THE POLITICAL ECONOMY OF STATE EMPLOYMENT AND INSTABILITY IN CHINA*

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Abstract

This paper demonstrates that China uses state employment to promote social stability via job provision. I use variation in an ethnic conflict in China's Xinjiang province to establish that, in years and counties with a higher threat of unrest spillover, state-owned firms hire more male minorities, the demographic most likely to participate in ethnic unrest. Concurrently, male minority wages rise and private firms hire fewer members of this group. These patterns are consistent with a model of government-subsidized, stability-oriented state employment, and a model-derived quantification exercise suggests that state firms implicitly receive a 26% subsidy on male minority wages. Furthermore, I find that state employment increases after natural disasters and poor trade shocks, evidence that suggests the stability role of Chinese state firms is general.

Keywords: State-owned enterprises, ethnic unrest, conflict, public employment, China.
JEL codes: O12, P26, D74, J30, J15

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1 Introduction

All governments must maintain social and political stability to stay in power. Many forces threaten stability, including low income levels (Collier and Hoeffler, 1998; Fearon and Laitin, 2003; Collier and Hoeffler, 2004; Hegre and Sambanis, 2006), negative labor income shocks (Miguel et al., 2004; Dube and Vargas, 2013; Bazzi and Blattman, 2014), and demographic factors like male-skewed sex ratios (Edlund et al., 2013), youth-skewed age profiles (Urdal, 2004), and ethnic polarization (Montalvo and Reynal-Querol, 2005).

Governments of all types have used economic policy to maintain control in the face of destabilizing forces. In nineteeth-century Germany, Otto von Bismark passed a package of social insurance and welfare reforms to prevent class conflict (Esping-Andersen, 1990; Sala-i Martin, 1997). During the Great Depression, the United States implemented a broad range of relief programs, partly spurred by the desire to quell mass disorder (Piven and Cloward, 2012). In the mid-2000s, Indian policymakers cited pacifying Maoist violence as one objective of the national workfare program, NREGA (Fetzer, 2014; Dasgupta et al., 2014; Khanna and Zimmermann, 2017). In the remainder of this paper, I investigate whether another economic policy, state employment, serves a stabilizing role, which may help explain its presence in a diversity of modern settings, including Ethiopia, Argentina, and Australia (Broussard and Tekleselassie, 2012; Kostzer, 2008; Burgess et al., 1999).

China is an ideal context in which to study this question. State-owned enterprises (SOEs) remain a large force in the economy: since 2006, they have employed about one-fifth of the labor force, about 70 million people, comparable to the entire population of France. Additionally, the Chinese government considers stability a goal of paramount importance and retains the *de jure* and *de facto* ability to influence SOE hiring.

With the goal of generating empirically-testable predictions, I begin by developing a model of SOE stabilization by embedding a government with multidimensional preferences for output and stability into a general equilibrium framework. In this setup, there are two types of firms, private and SOEs, as well as two types of individuals, an "unrest-prone" type and a "non-unrest" type. When unrest-prone individuals are not employed, they par-

ticipate in activities that decrease stability. To counteract these activities, the government can choose to subsidize SOE employment of the unrest-prone worker type to boost employment and stability, but at a cost to output. The subsidy is funded by a tax on non-unrest type workers in both types of firms.

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

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

The central unrest shock arises from an ongoing ethnic conflict in Xinjiang, China's westernmost province. There, some members of the Uyghur ethnic minority have been fighting for independence, citing discriminatory and oppressive policies. Over 85% of non-state participants in the conflict are male minorities (Congressional-Executive Com-

mission on China, 2019). I construct a measure of the degree to which conflict in Xinjiang generates threats of spillover unrest for counties in other Chinese provinces. This measure is high in years preceded by many Xinjiang unrest incidents, in non-Xinjiang counties with large Uyghur population shares. I omit Xinjiang from the sample of analysis because local conflict intensity and the local labor market are likely influenced by each other and common unobservable factors.

I estimate how male minority SOE employment, private employment, and wages respond to the unrest threat relative to those in the general population. The comparison between male minorities and the general population is essential. It addresses the plethora of ownership-specific reforms, fiscal programs, trade agreements, and other omitted variables that may covary with the county-year unrest shock and employment outcomes. As long as these forces affect male minorities and the general population equally, I can interpret the differential response of male minority employment as causal. In line with model predictions, I find that male minority SOE employment increases in response to the unrest shock, while private employment decreases. As predicted, male minority wages increase. The size of the SOE employment response at the mean value of the unrest shock corresponds to a 0.48 percentage-point increase in the probability of SOE employment, on a mean of 55%.

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

by creating counterfactual Uyghur population distributions and show that my baseline coefficients are larger than 94.9% of coefficients computed using counterfactual population data.

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

Furthermore, I use the model's sufficient statistic for the SOE labor subsidy to find that Chinese SOEs implicitly receive a 26% subsidy on male minority employment. This value is large but not unprecedented relative to targeted wage subsidies in other contexts. In the mid-2000s, Hungary implemented payroll tax subsidies for firms that hired workers out of long-term unemployment. The subsidies began at 25% for the first year of employment and declined to 15% for the worker's second year (Cseres-Gergely et al., 2015). In 2006, Finland implemented a subsidy for payroll taxes that represented approximately 16% of gross worker income. The program targeted older, full-time, low-wage workers (Huttunen et al., 2013).

Within the model, the male minority subsidy strictly decreases welfare, because individuals value only consumption and leisure and the subsidy hurts output 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.

Finally, I present two empirical patterns consistent with a general stability role for Chinese SOEs. First, employment in private firms falls in times and places with poor export demand, while employment in SOEs increases. Second, while private firms shed labor in

the year following a flood disaster, SOEs hire more labor. These patterns show that SOE employment counterbalances negative shocks that may generate instability, even outside of the context of ethnic unrest.

This paper contributes to several literatures. First, I add to work documenting the determinants of and policy responses to unrest. Among rationalist explanations of conflict, the opportunity cost hypothesis argues that people participate more in conflicts when they have less to lose. Empirical work shows that lower income levels do seem to accompany higher rates of civil conflict (Collier and Hoeffler, 1998; Fearon and Laitin, 2003; Collier and Hoeffler, 2004; Hegre and Sambanis, 2006) and drops in labor income increase unrest incidence (Miguel et al., 2004; Dube and Vargas, 2013; Bazzi and Blattman, 2014). Other determinants of conflict include demographic patterns like male-skewed sex ratios (Edlund et al., 2013), youth-skewed age profiles (Urdal, 2004), and ethnic polarization (Montalvo and Reynal-Querol, 2005). Some recent work documents how governments combat these forces with policy. Fetzer (2014) shows how India's national workfare program, NREGA, pacified Maoist conflicts, an outcome that policymakers explicitly sought. This paper contributes direct empirical evidence of pacifying intent: I show that state employment responds proactively to destabilizing shocks.

