STATE EMPLOYMENT AS A STRATEGY OF AUTOCRATIC CONTROL IN CHINA*

Jaya Y. Wen†

January 31, 2022

Abstract

This paper presents novel evidence that public employment is a means of autocratic control by demonstrating that China uses state jobs to pacify social unrest. I use variation in a regional conflict between Uyghur separatists and the government to establish that, in years and counties with a higher threat of unrest spillover, state-owned firms hire more male minorities, the demographic most likely to participate in ethnic conflict. Concurrently, male minority wages rise and private firms hire fewer members of this group. These patterns are consistent with a model of government-subsidized, pacification-motivated state employment, and a model-derived quantification exercise suggests that state firms implicitly receive a 26% subsidy on male minority wages. Furthermore, I find that state employment increases after natural disasters and poor trade shocks, suggesting that these jobs also play a general role in preventing unrest.

Keywords: Ethnic conflict, autocracy, public employment

JEL codes: O12, P26, D74, J45, J15

*I am grateful to my advisers Mushfiq Mobarak, Mark Rosenzweig, Nancy Qian, and Chris Udry for their guidance and support. Thanks also to Taha Choukhmane, Gaurav Chiplunkar, Meredith Startz, Jeff Weaver, Sharat Ganapati, Hannah Luk-Zilberman, Jakob Schneebacher, Martin Mattson, Ro'ee Levy, Dan Keniston, Nicholas Ryan, Tim Guinnane, Gerard Padro i Miquel, Fabrizio Zilibotti, Rafael di Tella, and participants at Yale Development Economics Seminar, the Northwestern Development Economics Seminar, the MIT-Harvard Joint Development Economics Seminar, the Stanford Political Economy Seminar, and the UC Berkeley Political Economy Seminar for their comments and suggestions. Financial support for this project was generously provided by the National Science Foundation Graduate Research Fellowship, the Yale Economic Growth Center, and the Sylff Foundation Research Grant.

†Harvard Business School. Email: jwen@hbs.edu

1 Introduction

Autocratic governments face two types of threats to their rule: those that arise from within the regime, like *coups d'etat*, and those that arise from without, like protests and revolutions (Egorov and Sonin, 2020). How do autocrats maintain control in the face of these internal and external threats? A rich theoretical literature has explored a variety of policies of autocratic control, including those that manipulate the information environment, award targeted benefits, and repress political participation.[1] When applied to internal threats, such policies may take the form of information withholding, patronage, and ideological purges, while for external threats, they may manifest as propaganda or censorship, welfare programs, and violent persecution.[2]

A much smaller literature documents these strategies empirically. Several papers show that autocrats manage information flows using propaganda (Adena et al., 2015; Yanagizawa-Drott, 2014), censorship (King et al., 2014; Rozenas and Stukal, 2019), and local elections (Martinez-Bravo et al., 2017). Jia et al. (2015) and Xu (2018) find that autocrats reward connected loyalists with jobs and rents. And Markevich et al. (2021) document that the Soviet leadership concentrated famine deprivation on ethnic Ukrainian areas. There is, however, no direct empirical evidence that autocrats use targeted benefits to mitigate external threats. This gap is especially conspicuous given indirect evidence suggesting an "autocratic bargain", in which citizens receive economic compensation in exchange for political freedoms. For example, Desai et al. (2009) uncover a negative association between political liberalization and welfare spending, Caselli and Tesei (2016) show that sudden windfalls entrench autocratic rule, and Brückner and Ciccone (2011) find that poor rainfall

[1] Egorov and Sonin (2020) and Gehlbach et al. (2016) provide surveys of theoretical work on non-democratic regimes. Egorov and Sonin (2020) focuses strategies of authoritarian control, particularly information management, while Gehlbach et al. (2016) focuses on theories of autocratic institutions.

[2] Gehlbach and Sonin (2014) and Guriev and Treisman (2020) model information withholding among governing elites, de Mesquita Bruce et al. (2003) model the logic of patronage in autocracies, and Montagnes and Wolton (2019) model how mass purges within a bureaucracy are a means of top-down accountability. Gehlbach and Sonin (2014) and Lorentzen (2014) present models of propaganda and censorship, Powell (2013) theorizes when autocrats will buy off opposition via rent-sharing, and Acemoglu and Robinson (2006); Rozenas (2020); Dagaev et al. (2019); Esteban et al. (2015); and Gregory et al. (2011) present models of disenfranchisement, repression, and mass killings.

shocks temporarily destabilize autocratic regimes.

This paper provides the first causal, empirical evidence that autocrats implement targeted benefit policies to pacify external threats. Specifically, I demonstrate that the Chinese government uses jobs in state-owned firms to quell Uyghur ethnic unrest, allocating jobs to minority men during high-risk periods. This result demonstrates how autocrats manage ethnic conflict, a pervasive and severe type of external threat (Esteban et al., 2012). Because ethnicity is generally observable and difficult to change, it can facilitate coordination in inter-group conflict (Caselli and Coleman, 2013; Esteban and Ray, 2008). This coordination is particularly problematic for autocracies, as it may help groups overcome the collective action problems that typically bedevil protests and revolutions (Egorov and Sonin, 2020). At the same time, autocrats can target pacification policies, both repression and benefits, by ethnicity, though the literature has to date focused on repression (Rozenas, 2020; Fox and Sandler, 2003).

China is a useful setting in which to study this question for two reasons. First, it is home to the Xinjiang conflict, a decades-long separatist conflict in Xinjiang, China's westernmost province. Separatists, over 85% of whom are male ethnic Uyghurs (Congressional-Executive Commission on China, 2019), cite discriminatory and oppressive policies as motives. In recent years, the government has escalated policy interventions substantially, drawing international attention and condemnation (The Economist, 2021; Buckley and Ramzy, 2020; Fifield, 2020).

At the same time, targeted, large-scale employment programs are feasible given China's high state capacity. The widespread presence of public employment in the form of state-owned enterprises (SOEs) provides a natural framework in which to implement such programs: since 2006, they have employed one-fifth of the urban workforce, about 65 million people (The National Bureau of Statistics, 2018). In this paper, I use the term "state employment" to distinguish SOE employees from other types of public workers, like civil servants. The use of state employment to prevent unrest has potentially large economic implications and may contribute to the Chinese government's continued support of SOEs, despite their low productivity and myriad calls for reform.

Though much work has hypothesized that SOEs serve social stability, none has demonstrated that the government intentionally allocates state employment to quell unrest. Empirically documenting such a fact is challenging for three key reasons. First, omitted variables may drive both social unrest and employment, and dramatic changes to China's economy during the period of study (2002 - 2009) provide ample candidates. Second, reverse causality is an issue: while state employment may respond to social unrest, unrest may simultaneously respond to state employment for the very reasons that make it a plausible pacification policy. Third, unrest may directly alter production - for example, by destroying capital or disrupting transportation - and change state employment through direct, apolitical channels.

This paper addresses these challenges and fills this evidentiary gap by leveraging a novel natural experiment using variation in the ongoing Uyghur ethnic conflict in Xinjiang. I design a triple-differences strategy that combines temporal variation in the intensity of the conflict in Xinjiang, geographic variation in the location of Uyghur diaspora outside Xinjiang, and individual variation in male minority status. The sample omits Xinjiang province, a step essential for causal identification[3]. The first two elements of the triple interaction capture the fact that conflict spillover is more likely in years preceded by many Xinjiang unrest incidents, in non-Xinjiang counties with large Uyghur population shares. The third element captures the fact that a government with limited resources would target pacification policies on the likeliest participants: male minorities.

The triple-differences approach addresses omitted variables, the first empirical challenge, by comparing the employment outcomes of male minorities to those of the general population, thus differencing out many aggregate shocks to China's economy during the period of study (2000-2009), like fiscal programs, privatization reforms, and trade agreements that comparably affected male minorities and other demographics. The approach addresses reverse causality, the second empirical challenge, by omitting Xinjiang province from the sample of study and focusing on how employment in the rest of China responds to

[3]Furthermore, including Xinjiang is not possible due to data constraints, as I discuss in Subsection 4.1.

conflict in Xinjiang. Thus, I consider only how employment reacts to conflict threats generated elsewhere and avoid the entanglement between local unrest and local labor markets. Finally, this approach addresses the third empirical challenge, direct production effects, by relying on unrealized threats of unrest. A feature of the Uyghur conflict during 2002 - 2009 is that the spillover threats do not generate actual unrest, so measured employment responses do not capture the direct production consequences of violence.[4]

Because increased state labor demand should also have equilibrium consequences for private employment and wages, I use a model to generate additional labor market predictions and to provide a means of quantifying the pacification motive. The model produces three empirically-testable comparative statics that should hold if the government uses state employment to pacify unrest threats. First, when the unrest threat rises, state employment of unrest-prone demographics should increase relative to the general population. Second, increased state demand for unrest-prone workers should drive up their relative wages. Finally, these higher wages should differentially depress private employment of unrest-prone groups.

I test these predictions using the triple-differences strategy and an original dataset of Uyghur conflict events and China's Urban Household Survey (UHS). In line with model predictions, I find that male minority state employment increases in response to the unrest shock, private employment decreases, and male minority wages increase. The size of the state employment response, for a one-standard deviation increase in county Uyghur population share at the median conflict intensity, represents a 0.48 percentage-point increase in the probability of state employment. In an average county, this value represents 22.6% of male minority state employment.

These 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 occupations, and county-specific industry composition, I control for county-specific sector shares interacted with year and

[4]However, the conflict does spill over with the expected pattern in previous time periods, lending credence to the assertion that the interaction captures risk of contagion. I discuss this evidence in Subsection 4.1.

demographic fixed effects. To address the possibility that unrest incidents may be sparked by economic shocks or events outside of Xinjiang, I use qualitative evidence to code the proximate trigger for each incident and repeat the tests 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 when using 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 the baseline coefficients are larger than 94.9% of coefficients computed using counterfactual Uyghur population data.

I enrich the baseline results by testing whether the government uses other policies in conjunction with SOE employment to address unrest threats. I find that *ad hoc* social relief transfers also increase in response to the Xinjiang unrest shock – but only for male minorities. Additionally, 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 prevent unrest.

Furthermore, I use a model-generated sufficient statistic and find that Chinese state firms implicitly receive a 26% subsidy on male minority employment. This value is large but comparable to targeted wage subsidies in other contexts. For example, mid-2000s Hungarian payroll subsidies for hiring the long-term unemployed represented 14-25% total wages (Cseres-Gergely et al., 2015), and a 2006 Finnish wage subsidy for low-wage workers represented approximately 16% of gross worker income (Huttunen et al., 2013).

Finally, I present two empirical patterns that suggest state employment plays a general stabilizing role beyond the context of ethnic separatism. First, employment in private firms falls in times and places with poor export demand, while employment in state-owned firms increases. Second, while private firms shed labor in the year following a flood disaster, state-owned firms hire more labor. These patterns suggest that state employment counterbalances negative shocks that may foment unrest, even outside of the context of ethnic conflict.

This paper contributes to several literatures. First, it fills a key gap in the literature on autocratic control by providing the first direct empirical evidence that regimes use public employment to quell external threats of unrest. The existing literature on how non-democratic regimes remain in power is predominantly theoretical, and strategies of control broadly fall into three categories: information manipulation, benefit targeting, and repression. Within the first category, models show how propaganda and censorship can be used to manage internal threats from elites (Guriev and Treisman, 2020) as well as external threats from the people (Gehlbach and Sonin, 2014; Lorentzen, 2014). Benefit targeting can take the form of patronage among the ruling class (de Mesquita Bruce et al., 2003) and rent-sharing with violent opposition (Powell, 2013), and is also implicit in models of collective action via the existence of potentially malleable outside options (De Mesquita, 2010; Shadmehr, 2018; Egorov and Sonin, 2020). Finally, models of repression show how purges among the governing class Montagnes and Wolton (2019) and oppression of the citizenry Acemoglu and Robinson (2006); Gregory et al. (2011); Esteban et al. (2015); Tyson (2018); Dagaev et al. (2019); Rozenas (2020) both serve to deprive potential threats of political clout.

A smaller empirical literature tests whether, and how, autocrats use these policies in practice. Within information strategies, Adena et al. (2015); Yanagizawa-Drott (2014) document that propaganda reinforced state-led violence campaigns in Nazi Germany and 1990s Rwanda, Rozenas and Stukal (2019); King et al. (2014) show how censorship takes place in Russia and China, and Martinez-Bravo et al. (2017) find that local elections in China were used as a means of collecting information on local leaders and a substitute for bureaucratic capacity. Among targeted benefit strategies, Jia et al. (2015); Xu (2018) describe patronage systems in China and the British Empire. And finally, empirical work on repression includes Markevich et al. (2021), which documents how the Soviet Union targeted food deprivation on ethnic Ukrainian places during a famine, consistent with a desire to suppress them politically. I contribute to this scholarship by studying another policy of autocratic control, public employment, and providing causal evidence that it is targeted on the groups posing external threats to the government. Moreover, this paper is a microeconomic complement to work that finds an association between spending on state social programs and autocratic rule Desai et al. (2009); Caselli and Tesei (2016); Brückner and Ciccone (2011).

Second, this paper adds to work studying the determinants of and policy responses to intrastate conflict. A large part of this literature maps the determinants of civil conflicts, both economic, like poverty and loss of labor income (Berman and Couttenier, 2015; Bazzi and Blattman, 2014; Miguel et al., 2004; Dube and Vargas, 2013; Hegre and Sambanis, 2006; Collier and Hoeffler, 2004; Fearon and Laitin, 2003; Collier and Hoeffler, 1998), and demographic, like ethnic polarization and male-skewed populations (Edlund et al., 2013; Esteban et al., 2012; Urdal, 2004; Montalvo and Reynal-Querol, 2005; Horowitz, 1985). While such research has strong policy implications - for example, social safety nets that prevent sudden income declines may mitigate internal conflict - relatively few studies analyze pacification policies directly. Blattman and Annan (2016) use a field experiment to document how job training and cash transfers pull high-risk men out of violent activities in Liberia, Fetzer (2014) shows how India's national workfare program, NREGA, decreased Maoist fighting, Beath et al. (2017) find that a development program reduced insurgent violence in Afghanistan, and Crost et al. (2016) conclude that conditional cash transfers reduced local conflict activity in the Philippines. However, these studies do not establish that governments dynamically use these policies to mitigate unrest. A central contribution of this paper is therefore to provide the first causal evidence that governments intentionally respond to unrest threats with economic policies.

Third, this paper highlights a new force behind the allocation of public employment. In many settings, public sector jobs are not awarded to individuals according to ability alone (Finan et al., 2017; Weaver, 2021). Instead, leaders may use a system of patronage, granting public jobs to political allies in exchange for loyalty or votes (Colonnelli et al., 2020; Xu, 2018; Brollo et al., 2017; Ornaghi, 2016; Anderson et al., 2015) . These existing studies focus on how public jobs can shift individuals over the margin of support for a leader. However, another margin is essential: that of unrest participation. This paper is the first to demonstrate that governments allocate public jobs not just to secure support from loyalists or swing voters, but also to entrenched opponents in an effort to prevent violent unrest. That is, for autocracies, public employment may be used to control both internal and external threats.

The rest of the paper proceeds as follows. Section 2 provides historical and institutional context. Section 3 introduces a conceptual framework that generates empirical predictions. Section 4 describes the empirical strategy with which I test model predictions. Section 5 introduces the data used for analysis. Section 6 presents results on how labor market outcomes repond to ethnic unrest threats, and Section 7 presents additional results on employment responses to floods and trade shocks. Section 8 concludes.

2 Background

2.1 State-Owned Enterprises in China

This subsection presents the recent history of Chinese SOE productivity and reform. A robust literature 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[5]. This fact has shaped the current consensus view of SOEs: they are inefficient behemoths, recipients of undue government favoritism, and in need of 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).

At the same time, a central policy priority of the Chinese government in the last half-

[5] I corroborate these results using multiple productivity estimation techniques in Online Appendix Subsection 9.4.

century has been economic growth. Deng Xiaoping, paramount 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 to 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). Until 2020, China was 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 aggressive 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.[6] The government did appear genuinely committed in the early years of reform. 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). In 1999, the Communist Party Central Committee announced that changes would follow the principle of "[g]rasping the large, letting go of the small" (Hsieh and Song, 2015). But reforms stalled in subsequent years. Appendix Figure A.1 vividly demonstrates the deceleration. Urban SOE employment decreased markedly for a few years following 1997, but since 2006 has remained stagnant at approximately 65 million people, comparable to the entire population of France. Why is the Chinese government, preoccupied as it is with economic growth, so reluctant to engage in further SOE rollbacks? This paper argues that SOEs persist because they offer an essential political benefit: unrest pacification.

[6]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).

2.2 Employment as a Pacification Policy

This subsection discusses how state employment offers particular advantages for pacifying unrest, 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 prevent unrest is by providing a wage income, which increases the opportunity cost of unrest participation to the extent that employees would need to give up or put in jeopardy this income stream in order to protest or rebel (Becker, 1968; Popkin, 1979). Previous work has established the pacifying role of labor income in numerous contexts: Bazzi and Blattman (2014) find that income from commodity shocks appears to reduce individual incentives to fight in wars, Dube and Vargas (2013) find that decreases in the price of labor-intensive coffee increase civil war violence in Colombia, and Fetzer (2014) finds that India's public employment program uncouples productivity shocks from conflict.

