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When "Success" Is Not an Outcome: Ninety-Day Ascertainment and Continuity of Care in Islamabad Residential Drug Rehabilitation

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Abstract

Introduction: Residential rehabilitation reports can conflate programme completion, contact after discharge and sustained recovery. This paper examines ninety-day outcome ascertainment and continuity of care using institutional data from fifteen residential drug rehabilitation centres in Islamabad, Pakistan. Methods: We analysed aggregated centre-level questionnaire data and interpretive interview material from rehabilitation professionals. Denominators across the post-discharge pathway were reconstructed to distinguish discharge, follow-up contact and completed outcome assessment. Because the available evidence was aggregate and centre-reported, the analysis evaluates outcome ascertainment and reporting practices rather than treatment efficacy. Results: The study dataset included 870 discharges eligible for follow-up, 400 contacted (46.0%), and 360 with an assessed substance-use status (41.4%). Among the assessed group, 200 had a recorded return to use and 160 did not. Outcomes remained unascertained for 510 eligible discharges (58.6%). The 55.6% return-to-use proportion among assessed cases cannot represent all eligible discharges. Management-reported success claims were not based on comparable documented outcome definitions. Conclusions: The available evidence supports a finding of incomplete outcome ascertainment, not a credible cohort-wide relapse rate or estimate of treatment efficacy. A transparent monitoring system should distinguish eligibility, contact, assessment, return to use, and continuing service engagement.

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1. Introduction and research problem

Residential treatment frequently supplies the most visible evidence that a response to substance-related problems exists: a physical institution, admissions registers, occupied beds, professional appointments and a point at which a person is recorded as discharged. Yet each of these markers answers a different question from the one most important to service users, their families and policymakers: what support remains accessible after admission ends, and what can actually be established about subsequent recovery? The challenge is not resolved by requiring a single universally applicable definition of recovery. It begins with ensuring that each stated outcome has an identifiable cohort, a specified time horizon and a transparent method of ascertainment. Where these basic properties are missing, a numerical estimate can appear precise without supporting the inference attached to it.

The present article examines this problem using institutional questionnaire data and interpretive professional material collected from fifteen rehabilitation centres in Islamabad. The study dataset describes a pooled group of 1,077 treatment entrants and a ninety-day

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follow-up classification for 870 eligible discharges. It also includes professional perspectives attributed to staff working in rehabilitation. These materials form a useful case for examining how different administrative stages become conflated in public or managerial accounts of treatment success. They do not, however, comprise an independently audited longitudinal cohort. The analytical purpose here is therefore to assess the information content of the reported categories, not to assess causal treatment effects, establish intentional misreporting, or estimate population-level relapse.

This distinction matters particularly in the setting of privately financed residential provision. Families may make substantial commitments before admission and reasonably seek assurance that care will help. Providers may need practical indicators for service management; regulators may need comparable reporting; clinicians may want indicators of risk and continuing needs. Each audience has a legitimate interest, but a percentage can function very differently across these audiences. A programme-completion figure can be administratively valid and still be misleading if presented as durable remission. A follow-up contact count can measure outreach and still omit clinical status. An assessed subgroup can provide useful information while remaining unsuitable for extrapolation to the full group of eligible discharges.

The research question is: how far do the outcome-reporting and ninety-day follow-up categories recorded in the participating rehabilitation centres support credible conclusions about continuity of care and post-discharge outcomes? The article makes three contributions. It reconstructs the administrative sequence with explicit denominators; treats unascertained outcomes as a substantive information problem rather than an inconvenient residual; and proposes a proportionate, auditable monitoring approach that separates service activity, engagement, outcome assessment and pathways back into care. These proposals are analytically derived, not evaluated interventions. The paper's clinical language follows WHO and UNODC (2020), while its measurement logic draws on care-cascade reasoning (Williams et al., 2019) and established approaches to missing data (Little & Rubin, 2019).

2. Conceptual framework: continuity, care cascades and missing outcomes

The paper treats continuity of care as a sequence of organizationally mediated events, not as an automatic consequence of admission. The care-cascade perspective is useful because each stage of engagement and follow-up requires a separately defensible denominator (Williams et al., 2019). At the same time, Donabedian’s (1988) separation of structure, process and outcome prevents a recorded telephone call from being mistaken for a clinical endpoint. The frameworks illuminate different facets of the same problem: institutions need not only services but a way to show what was delivered and what happened next. Neither framework is used here as a local prediction model.

