

Not Downgraded, But Displaced: Reverse Migration and Unemployment Risk in India

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Abstract

India's March 2020 COVID-19 lockdown triggered mass reverse migration as urban employment collapsed in construction, manufacturing, and informal services, straining rural absorption mechanisms like MGNREGA. Whether reverse migrants experienced lasting labour market downgrading compared to voluntary pre-pandemic migrants remained unclear due to limited nationally representative panel evidence. This study uses the migration module newly added to the 2020–21 PLFS, linking pre-migration activity status, industry, and occupation to current outcomes for working-age migrants (15 to 59). It compares reverse migrants (post-March 2020) to a placebo cohort of pre-pandemic migrants and district-matched non-migrants, using transition matrices, an occupation skill distance metric (NCO-2004), and a 0 to 4 job-quality index with regression and matching methods.

Results show reverse migration does not raise overall employment exit (19.29% vs. 18.41%) but sharply increases exits into unemployment, about eight times the placebo rate (12.32% vs. 1.55%), with robust gaps of 4.5 to 8.8 percentage points ($p < 0.001$) and lower re-employment among previously unemployed workers (77.83% vs. 86.42%). Among those remaining employed, there is no significant occupational skill change or job quality decline. The findings indicate the shock operates on the extensive margin, meaning access to employment, rather than through occupational downgrading, supporting employment absorption focused policy responses.

1. Introduction

India's national lockdown, imposed with little advance warning in March 2020, precipitated a large-scale return of internal migrant workers to their places of origin, a phenomenon subsequently documented in a growing body of research (Rajan, Sivakumar, & Srinivasan, 2020; Khan & Arokkiaraj, 2021; Jesline, Romate, Rajkumar, & George, 2021). The abrupt suspension of urban economic activity removed the livelihood base of a workforce concentrated in construction, manufacturing, and informal urban services (Srivastava, 2020), producing a movement of population that has been characterised in the literature as a “crisis of mobility” (Rajan, Sivakumar, & Srinivasan, 2020) and that was met, in policy terms, by a substantial expansion in demand for work under the Mahatma Gandhi National Rural Employment Guarantee Act (Lokhande & Gundimeda, 2021). What remains comparatively under-examined is a more specific and policy-relevant question: when these reverse migrants eventually re-entered the labour market, did they return to work of the same kind and quality they had left behind, or did the experience of reverse migration leave a lasting mark on their occupational standing and job security?

This question matters because policy responses to the reverse-migration crisis, including expanded MGNREGA allocations, portability schemes for welfare entitlements, and calls for rural skilling and micro-enterprise programmes, were premised on an assumption that displaced migrants would eventually need to be reabsorbed into gainful work, and that the quality of that reabsorption was itself a policy-relevant outcome, not merely the fact of employment or non-employment. Yet most of the existing evidence base is either qualitative and geographically localized, or aggregate and cross-sectional,

comparing national labour force indicators before and after the pandemic without linking individual workers' pre-migration and post-migration circumstances.

This paper addresses that gap using a source that has, to our knowledge, not previously been exploited for this specific purpose: the migration particulars module introduced into India's Periodic Labour Force Survey (PLFS) in the 2020–21 round. This module asks each household member who has changed their usual place of residence to report, among other things, whether the move occurred after March 2020, and, crucially, their usual principal activity status, industry, and occupation before leaving their last usual place of residence. Because the same schedule separately records each person's current activity status, industry, and occupation, PLFS 2020–21 offers a within-person, nationally representative, retrospective panel of exactly the kind needed to study labour market transitions around the reverse-migration episode, without requiring a purpose-built longitudinal survey.

The paper proceeds as follows. Section 2 reviews the empirical literature on COVID-19-induced reverse migration in India and its labour market consequences. Section 3 develops the theoretical framework motivating the paper's hypotheses, drawing on migration, labour market segmentation, and return-migration theory. Section 4 describes the data and key variables. Section 5 sets out the empirical strategy, including the formal specifications used to test each hypothesis. Section 6 presents the results, Section 7 discusses their implications in light of the theoretical framework, and Section 8 concludes.

2. Literature Review

2.1 The scale and character of COVID-19-induced reverse migration

The reverse migration that followed India's 2020 lockdown has been described as one of the largest peacetime population movements in the country's recent history, and a substantial descriptive and qualitative literature has emerged to document it. Comparative case-study work has examined how internal migrants and returnees from Gulf-country employment experienced markedly different institutional responses during their displacement, repatriation, and reintegration, highlighting gaps in the initial government response to internal migrants relative to more coordinated international repatriation efforts (Khan & Arokkiaraj, 2021). A systematic review of the broader literature on migrants' pandemic experience similarly emphasises the compounding of pre-existing precarity, including informal contracts, absent social security, and circular migration patterns that fall outside standard legal and welfare protections, with the acute shock of the lockdown itself (Jesline, Romate, Rajkumar, & George, 2021). District- and state-level accounts converge on a common narrative: workers in construction, manufacturing, and urban services, disproportionately drawn from a small number of origin states (Uttar Pradesh, Bihar, Madhya Pradesh, and Rajasthan prominent among them), lost both income and housing simultaneously and had little choice but to return to their native villages (Rajan, Sivakumar, & Srinivasan, 2020). Srivastava (2020) situates this episode within a longer-run argument that India's mobile informal

workforce had always sat outside the country's social protection architecture, and characterises the pandemic as a potential “inflexion point” that exposed, rather than created, this structural vulnerability.

2.2 Labour market consequences: informalisation and the absorption problem

A second strand of literature moves from the migration event itself to its labour market aftermath. Using the CMIE Consumer Pyramids Household Survey, Chatterjee and Dev (2023) construct the first labour flow charts and transition matrices tracking Indian workers' movement between employment, unemployment, and out-of-labour-force status across the pandemic period, finding that job loss was concentrated among informal and part-time workers even as headline unemployment recovered relatively quickly. Using the same panel data source, Kotia and Sharma (2021) show, via a difference-in-differences event-study design, that within the same industry and district, workers who were informally employed before the pandemic were significantly more likely to lose their jobs in its early phase than formally employed workers. This is direct evidence that informality functioned as a channel of vulnerability rather than merely a background characteristic. A World Bank synthesis of India's pandemic labour market experience reaches a similar conclusion at the aggregate level, noting that while cross-sectional indicators such as the unemployment rate recovered relatively quickly, the recovery was markedly uneven across gender, social group, and pre-pandemic job type (He, Narayan, Olinto, Sinha Roy, & Sarma, 2023). None of this literature, however, links the pre-pandemic job characteristics of a worker to their migration status and subsequent job characteristics at the individual level using a single nationally representative instrument. This is the gap this paper addresses.

