STATE EMPLOYMENT AS A STRATEGY OF AUTOCRATIC CONTROL IN CHINA*

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Abstract

This paper presents evidence that autocrats use state-owned firms to prevent unrest via employment provision, a role that helps explain these low-productivity firms' favorable treatment 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 ethnic unrest, state-owned firms hire more male minorities, the demographic most likely to participate in the 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 I use the framework to quantify the implicit subsidy that state firms receive for hiring male minorities. I also find that direct government transfers to male minorities increase when unrest threat is high, but primarily among the non-employed, indicative of a complementary multi-pronged policy response. Furthermore, state employment increases after poor trade shocks and natural disasters, consistent with a general pacification role.

Keywords: Autocracy, state-owned enterprises, ethnic conflict, public employment
JEL codes: O12, D74, J45, P23, J15

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1 Introduction

All governments must manage unrest to stay in power, particularly autocracies, whose leaders face high exit costs (Egorov and Sonin, 2020). To do so, they may leverage known deterrents of conflict participation, like employment and labor income (Dube and Vargas, 2013; Bazzi and Blattman, 2014). At the same time, state-owned enterprises (SOEs) are a persistent feature of many economies, despite an array of downsides like low productivity and depressed innovation (Shleifer, 1998). Even after decades of privatization, SOEs comprise one-sixth of global output, one-fifth of global investment, and have increased their reach in recent years (World Bank, 2014; International Monetary Fund, 2020). Could one reason behind the persistence of state-owned sectors in autocracies be their political role in preventing unrest through job provision?

To test this explanation, this paper examines whether state employment increases in response to threats of unrest. It does so in the context of China, a salient example of an autocracy with a large state-owned sector. The government highly values economic growth, yet SOEs are substantially less productive than their private counterparts, own 40% of the economy's fixed assets, and represent 20% of employment. Privatization has stalled since the mid-2000s and even reversed in recent years (Economist, 2015).

This test requires variation in the threat of unrest, which I obtain via a natural experiment generated by an ongoing conflict between Uyghur ethnic separatists and the Chinese government in Xinjiang, a province in western China. This conflict creates the potential for unrest in the rest of China, especially in years when fighting in Xinjiang is intense and in counties outside Xinjiang where there is a large Uyghur population. The potential for unrest is greatest among minority men, who comprise over 85% of separatists (CECC, 2019). Together, these elements form a triple-differences strategy that combines annual variation in Xinjiang conflict intensity, county-level variation in the location of Uyghur populations outside Xinjiang, and individual-level variation in male minority status. To validate this proxy for ethic unrest threat, I show that a marker of Uyghur cultural identity, pork abstention, increases in exactly these times and places, only among minorities, controlling for household incomes.

This strategy addresses several difficulties in testing whether unrest prevention is a motive behind state employment. First, omitted variables like local economic conditions may drive both unrest and employment. Second, reverse causality is an issue: unrest may respond to state em-

ployment, especially if it is a useful pacification policy. Third, unrest may directly alter production —for example, by destroying capital or disrupting transportation —and change state employment through direct, apolitical channels.

The triple-differences approach offers several benefits. First, it addresses omitted variables 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. Second, the approach addresses reverse causality 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 two-way relationship between local unrest and local labor markets. Third, this approach addresses the problem of direct production effects by leveraging unrealized threats of unrest. A feature of the Uyghur conflict during my period of study is that unrest threats vary in intensity outside of Xinjiang but do not erupt into realized conflict. As a consequence, observed employment responses reflect preventative government action but do not include the direct production consequences of unrest.

This study setting features stringent data limitations due to the political sensitivity of unrest and control policies as well as firm incentives to misreport employment to signal compliance with government objectives. I address these measurement challenges in several ways. I use unrest events from a high-profile conflict that are corroborated by multiple international sources. I use Census data, consumption data, and less granular measures of ethnicity to measure Uyghur identity in a setting where this identity is severely and intentionally obscured. Furthermore, I use household surveys to measure employment, which are not influenced by firm incentives to misreport.

This paper finds that, in accordance with the idea that state employment is a policy for unrest prevention, male minority employment in SOEs increases relative to the general population in the years and counties when the potential for unrest is high. The magnitude of the response is small compared to total state employment but sizable compared to state employment among male minorities. For a one-standard-deviation increase in county Uyghur population share at the median conflict intensity, male minority state employment increases by 0.48 percentage-points, which represents 22.6% of male minority SOE employment.

As increased SOE labor demand should also have equilibrium consequences for private em-

ployment 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. First, when the threat of unrest increases, state employment of unrest-prone demographics should increase relative to the general population. Second, this 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. In line with theoretical predictions, and in addition to the main result that unrest threats differentially increase male minority SOE employment, I find that unrest threats decrease private sector male minority employment and increase male minority wages relative to the general population.

I explore whether the SOE hiring targets all minority men, or just Uyghur or Muslim men. I find evidence that all minorities are included. However, I construct a proxy for Uyghur identity leveraging meat consumption norms to show that the response is strong among likely-Uyghur men. I also show that the employment response targets counties with high Uyghur shares, and not other minority shares.

In robustness checks, I discuss alternative interpretations of the results, including private firm discrimination, Uyghur migration patterns, endogenous Xinjiang conflict event triggers, and national anti-discrimination or affirmative action policies. I find that it is difficult to explain all the results with these narratives. Furthermore, I address measurement concerns. I use alternative measures of Xinjiang incidents and Uyghur county share and show that the main results are robust to these changes. I also perform random permutation tests to establish that the relatively small sample of male minorities and of counties outside Xinjiang with non-zero Uyghur share do not generate spurious results. The results are robust to a variety of other tests as well.

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 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 suggest state employment plays a general stabiliz-

ing role beyond the context of ethnic conflict. 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 spark unrest, even outside of the context of ethnic conflict.

The central contribution of this paper is to document that autocracies use SOE employment to prevent unrest, thus showing how autocrats' desire to stay in power may lead them to intervene in firm operations —even in ways that directly distort input allocation and sectoral composition. Past theoretical work on autocratic survival has explored political instruments of control, including censorship, propaganda, and repression, as well as some economic policies like targeted benefits (Egorov and Sonin, 2020; Gehlbach et al., 2016). Empirical papers in this area have shown that autocrats use propaganda, censorship, and local elections to generate support, silence dissent, and gather information in practice (Adena et al., 2015; Yanagizawa-Drott, 2014; Rozenas and Stukal, 2019; King et al., 2014; Martinez-Bravo et al., 2022). Other empirical work finds that autocracies use personnel management to provide bureaucratic incentives, offering promotions to reward good performance (Jia et al., 2015; Xu, 2018) and demotions to punish adverse outcomes (Campante et al., 2022). In contrast, this paper highlights state employment as an economic instrument of autocratic control, one that operates through existing firms and that directly distorts firm productivity and sectoral composition.

By doing so, this paper points to a political benefit of state-owned firms: unrest prevention. A longstanding literature has debated whether public enterprises exist primarily to correct for market failures (Atkinson and Stiglitz, 1980) or to provide political benefits (Shleifer and Vishny, 1994), like buying voter support. While some recent work identifies efficiency motives for state ownership, like subsidizing upstream industries (Liu, 2019) and enforcing regulations (Zeng, 2017), this paper instead pinpoints an explicitly political rationale for the state sector.

This rationale shows that governments may use state employment to mitigate unrest even before it takes place, which adds to a literature on intrastate conflict. Prior work shows that employment and labor income can decrease conflict participation and intensity (McGuirk and Burke, 2020; Bazzi and Blattman, 2014; Dube and Vargas, 2013; Miguel et al., 2004), indicating that employment could be a tool to manage unrest. Relatedly, some governments use economic policies like skills training (Blattman and Annan, 2016), conditional cash transfers (Crost et al., 2016),

and development grants (Beath et al., 2017) to prevent further violence in post-conflict settings and among ex-combatants. This paper shows that governments use state employment to prevent unrest, and do so even before conflict has occurred.

Finally, this paper highlights a new force behind the allocation of public employment. In many settings, public sector jobs are not awarded to individuals according to ability alone (Finan et al., 2017; Weaver, 2021). Instead, leaders may use a system of patronage, granting public jobs to political allies in exchange for loyalty or votes (Colonnelli et al., 2020; Xu, 2018; Brollo et al., 2017; Ornaghi, 2016; Anderson et al., 2015) . This paper points to another non-ability driver of public job allocation: unrest prevention.

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 the main results, and Section 7 performs robustness checks. Section 8 presents additional results on employment responses to floods and trade shocks. Section 9 concludes.

2 Background

2.1 State-Owned Enterprises in China

This subsection presents the recent history of Chinese SOE productivity and reform. A robust literature has established that SOEs are 20-50% less productive than their private counterparts (Song et al., 2011; Dong and Putterman, 2003; Jefferson et al., 2000), and thus greatly decrease the aggregate productivity of the Chinese economy[1]. 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

[1] I corroborate these results using multiple productivity estimation techniques in Online Appendix Subsection 10.4.

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.[2] 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 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 in part because they offer an essential political benefit: unrest pacification. Qualitative evidence strongly supports this idea. For example, the central government directs SOEs to "effectively maintain unity and social stability", "absorb the local ethnic minority labor force", and "deal with conflicts and disputes affecting ethnic unity in a timely manner" (National Ethnic Affairs Commission, 2012).

2.2 State Employment as a Pacification Policy

This subsection discusses why state employment may prevent unrest. One central mechanism is that state employment increases the opportunity cost of unrest. Because of time constraints, energy constraints, or even risks of being fired, unrest may lead state-employed individuals to forgo labor

[2]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.

income. These opportunity costs are weighed against the potential gains from unrest, and the higher they are, the less likely participation will become. A wide array of empirical papers document this mechanism (Dube and Vargas, 2013; Bazzi and Blattman, 2014; McGuirk and Burke, 2020).

More generally, employment programs display pacifying effects in diverse contexts. In a post-conflict setting, Blattman and Annan (2016) find that a job training and search assistance program in Liberia decreases the likelihood that ex-combatants participate in violence. And Fetzer (2020) finds that an Indian public employment program decreased conflict participation (though this outcome was not the program's central motivation).

State employment may also be easier to enforce and target than alternative control policies, like transfers. It is relatively straightforward to observe whether an employee is at work, whether they are producing output, and whether their identity conforms to that of a targeted demographic. In contrast, transfer programs are more susceptible to fraud; for example, in one survey of Chinese unemployment insurance recipients in Liaoning, 80% of recipients possessed disqualifying sources of income (Vodopivec et al., 2008). Moreover, the stakes of accurate targeting are high, as mis-targeted programs can increase resentment and unrest (Cameron and Shah, 2014).

