American Journal of Infection Control 45 (2017) 521-7

Contents lists available at ScienceDirect

American Journal of Infection Control

journal homepage: www.ajicjournal.org

State of the Science Review

Construct validity—Current issues and recommendations for future hand hygiene research

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Key Words:
Construct validity
Hand hygiene research
Hand hygiene compliance
Health services research
Research methods

Health care–associated infection is a leading cause of morbidity and mortality. Hand hygiene is widely regarded as an effective prevention strategy. Often, hand hygiene research is designed and conducted by health care practitioners who may lack formal training in research methods, particularly in the area of social science. In a research context, a construct is a concept that can be measured or observed in some way. A construct can be directly or indirectly measured. For example, height can be directly measured by centimeters, whereas depression can be indirectly measured by a scale of 20 items. Every construct needs to be operationalized by measure(s) to make it a variable. Hence, construct validity refers to the degree of fit between the construct of interest and its operational measure. However, issues with construct validity often weaken the translation from construct to measure(s). This article will (1) describe the common threats to construct validity pertaining to hand hygiene research, (2) identify practical limitations in current research design, and (3) provide recommendations to improve construct validity in future hand hygiene research. By understanding how construct validity may affect hand hygiene research design, there is great potential to improve the validity of future hand hygiene research findings.

BACKGROUND

There is a continued interest in examining hand hygiene (HH) as a strategy to prevent health care–associated infection. However, there is a lack of high-quality HH research.[1] Often, HH research is designed and conducted by health care practitioners who may lack formal training in research methods, particularly in the area of social science research.

Construct validity refers to the degree to which a test measures what it claims to be measuring.[2] In research, a construct relates to a trait (eg, hand hygiene compliance [HHC]) that is being evaluated, and it needs to be operationalized by measure(s) into a variable. A construct can be measured directly or indirectly. For example, height can be directly measured by centimeters, whereas satisfaction can be indirectly measured by a 10-item scale. However, poor construct validity often weakens the translation from a construct to measure(s) and makes the research vulnerable to inaccurate or weak measurements.[3]

In this article, the following aspects of construct validity will be discussed: (1) Hawthorne effect, (2) self-reporting, (3) experimenter effect, (4) evaluation apprehension, (5) hypothesis guessing, (6) timing, (7) restricted range, and (8) mono-operation bias. By understanding how construct validity may affect HH research design, there is great potential to improve the validity of future HH research findings. Table 1 provides a list of threats to construct validity for various measures of HH.

Hawthorne effect (observer effect)

Hawthorne effect refers to people's tendency to alter their behavior when they are aware of an observer's presence.[4] Given the tendency to identify information that conforms to the hypothesis, Hawthorne effect can lead to the experimenter interpreting results inaccurately. Hawthorne effect is a major threat to construct validity and therefore is usually controlled with double-blind experimental designs.[44]

Randomized covert observations should be used.[5,8] Different medical students or volunteers can be covert observers to provide more observation opportunities, reducing Hawthorne effect, while maintaining patients' privacy.[8,10] Observers should observe health care workers' (HCWs') HH during their usual care activity. Observers (especially student volunteers) need to understand the logic of care and typical workflow and should receive training in this area

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Conflicts of interest: None to report.

0196-6553/
http://dx.doi.org/10.1016/j.ajic.2017.01.028

Table 1
Threats to construct validity for various measures of HH: Practical limitations, examples, and recommendations

