STATE EMPLOYMENT AS A STRATEGY OF AUTOCRATIC CONTROL IN CHINA*

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Abstract

This paper presents new evidence that autocrats use state-owned enterprises to pacify social unrest via employment provision, which may reinforce state firms' low productivity and persistence across settings. I use variation in a regional conflict between Uyghur separatists and the Chinese government to establish that, in years and counties with a higher threat of unrest, state-owned firms hire more male minorities, the demographic most likely to participate in ethnic conflict. Concurrently, wages rise and private employment falls among this group. These patterns are consistent with a theoretical framework of government-subsidized, pacification-motivated state employment, and a quantification exercise indicates that state firms implicitly receive a 26% subsidy on male minority wages. Furthermore, I find that state employment increases after poor trade shocks and natural disasters, consistent with a general role in preventing unrest.

Keywords: Autocracy, state-owned enterprises, ethnic conflict, public employment
JEL codes: O12, P26, D74, J45, J15

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1 Introduction

All governments must manage unrest to stay in power, particularly autocracies. To do so, they may use instruments like surveillance and armed force, or economic policies like social insurance and skills training (Fetzer, 2020; Blattman and Annan, 2016). While key drivers of unrest like income shocks (Dube and Vargas, 2013; Bazzi and Blattman, 2014) and ethnic divisions (Montalvo and Reynal-Querol, 2005) are relatively well-documented, there is significantly less work studying pacification programs, especially their deployment *in situ*. Understanding the unrest-prevention role of economic policies may also illuminate why some institutions resist reform, despite important economic downsides.

The central contribution of this paper is to provide causal evidence that state-owned enterprise (SOE) employment is a tool of autocratic control, a motive that can help explain why some governments are protective of large, unproductive state sectors despite their aggregate productivity costs. SOEs are a widespread phenomenon that represent one-sixth of global output and one-fifth of global investment, even though they are less productive than private firms (World Bank, 2014). They have also resisted reform in many settings, even increasing their reach in recent years. One salient example of this dynamic is China: the government highly values economic growth, yet SOEs comprise 40% of the economy's fixed assets and 20% of employment, and are substantially less productive than their private counterparts (Economist, 2015). Unrest prevention may be one reason for this apparent contradiction.

However, empirically testing whether unrest prevention motivates state employment is challenging for three key reasons. First, omitted variables may drive both social unrest and employment 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 by leveraging a unique natural experiment generated by the ongoing Uyghur ethnic conflict located in Xinjiang, China's westernmost province. Separatists cite discriminatory and oppressive policies as motives and over 85% are male ethnic Uyghurs (Congressional-Executive Commission on China, 2019). Since 2018, the government has escalated policy interventions substantially, drawing international attention and condemnation (The Economist, 2021; Buckley and Ramzy, 2020; Fifield, 2020).

Within this setting, I design a triple-differences strategy that combines temporal variation in conflict intensity, 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. The first two elements of the triple interaction capture the fact that conflict propagation to non-Xinjiang counties is more likely in years preceded by many Xinjiang unrest incidents, in counties with large Uyghur population shares, and I validate this claim by showing that Uyghur cultural practices intensify in exactly those times and places. The third element captures the fact that a government with limited resources would target pacification policies on the likeliest unrest participants: male minorities.

This 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 like fiscal programs, privatization reforms, and trade agreements that comparably affected male minorities and other demographics. The approach also 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 conflict in Xinjiang. By considering how employment reacts to conflict threats generated elsewhere, I avoid capturing the entanglement between local unrest and local labor markets. Finally, this approach addresses the third empirical challenge, direct production effects, by leveraging unrealized threats of unrest. A feature of the Uyghur conflict during 2002 - 2009 is that unrest threats vary in intensity outside of Xinjiang but do not erupt into realized conflict, so observed employment responses reflect preventative government action but do not capture the direct production consequences of violence.

Because increased state labor demand should also have equilibrium consequences for private employment and wages, I use a conceptual framework to generate additional predictions and to provide a means of quantifying the pacification motive. The framework 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, an original dataset of Uyghur con-

flict events, and China's Urban Household Survey (UHS). In line with theoretical predictions, I find that male minority state employment increases in response to the unrest shock, private employment decreases, and male minority wages rise. The size of the state employment response is small compared total state employment, but large compared to state employment among male minorities. For a one-standard deviation increase in county Uyghur population share at the median conflict intensity, overall state employment increases by 0.48 percentage-points, which represents 22.6% of male minority SOE employment.

I discuss and rule out several alternative explanations for the employment responses, including private firm discrimination, Uyghur migration patterns, endogenous Xinjiang conflict event triggers, national anti-discrimination efforts, and more. I also implement a placebo test and find that none of the baseline coefficients are precisely different from zero when using the lead, rather than the lag, of conflict incidents.

Furthermore, I address measurement concerns. First, I ensure that the relatively small sample of male minorities and the relatively small, skewed sample of counties outside Xinjiang with non-zero Uyghur share do not generate spurious results. Second, I use abstention from pork consumption to proxy for Uyghur identity and find evidence that state employment increases among men who are likely Uyghur. I also find suggestive evidence that state jobs are allocated to men of other minority groups in response to the Xinjiang shock, not just Uyghur men. However, the effect is only present in counties where Uyghur shares are high, and does not follow non-Uyghur minority county population shares.

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 to male minorities also increase in response to unrest threats, and that the transfers are ten times larger among non-employed minority men. These patterns suggest that relief transfers are a complementary policy to state employment in a broad-based state effort to prevent unrest. Furthermore, I use a framework-derived 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.

Finally, I present two empirical patterns that indicate 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. 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 the literature on how autocracies stay in power. Theoretical work in this area has explored a variety of control policies, including those that manipulate the information environment, repress political participation, and award targeted benefits (Egorov and Sonin, 2020; Gehlbach et al., 2016).[1] A set of empirical papers document whether and how autocrats implement these tools in practice. Empirical papers about information manipulation show that states use propaganda to generate regime support (Adena et al., 2015; Yanagizawa-Drott, 2014), censorship to silence dissent (Rozenas and Stukal, 2019; King et al., 2014), and local elections to collect information on regional leaders (Martinez-Bravo et al., 2022). Other empirical work documents that states use repression to weaken opposition areas (Markevich et al., 2021) and reward connected politicians with promotions (Jia et al., 2015; Xu, 2018). I extend this work by documenting another tool of autocratic survival, state employment, that helps explain why so many autocracies tolerate pervasive state-owned sectors that distort resource allocation and hamper growth.[2]

This paper also adds to the literature on intrastate conflict and its prevention. Seminal work in this area finds that employment and labor income can pacify conflict via an opportunity cost channel (Bazzi and Blattman, 2014; Dube and Vargas, 2013; Miguel et al., 2004), and further research demonstrates that policies can leverage this channel and mitigate conflict by providing job opportunities and labor income in diverse contexts like Liberia (Blattman and Annan, 2016), India (Fetzer, 2020), Afghanistan (Beath et al., 2017), and the Philippines (Crost et al., 2016). I offer new evidence that such employment programs may take place within existing firms, thus distorting production. I also provide direct evidence of intent, complementing this literature's primary focus on the pacifying consequences of, rather than the motives behind, these policies.

This paper further contributes to the intrastate conflict literature by shedding light on a highly sensitive setting with extremely limited data. Uyghur policies in China have attracted intense international attention in recent years (The Economist, 2021; Buckley and Ramzy, 2020; Fifield, 2020)

[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] This contribution also documents a manifestation of the "autocratic bargain", in which populations 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 shocks temporarily destabilize autocratic regimes.

and the government severely restricts data that specifically identifies Uyghur people, particularly alongside socioeconomic information. Of the top eight individual-level datasets that contain repeated cross-section or panel data in China, none report disaggregated ethnicity data enough to generate a usable sample of Uyghur individuals[3]. By combining the Census, which has stronger coverage of ethnic minorities, and a novel empirical strategy, this paper sheds new light on an important scholarly and policy issue within stringent data constraints.

This paper also contributes to the literature on Chinese state firms. It complements research documenting other SOE policy motives, including subsidizing key upstream industries (Liu, 2019) and enforcing regulations (Zeng, 2017), and a broader literature on the relationship between state interests and firms in China (Jia et al., 2021; Cao, 2020; Fisman and Wang, 2015). It is closely related to work showing a negative association between firm privatization and employment (Bai et al., 2006), and builds on this work in three ways: first, by establishing the intent behind state employment, second, by showing that the tool dynamically responds to changing threats, and third, by ruling out other unobservable factors that could generate a negative correlation.

This paper also 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) . This paper demonstrates another non-ability driver of public job allocation: unrest prevention. In contrast with previously documented forces, the unrest prevention motive may increase public sector employment of otherwise disenfranchised demographics.

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 theoretical predictions. Section 5 introduces the data used for analysis. Section 6 presents results on how labor market outcomes respond to ethnic unrest threats, and Section 7 presents additional results on employment responses to floods and trade shocks. Section 8 concludes.

[3]I list and discuss these datasets more in Subsection 4.1.

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[4]. 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-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). 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 commitment to aggressive economic growth.

SOE reform and the government's stated goal of economic growth appear well aligned. With no further information, one might expect the Chinese government to ardently pursue SOE privatization.[5] 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", 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 demonstrates the deceleration. Urban SOE employment

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

[5] 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, banking, trade, and manufacturing.

decreased markedly for a few years following 1997, but since 2006 has remained stagnant at approximately 65 million people. 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.

