Does Easing Migration Barriers Increase Labor Unrest?*

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Abstract

Most evidence finds that granting formal status to migrants reduces crime and unrest. We find the opposite. Exploiting China's 2014 Hukou reform, which reduced within-country migration barriers, particularly for cities below one million residents, we show that per capita unrest almost doubled and peaked at nearly four times the pre-reform mean. The increase was concentrated in manufacturing, construction, and transportation sectors. Service employment expanded in low-skill sectors, but manufacturing employment remained unchanged. The reform expanded migrants' access to formal residency faster than labor markets could absorb, increasing pressure on employment, contracts, and social insurance.

Keywords: Hukou Reform; Internal Migration; Labor Unrest; Urban Labor Markets
JEL Codes: J52, J61, J68, O15, R23

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1 Introduction

Reducing migration barriers is among the most promising policy levers for economic growth,[1] yet the social consequences of such reforms are far less understood. Does easing migration barriers reduce labor unrest? Most evidence on international migrants suggests yes. Granting formal legal status reduces crime and social conflict, with causal evidence from high-income settings where formal employment absorbs new workers at stable wages (Marie and Pinotti, 2024). Whether this pattern extends to internal migration in developing countries, where labor markets face different structural constraints, including the existence of surplus labor, is an open question. This paper answers that question using China's 2014 Hukou reform. We find the opposite. Per capita unrest approximately doubled in cities that received the most extensive barrier reduction.

China's Hukou system restricts more than 145 million rural workers from full access to public services in their destination cities (Gao et al., 2023). By conditioning access to education, healthcare, and social welfare on a worker's place of birth, the system creates a large population of urban residents without formal residency rights. The 2014 reform relaxed these requirements based on city size, assigning the most extensive barrier reduction to cities below one million residents and only partial reduction to cities between one and five million. The gradient in reform intensity provides the identifying variation for this paper.

The population thresholds were set by the central government as part of a national urbanization strategy, not in response to local unrest trajectories. Pre-reform trends in treatment and control cities track each other closely across seven years. Exploiting this variation in a difference-in-differences design, we find that per capita unrest in treatment cities rose sharply after 2014, peaking at nearly four times the pre-reform mean in 2016. The increase was concentrated in manufacturing, construction, and transportation, the sectors in which migrants most often seek work. The service sectors, including retail and credentialed occupations, showed substantially smaller effects. The registered population grew 6–11% faster in treatment cities, with each post-reform year individually significant at the 5% level. We explore the mechanisms through which the Hukou reform affected these sectors differently. We find that the sectoral concentration in manufacturing, construction, and transportation, with near-zero effects in services, is consistent with the labor market channel rather than a generic city-size shock.

The key insight is that the reform expanded migrants' access to formal residency faster than accessible sectors could absorb. Manufacturing showed no differential growth after the reform, leaving formalized workers competing for a fixed stock of positions. Service employment grew in treatment cities, but the expansion concentrated in low-skill sectors such as transportation, where piece-rate earnings fell as more workers entered.[2] Workers who could not enter skilled services

[1] Empirical estimates suggest that eliminating international migration barriers could raise global GDP by 50–150% (Clemens, 2011), while easing internal barriers can increase labor productivity by more than 20% (Bryan and Morten, 2019).

[2] Piece-rate earnings in sectors such as transportation refer to the contractual practices where workers (i.e., drivers)

competed for manufacturing and construction positions that were not growing, or found work in transportation where piece-rate earnings compressed. The mechanism section tests this account against the employment, wage, and fiscal evidence.

The results demonstrate that the sectoral composition of local labor markets, not just aggregate economic growth, determines whether migration reform generates stability or instability. When formal status precedes economic integration and labor demand in accessible sectors adjusts slowly, migration reform can generate instability even where aggregate growth continues. An et al. (2024) show that the same reform depressed migrant wages by 2.6–7.9 percent in non-megacities, identifying the labor-market compression created by barrier reduction. We document the city-level collective consequence of that compression, showing that unrest rose where formalized workers entered sectors that did not expand. A related concern may arise in other household-registration settings, including Vietnam, where formal registration status similarly determines access to local services (Huỳnh, 2025).

Related Literature. A large body of research has documented significant internal migration costs that limit workers' ability to realize productivity and welfare gains from moving, including institutional barriers that restrict Hukou-registered workers in China (Ngai et al., 2019; Fan, 2019; Hsu and Ma, 2021; Wang et al., 2021; Jin and Zhang, 2023) and spatial frictions that reduce labor mobility more broadly (Imbert and Papp, 2020; Bryan and Morten, 2019). At the firm level, Imbert et al. (2022) show that large Chinese firms absorb internal migrants through expansion rather than wage adjustment, a channel that may not operate in the smaller cities where the 2014 reform had its deepest effect. Whether reducing these frictions affects social stability in destination cities is a different and less studied question. Most existing evidence on migration and conflict comes from contexts where migration is driven by displacement or economic shocks, in which observed social tensions may reflect the forced nature of inflows rather than the effects of policy itself. Even when governments deliberately ease internal migration barriers, reforms either apply uniformly across regions, leaving no variation to identify causal effects, or operate through infrastructure investment that simultaneously changes trade costs and market access, confounding the migration channel.

The 2014 Hukou reform in China is an exception. It changed administrative residency requirements without altering physical infrastructure or trade costs, and created a gradient of reform intensity across cities with different population sizes. Previous research using natural experiments finds that granting formal legal status to international migrants reduces individual crime (Pinotti, 2017; Mastrobuoni and Pinotti, 2015; Bell et al., 2013; Marie and Pinotti, 2024). That mechanism is employment-based. Legal status removes a ban on formal work and raises returns to legitimate activity. In Switzerland, the removal of immigration restrictions increased firm performance and worker productivity as labor markets absorbed inflows through firm creation and wage adjust-

are paid per trip or per delivery.

ment (Beerli et al., 2021). The Hukou system operates differently. Migrants participated in local labor markets regardless of Hukou status. Gaining formal residency changed service entitlements, not labor market participation, and the outcome of interest is collective labor action rather than individual crime. An et al. (2024) show that migrant wages fell by 2.6–7.9 percent on average in non-megacities after the reform, with declines reaching 10–14 percent in the smallest cities. We document the city-level unrest that followed.

Although granting legal status reduces individual crime, a separate literature finds that migration inflows can increase collective conflict when receiving areas lack absorptive capacity. Helms (2024) finds that a 10 percentage point increase in migrant inflows, induced by India's trade exposure, increased the incidence of anti-migrant riots by 6.5 percentage points. Knight and Tribin (2023) show that Venezuelan displacement to Colombian border municipalities increased homicide rates, driven by the victimization of migrants rather than crimes they committed. McGuirk and Nunn (2024) provide the closest analog to the absorptive capacity mechanism we document, showing that the implementation of agricultural development projects in traditionally pastoral areas of Africa, where the local economy could not absorb the resulting disruption, nearly doubled the risk of conflict. All three papers and ours share the underlying logic that conflict arises when labor inflows or policy interventions, or the speed at which they take place, outpace the receiving environment's capacity to accommodate them.

The main differences of our paper from these papers include the type of migration and the type of conflict. The conflict these studies document reflects anti-migrant backlash, physical vulnerability, or resource competition from migration driven by forces outside policy control. The unrest we document is collective labor conflict concentrated in manufacturing, construction, and transportation sectors, arising from a targeted government policy that expanded migrants' access to formal residency in these cities. Within the Chinese context, prior work examines the structural conditions and state responses that shape collective unrest (Elfstrom and Kuruvilla, 2014; Cai and Chen, 2022; Lorentzen et al., 2013; Qin et al., 2024; Wen, 2025), but no study identifies a specific policy reform as a causal driver.

We are not the first to study this question in the Chinese context. The closest to our study is Lai and Qiu (2025), who examine the 2014 Hukou reform and report lower unrest using a discontinuity design at the 3 million population threshold. However, our work differs from theirs in three fundamental ways, which also help with our identification. First, we estimate the average treatment effect across cities where the reform eliminated or substantially reduced migration requirements, not a local effect at the boundary where the contrast is between partial reform and no reform. Second, we exploit a five-group dose-response gradient that reveals monotone treatment heterogeneity pooled away by a single treatment indicator. Third, our panel provides six pre-reform years of parallel trends evidence. Section 3.2 examines these contributions in detail.

Section 2 presents the research design and data. Section 3 reports the main results. Section 4 investigates mechanisms. Section 5 concludes.

2 Research Design

The central challenge in identifying how easing migration barriers affects labor unrest is separating the reform's direct impact from other forces that shape city-level labor conditions over the same period. The 2014 reform's city-size classification provides a credible source of identification because the population thresholds determining reform intensity were set centrally as part of a national urbanization strategy, announced only in mid-2014, and bear no direct connection to pre-existing unrest patterns at the city level.

2.1 The 2014 Hukou Reform

In July 2014, the Chinese government announced a landmark reform of the household registration system (Hukou) that had shaped China's social and economic structure for over half a century. Originally introduced in the 1950s, the Hukou system tied individuals to their place of birth, classifying them as rural or urban residents and determining their access to education, healthcare, housing, and social welfare. The system created stark divisions between rural and urban populations while limiting the mobility of hundreds of millions of rural migrants seeking better opportunities in cities.

The 2014 reform, implemented through the "National New-Type Urbanization Plan (2014–2020)" and related State Council directives, aimed to encourage rural migrants to gain formal residency status in urban areas as part of China's broader urbanization strategy. The reform expanded the eligibility of migrants for local public services, including education, social security, healthcare, and public housing (Zhang and Treiman, 2013; Wu and Treiman, 2004), narrowing the welfare gap between migrants and registered urban residents (Xie and Zhou, 2014).

The 2014 reform classified Chinese cities into five categories based on the size of their urban population, each implementing differentiated approaches to reducing Hukou requirements (An et al., 2024). Cities with populations less than 0.5 million were required to remove all prior administrative restrictions, with stable formal employment as the sole remaining pathway to local Hukou. Medium-sized cities (0.5–1 million) experienced substantial reduction in requirements, with migrants needing one to three years of social security contributions along with stable employment. Larger cities (1–3 million residents) maintained controlled issuance, requiring three to five years of contributions, while cities with 3–5 million residents imposed even stricter criteria. Megacities such as Beijing and Shanghai, with populations exceeding 5 million, maintained strict controls with no significant reduction in requirements. In our empirical analysis, we map these five categories to a binary assignment. Cities in the two smallest categories (populations below 1 million, sizes 4 and 5) form the treatment group, which received the most extensive reduction in Hukou requirements. Cities in the two intermediate categories (populations between 1 and 5 million, sizes 2 and 3) form the control group, which experienced only partial barrier reduction. We use "treatment" and "control" throughout.

The Hukou registration confers permanent formal residency in the destination city, entitling holders to local public services, education, and social insurance. The reform therefore targeted a barrier different from the temporary residence permits that govern short-term labor mobility. The population response we document reflects permanent or long-term in-migration rather than circular flows.

2.2 Measurement of Labor Unrest

Data on labor unrest in China, including labor strikes and protests, are obtained from the China Labor Bulletin (CLB; China Labor Bulletin, 2024), a Hong Kong-based non-governmental organization dedicated to supporting the development of trade unions within China. This dataset is widely used in research on labor unrest, labor rights, and state-society relations (Qin et al., 2024) and is considered one of the most comprehensive sources of information on strike and protest events in China. Since 2007, the CLB has systematically collected labor strike and social protest statistics, with electronic coverage available from 2011 to date.[3] The CLB data measure reported incidents of labor unrest, including labor strikes, protests, demonstrations, and other forms of collective action. Each record identifies the timing, prefecture-level city location, employer name and description, industry classification, number of participants, worker actions, and government responses. The CLB data compilation draws from multiple sources, including overseas Chinese media, labor activists within China, and internet searches that cover social media platforms.

Our analysis examines labor unrest events from 2007 to 2019, a period longer than comparable studies in this literature. The sample begins in 2007, when the CLB commenced systematic data collection, and ends in 2019 to avoid confounding from COVID-19 lockdowns and concurrent changes in internet censorship enforcement that disrupted both labor market conditions and the CLB's collection methods after 2019.[4] The CLB recorded approximately 12,700 unrest events during this time span. We analyze two types of unrest, strikes and protests, with strikes comprising the majority at approximately 9,400 events. Manufacturing accounts for 78% of events within the manufacturing and services categories. Approximately 78% of incidents involve fewer than 100 participants, and wage arrears are the primary cause, cited in approximately 74% of events with identified grievances.

In addition to the CLB, we rely on city-level data from the China Statistical Yearbook (National Bureau of Statistics of China, 2019). We use 2013 urban population to classify cities into size categories, and annual registered population for per capita rates, employment, and fiscal variables for the mechanism analysis.[5] Our final dataset includes 285 prefecture-level cities, rep-

[3]For strike data prior to 2011, similar information was extracted from annual CLB reports and supplemented with additional sources such as Boxun, a U.S.-based political news website targeting Chinese audiences. Coverage of the smallest cities was particularly sparse before 2011, with only 3–5 total events per year across all 51 size-5 cities in 2007–2010.

[4]CLB coverage for 2009 is substantially below adjacent years (35 total incidents versus 69–90 in 2007–2008 and 2010, Appendix Table A2), but excluding it does not materially affect the estimates.

