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The Role of Communication Confidence and Professional Credibility in Linking Communication Skill-Development to Workplace Influence

By

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ABSTRACT: Organisations invest heavily in communication skill-development, yet evidence that such investment converts into workplace influence is thin, and the psychological and social mechanisms that might carry the conversion are rarely modelled together. This article develops and validates a serial mediation framework in which skill-development raises communication confidence, confidence is converted into professional credibility as judged by colleagues and supervisors, and credibility in turn produces workplace influence. Drawing on social cognitive theory, source credibility research and the study of influence, the framework yields six hypotheses and a pre-specified, multi-source, three-wave protocol for Kenyan formal-sector organisations. Because the protocol has not yet been fielded, the empirical contribution is a Monte Carlo validation of the planned estimator, anchored on published meta-analytic effect sizes (training effects of $d = 0.60$ to 0.63 ; self-efficacy and performance, $r = .38$). Across 85,800 simulated samples, the test of the serial indirect effect reached 80% power at 150 to 360 respondents, depending on path size and measurement reliability, whereas the test routed through the weakest direct link needed 680 to 1,380. Measurement error at a reliability of .80 shrank indirect effects by about 49%, Monte Carlo intervals covered the attenuated parameter at near-nominal rates (.931 to .963), and the Type I error rate for the serial test was .058 to .066. The article specifies sample-size rules, a rater design and reporting standards, and it argues that skill-development programmes should be judged by the confidence and credibility they generate, not by skill acquisition alone.

KEYWORDS: Communication confidence, Professional credibility, Communication skill-development, Workplace influence, Serial mediation, Monte Carlo simulation, Kenya.

Abbreviations: CFA, confirmatory factor analysis; CI, confidence interval; ICC, intraclass correlation; MC, Monte Carlo; NACOSTI, National Commission for Science, Technology and Innovation; SEM, structural equation modelling; T1–T3, measurement waves.

INTRODUCTION

Communication competence has long been understood as the combination of knowledge, skill and motivation that produces impressions of appropriateness and effectiveness in the eyes of others (Spitzberg, 1983; Rubin & Martin, 1994). In organisations those impressions are consequential. Whether a manager's proposal is adopted, whether a junior analyst's warning is heeded, and whether a team leader can mobilise resources across departmental lines depend less on what is said than on whether the speaker is listened to. This capacity to shape the

decisions, effort and resources of others is workplace influence, a long-standing concern of organisational scholarship (French & Raven, 1959; Kipnis et al., 1980; Yukl & Falbe, 1990).

Organisations respond by investing in communication skill-development: workshops, coaching, presentation and negotiation practice, and mentoring. Meta-analytic evidence shows that training is on average effective, with sizeable mean effects on learning and on trainee behaviour (Arthur et al., 2003). The same

literature shows that effects on organisational results depend on design, transfer climate and trainee characteristics (Blume et al., 2010). An unresolved question is why a gain in skill should translate into greater influence at all. A person who has learned to structure an argument is not automatically more persuasive, because persuasion is conferred by an audience.

Two mechanisms are plausible, and neither has been modelled in relation to the other. First, skill-development can raise communication confidence, the belief that one can communicate effectively in the situations that work presents (Bandura, 1977, 1997; McCroskey & McCroskey, 1988). Second, communicators can be judged credible by colleagues and supervisors, who assess competence, trustworthiness and goodwill (Hovland & Weiss, 1951; McCroskey & Teven, 1999). Confidence is a private state and credibility is a social judgement, and influence should follow the second more directly than the first. Research on self-efficacy tends to stop at the performer (Stajkovic & Luthans, 1998), whereas research on source credibility tends to treat credibility as a property of the source and rarely asks how it is built through development (Pornpitakpan, 2004). Table 2 summarises what each stream establishes and leaves open. We are not aware of studies that test the sequence from skill-development to confidence to credibility to influence, and evidence from African workplaces on any part of the sequence is scarce.

This article makes three contributions. Theoretically, it proposes a serial mediation framework and a credibility conversion principle: confidence affects influence mainly to the extent that it is converted into others' judgements of credibility. Methodologically, it specifies a multi-source, three-wave protocol for Kenyan formal-sector organisations that separates self-reported from other-reported constructs and models measurement error, and it validates the

planned estimator by Monte Carlo simulation before data are collected, using published meta-analytic effect sizes as anchors. Practically, it derives sample-size rules and evaluation indicators that training providers can use. The objectives are (1) to specify and justify the framework and hypotheses; (2) to evaluate the statistical properties of the planned analysis, including power, bias, coverage and Type I error; and (3) to derive design guidance for the primary study.

THEORETICAL BACKGROUND AND HYPOTHESES

Skill-development and communication confidence

Social cognitive theory holds that self-efficacy beliefs are built chiefly from mastery experiences, vicarious learning, social persuasion and physiological states (Bandura, 1977, 1997). Skill-development activities supply these sources: rehearsed performance with feedback, observation of capable models, and encouragement. Gist and Mitchell (1992) argued that training is a principal means of changing efficacy beliefs, and experimental work showed that modelling-based training raised self-efficacy and later performance (Gist et al., 1989). Communication confidence is defined here as the belief in one's capability to communicate effectively across the persons and contexts that work presents. It is distinct from communication apprehension, the anxiety associated with real or anticipated communication (McCroskey, 1977), and from skill itself, because people with similar skills hold different beliefs about them. The link between efficacy and performance is substantial: Stajkovic and Luthans (1998) reported a weighted mean correlation of .38 between self-efficacy and work-related performance. Together with the learning effect of training (Arthur et al., 2003), these findings imply the first hypothesis.

H1. Communication skill-development is positively related to communication confidence ($\alpha_1 > 0$).

Table 1. Constructs, definitions and theoretical anchors

Construct	Definition	Anchors
Skill-development (X)	Deliberate activities that build communication knowledge and practice	Bandura (1977); Arthur et al. (2003)
Confidence (M1)	Belief in one's capability to communicate effectively at work	Bandura (1997); McCroskey & McCroskey (1988)
Credibility (M2)	Others' judgement of competence, trustworthiness and goodwill	Hovland & Weiss (1951); McCroskey & Teven (1999); Mayer et al. (1995)
Influence (Y)	Capacity to shape others' decisions, effort and resources	French & Raven (1959); Brass (1984); Yukl et al. (2008)

Source: Compiled by the author from the works cited.

Confidence and professional credibility

Professional credibility is the degree to which others regard a person as competent, trustworthy and well-intentioned. Classical source-credibility research located the persuasive power of a communicator in perceived expertise and trustworthiness (Hovland & Weiss, 1951; Pornpitakpan, 2004), McCroskey and Teven (1999) added goodwill, and trust research converges on ability, benevolence and integrity as the antecedents of trustworthiness (Mayer et al., 1995; Colquitt et al., 2007). Social perception research likewise reduces judgements of persons to warmth and competence (Fiske et al., 2007). Credibility is therefore attributed by observers and is not possessed by the speaker. Confidence can shape those attributions through observable delivery: confident communicators speak more readily, contribute with composure and volunteer information, all of which supply cues that observers read as competence. Confidence without substance can also be penalised, which is why the framework retains a direct path from skill to credibility.

H2. Communication confidence is positively related to professional credibility ($d_{21} > 0$).

H3. Communication skill-development is positively related to professional credibility, over and above confidence ($a_2 > 0$).

