The Political Economy of State Employment and Instability in China*

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Abstract

State-owned enterprises (SOEs) continue to account for a large part of the Chinese economy, even though they are substantially less productive than their private counterparts. I propose that one reason for SOEs' persistence is their role in promoting social stability through job provision. I document that SOEs prevent widespread unemployment by hiring during natural disasters and periods of poor export demand. Motivated by these patterns, I build a model of unrest behavior and employment which predicts that SOE employment should increase in response to destabilizing shocks. I test model predictions with a novel natural experiment using variation in an ethnic conflict in China's Xinjiang province between the Uyghur minority and the government. I use a triple difference strategy for identification: unrest threat is higher following years with many conflict incidents in Xinjiang, in counties outside Xinjiang with a high share of Uyghur residents, and among minority men, the demographic most likely to participate in ethnic unrest. Using a combination of representative household data and original conflict data, which I construct from archival sources, I show that SOE employment of male minorities differentially increases in response to ethnic unrest threat. Male minority wages also rise, which cannot be explained by a labor supply shift or increased prejudice in private hiring alone. Additionally, relief transfers to male minorities increase, but mostly to the non-employed, indicating that SOE employment and relief transfers are complementary stability policies. Quantifying SOE favoritism of male minority employees through the model implies that SOEs implicitly receive a 26% subsidy on male minority wages.

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1 Introduction

All governments face threats to stability. To retain power, governments respond with a range of policy tools, like repression, information manipulation, and economic instruments like transfers, social insurance, and employment programs. These policies aim to counteract or pre-empt destabilizing activities in affected populations by providing income, occupying time, or threatening punishment. To the extent that these policies manipulate the economic incentives faced by individuals and firms, they may have implications for aggregate productivity and, therefore, for growth and development.

In this paper, I argue that the largest autocracy in the world – China – uses state employment to maintain social stability. I develop and test a model in which the government subsidizes state-owned enterprise (SOE) employment to prevent unrest participation. A key theoretical insight is that SOE employment should increase in response to unrest shocks, particularly for demographic groups most likely to participate. I test model predictions using novel ethnic unrest variation in China's Xinjiang province on a dataset that combines existing large-scale household data with a new dataset of Xinjiang conflict incidents, which I code from archival sources. In line with model predictions, when unrest threat is high, SOE employment increases for male minorities, the same demographic most likely to participate in unrest activities. Male minority wages also rise, which cannot be explained by a labor supply shift or increased prejudice in private hiring alone. Additionally, relief transfers to male minorities increase, especially among the non-employed, indicating that SOE employment is part of a suite of complementary stabilization policies.

This evidence answers a central question regarding the modern Chinese economy: what can explain the persistence of SOEs? A large literature documents that SOEs are 20-50% less productive than private firms, yet they receive preferential access to capital, land, and other inputs (Brandt et al., 2012; Song et al., 2011; Dong and Putterman, 2003; Jefferson et al., 2000; Chen et al., 2018; Cull and Xu, 2003; Liu et al., 2011; Gang and Hope, 2013). Privatization must be feasible, since the government has successfully implemented partial SOE reforms in the past (Hsieh and Song, 2015). These reforms also appear desirable, as they are associated with higher productivity and dovetail with the government's preoccupation with economic output and growth. China also faces mounting international pressure to pursue SOE reform due to their unfair trade practices (Anuar, 2018). Despite all these forces, SOEs employed 15% of China's urban labor force in 2017 and privatization has stalled for over a decade (The National Bureau of Statistics, 2015; Economist, 2017). This paper provides one explanation: SOEs provide compensating political benefits to the government in the form of social stability. The low productivity of SOEs may be justified, or perhaps even generated, by their policy responsibilities. Some of the SOE behavior documented in this paper is consistent with the provision of a general social safety net, but, critically, I also provide evidence of explicitly political motives by showing that SOEs respond to ethnic unrest.

I start by presenting three empirical patterns consistent with a stability role for Chinese SOEs. First, SOEs employ a higher proportion of men and male minorities than private firms, even conditioning on observable characteristics like education, age, and industry of employment. These demographic groups are the most likely to participate in destabilizing behaviors within China (Congressional-Executive Commission on China, 2019; Rosenzweig, 2010). Second, employment in private firms falls in times and places with poor export demand, while employment in SOEs increases. Third, while private firms shed labor in the year following a flood disaster, SOEs hire more labor. These patterns show that SOE employment targets demographics most likely to participate in destabilizing activities and counterbalance negative shocks that may otherwise trigger widespread unemployment, a major source of instability.

Motivated by these facts, I develop a model of SOE stabilization by embedding a government with multidimensional preferences for output and stability into a general equilibrium framework. In this setup, there are two types of firms, private and SOEs, as well as two types of individuals, an "unrest-prone" type and a "non-unrest" type. When unrest-prone individuals are not employed, they participate in activities that decrease stability. To counteract this force, the government can choose to subsidize SOE employment of the unrest-prone worker type to boost employment and stability, but at a cost to output. The subsidy is funded by a tax on non-unrest type workers type in both firms, as taxing unrest-prone types in either firm would harm stability.

The model produces three empirically-testable comparative statics. First, when society is subject to a shock that increases the threat of unrest, the model predicts that SOEs should differentially hire more unrest-prone workers. Second, private firms should hire fewer of that same group. Finally, the wages of the unrest-prone workers should increase, a consequence of the fact that the increase in SOE demand for their labor outweighs all other wage forces. The model also enables quantification of the SOE labor subsidy. The SOE should hire more unrest-prone workers, and the ratio of the unrest-prone worker share in SOEs versus private firms is a function of the SOE subsidy. This ratio is an empirically-estimable sufficient statistic, and it captures how far below market the effective SOE wages for unrest-prone workers are.

I test model predictions and quantify the subsidy using an original dataset of conflict events and China's Urban Household Survey (UHS), 2002-2009[1]. Isolating the causal effect of unrest on SOE employment is complicated by reverse causality and omitted variables: employment may directly affect unrest, or some unobserved factor may alter both simultaneously. Dramatic changes to China's economy during the period of study provide ample sources of omitted variables. To address these problems, I devise a triple-differences strategy. To address reverse causality, I use variation in the threat of ethnic unrest generated by conflict elsewhere. And by comparing the differential response of male minorities, the most unrest-prone group, with the general population, I difference out omitted variables that affect both groups equally.

My unrest shock arises from an ongoing ethnic conflict in Xinjiang, China's westernmost province. There, members of the Uyghur Muslim ethnic minority have been fighting for independence, citing discriminatory and oppressive policies. Over 85% of participants in the conflict are male minorities (Congressional-Executive Commission on China, 2019). I construct a measure of the degree to which conflict in Xinjiang spills over and generates threats of unrest for counties in other Chinese provinces. This measure is high in years preceded by many Xinjiang unrest incidents, in non-Xinjiang counties with large Uyghur population shares. I omit Xinjiang from the analysis sample because local conflict intensity and the local labor market are likely influenced by each other and common unobservable factors.

I estimate the differential response to the threat of unrest of male minority SOE employment, private employment, and wages relative to those in the general population. The comparison between male minorities and the general population is essential. It addresses the plethora of ownership-specific reforms, fiscal programs, trade agreements, and other omitted variables that may covary with the county-year unrest shock and employment outcomes. As long as these forces affect male minorities and the general population equally, I can interpret the differential response of male minorities as causal. In line with model predictions, I find that male minority SOE employment increases in response to the unrest shock, while private employment decreases. Male minority wages

[1] These are the only years for which ethnic minority information is available in the UHS.

increase as well. The size of the SOE employment response at the mean value of the unrest shock corresponds to a 0.48 percentage-point increase in the probability of SOE employment, on a mean of 55%.

These key results are highly robust to additional controls, alternative specifications, and changes in conflict incident coding rules. For example, to address sector-specific shocks that may be correlated with ownership, male minority work, and county-specific industry composition, I control for county-specific sector shares interacted with year and demographic fixed effects. To address the possibility that Xinjiang incidents may be triggered by economic shocks or events outside of Xinjiang, I use qualitative evidence to code the proximate trigger for each Xinjiang conflict incident and repeat my baseline using two alternative conflict measures. The first omits all incidents triggered by events outside Xinjiang, and the second omits all incidents triggered by economic shocks. As a placebo test, I show that none of the baseline coefficients are precisely different from zero if I use the lead, rather than the lag, of conflict incidents. Furthermore, I perform a random permutation test by creating counterfactual Uyghur population distributions and show that my baseline coefficients are larger than 92% of coefficients computed using counterfactual Uyghur population data.

I further enrich the baseline results by testing whether the government uses other policies in conjunction with SOE employment to address the threat of ethnic unrest. I find that ad hoc social relief transfers also increase in response to the Xinjiang unrest shock – but only for male minorities. Notably, unrest transfers to non-employed male minorities are over ten times larger than those to employed male minorities, which strongly suggests that relief transfers are a complementary policy to state employment in a broad-based government effort to preserve stability.

Finally, I use the model's sufficient statistic for the SOE labor subsidy to find that Chinese SOEs implicitly subsidize male minority employment by 26%. This value is large but not unprecedented relative to targeted wage subsidies in other contexts. In the late 1990s, Belgium implemented payroll tax subsidies, called "Maribel subsidies", whose magnitude remained below 3% of gross worker income (Goos and Konings, 2007). France implemented a similar program around the same time, subsidizing payroll taxes by about 5% of worker incomes (Kramarz and Philippon, 2001). In 2006, Finland implemented a subsidy for payroll taxes that represented approximately 16% of gross worker income. The program targeted older, full-time, low-wage workers (Huttunen et al., 2013). In the mid-2000s, Hungary implemented a payroll tax subsidies for firms that hired workers out of long-term unemployment. The subsidies began at 25% for the first year of employment and declined to 15% for the worker's second year (Cseres-Gergely et al., 2015).

Within the model, the male minority subsidy strictly decreases welfare, because individuals value only consumption and leisure, and the subsidy hurts output productivity by distorting prices. However, if citizens were to value stability or employment security, the government's usage of state employment would benefit citizens as well. The overall welfare effect of the program would depend on citizens' relative preferences for stability, consumption, and leisure.

The central theory of this paper is that Chinese SOEs are policy tools for social stability. Bai et al. (2006) also posit that patterns in SOE reform can be partially explained by the government's desire for stability, Leutert (2016) interprets qualitative evidence as consistent with SOE policy burdens, Dong and Putterman (2003) reason that SOE input patterns are consistent with an SOE policy role, and Lin et al. (1998) document explicit policy directives to SOEs relating to stability. A complementary work, Zeng (2017) posits that SOEs are easier to regulate and that they persist because the government wishes to maintain regulatory control over the economy, in part to preserve economic stability. I discuss additional studies of Chinese SOEs in Section 2. My paper is the first in this literature to provide causal evidence of politically-motivated SOE stabilization. I also document several new patterns of SOE behavior that can be explained by my theory.

Additionally, I contribute to the literature on autocratic governance and control. Social stability is particularly essential to autocratic regimes (Gehlbach et al., 2016; Svolik, 2012). While democratic politicians survive by winning elections, autocracies survive by maintaining control of the populace without potentially useful democratic means of preference aggregation and contract enforcement (Svolik, 2012). Scholars have modeled authoritarian dynamics, including regimes' inability to make credible commitments (Acemoglu et al., 2008), their lack of leader accountability (i Miquel et al., 2007), and the link between citizens' uncertainty over leader actions and power consolidation (Svolik, 2009). A subset of this literature has theorized and documented how autocracies use policy to maintain control. One strategy is violence: governments can exile or kill opposition to secure control (Gregory et al., 2011). However, repression has potentially large downsides, like increasing the risk of military coup (Acemoglu et al., 2010; Svolik, 2013) or increasing the signal value of protests that do take place (Kricheli et al., 2011). Another strategy is information manipulation: regimes can change information content or access to influence citizen beliefs (Gehlbach et al., 2016; Shadmehr and Bernhardt, 2015; Guriev and Treisman, 2015), though governments may have difficulty adapting to rapid changes in information technologies like social media (Qin et al., 2019). One of my contributions is to show that, in addition to these instruments of control, autocracies use targeted state employment to maintain stability, and thus, stay in power. While employment provides unique benefits relative to other interventions, which I discuss in Section 2, it also directly affects aggregate productivity. My paper is the first to empirically document the tradeoff between firm productivity and stability motives.

I also contribute to a literature on fiscal spending and political business cycles (surveyed in Drazen (2000)). Several theoretical papers posit that politicians use government spending to increase the chances of re-election or to consolidate support more generally (Nordhaus, 1975; Rogoff, 1987). On the empirical side, Akhmedov and Zhuravskaya (2004) find that public spending shifts toward transfers to voters before regional elections in Russia, Veiga and Veiga (2007) find that local governments in Portugal increase total expenditures and shift them toward visible goods prior to elections, Shi and Svensson (2006) find that across countries fiscal deficits increase during election years, and Drazen and Eslava (2010) find an increase in voter-targeted expenditures prior to elections in Colombian municipalities. This paper demonstrates that even in regimes without elections, other political concerns like social stablity can generate cyclical patterns in government spending. Additionally, I demonstrate that state employment can be a politically-motivated fiscal intervention, like monetary transfers or infrastructure.

2 Motivation

In this section, I briefly present the recent history of Chinese SOEs, focusing on their productivity and reform. Then, I discuss primary evidence that the Chinese government deeply values both economic output and stability. Finally, I explore alternative stability policies used within China and throughout the world, assessing their efficacy vis-à-vis state employment.

A body of work has established that SOEs are 20-50% less productive than their private counterparts (Song et al., 2011; Dong and Putterman, 2003; Jefferson et al., 2000), and thus greatly decrease the aggregate productivity of the Chinese economy[2]. This fact has shaped the current

[2] I corroborate these results using multiple productivity estimation techniques in Appendix Subsection .1.

consensus view of SOEs: that they are inefficient behemoths, recipients of undue government favoritism, and in need for further reform and curtailment. Voices from academia, policy circles, and the media have urged China to “remove the policy burdens of SOEs” (Lin et al., 1998), “use market criteria, not administrative criteria, to measure [SOE] performance” (Li and Xia, 2008), and “[cut] state firms down to size and [open] them up to competition” (Economist, 2017).

Yet, in the last half-century, a key policy priority of the Chinese government has been economic growth, which at times has bordered on obsession. Deng Xiaoping, leader of China from 1978 to 1989, stated: “According to Marxism, communist society is a society in which there is overwhelming material abundance. Socialism is the first stage of communism; it means expanding the productive forces” (Chang, 1996). In 1987, the Party’s motto for the 13th National Congress was “one central task, two basic points”; the central task was economic development (Jiang, 1997). Gao Shangquan, member of the National Consultative Conference from 1998 - 2003, put it thus: “to constantly improve people’s living standard... [t]his is the starting point and ultimate objective of all our work” (People’s Daily, 2001). China is also one of a few countries, and by far the largest, to maintain a GDP target (Economist, 2016), a symbol of the government’s devotion to economic growth.

SOE reform and the government’s stated goal of economic growth appear perfectly aligned. With no further information, one might expect the Chinese government to ardently pursue SOE privatization.[3] The government did appear genuinely committed to SOE reform in its early years. During the 15th Party Congress in 1997, state ownership was downgraded from a “principal” component of the economy to a “pillar” of the economy, and a push to privatize SOEs began in earnest (Qian, 2000). Then, in 1999, the Communist Party Central Committee announced that SOE reforms would follow the principle of “[g]rasping the large, letting go of the small” (Hsieh and Song, 2015). But reforms stalled in the subsequent years. Figure 1 demonstrates vividly the deceleration of reform. Urban SOE employment decreased markedly for a few years following 1997, but since 2006 has remained stagnant at approximately 70 million people, comparable to the entire population of France. Why is the Chinese government, preoccupied as it is with economic growth,

[3]It appears that Marxist or Maoist ideology is not a binding constraint, given the dramatic economic reforms that have already taken place since 1979. These reforms profoundly reshaped nearly every facet of economic life, including agriculture (Yao, 2016), banking (Dobson and Kashyap, 2006), trade (Lardy, 1993), and manufacturing (Huang, 2003).

so reluctant to engage in further SOE rollbacks? This paper argues that SOEs persist because they offer an essential political benefit: social stability.

Why might SOEs be useful policy instruments despite potentially large efficiency costs? I now discuss a number of properties of state employment that offer particular advantages for stability preservation, and when appropriate, I contrast these properties with those of leading alternative policies available to the Chinese government.

One channel through which SOE employment may decrease social instability is by providing a wage income, which increases the opportunity cost of instability participation to the extent that employees would need to give up, or put into jeopardy, this income stream in order to protest or rebel (Becker, 1968; Popkin, 1979). Previous work has established the pacifying role of wage income in numerous contexts: Bazzi and Blattman (2014) find that income from commodity shocks appear to reduce individual incentives to fight in wars, Dube and Vargas (2013) find that decreases in the price of labor-intensive coffee increases civil war violence in Colombia, and Fetzer (2014) finds that India's public employment program uncoupled productivity shocks from conflict. These results suggest that returns to labor income have a pacifying effect.

Another way to increase the opportunity cost of conflict would be to simply to give citizens a transfer conditioned on peaceful behavior. Depending on how transfers are funded, they could potentially avoid SOE-related distortions. Yet the observed extent of transfer programs in China is dwarfed by the reach of SOE employment. For example, the primary welfare transfer program, the *Dibao*, reaches only 5.5% of China's population (Gao et al., 2015). Unemployment insurance is paid out to less than 1% of the working population. And relief transfers, which are ad hoc transfers largely directed by local governments, are disbursed to just 1.6% of individuals in the Urban Household Survey (2002-2009). Why doesn't the Chinese government rely more, or rely exclusively, on transfers to ensure social stability?

The first reason is that targeted transfer programs are susceptible to fraud. In one survey of unemployment insurance recipients in Liaoning, 80% of recipients possessed disqualifying alternative sources of income, typically from unreported employment (Vodopivec et al., 2008). Moreover, some evidence suggests that mis-targeted transfers can actually increase social instability. Cameron and Shah (2013) found that a highly mis-targeted transfer program in Indonesia increased protests, economic crimes, and violent crimes. Verifying eligibility for transfers is therefore critical, but also difficult: for example, the correct targeting of unemployment-conditional transfers requires the government to know all sources of a person's income. In contrast, verifying compliance with state employment only requires information readily available to SOE managers, like worker attendance and output.

Additionally, employees who receive income and other transfers through state positions may appear to deserve these benefits, as they have been earned through work. Transfers may generate political audience costs, especially given the demographic groups most likely to participate in destabilizing behavior in China. The only publicly-available data set on Chinese political prisoners is collected by the United States Congressional-Executive Commission on China. The demographic breakdown of this data set suggests that 72.2% of Chinese dissidents are male and 74.5% of the male dissidents are between 20 and 50 years old. Chinese society may consider able-bodied men particularly undeserving of government handouts, given that they are capable of working. Indeed, only 25% of Chinese welfare recipients are working-age men, while male SOE employment share is well over 50% (Gao et al., 2015).