Additionally, this work contributes to the literature on autocratic governance and control. Social stability is particularly essential to autocratic regimes (Gehlbach et al., 2016; Svolik, 2012). Autocracies survive by maintaining control of the populace, without potentially useful democratic means of preference aggregation, like elections, and credible commitment, like independent judiciaries (Svolik, 2012). A subset of this literature has theorized and documented how autocracies use policy to maintain control. One strategy is violence: governments can exile or kill opposition to secure control (Gregory et al., 2011). However, repression has potentially large downsides, like increasing the risk of a military coup (Acemoglu et al., 2010; Svolik, 2013) or increasing the signalling value of protests that do take place (Kricheli et al., 2011). Another strategy is information manipulation: regimes can change information content or access to influence citizen beliefs (Gehlbach

et al., 2016; Shadmehr and Bernhardt, 2015; Guriev and Treisman, 2015), though governments may have difficulty adapting to rapid changes in information technologies like social media (Qin et al., 2019). Additionally, autocrats can introduce local elections, which improve the selection and performance of local officials, but decrease central control (Martinez-Bravo et al., 2017). I contribute to this literature by documenting that autocracies use targeted state employment to stay in power, with potentially large implications for the economy and society.

The idea that Chinese SOEs are policy tools for social stability has precedent: Bai et al. (2006) hypothesizes that patterns in SOE reform can be partially explained by the government's desire for stability, Leutert (2016) interprets qualitative evidence as consistent with SOE policy burdens, Dong and Putterman (2003) reason that SOE input patterns are consistent with an SOE policy role, and Lin et al. (1998) document policy directives to SOEs relating to stability. A complementary work, Zeng (2017) posits that SOEs are easier to regulate and persist because the government wishes to maintain regulatory control over the economy, and another, Liu (2019) argues that the government also uses SOEs to subsidize upstream sectors, which benefits the rest of the economy. I discuss additional studies of Chinese SOEs in Section 2. My paper is the first in this literature to provide causal empirical evidence of the stabilizing intent behind SOE employment and the first to uncover additional facts consistent with this SOE stability motive.

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 has been economic growth. Deng Xiaoping, paramount leader of China from 1978 to 1989, stated, "According to Marxism, communist society is a society in which there is overwhelming material abundance. Socialism is the first stage of communism; it means expanding the productive forces" (Chang, 1996). In 1987, the Party's motto for the 13th National Congress was "one central task, two basic points"; the central task was economic development (Jiang, 1997). Gao Shangquan, member of the National Consultative Conference from 1998 to 2003, put it thus: "to constantly improve people's living standard... [t]his is the starting point and ultimate objective of all our work" (People's Daily, 2001). Until 2020, China was also one of a few countries, and by far the largest, to maintain a GDP target (Economist, 2016), a symbol of the government's devotion to aggressive economic growth.

SOE reform and the government's stated goal of economic growth appear perfectly aligned. With no further information, one might expect the Chinese government to ardently pursue SOE privatization.[2] The government did appear genuinely committed to SOE reform in its early years. During the 15th Party Congress in 1997, state ownership was downgraded from a "principal" component of the economy to a "pillar" of the econ-

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

[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 (Yao, 2016), banking (Dobson and Kashyap, 2006), trade (Lardy, 1993), and manufacturing (Huang, 2003).

omy, and a push to privatize SOEs began in earnest (Qian, 2000). Then, in 1999, the Communist Party Central Committee announced that SOE reforms would follow the principle of "[g]rasping the large, letting go of the small" (Hsieh and Song, 2015). But reforms stalled in subsequent years. Appendix Figure A.6 vividly demonstrates the deceleration of reform. Urban SOE employment decreased markedly for a few years following 1997, but since 2006 has remained stagnant at approximately 70 million people, comparable to the entire population of France. Why is the Chinese government, preoccupied as it is with economic growth, so reluctant to engage in further SOE rollbacks? This paper argues that SOEs persist because they offer an essential political benefit: social stability.

2.2 Employment as a Stability Policy

Why might SOEs be useful policy instruments despite potentially large efficiency costs? This subsection discusses how state employment offers particular advantages for preserving social stability, and when appropriate, I contrast these properties with those of leading alternative policies available to the Chinese government.

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

Another way to increase the opportunity cost of conflict would be to simply to give transfers to citizens. Depending on how transfers are funded, they could potentially avoid SOE-related distortions. Yet the observed extent of transfer programs in China is dwarfed by the reach of SOE employment. For example, the primary welfare transfer program,

the *Dibao*, reaches only 5.5% of China's population (Gao et al., 2015). Unemployment insurance is paid out to less than 1% of the working population. And relief transfers, which are ad hoc transfers largely directed by local governments, are disbursed to just 1.6% of individuals in the Urban Household Survey (2002-2009). Why doesn't the Chinese government rely more, or rely exclusively, on transfers to ensure social stability?

The first reason is that targeted transfer programs are susceptible to fraud. In one survey of unemployment insurance recipients in Liaoning, 80% of recipients possessed disqualifying alternative sources of income, typically from unreported employment (Vodopivec et al., 2008). Moreover, some evidence suggests that mis-targeted transfers can actually increase social instability. Cameron and Shah (2014) found that a highly mis-targeted transfer program in Indonesia increased protests, economic crimes, and violent crimes. Verifying eligibility is therefore critical, but also difficult: for example, the correct targeting of unemployment-conditional transfers requires the government to know all sources of a person's income. In contrast, verifying compliance with state employment only requires information readily available to SOE managers, like worker attendance and output.

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

More generally, employment programs have demonstrated pacifying effects in other contexts. Heller (2014) finds evidence that summer jobs for youth in the United States decreases participation in violent activity. Blattman and Annan (2016) find that participation

in an employment program in Liberia decreases the likelihood that individuals participate in illicit activities and serve as mercenaries in a local conflict. Employment may prevent conflict participation through several channels: it provides an income; it enters the time constraint; and it may also engender a variety of social and psychological changes. In this vein, recent work suggests that SOE workers have different attitudes toward governance: Chen and Lu (2011) survey middle-class individuals in China regarding their attitudes toward democracy and find that SOE employment is strongly negatively correlated with support for democratization.

State employment also provides the government an alternative to armed force. The Chinese government has used this strategy to quell protests, including the student-led demonstrations in Beijing in the spring of 1989. Recent instability events have also been addressed with police action, including protests against land seizures in Dongzhou in 2004, anti-corruption protests in Guangdong in 2011, and anti-government protests in Hong Kong in 2019 and 2020 (Ma and Cheng, 2019; Wright, 2019). These demonstrate the downsides of armed suppression: political backlash and a lack of long-term effectiveness. The Tiananmen protest led to widespread domestic and international discontent, including sanctions and arms embargoes (Hufbauer et al., 1990). And in both the Dongzhou and Guangdong protests, once the police presence decreased, protests resumed. The Hong Kong protests have not yet been resolved, but China international standing has already suffered as a result (Roantree, 2019).

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

2.3 The Xinjiang Conflict

The central empirical strategy of this paper, described in Section 4, relies on variation in a violent conflict in Xinjiang province. Xinjiang is China's northwestern-most province and borders Mongolia, Russia, Kazakhstan, Kyrgyzstan, Tajikistan, Afghanistan, Pakistan, and India. Approximately half of the province's population is Uyghur, a Turkic ethnic group that primarily practices Islam (The National Bureau of Statistics, 2010b). For the last fifty years, a local Uyghur separatist movement has sought independence from Chinese rule, using a variety of violent and non-violent tactics (Millward, 2004).

