unhealthy behaviour is often accurate, compared to in SNTs and SNA. Such distinctions are crucial, as this supposes that public health interventions aimed at changing social norms around a given behaviour assume a different behaviour change-model, as argued by Berkowitz (2005) [134].

1.4. Evaluation of interventions using the SNA

The SNA has been most extensively applied for the prevention of episodic binge drinking and alcohol-related harm among college students [134, 139]. However, the effectiveness of the SNA in changing behaviours has been inconclusive [139-145]. Foxcroft et al. (2015) conducted a systematic review (SR) to assess the effectiveness of interventions using the SNA at reducing alcohol misuse negative behaviours and consequences, among college and university students

[139]. 66 randomised controlled trials (RCTs) (43,125 participants) were included in the review, of which 59 were examined in the meta-analyses (40,951 participants) [139]. The majority of studies were conducted in high income countries (HICs) (i.e. 52 studies in the U.S. and 4 in other HICs) [139]. The intervention included the use of mail-delivered, computer-delivered, individual face-to-face, group face-to-face and social marketing campaign normative feedback [139].

Foxcroft et al. (2015) found small intervention effect sizes [139]. For binge drinking, for instance, the authors reported a standardised mean difference (SMD) of -0.06 (95% CI: -0.11 to -0.02; 16 studies; I^2=0%, *moderate quality evidence*) [139]. This corresponded to a 2.7% reduction, in the previous month, in binge drinking (this is assuming a baseline prevalence of 44%) [139]. Similarly, for drinking quantity, Foxcroft et al. (2015) reported that the SMD was -0.08 (95% CI: -0.12 to -0.05; 31 studies, I^2=13%, *moderate quality evidence*) [139]. This corresponded to a reduction of 0.9 points in the daily drinking questionnaire (DDQ) [139]. The reported results are for trials with ≥4 months follow-up [139].

The interventions effect on the change in perceived drinking norms were different depending on the intervention delivery mode [139]. For instance, there was evidence of an intervention effect on perceived drinking norms, when the interventions were delivered via computer feedback (SMD = -0.34, 95% CI: -0.57,-0.11; 6 studies, I^2=13%) [139]. By contrast, there was no evidence of an intervention effect, when it was delivered using marketing campaign (SMD= -0.06, 95% CI: -0.23,-0.11; 2 studies; I^2=81%) [139]. The evidence is however based on a small number of studies, and should therefore be interpreted with caution. Foxcroft et al. (2015) concluded that there were no substantial benefits associated with the use of SNA in preventing the misuse of alcohol among university students [139]. The authors specified that, in general, the studies provided low to moderate quality evidence, which does not exclude the chances that the reported effects be overestimated [139].

As a way to explain the mixed results of interventions using the SNA, some scholars have argued that the descriptive norm-behaviour relationship is not as unidimensional as portrayed by the general norms literature (e.g. [119, 128, 129, 142, 144, 146, 147]). Humans do not solely choose to engage in a given behaviour based on its popularity [119]. The descriptive norm-behaviour relationship is more complex than depicted, with other underlying cognitive mechanisms and behaviour attributes coming into play [119, 128, 129, 142, 144, 146, 147].

2. Theory of normative social behaviour (TNSB): an extension of SNT

Lapinski & Rimal (2005) and Rimal & Real (2003; 2005); argue that four factors moderate the relationship between descriptive norms and behaviour: injunctive norms, outcome expectations, group identity, and ego involvement [119, 128, 146]. For the purpose of this research, we will only discuss the first three constructs. Each of the first three factors can directly influence behaviour, in addition to doing so through moderating the relationship between descriptive norms and behaviour [119, 128, 146].

2.1. Injunctive norms

Injunctive norms represent what people believe is right and ought to be done [119, 128, 129]. Collective injunctive norms could be assessed by, for instance, studying policies set by given communities to prescribe or proscribe specific behaviours [119]. On the other hand, perceived injunctive norms could be measured by assessing the pressure that individuals feel from people in their reference group to conform to/engage in a given behaviour (peer pressure) [119]. Thus perceived injunctive norms characterise social approval from others [128].

Lapinski & Rimal (2005) and Rimal et al. (2005) argue that descriptive norms and injunctive norms are often congruent [119, 128]. For instance, if individuals see a given behaviour being universally performed, they would feel that this is what ought to be done, and what is expected of them [119, 128]. They would, therefore, be likely to engage in the given behaviour [119, 128]. Homans (1950)[5] and Bendor & Swistak (2001) argue that norms are meaningful only if individuals perceive that violation will result in social sanctions [148, 149]. Consequently, and to avoid social sanctions, individuals usually conform to injunctive norms [148].

The injunctive-descriptive norm complementary relationship is particularly true if both injunctive and descriptive norms are strong [119, 128]. Indeed, if individuals can observe many in their social groups engaging in a given behaviour (i.e. strong descriptive norm), they could assume that the given behaviour is socially desirable and acceptable (strong perceived injunctive norm) [119, 128]. Strong perceived descriptive norms would thus reinforce strong perceived injunctive norms [119, 128]. On the other hand, strong injunctive norms by themselves would

---

[5] As cited in 148. Bendor, J. and P. Swistak, *The evolution of norms.* AJS, 2001. **106**: p. 1493-1545.

likely not have an impact on individuals' behaviour, if individuals observe very few members of their social groups engaging in the given behaviour (weak descriptive norms) [119, 128]. In such instances, individuals could rather suppose that the given behaviour is deviant in nature [119, 128].

For instance, Asch (1951)[6] conducted a study on the effect of group pressure on the modification and distortion of judgments [150]. As part of the study, in a small group setting, when the majority of confederates clearly and deliberately provided wrong answers (strong descriptive norm), credulous study participants felt pressured to conform with the group in the responses they gave (strong perceived injunctive norm) [150][7]. They assumed that others also knew the correct answers, but that the group would expect them to conform as well [150][8]. On the other hand, when other confederates disagreed with the majority (weak descriptive norm), then credulous participants also did not feel the pressure to conform [150][9]. Thus, whilst the injunctive-descriptive norm relationship is often complementary, there are also instances when people approval of a given behaviour, does not translate into actually performing the behaviour [119, 129]. This seems to be the case for HWWS (see Section 3 of this Chapter). Lapinski et al. (2008, 2013) argue that changing the perception of descriptive norms, in limited normative campaigns with exposure to a single message, would be substantially easier than changing the perception of injunctive norms [144, 151].

2.2. Outcome expectations

Outcome expectations refer to individuals' perception that engaging in a given behaviour would bring benefits to oneself or others [119, 128]. Individuals carry out a mental calculation comparing the benefits of engaging in a given behaviour and the cost related to performing the given behaviour [119, 152, 153]. Thus, if there are strong descriptive norms and individuals believe that there are 'significant' benefits in engaging in a given action, then their behaviour is more likely to be influenced by the descriptive norms [119, 128]. Additionally, and if they do not engage in a 'perceived' prevalent behaviour, individuals would feel they are missing out on 'significant' desirable outcomes that numerous others who perform the behaviour are gaining [119, 154]. The word 'significant' is important, as if individuals perceive that there are benefits

---

[6] As cited in 119. Lapinski, M.K. and R.N. Rimal, *An explication of social norms.* Communication Theory, 2005. **15**: p. 127-147.
[7] As cited in 119. Ibid.
[8] As cited in 119. Ibid.
[9] As cited in 119. Ibid.

from engaging in a given behaviour, but that they do not see the benefits as essential, then one could argue that individuals would be less likely to engage in the given behaviour.

2.3. Group identity

Lapinksi & Rimal (2005) and Rimal et al. (2005) argue that individuals are more likely to be susceptible to prevalent descriptive norms if they assimilate with people engaging in the 'perceived' prevalent behaviour [119, 128]. In other words, in order for social networks to influence individuals' behaviours, the latter must identify or feel some degree of affinity with their reference group [119]. These latter can be family members, work colleagues, friends and so forth. In instances when individuals perceive that a behaviour is widespread in their referent group, they would be more likely to conform, because they would experience a positive effect by doing so [119, 155]. Trafimow & Finlay (1996)[10] argue that injunctive norms are most influential in close social networks with high group identity [157]. Given social approval is crucial in such settings, this could strongly motivate individuals to adhere to injunctive norms, to maintain group membership and cohesion [156].

2.4. Behaviour attributes: Public vs. Private behaviour

In addition to the above moderators, the characteristics of a given behaviour also influence the descriptive norms-behaviour relationship [119, 128]. Lapinski & Rimal (2005) and Bagozzi et al. (2000) argue that by their attributes, certain behaviours are more susceptible to normative influences than others [119, 141]. For the purpose of this research, I will only elaborate on one such attribute: Behavioural privacy.

The likelihood that a given behaviour is susceptible to normative influences depends on the private/public nature of the behaviour [119, 129]. Indeed, a given behaviour can either be enacted in the private or in the public domain, or in both of these spheres [119, 129]. Condom use, for instance, is a behaviour performed in a private space. Although the perceived collective descriptive and injunctive norms could be favourable to condom use, individuals would be aware that defying the norms would not be observable by others, just as individuals themselves would

---

[10] As cited in 156. Larimer, M.E., et al., *Predicting drinking behavior and alcohol-related problems among fraternity and sorority members: Examining the role of descriptive and injunctive norms.* Psychology of Addictive Behaviors, 2004. **18**: p. 203-212.

not be able to scrutinize whether others conform to the norm or not [119]. On the other hand, if individuals can observe others' behaviour to get clues on the prevailing norms, and if there is a perceived high risk of social sanction for defying the norms, then the given behaviour is more likely to be influenced by injunctive and descriptive norms [119].

In cases where a behaviour is enacted in public settings, individuals are aware in their own mind that their behaviours are observable by others in their social environment [119]. Thus, if the behaviour is public in nature, individuals would implicitly understand that their lack of compliance to the norm would be known by members of their referent groups [119]. Consequently, public behaviours are more sensitive to normative influences than private behaviours [119]. Sherif (1935)[11] posits that for sustained normative effects, the presence of the reference group is not required when individuals internalise normative information; although at first individuals might engage in a given behaviour for compliance, in which case the presence of social referents is needed for behaviour enactment [119, 158].

In summary, in order to increase the likelihood of individuals being influenced by the behaviour of people around them, the given behaviour should be prevalent (strong descriptive norm) and enacted by people individuals identify with (group identity). The behaviour should also be socially approved (strong injunctive norm), and with perceived significant benefits to be gained from engaging in it (perceived *significant* outcome benefit).

3. Applicability of the TNSB to handwashing

The characteristics of handwashing behaviour make it potentially susceptible to normative influences [144]. Although handwashing is a behaviour that can on occasions be enacted in the private sphere, in many settings it may be commonly enacted in the public sphere [144] (e.g. public restrooms, or more relevant to this study, housing compounds). This, therefore, makes

[11] As cited in 119. Lapinski, M.K. and R.N. Rimal, *An explication of social norms.* Communication Theory, 2005. **15**: p. 127-147.

handwashing a behaviour open to others' scrutiny, and sensitive to normative influences [119, 144].

3.1. Collective descriptive and injunctive norms around HWWS after visiting the toilets

HWWS collective descriptive norms appear weak in many settings, as illustrated by the low rates of the practice. Similarly, the frequent absence of handwashing facilities in communities in low income settings, notably in public restrooms and residential compounds, suggests HWWS collective injunctive norms are weak.

3.2. Perceived descriptive norms around HWWS after visiting the toilets

Regarding HWWS perceived descriptive norms, individuals seem to be aware that people in their social groups do not wash their hands with soap. For instance, Dickie et al. (2018) conducted a study aimed at assessing the effect of perceived social norms on handwashing behaviour among students at a Scottish University [159]. The authors reported that participants rated their own handwashing practices as higher than that of their peers ($P<0.001$) [159]. Similarly, Curtis et al. (2009)'s study on communities in an 11-country formative research project, reported participants stating that handwashing was not a habit in their social environment [48].

Consequently, making individuals aware of the existing unhealthy descriptive norm around HWWS may be counterproductive, as it risks encouraging them to continue engaging in low handwashing practices. This is in line with Berkowitz's (2005) argument of using different normative strategies than the SNA when aiming to use social norms in public health interventions [134]. An appropriate social norms intervention could thus aim to increase the existing and accurate perceived low descriptive norm around HWWS, to establish a new strong descriptive norm.

3.3. Perceived injunctive norm around HWWS after visiting the toilets

By contrast, HWWS perceived injunctive norms suggest that it is a behaviour that is desirable, and ought to be done. This is illustrated by the high rates of HWWS practices commonly self-reported (e.g.[160, 161]). As reported in the systematic review (Chapter 3), Chase et al. (2012) and Langford et al. (2013) used both structured observations and self-report, to measure HWWS practices [78, 102]. Both authors found considerably higher HWWS frequencies for the self-reported measures compared to the actual observed measures [78, 102]. Similarly, in a baseline handwashing behaviour survey in Senegal, Orsola-Vidal & Yusuf (2011) stated that while 97% of interviewees reported that they washed their hands with soap, structured-observations revealed the practice to be one third as common as self-reported rates [160]. Only 20% of respondents were observed washing their hands with soap after defecation [160]. In a study conducted in Peru, while 66% of interviewees reported washing their hands with soap after coming in contact with faeces, only 34% were observed doing so [161].

Thus, and as hypothesized by the TNSB, the seemingly strong perceived HWWS injunctive norm has limited effect on individuals' choices to practice HWWS, given the perceived weak HWWS descriptive norm. Nevertheless, one may still be able to use the seemingly strong perceived injunctive norm around HWWS, in conjunction with other relevant norms-related constructs, to increase HWWS practices.

3.4. Perceived outcome expectation around HWWS after visiting the toilets

The majority of behaviour-change handwashing campaigns are based on the premise that health is a key handwashing motivator. This presupposes that individuals see the avoidance of disease from HWWS as a valuable outcome. The evidence from the systematic review that we conducted showed inconclusive results for the effect of handwashing interventions based on health motives on HWWS practices at key occasions. On the other hand, and as stated in Chapter 2, disgust seems a good candidate to motivate HWWS practices [48, 58, 59, 65]. One potentially important difference between health and disgust as motivators is the immediacy of the benefit. With respect to health any benefit is not immediate and may come hours or days later making the causal link much harder to discern. For disgust avoidance, the benefit is more immediate.

Consequently, an intervention which shifts the outcome expectation around HWWS from health to disgust (e.g. disgust of having faeces on one's hands) could be potentially effective at increasing HWWS practices. As previously discussed, the equally inconclusive results that we found, from the systematic review, regarding handwashing interventions based on non-health motives may be due to the paucity of trials conducted. This may also be due to the use of multiple motives in the intervention design. As explained, this may have contributed to mitigating the effect of a key non-health motive on HWWS practices at the specific occasion the motive was relevant for.

3.5. HWWS behaviour publicness

The role of handwashing facilities from a TNSB standpoint, in addition to facilitating HWWS (which is a function outside of the TNSB), could be seen as making handwashing a public and visible behaviour, when the facilities are located in public settings. Handwashing facilities, therefore, increase individuals' awareness that their compliance (or lack thereof) with handwashing at times socially considered as key, is more readily accessible to others' scrutiny. By contrast, the lack of handwashing facilities, commonly observed in economically disadvantaged communities, makes the practice harder to scrutinize. Providing handwashing stations (HWS) to communities where such facilities are lacking, as part of a handwashing intervention, could thus contribute to increasing HWWS practices, by making the behaviour more visible.

In summary, as part of a context where the perceived injunctive norm around HWWS is seemingly strong in LMICs, and as a practical implication of the TNSB, an intervention we believe could be effective at increasing HWWS practices after visiting the toilets could:

- Seek to change the perceived weak descriptive norm around HWWS;
- Shift the inadequate health outcome expectation to a valuable riddance of disgust outcome benefit;
- Make HWWS after using the toilet a public behaviour via the supply of HWS.

4. Effectiveness of SNTs-based interventions on handwashing practices in LMICs

From the systematic review reported in Chapter 3, we identified nine studies [65, 66, 88, 99, 101, 102, 105, 106] where consideration of social norms around HWWS seemed to have informed the intervention design. Table 4.1.1 in Appendix 4.1 summarises the studies and interventions. The studies identified did not typically distinguish between the different types of norms, but rather spoke about social norms in general. We can thus only speculate as to which specific norm(s) was/were being targeted. The authors commonly stated that either their intervention or some of their intervention's components aimed at establishing, encouraging or spreading social norms around HWWS.

Guiteras et al. (2016) is the only study which explicitly used SNTs, along with other behaviour change theories, to design their interventions [66, 162]. Injunctive norms, behaviour publicness and outcome expectation as the norms are identifiable as part of the intervention's behaviour change mechanism. The intervention aimed at increasing HWWS practices after faecal contact [66]. Guiteras et al. (2015) sought to invoke shame around being observed engaging in a behaviour which was deemed disgusting by the community (i.e. not washing hands with soap) [162].

Additionally, Guiteras et al. (2015) postulated that shame would trigger fear of social sanction and status loss [162] (perceived injunctive norms). These negative feelings and consequences of being seen not washing hands created by the intervention are equivalent to establishing new (negative) outcome expectations from not washing hands with soap when expected. Guiteras et al. (2015) also expected that the intervention would trigger some sort of policing of handwashing practices and sanctioning of poor handwashing practices [162] (behaviour publicness and injunctive norms). This would contribute to establishing strong injunctive norms around HWWS.

Guiteras et al. (2015) only found a modest increase in HWWS after visiting the toilet (health-intervention group: RR=1.26, 95% CI: 0.92-1.73; disgust intervention group: RR=1.35, 95% CI: 0.99-1.84) compared to the control group ([66]). Unfortunately, the authors did not measure social norms around HWWS after the targeted occasions in their study. We thus do not know whether the normative behaviour change mechanism Guiteras et al. (2015, 2016) [66, 162] anticipated actually occurred.

Nicholson et al. (2014) broadly described one of their intervention's mechanisms as "*[establishing] social norms for child and mother* [101]." No further information was given. We thus cannot assess how social norms were used to inform the intervention design or how they were expected to change handwashing practices. The handwashing occasions targeted were after faecal contact and before handling food [101]. As mentioned in Chapter 3, the authors used the estimated median soap consumption in the study households (assessed by collecting soap wrappers), as a soap consumption measure and HWWS practices proxy indicator [101].

Due to the absence of masking, we had judged this measurement methods to be at high risk of bias in favour of the intervention. Participants would be highly likely to present empty soap wrappers even if soap had not been used for handwashing.

Nicholson et al. (2014) reported that the estimated median soap consumption in the intervention households was 235g compared to 45g in control households [101]. The authors concluded that the observed intervention effect on the health outcomes might have been mediated by soap use [101]. As soap wrappers do not actually tell us whether soap was used for handwashing as opposed to other purposes (e.g. laundry, washing dishes), this proxy indicator does not enable us to confidently assess the actual intervention effect on HWWS practices. Additionally, Nicholson et al. (2004) did not report measuring social norms around HWWS [101]. Consequently, we cannot evaluate the intervention's effect on norms.

Christensen et al. (2015) mentioned social norms as part of the constructs used to inform their intervention [99]. However, the authors did not provide more details as of how this was achieved. The intervention targeted HWWS after faecal contact and before food handling occasions combined [99]. As reported previously, Christensen et al. (2015) used several proxy indicators of handwashing to measure the intervention effect, besides self-report [99]. In Kakamega village, the authors found some evidence of a modest intervention effect on reported HWWS at key occasions in the handwashing interventions groups combined, compared to the control group (RR=1.28, 95%CI: 1.11-1.41) [99]. By contrast, there was no evidence of a small intervention effect in Bungoma village (RR=1.40 (95% CI: 0.86-2.29) [99]. As in Nicholson et al. (2004)'s study, the authors did not measure social norms [99]. Therefore, we cannot assess the intervention's effect on 'norms'.

Langford et al. (2013) stated that their intervention *"targeted social norms around handwashing by emphasising the idea that this is what 'responsible' mothers do* [102]*."* The intervention targeted HWWS after faecal contact and before handling food [102]. Langford et al. (2013) found some evidence of a small intervention effect on HWWS after cleaning a child's bottom (RR=1.19, 95% CI: 1.04, 1.36). They also reported strong evidence of a large intervention effect on HWWS before cooking (RR=30.58, 95% CI: 4.37-214.07), before feeding a child (RR=3.34, 95% CI: 1.72-5.51), and before eating (RR=52.56, 95% CI: 14.70-187.91) [102]. The authors found weak evidence of a small intervention effect on HWWS after using the toilets (RR=1.10, 95% CI: 0.98-1.24) [102]. HWWS was measured using self-report measures. The results should thus be interpreted with caution. Additionally, and as reported previously, the large effect-point estimates observed were due to the quasi-no observation in the control group. This may be an indication that HWWS before cooking and before feeding a child were not socially desirable behaviours in this community.

In the qualitative analysis of their intervention's effect, the authors reported having been successful at making HWWS practices after faecal contact more visible in the community (behaviour publicness) [102]. One of the justifications given by the community motivators and which rose during discussions with participants was that:

> "[The mothers] have to use the public toilets down by the stream and that's right next to the rower pump where women wash their clothes. They come out and they know people are watching so they make sure to come over and ask for some soap so they can wash their hands [102]."

It was thus important to be seen as being clean [102]. However, Langford et al. (2013) did not actually measure handwashing-related social norms. We thus cannot assess whether the changes in HWWS practices observed were due to the normative mechanisms described by the authors [102].

Parvez et al. (2017) stated that the theory of change their intervention was based on relied on:

> [...] the importance of an enabling environment created by […] the community health workers' frequent visits of motivational counselling and problem-solving, to allow behaviour change to occur at the household level […] [and] change WASH-related social norms at the compound level […] [105]."

However, the authors did not provide details as of how the intervention operated to change social norms. Parvez et al. (2018) found evidence of a difference between the control group and the intervention group for HWWS after using the toilet (RR=2.42, 95% CI: 1.57-3.74), after cooking (RR=11.35, 95% CI: 0.99-129.98), before eating (RR=10.50, 95% CI: 3.09-35.64), before feeding a child (RR=5.65, 95% CI: 2.04-15.66), and before cleaning a child's bottom (RR=2.61, 95%: 1.56-4.35) [105]. Parvez et al. (2018) did not find evidence of a difference between the control group and intervention group for HWWS before cooking [105]. As social norms were not measured, we cannot assess whether the HWWS changes observed are due to a change in norms.

In Greenland et al. (2016)'s trial, the authors reported that their intervention was centred around a group of women who gossiped about other women who did not engage in the normative behaviours [106] (descriptive and injunctive norms and behaviour publicness). The 'deviant' women were then accepted in the women's group as a reward (outcome expectation), when it was witnessed that they were practicing the normative behaviours (behaviour publicness) [106]. The intervention targeted HWWS after faecal contact and food-handling occasions combined [106]. Greenland et al. (2016) found that there was no evidence of an intervention effect on HWWS at combined faecal contact and food-handling occasions (RR=0.85, 95% CI: 0.58-1.25), nor after contact with faeces alone (RR=1.15, 95% CI: 0.73-1.81) [106].

Greenland et al. (2017) also reported a process evaluation of their intervention [163]. They conducted focus groups to *"explore social norms and opinions on the importance of gossip and social approval and their role in determining perceptions and practice of the target behaviours* [106, 163]*"* No indication was given as to how social norms were explored. The authors also used a five-point Likert-type questionnaire implemented at household-level. The questionnaire aimed to measure the behavioural determinants targeted by the intervention, including social norms [106, 163]. The specific norms measured were not mentioned. Greenland et al. (2017) found that over two thirds of participants in the control and intervention arms believed that HWWS (among other targeted behaviours) was already the social norm [106, 163]. However, due to poor responses distribution, the authors were unable to use the Likert-type questionnaire in mediation analysis, as initially planned, and failed to measure social norms [106, 163]. We thus cannot assess the intervention effect on social norms, nor whether the level of the social norms around HWWS explain the lack of intervention effect. Nevertheless, Greenland et al. (2017) stated that social norms were not changed in a measurable fashion [106, 163]

Gautam et al. (2017)'s intervention targeted HWWS before feeding a child and before eating [88]. The authors reported some of their intervention's components as having normative purposes (e.g. public handwashing pledges, intervention posters in public spaces, intervention group activities) [88]. However, Gautam et al. (2017) did not clearly explain how norms were expected to play a role via these intervention components. For instance, the authors specified that social norms were encouraged by *"re-performing folk song, etc"*, or that group norms were *"elicited via cooking demonstration* [88]*."* Gautam et al. (2017) found evidence of a difference between the control group and the intervention group at both targeted HWWS occasions (RR=15.60, 95% CI: 5.42-44.92 and RR=13.00, 95% CI: 4.96-34.06) for HWWS before eating and before feeding a child respectively. As explained previously, the large effect sizes were due to the very small number of observations. This makes the reported estimates imprecise.

Gautam et al. (2017) reported that 116 (97%) of 120 respondents in the intervention group believed that social norms relating to HWWS before feeding changed in their village over time, compared to 10 (8%) of 119 respondents, in the control group [88]. However, and based on how the question seems to have been phrased, there was a high risk of response bias, especially in the group which received the handwashing intervention. The in-table variable reporting on these outcomes is stated as *"Reported belief that social norms changed over time in village as the following [e.g. handwashing occasions] became more common* [88]*."* Additionally, no details were given as to the type of change the respondents were referring to. The questions as it appears they were phrased are not measures of the perceived descriptive norm. Consequently, we cannot assess whether the results observed were due to actual changes in norms. These questions were also not asked at baseline. Thus, we cannot evaluate whether there were any group imbalances between groups in terms of norms perception around the targeted behaviours.

As in Gautam et al. (2017)'s study [88], Biran et al. (2014) also described some of their intervention components as having normative purposes (e.g. public handwashing pledges, intervention posters in public spaces, intervention group activities) without giving further precisions [65]. Thus, it was not clear how this was achieved. For instance, the authors mentioned that, through household visits, *"HWWS norms [spread] through [the] village* [65]*."* They also mentioned that video testimonies were used in community events *"to draw attendees and reinforce norms* [65]*."* The intervention aimed to increase HWWS after faecal contact and before food-handling occasions combined and disaggregated [65].

Biran et al. (2014) found evidence of an intervention effect on HWWS at handwashing occasions combined (RR=4.75, 95% CI: 1.58-14.24), and before food-handling occasions (RR=1.89, 95% CI: 1.33-2.69) [65]. However, they did not find evidence of a difference between the control group and the intervention group for HWWS after faecal contact (RR=4.00, 95% CI: 0.52-30.71) [65]. Biran et al. (2014) did not clearly state what the number of observations was. However, the total study sample was 14 clusters (i.e. villages) [65], which is relatively small. The number of observations may have been fairly small, which would make the reported estimates imprecise.

Rajaraman et al. (2014) conducted a process evaluation of Biran et al.'s (2014) [65] intervention to evaluate whether it had changed HWWS-related social norms and the perceived benefits associated with HWWS (perceived outcome expectation) [164]. The authors measured norms around HWWS after contact with faeces and before eating [164]. This was accomplished by asking participants whether they thought that almost everyone in their village washed their hands with soap after defecation (perceived descriptive norms around HWWS after defecation), and whether they thought that almost everyone in their village washed their hands with soap before eating (perceived descriptive norms around HWWS before eating) [164]. Participants were also asked whether they thought that people in their villages washed their hands with soap more than in other villages [164].

Rajaraman et al. (2014) found that 61 (35%) and 63 (36%) of 174 participants in the intervention villages reported that almost everybody in their villages washed their hands with soap after defecation and before eating respectively [164]. This was compared to 14 (8%) and 17 (10%) of 171 respondents ($P<0.05$) in the control group [164]. Additionally, 170 (98%) of 174 respondents in the intervention group reported that people in their villages washed their hands with soap more than in other nearby villages, compared to 72 (42%) of 171 respondents in the control group ($P<0.05$) [164]. Rajaraman et al. (2014) concluded that the intervention had successfully established HWWS as a social norm [164].

However, and as in Gautam et al. (2017) [88], social norms around HWWS were not measured at baseline. As the number of units randomised was relatively small, we cannot exclude the possibility that there were differences between groups regarding perception of 'norms' around HWWS at baseline. Additionally, the norms around HWWS at the key occasions were measured with one item. Due to the social desirability bias attached to HWWS, there was high risk of response bias. This is even more so as the 'norms' items were positively framed. The risk of response bias was even greater in the intervention group, given that the fieldworkers who administered the questionnaire were the same as those who had monitored the intervention implementation during the trial [164]. The fieldworkers were thus known to the respondents. The authors reported that there was no evidence of an association between the normative perceptions around HWWS and the observed changes in handwashing frequencies [164]. This is in contrast with SNTs in general.

In summary, very few studies in LMICs have used SNTs to inform their intervention design, and change norms around HWWS, in order to increase the practice. The identified body of evidence is inconclusive. Additionally, the general absence of measures of norms around handwashing does not enable us to assess how norms may have contributed to the observed intervention effects on HWWS practices. We also cannot assess whether social norms around HWWS changed as a result of the interventions.

Discussion

By its nature, handwashing is a behaviour highly susceptible to normative influences. This is due to the fact it is often performed in public. There is seemingly strong injunctive norm around HWWS practices, albeit in the context of low descriptive norms in LMICs. Limitations of reliance on health outcome expectations and suboptimal study of the norms-constructs around HWWS provides scope for further development of normative behaviour change interventions. For the past few years, social norms have been posited as a key motivator of handwashing [48, 56, 57, 64]. Thus, scholars have encouraged the design of interventions aimed at changing norms around HWWS, in the hope of significantly increasing the practice [48, 56, 57, 64].

Nevertheless, the results of interventions using social norms theories and approaches to change behaviour have been inconclusive thus far [139-145]. One systematic review found that such interventions had no major effect on reducing misuse of alcohol among university students, which is the field of research in which social norms theories have been the most extensively used [139]. We found a paucity of studies designing handwashing interventions informed by social norms theories in LMICs [65, 66, 88, 99, 101, 102, 105, 106].

The majority of studies identified were poorly reported. Additionally, most of the identified studies did not clearly specify the norms their interventions were targeting, nor the normative behaviour change mechanism by which they were expected to work. In general, there was an absence of (adequate) measurement of norms around the targeted handwashing occasion. This made it difficult for us to assess how the intervention had an impact on handwashing, and whether it was through changes in norms around the practice or not.

Conclusion

Considered as a whole, the evidence for the effectiveness of social norms-based handwashing interventions in increasing handwashing practices is inconclusive, as seen in the broader social norms literature. More studies need to evaluate social norms interventions to increase handwashing practices in LMICs. Such studies should clearly report how social norms informed their interventions design, and the intended normative behaviour-change mechanism. As well as measuring changes in handwashing practices, these studies should seek to measure the relevant norms relating to the targeted handwashing occasions, and examine the association between the targeted behaviour and the measured norms. Based on theoretical consideration and evidence from trials, future studies should consider designing handwashing interventions with a strong emphasis on changing outcome expectation from good health to a more valuable outcome benefit around HWWS, as the key normative behaviour change mechanism

Chapter 5 - Exploratory Handwashing Cross-Sectional Study in Abidjan, 2012

We conducted an exploratory cross-sectional study of handwashing practices in Abidjan, between June and August 2012. This was to explore the feasibility of conducting a cluster randomised controlled trial to evaluate an intervention to increase handwashing with soap (HWWS) after using the toilet. The study was conducted in housing compounds in Koumassi commune. We obtained ethical approval from both Côte d'Ivoire's Bioethics Committee (*Comité Consultatif de Bioéthique de Côte d'Ivoire*), and the London School of Hygiene and Tropical Medicine's (LSHTM) *Research Ethics Committee* (Ref. 6182).

1. Study overview and objectives

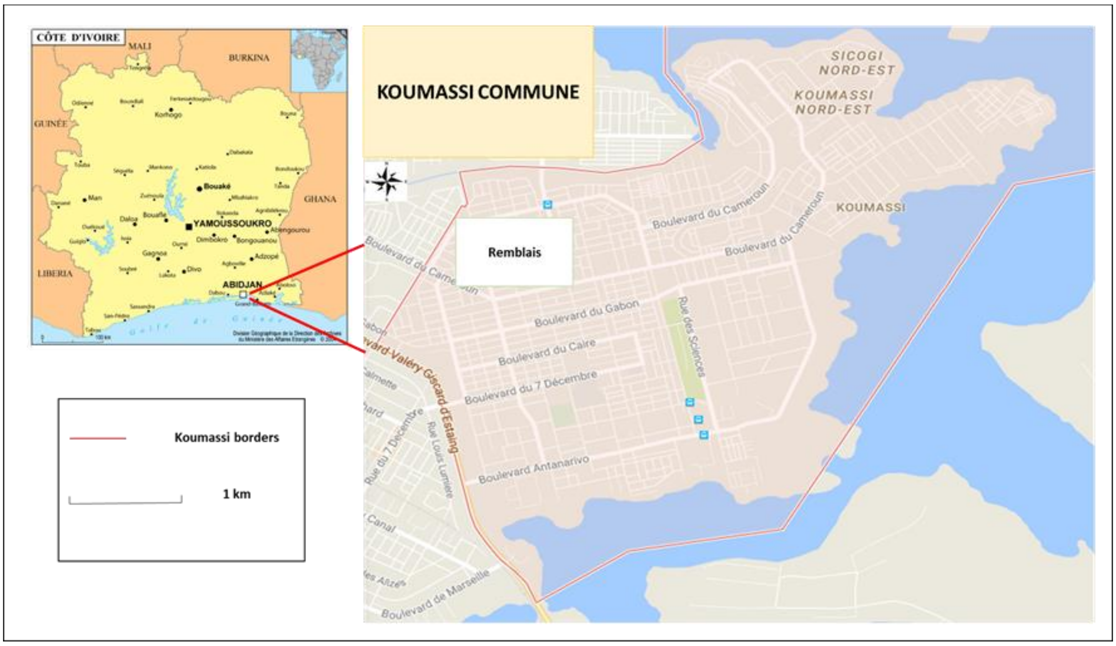


Figure 5.1. Map of Koumassi commune showing the location of the pilot study site

© Google Map and Image

The study was led in collaboration with the pan-African organisation Water and Sanitation for Africa (WSA). We conducted the study in Remblais neighbourhood, in Koumassi (Figure 5.1), as this was a known neighbourhood with compound structures likely to meet the study inclusion criteria. A meeting was held with the Director of Koumassi City Hall's Technical Service to inform them of the research, and collect information on Koumassi that could help with the study.

The objective of the study was to assess the feasibility of conducting a handwashing behaviour-change trial in compounds in Abidjan. The study aimed to:

- Determine whether handwashing practices were observable in compounds in Abidjan;
- Estimate the frequency of HWWS at five key health-related occasions, namely, before eating, before feeding a child, before cooking, after using the toilet, and after cleaning a child's bottom;
- Assess the social desirability of reported HWWS at key occasions, in the study setting;
- Determine the motivational factors involved in HWWS;
- Establish whether handwashing facilities were present in compounds, and if so, what kind of facilities they were, and
- Design and pilot test a compound-level handwashing station (HWS).

2. Justification for the choice of study setting

As mentioned in Chapter 2, Côte d'Ivoire remains among the countries with the highest child mortality rates in the world [8], with diarrhoea being the third leading infectious cause of death in children under five years old [8]. In this country, 51% (2010 Est.) of the population lives in urban areas [165]. Abidjan, the economic capital, contains the vast majority of the country's urban population (39%) [166]. In Abidjan, housing compounds (locally known as 'cours communes') constitute an estimated 53% of occupied living space [167].

Housing compounds, in the urban Côte d'Ivoire context, are an accommodation type made from permanent material, such as cement and iron sheets, built around a courtyard where the majority of daily living activities occur [167-170]. Water and sanitation facilities are usually shared among approximately five to ten or more households. Households are typically rented, and sometimes landlords live within their compounds [168]. While the total surface of a typical housing compound can be 400m^2, the population density is often quite high with as many as 50 people living in a single compound [168-170]. Although households within the same compounds are often from unrelated families, there is a strong sense of community and solidarity among residents, without which living in such a structure would be challenging [168].

Consequently, housing compounds can be seen as micro-communities. In that regard, they present a distinctive opportunity to test HWWS interventions in economically disadvantaged urban communities. The sharing of sanitation facilities with a high number of residents makes housing compounds highly unsanitary accommodation types [167]. With the high density of individuals living in housing compounds, and the high level of intra- and inter-compound interaction, the risk of transmission of diarrheal diseases, not only between residents of the same compounds but also from other compounds, is potentially high. Children under-5 living in compounds are most at risk of diarrheal diseases.

With an area of 874ha [171], Koumassi is located in the South of Abidjan. It has an estimated population of 433,000 inhabitants (2014 est.) [172], making it the third most populated economically disadvantaged commune in Abidjan [173]. Housing compounds constitute 45% of occupied living space in Koumassi [171]. The commune's population is relatively young, with 57% of inhabitants between 15 and 39 years [171]. In the two most recent Cholera epidemics to hit Côte d'Ivoire, in 2011 [174] and 2015 [175], Koumassi was among the communes the most affected in Abidjan.

3. Methods

As part of the study, data collection tools were piloted in six compounds in Koumassi, using the methods described below (see Section 3.2). We did so to ensure that structured observation of handwashing practices was possible in compounds, and assess whether there were any issues with the data collection tools and methods. The compounds were selected using convenience sampling. Informed consent was obtained using the methods described below (see Section 3.1.2.). All questionnaires were administered verbally, and data were recorded by gender and age group.

3.1. Recruitment

We conducted an inventory of all eligible compounds in the study area. The target population was compound inhabitants of Koumassi commune, and the study population, all residents in the study compounds in Remblais. A resident from one of the six pilot compounds volunteered to help delineate the study area, and enrolling compounds. We visited all compounds in the study area, and explained to residents that we were conducting a small research study, and gave them a broad explanation of the study's objectives (see Section 3.1.3). We also sought to speak to landlords, if they were present at the time of the visit. Eligibility of the compounds was assessed both by direct observations, and by asking residents questions regarding the sanitation and water facilities present in their compounds.

3.1.1. Eligibility criteria

- **Inclusion criteria**

Compounds that had structures that were compatible with structured observations were eligible for inclusion in the study. As this was an exploratory study, we did not place a limit on the number of households compounds should have. Nevertheless, compounds should not have been so large that observing key events would have been difficult. Other criteria included the presence of predominantly shared water and sanitation facilities in the compounds, and that said facilities were not located in corridors, or inside households, and thus not observable.

- **Exclusion criteria**

Compounds were excluded if they had structures that were not favourable to structured observations. When possible, we also excluded from structured observations subjects that were identified as non-compound residents (e.g. a person entering the compound, and asking actual residents if they can use their toilet).

3.1.2. Informed consent

We sought *provisional*[12] verbal informed consent from compounds' heads of households and landlords who were present at the time of the visit, to be considered for participation in the study by checking eligibility. Verbal consent was obtained as opposed to written consent, because we were cautious not to burden the study population. Given that Côte d'Ivoire had recently just come out of a Civil War (2010-2011), we anticipated that participants might be concerned that any documents they were asked to sign were liable to misinterpretation of a political nature. This was particularly so as the study took place in disadvantaged communities where education level was relatively low. Our concerns were warranted, as some of the compounds we visited asked us if we were sent by the Government. They also explained that, since the political crisis, residents did not usually accept strangers in their compounds.

The informed consent information sheet was read to household heads, and we answered any questions potential participants had. We emphasised that, at any point in the study, and without having to give any explanation, residents could decide to withdraw their consent. We also asked permission to take pictures of their compounds, for illustration purposes. We emphasised that, if residents appeared in the pictures, their faces would be blurred, so that they could not be identified.

Compounds meeting the inclusion criteria and giving consent were eligible to take part in the study. We asked household heads, who were present at the time of the visit, if they could inform absent residents on our behalf. We also informed them that, if their compound ended up being selected for the study, we would come back, prior to actual data collection. This would be to explain the study to compounds residents again, meet the residents who were absent at the first visit, and obtain confirmation that the compound still agreed to take part in the study. Additionally, residents were asked which days of the week were the ones when the most residents could be found at home. All compounds stated that weekends would be the best days, as these were usually resting days for residents. If a compound inhabitant refused to be part of the study, the entire compound was excluded.

[12] Provisional informed consent was sought, given the actual compounds that took part in the study were then randomly selected from the list of compounds that met the inclusion criteria.

Administrative identifiers[13] and geographic coordinates of each potential study compound were recorded in a spreadsheet. Administrative identifiers were usually located outside compounds, on the wall at the top of compounds' entrance door, or on households' door inside compounds. In cases where administrative identifiers were not visible, a unique identifier was allocated to the compound.

As this was an exploratory study, and due to resource constraints, 30 compounds were randomly selected from the list of eligible compounds, using a simple random number generator. We went back to each of the selected compounds and obtained formal verbal informed consent from all compounds.

3.1.3. Masking

As hygiene in general, and handwashing in particular, are sensitive topics prone to response bias (e.g. [33, 110, 176]) participants were masked to the study objectives. Thus, the study objectives given for informed consent was that this was a doctoral research on how compounds were organised, notably as with respect to water and sanitation issues.

3.2. Procedures

The PhD candidate and one WSA staff acted as field supervisors and supervised data collection.

3.2.1. Recruitment and training of fieldworkers

Fifteen potential fieldworkers, went through a selection process when we explained the study to them, and trained them on the data collection methods and tools, in a classroom. One of the recruitment criteria was that the potential fieldworkers spoke or had an understanding of at least Dioula, if not other local languages. Dioula is the most spoken dialect in Côte d'Ivoire. In order to minimise interviewer bias, no information was given to potential fieldworkers pertaining to the expected frequency of HWWS at key occasions (i.e. high or low frequencies).

[13] In Cote d'Ivoire, compounds administrative identifiers are composed of two sets of numbers, the lot, which is a unique identifier, and the 'islot' (block), which is a number shared by all compounds on the same block.

Initial training was divided into two sessions: a theoretical session in the morning, followed by a test; and a practical training session in the afternoon, which also acted as a test. The theoretical training consisted of explaining the study and data collection to the potential fieldworkers, and going through each data collection tool. The theoretical test assessed whether candidates could correctly fill in the data collection tools, based on hypothetical field situations they were given. The practical training and test took place in housing compounds in Treichville commune[14]. It consisted of 1.5 hours of structured observation and administering the questionnaire to compound residents.

We visited each candidate in their compound, to observe and take notes on how they were collecting data. At the end of the training, we corrected the theoretical and the practical tests, and the seven best candidates were hired. They then went through two weeks of intensive practical training, during which they had to conduct structured observations in Treichville compounds, as well as administer the questionnaires. Fieldworkers were also trained on residents' eligibility criteria and informed consent procedures.

3.2.2. Structured observations

Structured observations were used to record handwashing practices at key occasions in the study compounds, as used in other studies (e.g. [53, 112, 177, 178]). They were also used to record compounds' water, sanitation and handwashing-related information. Each fieldworker was assigned a compound, and was introduced to the residents of the compound. The masked study objective was explained to residents again, and we verified that they were still willing to take part in the study. We also explained again how fieldworkers would collect data through structured observations and interviews (e.g. anticipated amount of time the fieldworker would be in the compound for, type of information that would be collected, but without mentioning the observation of handwashing practices).

Upon entering their allocated compounds, fieldworkers were instructed to locate the toilet and water source(s). The field supervisors, then, placed them in a location where they would have an unobstructed view of both facilities, and on the entire compound. Care was taken to ensure

[14] Compounds were conveniently sampled prior to the practical test, and so that they would meet as much as possible the characteristics of the study compounds in Koumassi. Informed consent was obtained using the same methods as in Koumassi.

the observer's position and presence did not disturb the daily organisation and activities of compounds' inhabitants (Picture 5.1). Each observer was introduced in their compound at least 30 minutes before structured observations started. This was to ensure there would be enough time for the above pre-observations steps to be completed, for observations to start on time. Fieldworkers were given two observation grids on which to record data. One grid recorded data on handwashing practices at key occasions, the other, water, sanitation and handwashing-compounds related information (Appendix 5.1). Examples of data the observation grid for handwashing practices recorded were before or after which occasion(s) HWWS took (or did not take) place, and the gender and age group of the resident performing the action. Fieldworkers were instructed to fill up the second observation grid, during periods of low activity in the compounds, or upon completion of the handwashing practices observation session. Each fieldworker was also given a notebook in which they were instructed to record any issues encountered during data collection (e.g. observed events they were not sure how to classify). These were then discussed during debriefing sessions at the end of each day.

Observations were conducted from 8 a.m. until 12 p.m., and from 2 to 6 p.m. (eight hours of observations per day), on Saturdays and Sundays. Two observation periods during the day were chosen, to assess whether there was a preferable period of the day (i.e. period with more key events) when observations should take place. Additionally, due to the possible variability in handwashing behaviours [36], handwashing practices were observed twice in each compound, on two different days. We allowed an interval of six or eight days between repeat observations in each compound, so that observations would take place once on each of the two weekend days (i.e. one observation on Saturday and one on Sunday, in each compound).

Picture 5.1. A fieldworker conducting structured observations in a compound.

We conducted the structured observations before any other data collection activities, to minimize the chances that residents' handwashing practices were influenced by their knowledge of the questionnaire content.

3.2.3. Compound and household surveys

Eligible residents for the surveys were permanent adult compound residents, defined as residents aged ≥16 years who were not temporary residents (e.g. a resident who is visiting a family member in the compound). We required permanent residents as we anticipated that temporary residents would not have sufficient knowledge of actual residents' practices and habits. Individual consent was given for interview.

All questionnaires were translated into Dioula, and independently back-translated into French by two native Dioula speakers. We then edited the Dioula translation, when the back translation did not match the original meaning of the questions in French. The process continued, until the back translation matched the original questionnaires. Fieldworkers were instructed to conduct surveys in Dioula, when they saw that respondents were more comfortable in this language. Fieldworkers who spoke Dioula received additional training on how to conduct the surveys in Dioula.

Compound characteristics questionnaire

We used a structured questionnaire to collect data on compound characteristics such as number of households, number of water and sanitation facilities, and types of water facilities (Appendix 5.2). Verbal informed consent was obtained, and the questionnaire was administered at the compound level to a resident present in the courtyard at the time of the visit, and willing to take part in the survey. It took approximately 15 minutes to administer the questionnaire. Whilst structured observations were conducted at weekends, questionnaires were administered during any day of the week, usually between 2 p.m. and 6.30 p.m.

Household questionnaire

A unique household identifier was created for each household by combining the compound lot number with a letter (e.g. 201c indicates household c in compound lot number 201). We selected two households, for each of the 30 study compounds, and interviewed an adult member of the household. Households were selected using convenience sampling. Fieldworkers were however instructed not to sample households which were adjacent to each other. We did so in order to try to minimise the risk of information on the survey content and responses being shared among residents from adjacent households. Fieldworkers were also instructed to conduct the survey inside respondents' houses, when feasible. If this was not possible, they were instructed to conduct the survey away from other residents (Picture 5.2). This was done so that they would not hear the respondent's answers, and be influenced by them, or influence the respondent in the way they were answering the questions.

Prior to administering the questionnaire, the study aims were explained again, and fieldworkers sought verbal informed consent. If informed consent was given, fieldworkers asked the respondent to show them which household they resided in, so that a unique identifier could be attributed to them.

We used a structured questionnaire to conduct the household survey (Appendix 5.3). The aim of the questionnaire was to collect information on residents' handwashing practices. To gauge the social desirability around handwashing at key occasions, we asked respondents a single question, which was when they performed HWWS. We deliberately phrased the question as an open question (i.e. without prompting any key occasions), in order to minimise respondent bias. To assess what motivated respondents for HWWS, we asked them why they washed their hands, at the occasion(s) they had cited. If disease was among the motives stated, respondents were asked to name (a) disease(s) they thought handwashing could prevent. To help in the design of the HWS, the questionnaire also asked questions on where respondents' frequent handwashing locations in their compound were, and whether there were designated handwashing facilities at their frequent handwashing location. It, also, asked questions about residents' desire to have handwashing facilities in their compound and why, and the type of handwashing soap they would be willing to share with other residents of their compound and why.

Picture 5.2. A fieldworker administering a questionnaire to a resident outside a study compound.

Additionally, we collected socio-demographic and household characteristics data such as the level of education of the heads of households surveyed, the number of children under five years of age in the respondent's household, and the number of rooms in the surveyed household. The questionnaire also included questions that were not directly related to the study objectives.

These questions were essentially on compounds' water and sanitation facilities. It took approximately 45 minutes to administer the questionnaire.

3.3. Handwashing stations

3.3.1. Handwashing station design

The PhD candidate met the Minister of Health to discuss the research, who then arranged a meeting between the PhD candidate and the Director of the Hygiene Bureau. The Director told the PhD candidate that the Ministry of Health was planning mass-production of HWS to supply schools in rural areas. The project, at that time, was at a small scale, and HWS had been deployed in only a few schools. The Director showed the PhD candidate the design of the HWS they were using. The handwashing facility was made up of an iron stand, which held a 20-litre bucket with a conventional metal tap attached to it. It also had another 20-litre bucket not supported by the stand, but placed under the tap, to collect wastewater from handwashing. Additionally, the stand had a space where soap could be placed. Consequently, instead of designing a new HWS for this research, the PhD candidate decided to adapt the design of the handwashing station already used by the Ministry of Health for compound settings.

The PhD candidate took care to only choose items that were readily accessible to the study population. The trial station was made up of an iron stand, and two buckets. The buckets were bought in a market in Koumassi, and a sample of each was brought to blacksmiths in the commune, to whom the PhD candidate explained the handwashing station design. The stand was designed so that there was space to specifically hold a liquid soap bottle. It also had a space for any other soap type. Additionally, the stand was designed to support two buckets: one 10-litre bucket placed on top of the stand, and with a plastic tap attached to it, and a 5-litre bucket placed under the tap, to collect wastewater from handwashing. The choice of a 10-litre bucket was made to ensure that the water would last long enough in the station, so that replenishing it would not be too burdensome for the study population. The PhD candidate, also, ensured that the wastewater container was not so big in size that it would be too heavy for residents to carry it to dispose of the water. The plastic tap had a mechanism such that it could be operated by either one's hand or one's wrist. This was done for hygiene reasons, out of concern that some residents would not want to use a device touched by dirty hands.

A total of four HWS were manufactured. The blacksmiths constructed stands of two different shapes. Two were square so that the 10-litre bucket fitted loosely in it, and two were round so that the bucket fitted tightly in it (Pictures 5.3 and 5.4). Appendix 5.4 details the HWS pilot methods.

Pictures 5.3 and 5.4. Handwashing stations with a round-shape and square-shape respectively

3.3.3. Focus groups

Eligible residents for the focus groups were permanent adult compound residents. Convenience sampling was used to select a minimum of five compound residents in the eight pilot compounds, taking care to sample both men and women. Verbal informed consent was obtained. We told participants that we would be taking notes of the discussions, and that ideally, we would like to record the sessions, if they were comfortable with us doing so.

In order to moderate the discussion around the research questions, topic guides were used. Questions from the guide were around participants overall experience with the HWS; what they liked and what they disliked; whether they had any issues with the facility, and how the station's design could be improved (Appendix 5.5). As participants were not comfortable with us recording the discussions, we took notes. The focus groups usually lasted approximatively 45 minutes.

3.4. Quality assurance and control

The PhD candidate and the WSA staff visited each fieldworker, during data collection, including during training sessions, without informing them in advance. This was done to ensure that fieldworkers were in their compound at the expected times, and collecting data as intended. For structured observations, we observed how fieldworkers were collecting data, and whether they were missing any relevant events that were taking place whilst we were there. For the interviews, during the training, we monitored four interviews per fieldworker (two interviews for each of the survey tools). We took notes of issues we identified in the way the questionnaires were administered, and discussed them in debriefing sessions. For the subsequent monitoring sessions during training, we monitored parts of the surveys for each fieldworker, to assess improvements in questionnaire administration. We continued monitoring part of each fieldworker's questionnaires, during the study.

Each data collection session for both the training and the actual study were immediately followed by a debriefing session with all the fieldworkers present. We examined the data collected by each fieldworker. If there were mistakes, we pointed them out to fieldworkers, and explained to them why these were mistakes, and how to correct them. For the questionnaires, when there were missing data, fieldworkers were asked to revisit respondents as soon as possible (i.e. on the same day, if it was not too late; or the next day) to collect the missing information, unless the respondent had refused to answer the question. For the structured observations, when there were missing data, fieldworkers were usually able to remember the event, and thus provide the missing information. Debriefing sessions were also an opportunity for fieldworkers to discuss any issues they had encountered during data collection, and noted in their notebooks. We double checked all data collected. Corrections were made directly on the data collection tools, and initialled.

3.5. Data management

3.5.1. Confidentiality

All hard copies of the data collected were kept in a locked cabinet, at the PhD candidate's residence in Abidjan. I was the only person who had access to the cabinet. I, then, travelled back to London with all the collected data for data entry. I double entered data on my personal, password-protected drive, using EpiInfo 7. At the end of each data entry day, the hard copies were stored in my locker at LSHTM, to which only I had access. All forms were anonymous, as respondents' names were not recorded.

3.5.2. Data cleaning

Both Excel and STATA® 12 were used to clean and code the data. Data checks were performed to look for coding errors and any inconsistencies that may have been missed when performing initial data quality assurance checks (e.g. check that the presence of a child under five year was not recorded, whilst also recording that there was no child in the surveyed household). We also checked that all recorded data were within expected ranges.

4. Data analyses

4.1. Analysis of quantitative data

Quantitative data were analysed using the statistical package STATA® 12. All statistical analyses took into account the cluster sampling design, as per Hayes and Moulton (2009) recommendation [92], by using the *svy* command in STATA®, with compound as the cluster unit. The primary outcome measures were the observed proportions of occasions relating to key events on which hands were washed with soap. As each dataset had less than 10% of missing data, we performed complete case analysis.

