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From virtual referral to completed specialist care: defining rural access outcomes in Saudi Arabia

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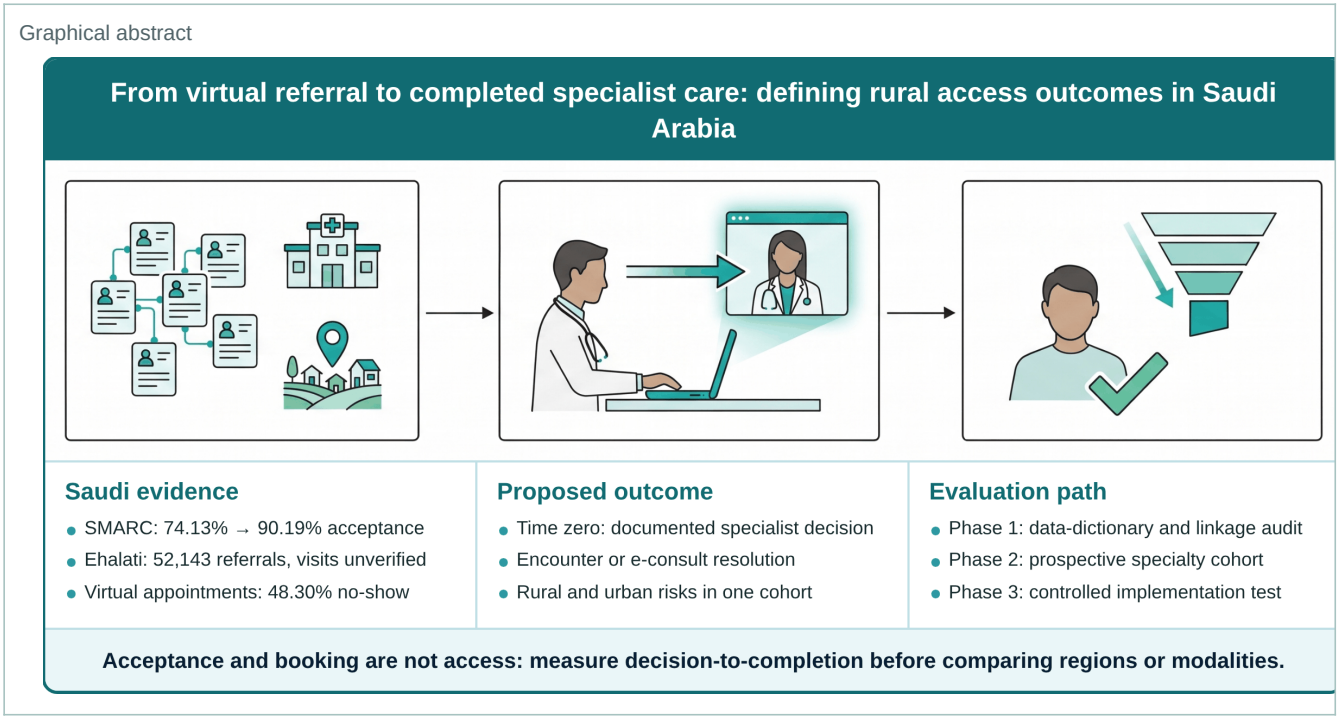
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Abstract

Graphical abstract



Saudi Arabia has expanded electronic referrals and virtual specialist services, yet their reported endpoints often differ from the outcome that matters to a patient: receiving indicated specialist assessment and an actionable plan. We critically examined purposefully selected Saudi service studies, broader reviews, and international trials, using a reproducible exploratory PubMed search to distinguish requests, acceptance, booking, consultation, and clinical resolution. This is a narrative analysis, not a systematic review or a new patient-data study. The Ehalati case study could not determine attendance after booking; national referral series likewise do not establish completion after a documented specialist-referral decision. A national virtual-appointment analysis observed high no-show frequency and geographic variation but did not supply a linked rural–urban completion cohort. We propose a conceptual, as-yet unvalidated auditable outcome: among residents of participating sites with a documented clinician decision that specialist input is indicated, the proportion completing a qualifying encounter or specialist-supported local resolution within a prespecified, clinically appropriate window. Residence is classified at that decision, permitting rural and urban stratum-specific risks. People whose need is unrecognized or never recorded remain outside this conditional cohort; population-level unmet need requires separate ascertainment. The numerator, denominator, time zero, alternative pathways, competing events, missing linkage, and geography must be defined before comparing regions or modalities. International trials demonstrate that downstream follow-up can be measured, but their effects cannot be transported to Saudi Arabia without local evaluation. A linked Saudi cohort and a pragmatic comparative design could test whether virtual-pathway availability narrows completion gaps without masking failed connections or delayed care. The framework does not assert a Saudi national completion rate or virtual-care benefit.