Employment programs have demonstrated promising pacifying effects in other contexts. Heller (2014) finds evidence that summer jobs for youth in the United States decreases participation in violent activity. Blattman and Annan (2010) find that participation in an employment program in Liberia decreases the likelihood that individuals participate in illicit activities and serve as mercenaries in a local conflict. A simple property of employment, in that it enters the time constraint, may be responsible; employment may also engender a variety of social and psychological changes. In this vein, recent work suggests that attitudes toward the government and democratization change when one's main source of income is from an SOE. Chen and Lu (2011) survey middle-class individuals in China regarding their attitudes toward democracy and find that SOE employment is strongly negatively correlated with support for democratization (Vodopivec et al., 2008).

State employment also provides the government an alternative to armed force. The Chinese government has used this strategy to quell protests, including the student-led demonstrations in Beijing in the spring of 1989. Recent instability events have also been addressed with police action, including protests against land seizures in Dongzhou in 2004, anti-corruption protests in Guangdong in 2011, and anti-government protests in Hong Kong in summer 2019 (Ma and Cheng, 2019; Wright, 2019). These demonstrate the downsides of armed suppression: political backlash and a lack of long-term effectiveness. The Tiananmen protest led to widespread domestic and international discontent, including sanctions and arms embargoes. And in both the Dongzhou and Guangdong protests, once the police presence decreased, protests resumed. The Hong Kong protests have not resolved yet, but China has already suffered in international standing as a result (Roantree, 2019).

While the Chinese government clearly employs many policy tools to secure domestic tranquility, state employment has a unique set of stabilizing properties that are not provided via other interventions, like direct transfers or armed suppression. These advantages include enforceability, targeting precision, lower audience costs, and the inculcation of loyalty. From the perspective of the government, these advantages may plausibly outweigh the corresponding efficiency costs.

3 Empirical Patterns in SOE Employment

In this section, I present three new facts about SOE employment in China that suggest these firms increase social stability. First, I show that SOEs disproportionately hire males and male minorities, and two groups that participate most in unrest in China. Second, I show that SOEs hire countercyclically to export demand, whereas private firms hire procyclically. Third, I show that, after natural disasters in the form of river floods, private firms fire labor but SOEs hire.

The first fact suggests that SOEs employment is concentrated in groups most likely to decrease stability, and the latter two facts suggest that SOEs hire labor during bad shocks to prevent widespread unemployment. While these patterns are all consistent with a stability motive, I acknowledge that they could be explained by alternative hypotheses. Therefore, motivated by these facts, I proceed to develop and test a model of SOE stabilization in Sections 4 through 7. I argue that the evidence presented in these latter sections, when taken in conjunction with the following three facts, present a preponderance of evidence of an SOE stability motive.

3.1 Demographics of SOE Employment and Unrest Participation

In China, the demographics of unrest participation differ from those of the general population. I show this fact using two datasets. I obtain the breakdown of China's total population from the 2000 Census (The National Bureau of Statistics, 2010a). Then, I use a dataset of all known

Chinese political prisoners to obtain the demographic composition of unrest participants in China (Congressional-Executive Commission on China, 2019). These data are collected by the United States Congressional-Executive Committee on China in conjunction with U.S. intelligence forces and contain the name, gender, ethnicity, and age of political prisoners in China.

Figure 2 plots the composition of these two groups by gender and minority. A few stark patterns emerge from this figure. Men comprise over 70% of unrest participants in China, while they are approximately 51% of the general population. Minority men are even more dramatically overrepresented: they are just 4% of the general population, yet they represent over 45% of unrest participants.

If SOEs perform a stabilizing role, they may focus their employment on the groups most likely to participate in unrest: men and minority men. To test this conjecture, I use data from the Urban Household Survey (UHS), a representative cross-sectional survey, a source I describe in detail in Subsection 6.2. On the left-hand chart in Figure 3, I plot the average proportion of men in private firms and the average proportion of men in SOEs. The dark blue bracket at the top of the SOE column indicates the standard error of the difference between the two columns; the difference is precise at the $p < 0.01$ level. Clearly, SOEs hire disproportionately more men than do private firms. On the right-hand chart of Figure 3, I repeat this process for male minorities. Analogously, I find that SOEs hire more male minorities than private firms. The difference in the proportion of male minorities between the firms is precise at the $p < 0.01$ level.

3.2 Export Demand

One determinant of firm employment is product demand. In general, decreasing demand will place downward pressure on the output price of a firm, which should decrease the equilibrium firm output and inputs, including employment. In this section, I show that Chinese private firms behave in exactly this matter in response to demand for Chinese exports, but SOEs exhibit the opposite patterns. In particular, I show that SOEs appear to buffer employment from demand fluctuations, hiring more when export demand is low.

Inspired by the import supply shock used in Autor et al. (2013), I create an analogous measure of export demand for Chinese products. Campante et al. (2019) use a similar setup to estimate how trade shocks affect Chinese labor strikes. The annual provincial demand shock exposure, DSE_{pt}, is constructed by multiplying two components: a weight variable and a trade flow variable. The trade flow variable E^a_{st} represents the dollar value of net Chinese exports (exports minus imports) to five of its largest trading partners by sector and year. In order to assign the aggregate flow of net exports to different regions of China, I multiply this aggregate net export flow with a weight variable, $\frac{X_{spt-1}}{X_{st-1}}$, that equals the ratio of all exports in a given sector, year and province by the aggregate net export flow out of China in the same sector and year. In effect, this weight measure assigns a greater shock intensity to provinces that export more (on net) in a given sector and year. I use one-year lagged province trade shares because, to the extent that contemporaneous export composition in a region is affected by anticipated trade changes, the use of lagged exports will mitigate simultaneity bias.

$$\Delta DSE_{pt} = \sum_s \left[\frac{X_{spt-1}}{X_{st-1}} \sum_{a \in A} \Delta E^a_{st} \right] \tag{1}$$

The letter s indexes sectors. Provinces are indexed with p and years are indexed with t. Lowercase a represent elements of the set A, which is is the set of China's five largest trading partners in 2004: United States, Japan, South Korea, Germany, and the Netherlands[4].

The geographic variation in equation 1 arises entirely from variation in the export structure by sector over provinces during period $t-1$. This variation arises from differential concentration of firms by sector in different provinces as well as the extent to which firms within provinces export their products. One key challenge to a specification that uses employment outcomes in a regression on ΔDSE_{pt} is that realized net export flows from China may be correlated with supply-side changes in China itself. If those supply-side changes are themselves correlated with employment quantities and composition, then the OLS regression of employment ownership indicators on ΔDSE_{pt} may have a bias of unknown sign. Therefore, I should be wary of interpreting the coefficient on ΔDSE_{pt} from such a regression as the response of employment to changes in export demand.

To address this concern, I use a related, but different proxy for export demand, in order to address the potential endogeneity of Chinese employment composition to export supply movements within China. To identify the component of export flows from China due to changes in partner demand for Chinese exports, I instrument for Chinese net export flows to its five largest partners

[4]This list represents the five largest purchasers of Chinese exports in 2004, a representative year from my sample. My results are robust to using the ranking of countries in alternative years.

using the contemporaneous composition and growth of partner inputs from their own partner countries. This measure is intended to capture the component of demand for China's exports that is due to changes in demand from China's largest partners rather than changes within China itself. The new variable, demand shock exposure IV, $DSEIV_{pt}$, is defined in equation 2.

$$\Delta DSEIV_{pt} = \sum_s \left[\frac{X_{spt-1}}{X_{st-1}} \sum_{a \in A} \sum_{b \in B} \Delta E^{ab}_{st} \right] \tag{2}$$

The letter s indexes sectors. Provinces are indexed with p and years are indexed with t. Lowercase a and b represent elements of the sets A and B, respectively. A is the set of China's five largest trading partners in 2004 and B is the set of each partner a's five largest trading partners in 2004, excluding China. The objects ΔE^{ab}_{st} represent the net exports (exports minus imports) into China's trading partner a from the partner's own largest trading partners, $b \in B$.

I obtain changes in net export flows ΔE^{a}_{st} and ΔE^{ab}_{st} from the *United Nations Comtrade Database* (United Nations, 2016). These data measure the trade flow in current dollar values between countries at the annual level. I deflate trade values to constant 2009 dollars using GDP deflators from the Bureau of Economic Analysis (Bureau of Economic Analysis, 2016). The current temporal coverage of Comtrade is 1962 to 2018 and it reports sectors using Harmonized System (HS) codes. I construct the weight variable $\frac{Y_{spt-1}}{Y_{st-1}}$ using Chinese data from the *Annual Surveys of Industrial Production* (ASIP)[5]. This dataset covers the years 1998 - 2013 and reports sectors using the Chinese Industrial Code system. In order to combine data from Comtrade with constructed weights from ASIP, I create a crosswalk between the two sector classification systems by hand.

The main regression I use to estimate the response of employment categories to the demand shock exposure measure is presented in equation 3. The outcome variable and additional controls come from individual-level data from the Urban Household Survey (UHS), which I describe in detail in Subsection 6.2.

$$\begin{aligned} Y_{ict} = {} & \alpha + \beta \Delta DSEIV_{pt} + \gamma Age_i + \delta Edu_i + \zeta Male_i \\ & + \delta_M Edu_i \times \text{Male}_i + \gamma_M Age_i \times \text{Male}_i \\ & + \tau_t + \eta_c + \epsilon_{ict} \end{aligned} \tag{3}$$

In this equation, i indexes individuals, p indexes provinces, c indexes counties, and t indexes

[5] I describe this dataset in detail in Section 6.

years. I estimate this regression using all individuals in the Urban Household Survey between the ages of 22 and 55.[6] The four dependent variables, Y_{ict}, relate to the individual's employment status. The variable $SOE\,Empl_{ict}$ is an indicator for SOE employment, which takes a value of 1 when the UHS employment variable reports an individual as working in a state-owned economic unit. The variable $Private\,Empl_{ict}$ similarly takes a value of 1 if an individual is employed in a privately-owned economic unit, and zero otherwise. The variable $NonEmpl_{ict}$ takes a value of 1 if an individual is not employed.[7]

This specification includes year fixed effects τ_t, county fixed effects η_c, and individual characteristics: age, a fixed effect for education level, as well as age and education interacted with gender. These effects will absorb any persistent differences in individual employment status due to age, education, gender, or differential effects of age and education by gender. Because the demand shock varies at the province and year level, I cluster standard errors at the province and year level.

I present the results of the three baseline regressions in Table 1. Column (1) shows that SOE employment response inversely to trade demand. I find a coefficient of -0.0529 that is precise at the $p < 0.01$ level. On the other hand, column (2) shows that private firms respond procycically with trade demand, with a coefficient of 0.0546, precise at the $p < 0.05$ level. These results suggest that SOEs are behaving in a way that does not maximize profits, but instead provides employment security during downturns.

However, there are some caveats to this analysis. For example, SOEs and private firms may be concentrated in different sectors that are in turn differentially exposed to trade. If private firms are more exposed to export fluctuations than SOEs, they may behave pro-cyclically with respect to trade demand. On the other hand, if SOEs did not export at all, their counter-cyclical behavior could be due to weakened competition from private firms during periods with poor trade demand. In order to address this concern, I control for base-year sector composition by county interacted with year fixed effects and report the results in Appendix Table A.13. The results are highly robust to these additional controls.

Additionally, the trade shock assumes that the export demand of China's trading partners for goods from their non-China trading partners is independent of the determinants of Chinese

[6] I discuss this dataset in detail in Section 6

[7] Because this category includes people who are not actively searching for a job, it differs from standard definitions of unemployment. For example, this category includes individuals of working age who are engaged in home production.

employment by ownership. However, because China represents a large share of world trade, it seems plausible that there may be omitted variables that determine both Chinese employment composition by ownership as well as my instrument for partner trade demand. To test whether China's influence on world trade generates spurious results, I re-construct the main trade shock $\Delta DSEIV_{pt}$ using only sectors in which China represents less that 5% of global trade flows. The results of re-running the main specification using this new measure are reported in Appendix Table A.14. The results are highly robust to this alternative measure.

To further increase confidence that these results are not elicited by spurious trends, I re-estimate Equation 3 using the lead of the export demand shock. I argue that it is less likely that employment should respond to future demand changes.The results from these regressions are reported in Appendix Table A.15. I find that employment composition by ownership does not change in response to future demand shocks.

Finally, I investigate whether the trade shock is more pronounced for either males or male minorities. In Appendix Tables A.16 and A.17, I report the baseline results estimated separately by gender and by male minority status. I find that males and females do not respond differentially to demand shocks. Male minority employment, however, is slightly more sensitive to the trade shock in both private firms and SOEs.

3.3 Flood Disasters

Natural disasters are also shocks to the economic environment of firms. One of the most common and damaging natural disasters in China is flooding, particularly riverine flooding (Shi, 2016). Such disasters may affect firms through numerous channels: by eroding infrastructure, depressing local demand, and more. However, in the short run, natural disasters are generally harmful for firms (Cavallo and Noy, 2009), and profit-maximizing firms tend to react by producing less output and demanding fewer inputs, like labor.

In this subsection, I show that private firms in China shed labor if their county is hit by a disaster-level riverine flood the prior year. I also document that SOE employment exhibits the exact opposite pattern. I demonstrate these patterns by running the following regression. The outcome variable and controls come from individual-level data from the Urban Household Survey

(UHS), which I describe in detail in Subsection 6.2.

$$\begin{aligned} Y_{ict} = \alpha &+ \beta \Delta Flood_{ct-1} + \gamma Age_i + \delta Edu_i + \zeta Male_i \\ &+ \delta_M Edu_i \times \text{Male}_i + \gamma_M Age_i \times \text{Male}_i \\ &+ \tau_t + \eta_c + \epsilon_{ict} \end{aligned} \tag{4}$$

In this equation, i indexes individuals, c indexes counties, and t indexes years. I estimate this regression using all individuals in the Urban Household Survey between the ages of 22 and 55[8]. The dependent variables, Y_{ipt}, are defined exactly the same as in Subsection 3.2. Just as in the export demand shock specification, this specification includes year fixed effects τ_t, county fixed effects η_c, and interactions of a vector of individual-level characteristics $\boldsymbol{X_i}$. In the vector $\boldsymbol{X_i}$ are age, a fixed effect for education level, as well as each of these controls interacted with gender. I cluster the standard errors at the county and year level, which is the level at which floods are observed.

I obtain data on riverine flooding from the Dartmouth Flood Observatory's *Global Active Archive of Large Flood Events* (Brakenridge, 2019), which uses news reports as well as governmental, instrumental, and remote sensing sources to locate floods. The flood data cover the years 1990 to 2017 and include the latitude and longitude of each flood's centroid. From these data, I generate a county-level riverine flooding indicator, $Flood_{ct-1}$, that equals one if the county geographic centroid is within 50 kilometers of the centroid of a recorded flood in the past year. For the period 1990-2017, 889 county-years are defined to suffer riverine flooding according to my definition. This value represents 1.12% of the total county-years in the data. I use the flood indicator in year $t-1$ because I assume that the adjustment of employment in response to natural disasters may take some time.

I present the baseline flood regressions in Table 2. I find that SOE employment increases in the year after floods, since the coefficient in column (1) is 0.0778 and precise at the $p < 0.05$ level. On the other hand, column (2) shows that private employment falls after flood disasters, with a coefficient of -0.093, precise at the $p < 0.01$ level.

There may be omitted variables that co-vary with county-year flood incidence as well as employment composition by ownership. For example, SOEs could be concentrated in sectors that happen

[8] I discuss this dataset in detail in Section 6

to be differentially less affected by floods, or perhaps even in sectors for which floods generate demand increases, like construction. To address this possibility, I further control for the base year sector share of each county interacted with year fixed effects. Appendix Table A.18 reports the results from when these controls are added to the baseline regressions. The same pattern is present and precisely estimated.

I also conduct a placebo check by re-estimating Equation 4 using the lead of the flood indicator variable. If I found similar firm responses to the lead of the variable, I would be especially concerned that omitted variables drive the observed patterns. The results from these regressions are reported in Appendix Table A.19. I find that employment composition by ownership does not change in response to future floods.

In Appendix Tables A.20 and A.21, I investigate whether the flood effect is heterogeneous by gender or male minority status. I find that the counter-cyclical behavior of SOE employment is much more precise for men, but male employment is also more sensitive to floods. A similar pattern holds for male minorities specifcially.

Finally, I estimate four county-level regressions to understand how floods affect production in the aggregate. Appendix Table A.22 reports these results. I regress county-year data on GDP, primary GDP, secondary GDP, and total exports onto the flood indicator as well as county and year fixed effects. I find that all of the point estimates are negative, but the only precisely estimated coefficient is that of primary GDP in column (2), which suggests that agricultural and extractive output are directly harmed by flood disasters.

4 Conceptual Framework

This economy consists of two types of individuals: $\mathcal{N}^U$ identical unrest-type individuals, indicated by superscript U, and $\mathcal{N}^N$ identical non-unrest-type individuals, indicated by superscript N. Let there also be many identical private firms, many identical SOEs, and a single government. Let the price of the consumer good be the numeraire.

4.1 Individuals

Since individuals are identical within each type, N and U, the behavior of each type can be expressed via those of a representative consumer. Because the discussion below applies equally to both consumers, I will use an index $j \in \{N, U\}$ to cover both individual types simultaneously.

Let both representative consumers value two goods: leisure, l^j, and consumption, c^j. U-type individuals differ from N-type individuals in that they use some amount of their leisure time to engage in instability activities, Z, such that instability is an increasing function of U-type leisure, $Z = z\left(\ell^U\right)$.

Let the utility derived from leisure and consumption be expressed by $V_j = u\left(l^j, c^j\right)$, such that utility is increasing in both terms and concave in both terms: $u_i > 0$, $u_{ii} < 0$ for $i \in \left(l^j, c^j\right)$. Furthermore, let it be the case that $lim_{i\to\infty} u_i\left(l^j, c^j\right) = 0$ and $lim_{i\to 0} u_i\left(l^j, c^j\right) = \infty$ for $i \in \left(l^j, c^j\right)$.

Near the equilibrium of the economy, let the labor supply curve be upward-sloping, such that $\frac{dL_S^j}{dw^j} > 0$, and let there be a unique L_S^j associated with each w^j. In this model, the two labor types will participate in separate labor markets, so the types may not necessarily receive the same wage.

The representative consumers are endowed with time, h, and their combined leisure and individual labor supply, L, cannot exceed this value. They earn income from working and cannot spend more than they earn, such that $c^j \le w_j L^j$. Since individuals do not value income other than for consumption, this constraint will always hold with equality.

$$\begin{aligned} max_{l^j, c^j}\ u\left(l^j, c^j\right) \\ \text{s.t. } c^j = w_j L^j \\ \text{and } \ell^j + L^j = h \end{aligned}$$

The individual's problem, written as a Lagrangian, simplifies to:

$$\mathcal{L}^j = u\left(l^j, c^j\right) - \lambda^j\left(w^j h - w_j \ell^j - c\right).$$

And the first order conditions yield:

$$\frac{du}{dl^j}^* = -\lambda^j w_j \tag{5}$$

$$\frac{du}{dc^j}^* = -\lambda^j. \tag{6}$$

The equilibrium consumption bundle, (ℓ^*, c^*), of the individual must satisfy:

$$\frac{u_\ell\left(\ell^{j*}, c^{j*}\right)}{u_c\left(\ell^{j*}, c^{j*}\right)} = w_j. \tag{7}$$

4.2 Private Firms

Let there be many private firms, each of which exhibits free entry and each of which operates with constant returns to scale. Production can, therefore, be expressed with a representative firm, which itself exhibits constant returns to scale.

