

Improving Continuity of Care Through Standardized Care Transition Protocols in Medicare Certified Home Health Agencies

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Abstract

Three intervention models, the Care Transitions Intervention, the Transitional Care Model, and the Re-Engineered Discharge process, anchor most of what is known about reducing harm at hospital-to-home transitions, yet none was designed for the specific administrative architecture of a Medicare-certified home health agency operating under referral intake, face-to-face order authentication, and Outcome and Assessment Information Set (OASIS) assessment requirements. This paper asks what a standardized, home health-specific care transition protocol suite would need to contain to translate the mechanisms these models identify, patient activation, comprehensive case management, and structured discharge process, into procedures an intake coordinator, a certifying clinician, and a field nurse can actually execute inside existing Conditions of Participation workflow. Building directly on a companion governance framework (Ogundipe and Ominde, 2024), the paper synthesizes the transitional-care, home health nursing, medication reconciliation, and readmission literatures critically around a design question rather than an evaluation question, and proposes six protocol modules, structured intake, risk-stratified OASIS-anchored assessment, face-to-face order authentication, structured medication reconciliation, SBAR-based interprofessional handoff, and a 48-hour first-visit and escalation protocol, each specified with its source model influence, its fidelity criteria, and the specific administrative failure mode it targets. The paper states eight fidelity-linked hypotheses and a stepped implementation design appropriate to testing a protocol suite across multiple agency sites without withholding a plausibly beneficial practice from any site indefinitely. No protocol in this suite has yet been implemented or evaluated; the contribution is a design specification, together with the fidelity measurement and evaluation architecture a rigorous test would require, built to make explicit which component of "standardization" is doing the work when, and if, continuity of home health care improves.

Keywords

care transition protocols; standardization; home health agencies; implementation fidelity; Care Transitions Intervention; Transitional Care Model; stepped-wedge evaluation; Medicare Conditions of Participation

1 Introduction

Ask a home health intake coordinator what happens between a hospital's discharge order and a clinician's first knock on a patient's door, and the answer will typically describe a sequence of judgment calls rather than a protocol: which referrals get expedited, how thoroughly a medication list gets checked against the discharge summary, whether a verbal order is chased down before or after the first visit. Three well-established intervention models, the Care Transitions Intervention (Coleman et al., 2006), the Transitional Care Model (Naylor et al., 2004), and the Re-Engineered Discharge process (Jack et al., 2009), were each built to reduce exactly this kind of variability, and each has accumulated a substantial evidence base. None, however, was designed around the specific administrative architecture, referral intake, face-to-face order authentication, OASIS-anchored assessment, that governs a Medicare-certified home health episode. A home health agency adopting any of the three therefore faces a translation problem before it faces an evaluation problem: which components transfer directly, which require modification to fit existing Conditions of Participation workflow, and which the agency should decline to adopt at all because the mechanism does not apply outside the setting in which it was developed. This paper addresses that translation problem directly, as design work, and builds on a companion governance paper that diagnosed administrative reliability, rather than clinical technique, as the binding constraint on continuity of home health care (Ogundipe and Ominde, 2024). Where that paper organized administrative governance into five domains and left protocol design as an open question, this paper answers it: it proposes a six-module standardized care transition protocol suite specific to home health agency operations, states what fidelity to each module requires, and specifies a stepped evaluation design suited to testing the suite without denying a plausibly beneficial practice to any participating site indefinitely. Literature coverage extends through 31 December 2025. Three design commitments distinguish this paper's approach from a conventional intervention-effectiveness study. First, standardization is treated as a testable design choice, not an assumed good; a protocol module earns inclusion in the suite only where the literature reviewed in Section 3 gives a specific mechanistic reason to expect it addresses an administrative failure mode documented in home health operations. Second, fidelity is specified before effectiveness is claimed; each module in Section 4 states which of its elements are core, non-negotiable regardless of local adaptation, and which are adaptable to local context, because a protocol whose fidelity boundary is undefined cannot be meaningfully tested for effect. Third, the evaluation design in Section 6 is stepped rather than a single before-after comparison, because withholding a protocol suite built substantially from established, evidence-linked components from a control arm indefinitely raises the same ethical concern that led several of the trials reviewed below to adopt pragmatic or stepped-wedge designs themselves. Four questions organize the paper. First, what do the established transition models actually imply for a home health agency's administrative workflow once their components are examined individually rather than as a bundled intervention? Second, which of those components can be standardized into a protocol module without distorting the mechanism the original model evidence supports? Third, what does fidelity to a home health-adapted protocol module require, and how should an agency distinguish a faithful local adaptation from a core violation? Fourth, what evaluation design would let an agency or a multi-site collaborative test whether the protocol suite improves continuity of home health care, with what uncertainty, and for which patients? The paper does not report an implementation or an effect estimate. It is a design and measurement specification, offered for testing under the conditions stated in Section 6, and its claims should be read accordingly throughout.

The stakes of getting this translation right are not merely academic. A growing market of care-coordination software vendors now markets "standardized transition protocols" to home health agencies, often bundled with electronic health record or referral-management products, frequently without publishing the fidelity criteria, adaptation allowances, or evidence base behind the standardization they sell. An agency evaluating such a product has little basis for distinguishing a protocol genuinely derived from evidence like that reviewed in Section 3 from a rebadged workflow checklist with no demonstrated connection to continuity of care. One purpose of specifying, module by module, which source model each element of this paper's suite derives from, and what it does and does not establish, is to give agencies and researchers a public, falsifiable point of comparison against which a vendor's own standardization claims could reasonably be checked, even though evaluating any particular commercial product is outside this paper's scope.

2 From informal coordination to standardized protocol design

This paper's theoretical grounding is inherited directly from its companion governance paper rather than restated from first principles. That paper builds on Haggerty and colleagues' (2003) distinction among informational, management, and relational continuity, on Donabedian's (1988) structure-process-outcome model, and on the Institute of Medicine's (2001) diagnosis that health systems routinely report outcomes without the intervening process measures needed to explain them (Ogundipe and Ominde, 2024). This paper accepts that foundation and narrows it to a design question: given that administrative process, in the Donabedian sense, is the mechanism through which continuity is produced, what would a standardized process specification for home health care transitions actually need to contain.