2.3 The policy response and its limits: MGNREGA as absorption mechanism

A closely related literature examines MGNREGA's role as the principal formal safety net for returning migrants. Lokhande and Gundimeda (2021) document that roughly 7.5 million seasonal migrant workers sought work under MGNREGA in the months following the lockdown, finding work for an average of around 23 days and recovering only about 28 percent of their pre-pandemic daily earnings. This is evidence that even where an absorption mechanism existed, it only partially compensated for the loss of prior livelihoods. District-level administrative data corroborate this picture: person-days generated under the scheme in migrant-heavy origin states such as Uttar Pradesh, Bihar, and Odisha vastly exceeded pre-pandemic norms in the months immediately following the lockdown. Marchang (2023) connect this evidence to a broader concern about the informalisation of employment during the recovery period, arguing that the speed and scale of job losses among salaried workers reflected weak employment contracts and an absence of portable social security. These are precisely the job-quality dimensions this paper's informality index (Section 4) is designed to measure directly.

2.4 PLFS-based migration research: an emerging but still nascent literature

Because PLFS did not carry a dedicated migration module before the 2020–21 round, PLFS-based migration research is itself a recent development. Using the same 2020–21 migration data exploited here, Mitra and Shrivastav (2024) document broad migration-rate patterns and returnee characteristics at the national level, notably finding that regular wage/salaried workers, not only casual labourers, suffered serious job disruption during the pandemic, a finding that complicates any assumption that reverse migration was primarily a casual-labour phenomenon and motivates this paper's disaggregation of transitions by pre-migration employment category rather than treating “migrant workers” as a homogeneous group. Complementary work using alternative, non-survey data sources, for instance mobile-network visitor location register data, has sought to triangulate the scale of interstate movement during the pandemic against official survey-based estimates (Nizam, Sivakumar, & Rajan, 2022), and recent work using both Census and NSS data over a longer horizon situates the pandemic episode within longer-run internal migration trends in India (Datta & Rajan, 2024). Complementary micro-evidence on the economic role of shorter-duration mobility, predating the pandemic, shows that short-term migration is itself an important consumption-smoothing strategy for rural households (Chandrasekhar, Das, & Sharma, 2015). This finding is consistent with this paper's household risk-diversification framing in Section 3. To our knowledge, no published study has yet used the paired pre-migration/current activity-status fields in PLFS to construct an individual-level, nationally representative transition matrix of the kind proposed here.

2.5 Gender, caste, and the unevenness of the reverse-migration shock

A final strand of literature cautions against treating reverse migrants as a homogeneous population. Evidence from panel studies of returnee cohorts shows that women migrants faced substantially greater barriers to labour market recovery than men over the fifteen months following the first lockdown, even conditional on similar pre-pandemic characteristics. Circular and seasonal migration in India has also long been documented as disproportionately drawing on Scheduled Caste, Scheduled Tribe, and otherwise socially marginalised households (Deshingkar & Farrington, 2009), which motivates this paper's inclusion of social group as a control variable and as a candidate dimension for heterogeneity analysis in later sections. Measurement-oriented work further cautions that survey instruments built around a usual-place-of-residence definition have historically understated the true scale of short-term and circular mobility in India (Keshri & Bhagat, 2012), a concern taken up directly in a companion paper and noted here as a scope condition on how the migrant sample analysed in this paper should be interpreted.

3. Theoretical Framework

This paper's hypotheses draw on six theoretical traditions that, taken together, motivate both the expectation of post-migration labour market downgrading and the specific empirical tests used to detect it. Each is summarised below with an explicit statement of what it predicts for the reverse-migration episode studied here.

3.1 Neoclassical and human-capital foundations of migration

The starting point for any theory of migration in a developing-country dual economy is the Lewis (1954) model of unlimited labour supply, in which surplus agricultural labour moves to a growing modern sector as long as the urban wage exceeds the rural marginal product of labour. Harris and Todaro (1970) refine this into an explicitly probabilistic framework in which migration responds not to the actual urban wage but to the expected urban wage, defined as the observed wage discounted by the probability of finding urban employment, which can rationalise persistent urban unemployment as an equilibrium outcome of rural–urban migration decisions. Sjaastad (1962) complements this macro-level account with a micro-level human-capital framework, treating migration itself as an investment: an individual incurs upfront monetary and psychological costs in exchange for a discounted stream of expected future earnings gains. Together, these frameworks generate a clear prediction for the reverse-migration episode: the March 2020 lockdown constituted an abrupt, economy-wide collapse in the probability of urban employment (and, for many workers, the elimination of urban earnings altogether), which the Harris–Todaro logic predicts should trigger mass return migration, exactly as observed, but these frameworks are silent on what happens to a worker's occupational standing upon return, since they model the migration decision itself rather than its labour market consequences. This is the gap the remaining theoretical traditions below are used to fill.

3.2 The new economics of labour migration: migration as household risk diversification

Stark and Bloom (1985) and the subsequent “new economics of labour migration” (NELM) literature reframe migration as a household-level, rather than purely individual, decision undertaken to diversify income sources against locally correlated (covariate) risk, such as crop failure or local demand shocks, with the migrant's urban earnings functioning as a form of self-insurance and the household as an implicit risk-sharing unit. This framework is particularly well suited to India's circular and seasonal migration streams, which micro-evidence shows are frequently deployed by rural households specifically as a consumption-smoothing strategy (Chandrasekhar, Das, & Sharma, 2015). Under a NELM lens, the COVID-19 lockdown represents not merely an individual labour-supply shock but the simultaneous collapse of a household risk-sharing arrangement across millions of households at once, a systemic rather than idiosyncratic shock, which NELM theory would predict households attempt to buffer through alternative channels (informal borrowing, asset sales, or public employment programmes such as MGNREGA) precisely because the usual insurance mechanism, the migrant's urban income, failed at the same moment for everyone. This motivates the paper's attention, in Section 5, to whether the household's substitute risk-management channel (MGNREGA participation, informal borrowing, or a downgraded job) itself constitutes a lower-quality equilibrium than the arrangement it replaced.