Practical limitations	Examples from relevant studies	Recommendations
1. Hawthorne effects (direct observation)		
Observer and selection bias—difficult for observers to be blinded to the hypothesis.[4]	300% increase in HHC when HH is measured using direct observation compared with electronic monitoring.[4]	Before any formal HH observation training, provide training (particularly for student volunteers) to educate observers on the logic of care and typical workflow.[5] Include pilot observation phase in research design to train, validate, and standardize observation techniques among observers.[5,6] Validate observations at the beginning and at regular intervals to ensure accuracy.[7] Be mindful not always to observe HCWs with extreme HH behavior.[5] Only observer HCW's HH during usual care activity.[5]
Infection control staff are usually well-recognized among HCWs—difficult to conduct covert observations.[8]	Lower HHC during covert observations where audits were not announced in advance.[9]	Recruit medical students or volunteers to conduct random covert observations.[8,10] Randomly select HCWs to observe HH.[5]
2. Self-report (self-report)		
Experimenter expectancy—volunteers of self-reports may be inclined to report higher levels of HH.[11]	Systematic overestimation of self-reported HH[12,13] may be because of unrealistic expectations of HHC[14] and the tendency to provide higher ratings of socially desirable behavior such as HHC.[15]	Avoid self-report as the sole or major measure of HH.[16] Ask questions in self-reports that can subsequently be compared with data from direct observation.[17] Surveys should be administered by a researcher who is not acquainted with the respondent.[17]
Selection bias—difficult to select HCWs randomly to participate in self-report of HH.[18]		Random selection of HCWs to participate in self-report is recommended.[18]
Various research designs used to examine self-reports of HH in different health care settings and demographics—difficult to compare results directly.[16]		Appropriate when trying to understand HCW's opinions (instead of using self-report to measure HH).[19] Use anonymous survey instruments.
3. Experimenter effect (direct observation)		
Lack of labor force, confidential observation process and reporting—often conducted by infection control staff. Difficult to randomize schedules and staff involved in direct observations.[20]	HCWs alerted to sanitize their hands whenever they recognized an infection control staff standing by the hallways.[21] HCWs recognized certain characteristics of the experimenter after some time.[23]	Use data-driven approaches (eg, simulations for different observer movement schedules) to generate randomized schedules, personnel, and locations for HH audits.[22] Do not perform covert observations in conjunction with promotional HHI.[24] Randomly select HCWs to observe HH.[5] Be mindful not to always observe HCWs with extreme HH behavior.[5] Observers should be required to observe a minimum number of HH opportunities across different types of HCWs.[25]
4. Evaluator apprehension (electronic monitoring)		
If it not yet possible to have electronic monitoring systems that are entirely hidden.	HCWs may observe HHC because they are apprehensive or concerned about being evaluated by electronic monitoring systems.[26,27]	Consider if it is ideal to provide relevant information on e-monitoring to HCWs and visitors.[28] Avoid disrupting workflow and operations.[29] Have a transparent decision process to allow staff to raise any concerns and get their buy-in.[30]
4. Evaluator apprehension (focus groups)		
Staff of different seniority are often represented from various departments.[31]	Because of social pressure, HCWs may only express opinions that are perceived to be normal or socially desirable.[32]	Questions should suggest that any response is normal (eg, some people said one answer, some said another answer). Use multiple experimenters so that no one is perceived as an evaluator.[33]
4. Evaluator apprehension (self-report)		
Researchers need to collect HHC data from different groups of HCWs.	Identifiers (eg, names, job roles) may cause respondents to feel apprehensive about providing their true opinions.[34]	Perform statistical control for the influence of a socially desirable response style.[17] Pipeline procedure: make respondents believe that the interviewers will learn their true HHC regardless of self-reports because an additional measure will be applied.[35] Questions should suggest that any response is normal (eg, some people said one answer, some said another answer). Use multiple experimenters so that no one is perceived as an evaluator.[33]
5. Hypothesis guessing (direct observation)		
Using initial direct observations to measure baseline HHC, researchers may not want HCWs to know that HH research is in progress.[21] For practical limitations associated with direct observations, see Experimenter effect.	Hypothesis guessing may affect baseline HHC and lead to inaccurate assessment of the actual effects of an HHI on HHC.[21]	Ask subjects for their views of the hypothesis after the study.[2] Do not confirm or alert subjects to the true hypothesis. Only use a small random sample for this inquiry to avoid alerting other subjects.
HCW does not notice HHI (eg, visual cues) after some time.	Although hypothesis guessing may initially increase HHC, sustainability of HHC is difficult when the novelty or awareness of a new HHI is no longer salient.[36]	If necessary, provide subjects with an alternative hypothesis that is not the true hypothesis. Indicate this step explicitly when seeking ethics approval.