2.2 State 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 alternative pacification policies.

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. 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 (2020) 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?

First, 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), 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 (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 efficiency costs. For further empirical evidence on the relationship between SOE employment and unrest, see Appendix Subsection 9.5.

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).

Since 2018, the government's policies toward Xinjiang and the Uyghur people have changed dramatically, garnering international attention and condemnation (The Economist, 2021; Buckley and Ramzy, 2020; Fifield, 2020). However, my period of study, 2002-2009, represented a different moment in the government's response, before the recent widespread intensification and emphasis on surveillance, incarceration, and policing. Scholars document the government using a greater diversity of policies, where "carrots" and "sticks" were employed in concert (Millward, 2004; Bovingdon, 2010). In particular, the early 2000s was a relatively quiet period for the conflict (Davis, 2008; Millward, 2004). 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 framework to generate empirically-testable predictions indicative of pacifying intent. The framework reveals how labor market outcomes should respond if a government were using state employment to pacify unrest shocks. I describe key dynamics and predictions in this section and present the full theory in Appendix Subsection 9.1. I test each prediction in Sections 4 - 6.

3.1 Setup

This framework 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: $\frac{u_\ell\left(\ell^{j*}, c^{j*}\right)}{u_c(\ell^{j*}, c^{j*})} = w_j$. 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, $Y = F(U,N)$[6], 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

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

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.

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

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 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 framework 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.

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

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).[7]

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

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 framework, 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

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

on citizens' relative preferences for stability, consumption, and leisure.

3.3 Sufficient Statistic

The framework 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. Because the production function assumption is very strong, I use this value only to benchmark the subsidy against relevant values.

4 Empirical Strategy

To test theoretical 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 incidents per year as a measure of intensity. For the baseline specification, I lag this variable by one year to reflect the fact that employment may be 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) [8]. 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.[9]

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

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

[9] 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.

response to contemporaneous, unobserved forces, like friendly local policies[10]. Figure 2 presents a choropleth map of county-level Uyghur population shares outside of Xinjiang, and Appendix Table A.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 1 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 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, and one fewer year of average schooling. They are more urbanized and have comparable rates of employment. I also report the means and standard deviations for individual-level variables from the UHS. Overall SOE employment is fairly consistent across the four columns. The minority population share is slightly lower for counties with non-zero Uyghur share (2.1% compared with 3.2%), but quite steady within that group of counties. A similar pattern holds for the percent of SOE-employed male minorities. In column (6), I report the correlation of these variables with county Uyghur share; for individual-level variables, I report p-values for county-clustered standard errors. Population, education, urbanization, employment, and SOE employment share are all significantly correlated with county Uyghur share. I perform robustness checks to ensure that these variables do not confound the main results in Section 6.1.

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.

[10]I also investigate the robustness of the main results to county Uyghur population changes from 2000 to 2010. I discuss these tests in Section 6.1.

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). 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. The setup uses 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.[11]

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[12], only four cover Xinjiang

[11] By design, this empirical strategy leverages unrealized unrest threats to avoid capturing the direct consequences of conflict on employment. Therefore, I cannot directly test whether state employment prevents Xinjiang conflict propagation. However, in Appendix Subsection 9.5, I present broader evidence that suggests SOE employment does prevent unrest in the Chinese context.

[12] 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).

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[13]. For these reasons, the baseline sample does not include Xinjiang, and it is infeasible to perform an analogous test using the province itself.

4.1.1 Validating the Relevance Assumption

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. I cannot directly validate the assumption using measures of unrest, as a key property of the empirical strategy is that it does not generate realized unrest.

However, I can provide indirect validation that the differences-in-differences shock $DD_{ct} = I_{t-1}^{XJ=1} \times U_{c,t=2000}^{XJ=0}$ increases the underlying risk of ethnic conflict by testing whether Uyghur cultural practices increase concurrently among minority individuals. This exercise is highly related to the revealed-preference approach in Atkin et al. (2021), which measures cultural identification via dietary choices in consumption data and finds increasing dietary adherence after inter-group conflict. As outcomes, I construct two measures of Uyghur cultural identity that vary at the individual-year level: zero consumption of pork and zero consumption of alcohol (Cesaro, 2000). I regress these measures on the differences-in-differences shock interacted with a minority fixed effect; all sub-interactions of incidents, Uyghur share, and minority status, year fixed effects; county fixed effects; age; gender; and years of education for both the full sample of individuals and a subsample of minorities only. The results are reported in Appendix Table A.3. Column (1) demonstrates that abstention from pork among minorities increases in response to the shock, but only among minority individuals, with $t = 2.82$ ($p < 0.000$). The coefficient in column (2) on alcohol abstention is also positive, though not statistically different from zero. To ensure that these movements in abstention from pork are not due to income effects, in columns (3) and (4), I add controls for household income interacted with minority fixed effects, which allows income to influence product consumption, and

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

differently so for minority and majority groups. The results are strongly robust.

Furthermore, an inter-disciplinary literature on the propagation of social conflict supports the relevance assumption. Forsberg (2014), Forsberg (2008), Cederman et al. (2009), and Buhaug and Gleditsch (2008) document this pattern of contagion in the interstate context, finding that 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. There is also qualitative evidence that this spillover pattern is present within the Xinjiang conflict. 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 participants 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 crucial for this empirical strategy, as long as some are present.

4.1.2 Triple Differences

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).

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[14] 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 it is difficult to separate a response targeting Uyghur men from one targeting minority men more generally. While both interpretations bolster the central hypothesis, I provide further evidence in Subsection 6.0.1 that Uyghur men are receiving state jobs, and furthermore, that the response exists only in counties where Uyghur shares are high, and not shares of other minorities.

Appendix Table A.2 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.

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

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

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 theory's three directional predictions on SOE employment, private employment, and salaries further decreases the possibility that an omitted variable could reject the joint 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. I investigate whether Uyghur men specifically receive jobs in Subsection 6.0.1.

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, $DistXJ_c$, interacted with year fixed effects, τ_t. This control addresses geographic- and time-varying government policies, like the 2000 "Open Up the West" campaign.[15]

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.[16]

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 theory predicts that when unrest threat rises, male minorities should work more for SOEs, less for private

[15] As a robustness check, I also run a specification that controls for logged kilometer distance of each county from Xinjiang times year fixed effects and the male minority indicator variable. The results are highly robust and available upon request.

[16] 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).

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) collected by the National Bureau of Statistics. I use data from 2002 to 2009, which include information on ethnic minority status.[17] The data also contain rich set of variables describing household composition, age, gender, employment, education, income, and consumption.[18] 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. 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 operational similarities to SOEs (Brandt and Rawski, 2008).[19] 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

[17] The ethnic minority variable is an indicator. I discuss whether Uyghur men specifically receive jobs in Subsection 6.0.1.

[18] Household sampling 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.

[19] In Appendix Subsection 9.2, I explore how the results change if SOEs are defined as state-owned units only.

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).[20] Figure 3a demonstrates that minority men are dramatically over-represented among political prisoners: they comprise over 45% of unrest participants but represent 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 $p < 0.001$ 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 theory 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 2 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).

[20] 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.

Column (1) of Table 2 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 framework; 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 2 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 2 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 3 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 2, 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 4 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[21]. 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.

Because I cannot separately identify minority groups from one another, it may be that state-owned firms are hiring men from other minority groups, not just Uyghurs, in response to unrest threats from Xinjiang. One piece of evidence that suggests hiring may not only be restricted to Uyghur individuals is the size of the salary response. Though Uyghurs comprise 8.4% of all minorities in China, outside of Xinjiang, they comprise approximately 0.3% of minorities. Therefore, if the salary treatment were concentrated only on Uyghur individuals, it would imply an implausibly large increase. It therefore seems likely that the government is hiring other male minorities, in addition to Uyghur men, in response to the Xinjiang conflict shock. For example, Muslim minority groups comprise 9.7% of all minorities residing outside Xinjiang, which would imply a scaled

[21] 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.

salary treatment of $742/0.097 = 7,649$ RMB. However, in Subsection 6.0.1, I provide evidence that, while the targets of SOE hiring may not just be Uyghurs, the hiring is taking place in counties where the Uyghur diaspora reside, not where other minority groups live.

This empirical strategy, by design, leverages unrealized unrest threats, so it is not feasible to directly test state employment's efficacy in preventing conflict. However, in Appendix Subsection 9.5, I discuss this issue in detail and present suggestive evidence that state employment successfully mitigates unrest.

6.0.1 Uyghurs or All Minorities?

One constraint of the UHS data is that ethnic minority status is not further disaggregated; hence, I cannot directly observe whether an individual is Uyghur. Therefore, it is difficult to establish whether state jobs are allocated to male Uyghurs or ethnic minorities more broadly when unrest threats rise, though either response would support the central hypothesis that the government provides SOE jobs to prevent unrest.

The UHS data contains detailed disaggregated information on consumption, including consumption of pork, which is considered taboo in Muslim tradition. Using this information, I construct a household-level proxy for Uyghur identity that equals one if an ethnic minority household ever reports zero pork consumption. This sample includes 9.5% of minorities and 0.35% of all individuals.