Credibility and workplace influence

Workplace influence is the capacity to shape the decisions, effort and allocation of resources of others. Social power research identifies bases such as expertise, reference and legitimate power (French & Raven, 1959), and the influence-tactics tradition catalogues the behaviours through which influence attempts are made and the conditions under which they succeed (Kipnis et al., 1980; Yukl & Falbe, 1990; Yukl et al., 2008). A structural tradition shows that influence is also positional: individuals sought out by many

colleagues hold influence in the organisation's communication network (Brass, 1984). Compliance research adds that people are more readily influenced by sources they perceive as credible (Cialdini & Goldstein, 2004), and political skill, the ability to understand others at work and use that knowledge to influence them, is a neighbouring construct that predicts workplace outcomes (Ferris et al., 2005). The credibility conversion principle follows: because influence is conferred by others, the effect of a private resource such as confidence should pass chiefly through the judgements of others.

H4. Professional credibility is positively related to workplace influence ($b_2 > 0$).

H5. Communication skill-development has a positive serial indirect effect on workplace influence through confidence and credibility ($a_1 \times d_{21} \times b_2 > 0$).

H6. The direct effect of confidence on influence, net of credibility, is not larger than its indirect effect through credibility ($b_1 \leq d_{21} \times b_2$).

Rival explanations and design implications

Four rival explanations constrain the design. Reverse causation is plausible, because successful influence can itself raise efficacy through mastery experience. Selection is plausible, because confident employees may seek out training. A common cause such as trait extraversion could raise confidence, credibility and influence together. Finally, if every construct were self-reported, shared method variance would inflate the associations (Podsakoff et al., 2012). The protocol responds with temporal ordering across three waves, which avoids the bias that cross-sectional tests of mediation can produce (Maxwell & Cole, 2007), with baseline confidence and extraversion (Donnellan et al., 2006) as controls, and with other-rated measurement of credibility and influence.

Table 2. Evidence map: what the literatures establish and leave open

Stream	Established	Open question
Training	Changes learning and behaviour on average (Arthur et al., 2003)	Effects on others' judgements (Blume et al., 2010)
Self-efficacy	Associated with work performance, $r = .38$ (Stajkovic & Luthans, 1998)	Whether efficacy yields influence via observers
Source credibility	Expertise, trustworthiness and goodwill drive persuasion (Pornpitakpan, 2004; McCroskey & Teven, 1999)	How credibility is built through development

Stream	Established	Open question
Trust	Ability, benevolence and integrity underpin trustworthiness (Mayer et al., 1995; Colquitt et al., 2007)	Link to the capacity to influence
Influence	Tactics and network position shape influence (Yukl & Falbe, 1990; Brass, 1984)	Antecedents in communication development

Source: Compiled by the author from the works cited.

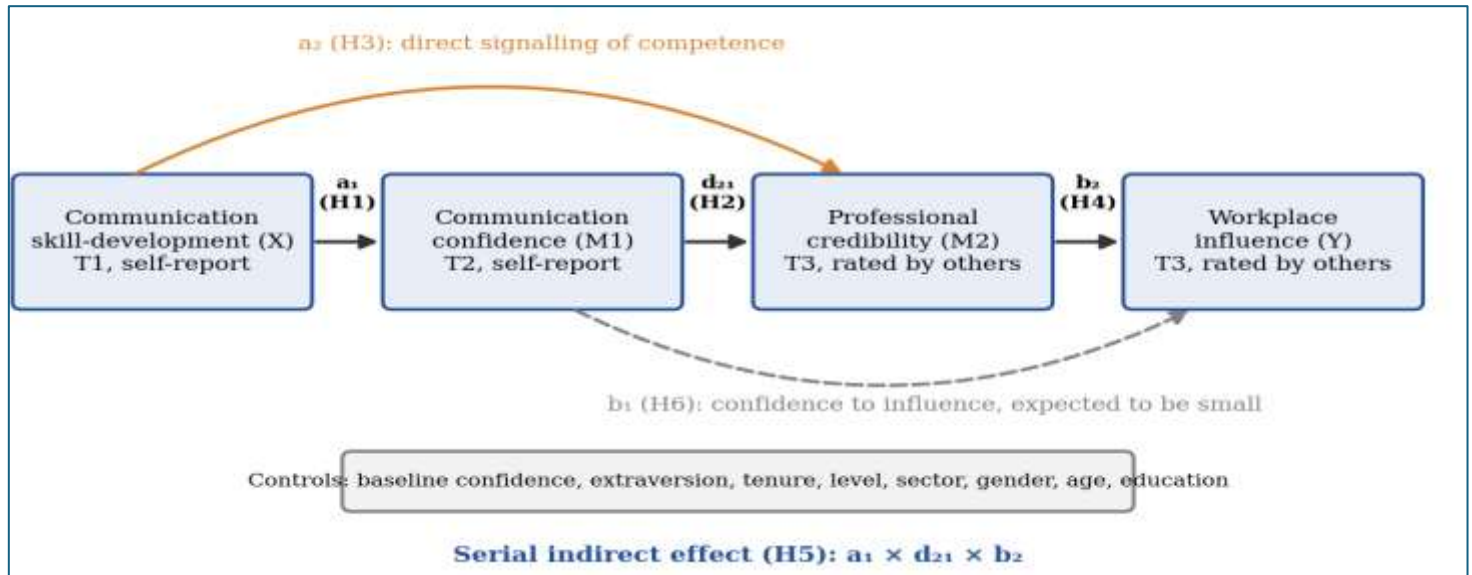


Figure 1. Conceptual model: serial mediation from skill-development to workplace influence

Source: Authors' design, based on Bandura (1977, 1997), McCroskey and Teven (1999) and French and Raven (1959). Dashed arrow: direct path expected to be small (H6).

Table 3. Hypotheses and statistical tests

Hypothesis	Path or effect	Decision rule
H1	$a_1: X \rightarrow M1$	$a_1 > 0$, 95% CI excludes zero
H2	$d_{21}: M1 \rightarrow M2$	$d_{21} > 0$, 95% CI excludes zero
H3	$a_2: X \rightarrow M2 \mid M1$	$a_2 > 0$, 95% CI excludes zero
H4	$b_2: M2 \rightarrow Y$	$b_2 > 0$, 95% CI excludes zero
H5	$a_1 \times d_{21} \times b_2$	Bootstrap 95% CI excludes zero
H6	b_1 vs $d_{21} \times b_2$	CI of the difference includes or exceeds zero

Source: Authors' analysis plan.

Boundary conditions (propositions for later work)

Three propositions mark where the framework may not hold, and they are not tested here. First, the conversion of confidence into credibility should weaken when confidence exceeds demonstrated competence, because overconfidence is penalised once observers detect it (Moore & Healy, 2008). Second, observers should lean more on delivery cues when they have little other information about a person, so the conversion should be stronger in newly formed

teams. Third, credibility should convert into influence more strongly where decisions are discretionary and weakly where rules prescribe outcomes. These propositions identify moderators for the multilevel extension discussed below.

MATERIALS AND METHODS

Research design

The study follows a two-component design. The first component is a pre-specified protocol for a primary

study of employees in Kenyan formal-sector organisations. The second is a Monte Carlo validation of the protocol's estimator. A validation study is appropriate at this stage because the main design decisions, namely sample size, number of waves, raters, measurement model and the test of indirect effects, interact, and the cost of a wrong decision is borne after data have been collected. Simulation

allows these decisions to be evaluated against a known truth (Fritz & MacKinnon, 2007; Schoemann et al., 2017). The validation uses no human participants. The primary study will be fielded only after institutional ethics approval and a research licence from NACOSTI have been obtained, and its hypotheses, measures and analysis plan will be pre-registered. Figure 2 summarises the sequence.

