

# Migrant Integration and Social Stability: Evidence from China

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This Version: August, 2025

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## Abstract

There are growing concerns that a relaxed 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 discontinuity rule based on population, we find that the reform significantly reduced labor unrest. We suggest that one important mechanism behind our finding is the enhancement of migrants' settlement intentions, which makes migrants less engaged in unrest to secure the opportunity of settlement offered by the reform. We provide evidence that the reform increased migrants' likelihood of staying in their destinations. Through a novel causal mediation analysis, we find that heightened settlement intentions can explain 61 percent of decreased labor unrest due to the reform in the immediate term and 27 percent in the long term. We find no evidence that the reform led to compositional changes among migrants, delivery of benefits to migrants, or tighter government social control. Our results highlight how migration policy can influence stability by shaping migrants' attachment to their destinations.

**Keywords:** Migration; Unrest; Settlement; Urbanization; Household Registration; China.

**JEL Codes:** D74; D79; E24; O15.

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# 1 Introduction

Public discourse and policy debates often raise the concern that migration may undermine social stability. In democracies, some voters and politicians *perceive* immigration as a source of social turmoil (e.g., crime or terrorism) (Bianchi et al., 2012; Hangartner et al., 2019; Campante et al., 2023).<sup>1</sup> The concern about instability from migration holds equal salience in autocracies. In these contexts, autocrats fear that population concentration in the seat of power (often urban centers) could foster unrest (Wallace, 2014). As a result, restrictive integration policies are often proposed to weaken migrants' ability to stay and to deter further migration.

Despite these prevalent concerns, the causal linkage between integration policy and social stability is not straightforward to assert. On the one hand, existing concerns posit that relaxing migration barriers may attract certain types of individuals more likely to engage in destabilizing activities (e.g., those with poorer labor market opportunities). However, the nature of compositional changes is fundamentally an empirical question, and thus, its influence on social stability is ambiguous. Relatedly, the literature is divided on the effect of immigration on crime.<sup>2</sup> On the other hand, relaxed migration barriers may make migrants more attached to their destinations; if the increased attachment incentivizes migrants' social integration—in the manner that longer migration duration begets destination-specific human capital investments (e.g., Adda et al., 2022)—and raises the costs of participating in unrest, then we may see a higher level of social stability. Therefore, it is an open question of how integration policy changes affect social stability.

To help answer this question, we study internal migration in China. China is an intriguing setting to study the relationship between integration policy and social stability. China's institutions governing internal migration share some similarities with the institutions of international migration elsewhere (Chan, 2019). In China, a *hukou* (household registration) system ties each person to a registration location. This registration location determines *where* the person has access to public services (e.g., public education, healthcare, subsidized housing, and social security).<sup>3</sup> There are substantial institutional barriers to transferring registration across regions, especially from rural areas to urban ones. This means that people would lose access to valuable public services once they leave their registration locations. Therefore, although movement is generally not restricted in contemporary China, migrants face substantial barriers to integration into their migration destinations, and migration tends to be temporary (Meng, 2012). Due to these features, some

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<sup>1</sup>Bianchi et al. (2012) document that from 1995 to 2003, many people in OECD countries exhibited concerns that immigrants increased crime rates; in some cases, the population share concerned about immigrant-related crime was higher than the share worried 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, terrorism, and burden on the country). Speaking to electoral consequences, Campante et al. (2020) show that during the 2014 US midterm elections, Republican members of Congress were more likely to connect the Ebola virus with immigration and terrorism in their newsletters and TV campaign ads, although Ebola had negligible impacts on American society.

<sup>2</sup>For instance, Spenkuch (2014) finds 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.

<sup>3</sup>More details about the *hukou* system are in Section 2.1.

scholars label the *hukou* system as a “local citizenship” or an “internal passport” system, despite China being a unified country (Jin and Zhang, 2023; Au and Henderson, 2006).

We exploit a natural experiment created by the nationwide reform of the *hukou* system in July 2014. This reform provided migrants an easier pathway to transfer their *hukou* to small- and medium-sized cities, gain access to local public services, and thus permanently relocate.<sup>4</sup> We study the causal effect of this *hukou* reform on social stability using data from 2011 to 2019. Social stability is measured by labor unrest, which represents workers’ collective action to protest employers and demand public attention. This is a relevant proxy for migrants’ behaviors. A large literature on labor politics in China 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). Meanwhile, the Chinese state is vigilant in monitoring and controlling labor unrest, despite the mostly apolitical nature of workers’ actions (Friedman, 2014), and the state has adopted various measures to prevent labor unrest from threatening regime survival.<sup>5</sup> As a result, migrants have to weigh the benefits of unrest against the costs of retaliation; the latter can encompass the losses from failing to seize settlement opportunities provided by the *hukou* reform.

Underpinning our research design is a population-based rule that determines reform status. The 2014 *hukou* reform only made it substantially easier to transfer *hukou* into regions with less than 3 million urban population while keeping strict restrictions in more populous regions. Exploiting this discontinuity in the urban population, we implement a difference-in-discontinuity (DiDC) design, which combines difference-in-differences and regression discontinuity designs. We estimate the causal effect of the reform by comparing the trends in labor unrest between reform and non-reform regions near the reform’s population cutoff. Identification follows if the underlying trends vary smoothly across the reform cutoff. We conduct a variety of exercises supporting the validity of our research design. First, the McCrary test shows that there is no discontinuity in the density of the urban population at the 3 million cutoff both before and after the reform. This suggests a lack of sorting into specific reform statuses. Second, we test for manipulation of urban population statistics by local officials, which can cause selection into certain reform statuses. 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 populations and a measure of local officials’ promotion prospects. Manipulation must benefit officials’ careers for it to exist, so any observed manipulation should be more significant among officials with stronger *ex-ante* promotion incentives. However, we find a null association between reported growth in urban populations and a measure of *ex-ante* promotion prospects that follows (Wang et al., 2020). This is against deliberate manipulation of urban population statistics. Furthermore, we show that our research design balances out pre-reform trends in the unrest rate and other variables that may

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<sup>4</sup>Note that the reform merely reduced the barriers to getting a local *hukou*, but did not grant migrants a local *hukou* immediately. The reform still sets some (minimum) requirements that a migrant must satisfy in order to get a local *hukou*, including housing, employment, and enrollment in the local social security system. See Section 2.1 for more details.

<sup>5</sup>We provide a more detailed discussion of labor unrest in China in Section 2.2.

influence occurrences of unrest, including population, GDP, expenditures on public security (i.e., policing), and expenditures on social security.

Our main finding is that the *hukou* reform *reduced* labor unrest by 1.419 incidents per million prime-age population (95 percent confidence interval [0.685, 2.153]). This effect is sizable: it amounts to 42 percent (95 percent confidence interval [20, 63]) of the mean of non-reform regions. We conduct several tests to show that this effect is not likely a consequence of confounding factors related to the urban population or a statistical artifact. First, we perform a placebo test that estimates the effects of the reform at other population cutoffs. The trends in labor unrest only discontinuously vary at the true cutoff of 3 million but not at other cutoffs, suggesting that our finding is not driven by confounders generally associated with population. Second, we show that the reform has no discernible effects on both total population and urban population during the period we study, suggesting that our results are not explained by changes in population. Lastly, as our measure of labor unrest is based on online information, we present evidence that our results are not driven by differential patterns in reporting local events, internet censorship, or self-censorship.

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

We then go on to carefully examine the mechanisms through which the *hukou* reform reduced labor unrest. We focus on one particularly important mechanism: the *hukou* reform may heighten migrants' intentions to settle in their destinations by offering the previously unattainable opportunity of permanent settlement; as a result, migrants opt against participating in labor unrest if they fear that government or employer retaliation may make them fail to secure the opportunity of permanent settlement. We present evidence consistent with this hypothesis. Analyzing a panel dataset with information on migration destinations, we find that migrants in a destination eligible for the *hukou* reform were significantly less likely to leave after the reform, indicating that they had stronger intentions to settle.

Besides establishing the link between the *hukou* reform and settlement intentions, we also formalize the link between heightened settlement intentions and decreased labor unrest. We do so by implementing a novel causal mediation method by which we can quantify the indirect effect of the *hukou* reform on labor unrest through increasing settlement intentions. To estimate the treatment effect mediated by a mechanism, the conventional practice, popularized by **Baron and Kenny (1986)**, relies on the comparison of coefficients on treatment (here, the *hukou* reform) between a regression model that excludes the mechanism variable and a regression model that

includes the mechanism variable. The change in the coefficient on treatment is then viewed as the indirect treatment effect that goes through the proposed mechanism. The magnitude of the indirect treatment effect measures the importance of a mechanism. Though intuitive, this approach requires strong assumptions. A researcher has to assume that: (i) conditional on the treatment, the mechanism variable is exogenous with respect to other unobserved determinants of the outcome; and (ii) the mechanism effect on the outcome is homogeneous (e.g., Imai et al., 2011). The first assumption is a key one: it ensures that the average mechanism effect can be identified. The second assumption allows the average mechanism effect estimated at the aggregate level to be applied at the individual level. Assumption (i) is often not plausible due to the existence of other post-treatment confounders related to the mechanism variable. In our method, we relax this assumption and employ an instrumental variable (IV) for the mechanism variable to identify the mechanism effect. We maintain assumption (ii) so that the IV-identified average mechanism effect in one population can be extrapolated to another population. By doing so, the simplicity of the conventional approach can be preserved: the change in the coefficient on treatment, after including the instrumented mechanism variable in a regression, can still be interpreted as the indirect treatment effect via a proposed mechanism. If the homogeneity assumption is relaxed, there are two biases in the estimate of the indirect effect: (i) the bias due to the difference between the IV-identified average mechanism effect and population average mechanism effect (“IV bias”), and (ii) the bias due to attributing an aggregate level average mechanism effect to all heterogeneous individuals (“ecological bias”, Glynn, 2012). We develop a simple sensitivity test for the conclusion about the importance of a mechanism when the homogeneity assumption is relaxed and the two biases arise.

When applying this method to our case, we construct a shift-share instrumental variable (IV) that leverages variation in trade shocks in migrants’ origins (i.e., registration locations). This IV is strongly associated with outmigration patterns and passes the balance tests recommended by Borusyak and Hull (2024). With this instrumentation, our results show that our measure of heightened settlement intentions (or equivalently, decreased outmigration rates) can account for 60.7 percent of the *hukou* reform’s total effect on labor unrest in the short term (2011–2015) and 27.3 percent in the long term (2011–2019). These results rest on the assumption that the effects of the mechanism variable on the outcome are homogeneous. When relaxing this assumption and implementing the simple sensitivity test we develop, the results suggest that even if a significant amount of IV bias and ecological bias is allowed, we can still conclude that heightened settlement intentions explain a nontrivial fraction of decreased labor unrest due to the *hukou* reform.

We reject other alternative mechanisms through which the *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) tightening of local governments’ social control.

In sum, we show evidence that the relaxation of integration barriers—in our case, driven by China’s *hukou* reform—reduces social unrest due to migrants’ stronger settlement intentions that discourage participation. It is important to consider the generalizability of our findings. First, for settlement intentions to play a role, some authority (oftentimes, the state) should exert a strong influence over migrants’ welfare, thereby creating credible threats to deter participation in unrest when migrants intend to stay longer. This condition is met in China, where the state controls various resources crucial to livelihoods. We envision that the condition can hold broadly in autocracies. Even in democracies, there can be scope for migrants’ settlement intentions to matter. Immigrants’ rights, especially for undocumented immigrants, are strongly influenced by immigration law enforcement (e.g., deportations). For instance, [Gonçalves et al. \(2024\)](#) argue that heightened immigration enforcement in the US discourages immigrant victims from reporting offenses, suggesting that the state’s threats can effectively alter the behaviors of immigrants (who presumably would like to stay in a perceivable future) even at the cost of victimization. Second, in this paper, we focus on internal migrants. Compared to international migrants, they may be more responsive to the relaxation of institutional barriers to integration, as they face fewer cultural barriers to integration. In contrast, international migrants may not experience a significant increase in settlement intentions even when institutional barriers are eased. However, some international migrants who have a smaller cultural distance from the natives can be similarly sensitive to the relaxation of institutional barriers to integration as internal migrants that we study in this paper. Taken together, our findings are most applicable to the group of migrants who depend on the state for their livelihoods and face primarily institutional barriers to integration.

Our paper engages with several strands of literature. First and foremost, it speaks to the relationship between migration policy and social stability. A noticeable body of literature in this domain focuses on the effect of international migration on crime ([Spenkuch, 2014](#); [Bianchi et al., 2012](#); [Bell et al., 2013](#); [Nunziata, 2015](#); [Ajzenman et al., 2023](#)).<sup>6</sup> This literature overall finds mixed effects. When there are significant effects, they relate to financial crime and the subgroup of immigrants characterized by poor labor market potential. However, people nonetheless view immigration as a source of crime and support strict immigration enforcement ([Nunziata, 2015](#); [Ajzenman et al., 2023](#); [Dinas and van Spanje, 2011](#)). Beyond migrants’ characteristics, our paper provides evidence that relaxed migration barriers could increase migrants’ expected length of stay; the longer planned stay can induce changes in their sociopolitical behaviors, leading them to take actions facilitating their integration and generating benefits in the long run. This is in the spirit of how stay plans affect migrants’ economic choices for assimilation ([Dustmann and Görlach, 2016](#)).

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<sup>6</sup>Two points should be noted when comparing our paper with this literature. First, we study internal migration instead of international migration. However, as discussed above, the *hukou* system makes internal migration in China similar to international migration elsewhere. Second, the measure of social stability in our paper is labor unrest. Unlike the illegal nature of crime, the labor unrest we study includes workers’ actions for their legal rights. However, China’s fears of labor unrest resemble other countries’ fears of crime, thus, migrants in both settings may similarly take into account government responses to their actions. Given this, we can still draw some lessons from Chinese migrants’ calculations of participation in unrest.



Second, our paper contributes to a small literature on China’s *hukou* system. Exploiting variation from different episodes of *hukou* reforms, several papers have studied the *economic* consequences of migration barriers induced by the *hukou* system, including labor market outcomes, marriage market matching, and productivity (e.g., [An et al., 2024](#); [Han et al., 2015](#); [Ngai et al., 2019](#)). Scholars largely agree that the *hukou* system causes significant efficiency losses, and some have hinted that the system continues to exist due to political constraints.<sup>7</sup> However, little attention has been paid to the *hukou* system’s *political* implications. To the best of our knowledge, we are the first to examine the impact of a *hukou* reform on social stability. In this regard, we also contribute to the literature on political control, namely, the tactics used by the state to induce citizen compliance (for a review, see [Hassan et al., 2022](#)). One prominent example of nonviolent tactics is buying hearts and minds through government transfers and employment (e.g., [Pan, 2020](#); [Rosenfeld, 2021](#); [Fish, 1905](#); [Borjas, 1980](#); [Kersting, 2023](#)).

Last but not least, our paper adds to the econometric literature on causal mediation, which focuses on formally disentangling how much of the average treatment effect can be attributed to the treatment effect via a mechanism, i.e., the indirect treatment effect ([Baron and Kenny, 1986](#); [Imai et al., 2011](#); [Pearl, 2009](#)). As mentioned above, one key challenge is to identify the effect of the mechanism variable on the outcome. We overcome this challenge using an IV. We are not the first to use an IV for causal mediation. [Imai et al. \(2011\)](#) provide a framework with homogeneous effects and a single IV for the mechanism variable, and they show that the comparison between coefficients on treatment in regressions with and without the mechanism variables identifies the indirect treatment effect. [Frölich and Huber \(2017\)](#) present a framework of non-parametric identification in conjunction with structural assumptions. Our method preserves the simple, regression-based approach in [Imai et al. \(2011\)](#), and proposes a sensitivity test to assess the robustness of the conclusion on a mechanism’s importance to bias introduced by instrumentation.

The remainder of this paper is organized as follows. Section 2 provides information on the institutional context. Section 3 describes our data and research design. Section 4 reports the effect of the *hukou* reform on labor unrest. Section 5 explores underlying mechanisms. Section 6 concludes. Additional results and discussions can be found in the [Online Appendices](#).

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<sup>7</sup>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, such as [Adamopoulos et al. \(2024\)](#) and [Gai et al. \(2024\)](#) among others.

## 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 obstructs free migration. We refer interested readers to [Chan \(2019\)](#) for a more comprehensive account. We then 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. Each Chinese citizen is assigned a *hukou* certificate upon birth, tying them to a locality (typically their parents' registration locality). Based on this registration locality, the system determines *where* a person is eligible for state transfers and public services. A person can only access state transfers and public services in their registration locality, even if their *de facto* residential locality is different. For many years, there were two types of *hukou*: agricultural and non-agricultural. Rural residents typically were issued an agricultural *hukou*,<sup>8</sup> which brought with it an allotment of land for cultivation to feed themselves and access to some social services provided by their rural localities. Urban residents obtained a non-agricultural *hukou* and were expected to work in factory or office jobs. Residents holding urban *hukou* could access district-funded social benefits, many of which were job-related and included food rations, subsidized medical care, education for their children, and social assistance. Starting from the 1990s, some localities gradually removed the distinction in *hukou* types. This removal was eventually extended to the entire country in 2014—by the reform we study in this paper. Despite removing the urban/rural differentiation, the key aspect of the *hukou* system remains unchanged: citizens can only access state transfers and public services in their registration localities.

**The *Hukou* System as a Migration Barrier.** Under Mao, the *hukou* system was created to restrict population mobility—people were expected to stay in their registration localities—which facilitated the government's urban-biased industrialization strategy, extracting resources from vast rural areas to subsidize urban areas. Transferring *hukou* across regions was difficult, especially from rural to urban localities, making permanent migration nearly impossible. Successful transfers were effectively only possible via state jobs, military service, and higher education. Even short-term trips required permits from the police; otherwise, the traveler would be expelled back to their *hukou* locality ([Cheng and Selden, 1994](#)). Since 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 economic reforms, the *hukou* system was gradually relaxed, eventually allowing free movement across the country by the late 1990s. However, the *hukou* system continues to exist under the management of local governments. Local governments have the incentive to limit transfers into their jurisdictions to avoid increasing their fiscal burden for social services

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<sup>8</sup>One exception is government officials who worked in rural areas.



provision. Limited transfers of *hukou* are made available to attract residents with financial means, such as investors, home buyers, or highly educated professionals. As such, transfers of *hukou* remain difficult, and the inability to transfer *hukou* constitutes a considerable cost of migration. Eli Friedman, a renowned scholar in labor politics in China, nicely summarizes this phenomenon as the “urbanization of labor [rather than people],”—that is, people are welcome to move to and work in cities, but they are not expected to permanently settle (Friedman, 2022). Indeed, the average Chinese migrant only stays in the destination locality for 5–7 years (Meng (2012) and our own calculations).

**The 2014 Reform.** In July 2014, the Chinese central government initiated a nationwide reform to the *hukou* system (State Council, 2014a). Unlike previous local reforms to the *hukou* system, this reform 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, this reform has a population-based rule for granting *hukou* transfers to urban areas, 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.<sup>9</sup> As a development strategy, the central government’s objective is to push the urbanization of medium- and small-sized cities while maintaining strict control over the expansion of large cities. Overall, the criteria for granting local *hukou* transfers are much stricter for larger cities. Large cities (those with an urban population exceeding 3 million) are directed to maintain tight control of *hukou* transfers using a points-based system that only incorporates select migrants, much like that seen in international migration settings.<sup>10</sup> In contrast, the criteria are much more lenient in medium and small cities (i.e., those with less than 3 million urban population). These smaller cities are expected to accept a much broader base of migrants so long as the migrant has a stable job, stable residence, and a minimum length of enrollment in local social security within the city.<sup>11</sup> We therefore focus on the 3 million urban population cutoff at which the criteria for granting local *hukou* are substantially relaxed. In fact, the central government reiterated in 2016 and 2024 that cities with an urban population below 3 million must abolish *all* barriers to *hukou* transfers (State Council, 2016, 2024).

## 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, a number of studies on China’s labor politics

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<sup>9</sup>These cutoffs come from the Chinese government’s official categorization of city sizes (State Council, 2014b).

<sup>10</sup>For example, Shanghai assesses migrants across several dimensions, including age, educational attainment, technical skills, enrollment in local social security, potential contributions to local development, and any history of penalties (Shanghai Government, 2015).

<sup>11</sup>Having a stable job often means having an employment contract or being a business owner (with minimum requirements on tax payments and/or registered capital). Having a stable residence means either having a rental contract registered with the government or owning an apartment.

**Table 1.** Summary of the 2014 *Hukou* Reform

Urban Population	Provisions on granting local <i>hukou</i>
> 5 million	Points-based rules must be established to select migrants.
3–5 million	Rules must be stricter than cities of the next tier. Cities are urged to establish points-based rules.
1–3 million	Local <i>hukou</i> is granted for migrants with a stable job, stable residence, and 1–5 years enrollment in basic social security.
0.5–1 million	Local <i>hukou</i> is granted for migrants with a stable job, stable residence, and 1–3 years enrollment in basic social security.
< 0.5 million	Local <i>hukou</i> is granted for migrants with a stable job and stable residence.

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

stress the role of institutional discrimination against migrant labor that results from 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, fuels migrants' grievances and thus contributes to the occurrence 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 a lack of official data on migrant labor's participation in labor unrest, many scholars have provided anecdotes from fieldwork suggesting that migrant workers do indeed make up the majority of participants in labor unrest, especially those offensive actions that demand more interests other than defending minimum rights (Friedman, 2014; Rho, 2023; Goebel, 2019).<sup>12</sup> Corroborating this view, Figure A1 shows a positive relationship between labor unrest and the share of migrants in a region.

The Chinese state has been increasingly vigilant about labor unrest, especially in the last decade (Franceschini and Nesossi, 2018; Rho, 2023). Lorentzen et al. (2013) argue that the central government strategically tolerates labor unrest in which workers voice demands for their rights and interests because such unrest can serve as a signal for the central government to identify discontented groups. With this information, the government can then allocate resources to address grievances and manage local officials accordingly. However, the acceptable space of unrest has been codified in informal rules with an implicit warning that whoever crosses the boundary of acceptable protests will be repressed. "Unacceptable" protests include mass collective actions that threaten 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

<sup>12</sup>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), migrant workers' labor disputes comprised nearly 70 percent of all labor disputes in Beijing in 2010. Goebel (2019) analyzes social unrest on social media and finds that migrant workers have engaged in the largest number of protests.