We computed descriptive statistics for all variables, to examine their distributions. To estimate the primary outcome measures, we calculated the total number of occasions on which hands were washed with soap, divided by the total number of events observed. 95% confidence intervals (CI) were computed for each proportion. The association between handwashing and HWWS, and the independent variables gender and age group was assessed using logistic regression, and expressed as odd ratios (ORs) with 95% confidence intervals (CI). Wald tests were used to obtain p-values.

4.2. Analysis of qualitative data

Data were transcribed using Microsoft Word, at the end of each data collection day. Data were analysed, using content analysis, and while data collection was on-going. Preliminary codes emerged from reading the first few transcripts. These were then used as coding schemes to code the remaining transcripts [179, 180]. From the codes, categories that were internally homogeneous and externally heterogeneous were created [180]. These were used to organise the coded data, with similar concepts grouped together and counted [179-182]. When needed, new codes and categories were created, when the data did not fit into the existing ones [179]. Data within each coding category were then examined, to assess whether the data could be further classified [179, 180]. Data were also trimmed, as not all information was meaningful and relevant to the research question (e.g. respondents talking about non handwashing-related issues) [183-185].

5. Results

We visited 452 compounds in Remblais neighbourhood. 198 met our inclusion criteria, from which we randomly selected 30 compounds. There was no refusal. Three compounds out of 30 had missing data on water and sanitation facilities, and on the number of households in the compounds, as residents refused to provide us with this information. One compound had missing data on shared water facilities, for the same reason. Three residents refused to take part in the household survey. Additionally, we failed to find eligible residents in six sampled households, from two study compounds. In the latter case, the present residents explained to us that these were households inhabited by single male residents. This meant that they were usually not back in their households before late at night (e.g. between 9 p.m. and 12 a.m.).

For the structured observations for the primary outcome, 12 observations had partially incomplete data (i.e. 10 missing data on the primary outcome, and two on the age and gender of the individuals observed). Two households had missing data on one item each.

5.1. Compounds and households' characteristics

The study compounds had a median of 10 households per compound (interquartile range (*IQR)*=5-16) (Table 5.1), with a median of two rooms per household (*IQR*=1-4), a median of five persons per household (*IQR*=1-12), and a median of 0.5 child under five years per household (*IQR*=0-4) (Table 5.2). Some of the compounds had a few households with screens erected in front of their doors. These were walls or barriers that would either still allow residents to have an unobstructed view on activities taking place in the communal courtyard, or partially or totally block their views (Pictures 5.5 and 5.6). This was done to reduce communality with other households. Households with screens would perform some of the activities that usually took place in the communal courtyards (e.g. cooking), in the created privatised area.

Picture 5.5. Household with grids completely isolating it from other households.

Picture 5.6. Household with a small wall creating a privatised space without isolating it from other households.

Primary school was the highest education level of heads of households for 23 (38.9%) of 60 surveyed households, while 17 (28.8%) heads of households did not have any education (Table 5.2). 46 (76.7%) households were Muslims, while the remaining were Christians. One of the sampled compounds was a family compound, with the majority of households belonging to the same family. In such instance, the family was from the compound landlord's family, and the remaining households were tenants. In such compound, it was difficult to survey more than one resident for the survey. This was because residents would say that the responses given by any of their surveyed family member, would be the same as the ones they would give, and therefore there was no need to survey them.

All study compounds had access to latrines (Table 5.1). These were shared permanent simple pit latrines with slabs, concrete lining, superstructures and doors. The facilities could be emptied periodically. There was a median of two latrines per compound (*IQR* = 1-3). The shared nature of the latrines makes them 'unimproved' facilities, under the definition of the World Health Organization (WHO) and UNICEF Joint Monitoring Program (JMP) [186].

Table 5.1. Compound characteristics

Characteristic	**Compound (N=27*)**
Number of households per compound*	1 (3.7)
5	2 (7.4)
7	5 (18.5)
8	4 (14.8)
9	9 (33.3)
10	3 (11.1)
11	2 (7.4)
12	1 (3.7)
16	
Number of latrines*	
1	7 (25.9)
2	15 (55.6)
3	5 (18.5)
Presence of a shared water standpipe in the compound* Yes	26 (96.3)
Compounds where some households had individual standpipes *	5 (19.2)

*Missing data for 3 observations

26 (96%) compounds had a water source (i.e. standpipes) within the compounds (Table 5.1). Standpipes are considered improved water facilities, under JMP definition [186]. The water facilities were shared among residents of the same compound. In 6 (19%) compounds, there was a mixture of shared and private standpipes, with some households having private water facilities in the courtyard. Residents, in the compound which did not have a water source, collected water from a neighbouring compound. There were no formal handwashing facilities (e.g. sinks, HWS) in any of the compounds.

Table 5.2.Households characteristics

Characteristics	Households (N=60)
Number of rooms per household*	
1	25 (42.4)
2	30 (50.9)
3	3 (5.1)
4	1 (1.7)
Number of inhabitants per household	
1	5 (8.3)
2	7 (11.7)
3	7 (11.7)
4	9 (15.0)
5	8 (13.3)
6	11 (18.3)
7	7 (11.7)
9	1 (1.7)
10	3 (5.0)
11	1 (1.7)
12	1 (1.7)
Children aged under 5 years per household	
0	30 (50)
1	21 (35)
2	6 (10)
3	2 (3)
4	1 (2)
Highest education level of head of household *	
None	17 (28.8)
Primary	23 (38.9)
Secondary	16 (27.1)
Higher education	3 (5.1)
Religion	
Christian	14 (23.3)
Muslim	46 (76.7)

Data are n(%) unless otherwise specified.
*Missing data for 1 compound

5.2. Observed and self-reported handwashing practices

5.2.1. Observed handwashing practices

We observed a total of 1,616 key occasions for handwashing (Table 5.4). Among these, 1,147 (71%) related to an adult of which 903 (79%) related to women (Table 5.3). 721 (45%) events were observed between 8 AM and 12 PM, and 895 (55%) between 2 PM and 6 PM. Overall, residents washed hands on 319 out of 1606 key occasions observed (20%; 95% CI:16-24). HWWS occurred on only 31 occasions (2%; 95% CI:1-3) of these events (Table 5.4). Moreover, residents were observed HWWS after using the toilet and after cleaning a child's bottom respectively on only 20 (3%; 95% CI:2-6) and 3 (4%, 95% CI: 1-12) occasions respectively. HWWS before eating, before feeding a child, and before cooking was even less frequent (1%, 1% and 1%, 95% CI: 0-2; 0-11; 0-2 respectively). By contrast, handwashing with water only at key occasions was more frequent. For instance, residents were observed washing their hands with water for 166 (27%, 95% CI: 24-31) events observed after using the toilet, 26 (33%, 95% CI:21-34) after cleaning a child's bottom, and 79 (17%, 95% CI:12-24) before eating.

Table 5.3. Age and sex distribution of compounds residents observed at key occasions

Characteristics	Male n (%)	Female n (%)	Total n (%)
Age groups (years)			
Adults (16>)	244 (21.3)	903 (78.7)	1,147 (71.1)
Child 5 to 15	113 (36.5)	197 (63.6)	310 (19.2)
Child under 5	58 (36.9)	99 (63.1)	157 (9.7)
Total	415 (25.7)	1, 199 (74.3)	1, 614*

*Missing data for 2 observations

Table 5.4. Handwashing practices at key occasions observed by structured observations

		Hands washed		Hands washed with soap	
Occasions	Observations	n (%)	95% CI	n (%)	95% CI
Before eating	453	79 (17.4)	12.2-24.2	3 (0.7)	0.0-1.9
Before feeding a child	66	6 (9.1)	4.6-17.1	1 (1.5)	0.2-10.9
Before cooking	394	42 (10.7)	6.2-17.6	4 (1.0)	0.4-2.4
After using the toilet	614	166 (27.0)	21.0-34.1	20 (3.3)	1.7-6.0
After cleaning a child's bottom	79	26 (32.9)	22.8-44.9	3 (3.8)	1.1-12.1
Total	1, 606*	319 (19.9)	16.5-23.8	31 (1.9)	1.1-3.3

*Missing data for 10 observations

Women were less likely to wash their hands in general (OR: 0.75, 95% CI: 0.54-1.05), and to HWWS (OR: 0.72, 95% CI: 0.35-1.47) at key occasions compared to men (Table 5.5). However, we found weak evidence (*P*=0.09) and no evidence (*P*=0.36) of a difference respectively. By contrast, we found evidence that adults had 1.65 times the odds of washing their hands in general at key occasions compared to children (95% CI: 1.13-2.41) (Table 5.6). On the other hand, there was weak evidence of a large difference between adults and children when it came to HWWS (OR: 2.81, 95% CI: 0.76-10.42). However, the upper limit of the confidence interval is compatible with a substantial difference. Adults tended to report that poor handwashing practices were more an issue in children than in adults. For instance, Mrs D explained:

> *"We [adults] know that we have to wash our hands with soap…It is more children who don't know and don't do it! We keep on reminding them to wash their hands when they come [home] from outside. But when they come [home], they go straight to eating!" (Compound resident)*

Table 5.5. Odd Ratios of the association between handwashing and gender and handwashing with soap and gender

		Handwashing			Handwashing with soap		
Variables	Observations	N=318**** n (%)	OR (95% CI)	p-value	N=31 n (%)	OR (95%CI)	p-value
Before eating	451*						
Male	140	20 (14.3)			1 (0.7)		
Female	311	58 (18.7)			2 (0.6)		
Before feeding a	66						
child	2	0 (0.0)			0 (0.0)		
Male	64	6 (9.4)			1 (1.6)		
Female							
Before cooking	394**						
Male	7	1 (14.3)			0 (0.0)		
Female	387	41 (10.6)			4 (1.0)		
After using the	614***						
toilet	259	75 (29.0)			9 (3.5)		
Male	355	91 (25.6)			11 (3.1)		
Female							
After cleaning a	79						
child's							
bottom	4	0 (0.0)			0 (0.0)		
Male	75	26 (34.7)			3 (4.0)		
Female							
Total	1,604						
Male	412	96 (23.3)	Base		10 (2.4)	Base	
Female	1,192	222 (18.6)	0.75 (0.54-1.05)	0.09	21 (1.8)	0.72 (0.35-1.47)	0.36

* Missing data for 2 observations
** Missing data for 4 observations
***Missing data for 6 observations
**** Missing data for 1 observation

Table 5.6. Odd Ratios of the association between handwashing and age group and handwashing with soap and age group

		Handwashing			Handwashing with soap		
Variables	Observations	N=318**** n (%)	OR (95% CI)	p-value	N=31 n (%)	OR (95%CI)	p-value
Before eating	451*						
Child	319	48 (15.1)			1 (0.3)		
Adult	132	30 (22.7)			2 (1.5)		
Before feeding a child	66						
Child	11	2 (18.2)			1 (9.1)		
Adult	55	4 (7.3)			0 (0.0)		
Before cooking	394**						
Child	25	0 (0.0)			0 (0.0)		
Adult	369	42 (11.4)			4 (1.1)		
After using the toilet	614***						
Child	98	17 (17.4)			2 (2.0)		
Adult	516	149 (28.9)			18 (3.5)		
After cleaning a child's bottom	79						
Child	13	1 (7.7)			0 (0.0)		
Adult	66	25 (37.9)			3 (4.6)		
Total	1,604						
Child	466	68 (14.6)	1.0		4 (0.9)	1.0	
Adult	1,138	250 (22.0)	1.65 (1.13-2.41)	0.01	27 (2.4)	2.81 (0.76-10.42)	0.12

Missing data for 2 observations
** Missing data for 4 observations
***Missing data for 6 observations
****Missing data for 1 observation

5.2.2. Self-reported handwashing practices

We surveyed 60 household residents. 44 (73%) respondents were women, and 43 (72%) respondents were between 16 and 34 years. In contrast, when asked when they washed their hands with soap in general, 36 (60%, 95% CI: 46-72) residents reported washing their hands with soap after using the toilet, 18 (30 %, 95% CI: 20-43) before cooking, and 17 (28%, 95% CI: 18-41) before eating (Table 5.7). Only washing hands with soap before feeding a child and after cleaning a child's bottom had reported figures that were close to the observed figures (both 5%, 95% CI:1-15). Residents seemed to be aware of the low occurrence of HWWS in their compounds.

For instance, Mr A. stated that:

> *"If you spend some time here, you will see that people do not use soap."*
> *(Compound resident)*

Table 5.7. Respondents' self-reported occasions when they wash their hands with soap

	Reported handwashing with soap occasions	
Occasions	Respondents* N=60 n (%)	95%CI
Before eating	17 (28)	18, 41
Before feeding a child	3 (5)	1, 15
Before cooking	18 (30)	20, 43
After using the toilet	36 (60)	46, 72
After cleaning a child's bottom	3 (5)	1, 15
After coming back from an outing	6 (10)	4, 20
After performing a soiling activity	5 (8)	3, 19

*Several answers per respondent were possible

When asked what generally motivated them to wash their hands, 41 (68%, 95% CI: 55-79) respondents stated that avoiding disease was their primary incentive, whilst 22 (37%, 95% CI: 25-50) stated that removing dirt on their hands (e.g. charcoal, grease) was their main motive (multiple answers possible). Among the respondents who stated disease as their key motive, 28 (68%, 95% CI: 52-81) were capable of naming at least one handwashing-related disease. 23 (56%, 95% CI: 40-71) respondents cited cholera as the disease handwashing could prevent. Only one respondent named a disease which was not related to handwashing (malaria), in addition to citing cholera.

Residents were observed using both plastic kettles (locally known as 'seridaca'), and water in buckets to wash their hands. For the former, residents would pour water on their hands using the kettle (Picture 5.7). When using the bucket, residents would either dip their hands in the bucket (Picture 5.8), pour water on their hands tilting the bucket, or use a smaller vessel to collect water from the bucket to pour it on their hands. In the instances where soap was used for handwashing, residents were observed using soap which was located in the laundry and dish washing area. On some occasions, they were observed using soap that was stored either next to their household's door or in their household (Picture 5.9).

We observed 13 (45%, 95% CI: 27-64) compounds with a commonly used handwashing location. In six compounds, the laundry and dishwashing areas were used, and in another six the area was near wastewater facilities (e.g. drain connected to a tank, drain connected to a gutter outside the compound). Structured observations showed that the laundry and dishwashing areas in compounds were areas with uncovered buckets containing water and soapy-water. For the remaining compound, the commonly used handwashing area was next to the toilet. These figures are consistent with respondents' answers. 21 (35%, 95% CI: 23-49) respondents reported having a regularly used handwashing location in their compounds. Among those respondents, seven named the compound entrance, seven the dishwashing and laundry area, three, the area next to the toilet, two the middle of the courtyard, and two near the compound's wastewater drainage system as their commonly used handwashing areas. These five areas broadly correspond to the above two observed areas, namely, the location of wastewater draining systems in the compound (i.e. at the compound entrance, next to the toilet), and the location of water and soapy water filled containers in the compound (i.e. dishwashing and laundry compound areas, the middle of the courtyard).

Picture 5.7. Handwashing using a kettle ('seridaca') on top of a wastewater drainage system.

Picture 5.8. Handwashing by dipping hands in a water-filled container in the middle of a compound

Picture 5.9: Soap bars stored at the window of a household.

5.3. Handwashing stations

5.3.1. Demand for handwashing facilities

Respondents were asked whether they would like to have a handwashing facility in their compound and why. Only 1 (2%, 95% CI: 0-12) respondent did not see the relevance of such facility. Mr X stated that:

> *"[Having a handwashing facility] would be useless, because there is already a water pump [in the compound]."*

39 (65%; 95% CI:53-75) respondents justified the need for a handwashing facility for hygiene-related reasons, eight (13%, 95% CI:7-24) for practical reasons, and five (8%, 95% CI:3-19) for both hygiene and practical reasons.

For example, Ms Y explained that:

> *"With a handwashing facility, wetting and dirtying the floor during handwashing would be prevented […]. The compound would be kept clean."*

Ms Z explained that:

> *"[With a handwashing facility], at least everything is already here, when you come out of the toilet…water….soap [are here]."*

The remaining six respondents considered having a handwashing station desirable for aesthetic, novelty or social status reasons.

We also asked respondents what type of soap they would be willing to share among themselves for handwashing. 24 (40%, 95% CI: 27-55) respondents chose soap bars, justifying it by the fact it was cost-effective, as it did not end fast. 18 (30%, 95% CI: 19-44) respondents cited soap powder and 14 (23%, 14-35) liquid soap, as they viewed both types as more hygienic. Ms X explained:

> *"[Powder and liquid soap] because it is hygienic…You just pour it on your hands, as opposed to everybody putting their hands on the soap itself."*

Two (3%, 95% CI: 1-13) respondents were indifferent to the type of soap shared, and another two (3%, 95% CI: 1-13) did not want to share soap with other residents, on the grounds that each household should have their own soap.

5.3.2. Handwashing stations pilot

We pilot tested the HWS in eight compounds (four in Koumassi and four in Treichville) (Pictures 5.10, 5.11 and 5.12). Data were collected through focus groups in each compound. Overall, respondents appreciated the design of the HWS, with some respondents commenting that it made their compounds look nice. In both pilot sites, respondents perceived both advantages and disadvantages of having the handwashing station. In general, respondents reported that the facilities had positively changed handwashing practices in their compounds, and made handwashing easier, as illustrated by the quotes below:

> *"Since it arrived, everybody washes their hands […]. When the water went down, we filled it up […]." (Ms. C, Koumassi compound resident)*

> *"Everybody loved it! Even the little kids used it! They would go out to buy food, and would come back and wash their hands." (Ms. X, Koumassi compound resident)*

> *"When, we come out of the toilet, we see water and soap, it is a good thing […]. Before, we would have to look for soap, if we wanted to use soap." (Mr Z, Treichville compound resident)*

Some respondents did not use the facility for lack of time or lack of interest.
Mr B stated:

> *"I am used to using something else [to wash my hands], so I used [the HWS], when I remembered it was there." (Koumassi resident)*

One compound lacked water and soap at the HWS, due to maintenance issues. The compound's owner who resided in the compound was an elderly woman who explained that:

> *"While all the children kept on filling up [the HWS] and using it, they did not care [about wastewater disposal]! I was the one who had to go back and forth to throw the water away, and it made me tired! So, in the end, I just wanted [the HWS] out of here!" (Mrs N, Koumassi resident)*

Picture 5.10: Residents testing the HWS in front of a fieldworker on the day the station was brought to their compound

Picture 5.11: Residents testing the HWS in front of two fieldworkers on the day the HWS was brought to their compound

Picture 5.12: A child on their way to dispose of handwashing wastewater in a gutter outside their compound.

This was the only compound which reported maintenance issues. Nevertheless, HWS maintenance concerns were raised by one resident, in one of the pilot compounds. Upon supply of the handwashing station, Mr C asked:

"We noticed that you had provided the station with soap…but I was wondering who would be in charge of replacing the soap, when this bottle will be finished?"
(Koumassi resident)

With regards to improving the station design, one participant suggested that towels be supplied with the stations, to dry hands. Another resident recommended that the station be directly connected to a wastewater drainage system, so that residents would not have to empty the wastewater container.

We observed that the HWS stand with the round design was better suited to the compound setting than the square one, because it took less space in compounds. However, it was noted that, on the round stand, both the spaces to hold the 10-litre bucket and the one to hold the liquid soap bottle were too tight. This made it difficult to remove containers.

During informal observations of the use of the facilities, we noticed that participants would not systematically use soap. This was also mentioned by compound residents during focus group discussions:

"Not all [users] used the soap. They just come, rinse their hands [with water only], and go." (Ms. Y, Koumassi compound resident)

Discussion

We observed a very low frequency of handwashing in general at five key handwashing occasions in this low-income housing compound setting, in Abidjan, Côte d'Ivoire. Handwashing with soap at key occasions was close to zero in this setting. These findings are in accordance with other studies measuring the frequency of HWWS at key occasions, in comparable low-income settings in developing countries [48, 89]. They are also consistent with UNICEF (2008) findings of a HWWS frequency of 4% after using the toilet, after cleaning a child's bottom, and before cooking, in Côte d'Ivoire [187]. Adults were more likely to wash their hands in general than children, in this setting. It takes time for children to acquire habits – though one might expect them to acquire habits which are emphasised more quickly than those which are not. Nevertheless, adults' observed HWWS practices after the key occasions were as poor as that of children.

We observed over-reporting of handwashing practices, and poor agreement between structured observations and self-reported handwashing practices. This is consistent with other study findings (e.g. [33, 78, 102, 110]). This may indicate a strong perceived injunctive norm around handwashing, in this setting, leading to social desirability bias [110, 176]. Handwashing with soap before eating, before cooking, and after using the toilet were the occasions for which HWWS was most often reported. This may be because handwashing at these moments are culturally ingrained. For instance, whilst there are no handwashing facilities in food establishments in economically disadvantaged areas in Abidjan, it is customary to be offered a bucket of water and soap for handwashing, before eating (Picture 13). This suggests there is a strong perceived injunctive norm around food-related occasions. This is in contrast with the findings from Langford et al. (2013)'s study [102], reported in Chapter 3, where the low self-report of HWWS practices before cooking, and before eating suggested a weak injunctive norm around these occasions. Regarding HWWS before feeding a child and after cleaning a child's bottom, the frequencies were comparable to the low observed ones. This may indicate a weak injunctive norm.

Whilst the perceived injunctive norms around HWWS before eating, before cooking, and after using the toilet appear strong in this population, the fact that residents seemed to be aware that other residents in their compounds did not wash their hands with soap suggests an accurate weak perceived descriptive norm around HWWS at the five key occasions.

Picture 13: A kettle and small bucket to wash hands in a restaurant in Koumassi

Interviewees most commonly cited disease avoidance as the reason for washing their hands. This may reflect the fact that HWWS is mostly promoted for the prevention of diseases in this setting. For instance, among the 1,261 cases of cholera reported in Côte d'Ivoire in 2011 [188], 10 were confirmed cases in Koumassi [174]. This prompted UNICEF to provide soap kits for handwashing in this community [174]. Additionally, the Director of Koumassi City Hall's Technical Service explained that, during the cholera outbreak, community-level hygiene promotion, including HWWS, took place. The fact that cholera was the disease respondents cited most commonly may indicate that they still remembered the information promulgated during the cholera prevention campaign efforts.

However, despite health being the most commonly reported motive for washing hands with soap in this setting, the low frequency of the practice may be an illustration that health is not actually an important handwashing driver. The awareness of handwashing as a disease-prevention practice, and the diseases it can prevent, in conjunction with the low HWWS frequency observed is consistent with previous study findings reporting that health may not be an effective handwashing motivator (e.g. [48, 52, 189]). From a theory of normative social behaviour (TNSB) standpoint, the perceived health outcome benefit associated with HWWS does not seem to be effective at motivating the practice. Given the non-practice of HWWS does not always produce illness, and when it does, the onset does not immediately follow the absence of HWWS, it is difficult for individuals to make the association between risk behaviour and adverse outcome [55]. Some respondents also cited the removal of dirty substances from one's hands as a reason for handwashing. Removal of "dirt" has an immediately obvious benefit, compared to disease prevention. Disgust may thus be an effective handwashing motivator, as suggested by Curtis & Biran (2001), the World Bank (2007), and Curtis et al. (2009) [48, 54, 58], and according to studies findings (e.g. [57, 59, 65]).

Out of the 452 compounds initially visited in Remblais, only one had a handwashing facility (i.e. sink) in the communal courtyard. In the absence of dedicated handwashing facilities, residents were observed washing hands in various areas in the compound. Thus, neither the areas where handwashing took place in the compound nor the soap used for handwashing were near the toilet. Having to fetch soap from a location that was not necessarily located where handwashing would take place also seemed impractical. Overall, the lack of handwashing facilities may have made handwashing inconvenient in this setting, and therefore discouraged engaging in the practice.

Given the absence of dedicated handwashing facilities in this setting, handwashing practices may not be readily noticeable. The absence of facilities could be indicative of weak collective descriptive and injunctive norms around handwashing, in this setting and in Côte d'Ivoire. For instance, in Abidjan and Grand-Bassam cities, it is common to encounter hospitals, schools and public toilet without any handwashing facilities. For establishments with such facilities, it is frequent for them not to have any soap. This may indicate the low importance attached to handwashing practices in this setting.

The results of the handwashing station pilot indicated that there was a high level of acceptability. However, maintaining the facilities would be the potential issue. Given its design, the station would have to be regularly replenished with water and soap, and wastewater from handwashing disposed of, when the container is full. If compounds residents do not see the handwashing station as a valuable good, and if handwashing remains insignificant, there will be no common ownership of the facility, and thus no maintenance. Holding discussions with compound residents around strategies for the stations' maintenance, and setting clearly defined roles would be crucial.

Thus, delivering a handwashing promotion intervention, in conjunction with handwashing station supply, may increase the use of the facility, and HWWS at key occasions. On the other hand, and from a normative standpoint, the hypothesis that handwashing stations may contribute to making handwashing in compounds more visible, and practices easier to scrutinise seems to be supported by the pilot findings. This is illustrated by the fact some inhabitants were able to notice that some residents in their compounds just used water at the facility but not soap.

The large percentage of women observed and surveyed compared to men can be explained by the fact that women tend to be the residents who spend the most time in compounds. Indeed, they tend to be in charge of compounds and household-related activities, such as cooking, cleaning, taking care of children, whilst the men tend to be the main breadwinners, and thus out at work. The men were more often seen in compounds from the late afternoon. This would have implications in terms of the targeted audience in the future design of an intervention to increase HWWS practices in such settings.

We attempted to minimise errors and missing data by training fieldworkers carefully, holding debriefing sessions at the end of each data collection day, and sending fieldworkers back to their respondents, if data had been incompletely collected. Very few data were missing for the questionnaires. By contrast, there was no other way of rectifying missing data from structured observations but by asking fieldworkers to remember the person they had observed for which some data was missing. We were unable to collect some information on compound characteristics (e.g. water and sanitation facilities) in three of the study compounds. This was due to the fact that residents believed that we were working for Côte d'Ivoire's water distribution company, SODECI[15], and Côte d'Ivoire's electricity company, CIE[16]. This was the case, despite our attempts to explain to them repeatedly that this was a doctoral research study, and

[15]SODECI stands for Société de distribution d'eau de la Côte d'Ivoire
[16]CIE stands for Compagnie ivoirienne d'électricité

remind them of its broad aim. However, the large amount of questions on water facilities may have contributed to residents being sceptical about the motive of our presence in their compounds. Care should be taken so that masking questions in the future cluster trial be on themes that put the residents at ease, and are not on subjects that may be judged as sensitive.

The three respondents who refused to take part in the study were Hausa women from Niger. Compound residents explained to us that these women's husbands (or male heads of households) usually prevented them from speaking to strangers. Because inter-ethnic conflict and xenophobia were among the causes of the 10-year political crisis Côte d'Ivoire, foreign nationals, especially from countries to the north of Côte d'Ivoire, had become used to being persecuted. There was also a language barrier, as the Hausa women expressed that they understood neither French nor Dioula. This may have been a strategy to dissuade us from asking them questions. Indeed, even when we would find someone in the compounds who spoke the same language as them, and who were willing to act as intermediary, the women still refused to take part in the survey.

One of the study limitations was the risk of reactivity (Hawthorne effect) linked to the use of structured observations [39]. Individuals under observation may modify their behaviour (e.g. increase usually low handwashing practices) as a reaction to the presence of observers, thereby introducing bias [39]. While reactivity is a major concern in any study using structured observations, the majority of the limited studies that have attempted to assess its impact found that reactivity had less impact than might be expected [33, 34, 36, 38]. For instance, in a malaria prevention study in Peru where reactivity was systematically measured, of the 339 events observers perceived as potentially reactive, only two were identified to be related to the study objectives [38]. Additionally, Curtis et al. (1993) conducted a hygiene behaviour study in Burkina-Faso where reactivity was assessed. The authors reported that, while for certain behaviours there were some evidence of reactivity, it diminished with time [33]. To minimise any Hawthorne effect, we masked the study participants to the study aims. The very low handwashing frequency observed in the study suggests that any reactivity was minimal.

A third study limitation was that we left the handwashing stations in compounds for approximately one week. This may have been insufficient to truly assess the potential advantages and disadvantages of such facilities. For instance, seven days may have been too short to assess the extent of maintenance issues, and the durability of the stations.

Conclusion

Handwashing with soap at key occasions is very infrequent in low-income compound settings in Abidjan, Côte d'Ivoire. The results of this exploratory study support the value of designing and evaluating an intervention to increase HWWS in this setting. It is also key to measure norms around HWWS after using the toilet, in this setting. This will enable to know which norm(s) the intervention should aim to increase to positively influence HWWS practices.

Chapter 6 - Development and Psychometric Testing of Scales for Four Handwashing Norms-Related Constructs, in Abidjan, 2014

In preparation for a trial to evaluate an intervention to increase handwashing with soap (HWWS) practices after using the toilet, designed using the Theory of Normative Social Behaviour (TNSB), we conducted a study to test the psychometric properties of four scales for handwashing norms-related constructs developed for the purposes of the trial. The study was conducted in housing compounds in Koumassi commune, in January 2014. Ethical approval was obtained from both Côte d'Ivoire's Bioethics Committee (*Comité Consultatif de Bioéthique de Côte d'Ivoire*), and the London School of Hygiene and Tropical Medicine's (LSHTM) *Research Ethics Committee* (Ref. 7029).

1. Development of four handwashing norms scales

The norms-related scales were developed based on previous work [144, 190, 191]. The items were modified or created specifically to be relevant to the study context, in accordance with good practice in scale development [192-198]. One scale aimed to measure the *perceived descriptive norm* (HWDN) around HWWS after using the toilet. This represents the individual's perception of the level of fellow compound residents' HWWS practices (e.g. low, average, high) after toilet use. A second scale aimed to measure the *perceived injunctive norm* (HWIN) around HWWS after using the toilet. This construct represents how much importance an individual perceives others in their compound give to the practice of HWWS after using the toilet. The third scale aimed to measure the *perceived publicness* of HWWS after using the toilet *(HWP)*. This represents individuals' perceptions of whether HWWS practices in their compound are visible to others. The fourth scale aimed to measure the *perceived outcome expectation* (HWOE) around HWWS after using the toilet. This represents the benefits (or disadvantages) individuals perceive they would derive from HWWS after using the toilet.

Initially, a total of 20 handwashing norms-related items were developed. The HWDN, HWIN, HWP and HWOE scales initially had five, four, five and six items respectively. Content experts and members of the study team assessed the face validity of the items, by examining the relationship between item content and conceptual definitions of each study construct. To minimise the impact of social desirability bias attached to handwashing [32, 33], 14 out of the 20 items were negatively framed (e.g. 'In your compound, *few people* wash their hands with soap after using the toilet'), but each scale included at least one positive item (e.g. for the HWDN, *'Most residents* in your compound wash their hands with soap after using the toilet*')*.

2. Study objectives

The main objective of the study was to test the psychometric properties of the four scales designed to measure social norms-related constructs around HWWS after visiting the toilet. A secondary objective was to test the ability of the questionnaire, within which the scales were nested, to mask participants and fieldworkers to the future trial's handwashing theme.

3. Methods

Two fieldworkers from the 2012 cross-sectional pilot study, reported in Chapter 5, were hired to assist with data collection and trained in all study procedures. The data collection tools were administered verbally in French, or, when requested by participants, in a local dialect, usually Dioula. All data were collected anonymously.

3.1. Masking

Both participants and fieldwork assistants were masked to the study objectives. They were told that the study aimed to understand how housing compounds were organised, particularly with respect to gender roles and social cohesion among residents. There was no mention of handwashing. This masking theme was chosen as the findings from the 2012 cross-sectional study had shown that it was important to avoid themes which were likely to be deemed sensitive by compound residents (e.g. water consumption). It was also important to find a masking theme which would be of interest to participants, and tackle issues that were relevant to the study population. This would increase the likelihood that attention was taken away from handwashing. The specific theme of gender roles in compounds was relevant, given a new law establishing women as co-heads of households, along with men, had been passed in 2012. The law was still the subject of debate in society, especially within economically disadvantaged communities.

3.2. Sampling

We used convenience sampling to select compounds with nine to ten households, with predominantly shared water and sanitation facilities, in Koumassi. We excluded compounds with layouts that did not support structured observations, including compounds with households with screens. We also excluded compounds occupied predominantly by single males, who tend to spend the majority of their time outside of their compounds, as per the 2012 pilot study findings. They are thus unlikely to be aware of their fellow residents' handwashing practices. Compounds where households were predominantly from the same family were also excluded. Eligible participants were permanent adults (aged 16 years old and above) compound residents, as they were able to individually consent to the interview. We planned to sample a minimum of 200 compound residents (as per scale development sample size recommendations (e.g. [192, 199, 200]), to evaluate the psychometric properties of the finalised norms scales.

3.3. Informed consent

In eligible compounds, we approached any adult resident who was willing to interact with us. Verbal informed consent was obtained, after an information sheet had been read to the participant.

3.4. Development of a five-point context-specific Likert type response scale

The use of Likert-type response scales can be difficult in low-income settings where education levels may be low. Moreover, response options in rating scales for self-completion questionnaires, conventionally used in studies conducted in western contexts, were deemed inappropriate in our study population. This was because conventional pre-coded responses (e.g. strongly agree, agree somewhat, etc.) do not match words or expressions which are commonly used in Côte d'Ivoire to express level of agreement in everyday conversation.

Prior to testing the four scales, a context-specific, five-point Likert response scale was developed and tested in compounds in Treichville and Koumassi, to ensure its suitability and acceptability in the target population. For each conventional response category (e.g. *Definitely untrue, Untrue, Neither true nor untrue, True, Definitely true*), we identified a comprehensive list of local expressions commonly used in everyday conversation, to express agreement and disagreement (Table 6.1). Appendix 6.1 describes the methods used to develop and test the response scale. Both French and Dioula versions of the response scale were developed. The pilot study indicated that, when prompted with questionnaire items, interviewees would naturally use expressions from the developed response scale, without being prompted with the available options.

3.5. Finalisation of the norms-related scales design

In line with good practice in scale development [192-198], we conducted a pilot study to finalise the design of the scales. The aim of the pilot study was to evaluate whether any scale items were redundant and if there were any ambiguous items. For masking purposes, all scale items were integrated within a 43-item interview schedule, including a socio-demographic section. The norms-related scale items around handwashing were positioned between two masking sections.

Definitely untrue	*Untrue*	*Neither true nor untrue*	*True*	*Definitely true*
(Ah) ca seulement c'est FAUX !/ vérité/ '…même'/'…deh !/ '…keh !'/ Hmmm ! 'En tout cas…'/ Ahh !!!/ Nonnn !!/ Jamais !/ Tu dis rien !/ Pas du tout !/ Quand même ! Non hein !!/ 'Tout le monde sait…'/ '*(C'est ca)* Ye dis…'/ C'est ça tu dis doucement ?/ Ou ça ?/ C'est pas ici !/ Non, t*k-tk/* 'Il faut reconnaitre…'/	C'EST FAUX/ Non/ Non-Non/ C'est pas vrai	'C'est PAS faux'/ 'C'est vrai AUSSI'/ DES FOIS aussi…/ SOUVENT aussi…/ C'est pas toujours/ Certains…/ On peut dire ça/ C'est PAS forcé	C'EST VRAI/ Oui/ Oui-oui/ Si/Si-si/ Voilà/ Tu vois non ?	*(Ah)* ca seulement c'est VRAI !/ '…vérité' '…même'/ '…deh !'/ '…keh !'/ 'En tout cas…'/ Wouhh !/ Ouiiiii !!/Tu dis rien/ Quand même !/ 'Tout le monde sait…'/ '*(C'est ca)* Ye dis…'/ C'est ca tu dis doucement ?/ Voiiila ! Effectivement !/ Justement !/ Forcé/ 'Il faut reconnaitre...''
Ni coni te tchan tai !	Tchan tai	Hmm…tchan tai/ Hmm..tchan lo	Tchan le/Tchan lo	Ni coni tchan le !
5	4	3	2	1

Table 6.1. Finalised, context-specific Likert-type response scale. The top row shows the conventional response categories in English. The second row shows the context-specific response expressions in French. The third row presents the response scale in Dioula, and the last row, the corresponding score for each response category.

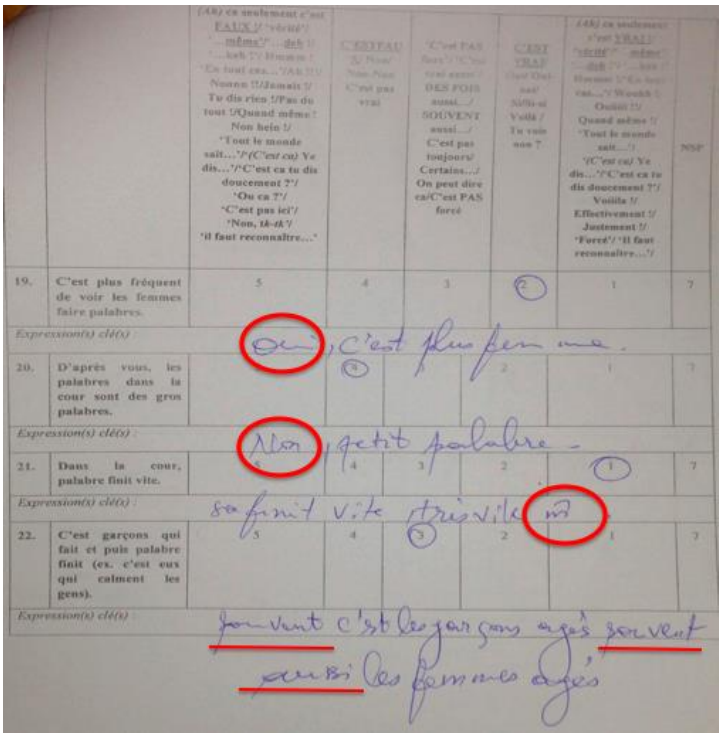
The masking items were Likert-type statements related to the theme used to mask participants. This format was adopted so that the masking sections would not stand out and break the interview flow. To put participants at ease, we included an initial masking section on their opinions on compound organisation-related topics. The second masking section aimed to assess participants' degree of identification with their fellow compound residents. The questionnaire ended with one question assessing the effectiveness of the masking items. Respondents were asked how they would explain the aim of the study to fellow compound residents. If they were unable to explain the study objective, respondents were asked for examples of questions they could give their fellow residents, if the latter inquired what type of questions participants were asked. The entire questionnaire was phrased using the local vernacular form of French.

We piloted the items in Koumassi, using an iterative process. A minimum of five participants were sampled using convenience sampling, and verbal informed consent was obtained, after reading an information sheet to eligible residents. Each item on the questionnaire was read to participants. Respondents' level of agreement with each statement was recorded on the response scale.

To assess whether interviewees had understood each item as per its intended meaning, they were asked to explain why they had rated the statement as they did. We examined respondents' understanding of each item on the questionnaire, by assessing whether their explanation of the way they rated each item was coherent with its intended meaning. When necessary, items were rephrased, and retested in subsequent piloting rounds. Some items were removed from the scales, based on participants' negative appraisal of the items. For the question assessing masking, we evaluated participants' ability to identify handwashing as the key study theme, or whether handwashing issues were identified, if participants had to give fellow compound residents examples of the questionnaire content. Sampling continued until there were no ambiguous items, no identification of handwashing as the key theme of the questionnaire, and until participants' reaction to the questionnaire was positive (e.g. no complaints about redundancy of items or length of the questionnaire).

3.6. Psychometric testing of the scales

The finalised questionnaire was administered to a convenience sample of eligible compound residents in Koumassi, after obtaining verbal informed consent. Individuals who had participated in the pilot rounds were excluded. When feasible, the interviews were conducted inside respondents' households or away from other residents. Each item was read out to respondents who were asked to indicate their level of agreement with the statement. Interviewees were not prompted with the different response options. A section was included under each scale item for interviewers to record the exact expression used by interviewees to rate items. This was in addition to circling the corresponding scale score (Picture 6.1). We did so for quality assurance. Masking was assessed in a subset of respondents and in the fieldwork assistants. The latter were asked one open-ended question at the end of the research, about what they thought the aim of the overall study was.



Picture 6.1. Context-specific five-point Likert-type rating scale with added space to record key rating expressions used by interviewees. Circled and underlined words are expressions which can be found in the context-specific response scale.

4. Data analysis

Data were analysed using STATA® 15. Descriptive statistics were computed for each item to assess the distribution of the responses, and identify items with highly skewed responses (e.g. items with an extreme response distribution which almost all participants (i.e. 95%) rate similarly) [193, 199]. Prior to analysis, scores on the response scales were reversed for items that were formulated positively. Complete case analysis was performed. Confirmatory factor analysis (CFA) was used to assess the measurement properties of scales with a minimum of three items [201]. Given the item response categories were ordinal, generalised structural equation modelling (GSEM) was used to fit an ordered probit model to the data [202]. The variances of the latent variables were constrained to equal one to obtain the loadings of each scale item. The internal consistency of each scale was assessed by either computing Cronbach's alpha (α) or the Spearman-Brown coefficient (ρ), depending on the number of items in the scale [203]. The Spearman-Brown inter-item correlation coefficient was also computed to assess the strength of the relationship between pairs of items in each scale. For each scale, the mean score across the different items was calculated.

5. Results

5.1. Finalisation of the norms-related scales

The scale items were finalised over four piloting rounds, involving a total of 20 residents. In the first two piloting rounds, six out of ten participants complained that the questionnaire was burdensome, as it was lengthy and many of the HWN items were redundant. When some items were presented, participants complained that they had already answered these questions previously. Two participants stopped the interviews before the end.

Additionally, some of the items were ambiguous. This was particularly the case for items belonging to the perceived handwashing publicness scale. These items sought to measure residents' perceptions of the publicness of handwashing practices in their compound (e.g. whether handwashing was easily observable in their compounds). However, some participants understood them as assessing whether they monitored the handwashing practices of their fellow compound residents. Other respondents understood the items as meaning that residents did not wash their hands with soap after using the toilet. The majority of interviewees were able to identify handwashing as the questionnaire theme, indicating that masking was ineffective.

The number of items per norm-related scale was therefore reduced by dropping those items participants commonly identified as being redundant. Ambiguous items were reformulated. Ambiguous masking items were also either dropped or reformulated, depending on the level of interest participants showed when presented with the items. As some participants did not have any opinion on some of the items, we added the answer *'Doesn't know'* off scale.

The edited questionnaire contained 15 HWN items, and was tested in a third piloting round. The results of this piloting round showed that there were no more ambiguous items. However, it was noted that participants tended to endorse positively phrased items, whilst also positively endorsing items that were discordant from the positively phrased items. As the number of items per scale was reduced, the decision was taken to drop positively phrased items from three of the scales, but not the HWOE scale. This was done to avoid inconsistent responses within the same scale due to social desirability bias [192, 197, 198, 204, 205]. The total number of items was thus reduced to ten, with two, three, two and three items for respectively the HWDN, HWIN, HWP and HWOE scales (Table 6.2).

In addition, the negatively framed items that appeared to carry the highest risk of response bias were reformulated to explicitly exclude the interviewee from the statement (e.g. 'In your compound, *except you*, few people wash their hands with soap after using the toilet') to minimize the risk that interviewees over-report other residents' handwashing practices.

The final questionnaire, tested in a fourth piloting round, had a total of 42 questions, with 19 masking items, one question assessing masking, and 12 socio-demographic questions, in addition to the ten norms scales items (please refer to Chapter 9 for full questionnaire). It took approximately 20 minutes to administer the questionnaire. This version was well received by participants, with no further complaints of redundancy. Participants also expressed their appreciation of the questionnaire's general theme, and the different subjects it touched upon. Handwashing was not one of the themes mentioned, which suggested that the masking items were effective. Appendix 6.2 presents the Likert-type section of the questionnaire, including the norms scales items, and the masking effectiveness assessment questions. This shortened version of the questionnaire is the one which was implemented to test the psychometric properties of the scales.

Table 6.2. Finalised handwashing norms scales

Items in local vernacular form of French	English translation
Echelle de mesure de la norme descriptive du lavage des mains	**Handwashing descriptive norm scale**
d1 Dans votre cour, si c'est pas vous, y'a pas assez de personnes qui lavent leur mains avec savon après les WC.	In your compound, except you, few people wash their hands with soap after using the toilet.
d2 Vous pensez que y'a pas beaucoup de gens qui lavent leurs mains avec savon après les WC dans la cour.	You think that there are not many people who wash their hands with soap after using the toilet in your compound.
Echelle de mesure de la norme injonctive du lavage des mains	**Handwashing injunctive norm scale**
i1 A part vous, y'a pas assez de personnes qui trouvent que c'est important de laver les mains avec savon après les WC dans la cour.	Except you, few people see handwashing with soap after using the toilet as important in your compound.
i2 La plus part des gens dans la cour trouvent que laver les mains avec savon après les WC ça ne leur dit rien.	The majority of people in your compound do not care about handwashing with soap after using the toilet.
i3. Si c'est pas vous, laver les mains avec savon après les WC n'est pas dans la tête des gens de la cour.	Except you, handwashing with soap after using the toilet is not something that people from your compound think about.
Echelle de mesure des attentes du lavage des mains	**Handwashing outcome expectation scale**
oe1t. Dans la cour, a part vous, les gens pensent que y'a pas assez de temps pour laver les mains avec savon après les WC.	In your compound, except you, people think that there is not enough time to wash hands with soap after using the toilet (reverse scored).
oe2m. C'est pas à cause de microbes que vous vous lavez les mains avec savon, en général.	Germs are not the reason why you wash your hands with soap, in general.
oe3d. Vous vous lavez les mains avec savon pour enlever la saleté sur les mains.	You wash your hands with soap to remove dirt from your hands (reverse scored).
Echelle de mesure du caractère public du lavage des mains	**Handwashing publicness scale**
bp1. Le lavage des mains de chacun dans la cour n'est pas facile à voir, comme y'a pas d'endroit fixe où se laver les mains.	It is not easy to see the handwashing practices of compound residents, as there is no fixed place where everybody washes their hands in your compound.
bp2. Pour savoir qui lave les mains dans la cour, les yeux doivent se fatiguer (c'est-à-dire, les yeux doivent regarder à gauche à droite), comme chacun lave les mains un peu partout dans la cour.	Knowing who practices handwashing in your compound requires a lot of effort, as each resident washes their hands anywhere and everywhere in your compound.

5.2. Psychometric testing of the handwashing norms scales

We interviewed 201 residents from 60 compounds in Koumassi using the final version of the questionnaire. 149 (74%) respondents were women, and 145 (72%) were aged between 16 and 34 years old (Table 6.3). The response rate exceeded 96% for all items (Table 6.4). All scale items had a relatively balanced distribution of responses. Participants who responded *'doesn't know'* to some of the items presented to them stated that they were only concerned with what took place in their own household, and did not look at what others did in their compound. Others replied that they were not inside other residents' heads to know what they thought. In such instances, interviewers were instructed to try to explain to respondents that the questionnaire was seeking perceptions only.

The Spearman-Brown coefficient indicated strong positive correlations between items designed to assess the same construct, with the exception of the outcome expectation (HWOE) construct (Table 6.5). For the latter scale, the Spearman-Brown correlation coefficient showed poor inter-item correlation (ρ negative or close to zero) for all pairwise combinations of items. The HWOE scale was thus dropped. CFA was used to assess the measurement properties of the HWIN scale. The ordered probit model converged, which is an indication that the specified factor was identified (Figure 6.1). The scale appeared reliable (α=0.83). As both the HWDN and HWP scales were reduced to two items each, CFA was not applied. Both the HWDN and HWP scales were internally consistent and reliable (respectively, ρ=0.74, α=0.88; and ρ=0.63, α=0.78). Table 6.6 summarizes the psychometric properties of the retained scales.

Table 6.3. Age and sex distribution of respondents to the handwashing norms scales

Characteristics	Male n (%)	Female n (%)	Total n (%)
Age groups (years)			
16-24	18 (34.6)	44 (29.5)	62 (30.9)
25-34	14 (26.9)	69 (46.3)	83 (41.3)
35-44	10 (19.2)	25 (16.8)	35 (17.4)
45-54	5 (9.6)	6 (4.0)	11 (5.5)
55-64	4 (7.7)	4 (2.7)	8 (4.0)
65+	1 (1.9)	1 (0.7)	2 (1.0)
Total	52 (26)	149 (74)	201

Table 6.4. Handwashing social norms-related scales items distribution

Scale items	Sample (%) N=201	Scale items	Sample (%) N=201
Handwashing descriptive norm		**Handwashing publicness**	
d1. In your compound, except you, few people wash their hands with soap after using the toilet.		bp1. It is not easy to see the handwashing practices of compound residents, as there is no fixed place where everybody washes their hands in your compound.	
Definitely true	34 (16.9)	Definitely true	29 (14.4)
True	86 (42.8)	True	95 (47.3)
Neither true nor untrue	14 (7.0)	Neither true nor untrue	12 (6.0)
Untrue	52 (25.9)	Untrue	60 (29.8)
Definitely untrue	12 (6.0)	Definitely untrue	4 (2.0)
Doesn't know	3 (1.49)	Doesn't know	1 (0.5)
d2. You think that there are not many people who wash their hands with soap after using the toilet in your compound.		bp2. Knowing who practices handwashing in your compound requires a lot of effort, as each resident washes their hands anywhere and everywhere in your compound.	
Definitely true	26 (12.9)	Definitely true	44 (21.9)
True	100 (49.7)	True	87 (43.3)
Neither true nor untrue	14 (7.0)	Neither true nor untrue	16 (8.0)
Untrue	42 (20.9)	Untrue	43 (21.4)
Definitely untrue	15 (7.5)	Definitely untrue	11 (5.5)
Doesn't know	4 (2.0)		
Scale items	Sample (%) N=201	Scale items	Sample (%) N=201
Handwashing injunctive norm		**Handwashing outcome expectation**	
i1. Except you, few people see handwashing with soap after using the toilet as important in your compound.		oet1*. In your compound, except you, people think that there is not enough time to wash hands with soap after using the toilet.	
Definitely true	16 (8.0)	Definitely untrue	18 (9.0)
True	59 (29.3)	Untrue	93 (46.3)
Neither true nor untrue	15 (7.5)	Neither true nor untrue	16 (8.0)
Untrue	92 (45.8)	True	55 (27.4)
Definitely untrue	14 (7.0)	Definitely True	17 (8.5)
Doesn't know	5 (2.5)	Doesn't know	2 (1.0)
i2. The majority of people in your compound do not care about handwashing with soap after using the toilet.		oe2m. Germs are not the reason why you wash your hands with soap, in general.	
Definitely true	35 (17.4)	Definitely true	4 (2.0)
True	63 (31.3)	True	24 (11.9)
Neither true nor untrue	19 (9.4)	Neither true nor untrue	5 (2.5)
Untrue	76 (37.8)	Untrue	152 (75.6)
Definitely untrue	6 (3.0)	Definitely untrue	16 (8.0)
Doesn't know	2 (1.0)		
i3. Except you, handwashing with soap after using the toilet is not something that people from your compound think about.		oe3d*. You wash your hands with soap to remove dirt from your hands.	
Definitely true	19 (9.4)	Definitely untrue	1 (0.5)
True	57 (28.4)	Untrue	15 (7.5)
Neither true nor untrue	24 (11.9)	Neither true nor untrue	6 (3.0)
Untrue	82 (40.8)	True	161 (80.1)
Definitely untrue	12 (6.0)	Definitely True	18 (9.0)
Doesn't know	7 (3.5)		

*Item reverse scored

Table 6.5. Matrix of inter-item correlations for each scale *(Spearman-Brown coefficient)*

Scale item	d1	d2	i1	i2	i3	oe1t[a]	oe2m	oe3d[a]	bp1	bp2
HWDN										
d1	.	.	.	.	.	.	.	.	.	.
d2	0.75	.	.	.	.	.	.	.	.	.
HWIN										
i1	.	.	.	.	.	.	.	.	.	.
i2	.	.	0.59	.						
i3	.	.	0.59	0.69	.	.	.	.	.	.
HWOE										
oe1t[a]	.	.	.	.	.	.	.	.	.	.
oe2m	.	.	.	.	.	-0.00	.	.	.	.
oe3d[a]						-0.08	-0.08	.	.	.
HWP										
bp1	.	.	.	.	.	.	.	.	.	.
bp2	.	.	.	.	.	.	.	.	0.62	.

[a]Item reverse scored

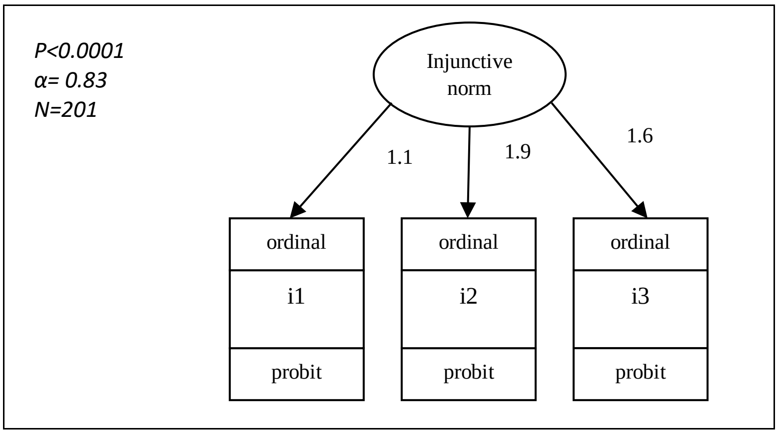


Figure 6.1.HWIN scale properties (unstandardized estimates)

Table 6.6. Summary of the psychometric properties of the HWDN, HWP and HWIN scales

Scale items	Scale reliability (ρ)	Scale reliability (α)
HWDN *(2 items)*	0.74	0.88[a]
HWP *(2 items)*	0.63	0.78[a]
HWIN *(3 items)*	.	0.83

[a]Alphas computed for comparison purposes.