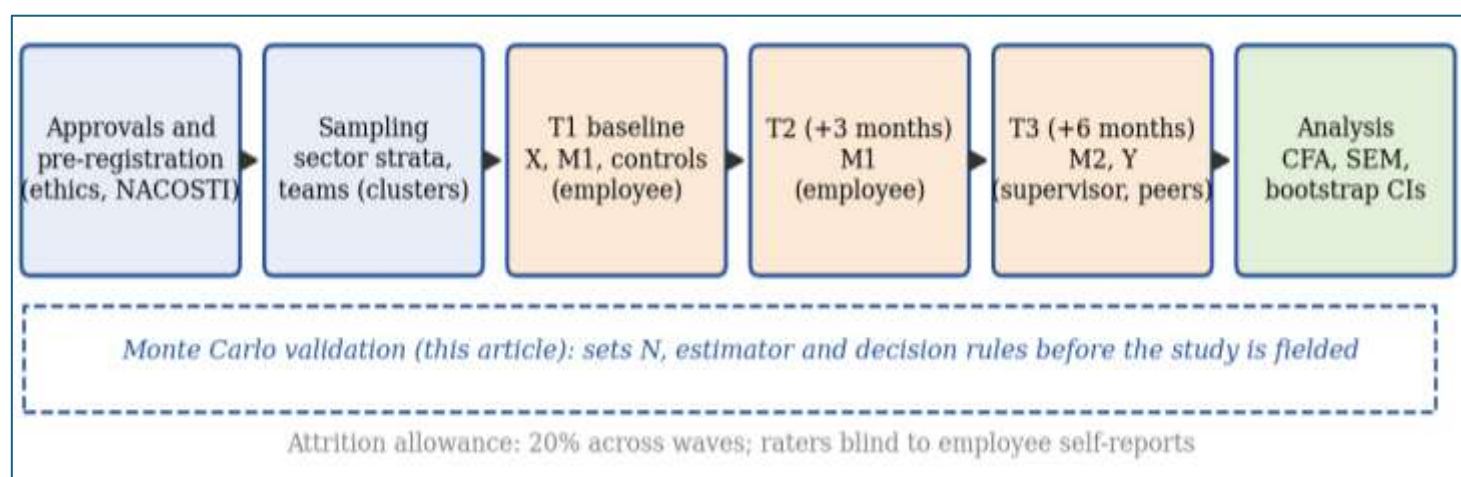


Figure 2. Research protocol and role of the Monte Carlo validation

Source: Authors' design. T1 to T3 are measurement waves; M1, M2 and Y are defined in Table 1.

Population, sampling and raters (protocol)

The target population is employees with at least 12 months of tenure in formal-sector organisations in Nairobi City County. Organisations are stratified by sector (for example banking, higher education, public service and manufacturing), and employees are sampled within teams, which serve as clusters. At T3 each focal employee's credibility is rated by two randomly assigned peers, and influence is rated by the immediate supervisor and measured by the employee's advice-network in-degree among the remaining peers, so that credibility and influence share no rater and neither depends on the employee's self-view. Because raters are shared within teams, observations are not independent. The design effect is $1 + (m - 1) \times ICC$ (Kish, 1965); with $m = 6$ respondents per team and a plausible ICC of .10 for rater-based variables, the design effect is 1.5, and 600 respondents in 100 teams carry the information of about 400 independent respondents. Cluster-robust standard errors will be used.

Measures

Table 4 lists the measures. Skill-development (X) is an index of structured training hours, deliberate practice

and developmental feedback in the previous 12 months. Confidence (M1) uses the Self-Perceived Communication Competence scale (McCroskey & McCroskey, 1988), which asks respondents to rate their competence across persons and contexts. Credibility (M2) uses the competence, goodwill and trustworthiness measures of McCroskey and Teven (1999), completed by two peers and averaged before modelling. Influence (Y) combines supervisor-rated influence outcomes adapted from the extended Influence Behavior Questionnaire (Yukl et al., 2008) and the employee's in-degree in a peer advice network, a structural indicator of influence (Brass, 1984). Controls are baseline confidence, extraversion measured with the Mini-IPIP (Donnellan et al., 2006), tenure, organisational level, sector, gender, age and education. Reliability will be reported as McDonald's omega, which is preferred to Cronbach's alpha when items are not tau-equivalent (Hayes & Coutts, 2020).

Instrument adaptation and pilot (protocol)

All instruments will be administered in English, and the Kiswahili version offered to participants who prefer it will be produced by independent forward translation and back-translation (Brislin, 1970).

Cognitive interviews with eight employees will check that items are read as intended (Willis, 2005). A pilot of about 60 employees in 12 teams outside the main sampling frame will estimate omega for each scale, item-total correlations, completion time and the ICCs

used to update the design effect. Items with corrected item-total correlations below .30 or that reduce omega when deleted will be reviewed, and decisions will be documented before the main waves begin.

Table 4. Measures, raters and waves in the primary-study protocol

Construct	Instrument and source	Rater	Wave	Items	Reliability target
Skill-development (X)	Training hours (log), deliberate practice, developmental feedback; developed for this study	Employee	T1	8	$\omega \geq .80$
Confidence (M1)	Self-Perceived Communication Competence (McCroskey & McCroskey, 1988)	Employee	T1, T2	12	$\omega \geq .80$
Credibility (M2)	Competence, goodwill, trustworthiness (McCroskey & Teven, 1999)	Two peers	T3	18	$\omega \geq .80$
Influence (Y)	Influence outcomes adapted from Yukl et al. (2008); advice-network in-degree (Brass, 1984)	Supervisor, other peers	T3	4 + network	$\omega \geq .80$
Extraversion (control)	Mini-IPIP (Donnellan et al., 2006)	Employee	T1	4	$\omega \geq .70$

Source: Compiled by the author from the instruments cited. Item counts follow the published instruments except for the study-specific index.

Statistical analysis plan (protocol)

The primary model is a latent-variable structural equation model estimated with the lavaan package in R (Rosseel, 2012), with maximum likelihood and robust standard errors. The measurement model will be evaluated first. Fit is judged with the combined criteria of Hu and Bentler (1999), namely $CFI \geq .95$, $RMSEA \leq .06$ and $SRMR \leq .08$, and convergent and discriminant validity with the procedure of Fornell and Larcker (1981). Measurement invariance across sectors will be tested sequentially, and a decline in CFI of no more than .01 will be taken to support invariance (Cheung & Rensvold, 2002). Specific indirect effects and the serial effect are estimated with 5,000 percentile bootstrap resamples (Preacher & Hayes, 2008; Shrout & Bolger, 2002). Missing data are handled by full-information maximum likelihood. A sensitivity analysis fits the same paths to observed composite scores with the PROCESS procedure for serial mediation (Hayes, 2022), which is the estimator evaluated in the simulation below.