3.3 Segmented labour markets and the Indian informality frame

Piore's (1979) dual/segmented labour market theory holds that labour markets are not unified but split into a primary segment (stable, protected, well-paid employment) and a secondary segment (casual, insecure, poorly paid employment), with structural barriers to mobility between the two that are largely independent of a worker's own productivity or effort. Although originally developed to explain migrant labour absorption in advanced industrial economies, the theory has a direct domestic analogue in Breman's (1996) concept of "footloose labour": a mobile, casually contracted workforce, pushed out of agriculture, that circulates between rural and urban secondary-sector jobs without secure attachment to either. This framework generates the paper's central occupational prediction: because entry into India's primary labour market segment (regular, socially secured, contractually protected employment) typically requires accumulated firm-specific tenure, network access, or credentialing that a sudden, involuntary return migration disrupts, reverse migrants should be expected to re-enter predominantly through the secondary segment even when their pre-migration job was in the primary segment. This is precisely the occupational and job-quality downgrading tested by Hypotheses H2 and H3 in Section 5.

3.4 Migration networks and cumulative causation

Massey et al. (1993) synthesise a network theory of migration in which migration flows become self-perpetuating: once a critical mass of migrants from a given origin community has established itself at a destination, the accumulated social capital, including information about jobs, employer contacts, and informal accommodation arrangements, lowers the cost and risk of migration for subsequent movers from the same community, a process termed cumulative causation. This theory implies that a reverse migrant's job-search efficiency at the destination was, prior to the pandemic, substantially a function of an established local migrant network; the lockdown's disruption of that network (through employer closures, the scattering of co-migrants, and the erosion of accumulated informal social capital at the destination) provides a distinct and independent channel, separate from the individual's own skills or effort, through which re-employment quality could deteriorate even after urban labour demand recovers. This motivates the paper's attention, in the heterogeneity analysis, to whether workers with a stated intention to re-migrate (Section 5.5) are disproportionately those whose pre-existing destination networks are inferred to remain intact.

3.5 Return migration theory

Cerase (1974), in a study of Italian return migrants, proposes an influential four-way typology of return migration according to the migrant's motivation and degree of success: "return of failure" (an involuntary return driven by an inability to establish oneself at the destination), "return of conservatism" (a planned return after accumulating savings sufficient to achieve a pre-set target at home), "return of retirement", and "return of innovation" (a return in which the migrant deploys new skills, capital, or ideas acquired abroad to transform their position at home). Gmelch (1980) extends this typology by emphasising that the economic outcome of any return migration episode is jointly determined by the migrant's own resources and the absorptive capacity of the origin-area economy into which they return. Applying this typology to the present setting, the pandemic-triggered reverse migration studied in this paper is, almost by construction, closer to Cerase's "return of failure" category than to "return of conservatism" or "return

of innovation”: it was involuntary, unplanned, and not preceded by the accumulation of savings or skills intended for productive redeployment at origin. Return-migration theory therefore predicts systematically worse reintegration outcomes for this cohort than for migrants whose return was voluntary and planned. This is the theoretical basis for using the pre-2020 (voluntary) migrant cohort as the placebo comparison group in this paper's empirical design, and for Gmelch's absorptive-capacity argument specifically motivating the attention paid to MGNREGA and local labour-market conditions as moderators of the downgrading outcome.

3.6 Scarring effects and the persistence of labour market disadvantage

A distinct literature in labour economics documents that spells of unemployment or involuntary job loss leave a persistent “scar” on subsequent labour market outcomes, including lower wages, lower job quality, and higher re-unemployment risk, even after the worker re-enters employment, above and beyond what can be explained by observed worker characteristics (Arulampalam, Gregg, & Gregory, 2001). This paper imports the scarring concept, developed primarily for unemployment spells, as an analogous mechanism for migration-induced labour market disruption: reverse migration functioned, for many workers, as an involuntary and unplanned interruption comparable in its labour market consequences to an unemployment spell, and scarring theory predicts that this interruption should leave a measurable, persistent penalty on subsequent job quality rather than a transient effect that disappears once the worker is re-employed. This is the theoretical basis for Hypothesis H3, and for testing job quality (Section 5.4) rather than mere re-employment status as the outcome of primary interest.

3.7 Synthesis and hypotheses

Table 1 below and the formal empirical specifications in Section 5 operationalise the predictions above. In summary: the Harris–Todaro and NELM frameworks (Sections 3.1–3.2) jointly explain why reverse migration occurred at the scale and speed observed, and motivate treating it as a systemic rather than idiosyncratic household shock; segmented labour market theory and the footloose-labour concept (Section 3.3) predict occupational and job-quality downgrading upon re-entry (H2, H3); network and cumulative-causation theory (Section 3.4) and Cerase's return-migration typology (Section 3.5) jointly predict that this downgrading should be most severe for economically (rather than socially) motivated migrants and should generate a continued propensity to re-migrate rather than settled reintegration (H4); and scarring theory (Section 3.6) provides the mechanism by which a temporary shock is expected to leave a persistent, rather than transient, mark on job quality. This is the central empirical claim tested throughout Section 6.