The cohesion and intensity of the separatist movement escalated in the 1990s. Qualitative accounts identify three spikes of violence in 1990, 1992-93, and 1996-97 (Davis, 2008; Millward, 2004) The early 2000s were a relatively quiet period for the conflict, with scattered bombings and assassination attempts. Tensions rose again in 2007, after a Chinese police raid on a suspected separatist training camp. In the ensuing years, several attacks took place in the cities of Kashgar, Kuqa, and Urumqi (Guo, 2015).

Primary evidence suggests that the timing and intensity of incidents were largely determined by the strategic considerations of the guerrilla forces and violent escalations of gatherings formed around local events, like mosque closures (Millward, 2004). Some violent incidents were triggered by economic phenomena, like firm layoffs. An even smaller proportion of violent incidents were explicit responses to events outside of Xinjiang, like a factory fight between Han and Uyghur workers in Guangdong Province, or Deng Xiaoping's funeral (Bovingdon, 2010).

3 Conceptual Framework

While the qualitative evidence presented in Section 2 is consistent with an SOE stability motive, it is not definitive proof thereof. Government rhetoric may or may not be backed by real policy behavior, and theoretically useful tools may never be implemented in practice. To test this idea more rigorously, I develop a model of SOE stabilization to generate

empirically-testable predictions indicative of stabilizing intent. The model reveals how labor market outcomes should respond if a government were using employment to maintain stability in the face of unrest shocks. I describe key model dynamics and predictions in this section and present the full model in Appendix Subsection 9.1. Afterward, I empirically test each prediction in Sections 4 - 6.

3.1 Setup

This model consists of individuals, firms, and a government. There are two types of individuals: a non-unrest type and an unrest type, who both value consumption and leisure and 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 $j \in (U,N)$, this equation holds: $\frac{u_\ell\left(\ell^{j*},c^{j*}\right)}{u_c(\ell^{j*},c^{j*})} = w_j$. The key difference among types is that unrest-type leisure time generates unrest activities that the government dislikes.

There are also two types of firms: private firms and SOEs. Both types convert inputs into output using the same constant returns to scale production function[3], but are subjected to different government policies. In particular, the government taxes all non-unrest type labor in the economy at rate τ_N, and then provides a subsidy to SOE unrest-type hiring in the form of subsidy τ_U. 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 instability shock, $\xi \in \mathbb{R}^+$, and the leisure of unrest-types, ℓ^U. The government maximizes this multidimensional objective conditional on its budget constraint.

[3] I assume that these production functions satisfy Inada conditions.

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

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

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

3.2 Comparative Statics

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

The intuitive relationship among these four propositions is straightforward. τ_U governs the relative prices of U-type labor and N-type labor: when it becomes more positive (representing a larger subsidy), U-type labor becomes relatively cheaper for both firms. This price change elicits Proposition 1: the entire market will use more U-type labor and less N-type labor. Additionally, because these changes emerge from labor demand, they generate shifts along the labor supply curve, resulting in wages that move in the same direction as quantity, yielding Proposition 2: w_U will increase, while w_N decreases.

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

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

Recall the government's first order condition: $\frac{dY}{d\tau_U} + \eta\xi\frac{dS}{dZ}\frac{dz}{dU}\frac{dU}{d\tau_U} = 0$. The first term is negative and captures the cost of the subsidy τ_U to output, whereas the second term is positive and expresses the subsidy's stability benefit. What happens to the government's first order condition when ξ increases? 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 empirically testable predictions. If $\eta > 0$:

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

3.3 Sufficient Statistic

One advantage of the model is that it generates an empirically-observable sufficient statistic for the male minority SOE wage subsidy. When I assume that the production function F

takes the following Cobb-Douglas form, such that $F = U^{\alpha} N^{1-\alpha}$, the first order conditions of the SOE and private firms become:

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

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

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

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

The last term of this equation can be obtained directly from employment data, which I do so in Subsection 6.4. I then use this value to compute τ_U, the implicit wage subsidy that SOEs receive to hire male minority employees.

4 Empirical Strategy

To test the model's implications, I use a natural experiment that captures how a regional ethnic conflict generates threats elsewhere. The threat of ethnic unrest corresponds to ξ, the instability shock. This natural experiment leverages variation in Uyghur ethnic unrest to produce causal estimates of model predictions. This conflict is endemic to Xinjiang, the westernmost province of China, where some residents seek independence, motivated by widespread discrimination and oppression. This section describes the construction of the Uyghur unrest shock. In Section 6, I present baseline results and robustness checks.

One potential objection to evidence based on the Uyghur ethnic unrest is the heightened sensitivity of this issue in Chinese politics; any observed responses may be unique. To explore this possibility, I present new and complementary facts in Section 7 that demonstrate the stability motive for SOE employment exists even outside this particular context.

4.1 Uyghur Unrest Shock

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

The first component of the shock is $I_{t-1}^{XJ=1}$, an annual measure that captures the number of conflict incidents in Xinjiang in the previous year. I interpret the number of conflict incidents per year as a measure of the intensity of the conflict, so that variation in the incident count reflects variation in the underlying conflict intensity. For the baseline specification, I lag this variable by one year to reflect the fact that employment may be sticky, and thus a fairly slow-moving policy instrument. I consider alternative lags and intensity measures as robustness checks.

The second component of the shock is the share of each Chinese county's population that is ethnically Uyghur, $U_{c,t=2000}^{XJ=0}$, as measured in China's 2000 Census, omitting all Xinjiang counties. For the entire analysis, I will use variation in conflict inside Xinjiang to generate variation in the propensity for conflict to spill over to counties outside of Xinjiang. This choice is critical for the satisfaction of the exclusion restriction, because changes in the intensity of the Xinjiang conflict may respond locally to my outcome variables of SOE employment, private employment, and wages. Even though the response would need to vary heterogeneously with the other components of the shock in order to generate spurious results, the observable and unobservable channels through which local economic factors might generate conflict are manifold. I cut this Gordian Knot by constructing the shock only for non-Xinjiang locations.

Of course, the distribution of Uyghur populations outside of Xinjiang in 2000 is not

random. One threat to my identification strategy is that some driver of Uyghur settlement patterns also influences employment and wages during my time period of study, 2002-2009, in a way that is correlated with the intensity of the Xinjiang conflict and, for the triple difference, also differentially affects male minorities. I turn to the ethnographic and historical literature to understand patterns of Uyghur settlement in China. The literature suggests that settlement patterns are generated by a combination of forces. Historical forces include Ming-dynasty military dispatches (Svanberg, 1988) and eighteenth century pilgrimages (Coughlin, 2006). More recent forces include local demand for service jobs (Brophy, 2016; Iredale et al., 2015). The latter clearly have the potential to generate employment and wage responses, even though it is difficult to imagine why those responses would be correlated temporally with the Xinjiang conflict or, in the triple differences specification, why those forces would differentially affect male minorities. Nonetheless, to address this source of possible confounders, I flexibly control for pre-period labor market conditions in the baseline specification. I describe these controls in Subsection 4.2.