State employment may also prevent unrest through social and psychological channels. One channel could be lower audience costs: benefits earned through employment may be perceived as more fair than transfers given without work. Over half of all participants in political conflict in China are working-age men, and transfers to this group may have particularly high audience costs. Another channel could be that state jobs inspire loyalty or support for the government. Consistent with this idea, surveys find that SOE employment in China is strongly negatively correlated with support for democratization (Chen and Lu, 2011).

While the Chinese government has many policy tools to manage conflict, state employment has a unique set of pacifying properties, including increasing the opportunity cost of unrest, ease of enforcement and targeting, relatively low audience costs, and the inculcation of loyalty. These advantages may help compensate the state for SOEs' downsides.[3]

[3] Appendix Subsection 10.5 presents further discussion about the efficacy of state employment as a pacification tool.

2.3 The Uyghur Conflict in Xinjiang

The central empirical strategy of this paper, described in Section 4, relies on variation in a 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).

Primary evidence suggests that the timing and intensity of incidents in this era 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 the 2008 Beijing Olympics (Bovingdon, 2010).

3 Conceptual Framework

To test the central hypothesis more rigorously, I develop a framework of pacification-motivated state employment to generate predictions to take to the data. 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 10.1. 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: an unrest type and a non-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: $\frac{u_\ell\left(\ell^{j*}, c^{j*}\right)}{u_c\left(\ell^{j*}, c^{j*}\right)} = w_j$. 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, $Y = F(U,N)$[4], but are subjected to different government policies. In particular, the government taxes all non-unrest type labor in the economy at rate $\tau_N > 0$ and provides a subsidy $\tau_U < 0$ to SOE unrest-type hiring. In equilibrium, the private firm will therefore equate its marginal rate of technical substitution to its implicit input price ratio $\frac{F_U^{priv*}}{F_N^{priv*}} = \frac{w_U}{w_N(1-\tau_N)}$, and SOEs will do the same $\frac{F_U^{soe*}}{F_N^{soe*}} = \frac{w_U(1-\tau_U)}{w_N(1-\tau_N)}$.

The government values total output and stability, S. Stability is a decreasing function of an unrest shock, $\xi \in \mathbb{R}^+$, and the leisure of unrest-types, ℓ^U. The unrest shock ξ captures the efficacy of unrest activity once individuals have already committed to participate. In this setup, the government dislikes unrest not because it directly depresses output, but because it could lead to more serious problems if left unchecked. This choice best reflects the nature of unrest threats in the empirical section: too small to disrupt production, but potential catalysts for severe conflict. The government solves $max_{\tau_U,\tau_N}\ Y^{priv} + Y^{soe} + \eta S\left(-\xi z\left(\ell^U\right)\right)$ subject to the budget constraint, $\tau_U w_U U^{soe} + \tau_N w_N N = 0$. 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, all agents' choices must 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.

[4]I assume that firm production satisfies Inada conditions.

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.

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

Via 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 generate empirical predictions for how labor market outcomes respond to unrest shocks. 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).[5] I provide a detailed discussion of these results in Appendix Subsection 10.1 and full proofs of each in the Online Mathematical Appendix.

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

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

3.3 Sufficient Statistic

The framework also generates a 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 $\frac{\alpha N^{soe}}{(1-\alpha)U^{soe}} = \frac{w_U(1-\tau_U)}{w_N(1-\tau_N)}$ and $\frac{\alpha N^{priv}}{(1-\alpha)U^{priv}} = \frac{w_U}{w_N(1-\tau_N)}$. By dividing the first equation by the second equation, I obtain:

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

Using this expression, I compute τ_U, the implicit SOE wage subsidy for male minority employees, in Subsection 7.3. Because the production function assumption is very strong, this result is intended as a benchmarking exercise.

4 Empirical Strategy

To test the theoretical predictions, I leverage variation in a regional ethnic conflict that generates threats elsewhere in China. This variation maps onto ξ, the unrest shock. The variation has three

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

components: temporal variation in conflict incidents in Xinjiang, geographic variation in Uyghur population shares across the rest of China, and whether individuals are male minorities. The sample of analysis will cover 2002-2009, the years for which minority data is available.

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 use the count of incidents per year as a measure of intensity. I use incident count instead of fatalities as the latter are more prone to strategic manipulation and reporting error. In the baseline specification, I use a one year lag as employment may be a fairly slow-moving policy instrument.[6]

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) [7]. Next, 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. The time series of lag Xinjiang conflict events for sample years is plotted in Figure 1 and a list of all incidents is presented in Appendix Table A.1.

The second component is each Chinese county c's Uyghur population share, $U_{c,t=2000}$. This variable is obtained by dividing the number of Uyghur individuals by the total population of the county in the 2000 Population Census of China (The National Bureau of Statistics, 2010). I use the 2000 Census as it predates of the baseline sample, thus removing some endogeneity in Uyghur population distribution that might arise from the migration of Uyghur peoples in response to contemporaneous, unobserved forces, like friendly local policies.[8] Figure 2 presents a map of county-level Uyghur population shares outside of Xinjiang[9]. Counties with high Uyghur shares are spread

[6] I consider alternative incident measures in Section 7 and present alternative lags in Section 7.1.

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

[8] I investigate the robustness of the main results to Uyghur migration further in Section 7.

[9] The first row of Table 1 presents detailed summary statistics

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. The literature suggests that Uyghur settlement patterns are generated by a combination of historical and modern forces. Historical forces include Ming-dynasty military dispatches (Svanberg, 1988) and eighteenth-century pilgrimages (Coughlin, 2006). More recent forces include local demand for service jobs (Brophy, 2016). These forces may represent omitted variables, so I flexibly control for pre-period labor market conditions in the baseline specification, detailed in Subsection 4.2.

I explore potential correlates of county Uyghur share in Table 1. I compare basic economic and demographic characteristics of populations across varying levels of county Uyghur share. For comparison, 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.[10]. 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, urbanization, employment, and individual 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 7.

The interaction of the first two components is $DD_{ct} = I_{t-1}^{XJ=1} \times U_{c,t=2000}^{XJ=0}$. At this stage, I introduce the superscript $XJ = 0$ onto the county Uyghur share variable to highlight the fact that the sample does not include counties in Xinjiang.

The reasons to omit Xinjiang from the sample of analysis fall into two categories: those con-

[10]These individual-level variables come from the Urban Household Survey, which is described in Section 5.

cerning 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 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. 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 Validating the Relevance Assumption

The relevance assumption required for the difference-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. By design, I cannot directly

[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 10.5, 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).

validate the assumption, as a key property of the empirical strategy is that it does not generate realized unrest. However, I can provide indirect validation that the difference-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 a measure of Uyghur cultural identification that varies at the individual-year level: abstention from pork (Cesaro, 2000). I regress this measure on the difference-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.2. Column (1) demonstrates that abstention from pork among minorities increases in response to the shock, but only among minority individuals, with $t = 2.82$ ($p < 0.000$). To ensure that these movements in abstention from pork are not due to income effects, in column (2), I add controls for household income interacted with minority fixed effects, which allows income to influence product consumption, and differently so for minority and majority groups. The results are strongly robust. In columns (3) and (4), I separate the results by gender and find that pork abstention increases among both male and female minorities, but not male or female Han individuals.

An interdisciplinary literature on conflict propagation also supports the relevance assumption. Forsberg (2014), Forsberg (2008), Cederman et al. (2009), and Buhaug and Gleditsch (2008) find 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 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 (Toops, 2009).

That 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. Another is that ethnic identity becomes salient during times of conflict, and preferences related to ethnic identity receive greater weight as a result.

4.2 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. Around 2002-2009, the Chinese economy underwent dramatic changes that very well could have produced such an omitted variable, including the privatization reforms, the 2001 accession to the World Trade organization, and the fiscal stimulus response to the 2008 global financial crises.

Due to these concerns, 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 identity is observable, so a government with a limited budget would likely 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 U.S. intelligence data known Chinese political prisoners (CECC, 2019). Male minorities comprise over 45% of unrest participants but represent just 4% of the general population. 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).[13]

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 minorities. In Subsection 6.0.1, I study whether SOE hiring targets male minorities in general or male Uyghurs specifically. I also test whether the policy focuses on where the Uyghur diaspora live.

Appendix Table A.3 summarizes individual characteristics among non-minorities, minorities, and minority men. On average, minorities are slightly younger, more likely to be female, and

[13] SOEs also hire more male minorities than private firms do. On the left-hand chart in Figure 3b, I plot the average share of male minorities in private firms versus SOEs: SOEs hire disproportionately more than private firms, and this difference is precise at the $p < 0.001$ level. These employment data are described in Section 5.

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 slightly 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.[14]

With the addition of the third interaction, the shock can be written as the following expression, where the additional index i represents individuals and the variable M_i represents an indicator if a person is a male ethnic minority: $DDD_{ict} = I_{t-1}^{XJ=1} \times U_{c,t=2000}^{XJ=0} \times M_i$. The exclusion restriction for a triple differences specification is substantially more difficult to violate, as 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 framework's three directional predictions on SOE employment, private employment, and salaries further decrease the possibility that an omitted variable could reject the joint null. Though it is difficult to identify concrete phenomena that satisfy these criteria, I nonetheless consider and control for potential sources of omitted variables in Subsection 7.

The baseline estimating equation is:

$$\begin{aligned} Y_{ict} = {} & \alpha + \beta_M \left(I_{t-1}^{XJ=1} \times U_{c,t=2000}^{XJ=0} \times M_i \right) + \beta \left(I_{t-1}^{XJ=1} \times U_{c,t=2000}^{XJ=0} \right) \\ & + \gamma_1 \left(I_{t-1}^{XJ=1} \times M_i \right) + \gamma_3 M_i + \eta_c \times M_i + \tau_t \\ & + \delta_{2i} \left(X_i \times I_{t-1}^{XJ=1} \times U_{c,t=2000}^{XJ=0} \right) + \delta_{1c} \left(X_c \times \tau_t \times M_i \right) + Dist\,XJ_c \times \tau_t + \varepsilon_{ict}, \end{aligned} \tag{2}$$

where i indexes individuals, c indexes counties, and t indexes years. The baseline sample includes all individuals surveyed in the Urban Household Survey (UHS)[15] 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

[14] 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. I also perform further robustness checks in Section 7.

