From Settlement to Stability: The Political Impact of Relaxing Migration Barriers in China

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Abstract

There are growing concerns that relaxing migration policy may undermine social stability. We study this issue by estimating the causal effect on labor unrest of China's recent reform to its internal migration institutions, which facilitated permanent settlement for migrants in small and medium sized cities. Exploiting variation due to the reform's population cutoff rule, we find that the reform significantly reduced labor unrest. We suggest that one mechanism behind our finding is the enhancement of migrants' settlement intentions, which promotes obedient behavior to seize the opportunity of settlement brought by the reform. Evidence shows that the reform raised the likelihood of migrants to continue staying in their destinations. Through a novel causal mediation analysis, we find that heightened settlement intentions can explain 61 percent of the reform's total effect on labor unrest in the immediate term and 27 percent in the longer term. We find no evidence that the reform led to compositional changes among migrants, immediate deliveries of benefits to migrants, and tighter government social control. Our results highlight the influence of migration policy on stability by shaping migrants' attachment to their destinations.

Keywords: Migration; Unrest; Settlement; Urbanization; Household Registration; China.
JEL Codes: D74; D79; E24; O15.

1   Introduction

Migration policy has become a focal point of politics. Interestingly, many concerns revolve around the impact of migration policy on social stability. In democracies, some voters and politicians perceive immigration as a source of social turmoil (e.g., crime, terrorism) and therefore lean toward anti-immigration platforms (e.g., Bianchi et al., 2012; Hangartner et al., 2019; Campante et al., 2023).[1] The concern about instability from migration holds equal salience in autocracies. Autocrats may worry about population concentration in their seat of power such as cities, and thus, they restrict free population movements or instead adopt measures to pacify migration hotspots (Wallace, 2014).[2]

Despite these concerns, the causal linkage between migration policy and social stability is not straightforward. Existing theory and evidence give mixed predictions due to the existence of multiple intervening factors. First, relaxing migration barriers may bring more population. This may result in instability because of more competition for limited resources (Acemoglu et al., 2020). However, compositional changes can have ambiguous influences on social stability. Relatedly, the literature on the effect of immigration on crime does not reach a consensus (e.g., Spenkuch, 2014; Ajzenman et al., 2023).[3] Second, a more friendly migration policy may raise migrants' attachment to the destinations and thus incentivize investments in integration (Adda et al., 2022). If this forward-looking behavior increases the cost of engaging in unrest, we may see a higher level of social stability. All in all, it is an empirical question of how social stability responds to migration policy changes.

To help answer this question, we look into the Chinese context. China is an intriguing setting to study the relationship between migration policy and social stability. China's household registration (a.k.a. *hukou*) system ties each person to a registration location, and on that basis, it determines *where* the person has access to public services (e.g., public education, healthcare, subsidized housing, and social security, etc.). It is difficult to transfer registration across regions, especially from rural areas to urban areas. This means that one would lose access to valuable public services once leaving the registration location. Therefore, although movement is in general not restricted in contemporary China, the *hukou* system has created a high migration cost and made migration tend to be temporary

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[1]As motivating piece, Bianchi et al. (2012) document that during 1995–2003, a high fraction of people in OECD countries exhibited concerns that immigrants increased crime rates; and in some cases, the fraction was higher than the fraction of worrying about being displaced from the job market. Hangartner et al. (2019) show that mere exposure to refugee arrivals induces lasting concerns about social turmoil caused by immigration (e.g., crime, terrorist attacks, and burden on the country). Speaking to electoral consequences, Campante et al. (2020) show that during the 2014 US midterm elections, the Republican candidates drew associations between the Ebola in connection and immigration and terrorism in newsletters and campaign ads, despite the fact that Ebola had negligible impacts on American society.

[2]Wallace (2014) offers a comprehensive analysis about the threats that cities pose to autocratic regimes. Zooming into China, he discusses how the Chinese state has survived in several challenges with the help of various migration restrictions and development strategies.

[3]Spenkuch (2014) find a small positive effect of immigration on crime in the US. In contrast, Ajzenman et al. (2023) report a null effect in Chile, however, they find that immigration leads to a *(mis-)perception* of increased crime.

([Meng, 2012](#)). Due to these features, some scholars label the *hukou* system as a "local citizenship system" or an "internal passport system" despite China being a unified country ([Jin and Zhang, 2023](#); [Au and Henderson, 2006](#)), and they draw similarities between internal migration in China and international migration ([Chan, 2019](#)).

We exploit a natural experiment from the *hukou* reform: in July 2014, the Chinese government initiated a nationwide reform to the *hukou* system, making it easier for migrants to transfer their *hukou* to small and medium sized cities and thus attain permanent settlement there. We estimate the causal effect of this *hukou* reform on social stability during 2011–2019. We measure social stability using labor unrest, which includes workers' collective actions to protest against employers and demand public attention. A large literature in China's labor politics underscores that migrants have constituted the majority of participants in labor unrest in contemporary China (e.g., [Lee, 2007](#); [Chan, 2010](#); [Friedman, 2014](#); [Rho, 2023](#), among others). In the meantime, labor unrest is a type of social unrest that the Chinese state remains very vigilant about, despite its mostly apolitical and non-anti-regime nature ([Friedman, 2014](#)). And the state adopts various measures to prevent labor unrest from threatening regime survival.[4]

Underpinning our research design is a population-based rule that determines reform status. The 2014 *hukou* reform substantially relaxed *hukou* transfers into regions with less than 3 million urban population while keeping strict restrictions in more populous regions. Exploiting this discontinuity in urban population, we implement a difference in discontinuity (DiDC) design, which combines difference-in-differences and regression discontinuity designs. In a nutshell, we estimate the causal effect of the reform by comparing the trends in unrest between reform and non-reform regions near the reform cutoff. Identification follows if the underlying trends vary smoothly across the reform cutoff. A variety of exercises validate this assumption. First, McCrary's test shows that there is no discontinuity in the density of urban population at the 3 million cutoff, both before and after the reform. This suggests a lack of sorting into specific reform status. Second, we test the existence of manipulation of urban population statistics by local officials. Specifically, given the role of promotion incentives in shaping Chinese officials' behaviors (e.g., [He et al., 2020](#); [Zeng and Zhou, 2024](#)), we examine the association between reported growth in urban population and a measure of local officials' promotion prospects — with the idea that manipulation must benefit officials' careers for it to exist, so manipulation if at all would be more significant among officials with stronger promotion prospects. We find no discernible association between reported growth in urban population and promotion prospects, which suggests the absence of deliberate manipulation of urban population. Furthermore, we show that our research design balances out pre-reform trends in the unrest rate and other variables that may influence occurrences of unrest, including population, GDP, expenditures on public security (i.e., policing), and expenditures on social security.

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[4]We provide a more detailed discussion of labor unrest in China in Section [2.2](#).

We find that the *hukou* reform reduced labor unrest by 1.419 (95 percent confidence interval [0.685, 2.153]) incidents per million prime-age population. This effect is sizeable: it amounts to 42 (95 percent confidence interval [20, 63]) percent of the mean of non-reform regions. We show that this effect is not likely a consequence of confounding factors related to urban population or a statistical artifact. First, we perform a placebo test that estimates the "effects" at other population cutoffs. Second, the results cannot be explained by changes in population. We find that the reform has no discernible effect on both total population and urban population during the period we study. And we again find a significant negative effect of the *hukou* reform on labor unrest after accounting for the evolution of population sizes. Lastly, as our measure of labor unrest is based on online information, we present evidence that our results are not driven by different patterns in reporting local events, internet censorship, or self-censorship.

We perform a body of robustness tests for our results, including alternative specifications and estimators, exclusion of likely outliers, and covariates balancing. In particular, we present consistent results derived from a supplementary identification strategy. It explores the heterogeneous response by *hukou* reform status to fluctuation in global demand for manufactured goods. We find that although the exogenous negative shock on average increases unrest, the impact is much weaker in reform regions, suggesting that the *hukou* reform may have altered underlying patterns of participation in unrest.

We carefully examine the mechanisms through which the *hukou* reform reduced labor unrest. One mechanism we primarily argue is: the *hukou* reform may raise migrants' settlement intentions by offering the previously unattainable opportunity of permanent settlement, and for migrants who desire to settle in the destination but have not yet made it, they may behave in an obedient way due to the fears that government and employer retaliations may make them unable to seize the opportunity. We provide a simple model to rationalize this mechanism. Empirically, using a dataset with information on migrants' destinations, we find that the *hukou* reform at the initial destination significantly reduced the likelihood of migrants leaving the initial destination. In addition, we find that the reduction in outmigration likelihood is more pronounced among more forward-looking migrants, who value benefits of prospective settlement to a larger extent. This further corroborates the hypothesis that the *hukou* reform raised settlement intentions.

We also establish the bridge between heightened settlement intentions and decreased labor unrest. We do so by implementing a novel causal mediation method, by which we can quantify the causal effect on labor unrest of the *hukou* reform through increasing settlement intentions.

Our methodology for causal mediation advances the conventional approach in social sciences, and it is simple to implement using linear regressions. The conventional practice, popularized by Baron and Kenny (1986), relies on the comparison of coefficients on treatment (here, the *hukou* reform) between a model that excludes the mechanism variable and a model that includes the mechanism variable. The change in the coefficient on treatment is viewed as the treatment effect that

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goes through the proposed mechanism. Though intuitive, this approach requires additional, strong assumptions. A key assumption is for consistently estimating the causal effect of the mechanism variable: the mechanism variable must be exogenous conditional on the treatment (e.g., among others, Imai et al., 2011). This assumption may be overly strong due to the existence of other interrelated post-treatment confounders. Our approach preserves the simplicity of looking at the change in the coefficient on treatment. But we employ an instrumental variable (IV) for the mechanism variable to identify the causal effect of the mechanism variable. A key assumption in our method is homogeneity of the effect of the mechanism variable on the outcome. This assumption allows a researcher to extrapolate the IV-identified effect in one population to another population.

When applying this method to our case, we construct a shift-share IV that leverages variation in trade shocks in migrants' origins. This IV is strongly associated with settlement intentions and passes the balance tests recommended by Borusyak and Hull (2024). With this instrumentation, our results show that our measure of heightened settlement intentions, namely, decreased outmigration rates, can account for 60.7 percent of the *hukou* reform's total effect on labor unrest in the immediate term (2011–2015), and 27.3 percent in the longer term (2011–2019). We are aware that our results rest on the assumption that the effects of the mechanism variable on the outcome are homogeneous. Relaxing the assumption may introduce bias in the estimate of the importance of a mechanism. We provide a simple sensitivity test for our conclusion when the homogeneity assumption is relaxed. The results suggest that even if a significant amount of bias is allowed, we can still conclude that heightened settlement intentions explain a nontrivial fraction of decreased labor unrest due to the *hukou* reform.

Besides the settlement intentions mechanism, we reject other alternative mechanisms through which *hukou* reform may reduce labor unrest: (i) shifts in characteristics within migrants, (ii) immediate improvements in benefits available to migrants (e.g., labor market outcomes and family union), and (iii) local governments' social control.

In sum, we show evidence of the relaxation of migration barriers — in our case, driven by China's *hukou* reform — enhancing social stability as measured by labor unrest. To some degree, this is surprising, given popular concerns that an open-door migration policy could result in social turmoil. Although we cannot exhaust all underlying mechanisms, we provide evidence suggesting that the removal of migration barriers could induce migrants to behave in an obedient manner, which can facilitate their integration and generate benefits in the long run. This is analogous to immigrants' economic choices for assimilation (Dustmann and Görlach, 2016). This finding can certainly depend on the context in question — for instance, if the state does not have credible command over people's welfare, i.e., the biopower as conceptualized by Michael Foucault (1990),[5]

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[5]The concept biopower is developed by Michael Foucault to describe the way in which modern states exercise power over their citizens by focusing on aspects of human life. Biopower operates by shaping norms and policies to manage life and maintain social order.

then obedience may not be coerced. The Chinese state may be special in this regard as it possesses strong biopower, given its monopoly of all kinds of resources and its overwhelming dominance over civil society (Acemoglu and Robinson, 2020, Ch. 7). Yet, we envision that the mechanism we discover may apply broadly to autocratic regimes. Even in democracies, there can be scope for the mechanism to play out. For instance, Gonçalves et al. (2024) argue that heightened immigration enforcement in the US could increase crime as the fears it creates may discourage victims from reporting offenses.

Our paper relates to several strands of literature. First, this paper adds to the research on political control (for a review, see Hassan et al., 2022). Specifically, our paper speaks to the non-violent tactics used by the state to induce compliance of the citizens. One prominent example is buying hearts and minds through government transfers and employment (e.g., Pan, 2020; Rosenfeld, 2021; Borjas, 1980; Fish, 1905). Relatedly, the world's first welfare state was created by Bismarck to counter rising social democratic movements (Kersting, 2023). Our paper presents causal evidence for a core mechanism to induce compliance, i.e., dependence on the state.

Second, our paper contributes to a small literature on China's *hukou* system. Exploiting variation due to different episodes of *hukou* reforms, several papers have studied the *economic* consequences of *hukou*-induced migration barriers, including labor market outcomes, marriage market matching, and productivity, among others (e.g., An et al., 2024; Han et al., 2015; Ngai et al., 2019). However, there is little attention to the *hukou* system's *political* implications. In some sense, this is surprising since scholars have the consensus that the *hukou* system is bad economics, and some have hinted that the system continues to exist due to political constraints.[6] Then, a natural question is whether the *hukou* system is good politics. Our paper fills this considerable gap, and our results suggest that the *hukou* system can even be bad politics, as the denial of migrants' settlement in the destination implicitly creates incentives for participation in unrest.

Last but not least, our paper engages in the literature on causal mediation, which focuses on formally disentangling how much of the average effect of a treatment can be attributed to the treatment effect on a mechanism (Imai et al., 2011; Pearl, 2009). The work by Frölich and Huber (2017) is the closest to ours; they present a framework of non-parametric identification using two IVs separately for treatment and mechanism variables. In contrast, we develop a simple, regression-based approach, and we also provide a simple method for assessing the robustness of conclusions to potential bias introduced by instrumentation.

The remainder of this paper is organized as follows. Section 2 provides information on institutional context. Section 3 describes our data and research design. Section 4 reports the effect

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[6]For instance, Au and Henderson (2006) argue that Chinese cities are undersized despite high urban agglomeration benefits; they claim that the *hukou* system is maintained in part due to "political pressure by urban residents who fear vast influxes of peasants." Ngai et al. (2019) document that the *hukou* system distorts labor allocations and thus causes efficiency losses. We refer interested readers to other studies that underscore the *hukou* system's economic costs: e.g., Adamopoulos et al. (2024); Gai et al. (2024); among others.

of *hukou* reform on labor unrest. Section 5 explores underlying mechanisms. Section 6 concludes. Additional results and discussions can be found in Online Appendices.

2 Institutional Context

2.1 China's *Hukou* System

In this section, we first briefly describe China's *hukou* (household registration) system and how it functions as an institution that obstructs free migration. We refer interested readers to Chan (2019) for a more comprehensive account. Then, we provide key information on the 2014 *hukou* reform that we study in this paper.

A Brief Overview. The *hukou* (household registration) system was instituted in 1958. It assigns each Chinese citizen a *hukou* certificate upon birth, which ties them to a locality (typically parents' registration locality), and on that basis, the system determines a person's eligibility for state transfers and public services. The *hukou* status has two aspects: location and type. A person can only have access to state transfers and public services in their registration locality, even if their *de facto* residential location is different. For a long time, there were two types of *hukou*: agricultural and non-agricultural. Rural residents typically got an agricultural *hukou*,[7] and they were given land for cultivation to feed themselves, and they were given access to some social services provided by their rural localities. Urban residents obtained a non-agricultural *hukou*, and they were expected to work in factory or office jobs and had access to social benefits, many of which were job-related and included food rations, subsidized medical care, education for their children, and social assistance, funded by the urban district that issued their *hukou*. Starting from the 1990s, some localities gradually removed the distinction in *hukou* types, and this was eventually extended to the entire country in 2014 (in fact, part of the reform we study). However, the key aspect of the *hukou* system remains unchanged: One can only have access to state transfers and public services in their registration locality.

The *Hukou* System as Migration Barriers. Under Mao, the *hukou* was created to restrict population mobility — people were expected to stay in their registration localities — which facilitated the government's urban-biased industrialization strategy that required extracting resources from vast rural areas and subsidizing urban areas. Transfers of *hukou* across regions were difficult, especially rural-urban transfers, making permanent migration nearly impossible. Successful transfers were only possible via e.g., state jobs, military service, and higher education. Even short-term trips required permits from the police, otherwise the persons would be expelled back to their *hukou* localities (Cheng and Selden, 1994). In addition, under central planning, since

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[7]An exception is the officials who work in rural areas.

most jobs were controlled by the state and food was rationed according to *hukou* location, mobility restrictions could be strictly enforced.

After Deng Xiaoping's reforms, the *hukou* system was gradually relaxed, eventually (by the late 1990s) allowing free population movements across the country. However, the *hukou* system continues to exist. In the reform era, the management of the *hukou* system was delegated to local governments. The local governments typically do not have strong incentives to allow transfers into their jurisdictions since that would increase their fiscal burdens for social services. Therefore, limited transfers of *hukou* are often made available to attract people with financial means, such as investors, home buyers, or highly educated professionals. As such, transfers of *hukou* remain difficult. Recall that access to public services is tied to *hukou* registration, the inability to transfer *hukou* constitutes a considerable cost of migration. Eli Friedman, a renowned scholar in China's labor politics, nicely summarizes this phenomenon as *"urbanization of labor [rather than people],"* that is, most people are welcomed to move and work in cities but are not expected to permanently settle (Friedman, 2022). Indeed, many migrations in China exhibit temporariness, with an average migrant only staying in the destination for 5–7 years (Meng, 2012 and our own calculations).

The 2014 Reform. On July 24, 2014, the Chinese central government initiated a nationwide reform drive to the *hukou* system (State Council, 2014a). Unlike previous reforms that had been carried out by local governments, this one was centrally mandated. At the time, it was widely perceived as one of the strongest attempts to reform the *hukou* system in the previous two decades (Wang et al., 2023).

Critical to our research design discussed later, this reform has a population-based rule to specify provisions on criteria of granting *hukou* transfers to urban areas. It is summarized in Table 1. Cities are categorized into five groups according to their urban population sizes: > 5 million, 3–5 million, 1–3 million, 0.5–1 million, and < 0.5 million. These cutoffs come from the Chinese government's official categorization of city sizes (State Council, 2014b). The central government's objective is to push the urbanization of medium and small cities while maintaining strict control over the expansion of large cities. Overall, the criteria of granting local *hukou* are much stricter for larger cities. Large cities, with an urban population exceeding 3 million, are directed to maintain tight control of *hukou* transfers; moreover, they are required to maintain a points-based system to only incorporate select migrants, like in international migration settings. In contrast, the criteria are much more lenient in medium and small cities, i.e., those with less than 3 million urban population. They are expected to accept a much broader base of migrants so long as a migrant has a stable job and residential place as well as a certain length of enrollment in local social security.[8] Given this feature, we focus on the 3 million cutoff, at which the criteria of granting local *hukou* are mandated to relax substantially. In fact, in a follow-up directive in 2016, the central government reiterated

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[8]Having a stable job is defined as having an employment contract or being a business owner (with minimum requirements on tax payments and/or registered capital). Having a stable residential place requires either a rental contract registered with the government or home ownership.

that cities with an urban population below 3 million must abolish all barriers to *hukou* transfers (State Council, 2016).

Table 1. Summary of the 2014 *Hukou* Reform

Urban Population	Provisions on granting local *hukou*
> 5 million	Point-based screening rules must be established.
3–5 million	Urged to establish point-based screening rules. Must be stricter than the next-tier cities.
1–3 million	Having a job, residence, and 1-5 years enrollment in basic social security.
0.5–1 million	Having a job, residence, and 1-3 years enrollment in basic social security.
< 0.5 million	Having a job, residence.

Note: This table summarizes the provisions of the 2014 *hukou* reform (State Council, 2014a).

2.2 Labor Unrest in China

Despite China's autocratic regime, labor unrest is common in China. Several structural factors have contributed to this phenomenon. Noticeably, voluminous studies on China's labor politics stress the role of institutional discrimination against migrant labor as a result of the *hukou* system (e.g., Lee, 2007; Chan, 2010; Friedman, 2014; Rho, 2023, among others). Restricted social and economic mobility for migrants due to limited rights, combined with employers' rampant violations of basic statutory protections, fuel migrants' grievances and thus contribute to the occurrences of labor unrest. More recently, legal reforms and labor shortages in labor-intensive sectors have shifted bargaining power in favor of migrant workers (Gallagher, 2017; Elfstrom and Kuruvilla, 2014). Despite lack of official data on migrant labor's participation in labor unrest, based on various sources of anecdotes and fieldwork, many scholars believe that migrant workers have make up the majority of participants in labor unrest, especially those offensive actions that demand more interests other than defend minimum rights (Friedman, 2014; Rho, 2023; Goebel, 2019).[9] Corroborating this view, Figure A1 shows a positive relationship between the labor unrest rate and the share of migrants in a region.

The Chinese state remains vigilant about labor unrest, and it has been increasingly so in the recent decade (Franceschini and Nesossi, 2018; Rho, 2023). Lorentzen et al. (2013) argue that the central government strategically tolerates labor unrest where workers voice demands for their rights and interests, because unrest can serve as a signal for the central government to identify discontented groups, and on that basis, it can help the central government to allocate resources to

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[9]Friedman (2014, pp.14) claims that "anecdotal evidence suggests that they [migrants] are the primary actors in contemporary insurgency." According to Rho (2023, pp.8), in 2010, migrant workers' labor disputes comprised nearly 70 percent of all labor disputes in Beijing. Goebel (2019) analyzes social unrest on social media and finds that migrant workers have engaged in the largest number of protests.

address grievances and manage local officials accordingly. However, the tolerated space of unrest has been codified in informal rules with an implicit warning that those who cross the boundary of acceptable protests will be repressed. It is not tolerated when an unrest event tends to extend to a mass collective action that threatens social stability. For instance, Rho (2023) finds that police are much more likely to intervene when workers go beyond the factory compound to protest. The regime has strictly restricted and punished independent labor organizing and social mobilization across workplaces or regions (Chen and Gallagher, 2018). Additionally, due to the cadre evaluation system's emphasis on stability maintenance (Edin, 2003), local officials respond to unrest seriously and use various measures to reduce potential threats to stability (Campante et al., 2023; You et al., 2022).

3   Data and Research Design

3.1   Sample Construction and Key Variables

Unit of Analysis. In this study, the unit of analysis is the prefecture. Prefectures, sometimes referred to as prefectural cities or simply cities, represent the administrative level between provinces and counties. In total, there are 333 prefectures.[10] We also consider 4 provincial-level municipalities: Beijing, Tianjin, Shanghai, and Chongqing, which, for brevity, we call prefectures in this paper. When constructing the sample, we exclude prefectures in Tibet and Xinjiang due to their distinct political environments. Our final sample consists of 287 prefectures for which data on urban population are available to define reform status (discussed next).[11] According to the population census of 2010, these prefectures account for 94.4 percent of the total population and 95.8 percent of the urban population in China.

Reform Status. As discussed in Section 2, a prefecture becomes subject to the relaxation of migration barriers if its urban population falls below 3 million. Thus, to define the reform status, it is crucial to consider how the Chinese government counts the population. According to State Council (2014b), a prefecture's urban population consists of all residents who have been in urban districts for more than six months, both natives and migrants, regardless of *hukou* registration status. For the purposes of this paper, we use the Urban Construction Statistical Yearbook (UCSY) published by the Ministry of Housing and Urban-Rural Development, which follows the same definition of urban population as in State Council (2014b).[12] We use the urban population in 2014, the year when the reform took place, to define reform status. The dummy variable, *Reform*$_i$, equals

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[10]This is based on the 2010 delineation, with no significant changes over time.

[11]Excluding prefectures without available urban population data removes 39 prefectures. Excluding Tibet and Xinjiang accounts for an additional 11 prefectures. In Appendix C, we show that our results are not driven by these exclusions.

[12]The UCSY reports the urban native and urban migrant populations separately. We aggregate the two groups to calculate the total urban population.

one if prefecture i's urban population is below 3 million. Under this definition, 37 prefectures in the sample are classified as non-reform prefectures, while the remaining 250 are classified as reform prefectures.

Two points are worth noting regarding the definition of reform status. First, we verify the accuracy of the population-based reform status in capturing policy changes. To do so, we conduct a thorough review of *hukou* policy documents issued by provincial and prefectural governments,[13] and based on our reading, we manually code up each prefecture's reform status, with the details of our coding process outlined in Appendix D. We then compare the manually coded reform status with the population-based reform status. The population-based definition proves to be highly accurate. Out of 287 prefectures in the sample, there are only 17 discrepancies ($17/287 = 6\%$) between manually coded and population-based reform statuses.[14] Therefore, in the subsequent analysis, we rely on the population-based reform status. However, to alleviate concerns about the discrepancies, we perform a robustness check by excluding 17 prefectures with discrepancies (see Section 4.3).

Second, our definition is based on urban population in 2014. One may wonder whether a prefectural government adjusts the reform status over time as its urban population varies. To the best of our knowledge, this is not the case. According to our review of local governments' documents regarding the *hukou* reform, we find that by 2015, most prefectures had guidelines for implementing the central government's directive, and we do not observe amendments made during subsequent years.

Labor Unrest. Our data on labor unrest are from the China Labor Bulletin (CLB), a non-profit organization based in Hong Kong that has monitored incidents of collective worker actions across China since 2011.[15] We use events during 2011–2019. The sample ends just before the outbreak of COVID-19. Due to the lack of administrative data on labor unrest in China, this dataset has been frequently cited by news media outside China to examine trends in Chinese workers' actions (e.g., Hernández, 2016); it has also been widely used in research on social unrest in China (e.g., among others, Campante et al., 2023; Qin et al., 2024).

Human coders of the CLB collect information on unrest events primarily from China's domestic social media platforms: Weibo, WeChat, Douyin, Kuaishou, and others. The coders verify the

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[13]We collect these documents from government websites, news reports, as well as a database on *hukou* reforms built by Zhang and Lu (2019). Appendix D offers an example of these documents.

[14]Table A7 in Appendix D tabulates the discrepancies. 8 small prefectures (urban population < 3 million) opted to maintain restrictions, whereas 9 big prefectures (urban population > 3 million) opted to relax migration barriers. We discuss possible reasons behind these discrepancies. Some small prefectures may have opted to maintain barriers because of their political importance or foreseeable population growth. For instance, Hebei provincial government explicitly required Langfang prefecture to maintain restrictions because the prefecture is adjacent to Beijing so it houses many migrants workers in Beijing. Guangdong provincial government required two prefectures in the Pearl River Delta, Zhuhai and Zhongshang, to maintain restrictions likely because they have had fast population growth. However, it is less clear why those big prefectures opted to relax restrictions.

[15]Data are accessed from the CLB website: https://clb.org.hk/en.

accuracy of collected information and incorporate into the dataset only the events with complete information on the location, date, causes, the industry, and the relevant company. The CLB data report 11,733 events between 2011 and 2019.[16]

Given how the CLB dataset is built, one should consider events in the dataset as those arguably more severe labor conflicts, where workers end up taking to the street and demand public attention. For 83 percent of the events in the dataset, workers conduct demonstrations in public spaces, in the forms of protests, marches, sit-ins, traffic obstructions, and even suicide threats.

A natural question is to what degree CLB data reflect underlying patterns of labor conflict in China. We show that the events in CLB data exhibit similar trends as in other data sources of labor conflict. We draw a comparison to the Global Database of Events, Language, and Tone (GDELT), one commonly used dataset on social unrest at the global level (e.g., see a review by Cantoni et al., 2023). The GDELT Project has conducted automated scraping of the world's news media since 1979. In the GDELT data, we define a labor unrest event as any event that falls in the "Protest" category and has labor involved. Because the CLB specifically focuses on labor unrest and has human coders carrying out data collection and cleaning, it includes many more labor unrest events than the GDELT: CLB includes 11,733 events, whereas GDELT includes only 4,681 events. Nonetheless, at the national level, both datasets display quite similar trends in labor unrest (see Figure A2).[17]

Auxiliary Data. We use multiple auxiliary datasets for validating the research design and exploring mechanisms. They include prefecture-level covariates collected from various sources, population censuses, migrant surveys, trade data, and biographical data on local officials, among others. Appendix E describes these data sources, and we will introduce them when they become pertinent to the analysis.

3.2 Estimating the Causal Effect of the *Hukou* Reform on Labor Unrest

To estimate the causal effect of *hukou* reform, an intuitive strategy is a difference-in-differences (DiD) design, which compares the trajectories of unrest between reform and non-reform prefectures. It is implemented by the following two-way fixed effects (TWFE) model:

$$\frac{Unrest_{it}}{L_{i,2010}} = \beta \left(Reform_i \times Post_t \right) + \lambda_i + \mu_t + \varepsilon_{it}. \tag{1}$$

The dependent variable, $\frac{Unrest_{it}}{L_{i,2010}}$, represents the unrest rate, measured as the number of unrest events per million prime-age population (aged 25–54 years old). $Reform_i$ is an indicator for prefecture i's reform status, taking value one if prefecture i's urban population in 2014 is below 3 million. $Post_t$ is an indicator that equals one for years from 2014 onward — we treat 2014 as the first year that the

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[16]The vast majority, 11,451 events, occurred in the 287 prefectures included in our main sample.

[17]GDELT is not suitable for a regression analysis at the subnational level. GEDELT only geocodes 1,112 labor unrest events at the prefecture level, and the rest events are given either the centroid of China or a province.

reform comes into effect. λ_i and μ_t are prefecture and year fixed effects, respectively. ε_{it} is the error term clustered at the prefecture level.

The ordinary least squares (OLS) estimand of β identifies an average causal effect of the *hukou* reform on labor unrest, provided that a parallel trends assumption is met: the reform and non-reform prefectures would have shared similar trends in unrest in the absence of reforms. This assumption is questionable in our setting. It relies on more populous regions to be on parallel trends in labor unrest with less populous regions. One particular concern is that urban population itself, which determines the reform status, may be associated with differential patterns in unrest. For instance, Acemoglu et al. (2020) document that there is a positive causal relationship between population and conflict due to competition for scarce resources. In addition, it can be easier to organize unrest in more populous regions (Wallace, 2014).

To address this concern, we modify Equation 1 by explicitly including flexible controls for the urban population. Specifically, the regression model is specified as follows:

$$\frac{Unrest_{it}}{L_{i,2010}} = \beta \left(Reform_i \times Post_t\right) + \lambda_i + \mu_t + f\left(\tilde{p}_i; \zeta_{Reform,t}\right) + \varepsilon_{it}. \tag{2}$$

The newly included variable, $\tilde{p}_i = \log(3) - \log\left(P_{i,2014}\right)$, is the centered log urban population; it captures the deviation of prefecture i's log urban population from $\log(3)$, the cutoff deciding reform status.[18] f is a polynomial function. $\zeta_{Reform,t}$ is a vector of coefficients on $\tilde{p}_i$ in the polynomial function. Importantly, as the subscripts indicate, coefficients in $\zeta_{Reform,t}$ are allowed to vary over time and by reform status.

This design is a marriage of difference-in-differences (DiD) and regression discontinuity (RD), that is, a difference in discontinuity (DiDC) design, with $\tilde{p}_i$ being the running variable. To estimate Equation 2, following Gelman and Imbens (2019), we let f be a first-order polynomial function. In most results, we conduct estimation using the full sample. Because the number of reform prefectures far exceeds the non-reform (250 versus 37), restricting to a narrow bandwidth around $\tilde{p}_i = 0$ may exclude a large portion of non-reform prefectures and lose much statistical power. We place equal weights on prefectures, i.e., we use the uniform kernel. In Section 4, we show that our results are robust to different empirical decisions, including alternative polynomial orders, bandwidth choices, and kernel choices.

In the spirit of RD, the estimated β identifies the average causal effect of the *hukou* reform at $\tilde{p}_i = 0$, under the assumption that in the absence of the *hukou* reform, the trends in labor unrest vary smoothly around $\tilde{p}_i = 0$. This is a local version of the parallel trends assumption, weaker than the global version that Equation 1 requires. In the next section, we discuss the validity of our research design.

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[18]We consider the deviation in log because the distribution of urban population $P_{i,2014}$ is very skewed (see Figure A3). In Table A1, we show that the using the deviation in level, $3 - P_{i,2014}$, to construct polynomials produce similar results.

4 Main Results

This section presents the main results of our paper. We start by discussing the validity of our research design in Section 4.1, followed Section 4.2 that report the estimated effects of *hukou* reform on labor unrest using the DiDC design. Section 4.3 reports results from an alternative identification strategy, which confirms the *hukou* reform's impact on labor unrest. Section 4.3 presents a battery of robustness checks.

4.1 Validity of Research Design

Recall that our research design leverages exploits variation in labor unrest trends across the reform cutoff of 3 million urban population, controlling for trends associated with urban population. Thus, the identification assumption is that there is no discontinuous change in labor unrest trends around the reform cutoff. In the following, we discuss a set of potential concerns about the identification assumption, and we address them using tools from the RD literature (e.g., McCrary's test, balance tests) as well tests specific to the Chinese context under study.

4.1.1 Concern 1: Confounding Policies Correlated with Urban Population

If urban population also determines policies other than the *hukou* reform, the estimates may conflate the effects of multiple policies, provided that other policies can also influence labor unrest. This makes it difficult to isolate the specific effect of the *hukou* reform.

For policies that have existed before the *hukou* reform, prefecture fixed effects should control for their influence, provided that their effects are time invariant. We can further evaluate the performance of prefecture fixed effects by examining pretrends. As we will show, there are no significant pretrends in labor unrest and other potentially unrest-conducive variables leading up to the *hukou* reform applying our research design, suggesting that our results are unlikely to be driven by preexisting policies correlated with urban population.

More concerning are policies with provisions varying by urban population and enacted simultaneously with the *hukou* reform. If such policies also influence labor unrest, it would be impossible to disentangle the effects of the *hukou* reform from those of other policies. To alleviate this concern, we conduct an extensive review of policies related to urban population. Appendix F provides more details of this exercise. We use the *PKULaw* database — a large database of Chinese laws that is maintained by Peking University and has been frequently used in research on policy-making in China (Wang and Yang, 2021; Tian, 2024) — to collect policies with mentions of "urban population" or other similar terms in the text.

