The Political Economy Consequences of China's Export Slowdown*

Filipe R. Campante
Johns Hopkins
and NBER

Davin Chor
Dartmouth, NUS
and NBER

Bingjing Li
NUS

May 2019

Abstract

We study how adverse economic shocks influence political outcomes in authoritarian regimes in strong states, by examining the export slowdown in China between 2013-2015. We exploit detailed customs data and the variation they reveal about Chinese prefectures' underlying exposure to the global trade slowdown, in order to implement a shift-share instrumental variables strategy. Prefectures that experienced a more severe export slowdown witnessed a significant increase in incidents of labor strikes. This was accompanied by a heightened emphasis in such prefectures on upholding domestic stability, as evidenced from: (i) textual analysis measures we constructed from official annual work reports using machine-learning algorithms; and (ii) data we gathered on local fiscal expenditures channelled towards public security uses and social spending. The central government was subsequently more likely to replace the party secretary in prefectures that saw a high level of "excess strikes", above what could be predicted from the observed export slowdown, suggesting that local leaders were held to account on yardsticks related to political stability.

Keywords: Economic shocks; labor unrest; Chinese politics; political stability; authoritarian regimes; strong states; export slowdown; shift-share instruments.

JEL codes: D74, F10, F14, F16, H10, J52, P26

*We thank Matilde Bombardini, Rafael Dix-Carneiro, Gordon Hanson, Thierry Mayer, Nina Pavcnik, Andres Rodriguez-Clare, Guillem Riambau, Daniel Trefler, Bernard Yeung, and Noam Yutchman for their valuable comments, as well as audiences at the Australasian Trade Workshop (Auckland), Workshop on Trade and the Chinese Economy (Stanford), ABFER Annual Conference, Cities and Development Conference at Harvard, SMU-NUS-Paris Joint Workshop, Chinese University of Hong Kong, IADB, Johns Hopkins SAIS, Kiel Centre for Globalization, Michigan, Yale, UC San Diego, Wisconsin, World Bank – University of Malaya, and INSEAD. Yuting Huang, Wei Lu, Han Loong Ng, Jocelyn Tan, Venetia Wong, and Jiajia Yao provided stellar research assistance.

1 Introduction

Negative economic shocks that adversely impact labor markets and the well-being of citizens often have repercussions for domestic political outcomes. The resulting citizen discontent can naturally translate into decreased support for the government, and in extreme cases, even give rise to public unrest that threatens the stability of the incumbent. How then do political leaders respond to the domestic pressures that are likely to arise following such shocks?

This question has been studied extensively in the context of democracies, with a vast literature demonstrating how weak economic conditions can translate into setbacks at the ballot box for incumbent politicians (e.g., Lewis-Beck 1988, Duch and Stevenson 2008). It has also been explored in the setting of weakly institutionalized polities, where economic shocks can lead to civil unrest and violence that pose an immediate existential threat to the regime in power (e.g., Haggard and Kaufman 1995, Miguel et al. 2004, Brückner and Ciccone 2011). Much less is known, however, about the impact on political outcomes in a non-democratic context where state institutions are relatively strong, so that citizens can neither vote an incumbent out, nor reasonably expect to remove him/her from office by force.

China, a prominent example of a non-democratic regime with relatively strong levels of state capacity, provides an opportune setting to explore this issue, especially as it has gone through a well-documented decline in its export performance.[1] While Chinese merchandize exports grew at a rapid average annual rate of 18% between 1992-2008 (Hanson 2012), this has gone into a sharp reversal since 2012 (see Figure 1). China's manufacturing exports even contracted 8.5% between 2015-2016, in line with the weak performance of trade flows in the rest of the world following the global financial crisis.[2] This slowdown has naturally sparked concerns regarding its potential impact on labor markets, given the prominent role that exports have played in driving China's economic development since the early 1990s.[3]