At this point, this difference-in-differences measure can be written as an interaction variable $DD_{ct} = I_{t-1}^{XJ=1} \times U_{c,t=2000}^{XJ=0}$. In this expression, c indexes counties and t indexes years. I argue that this object is a measure of the underlying propensity for the Uyghur conflict to spill over into county c during year t: its value is largest in years with many conflict incidents in Xinjiang the year before and in counties with the highest density of Uyghur residents.

Specifically, the relevance assumption required for this differences-in-differences shock is that conflict propagation is particularly likely during times of high conflict intensity in Xinjiang in counties with a large share of Uyghur residents in 2000. An inter-disciplinary literature on the propagation of social conflict supports this assumption. Forsberg (2014) and Forsberg (2008) document this pattern of contagion in ethnic conflict in the interstate context, where ethnic conflicts are more likely to spill over into places with higher shares of the aggrieved group(s) and during times where the conflict is most severe. Moreover, Buhaug and Gleditsch (2008) find that spatial and temporal correlations in intrastate con-

flict can be explained by ethnic ties among separatist conflicts. Cederman et al. (2009) provide correlational evidence that ethnic networks across state boundaries can facilitate the incidence of intrastate conflict. There is evidence that this pattern of conflict spillover is present within the Xinjiang conflict as well. In December 1985, Uyghur students demonstrated in Beijing against recent nuclear testing in Lop Nor (Toops, 2009); Beijing is home to one of the largest Uyghur diaspora communities in China.

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

At this stage, consider a regression of a labor market outcome, like SOE employment, on the interaction variable proposed in expression $DD_{ct} = I_{t-1}^{XJ=1} \times U_{c,t=2000}^{XJ=0}$ and other controls. Such a specification could produce spurious results if the county-year interaction variable were correlated with some omitted determinant of the Chinese labor market. During my time period of study, 2002-2009, the Chinese economy underwent dramatic changes that very well could have produced such an omitted variable, including the SOE ownership reforms of the 90's and 00's, the 2001 accession to the World Trade organization, and the fiscal stimulus response to the 2008 global financial crises. To explicitly control for all such changes would be difficult and potentially unconvincing.

Instead, I introduce a third comparison to my causal identification strategy: I compare the shock response of male minorities to that of everyone else. Male minorities are the demographic most likely to participate in ethnic unrest in China and their status is easily observable, so a government with a limited budget should and could target that group with stability policies during ethnic unrest shocks. Moreover, because all workers, not just male

minorities, are subject to the broad-based economic changes listed above, the differential response of male minorities will reveal the causal employment response of SOEs and private firms to the Uyghur unrest shock.

Qualitative and quantitative evidence support this approach. Anthropological work on the Xinjiang conflict suggests that a very large majority of insurgents are male, and nearly all are Uyghur (Bovingdon, 2004). I corroborate this observation using data from the United States Congressional-Executive Committee on China, which maintains a data set of all known Chinese political prisoners. A comparison of the demographics of those prisoners with the general Chinese population in Figure 1a reveals that male minorities are a disproportionately large share of political dissidents in China. This prevalence accords with the general sociological and criminological finding that men tend to participate in violence at much higher rates than women (Heidensohn and Gelsthorpe, 2002; Lauritsen et al., 2009).

The Chinese government is well aware of the demographics of the Xinjiang conflict, so any resource-constrained stability policies are likely to target the high-risk group: male Uyghurs. The reason I use an indicator variable for male minorities, rather than male Uyghurs, is due to data limitations: in the Urban Household Survey, my primary data source, the finest level of information on the ethnicity of respondents is whether they are Han or a minority. I discuss this data source in detail in Subsection 5.3. While the minority indicator is an imperfect proxy for Uyghur ethnicity, Uyghurs represent 8.4% of all minorities in provinces outside of Xinjiang (The National Bureau of Statistics, 2010a), a non-trivial share.

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

The exclusion restriction for this triple differences setup is substantially more difficult to violate. A spurious result can only be generated by some force that co-varies temporally with the number of Xinjiang incidents, co-varies geographically with Uyghur population

density, and furthermore, differentially affects male minorities. The model's three directional prections on SOE employment, private employment, and salaries further decreases the possibility that an omitted variable could reject the null. Though it is difficult to identify concrete phenomena that would satisfy these criteria, I nonetheless consider and control for potential sources of omitted variables in Subsection 6.1.

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

4.2 Baseline Specification

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

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

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

There will be 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. Similarly, another takes a value of 1 if an individual is employed in a privately-owned economic unit, and zero otherwise. The last outcome will be individual salary, measured as the continuous nominal value of

employment income in thousands of yuan. This variable is not defined for non-employed individuals.

In this specification, I assume that Y_{ict} is a function of a triple interaction between lagged violent incidents in Xinjiang, $I_{t-1}^{XJ=1}$, 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. This indicator takes a value of 1 for male minorities and takes a value of 0 for everybody else, including female minorities. Several of the triple interaction terms are absorbed by fixed effects, which I describe below.

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

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

The county and male minority fixed effects, $\eta_c \times M_i$, absorb any time-invariant differences in the labor composition of counties for male minorities and non-male minorities.

For example, if private firms in some counties were consistently less likely to hire male minorities over the entire time period, this fixed effect would absorb that potentially confounding variation. Finally, I cluster standard errors at the county level to account for the shock's level of geographic variation.[4]

These empirical objects correspond to theoretical objects in the model. The Uyghur unrest shock maps onto ξ, the model's unrest shock. The differential response of male minority labor outcomes can be interpreted as a causal estimate of the response of L to ξ. I can therefore rewrite the theoretical predictions in Section 3 in terms of real-world phenomena. I indicate the outcome variable of the regression as a superscript: for example, β_M^{PRIV} refers to the coefficient β_M estimated from the regression of $Private_{ict}$ on the baseline specification.

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

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

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

5 Data

5.1 Uyghur Unrest Incidents

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

I construct a time series of separatist unrest in Xinjiang using multiple primary and secondary historical sources. First, I conduct a systematic search of historical newspaper

[4] As a robustness check, I present standard errors with two-way clustering at the county and year level. However, I do not use this level of clustering for the baseline as the panel is only 8 years long.

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

The time series of Xinjiang conflict events for sample years are plotted in Figure 2. The baseline measure of Xinjiang violence intensity is a simple count of events in each year, regardless of the number of perpetrators or victims. I to use incident count instead of fatalities as the latter are more prone to strategic manipulation and reporting error. Whether an incident occurs at all is both easier to measure and more difficult to manipulate.

5.2 County Uyghur Shares

The second component of the Uyghur unrest shock is a cross-sectional measure of the share of the county population that is Uyghur. I use data on county population by ethnicity in the 2000 Population Census of China (The National Bureau of Statistics, 2010a) and divide the number of Uyghur individuals by the total population of the county. I use the Census of 2000 rather than more recent data because 2000 predates the coverage of the baseline sample, thus removing some of the potential endogeneity in Uyghur population distribution that might arise from the migration of Uyghur peoples in response to unobserved factors, like friendly local policies. Figure 3 presents a choropleth map of county-level Uyghur population shares outside of Xinjiang. Counties with high Uyghur shares are spread fairly evenly throughout China, though larger cities, like Beijing and Shanghai, as well as remote

Western counties, tend to be home to a denser concentration of Uyghur people. It is not the case that Uyghur residency patterns outside Xinjiang are concentrated in one province or geographic region of China, which permits a wide array of geographic fixed effects.

