

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

Weizheng Lai  
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Yu Qiu\*

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## Abstract

There are growing concerns that lax 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. Using the reform's population cutoff rule as identifying variation, we find that the reform significantly reduced labor unrest. We suggest that one mechanism behind our finding is the enhancement of migrants' settlement intentions, which increases their dependence on the state and promotes more obedient behavior. Evidence shows that the reform raised the likelihood of migrants staying in their destinations. Through a novel causal mediation analysis, we find that heightened settlement intentions explain up to 27 percent of the reform's total effect on labor unrest. We find no evidence that the reform led to compositional changes among migrants, immediate deliveries of benefits to migrants, and tighter government social control. Our results highlight the influence of migration policy on stability by shaping migrants' prospects in their destinations.

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

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

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\*Lai: Department of Economics, University of Maryland, [laiwz@umd.edu](mailto:laiwz@umd.edu). Qiu: Department of Economics, University of Pittsburgh, [yuq23@pitt.edu](mailto:yuq23@pitt.edu). Lai is indebted to Allan Drazen, Ethan Kaplan, and Peter Murrell for their guidance and encouragement throughout this project. We are grateful for helpful comments from Daniel Berkowitz, Osea Giuntella, Muhammad Yasir Khan, Palak Suri (discussant), Edoardo Teso, Sergio Urzúa, Sining Wang (discussant), Yang Xie, and David Yang, as well as many conference and seminar participants. All errors are our own.

# 1 Introduction

The distribution of opportunities lies at the heart of politics — as Harold Lasswell (1936) famously claimed, politics is about “who gets what, when, and how.” The arrangement of this distribution is often deeply intertwined with considerations of sociopolitical stability, regardless of the regime type. Indeed, throughout history, a variety of institutions can be viewed as adjusting the distribution of opportunities between classes to address potential conflicts. For example, the extension of the franchise in Western societies during the 19th century was employed by elites to stave off social unrest (Acemoglu and Robinson, 2000). The world’s first welfare state was created by Bismarck to appease the working class amidst rising social democratic movements. Imperial China instituted the civil service exam as a power-sharing mechanism between aristocracy and intellectuals (Huang, 2023). More recently, post-WWII East Asia witnessed land redistribution programs that aimed to weaken the landed class and to win the support of the peasantry (Jansen, 2002; Tsai, 2015; Kapstein, 2017).

Besides class, geography can be another natural, significant, albeit non-orthogonal cleavage. With spatially uneven development, migration barriers, in the forms of movement or settlement restrictions, effectively function as institutions governing the (geographical) distribution of opportunities. Interestingly, many concerns about reforms to migration policy revolve around their social stability consequences, as exemplified by autocrats’ control of population movements to shield their seat of power, voters’ fears of immigration-associated turmoil, as well as politicians’ anti-immigrant appeals (Feler and Henderson, 2011; Hangartner et al., 2019; Campante et al., 2020).<sup>1</sup>

Given the popular concerns, it is crucial to understand the causal linkage between migration barriers and social stability. This linkage is relevant to policy design and offers insights into how geography shapes political behaviors. Existing theory gives mixed predictions due to the multiplicity of intervening factors. On the one hand, a more open-door migration policy may extend migrants’ horizons in destinations and thus incentivize investments in integration (Adda et al., 2022); if this forward-looking behavior increases the cost to protest, one may expect less unrest engagement at the individual level and thus likely a higher level of overall stability. On the other hand, relaxing migration barriers may lead to an increase in population size, thereby causing instability because of more competition for limited resources (Acemoglu et al., 2020). Meanwhile, changes in population composition can have ambiguous influences on social stability; more or less resentful individuals may come or leave (Hirschman, 1970). Relatedly, empirical studies on the effect of immigration on crime do not reach a consensus (e.g., Spenkuch, 2014; Ajzenman et al.,

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<sup>1</sup>Feler and Henderson (2011) find that the dictatorial government of Brazil in the 1980s strategically withhold water access for urban slums to deter in-migration. Hangartner et al. (2019) show that mere exposure to refugee arrivals induces lasting anti-immigrant concerns (e.g., crime, terrorist attacks, and burden on the country). Campante et al. (2020) show that during the 2014 US midterm elections, the Republican candidates drew associations between the Ebola in connection and immigration and terrorism in newsletters and campaign ads, despite the fact that Ebola had no real impacts in the US.

2023).<sup>2</sup> As such, it is an empirical question of how the dynamics of social stability respond to migration policy.

To help answer this question, we exploit a natural experiment in China: In July 2014, the Chinese government initiated a nationwide reform to the household registration system (known as *hukou*), which substantially removed the barriers to permanent settlement in less urbanized regions. China's *hukou* system functions like an "internal passport system," which ties a person to a locality and restricts their access state transfers and social services (e.g., social security and public education) only in their registration locality (Ngai et al., 2019). During the Maoist era, the *hukou* system was strictly enforced so that free mobility was barred. Since Deng's economic reforms, movements were in general not restricted, however, transfers of *hukou* registration remained difficult, especially rural-urban transfers. As a result, the *hukou* system engendered material settlement barriers for migrants and made migration temporary.

We estimate the causal effect of the 2014 *hukou* reform on labor unrest during 2011–2019. Labor unrest is a category of social turmoil that the Chinese state remains very vigilant about, despite its mostly apolitical, non-antiregime nature (Friedman, 2014). We find that the *hukou* reform reduced the labor unrest rate by 1.419 incidents per million working-age population. This effect is sizeable, amounting to 42 percent of the mean of non-reform regions. We show that this effect cannot be explained by changes in population size — the reform has no discernible effect on total population during the period we study. In addition, we present results derived from a supplementary identification strategy, which explore heterogeneous responses by *hukou* reform status to fluctuation in global demand for manufactured goods. We find that though the exogenous negative shock on average increases unrest, the impact is much weaker in reform regions, suggesting that the reform may have altered individuals' calculations of participating in unrest.

We argue that one potential mechanism is migrants' heightened settlement intentions, which caused disengagement in unrest. The theory of political control suggests that people may be coerced into obedience if they are dependent on the state for welfare and resources (e.g., Albertus, 2015). When it comes to our case, the *hukou* reform opens up an opportunity for migrants to permanently settle in the destination, a possibility often unattainable before the reform. Therefore, the reform can create dependence on the state among those who value this opportunity. We provide a simple model to rationalize this mechanism. Echoing our hypothesis, we find that the destination's reform significantly retained more migrants, as reflected by the decrease in the outmigration rate among preexisting migrants. We moreover find that the retention effect is more pronounced amongst more forward-looking migrants, who value future benefits of settlement to a larger degree and thus are more willing to forgo today's gains from engagement in unrest. In addition, using a novel causal mediation analysis method, we show that heightened retention does reduce the unrest rate and it

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<sup>2</sup>Spenkuch (2014) find a small positive effect of immigration on crime in the US. In contrast, Ajzenman et al. (2023) report a null effect in Chile, however, they find that immigration leads to a (*mis-*)perception of increased crime.

can account for 27.3 percent of the *hukou* reform's total effect on the unrest rate. We reject other alternative mechanisms that *hukou* reform may have reduced unrest rate because of (i) shifts in population's characteristics, (ii) immediate improvements in migrants' labor market outcomes such as employment, wages, and access to social security, and (iii) local states' efforts to control society.

Underpinning our research design is a population-based rule that determines reform status. The 2014 *hukou* reform substantially relaxed *hukou* transfers into regions with less than 3 million urban population while maintaining strict restrictions for more populous regions. Exploiting this feature, we implement an identification strategy that combines difference-in-differences and regression discontinuity designs (DiD-RD). In a nutshell, we estimate the reform's effect by comparing the evolution of unrest between reform and non-reform regions near the reform cutoff. Identification follows if the underlying trends vary smoothly across the reform cutoff. We perform a variety of exercises to examine this smoothness assumption. We show that there is no discontinuity in the distribution of the urban population size around the 3 million cutoff, both before and after the reform, suggesting a lack of sorting into certain reform status. We find that reported growth in the urban population has no association with a measure of local officials' promotion prospects, suggesting the absence of deliberate manipulation of urban population due to certain political incentives. Furthermore, we show that in our research design, there are no strong differential pre-reform trends in the unrest rate and variables that may be conducive to occurrences of unrest, including population, GDP, fiscal expenditures, and expenditures on public security (i.e., policing).

To quantify the importance of the proposed mechanism, namely, heightened settlement intentions, we develop a novel causal mediation method. The conventional practice in social sciences, popularized by [Baron and Kenny \(1986\)](#), relies on the comparison of coefficients on the treatment of interest (here the *hukou* reform) between a model that excludes the mechanism variable and a model that includes the mechanism variable. The attenuation in the coefficient on treatment is thus interpreted as the treatment effect that goes through the proposed mechanism. Though intuitive, this approach requires a strong assumption to consistently estimate the causal effect of the mechanism variable: The mechanism variable must be exogenous conditional on the treatment (e.g., among others, [Imai et al., 2011](#)). This assumption may be not plausible due to the existence of other interrelated post-treatment confounders. We make improvements to this approach by using an instrumental variable (IV) for our proposed mechanism, i.e., heightened settlement intentions as captured by decreased outmigration rates. Under an assumption of homogeneous effects of the mechanism variable, we can identify the causal effect that can be attributed to heightened settlement intentions. Specifically, we construct a shift-share IV that leverages variation in trade shocks in migrants' origins. We conduct several validation exercises for this IV in light of the recent econometric literature on shift-share IVs ([Borusyak and Hull, 2024](#)). For the first stage, we find that negative trade shocks at home strongly predict migrants' stay in their destinations. With this instrumentation, we find much greater importance of the settlement intentions mechanism than using the conventional approach — 27.3 percent of the reform's effect

can be attributed to heightened settlement intentions. We also provide a sensitivity test if one were to relax the homogeneity assumption: We impose minimal distributional assumptions for heterogeneous effects to quantify bias resulting from the relaxation. Still, the results suggest that heightened retention intentions explain a nontrivial share of the total effect of *hukou* reform on labor unrest.

In sum, we show evidence of the relaxation of migration barriers — in our case, driven by China’s *hukou* reform — having a dampening effect on social stability as measured by labor unrest. To some degree, this is surprising given the popular concerns that an open-door migration policy could raise social turmoil. Although we cannot exhaust all alternative underlying mechanisms, we provide evidence suggesting that the removal of migration barriers could stimulate migrants’ adoption of obedient political behaviors that facilitate their integration and pay in the longer run, which is analogous to international immigrants’ economic choices for assimilation (e.g., human capital investment). This finding can certainly depend on the context in question — for instance, if the state does not have credible command over people’s welfare, i.e., the biopower as conceptualized by Michael Foucault (1990), then obedience may not be coerced. The Chinese state may be special in this regard as it possesses strong biopower, given its monopoly of all kinds of resources and its overwhelming dominance over civil society (Acemoglu and Robinson, 2020, Ch. 7). Yet, we envision that the mechanism we discover may apply broadly to autocratic regimes. Even in democracies, there can be scope for the mechanism to play out. For instance, Gonçalves et al. (2024) argue that heightened immigration enforcement in the US could increase crime as the fears it creates may discourage victims from reporting offenses.

Our paper relates to several strands of literature. First and foremost, this paper adds to the political-economy literature on the determinants of social unrest. One factor that frequently emerges from studies in this domain is income shocks (Dube and Vargas, 2013; Ponticelli and Voth, 2020; Caprettini and Voth, 2020; Campante et al., 2023). Our paper differs from this body of work as we do not detect significant immediate income changes due to the *hukou* reform. Rather, we document the role of extended horizons in the form of heightened retention intentions.

Second, by studying the linkage between migration institutions and social unrest, we contribute to research on political control (for a review, see Hassan et al., 2022). Specifically, our paper speaks to the non-violent tactics used by the state to induce compliance of the citizenry. This can be done via deliberate interventions: One prominent example is buying hearts and minds through government transfers and employment (e.g., Pan, 2020; Rosenfeld, 2021; Borjas, 1980; Fish, 1905). In contrast, our results show that even a less politically deliberate policy could induce compliance, so long as it stimulates a perceived dependency on the state.

Third, our paper contributes to a small literature on China’s *hukou* system. Exploiting variation due to different episodes of *hukou* reforms, several papers have studied the economic consequences of *hukou*-induced migration barriers, including labor market outcomes, marriage market matching,

and productivity, among others (e.g., [An et al., 2024](#); [Han et al., 2015](#); [Ngai et al., 2019](#)). However, there is little attention to the *hukou* system’s *political* implications. In some sense, this is surprising since scholars have the consensus that the *hukou* system is bad economics, and some have hinted that the system continues to exist due to political constraints.<sup>3</sup> Then, a natural question is whether the *hukou* system is good politics. Our paper fills this considerable gap, and our results suggest that the *hukou* system can even be bad politics, as the denial of migrants’ settlement in the destination implicitly creates incentives for participation in unrest.

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

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

## 2 Institutional Context

### 2.1 China’s *Hukou* System

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

**A Brief Overview.** The *hukou* (household registration) system was instituted in 1958. It assigns each Chinese citizen a *hukou* certificate upon birth, which ties them to a locality (typically parents’ registration locality), and on that basis, the system determines a person’s eligibility for state transfers and public services. The *hukou* status has two aspects: location and type. A person can only have access to state transfers and public services in their registration locality, even if their *de facto* residential location is different. For a long time, there were two types of *hukou*:

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<sup>3</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: e.g., [Adamopoulos et al. \(2024\)](#); [Gai et al. \(2024\)](#); among others.

agricultural and non-agricultural. Rural residents typically got an agricultural *hukou*,<sup>4</sup> and they were given land for cultivation to feed themselves, and they were given access to some social services provided by their rural localities. Urban residents obtained a non-agricultural *hukou*, and they were expected to work in factory or office jobs and had access to social benefits, many of which were job-related and included food rations, subsidized medical care, education for their children, and social assistance, funded by the urban district that issued their *hukou*. Starting from the 1990s, some localities gradually removed the distinction in *hukou* types, and this was eventually extended to the entire country in 2014 (in fact, part of the reform we study). However, the key aspect of the *hukou* system remains unchanged: One can only have access to state transfers and public services in their registration locality.

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

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

**The 2014 Reform.** On July 24, 2014, the Chinese central government initiated a nationwide reform drive to the *hukou* system (State Council, 2014a). Unlike previous reforms that had been carried out by local governments, this one was centrally mandated. At the time, it was widely

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<sup>4</sup>An exception is the officials who work in rural areas.

perceived as one of the strongest attempts to reform the *hukou* system reform in the previous two decades (Wang et al., 2023).

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

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

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

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

## 2.2 Labor Unrest in China

In this section, we provide a brief background on labor unrest in China. Appendix A.1 offers a more detailed discussion.

<sup>5</sup>Having a stable job is defined as having an employment contract or being a business owner (with minimum requirements on tax payments and/or registered capital). Having a stable residential place requires either a rental contract registered with the government or home ownership.



Despite China's autocratic regime, labor unrest is common in China. Several structural factors have contributed to this phenomenon. Noticeably, voluminous studies on China's labor politics stress the role of institutional discrimination against migrant labor as a result of the *hukou* system (e.g., Lee, 2007; Chan, 2010; Friedman, 2014; Rho, 2023, among others). Restricted social and economic mobility for migrants due to limited rights, combined with employers' rampant violations of basic statutory protections, fuel migrants' grievances and thus contribute to the occurrences of labor unrest. More recently, legal reforms and labor shortages in labor-intensive sectors have shifted bargaining power in favor of migrant workers (Gallagher, 2017; Elfstrom and Kuruvilla, 2014). Despite lack of official data on migrant labor's participation in labor unrest, based on various sources of anecdotes and fieldwork, many scholars believe that migrant workers have made up the majority of participants in labor unrest, especially those offensive actions that demand more interests other than defend minimum rights (Friedman, 2014; Rho, 2023; Goebel, 2019).<sup>6</sup> Corroborating this view, Figure A7 shows a positive relationship between the labor unrest rate and the share of migrants in a region.

The Chinese state remains vigilant about labor unrest, and it has been increasingly so in the recent decade (Franceschini and Nesossi, 2018; Rho, 2023). Lorentzen et al. (2013) argue that the central government strategically tolerates labor unrest where workers voice demands for their rights and interests, because unrest can serve as a signal for the central government to identify discontented groups, and on that basis, it can help the central government to allocate resources to address grievances and manage local officials accordingly. However, the tolerated space of unrest has been codified in informal rules with an implicit warning that those who cross the boundary of acceptable protests will be repressed. It is not tolerated when an unrest event tends to extend to a mass collective action that threatens social stability. For instance, Rho (2023) finds that police are much more likely to intervene when workers go beyond the factory compound to protest. The regime has strictly restricted and punished independent labor organizing and social mobilization across workplaces or regions (Chen and Gallagher, 2018). Additionally, due to the cadre evaluation system's emphasis on stability maintenance (Edin, 2003), local officials respond to unrest seriously and use various measures to reduce potential threats to stability (Campante et al., 2023; You et al., 2022).

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<sup>6</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), in 2010, migrant workers' labor disputes comprised nearly 70 percent of all labor disputes in Beijing. 2011). Goebel (2019) analyzes social unrest on social media and finds that migrant workers have engaged in the largest number of protests.

## 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. The prefecture, sometimes referred to as the prefectural city or city in the literature on the Chinese economy, is the administrative level between the province and the county. There are 333 prefectures in total.<sup>7</sup> We also include 4 provincial-level municipalities, i.e., Beijing, Tianjin, Shanghai, and Chongqing. For brevity, we call them prefectures in this paper. When constructing the sample, we exclude prefectures in Tibet and Xinjiang, which have political environments that are different from others. Our final sample consists of 287 prefectures for which data on urban population is available to define reform status (discussed next). According to the population census of 2010, these prefectures cover 94.4 percent of the total population and 95.8 percent of the urban population in China.

**Reform Status.** As discussed in Section 2, a prefecture’s reform status is determined by whether its urban population falls below 3 million. Thus, it is crucial to consider how the Chinese government counts the population. According to [State Council \(2014b\)](#), a prefecture’s urban population includes all residents who have been in urban districts for more than six months: It should include both natives and migrants, regardless of *hukou* registration status. We use the Urban Construction Statistical Yearbook (UCSY) published by the Ministry of Housing and Urban-Rural Development, which uses an identical definition as in [State Council \(2014b\)](#).<sup>8</sup> We use urban population in 2014, the year when the reform took place, to define reform status: The dummy variable,  $Reform_i$ , equals one if prefecture  $i$ ’s urban population falls below 3 million. Under this definition, 37 prefectures in the sample are non-reform prefectures, while the other 250 are classified as reform prefectures.

One may wonder if the prefectural government adjusts its reform status over time as its urban population changes. To the best of our knowledge, this is not the case. For all 287 prefectures in our sample, we review their government documents regarding the *hukou* reform.<sup>9</sup> We find that by 2015, most prefectural governments had guidelines for implementing the central government’s directive, and we do not observe amendments made during subsequent years. Therefore, it is reasonable to use the urban population of 2014 to define reform status.

We conduct verification of this population-based definition. Based on our reading of the government documents, we manually code up each prefecture’s reform status and compare it with

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<sup>7</sup>This is based on the delineation in 2010. There are no significant changes over time.