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 in China.<sup>13</sup> We also consider 4 provincial-level municipalities: Beijing, Tianjin, Shanghai, and Chongqing. For brevity, we refer to these municipalities as 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, allowing us to define the prefecture’s reform status (discussed next).<sup>14</sup> 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, prefectures with urban populations below 3 million are subject to the relaxed migration barriers. Thus, to define each prefecture’s reform status, it is crucial to consider how the Chinese government counts the population. According to National Bureau of Statistics (2008), a prefecture’s urban population consists of all residents who have been in urban districts for more than six months. This includes both natives and migrants, regardless of *hukou* registration status. For the purposes of this paper, we use population data from 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 National Bureau of Statistics (2008).<sup>15</sup> We use the urban population in 2014, the year when the reform took place, to define the prefecture’s reform status. Our binary treatment variable,  $Reform_i$ , equals one if prefecture  $i$ ’s 2014 urban population is below 3 million, and equals zero otherwise. 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 this, we conduct

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<sup>13</sup>This is based on the 2010 delineation; there are no significant changes to the delineation of prefectures over our sample period.

<sup>14</sup>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.

<sup>15</sup>The UCSY reports the urban native and urban migrant populations separately. We aggregate the two groups to calculate the total urban population.

a thorough review of *hukou* policy documents issued by provincial and prefectural governments.<sup>16</sup> 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 the manually coded and population-based reform statuses.<sup>17</sup> Therefore, in the subsequent analysis, we rely on the population-based reform status. However, to alleviate concerns about the discrepancies in reform status, we show the robustness of our results to excluding those 17 prefectures with status discrepancies (see Section 4.3).

Second, our definition is based on the urban population in 2014. A reader may wonder whether prefectural governments adjust their migration policies as the urban population moves above or below the 3 million threshold. To the best of our knowledge, this is not the case. According to our review of local governments’ documents regarding the *hukou* reform, by 2015 most prefectures had guidelines for implementing the central government’s directive, and we do not observe amendments made in 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 worker collective actions across China since 2011.<sup>18</sup> Our sample consists of collective actions from 2011 to 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 (e.g., Hernández, 2016) and used in research on social unrest in China (e.g., Campante et al., 2023; Qin et al., 2024).

The CLB uses human coders to collect information on unrest events primarily from China’s domestic social media platforms: Weibo, WeChat, Douyin, Kuaishou, and others. The coders verify the accuracy of collected information and record only those events that have complete information on the location, date, cause(s), industry, and relevant company. The CLB data report 11,733 labor collective action events between 2011 and 2019.<sup>19</sup>

Given how the CLB dataset is built, events in the dataset should be considered arguably more severe labor conflicts where workers take to the streets and demand public attention. For 83 percent of the events in the dataset, workers conduct demonstrations in public spaces in the forms of

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<sup>16</sup>We collect these documents from government websites and news reports, as well as a database on *hukou* reforms built by Zhang and Lu (2019). Appendix D offers an example of these documents.

<sup>17</sup>Table A7 in Appendix D tabulates the discrepancies. Eight small prefectures (urban population < 3 million) opted to maintain restrictions, whereas nine large prefectures (urban population > 3 million) opted to relax migration barriers. We discuss possible reasons for these discrepancies. Some small prefectures may have opted to maintain barriers because of their political importance or anticipated population growth. For instance, the Hebei provincial government explicitly required Langfang prefecture to maintain restrictions because the prefecture is adjacent to Beijing, meaning that it houses many of Beijing’s migrant workers. The Guangdong provincial government required two prefectures in the Pearl River Delta, Zhuhai and Zhongshan, to maintain restrictions likely because of recent population growth. However, it is less clear why those nine large prefectures opted to relax restrictions.

<sup>18</sup>Data are available from the CLB website: <https://clb.org.hk/en>.

<sup>19</sup>The vast majority (11,451 events) occurred in the 287 prefectures included in our main sample.

protests, marches, sit-ins, traffic obstruction, and even suicide threats. Recall from Section 2.2 that migrant workers are the major participants in labor unrest and especially these demonstrative actions.

A natural question is to what degree CLB data reflect underlying patterns of labor conflict in China. We show that the events in the CLB data exhibit similar trends to those seen in other data sources of labor conflict. We draw a comparison to the Global Database of Events, Language, and Tone (GDELT), a commonly used dataset on social unrest at the global level (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 any event as a labor unrest event if it falls in the “Protest” category and has labor recorded as one of the involved parties. 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: the CLB identifies 11,733 events, whereas GDELT identifies only 4,681 events. Nonetheless, both datasets display quite similar national trends in labor unrest (see Figure A2).<sup>20</sup>

**Auxiliary Data.** We use multiple auxiliary datasets to validate the research design and explore mechanisms. They include prefecture-level covariates collected from population censuses, migrant surveys, trade data, and biographical data on local officials, among others. Appendix E describes these data sources, and we introduce them at the point that 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 the *hukou* reform, an intuitive strategy is a difference-in-differences (DiD) design comparing the trajectories of unrest between reform and non-reform prefectures. We can implement a DiD design using the following two-way fixed effects (TWFE) model:

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

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 (the first year the reform went into effect) onward. We include prefecture and year fixed effects, denoted  $\lambda_i$  and  $\mu_t$ , respectively. We cluster the error term,  $\varepsilon_{it}$ , at the prefecture level.

The ordinary least squares (OLS) estimand,  $\beta$ , identifies an average causal effect of the *hukou* reform on labor unrest, provided that a parallel trends assumption is met: that reform and non-reform prefectures would have shared similar trends in unrest in the absence of the reform. This

<sup>20</sup>GDELT is not suitable for a regression analysis at the subnational level. GDELT only geocodes 1,112 labor unrest events at the prefecture level, while the remainder are recorded using either the centroid of China or a province.

assumption is questionable in our setting as it requires more populous regions to be on parallel trends in labor unrest with less populous regions. This assumption is violated since the urban population itself, which determines the reform status, can be associated with differential patterns in unrest. Evidence from the literature supports this notion. For example, [Acemoglu et al. \(2020\)](#) document 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. The regression model is specified as follows:

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

The newly included variable,  $\tilde{p}_i = \log(3) - \log(P_{i,2014})$ , 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.<sup>21</sup>  $f$  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) designs, creating a difference-in-discontinuity (DiDC) design where  $\tilde{p}_i$  is the running variable.<sup>22</sup> To estimate Equation 2, we follow [Gelman and Imbens \(2019\)](#) and let  $f$  be a first-order polynomial function. In most of our analysis, we estimate results using the full sample of prefectures. Because the number of reform prefectures far exceeds non-reform prefectures (250 versus 37), restricting our sample to a narrow bandwidth around  $\tilde{p}_i = 0$  may exclude a large portion of non-reform prefectures, costing us much in the way of 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 choices of alternative polynomial orders, bandwidths, and kernels.

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

<sup>21</sup>We consider the deviation in logs because the distribution of urban population  $P_{i,2014}$  is very skewed (see Figure A4). In Table A1, we show that using the deviation in levels,  $3 - P_{i,2014}$ , to construct polynomials produces similar results.

<sup>22</sup>Compared to the RD design, one strength of the DiDC design is that it can improve the precision of estimates because it exploits the panel structure to control for time-invariant unobservables. However, the results are similar if we implement an RD design for each period (see Figure A3, and compare it with Figure 2).



## 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. Section 4.2 then reports the estimated effects of the *hukou* reform on labor unrest using the DiDC design. Section 4.3 presents a battery of robustness checks.

### 4.1 Validity of the Research Design

Recall that our research design leverages variation in labor unrest trends across the reform cutoff of 3 million urban population, controlling for trends in the 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 address them using tools from the RD literature (e.g., the McCrary test and balance tests) as well as tests specific to the Chinese context.

#### 4.1.1 Concern 1: Confounding Policies Correlated with Urban Population

If the urban population also determines policies other than the *hukou* reform, our 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 existed prior to 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 later, 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 that vary by urban population and were 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 maintained by Peking University and frequently used in research on policy-making in China (Wang and Yang, 2021; Tian, 2024)—to identify policies that mention “urban population” or other similar terms.

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. We present null effects, which indicate that our estimates are not confounded by other policies correlated with the urban population.

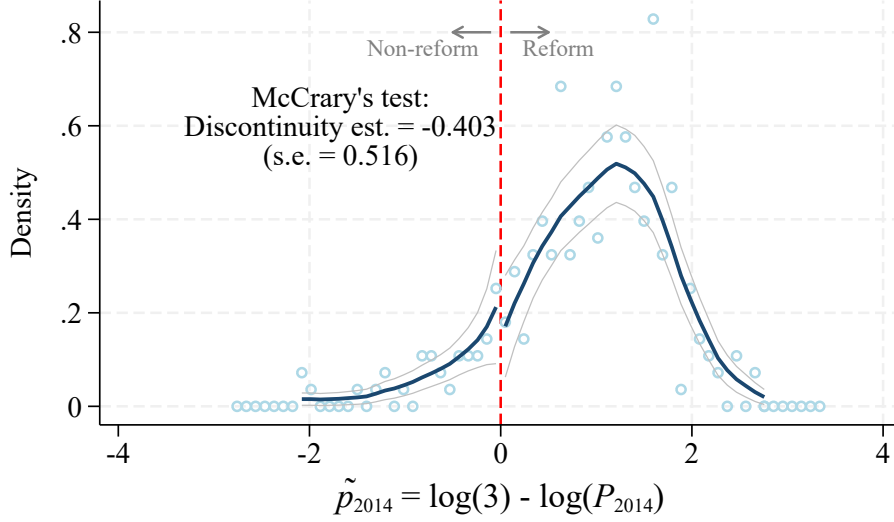
#### 4.1.2 Concern 2: Local Officials’ Manipulation of Urban Population to Select Reform Status

The validity of RD also requires that the agents (in our case, the prefectural governments) cannot and do not precisely manipulate urban population statistics to sort into certain reform status (Lee and Lemieux, 2010). In principle, manipulation is not infeasible, as prefectural governments can preemptively influence the statistical bureaus that report urban population. We perform several checks to rule out the presence of manipulation.

First, it is impractical to substantially manipulate urban population for specific reform status because the new urban population cannot change too greatly from 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 prefectures had urban population fall from above 3 million to below 3 million.

Second, if there is systematic manipulation of urban population to select certain reform status due to potential benefits, we would expect a significant bunching near the cutoff of 3 million (or equivalently,  $\tilde{p}_i = 0$ ). However, we do not detect this phenomenon. Figure 1 presents the density of the running variable  $\tilde{p}_i = 0$ . The McCrary (2008) test confirms the smoothness of the density function around  $\tilde{p}_i = 0$ , suggesting a lack of manipulation. In Figure A5, we also examine the density of the running variable defined using the 2015 urban population, and we again do not detect a discontinuity in the density. Therefore, it is not the case that prefectural governments manipulated urban population statistics *ex post* to make their decision to take up the reform appear to be more legitimate.

Lastly, for manipulation to happen in practice, there ought to be some benefits for local officials to do so. In the Chinese context, such benefits are predominantly bureaucratic 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., Wang et al., 2020; He et al., 2020; Jia, 2024). Zeng and Zhou (2024) find that promotion-motivated local officials may manipulate GDP statistics to deliver better observed performance. However, unlike with the manipulation of GDP statistics, there is no strong argument that the upper-level government would evaluate local officials directly based on the reported (changes to) urban population. An indirect argument is that local officials may want to *dodge* the *hukou* reform to avoid social instability due to increased population inflows. This may improve the official’s chance of promotion, given that stability maintenance has been



**Figure 1.** Density of Centered Log Urban Population of 2014

Note: This figure depicts the density of centered log urban population of 2014,  $\tilde{p}_{i,2014}$ . We report the McCrary's test of density discontinuity at  $\tilde{p}_{i,2014} = 0$ .

widely seen as necessary for career advancement (Edin, 2003). One immediate implication of this argument is that officials with stronger promotion incentives may over-report urban population growth. 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). We find this index 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 requires that, without the *hukou* reform, *trends* in the determinants of labor unrest are similar between prefectures near the 3 million cutoff. This assumption is untestable because we do not observe the outcomes in the absence of the reform. Nonetheless, we can assess the plausibility of the identification assumption by examining pretrends. Specifically, we run the following RD regression:

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

Here,  $\Delta W_{it}$  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 begin by estimating Equation 3 without including the polynomial function  $f(\tilde{p}_i; \zeta_{\text{Reform},t})$ ; thus,  $\alpha_1$  captures the average difference in pretrends between reform and non-reform prefectures. Columns (1) and (2) report the estimated  $\alpha_1$  and the standard error. We find that, on average, reform

prefectures 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.

We next estimate Equation 3, controlling for polynomials  $f(\tilde{p}_i; \zeta_{Reform,t})$ . As in RD designs,  $\alpha_1$  captures pre-trend differences between prefectures above and below the reform cutoff. Columns (3) and (4) in Panel A of Table 2 report the estimated  $\alpha_1$  and its standard error. By comparing prefectures barely eligible for the reform with those barely ineligible, controlling for the polynomial substantially shrinks the pretrends differences found in Columns (1) and (2) and eliminates all 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 in Figure A6A.

The checks for pretrends lend support to our research design. We also examine the differences in predetermined characteristics between reform and non-reform prefectures to better understand the differences between them. Specifically, we estimate a cross-sectional variant of Equation 3 where the dependent variable is a characteristic in the base year (2010, the most recent year prior to our sample period for which a population census is available). Inspired by existing research on unrest, we examine a set of population-level variables 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 the 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. 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 A6B visualizes the RD regressions reported in Columns (3) and (4), suggesting that imbalances are likely due to outliers at the right tail of  $\tilde{p}_i$ . As our research design only requires balance in underlying trends of unrest, it allows for imbalance in covariates so 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 prefectures from 2011 to 2019. Figure 2A depicts the time series of average unrest rates separately for reform and non-reform prefectures (solid blue and red lines,

**Table 2.** Examining Smoothness in Covariates

	(1)	(2)	(3)	(4)
Dependent	Coef. on <i>Reform</i>	SE	Coef. on <i>Reform</i>	SE
<b>Panel A: Pretrends (2011-2012, 2012-2013)</b>				
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	
<b>Panel B: Predetermined characteristics (2010)</b>				
Share of males	-0.001	(0.002)	-0.003	(0.003)
Share of migrants	-0.166***	(0.031)	-0.067*	(0.036)
Share of urban residents	-0.242***	(0.025)	-0.122***	(0.037)
Share of construction workers	-0.006	(0.004)	0.008	(0.006)
Share of manufacturing workers	-0.140***	(0.029)	-0.054	(0.042)
Share of college students	-0.094***	(0.011)	-0.054***	(0.016)
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  $\tilde{p}_{i,2014}$  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.

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

respectively). We also present the difference between the two groups (dashed green line). Clearly, during the period under 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 at in Table 2. This negative gap enlarges substantially after the reform, yielding a relative decrease in reform prefectures' unrest rates against overall trends.<sup>23,24</sup> In Figure 2B, this pattern is confirmed by the estimates of a TWFE

<sup>23</sup>We note that 2017 is an exception, possibly due to the rather special political environment. The 19th National Congress of the Chinese Communist Party was held at the end of the year. The uniform enforcement of social control nationwide eliminated any potential regional differences in unrest rates.

<sup>24</sup>In Figure A7A, 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.

event-study model (the dashed, light blue line).<sup>25</sup> As noted in Sections 3.2 and 4.1, this strategy cannot credibly estimate the *hukou* reform’s causal effect due to violations of the parallel trends assumption. We thus turn to our preferred research design to obtain a more credible estimate. The solid, dark blue line in Figure 2B reports the estimates from an event-study model that adds polynomial controls  $f(\tilde{p}_i; \zeta_{Reform,t})$ . Comparing prefectures around the reform cutoff, we see there are no differential trends in unrest leading up to the center’s reform initiative. After the reform goes into effect, reform prefectures experience a relative decline in unrest rates. To further evaluate the significance of this trend break, we implement a sensitivity test developed by Rambachan and Roth (2023), with results reported in Appendix G.1. The test extrapolates the differential trends indicated by estimated pretrends to the post-reform period and examines, conditional on the extrapolated trends, whether the post-reform effects are still statistically significant. The results imply that one 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 and near the reform cutoff, resulting in biased estimates. To alleviate this concern, we restrict the sample to a narrow bandwidth around the reform cutoff, which we refer to as the “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. In this narrow sample, the reform and non-reform prefectures share very similar trends and even levels of unrest rates before the reform initiative, but reform prefectures have much lower unrest after the reform.<sup>26</sup> This is evident from event-study estimates in Figure 3B.

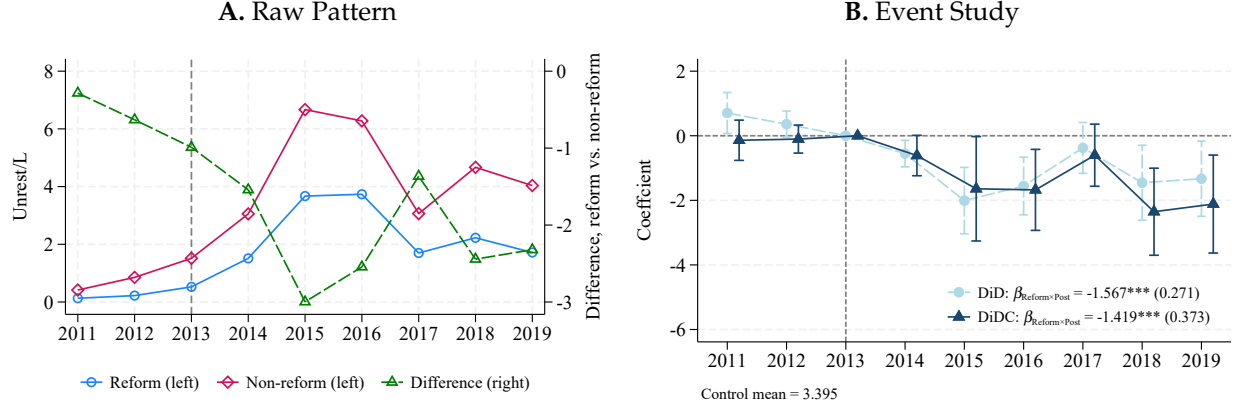
Table 3 summarizes the results for both the full and narrow samples. As the small number of non-reform prefectures may limit the validity of asymptotic testing, we also report  $p$ -values calculated from permutation tests. The permutation  $p$ -values confirm the significance of our estimates and show that the *hukou* reform had a strong effect on decreasing labor unrest rates. For our preferred specification (Column (2)), the point estimate implies that reform prefectures experienced 1.419 fewer unrest incidents per million prime-age population relative to non-reform prefectures. This is a sizable effect: the magnitude of the point estimate amounts to about 42 percent of the mean of non-reform prefectures.<sup>27</sup>

<sup>25</sup>The estimating equation here is  $\frac{Unrest_{it}}{L_{i,2010}} = \sum_{s \neq 2013} \beta_s (Reform_i \times D_s) + \lambda_i + \mu_t + \varepsilon_{it}$ , where  $D_s$  is a dummy variable that equals one for year  $s$  and zero otherwise, with 2013 as the omitted reference group.

<sup>26</sup>In Figure A7B compares the distributions of unrest rates before and after the reform. In the post-reform era, the distribution of unrest rates in reform prefectures is significantly further to the left of the distribution in non-reform prefectures, whereas the distributions of pre-reform unrest rates are balanced.

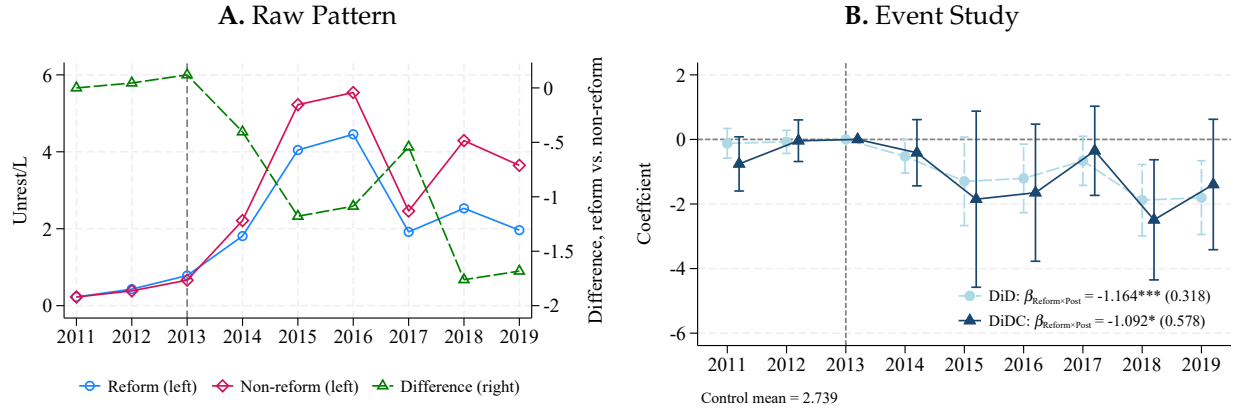
<sup>27</sup>We note that the 95 percent confidence intervals include a wide range of values. However, one can be 95 percent confident that the *hukou* reform decreased the labor unrest rate by 0.685 to 2.153 incidents per million prime-age population. This amounts to 20–63 percent of the average labor unrest rate in non-reform prefectures.





**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 the 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 point estimates, and the caps are 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 the 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 point estimates, and the caps are the 95 percent confidence intervals. Standard errors clustered at the prefecture level are used for constructing the confidence intervals.

In Appendix G.2, we show that our results are robust to alternative empirical decisions, including choice of bandwidth, kernel, and polynomial order. Point estimates remain stable when varying bandwidths, albeit with large standard errors 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.

**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 $\times$ Post	-1.567*** (0.271)	-1.419*** (0.373)	-1.164*** (0.318)	-1.092* (0.578)
Control mean	3.395	3.395	2.739	2.739
Permutation $p$ -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
Adj. $R^2$	0.543	0.546	0.603	0.606

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.