5.3. Masking assessment

We assessed the effectiveness of the masking items in the last 52 participants to be interviewed. 50 (96%) participants thought that the study aimed to understand life in their compounds, with the main themes being solidarity and good relations among compound residents. Among these participants, 11 (21%) mentioned 'cleanliness' in addition to mentioning the above themes. The remaining two (4%) respondents thought that the study's main theme was handwashing. These two participants also complained about the redundancy of some of the items. The two fieldwork assistants believed the study objectives were along the same lines as those given by study participants. Neither of the fieldwork assistants mentioned handwashing among the themes cited.

Discussion

We developed and assessed the psychometric properties of four scales designed to measure social norms-related constructs around HWWS after using the toilet in compounds, in Koumassi. The large percentage of women surveyed compared to men is in line with the 2012 pilot study findings. Our results support the construct validity of three out of the four scales; the HWDN, HWIN and HWP scales, but not that of the HWOE scale. More work is needed to design a reliable measurement tool for this construct in our study population. The scales were developed and adapted taking account of the study context. Thus, comparisons of the results of these psychometric tests with the small number of published studies identified (i.e. [52, 144, 191, 206]) conducted in different settings, must be done with caution.

Hernandez et al. (2012), working in Senegal and Peru, used 12 items (eight generic items, and four country-specific items) to measure social norms around handwashing, in a study of the behavioural determinants of HWWS [52]. However, whilst the items addressed at least two different norms (descriptive and injunctive norms), the authors used them to create a single construct, raising questions about construct validity. In addition, the items referred to different key handwashing opportunities (e.g. after going to the toilet, before eating, before preparing food). As noted by the authors, attitudes and norms around different handwashing occasions may vary [52]. For some of the items on the scale, it is difficult to assess which norm theoretical domain the authors intended to measure. Additionally, the items as formulated seem to be susceptible to acquiescence. We could not independently assess the validity and reliability of the scale, as the authors did not provide the results of the psychometric tests they conducted. However, they reported that the scale was reliable and valid in Senegal, but not in Peru.

Lapinski et al. (2013) designed a reliable two-item HWDN scale (α=0.93) using the TNSB, in a college campus study aimed at testing the effect of descriptive norms on behavioural privacy, in Midwestern United States [144]. Similarly, Lapinski et al. (2014) designed a reliable four-item HWDN scale (α=0.93), a six-item HWOE scale (α=0.70) and a four-item HWIN scale (α=0.82), in a study aimed at testing the TNSB, in a childcare centre in the same geographical area as above [191]. However, all items were positively framed which, based on our experience, might increase the risk of response bias. This is a type of response bias whereby respondents tend to answer survey questions in a fashion they see as socially acceptable, and thus in a misleading fashion. Chung & Lapinski (2018) tested the prediction of the TNSB by applying the theory to handwashing in Korea. The authors designed reliable four-item HWDN scale (α=0.87), including two items negatively framed, four-item HWIN scale (α=0.83), and three-item HWOE scale (α=0.84) [206]. Lapinski et al. (2014) attempted to measure the HWP construct with a three-item scale, but it was unreliable (α=0.36) [191]. At the time of the study, we had not identified any study which had successfully measured this construct. However, Chung & Lapinski (2018) designed a reliable four-item HWP scale (α=0.80) [206].

The results of the masking assessment of both fieldworkers and participants were encouraging for the main trial, and may have been helped by the use of a masking theme which was relevant and engaging the target population. Nevertheless, these results must be interpreted with caution. The sample size to assess masking effectiveness was quite small (i.e. 50 residents). This makes the results uncertain. Additionally, when the handwashing intervention is implemented, unless strategies are put in place to ensure that participants are not able to associate the survey fieldworkers with the intervention, masking may be less effective.

Several limitations of our study can be identified. First, convenience sampling may have resulted in a sample which is not representative of the broad population. This may compromise the generalisability of our results. Nevertheless, to our knowledge, this study is the first of its kind in Côte d'Ivoire and our findings provide valuable information. Additionally, the psychometric properties of the scales will be tested again within the context of the trial, to ensure that the results were not obtained by chance [192].

Secondly, although the HWP scale was internally consistent, the way the items were formulated appears conducive to acquiescence bias. This is a type of response bias whereby respondents tend to agree with questionnaire items presented to them, irrespective of the items' content. Indeed, the items referred to the absence of handwashing facilities or dedicated handwashing areas in compounds. We did not notice any dedicated handwashing facilities in any of our visits to compounds. As seen in the 2012 study, this seems typical of housing compounds in Côte d'Ivoire. Thus, interviewees may have rated the items with a focus on the absence of handwashing facilities in mind, rather than the publicness of the practice in mind. However, the relatively balanced distribution of responses suggests this may not have been a major problem.

The failure to design a HWOE scale can be explained by the fact that each item addressed a different outcome expectation. With hindsight, this decision was problematic. Two out of the three items were selected based on the 2012 study findings regarding participants' key HWWS motives. One item attempted to assess good health as an outcome expectation, as this was the motive participants had cited the most. Another item, which came second after health as a key HWWS motive, sought to assess dirt removal as an outcome expectation. Washing hands to remove the dirt on one's hands, as the item was formulated, may be more of a comfort outcome expectation than a riddance of disgust expectation. The last item posited time constraints as an outcome expectation. This referred to the inconvenience of washing hands with soap, in the absence of soap at the handwashing location. This latter outcome expectation was chosen based on Curtis et al. (2009)'s formative research findings in 11 countries [48]. Given the low correlation between all three, dropping one of the three items to increase the reliability of the HWOE scale was not an option.

It would have been more appropriate to design separate scales for each different outcome expectation, and we recommend doing so in future studies. For instance, three out of the six HWOE scale items in Lapinski et al. (2014) were health-related [191]. In Chung & Lapinski (2018) all three scale items were health-related [206]. However, it is likely that the health items as formulated were conducive to acquiescence in both studies settings. The items were positively framed, and implied that people expected good health and not spreading diseases, when washing their hands with soap [191, 206]. As handwashing campaigns in Côte d'Ivoire are usually based on such health messages, it is unlikely that our study population would disagree with items positing good health as their key expected outcome from handwashing. On the other hand, having a scale for each outcome expectation, in the face of redundant norms scales items, would make the questionnaire burdensome to participants. A possible solution would thus rather be to focus on identifying the key HWOE in the study population, and measuring changes in that specific HWOE. Given our attempt to design a HWOE scale was unsuccessful, it was decided that it would be dropped from the subsequent trial.

In scale development, it is usually recommended to be inclusive and test a large pool of items to increase the probability that the items exhaust all the possible content of the construct of interest [192, 193, 195, 207]. Factor analysis can then be used to reduce the pool of items, by identifying items that perform poorly and can be eliminated [192, 193, 207]. This study initially attempted to measure the constructs of interests with between four to six items per scale. However, the narrow content area resulted in many of the items being very similar, which was so irritating for the respondents that some had to be dropped, based on participants' feedback.

The negative reaction of respondents to the perceived repetitiveness of the questionnaire led us to reduce the items pool to two or three items per scale, before subjecting the scales to psychometric testing. However, the risk with scales with such small pools of items is that the items are not representative of the construct of interest, and thus do not load on their intended factor. On the other hand, one could argue that, if items in the same scale are highly redundant, it may be an indication that the construct of interest could be measured with a single item. For constructs such as (perceived) descriptive norms a single item may be sufficient in some case. However, the social desirability attached to handwashing, which may lead to response bias, makes the measurement of such a construct with one item prone to error and bias. This is the case, even when there is more than one item.

One issue that may arise in the main trial is respondents' reluctance to be forthcoming about the behaviour of other residents in their compounds, as well as about their own views more generally and their own behaviour. This could bias the handwashing norms estimates in the main trial, and make it difficult to assess the intervention effect on the handwashing norms outcomes.

Conclusion

More studies should be conducted to complement the work of this research in developing valid and reliable instruments, to measure social norms-related constructs around handwashing. The social desirability of handwashing, and the narrow content area of social norms constructs relating to handwashing present significant challenges when designing items to measure handwashing norms-related constructs. Future studies attempting to measure such constructs will need to take this into account, and develop and use appropriate strategies to generate scale items.

Chapter 7- Interventions Rationale and Design

We designed two handwashing interventions aimed at increasing handwashing with soap after using the toilet. The first intervention was designed using the Theory of Normative Social Behaviour (TNSB). The intervention comprised compound residents being shown short video clips, a Glo Germ© demonstration, provision of posters promoting handwashing, and a handwashing station (HWS) with an initial supply of soap. The second intervention consisted of providing compounds with an HWS with an initial supply of soap only, but without any promotional messages.

All the intervention components were piloted and/or designed using a participatory approach. The HWS were piloted in Koumassi and Treichville during the 2012 pilot study, as described in Chapter 5. The other intervention components were piloted among compound residents in Treichville. Convenience sampling was used to sample compounds and residents. Compounds and residents were not sampled more than once. For each intervention component, a minimum of five adults (i.e. ≥16 years old) was sampled. The intervention components were modified in the light of findings from the pilot testing, as appropriate.

This chapter is divided in two parts. The first part presents the rationale behind the intervention design. The second part describes the intervention design process. Briefly, the video clips were designed to elicit disgust when individuals did not wash their hands with soap after using the toilet. Posters were designed to remind compound residents of the messages from the videos. The Glo Germ© demonstration aimed to demonstrate that hands were soiled with faecal material, after using the toilet, even though they looked clean. The provision of HWS aimed to facilitate handwashing with soap by providing water and soap together at the handwashing location.

Part I: Interventions rationale

1. TNSB-based handwashing intervention

1.1. Rationale

The first handwashing intervention was designed based on the Theory of Social Normative Behaviour [119, 128, 146], which was described in Chapter 4, and using some social marketing techniques (See Section 1.3). As discussed previously, the TSNB posits that the relationship between the descriptive norm and actual behaviour is moderated by four factors: Injunctive norms, outcome expectations, group identity, and ego involvement (the latter is not relevant to this study) [119, 128, 146]. In addition to the above moderators, a behaviour is susceptible to normative influences if it is a behaviour enacted in the public sphere [119, 129].

From the 2012 pilot study (see Chapter 5) and UNICEF (2008) study findings, we anticipated that the frequency of handwashing with soap after using the toilet would be low in our study setting (i.e. ≤ 5%). From the 2014 study to design the norms-related scales around handwashing after the key occasion (Chapter 6), we expected that the perceived descriptive norm around HWWS (HWDN) and perceived publicness of HWWS (HWP) after the key occasion would also be low in our study setting. We hypothesised the perceived outcome expectation (HWOE) to be disease prevention (good health outcome expectation), and that this would be inadequate at inducing the behaviour, as per the findings from the 2012 pilot study. Riddance of disgust was rather chosen as the outcome expectation. We also expected the perceived injunctive norm (HWIN) around HWWS after the key occasion to be strong.

1.2. TNSB-based intervention conceptual framework

Based on the above findings, we designed an intervention which sought to increase HWWS after using the toilet by increasing residents' perception of the descriptive norm around HWWS after this occasion. The intervention also sought to increase residents' perception of the publicness of HWWS after the key occasion. Last but not least, the intervention was designed to refocus the outcome expectation around HWWS from good health to removing a disgusting contaminant left on hands after having used the toilet.

We aimed to achieve this by changing residents' perception of the descriptive norm of and behaviour publicness around HWWS after using the toilet. As the perceived injunctive norm around HWWS after using the toilet was anticipated to be already strong, we expected that the intervention would reinforce it. For the perceived outcome expectation around HWWS after the key occasion, we intended to use explicit images to trigger feelings of disgust about the contaminant (i.e. faeces) left on one's hands after using the toilet.

1.2.1. Increasing the perceived descriptive norm around HWWS through increasing the perceived behaviour publicness

We aimed to increase the perceived publicness of HWWS after using the toilet, by increasing residents' sense of awareness of handwashing practices after the toilet in their compound. We expected that this would be further facilitated by the provision of an HWS close to the toilet. Residents would be aware that, if they came out of the toilet and did not stop at the HWS to wash their hands, this would be noticeable to other residents, as depicted in the videos. Additionally, in the case where residents would use the HWS, this would contribute to giving the impression that (more) HWWS after using the toilet was taking place in the compound. This would be the case even if behaviour had not changed yet. This in turn would contribute to increasing the perceived handwashing descriptive norm, and encourage residents to conform to the new norm leading to behaviour change. As we anticipated the perceived injunctive norm around HWWS after using the toilet to be already strong, making HWWS after the toilet more salient in residents' mind and more visible would further reinforce this norm. Individuals would be under the impression that a lack of compliance would be known and disapproved. This would further motivate them to practice HWWS after using the toilet.

1.2.2. Increasing the perceived descriptive norm through changing the handwashing outcome expectation

'Riddance of disgust' was chosen as the new target outcome expectation around HWWS after using the toilet based on previous study findings (e.g. [48, 54, 58, 59, 65]), and as discussed in Chapters 2 to 5. The intervention was designed to elicit feelings of disgust with respect to unwashed hands, caused by the presence of a contaminant on hands following toilet events. HWWS was offered as the solution to alleviate the triggered disgust feeling, by effectively removing the contaminant. The triggered disgust feeling would thus motivate individuals to practice HWWS after using the toilet, and give them the impression that their fellow residents would also do so, thereby increasing the perceived descriptive norm further reinforcing the social pressure to conform (strengthened perceived injunctive norm).

Figure 7.1 shows the conceptual framework for the anticipated effect of the TNSB-based intervention on HWWS after using the toilet, through the intervention effect on the norms-related constructs.

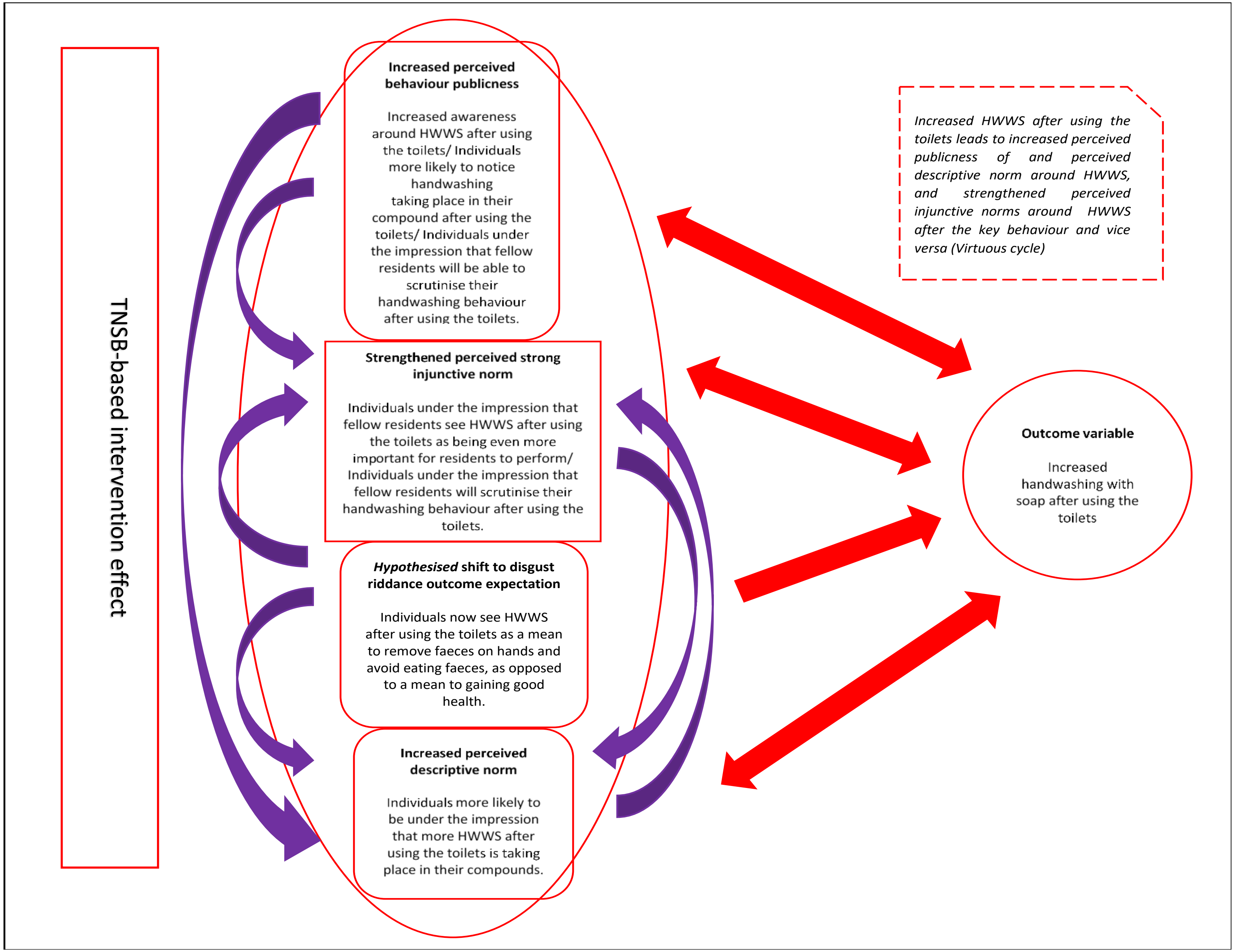


Figure 7.1. Conceptual framework of the anticipated effect of the TNSB-based handwashing intervention on HWWS after using the toilet, through the intervention effect on the norms-related constructs

The supply of an HWS would also contribute to motivating residents to engage in the new HWDN, by facilitating the practice (see Section 2.2.1 below). This enabling role of the handwashing facility is not part of the TNSB. The practical aspect of the HWS is thus not conceptualised in Figure 7.1, but in Figure 7.2.

As handwashing with soap after cleaning a child's bottom was a secondary outcome, the intervention did not strongly focus on changing this practice, though an effect is plausible, operating through the same mechanisms. From structured observations, we noticed that toilet was the location where residents would dispose of children's faeces, after having cleaned their bottom. Changing the outcome expectation to riddance of disgust, would thus potentially be the key driver of changes in the other norms-related constructs leading to an increase in HWWS practices after cleaning a child's bottom.

1.3. The use of social marketing techniques

In order to increase the likelihood that the intervention was effective, it was also designed using some social marketing techniques. Social marketing can be generally defined as the use of marketing techniques applied to social issues [208]. It is a method that is used to develop activities aiming to motivate or persuade people into changing or maintaining a behaviour [208, 209]. The targeted behaviour is usually considered as beneficial to individuals and society altogether [208, 209].

The TNSB-based intervention was designed with a specific target audience in mind (adult compound residents), the "consumers" [208, 210]. HWWS after using the toilet was the product that we aimed to "sell" to the consumers [208, 210]. Before designing the TNSB-based intervention, audience research (i.e. pilot studies) were conducted in order to understand the target audience current practices, and identify barriers to the uptake of the new behaviour that we wished to establish [208]. The consumers were then integral to the design of the intervention, through the piloting of each of the intervention components, and/or using a participatory approach to design the intervention components [208, 210]. This was key to ensuring consumers' uptake of the new behaviour that we aimed to establish, and thus ensure the product's success [208, 210]. Ensuring that the consumers were receptive to and believed in the intervention messages were also important to succeed in changing the target audience's behaviour [210].

In that sense, the negative videos (See Section 3.2., Part II) were also designed to create a need in the consumers for the product (i.e. HWWS after using the toilet). This was done by triggering feelings of disgust in the audience, and only presenting the problem (i.e. the presence of a disgusting substance on one's hands after using the toilet, and that we ended up eating), and no solution in these videos. The need created was how one could effectively get rid of the disgusting substance. The use of emotional appeal (i.e. disgust emotion) was key to develop persuasive intervention messages [208].

On the other hand, the solution videos (See Section 3.2., Part II) presented the product that all residents should have in order to remove the problem. HWWS after using the toilet was presented as the only product which responded to consumers' need, by effectively removing the disgusting substance on their hands, and preventing them from eating each other's faeces. This was compared to its rival products (e.g. no handwashing, handwashing with water only, handwashing with antibacterial gel). Glo Germ© demonstration showed the veracity of the effectiveness of the product we were selling. The HWS and initial soap supply were presented as facilitators for the adaption of the product. The intervention slogan (See Section 3.2., Part II) was simple and easy to remember, which increased the chances that it would stick in consumers' minds, and that they remembered the product.

2. Handwashing station-only intervention

2.1. Intervention rationale

The second intervention was designed based on study findings positing the importance of convenience when seeking to modify handwashing behaviour. Having both water and soap at the handwashing location would make it easier to wash hands with soap (e.g. [43, 46, 60, 67, 68, 211]), as discussed in Chapters 2 and 5. Additionally, having a constant context where a behaviour repeatedly takes place is key for habit formation [69-72].

From the 2012 pilot study findings, we anticipated that there would be a lack of handwashing facilities in our study setting and that water and soap would often not be located together at the handwashing location. Residents would rather have to go fetch soap and bring it to the water source used for handwashing. The absence of handwashing facilities would also mean that residents would likely use buckets or 'seridaca' (plastic kettles) to wash their hands.

Based on the above findings, we designed an intervention aimed at facilitating HWWS after using the toilet, by providing compounds with an HWS with an initial soap supply.

2.2. Handwashing station-only intervention conceptual framework

2.2.1. Intended intervention effect

We anticipated that residents would be motivated to wash their hands with soap after using the toilet, as both soap and water would be readily available as soon as they would exit the toilet. Having the station HWS placed at the toilet entrance would act as a visual cue reminding residents to wash their hands with soap not only after using the toilet, but also after cleaning a child's bottom.

2.3.2. Unintended normative intervention effect

The HWS-only intervention was not designed to have an impact on the norms-related constructs around handwashing after using the toilet. Nevertheless, we anticipated that the HWS would make handwashing after the key occasion more readily observable. The intervention would thus increase the perceived publicness of HWWS after the key occasion. This would in turn have the same reinforcing effect on perceived injunctive norms, and increase the perceived descriptive norm as described for the TNSB-based intervention.

Even prior to HWWS increasing, the fact the HWS would make the key behaviour more noticeable (if used) would leave individuals with the impression that more residents washed their hands with soap after using the toilet (increased perceived descriptive norm). Individuals would thus be under the impression that residents definitely saw handwashing with soap after the key occasion as an important behaviour to perform (strengthened injunctive norm). This would lead individuals to engage in the practice.

Figure 7.2 shows the conceptual framework of the anticipated HWS-only intervention effect on HWWS after using the toilet, both through the facility-alone and the unintended norms-related constructs effect.

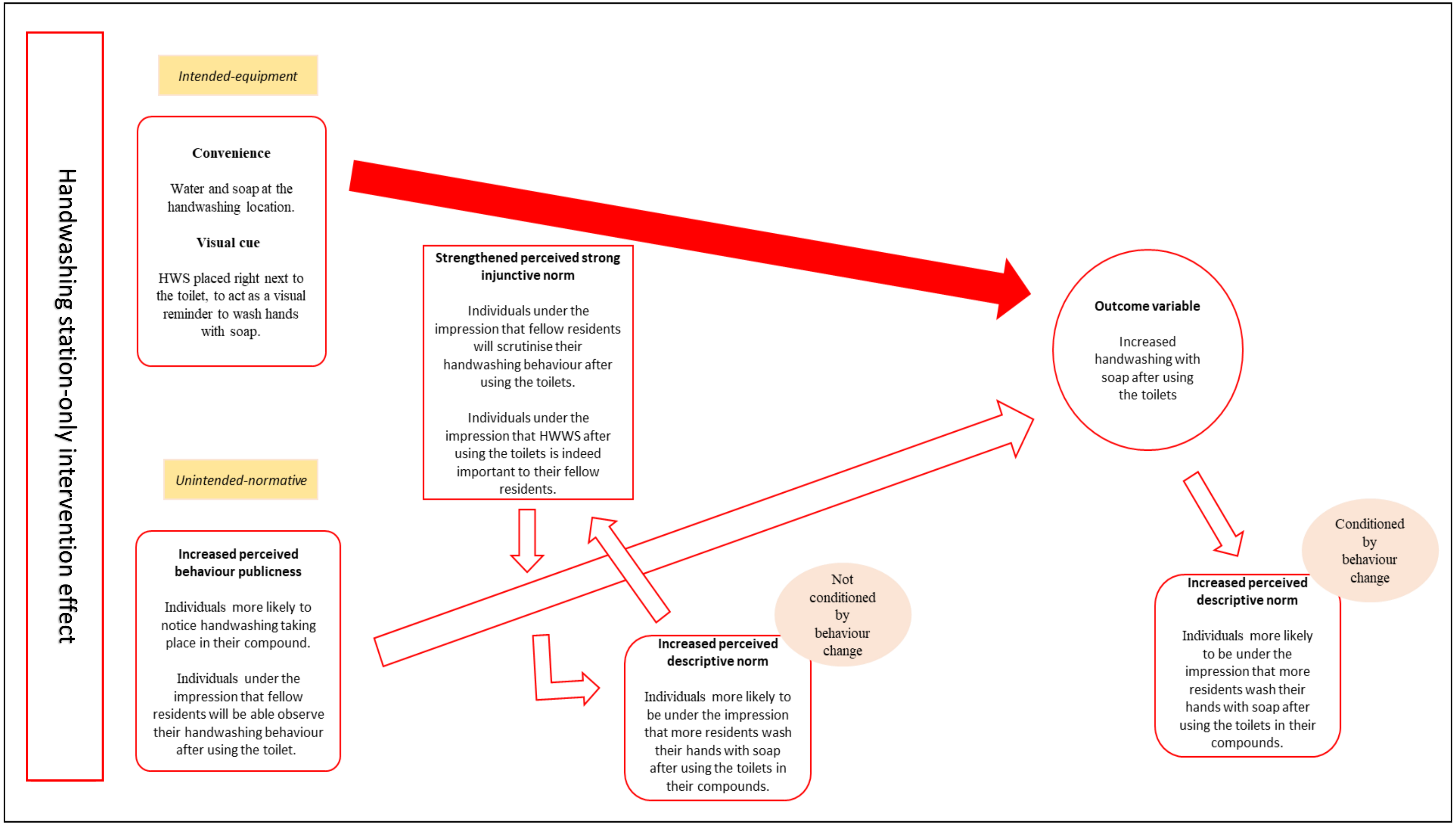


Figure 7.2. Conceptual framework of the anticipated handwashing station-only intervention effect on HWWS after using the toilet, both through the facility-alone and the unintended norms-related constructs effect.

Pictures 7.1. to 7.4 illustrate the handwashing station, the digital effect used to trigger disgust in the videos, and the intervention posters. Please refer to Appendix 7 for a full description of each intervention design process.

Picture 7.1: Handwashing station's finished product (top bucket's cover removed)

Picture 7.2: Disgust visual effect used in the videos

Image7.3: Negative poster, with the caption reading: "*Koutoubou!!*[17] *That's how he does and we drink poo here!"*. The poster is crossed out in red, to indicate the behaviour not to engage in.

Image 7.4: This image is the solution poster to Image 4, with the intervention slogan, *Water and soap after using the toilet, that's what really works! Well indeed!"* The samples of the three common soap types found in the study population are also Represented. The thumbs up under the slogan indicates that this is the behaviour to adopt.

[17] *Koutoubou* is a local vernacular expression to express shock or surprise.

3. Ethical considerations

The use of deception (i.e. portraying the substance on hands as faeces and not microbes) was key to shifting the outcome expectation from health to riddance of disgust. It was also key to testing the proposition that health may not be a key motivator of handwashing practices. Nevertheless, given the nature of the deception, we did not anticipate that this would cause any harm among the study participants.

Chapter 8 - Interventions Delivery Methods

This chapter describes the methods for the implementation of the Theory of Normative Social Behaviour (TNSB)-based handwashing intervention and the handwashing station (HWS)-only intervention presented in Chapter 7. The interventions were delivered between August and November 2016. The intervention providers were recruited and extensively trained over six weeks of theoretical and practical training (Appendix 8.1).

1. Interventions delivery methods

For both interventions, staff were instructed to deliver the intervention only if a minimum of five eligible compounds residents (i.e. permanent adult (≥16 years old) compound residents) were present. If it was not the case at the time of the visit, intervention providers were instructed to revisit the compound at a later date. The same approach was used when encountering compounds where ceremonies or events (e.g. weddings, funerals) prevented intervention delivery on the scheduled day.

Prior to the trial start, and when seeking informed consent from compound residents for this new trial phase (Chapter 9), the PhD candidate and fieldwork assistants took the opportunity to note the total number of toilets in each TNSB-based intervention compounds to determine the total number of posters required per compound.

To maintain masking, neither the PhD candidate nor the fieldwork assistants monitored the intervention delivery sessions (Chapter 9). Audio recorders were bought, and intervention providers were asked to record each of their intervention delivery sessions, for monitoring and quality assurance purposes.

1.1. TNSB-based handwashing intervention delivery methods

Please see Appendix 8.4. for the list of intervention equipment.

1.1.1. Methods

From the intervention pilot, we anticipated that it would take up to one hour and a half to deliver the TNSB-based handwashing intervention. Thus, only one compound a day was assigned to the intervention providers. Videos screening took place first, followed by the Glo© germ demonstration and HWS setting up. The posters were the last intervention components to be implemented. Please see Appendix 8.5. for the detailed intervention delivery procedure.

- **Introduction and setting up**

The intervention providers arrived in their assigned compound at 4.45 pm. Upon arrival they approached adult residents, and introduced themselves as volunteers from the Family Arc-en-Ciel association. They explained that the purpose of their visit was to share happy moments with residents, through the screening of funny videos, among other activities, and to provide the compound with a gift. The Family Arc-en-Ciel is a well-known volunteer-based association which aims to improve the quality of life of children that are disadvantaged, distressed, ill, and/or handicapped in Côte d'Ivoire [213]. They do so through volunteers who work in participating youth centres, and the Children's Cancer Unit at Treichville's University Hospital Centre [213]. The PhD candidate obtained permission from the head of The Family Arc-en-Ciel to use the Association's name in the study.

Residents were told that a minimum of five adult residents would be required for the screening to take place, and were asked to inform their fellow residents. The intervention providers identified a suitable location in the compound, and asked residents for the permission to project the videos in this area. When permission was granted, the intervention providers set up their equipment (Picture 8.4). All electronic equipment was powered by a portable generator, which was placed outside the compound, to minimise the noise in the compound (Picture 8.5). The videos were projected onto a white sheet placed on a wall in a shaded area of the compound's communal courtyard. The additional white sheets were used to create additional shade, in cases where there was no shaded area in the compound, or still too much light reducing the visibility of the video's images (Picture 8.6). The computer was connected to a portable bluetooth speaker, to ensure that the videos audio would be loud enough. It usually took approximately 15 minutes for the intervention providers to set up their equipment, at which point, the natural light would be sufficiently dim for the videos screening.

After setting up their equipment, intervention providers gathered the willing participants in the screening area. The intervention providers would then introduce themselves again, and explain to participants how the session would unfold. Participants were told that they could ask questions during the session.

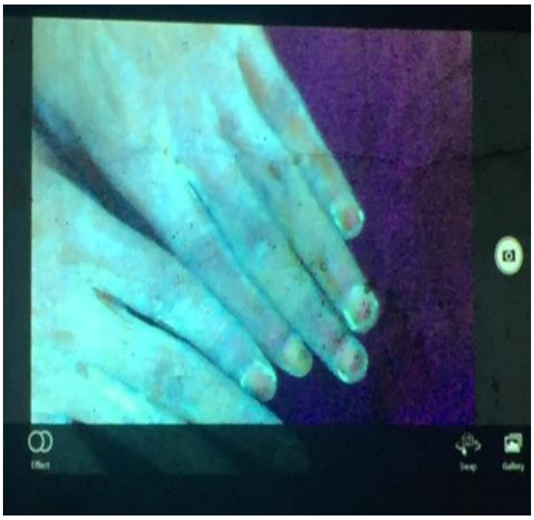
Picture 8.4: Intervention providers setting up

Picture 8.5: Portable generator outside a compound

Picture 8.6: Shaded area created by the intervention providers for the video screening session

The following pictures illustrate the TNSB-based intervention delivery

Picture 8.7: Video screening session in a compound with an intervention provider standing at the front and another one at the back of the audience.



Picture 8.8: Glo gel visible on a volunteer's hands under the UV-lights in the black box.

Picture 8.9: Handwashing station placed at the toilet entrance

Picture 8.10: Negative poster on a door inside the toilet. .

. **Picture 8.11:** A positive poster and a handwashing station at the toilet entrance in a compound.

We aimed to deliver the TNSB-based intervention six times in each compound, with each video group being shown twice. However, during the second round of screening of the first set of videos, intervention providers raised the issue that participants tended to be less enthused, when realising that they had already seen the videos being shown. Thus, the first group of videos was screened twice in each compound, but the remaining two video groups were only screened once each per compound. Additionally, Glo© germ demonstration was only implemented once (i.e. at the first intervention implementation round). The shortened intervention session took no more than 45 minutes. The posters were changed every time a new group of videos was shown.

One week post initial intervention delivery, intervention providers visited compounds to ensure that there were no problems with the handwashing station (e.g. broken stand, broken tap). In case of any problems, they were either addressed on the spot, when feasible (e.g. tightening of the tap joint), or the station was immediately replaced.

1.2. Handwashing station-only intervention delivery methods

1.2.1. Methods

From the intervention pilot, we anticipated that no more than 20 minutes would be needed for intervention providers to deliver the HWS-only intervention. Thus, a single intervention provider was required per compound. Intervention providers were each assigned two to three compounds to cover per day.

- **Introduction**

Intervention providers were in their first assigned compound at 4.30 pm. The intervention providers were trained to enter the compound shouting a catchy word or phrase to attract residents' attention, in the typical fashion of street vendors. For instance, one of the intervention providers entered compounds shouting *"Gift! Gift! Gift! Gift!"* and using a funny intonation. The street-vendor-like attitude was maintained throughout the intervention delivery. The intervention provider would then introduce themselves to the present residents, by stating that they were from The Family Arc-en-Ciel Association, and here to give a gift to the compound. The residents were told that the gift could only be given if there were at least five adult compounds residents present. The intervention providers kindly asked residents to help them gather other fellow compounds residents. When all willing participants were present, the intervention providers introduced themselves again, and the purpose of their visit.

Presentation of and setting up the handwashing station

The intervention providers explained that the gift brought was to make handwashing easy to perform. The audience was told that, instead of having to fetch water and soap at different locations in the compound, when they wanted to wash their hands, the HWS would enable them to have both water and soap readily available at the handwashing location. The remainder of the intervention delivery was similar to that of the TNSB-based intervention, but for the following exceptions. As no handwashing promotion messages were delivered, intervention providers were instructed to suggest that the HWS be placed at the toilet entrance, as this would be the location which would least disturb residents' activities.

Picture 8.12: An intervention provider showing residents where each HWS component goes on the stand

To ensure that any change in HWWS after the key occasions would be mainly due to the HWS, we limited the number of contacts between intervention providers and compounds residents to only one additional visit post-intervention delivery. As in the TNSB-based handwashing intervention, the visit was one week post initial intervention delivery, to verify that there were no problems with the HWS. In case of any problems, they were either addressed on the spot, when feasible, or the HWS was immediately replaced.

Chapter 9 – Methods: Trial Design and Conduct

The randomised trial design was informed by the 2012 (Chapter 5) and 2014 (Chapter 6) studies. The PhD candidate was assisted by two fieldwork assistants in conducting the trial. Both received training on the trial's methods, and on their new role as fieldwork supervisors. In this chapter, we will use the term *fieldworkers* to refer to trial staff supervised by the PhD candidate and fieldwork assistants. These trial staff collected data linked to the evaluation of the interventions' effect. *Intervention providers* will be used to refer to the trial staff who delivered the interventions, and collected process evaluation data. The two teams were distinct and did not interact with each other. We obtained ethical approval from Côte d'Ivoire's Bioethics Committee (*Comité Consultatif de Bioéthique de Côte d'Ivoire*), Côte d'Ivoire's Ministry of Higher Education and Scientific Research (Ref. 0758/MESRS/CAB 1/gsy), and the London School of Hygiene and Tropical Medicine's (LSHTM) Research Ethics Committee (Ref. 7029).

Research questions

The research aimed to answer the following questions:

1. Did handwashing practices after the key occasions improve in the intervention groups compared to the control/no intervention group?
2. Is there an association between norms-related constructs and HWWS after using the toilet?
3. Did the interventions have any impact on the norms-related constructs?

1. Trial design and overview of the study site

1.1. Study design

The study was a three-arm superiority cluster randomised trial (CRT), conducted in housing compounds, in Koumassi, from August 2014 to April 2017. Due to the nature of the intervention, which could only be delivered at compound level, the randomisation was at the compound (cluster) level rather than the individual level. We used the statistical package STATA® 13 to randomly assigned compounds to the TNSB-based handwashing intervention, the handwashing station (HWS)-only intervention, or non-intervention control, using a 1:1:1 ratio. The TNSB-based handwashing intervention consisted of compound sessions during which residents were shown short video clips and a Glow germ© demonstration, provision of posters promoting handwashing, and an HWS with an initial supply of soap (Chapter 8). The HWS-only intervention arm only received an HWS with soap, but without any promotional messages (Chapter 8). The third trial arm was a non-intervention control group. This latter group received HWS with soap, at the end of the trial.

1.2. Blinding

The PhD candidate and fieldwork assistants were not masked to the study objectives and hypotheses. However, they neither participated in data collection, except to supervise fieldworkers, nor in the intervention delivery.

Both participants and fieldworkers were masked to the study objectives and hypotheses, and fieldworkers were not told about the activities of the intervention providers. They were told that the study was part of a PhD research study aimed at understanding how housing compounds were organised, particularly as it pertains to gender roles and social cohesion among residents. However, fieldworkers were aware that there was a general hygiene component to the study. This was because, during their training, they were instructed not to mention anything related to hygiene, when they explained the study aims to participants. This was justified by explaining the notion of the Hawthorne effects to fieldworkers. Fieldworkers did not know that the hygiene component of the study was limited to handwashing, as there were masking items in their observation grids requiring them to collect data on other hygiene-related themes.

The intervention providers were not masked to the study objectives, and were aware of the work undertaken by the fieldworkers. Care was taken to ensure that the two trial teams never met in the field. On the days where field activities were carried out by the two trial teams simultaneously, the PhD candidate verified the list of all compounds to be visited (i.e. for data collection and interventions delivery), to ensure that there was no risk for the teams to meet. We also ensured that both teams never worked in the same area. Nevertheless, and as a precaution, intervention providers were instructed to ignore the fieldworkers, and supervision team, in the unlikely event they met in the field. This never occurred during the course of the trial.

Study participants were masked to the fact the intervention providers were part of the same study as that conducted by the team of fieldworkers. This was accomplished by having each team obtain separate informed consent, with different explanations given to residents regarding the purpose of their visits. The fieldworkers' information sheet, read to residents, explained that the research was related to the standard masking theme mentioned above. In both the TNSB-based handwashing and HWS-only intervention groups, the intervention providers introduced themselves to residents as volunteers from the Family Arc-en-Ciel association, as mentioned in Chapter 8. Section 2.2. details the informed consent methods for each trial teams.

The above masking measures were taken to both minimise the risk of reactivity among the study participants, and minimise the risks of differential misclassification[18] by fieldworkers.

There were no trial termination criteria.

1.3. Study site

The trial was conducted in six of Koumassi's eighteen neighbourhoods[19]. These were Inch Allah, Michigan, Port-Bouët II, Grand Marché, Adioukrou, and Grande Mosquée (Map 1). These neighbourhoods are divided into between two and six sub-neighbourhoods. We chose these neighbourhoods due to compounds being the predominant habitat type, and for their proximity

[18] Differential misclassification refers to systematic differences between trial arms in errors in measuring/recording outcomes.
[19] Koumassi Town Wall's website is not up to date in terms of the administrative division of the commune, among other data. The administrative division was thus done with the help of the fieldwrok assistants, who are Koumassi residents.

to each other. The remaining neighbourhoods either had individual houses as the predominant habitat type, were industrial zones, or were known for having crime-related security issues.

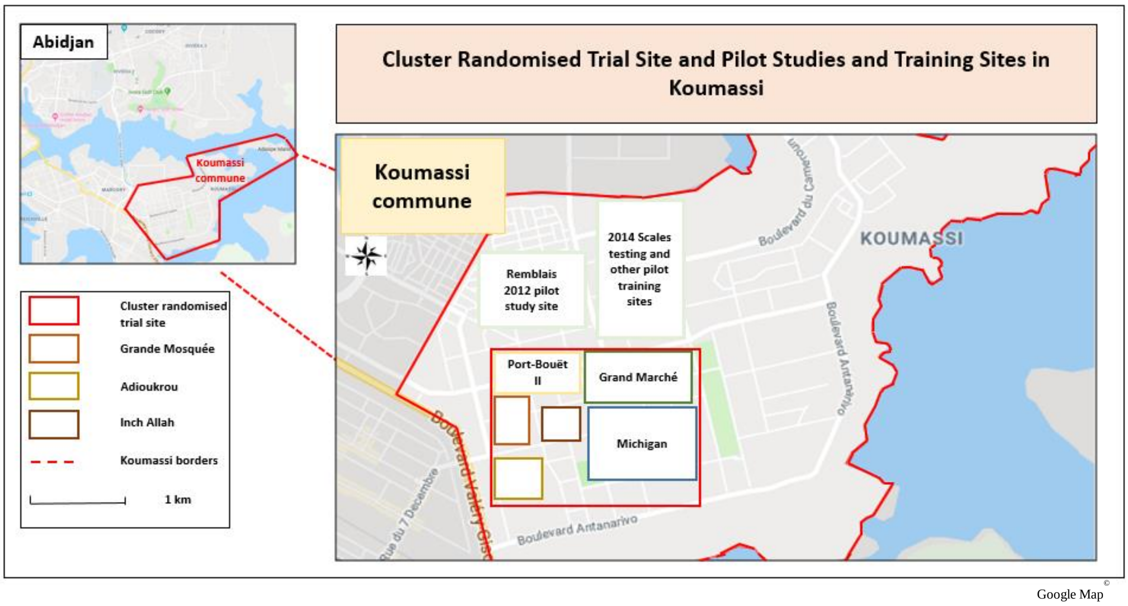
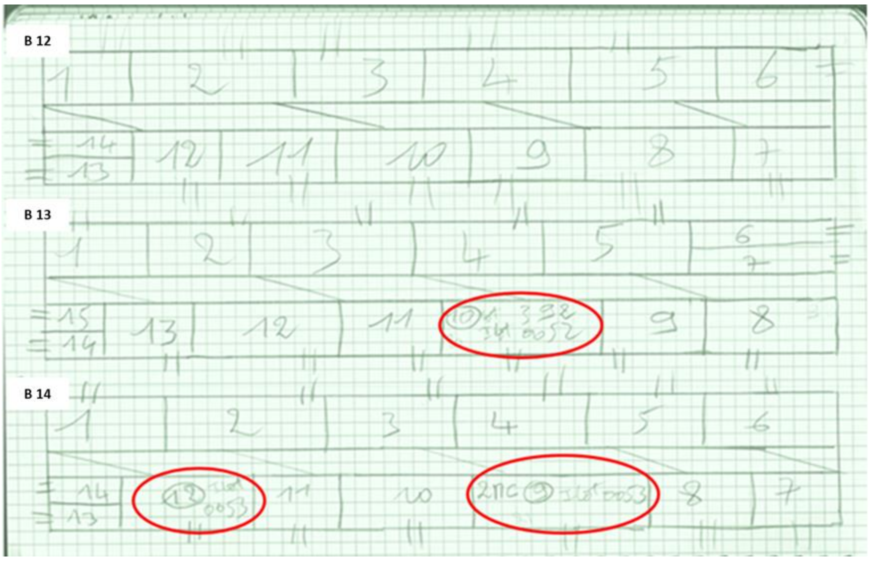


Figure 9. 1 Map of the cluster randomised trial site, pilot studies and training sites in Koumassi

2. Participants

In order to easily locate the study compounds, a sketch of the study area was prepared. The study area was laid out as a checkerboard, with each grid representing a block (islot), and each block separated from the next by a street. We started from the first block, when entering the study area, and attributed the number one to this block. Each compound on the block was then given a number in order (Picture 9.1).



Picture 9.1. Sketch of the study area showing Blocks 122 to 124, each separated by a street. The position of each compound on each block is represented. The circled compounds with administrative numbers are the ones which met the eligibility criteria on these blocks

2.1. Eligibility criteria

Eligible compounds were those with:

- 7≤N≤13 households per compound, as these were neither too small nor too big for the trial,
- with a maximum of two households with screens,
- predominantly shared water and sanitation facilities not located in corridors,
- and at least four children under five years old among residents.

The exclusion criteria were compounds with:

- predominantly single males as residents,
- where households were predominantly from the same family,
- and with handwashing facilities (e.g. sinks, HWS).

1,974 compounds were screened. The landlord of one compound refused to take part in the study. 92 out of 1,974 compounds (5%) met our inclusion criteria. The main reason for non-eligibility was the presence of more than two households with screens per compound. We had assumed that having more than two screens would obstruct the views for observations.

2.2 Informed consent

Verbal informed consent was obtained separately by each trial team, as mentioned above.

2.2.1. Informed consent from the fieldworkers

Provisional verbal informed consent was sought from eligible residents (i.e. permanent adult (≥ 16 years old) residents) and landlords who were present at the time of the visit, in eligible compounds. An information sheet was read to household heads, containing the standard masking theme. Any questions from potential participants were answered (Picture 9.2).

Participants were told that data would be collected at three different points in time in their compounds, via structured observations and interviews. We emphasized that, at any point in the study, and without having to give any explanation, residents could decide to withdraw their consent. We also asked permission to take pictures in the compounds, for illustrative purposes. We emphasised that, if residents appeared in the pictures, their faces would be masked, so that they could not be identified. We asked household heads, who were present at the time of the visit, if they would tell absent residents about the study on our behalf. We also informed them that, if their compound ended up being selected for the study, we would come back, prior to actual data collection, to obtain confirmation that the compound still agreed to take part in the study. If a compound inhabitant refused to be part of the study, the entire compound was excluded. Administrative identifiers[20] and geographic coordinates of each potential study compound were recorded in a spreadsheet (Picture 9.3). In cases where administrative identifiers were not visible, a unique identifier was allocated to the compound. Informed consent for data collection linked to the fieldworker's team was sought at the beginning of each trial phase (i.e. at baseline, one-month and five-month follow-ups).

[20] In Cote d'Ivoire, compounds' administrative identifiers are composed of two sets of numbers, the lot, which is a unique identifier, and the 'islot' (block), which is a number shared by all compounds on the same block.

Picture 9.2: The two fieldwork assistants responding to questions from a group of heads of households during ascertainment of informed consent.

Picture 3: The two fieldwork assistants exiting an eligible compound, with one circling its position on the sketched study area, and recording its administrative identifier, located on the wall at the top of the compound entrance.

2.2.2. Randomisation

STATA® 13 was used to randomly assign the 75 selected compounds to the three trial arms (control i.e. non-intervention, partial intervention and full intervention groups) in a 1:1:1 ratio (n=25 compounds per study arm), after baseline. No stratification, restriction or minimisation was used. Due to the nature of the interventions, the allocation sequence was not concealed.

2.2.3. Informed consent from the intervention providers

- **HWS-only intervention**

Intervention providers entered compounds in the typical fashion street vendors would do (i.e. speaking loudly upon entering the compound, to catch residents' attention), and announced that they were there to give a gift to the compound from the Family Arc-en-Ciel (Chapter 8). If residents agreed for the intervention providers to give them the gift, then the intervention was delivered, and if they refused, the intervention was not delivered.

- **TNSB-based handwashing intervention**

Intervention providers introduced themselves to residents in eligible compounds, as being volunteers from the Family Arc-en-Ciel. They explained that the purpose of their visit was to share happy moments with residents, through screening of funny videos, among other activities, and providing the compound with a gift (Chapter 8). If residents agreed for the intervention providers to spend time in their compounds, the intervention was delivered, and if they refused, the intervention was not delivered. At each subsequent intervention delivery visit, intervention providers told compound residents that they had come back to share more joyful moments with them, and inquire whether there were any issues with the HWS. They sought verbal informed consent to interview eligible residents and/or deliver the intervention.

Intervention providers obtained informed consent every time they delivered the intervention or collected process evaluation data (see Section 3.4).

2.2.3. Withdrawal of participants

During the course of the trial, some compounds withdrew their consent to be part of the study. As mentioned above, both trial teams obtained informed consent separately, and care was taken to ensure that participants could not link both teams to the same study. In 2016 and 2017, the PhD candidate and fieldwork assistants sought informed consent again from all study compounds to be part of the trial. At the time informed consent was sought in 2016, intervention delivery had not started yet. Thus, if a compound withdrew their consent to be in the study, it was removed from the entire trial. On the other hand, after intervention delivery had started, a compound withdrawing consent from one trial team had no bearing on the activities carried out by the other trial team in the compound. In other words, if post-intervention delivery a compound withdrew consent from the fieldworker's data collection activities, intervention providers could still continue delivering the intervention and collecting process evaluation data in the said compound, and vice-versa. This was the case, unless the compound withdrew consent from both trial teams.

During the course of the trial, we also ceased to collect data and deliver the intervention in compounds in which structures were modified to a point where they no longer met the inclusion criteria (i.e. compounds with sanitation facilities becoming individual vs. shared).

2.3. Compound selection

We located all 92 eligible compounds on the sketched map of the study area. Due to the nature of the interventions, care was taken to ensure that there was sufficient distance between compounds. We did so to minimize the risk of contamination between the different arms. This would tend to dilute the apparent effect of the intervention, biasing the trial towards a smaller effect estimate [92]. We used Excel to organise compounds in two different groups. One group contained the list of compounds with the lowest contamination risk (n=41 compounds). These were compounds on blocks with only one or two eligible compounds per block. In the latter case, the compounds were not on the same side of the block. All compounds in this group were selected. The remaining 51 compounds were the ones with moderate to high contamination risk. Examples of such compounds were blocks with more than one eligible compound per block and on the same block side; and compounds facing one or more compounds selected on an opposite block. 34 compounds were selected from this second group, after visiting the compounds again to exclude the ones with the highest contamination risk. In total, 75 compounds were selected, and from which formal verbal informed consent was obtained to take part in the trial.

3. Procedures

The PhD candidate and the fieldwork assistants acted as fieldwork supervisors for the trial.

3.1. Recruitment and training of fieldworkers

The trial was initially scheduled to last approximately 18 months, with data collected by the same fieldworkers for the entire trial duration. However, we encountered major issues with the production company contracted to produce the short intervention video-clips (e.g. failure to respect production timelines, refusal to finish editing the video clips). Consequently, the study was delayed by approximately a year (Table 9.1). There was thus a two-year gap between the baseline and the follow-up studies. As a result, not all fieldworkers who took part in the baseline study were available for the subsequent follow-up phases.

- **Baseline recruitment**

Fourteen potential fieldworkers went through a selection process during which we explained the study to them, and trained them on the data collection methods and tools. One of the recruitment criteria was that the potential fieldworkers spoke or had a good understanding of Dioula. Candidates underwent two weeks of theoretical and practical training on the data collection tools. For the questionnaire, we went through each item, and ensured that candidates understood the meaning of each statement. We had a strict script to be used to implement the questionnaire, to minimise between-interviewer variation in administration of the questionnaire. However, it was still important for fieldworkers to be able to accurately reformulate the items during interviews, in case respondents did not understand them. As part of their training, fieldworkers were shown pictures of a variety of sinks and HWS, so that they would be able to recognise HWS, during the follow-up phases, without the fieldwork supervisors having to bring their attention to the facilities. Doing so, could have unblinded fieldworkers.

The practical training took place in compounds in Treichville commune. The fieldwork supervisors sought verbal informed consent from adult residents. The training consisted of 1.5 hours of structured observation and administration of the questionnaire to compound residents, every day for 12 days. Throughout the training, we visited each fieldworker in their compound, to observe and take notes on how they were collecting data. A debriefing session was held at the end of each training day. Fieldworkers were also trained on residents' eligibility criteria and informed consent procedures.

Table 9.1. Anticipated and actual trial duration by activity

Activities	**Anticipated number of weeks**	**Actual number of weeks**
Trial set-up (e.g. screening and recruitment of participants and trial staff)	8 weeks	12 weeks
Baseline	8 weeks	8 weeks *(August-September 2014)*
Intervention design (including production)	20 weeks	72 weeks*
Intervention implementation (including intervention providers recruitment)	16 weeks	18 weeks *(July-November 2016)*
Follow up 1	8 weeks	8 weeks *(September-November 2016)*
Follow up-2	8 weeks	8 weeks *(January-March 2017)*
Process Evaluation	4 weeks	5 weeks *(March-April 2017)*
Total duration	**72 weeks**	**131 weeks**
* Large gap between anticipated and actual duration due to major issues with the intervention video clips production company.		

At the end of the training, the ten best candidates were hired. Eight out of the ten fieldworkers were selected to implement the handwashing norms questionnaire, in addition to conducting structured observations. The remaining two only undertook structured observations. Whilst the majority of fieldworkers had a good grasp of Dioula, we felt confident that only four were fluent in this dialect, and able to administer the questionnaire in Dioula. Therefore, they received additional training to do so. These four fieldworkers were instructed to administer the questionnaire in Dioula, when respondents were more comfortable using this language.

- **2016 One-month follow-up recruitment**

Twenty-five potential fieldworkers, including four from the 2014 baseline, went through the same selection process as that described above. Practical training took place in Koumassi, but outside of the study area (Map 1, Section 1.2). Among the ten candidates hired, four were selected to implement the norms questionnaires, in addition to conducting structured observations.

