

Introduction

In China, over 170 million rural workers migrate to cities for employment, often leaving their spouses, children, and elderly relatives behind in origin villages (1,2). These left-behind partners run the rural economy: they become the sole decision-makers for farming, childcare, and elderly care. If their mental health suffers, the consequences ripple across the entire household (3). While a substantial body of research examines the consequences of migration for migrant workers themselves, and a growing literature studies the effect on left-behind children (4,5), the mental health of left-behind spouses remains understudied. The few existing studies rely on cross-sectional associations and lack causal identification strategies, making it difficult to determine whether migration itself causes mental health deterioration or whether households with worse mental health select differently into migration (3,6).

In 2008, the Global Financial Crisis (GFC) struck China's export-manufacturing sector, causing an estimated 20 to 49 million migrant workers to lose their jobs. Many were forced to return to their rural villages (7–9), creating a sudden and large-scale natural experiment in which migrant spouses were reunited with their families under economic distress. This paper exploits this setting to ask: does spousal labour migration affect the mental health of the partners left behind?

We use two complementary Difference-in-Differences (DiD) designs. Analysis 1 compares left-behind partners whose migrant spouse returned home during the GFC to those whose spouse stayed in the city. Analysis 2 compares partners whose spouse departed for urban work to those where both stayed home. Neither analysis finds a statistically significant effect on overall mental health. However, the overall null result masks significant heterogeneity by gender: women's mental health deteriorates significantly when a migrant spouse returns during the crisis (gender interaction $p=0.012$), with effects for women concentrated in difficulty making decisions ($p=0.002$) and feelings of unhappiness ($p=0.059$). While previous research has focused on the mental health of migrants themselves (10), our study is the first to document the gendered mental health effects on left-behind spouses, adding a new dimension to the policy debate on migration and wellbeing. We argue that left-behind women in households experiencing return migration during economic downturns constitute a risk group that warrants targeted mental health support, and propose crisis-contingent mental health screening and financial decision-support for this risk group.

Setting and literature

China's internal migration is shaped by the hukou system, which ties access to public services, education, and healthcare to the place of household registration (2). Because rural-registered migrants cannot access urban social services, migration typically involves spatial separation: the migrant works in the city while the partner remains in the village to manage the household and retain access to rural land and services (11). By 2008, an estimated 61 million children, 47 million wives, and 45 million elderly relatives had been left behind by rural-to-urban migrants (1). This pattern is not unique to China, labour migration with family separation is common across developing countries, including the Philippines, India, and several Sub-Saharan African nations, though the scale in China is unprecedented (3).

The existing literature on left-behind populations focuses primarily on children. Studies from China, Mexico, and the Philippines document negative effects of parental migration on children's education, health, and psychological wellbeing, with effects differing by the gender of the child and the migrating parent (4,5). A recent Lancet meta-analysis of 111 studies finds that left-behind children face a 53% increased risk of depression compared to children of non-migrant parents (12).

Research on left-behind spouses is more limited. Tong et al. (6), using the China Family Panel Studies, find that spousal migration increases psychological distress among left-behind partners, with the effect operating through reduced emotional intimacy and elevated household responsibilities. Notably, they find that women who become the sole household decision-maker during their husband's absence experience more, not fewer, depressive symptoms, suggesting that autonomy gains are offset by the burden of managing the household with limited resources. However, their study and others like it (3), rely on observational variation in migration status, which is endogenous: households where the husband migrates may differ systematically from those where he stays. The existing evidence therefore documents associations between spousal migration and psychological distress, but cannot establish whether migration itself causes mental health deterioration.

The GFC provides a useful setting for overcoming this endogeneity problem. China's export-oriented manufacturing sector, which employed tens of millions of migrant workers, was severely affected in late 2008 (7–9), leading to mass layoffs and an unprecedented wave of return migration. The crisis was driven by a collapse in global demand, making the shock plausibly exogenous to individual household characteristics. This is the key advantage over existing studies such as Tong et al. (6): rather than relying on observational variation in migration, we exploit the GFC as a natural experiment that forced migrants to return regardless of household preferences. One prior study uses the same data and GFC setting to examine mental health among migrant workers themselves (10), but no study has examined the effects on left-behind spouses. Our

paper fills this gap. We contribute to the literature in two ways: first, by providing what is, to our knowledge, the first quasi-experimental evidence on the mental health effects of migration on left-behind spouses, and second, by using two complementary analyses as a triangulation strategy to shed light on which channels most plausibly drive any observed effects.