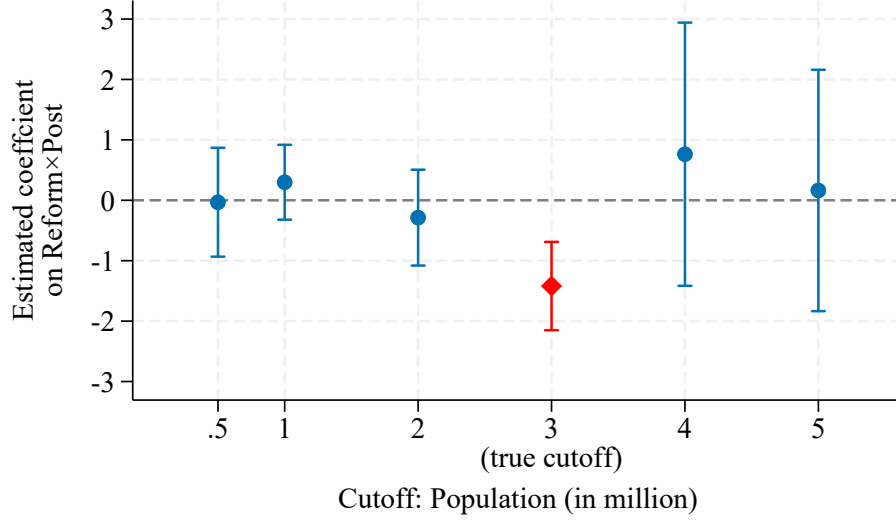
\*  $p < 0.1$  \*\*  $p < 0.05$  \*\*\*  $p < 0.01$

## 4.2.2 Alternative Interpretations

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

**Unobserved Determinants of Unrest.** One competing interpretation of 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. Nevertheless, we investigate this possibility using 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.<sup>28</sup> 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, which requires the smoothness of outcomes at points other than the cutoff of 3 million.

<sup>28</sup>For a given new cutoff  $c$ , the exercise is operationalized by redefining  $Reform_i$  as  $\mathbb{1}\{P_{i,2014} \leq c\}$  and  $\bar{p}_i$  as  $\log(c) - \log(P_{i,2014})$  in Equation 2. To avoid contamination due to treatment effect at the true cutoff of 3 million, we follow [Cattaneo and Titiunik \(2022\)](#) and only use the sample of prefectures with urban population below (above) 3 million when estimating the effect at a cutoff below (above) 3 million.



**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 prefectures below 3 million for 0.5 million, 1 million, and 3 million cutoffs, and only prefectures above 3 million 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.

**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. This would be the case if reform prefectures report fewer events than non-reform prefectures after the reform went into effect. In Appendix G.3, we present several results addressing this concern.

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

Second, we control for variation in labor unrest rates that is likely a result of China’s internet censorship. Specifically, we use as a measure of province-level censorship intensity from Qin et al. (2017) the share of deleted posts on *Weibo* (“Chinese Twitter,” China’s largest social media platform) as a proxy for the censorship apparatus’ attention or efforts within 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 in internet censorship.

Lastly, we address self-censorship as another source of differential reporting. If workers in reform prefectures are less likely to report their actions online than their counterparts in non-reform prefectures, 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 in which self-censorship would drive our results. If post-reform self-censorship rose primarily in reform prefectures that reported high pre-reform unrest rates, we would be 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 high pre-reform unrest rates would attenuate the estimated effect of the *hukou* reform on labor unrest. However, as shown by Figure A14, the estimated effect remains stable when excluding reform prefectures with 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 common concern that lax migration laws may induce instability because they increase the population. In Appendix G.4, we find that the reform had no discernible effects on population growth. If anything, the population size gap between reform and non-reform prefectures grew over time. This echoes recent evidence suggesting that Chinese migrants prefer to go to large cities because of the high wages there, and therefore a reform that makes migration small cities less costly can hardly alter the pattern (Chen and Fu, 2023).<sup>29</sup> Our baseline measure calculates the labor unrest rate using a fixed population size (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 observed decrease in labor unrest is mechanical, if most migrants had protested against the *hukou* policy, and the *hukou* reform would remove the need for protest by entitling migrants to local citizenship. We believe this interpretation is implausible in this context. 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 could not substantially eliminate resentments about the *hukou* policy. Second, protesting against the *hukou* policy is not the primary cause of observed unrest events. In the CLB data, 72 percent of protest events were over wage arrears, and 11 percent over job losses.<sup>30</sup> These are not closely associated with protests against the *hukou* policy. However, we notice that unrest events demanding social security and housing subsidies may be effectively against the *hukou* policy. As a robustness check, we show that the *hukou* reform reduced labor unrest unrelated to *hukou*, but had a null effect on *hukou*-related unrest (Table A3). Taken together, our results should not be interpreted as driven by a mechanical decline in protests against the *hukou* policy in the wake of the *hukou* reform.

<sup>29</sup>Chen and Fu (2023) find that big cities' policies that restrict migrant children's access to public schools increase the likelihood that migrants work in the cities alone while leaving their children at home. This suggests that, despite lower migration costs for relocating to small cities (in terms of family unity), migrants are still inclined to move to big cities.

<sup>30</sup>Note that an individual protest event can have multiple causes. In total, 77 percent of events in our sample are due to wage arrears and/or job losses.

## 4.3 Additional Robustness Checks

### 4.3.1 Alternative Specifications and Estimators

In Appendix H.1, Table A13 and Figure A17, we adopt alternative specifications and estimators to estimate the effect of the *hukou* reform on labor unrest: (i) different forms of the dependent variable (logarithmic and inverse hyperbolic sine transformations); (ii) Poisson regression to account for 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). Across specifications and estimators, we consistently find that the *hukou* reform lowered labor unrest rates.

### 4.3.2 Excluding Potential Outliers

In Appendix H.2, we show that our results are not due to outlier observations. We test whether our results are driven by any particular province by leaving out one province at a time and re-estimating Equation 2. As Figure A18 shows, the estimate is stable to the exclusion of any single province.

In Table A14, we consider the influences of several sets of outliers on the estimated effect of the *hukou* reform. First, we exclude prefectures that never had a labor unrest event recorded by the CLB to ensure that our results are not due to low variation in labor unrest (recorded by the CLB) in reform prefectures. Second, inspired by the “donut RD” exercise, we exclude prefectures near the 3 million cutoff to address potential self-selection into reform status and/or measurement error in reform status resulting from measurement error in urban population. Third, we exclude those 17 prefectures with discrepancies between population-defined and actual reform statuses to avoid the influence of likely endogenous non-compliance. Lastly, following Hansen (2022, pp. 84–86), based on goodness-of-fitting, we identify observations that are most influential in our estimation and exclude them to assess robustness of our results. Regardless of which set of outliers is excluded, we estimate a strong, often more pronounced than baseline effect of the *hukou* reform on decreasing labor unrest.

### 4.3.3 Covariate Balancing

In Table A15 in Appendix H.3, we address concerns about the heterogeneity between reform and non-reform prefectures. This heterogeneity is reflected in the 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. However, if reform status systematically overlaps with unobserved triggers of labor unrest, then our findings would simply reflect the broader trends of those triggers without capturing a causal effect of the *hukou* reform on labor unrest. To address this, 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 to a broad literature on the linkage between negative income shocks and unrest ([Ponticelli and Voth, 2020](#); [Fetzer, 2020](#); [Braggion et al., 2020](#)). Specifically, we estimate the following regression model:

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

$TradeShock_{it}$  is a plausibly exogenous measure of fluctuations 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,  $\beta_5$ , measures the differential response of a reform prefecture to the trade shock *relative* to that of a non-reform prefecture subject to the same level of the trade shock when the *hukou* reform is in effect ( $Post_t = 1$ ).

$TradeShock_{it}$  is constructed in a shift-share (Bartik) fashion:

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

In this expression,  $X_{ik,2010}^{CN}$  is prefecture  $i$ 's exports of product  $k$  (6-digit Harmonized System level);  $X_{k,2010}^{CN}$  is the national aggregate exports of product  $k$ ;  $\Delta X_{kt}^{ROW}$  is the increase/decrease in exports within the rest of the world (less China; ROW in short) in year  $t$ .<sup>31</sup> The "shift" component,  $\frac{\Delta X_{kt}^{ROW}}{X_{k,2010}^{CN}}$ , 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,  $\frac{X_{ik,2010}^{CN}}{L_{i,2010}}$ , which reflects the initial

<sup>31</sup>  $X_{ik,2010}^{CN}$  and  $X_{k,2010}^{CN}$  are measured using Chinese customs data from 2010.  $\Delta X_{kt}^{ROW}$  is measured using the BACI database, which improves upon the UN Comtrade database. All variables are in 1,000 dollars.



specialization of prefecture  $i$ .  $TradeShock_{it}$  can thus be interpreted as a proxy of an average worker's gains or losses due to fluctuations in global trade. To be a valid exogenous shock,  $TradeShock_{it}$  needs to be uncorrelated with the error term,  $\eta_{it}$ . According to [Borusyak et al. \(2022\)](#), this rests on the exogeneity of the product-level shock,  $\frac{\Delta X_{kt}^{ROW}}{X_{k,2010}^{CN}}$ , with respect to  $\eta_{it}$ . In Appendix H.4, we discuss this and related checks proposed by [Borusyak et al. \(2022\)](#). We show that  $\frac{\Delta X_{kt}^{ROW}}{X_{k,2010}^{CN}}$  passes balance tests and can therefore 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 wherein we estimate the average relationship between the trade shock and labor unrest. The negative estimate of  $\beta_1$  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. This shows that a future trade shock has no impact on contemporary labor unrest. In Columns (3) and (4), we estimate the triple-differences model, Equation 4. The positive estimate of  $\beta_5$  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, instead of spuriously representing broader trends in triggers of unrest.

**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*** (0.031)		-0.087** (0.036)	-0.049 (0.046)
Trade shock $_{t+1}$		0.003 (0.020)		
Trade shock $\times$ Reform $\times$ Post [ $\beta_5$ ]			0.176* (0.092)	0.192* (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.

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

**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, an artifact of differential coverage of labor unrest events, or a consequence of population growth. The next section is devoted to investigating mechanisms through which the *hukou* reform influences the occurrence of unrest.

## 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, as they are the major participants in labor unrest during the period of study (see Section 2.2) and the *hukou* reform had no impact on population sizes. In Section 5.1, we propose that the *hukou* reform may increase the cost of participating in labor unrest. Put simply, the *hukou* reform enables migrants to pursue permanent settlement in their destinations, creating dependence on the state and in turn deterring participation in labor unrest (which the state dislikes). We corroborate this hypothesis by showing that the *hukou* reform raises migrants' settlement intentions, reflected by longer migration duration. 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 explicit that we do not claim that heightened settlement intentions are the only mechanism by which the *hukou* reform impacts unrest rates. In Section 5.3, we 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 anticipated returns. For example, [Adda et al. \(2022\)](#) leverage a panel dataset on immigrants and a dynamic model to 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 raises Mexican immigrants' intentions to stay in the US and thereby increases their propensity for naturalization and marriage to US citizens. [Gathmann and Keller \(2018\)](#) exploit Germany's 1990 citizenship policy reform, and they find that faster access to citizenship increases migrants' investments in host country-specific skills, such as language proficiency and vocational training.

A similar logic can apply to *political* behavior. In the following, we outline our hypothesized 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, 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 of permanent settlement can lead to a special "investment" in the form of reduced engagement in labor unrest. Migrants hoping for permanent settlement may fear

participating in labor unrest for several reasons. First, the *hukou* reform does not immediately entitle migrants to local *hukou*. Instead, it makes settlement more attainable than before; thus, migrants have to consider whether their political actions would undermine their chance of settlement. Second, migrants depend on the government to acquire permanent settlement. It is well known that the government dislikes labor unrest and may aggressively retaliate against participants when it deems necessary. With this knowledge, migrants may be deterred from joining unrest activities. Lastly, participation in unrest can indirectly undermine a migrant worker's prospect of permanent settlement. 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 because of their prior participation in labor unrest (and, perhaps much worse, punishment by the government).

Taken together, migrants' increased settlement intentions in the wake of the *hukou* reform effectively function as a disincentive for engagement in unrest. Pre-reform, this disincentive was weak as institutional barriers to *hukou* transfers resulted in temporary migration. Short-term migrants may instead have incentives to engage in politically risky unrest to gain short-term benefits (e.g., recovering unpaid wages) as they face low costs in the form of permanent settlement prospects.

It is worth noting the group and time window in which the settlement intentions mechanism can occur. This mechanism primarily applies to migrants who desire to permanently settle in their destination but have not yet successfully transferred their *hukou*. 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 revoked once issued.

### 5.1.2 Testing the Hypothesis

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

**Data.** For the purpose of our investigation, we use the 1 percent population census from 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 at three moments in time. For instance, we may observe:

$$A, 2010 \rightarrow B, 2014 \rightarrow B, 2015.$$

With this information, we can infer whether an individual migrated during one period. For instance, in the example given above, we can infer that the person must have moved during 2010–2014 (from A to B) but not during 2014–2015.<sup>32</sup>

Using this information, we trace how the *hukou* reform influences a migrant’s decision on outmigration from their 2010 place of residence. For this purpose, we impose two restrictions on the sample. The first restriction is for identifying migrants in 2010. We define an individual as a migrant if their 2010 residential prefecture differs from their 2015 *hukou* registration prefecture. Although we observe an individual’s residential history, we do not observe *hukou* registration history. We have to rely on the registration prefecture reported in 2015 to define their 2010 migrant status. This may introduce measurement error if an individual had transferred their *hukou* by 2015.<sup>33</sup> Therefore, we impose a second restriction and limit our sample to individuals with rural origins (reported in 2015), who were less likely to have transferred their *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):

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

$$t \in \{2014, 2015\}.$$

In Equation 6, the dependent variable,  $Outmigration_{jkt}$ , 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 is estimated using data for 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  $t = 2014$ . Here, we treat 2015 as the post-reform period while 2014 as the pre-reform period.<sup>34</sup> We also include prefecture and year fixed effects as well as polynomial controls. In the spirit of survival analysis, for an individual who decided to leave their initial location  $k$  at year  $t$ , we drop subsequent observations to focus on that individual’s initial outmigration.

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<sup>32</sup>This definition does not capture circular migration. For example, someone recorded as living in A in both 2010 and 2014 may have moved temporarily to B in 2012, which goes unobserved. Consequently, a 2012 migration from A to B would not be measured. However, since circular migration often occurs due to an attachment to the original destination, outmigration captured under our definition likely reflects a more permanent departure. This focus aligns with our primary interest in investigating how the *hukou* reform influences such movements.

<sup>33</sup>For instance, consider an individual who originally registered in the 2010 prefecture but had successfully transferred their *hukou* to the 2015 residential prefecture. When relying on the rule of whether the 2010 residential location is the same as the 2015 registration, this individual would be misdefined as a migrant. As a result, the outmigration rate among migrants can be overstated.

<sup>34</sup>As we only observe the residential prefecture by the end of 2014 in the census data, we can only define outmigration during 2010–2014. However, for those individuals who had ever moved during this period, it is expected that the majority of them should have moved much earlier, possibly before the *hukou* reform went into place in July 2014. For this reason, we argue that this treatment definition is reasonable.

Our coefficient of interest,  $\rho$ , is estimated by comparing trends in outmigration rates among not-yet-migrated individuals between reform and non-reform prefectures. It thus captures the impact of the *hukou* reform on the outmigration rate. We expect  $\rho$  to be negative as the *hukou* reform facilitated migrants permanently settling in their destinations.

**Results.** Table 5 reports estimates of Equation 6. Column (1) shows that the estimate of  $\rho$  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.

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

	Outmigration from the 2010 destination		
	(1)	(2)	(3)
Reform $\times$ Post	-0.072** (0.034)	-0.072** (0.034)	-0.071** (0.034)
Control mean	0.141	0.141	0.141
Prefecture FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Polynomials	Yes	Yes	Yes
Origin FE		Yes	Yes
Individual covariates $\times$ Year FE			Yes
Observations	58,701	58,701	58,701
Adj. $R^2$	0.078	0.131	0.136

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

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

We interpret this decreased outmigration as reflecting heightened settlement intentions caused by the *hukou* reform. However, one competing interpretation is if migrants in reform prefectures have inherently distinct dynamics of integration into their destination than those in non-reform prefectures, then we would observe them staying longer anyway. To alleviate this concern, we control for origin (*hukou* prefecture) fixed effects to leverage variation within migrants of the same origin (Column (2)). 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 actually does not change our estimate. Further, to the degree that distinct dynamics of integration are due to differences in individual characteristics, we control for differential dynamics by including interactions between individual covariates and year indicators (Column (3)). 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, we also estimate a Cox proportional hazard model (common in survival analysis) and find similar results (Appendix G.6).

In summary, the decreased outmigration reflects 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 settlement intentions?

To answer this question, we need to conduct a causal mediation analysis—identifying the causal effect of the *hukou* reform that goes through enhancing settlement intentions (*indirect effect*) versus the causal effect that goes through other mechanisms (*direct effect*). This is a rather challenging task. The conventional approach relies on a regression of the outcome on the treatment, and interprets the change in the coefficient on treatment after including the mechanism variable as the indirect effect (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; as a result, the mechanism effect on the outcome is not identified, so that the coefficient change is biased for the indirect effect. We propose improvements upon the conventional approach. Our method uses an instrumental variable (IV) to identify the mechanism effect on the outcome, and meanwhile, it preserves the simplicity of regression techniques.

Next, we first discuss our methodology in Section 5.2.1. Then in Section 5.2.2, 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.

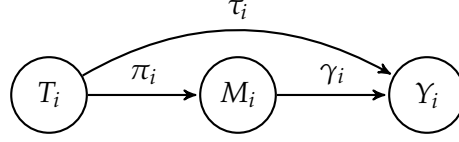
### 5.2.1 Methodology

**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 5 presents a directed cyclic graph (DAG) for the relations between the three variables.<sup>35</sup> Treatment  $T_i$  can influence outcome  $Y_i$  through two pathways: (i) direct influence, with a marginal effect of  $\tau_i$ , and (ii) indirect influence via a certain mechanism  $M_i$ , with a marginal effect of  $\gamma_i\pi_i$ .  $\gamma_i$  denotes the marginal effect of  $T_i$  on  $M_i$ , and  $\pi_i$  denotes the marginal effect of  $M_i$  on  $Y_i$ . Note that parameters  $(\tau_i, \pi_i, \gamma_i)$ , as the subscripts indicate, may vary across  $i$ , which extends the homogeneous effect framework in [Imai et al. \(2011\)](#). The following statement summarizes the parameters pertinent to our analysis.

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<sup>35</sup>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](#)).





**Figure 5.** Directed Acyclic Graph: Conventional Causal Mediation

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

1. The total effect of  $T_i$  on  $Y_i$  is defined as  $\beta_i \equiv \tau_i + \gamma_i\pi_i$ . The average total effect (ATE) is then  $E(\beta_i) \equiv E(\tau_i) + E(\gamma_i\pi_i)$ .
2. The indirect effect of  $T_i$  on  $Y_i$  attributable to mechanism  $M_i$  is  $\gamma_i\pi_i$ . The average indirect effect (AIE) is then  $E(\gamma_i\pi_i)$ .
3. The direct effect of  $T_i$  on  $Y_i$  is not attributable to mechanism  $M_i$  is  $\tau_i$ . The average direct effect (ADE) is then  $E(\tau_i)$ .

Note that  $ATE = ADE + AIE$ . An econometrician may be interested not only in the ATE but also in the AIE (and its magnitude relative to the ATE), as this reveals the significance of the mechanism. Consequently, they would seek an estimator for the AIE. Conventionally, this is achieved by estimating the following two regression models:

$$Y_i = \alpha_1 + \beta T_i + e_{i1}, \quad (7)$$

$$Y_i = \alpha_2 + \tau T_i + \gamma M_i + e_{i2} \quad (8)$$

For illustration, also consider a third regression model:

$$M_i = \alpha_3 + \pi T_i + e_{i3}. \quad (9)$$

For notations, let  $\hat{\kappa}$  denote the OLS estimator of  $\kappa$ . The conventional approach estimates the AIE by taking the difference in coefficients on  $T_i$  between Equations 7 and 8 (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:

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

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 the AIE. 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 many settings. In our context, this can be

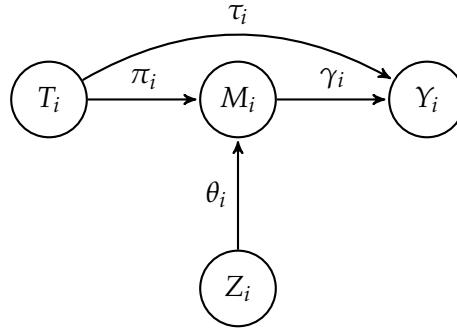
violated if, for instance, the *hukou* reform altered migrant networks, which simultaneously affected settlement intentions and the organization of labor unrest. In addition, as we discuss further later, one may need to assume that  $\gamma_i$  is homogeneous, so that  $\hat{\gamma}$ , as an average effect at the aggregate level, is applicable to all individuals (Glynn, 2012).<sup>36</sup> In Appendix J, we provide a proof for the consistency of  $\widehat{AIE}$  under given conditions.

**Proposed Approach.** Our proposed approach preserves the simple formula given by Equation 10 to estimate the AIE. To tackle the challenge in identifying the average 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 6 visualizes the relationship between variables in our proposed approach. It incorporates  $Z_i$  on the basis of Figure 5. 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  $\theta_i$ . More formally, consider the following potential outcome framework:

$$Y_i(T_i, M_i) = \tau_i T_i + \gamma_i M_i + u_i, \quad (11)$$

$$M_i(T_i, Z_i) = \pi_i T_i + \theta_i Z_i + v_i. \quad (12)$$

Equation 11 articulates that the value of  $Y_i$  depends on: (i) the values of  $(T_i, M_i)$ ; (ii) unit-specific effects  $(\tau_i, \gamma_i)$ ; 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  $Z_i$ .  $Z_i$  does not directly enter Equation 11, and the only way it can affect  $Y_i$  is through  $M_i$ .