- **2017 Five-month follow-up recruitment**

Two additional fieldworkers from the 2014 cohort rejoined the trial, and replaced two fieldworkers from the 2016 cohort, who were no longer available. The eight fieldworkers from the 2016 cohort received a short training to refresh their skills, whilst the two new fieldworkers from the 2014 cohort received an intensive 12 day-training

3.2. Structured observations

3.2.1. Piloting of the observation grids

Based on the 2012 pilot study results (Chapter 5) and the trial objectives, the trial observation grids were updated. The fieldwork supervisors conducted a pilot study within the study reported in Chapter 6. As a result of the latter pilot, an updated observation grid recorded handwashing practices at three key occasions (Appendix 9.1). Compound residents typically do not use toilet paper for cleaning after using the toilet, but rather use water. We thus made the distinction between two types of toilet events in the observation grid. One was handwashing after using the toilet, irrespective of a water container. The second occasion was the same as before, but including a water container (usually a plastic kettle). The distinction was made between these two occasions, as it was likely that toilet events involving the use of water for cleansing would include defecation events. The third key occasion was after cleaning a child's bottom. A fourth masking occasion was also added, and included any handwashing practice occurring outside of the above-mentioned occasions. This was added as an attempt to mask fieldworkers to the study's primary and secondary behavioural outcomes (See Section 4). This observation grid also recorded data on the type of facilities used for handwashing (e.g. sink, HWS or other), and whether any handwashing involved the use of soap.

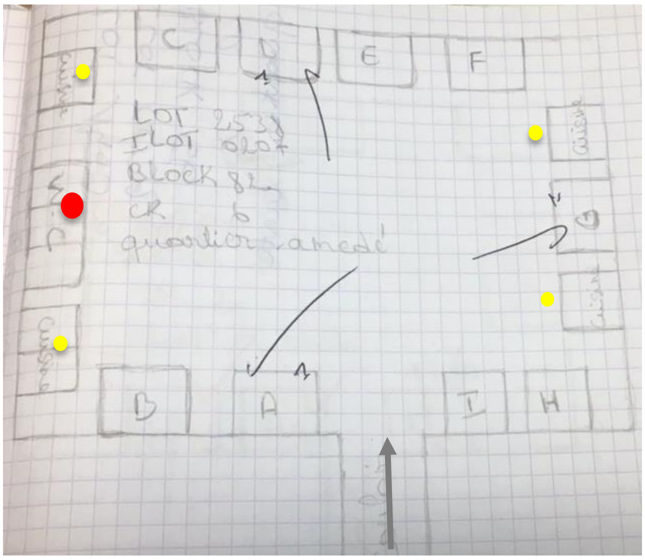
Two additional grids were also designed (Appendix 9.2). One was a grid aimed to mask the key behaviours of interest. It recorded domestic compound activities, such as who cooked and swept the floor in the compound, by gender and age group. The information collected was along the masking theme. The second grid aimed to collect handwashing-facilities-related information (e.g. presence of handwashing facility, presence of water and soap at the handwashing facility) (Appendix 3). This grid was masked with items recording sanitation-related facilities present in the compound.

3.2.2. Structured observation procedures

The structured observation methods were the same as those in the 2012 pilot study (Chapter 5). Structured observations were conducted for three hours, from 4 p.m. to 7 p.m., on Saturdays and Sundays. The trial masking theme were used when obtaining confirmation of informed consent from compounds residents.

To record whether a participant had washed their hands with soap, at a key occasion, we allowed a time frame of three minutes, after the resident had performed the action requiring handwashing. This was done to account for the time participants might take to fetch soap from their households or elsewhere in the compound, as experienced during the 2012 pilot study. If the observed resident had not washed their hands after the key occasion, but started to engage in an activity involving the use of soap (e.g. doing the laundry or washing the dishes), within three minutes following the key occasion, this was recorded as the resident having HWWS. Past this time, and if the resident engaged in any other activity, before performing an activity requiring the use of soap, the event was recorded as the resident not having washed their hands with soap. If an HWS was present in the compound and had not been used during the entire observation session, fieldworkers were instructed to go to wash their hands at the HWS, at the end of the observation, to determine whether there was any water in the HWS.

During the first two weeks of fieldwork, we visited each fieldworker in their compounds to draw a sketch of their compound, for the purpose of the household survey. The sketch showed the location of each household, sanitation facilities, and other relevant rooms (e.g. storage room, communal kitchen area) (Picture 9.4). A unique household identifier was created for each household by combining the compound lot number with a letter (e.g. 2538c indicates household c in compound lot number 2538). Fieldworkers were, subsequently, in charge of sketching their compounds, for the remainder of the trial. We checked each drawing, during supervisory visits.



Picture 9.4. Sketch of a study compound. Each household is codified with its identifying letter. The red dot indicates the location of the toilet, and the yellow dots, the shared kitchens. The grey arrow indicates the hall to the compound entrance. The check marks indicate to fieldworkers the households to be surveyed

3.3. Handwashing norms scales questionnaire

A questionnaire to measure three norms-related constructs around HWWS after using the toilet was developed, as described in detail in Chapter 6. The two items assessing handwashing publicness were reformulated, to remove the section referring to the lack of handwashing facilities or dedicated handwashing areas in compounds. This was done to reduce the risk of acquiescence bias. The two new items expressed only the idea that it was hard to notice who washed hands with soap, after using the toilet, in the compound (Appendix 9.4).

Two masking statements were used to assess respondents' propensity for acquiescence bias (Statements 5 and 6). One statement posited that when men came back from work, they took over women's domestic chores in the compound, so women could rest. The second statement stated that men helped women in their domestic chores. In the absence of acquiescence bias, we expected most participants to disagree or strongly disagree with the statements. As described in Chapter 6, the questionnaire ended with one question which assessed the effectiveness of the masking items. The questionnaire administration time was approximately 20 minutes.

3.3.1. Sampling of households

The questionnaire was administered once to each household over the course of the study. We defined a household as a group of people who ate together. In cases where residents lived in different households but ate together, an eligible resident from only one of these households was surveyed. Households interviewed at each time point were selected by random sampling, with k=3 as the sampling interval. In compounds with nine households, three households were sampled per study phase. In compounds with more than nine households, more than three households were surveyed per study phase. For each trial phase, we indicated to fieldworkers the households to be surveyed, on the compound sketch. The fieldwork supervisors monitored all survey data collection, to ensure that there were no errors in sampling. They also verified again, at the beginning of data collection in a compound, that there were no mapping errors (e.g. incorrect number of households sketched, storage room wrongly coded as a household).

Verbal informed consent was obtained from eligible residents. On the rare occasions when both household heads were present, the male head of household preferred for the female head of household to be interviewed. This was on the basis that the female head of household spent more time in the compound compared to her male counterpart. The interviews were conducted in the respondent's household or away from other residents. Fieldworkers went to the designated household and inquired whether an eligible resident was present. An information sheet was read to residents, using the standard masking theme, and residents were then asked if they agreed to be interviewed.

In the case of absences, fieldworkers were instructed to inquire with present compound residents, when the best day and time would be to find the absent resident. Fieldworkers were instructed to visit the household two additional times (thus a total of three visits). If the resident was still absent, then no further attempt was made to survey them. A record was kept of participants' consents and absences. When feasible, data on population characteristics (e.g. gender, marital status), and other relevant information were collected for participants who were absent or refused to take part in the survey.

- **Deviation from protocol**

Two socio-demographic questions were dropped from the survey questionnaire (Q.30 and 31, Appendix 4), after the one-month follow-up phase had started. Question 30 asked the number of rooms there was in the respondent's household, and question 31, the amount of rent the respondent paid. The decision to discontinue using those two items in the trial was made, after a landlord withdrew their consent for their compound to be part of the trial. A resident had informed the compound landlord of the two questions being asked, which annoyed the landlord. Whilst the compound withdrew its consent from the trial activities linked to the fieldworkers, the intervention providers reported that they had no problems continuing their activities in the compound. This suggested that the methods used to prevent study participants linking the two trial teams were effective.

3.4. Process Evaluation

During intervention delivery and at the end of the trial, the fieldworkers and intervention providers collected data, as part of a process evaluation.

3.4.1. TNSB-based handwashing intervention process evaluation

- **TNSB-based handwashing intervention disgust triggering assessment**

In order to assess whether the videos triggered disgust feelings in participants, a vote was conducted at the end of the screening of the negative videos and before the screening of the solution videos. The vote took place between these two video showings, because the solution videos were not designed to trigger disgust as the key emotion. The vote was implemented at each intervention delivery session (i.e. at initial intervention delivery, and at one month, two months and three months post initial intervention delivery).

As part of their training, the intervention providers were taught how to conduct the vote. Eligible participants were permanent adult residents present at the screening, and who were willing to participate in voting. The intervention providers explained to participants that they would hold a vote to understand how the videos made participants feel. We used emojis to depict each answer option. Participants were instructed that a set of images would be distributed to them, and would act as ballots, such as during elections. They were told that, as in elections, voting would be anonymous. They should not therefore attempt to look at what their neighbours were voting.

Each emoji was explained to participants. Initially, there were two response options. One was an emoji depicting a neutral emotion (i.e. neither happy nor sad) (Image 9.1). Participants were told that they should use this ballot, if the videos had not triggered any feeling in them. A second emoji depicted an alarmed emotion (Image 9.2). Participants were told that they should use this ballot, if the videos had *'made them feel weird in their bodies'*, the common local expression used to express disgust. The intervention providers then went around the audience with an envelope, and participants were instructed to place only one image in the envelope.

However, after conducting voting in the first seven intervention compounds and debriefing with the intervention providers, it was decided that only having two emotions to choose from, with none being positive, would risk bias. Indeed, it seemed unlikely that the intervention had not triggered any emotion in participants. Participants could thus tend to vote for the image expressing disgust, even if this was not what they had felt. Therefore, a third voting option with a picture depicting a laughing emoji was added. Participants were told that they should use this ballot, if the videos had made them laugh (Image 9.3). Given the videos scripts had been written to include some comical elements, we thought that this third answer option was appropriate. These three answer options were used in the remainder of the trial.

After voting, the intervention providers counted the votes and the result was revealed to participants, before moving to the next section of the intervention. In order to verify that voting for the disgust emoji really meant participants had been disgusted by the videos, intervention providers asked willing participants to explain what they meant when they voted that the videos had *'made them feel weird in their bodies.'* The expressions they used to explain what they meant were then recorded on the questionnaire. The questionnaire was implemented at each intervention video screening.

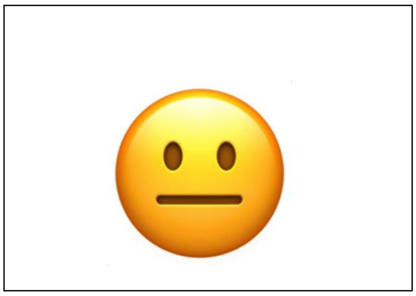


Image 9.1. Neutral emoji answer option

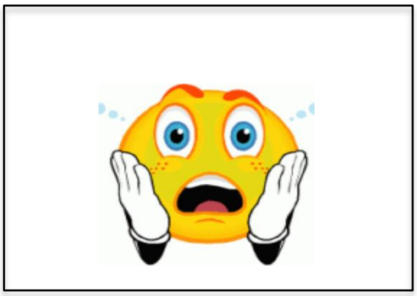


Image 9.2. Disgust emoji answer option

Image 9.3. Laughing emoji answer option

- **TNSB-based handwashing intervention process evaluation form**

A process evaluation questionnaire was also administered at household level to eligible residents, in compounds having received the TNSB-based handwashing intervention (Appendix 9.5). This was only done once at the end of the trial. The aim of the questionnaire was to evaluate the intervention coverage; assess how many and which intervention components respondents had been exposed to; whether respondents remembered the primary and secondary intervention messages; and their opinion of the intervention. The process evaluation also included questions to assess whether respondents had indeed been exposed to the intervention components they mentioned. For instance, if a respondent stated that they had seen the intervention posters, they were asked for the location of the posters.

We trained the intervention providers extensively on how to administer the questionnaire and on the sampling methods. The household coding used by the fieldworkers was explained to the intervention providers, using the compound sketches. Systematic random sampling was used to select the households to be interviewed in each compound, with k=2 as the sampling interval. We used this interval so that at least one household from each trial phase (i.e. baseline, one-month and five-month follow-ups) would be sampled in each compound. We sampled 3 households in compounds with less than 9 households, and 4 households in compounds with n≥9.

3.4.2. Handwashing station questionnaires

Three data collection forms were used to collect information relating to the HWS (Appendix 9.6). As part of their training, the intervention providers were taught how to fill in the form. All HWS data collection tools were implemented at the compound level to a group of eligible residents who were present at the time of the visit, and agreed to take part in the interviews. One form (HWS delivery form) was to be completed when the HWS was first delivered to a compound. It captured information such as where the HWS had been placed in the compound (e.g. at the toilet entrance, at the centre of the compound), and how residents had decided to organise themselves to ensure that there was always water and soap at the HWS (e.g. specific person designated or turns taken by household).

A second form (HWS follow-up form) collected information such as whether there were any issues with the HWS (e.g. damaged tap, broken stand), whether it had been moved from its initial location, and whether there were any maintenance issues. This questionnaire was administered within two weeks of supplying the HWS, so that any damaged HWS could be replaced as early as possible. In the TNSB-based handwashing intervention group, there were additional questions regarding the intervention posters (e.g. whether they were still present, and if not, why). In these compounds, the questionnaire was also administered every time the intervention was implemented (at one month, two months, and three months post the initial intervention implementation).

The third form (HWS process evaluation form) was administered at the end of the trial. The form collected the same information as the HWS follow-up form, but with additional questions regarding residents' views on the HWS, and how they would improve it. The form was implemented seven months post initial intervention delivery. Based on the answers given on the questions assessing the HWS maintenance, we added an additional question to the questionnaire. The question assessed whether respondents thought that there would be less maintenance issues, if each household had its own HWS. By the time, the form was amended, all compounds in the HWS-only intervention group had already been visited. Thus, only compounds in the TNSB-based handwashing intervention group answered this question.

For the second and third forms, the intervention providers were instructed to first go to the HWS to assess whether there were water and soap at the facility, before approaching residents. This was done to minimise the risk that residents rush to replenish the HWS at the sight of the intervention providers, and while they were administering the forms.

3.4.3. Informed consent for the process evaluation

The intervention providers sought verbal informed consent from eligible residents to conduct the interviews. At the visit following the first intervention delivery session, in both intervention groups, intervention providers told compound residents that they had come back to greet residents, and inquire whether there were any issues with the HWS.

- **HWS-only study group**

In the HWS-only intervention group, at seven months post-intervention delivery, the intervention providers told residents that they had come back to greet residents, and gather their opinions of the HWS, in addition to assessing if there had been any issues with the HWS.

- **TNSB-based handwashing study group**

For the end-of-trial process evaluation, the intervention providers informed residents that they would conduct individual interviews, after the HWS group discussion mentioned before, to gather information on what residents remembered of the intervention and their opinion of the intervention. After administering the HWS process evaluation questionnaire, the intervention providers identified the households to be sampled, and sought verbal informed consent from an eligible resident to interview in each of these households. As with the fieldworkers, intervention providers were instructed to conduct the interviews in respondents' households or away from other residents.

4. Outcomes

4.1. Primary outcomes

The primary outcome measure was the observed proportion of occasions after using the toilet, on which hands were washed with soap. The primary outcome was measured at baseline and at the one-month and five-month post-intervention delivery.

4.2. Secondary outcomes

4.2.2. Binary behavioural outcomes

The secondary binary behavioural outcomes measures were:

- The observed proportion of occasions on which hands were washed with soap after toilet use, restricted to toilet visits with a container for cleansing.
- The observed proportion of occasions on which hands were washed with soap after cleaning a child's bottom;
- The observed proportion of occasions, on which any form of handwashing took place after using the toilet;
- The observed proportion of occasions, on which any form of handwashing took place after using the toilet; restricted to toilet visits with a container for cleansing;
- The observed proportion of occasions on which any form of handwashing took place after cleaning a child's bottom.

These were measured at baseline, at the one-month and at the five-month post-intervention delivery.

4.2.3. Ordered categorical behavioural outcomes

The secondary ordered categorical behavioural outcome measures were:

- The proportion of occasions on which hands were washed with soap, with water or antibacterial gel only, or not cleaned at all after using the toilet.

- The proportion of occasions on which hands were washed with soap, with water or antibacterial gel only, or not cleaned at all, after cleaning a child's bottom.

These were measured at baseline, and at the one-month and at the five-month post-intervention delivery.

4.2.4. Handwashing norms-related constructs

The norms-related constructs were the perceived descriptive and injunctive norms around HWWS after using the toilet, and perceived HWWS publicness after using the toilet. For each respondent, the mean and median of the items related to each construct were calculated, and the scales mean scores were then computed. These were measured at baseline, at the one-month and at the five-month post-intervention delivery.

5. Sample size

The sample size for this study was calculated based on the observed proportion of occasions on which hands were washed with soap after visiting the toilet, and using parameter estimates from previous HWWS studies in comparable settings [58, 89], including the 2012 pilot study. The formula below [92], was used to compute the sample size:

$$c = 1 + (z_{\alpha/2} + z_{\beta})^2 \frac{\pi_0(1-\pi_0)/m + \pi_1(1-\pi_1)/m + k^2(\pi_0{}^2 + \pi_1{}^2)}{(\pi_0 - \pi_1)^2}$$

Where:

- *β* is the power
- *z* is the standard normal distribution value for upper tail probabilities
- π_0 is the true proportion of the primary outcome in the absence of the intervention
- π_1 is the true proportion of the primary outcome in the presence of the intervention
- *m* is the harmonic mean (HM) for the number of events observed in each cluster
- *k* is the between cluster coefficient of variation

From the 2012 pilot study, the HM of events observed over a period of 4 hours was 16 for the primary outcome. The total period of observation in the actual trial was longer (6 hours). Thus, the estimated HM of 16 is likely to be conservative. We assumed that the frequency of HWWS after using the toilet would be 5% in the control group, and that it would increase to 25% in the full intervention group. With a between-cluster coefficient of variation (k) estimated at 0.25 [92], a sample size of N≥66 compounds (n≥22 compounds per arm), with 80% power, and N≥87 (n≥29 compounds per arm), 90% power, was required to detect a 20% absolute increase in HWWS after using the toilet, with α=0.05. Assuming a 10% loss to follow-up/refusal, N≥73 (n=24 compounds per study group) would be required. We used the formula [214] below to compute this estimate:

$$N' = \frac{N}{(1-q)}$$

Where:

- *N'* is the required sample size accounting for loss to follow-up
- *N* the required sample size
- *q* is the expected proportion of refusal or loss to follow-up

The maximum sample size which was feasible, due to resource constraints, was N=75 (n=25 compounds per study group). The study was not powered to detect a difference in the absolute increase in HWWS after cleaning a child's bottom, given the small number of observed events during the 2012 pilot study (HM=2) (Chapter 5).

6. Statistical methods

Quantitative data were analysed using the statistical package STATA® 15. For the primary and secondary behavioural and norms-related outcomes, the analyses were conducted for each follow-up point (i.e. at the one-month follow-up point, for short-term interventions effects, and at the five-month follow-up point, for longer-term intervention effects). Complete case analyses were performed. As four compounds did not receive the intervention to which they were assigned (three control compounds erroneously became HWS-only intervention compounds, and one HWS-only compound became a control compound), both intention-to-treat and on-treatment analyses were performed.

All statistical analyses took into account the cluster randomisation [92]. For descriptive statistics, robust standard errors were used to account for clustering, using STATA®'s *svy* command. When assessing the intervention effects, for binary outcomes, random effects logistic regression models were used to compare the key outcomes between the intervention groups and within each intervention group. Random effects models were used as the preferred method to account for clustering, as the between-cluster variation is explicitly taken into account in these models, and included in the likelihood, compared to other approaches (e.g. generalised estimating equations or robust standard errors). Compounds were included as random intercepts. The following model was used: *Logodds(outcome) = overall intercept + dummy variable for treatment arm + covariates + compound-level random effect.* As the outcomes were binary, the reliability of the estimates was checked by using the *quadchk* command in STATA®.

For non-normally distributed continuous (norms-related) outcomes, Kruskal-Wallis one-way ANOVA ranks test was used to compare continuous outcomes between the intervention groups. For ordered categorical outcomes, random effects ordered logistic regression models were fitted to look at the association between the key outcomes and the intervention groups. We tested the proportional odds assumption using the *omodel* command in STATA®.

6.1. Strategy to adjust for covariates

Both unadjusted and adjusted analyses were performed. The adjusted analysis was chosen as the primary analysis for assessing the intervention effect. Both the primary behavioural outcomes and secondary norms related outcomes were adjusted for the same covariates.

These were selected based on *a priori* knowledge from past studies (e.g. [43, 65, 89, 215, 216]). individual-level covariates were gender and age group. These were data collected from handwashing events observed during structured observations. Compound-level covariates were the median level of education of the female head of household, average level of household rent, and median level of household crowdedness (i.e. number of inhabitants per room in a given household). These were collected from the norms-related handwashing questionnaire, administered at household level. Baseline levels of the behavioural outcomes were also adjusted for.

Studies have shown associations between handwashing and age group (e.g. [65, 89]), handwashing and education level (e.g.[65, 215, 216]), and handwashing and household wealth (e.g. [43, 215, 216]). Women were the compound residents for whom there would be the most observations, and the residents who would be exposed to the intervention the most. Therefore, only their education level was considered for covariate adjustment. Household rent and crowdedness were used as proxy indicators of household wealth/socioeconomic status [217-219]. These have been used in previous handwashing and hygiene studies to explain hygiene behaviours (e.g. [43, 215, 216]).

6.2. Baseline analysis

Data collected at baseline (i.e. 2 years pre intervention delivery) were analysed to assess compounds' background characteristics, and examine baseline comparability of the trial arms with respect to the key outcomes and covariates [92].

For the norms-related constructs, descriptive statistics were computed for each item within each construct to assess the distribution of the data and identify items with highly skewed or unbalanced responses [193]. Given this was the second time the norms-related scales were used, the psychometric properties of each scale were tested again, using data from the entire trial. We did so to ensure that the scales' psychometric properties were not previously obtained by chance [192]. Confirmatory Factor Analysis (CFA) was used to assess the measurement properties of the HWIN scale [201]. Generalised structural equation modelling (GSEM) was used to fit an ordered probit model to the data for this scale [202]. The variances of the latent variables were constrained to equal 1 to obtain the loadings of each scale item. The internal consistency of each scale was assessed by either computing the Cronbach's alpha (α) and/or Spearman-Brown coefficient (ρ), depending on the number of items in the scale [203]. Confidence intervals for α and ρ were computed. The Spearman-Brown inter-item correlation coefficient was also computed to assess the strength of the relationship between pairs of items in each scale. The mean and median scores for each norms-related construct and scale item were computed at household level, by study arm.

Random effects logistic regression models were used to look at the association between HWWS after using the toilet and each norm-related construct. For each construct, the adjusted analysis comprised the household-level and compound-level covariates stated above, as well as the level of the other two norms-related constructs. The following model was used: *Logodds(outcome) = overall intercept + dummy variable for treatment arm + covariates + compound-level random effect*.

6.3. One-month and five-month follow-up time-points

At each follow-up point (one month and five months), the following analyses were performed.

6.3.1. Primary and secondary behavioural outcomes

Random effects logistic regression models were used to estimate the interventions' effects on the primary and secondary handwashing outcomes at each trial phase. Both unadjusted and adjusted ORs were estimated. p-values were obtained using likelihood-ratio tests. For ease of comparability of results with the studies included in the systematic review reported in Chapter 3, risk ratios (RRs) were also computed using the xtgee command in STATA® with a log link, thus using a binomial regression, and adjusting for clustering and covariates.

Random effects ordered logistic regression models were fitted to examine the association between the three-level categorical handwashing outcome after using the toilet, (i.e. with soap (ranked 3), with water or antibacterial gel (ranked 2), and no handwashing (ranked 1), and the interventions. Both unadjusted and adjusted ORs were estimated. p-values were obtained using likelihood- ratio test. The same analysis was performed for the handwashing occasion 'after cleaning a child's bottom.'

6.3.2. Secondary norms-related outcomes

Descriptive statistics were computed for each variable to assess the distribution of the data and identify items with highly skewed or unbalanced responses [193]. The frequency distribution of each norm-related scale item was examined, and the mean and median scores of each norm-related construct computed. Random effects logistic regression models were used to look at the association between HWWS after using the toilet and each norm-related construct. Both unadjusted and adjusted ORs were estimated. P-values were obtained using likelihood-ratio tests. The same approach was used to assess the changes over time in each norm-related construct, and to look at the association between the way respondents rated the scales items and the intervention arms.

6.4. Process evaluation

6.4.1. Disgust-triggering intervention effect

The TNSB-based intervention's ability to trigger disgust as the dominant emotion was measured at the initial intervention delivery time-point, and one month, two months and three months post initial intervention delivery. The proportion of participants who picked disgust as the main emotion that the intervention videos triggered was computed, using robust standard errors to account for clustering, and calculate 95% CIs.

6.4.2. Process evaluation questionnaire

Descriptive statistics were used to analyse the data from the TNSB-based intervention process evaluation questionnaire. To assess the intervention coverage, we calculated the proportion of respondents who said that they were present during intervention delivery. We also calculated he proportion of respondents who remembered the primary and secondary intervention messages. Robust standard errors were used to account for clustering, and to compute 95% CIs around each proportion.

6.5. Handwashing station sustainability

The proportion of handwashing stations which had water and soap was computed for the TNSB-based handwashing intervention group and the HWS-only intervention group, at the one-month, five-month and seven-month post-intervention delivery. 95% CIs were computed. Pearson's Chi-Squared test was used to compare the proportions between the two treatment groups at each time point.

7. Qualitative data analysis

The qualitative data collected in the trial pertained to open questions around the HWS (i.e. HWS maintenance issues, participants' opinions of the HWS and suggestions for improvement), and participants' opinions and suggestions regarding the interventions. Data were transcribed using Microsoft Word, at the end of each data collection day. Data were analysed, using content analysis, and while data collection was on-going. Preliminary codes emerged from reading the first few transcripts. These were then used as coding schemes to code the remaining transcripts [179, 180]. From the codes, categories that were mutually exclusive were created. These were used to organise the coded data, with similar concepts grouped together and counted [179-182]. For instance, regarding qualitative data around participants' suggestions to improve the HWS, one of the coding categories which emerged was 'HWS design improvement'. Under this code, 'HWS stand improvement' and 'water evacuation system improvement' were some of the subcategories which emerged. When needed, new codes and categories were created, when the data did not fit into the existing ones [179]. Data within each coding category were then examined, to assess whether the data could be further classified [179, 180].

8. Quality assurance and control

The PhD candidate and fieldwork assistants were in charge of quality assurance and control. The trial and all data were also available for auditing by LSHTM Quality Assurance Manager.

8.1. Trial data collected by the fieldworkers

60% of fieldworker data collection visits were monitored. This was done to ensure that fieldworkers were in their compound at the expected times, and collecting data as intended. For structured observations, we visited compounds unannounced and observed how fieldworkers were collecting data, and whether they were missing any relevant events that were taking place whilst we were there. For the interviews, we monitored part of each fieldworker's interview.

Each data collection session was immediately followed by a debriefing session with all the fieldworkers present. The PhD candidate and fieldwork assistants double checked the data collected by each fieldworker. For the questionnaire, when there was missing data and depending on the type of data missing, fieldworkers were asked to revisit respondents as soon as possible (i.e. on the same day, if it was not too late; or the next day) to collect the missing information, unless the respondent had refused to answer the question. If the data were missing on scale items, fieldworkers could not go back to collect the information again, as this would have brought the respondent's attention on handwashing. There were no missing data for the structured observations. Debriefing sessions were also an opportunity for fieldworkers to discuss any issues they had encountered during data collection, and noted in their notebooks.

8.2. Trial data collected by the intervention providers

As mentioned in Chapter 8, we could not monitor the interventions' delivery for masking reasons. A recorder was given to each intervention provider, and they were instructed to record each of their intervention delivery sessions. The PhD candidate, then listened to the recorded sessions, and discussed with intervention providers if there were any issues with the way they were implementing the interventions (e.g. issues with the type of answers they provided to residents, how convincing they sounded).

At the end of each intervention delivery day, a debriefing session took place, during which the PhD candidate and fieldwork assistants doubled checked the data collected by the intervention providers. If there were mistakes, we either explained to them or had them explain to us why these were mistakes, and how to correct the mistakes. Corrections were made directly on the data collection tools, and initialled. When, rarely, there were missing data in the data collection tools, intervention providers were able to remember the missing information, using the collected data. Debriefing sessions were also an opportunity for the intervention providers to discuss any issues they had encountered during intervention delivery. When necessary (see Section 3.4.1), the PhD candidate amended the intervention delivery protocol, based on feedback from the intervention providers.

9. Data management

9.1. Confidentiality

All hard copies of the data collected were kept in a locked cabinet, at the PhD candidate's residence in Abidjan. The PhD candidate was the only person who had access to the cabinet. All forms were anonymous, as respondents' names were not recorded.

9.2. Data entry

Data were double entered by two data entry clerks in Abidjan, as the trial was on-going, using EpiInfo 7. Two computers were purchased for the purpose of the trial, and were password-protected. Data was backed up on the PhD candidate's external drive and the LSHTM network drive, both password protected. The PhD candidate performed data entry checks of all the data entered, every two weeks, throughout the trial.

10. Financing and Insurance

This trial was self-funded and insured by LSHTM.

11. Publication Policy

Papers reporting the trial findings will be written for publication by the PhD candidate and in collaboration with her supervisory committee.

Chapter 10 - An Analysis of the Effect of the Interventions on Handwashing Practices

We conducted a randomised controlled trial in six neighbourhoods in Koumassi commune, Abidjan, Côte d'Ivoire, from August 2014 until April 2017. The aim of the trial was to assess the effectiveness of the Theory of Normative Social Behaviour (TNSB)-based and handwashing station (HWS)-only interventions at increasing handwashing with soap (HWWS) after using the toilet (*primary outcome*) and after cleaning a child's bottom (*secondary outcome*), compared to control/no intervention. Figure 10.1 shows the trial flow diagram.

1. Baseline comparability

1.1. Study population characteristics

At baseline, we collected population characteristics data on 175 households (between 57 to 60 households surveyed per intervention group), out of the 203 households to be surveyed (Table 10.1). This was due to refusals (six (3%) households) and absences (22 (11%) households). Refusals were mainly from Hausa households (from Niger). Of the 22 absences, 13 (59%) were from single-person households, including nine (63%) males, three (23%) females, and one (8%) unknown. Regarding households with only one inhabitant, single males tended to return to their compound from work late (e.g. after 9 p.m.). In the case of single-female households, whilst other residents had provided us with a best day and/or time to encounter the absent residents, all three attempts on our part failed. We were not successful in collecting data for the remaining 9 (41%) households absent.

The characteristics of compounds, households and individuals were generally similar across study arms (Table 10.1). The median number of households per compound was 8.5 (IQR: 7-11) in the control group, nine (IQR: 7-12) in the HWS-only intervention group, and nine (IQR: 7-13) in the TNSB-based intervention group. The median household family size (≤5) and median number of rooms per household (2) were similar across study groups. Respondents were predominantly Muslim (greater than 60% in all study arms) and married (greater than 60% in all study arms).

There were some imbalances regarding the presence of at least one child under five years old in the household, radio ownership and the level of education of the female head of household. In the control arm, a higher proportion of surveyed households had at least one child under five years old (57%), compared to the HWS-only trial arm (44%), and the TSNB-based trial arm (43%). Radio ownership tended to be lower in the TNSB-based intervention arms (52%) compared to the other trial arms (62% and 60% in the control and HWS-only arms respectively). In the TNSB-based intervention group, a higher proportion of female heads of household surveyed had attended school (73%), compared to 61% in the HWS-only trial arm, and 50% in the control arm. Nevertheless, the number of households sampled per trial arm was small, which may explain the imbalances observed.

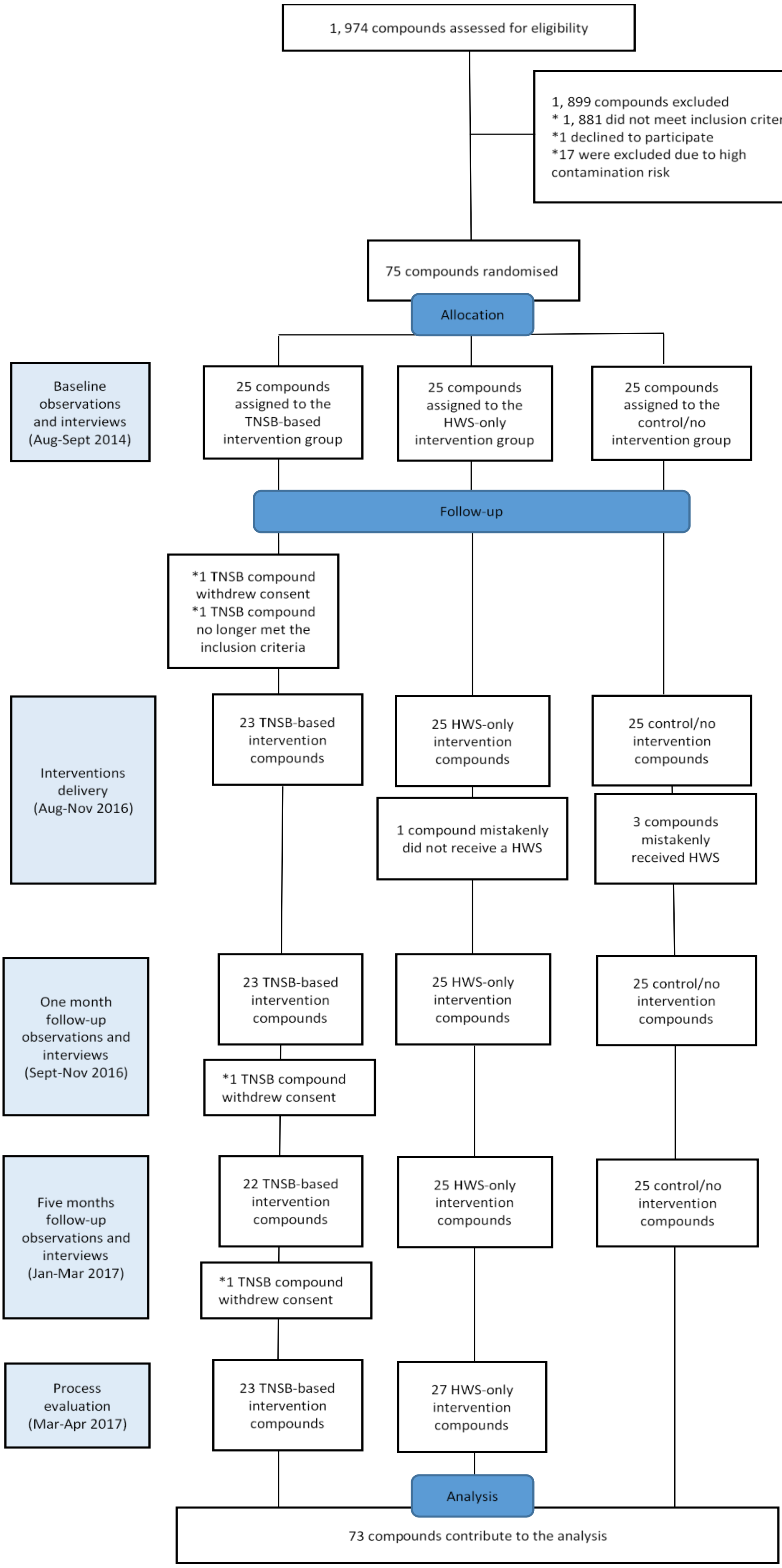


Figure 10.1. Trial flow diagram

Table 10.1. Baseline characteristics of compounds, households and individuals by trial arm (August-September 2014)

Characteristics	Control arm *n* (%)	HWS-only arm *n* (%)	TNSB-based arm *n* (%)
Compound-level characteristics			
Number of compounds	25	25	25
Median number of households (range)	8.5 (7-11)	9 (7-12)	9 (7-13)
Household-level characteristics			
Number of households to be surveyed	68	65	70
Number of households actually surveyed	58 (85.3)	57 (87.7)	60 (85.7)
Median family size per arm (range)	5 (1-11)	5 (1-13)	4 (1-11)
Presence of at least one child under 5 years old	33 (56.9)	25 (43.9)	26 (43.3)
Median number of rooms per household (range)	2 (1-3)	2 (1-4)	2 (1-3)
Crowding (n (%))			
≤1 person/room	6 (10.3)	6 (10.5)	9 (15.0)
>1 and 2≤ people/room	15 (25.9)	11 (19.3)	14 (23.3)
>2 people/room	37 (63.8)	40 (70.2)	37 (61.7)
Median household rent (range)	20 000 (0-60 000)*	17 500 (0-51 000)**	17 000 (0-50 000)***
TV ownership *(n (%))*	49 (84.5)	50 (87.7)	55 (91.7)
Radio ownership *(n (%))*	36 (62.1)	34 (59.7)	31 (51.7)
Individual-level characteristics			
Number of individuals surveyed	58	57	60
Religion			
Christian *(n (%))*	20 (34.5)	17 (29.8)	18 (30.0)
Muslim *(n (%))*	37 (63.8)	39 (68.4)	41 (68.3)
Other *(n (%))*	1 (1.7)	1 (1.8)	1 (1.7)
Marital status			
Married/cohabiting *(n (%))*	36 (62.1)	38 (66.7)	36 (60.0)
Single *(n (%))*	22 (37.9)	19 (33.3)	24 (40)
Female head of household education level			
No schooling *(n (%))*	24 (50.0)[a]	18 (39.1)[b]	12 (26.7)[c]
Primary *(n (%))*	17 (35.4)[a]	16 (34.8)[b]	21 (46.7)[c]
Secondary *(n (%))*	6 (12.5)[a]	8 (17.4)[b]	11 (24.4)[c]
Higher *(n (%))*	1 (2.1)[a]	4 (8.7)[b]	1 (2.2)[c]

*Missing data for 4 households
**Missing data for 5 households
***Missing data for 6 households
[a] No female head of household in 10 households
[b] No female head of household in 11 households
[c] No female head of household in 15 households

1.2. Observed handwashing with soap practices at baseline

HWWS was uncommon in all trial arms at baseline.

1.2.1. Handwashing with soap after using the toilet

We observed 2,117 occasions on which the toilet was visited (Table 10.2). Hands were washed with soap on 40 (6%) of 698 occasions in the control group, 24 (3%) of 710 occasions in the HWS-only intervention group, and 46 (7%) of 709 occasions in the TNSB-based handwashing intervention group.

1.2.2. Handwashing with soap after cleaning a child's bottom

The observed number of occasions when a child's bottom was cleaned was rather small (only 148 occasions observed in 62 compounds) (Table 10.2). Hands were washed with soap on 11 (22%) of 49 occasions in the control group, nine (20%) of 46 occasions in the HWS-only intervention group, and 18 (34%) of 53 occasions in the TNSB-based trial arm. Although the point estimates look different, the sample size in each arm is small, and the confidence intervals overlap substantially.

We had performed additional analyses restricted to HWWS after using the toilet for people who went with a container for cleansing. We also performed analyses where we looked at any handwashing (i.e. with water, gel or soap) after the key occasions. The details are presented in Appendices 10.2 to 10.4. The findings did not change the conclusion of the trial.

1.2.3. Masking assessment

At baseline, the results of the masking assessment were comparable across trial arms. Among the respondents,19 (33%) of 58 interviewees in the control arm, 16 (28%) of 57 respondents in the HWS-only intervention group, and 18 (30%) of 60 respondents in the TNSB-based intervention group cited hygiene-alone or coupled with another non-hygiene subject as the key theme of the questionnaire.

Table 10.2. Baseline observed handwashing behaviours after toilet use and after cleaning a child's bottom by trial arm (August-September 2014) (Intention-to-treat)

Event	Control arm n	% (95% CI)	HWS-only arm n (%)	% (95% CI)	TNSB-based arm n (%	% (95% CI)
Use of the toilet						
Number of events observed						
Total	698		710		709	
Female	402	57.6	351	49.4	411	58.0
Male	296	42.4	359	50.6	298	42.0
Adults (16≥)	561	80.4	574	80.8	588	82.9
Children	137	19.6	136	19.2	121	17.1
Hands washed with soap	40	5.7 (4.2-7.7)	24	3.4 (1.9-5.9)	46	6.5 (4.2-9.8)
Hands washed with water only or gel	205	29.4 (23.9-35.5)	189	26.6 (18.6-36.7)	199	28.1 (22.1-34.9)
Hands not washed	453	64.9 (58.6-70.7)	497	70.0 (59.6-78.6)	464	65.4 (58.6-71.7)
Cleaning a child's bottom						
Number of events observed						
Total	49		46		53	
Female	46	93.9	45	97.8	51	96.2
Male	3	6.1	1	2.2	2	3.8
Adults (16≥)	36	73.5	38	82.6	45	84.9
Children	13	26.5	8	17.4	8	15.1
Hands washed with soap	11	22.5 (11.7-38.7)	9	19.6 (10.2-34.4)	18	34.0 (16.1-57.9)
Hands washed with water only or gel	15	30.6 (18.3-46.6)	13	28.3 (16.4-44.2)	13	24.5 (13.7-40.0)
Hands not washed	23	46.9 (29.0-65.7)	24	52.2 (35.3-68.6)	22	41.5 (23.7-62.9)

2. Effect of the interventions on handwashing practices

As mentioned in Chapter 9, the trial was initially scheduled to last approximately 18 months. However, we encountered major issues with the production company contracted to produce the short intervention video-clips. This delayed the study by approximately a year. Thus, the interventions were not implemented until two years after the baseline study. Follow-ups to observe handwashing behaviour took place at the one-month and five-month follow-up rounds post initial intervention delivery.

Over the course of the two follow-up rounds, three compounds in the TNSB-based intervention arm withdrew their consent to take part in the observations and survey (Figure 10.1). Before intervention implementation, one compound withdrew consent to be part of the observations and surveys. During the one-month follow-up round, a second compound withdrew its consent to take part in the observations and survey, but did not withdraw from the intervention. The withdrawal happened after one observation session and some interviews had been conducted in the compound. This was due to discontent regarding some of the questionnaire's content (i.e. questions on rent and the number of rooms in the household) (Chapter 9). During the second follow-up round, a third compound withdrew its consent to be part of the observations and survey after one observation session had been conducted in the compound. However, the compound did not withdraw from the intervention.

- **Deviation from protocol**

As mentioned in Chapter 9 and shown in Figure 10.1, during intervention implementation, three control compounds mistakenly received HWS, and one HWS-only intervention compound mistakenly did not receive an HWS. As a result, both intention-to-treat and on-treatment analyses were performed. The results of the on-treatment analysis are presented in Appendix 10.1.

2.1. Study population characteristics

There were some modifications in compounds' physical structure between baseline and the two follow-up rounds, with compounds moving towards having more private spaces. As a result, one compound in the TNSB-based trial arm was excluded, as individual toilet within households replaced a shared compound toilet (Figure 10.1). Similarly, and across study groups, the number of households per compound with screens erected to create some privacy increased. As a result, some compounds exceeded the original inclusion criterion of a maximum of two households with screens per compound. We, however, chose not to exclude these compounds, as experience during the baseline observations showed that the screens did not obstruct fieldworkers view when recording handwashing events. There were also some small fluctuations in the numbers of households in some compounds (Tables 10.3 and 10.4).

2.1.1. One-month follow-up

At the one-month follow-up round, data on socio-demographic characteristics were collected from 249 households, with between 81 to 85 households surveyed per intervention arm, out of the 297 households which were selected to be surveyed (Table 10.3). For the households where interviews could not be completed, this was due to refusals (nine (3%) mainly from Hausa households), and absences (37 (12%)). The remaining two (1%) households were excluded, as one single-male occupant was not mentally fit to take part in the survey, and the other household's single-male occupant only spoke English. We were able to gather some information on 15 (41%) of 37 absent households. All 15 were single-person households, including 14 (93%) males, and one (7%) of unknown gender.

The trial arms remained generally balanced. The respondents were still mostly Muslims, and the heads of household surveyed were still predominantly married. The proportion of female heads of households who had attended school was comparable across study groups, as opposed to the imbalance observed at baseline.

There were however some imbalances between the arms at household- and individual -level, as observed at baseline. For instance, The TNSB-based handwashing intervention group had a higher proportion of surveyed households with at least one child under five years old, and a greater proportion of radio ownership compared to the other trial arms. Equal proportions of surveyed individuals were Muslims or Christians in the control arm (both 49%), while in the other trial arms the majority of surveyed individuals were Muslims. However, the number of households sampled per trial arm was small. This may explain the imbalances observed.

2.1.2. Five-month follow-up

At the five-month follow-up round, we collected socio-demographic data on 99 households (between 30 to 35 households surveyed per intervention arm), out of the 131 households which were planned to be surveyed (Table 4). The reasons for the inability to complete interviews in some households were refusals (6 (5%)) mainly from Hausa households), and absences (24 (18%)). The remaining two (2%) households were excluded, as the inhabitants only spoke Hausa, which was not understood by the fieldworkers. We were able to gather some information on 14 (58%) of 24 absent households. All were single-male households.

The study groups were similar regarding compound and household-level characteristics. There were some imbalances at individual-level regarding religion and the education level of the female heads of household. In the control group, the proportion of heads of household who were Christians was higher (53%) than in the HWS-only intervention arm (37%) and the TNSB-based handwashing intervention arm (33%). The proportion of females who had attended school in the HWS-only intervention arm (57%) was also lower than in the TNSB-based intervention arm (77%) and the control arm (74%). As with the one-month follow-up, the imbalances observed may be explained by the small number of households surveyed per trial arm.

Table 10.3. Characteristics of compounds clusters, households and individuals by study arm at the one-month follow-up round (September-November 2016) (Intention-to-treat)

Characteristics	Control arm *n* (%)	HWS-only arm *n* (%)	TNSB-based arm *n* (%)
Compound-level characteristics			
Number of compounds	25	25	23
Median number of households (range)	8 (6-12)	8 (7-12)	9 (7-13)
Household-level characteristics			
Number of households to be surveyed	100	102	95
Number of households actually surveyed	83 (83.0)	85 (83.3)	81 (85.3)
Median family size per arm (range)	4 (1-20)	4 (1-19)	5 (1-13)
Presence of at least one child under 5 years old	35 (42.2)	38 (44.7)	29 (35.8)
TV ownership *(n (%))*	75 (91.5)*	76 (89.4)	75 (93.8)*
Radio ownership *(n (%))*	37 (45.1)*	42 (50.0)*	46 (56.8)
Individual-level characteristics			
Number of individuals	83	85	81
Religion			
Christian *(n (%))*	31 (37.4)	42 (49.4)	22 (27.2)
Muslim *(n (%))*	51 (61.5)	42 (49.4)	57 (70.4)
Other (n ((%))	1 (1.2)	1 (1.2)	2 (2.4)
Marital status			
Married/cohabiting *(n (%))*	64 (77.1)	65 (76.5)	55 (67.9)
Single *(n (%))*	19 (22.9)	20 (23.5)	26 (32.1)
Female head of household education level			
No schooling *(n (%))*	22 (31.9)[a]	21 (30.4)[b]	24 (35.3)[c]
Primary *(n (%))*	30 (43.5)[a]	24 (34.8)[b]	27 (39.7)[c]
Secondary *(n (%))*	15 (21.7)[a]	20 (29.0)[b]	14 (20.6)[c]
Higher *(n (%))*	2 (2.9)[a]	4 (5.8)[b]	3 (4.4)[c]

*Missing data for 1 household
[a]No female head of household in 14 households
[b]No female head of household in 16 households
[c]No female head of household in 13 households

Table 10.4. Characteristics of compounds clusters, households and individuals by trial arm, at the five-month follow-up round (January-March 2017) (Intention-to-treat)

Characteristics	Control arm *n* (%)	HWS-only arm *n* (%)	TNSB-based arm *n* (%)
Compound-level characteristics			
Number of compounds	25	25	22
Median number of households (range)	9 (7-13)	8 (6-12)	9.5 (7-13)
Household-level characteristics			
Number of households to be surveyed	40	51	40
Number of households actually surveyed	34 (85.0)	35 (68.6)	30 (75.0)
Median family size per arm (range)	4 (1-14)	5 (1-14)	5 (1-11)
Presence of at least one child under 5 years old	12 (35.3)	12 (34.3)	8 (26.7)
TV ownership *(n (%))*	32 (94.1)	32 (91.4)	29 (96.7)
Radio ownership *(n (%))*	22 (64.7)	23 (65.7)	18 (60.0)
Individual-level characteristics			
Number of individuals	34	35	30
Religion			
Christian *(n (%))*	18 (52.9)	13 (37.1)	10 (33.3)
Muslim *(n (%))*	16 (47.1)	22 (62.9)	20 (66.7)
Marital status			
Married/cohabiting *(n (%))*	26 (70.3)	32 (74.4)	27 (73.0)
Single *(n (%))*	11 (29.7)	11 (25.6)	10 (27.0)
Female head of household education level			
No schooling *(n (%))*	7 (26.0)[a]	13 (43.4)[b]	6 (23.1)[c]
Primary *(n (%))*	10 (37.0)[a]	13 (43.3)[b]	9 (34.6)[c]
Secondary *(n (%))*	8 (29.6)[a]	3 (10.0)[b]	8 (30.8)[c]
Higher *(n (%))*	2 (7.4)[a]	1 (3.3)[b]	3 (11.5)[c]

[a]No female head of household in 7 households
[b]No female head of household in 5 households
[c]No female head of household in 4 households

Deviation from protocol

As there was some data missing on some of the covariates, and compounds where some handwashing occasions had not been observed, STATA® excluded some compounds from the regression analyses by default. Given this reduced the precision of our handwashing estimates, we decided to remove the covariates which did not lead to important changes in the odds ratios. These were household crowdedness and household rent. Thus, the four covariates included in the analyses were sex, age group, 'education level of the female head of household', and baseline handwashing estimates.

2.2. Effect of the interventions on handwashing practices after using the toilet (Intention-to-treat analysis)

We observed 1,749 and 1,345 occasions on which the toilet was used, with or without a container for cleansing, at the one-month and five-month follow-up rounds respectively (Table 10.5). This was fewer occasions than were observed at baseline (2,117 occasions). The decline in the number of occasions observed was seen in all three trial arms.

2.2.1. Handwashing with soap

- *Control group*

We observed minimal change from baseline (6%) in the proportion of occasions when hands were washed with soap after using the toilet in the control arm, at the one-month (29 (5%) of 604 occasions) and five-month (30 (7%) of 437 occasions) follow-up rounds (overall *P*=0.20) (Figure 10.2) (Table 10.5).

- *HWS-only intervention group*

In the HWS-only intervention arm, there was strong evidence of a change in the proportions of occasions at which hands were washed with soap after using the toilet. The proportion rose from 3% at baseline to 9% at both the one-month (49 of 557 occasions) and five-month (40 of 456 occasions) follow-up rounds (overall *P*=0.001) (Figure 10.2) (Table 10.5).

- *TNSB-based intervention group*

In the TNSB-based intervention group, there was strong evidence of a change in the proportions of occasions at which hands were washed with soap after using the toilet. The proportion went from 7% at baseline to 24% (143 of 588 occasions) and 22% (98 of 450 occasions), at the one-month and five-month follow-up rounds respectively (overall $P<0.001$) (Figure 10.2) (Table 10.5). Figure 10.2 shows the pattern of HWWS after using the toilet in each trial arm and by trial phase.