Keywords: Saudi Arabia; rural health; specialist referral; telemedicine; care completion; health equity

Introduction

The promise of virtual care is often expressed as geographic reach: a specialist can advise a distant clinician or speak with a patient without requiring the same journey. Saudi Arabia's Seha Virtual Hospital, national medical appointment and referral infrastructure, and regional teleconsultation services make that promise operationally plausible. A count of connected facilities, accepted requests, or video consultations, however, answers a service-volume question. It does not answer whether people in rural areas who needed specialist care received it promptly and safely. A national Saudi study published in 2024 found that 469,073 of 632,763 electronic referral requests during 2020–2021 were accepted (74.13%) [1]. Acceptance of a request must not be interpreted as verified specialist attendance or completed care; broader virtual-care implementation context is reviewed separately [2]. The outcome distinction deserves a separate methods discussion rather than another general list of telehealth advantages.

A 2024 systematic review of family-physician-to-specialist electronic consultation found possible improvements in access and referral avoidance, but the underlying evidence was rated low certainty and could not establish a causal benefit [3]. Our question is narrower. For Saudi rural access, what exactly constitutes a completed specialist referral, what would have to be linked to observe it, and how should virtual and conventional routes be compared without converting service activity into an unsupported patient outcome? A coherent answer is particularly important when a virtual specialist's advice appropriately resolves a problem at a local primary-care site: a hospital visit may be avoided because care succeeded, not because the referral failed. Conversely, an accepted referral that never produces an encounter is not access, however efficient the triage stage appeared.

The Saudi Ehalati case study illustrates this distinction unusually clearly. Among 52,143 referrals from nine primary-health-care centers during 2019–2021, its measured outcomes were referral rates and documented appointment dates; the investigators stated that they could not establish whether patients attended their appointments [4]. That is an informative local antecedent, not an error in the study. It defines the point at which a future evaluation must add evidence. Our contribution is a specification of an eligible-to-completion endpoint, its principal biases and competing routes, and a feasible staged evaluation. It neither reports new Saudi patient data nor claims priority over every referral framework.

Scope and selection of evidence

We ran three exploratory PubMed queries combining Saudi Arabia with telehealth or electronic referral and rural, remote, referral, specialist, completion, or attendance terms. The exact strings and returned counts (89, 2, and 160) are retained in an author-side source log. We then checked PubMed records for purposefully selected Saudi studies of national referral systems, virtual clinics, rural-facing concepts, and specific services; international trials that measured follow-up after referral; and reviews that bear on referral outcomes or video-service scale-up. An additional Saudi Ehalati paper and 2024 international systematic review of e-consultations were sought directly because they were close to the proposed question. Selection favored the observed endpoint and denominator over an article's use of the word “access.” There was no duplicate independent screening, pooled estimate, formal risk-of-bias assessment, or claim of exhaustive retrieval. Sources accessed through an abstract alone are interpreted only to the extent their abstract permits. We used full open-access primary text when the key interpretation depended on a paper's limitations or outcome definition.

This distinction also prevents a misleading aggregation of Saudi administrative articles. The national Saudi Medical Appointments and Referrals Centre (SMARC) analyses of 2020–2021 requests, their internal/external subgroups, and later 2023–2024 activity are related observations from one service lineage, not three independent interventions [5, 6, 7]. A mental-health subset from the earlier period and a pediatric subset from the later period are clinically useful but overlap the national referral frame [8, 9]. We use them to define what status fields have been reported, rather than counting each as independent proof that referral care was completed.