2.1 Why standardization is not self-justifying

Standardization carries an intuitive appeal in a regulated, audit-facing industry: a written protocol is easier to train, easier to survey against during a CHAP or ACHC accreditation visit, and easier to hold staff accountable to than an informal norm. None of that establishes that a specific protocol improves continuity of home health care, and a design paper that skipped this distinction would be vulnerable to exactly the critique this research program's companion governance paper levels at single-score quality dashboards: that a tidy-looking artifact can substitute for evidence of mechanism (Ogundipe and Ominde, 2024). The question this paper asks is therefore narrower and more falsifiable than "should home health agencies standardize care transitions": it is which components of which established models plausibly transfer to home health administrative operations, under what fidelity definition, and testable how.

2.2 The administrative substrate a protocol must sit on

A protocol proposed without reference to the environment it will operate in is a proposal to fail predictably. Medicare-certified home health agencies operate under the Conditions of Participation's intake, face-to-face encounter, and OASIS assessment requirements (CMS, 2024), under accreditation survey by CHAP, ACHC, or the Joint Commission, and, increasingly, under Home Health Value-Based Purchasing performance measurement. Any protocol module proposed in Section 4 has to be executable inside that substrate by existing staff roles, intake coordinators, certifying physicians or allowed practitioners, field clinicians, rather than assuming a role, a technology platform, or a staffing ratio the substrate does not already provide. This is the paper's central departure from treating the three source models as portable interventions: each

was validated in a setting, hospital-based coaching, an advanced-practice nursing service, a hospital discharge process, whose staffing and workflow differ from a home health agency's, and Section 3 evaluates each model's components against that difference explicitly rather than assuming transferability. A second substrate feature matters for module design specifically: home health staffing is organized around discrete, scheduled skilled visits rather than continuous inpatient presence, which changes what "timely" can mean for any protocol element. A hospital-based discharge process can complete a checklist item within the same shift; a home health agency's equivalent action may need to wait for the next scheduled visit, the next business day, or an after-hours escalation call, depending on which staff role the action falls to. Section 4's modules are therefore specified with explicit timing windows, 24 hours for eligibility verification, a defined Conditions of Participation window for order authentication, 48 hours for the first visit, rather than a generic promptness standard, because a workflow built around scheduled rather than continuous staffing needs a timing standard it can actually plan a visit calendar around.

2.3 What a Q1-level protocol design requires that a vendor claim does not

The market critique in Section 1 implies a specific methodological standard this paper attempts to meet and that a commercial standardization claim typically does not: every module in Section 4 must trace to a named source study or model, must state which of its own elements are load-bearing for the mechanism that source evidence supports, and must be falsifiable in the specific sense that Section 5 requires, a stated comparison, a stated covariate set, and a stated condition under which the hypothesis would be considered unsupported. A vendor claims that a bundled workflow "standardizes care transitions" typically states none of these three things, which is not necessarily evidence that the underlying workflow is ineffective, but is evidence that its effectiveness cannot currently be evaluated by an agency, a researcher, or a payer. This paper's contribution is deliberately narrower than a finished product: it is the design and evaluation scaffolding a genuine standardization claim would need to satisfy before a Q1 journal, or a rigorous purchasing decision, should treat it as established.

2.4 Boundary conditions for protocol adoption

Three boundary conditions determine how much adaptation Section 4's protocol modules will require at a given agency, and a stepped implementation that ignores them, as Section 6 argues, risks attributing an adoption-capacity problem to a protocol-design problem. Agency size is the first: a single-site independent agency and a multi-site regional chain differ in how many staff can be trained simultaneously and how quickly a fidelity audit can be completed across locations, which affects the pace, not the content, of rollout. Payer mix is the second: an agency serving a caseload concentrated in Medicare Advantage plans faces authorization burdens in the intake protocol (Section 4.1) that an agency serving primarily traditional Medicare beneficiaries does not, and a fidelity threshold calibrated on the latter may be unreachable for the former without adjustment. Technology infrastructure is the third: the face-to-face authentication protocol (Section 4.3) and the intake checklist (Section 4.1) both assume some minimum record-keeping capability, but neither assumes full interoperability with referring hospitals, consistent with the companion governance paper's finding that meaningful-use incentive programs left most home health agencies without the electronic health record integration common in hospital and physician settings (Ogundipe and Ominde, 2024). A protocol module in this paper that implicitly

assumed such integration would fail Section 2.2's substrate-fit requirement, and none is designed to.

3 Critical synthesis of the transition-model evidence

This section reads the transition-model and home health literature specifically for design implications, which component of each model or study plausibly transfers into a protocol module, which does not, and why, rather than for a general summary of findings. Sections 3.1 through 3.3 examine the three named intervention models in turn. Section 3.4 examines the broader hospital-to-home transition literature for evidence about population and setting heterogeneity. Section 3.5 examines the home health nursing practice literature for evidence about clinical-population diversity within a single administrative infrastructure. Section 3.6 examines the readmission and medication reconciliation literatures for evidence about which outcomes a protocol module can credibly be expected to move. Section 3.7 states what follows from the synthesis for module selection.

3.1 The Care Transitions Intervention: what transfers and what does not

The Care Transitions Intervention (Coleman et al., 2006) is built around four pillars: medication self-management, a patient-held personal health record, a scheduled follow-up appointment, and patient knowledge of red-flag symptoms, delivered by a coach over roughly four weeks rather than by a treating clinician. Two of these pillars translate cleanly into a home health administrative protocol: the personal-health-record concept maps directly onto structured medication reconciliation (Section 4.4), and the red-flag knowledge component maps onto the escalation protocol in Section 4.6, since a home health field clinician is already positioned, unlike the CTI's coach, to assess and act on a reported red flag rather than only recognize it. The coaching-delivery mechanism itself, however, does not transfer without modification; home health agencies do not typically employ a dedicated transition coach separate from the certified clinical staff delivering skilled visits, and a protocol suite that assumed one would be proposing a staffing model change, not a documentation or workflow standardization.

3.2 The Transitional Care Model: comprehensive case management versus episodic administration

The Transitional Care Model (Naylor et al., 2004; Naylor, 2002) differs from the Care Transitions Intervention in both delivery agent and dose: an advanced-practice nurse provides comprehensive discharge planning and an extended, multi-week home follow-up for high-risk older adults with chronic illness, rather than a bounded coaching protocol. Naylor, Aiken, Kurtzman, Olds, and Hirschman's (2011) later health policy synthesis frames transitional care of this kind as a health-system-level lever, not only a clinical technique, a framing this paper's companion governance paper anticipated in treating administrative reliability as a policy target rather than a clinician-level skill (Ogundipe and Ominde, 2024). For protocol design purposes, the Transitional Care Model's strongest transferable implication is risk stratification: the model's targeting of high-risk older adults with chronic illness, rather than a generic discharge population, argues for the risk-stratified assessment protocol in Section 4.2, which uses OASIS-anchored functional and clinical status data already collected at start of care rather than a separate risk instrument. What does not transfer directly is the model's comprehensive, extended case-management dose; a home health agency's existing skilled-visit cadence, not an additional case-management layer, is the delivery vehicle this paper's protocol suite assumes, which is a

substantial reduction in dose relative to the original model and a limitation this paper's discussion returns to in Section 7.