[5]We use only consistent population measures. In particular, remote or smaller cities with missing urban population

resenting around 85% of all Chinese prefecture-level cities, and providing coverage comparable to most existing studies examining the 2014 Hukou reform. Throughout the paper, "city" refers to a prefecture-level city, China's principal subnational administrative unit below the province. All fixed effects, clustering, and treatment assignments operate at this level. The sample contains 285 cities distributed across the five size categories: 12 megacities (Category 1), 11 large cities (Category 2), 108 medium cities (Category 3), 103 small cities (Category 4), and 51 towns (Category 5).

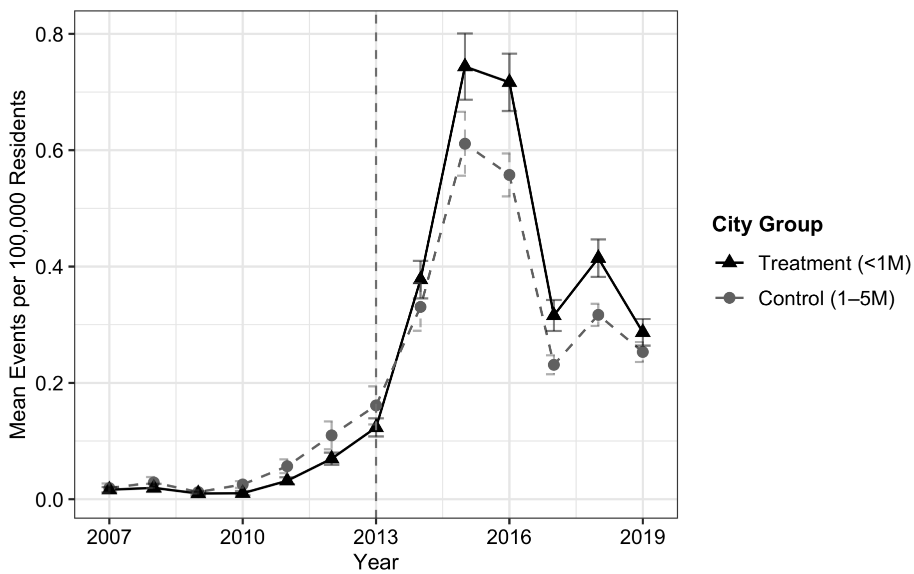
We construct three outcome variables. *Labor Unrest* is the total count of documented strikes and protests in a city in a given year t. *Strike* refers to labor-related unrest involving collective work stoppages, workplace demonstrations, or other organized responses by workers to disputes over wages, benefits, working conditions, or layoffs. *Protest* captures collective actions beyond standard work stoppages, including demonstrations, sit-ins, and road blockages, often arising from labor grievances, but extending to broader community concerns such as environmental conditions, housing, or public services. This disaggregation allows us to distinguish between workplace-specific labor disputes and broader collective grievances, which may respond differently to the Hukou reform.

For each of these three outcomes, we examine two complementary measures, the raw event count and the per capita rate (events per 100,000 registered population). The appropriate measure of labor unrest exposure is incidents per resident, not total incident volume. A city of five million with ten strikes and a city of 500,000 with ten strikes present identical events to a count-based measure, yet residents of the smaller city experience ten times the disruption per capita. Per capita rates are particularly relevant here because the reform itself induced differential population growth across city groups. When differential population growth alters the denominator, raw counts that appear comparable across groups can mask divergent per capita rates. Per capita unrest rose because incidents grew faster than the expanding registered population, a divergence our raw counts understate. Therefore, we treat per capita rates as the primary outcome variable. Per capita unrest rates are right-skewed with many zero city-year observations. OLS imposes a linear conditional mean on a non-negative outcome with this shape. Section 3.4 confirms that PPML, which requires no linearity assumption for non-negative outcomes, produces estimates that agree with OLS in direction and significance.

Two features of CLB coverage require discussion. First, reporting expanded substantially over the sample period. Appendix Table A2 documents total incident counts by year and city size. The CLB recorded fewer than 100 events per year before 2011, rising to over 600 in 2013 and peaking at roughly 2,600 in 2015–2016 before declining to about 1,300 by 2019. This growth reflects the CLB's shift to electronic crowd-sourced collection after 2011 and the broader expansion of social media reporting in China, not necessarily a proportional increase in actual unrest. Second, the reporting

data are excluded from the analysis due to incomplete observations. These cities together represent only a minor share of national GDP and economic activity.

Figure 1: Mean Per Capita Labor Unrest by City Group (2007–2019)



Notes: The figure plots mean per capita labor unrest (strikes and protests combined, events per 100,000 registered population) by year for control cities (population 1–5 million, sizes 2–3) and treatment cities (population below 1 million, sizes 4–5). Megacities (population above 5 million) are excluded. The dashed vertical line marks 2013.

growth was not uniform across city sizes. Smaller cities, which had the sparsest coverage in 2007–2010 (as few as 3–5 events per year across all 51 size-5 cities), gained disproportionately from electronic coverage expansion. If reporting improvements were concentrated in treatment cities, the post-reform increase could partly reflect better detection rather than more unrest. Section 3.4 examines this concern in detail and concludes that reporting bias is a live limitation, although several features of the results are difficult to generate through detection alone.

Figure 1 displays the mean per capita labor unrest from 2007–2019 across three city groups. All three groups have similar rates before 2014. After the reform, treatment cities show the largest increase.

2.3 Identification Strategy

We adopted a difference-in-differences event study design to estimate how variation in the intensity of Hukou barrier reduction across city types affected labor unrest. The design compares cities that received the most extensive reduction in migration barriers (the treatment group) to cities where restrictions were only modestly eased (the control group). Treatment cities are those with 2013 urban populations below 1 million (sizes 4 and 5 in the reform's classification scheme), where the 2014 policy mandated complete or near-complete elimination of Hukou requirements. Control cities have populations between 1 and 5 million (sizes 2 and 3), which experienced only partial barrier reduction through requirements such as multi-year social security contributions or stable employment thresholds.

We used 2013 urban population for classification to avoid potential endogeneity from cities adjusting reported population in response to the reform. Since the specific population thresholds were formalized only in mid-2014, the pre-announcement population reflects predetermined city characteristics rather than strategic responses to the policy.[6] The treatment group includes 154 cities and the control group includes 119 cities, yielding 3,549 city-year observations over the 2007–2019 panel. Appendix Table A1 reports pre-reform sample characteristics. Treatment cities are smaller by construction, since treatment is assigned by city size, and have correspondingly lower population, employment, and fiscal levels. Pre-reform per capita unrest rates are modestly lower in treatment cities (0.04 versus 0.06 per 100,000). These level differences are absorbed by city fixed effects and do not threaten identification. The relevant diagnostic is parallel pre-reform trends, which we examine in Section 3.

We excluded megacities with populations exceeding 5 million (size 1) from the baseline analysis. These cities did not experience any significant reduction in their Hukou barriers, and represent a fundamentally different political and economic environment. The post-reform period in megacities also coincided with major disruptions unrelated to the Hukou reform, including the stock market crash of mid-2015, associated economic anxiety in China's financial centers, and an intensified anti-corruption enforcement campaign. Protest activity in megacities, which averaged approximately 0.02 events per 100,000 residents per year before 2014, surged to roughly 0.7 per 100,000 in 2015–2016, a spike that far exceeds the patterns in all other city groups and reflects these concurrent shocks rather than the Hukou reform.

We estimate the following event study specification for the period 2007–2019:

$$y_{it} = \sum_{k=-6,k\neq 0}^{6} \beta_k \times \text{Treat}_i \times \mathbf{1}(t = 2013 + k) + \alpha_i + \gamma_{p(i)t} + \varepsilon_{it} \tag{1}$$

where y_{it} represents the outcome of interest in city i and year t, so each observation is a city-year row. The treatment indicator, Treat_i, equals one for treatment cities (2013 population below 1 million) and zero for control cities (population between 1 and 5 million). The event-time coefficients β_k capture the differential change in outcomes for treatment cities relative to control cities in year $2013 + k$ compared to the omitted reference year 2013, with k ranging from -6 (year 2007) to 6 (year 2019). City fixed effects α_i absorb time-invariant city characteristics, while $\gamma_{p(i)t}$ denotes province-by-year fixed effects for province $p(i)$ of city i, controlling for all time-varying shocks at the province level, including common trends, provincial economic conditions, and concurrent reforms. Standard errors are clustered at the city level to account for arbitrary serial correlation within cities.

The design identifies the average treatment effect of greater relative to lesser reform intensity

[6] As a robustness check, we verify that results are quantitatively similar when using 2014 population for classification (Appendix Table A15). Only 17 of 285 cities (6%) change size category between 2013 and 2014, confirming that the classification is stable.

on city-level per capita unrest, treating the 1 million population threshold as the assignment rule. Per capita is defined as events per 100,000 registered (Hukou) population. Because the registered population itself is an administrative outcome of the reform, the rate measures unrest per legal resident rather than per physical resident. This measure is the policy-relevant quantity, as local governments allocate services and enforce labor standards on the basis of registered population.

The design relies on three assumptions. The first is parallel trends, which requires that treatment and control cities would have followed parallel outcome paths absent the differential reform. The pre-reform event study coefficients provide a direct test. Appendix Table A5 reports joint F-tests that do not reject the null that pre-reform coefficients are jointly zero ($p = 0.42$ to 0.88), and we examine the pre-reform patterns for each outcome in Section 3. Flat pre-trends are necessary but not sufficient for the assumption to hold after the reform. Province-by-year fixed effects absorb all time-varying shocks common to cities within a province, but they cannot absorb within-province shocks that load differentially on city size, since city size defines treatment assignment. Section 3.4 verifies robustness to city-specific linear time trends, which additionally control for differential city-level trajectories.

The second assumption is no anticipation, which requires that cities did not differentially adjust behavior before the reform took effect. The specific population thresholds defining treatment intensity were announced only through mid-2014 State Council directives (An et al., 2024), so cities had no basis to anticipate their group assignment in advance. Even if the general direction of Hukou reform was signaled at the 18th Party Congress in November 2012, responding to yet-to-be-announced numerical thresholds would have required cities to anticipate specific cutoffs that had not yet been formalized. We examine whether population trajectories in treatment and control cities diverge before 2014 in Figure 2, and we verify robustness to alternative city-size classification windows in Section 3.4.

A related condition requires that the population thresholds be uncorrelated with pre-existing trends in labor unrest. The thresholds correspond to China's longstanding national city-size classification system and were determined centrally as a uniform policy, not in response to local unrest trajectories. Any pre-existing level differences between city groups are absorbed by city fixed effects, and the pre-reform patterns documented in Section 3 show no evidence of differential trends before 2014.

A final concern involves spillovers across cities (SUTVA). Qin et al. (2024) document that protest in one Chinese city increases protest probability in connected cities by 17 percent within two days through social media. Because spillover protests materialize within the same calendar year as the instigating event, annual aggregation fully captures any contamination of control-city counts rather than washing it out. If treatment-city unrest spills into control cities, our DiD estimate is biased toward zero, making the positive estimates a conservative lower bound on the true differential effect. A distinct spillover channel operates through migration itself. If the reform redirects migrants from partially reformed control cities toward fully reformed treatment cities, the control group experiences simultaneous relief, amplifying the measured differential. We cannot rule out longer-run demonstration effects or spatial reallocation.

The principal limitation of the design is that treatment assignment is determined by city size. Any contemporaneous shock that differentially affected smaller cities after 2014 would confound the estimates. Province-by-year fixed effects absorb shocks common to cities within a province, including provincial policy changes, economic cycles, and enforcement campaigns. These fixed effects cannot absorb within-province shocks that load differentially on city size, because city size defines treatment assignment. The monotone dose-response pattern documented in Section 3, where per capita unrest increases monotonically from the largest to the smallest city category across all five size groups, strengthens the case that the reform drives the result. A confounding shock would need to generate not only a post-2014 break at the treatment threshold but also a similarly monotone pattern across the full city-size distribution. That fact narrows, but does not eliminate, the range of plausible confounders. We are not aware of other national programs that change discontinuously at the 1 million population threshold used in the Hukou reform, though we cannot rule out the possibility. The design cannot fully separate the reform from city size. The contribution is a credible reduced-form pattern, that the cities assigned the deepest barrier reductions experienced the largest per capita increase in labor unrest, supported by mechanism evidence on employment, wages, and sectoral reallocation that narrows the set of plausible alternative explanations.

All specifications estimate Eq. (1) by OLS. Appendix Table A16 confirms that count-based Poisson pseudo-maximum likelihood (PPML) estimates agree with our main results in direction and significance.

3 Results

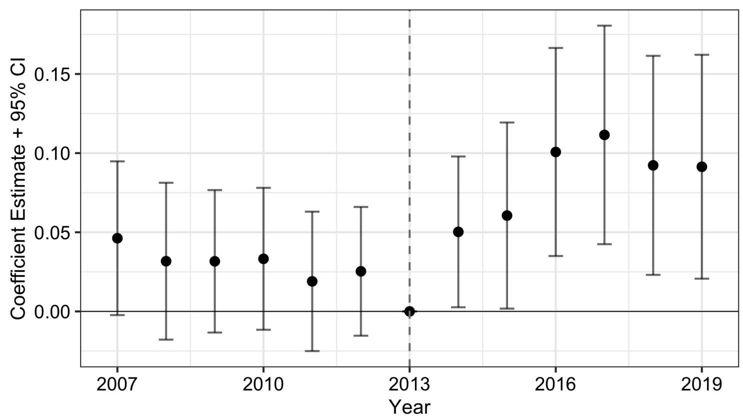
We present the main findings in three steps. Section 3.1 examines differential population growth as both a validity check and the first link in the causal chain. Section 3.2 estimates the reform's effect on per capita unrest rates. Section 3.3 decomposes the result by industry sector and incident scale.