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

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.[16] Several lower interactions are absorbed by fixed effects ($U_{c,t=2000}^{XJ=0} \times M_i$, $I_{t-1}^{XJ=1}$, and $U_{c,t=2000}^{XJ=0}$). 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. X_i includes age, gender, and a fixed effect for years of education, which are also interacted with the Xinjiang incident and Uyghur share variables.

I also control for the interaction of the logged kilometer distance of each county from Xinjiang, $Dist\,XJ_c$, interacted with year fixed effects, τ_t. This control addresses geographic- and time-varying government policies, like the 2000 "Open Up the West" campaign.[17] 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. I cluster standard errors at the county level ($N = 492$) to account for the shock's level of geographic variation.[18]

I reinterpret 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. The theory predicts that when unrest threat rises, male minorities should differentially work more for SOEs ($\beta_M^{SOE} > 0$), less for private firms ($\beta_M^{PRIV} < 0$), and earn higher salaries ($\beta_M^{Salary} > 0$).

[16] Because the UHS data only document individuals' minority status, not their precise ethnicity, the coefficient β_M captures the differential 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.

[17] 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 in Section 7.

[18] I use alternative clustering techniques in Section 7.

5 Data

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. The data also contain a rich set of variables describing household composition, age, gender, employment, education, income, and consumption.[19] 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 collective firms in China exhibit operational similarities to SOEs (Brandt and Rawski, 2008).[20] Additionally, the analysis will only focus on working-age individuals, defined as ages 18-55 for women and 18-60 for men.

The baseline measure of ethnicity in the UHS data is an indicator for whether an individual is an ethnic minority.[21] One downside of this measure is that I do not directly observe whether an individual is Uyghur, though this property is not unique. Of the top eight individual-level datasets that contain repeated cross-section or panel data in China, none report disaggregated ethnicity enough to generate a usable sample of Uyghur-identified individuals[22]. To establish whether state jobs go to Uyghur men specifically, in Subsection 6.0.1, I construct a proxy for Uyghur identity using household meat consumption patterns. I also check whether the government targets places with high Uyghur population shares, or other minority shares.

The UHS data are a representative sample of urban areas in 17 provinces covering 65.9% of China's 2000 population[23]. Within these provinces, counties were chosen via random sampling, stratified by per capita income and total population size. Remote areas and designated ethnic minority areas were over-sampled (UHS Codebook, 2009). The baseline sample of this paper

[19]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.

[20]I show that the main results are not sensitive to this choice in Section 7.

[21]The survey handbook does not specify whether the measure is self-reported, though ethnicity is listed on Chinese identity cards, which are commonly used during enumeration.

[22]I list and discuss these datasets more in Subsection 4.

[23]Anhui, Beijing, Gansu, Guangdong, Heilongjiang, Henan, Hubei, Jiangsu, Jiangxi, Liaoning, Shaanxi, Shandong, Shanghai, Shanxi, Sichuan, Yunnan, and Zhejiang

contains 492 counties.[24]

6 Results

This section builds to the baseline in three steps. First, I show how minority employment differentially responds to the threat of unrest; then, I partition that result by gender; and finally, I show the male minority triple difference. This approach highlights the variation behind the key results and addresses the fact that male minority identity is an interaction of both gender and ethnicity.[25]

To be concrete, the first step involves a specification wherein the male minority indicator term, M_i, in Equation (2) 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. Appendix Table A.4 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 Predictions (1), (2), and (3).

Column (1) of Appendix Table A.4 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. For all subsequent tables, I do not report the double interaction of Xinjiang incidents and Uyghur country share because omitted variables discussed in Subsection 4.2 prevent a causal interpretation.

Appendix Table A.4 likely sacrifices substantial statistical power. 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. Hence, I re-estimate Appendix Table A.4 for men and women separately. Table 2 demonstrates

[24] There are approximately 2,800 county-level divisions in China.

[25] Indeed, Equation (2) 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.

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. The coefficient in 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 column (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.

The coefficients for the female subsample are not statistically distinguishable from zero. This suggests that unrest threats do not generate differential SOE hiring of minority women relative to Han women. This heterogeneity rules out purely ethnicity-based affirmative action motives.

I now proceed with the baseline specification. Table 3 presents results from estimating Equation (2) 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 $p < 0.01$.[26]

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[27]. Finally,

[26] I do not report the double interaction of Xinjiang incidents and Uyghur country share because omitted variables discussed in Subsection 4.2 prevent a causal interpretation.

[27] 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

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.

By design, this empirical strategy leverages unrealized unrest threats, so it is not feasible to directly test state employment's efficacy in preventing conflict. However, in Appendix Subsection 10.5, I present facts that indicate that state employment mitigates unrest in this setting.

6.0.1 Are SOEs Hiring Uyghur Men or Minority Men in General?

Because I cannot separately identify minority groups in the UHS data, it may be that state-owned firms hire minority men in general, not just Uyghur men, in response to unrest threats. The size of the employment and salary responses support this idea. Across China, Uyghurs represent 8.4% of ethnic minorities and Muslim groups represent 27.5% of ethnic minorities. Outside of Xinjiang, these figures are 0.8% and 17.6%.[28] Given small populations, it would be difficult to increase male minority SOE employment by 22.6% by only hiring Uyghur men, or Muslim men. Similarly, if the salary treatment were concentrated only on Uyghur men, it would be implausibly large: $742/0.008 = 92,750$ RMB. It is therefore very likely that SOEs hire a broad group of male minorities in response to unrest threats.[29]

Nonetheless, I can provide direct evidence that SOEs are hiring men who are likely Uyghur, and SOEs hiring takes place in counties where the Uyghur diaspora reside, not where other minority groups live. To test whether Uyghur men specifically receive state jobs, I leverage detailed consumption information in the UHS data. Specifically, I use data on pork consumption, which is considered taboo in Uyghur tradition, and mutton consumption, which is a Uyghur staple protein. Using this information, I construct a household-level proxy for Uyghur identity that equals one if an ethnic minority household ever reports annual zero pork consumption at the same time as positive mutton consumption. This indicator includes 7.5% of minorities and 0.27% of all individuals. Note that this variable is time-invariant at the household level, unlike the measure used in Subsection 4.1, which varies at the individual-year level. Table 4 Panel A reports estimates using this proxy for Uyghur identity. I find that state employment differentially increases for males

loyalty, or the threat of having employment conditioned on good behavior, may be most important.

[28] Figures from the 2000 Census.

[29] These results are consistent with explicit state directives that require SOEs to hire ethnic minorities to maintain unity and social stability (National Ethnic Affairs Commission, 2012). The rules state that all ethnic minorities should be considered (not just Uyghurs in particular), and that hiring should "absorb the local ethnic minority labor force" and "deal with conflicts and disputes affecting ethnic unity in a timely manner".

in likely-Uyghur households and private employment differentially decreases. The coefficient for salary is not precisely estimated.

Additionally, I test whether the location of county Uyghur share drives the SOE response, rather than the shares of other minority groups. To do so, I add a triple-interaction term with county-level Hui share from the 2000 Census to the baseline equation 2 (and lower-level interactions). The Hui ethnic group are the largest Muslim minority ethnic group in China. Table 4 Panel B reports 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, but the Uyghur share interactions strongly resemble the baseline. I repeat this exercise using the share of non-Uyghur minorities in the 2000 Census. Table 4 Panel C shows that the non-Uyghur minority coefficients on state and private employment are nulls, and the magnitude of the salary coefficient is statistically precise but very small. The Uyghur share interactions enter precisely with the expected signs. Overall, these tests indicate that Uyghur population shares specifically drive the main result.

7 Robustness Checks

In section, I consider alternative hypotheses and present a variety of robustness checks. The first alternative hypothesis is that private firm discrimination drives all three baseline results. Specifically, suppose that private firms dismiess male minorities when unrest threat is high, but SOEs hire the surplus. Additionally, since SOEs offer a wage premium, this could generate an increase in average male minority salaries. To investigate, I report the treatment effect on salary among SOEs separately from private firms in columns (4) and (5) of Table 3. Salaries for male minorities increase differentially among both firm types, which is difficult to rationalize with simple private firm discrimination.

Could the within-private salary increase be due to selection, whereby private firms retain higher-ability minority men and fire the rest? To test this idea, I omit all education controls from the regressions above. I find that the private salary coefficient without education controls is $5,730$ ($p < 0.01$), compared to $5,997$ ($p < 0.01$) with education controls. A selection mechanism would predict that education controls should attenuate the private salary result, as long as ability and education are positively correlated. However, I observe the opposite. Furthermore,

in the robustness checks below, the SOE employment response is consistently precise and positive while the private response varies in precision. If private firing were the only mechanism, one might expect the reverse. To summarize, while private firm discrimination may be present, it is unlikely to generate all of the observed results without an increase in SOE male minority hiring.

County-Level Controls. I consider the possibility that correlated shocks to strategic sectors, like utilities and mining, could drive the key results. While SOEs may provide jobs to prevent unrest, SOEs are also used to retain control over strategic sectors (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. To perform a robustness check for these strategic motives, I compute the share of employment in mining, utilities, and public services for each county in China in base year of the sample. I then interact each variable with year fixed effects and male minority fixed effects and add the full interaction into the baseline specification. Table 5 Panel A reports this set of robustness checks. 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 are reported in Appendix Table A.5 Panel A.

Another source of spurious results could be correlates of county-level Uyghur share. Table 1 shows that county-level population, urbanization, and employment share are correlated with Uyghur share. I control for each of these variables interacted with year fixed effects and male minority fixed effects in Table 5 Panel B. The SOE and salary results are robust, though the private coefficient declines in magnitude and is not precisely estimated.

Another driver of spurious results could be place-based policies, like the "Open Up the West" Program, which was implemented starting in 2000 and sought to stimulate the economic development of western provinces with a variety of policies. Though the baseline sample already controls for distance to Xinjiang times year fixed effects, it is possible that the policies also differentially targeted certain demographics. In Table 5 Panel C, I run a specification that controls for logged kilometer distance of each county from Xinjiang times year fixed effects and times the male minority indicator. The results remain similar in precision, sign, and magnitude.

One might also be concerned that labor markets in counties with initially high Uyghur popula-

tion 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 reported in Table 5 Panel D and they are highly similar in magnitude and precision.