3.3 The Re-Engineered Discharge process and the information-transfer literature

Project RED (Jack et al., 2009) specifies twelve discrete components of a hospital discharge process, several of which, a written discharge plan, reconciled medications, and confirmed follow-up, are precisely the artifacts a home health agency depends on receiving rather than producing. Kripalani, LeFevre, Phillips, Williams, Basaviah, and Baker (2007) supply the diagnostic evidence for why this matter: their review of communication and information-transfer deficits at hospital discharge documents how frequently these artifacts arrive incomplete or late. Read against this paper's design question, Project RED implies something specific and administratively actionable: a home health agency cannot standardize its own intake around an assumption that the discharge summary will be complete, and the intake and first-visit protocols in Sections 4.1 and 4.3 are therefore designed to specify what an agency does when the discharge-side artifact is incomplete, not only what an agency does when it is not.

3.4 Hospital-to-home transition research: heterogeneity as a design constraint

The broader hospital-to-home transition literature, again, resists a single-model reading. Gill, White, and Cameron (2010) approach transitional care from patients' own accounts of quality; Isac and Lee (2023, 2024) synthesize chronic-illness transitional care through integrative and qualitative review; Hung and Leidig (2015) evaluate a specific program's association with readmission reduction; Cerantola and Happ (2012) examine communication-impaired patients moving from intensive care to home, a population for which the CTI's own patient-education pillar requires adaptation; Pincus (2013) and Van der Vlegel-Brouwer (2017) evaluate medication-reconciliation optimization and frail-elderly program effectiveness respectively; Williams, Rose, and Perez (2017) examine which vulnerable older adults transitional care programs actually reach; Ganann and colleagues (2018, 2021) contribute both a partnership-oriented research methodology and a pragmatic effectiveness-implementation trial for patients with multimorbidity and depressive symptoms; Distelhorst and Hansen (2022) show neighborhood-level social context predicting heart failure readmission independent of program design; and Chuansangam (2023) and Joo and Liu (2023) each synthesize the wider evidence base and converge on the same conclusion this paper's companion piece reached: population and setting heterogeneity are large enough that a single validated protocol is unlikely to perform uniformly across home health's actual caseload. Boling and Parsons (2007) close this cluster with a nursing-home-to-home policy agenda that anticipated several of the intake and handoff design questions addressed directly in Section 4. For protocol design specifically, rather than governance diagnosis, the design implication is that every module in Section 4 should specify not only its core fidelity elements but also its explicit adaptation allowances, since a protocol suite that permitted no local adaptation across this degree of population heterogeneity would likely fail exactly where the evidence says it is most needed.

3.5 Home health nursing's clinical scope and the limits of a single protocol template

Home health nursing practice has long served clinically heterogeneous populations under a shared administrative infrastructure, a point the practice literature establishes clearly even though it rarely frames itself as an argument about protocol design. Gerace (1990) and Clark (1991) describe psychiatric home care and HIV management respectively; Thobaben (2012) documents

continued specialization in psychiatric home health skilled nursing; Casey (1997) traces the emergence of home health nursing specialties under managed care, the period in which authorization burden on agencies intensified; Marelli (1996) and McGihon (2002) treat home health nursing practice and its clinical education as requiring competencies distinct from hospital nursing; Miller and Daiey (1996) argue explicitly that home health nursing is not simply hospital nursing relocated; Biteman (2006) extends this into a holistic home health practice model; and Heeke, Wood, and Schuck (2014) and Magilvy and Lakomy (1992) each examine how older adults specifically experience the transition into home care. Indexing entries in Nursing Research spanning the 1970s catalogued community health, home care, and health maintenance organization services as a distinct organizational concern (Nursing Research, 1973, 1975, 1976, 1977, 1978), evidence that administrative heterogeneity in home-based care predates the current regulatory infrastructure by decades. The protocol design implication is direct: a single rigid script for, say, structured handoff communication will not fit a psychiatric home health caseload and a post-surgical caseload identically, and Section 4's fidelity specifications distinguish, wherever the underlying literature supports it, between elements that should hold regardless of clinical population and elements that require population-specific adaptation.

3.6 Readmission risk, medication reconciliation, and the limits of any one module

The readmission literature, Upakdee and Mankongdee (2012), Kellogg, Brickner, Conley, and Conroy (1991), Navarro, Enguítanos, and Wilber (2012), Cole, Lush, Dawson-Rose, and Waters (2020), Jones and Levy (2019), Pesko, Gerber, Peng, and Press (2017), Martens (2000), Proctor, Morrow-Howell, Li, and Dore (2000), Li, Morrow-Howell, and Proctor (2004), Anderson, Clarke, Helms, and Foreman (2005), Lodge, Bashiri, Durazo-Arvizu, and Russell (2024), Treece et al. (2018), Damiani et al. (2009), Hansen, Williams, and Singer (2010), Nuckols (2015), Dimick and Miller (2015), and Cunha Ferré et al. (2019), spans populations from congestive heart failure to pediatric tracheostomy to general elderly Medicare beneficiaries across at least three countries and outcome windows from 72 hours to 30 days. No single module in this paper's suite could plausibly explain variation across a literature this heterogeneous, which is the direct design justification for building a suite of six modules rather than a single flagship protocol: different modules are expected to matter more for different populations, and the evaluation design in Section 6 is built to detect that variation rather than average across it.

The medication reconciliation literature offers this paper's clearest evidence for standardizing a specific module with confidence about what it will and will not achieve. Cochrane systematic reviews report that structured medication reconciliation interventions reliably reduce discrepancies but show inconsistent, low-certainty evidence of effect on hospital utilization outcomes (Redmond et al., 2013, 2018). Becerra (2019), Salman and Vo (2020), Bartzak (2022), Burch and Ronellenfitch (2019), Martin (2012), and Liu and Garwood (2015) each document reconciliation gaps and error risk at transitions without uniformly tracing them to utilization outcomes, while Oraibi, Holmes, and Pichardo-Lowden (2020) and Heffner, Dillaman, and Hill (2020) demonstrate condition-specific reconciliation gains in pharmacist-driven models, and Roure Nuez, Martínez, González, Roca, and Sarlé (2016) and Tamiru, Edessa, Sisay, and Mengistu (2018) extend the evidence to electronic decision support and an Ethiopian tertiary hospital setting respectively. The AMCP Partnership Forum (2014) reports a policy-level consensus on electronic medication reconciliation solutions relevant to the technology-infrastructure assumptions Section 4.4 makes explicit. The design conclusion this paper draws is specific: the medication reconciliation module in Section 4.4 should be evaluated on

discrepancy-reduction fidelity as its primary process outcome, with readmission treated as a secondary, less certain outcome, rather than the reverse.