The following sections describe the data and methods we use to implement this approach.

Data

Dataset

We use the Rural Household Survey (RHS), one of three components of the Longitudinal Survey on Rural-Urban Migration in China (RUMiC), a collaboration between the Australian National University, Beijing Normal University and the Institute for the Study of Labor (13,14). The RHS covers two waves (2008 and 2009) and surveys individuals registered in rural areas. The full dataset contains 63,966 person-year observations, of which 31,653 individuals appear in both waves, forming a balanced panel across 8,008 households in 84 counties (15).

The migration questions are retrospective: the 2008 wave asks about months away from the local township in 2007, while the 2009 wave asks about 2008. This ensures that the migration transition has occurred before the mental health outcome is measured. In our data, the migration rate drops from 26.6% to 22.4% between waves, consistent with the GFC-induced return migration documented in the literature (8,9).

In both analyses, we restrict the sample to the household heads and spouses who did not migrate themselves, ensuring we observe only individuals who remained in the village. We further require a balanced panel (present in both waves) and complete outcome data.

Variables

Outcome variable: Our primary outcome is the 12-item General Health Questionnaire (GHQ-12), a widely validated screening instrument for psychological distress (16), validated in Chinese populations (17,18) and used with RUMiC data to study migrant mental health during the GFC (10). The instrument consists of twelve items covering domains such as concentration, sleep quality, decision-making, and general happiness. Each item is scored 1 to 4, yielding a total from 12 (no distress) to 48 (maximum distress). We reverse-code the scale so that in all results, positive coefficients indicate improvement (less distress) and negative coefficients indicate deterioration (more distress). We refer to this as the mental distress scale throughout.

Migration status: We construct a binary indicator for labour migration using months away from the local township ($a27 > 0$), reason for absence being work ($a29 = 3$), and destination being an urban area ($a28 = 4, 5, 6$). This captures labour migration to urban areas specifically excluding absences for education, military service or family visits, consistent with approaches used in related research (7).

Controls: We include age, gender, and educational attainment (1=primary or no schooling, 2= junior secondary, 3= senior secondary or higher) as control variables.

Descriptive statistics

Table 1 provides an overview of the two analysis samples at baseline 2008

	Analysis 1	Analysis 2
Individuals	424	6,438
N in treatment/control group	131/293	81/6,357
Mean age (SD)	43.1 (7.0)	51.1 (9.7)
% Female	84.1%	36.4%
Mean education (SD)	1.99 (0.74)	2.18 (0.83)
Mean mental distress (SD)	20.38 (4.58)	19.93 (5.00)

Education 1=no schooling or primary, 2= junior secondary, 3= senior secondary or higher. Mental distress scale: 12 (least distress) to 48 (most distress).

The two samples differ substantially in composition. Analysis 1 consists mainly of women (84%) who are younger (mean age of 43), reflecting that the typical migrant is a male spouse. Analysis 2 includes both partners from non-migrant control households (explained in empirical strategy), yielding a near-equal gender split (36% female) and higher average age (51), reflecting that younger adults are more likely to be involved in migration (2) and are thus filtered into Analysis 1. Baseline mental distress levels are comparable across both samples (around 20 on a 12-48 scale).

These compositional differences between the two samples reflect the underlying migration patterns and inform our choice of controls, as discussed in the empirical strategy.

Empirical strategy

Research design

We employ two complementary DiD designs exploiting the panel structure of the RHS and the exogenous shock of the GFC.

Analysis 1 (spouse returns): Among couple households where one partner was a work migrant in 2007, we compare left-behind partners whose migrant spouse returned home in 2008 (treatment, $n=131$) to those whose spouse continued working in the city (control, $n=293$). The GFC provides the identifying variation: factory closures and mass layoffs forced migrants to return, making treatment assignment plausibly exogenous rather than driven by household preferences (7). The estimated effect captures both reunification with the spouse and the economic consequences of the spouse's forced return, we observe only the combined effect.