(*continued on next page*)

Table 1
Continued

Practical limitations	Examples from relevant studies	Recommendations
6. Timing (all measures of HH)		
Lack of labor force and budget to hire independent observers—direct observation often have short durations.	Several HH research have indicated short research durations,[37] or the lack of sustainable HHC beyond the research durations.[36]	Frequent performance feedback and targeted training programs may create sustained HH measurements.[38]
No standard guidelines that specify the number of data collection points or research durations.		Recruit medical students or volunteers to conduct random covert observations.[8,10] Avoid observation only during periods with a high number of HH opportunities and density of care.[5] Provide ongoing managerial interventions to sustain the benefits of electronic monitoring.[39]
High implementation cost—difficult for widespread implementations of electronic monitoring systems over long periods.[40]		Conduct time-in-motion studies to approximate the minimum required time HCWs must be present in a patient room for patient contact to happen.[41] Consider other aspects of construct validity (eg, creating distinct levels of HHIs, reducing Hawthorne effect) during research design to improve durations and sustainability of HHI.
7. Restricted range (educational HHIs)		
Time, costs, ethics, and diffusion of treatment—difficult to examine an educational HHI at different ranges or levels.[42]	Several studies have examined the influence of an educational HHI (eg, hands-on training, online training, HH techniques) on HHC,[23,42] but few have done so at different levels.[40]	Design programs with distinct features that can be classified into different levels. Simple: timing, use of jargons, presentation mode. Complex: message design, perceived difficulty.
Subjective nature of educational HHI—difficult to objectively quantify or assign variations of educational HHIs into different levels of IVs.[23,42]		Consult with education or communication specialist to design educational programs with clear variations that can be tested in different research conditions.
7. Restricted range (electronic monitoring)		
Convenience—no need for researchers to adjust system settings.	Most studies used only one level of IV (eg, using the same electronic monitoring setting throughout the research duration).[26]	For phased installation, if possible, select units with high HH rates.[43] For large-scale installation, assess all internal capacity of the organization.[43]
Ethical considerations—may be unethical to switch off a monitoring system occasionally.		Use different research conditions to explore the influence of (1) different hours of active monitoring, (2) level of staff identification, and (3) types of feedback in the event of non-HHC on HHC.
8. Mono-operation bias (all measures of HH)		
Lack of research training, capacity, and expertise—may limit some researchers from adopting a triangulation (multimethod) approach.[31]	Even with a single method, variations in measurement techniques (eg, different direct observation criteria, different electronic monitoring systems) have made direct comparisons of results difficult.[40]	Avoid using one exemplar to operationalize a construct.
See previous sections for practical limitations associated with different measures of HH.		When self-report or direct observation is used as sole HH measure, use other measures (eg, monitor HH product use) to increase validity and reliability of inferences.[31] When self-reports are appropriate, use multiple constructs to examine knowledge, attitudes and beliefs, and self-perception of HHC.[9]

HCW, health care worker; *HH*, hand hygiene; *HHC*, hand hygiene compliance; *HHI*, hand hygiene intervention; *IV*, independent variable.

before any formal training on HH observation.[5] A pilot observation phase may allow staff to train, validate, and standardize observation techniques among observers.[5,6] According to Ellingson et al, "it is crucial that observers are trained and that their observations are validated initially and at intervals to ensure accuracy; a suite of tools was developed by the WHO [World Health Organization] to help standardize the observation process."[24] However, with repeated observations, observers may know the intrinsic performance of individual HCWs. Therefore, observers should be mindful not always to observe HCWs with extreme HH behavior because this may affect the result.[5] Although the Hawthorne effect may initially improve HHC,[8] researchers should design sustainable observation techniques with periodic evaluation of HH programs.

Self-reporting

There is no objective measure to HH measured through self-reporting.[21] Lack of accuracy and verification of self-reported HH threaten construct validity.[3] If one method such as self-report is used to measure both constructs for the independent variable (IV) and dependent variable, methodologic overlaps between the 2 variables may subject the research design to monomethod bias (see respective sections).[3] For example, if research examines the influence of an HH education program on HHC, if both constructs are measured with self-reports, any observed association may be caused by the monomethod bias.