Measurement model and identification (protocol)

Each latent variable has at least three indicators, so that every measurement equation is identified. Skill-

development (X) is indicated by training hours (log), deliberate practice and developmental feedback. Confidence (M1) is indicated by four parcels of the 12 situations of the Self-Perceived Communication Competence scale, one for each context (public, meeting, group and dyad), which follows the structure of the instrument (McCroskey & McCroskey, 1988). Credibility (M2) is indicated by competence, goodwill and trustworthiness. Influence (Y) is indicated by supervisor-rated influence outcomes, an influence composite from the remaining peers and the log of advice-network in-degree. The first loading of each factor is fixed to one, and residual covariances are freed only between indicators that share a rater or an item stem. The structural model is given by three equations, with $M1 = \alpha_1 + a_1X + e_1$, $M2 = \alpha_2 + a_2X + d_{21}M1 + e_2$ and $Y = \alpha_3 + c'X + b_1M1 + b_2M2 + e_3$, and the serial indirect effect is the product $a_1 \times d_{21} \times b_2$.

Missing data, attrition and clustering

Attrition is the main threat across three waves, and a 20% loss is assumed. Attrition will be modelled by comparing completers and non-completers on baseline variables, and the model will be estimated by full-information maximum likelihood, which is valid when missingness depends on observed variables (Enders,

2010). Inverse-probability weights provide a sensitivity check. Clustering is handled with cluster-robust standard errors and by reporting ICCs for each latent variable; if an ICC exceeds .10 the structural model will be re-estimated as a two-level model, and the design effect used for planning will be updated from the pilot.

Alternative orderings and sensitivity analyses

A serial model with a fixed order has equivalent alternatives that fit the data identically (MacCallum et al., 1993). Fit therefore cannot arbitrate between the proposed order and, for example, credibility feeding back into confidence. The protocol relies on temporal precedence, theory and the pre-specified sensitivity analyses in Table 5, and it will report the reversed order as a rival model without claiming that fit favours either.

Ethical considerations and data protection

The primary study involves employees and raters, so informed consent will be obtained from every participant, and raters will be told that their ratings are confidential and will not be shown to the employee, the supervisor or the organisation. Research authorisation will be sought from NACOSTI under the Science, Technology and Innovation Act (Republic of Kenya, 2013). Personal data will be processed in line with the Data Protection Act (Republic of Kenya, 2019), with registration with the Office of the Data Protection Commissioner where required, pseudonymised identifiers, encrypted storage and a stated retention period. Reporting will be in aggregate, and teams with fewer than five members will not be reported separately.

Monte Carlo validation

Standardised latent variables X, M1, M2 and Y were generated recursively from the path model in Figure 1,

with residual variances set so that every latent variable has unit variance. Observed composites were formed as $\sqrt{\rho} \times \text{latent} + \sqrt{(1 - \rho)} \times \text{error}$, where ρ is composite reliability. Each composite was analysed by ordinary least squares in three equations (M1 on X; M2 on X and M1; Y on X, M1 and M2), and the three specific indirect effects, $X \rightarrow M1 \rightarrow Y$, $X \rightarrow M2 \rightarrow Y$ and the serial effect $X \rightarrow M1 \rightarrow M2 \rightarrow Y$, were tested with Monte Carlo confidence intervals based on 2,000 draws from the sampling distribution of each coefficient (Preacher & Selig, 2012). Table 8 gives the algorithm, and two path scenarios were used (Table 6). Sample size varied over 150, 200, 300, 400, 600, 800, 1,000 and 1,500, and reliability over 1.00, .90, .80 and .70, with 1,200 replications per cell and 3,000 replications for each Type I error cell. Performance criteria were power (the proportion of samples in which the interval excluded zero), relative bias, coverage of the 95% interval for the attenuated population parameter, and the false-positive rate when the serial effect was zero. In total, 85,800 samples were generated. The script, seeds and output are available from the author.

Scenario parameters were anchored on published evidence where it exists (Table 7). Standardised paths of .35 and .25 for a_1 correspond to standardised mean differences of about 0.75 and 0.52, bracketing the learning effect of training reported by Arthur et al. (2003). No meta-analytic anchor exists for d_{21} or b_2 , so these were set at moderate and small values in line with the conventions of Cohen (1992): S1 uses moderate paths and S2 uses small-to-moderate paths. The implied correlations between confidence and influence are .30 in S1 and .18 in S2, at or below the correlation of .38 reported for self-efficacy and performance (Stajkovic & Luthans, 1998).

Table 5. Pre-specified sensitivity and robustness analyses

Analysis	Purpose	Criterion
Reversed order (M2 before M1)	Show that conclusions do not depend on the assumed order	Report both; no claim from fit alone
Observed composites (PROCESS) versus latent model	Quantify attenuation from measurement error	Compare indirect effects and intervals
Excluding controls other than baseline confidence	Check sensitivity to control specification	Sign and significance unchanged
Dropping one sector at a time	Check dependence on a single sector	Range of serial estimates

Analysis	Purpose	Criterion
Inverse-probability weights for attrition	Check bias from non-random dropout	Estimates within one standard error
Single rater type for influence	Check rater-specific method effects	Same direction of the serial effect

Source: Authors' analysis plan.

Table 6. Simulation scenarios (standardised path coefficients)

Path	S1	S2	Interpretation
$a_1: X \rightarrow M1$	0.35	0.25	Training effect on confidence
$a_2: X \rightarrow M2 \mid M1$	0.15	0.10	Direct signalling of competence
$d_{21}: M1 \rightarrow M2$	0.35	0.25	Confidence converted into credibility
$b_1: M1 \rightarrow Y \mid M2$	0.10	0.08	Direct confidence effect (small)
$b_2: M2 \rightarrow Y$	0.40	0.30	Credibility conversion into influence
$c': X \rightarrow Y \mid M1, M2$	0.10	0.08	Residual skill effect
Serial effect $a_1 d_{21} b_2$	0.049	0.0187	Product of three paths

Source: Authors' simulation design. Scenario S1 uses moderate paths and S2 small-to-moderate paths (Cohen, 1992).

Table 7. Published effect-size anchors used to calibrate the simulation

Study	Estimate	Relationship	Use in this article
Arthur et al. (2003)	$d = 0.60, 0.63, 0.62, 0.62$	Training and reactions, learning, behaviour, results	Calibrates a_1 (learning effect)
Stajkovic & Luthans (1998)	$r = .38$ (weighted mean)	Self-efficacy and work-related performance	Upper bound for the implied confidence–influence correlation
Cohen (1992)	$r = .10, .30, .50$	Small, medium and large effects	Sets scenario magnitudes

Source: Arthur et al. (2003, Journal of Applied Psychology, 88(2), 234–245); Stajkovic and Luthans (1998, Psychological Bulletin, 124(2), 240–261); Cohen (1992, Psychological Bulletin, 112(1), 155–159).

Table 8. Algorithm of the Monte Carlo validation

Step	Operation
1	Set the path coefficients (Table 6), composite reliability ρ and sample size N .
2	Generate X from a standard normal distribution and then $M1$, $M2$ and Y from the structural equations, with residual variances set so that each latent variable has unit variance.
3	Add random measurement error to form observed composites with reliability ρ .
4	Estimate the three regression equations by ordinary least squares.
5	Draw 2,000 values of each coefficient from its estimated normal sampling distribution and form the products defining the three indirect effects.
6	Record whether the 95% percentile interval excludes zero and whether it covers the attenuated population value.
7	Repeat 1,200 times (3,000 times in Type I error cells) and summarise power, bias and coverage; the random seed is fixed at 20261009.

Source: Authors' implementation of the procedure described by Preacher and Selig (2012) and Schoemann et al. (2017).