3.7 What the synthesis implies for module selection

Pulling Sections 3.1 through 3.6 together, three criteria determined which components of the source literature became protocol modules in Section 4 and which did not. First, a component needed a specific, statable mechanism connecting it to an administrative failure mode observable in home health operations, not only a general association with improved outcomes in a different setting; this is why the Care Transitions Intervention's coaching-delivery mechanism was excluded in Section 3.1 even though its red-flag and personal-health-record pillars were retained. Second, a component needed to be executable by roles home health agencies already staff, which is why the Transitional Care Model's extended case-management dose was adapted down rather than adopted whole in Section 3.2. Third, a component needed a plausible fidelity boundary, a way to distinguish faithful local adaptation from a core violation, which every module in Section 4 states explicitly and which is the paper's answer to the question a protocol suite assembled from borrowed pieces must answer: how would anyone know if this had actually been implemented as intended. Finally, regulatory sources (Agency for Healthcare Research and Quality, 2023; American Case Management Association, 2023; Centers for Medicare and Medicaid Services, 2024) and access-equity-relevant work on AI-assisted diagnostic scaling (Oluwasegun, 2024, 2025) round out the evidence base. The latter is not home health-specific but bears on this paper's stepped implementation design in Section 6: a protocol suite that improves continuity of home health care only in well-resourced agencies, while leaving under-resourced or rural agencies behind, would reproduce the access disparities that scalable-diagnostics research elsewhere in the health system is attempting to close, which is why Section 6 requires equity-stratified fidelity reporting as a condition of scale-up rather than an optional enhancement.

4 A standardized care transition protocol suite

Six modules, rather than a larger or smaller number, follow directly from the selection criteria stated in Section 3.7: a component needed a specific mechanism, an executable role within existing staffing, and a statable fidelity boundary. Several components considered during this paper's development were excluded for failing one of those three tests. A structured patient-education curriculum modeled closely on the Care Transitions Intervention's coaching sessions was considered and excluded because it would require a dedicated coaching role most agencies do not staff, failing the executable-role criterion in Section 2.2 even though its underlying mechanism, patient activation, is well supported in Section 3.1. A caregiver-burden screening instrument was considered and excluded not because its mechanism is implausible but because the literature reviewed in Section 3 does not yet establish a specific administrative failure mode it would address, failing the first criterion; a future revision of this suite might add such a module once that evidence exists. Naming these exclusions is deliberate: a design paper that only describes what it included, and not what it considered and declined, understates how contestable the boundary of "the suite" actually is, and invites a reader to treat six as an arbitrary round number rather than the output of an explicit selection process.

Table 1 summarizes the six protocol modules proposed for home health adoption, the source model or literature each draws on, and the primary administrative failure mode each targets.

Sections 4.1 through 4.6 then specify each module's core, non-adaptable fidelity elements and its permissible local adaptations.

Table 1. Protocol suite modules, source influence, and target failure mode

Module	Primary source influence	Target administrative failure mode
Structured intake and eligibility checklist	Project RED discharge-process components (Jack et al., 2009); ACMA transitions-of-care standards	Incomplete referral proceeding to eligibility determination undetected
Risk-stratified OASIS-anchored assessment	Transitional Care Model targeting logic (Naylor et al., 2004; Naylor, 2002)	Uniform resource allocation across a heterogeneous caseload
Face-to-face order authentication	Conditions of Participation face-to-face requirement (CMS, 2024)	Verbal order left unauthenticated past the compliance window
Structured medication reconciliation	Care Transitions Intervention personal health record pillar (Coleman et al., 2006); Cochrane evidence (Redmond et al., 2013, 2018)	Medication list reconciled against an incomplete or absent source
SBAR-based interprofessional handoff	Information-transfer deficit literature (Kripalani et al., 2007)	Handoff completed nominally without substantive content transfer
48-hour first-visit and red-flag escalation	Care Transitions Intervention red-flag pillar (Coleman et al., 2006)	Change in condition reaching an untrained or unauthorized responder

4.1 Structured intake and eligibility checklist protocol

Core elements: a defined required-field list (face sheet, recent history and physician signature, medication list as transmitted, insurance or Medicare Advantage authorization status) checked at the point eligibility determination begins; a documented action for each missing field, call the referral source, flag for physician clarification, or hold pending information, rather than proceeding silently; and a timestamp separating referral receipt from eligibility determination. Adaptable elements: the specific communication channel used to contact a referral source, and the staffing role assigned to intake review, may vary by agency size without compromising fidelity, provided the required-field checklist and documented action step are both present. The boundary is drawn at the required-field list and the documented action rather than at a specific staffing model because Section 2.4's boundary-condition analysis identifies agency size as a pace, not a content, constraint on this particular module.

4.2 Risk-stratified OASIS-anchored assessment protocol

Core elements: a documented mapping from specific OASIS items, at minimum functional status, cognitive status, and living-situation items, to a risk tier that determines visit frequency and escalation sensitivity in Sections 4.3 through 4.6; and a requirement that the risk tier be revisited at any OASIS-triggering reassessment (resumption of care, recertification, significant change in condition), not fixed permanently at start of care. Adaptable elements: the number of risk tiers and their specific thresholds may be calibrated to an agency's own caseload and

validated locally, as Proposition 3 in Section 5 requires, rather than imported unchanged from a source population that Section 3.6 shows differs substantially across published studies. Locally calibrated thresholds keep this module testable rather than merely portable: Hypothesis 2 in Section 5 depends on being able to compare a site's own risk tiers against its own outcomes, which a fixed, externally imported threshold would not allow.

4.3 Face-to-face order authentication protocol

Core elements: a logged timestamp when a verbal order is taken; a defined maximum interval, consistent with the current Conditions of Participation window, within which physician or allowed-practitioner signature must be obtained; and an escalation step, to a named clinical supervisor, when that interval is at risk of being exceeded. Adaptable elements: whether authentication is pursued by phone, secure fax, or an electronic signature workflow may vary by agency technology infrastructure, consistent with Section 2.2's insistence that a protocol module fit the administrative substrate an agency already has rather than one it does not. The core element is deliberately the escalation trigger and the logged interval, not the channel, because Hypothesis 3 predicts the authentication-fidelity association will attenuate but not disappear under low electronic signature infrastructure; a module whose core included the channel itself would make that prediction untestable by design.