In addition to these data sources, I draw from a number of observational data sets on China to measure variables of interest.

5.3 Urban Household Survey

Outcome variables and individual-level controls come from the *Urban Household Survey* (UHS). These data are collected by the National Bureau of Statistics, and I use data from the years 2002 to 2009.[5] The sampling procedure for households is stratified at several levels, including the province, city, county, township, and neighborhood. The data set has a rotating panel structure such that selected households remain in the survey for three years before exiting. Households are legally obligated to respond, and illegal city residents are protected by law from prosecution based on this survey, though these households are likely underrepresented due to worse documentation and the perceived risks of responding.

The UHS data set includes a rich set of variables describing household composition, age, gender, ethnicity, employment, and education. It also records exceptionally detailed information on household income and consumption. Critically for this project, the "employment situation" variable contains information about the ownership of the employee's workplace and distinguishes between state-owned units, urban collective units, joint-stock and foreign units, township private enterprises, and urban private enterprises. This ownership information is crucial to the empirical tests presented in this paper. For the analyses below, I define SOE employment as the employees of state-owned units and urban collective units, as there is a literature documenting how collective firms in China exhibit similarities to SOEs (Brandt and Rawski, 2008). However, in Appendix Subsection 9.2, I explore how the results change if SOEs are defined as state-owned units only.

[5] I cannot use earlier available years from 1992 through 2001 as they do not include the minority status of respondents.

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

5.3.1 Demographics of Unrest and State Employment

In China, the demographics of unrest participation differ from those of the general population, which I illustrate by comparing the demographic composition of China's total population from the 2000 Census (The National Bureau of Statistics, 2010a) with information from a dataset of all known Chinese political prisoners (Congressional-Executive Commission on China, 2019).[6] Figure 1a demonstrates that men comprise over 70% of unrest participants in China and just 51% of the general population. Minority men are even more dramatically over-represented among political prisoners: they comprise over 45% of unrest participants but just 4% of the general population.

SOEs also hire more of these two demographics. On the left-hand chart in Figure 1b, I plot the average share of men in private firms versus SOEs from the UHS data: SOEs hire disproportionately more men than do private firms, and the difference is precise at the $p < 0.01$ level. Similarly, in the right-hand chart of Figure 1b, I find that SOEs hire a greater proportion of male minorities than private firms, and that this difference is precise at the $p < 0.01$ level.

6 Results

Table 1 presents results from estimating Equation (5) as a linear probability regression. The three outcome variables in this table are SOE employment, private employment, and salary; the coefficients from columns (1), (2), and (3) correspond to the predictions in Equations

[6] These data are collected by the United States Congressional-Executive Committee on China in conjunction with U.S. intelligence forces and contain the name, gender, ethnicity, and age of political prisoners in China.

(6), (7), and (8).

Prediction 1 states that the coefficient in column (1) should be positive, and it is, taking a value of 36.59 and differing from from zero at the $p < 0.01$ level. SOEs increase male minority employment in response to an increase in the threat of Uyghur unrest. Prediction 2 states that the coefficient column (2) should be negative, and I find that the magnitude of β_M^{PRIV} is -24.24 and different from zero with $p < 0.05$. Finally, Prediction 3 was that the coefficient in column (3) should be positive. The true coefficient is $5,422$ with $p < 0.01$, in accordance with the prediction.

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

I re-estimate a version of the baseline equation presented in Equation (9), that produces year-by-year estimates for the coefficient on $U_{c,t=2000}^{XJ=0} \times M_i$ and $U_{c,t=2000}^{XJ=0}$. In Figure 4, I plot the coefficients β_{Mt} alongside the time series of lagged Xinjiang incidents over time, which visually presents the correlation underlying the triple difference coefficient. The year-by-year coefficients on the interacted county Uyghur share and male minority term, plotted with the solid red line, co-move with the number of lagged Xinjiang incidents every year, plotted with the solid blue line.

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

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

6.1 Robustness Checks

One source of omitted variables in Table 1 would be alternative determinants of employment and wages that are correlated with the temporal variation in Xinjiang incidents, correlated geographically with the distribution of high-Uyghur share counties, and that differentially impact minority men relative to non-minority men. In particular, the literature suggests that, in addition to the provision of social stability, SOEs are also used to retain control over strategic sectors, like utilities and mining, or to maintain a large administrative capacity (Leutert, 2016). In order to test whether my main results are generated by motives temporally correlated with the Uyghur unrest shock, I conduct a set of robustness checks.

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

Table 2 reports this set of robustness checks for employment by ownership. I find that simultaneously controlling for these flexible interactions does little to change the magnitudes and precision of the baseline estimates. I also perform a complementary robustness

check by dropping public services workers, mining workers, and utilities workers from the sample and re-running the baseline regression. The results, reported in Appendix Table A.10, remain similar in sign and magnitude to those of the baseline.

Another potential source of endogeneity is Xinjiang unrest incidents triggered by events outside Xinjiang. If those outside events were in turn correlated with local economic conditions, then my estimates could potentially be ascribing labor market variation due to local conditions to variation arising from the unrest spillover propensity. To address this concern, I hand-code the inciting reason for each event in my database of Xinjiang unrest using primary evidence. I then drop every event whose trigger came from outside Xinjiang. One example of Xinjiang unrest triggered by outside events is a series of bombings in Urumqi that coincided with Deng Xiaoping's funeral in February of 1997. Rebel groups timed the attacks to publicize the struggle of the Uyghur people against the Chinese government (Steele and Kuo, 2007). Table 3 reports estimates using this amended Xinjiang incident time series as $I_{t-1}^{XJ=1}$. The baseline results hold when using this alternative time series.

Another potential source of endogeneity would be if Xinjiang unrest incidents were triggered by Xinjiang economic conditions, which in turn were correlated with the economic conditions of counties across China. To address this possibility, I construct an incident time series that removes all events sparked by economic issues. For example, I remove a series of protests that occurred in the city of Hotan in October 2001. There, workers were protesting local factory closures. Table 4 reports estimates that use this alternate series. I find that the main results are all corroborated.

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

One property of this empirical context is that the distribution of Uyghur population shares is not normal, as Figure 3 demonstrates. Thus, I should be particularly concerned that certain values, potentially mis-measured, are generating a spurious result. I run several robustness checks that explicitly address this concern. First, I perform a random permutation test on the Uyghur share variable. For this test, I run the baseline regression for the

SOE outcome variable 500 times, but each time, I randomly assign each county a Uyghur share value drawn from the observed distribution of Uyghur values in the data. In other words, for each of the 500 iterations, I generate a counterfactual Uyghur share map for China that follows the same distribution as the true map. Then, I plot a histogram of the coefficient β_M for each of these 500 iterations in Figure 5. I find that only 5.1% of these counterfactual coefficients have a value higher than the true estimate of 36.59. This distribution of counterfactual estimates increases my confidence that the baseline estimates could not be generated by a random assignment of county Uyghur share values.