Let the representative private firm's production function be $Y^{priv} = F\left(U^{priv}, N^{priv}\right)$. Because F exhibits constant returns to scale, the private firm earns zero profits in equilibrium, due to Euler's theorem. Additionally, let: $F_i > 0$, $F_{ii} < 0$ for $i \in (U, N)$. Let the cross-derivative be positive, such that $F_{UN}\left(U^{priv}, N^{priv}\right) > 0$. Finally, let it be the case that $lim_{i\to\infty} F_i\left(U, N\right) = 0$ and $lim_{i\to 0} F_i\left(U, N\right) = \infty$ for $i \in (U, N)$.

The firm faces a tax on N-type labor.

The representative private firm solves:

$$max_{U,N}\ F\left(U^{priv}, N^{priv}\right) - w_U U^{priv} - \left(1 - \tau_N\right) w_N N^{priv}.$$

The private firm's first order conditions yield:

$$F_U^{priv*} = w_U \tag{8}$$

$$F_N^{priv*} = w_N\left(1 - \tau_N\right). \tag{9}$$

The equilibrium input bundle, $\left(U^{priv*}, N^{priv*}\right)$, of the private firm must satisfy:

$$\frac{F_U^{priv*}}{F_N^{priv*}} = \frac{w_U}{w_N\left(1 - \tau_N\right)}. \tag{10}$$

4.3 SOEs

Let there be many state-owned firms, each operating with constant returns to scale. Production can, therefore, be expressed with a representative firm, which itself exhibits constant returns to scale. In equilibrium, constant returns to scale imply zero profits, due to Euler's theorem. Let there be no free entry of SOEs, to mimic the tight controls on SOE entry observed in the real world.[9]

Let the representative SOE's production function be the same as that of the representative private firms, such that $Y^{soe} = F\left(U^{soe}, N^{soe}\right)$.

Like private firms, SOEs face a tax on N-type labor, but they also receive a subsidy on U-type labor.

The representative SOE solves:

$$max_{U,N}\ F(U^{soe}, N^{soe}) - w_U\left(1-\tau_U\right)U^{soe} - w_N\left(1-\tau_N\right)N^{soe}.$$

The firm's first order conditions yield:

$$F_U^{soe*} = w_U\left(1-\tau_U\right) \tag{11}$$

$$F_N^{soe*} = w_N\left(1-\tau_N\right). \tag{12}$$

The equilibrium input bundle, $\left(U^{soe*}, N^{soe*}\right)$, of the SOE must satisfy:

$$\frac{F_U^{soe*}}{F_N^{soe*}} = \frac{w_U\left(1-\tau_U\right)}{w_N\left(1-\tau_N\right)}. \tag{13}$$

4.4 The Government

Let the government maximize a combination of output and stability, S. Stability is decreasing in instability, Z, as well as an instability shock, $\xi \in \mathbb{R}^{+}$, a positive real-valued number. Suppose that stability takes the form of $S\left(-Z\right)$ and suppose that it is a continuous, increasing, and concave function, such that $S^{'}\left(\cdot\right) > 0$ and $S^{''}\left(\cdot\right) < 0$. Additionally, let $lim_{-Z\rightarrow 0}S^{'}\left(\cdot\right) = \infty$ and $lim_{-Z\rightarrow\infty}S^{'}\left(\cdot\right) = 0$.

The government cannot spend more on subsidies than it raises on taxes, and since it does not

[9] All the results of the model are unchanged if SOEs are allowed free entry.

value revenue directly, its budget constraint will hold with equality.

$$max_{\tau_U,\tau_N}\ Y^{priv} + Y^{soe} + \eta S\left(-\xi z\left(\ell^U\right)\right) \tag{14}$$
$$\text{s.t. } \tau_U w_U U^{soe} + \tau_N w_N N = 0$$

In equilibrium, the firm's optimization problem, the consumer's optimization problem, and the clearing of the goods market implicitly constrain the government's problem via their optimal policy functions. Moreover, the government's budget constraint gives $\tau_N = -\frac{\tau_U w_U U^{soe}}{w_N N}$. Therefore, I can rewrite the government's problem with only one choice variable, τ_U.

$$G^{**} = max_{\tau_U}\ Y^{priv}\left(\tau_U\right) + Y^{soe}\left(\tau_U\right) + \eta S\left(-Z\left(h - U\left(\tau_U\right)\right) - \xi\right) \tag{15}$$

The first order condition of the government is:

$$\frac{dY^{soe}}{d\tau_U} + \frac{dY^{priv}}{d\tau_U} + \eta\xi \underbrace{\frac{dS}{dZ}}_{\ominus} \underbrace{\frac{dz}{dU}}_{\ominus} \frac{dU}{d\tau_U} = 0. \tag{16}$$

In the absence of stability concerns, $\eta = 0$, the government's problem becomes:

$$G^{*} = max_{\tau_L}\ Y^{priv}\left(\tau_U\right) + Y^{soe}\left(\tau_U\right). \tag{17}$$

4.5 Market Clearing

I now briefly discuss the conditions for equilibrium in this economy. In equilibrium, equations 7, 10, and 13 must hold, each of which respectively satisfies the consumer's, private firm's, and SOE's optimization problems. Constant returns to scale in production implies that equilibrium profits must be zero for private firms and SOEs.

$$Y^{soe*} - w_U\left(1 - \tau_U\right)U^{soe*} - w_N\left(1 - \tau_N\right)N^{soe*} = 0 \tag{18}$$

$$Y^{priv*} - w_U U^{priv*} - w_N \left(1 - \tau_N\right) N^{priv*} = 0 \tag{19}$$

Additionally, the labor, capital, and consumer product markets must clear.

The **labor market for U-type** workers clears when all U-type workers receive the same wage, w_U, and the quantities equalize:

$$U = U^{soe*} + U^{priv*}.$$

The **labor market for N-type** workers clears when all N-type workers receive the same wage, w_N, and the quantities equalize:

$$N = N^{soe*} + N^{priv*}.$$

Finally, the **consumer goods market** clears when total production equals the goods consumed by individuals.

$$Y = w_U U + w_N N \tag{20}$$

Equation 20 is equivalent to $Y^{soe*} + Y^{priv*} = w_U U^{soe**} + w_U U^{priv*} + w_N N^{soe*} + w_N N^{priv*}$.

4.6 Comparative Statics

The empirically testable comparative statics of this model are the responses to firm labor choices to the instability parameter, ξ. Because this parameter only enters the government's problem, it will only affect optimal labor choices via the government's optimal choice of τ_U. Hence, in this section, I first derive the signs of key equilibrium responses to τ_U. In the interest of brevity and clarity, I focus on the intuition behind each of these results and present full proofs of each in the Online Mathematical Appendix.

$$\begin{aligned}
\text{Propositions 1.1 and 1.2} \quad & \frac{dU^*}{d\tau_U} > 0 \text{ and } \frac{dN^*}{d\tau_U} < 0 \\
\text{Propositions 2.1 and 2.2} \quad & \frac{dw_U^*}{d\tau_U} > 0 \text{ and } \frac{dw_N^*}{d\tau_U} < 0 \\
\text{Proposition 3} \quad & \frac{dU^{priv*}}{d\tau_U} < \frac{dN^{priv*}}{d\tau_U} \\
\text{Proposition 4} \quad & \frac{dU^{soe*}}{d\tau_U} > \frac{dN^{soe*}}{d\tau_U}
\end{aligned}$$

The U-type Labor Market

For this analysis, it is useful to visualize the labor markets. I begin with the U-type market. Both labor markets in this model can be understood graphically as the intersection of the labor supply and aggregate labor demand curves in $\left(L^j, w^j\right) \in \mathbb{R}^2$ space. The labor supply curve is determined by the consumer's optimization problem and given in equation 21. Note also that the labor supply curve does not change with respect to τ_U.

$$w_U = \frac{u_\ell\left(\ell^U, c^U\right)}{u_c\left(\ell^U, c^U\right)} \tag{21}$$

The aggregate labor demand curve arises from the combination of the SOE and private firms' demand for U-type workers. For fixed levels of N^S and N^P, I can solve for w_U in both firms' first order conditions.

$$w_U = \frac{F_U^{soe}\Big|_{N=\overline{N^S}}}{(1-\tau_U)} \tag{22}$$

$$w_U = F_U^{priv}\Big|_{N=\overline{N^P}} \tag{23}$$

Because the function F is continuously differentiable and everywhere has non-zero slope in both terms, by the inverse function theorem, there exist two functions g^P and g^S such that $g^{priv}\left(F_U^{priv}\Big|_{N=\overline{N^{priv}}}\right) = U^{priv}$ and $g^{soe}\left(F_U^{soe}\Big|_{N=\overline{N^{soe}}}\right) = U^{soe}$. At any given U, the slope of these expressions are the reciprocal of the derivative of $F_U^{priv}\Big|_{N=\overline{N^P}}$ and $F_U^{soe}\Big|_{N=\overline{N^S}}$, respectively, and therefore both g^{priv} and g^{soe} are downward-sloping as well.

I draw a representation of these curves in Figure 4. The equilibrium of the labor market occurs at the star, where the aggregate demand and supply curves intersect. The coordinates of this star represent the equilibrium wage and aggregate labor of the economy, (U^*, w_U^*).

How does the U-type labor market respond to an increase in τ_U?

Since τ_U does not directly enter the individual's problem, it will not shift the labor supply curve, which is upward-sloping. Additionally, the private firm's labor demand curve also does not directly respond to τ_U.

Instead, τ_U enters the first-order condition of the SOE and shifts the labor demand curve of the

SOE by changing the denominator of $\frac{1}{(1-\tau_U)} F_U^{soe}\Big|_{N=\overline{N^S}}$. Specifically, an increase in τ_U will increase the SOE's demand for U at a given price, which can be drawn as a rightward shift of the SOE's labor demand curve. I depict this change with the dotted red line marked "SOE U'" in Figure 5.

As Figure 5 shows, two immediate implications of the shift are that $U'^* > U^*$ and $w_U'^* > w_U^*$. In other words, total labor, U, and the equilibrium wage, w_U, will both increase with respect to τ_U, and these responses correspond to **Propositions 1.1 and 2.1**.

The N-type Labor Market

Just as with the U-types, the N-type labor supply curve is determined by the consumer's optimization problem and given in equation 24. Note also that the labor supply curve does not change with respect to τ_U.

$$w_U = \frac{u_\ell\left(\ell^N, c^N\right)}{u_c\left(\ell^N, c^N\right)} \tag{24}$$

Demand for each firm arises from its conditions for optimality. Specifically, for fixed levels of U^{soe*} and U^{priv*}, I can solve for w_N in both firms' first-order conditions.

$$w_N = \frac{F_N^{soe}}{(1-\tau_N)}\Bigg|_{L=L^{soe*}} \tag{25}$$

$$w_N = \frac{F_N^{priv}}{(1-\tau_N)}\Bigg|_{L=L^{priv*}} \tag{26}$$

I draw a representation of these curves in Figure 6. The equilibrium of the market occurs at the blue star, where the aggregate demand and supply curves intersect. The coordinates of this star represent the equilibrium N-type wage and labor used in the economy, (N^*, w_N^*).

How does the N-type market react to a change in τ_U?

Because the government must maintain a balanced budget, if τ_U increases, τ_N must decrease. For a given value of w_N, a smaller value of τ_N will decrease the N demanded by both firms. This change is drawn as a leftward shift of both firms' labor demand curves, as depicted by the dashed red lines in Figure 5.

As Figure 5 shows, two immediate implications of the shift are that $N'^* < N^*$ and $w_N'^* > w_N^*$. In other words, total N-type labor, N, and its equilibrium wage, w_N, will both decrease with respect to τ_U. These responses correspond to **Propositions 1.2 and 2.2**.

The reasoning behind **Proposition 3** is now simple. Proposition 2 and the private firm's equilibrium condition imply that $\frac{d}{d\tau_U} F_U^{priv*} > 0$. By constant returns to scale and the Inada conditions, F_U^{priv*} is a decreasing function of $\frac{U^{priv*}}{N^{priv*}}$, so it must be that $\frac{d}{d\tau_U}\left[\frac{U^{priv*}}{N^{priv*}}\right] < 0$. This fact directly implies $\frac{dU^{priv*}}{d\tau_U} < \frac{dN^{priv*}}{d\tau_U}$.

Proposition 1 implies that $\frac{d}{d\tau_U}\left[\frac{U^*}{N^*}\right] > 0$, which can only be true simultaneously with $\frac{d}{d\tau_U}\left[\frac{U^{priv*}}{N^{priv*}}\right] < 0$ if $\frac{d}{d\tau_U}\left[\frac{U^{soe*}}{N^{soe*}}\right] > 0$. The change in the SOE's input ratio must offset the private firm's falling input ratio. The SOE's input ratio change directly implies $\frac{dU^{soe*}}{d\tau_U} > \frac{dN^{soe*}}{d\tau_U}$. This result is **Proposition 4**.

4.7 Testable Predictions

The key empirically testable relationships that can emerge from this model are how SOE and private employment respond to instability shocks. The last step needed to transform the propositions into testable results will be to derive the signs of equilibrium objects' responses to the instability parameter, ξ.

The parameter ξ enters the model only in the government's problem, so the only channel through which instability shocks change employment in the economy will be through the government's choice of τ_L. Recall the government's first order condition:

$$\underbrace{\frac{dY}{d\tau_L}}_{\text{"output cost"}<0} + \underbrace{\eta\xi \underbrace{\frac{dS}{dZ}}_{\ominus} \underbrace{\frac{dz}{dU}}_{\ominus} \frac{dU}{d\tau_U}}_{\text{"stability benefit"}>0} = 0. \tag{27}$$

The government's problem must have a unique solution[10], which implies that if $\eta > 0$, $\frac{dY}{d\tau_L}$ must be negative, and if $\eta = 0, \frac{dY}{d\tau_L} = 0$. Intuitively, the first term captures the marginal cost to output of the subsidy, while the second term captures the marginal benefit to stability of the labor subsidy.

[10]The firms' and government's objective functions are all continuous, differentiable, and strictly concave, and each agent's constraints can be expressed as compact sets. Therefore, there must exist a unique equilibrium.

What happens to the government's first order condition when ξ increases?

$$\frac{d}{d\xi}\left[\eta\xi \underbrace{\frac{dS}{dZ}}_{\ominus}\underbrace{\frac{dz}{dU}}_{\ominus}\frac{dU}{d\tau_U}\right] = \eta \underbrace{\frac{dS}{dZ}}_{\ominus}\underbrace{\frac{dz}{dU}}_{\ominus}\frac{dU}{d\tau_U} > 0$$

Therefore, as long as $\eta > 0$, the marginal benefit of τ_U is increasing in ξ, and we have $\frac{d\tau_U^*}{d\xi} > 0$. Hence, I arrive at:

$$\text{Proposition 5} \quad \begin{array}{ll} \text{Iff } \eta > 0, & \dfrac{d\tau_L^*}{d\xi} > 0 \text{ and } \dfrac{dY^*}{d\xi} < 0 \\ \text{Iff } \eta = 0, & \dfrac{d\tau_L^*}{d\xi} = 0 \text{ and } \dfrac{dY^*}{d\xi} = 0 \end{array}$$

By combining Proposition 5 with Propositions 2, 3, and 4, I derive the following empirically testable predictions:

$$\begin{array}{ll} \text{Prediction 1} & \dfrac{dU^{soe*}}{d\xi} - \dfrac{dN^{soe*}}{d\xi} > 0 \\ \text{Prediction 2} & \dfrac{dU^{priv*}}{d\xi} - \dfrac{dN^{priv*}}{d\xi} < 0 \\ \text{Prediction 3} & \dfrac{dw_U^*}{d\xi} - \dfrac{dw_N^*}{d\xi} > 0 \end{array}$$

These predictions state that unrest-prone labor employment should differentially increase in SOEs and differentially decrease in private firms. Additionally, wages should differentially increase for this group. These are the three key predictions that I will take to the data in the subsequent sections of the paper.

4.8 Sufficient statistic

One benefit of the model is that it generates an empirically-observable sufficient statistic for the male minority SOE wage subsidy. Assume that the production function F takes the following Cobb-Douglas form, such that

$$F = U^{\alpha}N^{1-\alpha}.$$

The first order conditions of the SOE and private firms become:

$$\frac{(1-\alpha)\, N^{priv}}{\alpha U^{priv}} = \frac{w_N\,(1-\tau_N)}{w_U}. \tag{28}$$

$$\frac{(1-\alpha)\,N^{soe}}{\alpha U^{soe}} = \frac{w_N\,(1-\tau_N)}{w_U\,(1-\tau_U)}. \tag{29}$$

Dividing equation 28 by equation 29, I find:

$$\tau_U = 1 - \frac{N^{soe}/U^{soe}}{N^{priv}/U^{priv}} \tag{30}$$

5 Empirical Strategy

To test the implications of my model, I use three natural experiments that correspond to global sources of instability: ethnic unrest, adverse trade conditions, and natural disasters. I will show that, in response to these threats to stability, Chinese employment behaves in a manner consistent with the theoretical predictions of the model. Each of these shocks will correspond to the theoretical object ξ, the instability shock parameter.

Sections 5 through 7 will focus on the first shock, which uses variation in Uyghur ethnic unrest in China to produce causal estimates of model predictions. The Uyghur separatist conflict is endemic to Xinjiang, the westernmost province of China, where some residents seek independence due to multidimensional discrimination and oppression, enacted through both official and unofficial channels. In this section, I discuss the construction of the Uyghur unrest shock. In Section 6, I briefly present the historical and cultural context of the Uyghur conflict in Xinjiang. In Section 6, I introduce the data used for the empirical portion of this paper, and in Section 7, I present baseline results and robustness checks for the Uyghur unrest shock.

One potential objection to evidence based on the Ugyhur ethnic unrest is that the conflict is uniquely inflammatory and sensitive for the Chinese government. Any observed responses in SOE employment may therefore be unique to this conflict and not a general property of SOE behavior. To investigate this possibility, I exploit variation in demand for Chinese exports and flood disasters in China and test whether employment responds in a manner consistent with the model. I describe the empirical approach, data, and results of these tests in Section 3.

5.1 Uyghur Unrest Shock

In this subsection, I introduce a measure that captures the threat of Uyghur unrest in non-Xinjiang counties. This measure is high when there are many unrest incidents in Xinjiang the year before and in non-Xinjiang counties with large Uyghur population shares. A key property of this measure is that it uses variation in unrest intensity in Xinjiang to predict the threat of unrest conflagrations elsewhere in China, thus shutting down direct channels of reverse causality. Another crucial element of causal identification is that I compare the shock response of male minorities, the demographic most likely to participate in ethnic unrest, to the response of everyone else.

The first component of the shock is an annual measure that captures the number of conflict incidents in Xinjiang. I interpret the number of conflict incidents per year as a measure of the intensity of the conflict, so that variation in the incident count reflects variation in the underlying conflict intensity. For the baseline specification, I lag this variable by one year to reflect the fact that employment may be sticky, and thus a fairly slow-moving policy instrument.