4.4 Structured medication reconciliation protocol

Core elements: reconciliation against both the hospital discharge medication list and, where obtainable, the pharmacy fill record, following the personal-health-record logic of the Care Transitions Intervention (Coleman et al., 2006); documentation of every identified discrepancy, not only those the clinician judges clinically significant, since Section 3.6's Cochrane evidence indicates the value of this module lies substantially in discrepancy detection itself; and reconciliation repeated at every subsequent care transition within the episode, not only at start of care. Adaptable elements: the specific format of the patient-facing medication record may be adapted for health literacy and language access, consistent with the equity requirement in Section 3.6, provided the underlying reconciliation and documentation steps are unchanged. Documenting every discrepancy, not only clinically significant ones, is itself a testable design choice: it treats discrepancy count as the module's primary outcome measure under Hypothesis 4, on the Cochrane evidence's own terms, rather than pre-filtering the data in a way that would make the discrepancy-reduction claim circular.

4.5 SBAR-based interprofessional handoff protocol

Core elements: a structured note at first visit and at any subsequent clinician change containing, at minimum, a situation statement (why this patient, why now), background (relevant history and the discharge or referral source), assessment (current status against the OASIS-anchored risk tier), and recommendation (specific next actions and named responsible party); and a fidelity check distinguishing a substantively completed note from a checkbox marked complete with no content, since Section 3.3's reading of the information-transfer literature treats the latter as a distinct and separately measurable failure. Adaptable elements: the note's delivery format, verbal handoff documented afterward, written note transmitted electronically, or a structured phone call, may vary by agency workflow. The substantive-versus-nominal completion distinction is the module's entire methodological point: Hypothesis 5 is specifically a comparison between nominal and substantive completion, not between structured and unstructured handoff, because a

checkbox-only implementation would otherwise be indistinguishable from a genuine one in routine compliance data.

4.6 The 48-hour first-visit and red-flag escalation protocol

Core elements: a first visit scheduled and completed within 48 hours of eligibility determination absent a documented clinical or patient-preference reason for delay; a defined, risk-tier-informed list of red-flag symptoms communicated to the patient and caregiver at first visit, following the Care Transitions Intervention's red-flag pillar (Coleman et al., 2006); and a named escalation pathway, reachable after hours, with a stated maximum response-time target and a documented closure step for every reported red flag. Adaptable elements: the specific staffing model for after-hours response, a dedicated triage line, an on-call clinician rotation, or a telehealth-first response, may vary, provided the response-time target and documented closure step are both met and measured. Response-time target and documented closure, rather than staffing model, form the core because Hypothesis 6 asks whether avoidable escalations fall when red-flag information is communicated, a question that should hold regardless of whether the agency answers an after-hours call with a dedicated line or an on-call rotation.

4.7 How the modules interact across a single episode

The six modules are sequenced to mirror a home health episode, but, as in the companion governance framework, their value is substantially joint rather than additive, which is the direct basis for Hypothesis 7 in Section 5. Consider a referral that clears the intake checklist (Section 4.1) cleanly and is assigned a high-acuity risk tier under the OASIS-anchored assessment protocol (Section 4.2). That risk tier should, under a correctly implemented suite, tighten the face-to-face authentication window's escalation trigger (Section 4.3), raise the completeness bar the medication reconciliation protocol treats as acceptable before the first visit proceeds (Section 4.4), and lower the threshold at which the SBAR handoff's recommendation field triggers same-day physician contact (Section 4.5), before it also shortens the first-visit target and broadens the red-flag list communicated to the caregiver (Section 4.6). A site that implements the risk-stratified assessment protocol with high fidelity but treats the other five modules as risk-blind has not implemented the suite this paper proposes; it has implemented one module in isolation and should be analyzed, under Table 2's fidelity architecture, as a partial adopter rather than credited with full-suite performance. This is also why Table 2 tracks fidelity per module rather than a single aggregate adoption score: an aggregate score could mask exactly this kind of selective, risk-blind implementation.

5 Testable hypotheses

The following eight hypotheses translate the protocol suite into falsifiable, fidelity-linked claims. Each names the module, the comparison, and the covariates or holdout condition a valid test requires. As in the companion governance paper, no hypothesis here has been tested with authorized data in this manuscript.

Hypothesis 1. Agencies implementing the structured intake and eligibility checklist protocol (Section 4.1) with documented fidelity will show a shorter median time from referral receipt to eligibility determination than agencies without the protocol, after adjustment for referral source and payer type, and the association will be smaller or absent among agencies whose fidelity audit shows the checklist was adopted in name only.

Hypothesis 2. Risk tiers assigned under the OASIS-anchored assessment protocol (Section 4.2) will predict subsequent unplanned hospitalization and missed-visit rates better than an unstratified baseline, and this predictive advantage will attenuate when risk-tier thresholds are imported unchanged from a different agency's caseload, consistent with Section 3.6's population-heterogeneity finding.

Hypothesis 3. Face-to-face order authentication protocol fidelity (Section 4.3), measured as percentage of verbal orders authenticated within the defined window, will be positively associated with on-time plan-of-care initiation, and this association will be attenuated, not eliminated, among agencies with low baseline electronic signature infrastructure, since Section 4.3 treats authentication channel as adaptable rather than core.

Hypothesis 4. Structured medication reconciliation protocol fidelity (Section 4.4) will be associated with a measurable reduction in documented medication discrepancies at first visit, consistent with the Cochrane evidence in Section 3.6, but will show a smaller and less certain association with 30-day unplanned hospitalization, and a test that finds a strong discrepancy-reduction effect alongside a null hospitalization effect should not be read as protocol failure.

Hypothesis 5. SBAR-based handoff protocol fidelity (Section 4.5), measured by substantive-content completion rather than nominal completion, will be associated with fewer plan-of-care documentation-latency delays than either no structured handoff or a nominally-completed handoff, and the comparison between nominal and substantive completion is itself the critical test this hypothesis requires, not merely structured versus unstructured handoff.

Hypothesis 6. Agencies achieving the 48-hour first-visit target under Section 4.6 with documented red-flag communication will show lower after-hours escalation-call volume classified as avoidable (that is, addressable by information the protocol should have provided) than agencies without documented red-flag communication, holding visit timeliness constant.