<sup>8</sup>The UCSY reports the urban native population and urban migrant population separately. We aggregate urban natives and migrants to obtain the total urban population. A previous paper, [An et al. \(2024\)](#), also uses the UCSY to measure reform status, however, they only use the urban native population. In Appendix B.9, we replicate their key results and compare them to results when different empirical decisions are made.

<sup>9</sup>These documents are from government websites, news reports, as well as a database on *hukou* reforms built by [Zhang and Lu \(2019\)](#).

the population-based reform status. The population-based definition exhibits remarkable accuracy. Out of 287 prefectures in the sample, there are only 17 disagreements ( $17/287 = 6\%$ ) between manual coding and population-based definition.

**Labor Unrest.** Our data on labor unrest are from the China Labor Bulletin (CLB), a non-profit organization based in Hong Kong that has monitored incidents of collective worker actions across mainland China since 2011.<sup>10</sup> We use events during 2011–2019, preceding the outbreak of COVID-19. Due to the lack of administrative data on labor unrest in China, this dataset has been frequently cited by news media outside China to examine trends in Chinese workers’ actions (e.g., [Hernández, 2016](#)); it is also widely used by research on social unrest in China (e.g., among others, [Campante et al., 2023](#); [Qin et al., 2024](#)).

Human coders of the CLB collect information on unrest events primarily from China’s domestic social media platforms, Weibo, WeChat, Douyin, Kuaishou, and others. The coders verify the accuracy of collected information and incorporate into the dataset only the events with complete information on the location, date, workers’ demands, the industry, and the relevant company. Given how the CLB dataset is built, one should consider events in the dataset as those arguably more severe labor conflicts, where workers end up taking to the street and demand public attention.

A natural question is to what degree CLB data reflect underlying patterns of labor conflict in China. We show that the events in CLB data exhibit similar trends as in other data sources of labor conflict. We draw a comparison to the Global Database of Events, Language, and Tone (GDELT), one commonly used dataset on social unrest at the global level (e.g., see a review by [Cantoni et al., 2023](#)). The GDELT Project has conducted automated scraping of the world’s news media since 1979. In the GDELT data, we define a labor unrest event as any event that falls in the “Protest” category and has labor involved. Because the CLB specifically focuses on labor unrest and has human coders carrying out data collection and cleaning, it includes many more labor unrest events than the GDELT. Nonetheless, at the national level, both datasets display quite similar trends in labor unrest (see Figure A8).

**Auxiliary Data.** We use multiple auxiliary datasets for validating the research design and exploring mechanisms. They include prefecture-level covariates from various sources, microfiles of population censuses, migrant surveys, and so on. Appendix A.4 describes these data sources, and we will introduce them when they become pertinent to the analysis.

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

To estimate the causal effect of *hukou* reform, an intuitive strategy is a difference-in-differences (DiD) design, which compares the trajectories of unrest between reform and non-reform prefectures.

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<sup>10</sup>We refer interested readers to the CLB website for more details: <https://clb.org.hk/en>.

It is implemented by 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 onward — we treat 2014 as the first year that the reform comes into effect.  $\lambda_i$  and  $\mu_t$  are prefecture and year fixed effects, respectively.  $\varepsilon_{it}$  is the error term clustered at the prefecture level.

The ordinary least squares (OLS) estimand of  $\beta$  identifies an average causal effect of the *hukou* reform on labor unrest, provided that a parallel trends assumption is met: The reform and non-reform prefectures have shared similar trends in unrest in the absence of reforms. However, the parallel trends assumption can be questionable in our setting. The DiD approach in essence compares less populous (reform) to more populous (non-reform) prefectures. One particular concern is that urban population itself, which determines the reform status, may be associated with differential patterns in unrest. For instance, [Acemoglu et al. \(2020\)](#) document that there is a positive causal relationship between population and conflict due to competition for scarce resources.

To make the research design more credible, we modify Equation 1 by explicitly including flexible controls for the urban population. Specifically, the regression model is as follows:

$$\frac{Unrest_{it}}{L_{i,2010}} = \beta (Reform_i \times Post_t) + \lambda_i + \mu_t + f [\Delta \log (P_{i,2014}); \zeta_{Reform,t}] + \varepsilon_{it}. \quad (2)$$

The newly included regressor,  $\Delta \log (P_{i,2014}) = \log(3) - \log (P_{i,2014})$ , 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.  $f$  is a polynomial function.  $\zeta_{Reform,t}$  is a vector of coefficients on  $\Delta \log (P_{i,2014})$  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 and regression discontinuity (DiD-RD), with  $\Delta \log (P_{i,2014})$  being the running variable. To estimate Equation 2, following [Gelman and Imbens \(2019\)](#), we let  $f$  be the first-order polynomial function. In most results, we use the full sample for estimation. Because the number of reform prefectures far exceeds the non-reform (250 versus 37), restricting to a narrow bandwidth around  $\Delta \log (P_{i,2014}) = 0$  may exclude a large portion of non-reform prefectures and lose much statistical power. Nonetheless, we demonstrate the robustness of our results using different bandwidths, despite the loss of statistical power.

In the spirit of RD, the estimated  $\beta$  identifies the average causal effect of the *hukou* reform at  $\Delta \log (P_{i,2014}) = 0$ , under the assumption that the trends of confounders vary smoothly around

$\Delta \log (P_{i,2014}) = 0$  in the absence of the *hukou* reform. This is a local version of the parallel trends assumption as opposed to the global one that Equation 1 requires.

### 3.3 Validity of Research Design

One potential pitfall of the identification assumption, as in studies using population as the running variable, is that the same population-based rule may determine policies other than the *hukou* reform (Eggers et al., 2018). If these policies are before the *hukou* reform and their effects do not vary over time, then the prefecture fixed effects would control for their influence. A more concerning scenario is that, a policy has different provisions across the 3 million cutoff and it is enacted at the same time as the *hukou* reform. If so, there is no way to disentangle the effects of the *hukou* reform and the other policy. To alleviate this concern, we extensively search for policies that are determined by population. We use the PKULaw database maintained by Peking University, a comprehensive source frequently used by research on policy-making in China (Wang and Yang, 2021; Tian, 2024). To the best of our reading, we do not find policies that rely on the same population-based rule.

The validity of RD also requires that the agents, in our case the prefectural governments, do not or cannot precisely manipulate their urban population to fall on either side of the cutoff (Lee and Lemieux, 2010). In principle, it is possible for prefectural governments to preemptively influence their statistical bureaus regarding the reporting of urban population. If so, the only possible direction of manipulation is to *overstate* urban population to avoid relaxation of the *hukou* system due to, e.g., concerns of turmoil brought by population inflows. The other direction is impossible because if a *hukou* reform involves any benefits that incentivize manipulation, prefectural governments in fact have no reason to do that, since they already have the discretion to implement a reform themselves, which makes manipulation unnecessary. Therefore, were manipulation pervasive, one would expect a significant bunching just below  $\Delta \log (P_{i,2014}) = 0$  (or equivalently, above 3 million). However, we do not observe this phenomenon in Figure 1, which presents the density of running variable  $\Delta \log (P_{i,2014})$ , though admittedly, the density just below  $\Delta \log (P_{i,2014}) = 0$  is slightly higher. McCrary’s test (McCrary, 2008) also confirms the smoothness of the density function. In addition, Figure A10 inspects the density of (log) urban population in 2015, the year when most prefectural governments had followed up the center’s reform initiative and thus manipulation could be more responsive if at all; however, we continue observing a smooth density function.

To further examine the possibility of manipulation, we relate (observed) urban population growth to local officials’ promotion incentives. A large body of literature has documented that promotion incentives play a critical role in determining Chinese bureaucrats’ choices and policy-making (e.g., among others, Wang et al., 2020; He et al., 2020; Jia, 2024). When it comes to our setting, if there exists manipulation of urban population to dodge reform, it ought to benefit local officials’ careers in certain ways. For instance, avoiding social instability due to population inflow may help

secure an official’s chance of promotion, given that the failure to maintain stability has been widely seen as a veto criterion for career advancement (Edin, 2003). One immediate implication of this argument is that officials with stronger promotion incentives would 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 A.4 for estimation details), and we find that it has no association with observed urban population growth (see Table A15). These results suggest the absence of manipulation.

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

$$\Delta W_{it} = \alpha_0 + \alpha_1 Reform_i + f[\Delta \log(P_{i,2014}); \zeta_{Reform,t}] + \mu_t + v_{it}, \quad t \leq 2013. \quad (3)$$

$\Delta W_{it}$  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 start with estimating Equation 3 without including the polynomial function  $f[\Delta \log(P_{i,2014}); \zeta_{Reform,t}]$ , thus,  $\alpha_1$  captures the average difference in pretrends between reform versus non-reform prefectures. Columns (1) and (2) report the estimated  $\alpha_1$  and the standard error. We find that reform prefectures on average have lower growth in unrest than their non-reform counterparts. We also find that reform prefectures have differential pretrends in other dimensions. They exhibit lower population growth, higher growth in total fiscal spending, and importantly, higher growth in spending on public security, which may explain the differential pattern in unrest. Interestingly, there is no significant difference in GDP growth. These patterns indicate that a simple DiD design cannot reliably estimate the causal effect of *hukou* reform on unrest.

We then estimate the complete specification of Equation 3, controlling for polynomials  $f[\Delta \log(P_{i,2014}); \zeta_{Reform,t}]$ . As in RD designs,  $\alpha_1$  captures pretrends differences among prefectures near the reform cutoff. Columns (3) and (4) in Panel A of Table 2 report the estimated  $\alpha_1$  and its standard error. Apparently, by comparing prefectures barely eligible for the reform with those barely ineligible, the inclusion of polynomial controls shrinks pretrends differences found in Columns (1) and (2) substantially, and eliminates three of four significant differences. Importantly, there are no longer differential trends in labor unrest. The lack of pretrends differences is also evident in the RD plots displayed by Figure A11A.

The pretrends checks lend support to our research design. To better understand reform and non-reform prefectures, we also examine the differences in predetermined characteristics between reform and non-reform prefectures. Specifically, we estimate a cross-sectional variant of Equation 3 with the dependent variable being a characteristic in the base year (2010, for which a population census is available). Inspired by existing research on unrest, we examine a set of variables that may

be associated with unrest occurrences, including the share of males, the share of migrants, the share of urban residents, the share of secondary and tertiary sector workers, as well as share of internet users. Panel B of Table 2 presents the results. As above, Columns (1) and (2) report estimates from the specification without including polynomials, and Columns (3) and (4) report estimates after adding polynomial controls. Apparently, one can see that the inclusion of polynomial controls largely attenuates the differences in predetermined characteristics, though there are still statistically significant imbalances in shares of migrants, urban residents, and tertiary sector workers. Figure A11B visualizes the RD regressions reported in Columns (3) and (4), suggesting that imbalances are likely due to outliers at the right tail of  $\Delta \log (P_{i,2014})$ . Recall that our research design only requires balance in underlying trends of unrest, so it allows for imbalance in covariates, as long as they are not associated with differential trends of unrest. In Section 4.3, we show the robustness of our results using a variety of strategies to control for possible covariate-related differential trends.

## 4 The Effect of *Hukou* Reform on Labor Unrest

### 4.1 Main Results

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

If we compare the dynamics between reform and non-reform prefectures, the dashed green line shows that reform prefectures exhibit a smaller growth rate in unrest relative to non-reform prefectures even before the reform initiative, as already hinted in Table 2, and this negative gap enlarges after the reform, yielding a relative decrease in reform prefectures’ unrest rates against overall trends.<sup>11,12</sup> In Figure 2B, this pattern is confirmed by the estimates of a TWFE event-study model (the dashed, light blue line).<sup>13</sup> As noted in Sections 3.2 and 3.3, this strategy cannot credibly estimate the *hukou* reform’s casual effect due to violations of the parallel trends assumption. Then, we implement our preferred research design to obtain a more credible estimate. The sold, dark blue line in Figure 2B reports the estimates from an event-study model that adds polynomial

<sup>11</sup>We note that 2017 is an exception. One possible reason is that 2017 featured a rather special political environment — the 19th National Congress of the Chinese Communist Party was held at the end of the year. The uniform, nationwide enforcement of social control eliminated possible regional differences in unrest rates.

<sup>12</sup>In Figure A12A, 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.

<sup>13</sup>The estimating equation is  $\frac{Unrest_{it}}{L_{i,2010}} = \sum_{s \neq 2013} \beta_s (Reform_i \times D_s) + \lambda_i + \mu_t + \varepsilon_{it}$ .  $D_s$  is a dummy variable that equals one for year  $s$  and zero otherwise. Year 2013 is the omitted reference group.

controls  $f[\Delta \log(P_{i,2014}); \zeta_{Reform,t}]$ . Tellingly, by comparing prefectures around the reform cutoff, there are no differential trends in unrest leading up to the center’s reform initiative. But after the reform comes into effect, reform prefectures experience a relative decline in unrest rates. To further evaluate this trend break, in Appendix B.2, we implement the sensitivity test developed by [Rambachan and Roth \(2023\)](#). This test reveals to conclude a null effect of the *hukou* reform, how severe the confounding differential trends in post-reform periods need to be. The results imply that one can reject a null effect unless there exist very wiggly differential trends.

The results presented above are based on the full sample of 287 prefectures. One concern is the polynomials may not adequately control for heterogeneity between prefectures far from the reform cutoff and those near the cutoff, resulting in biased estimates. To alleviate this concern, we restrict the sample to a narrow bandwidth around the reform cutoff, which is referred to as a “narrow sample.” We use the optimal bandwidth proposed by [Imbens and Kalyanaraman \(2012\)](#). Using this narrow sample, we repeat the previous analysis. Figure 3A displays the raw patterns, which are qualitatively similar to Figure 2A. It is worth noting that now reform and non-reform prefectures share very similar trends and even levels of unrest rates before the reform initiative, but after the reform, reform prefectures have much lower unrest.<sup>14</sup> This is evident from event-study estimates in Figure 3B. In Appendix B.3, we show the robustness to other bandwidth choices.

Table 3 summarizes these results from both full and narrow samples. Given the small group of non-reform prefectures, we also report  $p$ -values calculated from permutation tests. The estimates show a strong negative effect of *hukou* reform on unrest rates. For our preferred specification, Column (2), the estimate implies that reform prefectures experienced a decrease in unrest rate by 1.419 incidents per million population relative to non-reform prefectures. This is a sizeable effect. The magnitude amounts to about 42 percent of the mean of non-reform prefectures.

Our analysis mainly focuses on the reform cutoff of 3 million urban population, since it marks the most salient change to provisions on granting *hukou* transfers. However, the reform initiative sets other cutoffs of the urban population to differentiate reform provisions, namely, 0.5 million, 1 million, and 5 million. To see whether potential changes to *hukou* policy at these margins influence labor unrest, we implement our research design using falsified cutoffs to estimate the causal effects.<sup>15</sup> Figure 4 reports the results of these exercises. At cutoffs other than 3 million, there are null effects. These results also provide additional validation for our research design — the outcome does not change discontinuously except at the 3 million cutoff.

Our results are not due to mechanical size differences between reform and non-reform prefectures. In Appendix B.4, we find that the reform had no discernible effects on the population.

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<sup>14</sup>In Figure A12B, 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, whereas the distributions of pre-reform unrest rates are balanced.

<sup>15</sup>To avoid contamination due to treatment effect at the 3 million cutoff, following [Cattaneo and Titiunik \(2022\)](#), we use only below 3 million prefectures for the 0.5 million and 1 million cutoffs, and only above 3-million prefectures for the 5 million cutoff.



In Appendix B.5, we show that the results hold even if we scale the number of unrest events by time-varying population size.

Since our measure of unrest rate is based on the collection of labor unrest events reported online, one may be concerned whether our finding is an artifact of differential coverage of local events between reform and non-reform prefectures. In Appendix B.6, we address this concern by showing that (i) the *hukou* reform has no impact on the number of events and protests reported in GDEL, a larger database that cover a variety of topics, and (ii) there is no differential effect of *hukou* reform on unrest rate by the capacity of local censorship, suggesting that our finding is not driven by places more likely to block information.

## 4.2 Alternative Identification Strategy

The results presented above rely on variation in reform status. It remains agnostic regarding potential triggers of unrest. Were there unobserved triggers associated with the reform status despite controls that have been included, our findings can be confounded. To address this issue, in this section, we present results from an alternative identification strategy to supplement previous findings.

This basic idea is as follows: When an exogenous, unrest-conducive shock hits, does the *hukou* reform engender heterogeneous responses to it? Following [Campante et al. \(2023\)](#) as well as a broader literature ([Ponticelli and Voth, 2020](#)), we consider the income shock induced by international trade. Specifically, we estimate the following regression model:

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

$TradeShock_{it}$  is a measure of trade shock that we discuss in detail later. Equation 4 is essentially a triple-differences model. The coefficient of interest is  $\beta_5$ , which captures the differential response to trade shock if the *hukou* reform is in effect.

We construct  $TradeShock_{it}$  in a shift-share fashion, following [Campante et al. \(2023\)](#) as well as [Autor et al. \(2013\)](#):

$$TradeShock_{it} = \left[ \sum_k \frac{X_{ik,2010}^{CN}}{\sum_i X_{ik,2010}^{CN}} \Delta X_{kt}^{ROW} \right] / L_{i,2010}. \quad (5)$$

In this expression,  $X_{ik,2010}^{CN}$  is prefecture  $i$ 's exports of manufactured product  $k$  (six-digit Harmonized System level), measured using 2010 Chinese customs data.  $\frac{X_{ik,2010}^{CN}}{\sum_i X_{ik,2010}^{CN}}$  is prefecture  $i$ 's share in total Chinese exports of product  $k$ .  $\Delta X_{kt}^{ROW}$  is the change in exports within the rest of the world

(China excluded) in year  $t$  (unit: 1,000 US dollars), reflecting fluctuation in global demand for manufactured goods. Therefore,  $\sum_k \frac{X_{ik,2010}^{CN}}{\sum_i X_{ik,2010}^{CN}} \Delta X_{kt}^{ROW}$  is a proxy for prefecture  $i$ 's total losses or gains from fluctuations in global demand; we scale it by the size of working-age population so that  $TradeShock_{it}$  reflects the losses or gains per person.

Table 4 reports the results of our analysis. Column (1) is a minimum specification that estimates the average relationship between trade shock and unrest rate. It shows that on average, a sluggish growth in global demand would increase labor unrest. To further verify causation, in Column (2) we perform a falsification test, showing that there is a null effect of future trade shock on current labor unrest. In Columns (3)–(5), we estimate the triple-differences model Equation 4. They show that when *hukou* reform is in force, the negative trade shock has a weaker effect on increasing labor unrest, confirming the reform's role in influencing unrest participation.

### 4.3 Additional Robustness Checks

We conduct several additional robustness checks for our results. We summarize them below. More details can be found in Appendix C.

Given the imbalance in baseline characteristics despite polynomial controls (cf. Table 2), in Appendix C.1, we show that our results survive different strategies to balance baseline characteristics: (i) directly controlling for nonparametric trends associated with those characteristics; (ii) weighting observations to balance the propensity score predicted by those characteristics; and (iii) using the coarsened exact matching proposed by Iacus et al. (2012) to balance distributions of those characteristics.