Table 1
Hypotheses, theoretical basis, and empirical tests

Hypothesis	Statement	Theoretical basis	Empirical test
H1	Reverse migrants are more likely than the placebo group to transition from employment into unemployment or out-of-labour-force status, rather than back into self-employment or regular wage work.	Harris–Todaro (1970); Cerase (1974) return-of-failure typology (Sections 3.1, 3.5)	Weighted pre/post transition matrix and design-based chi-squared test (Section 5.2)
H2	Conditional on remaining employed, reverse migrants show a larger pre/post occupational skill-distance than the placebo group.	Piore (1979) segmented labour markets; Breman (1996) footloose labour (Section 3.3)	NCO skill-distance measure and Kolmogorov–Smirnov test (Section 5.3)
H3	Reverse migrants' current jobs score lower on the informality index than jobs held by non-migrants in the same district and industry.	Arulampalam, Gregg, & Gregory (2001) scarring effects; Piore (1979) (Sections 3.3, 3.6)	OLS with district fixed effects and ATT matching estimator (Section 5.4)
H4	Workers who experienced downward transitions are more likely to report an intention to re-migrate than those who did not.	Stark & Bloom (1985) NELM; Massey et al. (1993) cumulative causation (Sections 3.2, 3.4)	Logit model of return intention on the downgrading indicator (Section 5.5)

4. Data and Variables

The analysis draws on unit-level data from the 2020–21 round of India's Periodic Labour Force Survey (PLFS), conducted by the National Statistical Office. This round is used because it is, to date, the only PLFS round to carry a dedicated migration particulars module alongside the survey's standard demographic, usual-activity, and current-weekly-activity modules. This is the combination required to construct the pre-migration/post-migration comparison at the centre of this paper. The migration module was completed only for household members who reported that their present place of enumeration differed from their last usual place of residence; this sub-population, restricted further to the working-age band (15–59 years), constitutes the paper's analytic sample. All estimates use PLFS's person-level sampling weights together with its stratified multi-stage design (primary sampling unit and stratum identifiers), so that reported statistics are representative at the intended level of aggregation rather than raw sample proportions. Table 2 summarises the key variables drawn from the survey and how each is used in the analysis; full derivation details are provided in the accompanying do-file documentation.

Table 2
Key PLFS variables selected for the analysis

Variable (analysis name)	Description	PLFS source	Coding used in analysis
Reverse Migrant / Premig Migrant / Non-migrant	Group indicators: moved after March 2020, moved before March 2020 (placebo), or did not migrate	Migration particulars module	Constructed from the migration-status and migration-timing items
Age, Sex	Demographic controls	Household roster (demographic block)	Age in years; sex (male/female/transgender)
Education	General educational level, used as a control and for skill-based heterogeneity	Household roster (education items)	Collapsed to: below secondary / secondary-higher secondary / graduate and above
Social group	Social group (ST/SC/OBC/Others), used as a control	Household characteristics block	4-category classification
Religion	Hindu, Muslim and Others used as control	Household characteristics block	3-category classification
District, State	Geographic identifiers, used for fixed effects and clustering	Sample identification block	As coded in the frame
Survey weight, FSU, Stratum	Sampling weight and design variables	Sample identification block	Used to svyset the data for all weighted estimates
Pre-status (3-way / 5-way)	Usual principal activity status before leaving last usual place of residence	Migration particulars module	Recoded to employed/unemployed/out-of-labour-force, and separately to a 5-way status classification
Pre-industry, Pre-occupation	Industry and occupation of the pre-migration job	Migration particulars module	2-digit industry and 3-digit occupation codes, standard national classifications
Migration reason	Reason for leaving last usual place of residence	Migration particulars module	Collapsed to 4 categories: economic distress / family-education-marriage / shock-displacement / health-housing-other
Return intent	Whether the respondent intends to move out again, and to where	Migration particulars module	3 categories: return to origin / move elsewhere / stay

Variable (analysis name)	Description	PLFS source	Coding used in analysis
Post status (3-way / 5-way)	Current usual principal activity status	Usual principal activity block	Same recoding scheme as pre-status, for direct comparability
Post industry, post-occupation	Industry and occupation of the current job	Usual principal activity block	Same classification as the pre-migration equivalents
Enterprise type, Firm size, Contract type, social security	Job-quality attributes of the current job	Usual principal activity block	Combined into the 0–4 informality index (Section 5)

5. Methodology

5.1 Sample and comparison groups

The estimation sample consists of three mutually exclusive groups of working-age (15–59) household members: (i) reverse migrants, who report having changed their usual place of residence after March 2020; (ii) a placebo group of migrants whose relocation predates March 2020, included to test whether any downgrading found is specific to the pandemic-period cohort rather than a generic feature of migration; and (iii) a non-migrant comparison group of household members with no reported change of residence, matched at the district level, used to benchmark post-migration job quality against workers who did not migrate at all. This three-group design follows directly from the return-migration typology in Section 3.5: the placebo group approximates Cerase's voluntary “return of conservatism” category, while the treatment group approximates the involuntary “return of failure” category, so that any statistically significant difference between the two groups' outcomes can be attributed to the involuntary, shock-driven character of the pandemic-period moves rather than to migration per se.

5.2 Transition-matrix approach to downward mobility (H1)

Following the transition-matrix approach used elsewhere in the Indian labour-flow literature (Chatterjee & Dev, 2023), each individual's usual principal activity status is classified into a three-way scheme: employed (E), unemployed (U), or out of the labour force (O), separately for the pre-migration period (from the migration module) and the current period (from the usual-activity module). For group $g \in \{\text{reverse migrant, placebo}\}$, the estimated transition probability from pre-migration state j to current state k is

$$p_{jk}^g = \Pr(\text{Post}_i = k \mid \text{Pre}_i = j, \text{Group}_i = g) = N_{jk}^g / N_{j\cdot}^g \quad (1)$$

where N_{jk} is the (survey-weighted) count of individuals in group g observed in pre-migration state j and current state k , and $N_{j\cdot}$ is the corresponding row total within group g . Hypothesis H1 is tested by

comparing the estimated transition matrices p_{jk} for the treatment and placebo groups using a design-based chi-squared test of independence,

$$\chi^2 = \sum_{j,k} (O_{jk} - E_{jk})^2 / E_{jk} \quad (2)$$

with observed counts O_{jk} taken from the treatment group and expected counts E_{jk} computed from the placebo group's transition shares applied to the treatment group's row totals. H1 is supported if the treatment group shows a significantly higher share of transitions from E into U or O than the placebo group, with particular attention to the E→U and “regular wage→casual” cells in the finer five-way classification reported alongside the headline three-way matrix.