Alternative Incident Time Series. 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 incident using primary evidence. I then omit every event whose trigger came from outside Xinjiang. For example, rebel groups timed attacks to coincide with the Beijing Olympics in August 2008 to publicize grievances. Table 6 Panel A reports estimates using this amended Xinjiang incident time series as $I_{t-1}^{XJ=1}$. The baseline results hold.[30]

Endogeneity could also arise if Xinjiang unrest incidents are triggered by local economic conditions that are in turn correlated with economic conditions across China. To address this possibility, I construct an incident time series that removes all events sparked by economic issues. For example, I remove factory closure protests that occurred in October 2001. Table 6 Panel B reports estimates using this alternate series, which corroborate the main results.

Another robustness check addresses potential mis-measurement in the number of Xinjiang incidents per year. The government aggressively manages coverage of sensitive topics like the Xinjiang conflict. 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 influence. Relatedly, 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 occurred on August 27 and September 2. I code events on separate calendar days as separate incidents, but one could argue that these attacks were part of one longer event.

I address these problems of government manipulation and incident ambiguity via 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. Table 6 Panel C reports estimates using this binary measure. The triple

[30] A list of all incidents and triggers is presented in Appendix Table A.1.

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 Table 6 Panel D, 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 are small in magnitude and not precisely different from zero for all three outcome variables.

Small Sample Tests. One may be concerned that the main specification relies on variation in small subsets of the data: male minorities and counties with high Uyghur shares. 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 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 4.4% 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 0.21 and 0.01, respectively. I find that the baseline employment and salary results are unlikely to be generated by random assignment of county Uyghur share, and again, the private employment is less robust. This fact is consistent with the idea that the private employment response is an indirect effect of SOE hiring.

Alternative Uyghur Share Measures. One may wonder whether the fine gradations in continuous Uyghur share are salient to the government. In Table A.6 Panel A, I instead use a binary measure of county Uyghur share that equals one for strictly positive shares and zero otherwise. As in the baseline, SOE employment increases, private employment falls, and salary increases differentially for male minorities.

Next, 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 Panel B. The signs of the interacted treatment effects all remain consistent with the predictions of the conceptual framework. These results rule out another alternative interpretation of the results. The latter two terms of $\beta_M I_{t-1}^{XJ=1} \times U_{c,t=2000}^{XJ=0} \times M_i$ could proxy for the likelihood that a given minority man is Uyghur. In that case, any nation-wide push by SOEs to hire more male Uyghurs in the wake of conflict incidents in Xinjiang may generate the observed results, though without the channel of geographic targeting on high-Uyghur-share counties. The nonlinearity I observe across Uyghur share categories strongly rejects this interpretation.

Alternative Samples. Do sub-samples of the data drive the main result? First, I consider outliers. 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. Appendix Table A.5 Panel B shows that SOE and salary results are robust to this procedure, and the private employment result remains negative but is not precisely estimated. Next, I check whether omitting collective firm employment from the definition of SOE employment matters. In Appendix Table A.5 Panel C, I find that SOE and salary results remain precise and positive when omitting collectives. The private employment result is imprecisely estimated.

Migration. Uyghur migration patterns may be correlated with county-level minority friendly policies and temporal patterns in Xinjiang conflict incidents. For example, Uyghur men could migrate to places with beneficial policies when conflict is high. To test this idea, 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 results are robust and in Table 7 Panel A.

Another way to ensure that migrants do not drive the main results is to restrict the sample to local hukou holders only. Though it is possible to convert hukou in China, the requirements for doing so are highly restrictive and generally take long periods of time. Therefore, temporary,

seasonal, or recent migrants would not hold local hukou. 96.8% of individuals in the baseline sample hold local hukou, and restricting the sample to this group gives very similar results to the baseline, as seen in Table 7 Panel B.

Alternative Specifications and Standard Error Clustering. I check whether the rich baseline controls affect the main result. I run a sparse specification that includes only the components of the triple interaction, the distance of each county from Xinjiang interacted with year fixed effects, and county-year fixed effects. Appendix Table A.7 Panel A 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. I also test whether the results hold with logit regressions. They do; results are reported in Appendix Table A.7 Panel B.

I test whether the results are robust to using two-way clustered standard errors, with counties (492) and years (8) as the units of clustering. Results are reported in Appendix Table A.7 Panel C. 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).

I use province-level clustering in Appendix Table A.7 Panel D; the results remain precise. I also implement individual-level clustering, leveraging the short panel sampling of the UHS. Appendix Table A.7 Panel E demonstrates that both the SOE and salary results are precise at the $p<0.05$ level, but the private employment result is not statistically different from zero. The relative imprecision of the private employment response may be consistent with unrest threats changing it indirectly through SOE hiring.

7.1 Heterogeneity

Heterogeneity by Sector. Are there sectors in which the SOE pacification response is more pronounced? To answer this question, I divide 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 by the state, 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.

Table 8 reports the results. Only the service sector 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. The fact that state hiring is most pronounced in the service sector is notable, as it is the state sector with the highest proportion of male minority SOE employees. It also has the highest rates of turnover among SOE sectors, perhaps an advantage for countering changing threats over time and space.

Heterogeneity over Time. How durable is the employment response? I introduce five total lags of the unrest threat shock to the baseline equation to investigate (the full specification is presented in Appendix Equation (22). One important caveat for this exercise is that the dataset's coverage 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. This variation will be less representative of the whole period.

I plot the five lag coefficients in Appendix Figure A.2. The three sub-figures reveal that the labor market responses to the Uyghur unrest shock in year t are most pronounced initially 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 those of state employment, consistent with the preferred interpretation that changes in state labor demand drive these market responses.

7.2 Complementary Policies: Social Relief Transfers

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 (2) using social relief transfers in thousands of RMB as the outcome variable. Results are reported in Table 9. In column (1), I find that in response to the shock, average social relief transfers to male minorities differentially increase by 17.51 thousand RMB, and the change is precisely different from zero at the $p < 0.01$ level. This result indicates that the government complements its employment pacification policies with targeted relief transfers. For a year with the median value of incidents, the magnitude of this estimate implies that individuals will receive 2.4 yuan more when moving to a county with a one standard-deviation higher Uyghur population share. 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 167.8 RMB among relief transfer recipients. The same calculation for column (4) yields an increase of 845.2 RMB among non-employed 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.

7.3 Sufficient Statistic

Finally, I substitute empirical moments from the baseline regression sample (UHS data, 2002-2009) into Equation (1) and compute of τ_U, the value of male minority wage subsidies:

$$\tau_U = 1 - \frac{N^{soe}/U^{soe}}{N^{priv}/U^{priv}} = 1 - \frac{45.95}{62.17} = 1 - 0.739 = 0.261. \tag{3}$$

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 $(20\%, 32\%)$.[31] 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.

8 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.

[31] 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.

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).[32] The annual provincial demand shock exposure, $\Delta DSEIV_{pt} = \sum_s \left[\frac{X_{spt-1}}{X_{st-1}} \sum_{a \in A} \sum_{b \in B} \Delta E_{st}^{ab} \right]$, has two components: a weight variable and a trade flow variable.

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.[33] This setup avoids using flows that directly involve China itself, which are certainly influenced by China's domestic situation.[34] I estimate the following regression using UHS data to uncover the response of employment to the trade shock.

$$\begin{aligned} Y_{ict} = \alpha + \beta \Delta DSEIV_{pt} + \gamma Age_i + \delta Edu_i + \zeta Male_i \\ + \delta_M Edu_i \times \text{Male}_i + \gamma_M Age_i \times \text{Male}_i + \tau_t + \eta_c + \varepsilon_{ict} \end{aligned} \tag{4}$$

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.

[32] Campante et al. (2022) use a similar setup to estimate how trade shocks affect Chinese labor strikes.

[33] 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.

[34] 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 10.3. 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.

Column (1) of Appendix Table A.8 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 pro-cyclically 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.9. 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.9. To further increase confidence that these results are not elicited by spurious trends, I re-estimate Equation (4) 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.10 - neither coefficient is statistically different from zero.

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.

$$\begin{aligned} Y_{ict} = \alpha + \beta \Delta Flood_{ct-1} + \gamma Age_i + \delta Edu_i + \zeta Male_i \\ + \delta_M Edu_i \times \text{Male}_i + \gamma_M Age_i \times \text{Male}_i + \tau_t + \eta_c + \varepsilon_{ict} \end{aligned} \tag{5}$$

In this equation, i indexes individuals, c indexes counties, and t indexes years. The dependent variables, Y_{ipt}, are the same as above. 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.[35]

Appendix Table A.8 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.11. I also conduct a placebo check by re-estimating Equation (5) using the lead of the flood indicator variable. The results from these regressions are reported in Online Appendix Table A.10, and reassuringly, employment composition by ownership does not respond to future floods.

9 Conclusion

This paper provides causal evidence that autocrats use state-owned enterprises as a means of preventing unrest. This finding provides a political justification for the persistence and favoritism of these unproductive firms in autocratic regimes. The central empirical test in this paper uses a triple-differences approach to document the response of state 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, indicating an increase in SOE labor demand rather than a decrease in 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 histori-

[35] For the period 1990-2017, 889 county-years suffer riverine flooding according to this definition, about 1.1% of all county-years. I cluster the standard errors at the county and year level, the level at which floods vary.

cally 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? And what consequences do these choices have for welfare and development? More broadly, this paper invites the evaluation of other unproductive firms and institutions in the light of regime survival. Could the persistence of some economic distortions be due to states' desire to prevent unrest? Are there other political economy motives that may prompt such distortions? These questions all relate to the fundamental theme of how, and why, the political objectives of states manifest as forces of economic development.