Another way to increase the opportunity cost of conflict would be to give transfers to citizens. 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 manage unrest?

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 inadvertently increase social instability. Cameron and Shah (2014) found that a highly mis-targeted transfer program in Indonesia increased protests, economic crimes, and violent crimes. Verifying eligibility 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 jobs may appear to deserve these benefits, as they have been earned through work. In contrast, transfers may generate 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 working-age men particularly undeserving of government handouts. Indeed, this group represents just 25% of Chinese welfare recipients but over 50% of SOE employment (Gao et al., 2015).

More generally, employment programs have demonstrated pacifying effects in other contexts. Heller (2014) finds evidence that summer jobs for youth in the United States decrease participation in violent activity. Blattman and Annan (2016) 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. The inverse is also true: Chinese SOE privatization in the late 1990s and early 2000s was associated with widespread unrest (Chen, 2006, 2009), and this phenomenon is common among privatization processes in other countries (McKenzie et al., 2003; Akoum, 2012). Employment may prevent conflict participation through several channels: it provides an income; it enters the time constraint; and it may also engender a variety of social and psychological changes. In this vein, surveys of Chinese citizens find that SOE employment is strongly negatively correlated with support for democratization (Chen and Lu, 2011).

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 unrest 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 2019 and 2020 (Ma and Cheng, 2019; Wright, 2019). These demonstrate the downsides of armed suppression: political backlash and a lack of long-term effectiveness. The Tiananmen response led to widespread domestic and international discontent, including sanctions and arms embargoes (Hufbauer et al., 1990). And in both the Dongzhou and Guangdong protests, once the police presence decreased, protests resumed. Relatedly, the Hong Kong protest response has harmed China's diplomatic standing (Roantree, 2019).

While the Chinese government clearly employs many policy tools to secure domestic tranquility, state employment has a unique set of pacifying 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 outweigh the marginal efficiency costs of distorting employment.

2.3 The Uyghur Conflict in Xinjiang

The central empirical strategy of this paper, described in Section 4, relies on variation in a violent conflict in Xinjiang province between the Uyghur ethnic minority group and the state. 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, 2018). For the last fifty years, a local Uyghur separatist movement has sought independence from Chinese rule, using a variety of violent and non-violent tactics (Millward, 2004).

The cohesion and intensity of the separatist movement escalated in the 1990s. Qualitative accounts identify three spikes of violence in 1990, 1992-93, and 1996-97 (Davis, 2008; Millward, 2004). The early 2000s was 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 suspected separatist training camp. In the ensuing years, several attacks took place in the cities of Kashgar, Kuqa, and Urumqi (Guo, 2015).

Primary 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 (Millward, 2004). Some violent incidents were triggered by economic phenomena, like firm layoffs. An even smaller proportion of violent 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).

3 Conceptual Framework

While the qualitative evidence presented in Section 2 is consistent with an SOE pacification motive, it is not definitive proof. Government rhetoric may or may not be backed by real policy behavior, and theoretically useful tools may never be implemented in practice. To test this hypothesis more rigorously, I develop a model to generate empirically-testable predictions indicative of pacifying intent. The model reveals how labor market outcomes should respond if a government were using state employment to pacify unrest shocks. I describe key model dynamics and predictions in this section and present the full model in Appendix Subsection 9.1. Afterward, I test each prediction in Sections 4 - 6.

3.1 Setup

This model consists of individuals, firms, and a government. There are two types of individuals: a non-unrest type and an unrest type, $j \in (U,N)$, who both value consumption and leisure via utility function $u\left(l^j, c^j\right)$. Individuals are endowed with time, which they can spend directly on leisure or convert into consumption by working. In equilibrium, individuals equate the ratio of their marginal utilities of leisure over consumption to equal the prevailing wage (the price of the consumption good is set to the numeraire), such that for individual type j, this equation holds: $\frac{u_\ell(\ell^{j*},c^{j*})}{u_c(\ell^{j*},c^{j*})} = w_j$. The key difference among types is that unrest-type leisure time generates unrest activities that the government dislikes.

There are also two types of firms: private firms and SOEs. Both types convert inputs into output using the same constant returns to scale production function, $Y = F(U,N)$[7], but are subjected to different government policies. In particular, the government taxes all non-unrest type labor in the economy at rate $\tau_N > 0$ and provides a subsidy $\tau_U < 0$ to SOE unrest-type hiring. In equilibrium, the private firm will therefore equate its marginal rate of technical substitution to its implicit input price ratio $\frac{F_U^{priv*}}{F_N^{priv*}} = \frac{w_U}{w_N(1-\tau_N)}$, and SOEs will do the same $\frac{F_U^{soe*}}{F_N^{soe*}} = \frac{w_U(1-\tau_U)}{w_N(1-\tau_N)}$.

The government values total output and stability, S. Stability is a decreasing function of an unrest shock, $\xi \in \mathbb{R}^+$, and the leisure of unrest-types, ℓ^U. The unrest shock ξ captures the efficacy of unrest activity once individuals have already committed to participate. In this setup, the government dislikes unrest not because it directly depresses output, but because it could lead to more serious problems if left unchecked. This choice best reflects the nature of unrest threats in the empirical section: too small to disrupt production, but potential catalysts for severe conflict. The government maximizes the following objective function conditional on its budget constraint.

$$
\begin{aligned}
max_{\tau_U,\tau_N}\, Y^{priv} + Y^{soe} + \eta S\left(-\xi z\left(\ell^U\right)\right) \qquad (1)\\
\text{s.t. } \tau_U w_U U^{soe} + \tau_N w_N N = 0
\end{aligned}
$$

The government's budget constraint gives $\tau_N = -\frac{\tau_U w_U U^{soe}}{w_N N}$, so I can rewrite the government's problem with only one choice variable, τ_U, and solve for the first order condition, $\frac{dY^{soe}}{d\tau_U} + \frac{dY^{priv}}{d\tau_U} + \eta\xi \frac{dS}{dZ}\frac{dz}{dU}\frac{dU}{d\tau_U} = 0$.

In equilibrium, the individuals, firms, and government must all make choices that satisfy

[7] Additionally, I assume that firm production satisfies Inada conditions.

their first order conditions. As both firms exhibit constant returns to scale in production, both types make zero profits. Additionally, several market clearing conditions hold: the labor markets for unrest-type workers and non-unrest type workers must clear, as well as that of the consumer goods market.

3.2 Comparative Statics

The testable comparative statics of this model are firm labor responses to the unrest efficacy parameter, ξ. Because this parameter only enters the government's problem, it only affects optimal labor choices via the government's choice of labor subsidy τ_U. The responses of key objects to τ_U in equilibrium are given in Propositions 1-4 below.

When τ_U becomes more positive (representing a larger subsidy), U-type labor becomes cheaper on average. This change elicits Proposition 1: the entire market will use more U-type labor and less N-type labor when subsidy τ_U grows. Additionally, because these changes arise from increasing labor demand, they generate shifts along the labor supply curve, resulting in wages that move in the same direction as quantity, yielding Proposition 2: U-type wages w_U increase, while N-type wages w_N decrease.

Because private firms do not receive the labor subsidy, their input mix reflects the change in equilibrium wages; since U-type workers are now relatively more expensive to hire, it follows that when the subsidy τ_U rises, private firms hire relatively more N-types. This result is Proposition 3.

Together, Proposition 1 and Proposition 3 imply a change in the SOE input mix. The only way for the proportion of employed U-types to increase in the aggregate, but decrease within private firms, is if SOEs hire more U-types than N-types when subsidy τ_U increases. This result is Proposition 4.

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

By recalling the government's first order condition, $\frac{dY}{d\tau_U} + \eta\xi\frac{dS}{dZ}\frac{dz}{dU}\frac{dU}{d\tau_U} = 0$, I can connect these propositions to empirical predictions, which relate unrest shocks to labor market outcomes. The governments' choice of subsidy τ_U is a function of unrest efficacy ξ . In particular, the marginal benefit of τ_U is increasing in ξ, and we have $\frac{d\tau_U^*}{d\xi} > 0$. By combining this insight with Propositions 2, 3, and 4, I derive the following predictions. When the threat of unrest rises, relative to N-types, U-type SOE employment should increase (Prediction 1), U-type private employment should fall (Prediction 2), and U-type wages should increase (Prediction 3).[8]

$$\text{Prediction 1} \quad \frac{dU^{soe*}}{d\xi} - \frac{dN^{soe*}}{d\xi} > 0$$
$$\text{Prediction 2} \quad \frac{dU^{priv*}}{d\xi} - \frac{dN^{priv*}}{d\xi} < 0$$
$$\text{Prediction 3} \quad \frac{dw_U^*}{d\xi} - \frac{dw_N^*}{d\xi} > 0$$

I provide a detailed discussion of these results in Appendix Subsection 9.1 and full proofs of each in the Online Mathematical Appendix.

Within the model, the subsidy strictly decreases welfare, because individuals value only consumption and leisure and the subsidy hurts output efficiency by distorting prices. However, if citizens were to value social 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

[8]These predictions hold whenever the government places non-zero preference weight on stability, such that $\eta > 0$.

leisure.

3.3 Sufficient Statistic

The model also generates an empirically-observable sufficient statistic for the male minority wage subsidy. When the production function F is Cobb-Douglas, such that $F = U^{\alpha}N^{1-\alpha}$, the first order conditions of the SOE and private firms become:

$$\frac{(1-\alpha)N^{priv}}{\alpha U^{priv}} = \frac{w_N(1-\tau_N)}{w_U}. \tag{2}$$

$$\frac{(1-\alpha)N^{soe}}{\alpha U^{soe}} = \frac{w_N(1-\tau_N)}{w_U(1-\tau_U)}. \tag{3}$$

By dividing equation (2) by equation (3), I obtain:

$$\tau_U = 1 - \frac{N^{soe}/U^{soe}}{N^{priv}/U^{priv}}. \tag{4}$$

The latter term can be obtained directly from employment data, as I do in Subsection 6.5. I then compute τ_U, the implicit wage subsidy that SOEs receive to hire male minority employees.

4 Empirical Strategy

To test model predictions, I devise a natural experiment that captures how a regional ethnic conflict generates threats elsewhere in China. The threat of ethnic unrest corresponds to ξ, the unrest shock.

4.1 Uyghur Unrest Shock

This subsection presents a measure of Uyghur unrest threat in non-Xinjiang counties. This measure uses three sources of variation: temporal variation in conflict incidents in Xinjiang, geographic variation in Uyghur population shares across the rest of China, and whether individuals are male minorities. Due to data constraints described in Section 5, this measure covers 2002-2009.

The first component is $I_{t-1}^{XJ=1}$, an annual measure that captures the number of conflict incidents in Xinjiang (XJ) in the previous year $(t-1)$. 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. I consider alternative lags and intensity measures as robustness checks.

I construct $I_{t-1}^{XJ=1}$ using multiple primary and secondary historical sources. First, I conduct a systematic search of historical newspaper archives using the Proquest Historical Newspapers Database, generating a data set of unique incidents and record the date, province, county, and type of each incident. An incident is included in the sample if it is documented by an internationally reputable media outlet and falls under conflict event codes in the GDELT dataset (Leetaru and Schrodt, 2013) [9]. 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 narrative details reported in these data.[10]

The time series of lag Xinjiang conflict events for sample years are plotted in Figure

[9] Specifically, I include these event groups: "threat", "protest", "exhibit force posture", "coerce", "assault", "fight", and "use unconventional mass violence".

[10] To illustrate discrete incidents, I present an exhaustive list of events from 2005. January 20: a bus bombing in the city of Kuytun. Februrary 22: a knife attack in Pizhan Township. March 12: a clash between Uyghur and Han students in Poskam. April 16: a taxi-driver strike in Korla against a tax increase. October 1: a video message from the East Turkestan Liberation Organization calling on Uyghur people to boycott 50th anniversary celebrations of the founding of Xinjiang and declaring war on the Chinese government.

1. 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 use incident count instead of fatalities as the latter are more prone to strategic manipulation and reporting error. Whether an incident occurs at all is both easier to measure and more difficult to manipulate.

The second component is each Chinese county c's Uyghur population share, $U_{c,t=2000}$. This variable is obtained by using the 2000 Population Census of China (The National Bureau of Statistics, 2010) and dividing the number of Uyghur individuals by the total population of the county. I use the 2000 Census as it predates the coverage of the baseline sample, thus removing some endogeneity in Uyghur population distribution that might arise from the migration of Uyghur peoples in response to contemporaneous, unobserved forces, like friendly local policies. Figure 2 presents a choropleth map of county-level Uyghur population shares outside of Xinjiang, and Table 1 presents detailed summary statistics. 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 permits a wide array of geographic controls.

Of course, the distribution of Uyghur populations outside of Xinjiang in 2000 is not random. Table 2 compares basic economic and demographic characteristics of populations across varying levels of county-level Uyghur share. As a benchmark, columns (1) and (2) report the means and standard deviations of each variable for all of China and all of China excluding Xinjiang, respectively. Column (3) reports statistics for the sample of counties that have non-zero Uyghur share, which represent 14.5% of counties outside Xinjiang. Finally, among counties with non-zero Uyghur share, columns (4) and (5) report statistics for those with below- and above-median values of Uyghur share. On average, high Uyghur share counties have lower GDP per capita, lower population, one fewer year of schooling, and a higher proportion of illiterate adults. They are also somewhat more urbanized and have comparable rates of employment.

One threat to this 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 differentially affects male minorities. To concretize these drivers, I turn to the ethnographic and historical literature. The literature suggests that Uyghur settlement patterns are generated by a combination of historical and modern 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 has 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 why those forces would differentially affect male minorities. Nonetheless, to address possible confounders, I flexibly control for pre-period labor market conditions in the baseline specification. I describe these controls in Subsection 4.2.

At this point, this difference-in-differences measure can be written as an interaction variable $DD_{ct} = I_{t-1}^{XJ=1} \times U_{c,t=2000}^{XJ=0}$. For clarity, I have introduced the superscript $XJ = 0$ onto the county Uyghur share variable to highlight the fact that for the entire analysis, the sample will omit counties within Xinjiang. In other words, this means I will use variation in conflict intensity inside Xinjiang to generate variation in the threat of unrest spillover to counties outside of Xinjiang.

The reasons to omit Xinjiang from the sample of analysis fall into two categories: those concerning causality and those concerning measurement. To understand the causal inference reasons to omit Xinjiang, it is important to clarify that the heart of my empirical strategy relies on two key properties of the unrest spillover shock: unrest threats are not generated locally, and the unrest threats do not generate realized conflict. Both of these properties are essential. First, the fact that unrest threats are generated in Xinjiang, while the outcome variables are measured in other parts of China, eliminates the possibility that key omitted variables, like price changes or local downturns, could drive both local unrest and employment. Second, these two properties address the reverse causality problem: local employment conditions should directly influence the extent of local unrest (and unrest threats) through precisely the mechanisms that make state employment a useful pacification policy. Therefore, the omission of Xinjiang is essential to a causal interpretation.

On the measurement side, there is a marked dearth of datasets that contain enough information to implement an analogous version of this empirical strategy within Xinjiang. Of the eight most commonly-used individual- or household-level surveys in China[11], only four cover Xinjiang province in more than one time period: the China General Social Survey (CGSS), China Family Panel Studies (CFPS), the China Household Finance Survey (CHFS), and the and the Urban Household Survey (UHS). However, the Xinjiang samples from the CGSS, CFPS, and CHFS are unusably small: they never contain more than 180 observations per wave. Furthermore, the UHS data from the province of Xinjiang are not available for research use due to the political sensitivity of the province[12]. For these reasons, the baseline sample does not include Xinjiang, and it is infeasible to perform an analogous test using the province itself.

The relevance assumption required for the differences-in-differences shock $DD_{ct} = I_{t-1}^{XJ=1} \times U_{c,t=2000}^{XJ=0}$ 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. There is evidence that this spillover pattern is

[11]The Chinese Household Income Project (CHIP), the China Health and Nutrition Survey (CHNS), the China Family Panel Studies (CFPS), the China Household Finance Survey (CHFS), the China Multi-Generational Panel Datasets (CMGPD), the China Health and Retirement Survey (CHARLS), the China General Social Survey (CGSS) and the Urban Household Survey (UHS).

[12]I have tried to obtain this data from four different outlets and was unable to do so due to security concerns.

present within the Xinjiang conflict as well. In December 1985, Uyghur groups in Xinjiang protest vigorously against nuclear testing in Lop Nur. These protests then expanded to Beijing, home to one of the largest Uyghur diaspora communities in China (Toops, 2009).

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 is that ethnic identity becomes 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.

At this stage, consider a regression of a labor market outcome, like SOE employment, on the interaction variable proposed in expression $DD_{ct} = I_{t-1}^{XJ=1} \times U_{c,t=2000}^{XJ=0}$ and other controls. 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 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 '90s and '00s, 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 likely 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 pacification policies. 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.