Donabedian's (1988) distinction among structure, process and outcome helps explain why the institutional records are informative but incomplete. Structural indicators concern resources and organisational conditions: beds, named personnel, visiting clinical hours and facilities. Process indicators concern what actually takes place, including assessment, care planning, consent, clinical review, referral and discharge preparation. Outcome indicators concern subsequent changes in health, functioning or substance use. These dimensions are connected but cannot substitute for one another. Good infrastructure can enable high-quality practice,

yet infrastructure alone cannot establish that an appropriate intervention occurred; similarly, a documented consultation does not, by itself, establish its clinical effect. Read in these terms, the study has comparatively detailed structural information and much weaker evidence about completed processes and sustained outcomes.

The WHO and UNODC (2020) standards provide a complementary clinical and ethical orientation. They describe treatment as a system of assessment, suitable interventions and continuing support, not simply a period of separation from the community. The standards should not be used as if the present study had conducted a full compliance audit. Their usefulness is analytical: they clarify what would have to be documented before claims about treatment quality could be substantiated. For example, recording the availability of group sessions answers a different question from documenting individual needs assessment, treatment selection, progress review and a feasible discharge plan. The institutional questionnaires make several of these distinctions visible, but they do not supply enough individual-level evidence to decide how consistently they were implemented.

Implementation research sharpens the distinction further. Proctor et al. (2011) separate implementation outcomes, such as adoption, feasibility, fidelity and sustainability, from service and clinical outcomes. A centre may adopt a protocol without delivering it consistently; it may deliver a programme as planned without demonstrating improvement in the outcomes that matter to residents. This offers a more precise interpretation of the recurrent distance between policy documents and daily practice evident in the professional accounts reviewed in this study. An institution's claim to offer aftercare describes availability or intended adoption. The number of discharged people successfully contacted describes a further process. A completed standardised assessment describes a further achievement. None of these indicators, independently, proves sustained recovery.

The pathway perspective discussed by Williams et al. (2019) adds attention to transitions and loss of contact between successive stages. Their specific cascade was developed in response to opioid use disorder in the United States, so its clinical categories cannot simply be transferred to a mixed-substance sample of Islamabad centres. The transferable insight is conceptual: counts at one stage must not be treated as counts at another, and difficulties at transitions deserve analysis in their own right. The present findings demonstrate the value of this insight because the denominator changes depending on whether the unit is a reported entrant, an admission episode, a completed or closed treatment episode, an eligible discharge, a contacted person, or an assessed outcome.

Taken together, these three lenses support an analytical proposition that runs through this discussion: the visibility of a resource, the documentation of its use, and evidence of its results form different levels of knowledge. This proposition is especially relevant to governance research because institutions can readily report tangible inputs while the ethically and clinically significant details of care may be less readily visible. The appropriate policy response is not to reject institutional data but to design information systems that connect the levels without collapsing them. It is also to avoid treating incomplete documentation as proof either of good practice or of malpractice. The evidence should be strong enough to support the claim being made.

3. Materials and methods

This article reports a structured analysis of centre-level questionnaire data and attributed professional interpretations collected for the study. The object of analysis is the claim supported by each reported indicator, not the individual experience of a patient. The article draws on aggregated institutional records rather than a patient-level longitudinal database. The evidence should therefore be read as centre-reported institutional material. All numerical calculations were independently recomputed from the aggregate counts provided in the study dataset.

The institutional unit is the centre report. The main pooled descriptive cohort comprises fifteen centres, with RC01 treated as a separate contrasting case because its forms contained incompatible patient denominators. The published counts should not be mistaken for a single closed annual cohort. The questionnaires refer to twelve-month windows preceding institution-specific completion dates, while the outcome material refers to discharges eligible for a ninety-day status assessment. For this reason, entrants, admission episodes, closed episodes and eligible discharges cannot be assumed to be successive observations of identical individuals. Nor do the reported 1,077 entrants establish a population-based sampling frame for Islamabad. They are the sum of counts attributed to selected institutional records.