What Saudi service evidence actually measures

The 2020–2021 national SMARC study analyzed 632,763 electronic referral requests and reported 74.13% acceptance [5]. A related analysis reported 645,425 e-referrals and patterns of external versus internal movement; the different extraction populations and denominators should not be silently reconciled or added together [6]. A later SMARC study reported 755,145 referrals in 2023–2024 and 90.19% acceptance, compared with 74.13% in its earlier baseline [7]. Those are meaningful administrative process measures. They do not prove that an appointment occurred, that a specialist plan was delivered, or that residents of remote localities became more likely to complete indicated care. Even an observed fall in cross-region referrals could reflect changed local capacity, referral rules, clinical mix, or coding rather than equitable access.

The Ehalati case study found documented appointment scheduling increased from 79.6% to 95.7% over its study period. It also compared primary-care center proximity to the receiving hospital; centers within 13 km had higher referral propensity than more distant centers [4]. Facility-to-hospital distance is not the same as a patient's home-to-service burden, and a dated booking is not an attended visit. The authors' explicit

inability to verify attendance is therefore central to interpretation. A Saudi radiology reminder study followed 493 appointment recipients and reported that patients living more than 35 km from the hospital were more likely to miss appointments [10]. It establishes that distance and attendance can be observed in a selected local service, but it is not a rural-residence specialist-referral cohort or a national access estimate. A separate nationwide analysis of more than one million Saudi virtual appointment/consultation records during 2021–2022 used appointment closure fields and place of residence. It reported 48.30% patient no-shows and lower utilization in rural than urban governorates [11]. This is evidence that scheduled virtual contact can fail and that observed users are unevenly distributed. Its unit includes primary-care and follow-up appointments, not a linked cohort of people with a documented specialist-referral decision; a no-show proportion from that stream should not be subtracted from SMARC acceptance to invent a completion rate.

These administrative streams may in principle be joined, but their publication-level counts cannot be joined after the fact. The SMARC papers count requests and characterize their acceptance or destination, whereas the virtual-service paper counts appointments or consultation events. Neither provides a published person-level crosswalk between a particular referral decision and a completed specialist encounter. Even if a shared identifier were technically present, a documented-decision denominator would need clinical records that retain decisions for which no electronic request was submitted. People unable to reach care, whose specialist need is unrecognized, or whose decision is undocumented would remain outside that cohort. Request-only data are selected by care seeking, documentation, and clinician submission. If only submitted requests can be observed, the narrower “submitted-referral completion” estimand should be named in the title and conclusions; it cannot be relabeled as the rural population's access probability. A population-level unmet-need estimand would require community or all-encounter ascertainment in addition to linkage.

Small service studies show why clinical meaning also matters. In a preliminary Saudi pediatric-urology virtual clinic, 105 new referrals were assessed remotely; the investigators reported an estimated 203 trips saved and shorter time to treatment, but not a national rural denominator or independently documented downstream completion of every treatment plan [12]. A remote Jazan NARAKOM report describes a consultant-to-primary-care communication model for mountainous and island settings [13]. It is relevant to pathway design, but its implementation description is not a controlled estimate of patient-level completed access. A retrospective Seha Virtual Hospital paper tracks expansion of services, beneficiaries, and visits [14]. These operational series tell a different story from a rural eligible-to-care cohort. The Najran neonatal tele-critical-care report is also service-specific: it reports an intervention period but no statistically significant difference in mean mortality before and after implementation ($p=0.856$), and mortality is not a specialist referral-completion endpoint [15].

Survey and event-based studies can be valuable without solving the denominator problem. A nationwide convenience survey recruited 916 people who had already experienced virtual appointments and measured usability and satisfaction [16]. Excluding non-users at recruitment makes it unsuitable for estimating what proportion of eligible rural people obtain specialist care. A rural autism-spectrum-disorder telehealth overview discusses pathways and barriers but does not supply a national completed-referral estimate [17]. During two Hajj seasons, the Saudi telestroke series observed 459 patients who completed telemedicine consultations; a mass-gathering acute-stroke population cannot stand for a resident rural referral cohort [18]. Mental-health and pediatric SMARC papers describe request acceptance in their specialty subgroups rather than documented attendance or resolution [8, 9]. A recent 27-patient Jeddah inpatient smoking-cessation quality-improvement report did track appointment attendance after discharge; its highly selected

single-ward pathway shows local end-to-end documentation is possible, but neither its 100% within-sample attendance nor its uncontrolled design warrants rural specialist extrapolation [19]. Table 1 aligns these sources with the endpoints they can actually support.