Qualitative and quantitative evidence support this approach. Anthropological work on the Xinjiang conflict suggests that a very large majority of insurgents 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 Figure 3a reveals that male minorities are a disproportionately large share of political dissidents in China. This prevalence accords with the general sociological and criminological observation 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 keenly aware of the demographics of the Xinjiang conflict, so any resource-constrained pacification policies are likely to target the highest-risk group: male Uyghurs. However, one limitation of my main dataset[13] is that I cannot distinguish Uyghur individuals from those belonging to other minority groups. Given that only 8.4% of minorities in China identify as Uyghur (The National Bureau of Statistics, 2010), this constraint means I cannot separate an SOE response targeting Uyghur men from one targeting minority men more generally. While I cannot rule out either interpretation, both bolster the idea that SOE employment is means to manage unrest, the central hypothesis.

Appendix Table A.1 reports individual characteristics for non-minorities, minorities, and minority men. On average, minorities are slightly younger, more likely to be female, slightly more educated. Conditional on being employed, minorities earn lower salaries on average. They are also less likely to be employed at all and less likely to work for private firms, but more likely to be employed in SOEs. Minority men are also on average younger and more educated than non-minorities. They earn lower salaries on average, though they are more likely to be employed at all and much more likely to be employed in SOEs. In the baseline specification, I control for years of education, age, and their interaction with Xinjiang conflict incidents and county Uyghur share, in order to remove spurious effects due to these correlates of male minority identity.

[13] I present this data in Section 5. This issue applies to all other datasets with adequate labor market information.

With the addition of the third interaction, the shock can be written as the following expression, where the additional index i represents individuals and the variable M_i represents an indicator if a person is a male ethnic minority: $DDD_{ict} = I_{t-1}^{XJ=1} \times U_{c,t=2000}^{XJ=0} \times M_i$.

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. The model's three directional predictions on SOE employment, private employment, and salaries further decreases the possibility that an omitted variable could reject the null. 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 6.1.

4.2 Baseline Specification

The baseline estimating equation is:

$$\begin{aligned} Y_{ict} = \alpha &+ \beta_M I_{t-1}^{XJ=1} \times U_{c,t=2000}^{XJ=0} \times M_i + \beta I_{t-1}^{XJ=1} \times U_{c,t=2000}^{XJ=0} \\ &+ \gamma_1 I_{t-1}^{XJ=1} \times M_i + \gamma_2 U_{c,t=2000}^{XJ=0} \times M_i + \gamma_3 M_i \\ &+ \delta_c X_c \times \tau_t \times M_i + \delta_i X_i \times I_{t-1}^{XJ=1} \times U_{c,t=2000}^{XJ=0} + \tau_t + Dist\,XJ_c \times \tau_t + \eta_c \times M_i + \varepsilon_{ict}, \end{aligned} \tag{5}$$

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 18 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. Xinjiang province is excluded.

There are three dependent variables Y_{ict} . One 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 or urban collective economic unit. Another is an indicator for employment in a privately-owned economic unit. The last outcome is salary in thousands of yuan. This variable is not defined for non-employed individuals.

Y_{ict} is a function of a triple interaction between lagged violent incidents in Xinjiang, $I_{t-1}^{XJ=1}$, 2000 non-Xinjiang county Uyghur population share, $U_{c,t=2000}^{XJ=0}$, and an indicator for whether an individual is a male minority, M_i. As previously discussed, because the household data only document individuals' minority status, not their precise ethnicity, the coefficient β_M captures the labor market response among *all minority men* to the *Uyghur* unrest shock. In principle, it would be desirable to also test the labor market response of Uyghur men specifically, as Uyghur people represent 8.4% of ethnic minorities in China, but data constraints bind.

Several lower interactions are absorbed by fixed effects. This specification includes year fixed effects τ_t, county and male minority fixed effects $\eta_c \times M_i$, interactions of a vector of county-level characteristics X_c, and a vector of individual-level characteristics X_i. The vector 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 X_i are age, gender, and a fixed effect for years of education, which are also interacted with the Xinjiang incident and Uyghur share variables.

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 addresses geographic- and time-varying government policies, like the 2000 "Open Up the West" campaign.

The county and male minority fixed effects, $\eta_c \times M_i$, absorb any time-invariant differences in the labor composition of counties by group, like if some counties were consistently prejudiced against hiring male minorities. Finally, I cluster standard errors at the county level to account for the shock's level of geographic variation.[14]

I can rewrite the theoretical predictions in Section 3 in terms of real-world phenomena. 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 $Private_{ict}$ on the baseline specification. In words, the model predicts that when unrest threat rises, male minorities should work more for SOEs, less for private firms, and earn higher salaries, compared to the general population.

$$\text{Prediction 1} \quad \frac{dL^{soe*}}{d\xi} > 0 \rightarrow \beta_M^{SOE} > 0 \tag{6}$$

$$\text{Prediction 2} \quad \frac{dL^{priv*}}{d\xi} < 0 \rightarrow \beta_M^{PRIV} < 0 \tag{7}$$

$$\text{Prediction 3} \quad \frac{dw^{*}}{d\xi} > 0 \rightarrow \beta_M^{Salary} > 0 \tag{8}$$

5 Data

5.1 Urban Household Survey

Outcome variables and individual-level controls come from the *Urban Household Survey* (UHS). These data are collected by the National Bureau of Statistics, and I use data from the years 2002 to 2009. 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 due to worse documentation and the perceived risks of responding.

The 2002-2009 UHS data include data on individual ethnicity, as well as a rich set of variables describing household composition, age, gender, employment, and education.

[14] 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 for the baseline as the panel is only 8 years long (Baum et al., 2010).

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 the empirical tests presented in this paper. For the analyses below, I 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 Appendix Subsection 9.2, I explore how the results change if SOEs are defined as state-owned units only. Additionally, the analysis will only focus on working-age individuals, defined as ages 18-55 for women and 18-60 for men.

The 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 represent a wide array of income levels and geographic locations. The availability of these provinces for research use implies that they are not politically sensitive and that the government is unlikely to alter or censor these data. In comparison, data from Tibet and Xinjiang are highly confidential, and were they available, data quality would be a first-order concern.

5.1.1 Demographics of Unrest and State Employment

In China, the demographics of unrest participation differ from those of the general population, which I illustrate by comparing the composition of China's total population from the 2000 Census (The National Bureau of Statistics, 2010) with information from a dataset of all known Chinese political prisoners (Congressional-Executive Commission on China, 2019).[15] Figure 3a demonstrates that minority men are dramatically over-represented among political prisoners: they comprise over 45% of unrest participants but represent

[15]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.

just 4% of the general population.

SOEs also hire more of this demographic. On the left-hand chart in Figure 3b, I plot the average share of male minorities in private firms versus SOEs from the UHS data: SOEs hire disproportionately more than private firms, and this difference is precise at the $p < 0.000$ level.

6 Results

This section builds to the central baseline result in three steps. First, I show how all minority employment differentially responds to the threat of unrest; then, I present those results partitioned by gender; and finally, I show the full baseline male minority triple difference. This approach highlights the variation behind the key results and addresses the fact that male minority identity is an intersection of both gender and ethnicity. Indeed, Equation (5) could be presented as a quadruple differences specification, interacting time-series incidents, cross-county population shares, gender, and ethnicity, though I avoid this formulation for the sake of expositional clarity.

To be concrete, the first step involves a specification wherein the male minority indicator term, M_i, in Equation (5) is replaced with an indicator variable for all minorities. Theoretically, this specification tests a version of the model in which the unrest-prone demographic (U) maps onto all minorities, not just male minorities. Given that the source of unrest threat is ethnic conflict, this mapping seems a priori reasonable. Table 3 reports the resulting estimates. The three outcome variables in this table are SOE employment, private employment, and salary; the coefficients from columns (1), (2), and (3) correspond to the predictions in Equations (6), (7), and (8).

Column (1) of Table 3 shows that, in the face of unrest threat, SOEs differentially hire more minorities: the coefficient is 18.30 and precise at the $p < 0.1$ level. Column (2) reveals that the opposite is true for private firms: they differentially hire fewer minorities when unrest threat rises: the coefficient is -15.34 and different from zero with $p < 0.1$. Finally, column (3) reports that the salaries of minorities differentially increase with unrest threat, with a coefficient of $3,131$ and a p-value less than 0.1. These three results correspond exactly with Predictions 1, 2, and 3 of the model; these are the exact labor market responses we would expect to observe if SOEs responded to unrest threats by hiring minority workers.

However, there is reason to believe that Table 3 sacrifices substantial statistical power by combining male and female minorities. Figure 3a displays how male minorities are overwhelmingly over-represented among detained unrest participants, suggesting that government pacification policies, if targeted, are likely to focus much more on this group. To investigate this possibility and set the stage for the baseline results, I re-estimate Table 3 for men and women separately, to see whether the differential response of minority men relative to Han men, rather than those of minority women, drive the results.

Table 4 demonstrates that male minority labor market responses drive the entire relationship. Panel A reports results for men; Panel B reports results for women. The coefficient in Panel A, column (1) is 37.13 and precise at the $p < 0.01$ level - SOEs hire differentially more minority men relative to Han men when unrest threats increase, in line with Prediction 1. Notably, the coefficient's magnitude is almost exactly double that of column (1) in Table 3, suggesting that half of minorities, the women, exhibited little labor market response to unrest threat.

The coefficient in Panel A, column (2) is -23.25 and precise at the $p < 0.1$ level, as private firms hire differentially fewer minority men than Han men, in accordance with Prediction 2 of the conceptual framework. Finally, the coefficient in Panel A, (3) is $5,332$ and precise at the $p < 0.05$ level, recording a differential increase in minority male salaries, relative to Han male salaries, in accordance with Prediction 3.

All three coefficients for the female subsample are not statistically distinguishable from zero. This suggests that unrest threats do not generate differential labor market treatment of minority women relative to Han women, and, consistent with the patterns in Figure 3a, male minorities are the central focus of the SOE hiring response to unrest. Motivated by these results and the qualitative evidence supporting a focus on minority men, I now proceed with the full baseline specification.

Table 5 presents results from estimating Equation (5) as a linear probability regression. Prediction 1 of the conceptual framework states that when unrest threat increases, SOEs should hire more male minorities relative to all other demographics. Indeed, the coefficient in column (1) is positive and precise, taking a value of 36.59 and different from from zero at the $p < 0.01$ level. The coefficient in column (2) is negative; β_M^{PRIV} is -24.24 and different from zero with $p < 0.05$. Finally, Prediction 3 is that male minority salaries should differentially rise in response to increasing unrest threat. The coefficient in column (3) is $5,422$ with $p < 0.01$.

The coefficient in column (1) implies that, during a period of median conflict intensity in Xinjiang, a one-standard-deviation increase in Uyghur share leads male minority SOE employment to increase by 0.48 percentage points. This value represents $0.00483/0.0213 =$ 22.6% of male minority SOE employment. Analogously, the coefficient in column (2) implies a 0.33 percentage point decline in male minority private employment when increasing Uyghur share by one standard deviation during a period of median conflict intensity. This decline represents $0.00332/0.0158 = 20.9\%$ of total male minority private employment. The net effect on total male minority employment is positive but statistically insignificant, which suggests that the general equilibrium effects of employment policies may mitigate the effect of such programs on aggregate employment[16]. Finally, the coefficient in column (3) represents an annual salary increase of 742 RMB (approximately $100 USD) over a sample average of $45,510$ thousand RMB.

To visually display the variation driving these baseline results, I re-estimate a version of the baseline equation that produces year-by-year estimates for the coefficient on $U_{c,t=2000}^{XJ=0} \times M_i$ and $U_{c,t=2000}^{XJ=0}$.

[16]Even in the absence of aggregate employment effects, an increase in state employment may pacify violence, though the relevant mechanisms would then be those that distinguish state employment from private employment, and not those that distinguish state employment from non-employment. For example, the psychological effects of inspiring loyalty, or the threat of having employment conditioned on good behavior, may be most important. However, a feature of this paper's empirical design is to use threats of conflict, rather than realized conflict, so it is by design not possible to evaluate the direct effect of the policy response on unrest.

$$
\begin{aligned}
SOE_{ict} = \alpha + & \sum_{t=2002}^{2008} \beta_{Mt} \mathbb{I}_t \times U_{c,t=2000}^{XJ=0} \times M_i + \sum_{t=2002}^{2008} \beta_t \mathbb{I}_t \times U_{c,t=2000}^{XJ=0} \\
& + \gamma_1 I_{t-1}^{XJ=1} \times M_i + \gamma_2 U_{c,t=2000}^{XJ=0} \times M_i + \gamma_3 M_i \\
& + \delta_c X_c \times \tau_t \times M_i + \delta_i X_i \\
& + \tau_t + Dist\, XJ_c \times \tau_t + \eta_c \times M_i + \varepsilon_{ict}
\end{aligned} \tag{9}
$$

I estimate Equation (9) and plot the coefficients β_{Mt} for each sample year in Figure 4 with a thick red line and shaded 95% confidence band. This figure also displays the time series of lagged Xinjiang incidents over time with a blue line. The co-movement of these two lines represents the correlation behind the triple difference coefficient: the association is positive and no single year drives this positive relationship.

6.1 Robustness Checks

Table 5 could be generated by alternative determinants of employment and wages that are correlated temporally with 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). I conduct a set of robustness checks for these strategic motives.

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 utilities and public services.

Table 6 reports this set of robustness checks for employment by ownership. I find that introducing these controls does little to change the magnitudes and precision of the baseline estimates. I also 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.2, remain similar in sign and magnitude to those of the baseline.

Another potential source of endogeneity is Xinjiang unrest incidents triggered by events outside Xinjiang, which may themselves be correlated with local economic conditions. To address this concern, I hand-code the inciting reason for each event in my database of Xinjiang unrest using primary evidence. I then drop every event whose trigger came from outside Xinjiang. One example is a series of bombings in Urumqi that coincided with Deng Xiaoping's funeral in February of 1997. Rebel groups timed the attacks to publicize the struggle of the Uyghur people against the Chinese government (Steele and Kuo, 2007). Panel A of Table 7 reports estimates using this amended Xinjiang incident time series as $I_{t-1}^{XJ=1}$. The baseline results hold.

Another 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. To address this possibility, I construct an incident time series that removes all events sparked by economic issues. For example, I remove a series of factory closure protests that occurred in the city of Hotan in October 2001. Panel B of Table 7 reports estimates using this alternate series, which corroborate the main results.

I also test whether my results are robust to logit and probit, rather than linear probability regressions. They are, and these tables are available upon request.

One property of this empirical context is that the distribution of Uyghur population shares is not normal, as Table 1 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 perform a random permutation test on the Uyghur share variable. For this test, I generate 500 counterfactual Uyghur share maps for China, following the variable's true distribution. Then, I re-run the baseline specification using these counterfactual Uyghur shares. Figure 5 plots a histogram of the resulting coefficients. I find that only 5.1% of these counterfactual coefficients have a value higher than the true estimate of 36.59. The baseline results are highly unlikely to be generated by random assignment of county Uyghur share.

I also test whether the baseline results are sensitive to the removal of outliers in Online Appendix Table A.3. 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. The SOE and salary results are robust to this procedure, and the private employment result remains negative but is no longer precisely different from zero.

In Table 8, 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 should not respond to incidents in the future. The estimates in this table are consistent with this reasoning. The coefficients β_M are small in magnitude and not precisely different from zero for all three outcome variables.

A different type of placebo uses a property of the unrest spillover. I should not observe an employment response to an interaction between Xinjiang conflict incidents and population shares from another ethnicity. To test this idea, I construct county-level population shares of Hui people, who are a Muslim ethnic minority that the Chinese government considers a lower unrest threat than the Uyghur people (Friedrichs, 2017). To perform this additional placebo test, I introduce the triple interaction of Hui population share with Xinjiang incidents and the male minority indicator (and all appropriate double-interactions) Equation (5). Appendix Table A.4 reports these coefficients; I find that male minority employment and wages do not respond to the Hui share triple interaction.

6.2 Heterogeneity by Sector

Are there sectors in which the SOE pacification response is more pronounced? To answer this question, I divided the UHS sample into six sector categories: manufacturing, mining and construction, retail and transportation, services, agriculture, and Communist Party work. Since urban agriculture is rare and Party employment is always state-owned, I focus on the first four categories and run the baseline specification separately for SOE employment and private employment for each. Because sector is only defined for employed individuals, the SOE and private coefficients are perfect inverses, though I report both regressions for completeness.

Appendix Table 9 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.

The fact that state hiring is most pronounced in the service sector is interesting for two reasons. First, it is the state sector with the highest proportion of male minority SOE employees. Secondly, it is the most customer-facing sector, where anti-minority discrimation is likely to be the highest, particularly during times when ethnic conflict is salient. Yet, we find the most pronounced increase in male minority state employment in this sector, suggesting the state labor demand overrides even the potential increase in customer discrimination.

This table suggests that there may be a stronger pacification response in service-sector SOEs. There may be several reasons for this pattern, including the fact that SOEs employ 75% of the workers in this category, and that a slightly higher share of service-sector employees are male minorities than in other sectors.