**Figure 6.** Directed Acyclic Graph: Causal Mediation with IV

We now discuss the assumptions in our approach.

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

<sup>36</sup>This is because the expectation of a product does not equal to the product of expectations. To illustrate, suppose that OLS estimators identify average causal effects of interest, i.e.,  $\text{plim } \hat{\pi} = E(\pi_i)$  and  $\text{plim } \hat{\gamma} = E(\gamma_i)$ . Then,  $\text{plim } \widehat{AIE} = E(\gamma_i)E(\pi_i)$ , which is not the same as  $AIE = E(\gamma_i \pi_i)$  unless  $\gamma_i$  and  $\pi_i$  are uncorrelated, or in a special case,  $\gamma_i$  is constant.

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)  $Y_i(t, m) \big|_{z=z'} = Y_i(t, m) \big|_{z=z''}$  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  $\tau_i$ ,  $\gamma_i$ , and  $\pi_i$  is bounded below or above by 0.

Assumption 3 is not required for identifying the AIE, but it aids in interpretations. The reason is as follows. Without sign restrictions, it is likely that the AIE ( $E(\gamma_i \pi_i)$ ) may be zero at the aggregate level, even if the indirect effect ( $\gamma_i \pi_i$ ) is in fact nonzero for most individuals. Since regression coefficients, under necessary assumptions, can only identify average effects, the absence of sign restrictions may limit their informativeness regarding the role of a mechanism.<sup>37</sup>

ASSUMPTION 4 (Homogeneous Mechanism Effect).  $\gamma_i$  is constant across  $i$ .

Assumption 4 is a key assumption in our approach, positing that the causal effect of  $M_i$  on  $Y_i$  is constant. While strong, this assumption is not uncommon in the literature (Dippel et al., 2022; Dix-Carneiro et al., 2018). IV, like any other identification strategy, can only identify the effect of  $M_i$  on  $Y_i$  within a particular subpopulation, which may differ from the population in which the effect of  $T_i$  on  $Y_i$  is identified. Only with a homogeneity assumption, such as Assumption 4, can an econometrician extrapolate the identified effect of  $M_i$  on  $Y_i$  across populations to evaluate the mechanism's contribution to the outcome. In addition, this homogeneity assumption ensures that the average effect aligns with individual-level effects. In Section 5.2.3, we will discuss the robustness of our conclusion when relaxing this assumption.

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

---

<sup>37</sup>Researchers may implicitly make Assumption 3. For instance, after estimating an average treatment effect on the outcome, a researcher often asserts that the finding can be explained by a mechanism by showing a significant average treatment effect on the mechanism. For this statement to be valid, one typically has a strong belief about the direction of the mechanism's effect on the outcome, although a formal causal mediation analysis may not be performed.

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

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

*Proof.* See Appendix J. ■

Proposition 1 retains the simplicity of the conventional approach based on linear regression: 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 the AIE. A similar result applies to RD designs that identify treatment effects at the cutoff determining treatment status, which we employ 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:  $ATE = E(\beta_i | r_i = 0)$ ,  $AIE = E(\gamma_i \pi_i | r_i = 0)$ , and  $ADE = E(\tau_i | r_i = 0)$ . Least squares estimators of Equations 7 and 8, with  $M_i$  instrumented by  $Z_i$  and flexible polynomial functions of  $r_i$  included, satisfy:  $\widehat{ATE} \equiv \hat{\beta} - \hat{\tau} = \hat{\gamma}\hat{\pi}$ . Under Assumptions 1, 2, and 4,

$$\text{plim } \widehat{ATE} = E(\gamma_i \pi_i | r_i = 0) \equiv AIE. \quad (14)$$

*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 adapt a panel regression model from the previous analysis into its cross-sectional equivalent. Specifically, we estimate the following models, paralleling Equations 7 and 8:

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

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

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,  $\frac{\bar{\Delta}Unrest_i}{L_{i,2010}} = \left( \frac{\sum_{t=2014}^{2019} Unrest_{it}}{L_{i,2010}} \right) / 6 - \left( \frac{\sum_{t=2011}^{2013} Unrest_{it}}{L_{i,2010}} \right) / 3$ , captures the change in the average unrest rate from the pre-reform period to the post-reform period. With this transformation, the estimate of  $\beta$  in Equation 15 is numerically identical to that in Equation 2. In Equation 16, the mechanism variable is  $\bar{\Delta}Outmigration_i = Outmigration_{i,2015} - Outmigration_{i,2014}$ . Here,  $Outmigration_{it}$  denotes the outmigration rate in prefecture  $i$  during period  $t$ . Thus,  $\bar{\Delta}Outmigration_i$  represents the change in the outmigration rate for prefecture  $i$  from the 2010–2014 period to the 2014–2015 period.

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

**Instrument.** To instrument for outmigration, we consider export shocks to migrants' home prefectures. Specifically, the IV is constructed as follows:

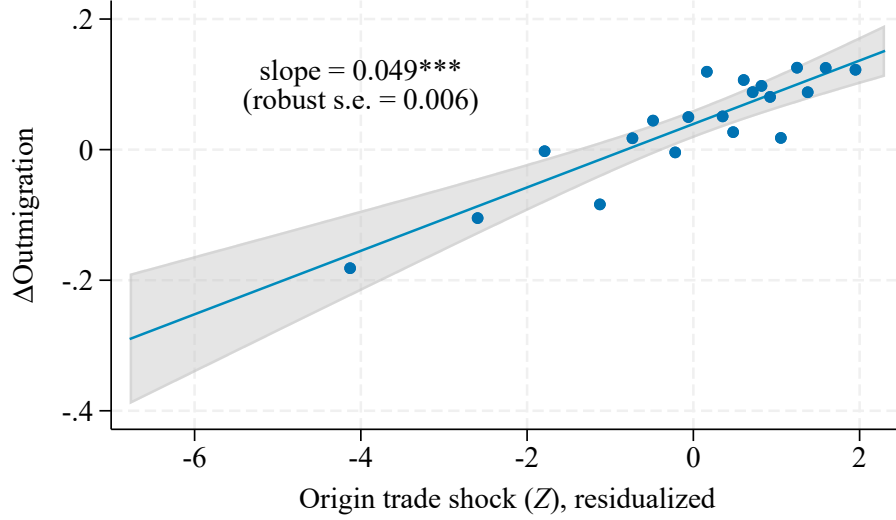
$$Z_i = \sum_h s_{h \rightarrow i} \times \bar{\Delta}TradeShock_h. \quad (17)$$

$s_{h \rightarrow i}$  represents the share of migrants moving from their home (*hukou*) prefecture  $h$  to destination prefecture  $i$  relative to all migrants in prefecture  $i$ .  $\bar{\Delta}TradeShock_h$  denotes the change in the average global demand shock (i.e.,  $TradeShock_{it}$  as defined in Section 4.3) from the 2011–2014 period to the 2014–2015 period, aligning with the time frames for calculating outmigration rates based on the 2015 census data. Thus,  $Z_i$  captures fluctuations in global demand experienced at home by an average migrant in prefecture  $i$ .

$Z_i$  represents a pull factor at the origin that influences 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).<sup>38</sup> In our outmigration analysis sample, we observe that 88 percent of migrants who eventually left their 2010 destinations returned to their home prefectures, suggesting that the origin is an important outside option when migrants decide whether to remain in their destination. Figure 7 shows that  $Z_i$  is predictive for  $\Delta Outmigration_i$ : sluggish growth in global demand at the origin is associated with a decrease in outmigration from the migration destination.

Shocks at migrants' origins are less likely to be associated with factors that could simultaneously drive outmigration and labor unrest. Formally, given the shift-share construction

<sup>38</sup>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-to-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.



**Figure 7.** Origin Trade Shocks and Outmigration Rates

Note: This figure plots the changes in the outmigration rate ( $\Delta Outmigration_i$ ) 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).

of  $Z_i$  and  $\Delta TradeShock_{it}$ , this exclusion restriction relies on the quasi-exogeneity of product-level shocks (Borusyak et al., 2022). A potential violation would occur if the migrants in prefectures with a low likelihood of labor unrest systematically come from origins that specialize in products with high global demand fluctuations. In Appendix K.1, we show that  $Z_i$  passes the balance tests recommended by Borusyak et al. (2022). Additional checks for the exclusion restriction are provided with the results presented below.

**Results.** We focus on assessing the importance of increased settlement intentions in the evolution of labor unrest induced by the *hukou* reform from 2011–2013 to 2014–2015. This period aligns with our measure of changes in settlement intentions, specifically the shifts in outmigration rates between 2011–2013 and 2014–2015. Unfortunately, we lack a measure of settlement intentions that spans the entire period of our study (2011–2019). As noted in Section 5.1.1, the settlement intentions mechanism is time-sensitive—relating short-term changes in settlement intentions to long-term changes in unrest may understate the significance of settlement intentions. Nonetheless, for completeness, we also report results examining the *hukou* reform-induced reduction of labor unrest during 2011–2019.

Table 6 presents the results of the causal mediation analysis. Columns (1)–(3) examine changes in unrest rates during 2011–2015. Column (1) shows the estimates of Equation 15. In Column (2), we implement the conventional approach for causal mediation analysis by directly adding  $\Delta Outmigration_i$  to the specification from Column (1). We observe a significant positive association between outmigration and unrest, suggesting that increased settlement intentions may reduce



unrest. Additionally, the coefficient on  $Reform_i$  in Column (2) attenuates by 18.8 percent compared to Column (1). Interpreting the estimated coefficient on  $\bar{\Delta}Outmigration_i$  as causal, this change implies that heightened settlement intentions explain 18.8 percent of the *hukou* reform's total effect on labor unrest.

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

	$\bar{\Delta}Unrest/L$ , 2011–2015			$\bar{\Delta}Unrest/L$ , 2011–2019		
	(1) Baseline	(2) Mediation-OLS	(3) Mediation-IV	(4) Baseline	(5) Mediation-OLS	(6) Mediation-IV
Reform [ $\beta$ or $\tau$ ]	-1.047** (0.518)	-0.851 (0.536)	-0.411 (0.608)	-1.419*** (0.370)	-1.237*** (0.364)	-1.033*** (0.383)
$\bar{\Delta}Outmigration [\gamma]$		1.855*** (0.453)	6.005*** (1.304)		1.726*** (0.360)	3.654*** (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 $\gamma$			4.767			2.758
Gap due to OVB			4.780			2.990
Observations	287	287	287	287	287	287

Note: This table reports causal mediation analysis that quantifies the importance of the settlement intentions mechanism, as captured by the outmigration rate. Columns (1)–(3) examine the immediate effect between 2011 and 2015. Columns (4)–(6) examine the longer-term effect between 2011 and 2019. Columns (1) and (4) report the baseline results. Columns (2) and (5) represent the conventional approach. Columns (3) and (6) 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.

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

In Column (3), we apply our approach that instruments outmigration using trade shocks at migrants' origins. As Figure 7 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 suggest.

Columns (4)–(6) conduct the same analysis for the longer time window (2011–2019). Note that Column (4), as expected, produces an estimate of  $\beta$  identical to that in Column (2) of Table 3. The mediation analysis in Columns (5) and (6) indicates that our IV-augmented approach suggests heightened settlement intentions play a more significant role than the conventional approach implies. For this extended time window, our measure of increases in settlement intentions explains 27.3 percent of decreased labor unrest due to the *hukou* reform. This magnitude is notably smaller than that found when considering the 2011–2015 period. The attenuation can be due to two reasons: (i)  $\bar{\Delta}Outmigration_i$  exhibits pronounced measurement error over the longer period; and

(ii)  $\bar{\Delta}Outmigration_i$  has no effect in the long term after migrants have successfully attained local *hukou*.<sup>39</sup>

The increase in the share of the total effect explained is due to inflation in the coefficient on  $\bar{\Delta}Outmigration_i$  after instrumentation. Suppose that the IV is valid, the *empirical* IV-OLS gap can arise from two sources: (i) omitted variables bias (OVB) purged by IV, and (ii) the different weighting schemes used by IV and OLS aggregate potentially heterogeneous effects.<sup>40</sup> We apply the decomposition method developed by Ishimaru (2024) to calculate the contribution of each source. The results indicate that nearly all the IV-OLS gap can be attributed to OVB. This suggests that the increased importance of the mechanism after instrumentation is not mechanically driven by a different weighting scheme.

Our results rely on the validity of the IV, i.e., that trade shocks at origins influence labor unrest solely through their effect on outmigration. The balance tests provide some confidence in this assumption. We conduct three additional checks in Appendix K.2. First, one may be concerned that besides outmigration, the trade shocks at origins can affect labor unrest by inducing in-migration. If so, the exclusion restriction is violated. To alleviate this concern, in Table A18, we conduct a robustness check for our causal mediation analysis by controlling for in-migration, which is measured by the change in the average population growth rate from the pre-reform period to the post-reform period. We find that controlling for in-migration does not markedly change our results. If anything, it slightly accentuates the importance of the outmigration channel. Second, one may be concerned that trade shocks at nearby origins can have spatial spillover impacts on labor unrest at the destination. To address this concern, we construct the IV,  $Z_i$ , using only origins that are sufficiently distant from prefecture  $i$  to minimize potential spillovers. Table A19 shows that we can still conclude that settlement intentions are a quantitatively important mechanism. Third, we use the methodology developed by Conley et al. (2012) to assess how exogenous the IV is. Specifically, we test whether we can conclude that outmigration has a significant effect on unrest after allowing for some violations of the exclusion restriction. We find that the positive relationship between outmigration and unrest holds up even under substantial violations of the exclusion restriction. This supports that outmigration is a plausible mechanism through which the *hukou* reform influences unrest.

<sup>39</sup>We suspect that (i) may play a relatively larger role than (ii), as we find that the *hukou* reform did not significantly affect the size of native urban population, suggesting that the impacts of settlement intentions should not dissipate rapidly during the period under study.

<sup>40</sup>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 differences in how IV and OLS weight 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. In this case, OLS and IV would aggregate nonlinear effects differently, and the IV-OLS gap would be entirely due to differences in weighting schemes.

### 5.2.3 Sensitivity Test

**Methodology.** The results above rely on Assumption 4 that the mechanism effect on the outcome ( $\gamma_i$ ) 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 immediate term and 27.3 percent 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  $\gamma_i$ , then we can obtain (see Appendix J for derivations):<sup>41</sup>

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

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

We first inspect Equation 18. In the expression,  $\text{plim } \hat{\gamma} = E(\phi_i \gamma_i)$  is the IV estimand, and  $\phi_i = \frac{\tilde{Z}_i M_i}{E(\tilde{Z}_i M_i)}$ , where  $\tilde{Z}_i$  is the residual of the linear projection of  $Z_i$  onto  $T_i$  (and possibly other controls). Equation 18 implies that even with instrumentation,  $\widehat{AIE}$ , the change in the coefficient on  $T_i$  after controlling for instrumented  $M_i$ , may still be biased for the 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 occurs because the 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 generalized to the entire population.
2. **Bias 2 (“ecological bias”).** The second bias is due to the correlation between  $\gamma_i$  and  $\pi_i$ . This is the classical ecological bias, which occurs when the characteristics of a population are attributed to an individual (Glynn, 2012). The intuition is as follows. Ideally, one would like to use  $\gamma_i$ , *individual-specific* effect of  $M_i$  on  $Y_i$ , as the loading on  $\pi_i$  to evaluate the contribution of the treatment effect on  $M_i$  to the total treatment effect. However, the linear regression can only use  $\hat{\gamma}$ , an *average* impact of  $M_i$  on  $Y_i$ , as the loading on  $\pi_i$ . As a result, the linear regression overlooks co-movement of  $\gamma_i$  and  $\pi_i$ , thus, it may systematically mis-measure the importance

<sup>41</sup>A similar result holds for RD designs by assuming that  $\gamma_i$  and  $\pi_i$  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).

of the treatment-induced change in  $M_i$  to the outcome at the individual level and also at the aggregate level.<sup>42</sup> Clearly, with Assumption 4, such bias is zero.

In summary, when Assumption 4 is not imposed,  $\widehat{AIE}$  does not consistently estimate AIE. The goal of this section is to develop a method for underpinning AIE by leveraging available information along with some additional assumptions.

We now turn to Equation 19. It writes the bias terms in terms of standard deviations and correlation coefficients.  $\sigma_U$  denotes the standard deviation of  $U$ , and  $\rho_{UV}$  denotes the covariance between  $U$  and  $V$ . We see that bias of  $\widehat{AIE}$  is determined by the distributions of  $(\phi_i, \gamma_i)$  and  $(\gamma_i, \pi_i)$ . Correlation coefficients  $\rho_{\phi\gamma} = \text{Corr}(\phi_i, \gamma_i)$  and  $\rho_{\gamma\pi} = \text{Corr}(\gamma_i, \pi_i)$  govern the severity of bias 1 and bias 2 in Equation 19, respectively.<sup>43</sup> 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  $\gamma_i$  and  $\pi_i$  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  $\rho_{\phi\gamma} = \text{Corr}(\phi_i, \gamma_i)$  and  $\rho_{\gamma\pi} = \text{Corr}(\gamma_i, \pi_i)$ ,*

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

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. This also gives us a lower bound of the effect mediated by the mechanism.<sup>44</sup>

<sup>42</sup>For instance, suppose  $\text{Cov}(\gamma_i, \pi_i) > 0$  and  $\gamma_i, \pi_i > 0$ . Then, within the low- $\gamma_i$  group, the contribution of the treatment-induced change in  $M_i$  (i.e.,  $\pi_i$ ) is systematically overstated, while within the high- $\gamma_i$  group, the contribution is systematically understated.

<sup>43</sup>Note that when  $\rho_{\gamma\pi} = \rho_{\gamma\pi} = 0$ , bias 1 and bias 2 are zero, so that  $\text{plim } \widehat{AIE} = AIE$ . Assumption 4 is a sufficient condition for this.

<sup>44</sup>Note that since there is no information on the actual distribution of heterogeneous effects, the lower bound tends to be overly conservative, as it assumes a scenario where biases are the largest.

**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 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 significantly when only small values  $\rho_{\phi\gamma}$  and  $\rho_{\gamma\pi}$  are imposed, then it is not reliable to maintain our previous conclusion under Assumption 4, stating heightened settlement intentions as an important mechanism linking the *hukou* reform to decreased labor unrest.

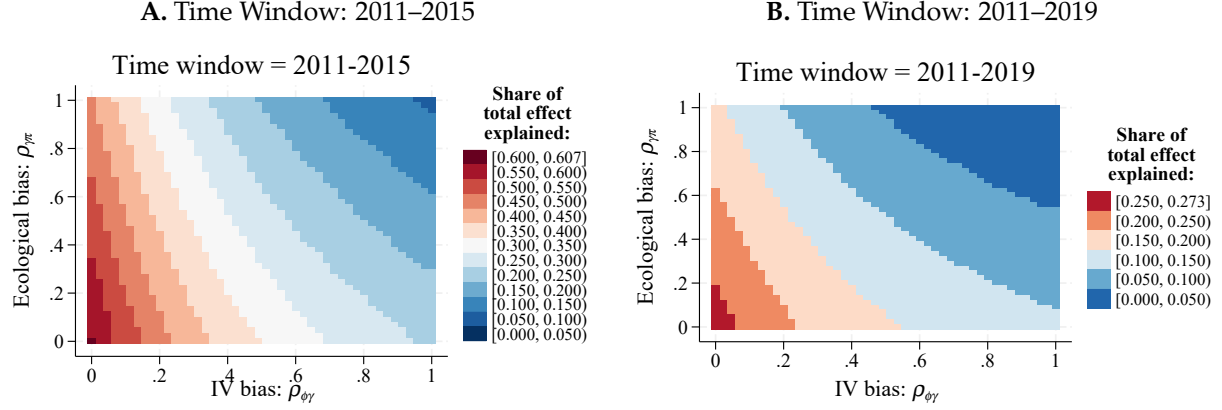
Figure 8A reports the results of our sensitivity test based on Proposition 3 for the immediate term. We see that, when  $\rho_{\phi\gamma} = 0$  and  $\rho_{\gamma\pi} = 0$ , which is true when Assumption 4 holds, we obtain the highest  $ShareExplained$ , 60.7 percent, as we have seen previously. Overall,  $ShareExplained$  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., with  $\rho_{\gamma\pi} = \rho_{\phi\gamma} = 0.5$ .

Figure 8B 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 of 10 percent survives even if we allow for moderate severity of both bias 1 and bias 2, e.g., with  $\rho_{\gamma\pi} = \rho_{\phi\gamma} = 0.5$ .

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 this is the sole mechanism of action. 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.



**Figure 8. Sensitivity Test for Causal Mediation**

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

### 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, if migrants with certain characteristics are more likely to participate in unrest because they have low costs of participation or tend to work in industries with poor working conditions—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). It is only relevant if there is churning within the migrant pool, where the overall size remains constant but the composition changes.

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 of migrants between 2011–2018, the China Migrants Dynamic Survey (CMDS). In the 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 at the individual level where the dependent variable is the migrant’s characteristic of interest. Because the CMDS is conducted annually in May, for these regressions, we treat years from 2015 onward as post-reform.

Table 7 reports the results. We examine the composition of 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.

**Table 7. *Hukou* Reform and Migrant Characteristics**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Female	Han ethnic	Age below 35	Married	High school completion	Cross-province migrant	Co-residence w. Spouse	Co-residence w. Child
<b>Panel A: All migrants</b>								
Reform $\times$ Post	0.006 (0.013)	-0.003 (0.004)	0.011 (0.010)	0.005 (0.010)	-0.005 (0.014)	-0.000 (0.011)	-0.020* (0.010)	-0.006 (0.015)
Control mean	0.474	0.953	0.536	0.876	0.389	0.571	0.885	0.654
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	867,392	661,846
<b>Panel B: New arrivals</b>								
Reform $\times$ Post	-0.001 (0.016)	0.005 (0.005)	0.007 (0.015)	0.025 (0.016)	-0.016 (0.016)	0.004 (0.019)	-0.010 (0.015)	-0.008 (0.019)
Control mean	0.459	0.947	0.639	0.809	0.395	0.524	0.833	0.537
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	192,728	146,772

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.