In Appendix C.2, we show that our results hold for a range of alternative specifications and estimators: (i) different orders of polynomial function; (ii) alternative forms of the dependent variable; (iii) Poisson regression; (iv) spatial autoregressive model to take into account potential spillovers; and (v) the synthetic difference-in-differences estimator proposed by Arkhangelsky et al. (2021).

In addition, our results are not driven by outliers (Appendix C.3).

## 5 Unpacking Mechanisms

Having established that the *hukou* reform reduces unrest rates, herein we turn to the question of what mechanisms underlie this consequence. In particular, we are interested in how the *hukou* reform alters migrants' horizons in their destination and thus influences their calculations in participating in unrest activities.

A burgeoning literature has underscored the temporariness of many migrations. The expected migration duration has important implications for migrants' economic behaviors, especially those concerning their integration and settlement in their destination, such as human capital investment and marital searching (Adda et al., 2022; Zaiour, 2023). In this vein, it is natural to expect that migrants in our case would also take into account their expected stay when deciding on participating in unrest. As discussed in Section 2.1, due to barriers of *hukou* transfers, migrants typically do not have the chance of permanent settlement and reap associated benefits; after staying a short period in one destination, most migrants would either return home or migrate to other places. As a result, they have short horizons in the destination, which may provide them incentives to engage in politically risky labor unrest, through which they gain short-term benefits such as getting back wage arrears but bear little long-term costs such as retaliation by local governments and employers. But after the *hukou* reform, it becomes possible for migrants to permanently settle in their destination. This possibility can create a disincentive of unrest participation because it increases the cost of permanent settlement. In Appendix D, we provide a simple model to clarify the linkage between retention intention and unrest participation. Whether to participate in unrest is essentially an intertemporal tradeoff — it brings benefits to the current period but adds cost of settlement in the future. Before the *hukou* reform, the current period dominates in the tradeoff due to the low chance of permanent settlement, but after the reform, the future dominates.

We want to be upfront that we do not claim that heightened retention intentions are the only factor that explains the *hukou* reform's impact on unrest rates. In Section 5.2, we implement a causal mediation analysis method to quantitatively assess the importance of retention intentions in explaining our findings. In Section 5.3, we explore other likely mechanisms.

## 5.1 The Retention Effect of the *Hukou* Reform

In light of our proposed mechanism, we study how the *hukou* reform influences migrants' retention intentions. We test it through actual retention.

**Data and Specification.** Our main data source is the 1 percent population census of 2015. It elicits an individual's history of residence at three time points: November 2010, November 2014, and November 2015. With this information, we can infer if one had migrated between these points. For instance, if we observe an individual who reports that he lived in Beijing in 2010 and 2014 but lived in Shanghai in 2015, then we can infer that he must have moved at a certain time between 2014 and 2015. We study how the *hukou* reform in the 2010 location affects an individual's migration choices to other locations. In the language of econometrics, we conduct a survival analysis where the event of interest is leaving the 2010 location.

We make two restrictions to our sample. First, for our purpose of understanding migrants' horizons and subsequent plans in destination, we focus our attention on migrants in 2010, defined as those whose residing prefecture in 2010 is different from the *hukou* registration prefecture in 2015 that we can observe. Second, we restrict the sample to individuals whose *hukou* was registered in rural areas as of 2015. This is mainly to ensure the precision of defining migrant status. Note that though we have an individual's residence history, we do not have his *hukou* registration history. Thus, we have to rely on the registration prefecture observed in 2015 to define migrant status back in 2010. This would lead to a measurement error if an individual had migrated and transferred *hukou* before 2015. For instance, if an individual was originally registered in the 2010 prefecture but had successfully transferred his *hukou* to the 2015 residence, then using the 2015 registration information would misclassify him as a migrant back to 2010 and thus overstate the departure rate in the migrant sample.

We estimate the effect of *hukou* reform on retention using the following linear probability model (LPM):

$$Outmigration_{jkt} = \rho (Reform_k \times Post15_t) + \lambda_k + \mu_t + f [\Delta \log (P_{k,2014}) ; \zeta_{Reform,t}] + \varepsilon_{jkt}, \quad (6)$$

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

The dependent  $Outmigration_{jkt}$  is a dummy variable that equals one if by year  $t$ , migrant  $j$  has left his/her residential prefecture in 2010, denoted by  $k$ .  $Reform_k$  is the reform status of prefecture  $k$ .  $Post15_t$  is a dummy variable that equals one for 2015 but zero for 2014. Though the residential prefecture in 2014 is the one observed as of November 2014, if having ever moved, a large fraction of migrants could have moved much earlier than November 2014 and July 2014, the time when the *hukou* reform was initiated. Therefore, we treat 2014 as the pre-reform period while 2015 as the post-reform period. In the model, as before, we also include prefecture fixed and year fixed effects as well as polynomial controls.  $\varepsilon_{jkt}$  is the error term. In the spirit of survival analysis, we only consider a migrant's first migration decision, thus, we drop subsequent observations of a migrant if he/she had decided to leave  $k$ . Thus, the coefficient of interest,  $\rho$ , is estimated off variation in the trends of out-migration rate among not-yet-migrate migrants between reform and non-reform prefectures.

**Results.** As the first pass at the data, Figure 5 visualizes the outmigration rate from the residence in 2010, separately for reform and non-reform prefectures. The outmigration rate is estimated by the Kaplan-Meier estimator, i.e., the share of leavers among existing individuals. By construction, the outmigration rate of 2010 is zero. By the end of 2014, reform prefectures witness a higher outmigration rate than non-reform prefectures. But when it comes to the end of 2015, when the *hukou* reform had become in effect, the outmigration rate rose sharply in non-reform prefectures, surpassing the reform prefectures. This suggests that the *hukou* reform may have led to an increase in retention in the 2010 residential location.

Inspired by this observation, we implement our research design and report the estimates in Table 5. In Column (1), we estimate the minimum specification. It indicates that the *hukou* reform reduces the outmigration rate by 7.2 percentage points, which is about 51 percent of the control mean. An alternative interpretation of this result is that migrants in reform prefectures who kept staying until the post-reform period are better integrated into the destination or better matched with their jobs as opposed to their counterparts in non-reform prefectures, thus leading to a drop in the outmigration rate. To address this concern, in Column (2), we control for a range of individual covariates (interacted with year indicators), including birth cohort, gender, educational attainment, and employment status. By doing so, we tease out the potential influence of these factors on outmigration. It is apparent from Column (2) that the estimated effect does not change markedly. Column (3) excludes prefectures with very few individuals in the beginning, whose outmigration rate changes are more sensitive to a single individual’s outmigration (dropping bottom 10 percent). In Appendix A3, we also estimate a Cox proportional hazard model that is commonly used for survival analysis, which yields similar results.

We interpret the decrease in the outmigration rate as heightened retention intentions because of the possibility opened up by the *hukou* reform. If so, then one would expect that this effect is more pronounced among migrants who are more forward-looking.<sup>16</sup> Column (4) corroborates this idea — it shows that the reform has a larger effect on reducing the outmigration rate if a migrant is more patient — we measure the patience level using one’s home province average from the Global Preference Survey (GPS, Falk et al., 2018). In Column (5), we show that this differential effect by patience does not pick up other possibly correlated factors, including the risk-taking preference as reported in the GPS and high school completion that is observed in census data.

## 5.2 Quantifying the Importance of Retention Intentions

Thus far, we have demonstrated that the *hukou* reform enhances retention intention by showing a decrease in the outmigration rate among existing migrants. One natural question is how much of the *hukou* reform’s effect on unrest rates can be explained by changes in retention intention. To answer this question, we need to conduct a causal mediation analysis, that is, identify the causal effect that goes through the retention mechanism (indirect effect) and the causal effect that is not through the retention mechanism (direct effect). In the following, we first discuss some problems in a commonly used methodology. We proposed some improvements. Then, we investigate the importance of the retention mechanism using the improved methodology.

**Methodology.** Conventionally a causal mediation analysis is done by estimating the following linear simultaneous equations model (LSEM):

$$Y_i = \alpha_1 + \beta T_i + e_{i1}, \tag{7}$$

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<sup>16</sup>We show this in our simple conceptual model. See Result 2 in Appendix D.

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

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

$Y_i$  denotes the outcome of interest,  $T_i$  denotes the treatment, and  $M_i$  denotes the proposed mechanism. Then, the OLS estimand (i.e., the probability limit of the OLS estimator)

$$\hat{\beta} - \hat{\tau} = \hat{\gamma}\hat{\pi} \quad (10)$$

is interpreted as the indirect effect of  $T_i$  on  $Y_i$  through  $M_i$ . Note that  $\hat{\beta} - \hat{\tau}$  is the attenuation in the coefficient on  $T_i$  after controlling for  $M_i$ , which, by mechanics of OLS, is numerically equivalent to  $\hat{\gamma}\hat{\pi}$ , the product of the effect of  $M_i$  on  $Y_i$  ( $\hat{\gamma}$ ) and the effect of  $T_i$  on  $M_i$  ( $\hat{\pi}$ ). This approach is popularized by [Baron and Kenny \(1986\)](#) and is widely used in social sciences ([Cutler and Lleras-Muney, 2010](#)). However, one key problem in this approach is that it requires a strong assumption on the exogeneity of  $M_i$ . A typical research design only involves a valid exogeneity assumption of  $T_i$ , which warrants identification of  $\hat{\beta}$  and  $\hat{\pi}$ , that is,  $\hat{\beta} = \beta$  and  $\hat{\pi} = \pi$ . But Equation 10 requires the consistency of  $\hat{\pi}$ , i.e., identifying the causal effect of  $M_i$  on  $Y_i$ . Without a strong assumption that  $M_i$  is exogenous conditional on  $T_i$ , and  $\hat{\pi}$  and so  $\hat{\tau}$  are biased ([Imai et al., 2011](#); [Angrist and Pischke, 2009](#)).

To address the problem, one idea is to find an instrumental variable (IV) for  $M_i$ , denoted by  $Z_i$ . To elaborate, consider the following potential outcome framework:

$$Y_i(t, m) = \tau_i t + \gamma_i m + u_i, \quad (11)$$

$$M_i(t, z) = \pi_i t + \theta_i z + v_i. \quad (12)$$

Note that this framework already assumes the excludability of IV  $Z_i$ : It does not directly enter the outcome equation 11, and the only way it could influence the outcome is through  $M_i$ . Combining Equations 11 and 12 yields:

$$Y_i(t, M_i(t, z)) = (\tau_i + \gamma_i \pi_i) t + \gamma_i \theta_i z + u_i + \gamma_i v_i. \quad (13)$$

DEFINITION 1 (Parameters of Interest).

1. The total effect is  $\beta_i = \tau_i + \gamma_i \pi_i$ . The average total effect (ATE) is then  $E(\beta_i) = E(\tau_i) + E(\gamma_i \pi_i)$ .
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  $ATE = ADE + AIE$ . We are interested in the estimation of AIE and how its magnitude when compared to ATE, which informs us of the importance of a mechanism. We make the following assumptions.

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

ASSUMPTION 2 (IV Validity).

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

ASSUMPTION 3 (Homogeneity of Mechanism Effect).  $\gamma_i$  is constant.

Assumption 1 supposes the exogeneity of treatment  $T_i$ . Assumption 2 is the standard assumption on IV validity (Angrist and Pischke, 2009, pp. 151–158). Assumption 3 posits that the causal effect of  $M_i$  on  $Y_i$  is constant. This is a strong assumption, albeit common in the literature (Dippel et al., 2022; Dix-Carneiro et al., 2018). It allows us to extrapolate the causal effect of  $M_i$  on  $Y_i$  identified in a possibly distinct subpopulation to the population in which we identify the effect of  $T_i$  on  $Y_i$ . We will consider the implications of relaxing this assumption. With these settings, one can estimate the LSEM, with  $M_i$  instrumented by  $Z_i$ , to identify AIE. The following Proposition 1 summarizes the results.

PROPOSITION 1. Under Assumptions 1, 2, and 3, with  $M_i$  instrumented by  $Z_i$ , least squares estimands of Equations 7 and 8 satisfy:

$$\widehat{AIE} = \hat{\beta} - \hat{\tau} = \hat{\gamma}\hat{\pi} = E(\gamma_i\pi_i) \equiv AIE. \quad (14)$$

*Proof.* See Appendix E. ■

A similar result holds for RD designs that identify treatment effects at the cutoff, which we use in this paper. The difference is that our approach would identify the average indirect effect at the cutoff.

PROPOSITION 2. Let  $r_i$  denote the running variable.  $T_i = \mathbb{1}\{r_i \geq 0\}$ . Under Assumptions 1, 2, and 3, least squares estimands of Equations 7 and 8, with  $M_i$  instrumented by  $Z_i$  and flexible polynomial functions of  $r_i$  included, satisfy:

$$\widehat{AIE} = \hat{\beta} - \hat{\tau} = \hat{\gamma}\hat{\pi} = E(\gamma_i\pi_i | r_i = 0) \equiv AIE. \quad (15)$$

*Proof.* See Appendix E. ■

**Specification and Results.** To implement our IV-augmented approach, we estimate the following model:

$$\frac{\Delta Unrest_i}{L_{i,2010}} = \alpha_1 + \beta Reform_i + f[\Delta \log(P_{i,2014}); \zeta_{Reform}] + e_{i1}, \quad (16)$$

$$\frac{\Delta Unrest_i}{L_{i,2010}} = \alpha_2 + \tau Reform_i + \gamma \Delta Outmigration_i + f[\Delta \log(P_{i,2014}); \zeta_{Reform}] + e_{i2}, \quad (17)$$

where  $\Delta Outmigration_i$  is instrumented by  $Z_i$ . The dependent variable,  $\frac{\Delta Unrest_i}{L_{i,2010}} = \frac{\sum_{t=2014}^{2019} \frac{Unrest_{it}}{L_{i,2010}}}{6} - \frac{\sum_{t=2011}^{2013} \frac{Unrest_{it}}{L_{i,2010}}}{3}$ , is the change in average unrest rate from the pre-reform periods to post-reform periods. With this conversion, Equation 16 produces a numerically identical estimate of  $\beta$  as Equation 2 does. The mechanism variable is  $\Delta Outmigration_i = \frac{\sum_j Outmigration_{ji,2015}}{N_{2015}} - \frac{\sum_j Outmigration_{ji,2014}}{N_{2014}}$ , namely, the change in outmigration rate from 2010–2014 period to 2014–2015 period in prefecture  $i$ , which we calculate from the mini population census of 2015.

For IV  $Z_i$ , we consider variation in export demand faced by migrants at their home prefectures.

$$Z_i = \sum_h s_{ih} \times \Delta TradeShock_h, \quad (18)$$

where  $\Delta TradeShock_h = TradeShock_{h,2015} - \frac{1}{4} \sum_{t=2011}^{2014} TradeShock_{ht}$  is the change in global demand shocks from 2011–2014 period to 2014–2015 period. It is reasonable to consider the economic conditions at migrants' homes as the IV for outmigration from their destinations. We observe that, among migrants who eventually left the 2010 destination, 88% of them returned to home prefectures, suggesting that when migrants decide on retention, conditions at home should be an important factor. In addition, using shocks at home is less likely to pick up factors in the destination that can influence both outmigration and labor unrest, thus causing violations of the exclusion restriction.

Table 6 reports the results of causal mediation analysis. Columns (1) and (2) present the results using the conventional approach. For ease of comparison, Column (1) presents the baseline estimate. It produces the same estimate as in Column (2) of Table 3. In Column (2), we directly control for the change in the outmigration rate in the regression. There is a significant positive correlation between the unrest rate and the outmigration rate, suggesting that heightened retention may reduce unrest. We also see that the reform's effect is attenuated by 12.9 percent. If one takes the estimated coefficient on the outmigration rate as causal, this implies that 12.9 percent of the total effect is due to the retention mechanism.

In Columns (3) and (4), we then apply our proposed IV-augmented approach. Column (3) displays the first stage, and Column (4) reports the second stage. Our IV is strong as indicated by an effective  $F$  statistic of 59.629 (Olea and Pflueger, 2013). Column (3) shows that having a positive trade shock at the origin significantly increases the outmigration rate. Again, we find that



outmigration has a strong effect on unrest, which is robust to using *tF* inference proposed by Lee et al. (2022). According to this estimate, 27.4 percent of the total effect of *hukou* reform on unrest rate can be attributed to heightened retention intentions. This increase in the explained share is due to inflation in the coefficient on  $\Delta Outmigration_i$  after instrumentation. Provided that IV is valid, the empirical IV-OLS gap can be due to (i) omitted variables bias (OVB) and (ii) the difference between weighting schemes placed by IV and OLS. Our homogeneous effect and linear functional form assumptions assume away (ii). To assess how much bias is driven by specification error, we use the decomposition method developed by Ishimaru (2024) and find that nearly the entirety of the IV-OLS gap is due to OVB.

**Sensitivity Test.** Assumption 3 is a key assumption in our approach. If we relax it to allow full heterogeneity in  $\gamma_i$ , the effect of  $M_i$  on  $Y_i$ , then we obtain (see Appendix E for derivations):

$$\begin{aligned} \widehat{ATE} &= \underbrace{E(\gamma_i \pi_i)}_{\text{AIE}} + \underbrace{[\hat{\gamma} - E(\gamma_i)] E(\pi_i)}_{\text{bias 1}} - \underbrace{Cov(\gamma_i, \pi_i)}_{\text{bias 2}} \\ &= \underbrace{E(\gamma_i \pi_i)}_{\text{AIE}} + \underbrace{Cov(\phi_i, \pi_i) E(\pi_i)}_{\text{bias 1}} - \underbrace{Cov(\gamma_i, \pi_i)}_{\text{bias 2}}, \end{aligned} \quad (19)$$

where  $\hat{\gamma} = E(\phi_i \gamma_i)$ , and  $\phi_i = \frac{\tilde{Z}_i M_i}{E(\tilde{Z}_i M_i)}$  with  $\tilde{Z}_i$  being the residual of linear projection of  $Z_i$  on  $T_i$  (and possibly other controls). A similar result holds for RDDs by assuming causal effects  $\{\gamma_i, \pi_i\}$  are independent of the running variable  $r_i$ . This expression implies that in general,  $\widehat{ATE}$ , the attenuation in the coefficient on  $T_i$  after controlling for instrumented  $M_i$ , is still biased for AIE. The intuition is that, by relaxing Assumption 3, the effect of  $M_i$  on  $Y_i$  identified in a possibly different subpopulation cannot be perfectly extrapolated to the population in which we identify the effect of  $T_i$  on  $Y_i$ . Specifically, there are two sources of bias. The first bias comes from the discrepancy between the IV-identified average effect and the population average effect, i.e.,  $\hat{\gamma}$  and  $E(\gamma_i)$ . It arises because the IV estimates the average slope of  $Y_i$  for  $M_i$  within a subpopulation of compliers ( $\hat{\gamma} = E(\phi_i \gamma_i)$ ), which is different from the full population average relationship ( $E(\gamma_i)$ ). The second bias is due to the correlation between  $\gamma_i$  and  $\pi_i$ . Using  $\hat{\gamma}$  as the loading on  $\pi_i$  to evaluate  $M_i$ 's contribution to the average total treatment effect can produce a bias if  $\pi_i$  is correlated with  $\gamma_i$ . For instance, if there is a positive correlation, the contribution of  $M_i$  is systematically overstated in the low- $\gamma_i$  group, and is understated in the high- $\gamma_i$  group.