Analysis 2 (spouse departs): Among couple households where both partners were home in 2007, we compare partners whose spouse departed for urban work in 2008 (treatment, $n=81$) to those whose spouse stayed home (control, $n=6,357$). This captures the effect of separation, that is, the physical absence of the spouse, in a context where the departing spouse is moving toward employment. In treatment households, we observe only the partner who stayed behind. In control households, neither partner departed, so we keep both the head of the household and the spouse, since there is no natural criterion for selecting one over the other, which is why the control group is roughly twice the number of control households. This also means that the standard errors in Analysis 2 must be clustered at the household level to account for within-household correlation.

The motivation for using two analyses rather than one is that the effect of spousal migration on mental health can operate through multiple channels, including separation from a partner, loss of urban cash income and remittances, and changes in household dynamics such as disrupted power relations or domestic conflict (3,19). In Analysis 1, a migrant spouse returns home after losing their urban job, so we observe reunion, income loss, and changes in household dynamics bundled together. Analysis 2 provides a way to assess the separation channel in isolation: if separation itself harms mental health, we would expect a negative effect when a spouse departs. If instead we find no effect or even improvement in Analysis 2, this would suggest that the negative effect in Analysis 1 is more plausibly driven by the circumstances of return rather than the end of separation. The two analyses therefore work together as a triangulation strategy to shed light on which channels are most likely at play, though they cannot definitively isolate individual mechanisms.

Regression specification

The standard DiD model takes the following form:

$$Y_{it} = \beta_0 + \beta_1 \text{Treatment}_i + \beta_2 \text{Post}_t + \delta(\text{Treatment}_i \times \text{Post}_t) + X_{it}\gamma + \varepsilon_{it}$$

where Y_{it} is the mental distress for individual i at time t , Treatment_i is a binary indicator equal to 1 for individuals in the treatment group, Post_t is a binary indicator equal to 1 in the second wave (2009), and X_{it} is a vector of control variables. The coefficient β_1 captures any baseline level difference between the groups, β_2 captures the common time trend affecting both groups. The coefficient δ is the DiD estimator: the additional change in mental health experienced by the treatment group beyond the shared time trend. Standard errors are clustered at the household level in Analysis 2.

We estimate four specifications to assess sensitivity to controls. Model 1 is the basic DiD without controls. Model 2 adds controls for the variables that are significantly imbalanced at baseline (identified in Table 2 below: age for Analysis 1, age and gender for Analysis 2). Model 3 adds all available controls (age, gender and education). These three models use pooled OLS. Model 4 is our preferred specification: it replaces the pooled OLS intercept with individual fixed effects α_i , absorbing all time-invariant characteristics, both observed and unobserved. If δ remains stable across all models, this suggests that confounders are not driving the result. This model follows the following form:

$$Y_{it} = \alpha_i + \beta_2 \text{Post}_t + \delta(\text{Treatment}_i \times \text{Post}_t) + \varepsilon_{it}$$

Note that in the individual fixed effects model, the Treatment main effect β_1 is absorbed by the individual fixed effects and therefore drops out of the equation.

Identification

The DiD design requires the parallel trends assumption: absent treatment, both groups would have followed the same mental health trajectory. We cannot formally test this with only two periods. However, the baseline balance tables below provide suggestive evidence.

Table 2 presents baseline balance for Analysis 1

	Control		Treatment		Difference	p-value
Variable	Mean	SD	Mean	SD		
Mental distress	20.33	4.76	20.50	5.04	-0.16	0.751
Age	42.38	7.32	44.73	6.29	-2.35	0.002**

Female	0.85	0.35	0.82	0.38	+0.03	0.451
Education	1.96	0.71	2.06	0.78	-0.11	0.173
N	293		131			

*p-values from two-sample t-tests. ** $p < 0.01$. Mental distress scale: 12 (least distress) to 48 (most distress). Education: 1=no schooling or primary, 2= junior secondary, 3= senior secondary or higher.*

Baseline mental distress is virtually identical across groups in Analysis 1 (20.33 vs. 20.50, $p=0.751$), supporting the parallel trends assumption. Only age is significantly different ($p=0.002$), which we address in Models 2-4.