Our reading suggests that these policies are unlikely to contaminate our estimates of the effects of the *hukou* reform on labor unrest. We find that most policies only reference urban population as part of a description, rather than specifying provisions tiered by urban population. For example, the central government approval of a prefecture's urban planning may include a projection of urban population. A small number of policies do include provisions based on population tiers, but these tend to focus on domains unrelated to labor unrest, such as prefabricated construction, public transit systems, and domestic services. Additionally, in Section 4.2, we conduct placebo tests estimating "effects" at cutoffs other than 3 million, and the null results indicate that our estimates are not confounded other policies correlated with urban population.

4.1.2 Concern 2: Local Officials' Manipulation of Urban Population to Select Reform Status

The validity of RD also requires that the agents, in our case the prefectural governments, cannot or do not precisely manipulate urban population to sort into certain reform status (Lee and Lemieux, 2010). In principle, manipulation is not infeasible, as prefectural governments can preemptively influence their statistical bureaus regarding the reporting of urban population. We perform several checks, and the results imply a lack of manipulation.

First, recall that in Section 3.1, we find that for 94 percent of prefectures within our sample, the reform status defined by urban population is in line with the actual reform status coded from government documents. If the primary goal of manipulating urban population is to legitimately attain or dodge the *hukou* reform, one would not expect to see such a high overlap between population-defined and actual reform statuses. In addition, it is impractical to substantially manipulate urban population for specific reform status, because the new level of urban population cannot change too much relative to the historical level. In fact, from 2013 to 2014, only one prefecture's urban population grew from below 3 million to above 3 million, and zero prefecture had urban population fall from above 3 million to below 3 million. All these observations indicate that manipulation is not prevalent in practice.

Second, if there is systematic manipulation of urban population to select certain reform status due to potential benefits, one would expect a significant bunching near the cutoff of 3 million, or equivalently, $\tilde{p}_i = 0$. However, we do not detect this phenomenon, as in Figure 1 that presents the density of the running variable $\tilde{p}_i = 0$. McCrary's test (McCrary, 2008) confirms the smoothness of the density function around $\tilde{p}_i = 0$, suggesting lack of manipulation. In Figure A4, we also examine the density of the running variable defined using urban population in 2015, and we again do not detect discontinuity in the density. Therefore, it is not the case where prefectural governments manipulated urban population in an *ex post* manner such that their decisions on taking up the reform would appear to be more legitimate with respect to the center's initiative.

Lastly, for manipulation to happen in practice, there ought to be some benefits for local officials to do so. In the Chinese context under study, such a benefit is predominantly promotion. A large body of literature has documented that promotion incentives play a central role in determining Chinese bureaucrats' choices and policy-making (e.g., among others, Wang et al., 2020; He et al., 2020; Jia, 2024). Relatedly, Zeng and Zhou (2024) find that promotion-motivated local officials may manipulate GDP statistics to deliver better observed performance. When considering our setting, however, unlike GDP, there is no strong argument that the upper level government would evaluate local officials directly based urban population. One potential, indirect argument is that local officials may want to dodge the *hukou* reform, so that they can avoid social instability due to increased population inflows may help secure an official's chance of promotion, given that stability maintenance has been widely seen as necessary for career advancements (Edin, 2003). One immediate implication of this argument is that officials with stronger promotion incentives may over-report growth in urban population. However, we find little support for this hypothesis. Following Wang et al. (2020), we estimate an index for *ex-ante* promotion prospects (see Appendix E for estimation details), and we find it has no discernible association with observed growth in urban population between 2013 and 2014 (see Table A2). If anything, the association has a negative sign, the opposite of what a promotion-motivated manipulation story would predict.

4.1.3 Concern 3: Heterogeneity between Prefectures

Our identification assumption supposes that without the *hukou* reform, the *trends* in the determinants of labor unrest are similar between prefectures near the 3 million cutoff. This assumption is untestable because the reform-absent counterfactual is not observed. Nonetheless, we can assess its plausibility by examining pretrends. Specifically, we run the following RD regression:

$$\Delta W_{it} = \alpha_0 + \alpha_1 Reform_i + f\left(\tilde{p}_i; \zeta_{Reform,t}\right) + \mu_t + \nu_{it}, \quad t \leq 2013. \tag{3}$$

ΔW_{it} is the change in a covariate. Our sample includes two pre-reform episodes: 2011–2012 and 2012–2013. Equation 3 stacks these two episodes. Panel A of Table 2 reports the results. We start with estimating Equation 3 without including the polynomial function $f\left(\tilde{p}_i; \zeta_{Reform,t}\right)$, thus, α_1 captures the average difference in pretrends between reform versus non-reform prefectures. Columns (1) and (2) report the estimated α_1 and the standard error. We find that reform prefectures on average have lower growth in unrest than their non-reform counterparts. We also find that reform prefectures have differential pretrends in other dimensions. They exhibit lower population growth and interestingly, higher GDP growth. But there are no discernible differential trends in local governments' "carrot and stick," as measured by expenditures on social security and public security (police). These patterns indicate that a simple DiD design cannot reliably estimate the causal effect of the *hukou* reform on labor unrest.

Then, we estimate the complete specification of Equation 3, controlling for polynomials $f\left(\tilde{p}_i; \zeta_{Reform,t}\right)$. As in RD designs, α_1 captures pretrends differences among prefectures near the reform cutoff. Columns (3) and (4) in Panel A of Table 2 report the estimated α_1 and its standard error. By comparing prefectures barely eligible for the reform with those barely ineligible, the inclusion of polynomial controls shrinks pretrends differences found in Columns (1) and (2) substantially, and eliminates all the three significant differences. Importantly, there are no longer differential trends in labor unrest. The lack of differential pretrends is also evident in the RD plots displayed by Figure A5A.

The checks for pretrends lend support to our research design. To better understand reform and non-reform prefectures, we also examine the differences in predetermined characteristics between reform and non-reform prefectures. Specifically, we estimate a cross-sectional variant of Equation 3 with the dependent variable being a characteristic in the base year (2010, for which a population census is available). Inspired by existing research on unrest, we examine a set of variables that may be associated with unrest occurrences, including the share of migrants, the share of urban residents, the share of secondary and tertiary sector workers, as well as share of internet users. Panel B of Table 2 presents the results. As above, Columns (1) and (2) report estimates from the specification without including polynomials, and Columns (3) and (4) report estimates after adding polynomial controls. One can see that the inclusion of polynomial controls largely shrinks the differences in predetermined characteristics, but there are still statistically significant imbalances in shares of migrants, urban residents, and tertiary sector workers. Figure A5B visualizes the RD regressions reported in Columns (3) and (4), suggesting that imbalances are likely due to outliers at the right tail of $\tilde{p}_i$. Recall that our research design only requires balance in underlying trends of unrest, so it allows for imbalance in covariates, as long as they are not associated with differential trends of unrest. In Section 4.3, we show that our results are robust to a variety of strategies to control for potential covariate-related differential trends.

4.2 The Effect of the *Hukou* Reform on Labor Unrest

4.2.1 Main Findings

We report our findings for the effect of the *hukou* reform on labor unrest. We begin by reviewing the dynamics of labor unrest in 287 sampled prefectures during the period under study, 2011–2019. Figure 2A depicts the time series of average unrest rates separately for reform and non-reform prefectures (solid blue and red lines). We also present the difference between the two groups (dashed green line). Clearly, during the period we study, labor unrest was increasing in China.

If we compare the dynamics between reform and non-reform prefectures, the dashed green line shows that reform prefectures exhibit a smaller growth rate in unrest relative to non-reform prefectures even before the reform initiative, as already hinted in Table 2. This negative gap enlarges

after the reform, yielding a relative decrease in reform prefectures' unrest rates against overall trends.[19],[20] In Figure 2B, this pattern is confirmed by the estimates of a TWFE event-study model (the dashed, light blue line).[21] As noted in Sections 3.2 and 4.1, this strategy cannot credibly estimate the *hukou* reform's casual effect due to violations of the parallel trends assumption. We thus turn to our preferred research design to obtain a more credible estimate. The sold, dark blue line in Figure 2B reports the estimates from an event-study model that adds polynomial controls $f\left(\tilde{p}_i; \zeta_{Reform,t}\right)$. From comparing prefectures around the reform cutoff, we see there are no differential trends in unrest leading up to the center's reform initiative. But after the reform comes into effect, reform prefectures experience a relative decline in unrest rates. To further evaluate the significance of this trend break, in Appendix G.1, we implement the sensitivity test developed by Rambachan and Roth (2023). The test extrapolates the differential trends indicated by estimated pretrends to the post-reform period, and then examines conditional on the extrapolated trends, whether the post-reform effects are still statistically significant. The results imply that we can conclude that the *hukou* reform reduced labor unrest significantly, unless there exist very nonlinear differential trends.

The results presented above are based on the full sample of 287 prefectures. One concern is that the polynomials of urban population may not adequately model all unobserved heterogeneity between prefectures far from the reform cutoff and those near the cutoff, resulting in biased estimates. To alleviate this concern, we restrict the sample to a narrow bandwidth around the reform cutoff, which is referred to as a "narrow sample." We use the optimal bandwidth proposed by Imbens and Kalyanaraman (2012). Using this narrow sample, we repeat the previous analysis. Figure 3A displays the raw patterns, which are qualitatively similar to Figure 2A. It is worth noting that now reform and non-reform prefectures share very similar trends and even levels of unrest rates before the reform initiative, but after the reform, reform prefectures have much lower unrest.[22] This is evident from event-study estimates in Figure 3B.

Table 3 summarizes these results from both the full and the narrow samples. Given the small number of non-reform prefectures, which may cause poor properties of asymptotic testing, we also report *p*-values calculated from permutation tests. The permutation *p*-values confirm the significance of our estimates. The estimates show that the *hukou* reform had a strong effect of *hukou* reform on decreasing labor unrest rates. For our preferred specification in Column (2),

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[19]We note that 2017 is an exception. One possible reason is that 2017 featured a rather special political environment — the 19th National Congress of the Chinese Communist Party was held at the end of the year. The uniform, nationwide enforcement of social control eliminated possible regional differences in unrest rates.

[20]In Figure A6A, we compare the distributions of unrest rates before and after the reform. In the post-reform era, unrest rates in reform prefectures are distributed further to the left than in non-reform prefectures, though the distributions of pre-reform unrest rates are not balanced between the two groups of prefectures.

[21]The estimating equation is $\frac{Unrest_{it}}{L_{i,2010}} = \sum_{s \neq 2013} \beta_s \left(Reform_i \times D_s\right) + \lambda_i + \mu_t + \varepsilon_{it}$. D_s is a dummy variable that equals one for year s and zero otherwise. Year 2013 if the omitted reference group.

[22]In Figure A6B, we compare the distributions of unrest rates before and after the reform. In the post-reform era, the distribution of unrest rates in reform prefectures are significantly further to the left of the distribution in non-reform prefectures, whereas the distributions of pre-reform unrest rates are balanced.

the point estimate implies that reform prefectures experienced a decrease in unrest rate by 1.419 incidents per million prime-age population relative to non-reform prefectures. This is a sizeable effect: the magnitude of the point estimate amounts to about 42 percent of the mean of non-reform prefectures.[23]

In Appendix G.2, we show that our results are robust to alternative empirical decisions, including choices of bandwidths, kernels, and polynomial orders. We show that point estimates remain stable when varying bandwidths, though, as expected, standard errors are high when restricting to small bandwidths. We also continue to see a strong effect of the *hukou* reform on decreasing labor unrest rates when using different kernels and orders of polynomials.

4.2.2 Alternative Interpretations

We interpret our findings as evidence of the causal effects of the *hukou* reform in reducing labor unrest rates. In the following, we present additional results to tease out alternative factors or statistical artifacts that may explain our findings.

Unobserved Determinants of Unrest. One competing interpretation for our findings is that, they merely reflect unobserved determinants of labor unrest that correlate with urban population *and* have time-varying impacts. This may not be very likely, given the lack of urban population-correlated policies and the balanced pretrends. Nonetheless, we examine this possibility through a placebo test. Specifically, we implement our research design to estimate the "causal effect" of having urban population below a cutoff other than 3 million.[24] Figure 4 reports the results of this exercise. We see that 3 million is the only point where there is a significant negative effect, whereas there is a null effect elsewhere. These results indicate that our findings are not likely driven by the impacts of other urban population-correlated factors. In addition, they imply that the outcome only changes discontinuously at the cutoff of 3 million, which strengthens the validity of our research design.

Differential Reporting of Local Events. Since our measure of the unrest rate from the CLB relies on online posts about labor unrest events, one may be concerned that our finding is an artifact of differential reporting of local events between reform and non-reform prefectures, with reform prefectures having fewer events reported than non-reform prefectures after the reform became in effect. In Appendix G.3, we present several results to address this concern.

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[23]We note that the 95% confidence intervals include a wide range of values. However, one can be 95% confident that the *hukou* reform decreased the labor unrest rate by 0.685 to 2.153 incident per million prime-age population, and this amounts to 20 percent to 63 percent of the average labor unrest rate in non-reform prefectures.

[24]For a given new cutoff c, the exercise is operationalized by redefining $Reform_i$ as $\mathbb{1}\{P_{i,2014} \le c\}$ and $\tilde{p}_i$ as $\log(c) - \log(P_{i,2014})$ in Equation 2. To avoid contamination due to treatment effect at the true cutoff of 3 million, following Cattaneo and Titiunik (2022), when estimating the effect at a cutoff below (above) 3 million, we only use the sample of prefectures with urban population below (above) 3 million.

First, we show that the *hukou* reform had no significant impacts on the number of general events and the number of protests reported in GDELT, a database that focus on a wide range of topics, not just labor unrest (see Columns (1) and (2) of Table A11). This suggests a lack of differential changes in coverage of local events.

Second, we control for variation in labor unrest rates likely resulting from China's internet censorship. Specifically, we use a measure of provincial level censorship intensity from Qin et al. (2017), the share of deleted posts on *Weibo* ("Chinese Twitter," China's largest social media platform), which can be a proxy of the censorship apparatus' attention or efforts with respect to a region. We control for interactions of this measure and year dummies in Equation 2. We find that the estimated effect on labor unrest of the *hukou* reform does not change markedly (see Column (3) of Table A11), suggesting that our findings can not be explained by differential trends associated with internet censorship.

Lastly, we address another source of differential reporting, that is, self-censorship. If workers in reform prefectures became less likely to report their actions online than their counterparts in non-reform prefectures after the reform, then we would observe a mechanical decrease in labor unrest rates in reform prefectures. To examine the role of self-censorship, we consider the most influential scenario for self-censorship to drive our results. When post-reform self-censorship rose primarily in reform prefectures that reported high unrest rates before the reform, we are more likely to obtain an artificial, negative effect of the *hukou* reform on labor unrest rates. If the dynamics of self-censorship play a major role in our findings, then excluding reform prefectures with reported high pre-reform unrest rates would attenuate the estimated effect of the *hukou* reform on labor unrest. However, as shown by Figure A13, the estimated effect remains stable when excluding reform prefectures with reported high pre-reform unrest rates, indicating a limited role of self-censorship in explaining our results.

Population Growth. We investigate if our results are due to mechanical changes in population growth. Note that our results are against the commonly concerned instability from lax migration laws that may increase population. In Appendix G.4, we find that the reform had no discernible effects on population growth. If anything, the size gap between reform and non-reform prefectures enlarges over time. This echos recent findings that Chinese migrants prefer to go to large cities, and the reform that liberates migration to small cities does not alter this pattern (Chen and Fu, 2023). Our base measure calculates the labor unrest rate using a fixed population size, namely, prime-age population in 2010 (see Equation 2). In Appendix G.5, we show that the results hold even if we account for the time varying population size. Taken together, variation in population size cannot fully explain our findings.

A Natural Decline in Protests Against the *Hukou* Policy? One may argue that the decrease in labor unrest is mechanical, if most migrants had protested against the *hukou* policy, and the *hukou* reform simply shut down this cause since it entitled migrants local citizenship. We think

this interpretation is not plausible here. First, in Appendix G.4, we find that the *hukou* reform had no significant impacts on native urban population (i.e., urban residents with local *hukou*), suggesting that the reform did not substantially eliminate resentments about the *hukou* policy. Second, regarding the causes of the unrest events, protesting against the *hukou* policy is not the majority. In the CLB data, 72 percent report wage arrears as a cause, and 11 percent are related to job losses.[25] These are not closely associated with protests against the *hukou* policy. We notice the existence of unrest events that may be potentially against the *hukou* policy, which demand for social security and housing subsidies. As a robustness check, in Table A3, we show that the *hukou* reform reduced labor unrest unrelated to *hukou*, but had a null effect on *hukou*-related unrest. Taken together, our results should not be interpreted as driven by a natural decline in protests against the *hukou* policy in the wake of the *hukou* reform.

4.3 Additional Robustness Checks

4.3.1 Alternative Specifications and Estimators

In Table A13 and Figure A16 in Appendix H.1, we adopt alternative specifications or estimators to estimate the effect of the *hukou* reform on labor unrest: (i) different forms of the dependent variable (logarithmic and inverse hyperbolic since transformations); (ii) the Poisson regression due to the non-negative nature of the dependent variable (Silva and Tenreyro, 2006); (iii) the spatial autoregressive (SAR) model to take into account spatial spillovers; and (iv) the synthetic difference-in-differences estimator proposed by Arkhangelsky et al. (2021). Regardless of specifications or estimators applied, we consistently find that the *hukou* reform lowered labor unrest rates.

4.3.2 Excluding Potential Outliers

In Appendix H.2, we show that results are not due to outlier observations. We examine if our results are driven by any particular province. Specifically, we leave out a province at a time and re-estimate Equation 2. As Figure A17 shows, the estimate is stable to the exclusion of any province.

In Table A14, we consider the influences of several sets of outliers on the estimated effect of the *hukou* reform on labor unrest. First, we can exclude prefectures that never had a labor unrest event recorded by the CLB. This is to ensure that our results are not due to low variation in labor unrest (recorded by the CLB) in some prefectures that overlap with reform prefectures. Second, inspired by the "donut RD" exercise, we can exclude prefectures near the cutoff of 3 million to address potential self-selection into reform status and/or measurement error in reform status resulting from measurement error in urban population. Third, recall from Section 2.1 that 17 prefectures have discrepancies between population-defined and actual reform statuses, we can exclude these

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[25]Note that the an event can have multiple causes. In total, 77 percent of events are due to wage arrears and/or job losses.

prefectures to avoid the influence of likely endogenous non-compliance. Lastly, following Hansen (2022, pp. 84–86), based on goodness of fitting, we can identify observations that are most influential in our estimation and exclude them to assess robustness of our results. Regardless of which set of outliers are excluded, we estimate a strong, often more pronounced than baseline effect of the *hukou* reform on decreasing labor unrest.

4.3.3 Covariates Balancing

In Table A15 in Appendix H.3, we address concerns about the heterogeneity between reform and non-reform prefectures, as reflected by unbalanced baseline covariates despite the inclusion of polynomials in Table 2. We show that our results survive different strategies to balance baseline covariates: (i) regression adjustments by controlling for interactions between the covariates and year dummies; (ii) balancing the propensity score (probability of being under the reform) predicted by the covariates; and (iii) balancing the *distributions* of covariates using the coarsened exact matching (CEM) proposed by Iacus et al. (2012). Our results survive these strategies to balance covariates.

4.3.4 Alternative Identification Strategy

The results above leverage variation in reform status. One may worry if the reform status systematically overlaps with unobserved triggers of labor unrest, then our findings would simply reflect the broader trends of those triggers but not in any way capture a causal effect of the *hukou* reform on labor unrest. To address this issue, we present results from an alternative identification strategy that allows us to make a cleaner comparison between reform and non-reform prefectures under similar unrest-conducive conditions.

Our exercise follows (Campante et al., 2023) to consider unemployment pressure in China's export sector due to the global trade slowdown. This relates a broad literature on the linkage between negative income shocks and unrest (e.g., Ponticelli and Voth, 2020; Fetzer, 2020; Braggion et al., 2020). Specifically, we estimate the following regression model:

$$
\begin{aligned}
\frac{Unrest_{it}}{L_{i,2010}} = {} & \beta_1 TradeShock_{it} + \beta_2 \left(Reform_i \times Post_t \right) \\
& + \beta_3 \left(TradeShock_{it} \times Reform_i \right) + \beta_4 \left(TradeShock_{it} \times Post_t \right) \\
& + \beta_5 \left(TradeShock_{it} \times Reform_i \times Post_t \right) + \lambda_i + \mu_t + \eta_{it}.
\end{aligned}
\tag{4}
$$

$TradeShock_{it}$ is a plausibly exogenous measure of fluctuation in global demand for manufactured goods, which we discuss in detail later. Equation 4 is essentially a triple-differences model (Olden and Møen, 2022). The coefficient of interest is β_5, which measures the differential response of a reform prefecture to trade shock *relative* to that a non-reform prefecture subject to the same level of trade shock, when the *hukou* reform is in effect ($Post_t = 1$).

*TradeShock*_{it} is constructed in a shift-share (Bartik) fashion:

$$TradeShock_{it} = \sum_k \underbrace{\frac{X^{CN}_{ik,2010}}{L_{i,2010}}}_{\text{share}} \times \underbrace{\frac{\Delta X^{ROW}_{kt}}{X^{CN}_{k,2010}}}_{\text{shift}} . \tag{5}$$

In this expression, $X^{CN}_{ik,2010}$ is prefecture i's exports of product k (6-digit Harmonized System level); $X^{CN}_{k,2010}$ is the national aggregate exports of product k; ΔX^{ROW}_{kt} is the increase/decrease in exports within the rest of the world (less China) in year t.[26] The "shift" component, $\frac{\Delta X^{ROW}_{kt}}{X^{CN}_{k,2010}}$, is the growth in global demand for product k in year t, *relative* to industry size of product k in China in 2010. It is apportioned to prefectures by the "share" term, $\frac{X^{CN}_{ik,2010}}{L_{i,2010}}$, which reflects the initial specialization of prefecture i. *TradeShock*_{it} can thus be interpreted as a proxy of an average worker's gains or losses due to fluctuation in global trade. To be a valid exogenous shock, *TradeShock*_{it} needs to be uncorrelated with error term η_{it}. According to Borusyak et al. (2022), this rests on the exogeneity of product-level shock $\frac{\Delta X^{ROW}_{kt}}{X^{CN}_{k,2010}}$ with respect to η_{it}. In Appendix H.4, we discuss this and related checks proposed by Borusyak et al. (2022), showing that $\frac{\Delta X^{ROW}_{kt}}{X^{CN}_{k,2010}}$ passes balance tests, so that they can be seen to be as good as randomly assigned to Chinese prefectures.

Table 4 reports the results of our analysis. Column (1) is a minimum specification, where we estimate the average relationship between trade shock and labor unrest. The negative estimate of β_1 implies that on average, a sluggish growth in global demand leads to an increase in labor unrest. To further verify causation, in Column (2) we perform a falsification test. It shows that the future trade shock has no impact on contemporary labor unrest. In Columns (3) and (5), we estimate the triple-differences model, Equation 4. The positive estimate of β_5 indicates that when the *hukou* reform is in force, the same level of negative trade shock causes less labor unrest in reform prefectures than in non-reform prefectures. These results corroborate that the *hukou* reform has causally intervened in the occurrence of unrest, rather than spuriously represents broader trends in triggers of unrest.

Summary. To summarize, in this section, we provide robust evidence that the *hukou* reform led to a significant decrease in labor unrest rates. Such a decrease is not a result of population-correlated confounders, not an artifact of differential coverage of labor unrest events, and also not a consequence of population growth. The next section is devoted to investigating mechanisms through which the *hukou* reform influences the occurrence of unrest.

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[26]$X^{CN}_{ik,2010}$ and $X^{CN}_{k,2010}$ are measured using Chinese customs data of 2010. ΔX^{ROW}_{kt} is measured using the BACI database, which improves upon the UN Comtrade database. All these variables are in 1,000 dollars.

5   Unpacking Mechanisms

Having established that the *hukou* reform reduces labor unrest rates, we now turn to the question of what mechanisms explain our results. We concentrate on impacts on migrants, especially existing migrants, given that migrants are the major participants in labor unrest during the period under study (see Section 2.2) and the *hukou* reform had no impact on population size. In Section 5.1, we propose that the *hukou* reform may add the cost of participating in labor unrest. In a nutshell, the *hukou* reform facilitates migrants to pursue permanent settlement in their destinations; this pursuit creates dependence on the state and in turn deters participation in labor unrest that the state dislikes. We corroborate this hypothesis by showing that the *hukou* reform raises migrants' settlement intentions, reflected by longer migration duration. And in Section 5.2, we develop a novel econometric approach to quantify the importance of heightened settlement intentions in explaining the *hukou* reform's effect on decreasing labor unrest.

We want to be upfront that we do not claim that heightened settlement intentions are the only mechanism that explains the *hukou* reform's impact on unrest rates. Toward the end of this section, in Section 5.3, we will discuss other potential mechanisms.

5.1   Migrants' Settlement Intentions

5.1.1   Hypothesis Development

A burgeoning literature has documented the role of (intended) migration duration in shaping migrants' *economic* choices, especially those concerning settlement and integration in the destination, because of high returns in the future. For example, relying on a rich panel dataset on immigrants and a dynamic model, Adda et al. (2022) highlight migrants' return/stay plans as an important source of heterogeneity in their skill acquisition and career paths. Zaiour (2023) argues that violence at home raised Mexican immigrants' intentions to stay in the US and thereby increased their propensity of naturalization and marriage with US citizens. Gathmann and Keller (2018) exploit Germany's reform to citizenship policy in 1990, and they find that faster access to citizenship increases migrants' investments in host country-specific skills, such as language and vocational training.

We envision a similar logic can apply to *political* behavior as well. In the following, we outline our hypothesis on the interplay between migrants' settlement intentions and their engagement in labor unrest. In Appendix I, we also present a simple model to formalize our argument.

For Chinese migrant workers in our case, the *hukou* reform creates an opportunity for permanent settlement (i.e., obtaining local *hukou*) that is valued by migrants. Migrants are thus more likely to plan on settlement, and this pursuit for permanent settlement can lead to a special "investment"

in politically obedient behavior, thereby reducing engagement in labor unrest. Several reasons jointly contribute to migrants' fears of participating in labor unrest in relation to permanent settlement. First, it is important to recall that the *hukou* reform does not immediately entitles migrants local *hukou*. Instead, it makes settlement more attainable than before, thus, migrants have to taken into account if their actions would undermine their chance of settlement. Second, migrants depend on the government for acquiring permanent settlement. It is well known to workers that the government dislikes labor unrest, and may relentlessly retaliate participants in unrest when necessary. Within this knowledge in mind and the reliance on the government's favor, migrants may be deterred away from unrest activities. Lastly, participation in unrest can undermine a migrant worker's prospect of permanent settlement. Recall that settlement requires migrants to have stable jobs and housing (see Section 2.1). These conditions may not be easily satisfied if a migrant worker is not welcomed by employers and landlords as they have participated in unrest (and perhaps more worse, been punished by the government).

Taken together, in the wake of the *hukou* reform, migrants' increased settlement intentions effectively functions as a disincentive for engagement in unrest. Such a disincentive is weak before the reform, since institutional barriers of *hukou* transfers lead to temporary migration. With a short stay in a destination, migrants may instead have incentives to engage in politically risky unrest, through which they can gain short-term benefits (e.g., recovering unpaid wages) but bear little cost in relation to future settlement, which is not warranted in most cases.

It is worth noting the group and time window for which the settlement intentions mechanism can play out. This mechanism mainly applies to migrants who desire to permanently settle in the destinations but have not yet made it. As such, it is a short-term mechanism — it no longer applies once a migrant has successfully obtained local *hukou* and settled in the destination — as the *hukou* cannot be deprived once given.

5.1.2   Testing the Hypothesis

For our hypothesis be at play, the *hukou* reform should enhance migrants' settlement intentions and lengthen their stay in destinations. This is supported by the evidence we report below.

Data. For the purpose of our investigation, we use the 1 percent population census of 2015. The census includes information on where an individual resided by the end of 2010, 2014, and 2015. This allows us to construct an individual's residential history with three snapshots. For instance, we may observe:

$$\text{Beijing, 2010} \rightarrow \text{Shanghai, 2014} \rightarrow \text{Shanghai, 2015.}$$

With this information, we can infer one's outmigration during a period. For instance, in the example given above, we can infer that the person must have moved during 2010–2014 but not during 2014–2015.

Using such information, we trace how the *hukou* reform influences a migrant's decision on outmigration from their 2010 destination. For this purpose, we impose two restrictions to the sample. The first restriction is for identifying migrants in 2010. We define an individual as a migrant if their residential prefecture in 2010 differs from the *hukou* registration prefecture in 2015. Note that although we observe an individual's residential history, we do not observe the *hukou* registration history. We have to rely on the registration prefecture reported in 2015 to define migrant status back in 2010. This may introduce measurement error if an individual had transferred *hukou* by 2015.[27] Therefore, we impose a second restriction. We restrict our sample to individuals with rural origins (reported in 2015), who were less likely to have changed *hukou* registration.

Specification. Our analysis resembles a survival analysis where the event of interest ("failure") is outmigration from the 2010 location. Specifically, we estimate the following linear probability model (LPM):

$$\Pr\left(Outmigration_{jkt} \mid \mathbf{W}_{jkt}\right) = \rho\left(Reform_k \times Post15_t\right) + \lambda_k + \mu_t + f\left(\breve{p}_i; \zeta_{Reform,t}\right),$$
$$t \in \{2014, 2015\}. \tag{6}$$

In Equation 6, the dependent $Outmigration_{jkt}$ is a dummy variable that equals one if individual j has left their 2010 residential prefecture k by year t. Given the nature of our data, we observe this outcome for 2014 and 2015. Equation 6 stacks these two periods.

$\mathbf{W}_{jkt}$ denotes a set of explanatory variables on the right hand side. $Reform_k$ is the reform status of prefecture k. $Post15_t$ is a dummy variable that equals one if $t = 2015$ but zero if $t = 2014$. Thus, for this regression analysis, we treat 2015 as the post-reform period while 2014 as the pre-reform period.[28] We also include prefecture and year fixed effects as well as polynomial controls. In the spirit of survival analysis, for an individual had decided to leave initial location k at year t, we drop subsequent observations to focus on initial outmigration.

ρ is the coefficient of interest. It is estimated off comparing trends in outmigration rates among not-yet-migrate individuals between reform and non-reform prefectures. It thus captures the impact of the *hukou* reform on the outmigration rate. We expect ρ to be negative, as the *hukou* reform facilitated migrants to permanently settle in their destinations.

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[27]For instance, consider an individual whose originally registered in the 2010 prefecture but had successfully transferred their *hukou* to the 2015 residential prefecture. This individual would be misdefined as a migrant, if relying on the rule of whether the 2010 residential location is the same as the 2015 registration. As a result, the outmigration rate within migrants can be overstated.

[28]We argue that this is a reasonable decision. From the census data, we can only the residential prefecture by the end of 2014 and so we can only define outmigration during 2010–2014. But for individuals had ever moved during this period, it is expected that the majority of them should have moved much earlier, possibly before July 2014, the time when the *hukou* reform was initiated.

Results. As the first pass at the data, Figure 5 depicts outmigration rates from the 2010 residential location. We do this separately reform status and by period. The outmigration rate is estimated by the Kaplan-Meir estimator, namely, the share of leavers among existing migrants. During the pre-reform period 2010–2014, reform prefectures had higher outmigration rates than non-reform prefectures. For the post-reform period 2014–2015, outmigration rates grew in all prefectures. However, the growth was much smaller in reform prefectures, suggesting that the *hukou* reform raised migrants' intentions to settle in their initial destinations.

Table 5 reports estimates of Equation 6. Column (1) shows that the estimate of ρ is negative, consistent with our hypothesis. The estimate indicates that the *hukou* reform reduces the likelihood of outmigrating from the initial destination by 7.2 percentage points. This effect amounts to 51 percent of the average outmigration rate in non-reform prefectures.

We interpret this decreased outmigration as reflecting heightened settlement intentions caused by the *hukou* reform. However, one competing interpretation is: migrants in reform prefectures may have inherently distinct dynamics of integration into the destination than those in non-reform prefectures, so that we observe them staying longer anyway. To alleviate this concern, in Column (2), we control for origin (*hukou* prefecture) fixed effects to leverage variation within migrants of the same origin. In other words, we conceptually compare two migrants of the same origin but subject to distinct paths of the *hukou* regime (reform vs. non-reform). This within-origin comparison is arguably cleaner, given the importance of origin conditions to migration (Zaiour, 2023). As Column (2) shows, however, including origin fixed effects virtually does not change our estimate. Further, to the degree the distinct dynamics of integration are due to differences in individual characteristics, in Column (3), we control for differential dynamics by including interactions between individual covariates and year indicators. The covariates include birth cohort, gender, educational attainment, and employment status. We see that our estimate does not change markedly with the inclusion of these controls. As a robustness check, in Appendix A3, we also estimate a Cox proportional hazard model that is common in survival analysis, which yields similar results.

If decreased outmigration results from high settlement intentions raised by the *hukou* reform, one may expect the decrease to be more pronounced among more forward-looking migrants, who take into account the future benefits from settlement to a larger extent.[29] Column (4) shows this heterogeneity: the *hukou* reform reduces outmigration more among more patient migrants, with the patience level measured by a migrant's home province average in the Global Preference Survey (GPS, Falk et al., 2018).[30] In Column (5), we show that this heterogeneous effect by patience is not driven not by other possibly correlated factors. We focus on two factors: (i) risk-taking preference obtained from the GPS data, and (ii) high school completion reported in census data. We see conditional these two factors, there is still strong heterogeneous effect by patience.

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[29]Result 2 of our simple conceptual model in Appendix I shows this heterogeneity.

[30]The patience measure is at the home province level. To account for potential correlated idiosyncratic disturbances within home province, we perform a permutation test for the heterogeneous effect (see Figure A7). The results confirm the significance of the heterogeneous effect by patience.

In summary, we view the decreased outmigration as evidence of heightened settlement intentions in the wake of the *hukou* reform.

5.2 Importance of Heightened Settlement Intentions

Thus far, we have demonstrated that the *hukou* reform enhances settlement intentions. One natural question is: how much of the *hukou* reform's effect on labor unrest can be explained by increased attachment to the destination?