Inspecting Equation 19, one can find the bias of  $\widehat{ATE}$  is determined by the distributions of  $(\phi_i, \gamma_i)$  and  $(\gamma_i, \pi_i)$ . Moreover, since  $\phi_i = \frac{\tilde{Z}_i M_i}{E(\tilde{Z}_i M_i)}$ , we can estimate its moments from the sample using the method of moments. Therefore, we can impose minimal distributional assumptions so that we can de-bias  $\widehat{ATE}$ . The following proposition summarizes our result.

**PROPOSITION 3.** *Suppose that (i) either  $\text{sgn}(\gamma_i) = 1$  or  $\text{sgn}(\gamma_i) = -1$  for all  $i$ ; (ii) either  $\text{sgn}(\pi_i) = 1$  or  $\text{sgn}(\pi_i) = -1$  for all  $i$ ; and (iii)  $\gamma_i$  and  $\pi_i$  are uniformly distributed. Additionally suppose  $\{\gamma_i, \pi_i\} \perp\!\!\!\perp r_i$  for RD designs. Then, for every given combination of  $\rho_{\phi\gamma}$  and  $\rho_{\phi\gamma} = \text{Corr}(\phi_i, \gamma_i)$  and  $\rho_{\gamma\pi} = \text{Corr}(\gamma_i, \pi_i)$ ,*

*AIE is identified by estimand*

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

where  $\hat{\sigma}_\phi = \text{plim} \sqrt{\frac{1}{n} \sum_{i=1}^n \left( \frac{\tilde{Z}_i M_i}{\frac{1}{n} \sum_{i=1}^n \tilde{Z}_i M_i} - 1 \right)^2}$ .

*Proof.* See Appendix E. ■

In this proposition, correlation coefficients  $\rho_{\phi\gamma} = \text{Corr}(\phi_i, \gamma_i)$  and  $\rho_{\gamma\pi} = \text{Corr}(\gamma_i, \pi_i)$  govern the sizes of bias 1 and bias 2 in Equation 19, respectively. Assumption 3 makes both  $\rho_{\phi\gamma}$  and  $\rho_{\gamma\pi}$  zero. By Proposition 3, we can examine how much  $\widetilde{AIE}$  changes when  $\rho_{\phi\gamma}$  and  $\rho_{\gamma\pi}$  deviate from zero. In our case, it is assumed that  $\gamma_i \geq 0$  and  $\pi_i \leq 0$ , therefore, we consider positive  $\rho_{\phi\gamma}$  and  $\rho_{\gamma\pi}$  to correct *overestimation* of the average indirect effect.

In particular, we report the share of total effect explained by our proposed mechanism, namely,  $\text{ShareExplained} = \frac{\widetilde{AIE}}{\hat{\beta}}$ , for different combinations of  $\rho_{\phi\gamma}$  and  $\rho_{\gamma\pi}$ . If  $\text{ShareExplained}$  drops a lot when only imposing very small values to  $\rho_{\phi\gamma}$  and  $\rho_{\gamma\pi}$ , then the conclusion under Assumption 3 for heightened retention intentions being an important mechanism may not be reliable. Figure 6 reports the results of our sensitivity test. Clearly, when  $\rho_{\phi\gamma} = 0$  and  $\rho_{\gamma\pi} = 0$ , which is true when Assumption 3 holds, we obtain the highest share of total effect explained, 27.3 percent, as we have seen previously. Overall,  $\text{ShareExplained}$  is more sensitive to  $\rho_{\phi\gamma}$ , that is, the bias due to the gap between IV-identified and population average effects. If we assume  $\rho_{\gamma\pi} = 0$  but  $\rho_{\phi\gamma} = 1$  to the extreme, we find that heightened retention intentions can still explain 10 percent of the *hukou* reform's total effect on labor unrest. A similar magnitude of 10 percent can also be maintained even if we allow moderate sizes of bias 1 and bias 2, e.g.,  $\rho_{\gamma\pi} = \rho_{\phi\gamma} = 0.5$ .

Taken together, we conclude that heightened retention intentions do play an important role in mediating the *hukou* reform's effect on reducing labor unrest, and it can explain up to 27.4 percent of the average total effect.

### 5.3 Other Mechanisms

Thus far, we have presented evidence supporting the role of heightened retention intentions. Of course, this is certainly not the sole mechanism. In this section, we investigate some other potential mechanisms underlying our findings.

**Compositional Changes.** One possible mechanism is that the *hukou* reform induces population movements, thus altering the composition of the population. If there are certain changes to traits that are conducive to unrest participation, we would observe a decline in unrest rates. We

investigate this possibility in Table 7. We use a large nationally representative survey on migrants, the China Migrants Dynamic Survey (CMDS), and estimate the impacts of *hukou* reform on a range of migrants' characteristics, including gender, ethnicity, age, marital status, educational attainment, and cross-province migration. We examine all migrants as well as new arrivals (who arrived within the past year). Since the survey of CMDS is conducted in May every year, for these regressions, we treat years from 2015 onward as the post-reform period. We do not detect strong compositional changes, suggesting that the shift of migrant characteristics does not explain much of our finding.

**Available Benefits.** Designed to facilitate migrants' permanent settlement, the *hukou* reform may have conferred some benefits to migrants, thus reducing the likelihood of unrest occurrence in the first place. The benefits could be either tangible improvements in labor market outcomes or intangible ones that provide psychological values. Table 8 reports the results of our investigation of this hypothesis. We do not find significant effects on labor market outcomes, in terms of workforce participation, wages, and access to social security (ASS). For intangible ones, we look at co-residence with a spouse or children, as the relaxation of migration barriers may facilitate family union. However, if anything, we find a weak negative effect, which works against our finding.

**Autocratic Control.** Another explanation for why the *hukou* reform reduced unrest is that the reform may stimulate some strategic responses from local governments to tighten social control, as they expect increases in population inflows that may cause social turmoil. We examine this possibility in Table 9. In Column (1), we find a null effect on expenditures on public security (police), indicating the reform does not cause investments in social control. However, it is possible that instead of financial means, the local governments prioritize the maintenance of social stability, thus diverting toward this issue more manpower or efforts from the bureaucratic apparatus. To measure the priority of stability maintenance, we use the share of stability-related keywords in next year's government work report, which the local government head addresses annually to the local People's Congress about what the government has done in the past year. Using this measure, we do not find a discernible shift of priority toward stability maintenance. Lastly, if our finding is driven by increased autocratic control, one would expect that there is a higher fraction of unrest events being repressed, despite the overall drop in unrest rate. But as Column (3) shows, there is also no significant change in the share of unrest events repressed. Taken together, it appears that autocratic control is not the main mechanism of our findings.

## 6 Concluding Remarks

In this paper, we study the causal relationship between geographic distribution of opportunities and sociopolitical stability. By examining the impact of China's *hukou* reform on labor unrest, we find that reducing geographic barriers to opportunities can have an appeasing effect. This effect is not because of immediate changes in benefits delivered to people, nor because of changes in

population characteristics or tightening local governments' social control. In contrast, we document the role of people's attachment to the state due to horizons for future settlement that make them comply with the state's demands for stability.

Generally speaking, we view our results as highlighting a source of state capacity and a force behind social changes. The dependence on the state constitutes coercive power that the state can use to induce citizens' compliance with its objectives. This is in stark contrast to the social contract view, pioneered by Jean-Jacques Rousseau, that compliance reciprocity between the state and the citizenry. The states have been witnessed using such coercive power more or less explicitly. For instance, against the backdrop of falling fertility rates, a Chinese local government proposed to urge party members and civil servants to bear three children.<sup>17</sup> A similar push for civil servants was also proposed by the South Korean government.<sup>18</sup> Consequently, on the other side of the same coin, weakening the dependence on the state is a factor conducive to civil disobedience and the momentum of social changes.

We close this paper by noting some limitations, which may be interesting avenues for future research. First, our results should be considered as the short-run impact on social stability. It is possible that in the longer term, migrants may alter the way they behave, causing new dynamics of labor unrest. For instance, the mechanism of "a revolution of rising expectations" may be at play (Tocqueville, 1856): The settlement can raise expectations for improvements that cannot be matched, thus deteriorating stability. Understanding the full dynamics can help better understand the interplay between social policy and sociopolitical stability — given that movements that ultimately transform society feature persistent, recurring interactions between civil disobedience and state cooptation and repression. Second, our paper primarily focuses on migrants' behaviors. However, it is likely that natives also react to the reform initiative and integration of migrants, as suggested by evidence on immigration's electoral effects among existing citizens (Mayda et al., 2022). Finally, it is important to consider the extent to which our results generalize. As we have noted in the introduction of this paper, our results for the role of extended horizons should primarily apply to scenarios where the state has strong control over resources. It is an open question of how an open-door migration policy, or more broadly, a socially inclusive reform, affects sociopolitical stability. We believe that this question is worthy of further investigation.

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<sup>17</sup><http://chinascope.org/archives/35543>

<sup>18</sup>[https://www.koreatimes.co.kr/www/nation/2024/08/113\\_361718.html](https://www.koreatimes.co.kr/www/nation/2024/08/113_361718.html)

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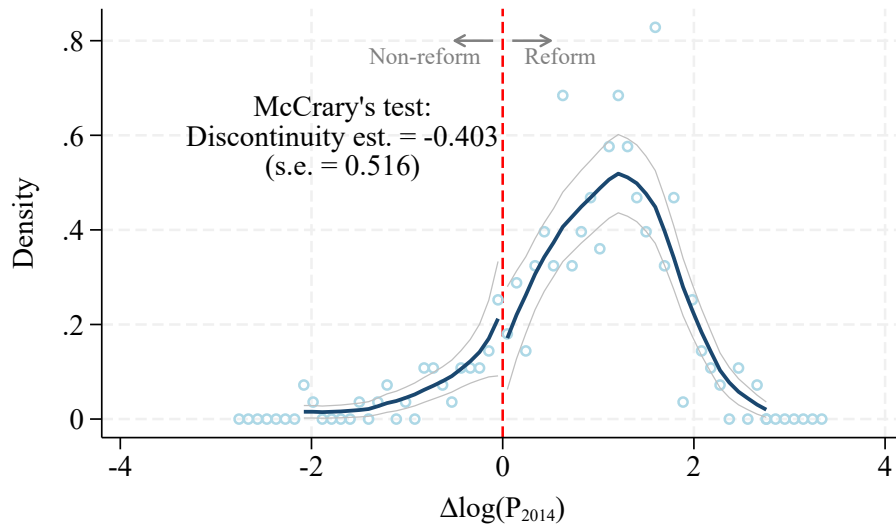
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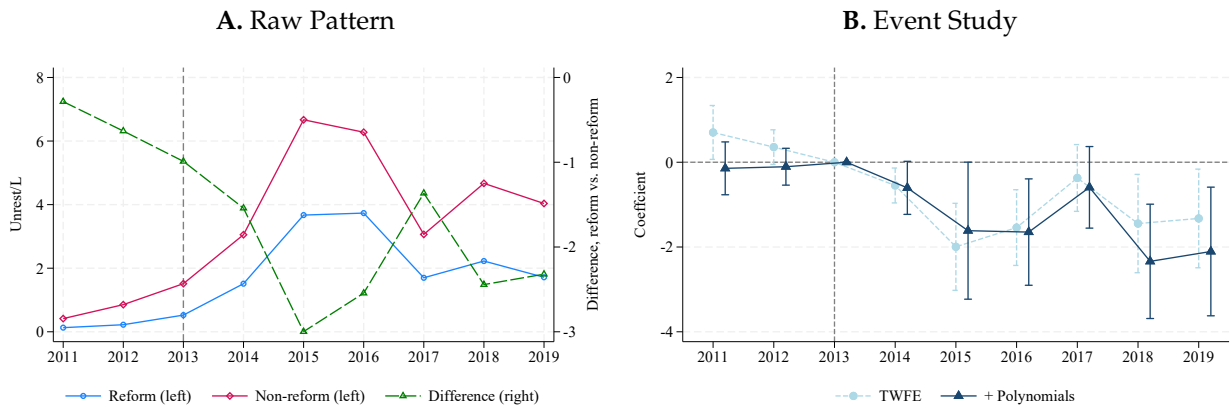
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# Figures



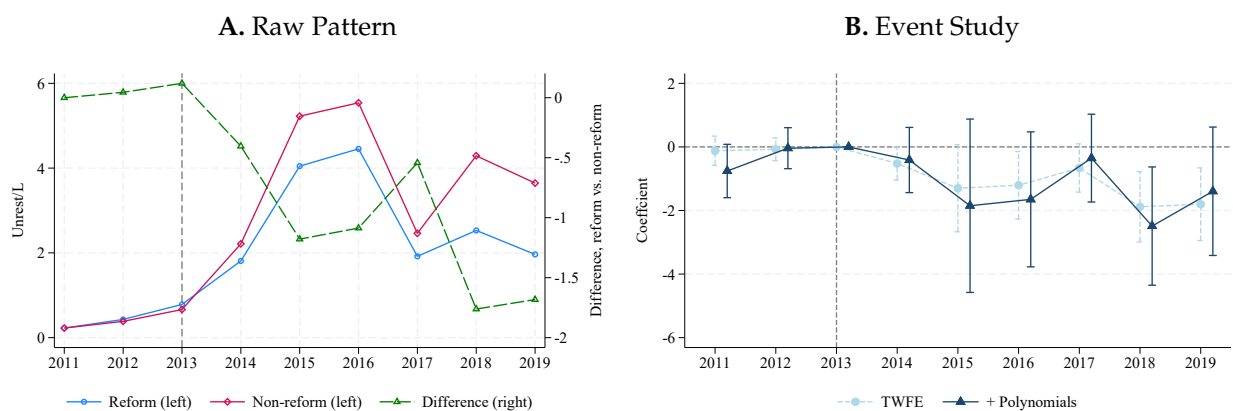
**Figure 1.** Density of Centered Log Urban Population of 2014

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



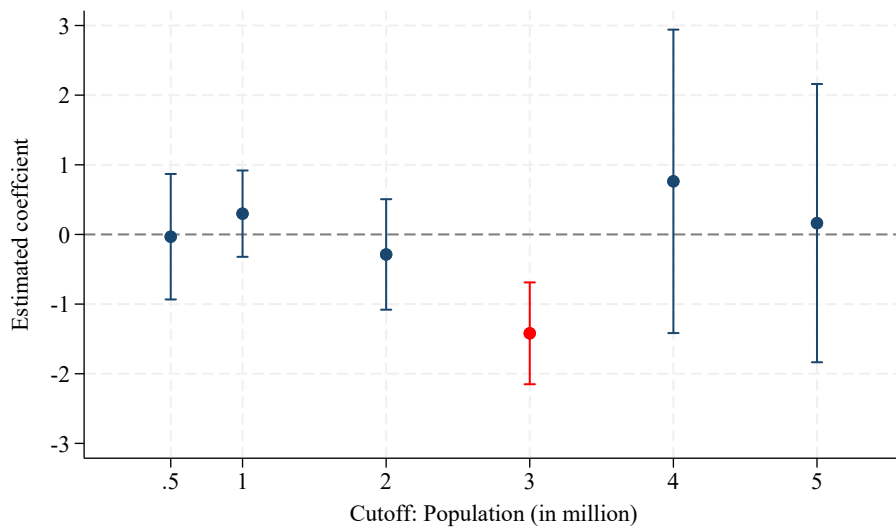
**Figure 2.** Dynamics of Labor Unrest: Full Sample

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



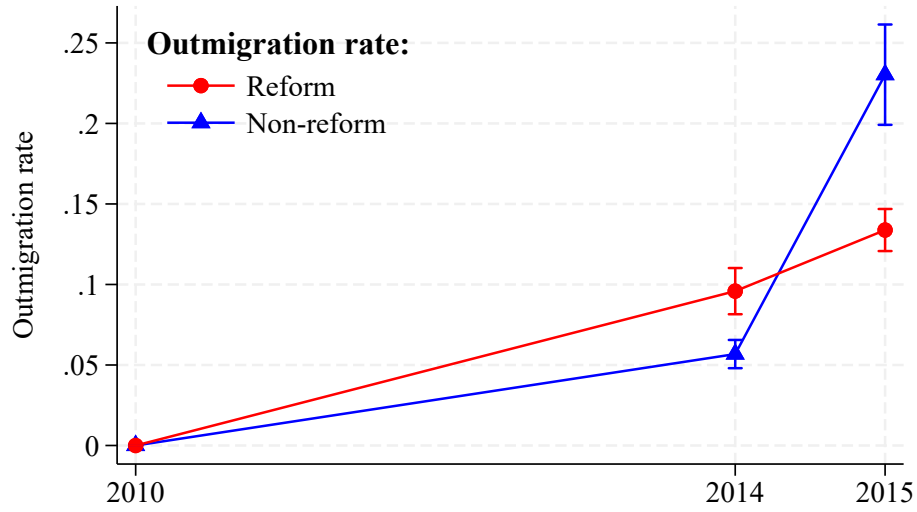
**Figure 3. Dynamics of Labor Unrest: Narrow Sample**

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



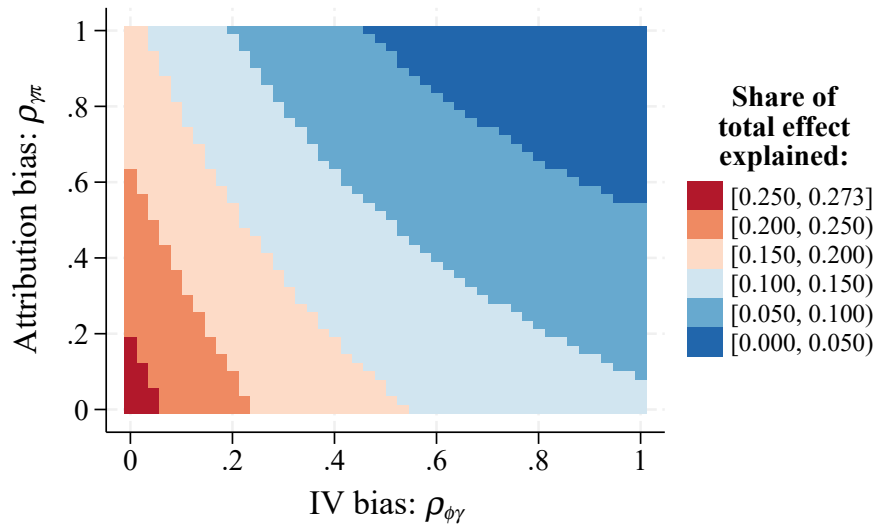
**Figure 4. Estimates Using Falsified Cutoffs**

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



**Figure 5. Rates of Outmigration from the 2010 Residential Location**

Note: This figure depicts the Kaplan–Meier estimates of the rates of out-migration from the 2010 residence by the end of 2010, 2014, and 2015, separately for reform and non-reform prefectures. The solid dots are point estimates, and the caps are 95 percent confidence intervals. The bars present the number of staying individuals in the sample, separately for reform and non-reform prefectures.