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

### 5.3.2 Benefits Available to Migrants

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

Columns (1)–(3) of Table 8 first investigate if there are improvements in migrants' labor market outcomes. We consider three measures: workforce participation, earned wages, and employees' access to social security (ASS; only available from the CMDS in 2011, 2013, and 2016). We do not find significant effects on these labor market outcomes, suggesting that reduced labor unrest is not due to improved labor market outcomes.<sup>45</sup> 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 give employers greater market power, which they can use to deter workers' unrest. If so, wages should also decrease, which we do not observe in the data. This is not surprising given that we do not find that the *hukou* reform increased population growth (see Section 4.2.2).

In Columns (4) and (5) of Table 8, we look at two measures of intangible benefits that may increase migrants' satisfaction and thus reduce their incentive to participate in unrest: migrants' co-residence with spouses or children. This measure is appropriate in our context as relaxed migration barriers may facilitate family (re)union. The results show that, if anything, the *hukou*

<sup>45</sup>A recent paper by An et al. (2024) finds 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 under a series of 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 show why such a comparison can be questionable.



reform slightly reduced the likelihood of spousal co-residence, which should in fact 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 unlikely that our findings are a mechanical consequence of reform-induced improvements in migrants' well-being.

**Table 8.** *Hukou* Reform on Available Benefits

	(1) Working	(2) Log wage	(3) ASS
Reform $\times$ Post	-0.003 (0.007)	0.003 (0.019)	0.006 (0.029)
Control mean	0.883	8.153	0.522
Sample period	2011–18	2011–18	2011, 13, 16
No. prefectures	255	255	255
Observations	990,912	810,696	162,239

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.

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

### 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. This would occur if officials *believed* population inflows would threaten 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, local governments prioritizing maintenance of social stability may divert more bureaucratic apparatus manpower to the issue instead of additional financial investment. To test if local governments paid more attention to stability maintenance, we use the share of stability-related keywords in the subsequent year's government work report (GWR). GWRs are comprehensive policy documents that local government heads present annually to local People's Congresses, in which they summarize accomplishments from the previous year and lay out work plans for the

next.<sup>46</sup> However, Column (3) shows that there is no discernible shift of policy focus toward stability maintenance in the text of these documents.

Finally, if our finding is driven by tighter social control, we would expect a higher fraction of unrest events repressed by the government, despite the overall drop in unrest occurrences. The CLB documents the government response for about 30 percent of reported events. Using this information, we calculate the share of unrest events known to have been repressed by the government (in the form of detention of workers and police intervention).<sup>47</sup> Column (4) shows that the *hukou* reform had no significant impact on the share of unrest events that were repressed.

Taken together, the tightening of social control does not appear to be the main mechanism underpinning our findings.

**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 $\times$ Post	-0.016 (0.029)	0.041 (0.033)	0.007 (0.009)	0.088 (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.

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

## 5.4 Discussion

Throughout this section, we discuss the mechanisms through which the *hukou* reform reduces labor unrest. One mechanism we highlight is heightened settlement intentions deterring participation in unrest due to fears of political and social consequences (namely, losing the opportunity to permanently settle). 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. This mechanism primarily applies to migrants who desire to

<sup>46</sup>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).

<sup>47</sup>This measure is missing if there are no unrest events in a given prefecture and year.

permanently settle in their destination but have not yet transferred *hukou*, meaning that our mechanism specifically plays a short-term role. Indeed, in Sections 5.1 and 5.2, we show that settlement intentions, as measured by outmigration rates, have better explanatory power for the *hukou* reform's effect on unrest in the immediate term than in the long term.

In principle, the salience of the settlement intentions mechanism ought to die out as existing migrants gradually attain local *hukou* and there are no significant increases in new migrants. However, the *hukou* reform had a persistent effect on reducing labor unrest (see Figure 2B). The *hukou* reform has therefore likely stimulated lasting changes in patterns of labor unrest. Our results suggest that such lasting changes are unrelated to migrants' characteristics, benefits available to migrants, or governments' social control strategies. The *hukou* reform may change other deeper factors relevant to unrest. For instance, by permitting some migrants to integrate into their destinations or simply raising migrants' settlement intentions, the *hukou* reform may weaken the social networks conducive to unrest activities, thereby leading to a persistent decrease in labor unrest. As we do not have micro-level information on the organizing of labor unrest due to data limitations, we leave it to future researchers to explore how the *hukou* reform shapes the dynamics of labor unrest in the longer term.

## 6 Conclusions

This paper investigates the causal relationship between integration policy and social stability. By examining the impact of China's *hukou* reform on labor unrest, we find that relaxing migration barriers may actually enhance social stability, countering the common concern of increased social turmoil. Our findings suggest that heightened settlement intentions among migrants are a key mechanism: in seeking to secure the state-controlled opportunity for permanent settlement introduced by the reform, migrants may be discouraged from participating in unrest.

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 the state's coercive power to induce citizens' compliance—in our case, migrants rely on the state for permanent settlement in their destinations. In this regard, weakening dependence on the state may facilitate civil disobedience and the momentum of social changes.

We close this paper by noting two limitations that may offer interesting avenues for future research. First, as noted in Section 5.4, migrants' heightened settlement intentions tend to apply in the short term and do not explain all the observed reduction in labor unrest. It would be valuable to investigate how migrants behave once they have established permanent settlement. Second, our paper primarily focuses on migrants' behaviors, partly because the literature has widely documented that migrants are the major participants in labor unrest. However, it remains

likely that natives also react to the reform's initiative and integration of migrants, as suggested by evidence of immigration's electoral effects among existing citizens (Mayda et al., 2022).

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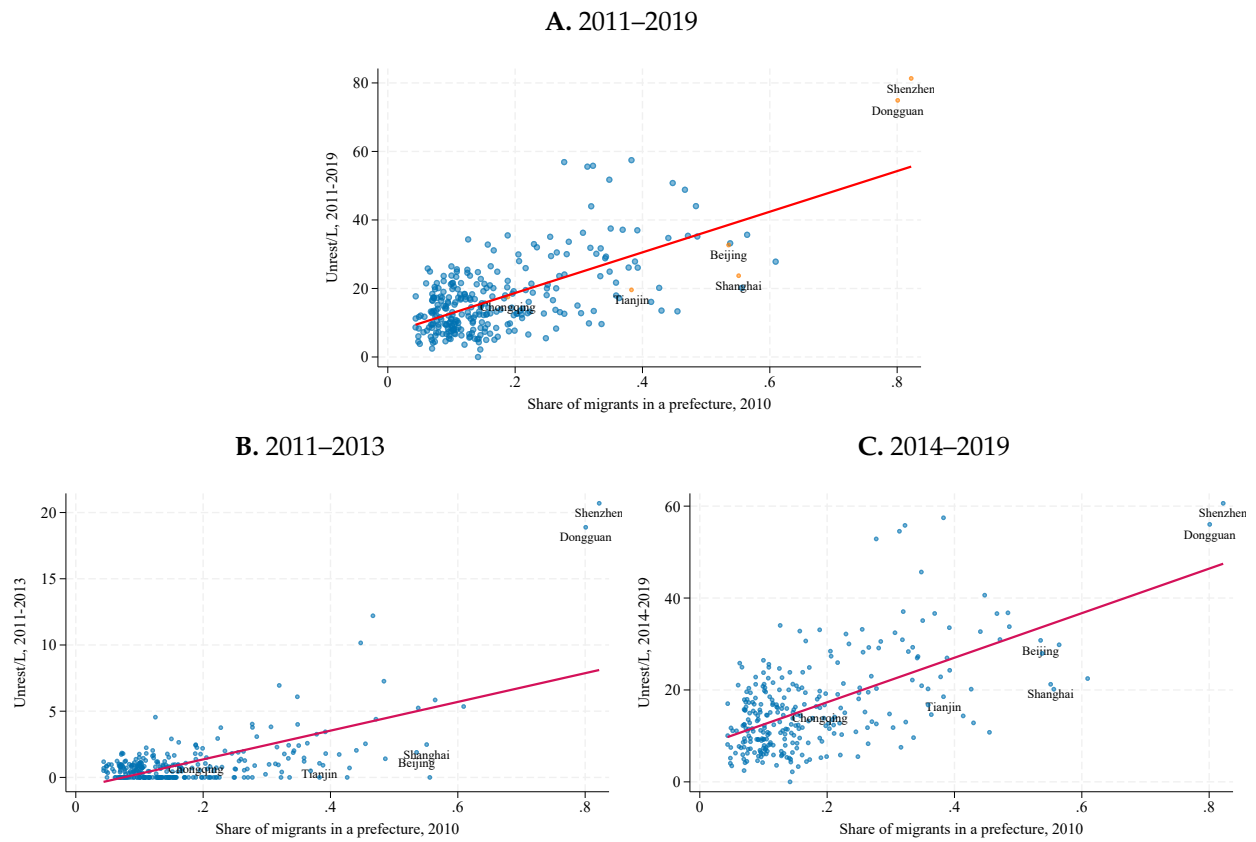
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# Online Appendices

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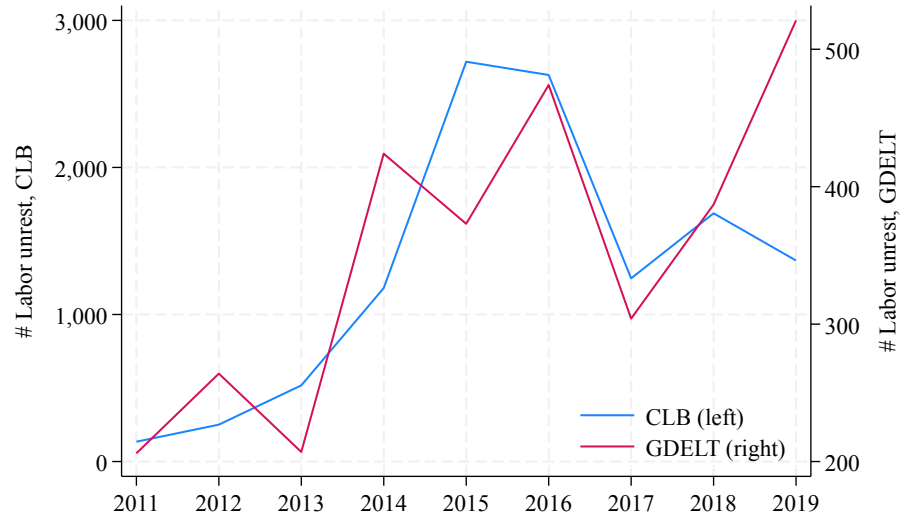
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## A Additional Figures



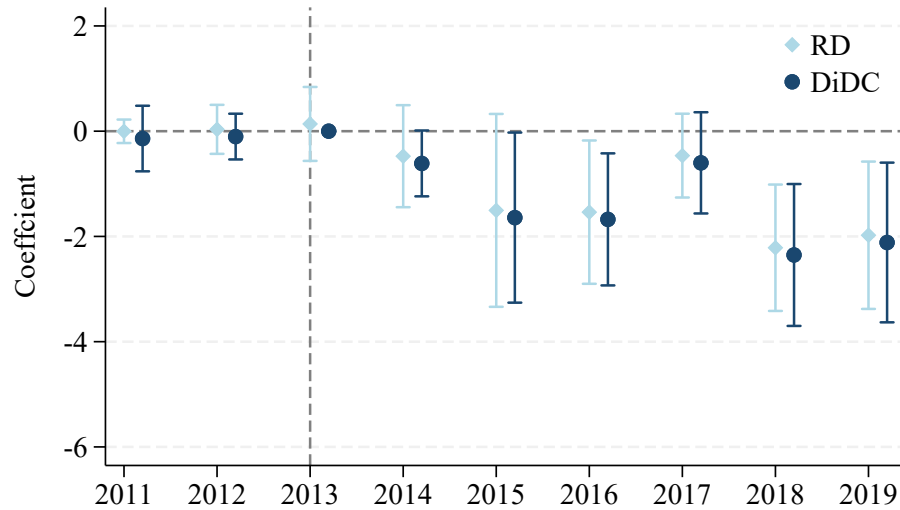
**Figure A1. Migrant Share and Labor Unrest Rate**

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.



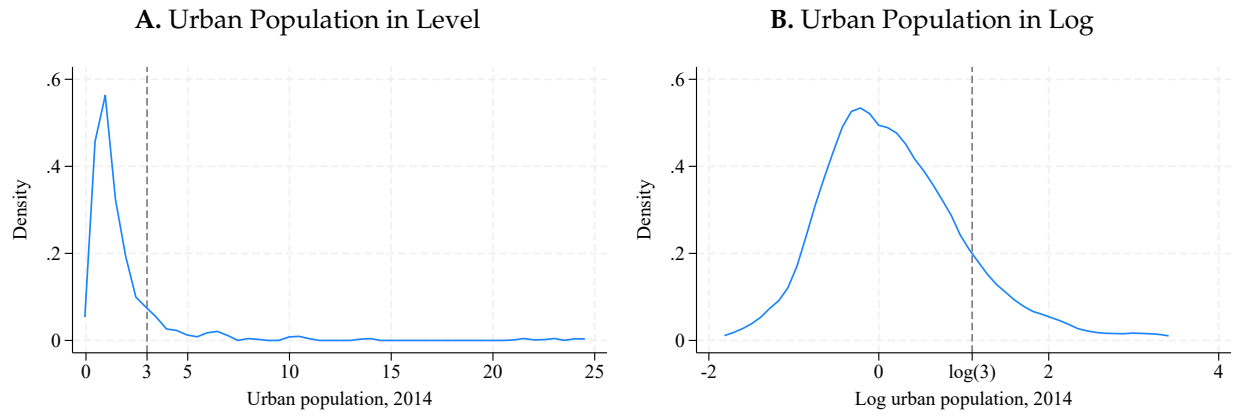
**Figure A2.** Labor Unrest in CLB and GDELT

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.



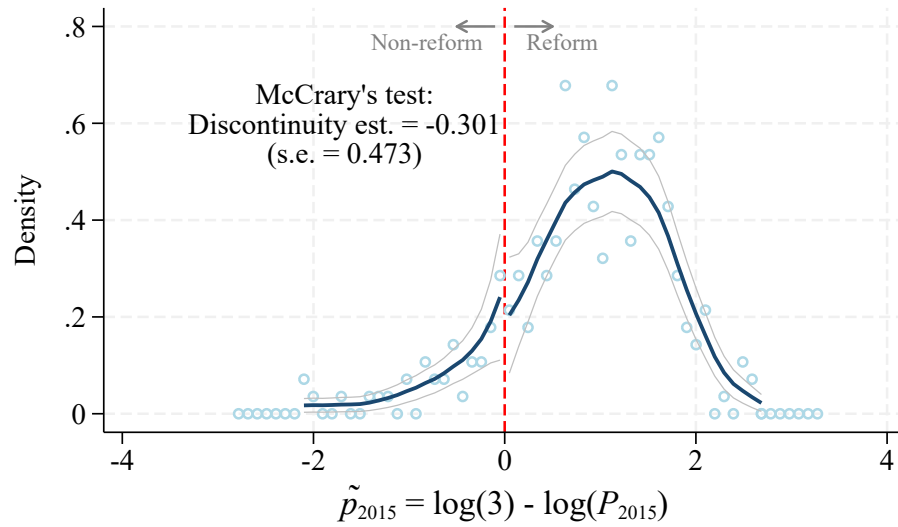
**Figure A3.** Effects of the *Hukou* Reform on Labor Unrest: RD Estimates

Note: For each period  $t$ , we estimate the RD specification:  $\frac{Unrest_{it}}{L_{i,2010}} = \alpha_t + \beta_t Reform_i + \zeta_{Reform,t} \tilde{p}_i + v_{it}$ . The coefficients of interests are  $\beta_t$ 's, which capture the differences in occurrences of labor unrest between reform and non-reform prefectures. In the figure, the solid points are point estimates of  $\beta_t$ 's, and the caps are the 95 percent confidence intervals.



**Figure A4.** 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.

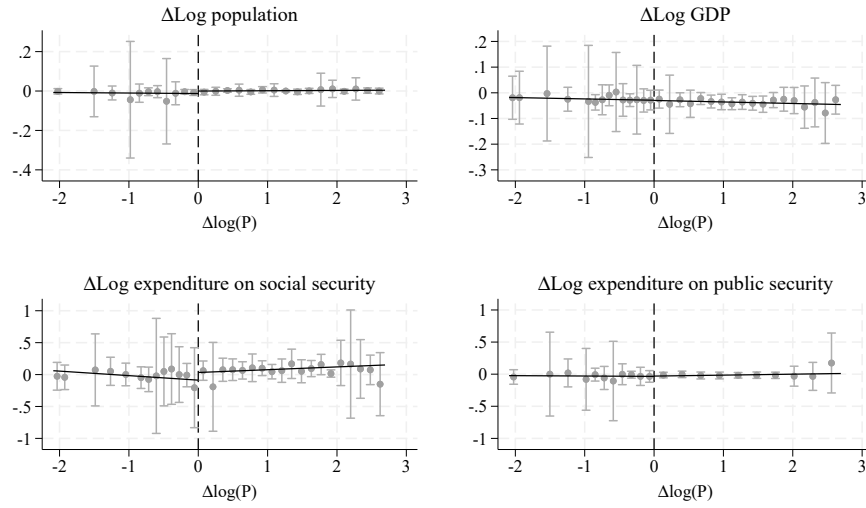


**Figure A5.** Density of Centered Log Urban Population in 2015

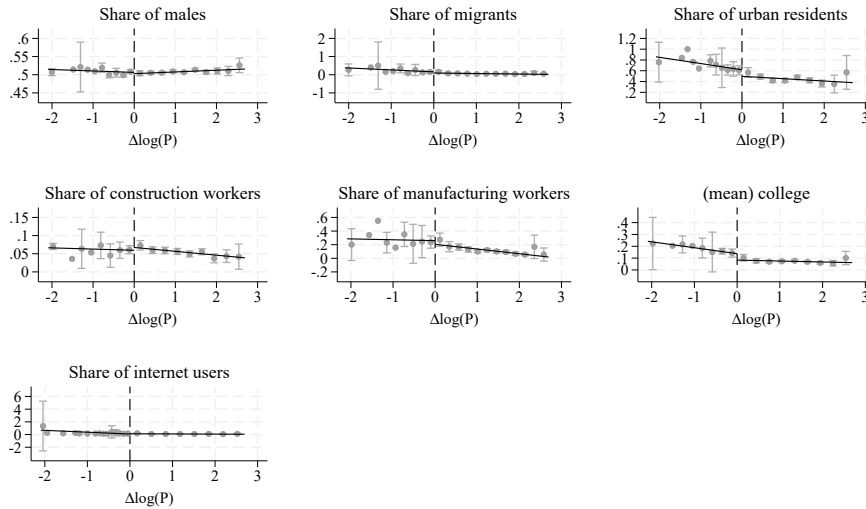
Note: This figure A5 depicts the density of  $\tilde{p}_{i,2015} = \log(3) - \log(P_{i,2015})$ . We report McCrary's test for density discontinuity at 0.



## A. pretrends

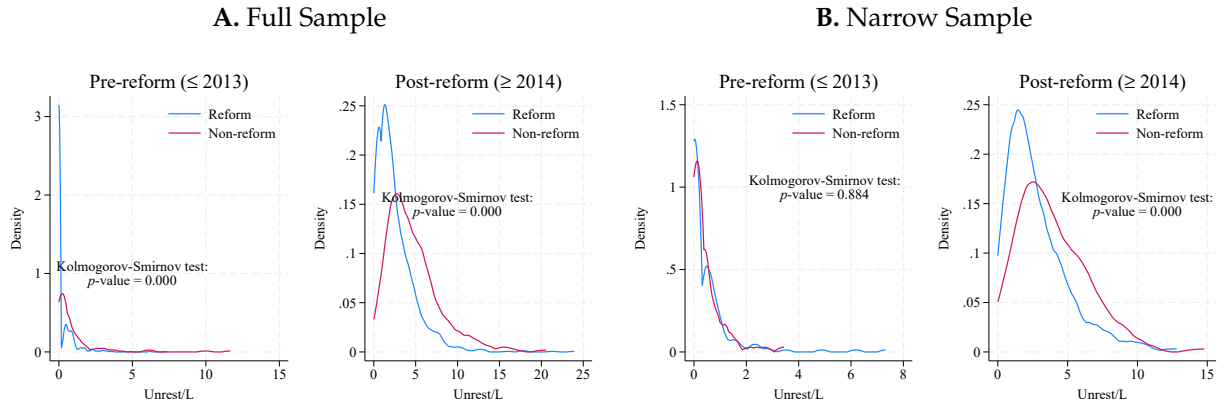


## B. Predetermined Characteristics



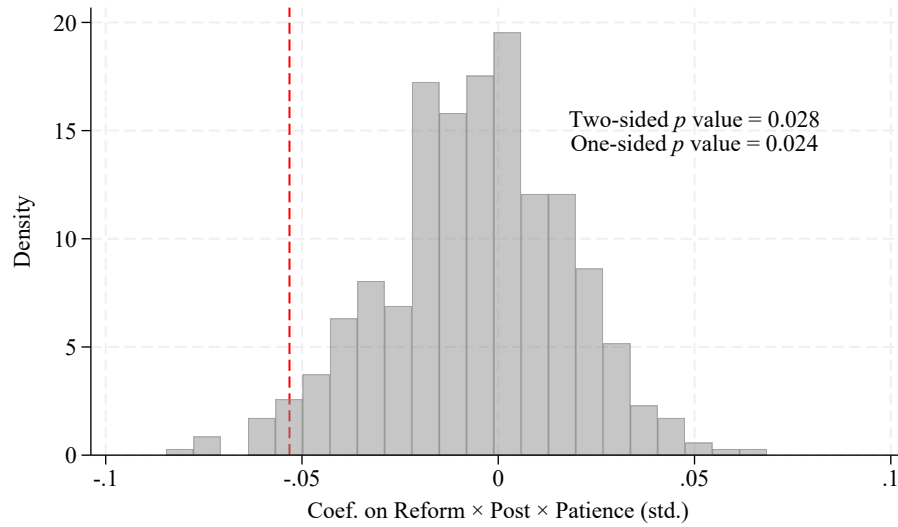
**Figure A6. RD Plots of Pre-reform Covariates**

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.