Figure 1: Plot of Lag Xinjiang Unrest Incidents

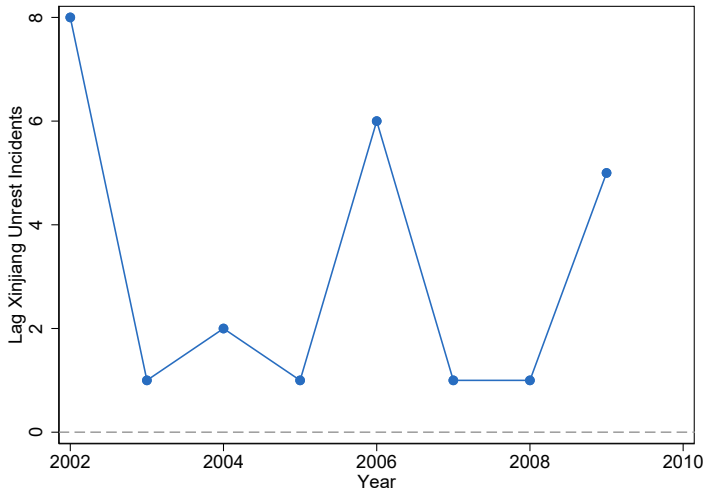
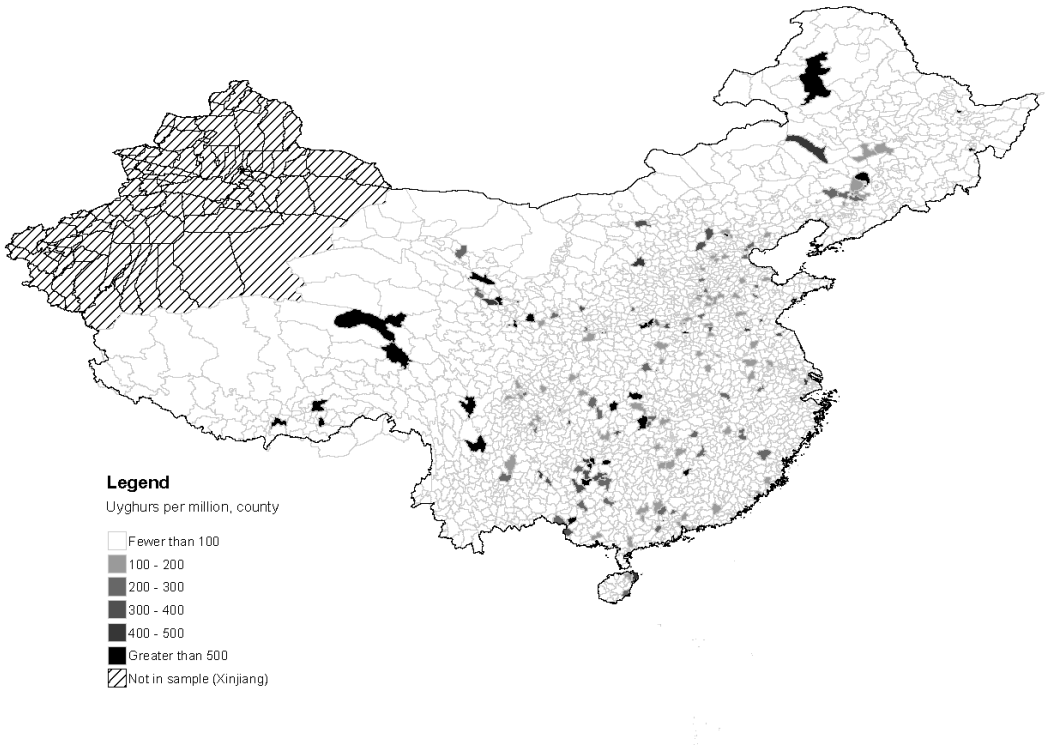


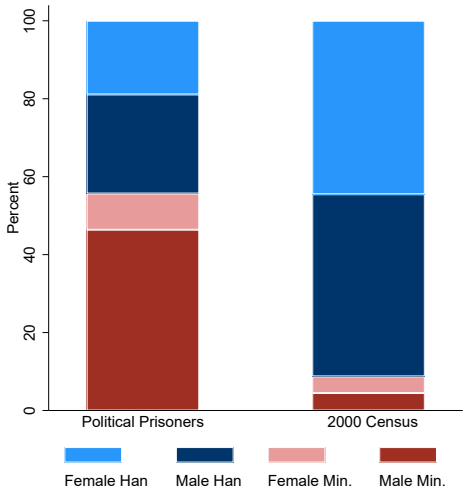
Figure 2: Choropleth of County Uyghur Share Outside Xinjiang



Notes: Data from 2000 Census of China.

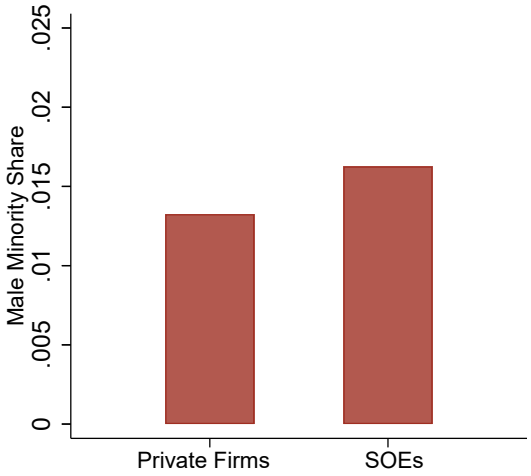
Figure 3: Demographic Comparisons

(a) Political Prisoners vs. General Population



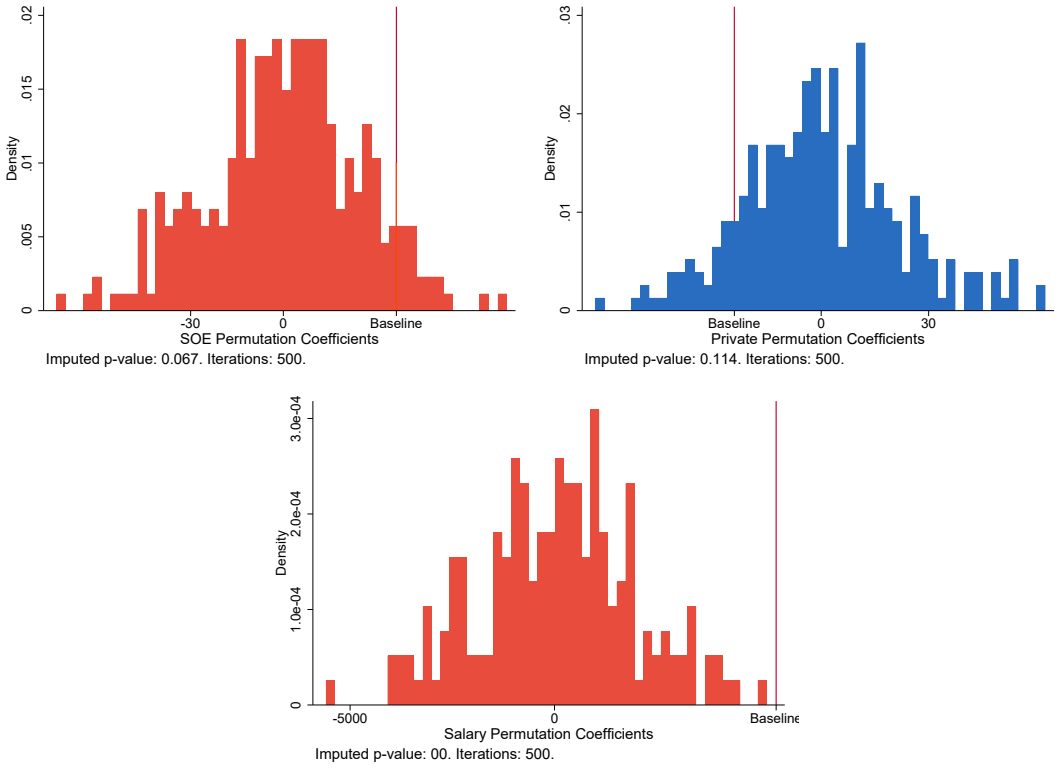
Notes: Means conditional on age, years of education, county, survey year
Source: Census 2000, China Political Prisoners Database.

(b) Private Firms vs. SOEs



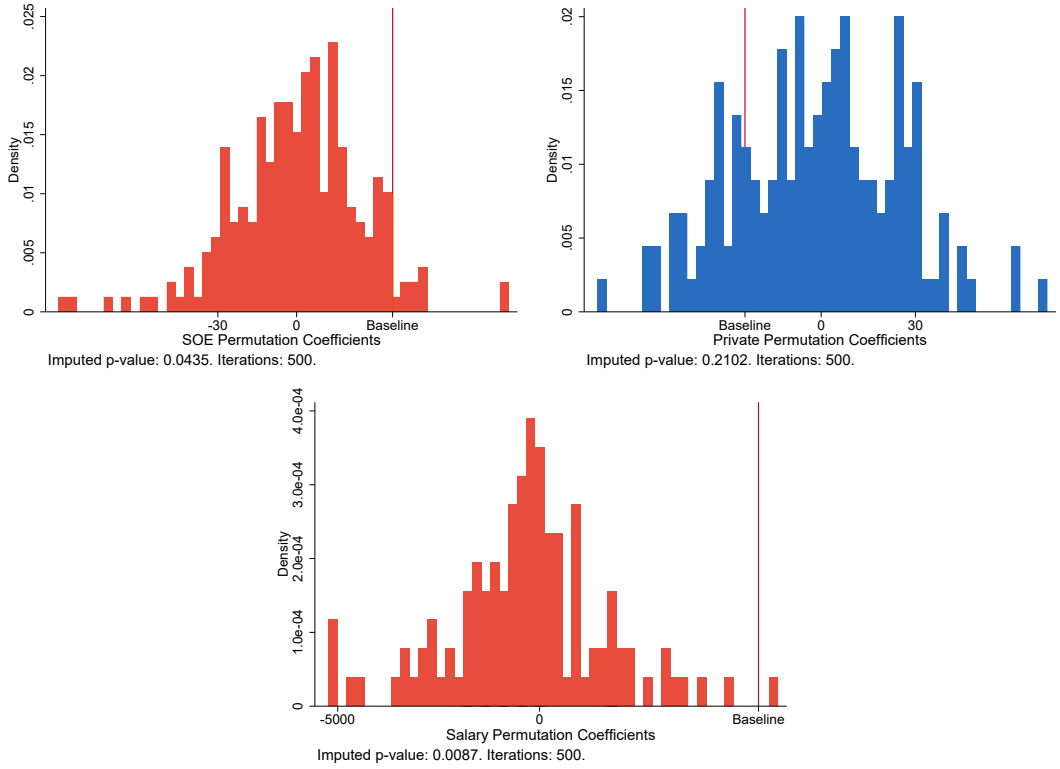
Notes: Means conditional on age, years of education, county of residence, survey year, sector. Male min t-test p-value: 0.000.
Source: Urban Household Survey 2002-2009

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



Notes: The implied p-values are 0.067, 0.114, and 0.011. All tests ran for 500 iterations. Coefficients are obtained by re-running the baseline regression 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-values are 0.04, 0.21, and 0.01. All tests ran for 500 iterations. Coefficients are obtained by re-running the baseline regression with counterfactual county Uyghur shares. For each iteration, counties are assigned a Uyghur share value from the existing distribution, without replacement. All other baseline controls are included.