3.1 Population Response to the Hukou Reform

If the 2014 reform differentially eased migration barriers in treatment cities, the most direct observable consequence should be differential population growth. We begin by examining whether this population response materialized.

Figure 2 presents event study estimates for log registered population, comparing treatment and control cities. An increase in registered population suggests that more migrants gained formal residency in these cities relative to cities that retained higher Hukou requirements. The pre-reform coefficients are statistically indistinguishable from zero across all years, supporting the parallel

Figure 2: Population Response to the Hukou Reform (2007–2019)



Notes: The figure plots event study coefficient estimates and 95% confidence intervals for log registered population, comparing treatment cities (sizes 4–5, population below 1 million) to control cities (sizes 2–3, population 1–5 million). The omitted reference year is 2013. All regressions include city and province-by-year fixed effects with standard errors clustered at the city level. Megacities (size 1, population above 5 million) are excluded.

trends assumption for population dynamics. After 2014, a clear divergence emerges. Treatment cities experienced approximately 5–11% higher registered population growth than control cities by 2014–2019, with the differential widening over time. Each individual post-reform year coefficient is statistically significant at the 5% level, though the 2014 estimate ($p = 0.04$) only narrowly clears the threshold. The pooled DiD estimate of 0.058 ($p < 0.10$, Appendix Table A6) averages across the buildup period and attenuates the magnitude relative to the peak years.

Against a pre-reform mean of roughly 616,000 registered residents in treatment cities, this differential translates to approximately 31,000–68,000 additional Hukou holders per city.[7][8] The timing and magnitude of this population response are consistent with the most extensive barrier reduction in treatment cities easing access to formal residency status, while partially reformed cities continued to restrict Hukou transfers. The question that follows is whether this growth in the registered population affected labor unrest, and if so, through what channels.

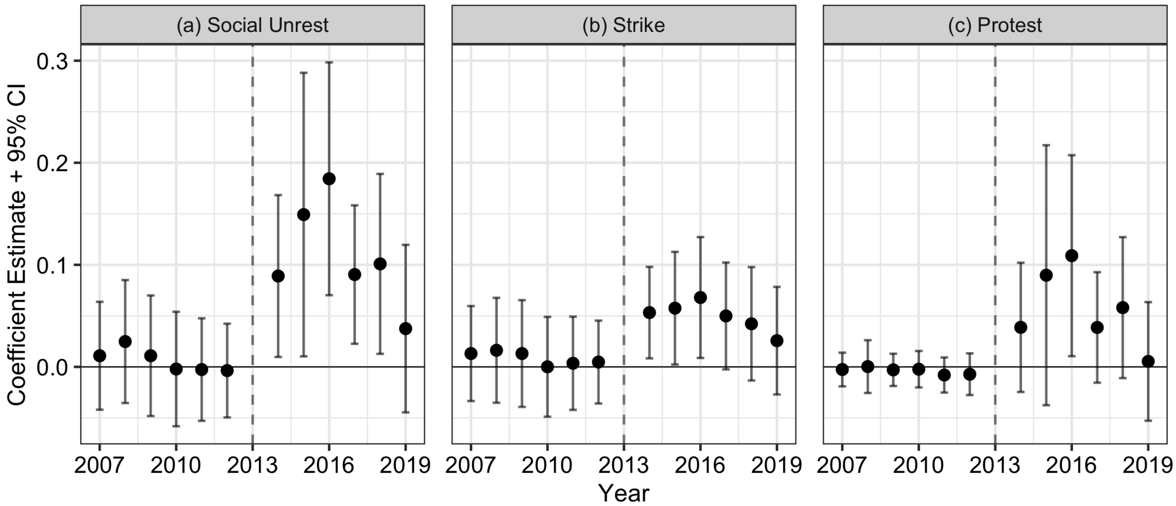
3.2 Per Capita Unrest Rates

Per capita unrest rates (events per 100,000 registered population) are reported in columns (1)–(3) of Table 1.

[7]The pre-reform level mean of treatment-city registered population is 61.6 in units of 10,000. The event study coefficients range from 0.050 in 2014 to 0.111 in 2017–2018, corresponding to 5–11 percent growth. Appendix Table A6 reports the mean of log registered population (4.496), from which the level mean cannot be directly recovered due to Jensen's inequality.

[8]Lai and Qiu (2025) find no discernible effect of the reform on actual urban population, measured using the Urban Construction Statistical Yearbook, which counts all physical residents regardless of Hukou registration status. The null result on actual urban population and our positive finding on registered population are consistent: the reform likely facilitated Hukou formalization of migrants already physically present in treatment cities, which raises Hukou holder counts without changing the count of physical residents.

Figure 3: Per Capita Unrest Rates (2007–2019)



Notes: The figure plots event study coefficient estimates and 95% confidence intervals comparing treatment cities (sizes 4–5, population below 1 million) to control cities (sizes 2–3, population 1–5 million). The dependent variables are per capita unrest rates (events per 100,000 registered population). Panel (a) reports overall labor unrest, panel (b) reports strikes, and panel (c) reports protests. The omitted reference year is 2013. All specifications include city and province-by-year fixed effects with standard errors clustered at the city level. The sample excludes megacities (size 1, population above 5 million).

The time-varying registered population is the primary and policy-relevant denominator given that the Hukou reform led to an increase in registered populations in treatment cities (Section 3.1). Because the denominator itself grows with the reform, dividing by an expanding population causes the estimated per-capita increase to understate what a fixed-population baseline would show. Columns (4)–(6) of Table 1 make this concrete: replicating the specification with 2010 registered population as a fixed denominator yields estimates of 0.139 for overall unrest and 0.082 for protests, both larger than the corresponding time-varying estimates of 0.104 and 0.060. The gap is the share of the per-capita increase that the growing denominator absorbs. Far from undermining the main result, the fixed-denominator columns confirm that unrest rose faster than the expanding registered population, and that the time-varying estimates are conservative. The fixed-denominator columns also serve as a direct comparison with Lai and Qiu (2025), who normalize by pre-reform prime-age physical population.[9]

Figure 3 presents the event study estimates of Eq. (1) for per capita unrest rates. Pre-reform coefficients range between -0.004 and $+0.025$ events per 100,000, with p-values from 0.42 to 0.94, and the same pattern holds for strikes and protests separately, supporting the parallel trends assumption.[10] After 2014, per capita unrest increased in treatment cities relative to control cities.

[9]Count-based (PPML) estimates show a positive but smaller differential than the per-capita specification, for the mechanical reason that a single additional event in a treatment city of 600,000 contributes 0.17 events per 100,000 residents while the same event in a control city of two million contributes only 0.05.

[10]The parallel trends pattern is robust to adding city-specific linear time trends (Section 3.4, panel c).

Table 1: Difference-in-Differences Estimates for Unrest Outcomes

	Time-Varying Population (OLS)			Fixed 2010 Population (OLS)		
	Unrest/100k	Strike/100k	Protest/100k	Unrest/100k	Strike/100k	Protest/100k
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	0.104***	0.042***	0.060**	0.139***	0.054***	0.082**
	(0.033)	(0.010)	(0.028)	(0.038)	(0.010)	(0.033)
Dep. var. mean	0.049	0.040	0.010	0.051	0.041	0.011
Observations	3,487	3,487	3,487	3,461	3,461	3,461
R^2	0.64	0.48	0.61	0.66	0.49	0.64

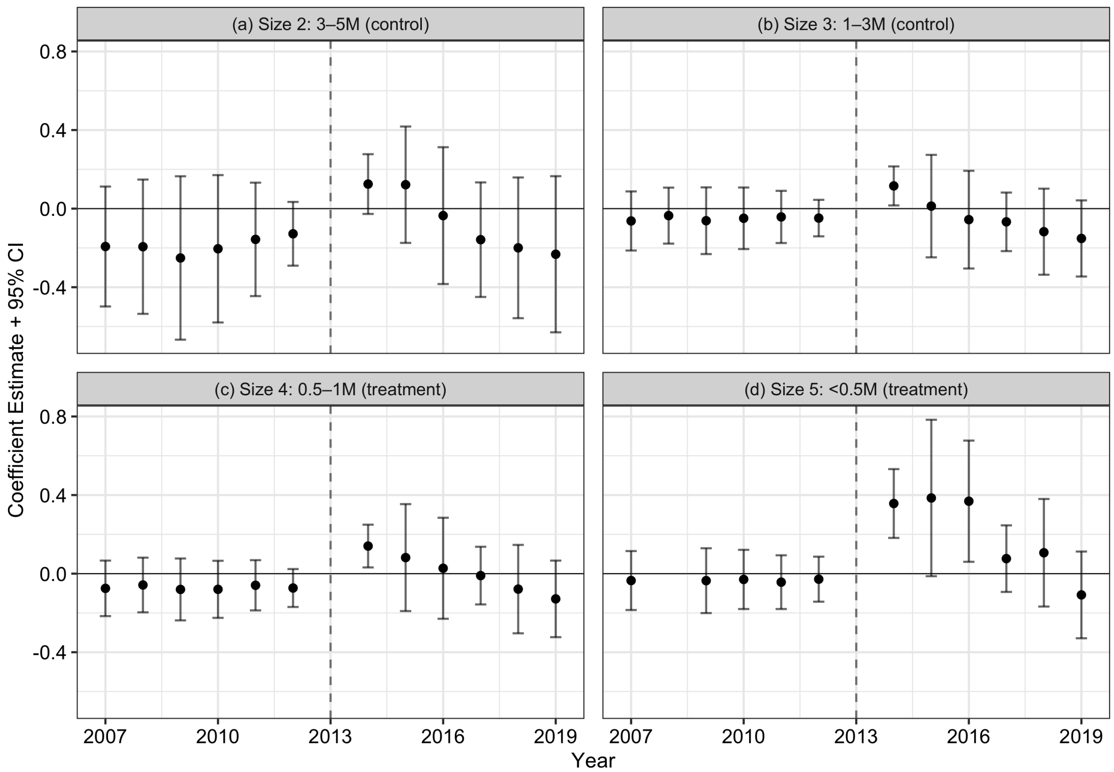
Notes: All columns report per capita unrest rates (events per 100,000 registered population). Columns (1)–(3) use time-varying registered population as the denominator. Columns (4)–(6) use 2010 registered population as a fixed denominator. Treat = 1 for cities with population below 1 million (sizes 4–5). Treat = 0 for cities with population 1–5 million (sizes 2–3). Post = 1 for years after 2013. Dep. var. mean reports the pre-reform sample mean (2007–2013) for columns (1)–(3). All specifications include city and province-by-year fixed effects with standard errors clustered at the city level. Megacities excluded. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The overall effect builds from 0.089 events per 100,000 in 2014 to a peak of 0.184 in 2016 ($p < 0.01$), nearly four times the pre-reform sample mean of 0.049 events per 100,000. Column (1) of Table 1 confirms a sustained increase of 0.104 events per 100,000 ($p < 0.01$), approximately twice the pre-reform mean of 0.049. The fixed-denominator estimate in column (4) is 0.139 ($p < 0.01$), confirming the attenuation interpretation. Effects remain elevated through 2018 before declining toward zero in 2019. Both strikes and protests contribute, with rates elevated throughout 2014–2018 in panels (b) and (c) of Figure 3. Attenuation after 2016 is consistent with labor market adjustment as migrants sorted into accessible employment over time, a channel we explore further in Section 4.

Figure 4 extends the event study to all five city-size groups, using megacities as the common reference. The post-reform pattern is monotone in reform intensity. Size 5 cities (below 0.5 million, complete elimination of requirements) show the largest increase in per capita unrest, size 4 cities (0.5–1 million, most extensive reduction) show a moderate and significant increase, size 3 cities show no detectable change, and size 2 cities are indistinguishable from megacities throughout the sample period. The gradient is sharpest for strikes. The monotone ordering across five distinct size groups is consistent with treatment intensity driving the differential response, though any confounder that loads monotonically on city size could in principle generate a similar gradient. Some early-year coefficients are absent for size 5 cities because the size-5 year interaction is collinear with the province-by-year fixed effects in sparse province-year cells. This affects only the pre-2011 years, when CLB coverage of the smallest cities was extremely thin (3–5 total events per year across all 51 size-5 cities), and does not affect the post-reform estimates.

In contrast, Lai and Qiu (2025) use a difference-in-discontinuity design around the 3 million population threshold and report a 42 percent reduction in labor unrest. The two designs test different margins of the same reform. Their 3 million cutoff compares cities that received minimal

Figure 4: Per Capita Unrest by City Size Category (2007–2019)



Notes: Event study coefficients and 95% confidence intervals using megacities (size 1, population above 5 million) as the reference group. Panel (a) shows size 2 cities (3–5 million), panel (b) shows size 3 cities (1–3 million), panel (c) shows size 4 cities (0.5–1 million), and panel (d) shows size 5 cities (below 0.5 million). The outcome is overall labor unrest per 100,000 registered population. Reference year is 2013. Some early-year coefficients for size 5 are absent when absorbed by province-by-year fixed effects in sparse cells. City and province-by-year fixed effects. Standard errors clustered at the city level.

barrier reduction (1–3 million) against cities that received essentially none (3–5 million). Our 1 million cutoff compares cities that received the most extensive barrier reduction (below 1 million) against cities with only partial reduction. We cannot replicate their results using our data. When we apply the same 3 million cutoff to our sample, the estimated coefficient is -0.045 ($p > 0.3$), with only 11 control cities, and standard errors roughly 60 percent larger than our baseline (Appendix Table A17). Appendix Figure A3 shows that the event-study coefficients are imprecise and exhibit no clear break at 2013, consistent with no detectable effect at this cutoff.