6.3 Response Over Time

In this section, I estimate the unrest response of state employment over time. This test addresses key question: how durable is the employment response? I use Equation (10), which introduces more lags of the unrest threat shock to the baseline equation. The variable $I^{XJ=1}_{t-j}$ captures the number of unrest incidents that took place in Xinjiang j years ago, so the vector of coefficients $< \beta_{M1}, ..., \beta_{M5} >$ expresses the differential shock response of the outcome variable Y_{ict} for male minorities as time elapses. I estimate Equation (10) for the outcomes of SOE employment, private employment, and salary.

$$\begin{aligned} Y_{ict} = \alpha &+ \sum_{j=1}^{5} \beta_{Mj} I^{XJ=1}_{t-1} \times U^{XJ=0}_{c,t=2000} \times M_i + \sum_{j=1}^{5} \beta_j I^{XJ=1}_{t-1} \times U^{XJ=0}_{c,t=2000} \\ &+ \sum_{j=1}^{5} \gamma_{1j} I^{XJ=1}_{t-1} \times M_i + \gamma_2 U^{XJ=0}_{c,t=2000} \times M_i + \gamma_3 M_i \\ &+ \delta_c X_c \times \tau_t \times M_i + \delta_i X_i + \tau_t + Dist\,XJ_c \times \tau_t + \eta_c \times M_i + \varepsilon_{ict} \end{aligned} \tag{10}$$

One important caveat for this exercise is that the data's temporal range of 2002-2009 is relatively short, so longer lags are estimated using fewer years of data. For example, the 5-year lag coefficient relies on conflict data from 2002-2004 and labor market data from 2007-2009. Encouragingly, Figure 1 shows that Xinjiang incidents do vary during those years, but that variation will be inherently less representative of the whole period.

I plot the regression coefficients $< \beta_{M1}, ..., \beta_{M5} >$ in Appendix Figure A.2. The three sub-figures in Figure A.2 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 for three years and then takes a small negative value in the fourth. The size of the negative correction is much smaller in magnitude than the initial positive employment response, suggesting that some, but not all, of the newly hired employees are shed after the initial shock. This result may be surprising given the persistence of state employment in most sectors. However, the previous result suggested that the most pronounced hiring response occurs in the service industry, where both state and private employment turn over more frequently, and jobs may require fewer highly technical skills.

The response of private employment mirrors that of SOE employment. A 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 reverts in the fifth year. Salary follows the same approximate path as SOE employment: in the first year following a shock, the prevailing salary increases precisely and positively, but then declines and appears to correct slightly in the fourth year post-shock. The timing of the salary and private employment responses align exactly with the those of state employment, consistent with the preferred interpretation that changes in state labor demand drive these market responses.

6.4 Complementary Policies

In this section, I test whether the government uses other policies, like social relief transfers, in conjunction with SOE employment to stabilize. The Urban Household Survey directly documents these transfers, which encompass financial and in-kind assistance disbursed in response to natural disasters, sudden disability, extreme poverty, and other subsistence challenges (Hussain, 1994; Cook, 2002; Wong, 2005). These transfers are designed to be nimble and the government retains a great deal of discretion in their disbursement.

I re-estimate Equation (5) using social relief transfers as the outcome variable. To further enrich the analysis, I repeat the regression for four samples: the full baseline sample, SOE employees only, private employees only, and individuals who are not employed. Results from these regressions are reported in Table 10. In Column (1), I find that in response to the shock, average social relief transfers to male minorities differentially increase by 17.51 yuan, and the change is precisely different from zero at the $p < 0.01$ level. This column suggests that the government complements its employment pacification policies with targeted relief transfers. For a county outside Xinjiang with an average level of Uyghur share, the magnitude of this estimate implies that individuals will receive 3.19 yuan more in a year with 75th percentile incident counts in Xinjiang compared to year with 25th percentile Xinjiang incident counts. Though this amount appears small, only 1.43% of the population receives any relief transfers. Scaling by the proportion of non-zero values (and assuming no movement on the extensive margin), the magnitudes imply an increase of

222.94 yuan among relief transfer recipients.

In Columns (2)-(4), I subdivide the response 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 precisely different from the response for both SOE and private workers. These columns suggest that the relief transfers are targeted on the population of male minorities not reached by the SOE employment expansion: the non-employed.

6.5 Sufficient statistic

Finally, I substitute empirical moments into Equation (4) and compute of τ_U, the value of male minority wage subsidies:

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

The data imply a 26% equilibrium wage subsidy for male minorities. This subsidy can be interpreted as the price-equivalent value of all financial and non-financial support that the government provides to SOEs to encourage the hiring of male minorities. The exact 95% confidence interval for this value is $(20\%, 32\%)$ (Mehta et al., 1985).

7 Evidence of Generality: Exports and Floods

In this section, I present new facts suggesting that the SOE pacification role is not relegated to the domain of Uyghur unrest. First, I show that SOEs hire countercyclically with respect to export demand, whereas private firms hire procyclically. Next, I show that, after natural disasters, private firms shed labor but SOEs hire. While these patterns could be explained by alternative hypotheses, like unobserved differences in SOE exposure to bad shocks, when viewed in light of the evidence presented in Section 6, these facts paint a consistent picture of Chinese SOEs' stabilizing role.

7.1 Export Demand

In general, profit-maximizing firms should decrease both output and inputs, including employment, when demand falls. In this section, I show that when demand for Chinese exports falls, private firms shed labor as expected, yet SOEs hire more. I construct a measure of export demand based on the setup used in Autor et al. (2013).[17] The annual provincial demand shock exposure, $\Delta DSEIV_{pt}$, has two components: a weight variable and a trade flow variable.

$$\Delta DSEIV_{pt} = \sum_{s} \left[\frac{X_{spt-1}}{X_{st-1}} \sum_{a \in A} \sum_{b \in B} \Delta E_{st}^{ab} \right] \tag{11}$$

The letter s indexes sectors. Provinces are indexed with p and years are indexed with t. The weight variable, $\frac{X_{spt-1}}{X_{st-1}}$, equals the ratio of exports from a given sector, year, and province to all exports out of China from that sector and year. Provinces that export more will thus receive a higher weight. The trade flow variable ΔE_{st}^{ab} represents the net exports (exports minus imports) into China's trading partner $a \in A$ from the partner's own largest trading partners, $b \in B$. 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.[18] This setup avoids using flows that directly involve China itself, which are certainly influenced by China's domestic situation.[19] The provincial variation in Equation (11) arises entirely

[17] Campante et al. (2019) use a similar setup to estimate how trade shocks affect Chinese labor strikes.

[18] Set A includes the United States, Japan, South Korea, Germany, and the Netherlands. 2004 is a representative year from my sample, and the results are robust to using top partners from alternative years.

[19] I obtain changes in net export flows ΔE_{st}^{ab} from the *United Nations Comtrade Database* (UN Comtrade) (United Nations, 2016). I construct the weight variable $\frac{Y_{spt-1}}{Y_{st-1}}$ using Chinese data from the *Annual Surveys of Industrial Production* (ASIP), which I describe in detail in Online Appendix Subsection 9.3. The UN Comtrade data measure the trade flow in current dollar values between countries at the annual level. The current temporal coverage of UN Comtrade is 1962 to 2018 and it reports sectors using Harmonized System (HS) codes. The ASIP dataset covers the years 1998 - 2013 and reports sectors using the Chinese Industrial Code system. In order to combine data from UN Comtrade with constructed weights from ASIP, I hand-construct a concordance table.

from variation in the sectoral export structure across provinces during period $t-1$.

I estimate the following regression using UHS data to uncover the response of employment to the trade shock.

$$
\begin{aligned}
Y_{ict} = \alpha + \beta \Delta DSEIV_{pt} + \gamma Age_i + \delta Edu_i + \zeta Male_i \\
+ \delta_M Edu_i \times \text{Male}_i + \gamma_M Age_i \times \text{Male}_i + \tau_t + \eta_c + \varepsilon_{ict}
\end{aligned}
\tag{12}
$$

In this equation, i indexes individuals, p indexes provinces, c indexes counties, and t indexes years. The two dependent variables, Y_{ict}, are indicators for whether an individual works for an SOE or a private firm, respectively. 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. Because the demand shock varies at the province and year level, I cluster standard errors at the province and year level.

Column (1) of Table 11 shows that SOE employment responds inversely to trade demand. The coefficient is -0.0529 and is precise at the $p < 0.01$ level. On the other hand, column (2) shows that private firms respond procyclically to 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. SOEs may be concentrated in sectors that are differentially exposed to trade. As a robustness check, I control for base-year sector composition by county interacted with year fixed effects and report the results in Columns (1) and (2) of Appendix Table A.5. Additionally, I re-construct the main trade shock $\Delta DSEIV_{pt}$ using only sectors in which China represents less that 5% of global trade flows to account for the possibility that China's large role in global trade may lead to exclusion restriction violations. Results from this test are reported in Columns (3) and (4) of Appendix Table A.5. To further increase confidence that these results are not elicited by spurious trends, I re-estimate Equation (12) 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 Online Appendix Table A.6 - neither coefficient is statistically different from zero.

7.2 Flood Disasters

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. In the short run, natural disasters are generally harmful for firms (Cavallo and Noy, 2009), which tend to react by producing less output and demanding fewer inputs. I examine employment responses to flood disasters with the following regression, again using UHS data.

$$\begin{aligned} Y_{ict} = \alpha + \beta \Delta Flood_{ct-1} + \gamma Age_i + \delta Edu_i + \zeta Male_i \\ + \delta_M Edu_i \times \text{Male}_i + \gamma_M Age_i \times \text{Male}_i + \tau_t + \eta_c + \varepsilon_{ict} \end{aligned} \tag{13}$$

In this equation, i indexes individuals, c indexes counties, and t indexes years. The dependent variables, Y_{ipt}, follow the definitions from Subsection 7.1. This specification includes year fixed effects τ_t, county fixed effects η_c, and interactions of a vector of individual-level characteristics X_i: age, a fixed effect for education level, as well as each of these controls interacted with gender.

Data on riverine flooding come from the Dartmouth Flood Observatory's *Global Active Archive of Large Flood Events* (Brakenridge, 2019). The flood data cover the years 1990 to 2017 and include the latitude and longitude of each flood's centroid, from which 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 suffer riverine flooding according to this definition, about 1.1% of all county-years. I use the flood indicator in year $t-1$ because I assume that employment is somewhat sticky. I cluster the standard errors at the county and year level, the level at which floods vary.

Table 12 shows SOE employment increases in the year after floods: 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 both county-year flood incidence and employment by ownership. To address some concerns, I control for the base year sector share of each county interacted with year fixed effects and report results in Appendix Table A.7. I also conduct a placebo check by re-estimating Equation (13) using the lead of the flood indicator variable. The results from these regressions are reported in Online Appendix Table A.8, and reassuringly, employment composition by ownership does not respond to future floods.

8 Conclusion

This paper documents how the Chinese government uses state employment to pacify social unrest. In doing so, it provides the first causal, empirical evidence that autocrats use public employment as a means of managing external threats. Morevoer, this finding provides a political economy explanation for the persistence of state-owned enterprises in China, and consequently, a downward force on productivity in a major world economy.

The central empirical test in this paper uses a triple-differences approach to document the response of employment to ethnic unrest threats. The unrest shock combines annual variation in Xinjiang conflict intensity, county-level variation in Uyghur population densities, and individual-level variation in whether individuals are male minorities. In response to these threats, SOEs increase their employment of minority men and private firms shed employment from the same group. I find that salaries increase, but only for male minorities, suggesting that the observed patterns result from increasing SOE labor demand rather than falling private labor demand. This entire suite of results is consistent with a theoretical framework wherein the government subsidizes state firms to boost employment of certain demographics, using employment to depress the likelihood of unrest.

This project reveals a number of questions for future research. For example, could alternative autocratic policies for controlling external threats generate fewer economic costs than state employment, and if so, what advantage does public employment offer? Do autocrats face higher benefits, or lower costs, from allocating public employment in this manner? And, more broadly, what other political economy motives, for autocracies and beyond, might generate economic distortions? These questions all relate to the fundamental theme of how, and why, political concerns manifest as forces of economic development, and how those forces interact with regime type.

Figure 1: Plot of Lag Xinjiang Unrest Incidents

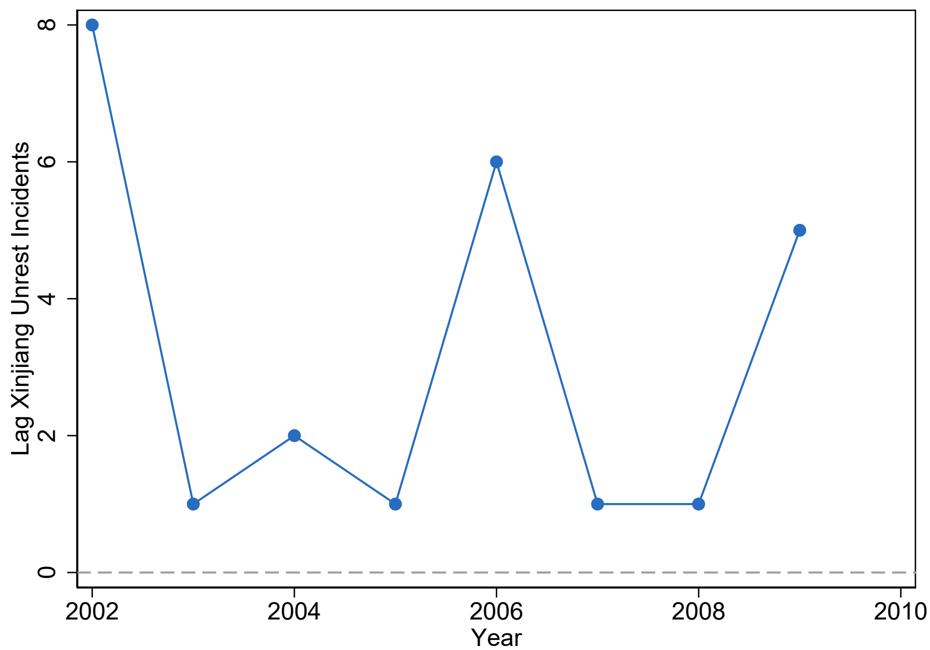
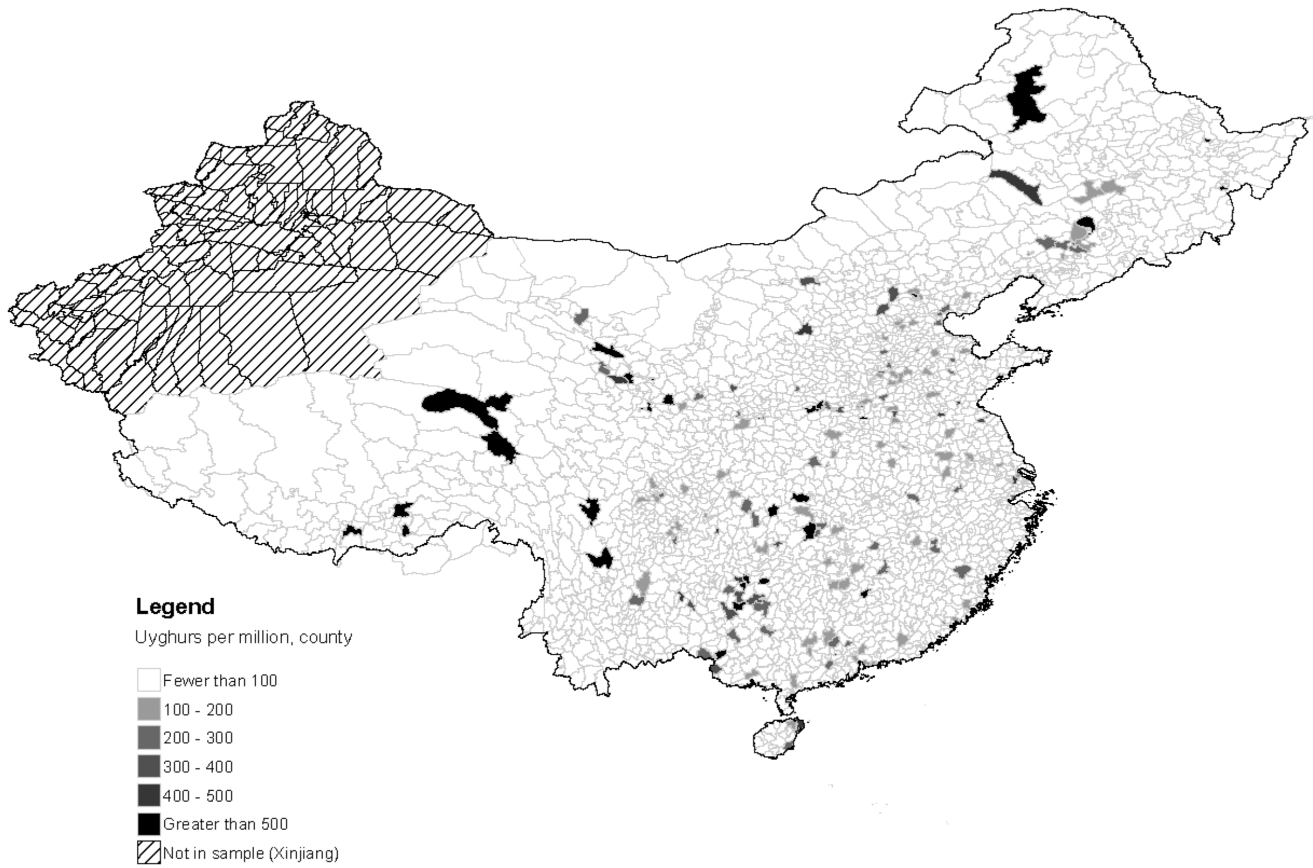


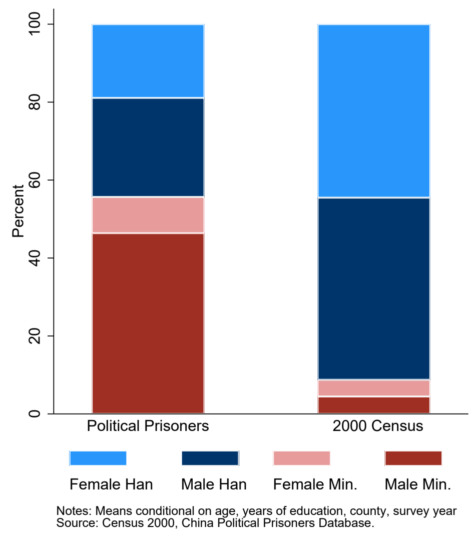
Figure 2: Choropleth of County Uyghur Share Outside Xinjiang



Notes: Data from 2000 Census of China.