**Figure A7. 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 A8. Permutation Test for the Differential Effect on Outmigration by Patience Level**

Note: This figure reports a permutation test for the coefficient on  $Reform \times Post \times Patience$  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  $Reform \times Post \times Patience$ . 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  $Reform \times Post \times Patience$  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 $\times$ Post	-1.567*** (0.271)	-1.419*** (0.373)	-1.237** (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. Column (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 (0.082)	-0.024 (0.091)	-0.203 (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)	(2)	(3)
	Unrest/L	(Hukou-Unrelated Unrest)/L	(Hukou-Related Unrest)/L
Reform $\times$ Post	-1.419*** (0.373)	-1.342*** (0.353)	-0.077 (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 $\times$ Post	-0.068** (0.031)	-0.066** (0.031)	-0.066** (0.031)
Origin trade shock	0.014** (0.006)	0.015** (0.006)	0.021 (0.016)
Origin trade shock <sub><i>t</i>+1</sub>			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
Individual covariates $\times$ 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, 298 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.

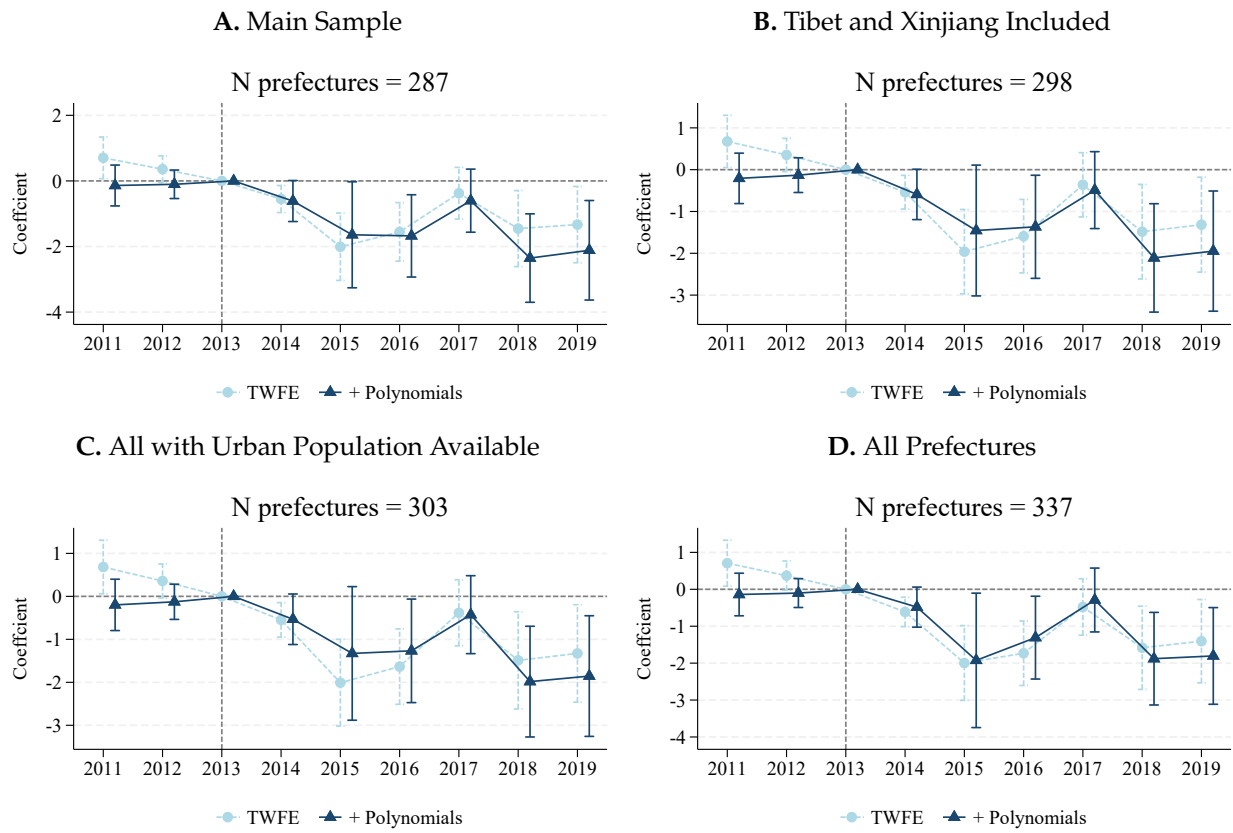
Table A5 reports the results. Clearly, no matter which sample is used, the results consistently imply that the *hukou* reform reduced labor unrest. The effect size does not vary markedly by sample used. Figure A9 displays the event-study estimates based on different samples. Again, the negative effect 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 $\times$ Post	-1.567*** (0.271)	-1.419*** (0.373)	-1.551*** (0.271)	-1.214*** (0.375)	-1.578*** (0.270)	-1.124*** (0.370)	-1.661*** (0.269)	-1.201*** (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 the sampling of prefectures. Columns (1)-(2) display results based on the main sample of 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 the 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.

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



**Figure A9. 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.<sup>1</sup>

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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<sup>1</sup>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 <sup>†</sup>	-/-/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. <sup>†</sup> = only the release year is known.

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

<b>Panel A:</b> Population-based = 0, manually-coded = 1			<b>Panel B:</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](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.<sup>2</sup> Specifically, we estimate the following Probit model:

$$\Pr(\text{Promotion}_{it}) = \Phi [\beta_0 \text{StartAge}_{it} + \beta_1 \text{HighRank}_{it} + \beta_2 (\text{StartAge}_{it} \times \text{HighRank}_{it}) + \mathbf{X}'_{it} \delta] . \quad (\text{A1})$$

$i$  indexes prefectures and  $t$  indexes terms. The unit of analysis is prefecture-by-term.  $\text{Promotion}_{it}$  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.  $\text{StartAge}_{it}$  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*).  $\text{HighRank}_{it}$  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 show estimates by a linear

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<sup>2</sup>The retirement age is 60 for both prefecture level and deputy-province-level leaders and 65 for province-level leaders.

probability 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*** (0.003)	-0.025*** (0.003)	-0.093*** (0.009)	-0.089*** (0.009)
Deputy province or above	-1.921*** (0.197)	-1.925*** (0.200)	-8.615*** (1.221)	-8.752*** (1.245)
Start age $\times$ Deputy province or above	0.035*** (0.004)	0.035*** (0.004)	0.157*** (0.023)	0.159*** (0.023)
Graduate degree		0.035** (0.017)		0.152** (0.074)
Central govt. expenditure		0.057 (0.040)		0.228* (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.

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

**GDELТ.** GDELТ, the abbreviation of the Global Database of Events, Language, and Tone, is a commonly used dataset on global events ([www.gdelтproject.org](http://www.gdelтproject.org)), especially on social unrest (Cantoni et al., 2023). GDELТ 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 GDELТ'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 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.

## 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,<sup>3</sup> 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.<sup>4</sup> The policy of this domain in 2017 was to divide tasks between branches within the central government.<sup>5</sup> Both policies did not outline provisions that vary by urban population.

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<sup>3</sup>[https://www.gov.cn/gongbao/content/2015/content\\_2912363.htm](https://www.gov.cn/gongbao/content/2015/content_2912363.htm)

<sup>4</sup>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](https://www.gov.cn/zhengce/content/2014-09/25/content_9092.htm)).

<sup>5</sup>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](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.<sup>6</sup> Thus, again, it would not be a confounding policy for the purpose of our paper.

Among all these policies during the period under study, we identify six 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.**<sup>7</sup> 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.**<sup>8</sup> 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).**<sup>9</sup> “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.**<sup>10</sup> 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.**<sup>11</sup> 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.**<sup>12</sup> 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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<sup>6</sup>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](https://www.gov.cn/zhengce/content/2017-02/06/content_5165797.htm)).

<sup>7</sup>[https://www.gov.cn/zhengce/content/2014-07/30/content\\_8944.htm](https://www.gov.cn/zhengce/content/2014-07/30/content_8944.htm).

<sup>8</sup>[https://www.gov.cn/zhengce/content/2016-09/30/content\\_5114118.htm](https://www.gov.cn/zhengce/content/2016-09/30/content_5114118.htm).

<sup>9</sup>[https://www.gov.cn/zhengce/content/2016-12/05/content\\_5143290.htm](https://www.gov.cn/zhengce/content/2016-12/05/content_5143290.htm).

<sup>10</sup>[https://www.gov.cn/zhengce/content/2017-02/28/content\\_5171345.htm](https://www.gov.cn/zhengce/content/2017-02/28/content_5171345.htm).

<sup>11</sup>[https://www.gov.cn/zhengce/content/2018-07/13/content\\_5306202.htm](https://www.gov.cn/zhengce/content/2018-07/13/content_5306202.htm).

<sup>12</sup>[https://www.gov.cn/zhengce/content/2019-06/26/content\\_5403340.htm](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.**<sup>13</sup> 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.

<sup>13</sup>[https://www.gov.cn/gongbao/content/2015/content\\_2864050.htm](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.

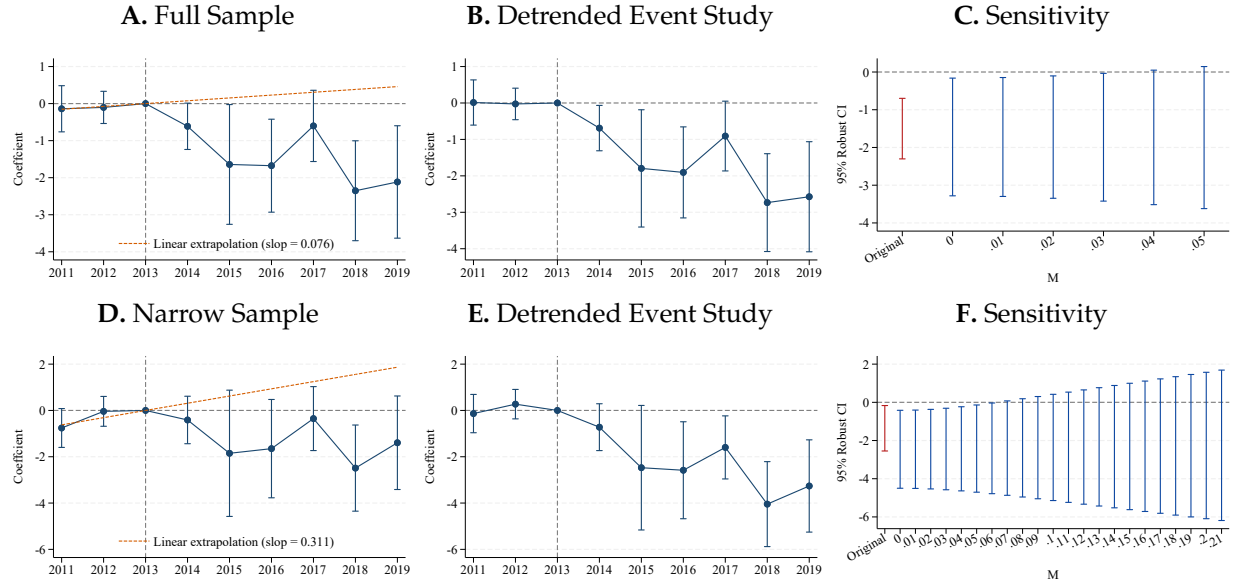
First, we can fit a linear trend based on the pre-reform event study estimates and extrapolate it to the post-reform periods, as shown by the first column of Figure [A10](#). Apparently, the pretrends are slightly upward. If these trends persist to the post-reform periods, the decline in unrest rates 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 [A10](#) 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 [A10](#), 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:

$$|(\theta_{t+1} - \theta_t) - (\theta_t - \theta_{t-1})| \leq M. \quad (\text{A2})$$

$\theta_t$  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 [A10](#) shows that our results can withstand very nonlinear differential trends. For instance, consider Figure [A10C](#) 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 [A10F](#) 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 pre-trend 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.



**Figure A10.** Detrended Event-Study Coefficients and Application of **Rambachan and Roth (2023)**

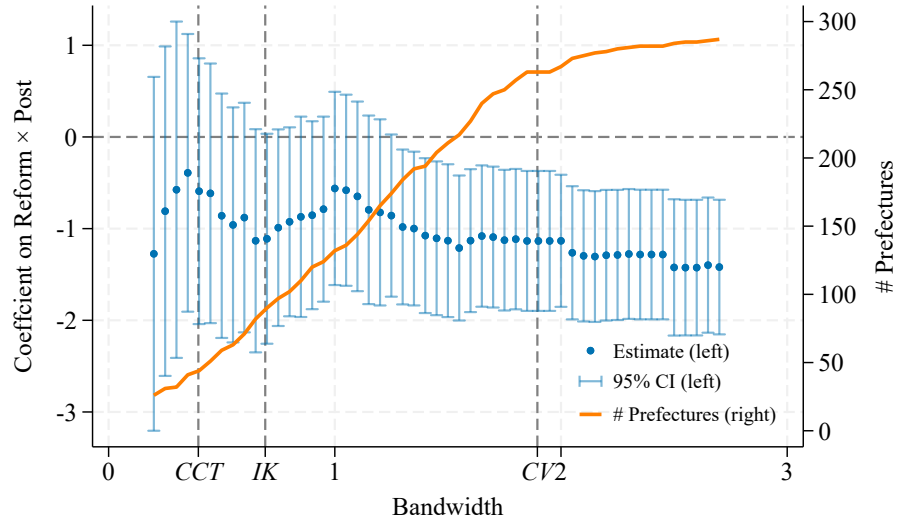
Note: This figure reports the sensitivity test for the event study results from both full and narrow samples. The first column depicts the linear trend implied by pre-reform event study coefficients and it is extrapolated to post-reform periods. The middle column shows the deviations of event study coefficients 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 A11 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 below 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.

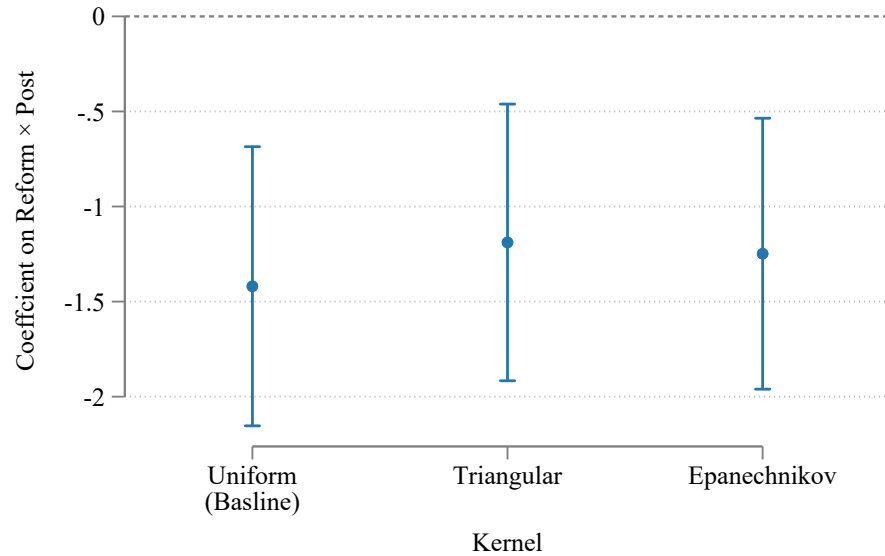
**Kernels.** Our baseline results place equal weights on prefectures, i.e., the uniform kernel is used. We examine the robustness to choices of kernels. We consider two alternative kernels common in the literature: triangular and Epanechnikov kernels, which place high weights on prefectures closer to the cutoff  $\tilde{p}_i = 0$ . Using these two kernels, we then re-estimate Equation 2. Figure A12 shows that using alternative kernels yields similar estimates of  $\beta$  in Equation 2 as in the baseline.

**Polynomial Orders.** Figure A13 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 A11.** Estimates under Different Bandwidth Choices

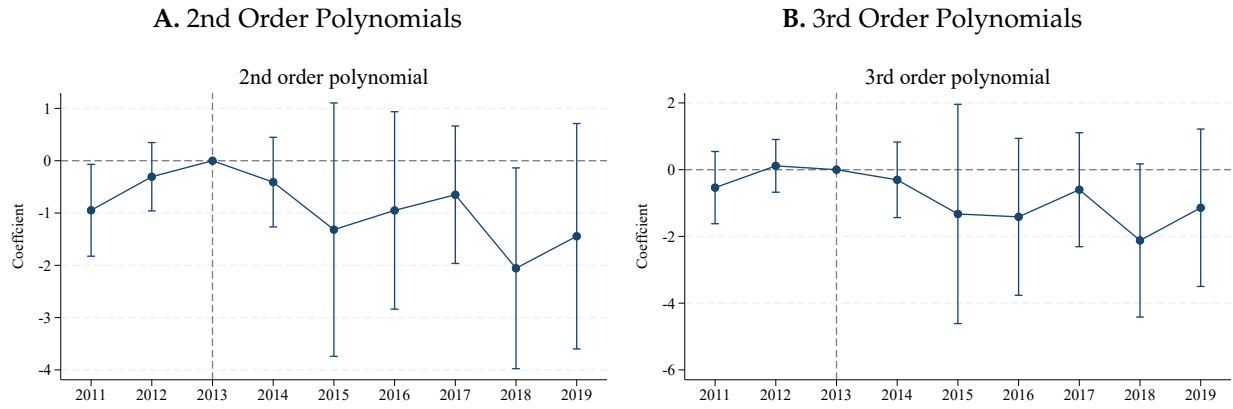
Note: This figure presents the estimated coefficient on  $Reform_i \times Post_t$  in Equation 2, when we impose different bandwidth restrictions to our sample. We note three commonly used optimal bandwidths proposed in the literature: [Calonico et al. \(2014\)](#) a.k.a. CCT, [Imbens and Kalyanaraman \(2012\)](#) 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 A12.** 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  $\beta$  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.



**Figure A13. 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.<sup>14</sup> 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. To rule out this concern, we measure censorship as the share of deleted Weibo posts in the prefecture's affiliated province using data from Qin et al. (2017),<sup>15</sup> and control the interaction term of censorship level and the year fixed effect. Column (3) shows that the effect of the reform persists after incorporating the time-variant impact of censorship.

<sup>14</sup>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.

<sup>15</sup>Qin et al. (2017) measure the level of censorship using the share of deleted posts on Weibo at the provincial level in 2013.



**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 high reported 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 A14, 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

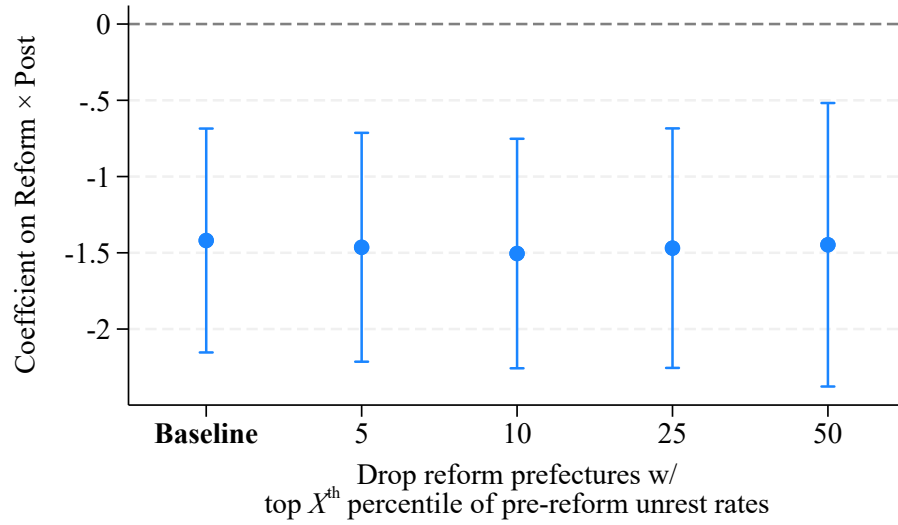
	(1) All events/L GDELT	(2) Protests/L GDELT	(3) Unrest/L CLB
Reform $\times$ Post	527.355 (522.334)	6.523 (4.992)	-1.323*** (0.372)
Control mean	1108.759	9.919	3.416
Prefecture FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Polynomials	Yes	Yes	Yes
Censorship level $\times$ Year FE			Yes
Observations	2,583	2,583	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. “Censorship level” is the share of deleted Weibo posts in a prefecture’s affiliated province, based on data from Qin et al. (2017). Robust standard errors are clustered at the prefecture level and reported in parentheses.