We can also narrow the control group in the opposite direction, using only cities with population 1–3 million as controls for our treatment cities. This drops the largest control cities (3–5 million) and asks whether the result holds against a more homogeneous comparison group. Appendix Table A17 reports that it does. The estimated effect increases monotonically with reform intensity, 0.082 per 100,000 for cities with population 0.5–1 million ($p = 0.010$) and 0.225 for cities below 0.5 million ($p < 0.001$). Appendix Figure A4 shows broadly flat pre-trends and a post-reform increase for both treatment groups, with larger effects for the smallest cities. The monotone gradient is what the reform predicts.

The divergence between the two papers reflects differences in identification strategy, treatment definition, and outcome measurement. Their difference-in-discontinuity design relies on a polynomial in the running variable to absorb the relationship between city size and unrest dynamics, an assumption sensitive to functional form. Their single treatment indicator pools 250 reform cities spanning our entire treatment and control groups, masking dose-response heterogeneity. Their outcome denominator is prime-age physical population, which did not change after the reform. Our denominator is registered population, which captures the formalization margin the reform directly affected.[11] The sectoral composition of the per capita increase, to which we now turn, provides evidence on the labor market channel driving this result.

3.3 Sectoral Heterogeneity

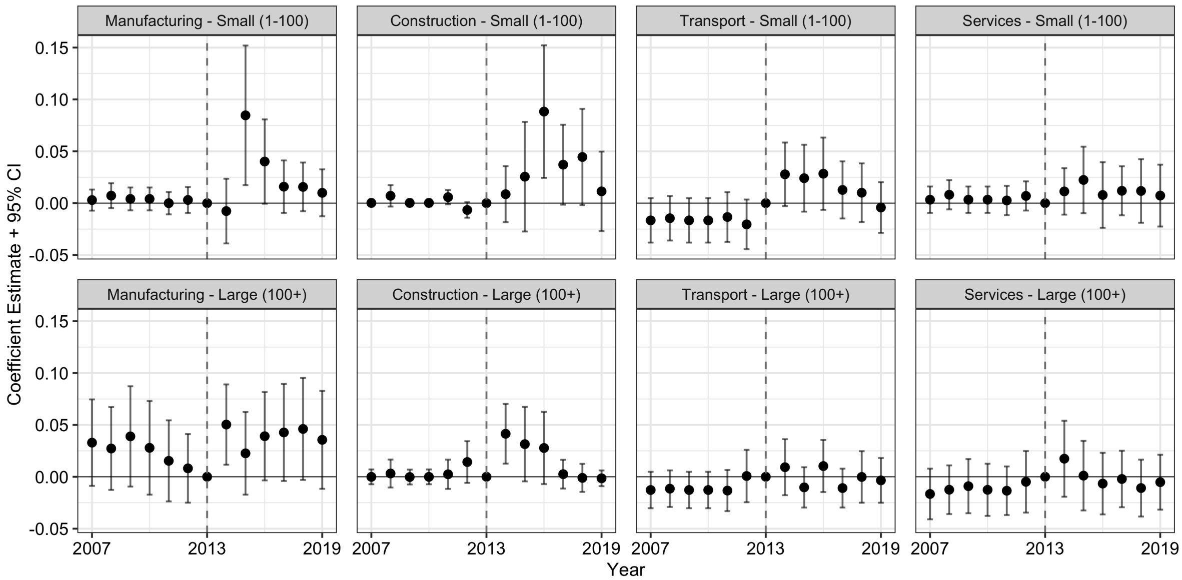
A distinctive feature of the CLB incident database is that each record identifies both the industrial sector and the participant count in each incident. This enables a decomposition of the aggregate per capita effect by the type of unrest that would not be possible with administrative data on aggregate incident counts. Both the sector and participant-count fields are recorded directly in the CLB data, and the grouping into four sectors is the authors' aggregation. We classify the raw CLB sector categories into four groups: Manufacturing (manufacturing, heavy industry, and mining), Construction (construction), Transportation (transportation, storage, logistics, and postal services), and Services (retail, education, and the public sector). The CLB records participant counts in pre-coded bins (1–100, 101–1,000, 1,001–10,000, and 10,000+). We follow the natural break in the CLB coding and classify events with 1–100 participants as small-scale and events with 100 or more participants as large-scale. Figure 5 presents per capita event study estimates for the eight categories formed by the intersection of sector and scale, using the same fixed effects and sample as the baseline specification. Appendix Table A8 reports pooled DiD estimates for all eight sector-scale categories under both time-varying and fixed 2010 population denominators.

The per capita increase in unrest is concentrated in manufacturing, construction, and transportation, the three sectors migrants are most likely to enter.[12] Small-scale manufacturing incidents show a sharp post-reform increase in the event study, with coefficients rising from near zero in 2013 to roughly 0.04–0.09 events per 100,000 in 2015–2016. Large-scale manufacturing incidents show a more persistent positive differential of 0.03–0.05 throughout the post-reform pe-

[11]Without the polynomial control, reform and non-reform cities in Lai and Qiu (2025) show significant pre-existing differences in unrest growth, population growth, and migrant shares (their Table 2), so identification depends on the polynomial correctly absorbing these differences. Our panel provides six pre-reform years of parallel trends evidence compared to two in their design.

[12]The National Bureau of Statistics Migrant Worker Survey, covering 31 provinces and approximately 1.5 million observations, reports that manufacturing and construction together provide 52–55 percent of employment for migrant workers nationally, making them the two largest entry sectors (National Bureau of Statistics of China, 2015). Transport, storage, and postal services account for an additional share.

Figure 5: Per Capita Unrest by Sector and Incident Scale (2007–2019)



Notes: Event study coefficients and 95% confidence intervals for per capita unrest (events per 100,000 registered population) comparing treatment cities (population < 1 million) with control cities (population 1–5 million), by sector and incident scale. Manufacturing covers manufacturing, heavy industry, and mining. Construction covers construction. Transport covers transport, storage, logistics, and postal services. Services covers retail, education, and the public sector. Small = 1–100 participants. Large = 100+. Reference year is 2013. City and province-by-year fixed effects. Standard errors clustered at the city level. Megacities excluded.

riod.[13] Construction incidents also rose, with small-scale construction events showing a sharp post-reform increase to approximately 0.09 per 100,000 in 2015–2016. Transportation incidents show a more modest but consistent positive post-reform differential. Services (retail, education, and the public sector) show no meaningful differential change. Small-scale service incidents are statistically indistinguishable from zero throughout the post-reform period in the event study, and the pooled DiD estimate is 0.001 per 100,000 ($p > 0.10$, Appendix Table A8). Large-scale service incidents show a small estimate of 0.002 ($p > 0.10$). Pooled DiD estimates for manufacturing (0.019 small-scale, $p < 0.10$; 0.013 large-scale, $p < 0.01$), construction (0.035 small-scale, $p < 0.05$), and transportation (0.027 small-scale, $p < 0.01$; 0.005 large-scale, $p < 0.10$) are each statistically significant at conventional levels (Appendix Table A8). Construction large-scale incidents (0.002, $p > 0.10$) are the exception, reflecting that major construction disputes (100+ participants) are relatively rare in smaller cities.

Because the per-capita rates use total registered population as the denominator, cities with a

[13]The Manufacturing Large panel shows positive point estimates in most pre-reform years. Large-scale manufacturing incidents (100 or more participants) are relatively rare in smaller cities, so pre-reform coefficients for this subcategory carry wider confidence intervals than those for small-scale incidents, and none of the pre-2013 estimates for Manufacturing Large are individually distinguishable from zero. The main per capita result holds when this subcategory is excluded, and the Manufacturing Small panel, which shows a cleaner flat pre-trend, accounts for the sharper identification.

Table 2: Pooled DiD Estimates Under Alternative Specifications

Specification	Treat × Post (SE)	N
(a) Drop autonomous minority regions	0.110*** (0.035)	3,096
(b) Restrict to 2011–2019 (electronic CLB coverage)	0.111*** (0.032)	2,456
(c) Add prefecture linear trends	0.107*** (0.034)	3,487
(d) Add trade openness	0.108*** (0.032)	3,475
(e) Add environmental policy	0.107*** (0.032)	3,475
(f) Add PM2.5 concentration	0.111*** (0.033)	3,414
(g) Control supply-side reform (2010 sec. share × post-2015)	0.103*** (0.033)	3,461
(h) Control SOE share (2010 urban unit share × post)	0.105*** (0.033)	3,461
(i) Year FE only (no province × year)	0.109*** (0.031)	3,487

Notes: Each row reports the pooled DiD estimate (Treat × Post) for per capita overall unrest (events per 100,000 registered population). Treat = 1 for cities with population below 1 million (sizes 4–5). Post = 1 for years after 2013. N refers to city-year observations. All specifications include city and province-by-year fixed effects with standard errors (in parentheses) clustered at the city level. Megacities (population above 5 million) are excluded. Specification (g) interacts each city's 2010 secondary employment share with an indicator for years from 2015 onward. Specification (h) interacts the 2010 urban unit employment share with the post-reform indicator. Specification (i) replaces province-by-year fixed effects with year fixed effects only. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

more goods-producing workforce could show higher sector-specific rates mechanically. Appendix Figure A1 and Table A9 repeat the analysis using secondary-sector employment as the denominator for goods-producing incidents and tertiary-sector employment for service incidents. The sectoral pattern is unchanged. Manufacturing and construction small-scale incidents remain statistically significant (Manufacturing: 0.398 small-scale and 0.187 large-scale per 100,000 secondary workers, both $p < 0.10$. Construction: 0.724 small-scale, $p < 0.10$). Transportation is also significant (0.205 small-scale, $p < 0.01$). Both service categories and construction large-scale incidents remain indistinguishable from zero. The sectors where migrants compete, manufacturing, construction, and transportation, experienced rising per capita unrest. Services, where migrants face higher entry barriers, showed no meaningful differential change. This divergence points to the labor market mechanism examined in Section 4.

3.4 Robustness

The baseline per capita finding could reflect sample composition, concurrent shocks, measurement choices, or data limitations rather than the reform itself. We address four categories of concern in turn, alternative sample restrictions and confound controls, estimator and denominator choice, an alternative control group (megacities), and differential CLB reporting.

Alternative specifications. Table 2 reports per capita unrest estimates under eight alternative specifications.

Panels (a) and (b) restrict the sample. Panel (a) excludes autonomous minority regions (Tibet, Xinjiang, Guangxi, Inner Mongolia, and Ningxia), where unique political and societal factors could drive labor unrest independent of Hukou policy changes. Panel (b) restricts the sample to 2011–2019, the period with consistent electronic CLB coverage, dropping the pre-2011 years reconstructed from annual reports. The estimates remain virtually identical to the baseline in both cases. Panel (c) adds city-specific linear time trends, directly addressing the concern that pre-trend tests alone cannot rule out confounds that activate at the reform date. City-specific trends also absorb differential minimum wage trajectories, which are set at the provincial level but can vary across cities within a province. The estimated effect remains positive and statistically significant, though slightly attenuated.

Panels (d) through (h) address specific concurrent shocks that could confound the baseline estimate. Panel (d) controls for city-level trade openness, since China's export slowdown after 2014 differentially affected manufacturing-heavy smaller cities and could independently generate labor unrest. Panels (e) and (f) address environmental regulation. Panel (e) adds interaction terms between year fixed effects and an indicator for the "2+26" cities subject to intensified environmental monitoring and factory shutdowns post-2014,[14] and panel (f) adds city-level PM2.5 concentrations (Khanna et al., 2025; Li and Meng, 2023), since pollution-related protests are a documented form of collective action in China. Panel (g) controls for the supply-side structural reform campaign that began in 2015, which shut excess capacity in coal, steel, and cement and could have generated layoff-driven unrest in manufacturing-heavy cities, by interacting each city's 2010 secondary employment share with an indicator for years from 2015 onward. Panel (h) addresses the anti-corruption campaign that intensified after 2012, which disrupted patronage networks and could provoke protest in cities with larger state-owned enterprise presence, by interacting the 2010 urban unit employment share with the post-reform indicator. Panel (i) replaces province-by-year fixed effects with year fixed effects only. The estimate is virtually unchanged (0.109 versus 0.104 in the baseline), confirming that the result does not depend on the more demanding fixed effect structure. Across all nine specifications the estimated DiD coefficient remains positive, statistically significant, and close to the baseline.

Inference. With 273 clusters (cities), cluster-robust standard errors may over-reject if the cluster-size distribution is unbalanced. Appendix Table A4 reports wild cluster bootstrap p-values (9,999 Rademacher draws). All three headline outcomes remain statistically significant, with bootstrap p-values close to the cluster-robust values ($p = 0.001$ for overall unrest, $p < 0.001$ for strikes, $p = 0.037$ for protests). Appendix Table A5 reports joint F-tests of pre-reform event study coefficients. For all headline specifications, the null that pre-reform coefficients are jointly zero cannot be rejected ($p = 0.42$ to 0.88). For the manufacturing-large subcategory, the F-test yields $p = 0.11$,

[14] The "2+26 Cities" refers to a regional air-quality management strategy that includes Beijing, Tianjin, plus 26 nearby prefecture-level cities in northern China (Shu et al., 2022).

consistent with the slightly noisier pre-reform patterns visible in the event study for that panel. Appendix Figure A6 presents a sensitivity analysis following Rambachan and Roth (2023). The confidence interval for the pooled post-treatment effect excludes zero when post-treatment violations of parallel trends are permitted to be up to half as large as the maximum pre-reform violation ($\bar{M} = 0.5$), and includes zero at $\bar{M} = 1.0$.