To answer this question, we need to conduct a causal mediation analysis, that is, identifying the causal effect of the *hukou* reform that goes through heightened settlement intentions (*indirect effect*) versus the causal effect that goes through other mechanisms (*direct effect*). This is a rather challenging task. The conventional, popular approach relies on a regression of the outcome variable on the treatment variable, and interprets the change in the treatment coefficient after including the mechanism variable (e.g., Baron and Kenny, 1986; Cutler and Lleras-Muney, 2010). However, this approach is subject to an important identification hurdle: the mechanism variable is typically not quasi-exogenous, so that the causal effect of the mechanism variable is not identified, and the coefficient change is biased for the indirect effect, i.e., the treatment effect that can be attributed to the mechanism of interest. We propose improvements upon the conventional approach, which preserve the simplicity of regression techniques and use an instrumental variable (IV) to tackle endogeneity of the mechanism.

Next, we first discuss our methodology. Then, we apply the method to our case, investigating the importance of heightened settlement intentions in explaining the *hukou* reform's impact on reducing labor unrest.

5.2.1 Methodology

Basic Setup and Conventional Approach. To illustrate our method for causal mediation, we consider a more general setup. Let i denote the unit of observation. Let Y_i denote the outcome of interest, T_i denote the treatment, and M_i denote the mechanism of interest. Figure 6 presents a directed cyclic graph (DAG) for the relations between the three variables.[31] The treatment T_i can influence outcome Y_i through two pathways: (i) direct influence, with a marginal effect of τ_i, and (ii) indirect influence via a certain mechanism M_i, with a marginal effect of $\gamma_i \pi_i$ — a combination of a marginal effect of T_i on M_i of π_i and a marginal effect of M_i on Y_i of γ_i. Note that parameters $(\tau_i, \pi_i, \gamma_i)$, as the subscripts indicate, may vary across i. The following statement summarizes some parameters pertinent to our analysis.

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[31]We use the DAG here for the ease of presentation. In Appendix J, we provide technical details for our methodology using the potential outcome framework, which is equivalent to the DAG framework (Imbens, 2020).

DEFINITION 1 (Parameters). Let $E(\cdot)$ be the expectation operator across i.

1. The total effect is $\beta_i \equiv \tau_i + \gamma_i \pi_i$. The average total effect (ATE) is then $E(\beta_i) \equiv E(\tau_i) + E(\gamma_i \pi_i)$.

2. The indirect effect that is due to mechanism variable M_i is $\gamma_i \pi_i$, thus, the average indirect effect (AIE) is $E(\gamma_i \pi_i)$.

3. The direct effect that is not due to mechanism variable M_i is τ_i, thus, the average direct effect (ADE) is $E(\tau_i)$.

Note that $ATE = ADE + AIE$. An econometrician may be interested in not only ATE but also AIE (and its magnitude relative to ATE), which informs them of the importance of a mechanism. We make the following assumptions. Thus, they would want to find an estimator for AIE. Conventionally, this is done by estimating the following two regression models:

$$Y_i = \alpha_1 + \beta T_i + e_{i1}, \tag{7}$$
$$Y_i = \alpha_2 + \tau T_i + \gamma M_i + e_{i2} \tag{8}$$

For illustration, also consider a third regression model:

$$M_i = \alpha_3 + \pi T_i + e_{i3}. \tag{9}$$

Let $\hat{\kappa}$ denote the OLS estimator of κ. The conventional approach uses the difference in coefficients on T_i between Equations 7 and 8 as an estimator for AIE (Baron and Kenny, 1986), i.e., the estimator is defined as $\widehat{AIE} \equiv \hat{\beta} - \hat{\tau}$. By properties of least squares, the following result holds:

$$\widehat{AIE} \equiv \hat{\beta} - \hat{\tau} = \hat{\gamma}\hat{\pi}. \tag{10}$$

Note that $\hat{\gamma}\hat{\pi}$ is the product of the estimated effect of M_i on Y_i ($\hat{\gamma}$) and the estimated effect of T_i on M_i ($\hat{\pi}$). This expression helps clarify the challenge of the conventional approach in identifying AIE. A research design typically only involves valid exogeneity of T_i, warranting $\hat{\pi}$ to be consistent for the average effect of T_i on M_i. The key challenge here is that $\hat{\gamma}$ from Equation 8 needs to consistently estimate the average effect of M_i on Y_i. This would require that M_i is exogenous conditional on T_i (e.g., Imai et al., 2011; Acharya et al., 2016). That is to say, there are no unobserved confounders related to M_i and Y_i once T_i is conditioned on. It is a strong assumption in may settings. In our context, this can be violated if, for instance, the *hukou* reform altered migrant networks, which simultaneously affected settlement intentions and the organization of labor unrest.

Proposed Approach. To tackle the challenge in identifying the average causal effect of M_i on Y_i, we propose to use an instrumental variable (IV) for M_i, denoted by Z_i. We refer to this as an "IV-augmented approach." We leave technical details in Appendix J. Figure 7 visualizes the

relationship between variables in our proposed approach. It incorporates Z_i on the basis of Figure 6. Note that IV Z_i only influences outcome Y_i indirectly through mechanism M_i. The marginal effect of Z_i on M_i is γ_i.

More formally, consider the following potential outcome framework:

$$Y_i(t,m) = \tau_i t + \gamma_i m + u_i, \tag{11}$$

$$M_i(t,z) = \pi_i t + \theta_i z + v_i. \tag{12}$$

Equation 11 articulates that the value of Y_i depends on: (i) the values of (T_i, M_i), (t,m); (ii) unit-specific effects (τ_i, γ_i); and (iii) idiosyncratic disturbance u_i. Likewise, Equation 12 specifies the functional form of M_i. It is worth noting that this framework implicitly assumes the excludability of Z_i. The value of Z_i, z, does not directly enter Equation 11, and the only way it can affect Y_i is through M_i.

We now discuss the assumptions that our approach imposes.

ASSUMPTION 1 (Exogeneity of Treatment). $\{Y_i(t',m), M_i(t,z), Z_i\} \perp\!\!\!\perp T_i$, for all t, t', and z.

ASSUMPTION 2 (IV Validity).

1. (Independence) $\{Y_i(t,m), M_i(t',z)\} \perp\!\!\!\perp Z_i$ for all t, t', and z.
2. (Exclusion) $Y_i(t,m) \mid_{z=z'} = Y_i(t,m) \mid_{z=z'}$ for all z' and z''.
3. (Relevance) $E(\theta_i) \neq 0$.

Assumption 1 supposes the exogeneity of treatment T_i. Assumption 2 is an standard assumption on IV validity (Angrist and Pischke, 2009, pp. 151–158).

ASSUMPTION 3 (Sign Restrictions). Each of τ_i, γ_i, and π_i maintains the same sign across i.

This assumption is not required for identifying AIE. However, it facilitates interpretations.

ASSUMPTION 4 (Homogeneous Mechanism Effect). γ_i is constant across i.

Assumption 4 is a key assumption in our approach. It posits that the causal effect of M_i on Y_i is constant. It is a strong assumption, albeit not uncommon in the literature (Dippel et al., 2022; Dix-Carneiro et al., 2018). IV, as any other identification strategies, can only identifies the effect of M_i on Y_i identified in a certain subpopulation, which is potentially different from the population in

which the effect of T_i on Y_i is identified. Only with a homogeneity assumption like Assumption 4 can an econometrician extrapolate the identified effect of M_i on Y_i between populations to assess the contribution of a mechanism. In Section 5.2.3, we will the robustness of our conclusion when relaxing this assumption.

With these assumptions, we derive the following result for the identification of AIE.

PROPOSITION 1. *With M_i instrumented by Z_i, least squares estimators of Equations 7 and 8 satisfy:* $\widehat{AIE} \equiv \hat{\beta} - \hat{\tau} = \hat{\gamma}\hat{\pi}$. *Under Assumptions 1, 2, and 4,*

$$\text{plim } \widehat{AIE} = E(\gamma_i \pi_i) \equiv AIE. \tag{13}$$

Proof. See Appendix J. ∎

Proposition 1 preserves the conventional approach's simplicity based on linear regression techniques: in a regression of Y_i on T_i, the change in the coefficient on T_i after controlling for M_i can be interpreted as AIE. A similar result holds for RD designs that identify treatment effects at the cutoff deciding treatment status, which we use in this paper. The following proposition summarizes the result for RD designs.

PROPOSITION 2. *Let r_i denote the running variable. $T_i = \mathbb{1}\{r_i \geq 0\}$. Average effects of interest in Definition 1 are re-defined as those at the cutoff 0: $ATE = E(\beta_i \mid r_i = 0)$, $AIE = E(\gamma_i \pi_i \mid r_i = 0)$, and $ADE = E(\tau_i \mid r_i = 0)$.*

Least squares estimators of Equations 7 and 8, with M_i instrumented by Z_i and flexible polynomial functions of r_i included, satisfy: $\widehat{AIE} \equiv \hat{\beta} - \hat{\tau} = \hat{\gamma}\hat{\pi}$. *Under Assumptions 1, 2, and 4,*

$$\text{plim } \widehat{AIE} = E(\gamma_i \pi_i \mid r_i = 0) \equiv AIE. \tag{14}$$

Proof. See Appendix J. ∎

Next, we apply our methodology to quantify the importance of heightened settlement intentions in explaining the effect on labor unrest of the *hukou* reform.

5.2.2 Specification and Results

Estimating Equation. To implement our IV-augmented approach for causal mediation, we convert a panel regression model used in previous analysis to its cross-sectional equivalent. Specifically, we estimate the following models in parallel to Equations 7 and 8:

$$\frac{\bar{\Delta} Unrest_i}{L_{i,2010}} = \alpha_1 + \beta Reform_i + f\left(\tilde{p}_i; \zeta_{Reform}\right) + e_{i1}, \tag{15}$$

$$\frac{\bar{\Delta} Unrest_i}{L_{i,2010}} = \alpha_2 + \tau Reform_i + \gamma \bar{\Delta} Outmigration_i + f\left(\tilde{p}_i; \zeta_{Reform}\right) + e_{i2}, \tag{16}$$

where $\bar{\Delta} Outmigration_i$ is instrumented by Z_i, which we discuss below. In the models, for any fixed prefecture i, $\bar{\Delta} R_i$ denotes the mean difference in R_{it} between post- and pre-reform periods. For example, the dependent variable, $\frac{\bar{\Delta} Unrest_i}{L_{i,2010}} = \left(\sum_{t=2014}^{2019} \frac{Unrest_{it}}{L_{i,2010}}\right)/6 - \left(\sum_{t=2011}^{2013} \frac{Unrest_{it}}{L_{i,2010}}\right)/3$, is the change in average unrest rate from the pre-reform period to post-reform period. With this conversion, the estimate of β in Equation 15 is a numerically identical to that in Equation 2. In Equation 16, the mechanism variable is $\bar{\Delta} Outmigration_i = Outmigration_{i,2015} - Outmigration_{i,2014}$. $Outmigration_{it}$ is the outmigration rate in prefecture i during period t. Thus, $\bar{\Delta} Outmigration_i$ is the change in the outmigration rate from 2010–2014 period to 2014–2015 period in prefecture i.

We are interested in how the estimated coefficient on $Reform_i$ changes after the inclusion of $\bar{\Delta} Outmigration_i$, namely, $\hat{\beta} - \hat{\tau}$. Under the conditions of Proposition 2, this coefficient change can be interpreted as the effect of the *hukou* reform on labor unrest through the channel of settlement intentions measured by outmigration.

Instrument. To instrument for outmigration, we consider variation in global demand faced by migrants at their home prefectures. Specifically, the IV is constructed as follows:

$$Z_i = \sum_h s_{h \to i} \times \bar{\Delta} TradeShock_h. \tag{17}$$

$s_{h \to i}$ is the share of migrants moving from home (*hukou*) prefecture h to destination prefecture i in all migrants in prefecture i. $\bar{\Delta} TradeShock_h$ is the change in the average global demand shock (i.e., $TradeShock_{it}$ as in Section 4.3) from 2011–2014 period to 2014–2015 period, in line with the time frames of outmigration rates that can be calculated in the 2015 census data. Z_i thus measures the fluctuation in global demand at home experienced by an average migrant in prefecture i.

Z_i represents a pull factor at the origin of outmigration from the destination. As documented by existing studies, economic conditions at origins, captured by Z_i, are relevant to migration decisions (Zaiour, 2023; Imbert et al., 2022; Karadja and Prawitz, 2019).[32] In our sample used for outmigration

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[32]Zaiour (2023) shows that homicide in Mexico reduces returns of Mexican migrants from the US to Mexico. Imbert et al. (2022) use agricultural income shocks at origins to instrument for rural-urban migration in China. Similarly, Karadja and Prawitz (2019) exploit local frost shocks to predict emigration from Sweden to the US in the late 19th and the early 20th centuries.

analysis, we observe that among migrants who eventually left their 2010 destinations, 88 percent returned to home prefectures. This suggests that the origin is an important outside option when a migrant decides on whether to continue staying in the destination. Figure 8 shows that Z_i is predictive for $\Delta Outmigration_i$: a sluggish growth in global demand faced by the origin is associated with a decrease in outmigration from the migration destination.

Shocks at migrants' origins are less likely to relate to the factors that may drive outmigration and labor unrest simultaneously. Formally, given the shift-share construction of Z_i and $\bar{\Delta}TradeShock_h$, such an exclusion restriction relies on the quasi-exogeneity of product level shocks (Borusyak et al., 2022). One violation would be: migrants in prefectures with low odds of labor unrest are systematically from origins specializing in products with high fluctuations in global demand. In Appendix H.4, we show that Z_i passes the balance tests recommended by Borusyak et al. (2022). We provide other checks for the exclusion restriction along the results presented below.

Results. We focus on using our approach to evaluate the importance of increased settlement intentions to the *hukou* reform-induced evolution of labor unrest from 2011–2013 to 2014–2015. It consistent with the time window of our measure of changes in settlement intentions, the changes in outmigration rates from 2011–2013 to 2014–2015. We do not have a measure of settlement intentions for the entire period that we study (2011–2019). And as noted in Section 5.1.1, the settlement intentions mechanism is time sensitive — relating short-term changes in settlement intentions to longer-term changes in unrest may understate the importance of settlement intentions. Nonetheless, for completeness, we also report the results that look at *hukou* reform-induced evolution of labor unrest between 2011–2019.

Table 6 reports the results of causal mediation analysis. In Columns (1)–(3), we look at changes in unrest rates during 2011–2015. Column (1) presents the estimates of Equation 15. In Column (2), we implement the conventional approach for causal mediation analysis by directly adding $\bar{\Delta}Outmigration_i$ to the specification of Column (1). We see a significant positive association between outmigration and unrest, suggesting that heightened settlement intentions may reduce unrest. We also see that the coefficient on $Reform_i$, compared to that in Column (1), attenuates by 18.8 percent. If one takes the estimated coefficient on $\bar{\Delta}Outmigration_i$ as causal, the coefficient change implies that heightened settlement intentions can explain 18.8 percent of the *hukou* reform's total effect on labor unrest.

In Column (3), we apply the approach that instruments outmigration using trade shocks at migrants' origins. As Figure 8 shows, negative trade shocks strongly predict a reduction in outmigration rates. The first-stage strength is confirmed by an effective F statistic of 58.750 (Olea and Pflueger, 2013). In Column (3), we see that outmigration has a strong effect on unrest. The significance of this effect survives the robust tF inference for IV proposed by Lee et al. (2022). By Proposition 2, we can conclude that 60.7 percent of the total effect of the *hukou* reform on unrest

can be attributed to heightened settlement intentions. This magnitude is higher than what the conventional approach would conclude.

Columns (4)–(6) conduct the same analysis for the longer time window (2011–2019). Note that Column (4), as expected, produces an estimate of β identical to that in Column (2) of Table 3. Mediation analysis in Columns (5) and (6) shows that our IV-augmented approach implies a greater importance of heightened settlement intentions than the conventional approach would do. For this longer time window, our measure of increases in settlement intentions can explain 27.3 percent of decreased labor unrest due to the *hukou* reform. This magnitude is much smaller than that when consider the time window of 2011–2015. The attenuation can be due to two reasons: (i) $\bar{\Delta}Outmigration_i$ has pronounced measurement error for the longer time window; and (ii) $\bar{\Delta}Outmigration_i$ has zero effect in the long term after migrants have successfully attained local *hukou*.[33]

The increase in the share of the total effect explained due to inflation in the coefficient on $\bar{\Delta}Outmigration_i$ after instrumentation. Provided that the IV is valid, the *empirical* IV-OLS gap can come from two sources: (i) omitted variables bias (OVB) purged by IV, and (ii) different weighting schemes that IV and OLS adopt to aggregate potentially heterogeneous effects.[34] We implement the decomposition method developed by Ishimaru (2024) to calculate the contribution of each source. The results show that nearly all the IV-OLS gap can be attributed to OVB. This implies that the importance of the mechanism after instrumentation is not mechanically driven by a different weighting scheme.

Our results rest on the validity of IV, i.e., trade shocks at origins influence labor unrest solely through intervening in outmigration. The balance tests offer some confidence. We conduct two additional checks in Appendix K. First, one may be concerned that trade shocks at nearby origins can have spatial spillover impacts on labor unrest at the destination. In light of this concern, we alternatively construct the IV, Z_i, using only origins that are sufficiently far away from prefecture i to limit the extents of spillovers. Table A18 shows that one can still conclude that settlement intentions are a quantitatively important mechanism. Second, we use the methodology developed by Conley et al. (2012) to assess whether we can conclude outmigration has a significant effect on unrest when allowing some degrees of violations of the exclusion restriction. We find that the positive link between outmigration and unrest can withstand large degrees of violations of the exclusion restriction. This confirms that outmigration can be a plausible mechanism for the *hukou* reform to exert influence on unrest.

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[33](i) may play a relatively bigger role than (ii), as we find that the *hukou* reform did not have a significant effect on the size of native urban population, suggesting that the impacts of settlement intentions should not die out very quickly during the period under study.

[34]In theory, Assumption 4 assumes away (ii). But the least squares estimation does not impose this assumption. Thus, when the assumption does not hold, the empirical IV-OLS gap can be driven by different weights placed by IV and OLS on heterogeneous effects. For instance, imagine a scenario where $\bar{\Delta}Outmigration_i$ is exogenous so that OVB is zero, and its impact on $\frac{\bar{\Delta}Unrest_i}{L_{i,2010}}$ is nonlinear. Then, OLS and IV would aggregate nonlinear effects differently, making the IV-OLS gap entirely due to the difference in weighting schemes.

5.2.3 Sensitivity Test

Methodology. The results above rely on Assumption 4 that the effect of the mechanism variable on the outcome (γ_i) is homogeneous. With this assumption, we conclude that the heightened settlement intentions can explain 60.7 percent of the effect of the *hukou* reform on labor unrest in the intermediate term and 27.3 in the longer term. In this section, we explore the robustness of our conclusion to relaxing Assumption 4.

If we relax Assumption 4 to introduce heterogeneity in γ_i, then we can obtain (see Appendix J for derivations):[35]

$$\text{plim } \widehat{AIE} = \underbrace{E(\gamma_i \pi_i)}_{\text{AIE}} + \underbrace{[\text{plim } \hat{\gamma} - E(\gamma_i)] \, E(\pi_i)}_{\substack{\text{bias 1} \\ \text{(IV bias)}}} - \underbrace{Cov(\gamma_i, \pi_i)}_{\substack{\text{bias 2} \\ \text{(ecological bias)}}} \tag{18}$$

$$= \underbrace{E(\gamma_i \pi_i)}_{\text{AIE}} + \underbrace{\rho_{\phi\pi}\sigma_\phi\sigma_\pi E(\pi_i)}_{\substack{\text{bias 1} \\ \text{(IV bias)}}} - \underbrace{\rho_{\gamma\pi}\sigma_\gamma\sigma_\pi}_{\substack{\text{bias 2} \\ \text{(ecological bias)}}} . \tag{19}$$

We first inspect Equation 18. In the expression, $\text{plim } \hat{\gamma} = E(\phi_i \gamma_i)$ is the IV estimand, and $\phi_i = \frac{\widetilde{Z}_i M_i}{E(\widetilde{Z}_i M_i)}$, where $\widetilde{Z}_i$ is the residual of the linear projection of Z_i onto T_i (and possibly other controls). Equation 18 implies that in general, $\widehat{AIE}$, the change in the coefficient on T_i after controlling for instrumented M_i, can still be biased for AIE. Specifically, the bias comes from two sources.

1. **Bias 1 ("IV bias").** The first bias comes from the discrepancy between the IV-identified average effect and the population average effect, i.e., $\text{plim } \hat{\gamma}$ versus $E(\gamma_i)$. Intuitively, this bias emerges because IV identifies the effect of M_i on Y_i within a subpopulation that may differ from the population in which the effect of T_i on Y_i is identified; therefore, without Assumption 4, the IV-identified effect of M_i on Y_i cannot be perfectly extrapolated across populations.

2. **Bias 2 ("ecological bias").** The second bias is due to the correlation between γ_i and π_i. It is the classical ecological bias caused by attributing the characteristics of a population to an individual (Glynn, 2012). The intuition is as follows. Ideally, one would like to use γ_i, *individual-specific* effect of M_i on Y_i, as the loading on π_i to evaluate the contribution of the treatment-induced change in M_i to the total treatment effect. However, the linear regression can only uses $\hat{\gamma}$, an *average* impact of M_i on Y_i, as the loading on π_i. As a result, the linear regression overlooks co-movement of γ_i and π_i, thus, it may systematically mis-measure the importance of the

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[35]A similar result holds for RD designs by assuming that γ_i and π_i are independent of the running variable r_i or replacing unconditional moments in the expression with conditional moments at $r_i = 0$ (see Appendix J).

treatment-induced change in M_i to the outcome at the individual level and also at the aggregate level.[36] Clearly, with Assumption 4, such bias is zero.

In summary, when Assumption 4 is not imposed, $\widehat{AIE}$ does not consistently estimate AIE. Our goal in this section is to find a way to underpin AIE using available information in conjunction with some additional assumptions.

We now turn to Equation 19. It writes the bias terms in terms of standard deviations and correlation coefficients. σ_U denotes the standard deviation of U, and ρ_{UV} denotes the covariance between U and V. We see that bias of $\widehat{AIE}$ is determined by the distributions of (ϕ_i, γ_i) and (γ_i, π_i). Correlation coefficients $\rho_{\phi\gamma} = Corr(\phi_i, \gamma_i)$ and $\rho_{\gamma\pi} = Corr(\gamma_i, \pi_i)$ govern the severity of bias 1 and bias 2 in Equation 19, respectively.[37] It is possible to estimate the moments of $\phi_i = \frac{\tilde{Z}_i M_i}{E(\tilde{Z}_i M_i)}$ from the sample using the method of moments. Therefore, we may impose some distributional assumptions to de-bias $\widehat{AIE}$ and underpin AIE. The following proposition summarizes our result.

PROPOSITION 3. *Suppose that γ_i and π_i are uniformly distributed. For RD designs, additionally suppose $\{\gamma_i, \pi_i\} \perp\!\!\!\perp r_i$. Then, under Assumptions 1, 2, and 3, for every given combination of $\rho_{\phi\gamma} = Corr(\phi_i, \gamma_i)$ and $\rho_{\gamma\pi} = Corr(\gamma_i, \pi_i)$,*

$$\text{plim } \widehat{\widehat{AIE}} = \text{plim} \left[\frac{\sqrt{3}\hat{\gamma}}{\sqrt{3}\,\text{sgn}(\gamma_i) + \rho_{\phi\gamma}\hat{\sigma}_\phi} + \frac{\rho_{\gamma\pi}\hat{\sigma}_\phi\,\text{sgn}(\pi_i)}{\sqrt{3}} \right] \hat{\pi} = AIE. \tag{20}$$

In Equation 20, $\hat{\sigma}_\phi = \sqrt{\frac{1}{n}\sum_{i=1}^{n}\left(\frac{\tilde{Z}_i M_i}{\frac{1}{n}\sum_{i=1}^{n}\tilde{Z}_i M_i} - 1\right)^2}$. $\text{sgn}(\cdot)$ is the sign function that satisfies: $\text{sgn}(x) = 1$ if $x > 0$, $\text{sgn}(x) = 0$ if $x = 0$, and $\text{sgn}(x) = -1$ if $x < 0$.

Proof. See Appendix J. ∎

By Proposition 3, for each $(\rho_{\phi\gamma}, \rho_{\gamma\pi})$ pair, $\widehat{\widehat{AIE}}$ identifies AIE. Note that when Assumption 4 holds, $\rho_{\phi\gamma}$ and $\rho_{\gamma\pi}$ are zero so that $\widehat{\widehat{AIE}} = \widehat{AIE}$. Based on Proposition 3. We can examine how much $\widehat{\widehat{AIE}}$ changes when $\rho_{\phi\gamma}$ and $\rho_{\gamma\pi}$ deviate from zero. In particular, we are interested in the scenarios where the magnitude of $\widehat{\widehat{AIE}}$ shrinks when $\rho_{\phi\gamma}$ and $\rho_{\gamma\pi}$ are set to be nonzero, i.e., the scenarios where imposing Assumption 4 and using $\widehat{AIE}$ would overstate the importance of a mechanism.

Results. For our case, we expect that $\gamma_i \geq 0$ and $\pi_i \leq 0$, i.e., a high outmigration rate (low settlement intention) universally increases unrest, and the *hukou* reform reduces outmigration from

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[36] For instance, suppose $Cov(\gamma_i, \pi_i) > 0$ and $\gamma_i, \pi_i > 0$. Then, within the low-γ_i group, the contribution of the treatment-induced change in M_i (i.e., π_i) is systematically overstated, while within the high-γ_i group, the contribution is systematically understated.

[37] Note that when $\rho_{\gamma\pi} = \rho_{\gamma\pi} = 0$, bias 1 and bias 2 are zero, so that plim $\widehat{AIE} = AIE$. Assumption 4 is a sufficient condition for this.

the destination (or raises settlement intentions). Then, we consider $\rho_{\phi\gamma} > 0$ and $\rho_{\gamma\pi} > 0$, so that $\widehat{\widehat{AIE}}$ corrects the *overestimation* of AIE by $\widehat{AIE}$.

Specifically, we look at the share of total effect explained by the proposed mechanism: *ShareExplained* $= \widehat{\widehat{AIE}}/\hat{\beta}$, for different pairs of $\rho_{\phi\gamma} > 0$ and $\rho_{\gamma\pi} > 0$. If *ShareExplained* drops substantially when very small values $\rho_{\phi\gamma}$ and $\rho_{\phi\gamma}$ are imposed, then it is not reliable to maintain our previous conclusion under Assumption 4, stating heightened settlement intentions as an important mechanism that bridges between the *hukou* reform and decreased labor unrest.

Figure 9A reports the results of our sensitivity test based on Proposition 3 for the immediate term. We see that, when $\rho_{\phi\gamma} = 0$ and $\rho_{\phi\gamma} = 0$, which is true when Assumption 4 holds, we obtain the highest *ShareExplained*, 60.7 percent, as we have seen previously. Overall, *ShareExplained* is more sensitive to bias 1 (IV bias): *ceteris paribus*, *ShareExplained* drops more quickly when $\rho_{\phi\gamma}$ increases than when $\rho_{\gamma\pi}$ increases. For the most extreme case $\rho_{\gamma\pi} = 0$ but $\rho_{\phi\gamma} = 1$, we see that heightened settlement intentions can still explain 20 percent of the *hukou* reform's total effect on decreasing labor unrest. A magnitude of 25 percent can be maintained if we allow for moderate severity of both bias 1 and bias 2, e.g., $\rho_{\gamma\pi} = \rho_{\phi\gamma} = 0.5$.

Figure 9B reports the results of our sensitivity test based on Proposition 3 for the longer term. Similarly, *ShareExplained* is more sensitive to bias 1 (IV bias). When $\rho_{\gamma\pi} = 0$ but $\rho_{\phi\gamma} = 1$, we see that heightened settlement intentions can still explain 10 percent of the *hukou* reform's total effect on decreasing labor unrest in the long term. This magnitude survives even if we allow for moderate severity of both bias 1 and bias 2, e.g., $\rho_{\gamma\pi} = \rho_{\phi\gamma} = 0.5$.

In conclusion, the results above demonstrate the resilience of the settlement intentions mechanism, especially in the immediate term. we conclude that heightened settlement intentions play a nontrivial role in mediating the *hukou* reform's effect on labor unrest, and it can explain up to 60.7 percent of decreased labor unrest in the immediate term and 27.3 percent in the long term.

5.3 Other Mechanisms

Thus far, we have presented evidence supporting the role of heightened settlement intentions. Of course, we do not claim that it is the sole mechanism. In this section, we investigate other potentially important mechanisms through which the *hukou* reform leads to decreased labor unrest, including (i) compositional changes among migrants, (ii) benefits available to migrants, and (iii) governments' social control.

5.3.1 Compositional Changes

One possible mechanism is that the *hukou* reform may alter the characteristics of migrants as it induces population movements. If certain characteristics are strongly associated with participation

in unrest — for instance, migrants with certain characteristics may be more likely to participate in unrest because they have high costs of participation or tend to work in industries with poor treatments, then we would observe decreased unrest rates after the *hukou* reform. However, the compositional shift may not be a first order explanation of our findings, as we do not find that the *hukou* reform had significant impacts on population sizes (see Section 4.2.2).

Nonetheless, we investigate the role of compositional changes by estimating the impacts of the *hukou* reform on several characteristics of migrants. We use a large nationally representative survey on migrants between 2011–2018, the China Migrants Dynamic Survey (CMDS). In CMDS, we observe gender, ethnicity, age, marital status, educational attainment, whether the migration is cross-province, and employment industry (manufacturing or construction). We estimate Equation 2 with the dependent variable being the characteristic at the individual level. Because CMDS is conducted in May every year, for these regressions, we treat years from 2015 onward as post-reform periods.

Table 7 reports the results. We examine the composition within all migrants as well as new arrivals (defined as those who arrived in the past year). We do not detect any significant effect of the *hukou* reform on migrants' characteristics, suggesting that compositional changes cannot play a major role in explaining our findings.

5.3.2   Benefits Available to Migrants

One may hypothesize that the *hukou* reform may have improved treatments of migrants, thereby reducing the likelihood for labor unrest to occur. We examine this possibility using information from CMDS.

Columns (1)–(3) of Table 8 first investigate if there are improvements in migrants' labor market outcomes. We consider three measures: workforce participation, wages, and employees' access to social security (ASS; only available from CMDS in 2011, 2013, and 2016). We do not find significant effects on these labor market outcomes, suggesting that reduction in labor unrest is not due to improved labor market outcomes.[38] The lack of labor market effects also indicates that the decreased labor unrest we detect should not result from an increase in employers market power. One may propose that the *hukou* reform, by increasing labor supply, can given employers greater market power, which they can use to deter workers' unrest. If so, one would expect wages should decrease, however, it is not supported by the data. This is not surprising, given that we do not find that the *hukou* reform increased population growth (see Section 4.2.2).

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[38]A recent paper by An et al. (2024) find significant negative effects of the *hukou* reform on wages and ASS. However, their definition of reform status and empirical strategy (DiD) are different from ours. In Appendix L, we replicate their results and compare them to the results under different empirical decisions. We find that their results are due to the use of a DiD strategy that simply compares more populous prefectures vs. less populous prefectures. In Section 4.1, we have shown such a comparison can be questionable.

In Columns (4) and (5) of Table 8, we look at two measures of intangible benefits that may increase migrants' satisfaction and thus reduce incentive to participate in unrest: migrants' co-residence with spouses or children, given that the relaxation of migration barriers may facilitate family union. The results show that, if anything, the *hukou* reform slightly reduced the likelihood of spousal co-residence, which in fact would accentuate our findings that the *hukou* reform reduced labor unrest.

Given the lack of discernible changes in benefits available to migrants, at least during the short period that we study, it is not likely that our findings are a mechanical consequence of the *hukou* reform-induced improvements in migrants' wellbeing.

5.3.3 Governments' Social Control

Another explanation for why the *hukou* reform reduces labor unrest is that the *hukou* reform may induce local officials to tighten social control, as they *perceived* a higher risk that population inflows may cause threats to social stability.

In Table 9, we examine this possibility using several measures of local governments' efforts to maintain social stability. In Columns (1) and (2), we see null effects of the *hukou* reform on local governments' expenditures on public security (police) and social security, indicating the reform did not lead to material investments in social control.

However, it is likely that instead of financial means, the local governments prioritized the maintenance of social stability, thus diverting toward this issue more manpower of the bureaucratic apparatus and in turn creating a higher level of stability. To test if local governments paid more attention to stability maintenance, we use the share of stability-related keywords in next year's government work report (GWR). GWRs are comprehensive policy documents that local government heads address at the beginning of each year to local People's Congresses to summarize accomplishments in the previous year and to lay out work plans for the next.[39] However, using the textual measure, Column (3) shows that there is no discernible shift of policy focus toward stability maintenance.

Finally, if our finding is driven by tighter social control, one would expect a higher fraction of unrest events repressed by the government, despite the overall drop in unrest occurrences. CLB documents the government response for about 30 percent of events. Using this information, we calculate the share of unrest events know to have been repressed by the government (in the form of detention of workers and police interventions).[40] Column (4) shows that the *hukou* reform had no significant impact on the share of unrest events repressed.

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[39]The GWR text has been adopted in the literature to measure Chinese officials' policy focus (Jiang, 2018; Jiang et al., 2019; Jiang and Zhang, 2020).

[40]This measure is missing if there are no unrest events in a given prefecture and year.

Taken together, it appears that the tightening of social control is not the main mechanism of our findings.

5.4 Discussions

Throughout this section, we discuss the mechanisms through which the *hukou* reform reduces labor unrest. One mechanism we highlight is the heightened settlement intentions in the wake of the *hukou* reform, which deter participation in unrest due to fears of failing to seize the opportunity of settling in the destination. Our novel causal mediation analysis shows that this mechanism is nontrivial and can account for 60.7 percent of the *hukou* reform's total effect on labor unrest during the period under study.

It is worth reiterating the group and time window for which the settlement intentions mechanism can play out. It mainly applies to migrants who desire to permanently settle in the destinations but have not yet made it and thus tends to play a short-term role. Indeed, in Sections 5.1 and 5.2, we see that outmigration has a better explanatory power for the *hukou* reform's on unrest in the immediate term than in the long term. In principle, the salience of the settlement intentions mechanism ought to die out, given that existing migrants gradually attain local *hukou* and there are no significant increases in new migrants. However, recall that we observe that the *hukou* reform had a persistent effect on reducing labor unrest (see Figure 2B). The *hukou* reform likely has stimulated lasting changes that decrease labor unrest persistently.