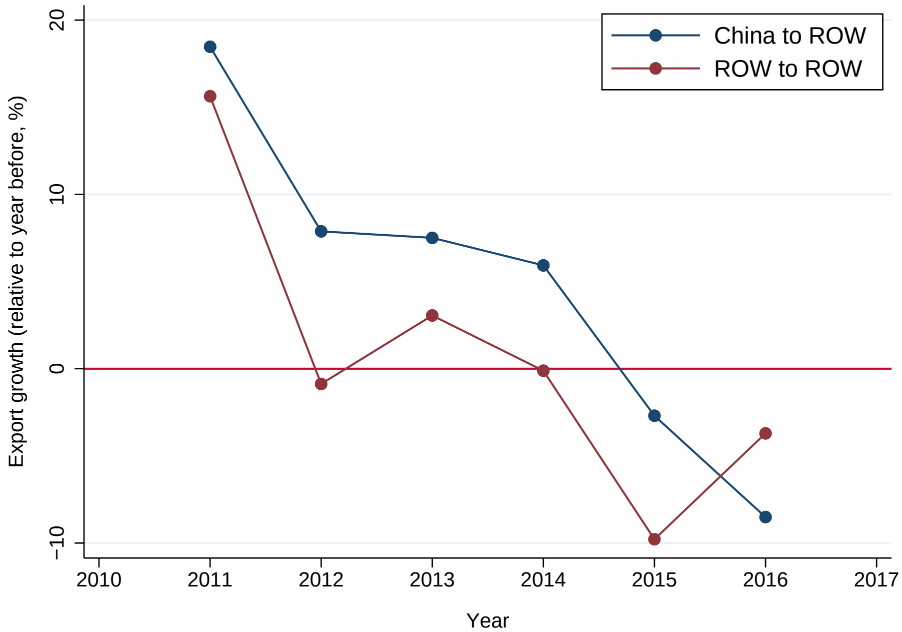
Indeed, at the same time as export manufacturing orders have declined, reports have emerged of a rise in layoffs and the shutdown of factories; in several instances, factory man-

[1]China is classified as an "autocracy" in the widely-used Polity IV Project dataset, and is ranked at the 8th percentile (from the bottom) in "Voice and Accountability" in the 2017 edition of the World Bank's World Governance Indicators (WGI). In contrast, it is at the 68th percentile in the WGI's "Government Effectiveness" score, which is above the average position of its "upper middle income" country peers (51st percentile).

[2]Calculated from UN Comtrade data; the corresponding decrease in the nominal value of exports from the rest of the world to all destinations excluding China was 3.7%. While there has been a recovery in China's export growth since 2016, the trade disputes between the US and China triggered in 2017 have cast a further dampener on the prospects for China's exports. Our analysis will focus on the period up to 2016, given constraints on the data after 2016 on labor strikes and the political response measures.

[3]As an illustration, in a State Council executive meeting on 21 Apr 2016, Premier Li Keqiang emphasized the need to stabilize China's exports amid the "harsh" foreign trade environment, as this was of concern not only to GDP, but also to a large volume of employment (http://www.gov.cn/xinwen/2016-04/21/content_5066423.htm). More formally, Feenstra and Hong (2010) have found that export growth accounted for employment growth of 7.5 million workers per year in China between 2000 and 2005. See also Los et al. (2015) for a related exercise that arrives at even larger estimates of the importance of exports for China's employment.

Figure 1: Manufacturing Export Growth: China and the Rest of the World (ROW)



agers were even alleged to have fled and absconded with company funds. This in turn has triggered localized strikes over job losses and unpaid wage arrears.[4] As we will show, these events have been spread out geographically across China, and their occurrence has shown few signs of abating. This has led to concerns that the cumulation of such labor-related "events" could undermine domestic political stability.[5]

We present formal empirical evidence linking the slowdown in China's exports to a rise in incidents of labor unrest. Building on this, we then show that the export slowdown prompted a range of political responses, from local party leaders (in terms of the heightened attention paid to enforcing public security on the ground), as well as from the central government (in terms of how closely local leaders were held to account for their performance in maintaining stability). Throughout the paper, we study these developments within China at the local level, specifically at the prefecture level (a sub-provincial administrative unit). Our empirical design thus exploits the substantial variation in the severity of the export slowdown both across prefectures within provinces and within prefectures over time.