Table 1: County 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 SO	-	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 and Gender

Dependent Variable:	(1) SOE	(2) Private	(3) 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 3: The Effect of Unrest Threat on Employment — Baseline

Dependent Variable: Employment	(1) SOE	(2) Private	(3)	(4) Salary (000s RMB) In SOE	(5) 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-year 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 4: The Effect of Unrest Threat on Employment — Proxy for Uyghur Identity and Non-Uyghur Ethnic Shares

	(1)	(2)	(3)
Dependent Variable:	SOE	Private	Salary (000s RMB)
	Panel A: Proxy for Uyghur Identity		
Cty. Uyg. Share × Lag Xinjiang Incid.	75.04***	-48.69**	2,650
× Male Uyghur Proxy	(17.81)	(24.22)	(2,061)
Observations	212,524	212,524	170,315
R-squared	0.233	0.156	0.406
	Panel B: Placebo with Ethnic Hui Share		
Cty. Hui Share × Lag Xinjiang	3.45e-05	-2.11e-05	0.00271
Incid. × Male Minority	(2.98e-05)	(2.20e-05)	(0.00253)
Cty. Uyghur Share × Lag Xinjiang Incid.	27.81**	-19.80	4,878**
× Male Minority	(13.60)	(14.15)	(2,315)
Observations	224,412	224,412	176,962
R-squared	0.233	0.158	0.431
	Panel C: Placebo with Non-Uyghur Minority Share		
Non-Uyghur Minority Share × Lag Xinjiang	0.00243	-0.00600	2.967**
Incid. × Male Minority	(0.00688)	(0.00593)	(1.247)
Cty. Uyghur Share × Lag Xinjiang Incid.	36.49***	-26.67**	5,468**
× Male Minority	(13.28)	(11.52)	(2,127)
Observations	224,412	224,412	176,962
R-squared	0.231	0.156	0.432

Notes: Observations are at the individual-year level. Household abstension from pork while consuming mutton is used as a proxy for Uyghur identity. 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 lag Xinjiang incident time series. 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 — Robustness to County-Level Controls

Dependent Variable:	(1) SOE	(2) Private	(3) Salary (000s RMB)
	Panel A: Control for Strategic Sector Employment Shares x Year FE x Male Minority FE		
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
	Panel B: Control for Correlates of County Uyghur Share x Year FE x Male Minority FE		
Cty. Uyg. Share × Lag Xinjiang Incid. × Male Minority	28.79**	-12.73	3,833*
	(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
	Panel C: Control for Distance from Xinjiang x Year FE x Male Minority FE		
Cty. Uyg. Share × Lag Xinjiang Incid. × Male Minority	36.59***	-24.24**	5,422***
	(12.59)	(11.04)	(2,075)
Observations	224,412	224,412	176,962
R-squared	0.231	0.156	0.431
	Panel D: Control for Baseline County Uyghur Share x Year FE		
Cty. Uyg. Share × Lag Xinjiang Incid. × Male Minority	35.62***	-22.88**	4,468**
	(12.38)	(10.98)	(1,958)
Observations	224,412	224,412	176,962
R-squared	0.231	0.156	0.432

Notes: Observations are at the individual-year 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 lag Xinjiang incident time series. 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 Alternative Incident Time 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.	49.34***	-39.52**	7,051***
× Male Minority	(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.	60.08***	-46.63**	7,312***
× Male Minority	(19.20)	(18.14)	(2,336)
Observations	108,173	108,173	78,225
R-squared	0.275	0.191	0.429
	Panel C: Binary Measure of Incidents		
Cty. Uyg. Share × Lag Alternative 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
	Panel D: Placebo with Lead of Xinjiang Incidents		
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-year 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 7: The Effect of Unrest Threat on Employment — Robustness to Migration Flows

Dependent Variable:	(1) SOE	(2) Private	(3) Salary (000s RMB)
	Panel A: Control for Net Uyghur Migration 2000-2010 x Year FE x Male Minority FE		
Cty. Uyg. Share × Lag Xinjiang Incid.	37.96***	-28.12**	6,601***
× Male Minority	(14.35)	(11.68)	(1,934)
Observations	215,784	215,784	170,365
R-squared	0.228	0.154	0.425
	Panel B: Local Hukou Holders Only		
Cty. Uyg. Share × Lag Xinjiang Incid.	42.33***	-32.39***	5,555***
× Male Minority	(11.86)	(6.271)	(1,601)
Observations	220,099	220,099	174,229
R-squared	0.229	0.154	0.434

Notes: Observations are at the individual-year 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 lag Xinjiang 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 — Heterogeneity 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 9: 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

Notes: Observations are at the individual-year 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

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10 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.

10.1 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.

10.1.1 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:

$$\frac{u_\ell\left(\ell^{j*}, c^{j*}\right)}{u_c\left(\ell^{j*}, c^{j*}\right)} = w_j. \tag{6}$$

10.1.2 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:

$$\frac{F_U^{priv*}}{F_N^{priv*}} = \frac{w_U}{w_N(1-\tau_N)}. \tag{7}$$

10.1.3 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.[36]

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:

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

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

$$\frac{F_U^{soe*}}{F_N^{soe*}} = \frac{w_U\left(1-\tau_U\right)}{w_N\left(1-\tau_N\right)}. \tag{8}$$

10.1.4 The Government

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

The government cannot spend more on subsidies than it raises on taxes, and since it does not value revenue directly, its budget constraint will hold with equality.

$$\begin{aligned} max_{\tau_U,\tau_N}\, & Y^{priv} + Y^{soe} + \eta S\left(-\xi z\left(\ell^U\right)\right) \\ & \text{s.t. } \tau_U w_U U^{soe} + \tau_N w_N N = 0 \end{aligned} \tag{9}$$

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

$$G^{**} = max_{\tau_U}\, Y^{priv}\left(\tau_U\right) + Y^{soe}\left(\tau_U\right) + \eta S\left(-Z\left(h - U\left(\tau_U\right)\right) - \xi\right) \tag{10}$$

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

The first order condition of the government is:

$$\frac{dY^{soe}}{d\tau_U} + \frac{dY^{priv}}{d\tau_U} + \eta\xi \underbrace{\frac{dS}{dZ}}_{\ominus} \underbrace{\frac{dz}{dU}}_{\ominus} \frac{dU}{d\tau_U} = 0. \tag{11}$$

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

$$G^* = max_{\tau_L} Y^{priv}(\tau_U) + Y^{soe}(\tau_U). \tag{12}$$

10.1.5 Market Clearing

In equilibrium, Equations (6), (7), and (8) 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.

$$Y^{soe*} - w_U(1-\tau_U)U^{soe*} - w_N(1-\tau_N)N^{soe*} = 0 \tag{13}$$

$$Y^{priv*} - w_U U^{priv*} - w_N(1-\tau_N)N^{priv*} = 0 \tag{14}$$

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: $Y = w_U U + w_N N$.

10.1.6 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.

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

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

$$w_U = \frac{u_\ell\left(\ell^U, c^U\right)}{u_c\left(\ell^U, c^U\right)} \tag{15}$$

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.

$$w_U = \frac{F_U^{soe}\Big|_{N=\overline{N^S}}}{(1-\tau_U)} \tag{16}$$

$$w_U = F_U^{priv}\Big|_{N=\overline{N^P}} \tag{17}$$

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**.

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

$$w_U = \frac{u_\ell\left(\ell^N, c^N\right)}{u_c\left(\ell^N, c^N\right)} \tag{18}$$

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

$$w_N = \frac{F_N^{soe}}{(1-\tau_N)}\Bigg|_{L=L^{soe*}} \tag{19}$$

$$w_N = \frac{F_N^{priv}}{(1-\tau_N)}\Bigg|_{L=L^{priv*}} \tag{20}$$

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**.

10.1.9 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:

$$\underbrace{\frac{dY}{d\tau_U}}_{\text{"output cost"}<0} + \underbrace{\eta\xi \underbrace{\frac{dS}{dZ}}_{\ominus}\underbrace{\frac{dz}{dU}}_{\ominus}\frac{dU}{d\tau_U}}_{\text{"stability benefit"}>0} = 0. \tag{21}$$

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.

10.2 Effects over Time

I use the equation below to estimate the employment response to unrest over time. 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.

$$\begin{aligned} Y_{ict} = \alpha &+ \sum_{j=1}^{5} \beta_{Mj} I_{t-1}^{XJ=1} \times U_{c,t=2000}^{XJ=0} \times M_i + \sum_{j=1}^{5} \beta_j I_{t-1}^{XJ=1} \times U_{c,t=2000}^{XJ=0} \\ &+ \sum_{j=1}^{5} \gamma_{1j} I_{t-1}^{XJ=1} \times M_i + \gamma_2 U_{c,t=2000}^{XJ=0} \times M_i + \gamma_3 M_i \\ &+ \delta_c X_c \times \tau_t \times M_i + \delta_i X_i + \tau_t + Dist\,XJ_c \times \tau_t + \eta_c \times M_i + \varepsilon_{ict} \end{aligned} \tag{22}$$

10.3 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.

10.4 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 10.3. The firm productivity measures have been normalized by sector and province medians to ensure compara-

bility 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.

$$TFPR_{ipst} = \alpha + \beta SOE_{it} + \tau_t + \eta_p + \gamma_s + \varepsilon_{ipst} \tag{23}$$

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.12. 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.

10.5 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.

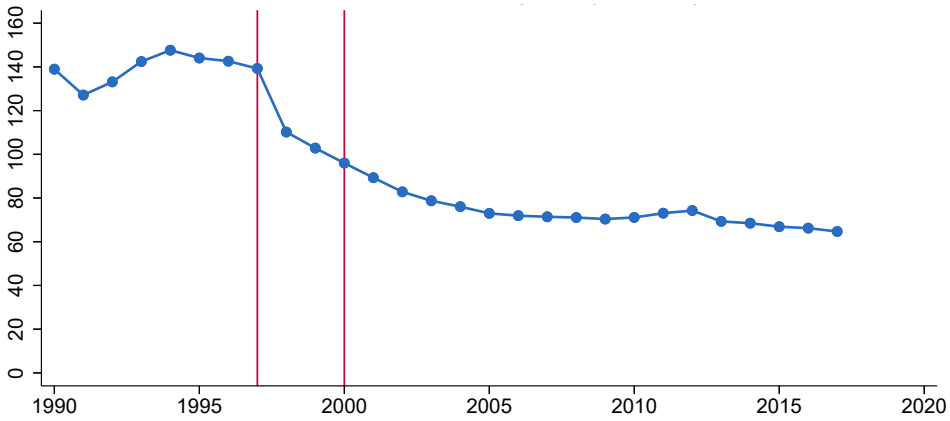
In this subsection, I present suggestive evidence that Chinese SOE employment is associated with lower unrest. To do so, I plot the relationship between provincial state employment share and protest intensity, as well as between private employment share and protest intensity. I construct a province-year 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.