Figure 3: Demographic Comparisons

(a) Political Prisoners vs. General Population



(b) Private Firms vs. SOEs

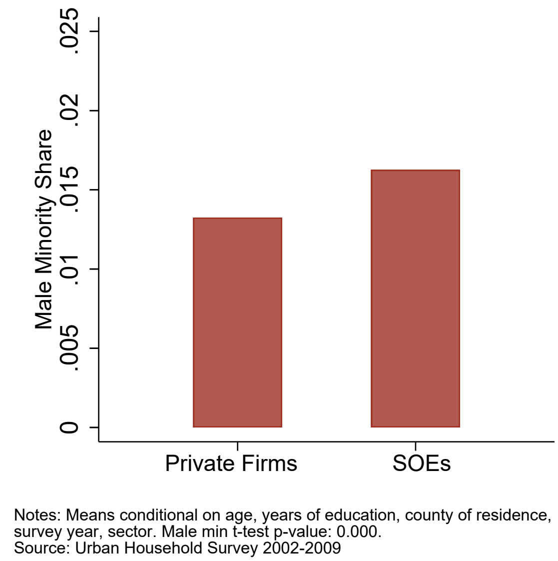
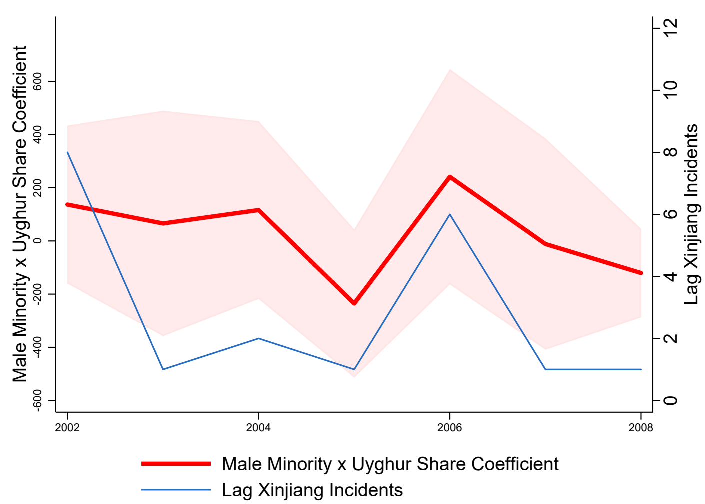
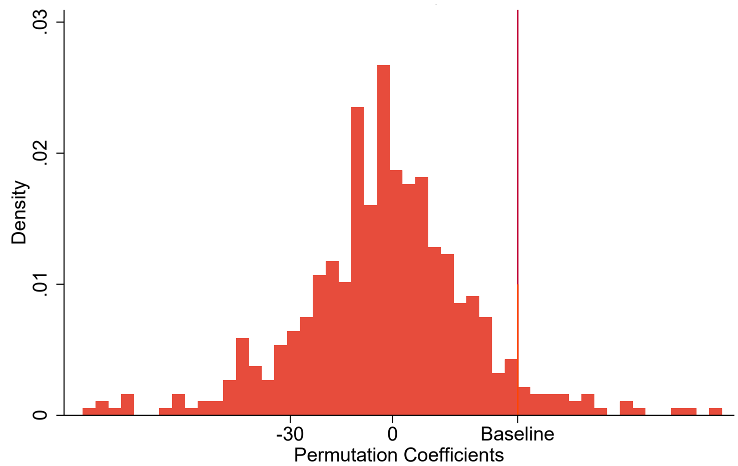


Figure 4: Positive Correlation Between Lag Xinjiang Incidents and Double Interaction



Notes: Double interaction variables are obtained by regressing an indicator for SOE Employment onto the interaction of male minority status, county Uyghur share, and year fixed effects. The coefficient for each year's double interaction is plotted separately along the x-axis. The 95% confidence interval for the double interaction coefficients is shaded red.

Figure 5: Distribution of Triple Interaction Coefficients from Random Permutation Test



Notes: The implied p-value is 0.051 and the test ran for 500 iterations. Coefficients are obtained by re-running the baseline regression with SOE Employment as an outcome variable and counterfactual county Uyghur shares. For each iteration, counties are assigned a Uyghur share value from the existing distribution, without replacement. All other baseline controls are included.

Table 1: County Uyghur Share Summary Statistics

	(1) Mean	(2) S.D.	(3) N	(4) Median	(5) % Obs. > 0	(6) N Obs. > 0
All Counties	1.314	9.784	2870	0.000	11.7%	335
Excl. Xinjiang	0.004	0.026	2774	0.000	8.7%	241
Excl. Xinjiang, Non-Zero	0.042	0.080	241	0.021	100.0%	241

Notes: Data from the 2000 Census of China. Observations are at the county level.

Table 2: County Characteristics by Percent Uyghur Share

	(1)	(2)	(3)	(4)	(5)
		Excluding Xinjiang Counties			
			Non-Zero Uyghur Share		
	All	All	All	Below median	Above median
% Uyghur Share	1.314	0.004	0.042	0.017	0.098
	(9.78)	(.026)	(.08)	(.006)	(.128)
GDP per capita	.711	.71	.776	.833	.651
	(.337)	(.343)	(.429)	(.439)	(.377)
Population	432,062	440,342	665,179	812,826	351,197
	(341,361)	(343,320)	(553,832)	(578,131)	(323,990)
Avg. Years of Schooling	6.861	6.838	7.042	7.346	6.394
	(1.679)	(1.693)	(2.066)	(1.413)	(2.926)
% Illiterate Adults	11.462	11.598	9.931	8.162	13.694
	(11.194)	(11.348)	(10.395)	(4.952)	(16.345)
% Urban	38.461	38.667	43.093	41.439	46.849
	(31.698)	(31.809)	(35.49)	(34.014)	(38.612)
% Employed	5.306	5.294	5.303	5.405	5.073
	(.838)	(.791)	(.889)	(.804)	(1.026)
Observations	3,058	2,962	429	168	261

Notes: GDP data come from the 2000 Provincial Yearbooks and are observed at the province level. All other variables come from the 2000 Census are are observed at the county level.

Table 3: The Effect of Unrest Threat on Employment — Heterogeneity by Minority Status

	(1)	(2)	(3)
Dependent Variable: Employment	SOE	Private	Salary (000s RMB)
Mean of Dependent Variable	*0.550*	*0.250*	*45.51*
Cty. Uyg. Share × Lag Xinjiang Incid.	18.30*	-15.34*	3,131*
× Minority	(10.56)	(8.339)	(1,674)
Observations	224,412	224,412	176,962
R-squared	0.232	0.156	0.432
SUR p-value:	(1) vs. (2) <0.007		

Notes: Observations are at the individual level. All regressions control for year fixed effects; county times minority fixed effects; log kilometers county distance from Xinjiang times year fixed effects; the average base period county employment share by ownership times year and county fixed effects; age, gender, years of education; and these three controls interacted with county Uyghur share and lag Xinjiang incidents. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table 4: The Effect of Unrest Threat on Employment — Heterogeneity by Minority Status and Gender

	(1)	(2)	(3)
Dependent Variable:	SOE	Private	Salary (000s RMB)
	Panel A: Men		
Cty. Uyg. Share × Lag Xinjiang Incid.	37.13***	-23.25*	5,332**
× Minority	(12.16)	(12.01)	(2,065)
Observations	116,239	116,239	98,737
R-squared	0.203	0.146	0.440
	Panel B: Women		
Cty. Uyg. Share × Lag Xinjiang Incid.	0.640	-8.616	305.7
× Minority	(14.09)	(10.09)	(1,261)
Observations	108,173	108,173	78,225
R-squared	0.275	0.191	0.429

Notes: Observations are at the individual level. All regressions control for year fixed effects; county times minority fixed effects; log kilometers county distance from Xinjiang times year fixed effects; the average base period county employment share by ownership times year and county fixed effects; age and years of education and these two controls interacted with county Uyghur share and lag Xinjiang incidents. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table 5: The Effect of Unrest Threat on Employment — Baseline

	(1)	(2)	(3)
Dependent Variable: Employment	SOE	Private	Salary (000s RMB)
Mean of Dependent Variable	*0.550*	*0.250*	*45.51*
Cty. Uyg. Share × Lag Xinjiang Incid.	36.59***	-24.24**	5,422***
× Male Minority	(12.59)	(11.04)	(2,075)
Observations	224,412	224,412	176,962
R-squared	0.231	0.156	0.431
SUR p-value:	(1) vs. (2)		
	<0.000		

Notes: Observations are at the individual level. All regressions control for year fixed effects; county times male minority fixed effects; log kilometers county distance from Xinjiang times year fixed effects; the average base period county employment share by ownership times year and county fixed effects; age, gender, years of education; and these three controls interacted with county Uyghur share and lag Xinjiang incidents. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table 6: The Effect of Unrest Threat on Employment — Robustness to Initial County Strategic Sector Employment Shares

	(1)	(2)	(3)
Dependent Variable: Employment	SOE	Private	Salary (000s RMB)
Cty. Uyg. Share × Lag Xinjiang Incid.	38.70***	-25.38**	5,892***
× Male Minority	(13.85)	(11.57)	(2,024)
Control for Year FE × Male Minority ×			
Cty. Public Service Share, 2002	Y	Y	Y
Cty. Mining Share, 2002	Y	Y	Y
Cty. Utilities Share, 2002	Y	Y	Y
Observations	224,412	224,412	176,962
R-squared	0.232	0.156	0.435

Notes: Observations are at the individual level. All regressions control for year fixed effects; county times male minority fixed effects; log kilometers county distance from Xinjiang times year fixed effects; the average base period county employment share by ownership times year and county fixed effects; age, gender, years of education; and these three controls interacted with county Uyghur share and lag Xinjiang incidents. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table 7: The Effect of Unrest Threat on Employment — Alternative Incident Series

	(1)	(2)	(3)
Dependent Variable:	SOE	Private	Salary (000s RMB)
	Panel A: No Incidents with Triggers Outside Xinjiang		
Cty. Uyg. Share × Lag Alternative Incid. × Male Minority	49.34***	-39.52**	7,051***
	(17.44)	(17.46)	(2,174)
Observations	116,239	116,239	98,737
R-squared	0.203	0.146	0.440
	Panel B: No Incidents with Economic Triggers		
Cty. Uyg. Share × Lag Alternative Incid. × Male Minority	60.08***	-46.63**	7,312***
	(19.20)	(18.14)	(2,336)
Observations	108,173	108,173	78,225
R-squared	0.275	0.191	0.429

Notes: Observations are at the individual level. All regressions control for year fixed effects; county times minority fixed effects; log kilometers county distance from Xinjiang times year fixed effects; the average base period county employment share by ownership times year and county fixed effects; age and years of education and these two controls interacted with county Uyghur share and the appropriate incident time series. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table 8: The Effect of Unrest Threat on Employment — Placebo with Lead of Shock

	(1)	(2)	(3)
Dependent Variable: Employment	SOE	Private	Salary (000s RMB)
Cty. Uyg. Share × Lead Xinjiang Incid. × Male Minority	-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

Notes: Observations are at the individual level. All regressions control for year fixed effects; county times male minority fixed effects; log kilometers county distance from Xinjiang times year fixed effects; the average base period county employment share by ownership times year and county fixed effects; age, gender, years of education; and these three controls interacted with county Uyghur share and lead Xinjiang incidents. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table 9: The Effect of Unrest Threat on Employment — Heterogeneity in Response by Sector

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample:	Manufacturing		Mining and Construction		Retail and Transportation		Services	
Dependent Variable: Employment	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 Xinjiang Incid.	31.34	-31.34	39.05	-39.05	-66.87	66.87	62.02***	-62.02***
× Male Minority	(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

Notes: Observations are at the individual level. All regressions control for year fixed effects; county times male minority fixed effects; log kilometers county distance from Xinjiang times year fixed effects; the average base period county employment share by ownership times year and county fixed effects; age, gender, years of education; and these three controls interacted with county Uyghur share and lag Xinjiang incidents. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table 10: The Effect of Unrest Threat on Social Relief Transfers

	(1)	(2)	(3)	(4)
Dependent Variable:	Social Relief Transfers (000s RMB)			
Sample:	All	SOE	Private	Not Empl.
Mean of Dependent Variable	*0.0200*	*0.0100*	*0.0200*	*0.0300*
Percent Non-Zero Observations	*1.43%*	*0.65%*	*1.92%*	*2.98%*
Cty. Uyg. Share × Lag Xinjiang Incid.	17.51***	6.419**	7.701	88.22**
× Male Minority	(4.703)	(3.042)	(5.733)	(35.63)
Observations	224,412	123,828	55,907	44,677
R-squared	0.017	0.023	0.049	0.045
SUR p-values:		(2) vs. (3)	(2) vs. (4)	(3) vs. (4)
		0.572	0.0211	0.023
Magnitudes (RMB):				
(β) × (Incid. P75-P25). × Uyg. Share mean	0.00319	0.00125	0.00135	0.0137
Magnitude ÷ Percent Non-Zero Observations	0.22	0.19	0.07	0.46

Notes: Observations are at the individual level. All regressions control for year fixed effects; county times male minority fixed effects; log kilometers county distance from Xinjiang times year fixed effects; the average base period county employment share by ownership times year and county fixed effects; age, gender, years of education; and these three controls interacted with county Uyghur share and lag Xinjiang incidents. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table 11: The Effect of Export Demand Shocks on Employment

	(1)	(2)
Dependent Variable: Employment	SOE	Private
Mean of Dependent Variable	*0.650*	*0.170*
Export Demand Shock	-0.0529***	0.0546**
	(0.0203)	(0.0238)
Observations	346,531	346,531
R-squared	0.217	0.124
SUR p-value (1) vs. (2):	0.006	

Notes: Observations are at the individual level. All regressions control for age, years of education, these two controls interacted with gender, year fixed effects, and county fixed effects. Standard errors are clustered at the province-year level. *** p<0.01, ** p<0.05, * p<0.1

Table 12: The Effect of Flood Disasters on Employment

Dependent Variable: Employment	(1) SOE	(2) Private
Mean of Dependent Variable	*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
SUR p-value (1) vs. (2):	0.008	

Notes: Observations are at the individual level. All regressions control for age, years of education, these two controls interacted with gender, year fixed effects, and county fixed effects. Standard errors are clustered at the county-year level. *** p<0.01, ** p<0.05, * p<0.1

References

ACEMOGLU, D. AND J. A. ROBINSON (2006): *Economic Origins of Dictatorship and Democracy*, Cambridge University Press.

ACKERBERG, D. A., K. CAVES, AND G. FRAZER (2015): "Identification Properties of Recent Production Function Estimators," *Econometrica*, 83, 2411–2451.

ADENA, M., R. ENIKOLOPOV, M. PETROVA, V. SANTAROSA, AND E. ZHURAVSKAYA (2015): "Radio and the Rise of the Nazis in Prewar Germany," *The Quarterly Journal of Economics*, 130, 1885–1939.

AKOUM, I. (2012): "The Political Economy of SOE Privatization and Governance Reform in the MENA Region," *International Scholarly Research Notices*, 2012.

ANDERSON, S., P. FRANCOIS, AND A. KOTWAL (2015): "Clientelism in Indian Villages," *American Economic Review*, 105, 1780–1816.

AUTOR, D., D. DORN, AND G. H. HANSON (2013): "The China Syndrome: Local Labor Market Effects of Import Competition in the United States," *The American Economic Review*, 103, 2121–2168.

BAUM, C. F., A. NICHOLS, M. E. SCHAFFER, ET AL. (2010): "Evaluating One-Way and Two-Way Cluster-Robust Covariance Matrix Estimates," in *BOS10 Stata Conference*, Stata Users Group, vol. 11.

BAZZI, S. AND C. BLATTMAN (2014): "Economic Shocks and Conflict: Evidence from Commodity Prices," *American Economic Journal: Macroeconomics*, 6, 1–38.

BEATH, A., F. CHRISTIA, AND R. ENIKOLOPOV (2017): "Can Development Programs Counter Insurgencies?: Evidence from a Field Experiment in Afghanistan," .