The analytical sequence comprised four steps. First, variables were classified by their unit: reported entrant, residential admission episode, closed episode, eligible discharge, contacted eligible discharge or assessed eligible discharge. Second, the information pathway was reconstructed from the printed discharge and follow-up totals. Third, the numerator and denominator of every reported percentage used in the article were recalculated. Fourth, claims of treatment success were compared conceptually, not inferentially, with the follow-up categories; their populations and observation windows were not presumed equivalent. No significance tests, confidence intervals, causal estimates or pooled centre-effect models were applied because the paper lacks the underlying cases, a probability design and a verifiable common follow-up protocol.

The thematic material was used differently. The study attributes excerpt to seven psychologists, three psychiatrists, two rehabilitation technicians and three owners. These accounts are used to illuminate how rehabilitation professionals understood administrative, clinical and organisational problems surrounding discharge and aftercare. They do not, however, verify the quantitative counts, establish actual patient consent procedures in every centre, or document individual recovery trajectories. No direct patient, family, police or regulator perspective is claimed in this article.

The appraisal draws on two compatible frames. In care-cascade reasoning, the question is what happens to the number of individuals for whom information is available at each successive stage. In quality-of-care reasoning, the distinction is between structural inputs, actual processes and outcomes (Donabedian, 1988). We do not borrow an opioid-specific clinical cascade as a validated Pakistani performance indicator; instead, we use the principle of staged denominators to expose the consequences of assuming that service provision, contact and outcome documentation are equivalent. Missing-data theory identifies a further limitation: assessment is potentially selective. Without characteristics of those assessed and

not assessed, the mechanism of missingness is unknown and generalisation from the assessed subset cannot be justified (Little & Rubin, 2019).

4. Results

The study reports 1,077 treatment entrants and 1,198 residential admission episodes in the fifteen-centre pooled data. The 121-episode difference cannot be interpreted as 121 distinct relapses because repeat admissions may occur within individuals and across centres; the necessary individual linkage is absent. For discharge administration, the study lists 966 closed episodes, of which 579 were classified as planned completions, 290 as early departures, 58 as transfers and 39 as administrative terminations. The categories add exactly to 966. Planned completion therefore represented 59.9% of closed episodes, not of all entrants, and did not carry a validated post-discharge outcome definition. The article retains the institutional category names to avoid giving them clinical meanings that were not measured.

For the ninety-day follow-up indicator, 870 discharges were reported as eligible for assessment. Contact was recorded for 400 individuals (46.0% of those eligible), while 360 (41.4%) had a documented substance-use status. This leaves 510 eligible discharges (58.6%) with no recorded assessed outcome. Figure 1 presents the follow-up cascade, and Figure 2 summarises the distribution of assessed outcomes.

Management descriptions across participating centres included success figures ranging from 78% to 92% and relapse figures ranging from 8% to 22%. Their denominators, reference periods and definitions were not standardised. One may compare the language of these claims with the existence of incomplete follow-up data, but one cannot legitimately calculate a pooled discrepancy as if all figures measured the same event in the same cohort. Specific centre-level contrasts should be interpreted cautiously unless audited against the original source forms.

Among the 360 assessed individuals, 200 had a recorded return to use: 55.6% of the assessed subgroup. The corresponding proportion of all 870 eligible discharges with a *documented* return to use is 23.0%, but this is not a cohort relapse rate either. It is the minimum recorded proportion under the available classifications. At the opposite extreme, if all 510 unassessed individuals had returned to use, the overall proportion would be 710/870, or 81.6%. Neither extreme is an empirical estimate. The width of this descriptive bound demonstrates how heavily any overall outcome rate depends on assumptions about the missing group. There is no basis in the records to identify where within the bound the actual percentage falls.

In addition to missing status, the study reports no consistent and verified measurement of social functioning, treatment re-entry, wellbeing, quality of life, housing stability or voluntary engagement with follow-up services. This matters because post-discharge recovery need not be reducible to a binary substance-use field. A recorded return to use may call for renewed care; no documented return does not in itself establish stable functioning, absence of distress or durable recovery. The distinction between outcomes unknown and outcomes unfavourable is equally important. Administrative absence should not be silently counted as either success or failure.