The second component of the shock is the share of each Chinese county's population that is ethnically Uyghur, as measured in China's 2000 Census, omitting all Xinjiang counties. For the entire analysis, I will use variation in conflict inside Xinjiang to generate variation in the propensity for conflict to spill over to counties outside of Xinjiang. This choice is critical for the satisfaction of the exclusion restriction, because changes in the intensity of the Xinjiang conflict may respond locally to my outcome variables of SOE employment, private employment, and wages. Even though the response would need to vary heterogeneously with the other components of the shock in order to generate spurious results, the observable and unobservable channels through which local economic factors might generate conflict are manifold. I cut this Gordian Knot by constructing the shock only for non-Xinjiang locations.

Of course, the distribution of Uyghur populations outside of Xinjiang in 2000 is not random. One threat to my identification strategy is that some driver of Uyghur settlement patterns also influences employment and wages during my time period of study, 2002-2009, in a way that is correlated with the intensity of the Xinjiang conflict and, for the triple difference, also differentially affects male minorities. I turn to the ethnographic and historical literature to understand patterns of Uyghur settlement in China. The literature suggests that settlement patterns are generated by a combination of forces. Historical forces include Ming-dynasty military dispatches (Svanberg, 1988) and eighteenth century pilgrimages (Coughlin, 2006). More recent forces include local demand for service jobs (Brophy, 2016; Iredale et al., 2015). The latter clearly have the potential to generate employment and wage responses, even though it is difficult to imagine why those responses would be correlated temporally with the Xinjiang conflict or, in the triple differences specification, why those forces would differentially affect male minorities. To address this possibility, I flexibly control for pre-period labor market conditions in the baseline specification. I describe these controls in Subsection 5.2.

At this point, this difference-in-differences measure can be written as an interaction variable:

$$DD_{ct} = Incidents_{t-1}^{Xinjiang} \times \frac{Uyghur\, pop_{c,\,2000}^{Not\, Xinjiang}}{Total\, pop_{c,\,2000}^{Not\, Xinjiang}} \tag{31}$$

In the expression above, I let c index counties and t index years. I argue that this object is a measure of the underlying propensity for the Uyghur conflict to spill over into county c during year t: its value is largest in years with many conflict incidents in Xinjiang the year before and in counties with the highest density of Uyghur residents.

Specifically, the relevance assumption required for this differences-in-differences shock is that conflict propagation is particularly likely during times of high conflict intensity in Xinjiang in counties with a large share of Uyghur residents in 2000. An inter-disciplinary literature on the propagation of social conflict supports this assumption. Forsberg (2014) and Forsberg (2008) document this pattern of contagion in ethnic conflict in the interstate context, where ethnic conflicts are more likely to spill over into places with higher shares of the aggrieved group(s) and during times where the conflict is most severe. Moreover, Buhaug and Gleditsch (2008) find that spatial and temporal correlations in intrastate conflict can be explained by ethnic ties among separatist conflicts. Cederman et al. (2009) provide correlational evidence that ethnic networks across state boundaries can facilitate the incidence of intrastate conflict. In December 1985, China experienced the potential for ethnic unrest propogation firsthand, when Uyghur students demonstrated in Beijing against nuclear testing in Lop Nor (Toops, 2009). Together, this evidence suggests that this spatial and temporal pattern of conflict spillover is plausible and precedented in China.

That social unrest is a contagion and that the contagion is particularly great for groups that share an ethnic identity with combatants may arise from several mechanisms. One possible explanation is information sharing within ethnic networks (Weidmann, 2015). Another explanation is that ethnic identity is made salient during times of conflict, and preferences related to ethnic identity receive greater weight as a result (Cornell and Hartmann, 2006). The precise mechanism, or combination of mechanisms, that generate the potential for unrest spillover is not critical to my argument, as long as some are present in this context.

At this stage, consider a regression of a labor market outcome, like SOE employment, on the interaction variable proposed in expression 31. Such a specification could produce spurious results if the county-year interaction variable were correlated with some omitted determinant of the Chinese labor market. During my time period of study, 2002-2009, the Chinese economy underwent dramatic changes that very well could have produced such an omitted variable, including the SOE ownership reforms of the 90's and 00's, the 2001 accession to the World Trade organization, and the fiscal stimulus response to the 2008 global financial crises. To explicitly control for all such changes would be difficult and unconvincing.

Instead, I introduce a third comparison to my causal identification strategy: I compare the shock response of male minorities to that of everyone else. Male minorities are the demographic most likely to participate in ethnic unrest in China and their status is easily observable, so a government with a limited budget should and could target that group with stability policies during ethnic unrest shocks. Moreover, because all workers, not just male minorities, are subject to the broad-based economic changes listed above, the differential response of male minorities will reveal the causal employment response of SOEs and private firms to the Uyghur unrest shock.

Data and qualitative evidence support this approach. Anthropological work on the Xinjiang conflict suggests that a very large majority of participants are male, and nearly all are Uyghur (Bovingdon, 2004). I corroborate this observation using data from the United States Congressional-Executive Committee on China, which maintains a data set of all known Chinese political prisoners. A comparison of the demographics of those prisoners with the general Chinese population in Table 3 reveals that male minorities are a disproportionately large share of political dissidents in China. This prevalence accords with the general sociological and criminological finding that men tend to participate in violence at much higher rates than women (Heidensohn and Gelsthorpe, 2002; Lauritsen et al., 2009).

The Chinese government is well aware of the demographics of the Xinjiang conflict, so any resource-constrained stability policies are likely to target the high-risk group: male Uyghurs. The reason I use an indicator variable for male minorities, rather than male Uyghurs, is due to data limitations: in the Urban Household Survey, my primary data source, the finest level of information on the ethnicity of respondents is whether they are Han or a minority. I discuss this data source in detail in Subsection 6.2. Uyghurs represent 8.4% of all minorities in provinces outside of Xinjiang (The National Bureau of Statistics, 2010a).

With the addition of this third interaction, the shock can be written as the following expression, where the additional index i represents individuals.

$$DDD_{ict} = Male\,Minority_i \times \\ Incidents_{t-1}^{Xinjiang} \times \frac{Uyghur\,pop_{c,\,2000}^{Not\,Xinjiang}}{Total\,pop_{c,\,2000}^{Not\,Xinjiang}} \tag{32}$$

The exclusion restriction for this triple differences setup is substantially more difficult to violate. A spurious result can only be generated by some force that co-varies temporally with the number of Xinjiang incidents, co-varies geographically with Uyghur population density, and furthermore, differentially affects male minorities. Though it is difficult to identify concrete phenomena that would satisfy these criteria, I nonetheless consider and control for potential sources of omitted variables in Subsection 7.1.

For the empirical analysis, I will take the stance that my triple differences estimator captures the causal effect of ethnic unrest threat on SOE employment, private employment, and wages. I discuss the link between my model and the empirical setup in much greater detail in the following subsection.

5.2 Baseline Specification

My baseline estimating equation is designed to produce estimates of model relationships.

$$
\begin{aligned}
Y_{ict} = \alpha &+ \beta_M Incidents_{t-1} \times Uyghur\,Share_c \times Male\,Minority_i \\
&+ \beta Incidents_{t-1} \times Uyghur\,Share_c \\
&+ \gamma_1 Incidents_{t-1} \times Male\,Minority_i \\
&+ \gamma_2 Uyghur\,share_c \times Male\,Minority_i \\
&+ \gamma_3 Male\,Minority_i \\
&+ \delta_c \boldsymbol{X_c} \times \tau_t \times Male\,Minority_i \\
&+ \delta_i \boldsymbol{X_i} + \tau_t + Dist\,XJ_c \times \tau_t + \eta_c \times Male\,Minority_i + \epsilon_{ict}
\end{aligned}
\tag{33}
$$

$$
Y_{ict} \in \{SOE\,Empl_{ict},\ Private\,Empl_{ict},\ NonEmpl_{ict},\ Salary_{ict}\}
$$

where i indexes individuals, c indexes counties, and t indexes years. The baseline sample includes all individuals surveyed in the Urban Household Survey between the ages of 22 and 55 for the years 2002 - 2009. The temporal coverage does not extend to the full UHS time span of 1992 - 2009 because the ethnicity variable is only available for the later time period. All observations from the province of Xinjiang are excluded.

Three of the dependent variables relate to the individual's employment status. The variable $SOE\,Empl_{ict}$, is an indicator for SOE employment, which takes a value of 1 when the UHS employment variable reports an individual as working in a state-owned economic unit. The variable $Private\,Empl_{ict}$ similarly takes a value of 1 if an individual is employed in a privately-owned economic unit, and zero otherwise. The variable $NonEmpl_{ict}$ takes a value of 1 if an individual is not employed.[11] The variable $Salary_{ict}$ is the continuous nominal value of employment income in thousands of yuan and takes a missing value for non-employed individuals.

In this specification, I assume that Y_{ict} is a function of a triple interaction between lagged violent incidents in Xinjiang, $Incidents_{t-1}$, 2000 county Uyghur population share, $Uyghur\,Share_c$, and

[11] Because this category includes people who are not actively searching for a job, it differs from standard definitions of unemployment. For example, this category includes individuals of working age who are engaged in home production.

an indicator for whether an individual is a male minority, $Male\,Minority_i$. This indicator takes a value of 1 for male minorities and takes a value of 0 for everybody else, including female minorities. Several of the triple interaction terms are absorbed by fixed effects, which I describe below.

This specification includes year fixed effects τ_t, county and male minority fixed effects $\eta_c \times Male\,Minority_i$, interactions of a vector of county-level characteristics $\boldsymbol{X_c}$, and a vector of individual-level characteristics $\boldsymbol{X_i}$. The vector $\boldsymbol{X_c}$ includes base year (2002) county-level characteristics, including the shares of the labor force employed in SOEs, private firms, and non-employed, as well as the percent growth from 2001 to 2002 of each of those objects. I interact this vector with year fixed effects and an indicator for male minority. This set of controls absorbs systematic differences in later employment among counties that had different employment composition and growth in 2002, and allows those differences to change over years and occur differently for male minorities. In the vector $\boldsymbol{X_i}$ are age, gender, and a fixed effect for years of education. These effects will absorb any persistent differences in provinces due to policy or institutions and any global trends that affect all provinces similarly.

I also control for the interaction of the logged kilometer distance of each county from Xinjiang, $Dist\,XJ_c$, interacted with year fixed effects, τ_t. This control removes variation from omitted variables correlated with both Uyghur share and distance from Xinjiang, that determine government policy or economic conditions. Such spatial phenomena could potentially bias the estimate of interest. My baseline estimates are very robust to the inclusion or exclusion of these controls.

The county and male minority fixed effects, $\eta_c \times Male\,Minority_i$, absorb any time-invariant differences in the labor composition of counties for male minorities and non-male minorities. For example, if private firms in some counties were consistently less likely to hire male minorities over the entire time period, this fixed effect would absorb that potentially confounding variation. Finally, I cluster my standard errors at the county level to account for the shock's level of geographic variation.[12]

I now describe how these empirical objects correspond to theoretical objects in the model. The Uyghur unrest shock maps onto ξ, the model's unrest shock. The differential response of male minority labor outcomes is meant to be a causal estimate of the response of L to ξ. I can

[12] As a robustness check, I present standard errors with two-way clustering at the county and year level. However, I do not use this level of clustering as the baseline because my dataset has only 8 years.

therefore rewrite the theoretical predictions in Section 4 as empirical hypotheses. I indicate the outcome variable of the regression as a superscript: for example, β_M^{PRIV} refers to the coefficient β_M estimated from the regression of $Private\,Empl_{ict}$ on the baseline specification.

Prediction 1 $\dfrac{dL^{soe*}}{d\xi} > 0 \rightarrow \beta_M^{SOE} > 0$

Prediction 2 $\dfrac{dL^{priv*}}{d\xi} < 0 \rightarrow \beta_M^{PRIV} > 0$

Prediction 3 $\dfrac{dw^{*}}{d\xi} > 0 \rightarrow \beta_M^{Salary} > 0$

6 Empirical Context and Data

Xinjiang is China's northwestern-most province and borders Mongolia, Russia, Kazakhstan, Kyrgyzstan, Tajikistan, Afghanistan, Pakistan, and India. Approximately half of the province's population is Uyghur, a Turkic ethnic group that primarily practices Islam (The National Bureau of Statistics, 2010b). The roots of separatist sentiments in Xinjiang can be traced back to the 16th Century Qing Dynasty, through the period of Republican control, and into the modern era (Millward, 2004). The separatists support Turkic and Uyghur nationalism and seek autonomous self-rule.

In the 1980s, demonstrations over ethnic issues took place in Xinjiang, but they were disorganized and did not precipitate significant violence. The 1990s saw an escalation in the cohesion and intensity of the separatist movement. Qualitative accounts identify three spikes of violence in 1990, 1992-93, and 1996-97 (Davis, 2008; Millward, 2004), which my time series data, plotted in Figure 8, corroborate.[13] The first cluster involved an armed uprising in April of 1990 in the township of Baren. The conflict's onset has been attributed to several factors, including coordination and recruitment by Afghan Islamists (Patrick, 2010) and government-mandated abortions (Guo, 2015). The second cluster in 1992 and 1993 involved a series of bombings in public locations, like buses, retail stores, and a cinema (Millward, 2004; Gurr, 2000). The third cluster, starting in 1996, was likely influenced by a policy shift towards a stricter government stance against separatism. The policy change was accompanied by systematic arrests of alleged separatists. The climax of violence in this period took place in February of 1997 and is often called the "Ghulja Incident", in which approximately 20 people died, though exact estimates of fatalities differ. Primary sources suggest

[13] I discuss the source of these data in Subsection 6.1.

that the riots broke out after Chinese police officers confronted a Uyghur family for resisting arrest (Gurr, 2000).

The early 2000s were a relatively quiet period for the conflict, with scattered bombings and assassination attempts. Tensions rose again in 2007, after a Chinese police raid on a suspect separatist training camp. In the ensuing years, several attacks took place in the cities of Kashgar, Kuqa, and Urumqi (Guo, 2015).

Qualitative evidence suggests that the timing and intensity of incidents were largely determined by the strategic considerations of the guerrilla forces and violent escalations of gatherings formed around local events, like mosque closures. A few violent incidents were triggered by economic phenomena, like firm layoffs, but these events represent a small share of incidents. An even smaller proportion of Xinjiang incidents were explicit responses to events outside of Xinjiang, like a factory fight between Han and Uyghur workers in Guangdong Province, or Deng Xiaoping's funeral (Bovingdon, 2010). Because of the rich historical documents from which I code my incident data, I am able to code the proximate trigger for each conflict incident. I will use this information later, in robustness checks, to systematically eliminate the incident observations most likely to violate the assumptions of my identification strategy.

6.1 Uyghur Unrest Shock

My triple-differences Uyghur unrest shock relies on three sources of variation: annual variation in Xinjiang conflict incidents, county-level variation in the share of the Uyghur population, and individual-level variation in whether a person is a male minority. In this section, I discuss the measurement of each component.

I construct a time series of separatist unrest in Xinjiang using multiple primary and secondary historical sources. First, I conduct a systematic search of historical newspaper archives using the Proquest Historical Newspapers Database. I generate a data set of unique incidents and record the date, province, county or city, and type of each incident. An incident is included in the sample if it is documented by an internationally reputable media outlet and if it is explicitly linked to separatist sentiments. To these events, I incorporate incidents from a similar data set constructed by Hastings (2011). The author used several resources to identify incidents: START's Global Terrorism Database (LaFree and Dugan, 2007), contemporaneous newspaper articles, and wire service reports. Finally, I incorporate incidents reported in Bovingdon (2010), who consulted Wisenews Chinese language newspapers, Chinese government white papers, security almanacs, and contemporaneous newspaper reports. I identify and remove any duplicate incidents using date, location, and additional information reported in these data.

The time series of Xinjiang conflict events are plotted in Figure 8. The baseline measure of Xinjiang violence intensity is a simple count of events in each year, regardless of the number of perpetrators or victims. I choose to use incident count instead of fatalities because fatality estimates are more prone to strategic manipulation. Whether an incident occurs at all is both easier to measure and more difficult to manipulate.

The second component of the Uyghur unrest shock is a cross-sectional measure of the share of the county population that is Uyghur. I use data on county population by ethnicity in the 2000 Population Census of China (The National Bureau of Statistics, 2010a) and divide the number of Uyghur individuals by the total population of the county. I use the Census of 2000 rather than more recent data because 2000 predates the coverage of my main data set, thus weakening some of the potential endogeneity in Uyghur population distribution that might arise from the migration of Uyghur peoples in response to unobserved factors, like friendly local policies. Figure 9 presents a choropleth map of county-level Uyghur population shares outside of Xinjiang. Counties with high Uyghur shares are spread fairly evenly throughout China, though larger cities, like Beijing and Shanghai, as well as remote Western counties, tend to be home to a denser concentration of Uyghur people. It is not the case that Uyghur residency patterns outside Xinjiang are concentrated in one province or geographic region of China, which allows me to control for a wide array of geographic fixed effects.

In addition to the data sources used to construct my shock, I draw from a number of additional observational data sets on China to measure variables of interest. I describe these data sets in the following subsections.

6.2 Urban Household Survey

My outcome variables and individual-level data come from the *Urban Household Survey* (UHS). These data are collected by the National Bureau of Statistics, and I will use data from the years

2002 to 2009.[14] The sampling procedure for households is stratified at several levels, including the province, city, county, township, and neighborhood. The data set has a rotating panel structure such that selected households remain in the survey for three years before exiting. Households are legally obligated to respond, and illegal city residents are protected by law from prosecution based on this survey, though these households are likely underrepresented nonetheless due to worse documentation and the perceived risks of responding.

The UHS data set includes a rich set of variables describing household composition, age, gender, ethnicity, employment, and education. It also records exceptionally detailed information on household income and consumption. Critically for this project, the "employment situation" variable contains information about the ownership of the employee's workplace and distinguishes between state-owned units, urban collective units, joint-stock and foreign units, township private enterprises, and urban private enterprises. This ownership information is crucial to many of the empirical tests presented in this paper. For the analyses below, I will define SOE employment as the employees of state-owned units and urban collective units, as there is a literature documenting how collective firms in China exhibit similarities to SOEs (Brandt and Rawski, 2008). However, in the Appendix, I explore how my results change if I define SOEs as state-owned units only.

My UHS data are a representative sample of urban areas in 17 provinces: Anhui, Beijing, Gansu, Guangdong, Heilongjiang, Henan, Hubei, Jiangsu, Jiangxi, Liaoning, Shaanxi, Shandong, Shanghai, Shanxi, Sichuan, Yunnan, and Zhejiang. These provinces were chosen by the surveyors to represent a wide array of income levels and geographic locations.

6.3 Annual Survey of Industrial Production

Though the Urban Household Survey has many advantages, it is not possible to estimate firm productivity from household data, since they definitionally lack firm-specific balance sheet variables. Therefore, to corroborate the fact that SOEs have lower productivity than private firms, I use a popular firm dataset from China, the *Annual Surveys of Industrial Production* (ASIP), which are also collected by the National Bureau of Statistics. These data are sometimes called the "Annual Surveys of Manufacturing". I use surveys from 1998-2008, which are widely considered the most

[14] I do not use earlier available years from 1992 through 2001 as the data from these years does not include the minority status of respondents.

reliable (Brandt et al., 2014). The unit of observation in this data set is the firm, and all entities with separate legal registration are considered separate firms, a situation that applies to most subsidiary companies in China. The data set is intended to be a census of all state-owned enterprises and a census of all non-state manufacturing firms with sales that exceed five million RMB. As a result, the inclusion criteria for different ownership types are different. In order to restrict the sample to firms of comparable size across ownership type, I impose a strict five million RMB cutoff in sales and keep only firms that exceed it.