Table 3 presents baseline balance for Analysis 2

	Control		Treatment			
Variable	Mean	SD	Mean	SD	Difference	p-value
Mental distress	19.92	5.00	20.68	5.08	-0.76	0.175
Age	51.14	9.75	45.80	7.63	+5.33	0.000***
Female	0.36	0.48	0.64	0.48	-0.28	0.000***
Education	2.18	0.83	2.03	0.85	+0.15	0.101
N	6,357		81			

** $p < 0.1$, ** $p < 0.01$, *** $p < 0.001$. Mental distress: 12 (least distress) to 48 (most distress). Education 1=no schooling or primary, 2= junior secondary, 3= senior secondary or higher.*

In Analysis 2, the treatment group is about 5 years younger ($p < 0.001$) and has a higher share of women (64 versus 36%, $p < 0.001$). These imbalances are expected: younger men are most likely to depart for urban work, leaving behind younger female partners (2). Baseline mental distress does not differ significantly ($p=0.175$). We control for age and gender in Models 2-4.

Limitations of the research design

Several limitations should be noted. First, Analysis 1 has stronger causal credibility than Analysis 2, because the GFC provides an exogenous reason for return. In Analysis 2, departure is a household choice, making treatment assignment potentially endogenous. Second, the treatment groups are small (131 and 81), limiting statistical power. Third, the control group in Analysis 2 includes both partners per household while the treatment group includes only one, creating a structural difference in composition. Fourth, we cannot observe household income, domestic violence, relationship quality,

which are plausible mediators. Fifth, migrants who returned during the GFC may have held different types of urban employment than those who remained, limiting generalisation to non-crisis return migration.

Supplementary analyses

We test for heterogeneous effects by gender using an interaction term (Treatment x Post x Female), as the literature on left-behind children documents gender-differentiated effects of parental absence, for example, effects on children's education and health differ depending on the gender of the child and whether it is the mother or father who migrates (4). We also decompose the aggregate mental distress score into its twelve constituent items to identify which specific dimensions drive any observed effects. This item-level analysis is conducted for the female subsample in Analysis 1, where we find the strongest gender-specific effects. Finally, we report Cohen's *d* to assess practical significance.

Results

Main results

Table 4 reports the DiD estimates and Cohen's d for Analysis 1 (spouse returns)

Specification	Coeff.	SE	p-value	95% CI	Cohen's d	Interpretation Cohen's d
Model 1: No controls	-0.865	0.619	0.163	[-2.08, 0.35]	0.18	<0.20 = small effect
Model 2: + Age	-0.874	0.620	0.159	[-2.09, 0.34]		
Model 3: + Age, Gender, Education	-0.718	0.608	0.238	[-1.91, 0.48]		
Model 4: Individual FE (preferred)	-0.865	0.619	0.163	[-2.08, 0.35]		

N=848 (424 individuals x 2 waves). Outcome: mental distress scale (positive=improvement, negative=deterioration).

The preferred Model 4 estimates a coefficient of -0.865 (SE=0.619): left-behind partners whose migrant spouse returned home during the GFC experience a deterioration of 0.87 points on the mental distress scale, compared to those whose spouse stayed away during the GFC. On a scale ranging from 12 to 48 (36 point range), this represents a shift of about 2.4% of the total scale, and a Cohen's d of 0.18, which is below the conventional threshold of 0.20 for a small effect. The effect is not statistically significant at a 10% level ($p=0.163$), and the confidence interval includes zero. Taken at face value, this suggests that on average, the return of a migrant spouse during the GFC does not meaningfully affect the mental health of the left-behind partner. The coefficient is stable across specifications (-0.72 to -0.87), suggesting that observable confounders are not driving the result.

Table 5 reports the estimates and Cohen's d for Analysis 2 (spouse departs)

Specification	Coeff.	SE	p-value	95% CI	Cohen's d	Interpretation Cohen's d
Model 1: No controls	-0.047	0.642	0.941	[-1.31, 1.21]		
Model 2: + Age, Gender	-0.078	0.643	0.903	[-1.34, 1.18]		
Model 3: + Age, Gender, Education	-0.012	0.640	0.985	[-1.27, 1.24]		

Model 4: Individual FE (preferred)	-0.047	0.642	0.941	[-1.31, 1.21]	0.009	<0.20 = very small effect
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N = 12,876 (6,438 individuals x 2 waves). Outcome is mental distress scale (positive coefficients indicating improvement, negative indicating deterioration). SE clustered at household level.