I also test whether the baseline results are sensitive to the removal of outliers in Online Appendix Table A.18. To identify outliers, I compute DFITS for each observation (Langford and Lewis, 1998) and drop all observations with DFITS greater than $2\sqrt{k/N}$, where k is the number of regressors and N is the number of observations. The SOE and salary results are robust to this procedure, and the private employment result remains negative but is no longer precisely different from zero.

In Table 5, I conduct a placebo test. Instead of using lagged Xinjiang incidents in the shock, I use instead the lead of Xinjiang incidents. Theoretically, SOE employment should not respond to incidents in the future. The estimates in this table are consistent with this reasoning. The coefficients β_M are small in magnitude and not precisely different from zero for all three outcome variables. Furthermore, in Appendix Table A.15, I split the baseline sample by gender to form a different kind of placebo test: the stability employment response should be much weaker or nonexistient among female minorities, who are much less likely to participate in unrest. Accordingly, I find that male minorities drive the entire documented response.

6.2 Heterogeneity by Sector

Are there sectors in which the SOE stability response is more pronounced? To answer this question, I construct a variable that records the sector of employment for each individual using the UHS sector variable. I consolidate the twenty raw sector categories into

six groups: agriculture, manufacturing, mining and construction, retail and transportation, services, and Communist Party work. Urban agriculture is rare and there is no variation in firm ownership in Party work, so I drop the first and last categories. I then run the baseline specification separately for SOE employment and private employment for the remaining four sectors. Because the sector of employment is only defined for employed individuals, the SOE and private coefficients are inverses of each other. I report both regressions for each sector for completeness.

Appendix Table A.9 reports estimates from the remaining sectors: manufacturing, mining and construction, retail and transportation, services. The services sector is the only one that displays a precise and positive SOE employment response to the Uyghur unrest shock, and the response only takes place for male minorities. The coefficient of 62.02 is precisely different from zero at the $p < 0.01$ level. Due to the large standard errors belonging to the β_M coefficient for each of the other sectors, the services sector response is not significantly different from the others.

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

6.2.1 Response Over Time

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

variable.

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

The variable $Inci_{t-j}^{XJ=1}$ 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. I estimate Equation (10) for the outcomes of SOE employment, private employment, non-employment, and salary. I plot the regression coefficients $< \beta_{M1}, ..., \beta_{M5} >$ in Appendix Figure A.10.

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

The response of private employment 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 then reverts in the fifth year. Finally, average 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.

Overall, Appendix Figure A.10 suggests that male minority employment and wages display the largest responses to unrest immediately after the incidents take place and then slowly converge with those of everyone else over time. During the convergence process, there even appears to be a slight reversal of the initial shock response around year four, but the magnitude of the correction is not large enough to swamp the initial changes.

6.3 Complementary Policies

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

I re-estimate Equation (5) using social relief transfers as the outcome variable. To further enrich the analysis, I repeat the regression for four samples: the full baseline sample, SOE employees only, private employees only, and individuals who are not employed. Results from these regressions are reported in Table 6. In Column (1), I find that in response to the shock, average social relief transfers to male minorities differentially increase by $17,507$ yuan, and the change is precisely different from zero at the $p < 0.01$ level. This column suggests that the government complements its employment stability policies with targeted relief transfers. For a county outside Xinjiang with an average level of Uyghur share, the magnitude of this estimate implies that individuals will receive 3.19 yuan more in a year with 75th percentile incident counts in Xinjiang compared to year with 25th percentile Xinjiang incident counts. Though this amount appears small, only 1.43% of the population receives any relief transfers. Scaling by the proportion of non-zero values (and

assuming no movement on the extensive margin), the magnitudes imply an increase of 222.94 yuan among relief transfer recipients.

In Columns (2)-(4), I subdivide the response of relief transfers by employment status: SOE, private, or non-employed. I find that, while the point estimate for the male minority interaction is positive in all columns, the magnitude is only precise for SOE employees and non-employed individuals. Moreover, the transfer response for non-employed male minorities is over ten times as large as those of the employed workers and precisely different from the response for both SOE and private workers. These columns suggest that the relief transfers are targeted on the population of male minorities not reached by the SOE employment expansion: the non-employed.

6.4 Sufficient statistic

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

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

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

7 Evidence of Generality: Exports and Floods

In this section, I present new facts suggesting that the SOE stability role is not just 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 in the form of river floods, 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 state enterprise's stabilizing role.

7.1 Export Demand

In general, profit-maximizing firms should decrease both output and inputs, including employment, when demand falls. In this section, I show that when demand for Chinese exports falls, private firms shed labor as expected, yet SOEs hire more. I construct a measure of demand for Chinese exports based on the setup used in Autor et al. (2013).[7] The annual provincial demand shock exposure, $\Delta DSEIV_{pt}$, has two components: a weight variable and a trade flow variable.

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

The letter s indexes sectors. Provinces are indexed with p and years are indexed with t. The weight variable, $\frac{X_{spt-1}}{X_{st-1}}$, equals the ratio of exports from a given sector, year, and province to all exports out of China from that sector and year. Provinces that export more will thus receive a higher weight. The trade flow variable ΔE_{st}^{ab} represents the net exports (exports minus imports) into China's trading partner $a \in A$ from the partner's own largest trading partners, $b \in B$. A is the set of China's five largest trading partners in 2004 and B is the set of each partner a's five largest trading partners in 2004, excluding China.[8] This setup avoids using flows that directly involve China itself, which are likely influenced by China's domestic situation.[9] The geographic variation in equation (11) arises entirely from

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

[8] 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 the ranking of trading partners in alternative years.

[9] I obtain changes in net export flows ΔE_{st}^{ab} from the *United Nations Comtrade Database* (UN Comtrade) (United Nations, 2016). I construct the weight variable $\frac{Y_{spt-1}}{Y_{st-1}}$ using Chinese data from the *Annual Surveys of Industrial Production* (ASIP), which I describe in detail in Online Appendix Subsection 9.3. The UN

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

I use the following regression to uncover the response of employment to the trade shock.

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

In this equation, i indexes individuals, p indexes provinces, c indexes counties, and t indexes years. I estimate this regression using on the Urban Household Survey. The two dependent variables, Y_{ict}, are indicator variables for whether an individual works for an SOE or a private firm, respectively. This specification includes year fixed effects τ_t, county fixed effects η_c, and individual characteristics: age, a fixed effect for education level, as well as age and education interacted with gender. Because the demand shock varies at the province and year level, I cluster standard errors at the province and year level.

Column (1) of Table 7 shows that SOE employment responds inversely to trade demand. The coefficient is -0.0529 and is precise at the $p < 0.01$ level. On the other hand, column (2) shows that private firms respond procyclically to trade demand, with a coefficient of 0.0546, precise at the $p < 0.05$ level. These results suggest that SOEs are behaving in a way that does not maximize profits, but instead provides employment security during downturns.

However, there are some caveats to this analysis. SOEs may be concentrated in sectors that are differentially exposed to trade. As a robustness check, I control for base-year sector composition by county interacted with year fixed effects and report the results in Appendix Table A.16. 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 Appendix Table A.16. To further increase confidence that

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.

these results are not elicited by spurious trends, I re-estimate Equation (12) using the lead of the export demand shock. I argue that it is less likely that employment should respond to future demand changes. The results from these regressions are reported in Online Appendix Table A.19 - both coefficients of interest are not statistically different from zero.