Our results suggest that such lasting changes do not pertain to characteristics of migrants, benefits available to migrants, as well as governments' social control strategies. The *hukou* reform may change other deeper factors relevant for unrest occurrences. For instance, it is possible that by permitting some migrants' to integrate into the destinations or simply raising those some migrants' settlement intentions, the *hukou* reform can weaken the social networks conducive to unrest activities, which leads to a persistent decrease in labor unrest. Due to data limitations that we do not have mico-level information on the organizing of labor unrest, we leave it for future research to explore how the *hukou* reform shapes the dynamics of labor unrest in the longer term.

6 Conclusions

This paper studies the causal relationship between migration barriers and social stability. By examining the impact of China's *hukou* reform on labor unrest, contrary to the common concern, we find that relaxing migration barriers may enhance social stability. We document the role of migrants' settlement intentions raised by the *hukou* reform: to seize the state-controlled opportunity of permanent settlement offered by the reform, migrants exhibit compliance with the state's demands for stability.

Looking beyond, we view our results as highlighting a source of state capacity and a force behind social changes. The dependence on the state constitutes coercive power that the state can use to induce citizens' compliance with its objectives — in our case, migrants rely on the state for permanent settlement in their destinations.[41] The states have been witnessed using such coercive power more or less explicitly. For instance, against the backdrop of falling fertility rates, a Chinese local government proposed to urge party members and civil servants to bear three children.[42] A similar push for civil servants was also proposed by the South Korean government.[43] Consequently, on the other side of the same coin, weakening the dependence on the state is a factor conducive to civil disobedience and the momentum of social changes.

We close this paper by noting two limitations, which may be interesting avenues for future research. First, our paper primarily focuses on migrants' behaviors, given that the literature has widely documented that migrants are the major participants in labor unrest. However, it is still likely that natives also react to the reform initiative and integration of migrants, as suggested by evidence on immigration's electoral effects among existing citizens (Mayda et al., 2022). Second, it is important to consider the extent to which our results generalize. As we have noted in the introduction of this paper, our results for the role of extended horizons should primarily apply to scenarios where the state has strong control over resources. It is an open question of how an open-door migration policy, or more broadly, a socially inclusive reform, affects sociopolitical stability when the state has different levels of control over resources. We believe that this question is worthy of further investigation.

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[41]The coercive power view in stark contrast to the social contract view, pioneered by Jean-Jacques Rousseau, that compliance follows from reciprocity between the state and the citizenry.

[42]http://chinascope.org/archives/35543

[43]https://www.koreatimes.co.kr/www/nation/2024/08/113_361718.html

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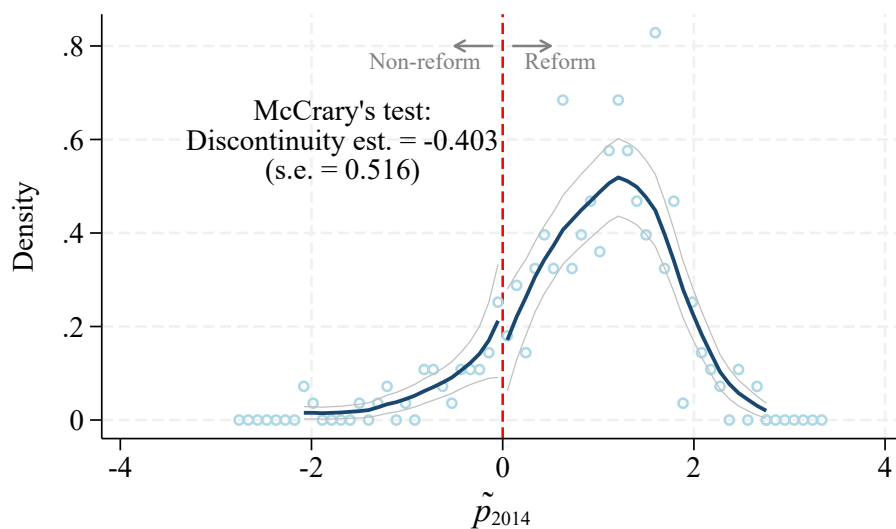
Figures

Figure 1. Density of Centered Log Urban Population of 2014

Note: This figure depicts the density of centered log urban population of 2014, $\Delta \log(P_{i,2014})$. We report the McCrary's test of density discontinuity at $\Delta \log(P_{i,2014}) = 0$.
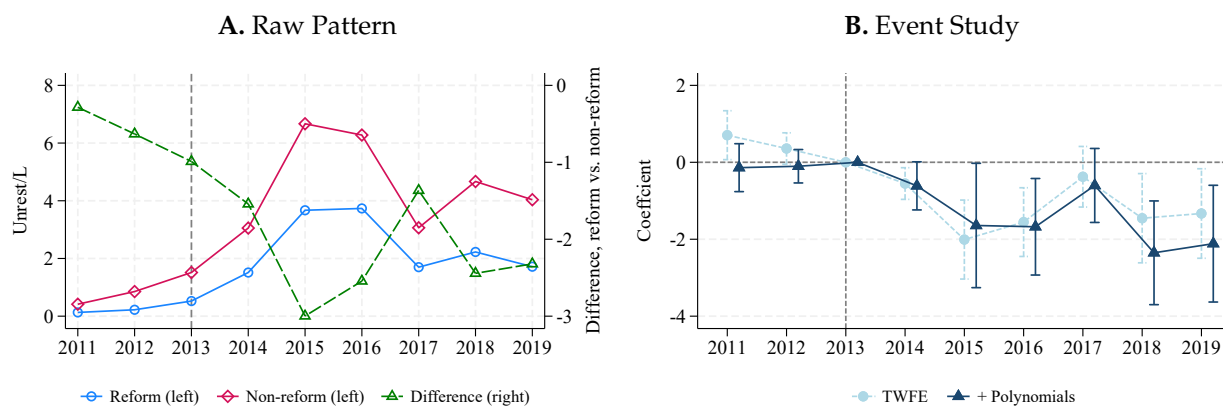
A. Raw Pattern   **B.** Event Study

Figure 2. Dynamics of Labor Unrest: Full Sample

Note: This figure presents the dynamics of labor unrest using the full sample. Panel A depicts the raw time series for reform and non-reform prefectures as well as the difference between two groups. Panel B presents estimates from the event study of two specifications: one with two-way fixed effects (TWFE) and the other further including polynomial controls. The solid dots are points estimates, and the caps the 95 percent confidence intervals. Standard errors clustered at the prefecture level are used for constructing the confidence intervals.
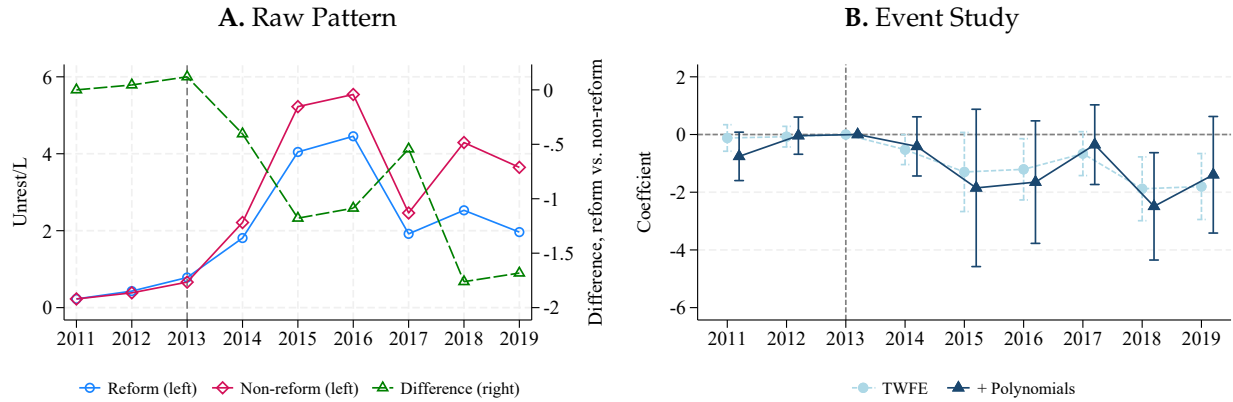
Figure 3. Dynamics of Labor Unrest: Narrow Sample

Note: This figure presents the dynamics of labor unrest using the narrow sample, that is, the optimal bandwidth proposed by Imbens and Kalyanaraman (2012) is imposed. Panel A depicts the raw time series for reform and non-reform prefectures as well as the difference between two groups. Panel B presents estimates from the event study of two specifications: one with two-way fixed effects (TWFE) and the other further including polynomial controls. The solid dots are points estimates, and the caps the 95 percent confidence intervals. Standard errors clustered at the prefecture level are used for constructing the confidence intervals.
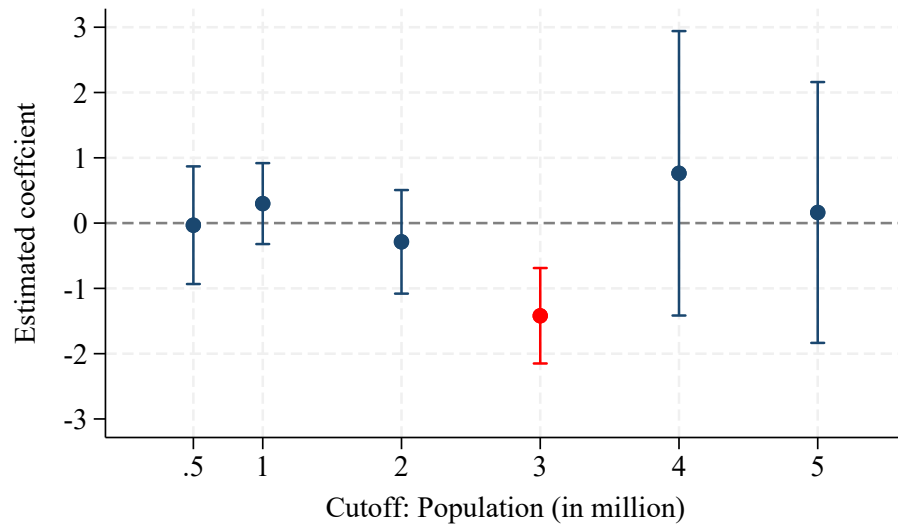

Figure 4. Estimates Using Falsified Cutoffs

Note: This figure presents the RD-DiD estimates from Equation 2 using falsified cutoffs: 0.5 million, 1 million, 2 million, 4 million, and 5 million. To avoid contamination due to real treatment effects at the 3 million cutoff, following Cattaneo and Titiunik (2022), we use only below-3 million prefectures for 0.5 million, 1 million, and 3 million cutoffs, and only above 3-million prefectures for 4 million and 5 million cutoffs. The solid dots are point estimates, and the caps are 95 percent confidence intervals. Standard errors clustered at the prefecture level are used to construct confidence intervals.
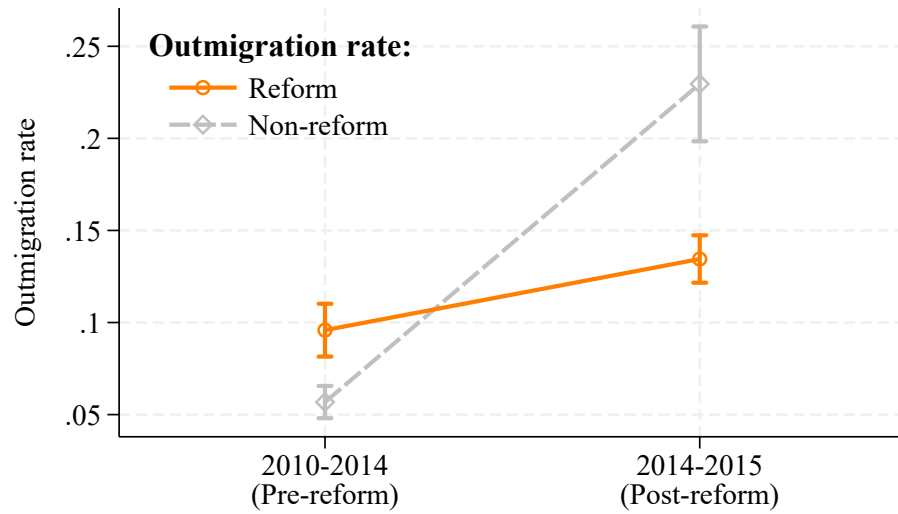
Figure 5. Rates of Outmigration from the 2010 Residential Location

Note: This figure depicts the Kaplan–Meier estimates of the rates of out-migration from the 2010 residence by the end of 2010, 2014, and 2015, separately for reform and non-reform prefectures. The solid dots are point estimates, and the caps are 95 percent confidence intervals. The bars present the number of staying individuals in the sample, separately for reform and non-reform prefectures.
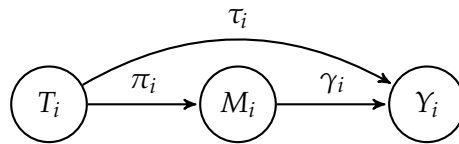
Figure 6. Directed Acyclic Graph for Conventional Causal Mediation
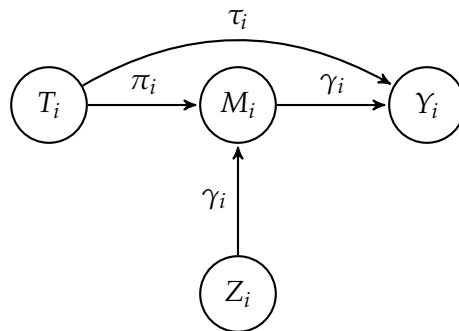
Figure 7. Directed Acyclic Graph for Conventional Causal Mediation with IV
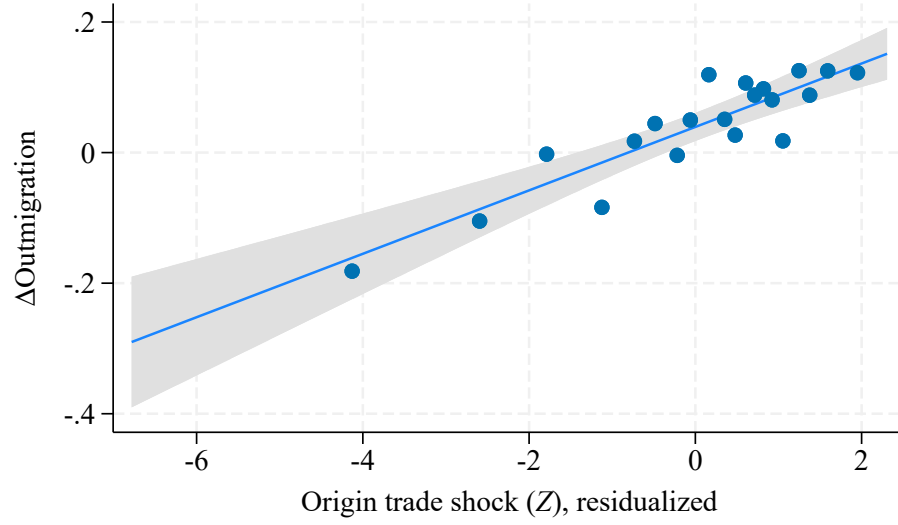
Figure 8. Origin Trade Shocks and Outmigration Rates

Note: This figure plots the changes in the outmigration rate ($\bar{\Delta}Outmigration_i$) against average trade shocks at origins of migrants in a destination (Z_i, partialing out variation attributed to reform status and polynomials of urban population). The binscatter plot is created using the data-driven method to estimate conditional means (Cattaneo et al., 2024). The fitted line is created using all observations (rather than just binscatter points).

A. Time Window: 2011–2015   **B.** Time Window: 2011–2019

Figure 9. Sensitivity Test

Note: This figure presents the share of total effect explained by the retention mechanism, calculated as $\widehat{AIE}/\hat{\beta}$, for each given combination of $\rho_{\phi\gamma}$ and $\rho_{\gamma\pi}$.

Tables

Table 2. Examining Smoothness in Covariates

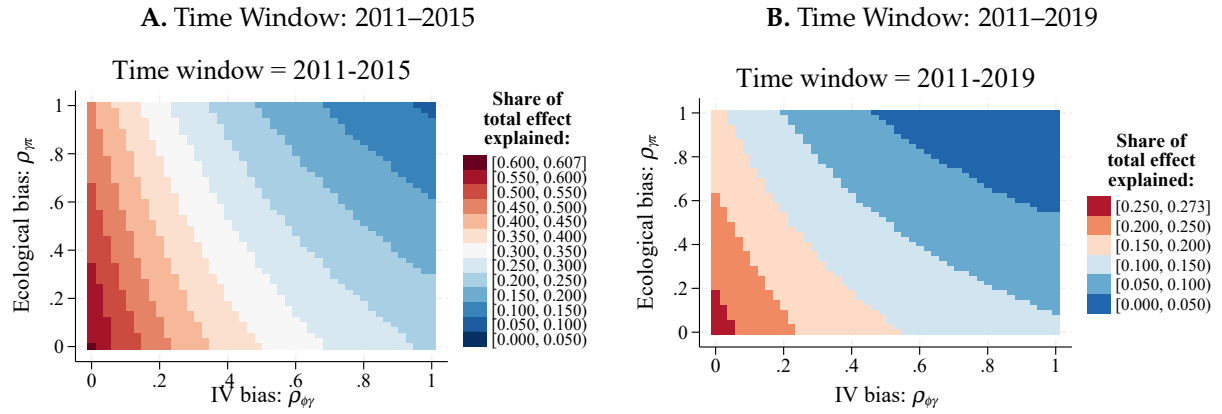|	(1) Coef. on *Reform*	(2) SE	(3) Coef. on *Reform*	(4) SE
Dependent				
Panel A: Pretrends (2011-2012, 2012-2013)				
ΔUnrest/L	-0.352**	(0.161)	0.070	(0.158)
ΔLog population	-0.019***	(0.003)	-0.005	(0.005)
ΔLog GDP	0.009*	(0.005)	0.001	(0.008)
ΔLog expenditure on social security	0.018	(0.013)	0.014	(0.021)
ΔLog expenditure on public security	0.012	(0.007)	-0.005	(0.011)
Year FE	Yes		Yes	
Polynomials			Yes	
Panel B: Predetermined characteristics (2010)				
Share of migrants	-0.161***	(0.031)	-0.063*	(0.035)
Share of urban residents	-0.242***	(0.025)	-0.122***	(0.037)
Share of secondary sector workers	-0.133***	(0.028)	-0.047	(0.043)
Share of tertiary sector workers	-0.141***	(0.019)	-0.067**	(0.027)
Share of internet users	-0.165***	(0.061)	0.044	(0.086)
Polynomials			Yes	

Note: This table examines the smoothness in covariates. Panel A looks at pretrends for 2011–2012 and 2012–2013. Panel B looks at predetermined prefectural characteristics measured in 2010. Columns (1) and (2) report the regression of the dependent on the reform indicator $Reform_i$ (controlling for year fixed effects for Panel A). Columns (3) and (4) report estimation results for the regression that additionally controls for the linear polynomial of $\Delta \log \left(P_{i,2014} \right)$ that is allowed to vary on each side of the reform cutoff. Standard errors clustered at the prefecture level are reported for Panel A, and heteroskedasticity-robust standard errors are reported for Panel B.

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table 3. The Effect of *Hukou* Reform on Labor Unrest

	Full Sample		Narrow Sample	
	(1)	(2)	(3)	(4)
	Unrest/L	Unrest/L	Unrest/L	Unrest/L
Reform × Post	-1.567***	-1.419***	-1.164***	-1.092*
	(0.271)	(0.373)	(0.318)	(0.578)
Control mean	3.395	3.395	2.739	2.739
Permuation *p*-value	0.000	0.000	0.000	0.000
Prefecture FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Polynomials		Yes		Yes
Observations	2,583	2,583	801	801

Note: This table presents the results for the effect of *hukou* reform on labor unrest rates. Columns (1) and (2) use the full sample. Columns (3) and (4) use the narrow sample that uses the optimal bandwidth proposed by Imbens and Kalyanaraman (2012). Standard errors clustered at the prefecture level are reported in parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table 4. Trade Shock, *Hukou* Reform, and Labor Unrest

	(1)	(2)	(3)	(4)
	Unrest/L	Unrest/L	Unrest/L	Unrest/L
Trade shock $[\beta_1]$	-0.148***	-0.148***	-0.087**	-0.049
	(0.031)	(0.031)	(0.036)	(0.046)
Trade shock$_{t+1}$		0.008		
		(0.021)		
Trade shock × Reform × Post $[\beta_5]$			0.176*	0.192*
			(0.092)	(0.099)
Prefecture FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Polynomials				Yes
Observations	2,583	2,583	2,583	2,583

Note: This table presents the effect of trade shock on labor unrest and how it varies by the *hukou* reform status. Standard errors clustered at the prefecture level are reported in parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table 5. The Effect of the *Hukou* Reform on Outmigration

	Outmigration from the 2010 destination				
	(1)	(2)	(3)	(4)	(5)
Reform × Post [ρ]	-0.072**	-0.072**	-0.071**	-0.089**	-0.090**
	(0.034)	(0.034)	(0.034)	(0.040)	(0.041)
Reform × Post × Patience (std.)				-0.053**	-0.054**
				(0.026)	(0.025)
Reform × Post × Risk taking (std.)					0.005
					(0.024)
Reform × Post × High school completion					0.016
					(0.021)
Control mean	0.141	0.141	0.141	0.147	0.147
Prefecture FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Polynomials	Yes	Yes	Yes	Yes	Yes
Origin FE		Yes	Yes	Yes	Yes
Indiviudal covariates × Year FE			Yes	Yes	Yes
Observations	58,701	58,701	58,701	51,769	51,769

Note: This table reports the effect of *hukou* reform on the outmigration rate. Standard errors clustered at the prefecture level are reported in parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table 6. *Hukou* Reform, Outmigration Rate, and Labor Unrest

	ĀUnrest/L, 2011–2015			ĀUnrest/L, 2011–2019		
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Mediation-OLS	Mediation-IV	Baseline	Mediation-OLS	Mediation-IV
Reform [β or τ]	-1.047**	-0.851	-0.411	-1.419***	-1.237***	-1.033***
	(0.518)	(0.536)	(0.608)	(0.370)	(0.364)	(0.383)
ĀOutmigration [γ]		1.855***	6.005***		1.726***	3.654***
		(0.453)	(1.304)		(0.360)	(1.018)
Polynomials	Yes	Yes	Yes	Yes	Yes	Yes
% Total effect explained		0.188	0.607		0.129	0.273
Effective F stat.			58.750			58.750
tF 95% CI			[3.286, 8.724]			[1.532, 5.776]
IV-OLS gap in γ			4.250			1.989
Gap due to OVB			4.183			1.997
Observations	287	287	287	287	287	287

Note: This table reports causal mediation analysis that quantifies the importance of the retention mechanism, as captured by the outmigration rate. Columns (1) and (2) represent the conventional approach. Columns (3)-(4) use the IV-augmented approach. The effective F statistic is calculated following Olea and Pflueger (2013). *tF* 95 percent confidence interval follows Lee et al. (2022). The IV-OLS gap is decomposed using the methodology by Ishimaru (2024). Robust standard errors are reported in parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table 7. *Hukou* Reform and Migrant Characteristics

	(1) Female	(2) Han ethnic	(3) Age below 35	(4) Married	(5) High school completion	(6) Cross-province migrant	(7) Manufacturing worker	(8) Construction worker
Panel A: All migrants								
Reform × Post	0.006	-0.003	0.011	0.005	-0.005	-0.000	0.035	0.012
	(0.013)	(0.004)	(0.010)	(0.010)	(0.014)	(0.011)	(0.044)	(0.008)
Control mean	0.474	0.953	0.536	0.876	0.389	0.571	0.149	0.049
Sample period	2011–18	2011–18	2011–18	2011–18	2011–18	2011–18	2011–18	2011–18
No. prefectures	255	255	255	255	255	255	255	255
Observations	990,912	990,912	990,912	990,912	990,912	990,912	863,055	863,055
Panel B: New arrivals								
Reform × Post	-0.001	0.005	0.007	0.025	-0.016	0.004	0.054	0.003
	(0.016)	(0.005)	(0.015)	(0.016)	(0.016)	(0.019)	(0.051)	(0.013)
Control mean	0.459	0.947	0.639	0.809	0.395	0.524	0.153	0.063
Sample period	2011–18	2011–18	2011–18	2011–18	2011–18	2011–18	2011–18	2011–18
No. prefectures	255	255	255	255	255	255	255	255
Observations	238,161	238,161	238,161	238,161	238,161	238,161	208,191	208,191

Note: Note: This table presents the effects of *hukou* reform on migrants' characteristics. All regressions control for prefecture and year fixed effects and polynomials of centered log urban population. Standard errors clustered at the prefecture level are reported in parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table 8. *Hukou* Reform on Available Benefits

	(1) Working	(2) Log wage	(3) ASS	(4) Co-residence w. Spouse	(5) Co-residence w. Child(ren)
Reform × Post	-0.003	0.003	0.006	-0.020*	-0.006
	(0.007)	(0.019)	(0.029)	(0.010)	(0.015)
Control mean	0.883	8.153	0.522	0.885	0.654
Sample period	2011–18	2011–18	2011, 13, 16	2011–18	2012–16, 18
No. prefectures	255	255	255	255	255
Observations	990,912	810,696	162,239	867,392	661,846

Note: This table presents the effects of *hukou* reform on benefits available to migrants. Dependent variables are: indicator for working currently, log wages, access to social security (ASS), indicator of co-residence with spouse (conditional on having got married), and indicator of co-residence with children (conditional on having children). All regressions control for prefecture and year fixed effects and polynomials of centered log urban population. Standard errors clustered at the prefecture level are reported in parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table 9. *Hukou* Reform and Autocratic Control

	(1) Log expenditure on public security	(2) Log expenditure on social security	(3) Share of stability related keywords	(4) Share of unrest events known to be repressed
Reform × Post	-0.016	0.041	0.007	0.088
	(0.029)	(0.033)	(0.009)	(0.077)
Control mean	13.138	13.624	0.081	0.228
Sample period	2011–17	2011–17	2011–15	2011–19
No. prefectures	287	287	287	285
Observations	1,982	1,991	1,410	1,806

Note: This table presents the effects of *hukou* reform on autocratic control. Dependent variables are: log expenditure on public security (police), share of stability-related mentions in next year's government work report, and share of unrest events repressed. All regressions control for prefecture and year fixed effects and polynomials of centered log urban population. Standard errors clustered at the prefecture level are reported in parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Online Appendices

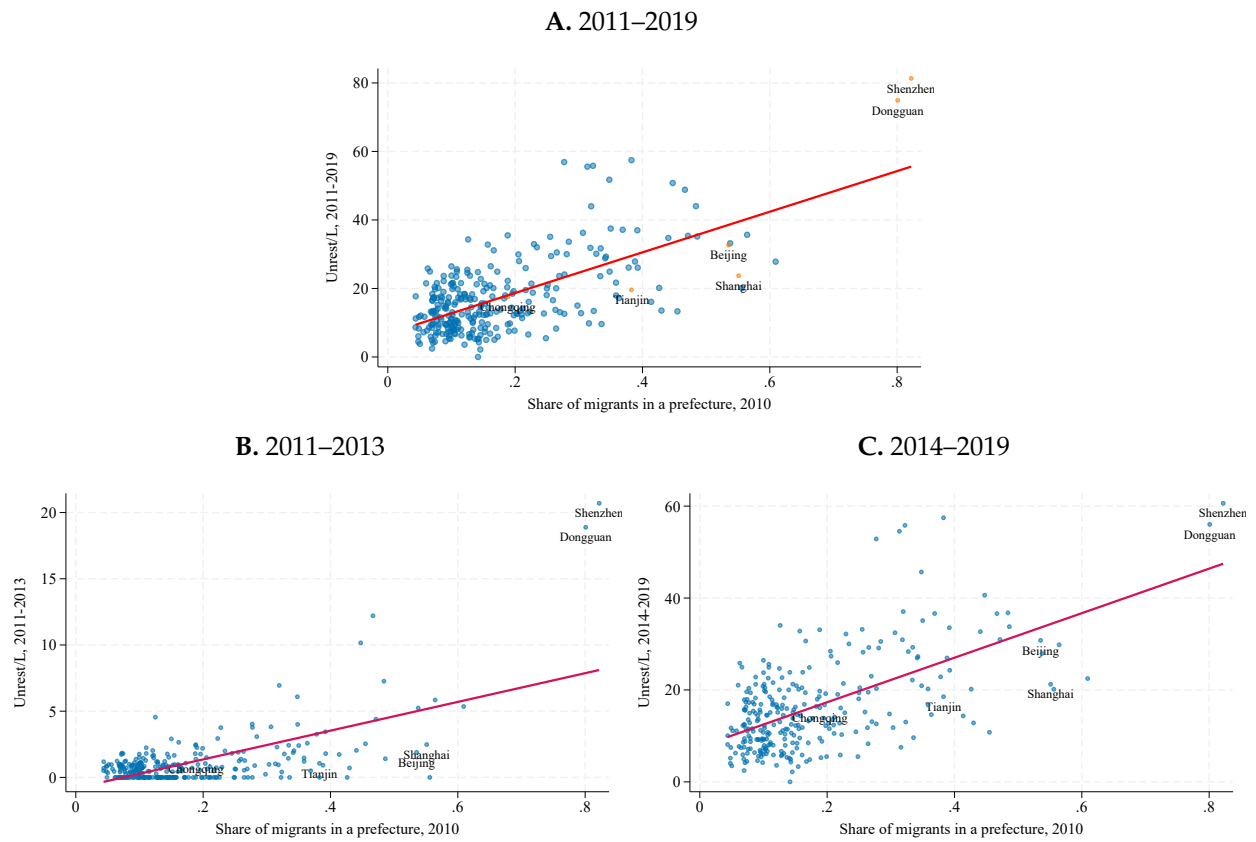## Contents

A   Additional Figures

Figure A1. Migrant Share and Labor Unrest Rate

Note: This figure depicts the relationship between the migrant share and the labor unrest rate. The migrant share is measured using the 2010 population census. It is defined as the share of the population whose *hukou* registration is not in the current prefecture. The labor unrest rate is measured using the China Labor Bulletin: it is defined as the total number of unrest events per million working-age population. Panel A is for the period of 2011–2019, Panel B is for the pre-reform period, 2011–2013, and Panel C is for the post-reform period, 2014–2019.
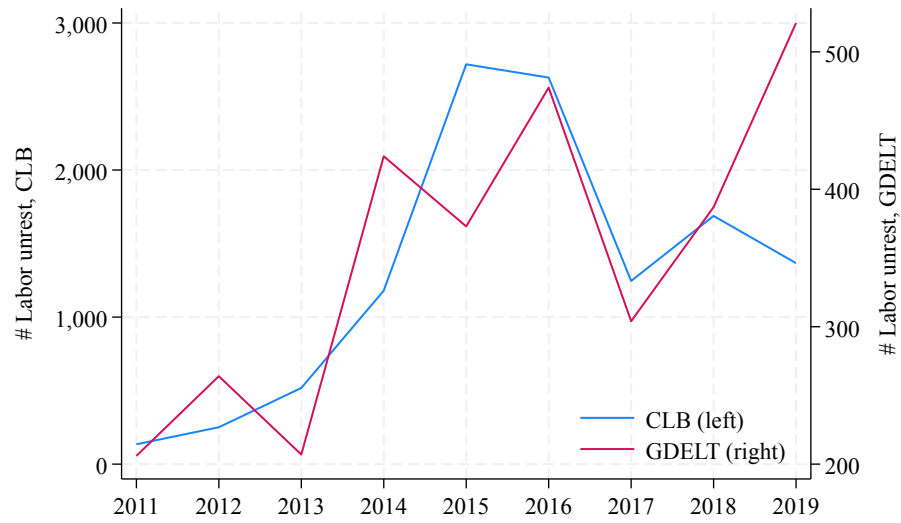
Figure A2. Labor Unrest in CLB and GDELT

Note: This figure presents the national trends of labor unrest events recorded in CLB and GDELT. In GDELT, an event is defined as a labor unrest event if it is classified into the "Protest" category and involves labor.
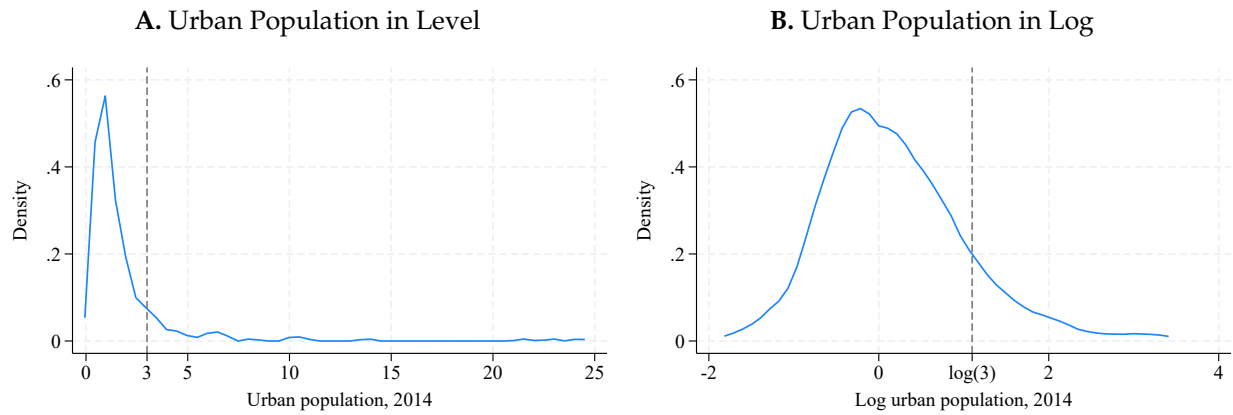
Figure A3. Distribution of Urban Population

Note: This figure depicts the distributions of urban population in level and in log. The vertical lines the reform cutoff, 3 and log(3), respectively.