5.3 Occupational skill-distance measure (H2)

Occupation is recorded in both the migration module and the usual-activity module using the National Classification of Occupations (NCO-2004). Each occupation's 1-digit major group is mapped to an ordinal skill level $Skill \in \{1, 2, 3, 4\}$, following the standard correspondence between NCO major groups and skill levels used in the international occupational-classification literature. For each individual with a codeable occupation in both periods, the skill-distance measure is

$$Skill\ Distance_i = | Skill_i^{post} - Skill_i^{pre} | \quad (3)$$

The distributions of $Skill\ Distance_i$ for the treatment and placebo groups are compared using a two-sample Kolmogorov–Smirnov test,

$$D = \sup_x | F_T(x) - F_C(x) | \quad (4)$$

where F_T and F_C are the empirical cumulative distribution functions of Skill Distance for the treatment and placebo (comparison) groups respectively. H2 is supported if D is statistically significant and the treatment group's distribution is stochastically shifted toward larger absolute skill changes, particularly downward changes (post-migration skill level below pre-migration skill level).

5.4 Job-quality regression and matching estimator (H3)

Current job quality is measured with a composite index built from four binary job-quality indicators available in the usual-activity module: formal enterprise type, large-firm employment, written job contract, and social security eligibility, summed into an index running from 0 (least formal) to 4 (most formal):

$$Informality\ Index_i = \sum_{m=1}^4 1(Formal\ Attribute_{im} = 1) \quad (5)$$

The primary specification regresses this index on treatment status with demographic controls, pre-migration activity status, and district fixed effects:

$$Informality\ Index_i = \beta_0 + \beta_1 Reverse\ Migrant_i + \beta_2' X_i + \gamma_k + \delta_d + \varepsilon_i \quad (6)$$

where X_i is a vector of individual controls (age, sex, education, social group, religion), γ_k are pre-migration activity-status fixed effects (indexed by the 3-way status j used in Section 5.2), and δ_d are district fixed effects absorbing time-invariant local labour-market conditions. Because assignment to reverse-migrant status is plausibly non-random, β_1 is supplemented, not replaced, by an average-treatment-effect-on-the-treated (ATT) estimator obtained through nearest-neighbour covariate matching on age, sex, education, and pre-migration industry:

$$\hat{\tau}_{ATT} = (1/N_T) \sum_{i \in T} [Y_i - \sum_{j \in C} w_{ij} Y_j] \quad (7)$$

where T is the treatment (reverse-migrant) group, C is the matched comparison pool (non-migrants), and w_{ij} are the matching weights assigned to comparison unit j as a match for treated unit i . H3 is supported if both β_1 and $\hat{\tau}_{ATT}$ are negative and statistically significant, indicating lower job quality for reverse migrants than for otherwise comparable non-migrants.

5.5 Heterogeneity by migration reason and the return-intention model (H4)

Migration reason is collapsed into four categories (economic distress, family/education/marriage, shock/displacement, health/housing/other). The transition-matrix comparison in Section 5.2 is re-estimated separately within the economic-distress and family/education/marriage subgroups of the treatment group, testing whether downgrading (as defined below) is concentrated among economically motivated migrants, consistent with the return-migration and cumulative-causation predictions in Sections 3.4–3.5. A binary downgrading indicator is defined as

$$Downgraded_i = 1\{Post_i \in \{U, 0\} \mid Pre_i = E\} \cup 1\{Post_i = Casual \mid Pre_i = RegularWage\} \quad (8)$$

The association between downgrading and stated return intention is estimated using a logit specification,

$$\text{logit}[Pr(\text{Return Intent}_i = 1)] = \theta_0 + \theta_1 \text{Downgraded}_i + \theta_2' X_i + \varepsilon_i \quad (9)$$

with $\text{Return Intent}_i = 1$ if the respondent reports an intention to return to their origin or move elsewhere, and 0 if they report an intention to stay. H4 is supported if θ_1 is positive and statistically significant, indicating that workers who experienced downward transitions are more likely to report an intention to re-migrate. This is consistent with reverse migration functioning, per Section 3.2, as a temporary risk-management adjustment rather than a settled reintegration.

5.6 Limitations

Three limitations should be stated plainly. First, the PLFS migration module is administered as part of a single cross-sectional interview; the “pre-migration” data are retrospective self-reports collected at the same interview as the “post-migration” data, and are therefore subject to recall error and possible ex post rationalisation regarding activity status several months or years in the past. Second, assignment to

reverse-migrant status is not random, so despite the matching estimator in Section 5.4, the results throughout this paper should be interpreted as descriptive and associational rather than strictly causal. Third, the NCO major-group-to-skill-level correspondence used to construct the skill-distance measure (Section 5.3) is a standard simplification, and results using this measure are reported alongside the raw occupation-transition tables in the appendix as a robustness check.

6. Results

6.1 H1: Reverse migration and the risk of unemployment

Among household members employed prior to migration, the overall probability of leaving employment by the time of the survey is almost identical for reverse migrants and for the pre-2020 placebo group (19.29 percent versus 18.41 percent). The two groups diverge sharply, however, in where that exit leads. Reverse migrants who leave employment land in unemployment, actively seeking work and not finding it, at roughly eight times the rate of placebo migrants (12.32 percent versus 1.55 percent), while placebo migrants who exit employment are far more likely to move into out-of-labour-force status instead. The same asymmetry appears from the other direction: among those unemployed prior to migration, reverse migrants have a markedly lower re-employment rate (77.83 percent versus 86.42 percent) and a much higher rate of remaining unemployed (20.62 percent versus 8.57 percent).

Table 3 summarises the formal tests of this pattern. A simple linear probability model of the probability of transitioning specifically into unemployment, conditional on pre-migration employment, returns a raw gap of 10.8 percentage points between reverse migrants and the placebo group. This estimate is stable under demographic controls (sex, age, education, caste, religion), falling only to 8.8 points, and barely moves when district fixed effects are added (8.6 points), indicating that the effect is not explained by reverse migrants disproportionately settling in weaker local labour markets. A probit specification, more appropriate given the outcome's low base rate (1.7 percent among placebo migrants), returns a smaller but still highly significant average marginal effect of 4.5 percentage points, essentially unchanged when district fixed effects are added. Every specification is significant at p less than 0.001. By contrast, a placebo regression of general employment exit (not specifically unemployment) on reverse-migrant status returns a small, statistically insignificant coefficient (0.9 percentage points, $p = 0.670$), confirming that the effect is specific to the destination of labour market exit rather than the fact of exit itself.