RESULTS AND DISCUSSION

Published effect-size anchors

Figure 3 displays the published anchors on a common metric. The four training effects reported by Arthur et al. (2003) cluster tightly around $d = 0.60$ to 0.63 , which implies a standardised association of about $r = .29$ to $.30$ between exposure to training and learning. The self-efficacy and performance correlation of $.38$ (Stajkovic & Luthans, 1998) corresponds to $d = 0.82$. Both bodies of evidence come from largely North American and European samples, and their transfer to Kenyan workplaces is an empirical question that the primary study is designed to address.

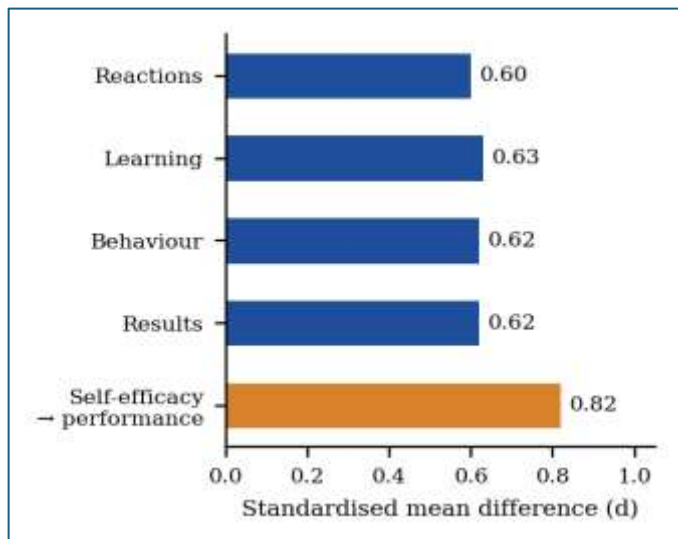


Figure 3. Published effect-size anchors

Source: Arthur et al. (2003) for training effects; Stajkovic and Luthans (1998) for self-efficacy and

performance ($r = .38$ converted to $d = 2r/\sqrt{1 - r^2} = 0.82$).

Power of the indirect-effect tests

Table 9 reports power at a composite reliability of $.80$. In S1 the serial test exceeded the 80% criterion at 150 respondents ($.84$) and was close to certain by 300. In the conservative S2 it reached $.57$ at 200 respondents and $.88$ at 300. The indirect effects routed through a weak direct path behaved very differently. The test of $X \rightarrow M1 \rightarrow Y$, which depends on the small path b_1 , reached $.80$ only at about 680 respondents in S1 and 1,320 in S2 (Table 10). The test of $X \rightarrow M2 \rightarrow Y$, which depends on the small path a_2 , needed 390 and 970 respondents.

This is a weakest-link property. The power of a product test is limited by the least precisely estimated path, so the serial effect, with three moderate paths, can be detected readily even though its size as a product is small (0.049 in S1 and 0.019 in S2). The practical implication is that sample-size planning must be done path by path and not from the size of the product alone. Figure 4 shows the full curves, and Figure 5 shows how reliability and sample size jointly determine power for the serial effect in S2.

Table 9. Power of the three indirect-effect tests at composite reliability .80

Respondents	S1: $X \rightarrow M1 \rightarrow Y$	S1: $X \rightarrow M2 \rightarrow Y$	S1: serial	S2: $X \rightarrow M1 \rightarrow Y$	S2: $X \rightarrow M2 \rightarrow Y$	S2: serial
150	.26	.38	.84	.11	.13	.33
200	.32	.51	.94	.14	.22	.57
300	.45	.70	1.00	.25	.35	.88
400	.57	.81	1.00	.33	.44	.96
600	.75	.93	1.00	.46	.60	.99
800	.87	.98	1.00	.63	.74	1.00
1,000	.93	.99	1.00	.71	.81	1.00
1,500	.98	1.00	1.00	.85	.95	1.00

Source: Monte Carlo simulation by the author; 1,200 replications per cell; Monte Carlo 95% intervals with 2,000 draws.

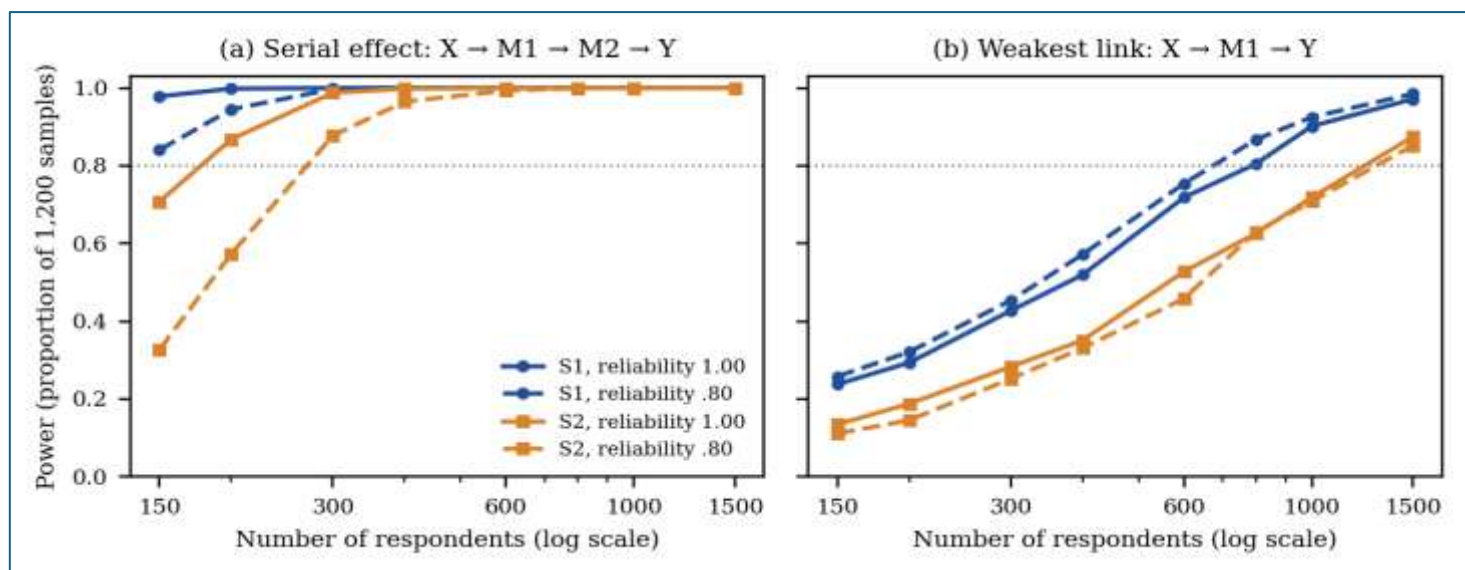


Figure 4. Power curves for the serial effect (a) and the weakest-link effect (b)

Source: Monte Carlo simulation by the author. S1 = moderate paths, S2 = small-to-moderate paths (Table 6); dotted line = .80.

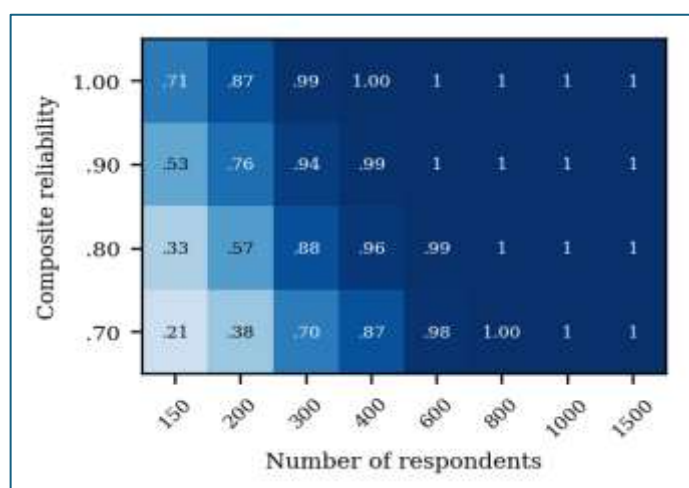


Figure 5. Power of the serial effect in S2 by sample size and reliability

Source: Monte Carlo simulation by the author (1,200 replications per cell).