A.3
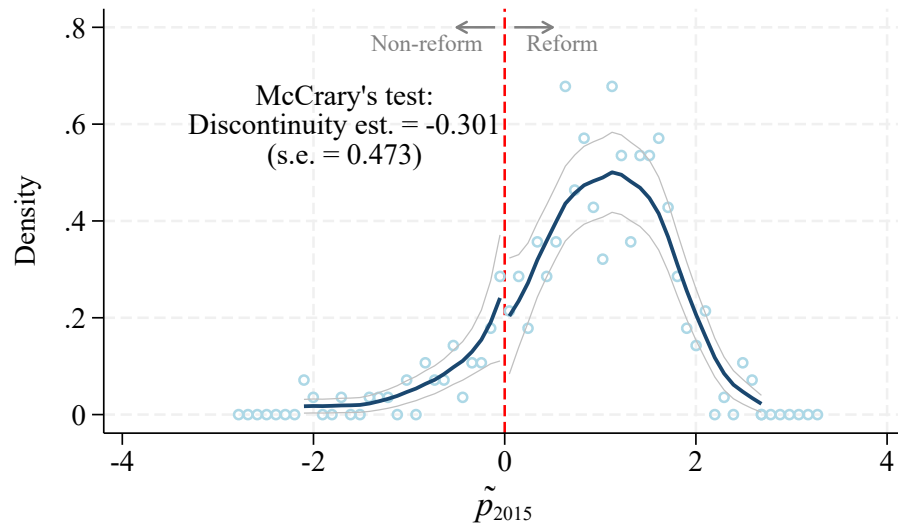
Figure A4. Density of Centered Log Urban Population in 2015

Note: This figure A4 depicts the density of $\tilde{p}_{i,2015} = \log(3) - \log(P_{i,2015})$. We report McCrary's test for density discontinuity at 0.

A. Pretrends
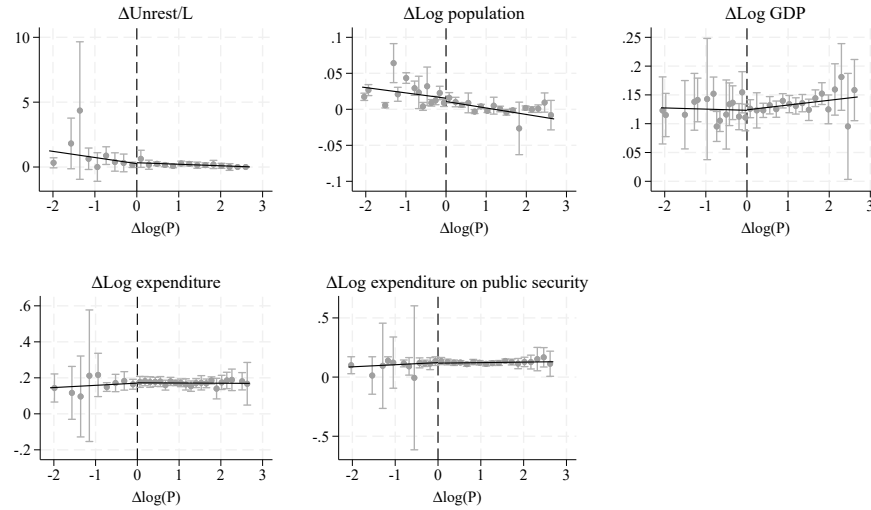
B. Predetermined Characteristics
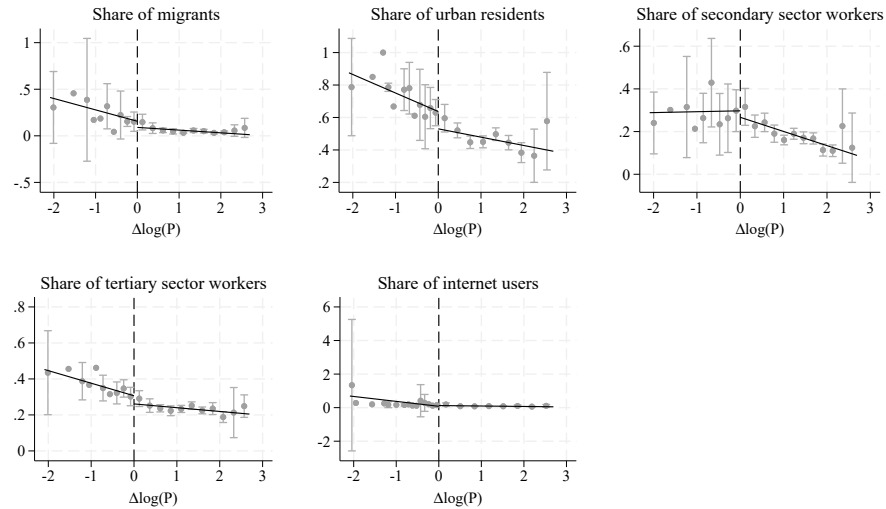
Figure A5. RD Plots of Pre-reform Covariates

Notes: This figure visually presents balance tests of pre-reform covariates. Panel A examines pretrends in unrest rate and a set of variables that may be conducive to unrest. Panel B examines a set of predetermined characteristics.

A. Full Sample

B. Narrow Sample
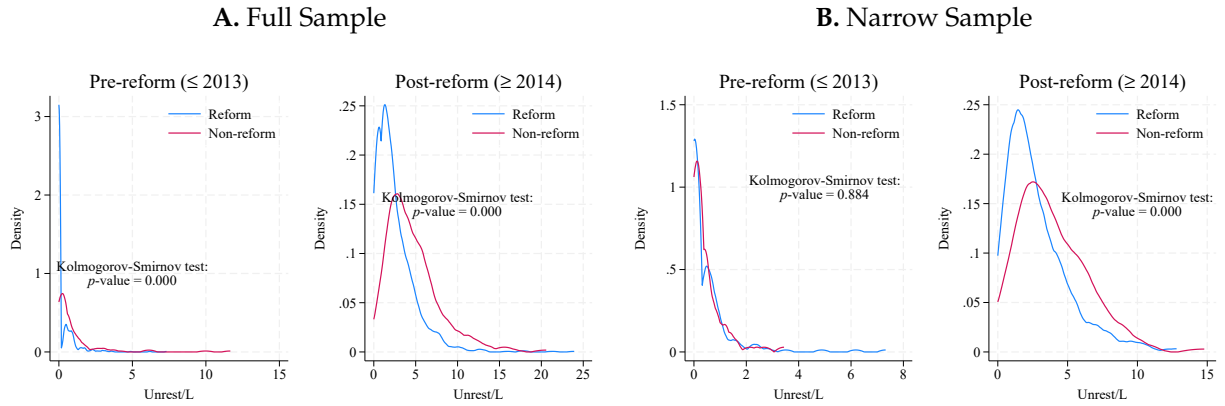
Figure A6. Distributions of Labor Unrest Rates: 2011–2019

Note: This figure compares the distributions of labor unrest rates for pre- and post-reform periods. We report a Kolmogorov-Smirnov test for the density equality null. We conduct the comparison for both the full and narrow samples.
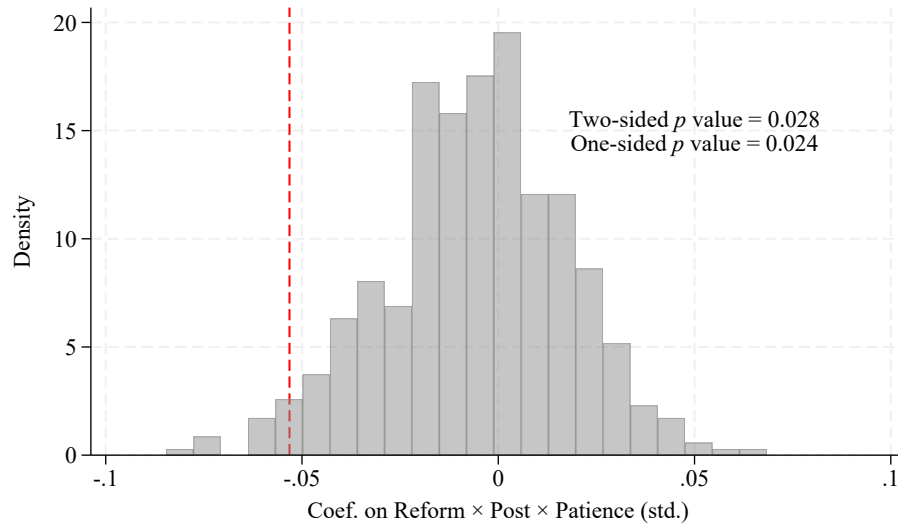
Figure A7. Permutation Test for the Differential Effect on Outmigration by Patience Level

Note: This figure reports a permutation test for the coefficient on *Reform × Post × Patience* in Column (4) of Table 5. We permute the provincial level measure of the average patience level, and re-estimate the specification for Column (4) of Table 5 to get a counterfactual estimated coefficient on *Reform × Post × Patience*. We repeat this permutation for 500 times. Comparing the distribution of counterfactual estimated coefficients (as plotted by the bars) with the actual estimate (the vertical dashed line), we calculate the *p* value for the null that the coefficient on *Reform × Post × Patience* is zero. We report *p*-values for one-sided and two-sided tests.

B Additional Tables

Table A1. Results Using Polynomials of Logarithmic vs. Level Urban Population

	(1)	(2)	(3)
	Unrest/L	Unrest/L	Unrest/L
Reform × Post	-1.567***	-1.419***	-1.237**
	(0.271)	(0.373)	(0.385)
Control mean	3.395	3.395	3.395
Prefecture FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Polynomials (log)		Yes	
Polynomials (level)			Yes
Observations	2,583	2,583	2,583

Note: This table presents the results for the effect of *hukou* reform on labor unrest rates. The first two columns are identical to the first two columns reported in Table 3. Columns (1) reports the two-way fixed effects estimate. Column (2) includes polynomials of centered log urban population, that is, $\log(3) - \log(P_{i,2014})$. Column (3) includes polynomials of centered urban population, i.e., $3 - P_{i,2014}$. Standard errors clustered at the prefecture level are reported in parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table A2. Promotion Prospect and Urban Population Change

	Dependent: $\Delta \log(P)$, 2013–2014		
	(1)	(2)	(3)
	All	$P_{2013} < 3M$	$P_{2013} > 3M$
Promotion prospect	-0.026	-0.024	-0.203
	(0.082)	(0.091)	(0.279)
Observations	287	251	36

Note: This table reports the association between a prefectural party secretary's promotion prospect and the growth in urban population from 2013 and 2014 as observed in the UCSY. The promotion prospect index is estimated following Wang et al. (2020): the higher, the better prospect. We report the association for the entire sample (287 prefectures), prefectures with less than 3 million urban population before the reform initiative (2013), and prefectures with more than 3 million urban population before the reform initiative. Robust standard errors are reported in parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table A3. Excluding Welfare-Related Labor Unrest

	(1) Unrest/L	(2) (Hukou-Unrelated Unrest)/L	(3) (Hukou-Related Unrest)/L
Reform × Post	-1.419***	-1.342***	-0.077
	(0.373)	(0.353)	(0.051)
Control mean	3.395	3.138	0.257
Prefecture FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Polynomials	Yes	Yes	Yes
Observations	2,583	2,583	2,583

Note: This table presents the effect of the *hukou* reform on labor unrest that are not directly related to *hukou* policy itself. Column (1) reports the baseline that looks at all types of unrest. Column (2) excludes unrest events that are due to demands for social security and housing subsidies, as reported in CLB. In contrast, Column (3) looks at unrest events that are due to demands for social security and housing subsidies. Standard errors clustered at the prefecture level are reported in parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table A4. *Hukou* Reform, Origin Trade Shock, and Outmigration

	Outmigration from 2010 residence		
	(1)	(2)	(3)
Reform × Post	-0.068**	-0.066**	-0.066**
	(0.031)	(0.031)	(0.031)
Origin trade shock	0.014**	0.015**	0.021
	(0.006)	(0.006)	(0.016)
Origin trade shock$_{t+1}$			0.009
			(0.021)
Control mean	0.130	0.130	0.130
Prefecture FE	Yes	Yes	Yes
Origin FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Polynomials	Yes	Yes	Yes
Indiviudal covariates × Year FE		Yes	Yes
Observations	58,306	58,306	58,306

Note: This table reports the effect of origin trade shock on the outmigration rate. Standard errors are clustered at the residential prefecture of 2010 and origin levels.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

C Robustness to Sampling of Prefectures

The main sample includes 287 prefectures that urban population in 2014 is available (from the Urban Construction Statistical Yearbook). We show that our results are not driven by this sampling decision. Specifically, we present results based on three alternative samples:

1. 287 prefectures in the main sample and prefectures in Tibet and Xinjiang that urban population in 2014 is available (in total, 297 prefectures);

2. All prefectures that urban population in 2014 is available (in total, 303 prefectures);

3. All prefectures in China (in total, 337 prefectures).

For each sample, we report both the DiD and DiDC estimates. One thing to note is, when using the sample of all 337 prefectures, some 34 of them do not have urban population in 2014 available. To include them in estimation, we define their reform status as reform, i.e., $Reform_i = 1$, as they are presumably small. This is confirmed by the 2010 population census: all of them had an urban population below 3 million in 2010 (maximum = 1.597 million). To implement the DiDC estimation, we also calculate the 34 prefectures' running variable $\tilde{p}_i$ using urban population in 2010 as reported by the 2010 population census.

Table A5 reports the results. Clearly, no matter which sample is used, the results consistently imply that the *hukou* reform reduced labor unrest. The effect size does not vary markedly by sample used. Figure A8 displays the event-study estimates based on different samples. Again, the negative effect of *hukou* reform on labor unrest is not due to a particular sampling choice.

Table A5. Robustness to Sampling of Prefectures

	Main sample		Plus Tibet & Xinjiang		Urban population available		All prefectures	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Unrest/L	Unrest/L	Unrest/L	Unrest/L	Unrest/L	Unrest/L	Unrest/L	Unrest/L
Reform × Post	-1.567***	-1.419***	-1.551***	-1.214***	-1.578***	-1.124***	-1.661***	-1.201***
	(0.271)	(0.373)	(0.271)	(0.375)	(0.270)	(0.370)	(0.269)	(0.360)
Control mean	3.395	3.395	3.335	3.335	3.335	3.335	3.335	3.335
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Polynomials		Yes		Yes		Yes		Yes[†]
No. prefectures	287	287	298	298	303	303	337	337
Observations	2,583	2,583	2,682	2,682	2,727	2,727	3,033	3,033

Note: This table presents the robustness of our results to sampling of prefectures. Columns (1)-(2) display results based on the main sample 287 prefectures, which are the same as Columns (1)-(2) in Table 3. Columns (3)-(4) use a sample that includes prefectures in Tibet and Xinjiang (whose urban population in 2014 is available). Columns (5)-(6) use sample of all prefectures whose urban population in 2014 is available. Columns (7)-(8) use all 337 prefectures. In these regressions, the prefectures whose urban population in 2014 is not available are defined as reform prefectures, as they are presumably small and have an urban population below 3 million. Their $\tilde{p}_i$ is calculated based on urban population from the 2010 census. Standard errors clustered at the prefecture level are reported in parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

A. Main Sample

N prefectures = 287

B. Tibet and Xinjiang Included

N prefectures = 298

C. All with Urban Population Available

N prefectures = 303

D. All Prefectures
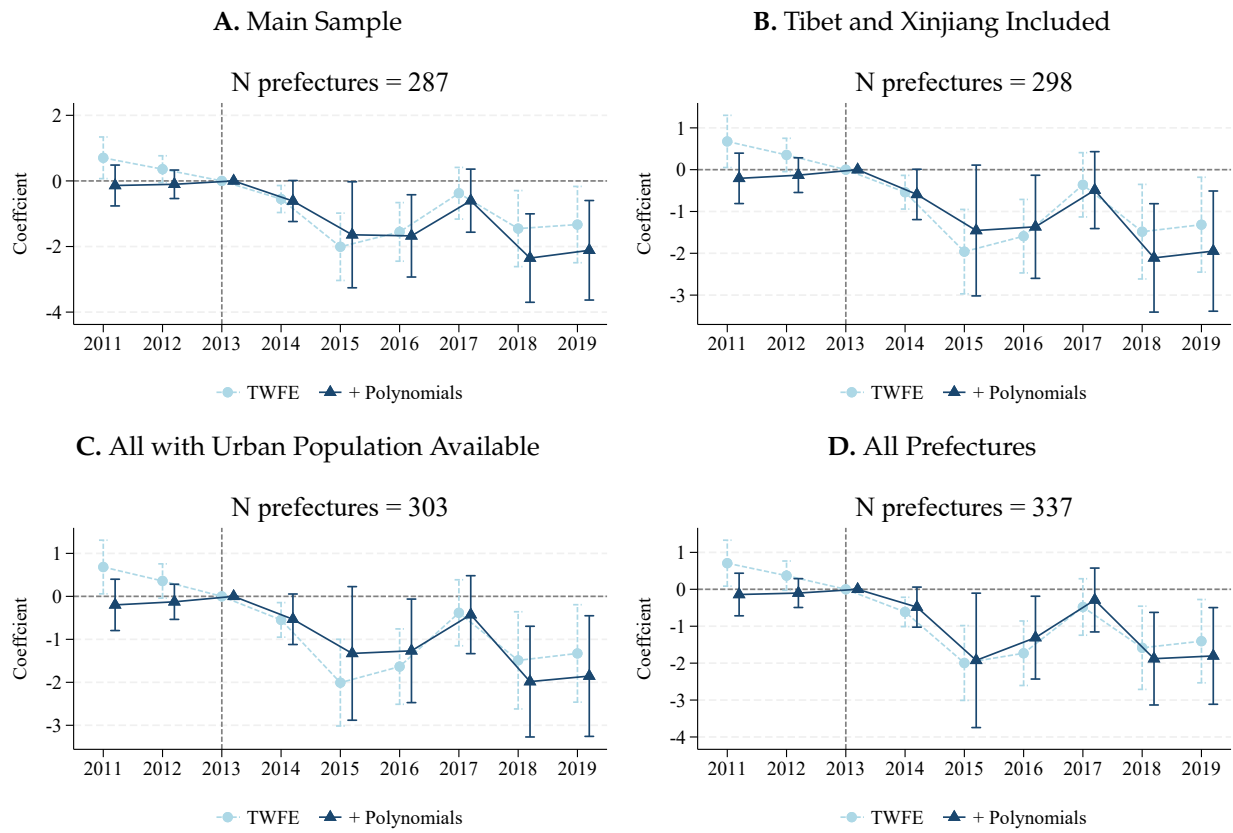
N prefectures = 337

Figure A8. Robustness to Sampling of Prefectures: Event Study

D   Verifying the Definition of Reform Status

To verify the population-based definition of reform status, we extensively collect official documents published by local governments regarding the reform, carefully read them, and code up a prefecture's reform status based on the content for comparison. The documents are collected from government websites, media outlets, and a database of *hukou* reforms by Zhang and Lu (2019).

The convention of policy-making in China is that each level of government, adapting to local conditions, would issue implementation guidelines to follow the upper-level government's guidelines. Typically, the guidelines become more specific about provisions when they get to lower levels. In the case we study, after the central government launched the *hukou* reform initiative, the provincial governments would release broad guidelines regarding implementation for their prefectures, and prefectural governments would further decide their provisions on the basis of the national initiative and provincial guidelines.[1]

To explain the information in these guidelines, consider one example of Hebei Province's guidelines, issued on November 20, 2014 (Hebei Government, 2014).

[...]

(II.2) **Fully lift restrictions on settlement in urban areas of prefectures with a population of less than 1 million.** In the urban areas of Chengde, Zhangjiakou, Qinhuangdao, Cangzhou, Hengshui, and Xingtai, where the population is less than 1 million, people who have a legal and stable residence, and their spouses, children, and parents of both spouses who live together, can apply for a local *hukou*.

(II.3) **Reasonably determine the conditions for settlement in urban areas of prefectures with a population of more than 1 million.** In urban areas of Shijiazhuang, Tangshan, Baoding, and Handan, where the population is more than 1 million, if one of the following conditions is met, the individual and his/her spouse, children, and parents of both spouses living together can apply for a local permanent residence permit:

  (a) Persons with legal and fixed residences. Legal and fixed residences include self-purchased houses and legally self-built houses (the same below) that meet the living conditions and are actually lived in;

  (b) Persons with legal and stable occupations and other legal and stable residences.   Persons with legal and stable occupations include those

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[1]In principle, the county governments can further specify their own guidelines. However, the prefectural guidelines are already detailed enough, leaving little room for further discretion to be used. Therefore, in practice, we find very few guidelines at the county level.

employed by administrative organs, people's organizations, and institutions or those who have signed labor contracts with enterprises in accordance with the law and have participated in the basic pension insurance for urban employees for a certain number of years; those who have obtained industrial and commercial business licenses in accordance with the law and paid taxes in accordance with the law for a certain number of years; and those who are engaged in freelance occupations and have obtained residence permits for a certain number of years. Other legal and stable residences refer to legal and stable residences other than legal and fixed residences. The specific time limit for participating in the basic pension insurance for urban employees, paying taxes in accordance with the law, and obtaining residence permits shall be determined by the governments of each prefecture-level city in light of local conditions, and in principle shall not exceed 1 year.

As the provincial capital, Shijiazhuang should optimize the personnel structure in the urban area. On the basis of the above conditions, reasonable regulations can be made on the scope of legal and stable occupations and legal and stable residences, and the time limit for participating in the basic pension insurance for urban employees, paying taxes in accordance with the law, and obtaining residence permits according to the actual situation.

(II.4) **Reasonably determine the settlement conditions in towns around the national capital.** In the urban area of Langfang City, Sanhe City and its subordinate towns, Dachang Hui Autonomous County, Xianghe County, Yongqing County, Gu'an County government seat town, and other towns around the capital, those who have a legal and stable job and a legal and stable residence and have participated in social insurance for a certain number of years in accordance with national regulations, and the person himself and his spouse, children, and parents of both spouses who live together, can apply for a local permanent residence. The scope of legal and stable jobs, legal and stable residences, and the number of years of participation in social insurance shall be specifically stipulated by the Langfang Municipal Government based on actual conditions. No requirements such as area and amount shall be set for legal and stable residences, and the number of years of participation in social insurance shall not exceed 5 years.

[...]

The guidelines provided a broad picture of how the *hukou* reforms would unfold in the prefectures of Hebei. Per (II.2), one can know that Chengde, Zhangjiakou, Zhangjiakou, Qinhuangdao, Cangzhou, Hengshui, and Xingtai are mandated to lift their restrictions on *hukou* transfers, thus, they are coded to be reform prefectures. By contrast, the tone in (II.3) is more subtle for Shijiazhuang, Tangshan, Baoding, and Handan — they have some discretion in deciding their

requirements regarding residence and jobs, but they need to abide by the provincial government's guidelines. Thus, it is necessary to examine the actual provisions made by these prefectural governments to know their reform status. For instance, after review, we find that Shijiazhuang provided lenient requirements although the provincial government specifically allowed it to maintain tight control due to its provincial capital status. Lastly, per (III.3), one can know that Langfang should maintain tight control since it neighbors Beijing.

Generalizing this reading of Hubei Province's reform guidelines, we manually code up each prefecture's actual reform status through the following two-step procedure.

1. A prefecture is coded as "reform" if the provincial guidelines unambiguously mandate it to "fully lift restrictions."
2. If the reform status is not determined at step 1, we obtain and read the prefecture's own guidelines to find out its reform status. We code a prefecture as "non-reform" if it has any restrictive provisions on granting *hukou* transfers as follows:
   (a) A point-based system to screen migrants;
   (b) A requirement of having had a long duration of stay in the prefecture or enrollment in the local social security system (> 3 years);
   (c) A requirement of select types of stable residences and/or jobs, e.g., only home ownership is eligible or a minimum duration of work experience is required.

Otherwise, a prefecture is coded as "reform."

Table A6 shows the release time of each province's guidelines following the national initiative. Most of them were released in late 2014 or the first half of 2015.

Comparing the population-based ($\leq$ 3 million or not) and manually coded reform status, only 17 out of 287 prefectures, or 6 percent of the sampled prefectures, have a disagreement between the two definitions. Table A7 tabulates these disagreements. In Panel A, 9 large-sized prefectures (urban population > 3 million) relaxed their *hukou* transfers, whereas in Panel B, 8 small-sized prefectures ($\leq$ 3 million) did not relax. We do not interpret these disagreements as local governments' deliberate defiance of the national reform initiative, instead, they can be due to discretion based on local conditions. Prefectures in Panel A may make lenient provisions to attract labor. In contrast, prefectures in Panel B may be the hot spots of population inflows. Langfang is adjacent to Beijing and thus is home to many migrants who work in Beijing, thus, it is urged to maintain tight control. The remaining ones are all prefectures in coastal provinces attractive to new migrants. The Guangdong government even explicitly required Zhuhai and Zhongshan, two manufacturing hubs in the Pearl River Delta, to impose strict restrictions on granting local *hukou*.

Table A6. Policy Time by Province

Province	Policy Time	Province	Policy Time
Xinjiang*	10/13/2014	Guizhou	05/04/2015
Heilongjiang	11/03/2014	Anhui	05/08/2015
Henan	11/04/2014	Hunan	05/11/2015
Jiangxi	11/14/2014	Yunnan	06/01/2015
Shandong	11/19/2014	Guangdong	06/24/2015
Hebei	11/20/2014	Liaoning	07/10/2015
Sichuan	11/22/2014	Chongqing	08/25/2015
Ningxia[†]	-/-/2015	Hubei	09/06/2015
Gansu	01/01/2015	Inner Mongolia	09/08/2015
Jiangsu	01/12/2015	Zhejiang	12/10/2015
Shanxi	01/14/2015	Hainan	12/24/2015
Qinghai	01/27/2015	Shanghai	04/15/2016
Jilin	01/29/2015	Tianjin	04/20/2016
Fujian	02/13/2015	Tibet*	05/16/2016
Guangxi	02/25/2015	Beijing	09/08/2016
Shaanxi	03/19/2015		

Note: * = excluded from the main sample. [†] = only the release year is known.

Table A7. Discrepancies between Population-Based and Manually-Coded Definitions

Panel A: Population-based = 0, manually-coded = 1			Panel B: Population-based = 1, manually-coded = 0		
Province	Prefecture	Population (million)	Province	Prefecture	Population (million)
Hebei	Shijiazhuang	4.678	Hebei	Langfang	0.983
Hebei	Tangshan	3.088	Jiangsu	Changzhou	2.767
Jilin	Changchun	4.181	Zhejiang	Wenzhou	2.008
Jiangsu	Huai'an	3.317	Fujian	Fuzhou	2.495
Zhejiang	Shaoxing	3.562	Guangdong	Zhuhai	2.514
Anhui	Hefei	4.490	Guangdong	Zhongshan	2.878
Shandong	Zibo	3.063	Hainan	Haikou	2.667
Shandong	Linyi	3.086	Hainan	Sanya	0.764
Guangdong	Shantou	5.720			

Note: This table tabulates prefectures that have a disagreement between population-based and manually-coded definitions of reform status.

E   Auxiliary Data

Local Socioeconomic Variables. Our analysis uses various local socioeconomic variables, which provide detailed information on economic growth, demographics, fiscal expenditures, and local governance. We collect them from several sources: China City Statistical Yearbooks, population census tabulations, as well as data other researchers compile from the Chinese government's releases (Campante et al., 2023; Rogoff and Yang, 2024).

Trade Data. To construct trade shock measures, we use: (i) the prefecture-level export structure measured using the 2010 Chinese customs database, obtained from Campante et al. (2023), and (ii) global export volumes recorded by the BACI database that improves the UN Comtrade database (`https://www.cepii.fr/CEPII/en/bdd_modele/bdd_modele_item.asp?id=37`).

Officials' Promotion Prospects. Following Wang et al. (2020), we estimate a local leader's *ex ante* promotion prospect in a year. We focus on the party secretary, who is the chief leader of a prefecture. The estimated promotion prospect is a flexible function of the age when he starts the term, his official rank in the bureaucratic system, and some individual characteristics, which can be used as a proxy for his career concerns. This hinges on the personnel rule that mandates retirement ages that increase with bureaucratic ranks).[2] Specifically, we estimate the following Probit model:

$$\Pr(Promition_{it}) = \Phi\left[\beta_0 StartAge_{it} + \beta_1 HighRank_{it} + \beta_2\left(StartAge_{it} \times HighRank_{it}\right) + \mathbf{X}'_{it}\delta\right].$$
(A1)

i indexes prefectures and t indexes terms. The unit of analysis is prefecture-by-term. $Promotion_{it}$ is a dummy that equals one if prefecture i's leader is promoted after term t. An outcome after a term ends is considered as promotion if the prefectural party secretary is appointed to a position ranked higher than his previous rank. However, we exclude rank enhancement as promotion if the prefectural party secretary is placed in an honorary position in the Chinese People's Political Consultative Conference (CPPCC) or the People's Congress (PC) at the prefectural or provincial level, which is commonly regarded as semi-retirement in China since these positions carry no real power. $StartAge_{it}$ is the age when a party secretary starts the term t of prefecture i. Most prefectural party secretaries have a prefectural (*zhengting*) rank, but some have a higher rank: deputy provincial (*fubu*), provincial (*zhengbu*), or even deputy national (*fuguo*). $HighRank_{it}$ is an indicator of the above deputy provincial rank. $\mathbf{X}_{it}$ includes an officials' characteristics, including indicators for graduate degree indicator and central government experience.

We use biographical data on local officials compiled by Yao et al. (2022) and Jiang (2018). Our data include 2,305 party secretary terms in 337 prefectures between 2000 and 2017. The average start age is 50.8 years old, and 18.3 percent of party secretaries have an above deputy provincial rank. Table A8 reports the estimation results. The first two columns shows estimates by a linear

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[2]The retirement age is 60 for both prefecture level and deputy-province-level leaders and 65 for province-level leaders.

probility model (LPM), and Columns (3) and (4) show estimates by a Probit model. The results are consistent with Table 2 in Wang et al. (2020). We use the estimated model in Column (4) to generate the predicted probability of promotion and use that as an index of promotion prospects.

Table A8. Prediction of Promotion Prospects

	Dependent: Promotion			
	(1)	(2)	(3)	(4)
	LPM	LPM	Probit	Probit
Start age	-0.026***	-0.025***	-0.093***	-0.089***
	(0.003)	(0.003)	(0.009)	(0.009)
Deputy province or above	-1.921***	-1.925***	-8.615***	-8.752***
	(0.197)	(0.200)	(1.221)	(1.245)
Start age × Deputy province or above	0.035***	0.035***	0.157***	0.159***
	(0.004)	(0.004)	(0.023)	(0.023)
Graduate degree		0.035**		0.152**
		(0.017)		(0.074)
Central govt. expenditure		0.057		0.228*
		(0.040)		(0.138)
Dependent mean	0.185	0.185	0.185	0.185
Covariates		Yes		Yes
R^2	0.073	0.076		
Pseudo R^2			0.076	0.079
Observations	2,244	2,244	2,244	2,244

Note: This table reports how we constructed the promotion prospect variable following Wang et al. (2020). Officials' characteristics include indicators for graduate degree and central government experience. Robust standard errors are reported in parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

GDELT. GDELT, the abbreviation of the Global Database of Events, Language, and Tone, is a commonly used dataset on global events (`www.gdeltproject.org`), especially on social unrest (Cantoni et al., 2023). GDELT has conducted automated scraping of the world's broadcast, print, and web news since 1979, and uses machine learning methods to code an event's types, participants, date, location, etc. Given GDELT's wide coverage, we use it to measure the degree of media attention or reporting of local events.

Migrant Survey. We use a nationally representative survey on migrants — China Migrants Dynamic Survey (CMDS hereafter) — conducted annually by China's National Health Commission since 2009, targeting migrants living and working in more than 300 prefectures in 31 provinces across the country. CMDS employs a stratified probability-proportional-to-size sampling method is adopted so that the sample size is proportional to the number of migrants in each region. The data provide rich information on migrants' demographics, families, labor market outcomes, and attitudes. CMDS has a much larger sample size of migrants, ranging from 100 to 200 thousand for

each year, than any other survey data in China. We use eight waves of surveys between 2011 and 2018. We focus our attention on individuals of the working ages, 25–54. We only keep individuals in prefectures that appear in all eight waves; as such, our sample covers 255 prefectures.

Population Census Microfile. To study how the *hukou* reform influences migrants' re-migration decisions, we use the microfile of the 2015 mini population census that surveys 1 percent population. The survey was conducted in November 2015. The census asks an individual to retrospect residential locations as of November 2010 and November 2014. Thus we can construct the residential history at three time points: (the end of) 2010, 2014, and 2015. We also observe one's *hukou* registration location in 2015 — for those whose *hukou* registration did not change between 2010 and 2015, we can use this information to define migration status back to 2010.

Global Preference Survey (GPS). We use the China sample of GPS by Falk et al. (2018) for information on preferences (https://gps.iza.org/home). The China sample includes a total of 2,574 individuals. We use only those at the ages of 25–54, which leaves us with 1,422 individuals. The GPS only provides a province identifier. Hence, we aggregate preference variables to the province level, using the sampling weights provided by GPS.

F Other Population-Based Policies

Our estimates for the effects of the *hukou* reform would be contaminated, if there exist other concurrent policies that (i) have provisions correlated with urban population and (ii) can influence labor unrest. To examine this possibility, we conduct a comprehensive search of population-related policies using the *PKULaw* database (`https://www.pkulaw.com`), which provides extensive information on Chinese laws and regulations. It has been used in research on policy-making in China (e.g., Tian, 2024; Wang and Yang, 2021). We use two keywords to search for urban population-related policies: "urban population (城区人口 in Chinese)" and "city size (城市规模 in Chinese)." The *PKULaw* database has a fuzzy search feature and it would return policies that contain the search keyword and its synonyms. We focus on policies published by the central government, which have the potential of creating provisions tiered by urban population.

Results of Searches Based on "Urban Population." We first use the keyword "urban population." Table A9 tabulates the number of policies published by the central government. Overall, *PKULaw* returns a total of 126 relevant policies during 2011–2019. We take a closer look at these policies. The *PKULaw* database classifies the policies into 9 domains. The "regional planning" domain accounts for the largest share of policies mentioning "urban population." It includes: (i) the central government's approvals of specific regions' development plans, and (ii) the central government's own development plans for particular regions. In these policies, "urban population" or synonyms are mentioned to describe the population status of a region, rather than systematically specifying provisions tiered by urban population. For example, in the State Council's approval of the Fuzhou Prefecture's urban development plan in 2015,[3] it was stated:

> "By 2020, the permanent population in the central urban area will be controlled within 4.1 million people, and the urban construction land will be controlled within 378 square kilometers."

The 2014 *hukou* reform falls in the domain of "government work." The other policy in 2014 in the "government tasks" domain was a general proposal regarding the development of regions near the Yangtze River.[4] The policy of this domain in 2017 was to divide tasks between branches within the central government.[5] Both policies did not outline provisions that vary by urban population.

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[3] `https://www.gov.cn/gongbao/content/2015/content_2912363.htm`

[4] State Council's Notice on Relying on Golden Waterways Guidance on promoting the development of the Yangtze River Economic Belt (`https://www.gov.cn/zhengce/content/2014-09/25/content_9092.htm`).

[5] State Council's Opinions on the Implementation of the Government Work Report: The Division of labor among departments (`https://www.gov.cn/zhengce/content/2017-03/28/content_5181530.htm`).