BECKER, G. S. (1968): "Crime and Punishment: An Economic Approach," in *The Economic Dimensions of Crime*, Springer, 13–68.

BERMAN, N. AND M. COUTTENIER (2015): "External Shocks, Internal Shots: The Geography of Civil Conflicts," *Review of Economics and Statistics*, 97, 758–776.

BLATTMAN, C. AND J. ANNAN (2016): "Can Employment Reduce Lawlessness and Rebellion? A Field Experiment with High-Risk Men in a Fragile State," *American Political Science Review*, 110, 1–17.

BOVINGDON, G. (2004): "Autonomy in Xinjiang: Han Nationalist Imperatives and Uyghur Discontent," .

——— (2010): *The Uyghurs: Strangers in Their Own Land*, Columbia University Press.

BRAKENRIDGE, G. (2019): "Global Active Archive of Large Flood Events, Dartmouth Flood Observatory, University of Colorado," .

BRANDT, L. AND T. G. RAWSKI (2008): *China's Great Economic Transformation*, Cambridge University Press.

BRANDT, L., J. VAN BIESEBROECK, AND Y. ZHANG (2014): "Challenges of Working with the Chinese NBS Firm-Level Data," *China Economic Review*, 30, 339–352.

BROLLO, F., P. FORQUESATO, AND J. C. GOZZI (2017): "To the Victor Belongs the Spoils? Party Membership and Public Sector Employment in Brazil," *Party Membership and Public Sector Employment in Brazil (October 2017)*.

BROPHY, D. (2016): *Uyghur Nation*, Harvard University Press.

BRÜCKNER, M. AND A. CICCONE (2011): "Rain and the Democratic Window of Opportunity," *Econometrica*, 79, 923–947.

BUCKLEY, C. AND A. RAMZY (2020): "Night Images Reveal Many New Detention Sites in China's Xinjiang Region," .

BUHAUG, H. AND K. S. GLEDITSCH (2008): "Contagion or Confusion? Why Conflicts Cluster in Space," *International Studies Quarterly*, 52, 215–233.

CAI, H. AND Q. LIU (2009): "Competition and Corporate Tax Avoidance: Evidence from Chinese Industrial Firms," *The Economic Journal*, 119, 764–795.

CAMERON, A. C., J. B. GELBACH, AND D. L. MILLER (2011): "Robust Inference with Multiway Clustering," *Journal of Business & Economic Statistics*, 29, 238–249.

CAMERON, L. AND M. SHAH (2014): "Can Mistargeting Destroy Social Capital and Stimulate Crime? Evidence from a Cash Transfer Program in Indonesia," *Economic Development and Cultural Change*, 62, 381–415.

CAMPANTE, F. R., D. CHOR, AND B. LI (2019): "The Political Economy Consequences of China's Export Slowdown," Tech. rep., National Bureau of Economic Research.

CASELLI, F. AND W. J. COLEMAN (2013): "On the Theory of Ethnic Conflict," *Journal of the European Economic Association*, 11, 161–192.

CASELLI, F. AND A. TESEI (2016): "Resource Windfalls, Political Regimes, and Political Stability," *Review of Economics and Statistics*, 98, 573–590.

CAVALLO, E. A. AND I. NOY (2009): "The Economics of Natural Disasters: A Survey," .

CEDERMAN, L.-E., L. GIRARDIN, AND K. S. GLEDITSCH (2009): "Ethnonationalist Triads: Assessing the Influence of Kin Groups on Civil Wars," *World Politics*, 61, 403–437.

CHANG, M. H. (1996): "The Thought of Deng Xiaoping," *Communist and Post-communist Studies*, 29, 377–394.

CHEN, C.-J. J. (2009): "2 Growing Social Unrest in China," *Socialist China, Capitalist China: Social Tension and Political Adaptation Under Economic Globalization*, 10.

CHEN, F. (2006): "Privatization and Its Discontents in Chinese Factories," *The China Quarterly*, 185, 42–60.

CHEN, J. AND C. LU (2011): "Democratization and the Middle Class in China: The Middle Class's Attitudes Toward Democracy," *Political Research Quarterly*, 64, 705–719.

COLLIER, P. AND A. HOEFFLER (1998): "On Economic Causes of Civil War," *Oxford Economic Papers*, 50, 563–573.

——— (2004): "Greed and Grievance in Civil War," *Oxford Economic Papers*, 56, 563–595.

COLONNELLI, E., M. PREM, AND E. TESO (2020): "Patronage and Selection in Public Sector Organizations," *American Economic Review*, 110, 3071–99.

CONGRESSIONAL-EXECUTIVE COMMISSION ON CHINA, T. (2019): "Political Prisoner Database," .

COOK, S. (2002): "From Rice Bowl to Safety Net: Insecurity and Social Protection During China's Transition," *Development Policy Review*, 20, 615–635.

CORNELL, S. AND D. HARTMANN (2006): *Ethnicity and Race: Making Identities in a Changing World*, Sage Publications.

COUGHLIN, K. M. (2006): *Muslim Cultures Today: A Reference Guide*, Greenwood Publishing Group.

CROST, B., J. H. FELTER, AND P. B. JOHNSTON (2016): "Conditional Cash Transfers, Civil Conflict and Insurgent Influence: Experimental Evidence from the Philippines," *Journal of Development Economics*, 118, 171–182.

CSERES-GERGELY, Z., Á. SCHARLE, AND Á. FÖLDESSY (2015): "Evaluating the Impact of a Well-Targeted Wage Subsidy Using Administrative Data," .

DAGAEV, D., N. LAMBEROVA, AND A. SOBOLEV (2019): "Stability of Revolutionary Governments in the Face of Mass Protest," *European Journal of Political Economy*, 60, 101812.

DAVIS, E. V. W. (2008): "Uyghur Muslim Ethnic Separatism in Xinjiang, China," *Asian Affairs: An American Review*, 35, 15–30.

DE MESQUITA, E. B. (2010): "Regime Change and Revolutionary Entrepreneurs," *American Political Science Review*, 104, 446–466.

DE MESQUITA BRUCE, B., A. SMITH, R. M. SIVERSON, AND J. D. MORROW (2003): "The Logic of Political Survival," .

DESAI, R. M., A. OLOFSGÅRD, AND T. M. YOUSEF (2009): "The Logic of Authoritarian Bargains," *Economics & Politics*, 21, 93–125.

DOBSON, W. AND A. K. KASHYAP (2006): "The Contradiction in China's Gradualist Banking Reforms," *Brookings Papers on Economic Activity*, 2006, 103–162.

DONG, X.-Y. AND L. PUTTERMAN (2003): "Soft Budget Constraints, Social Burdens, and Labor Redundancy in China's State Industry," *Journal of Comparative Economics*, 31, 110–133.

DUBE, O. AND J. F. VARGAS (2013): "Commodity Price Shocks and Civil Conflict: Evidence from Colombia," *The Review of Economic Studies*, 80, 1384–1421.

ECONOMIST, T. (2016): "China Sets Itself a Lofty Growth Target," *The Economist Magazine*.

——— (2017): "Reform of China's Ailing State-Owned Firms Is Emboldening Them," *The Economist Magazine*.

EDLUND, L., H. LI, J. YI, AND J. ZHANG (2013): "Sex Ratios and Crime: Evidence from China," *Review of Economics and Statistics*, 95, 1520–1534.

EGOROV, G. AND K. SONIN (2020): "The Political Economics of Non-democracy," Tech. rep., National Bureau of Economic Research.

ESTEBAN, J., L. MAYORAL, AND D. RAY (2012): "Ethnicity and Conflict: Theory and Facts," *Science*, 336, 858–865.

ESTEBAN, J., M. MORELLI, AND D. ROHNER (2015): "Strategic Mass Killings," *Journal of Political Economy*, 123, 1087–1132.

ESTEBAN, J. AND D. RAY (2008): "On the Salience of Ethnic Conflict," *American Economic Review*, 98, 2185–2202.

FEARON, J. D. AND D. D. LAITIN (2003): "Ethnicity, Insurgency, and Civil War," *American Political Science Review*, 75–90.

FETZER, T. (2014): "Social Insurance and Conflict: Evidence from India," *Job Market Paper*, 14.

FIFIELD, A. (2020): "China Is Building Vast New Detention Centers for Muslims in Xinjiang," .

FINAN, F., B. A. OLKEN, AND R. PANDE (2017): "The Personnel Economics of the Developing State," *Handbook of Economic Field Experiments*, 2, 467–514.

FORSBERG, E. (2008): "Polarization and Ethnic Conflict in a Widened Strategic Setting," *Journal of Peace Research*, 45, 283–300.

——— (2014): "Transnational Transmitters: Ethnic Kinship Ties and Conflict Contagion 1946–2009," *International Interactions*, 40, 143–165.

FOX, J. AND S. SANDLER (2003): "Regime Types and Discrimination Against Ethnoreligious Minorities: A Cross-Sectional Analysis of the Autocracy-Democracy Continuum," *Political Studies*, 51, 469–489.

FRIEDRICHS, J. (2017): "Sino-Muslim Relations: The Han, the Hui, and the Uyghurs," *Journal of Muslim Minority Affairs*, 37, 55–79.

GANDHI, A., S. NAVARRO, AND D. A. RIVERS (2011): "On the Identification of Production Functions: How Heterogeneous Is Productivity?" .

GAO, Q., S. WU, AND F. ZHAI (2015): "Welfare Participation and Time Use in China," *Social Indicators Research*, 124, 863–887.

GEHLBACH, S. AND K. SONIN (2014): "Government Control of the Media," *Journal of Public Economics*, 118, 163–171.

GEHLBACH, S., K. SONIN, AND M. W. SVOLIK (2016): "Formal Models of Nondemocratic Politics," *Annual Review of Political Science*, 19, 565–584.

GREGORY, P. R., P. J. SCHRÖDER, AND K. SONIN (2011): "Rational Dictators and the Killing of Innocents: Data from Stalin's Archives," *Journal of Comparative Economics*, 39, 34–42.

GUO, R. (2015): *China's Spatial (Dis)integration: Political Economy of the Interethnic Unrest in Xinjiang*, Chandos Publishing.

GURIEV, S. AND D. TREISMAN (2020): "A Theory of Informational Autocracy," *Journal of Public Economics*, 186, 104158.

HASSID, J. (2008): "Controlling the Chinese Media: An Uncertain Business," *Asian Survey*, 48, 414–430.

HASTINGS, J. V. (2011): "Charting the Course of Uyghur Unrest," *The China Quarterly*, 208, 893–912.

HEGRE, H. AND N. SAMBANIS (2006): "Sensitivity Analysis of Empirical Results on Civil War Onset," *Journal of Conflict Resolution*, 50, 508–535.

HEIDENSOHN, F. AND L. GELSTHORPE (2002): "Gender and Crime," *The Oxford Handbook of Criminology*, 3.

HELLER, S. B. (2014): "Summer Jobs Reduce Violence Among Disadvantaged Youth," *Science*, 346, 1219–1223.

HOROWITZ, D. (1985): "Ethnic Groups in Conflict, University of California Press Ed," .

HSIEH, C.-T. AND P. J. KLENOW (2009): "Misallocation and Manufacturing TFP in China and India," *Quarterly Journal of Economics*, 124, 1403 –1448.

HSIEH, C.-T. AND Z. M. SONG (2015): "Grasp the Large, Let Go of the Small: The Transformation of the State Sector in China," Tech. rep., National Bureau of Economic Research.

HUANG, Y. (2003): *Selling China: Foreign Direct Investment During the Reform Era*, Cambridge University Press.

HUFBAUER, G. C., J. J. SCHOTT, AND K. A. ELLIOTT (1990): *Economic Sanctions Reconsidered: History and Current Policy*, vol. 1, Peterson Institute.

HUSSAIN, A. (1994): "Social Security in Present-Day China and Its Reform," *The American Economic Review*, 84, 276–280.

HUTTUNEN, K., J. PIRTTILÄ, AND R. UUSITALO (2013): "The Employment Effects of Low-wage Subsidies," *Journal of Public Economics*, 97, 49–60.

IREDALE, R., N. BILIK, AND G. FEI (2015): *China's Minorities on the Move: Selected Case Studies: Selected Case Studies*, Routledge.

JEFFERSON, G. H., T. G. RAWSKI, W. LI, AND Z. YUXIN (2000): "Ownership, Productivity Change, and Financial Performance in Chinese Industry," *Journal of Comparative Economics*, 28, 786–813.

JIA, R., M. KUDAMATSU, AND D. SEIM (2015): "Political Selection in China: The Complementary Roles of Connections and Performance," *Journal of the European Economic Association*, 13, 631–668.

JIANG, Z. (1997): "Report to the 15th Party Congress," 13, 97.

JINGRONG, T. (2010): "The Crisis of the Centralized Media Control Theory: How Local Power Controls Media in China," *Media, Culture & Society*, 32, 925–942.

KING, G., J. PAN, AND M. E. ROBERTS (2014): "Reverse-Engineering Censorship in China: Randomized Experimentation and Participant Observation," *Science*, 345.

LAFREE, G. AND L. DUGAN (2007): "Introducing the Global Terrorism Database," *Terrorism and Political Violence*, 19, 181–204.

LANGFORD, I. H. AND T. LEWIS (1998): "Outliers in Multilevel Data," *Journal of the Royal Statistical Society: Series A (Statistics in Society)*, 161, 121–160.

LARDY, N. R. (1993): *Foreign Trade and Economic Reform in China*, Cambridge University Press.

LAURITSEN, J. L., K. HEIMER, AND J. P. LYNCH (2009): "Trends in the Gender Gap in Violent Offending: New Evidence from the National Crime Victimization Survey," *Criminology*, 47, 361–399.

LEETARU, K. AND P. A. SCHRODT (2013): "Gdelt: Global Data on Events, Location, and Tone, 1979–2012," in *ISA Annual Convention*, Citeseer, vol. 2, 1–49.

LEUTERT, W. (2016): "Challenges Ahead in China's Reform of State-Owned Enterprises," *Asia policy*, 21, 83–99.

LI, S. AND J. XIA (2008): "The Roles and Performance of State Firms and Non-state Firms in China's Economic Transition," *World Development*, 36, 39–54.

LIN, J. Y., F. CAI, AND Z. LI (1998): "Competition, Policy Burdens, and State-Owned Enterprise Reform," *The American Economic Review*, 88, 422–427.

LORENTZEN, P. (2014): "China's Strategic Censorship," *American Journal of Political Science*, 58, 402–414.

MA, N. AND E. W. CHENG (2019): *The Umbrella Movement: Civil Resistance and Contentious Space in Hong Kong*, Amsterdam University Press.

MARKEVICH, A., N. NAUMENKO, AND N. QIAN (2021): "The Political-Economic Causes of the Soviet Great Famine, 1932–33," Tech. rep., National Bureau of Economic Research.

MARTINEZ-BRAVO, M., G. P. I. MIQUEL, N. QIAN, AND Y. YAO (2017): "The Rise and Fall of Local Elections in China: Theory and Empirical Evidence on the Autocrat's Trade-off," .

MCKENZIE, D., D. MOOKHERJEE, G. CASTAÑEDA, AND J. SAAVEDRA (2003): "The Distributive Impact of Privatization in Latin America: Evidence from Four Countries [with comments]," *Economia*, 3, 161–233.

MEHTA, C. R., N. R. PATEL, AND R. GRAY (1985): "Computing an Exact Confidence Interval for the Common Odds Ratio in Several $2\times$ 2 Contingency Tables," *Journal of the American Statistical Association*, 80, 969–973.

MIGUEL, E., S. SATYANATH, AND E. SERGENTI (2004): "Economic Shocks and Civil Conflict: An Instrumental Variables Approach," *Journal of Political Economy*, 112, 725–753.

MILLWARD, J. (2004): "Violent Separatism in Xinjiang: A Critical Assessment," *Policy studies*, I.

MONTAGNES, B. P. AND S. WOLTON (2019): "Mass Purges: Top-Down Accountability in Autocracy," *American Political Science Review*, 113, 1045–1059.

MONTALVO, J. G. AND M. REYNAL-QUEROL (2005): "Ethnic Polarization, Potential Conflict, and Civil Wars," *American Economic Review*, 95, 796–816.

ORNAGHI, A. (2016): "Civil Service Reforms: Evidence from US Police Departments," .

PEOPLE'S DAILY, T. (2001): "Development Is Absolute Principle Interpreted," .

POPKIN, S. L. (1979): *The Rational Peasant: The Political Economy of Rural Society in Vietnam*, Univ of California Press.

POWELL, R. (2013): "Monopolizing Violence and Consolidating Power," *The Quarterly Journal of Economics*, 128, 807–859.

QIAN, Y. (2000): "The Process of China's Market Transition (1978-1998): The Evolutionary, Historical, and Comparative Perspectives," *Journal of Institutional and Theoretical Economics (JITE)/Zeitschrift für die gesamte Staatswissenschaft*, 151–171.

ROANTREE, A. M. (2019): "What's Next for Hong Kong's Protest Movement," *Reuters*.

ROZENAS, A. (2020): "A Theory of Demographically Targeted Repression," *Journal of Conflict Resolution*, 64, 1254–1278.

ROZENAS, A. AND D. STUKAL (2019): "How Autocrats Manipulate Economic News: Evidence from Russia's State-Controlled Television," *The Journal of Politics*, 81, 982–996.