Table 5 reports estimates using this proxy for Uyghur identity. I find that state employment differentially increases with a coefficient of 40.77 and $p < 0.1$. The coefficients fro private employment and salary are not statistically different from the baseline results, though they are not precisely estimated.

Additionally, I test whether it is the location of Uyghur diaspora at the county level, rather than the location of other Muslim minority groups that generates government responses. To do so, I replace county-level Uyghur share in baseline equation 5 with county-level Hui share from the 2000 Census; the Hui ethnic group are the largest Muslim minority ethnic group in China. Columns (1) through (3) of Table A.9 report the results. I find that neither of the three key outcome variables, state employment, private employment, and salaries change in response to Xinjiang conflict interacted with county Hui population shares, suggesting that it is the location of Uyghur populations, not Muslim populations in general, that drives the government's response.

Similarly, I provide evidence that the propagation of the unrest threat follows Uyghur share

across counties, rather than the location of other minority groups. I replace county Uyghur share in the baseline equation 5 with the share of non-Uyghur minorities in the 2000 Census. Columns (4) through (6) of Table A.9 show that the coefficient on both state and private employment are statistical zeros, and the magnitude of the salary coefficient is a precisely-estimated zero. Overall these checks strongly suggest that the government is specifically responding to Uyghur conflict threats.

6.1 Robustness Checks

In this section, I address alternative hypotheses that could potentially generate spurious main results. I also implement additional checks addressing the relatively small samples of male minorities and positive Uyghur share counties and other assorted issues.

6.1.1 Alternative Hypotheses

The first alternative hypothesis I consider is private firm discrimination and a compositional salary effect. Specifically, suppose that private firms were to employ fewer male minorities during times of increased unrest threat, but SOEs were able to hire the surplus, generating a differential fall in male minority employment and a differential rise in SOE male minority employment. Additionally, since in the data SOEs offer a 24.9% wage premium, this could generate an increase in average male minority salaries. To address this issue, I report the treatment effect on salary among SOEs separately from private firms in columns (4) and (5) of Table 4. Salaries for male minorities increase differentially among both firm types, which is not consistent with increased private firm discrimination.

Next, I address is the possibility that correlated shocks to strategic sectors, like utilities and mining, could drive the key results. In addition to my hypothesis that SOEs facilitate job provision to individuals at risk of unrest, SOEs are also used to retain control over strategic sectors and to maintain a large administrative capacity (Leutert, 2016). If time-series shocks to these sectors 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, they could generate spurious results. I conduct a set of robustness checks for these strategic motives. 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 and male minority 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.4, remain similar in sign and magnitude to those of the baseline.

Another source of spurious results could be that county-level Uyghur share is correlated with other county characteristics, as Table 1 demonstrates. Specifically, county-level population, education, urbanization, employment, and SOE employment share in the year 2000 were correlated with Uyghur share in 2000. To ensure that these characteristics do not drive spurious main results, I control for each of these variables interacted with year fixed effects and male minority fixed effects. These checks are reported in Appendix Table A.5: the SOE employment and salary results are robust in magnitude and precision, and the private coefficient declines in magnitude by 47% to -12.73 and is not precisely estimated.

Another alternative hypothesis is that Xinjiang unrest incidents could be 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.

A related 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 consider the possibility that the latter two terms of $\beta_M I_{t-1}^{XJ=1} \times U_{c,t=2000}^{XJ=0} \times M_i$ proxy for

the likelihood that a male minority in the sample is of Uyghur ethnicity. In that case, any nationwide push by SOEs to hire more male minorities in the wake of conflict incidents in Xinjiang may generate the observed results, though without the proposed channel of geographic targeting on high-Uyghur-share counties. To address this concern, I test for nonlinearity in the response to Uyghur share, as the probability of a male minority being Uyghur would be linear in the Uyghur share term. I create a categorical variable representing twenty quantiles of county Uyghur share and interact this variable with the main triple interaction. The results are reported in Appendix Table A.6 and demonstrate a highly nonlinear response across groups. However, the signs of the interacted treatment effects all remain consistent with the predictions of the conceptual framework.

Finally, I address the possibility that Uyghur migration patterns may be correlated with county-level minority friendly policies and temporal patterns in Xinjiang conflict incidents. To understand whether Uyghur out-migration from Xinjiang could spuriously drive the baseline results, I create a county-level measure of the change in Uyghur population share from the 2000 Census to the 2010 Census. I then control for this value interacted with year fixed effects and minority male fixed effects in the baseline regression. The baseline results are highly robust and reported in Appendix Table A.7.

6.1.2 Small Sample Tests

One might be concerned that key variation relies on a relatively small subset of the data. Specifically, male minorities represent 1.8% of individual-level observations ($4,105$ observations) and are unevenly distributed across counties. To understand whether this fact could spuriously generate the main results, I perform a random permutation test. I create 500 counterfactual datasets by re-assigning each individual a new male minority status, following the variable's true distribution and without replacement. I then re-run the baseline specification using this new indicator variable for all interactions. Figure 4 plots a histogram of the counterfactual coefficients. Just 6.7% of these counterfactual coefficients are greater than the true estimate of 36.59, for a p-value of 0.067. The corresponding p-value for private employment is 0.236, and it is 0.001 for salary. I conclude that the baseline SOE employment and salary results are highly unlikely to be generated spuriously by the distribution of male minority status, whereas the private results are somewhat less robust.

Another concern is that Uyghur share is distributed in a non-normal way across counties, such that 8.6% of counties and 16.1% of individual observations in the baseline sample have non-zero

value. I investigate whether this non-normal distribution of Uyghur shares across counties might generate spurious results by performing another random permutation test. I generate 500 counterfactual Uyghur share maps for China, following the variable's true distribution without replacement. 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 same procedure for private employment and salary yield p-values of 0.24 and 0.01, respectively. I find that the baseline SOE employment and salary results are highly unlikely to be generated by random assignment of county Uyghur share, and again, the private employment is less robust.

6.1.3 Additional Tests

I investigate whether the baseline results are sensitive to the removal of outliers in Online Appendix Table A.8. To identify outliers, I compute DFITS for each observation 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. 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.

In Table 8, I conduct a placebo test. Instead of using lagged Xinjiang incidents in the shock, I use instead the lead of Xinjiang incident, since SOE employment should not respond to incidents in the future. 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 of 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.9 reports these coefficients; I find that male minority employment and wages do not respond to the Hui share triple interaction.

To demonstrate that no single year drives the main results, I re-estimate a version of the baseline

equation that produces year-by-year estimates for the coefficient on $U^{XJ=0}_{c,t=2000} \times M_i$ and $U^{XJ=0}_{c,t=2000}$.

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

I plot the coefficients β_{Mt} in Figure 6 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.

6.2 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, private employment, and salary 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.

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.

Though one concern may be that private firms are employing fewer male minorities in this period, and SOEs are absorbing the surplus, column (6) demonstrates that salaries for male minorities within the service sector are differentially increasing with the treatment. This pattern is not consistent with a fall in aggregate labor demand due to private firms. Additionally, when the within-sector salary result is disaggregated by private and SOE firms, male minority salaries in both groups in-

crease, evidence against the possibility that the salary result arises from a change in employment composition and an SOE wage premium.[22]

The fact that state hiring is most pronounced in the service sector is notable for several reasons. First, it is the state sector with the highest proportion of male minority SOE employees. Second, it has the highest rates of turnover among SOE sectors, perhaps an advantage for countering changing threats over time and space. Third, it is the most customer-facing sector, where anti-minority discrimination 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.

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 $I_{t-j}^{XJ=1}$ 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.

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

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 subfigures in Figure A.2 reveal that the labor market responses to the Uyghur unrest shock in year t

[22] These salary composition results are omitted for brevity and available upon request.

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. However, it is important to note that, as new conflict incidents are happening throughout the time period, these corrections do not create net declines of SOE employment or salaries among male minorities. In particular, the SOE employment decline in the fourth year is 23% the magnitude of the cumulative increases in the three years before.

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 pacification policies, like social relief transfers, in conjunction with SOE employment. 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). 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. Results 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 from the baseline regression sample (UHS data, 2002-2009) into Equation (4) and compute of τ_U, the value of male minority wage subsidies:

$$\tau_U = 1 - \frac{N^{soe}/U^{soe}}{N^{priv}/U^{priv}} = 1 - \frac{45.95}{62.17} = 1 - 0.739 = 0.261. \tag{11}$$

The data imply a 26.1% 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 $(20\%, 32\%)$.[23]

This subsidy is comparable in size 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).

One strong caveat to this statistic is that it requires both SOEs and private firms to have identical Cobb-Douglas production functions. As such, it is only intended as a benchmarking exercise. The wage premium that non-male-minority individuals receive relative to male minorities in the data is 20.5%, and the wage premium that non-minority people receive relative to minority people is 19.04%. The implied subsidy 26.1% is comparable to these values. It is also comparable to 24.9%, the average difference in observed wages in SOEs versus private firms in the baseline sample.

[23] This confidence interval uses the formula for odds ratios in unstratified case-control data. The treatment is male minority, and the two cases are state firms and private firms.