[4]To give but one example, several hundred workers reportedly staged a peaceful march on 30 April and 1 May 2015 along the streets of Dongguan prefecture, a major manufacturing hub in Guangdong province, when the apparel factory where they were employed shuttered overnight and the factory manager became untraceable. See https://www.rfa.org/mandarin/yataibaodao/renquanfazhi/yf1-05012015100541.html. Dongguan has been a particularly hard-hit prefecture during the export slowdown (*New York Times*, 20 Jan 2016).

[5]This is aptly captured in the following quote from Eli Friedman, a Cornell University scholar on Chinese labor relations: "This is probably the thing that keeps Xi Jinping up at night. Governments are not swimming in money the way they used to be, and there's less room to compromise" (*New York Times*, 14 Mar 2016). The assessment that incidents of labor unrest have been on the rise is consistent with media reporting (e.g., *New York Times*, 14 Mar 2016), as well as analysis by China political watchers (e.g., Tanner 2014).

Given the tight state control of news and information within China, it is by nature challenging to obtain systematic data on labor-related incidents and the government's response to these events, particularly at the local level. We overcome this hurdle through a combination of both conventional and novel sources. The core data on labor strikes comes from a non-governmental organization (the China Labor Bulletin, or CLB) that monitors developments on labor rights issues in mainland China. Importantly, the CLB data allow us to map the location of incidents to prefectures within China. We have moreover corroborated the credibility of this data source by correlating it against official figures (albeit at the more aggregate province level) on labor dispute cases formally raised for arbitration or mediation.

We first establish a robust relationship from the slowdown in exports to a rise in the number of CLB-recorded labor unrest events per worker, using a prefecture-level panel dataset of annual observations from 2013-2015. To support the claim that this relationship is causal in nature, we adopt a shift-share (or Bartik) instrumental variable (IV) for the severity of the export slowdown. This exploits the fact that prefectures differ in the initial product composition of their export baskets, which generates variation in how inherently exposed each prefecture would be to shocks to global trade flows across products (c.f., Autor et al. 2013). Such shift-share IVs are valid to the extent that movements in rest-of-the-world trade flows are being driven by forces exogenous to developments within China. We draw support on this front from studies such as the 2016 IMF World Economic Outlook, which found that the world trade slowdown was accounted for largely by weak global demand, with supply-side forces and trade frictions playing smaller roles in comparison (Aslam et al. 2016). Moreover, our findings do not appear to be driven by shocks to domestic demand or output within China, when we seek to directly control for these using measures constructed at the prefecture level.

Our preferred IV specifications indicate that, were a given prefecture to experience a one-standard-deviation more severe contraction in exports, this would be associated with 0.27 more recorded labor events per million workers – a sizable effect, given that the median strike intensity in our dataset is 0.96. We confirm that this effect is driven by labor events in the manufacturing sector, and where the underlying cause recorded was "wage arrears". These findings are robust under a battery of sensitivity checks, including exercises that speak to recent Bartik IV best-practice recommendations, in terms of validating the case for causal identification (Goldsmith-Pinkham et al. 2018, Borusyak et al. 2018) and improving the statistical inference drawn (Adão et al. 2018). On a related note, we obtain a consistent picture of the adverse impact of the export shock when examining other contemporaneous economic outcome variables, such as the manufacturing share of employment and average wages at the prefecture level.

We then turn to the political responses that the export slowdown has triggered, in the face of this increase in labor strife. First, we confirm that the export slowdown has raised the attention paid to the issue of public security. To gain empirical traction on this, we rely on the observation that there are phrases – most notably, "weiwen" ("维稳") or "maintaining

social stability" – that have been adopted by the party establishment in China as watchwords to communicate the importance of domestic law and order as a political priority (*New York Times*, 2012).

On the part of the general public, we demonstrate via an event study empirical specification that occurrences of CLB strikes are accompanied by a significant increase, in the weeks thereafter, in Baidu internet search engine queries related to "weiwen" (维稳) originating from the prefecture in question. On the part of local political officeholders, we undertake a textual analysis of prefecture work reports – an official speech delivered annually by the highest-ranking local party official – to construct measures of the degree of "weiwen" emphasis in this policy document. This includes a basic keyword count measure, as well as scores obtained from more sophisticated machine-learning algorithms, namely the Multinomial Naive Bayes (MNB) and Support Vector Machine (SVM) procedures, that have been used for the purposes of classifying text passages. Across this range of textual analysis measures, and using a similar shift-share identification strategy as above, we indeed find that a more severe shock to a prefecture's exports is associated in the subsequent year with a discernible rise in the emphasis placed on "weiwen" in the annual work report.