Table 3

H1: Probability of transitioning into unemployment, conditional on pre-migration employment

Specification	Coefficient / AME	SE	p	N
Raw group difference (unadjusted)	0.1077	0.0165	< 0.001	20,534
LPM, demographic controls	0.0882	0.0157	< 0.001	20,534
LPM, demographic controls + district FE	0.0864	0.0133	< 0.001	20,534
Probit AME, demographic controls	0.0452	0.0078	< 0.001	20,529
Probit AME, demographic controls + district FE	0.0464	0.0079	< 0.001	19,878
Placebo: general employment exit (no controls)	0.0088	0.0207	0.670	20,534
<i>Note. All specifications restrict the sample to persons employed prior to migration, comparing reverse migrants (moved after March 2020) to the placebo group (moved before March 2020). LPM and probit specifications include controls for sex, age, education, caste, and religion; district fixed effects are added where indicated. The placebo row regresses a general employment-exit indicator (unemployed or out of the labour force) on reverse-migrant status with no controls, reported for comparison. All survey-weighted (svy) estimates use PLFS sampling weights and design variables.</i>				

Read together, these results support H1 in a more specific form than originally stated: reverse migration does not raise the probability of leaving employment in general, but it substantially raises the probability that any such exit results in unemployment rather than a voluntary withdrawal from the labour force. The result is robust across linear and non-linear functional forms, survives demographic controls, and is unaffected by local labour-market conditions. This is consistent with the scarring mechanism proposed in Section 3.6 and with the Harris-Todaro logic in Section 3.1: an abrupt collapse in the probability of urban re-employment, rather than a general disruption of livelihoods, is the specific channel through which reverse migration harms workers who were employed before the shock.

6.2 H2: Occupational skill distance conditional on continued employment

Among reverse migrants and placebo migrants who remained employed in both the pre- and post-migration periods, a two-sample Kolmogorov-Smirnov test finds no evidence that the two groups differ in the magnitude of occupational skill change ($D = 0.0148$, $p = 0.978$). This null result holds for both directions of the test, and the sample retained for this comparison is not small, so the result is unlikely to reflect a lack of statistical power (Table 4).

One feature of this subsample is worth reporting alongside the null result. Only 29.6 percent of reverse migrants who were employed pre-migration remain employed in both periods and have a codeable occupation in each, compared with 14.2 percent of placebo migrants, meaning reverse migrants are retained into this comparison at roughly twice the rate. Because H1 shows that reverse migrants are

specifically more likely to fall into unemployment, the population that survives into the H2 comparison, those who kept working throughout, is a selected subset of an already smaller group, and this should be read as a comparison conditional on continued employment rather than an unconditional test (Table 4).

Table 4
H2: Kolmogorov-Smirnov test of occupational skill-distance distributions

Group	N retained	Retention rate	KS statistic (D)	P
Reverse migrants	1,092 of 3,683	29.6%		
Placebo migrants	15,634 of 110,315	14.2%		
Combined K-S test	16,726		0.0148	0.978
<i>Note. Sample restricted to persons employed in both the pre-migration and post-migration periods, with a codeable occupation in each. Retention rate is the share of each group's pre-migration-employed population that remains in this restricted comparison.</i>				

Taken together with the H1 results, this null finding sharpens rather than weakens the paper's overall account of reverse migration's labour market consequences. The evidence points to an extensive-margin effect operating through employment status (H1: a higher risk of unemployment specifically), rather than an intensive-margin effect operating through the kind of job held conditional on staying employed (H2: no detectable change in occupational skill level). Reverse migrants who keep working do not appear to trade down into markedly different occupations; the labour market cost of reverse migration falls instead on whether they are working at all.

6.3 H3: Job quality conditional on continued employment

An initial regression of the informality index on reverse-migrant status, using the full sample of currently employed persons (N = 92,734) rather than the subsample restricted to continuous pre- and post-migration employment, returns a negative and highly significant coefficient (-0.098, $p < 0.001$), consistent with H3 as originally stated. However, this specification does not isolate the intensive-margin comparison H3 is meant to test: because it conditions only on current employment, and H1 shows that reverse migrants and placebo migrants differ systematically in their pre-migration-to-post-migration employment transitions, part of this estimate plausibly reflects compositional differences in who remains in the employed pool at all, rather than a clean measure of job quality conditional on having stayed employed throughout (Table 5).

Restricting the sample to persons employed in both periods, exactly mirroring the H1 and H2 samples, sharply changes the result. The aligned regression coefficient falls to -0.038 and loses significance entirely ($p = 0.422$). A nearest-neighbour matching estimator on the same aligned sample tells the same story: with a limited covariate set (age, sex, education, industry), the average treatment effect on the treated is -0.016 ($p = 0.687$); adding the confounders omitted from that first attempt, pre-migration activity status and caste, moves the estimate to -0.064 but it remains insignificant ($p = 0.141$). The

regression and matching estimates, once computed on the same aligned sample with a comparable set of controls, now agree closely in both sign and magnitude, and neither reaches significance.

Table 5
H3: Job quality (informality index) regression and matching estimates

Specification	Coefficient	SE	p	N
Regression, full sample	-0.0980	0.0273	< 0.001	92,734
Regression, aligned sample (pre-employed only)	-0.0381	0.0474	0.422	20,534
Matching (age, sex, education, industry only)	-0.0162	0.0401	0.687	20,534
Matching, full covariates (+ pre-migration status, caste)	-0.0639	0.0433	0.141	20,534
<i>Note. The full-sample regression conditions only on current employment and is reported for comparison; it does not restrict to persons employed in both the pre- and post-migration periods. All other rows restrict the sample to persons employed prior to migration and compare reverse migrants to the placebo group, mirroring the H1 and H2 samples. Regression specifications control for sex, age, education, caste, religion, pre-migration activity status, and district; the first matching specification balances on age, sex, education, and industry only, the second adds pre-migration activity status and caste.</i>				

The full-sample estimate should therefore not be read as the paper's primary test of H3. Once the comparison is correctly restricted to workers who remained employed throughout, as it must be to isolate job quality from the employment-status effect already established under H1, there is no evidence that reverse migrants hold lower-quality jobs than placebo migrants. This completes a consistent pattern across the first three hypotheses tested: reverse migration's labour market cost operates through the extensive margin, a substantially elevated risk of unemployment (H1), rather than through the intensive margin of occupational skill (H2) or job quality (H3) conditional on remaining employed. Rather than three independent findings, this is a single, specific mechanism observed from three angles.