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 ethnic 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).[24] The annual provincial demand shock exposure, $\Delta DSEIV_{pt}$, has two components: a weight variable and a trade flow variable.

$$\Delta DSEIV_{pt} = \sum_{s} \left[\frac{X_{spt-1}}{X_{st-1}} \sum_{a \in A} \sum_{b \in B} \Delta E_{st}^{ab} \right] \tag{12}$$

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.[25] This setup avoids using flows that directly involve China itself, which are certainly influenced by China's domestic situation.[26] The provincial variation in Equation (12)

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

[25] 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.

[26] 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.

arises entirely 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.

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

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.10. 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.10. To further increase confidence that these results are not elicited by spurious trends, I re-estimate Equation (13) 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.11 - 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.

$$\begin{aligned} Y_{ict} = \alpha + \beta \Delta Flood_{ct-1} + \gamma Age_i + \delta Edu_i + \zeta Male_i \\ + \delta_M Edu_i \times \text{Male}_i + \gamma_M Age_i \times \text{Male}_i + \tau_t + \eta_c + \varepsilon_{ict} \end{aligned} \tag{14}$$

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 11 shows SOE employment increases in the year after floods: the coefficient in column (3) is 0.0778 and precise at the $p < 0.05$ level. On the other hand, column (4) 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.12. I also conduct a placebo check by re-estimating Equation (14) using the lead of the flood indicator variable. The results from these regressions are reported in Online Appendix Table A.11, and reassuringly,

employment composition by ownership does not respond to future floods.

8 Conclusion

This paper provides new causal evidence that autocrats use public employment as a means of managing unrest threats. This finding provides a political economy explanation for the persistence of state-owned enterprises, and consequently, a downward force on productivity in a economies around the world.

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 shares, 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 raises a number of questions for future research. For example, China has historically used both rewards and punishments to control ethnic unrest, but in the last five years, Uyghur-targeted policy has taken a marked turn toward punishments (The Economist, 2021; Buckley and Ramzy, 2020; Fifield, 2020). How do governments choose the tools, and the composition of tools, that they use to manage internal threats?

More broadly, this paper invites the evaluation of other seemingly inefficient firms and institutions in the light of regime survival. Could the persistence of some economic distortions be due to states' desire to prevent social unrest? Are there other political economy motives beyond social unrest that also prompt such distortions? These questions all relate to the fundamental theme of how, and why, the survival motives of states manifest as forces of economic development.

Figure 1: Plot of Lag Xinjiang Unrest Incidents

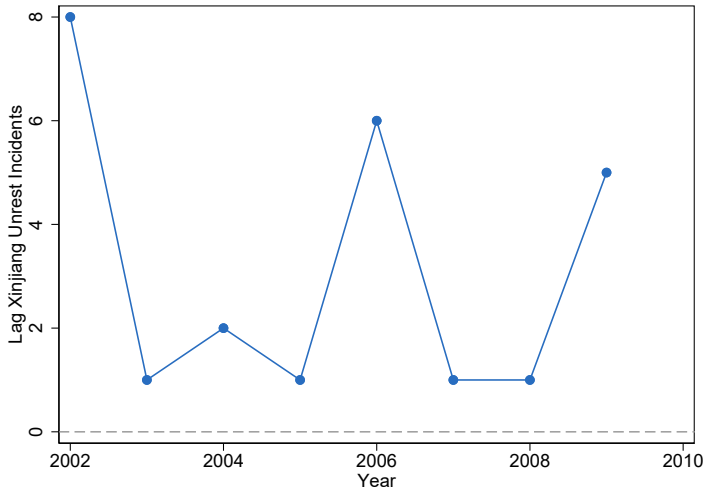
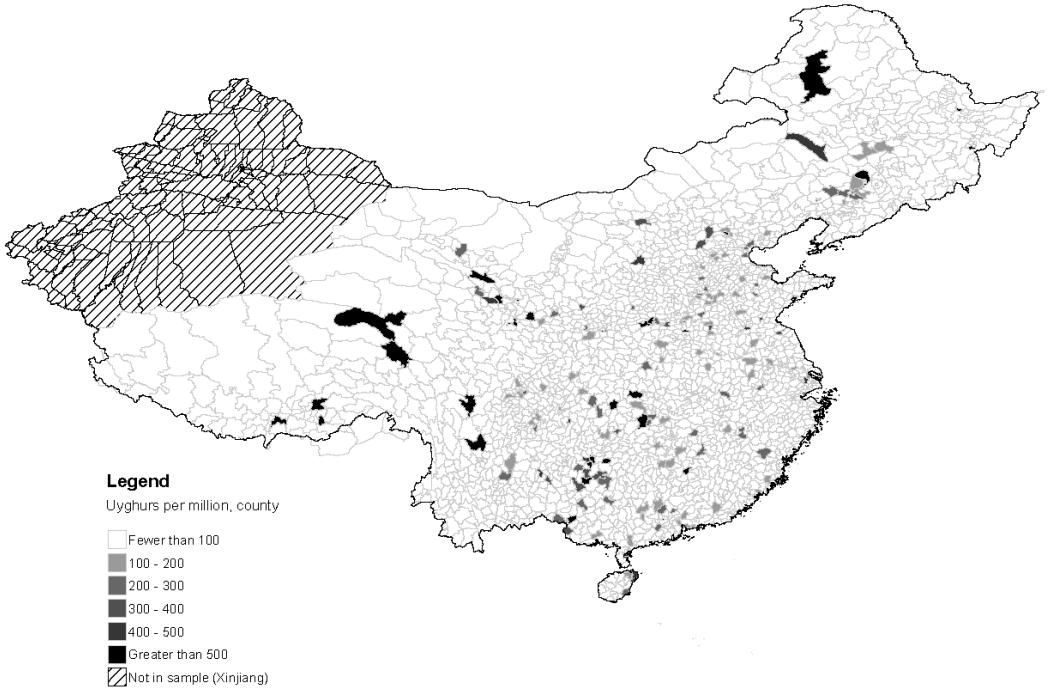


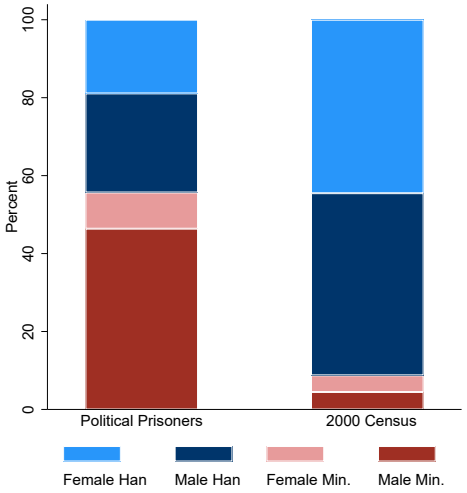
Figure 2: Choropleth of County Uyghur Share Outside Xinjiang



Notes: Data from 2000 Census of China.

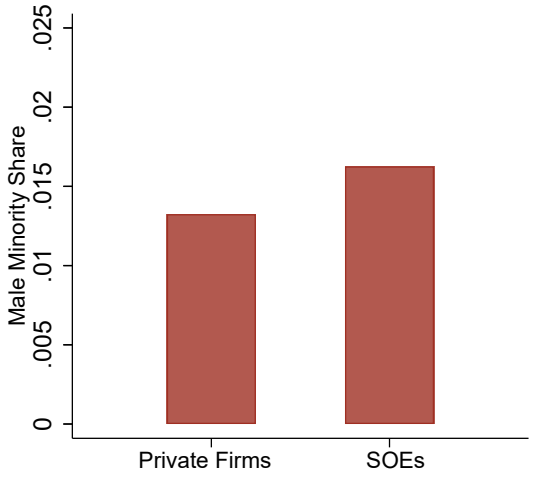
Figure 3: Demographic Comparisons

(a) Political Prisoners vs. General Population



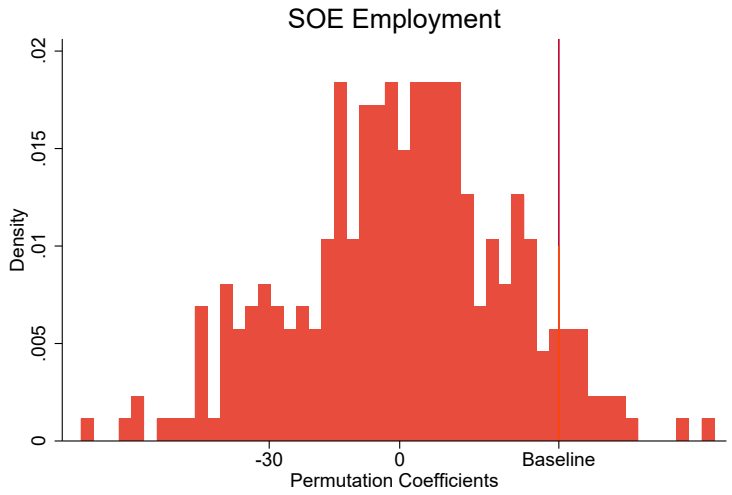
Notes: Means conditional on age, years of education, county, survey year
Source: Census 2000, China Political Prisoners Database.