Table 1. What selected Saudi sources identify—and the completion question they leave open [4, 5, 6, 7, 8, 9, 11, 12, 13, 14, 15].

Source and setting	Observed unit and reported outcome	What it can support	What it cannot establish
Ehalati PHC case study [4]	52,143 referrals; documented appointment dates and PHC-to-hospital distance	Booking documentation and facility-distance associations	Patient attendance, home-rural status, or completed clinical care
National SMARC series [5, 6, 7]	Requests; acceptance and internal/external routing in overlapping service periods	Referral status and routing patterns	Attendance or rural eligible-to-completion risk
National virtual appointments [11]	1,008,228 appointment/consultation records; closure/no-show and residence governorate	Scheduled-service use and nonattendance	Linked specialist completion after a documented referral decision
Pediatric-urology virtual clinic [12]	105 assessed referrals; estimated trips saved and treatment timing	Feasibility and preliminary clinic outcomes	National rural denominator or universal downstream treatment completion
NARAKOM remote Jazan concept [13]	Consultant-to-primary-care communication model	Plausible remote pathway and implementation needs	A measured comparison of completed specialist care
Seha Virtual Hospital series and Najran NICU [14, 15]	Services, visits, or neonatal service episodes	Operational reach and context-specific care processes	Rural referral-level completion or causal equity improvement
SMARC specialty subsets [8, 9]	Mental and pediatric requests; acceptance	Specialty-specific triage status	Independent national effect or attended specialist care

PHC = primary health care; SMARC = Saudi Medical Appointments and Referrals Centre; NICU = neonatal intensive care unit. The rows summarize different source populations and sometimes overlapping administrative periods; their counts must not be added. “Cannot establish” refers to the published endpoint/denominator, not to what might be possible with authorized linked data. This table is an original critical synthesis, not a reproduced source table.

Defining the outcome before measuring it

The proposed primary estimand is the specialty-specific fixed-window risk of completed indicated care among all residents, rural and urban, in participating sites with a documented clinician decision for specialist input. The analytic unit is a prespecified clinical-need episode, keyed to a person; a second independent indication starts a new episode, while duplicate requests for the same need remain one. Residence at time zero defines stratum-specific rural and urban risks on this common source population and an absolute rural-minus-urban contrast. Ninety days is merely an illustrative window, not an agreed Saudi standard; urgent stroke, neonatal, oncology, dialysis, and elective pathways require justified alternatives. Time zero is the first recorded clinical decision, not acceptance or booking. The cohort can include decisions never submitted electronically if the clinical record captures them; it excludes unrecognized or undocumented need. Restricting to accepted requests conditions on geography, specialty supply, and the digital pathway. Secondary proportions—submitted, accepted, booked, attended, and resolved—must not replace the decision-to-completion risk. A separate community or source-encounter study would be needed to estimate population-level unmet specialist need.

A qualifying completion event needs clinical specificity. It may be an attended in-person or virtual specialist encounter with a documented assessment and plan, or a specialist-to-clinician e-consult with a documented recommendation that the primary-care clinician acts on and communicates to the patient. A message sent without receipt, a booking without attendance, or a short call with a failed technical connection should remain separate statuses. If an e-consult advises that no specialist visit is indicated, the outcome can still count as an appropriate specialist-supported resolution only when the advice, local action, and patient communication are documented. A later procedure or definitive treatment is a distinct downstream outcome; labeling it “completion” may unfairly classify necessary watchful waiting as failure. Conversely, a completed consultation can be clinically inadequate. Hence an independent safety/appropriateness indicator—such as unplanned urgent re-referral, unresolved diagnostic need, or local clinician disagreement—should accompany a process completion rate, with adjudication rules set by clinicians in each specialty [20, 21].