Self-report is not recommended as the sole or major method to measure HH[16] and should be used with extreme caution.[45] It might be helpful to ask questions on HH in self-reports that can subsequently be compared with data from direct observation. The tendency of overreporting of HHC may be reduced if the survey is administered by someone who is not acquainted with the respondent.[17]

Self-report may be appropriate if research is trying to understand HCW opinion that will inherently require self-report.[19] For research with limited resource capacities (eg, HH research in a resource-limited health care setting) that may still use self-reports and survey, instruments that promote self-reflection of HHC among HCWs may produce a more accurate prediction of HH.[46] Random selection of HCWs to participate in self-reporting is recommended.[18] Although the topic of reliability is not discussed in this article, an HH survey with low interrater and retest reliability may suggest the inadequacy of a single assessment (ie, self-report) to evaluate HHC.[17] More research is needed in this area.

Experimenter effect

Experimenter effect occurs when characteristics of the experimenter, such as job role and sex, affect research outcomes. Experimenter effect can disrupt the operationalization of a construct, such as HH, to a valid measure, such as by direct observation.[47]

Data-driven approaches (eg, simulations for different observer movement schedules) may be used to generate randomized schedules, personnel, and locations for HH audits.[22] Using architectural drawings, ideal locations where observers should stand during observations are identified. In the study by Fries et al, such ideal locations are those "line-of-sight positions that maximize the total number of visible patient room doorways." A stochastic process can then be used to generate a variety of HHC behaviors among HCWs. This process can allow researchers to simulate each work shift using different scenarios (ie, different HHC rates), but with identical HCW movement. This simulation framework can allow researchers to quantitatively determine if the accuracy of HHC would vary with different observation scenarios. For more information on the approach, please refer to Fries et al.[22]

Infection control programs using direct observations should consider the unique challenges of their hospitals.[40] Rosenthal et al implemented an infection control program (Measure to Achieve Patient Safety) at the UCLA Medical Center to allow undergraduate students to conduct direct observations which reduced cost, labor force, and time.[10] Covert observations may eliminate observation bias. Covert observations, however, should not be performed in conjunction with promotional hand hygiene intervention (HHI) because they can induce mistrust in the observed HCW. If baseline HHC is derived from covert observations, a change of method (ie, using overt follow-up observations) may confound the results.[24] With repeated observations, observers may know the intrinsic performance of individual HCWs. Therefore, observers should be mindful not to always observe HCWs with extreme HH behavior because this may affect the result.[5] Observers should be required to observe a minimum number of HH opportunities across different types of HCWs.[25]

For self-reports, researchers should administer a survey such that the experimenter's characteristics, such as job role, would not influence survey outcomes.[47] For example, anonymous mandatory surveys may be distributed to participants after a program.

Evaluation apprehension

In research, subjects may be apprehensive or concerned about being evaluated by experimenters. Subjects may tend to want to look good and therefore provide politically correct or socially desirable responses. This tendency constitutes a form of confounder that may threaten construct validity.[48]

For electronic monitoring, researchers should consider if it is ideal to provide some information to HCWs and visitors, yet without compromising the research objectives.[28] For example, should users be informed of the true purpose of the monitoring system (ie, HH monitoring), or should they simply be told that the system was installed for security purposes? Ultimately, HHI should avoid disruption to workflow and operations,[29] yet maintain the integrity of the research objectives. For example, an electronic monitoring system that is positioned over a patient's bed can detect whether HCWs observed HH. HCWs wear a device that beeps if HH is not observed before or after patient contact.[26] When considering whether to implement electronic monitoring, having a transparent decision process can allow staff to raise any concerns regarding the system and get their buy-in at the same time.[30]

For focus groups or HH surveys, researchers should reduce possible evaluation apprehension with the phrasing of questions. Questions may be presented in a way to highlight that any response is normal and that some people have indicated one answer, whereas others have indicated another. Questions should be framed in a way to make respondents feel comfortable and respected.[35,49] According to Contzen et al, statistical control for the influence of a socially desirable response style in self-reports can be done "by partialling out its effect through multiple regression analysis or partial correlations."[17] To reduce the tendency of socially desirable responding, a pipeline procedure may be applied. As suggested by Tourangeau and Yan, "in this method, respondents are made to believe that the interviewers will learn their true behavior regardless of self-reports because an additional measure (e.g. a microbiological hand contamination assay) will also be applied."[35] Focus groups or surveys should be conducted by multiple experimenters so that no one is perceived as an evaluator.[33]