The policy of the "labor unions" domain may be worth noting. However, it called labor unions to facilitate increasing employment and had no provisions based on urban population tiers.[6] Thus, again, it again would not be a confounding policy for the purpose of our paper.

Among all these policies during the period under study, we identify five policies as population-based policies, which are listed as follows:

1. **Opinions of the State Council on Further Promoting the Reform of the Household Registration System in 2014.**[7] The detail of this policy is described in Section 2.1.

2. **Opinions of the State Council on Promoting the Development of Prefabricated Construction in 2016.**[8] The document states that cities with population over 3 million are regions for active promotion of the prefabricated construction development, while other cities are considered regions for encouraged promotion.

3. **Notice of the State Council on Issuing the Ecological and Environmental Protection Plan for the 13th Five-Year Plan (2016).**[9] "By 2020, public transportation in built-up areas of cities with a permanent population of more than 3 million will account for 60% of motorized travel."

4. **Notice of the State Council on Issuing the 13th Five-Year Plan for the Development of a Modern Comprehensive Transportation System in 2017.**[10] This document states that by 2020 high-speed rail should cover more than 80% of the cities with an urban population over 1 million, while railways, highways, and civil aviation airports should basically cover cities with urban population over 0.2 million.

5. **Opinions of the State Council on Further Strengthening the Planning and Management of Urban Rail Transit Construction in 2018.**[11] This document makes the strict requirement that cities applying to build a metro system should generally have a public fiscal budget of more than 30 billion yuan, a regional GDP of over 300 billion yuan, and an urban population of over 3 million.

6. **Opinions of the State Council on Promoting the Improvement and Expansion of the Domestic Service Industry in 2019.**[12] The document brings that cities with urban population over 1 million should achieve full coverage of domestic service training capabilities by 2022.

Aside from the *hukou* policy, only the 2016 policy on the development of prefabricated construction uses the 3 million population threshold. Since this policy focuses on altering the construction process by separating material production from assembly, it is not very concerning to our analysis

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[6]Notice of the State Council on Issuing the Employment Promotion Plan for the 13th Five-Year Plan (https://www.gov.cn/zhengce/content/2017-02/06/content_5165797.htm).

[7]https://www.gov.cn/zhengce/content/2014-07/30/content_8944.htm.

[8]https://www.gov.cn/zhengce/content/2016-09/30/content_5114118.htm.

[9]https://www.gov.cn/zhengce/content/2016-12/05/content_5143290.htm

[10]https://www.gov.cn/zhengce/content/2017-02/28/content_5171345.htm.

[11]https://www.gov.cn/zhengce/content/2018-07/13/content_5306202.htm.

[12]https://www.gov.cn/zhengce/content/2019-06/26/content_5403340.htm.

of the *hukou* reform. The domains of other policies are also not likely to have first-order impacts on labor unrest.

Results of Searches Based on "City Size." Table A10 tabulates the number of policies containing the keyword "city size" or terms with similar meanings by year and policy domain. Similar to the results using keyword "urban population," most policies containing the keyword "city size" or its synonyms fall into the "regional planning" domain, where the word "city size" is used to describe the city status. Among all the other policies, we identify one policy with provisions tiered by urban population.

1. **Opinions of the State Council on Accelerating the Promotion of Ecological Civilization Construction in 2015.**[13] The document proposes to base urban planning on the carrying capacity of resources and the environment, strictly control the size of mega-cities, and enhance the capacity of small and medium-sized cities.

Due to its environmental focus, we do not believe it can have a first-order impact on labor unrest.

Table A9. Search Results Using Keyword *"Urban Population"*

Year	Regional Planning	Government Tasks	Resources & Environment	Labor Union	Transport	Science & Education	Health	Others	Total
2011	15	0	1	0	0	0	0	1	17
2012	28	0	0	0	0	0	0	4	32
2013	7	0	1	0	0	0	1	3	12
2014	4	2	0	0	0	0	0	2	7
2015	7	0	1	0	0	0	0	1	10
2016	10	0	2	0	0	1	1	6	20
2017	16	1	1	1	1	1	0	1	22
2018	3	0	0	0	0	0	0	1	4
2019	2	0	0	0	0	0	0	0	2

Note: This table summarizes the count of policies containing the keyword "urban population" or terms with similar meanings by year and policy domain.

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[13] https://www.gov.cn/gongbao/content/2015/content_2864050.htm.

Table A10. Search Results Using Keyword *"City Size"*

Year	Regional Planning	Resources Agriculture	Planning	Standardized Management	Others	Total
2011	7	0	0	0	0	7
2012	1	1	0	0	0	2
2013	2	0	0	0	0	2
2014	2	0	2	1	0	5
2015	7	0	2	0	0	9
2016	12	1	1	0	0	14
2017	15	0	1	0	0	16
2018	2	0	0	0	0	2
2019	0	0	0	0	0	0

Note: This table summarizes the count of policies containing the keyword "city size" or terms with similar meanings by year and policy domain.

G Ancillary Results

G.1 Sensitivity Test for Potential Violations of Local Parallel Trends

This section follows Fenizia and Saggio (2024) and Rambachan and Roth (2023) to address potential concerns about violations of *local* parallel trends (for prefectures around the reform cutoff). We do this for results from both full and narrow samples.

First, we can fit a linear trend based on the pre-reform event study estimates and extrapolate it to the post-reform periods, as shown by the first column of Figure A9. Apparently, the pretrends are slightly upward. If these trends persist to the post-reform periods, the decline in unrest rates indicated by the post-reform event study estimates would in fact *underestimate* the true effects. We can correct this bias for these estimates by calculating their deviations from the extrapolated linear trend. The middle column of Figure A9 reports these detrended event study results. They confirm that the *hukou* reform has significantly negative effects on unrest rates.

In the last column of Figure A9, we use the methodology developed by Rambachan and Roth (2023) to evaluate the sensitivity of our results to violations of local parallel trends. This approach allows more nonlinear differential trends. Specifically, it imposes the following condition to the change in the slope of the differential trend between reform and non-reform prefectures between two consecutive periods:

$$|(\theta_{t+1} - \theta_t) - (\theta_t - \theta_{t-1})| \le M. \tag{A2}$$

θ_t is the slope of the differential trend in period t. M governs the range of slope changes, namely, the degree of non-linearity of the differential trend. $M = 0$ corresponds to a linear differential trend. A larger M thus allows a more nonlinear differential trend. For every given M, the method then tests the null, conditional on the possible differential trend, whether the *hukou* reform has a significant effect on unrest — defined as the average of post-reform event study coefficients. Figure A9 shows that our results can withstand very nonlinear differential trends. For instance, consider Figure A9C that reports the sensitivity test for the event study using the full sample, we can reject a null effect up to when M is 0.03. Compared to the slope of the linear trend implied by pre-reform event study coefficients, 0.076, this means that the differential trend's slope must change by more than $\pm \frac{0.03}{0.076} \approx \pm 39.4\%$ of the slope the linear extrapolation in each period. In other words, only when a very wiggly differential trend is imposed should we not reject a null effect. Likewise, the narrow sample results can also tolerate a high degree of non-linearity in differential trends. Figure A9F suggests that we cannot reject a null effect only if we are willing to assume that the differential trend's slope is more than $\pm \frac{0.06}{0.311} \approx \pm 19.3\%$ off the linear pretrend slope.

Taken together, we show our results are robust to allowing a linear differential trend implied by the pretrends. In addition, the results hold even if there is a significant amount of non-linearity in differential trends.
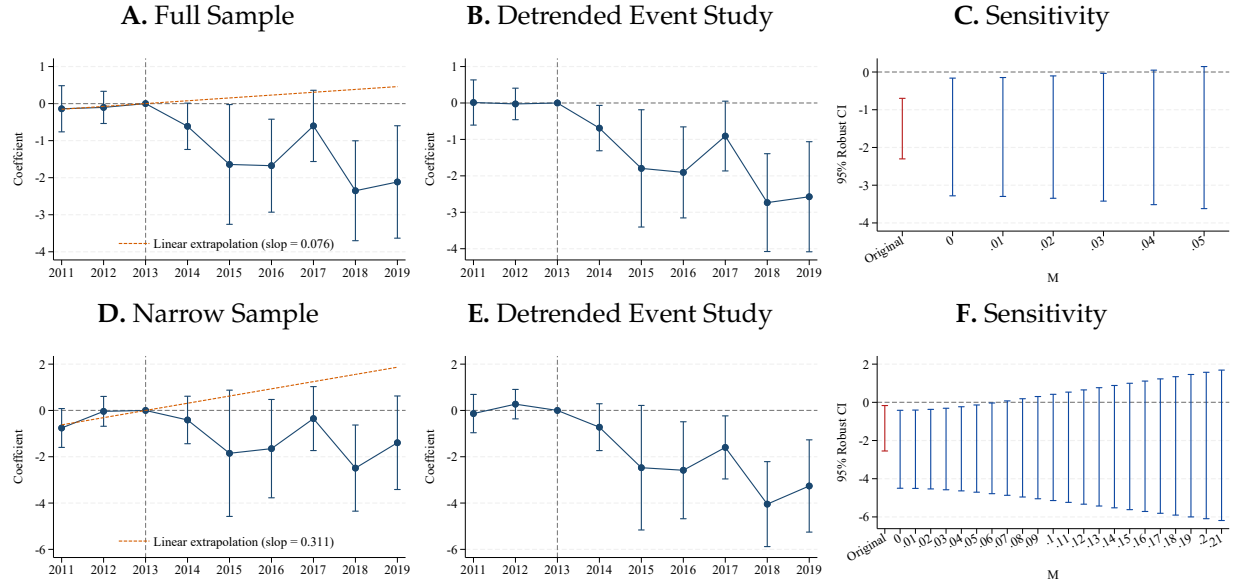
A. Full Sample   **B.** Detrended Event Study   **C.** Sensitivity

D. Narrow Sample   **E.** Detrended Event Study   **F.** Sensitivity

Figure A9. Detrended Event-Study Coefficients and Application of Rambachan and Roth (2023)

Note: This figure reports the sensitivity test for the event study results from both full and narrow samples. The first column depicts the linear trend implied by pre-reform event study coefficients and it is extrapolated to post-reform periods. The middle column shows the deviations of event study coefficients from the linear trend. The last column reports the results of applying the methodology developed by Rambachan and Roth (2023), which tests whether the average of post-reform coefficients are statistically distinguishable from zero given different levels of non-linearity in potential differential trends (governed by M).

G.2 Robustness to Choices of Bandwidths, Kernels, and Polynomial Orders

Bandwidths. Figure A10 presents the estimated coefficient on $Reform_i \times Post_t$ in Equation 2, when we impose different bandwidth restrictions to our sample. We note three commonly used optimal bandwidths proposed in the literature: Calonico et al. (2014) a.k.a. CCT, Imbens and Kalyanaraman (2012) a.k.a. IK, and cross validation a.k.a. CV. The estimates are overall stable across different bandwidth choices. Note that when the bandwidth falls bellow the IK optimal bandwidth, the estimates become much noisier due to the small sample size. Thus, we opt to use the IK optimal bandwidth for our narrow sample.

Kernels. Our baseline results place equal weights on prefectures, i.e., the uniform kernel is used. We examine the robustness to choices of kernels. We consider two alternative kernels common in the literature: triangular and Epanechnikov kernels, which place high weights on prefectures closer to the cutoff $\tilde{p}_i = 0$. Using these two kernels, we then re-estimate Equation 2. Figure A11 shows that using alternative kernels yields similar estimates of β in Equation 2 as in the baseline.

Polynomial Orders. Figure A12 presents event-study results for the effects of the *hukou* reform on labor unrest using quadratic and cubic polynomials of $\tilde{p}_i$. Compared the the baseline results that use linear polynomials in Figure 2B, these results are nosier, likely due to over-fitting with the
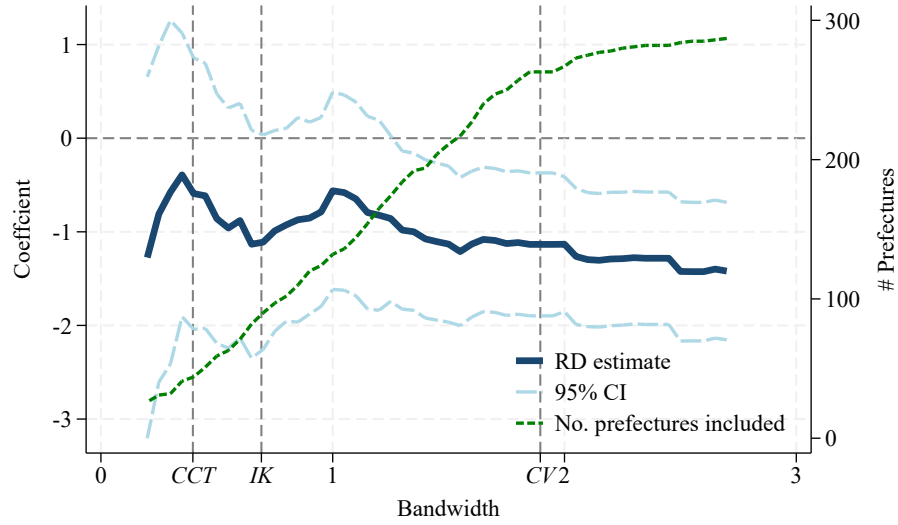
Figure A10. Estimates under Different Bandwidth Choices

Note: This figure presents the estimated coefficient on $Reform_i \times Post_t$ in Equation 2, when we impose different bandwidth restrictions to our sample. We note three commonly used optimal bandwidths proposed in the literature: Calonico et al. (2014) a.k.a. CCT, Imbens and Kalyanaraman (2012) a.k.a. IK, and cross validation a.k.a. CV. Standard errors clustered at the prefecture level are used to construct the 95 percent confidence intervals.
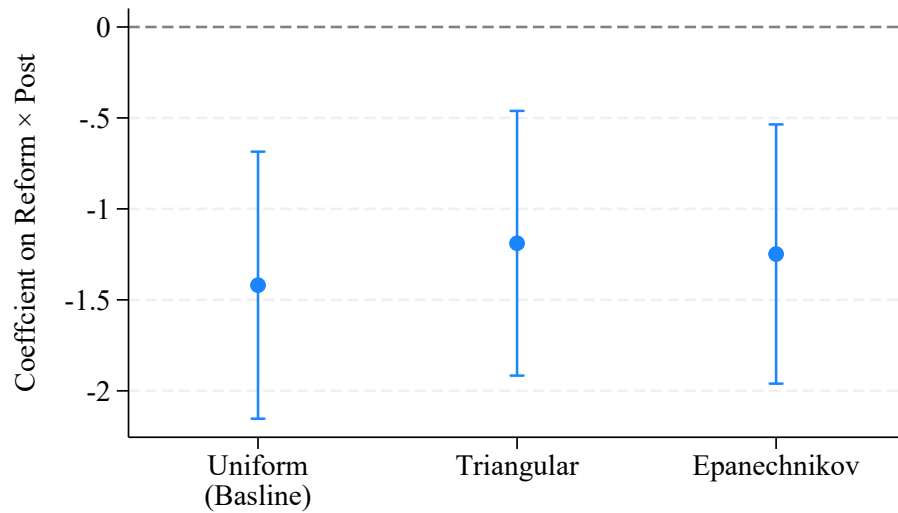

Figure A11. Robustness to Kernel Choices

Note: This table presents the robustness to kernel choices. We estimate Equation 2 using three kernels: uniform kernel (baseline), triangular kernel, and Epanechnikov kernel. The solid dots denote the point estimates of β in Equation 2. The caps are 95 percent confidence intervals. Standard errors are clustered at the prefecture level.

introduction of higher-order polynomials Gelman and Imbens (2019). Nonetheless, it is clear that after the *hukou* reform became in effect, labor unrest rates in reform prefectures decreased relatively.

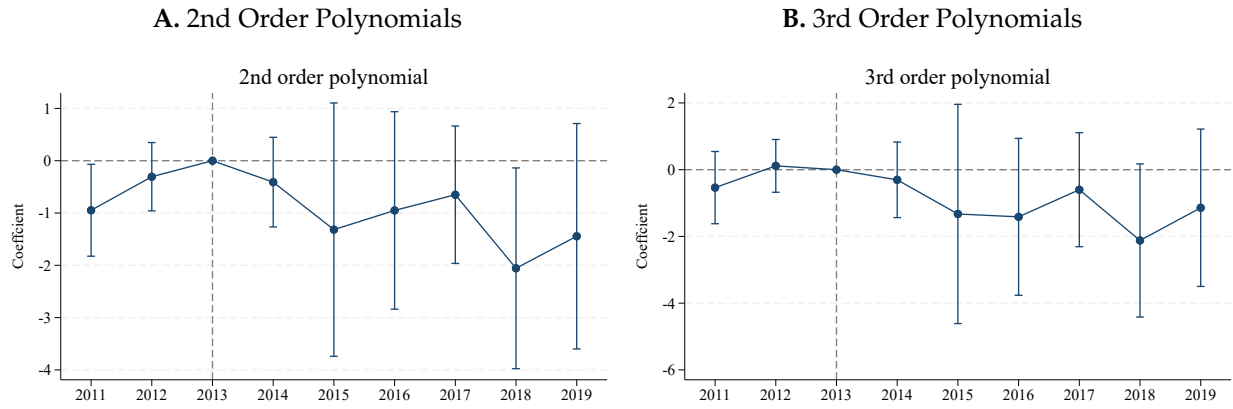| **A.** 2nd Order Polynomials | **B.** 3rd Order Polynomials |

Figure A12. Robustness to Alternative Polynomial Orders

Note: This figure presents the event study results using polynomial orders. The solid dots are point estimates, and the caps are 95 percent confidence intervals. Standard errors clustered at the prefecture level are used to construct confidence intervals.

G.3 Reporting of Local Events

General Coverage of Local Events. Given that CLB data rely on online reports about labor unrest, we investigate if the *hukou* reform affects reporting of local events so that we observe a decrease in unrest rates in reform prefectures. We make use of the Global Database of Events, Language, and Tone Project (GDELT). It records events based on articles from a comprehensive, global set of news feeds, and it also uses automated textual analysis to extract characteristics of recorded events, such as date, location, type of the event, parties involved, etc. Thus, we use the number of events recorded by GDELT (scaled by working-age population) as a measure of coverage of local events, the variation of which can be due to either media attention or information outflows. Column (1) of Table A11 shows that there are no significant differential trends in the number of events reported between reform and non-reform prefectures. If anything, reform prefectures experienced an increase in coverage. Column (2) looks at the reporting of protest events.[14] Likewise, we do not find the reporting of protests varies significantly by reform status.

Internet Censorship. Internet censorship was rising during the period we study (King et al., 2017). One may be concerned that reform prefectures' decrease in unrest rates is an artifact of changes in reporting of local events due to censorship. If so, one may expect that the decrease concentrates in reform prefectures where information tends to be censored to a greater extent. We test this hypothesis by examining the *hukou* reform's heterogeneous effect by censorship. To measure censorship, we use data from Qin et al. (2017), who measure the level of censorship using the share of deleted posts on Weibo at the provincial level. We create a dummy variable for high,

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[14]The type of an event is identified by the Conflict and Mediation Event Observations (CAMEO) code using machine learning. Column (2) restricts analysis to events with CAMEO code "14: Protest", which includes a range of protest activities including demonstrations, rallies, strikes, and violent protests.

above-median levels of censorship. Column (3) shows that there is no strong heterogeneity by censorship in the reform's effect on labor unrest.

Self-Censorship. If workers in reform prefectures became less likely to report their actions online than their counterparts in non-reform prefectures after the reform, then we would observe a mechanical decrease in labor unrest rates in reform prefectures. To examine the role of self-censorship, we consider the most influential scenario for self-censorship to drive our results. When post-reform self-censorship rose primarily in reform prefectures that reported high unrest rates before the reform, we are more likely to obtain an artificial, negative effect of the *hukou* reform on labor unrest rates. If the dynamics of self-censorship play a major role in our findings, then excluding reform prefectures with reported high pre-reform unrest rates would attenuate the estimated effect of the *hukou* reform on labor unrest.

In light of this idea, within reform prefectures, we exclude reform prefectures with high pre-reform unrest rates in the top X-th percentile (X is varied), and re-estimate Equation 2 using the sample with some reform prefectures excluded (depending on X). However, as shown by Figure A13, the estimated effect remains stable when excluding reform prefectures with reported high pre-reform unrest rates, indicating a limited role of self-censorship in explaining our results.

In conclusion, the negative effect of the *hukou* reform on labor unrest is not likely due to variation in reporting of local events.

Table A11. *Hukou* Reform and Reporting of Local Events

	(1) All events/L GDELT	(2) Protests/L GDELT	(3) Unrest/L CLB
Reform × Post	527.355	6.523	-1.323***
	(522.334)	(4.992)	(0.372)
Control mean	1108.759	9.919	3.416
Prefecture FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Polynomials	Yes	Yes	Yes
Censorship level × Year FE			Yes
Observations	2,583	2,583	2,574

Note: The dependent variables in Columns (1) and (2) are the number of local events and the number of protests recorded in GDELT (scaled by working-age population). The dependent variable in Column (3) is the number of labor unrest events in CLB data with the same scaling. "High level of censorship" is a dummy variable that equals one if a prefecture's affiliated province has an above-median share of deleted Weibo posts, based on data from Qin et al. (2017). Robust standard errors are clustered at the prefecture level and reported in parentheses. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$
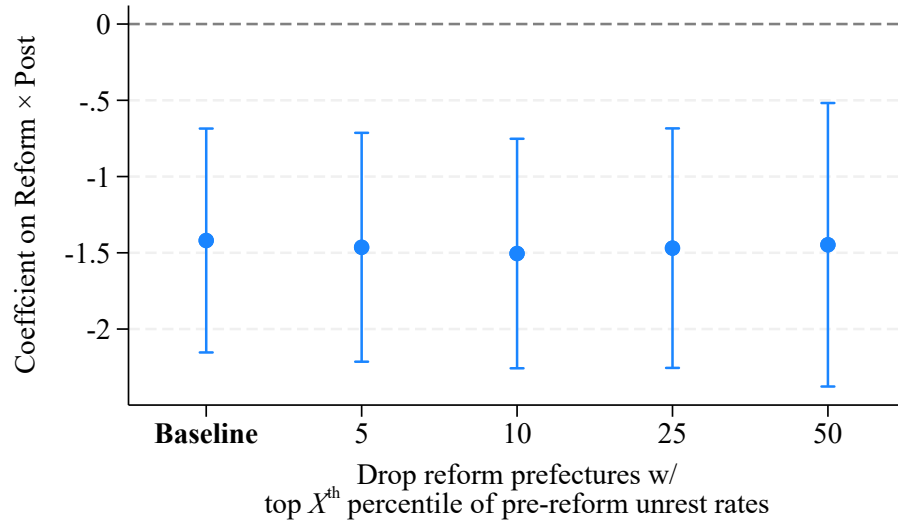
A.26

Figure A13. Addressing the Alternative Interpretation of Increased Self-Censorship

Note: This table examines to what extent the results are driven by differential trends in self-censorship. Within reform prefectures, we exclude those with high *pre-reform* unrest rates in the top *X*-th percentile (*X* is varied). When increased self-censorship occurs in these prefectures, we are more likely to obtain a negative effect of the *hukou* reform on labor unrest rates (as recorded by the CLB). We estimate Equation 2 using the sample with some reform prefectures excluded (depending on *X*). The solid dots in the figure are the point estimates of β. The caps are 95 percent confidence intervals. Standard errors are clustered at the prefecture level.

G.4 The Effects of the *Hukou* Reform on Population

In this section, we investigate the effects of the *hukou* reform on total population, urban population, and native urban population (urban residents with local *hukou*). We study effects on both levels and growth rates. Figure A14 reports event study estimates using the DiDC strategy. For completeness, we also report estimates using the DiD strategy. Regarding total population, the DiD estimates in Figure A14A show that reform prefectures exhibit a downward linear trend in total population (in log), and the growth rate does not vary significantly over time (see Figure A14B); by flexibly controlling for heterogeneity due to urban sizes, the DiDC estimates show that despite moderate pretrends, there appears to be a relative decline in total population after the reform starts, which is due to a drop in population growth rate. These results indicate the *hukou* reform has a null effect or possibly a negative effect on a prefecture's total population. When it comes urban population and native urban population, both DiD and DiDC estimates indicate null effects.

In sum, these results suggest that the *hukou* reform has no discernible effects on both total and urban population. If anything, there may be a negative effect on total population.
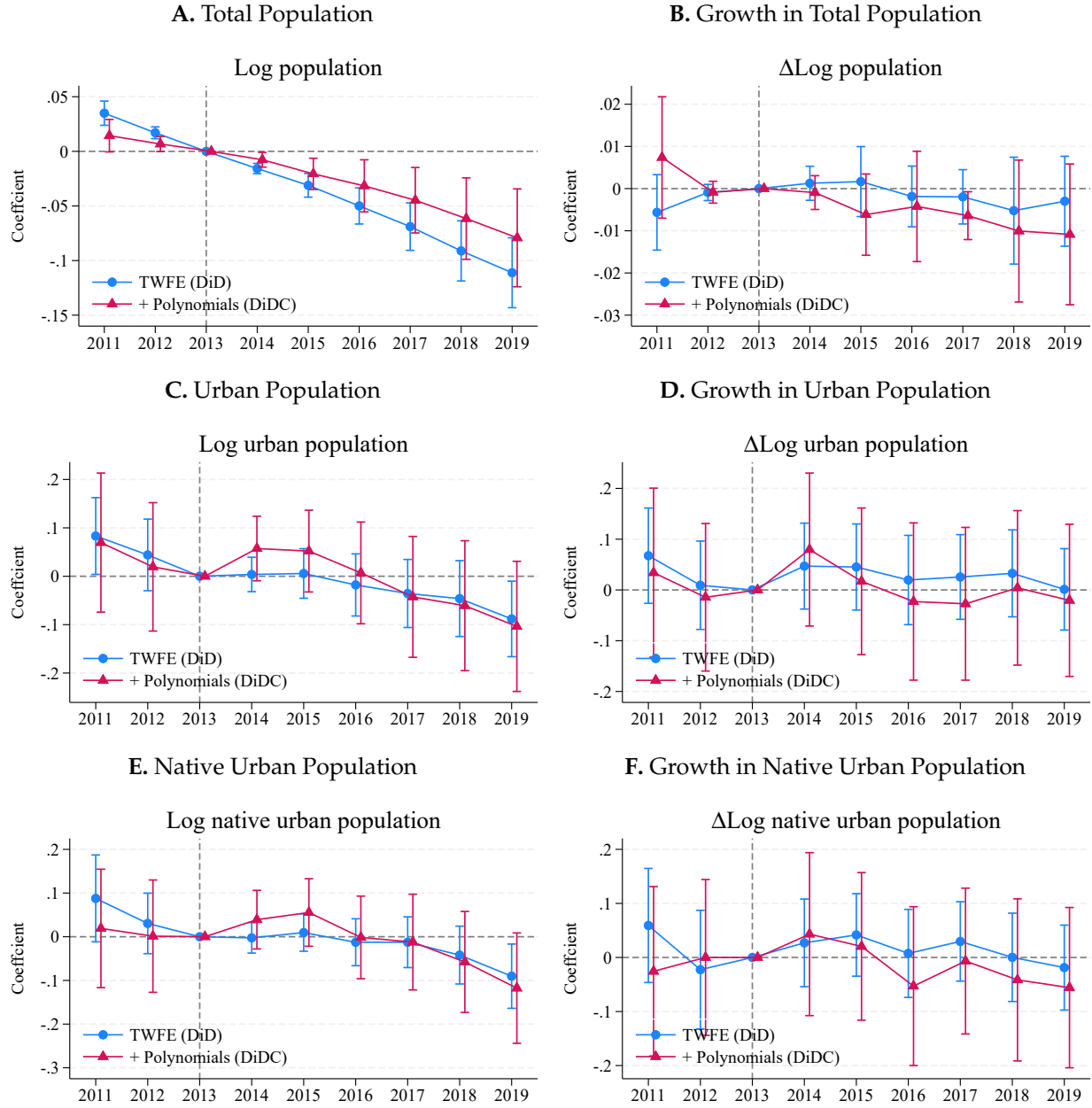
Figure A14. Dynamic Effects of the *Hukou* Reform on Population

Note: This figure reports the dynamic effects of the *hukou* reform on total population, urban population, and native urban population (urban residents with local *hukou*). We look at both their levels (in log points) and growth rates. We estimate event study models modified respectively from Equations 1 and 2. Th solid dots are point estimates, and the caps are 95 percent confidence intervals. Standard errors clustered at the prefecture level are used for constructing confidence intervals.

G.5 Addressing Time-Varying Prefecture Sizes

For our main results reported in Section 4, we scale the number of labor unrest events using working-age population, measured in the population census of 2010. One concern is that the results are simply due to time varying prefectures sizes rather than changes in underlying engagement of

unrest. We show that our results hold even if we use time-varying population for scaling. There are no annual data on working population. Instead, we use time-varying total population and urban population, sourced from Rogoff and Yang (2024) and the Urban Construction Statistical Yearbooks, respectively. Figure A15 reports the results, confirming our findings that the *hukou* reform leads to a significant decrease in unrest rates.
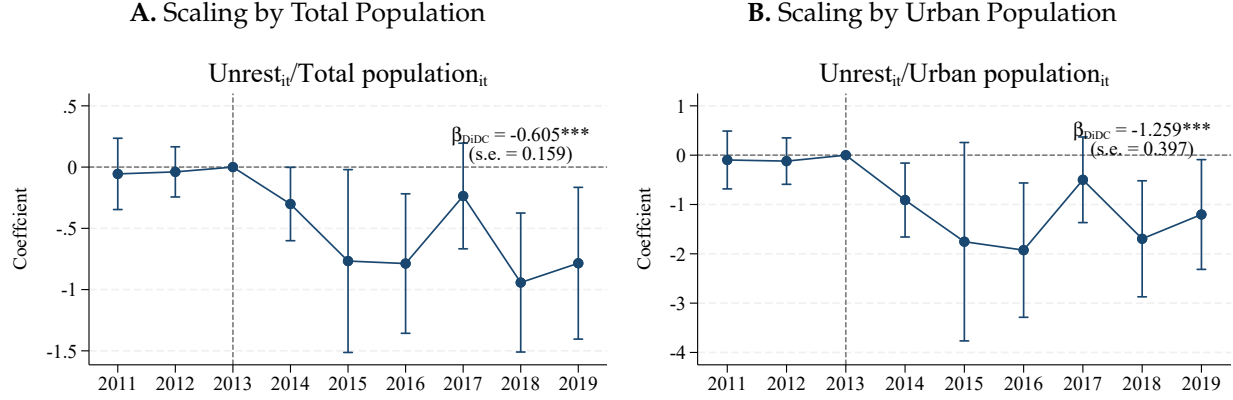
A. Scaling by Total Population   **B.** Scaling by Urban Population

Figure A15. Scaling Unrest Events Using Time-Varying Prefecture Size

Note: This figure reports the results when the number of unrest events is scaled by time-varying prefecture size. Figure A15A uses total population, and Figure A15B uses urban population. We visualize estimates from a dynamic specification: $Y_{it} = \sum_{s \neq 2013} \beta_s (Reform_i \times D_s) + f [\Delta \log (P_{i,2014}); \zeta_{Reform,t}] + \lambda_i + \mu_t + \varepsilon_{it}$. The solid points are points estimates of β_s's, and the caps are 95 percent confidence intervals. We also report the estimate from a static specification: $Y_{it} = \beta (Reform_i \times Post_t) + f [\Delta \log (P_{i,2014}); \zeta_{Reform,t}] + \lambda_i + \mu_t + \varepsilon_{it}$. All standard errors are clustered at the prefecture level. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

G.6   The Effect of the *Hukou* Reform on Outmigration: Cox Hazard Model

For robustness, we estimate a Cox proportional hazard model that is commonly used in survival analysis:

$$h (Outmigration_{jkt} \mid t, X) = h_0(t) \exp [\rho_1 (Reform_k \times Post15_t) + \rho_2 Reform_k + \rho_3 Post15_t]. \quad \text{(A3)}$$

$h (Outmigration_{jkt} \mid t, X)$ is the hazard rate of outmigration as of year t, conditional on a vector of explanatory variables, X, that one can see from the right-hand side of Equation A3. $h_0(t)$ is a common function of the time-at-risk. Following the semiparametric approach devised by Cox (1972), we leave the baseline hazard function $h_0(t)$ unrestricted and estimate the other coefficients by partial maximum likelihood. This way we take advantage of the tractability of the proportional hazard model, while allowing at the same time for significant flexibility in terms of functional form. Standard errors are clustered at the prefecture level.

Table A12 presents the results using the Cox model, which a provide consistent picture as in Table 5. The *hukou* reform reduced the hazard ratio of outmigration — according to Column (3) — by 58.2 percent. This effect is more pronounced among more patient, forward-looking migrants,

although the heterogeneous effects in Columns (4) and (5) are less precise (p-value = 0.139 and 0.104, respectively).

Table A12. Effect of *Hukou* Reform on Outmigration Rate: Cox Hazard Model

	Outmigration from 2010 residence				
	(1)	(2)	(3)	(4)	(5)
Reform × Post	-1.058***	-0.863***	-0.872***	-0.973***	-0.999***
	(0.143)	(0.244)	(0.240)	(0.258)	(0.269)
Reform × Post × Patience (std.)				-0.232	-0.252
				(0.156)	(0.155)
Reform × Post × Risk taking (std.)					0.150
					(0.127)
Reform × Post × High school completion					0.064
					(0.166)
Mfx. on hazard rate	-0.653	-0.578	-0.582	-0.622	-0.632
Polynomials		Yes	Yes	Yes	Yes
Stratified hazard function			Yes	Yes	Yes
Observations	58,701	58,701	58,701	51,769	51,769

Note: This table reports the effect of *hukou* reform on outmigration rate. When applicable, the hazard function is stratified by birth cohort, gender, educational attainment, and employment status. Standard errors clustered at the prefecture level are reported in parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

H   Additional Robustness Checks

H.1   Alternative Specifications and Estimators

We show that our results are robust to using alternative specifications and estimators. First, instead of the linear polynomial function, we use quadratic and cubic polynomial functions. Second, we use alternative forms of unrest rate $\frac{Unrest_{it}}{L_{i,2010}}$. We take the log of unrest rate (plus one) or use the inverse hyperbolic sine (IHS) transformation. Third, due to the count data nature, we use the pseudo Poisson maximum likelihood estimation. Fourth, we estimate a spatial autoregressive model to take into account potential spatial spillovers. Lastly, we implement the synthetic difference-in-differences (SDID) developed by Arkhangelsky et al. (2021).