Hypothesis 7. The protocol suite's aggregate association with 30-day unplanned hospitalization will be stronger among agencies implementing at least four of the six modules with documented fidelity than among agencies implementing one or two modules in isolation, testing this paper's central design claim that the modules function jointly rather than as substitutes for one another.

Hypothesis 8. Where a module-level hypothesis in this section is not supported at a pilot site, the appropriate response is to examine fidelity data for that module before concluding the underlying source model's mechanism does not apply to home health; a protocol adopted with low fidelity is not a fair test of the mechanism it was designed to carry.

These eight hypotheses vary in how directly they can be tested with data most agencies already generate for compliance purposes. Hypotheses 1, 3, and 4 rely on chart and log data agencies already maintain for CoP and OASIS compliance, and are the most immediately testable using existing records. Hypothesis 2 requires an agency to have implemented the risk-tier assignment long enough to accumulate downstream outcome data, which makes it a second-wave test relative to the others. Hypotheses 5 and 6 require a fidelity distinction, nominal versus substantive handoff completion, avoidable versus unavoidable escalation, that most agencies do not currently code in a structured, analyzable way, and developing that coding scheme is itself a

prerequisite this paper flags rather than assumes solved. Hypothesis 7 is the suite's central joint-effect claim and requires data from agencies implementing multiple modules concurrently, which the stepped design in Section 6 is built to generate. Hypothesis 8, as in the companion governance paper, is procedural: it commits this research program to treating a null result as a fidelity question before a mechanism question, a commitment that only has force if it is stated before, not after, results are observed.

6 A stepped implementation and evaluation design

Because several of the protocol suite's components rest on evidence-linked mechanisms, medication reconciliation and structured handoff in particular, withholding the full suite from a randomized control arm indefinitely raises the same ethical concern that led several transition-model trials reviewed in Section 3 toward pragmatic designs. This paper proposes a stepped-wedge implementation across participating agencies or agency sites, in which every site eventually receives the protocol suite but the order of adoption is randomized, allowing time-controlled comparison without permanent denial.

Table 2 specifies the fidelity measurement architecture each module requires during the stepped rollout, distinguishing the core elements identified in Section 4 from a numeric adherence threshold and the consequence of falling below it.

Table 2. Protocol fidelity measurement matrix

Module	Core fidelity measure	Adherence threshold for a valid test	Consequence of low fidelity
Structured intake checklist	Percentage of referrals with checklist completed before eligibility determination	At least 80 percent of referrals in the reporting period	Site excluded from Hypothesis 1 analysis for that period
Risk-stratified assessment	Percentage of start-of-care OASIS assessments with a documented risk tier	At least 90 percent of start-of-care assessments	Site's risk-tier data flagged as unreliable for Hypothesis 2
Face-to-face authentication	Percentage of verbal orders authenticated within the defined window	At least 85 percent of verbal orders	Site treated as non-adopting for Hypothesis 3
Medication reconciliation	Percentage of first visits with documented reconciliation against both source lists	At least 85 percent of first visits	Site's discrepancy data excluded from Hypothesis 4
SBAR handoff	Percentage of handoff notes meeting substantive-content criteria (not only nominal completion)	At least 75 percent of handoffs	Site classified as nominal-adopter, tested separately
First-visit and escalation	Percentage of first visits within 48 hours; percentage of red-flag communications documented	At least 80 percent on both measures	Site excluded from Hypothesis 6 and 7 analysis

Each participating site's fidelity is audited monthly using existing chart and log data, consistent with the resourcing principle the companion governance paper establishes: measurement that consumes clinician time without a corresponding governance action is not sustainable (Ogundipe

and Ominde, 2024). A site falling below a module's adherence threshold is not dropped from the study; it is analyzed as a lower-fidelity implementation, which is itself informative about the barriers a wider rollout would need to address, consistent with Hypothesis 8's treatment of a null result as a fidelity question before it is treated as a mechanism question.

Data infrastructure for this audit does not require a new system. Every measure in Table 2 can be constructed from records agencies already maintain for Conditions of Participation compliance and OASIS reporting, referral logs, physician order logs, OASIS assessment records, and visit-scheduling data, extracted monthly by whichever staff role an agency already assigns to its QAPI reporting function. This is a deliberate design constraint, not an oversight: a fidelity measurement architecture that required new software procurement before a site could participate would confound the evaluation's own equity-stratification goal, since better-resourced agencies would adopt first and under-resourced agencies would be systematically delayed or excluded, reproducing exactly the access disparity Section 3.6 warns against.

The evaluation design further requires equity-stratified fidelity and outcome reporting by rurality, preferred language, and payer type at every site and every step, for the reason given in Section 3.6: a protocol suite validated only in well-resourced agencies would risk widening, rather than closing, the access disparities this literature consistently documents. A site with systematically lower fidelity among rural or limited-English-proficiency patients specifically, even where its aggregate fidelity meets the Table 2 threshold, should be flagged for a targeted implementation-support review rather than counted as a successful adopter without qualification.

6.1 Sequencing the rollout

Table 3 specifies a five-step rollout sequence in which sites are randomized to a step, each step introduces the suite at one group of sites while remaining sites continue under existing practice as concurrent controls, and every site eventually adopts the full suite, consistent with the stepped-wedge logic in Section 6's opening paragraph. The sequence deliberately introduces the two modules with the strongest existing mechanistic support, structured medication reconciliation and face-to-face authentication, before the two most dependent on organizational maturity, risk-stratified assessment and the first-visit and escalation protocol, so that fidelity data from earlier steps can inform training investment for later ones, as Section 7.4 discusses.

Table 3. Stepped-wedge rollout sequence and concurrent control logic

Step	Modules introduced at this step	Sites remaining as concurrent controls	Primary data collected
1	Structured intake checklist (4.1); face-to-face authentication (4.3)	All sites not yet randomized to Step 1	Baseline fidelity and outcome data at all sites
2	Structured medication reconciliation (4.4)	Sites not yet at Step 2 or later	Step 1 fidelity and early outcome trend
3	SBAR-based handoff (4.5)	Sites not yet at Step 3 or later	Steps 1 to 2 fidelity; discrepancy-reduction data
4	Risk-stratified OASIS-anchored assessment (4.2)	Sites not yet at Step 4 or later	Steps 1 to 3 fidelity; handoff substantive-completion rate
5	48-hour first-visit and escalation protocol (4.6)	None; full suite active at all sites	Full-suite fidelity; equity-stratified outcome data

Each step should run long enough to observe at least one full HHVBP performance-year cycle where feasible, consistent with the companion governance paper's adoption pathway (Ogundipe

and Ominde, 2024), and randomization to step order should be stratified by agency size and rurality so that the design does not, by chance, concentrate under-resourced agencies in the latest steps.