I also apply the data preparation procedure first used in Cai and Liu (2009) that has since been widely adopted within this literature. I drop all observations for which the start month does not fall between 1 and 12, as well as any observations whose start year is later than that of the survey year. I also drop all observations whose total assets do not exceed any reported component of assets.

I do not use this data set to test predictions about aggregate employment, which is more accurately measured using the UHS. Appendix Figure A.14 plots the share of employment covered by the ASIP alongside the share of China's employment in industrial activities. Over the time period for which I have firm data, the ASIP covers approximately 20%-35% of all Chinese employment.

7 Results

In this section, I present the results of my empirical analysis.

Table 4 presents results from estimating Equation 33 as a linear probability regression. The three outcome variables covered in this table are SOE employment, private employment, and non-employment. The three predictions correspond to the first coefficient in each column of Table 4.

Prediction 1 states that the first row of column (1) should be positive. Indeed, the coefficient in the row labeled β^M is positive 36.59 and different from zero at the $p < 0.01$ level. This result demonstrates that SOEs increase male minority employment in response to an increase in the threat of Uyghur unrest. Prediction 2 states that the first row of column (2) should be negative, and I find that the magnitude of β_M^{PRIV} is -24.24 and different from zero with $p < 0.05$. Finally, Prediction 3 was that the first coefficient of column (3) should be positive. The true coefficient is $5,422$ with

$p < 0.01$, in accordance with the prediction.

I translate each triple interaction into real units by multiplying it with the mean of lag Xinjiang incidents variable and the mean of county Uyghur share. The first coefficient in column (1) implies that, when the shock moves from its lowest value to its mean value, SOEs will hire an additional $226,040$ minority men. This number represents a 0.48 percentage point change in SOE employment, over a mean SOE employment probability of 55%. The first coefficient of column (2) implies a decline of $-149,910$ minority men in private employment. This number represents a 0.32 percentage point fall in private employment, over a mean private employment probability of 25%. The first coefficient in column (3) represents an annual salary increase of 713 RMB (approximately $100 USD).

I also re-estimate a version of the baseline equation, presented in Equation 34, that produces separate estimates for β_M and β by year. In Figure 10, I plot these coefficients along with the time series of lagged Xinjiang incidents over time. As the positive triple interaction coefficient in column (1) of Table 4 implies, the coefficients on the interacted county Uyghur share and male minority term, plotted with the solid red line, co-move with the number of lagged Xinjiang incidents every year, plotted with the solid blue line. Conversely, the coefficients on Uyghur share variable alone do not correlate strongly with the number of lagged Xinjiang incidents.

$$
\begin{aligned}
SOE_{ict} = \alpha &+ \sum_{t=2002}^{2008} \beta_{Mt} \mathbb{I}_t \times Uyghur\,Share_c \times Male\,Minority_i \\
&+ \sum_{t=2002}^{2008} \beta_t \mathbb{I}_t \times Uyghur\,Share_c \\
&+ \gamma_1 Incidents_{t-1} \times Male\,Minority_i \\
&+ \gamma_2 Uyghur\,share_c \times Male\,Minority_i \\
&+ \gamma_3 Male\,Minority_i \\
&+ \delta_c \boldsymbol{X_c} \times \tau_t \times Male\,Minority_i \\
&+ \delta_i \boldsymbol{X_i} + \tau_t + Dist\,XJ_c \times \tau_t + \eta_c \times Male\,Minority_i + \epsilon_{ict}
\end{aligned}
\tag{34}
$$

In addition to providing a visual representation of my main result, this figure also shows that the effect is not generated by a single year of anomalous data.

7.1 Robustness Checks

One source of omitted variables for my main regressions would be alternative determinants of employment and wages that are correlated with the temporal variation in Xinjiang incidents, correlated geographically with the distribution of high-Uyghur share counties, and that differentially impact minority men relative to non-minority men. In particular, the literature suggests that, in addition to the provision of social stability, SOEs are also used to retain control over strategic sectors, like utilities and mining, or to maintain a large administrative capacity (Leutert, 2016). In order to test whether my main results are generated by these other motives that may happen to be correlated with the Uyghur unrest shock, I conduct a set of robustness checks.

First, to control for the local share of the economy in mining and allow high-mining and low-mining districts to traverse different time paths, I compute the district-level share of employment in mining for each district in China for the year 2002, which is the base year of my main UHS sample. There are 182 districts. I then interact this district-level variable with year fixed effects, minority fixed effects, and male fixed effects and add the full interaction into the baseline specification. I repeat this process for the district level share of employment in public services and utilities.

Table 5 reports this set of robustness checks for employment by ownership. I find that these very flexible controls for alternative SOE motives change the magnitudes and precision of the baseline estimates very little; each point estimate remains steady with the addition of all of the new controls simultaneously. I perform a complementary robustness check by dropping public services workers, mining workers, and utilities workers from the sample and re-running the baseline regression. The results, reported in Appendix Table A.23, remain similar in sign and magnitude to those of the baseline.

Another potential source of endogeneity in my Uyghur unrest shock is if Xinjiang unrest incidents were triggered by events outside Xinjiang. If those outside events were in turn correlated with local economic conditions, then my estimates could potentially be ascribing variation in local conditions to variation arising from the unrest spillover propensity. To address this concern, I return to the primary evidence and hand-code the inciting reason for each event in my database of Xinjiang unrest. I then drop every event whose trigger came from outside Xinjiang. One example of Xinjiang unrest triggered by outside events includes a series of bombings in Urumqi that rebel groups timed to coincide with Deng Xiaoping's funeral in February of 1997. The timing of these bombings was meant to publicize the struggle of the Uyghur people against the Chinese government. Table 6 reports estimates that use the baseline specification, but use the amended Xinjiang incident time series instead of the baseline. I find that the results are corroborated when using this alternative time series.

Another potential source of endogeneity would be if Xinjiang unrest incidents were triggered by Xinjiang economic conditions, which in turn were correlated with the economic conditions of counties across China in a way that would generate spurious results. To address this possibility, I construct a time series of Xinjiang incidents that removes all incidents related to economic issues. For example, I remove a series of protests that occurred in October of 2001 in the city of Hotan in southern Xinjiang. The workers were protesting local factory closures. Table 7 reports estimates from regressions that use the time series of Xinjiang unrest without any economically-triggered incidents. I find that the main results are all corroborated when using this second alternative time series.

Furthermore, I test that the baseline results are robust to logit estimation, since my employment ownership outcome variables are binary. Table 8 reports estimates from the baseline specification when estimated with a logistic link function. I find that the main results for SOE employment and private employment are robust to this choice.

One property of this empirical context is that the distribution of Uyghur population shares is not normal, as Figure 9 demonstrates. Thus, I should be particularly concerned that certain values, potentially mis-measured, are generating a spurious result. I run several robustness checks that explicitly address this concern.

First, I test whether the baseline results are sensitive to the removal of outliers. To identify outliers, I compute DFITS for each observation (Langford and Lewis, 1998) and drop all observations with DFITS greater than $2\sqrt{k/N}$, where k is the number of regressors and N is the number of observations. Table 9 reports the baseline regressions re-estimated on the no-outlier sample and demonstrates that the SOE and salary results are robust to this procedure. The private employment result remains negative but is no longer precisely estimated.

Second, I perform a random permutation test on the Uyghur share variable. For this test, I run the baseline regression for the SOE outcome variable 500 times, but each time, I randomly assign each county a Uyghur share value drawn from the observed distribution of Uyghur values in the data. In other words, for each of the 500 iterations, I generate a counterfactual Uyghur share map for China that follows the same distribution as the true map. Then, I plot a histogram of the coefficient β_M for each of these 500 iterations in Figure 11. I find that only 8.3% of these counterfactual coefficients have a value higher than the true estimate of 36.61. This distribution of counterfactual estimates increases my confidence that my baseline estimates could not be generated by a random assignment of county Uyghur share values.

To address the possibility that the extreme right tail drives my baseline results, I take all the non-zero county Uyghur share values and winsorize them at the 95th percentile of their non-zero values. I then use these values in the baseline regression instead of the original Uyghur share variable. The baseline table using these winsorized data are reported in Appendix Table A.24. The results are identical in sign to the baseline results and remain similar in precision and magnitude.

Finally, in Table 10, I conduct a placebo test. Instead of using lagged Xinjiang incidents in the shock, I use instead the lead of Xinjiang incidents. Theoretically, SOE employment cannot respond to incidents in the future. The estimates in this table are consistent with this reasoning. The coefficients β_M and β are small in magnitude and not precisely different from zero for all three outcome variables.

7.2 Heterogeneity by Sector

Are there sectors in which the SOE stability response is more pronounced? To answer this question, I construct a variable that records the sector of employment for each individual using the UHS sector variable. I consolidate the twenty raw sector categories into six aggregate sectors: agriculture, manufacturing, mining and construction, retail and transportation, services, and Communist Party work. Urban agriculture is rare and there is no variation in firm ownership in Party work, so I drop the first and last categories. I then run the baseline specification separately for SOE employment and private employment for the remaining four sectors. Because the sector of employment is not defined for non-employed people, I do not report regressions with that outcome variable. Moreover, because SOE employment and private employment are therefore mirrors of each other, the coefficients will be identical but of opposing signs. I report both regressions for each sector for completeness.

Table 11 reports estimates from the remaining sectors: manufacturing, mining and construction, retail and transportation, services. The services sector is the only one that displays a precise and positive SOE employment response to the Uyghur unrest shock, and the response only takes place for male minorities. The coefficient of 62.02 is precisely different from zero at the $p < 0.01$ level. Due to the large standard errors belonging to the β_M coefficient for each of the other sectors, the services sector response is not significantly different from the others.

This table suggests that there may be a stronger stability response in service-sector SOEs, though large standard errors prevent me from making a definitive statement. There may be several reasons for this pattern, including the fact that SOEs are 75% of the employers in this category, and that a slightly higher share of service-sector employees are male minorities than in other sectors.

7.2.1 Response Over Time

In this section, I characterize the time path of the employment response to unrest. The way in which the shock affects employment in the medium run is essential for the interpretation of the result, because it contains information about the persistence of the stability policy. Therefore, I expand the baseline specification to include more lags of the Xinjiang incident variable.

$$
\begin{aligned}
Y_{ict} = \alpha &+ \sum_{j=1}^{5} \beta_{Mj} Incidents_{t-j} \times Uyghur\, Share_c \times Male\, Minority_i \\
&+ \sum_{j=1}^{5} \beta_j Incidents_{t-j} \times Uyghur\, Share_c \\
&+ \sum_{j=1}^{5} \gamma_{1j} Incidents_{t-j} \times Male\, Minority_i \\
&+ \gamma_2 Uyghur\, share_c \times Male\, Minority_i \\
&+ \gamma_3 Male\, Minority_i \\
&+ \delta_c \boldsymbol{X_c} \times \tau_t \times Male\, Minority_i \\
&+ \delta_i \boldsymbol{X_i} + \tau_t + Dist\, XJ_c \times \tau_t + \eta_c \times Male\, Minority_i + \epsilon_{ict}
\end{aligned}
\tag{35}
$$

The variable $Incidents_{t-j}$ will capture the number of unrest incidents that took place in Xinjiang j years ago, so the vector of coefficients $< \beta_{M1}, ..., \beta_{M5} >$ will express the differential shock response of the outcome variable Y_{ict} for male minorities as time elapses. I estimate Equation 35 for the outcomes of SOE employment, private employment, non-employment, and salary. I plot the regression coefficients $< \beta_{M1}, ..., \beta_{M5} >$ in Figure 12.

The four sub-figures reveal that the labor market responses to the Uyghur unrest shock in year t are most pronounced in the year following the shock and slowly decline in magnitude. For SOE employment, the initial positive differential response for male minorities declines steadily for three years and then appears to "correct" to a negative value four years after the initial shock. The size of the negative correction is much smaller in magnitude than the initial positive employment response. This pattern suggests that SOE employment adjusts slightly, but not completely, after the initial expansion due to an unrest shock.

The response of private employment is nearly a perfect mirror image to that of SOE employment. A large, precise, and negative initial response slowly decreases in magnitude. In the fourth year following the shock, there appears to be a slight positive correction in private employment, which then reverts to a null effect in the fifth year. Non-employment displays null responses throughout the time period. Finally, average salary follows the same approximate path as SOE employment: in the first two years following a shock, the prevailing salary increases precisely and positively, but then declines and appears to correct slightly in the fourth year after a shock.

Overall, Figure 12 suggests that male minority employment and wages display the largest responses to unrest immediately after the incidents take place and then slowly converge with those of everyone else over time. During the convergence process, there even appears to be a slight reversal of the initial shock response around year four, but the magnitude of the correction is not large enough to swamp the initial changes.

7.3 Complementary Policies

In this section, I test whether the government uses other policies in conjunction with SOE employment to address the possibility of ethnic unrest. Toward this end, I consider the response of social relief transfers. The Urban Household Survey includes rich, disaggregated data on the transfer income of individuals, one type of which is social relief transfers from the government. This category encompasses financial and in-kind assistance disbursed in response to natural disasters, sudden disability, extreme poverty, and other challenges to subsistence (Hussain, 1994; Cook, 2002;

Wong, 2005). These transfers can take many forms and are designed to be nimble and responsive in the face of changing circumstances. As a result, the government retains a great deal of discretion in their disbursement. Is it possible that these transfers are also used to preserve social stability in conjunction with state employment policies?

I re-estimate Equation 33 using social relief transfers as the outcome variable. To further enrich the evidence, I also repeat the regression for three sub-samples: SOE employees, private employees, and individuals who are not employed. Results from these regressions are reported in Table 12. In Column (1), I see that in response to the shock, average social relief transfers to male minorities differentially increase by $17,507$ yuan, and the change is precisely different from zero at the $p < 0.01$ level. This column suggests that the government does complement its employment stability policies with targeted relief transfers. At the top of the column, I report average relief transfers in the sample, which is a tiny value, just 18.57 yuan. The reason for this low average is the large proportion of workers receiving no relief transfers: only $3,208$ workers in the sample, or 1.3%, receive non-zero relief transfers.

In Columns (2)-(4), I consider the response of relief transfers by employment status: SOE, private, or non-employed. I find that, while the point estimate for the male minority interaction is positive in all columns, the magnitude is only precise for SOE employees and non-employed individuals. Moreover, the transfer response for non-employed male minorities is over ten times as large as those of the employed workers, and it is precisely different from the response for SOE and private workers. These columns suggest that the relief transfers are most targeted on the population of male minorities not reached by employment policy. These joint policy responses to unrest shocks are indicative of a broad-based government effort to preserve stability in the face of ethnic unrest.

However, other interpretations are also possible. For example, these results might also be generated if the government increases bureaucratic capacity in response to the shock. If the eligible recipients of relief transfers happen to be mostly employed in SOEs or not employed, a similar pattern could emerge. However, it would have to be true that the newly included eligible recipients are all male minorities, which seems prima facie unlikely.

7.4 Sufficient statistic

I substitute empirical moments into equation 30 and conclude:

$$\tau_U = 1 - \frac{N^{soe}/U^{soe}}{N^{priv}/U^{priv}} = 1 - \frac{45.95}{62.17} = 1 - 0.739 = 0.261$$

The model implies that the equilibrium wage subsidy for male minorities is 26 of prevailing wages. The 95% confidence interval for this figure is $(20\%, 32\%)$.

8 Conclusion

The goal of this paper is to document one political economy reason for the persistence of state-owned enterprises in China, and consequently, a downward force on productivity in a major world economy. I argue that the Chinese government uses SOEs not only as units of production but also as policy instruments for maintaining social stability. In this paper, I provide a theoretical framework wherein a government subsidizes state firms to boost employment of certain demographics; the increase in employment depresses the likelihood of unrest.

To test the implications of the theory, I construct a triple-differences estimation strategy that uses annual variation in Xinjiang conflict intensity, county-level variation in Uyghur population densities, and individual-level variation in whether individuals are male minorities. My empirical results suggest a strong, precise response: SOEs increase their employment of minority men in response to the unrest shock. Private firms shed employment from the same group. Furthermore, the employment of female minorities and that of the ethnic majority (Han) do not change in response to the shock. I find that salaries of SOEs and private firms increase only for male minorities, suggesting that the patterns observed are due to increasing SOE labor demand rather than falling private labor demand.

By uncovering a political economy source of inefficiency in an important context, I show that one source of cross-country income variation may be the extent to which output efficiency and political efficiency differ across countries. This project points to a number of future research topics. Could alternative stability policies generate fewer distortions than state employment? Does regime type constrain which stabilizing policies governments can use? And what other political economy concerns generate economic distortions? These questions all relate to the fundamental theme of how, and why, political concerns manifest as forces of economic development.