It is useful to report two estimands rather than force all clinical questions into one endpoint. The first is process completion: the probability that an eligible patient has a documented qualifying specialist encounter or an adjudicated specialist-supported resolution by the clinical deadline. The second is effective completion: among the same baseline cohort, the probability that a prespecified clinical next step is performed or a documented decision that no additional step is indicated is reached safely. The first allows comparison across specialties with careful qualification; the second is more patient-relevant but requires specialty-specific adjudication and follow-up. Both should use the same time-zero cohort, so the more demanding effective endpoint does not inadvertently exclude people who disengage before treatment. Where effective completion cannot be recorded, the manuscript should state that limitation and not rename process completion as a clinical outcome. Time-to-event curves and the proportion still awaiting care can complement fixed-window risks, especially when delays rather than permanent failure are the main concern.

Figure 1 separates the shared baseline cohort from two qualifying resolution routes and the observed noncompletion/unknown states. It is informed conceptually by published referral-stage literature [3, 4] and the trial practice of following patients after screening and referral [22, 23]. The labels and proposed measures are ours; the figure is not a reproduced third-party instrument.

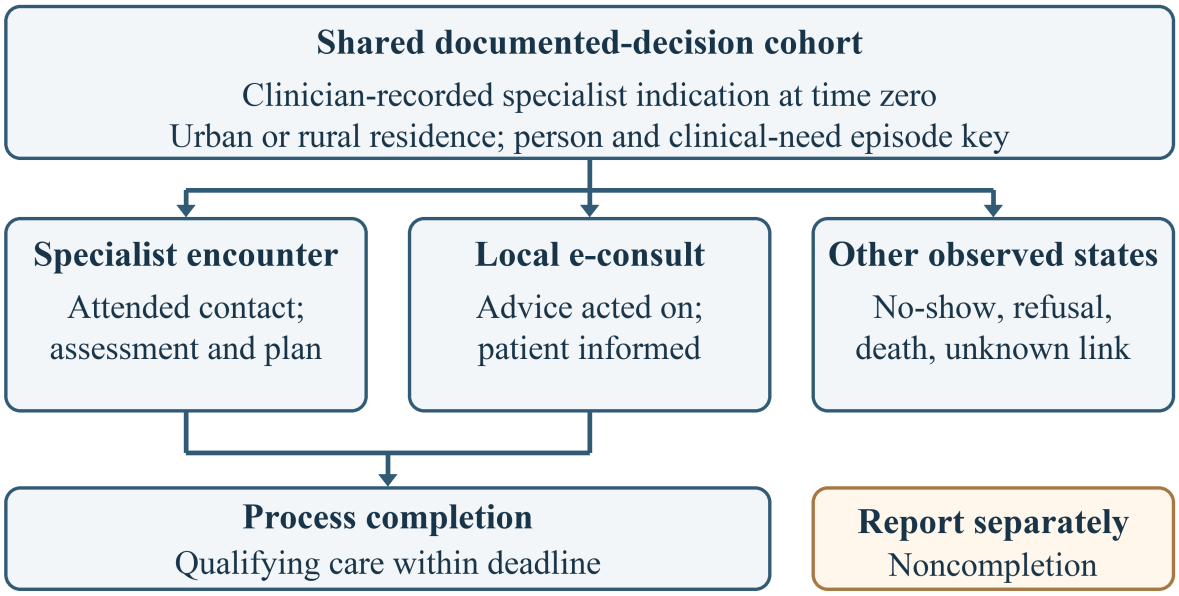


Figure 1. Shared referral-decision cohort, alternative qualifying routes, and endpoint ascertainment [3, 4, 22, 23]. This original schema is informed by the cited referral literature. The branches are outcome ascertainment paths, not a claim that every referral passes through every administrative stage. An attended specialist encounter requires an assessment and plan; a local e-consult qualifies only when advice is acted on and communicated to the patient. Rejected requests, non-submission, failed connections, private routes, competing events, and unknown linkage remain separately coded. Effective clinical completion requires additional specialty-specific adjudication. The image does not reproduce a third-party figure.

“Rural” itself cannot be assigned by the site that made the referral. A regional hospital can serve urban and rural residents, while a remote facility can receive patients from neighboring towns. The analytic file should use a geocoded or otherwise validated residential locality at time zero, apply a preregistered Saudi geographic classification, and report distance or travel time as continuous companion variables. If precise address data cannot be governed safely, a coarser small-area code with documented misclassification limits is preferable to treating an administrative region as a binary rural variable. Device access, connectivity, disability, language, caregiver help, and transportation may determine which patients appear in virtual-service data; when permissible, their ascertainment belongs before modality selection rather than only in a satisfaction survey of users [24, 25].