The results using these methods are reported in Table A13. We also report the event study estimates in Figure A16. All results confirm that the *hukou* reform significantly reduces unrest rates.

Table A13. Robustness: Alternative Specifications and Estimators

	Alt. Unrest Measures		PPML	SAR	SDID
	(1)	(2)	(3)	(4)	(5)
	Log(Unrest/L)	IHS(Unrest/L)	Unrest/L	Unrest/L	Unrest/L
Reform × Post	-0.305***	-0.383***	-0.545*	-1.456***	-1.604***
	(0.086)	(0.112)	(0.308)	(0.363)	(0.258)
Control mean	1.209	1.549	3.395	3.395	3.395
Method	OLS	OLS	PPML	SAR	SDID
Prefecture FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Polynomials	Yes	Yes	Yes	Yes	Yes
Observations	2,583	2,583	2,583	2,583	2,583

Note: This table examines the robustness to alternative specifications and estimators. Columns (1) and (2) control for quadratic and cubic polynomial functions, respectively. Column (3) takes the log of unrest rate (plus one). Column (4) takes the inverse hyperbolic sine (IHS) transformation. Column (5) implements pseudo Poisson maximum likelihood (PPML) estimation. Column (6) estimates a spatial autoregressive (SAR) model. Column (7) uses the the synthetic difference-in-differences (SDID) developed by Arkhangelsky et al. (2021). Standard errors are clustered at the prefecture level and reported in parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

H.2   Addressing Potential Outliers

To investigate if there are any special regional factors driving our results, we exclude one province each time and re-estimate Equation 2. Figure A17 shows the estimated coefficients on *Reform_i* × *Post_t*. Compared to the baseline estimate using the entire sample, dropping any province has no marked influence. We consistently find a negative effect of the *hukou* reform on unrest rates.
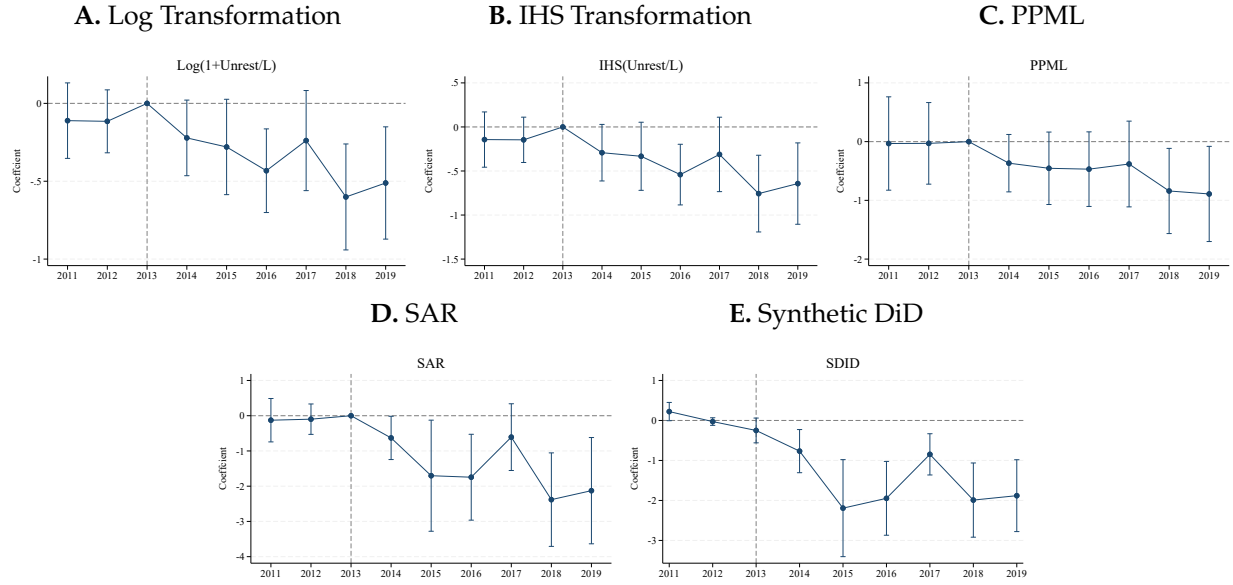
A. Log Transformation   **B.** IHS Transformation   **C.** PPML

D. SAR   **E.** Synthetic DiD

Figure A16. Robustness: Alternative Specifications and Estimators, Event-Study Results

Note: This figure presents the event study results using alternative specifications and estimators. The solid dots are point estimates, and the caps are 95 percent confidence intervals. Standard errors clustered at the prefecture level are used to construct confidence intervals.
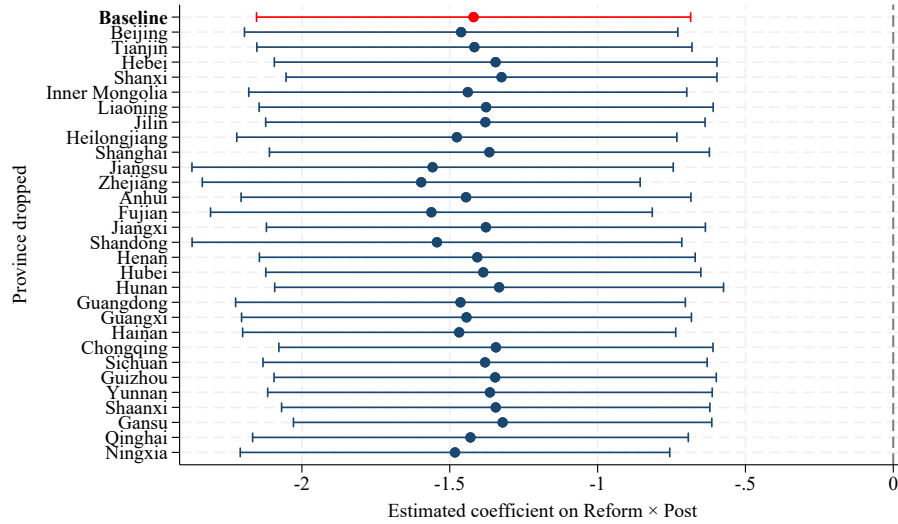

Figure A17. Robustness: Dropping One Province Each Time

Note: This figure reports the estimated coefficient on $Reform_i \times Post_t$ from Equation 2, using the enter sample less one province. For comparison, we also present the baseline estimated coefficient using the entire sample (in red). The solid points are point estimates, and the caps are 95 percent confidence intervals. Standard errors are clustered at the prefecture level.

In Table A14, we show our results are not driven by other potential outlier observations. In Column (1), we exclude prefectures that never had unrest events recorded in the CLB data. However, there was only one such prefecture. In Column (2), in the spirit of donut hole RD that aims to address likely sorting around the cutoff, we remove prefectures that have urban population

size very close to the reform cutoff, specifically, $|\tilde{p}_i| < 0.2$. In Column (3), we exclude prefectures whose population-based reform status does not agree with manually coded reform status, for the concern that these prefectures do not "comply" due to unrest considerations. But recall from Section 3.1, we only have 17 prefectures with such discrepancies. Our results survive these exercises, and become even stronger in some cases.

In Column (4), we follow Hansen (2022, pp. 84–86) to calculate an index for each prefecture's influence the overall fitting of data. The index is calculated as follows. We estimate a cross-sectional RD regression that is numerically equivalent to the panel regression, Equation 2:

$$Y_i \equiv \frac{\sum_{t=2014}^{2019} \frac{Unrest_{it}}{L_{i,2010}}}{6} - \frac{\sum_{t=2011}^{2013} \frac{Unrest_{it}}{L_{i,2010}}}{3} = Reform_i + f\left(\tilde{p}_i; \zeta_{Reform}\right) + \varepsilon_i. \tag{A4}$$

Then, the influence index is calculated as $d_i = \hat{Y}_i - \tilde{Y}_i$, where $\hat{Y}_i$ is the predicted value based on a full-sample regression, while $\tilde{Y}_i$ is the predicted value based on the leave-prefecture i-out regression. A high $|d_i|$ implies that prefecture i is an influential observation for the overall fitting of data. Thus, we drop prefectures with a high d_i: for reform prefectures we drop the top 25 percent, and for non-reform prefectures, we drop the top 10 percent. As shown by Column (4) of Table A14, this in facts accentuates our finding.

Table A14. Robustness: Addressing Potential Outliers

	(1) Never having unrest recorded	(2) Near the cutoff	(3) w/ a diff. btw. population-based & manually-coded reforms	(4) w/ high influence
Reform × Post	-1.401***	-1.624***	-1.834***	-1.631***
	(-3.76)	(-4.62)	(-4.36)	(-7.29)
Control mean	3.395	3.750	3.769	4.053
Prefecture FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Polynomials	Yes	Yes	Yes	Yes
Observations	2,574	2,349	2,430	2,007

Note: This table checks the robustness of our results to potential outliers. Column (1) excludes prefectures that never had unrest events recorded in the CLB data. Column (2) removes prefectures that have urban population size very close to the reform cutoff, specifically, $|\tilde{p}_i| < 0.2$. Column (3) excludes prefectures whose population-based reform status does not agree with manually coded reform status. Column (4) follows Hansen (2022) to exclude prefectures that have a high influence on the overall fitting of data. Standard errors are clustered at the prefecture level and reported in parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

H.3 Balancing of Baseline Characteristics

In Section 4.1, we note that there remains some difference in baseline covariates between reform and non-reform prefectures, despite the inclusion of polynomial controls. Such imbalances could threaten our results they are associated with differential trends in unrest. To address this issue, in this section, we use several strategies to balance the baseline covariates and explore the robustness of our results.

Table A15 reports our investigation. For comparison, Column (1) re-estimates Equation 2 using the sample that we have all data for baseline covariates, which is slightly smaller than the full sample. Column (2) directly controls for interactions between baseline covariates and year indicators in the model. Column (3) weights observations to the propensity score predicted by the baseline covariates. Column (4) implements the coarsened exact matching (CEM) proposed by Iacus et al. (2012), which weights observations such that reform and non-reform prefectures have the same distributions of baseline covariates (we target tertiles). All approaches consistently show a negative effect of the *hukou* reform on unrest rates.

Table A15. Robustness: Covariates Balancing

	(1)	(2)	(3)	(4)
	Unrest/L	Unrest/L	Unrest/L	Unrest/L
Reform × Post	-1.439***	-1.024***	-1.191*	-1.446***
	(0.376)	(0.359)	(0.686)	(0.492)
Balancing approach	-	Controls added	P-score	CEM
Control mean	3.395	3.395	2.954	3.260
Prefecture FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Polynomials	Yes	Yes	Yes	Yes
Observations	2511	2511	1764	2484

This table presents multiple approaches to control for baseline covariates, including share of male, share of urban residents, share of migrants, share of secondary sector workers, share of tertiary sector workers, and share of internet users. Column (1) presents the baseline result for the ease of comparison. Column (2) directly controls for these covariates interacted with year indicators in the regression. Column (3) weights observations to balance the propensity score predicted by covariates. Column (4) implements the coarsened exact matching (CEM, Iacus et al., 2012) to balance distributions of covariates. Standard errors are clustered at the prefecture level and reproted in parentheses.

Note: * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

H.4 Balance Tests for Trade Shocks

We discuss if the trade shocks measured in Equation 5 can be viewed as quasi-exogenous triggers of labor unrest. Recall that the trade shock is constructed as a shift-share variable:

$$TradeShock_{it} = \sum_k \underbrace{\frac{X^{CN}_{ik,2010}}{L_{i,2010}}}_{\text{share}} \times \underbrace{\frac{\Delta X^{ROW}_{kt}}{X^{CN}_{k,2010}}}_{\text{shift}} \equiv \sum_k s_{ik} g_{kt}, \tag{A5}$$

where $s_{ik} = \frac{X^{CN}_{ik,2010}}{L_{i,2010}}$ and $g_{kt} = \frac{\Delta X^{ROW}_{kt}}{X^{CN}_{k,2010}}$. The "shift" component, g_{kt}, is the growth in global demand for product k in year t, *relative* to industry size of product k in China in 2010. It is apportioned to prefectures by the "share" term, s_{ik}, which reflects the initial specialization of prefecture i.

Borusyak et al. (2022) show that the quasi-exogeneity of $TradeShock_{it}$ can follow from the quasi-exogeneity of the shift, g_{kt}. Intuitively, this condition is violated if products with high g_{kt} are systematically produced in regions with differential dynamics of labor unrest. To test the quasi-exogeneity of g_{kt}, we conduct balance tests following (Borusyak et al., 2022). Specifically, we estimate the following product-level regression model:

$$q_k = \beta_0 + \beta_1 g_{kt} + \mu_t + \varepsilon_k. \tag{A6}$$

$q_k = \sum_i \frac{s_{ik}}{\sum_i s_{ik}} v_i$. v_i is a covariate of prefecture i, thus, q_k captures the s_{ik}-weighted average covariate of prefectures that produce product k. μ_t is the year fixed effect. Therefore, Equation A6 stacks all years to estimate the associations between trade shocks and predetermined covariates. The regression is weighted by $\sum_i s_{ik}$. Standard errors are clustered at the 2-digit HS section level to account for correlated disturbances of products within the same sector. The coefficient of interest is β_1. If β_1 is close to zero, it suggests that g_{kt} is not distributed is a way that may relate to differential trends of labor unrest.

Table A16 reports the results for balance tests. Covariates considered here are the same as those in Table 2 when we conduct the balance test for reform status $Reform_i$.[15] In Panel A, we consider trends in variables that may influence labor unrest before the start of our sample. In Panel B, we consider some baseline characteristics. We see all estimates of β_1 are statistically insignificant, supporting that $TradeShock_{it}$ can be viewed as quasi-exogenous triggers of labor unrest.

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[15]However, we are unable to examine the pretrend in labor unrest, because the starting year of CLB is 2011.

Table A16. Balance Tests for Product-Level Trade Shocks

Dependent	Coef.	SE
Panel A: Pretrends		
ΔLog population, 2009–2010	-0.231	(0.213)
ΔLog GDP, 2009–2010	1.117	(1.359)
ΔLog expenditure, 2009–2010	0.140	(0.403)
ΔLog expenditure on public security, 2009–2010	0.515	(0.404)
Panel B: Predetermined characteristics		
Share of migrants, 2010	-2.951	(2.725)
Share of urban residents, 2010	-1.360	(1.479)
Share of secondary sector workers, 2010	-2.044	(1.463)
Share of tertiary sector workers, 2010	0.578	(0.551)
Share of internet users, 2010	0.386	(1.854)

Note: This table presents balance tests for product-level trade shocks, following Borusyak et al. (2022). Each row represents a regression of the predetermined variable, transformed to the product level, on the product-level shock (see Equation A6). The sample includes 4,374 six-digit HS products and 9 years between 2011 and 2019. For readability, all estimated coefficients are multiplied by 1,000,000. Standard errors are clustered at the 2-digit HS section level (N = 93).
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

I Conceptual Model: Retention and Unrest Participation

In this section, we present a simple conceptual model to clarify the linkage between retention plans and unrest participation. Our modeling draws upon insights from literature on temporary migration (for a review, see Dustmann and Görlach, 2016).

I.1 Model Setup

Consider a migrant in destination d. He has a two-period horizon: the current period (period 1) and the future (period 2). This migrant's lifetime utility is written as:

$$V = u_1 + \beta u_2, \tag{A7}$$

where u_t is the utility in period $t \in \{1, 2\}$, which we specify in detail below, and β is the discount factor.

Period 1 Utility. In period 1, the migrant receives basic earnings w. However, there is one component of income that depends on unrest participation, for example, wage arrears, the resolution of which requires substantive negotiations, possibly by expressive and even violent means.[16] We let $e \geq 0$ denote the level of engagement in unrest. Its marginal return is $a > 0$. However, participation in unrest incurs some cost, which can be due to simple psychological stress, time cost, or even government repression. The cost is given by $\frac{1}{2}\gamma e^2$. The cost function is convex in e, and its magnitude is governed by parameter $\gamma > 0$. Collecting these terms gives the utility in period 1:

$$u_1 = w + ae - \frac{1}{2}\gamma e^2. \tag{A8}$$

Period 2 Utility. In period 2, the migrant may either return to his origin o or stay in destination d. The utility of returning home is normalized to be zero. For ease of discussion below, this is labeled as $u_{2o} = 0$.

In contrast, the utility of staying in the destination is:

$$u_{2d} = x - c - \frac{1}{2}\kappa e^2. \tag{A9}$$

x is the present value of continuing to stay in destination d, which may include earnings, local public services, and simple taste. We assume that x is normally distributed among migrants. However, to stay, the migrant has to bear some cost that consists of two parts. The first part, denoted by c, is the exogenous fixed cost of settlement. It includes all kinds of movement costs, and importantly, institutional barriers of *hukou* transfers that one has to overcome. The other part of the cost is

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[16] In fact, wage arrears are a reason for 70.58% unrest events recorded in CLB data during 2011–2019.

associated with unrest participation in the first period. A migrant with unrest history may find it harder to settle, because of, for instance, government retaliation that makes settlement impossible or employer retaliation that imposes challenges in satisfying *hukou* transfer requirements.

In summary, a migrant's utility in period 2 depends on where he lives. Let r be a binary binary variable r that equals one if staying. Then,

$$u_2 = ru_{2d} + (1-r)u_{2o} = r\left(x - c - \frac{1}{2}\kappa e^2\right).\tag{A10}$$

Migrant's Problem. In period 1, in anticipation of the future benefits and costs, a migrant decides migration plan r and chooses unrest participating level e. Thus, the migrant's problem is:

$$\begin{aligned}\mathcal{V} = \max_{e,r} V &= u_1 + \beta u_2\\ &= w + ae - \frac{1}{2}\gamma e^2 + \beta r\left(x - c - \frac{1}{2}\kappa e^2\right).\end{aligned}\tag{A11}$$

I.2 Results

We solve the model by first finding the optimal e for $r = 1$ and $r = 0$, and then comparing the resulting lifetime utility between two scenarios.

If $r = 1$, by first order conditions (FOCs), it is easy to see the optimal unrest participating level is, $e_1 = \frac{a}{\gamma+\kappa}$. Thus, the lifetime utility is

$$\mathcal{V}_1 = w + e_1 + \beta E(x) - \frac{1}{2}(\gamma + \beta\kappa)e_1^2 - \beta c.\tag{A12}$$

When $r = 0$, by FOCs, the optimal unrest participating level is $e_0 = \frac{a}{\gamma}$. Then, the lifetime utility is

$$\mathcal{V}_0 = w + e_0 - \frac{1}{2}\gamma e_0^2.\tag{A13}$$

Taken together, a migrant would plan on staying, namely, $r = 1$, if

$$\mathcal{V}_1 > \mathcal{V}_0\tag{A14}$$

$$x > c + \frac{a^2\kappa}{2\gamma(\gamma + \beta\kappa)}\tag{A15}$$

At the aggregate level, the share for an individual migrant to stay is

$$p \equiv \Pr(r = 1) = 1 - F\left[c + \frac{a^2\kappa}{2\gamma(\gamma + \beta\kappa)}\right].\tag{A16}$$

The aggregate level of unrest is

$$\mathcal{E} \equiv E(e) = pe_1 + (1-p)e_0 = \frac{a}{\gamma}\left(1 - \frac{p\beta\kappa}{\gamma + \beta\kappa}\right). \tag{A17}$$

RESULT 1. $\frac{\partial p}{\partial c} < 0$ and $\frac{\partial \mathcal{E}}{\partial c} > 0$. *The hukou reform induces more migrants to stay in the destination, thus, it reduces the aggregate level of unrest.*

Proof. Take partial derivatives:

$$\frac{\partial p}{\partial c} = -f\left[c + \frac{a^2\kappa}{2\gamma(\gamma + \beta\kappa)}\right] < 0, \tag{A18}$$

$$\frac{\partial \mathcal{E}}{\partial c} = -\frac{a\beta\kappa}{\gamma(\gamma + \beta\kappa)}\frac{\partial p}{\partial c} > 0. \tag{A19}$$

∎

RESULT 2. *If c is sufficiently high,* $\frac{\partial^2 p}{\partial c \partial \beta} < 0$. *That said, if the initial settlement cost is sufficiently high, i.e., most migrants would not settle, then the hukou reform has a larger impact on increasing retention among more forward-looking migrants.*

Proof.

$$\frac{\partial^2 p}{\partial c \partial \beta} = f'\left[c + \frac{a^2\kappa}{2\gamma(\gamma + \beta\kappa)}\right]\frac{2\gamma\kappa^2 a^2}{[2\gamma(\gamma + \beta\kappa)]^2} < 0, \tag{A20}$$

provided that c is sufficiently high. ∎

A.39

J Causal Mediation Analysis

J.1 Conventional Approach

Suppose that the unit of analysis is denoted by i. This does not lose generality. In a panel, one can define $i \equiv (j, t)$ where j and t index units and periods, respectively. For brevity, we let i index individuals in subsequent discussion.

Let Y_i denote the outcome variable. T_i is the treatment of interest. M_i is a mechanism variable. We are interested in the causal effect of that T_i has on Y_i and how much of this effect is achieved by T_i intervening in M_i. To clarify parameters of interest, consider a potential outcome framework with linear heterogeneity of causal effects:

$$Y_i(t, m) = \tau_i t + \gamma_i m + u_i, \tag{A21}$$

$$M_i(t) = \pi_i t + v_i. \tag{A22}$$

Therefore,

$$Y_i(t) \equiv Y_i(t, M_i(t)) = (\tau_i + \gamma_i \pi_i)\, t + u_i + \gamma_i v_i \equiv \beta_i t + \eta_i, \tag{A23}$$

where $\beta_i = \tau_i + \gamma_i \pi_i$ and $\eta_i = u_i + \gamma_i v_i$.

DEFINITION 2 (Causal Parameters of Interest).

1. The total effect is $\beta_i = \tau_i + \gamma_i \pi_i$, thus, the average total effect (ATE) is $E(\beta_i) = E(\tau_i) + E(\gamma_i \pi_i)$.
2. The indirect effect that is due to mechanism variable M_i is $\gamma_i \pi_i$, thus, the average indirect effect (AIE) is $E(\gamma_i \pi_i)$.
3. The direct effect that is not due to mechanism variable M_i is τ_i, thus, the average direct effect (ADE) is $E(\tau_i)$.

Note that $\beta_i = \tau_i + \gamma_i \pi_i$ and $ATE = ADE + AIE$.

We further assume that T_i is exogenous. It abstracts away from identification issues of the effects of T_i, allowing us to focus on identification of mediation effects.

ASSUMPTION 5 (Treatment Exogeneity). $\{Y_i(t', m), M_i(t)\} \perp\!\!\!\perp T_i$, for all t, t', and m.

The conventional approach, popularized by Baron and Kenny (1986), is to estimate the following linear simultaneous equations model (LSEM):

$$Y_i = \alpha_1 + \beta T_i + e_{i1}, \tag{A24}$$

$$Y_i = \alpha_2 + \tau T_i + \gamma M_i + e_{i2}, \tag{A25}$$

$$M_i = \alpha_3 + \pi T_i + e_{i3}. \tag{A26}$$

The change in estimated coefficient on T_i after including M_i is interpreted as AIE, that is,

$$\widehat{AIE} \equiv \hat{\beta} - \hat{\tau} = \hat{\gamma}\hat{\pi}. \tag{A27}$$

$\hat{w}$ denotes the OLS estimand of coefficient w, i.e., probability limit of OLS estimator. The equality is by the properties of least squares, which is the estimated effect of M_i on Y_i times the estimated effect of T_i on M_i. However, strong assumptions are required in order to interpret $\widehat{AIE}$ as AIE (e.g., among others, Imai et al., 2011).

To see this, first consider $\hat{\beta}$.

$$\hat{\beta} = \frac{Cov(T_i, Y_i)}{Var(T_i)} \tag{A28}$$

$$= \frac{Cov(T_i, \beta_i T_i + \eta_i)}{Var(T_i)} \tag{A29}$$

$$= E(\beta_i). \tag{A30}$$

The second equality is by plugging in the potential outcome Equation A23. The third equality is by Assumption 5. Thus, $\hat{\beta}$ identifies the ATE.

Next, consider $\hat{\tau}$. By the property of least squares,

$$\hat{\tau} = \frac{Cov(T_i, Y_i - \hat{\gamma}M_i)}{Var(T_i)} = \hat{\beta} - \hat{\gamma}\hat{\pi}, \tag{A31}$$

where $\hat{\pi} = E(\pi_i)$ identifies the population average effect of T_i on M_i under Assumption 5. Now, we derive $\hat{\gamma}$. Let $L(R \mid T)$ denote the linear projection of R on T and constant 1, and $\widetilde{R}$ is the linear projection residual, namely, $\widetilde{R} = R - L(R \mid T)$. By Assumption 5 and Equation A22, $L(M_i \mid T_i) = E(M_i \mid T_i)$, i.e., the linear projection recovers the conditional mean.[17] $\hat{\gamma}$ has the following expression:

$$\hat{\gamma} = \frac{E(\widetilde{M}_i Y_i)}{E(\widetilde{M}_i^2)} \tag{A32}$$

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[17] $E(M_i \mid T_i) = E(\pi_i \mid T_i)T_i + E(v_i \mid T_i) = E(\pi_i)T_i + E(v_i)$ is linear.

$$= \frac{E\left[\widetilde{M}_i(\tau_i T_i + \gamma_i M_i + u_i)\right]}{E(\widetilde{M}_i^2)} \tag{A33}$$

$$= \frac{E(\widetilde{M}_i M_i \gamma_i)}{E(\widetilde{M}_i^2)} + \frac{E(\widetilde{M}_i T_i \tau_i)}{E(\widetilde{M}_i^2)} + \frac{E(\widetilde{M}_i u_i)}{E(\widetilde{M}_i^2)}. \tag{A34}$$

The first term is a weighted average of γ_i, albeit different from the population average $E(\gamma_i)$. The second term picks up the causal effect of T_i, representing a contamination bias as in Goldsmith-Pinkham et al. (2022). The third term is the endogeneity bias. Therefore, $\hat{\gamma}$ is biased for $E(\gamma_i)$, and in fact, it does not even identify a causal effect of M_i on Y_i, regardless of weighting schemes. Such bias is expected, since there is no exogenous variation in M_i conditional on T_i that warrants identification.

Taken together,

$$\widehat{AIE} = \hat{\beta} - \hat{\tau} \tag{A35}$$

$$= \left[\frac{E(\widetilde{M}_i M_i \gamma_i)}{E(\widetilde{M}_i^2)} + \frac{E(\widetilde{M}_i T_i \tau_i)}{E(\widetilde{M}_i^2)} + \frac{E(\widetilde{M}_i u_i)}{E(\widetilde{M}_i^2)}\right] E(\pi_i) \tag{A36}$$

$$\neq AIE \tag{A37}$$

$$\equiv E(\gamma_i \pi_i) = E(\gamma_i)E(\pi_i) - Cov(\gamma_i, \pi_i). \tag{A38}$$

$\widehat{AIE}$ is biased for AIE. The bias is expressed as:

$$\text{Bias} = \widehat{AIE} - AIE = \underbrace{[\hat{\gamma} - E(\gamma_i)]\, E(\pi_i)}_{\text{bias 1}} + \underbrace{Cov(\gamma_i, \pi_i)}_{\text{bias 2}}. \tag{A39}$$

There are two sources of bias. Bias 1 is due to that $\hat{\gamma}$ does not identify $E(\gamma_i)$. Bias 2 is a mechanical consequence of LSEM. The LSEM overlooks heterogeneity in γ_i and uses an *average* slope of Y_i for M_i, $E(\gamma_i)$, to evaluate M_i's marginal contribution to Y_i, π_i. This produces bias if π_i is correlated with γ_i: Consider a positive correlation, the average slope systemically understates contributions of high π_i's and overstates contributions of low π_i's.

Researchers often assume homogeneity of γ_i, which eliminates bias 2. But bias 1 still exists. Imai et al. (2011) show a sufficient condition to eliminate this bias is a "sequential ignorability" assumption.

ASSUMPTION 6 (Sequential Ignorability). $\{Y_i(t', m), M_i(t)\} \perp\!\!\!\perp M_i \mid T_i = t, \forall t, t', m.$

That said, M_i is exogenous conditional on T_i, indicating that there is no other post-treatment confounders. With this assumption, we can derive the following results.

$$E(\widetilde{M}_i T_i \tau_i) = E[E(\widetilde{M}_i T_i \tau_i \mid T_i)] \tag{A40}$$

$$= E[E(\widetilde{M}_i T_i \mid T_i)E(\tau_i \mid T_i)] \tag{A41}$$

$$= E(\widetilde{M}_i T_i)E(\tau_i) \tag{A42}$$

$$= 0. \tag{A43}$$

The first equality is by the law of iterated expectations (LIE). The second equality is by Assumption 6. The third equality is by Assumption 5. The fourth equality is by the orthogonality of linear projection residuals. Using similar tricks yields

$$E(\widetilde{M}_i u_i) = E[E(\widetilde{M}_i u_i \mid T_i)] \tag{A44}$$

$$= E[E(\widetilde{M}_i \mid T_i)E(u_i \mid T_i)] \tag{A45}$$

$$= E(\widetilde{M}_i)E(u_i) \tag{A46}$$

$$= 0. \tag{A47}$$

Together with homogeneity assumption $\gamma_i = \gamma$, $\hat{\gamma} = \gamma = E(\gamma_i)$. Therefore, $\widehat{AIE} = AIE$.[18]

J.2 IV-Augmented Approach

The plausibility of Assumption 6 is questionable. Without this assumption, the key problem is that $\hat{\gamma}$ is not consistent for a causal effect of M_i on Y_i, not to mention $E(\gamma_i)$. We propose to use an IV for M_i, denoted by Z_i, to identify a causal effect of M_i on Y_i. We want to upfront regarding potential issues in this approach. First of all, the validity of IV requires additional assumptions. Second, provided that IV is valid, it in general does not identify the population average effect $E(\gamma_i)$, unless γ_i is constant or other assumptions are imposed. Nonetheless, we view the IV-augmented approach as bringing some improvements to the conventional approach.

We extend the potential outcome framework to incorporate IV Z_i.

$$Y_i(t, m) = \tau_i t + \gamma_i m + u_i, \tag{A48}$$

$$M_i(t, z) = \pi_i t + \theta_i z + v_i. \tag{A49}$$

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[18]This result does not require the homogeneity assumption. With Assumption 6, one can further show that $E(\widetilde{M}_i M_i \gamma_i) = E(\widetilde{M}_i M_i \gamma_i \mid T_i) = E(\widetilde{M}_i M_i)E(\gamma_i) = E(\widetilde{M}_i^2)E(\gamma_i)$. Thus, $\hat{\gamma} = E(\gamma_i)$, hence, bias 1 = 0. Assumption 6 also implies that $\gamma_i \perp\!\!\!\perp \pi_i \mid T_i$. Thus, bias 2 = $E[E(\gamma_i \pi_i \mid T_i)] - E(\gamma_i)E(\pi_i) = E[E(\gamma_i \mid T_i)E(\pi_i \mid T_i)] - E(\gamma_i)E(\pi_i) = 0$, where the first equality is by the definition of covariance and the LIE, the second equality is by $\gamma_i \perp\!\!\!\perp \pi_i \mid T_i$, and the last equality is by Assumption 5. Taken together, $\widehat{AIE} = AIE$, even if the homogeneity assumption is not made.

Therefore, we have the following reduced-form model:

$$Y_i(t, z) \equiv Y_i\left(t, M_i(t, z)\right) \tag{A50}$$

$$= \left(\pi_i + \gamma_i \pi_i\right) t + \gamma_i \theta_i z + \left(u_i + \gamma_i v_i\right) \tag{A51}$$

$$= \beta_i t + \rho_i z + \eta_i, \tag{A52}$$

where $\beta_i = \tau_i + \gamma_i \pi_i$, $\rho_i = \gamma_i \theta_i$, and $\eta_i = u_i + \gamma_i v_i$.

We impose the following assumptions.

ASSUMPTION 7 (Treatment Exogeneity). $\{Y_i(t', m), M_i(t, z), Z_i\} \perp\!\!\!\perp T_i$, for all t, t', m, and z.

ASSUMPTION 8 (IV Validity).

1. (Independence) $\{Y_i(t', m), M_i(t, z)\} \perp\!\!\!\perp Z_i$ for all t, t', m, and z.
2. (Exclusion) $Y_i(t, m) \mid_z = Y_i(t, m) \mid_{z'}$ for all z and z'.
3. (Relevance) $E(\theta_i) \neq 0$.
4. (Monotonicity) Either $\Pr(\theta_i \geq 0) = 1$ or $\Pr(\theta_i \leq 0) = 1$.

PROPOSITION 4. *Under Assumptions 7 and 8, two stage least squares (2SLS) estimation of the LSEM, with M_i instrumented by Z_i, yields*

$$\widehat{AIE} = \hat{\beta} - \hat{\tau} = \hat{\gamma}\hat{\pi} = \underbrace{E(\gamma_i \pi_i)}_{AIE} + \underbrace{[\hat{\gamma} - E(\gamma_i)] E(\pi_i)}_{bias\ 1} - \underbrace{Cov(\gamma_i, \pi_i)}_{bias\ 2}, \tag{A53}$$

where $\hat{\beta} = E(\beta_i)$ and $\hat{\gamma} = E\left[\frac{\theta_i}{E(\theta_i)}\gamma_i\right]$. $\hat{\beta} - \hat{\tau}$ identifies AIE if (i) γ_i is constant, or (ii) $\{\theta_i, \pi_i\} \perp\!\!\!\perp \gamma_i$.