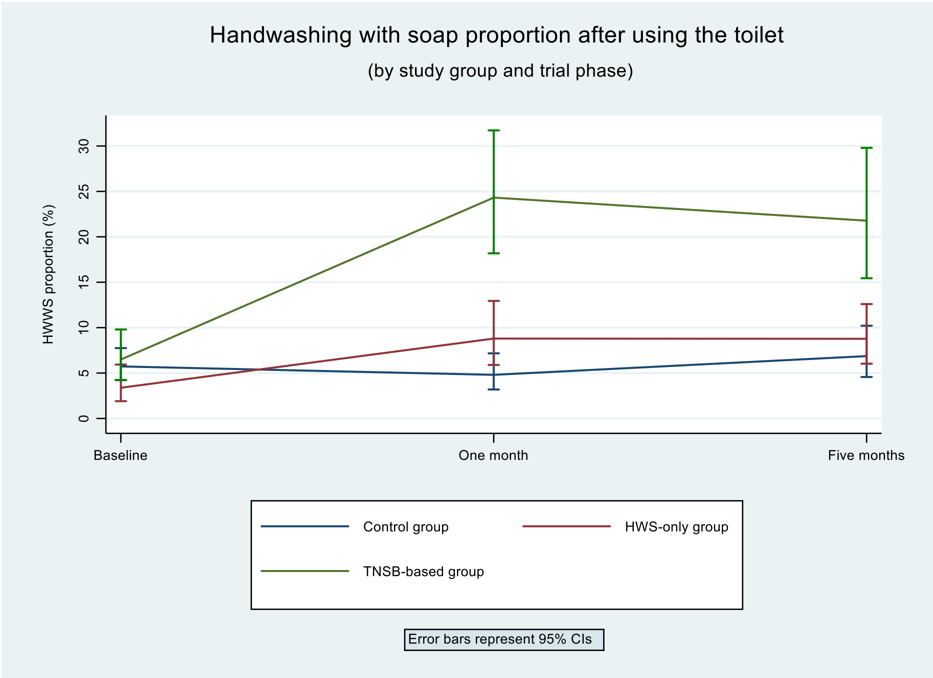


Figure 10.2. HWWS after using the toilet pattern by trial arm and trial phase

Table 10.5. Observed handwashing behaviours after using the toilet, by trial phase and trial arm (Baseline: August-September 2014; One-month follow-up: September-November 2016; Five-month follow-up: January-March 2017) (Intention-to-treat)

Event	Control arm n	% (95% CI)	HWS-only arm n (%)	% (95% CI)	TNSB-based arm n (%)*	% (95% CI)
Baseline						
Number of events observed						
Total	698		710		709	
Hands washed with soap	40	5.7 (4.2-7.7)	24	3.4 (1.9-5.9)	46	6.5 (4.2-9.8)
Hands washed with water only or gel	205	29.4 (23.9-35.5)	189	26.6 (18.6-36.7)	199	28.1 (22.1-34.9)
Hands not washed	453	64.9 (58.6-70.7)	497	70.0 (59.6-78.6)	464	65.4 (58.6-71.7)
One-month follow-up						
Number of events observed						
Total	604		557		588	
Female	328	54.3	269	48.2	337	57.3
Male	276	45.7	288	51.8	251	42.7
Adults (16≥)	492	81.5	467	83.8	490	83.3
Children	112	18.5	90	16.2	98	16.7
Hands washed with soap	29	4.8 (3.2-7.2)	49	8.8 (6.3-13.4)	143	24.3 (18.2-31.7)
Hands washed with water only or gel	183	30.3 (24.1-37.3)	199	35.7 (25.3-39.40)	195	33.2 (24.9-42-7)
Hands not washed	392	64.9 (58.4-70.9)	309	55.5 (47.9-65.1)	250	42.5 (35.4-49.9)
Five-month follow-up						
Number of events observed						
Total	437		456		450	
Female	217	49.7	255	55.9	242	53.8
Male	220	50.3	201	44.1	208	46.2
Adults (16≥)	363	83.1	356	78.1	363	80.7
Children	74	16.9	100	21.9	87	19.3
Hands washed with soap	30	6.9 (4.6-10.2)	40	8.8 (6.0-12.6)	98	21.8 (15.4-29.8)
Hands washed with water only or gel	137	31.3 (22.5-41.9)	104	22.8 (16.5-30.7)	122	27.1 (19.4-36.6)
Hands not washed	270	61.8 (51.3-71.3)	312	68.4 (60.2-75.6)	230	51.1 (43.3-58.8)

2.2.2. Effect of the interventions on handwashing with soap practices after using the toilet

We found strong evidence that the proportion of occasions at which hands were washed with soap varied between the trial arms at the one-month (*P*<0.001) and five-month (*P*=0.0006) follow-up rounds (Table 10.6). After controlling for covariates, compound residents who received the HWS-only intervention had 2.00 (95% CI: 1.03-3.90) times the odds of HWWS after using the toilet (risk ratio (RR)=1.89, 95% CI: 1.16-3.08)[21], compared to compound residents in the control arm, one month post-intervention delivery. In the TNSB-based intervention arm, compound residents had 7.17 (95% CI: 3.91-13.12) times the odds of HWWS after using the toilet, compared to compound residents in the control arm (RR=4.82, 95% CI: 3.06-7.59). Five months post-intervention delivery, there was no evidence of a difference in the odds of HWWS after using the toilet in the HWS-only intervention group, compared to the control group (adjusted OR: 1.01, 95% CI: 0.50-2.04 and adjusted RR=1.06, 95% CI: 0.63-1.79). By contrast, in the TNSB-based intervention group, the odds of HWWS after using the toilet were still greater than those in the control arm (adjusted OR: 3.11, 95% CI: 1.62-6.00 and adjusted RR=2.68, 95% CI: 1.65-4.34).

2.2.3. Observed use of handwashing stations after using the toilet

At baseline, compounds did not have specific handwashing locations or formal facilities where residents washed their hands after using the toilet. In the intervention groups, a high proportion of residents who washed their hands with soap used the HWS, at both follow-up rounds (115 (59%) of 196 and 79 (56%) of 141 HWWS events, at the one-month and five-month follow-up rounds respectively) (Table 10.7) (Pictures 10.1, 10.2, 10.3 and 10.4). The proportion was higher in the TNSB-based trial arm (95 (66%) of 143 and 62 (63%) of 98 HWWS events, at the one-month and five-month follow-up rounds respectively), compared to the HWS-only trial arm (20 (38%) of 53 and 17 (40%) of 43 HWWS events, at the one-month and five-month follow-up rounds respectively). By contrast, using informal handwashing facilities remained residents' most common practice, when washing hands with water at the one-month (374 (96%) of 391) and five-month (206 (94%) of 222) follow-up rounds (Table 10.7).

[21] As mentioned in Chapter 9, for comparison purposes, the odds ratios for the intervention effect on HWWS after toilet use, and after cleaning a child's bottom were converted into risk ratios.

Picture 10.1: A woman HWWS at the HWS after using the toilet

Picture 10.2: A girl HWWS at the HWS after using the toilet

Picture 10.3: A woman HWWS at the HWS after using the toilet

Picture 10.4: A man HWWS at the HWS after using the toilet

Table 10.6. Unadjusted and adjusted analyses of the association between the interventions and handwashing with soap practices after using the toilet, by follow-up phases *(Intention-to-treat random effects logistic model)*

HWWS vs no HWWS			
One-month follow-up (n=71 compounds)			
Unadjusted			
	OR (95% CI)	p-value LRT	Overall model p-value
Interventions			
Control	1.0		
HWS-only	2.01 (1.01-4.00)	0.05	<0.0001
TNSB-based	7.54 (4.00-14.33)	<0.0001	
Five-month follow-up (n=70 compounds)			
Interventions			
Control	1.0		
HWS-only	1.35 (0.68-2.67)	0.40	0.0001
TNSB-based	3.71 (1.93-7.10)	<0.0001	
*Adjusted**			
One-month follow-up (n=71 compounds)			
Interventions			
Control	1.0		
HWS-only	2.00 (1.03-3.90)	0.04	<0.0001
TNSB-based	7.17 (3.91-13.12)	<0.0001	
Five-month follow-up (n=70 compounds)			
Interventions			
Control	1.0		
HWS-only	1.01 (0.50-2.04)	0.97	0.0006
TNSB-based	3.11 (1.62-6.00)	0.001	
*Adjusted for sex, age group, education level of the female head of household, and baseline handwashing estimates			

Table 10.7. Observed toilet occasions when hands were washed with soap and with water-only using the handwashing station, by trial phase (on-treatment)

	HWS-only n	% (95% CI)	TNSB-based n	% (95% CI)	Intervention groups combined n	% (95% CI)
One-month follow-up						
Number of events observed Total	595		588		1, 229	
Hands washed with soap	53		143		196	
HWS used	20	38 (20-59)	95	66 (54-77)	115	59 (48-69)
Hands washed with water or gel	196		195		391	
HWS used	6	3 (0-19)	11	6 (2-13)	17	4 (2-10)
Five-month follow-up						
Number of events observed Total	468		450		918	
Hands washed with soap	43		98		141	
HWS used	17	40 (20-62)	62	63 (49-76)	79	56 (43-68)
Hands washed with water or gel	100		122	27 (19-37)	222	
HWS used	9	9 (3-25)	5	4 (2-9)	14	6 (3-13)

2.4. Effect of interventions on handwashing practices after cleaning a child's bottom (Intention to treat analysis)

We observed few occasions on which a child's bottom was cleaned: 77 occasions (in 36 compounds) and 66 occasions (in 32 compounds), at the one-month and five-month follow-up rounds respectively (Table 10.8). This was also lower than the number of occasions observed at baseline (148 occasions).

- **Deviation from protocol**

Given the small number of observed occasions after which a child's bottom was cleaned, we chose to only adjust for the baseline estimates rather than multiple factors. This was due to the fact there were too few observations to support a complex model.

When computing risk ratios, using generalised estimating equations (GEE) and adjusting for covariates, the model did not converge. We thus computed the estimates without accounting for baseline proportions.

2.4.1. Handwashing with soap

- *Control group*

While the frequency of HWWS after cleaning a child's bottom appeared to have increased at one-month (9 (31%) of 29 of occasions), and five-month (9 (41%) of 22 occasions) follow-up rounds compared to the baseline estimates (23%) (Figure 10.3) (Table 10.8), there was little statistical evidence of change (*P*=0.15).

- *HWS-only intervention group*

In the HWS-only intervention arm, there was some evidence of change in the proportion of occasions at which hands were washed with soap after cleaning a child's bottom, at the one-month follow-up round (*P*=0.02). The HWWS proportion went from 20% at baseline to 46% (13 of 28 occasions) (Figure 10.3) (Table 8). At the five-month follow-up round, there was little evidence of change in the HWWS proportion observed (9 (35%) of 26 occasions) compared to the baseline estimate (*P*=0.25).

- *TNSB-based intervention group*

In the TNSB-based intervention group, there was weak evidence of a change in the proportions of occasions at which hands were washed with soap after cleaning a child's bottom, at the one-month and five-month follow-up rounds (overall *P*=0.06). The observed HWWS proportion went from 34% at baseline to 65% (13 of 20 occasions) one-month post-intervention delivery. Five months post-intervention delivery, the observed HWWS proportion (7 (39%) of 18 occasions) was comparable to that observed at baseline (Figure 10.3) (Table 10.8). Figure 10.3 shows the pattern of HWWS after cleaning a child's bottom by trial arm and by trial phase.

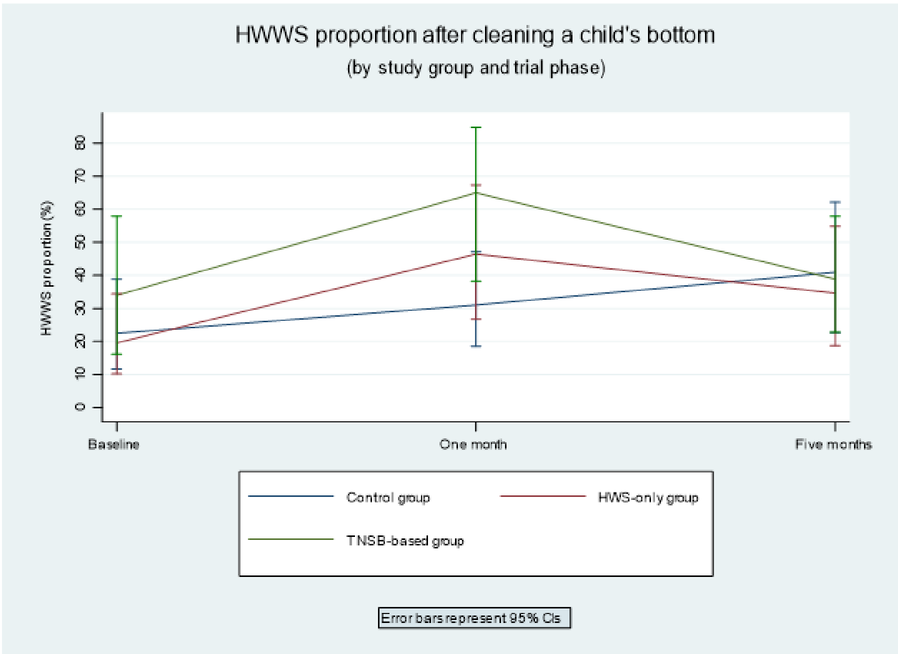


Figure 10.3. HWWS after cleaning a child's bottom pattern by trial arm and trial phase

Table 10.8. Observed handwashing behaviours after cleaning a child's bottom, by trial phase and trial arm (Baseline: August-September 2014 One-month follow-up: September-November 2016; Five-month follow-up: January-March 2017) (Intention-to-treat)

Event	Control arm n	% (95% CI)	HWS-only arm n (%)	% (95% CI)	TNSB-based arm n (%)	% (95% CI)
			Baseline			
Number of events observed						
Total	49		46		53	
Hands washed with soap	11	22.4 (11.7-38.7)	9	19.6 (10.2-34.3)	18	34.0 (16.2-57.8)
Hands washed with water only or gel	15	30.6 (18.3-46.6)	13	28.3 (6.4-44.2)	13	24.5 (13.7-40.0)
Hands not washed	23	46.9 (29.0-65.7)	24	52.2 (35.3-68.6)	22	41.5 (23.7-62.9)
			One-month follow-up			
Number of events observed						
Total	29		28		20	
Female	29	100	26	92.9	20	100
Male	0	0	2	7.1	0	0
Adults (16≥)	23	79.3	21	75.0	15	75.0
Children	6	20.7	7	25.0	5	25.0
Hands washed with soap	9	31.0 (18.8-46.6)	13	46.4 (27.1-66.9)	13	65.0 (38.8-84.50
Hands washed with water only or gel	7	24.1 (13.0-40.5)	11	39.3 (25.7-54.8)	3	15.0 (3.7-44.60)
Hands not washed	13	44.8 (26.4-64.8)	4	14.3 (5.2-33.4)	4	20.0 (5.9-49.7)
			Five-month follow-up			
Number of events observed						
Total	22		26		18	
Female	19	86.4	26	100	18	100
Male	3	13.6	0	0	0	0
Adults (16≥)	17	77.3	19	73.1	11	61.1
Children	5	22.7	7	26.9	7	38.9
Hands washed with soap	9	40.9 (23.1-61.5)	9	34.6 (19.2-54.2)	7	38.9 (23.2-57.3)
Hands washed with water only or gel	5	22.7 (9.9-46.7)	4	15.4 (6.9-30.8)	3	16.7 (5.1-42.6)
Hands not washed	8	36.4 (18.6-58.9)	13	50.0 (32.6-67.3)	8	44.4 (33.9-55.5)

2.4.2. Association between HWWS practices after cleaning a child's bottom and the intervention received

We found no evidence of an association between HWWS after cleaning a child's bottom and the type of intervention received at the one-month (*P*=0.39), and five-month (*P*=0.93) follow-up rounds (Table 10.9). (HWS-only group RR=1.38, 95% CI: 0.72-2.63, TNSB-based group RR=2.10, 95% CI: 1.16-3.81 (overall *P*=0.05); HWS-only RR=0.81, 95% CI: 0.37-1.79 and TNSB-based group RR=0.83, 95% CI: 0.40-1.75, at the respective follow-up rounds). However, at one-month follow-up, the points estimates were consistent with an improvement in both intervention groups.

Picture 10.5: A woman cleaning a child's bottom next to the HWS, and then…*(continued on Picture 6)*

Picture 10.6: […] HWWS after having cleaned the child's bottom

Table 10.9. Unadjusted analyses of the association between the interventions and handwashing with soap practices after cleaning a child's bottom (Intention-to-treat random effects logistic model)

HWWS vs no HWWS			
One-month follow-up (n=32 compounds)			
Unadjusted			
	OR (95% CI)	p-value LRT	Overall model p-value
Interventions			
Control	1.0		
HWS-only	1.70 (0.54-5.39)	0.37	0.09
TNSB-based	4.67 (1.18-18.54)	0.03	
Five-month follow-up (n=28 compounds)			
Interventions			
Control	1.0		
HWS-only	0.81 (0.24-2.76)	0.74	0.93
TNSB-based	0.81 (0.20-3.26)	0.77	
*Adjusted**			
One-month follow-up (n=32 compounds)			
Interventions			
Control	1.0		
HWS-only	1.50 (0.49-4.60)	0.48	0.39
TNSB-based	2.84 (0.63-12.72)	0.17	
Five-month follow-up (n=28 compounds)			
Interventions			
Control	1.0		
HWS-only	0.79 (0.23-2.72)	0.72	0.93
TNSB-based	0.84 (0.21-3.43)	0.81	
*Adjusted for baseline handwashing estimates			

Figures 10.4 and 10.5 present the forest plots of the effect of handwashing interventions on handwashing with soap after toilet use and after cleaning a child's bottom (see Chapter 3), including our trial's one-month and five-month follow-up point estimates respectively

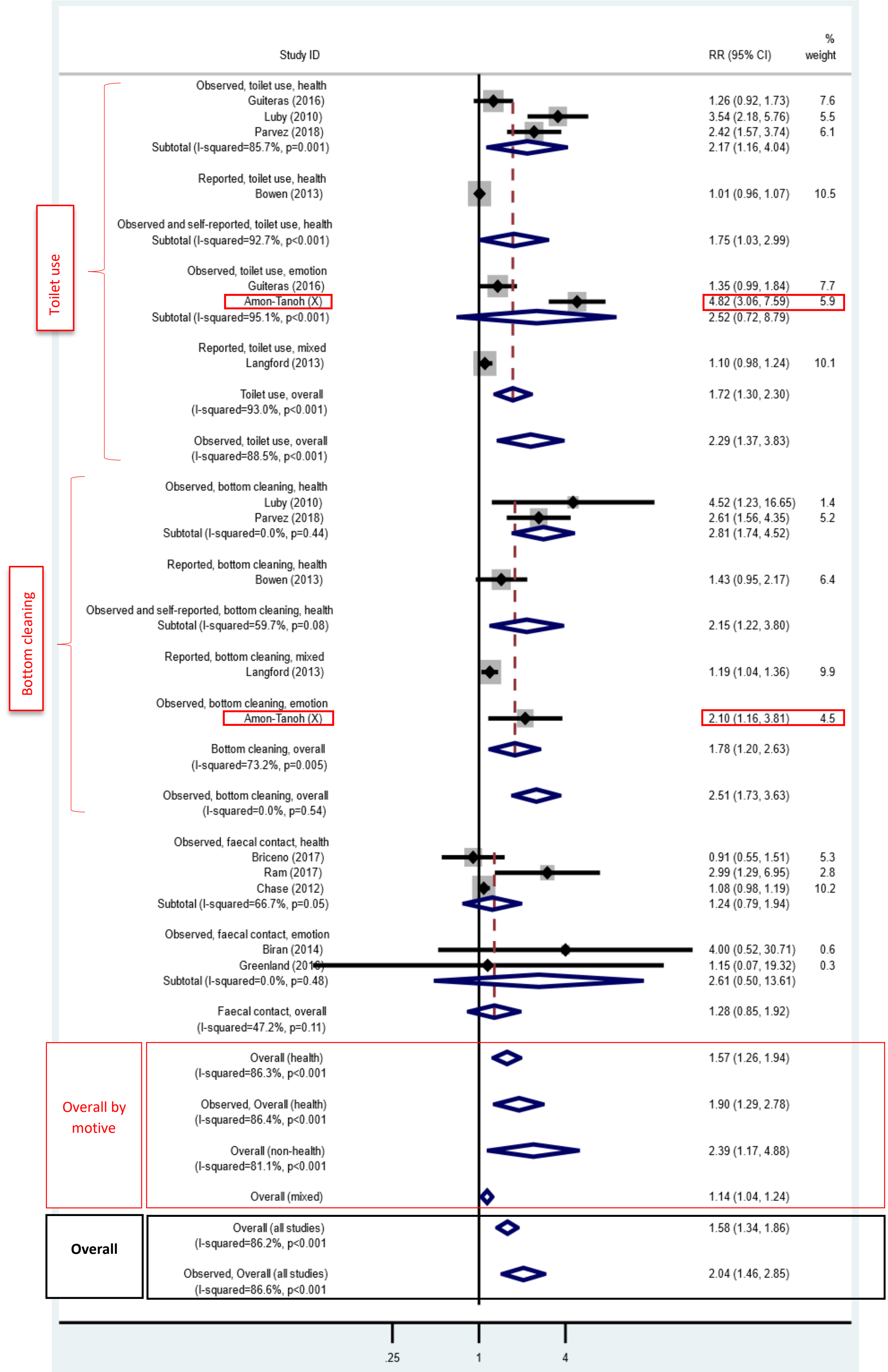


Figure 10.4. Effect of handwashing interventions on handwashing with soap after faecal contact, by occasion, intervention motive and measuring methods (*with PhD candidate's one-month follow-up point estimates*).

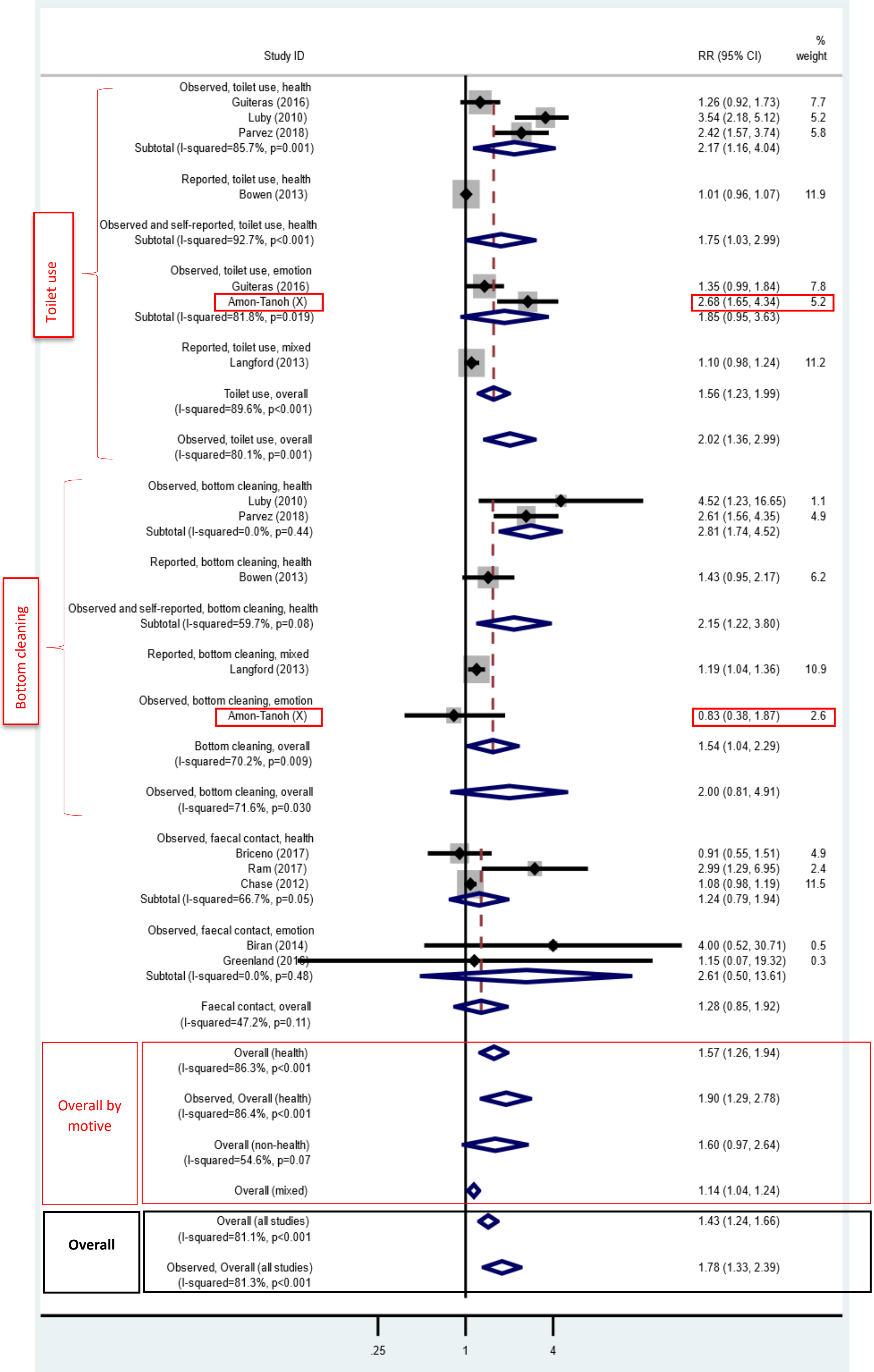


Figure 10.5. Effect of handwashing interventions on handwashing with soap after faecal contact, by occasion, intervention motive and measuring methods (*with PhD candidate's five-month follow-up point estimates*).

3. Post-hoc stratified analyses by age group of the intervention's effect on HWWS after toilet events

We conducted post-hoc analyses to assess whether the effect of the interventions on HWWS after toilet events varied by age group (adults vs. children).

3.1. Test of effect modification

After adjusting for covariates and baseline estimates, there was some evidence that the interventions' effects on HWWS after using the toilet varied by age group, in the one-month and five-month follow-up rounds (respectively P=0.03 and P=0.02) (Tables 10.10 and 10.11). The HWS-only intervention effect appeared to be greater in adults than in children. At one-month follow-up, adults had 2.28 (95% CI: 1.10-4.73) times the odds of washing their hands with soap after using the toilet, compared to adults in the control group, while the intervention appeared to have little effect among children (OR: 1.23, 95% CI: 0.39-3.90). Conversely, in the TNSB-based trial arm, the intervention effect appeared greater among children compared to adults. Children had 11.50 (95% CI: 4.35-30.38) times the odds of washing their hands with soap after using the toilet compared to children in the control group, while adults had 6.29 (95% CI: 3.24-12.20) times the odds of washing their hands with soap compared to adults in the control group.

Five months post-intervention delivery, there was little evidence of an effect of the HWS intervention among either adults or children (adults: OR: 1.12. 95%: 0.53-2.39; children: OR: 0.88. 95% CI: 0.28-2.78) (Tables 10.10 and 10.11). In the TNSB-based trial arm, the intervention effect on HWWS after using the toilet remained larger in children than in adults, though both were statistically significant. Children had 6.42 (95% CI: 2.25-18.90) times the odds of washing their hands with soap after using the toilet compared to children in the control group. By contrast, adults had 2.37 (95% CI: 1.17-4.82) times the odds of washing their hands with soap compared to adults in the control group.

Table 10.10. Age-group stratified analysis of observed HWWS practices after using the toilet, by intervention arm and follow-up point (Intention-to-treat)

	Adult			Children (under 16 years old)		
	Total	n	% (95% CI)	Total	n	% (95%CI)
Occasion ***After using the toilet***						
Baseline						
Control	561	33	5.9 (4.1-8.4)	137	7	5.1 (2.7-9.6)
HWS-only arm	574	16	2.8 (1.4-5.4)	136	8	5.9 (3.1-10.7)
TSNB-based arm	588	40	6.8 (4.3-10.7)	121	6	5.0 (2.6-9.4)
One-month follow-up						
Control	492	21	4.3 (2.8-6.4)	112	8	7.1 (2.6-18.4)
HWS-only arm	467	41	8.8 (5.8-13.1)	90	8	8.9 (3.5-20.7)
TSNB-based arm	490	103	21.0 (14.7-29.1)	98	40	40.8 (29.8-52.8)
Five-month follow-up						
Control	363	23	6.3 (3.9-10.2)	74	7	9.5 (4.5-18.8)
HWS-only arm	356	29	8.2 (5.4-12.2)	100	11	11.0 (5.0-22.4)
TSNB-based arm	363	59	16.3 (11.2-23.0)	87	39	44.8 (29.8-60.9)

Table 10.11. Age group-stratified odds ratios of the association between the interventions and HWWS practices after using the toilet (Intention-to-treat adjusted analysis)

	Adult	Child (<16 years old)	Adjusted* test for effect modification p-value
	OR (95% CI)	OR (95% CI)	
One-month follow-up (71 compounds)			
Intervention arm			
Control	1.0	1.0	
HWS-only arm	2.28 (1.10-4.73)	1.23 (0.39-3.90)	.034
TNSB-based arm	6.29 (3.24-12.20)	11.50 (4.35-30.38)	
Five-month follow-up (70 compounds)			
Intervention arm			
Control	1.0	1.0	
HWS-only arm	1.12 (0.53-2.39)	0.88 (0.28-2.78)	.019
TNSB-based arm	2.37 (1.17-4.82)	6.42 (2.25-18.34)	

*Adjusted for sex, age group, education level of the female head of household and baseline handwashing estimates

We observed similar patterns with respect to age group, when restricting the analysis to toilet occasions with a container for cleansing (Appendix 10.4).

Picture 10.7: A boy assisting a girl wash her hands with soap…(continued on Picture 10.8)

Picture 10.8: […] at the HWS station, after she had visited the toilet.

Picture 10.9: A little girl washing her hands with soap at the HWS, after using the toilet.

Picture 10.10: A little boy washing his hands with soap at the HWS, after using the toilet.

4. Masking assessment

At the one-month and five-month follow-ups, 47 (16%) and 26 (26%) of respondents respectively identified hygiene, alone or coupled with another non-hygiene subject, as the main theme of the questionnaire. At one month, we found no evidence and weak evidence respectively of a difference between the ability of respondents in the HWS-only intervention group (15 (15%) of respondents (*P*=0.45)), and in the TNSB-based group (21 (22%) of respondents (*P*=0.05)) to identify hygiene as the questionnaire key theme compared to the control group (11 (11%)). At the five-month follow-up, there was no evidence of a difference between the intervention groups compared to the control group. 13 (38%) of respondents in the control group cited hygiene-alone or coupled with another non-hygiene subject as the main questionnaire theme, compared to 7 (20%) in the HWS-only intervention group (*P*=0.15), and 6 (20%) in the TNSB-based intervention group (*P*=0.16).

5. Comparison of the assumptions used for the trial's sample size calculation and actual observed estimates.

We had assumed a number of parameters to compute the trial's sample size (see Chapter 9). After examining the trial's data, we can see that our assumptions were reasonably accurate (Table 10.12). The between cluster variation was higher than anticipated. Nevertheless, we adjusted for HWWS baseline differences. The number of observed events was greater than anticipated at baseline and at the one-month follow-up.

Table 10.12. Comparison of the assumptions used for the trial's sample size calculation and actual observed estimates.

	Assumptions	Observed
HWWS frequency (%) in control group	5	6
HWWS frequency (%) in intervention group post intervention	25	22-24
Harmonic mean (baseline)	16	23
Harmonic mean (one month)	.	19
Harmonic mean (five months)	.	10
Between-cluster coefficient of variation (95% CI)	0.25	0.97

Discussion

Shortly after intervention delivery, we found some evidence of a small HWS-only intervention effect on HWWS practices after using the toilet (increase from 4% to 9%), which was not sustained five months post intervention delivery. By contrast, in the TNSB-based group we found evidence of a much larger intervention effect (increasing from 6% to 24%), and which was sustained five months post intervention delivery. It should be noted that the trial took place during the large West-African Ebola epidemic that seriously affected Côte d'Ivoire's neighbours, Liberia and Sierra Leone. The epidemic started in March 2014 and ended around June 2016 [221]. The epidemic received a lot of media attention in Côte d'Ivoire, and resulted in high intensity handwashing promotion, including HWWS after toilet use, at the national level as part of the prevention efforts. Whilst there were thousands of Ebola cases in Liberia and Sierra Leone, no cases were reported in Côte d'Ivoire. The low HWWS frequencies observed in the control group and HWS-only intervention, despite having been exposed to a high intensity handwashing campaign over two years, may illustrate the limitations of such interventions in this population, at least as they pertain to HWWS after toilet use.

The nature of handwashing interventions makes blinding difficult to implement in handwashing behaviour-change studies. Nevertheless, we made considerable efforts to mask participants and outcome assessors, notably by having a general study masking theme, which was relevant to the study population. We sought to assess the effectiveness of the strategies to mask participants at each of the three trial phases. The results seem to indicate that at the one-month follow-up round, the impact of the intervention might have heightened participants' sensitivity to hygiene issues, in the TNSB-based intervention group. The latter were more likely to identify hygiene as the key theme of the questionnaire compared to the other trial arms. This raises the possibility that the increase in HWWS practices observed may have been due in part to a Hawthorne effect.

However, the difference between trial arms in the ability to identify hygiene as the key theme was no longer evident at the five-month follow-up round. Additionally, despite the intervention implementation having ended two months prior to this latter assessment point, the observed increase in HWWS proportions after toilet use was sustained. We therefore believe that it is unlikely that the observed increase in the TNSB arm was solely due to a reaction to the presence of the observers. This finding coupled with the randomised design along with good baseline comparability increases our confidence in a causal effect of the intervention.

While the results of the masking assessment should be interpreted with caution, as the number of respondents surveyed was small, the results are valuable, given that little effort is made to reduce performance and detection bias in many behaviour-change handwashing studies, as seen in the findings from the systematic review reported in Chapter 3. To our knowledge, this is the first study that has attempted to assess the effectiveness of masking strategies at blinding study participants in a handwashing trial in LMICs. Future studies should try to include strategies to assess masking.

We did not find evidence of an effect of either the HWS-only or the TNSB intervention on HWWS practices after cleaning a child's bottom. These results should be interpreted with caution, given the small number of events observed. The upper limits of the confidence intervals do not exclude the possibility that there may have been some effect of the intervention one month and five months post-intervention delivery. Larger studies are needed to assess possible effects on HWWS after cleaning a child's bottom.

To our knowledge, our trial is the first to test the effect of HWS-alone (i.e. without handwashing promotional messages), on HWWS practices after using the toilet, in a low to middle income country (LMIC) community setting. The study findings suggest that the lack of handwashing facilities and products is not the main factor behind the low rates of HWWS observed in this setting. The large difference in intervention effects between the TNSB-based intervention and HWS-only intervention is in line with our premise that, without the internalisation of the importance of HWWS, the presence of handwashing facilities alone will not substantially increase HWWS. This raises questions regarding the reliability of using the presence of water and soap at designated handwashing location or facility as a proxy indicator for actual HWWS practices. Nevertheless, the fact that the HWS was the preferred facility used for handwashing when soap was used supports the argument that it is important that both soap and water are available at the handwashing location (e.g. [43, 48, 68]).

We found some evidence of differences in intervention effects between adults and children. The findings from this post-hoc analysis should be interpreted with caution, and treated as hypothesis generated, and not hypothesis testing. One-month post-intervention delivery, the HWS-only intervention seemed to have been slightly more effective at changing HWWS practices after toilet use among adults, but to have had little to no effect among children. There was no evidence of effect modification by age group five months post-intervention delivery. By contrast, the TNSB-based handwashing intervention seemed to have been more effective among children compared to adults. This difference was sustained five months post-intervention delivery.

A possible explanation for the difference observed by age group in the HWS-only intervention effect may be that adults have more knowledge of the benefits of HWWS, given they have been exposed to more handwashing campaigns over their life course than children. In the absence of any handwashing promotion messages, the presence of the facility may be enough to trigger HWWS after using the toilet in some adults. Among children who have been exposed to few if any handwashing campaigns, clear HWWS intervention messages alongside the HWS facility may be needed to trigger a change in their HWWS practices. On the other hand, children may be more impressionable and receptive to new ideas and information, and more eager to learn than adults. They are still in the midst of habit formation, with their acquired habits less strongly engrained. Thus, it may be easier to change their habits compared to adults using the type of messages delivered in the TNSB arm.

We have earlier presented the results of a systematic review which looked at studies evaluating different interventions in different settings (Chapter 3). Unsurprisingly, there was evidence of substantial between-study heterogeneity. Among these studies, one had a mixed-motives intervention (i.e. [102]), and one study used non-health motive interventions (i.e. [66]). The remaining studies relied on health motive interventions (i.e. [44, 89, 105]). Guiteras et al. also used a health-motive intervention in addition to the non-health motive one [66].

Regarding interventions' effects on HWWS after using the toilet, at the one-month follow up, the magnitude of effect of the TNSB-based intervention (i.e. RR=4.82, 95% CI: 3.06-7.59) was bigger than that observed in the other studies. At the five-month follow-up, the estimate (i.e. RR=2.68, 95% CI: 1.65-4.34) was larger than all estimates except that of Luby et al. (2010) [89].

The study most similar to ours in terms of the intervention applied was Guiteras et al. [66]'s non-health motive intervention. The authors' used disgust and shame to design the intervention. They included a Glo Germ© demonstration, along with the message that not HWWS not only led the 'non-handwasher' to eat their faeces, but also their neighbours eating the former's faeces [66]. Other intervention components were presentations with flipcharts, and plastic bottle supply with small detergent packs for soapy water [66]. Videos were not part of the intervention. Soap was replenished periodically over four months [66]. Two months post-intervention delivery, Guiteras et al. [66] found weak evidence of an intervention effect on HWWS after toilet use (i.e. RR=1.35, 95% CI: 0.99, 1.84). The limited effectiveness of Guiteras et al. [66]'s intervention compared to ours may indicate that the videos played a key role in the TNSB-based intervention's success.

As it pertains to HWWS after cleaning a child's bottom, we found evidence of the TNSB-based intervention effect at the one-month follow-up (non-adjusted estimates: RR=2.10, 95% CI: 1.16-3.81). The point estimate was very consistent with the findings of the previous studies. The effect was not sustained at the five-month follow-up (non-adjusted estimates: RR=0.83, 95% CI: 0.40-1.75). Given the absence of evidence of intervention effect from the random effect model adjusting for baseline estimates, we should be cautious of the results found when computing the unadjusted risk ratios.

The risk of bias and quality of evidence of the identified studies was extensively discussed in the systematic review presented in Chapter 3. In summary, all studies were judged to be of low to very low quality, due to a high risk of bias (Chapter 3). The main risks were performance and detection bias. By contrast, an independent reviewer from Imperial College London (Ms Rhea Harewood, Epidemiologist), ranked our study as having low risk of bias, and of being of high-quality, using the Cochrane Risk of Bias Assessment Tool (V.5.1) [80], and GRADE approach to assess studies' quality of evidence. The study was marked as having unclear risk of bias regarding detection bias, as we only assessed if our masking strategies were effective on participants, but not on fieldworkers.

Several factors may explain the success of the TNSB-based intervention compared to other studies. The extensive piloting in the study population ensured the clarity of the key intervention messages, and the relevance of the intervention content and format to the intended recipients. The stories in the videos were based on everyday interactions common in our study population, and were around themes relevant to our study population. This was done to ensure that participants identified with the characters and stories in the videos. The intervention was also designed to be humorous rather than didactic as has been traditional in handwashing campaigns. The disgust triggering digital effect used in the videos was the one which had generated the strongest disgust reactions during piloting, increasing the chances that the intervention elicited the desired effect. Having three different video groups also reduced intervention fatigue and helped to keep participants interested in attending the intervention throughout the implementation period.

Each TNSB intervention compound was visited five times, compared to twice in the HWS-only intervention group. This intensity may have contributed to maintaining the key intervention message at the forefront of intervention recipients' minds. The fact that posters were permanent intervention reminders and strategically placed at toilet entrances and inside the toilet, may have also contributed to establishing and sustaining the new HWWS practice. This could explain the sustained behaviour change observed five months post-intervention delivery. This has implications in terms of the intensity that future mass-media handwashing campaigns should have to increase the chances that they are effective, and that the changes persist over time if such interventions are informed by the findings of the present study.

Compared to most studies discussed above, the intervention was designed to target handwashing occasions belonging to one domain (i.e. HWWS after faecal contact). This allowed the design of focused and concise key messages, contributing to their clarity. Although HWWS after cleaning a child's bottom was a secondary outcome measure also targeted by the intervention, this intervention message was only introduced in a single intervention video, screened in the last video group to be implemented. This allowed time for the main intervention message (i.e. HWWS after using the toilet) to be repeated several times, with the hope that it would be adopted, before introducing a 'new' intervention message. The intervention providers had also received extensive training and practice prior to delivering the intervention. As each intervention delivery session was recorded for monitoring purpose, they also received feedback from the author throughout the intervention delivery period. This was to ensure that the quality of the intervention delivery was high.

This study is not without limitations. Out of the 1,974 compounds visited initially, only 93 (5 %) met the inclusion criteria. This raises serious questions as to the generalisability of the study findings. The main reason for exclusion of compounds was the presence of more than two households with screens. We assumed that the privatisation of communal space, and thus the reduced visibility of residents' activities would make it difficult to observe handwashing behaviour, and for residents to answer the norms-related questionnaire. However, and in light of the observed rapid change in the structure of study compounds over the course of the trial, some compounds which met the inclusion criteria at enrolment subsequently erected screens but were retained in the trial. Experience from the baseline observations indicated that the presence of screens did not have a substantial impact on data collection. We thus believe that the trial results could be generalised to similar study populations in Côte d'Ivoire.

Similarly, due to financial constraints, a relatively small number of clusters were included in the study (N=75). Thus, the intervention effect estimates are not as precise as they would be in a larger trial. Nevertheless, we were able to detect strong evidence of an intervention effect. The study was not powered to detect a change in HWWS after cleaning a child's bottom, due to the tendency to observe a considerably smaller number of such events. Detecting an intervention effect on this behaviour would have required a substantially large sample size.

Another study limitation lies in the erroneous implementation of the HWS-only intervention in three control compounds, and non-implementation of the HWS-only intervention in one HWS-only compound. Care had been taken so that compounds could be accurately identified, notably using their unique administrative identifiers and a study area sketch. However, two years post baseline some compounds' administrative numbers had disappeared. This was mainly the case for compounds on which administrative numbers had been written using chalk, during a population census. In such cases, it was harder to identify compounds, without entering the compound. However, for masking purposes, the trial managers could not enter compounds with intervention providers, when discreetly showing them in which compounds they had to implement the interventions. The difficulty in identifying such compounds may have also been increased by the fact that, the intervention was implemented two years post baseline. Nevertheless, the results of the on-treatment analysis were comparable to that of the intention-to-treat findings.

We observed a decrease in the observed number of events relating to the key handwashing occasions over time, even though the structured observation time period (i.e. three hours) did not change from one trial phase to the next. The decrease in the number of events observed may be due to seasonal variations. The baseline phase (August to September 2014) and the one-month follow-up post intervention phase (September to November 2016) were both conducted during the rainy season in Côte d'Ivoire (between June and November). Compound residents may tend to be at home more, due to the rain, hence the greater number of events observed. By contrast, the second follow-up phase (January to March 2017) took place during the dry season (between December and May). Residents may tend to spend more time out of their compound, and the number of ceremonies to attend, such as weddings and baptisms, may be greater in the dry season. Nevertheless, the decrease in the number of observed handwashing events was similar across trial arms, and thus should not bias our estimates of the interventions' effects.

Conclusion

We found evidence indicating that an intervention designed without health messages but eliciting the emotion of disgust, combined with the supply of handwashing stations was effective at increasing HWWS after using the toilet. The intervention effect was largely sustained five months post-intervention delivery. The supply of HWS alone appeared to have a small short-term effect on HWWS after using the toilet, but this was not sustained five months post-intervention delivery. Chapters 11 and 12 will present the findings from the TNSB-based intervention process evaluation and HWS sustainability assessment results, respectively, to help further understand the differences in interventions' effects. The study has implications for the design of future emotion-driven interventions aimed at increasing handwashing at key handwashing with soap occasions. Such interventions should ensure that the emotion(s) chosen is relevant to the handwashing occasion(s) targeted. More trials, in other settings, aimed at designing and evaluating handwashing with soap interventions using emotion-based motives only should be designed to ascertain the effect of such interventions in other settings.

Chapter 11 - Process Evaluation of the TNSB-Based Intervention

We conducted a relatively simple process evaluation of the Theory of Normative Social Behaviour (TNSB)-based handwashing intervention, to help understand the trial results. The process evaluation took place concurrently with the trial, and ended seven months post-intervention delivery. The description of the intervention design and hypothesised pathways to impact are described in Chapter 7. Intervention piloting, training, delivery methods, monitoring, and process evaluation data collection methods are described in Chapters 8 and 9.

1. Intervention implementation fidelity and participants' reactions to the intervention

1.1. Intervention delivery and fidelity assessment

As discussed in Chapter 8, intervention delivery was not monitored directly by the trial management team, to preserve blinding. The intervention providers were therefore instructed to record each delivery session, using the recorders they were provided with. During the first week of intervention delivery, the PhD candidate listened to each recorded session to ensure that the intervention was delivered as intended, and to address any mistakes early on. After the first week, the PhD candidate listened to two recorded sessions per week, chosen randomly.

Assessing intervention fidelity consisted of verifying that the intervention was delivered using a participatory, non-didactic approach, and that the relevant discourse was used. This included ensuring that no health messages were mentioned, that all intervention components were implemented as planned, and intervention messages clearly stated. Debriefings with the intervention providers were held at the end of each day, so that intervention providers could share their experiences of delivering the intervention, and discuss any issues they may have encountered. Feedback from listening to the recorded sessions was also given to intervention providers before further intervention delivery sessions.

All intervention compounds received the intervention as intended (i.e. 23 TNSB-based compounds). As mentioned in Chapter 8, the TNSB-based intervention ‘dose’ was reduced with the last two video groups shown once each, as opposed to twice as originally planned.

1.2. Participants’ reactions to the intervention

The intervention providers reported that they were well received by the participants, and that the latter shared their joy and enthusiasm at the intervention being implemented in their compounds. Some participants expressed their appreciation of the fact that the videos contained educational messages, as opposed to only being entertaining. This was in contrast with local TV-series, which such participants deemed as only having entertainment value. Participants also appreciated the gift of a handwashing station (HWS) (Chapter 11). On the other hand, the intervention providers reported that the Glo Germ© demonstration seemed to slow down the momentum of the session, and dampen participants’ enthusiasm. It was during the Glo Germ© section of the intervention, that some participants would leave the intervention delivery session.

The intervention providers stated that it was a pleasure to deliver the interventions. The acceptability of the intervention to the participants, and the good rapport built between the intervention providers and participants is illustrated by the fact no compounds withdrew from taking part in the interventions.

2. Participants’ intervention exposure and message recall

A total of 78 participants were surveyed, using systematic random sampling, seven months post-intervention delivery (Table 11.1). The majority of respondents were female (77%), and between 25 and 44 years old (56%). Of the 78 respondents, 63 (81%) reported having attended at least one intervention session. The most common reason given for non-attendance was having travelled (7 (47%) of 15 respondents). Overall, 54 (69%) of 78 respondents had been exposed to all 4 intervention components (i.e. videos, Glo Germ© demonstration, posters and HWS), with only one respondent (1%) who had only been exposed to the HWS.

2.1. Exposure to videos

All 63 respondents who had attended an intervention session had seen at least one or more videos: 11 (17%) had attended two video sessions (Table 11.1), 20 (32%) respondents had attended three video sessions, and 23 (37%) had partaken in all four screening sessions. 9 (14%) respondents reported attending more than four sessions, which likely reflects recall error. In total, there were between 186 and 239 adults who were exposed to each group of videos (Table 11.2).

2.2. Exposure to posters and handwashing station

2.2.1 Exposure to posters

One month post initial intervention delivery, 22 (96%) of 23 compounds were observed to still have at least one group of posters present (i.e. positive or negative posters). This number decreased to 74%, seven months post-intervention delivery. Whilst residents did not volunteer reasons why the posters were no longer present, the fieldwork assistants suggested that it was quite possible that they had been used as wiping material, after defecation. The use of paper as toilet paper substitute is common in our study population.

Only 1 (1%) out of 78 respondents had not seen the posters (Table 11.1). This respondent was living in a compound where the posters were no longer present. Additionally, the respondent had not been exposed to the videos or Glo Germ© demonstration, as they had travelled at the time. It is likely that, by the time they had returned from their travel, the posters had already been removed. Among the respondents who had seen the posters, 62 (81%) of 77 were able to accurately state the posters' locations (i.e. at the toilet entrance and inside the toilet). 11 (14%) respondents stated that the posters were located inside the toilet, and 4 (5%) respondents did not remember where the posters were located. Among those latter respondents, 3 (75%) were from compounds were the posters were no longer present.

2.2.2. Handwashing station exposure

All 78 respondents had seen the HWS in their compound.

Table 11.1. Compound residents TNSB-based handwashing intervention exposure and key intervention messages recollection (seven months post initial intervention delivery-March-April 2017)

Intervention exposure	Respondents N= 78	% (95% CI)
Respondents characteristics		
Respondents gender		
Male	18	23
Female	60	77
Respondents age group		
16-24	13	17
25-34	22	28
35-44	22	28
45-54	14	18
55+	7	9
Intervention exposure and content knowledge		
Has attended an intervention session	63	81 (70-88)
Reason for not attending		
New inhabitant	3	20 (4-62)
Had travelled	7	47 (20-76)
Was at work	4	27 (8-60)
Other reason	1	7 (1-40)
Has seen the videos	63	100
Number of video screening sessions attended		
1	0	0
2	11	17 (9-32)
3	20	32 (21-45)
4	23	37 (24-51)
Other	9	14 (8-25)
Has seen the posters	77	99 (90-100)
Where are the posters located?		
At the toilet entrance and on the toilet door	62	81 (70-88)
At the toilet entrance	0	0
Inside the toilet	11	14 (8-23)
Does not remember	4	5 (2-16)
Has seen the Glo Germ© demonstration	54	86 (76-92)
What is the role of the Glo gel product?		
Shows areas where faeces are located	27	50 (36-64)
Hand cream	1	2 (0-13)
Soap	5	9 (21)
Other	13	24 (15-36)
Does not remember	8	15 (6-32)
Has seen the handwashing station	78	100
Number of intervention components exposed to:		
0	0	0
1. Handwashing station-only	1	1 (0-10)
2. Handwashing station and posters	14	18 (11-29)
3 Videos, handwashing station, and posters	9	12 (7-20)
4 Videos Glo Germ© demonstration, handwashing stations and posters	54	69 (58-79)
Proportion of respondents who remember any intervention message	60	95 (86-98)
Proportion of respondents who had attended an intervention session who identified handwashing with soap after using the toilet as the key intervention message	49/60	82 (72-89)
Proportion of respondents who remember any secondary intervention message	19	32 (21-45)
Most remembered secondary intervention messages: *"[…] We eat our faeces.*	8	42 (22-65)

2.3. Glo Germ© demonstration exposure

54 (86%) of 63 respondents exposed to the TNSB-intervention had attended a Glo Germ© demonstration (Table 11.1). Among these, 27 (50%) were able to accurately state that Glo gel product's purpose was to show the areas (on one's hands or on objects) where faeces were located. 8 (15%) respondents did not remember what the product's function was. The remaining respondents thought that Glo gel was soap (5 respondents (9%)), hand cream (1 respondent, (2%)), whilst 13 (24%) respondents thought that the product removed faeces from one's hands.

2.4. Intervention message(s) recall

Among the 63 respondents who attended an intervention session, 60 (95%) reported remembering at least one intervention message (Table 11.1). Among these 60 respondents, 49 (82%) remembered (without being prompted) handwashing with soap after using the toilet as the key intervention message. 19 (32%) of 60 respondents remembered (a) secondary intervention message(s). Among these respondents, 18 (95%) remembered (a) secondary intervention message(s) in addition to remembering the primary intervention message. One respondent (5%) only remembered a secondary intervention message. The secondary intervention message the most often remembered was: *"If we don't wash our hands with soap after using the toilet, we eat our faeces"* (8 (42%) of 19 respondents).

2.5. Assessment of Videos' ability to trigger feeling of disgust

As described in Chapter 9, we assessed the negative videos' ability to trigger the emotion of disgust, by having willing participants take part in a vote. The vote was implemented at each intervention delivery session (i.e. at the initial intervention delivery session, and at one month, two months and three months post initial intervention delivery). The vote took place after the screening of the negative videos and before the screening of the positive video. The vote was conducted using cards, distributed to participants. Each card had an emoji depicting an emotion. Initially, participants could choose between two responses (an emoji representing disgust or another representing indifference). However, after implementing the vote in seven compounds, we decided that having only two response categories, including no positive emoji, risked biasing the vote results. We therefore added a third possible response with an emoji depicting amusement. Consequently, the 77 respondents from the seven compounds where the first vote version was implemented were excluded from the disgust assessment analysis.

Overall, we observed similar voting patterns in terms of the emotions triggered by the videos, irrespective of the video group shown (Table 11.2). For the first showing of video group 1, 271 adults were present at the time of the vote, with 181 (67%) consenting to take part in the vote, including 145 (80%) female voters (Table 11.2). Among the 181 voters, 131 (72%) reported that the videos generated a feeling of disgust; 37 (20%) felt amused, and 13 (7%) were unaffected by the videos.

One month post initial intervention delivery (video group 1 again), the number of adult attendees was 201, with 187 (93%) taking part in the vote, 86% of whom were women (Table 11.2). A total of 152 (81%) voters felt disgusted by the videos, compared to 28 (15%) who felt amused, and 7 (4%) who were not affected by the videos. Two months post initial intervention delivery (video group 2), 186 adults were present, of whom 173 (93%) took part in the vote. 141 (82%) of voters were women. Out of the 173 voters, 158 (91%) felt disgusted by the videos, 9 (5%) felt amused and 6 (3%) were unaffected by the videos. At the final intervention delivery round (video group 3), the number of adult attendees was 239, of whom 206 (86%) took part in the vote. 159 (77%) were woman. 179 (87%) voters felt disgusted by the videos, compared to 20 (10%) who felt amused, and 7 (3%) who were not affected by the videos.

2.5.1. Assessing the accuracy of the feeling of disgust reported by participants

As mentioned in Chapter 7, the literal translation of the word 'disgust' is not the local term commonly used to express disgust, in our study population. When describing the disgust emoji, we therefore used the expression which compound residents had frequently used during the intervention piloting (i.e. *"Ça te fait te sentir bizarre dans ton corps"*, which literally translates to *"It makes you feel weird in your body"*). To ensure that disgust was the emotion participants experienced, intervention providers asked voters who had picked this emotion to volunteer what they meant when they said that the videos had made them feel 'weird in their bodies'. Examples of explanations volunteered included (translated in English): *"It gave me chills"*; *"It made me feel like little insects were crawling on my body."* All the expressions cited belong to the local vernacular used to express disgust.

Table 11.2. Results of the vote to assess the intervention ability to trigger disgust in participants, by intervention delivery round

	Round 1: Video group 1 (Initial intervention delivery) n=23	Round 2: Video group 1** (one month post initial intervention delivery) n=23	Round 3: Video group 2 (two months post-initial intervention delivery) n=23	Round 4: Video group 3 (three months post-initial intervention delivery) n=23
Number of adults attending the intervention at the time of the vote	271	201	186	239
Number of voters, n (%) Total Female Male	 181 (67%)* 145 (80) 36 (20)	 187 (93%) 160 (86) 27 (14)	 173 (93%) 141 (82) 32 (18)	 206 (86%) 159 (77) 47 (23)
Votes results				
Feeling triggered None Amused Disgusted	 13 (7) 37 (20) 131 (72)	 7 (4) 28 (15) 152 (81)	 6 (3) 9 (5) 158 (91)	 7 (3) 20 (10) 179 (87)

*77 respondents dropped from the analysis due to potential bias in the initial voting method (i.e. choice between neutral and disgust only as emotions the two emotions triggered by the videos)
** Second screening

Discussion

The findings from the TNSB-based process evaluation indicate that the intervention had high coverage. At least 80% of adult compound residents were exposed to the intervention, and with a little less than 70% exposed to all four intervention components. The fact that more women than men attended the intervention sessions is consistent with the fact they are the residents most present in compounds, as men are generally the breadwinners. Over 80% of respondents were able to recall the key intervention message. This indicates understanding of the intervention's message, which is presumably an important step in the causal pathway via which the intervention worked. This is also an indication of the clarity of the intervention. The results also indicate that the intervention seems to have been successful at triggering feelings of disgust. Given the effectiveness of the TNSB-based intervention, this supports the argument that disgust is an effective motivator for changing HWWS practices after using the toilet.