**Figure 6. Sensitivity Test**

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

## Tables

**Table 2.** Examining Smoothness in Covariates

Dependent	(1) Coef.	(2) SE	(3) Coef.	(4) SE
<b>Panel A: Pretrends (2011-2012, 2012-2013)</b>				
$\Delta$ Unrest/L	-0.352**	(0.161)	0.070	(0.158)
$\Delta$ Log population	-0.019***	(0.003)	-0.005	(0.005)
$\Delta$ Log GDP	0.009*	(0.005)	0.001	(0.008)
$\Delta$ Log expenditure	0.009	(0.007)	0.001	(0.009)
$\Delta$ Log 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 migrants	-0.174***	(0.033)	-0.066*	(0.040)
Share of urban residents	-0.235***	(0.023)	-0.107***	(0.035)
Share of secondary sector workers	-0.103***	(0.023)	-0.030	(0.037)
Share of tertiary sector workers	-0.112***	(0.013)	-0.045**	(0.019)
Share of internet users	-0.165***	(0.061)	0.044	(0.086)
Polynomials			Yes	

Note: This table examines the smoothness in covariates. Panel A looks at pretrends for 2011–2012 and 2012–2013. Panel B looks at predetermined prefectural characteristics measured in 2010. Columns (1) and (2) report the regression of the dependent on the reform indicator  $Reform_i$  (controlling for year fixed effects for Panel A). Columns (3) and (4) report estimation results for the regression that additionally controls for the linear polynomial of  $\Delta \log (P_{i,2014})$  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.

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

**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

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

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

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

	(1)	(2)	(3)	(4)	(5)
	Unrest/L	Unrest/L	Unrest/L	Unrest/L	Unrest/L
Trade shock $[\beta_1]$	-0.158*** (0.034)	-0.158*** (0.034)	-0.083** (0.036)	-0.039 (0.047)	0.085 (0.088)
Trade shock $_{t+1}$		0.005 (0.024)			
Trade shock $\times$ Reform $\times$ Post $[\beta_5]$			0.218** (0.098)	0.239** (0.105)	0.265* (0.139)
Trade shock $\times$ Log urban pop. $\times$ Post					0.027 (0.063)
Prefecture FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Polynomials				Yes	Yes
Observations	2,583	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 *hukou* reform. Standard errors clustered at the prefecture level are reported in parentheses.

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

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

	Outmigration from 2010 residence				
	(1)	(2)	(3)	(4)	(5)
Reform $\times$ Post	-0.072**	-0.071**	-0.066*	-0.090**	-0.090**
	(0.034)	(0.035)	(0.035)	(0.040)	(0.042)
Reform $\times$ Post $\times$ Patience (std.)				-0.054**	-0.054**
				(0.026)	(0.026)
Reform $\times$ Post $\times$ Risk taking (std.)					0.005
					(0.024)
Reform $\times$ Post $\times$ High school completion					0.016
					(0.021)
Control mean	0.141	0.141	0.141	0.147	0.147
Prefecture FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Polynomials	Yes	Yes	Yes	Yes	Yes
Individual covariates $\times$ Year FE		Yes	Yes		
Drop prefectures w/ few obs.			Yes		
Observations	58,701	58,701	56,667	51,769	51,769

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

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

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

	Conventional approach		IV augmented	
	(1)	(2)	(3)	(4)
	$\Delta$ Unrest/L	$\Delta$ Unrest/L	$\Delta$ Outmigration 1st stage	$\Delta$ Unrest/L 2nd stage
Reform	-1.419*** (0.370)	-1.237*** (0.364)	-0.106*** (0.034)	-1.033*** (0.383)
$\Delta$ Outmigration		1.726*** (0.360)		3.654*** (1.018)
$\Delta$ Origin trade shock			0.049*** (0.006)	
Polynomials	Yes	Yes	Yes	Yes
% Total effect explained		0.129		0.273
Effective <i>F</i> stat.				58.750
<i>tF</i> 95% CI				[1.532, 5.776]
IV-OLS gap (4)-(2)				1.989
Gap due to OVB				1.997
Observations	287	287	287	287

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

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



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

	(1)	(2)	(3)	(4)	(5)	(6)
	Female	Han ethnic	Age below 35	Married	High school completion	Cross-province migrant
<b>Panel A: All migrants</b>						
Reform × 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)
Control mean	0.474	0.953	0.536	0.876	0.389	0.571
Sample period	2011–18	2011–18	2011–18	2011–18	2011–18	2011–18
No. prefectures	255	255	255	255	255	255
Observations	990,912	990,912	990,912	990,912	990,912	990,912
<b>Panel B: New arrivals</b>						
Reform × 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)
Control mean	0.459	0.947	0.639	0.809	0.395	0.524
Sample period	2011–18	2011–18	2011–18	2011–18	2011–18	2011–18
No. prefectures	255	255	255	255	255	255
Observations	238,161	238,161	238,161	238,161	238,161	238,161

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

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

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

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

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

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

**Table 9. *Hukou* Reform and Autocratic Control**

	(1)	(2)	(3)
	Log expenditure on public security	Share of stability related keywords	Share of unrest events repressed
Reform $\times$ Post	-0.016 (0.029)	0.007 (0.009)	0.088 (0.077)
Control mean	13.138	0.081	0.228
Sample period	2011–17	2011–15	2011–19
No. prefectures	287	287	285
Observations	1,982	1,410	1,806

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

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

# Online Appendices

## Appendix A Supplementary Materials

### A.1 Labor Unrest in China

**Contributing Factors.** Labor politics is an important issue in any country. China is not an exception. Although the communist regime claims that the country is “led by the working class and based on an alliance of workers and peasants,” it is very vigilant about labor unrest and has adopted various strategies to prevent its occurrences. Workers are not allowed to form unions. All existing “unions” are government agencies that deal with labor affairs (Friedman, 2014). They do not actively represent workers in bargaining with employers, but they might step in to divide workers if they observe patterns that workers are going to resort to unrest for their demands. The court is another tool to deal with labor affairs. Workers are encouraged to submit their disputes through the legal system (Chen and Gallagher, 2018), where the Chinese state can easily influence. Despite these measures, labor unrest nonetheless occurs and grows rapidly in China. The government relentlessly cracks down on labor unrest, in particular the organizers.

Importantly, the Chinese *central* government tolerates labor unrest where workers voice demands for their rights and interests, insofar as it does not extend to “mass incidents” that threaten political stability. Lorentzen et al. (2013) argue that unrest can serve as a signal for the central government to identify discontented groups, and on that basis, the central government can allocate resources to address grievances and manage local officials accordingly.

**Unions and NGOs.**

**Government Responses.**

### A.2 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 release 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, released on November 20, 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 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

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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.

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 A1 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 A2 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*.

### A.3 Other Population-Based Policies

If there exist other concurrent policies that are based on the same population rule and also influence labor unrest, our estimates for the effects of the *hukou* reform can be contaminated. To examine this possibility, we conduct a comprehensive search of population-related policies using the PKULaw database (<https://home.pkulaw.com>), which provides extensive information on Chinese laws and regulations and has been used in research on policymaking in China (e.g., Tian, 2024; Wang and Yang, 2021). We use two keywords to identify the population-related policies, “urban population (城区人口 in Chinese)” and “city size (城市规模 in Chinese).”

Table A3 summarizes the counts of policies containing the keyword “urban population” or terms with similar meanings by year and policy categories. Among these policy categories, the regional plan represents the largest share of all policies related to the “urban population”. This category includes both the central government’s approval of development plans for specific cities

**Table A1.** 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 A2.** Disagreements 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	Province	Prefecture
Hebei	Shijiazhuang	Hebei	Langfang
Hebei	Tangshan	Jiangsu	Changzhou
Jilin	Changchun	Zhejiang	Wenzhou
Jiangsu	Huaian	Fujian	Fuzhou
Zhejiang	Shaoxing	Guangdong	Zhuhai
Anhui	Hefei	Guangdong	Zhongshan
Shandong	Zibo	Hainan	Haikou
Shandong	Linyi	Hainan	Sanya
Guangdong	Shantou		

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

and its broader development plans for particular regions. In these documents, “urban population” or its synonyms are used to describe the population status of the city or region, which wouldn’t be

viewed as population-based policies. The policy of *hukou* reform falls in the category of government tasks in the year 2014 and urban-population-related policy in government tasks is the follow-up of *hukou* policy. The search results in the Labor Union policy category are documents that commend the model workers and encourage employment, which again wouldn't be a concern of confounding population-based policy. Among all these policies within the study period, we identify five policies as population-based policies, which are listed as follows:

1. **Opinions of the State Council on Further Promoting the Reform of the Household Registration System in 2014.**<sup>2</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>3</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 13th Five-Year Plan for the Development of a Modern Comprehensive Transportation System in 2017.**<sup>4</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.
4. **Opinions of the State Council on Further Strengthening the Planning and Management of Urban Rail Transit Construction in 2018.**<sup>5</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.
5. **Opinions of the State Council on Promoting the Improvement and Expansion of the Domestic Service Industry in 2019.**<sup>6</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 less concerning to our analysis of *hukou* reform.

As the size of a city is defined using its population, we also conducted a comprehensive search on policies using the keyword "city size". Table A4 presents the counts of policies containing the keyword "city size" or terms with similar meanings by year and policy categories. Similar to the policies search of "urban population", most policies containing the keyword "city size" or its

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<sup>2</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>3</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>4</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>5</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>6</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).



synonyms fall into the category of the regional plan, where the word “city size” is used to describe the city status and should not be viewed as population-based policies. Among all the other policies, we identify one population-based policy except for the *hukou* policy:

1. **Opinions of the State Council on Accelerating the Promotion of Ecological Civilization Construction in 2015.**<sup>7</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.

We view this policy as an addition of *hukou* policy that both policies are placed under the *National New-type Urbanization Plan (2014-2016)*. Overall, we don’t find any population-based policies confounding our analysis of *hukou* reform.

**Table A3.** Counts of Policies Using Keyword “Urban Population”

Year	Regional Plan	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	1	0	0	0	0	0	2	7
2015	7	1	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 category.

## A.4 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)).

<sup>7</sup>[https://www.gov.cn/gongbao/content/2015/content\\_2864050.htm](https://www.gov.cn/gongbao/content/2015/content_2864050.htm).

**Table A4.** Counts of Policies Using Keyword “City Size”

Year	Regional Plan	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 category.

**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>8</sup> Specifically, we estimate the following Probit model:

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

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

<sup>8</sup>The retirement age is 60 for both prefecture level and deputy-province-level leaders and 65 for province-level leaders.

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 A5 reports the estimation results. The first two columns shows estimates by a linear 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 A5.** Prediction of Promotion Prospects

	Dependent: Promotion			
	(1) LPM	(2) LPM	(3) Probit	(4) 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 × 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 is adopted so that the sample size is proportional to the number of migrants in each region. The data provide rich information on migrants' demographics, families, labor market outcomes, and attitudes. CMDS has a much larger sample size of migrants, ranging from 100 to 200 thousand for each year, than any other survey data in China. We use eight waves of surveys between 2011 and 2018. We focus our attention on individuals of the working ages, 25–54. We only keep individuals in prefectures that appear in all eight waves; as such, our sample covers 255 prefectures.

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

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

## Appendix B Ancillary Results

### B.1 Temporariness of Internal Migration in China

### B.2 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.

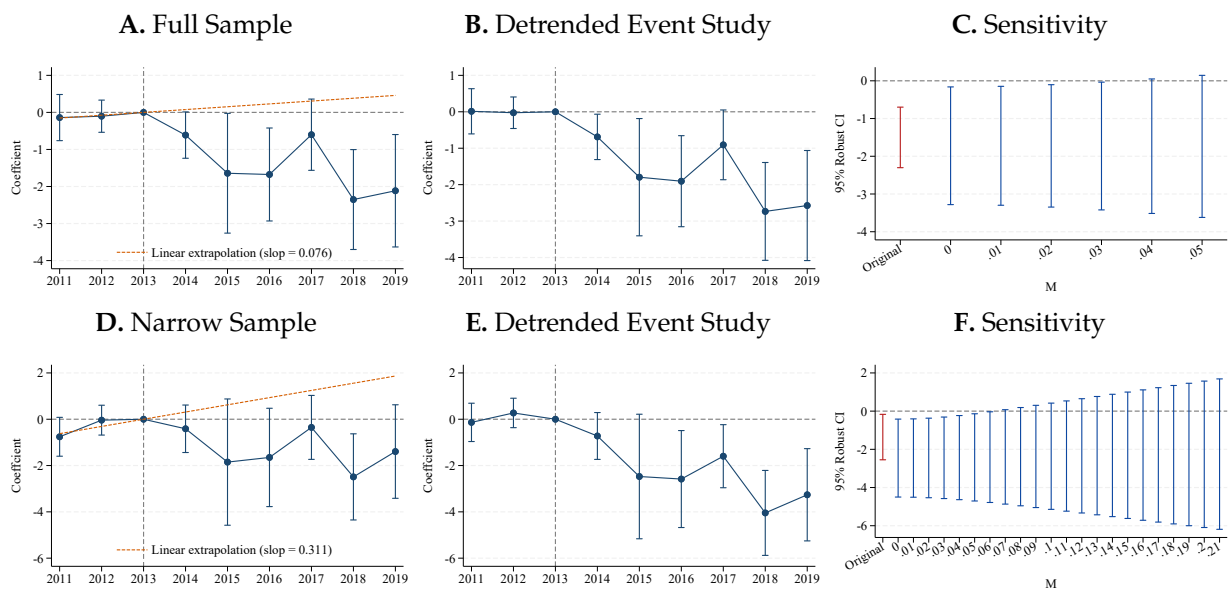
First, we can fit a linear trend based on the pre-reform event study estimates and extrapolate it to the post-reform periods, as shown by the first column of Figure [A1](#). Apparently, the pretrends are slightly upward. If these trends persist to the post-reform periods, the decline in unrest rates 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 [A1](#) 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 [A1](#), 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 [A1](#) shows that our results can withstand very nonlinear differential trends. For instance, consider Figure [A1C](#) 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 [A1F](#) suggests that we cannot reject a null effect only if we are willing to assume that the differential trend's slope is more than  $\pm \frac{0.06}{0.311} \approx \pm 19.3\%$  off the linear pretrend slope.

Taken together, we show our results are robust to allowing a linear differential trend implied by the pretrends. In addition, the results hold even if there is a significant amount of non-linearity in differential trends.



**Figure A1.** Detrended Event-Study Coefficients and Application of **Rambachan and Roth (2023)**

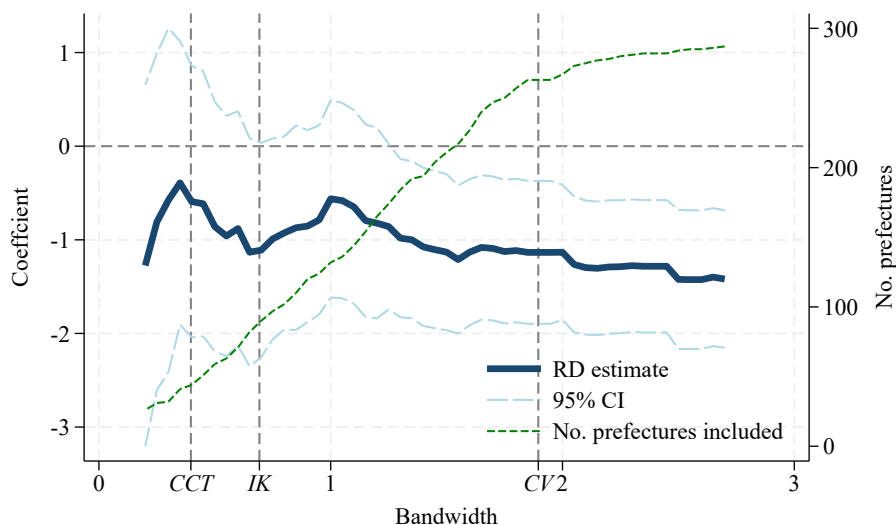
Note: This figure reports the sensitivity test for the event study results from both full and narrow samples. The first column depicts the linear trend implied by pre-reform event study coefficients and it is extrapolated to post-reform periods. The middle column shows the deviations of event study coefficients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

### B.3 Bandwidth Choices

Figure **A2** 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.

### B.4 The Effects of *Hukou* Reform on Population

In this section, we investigate the effects of the *hukou* reform on both total population and urban population. We study effects on both levels and growth rates. Figure **A3** reports event study estimates using the DiD-RD strategy. For completeness, we also report estimates using the DiD



**Figure A2.** Estimates under Different Bandwidth Choices

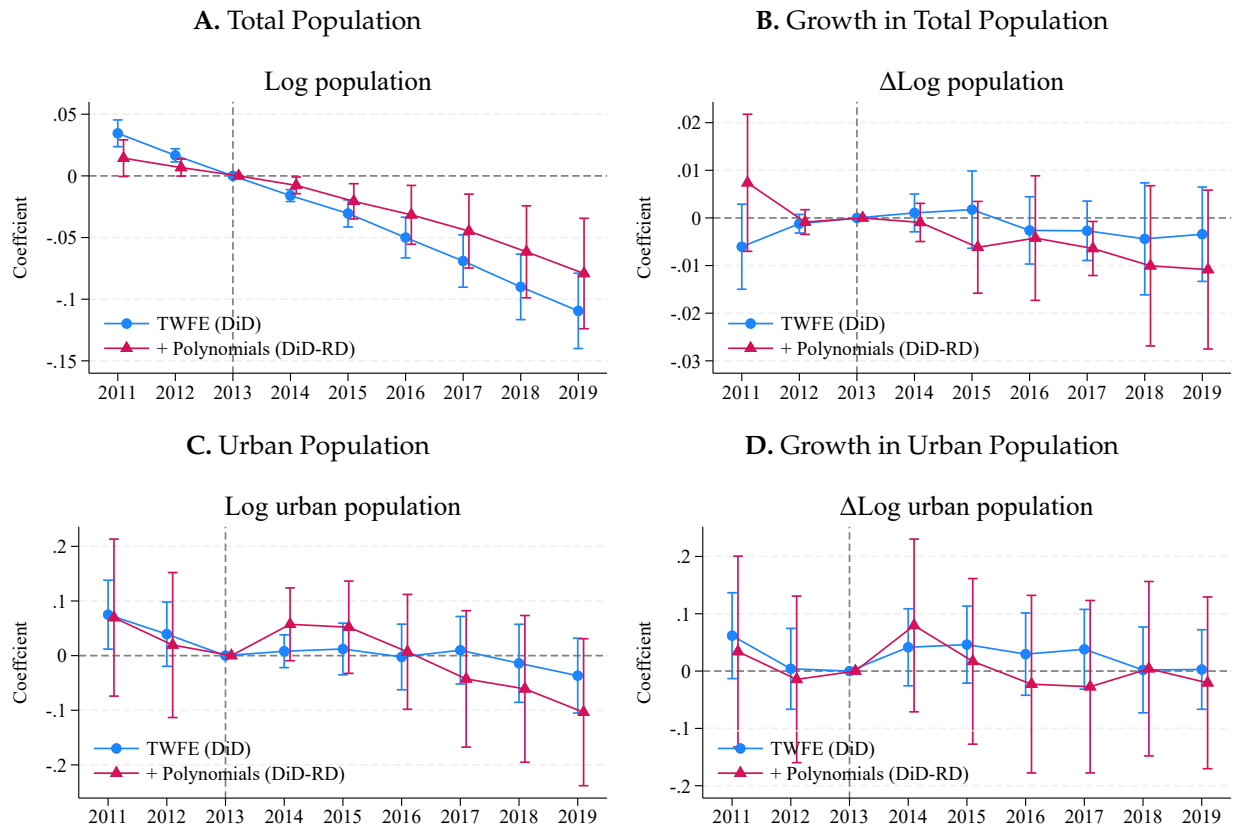
Note: This figure presents the estimated coefficient on  $Reform_i \times Post_t$  in Equation 2, when we impose different bandwidth restrictions to our sample. We note three commonly used optimal bandwidths proposed in the literature: [Calonico et al. \(2014\)](#) a.k.a. CCT, [Imbens and Kalyanaraman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

strategy. Regarding total population, the DiD estimates in Figure A3A 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 A3B); by flexibly controlling for heterogeneity due to urban sizes, the DiD-RD estimates show that despite moderate pretrends, there appears to be a relative decline in total population after the reform starts, which is due to a drop in population growth rate. These results indicate the *hukou* reform has a null effect or possibly a negative effect on a prefecture's total population. When it comes urban population, both DiD and DiD-RD 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.