Referral requests are not necessarily patients. The same person may have duplicate requests, referrals to multiple specialties, revised urgency, a rejected route followed by successful in-person care, or movement to a private provider. A minimal cohort requires stable person and clinical-need episode keys, dates for every status, specialty and urgency, residence, documented indication, and encounter verification across participating systems. Deduplicate requests for the same need while retaining distinct indications. Missing encounter linkage is unknown, not failed; report its proportion by residence and assigned pathway. For a fixed-window intention-to-offer risk, death before completion is a competing event reported separately, and refusal is a documented noncompletion/choice state, not loss to follow-up. A medically appropriate specialist-supported alternative meeting the adjudication rule counts as completion; unverified private care remains unknown. Outmigration without observable care is censored for time-to-event analysis and assigned a prespecified bound in fixed-window risk sensitivity analysis. A contraindication that removes the original indication requires adjudication as appropriate resolution or a separately reported competing state. These rules and the proposed variables appear in Table 2.

Data quality can materially change the sign of an equity comparison. If rural patients are more likely to complete care at a private hospital outside the linked public network, a public-only extract will undercount

their completion; if urban patients more often use such care, the reverse may occur. If rural referrals are more frequently handled by telephone and the system logs only video appointments as completed, modality-specific misclassification will create a spurious gap. A validation sample should therefore deliberately oversample rural areas, failed-status episodes, and alternative pathways, with blinded chart or patient-contact adjudication when permitted. Report sensitivity and positive predictive value of each electronic completion code against the adjudicated record, including uncertainty, and show whether these measures differ by residence. One cannot repair unknown differential misclassification with a larger sample of the same status codes. A limited audit may be more valuable than another national dashboard of requests.

Table 2. Minimum data and event-state rules for a conditional Saudi referral-completion cohort [3, 4, 20, 21, 22, 23].

Domain	Required field or rule	Audit and analytic role
Common baseline	Person and clinical-need episode keys; documented indication, specialty, urgency and decision date	Include rural and urban decisions before submission/acceptance; audit unsubmitted decisions and duplicate requests
Geography	Residential locality at decision; classification version; travel time	Audit missing/facility-derived residence; estimate both stratum risks and travel gradient
Route and contact	Submission, rejection/redirection, booking, modality, no-show, technical failure, specialist encounter or e-consult	Verify status meanings across software versions; retain alternate and unknown routes
Completion and safety	Encounter note and plan, or specialist advice acted on locally and communicated; indicated next step and harms	Adjudicate process and effective completion separately within specialty-specific windows
Other outcomes	Verified private care, refusal, death, outmigration, contraindication and lost linkage	Classify completion, noncompletion, competing event, censoring or unknown as prespecified; show sensitivity bounds

This is a proposed specification, not a validated Saudi dataset or existing national linkage. Ninety days is illustrative only for suitable nonurgent specialties; clinicians set urgency-specific windows. No individual-level Saudi data were accessed.

Estimation and comparison without false causal claims

An initial descriptive cohort can report the number of residents with documented indications in both rural and urban strata and their cumulative incidence of completion at specialty-specific time points, by travel burden, urgency, age band, and sex where lawful. Estimate the absolute rural-minus-urban risk difference with uncertainty, alongside each stratum's risk and any ratio. Show the whole cascade because an equal final completion rate can hide inequitable waiting, unsafe redirection, or different patterns of nonattendance. In time-to-event analysis, account for death as a competing event; censor outmigration only with a defensible independent-censoring assumption and show bounds where that assumption fails. For fixed-window risks, present complete-case and missing-outcome bounds or prespecified sensitivity scenarios for unlinked private care, migration, and unknown status. Refusal remains a reported no-completion state unless a distinct patient-choice estimand is defined. Do not turn missing outcomes into completed visits or “closed” tickets into clinical resolution. National SMARC reports offer administrative fields and request denominators, not the linked clinical endpoint [5, 6, 7].