Table 10 converts the curves into minimum sample sizes for 80% power. For the confirmatory test of H5,

180 to 360 independent respondents suffice in S2 across reliabilities from 1.00 to .70, and no more than 190 in S1. Applying the design effect of 1.5 and a 20% allowance for attrition across three waves, the S2 requirement at reliability .80 (270 respondents) implies about 510 initial respondents. The protocol therefore plans 600 respondents in 100 teams, which leaves a margin for lower reliability. Tests of H3 and of the $X \rightarrow M1 \rightarrow Y$ path would require 770 to 1,380 independent respondents in S2 and are declared exploratory.

Path-level tests tell the same story (Table 11). The paths a_1 , d_{21} and b_2 , which underlie H1, H2 and H4, reached 80% power at no more than 100 to 210 respondents at reliability .80 in both scenarios, whereas a_2 (H3) needed 400 respondents in S1 and 980 in S2, and b_1 , which bears on H6, needed 680 and 1,340. H1, H2 and H4 are therefore confirmatory and H3 and H6 exploratory unless the sample is much larger than planned (Figure 6).

Table 10. Respondents needed for 80% power, by scenario, reliability and effect

Reliability	S1: $X \rightarrow M1 \rightarrow Y$	S1: $X \rightarrow M2 \rightarrow Y$	S1: serial	S2: $X \rightarrow M1 \rightarrow Y$	S2: $X \rightarrow M2 \rightarrow Y$	S2: serial
1.00	790	320	≤ 150	1,260	770	180
.90	700	350	≤ 150	1,170	870	220
.80	680	390	≤ 150	1,320	970	270
.70	730	470	190	1,380	1,180	360

Source: Linear interpolation of the simulated power curves (Monte Carlo simulation by the author). ≤ 150 indicates that power was already at least .80 at the smallest simulated sample.

Table 11. Respondents needed for 80% power for each path, by scenario and reliability

Scenario, reliability	a_1 (H1)	d_{21} (H2)	a_2 (H3)	b_2 (H4)	b_1 (H6)
S1, 1.00	≤ 100	≤ 100	340	≤ 100	770
S1, .80	≤ 100	≤ 100	400	≤ 100	680
S2, 1.00	120	130	790	≤ 100	1,280
S2, .80	200	210	980	140	1,340

Source: Monte Carlo simulation by the author (1,000 replications per cell; two-sided t tests at .05; ≤ 100 means power was at least .80 at the smallest simulated sample).

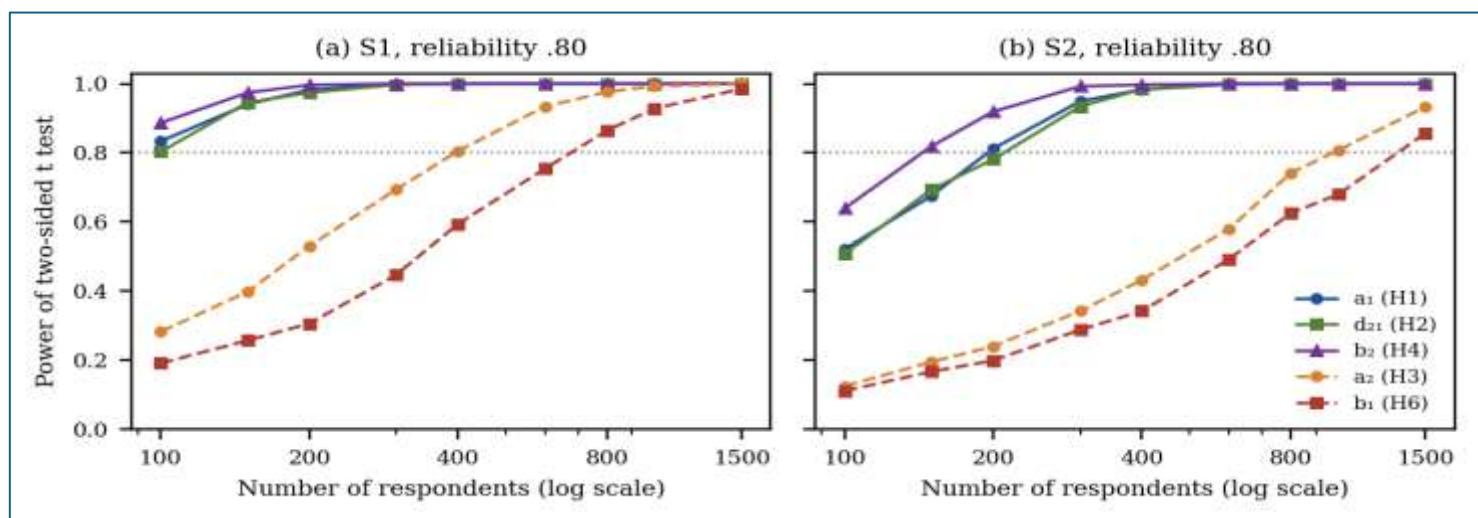


Figure 6. Power curves for the individual paths

Source: Monte Carlo simulation by the author (1,000 replications per cell). Dotted line = .80; a_2 and b_1 are the small paths (Table 6).

Measurement error, bias and coverage

Measurement error was the dominant source of bias. At a reliability of .80 the mean estimate of the serial effect was 0.025 in S1 against a true latent value of 0.049, a relative bias of -49% (Table 12, Figure 8a). Attenuation was practically identical in the two scenarios (-27% at .90, -49% at .80 and -65% at .70), because each coefficient in the chain loses about the same fraction of its size. Coverage of the Monte Carlo interval for the attenuated parameter ranged from 0.931 to 0.963 (mean 0.949; Figure 8b), so the interval is trustworthy for what a composite-score analysis estimates, though not for the latent effect.

When the serial effect was truly zero ($d_{21} = 0$), the serial test rejected in 6.3%, 5.8% and 6.6% of samples at 300, 600 and 1,000 respondents. With 3,000

replications the Monte Carlo standard error is about .004, so this is a small but real excess over the nominal .05. It argues for percentile bootstrap intervals, as planned for the primary analysis, in preference to the Monte Carlo intervals used here for speed.

Reliability is unlikely to be equal across constructs in practice, because other-rated measures are usually less reliable than self-reports. An additional run therefore set reliability at .85 for skill-development and confidence, .75 for credibility and .70 for influence (Table 13). Power for the serial effect was similar to that at a uniform reliability of .80 (.80 at 150 respondents in S1 and .84 at 300 in S2), and the relative bias was -52% in S1 and -51% in S2. Coverage of the attenuated parameter stayed between 0.935 and 0.967. Unequal reliabilities therefore change the planning figures little but do not remove the need to model measurement error.