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

## 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



**Figure A14.** Addressing the Alternative Interpretation of Increased Self-Censorship

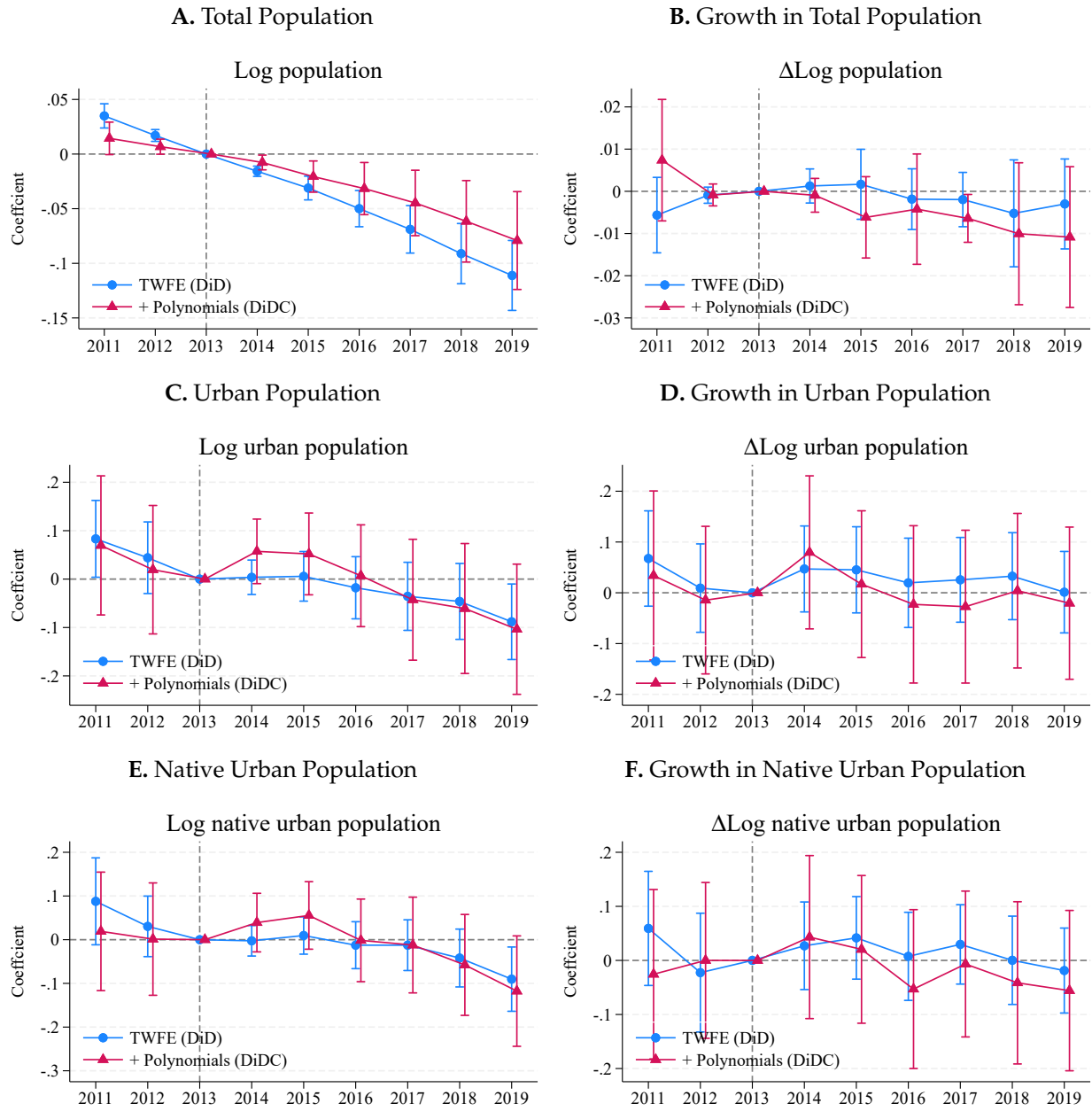
Note: This table examines to what extent the results are driven by differential trends in self-censorship. Within reform prefectures, we exclude those with high *pre-reform* unrest rates in the top  $X$ -th percentile ( $X$  is varied). When increased self-censorship occurs in these prefectures, we are 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  $\beta$ . The caps are 95 percent confidence intervals. Standard errors are clustered at the prefecture level.

growth rates. Figure A15 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 A15A 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 A15B); 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 to 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.

## G.5 Addressing Time-Varying Prefecture Sizes

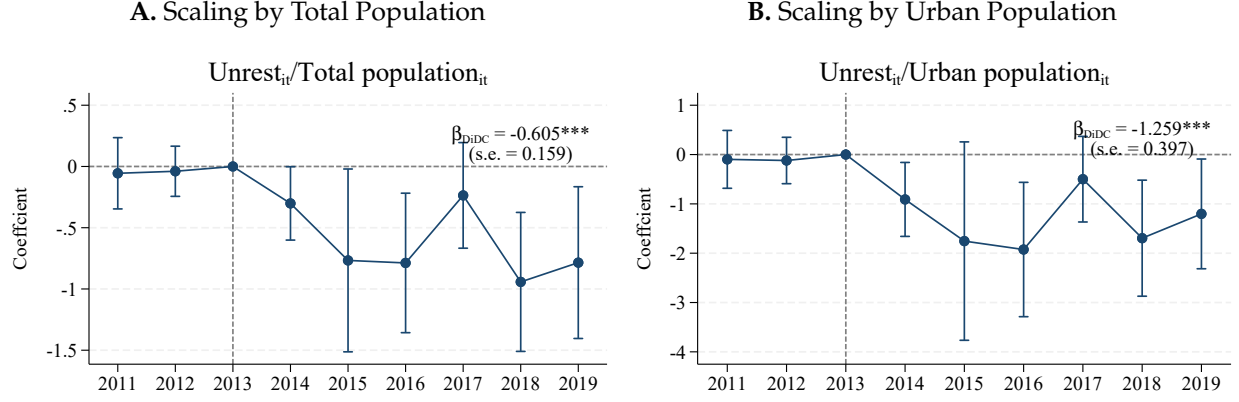
For our main results reported in Section 4, we scale the number of labor unrest events using working-age population, measured in the population census of 2010. One concern is that the results are simply due to time varying prefectures sizes rather than changes in underlying engagement of unrest. We show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



**Figure A15.** 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. The 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.

population, sourced from Rogoff and Yang (2024) and the Urban Construction Statistical Yearbooks, respectively. Figure A16 reports the results, confirming our findings that the *hukou* reform leads to a significant decrease in unrest rates.



**Figure A16. 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 population size. Figure A16A uses total population, and Figure A16B uses urban population. We visualize estimates from a dynamic specification:  $Y_{it} = \sum_{s \neq 2013} \beta_s (\text{Reform}_i \times D_s) + f(\tilde{p}_{i,2014}; \zeta_{\text{Reform},t}) + \lambda_i + \mu_t + \varepsilon_{it}$ . The solid points are points estimates of  $\beta_s$ 's, and the caps are 95 percent confidence intervals. We also report the estimate from a static specification:  $Y_{it} = \beta (\text{Reform}_i \times \text{Post}_t) + f(\tilde{p}_{i,2014}; \zeta_{\text{Reform},t}) + \lambda_i + \mu_t + \varepsilon_{it}$ . 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:

$$h(\text{Outmigration}_{jkt} | t, X) = h_0(t) \exp [\rho_1 (\text{Reform}_k \times \text{Post15}_t) + \rho_2 \text{Reform}_k + \rho_3 \text{Post15}_t]. \quad (\text{A3})$$

$h(\text{Outmigration}_{jkt} | t, X)$  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 provides consistent picture as in Table 5. The *hukou* reform reduced the hazard ratio of outmigration—according to Column (3)—by 58.2 percent.

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

	Outmigration from 2010 residence			
	(1)	(2)	(3)	(4)
Reform $\times$ Post	-1.058*** (0.143)	-0.863*** (0.244)	-0.872*** (0.240)	-0.973*** (0.258)
Mfx. on hazard rate	-0.653	-0.578	-0.582	-0.622
Polynomials		Yes	Yes	Yes
Stratified hazard function			Yes	Yes
Observations	58,701	58,701	58,701	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 A17. All results confirm that the *hukou* reform significantly reduces unrest rates.

**Table A13.** Robustness: Alternative Specifications and Estimators

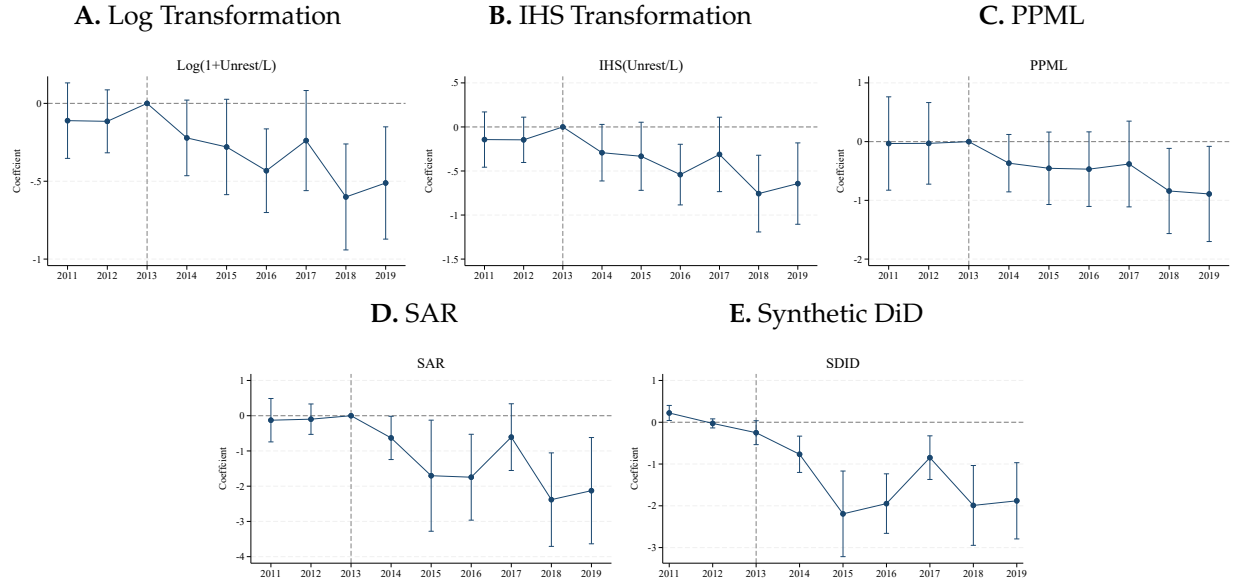
	Alt. Unrest Measures		PPML	SAR	SDID
	(1)	(2)	(3)	(4)	(5)
	Log(Unrest/L)	IHS(Unrest/L)	Unrest/L	Unrest/L	Unrest/L
Reform $\times$ Post	-0.305*** (0.086)	-0.383*** (0.112)	-0.545* (0.308)	-1.456*** (0.363)	-1.604*** (0.258)
Control mean	1.209	1.549	3.395	3.395	3.395
Method	OLS	OLS	PPML	SAR	SDID
Prefecture 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. Column (1) takes the log of unrest rate (plus one). Column (2) takes the inverse hyperbolic sine (IHS) transformation. Column (3) implements pseudo Poisson maximum likelihood (PPML) estimation. Column (4) estimates a spatial autoregressive (SAR) model. Column (5) uses the synthetic difference-in-differences (SDID) developed by [Arkhangelsky et al. \(2021\)](#). Standard errors are clustered at the prefecture level and reported in parentheses.

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

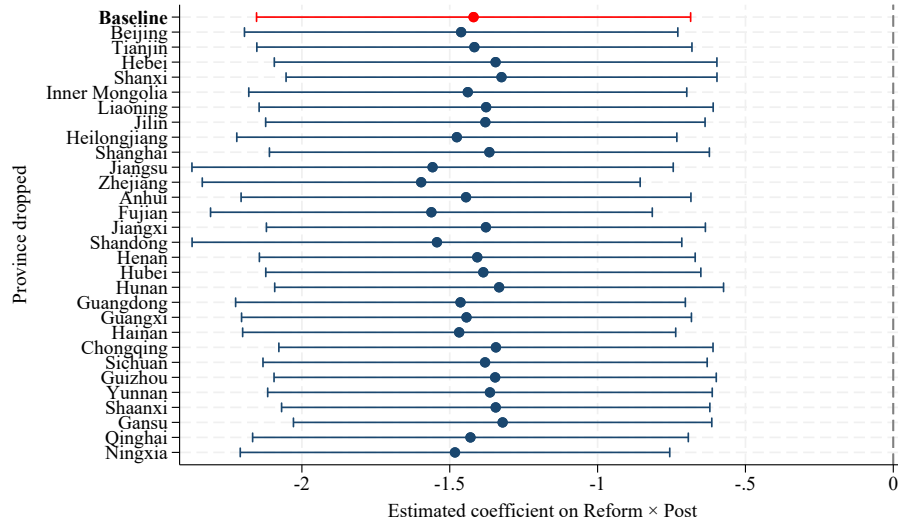
### H.2 Addressing Potential Outliers

To investigate if there are any special regional factors driving our results, we exclude one province each time and re-estimate Equation 2. Figure A18 shows the estimated coefficients on  $Reform_i \times Post_t$ . 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.



**Figure A17. 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 A18. Robustness: Dropping One Province Each Time**

Note: This figure reports the estimated coefficient on  $Reform_i \times Post_t$  from Equation 2, using the entire 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 on 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:

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

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 fact accentuates our finding.

**Table A14.** Robustness: Addressing Potential Outliers

	(1)	(2)	(3)	(4)
	Never having unrest recorded	Near the cutoff	w/ a diff. btw. population-based & manually-coded reforms	w/ high influence
Reform $\times$ Post	-1.401*** (-3.76)	-1.624*** (-4.62)	-1.834*** (-4.36)	-1.631*** (-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 if 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 $\times$ Post	-1.439*** (0.376)	-1.024*** (0.359)	-1.191* (0.686)	-1.446*** (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	2,511	2,511	1,764	2,484

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 reported 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:

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

where  $s_{ik} = \frac{X_{ik,2010}^{CN}}{L_{i,2010}}$  and  $g_{kt} = \frac{\Delta X_{kt}^{ROW}}{X_{k,2010}^{CN}}$ . 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:

$$q_k = \beta_0 + \beta_1 g_{kt} + \mu_t + \varepsilon_k. \quad (\text{A6})$$

$q_k = \sum_i \frac{s_{ik}}{\sum_i s_{ik}} v_i$ .  $v_i$  is a covariate of prefecture  $i$ , thus,  $q_k$  captures the  $s_{ik}$ -weighted average covariate of prefectures that produce product  $k$ .  $\mu_t$  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  $\beta_1$ . If  $\beta_1$  is close to zero, it suggests that  $g_{kt}$  is not distributed in 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  $Reform_i$ .<sup>16</sup> 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  $\beta_1$  are statistically insignificant, supporting that  $TradeShock_{it}$  can be viewed as quasi-exogenous triggers of labor unrest.

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<sup>16</sup>However, we are unable to examine the pre-trend in labor unrest, because the starting year of CLB is 2011.

**Table A16.** Balance Tests for Product-Level Trade Shocks

Dependent	Coef.	SE
<b>Panel A: Pretrends</b>		
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)
<b>Panel B: Predetermined characteristics</b>		
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: Settlement and Unrest Participation

In this section, we present a simple conceptual model to clarify the linkage between settlement 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  $d$ . She has a two-period horizon: the current period (period 1) and the future (period 2). This migrant's lifetime utility is written as:

$$V = u_1 + \beta u_2, \quad (\text{A7})$$

where  $u_t$  is the utility in period  $t \in \{1, 2\}$ , which we specify in detail below, and  $\beta$  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.<sup>17</sup> 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:

$$u_1 = w + ae - \frac{1}{2}\gamma e^2. \quad (\text{A8})$$

**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:

$$u_{2d} = x - c - \frac{1}{2}\kappa e^2. \quad (\text{A9})$$

$x$  is the present value of continuing to stay in destination  $d$ , which may include earnings, local public services, and taste. However, to stay, a 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 associated with unrest participation in the first period. A migrant with

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<sup>17</sup>In fact, wage arrears are a reason for 70.58% unrest events recorded in CLB data during 2011–2019.

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 variable  $r$  that equals one if staying. Then,

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

**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:

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

## 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

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

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

$$\mathcal{V}_0 = w + e_0 - \frac{1}{2}\gamma e_0^2. \quad (\text{A13})$$

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

$$\mathcal{V}_1 > \mathcal{V}_0 \quad (\text{A14})$$

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

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

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

The aggregate level of unrest is

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

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:

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

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

■

## 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:

$$Y_i(T_i, M_i) = \tau_i T_i + \gamma_i M_i + u_i, \quad (\text{A20})$$

$$M_i(T_i) = \pi_i T_i + v_i. \quad (\text{A21})$$

Therefore,

$$Y_i(T_i) \equiv Y_i(T_i, M_i(T_i)) = (\tau_i + \gamma_i \pi_i) T_i + u_i + \gamma_i v_i \equiv \beta_i T_i + \eta_i, \quad (\text{A22})$$

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  $\tau_i$ , 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):

$$Y_i = \alpha_1 + \beta T_i + e_{i1}, \quad (\text{A23})$$

$$Y_i = \alpha_2 + \tau T_i + \gamma M_i + e_{i2}, \quad (\text{A24})$$

$$M_i = \alpha_3 + \pi T_i + e_{i3}. \quad (\text{A25})$$

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

$$\widehat{ATE} \equiv \hat{\beta} - \hat{\tau} = \hat{\gamma} \hat{\pi}. \quad (\text{A26})$$

$\hat{w}$  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  $\widehat{ATE}$  as AIE (e.g., among others, [Imai et al., 2011](#)).

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

$$\hat{\beta} = \frac{\text{Cov}(T_i, Y_i)}{\text{Var}(T_i)} \quad (\text{A27})$$

$$= \frac{\text{Cov}(T_i, \beta_i T_i + \eta_i)}{\text{Var}(T_i)} \quad (\text{A28})$$

$$= E(\beta_i). \quad (\text{A29})$$

The second equality is by plugging in the potential outcome Equation [A22](#). The third equality is by Assumption [5](#). Thus,  $\hat{\beta}$  identifies the ATE.

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

$$\hat{\tau} = \frac{\text{Cov}(T_i, Y_i - \hat{\gamma} M_i)}{\text{Var}(T_i)} = \hat{\beta} - \hat{\gamma} \hat{\pi}, \quad (\text{A30})$$

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  $L(R | T)$  denote the linear projection of  $R$  on  $T$  and constant 1, and  $\tilde{R}$  is the linear projection residual, namely,  $\tilde{R} = R - L(R | T)$ . By Assumption [5](#) and Equation [A21](#),  $L(M_i | T_i) = E(M_i | T_i)$ , i.e., the linear projection recovers the conditional mean.<sup>18</sup>  $\hat{\gamma}$  has the following expression:

$$\hat{\gamma} = \frac{E(\tilde{M}_i Y_i)}{E(\tilde{M}_i^2)} \quad (\text{A31})$$

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<sup>18</sup> $E(M_i | T_i) = E(\pi_i | T_i)T_i + E(v_i | T_i) = E(\pi_i)T_i + E(v_i)$  is linear.

$$= \frac{E \left[ \tilde{M}_i(\tau_i T_i + \gamma_i M_i + u_i) \right]}{E(\tilde{M}_i^2)} \quad (\text{A32})$$

$$= \frac{E(\tilde{M}_i M_i \gamma_i)}{E(\tilde{M}_i^2)} + \frac{E(\tilde{M}_i T_i \tau_i)}{E(\tilde{M}_i^2)} + \frac{E(\tilde{M}_i u_i)}{E(\tilde{M}_i^2)}. \quad (\text{A33})$$

The first term is a weighted average of  $\gamma_i$ , 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,

$$\widehat{ATE} = \hat{\beta} - \hat{\tau} \quad (\text{A34})$$

$$= \left[ \frac{E(\tilde{M}_i M_i \gamma_i)}{E(\tilde{M}_i^2)} + \frac{E(\tilde{M}_i T_i \tau_i)}{E(\tilde{M}_i^2)} + \frac{E(\tilde{M}_i u_i)}{E(\tilde{M}_i^2)} \right] E(\pi_i) \quad (\text{A35})$$

$$\neq AIE \quad (\text{A36})$$

$$\equiv E(\gamma_i \pi_i) = E(\gamma_i)E(\pi_i) - Cov(\gamma_i, \pi_i). \quad (\text{A37})$$

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

$$\text{Bias} = \widehat{ATE} - AIE = \underbrace{[\hat{\gamma} - E(\gamma_i)] E(\pi_i)}_{\text{bias 1}} + \underbrace{Cov(\gamma_i, \pi_i)}_{\text{bias 2}}. \quad (\text{A38})$$

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  $\gamma_i$  and uses an *average* slope of  $Y_i$  for  $M_i$ ,  $E(\gamma_i)$ , to evaluate  $M_i$ 's marginal contribution to  $Y_i$ ,  $\pi_i$ . This produces bias if  $\pi_i$  is correlated with  $\gamma_i$ : Consider a positive correlation, the average slope systemically understates contributions of high  $\pi_i$ 's and overstates contributions of low  $\pi_i$ 's.

Researchers often assume homogeneity of  $\gamma_i$ , 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).  $Y_i(t', m) \perp\!\!\!\perp M_i(t) \mid T_i = t, \forall t, t', m$ .

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.

$$E(\tilde{M}_i T_i \tau_i) = E[E(\tilde{M}_i T_i \tau_i \mid T_i)] \quad (\text{A39})$$

$$= E[E(\tilde{M}_i T_i \mid T_i) E(\tau_i \mid T_i)] \quad (\text{A40})$$

$$= E(\tilde{M}_i T_i) E(\tau_i) \quad (\text{A41})$$

$$= 0. \quad (\text{A42})$$

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

$$E(\tilde{M}_i u_i) = E[E(\tilde{M}_i u_i \mid T_i)] \quad (\text{A43})$$

$$= E[E(\tilde{M}_i \mid T_i) E(u_i \mid T_i)] \quad (\text{A44})$$

$$= E(\tilde{M}_i) E(u_i) \quad (\text{A45})$$

$$= 0. \quad (\text{A46})$$

Together with homogeneity assumption  $\gamma_i = \gamma$ ,  $\hat{\gamma} = \gamma = E(\gamma_i)$ . Therefore,  $\widehat{AIE} = AIE$ .<sup>19</sup>

## 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  $\gamma_i$  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  $Z_i$ .

$$Y_i(T_i, M_i) = \tau_i T_i + \gamma_i M_i + u_i, \quad (\text{A47})$$

$$M_i(T_i, Z_i) = \pi_i T_i + \theta_i Z_i + v_i. \quad (\text{A48})$$

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<sup>19</sup>This result does not require the homogeneity assumption. With Assumption 6, one can further show that  $E(\tilde{M}_i M_i \gamma_i) = E(\tilde{M}_i M_i \gamma_i \mid T_i) = E(\tilde{M}_i M_i) E(\gamma_i) = E(\tilde{M}_i^2) E(\gamma_i)$ . 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:

$$Y_i(T_i, Z_i) \equiv Y_i(T_i, M_i(T_i, Z_i)) \quad (\text{A49})$$

$$= (\pi_i + \gamma_i \pi_i) T_i + \gamma_i \theta_i Z_i + (u_i + \gamma_i v_i) \quad (\text{A50})$$

$$= \beta_i T_i + \rho_i Z_i + \eta_i, \quad (\text{A51})$$

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

$$\widehat{AIE} = \hat{\beta} - \hat{\tau} = \hat{\gamma} \hat{\pi} = \underbrace{E(\gamma_i \pi_i)}_{AIE} + \underbrace{[\hat{\gamma} - E(\gamma_i)] E(\pi_i)}_{\text{bias 1}} - \underbrace{\text{Cov}(\gamma_i, \pi_i)}_{\text{bias 2}}, \quad (\text{A52})$$

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)  $\gamma_i$  is constant, or (ii)  $\{\theta_i, \pi_i\} \perp\!\!\!\perp \gamma_i$ .