## B.5 Addressing Time-Varying Prefecture Sizes

For our main results reported in Section 4, we scale the number of labor unrest events using working-age population, measured in the population census of 2010. One concern is that the results are simply due to time varying prefectures sizes rather than changes in underlying engagement of unrest. We show that our results hold even if we use time-varying population for scaling. There are no annual data on working population. Instead, we use time-varying total population and urban population, sourced from [Rogoff and Yang \(2024\)](#) and the Urban Construction Statistical Yearbooks, respectively. Figure A4 reports the results, confirming our findings that the *hukou* reform leads to a significant decrease in unrest rates.



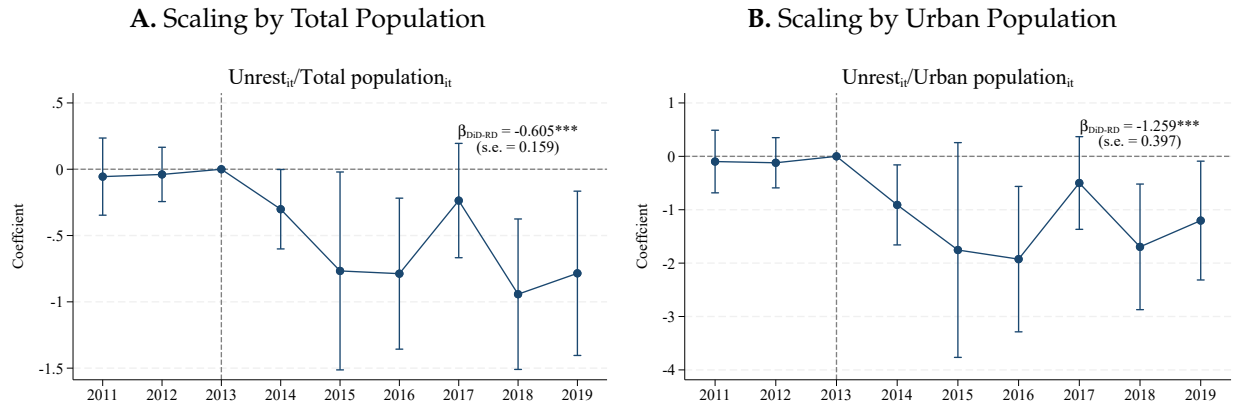
**Figure A3.** Dynamic Effects of the *Hukou* Reform on Population

Note: This figure reports the dynamic effects of the *hukou* reform on population. We study two metrics of population, total population and urban population, and 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.

## B.6 Reporting 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 A6 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)





**Figure A4. Scaling Unrest Events Using Time-Varying Prefecture Size**

Note: This figure reports the results when the number of unrest events is scaled by time-varying prefecture size. Figure A4A uses total population, and Figure A4B uses urban population. We visualize estimates from a dynamic specification:  $Y_{it} = \sum_{s \neq 2013} \beta_s (Reform_i \times D_s) + f[\Delta \log(P_{i,2014}); \zeta_{Reform,t}] + \lambda_i + \mu_t + \varepsilon_{it}$ . The solid points are points estimates of  $\beta_s$ 's, and the caps are 95 percent confidence intervals. We also report the estimate from a static specification:  $Y_{it} = \beta (Reform_i \times Post_t) + f[\Delta \log(P_{i,2014}); \zeta_{Reform,t}] + \lambda_i + \mu_t + \varepsilon_{it}$ . All standard errors are clustered at the prefecture level. \*  $p < 0.1$  \*\*  $p < 0.05$  \*\*\*  $p < 0.01$

looks at the reporting of protest events.<sup>9</sup> Likewise, we do not find the reporting of protests varies significantly by reform status.

Internet censorship was rising during the period we study (King et al., 2017). One may be concerned that reform prefectures' decrease in unrest rates is an artifact of changes in reporting of local events due to censorship. If so, one may expect that the decrease concentrates in reform prefectures where information tends to be censored to a greater extent. We test this hypothesis by examining the *hukou* reform's heterogeneous effect by censorship. To measure censorship, we use data from Qin et al. (2017), who measure the level of censorship using the share of deleted posts on Weibo at the provincial level. We create a dummy variable for high, above-median levels of censorship. Column (3) shows that there is no strong heterogeneity by censorship in the reform's effect on labor unrest.

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

**Table A6.** *Hukou* Reform and Reporting of Local Events

	(1)	(2)	(3)
	All events/L GDELТ	Protests/L GDELТ	Unrest/L CLB
Reform × Post	527.355 (522.334)	6.523 (4.992)	-1.420*** (0.371)
Reform × Post × High level of censorship			0.005 (0.176)
Control mean	1108.759	9.919	3.395
Prefecture FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Polynomials	Yes	Yes	Yes
Observations	2,583	2,583	2,583

Note: The dependent variables in Columns (1) and (2) are the number of local events and the number of protests recorded in GDELТ (scaled by working-age population). The dependent variable in Column (3) is the number of labor unrest events in CLB data with the same scaling. “High level of censorship” is a dummy variable that equals one if a prefecture’s affiliated province has an above-median share of deleted Weibo posts, based on data from [Qin et al. \(2017\)](#). Robust standard errors are clustered at the prefecture level and reported in parentheses.

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

**Table A7.** 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. 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.

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

**Table A8.** Balance Tests for Origin-Level Trade Shocks

Dependent	Coef.	SE
<b>Panel A: Pretrends</b>		
$\Delta\text{Log population, 2009–2010}$	0.002***	(0.000)
$\Delta\text{Log GDP, 2009–2010}$	-0.001	(0.000)
$\Delta\text{Log expenditure, 2009–2010}$	0.000	(0.001)
$\Delta\text{Log expenditure on public security, 2009–2010}$	0.001	(0.001)
<b>Panel B: Predetermined characteristics</b>		
Share of migrants, 2010	0.006***	(0.001)
Share of urban residents, 2010	0.009***	(0.001)
Share of secondary sector workers, 2010	0.002**	(0.001)
Share of tertiary sector workers, 2010	0.003***	(0.001)
Share of internet users, 2010	0.005***	(0.001)

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

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

## B.7 Validity Tests for Shift-Share Designs

## B.8 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:

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

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

<sup>9</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.

**Table A9.** 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)	-0.791*** (0.246)
Mfx. on hazard rate	-0.653	-0.578	-0.582	-0.546
Polynomials		Yes	Yes	Yes
Stratified hazard function		Yes	Yes	Yes
Drop prefectures w/ few obs.				Yes
Observations	58,701	58,701	58,701	56,667

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.

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

## B.9 Replication of An et al. (2024)

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

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

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

Table A10 explores how results change due to these decisions. To avoid other sources of differences, e.g., sample construction, we produce these results using the sample in An et al. (2024)'s replication package. Panel A adopts the An et al. (2024)'s definition of reform status, whereas Panel A uses our definition. Columns (1)–(3) report DiD estimates, and Columns (4)–(6) report the DiD-RD estimates. Clearly, different definitions of reform status do change the reform and non-reform groups. We, by using the total urban population, define 10 less prefectures as reform ones. Nonetheless, different definitions of reform status do not change the results markedly. The major change is due to identification strategy. Once flexible polynomial functions of log urban

population are included, there are no longer discernible effects of the *hukou* reform on wages and ASS. This pattern also appears when we use our own working sample (see Table A11).

The DiD-RD strategy may produce cleaner, more reliable results than a DiD design as it flexibly controls heterogeneity due to urban sizes. Also, we find it difficult to reconcile the *hukou* reform's negative effects on wages and ASS. An et al. (2024) interpret them as reflecting that the reform has induced labor inflow and thus reduces wages and imposes pressure to the local social security system. However, as we show in Appendix B.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 A10.** 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.) × Post	0.006	-0.077***	-0.041**	-0.001	0.018	0.008
	(0.006)	(0.018)	(0.018)	(0.009)	(0.021)	(0.020)
Control mean	0.872	8.131	0.526	0.872	8.131	0.526
Sample period	2011–17	2011–17	2011, 13, 16	2011–17	2011–17	2011, 13, 16
No. prefectures (No. reform prefectures)	267 (241)	267 (241)	266 (239)	267 (241)	267 (241)	266 (239)
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Polynomials				Yes	Yes	Yes
Observations	788,219	642,700	144,145	788,219	642,700	144,145
<b>Panel B: Our definition of treatment</b>						
Reform × Post	0.007	-0.087***	-0.038**	0.004	-0.004	0.010
	(0.006)	(0.016)	(0.018)	(0.009)	(0.021)	(0.027)
Control mean	0.872	8.131	0.526	0.872	8.131	0.526
Sample period	2011–17	2011–17	2011, 13, 16	2011–17	2011–17	2011, 13, 16
No. prefectures (No. reform prefectures)	267 (230)	267 (230)	266 (228)	267 (230)	267 (230)	266 (228)
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Polynomials				Yes	Yes	Yes
Observations	788,219	642,700	144,145	788,219	642,700	144,145

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

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

**Table A11.** DiD versus DiD-RD 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 DiD-RD 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.

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

## Appendix C Additional Robustness Checks

### C.1 Balancing of Baseline Characteristics

In Section 3.3, we note that there remains some difference in baseline covariates between reform and non-reform prefectures, despite the inclusion of polynomial controls. Such imbalances could threaten our results they are associated with differential trends in unrest. To address this issue, in this section, we use several strategies to balance the baseline covariates and explore the robustness of our results.

Table A12 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 A12.** 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	2511	2511	1764	2484

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

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

## C.2 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 A5](#). All results confirm that the *hukou* reform significantly reduces unrest rates.

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

	Alt. Polynomials		Alt. Unrest Measures		PPML	SAR	SDID
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Unrest/L	Unrest/L	Log(Unrest/L)	IHS(Unrest/L)	Unrest/L	Unrest/L	Unrest/L
Reform $\times$ Post	-0.720 (0.524)	-1.010 (0.657)	-0.305*** (0.086)	-0.383*** (0.112)	-0.545* (0.308)	-1.456*** (0.363)	-1.604*** (0.258)
Control mean	3.395	3.395	1.209	1.549	3.395	3.395	3.395
Method	OLS	OLS	OLS	OLS	PPML	SAR	SDID
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Polynomials	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial order	2	3	1	1	1	1	-
Observations	2,583	2,583	2,583	2,583	2,583	2,583	2,583

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

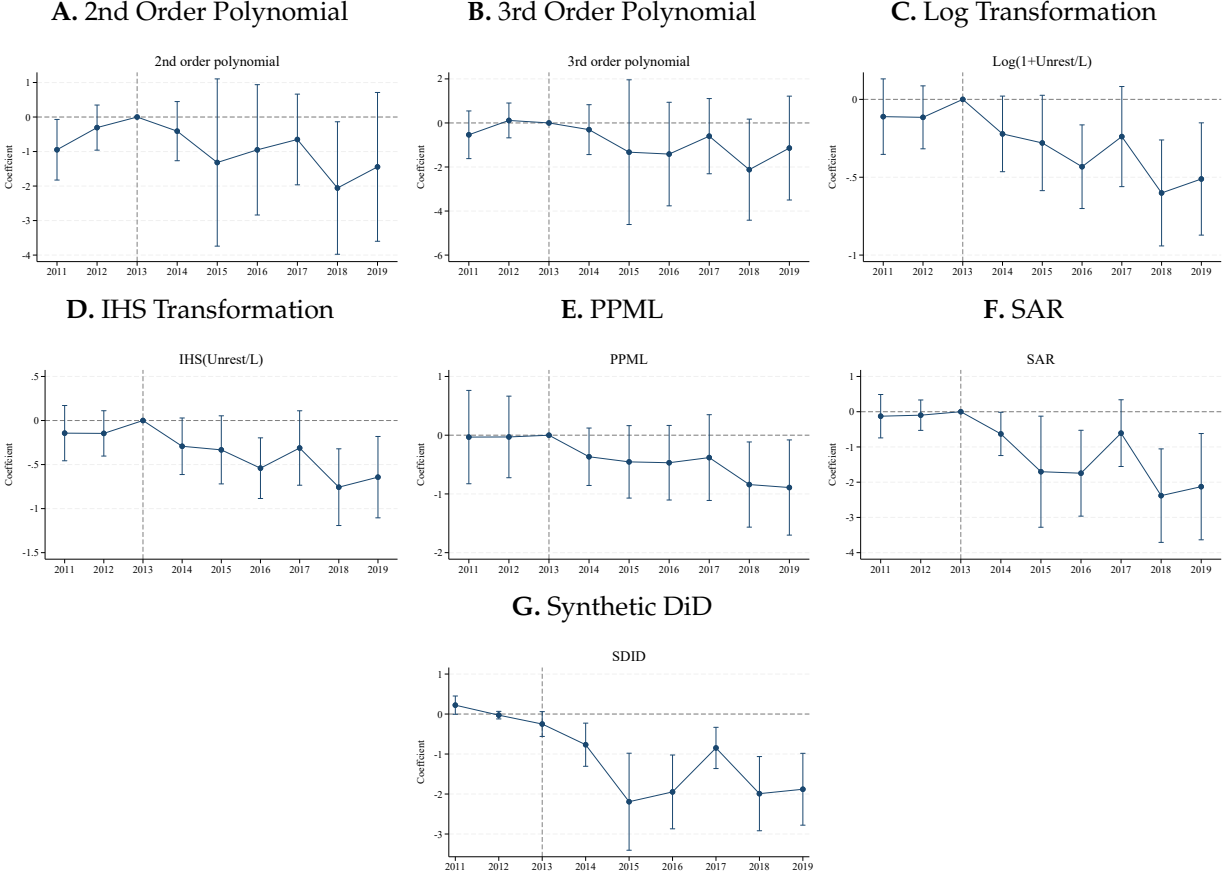
\*  $p < 0.1$  \*\*  $p < 0.05$  \*\*\*  $p < 0.01$

## C.3 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 A6](#) 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.

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





**Figure A5. Robustness: Alternative Specifications and Estimators, Event-Study Results**

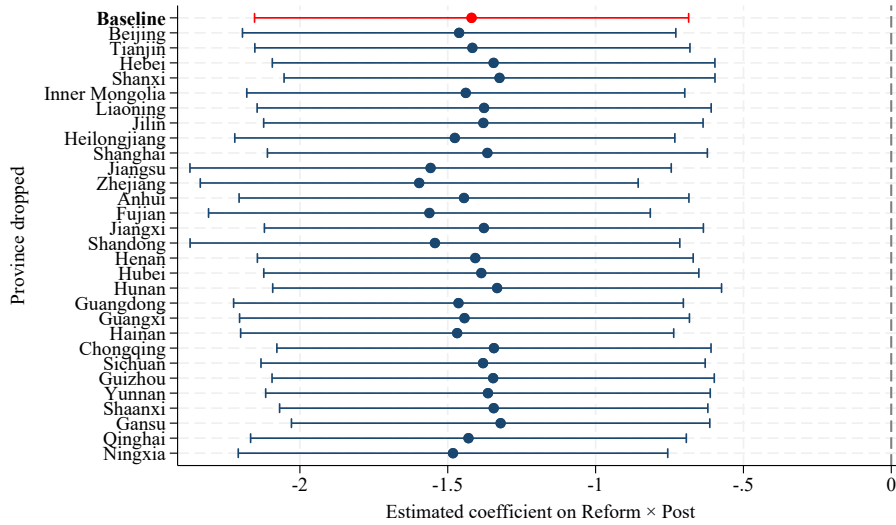
Note: This figure presents the event study results using alternative specifications and estimators. The solid dots are point estimates, and the caps are 95 percent confidence intervals. Standard errors clustered at the prefecture level are used to construct confidence intervals.

size very close to the reform cutoff, specifically,  $|\Delta \log (P_{i,2014})| < 0.2$ . In Column (3), we exclude prefectures whose population-based reform status does not agree with manually coded reform status, for the concern that these prefectures do not “comply” due to unrest considerations. But recall from Section 3.1, we only have 17 prefectures with such discrepancies. Our results survive these exercises, and become even stronger in some cases.

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

$$Y_i = \frac{\sum_{t=2014}^{2019} \frac{Unrest_{it}}{L_{i,2010}}}{6} - \frac{\sum_{t=2011}^{2013} \frac{Unrest_{it}}{L_{i,2010}}}{3} = Reform_i + f [\Delta \log (P_{i,2014}); \zeta_{Reform}] + \varepsilon_i. \quad (A4)$$

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,



**Figure A6. Robustness: Dropping One Province Each Time**

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

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,  $|\Delta \log(P_{i,2014})| < 0.2$ . 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.

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

## Appendix D Conceptual Model: Retention and Unrest Participation

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

### D.1 Model Setup

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

$$V = u_1 + \beta u_2, \quad (\text{A5})$$

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>10</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:

$$u_1 = w + ae - \frac{1}{2}\gamma e^2. \quad (\text{A6})$$

**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:

$$u_{2d} = x - c - \frac{1}{2}\kappa e^2. \quad (\text{A7})$$

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

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<sup>10</sup>In fact, wage arrears are a reason for 70.58% unrest events recorded in CLB data during 2011–2019.