(b) Private Firms vs. SOEs



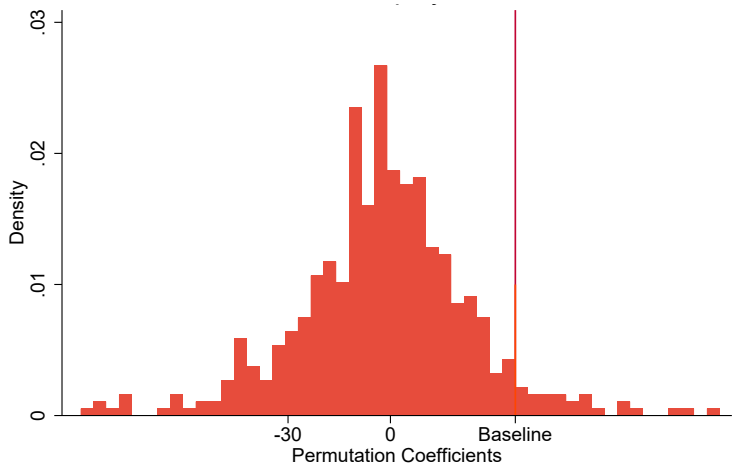
Notes: Means conditional on age, years of education, county of residence, survey year, sector. Male min t-test p-value: 0.000.
Source: Urban Household Survey 2002-2009

Figure 4: Counterfactual Coefficient Distribution from Male Minority Random Permutation Test



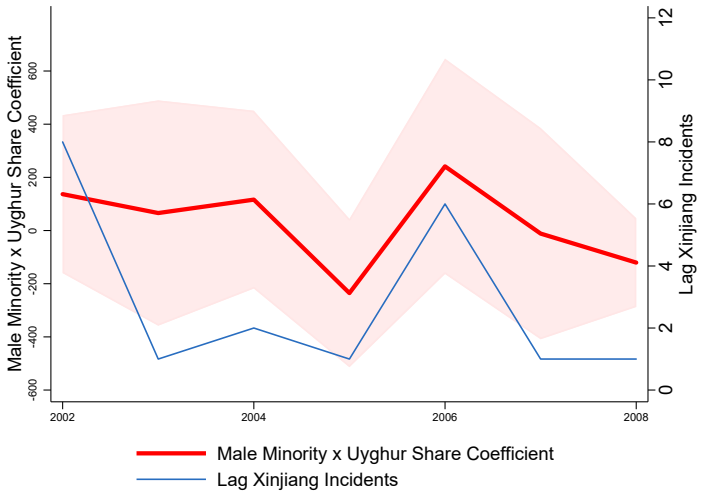
Notes: The implied p-value is 0.067 and the test ran for 500 iterations. Coefficients are obtained by re-running the baseline regression with SOE Employment as an outcome variable and counterfactual male minority identity. For each iteration, individuals are assigned a male minority value from the existing distribution, without replacement. All other baseline controls are included.

Figure 5: Counterfactual Coefficient Distribution from Uyghur Share 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.

Figure 6: 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.

Table 1: County Characteristics by Percent Uyghur Share

	(1)	(2)	(3)	(4)	(5)	(6)
		Excluding Xinjiang Counties				
			Non-Zero Uyghur Share			Correlation with County Uyghur Share
	All	All	All	Below median	Above median	
County-level Variables						
% Uyghur Share	1.314	0.004	0.042	0.017	0.098	-
	(9.78)	(.026)	(.08)	(.006)	(.128)	
GDP per capita	.711	.71	.778	.832	.657	.011
	(.337)	(.343)	(.431)	(.44)	(.385)	[0.548]
Population	432062	440342	665179	812826	351197	-.083
	(341,361)	(343,320)	(553,832)	(578,131)	(323,990)	[<0.001]
Avg. Years of Schooling	6.861	6.838	7.042	7.346	6.394	.002
	(1.679)	(1.693)	(2.066)	(1.413)	(2.926)	[0.903]
% Urban	38.461	38.667	43.093	41.439	46.849	-.068
	(31.698)	(31.809)	(35.49)	(34.014)	(38.612)	[<0.001]
% Employed	5.306	5.294	5.303	5.405	5.073	.080
	(.838)	(.791)	(.889)	(.804)	(1.026)	[<0.001]
Observations	2,870	2,774	241	167	74	2,870
Individual-level Variables						
Employed in SOE	-	54.191	53.126	52.828	53.798	-.027
		(16.696)	(14.271)	(13.874)	(15.48)	[0.056]
Minority	-	3.241	2.139	2.129	2.162	-.008
		(9.416)	(2.757)	(2.707)	(2.939)	[0.485]
Male Minority	-	1.624	.944	.967	.893	-.007
		(4.797)	(1.33)	(1.362)	(1.29)	[0.376]
Male Minority Employed in SOE	-	1.016	.552	.578	.492	-.008
		(3.23)	(.757)	(.788)	(.698)	[0.235]
Observations	-	244,412	39,500	22,782	16,718	244,412

Notes: Standard deviations reported in parentheses. GDP data come from the 2000 Provincial Yearbooks and are observed at the province level. All other county-level variables come from the 2000 Census are are observed at the county level. Individual-level variables are from the UHS, 2002-2009. The full sample does not contain Xinjiang. For column (6), beta coefficients are reported with p-values in brackets; p-values for standard errors clustered at the county level are reported for individual-level variables.

Table 2: 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 3: 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 4: The Effect of Unrest Threat on Employment — Baseline

	(1)	(2)	(3)	(4)	(5)
Dependent Variable: Employment	SOE	Private		Salary (000s RMB)	
				In SOE	In Private
Mean of Dependent Variable	*0.550*	*0.250*	*45.51*	*49.19*	*36.92*
Cty. Uyg. Share × Lag Xinjiang Incid.	36.59***	-24.24**	5,422***	4,960**	5,997***
× Male Minority	(12.59)	(11.04)	(2,075)	(2,301)	(2,128)
Observations	224,412	224,412	176,962	123,822	53,140
R-squared	0.231	0.156	0.431	0.499	0.405
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 5: The Effect of Unrest Threat on Employment — Proxy for Uyghur Identity

	(1)	(2)	(3)
Dependent Variable: Employment	SOE	Private	Salary (000s RMB)
Mean of Dependent Variable	*0.550*	*0.250*	*40.83*
Cty. Uyg. Share × Lag Xinjiang Incid.	40.77*	-16.84	2,318
× Uyghur Proxy	(21.79)	(15.65)	(2,288)
Observations	212,524	212,524	170,315
R-squared	0.234	0.156	0.406

Notes: Observations are at the individual level. All regressions control for year fixed effects; county 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

Dependent Variable: Employment	(1) SOE	(2) Private	(3) Salary (000s RMB)
Cty. Uyg. Share × Lag Xinjiang Incid. × Male Minority	38.70***	-25.38**	5,892***
	(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

Dependent Variable:	(1) SOE	(2) Private	(3) 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

Dependent Variable: Employment	(1) SOE	(2) Private	(3) Salary (000s RMB)
Cty. Uyg. Share × Lead Xinjiang Incid.	-16.04	7.605	-2,513
× Male Minority	(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)
Sample:	Manufacturing			Mining and Construction		
Dependent Variable: Employment	SOE	Private	Salary	SOE	Private	Salary
Mean of Dep. Var.	*0.560*	*0.440*	*39.49*	*0.710*	*0.290*	*45.54*
Male Minority Share	*0.0170*	*0.0140*		*0.0210*	*0.0230*	
Cty. Uyg. Share × Lag Xinjiang Incid.	31.34	-31.34	6,820**	39.05	-39.05	3,611
× Male Minority	(53.07)	(53.07)	(3,096)	(65.29)	(65.29)	(4,033)
Observations	36,138	36,138	35,538	21,858	21,858	21,474
R-squared	0.375	0.375	0.443	0.272	0.272	0.450
Sample:	Retail and Transportation			Services		
Dependent Variable: Employment	SOE	Private	Salary	SOE	Private	Salary
Mean of Dep. Var.	*0.350*	*0.650*	*38.18*	*0.750*	*0.250*	*47.68*
Male Minority Share	*0.0170*	*0.0130*		*0.0220*	*0.0170*	
Cty. Uyg. Share × Lag Xinjiang Incid.	-66.87	66.87	-5,328	62.02***	-62.02***	3,047**
× Male Minority	(79.73)	(79.73)	(3,404)	(12.30)	(12.30)	(1,472)
Observations	23,426	23,426	22,648	72,684	72,684	71,689
R-squared	0.325	0.325	0.420	0.220	0.220	0.448

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 and Floods on Employment

	(1)	(2)	(3)	(4)
	Export Demand Shock		Lag County Flood	
Dependent Variable:	SOE	Private	SOE	Private
Mean of Dependent Variable	*0.650*	*0.180*	*0.550*	*0.250*
Export Demand Shock	-0.0529***	0.0546**	-	-
	(0.0203)	(0.0238)		
Lag County Flood	-	-	0.0778**	-0.0930***
			(0.0361)	(0.0318)
Observations	346,531	346,531	225,039	225,039
R-squared	0.217	0.124	0.248	0.166
P-value for equality in β	(1) vs. (2)		(3) vs. (4)	
	0.006		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 province-year level for the trade shock regressions and at the county level for flood regressions. *** p<0.01, ** p<0.05, * p<0.1

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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: $\mathcal{N}^U$ identical unrest-type individuals, indicated by superscript U, and $\mathcal{N}^N$ 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 framework, 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.

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

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

$$\frac{u_\ell\left(\ell^{j*}, c^{j*}\right)}{u_c\left(\ell^{j*}, c^{j*}\right)} = w_j. \tag{15}$$

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:

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

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.[27]

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:

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

[27] All the results of the framework are unchanged if SOEs are allowed free entry.