6.4 H4: Migration reason and return intention

H4 was tested in two parts: whether the unemployment-transition risk identified under H1 is concentrated among economically motivated reverse migrants rather than those who moved for family, education, or marriage reasons, and whether workers who experienced a downward transition are more likely to report an intention to move again.

On the first part, restricting to reverse migrants employed prior to migration, economic-distress migrants show a somewhat higher unemployment-transition rate than family and marriage migrants (13.13 percent versus 9.57 percent), but a regression testing this gap directly returns a small, non-significant coefficient (0.0355, SE 0.0509, $p = 0.486$). This should be read as an inconclusive rather than a confident null: the family and marriage subgroup contains only 176 unweighted observations in the relevant cell, and a gap of this size is plausibly underpowered at that sample size rather than genuinely absent. A

related comparison, not part of the formal test but worth noting descriptively, is the destination of labour market exit: among those employed pre-migration, family and marriage migrants who leave employment move overwhelmingly into out-of-labour-force status rather than unemployment (27.8 percent versus 2.0 percent for economic-distress migrants), consistent with this subgroup's exits reflecting domestic transitions rather than displacement. Percentages conditioned on the pre-migration unemployed subgroup are not reported for either reason category, as the relevant cells contain as few as 2 to 16 unweighted observations and are not reliable at that scale (Table 6).

Table 6
H4a: Unemployment-transition risk by migration reason, among reverse migrants employed pre-migration

Group	→ Unemployed	N (unweighted)	Regression gap
Economic distress (reason4 = 1)	13.13%	645	0.0355
Family / education / marriage (reason4 = 2)	9.57%	176	(SE 0.0509, p = 0.486)
<i>Note. Regression gap is the coefficient on an economic-distress indicator from a design-based regression of the unemployment-transition outcome on reason category, restricted to reverse migrants employed pre-migration with a codeable reason in one of the two categories shown (N = 821). Strata containing a single sampling unit are centred at the overall mean (svyset ..., singleunit(centered)).</i>			

On the second part, three specifications, a logit and a linear probability model using the unemployment-specific downgrading measure consistent with H1's findings, and a logit using the broader downgrading definition originally specified in Eq. 8, all return small, non-significant coefficients, and the two downgrading definitions do not even agree in sign (-0.22 versus + 0.15). Unlike the reason-heterogeneity test above, this result does not appear to be a power problem: the primary specification retains 1,347 observations and produces a tightly estimated null (95 percent confidence interval for the average marginal effect: -0.19 to 0.08), and the broader-definition specification, with 3,109 observations, is similarly precise (Table 7). Workers who experienced a downward labour market transition are not more likely, in this data, to report an intention to migrate again.

Table 7
H4b: Downgrading and stated intention to migrate again

Specification	Coefficient / AME	SE	P	N
Logit, unemployment-specific downgrading (coef.)	-0.2185	0.294	0.457	1,347
Logit, unemployment-specific downgrading (AME)	-0.0512	0.069	0.457	1,347
Logit, broad downgrading, Eq. 8 definition (AME)	+ 0.0308	0.049	0.533	3,109
LPM, unemployment-specific downgrading	-0.0504	0.069	0.466	1,347
<i>Note. Sample restricted to reverse migrants; unemployment-specific specifications further restrict to those employed pre-migration. All specifications control for sex, age, education, caste, and religion. Return-intention outcome equals 1 if the respondent intends to return to their origin or move elsewhere, and 0 if they intend to stay.</i>				

H4 is therefore not supported as originally specified. The reason-heterogeneity component (H4a) returns an inconclusive null, likely underpowered given the small family and marriage subgroup, while the return-intention component (H4b) returns a precise null across three specifications and two different downgrading definitions. One statistically robust pattern does emerge from the return-intention models, women and older reverse migrants report lower intention to migrate again, consistent with the broader pattern in this data of female exits concentrating in out-of-labour-force status rather than continued labour-market engagement, though this falls outside H4's original scope and is noted here as a candidate for a separate heterogeneity discussion rather than a test of the hypothesis itself.

6.5 Summary across hypotheses

Across all four hypotheses, the evidence identifies a single, specific mechanism rather than a general pattern of downward mobility. Reverse migration substantially raises the risk of falling into unemployment among workers who were employed before migrating (H1: 4.5 to 8.8 percentage points depending on specification, robust to demographic controls and district fixed effects), but shows no detectable effect on occupational skill level (H2), job quality (H3), or stated re-migration intention (H4b) among those who remain employed. The one heterogeneity test with a plausible, if inconclusive, positive signal, a larger unemployment-transition gap among economically motivated migrants (H4a), is likely limited by sample size rather than ruled out. Taken together, these results indicate that reverse migration's labour market cost operates almost entirely through the extensive margin, whether a worker remains employed at all, rather than through the type or quality of the job they hold if they do.

7. Discussion

7.1 An extensive-margin, not intensive-margin, shock

The consistent pattern across H1 through H4 is that reverse migration's labour market cost is concentrated almost entirely on whether a worker remains employed at all, not on the kind or quality of work available to those who do. This is a more specific claim than the literature reviewed in Section 2 has generally made. Accounts of the reverse-migration crisis (Rajan, Sivakumar, & Srinivasan, 2020; Srivastava, 2020) and of pandemic-era informalisation (Marchang, 2023) largely describe a general deterioration in migrant workers' circumstances, without distinguishing whether that deterioration operates through job loss itself or through the terms of subsequent re-employment. The results here suggest the former channel dominates: workers who kept working, whatever the terms, look statistically indistinguishable from voluntary pre-2020 migrants on occupational skill (H2) and job quality (H3), while the risk of falling into unemployment specifically, rather than exiting the labour force voluntarily, is where the entire measurable effect resides.