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

$$\hat{\beta} = \frac{\text{Cov}(T_i, Y_i)}{\text{Var}(T_i)} = E(\beta_i), \quad (\text{A53})$$

$$\hat{\pi} = \frac{\text{Cov}(T_i, M_i)}{\text{Var}(T_i)} = E(\pi_i). \quad (\text{A54})$$

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

$$\hat{\gamma} = \frac{E(\tilde{Z}_i Y_i)}{E(\tilde{Z}_i M_i)}, \quad (\text{A55})$$

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

$$E(\tilde{Z}_i Y_i) = E[\tilde{Z}_i(\beta_i T_i + \rho_i Z_i + \eta_i)] \quad (\text{A56})$$

$$= E(\tilde{Z}_i T_i)E(\beta_i) + E(\tilde{Z}_i Z_i)E(\rho_i) + E(\tilde{Z}_i)E(\eta_i), \quad (\text{A57})$$

$$= E(\tilde{Z}_i Z_i)E(\rho_i). \quad (\text{A58})$$

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

$$E(\tilde{Z}_i M_i) = E[\tilde{Z}_i(\pi_i T_i + \theta_i Z_i + u_i)] \quad (\text{A59})$$

$$= E(\tilde{Z}_i T_i)E(\pi_i) + E(\tilde{Z}_i Z_i)E(\theta_i) + E(\tilde{Z}_i)E(u_i), \quad (\text{A60})$$

$$= E(\tilde{Z}_i Z_i)E(\theta_i). \quad (\text{A61})$$

Taken together,

$$\hat{\gamma} = \frac{E(\rho_i)}{E(\theta_i)} = \frac{E(\theta_i \gamma_i)}{E(\theta_i)}. \quad (\text{A62})$$

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

$$\hat{\gamma} = \frac{E(\tilde{Z}_i M_i \gamma_i)}{E(\tilde{Z}_i M_i)}. \quad (\text{A63})$$

Therefore,

$$\widehat{AIE} = \hat{\beta} - \hat{\tau} \quad (\text{A64})$$

$$= \hat{\gamma}E(\pi_i) \quad (\text{A65})$$

$$= \hat{\gamma}E(\pi_i) + E(\gamma_i \pi_i) - Cov(\gamma_i, \pi_i) + E(\gamma_i)E(\pi_i) \quad (\text{A66})$$

$$= \underbrace{E(\gamma_i \pi_i)}_{\text{AIE}} + \underbrace{[\hat{\gamma} - E(\gamma_i)]E(\pi_i)}_{\text{bias 1}} - \underbrace{Cov(\gamma_i, \pi_i)}_{\text{bias 2}}, \quad (\text{A67})$$

where  $\hat{\gamma} = \frac{E(\tilde{Z}_i M_i \gamma_i)}{E(\tilde{Z}_i M_i)} = \frac{E(\theta_i \gamma_i)}{E(\theta_i)}$ . It is obvious that bias 1 = bias 2 = 0 if (i)  $\gamma_i$  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.  $T_i = \mathbb{1}\{r_i \geq 0\}$ .  $\mathbf{R}_i = (r_i, r_i^2, \dots, r_i^p)$  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  $\mathbf{X}_i^{(0)} = [\mathbf{R}_i^{(0)} T_i, \mathbf{R}_i^{(0)} (1 - T_i)]'$ . With the introduction of running variable, the LSEM to estimate now becomes:

$$Y_i = \alpha_1 + \beta T_i + \mathbf{X}_i' \zeta_1 + e_{i1}, \quad (\text{A68})$$

$$Y_i = \alpha_2 + \tau T_i + \gamma M_i + \mathbf{X}_i' \zeta_2 + e_{i2}, \quad (\text{A69})$$

$$M_i = \alpha_3 + \pi T_i + \mathbf{X}_i' \zeta_3 + e_{i3}. \quad (\text{A70})$$

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  $r_i = 0$ ,  $E(\beta_i | r_i = 0) = E(\tau_i | r_i = 0) + E(\gamma_i \pi_i | r_i = 0)$ .
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,  $E(\gamma_i \pi_i | r_i = 0)$ .
3. The direct effect that is not due to mechanism variable  $M_i$  is  $\tau_i$ . ADE is defined as the average direct effect at cutoff,  $E(\tau_i | r_i = 0)$ .

ASSUMPTION 9 (Linearity of Conditional Means).

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

ASSUMPTION 10 (IV Validity).

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

ASSUMPTION 11 (IV Linearity).  $E(Z_i | r_i)$  is linear in  $\mathbf{X}_i^{(0)}$ .

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

$$\begin{aligned} \widehat{AIE} &= \hat{\beta} - \hat{\tau} = \hat{\gamma} \hat{\pi} \\ &= \underbrace{E(\gamma_i \pi_i | r_i = 0)}_{AIE} + \underbrace{[\hat{\gamma} - E(\gamma_i | r_i = 0)] E(\pi_i | r_i = 0)}_{bias\ 1} - \underbrace{Cov(\gamma_i, \pi_i | r_i = 0)}_{bias\ 2}, \end{aligned} \quad (A71)$$

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

$$\hat{\beta} = \lim_{r \downarrow 0} E[Y_i(1, Z_i) | r_i = r] - \lim_{r \uparrow 0} E[Y_i(0, Z_i) | r_i = r] \quad (A72)$$

$$= \lim_{r \downarrow 0} E[\beta_i + \rho_i Z_i + \eta_i | r_i = r] - \lim_{r \uparrow 0} E[\rho_i Z_i + \eta_i | r_i = r] \quad (A73)$$

$$= E(\beta_i | r_i = 0), \quad (A74)$$

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  $\hat{\pi} = E(\pi | r_i = 0)$ .

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

$$\hat{\gamma} = \frac{E[\tilde{Z}_i (\beta_i T_i + \rho_i Z_i + \eta_i)]}{E[\tilde{Z}_i (\pi_i T_i + \theta_i Z_i + v_i)]}. \quad (A75)$$

Analyze term by term.

$$E(\tilde{Z}_i T_i \beta_i) = E[E(\tilde{Z}_i T_i \beta_i | r_i)] = E[E(\tilde{Z}_i | r_i) T_i E(\beta_i | r_i)] = 0. \quad (A76)$$

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  $L[Z_i | \mathbf{X}_i^{(0)}] = E[Z_i | \mathbf{X}_i^{(0)}]$ . Similarly,

$$E(\tilde{Z}_i T_i \pi_i) = 0, \quad (\text{A77})$$

$$E(\tilde{Z}_i \eta_i) = 0, \quad (\text{A78})$$

$$E(\tilde{Z}_i v_i) = 0. \quad (\text{A79})$$

Therefore,

$$\hat{\gamma} = \frac{E(\tilde{Z}_i Z_i \theta_i \gamma_i)}{E(\tilde{Z}_i Z_i \theta_i)} = \frac{E[E(\tilde{Z}_i Z_i | r_i) E(\theta_i \gamma_i | r_i)]}{E[E(\tilde{Z}_i Z_i | r_i) E(\theta_i | r_i)]} = \frac{E[\text{Var}(Z_i | r_i) E(\theta_i \gamma_i | r_i)]}{E[\text{Var}(Z_i | r_i) E(\theta_i | r_i)]}. \quad (\text{A80})$$

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

$$\hat{\gamma} = \frac{E(\tilde{Z}_i M_i \gamma_i)}{E(\tilde{Z}_i M_i)}. \quad (\text{A81})$$

Taken together,

$$\hat{\beta} - \hat{\tau} = \hat{\gamma} E(\pi_i | r_i = 0) \quad (\text{A82})$$

$$= \hat{\gamma} E(\pi_i | r_i = 0) + E(\gamma_i \pi_i | r_i = 0) - \text{Cov}(\gamma_i, \pi_i | r_i = 0) + E(\gamma_i | r_i = 0) E(\pi_i | r_i = 0) \quad (\text{A83})$$

$$= \underbrace{E(\gamma_i \pi_i | r_i = 0)}_{\text{AIE}} + \underbrace{[\hat{\gamma} - E(\gamma_i | r_i = 0)] E(\pi_i | r_i = 0)}_{\text{bias 1}} - \underbrace{\text{Cov}(\gamma_i, \pi_i | r_i = 0)}_{\text{bias 2}}, \quad (\text{A84})$$

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

## J.4 Sensitivity Test

Our results imply that  $\hat{\beta} - \hat{\tau}$  identifies AIE if  $\gamma_i$  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  $\gamma_i$  heterogeneous.

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

$$\text{Bias} = \hat{\gamma} \hat{\tau} - \underbrace{E(\gamma_i \pi_i)}_{\text{AIE}} \quad (\text{A85})$$

$$= [\hat{\gamma} - E(\gamma_i)] \hat{\tau} - \text{Cov}(\gamma_i, \pi_i) \quad (\text{A86})$$

$$= \text{Cov}(\phi_i, \gamma_i) \hat{\tau} - \text{Cov}(\gamma_i, \pi_i) \quad (\text{A87})$$

$$= \rho_{\phi\gamma} \sigma_{\phi} \sigma_{\gamma} \hat{\tau} - \rho_{\gamma\pi} \sigma_{\gamma} \sigma_{\pi}, \quad (\text{A88})$$



where  $\phi_i = \frac{\tilde{Z}_i M_i}{E(\tilde{Z}_i M_i)}$ ,  $\rho_{\phi\gamma} = \text{Corr}(\phi_i, \gamma_i)$ ,  $\rho_{\gamma\pi} = \text{Corr}(\gamma_i, \pi_i)$ ,  $\sigma_\phi = SD(\phi_i)$ ,  $\sigma_\gamma = SD(\gamma_i)$ , and  $\sigma_\pi = SD(\pi_i)$ . If there is knowledge of  $\text{Cov}(\phi_i, \gamma_i)$  and  $\text{Cov}(\gamma_i, \pi_i)$ , 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  $\gamma_i$  has the same sign. All  $\pi_i$  has the same sign.  $\gamma_i$  and  $\pi_i$  are uniformly distributed.

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

$$\widehat{\widehat{AIE}} = \left[ \frac{\sqrt{3}\hat{\gamma}}{\sqrt{3}\text{sgn}(\gamma_i) + \rho_{\phi\gamma}\hat{\sigma}_\phi} + \frac{\rho_{\gamma\pi}\hat{\sigma}_\phi \text{sgn}(\pi_i)}{\sqrt{3}} \right] \hat{\pi}. \quad (\text{A89})$$

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

*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,  $E(\gamma_i) = \sqrt{3}\text{sgn}(\gamma_i)\sigma_\gamma$ . Thus,  $\sigma_\gamma = \frac{\hat{\gamma}}{\sqrt{3}\text{sgn}(\gamma_i) + \rho_{\phi\gamma}\sigma_\phi}$ . Since  $\pi_i$  satisfies a uniform distribution and  $\hat{\pi} = E(\pi_i)$ ,  $\sigma_\pi = \frac{\hat{\pi}\text{sgn}(\pi_i)}{\sqrt{3}}$ . Taken together,

$$AIE = \left[ \frac{\sqrt{3}\hat{\gamma}}{\sqrt{3}\text{sgn}(\gamma_i) + \rho_{\phi\gamma}\sigma_\phi} + \frac{\rho_{\gamma\pi}\sigma_\phi \text{sgn}(\pi_i)}{\sqrt{3}} \right] \hat{\pi}. \quad (\text{A90})$$

In this expression,  $\sigma_\phi$  is unknown. However, consider estimator  $\sqrt{\frac{1}{n} \sum_{i=1}^n \left( \frac{\tilde{Z}_i M_i}{\frac{1}{n} \sum_{i=1}^n \tilde{Z}_i M_i} - 1 \right)^2}$ , 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

$$\widehat{\widehat{AIE}} = \left[ \frac{\sqrt{3}\hat{\gamma}}{\sqrt{3}\text{sgn}(\gamma_i) + \rho_{\phi\gamma}\hat{\sigma}_\phi} + \frac{\rho_{\gamma\pi}\hat{\sigma}_\phi \text{sgn}(\pi_i)}{\sqrt{3}} \right] \hat{\pi}. \quad (\text{A91})$$

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

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

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

*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 operationalize this, rewrite  $Z_i$ :

$$Z_i = \sum_h s_{h \rightarrow i} \times \bar{\Delta} Trade Shock_h \quad (A93)$$

$$= \sum_h s_{h \rightarrow i} \times \left( \sum_k s_{hk} \bar{\Delta} g_k \right) \quad (A94)$$

$$= \sum_k \left( \sum_h s_{h \rightarrow i} s_{hk} \right) \bar{\Delta} g_k \quad (A95)$$

$$\equiv \sum_k \tilde{s}_{ik} \tilde{g}_k. \quad (A96)$$

$\tilde{s}_{ik} \equiv \sum_h s_{h \rightarrow i} s_{hk}$  captures origin production specialization of an average migrant in prefecture  $i$ .  $\tilde{g}_k \equiv \bar{\Delta} g_k = g_{k,2015} - \frac{1}{4} \sum_{t=2011}^{2014} g_{kt}$  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:

$$\tilde{q}_k = \beta_0 + \beta_1 \tilde{g}_k + v_k. \quad (A97)$$

$\tilde{q}_k = \sum_i \frac{\tilde{s}_{ik}}{\sum_i \tilde{s}_{ik}} v_i$ .  $v_i$  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  $\beta_1$ . If  $\beta_1$  is close to zero, it suggests that  $\tilde{g}_k$  is not distributed in 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  $\beta_1$  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
<b>Panel A: Pretrends</b>		
$\Delta \text{Log population, 2009–2010}$	-0.777	(0.556)
$\Delta \text{Log GDP, 2009–2010}$	0.220	(0.199)
$\Delta \text{Log expenditure on social security, 2009–2010}$	0.323	(0.713)
$\Delta \text{Log expenditure on public security, 2009–2010}$	-0.056	(0.233)
<b>Panel B: Predetermined characteristics</b>		
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 product level, on the product-level trade shock change. Standard errors are clustered at the 2-digit HS section level. For readability, all estimated coefficients are multiplied by 1,000,000.

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

## K.2 Robustness

**Controlling for In-Migration.** In Table A18, we conduct a robustness check for our causal mediation analysis by controlling for in-migration, which is measured by the change in the average population growth rate from the pre-reform period to the post-reform period. Apparently, controlling for in-migration does not markedly change our results. If anything, it slightly accentuates the importance of the outmigration channel.

**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 A19 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 an important role. 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  $\gamma$  in Equation 16 to violations of the exclusion restriction, we use the methodology developed by [Conley et al. \(2012\)](#).

**Table A18.** *Hukou* Reform, Outmigration Rate, and Labor Unrest—Controlling for In-Migration

	$\bar{\Delta}\text{Unrest}/L$ , 2011–2015			$\bar{\Delta}\text{Unrest}/L$ , 2011–2019		
	(1) Baseline	(2) Mediation-OLS	(3) Mediation-IV	(4) Baseline	(5) Mediation-OLS	(6) Mediation-IV
Reform [ $\beta$ or $\tau$ ]	-1.039** (0.520)	-0.839 (0.538)	-0.369 (0.613)	-1.435*** (0.372)	-1.249*** (0.366)	-1.023*** (0.386)
$\bar{\Delta}\text{Outmigration}$ [ $\gamma$ ]		1.795*** (0.464)	6.011*** (1.324)		1.675*** (0.365)	3.705*** (1.039)
$\bar{\Delta}\text{Population growth}$	0.855 (0.761)	0.685 (0.745)	0.287 (0.773)	0.841 (0.606)	0.683 (0.578)	0.491 (0.561)
Polynomials	Yes	Yes	Yes	Yes	Yes	Yes
% Total effect explained		0.192	0.645		0.130	0.287
Effective $F$ stat.			57.905			57.905
Observations	282	282	282	282	282	282

Note: This table reports causal mediation analysis, controlling for the influence of in-migration that is measured by the population growth rate. Columns (1)–(3) examine the immediate effect between 2011 and 2015. Columns (4)–(6) examine the longer-term effect between 2011 and 2019. The effective  $F$  statistic is calculated following [Olea and Pflueger \(2013\)](#). Robust standard errors are reported in parentheses.

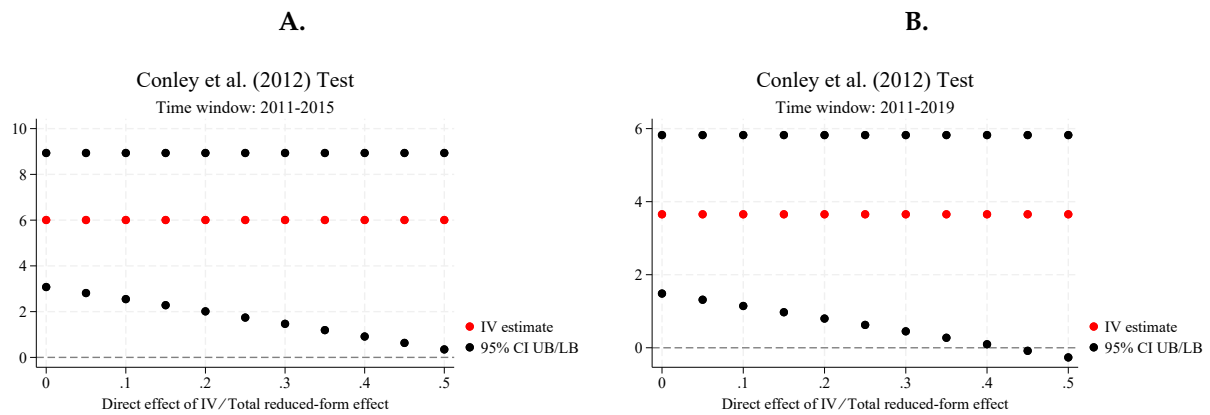
\*  $p < 0.1$  \*\*  $p < 0.05$  \*\*\*  $p < 0.01$

This approach allows the IV,  $Z_i$ , to directly enter the second stage of the model (Equation 16) with a coefficient of  $\psi$ , 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  $\psi$ . Since we find that outmigration has a positive effect, violations of the exclusion restriction are problematic only when  $\psi$  is positive. We look at the largest positive value of  $\psi$  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,  $\psi_{RF}$ . 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 A19 reports the results, where we plot the 95% confidence interval of the 2SLS estimate  $\hat{\gamma}$  against  $\psi/\psi_{RF}$ . Figure A19B shows that for the long time window (2011–2019),  $\hat{\gamma}$  is not statistically significant at the 5 percent level when  $\psi/\psi_{RF}$  reaches 0.4, i.e.,  $\bar{\psi}/\psi_{RF} = 0.4$ . When it comes to the short time window (2011–2015), Figure A19A shows that  $\bar{\psi}/\psi_{RF} = 0.5$ . These results indicate that 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.<sup>20</sup>

<sup>20</sup>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).



**Figure A19. 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).

**Table A19.** *Hukou* Reform, Outmigration Rate, and Labor Unrest—Alternative IVs

	Baseline		Alt. Network I		Alt. Network II	
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Panel A: Time window = 2011–2015</b>						
Reform	-1.047** (0.518)	-0.411 (0.608)	-1.033** (0.519)	-0.146 (0.663)	-0.979* (0.520)	-0.072 (0.707)
$\bar{\Delta}$ Outmigration		6.005*** (1.304)		7.996*** (2.179)		7.946*** (3.055)
Polynomials	Yes	Yes	Yes	Yes	Yes	Yes
Network in 2015 census	Yes	Yes				
Network in 2015 census, only $\geq 300$ km origins			Yes	Yes		
Network in 2015 census, only $\geq 500$ km origins					Yes	Yes
% Total effect explained		0.607		0.858		0.927
1st stage coef.		0.049		0.019		0.013
Effective <i>F</i> stat.		60.978		18.870		9.386
Observations	287	287	283	283	271	271
<b>Panel B: Time window = 2011–2019</b>						
Reform	-1.419*** (0.370)	-1.033*** (0.383)	-1.388*** (0.372)	-0.901** (0.412)	-1.336*** (0.373)	-0.762 (0.463)
$\bar{\Delta}$ Outmigration		3.654*** (1.018)		4.392*** (1.539)		5.036** (2.312)
Polynomials	Yes	Yes	Yes	Yes	Yes	Yes
Network in 2015 census	Yes	Yes				
Network in 2015 census, only $\geq 300$ km origins			Yes	Yes		
Network in 2015 census, only $\geq 500$ km origins					Yes	Yes
% Total effect explained		0.273		0.351		0.430
1st stage coef.		0.049		0.019		0.013
Effective <i>F</i> 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 settlement intentions mechanism, as captured by the outmigration rate. 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 effective *F* statistic is calculated following [Olea and Pflueger \(2013\)](#). Robust standard errors clustered at the prefecture level are reported in parentheses.

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

## 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 CMD5 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 A20 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 B 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 fewer prefectures as reform ones. Nonetheless, different definitions of reform status do not change the results markedly. The major change is due to the 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 A21).

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 on 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 A20.** Replicating Main Results of [An et al. \(2024\)](#)

	(1)	(2)	(3)	(4)	(5)	(6)
	Working	Log wage	ASS	Working	Log wage	ASS
<b>Panel A: An et al.'s definition of treatment</b>						
Reform (An et al.) $\times$ Post	0.006 (0.006)	-0.077*** (0.018)	-0.041** (0.018)	-0.001 (0.009)	0.018 (0.021)	0.008 (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
<b>Panel B: Our definition of treatment</b>						
Reform $\times$ Post	0.007 (0.006)	-0.087*** (0.016)	-0.038** (0.018)	0.004 (0.009)	-0.004 (0.021)	0.010 (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 A21.** DiD versus DiDC Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	Working	Log wage	ASS	Working	Log wage	ASS
Reform $\times$ Post	-0.005 (0.005)	-0.066*** (0.013)	-0.047** (0.018)	-0.003 (0.007)	0.003 (0.019)	0.006 (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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