For Analysis 2, the coefficient is essentially zero (coeff. -0.047, SE=0.642, p=0.941): the departure of a spouse for urban work has no detectable effect on the left-behind partner's mental health. Cohen's d is 0.009, which is negligible, meaning the effect is very small. Spousal separation alone, at least in this context, does not appear to harm mental health. As discussed in the empirical strategy, this finding is important for interpreting Analysis 1: if separation were harmful, we would expect a negative effect here. The coefficient is stable and close to zero across all models (-0.01 to -0.08), suggesting that observable confounders are not driving the result.

Heterogeneity by gender

Given the non-significant overall results, we examine whether effects differ between men and women.

Table 6 presents the gender-stratified results

Analysis 1: spouse returns	Coeff	SE	p-value	95% CI	Cohen's d	Interpretation Cohen's d
Effect for men (base)	+1.553	1.032	0.133	[-0.48, 3.58]		
Additional effect for women	-2.943	1.171	0.012**	[- 5.25, -0.64]		
Total effect for women	-1.39	-	-	-	0.29	<0.5 = medium effect

Analysis 2: spouse departs	Coeff	SE	p-value	95% CI	Cohen's d	Interpretation Cohen's d
Effect for men (base)	-1.951	1.143	0.088*	[-4.19, 0.29]		
Additional effect for women	+2.917	1.386	0.035**	[0.20, 5.63]		
Total effect for women	+0.97	-	-	-	0.19	<0.2= small effect

p-value: **p*<0.10, ***p*<0.05. Outcome: mental distress scale (positive= improvement, negative= deterioration)

The overall null result masks significant gender differences, which represent our strongest finding.

In Analysis 1, men show a non-significant improvement of 1.55 points when their spouse returns (*p*=0.133), while the gender interaction is -2.943 (*p*=0.012): women's mental health deteriorates significantly more than men's. The total effect for women is -1.39 points, corresponding to a Cohen's *d* of 0.29, which is a small to medium effect. To put this in perspective, a 1.39-point deterioration means that treated women shift roughly 4% across the total scale relative to the control group, concentrated in specific dimensions as shown below. While this is modest at the individual level, it represents a population-level shift in the distribution of mental distress among a large and vulnerable group.

In Analysis 2, the pattern is reversed: men show a marginally significant deterioration of 1.95 points when their spouse departs (*p*=0.088), while the gender interaction is +2.917 (*p*=0.035), and the total effect for women is +0.97 (a slight improvement). Women improve when the spouse leaves and deteriorate when the spouse returns under economic distress, a mirror pattern whose implications for identifying the mechanism we discuss below. Note that because Analysis 2 lacks the exogenous variation of the GFC shock, this finding is descriptive rather than causal.

Item-level analysis (female subsample, Analysis 1)

To identify which mental health dimensions drive the gender effect, we estimate the DiD separately for each GHQ-12 item within the female subsample of Analysis 1.

Table 7 presents the DiD estimates for each individual mental health dimension within the female subsample of Analysis 1

	Dimension	Coeff	SE	p	95% CI
a33	Concentration	-0.071	0.108	0.511	[-0.28, 0.14]
a34	Sleep loss	-0.050	0.083	0.549	[-0.21, 0.11]
a35	Playing useful role	-0.084	0.092	0.359	[-0.26, 0.10]
a36	Decision-making	-0.282	0.092	0.002***	[-0.46, -0.10]
a37	Under strain	-0.028	0.095	0.771	[-0.21, 0.16]
a38	Overcoming problems	-0.062	0.085	0.468	[-0.23, 0.11]
a39	Enjoying activities	-0.140	0.089	0.117	[-0.31, 0.04]

a40	Facing problems	-0.114	0.114	0.319	[-0.34, 0.11]
a41	Feeling unhappy	-0.167	0.088	0.059*	[-0.34, 0.01]
a42	Losing confidence	+0.023	0.083	0.781	[-0.14, 0.19]
a43	Feeling worthless	-0.107	0.092	0.192	[-0.29, 0.08]
a44	General happiness	+0.057	0.083	0.491	[-0.11, 0.22]

*p-values: * $p < 0.10$, *** $p < 0.01$. Outcome: mental distress scale (positive= improvement, negative= deterioration).*

Two items show significant effects. Decision-making (-0.282, $p=0.002$) shows a clear deterioration: on a single item scored 1 to 4, a shift of 0.28 points represents roughly 9% of the item range, which is a substantial effect at the item level. This means that women whose spouse returned during the GFC, in most cases having lost their urban job, report meaningfully more difficulty making decisions compared to women whose spouse stayed away. Feeling unhappy (-0.167, $p=0.059$) shows a marginal deterioration of about 6% of the item range. The remaining ten items are not significant. This concentration of effects in two specific domains, cognitive functioning and mood, rather than generalised distress across all twelve items suggests a targeted burden rather than a diffuse mental health decline. The decision-making finding is the single strongest result in our analysis and, as we discuss below, directly informs the type of policy intervention needed.