SHADMEHR, M. (2018): "Multiplicity and Uniqueness in Generalized Regime Change Games," *Available at SSRN 3121241*.

SHI, P. (2016): *Natural Disasters in China*, Springer.

SONG, Z., K. STORESLETTEN, AND F. ZILIBOTTI (2011): "Growing Like China," *The American Economic Review*, 101, 196–233.

STEELE, L. AND R. KUO (2007): "Terrorism in Xinjiang?" *Ethnopolitics*, 6, 1–19.

SVANBERG, I. (1988): *The Altaic-Speakers of China: Numbers and Distribution*, vol. 14, Centre for Mult [i] ethnic Research, Uppsala University, Faculty of Arts.

THE ECONOMIST (2021): "What Is Happening to the Uyghurs in Xinjiang?" .

THE NATIONAL BUREAU OF STATISTICS (2010): "2000 Population Census of China," *Chinese Statistical Bureau, Beijing, China.*

——— (2018): "China Statistical Yearbook," *Chinese Statistical Bureau, Beijing, China.*

TOOPS, S. (2009): "Understanding the Disturbances in Xinjiang," .

TYSON, S. A. (2018): "The Agency Problem Underlying Repression," *The Journal of Politics*, 80, 1297–1310.

UNITED NATIONS, T. (2016): "United Nations Comtrade Database," .

URDAL, H. (2004): "The Devil in the Demographics: The Effect of Youth Bulges on Domestic Armed Conflict, 1950-2000," *Social Development Papers*, 14.

VODOPIVEC, M., M. H. TONG, ET AL. (2008): *China: Improving Unemployment Insurance*, World Bank, Social protection & Labor.

WEAVER, J. (2021): "Jobs for Sale: Corruption and Misallocation in Hiring," *American Economic Review*.

WEIDMANN, N. B. (2015): "Communication Networks and the Transnational Spread of Ethnic Conflict," *Journal of Peace Research*, 52, 285–296.

WONG, L. (2005): *Marginalization and Social Welfare in China*, Routledge.

WRIGHT, T. (2019): *Handbook of Protest and Resistance in China*, Edward Elgar Publishing.

XU, G. (2018): "The Costs of Patronage: Evidence from the British Empire," *American Economic Review*, 108, 3170–98.

YANAGIZAWA-DROTT, D. (2014): "Propaganda and Conflict: Evidence from the Rwandan Genocide," *The Quarterly Journal of Economics*, 129, 1947–1994.

YAO, S. (2016): *Agricultural Reforms and Grain Production in China*, Springer.

9 Online Appendix *(not for publication)*

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

9.1 Full Conceptual Framework

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

9.1.1 Individuals

Since individuals are identical within type, their behavior can be expressed via those of two representative consumers. I use index $j \in \{N, U\}$ when discussing both 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.

Representative consumers are endowed with time, h. They earn income from working and cannot spend more than they earn, such that $c^j \leq w_j L^j$. Since individuals do not value income other than for consumption, this constraint will hold with equality.

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

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

$$\frac{u_\ell\left(\ell^{j*}, c^{j*}\right)}{u_c\left(\ell^{j*}, c^{j*}\right)} = w_j. \tag{14}$$

9.1.2 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 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 (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 $lim_{i\to\infty} F_i(U,N) = 0$ and $lim_{i\to 0} F_i(U,N) = \infty$ for $i \in (U,N)$. The firm faces a tax on N-type labor. The representative private firm solves:

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

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

$$\frac{F_U^{priv*}}{F_N^{priv*}} = \frac{w_U}{w_N(1-\tau_N)}. \tag{15}$$

9.1.3 SOEs

Let there be many state-owned firms, each operating with constant returns to scale. Production can again be expressed with a representative firm producing with constant returns to scale and earning zero profits. Let there be no free entry of SOEs, to mimic the controls on SOE entry observed in the real world.[20]

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

$$\frac{F_U^{soe*}}{F_N^{soe*}} = \frac{w_U\left(1-\tau_U\right)}{w_N\left(1-\tau_N\right)}. \tag{16}$$

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

$$max_{\tau_U,\tau_N}\, Y^{priv} + Y^{soe} + \eta S\left(-\xi z\left(\ell^U\right)\right) \tag{17}$$

$$\text{s.t. } \tau_U w_U U^{soe} + \tau_N w_N N = 0$$

In equilibrium, the firm's optimization problem, the consumer's optimization prob-

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

lem, 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.

$$G^{**} = max_{\tau_U} Y^{priv}(\tau_U) + Y^{soe}(\tau_U) + \eta S(-Z(h - U(\tau_U)) - \xi) \tag{18}$$

The first order condition of the government is:

$$\frac{dY^{soe}}{d\tau_U} + \frac{dY^{priv}}{d\tau_U} + \eta\xi \underbrace{\frac{dS}{dZ}}_{\ominus} \underbrace{\frac{dz}{dU}}_{\ominus} \frac{dU}{d\tau_U} = 0. \tag{19}$$

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

$$G^{*} = max_{\tau_L} Y^{priv}(\tau_U) + Y^{soe}(\tau_U). \tag{20}$$

9.1.5 Market Clearing

In equilibrium, Equations (14), (15), and (16) 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.

$$Y^{soe*} - w_U(1-\tau_U)U^{soe*} - w_N(1-\tau_N)N^{soe*} = 0 \tag{21}$$

$$Y^{priv*} - w_U U^{priv*} - w_N(1-\tau_N)N^{priv*} = 0 \tag{22}$$

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: $Y = w_U U + w_N N$.

9.1.6 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. In the interest of brevity and clarity, I focus on the intuition behind these results and present full proofs of each in the Online Mathematical Appendix at this link.

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

9.1.7 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 (23). Note that the labor supply curve does not change with respect to τ_U.

$$w_U = \frac{u_\ell\left(\ell^U, c^U\right)}{u_c\left(\ell^U, c^U\right)} \tag{23}$$

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.

$$w_U = \frac{F_U^{soe}\Big|_{N=\overline{N^S}}}{(1-\tau_U)} \tag{24}$$

$$w_U = F_U^{priv}\Big|_{N=\overline{N^P}} \tag{25}$$

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 Online Appendix Figure A.3a. 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 Appendix Figure A.4a.

As Appendix Figure A.4a 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**.

9.1.8 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 (26). Note that the labor supply curve does not change with respect to τ_U.

$$w_U = \frac{u_\ell\left(\ell^N, c^N\right)}{u_c\left(\ell^N, c^N\right)} \tag{26}$$

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.

$$w_N = \left.\frac{F_N^{soe}}{(1-\tau_N)}\right|_{L=L^{soe*}} \tag{27}$$

$$w_N = \left.\frac{F_N^{priv}}{(1-\tau_N)}\right|_{L=L^{priv*}} \tag{28}$$

I draw a representation of these curves in Online Appendix Figure A.3b. 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^*).

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 Appendix Figure A.4b.

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**.

9.1.9 Testable Predictions

The key testable relationships that emerge from this model are how SOE and private employment respond to instability shocks. Empirically, these shocks map 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_U. Recall the government's first order condition:

$$\underbrace{\frac{dY}{d\tau_U}}_{\text{"output cost"}<0} + \underbrace{\eta\xi \underbrace{\frac{dS}{dZ}}_{\ominus}\underbrace{\frac{dz}{dU}}_{\ominus}\frac{dU}{d\tau_U}}_{\text{"stability benefit"}>0} = 0. \tag{29}$$

As long as $\eta > 0$, the marginal benefit of τ_U is increasing in ξ, and we have $\frac{d\tau_U^*}{d\xi} > 0$. By combining this insight with Propositions 2, 3, and 4, I derive the following testable predictions. If $\eta > 0$:

$$\text{Prediction 1} \quad \frac{dU^{soe*}}{d\xi} - \frac{dN^{soe*}}{d\xi} > 0$$

$$\text{Prediction 2} \quad \frac{dU^{priv*}}{d\xi} - \frac{dN^{priv*}}{d\xi} < 0$$

$$\text{Prediction 3} \quad \frac{dw_U^*}{d\xi} - \frac{dw_N^*}{d\xi} > 0$$

In other words, if the government values social stability, unrest threats lead U-type employment to differentially increase in SOEs and differentially decrease in private firms. Additionally, wages differentially rise for U-types.

9.2 Additional Robustness Checks

In this subsection, I describe 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 the baseline regressions, I defined SOEs as officially-registered state-owned firms and urban collectives. However, collectives may behave differently, so I omit collective workers from the sample and re-run the baseline regressions in Appendix Table A.9. I find the SOE and salary results are robust to this change, but the private triple-interaction coefficient 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.10 shows that the removal of these controls does not affect the sign or precision of the SOE and private coefficients, but does change the salary result to a null. This change suggests that demographic and pre-period economic controls are particularly important for the salary result.

Another robustness check addresses potential mis-measurement 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 the annual Xinjiang incident count, one still might be concerned that this measure is altered by government censorship or fabrication. Additionally, it may be difficult to determine how to define separate incidents. For example, in 2008, a string of attacks by ethnic Uyghurs against Xinjiang police occurred in Kashgar prefecture in close succession. The closest incidents in the data occured on August 27 and September 2. By default, I treat events on separate calendar days as separate incidents, but one could argue that these attacks were part of one larger separatist action.

To address these problems of government manipulation and incident ambiguity, I perform an additional robustness check. In lieu of the count of Xinjiang incidents per year, I use a binary measure for low-incident and high-incident years in the specification. I code all years with one (the sample median) or fewer lag Xinjiang incidents as a 0, and all years with two or more lag Xinjiang incidents as a 1, resulting in five years coded as following low conflict and three years (2002, 2006, and 2009) coded as following high conflict. Appendix Table A.11 reports estimates using this alternative measure. The triple interaction coefficients for SOE employment and salary remain positive and significant but become much larger in magnitude. Similarly, the triple interaction coefficient for private employment remains negative and significant but is much larger in magnitude.

In principle, since the unrest shock varies at the county and year level, I test whether my results are robust to using two-way clustered standard errors, with counties and years as the units of clustering. Results are reported in Appendix Table A.12. The SOE coefficient remains precise, but the private and salary results are no longer precisely different from zero. These results should be interpreted with caution due to the short panel of just 8 years (Cameron et al., 2011).

9.3 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 by definition 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 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 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 apply the data preparation procedure first used in Cai and Liu (2009) and 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. Online Appendix Figure A.5 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.

9.4 SOEs Exhibit Lower Productivity

Because I argue that the stability role of SOEs has potentially large economic consequences, I corroborate a 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.

Online Appendix Figure A.6 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 9.3. The firm productivity measures have been normalized by sector and province medians to ensure comparability across industries and places. 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 firm-level regression.

$$TFPR_{ipst} = \alpha + \beta SOE_{it} + \tau_t + \eta_p + \gamma_s + \varepsilon_{ipst} \tag{30}$$

Here, 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 almost all privatized SOEs.

Results from this regression are reported in Table A.13. Across all measures, SOEs are significantly less productive than their domestic counterparts in the same year, province, and industry. This pattern is robust to controlling for 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.

I also observe an interesting correlation in the data: the per capita GDP of provinces is negatively correlated with provincial SOE employment share. I plot this relationship in Appendix Figure A.7, 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.1: Urban SOE Employment Over Time (millions of workers)

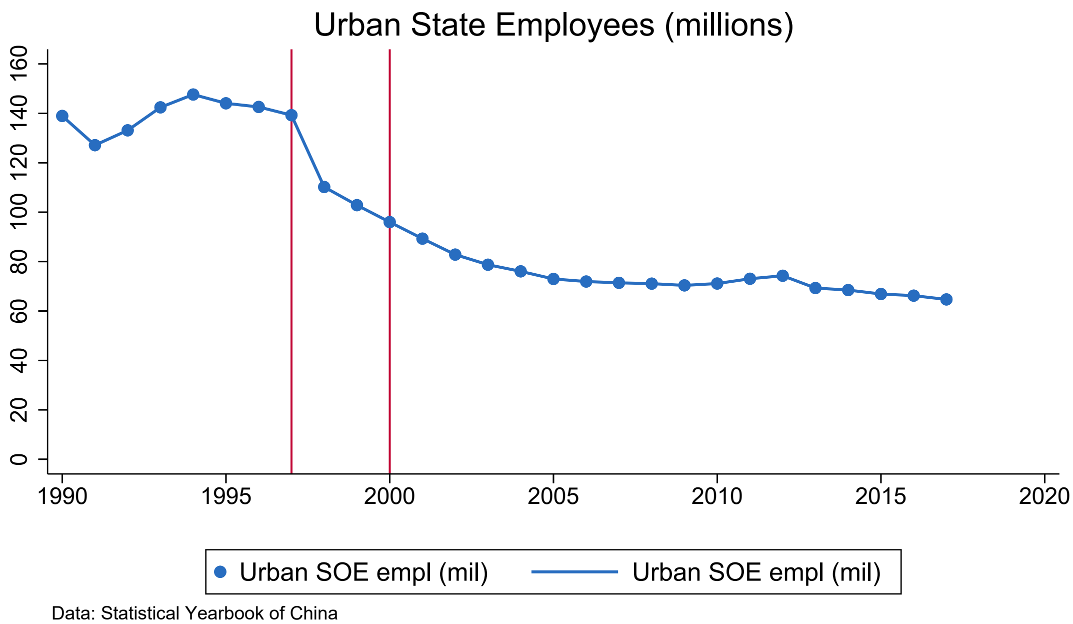
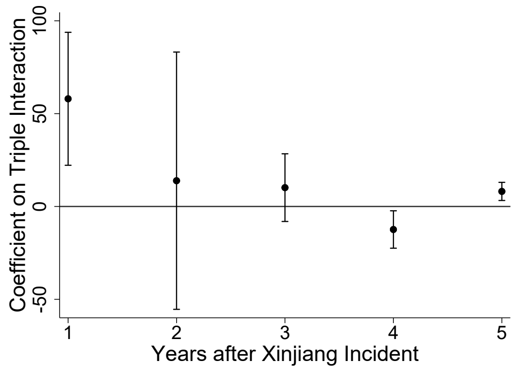
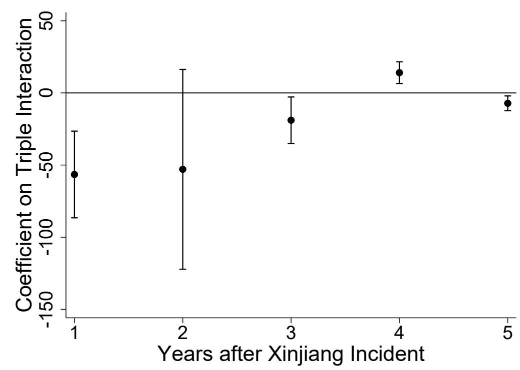


Figure A.2: The Effect of Unrest Threat on Employment — Medium-Run Responses

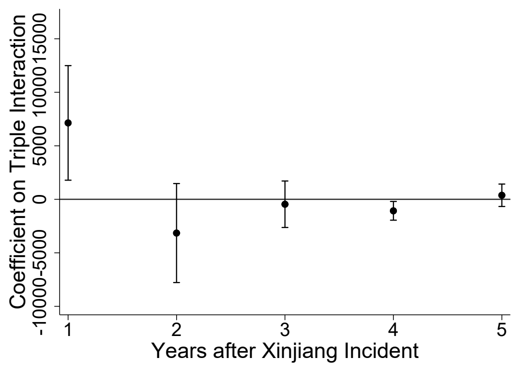
(a) SOE Employment



(b) Private Employment



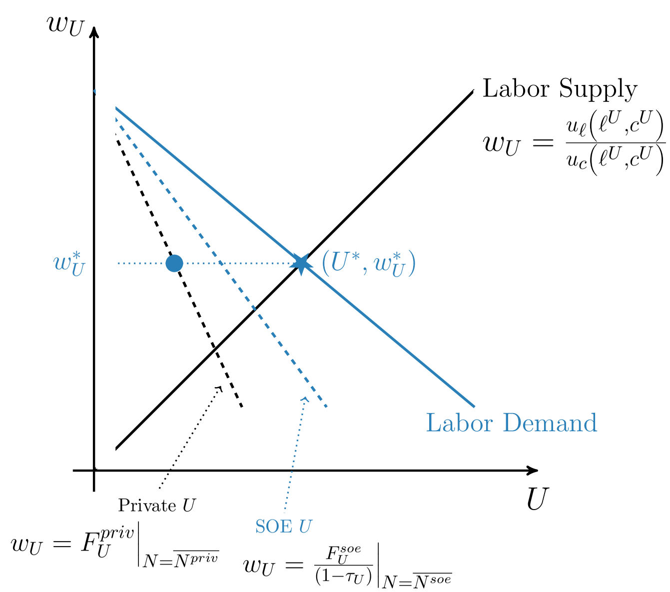
(c) Salary



Notes: These coefficients come from a regression in which all five lags of the triple interaction of interest are included simultaneously in addition to all of baseline control variables. A test for the joint significance of all five lag coefficients yields p < 0.001 for SOE employment, private employment, and salary.