Table 1. Follow-up sequence reconstructed from the aggregated study data

Stage or classification	Reported number	Appropriate interpretation
Closed treatment episodes	966	Discharge episode unit; not unique long-term outcomes
Planned programme completion	579 (59.9% of closed)	Administrative end-of-programme classification
Eligible for 90-day follow-up	870	Denominator for reported follow-up coverage
Contacted	400 (46.0% of eligible)	Contact recorded; not necessarily assessed
Substance-use status assessed	360 (41.4% of eligible)	Subset with recorded outcome classification
Return to use among assessed	200/360 (55.6%)	Proportion only among assessed subgroup
No documented return among assessed	160	Not synonymous with sustained recovery
Unassessed outcome	510 (58.6% of eligible)	Unknown; do not classify as success or failure

Source: Centre-level rehabilitation questionnaires and aggregated study dataset.

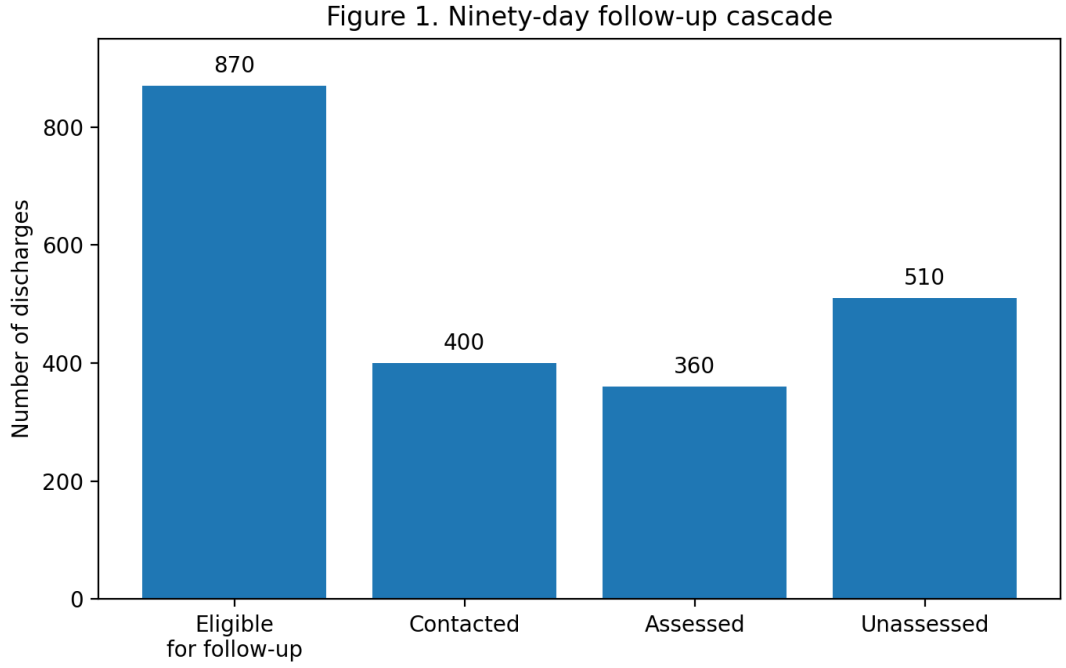


Figure 1. Ninety-day follow-up cascade among eligible discharges (n = 870).