Alternative estimator. Per capita unrest rates are non-negative and right-skewed. OLS may therefore impose an inappropriate linear conditional mean. Appendix Table A7 shows that pooled DiD estimates remain positive and statistically significant for overall unrest and strikes under Poisson pseudo-maximum likelihood (PPML), while the protest estimate becomes imprecise. The directional agreement confirms that the OLS results are not an artifact of the linear specification.

Alternative control groups. We compare treatment cities to megacities (population above 5 million) rather than to control cities, using an entirely different control group. Appendix Figure A5 shows event study dynamics and Appendix Table A18 reports pooled DiD estimates. As discussed in Section 2.3, megacities experienced large idiosyncratic protest shocks during 2015–2016 unrelated to the Hukou reform, which contaminate the overall unrest measure when megacities serve as controls. We therefore focus on strikes. Strikes are positive and statistically significant under both time-varying and fixed denominators (0.066 and 0.087, both $p < 0.01$). The consistency of the strikes result across this alternative control group provides additional evidence that the baseline finding is not an artifact of the control comparison.

Data validity. As discussed in Section 2, the CLB's coverage expanded substantially over the sample period, with smaller cities gaining disproportionately from the shift to electronic collection after 2011. If reporting improvements were concentrated in treatment cities, the post-reform increase could partly reflect better detection rather than more unrest. Three features of the results make pure reporting bias a less complete explanation. First, the CLB is a nongovernmental organization based in Hong Kong that compiles incidents from social media, overseas Chinese media, and labor activists (Qin et al., 2024; Elfstrom and Kuruvilla, 2014). Its coverage does not depend on local government cooperation or reporting incentives. Second, the sectoral decomposition shows positive differentials for both small-scale (fewer than 100 participants) and large-scale (100 or more participants) manufacturing and construction incidents. Large collective actions attract regional and national media attention regardless of city size, so differential social media penetration in smaller cities cannot explain the large-scale pattern. A detection-bias mechanism would need to generate spuriously elevated counts for both scale categories simultaneously while leaving service sector counts unaffected. Third, the individual-level CMDS wage evidence, which comes from an independent government survey unaffected by CLB reporting changes, shows migrant wage declines concentrated in the same sectors where unrest rose. Reporting bias is nonetheless a live concern that the design cannot fully eliminate, and we interpret the magnitude of the estimates with this limitation in mind.

4 Mechanisms: Labor Market Absorption

The concentration of post-reform unrest in manufacturing, construction, and transportation raises a mechanism question. Why these sectors and not others?

Two complementary accounts fit the sectoral pattern. The first is absorptive capacity. The reform expanded migrants' access to formal residency in treatment cities, granting them legal claims on local employment, but manufacturing, where migrants most commonly seek work, did not expand. A simple queueing logic in the tradition of Harris and Todaro (1970) and Fields (1975) implies that when workers compete for a fixed stock of positions, congestion rises even if wages are rigid. The second account is a formalization gap. Workers who previously accepted informal conditions gained formal residency status and the legal standing it confers, raising expectations about wages, contracts, and social insurance. When employers in piece-rate and informal arrangements did not adjust, workers organized. The two accounts reinforce each other. Both predict rising grievances where the gap between formal status and actual conditions is largest, and both predict concentration in manufacturing, construction, and transportation, where informal and piece-rate arrangements are most prevalent.

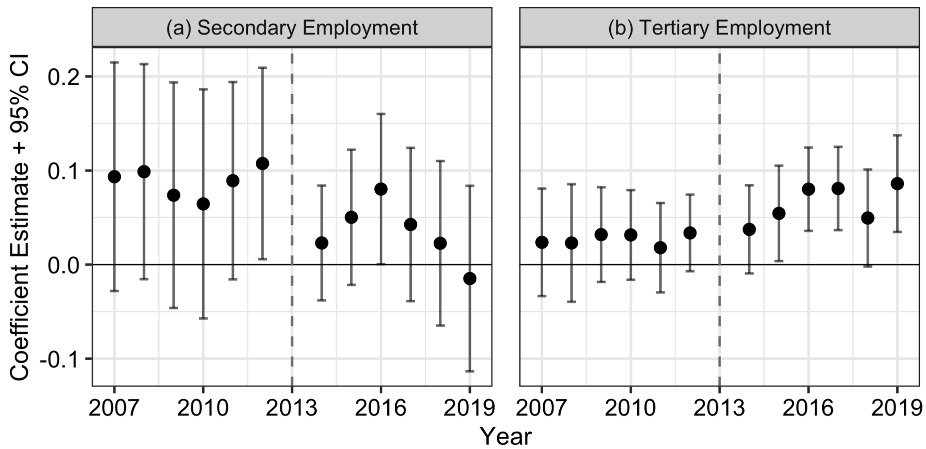
These accounts generate four testable predictions. First, secondary-sector employment should show no differential expansion in treatment cities. Second, tertiary-sector growth should concentrate in accessible service margins rather than expand broadly. Third, congestion should appear through lower earnings, lower employment access, or a wider gap between formal entitlements and realized conditions in the sectors where unrest rose. Fourth, the effect should be strongest during the initial adjustment period and attenuate as workers and employers adapt. If instead unrest rose uniformly across all sectors, or if manufacturing expanded alongside the formalization wave, or if service employment grew broadly across all subsectors, these accounts would be much less plausible. We examine employment growth, wages, and fiscal capacity against each prediction.

4.1 Sectoral Employment Responses

Figure 6 presents event study estimates for log employment in the secondary sector (which includes manufacturing) and the tertiary sector (services), comparing treatment and control cities.

The two sectors tell sharply different stories. In panel (a), secondary employment coefficients are positive throughout the pre-reform period (0.07–0.11), reflecting a gap between treatment and control cities that had been narrowing toward the 2013 reference level. This convergence reached approximate parity around the 2013 reference year. The post-reform coefficients remain near zero

Figure 6: Employment Responses by Sector (2007–2019)



Notes: The figure reports event study coefficients and 95% confidence intervals for log employment outcomes comparing treatment cities (sizes 4–5, population below 1 million) to control cities (sizes 2–3, population 1–5 million). Panel (a) reports secondary-sector employment and panel (b) reports tertiary-sector employment. The omitted reference year is 2013. All regressions use log-transformed outcomes and include city and province-by-year fixed effects with standard errors clustered at the city level. Megacities (population above 5 million) are excluded.

with wide confidence intervals, and the pooled DiD estimate is -0.041 (Appendix Table A10), statistically indistinguishable from zero. The reform did not generate additional differential growth in secondary employment. Manufacturing could not absorb incoming workers at a faster rate than it had in the years leading up to the reform.

Tertiary employment responded differently. Panel (b) shows a clear positive break after 2013, with coefficients rising to 0.05–0.09 by 2016–2019. The pooled DiD estimate of 0.042 ($p < 0.10$, Table A10) corresponds to approximately 4 percent higher service employment in treatment cities after the reform. Service employment expanded; manufacturing did not. The reform attracted population to smaller cities but created additional jobs primarily in services, leaving secondary-sector capacity essentially unchanged. This asymmetry also clarifies the denominator robustness check in Appendix Table A9: because secondary employment grew no faster in treatment cities, the manufacturing unrest rate per secondary worker and per resident tell the same story. For services, the growing tertiary employment denominator further compresses the already-zero per-worker service unrest rate, reinforcing the null result.

4.2 Labor Absorption in the Service Sector

The aggregate tertiary employment expansion documented above leaves open the question of which service channels absorbed the incoming workers. The distinction matters for the congestion account. If growth concentrated in sectors with low entry barriers, such as transportation and logistics, where a newly arrived worker can quickly find employment without credentials or substantial employer investment, then the sector absorbs migrants at the pace they arrive. If instead growth concentrated in subsectors requiring physical capital investment or formal qualifications, incoming migrants would face genuine congestion in the limited positions they can actually fill. Appendix Figure A2 decomposes tertiary employment into six subsectors.

Among the six subsectors, transportation and communication is the only one showing a statistically significant differential expansion (Appendix Table A10). The pooled DiD estimate of 0.101 ($p < 0.10$) corresponds to approximately 10 percent higher transportation employment in treatment cities after the reform, or roughly 540 additional workers per city against a pre-reform base of about 5,400 ($\exp(8.60)$). The event study for transportation shows a clean break at 2013, with coefficients rising from near zero to 0.07–0.10 in the post-reform years, and no indication of pre-reform divergence. Wholesale and retail trade shows a directionally positive post-reform trend in the event study (coefficients reaching 0.07–0.13 before moderating), but the pooled estimate of 0.071 is not statistically significant. Healthcare (0.081) and scientific and technical services (0.038) display gradual upward movement in the post-reform event study, though neither approaches conventional significance levels. Finance (0.018) is essentially flat throughout, and social services (0.040) exhibits large pre-reform swings followed by convergence toward zero, a pattern more consistent with structural catch-up than a reform response.[15]

The transportation sector presents an apparent paradox. Employment expanded by approximately 10 percent, yet per capita unrest in this sector rose. The resolution is that transportation absorbed additional workers without absorbing their earnings expectations. Transportation in Chinese cities operates predominantly through piece-rate arrangements, where drivers earn per trip or per delivery. More drivers competing for a given volume of freight or passengers mechanically compresses per-trip earnings even as total sector employment grows. CLB incident descriptions confirm this interpretation. Among the 499 transportation events recorded in treatment cities during 2014–2019, 36 percent cite earnings grievances (fare disputes, wage arrears, fee complaints) and 45 percent cite competitive pressure (ride-hailing apps, unlicensed vehicles), with 82 percent matching at least one category.[16] The sector created positions but not the earnings that newly formalized workers expected. The individual-level wage evidence in the next subsection confirms the compression directly.

These patterns reflect differential adjustment speeds across service sectors. Transportation, retail, and logistics are low-capital services with minimal credentialing requirements, so they can expand relatively quickly when population inflows arrive. Workers drawn into these accessible sectors are plausibly the same workers who would otherwise compete for manufacturing positions, as rural migrants to smaller cities typically seek employment in industry or basic services. Higher-skill services such as finance and healthcare require physical capital investment or regulatory approvals, constraining how quickly they can expand. Even if these sectors eventually grow,

[15]Scientific and technical services shows some positive pre-reform drift in the event study. The post-reform trajectory for that subsector should therefore be interpreted cautiously.

[16]Keywords were pre-specified. Categories overlap because some events mention both earnings grievances and competition.

the workers they attract are unlikely to have been competing for manufacturing jobs in the first place. The net result is that accessible employment grew in a narrow set of service subsectors, insufficient relative to the 6–11 percent population inflow documented in Section 3.1. Workers who could not find positions in transportation or retail competed for manufacturing jobs, where secondary employment showed no differential expansion, contributing to the elevated per capita unrest documented in the preceding sections.

4.3 Migrant Wages and Sectoral Reallocation

The employment dynamics described above raise the question whether wage pressure and worker reallocation can be observed directly in individual-level data. To address this question, we use the China Migrants Dynamic Survey (CMDS) and examine both aggregate wages and sector-specific patterns in Eq. (2).[17]

$$\ln w_{ict} = \alpha + \beta\, \mathrm{Treat}_c \times \mathrm{Post}_t + X_{ict}'\gamma + \mu_c + \delta_{p(c),t} + \phi_{o(i),t} + \varepsilon_{ict} \tag{2}$$

where w_{ict} is monthly wage of migrant i in city c at time t, X_{ict} includes gender, age, age squared, education, marital status, and Hukou type. We follow the specification in An et al. (2024), who use the same survey to study migrant wage effects of the Hukou reform. μ_c are city fixed effects, $\delta_{p(c),t}$ are province-by-year fixed effects, and $\phi_{o(i),t}$ are occupation-by-year fixed effects. Standard errors are clustered at the city level. We first estimate Eq. (2) pooling all sectors, then separately for manufacturing, construction, transportation, and other services to identify where wage pressure was concentrated. Panel B replaces the dependent variable with a sector indicator and uses the full sample of migrants with non-missing industry codes to test whether the reform shifted which sectors migrants work in.

Table 3 presents the results. Wages are in nominal terms. Province-by-year fixed effects absorb province-level price trends in each year, so the identifying variation comes from within-province differences across city sizes net of common price movements. Panel A, column (1) pools all sectors and shows that migrant wages fell by 3.7 percent in treatment cities relative to control cities ($p < 0.01$). Columns (2)–(5) decompose the effect by sector. Wages fell significantly in construction (-4.1%, $p < 0.01$), transportation (-3.8%, $p < 0.05$), and other services (-4.1%, $p < 0.01$). These are the sectors where per capita unrest rose. Manufacturing wages declined by 1.7 percent but the estimate is not statistically significant (Appendix Table A13 confirms this result is stable across alternative specifications). These findings are consistent with An et al. (2024), who use the same survey and find migrant wage declines of 2.6–7.9 percent on average in non-megacities, with larger effects concentrated among less-educated workers in the smallest cities.

The non-significant manufacturing wage result is consistent with institutional wage floors that

[17] The CMDS covers approximately 485,000 migrant workers across our sample cities from 2011 to 2017, providing only three pre-reform years (2011–2013) that limit the scope for pre-trends diagnostics on wage outcomes (Appendix Table A3 reports summary statistics).

limit downward adjustment in this sector. Manufacturing wages in Chinese cities are subject to minimum wage floors and firm-level wage grids that compress downward adjustment. The grievance channel in manufacturing operates through employment access and working conditions rather than through wage levels. Newly formalized workers expected formal contracts and social insurance protections but encountered the same informal arrangements that prevailed before the reform. CLB incident descriptions for manufacturing predominantly cite wage arrears and unpaid overtime rather than wage cuts, consistent with a formalization gap where conditions did not match the entitlements that formal status implied.