Several study limitations can be identified. The first limitation lies in the relatively small sample size on which the survey used to collect intervention coverage information is based (i.e. 78 residents). This was due to resource and time constraints. Nevertheless, the findings are still valuable, consistent with the intervention results, and help shed light on the trial's results.

Half of the respondents did not understand what the Glo gel product was for. The way the product was described may not have been clear to participants. As the process evaluation did not assess respondents understanding of the message(s) put forward by the Glo Germ© demonstration, we cannot assess whether not clearly understanding the role of the gel impeded the overall comprehension of the demonstration. Glo Germ© also seems to have been the component least appreciated by participants, as reported by the intervention providers. This is in line with the pilot findings (Chapter 7) which seemed to suggest that Glo Germ© lessened the disgust emotion triggered by the videos, rather than strengthening it, as intended. Future studies might consider dropping Glo Germ© from the intervention.

Conclusion

The results of the TNSB-based handwashing intervention's process evaluation showed that the intervention was well received and accepted by the study participants. The latter liked the videos, but seemed less taken by the Glo Germ© component. The intervention also achieved high coverage, based on the survey findings, and there was a good retention of the intervention message. The videos appeared to have been effective at eliciting feelings of disgust as intended.

Chapter 12 - Sustainability of the Handwashing Station

This chapter presents the results of the handwashing station (HWS) acceptability and sustainability assessment. The aim was to evaluate the proportion of HWS which remained replenished with water and/or soap by the end of the trial. This was measured at the one-month, five-month (data collected by the observers), and the seven-month follow-up (intervention providers data). We also appraised the number of HWS which were damaged.

Fifty HWS were delivered in total (i.e. 27 compounds receiving only an HWS, as per on-treatment analysis, and 23[22] compounds in the TNSB-based group). Due to a misunderstanding between the fieldwork assistants and the PhD candidate, the HWS sustainability data for two out of the three control compounds which received an HWS were not taken into consideration during data entry pertaining to the intervention providers. Thus, when reporting the HWS sustainability results from data collected by the intervention providers, the analyses was conducted per intention-to-treat (24 HWS-only compounds). When reporting the results of the data collected by the fieldworkers, the analysis was conducted on treatment (27 HWS-only compounds).

1. Handwashing initial location and maintenance strategy

1.1. Handwashing station location

When the HWS were delivered, 42 (89%) of 47 could be placed at the toilet entrance (Table 12.1). The proportions were comparable across intervention groups. The HWS which could not be placed at the toilet entrance were either in compounds where there were toilets in two different locations, or in compounds where placing the HWS at the toilet entrance would block residents' passage.

---

[22] As stated in Chapter 10, one compound in the TNSB-based group was excluded from the study, as it no longer met inclusion criteria, and another withdrew consent.

1.2. Participants' handwashing station maintenance strategy

When asked how they would keep the HWS replenished in water, about half of compounds chose to have specific maintenance arrangements (23 (49%) of 47 compounds) (Table 12.1). In the HWS-only trial arm, having a specific arrangement was more common than in the TNSB-based intervention group (15 (63%) of 24 compounds vs. 9 (39%) of 23 compounds). Regarding soap replenishment, all compounds but one (4%) chose to put specific arrangements in place.

Generally, having a specific arrangement to maintain the HWS entailed having one or several designated resident(s) in charge of replenishing the HWS or having a household rotation system. Besides having (a) specific resident(s) in charge or operating a household rotation system, some compounds also proposed that each household pay a small contribution for the soap to be purchased. In compounds with some inhabitants who made and sold liquid soap for a living, the latter offered to freely replenish the HWS with soap. In contrast, in the absence of specific arrangements, anyone who saw that the HWS was empty could replenish it. In some compounds where a specific arrangement was chosen, participants still decided this should not prevent anyone from replenishing the HWS in water and/or soap, if someone observed the HWS to be empty.

Table 12.1. Handwashing station location in the compound and residents chosen HWS maintenance arrangement (*data collected at the initial intervention delivery: August-November 2016)*

	HWS-only arm *n* (%)	TNSB-based arm *n* (%)	Intervention groups combined
Number of compounds with HWS delivered	24*	23	47
Handwashing station location			
At the toilet entrance	22 (88)	21 (91)	42 (89)
In the middle of the compound	3 (12)	2 (9)	5 (11)
Compound chosen arrangement to replenish water at the HWS:			
No specific arrangement	9 (36)	14 (61)	23 (49)
Specific arrangement	16 (64)	9 (39)	24 (51)
Compound chosen arrangement to replenish soap at the HWS:			
No specific arrangement	0 (0)	1 (4)	1 (2)
Specific arrangement	25 (100)	22 (96)	47 (98)

* One HWS-only compound mistakenly did not receive a handwashing station

2. Handwashing station sustainability assessment

2.1. Handwashing station sustainability assessment at one-month and five-month follow-up

At both one-month and five-month post HWS delivery, the HWS sustainability data were collected by the fieldworkers in both intervention groups. The latter did so during each of their two observation rounds in each compound.

One month post HWS delivery, 3 (6%) of 50 compounds were observed to have missing HWS at both observations (Table 12.2). All missing HWS were from HWS-only intervention compounds. In one compound the HWS was no longer at the toilet entrance, but located at the household entrance of the compound landlord (Picture 12.1). In the bucket to collect dirty water from handwashing, a piece of cloth was being soaked (Picture 12.2). At five months follow-up, the HWS was no longer visible.

Picture 12.1: Compound with an HWS removed from the toilet entrance, placed next to the landlord's household, and […]

Picture 12.2: […] with a piece of cloth being soaked in the container for HW wastewater.

2.1.1. Handwashing station maintenance assessment at one-month follow-up

- **Overall**

Overall, the HWS showed signs of maintenance at 86 (86%) of 100 observations (Table 12.2). At 65% of observations, the HWS was observed to have both water and soap at the HWS. Compounds with specific maintenance arrangements were observed to have both water and soap present at the HWS at 38 (79%) of 48 observations, compared to 25 (63%) of 40 observations, in compounds with no specific maintenance arrangement (Table 12.3). At 18% of observations, water-only was present at the HWS (Pictures 12.3 and 12.4) and, at 3% of observations, only soap was present at the HWS. At the remaining 14% of observations, neither water nor soap was present. This proportion was greater in compounds with no maintenance arrangements (5 (13%) of 40 observations), compared to compounds with specific maintenance arrangements (2 (4%) of 48 observations). 4 (8%) of 50 compounds were observed to have HWS with neither water nor soap at both observations. These were all compounds in the HWS-only intervention group. On one occasion when there was no water at the HWS at the start of the observation period, the HWS was filled up with water during the observation period. On occasions when soap was not present at the HWS (32 observations), HWS were observed to be replenished with soap on two (6%) occasions (Pictures 12.5 and 12.6).

By study group

When comparing sustainability between intervention groups, the TNSB-based intervention group seemed to perform better than the HWS-only intervention group, at one-month follow-up (Table 12.2). Compounds in the TNSB-based group were recorded to have HWS with signs of maintenance at 92% (43 of 46) of observations (Table 12.2), with 37 (80%) observed with both water and soap present. In 3 (7%) of 46 observations, neither water nor soap were present at the HWS. By contrast, in the HWS-only intervention group, compounds were observed to have HWS with signs of maintenance at 80% (43 of 54) of observations. In 28 (52%) observations, compounds were recorded as having both water and soap present at the HWS. Neither water nor soap were recorded as being present in 11 (20%) of 54 observations.

Picture 12.3: HWS with a bottle of liquid soap

Picture 12.4: HWS with no soap supply.

Table 12.2. Handwashing station sustainability assessment by study arm, at one-month follow-up (September-November 2016)

	HWS-only *n* (%) N=27*	Visit 1	Visit 2	TNSB-based *n* (%) N=23	Visit 1	Visit 2	Intervention groups combined N=50
Total number of observations	54	27	27	46	23	23	100
Number of compounds with missing HWS	3 (11)	3 (11)	3 (11)	0 (0)	0 (0)	0 (0)	3 (6)
Presence of water and soap at the HWS:							
Both present	28 (52)	15 (56)	13 (48)	37 (80)	19 (83)	18 (78)	65 (65)
Water-only	13 (24)	4 (15)	9 (33)	5 (11)	2 (9)	3 (13)	18 (18)
Soap-only	2 (4)	1 (4)	1 (4)	1 (2)	0 (0)	1 (4)	3 (3)
Neither water nor soap (*including missing HWS)*	11 (20)	7 (26)	4 (15)	3 (7)	2 (9)	1 (4)	14 (14)
Number of compounds with neither water nor soap present at the HWS at both visits (*including missing HWS)*	4 (15)	.	.	0 (0)	.	.	4 (8)

*One HWS-only compound mistakenly did not receive a handwashing station and 3 control compounds mistakenly received handwashing stations.

Table 12.3. Observed handwashing station maintenance status according to the type of arrangement chosen to replenish the HWS with water, at one-month follow-up (September-November 2016).

	HWS-only		TNSB-based		Intervention group combined	
	No specific arrangement n (%)	Specific arrangement n (%)	No specific arrangement n (%)	Specific arrangement n (%)	No specific arrangement n (%)	Specific arrangement n (%)
Number of observations	12	30	28	18	40	48
Water and soap	3 (25)	23 (77)	22 (79)	15 (83)	25 (63)	38 (79)
Water-only	4 (33)	6 (20)	3 (11)	2 (11)	7 (18)	8 (17)
Soap-only	2 (17)	0 (0)	1 (4)	0 (0)	3 (8)	0 (0)
Neither water nor soap	3 (25)	1 (3)	2 (7)	1 (6)	5 (13)	2 (4)

Picture 12.5: HWS replenished with a soap bar.

Picture 12.6: HWS replenished with a bottle of liquid soap.

2.1.2. HWS maintenance assessment at five-month follow-up

- **Overall**

The number of compounds with missing HWS had increased to 6 (12%) of 49[23] compounds, recorded at both observation rounds (Table 12.4). These were still all HWS-only compounds. We observed a small decrease in the proportion of HWS which showed signs of maintenance (58 (60%) of 97[24] observations). The proportion of observations at which both water and soap were recorded as present at the HWS decreased to 35 (36%) of 97 observations. The observed proportions were comparable irrespective of whether the compounds had chosen a specific maintenance arrangement or not (Table 12.5). In 39 (40%) of 97 observations, HWS were recorded to have neither water nor soap present. 17 (35%) of 49 compounds lacked both water and soap at the HWS at both observation rounds. No HWS without water was observed being refilled. In the cases where soap was not present at the HWS (61 observations), replenishment of soap was observed once (2%*)*.

- **By study group**

At five months follow-up, the proportions of HWS showing signs of maintenance were more comparable between groups (31 (58%) of 54 observations in the HWS-only trial arm compared to 27 (63%) of 43 observations in the TNSB-based trial arm) (Table 12.4). In the latter group, HWS were recorded as having both water and soap present at the HWS in 21 (49%) of 43 observations, and neither soap nor water present in 16 (37%) observations. In the HWS-only intervention group, compounds were recorded to have HWS with both water and soap present at 14 (26%) of 54 observations; and neither soap nor water present in 23 (43%) observations. In the HWS-only trial arm, the proportion of compounds observed to have neither water nor soap at the HWS at both visits was comparable across trial arms.

---

[23] As stated in Chapter 10, one compound withdrew consent during the one-month follow-up.
[24] As mentioned in Chapter 10, one compound withdrew consent during the five months follow-up.

Table 12.4. On-treatment handwashing station sustainability assessment by study arm, at five-month follow-up (January-March 2017)

	HWS-only *n* (%) N=27*	Visit 1	Visit 2	TNSB-based *n* (%) N=22	Visit 1	Visit 2	Intervention groups combined N=49
Total number of observations	54	27	27	43**	22	21	97
Number of compounds with missing HWS	6 (22)	6 (22)	6 (22)	0 (0)	0 (0)	0 (0)	6 (6)
Presence of water and soap at the HWS***:							
Both present	14 (26)	8 (30)	6 (22)	21 (49)	12 (55)	9 (43)	35 (36)
Water-only	16 (30)	8 (30)	8 (30)	6 (14)	1 (5)	5 (24)	22 (23)
Soap-only	1 (2)	1 (4)	0 (0)	0 (0)	0 (0)	0 (0)	1 (1)
Neither water nor soap (*including missing HWS)*	23 (43)	10 (37)	137 (48)	16 (37)	9 (41)	7 (33)	39 (40)
Number of compounds with neither water nor soap present at the HWS at both visits***	10 (37)	.	.	7 (33)***	.	.	17 (35)

*One HWS-only compound mistakenly did not receive a handwashing station and 3 control compounds mistakenly received handwashing stations.
**One compound withdrew from the trial after the first observation round.
***Compound with second observation round missing excluded from the analysis.

Table 12.5. Observed handwashing station maintenance status according to the type of arrangement chosen to replenish the HWS with water, at five-month follow-up

	HWS-only		TNSB-based		Intervention group combined	
	No specific arrangement n (%)	Specific arrangement n (%)	No specific arrangement n (%)	Specific arrangement n (%)	No specific arrangement n (%)	Specific arrangement n (%)
Number of observations	8	28	26	17	34	45
Water and soap	3 (38)	11 (39)	11 (42)	10 (59)	14 (41)	21 (47)
Water-only	2 (25)	12 (43)	3 (12)	3 (18)	5 (15)	15 (33)
Soap-only	0 (0)	1 (4)	0 (0)	(0)	0 (0)	1 (2)
Neither water nor soap	3 (38)	4 (14)	12 (46)	4 (24)	15 (44)	8 (18)

2.2. Handwashing station sustainability assessment at seven-month follow-up

2.2.1. HWS maintenance assessment

At the seven-month follow-up, the data on HWS sustainability were collected by the intervention providers.

- **HWS location and condition**

The number of compounds with missing HWS, observed at both observation rounds, decreased to 4 (9%) of 47 compounds (Table 12.6), compared to six compounds at five months follow-up. 36 (77%) of 47 compounds still had HWS located at the toilet entrance, compared to 89% when the HWS was first delivered. The observed proportion was slightly lower, in the HWS-only group (17 (71%) of 24 compounds), compared to 19 (83%) of 23 compounds in the TNSB-based trial arm. The reasons participants gave for having moved the HWS was that its initial location bothered the households adjacent to the toilet. Another reason given was that the HWS was initially placed at a location where one inhabitant was used to parking their bike, and they did not agree to park their bike elsewhere (Picture 12.7).

Picture 12.7: HWS initially placed at the toilet entrance where an inhabitant usually parked their bike.

Overall, we observed 11 (26%) of 43 present HWS which were damaged at the seven-month follow-up (Table 12.6). The main type of damage was breakage of the HWS stand (9 (82%) of 11 damaged HWS) (Pictures 12.8, 12.9 and 12.10). One HWS had a broken soap holder (Picture 12.11). No handwashing HWS had a broken tap. Among the 11 HWS which were damaged, four (36%), including three (60%) of five HWS in the TNSB-based group, were no longer usable. In such cases, the HWS stands were broken to such a degree that the HWS could no longer stand (Picture 12.12). Thus, 39 (83%) of 47 HWS were still present and usable, at the seven-month follow-up.

Picture 12.8: HWS with a broken stand (1)

Picture 12.9: HWS with a broken stand (2)

Picture 12.10: HWS with a broken stand (3)

Picture 12.11: HWS with a broken soap holder

Picture 12.12: Handwashing station with a stand broken to the point the station is no longer usable.

Presence of water and soap at the HWS

Overall, the observed proportion of HWS showing signs of maintenance was comparable to that observed at five-month follow-up (27 (69%) of 39 HWS compared to 60% at seven- and five-months follow-ups respectively) (Table 12.6). The proportion of observations at which HWS were recorded as having both water and soap at the HWS was higher than that observed at five-month follow-up (19 (49%) of 39 compounds, compared to 35 (36%) of 97 observations, at seven- and five-months follow-ups respectively). The proportions were comparable irrespective of the choice of HWS maintenance arrangement (Table 12.7). We observed 12 (31%) of 39 compounds with neither water nor soap at the HWS. Among these 20 compounds, one (5%) did not have any water in the HWS because the compound had been experiencing a water shortage for a few days.

Table 12.6. Handwashing station sustainability assessment by study arm, and presence of intervention posters in the TNSB-based intervention group, at seven-month follow-up (March-April 2017)

	HWS-only *n* (%)	TNSB-based n (%)	Intervention groups combined n (%)
Total number of compounds	**24***	**23**	**47**
Handwashing station location			
At the toilet entrance	17 (71)	19 (83)	36 (77)
In the middle of the compound	3 (13)	4 (17)	7 (15)
Missing HWS	4 (17)	0 (0)	4 (9)
Number of HWS present	**20 (83)**	**23 (100)**	**43 (91)**
Number of broken HWS	6 (30)	5 (22)	11 (26)
Type of HWS damage***:			
Broken tap	0 (0)	0 (0)	0 (0)
Broken stand	4 (67)	5 (100)	9 (82)
Broken bucket	1 (17)	1 (20)	2 (18)
Other broken part	1 (17)	0 (0)	1 (9)
Usability			
Damaged but usable	5 (83)	2 (40)	7 (64)
Unusable	1 (17)	3 (60)	4 (36)
Reported time since which HWS had been unusable due to broken part(s):			
Less than one week	0 (0)	0 (0)	(0)
One week	0 (0)	1 (33)	1 (25)
Two to three weeks	(0)	0 (0)	0 (0)
One month	1 (100)	0 (0)	1 (25)
More than a month	0 (0)	2 (67)	2 (50)
Number of HWS present and usable	**19 (79)**	**20 (87)**	**39 (83)**
Presence of water and soap at the HWS:			
Both present	7 (37)	12 (60)	19 (49)
Water-only	6 (32)	2 (10)	8 (21)
Soap-only	0 (0)	0 (0)	0 (0)
Neither water nor soap	6 (32)	6 (30)	12 (31)
*Reported time since which HWS had been unusable due to lack of water and soap**:*			
Less than one week	1 (17)	2 (33)	3 (25)
One week	1 (17)	0 (0)	1 (8)
Two to three weeks	2 (33)	2 (33)	4 (33)
One month	1 (17)	0 (0)	1 (8)
More than a month	1 (17)	2 (33)	3 (25)
Number of respondents who wished for HWS to be improved	**21 (88)**	**13 (57)**	**34 (72)**
Recommendations to improve the HWS			
More solid stand	18 (86)	12 (92)	30 (88)
More solid tap	1 (5)	5 (39)	6 (18)
More solid water bucket	1 (5)	2 (15)	3 (9)
Other	2 (10)	2 (15)	4 (12)

*One HWS-only compound mistakenly did not receive a handwashing station

**Excluding HWS which lacked water and soap and were also broken

Table 12.7. Observed handwashing station maintenance status according to the type of arrangement chosen to replenish the HWS with water, seven months post intervention.

	HWS-only		TNSB-based		Intervention group combined	
	No specific arrangement n (%)	Specific arrangement n (%)	No specific arrangement n (%)	Specific arrangement n (%)	No specific arrangement n (%)	Specific arrangement n (%)
Number of observations	5	14	12	8	17	22
Water and soap	1 (20)	6 (43)	7 (58)	5 (63)	8 (47)	11 (50)
Water-only	2 (40)	4 (29)	1 (8)	1 (13)	3 (18)	5 (23)
Soap-only	0 (0)	0 (0)	0 (0)	0 (0)	0 (0)	0 (0)
Neither water nor soap	2 (40)	4 (29)	4 (33)	2 (25)	6 (35)	8 (27)

The proportions of HWS showing signs of maintenance were similar in the two intervention groups (13 (68%) of 19 compounds in the HWS-only intervention group, compared to 14 (70%) of 20 compounds in the TNSB-based intervention group) (Table 12.6). The observed proportions were higher than the ones observed at five months follow-up. We observed 7 (37%) of 19 HWS with both water and soap present in the HWS-only intervention group, compared to 12 (60%) of 20 HWS in the TNSB-based intervention group. In general, at each follow-up point, the observed proportion of HWS with both water and soap at the HWS was greater than the one with water-only, in the TNSB-based intervention group. By contrast, the latter proportion was greater than the former in the HWS-only intervention group, at the five and seven-month follow-ups.

- **Suggestions to improve the HWS**

When asked if they would like to see improvements to the HWS, 34 (72%) of 47 compounds were in favour of some improvements (Table 12.6). The main recommendation was that the HWS stand be made of a more solid material (30 (88%) of 34 compounds).

Discussion

To our knowledge, this is the first study conducting such an in-depth quantitative assessment of HWS sustainability. The presence of damaged HWS, mainly on the HWS stands, was not unexpected. As mentioned in Chapter 9, due to financial constraints, the HWS was made of wood as opposed to iron, as in the 2012 pilot. This meant that the HWS stands were less sturdy, and thus more susceptible to breakage over time. This is especially the case if the HWS was frequently used. When delivering the HWS, we had advised compound residents against moving the HWS from its location, especially when filled with water. Residents were also given specific instructions on how to refill the HWS with water to minimise the risk of breakage. Frequent removal and replacement of the smaller bucket to dispose of handwashing wastewater may also have caused damage to the HWS. The vast majority of participants who wished for the HWS design to be improved suggested that the stand be made of a stronger material. Nevertheless, that 74% of HWS remained in good condition, and that no HWS taps were broken is encouraging.

The data is inconsistent with the view that having a predefined arrangement in place to replenish the HWS with water and soap may help in maintaining the HWS, although the sample size is small and this could be due to chance. We had assumed that having designated individuals in charge of the HWS would create a certain accountability from the chosen/volunteering individuals vis-à-vis their fellow compound residents. Although, that in the short term there may have been a benefit of having a defined maintenance system in place, this appeared not to be the case in the longer term. This may suggest that, while at the beginning compounds with a predefined maintenance system in place may have followed the chosen organisation, this may have functioned less well as time passed, and as the novelty of the HWS and initial motivation to maintain the HWS decreased with time. An alternative explanation could be that, as the habit of maintaining the HWS was established over time, the need for having a specific organisation in place was not as crucial as initially. We did not identify any handwashing studies in similar settings which discussed the type of organisation used to ensure handwashing facilities were supplied with water and soap.

The choice of all compounds but one to have a specific arrangement in place to replenish the HWS with soap compared to fewer with arrangements for water is understandable, due to the cost of soap. In compound settings in Côte d'Ivoire, the water cost is often a fixed monthly fee, thus independent of residents' consumption. The financial burden of maintaining the HWS in soap is tangible, whilst the burden of maintaining the HWS in water is more likely linked to motivation. The proportion of HWS with both water and soap at the HWS was higher at one-month follow-up, compared to at the five and seven-month follow-ups. A possible explanation for this might be that the initial soap supply had not yet ended at one-month follow-up, and thus replenishing the HWS in soap was not yet a (financial) burden.

Our findings are consistent with handwashing promotion study findings in Bangladesh published by Guiteras et al (2016) [66]. The authors found that, in the compounds which ceased to receive free soap, the proportion of observed compounds with both water and soap next to the latrine went from 51% (3.5-month follow-up) to 36% (seven-month follow-up) [66]. This was in contrast to the pattern observed in the compounds group which continued to receive a free soap supply (63% at 3.5-month and seven-month follow-ups). As pointed out by Guiteras et al. (2016), the differences observed between both soap supply groups could be explained by the new transaction cost attached to keeping the handwashing area supplied with soap [66]. This would include having to track those whose turn it would be to replenish the HWS with soap, and ensure that they did so [66]. Although, the cost of soap was cheap, the new cost attached to maintaining the handwashing area with soap was still meaningful enough to act as a deterrent to participants [66].

We observed a somewhat higher proportion of HWS with both water and soap in the TNSB-based group compared to the HWS-only group, at all follow-up assessment points. While this could be a chance finding, it may indicate that, based on the TNSB intervention success, participants in this trial arm may have had more motivation to maintain the HWS. As there was no HWWS promotion message in the HWS-only intervention group, the importance of HWWS after using the toilet, and therefore having soap at the handwashing location may not have been assimilated to the same extent.

In our study, the proportion of observations when HWS were recorded to have both water and soap at the HWS was 80% in the TNSB-based intervention at one-month follow-up. This proportion decreased to 49%, at five-month follow-up. However, the observed proportion of occasions when HWWS occurred after using the toilet, at each follow-up, remained comparable (24% and 22% respectively). This brings into question the reliability of using the presence of both water and soap at the handwashing location as proxy indicators for HWWS practices, and thus as a measure of intervention success. As argued by Chase et al. (2012), handwashing proxy indicators may indicate the direction of intervention effect, but should not be relied on to provide point estimates that are close to accurate [78]. As also reported by Wolf et al. [21], the presence of water and soap at the handwashing location or changes in availability of these goods may not reflect the actual level of handwashing with soap practices.

We observed 60% of HWS with both water and soap present in the TNSB-based group, at seven-month follow-up. This is comparable to the observed proportions in four [46, 99, 105] of the six identified studies, from the systematic review reported in Chapter 3., which supplied HWS, as part of their interventions, and provided data on the supplied HWS sustainability. The small differences observed with our study results may be due to chance. In the study by Ram et al. (2017) [45], as soap was replenished as needed throughout the trial, it is surprising that the authors found a smaller percentage (i.e. 39%) of maintained HWS six-week post-intervention delivery. In that case, the intervention may not have been successful at inculcating new habits in the study participants. Briceño et al. (2017) [91] did not report HWS sustainability data. Table 12.8 describes and presents the results of the identified studies in comparison with our study.

It is important to point out that our HWS maintenance assessment was made at one point in time. This may have underestimated the proportion of HWS which were maintained. For instance, there were occasions when HWS were replenished with water and/or soap during observation sessions. The proportion of maintained HWS may thus be underestimated.

A limitation of our study is that the estimates may be imprecise, due to the relatively small simple size. However, our findings are relatively similar to the results reported by the comparable studies identified. Three of these studies (i.e. Parvez, Ram, Biswas) had relatively larger sample sizes.

Conclusion

Our study findings showed that the HWS was well accepted by the study population. Additionally, the facilities generally lasted well. In terms of design, the component that needs improvement is the stand, which should be built with a stronger material. Nevertheless, even after seven months, there was a relatively high proportion of HWS that showed signs of maintenance, perhaps more so in the TNSB-based intervention group.

Table 12.8. Description and results of comparable studies

Authors (year)	Setting	Study type	Type of HW equipment	Behaviour change motive used	HWS sustainability results:
PhD trial	Côte d'Ivoire	CRCT	HWS and four 50 cl bottles of liquid soap *(soap non replenished)*	Disgust	- *Baseline:* No HWS or observed fixed handwashing location after using the toilet - *One month*: 80% (37/46) of observations with HWS with both water and soap present. - Five *months*: 49% (21/43) of observations with HWS with both water and soap present. - Seven *months*: 60% (12/23) of compounds with HWS with both water and soap present, and 74% in good condition
Relevant studies identified					
Biswas (2012)	Bangladesh	CRCT	HWS and one 1,5 L plastic bottle to make soapy water.	Health	- Baseline: 22% of households with water and soap present at the handwashing location - *13 months*: 60% of households with water and soap present at the handwashing location
Briceño et al. (2017)	Tanzania	CRCT	Technical assistance provided to build tippy taps	Health Aspiration	- *Baseline:* 48% of households with some sort of HWS - ≥ 12 months: 17% (22/128) of facilities built observed to be present. No sustainability assessment reported.
Christensen et al. (2015) - Kakamega	Kenya	CRCT	Tippy taps with limited quantity of small powder soup packs for soapy water	Mixed emotions	- *Baseline*: 94% of households with multipurposed basin, including for handwashing, with 12% with water and soap present. - Four *months*: 57% of HWS with water and soap present at both handwashing locations.
Christensen et al. (2015) - Bungoma	Kenya	CRCT	Tippy taps with limited quantity of small powder soup packs for soapy water	Mixed emotions	- *Baseline*: 29% of HWS with water and soap present at handwashing location. - Four *months*: 56% of HWS with water and soap present at both handwashing locations.
Parvez et al. (2018)	Bangladesh	CRCT	Handwashing station supply with soapy water bottle with regular supply of detergent sachets	Health Nurture Social norms	- *15 months:* 66% to 77% of HWS with water and soap present.
Ram et al. (2017)	Bangladesh	RCT	3 HWS supplied per household with soap - Soap replenished throughout the trial as needed	Health and nurture	- Baseline: 22% of households with water and soap present at HWS - *6 weeks:* 39% of households with water and soap present at HWS

Chapter 13 - An Analysis of the Association of Norms-Related Constructs and Handwashing with Soap at Baseline, and of Changes in Norms-Related Constructs over Time and by Trial arm

This chapter presents an analysis of the interventions' effects on the perceived descriptive and injunctive norms and behaviour publicness around handwashing with soap (HWWS) after using the toilet. These were measured within the trial, at baseline, and at the one-month and five-month post intervention. The characteristics of the trial population have already been presented in detail in Chapter 10.

This chapter is divided in four sections:

1. A reassessment of the psychometric properties of the handwashing norms-related scales
2. A descriptive analysis of the handwashing norms-related constructs at baseline
3. An analysis of the association between HWWS after using the toilet and the perceived norms-related constructs at baseline
4. An analysis of the interventions' effects on the perceived norms-related constructs around HWWS after toilet use

1. Psychometric properties of the handwashing norms-related scales

The psychometric properties of each scale were initially evaluated in January 2014, when the scales were designed (see Chapter 6). This section comprises a re-evaluation of the scales combining the trial's baseline data (August 2014) with the one-month follow-up (September 2016) and five-month follow-up (January 2017) data. We did so to confirm that the scales' psychometric properties were not previously obtained by chance. A total of 523 residents were surveyed (all trial phases combined).

The Spearman-Brown coefficient indicated moderate to strong correlations between items designed to assess the same construct (Table 13.1). Confirmatory factor analysis (CFA) was used to assess the measurement properties of the scale measuring the perceived injunctive norm around HWWS after toilet use (HWIN). The ordered probit model converged, indicating that the specific factor was identified (Figure 1). The scale appeared reliable (α=0.74, N=522). The scales measuring the perceived descriptive norm around HWWS (HWDN) and the perceived publicness of HWWS (HWP) after toilet use were both internally consistent and reliable (respectively, ρ=0.78, α=0.89, N=521 and ρ=0.75, α=0.86, N=520). Table 13.2 summarises the psychometric properties of the scales, alongside the results from the 2014 scale design study (Chapter 6), for comparison purposes. The results for the HWDN are remarkably consistent. The results for the HWP and HWIN scales appeared to be relatively better and relatively weaker respectively in this second assessment, compared to the first one.

Table 13.1. Matrix of scales inter-item correlation (Spearman-Brown coefficient)

Scale item	d1	d2	i1	i2	i3	bp1	bp2
HWDN							
d1	.	.	.	.	.	.	.
d2	0.77	.	.	.	.	.	.
HWIN							
i1	.	.	.	.	.	.	.
i2	.	.	0.37	.			
i3	.	.	0.41	0.60	.	.	.
HWP							
bp1	.	.	.	.	.	.	
bp2	.	.	.	.	.	0.74.	.

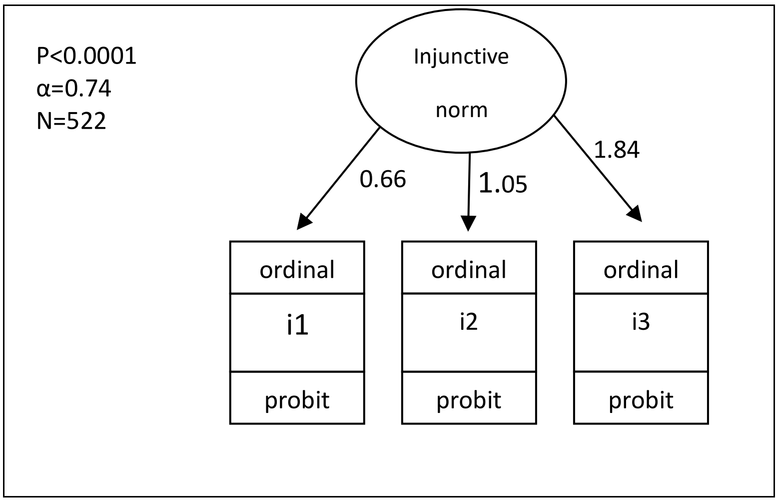


Figure 13.1. HWIN scale properties (unstandardized estimates)

Table 13.2. Summary of the psychometric properties of the HWDN, HWP and HWIN compared to the ones from the 2014 scales design study

Table 13.2. Summary of the psychometric properties of the HWDN, HWP and HWIN compared to the ones from the 2014 scales design study				
	2014 scales design		**Trial**	
Scale items	**Scale reliability (ρ) (95% CI)**	**Scale reliability (α) (95% CI)**	**Scale reliability (ρ) (95% CI)**	**Scale reliability (α) (95% CI)**
HWDN (2 items)	0.74 (0.65-0.83)	0.88[a] (0.83-0.93)	0.78 (0.74-0.82)	0.89[a] (0.86-0.91)
HWP (2 items)	0.63 (0.52-0.73)	0.78[a] (0.71-0.86)	0.75 (0.70-0.80)	0.86[a] (0.83-0.89)
HWIN (3 items)	.	0.83 (0.78-0.88)	.	0.74 (0.69-0.79)

[a]Alphas computed for comparison purposes.

The results of the first (Chapter 6) and second psychometric testing of the handwashing norms-related scales suggest that we achieved some degree of success in designing scales to measure the perceived HWDN, HWIN and HWP around HWWS after toilet use which are applicable to economically disadvantaged communities in Côte d'Ivoire.

2. Descriptive analysis of the handwashing norms-related constructs at baseline

At baseline, we interviewed 175 (86%) of the 203 residents selected for the survey. 121 (69%) of 175 respondents were women, and 95 (55%) of 174 respondents (excluding one respondent who refused to answer this question) were between 16 and 34 years old (Table 13.3).

We had used two masking statements (statements 5 and 6) to assess respondents' propensity for acquiescence bias, as stated in Chapter 9. One statement posited that men took over women's domestic chores, when they returned from work (statement 5). The second statement suggested that men helped women in their domestic chores (statement 6). We had expected that most participants would disagree or strongly disagree with the statements, if this bias was low. At baseline, 158 (91%) and 137 (79%) of 175 respondents disagreed or strongly disagreed with statements 5 and 6 respectively suggesting that the level of acquiescence bias was low.

Table 13.3. Age and sex distribution of respondents to the handwashing norms-related scales at baseline (August-September 2014)

Characteristics	Male n (%)	Female n (%)	Total n (%)
Age groups *(years)*			
16-24	8 (14.8)	21 (17.5)	29 (16.7)
25-34	21 (38.9)	45 (37.5)	66 (37.9)
35-44	15 (27.8)	33 (27.5)	48 (27.6)
45-54	3 (5.6)	16 (13.3)	19 (10.9)
55-64	3 (5.6)	4 (3.3)	7 (4.0)
65+	4 (7.4)	1 (0.8)	5 (2.9)
Total	54 (31.0)	120 (69.0)	174

2.1. Perceived norms-related constructs around HWWS after toilet use at baseline

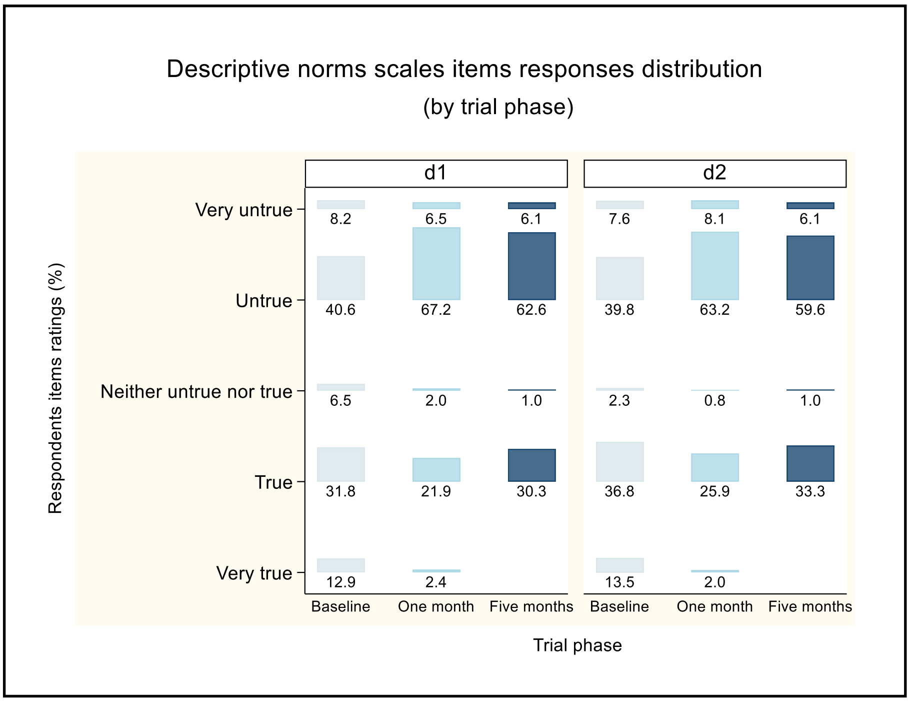
All scale items' responses had a bimodal distribution with few respondents answering 'neither untrue nor true'. Less than 3% of respondents answered 'I do not know'. We thus excluded these latter from the analyses. In general, the perceived norms-related constructs around HWWS after toilet use were not strong at baseline. Additionally, there seemed to be some imbalances in the norms-related constructs level between intervention groups (Table 13.4). In general, norms-related constructs seemed to be less weak and relatively similar in the HWS-only and TNSB-based trial arms, compared to the control group where they seemed to be lower.

Table 13.4. Norms-related scale items' response distribution at baseline by trial arm *(Intention-to-treat)*

	Control arm n=56*		HWS-only arm n=55*		TNSB-based arm n=60	
Scales	Untrue to very untrue	95% CI	Untrue to very untrue	95% CI	Untrue to very untrue	95% CI
Descriptive						
d1	23 (41.8)	30.2-54.4	26 (46.4)	33.1-60.3	34 (57.6)	45.1-69.2
d2	18 (32.7)	20.9-47.2	29 (51.8)	37.8-65.5	34 (56.7)	42.5-69.8
Publicness						
bp1	13 (23.2)	13.5-36.9	22 (40.0)	25.9-56.0	22 (36.7)	23.3-52.4
bp2	16 (27.6)	16.6-42.2	25 (45.5)	32.7-58.8	26 (43.3)	28.5-59.5
Injunctive						
i1	35 (63.6)	50.4-75.1	43 (78.2)	64.2-87.7	45 (75.0)	59.8-85.8
i2	22 (40.0)	25.6-56.3	18 (32.1)	20.3-46.8	29 (50.9)	38.3-63.4
i3	26 (46.4)	32.4-61.0	31 (57.4)	43.5-70.3	35 (60.3)	48.7-70.9
*Missing data for 2 households						

2.1.1. Perceived descriptive norm at baseline

Across all trial arms, the perceived HWDN around HWWS after toilet use was not strong, at baseline. For items d1 and d2, around half of respondents agreed or strongly agreed with the perception that few people in their compounds performed HWWS after toilet use. Comparable proportions disagreed or strongly disagreed. Figure 13.2 presents the distribution of HWDN scale items' responses by trial phase. he x-axis shows the responses for each item at different time points, while the y-axis shows the proportion of respondents choosing each category. For example, the top of the figure shows how the proportion of "very untrue" responses varied over time for each item. The scale's mean and median scores were 2.96 and 3 respectively (Table 13.5).



d1: In your compound, except you, few people wash their hands with soap after using the toilet.

d2: You think that there are not many people who wash their hands with soap after using the toilet in your compound.

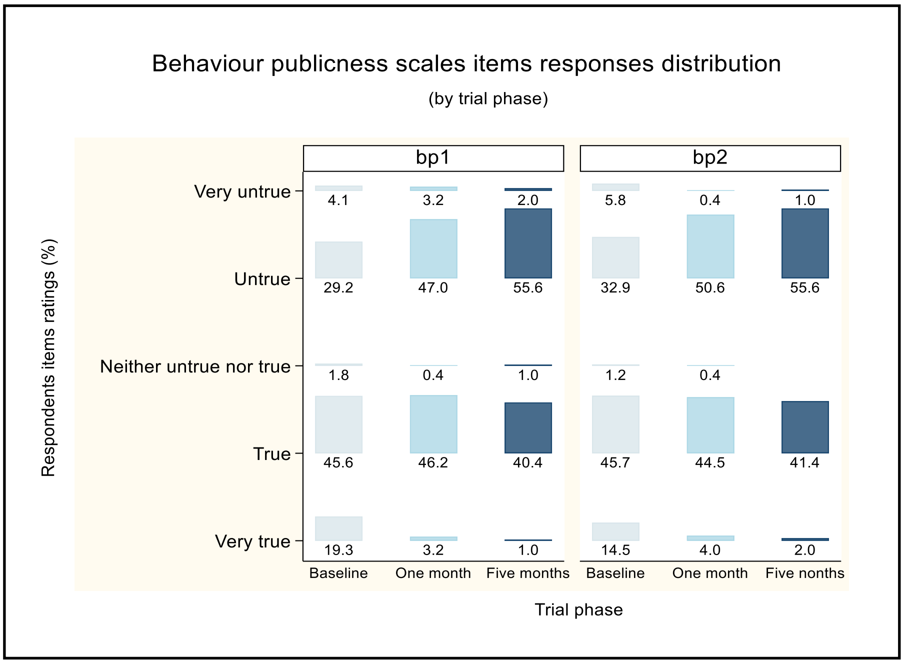
Figure 13.2. Graph of the perceived handwashing descriptive norm scale items responses distribution at baseline.

Table 13.5. Mean and median scores of the perceived descriptive norm around handwashing with soap after toilet use at baseline (August-September 2014)

Scale	
Descriptive (mean score, (95% CI))*	2.96 (2.77-3.16)
d1	2.99 (2.79-3.20)
d2	2.91 (2.70-3.12)
Descriptive (median (IQR))*	3
d1	3 (1-5)
d2	2 (1-5)

2.1.2. Perceived behaviour publicness at baseline

The HWP of HWWS after using the toilet was weaker than the HWDN at baseline, across trial arms. For items bp1 and bp2, almost two thirds of respondents agreed or strongly agreed with the notion that it was hard to observe the HWWS practices after using the toilet of fellow residents in their compounds (Figure 13.3). This was compared to a little over one third of respondents who disagreed or strongly disagreed. The scale mean and median scores were 2.62 and 2 respectively (Table 13.6). The difference between the mean and the median is due to the fact the median is more sensitive to small changes in the bimodal distribution than the mean.



bp1: In the compound, it is not easy to notice who washes and who does not wash their hands with soap after using the toilet.

bp2: It is difficult to see who washes their hands with soap after using the toilet, in the compound.

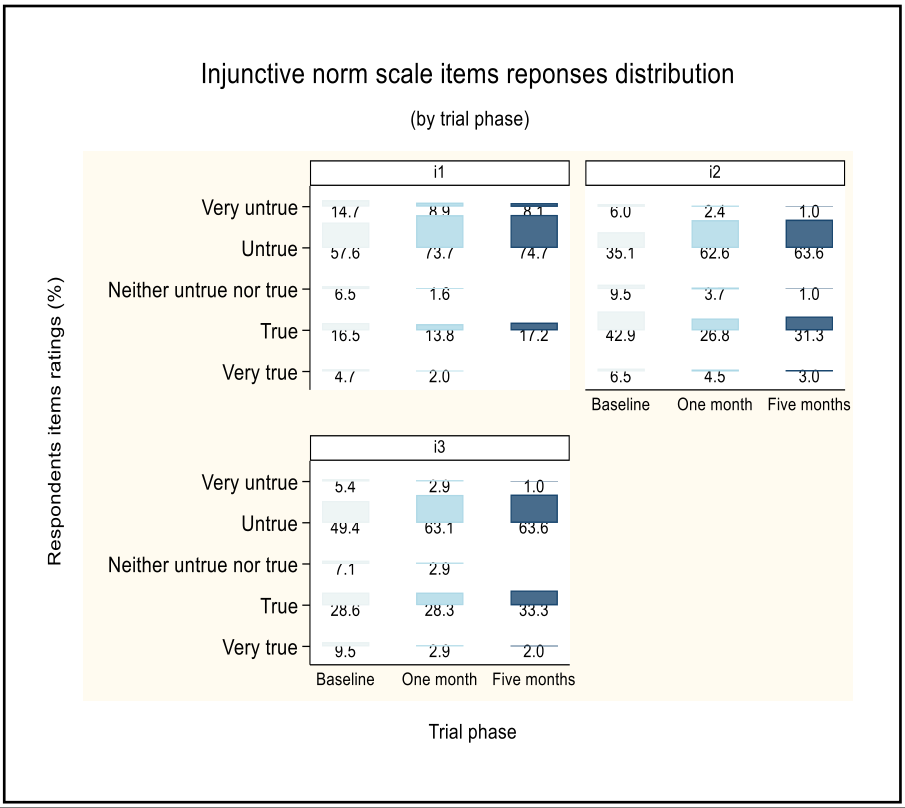
Figure 13.3. Graph of the perceived handwashing publicness scale items responses distribution at baseline

Table 13.6. Mean and median scores of the perceived behaviour publicness around HWWS after toilet use at baseline (August-September 2014)

Scale	
Publicness (mean score, (95% CI)) bp1 bp2	2.62 (2.42-2.82) 2.53 (2.32-2.74) 2.70 (2.48-2.92)
Publicness (median (IQR)) bp1 bp2	2 2 (1-5) 2 (1-5)

2.1.3. Perceived injunctive norm at baseline

Across trial arms, and compared to the HWDN and HWP, the perceived HWIN around HWWS after toilet use lied towards the strong end of the scale, at baseline. For items i1, i2 and i3, 123 (73%), 69 (41%) and 92 (54%) of 170 and 168 respondents respectively disagreed or strongly disagreed with the perception that compounds residents did not think that HWWS after toilet use was important (Figure 13.4). By contrast, 36 (21%), 83 (49%) and 64 (39%) of respondents agreed or strongly agreed with the scale items. The scale mean and median scores were 3.22 and 3.33 respectively (Table 13.7). Respondents tended to rate the first item differently from the remaining two items



i1: Except you, few people see HWWS after using the toilet as important, in your compound.

i2: The majority of people in your compound do not care about HWWS after using the toilet.

i3: Except you, HWWS after using the toilet is not something that people from your compound think about.

Figure 13.4. Graph of the perceived handwashing injunctive norm scale items responses distribution at baseline

Table 13.7. Mean and median scores of the perceived injunctive norm around handwashing with soap after toilet use at baseline (August-September 2014)

Scale	
Injunctive (mean score, (95% CI))	3.22 (3.07-3.36)
i1	3.61 (3.44-3.78)
i2	2.91 (2.72-3.10)
i3	3.13 (2.93-3.31)
Injunctive (median (IQR))	3.33
i1	4 (1-5)
i2	3 (1-5)
i3	4 (1-5)

2.1.4. *Hypothesised* perceived outcome expectation at baseline

As discussed in Chapter 6, we were not successful in designing a scale to measure the outcome expectation around HWWS after toilet use (HWOE) in our study population. Nevertheless, the key motive used in national handwashing campaigns in Côte d'Ivoire is health and, as reported in Chapter 5, approximately two third of respondents cited disease avoidance as the reason why they washed their hands with soap in our 2012 pilot study. Similarly, as reported in Chapter 10, study participants in the TNSB-based group tended to use health-related language to describe the scenes in the intervention videos. Based on these observations, we hypothesised that the perceived HWOE at baseline was a 'low benefit' health one.

3. A compound-level analysis of the association between HWWS after using the toilet and the perceived norms-related constructs at baseline

- **Deviation from protocol**

As mentioned in Chapter 10, STATA® excluded some compounds from the regression analyses by default, due to some data on covariates being missing. The precision of our estimates was thus reduced. Given the already small number of households surveyed by study phase, we decided to only compute and present the unadjusted regression analysis results, enabling the inclusion of all compounds.

As stated in Chapter 9, we used random effects logistic regression models to look at the association between HWWS after toilet use and each norm-related construct. Individual responses to the norms-related scales could not be linked to individual handwashing events. This was because handwashing practices data were collected at the compound as opposed to at the individual level. The analysis was essentially conducted at compound level.

We did not observe obvious trends in the scatter plots of the association between HWWS after toilet use and the perceived norms-related constructs, at baseline (Figures 13.5, 13.6 and 13.7).

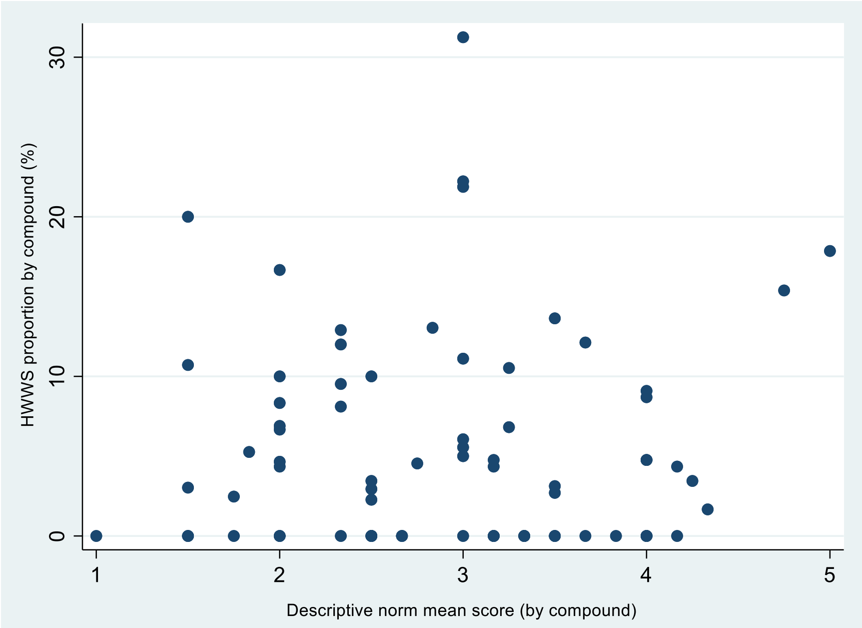


Figure 13.5. Scatterplot of the association between HWWS after toilet use and the perceived HWDN

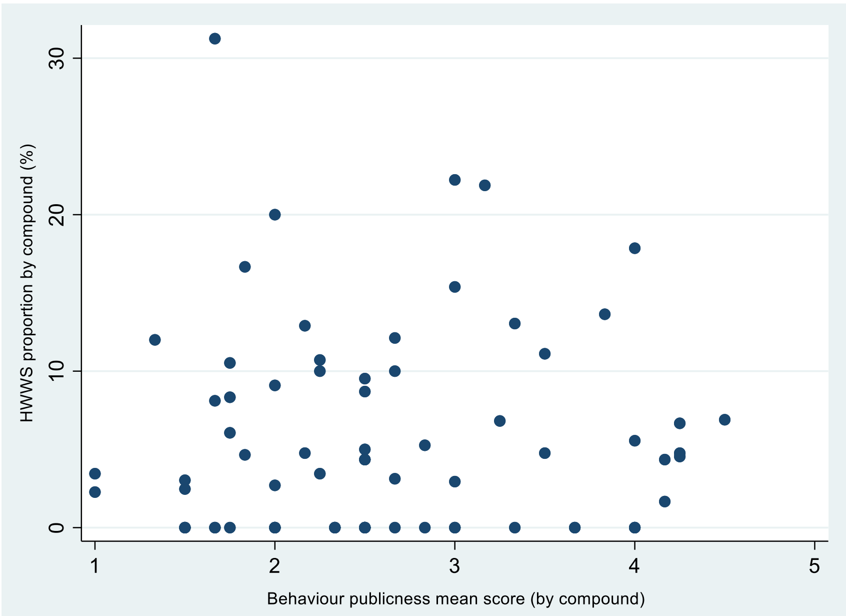


Figure 13.6. Scatterplot of the association between HWWS after toilet use and the perceived HWP

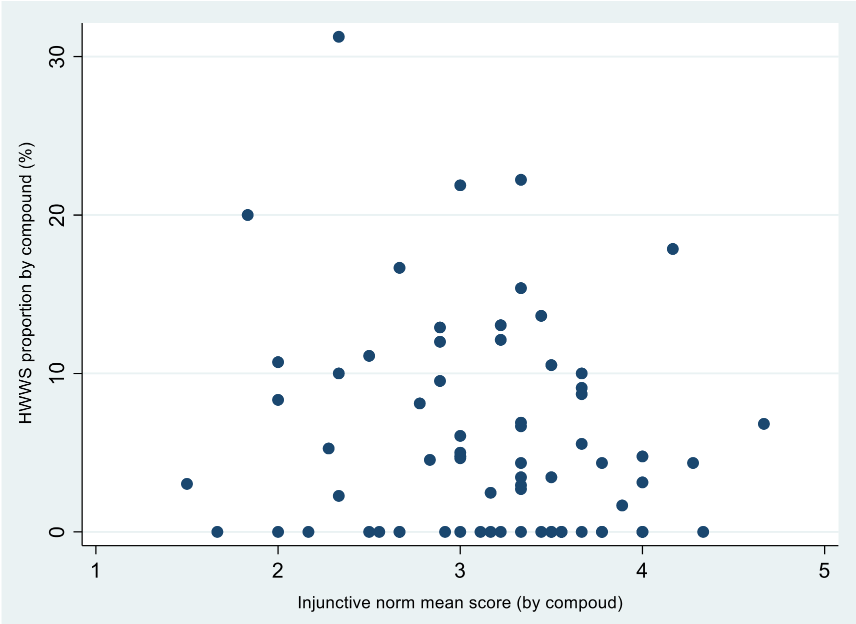


Figure 13.7. Scatterplot of the association between HWWS after toilet use and the perceived HWIN

We found no evidence of an association between HWWS after toilet use and the perceived HWDN (*P*=0.90) or the perceived HWP (*P*=0.72) (Table 13.8). For the perceived HWIN, whilst there was no strong statistical evidence of an association with HWWS after using the toilet (*p*=0.25), we found that for a 1 unit increase in this norm, the odds of HWWS after using the toilet decreased by 23%. Nevertheless, the relatively large decrease observed, could indicate an important association between the perceived HWIN and HWWS after toilet use. The direction of the association is however contradictory to what we would expect, and to what social norms theories predict.