Proof. By Assumption 7, it is straightforward to show that

$$\hat{\beta} = \frac{Cov(T_i, Y_i)}{Var(T_i)} = E(\beta_i), \tag{A54}$$

$$\hat{\pi} = \frac{Cov(T_i, M_i)}{Var(T_i)} = E(\pi_i). \tag{A55}$$

By the property of least squares, $\hat{\tau} = \frac{Cov(T_i, Y_i - \hat{\gamma} M_i)}{Var(T_i)} = \hat{\beta} - \hat{\gamma}\hat{\pi}$. We now derive the 2SLS estimand $\hat{\gamma}$. It can be written as:

$$\hat{\gamma} = \frac{E(\tilde{Z}_i Y_i)}{E(\tilde{Z}_i M_i)}, \tag{A56}$$

where $\widetilde{Z}_i = Z_i - L(Z_i \mid T_i)$ is the linear projection residual. By Assumption 7, $E(Z_i \mid T_i) = E(Z_i)$ is linear, thus, $L(Z_i \mid T_i) = E(Z_i \mid T_i)$. We can show the following result.

$$E(\widetilde{Z}_i Y_i) = E[\widetilde{Z}_i(\beta_i T_i + \rho_i Z_i + \eta_i)] \tag{A57}$$

$$= E(\widetilde{Z}_i T_i)E(\beta_i) + E(\widetilde{Z}_i Z_i)E(\rho_i) + E(\widetilde{Z}_i)E(\eta_i), \tag{A58}$$

$$= E(\widetilde{Z}_i Z_i)E(\rho_i). \tag{A59}$$

The second equality is by Assumptions 7 and 8(1). The third equality uses the fact that $\widetilde{Z}_i = Z_i - E(Z_i \mid T_i) = Z_i - E(Z_i)$. With similar tricks, the following result follows:

$$E(\widetilde{Z}_i M_i) = E[\widetilde{Z}_i(\pi_i T_i + \theta_i Z_i + u_i)] \tag{A60}$$

$$= E(\widetilde{Z}_i T_i)E(\pi_i) + E(\widetilde{Z}_i Z_i)E(\theta_i) + E(\widetilde{Z}_i)E(u_i), \tag{A61}$$

$$= E(\widetilde{Z}_i Z_i)E(\theta_i). \tag{A62}$$

Taken together,

$$\hat{\gamma} = \frac{E(\rho_i)}{E(\theta_i)} = \frac{E(\theta_i \gamma_i)}{E(\theta_i)}. \tag{A63}$$

An alternative expression for $\hat{\gamma}$ is

$$\hat{\gamma} = \frac{E(\widetilde{Z}_i M_i \gamma_i)}{E(\widetilde{Z}_i M_i)}. \tag{A64}$$

Therefore,

$$\widehat{AIE} = \hat{\beta} - \hat{\tau} \tag{A65}$$

$$= \hat{\gamma}E(\pi_i) \tag{A66}$$

$$= \hat{\gamma}E(\pi_i) + E(\gamma_i \pi_i) - Cov(\gamma_i, \pi_i) + E(\gamma_i)E(\pi_i) \tag{A67}$$

$$= \underbrace{E(\gamma_i \pi_i)}_{\text{AIE}} + \underbrace{[\hat{\gamma} - E(\gamma_i)]E(\pi_i)}_{\text{bias 1}} - \underbrace{Cov(\gamma_i, \pi_i)}_{\text{bias 2}}, \tag{A68}$$

where $\hat{\gamma} = \frac{E(\widetilde{Z}_i M_i \gamma_i)}{E(\widetilde{Z}_i M_i)} = \frac{E(\theta_i \gamma_i)}{E(\theta_i)}$. It is obvious that bias 1 = bias 2 = 0 if (i) γ_i is constant, or (ii) $\{\theta_i, \pi_i\} \perp\!\!\!\perp \gamma_i$, making $\widehat{AIE} = AIE$. ∎

J.3   Extension: Regression Discontinuity

In light of the research design of this paper, we extend the results above to RDDs. If one takes a local randomization view of RDDs, then our results above can be directly applied within a very narrow bandwidth around the cutoff. No polynomial controls are necessary provided rich data are available even after bandwidth restrictions. In the following, we focus our attention on parametric

RDDs, which impose some functional form assumptions on the conditional means of potential outcomes and thus necessitate polynomial controls.

Let r_i denote the running variable. $T_i = \mathbb{1}\{r_i \geq 0\}$. $\mathbf{R}_i = (r_i, r_i^2, \cdots, r_i^p)$ is a set of power functions of r_i, up to order p. Also define $\mathbf{R}_i^{(0)} = (1, \mathbf{R}_i)$. $\mathbf{X}_i = [\mathbf{R}_i T_i, \mathbf{R}_i(1 - T_i)]'$ is the polynomial function to be included in RD regressions. Also define $\mathbf{X}_i^{(0)} = \left[\mathbf{R}_i^{(0)} T_i, \mathbf{R}_i^{(0)}(1 - T_i)\right]'$. With the introduction of running variable, the LSEM to estimate now becomes:

$$Y_i = \alpha_1 + \beta T_i + \mathbf{X}_i' \zeta_1 + e_{i1}, \tag{A69}$$

$$Y_i = \alpha_2 + \tau T_i + \gamma M_i + \mathbf{X}_i' \zeta_2 + e_{i2}, \tag{A70}$$

$$M_i = \alpha_3 + \pi T_i + \mathbf{X}_i' \zeta_3 + e_{i3}. \tag{A71}$$

In RDDs, parameters of interest are causal effects at cutoff $r_i = 0$. They are defined as follows.

DEFINITION 3 (Parameters of Interest in RDDs).

1. The total effect is $\beta_i = \tau_i + \gamma_i \pi_i$. ATE is defined as the average total effect conditional at $r_i = 0$, $E(\beta_i \mid r_i = 0) = E(\tau_i \mid r_i = 0) + E(\gamma_i \pi_i \mid r_i = 0)$.
2. The indirect effect that is due to mechanism variable M_i is $\gamma_i \pi_i$. AIE is defined as the average indirect effect at cutoff, $E(\gamma_i \pi_i \mid r_i = 0)$.
3. The direct effect that is not due to mechanism variable M_i is τ_i. ADE is defined as the average direct effect at cutoff, $E(\tau_i \mid r_i = 0)$.

ASSUMPTION 9 (Linearity of Conditional Means).

1. $E[Y_i(1, Z_i) \mid r_i]$ and $E[Y_i(0, Z_i) \mid r_i]$ are linear in $\mathbf{R}_i^{(0)}$.
2. $E[M_i(1, Z_i) \mid r_i]$ and $E[M_i(0, Z_i) \mid r_i]$ are linear in $\mathbf{R}_i^{(0)}$.

ASSUMPTION 10 (IV Validity).

1. (Independence) $\{Y_i(t', m), M_i(t, z)\} \perp\!\!\!\perp Z_i \mid r_i$ for all t, t', m, and z.
2. (Exclusion) $Y_i(t, m) \mid_z = Y_i(t, m) \mid_{z'}$ for all z and z'.
3. (Relevance) $E(\tilde{Z}_i M_i) \neq 0$, where $\tilde{Z}_i = Z_i - L\left[Z_i \mid \mathbf{X}_i^{(0)}\right]$ is the linear projection residual.
4. (Monotonicity) Either $\Pr(\theta_i \geq 0) = 1$ or $\Pr(\theta_i \leq 0) = 1$.

Assumption 9 specifies conditional means of potential outcomes. It implies the continuity of conditional means. The assumption also allow us to abstract away from estimation complications in parametric RDDs, e.g., bandwidth selection. Assumption 10 warrants validity of IV. Importantly, independence only needs to hold conditional on running variable r_i. Assumption 11 assumes linearity of IV, as in Ishimaru (2024).

PROPOSITION 5. *Under Assumptions 9, 10, and 11, 2SLS estimation of the LSEM, with M_i instrumented by Z_i, yields*

$$\widehat{AIE} = \hat{\beta} - \hat{\tau} = \hat{\gamma}\hat{\pi}$$
$$= \underbrace{E(\gamma_i \pi_i \mid r_i = 0)}_{AIE} + \underbrace{[\hat{\gamma} - E(\gamma_i \mid r_i = 0)] E(\pi_i \mid r_i = 0)}_{bias\ 1} - \underbrace{Cov(\gamma_i, \pi_i \mid r_i = 0)}_{bias\ 2}, \quad (A72)$$

where $\hat{\beta} = E(\beta_i \mid r_i = 0)$ and $\hat{\gamma} = \frac{E(\widetilde{Z}_i M_i \gamma_i)}{E(\widetilde{Z}_i M_i)} = \frac{E[Var(Z_i \mid r_i) E(\theta_i \gamma_i \mid r_i)]}{E[Var(Z_i \mid r_i) E(\theta_i \mid r_i)]}$. $\widehat{AIE}$ identifies AIE if (i) γ_i is constant, or (ii) $\{\theta_i, \pi_i\} \perp\!\!\!\perp \gamma_i \mid r_i$ and $\gamma_i \perp\!\!\!\perp r_i$.

Proof. By Assumption 9, linear regression identifies conditional means of potential outcomes. Thus, a linear regression of Y_i on 1, T_i and $\mathbf{X}_i$ yields

$$\hat{\beta} = \lim_{r \downarrow 0} E[Y_i(1, Z_i) \mid r_i = r] - \lim_{r \uparrow 0} E[Y_i(0, Z_i) \mid r_i = r] \quad (A73)$$

$$= \lim_{r \downarrow 0} E[\beta_i + \rho_i Z_i + \eta_i \mid r_i = r] - \lim_{r \uparrow 0} E[\rho_i Z_i + \eta_i \mid r_i = r] \quad (A74)$$

$$= E(\beta_i \mid r_i = 0), \quad (A75)$$

where the first equality is by linearity assumed in Assumption 9, the second equality plugs in potential outcomes, and the last equality uses continuity implied by 9. Similarly, one can show that $\hat{\pi} = E(\pi \mid r_i = 0)$.

By the property of least square, $\hat{\tau} = \hat{\beta} - \hat{\gamma}\hat{\pi}$. Now derive $\hat{\gamma} = \frac{E(\widetilde{Z}_i Y_i)}{E(\widetilde{Z}_i M_i)}$, where $\widetilde{Z}_i = Z_i - L\left[Z_i \mid \mathbf{X}_i^{(0)}\right]$. $\hat{\gamma}$ is written as:

$$\hat{\gamma} = \frac{E[\widetilde{Z}_i(\beta_i T_i + \rho_i Z_i + \eta_i)]}{E[\widetilde{Z}_i(\pi_i T_i + \theta_i Z_i + v_i)]}. \quad (A76)$$

Analyze term by term.

$$E(\widetilde{Z}_i T_i \beta_i) = E[E(\widetilde{Z}_i T_i \beta_i \mid r_i)] = E[E(\tilde{Z}_i \mid r_i) T_i E(\beta_i \mid r_i)] = 0. \quad (A77)$$

The first equality is by the LIE. The second equality is by Assumption 10(1) and the fact that T_i is completely determined by r_i in a RDD. The last equality is due to Assumption 11 that implies $L\left[Z_i \mid \mathbf{X}_i^{(0)}\right] = E\left[Z_i \mid \mathbf{X}_i^{(0)}\right]$. Similarly,

$$E(\widetilde{Z}_i T_i \pi_i) = 0, \tag{A78}$$

$$E(\widetilde{Z}_i \eta_i) = 0, \tag{A79}$$

$$E(\widetilde{Z}_i v_i) = 0. \tag{A80}$$

Therefore,

$$\hat{\gamma} = \frac{E(\widetilde{Z}_i Z_i \theta_i \gamma_i)}{E(\widetilde{Z}_i Z_i \theta_i)} = \frac{E[E(\widetilde{Z}_i Z_i \mid r_i) E(\theta_i \gamma_i \mid r_i)]}{E[E(\widetilde{Z}_i Z_i \mid r_i) E(\theta_i \mid r_i)]} = \frac{E[Var(Z_i \mid r_i) E(\theta_i \gamma_i \mid r_i)]}{E[Var(Z_i \mid r_i) E(\theta_i \mid r_i)]}. \tag{A81}$$

An alternative expression for $\hat{\gamma}$ is

$$\hat{\gamma} = \frac{E(\widetilde{Z}_i M_i \gamma_i)}{E(\widetilde{Z}_i M_i)}. \tag{A82}$$

Taken together,

$$\hat{\beta} - \hat{\tau} = \hat{\gamma} E(\pi_i \mid r_i = 0) \tag{A83}$$

$$= \hat{\gamma} E(\pi_i \mid r_i = 0) + E(\gamma_i \pi_i \mid r_i = 0) - Cov(\gamma_i, \pi_i \mid r_i = 0) + E(\gamma_i \mid r_i = 0) E(\pi_i \mid r_i = 0) \tag{A84}$$

$$= \underbrace{E(\gamma_i \pi_i \mid r_i = 0)}_{\text{AIE}} + \underbrace{[\hat{\gamma} - E(\gamma_i \mid r_i = 0)] E(\pi_i \mid r_i = 0)}_{\text{bias 1}} - \underbrace{Cov(\gamma_i, \pi_i \mid r_i = 0)}_{\text{bias 2}}, \tag{A85}$$

where $\hat{\gamma} = \frac{E(\widetilde{Z}_i M_i \gamma_i)}{E(\widetilde{Z}_i M_i)} = \frac{E[Var(Z_i \mid r_i) E(\theta_i \gamma_i \mid r_i)]}{E[Var(Z_i \mid r_i) E(\theta_i \mid r_i)]}$. It is obvious that bias 1 = bias 2 = 0 if (i) γ_i is constant, or (ii) $\{\theta_i, \pi_i\} \perp\!\!\!\perp \gamma_i \mid r_i$ and $\gamma_i \perp\!\!\!\perp r_i$. ∎

J.4 Sensitivity Test

Our results imply that $\hat{\beta} - \hat{\tau}$ identifies AIE if γ_i is constant. We maintain this assumption to perform our analysis. However, this assumption may be overly strong in many applications. In this section, we gauge under what conditions $\hat{\beta} - \hat{\tau}$ is still informative about AIE even if γ_i heterogeneous.

Consider our baseline setups. Note that the bias is expressed as

$$Bias = \hat{\gamma}\hat{\pi} - \underbrace{E(\gamma_i \pi_i)}_{\text{AIE}} \tag{A86}$$

$$= [\hat{\gamma} - E(\gamma_i)]\hat{\pi} - Cov(\gamma_i, \pi_i) \tag{A87}$$

$$= Cov(\phi_i, \gamma_i)\hat{\pi} - Cov(\gamma_i, \pi_i) \tag{A88}$$

$$= \rho_{\phi\gamma}\sigma_\phi\sigma_\gamma\hat{\pi} - \rho_{\gamma\pi}\sigma_\gamma\sigma_\pi, \tag{A89}$$

where $\phi_i = \frac{\widetilde{Z}_i M_i}{E(\widetilde{Z}_i M_i)}$, $\rho_{\phi\gamma} = Corr(\phi_i, \gamma_i)$, $\rho_{\gamma\pi} = Corr(\gamma_i, \pi_i)$, $\sigma_\phi = SD(\phi_i)$, $\sigma_\gamma = SD(\gamma_i)$, and $\sigma_\pi = SD(\pi_i)$. If there is knowledge of $Cov(\phi_i, \gamma_i)$ and $Cov(\gamma_i, \pi_i)$, together with $\hat{\gamma}\hat{\pi}$, we can de-bias or at least bound AIE. We propose one approach below.

ASSUMPTION 12 (Sign and Distributional Restrictions). All γ_i has the same sign. All π_i has the same sign. γ_i and π_i are uniformly distributed.

PROPOSITION 6. *Under Assumption 12, for every given $\rho_{\phi\gamma}$ and $\rho_{\phi\gamma}$ and $\rho_{\gamma\pi}$, AIE is identified by estimand*

$$\widehat{AIE} = \left[\frac{\sqrt{3}\hat{\gamma}}{\sqrt{3}\,\mathrm{sgn}(\gamma_i) + \rho_{\phi\gamma}\hat{\sigma}_\phi} + \frac{\rho_{\gamma\pi}\hat{\sigma}_\phi\,\mathrm{sgn}(\pi_i)}{\sqrt{3}} \right] \hat{\pi}. \tag{A90}$$

where $\hat{\sigma}_\phi = \mathrm{plim}\sqrt{\frac{1}{n}\sum_{i=1}^{n}\left(\frac{\widetilde{Z}_i M_i}{\frac{1}{n}\sum_{i=1}^{n}\widetilde{Z}_i M_i} - 1\right)^2}$, and $\mathrm{sgn}(\gamma_i)$ and $\mathrm{sgn}(\pi_i)$ respectively give signs of γ_i and π_i.

Proof. By the expression of *Bias*, $\hat{\gamma}\hat{\pi} - E(\gamma_i)\hat{\pi} = \rho_{\phi\gamma}\sigma_\phi\sigma_\gamma\hat{\pi}$. Under Assumption 12, $E(\gamma_i) = \sqrt{3}\,\mathrm{sgn}(\gamma_i)\sigma_\gamma$. Thus, $\sigma_\gamma = \frac{\hat{\gamma}}{\sqrt{3}\,\mathrm{sgn}(\gamma_i)+\rho_{\phi\gamma}\sigma_\phi}$. Since π_i satisfies a uniform distribution and $\hat{\pi} = E(\pi_i)$, $\sigma_\pi = \frac{\hat{\pi}\,\mathrm{sgn}(\pi_i)}{\sqrt{3}}$. Taken together,

$$AIE = \left[\frac{\sqrt{3}\hat{\gamma}}{\sqrt{3}\,\mathrm{sgn}(\gamma_i) + \rho_{\phi\gamma}\sigma_\phi} + \frac{\rho_{\gamma\pi}\sigma_\phi\,\mathrm{sgn}(\pi_i)}{\sqrt{3}} \right] \hat{\pi}. \tag{A91}$$

In this expression, σ_ϕ is unknown. However, consider estimator $\sqrt{\frac{1}{n}\sum_{i=1}^{n}\left(\frac{\widetilde{Z}_i M_i}{\frac{1}{n}\sum_{i=1}^{n}\widetilde{Z}_i M_i} - 1\right)^2}$, the probability limit of which is denoted by $\hat{\sigma}_\phi$. By the weak law of large numbers, $\hat{\sigma}_\phi = \sigma_\phi$. As such, AIE is identified by

$$\widehat{AIE} = \left[\frac{\sqrt{3}\hat{\gamma}}{\sqrt{3}\,\mathrm{sgn}(\gamma_i) + \rho_{\phi\gamma}\hat{\sigma}_\phi} + \frac{\rho_{\gamma\pi}\hat{\sigma}_\phi\,\mathrm{sgn}(\pi_i)}{\sqrt{3}} \right] \hat{\pi}. \tag{A92}$$

for every given $\rho_{\phi\gamma}$ and $\rho_{\gamma\pi}$. ∎

The following proposition extends to the case of RDDs.

PROPOSITION 7. *Suppose $\{\gamma_i, \pi_i\} \perp\!\!\!\perp r_i$. Under Assumption 12, for every given $\rho_{\phi\gamma}$ and $\rho_{\phi\gamma}$ and $\rho_{\gamma\pi}$, AIE is identified by estimand*

$$\widehat{AIE} = \left[\frac{\sqrt{3}\hat{\gamma}}{\sqrt{3}\,\text{sgn}(\gamma_i) + \rho_{\phi\gamma}\hat{\sigma}_\phi} + \frac{\rho_{\gamma\pi}\hat{\sigma}_\phi\,\text{sgn}(\pi_i)}{\sqrt{3}} \right] \hat{\pi}. \tag{A93}$$

where $\hat{\sigma}_\phi = \text{plim}\sqrt{\frac{1}{n}\sum_{i=1}^n \left(\frac{\widetilde{Z}_i M_i}{\frac{1}{n}\sum_{i=1}^n \widetilde{Z}_i M_i} - 1 \right)^2}$, and $\text{sgn}(\gamma_i)$ and $\text{sgn}(\pi_i)$ respectively give signs of γ_i and π_i.

Proof. By assuming $\{\gamma_i, \pi_i\} \perp\!\!\!\perp r_i$, the proof is the same as in basic setups. ∎

K Additional Empirical Results for Causal Mediation Analysis

K.1 Instrument Validity

Given that the instrument Z_i is a shift-share variable, we may test its validity using the same approach in Appendix H.4. To operationalze this, rewrite Z_i:

$$Z_i = \sum_h s_{h \to i} \times \bar{\Delta} TradeShock_h \tag{A94}$$

$$= \sum_h s_{h \to i} \times \left(\sum_k s_{hk} \bar{\Delta} g_k \right) \tag{A95}$$

$$= \sum_k \left(\sum_h s_{h \to i} s_{hk} \right) \bar{\Delta} g_k \tag{A96}$$

$$\equiv \sum_k \tilde{s}_{ik} \tilde{g}_k. \tag{A97}$$

$\tilde{s}_{ik} \equiv \sum_h s_{h \to i} s_{hk}$ captures origin production specialization of an average migrant in prefecture i. $\tilde{g}_k \equiv \bar{\Delta} g_k = g_{k,2015} - \frac{1}{4} \sum_{t=2011}^{2015} g_{kt}$ is the average change in global demand for product k from 2011–2014 period to 2014–2015 period.

The excludability of Z_i follows from the quasi-exogeneity of $\tilde{g}_k$. In other words, one needs to assume that prefectures with differential odds of labor unrest do not systematically receive migrants from origins specializing in products with high fluctuations in global demand. We can conduct balance tests using the following regression model:

$$\tilde{q}_k = \beta_0 + \beta_1 \tilde{g}_k + \nu_k. \tag{A98}$$

$\tilde{q}_k = \sum_i \frac{\tilde{s}_{ik}}{\sum_i \tilde{s}_{ik}} v_i$. v_i is a covariate of recipient prefecture i, thus, $\tilde{q}_k$ captures the $\tilde{s}_{ik}$-weighted average covariate of prefectures that receive migrants from regions producing product k. The regression is weighted by $\sum_i \tilde{s}_{ik}$. Standard errors are clustered at the 2-digit HS section level to account for correlated disturbances of products within the same sector. The coefficient of interest is β_1. If β_1 is close to zero, it suggests that $\tilde{g}_k$ is not distributed is a way that may relate to trends of labor unrest in migrants' destinations.

Table A17 reports the results for balance tests. Covariates considered here are the same as those in Table 2 when we conduct the balance test for reform status $Reform_i$. In Panel A, we consider trends in variables that may influence labor unrest before the start of our sample. In Panel B, we consider some baseline characteristics. All estimates of β_1 are statistically insignificant, indicating that Z_i can be a valid instrument that predicts outmigration without picking up unobserved determinants of labor unrest.

Table A17. Balance Tests for Product-Level Trade Shocks at Origins

Dependent	Coef.	SE
Panel A: Pretrends		
ΔLog population, 2009–2010	-0.777	(0.556)
ΔLog GDP, 2009–2010	0.220	(0.199)
ΔLog expenditure on social security, 2009–2010	0.323	(0.713)
ΔLog expenditure on public security, 2009–2010	-0.056	(0.233)
Panel B: Predetermined characteristics		
Share of migrants, 2010	-1.191	(1.467)
Share of urban residents, 2010	-3.312	(2.741)
Share of secondary sector workers, 2010	-0.970	(1.458)
Share of tertiary sector workers, 2010	-1.792	(1.571)
Share of internet users, 2010	-2.765	(2.786)

Note: This table presents balance tests for origin-level trade shocks, following Borusyak et al. (2022). Each row represents a regression of the predetermined variable, transformed to the origin level, on the origin-level trade shock change. The sample includes 332 origin prefectures. Heteroskedasticity-robust standard errors are reported.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

K.2 Robustness

Alternative Construction of IV. One may be concerned that trade shocks at nearby origins can have spatial spillover impacts on labor unrest at destinations, leading to violations of the exclusion restriction. To address this concern, we construct the IV, Z_i, using only origins that are sufficiently far away from prefecture i to limit the extents of spillovers. We restrict origins to be 300km or 500km away from a destination prefecture. Table A18 reports the results using these alternative IVs. Columns (1) and (2) present the baseline results for ease of comparison. Columns (3) and (4) use an IV constructed using origins that are 300km away. Columns (5) and (6) use an IV constructed using origins that are 500km away. The results show that one can still conclude that settlement intentions play a important mechanism. In fact, results using alternative IVs indicate a more salient role of settlement intentions.

Sensitivity to Violations of the Exclusion Restriction. Uncontrolled confounders can lead to violations of the exclusion restriction, but it is challenging to find proxies for all possible confounding factors. To examine the sensitivity of our 2SLS estimate of γ in Equation 16 to violations of the exclusion restriction, we use the methodology developed by Conley et al. (2012). This approach allows the IV, Z_i, to directly enter the second stage of the model (Equation 16) with a coefficient of ψ, which measures the extent to which the exclusion restriction is violated and is set by the researcher.

We test whether instrumented outmigration still has a significant effect on labor unrest for different values of ψ. Since we find that outmigration has a positive effect, violations of the

A.52

Table A18. *Hukou* Reform, Outmigration Rate, and Labor Unrest — Robustness Checks

	Baseline		Alt. Network I		Alt. Network II	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Time window = 2011–2015						
Reform	-1.047**	-0.411	-1.033**	-0.146	-0.979*	-0.072
	(0.518)	(0.608)	(0.519)	(0.663)	(0.520)	(0.707)
$\bar{\Delta}$Outmigration		6.005***		7.996***		7.946***
		(1.304)		(2.179)		(3.055)
Polynomials	Yes	Yes	Yes	Yes	Yes	Yes
Network in 2015 census	Yes	Yes				
Network in 2015 census, only ≥300km origins			Yes	Yes		
Network in 2015 census, only ≥500km origins					Yes	Yes
% Total effect explained		0.607		0.858		0.927
1st stage coef.		0.049		0.019		0.013
Effective *F* stat.		60.978		18.870		9.386
Observations	287	287	283	283	271	271
Panel B: Time window = 2011–2019						
Reform	-1.419***	-1.033***	-1.388***	-0.901**	-1.336***	-0.762
	(0.370)	(0.383)	(0.372)	(0.412)	(0.373)	(0.463)
$\bar{\Delta}$Outmigration		3.654***		4.392***		5.036**
		(1.018)		(1.539)		(2.312)
Polynomials	Yes	Yes	Yes	Yes	Yes	Yes
Network in 2015 census	Yes	Yes				
Network in 2015 census, only ≥300km origins			Yes	Yes		
Network in 2015 census, only ≥500km origins					Yes	Yes
% Total effect explained		0.273		0.351		0.430
1st stage coef.		0.049		0.019		0.013
Effective *F* stat.		60.978		18.870		9.386
Observations	287	287	283	283	271	271

 Note: This table reports causal mediation analysis that quantifies the importance of the retention mechanism, as captured by the outmigration rate. Columns (1) and (2) represent the conventional approach. Columns (3)-(4) use the IV-augmented approach. The effective *F* statistic is calculated following Olea and Pflueger (2013). *tF* 95 percent confidence interval follows Lee et al. (2022). The IV-OLS gap is decomposed using the methodology by Ishimaru (2024). Robust standard errors are reported in parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

exclusion restriction are problematic only when ψ is positive. We look at the largest negative value of ψ such that the resultant 2SLS estimate $\hat{\gamma}$ is still significant at the 5% level. This value is denoted by $\bar{\psi}$, and it is scaled by Z_i's reduced-form effect on labor unrest, ψ_{RF}. The ratio $\bar{\psi}/\psi_{RF}$ represents the maximum hypothetical violation of the exclusion restriction that can be allowed while the 2SLS estimate $\hat{\gamma}$ remains statistically significant. A higher $\bar{\psi}/\psi_{RF}$ indicates stronger resilience of the IV to violations of the exclusion restriction.

Figure A18 reports the results, where we plot the 95 confidence interval of the 2SLS estimate $\hat{\gamma}$ against ψ/ψ_{RF}. Figure A18B shows that for the long time window (2011–2019), $\hat{\gamma}$ is not statistically significant at the 5 percent level when ψ/ψ_{RF} reaches 0.4, i.e., $\bar{\psi}/\psi_{RF} = 0.4$. When it comes to the short time window (2011–2015), Figure A18A shows that $\bar{\psi}/\psi_{RF} = 0.5$. These results indicate that
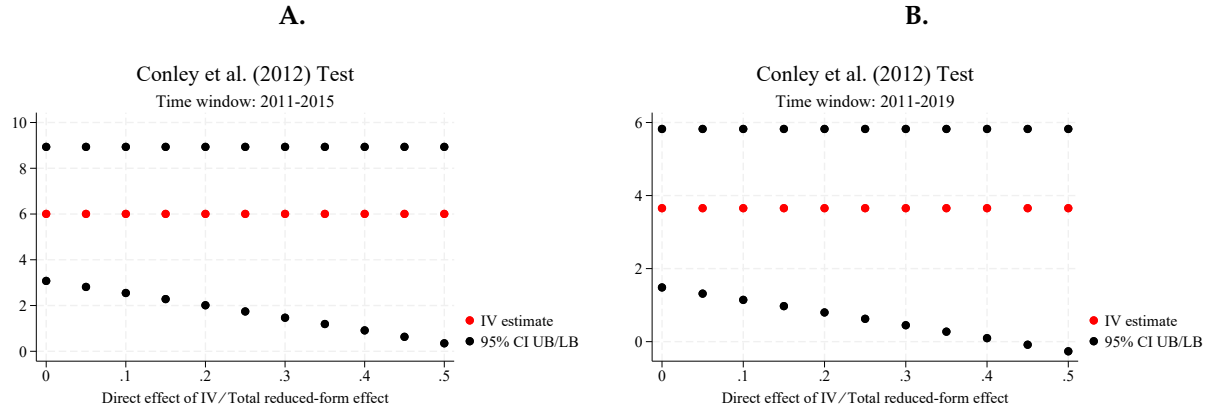
Figure A18. Conley et al. (2012) Test

Note: This figure reports the test proposed by Conley et al. (2012). This test allows the IV to have a direct effect on the outcome and on that basis, it examines if the IV estimate remains statistically significant. In this figure, we plot the confidence interval of the IV estimate against the hypothesized direct effect of the IV (relative to the total reduced effect of the IV).

unobserved confounders must contribute to at least 40–50 percent of Z_i's reduced effect in order to accept a null effect of outmigration on labor unrest. We believe this fraction is unrealistically high, and our instrumentation procedure should be valid.[19]

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[19]Conley et al. (2012) do not provide a rule-of-thumb cutoff for $\bar{\psi}/\psi_{RF}$. However, using Conley et al. (2012)'s approach, researchers have demonstrated the robustness of their 2SLS estimates given the following $\bar{\psi}/\psi_{RF}$ ratios: 0.3 in Fatás and Mihov (2013) and 0.46 in Bentzen et al. (2017).

L Replication of An et al. (2024)

In this section, we replicate the main results in An et al. (2024): the effects of *hukou* reform on workforce participation, wages, and access to social security (ASS), which we also investigate in Table 8. They also use the CMDS data and their findings are: (i) the reform has no effect on migrants' workforce participation; (ii) the reform significantly lowers wages; and (iii) the reform reduces ASS. However, we do not find (ii) and (iii). We show how this discrepancy arises.

There are two major differences between their and our empirical implementations.

1. *Reform Status Definition.* To measure reform status, we use the total urban population that includes both natives and migrants from the Urban Construction Yearbook (UCSY), whereas An et al. (2024) use only the native urban population.
2. *Identification Strategy.* An et al. (2024) implement a DiD design as specified by Equation 1 that in essence compares . In contrast, our strategy given by Equation 2 combines DiD and RD designs (DiDC).

Table A19 explores how results change due to these decisions. To avoid other sources of differences, e.g., sample construction, we produce these results using the sample in An et al. (2024)'s replication package. Panel A adopts the An et al. (2024)'s definition of reform status, whereas Panel A uses our definition. Columns (1)–(3) report DiD estimates, and Columns (4)–(6) report the DiDC estimates. Clearly, different definitions of reform status do change the reform and non-reform groups. We, by using the total urban population, define 10 less prefectures as reform ones. Nonetheless, different definitions of reform status do not change the results markedly. The major change is due to identification strategy. Once flexible polynomial functions of log urban population are included, there are no longer discernible effects of the *hukou* reform on wages and ASS. This pattern also appears when we use our own working sample (see Table A20).

The DiDC strategy may produce cleaner, more reliable results than a DiD design as it flexibly controls heterogeneity due to urban sizes. Also, we find it difficult to reconcile the *hukou* reform's negative effects on wages and ASS. An et al. (2024) interpret them as reflecting that the reform has induced labor inflow and thus reduces wages and imposes pressure to the local social security system. However, as we show in Appendix G.4, the reform in fact has at most a zero effect on population. Taken together, we argue that the *hukou* reform does not affect wages and ASS much.

Table A19. Replicating Main Results of An et al. (2024)

	(1) Working	(2) Log wage	(3) ASS	(4) Working	(5) Log wage	(6) ASS
Panel A: An et al.'s definition of treatment						
Reform (An et al.) × Post	0.006	-0.077***	-0.041**	-0.001	0.018	0.008
	(0.006)	(0.018)	(0.018)	(0.009)	(0.021)	(0.020)
Control mean	0.872	8.131	0.526	0.872	8.131	0.526
Sample period	2011–17	2011–17	2011, 13, 16	2011–17	2011–17	2011, 13, 16
No. prefectures (No. reform prefectures)	267 (241)	267 (241)	266 (239)	267 (241)	267 (241)	266 (239)
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Polynomials				Yes	Yes	Yes
Observations	788,219	642,700	144,145	788,219	642,700	144,145
Panel B: Our definition of treatment						
Reform × Post	0.007	-0.087***	-0.038**	0.004	-0.004	0.010
	(0.006)	(0.016)	(0.018)	(0.009)	(0.021)	(0.027)
Control mean	0.872	8.131	0.526	0.872	8.131	0.526
Sample period	2011–17	2011–17	2011, 13, 16	2011–17	2011–17	2011, 13, 16
No. prefectures (No. reform prefectures)	267 (230)	267 (230)	266 (228)	267 (230)	267 (230)	266 (228)
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Polynomials				Yes	Yes	Yes
Observations	788,219	642,700	144,145	788,219	642,700	144,145

Note: This table replicates the key results in An et al. (2024). ASS = access to social security. Standard errors clustered at the prefecture level are reported in parentheses.
*p < 0.1 ** p < 0.05 *** p < 0.01*

Table A20. DiD versus DiDC Estimates

	(1) Working	(2) Log wage	(3) ASS	(4) Working	(5) Log wage	(6) ASS
Reform × Post	-0.005	-0.066***	-0.047**	-0.003	0.003	0.006
	(0.005)	(0.013)	(0.018)	(0.007)	(0.019)	(0.029)
Control mean	0.883	8.153	0.522	0.883	8.153	0.522
Sample period	2011–18	2011–18	2011, 13, 16	2011–18	2011–18	2011, 13, 16
No. prefectures	255	255	255	255	255	255
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Polynomials				Yes	Yes	Yes
Observations	990,912	810,696	162,239	990,912	810,696	162,239

Note: This table compares the DiD and DiDC estimates for the *hukou* reform's labor market effects. The same sample is used as in Table 8. ASS = access to social security. Standard errors clustered at the prefecture level are reported in parentheses.
*p < 0.1 ** p < 0.05 *** p < 0.01*

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