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

$$\frac{F_U^{soe*}}{F_N^{soe*}} = \frac{w_U\left(1-\tau_U\right)}{w_N\left(1-\tau_N\right)}. \tag{17}$$

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 $S(-Z)$ and suppose that it is a continuous, increasing, and concave function, such that $S'(\cdot) > 0$ and $S''(\cdot) < 0$. Additionally, let $lim_{-Z\to 0} S'(\cdot) = \infty$ and $lim_{-Z\to\infty} S'(\cdot) = 0$.

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.

$$\begin{aligned} max_{\tau_U,\tau_N}\, Y^{priv} + Y^{soe} + \eta S\left(-\xi z\left(\ell^U\right)\right) \\ \text{s.t. } \tau_U w_U U^{soe} + \tau_N w_N N = 0 \end{aligned} \tag{18}$$

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

$$G^{**} = max_{\tau_U}\, Y^{priv}(\tau_U) + Y^{soe}(\tau_U) + \eta S(-Z(h - U(\tau_U)) - \xi) \tag{19}$$

The first order condition of the government is:

$$\frac{dY^{soe}}{d\tau_U} + \frac{dY^{priv}}{d\tau_U} + \eta\xi \underbrace{\frac{dS}{dZ}}_{\ominus}\underbrace{\frac{dz}{dU}}_{\ominus}\frac{dU}{d\tau_U} = 0. \tag{20}$$

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

$$G^* = max_{\tau_L}\, Y^{priv}(\tau_U) + Y^{soe}(\tau_U). \tag{21}$$

9.1.5 Market Clearing

In equilibrium, Equations (15), (16), and (17) 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.

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

$$Y^{priv*} - w_U U^{priv*} - w_N(1-\tau_N)N^{priv*} = 0 \tag{23}$$

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 framework 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 framework 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 (24). Note that the labor supply curve does not change with respect to τ_U.

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

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.

$$w_U = \frac{F_U^{soe}\Big|_{N=\overline{N^S}}}{(1-\tau_U)} \tag{25}$$

$$w_U = F_U^{priv}\Big|_{N=\overline{N^P}} \tag{26}$$

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

$$w_U = \frac{u_\ell\left(\ell^N, c^N\right)}{u_c\left(\ell^N, c^N\right)} \tag{27}$$

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.

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

$$w_N = \left.\frac{F_N^{priv}}{(1-\tau_N)}\right|_{L=L^{priv*}} \tag{29}$$

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 framework are how SOE and private employment respond to instability shocks. Empirically, these shocks map to the instability parameter, ξ. The parameter ξ enters the framework 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:

$$\underbrace{\frac{dY}{d\tau_U}}_{\text{"output cost"}<0} + \underbrace{\eta\xi \underbrace{\frac{dS}{dZ}}_{\ominus}\underbrace{\frac{dz}{dU}}_{\ominus}\frac{dU}{d\tau_U}}_{\text{"stability benefit"}>0} = 0. \tag{30}$$

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.13. 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.14 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.

One might also be concerned that labor markets in counties with initially high Uyghur population shares moved on different trajectories during the period of study. To address this possibility, I control for county Uyghur share in the year 2000 interacted with year fixed effects. The results are highly similar in magnitude and precision and are available upon request.

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.15 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.16. 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). Additionally, it is possible to link individual respondents in the UHS for up to three years. After implementing this procedure, I cluster standard errors at the individual level as an additional robustness check. Both the state employment and salary results are precise at the $p<0.05$ level, but the private employment result is not statistically different from zero. This table is available upon request.

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.

$$TFPR_{ipst} = \alpha + \beta SOE_{it} + \tau_t + \eta_p + \gamma_s + \varepsilon_{ipst} \tag{31}$$

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.17. 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.

9.5 Does State Employment Mitigate Unrest?

The empirical strategy of this paper documents how labor market outcomes respond to unrealized unrest threats. These threats must not, and in practice do not, become conflict incidents - otherwise, labor market responses could arise due to the direct consequences of conflict and not preventative policy. However, one drawback of the main approach is that it cannot directly test whether state employment is effective at mitigating conflict.

In this subsection, I present suggestive evidence that Chinese SOE employment is associated with lower unrest. To do so, I plot the relationship between provincial state employment share and protest intensity, as well as between private employment share and protest intensity. I construct a province-year dataset of protest incidents in China using the GDELT dataset (Leetaru and Schrodt, 2013), covering the years 1993-2000. Then, I obtain the province-level employment shares of SOE and private firms from the China Statistical Yearbooks (The National Bureau of Statistics, 2015). I normalize each of these three measures by province population and year fixed effects. Then, I create quantiles of normalized protest counts and plot the average SOE and private employment shares within each level. Figure A.8 displays these results. A clear pattern emerges: SOE employment share is correlated precisely with lower levels of protest, with $p < 0.01$. This relationship suggests that state employment may be an effective means of pacifying protests. The same pattern is not present for private employment. Additionally, employment in state-owned firms is positively correlated with support for the CCP and negatively correlated with support for democratization (Chen

and Lu, 2011).

There is also ample evidence of the converse relationship: historically, when state employment declined, unrest increased. In the wake of SOE privatization and layoffs in the late 1990s, labor unrest spiked across the country (Silver and Zhang, 2009; Cai, 2002). In the industrial northeast, worker protests were especially common: in the cities of Liaoyang and Daqing, laid-off SOE workers demonstrated against privatization for several months starting in March 2002 (Chen, 2002). In a survey of laid-off state workers in 1999, 23.8% responded that they took part in collective action - likely an underestimate, given the potential costs of admitting participation (Cai, 2006). Cases brought to labor arbitration committees also increased dramatically during the SOE reform period, from 78 thousand in 1994 to over eight-hundred thousand in 2003 (White, 2007). This phenomenon is not limited to China: SOE privatization triggered unrest in Latin America, the Middle East, and North Africa (McKenzie et al., 2003; Akoum, 2012).

Evidence from other contexts also supports the role of employment in pacifying conflict. 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, and Heller (2014) finds evidence that summer jobs for youth in the United States decrease participation in violent activity. More broadly, labor income mitigates intrastate conflict across a variety of settings (Bazzi and Blattman, 2014; Dube and Vargas, 2013; Fetzer, 2020). Overall, evidence from within and outside of China suggest that SOE employment is an effective means of managing internal unrest.

Figure A.1: Urban SOE Employment Over Time (millions of workers)

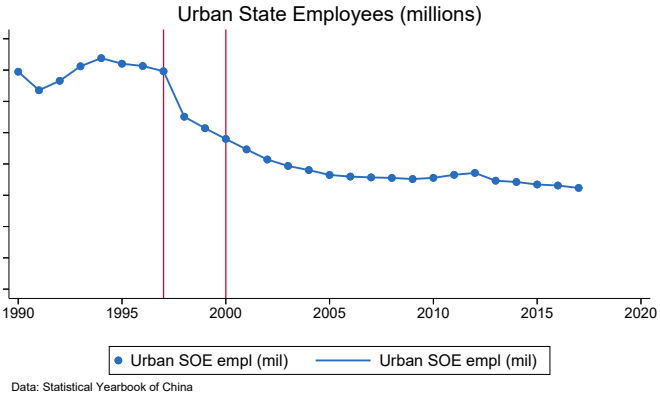
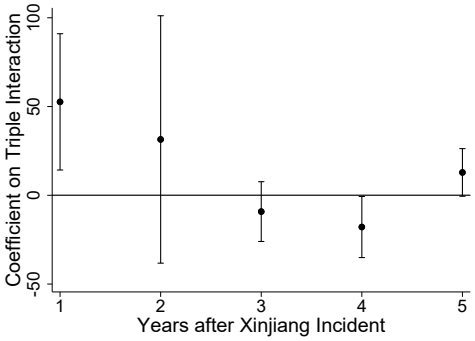
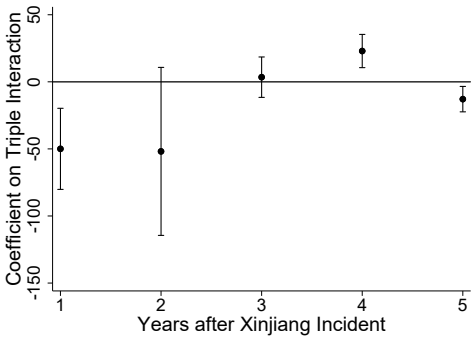