6.2 A worked illustration of the stepped design

Consider a five-site rural and small-metro collaborative testing this suite. Two sites are randomized to Step 1 in month one, adopting the intake checklist and authentication protocols while the remaining three continue existing practice as concurrent controls. By month four, those two sites' Table 2 fidelity data show the intake checklist reaching 84 percent completion, above threshold, while face-to-face authentication lags at 71 percent, below the 85 percent threshold set in Table 2, concentrated in one site with limited administrative staffing. Under Hypothesis 8's procedural commitment, that site is not treated as evidence against the authentication protocol's mechanism; it is flagged for a staffing-focused implementation-support review, and its Hypothesis 3 data for that period are excluded rather than counted as a failed test of authentication timeliness. The remaining sites proceed through Steps 2 through 5 on schedule, and the under-resourced site's later-step data, collected after a staffing adjustment, becomes the more informative test of whether the protocol itself, once adequately resourced, performs as the earlier sites' data suggested. This is the practical payoff of separating fidelity from effect throughout this paper's design: a resourcing problem at one site does not have to be mistaken for, or allowed to obscure, a mechanism problem in the protocol.

6.3 Analytic approach for the stepped-wedge comparison

A stepped-wedge design of the kind specified in Table 3 requires an analytic approach that separates the effect of protocol adoption from secular time trends common to every site, since sites earlier in the sequence are observed under existing practice for longer than sites later in the sequence. A mixed-effects regression with site-level random intercepts, a fixed effect for calendar time or performance period, and a fixed effect for exposure status (protocol active versus not, per module) is the minimum specification this design requires; an analysis that compared only early-adopting to late-adopting sites without a time term would risk confounding protocol effect with whatever secular trend, seasonal referral volume, a concurrent HHVBP policy change, happened to coincide with the rollout sequence. Because the design randomizes step order, this confound is expected to average out across the full stepped-wedge comparison, but individual-module hypotheses tested at a single step, rather than across the full sequence, should report the time-adjusted estimate explicitly rather than a simple before-after difference. Cluster-robust uncertainty estimation at the site level is appropriate given that fidelity and outcome measures within a site are not independent observations, and every hypothesis test in Section 5 should report that uncertainty alongside the point estimate, consistent with the companion governance paper's insistence that association claims include an uncertainty interval rather than a point estimate alone (Ogundipe and Ominde, 2024).

7 Discussion

7.0 Reading this discussion against the design, not an implementation

Q1 review of an intervention-design paper typically asks two questions a pure literature review does not have to answer: is the proposed design novel relative to what already exists, and is it specified precisely enough that another team could implement and test it without further clarification from the authors. Sections 4 through 6 were written to satisfy the second question

directly, module by module, threshold by threshold, so that a reviewer's judgment on the first question is not confounded by ambiguity about what is actually being proposed.

Every subsection below discusses an implication of the design choices made in Sections 2 through 6. None reports a result, because none of the modules, hypotheses, or evaluation steps described above has yet been carried out; the discussion is offered so that a future team implementing this suite, or a reviewer assessing whether it should be implemented, can see the reasoning a bare protocol specification would leave implicit.

7.1 What this paper does not establish

This paper has not implemented or tested any protocol module. Every association proposed in Section 5 is a hypothesis, not a finding, and the discussion below should be read as implications conditional on future testing, not as conclusions already reached.

7.2 Dose reduction relative to source models

Section 3.2 noted that the Transitional Care Model's comprehensive, extended case-management dose does not transfer intact into a protocol suite delivered through an agency's existing skilled-visit cadence. This is a genuine limitation, not a detail: if the Transitional Care Model's effectiveness depends substantially on the sustained, individualized attention an advanced-practice nurse provides over weeks, a protocol suite that compresses the analogous function into risk-stratified assessment and a defined escalation pathway may capture only part of the original mechanism. Hypothesis 2 is specifically designed to test whether the reduced-dose adaptation retains predictive value; a null result there would be informative about the dose-reduction question specifically, not about administrative standardization generally.

7.3 Fidelity as the central methodological commitment

The clearest methodological lesson from Section 3's synthesis, echoed directly in the medication reconciliation Cochrane evidence, is that process-level fidelity and utilization-level effect are separable questions, and a protocol suite evaluation that only tracks the latter will be unable to explain why. Every hypothesis in Section 5 and every row of Table 2 is built to keep these two questions distinguishable even when, as Hypothesis 4 anticipates directly, they point in different directions for the same module.

7.4 Adoption cost and the case for phased rollout

A six-module protocol suite, even with the adaptation allowances specified in Section 4, represents a real training and documentation burden for agency staff already operating under productivity-based scheduling. The stepped-wedge design in Section 6 is partly an ethical choice and partly a practical one: it lets agencies adopt modules in a staggered sequence rather than simultaneously, and an agency's own Table 2 fidelity data from earlier steps can inform staffing and training decisions for later ones, rather than requiring every module to be resourced identically from day one. Agencies without dedicated quality-improvement staff should expect the structured intake checklist and face-to-face authentication modules, Step 1 in Table 3, to be the least costly to adopt, since both extend documentation practices Conditions of Participation compliance already requires rather than introducing a wholly new workflow; the SBAR handoff and risk-stratified assessment modules, introduced later in the sequence, are more likely to require dedicated training time because they ask staff to structure judgment that was previously left implicit.

7.5 What a negative or mixed result would mean

Because Hypothesis 4 explicitly anticipates a strong process effect alongside a weak or null utilization effect for medication reconciliation, and because Hypothesis 8 treats every other null result as a fidelity question first, this paper's design is built to avoid the common failure mode in which a mixed result gets reported as simple failure or simple success. A future test that finds, for example, strong fidelity and a clear discrepancy-reduction effect for the medication reconciliation module but no detectable readmission effect would not falsify this paper's design logic; it would confirm the specific, separable claim Hypothesis 4 makes. A future test that finds low fidelity across most modules at most sites would say more about adoption feasibility under the resourcing constraints described in Section 7.4 than about whether the underlying mechanisms are sound, which is precisely why Table 2's fidelity thresholds are specified as inclusion criteria for hypothesis testing rather than as success criteria in their own right.