Panel B asks whether the reform shifted which sectors migrants work in. The only significant reallocation is into other services (+1.7 percentage points, $p < 0.01$, on a pre-reform base of 44.1 percent). Manufacturing, construction, and transportation shares are unchanged (Appendix Table A13 confirms these results across alternative fixed effect structures). Migrants entered the most accessible service occupations, including delivery, catering, and resident services, where wages fell as a result.

Appendix Table A14 decomposes the wage effect by migrant tenure. The wage decline is concentrated among longer-tenure migrants (incumbents already present when the reform took effect) rather than disproportionately affecting recent arrivals. The interaction between treatment and recent arrival status is insignificant in all four sectors. The reform did not protect incumbents from wage pressure, consistent with a broad congestion effect rather than a compositional shift driven exclusively by new entrants.

The aggregate city-level average wage DiD is -0.006 and statistically indistinguishable from zero (Table A11), because it pools migrants and natives. Native wages showed no corresponding decline and were slightly positive (An et al., 2024). In a city-level average, the negative migrant effect and the flat native effect largely cancel. Industrial output showed no differential response (pooled DiD $= 0.049$, not significant), reinforcing the conclusion that economic expansion in treatment cities was concentrated in services rather than in industry. Per capita local public revenue declined by approximately 7 percent in treatment cities relative to control cities (-0.074, $p < 0.10$, Table A11), as population grew faster than fiscal revenues. Declining per capita revenue reduces local governments' capacity to enforce labor standards, staff labor arbitration courts, and fund the transitional public services that the reform promised to newly formalized residents. The gap between expanded formal entitlements and diminished fiscal capacity to deliver on those entitlements may constitute a grievance channel distinct from wage congestion, though the marginally significant estimate ($p < 0.10$) warrants caution in interpreting the magnitude. Table A11 reports the pooled DiD estimates for aggregate wages, industrial output, and per capita fiscal indicators.

An important distinction is that the reform primarily formalized migrants already physically present in treatment cities rather than generating large new physical inflows. The population response documented in Section 3.1 reflects growth in registered population (Hukou holders),

Table 3: Migrant Wages and Sector Choice (2011–2017)

	All (1)	Mfg. (2)	Constr. (3)	Transport (4)	Other Svc. (5)
Panel A. (Log) Wage					
Treat × Post	-0.037***	-0.017	-0.041***	-0.038**	-0.041***
	(0.009)	(0.012)	(0.015)	(0.015)	(0.012)
Pre-reform control mean	7.885	7.876	8.028	7.777	7.892
Observations	424,723	99,096	36,890	59,115	229,622
R^2	0.26	0.36	0.27	0.26	0.24
Panel B. Sector choice (= 1 if in sector)					
Treat × Post		0.003	-0.004	-0.001	0.017***
		(0.004)	(0.002)	(0.002)	(0.005)
Pre-reform control mean		0.246	0.082	0.121	0.441
Observations		485,496	485,496	485,496	485,496
R^2		0.61	0.62	0.64	0.53

Notes: Panel A estimates Eq. (2). Wages are in nominal values. Column (1) pools all sectors. Columns (2)–(5) restrict to migrants in the indicated sector. Panel B replaces the dependent variable with a sector indicator and uses the full sample. All specifications include individual controls (gender, age, age squared, education, marital status, Hukou type), city, province-by-year, and occupation-by-year fixed effects. Standard errors clustered at the city level. Data from the CMDS (2011–2017). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

not physical residents.[18] Formalization under the reform changed the institutional relationship between workers and the city. Before the reform, migrants without local Hukou worked under informal arrangements with limited recourse. After gaining formal residency, the same workers acquired legal standing to claim social insurance, formal contracts, and access to labor dispute resolution. The elevated unrest is therefore more plausibly linked to the gap between newly acquired entitlements and unchanged employment conditions than to a physical influx overwhelming housing or infrastructure. The registered population measure captures this formalization margin directly.

Hukou formalization may also have directly strengthened migrants' capacity to organize collectively. Workers with formal residency have access to labor dispute resolution channels, stronger legal standing in wage claims, and reduced vulnerability to employer retaliation. The insurance composition test (Appendix Table A12) measures the characteristics of new migrants rather than the bargaining position of incumbents, so the negative insurance result (-2.1 percentage points, $p < 0.05$) does not rule out empowerment among workers who were already present.

To summarize, the mechanism evidence establishes three stylized facts. First, secondary em-

[18]Lai and Qiu (2025) find no discernible effect of the reform on actual urban population using a measure that counts all physical residents regardless of Hukou status.

ployment did not expand differentially in treatment cities. Second, the growth in tertiary employment concentrated in the transportation sector, a low-barrier sector. Third, migrant wages fell in the sectors where unrest rose. These facts are consistent with absorptive-capacity constraints, possibly amplified by formalization-induced organizing capacity, and harder to reconcile with a story in which the reform expanded economic opportunity broadly. The evidence does not uniquely identify the relative importance of congestion, formalization gaps, and empowerment. These channels are reinforcing rather than competing, and distinguishing among them would require worker-firm data on contract terms, grievance filing, and dispute resolution outcomes that are not available at the city-year level.

5 Conclusion

Per capita labor unrest approximately doubled in cities that received the most extensive barrier reduction under China's 2014 Hukou reform. The effect peaked at nearly four times the pre-reform mean in 2016 and concentrated in manufacturing, construction, and transportation, where informal and piece-rate arrangements are most prevalent. The increase was monotonically graded across city-size categories, with cities receiving deeper barrier reductions showing larger per capita effects. The effect attenuated by 2019, a pattern consistent with labor market adjustment as workers and employers adapted over time, though the broader data-validity discussion cautions against over-interpreting trends in later years when CLB observability may also have shifted.

The interpretation of this pattern is less certain than the pattern itself. The sectoral evidence is consistent with absorptive-capacity constraints. Secondary employment did not expand, tertiary growth concentrated in a single low-barrier subsector, and migrant wages fell in the sectors where unrest rose. Formalization may have simultaneously raised workers' expectations and their capacity to organize, amplifying the effect. The reduced-form design cannot distinguish the relative importance of congestion, formalization gaps, and empowerment, and these channels are reinforcing rather than competing. The principal design limitation is that treatment intensity is determined by city size, so the estimates cannot fully separate the reform from other post-2014 shocks correlated with city size, even though the monotone dose-response pattern narrows the set of plausible confounders.

For policy, the results imply that easing migration barriers does not mechanically reduce labor instability. If congestion dominates, the priority is expanding absorptive capacity in accessible sectors before or alongside barrier reduction, through targeted investment in infrastructure, logistics, and construction capacity in receiving cities. If the formalization gap dominates, the priority is aligning employer obligations with the legal standing the reform confers, through strengthened labor inspection and enforcement of contract and social insurance requirements. If empowerment dominates, elevated unrest reflects a welfare-improving reallocation of bargaining power, and the policy response should facilitate rather than suppress collective action. The channels are reinforcing, and the reduced-form design cannot rank them. The evidence is most directly informative about the congestion channel, where sectoral employment and wage data confirm the mechanism. The formalization and empowerment channels remain plausible but less directly tested.

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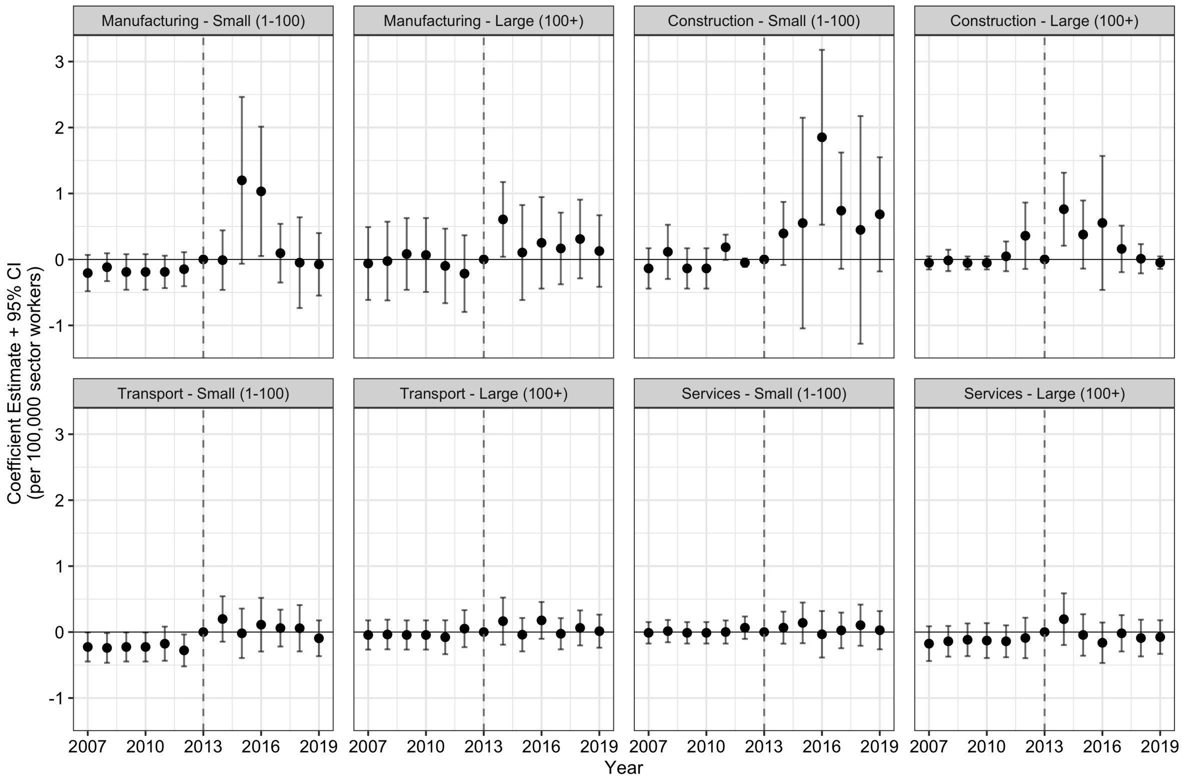
A Appendix

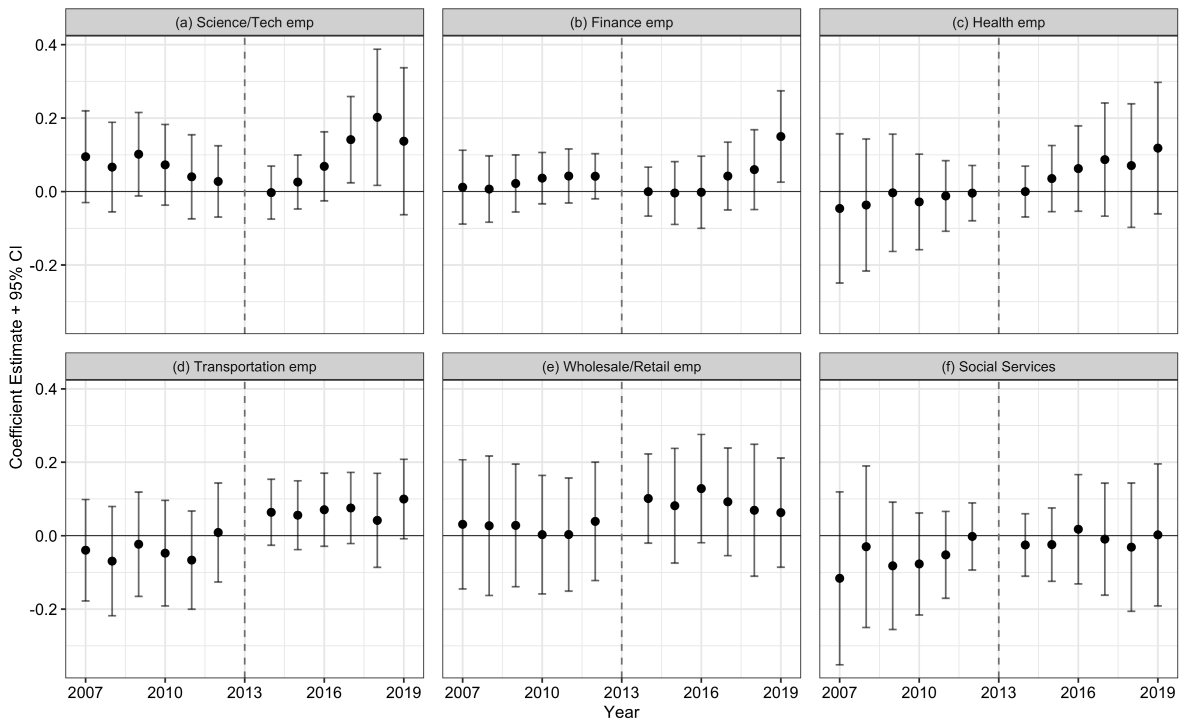
A.1 Additional Figures

Figure A1: Unrest by Sector and Scale, Sector-Employment Denominator (2007–2019)



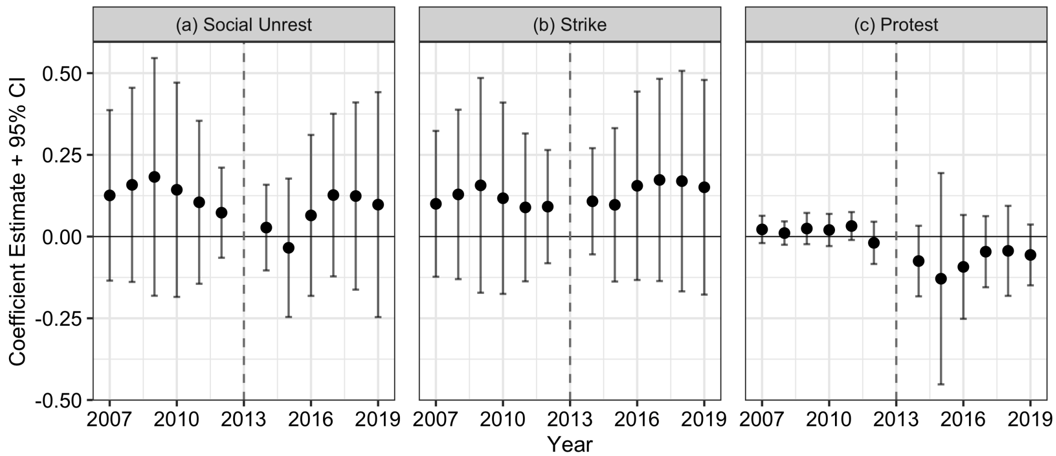
Notes: Event study coefficients and 95% confidence intervals for unrest per 100,000 sector workers, comparing treatment cities (population < 1 million) with control cities (population 1–5 million), by sector and incident scale. The denominator is secondary-sector employment for manufacturing and construction incidents, and tertiary-sector employment for transport and service incidents. Sector definitions as in Figure 5. Reference year is 2013. City and province-by-year fixed effects. Standard errors clustered at the city level. Megacities excluded.