If the question is whether offering a virtual referral option causes better completion, assignment must precede outcome inspection and a common indication rule must be applied at participating encounters

before pathway choice. A pragmatic stepped introduction across comparable sites, randomized clinic-level availability when acceptable, or a controlled interrupted time series could compare decision-to-completion among the common documented-indication cohort. If virtual availability changes whether clinicians record an indication, conditioning on the recorded decision after rollout can itself select different patients; investigators must audit indication recording across arms and periods, or define a pre-exposure all-encounter eligibility cohort for the causal analysis. The comparator is availability of the usual pathway in comparable sites and calendar periods, with clinic clustering, specialty, urgency, and coding changes accounted for. A retrospective difference-in-differences design needs parallel-trend and stable-coding checks; patient uptake is self-selected by connectivity, clinician choice, severity, and preference. Regional specialist capacity, acceptance-rule changes, seasonal Hajj demand, and national reforms may confound comparisons. Do not “adjust away” structural geographic barriers when equity is the outcome. Predefine heterogeneity by residence, age, disability, language, and deprivation where defensible. The 2024 systematic review's low-certainty access evidence supports this caution [3], not a Saudi effect estimate.

For geographic equity, the principal contrast is rural-minus-urban completion risk within the same documented-indication source cohort and the change in that contrast after pathway introduction. A virtual program could raise both groups' completion while leaving an absolute rural disadvantage intact; the gap could also narrow because urban completion deteriorates. Report both stratum-specific risks, the overall risk, and the gap. A travel-time gradient adds information to a rural/urban binary. The primary program contrast should be clinic-level offer/availability, analyzed by assignment or introduction regardless of patient uptake. The contrast among people who used virtual care is secondary and selected; inverse-probability or matching methods can balance measured baseline covariates but cannot remove unmeasured preference, digital literacy, or clinician selection.

Process improvement should be tested with balancing measures. Virtual care might reduce travel for people who successfully connect, while worsening delayed diagnosis if examination or imaging is unavailable locally. Site dashboards should therefore pair completed specialist contact with time to decision, time to indicated treatment, urgent transfers, repeat referrals, technical failures, patient-reported travel and cost, and clinician workload. Routine counts of remote visits cannot replace this set. The review of e-consults before 2015 found favorable timeliness and provider perceptions but limited rigorous clinical outcomes [20]; the later systematic review likewise judged empirical evidence modest and largely observational [21]. Rural emergency-department telehealth evidence shows that “effectiveness” varies across clinical contexts and outcome definitions [24], and a specialty-telemedicine access review identified technology, language, income, and geographic barriers from the pandemic-era evidence base [25]. Those findings argue for specialty-specific measures and a documented failure state, not for a universal promise that a digital route improves rural care.

What international experiments contribute and what they do not

Two Alaska hearing-screening publications give a concrete example of following the referral through to completed specialty contact. In a cluster randomized school program, 268 of 391 referred children in the telemedicine arm and 128 of 399 in standard care received follow-up within nine months [22]. A preschool analysis reported 30 of 39 versus 16 of 32 completing follow-up in that interval [23]. These reports arise from related community screening programs with overlapping context, not two independent Saudi-like replications. Their age group, hearing pathway, geographic infrastructure, and intervention package differ from Saudi adult medical referrals. Their transferable lesson is methodological: enumerate all positive

screens or indicated referrals, follow each person after the referral event, and name the time window. The effect size is not transportable as an anticipated national Saudi benefit.

A US randomized trial integrating telemedicine into opioid-treatment settings found much greater hepatitis C treatment initiation and cure than offsite specialist referral [26]. Those are downstream care outcomes, not merely completed video connections. Yet the integrated treatment platform, disease, referral alternative, and patient population make the result inappropriate as an estimate for Saudi rural specialist access. A Norwegian remote-orthopedic randomized economic evaluation framed value in travel and visit costs under local volume assumptions [27], while a small Swedish randomized remote-echocardiography pilot examined time to clinical decision rather than a national equity denominator [28]. They help select secondary outcomes; they cannot validate our proposed Saudi rural classification or completion threshold. The NASSS-guided video-consultation synthesis underscores that technical platform, clinical condition, adopter work, organizational relationships, and sustainability affect scale-up [29]. A Saudi evaluation should measure these implementation conditions alongside patient-level completion so that an apparent failure is not wrongly attributed to rural residence when a remote clinic lacked equipment or specialist coverage.