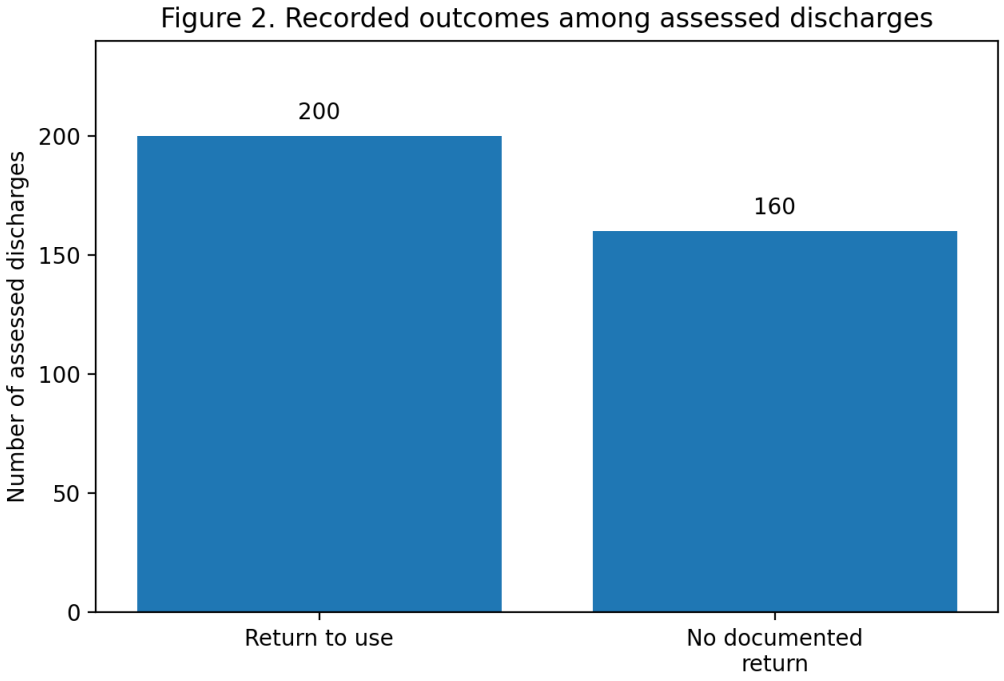


Figure 2. Recorded outcomes among assessed discharges (n = 360).

5. Interpreting reported treatment success

Perhaps the most consequential interpretive issue concerns the difference between completing residential treatment and demonstrating recovery after discharge. The participating centres reported favorable success narratives, but the underlying follow-up records did not provide a common, sufficiently observed outcome measure. This is not simply a problem of optimistic language. It is a problem of the event being measured. Programme completion might mean that someone remained for an expected period. Discharge status might document an administrative decision. Being contacted establishes that a communication was recorded. A substance-use assessment supplies information about one aspect of a person's subsequent status. None of these events is equivalent to sustained recovery, and the article's analysis depends on keeping them separate.

The distinction matters because recovery is multidimensional. A person may report reduced or discontinued substance use while facing difficulties in relationships, work, housing or mental health. Another person may return to use after discharge while continuing to engage with services, manage other responsibilities or seek renewed help. The present data do not support classification of these trajectories into simple success and failure categories. The WHO and UNODC (2020) framework favours continuing, appropriately selected care; it therefore provides grounds for examining support across time rather than defining a fixed residential endpoint as a complete clinical result.

The contrast between management claims and documented assessments cannot, by itself, establish deliberate exaggeration. The stated percentages may have been constructed from different populations, time horizons, definitions or informal follow-up methods. Without knowing the exact numerator, denominator, follow-up window and assessment method for each claim, two percentages that appear inconsistent may not in fact refer to the same quantity.

Equally, their apparent agreement would not prove validity if both relied on incomplete information. The appropriate response is to request an outcome definition before using any figure in institutional comparisons, research publications or discussions with prospective residents.

The study also illustrates the distinction between a service's performance and the quality of its measurement. A facility with rigorous follow-up might identify more instances of return to use than one that rarely contacts former residents. A superficially higher recorded rate could therefore reflect better ascertainment rather than a worse clinical programme. The reverse problem is equally possible: a reassuring rate calculated only among those who respond may conceal outcomes in a large unobserved subgroup. Consequently, simple league tables based on self-reported success percentages would be methodologically unsound on the present evidence.

For the present study, the conclusion is narrower than an evaluation of treatment effectiveness but analytically important: claims of sustained recovery were not adequately supported by comparable follow-up data. That result remains relevant even if the underlying treatment was beneficial. It identifies an accountability gap between what institutions communicate and what the available records can demonstrate. Closing the gap would be a precondition for, rather than proof of, meaningful outcome evaluation.

The findings support a distinction between two forms of uncertainty. Clinical uncertainty concerns an individual's changing health or substance use after discharge. Measurement uncertainty concerns whether institutions possess sufficient information to describe that change. The first is an expected feature of many long-term conditions; the second is partly susceptible to organisational improvement. A service cannot prevent all post-treatment difficulty, but it can adopt coherent definitions, conduct appropriate follow-up and report what remains unknown. Treating the two uncertainties as identical risks making poor data appear to be a clinical verdict, or conversely making favourable administrative language appear to be proof of sustained recovery.