Figure A2: Service Employment by Subsector (2007–2019)



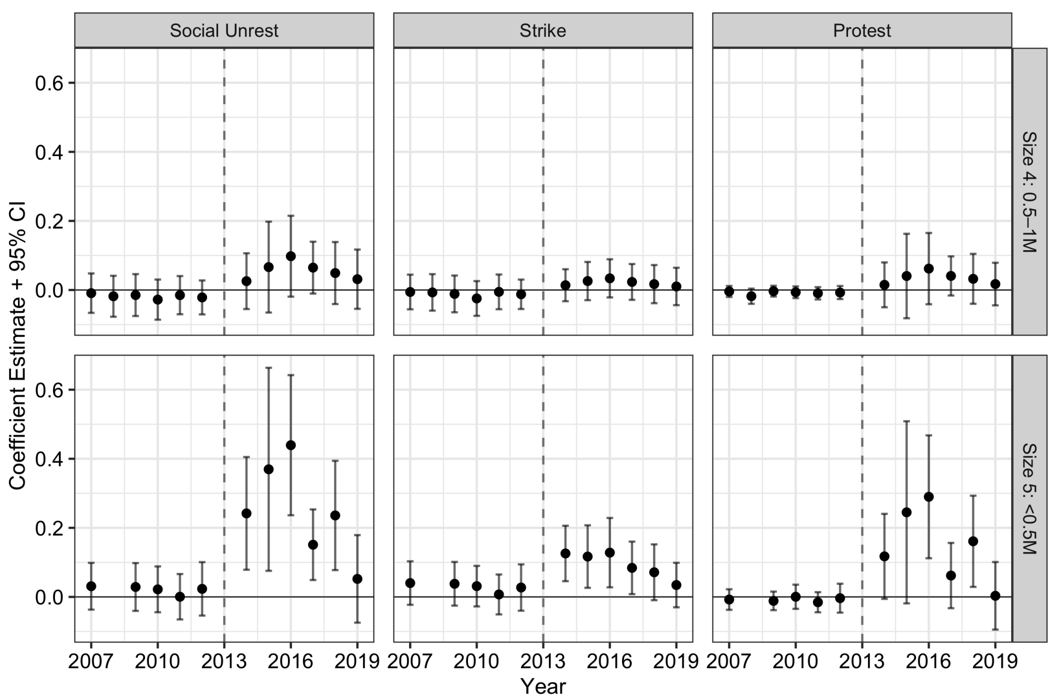
Notes: The figure reports event study coefficients and 95% confidence intervals for log employment in six service subsectors, comparing treatment cities (sizes 4–5, population below 1 million) to control cities (sizes 2–3, population 1–5 million). The top row shows (a) scientific and technical services, (b) finance and insurance, and (c) healthcare. The bottom row shows (d) transportation and communication, (e) wholesale and retail trade, and (f) social services. The omitted reference year is 2013. All regressions include city and province-by-year fixed effects with standard errors clustered at the city level. Megacities (population above 5 million) are excluded.

Figure A3: Event Study, 3 Million Population Cutoff



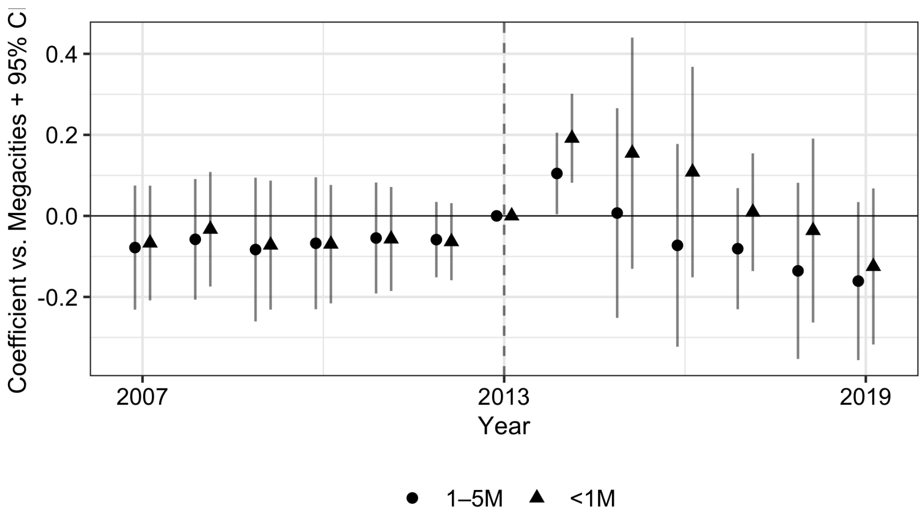
Notes: The figure plots event study coefficient estimates and 95% confidence intervals using a 3 million population cutoff. Treat = 1 for cities with population below 3 million (sizes 3–5, 262 cities); Treat = 0 for cities with population 3–5 million (size 2, 11 cities). Megacities (population above 5 million) are excluded. The dependent variable is per capita unrest per 100,000 registered population. The omitted reference year is 2013. All specifications include city and province-by-year fixed effects with standard errors clustered at the city level.

Figure A4: Event Study by City Size, Size 3 as Sole Control (1–3 Million Population)



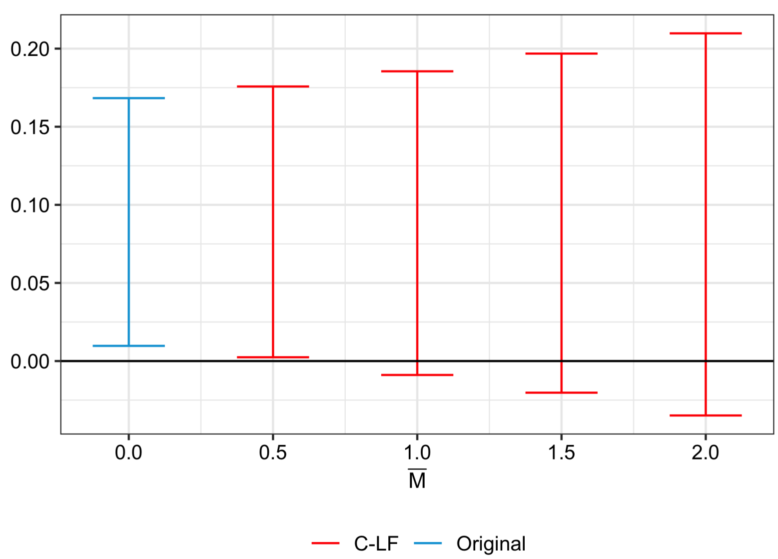
Notes: The figure plots event study coefficient estimates and 95% confidence intervals using size 3 cities (population 1–3 million, 108 cities) as the sole control group. Rows show size 4 cities (population 0.5–1 million, 103 cities) and size 5 cities (population below 0.5 million, 51 cities). Columns show overall labor unrest, strikes, and protests, each measured per 100,000 registered population. All specifications include city and province-by-year fixed effects with standard errors clustered at the city level.

Figure A5: Per Capita Labor Unrest with Megacities as Control (2007–2019)



Notes: Event study coefficients and 95% confidence intervals for overall per capita labor unrest (events per 100,000 registered population), using megacities (population above 5 million) as the reference group. Triangles represent treatment cities (sizes 4–5, population below 1 million). Circles represent control cities (sizes 2–3, population 1–5 million). Reference year is 2013. City and province-by-year fixed effects. Standard errors clustered at the city level.

Figure A6: Sensitivity to Violations of Parallel Trends



Notes: 95% confidence intervals for the pooled post-treatment effect on per capita overall unrest (events per 100,000 registered population) under the relative magnitudes approach of Rambachan and Roth (2023). Blue interval ($\bar{M} = 0$) assumes exact parallel trends. Red intervals allow post-treatment violations up to $\bar{M}$ times the maximum pre-treatment violation. Six pre-reform coefficients (2007–2012) and six post-reform coefficients (2014–2019).

A.2 Additional Tables

Table A1: Pre-Reform Sample Characteristics

	Treatment (<1M)		Control (1–5M)		Diff.
	Mean	SD	Mean	SD	
Registered population (10,000s)	61.33	(21.00)	169.55	(75.77)	-108.22
Per capita GRP (yuan)	42,250.33	(29,439.69)	51,315.42	(39,749.82)	-9,065.09
Secondary employment	54,759.86	(42,982.12)	186,058.87	(230,164.85)	-131,299.00
Tertiary employment	51,742.31	(24,335.94)	159,869.48	(170,202.26)	-108,127.17
Average wage (yuan)	33,627.18	(11,110.64)	35,606.05	(11,311.66)	-1,978.87
Public revenue per capita	3,703.38	(4,053.32)	4,995.76	(5,599.19)	-1,292.38
Public expenditure per capita	6,607.65	(4,754.46)	6,716.60	(5,789.37)	-108.95
Industrial enterprises	193.78	(191.10)	990.43	(1,515.37)	-796.66
Unrest events (count)	0.26	(0.70)	1.11	(4.34)	-0.85
Unrest events per 100k	0.04	(0.11)	0.06	(0.19)	-0.02
Cities	154		119		

Notes: Pre-reform means (2007–2013) for treatment cities (sizes 4–5, population below 1 million) and control cities (sizes 2–3, population 1–5 million). Population is registered (Hukou) population in units of 10,000. Per capita GRP, public revenue, and public expenditure are in yuan per registered resident. "Diff." reports the difference in means (treatment minus control). Megacities excluded.

Table A2: CLB Incident Counts by Year and City Size

	By City Size						Analysis Sample	
Year	Size 1	Size 2	Size 3	Size 4	Size 5	Total	Treatment	Control
2007	14	19	20	12	4	69	16	39
2008	17	18	38	13	0	86	13	56
2009	4	6	17	5	3	35	8	23
2010	27	20	34	4	5	90	9	54
2011	31	44	86	29	5	195	34	130
2012	82	80	158	45	14	379	59	238
2013	117	135	246	105	20	623	125	381
2014	201	208	555	265	82	1,311	347	763
2015	474	402	1,059	559	157	2,651	716	1,461
2016	485	323	1,038	550	184	2,580	734	1,361
2017	288	147	449	266	75	1,225	341	596
2018	363	191	648	346	105	1,653	451	839
2019	317	191	526	251	61	1,346	312	717

Notes: Total CLB-recorded labor unrest incidents (strikes and protests combined) by year and city-size category. "Treatment" pools sizes 4–5 (population below 1 million). "Control" pools sizes 2–3 (population 1–5 million).

Table A3: CMDS Migrant Sample Summary Statistics

	Treatment (<1M)		Control (1–5M)	
	Pre	Post	Pre	Post
Log monthly wage	7.854	8.056	7.869	8.150
	(0.553)	(0.575)	(0.507)	(0.525)
Age	35.687	36.763	34.836	36.084
	(8.822)	(9.662)	(8.589)	(9.432)
Male	0.549	0.551	0.522	0.538
	(0.498)	(0.497)	(0.500)	(0.499)
Low education ($\leq$ 9 yrs)	0.753	0.695	0.704	0.650
	(0.431)	(0.461)	(0.456)	(0.477)
Married	0.839	0.848	0.831	0.839
	(0.368)	(0.359)	(0.375)	(0.368)
Any social insurance	0.173	0.147	0.273	0.259
	(0.378)	(0.354)	(0.446)	(0.438)
Recent migrant ($\leq$ 3 yrs)	0.373	0.299	0.386	0.334
	(0.484)	(0.458)	(0.487)	(0.472)
Manufacturing	0.137	0.143	0.246	0.234
	(0.344)	(0.350)	(0.431)	(0.423)
Construction	0.098	0.078	0.082	0.074
	(0.297)	(0.268)	(0.275)	(0.261)
Transport	0.126	0.139	0.121	0.131
	(0.332)	(0.346)	(0.326)	(0.338)
Observations	51,546	97,822	108,148	206,798

Notes: Summary statistics from the China Migrants Dynamic Survey (CMDS), 2011–2017. Treatment cities have population below 1 million (sizes 4–5). Control cities have population 1–5 million (sizes 2–3). "Pre" covers 2011–2013; "Post" covers 2014–2017. Low education is defined as 9 or fewer years of schooling. Recent migrant is defined as having arrived in the current city within the past 3 years. Standard deviations in parentheses.