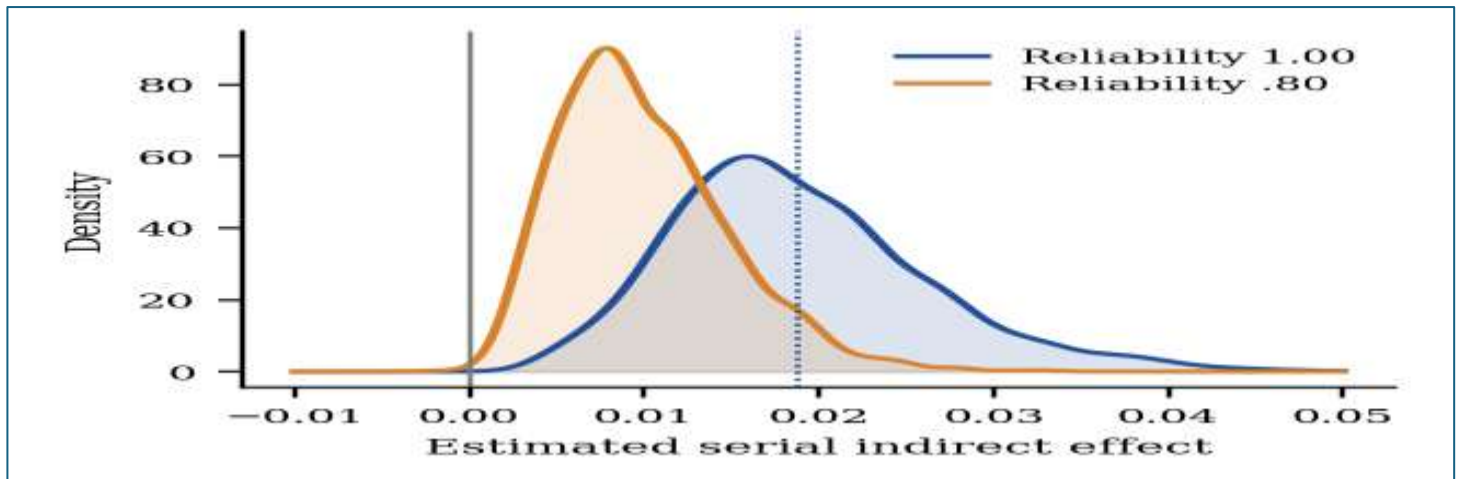


Figure 7. Sampling distribution of the serial

estimate (S2, N = 300)

Source: Monte Carlo simulation by the author (3,000 samples per curve); dotted line = true latent value.

Figure 7 shows how attenuation and sampling error combine. At 300 respondents in S2, estimates of the serial effect centred on 0.0188 (SD 0.0074) at perfect

reliability and on 0.0099 (SD 0.0048) at reliability .80. The attenuated distribution lies close to zero, and in practice an analyst would then describe a genuine effect as marginal. Reporting intervals and reliability together, and not significance alone, is therefore essential.

Table 12. Bias and coverage of the serial indirect effect

Reliability	Mean estimate, S1	Mean estimate, S2	Relative bias, S1 and S2	Coverage range	Mean coverage
1.00	0.049	0.019	-1% and -1%	0.931–0.958	0.949
.90	0.036	0.014	-27% and -27%	0.937–0.961	0.949
.80	0.025	0.010	-49% and -49%	0.934–0.960	0.949
.70	0.017	0.007	-65% and -64%	0.936–0.963	0.950
True latent value	0.049	0.0187	0%	—	—

Source: Monte Carlo simulation by the author. Estimates at N = 600; coverage is of the attenuated population parameter across all sample sizes and the three indirect effects.

Table 13. Power with unequal reliabilities (X .85, M1 .85, M2 .75, Y .70)

Respondents	S1: X→M1→Y	S1: serial	S2: X→M1→Y	S2: serial
150	.27	.80	.10	.34
200	.33	.96	.16	.56
300	.49	1.00	.25	.84
400	.60	1.00	.35	.95
600	.74	1.00	.46	1.00
800	.86	1.00	.61	1.00
1,000	.93	1.00	.69	1.00

Source: Monte Carlo simulation by the author (1,200 replications per cell; Monte Carlo 95% intervals).

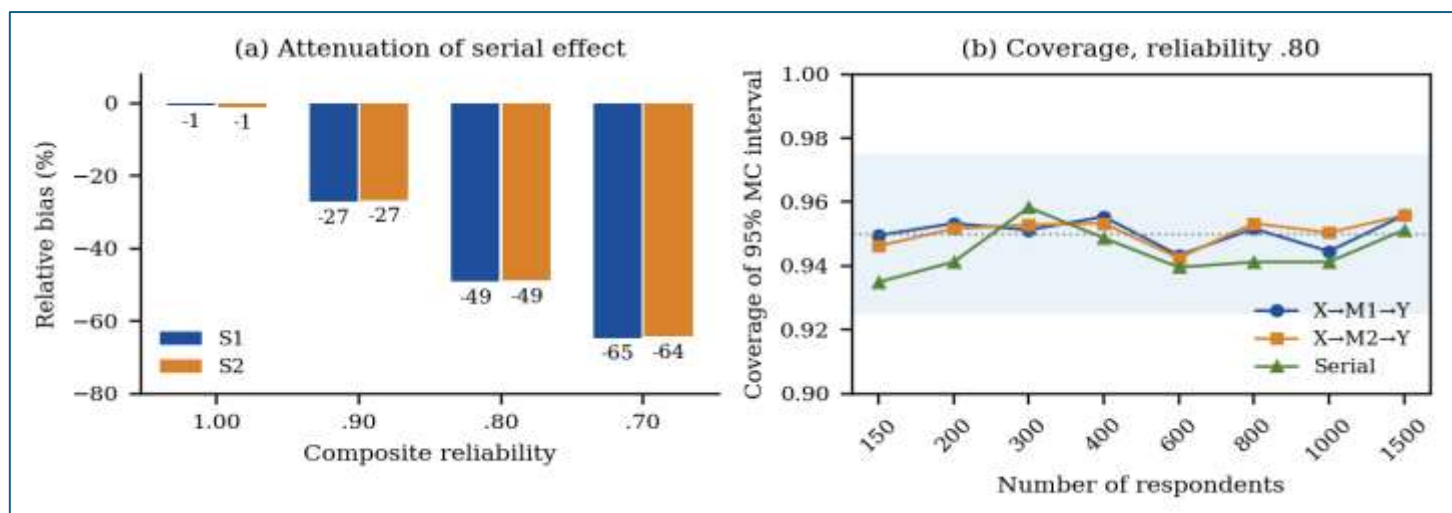


Figure 8. Attenuation and coverage

Source: Monte Carlo simulation by the author. Panel (a): relative bias of the serial effect at $N = 600$. Panel (b): coverage averaged over scenarios at reliability .80; shaded band = .925 to .975.

Summary of results

Four results stand out. The serial effect was detectable with a few hundred respondents, because each of its paths was individually large enough. Paths that were small, notably the direct path from skill-development to credibility and from confidence to influence, needed many hundreds. Measurement error at reliability .80 halved every indirect effect while leaving intervals correctly calibrated for the attenuated quantity. And the false-positive rate for the serial test was slightly above nominal, which favours bootstrap intervals in the primary analysis.

Implications for theory

The results support three methodological points that bear on theory testing. First, the credibility conversion principle is testable only with designs in which credibility and influence are rated by others, because self-reported credibility would confound the mediator with the actor's self-view (Podsakoff et al., 2012). Second, because power is governed by the weakest path, a null finding for a mediation chain with one small link may reflect insufficient power and not absence of an effect. Third, because reliability of .80 halves indirect effects, studies that analyse composite scores understate mediation, and latent-variable or reliability-corrected estimators are required if the size of the process matters. These points qualify, but do not test, the substantive propositions of the framework.