Figure 1: Urban SOE Employment Over Time

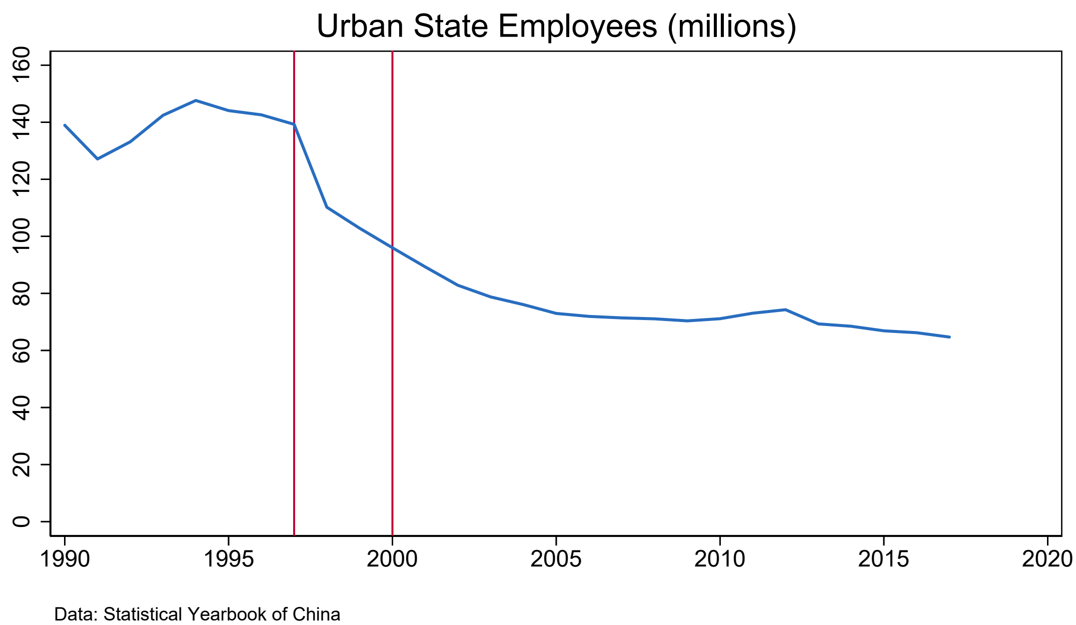


Figure 2: The Demographics of Political Prisoners versus General Population

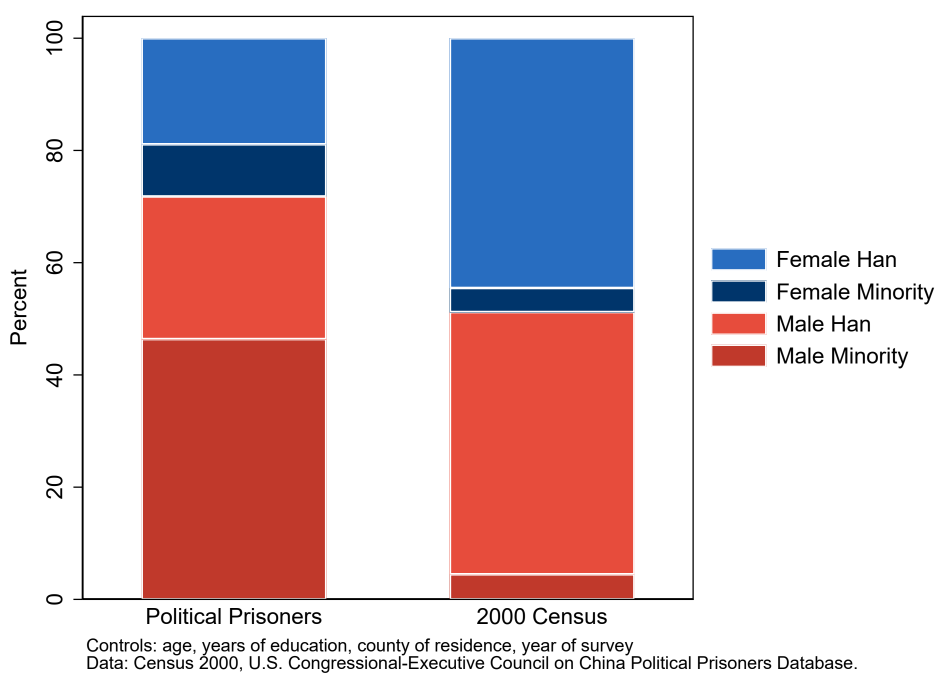


Figure 3: The Demographics of Private Firms versus SOEs

Controls: age, years of education, county of residence, year of survey, sector
Data: Urban Household Survey 2002-2009

Figure 4: U-type Labor Market

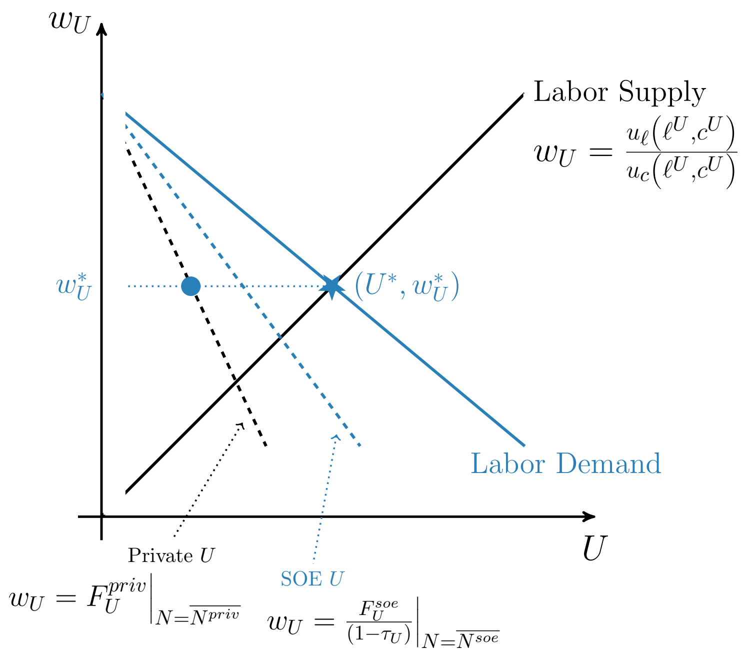


Figure 5: U-type Labor Market: $\tau_U \uparrow$

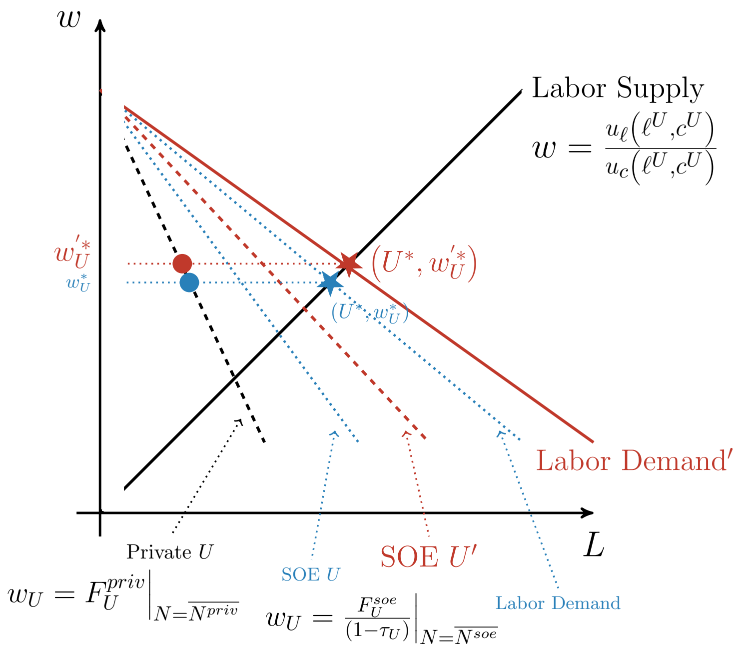


Figure 6: N-type Labor Market

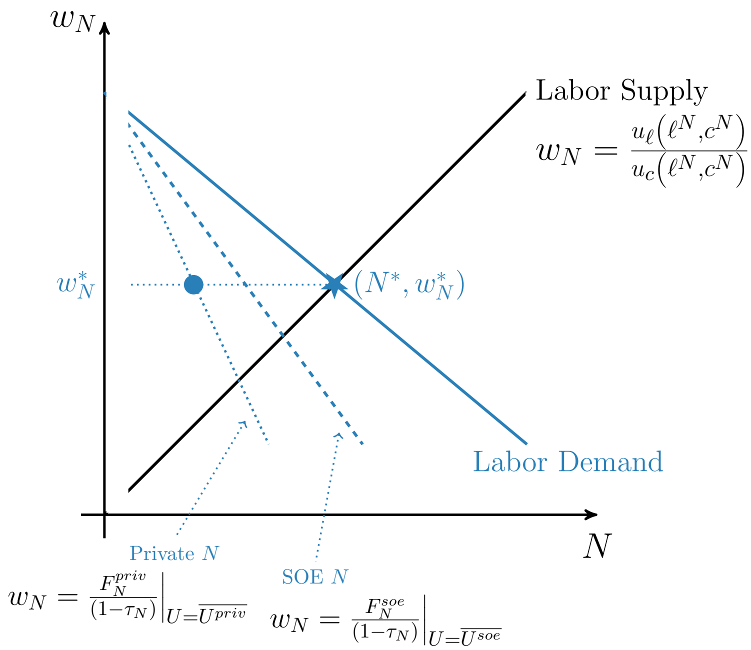


Figure 7: N-type Labor Market: $\tau_U \uparrow$

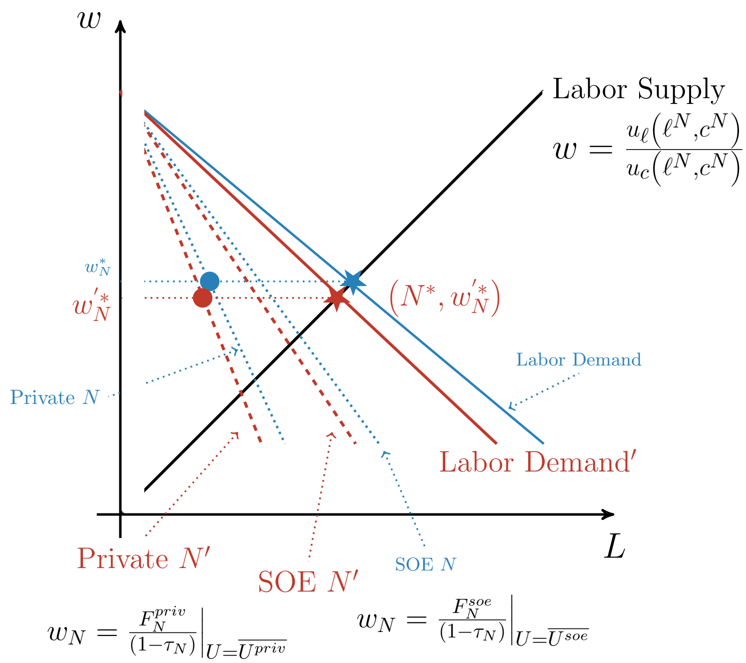


Figure 8: Timeline of Xinjiang Unrest Incidents

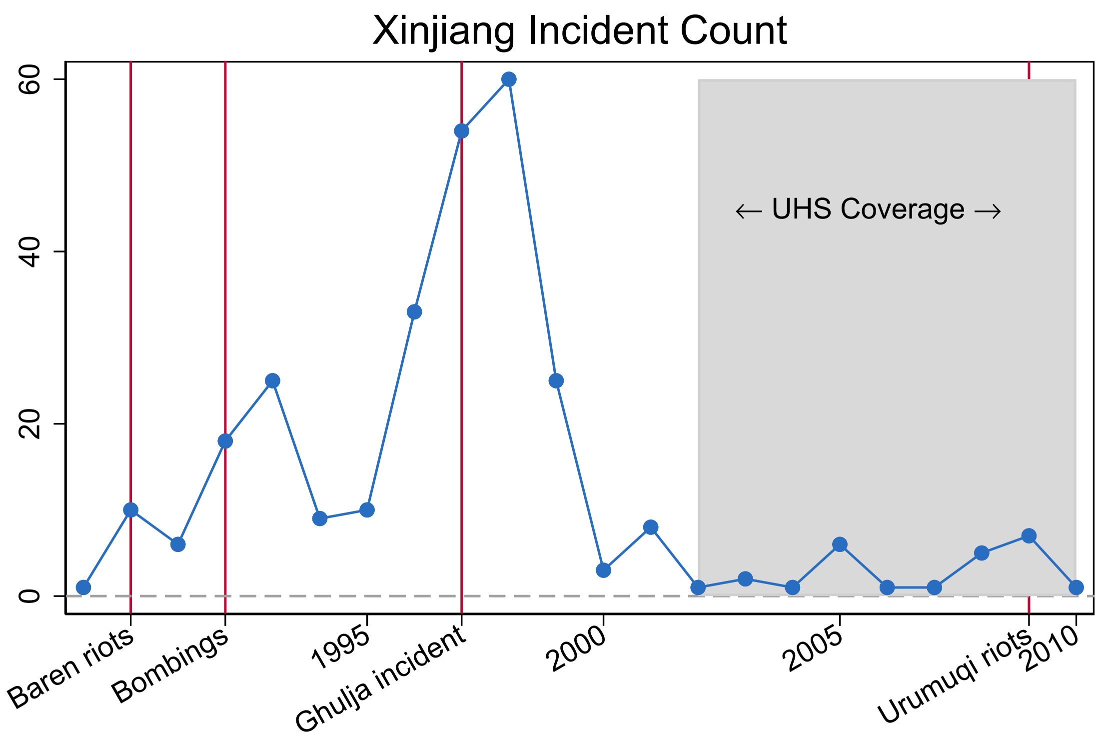
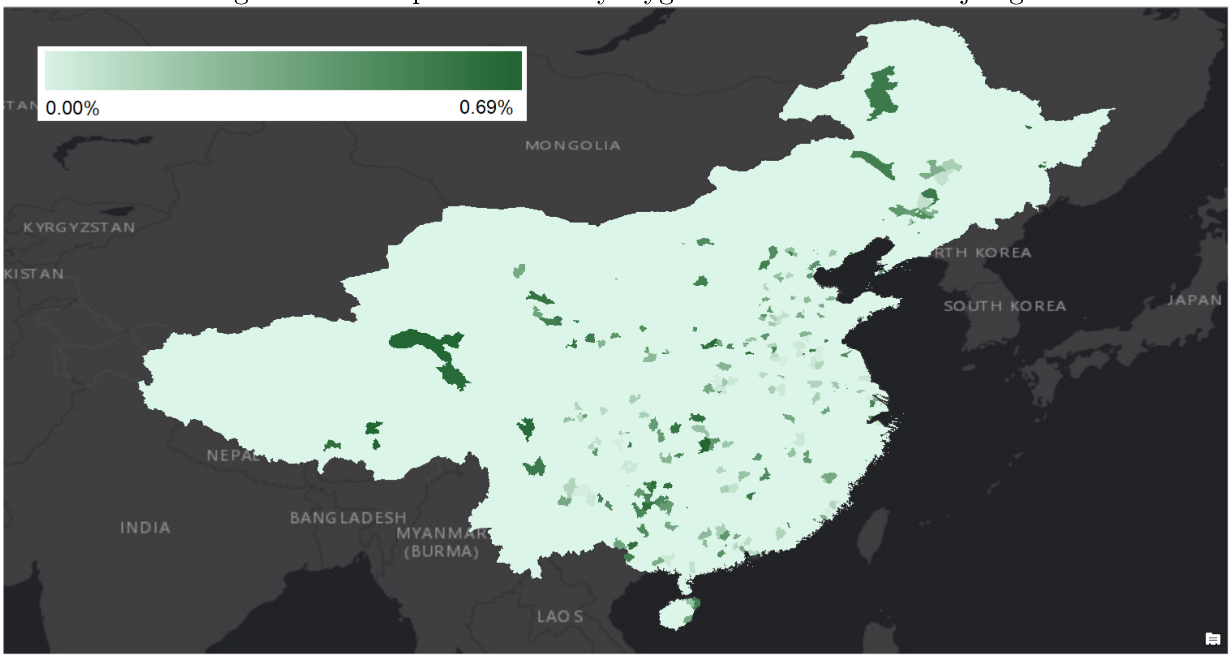


Figure 9: Choropleth of County Uyghur Share Outside Xinjiang



Darker shades of green correspond to counties with a higher share of Uyghur residents.

Figure 10: Year-by-Year Coefficients Plotted with Lag Xinjiang Incidents

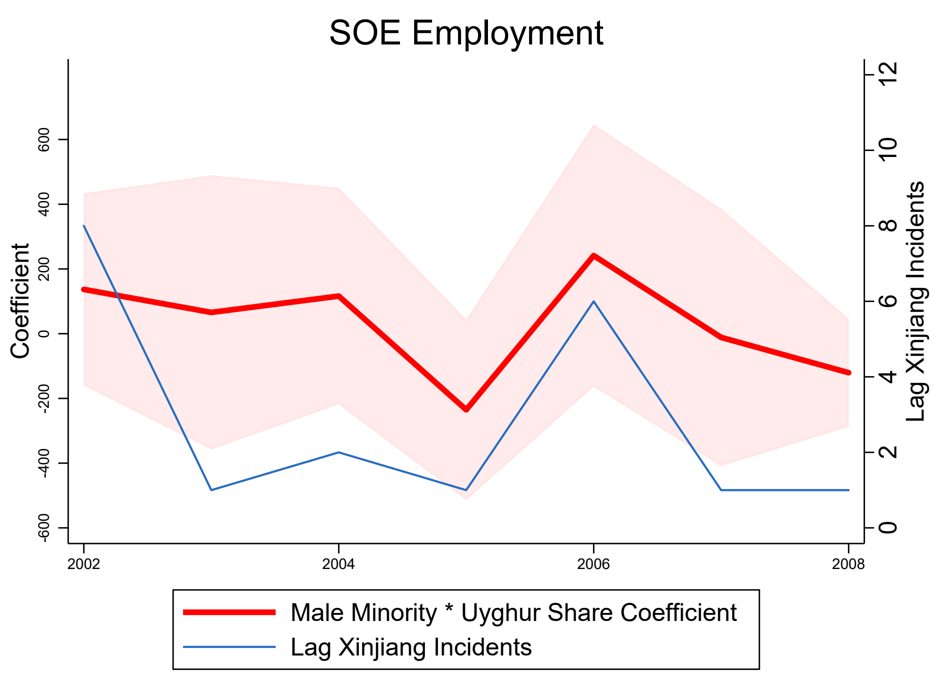


Figure 11: Distribution of Triple Interaction Coefficients from Random Permutation Test

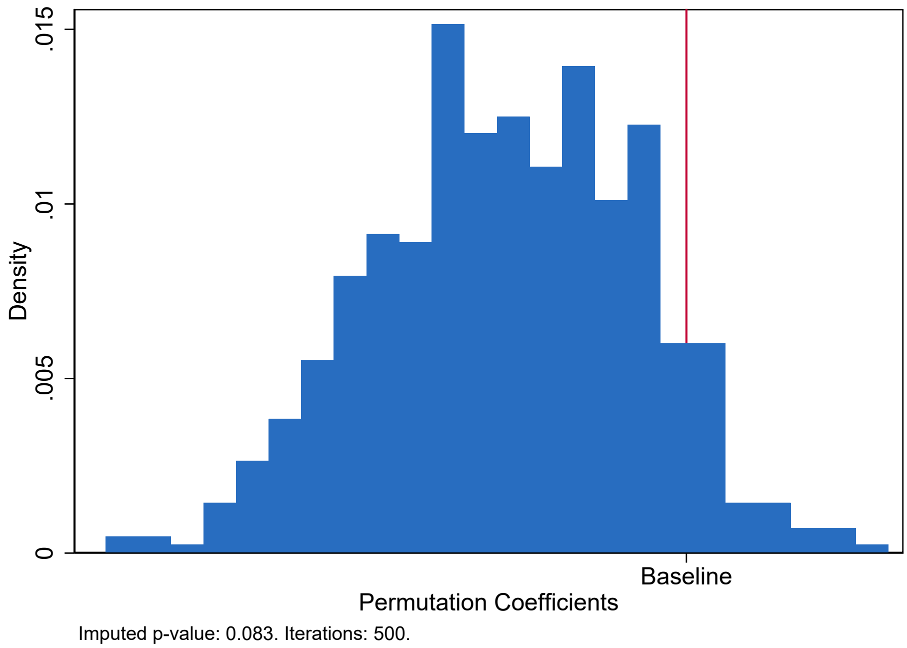


Figure 12: Unrest Shock Impulse Response Functions

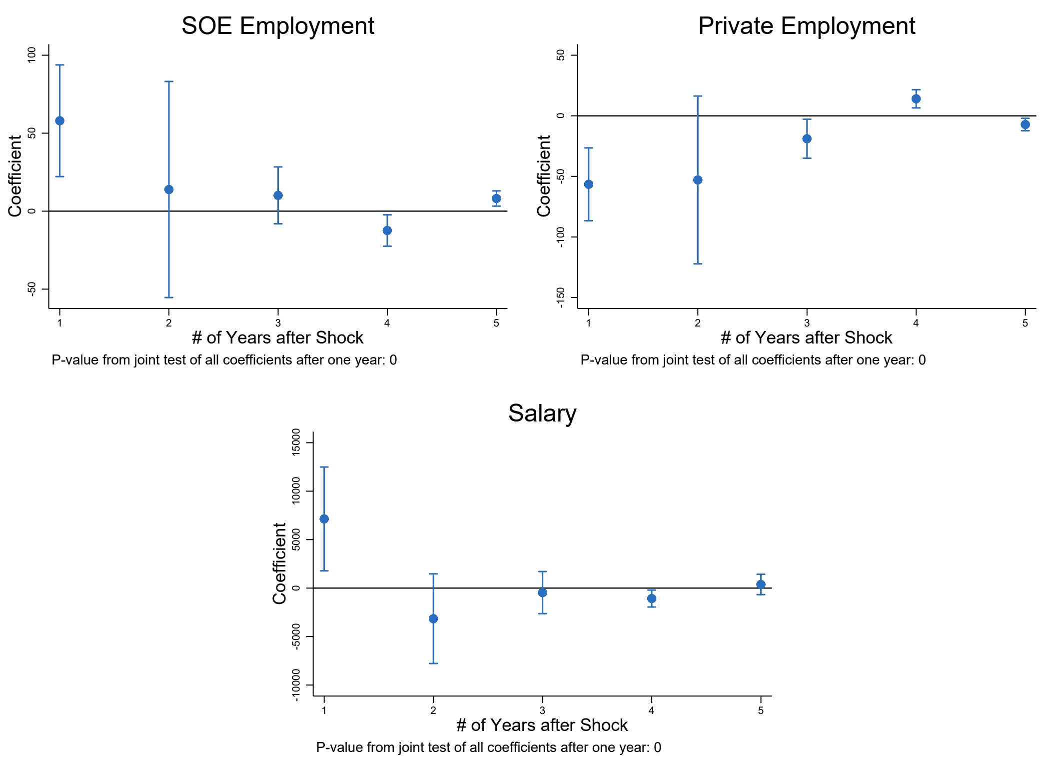


Table 1: SOE vs. Domestic Private Response to Export Demand

	(1)	(2)
Dependent Variable:	SOE	Private
Mean of Dep. Var.	*0.650*	*0.170*
Export Demand Shock (β)	-0.0529***	0.0546**
RF using partner trade demand	(0.0203)	(0.0238)
Observations	346,531	346,531
R-squared	0.217	0.124
P-value for equality in β	(1) vs. (2)	(2) vs. (3)
	0.006	0.137

Controls: Specification includes year FE and county FE. It also includes individual characteristics (age and edu. years), and each of these controls interacted with gender. Standard errors are clustered at the province-year level.