7.2 Flood Disasters

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

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

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

Data on riverine flooding come from the Dartmouth Flood Observatory's *Global Active Archive of Large Flood Events* (Brakenridge, 2019). The flood data cover the years 1990 to 2017 and include the latitude and longitude of each flood's centroid, from which I generate a county-level riverine flooding indicator, $Flood_{ct-1}$, that equals one if the county geographic centroid is within 50 kilometers of the centroid of a recorded flood in the past

year. For the period 1990-2017, 889 county-years are defined to suffer riverine flooding according to my definition, about 1.1% of all county-years. I use the flood indicator in year $t-1$ because I assume that employment is somewhat sticky. I cluster the standard errors at the county and year level, which is the level at which floods vary.

Table 8 shows SOE employment increases in the year after floods: the coefficient in column (1) is 0.0778 and precise at the $p < 0.05$ level. On the other hand, column (2) shows that private employment falls after flood disasters, with a coefficient of -0.093, precise at the $p < 0.01$ level.

There may be omitted variables that co-vary with both county-year flood incidence and employment by ownership. To address some concerns, I control for the base year sector share of each county interacted with year fixed effects and report results in Appendix Table A.17. I also conduct a placebo check by re-estimating Equation (13) using the lead of the flood indicator variable. The results from these regressions are reported in Online Appendix Table A.20, and reassuringly, employment composition by ownership does not respond to future floods.

8 Conclusion

This paper documents how the Chinese government uses SOEs not only as units of production but also as policy instruments for maintaining social stability. This fact provides one political economy explanation for the persistence of state-owned enterprises in China, and consequently, a downward force on productivity in a major world economy.

The central empirical test in this paper uses a triple-differences approach to document the response of state employment to ethnic unrest threats. The unrest shock combines annual variation in Xinjiang conflict intensity, county-level variation in Uyghur population densities, and individual-level variation in whether individuals are male minorities. In response to these threats, SOEs increase their employment of minority men and private firms shed employment from the same group. I find that salaries increase, but only for male

minorities, suggesting that the observed patterns result from increasing SOE labor demand rather than falling private labor demand. This entire suite of results is consistent with a theoretical framework wherein the government subsidizes state firms to boost employment of certain demographics, using employment to depress the likelihood of unrest.

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

Table 1: The Effect of Unrest Threat on Employment and Salary

	(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. × 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
SUR p-value:	(1) vs. (2) <0.000		

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

Table 2: The Effect of Unrest Threat on Employment and Salary — Robustness to Initial County Strategic Sector Employment Shares

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

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

Table 3: The Effect of Unrest Threat on Employment and Salary — Omit Incidents Triggered by Events Outside Xinjiang

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

Notes: Observations are at the individual level. All regressions control for year fixed effects; county times male minority fixed effects; log kilometers county distance from Xinjiang times year fixed effects; the average base period county employment share by ownership times year and county fixed effects; age, gender, years of education; and these three controls interacted with county Uyghur share and lag Xinjiang incidents (without outside-triggered 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 and Salary — Omit Incidents Triggered by Economic Events

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

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 (without economically-triggered incidents). Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table 5: The Effect of Unrest Threat on Employment and Salary — Placebo Using Lead of Shock

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

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

Table 6: The Effect of Unrest Threat on Social Relief Transfers

	(1)	(2)	(3)	(4)
Dependent Variable: Employment	Social Relief Transfers (RMB)			
Sample:	All	SOE	Private	Not Empl.
Mean of Dependent Variable	*18.57*	*1.510*	*1.690*	*1.790*
Percent Non-Zero Observations	*1.43%*	*0.65%*	*1.92%*	*2.98%*
Cty. Uyg. Share × Lag Xinjiang Incid.	17,507***	6,419**	7,701	88,221**
× Male Minority	(4,703)	(3,042)	(5,733)	(35,632)
Observations	224,412	123,828	55,907	44,677
R-squared	0.017	0.023	0.049	0.045
SUR p-values:		(2) vs. (3)	(2) vs. (4)	(3) vs. (4)
		0.572	0.0211	0.023
Magnitudes (RMB):				
(β) × (Incid. P75-P25). × Uyg. Share mean	3.187	1.251	1.345	13.72
Magnitude ÷ Percent Non-Zero Observations	222.94	193.39	70.01	459.84

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

Table 7: The Effect of Export Demand Shocks on Employment

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

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

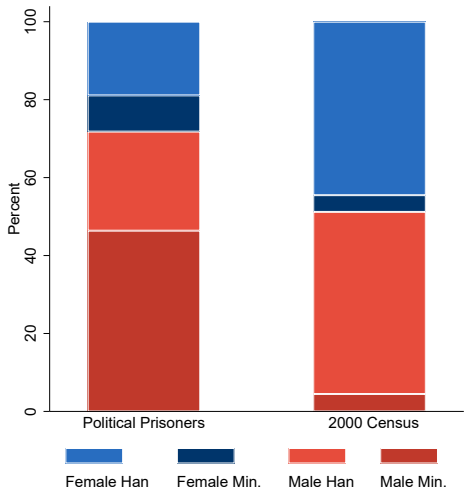
Table 8: The Effect of Flood Disasters on Employment

	(1)	(2)
Dependent Variable: Employment	SOE	Private
Mean of Dependent Variable	*0.550*	*0.250*
Lag County Flood Indicator	0.0778**	-0.0930***
	(0.0361)	(0.0318)
Observations	225,039	225,039
R-squared	0.248	0.166
SUR p-value (1) vs. (2):	0.008	

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

Figure 1: 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 t-test p-value: 0.00. Male min t-test p-value: 0.00.
Source: Urban Household Survey 2002-2009