The study's numerical sequence shows why a performance dashboard needs separate columns for successful contact and completed assessment. A telephone call may provide logistical information without an adequate measure of clinical status. An assessment may occur without a substantive plan to address emerging needs. Even an adequate assessment does not establish that a patient received the next indicated service. In implementation terms, these are different outcomes: availability, reach, delivery and continuity (Proctor et al., 2011). The practical challenge is to specify who completes each action, when it should occur, what information is recorded and how the next responsible professional becomes involved when risk is identified.

The article's contribution should not be reduced to the observation that follow-up rates are low. Low ascertainment is a measurable feature of the provided records. The stronger finding concerns the interpretation of any claim made in the presence of that gap. If a provider reports a high programme-completion rate, it should be presented as completion. If it reports an abstinence or return-to-use figure, it should specify assessment coverage and the observation window. If it invokes a broader concept of recovery, it should explain the domains used and the method by which information was obtained. These distinctions make claims more useful even before any service achieves complete follow-up.

There is also a justice dimension. Families purchasing residential care may be making high-stakes decisions with limited clinical information. A percentage without a denominator does not allow them to judge what is known. The same informational problem affects referral professionals, regulators and policy planners. Transparent uncertainty is not a sign that the institution has failed; it is an element of informed accountability. The standard should be sufficiently practical for smaller services, without encouraging claims that cannot be verified or imposing data collection that compromises privacy. High-quality reporting can be modest in scale but must be explicit about its limits.

6. Unknown outcomes and uncertainty

The follow-up figures provide a particularly clear example of how missing information affects interpretation. In the reported records, 870 discharges were eligible for a ninety-day assessment, 400 had recorded contact, and 360 had an assessed substance-use status. Of those assessed, 200 were recorded as returning to use and 160 as having no documented return. Thus, the assessment proportion was 360 divided by 870, or 41.4%, while the proportion with recorded return to use within the assessed group was 200 divided by 360, or 55.6%. The number without an ascertained outcome was 510. These totals reconcile arithmetically; they do not establish the representativeness or clinical validity of the assessments.

The reason for missing assessment is unknown. Some discharged individuals may have been unreachable because they changed contact details. Others may have declined further communication, continued treatment elsewhere, been contacted without a completed status assessment, or had contact recorded without a standardised instrument. These possibilities could relate differently to their outcomes. Treating the assessed subgroup as a random sample of all eligible discharges would require a justification that the dataset does not provide. Therefore, the assessed return-to-use proportion must not be projected to the full eligible group.

The broad arithmetic bounds make the uncertainty concrete. If none of those with unascertained status had returned to use, the recorded minimum would remain 200 of 870. If all had returned, the maximum under that extreme assumption would be 710 of 870. Neither scenario is a clinically justified estimate; they illustrate the range left unresolved by the available data. More elaborate modelling would not necessarily be more credible without reliable information about why outcomes were missing. Precision in the calculation cannot compensate for uncertainty about the people not observed.

There is a practical implication for the design of aftercare. The number of contacts is not a sufficient indicator if contact does not produce useful information or identify a person who requires assistance. Conversely, an outcome assessment should not be reduced to a surveillance exercise that residents experience as judgement or punishment. It needs an explained purpose, suitable confidentiality, opportunities to decline and a clear route to further support. Thus improved ascertainment is both a measurement objective and a relationship-building process. The latter cannot be evaluated from contact totals alone.

Future monitoring could report several distinct indicators: proportion with a documented discharge plan, proportion eligible for follow-up, proportion for whom contact was attempted, proportion actually reached, proportion completing an agreed assessment, proportion

requiring additional support and proportion linked to that support. Such measures would reveal where attrition occurs and what response follows. They should be reported alongside reasons for non-assessment where these can be obtained ethically. This would turn a broad, ambiguous success claim into a set of interpretable operational questions while still avoiding the assumption that a single measure captures recovery.

7. Continuity of care after residential discharge

The problem of aftercare is not merely that institutions may fail to make a telephone call. It is that the transition from residential treatment to ordinary life redistributes responsibility among a centre, a resident, relatives, clinicians, employers, educational institutions and sometimes public services. The facility may cease to provide accommodation or daily structure at the moment when the resident encounters familiar social conditions, practical obligations and new costs. If nobody is clearly responsible for making the next appointment, supplying a relevant clinical summary or checking whether a referred service can receive the person, continuity may depend almost entirely on individual initiative and household resources.