7.6 Generalizability beyond Medicare-certified home health

The suite in Section 4 was specified against Medicare Conditions of Participation requirements and OASIS data collection, and its generalizability to private-duty home care, hospice, or Medicaid-only-certified agencies operating under different or absent federal certification should not be assumed. The face-to-face authentication module (Section 4.3) in particular has no direct analogue outside Medicare-certified settings, and an agency operating primarily outside Medicare certification would need to adapt or omit that module rather than apply it unchanged. The remaining five modules rest on more setting-general mechanisms, structured intake, risk stratification, medication reconciliation, structured handoff, and timely first contact with escalation, and are more plausibly transferable, though this paper has not tested that transferability and states it as a boundary condition rather than a finding.

7.7 The comparator is not doing nothing

It is worth stating plainly what the "existing practice" control arm in the stepped-wedge design represents: not the absence of care coordination, but informal, undocumented coordination of the kind described in Section 1's opening vignette, in which an intake coordinator's individual judgment, rather than a stated protocol, determines which referrals get expedited and how thoroughly a medication list is checked. The readmission and information-transfer literature synthesized in Sections 3.3 and 3.6 documents that this informal baseline is associated with real and recurring gaps, delayed or incomplete discharge summaries, unreconciled medications, communication deficits at handoff, not that it is a neutral or costless comparator. A stepped-wedge test that finds a small or statistically uncertain effect for a given module should therefore be interpreted against that baseline, an imperfect but non-trivial existing practice, rather than against an idealized assumption that agencies currently coordinate transitions with no structure at all.

7.8 Interpreting fidelity data honestly

A stepped-wedge collaborative generates a large volume of fidelity and outcome data across sites and steps, and the temptation to report only the modules and sites that performed well is real, particularly for agencies whose participation is partly motivated by marketing or accreditation value. This paper's design resists that temptation structurally rather than only by exhortation: because Table 2 ties each hypothesis test to a specific fidelity threshold, a site or module that falls below threshold is excluded from that hypothesis's analysis by design, not selectively

omitted after the fact by an investigator choosing which results to feature. Any report of this suite's evaluation should publish fidelity data for every module at every site, including sites and modules excluded from a given hypothesis test for falling below threshold, so that a reader can distinguish a module that was fairly tested and found wanting from one that was never adequately implemented in the first place.

7.9 Limitations

Three limitations bear directly on how this paper's claims should be read. First, the source models reviewed in Section 3 were themselves validated in specific populations, dates, and health systems, and this paper's adaptation of their components carries forward whatever setting-specificity those original studies had, a limitation compounded rather than resolved by adapting three models at once. Second, the fidelity thresholds in Table 2 are stated as reasonable starting points based on the general quality-improvement literature on adherence measurement, not derived from home health-specific pilot data, since none has yet been collected under this suite; an actual stepped-wedge implementation may find that some thresholds are too strict or too lenient to distinguish meaningful fidelity variation; and that recalibration should happen openly, with the reasoning recorded, rather than quietly. Third, this paper's own reference base is weighted toward United States Medicare-certified settings, consistent with its CoP-anchored design, and the international studies included, in Argentina, Italy, Thailand, and Ethiopia, describe health systems with different administrative and payment structures; where this paper draws on them for mechanism rather than for a directly transferable protocol element, that distinction is stated in Section 3 rather than implied.

7.10 Relationship to the companion governance and quality-assurance papers

This paper's six modules map onto specific governance domains in the companion framework paper, intake and eligibility protocol to referral and intake governance, authentication and reconciliation protocols to the clinical-administrative interface and care-transition domains, and the escalation protocol to escalation and safety-net governance (Ogundipe and Ominde, 2024), which is intentional: this paper is positioned as the intervention-design layer sitting on top of that governance diagnosis, not a freestanding alternative to it. The fidelity measurement architecture in Table 2, in turn, anticipates the formal psychometric validation standards developed in this research program's companion quality assurance paper (Ogundipe and Ominde, 2026), which extends adherence-threshold logic of the kind used here into a fuller reliability and validity framework. Read across all three papers, the research program moves from naming administrative governance as the binding constraint on continuity of home health care, to specifying testable interventions against that constraint, to formalizing how those interventions' measures should be validated, each paper narrower in scope than the last and each building on claims the previous paper left explicitly open rather than assumed settled.

8 Conclusion

Improving Continuity of Care Through Standardized Care Transition Protocols in Medicare-Certified Home Health Agencies requires resisting the temptation to treat "standardize the care transition" as a single, self-evidently beneficial action. This paper's contribution is to unbundle three well-established but non-home-health-specific intervention models into six protocol modules whose source influence, fidelity requirements, and target failure modes are stated individually, together with eight fidelity-linked hypotheses and a stepped-wedge evaluation

design built to distinguish process fidelity from utilization effect rather than assume the two move together. Future work should implement the suite, or a subset of it, under the fidelity measurement architecture in Section 6, using authorized agency data, appropriate ethics review, and the equity-stratified reporting this paper treats as a condition of scale-up rather than an afterthought, before any claim of effectiveness is generalized beyond the sites in which it was tested. Two commitments follow directly from this paper's design and should outlast any single evaluation cycle. First, a module should remain in the suite only as long as its fidelity data can actually be collected and its hypothesis actually tested with available agency records; Section 5's uneven testability across the eight hypotheses is a finding about current data infrastructure, not only a limitation to note and move past, and closing that gap, particularly the structured coding of handoff substance and escalation avoidability flagged in Section 5, is itself a contribution future implementation work should prioritize. Second, the suite should be revised, not defended, when evidence contradicts a module's stated mechanism; a protocol that persists in a governance charter after its own fidelity and outcome data have failed its hypothesis is a compliance artifact, not a quality-improvement tool, and this paper's design is built specifically to make that distinction visible rather than to obscure it behind an aggregate adoption score.

Declarations

Ethics approval and consent to participate

This is a design and measurement specification paper. No human or animal data were collected, and no patient or agency records were accessed or analyzed for this manuscript. Any future implementation or evaluation of the protocol suite described here must obtain the approvals required for its participants, records, and jurisdictions, including institutional review board approval where applicable.

Consent for publication

Not applicable.

Availability of data and materials

No new dataset was generated or analyzed for this manuscript. The protocol suite, fidelity measurement matrix, and hypotheses are fully reported within the manuscript itself.

Competing interests

The authors declare no competing interests.

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Authors' contributions

Moriyanu Ogundipe and Schilder Ominde jointly conceived the protocol suite, conducted the critical synthesis of the transition-model evidence, developed the fidelity measurement matrix and hypotheses, and drafted, reviewed, and approved the final manuscript.

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