Table A4: Wild Cluster Bootstrap Inference

	Unrest/100k (1)	Strike/100k (2)	Protest/100k (3)
Panel A. Cluster-robust standard errors			
Treat × Post	0.104***	0.042***	0.060**
	(0.033)	(0.010)	(0.028)
Panel B. Wild cluster bootstrap ($B = 9{,}999$, Rademacher)			
Bootstrap p-value	0.001***	0.000***	0.037**
Bootstrap 95% CI	[0.040, 0.168]	[0.024, 0.061]	[0.004, 0.116]

Notes: Panel A reproduces the pooled DiD estimates (Treat × Post) with cluster-robust standard errors at the city level. Panel B reports p-values and 95% confidence intervals from a wild cluster bootstrap with 9,999 Rademacher draws (Cameron et al., 2008). Treatment = sizes 4–5 (population below 1 million). Control = sizes 2–3 (population 1–5 million). City and province-by-year fixed effects. Megacities excluded. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A5: Pre-Trends Joint F-Tests

Specification	F-statistic	p-value	Pre-reform coefficients
Panel A. Headline per capita specifications			
Unrest/100k	1.00	0.422	6
Strike/100k	0.76	0.603	6
Protest/100k	0.40	0.882	6
Panel B. Industry-participant subcategories			
Manufacturing Large	1.73	0.109	6
Manufacturing Small	0.64	0.672	5

Notes: Panel A tests headline per capita specifications. Panel B tests manufacturing industry-participant subcategories (Large = 100+ participants, Small = fewer than 100). All specifications include city and province-by-year fixed effects with standard errors clustered at the city level.

Table A6: Difference-in-Differences Estimates for Log Registered Population

	(Log) Population (1)
Treat × Post	0.058*
	(0.033)
Dep. var. mean (log)	4.496
Observations	3,487
R^2	0.95

Notes: Treat × Post coefficient for log registered population. Treatment = sizes 4–5 (population below 1 million). Control = sizes 2–3 (population 1–5 million). Post = 1 for years after 2013. Dep. var. mean reports the pre-reform sample mean (2007–2013). City and province-by-year fixed effects. Standard errors clustered at the city level. Megacities excluded. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A7: OLS vs. PPML on Per Capita Outcomes

	OLS			PPML		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	0.104***	0.042***	0.060**	0.304**	0.506***	0.231
	(0.033)	(0.010)	(0.028)	(0.150)	(0.170)	(0.227)
Observations	3,487	3,487	3,487	3,158	2,997	2,498

Notes: Columns (1)–(3) use OLS; columns (4)–(6) use Poisson pseudo-maximum likelihood (PPML). OLS coefficients are level effects; PPML coefficients are log-multiplicative. All specifications include city and province-by-year fixed effects with standard errors clustered at the city level. Megacities excluded. $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A8: Heterogeneity by Industry and Incident Scale, Per Capita Rates

	Estimate	*N*
Manufacturing		
Small (1–100 participants)	0.019*	3,549
	(0.012)	
Large (100+ participants)	0.013***	3,549
	(0.004)	
Construction		
Small	0.035**	3,549
	(0.014)	
Large	0.002	3,549
	(0.003)	
Transport		
Small	0.027***	3,549
	(0.005)	
Large	0.005*	3,549
	(0.003)	
Services		
Small	0.001	3,549
	(0.006)	
Large	0.002	3,549
	(0.003)	

Notes: Each column reports the pooled DiD estimate (Treat × Post) for per capita unrest (events per 100,000 registered population). Sector definitions: Manufacturing (manufacturing, heavy industry, and mining); Construction; Transport (transport, storage, logistics, and postal services); Services (retail, education, and the public sector). Incident scale: small = 1–100 participants; large = 100+. Standard errors clustered at the city level. All specifications include city and province-by-year fixed effects. Megacities excluded. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A9: Heterogeneity by Industry and Scale, Per Sector-Worker Rates

	Estimate	N
Manufacturing		
Small (1–100 participants)	0.398*	3,549
	(0.227)	
Large (100+ participants)	0.187*	3,549
	(0.097)	
Construction		
Small	0.724*	3,549
	(0.377)	
Large	-0.008	3,549
	(0.046)	
Transport		
Small	0.205***	3,549
	(0.061)	
Large	0.052	3,549
	(0.033)	
Services		
Small	-0.014	3,549
	(0.056)	
Large	0.013	3,549
	(0.027)	

Notes: Treat × Post coefficients for unrest per 100,000 sector workers. Manufacturing and construction use secondary-sector employment as the denominator. Transport and services use tertiary-sector employment. Sector and scale definitions as in Table A8. City and province-by-year fixed effects. Standard errors clustered at the city level. Megacities excluded. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A10: Employment Responses by Sector

Panel A. Broad Sectors

	Secondary (1)	Tertiary (2)
Treat × Post	-0.041	0.042*
	(0.045)	(0.022)
Observations	3,476	3,476
R^2	0.95	0.97

Panel B. Service Subsectors

	SciTech (1)	Finance (2)	Health (3)	Transport (4)	Retail (5)	SocialServ (6)
Treat × Post	0.038	0.018	0.081	0.101*	0.071	0.040
	(0.063)	(0.036)	(0.081)	(0.053)	(0.056)	(0.078)
Observations	3,476	3,476	3,194	3,476	3,476	3,190
R^2	0.91	0.92	0.88	0.92	0.90	0.92

Notes: Treat × Post coefficients for log employment. Panel A reports secondary and tertiary sectors. Panel B decomposes tertiary employment into six subsectors. Treatment = sizes 4–5 (population below 1 million). Control = sizes 2–3 (population 1–5 million). City and province-by-year fixed effects. Standard errors clustered at the city level. Megacities excluded. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A11: Economic and Fiscal Responses

	AvgWage (1)	Output (2)	RevPC (3)	ExpPC (4)
Treat × Post	-0.006	0.049	-0.074*	-0.052
	(0.014)	(0.034)	(0.039)	(0.035)
Observations	3,476	3,461	3,487	3,487
R^2	0.95	0.99	0.94	0.90

Notes: Treat × Post coefficients for log outcomes. Columns report (1) log average wages, (2) log industrial output, (3) log local public revenue per capita, and (4) log local public expenditure per capita. Treatment = sizes 4–5 (population below 1 million). Control = sizes 2–3 (population 1–5 million). City and province-by-year fixed effects. Standard errors clustered at the city level. Megacities excluded. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A12: Migrant Composition Response to Hukou Reform (2011–2017)

	Recent (1)	Low Edu. (2)	Intra-prov. (3)	Any Insur. (4)
Treat × Post	0.004	-0.0007	0.007	-0.021**
	(0.008)	(0.005)	(0.006)	(0.008)
Pre-reform control mean	0.386	0.700	0.513	0.301
Individual controls	✓	✓	✓	✓
Observations	485,496	485,496	485,496	196,046
R^2	0.06	0.21	0.26	0.29

Notes: Each column reports the pooled DiD estimate (Treat × Post) for the indicated migrant characteristic. Recent = 1 if arrived within 3 years. Low education = 1 if 9 or fewer years of schooling. Any insurance = 1 if holding any social insurance. Individual controls include gender, marital status, and Hukou type. All specifications include city, province-by-year, and occupation-by-year fixed effects with standard errors clustered at the city level. Data from the CMDS. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A13: Robustness of CMDS Migrant Results (2011–2017)

Panel A. Migrant Wage Robustness

	All (1)	Mfg. (2)	Constr. (3)	Transport (4)	Other Svc. (5)
(a) Baseline	-0.037***	-0.017	-0.041***	-0.038**	-0.041***
	(0.009)	(0.012)	(0.015)	(0.015)	(0.012)
(b) No Occ×Year FE	-0.045***	-0.021	-0.037**	-0.039**	-0.051***
	(0.010)	(0.013)	(0.016)	(0.015)	(0.012)
(c) No Hukou control	-0.037***	-0.017	-0.040***	-0.038**	-0.041***
	(0.009)	(0.012)	(0.015)	(0.015)	(0.012)
Pre-reform control mean	7.885	7.876	8.028	7.777	7.892
Observations	424,723	99,096	36,890	59,115	229,622

Panel B. Sector Choice Robustness

	Mfg. (1)	Constr. (2)	Transport (3)	Other Svc. (4)
(a) Baseline	0.003	-0.004	-0.001	0.017***
	(0.004)	(0.002)	(0.002)	(0.005)
(b) No Occ×Year FE	0.006	-0.007*	0.004	0.015**
	(0.006)	(0.004)	(0.004)	(0.008)
(c) No Hukou control	0.003	-0.004	-0.001	0.017***
	(0.004)	(0.002)	(0.002)	(0.005)
Pre-reform control mean	0.246	0.082	0.121	0.441
Observations	485,496	485,496	485,496	485,496

Notes: Panel A reports the Treat × Post coefficient with log monthly wage as the dependent variable. Panel B uses a sector indicator as the dependent variable. Row (a) is the baseline from Table 3. Row (b) drops occupation-by-year FE. Row (c) drops the Hukou type control. All specifications include individual controls, city FE, and province-by-year FE. Standard errors clustered at the city level. Data from the CMDS (2011–2017). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A14: Migrant Wage Effects by Tenure (2011–2017)

	Mfg. (1)	Constr. (2)	Transport (3)	Other Svc. (4)
Treat × Post	-0.021	-0.037**	-0.032*	-0.034**
	(0.014)	(0.017)	(0.018)	(0.013)
Treat × Post × Recent	0.017	-0.006	-0.014	-0.020
	(0.019)	(0.022)	(0.021)	(0.013)
Pre-reform control mean	7.876	8.028	7.777	7.892
Observations	99,096	36,890	59,115	229,622
R^2	0.36	0.28	0.26	0.24

Notes: Each column reports the Treat × Post coefficient and its interaction with a Recent indicator (arrived within 3 years). Columns restrict to migrants in the indicated sector. Individual controls, city, province-by-year, and occupation-by-year fixed effects. Standard errors clustered at the city level. Data from the CMDS (2011–2017). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A15: Robustness Check, Classification Using 2014 Population

	Unrest/100k (1)	Strike/100k (2)	Protest/100k (3)
Treat × Post	0.122***	0.052***	0.069**
	(0.033)	(0.011)	(0.029)
Observations	3,205	3,205	3,205
R^2	0.64	0.49	0.61

Notes: Treat × Post coefficients for per capita unrest (events per 100,000 registered population) using 2014 population for city size classification instead of the baseline 2013 classification. Treatment = sizes 4–5 (population below 1 million in 2014). Control = sizes 2–3 (population 1–5 million in 2014). City and province-by-year fixed effects. Standard errors clustered at the city level. Megacities excluded. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A16: Count-Based Estimates (PPML)

	Unrest (1)	Strike (2)	Protest (3)
Treat × Post	0.24**	0.44***	0.24
	(0.10)	(0.12)	(0.19)
Observations	3,163	3,002	2,502

Notes: Treat × Post coefficients from PPML regressions using raw event counts. PPML coefficients are semi-elasticities. Treatment = sizes 4–5 (population below 1 million). Control = sizes 2–3 (population 1–5 million). City and province-by-year fixed effects. Standard errors clustered at the city level. Megacities excluded. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A17: Alternative Population Cutoffs

Panel A. 3 Million Cutoff

	Unrest/100k	Strike/100k	Protest/100k
	(1)	(2)	(3)
Treat × Post	-0.045	0.045	-0.087
	(0.052)	(0.032)	(0.066)
Observations	3,487	3,487	3,487
R^2	0.63	0.47	0.61

Panel B. Size 3 as Sole Control Group

	Unrest/100k	Strike/100k	Protest/100k
	(1)	(2)	(3)
Treat × Post	0.115***	0.041***	0.072**
	(0.034)	(0.010)	(0.028)
Observations	3,344	3,344	3,344
R^2	0.63	0.44	0.61

Notes: Panel A uses a 3 million cutoff (Treat = sizes 3–5, 262 cities; Control = size 2, 11 cities). Panel B uses size 3 cities only (108 cities, population 1–3 million) as the control group. Post = 1 for years after 2013. City and province-by-year fixed effects. Standard errors clustered at the city level. Megacities excluded. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A18: DiD Estimates with Megacities as Control

Panel A. Time-Varying Population

	Unrest/100k (1)	Strike/100k (2)	Protest/100k (3)	Log Pop (4)
1–5M × Post	0.0006	0.024*	-0.023	0.003
	(0.091)	(0.014)	(0.090)	(0.074)
<1M × Post	0.103	0.066***	0.035	0.057
	(0.092)	(0.016)	(0.089)	(0.077)
Observations	3,643	3,643	3,643	3,643
R^2	0.64	0.47	0.62	0.97

Panel B. Fixed 2010 Population

	Unrest/100k (1)	Strike/100k (2)	Protest/100k (3)
1–5M × Post	0.042	0.033**	0.009
	(0.098)	(0.015)	(0.093)
<1M × Post	0.177*	0.087***	0.088
	(0.098)	(0.018)	(0.091)
Observations	3,617	3,617	3,617
R^2	0.66	0.49	0.64

Notes: Treat × Post coefficients using megacities (population above 5 million) as the reference group. "<1M" denotes sizes 4–5. "1–5M" denotes sizes 2–3. Panel A uses time-varying registered population as the denominator. Panel B uses 2010 registered population as a fixed denominator. Post = 1 for years after 2013. City and province-by-year fixed effects. Standard errors clustered at the city level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.