Figure A.2: The Effect of Unrest Threat on Employment — Medium-Run Responses

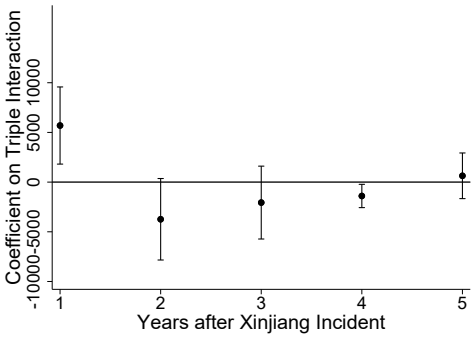
(a) SOE Employment



(b) Private Employment



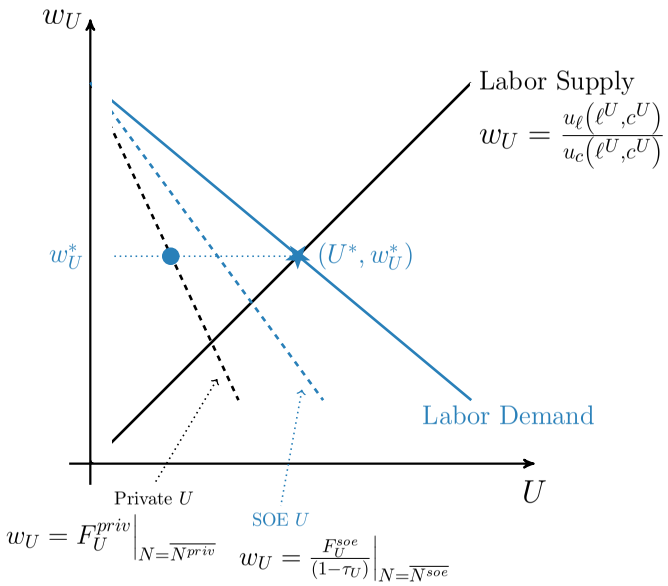
(c) Salary



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: Theoretical Equilibrium

(a) U-type Labor Market



(b) N-type Labor Market

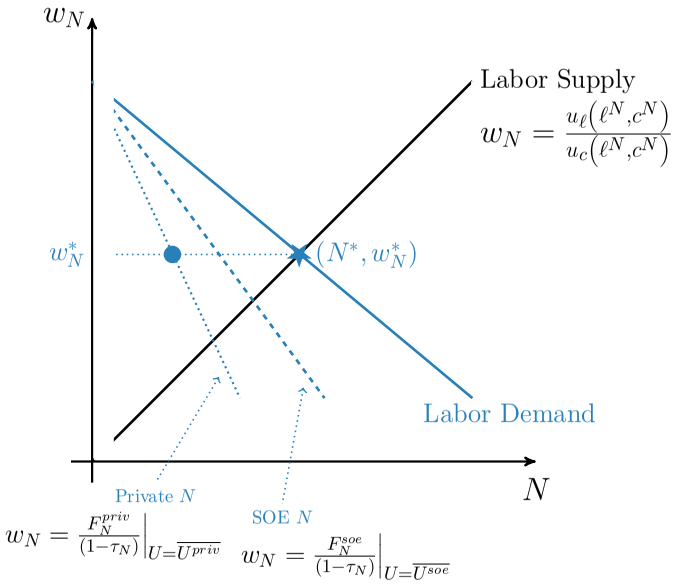
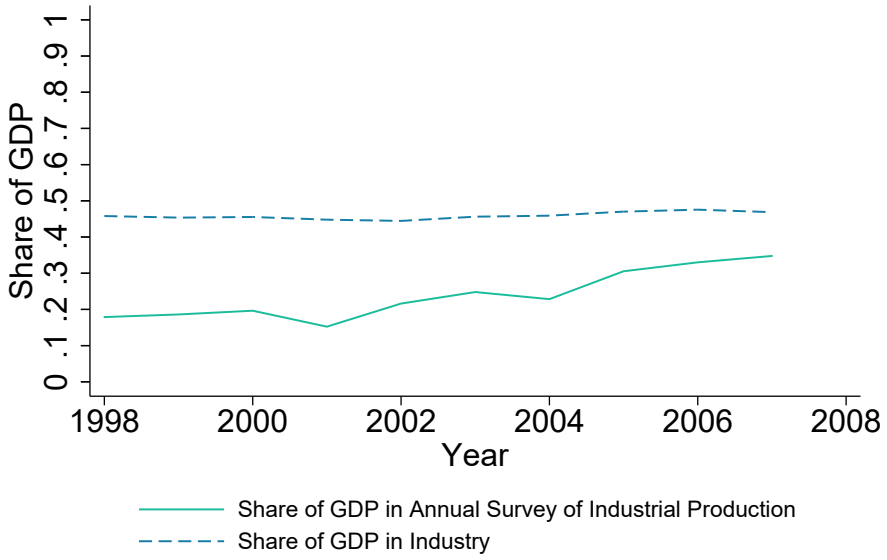


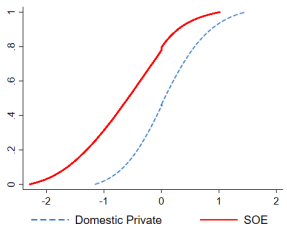
Figure A.5: Share of GDP covered by the *Annual Survey of Industrial Production*



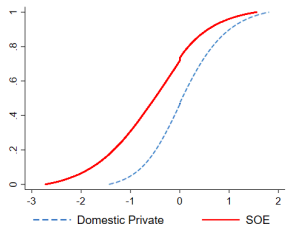
Source: China Statistical Yearbook 2017, World Bank 2017, Annual Survey of Industrial Production 1998-2007.

Figure A.6: Firm Productivity by Ownership

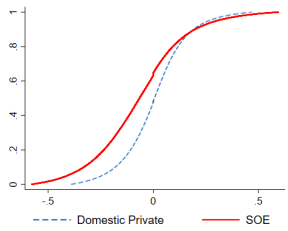
(a) Labor Productivity



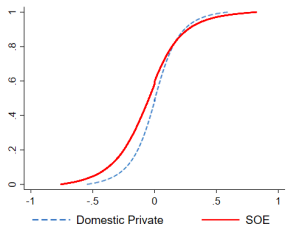
(b) TFPR (Hsieh and Klenow, 2009)



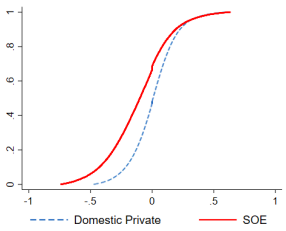
(c) TFPR (OLS)



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



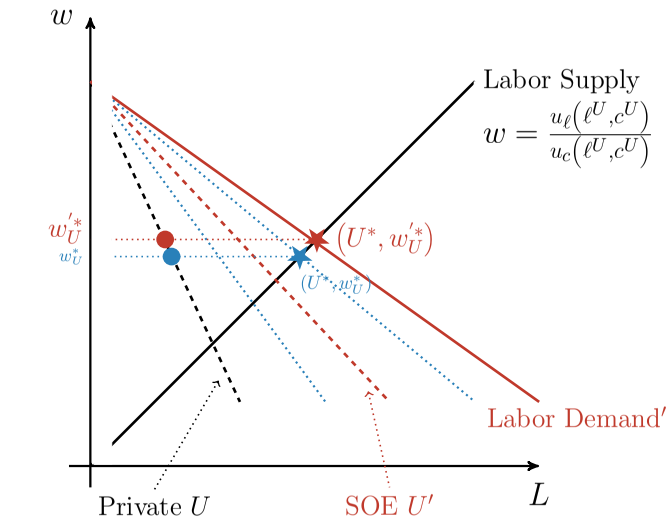
(e) TFPR (Gandhi et al., 2011)



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.

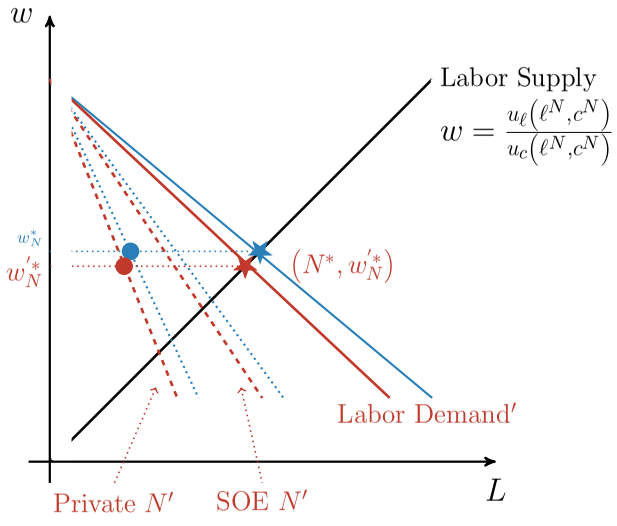
Figure A.4: Equilibrium Responses to $\tau_U \uparrow$

(a) U-type Labor Market



$w_U = F_U^{priv}\big|_{N=\overline{N^{priv}}}$ $\quad w_U = \frac{F_U^{soe}}{(1-\tau_U)}\big|_{N=\overline{N^{soe}}}$

(b) N-type Labor Market



$w_N = \frac{F_N^{priv}}{(1-\tau_N)}\big|_{U=\overline{U^{priv}}}$ $\quad w_N = \frac{F_N^{soe}}{(1-\tau_N)}\big|_{U=\overline{U^{soe}}}$