Hypothesis guessing

Research subjects do not simply participate in a passive manner and may try to guess the study hypothesis. Hence, subjects may base or change their behavior depending on their guess, and this observed behavior change is not an actual result of the treatment.[48] For example, when HH is measured through direct observations along the hallways, subjects might guess that the dependent variable involves measuring HHC.[23] If subjects increase their HHC, this may not be because of the HHI but simply because they are aware of the research hypothesis.[20] Therefore, the increase in HHC cannot be labeled as an effect from the HHI. The labeling issues resulting from hypothesis guessing creates a threat to construct validity.[48]

To determine if subjects could guess the hypothesis, researchers may ask subjects for their views of the hypothesis.[2] However, this step should be executed with caution in order not to confirm or alert subjects on the true hypothesis. Researchers may simply ask subjects what they think the hypothesis might be, instead of providing them with a list of possible hypotheses. A small random pilot sample of subjects should be chosen for such investigation in order not to alert the majority. Researchers may also provide an alternative hypothesis that is not the true hypothesis. For example, subjects may simply be told that their walking behavior is being observed. With an alternative hypothesis, it may be less likely for subjects to guess the true hypothesis. Behavior in the real hypothesis should not be affected because of behavioral change as a result of the alternative hypothesis. Researchers should explicitly express this step as a way to address hypothesis guessing as a threat to construct validity when seeking ethics approval.

Timing

The timing of measurement may affect research outcomes.[50] When researchers examined the effects of an HHI, any observed effects over short durations may not be applicable over longer durations.

By providing due considerations to labor force, costs, and time, researchers should design HHIs that allow for repeated or extended HH measurements. Frequent performance feedback and targeted training programs might create sustained HH measurements.[38] Direct observations with medical students or volunteers may alleviate issues involving labor force and cost.[10] A representative selection of time should be assigned for observation. Avoid observation only during periods with a high number of HH opportunities and density of care because there is usually a correlation between the density of HH opportunities and HHC.[5]

For electronic monitoring, without proper follow-up evaluations, the use of electronic monitoring may ultimately reduce HHC after an extended duration.[39] As highlighted by Staats et al,[39] "man-

agers cannot simply 'monitor and forget.' Rather, the observed drop-off in compliance after a lengthy period of monitoring suggests that there is a need for ongoing managerial interventions to sustain the benefits of monitoring." Before collecting HH data by electronic monitoring, units can conduct time-in-motion studies to approximate the minimum required time HCWs must be present in a patient room for patient contact to happen. The time can be derived via direct observation of HCWs performing various patient care activities. This minimum time can be used to determine the number of entries or exits from a patient's room. Therefore, if a HCW entered or exited a patient room that was faster than the minimum time, these movements should not be considered a valid entry or exit.[41] However, this recommendation is only applicable to sensors with time functions (ie, delayed sensing capabilities).[51] As electronic monitoring becomes more affordable, increased implementation and sustainability of such HH measures can be expected.[27] By considering other aspects of construct validity (eg, creating distinct levels of HHIs, reducing the Hawthorne effect), it may be easier to expose subjects to HHIs over extended durations.

Restricted range

In research design, if the IV has an insufficient or narrow range, any findings may only be associated with that range, and not the overall construct.[2] For example, a study examining the influence of an HHI on HHC may reveal no effect. However, when a wider range for the IV (ie, HHI) is used, other outcomes or trends might be identified.

Researchers should design HH educational programs with distinct features that allow programs to be classified into different levels. These features may vary in complexity from simple (eg, duration, number of jargons used in a presentation, mode of presentation) to complex (eg, message design, perceived difficulty). During research design, researchers should consult with education or communication specialists to provide a clear distinction between different levels of educational programs. This may allow researchers to identify key components of educational programs to create more targeted educational HHIs.

For phased implementation of electronic monitoring systems, if possible, researchers should select units with high HH rates during the initial installation phases to better anticipate and resolve any technical issues before subsequent installations. Before any large-scale installation of electronic monitoring systems, administrators should assess all internal capacity (ie, cost-benefit analysis, financial and human resources, information technology, leadership and management, operations) of the organization.[43] There are also various ways to study the influence of electronic monitoring on HHC at different levels. These systems can vary in terms of hours of active monitoring, level of staff identification, or types of feedback in the event of non-HHC. Researchers should consider how examining the IV at different levels may improve infection control efforts, sustainability of HHIs, and costs savings.