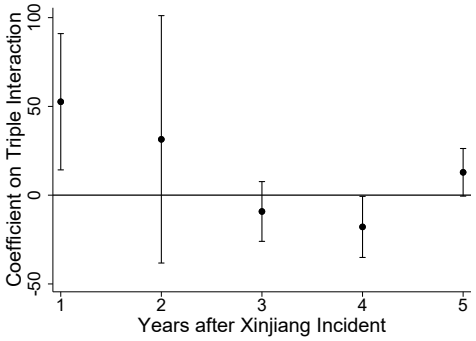
Figure A.1: Urban SOE Employment Over Time (millions of workers)



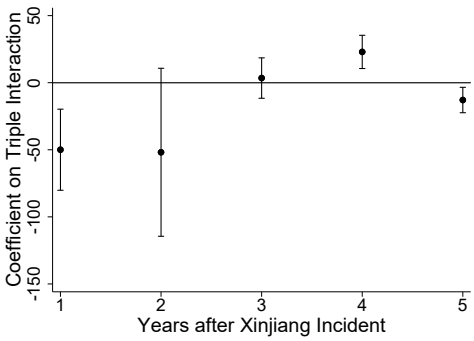
Notes: Data from the 1990-2018 Statistical Yearbooks of China.

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

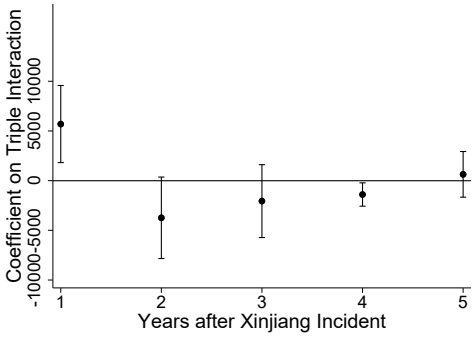
(a) SOE Employment



(b) Private Employment



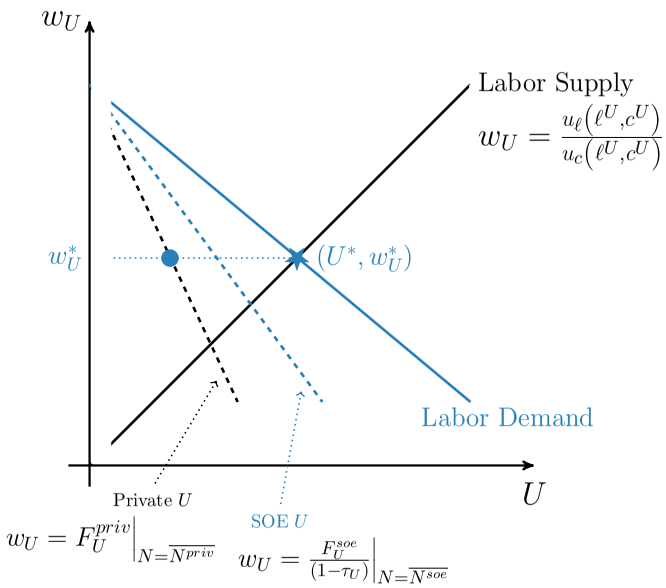
(c) Salary



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

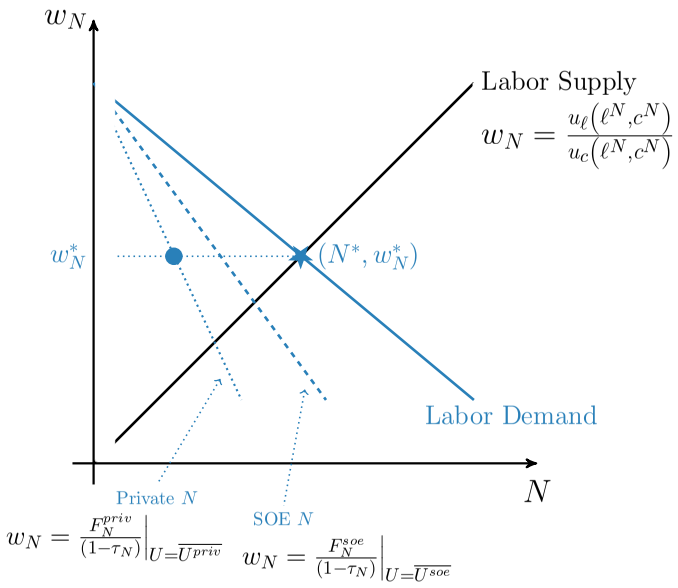
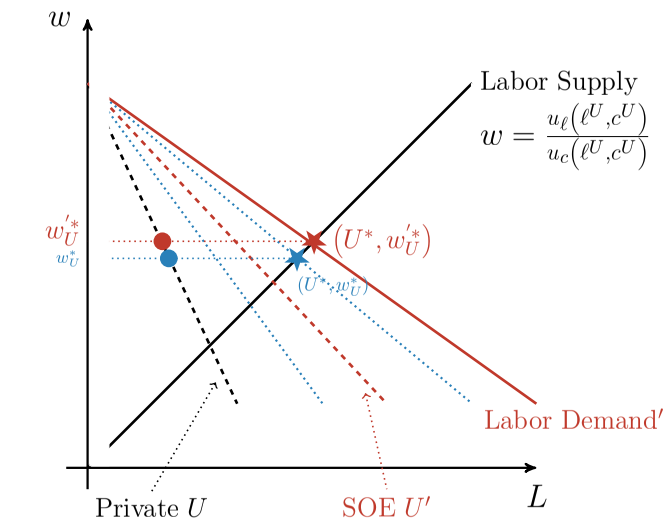


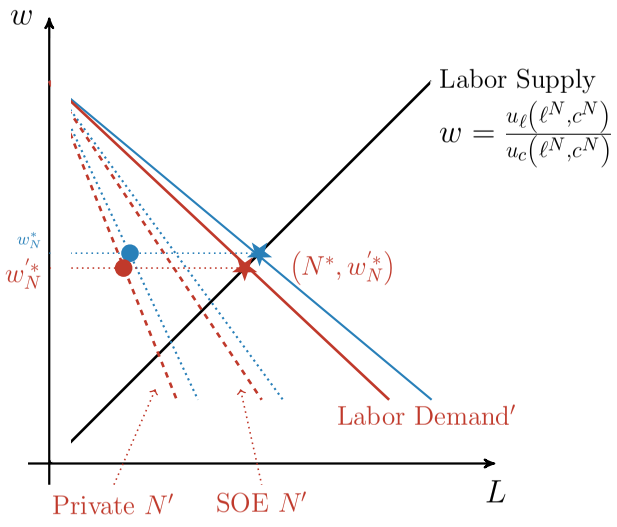
Figure A.4: Equilibrium Responses to $\tau_U \uparrow$

(a) U-type Labor Market



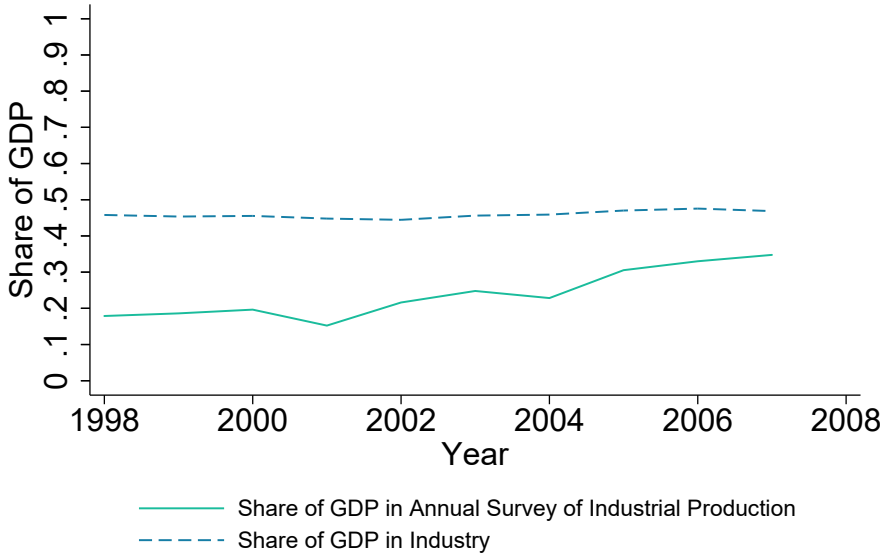
$w_U = F_U^{priv}\big|_{N=\overline{N^{priv}}}$ $\quad w_U = \frac{F_U^{soe}}{(1-\tau_U)}\big|_{N=\overline{N^{soe}}}$

(b) N-type Labor Market



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

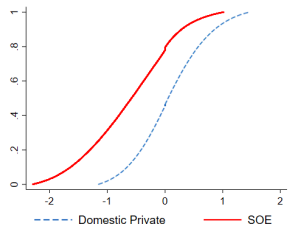
Figure A.5: Share of GDP covered by the *Annual Survey of Industrial Production*



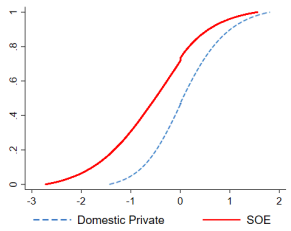
Source: China Statistical Yearbook 2017, World Bank 2017, Annual Survey of Industrial Production 1998-2007.