The concept of recovery capital helps interpret this vulnerability. Cloud and Granfield (2008) discuss the personal and social resources available to individuals as they attempt to change substance-use patterns, while recognising that resources and disadvantages are unevenly distributed. Applied carefully, this framework suggests that identical discharge instructions may be far easier to enact for some people than for others. A resident with stable housing, available transport and a supportive relationship may have options unavailable to somebody experiencing conflict, insecure employment or limited funds. The present study does not measure recovery capital or establish which resources individual residents possessed. The concept is useful because it frames continuity as requiring more than personal motivation.

A potential limitation of residential practice is that the institution may develop a plan that is administratively complete but difficult to follow outside its walls. For example, an instruction to seek weekly counselling does not demonstrate that an affordable counsellor has been identified, that the person knows how to arrange an appointment, or that appropriate information can be transferred with consent. The important question is whether the recommendation has been converted into a feasible handover. A practical care transition includes a named destination, an agreed appointment or communication channel, clarity about payment, and an arrangement for re-engagement if the first plan does not work.

This perspective does not support a blanket assumption that all residents need the same intensity of aftercare. People differ in clinical needs, preferences and resources, and any continuing plan should be appropriate rather than automatic. Nor does it establish that the participating centres caused later difficulties; the present outcome data are too incomplete for that conclusion. What the findings establish is that post-discharge status is much less visible than residential activity. The policy question is therefore whether the institutional system has mechanisms to learn what happened after discharge and provide appropriate further help, not simply whether it advertises an aftercare programme.

It is important to resist treating return to use as proof that a person or programme has failed irreversibly. Ongoing engagement can include periods of difficulty, repeated contact and changes in care needs. A responsive service should be able to offer renewed assessment

without requiring that the person fit an idealised narrative of linear progress. This is consistent with a continuity-oriented understanding of treatment and with the article's refusal to infer individual recovery from institutional discharge labels. The potential contribution of the research is to identify the handover between residence and community as a key site where responsibility needs to remain explicit.

8. Recommendations for reliable outcome reporting

A proportionate monitoring architecture would begin with a shared discharge register. Each record should identify the discharge date, planned or unplanned departure category, an internally assigned confidential patient identifier, the clinical professional responsible for the discharge plan, and whether the individual agreed to subsequent contact. These items are process indicators. They should not automatically be converted into judgements of patient motivation. The register should distinguish persons from episodes, permit legitimate re-entry and document loss to follow-up without treating missing information as a negative outcome.

The second stage would define a follow-up window prospectively. Ninety days is used here because it was the follow-up window adopted in the study; it is not established as a universal optimum. Services could additionally define six- and twelve-month checkpoints if resources permit. For each window, the denominator should be those eligible under transparent rules, with explicit exclusions for deaths, transfers or other circumstances if justified and counted. Contact attempts and completed contacts should be separately recorded. A specific instrument for assessing substance use and functioning should be chosen and its limitations explained; the present article does not validate any instrument for the local population.

A third stage would distinguish measured status from the support response. Where a person requests help, experiences a return to use or reports difficulty with housing, employment, distress or relationships, the record should indicate the relevant offer or referral and whether it was received. Where no contact occurs, the service should be able to state how many attempts were made under an ethically acceptable plan and when further attempts ceased. Recontact must respect consent, confidentiality and the risk that revealing a person's treatment history could cause harm. The goal is reliable, patient-centred continuity rather than surveillance for its own sake.

At the institutional reporting stage, every published indicator should display a numerator, denominator, timeframe and operational definition. It should distinguish within-programme outcomes from longer-term outcomes and indicate whether the data were externally verified. An independent audit could sample de-identified records to assess internal consistency without granting researchers or regulators unrestricted access to confidential histories. The paper proposes auditing the *traceability* of reported percentages before comparing their clinical magnitude. This is not a substitute for patient-reported outcomes or rigorous effectiveness research; it establishes a necessary basis for both.

Finally, implementation should be monitored rather than assumed. A policy requiring a ninety-day call may be adopted on paper but poorly carried out; a high contact rate may be achieved without collecting clinically meaningful information. Quality improvement therefore requires feedback on why contact fails, how resident preferences are handled, whether follow-up appointments are affordable, and how staff manage the workload. An

iterative audit-and-feedback process would be more credible than setting targets detached from staffing, confidentiality and referral capacity. These recommendations are derived from the recorded gaps; their acceptability, cost and effects need prospective evaluation.