Table 13.8. Unadjusted analyses of the association between HWWS after using the toilet and the perceived norms-related constructs at baseline *(random effects logistic model) (Intention-to-treat)*

	(n=74 compounds)*	
	Unadjusted	
	OR (95% CI)	p-value LRT
Perceived norms-related construct Descriptive	1.02 (0.74-1.42)	0.90
Perceived norms-related construct Publicness	1.06 (0.77-1.46)	0.72
Perceived norms-related construct Injunctive	0.77 (0.50-1.19)	0.25

*Analysis conducted at compound level.

4. An analysis of the interventions' effects on the perceived norms-related constructs around HWWS after toilet use

As mentioned in Chapters 9 and 10, due to the issues encountered with the production company in charge of making the intervention videos, there was a two-year gap between baseline and the first follow-up phase (i.e. August-September 2014 and September-November 2016). This was also the period of the substantial West-African Ebola epidemic as discussed in Chapter 10.

We interviewed 249 (83%) of the 297 residents selected for the survey. 174 (70%) of 249 respondents were women, and 155 (62%) of 249 respondents were between 16 and 34 years old at the one-month follow-up round (Table 13.9). The results of the acquiescence bias assessment were similar to that found at baseline. 210 (85%) and 161 (65%) of 249 respondents disagreed to strongly disagreed with statements 5 and 6 respectively.

- **Deviation from protocol**

Due to the bimodal distribution of the scales items responses, in order to analyse the data sensibly, we decided to recode the items as binary variables. 'Neither untrue nor true' answers were grouped with 'True' and 'Very true' answer categories to form one 'do not disagree' response category. This was opposed to 'Untrue' and 'Very untrue' which were grouped together to form a single 'Untrue' response category. We then used random effects logistic regression to assess the change over time in the norms-related constructs, and whether the changes observed were due to the interventions received.

Table 13.9. Age and sex distribution of respondents to the handwashing norms-related scales at one-month follow-up (September-November 2016)

Characteristics	Male n (%)	Female n (%)	Total n (%)
Age groups (years)			
16-24	18 (24.0)	42 (24.1)	29 (16.6)
25-34	26 (34.7)	69 (40.0)	66 (37.7)
35-44	17 (22.7)	33 (19.0)	48 (27.4)
45-54	8 (10.7)	16 (9.2)	19 (10.9)
55-64	2 (2.7)	9 (5.2)	7 (4.0)
65+	4 (5.3)	5 (2.9)	5 (2.9)
Total	75 (30.1)	174 (69.9)	249

At the five-month follow-up, we interviewed 99 (76%) of 131 residents selected for the survey. 63 (64%) of 99 respondents were women, and 58 (59%) of 99 respondents were between 16 and 34 years old (Table 13.10). 87 (88%) and 72 (73%) of 99 respondents disagreed or strongly disagreed with the acquiescence bias test statements 5 and 6 respectively at the five-month follow-up. This was comparable to the proportions found at baseline and at the one-month follow-up.

Table 13.10. Age and sex distribution of respondents to the handwashing norms-related scales at five-month follow-up (January-March 2017)

Characteristics	Male n (%)	Female n (%)	Total n (%)
Age groups *(years)*			
16-24	9 (25.0)	12 (19.0)	21 (21.2)
25-34	10 (27.8)	27 (42.9)	37 (37.4)
35-44	6 (16.7)	14 (22.2)	20 (20.2)
45-54	9 (25.0)	7 (11.1)	16 (16.2)
55-64	1 (2.8)	3 (4.8)	4 (4.0)
65+	1 (2.8)	0 (0.0)	1 (1.0)
Total	36 (36.4)	63 (63.6)	99

4.1. Changes in the norms related-constructs over time

We found strong evidence that the norms-related constructs increased over the course of the trial (Graphs 13.1, 13.2 and 13.3 in Section 2 and Table 13.11). At the one-month and five-month post initial intervention delivery, participants had approximately 3.35 times the odds of disagreeing with the notion that few people HWWS after using the toilet in their compound for each HWDN item, compared to baseline ($P<0.0001$). Although there was a decrease in the HWDN, at the five-month follow-up round, there was still evidence that it was stronger than at baseline (OR≈2.50 for each scale item, $P=0.001$). For the HWP, respondents were 2.14 (bp1) and 1.68 (bp2) times more likely to disagree with the notion that HWWS after toilet use was difficult to observe in their compounds, at the one-month follow-up round ($P<0.0001$ and $P=0.013$ respectively) compared to baseline. The increase in odds was even greater at the five-month follow-up round 3.03 (bp1, $P<0.0001$) and 2.18 (bp2, $P=0.003$).

We found evidence that the HWIN increased at one-month follow-up compared to the baseline observations (Table 13.10). Participants had 1.81 (i1), 2.84 (i2) and 1.67 (i3) times the odds of disagreeing with the notion that HWWS after using the toilet was not important to their fellow compound residents compared to the baseline estimates ($P=0.013$, $P<0.0001$ and $P=0.018$, respectively). However, we found weak evidence of a difference in participants' odds of disagreeing with items i1 and i3 at the five-month follow-up round compared to baseline

(OR=1.84, *P*=0.05 and OR=1.59, P=0.09 respectively). On the other hand, we still found strong evidence of a difference in the way participants rated item i2 compared to baseline (P<0.0001). The difference was comparable to that observed at one-month follow-up.

Table 13.11. Unadjusted analyses of the changes over time in the perceived descriptive norm, behaviour publicness and injunctive norm around HWWS after toilet use (Intention-to-treat random effects logistic model)

	Disagree vs agree	
	(n=75 compounds)	
Unadjusted		
	OR (95% CI)	p-value LRT
	Descriptive norm	
	Item d1	
Trial phase Baseline One-month Five months	 1.0 3.36 (2.14-5.27) 2.60 (1.48-4.56)	 <0.0001 0.001
	Item d2	
Baseline One-month Five months	1.0 3.34 (2.12-5.26) 2.49 (1.42-4.39)	 <0.0001 0.002
	Behaviour publicness	
	Item bp1	
Baseline One-month Five months	1.0 2.14 (1.40-3.27) 3.03 (1.76-5.21)	 <0.0001 <0.0001
	Item bp2	
Baseline One-month Five months	1.0 1.68 (1.12-2.53) 2.18 (1.29-3.68)	 0.013 0.003
	Injunctive norm	
	Item i1	
Baseline One-month Five months	1.0 1.81 (1.13-2.90) 1.84 (0.99-3.43)	 0.013 0.054
	Item i2	
Baseline One-month Five months	1.0 2.84 (1.86-4.34) 2.84 (1.66-4.88)	 <0.0001 <0.0001
	Item i3	
Baseline One-month Five months	1.0 1.67 (1.09-2.54) 1.59 (0.93-2.73)	 0.018 0.092

4.2. Interventions effects on the perceived norms-related constructs around HWWS after toilet use

We also investigated whether there was any difference in the way participants rated the scales' items depending on the trial arm, at each follow-up round. Table 13.12 presents the distribution of responses to the norms-related scales' items by trial arm and follow-up round. Due to the imbalances observed in the norms-related constructs strength between study groups, we tried to adjust for the baseline proportions for each scales' items. However, it was not predictive of the end-line measures and did not change the conclusion (Appendix 13.1).

We found evidence that the manner in which respondents rated the scale items varied between the trial arms at one-month follow-up (Tables 13.13 to 13.15). For the HWDN, respondents in the HWS-only intervention group had 3.38 (d1) and 4.13 (d2) times the odds of disagreeing with the notion that few people in their compound HWWS after using the toilet compared to the control arm (*P*<0.001 for both items). In the TNSB-based intervention arm, the odds were 14.24 (d1) and 12.06 (d2) times greater than in the control arm (*P*<0.001 for both items). Regarding the HWP, participants in the HWS-only intervention arm had 2.39 (bp1) and 2.78 (bp2) times the odds of disagreeing with the notion that it was hard to notice HWWS practices in the compound, compared to the control arm (*P*=0.011 and *P*=0.013 respectively) (Table 13.14). In the TNSB-based intervention group, the odds were 2.78 (bp1) and 2.47 (bp2) times greater than in the control group (*P*=0.004 and *P*=0.005 respectively).

For the HWIN, we found weak evidence of a difference in the manner in which participants rated items i1 (OR=1.90, *P*=0.1) and i3 (OR=2.07, *P*=0.06) in the HWS-only intervention group (Table 13.15). However, for item i2, respondents had 2.14 times the odds of disagreeing with the notion that residents in their compound did not care about HWWS after toilet use compared to the control group (*P*=0.02). By contrast, in the TNSB-based intervention arm, participants had 4.50 (i1), 4.42 (i2) and 11.79 (i3) times the odds of disagreeing with the notion that fellow compound residents did not see HWWS after using the toilet as an important practice (*P*=0.002 for item i1 and *P*<0.001 for items i2 and i3 respectively).

At five months post-intervention delivery, we did not find evidence of a difference between trial arms in the way respondents rated each scale item (Table 13.13 to 13.15).

Table 13.12. Norms-related scales items response distribution at the one-month and five-month follow-ups by trial arm *(Intention-to-treat)*

One month						
	Control arm n=81*		HWS-only arm n=85		TNSB-based arm n=81	
Scales	Untrue to very untrue	95% CI	Untrue to very untrue	95% CI	Untrue to very untrue	95% CI
Descriptive						
d1	41 (50.6)	40.7-60.4	66 (77.7)	67.4-85.4	75 (92.6)	85.1-96.5
d2	37 (45.7)	35.8-55.9	66 (77.7)	67.2-85.5	73 (90.1)	81.0-95.1
Publicness						
bp1	29 (35.8)	25.6-47.5	48 (56.5)	44.3-67.9	47 (58.0)	47.0-68.3
bp2	30 (37.0)	27.7-47.5	48 (56.5)	47.1-65.4	48 (59.3)	48.4-69.3
Injunctive						
i1	59 (72.8)	62.8-81.0	71 (83.5)	75.4-89.4	74 (91.4)	82.3-96.0
i2	39 (48.8)**	38.9-58.7	57 (67.1)	55.1-77.2	64 (79.0)	69.1-86.4
i3	37 (46.8)***	36.2-57.8	53 (63.1)****	50.9-73.8	71 (87.7)	75.3-94.3
Five months						
	Control arm n=34		HWS-only arm n=35		TNSB-based arm n=30	
Scales	Untrue to very untrue	95% CI	Untrue to very untrue	95% CI	Untrue to very untrue	95% CI
Descriptive						
d1	19 (55.9)	36.2-73.8	24 (68.6)	50.5-82.3	25 (83.3)	61.3-94.1
d2	17 (50.0)	30.6-69.4	24 (68.6)	48.3-83.6	24 (80.0)	58.0-92.0
Publicness						
bp1	19 (55.9)	39.3-71.2	20 (57.1)	40.6-72.2	18 (57.6)	43.6-72.2
bp2	20 (58.8)	41.2-74.4	18 (51.43)	36.3-66.3	18 (60.0)	41.4-76.1
Injunctive						
i1	28 (82.4)	64.1-92.4	27 (77.1)	59.4-88.6	27 (90.0)	73.9-96.6
i2	21 (61.8)	43.1-77.5	19 (54.3)	38.0-69.7	24 (80.0)	58.9-91.8
i3	22 (64.7)	45.9-79.9	18 (51.4)	34.4-68.1	24 (80.0)	61.5-90.9
*Missing data for 2 households **Missing data for 3 households ***Missing data for 4 households ****Missing data for 1 household						

Table 13.13. Unadjusted analyses of the difference in the way respondents rated the norms-related scale items, by trial arm and trial phase (Intention-to-treat random effects logistic model)

Disagree vs agree						
One-month follow-up (n=71 compounds)						
Unadjusted						
	OR (95% CI)	p-value LRT	OR (95% CI)	p-value LRT	OR (95% CI)	p-value LRT
	Descriptive norm		**Behaviour publicness**		**Injunctive norms**	
	Item d1		Item bp1		Item i1	
Trial phase						
Control	1.0		1.0		1.0	
HWS-only	3.38 (1.73-6.63)	<0.001	2.39 (1.22-4.70)	0.011	1.90 (0.89-4.07)	0.10
TNSB-based	14.24 (5.21-38.92)	<0.001	2.78 (1.40-5.54)	0.004	4.50 (1.70-11.89)	0.002
	Item d2		Item bp2		Item i2	
Control	1.0		1.0		1.0	
HWS-only	4.13 (2.11-8.09)	<0.001	2.21 (1.18-4.11)	0.013	2.14 (1.14-4.02)	0.018
TNSB-based	12.06 (4.94-29.40)	<0.001	2.47 (1.31-4.66)	0.005	4.42 (2.16-9.01)	<0.001
					Item i3	
Control					1.0	
HWS-only	.	.	.	.	2.07 (0.97-4.41)	0.06
TNSB-based					11.79 (4.20-33.09)	<0.001
Five months follow-up (n=62 compounds)						
	OR (95% CI)	p-value LRT	OR (95% CI)	p-value LRT	OR (95% CI)	p-value LRT
	Item d1		Item bp1		Item i1	
Trial phase						
Control	1.0		1.0		1.0	
HWS-only	2.10 (0.46-9.66)	0.34	1.05 (0.41-2.73)	0.92	0.71 (0.19-2.60)	0.602
TNSB-based	5.23 (0.82-33.15)	0.08	1.18 (0.44-3.21)	0.74	1.97 (0.40-9.73)	0.404

Unadjusted analyses of the difference in the way respondents rated the norms-related scale items *(continued)*

Five months follow-up (n=62 compounds)						
	OR (95% CI)	p-value LRT	OR (95% CI)	p-value LRT	OR (95% CI)	p-value LRT
	Item d2		Item bp2		Item i2	
Trial phase						
Control	1.0		1.0		1.0	
HWS-only	4.75 (0.47-47.66)	0.19	0.74 (0.29-1.92)	0.54	0.71 (0.22-2.23)	0.55
TNSB-based	8.94 (0.67-118.83)	0.10	1.05 (0.39-2.85)	0.92	2.76 (0.70-10.93)	0.149
					Item i3	
Control					1.0	
HWS-only	.	.	.		0.53 (0.16-1.73)	0.30
TNSB-based					2.24 (0.59-8.46)	0.23

Figures 13.8 to 13.10 conceptualise the measured norms-related constructs and HWWS frequencies in each trial arm at each trial phase, including what we believe was happening with the perceived HWOE.

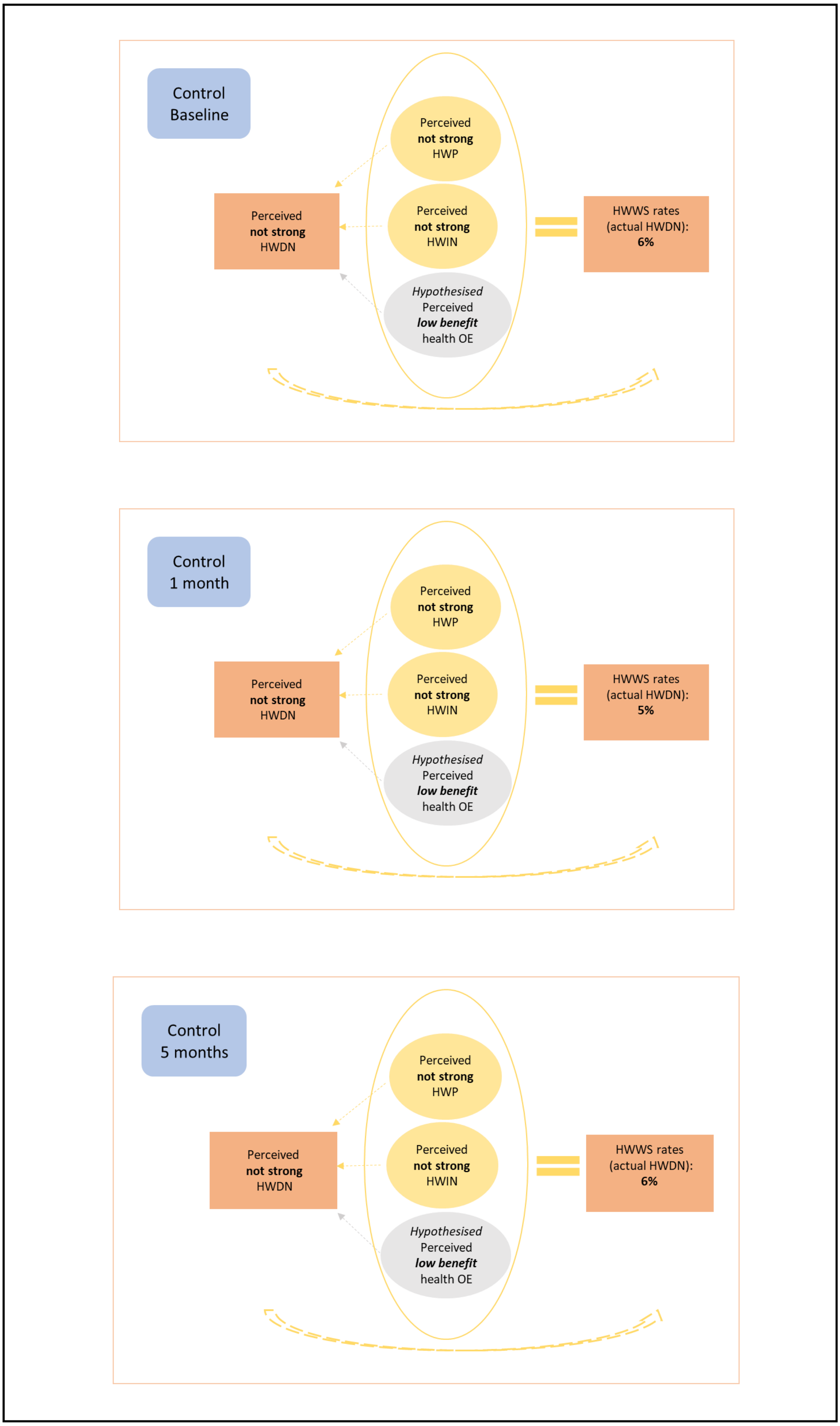


Figure 13.8. Perceived norms-related constructs around HWWS after toilet use in the control group by trial phase

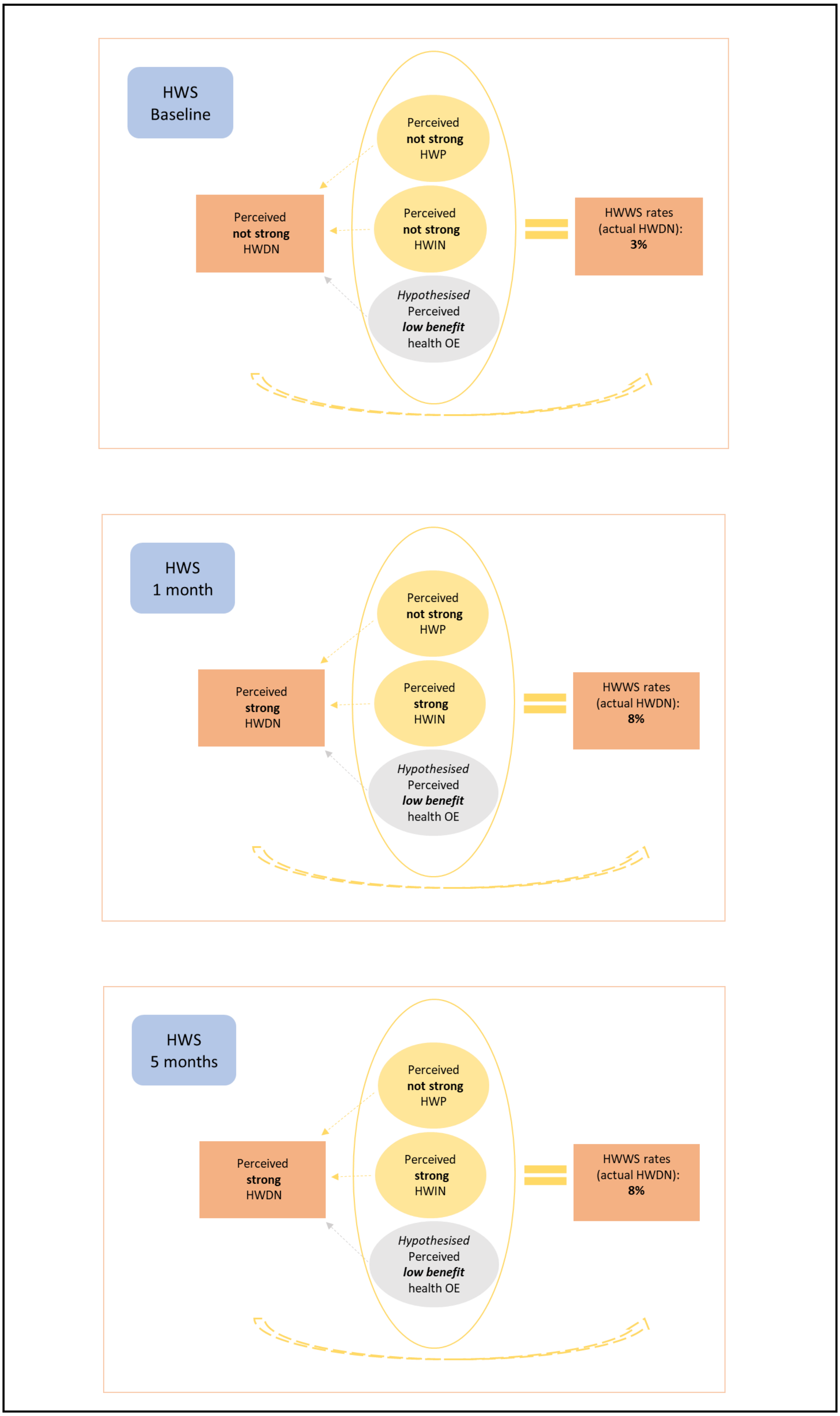


Figure 13.9. Level of the perceived norms-related constructs around HWWS after toilet use in the HWS-only group by trial phase

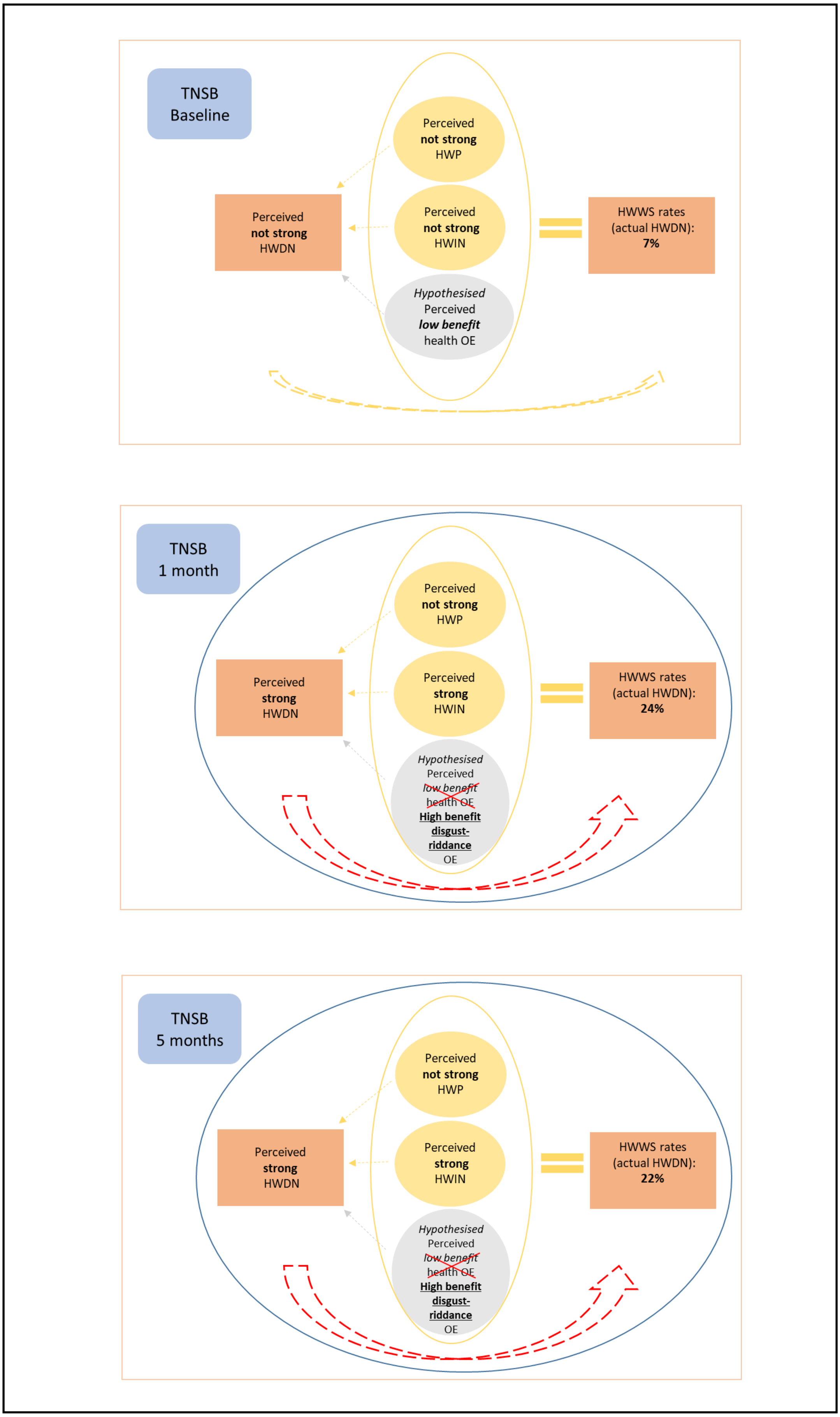


Figure 13.10. Level of the perceived norms-related constructs around HWWS after toilet use in the TNSB-based group by trial phase

Discussion

We designed three scales to measure the perceived descriptive and injunctive norms and behaviour publicness around HWWS after toilet use in the population of interest (Chapter 6). We observed some imbalances in the norms-related constructs strength between trial arms at baseline. These seemed to be less weak in the HWS-only and TNSB-based intervention arms, compared to the control arm. The differences observed were despite the randomised design and good baseline comparability in the primary handwashing outcomes, as well as compounds', households' and individuals' characteristics. The number of households sampled per trial arm was small (i.e. 2 to 3) which may explain the imbalances observed. Additionally, and due to the small number of interviews at baseline, the norms-related measures were at compound-level. The instruments are also subject to noise. For the reasons given above, it may not have been possible to efficiently control for the baseline imbalances observed.

As discussed in Chapter 6, the TNSB states that both the perceived HWDN and HWIN need to be strong, for the descriptive norm-behaviour relationship to be positively affected [119, 128]. The perceived HWOE, which we didn't measure, should also be seen as providing high benefits for it to have a positive influence on the descriptive norm-behaviour relationship [119, 128]. We expected the perceived descriptive norm and behaviour publicness around HWWS after using the toilet not to be strong, and the perceived injunctive norm to be strong in our study population, at baseline. While our results tend to confirm that the perceived HWDN and HWP were not strong at baseline, the perceived HWIN did not appear to be as strong as anticipated. This latter finding was unexpected, given our assumption that people's tendency to over-report HWWS practices is due to their knowledge that this is a socially desirable behaviour, indicating a strong injunctive norm around the practice.

Respondents tended to disagree more strongly with item i1 compared to the remaining two items of the HWIN scale. Item i1 explicitly used the word 'important' to state that HWWS was not important. By contrast, items i2 and i3 used local synonyms of the latter word. Item i2 used an expression which approximately translates to 'do not care', and item i3 used an expression which could translate to 'do not think about' to express the notion of the non-importance of HWWS after toilet use. These expressions were retained, based on the fact they were understood as intended during scales piloting. A possible explanation of the discrepancy may be that, with the threat of the Ebola outbreak and intensive prevention campaign, whilst HWWS may have remained not important, it was still a practice that people thought about and which potentially had meaning to them. Thus, we cannot exclude the possibility that the injunctive norm may have been strong at baseline. Our findings may reflect the difficulty in the measurement of the norms constructs, and possibly that the HWIN scale was not a very good measure of that norm. Nevertheless, the items were comparable to that of other studies measuring this construct (e.g. [191, 206]). Thus, At least three norms-related constructs' levels were suboptimal for HWWS practices after using the toilet.

Based on the HWDN, we might have expected to observe higher HWWS frequencies than we actually did. We had attempted to phrase the scale items in such a way that social desirability bias attached to HWWS would be minimised. This was achieved by formulating the scales' items so that the interviewee would be excluded from the statement, and by negatively framing all scales items (Chapter 6). However, we cannot exclude the possibility that interviewees rated the scale items in a fashion that provided information on their own (overestimated) HWWS practices after toilet use, as opposed to those of their fellow compound residents.

We did not find any evidence of an association between HWWS after toilet use and the norms-related constructs at the compound level. There was relatively limited variation in HWWS frequencies at baseline between compounds. This may explain the lack of association found between the norms-related constructs and HWWS practices. None of the confidence intervals around the computed estimates excluded an important association between the norms related construct and HWWS practices. In general, the small sample size in each compound and the relatively small number of compounds in each arm are limitations which likely reduced our ability to detect any such associations which might have existed. The imperfect measurement tool used also makes it harder to detect change. It is worth pointing out that we found an inverse relationship between the perceived HWIN and HWWS after using the toilet. As the HWIN increased by 1 unit, the HWWS decreased by 23% (though not statistically significant). This is contrary to social norms theories and our prediction. As mentioned previously, the imprecise measurement instrument may explain the findings.

We found evidence of an increase in the perceived HWDN and HWP at the one-month and five-month follow-up rounds compared to baseline. Although there was evidence of an increase in the HWIN at one month post initial intervention delivery compared to baseline, this was no longer the case at five months follow-up. However, the upper limit of the 95% CI does not exclude the possibility that there may have been a substantial difference. The exceptional context of the Ebola epidemic and resulting intensive handwashing campaigns taking place in Côte d'Ivoire between baseline and the one-month follow-up phase may explain the general increase in the perceived norms. HWWS may have become even more of a socially desirable behaviour, which would explain the increase in the perceived HWIN at the one-month follow-up round. Additionally, given the severity of the epidemic, and frequency of handwashing campaigns (e.g. daily messages on TV), people may have been under the impression that most people adhered to the handwashing recommendations, as a preventive measure (increased HWDN). This could also help to explain the discrepancy observed between the perceived HWDN and very low actual HWWS frequencies. As the campaign efforts declined in 2016, this may explain the observed decrease in the perceived HWIN at the five-month follow-up. As mentioned previously, the sample size was small and the imprecision around the measuring instrument may have impeded our ability to detect a possibly existing difference.

We found evidence that the differences observed in the perceived norms-related constructs were explained by the interventions received at one month post initial intervention delivery. In the HWS-only intervention group this was the case for all norms-related constructs but the HWIN. This may be explained by the fact that there was no handwashing intervention message in this trial arm. The handwashing station (HWS) may have made HWWS more visible (increase in HWP), and thus given the impression that more people HWWS after toilet use in the compound (increase in HWDN). This seems to have been the case despite the small intervention effect observed in this trial arm. However, in the absence of handwashing messages, residents may not have felt that other residents now viewed HWWS after toilet use as important. Thus, there may not have been a heightened social pressure from other residents to HWWS. At the five-month follow-up round, the odds of disagreeing with the notion that HWWS after toilet use was not important to compound residents was lower than that observed in the control group. This indicates that the HWIN had become stronger in the control group compared to the HWS-only group. However, we found weak evidence of a difference. The small sample size and noise around the measuring methods may justify the findings.

In the TNSB-based intervention arm, the odds of disagreeing with the notion that few residents HWWS after toilet use was substantial for both HWDN scale items (OR>10.00) at the one-month follow-up round, compared to the control arm. The difference observed was also considerably greater than that observed between the HWS-only intervention and the control arms. This is in line with the large intervention effect on HWWS after toilet use detected in this trial arm (Chapter 10). This seems to indicate that the HWDN played a key role in the important increase observed in HWWS practices after toilet use in the TNSB-based intervention group. However, the confidence intervals were quite large, which is an illustration of the small sample size and imprecision around the estimates.

Similar to the HWS-only intervention group, in the TNSB-based trial arm, the HWS may have contributed to making HWWS after toilet use stand out more, including the substantial increase in HWWS practices after using the toilet (increase in actual HWDN). This could explain the increase observed in the HWP. In contrast with the HWS-only intervention arm, respondents in the TNSB-based intervention group had significantly greater odds of disagreeing with the notion that residents in their compound did not see HWWS after toilet use as important, at the one-month follow-up. The intervention messages may have contributed to giving individual the impression that their fellow compound residents now expected them to HWWS after toilet use, so that no one would eat each other's faeces. Additionally, as the perceived and actual HWDN increased, and as HWWS became more visible, this may have heightened the social pressure to conform to the new norm, hence the increase in the HWIN.

We did not find evidence of a difference between trial arms in the way participants rated the scales items, at the five-month follow-up round. Nevertheless, in both intervention groups, the upper limit of the confidence intervals does not exclude the possibility that there was an important contrast with the control arm. The small sample size and measurement error will have limited our power to detect a difference, especially if it was small.

Nonetheless, there was a sustained large intervention effect on HWWS practices observed in the TNSB-based intervention group, despite the absence of difference in the perceived HWDN, HWP and HWIN with the control group. This may suggest that these constructs may not have played a key role in the HWWS increase observed. Another possible explanation for the differences observed in HWWS frequencies between study groups may therefore have been due to a change in the perceived HWOE from health to riddance of disgust, in the TNSB-based group. It is thus unfortunate that we were not able to measure this construct. To measure a shift in outcome expectation from health to riddance of disgust, a scale may not be the most appropriate method. Perhaps using some form of discrete choice experiment [222] would have been more appropriate.

At all trial phases and regarding the differences between trials arms, the change in the HWP and HWIN seem to have been less substantial than that observed for the HWDN. As the HWIN seems to have been the least weak at baseline, this may explain why the differenced observed were not as important as that observed for the HWDN. Regarding the HWP, the HWS intervention component, placed at toilet entrances, aimed to make HWWS practices after toilet use more visible, and persuade individuals that their behaviour was now open to fellow residents' scrutiny. However, it appears that the HWS and the TNSB intervention as a whole did not have an important effect on this norm-related construct. Our findings are in contrast to studies conducted in public restrooms that have shown that the presence of others has an effect on handwashing practices (e.g. [63, 64, 144]). This may be due to the fact that compounds are not small enclosed spaces like restrooms. In a public restroom for instance, given the small space, it is hard to ignore the presence of another person standing near the sink area, when coming out of a toilet cubicle. The pressure to wash hands with soap or the feeling that one would be judged if they were witnessed exiting the restroom without having washed their hands with soap may thus be greater in such small enclosed spaces.

In contrast, compounds with shared facilities are usually relatively large spaces. Additionally, compound residents (especially women) are commonly busy with their daily household chores (e.g. cooking, doing laundry, washing dishes). It is thus possible that, even with the HWS present, individuals still did not feel as though their HWWS practices after toilet use could be easily observed or that it would be scrutinised. Individuals might perceive other compound residents to be as busy taking care of chores, as they were. The results of our study suggest that it is quite difficult to achieve the necessary level of publicness required to impact behaviour in our study context.

While some of our findings are consistent with TNSB predictions, others are not. Three other studies (i.e. [144, 191, 206]) have explored the relationship between handwashing practices and some or all of the norms-related constructs around HWWS after toilet use which were of interest in our study. Their study findings also vary in their support of the TNSB predictions. Lapinski et al. (2014) assessed the social context of and motivation behind handwashing practices in childcare centres in a Midwestern city in the United States [191]. The authors found strong evidence that as the perceived HWIN became stronger, self-reported handwashing increased ($P<0.001$), and the descriptive norm-handwashing relationship increased in magnitude ($P<0.01$).

Similarly, Chung & Lapinski (2018) tested the prediction of the TNSB by applying the theory to handwashing in Korea [206]. The authors found strong evidence of a positive main effect of the perceived HWDN and HWIN on self-reported handwashing ($P<0.001$). On the other hand, Chung & Lapinski (2018) found weak evidence of an interaction between the perceived HWIN and HWDN ($P>0.05$) [206]. Lapinski et al. (2013) conducted a study in men's restrooms in a college campus in the Midwestern United States. The study aimed at assessing the effect of manipulating both the level of the descriptive norm around handwashing, and handwashing privacy on handwashing practices [144]. The authors found some evidence of a positive main effect of the perceived HWDN on handwashing practices ($P<0.05$). Nevertheless, the fact that self-reporting of handwashing was used in Lapinski et al. (2014) and Chung & Lapinski's (2018) studies, compared to observed HWWS frequencies in our study, may explain the differences found between studies. Norms may have a stronger influence on what people report compared to on actual behaviour.

Regarding the perceived HWOE, Lapinski et al. (2014) found some evidence of an interaction between the perceived HWDN and perceived HWOE ($p<0.05$) in relation to reported handwashing [191]. When childcare workers believed the handwashing frequency to be high among their co-workers (strong perceived HWDN), and when they believed that the perceived benefit of handwashing was high (strong perceived HWOE), they were more likely to self-report handwashing more frequently. Similarly, Chung & Lapinski (2018) found strong evidence of a positive main effect of the perceived HWOE on self-reported handwashing ($P<0.001$), and of an interaction between the perceived HWOE and the HWDN-handwashing relationship $P<0.01$). When the perceived HWOE became more positive, the descriptive norms-handwashing relationship increased in magnitude [223].

Lapinski et al. (2014) and Chung & Lapinski's (2018) findings are consistent with the putative role of HWOE in this trial. Although we did not directly measure the perceived HWOE, our results are consistent with the view that a shift of the perceived outcome expectation around HWWS after toilet use from health to riddance of disgust in the TNSB-based group resulted in the behaviour change observed. However, Chung & Lapinski (2018) also found that the descriptive norm-handwashing relationship was stronger when the perceived HWOE were at lower, as opposed to higher levels. This is inconsistent with the TNSB predictions, and contrary to what Lapinski et al. (2014) found.

Our findings regarding the influence of the perceived HWP on the perceived descriptive norm-handwashing relationship are consistent with Lapinski et al.'s (2013) study findings, but inconsistent with Chung et al.'s (2018) study findings. Lapinski et al. (2013) found no evidence of an association between behaviour publicness around handwashing and handwashing practices (P=0.43). By contrast, Chung et al. (2018) found strong evidence that as the perceived HWP increased, self-reported handwashing practices also increased (P<0.001). However, they also found that among participants with high perceived HWP, whilst an increase in perceived HWDN was associated with increased self-reported handwashing, the effect of the perceived descriptive norms on self-reported handwashing was greater for those who perceived HWP to be low. This is not in line with the TNSB predictions. Chung & Lapinski (2018) assumed that the odd pattern observed may be due to the socially desirable nature of handwashing. This echoes Lapinski et al.'s (2013) assumption that the perceived HWP moderating effect on the HWDN-behaviour relationship may only occur when the behaviour of interest is socially unacceptable. An alternative explanation would be that the HWP has more influence on what people do, compared to what they say.

Lapinski et al.'s (2013) and Chung et al.'s (2018) study results should be interpreted with caution, as handwashing was measured via self-report, which is prone to social desirability bias [32, 33], a reflection of norms. Similarly, in all three comparison studies, the scales' items were formulated in a fashion prone to social desirability bias. Additionally, all three studies were non-randomised studies. As stated by Lapinski et al. (2014), causation cannot therefore be established with confidence. The relatively small sample sizes in Lapinski et al.'s (2013 and 2014) studies (252 and 201 participants respectively), as in our study, also make the studies' estimates imprecise. This is compared to the relatively large sample size in Chung & Lapinski's (2018) study (i.e. 788 participants).

Besides these limitations, the differences in study findings observed between studies could also be explained by the fact that the four studies were conducted in three different populations and contexts. As pointed out by Chung & Lapinski (2018), this raises questions about the universality of what the TNSB predicts [206]. It appears that the way the moderators of the descriptive norm-behaviour relationship operate, but also the way norms in general operate, may vary with context.

One major limitation of our study lies in the fact we failed to design a scale to measure outcome expectation around HWWS after toilet use. As mentioned above, a discrete choice experiment may have been more appropriate to detect a shift in outcome expectation from health to riddance of disgust. Our supposition that the perceived HWOE was health at baseline, and that it shifted to riddance of disgust in the TNSB-based group, as result of the intervention, is just that, a supposition. Nevertheless, data from the 2012 pilot showed that the majority of respondents cited health as the reason why they washed their hands. Additionally, the exceptional Ebola context and subsequent intensive handwashing promotion activities within which the trial took place, and the use of a predominantly health-related language by participants during the intervention sessions in the TNSB-based group indicate that it is plausible that an initial perceived health HWOE shifted to a perceived riddance of disgust HWOE in the TNSB-based group.

Another limitation of our study relates to measurement error. This is compounded by the small sample sizes at compound level on which the norms-related constructs were measured. When looking at the association between the constructs and HWWS after toilet use, the analysis was performed at compound-level, using the norms-related constructs compounds' mean scores. Biased norms related constructs arising from small sample sizes and imperfect measurement instruments will tend to have diluted any association between norms and observed behaviour.

Conclusion

Our results are in line with the interventions effect reported in Chapter 10. The findings seem to suggest that the TNSB-based intervention had a substantial impact on the measured norms-related constructs around HWWS after toilet use. The HWDN seems to have played a key role in the observed intervention effect, whilst the HWP and HWIN appeared to not have been as significant. Our findings also indicate that increasing the perceived HWP may have been hard to achieve in the context of compound settings. We were unable to measure the perceived HWOE. Nevertheless, based on the findings, we could speculate that a change in the perceived HWOE, which was a key focus of the TNSB-based intervention, may have also been a key driver of behaviour change, along with the HWDN. This needs to be confirmed in a study which is able to detect shifts in perceived HWOE. The lack of evidence supporting the association between the norms-related constructs and HWWS after toilet use may have been due to the small sample size.

The inconsistencies found with the predictions of the TNSB, notably with the HWIN and HWP, suggest that the way norms-related constructs operate to influence behaviour may be context specific. This has implications when seeking to replicate the handwashing TNSB-based intervention in other settings, and also when using social norms theories in general to design interventions aimed at changing handwashing or other behaviours. Larger studies which can link individuals' norms-related responses to their own HWWS behaviours would enable a better assessment of the association between HWWS and norms-related constructs around the practice. Additionally, future studies aimed at replicating this study should aim to identify a way to measure shifts in perceived HWOE.

Chapter 14 – Discussion and Concluding Remarks

This thesis contributes to the evidence-base on handwashing behaviour change interventions in low and middle-income country (LMIC) settings, adding to the small pool of studies that have used predominantly non-health motives to increase HWWS practices at key occasions.

The TNSB-based intervention sought to trigger disgust as the key motive to achieve HWWS behaviour change after toilet use (primary outcome), and after cleaning a child's bottom (secondary outcome). Health was not part of the intervention messages. The TNSB was chosen as the intervention framework because HWWS practices are commonly enacted in public sphere (i.e. compounds' communal areas) in our study setting, making handwashing behaviour potentially susceptible to normative influences. The intervention was also designed using some social marketing elements, a further step away from traditional health-motives handwashing interventions. The intervention was centred on 10 short video-clips. These were designed to be entertaining as opposed to being didactic, as has been common in traditional campaigns. The intervention also entailed providing handwashing stations (HWS) with an initial soap supply.

This research also sheds light on the role of handwashing facilities in HWWS behaviour enactment, by assessing the effect of providing handwashing stations (HWS) alone, without handwashing promotion, on HWWS after using the toilet and after cleaning a child's bottom.

1. Summary of main findings

1.1. TNSB-based intervention effect on HWWS after toilet use

One month post initial intervention delivery, we found strong evidence of a substantial increase in HWWS after using the toilet in the TNSB-based intervention arm. This intervention effect was largely sustained five months post-intervention delivery. The results of the participant masking assessment, along with the sustained behaviour change observed five months post initial intervention delivery, support a causal effect of the intervention. The magnitude of the intervention effect in relative terms was relatively large compared to the effects observed in other studies which have assessed intervention effects on HWWS after toilet use [44, 66, 89, 102, 105]. The sustained large effect of our low-intensity TNSB-based intervention highlights the inadequacy of traditional health-motives handwashing campaigns implemented in Côte d'Ivoire on HWWS after using the toilet. This has implications for future campaigns aimed at increasing HWWS practices in Côte d'Ivoire. This is discussed in Section 3.3.2.

The Glo Germ© demonstration seems to have been the component of the intervention least appreciated by the participants, and may even have lessened the initial negative emotion triggered by the videos. Consequently, future studies aimed at replicating this intervention should consider dropping this component.

1.2. HWS-only intervention effect on HWWS after toilet use

We found some evidence that provision of an HWS alone led to a small increase in HWWS after using the toilet compared to the control arm. The small magnitude of effect observed for the HWS-only intervention, compared to that of the TNSB-based intervention, suggests that having access to handwashing facilities is not a decisive factor for HWWS practices in our study setting. Rather, it appears that internalisation of the importance of HWWS is key for the enactment of HWWS.

That said, in both intervention groups, the HWS was the preferred facility for handwashing when soap was used. This supports the argument that having both soap and water at the handwashing location facilitates the practice.

1.3. Interventions' effects on HWWS after cleaning a child's bottom

We did not find clear evidence of sustained intervention effects on HWWS after cleaning a child's bottom for either of the two interventions. Nonetheless, the upper limits of the confidence intervals around the intervention effect estimate in both intervention trial arms do not exclude the possibility that there was an effect of the intervention. The study was not powered to detect a change in HWWS after cleaning a child's bottom, due to the anticipated small number of observations. Therefore, these "negative" findings should be interpreted with caution.

1.4. HWS sustainability assessment

We found that the HWS lasted well, and over two thirds of HWS remained in good condition, with no taps broken, which is encouraging.

1.5. Norms results

Regarding norms-related constructs, some of our findings were consistent with the TNSB predictions, whilst others were not. Contrary to the TNSB, we did not find evidence of an association between HWWS after toilet use and the norms-related constructs. There was however low variability in HWWS prevalence at baseline. Generally, the small number of households sampled per compound and small number of compounds per arm are likely to have reduced our ability to detect any such association. The measuring scales were also imprecise. Nevertheless, none of the confidence intervals around the computed estimates exclude the possibility of an important association between the norms-related constructs and HWWS practices.

On the other hand, the level of the norms-related constructs at one-month follow-up, and HWWS prevalence in each trial arm were relatively in-line with the TNSB predictions. We found evidence that the interventions had an effect on the HWDN, HWP and HWIN, at the one-month follow-up. The norms-related constructs were stronger in the TNSB-based and HWS-only intervention groups, compared to the control group. The difference appeared greatest for the TNSB-based arm. Despite the lack of association observed, the changes in the norms-related constructs were in line with those observed in HWWS frequency in the TNSB-based and HWS-only intervention groups. The differences observed in the norms-related construct levels were not sustained five months post initial intervention delivery following an increase in the level of the norms-related constructs in the control group. This may reflect the small sample size, and imprecise measuring instruments.

Generally, the perceived HWDN seems to have been a key norm in the observed intervention effect compared to the HWP and HWIN. We were unable to measure the HWOE to assess a shift from health to disgust, which was the key motive in the TNSB-based intervention. Nonetheless, the substantial increase in HWWS prevalence in the TNSB-based group compared to the HWS-only group may be an indication that the HWOE was an important driver of the observed behaviour change.

2. Reflections on the process of conducting this research

I am very grateful for the chance that I had to conduct a PhD where I researched exactly what I wished to explore, despite the substantial challenges that I faced in seeing the project through to its conclusion. Additionally, I was very fortunate to be able to design a project tailored to my home country, Côte d'Ivoire. This gave me the opportunity to address a key public health concern in this setting in a meaningful manner, while also producing results of relevance to similar settings. The multiple skill set that I have gained in the process of conducting this research exceed my expectations when initially designing this project. The most unexpected skill is undoubtedly learning the process of designing movies, including script writing, storyboard conception, casting, production and post-production.

I was initially told that conducting research in a setting I was too familiar with could lead to researcher bias. Despite this, I believe conducting research in my home country was an asset rather than impeding the impartiality of the research. In-depth knowledge of the study population, culture and customs were important when designing suitable intervention content, and may have contributed to the development of an effective intervention, with positive results that are much needed. My knowledge of the study context also played a role in identifying a suitable masking theme that would be of interest to the study population, and increase the chances that they be effectively blinded to the study objectives. With the support of my supervisory team, I designed a study as rigorously as I could, in the context of handwashing studies, to minimise bias as far as possible. Thus, I had a minimal role in data collection, other than providing training and supervision. Similarly, I was neither involved directly in intervention implementation, nor in data entry. Whilst double blinding is not possible in handwashing studies, due to the nature of the intervention, I believe that the measures taken in this work masked the participants, as far as is possible, to the objectives of the study.

Although I am overall satisfied with the work accomplished and the way this study was designed and implemented, there remains some aspects that, with hindsight, I would do differently. I discuss these in Sections 2.1. to 2.5.

2.1. Study design

As mentioned previously, one of the study limitations lies in our inability to assess whether the TNSB-based intervention would have been as effective as the results observed, if HWS had not also been supplied. Inclusion of a fourth trial arm, receiving the TNSB-based intervention alone without the addition of handwashing facilities, was not possible due to funding limitations. A second alternative, a factorial design, would not have been powered to answer the question of the effectiveness of the TNSB intervention alone.

2.2. Selection criteria

The social norms-related secondary outcome was taken into consideration in the inclusion criteria. The initial decision to exclude compounds with more than two households with a screen was based on the social norms-related questionnaire, and the anticipated visibility issues it would cause during observations. I assumed that if activities that initially took place in communal space were now performed in private areas, residents would be likely to state that they did not know what others did. This would then lead to potentially a larger proportion of "Neither agree nor disagree" or "Does not know" answers than that observed. This decision led to the exclusion of 1,881 (95%) of 1,974 compounds screened at baseline, a very high proportion. During the follow-up observation sessions, even in the presence of more than two screens in some compounds, fieldworkers did not have an issue positioning themselves to have an unobstructed view of the toilet entrance, and effectively observe handwashing practices. In retrospect I would increase the number of households allowed to have screens, but still have a cut-off. This latter would have to be determined based on prior observations in a pilot study.

2.3. Questionnaire content

From the 2012 pilot study, and with my knowledge of the study setting, it should have been obvious that some of the questions to assess participants' socio-economic status were problematic. The questionnaire contained two questions which, upon reflection, had the potential to shed a poor light on compounds' landlords. If these had been excluded, I believe that the loss of at least one of the three TNSB-based intervention compounds which withdrew consent could have been avoided.

2.4. Measure of the perceived handwashing outcome expectation around HWWS

One of my regrets is not to have been able to measure changes in the HWOE. In retrospect, I should not have adopted a rigid approach to the methods that could have been used to measure this construct. When failing to design the HWOE scale, instead of removing it from the study altogether, I should have explored other approaches to measure this construct. A discrete choice experiment could have been one such alternative [222].

2.5. TNSB-based intervention component

Based on the findings from the intervention pilot, I should have removed Glo Germ© demonstration from the intervention. The pilot had shown that, although intended to, this component was not enhancing the feelings of disgust elicited by the videos, but perhaps actually reducing them. Although I do not believe that the intervention effect would have been substantially different, removing this intervention component would have considerably shortened the intervention implementation time. This would have been appreciated by both the participants and the intervention providers.

2.6. Process evaluation

The process evaluation of the TNSB-based intervention did not contain items which enable us to assess which intervention component participants believed contributed the most to their motivation to adopt the promoted behaviour. We thus cannot ascertain which component was key in increasing HWWS practices. Being able to pinpoint the key intervention components(s) would be important for maximising the cost-effectiveness of interventions in future studies aiming to replicate this study.

3. Recommendations for research, policy and practice

Based on the study findings, several recommendations arise for future research, policy and practice in this field.

3.1. Design and evaluation of HWWS interventions

3.1.2. Study design

Based on the quality of evidence of the pool of studies included in the systematic review, researchers should make substantial efforts to develop strategies to systematically attempt to blind study participants and fieldworkers. The results from the masking assessment suggest that the approaches used in this study could be tested more widely. Similarly, researchers should, whenever possible, avoid using self-report or HWWS proxy-measures to measure HWWS practices. They should rather favour direct observations. The results of the systematic review show lower estimates of interventions' effects (with one exception) when studies which used self-report to measure HWWS practices are included. Using direct observations, rather than self-report, should be considered the "gold standard" for collecting accurate measures of HWWS practices.

3.1.1. Reporting of results

Based on the experience of conducting a systematic review, better reporting of handwashing with soap studies are needed, notably in terms of describing HWWS interventions and the behaviour motives which informed the intervention. Regarding intervention results, more consistent reporting is needed. We would recommend risk ratios, with confidence intervals and exact p-values. This would facilitate the conduct of systematic reviews and meta-analyses, and make it easier to compare results between studies. Similarly, when handwashing events are measured by occasion, researchers should report estimates for each occasion, rather than presenting a single estimate combining all handwashing occasions. This would also facilitate cross study comparisons.