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

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

$$u_2 = ru_{2d} + (1-r)u_{2o} = r \left( x - c - \frac{1}{2}\kappa e^2 \right). \quad (\text{A8})$$

**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:

$$\begin{aligned} \mathcal{V} &= \max_{e,r} V = u_1 + \beta u_2 \\ &= w + ae - \frac{1}{2}\gamma e^2 + \beta r \left( x - c - \frac{1}{2}\kappa e^2 \right). \end{aligned} \quad (\text{A9})$$

## D.2 Results

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

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

$$\mathcal{V}_1 = w + e_1 + \beta E(x) - \frac{1}{2}(\gamma + \beta\kappa)e_1^2 - \beta c. \quad (\text{A10})$$

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

$$\mathcal{V}_0 = w + e_0 - \frac{1}{2}\gamma e_0^2. \quad (\text{A11})$$

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

$$\mathcal{V}_1 > \mathcal{V}_0 \quad (\text{A12})$$

$$x > c + \frac{a^2\kappa}{2\gamma(\gamma + \beta\kappa)} \quad (\text{A13})$$

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

$$p \equiv \Pr(r = 1) = 1 - F \left[ c + \frac{a^2\kappa}{2\gamma(\gamma + \beta\kappa)} \right]. \quad (\text{A14})$$

The aggregate level of unrest is

$$\mathcal{E} \equiv E(e) = pe_1 + (1-p)e_0 = \frac{a}{\gamma} \left( 1 - \frac{p\beta\kappa}{\gamma + \beta\kappa} \right). \quad (\text{A15})$$

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:

$$\frac{\partial p}{\partial c} = -f \left[ c + \frac{a^2\kappa}{2\gamma(\gamma + \beta\kappa)} \right] < 0, \quad (\text{A16})$$

$$\frac{\partial \mathcal{E}}{\partial c} = -\frac{a\beta\kappa}{\gamma(\gamma + \beta\kappa)} \frac{\partial p}{\partial c} > 0. \quad (\text{A17})$$

■

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

*Proof.*

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

provided that  $c$  is sufficiently high.

■

## Appendix E Causal Mediation Analysis

### E.1 Conventional Approach

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

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

$$Y_i(t, m) = \tau_i t + \gamma_i m + u_i, \quad (\text{A19})$$

$$M_i(t) = \pi_i t + v_i. \quad (\text{A20})$$

Therefore,

$$Y_i(t) \equiv Y_i(t, M_i(t)) = (\tau_i + \gamma_i \pi_i) t + u_i + \gamma_i v_i \equiv \beta_i t + \eta_i, \quad (\text{A21})$$

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

DEFINITION A1 (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 A1 (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):

$$Y_i = \alpha_1 + \beta T_i + e_{i1}, \quad (\text{A22})$$

$$Y_i = \alpha_2 + \tau T_i + \gamma M_i + e_{i2}, \quad (\text{A23})$$

$$M_i = \alpha_3 + \pi T_i + e_{i3}. \quad (\text{A24})$$

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

$$\widehat{ATE} \equiv \hat{\beta} - \hat{\tau} = \hat{\gamma}\hat{\pi}. \quad (\text{A25})$$

$\hat{w}$  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  $\hat{\beta}$ .

$$\hat{\beta} = \frac{\text{Cov}(T_i, Y_i)}{\text{Var}(T_i)} \quad (\text{A26})$$

$$= \frac{\text{Cov}(T_i, \beta_i T_i + \eta_i)}{\text{Var}(T_i)} \quad (\text{A27})$$

$$= E(\beta_i). \quad (\text{A28})$$

The second equality is by plugging in the potential outcome Equation [A21](#). The third equality is by Assumption [A1](#). Thus,  $\hat{\beta}$  identifies the ATE.

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

$$\hat{\tau} = \frac{\text{Cov}(T_i, Y_i - \hat{\gamma}M_i)}{\text{Var}(T_i)} = \hat{\beta} - \hat{\gamma}\hat{\pi}, \quad (\text{A29})$$

where  $\hat{\pi} = E(\pi_i)$  identifies the population average effect of  $T_i$  on  $M_i$  under Assumption [A1](#). 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 [A1](#) and Equation [A20](#),  $L(M_i | T_i) = E(M_i | T_i)$ , i.e., the linear projection recovers the conditional mean.<sup>11</sup>  $\hat{\gamma}$  has the following expression:

$$\hat{\gamma} = \frac{E(\tilde{M}_i Y_i)}{E(\tilde{M}_i^2)} \quad (\text{A30})$$

$$= \frac{E\left[\tilde{M}_i(\tau_i T_i + \gamma_i M_i + u_i)\right]}{E(\tilde{M}_i^2)} \quad (\text{A31})$$

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

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  $E(\gamma_i)$ ,

<sup>11</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.

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,

$$\widehat{ATE} = \hat{\beta} - \hat{\tau} \tag{A33}$$

$$= \left[ \frac{E(\tilde{M}_i M_i \gamma_i)}{E(\tilde{M}_i^2)} + \frac{E(\tilde{M}_i T_i \tau_i)}{E(\tilde{M}_i^2)} + \frac{E(\tilde{M}_i u_i)}{E(\tilde{M}_i^2)} \right] E(\pi_i) \tag{A34}$$

$$\neq AIE \tag{A35}$$

$$\equiv E(\gamma_i \pi_i) = E(\gamma_i)E(\pi_i) - Cov(\gamma_i, \pi_i). \tag{A36}$$

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

$$\text{Bias} = \widehat{ATE} - AIE = \underbrace{[\hat{\gamma} - E(\gamma_i)] E(\pi_i)}_{\text{bias 1}} + \underbrace{Cov(\gamma_i, \pi_i)}_{\text{bias 2}}. \tag{A37}$$

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 A2 (Sequential Ignorability).  $\{Y_i(t', m), M_i(t)\} \perp\!\!\!\perp M_i \mid T_i = t, \forall t, t', m$ .

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

$$E(\tilde{M}_i T_i \tau_i) = E[E(\tilde{M}_i T_i \tau_i \mid T_i)] \tag{A38}$$

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

$$= E(\tilde{M}_i T_i) E(\tau_i) \tag{A40}$$

$$= 0. \tag{A41}$$

The first equality is by the law of iterated expectations (LIE). The second equality is by Assumption [A2](#). The third equality is by Assumption [A1](#). The fourth equality is by the orthogonality of linear projection residuals. Using similar tricks yields

$$E(\tilde{M}_i u_i) = E[E(\tilde{M}_i u_i \mid T_i)] \tag{A42}$$



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

$$= E(\tilde{M}_i)E(u_i) \quad (\text{A44})$$

$$= 0. \quad (\text{A45})$$

Together with homogeneity assumption  $\gamma_i = \gamma$ ,  $\hat{\gamma} = \gamma = E(\gamma_i)$ . Therefore,  $\widehat{AIE} = AIE$ .<sup>12</sup>

## E.2 IV-Augmented Approach

The plausibility of Assumption A2 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  $Z_i$ .

$$Y_i(t, m) = \tau_i t + \gamma_i m + u_i, \quad (\text{A46})$$

$$M_i(t, z) = \pi_i t + \theta_i z + v_i. \quad (\text{A47})$$

Therefore, we have the following reduced-form model:

$$Y_i(t, z) \equiv Y_i(t, M_i(t, z)) \quad (\text{A48})$$

$$= (\pi_i + \gamma_i \pi_i) t + \gamma_i \theta_i z + (u_i + \gamma_i v_i) \quad (\text{A49})$$

$$= \beta_i t + \rho_i z + \eta_i, \quad (\text{A50})$$

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

We impose the following assumptions.

ASSUMPTION A3 (Treatment Exogeneity).  $\{Y_i(t', m), M_i(t, z), Z_i\} \perp\!\!\!\perp T_i$ , for all  $t, t', m$ , and  $z$ .

ASSUMPTION A4 (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)  $Y_i(t, m) |_{z=} = Y_i(t, m) |_{z'}$  for all  $z$  and  $z'$ .

<sup>12</sup>This result does not require the homogeneity assumption. With Assumption A2, one can further show that  $E(\tilde{M}_i M_i \gamma_i) = E(\tilde{M}_i M_i \gamma_i | T_i) = E(\tilde{M}_i M_i)E(\gamma_i) = E(\tilde{M}_i^2)E(\gamma_i)$ . Thus,  $\hat{\gamma} = E(\gamma_i)$ , hence, bias 1 = 0. Assumption A2 also implies that  $\gamma_i \perp\!\!\!\perp \pi_i | T_i$ . Thus, bias 2 =  $E[E(\gamma_i \pi_i | T_i)] - E(\gamma_i)E(\pi_i) = E[E(\gamma_i | T_i)E(\pi_i | T_i)] - E(\gamma_i)E(\pi_i) = 0$ , where the first equality is by the definition of covariance and the LIE, the second equality is by  $\gamma_i \perp\!\!\!\perp \pi_i | T_i$ , and the last equality is by Assumption A1. Taken together,  $\widehat{AIE} = AIE$ , even if the homogeneity assumption is not made.

3. (Relevance)  $E(\theta_i) \neq 0$ .  
4. (Monotonicity) Either  $\Pr(\theta_i \geq 0) = 1$  or  $\Pr(\theta_i \leq 0) = 1$ .

PROPOSITION A1. Under Assumptions A3 and A4, two stage least squares (2SLS) estimation of the LSEM, with  $M_i$  instrumented by  $Z_i$ , yields

$$\widehat{AIE} = \hat{\beta} - \hat{\tau} = \hat{\gamma}\hat{\pi} = \underbrace{E(\gamma_i\pi_i)}_{AIE} + \underbrace{[\hat{\gamma} - E(\gamma_i)]E(\pi_i)}_{bias\ 1} - \underbrace{Cov(\gamma_i, \pi_i)}_{bias\ 2}, \quad (A51)$$

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 A3, it is straightforward to show that

$$\hat{\beta} = \frac{Cov(T_i, Y_i)}{Var(T_i)} = E(\beta_i), \quad (A52)$$

$$\hat{\pi} = \frac{Cov(T_i, M_i)}{Var(T_i)} = E(\pi_i). \quad (A53)$$

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

$$\hat{\gamma} = \frac{E(\tilde{Z}_i Y_i)}{E(\tilde{Z}_i M_i)}, \quad (A54)$$

where  $\tilde{Z}_i = Z_i - L(Z_i | T_i)$  is the linear projection residual. By Assumption A3,  $E(Z_i | T_i) = E(Z_i)$  is linear, thus,  $L(Z_i | T_i) = E(Z_i | T_i)$ . We can show the following result.

$$E(\tilde{Z}_i Y_i) = E[\tilde{Z}_i(\beta_i T_i + \rho_i Z_i + \eta_i)] \quad (A55)$$

$$= E(\tilde{Z}_i T_i)E(\beta_i) + E(\tilde{Z}_i Z_i)E(\rho_i) + E(\tilde{Z}_i)E(\eta_i), \quad (A56)$$

$$= E(\tilde{Z}_i Z_i)E(\rho_i). \quad (A57)$$

The second equality is by Assumptions A3 and A4(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:

$$E(\tilde{Z}_i M_i) = E[\tilde{Z}_i(\pi_i T_i + \theta_i Z_i + u_i)] \quad (A58)$$

$$= E(\tilde{Z}_i T_i)E(\pi_i) + E(\tilde{Z}_i Z_i)E(\theta_i) + E(\tilde{Z}_i)E(u_i), \quad (A59)$$

$$= E(\tilde{Z}_i Z_i)E(\theta_i). \quad (A60)$$

Taken together,

$$\hat{\gamma} = \frac{E(\rho_i)}{E(\theta_i)} = \frac{E(\theta_i \gamma_i)}{E(\theta_i)}. \quad (A61)$$

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

$$\hat{\gamma} = \frac{E(\tilde{Z}_i M_i \gamma_i)}{E(\tilde{Z}_i M_i)}. \quad (\text{A62})$$

Therefore,

$$\widehat{ATE} = \hat{\beta} - \hat{\tau} \quad (\text{A63})$$

$$= \hat{\gamma} E(\pi_i) \quad (\text{A64})$$

$$= \hat{\gamma} E(\pi_i) + E(\gamma_i \pi_i) - \text{Cov}(\gamma_i, \pi_i) + E(\gamma_i) E(\pi_i) \quad (\text{A65})$$

$$= \underbrace{E(\gamma_i \pi_i)}_{\text{AIE}} + \underbrace{[\hat{\gamma} - E(\gamma_i)] E(\pi_i)}_{\text{bias 1}} - \underbrace{\text{Cov}(\gamma_i, \pi_i)}_{\text{bias 2}}, \quad (\text{A66})$$

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  $\widehat{ATE} = AIE$ . ■

### E.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:

$$Y_i = \alpha_1 + \beta T_i + \mathbf{X}_i' \zeta_1 + e_{i1}, \quad (\text{A67})$$

$$Y_i = \alpha_2 + \tau T_i + \gamma M_i + \mathbf{X}_i' \zeta_2 + e_{i2}, \quad (\text{A68})$$

$$M_i = \alpha_3 + \pi T_i + \mathbf{X}_i' \zeta_3 + e_{i3}. \quad (\text{A69})$$

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

DEFINITION A2 (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 A5 (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 A6 (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 A7 (IV Linearity).  $E(Z_i | r_i)$  is linear in  $\mathbf{X}_i^{(0)}$ .

Assumption A5 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 A6 warrants validity of IV. Importantly, independence only needs to hold conditional on running variable  $r_i$ . Assumption A7 assumes linearity of IV, as in Ishimaru (2024).

PROPOSITION A2. Under Assumptions A5, A6, and A7, 2SLS estimation of the LSEM, with  $M_i$  instrumented by  $Z_i$ , yields

$$\begin{aligned} \widehat{AIE} &= \hat{\beta} - \hat{\tau} = \hat{\gamma}\hat{\pi} \\ &= \underbrace{E(\gamma_i\pi_i | r_i = 0)}_{AIE} + \underbrace{[\hat{\gamma} - E(\gamma_i | r_i = 0)] E(\pi_i | r_i = 0)}_{bias\ 1} - \underbrace{Cov(\gamma_i, \pi_i | r_i = 0)}_{bias\ 2}, \end{aligned} \quad (A70)$$

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 A5, linear regression identifies conditional means of potential outcomes. Thus, a linear regression of  $Y_i$  on 1,  $T_i$  and  $\mathbf{X}_i$  yields

$$\hat{\beta} = \lim_{r \downarrow 0} E[Y_i(1, Z_i) | r_i = r] - \lim_{r \uparrow 0} E[Y_i(0, Z_i) | r_i = r] \quad (A71)$$

$$= \lim_{r \downarrow 0} E[\beta_i + \rho_i Z_i + \eta_i | r_i = r] - \lim_{r \uparrow 0} E[\rho_i Z_i + \eta_i | r_i = r] \quad (A72)$$

$$= E(\beta_i | r_i = 0), \quad (A73)$$

where the first equality is by linearity assumed in Assumption A5, the second equality plugs in potential outcomes, and the last equality uses continuity implied by A5. 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:

$$\hat{\gamma} = \frac{E[\tilde{Z}_i(\beta_i T_i + \rho_i Z_i + \eta_i)]}{E[\tilde{Z}_i(\pi_i T_i + \theta_i Z_i + v_i)]}. \quad (\text{A74})$$

Analyze term by term.

$$E(\tilde{Z}_i T_i \beta_i) = E[E(\tilde{Z}_i T_i \beta_i | r_i)] = E[E(\tilde{Z}_i | r_i) T_i E(\beta_i | r_i)] = 0. \quad (\text{A75})$$

The first equality is by the LIE. The second equality is by Assumption A6(1) and the fact that  $T_i$  is completely determined by  $r_i$  in a RDD. The last equality is due to Assumption A7 that implies  $L[Z_i | \mathbf{X}_i^{(0)}] = E[Z_i | \mathbf{X}_i^{(0)}]$ . Similarly,

$$E(\tilde{Z}_i T_i \pi_i) = 0, \quad (\text{A76})$$

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

$$E(\tilde{Z}_i v_i) = 0. \quad (\text{A78})$$

Therefore,

$$\hat{\gamma} = \frac{E(\tilde{Z}_i Z_i \theta_i \gamma_i)}{E(\tilde{Z}_i Z_i \theta_i)} = \frac{E[E(\tilde{Z}_i Z_i | r_i) E(\theta_i \gamma_i | r_i)]}{E[E(\tilde{Z}_i Z_i | r_i) E(\theta_i | r_i)]} = \frac{E[\text{Var}(Z_i | r_i) E(\theta_i \gamma_i | r_i)]}{E[\text{Var}(Z_i | r_i) E(\theta_i | r_i)]}. \quad (\text{A79})$$

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

$$\hat{\gamma} = \frac{E(\tilde{Z}_i M_i \gamma_i)}{E(\tilde{Z}_i M_i)}. \quad (\text{A80})$$

Taken together,

$$\hat{\beta} - \hat{\tau} = \hat{\gamma} E(\pi_i | r_i = 0) \quad (\text{A81})$$

$$= \hat{\gamma} E(\pi_i | r_i = 0) + E(\gamma_i \pi_i | r_i = 0) - \text{Cov}(\gamma_i, \pi_i | r_i = 0) + E(\gamma_i | r_i = 0) E(\pi_i | r_i = 0) \quad (\text{A82})$$

$$= \underbrace{E(\gamma_i \pi_i | r_i = 0)}_{\text{AIE}} + \underbrace{[\hat{\gamma} - E(\gamma_i | r_i = 0)] E(\pi_i | r_i = 0)}_{\text{bias 1}} - \underbrace{\text{Cov}(\gamma_i, \pi_i | r_i = 0)}_{\text{bias 2}}, \quad (\text{A83})$$

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  $\gamma_i \perp\!\!\!\perp r_i$ . ■

## E.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

$$\text{Bias} = \hat{\gamma}\hat{\pi} - \underbrace{E(\gamma_i\pi_i)}_{\text{AIE}} \quad (\text{A84})$$

$$= [\hat{\gamma} - E(\gamma_i)]\hat{\pi} - \text{Cov}(\gamma_i, \pi_i) \quad (\text{A85})$$

$$= \text{Cov}(\phi_i, \gamma_i)\hat{\pi} - \text{Cov}(\gamma_i, \pi_i) \quad (\text{A86})$$

$$= \rho_{\phi\gamma}\sigma_\phi\sigma_\gamma\hat{\pi} - \rho_{\gamma\pi}\sigma_\gamma\sigma_\pi, \quad (\text{A87})$$

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 A8 (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 A3.** *Under Assumption A8, for every given  $\rho_{\phi\gamma}$  and  $\rho_{\gamma\pi}$ , AIE is identified by estimand*

$$\widetilde{\text{AIE}} = \left[ \frac{\sqrt{3}\hat{\gamma}}{\sqrt{3}\text{sgn}(\gamma_i) + \rho_{\phi\gamma}\hat{\sigma}_\phi} + \frac{\rho_{\gamma\pi}\hat{\sigma}_\phi \text{sgn}(\pi_i)}{\sqrt{3}} \right] \hat{\pi}. \quad (\text{A88})$$

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 A8,  $E(\gamma_i) = \sqrt{3}\text{sgn}(\gamma_i)\sigma_\gamma$ . 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,

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

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

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

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

The following proposition extends to the case of RDDs.