This pattern maps cleanly onto two of the theoretical frameworks developed in Section 3. The Harris-Todaro logic (Section 3.1) treats migration as governed by the probability of finding urban employment; an abrupt, economy-wide collapse in that probability, rather than a change in the terms of employment for those who do find it, is exactly what an unemployment-specific effect implies. Scarring theory (Section 3.6) predicts that an involuntary labour market interruption leaves a persistent penalty on subsequent outcomes; the data indicate that penalty is paid primarily at the point of re-entry into employment, rather than as a discount applied to the job eventually obtained. Segmented labour market theory (Section 3.3) fares less well as an explanation of the specific pattern observed: it predicts that workers excluded from the primary segment would be pushed into lower-quality secondary-segment work, but H2 and H3 find no evidence of this conditional on remaining employed. One interpretation, worth stating cautiously given the associational nature of the design, is that segmentation in this setting operates as a barrier to employment itself rather than as a sorting mechanism across job qualities, workers either clear the bar into some form of employment or they do not, and the terms of that employment do not appear to differ systematically by migration status once they do.

7.2 Why the return-migration typology is only partly borne out

Cerase's (1974) return-migration typology motivated treating the placebo group of pre-2020 migrants as an approximation of voluntary, planned return, against which the involuntary, shock-driven reverse-migrant cohort could be compared. The unemployment-specific results (H1) are consistent with this framing: the gap between the two groups is large and robust. The null results for return intention (H4b), however, complicate a simple reading of the typology. If reverse migration were purely a "return of failure" in Cerase's sense, one might expect workers who experienced a downward transition to express a stronger intention to move again in search of better circumstances, yet no such association is detectable, and the estimate is precise enough to rule out a moderate-sized effect in either direction. A plausible explanation is that the decision to migrate again, unlike the initial reverse-migration event, is governed less by an individual's realised labour market outcome and more by household-level and structural constraints, the state of destination-area labour demand, the cost of a repeat move, or the

network disruption described in Section 3.4, none of which this analysis can isolate. This is a limitation of the design rather than a rejection of the underlying theory, and it points toward a data requirement, ideally panel data following the same individuals across multiple rounds, that PLFS's cross-sectional structure cannot meet.

7.3 Policy implications

Two implications follow reasonably directly from the extensive-margin finding. First, policy responses aimed at reverse migrants, MGNREGA expansion prominent among them (Lokhande & Gundimeda, 2021), are targeting broadly the right margin: the evidence here supports the view that the binding constraint for this population is access to any employment, not the quality of employment once obtained. This does not imply the terms of MGNREGA work are adequate, only that the analysis does not find a distinct, additional job-quality penalty layered on top of the employment-access problem that would call for a different instrument. Second, the reason-based heterogeneity result (H4a), while inconclusive given its sample size, points toward economically motivated migrants as the plausible highest-risk subgroup for continued unemployment; if confirmed with a larger sample, this would support targeting active labour market programmes, job-matching services, portable social security, at economic migrants specifically, rather than at reverse migrants as an undifferentiated population defined solely by the fact and timing of their move.

7.4 Limitations

Beyond the data-level limitations already noted in Section 5.6, three broader limitations bear on how these results should be read. First, the analysis is associational: reverse-migrant status is not randomly assigned, and while the placebo comparison group and the demographic and geographic controls address the most salient sources of confounding, unobserved differences between workers who happened to move before versus after March 2020 cannot be fully ruled out. Second, several subgroup comparisons, most notably the reason-based heterogeneity test in H4a, are constrained by small cell sizes once the treatment group is split further; these should be treated as suggestive rather than conclusive pending a larger or pooled sample. Third, and most fundamentally, this paper's entire sample is drawn from workers who were successfully classified as migrants under PLFS's usual-place-of-residence definition. This definition has long been argued to miss circular and short-duration migrants, precisely the population most associated with casual and precarious work (Deshingkar & Farrington, 2009; Keshri & Bhagat, 2012). If the workers excluded from the present sample differ systematically from those included, in the direction of even greater labour market precarity, the results reported here likely understate rather than overstate the true labour market cost of reverse migration.

8. Conclusion

This paper set out to test whether India's COVID-19-era reverse migrants experienced a measurable deterioration in labour market position relative to voluntary, pre-pandemic migrants, using the individual-level pre-migration and post-migration activity status fields newly available in PLFS's 2020-21 migration

module. The evidence supports a specific, rather than general, version of that claim. Reverse migrants who were employed before migrating face a substantially and robustly elevated risk of falling into unemployment, roughly 4.5 to 8.8 percentage points depending on specification, a result that survives demographic controls, district fixed effects, and both linear and non-linear estimators. By contrast, conditional on remaining employed, there is no detectable difference between reverse migrants and voluntary migrants in occupational skill level, job quality, or stated intention to migrate again. The labour market cost of reverse migration, in this data, is paid almost entirely at the point of employment access, not through the terms of employment for those who secure it.

This finding refines rather than contradicts the existing literature's broader narrative of migrant vulnerability during the pandemic; it identifies the specific margin on which that vulnerability operated, information that matters for how absorption policies are targeted and evaluated. It also leaves open questions this cross-sectional design cannot answer: how persistent the unemployment risk documented here proves to be over a longer horizon, whether the same pattern holds among the circular and short-duration migrants excluded from PLFS's usual-place-of-residence definition, and what specifically explains the absence of an association between labour market downgrading and the intention to migrate again. Future work combining panel data on reverse migrants with supplementary, definition-independent approaches to migration measurement would be well placed to address each of these in turn.

Declarations

Competing Interests:

There is no competing interest

Author Contribution

The S.H. formulated the idea, and carried out the calculations. In addition, he gathered secondary data and verified the analytical methods.

Availability of Data

The raw dataset utilised for this study is accessible from the website of the Ministry of Statistics and Programme Implementation, Government of India (<https://microdata.gov.in/nada43/index.php/home>).

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