9. Limitations and study integrity

This article reports only the evidence available from institutional questionnaires and professional interview material. It does not include direct interviews with residents, families, police personnel or regulators. The absence of these perspectives limits the study's ability to interpret consent, coercion, patient experience and help-seeking trajectories from the viewpoint of service users.

The professional excerpts are valuable as attributed interpretations of clinical and managerial problems, but they do not substitute for direct observation or resident testimony. An owner account may suggest a plausible connection between delayed payment and recruitment decisions, but the data do not establish why a particular post was vacant. A psychologist may describe a consent concern without verifying every admission procedure. Comparing professional statements with centre-reported counts can reveal coherence, tension and new questions; it cannot replace direct observation or resident testimony.

There are further limitations concerning time and population. Institutional reporting windows cover the preceding twelve months relative to different questionnaire dates, not a common closed annual cohort. Some entries concern individuals, others admissions or discharge episodes, and these cannot be combined without identifying repeat cases. The exclusion of RC01 from pooled statistics was a prudent response to incompatible totals, but it also means that its distinctive data must not be treated as an additional member of the pooled denominator. These issues prevent prevalence estimates, causal modelling and defensible comparisons of clinical effectiveness.

Several limitations directly delimit this manuscript. The study relies on centre-reported aggregate records and attributed professional accounts. Although the underlying questionnaires and interview materials existed in hard-copy form during fieldwork, this manuscript draws on collated study data rather than an independently audited patient-level archive. The article should therefore be read as an institutional analysis, not as a verified clinical cohort study. Future archiving should retain the original questionnaires, interview records, permissions and ethics materials for editorial review if required.

The population and time denominators are also limited. Fifteen selected centres are not a representative sample of all rehabilitation provision. Entrants, repeat admission episodes, closed episodes and eligible discharges cannot be connected at individual level; the annual reporting windows were not identical. Although the study identifies 459 entrants aged 18–29 among all-age records, the follow-up table does not isolate their outcomes. The article consequently makes no young-adult-specific recovery claims. All results also derive from male entrant records; nothing here demonstrates how services reach women or other populations not represented by the source frame.

Other limitations are substantive rather than merely documentary. The meaning of 'return to use' is unspecified: self-report, clinician judgement, family report and biological testing could yield different results, and individual follow-up times may vary. Because the 510 unassessed

cases cannot be characterised, the missingness mechanism is unknown. The extreme-case bounds communicate identification limits, not likely values or statistical confidence. The attributed professional quotations are not independently verified and cannot substitute for patient testimony. The study also cannot assess whether a reported return to use was followed by renewed engagement, reduction in harm or improved functioning.

Despite these limitations, careful reconstruction has value. It identifies exactly which inferences the reported data can support, where claims exceed evidence, and which documentation practices would make future evaluation stronger. The principal conclusion is about the reliability of "knowledge concerning recovery", not about the effectiveness of specific treatments. This narrower contribution is defensible only if the source status remains transparent throughout the article.

10. Conclusion

The study records make the start of residential treatment more visible than the subsequent course of recovery. Reconstructing the ninety-day follow-up sequence identifies a measurable deficit in outcome ascertainment: fewer than half of eligible discharges had an assessed status. The observed return-to-use percentage applies to those assessed only. It cannot validate or refute provider success claims formulated using unknown populations or periods. Better governance requires each claim to be tied to a defined event, denominator and timeframe, with follow-up coverage stated alongside outcome percentages. This article therefore advances an accountability standard for claims about care, not an estimate of clinical effectiveness. Whether the proposed approach improves services is a question for prospective, ethically approved research using authenticated records.

Declarations

Data availability. The data underlying this article consist of centre-level rehabilitation questionnaires and professional interview material collected for the study. Because the underlying records contain institutionally sensitive information, de-identified aggregate data may be made available by the corresponding author on reasonable request and subject to institutional approval. Source forms should be archived by the research team for editorial review if required.

Ethics and consent. The final submitted version should state the applicable ethics approval, institutional permissions and consent procedures exactly as documented by the research team. No patient-identifying information is reported in this manuscript.

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