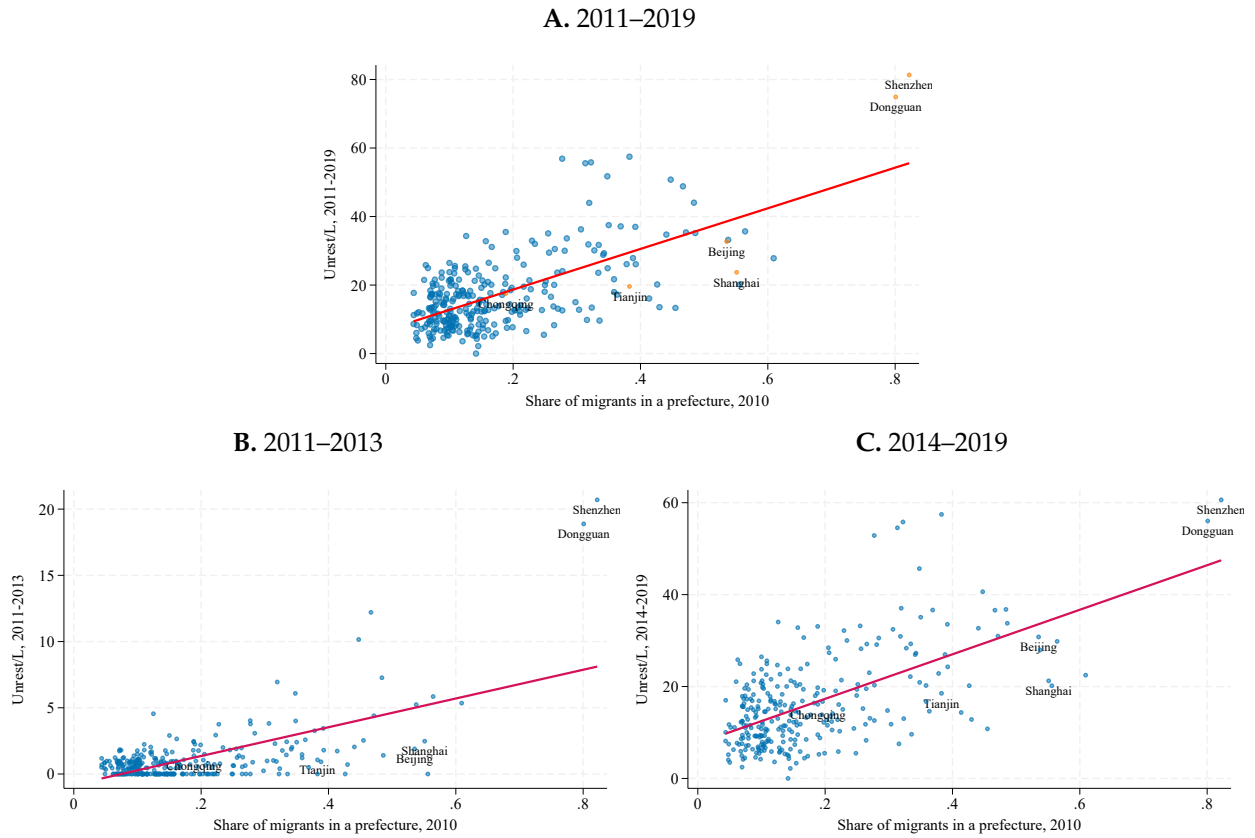
PROPOSITION A4. *Suppose  $\{\gamma_i, \pi_i\} \perp\!\!\!\perp r_i$ . Under Assumption A8, for every given  $\rho_{\phi\gamma}$  and  $\rho_{\gamma\pi}$  and  $\rho_{\gamma\pi}$ , AIE is identified by estimand*

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

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. ■

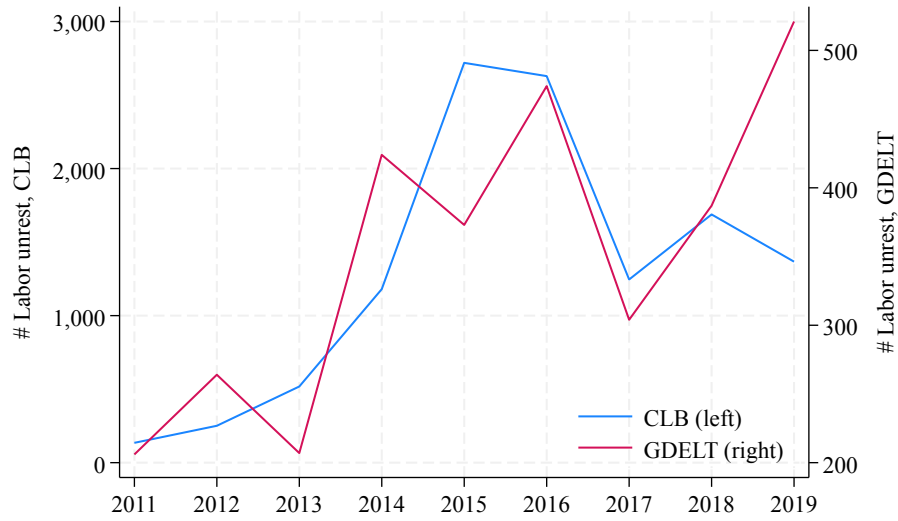
## Appendix F Additional Figures



**Figure A7. Migrant Share and Labor Unrest Rate**

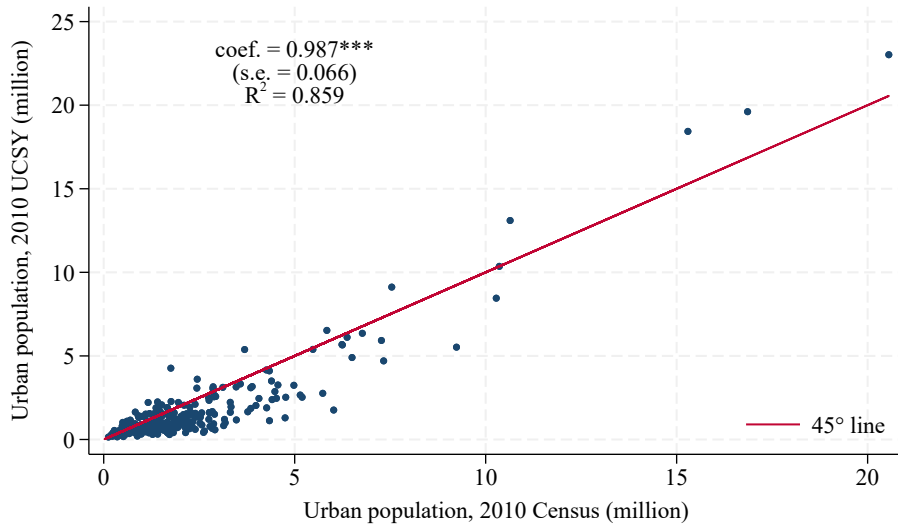
Note: This figure depicts the relationship between the migrant share and the labor unrest rate. The migrant share is measured using the 2010 population census: It is defined as the share of the population whose *hukou* registration is not in the current prefecture. The labor unrest rate is measured using 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.





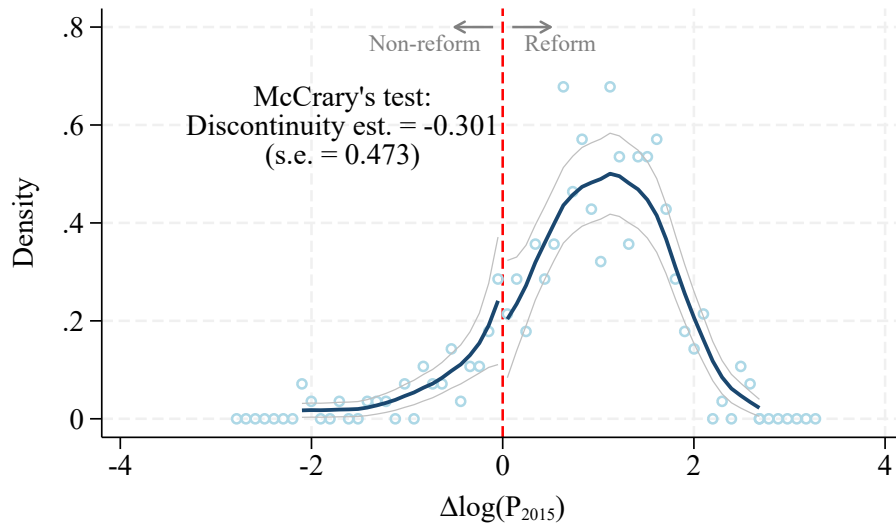
**Figure A8. Labor Unrest in CLB and GDELT**

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 A9. Urban Population: Census versus UCSY, 2010**

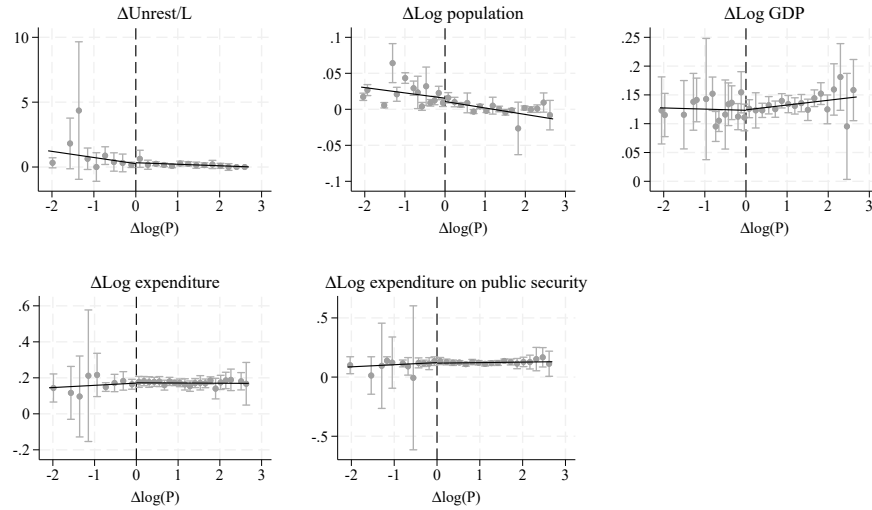
Note: This figure depicts prefecture-level data on urban population in 2010 from two sources: UCSY and census.



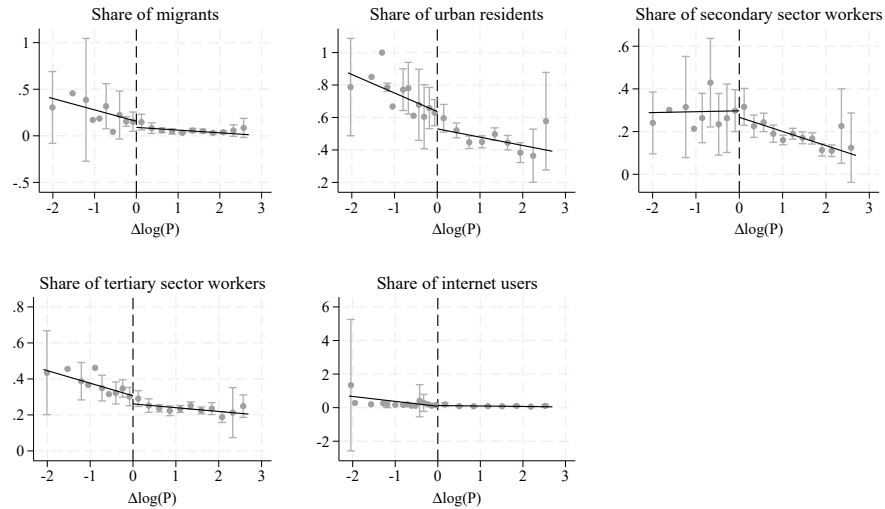
**Figure A10.** Density of Centered Log Urban Population of 2015

Note: This figure depicts the density of centered log urban population of 2015,  $\Delta \log(P_{i,2015})$ . We report the McCrary's test of density discontinuity at  $\Delta \log(P_{i,2015}) = 0$ .

### A. Pretrends

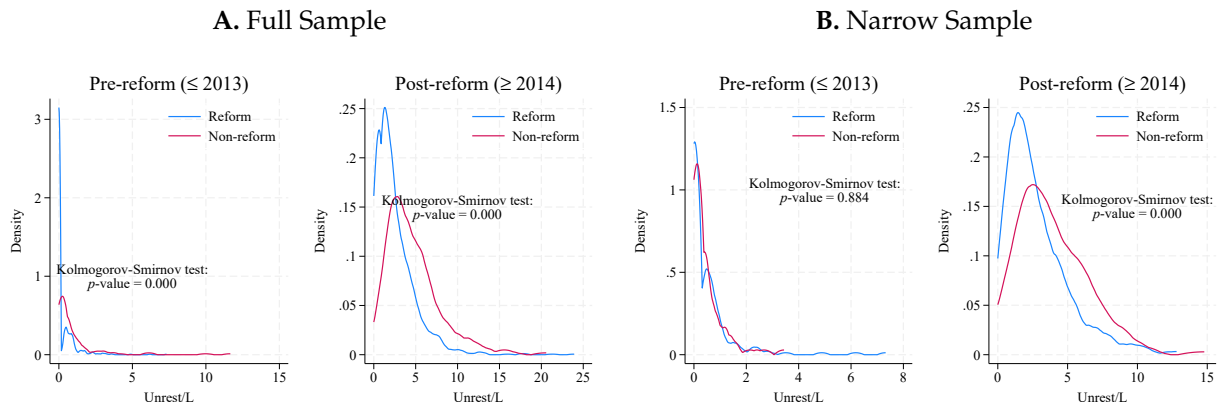


### B. Predetermined Characteristics



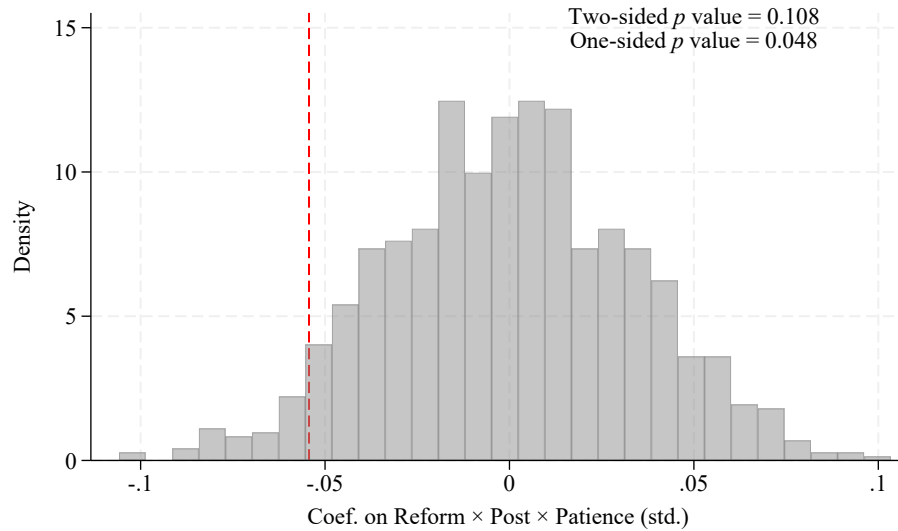
**Figure A11. RD Plots of Pre-reform Covariates**

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.



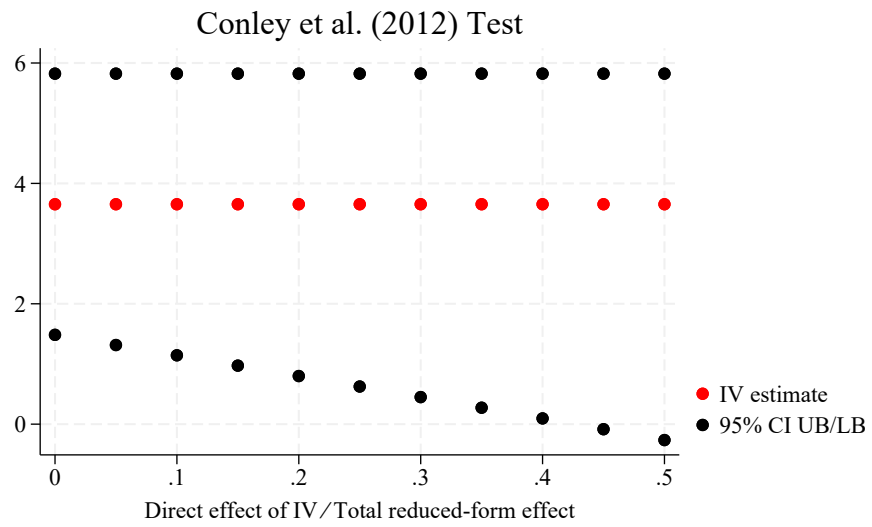
**Figure A12.** Distributions of Labor Unrest Rates: 2011–2019

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 A13.** Permutation Test for the Differential Effect by Patience Levels

Note:



**Figure A14.** Conley et al. (2012) Test

Note: This figure reports the test for IV excludability proposed by Conley et al. (2012). The test

## Appendix G Additional Tables

**Table A15.** 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)
Mean promotion prospect	0.168	0.177	0.108
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 A16.** Hukou Reform, Origin Trade Shock, and Outmigration Rate

	Outmigration from 2010 residence			
	(1)	(2)	(3)	(4)
Reform $\times$ Post	-0.068** (0.031)	-0.066** (0.031)	-0.066** (0.031)	-0.063** (0.031)
Origin trade shock	0.014** (0.006)	0.015** (0.006)	0.021 (0.016)	0.013** (0.006)
Origin trade shock $_{t+1}$			0.009 (0.021)	
Control mean	0.130	0.130	0.130	0.129
Prefecture FE	Yes	Yes	Yes	Yes
Origin FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Polynomials	Yes	Yes	Yes	Yes
Individual covariates $\times$ Year FE		Yes	Yes	Yes
Drop prefectures w/ few obs.				Yes
Observations	58,306	58,306	58,306	56,263

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

**Table A17. Hukou Reform, Outmigration Rate, and Labor Unrest — Robustness Checks**

	Baseline		Alt. Network I		Alt. Network II		Predetermined Covariates	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\Delta$ Unrest/L	$\Delta$ Unrest/L	$\Delta$ Unrest/L	$\Delta$ Unrest/L	$\Delta$ Unrest/L	$\Delta$ Unrest/L	$\Delta$ Unrest/L	$\Delta$ Unrest/L
Reform	-1.419*** (0.370)	-1.033*** (0.383)	-1.419*** (0.370)	-0.878** (0.439)	-1.388*** (0.372)	-0.901** (0.412)	-1.038*** (0.356)	-0.890** (0.365)
$\Delta$ Outmigration		3.654*** (1.018)		5.113** (2.275)		4.392*** (1.539)		2.031* (1.080)
Polynomials	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Network measured in 2015 census	Yes	Yes					Yes	Yes
Network measured in 2010 census			Yes	Yes				
Network measured in 2015 census, only >300km origins					Yes	Yes		
Predetermined covariates							Yes	Yes
% Total effect explained		0.273		0.381		0.351		0.143
1st stage coef.		0.049		0.082		0.019		0.045
Effective <i>F</i> stat.		60.978		9.400		18.870		40.806
Observations	287	287	287	287	283	283	279	279

Note: This table reports causal mediation analysis that quantifies the importance of the retention mechanism, as captured by the outmigration rate. Columns (1) and (2) represent the conventional approach. Columns (3)-(4) use the IV-augmented approach. The effective *F* statistic is calculated following [Olea and Pflueger \(2013\)](#). *IF* 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.

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

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