Mono-operation bias

Mono-operation bias occurs when only one exemplar is used to operationalize a construct.[52] It further threatens construct validity when the only exemplar (eg, direct observation) contains irrelevancies,[53] such as using irrelevant observation criteria that do not get at the actual HH behaviors.[21] For example, in an HH observation form, if an observer is required to indicate the job role of the observed caregiver (eg, physician, nurse, therapist, aide, custodian, technician), this criteria (job role) may be irrelevant for generic observations where the aim is only to calculate the overall HHC rates, and the role of the observed caregiver will not be analyzed in any way during data analysis. Therefore, during research planning, researchers should determine the truly important data that need to be collected using direct observation. Having irrelevant observation criteria can also impose additional cognitive load on observers and may lead to missed HH observations.

The use of a single exemplar to operationalize a construct is not recommended for HH research. Without compromising the intended research objectives, researchers should consider possible way(s) to include multiple measures during the research design planning process. When less reliable and overt measures (eg, self-reports) are used as the sole HH measure, additional measures (eg, monitoring volume of HH product used) should be used to strengthen the validity and reliability of inferences to build a more solid ground for a more powerful conclusion.[31] When appropriate, randomized direct observations may be used to complement electronic monitoring to better understand HH behaviors among specific groups of HCWs.[37] Because electronic monitoring systems may not always measure the denominator (ie, total opportunities for HH) in HHC calculations, researchers may consider substituting the denominator with an alternative figure (eg, patient days, workload indicators) derived from a computerized database of nursing activities.[54] When self-reports are appropriate, multiple constructs should be used in the survey to examine knowledge, attitudes and beliefs, and self-perception of HHC.[9]

CONCLUSIONS

To improve HH research design, it is important to consider issues related to construct validity. Well-designed HH research can allow researchers to better understand the influence of HHI on improved HHC. In general, each HH measure has its strengths and weaknesses. Using multiple methods to measure HH is a way to address the strengths and limitations associated with a single measurement approach.[55-57] The following ways to improve construct validity are proposed for future HH research.

General

1. The World Health Organization, Centers for Disease Control and Prevention, and Society for Healthcare Epidemiology of America provide recommendations, guidelines, and resources on HH observation tools which may be modified by facilities and units for their own observations.[5,58]
2. Researchers should consult with staff from the hospital's ethics review board to ensure that research will be conducted in accordance with ethical guidelines.

Direct observation

3. Although routine observation needs to be simple and straightforward, observations for HH research may be more detailed or complex. Considerations should be made when deciding if observation protocols from an HH study should remain constant or be modified for the purpose of routine monitoring.[55,56]
4. Observers should not always observe HCWs with extreme HH behavior.[5]
5. Include a pilot observation phase to train, validate, and standardize observation techniques among observers.[5]
6. Random covert observations should be performed using students or volunteers.[8,10]
7. Do not perform covert observations in conjunction with promotional HHIs.[24]
8. Observe a minimum number of HH opportunities across different types of HCWs.[25]

Electronic monitoring

9. Conduct time-in-motion studies to approximate the minimum required time HCWs must be present in a patient room for patient contact to happen.[41]
10. For phased installation, if possible, select units with high HH rates to better anticipate and resolve any technical issues before subsequent installations.[43]
11. Assess internal capacity (eg, cost-benefit analysis) of the hospital before large-scale installation.[43]

Self-reports

12. Self-reports are not recommended as the sole method to measure HH.[16]
13. Apply a pipeline procedure to make respondents believe that the interviewers will learn their true HHC regardless of self-reports because an additional measure will be applied.[35]
14. Surveys should be administered by a researcher who is not acquainted with the respondent.[17]
15. Questions should suggest that any response is normal.

Acknowledgments

The author would like to acknowledge the contributions of Dr. Gene Resnick, Senior Medical Consultant, ICON plc and Dr. Ole Vielemeyer, Associate Attending Physician, New York-Presbyterian Hospital and for their time, clinical expertise, and feedback on this paper.

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