Figure 2: Timeline of Xinjiang Unrest Incidents

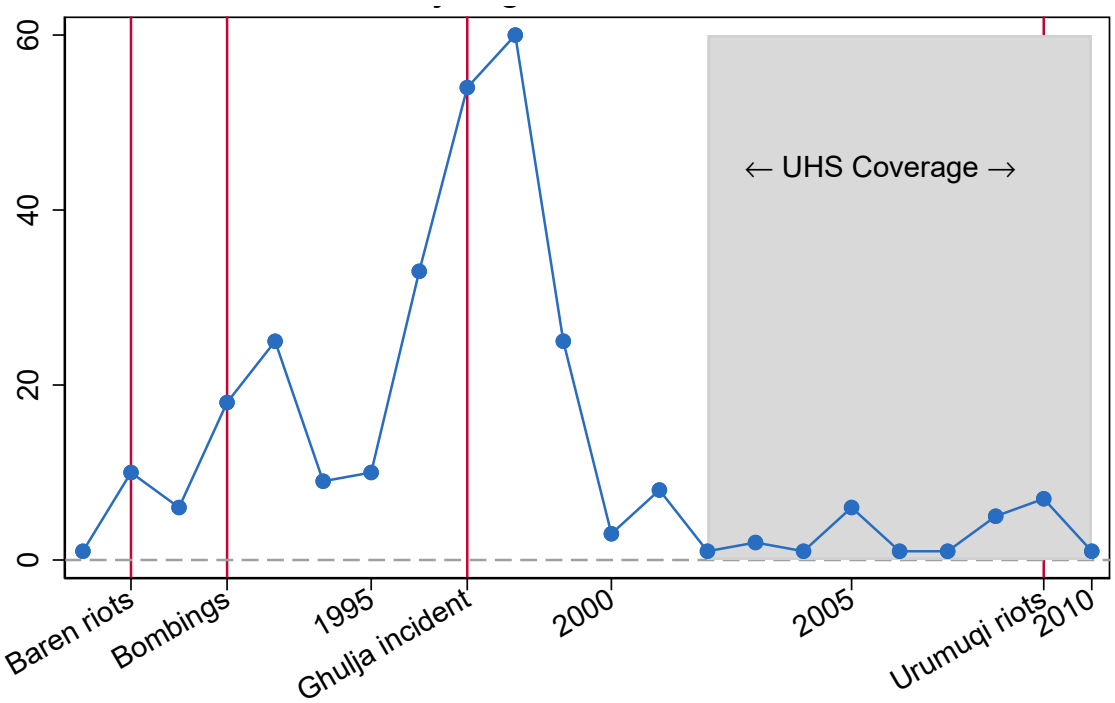
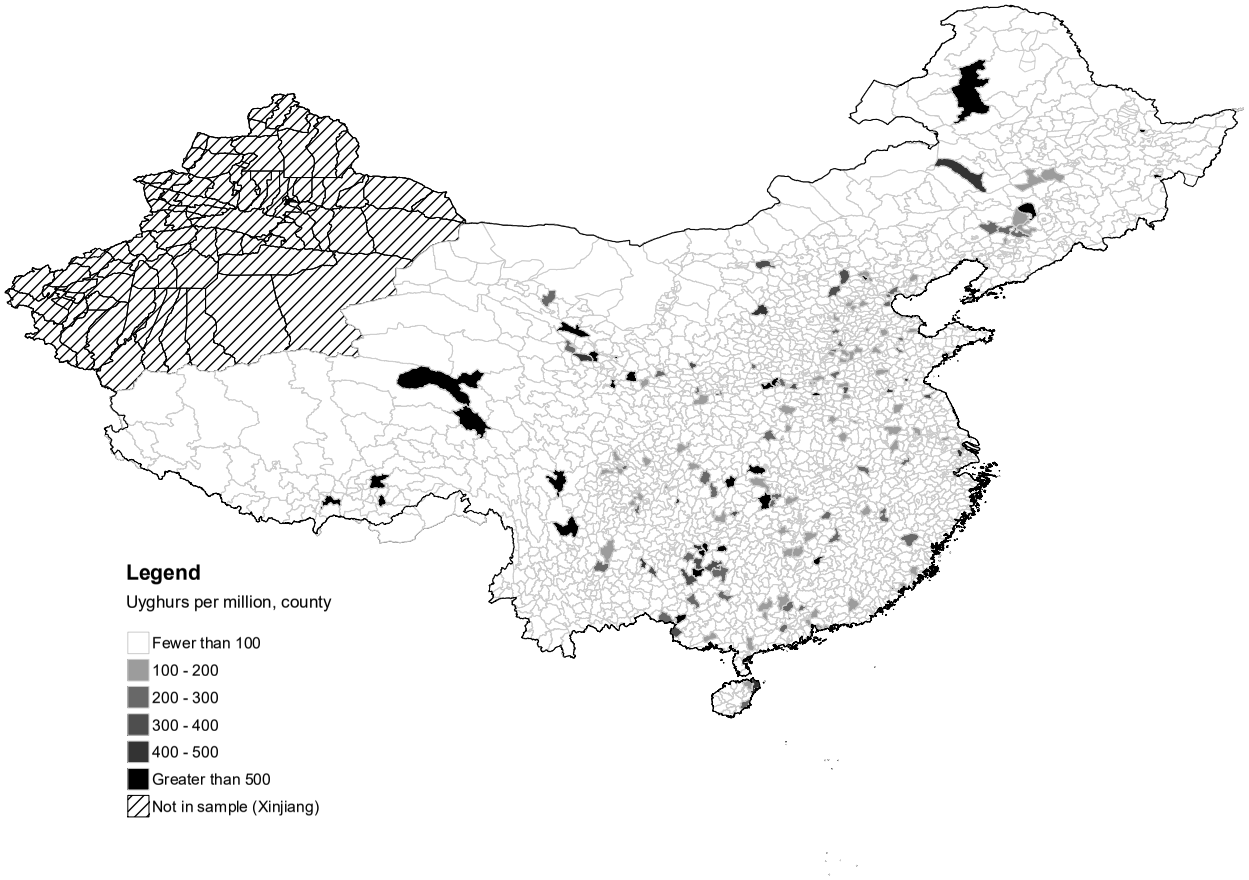
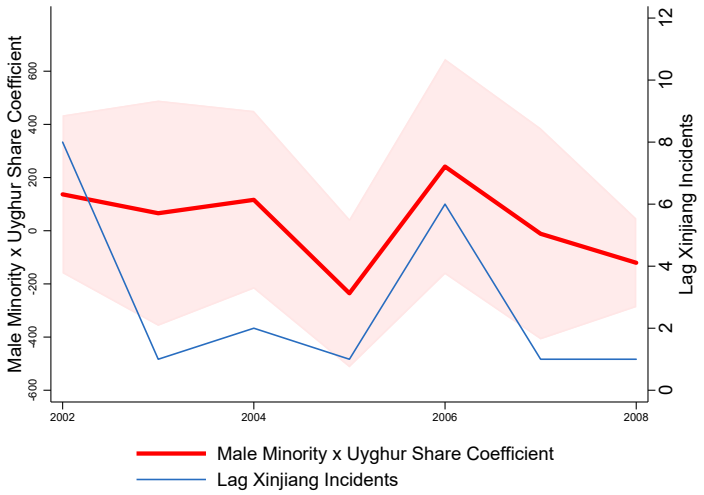


Figure 3: Choropleth of County Uyghur Share Outside Xinjiang



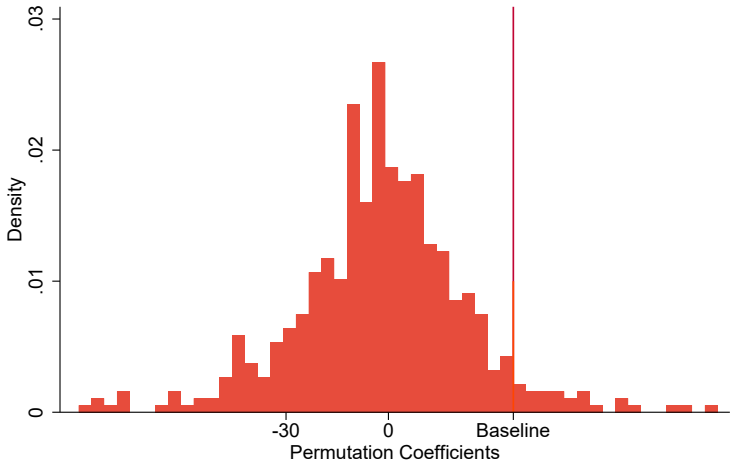
Notes: Data from 2000 Census of China.

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



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

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



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

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