Discussion

Interpreting the two analyses together

The overall null result masks significant gender heterogeneity, as shown in the previous section. The two analyses are designed to work together and comparing their results helps us interpret, though not definitively explain, what most plausibly drives the effect on women.

In Analysis 1, women's mental health deteriorates when their migrant husband returns (-1.39 points). This effect bundles multiple possible channels: the end of separation (reunion) and the circumstances of the husband's return, including the loss of urban cash income and remittances, changes in household dynamics and potentially domestic conflict. Analysis 2 allows us to assess the separation channel in isolation. If separation itself were harmful to women, we would expect women's mental health to worsen when the spouse departs. Instead, we find the opposite: women's mental health improves slightly (+0.97) when the spouse leaves for work. This is the mirror image of Analysis 1.

This pattern suggests an important implication: if women improve when the husband leaves and deteriorate when he returns under economic distress, separation itself is unlikely to be the harmful channel. The negative effect in Analysis 1 may be more plausibly driven by the circumstances of the return, which could include the loss of urban wages and remittances, the financial uncertainty, and changes in household dynamics that come with a spouse returning home under distress. We observe only the combined effect and cannot isolate which of these channels is responsible. Similarly, the positive effect observed in Analysis 2 when the spouse departs bundles multiple channels: women may benefit from incoming remittances, from greater household autonomy or from relief from domestic tension, we cannot distinguish between these. The triangulation therefore does not cleanly separate 'separation' from 'economic disruption', rather, it shows that the net effect of a husband's departure toward employment is positive for women, while the net effect of his return under economic distress is negative. The contrast between the two analyses narrows the interpretation but does not isolate a single mechanism.

To illustrate the interpretive logic further, consider the counterfactual scenarios. If Analysis 2 had instead shown a significant negative effect on women (deterioration when the spouse departs), this would have indicated that separation itself harms women's mental health. In that scenario, the interpretation of Analysis 1 would have been ambiguous: the deterioration upon return could have reflected women being harmed by reunification, or alternatively, two competing forces (reunion= good, job loss= bad) that partially cancel each other, with the net effect depending on their relative magnitudes. Policy would then need to focus on combating loneliness and social

isolation rather than financial support. If Analysis 2 had shown no effect on women at all, the evidence would still point toward the circumstances of return as the driver of the Analysis 1 result, but less convincingly: if separation has no effect at all, we could not rule out that some other unobserved factor, unrelated to either separation or economic disruption, explains the deterioration upon return. The fact that we observe improvement rather than deterioration or a null effect in Analysis 2 provides the strongest available support for the interpretation that the conditions surrounding the husband's return (like loss of income), rather than the fact of reunion itself, drive the negative effect: women do better when the husband leaves for work and worse when he returns without it.

Possible mechanisms

We emphasise that the following discussion is speculative. Our data allow us to observe that women's mental health deteriorates upon the husband's return during the GFC, and that the effect concentrates in decision-making difficulty and unhappiness. We cannot observe why. The mechanisms discussed below are informed by existing literature and are consistent with our findings, but we cannot test or confirm any of them with the available data.

With that caveat, several potential mechanisms could explain why women are disproportionately affected by return migration under economic distress. First, during the GFC, many returning migrants had lost their urban jobs and came home without urban income (7–9). While these workers typically resume farming upon return, farming provides subsistence rather than cash income. The household thereby loses its primary source of monetary income, the urban wages and remittances that funded school fees, medical costs, and other expenditures, creating financial uncertainty even if the returning migrant is technically 'employed' in agriculture (9). The wife, who had adapted to managing the household independently, faces a new situation with suddenly constrained household resources. The significant effect on decision-making could be consistent with this interpretation: women may face increased cognitive burden from navigating difficult household financial decisions. This would align with Tong et al. (6), who find that becoming the household decision-maker during spousal migration increases rather than decreases depressive symptoms among left-behind women in China, driven by elevated responsibilities under constrained resources. In their study, the autonomy gain that might be expected to benefit women is offset by the burden of managing the household with limited means, a pattern consistent with our finding that women deteriorate precisely when the economic situation worsens upon the husband's return. Our findings are consistent with this: although women's mental health improves when the spouse departs (Analysis 2), this need not reflect an autonomy benefit, it may equally reflect incoming remittances or relief from household tension. The key pattern is that women's mental health deteriorates precisely when economic circumstances