Figure A.6: Firm Productivity by Ownership

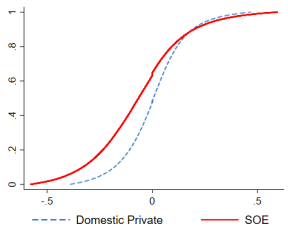
(a) Labor Productivity



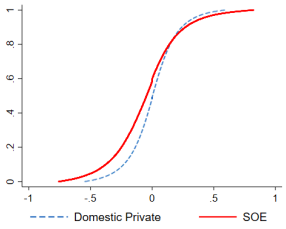
(b) TFPR (Hsieh and Klenow, 2009)



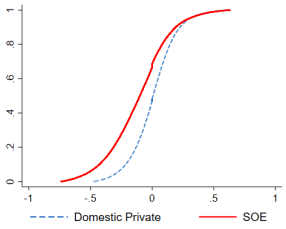
(c) TFPR (OLS)



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

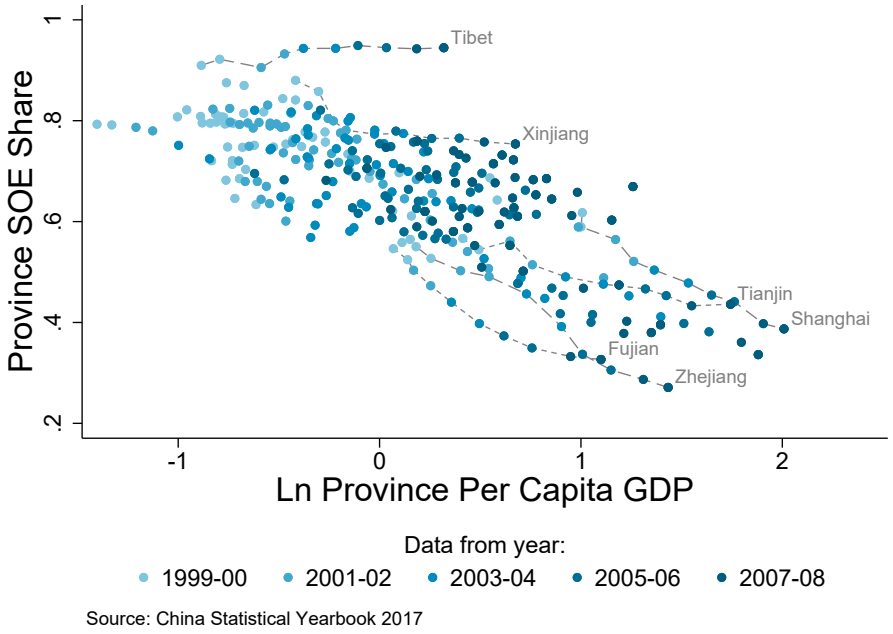


(e) TFPR (Gandhi et al., 2011)



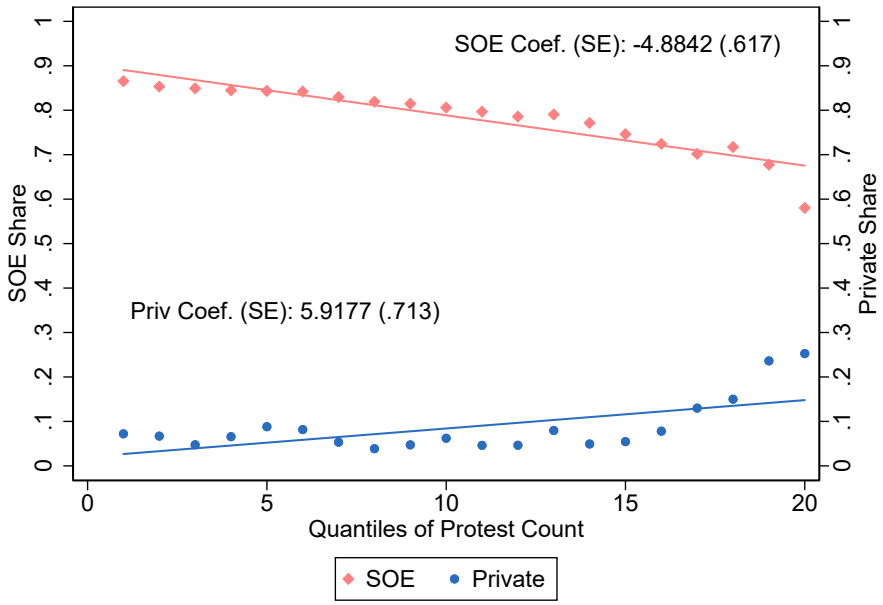
Notes: These figures plot the cumulative distribution of five alternative productivity measures by firm ownership. Data are from the Annual Survey of Industrial Production. Each productivity measure is demeaned by four-digit sector and province.

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



Source: China Statistical Yearbook 2017

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



Note: All variables are normalized by province popuation and year fixed effects.

Table A.1: All Xinjiang Conflict Incidents in Baseline Sample

(1) Year of Incident*	(2) Month	(3) Day	(4) Location	(5) Event	(6) Trigger
2001	2	3	Kashgar	Assassination	Rebel strategy
2001	8	7	Aksu	Attack	Rebel strategy
2001	8	7	Urumqi	Riots	Inspection of unlicensed fruit vendor
2001	10	1	Hotan	Protests	Mosque destruction
2001	10	14	Hotan	Arson	Mosque destruction
2001	11	2	Kizilsu	Attack	Gunfight
2001	12	24	Hotan	Protests	Factory layoffs
2001	12	27	Hotan	Protests	Factory layoffs
2002	5	27	Kashi	Attack	Religious text inspection
2003	3	2	Urumqi	Bombing	Unknown
2003	4	5	Aksu	Attack	Doctor attacked by relative of patient
2004	6	11	Yili	Protests	Work relocation packages
2005	1	20	Urumqi	Bombing	Rebel strategy
2005	2	22	Turpan	Attack	Individual knife attack
2005	3	12	Poskam	Clash	Uyghur students fought with Han
2005	4	16	Korla	Strike	Tax on taxi driver revenues
2005	7	15	Kashgar	Attack	Attempt to build mosque
2005	10	1	N/A	Threat	Rebel strategy
2006	12	15	Urumqi	Bombing	Unknown
2007	1	10	Taxkorgan	Raid	Police action
2008	8	4	Kashgar	Attack	Rebel strategy
2008	8	11	Kuqa	Bombing	Rebel strategy
2008	8	13	Yamnaya	Attack	Rebel attack on policemen
2008	8	27	Kashgar	Attack	Rebel attack on policemen
2008	9	2	Kashgar	Clashes	Rebel attack on policemen

Notes: * The baseline specification uses the one-period lag of incidents, so 2001-2008 incidents are used for the 2002-2009 baseline sample. All events take place in Xinjiang.

Table A.2: Validation of Relevance Assumption: Uyghur Cultural Practices

	(1)	(2)	(2)	(2)
		Control for Income x Minority FE		
Dependent Variable: No Pork		Full Sample	Male	Female
Mean of Dependent Variable	*0.0100*	*0.0100*	*0.0100*	*0.0100*
Cty. Uyg. Share × Lag Xinjiang Incid.	34.07***	32.95***	27.22*	37.96**
× Minority	(12.10)	(12.25)	(14.83)	(15.93)
Cty. Uyg. Share × Lag Xinjiang Incid.	1.214	1.121	1.146	1.050
	(0.815)	(0.813)	(0.753)	(0.952)
Observations	219,808	176,112	97,967	78,145
R-squared	0.064	0.060	0.061	0.063

Notes: Observations are at the individual-year 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 consumption in a given year in the Urban Household Survey. *** p<0.01, ** p<0.05, * p<0.1

Table A.3: Individual Characteristics by Minority Status

	(1)	(2)	(3)
	Non-Minorities	Minorities	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. Standard deviations are reported in parentheses.

Table A.4: 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. × Minority	18.30*	-15.34*	3,131*
	(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 A.5: The Effect of Unrest Threat on Employment — Robustness to Alternative Samples

Dependent Variable:	(1) SOE	(2) Private	(3) Salary (000s RMB)
	Panel A: Omit Public Service, Mining, and Utilities		
Cty. Uyg. Share × Lag Xinjiang Incid.	29.21**	-26.76**	8,416***
× Male Minority	(13.95)	(11.18)	(2,796)
Observations	178,016	178,016	135,731
R-squared	0.247	0.167	0.441
	Panel B: Omit Outliers		
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
	Panel C: Omit Collective Firms		
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-year 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 lag Xinjiang incident time series. 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 — Robustness to Alternative Uyghur Share Measures

Dependent Variable:	(1) SOE	(2) Private	(3) Salary (000s RMB)
	Panel A: Binary Measure of Uyghur Share		
Binary Cty. Uyg. Share × Lag Xinjiang Incid.	0.0169**	-0.00917*	1.694**
× Male Minority	(0.00681)	(0.00525)	(0.693)
Observations	224,412	224,412	176,962
R-squared	0.231	0.156	0.431
	Panel B: Quantiles of Uyghur Share		
Cty. Uyg. Share × Lag Xinjiang Incid. × Male Minority × 0 - 80th percentile	-	-	-
Cty. Uyg. Share × Lag Xinjiang Incid.	248.7**	-30.23	8,952
× Male Minority × 80-85th percentile	(115.7)	(67.01)	(10,757)
Cty. Uyg. Share × Lag Xinjiang Incid.	129.0**	-69.36*	6,271***
× Male Minority × 85-90th percentile	(54.72)	(37.80)	(2,102)
Cty. Uyg. Share × Lag Xinjiang Incid.	14.16	1.591	6,578**
× Male Minority × 90-95th percentile	(58.43)	(45.34)	(3,215)
Cty. Uyg. Share × Lag Xinjiang Incid.	30.61***	-22.09**	4,218*
× Male Minority × 95-100th percentile	(11.06)	(10.91)	(2,326)
Observations	224,412	224,412	176,962
R-squared	0.231	0.156	0.432

Notes: Observations are at the individual-year 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 the appropriate Uyghur share measure and the lag Xinjiang time series. 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 — Robustness to Alternative Specifications, Logit, and Alternative Standard Error Clustering

	(1)	(2)	(3)
Dependent Variable:	SOE	Private	Salary (000s RMB)
	Panel A: Sparse Specification		
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
	Panel B: Logit Regression		
Cty. Uyg. Share × Lag Xinjiang Incid.	195.4***	-192.1***	-
× Male Minority	(73.71)	(68.56)	-
Observations	224,048	223,832	-
	Panel C: Two-way Clustered Standard Errors		
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
	Panel D: Province Clustered Standard Errors		
Cty. Uyg. Share × Lag Xinjiang Incid.	36.59***	-24.24***	5,422***
× Male Minority	(11.26)	(7.774)	(1,657)
Observations	224,412	224,412	176,962
R-squared	0.231	0.156	0.431
	Panel E: Individual Clustered Standard Errors		
Cty. Uyg. Share × Lag Xinjiang Incid.	36.59*	-24.24	5,422**
× Male Minority	(20.37)	(18.96)	(2,705)
Observations	224,412	224,412	176,962
R-squared	0.231	0.156	0.431

Notes: Observations are at the individual-year level. The sparse specification controls for log kilometers county distance from Xinjiang times year fixed effects, year fixed effects, and county fixed effects. All other 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 lag Xinjiang incident time series. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table A.8: 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

Table A.9: 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.10: 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.11: The Effect of Flood Disasters on Employment — Base Year Sector Shares

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

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

Table A.12: SOE vs. Domestic Private Manufacturing Productivity

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

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.