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

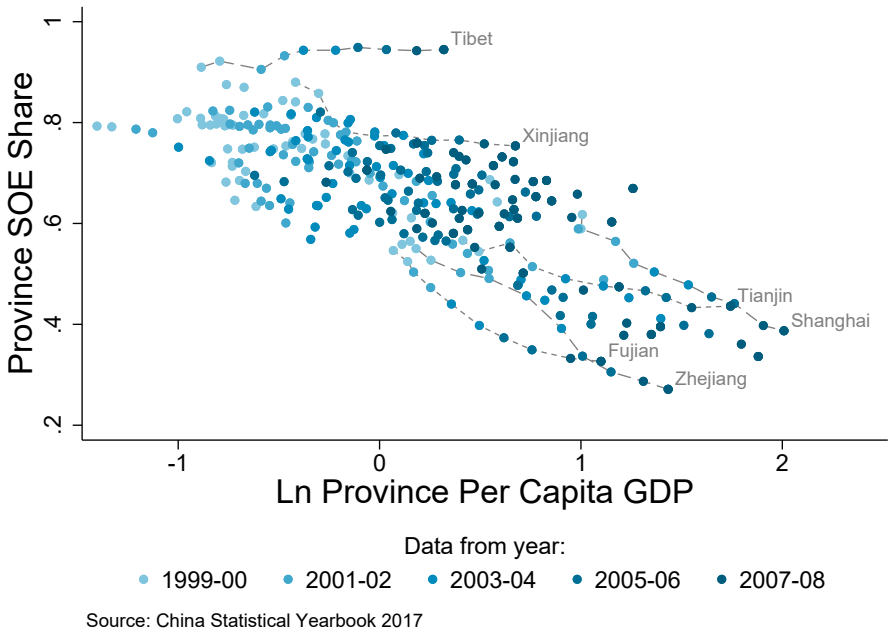


Figure A.8: Provincial Employment Composition by Quantiles of Protest

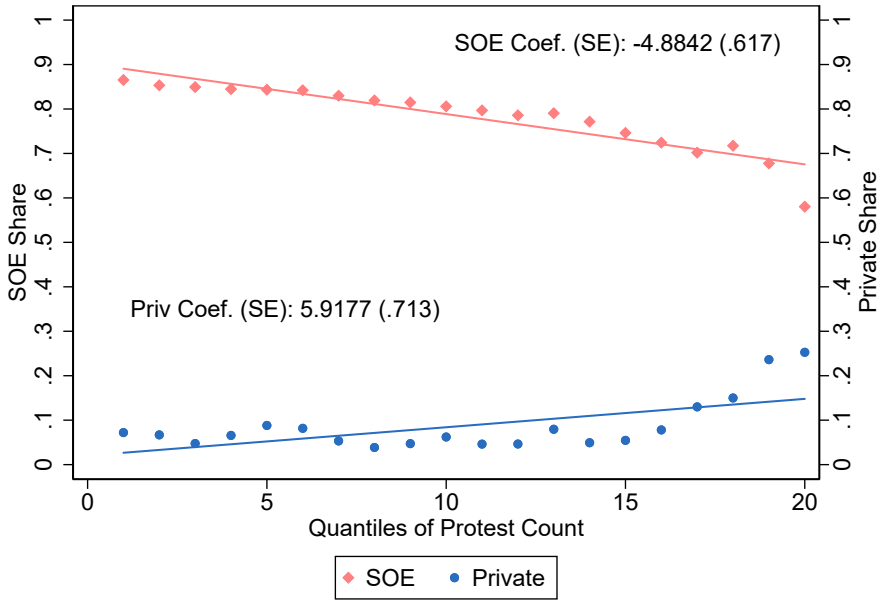


Table A.1: County Uyghur Share Summary Statistics

	(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 A.2: 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, 2002-2009.

Table A.3: Validation of Relevance Assumption: Uyghur Cultural Practices

	(1)	(2)	(3)	(4)
Dependent Variable:	No Pork	No Alcohol	No Pork	No Alcohol
			Control for Income x Minority FE	
Mean of Dependent Variable	*0.0100*	*0.110*	*0.0100*	*0.120*
Cty. Uyg. Share × Lag Xinjiang Incid.	34.07***	9.404	32.95***	14.42
× Minority	(12.10)	(14.39)	(12.25)	(14.09)
Cty. Uyg. Share × Lag Xinjiang Incid.	1.214	2.980	1.121	2.569
	(0.815)	(2.597)	(0.813)	(2.570)
Observations	219,808	219,808	176,112	176,112
R-squared	0.064	0.062	0.060	0.064

Notes: Observations are at the individual level. All regressions control for year fixed effects, county fixed effects, age, gender, and years of education. Standard errors are clustered at the county level. The outcome variables are indicators for whether an individual reported zero pork (alcohol) consumption in a given year in the Urban Household Survey. *** p<0.01, ** p<0.05, * p<0.1

Table A.4: 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.	31.97*	-24.89**	7,167***	38.47***	-28.87**	5,778***	34.46***	-22.19**	5,618**
× Male Minority	(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.5: The Effect of Unrest Threat on Employment — Robustness to Correlates of Uyghur Share

Dependent Variable: Employment	(1) SOE	(2) Private	(3) Salary (000s RMB)
Cty. Uyg. Share × Lag Xinjiang Incid.	28.79**	-12.73	3,833*
× Male Minority	(13.60)	(11.28)	(2,222)
Control for Year FE × Male Minority ×			
Cty. Population, 2000	Y	Y	Y
Cty. Percent Urbanization, 2000	Y	Y	Y
Cty. Percent Employed, 2000	Y	Y	Y
Observations	224,412	224,412	176,962
R-squared	0.231	0.156	0.438

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.6: The Effect of Unrest Threat on Employment — Interaction with Quantiles of County Uyghur Share

Dependent Variable: Employment	(1) SOE	(2) Private	(3) Salary (000s RMB)
Mean of Dependent Variable	*0.550*	*0.250*	*45.51*
Cty. Uyg. Share × Lag Xinjiang Incid. × Male Minority × 0 - 80th percentile	-	-	-
Cty. Uyg. Share × Lag Xinjiang Incid. × Male Minority × 80-85th percentile	248.7** (115.7)	-30.23 (67.01)	8,952 (10,757)
Cty. Uyg. Share × Lag Xinjiang Incid. × Male Minority × 85-90th percentile	129.0** (54.72)	-69.36* (37.80)	6,271*** (2,102)
Cty. Uyg. Share × Lag Xinjiang Incid. × Male Minority × 90-95th percentile	14.16 (58.43)	1.591 (45.34)	6,578** (3,215)
Cty. Uyg. Share × Lag Xinjiang Incid. × Male Minority × 95-100th percentile	30.61*** (11.06)	-22.09** (10.91)	4,218* (2,326)
Observations	224,412	224,412	176,962
R-squared	0.231	0.156	0.432

Notes: Observations are at the individual level. There is no variation in county Uyghur share for the bottom 80th percentile of observations, so these groups are combined into one large group.This group is the omitted category. All regressions control for year fixed effects; county 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.7: The Effect of Unrest Threat on Employment — Uyghur Outmigration

	(1)	(2)	(3)
Dependent Variable: Employment	SOE	Private	Salary (000s RMB)
Cty. Uyg. Share × Lag Xinjiang Incid.	37.96***	-28.12**	6,601***
× Male Minority	(14.35)	(11.68)	(1,934)
Control for Year FE × Male Minority × Change in Cty. Uyghur Share, 2000-2010	Y	Y	Y
Observations	215,784	215,784	170,365
R-squared	0.228	0.154	0.425

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 Unrest Threat on Employment — Omit Outliers

	(1)	(2)	(3)
Dependent Variable: Employment	SOE	Private	Salary (000s RMB)
Cty. Uyg. Share × Lag Xinjiang Incid.	76.03***	-0.710	2,719**
× Male Minority	(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.9: The Effect of Unrest Threat on Employment — Hui Share Placebo

	(1)	(2)	(3)	(1)	(2)	(3)
Dependent Variable: Employment	SOE	Private	Salary (000s RMB)	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)			
Non-Uyghur Minority Share × Lag Xinjiang Incid. × Male Minority				-0.00143	-0.00304	2.330*
				(0.00675)	(0.00556)	(1.249)
Observations	224,412	224,412	176,962	224,412	224,412	176,962
R-squared	0.233	0.158	0.431	0.230	0.156	0.431

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

Table A.10: 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.11: The Effect of Export Demand Shocks and Floods on Employment— Placebo

	(1)	(2)	(3)	(4)
	Lead Demand Shock		Lead County Flood	
Dependent Variable:	SOE	Private	SOE	Private
Mean of Dependent Variable	*0.660*	*0.160*	*0.550*	*0.250*
Shock Coefficient (β)	-0.0486	0.0267	0.0381	-0.0210
	(0.0493)	(0.0393)	(0.0349)	(0.0394)
Observations	291,203	291,203	225,039	225,039
R-squared	*0.214*	*0.111*	*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 province-year level for the trade shock regressions and at the county level for flood regressions. *** p<0.01, ** p<0.05, * p<0.1

Table A.13: 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.	25.87**	-18.20	5,429**
× Male Minority	(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.12: The Effect of Flood Disasters on Employment — Robustness to Base Year Sector Shares

Dependent Variable: Employment	(1) SOE	(2) 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.14: The Effect of Unrest Threat on Employment — Sparse Specification

Dependent Variable: Employment	(1) SOE	(2) Private	(3) Salary (000s RMB)
Cty. Uyg. Share × Lag Xinjiang Incid.	37.44***	-37.29***	-0.150
× Male Minority	(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.15: The Effect of Unrest Threat on Employment — Robustness to Binary Measure of Xinjiang Conflict Intensity

Dependent Variable: Employment	(1) SOE	(2) Private	(3) Salary (000s RMB)
Cty. Uyg. Share × Lag Binary Xinjiang Incid.	181.7***	-91.49**	23,939**
× Male Minority	(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.16: 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.17: 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.