worsen upon the husband's return, which aligns with Tong et al.'s (6) finding that constrained resources, not separation per se, drive psychological distress.

Second, the sudden return may disrupt established household routines and power dynamics. Liu (20) describes a 'patriarchal trap' in which left-behind wives gain a degree of autonomy during the husband's absence, but this autonomy is fragile and constrained. When the husband returns, the wife may lose the independence she had adapted to, creating tension over household authority and decision-making, which could directly contribute to our observed decision-making effect.

Third, domestic conflict is a plausible channel that we cannot rule out (3). Li and Wang (19) find that return-migrant women in China face a higher risk of all three forms of intimate partner violence (physical, psychological and sexual), compared to non-migratory women, suggesting that migration-related reunification can be accompanied by domestic conflict. Although their study focuses on migrant women rather than left-behind wives, the broader finding that return migration is associated with elevated domestic violence risk is relevant to our context. We cannot measure domestic violence in our data, nor can we distinguish between any of the mechanisms discussed above. In reality, the deterioration we observe likely reflects some combination of these channels rather than any single mechanism. The key finding is that women are adversely affected when a spouse returns under economic distress, the precise pathway remains an open question for future research with richer data.

Credibility of the results

The credibility of our findings varies across the two analyses. Analysis 1 has the strongest causal claim because the GFC provides plausibly exogenous variation in the return of migrants: factory closures forced workers home regardless of household preferences. The coefficient is stable across four specifications with progressively more controls and individual fixed effects, which suggests that selection on observables is not driving the result. Baseline mental distress is balanced between treatment and control ($p=0.751$) for Analysis 1. However, we cannot formally test the parallel trends assumption with only two periods, so this is suggestive evidence. The treatment group is small ($n=131$), limiting statistical power.

Analysis 2 is weaker causally because the departure decision is endogenous. Its primary value lies not in standalone causal claims but in the triangulation with Analysis 1: the mirror pattern across analyses is consistent with the interpretation that the circumstances of return, not separation, drive the effect on women. This interpretation is supported by existing literature: Tong et al. (6) similarly find that left-behind women's mental health suffers not from separation per se but from the elevated household burden, and the broader migration literature documents that the conditions of migration matter more for mental health than the fact of separation itself (3). The gender interaction findings are statistically robust ($p=0.012$ and $p=0.035$) but rest on relatively

small gender-specific subsamples, so they should be interpreted as identifying a pattern that warrants further investigation with larger samples.

Additional limitations

Beyond the methodological limitations discussed in the empirical strategy section, several additional considerations merit attention. The labour market recovered rapidly after the GFC: de Brauw et al. (9) document that by August 2009 fewer than 2% of the rural labour force remained unemployed due to the crisis. The mental health effects we observe may therefore be temporary. Our study covers only one year of post-treatment data, which prevents us from testing whether effects persist or disappear and from conducting a formal pre-trends test. We also cannot observe remittances, household income, domestic violence or intra-household dynamics, and the RUMiC survey's high attrition rate for migrant tracking (64% between waves) means the returning migrants we observe may not be representative of all returnees.

Finally, our findings are specific to rural China during the GFC. The institutional context, the hukou system, the structure of rural agriculture, and patriarchal household norms, shapes both the migration patterns and the mental health effects we observe. While spousal migration with family separation is common across developing countries including India, the Philippines, and Sub-Saharan Africa (3), the specific mechanisms through which return migration affects mental health may differ across institutional settings. Our results should therefore be seen as evidence for one important context rather than as directly generalisable to other countries.