Figure A.3: Model Equilibrium

(a) U-type Labor Market



(b) N-type Labor Market

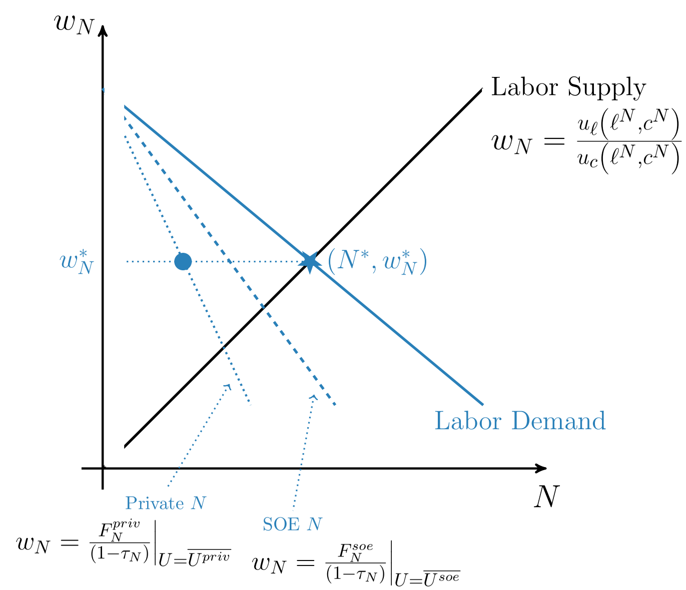


Figure A.4: Equilibrium Responses to $\tau_U \uparrow$

(a) U-type Labor Market

(b) N-type Labor Market

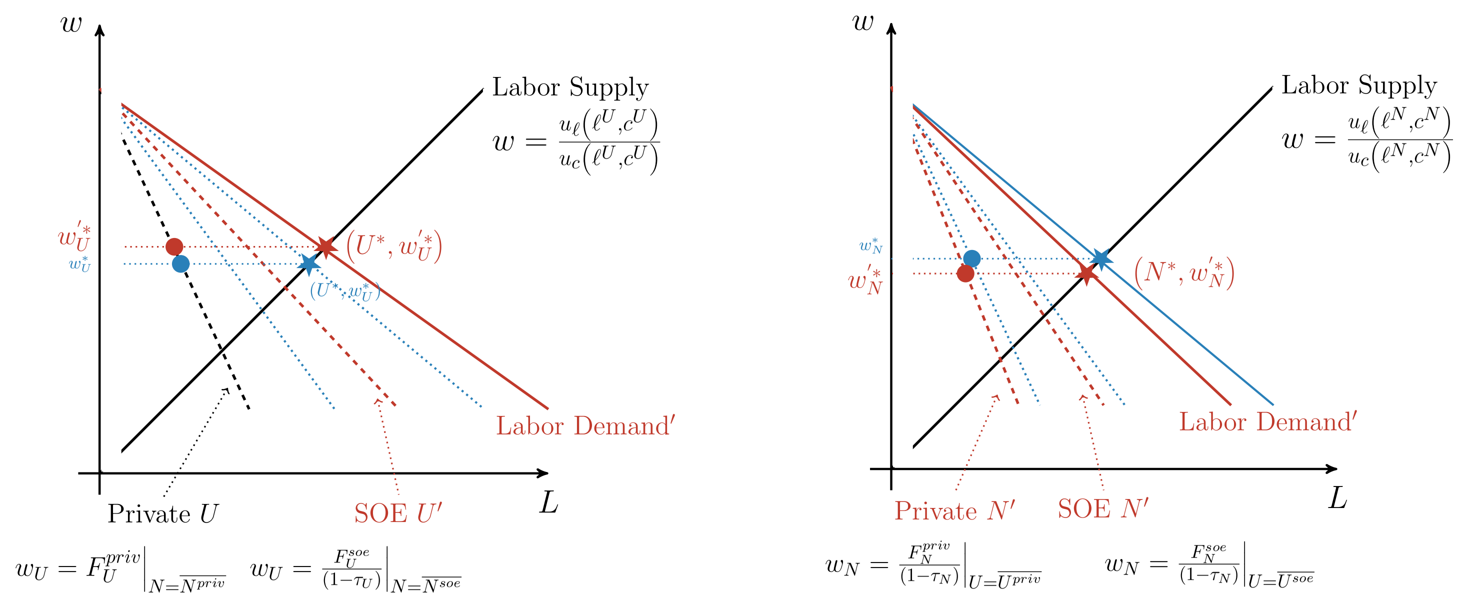


Figure A.5: Share of GDP covered by the *Annual Survey of Industrial Production*

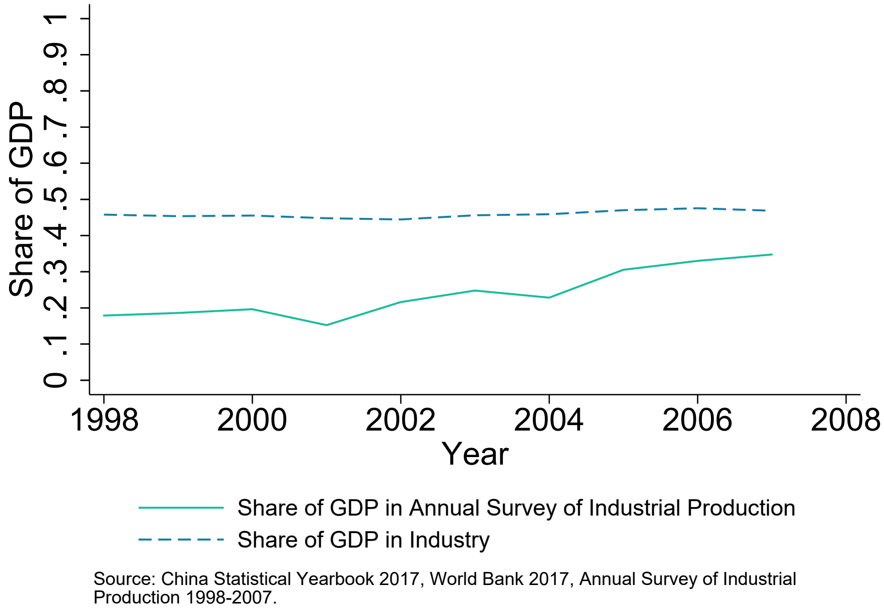
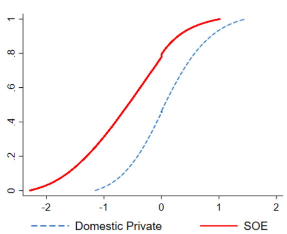
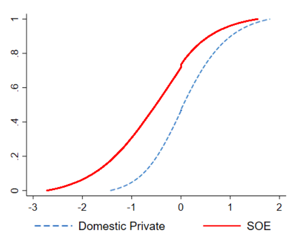


Figure A.6: Firm Productivity by Ownership

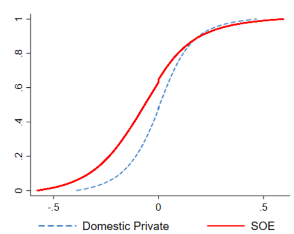
(a) Labor Productivity



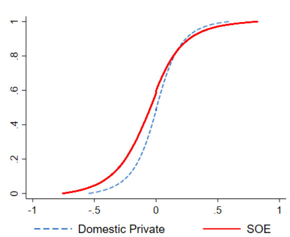
(b) TFPR (Hsieh and Klenow, 2009)



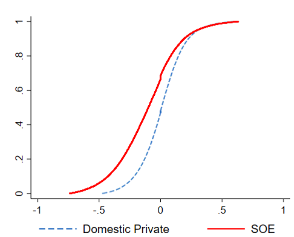
(c) TFPR (OLS)



(d) TFPR (Ackerberg et al., 2015)



(e) TFPR (Gandhi et al., 2011)



Notes: These figures plot the cumulative distribution of five alternative productivity measures by firm ownership. Data are from the Annual Survey of Industrial Production. Each productivity measure is demeaned by four-digit sector and province.

Figure A.7: Cross-province Relationship between GDP and SOE Share

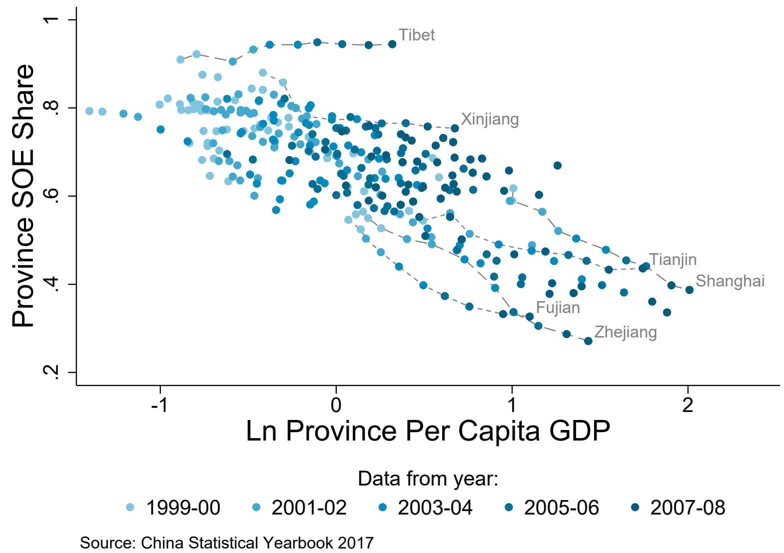


Table A.1: Individual Characteristics by Minority Status

	(1) Non-Minorities	(2) Minorities	(3) Male Minorities
Age	36.2	35.3	35.9
	(9.367)	(9.694)	(9.738)
Male	.518	.508	-
	(.5)	(.5)	-
Years of Schooling	12.18	12.39	12.47
	(2.542)	(2.574)	(2.535)
Salary (000s of RMB)	45.8	38.5	37.9
	(39.2)	(28.8)	(29.1)
Employed	.883	.879	.931
	(.322)	(.326)	(.253)
Employed in SOE	.55	.60	.64
	(.50)	(.49)	(.48)
Employed in Private	.251	.209	.216
	(.433)	(.407)	(.411)
Observations	216,325	8,087	4,105

Notes: Data are from the Urban Household Survey, 1998-2009.

Table A.2: The Effect of Unrest Threat on Employment — Omit Strategic Sectors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample:	Omit Public Administration			Omit Mining			Omit Utilities		
Dependent Variable: Employment	SOE	Private	Salary	SOE	Private	Salary	SOE	Private	Salary
Cty. Uyg. Share × Lag Xinjiang Incid. × Male Minority	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

Notes: Observations are at the individual level. All regressions control for year fixed effects; county times male minority fixed effects; log kilometers county distance from Xinjiang times year fixed effects; the average base period county employment share by ownership times year and county fixed effects; age, gender, years of education; and these three controls interacted with county Uyghur share and lag Xinjiang incidents. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table A.3: The Effect of Unrest Threat on Employment — Omit Outliers

	(1)	(2)	(3)
Dependent Variable: Employment	SOE	Private	Salary (000s RMB)
Cty. Uyg. Share × Lag Xinjiang Incid. × Male Minority	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

Notes: Observations are at the individual level. This table omits all observations with DFITS greater than 2*(k/N)^0.5. All regressions control for age, gender, years of education, these three controls interacted with county Uyghur share and lag Xinjiang incidents, 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, year fixed effects, and county fixed effects. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table A.4: The Effect of Unrest Threat on Employment — Hui Share Placebo

	(1)	(2)	(3)
Dependent Variable: Employment	SOE	Private	Salary (000s RMB)
Cty. Hui Share × Lag Xinjiang Incid. × Male Minority	3.45e-05	-2.11e-05	0.00271
	(2.98e-05)	(2.20e-05)	(0.00253)
Observations	224,412	224,412	176,962
R-squared	0.233	0.158	0.431

Notes: Observations are at the individual level. All regressions control for the entire baseline equation, Hui share interacted with male minority, Hui share interacted with Lag Xinjiang incidents, plus age, gender, years of education interacted with county Hui share. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table A.5: The Effect of Export Demand Shocks on Employment — Robustness to Sector Composition and Global China Shares

	(1)	(2)	(3)	(4)
	County base year sector composition * Year FE		Sectors wherein China is <5% global trade	
Dependent Variable: Employment	SOE	Private	SOE	Private
Mean of Dependent Variable	*0.650*	*0.170*	*0.650*	*0.170*
Export Demand Shock	-0.0447**	0.0480***	-0.0516**	0.0535**
	(0.0172)	(0.0175)	(0.0208)	(0.0249)
Observations	346,531	346,531	346,531	346,531
R-squared	0.218	0.125	0.217	0.124

Controls: Observations are at the individual level. All regressions control for age, years of education, these two controls interacted with gender, year fixed effects, and county fixed effects. Standard errors are clustered at the province-year level. *** p<0.01, ** p<0.05, * p<0.1

Table A.6: The Effect of Export Demand Shocks on Employment — Placebo

	(1)	(2)
Dependent Variable: Employment	SOE	Private
Mean of Dep. Var.	*0.660*	*0.160*
Lead of Export Demand Shock	-0.0486	0.0267
	(0.0493)	(0.0393)
Observations	291,203	291,203
R-squared	0.214	0.111

Notes: Observations are at the individual level. All regressions control for age, years of education, these two controls interacted with gender, year fixed effects, and county fixed effects. Standard errors are clustered at the province-year level. *** p<0.01, ** p<0.05, * p<0.1

Table A.7: The Effect of Flood Disasters on Employment — Robustness to Base Year Sector Shares

	(1)	(2)
Dependent Variable: Employment	SOE	Private
Mean of Dep. Var.	*0.550*	*0.250*
Lag County Flood Indicator	0.0710**	-0.0913***
	(0.0339)	(0.0315)
Observations	225,039	225,039
R-squared	0.248	0.166

Notes: Observations are at the individual level. All regressions control for age, years of education, these two controls interacted with gender, year fixed effects, county fixed effects, and for the share of each sector within each county for the first year the county appears in the dataset interacted with year fixed effects. Standard errors are clustered at the province-year level. *** p<0.01, ** p<0.05, * p<0.1

Table A.9: The Effect of Unrest Threat on Employment — Omit Collective Firms

Dependent Variable: Employment	(1) SOE	(2) Private	(3) Salary (000s RMB)
Cty. Uyg. Share × Lag Xinjiang Incid. × Male Minority	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

Notes: Observations are at the individual level. All regressions control for year fixed effects; county times male minority fixed effects; log kilometers county distance from Xinjiang times year fixed effects; the average base period county employment share by ownership times year and county fixed effects; age, gender, years of education; and these three controls interacted with county Uyghur share and lag Xinjiang incidents. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table A.8: The Effect of Flood Disasters on Employment — Placebo

Dependent Variable: Employment	(1) SOE	(2) 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

Notes: Observations are at the individual level. All regressions control for age, years of education, these two controls interacted with gender, year fixed effects, and county fixed effects. Standard errors are clustered at the county-year level. *** p<0.01, ** p<0.05, * p<0.1

Table A.10: The Effect of Unrest Threat on Employment — Sparse Specification

	(1)	(2)	(3)
Dependent Variable: Employment	SOE	Private	Salary (000s RMB)
Cty. Uyg. Share × Lag Xinjiang Incid. × Male Minority	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

Notes: Observations are at the individual level. All regressions control for log kilometers county distance from Xinjiang times year fixed effects, year fixed effects, and county fixed effects. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table A.11: The Effect of Unrest Threat on Employment — Robustness to Binary Measure of Xinjiang Conflict Intensity

	(1)	(2)	(3)
Dependent Variable: Employment	SOE	Private	Salary (000s RMB)
Cty. Uyg. Share × Lag Binary Xinjiang Incid. × Male Minority	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

Notes: Observations are at the individual level. All regressions control for year fixed effects; county times male minority fixed effects; log kilometers county distance from Xinjiang times year fixed effects; the average base period county employment share by ownership times year and county fixed effects; age, gender, years of education; and these three controls interacted with county Uyghur share and lag Xinjiang incidents. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table A.12: The Effect of Unrest Threat on Employment — Two-Way Clustered Standard Errors

Dependent Variable: Employment	(1) SOE	(2) Private	(3) Salary (000s RMB)
Cty. Uyg. Share × Lag Xinjiang Incid.	36.59**	-24.24	5,422
× Male Minority	(11.87)	(15.06)	(3,081)
Observations	224,353	224,353	176,907
R-squared	0.231	0.156	0.431
Clusters:			
Counties	492	492	492
Years	8	8	8

Notes: Observations are at the individual level. All regressions control for year fixed effects; county times male minority fixed effects; log kilometers county distance from Xinjiang times year fixed effects; the average base period county employment share by ownership times year and county fixed effects; age, gender, years of education; and these three controls interacted with county Uyghur share and lag Xinjiang incidents. Standard errors are clustered two ways at the county and year level. *** p<0.01, ** p<0.05, * p<0.1

Table A.13: SOE vs. Domestic Private Manufacturing Productivity

Dependent Variable:	(1) Labor Productivity	(2) TFPR (HK)	(3) TFPR (OLS)	(4) TFPR (ACF)	(4) 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

Notes: Observations are at the firm-year level. All regressions control for year fixed effects, province fixed effects, and four-digit Chinese Industrial Code fixed effects. Standard errors are clustered at the industrial code level. Data come from the Annual Survey of Industrial Production, 1998-2008.