Relation to earlier work on mediation power

The weakest-link pattern is consistent with the general finding that mediated effects are hard to detect when one component path is small, and that required samples fall sharply when both paths are medium or large (Fritz & MacKinnon, 2007; Schoemann et al., 2017). The present results extend that finding to a three-path chain and show that the problem compounds with measurement error. They also caution against interpreting the absence of a significant direct path from confidence to influence as support for H6: in S1 the direct path was set to .10 and still needed about 680 respondents to detect.

Positioning against neighbouring constructs

Political skill (Ferris et al., 2005) overlaps with the framework because it joins social perceptiveness with the ability to appear sincere. The framework differs in treating credibility as a judgement made by others and as a mediator that can be developed, and in separating private confidence from public credibility. Trust research (Mayer et al., 1995; Colquitt et al., 2007) supplies the dimensions of credibility but is usually organised around a trustor's decision to take risk, whereas influence here is a broader capacity to move decisions. These distinctions are conceptual, and discriminant validity between credibility, trust and political skill is a measurement question for the primary study.

Implications for practice

If the framework holds, programme evaluation should track what skill-development produces in the minds of others and not only what it produces in the trainee.

Table 14 sets out candidate indicators. They are hypotheses for the primary study and are not findings, and Table 16 sets the reporting standards that should accompany them. We also do not assume that the sequence is uniform across organisations; hierarchical or multilingual workplaces may alter how confidence is read by observers, which is a boundary condition for

later work. Training providers in Kenyan organisations can use the planning figures immediately: an evaluation of a single programme with fewer than about 200 trainees is unlikely to detect indirect effects that pass through a small path, and any evaluation that relies on self-report alone cannot separate confidence from credibility.

Table 14. Candidate evaluation indicators implied by the framework

Programme stage	Indicator	Instrument
Immediately after training	Change in communication confidence	Self-Perceived Communication Competence (McCroskey & McCroskey, 1988)
After 3 months	Credibility as seen by supervisor and peers	Source credibility measures (McCroskey & Teven, 1999)
After 6 months	Influence outcomes and advice-network position	Influence outcomes (Yukl et al., 2008); in-degree (Brass, 1984)
Throughout	Reliability of each indicator and rater agreement	McDonald's omega (Hayes & Coutts, 2020); ICC

Source: Authors' synthesis.

Table 15. Planning examples for a training evaluation (S2, reliability .80)

Respondents enrolled	Teams of 6	Effective N after attrition and clustering	Power: serial effect	Power: $X \rightarrow M2 \rightarrow Y$	Power: $X \rightarrow M1 \rightarrow Y$
300	50	160	.38	.15	.12
450	75	240	.69	.27	.18
600	100	320	.90	.37	.27
900	150	480	.97	.50	.38

Source: Computed by the author by linear interpolation of the S2 power results at reliability .80 (Table 9); effective N = enrolled $\times$ 0.80 $\div$ 1.5.

Table 16. Reporting standards for the primary study

Element	Standard	Rationale
Pre-registration	Hypotheses, measures, N and analysis plan registered before T1	Limits flexibility in analysis
Reliability	McDonald's omega for every scale and rater type	Attenuation halves indirect effects at .80
Effects	Path coefficients, indirect effects and intervals, not p values alone	Weak-link paths need wide intervals
Power	Path-by-path, not from the product (Tables 10 and 11)	Weakest-link property
Raters	Number per target, assignment rule and agreement	Separates credibility from influence
Attrition	Loss by wave, comparison of completers, weighted sensitivity	Three waves expose attrition bias

Element	Standard	Rationale
Sensitivity	All analyses in Table 5 reported, including reversed order	Equivalent models cannot be ruled out

Source: Authors' synthesis.

Implications for professional bodies and policy

Bodies that accredit or recognise communication training, including those operating within national qualifications frameworks (Republic of Kenya, 2014), typically judge programmes by attendance, content and assessed learning. If the framework is supported, such judgements would be complemented by evidence on whether trainees become more credible in the eyes of colleagues, because credibility is the stage at which private gains in skill and confidence become social influence. Funders and organisations commissioning evaluations should budget for other-rated measures and for samples large enough to test paths through weak links, since an evaluation that is too small will systematically understate indirect effects. We stress that these implications are conditional on the primary study and should not be treated as findings.

Contribution to measurement practice

Beyond its substantive framework, the article offers a measurement design that other studies of interpersonal effectiveness can reuse. Separating raters by construct, combining questionnaire ratings with a structural indicator of influence, planning samples path by path and reporting reliability alongside intervals each address a weakness that is common in mediation research on workplace behaviour (Podsakoff et al., 2012; Maxwell & Cole, 2007).

A worked planning example

Suppose a provider can enrol 300 trainees in teams of six and expects 20% attrition. The effective sample is $300 \times 0.80 \div 1.5 = 160$, which in the conservative scenario S2 at reliability .80 gives a power of about .38 for the serial effect (Table 15). Doubling enrolment to 600 gives an effective sample of 320 and power of about .90. The example shows why small programme evaluations are unlikely to detect indirect effects, and it also shows that the gain from enrolling more people is steepest between 300 and 600.

Generalisability and external validity

The framework is stated for formal-sector employees in an urban Kenyan setting, and its application elsewhere should not be assumed. Sectors differ in how communication is valued and observed, informal-

sector work offers few raters of the kind the protocol requires, and the languages in which workplace communication takes place may shape which cues observers treat as credible. The anchors used for calibration come from meta-analyses that are largely based on Western samples, so the primary study is also a test of whether those magnitudes transfer. Replication in other African contexts and in other occupational groups would show how far the sequence is general.

Limitations

Several limitations qualify the results. The validation is a simulation, so it contains no empirical findings about communication confidence, credibility or influence, and none should be inferred from it. The data-generating model assumes linear paths, normally distributed variables, random measurement error and independent observations; clustering within teams and non-random error would change the figures. The anchors come from meta-analyses of mostly Western samples, and the values for d_{21} and b_2 are assumptions. The simulated estimator analyses composites with Monte Carlo intervals, whereas the primary study plans a latent-variable model with bootstrap intervals. Finally, the model omits moderators and feedback from influence to confidence, both of which are theoretically plausible.

Future research

The next step is to field the protocol. Analyses should add multilevel structure for teams, test moderators such as hierarchical level and language of communication, and examine reciprocal effects with cross-lagged designs (Maxwell & Cole, 2007). Field experiments that randomise access to skill-development would address selection. Qualitative work with supervisors could clarify which behaviours observers treat as signals of credibility.

CONCLUSION

This article proposed that communication skill-development leads to workplace influence through a sequence of confidence and credibility, specified a multi-source, three-wave protocol to test the proposition in Kenyan organisations, and validated the

planned estimator by Monte Carlo simulation anchored on published effect sizes. The simulation shows that the serial effect is detectable with a few hundred respondents, that effects routed through weak links need far more, and that unreliable composites roughly halve indirect effects. The primary study should therefore use at least 600 respondents in about 100 teams, model measurement error explicitly and treat weak-link paths as exploratory. If the framework is supported, skill-development programmes should be judged by the credibility they build among colleagues and not by skill acquisition alone.

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CONFLICT OF INTEREST AND DATA AVAILABILITY

The author declares no competing financial interests. The author serves as an editor of this journal and had no role in the editorial handling or review of this manuscript. The simulation script, seeds and output are available from the author on request.

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