Policy implications

The policy recommendations below are conditional on the interpretation that the circumstances of return, rather than separation itself, most plausibly drive the observed effect. If alternative mechanisms such as domestic conflict play a larger role than we assume, the recommendations would need to be adjusted accordingly. Our findings point to a specific risk-group: women whose migrant spouse returns during an economic downturn, and inform the following targeted interventions. In practice, policy targeting should prioritise regions with high-out migration rates, as a large share of women in these areas are likely to be exposed to crisis-induced return migration (2).

(1) Bundling mental health screening with existing healthcare visits. Rural women in China typically have contact with local health clinics through routine care and, where applicable, maternal or elderly care visits. Mental health screening could be integrated into these visits, for example by administering a short distress questionnaire before or after a standard consultation. This low-cost approach avoids the stigma of dedicated mental health services and reaches women where they already are. This is particularly relevant given that rural Chinese women rarely seek help for psychological distress: Qiu

et al. (21) find that only 36% of rural women with depressive symptoms would recommend seeing a doctor, with the majority preferring to solve the problem themselves or relying on family support. However, our results show that the mental health effect is driven specifically by return migration during economic crisis, not by the everyday reality of having a migrant spouse. Permanent, large-scale screening of all left-behind women would therefore not be cost-effective in normal times. Instead, we recommend a two-tier approach: establish the basic infrastructure now, that is, train healthcare workers to administer and interpret short screening tools and develop referral protocols, but activate intensive screening when an economic shock triggers mass return migration. This is analogous to emergency preparedness: the protocols and trained personnel are in place before the crisis, so that when a shock such as a trade disruption, pandemic or regional downturn, forces migrants home, the system can scale up quickly. We note that our data capture only the crisis period and do not allow us to observe mental health effects of having a migrant spouse under normal economic conditions. The two-tier approach is therefore a precautionary recommendation rather than one directly supported by a comparison between crisis and non-crisis periods. The finding that effects concentrate in specific mental health dimensions (decision-making, unhappiness) rather than generalised distress means screening tools should include items capturing these specific domains.

(2) Financial planning and decision-support rather than general counselling. The concentration of effects in decision making directly informs the type of intervention needed. For women whose primary burden is navigating household financial decisions under income uncertainty, general psychological counselling alone may be insufficient. Financial literacy programmes, budgeting assistance and support in navigating government benefit systems would more directly target the cognitive burden our results identify. This recommendation is strengthened by the triangulation evidence: because the circumstances of the husband's return appear to drive the effect, interventions addressing the practical burdens women face are more likely to be effective than those targeting loneliness or separation distress. However, we cannot rule out alternative mechanisms such as domestic conflict or disrupted household dynamics (19). If domestic violence is a contributing factor, mental health screening becomes even more important as an entry point for identifying at-risk women. Our policy recommendations are therefore designed to be robust to multiple mechanisms: screening identifies affected women regardless of the underlying cause, while financial support addresses what we consider the most plausible channel without precluding other interventions.

(3) Tailored support by gender. The finding that men and women respond in opposite directions to the same migration event is critical for programme design. A one-size-fits-all approach would dilute resources: women need support during return migration, while men show marginally worse outcomes during spousal departure (Analysis 2). Policy

should explicitly target women as the primary risk group in return-migration contexts, while acknowledging that the evidence for men is weaker given the endogenous nature of Analysis 2.

What we cannot recommend: Although the triangulation evidence points toward the circumstances of return as the most plausible driver, we cannot confirm which specific mechanism, loss of cash income, disrupted household dynamics, domestic conflict, loss of autonomy, or a combination, is responsible. We therefore cannot make evidence-based claims about whether re-employment programmes or direct income transfers would address the channel through which women are affected. Further research with household income data, remittance records, domestic violence indicators and employment histories would be needed to identify the precise mechanism and design appropriately targeted interventions. These data limitations point to several productive avenues for future work.

Directions for future research

Longer panels with more than two waves would allow formal pre-trends tests and examination of whether effects persist. Data on household income and remittances would enable direct testing of whether the loss of cash income is the primary channel. Information on relationship quality and domestic violence would help disentangle whether interpersonal conflict is a mediating factor. Tong et al. (6) demonstrate that mechanisms such as intimacy, autonomy, and household responsibility can be examined directly when appropriate data are available, providing a model for future research in this area. Finally, linking left-behind women's mental health to child outcomes would reveal whether the burden on women spills over to the next generation, a concern supported by Fellmeth et al (12), whose meta-analysis of 111 studies finds that left-behind children face a 52% increased risk of depression.

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