3.1.3. Future research

- Future research pertaining to the trial's results

The results of the post-hoc analyses by age sub-groups highlight the worth of adapting and evaluating the TNSB-based handwashing intervention at school-level. One of the major challenges to consider in such settings would be the maintenance of handwashing facilities, including ones such as the HWS used in our study. This would require compliance from school staff. For instance, Freeman (2012) [77] conducted a behaviour-change handwashing trial in primary schools in Nyanza Province, Kenya. School teachers were in charge of replenishing the HWS in water and soap supplies [77]. The authors found that less than 40% of students reported that soap was always available at the HWS. Thus, whilst the TNSB-based intervention could be potentially effective among school-aged children, poor management of handwashing supplies at the handwashing facility could reduce the magnitude of the intervention effect. Minimising performance and detection bias would also be key challenges to address in the evaluation of the intervention in school settings.

For intervention scaling-up purposes, we recommend that solely the media components of the intervention (i.e. videos and posters) be evaluated, to assess the potential effect that mass media campaigns could have. Scaling-up using a high intensity mass media campaign may also require the design of additional intervention clips and posters, to minimise the risk of intervention fatigue.

- Future research pertaining to the broader handwashing behaviour change field

Based on the systematic review findings, the most effective way of improving HWWS practices after faecal contact remains unclear. Larger studies are needed to ascertain the effect of disgust-motivated behaviour change interventions on HWWS after toilet use and after cleaning a child's bottom. More generally, more studies are needed to assess the effectiveness of non-health-motivated handwashing interventions on HWWS practices after key occasions.

Similarly, the question of a possible interaction (or lack thereof) between handwashing facility provision and handwashing promotion remains unresolved. Larger studies are needed to address this question.

3.2. Measuring social norms-related constructs around handwashing with soap

3.2.1. Norms-related scale design

Researchers who aim to design scales to measure social norms-related constructs around HWWS should adhere to good practice in scale development (e.g. [192, 197]). In that regard, the scales should be piloted extensively, as part of the scales' design process. This would increase the chances of designing good measuring instruments and thus collecting accurate data. The scales should be culturally-appropriate, with locally-adapted response-categories. Researchers should refrain from assuming that norms-related scales which were 'successful' in one type of setting would also be adequate for the setting where the research is to take place. Failure to do so could considerably lower the quality of the data collected.

Researchers and practitioners should aim to design scales with no more than 4-items, due to the tendency of norms scales' items relating to HWWS to be highly redundant. This would be to avoid burdensome scales, which might negatively affect the quality of responses. When designing the scale items, researchers and practitioners should use strategies to minimise the risk of social desirability-bias attached to HWWS. Phrasing the items negatively and excluding respondents themselves from the items, as used in this research, are examples of potential strategies.

We would recommend that future attempts at measuring the HWOE be made by designing a scale which solely aims to measure changes in one key identified outcome expectation for the targeted handwashing occasion. As mentioned previously, perhaps an alternative approach to measuring the shift from one inefficient HWOE to another suitable one would be to conduct discreet choice experiment [222]. The design of the measuring instrument would still need to be subjected to the same rigour as in scale design, and pre-tested [222].

Larger studies which can link individuals' norms-related responses to their own HWWS behaviours would enable a better assessment of the association between HWWS and norms-related constructs around the practice. However, given the relatively large number of individuals in compounds in settings like Abidjan, there are practical challenges in uniquely identifying individuals in the observational data, so that HWWS practices are accurately linked to the right participant.

3.3. Policies

3.3.1. Study findings in the context of the 2014-2016 West African Ebola epidemics

This study took place during the exceptional context of the West-African Ebola epidemic, with a real threat of an outbreak in Côte d'Ivoire. This resulted in an intensive national prevention campaign in the country, which included promotion of handwashing. The prevention campaign was at its lowest in 2014 (trial's baseline period), with a peak in intensity in 2015, and a decline from the first quarter of 2016. The campaign had almost ceased by the last quarter of 2016 and beyond (the trial's one-month and five-month follow-up-periods). The campaign mainly used mass media (e.g. TV and radio spots, posters in public places), but also included some community-level components (e.g. demonstrations of HWWS techniques, health-education on Ebola and the link between Ebola prevention and HWWS, as well as handwashing facility supply (e.g. in restaurants and schools)). The TV spots were broadcast at peak TV viewing times (e.g. before, during and after the news; before, during, and after popular soap operas) to increase the chances that the campaign messages achieved high coverage. The campaign promoted frequent HWWS or with other known cleansing antibacterial agents (e.g. antibacterial gel) as well as HWWS at specific occasions (e.g. after an outing, before eating, after working, after coming in contact with a sick person), including after using the toilet. More and more handwashing facilities stocked with water and soap, and more and more antibacterial gel could be observed in the city, such as in popular restaurants, at banks, and other services (Pictures 14.1 to 14.4). Thus, the Ebola context in which this trial was conducted provides an opportunity to compare a high intensity national health-motivated handwashing campaign, with our comparatively low intensity intervention using disgust as a motive.

Picture 14.1: Handwashing station in front of the entrance to Abidjan's airport

Pictures 14.2 and 14.3: Handwashing stations distribution in a school broadcasted on the news on the main national TV channel, RTI 1

Picture 14.4: Anti-bacterial gel at the entrance of a media store

Koumassi was one of the economically disadvantaged neighbourhoods where the community-level Ebola prevention campaign took place in 2015. We were aware of at least two handwashing promotion community events which took place in this commune, although outside of the study area. Whilst the Ebola prevention campaign had already started at the trial baseline, although at its lowest intensity, the observed baseline HWWS estimates after using the toilet do not seem to indicate that the campaign messages, and serious outbreak threat, had a significant impact on the study population's HWWS practices after this occasion. The proportion was comparable to the findings of previous studies. However, as we did not measure HWWS at other occasions, we do not know whether the campaign rather had an effect on HWWS at other key times.

3.3.2. Country-level recommendations

Côte d'Ivoire remains among the countries with the highest child mortality rates in the world [8], with diarrhoeal diseases being the third leading infectious cause of death in children under five years old [9]. Goal 3 of the SDGs aims to *"Ensure healthy lives and promote well-being for all at all ages".* This includes ending the preventable deaths of children under 5 years old, by reducing under-five mortality rate (U5MR) below 25 per 1,000 live births [3]. Given little observed change in the prevalence of HWWS after key occasions over the past 10 years in Côte d'Ivoire, and in the light of our study findings, there is a need for new and more innovative national handwashing campaigns. A national campaign effective in increasing HWWS frequencies could contribute to reducing the country's high child mortality rate. Given our findings, the MoH's plan to mass-supply HWS to schools would be unlikely to be adequate, unless coupled with a suitable handwashing promotion campaign. We deliberately designed the TNSB-based intervention with a predominantly media format for ease of replicability and scaling up of the intervention in Côte d'Ivoire, but also other similar settings. However, the intervention could not be scaled up in exactly the same format as it was in our trial.

- Possible approach and challenges to scaling-up the TNSB-based intervention

As mentioned previously, we recommend that only the video and poster elements of the interventions be considered for scaling up. In Côte d'Ivoire, 43% of the population owned a TV and 55% owned a radio in 2012 [218]. These figures are likely to have substantially increased seven years later. One issue arises with the videos in their current form. Their length (about three minutes long) is substantially longer than commercial adverts (no more than one minute long). The air time cost would thus be considerable and potentially unaffordable, unless public service messages broadcast on national TV do not need to pay for air time. New clips with a maximum duration of one minute would thus have to be redesigned. Alternatively, the possibility of shortening the current clips could also be explored. Regarding the posters, it would not be possible to place them on toilet doors. Posters could instead be placed on billboards, as is common practice for national campaigns.

In the past 10 years or so, there has been a boom in the mobile phone industry in Côte d'Ivoire, and, with it, mobile internet subscribers [224]. Therefore, another promising avenue to explore regarding broadcasting of the intervention videoclips and messages would be via mobile phones and the internet, in addition to the TV and the radio. In 2012, 80% of the population owned a mobile phone (93% and 71% in urban and rural areas respectively) [225]. Given the high mobile phone ownership in rural areas, broadcasting intervention messages via text-messages would be one way to improve coverage of populations in remote areas, for instance.

Regarding the internet, whilst the penetration rate in Côte d'Ivoire was lower than 1% [226], in 2012, this had substantially increased to 44% in 2019 [227]. Additionally, out of approximately 25 million inhabitants, it is estimated that 40% of the population are mobile internet users [227]. In 2014, 98% of internet subscribers used mobile technology which were mainly smartphones [226]. The boom in smartphone ownership in the country can be explained by the substantial reduction in mobile devices' prices [226]. For instance, JUMIA is the largest online shopping platform of the country, and known for its discounted prices, notably in mobile devices [226]. The considerable increase in smartphone ownership significantly contributed to the increase in the number of users accessing the internet via their mobile phones [226]. Thus, if using social media such as Facebook to broadcast the TNSB intervention videoclips, for example, one advantage would be that the length of the clips would not need to be restricted to a maximum of one minute. A second advantage would be the possibility of targeting populations with specific characteristics.

The issue of cost-effectively delivering the intervention at large scale also raises another question regarding the scalability of the interventions, due to the handwashing station facility component and initial soap provision. As mentioned previously, we do not know whether the TNSB-based intervention would have been effective without the provision of HWS. However, the scaling up of the provision of handwashing facilities in their current design would not be feasible at the national level, in part due to cost considerations. Perhaps short video and radio spots, and posters aimed at demonstrating simple ways to build and maintain HWS could supplement the campaign. As having a tap attached to the bucket is a convenient feature of the HWS, it would be useful to assess whether these are readily available to purchase. Buying the parts separately to assemble could act as a deterrent. Nonetheless, if a demand for HWS arises, it is likely that suppliers would emerge in response. Having supplementary clips would raise the same issue of substantial additional air time cost as discussed above.

One element which would be missing from an adaptation of the intervention at the national level would be the discussion session. Being able to discuss the intervention content and messages with other compound members may have been key for recipients to adopt the promoted behaviour. This is in contrast with passively receiving a message. Another possible benefit of using smart phones to disseminate the campaign messages is that they enable the sharing of video clips, among other intervention components. Whilst this would not be the same as being able to actively discuss the intervention messages, this would still make people more than just passive recipients.

Last but not least, as discussed, the Glo germ© demonstration could be removed from the intervention, as we do not believe that this component played an important role in the intervention effect, but rather lessened the videos' disgust effect.

- Scale-up intervention recipients

The seemingly substantially greater effect of the TNSB-based intervention on children compared to adults should inform decisions on key populations to target in a national handwashing campaign in order to have the most cost-effective public health impact. As discussed above, ensuring that the school environment is conducive to HWWS habit formation would be key, although challenging. In case of a campaign with school-aged children as the key recipients, designing and implementing a campaign targeting both school and household-settings would be beneficial. Not only school-aged children would be targeted, but also adults, including children's parents. This could contribute to intervention recipients bolstering each other's adoption of the intervention messages. School-aged children would also be exposed to the intervention messages at home, which should increase the chances that these are effective. Additionally, targeting both settings would increase the chances that an environment conducive to school-aged children HWWS habit formation be present not only in schools, but also in their households. In case of the effectiveness of such dual intervention, this would also warrant the introduction of educational materials derived from the TNSB-based handwashing intervention, as part of schools' hygiene education curriculum.

3.3.3. Regional-level recommendations

Comparably low rates of HWWS have been observed in other West African countries, notably in Ghana, Senegal and Burkina-Faso [31]. We believe that the TNSB-based handwashing intervention could be implemented with minor modifications in other West African countries with similar contexts. In Ghana and Burkina-Faso, for instance, the attitudes, customs, interactions and settings depicted in the intervention clips are comparable. However, the nature of the digital disgust effect may need to be modified according to study population preferences. This would increase the chances that the key disgust component of the intervention be effective at creating a new incentive for HWWS after using the toilet.

Nevertheless, such media campaign would only be suitable in other settings if TV and radio coverage are significant. In Burkina-Faso, for instance, this might prove challenging as 16% and 86% of populations owned a TV and radio respectively in 2010 [228]. This is compared to 62% and 69% respectively in Ghana in 2014[229]. Although it is likely that the figures have improved over the past five to nine years, coverage may still be an issue in Burkina-Faso. In general, and whether in Côte d'Ivoire or other comparable settings, whilst TV may be an effective way of reaching urban populations, it is likely to have limitations in rural areas. In terms of mobile ownership, the estimates in Ghana in 2014 were comparable to that of Côte d'Ivoire (over 80%, with over 90% in urban areas and over 70% in rural areas) [229]. Additionally, in 2017, 35% of the adult population in Ghana reported owning a smartphone [230]. By contrast, the estimates were considerably lower in Burkina-Faso in 2012, with about 60% of mobile ownership (over 80% in urban areas and over 50% in rural areas) [228], and 14% of adults reporting owning a smartphone in 2015 [231]. Regarding the internet, in Ghana, the penetration rate is estimated at 35%, with 31% of mobile internet users [232]. This is compared to 19% in Burkina-Faso, with a comparable percentage of mobile internet users [233].

Conclusion

In conclusion, the TNSB-based intervention appears to hold promise regarding sustainably improving HWWS practices after using the toilet, and potentially after cleaning a child's bottom in Côte d'Ivoire. However, areas of uncertainty remain which need to be addressed in future studies. These include assessing the effect of the intervention without the addition of handwashing facilities; the adaptation and evaluation of the TNSB-based intervention in school settings; as well as the challenges pertaining to intervention scaling-up (e.g. intervention reach and coverage, solutions to the absence of the discussion component). These are all challenges that we are looking forward to tackling in future research.

References

1. UNICEF, *Levels and Trends in Child Mortality: Report* 2019, UNICEF.
2. UNITED NATIONS, *Sustainable development goals. The sustainable development agenda.* 2018.
3. UNited NATIONS, *Sustainable development goals. Goal 3: Ensure healthy lives and promote well-being for all at all ages.* 2018.
4. Liu, L., et al., *Global, regional, and national causes of under-5 mortality in 2000-15: an updated systematic analysis with implications for the Sustainable Development Goals.* Lancet, 2016. **388**: p. 3027-3035.
5. Collaborators, G.B.D.D.D., *Estimates of global, regional, and national morbidity, mortality, and aetiologies of diarrhoeal diseases: a systematic analysis for the Global Burden of Disease Study 2015.* Lancet Infect Dis, 2017. **17**: p. 909-948.
6. UNICEF. *Levels and trends in child mortality: Report* 2017. Unicef.
7. Collaborators, G.B.D.D.D., *Estimates of the global, regional, and national morbidity, mortality, and aetiologies of diarrhoea in 195 countries: a systematic analysis for the Global Burden of Disease Study 2016.* Lancet Infect Dis, 2018. **18**: p. 1211-1228.
8. UNICEF, *Côte d'Ivoire: Key demographic indicators*. 2017.
9. UNICEF, *Situation of children in Cote d'Ivoire: The big picture.* 2016.
10. World Health Organization, *Diarrhoeal disease. Key facts.* 2017.
11. Saeed, A., H. Abd, and G. Sandstrom, *Microbial aetiology of acute diarrhoea in children under five years of age in Khartoum, Sudan.* J Med Microbiol, 2015. **64**: p. 432-437.
12. Ejemot-Nwadiaro, R.I., et al., *Hand washing promotion for preventing diarrhoea.* Cochrane Database Syst Rev, 2015: p. Cd004265.
13. Eisenberg, J.N., et al., *Toward a systems approach to enteric pathogen transmission: from individual independence to community interdependence.* Annu Rev Public Health, 2012. **33**: p. 239-257.
14. Pickering, A.J., et al., *Bacterial hand contamination among Tanzanian mothers varies temporally and following household activities.* Tropical medicine & international health, 2011. **16**: p. 233-239.
15. Scott, B., *WELL factsheet. Health impacts of improved household sanitation.* 2006.
16. Alirol, E., et al., *Urbanisation and infectious diseases in a globalised world.* Lancet Infect Dis, 2011. **11**: p. 131-141.
17. Greenland, K., et al., *Can we afford to overlook hand hygiene again?* Trop Med Int Health, 2013. **18**: p. 246-249.
18. Curtis, V., S. Cairncross, and R. Yonli, *Review: Domestic hygiene and diarrhoea - pinpointing the problem.* Journal of Tropical Medicine & International Health,

2000. **5**: p. 22-32.
19. Ejemot, R.I., et al., *Hand washing for preventing diarrhoea.* Int J Epidemiol, 2008. **37**: p. 470-472.
20. Wolf, J., et al., *Impact of drinking water, sanitation and handwashing with soap on childhood diarrhoeal disease: updated meta-analysis and meta-regression.* Tropical Medicine & International Health, 2018. **23**(5): p. 508-525.
21. Wolf, J., et al., *Handwashing with soap after potential faecal contact: Global, regional and country estimates for handwashing with soap after potential faecal contact.* Int J Epidemiol, 2018.
22. Mbakaya, B.C., P.H. Lee, and R.L.T. Lee, *Hand Hygiene Intervention Strategies to Reduce Diarrhoea and Respiratory Infections among Schoolchildren in Developing Countries: A Systematic Review.* Int J Environ Res Public Health, 2017. **14**.
23. Cairncross, S., et al., *Water, sanitation and hygiene for the prevention of diarrhoea.* Int J Epidemiol, 2010. **39 Suppl 1**: p. i193-205.
24. Aiello, A.E., et al., *Effect of hand hygiene on infectious disease risk in the community setting: a meta-analysis.* Am J Public Health, 2008. **98**: p. 1372-1381.
25. de Witt Huberts, J., et al., *Exploring the potential of antimicrobial hand hygiene products in reducing the infectious burden in low-income countries: An integrative review.* Am J Infect Control, 2016. **44**: p. 764-771.
26. Centers for Disease Control and Prevention, *Show me the science - How to wash your hands*. 2018.
27. Luby, S., et al., *The effect of antibacterial soap on impetigo incidence, Karachi, Pakistan.* Am J Trop Med Hyg, 2002. **67**(4): p. 430-5.
28. Luby, S.P., et al., *Effect of intensive handwashing promotion on childhood diarrhea in high-risk communities in Pakistan: a randomized controlled trial.* Global Health, 2004. **291**: p. 2547-2554.
29. Luby, S.P., et al., *Effect of handwashing in child health: a randomized controlled trial.* Lancet, 2005. **366**: p. 225-233.
30. Larson, E.L., et al., *Effect of antibacterial home cleaning and handwashing products on infectious disease symptoms: a randomized, double-blind trial.* Ann Intern Med, 2004. **140**: p. 321-329.
31. Freeman, M.C., et al., *Hygiene and health: systematic review of handwashing practices worldwide and update of health effects.* Trop Med Int Health, 2014. **19**: p. 906-916.
32. Biran, A., et al., *Comparing the performance of indicators of hand-washing practices in rural Indian households.* Journal of Tropical Medicine and International Health, 2008. **13**: p. 278-285.
33. Curtis, V., et al., *Structured Observations of Hygiene Behaviors in Burkina-Faso - Validity, Variability, and Utility.* Bull World Health Organ, 1993. **71**: p. 23-32.
34. Danquah, L.O. *Measuring hand washing behaviour: methodological and validity issues*. in *South Asia Hygiene Practitioners' Workshop*. 2010. Dhaka, Bangladesh.
35. Manun'Ebo, M., et al., *Measuring hygiene practices: a comparison of questionnaires with direct observations in rural Zaire.* Trop Med Int Health, 1997. **2**: p. 1015-1021.

36. Cousens, S., et al., *Reactivity and repeatability of hygiene behaviour: Structured observations from Burkina Faso.* Social Science & Medicine, 1996. **43**: p. 1299-1308.
37. Ram, P.K., et al., *Is structured observation a valid technique to measure handwashing behavior? Use of acceleration sensors embedded in soap to assess reactivity to structured observation.* Am J Trop Med Hyg, 2010. **83**: p. 1070-1076.
38. Harvey, S.A., et al., *They'll change what they're doing if they know that you're watching: Measuring reactivity in health behavior because of an Observer's Presence-- A Case from the Peruvian Amazon.* Field Methods, 2009. **21**: p. 3-25.
39. Bentley, M.E., et al. *The use of structured observations in the study of health behaviour*. in *Occasional Paper 27.* 1994. The Hague: IRC International Water and Sanitation Centre.
40. UNICEF, *Côte d'Ivoire. Global handwashing day.* 2018.
41. Kumar, S., et al., *Handwashing in 51 Countries: Analysis of Proxy Measures of Handwashing Behavior in Multiple Indicator Cluster Surveys and Demographic and Health Surveys, 2010-2013.* American Journal of Tropical Medicine and Hygiene, 2017. **97**: p. 447-459.
42. Luby, S.P., et al., *Difficulties in maintaining improved handwashing behavior, Karachi, Pakistan.* Am J Trop Med Hyg, 2009. **81**: p. 140-145.
43. Luby, S.P. and A.K. Halder, *Associations among handwashing indicators, wealth, and symptoms of childhood respiratory illness in urban Bangladesh.* Tropical medicine & international health, 2008. **13**: p. 835-844.
44. Bowen, A., et al., *Sustained improvements in handwashing indicators more than 5years after a cluster-randomised, community-based trial of handwashing promotion in Karachi, Pakistan.* Journal of Tropical Medicine & International Health, 2013. **18**: p. 259-267.
45. Ram, P.K., et al., *Impact of an Intensive Perinatal Handwashing Promotion Intervention on Maternal Handwashing Behavior in the Neonatal Period: Findings from a Randomized Controlled Trial in Rural Bangladesh.* Biomed Res Int, 2017. **2017**: p. 6081470.
46. Biswas, S., *Uptake of hand washing with soap or soapy water from a large-scale cluster randomized community trial in urban Bangladesh.* ICDDRB Health & Science Bulletin, 2012. **10**: p. 9-15.
47. World Bank, Bank Netherlands Water Partnership, and Water and Sanitation Program, *The Handwashing handbook: a guide for developing a hygiene promotion program to increase handwashing with soap*. 2005.
48. Curtis, V.A., L.O. Danquah, and R.V. Aunger, *Planned, motivated and habitual hygiene behaviour: an eleven country review.* Health Educ Res, 2009. **24**: p. 655-673.
49. Hoque, B.A., *Handwashing practices and challenges in Bangladesh.* Int J Environ Health Res, 2003. **13 Suppl 1**: p. S81-7.
50. Curtis, V.A., N. Garbrah-Aidoo, and B. Scott, *Ethics in public health research: masters of marketing: bringing private sector skills to public health partnerships.* Am J Public Health, 2007. **97**: p. 634-641.
51. Scott, B., et al., *Health in our hands, but not in our heads: understanding hygiene*

motivation in Ghana. Health Policy Plan, 2007. **22**: p. 225-233.

52. Hernandez, O., et al. *Behavioral determinants of handwashing with soap among mothers and caretakers: Emergent learning from Senegal and Peru*. 2012. The World Bank.
53. Biran, A., et al., *The effect of a soap promotion and hygiene education campaign on handwashing behaviour in rural India: a cluster randomised trial.* Trop Med Int Health, 2009. **14**: p. 1303-1314.
54. World Bank, *Formative and baseline survey on handwasing with soap: Final report.* 2007.
55. Pinfold, J.V., *Analysis of different communication channels for promoting hygiene behaviour.* Health Educ Res, 1999. **14**: p. 629-639.
56. Curtis, V., M. de Barra, and R. Aunger, *Disgust as an adaptive system for disease avoidance behaviour.* Philos Trans R Soc Lond B Biol Sci, 2011. **366**: p. 389-401.
57. Scott, B.E., et al., *Marketing hygiene behaviours: the impact of different communication channels on reported handwashing behaviour of women in Ghana.* Health Educ Res, 2008. **23**: p. 392-401.
58. Curtis, V. and A. Biran, *Dirt, disgust, and disease - is hygiene in our genes?* Perspectives in Biology and Medicine, 2001. **44**: p. 17-31.
59. Porzig-Drummond, R., et al., *Can the emotion of disgust be harnessed to promote hand hygiene? Experimental and field-based tests.* Social Science & Medicine, 2009. **68**: p. 1006-1012.
60. World Bank and Program Water and Sanitation, *Insights from designing a handwashing station for rural Vietnamese households*. 2010, World Bank.
61. Apisarnthanarak, A., H.M. Babcock, and V.J. Fraser, *Compliance with universal precautions among medical students in a tertiary care center in Thailand.* Infect Control Hosp Epidemiol, 2006. **27**: p. 1409-1410.
62. Lankford, M.G., et al., *Influence of role models and hospital design on hand hygiene of healthcare workers.* Emerg Infect Dis, 2003. **9**: p. 217-223.
63. Munger, K. and S.J. Harris, *Effects of an Observer on Handwashing in a Public Restroom.* Perceptual and Motor Skills, 1989. **69**: p. 733-734.
64. Judah, G., et al., *Experimental Pretesting of Hand-Washing Interventions in a Natural Setting.* Am J Public Health, 2009. **99**: p. S405-S411.
65. Biran, A., et al., *Effect of a behaviour-change intervention on handwashing with soap in India (SuperAmma): a cluster-randomised trial.* Lancet Glob Health, 2014. **2**: p. e145-54.
66. Guiteras, R.P., et al., *Disgust, Shame, and Soapy Water: Tests of Novel Interventions to Promote Safe Water and Hygiene.* Journal of the Association of Environmental and Resource Economists, 2016.
67. Luby, S.P., et al., *Household Characteristics Associated with Handwashing with Soap in Rural Bangladesh.* American Journal of Tropical Medicine and Hygiene, 2009. **81**: p. 882-887.
68. Biran, A., *Enabling technologies for handwashing with soap: A case study on the tippy-tap in Uganda*

in *Water and sanitation program working paper*. 2011, World Bank: Washington DC.

69. Verplanken, B. and S. Orbell, *Reflections on past behavior: A self-report index of habit strength.* Journal of Applied Social Psychology, 2003. **33**: p. 1313-1330.
70. Wood, W. and D.T. Neal, *A new look at habits and the habit-goal interface.* Psychological Review, 2007. **114**: p. 843-863.
71. Wood, W. and D.T. Neal, *The habitual consumer.* Journal of Consumer Psychology, 2009. **19**: p. 579-592.
72. Neal, D.T., et al., *How do habits guide behavior? Perceived and actual triggers of habits in daily life.* Journal of Experimental Social Psychology, 2012. **48**: p. 492-498.
73. Freeman, M.C., et al., *The impact of a school-based water supply and treatment, hygiene, and sanitation programme on pupil diarrhoea: A cluster-randomized trial.* Epidemiol Infect, 2014. **142**: p. 340-351.
74. Liberati, A., et al., *The PRISMA statement for reporting systematic reviews and meta-analyses of studies that evaluate healthcare interventions: explanation and elaboration.* Bmj-British Medical Journal, 2009. **339**.
75. World Bank, *World Bank Country and Lending Groups*. 2019.
76. Saboori, S., et al., *Impact of regular soap provision to primary schools on hand washing and E. coli hand contamination among pupils in Nyanza Province, Kenya: a cluster-randomized trial.* Am J Trop Med Hyg, 2013. **89**: p. 698-708.
77. Freeman, M.C., et al., *Assessing the impact of a school-based water treatment, hygiene and sanitation programme on pupil absence in Nyanza Province, Kenya: A cluster-randomized trial.* Tropical Medicine and International Health, 2012. **17**: p. 380-391.
78. Chase, C. and Q.-T. Do. *Handwashing behavior change at scale : evidence from a randomized evaluation in Vietnam*. 2012.
79. Hospital Care for Children, *Summary of Randomised Controlled Trials in Developing Countries.* Evidence, 2016.
80. Cochrane, *Assessing risk of bias in cluster-randomized trials*, in *Cochrane handbook for systematic reviews of interventions*, J.P. Higgins and S. Green, Editors. 2011.
81. Ivers, N.M., et al., *Allocation techniques for balance at baseline in cluster randomized trials: a methodological review.* Trials, 2012. **13**.
82. BMJ. *What is GRADE?* 2019 [cited 2018.
83. Ryan, R. and S. Hill, *How to GRADE the quality of the evidence. Cochrane Consumers and Communication Group.* 2016. **2018**.
84. GRADEpro, *GRADE your evidence and improve your guideline development in health care.* 2015.
85. Atkins, D., et al., *Grading quality of evidence and strength of recommendations.* Bmj-British Medical Journal, 2004. **328**(7454): p. 1490-+.
86. Deeks, J. and D.G. Altman, *Effect measures for meta–analysis of trials with binary outcomes*, in *Systematic reviews in health care: Meta-analysis in context*, M. Egger, G.D. Smith, and D.G. Altman, Editors. 2001, BMJ Publishing Group.
87. Engels, E.A., et al., *Heterogeneity and statistical significance in meta-analysis: an empirical study of 125 meta-analyses.* Stat Med, 2000. **19**: p. 1707-1728.
88. Gautam, O.P., et al., *Trial of a Novel Intervention to Improve Multiple Food*

Hygiene Behaviors in Nepal. Am J Trop Med Hyg, 2017. **96**: p. 1415-1426.
89. Luby, S.P., et al., *A community-randomised controlled trial promoting waterless hand sanitizer and handwashing with soap, Dhaka, Bangladesh.* Tropical Medicine & International Health, 2010. **15**: p. 1508-1516.
90. Friedrich, M.N.D., A. Kappler, and H.-J. Mosler, *Enhancing handwashing frequency and technique of primary caregivers in Harare, Zimbabwe: A cluster-randomized controlled trial using behavioral and microbial outcomes.* Soc Sci Med, 2018. **196**: p. 66-76.
91. Briceno, B., et al., *Are there synergies from combining hygiene and sanitation promotion campaigns: evidence from a large-scale cluster-randomized trial in rural Tanzania.* PLoS One, 2017. **12**: p. e0186228.
92. Hayes, R.J. and L.H. Moulton, *Cluster randomised trials.* 2009.
93. Kelly, M. and M. Barker, *Why is changing health-related behaviour so difficult?* Public health, 2016. **136**: p. 109-116.
94. Higgins, J.P. and S.G. Thompson, *Quantifying heterogeneity in a meta-analysis.* Stat Med, 2002. **21**: p. 1539-1558.
95. Higgins, J.P., et al., *Measuring inconsistency in meta-analyses.* BMJ, 2003. **327**: p. 557-560.
96. Huedo-Medina, T.B., et al., *Assessing heterogeneity in meta-analysis: Q statistic or I-2 index?* Psychological Methods, 2006. **11**: p. 193-206.
97. Kamm, K.B., et al., *Impact of a brief in-home neonatal health promotion on self-reported birth and neonatal care practices among primiparous women in the third trimester in rural Bangladesh.* American Journal of Tropical Medicine and Hygiene, 2012. **87**: p. 450.
98. Ram, P.K., et al., *Behavioral impact of promoting waterless hand cleansing with chlorhexidine during the perinatal period: results from a randomized controlled trial.* American journal of tropical medicine and hygiene. Conference: 64th annual meeting of the american society of tropical medicine and hygiene, ASTMH 2015. United states. Conference start: 20151025. Conference end: 20151029, 2015. **93**: p. 175-176.
99. Christensen, G., et al., *Pilot cluster randomized controlled trials to evaluate adoption of water, sanitation, and hygiene interventions and their combination in rural western Kenya.* Am J Trop Med Hyg, 2015. **92**: p. 437-447.
100. Luby, S.P., et al., *Combining drinking water treatment and hand washing for diarrhoea prevention, a cluster randomised controlled trial.* Trop Med Int Health, 2006. **11**: p. 479-489.
101. Nicholson, J.A., et al., *An investigation of the effects of a hand washing intervention on health outcomes and school absence using a randomised trial in Indian urban communities.* Tropical Medicine & International Health, 2014. **19**: p. 284-292.
102. Langford, R. and C. Panter-Brick, *A health equity critique of social marketing: Where interventions have impact but insufficient reach.* Social Science & Medicine, 2013. **83**: p. 133-141.
103. Stanton, B.F. and J.D. Clemens, *An educational intervention for altering water-sanitation behaviors to reduce childhood diarrhea in urban Bangladesh. II. A*

randomized trial to assess the impact of the intervention on hygienic behaviors and rates of diarrhea. Am J Epidemiol, 1987. **125**: p. 292-301.
104. Burns, J., et al., *Washing with hope: evidence of improved handwashing among children in South Africa from a pilot study of a novel soap technology.* BMC Public Health, 2018. **18**: p. 709.
105. Parvez, S.M., et al., *Achieving optimal technology and behavioral uptake of single and combined interventions of water, sanitation hygiene and nutrition, in an efficacy trial (WASH benefits) in rural Bangladesh.* Trials, 2018. **19**: p. 358.
106. Greenland, K., et al., *Multiple behaviour change intervention for diarrhoea control in Lusaka, Zambia: a cluster randomised trial.* Lancet Glob Health, 2016. **4**: p. e966-e977.
107. U S Department of Education, *The randomized controlled trial: What it is, and why it is a crucial factor in establishing "strong" evidence of an intervention's effectiveness.* Identifying and implementing educational practices supported by rigorous evidence: a User friendly guide, 2004.
108. Altman, D.G. and J.M. Bland, *How to obtain the confidence interval from a P value.* Bmj-British Medical Journal, 2011. **343**.
109. Briceno, B., A. Coville, and S. Martinez, *Promoting handwashing and sanitation: evidence from a large-scale randomized trial in rural Tanzania.* Policy Research Working Paper - World Bank, 2015: p. 58-pp.
110. Contzen, N., S. De Pasquale, and H.J. Mosler, *Over-Reporting in Handwashing Self-Reports: Potential Explanatory Factors and Alternative Measurements.* Plos One, 2015. **10**.
111. Watson, J.A., et al., *Does targeting children with hygiene promotion messages work? The effect of handwashing promotion targeted at children, on diarrhoea, soil-transmitted helminth infections and behaviour change, in low- and middle-income countries.* Tropical Medicine & International Health, 2017: p. 526-538.
112. Luby, S.P., et al., *The effect of handwashing at recommended times with water alone and with soap on child diarrhea in rural Bangladesh: an observational study.* PLoS Med, 2011. **8**: p. e1001052.
113. Willmott, M., et al., *Effectiveness of hand hygiene interventions in reducing illness absence among children in educational settings: a systematic review and meta-analysis.* Arch Dis Child, 2016. **101**: p. 42-50.
114. Curtis, V. and S. Cairncross, *Effect of washing hands with soap on diarrhoea risk in the community: a systematic review.* Lancet Infect Dis, 2003. **3**: p. 275-281.
115. Collaborators, G.B.D.R.F., et al., *Global, regional, and national comparative risk assessment of 79 behavioural, environmental and occupational, and metabolic risks or clusters of risks in 188 countries, 1990-2013: a systematic analysis for the Global Burden of Disease Study 2013.* Lancet, 2015. **386**: p. 2287-2323.
116. Hechter, M. and K.D. Opp, *Social Norms.* 2001.
117. Bicchieri, C. and R. Muldoon, *Social norms*, in *The Stanford Encyclopedia of Philosophy*, Z. E.N., Editor. 2011.
118. Bicchieri, C., *The Grammar of society: The nature and dynamics of social norms.* 2006.
119. Lapinski, M.K. and R.N. Rimal, *An explication of social norms.* Communication

Theory, 2005. **15**: p. 127-147.
120. Hardin, R., *Collective Action.* 1982.
121. Bicchieri, C., *Rationality and coordination.* 1990.
122. Kincaid, D.L., *From innovation to social norm: Bounded normative influence.* Journal of Health Communication: International Perspectives, 2004. **9**: p. 37-57.
123. Dyalchand, A., M. Khale, and S. Vasudevan, *What communication and institutional arrangements influence sanitation related social norms in rural India?* 2009.
124. Bettenhausen, K.L. and J.K. & Murnighan, *The emergence of norms in competitive decision making groups.* Administrative Science Quarterly, 1985. **30**: p. 350-372.
125. Arrow, H. and K.L. Burns, *Self-organizing culture: How norms emerge in small groups*, in *The Psychological Foundations of Culture* M. Schaller and S.C. Crandall, Editors. 2004, Erlbaum: Mahwah, NJ. p. 171-200.
126. Cruz, M.G., D.D. Henningsen, and M.L.M. Williams, *The presence of norms in the absence of groups? The impact of normative influence under hidden-profile conditions.* Human Communication Research, 2000. **26**: p. 104-124.
127. Berkowitz, A.D., *The Social norms approach: Theory, research, and annotated bibliography*. 2004: Trumansburg, NY.
128. Rimal, R.N., et al., *Moving toward a theory of normative influences: How perceived benefits and similarity moderate the impact of descriptive norms on behaviors.* Journal of Health Communication, 2005. **10**: p. 433-450.
129. Cialdini, R.B., R.R. Reno, and C.A. Kallgren, *A Focus Theory of Normative Conduct - Recycling the Concept of Norms to Reduce Littering in Public Places.* Journal of Personality and Social Psychology, 1990. **58**: p. 1015-1026.
130. Haines, M. and S.F. Spear, *Changing the perception of the norm: A strategy to decrease binge drinking among college students.* Journal of American College Health, 1996. **45**(3): p. 134-140.
131. Perkins, H. and D. Craig. *A Multifaceted social norms approach to reduce high-risk drinking: Lessons from Hobart and William Smith Colleges*. 2002. Massachussetts: Higher Education Center for Alcohol and Other Drug Prevention.
132. Berkowitz, A.D., *Application of social norms theory to other health and social justice issues*, in *The Social Norms Approach to Peventing School and College Age Substance Abuse: A handbook for educators, counselors, clinicians*, H.W. Perkins, Editor. 2002, Jossey-Bass: San Francisco.
133. Haines, M. and S.F. Spear, *A strategy to decrease binge drinking among college students.* Journal of American College Health, 1996. **45**: p. 134-140.
134. Berkowitz, A.D., *An overview of the social norms approach*, in *Changing the culture of college drinking: A socially prevention campaign*, L. Lederman, et al., Editors. 2005, Hampton Press.
135. Fabiano, P., *Applying the social norms model to universal and indicated alcohol interventions at Westen Washington University*, in *The social norms approach to preventing school and college age substance abuse: A handbook for educators, counselors, clinicians*, H. Perkins, Editor. 2003, Jossey-Bass: San Francisco.
136. Pischke, C.R., et al., *Normative misperceptions of tobacco use among university*

students in seven European countries: baseline findings of the 'Social Norms Intervention for the prevention of Polydrug usE' study. Addict Behav, 2015. **51**: p. 158-164.
137. Perkins, H.W. and H. Wechsler, *Variation in perceived college drinking norms and its impact on alcohol abuse: A nationwide study.* Journal of Drug Issues, 1996. **26**: p. 961-974.
138. Haines, P.M. *A Social norms approach to preventing binge drinking at colleges and universities*. 1996. The Higher Education Center for Alcohol and Other Drug Prevention
139. Foxcroft, D.R., et al., *Social norms information for alcohol misuse in university and college students.* Cochrane Database Syst Rev, 2015(12): p. CD006748.
140. Wechsler, H., et al., *Perception and reality: A national evaluation of social norms marketing interventions to reduce college students' heavy alcohol use.* Journal of Studies on Alcohol, 2003. **64**: p. 484-494.
141. Bagozzi, R.P., et al., *Cultural and situational contingencies and the theory of reasoned action: Application to fast food restaurant consumption.* Journal of Consumer Psychology, 2000. **9**: p. 97-106.
142. Kallgren, C.A., R.R. Reno, and R.B. Cialdini, *A focus theory of normative conduct: When norms do and do not affect behavior.* Personality and Social Psychology bulletin, 2000. **26**: p. 1002-1012.
143. Campo, S. and K.A. Cameron, *Differential effects of exposure to social norms campaigns: A cause for concern.* Health Communication, 2006. **19**: p. 209-219.
144. Lapinski, M.K., et al., *Testing the effects of social norms and behavioral privacy on hand washing: A field experiment.* Human Communication Research, 2013. **39**: p. 21-46.
145. Rimal, R.N., *Modeling the relationship between descriptive norms and behaviors: A test and extension of the theory of normative social behavior (TNSB).* Health Communication, 2008. **23**: p. 103-116.
146. Rimal, R.N. and K. Real, *Understanding the influence of perceived norms on behaviors.* Communication Theory, 2003. **13**: p. 184-203.
147. Rimal, R.N. and K. Real, *How behaviors are influenced by perceived norms - A test of the theory of normative social behavior.* Communication Research, 2005. **32**: p. 389-414.
148. Bendor, J. and P. Swistak, *The evolution of norms.* AJS, 2001. **106**: p. 1493-1545.
149. Homans, G.C., *The Human Group.* 1950.
150. Asch, S.E., *Effects of group pressure upon the modification and distortion of judgement*, in *Groups, Leadership, and Men*, H. Guetzkow, Editor. 1951, Carnegie Press: Pittsburgh, PA. p. 177-190.
151. Lapinski, M.K., et al., *Injunctive norms: Designing messages to modify perceived social sanctions*, in *National Communication Association*. 2008. p. 1.
152. Rosenstock, M., *The health belief model: Origins and correlates.* Health Education Monographs, 1974. **2**: p. 336-353.
153. Roger, R.W., *A protection motivation theory of fear appeals and attitude change.* Journal of Psychology, 1975. **91**: p. 93-114.
154. Rimal, R.N., K. Real, and D. Morrison, *Expanding the theory of normative*

influences: The role of ambiguity and identity, in *International Communication Association Annual Conference*. 2004: New Orleans, LA.
155. Christensen, P.N., et al., *Social norms and identity relevance: A motivational approach to normative behavior.* Personality and Social Psychology bulletin, 2004. **30**: p. 1295-1309.
156. Larimer, M.E., et al., *Predicting drinking behavior and alcohol-related problems among fraternity and sorority members: Examining the role of descriptive and injunctive norms.* Psychology of Addictive Behaviors, 2004. **18**: p. 203-212.
157. Trafimow, D. and K.A. Finlay, *The importance of subjective norms for a minority of people: Between-subjects and within-subjects analyses.* Personality and Social Psychology bulletin, 1996. **22**: p. 820-828.
158. Kelman, H.C., *Processes of opinion change.* Public Opinion Quaterly, 1961. **25**: p. 57-78.
159. Dickie, R., et al., *The effects of perceived social norms on handwashing behaviour in students.* Psychology Health & Medicine, 2018. **23**: p. 154-159.
160. Orsola-Vidal, A. and A. Yusuf. *Scaling up handwashing behavior: Findings from the impact evaluation baseline srvey in senegal*. 2011. World Bank.
161. Briceno, B., et al. *Promoting Handwashing Behavior: The effect of mass media and community level interventions in Peru*. 2012. World Bank.
162. Guiteras, R.P., et al. *Testing disgust- and shame-based safe water and handwashing promotion in urban Dhaka, Bangladesh*. 2015. International Initiative for Impact Evaluation.
163. Greenland, K., et al., *Disentangling the effects of a multiple behaviour change intervention for diarrhoea control in Zambia: a theory-based process evaluation.* Global Health, 2017. **13**: p. 78.
164. Rajaraman, D., et al., *Implementing effective hygiene promotion: lessons from the process evaluation of an intervention to promote handwashing with soap in rural India.* BMC Public Health, 2014. **14**: p. 1179.
165. CIA. *The World Factbook*. Africa: Côte d'Ivoire 2019 2018 [cited 2019 April 8, 2019]; Available from: https://www.cia.gov/LIBRARY/publications/the-world-factbook/geos/iv.html.
166. Trading Economics. *Ivory Coast - Urban population growth (annual %)*. 2019 [cited 2019 April 8, 2019]; Available from: https://tradingeconomics.com/cote-d-ivoire/urban-population-growth-annual-percent-wb-data.html.
167. UN-HABITAT, *Côte d'Ivoire : Profil urbain d'Abidjan*, in *Programmes des Nations Unies pour les etablissements humains*. 2012, UN-Habitat.
168. Djeguema, A., *Crises et recompositions dans les modes d'habiter et les pratiques sociales a Abidjan*, in *42nd ISoCaRP Congress*. 2006.
169. Manou-Savina, *Eléments pour une histoire de la cour commune en milieu urbain réflexions sur le cas ivoirien*, in *Tropiques, Lieux et liens*. 1989, Orstom: Paris. p. 73-97.
170. Philippe, A., A. Dubresson, and A. Manou-Savina, *Abidjan "Côté Cours": pour comprendre la question de l'habitat.* 1987.
171. Commune de Koumassi, *Commune de Koumassi.* 2017. **2017**: p. Site de la mairie de Koumassi.

172. Geohive, *Cote d'Ivoire: Administrative units, extended.* 2015. **2017**: p. Abidjan population data by Commune.
173. Vinceslas, *Les 10 communes les plus grandes d'Abidjan*, in *OUESTIN*. 2018.
174. UNICEF, *Cholera outbreak in Abidjan is prompting UNICEF to respond quickly.* 2011. **2013**.
175. Abidjan.net, *Epidémie de choléra en Côte d'Ivoire : des dizaines de cas et plusieurs décès enregistrés*, in *Abidjan.net*. 2015.
176. Tourangeau, R. and T. Yan, *Sensitive questions in surveys.* Psychological Bulletin, 2007. **133**: p. 859-883.
177. Biran, A., A. Tabyshalieva, and Z. Salmorbekova, *Formative research for hygiene promotion in Kyrgyzstan.* Health Policy Plan, 2005. **20**: p. 213-221.
178. Curtis, V., et al., *Dirt and diarrhoea: Formative research in hygiene promotion programmes.* Health Policy Plan, 1997. **12**: p. 122-131.
179. Hsieh, H. and S. Shannon, *Three approaches to qualitative content analysis.* Qualitative health research, 2005. **15**: p. 1277-1288.
180. Bengtsson, M., *How to plan and perform a qualitative study using content analysis.* NursingPlus open, 2016. **2**: p. 8-14.
181. Patton, M., *Qualitative research and evaluation methods.* 2002.
182. Coffey, A. and P. Atkinson, *Making sense of qualitative data: Complementary research strategies.* 1996.
183. Lacey, A. and D. Luff, *Qualitative research analysis*. 2007, The NIHR RDS for the East Midlands/ Yorkshire & the Hunter.
184. National Science Foundation, *User friendly handbook for mixed methods evaluations.* 1997.
185. Stemler, S., *An overview of content analysis.* Practical assessment, research and evaluation, 2001. **7**: p. 1-6.
186. World Health Organization and UNICEF, *Types of drinking-water sources and sanitation.* 2003.
187. UNICEF, *Journée mondiale du lavage des mains*. 2008, United Nations Children's Fund.
188. World Health Organization, *Cholera: Number of reported cases by country*. 2013, The World Health Organisation.
189. Whitby, M., et al., *Behavioural considerations for hand hygiene practices: the basic building blocks.* J Hosp Infect, 2007. **65**: p. 1-8.
190. Lapinski, M.K., et al., *The role of group orientation and descriptive norms on water conservation attitudes and behaviors.* Health Communication, 2007. **22**: p. 133-142.
191. Lapinski, M.K., et al., *Social influence in childcare centers: A test of the theory of normative social behavior.* Health Communication, 2014. **29**: p. 219-232.
192. DeVellis, R., *Scale development: theory and applications.* Applied Social Research Methods Series, 2012.
193. Clark, L.A. and D. Watson, *Constructing validity: Basic issues in objective scale development.* Psychological Assessment, 1995. **7**: p. 309-319.
194. Krosnick, J., *The threat of satisficing in surveys: the shortcuts respondents take in answering questions.* Survey Methods Newsletter, 2000. **20**: p. 4-8.

195. Loevinger, J., *Objective tests as instruments of psychological theory.* Psychological Reports, 1957. **3**: p. 635-694.
196. Fowler, F., *Improving survey questions: Design and evaluation* 1995.
197. Netemeyer, R., B. William, and S. Subhash, *Scaling procedures: Issues and applications.* 2003.
198. Swain, S.D., D. Weathers, and R.W. Niedrich, *Assessing three sources of misresponse to reversed Likert items.* Journal of Marketing Research, 2008. **45**: p. 116-131.
199. Comrey, A.L., *Factor-Analytic Methods of Scale Development in Personality and Clinical-Psychology.* Journal of Consulting and Clinical Psychology, 1988. **56**: p. 754-761.
200. Tinsley, H.E.A. and D.J. Tinsley, *Uses of Factor-Analysis in Counseling Psychology Research.* Journal of Counseling Psychology, 1987. **34**: p. 414-424.
201. Acock, A., *Discovering structural equation modeling using stata.* 2013.
202. Skrondal, A. and S. Rabe-Hesketh, *Structural equation modeling: categorical variables*, in *Encyclopedia of Statistics in Behavioral Science*. 2005, Wiley.
203. Eisinga, R., M. te Grotenhuis, and B. Pelzer, *The reliability of a two-item scale: Pearson, Cronbach, or Spearman-Brown?* International Journal of Public Health, 2013. **58**: p. 637-642.
204. Gordon, B., *Don't use reverse-coded items in scales*, in *Scale-related pet-peeves*. 2013.
205. Stewart, T.J. and A.W. Frye, *Investigating the use of negatively phrased survey items in medical education settings: Common wisdom or common mistake?* Academic Medicine, 2004. **79**: p. S18-S20.
206. Chung, M. and M.K. Lapinski, *Extending the Theory of Normative Social Behavior to Predict Hand-Washing among Koreans.* Health Communication, 2018: p. 1-10.
207. Floyd, F.J. and K.F. Widaman, *Factor analysis in the development and refinement of clinical assessment instruments.* Psychological Assessment, 1995. **7**: p. 286-299.
208. Birkinshaw, M. *Social marketing for health*. 1993. Wolrd Health Organization.
209. The National Social Marketing Centre, *What is social marketing ?* 2016. **2017**.
210. Evans, W., *How social marketing works in health care.* British medical journal, 2006: p. 1207-1210.
211. Bischoff, W.E., et al., *Handwashing compliance by health care workers: the impact of introducing an accessible, alcohol-based hand antiseptic.* Arch Intern Med, 2000. **160**: p. 1017-1021.
212. Glogerm, *Glo germ.* 2017. **2017**.
213. Arc-en-Ciel, L.F., *La Famille Arc-en-Ciel*. 2017.
214. Whitley, E. and J. Ball, *Statistics review 4: sample size calculations.* Crit Care, 2002. **6**(4): p. 335-41.
215. Gorter, A., et al., *Hygiene behaviour in rural Nicaragua in relation to diarrhoea.* International Journal of Epidemiology, 1998. **27**: p. 1090-1100.
216. Curtis, V., et al., *Potties, pits and pipes: explaining hygiene behaviour in Burkina Faso.* Social Science & Medicine, 1995. **41**: p. 383-393.
217. Galobardes, B., et al., *Indicators of socioeconomic position (part 1).* Journal of

Epidemiology and Community Health, 2006. **60**: p. 7-12.
218. Institut national de la statistique (INS) and I C F International. *Enquete demographique et de sante et à indicateurs multiples de Côte d'Ivoire*. 2012. Calverton, Maryland, USA: INS et ICF International.
219. World Bank, *Measuring Living Standards: Household consumption and wealth indices*. Quantitative techniques for health equity analysis-technical note 4. 2003.
220. UNICEF, *UNICEF Cote d'Ivoire 2009-2013.* 2009.
221. Centers for Disease Control and Prevention, *2014-2016 Ebola outbreak in West Africa*. 2019.
222. Mangham, L.J., K. Hanson, and B. McPake, *How to do (or not to do) ... Designing a discrete choice experiment for application in a low-income country.* Health Policy Plan, 2009. **24**(2): p. 151-8.
223. Benjamin-Chung, J., et al., *A Randomized Controlled Trial to Measure Spillover Effects of a Combined Water, Sanitation, and Handwashing Intervention in Rural Bangladesh.* American Journal of Tropical Medicine and Hygiene, 2018. **97**: p. 213.
224. Lucini, B.A. and K. Bahia, *Country overview: Côte d'Ivoire, driving mobile-enabled digital transformation* 2017, GSMA.
225. Institut National de la Statistique (INS) et ICF International, *Enquête Démographique et de Santé et à Indicateurs Multiples de Côte d'Ivoire 2011-2012.* , in *DHS*. 2012, INS et ICF International: Calverton, Maryland, USA.
226. JUMIA, *Growth of the smarthphone market in Africa 2015*. 2015, JUMIA.
227. Digital. *Digital 2019 Cote d'Ivoire (January 2019) v01*. 2019 [cited 2019 29/10/2019]; Available from: https://www.slideshare.net/DataReportal/digital-2019-cote-divoire-january-2019-v01.
228. Institut National de la Statistique et de la Démographie (INSD) et ICF International, *Enquête Démographique et de Santé et à Indicateurs Multiples du Burkina Faso 2010*, in *DHS*. 2012, INSD et ICF International: Calverton, Maryland, USA.
229. Ghana Statistical Service (GSS), G.H.S.G., and ICF International, *Ghana Demographic and Health Survey 2014*, in *DHS*. 2015, GSS, GHS, and ICF International: Rockville, Maryland: USA.
230. Center, P.R. *Global divide on smartphone ownership* Global divide on smartphone ownership 2019 [cited 2019 29/10/2019]; Available from: https://www.pewresearch.org/global/2018/06/19/2-smartphone-ownership-on-the-rise-in-emerging-economies/pg_2019-01-10_smartphoneupdate_0-01/.
231. Center, P.R. *Global divide on smartphone ownership*. Global divide on smartphone ownership 2016 [cited 2019 29/10/2019]; Available from: https://www.pewresearch.org/global/2016/02/22/smartphone-ownership-rates-skyrocket-in-many-emerging-economies-but-digital-divide-remains/technology-report-03-06/.
232. Digital. *Digital 2019 Ghana (January 2019) v01*. 2019 [cited 2019 29/10/2019]; Available from: https://www.slideshare.net/DataReportal/digital-2019-ghana-january-2019-v01.

233. Digital, *Digital 2019 Burkina Faso (January 2019) v01.* 2019.