A practical Saudi evaluation sequence

The first phase is a data-dictionary and linkage audit rather than a headline impact analysis. Ministry, referral-center, primary-care, receiving-service, and virtual-platform data owners would agree which fields capture decision to refer, referral submission, triage, booking, attendance, specialist advice, and follow-up. A limited sample of episodes should be manually checked against clinical records to estimate false “closed” statuses, duplicates, and missing encounters. The Ehalati study suggests that appointment documentation exists [4], and the national virtual-consultation study shows that closure and no-show fields exist in at least one service extract [11]; it does not follow that identifiers, permissions, or interoperability already permit national linkage. Field availability and validity must be verified in each participating system. The definitions should distinguish primary-care e-consults that appropriately resolve need locally from requests that are rejected or lost; both can reduce hospital appointments, but only the former may represent successful care.

The audit should also verify which administrative events mean what they say. A referral “accepted” by a receiving unit may mean only that a case entered a queue. An appointment “closed” may mean patient attendance, cancellation, administrative expiry, or a completed telephone call, depending on the software and reporting period. The data dictionary should capture every status version and its historical changes so that an apparent improvement after a platform upgrade is not a coding artifact. Before cross-region comparison, teams should check whether receiving hospitals use the same triage rules and whether virtual consultations can be booked without passing through SMARC. The resulting metadata may reveal that a national pooled completion rate is not yet identifiable; publication of that gap, with a clear route to resolution, would be more useful than a confident but invalid estimate.

The second phase is a prospective, specialty-specific cohort in sites spanning different travel burdens. A clinician should set urgency-specific windows and determine which e-consult resolutions count as clinically adequate. Patients or representatives should be involved in defining burden: an avoided 300-km trip is valuable even if the statistical completion proportion is unchanged, whereas a technically completed call may offer little benefit if the patient still must travel for an unavailable test. Independent data checks would compare electronic statuses with encounter notes and, where feasible, patient confirmation. The study should publish a denominator-flow table and missing-linkage percentages for each residential stratum

before comparative claims. If a comparison is possible, sites should document virtual option availability, training, bandwidth, and specialist response capacity, because a service that exists on a national map may be unusable in a particular clinic.

The third phase can test an implementation change, such as facilitated virtual booking or primary-care-assisted specialist e-consultation, with a concurrent control and prespecified outcomes. Intervention assignment, protocol deviations, and care outside the public system must be recorded. A qualitative companion study can explain why people did not complete care rejected request, uncontactable patient, travel burden, unsuitable technology, or clinically appropriate resolution—without treating all noncompletion as the same failure. Any multi-system linked study would require data-owner authorization, privacy safeguards, Saudi ethics review as applicable, and accountable clinical oversight. We have not obtained those permissions or analyzed identifiable records. The proposed work is a blueprint for a future study, not a claim that national patient linkage or a causal evaluation is already feasible.

Limitations and conclusion

This focused narrative review purposefully selected sources and did not measure unpublished Saudi operational practice. A different database, Arabic-language search, or local service audit could identify additional completion work. Some included studies were assessed through PubMed abstracts, which constrain appraisal; our central Ehalati and national referral/virtual-consultation interpretations were checked against open primary text. The framework also trades off a simple national headline against specialty-specific clinical meaning: a uniform 90-day endpoint would be unsafe for emergency care and potentially too short for some elective pathways. Its value lies in making that choice explicit before data analysis. The inspected Saudi literature supports strong statements about expansion of referral and virtual-care activity and narrower statements about acceptance, scheduling, and selected consultations. It does not by itself support a national rural eligible-to-completed-specialist-care rate. A linked cohort with a clinically meaningful endpoint, transparent geographic definition, and credible comparator would allow that consequential question to be tested.

Declarations

Ethics and consent: Not applicable; this work uses publicly available literature or aggregate published information and does not involve newly collected individual patient data.

Data availability: All underlying cited sources are available online, as identified in the references.

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Conflicts of interest: The authors declare no competing interests.

Generative AI disclosure: Artificial intelligence was not used in the preparation of this manuscript.

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