Table 2: SOE vs. Domestic Private Response to Flood Disasters

	(1)	(2)
Dependent Variable:	SOE	Private
Mean of Dep. Var.	*0.550*	*0.250*
Lag County Flood Indicator	0.0778**	-0.0930***
	(0.0361)	(0.0318)
Observations	225,039	225,039
R-squared	0.248	0.166
P-value for equality in β	(1) vs. (2)	(2) vs. (3)
	0.008	0.006

Controls: Specification includes year by district fixed effects and county fixed effects. It also includes fixed effects for individual characteristics (gender, age, minority status, and edu. years). Standard errors are clustered at the county-year level.

Table 3: Demographics of Chinese Political Prisoners

A. Chinese Political Prisoners	Male	Female	
Minority	46.38%	9.29%	55.67%
Han	25.42%	18.91%	44.33%
	71.80%	28.20%	

B. Chinese 2000 Census	Male	Female	
Minority	5.46%	5.39%	10.85%
Han	44.69%	44.46%	89.15%
	50.15%	49.85%	

Notes: Data in Panel A come from the United States Congressional-Executive Committee on China. Data in Panel B come from the 2000 Census of China.

Table 4: Xinjiang Unrest Baseline

	(1)	(2)	(3)
Dependent Variable:	SOE	Private	Salary (000s RMB)
Mean of dependent variable	*0.550*	*0.250*	*45.51*
Coun. Uyg. Share × Lag Xinjiang Incid. × Male Minority (βM)	36.59***	-24.24**	5,422***
	(12.59)	(11.04)	(2,075)
Observations	224,412	224,412	176,962
R-squared	0.231	0.156	0.431

Controls: Specification includes year FE and county FE. It also includes individual characteristics (age, gender, edu. years), and each of these controls interacted with county Uyghur share and lag Xinjiang incidents. The regression also controls for for log kilometers county distance from Xinjiang times year fixed effects, and the interaction of year fixed effects, male minority fixed effects, and the base period average employment share by ownership in each county. Standard errors are clustered at the county level.

Table 5: Robustness: SOE Employment

	(1)	(2)	(3)
Dependent Variable:	SOE	Private	Salary (000s RMB)
Coun. Uyg. Share × Lag Xinjiang Incid.	38.70***	-25.38**	5,892***
× Male Minority (βM)	(13.85)	(11.57)	(2,024)
Control for '02 share in:			
Public services * Year FE * Male Min.	Y	Y	Y
Mining * Year FE * Male Min.	Y	Y	Y
Utilities * Year FE * Male Min.	Y	Y	Y
Observations	224,412	224,412	176,962
R-squared	0.232	0.156	0.435

Controls: Specification includes year FE and county FE. It also includes individual characteristics (age, gender, edu. years), and each of these controls interacted with county Uyghur share and lag Xinjiang incidents. The regression also controls for for log kilometers county distance from Xinjiang times year fixed effects, and the interaction of year fixed effects, male minority fixed effects, and the base period average employment share by ownership in each county. Standard errors are clustered at the county level.

Table 6: Robustness: Drop Incidents Triggered by Events Outside Xinjiang

	(1)	(2)	(3)
Dependent Variable:	SOE	Private	Salary (000s RMB)
Shock without outside-triggered incidents	49.34***	-39.52**	7,051***
× Male Minority (βM)	(17.44)	(17.46)	(2,174)
Observations	224,412	224,412	176,962
R-squared	0.231	0.156	0.431

Controls: Specification includes year FE and county FE. It also includes individual characteristics (age, gender, edu. years), and each of these controls interacted with county Uyghur share and lag Xinjiang incidents. The regression also controls for for log kilometers county distance from Xinjiang times year fixed effects, and the interaction of year fixed effects, male minority fixed effects, and the base period average employment share by ownership in each county. Standard errors are clustered at the county level.

Table 7: Robustness: Drop Incidents Triggered by Economic Events

	(1)	(2)	(3)
Dependent Variable:	SOE	Private	Salary (000s RMB)
Shock without economically-triggered incidents × Male Minority (βM)	60.08***	-46.63**	7,312***
	(19.20)	(18.14)	(2,336)
Observations	224,412	224,412	176,962
R-squared	0.231	0.156	0.431

Controls: Specification includes year FE and county FE. It also includes individual characteristics (age, gender, edu. years), and each of these controls interacted with county Uyghur share and lag Xinjiang incidents. The regression also controls for for log kilometers county distance from Xinjiang times year fixed effects, and the interaction of year fixed effects, male minority fixed effects, and the base period average employment share by ownership in each county. Standard errors are clustered at the county level.

Table 8: Robustness: Logit

	(1)	(2)
Dependent Variable:	SOE	Private
Mean of Dep. Var.	*0.550*	*0.250*
Male Minority Share	*0.0190*	*0.0150*
Coun. Uyg. Share × Lag Xinjiang Incid. × Male Minority (βM)	195.4***	-192.1***
	(73.71)	(68.56)
Observations	224,048	223,832

Controls: Specification includes year FE and county FE. It also includes individual characteristics (age, gender, edu. years), and each of these controls interacted with county Uyghur share and lag Xinjiang incidents. The regression also controls for for log kilometers county distance from Xinjiang times year fixed effects, and the interaction of year fixed effects, male minority fixed effects, and the base period average employment share by ownership in each county. Standard errors are clustered at the county level.

Table 9: Robustness: Drop Outliers

	(1)	(2)	(3)
Dependent Variable:	SOE	Private	Salary (000s RMB)
Coun. Uyg. Share × Lag Xinjiang Incid. × Male Minority (βM)	76.03***	-0.710	2,719**
	(22.22)	(3.446)	(1,071)
Observations	222,035	220,112	172,541
R-squared	0.244	0.176	0.481

This table drops all observations with DFITS greater than 2(k/N)^0.5.*
Controls: Specification includes year FE and county FE. It also includes individual characteristics (age, gender, edu. years), and each of these controls interacted with county Uyghur share and lag Xinjiang incidents. The regression also controls for for log kilometers county distance from Xinjiang times year fixed effects, and the interaction of year fixed effects, male minority fixed effects, and the base period average employment share by ownership in each county. Standard errors are clustered at the county level.

Table 10: Placebo: Lead of Shock

	(1)	(2)	(3)
Dependent Variable:	SOE	Private	Salary (000s RMB)
Coun. Uyg. Share × Lead Xinjiang Incid. × Male Minority (βM)	-16.04	7.605	-2,513
	(13.40)	(7.529)	(1,580)
Observations	224,412	224,412	176,962
R-squared	0.231	0.156	0.431

Controls: Specification includes year FE and county FE. It also includes individual characteristics (age, gender, edu. years), and each of these controls interacted with county Uyghur share and lag Xinjiang incidents. The regression also controls for for log kilometers county distance from Xinjiang times year fixed effects, and the interaction of year fixed effects, male minority fixed effects, and the base period average employment share by ownership in each county. Standard errors are clustered at the county level.

Table 11: Heterogeneity in Response by Sector

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample:	Manufacturing		Mining and Construction		Retail and Transportation		Services	
Dependent Variable:	SOE	Private	SOE	Private	SOE	Private	SOE	Private
Mean of Dep. Var.	*0.560*	*0.440*	*0.710*	*0.290*	*0.350*	*0.650*	*0.750*	*0.250*
Male Minority Share	*0.0170*	*0.0140*	*0.0210*	*0.0230*	*0.0170*	*0.0130*	*0.0220*	*0.0170*
Cty. Uyg. Share × Lag XJ Incid. × Male Minority (β_M)	31.34	-31.34	39.05	-39.05	-66.87	66.87	62.02***	-62.02***
	(53.07)	(53.07)	(65.29)	(65.29)	(79.73)	(79.73)	(12.30)	(12.30)
Observations	36,138	36,138	21,858	21,858	23,426	23,426	72,684	72,684
R-squared	0.375	0.375	0.272	0.272	0.325	0.325	0.220	0.220

Controls: The coefficients for two sectors, agriculture and government, cannot be estimated. Agriculture is very limited in urban settings; the sample in the UHS is too small to use. And the government sector is entirely state-owned, so there is no variation in the outcome variables. Specification includes year FE and county FE. It also includes individual characteristics (age, gender, edu. years), and each of these controls interacted with county Uyghur share and lag Xinjiang incidents. The regression also controls for for log kilometers county distance from Xinjiang times year fixed effects, and the interaction of year fixed effects, male minority fixed effects, and the base period average employment share by ownership in each county. Standard errors are clustered at the county level.

Table 12: Social Relief Income Response by Employment Status

	(1)	(2)	(3)	(4)
Dependent Variable:	Social Relief Transfers			
Sample:	All	SOE	Private	Not Empl.
Mean of Dep. Var.	*18.57*	*1.510*	*1.690*	*1.790*
Mean of Non-Zero Values	*1299*	*1.981*	*2.118*	*2.007*
Count of Non-Zero Values	*3208*	*801*	*1074*	*1333*
Coun. Uyg. Share × Lag Xinjiang Incid. × Male Minority (β_M)	17,507***	6,419**	7,701	88,221**
	(4,703)	(3,042)	(5,733)	(35,632)
Observations	224,412	123,828	55,907	44,677
R-squared	0.017	0.023	0.049	0.045
P-value for equality in β_M		(2) vs. (3)	(2) vs. (4)	(3) vs. (4)
		0.572	0.0211	0.023

Controls: Specification includes year FE and county FE. It also includes individual characteristics (age, gender, edu. years), and each of these controls interacted with county Uyghur share and lag Xinjiang incidents. The regression also controls for for log kilometers county distance from Xinjiang times year fixed effects, and the interaction of year fixed effects, male minority fixed effects, and the base period average employment share by ownership in each county. Standard errors are clustered at the county level.

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In this Appendix, I present additional results to enrich the the main paper. The Online Mathematical Appendix can be found here.

.1 SOEs Exhibit Lower Productivity

Because I argue that the stability role of SOEs has potentially large economic consequences, I first corroborate one fact documented by previous work on Chinese SOEs: that they are significantly less productive than their privately-owned counterparts (Song et al., 2011; Dong and Putterman, 2003; Jefferson et al., 2000). I test these previous assessments using several methods of production function estimation, including those presented in Hsieh and Klenow (2009) (HK), Ackerberg et al. (2015) (ACF), and Gandhi et al. (2011) (GNR). I complement these techniques by computing labor productivity (revenue divided by number of workers) and by estimating a simple OLS regression of revenue on inputs. Each of these alternative methods has specific advantages and drawbacks, so I use all of these methods in conjunction to corroborate one fact: that SOEs are indeed less productive.

Figure A.13 presents density plots of the productivity of Chinese manufacturing firms by ownership: either SOE or domestic private. The firm data come from the Chinese Annual Surveys of Industrial Production, which I describe in detail in Subsection 6.3. The firm productivity measures have been normalized by the sector median and the province median to ensure comparability across places and industries. In each plot, we see that the distribution of SOE productivity is noticeably lower than that of privately-owned domestic firms.

I also estimate these differences using a regression at the firm level. I run the following regression.

$$TFPR_{ipst} = \alpha + \beta SOE_{it} + \tau_t + \eta_p + \gamma_s + \epsilon_{ipst} \tag{36}$$

This regression assumes that firm-year productivity is a function of a constant, an indicator variable for state ownership as defined by official registration, as well as year, province, and sector fixed effects. I cluster the standard errors at the sector level. I do not include firm fixed effects, for if they were included, β would be identified only off of firms that switch ownership type, which are known to be unrepresentative of most firms (Hsieh and Song, 2015). During this time period, firms that switch ownership are mostly privatized SOEs.

Results from this regression are reported in Table A.30. I find that across each of these measures, SOEs are significantly less productive than their domestic counterparts in the same year, province, and industry. This pattern is robust to the inclusion of several additional controls: firm size, labor intensity, and time trends. Together, this evidence strongly suggests that SOEs are indeed less productive than private firms in China, both unconditionally and conditional on observable firm characteristics.

.2 Xinjiang Unrest Shock Appendix

In this subsection, I present additional results relating to the Xinjiang unrest shock.

One robustness check that I perform is to re-define SOE employment in the UHS data. For my baseline regressions, I defined SOEs as officially-registered state-owned firms as well as urban collectives. It may therefore behoove me to eliminate collective firms from this definition and re-run the baseline regressions. I do so in Appendix Table A.25. I find that the SOE and salary results are robust to this drop, but the private triple-interaction coefficient remains negative but becomes imprecisely estimated.

Another robustness check I perform is to omit all controls from the baseline specification except the components of the triple interaction, the distance of each county from Xinjiang interacted with year fixed effects, and county-year fixed effects. The benefit of performing this check is to determine whether the additional controls generate spurious variation or, perhaps, remove useful variation. Appendix Table A.26 shows that the removal of these controls does not affect the signs of the male minority coefficients nor does it heavily affect their precision or magnitude. However, the coefficient for non-male minority SOE employment appears as a small and positive value, and it is precisely estimated at the $p < 0.1$ level. Additionally, non-male minority non-employment also appears to decrease somewhat in response to the shock, when the controls are omitted. I draw two main conclusions from these results: first, the male minority differential effect is not driven by the addition of the flexible pre-period economic controls described in Subsection 5.2. Second, there appear to be unobserved economic changes that are correlated with the double-interaction term that also affect the employment composition of the population as a whole.

Another robustness check addresses potential mismeasurement in the number of Xinjiang incidents per year. The Chinese media environment is aggressively managed by the government, especially for a subject as sensitive as the Xinjiang conflict (Hassid, 2008; Jingrong, 2010). Even though I rely on a combination of domestic and foreign news sources to construct my annual Xinjiang incident count, one still might be concerned that my measure is affected by government censorship or fabrication. As a result, I perform an additional robustness check that removes a dimension of government manipulation. Rather than use a continuous measure of the count of Xinjiang incidents per year, I use instead a binary measure for low-incident and high-incident years. I code all years with one or fewer Xinjiang incidents as a 0, and all years with two or more Xinjiang incidents as a 1. In my sample, this results in four years coded as low conflict, and four years coded as high-conflict. I report the result of using this binary measure in Appendix Table A.27. I find that the broad picture remains similar to that in the baseline results. The male minority triple interaction coefficient for SOE employment remains positive and significant but becomes larger in magnitude. Similarly, the triple interaction coefficient for private employment remains negative and significant but is much larger in magnitude. Interestingly, two coefficients move from imprecision to statistical precision in this specification: SOEs seem to employ fewer non-male minorities in response to the shock, and male minorities seem to leave non-employment.

In principle, my unrest shock varies at the county and year level. Hence, it may be preferable to check that my results are robust to using two-way clustered standard errors, using counties and years as the units of clustering. I re-estimate the standard errors of the baseline specification in this way and report the results in Appendix Table A.28. I find that the SOE coefficient remains precise, but the private and salary results are no longer precisely different from zero. However, I interpret these results with caution because my dataset has only 8 years, and small numbers of clusters can yield incorrect standard errors (Cameron et al., 2011).

I also provide evidence that the differential response of SOE employment is unique to minority men and does not exist for minority women. To do so, I re-estimate a slightly different version of the baseline specification. Instead of an indicator for male minorities, I use instead an indicator for minority. I then run separate regressions for men and women. The results are presented in Appendix Table A.29. I find that the differential response in SOE employment and private employment exists only for minority men. The interaction coefficient β_M is not precisely different from zero in any of the three regressions for women.

I also observe an interesting correlation in the data. I find that the per capita GDP of provinces is strongly negatively correlated with SOE employment share at the province level. I plot this relationship in Appendix Figure A.15, using data from the Chinese Statistical Yearbooks for both measures. Two straightforward interpretations of this correlation are consistent with my theory. First, such a pattern would emerge if SOEs were less productive than their private counterparts. Second, such a pattern could also be generated if SOEs were performing geographic redistribution in China as part of their hypothesized stability role. Of course, this correlation has many alternative interpretations, and is not intended to be definitive evidence of my theory.

Figure A.13: Firm Productivity by Ownership

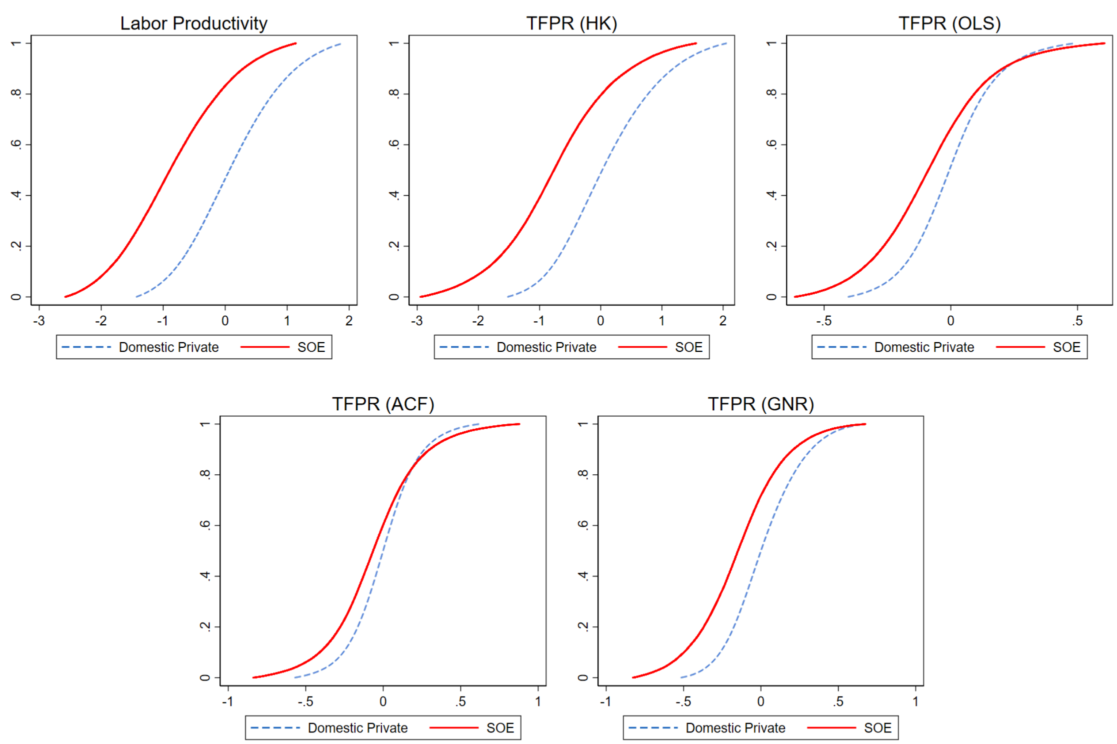

Figure A.15: Cross-province Relationship between GDP and SOE Share

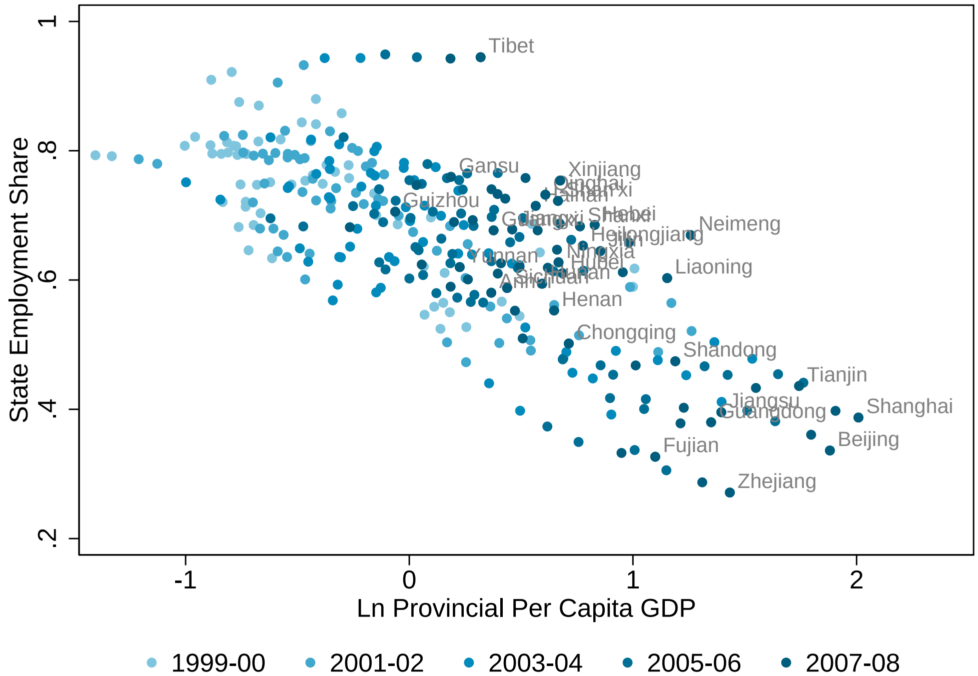

Figure A.14: Share of GDP covered by the *Annual Survey of Industrial Production*

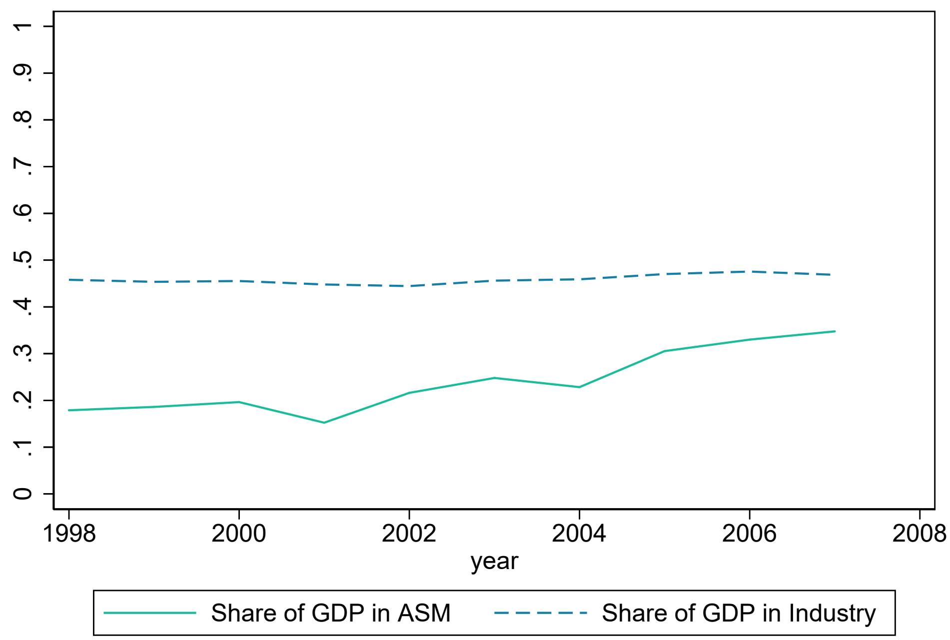

Table A.13: Trade Shock: Sector Shares * Year FE

	(1)	(2)
Dependent Variable:	SOE	Private
Mean of Dep. Var.	*0.650*	*0.170*
Export Demand Shock (β)	-0.0447**	0.0480***
RF using partner trade demand	(0.0172)	(0.0175)
Observations	346,531	346,531
R-squared	0.218	0.125

Controls: Specification includes year FE and county FE. It also includes individual characteristics (age and edu. years), and each of these controls interacted with gender. These regressions also control for the share of each sector within each county for the first year the county appears in the dataset. Those county-specific sector shares are then interacted with year FE. Standard errors are clustered at the province-year level.

Table A.14: Trade Shock: only sectors with <5% China trade

	(1)	(2)
Dependent Variable:	SOE	Private
Mean of Dep. Var.	*0.650*	*0.170*
Demand Shock Exposure	-0.0516**	0.0535**
RF using partner trade demand	(0.0208)	(0.0249)
Observations	346,531	346,531
R-squared	0.217	0.124
P-value for equality in β	(1) vs. (2)	(2) vs. (3)
	0.012	0.160

Controls: Specification includes year FE and county FE. It also includes individual characteristics (age and edu. years), and each of these controls interacted with gender. Standard errors are clustered at the province-year level.

Table A.15: Trade Shock: Placebo

	(1)	(2)
Dependent Variable:	SOE	Private
Mean of Dep. Var.	*0.660*	*0.160*
Demand Shock Exposure	-0.0486	0.0267
RF using partner trade demand	(0.0493)	(0.0393)
Observations	291,203	291,203
R-squared	0.214	0.111
P-value for equality in β	(1) vs. (2)	(2) vs. (3)
	0.391	0.887

Controls: Specification includes year FE and county FE. It also includes individual characteristics (age and edu. years), and each of these controls interacted with gender. Standard errors are clustered at the province-year level.

Table A.16: Trade Shock: by Gender

	(1)	(2)	(1)	(2)
	Male		Female	
Dependent Variable:	SOE	Private	SOE	Private
Mean of Dep. Var.	*0.700*	*0.170*	*0.590*	*0.180*
Export Demand Shock (β)	-0.0517**	0.0628***	-0.0537**	0.0460*
RF using partner trade dem	(0.0218)	(0.0236)	(0.0228)	(0.0245)
Observations	180,940	180,940	165,590	165,590
R-squared	0.191	0.123	0.236	0.133

Controls: Specification includes year FE and county FE. It also includes individual characteristics (age and edu. years), and each of these controls interacted with gender. Standard errors are clustered at the province-year level.

Table A.17: Trade Shock: by Male Minority

	(1)	(2)	(1)	(2)
	Male Minority		Non-Male Minority	
Dependent Variable:	SOE	Private	SOE	Private
Mean of Dep. Var.	*0.680*	*0.170*	*0.620*	*0.190*
Lag County Flood Indicator	-0.186**	0.144***	-0.0584**	0.0613**
	(0.0739)	(0.0514)	(0.0223)	(0.0246)
Observations	4,589	4,589	296,416	296,416
R-squared	0.286	0.214	0.210	0.119

Controls: Specification includes year FE and county FE. It also includes individual characteristics (age and edu. years), and each of these controls interacted with gender. Standard errors are clustered at the province-year level.

Table A.18: Flood Results: Control for Base Year Sector Shares

	(1)	(2)
Dependent Variable:	SOE	Private
Mean of Dep. Var.	*0.650*	*0.170*
Export Demand Shock (β)	-0.0447**	0.0480***
RF using partner trade demand	(0.0172)	(0.0175)
Observations	346,531	346,531
R-squared	0.218	0.125

Controls: Specification includes year FE and county FE. It also includes individual characteristics (age and edu. years), and each of these controls interacted with gender. These regressions also control for the share of each sector within each county for the first year the county appears in the dataset. Those county-specific sector shares are then interacted with year FE. Standard errors are clustered at the province-year level.

Table A.19: Flood Results: Placebo

	(1)	(2)
Dependent Variable:	SOE	Private
Mean of Dep. Var.	*0.550*	*0.250*
Lead County Flood Indicator	0.0381	-0.0210
	(0.0349)	(0.0394)
Observations	225,039	225,039
R-squared	0.248	0.166
P-value for equality in β	(1) vs. (2)	(2) vs. (3)
	0.397	0.943

Controls: Specification includes year by district fixed effects and county fixed effects. It also includes fixed effects for individual characteristics (gender, age, minority status, and edu. years). Standard errors are clustered at the county-year level.

Table A.20: Flood Results by Gender

	(1)	(2)	(1)	(2)
	Male		Female	
Dependent Variable:	SOE	Private	SOE	Private
Mean of Dep. Var.	*0.730*	*0.170*	*0.640*	*0.160*
Lag County Flood Indicator	0.0358**	-0.0344*	0.000100	-0.0174
	(0.0174)	(0.0198)	(0.0201)	(0.0180)
Observations	199,196	199,196	188,245	188,245
R-squared	0.263	0.192	0.348	0.217

Controls: Specification includes year FE and county FE. It also includes individual characteristics (age and edu. years), and each of these controls interacted with gender. Standard errors are clustered at the province-year level.

Table A.21: Flood Results by Male Minority

	(1)	(2)	(1)	(2)
	Male Minority		Non-Male Minority	
Dependent Variable:	SOE	Private	SOE	Private
Mean of Dep. Var.	*0.640*	*0.220*	*0.540*	*0.250*
Lag County Flood Indicator	0.0731	-0.135***	0.0148	-0.0228
	(0.0769)	(0.0492)	(0.0166)	(0.0172)
Observations	3,777	3,777	221,203	221,203
R-squared	0.315	0.241	0.241	0.159

Controls: Specification includes year FE and county FE. It also includes individual characteristics (age and edu. years), and each of these controls interacted with gender. Standard errors are clustered at the province-year level.

Table A.22: County Flood Regressions

	(1)	(2)	(3)	(4)
Dependent Variable:	Total GDP	Agricuture and Extraction	Manufacturing	Exports
Mean of Dep. Var.	*94.34*	*14.08*	*46.27*	*312.9*
Shock Coefficient (β)	-5.948	-1.171**	-3.543	-76.94
	(5.727)	(0.500)	(3.367)	(49.86)
Data structure		County-year		
Units		100 millions RMB		Millions USD
Observations	20,478	20,667	20,627	4,440
R-squared	0.656	0.811	0.652	0.435

Notes: Specification includes year FE and county FE. Standard errors are clustered at the county level.

Table A.23: Robustness: Drop Strategic Sectors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample:	Drop Public Administration			Drop Mining			Drop Utilities		
Dependent Variable:	SOE	Private	Salary	SOE	Private	Salary	SOE	Private	Salary
Cty. Uyg. Share × Lag XJ Incid. × Male Minority (β_M)	31.97*	-24.89**	7,167***	38.47***	-28.87**	5,778***	34.46***	-22.19**	5,618**
	(16.46)	(9.987)	(2,648)	(11.63)	(12.45)	(2,019)	(12.91)	(10.91)	(2,189)
Observations	202,997	202,997	158,614	206,353	206,353	160,863	217,490	217,490	170,178
R-squared	0.229	0.155	0.433	0.244	0.163	0.435	0.234	0.158	0.433

Controls: Specification includes year FE and county FE. It also includes individual characteristics (age, gender, edu. years), and each of these controls interacted with county Uyghur share and lag Xinjiang incidents. The regression also controls for for log kilometers county distance from Xinjiang times year fixed effects, and the interaction of year fixed effects, male minority fixed effects, and the base period average employment share by ownership in each county. Standard errors are clustered at the county level.

Table A.24: Robustness: Winsorize Uyghur Share Variable

	(1)	(2)	(3)
Dependent Variable:	SOE	Private	Salary (000s RMB)
Coun. Uyg. Share × Lag Xinjiang Incid. × Male Minority (βM)	40.97***	-26.32**	-14.65
	(13.21)	(10.64)	(14.69)
Observations	224,412	224,412	224,412
R-squared	0.231	0.156	0.196

This table replicates the baseline analysis using a winsorized county Uyghur Share variable. The variable is winsorized at the 95th percentile.
Controls: Specification includes year FE and county FE. It also includes individual characteristics (age, gender, edu. years), and each of these controls interacted with county Uyghur share and lag Xinjiang incidents. The regression also controls for for log kilometers county distance from Xinjiang times year fixed effects, and the interaction of year fixed effects, male minority fixed effects, and the base period average employment share by ownership in each county. Standard errors are clustered at the county level.

Table A.25: Robustness: Omit Collective Firms

	(1)	(2)	(3)
Dependent Variable:	SOE	Private	Salary (000s RMB)
Coun. Uyg. Share × Lag Xinjiang Incid. × Male Minority (βM)	25.87**	-18.20	5,429**
	(10.99)	(15.69)	(2,193)
Observations	212,650	212,650	165,200
R-squared	0.258	0.167	0.430

Controls: Specification includes year FE and county FE. It also includes individual characteristics (age, gender, edu. years), and each of these controls interacted with county Uyghur share and lag Xinjiang incidents. The regression also controls for for log kilometers county distance from Xinjiang times year fixed effects, and the interaction of year fixed effects, male minority fixed effects, and the base period average employment share by ownership in each county. Standard errors are clustered at the county level.

Table A.26: Robustness: Sparse Specification

	(1)	(2)	(3)
Dependent Variable:	SOE	Private	Not Employed
Mean of Dep. Var.	*0.550*	*0.250*	*0.200*
Coun. Uyg. Share × Lag Xinjiang Incid. × Male Minority (βM)	37.44***	-37.29***	-0.150
	(11.86)	(14.11)	(14.41)
Observations	231,696	231,696	231,696
R-squared	0.102	0.095	0.035

Controls: Specification includes year FE and county FE. I also control for log kilometers county distance from Xinjiang times year fixed effects. Standard errors are clustered at the county level.

Table A.27: Robustness: Binary Measure of Xinjiang Conflict Intensity

	(1)	(2)	(3)
Dependent Variable:	SOE	Private	Salary (000s RMB)
Coun. Uyg. Share × Binary Lag Xinjiang Incid. × Male Minority (βM)	181.7***	-91.49**	23,939**
	(51.42)	(44.04)	(11,923)
Observations	224,412	224,412	176,962
R-squared	0.231	0.156	0.431

Controls: Specification includes year FE and county FE. It also includes individual characteristics (age, gender, edu. years), and each of these controls interacted with county Uyghur share and lag Xinjiang incidents. The regression also controls for for log kilometers county distance from Xinjiang times year fixed effects, and the interaction of year fixed effects, male minority fixed effects, and the base period average employment share by ownership in each county. Standard errors are clustered at the county level.

Table A.28: Robustness: Two-Way Clustered Standard Errors

	(1)	(2)	(3)
Dependent Variable:	SOE	Private	Not Employed
Mean of Dep. Var.	*0.550*	*0.250*	*45.51*
Coun. Uyg. Share × Lag Xinjiang Incid.	36.59**	-24.24+	5,422*
× Male Minority (βM)	-11.86	-15.06	(3,080)
Observations	224,353	224,353	176,907
R-squared	0.231	0.156	0.431

Controls: Specification includes year FE and county FE. It also includes individual characteristics (age, gender, edu. years), and each of these controls interacted with county Uyghur share and lag Xinjiang incidents. The regression also controls for for log kilometers county distance from Xinjiang times year fixed effects, and the interaction of year fixed effects, male minority fixed effects, and the base period average employment share by ownership in each county. Standard errors are clustered at the county level. I present two-way clustered standard errors using counties and years. P-values computed based on one-tailed tests.

Table A.29: Baseline by Gender

	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	Men			Women		
Dependent Variable:	SOE	Private	Salary (000s RMB)	SOE	Private	Salary (000s RMB)
Coun. Uyg. Share × Lag Xinjiang Incid.	36.25***	-22.49*	5,350**	2.741	-8.842	272.4
× Minority (βM)	(12.21)	(12.35)	(2,081)	(13.15)	(10.01)	(1,246)
Observations	116,239	116,239	98,737	108,173	108,173	78,225
R-squared	0.204	0.146	0.440	0.276	0.191	0.429

Controls: Specification includes year FE and county FE. It also includes individual characteristics (age, gender, edu. years), and each of these controls interacted with county Uyghur share and lag Xinjiang incidents. The regression also controls for for log kilometers county distance from Xinjiang times year fixed effects, and the interaction of year fixed effects, male minority fixed effects, and the base period average employment share by ownership in each county. Standard errors are clustered at the county level.

Table A.30: SOE vs. Domestic Private Manufacturing Productivity

	(1)	(2)	(3)	(4)	(5)
Dependent Variable:	Labor Productivity	TFPR (HK)	TFPR (OLS)	TFPR (ACF)	TFPR (GNR)
Mean of dependent variable	*5.320*	*0.440*	*2.060*	*0.570*	*3.880*
S.D. of dependent variable	*0.990*	*1.030*	*0.490*	*1.220*	*1.370*
Indicator for SOE	-0.978***	-0.857***	-0.0867***	-0.0657**	-0.161***
	(0.117)	(0.0567)	(0.0215)	(0.0244)	(0.0256)
Observations	781,504	781,504	781,504	781,504	499,283
R-squared	0.249	0.132	0.688	0.874	0.857

Controls: Specification includes year FE, province FE, and four-digit Chinese Industrial Code FE. Standard errors are clustered at the industrial code level. Data come from the Annual Survey of Industrial Production, 1998-2008.