Next, we marshal evidence on the increased attention paid to domestic stability from data on prefecture governments' fiscal expenditures, put together from disparate local statistical yearbooks. In line with the above emphasis on "weiwen," we find that a more severe export slowdown led to a subsequent rise in expenditures directed towards public security uses and social spending, which can be viewed as measures to bolster stability by curbing unrest and by ameliorating worker grievances respectively. We moreover find that younger local party secretaries were inclined to raise both forms of spending more, consistent with their having stronger career concerns tied in with their performance in delivering stability. Interestingly, we detect evidence that prefectures with stronger initial fiscal capacity were less inclined (relatively speaking) to raise public security spending and more willing to expand social spending, suggesting that the latter measures require a more sustained and intensive use of fiscal resources.

As a final piece of evidence on political consequences, we examine patterns in the turnover of local party secretaries (the highest ranked official at the prefecture level), using information on career histories collected from their curricula vitae. We find that a more severe export shock raised the likelihood of incumbent turnover, specifically that he/she would be laterally re-assigned early in his/her tenure (before the three-year mark), leaving a dent on his/her eventual promotion prospects. This was especially so in prefectures that also witnessed a high level of "excess strikes", namely in excess of the level of labor events that would be predicted from observed local economic conditions (including the extent of the export shock).

These features are consistent with a simple model of political accountability "with Chinese characteristics" that we develop, in which local officeholders can be removed by an upper-level government who cares primarily about regime stability. This framework helps to organize our

set of findings on the political economy consequences of negative economic shocks in China and, more broadly, in nondemocratic polities that feature strong state institutions. At the level of the individual politician, the response measures adopted are similar to those which one might observe in either a democracy or weak autocracy, entailing a combination of "carrots" (to shore up support) and "sticks" (repression). Where the strong autocracy is distinct, though, lies in the nature of how political accountability is exercised, namely within the system from above, in contrast with democracies and weak autocracies, where the accountability often comes from the ground, through the ballot box or violent removal respectively. We show that the distinction has meaningful implications: It entails for instance that in strong autocracies, it is the local officials with brighter prospects who are more likely to respond through repression. Moreover, a key takeaway is that a central government who wants to properly incentivize local incumbents in the face of negative economic shocks would indeed assess them by a relative benchmark ("excess strikes"), rather than against an absolute standard (the level of strikes).

The paper proceeds as follows. Section 2 reviews the related literature. Section 3 describes our main data sources, before Section 4 discusses the estimation framework and identification strategy. Section 5 presents the main findings on the effects of the export slowdown on labor strikes. Section 6 lays out a model linking export shocks, unrest, and incumbent behavior. Sections 7 and 8 then report our empirical findings on the political emphasis on domestic stability and on incumbent turnover. Section 9 concludes. Appendix A documents further details related to the data, while Appendix B reports additional results and checks.

2 Related Literature

The empirical approach and the substantive findings in our paper engage three strands of work in the literature.

First and foremost, it connects with a broader set of studies on the political implications of weak economic conditions. In the context of democracies, it has been argued that an incumbent's response to economic shocks can reveal information about his/her quality (e.g., Fearon 1999), and the threat of electoral punishment for a bad response can in turn be a powerful incentive that shapes incumbents' behavior (e.g., Barro 1973, Ferejohn 1986).[6] On the empirical side, this has led to an extensive body of work on "economic voting", to examine whether voters do in fact hold politicians accountable for a weak economy at the ballot box (see for example, Lewis-Beck 1998, Duch and Stevenson 2008).[7] As a cautionary note though, this line

[6]The systematic manner in which economic conditions can influence incumbent's decisions would also raise the possibility of political business cycles (see Persson and Tabellini 2000).

[7]It has been further argued that the nature of the relationship between economic weakness and voting patterns would vary depending on the presence of an independent media (Besley and Burgess 2002), on other local institutions (van der Brug et al. 2007), and on culture (Nunn et al. 2018). See also Healy et al. (2017) who examine data on the motivations that drive economic voting at the individual level.

of research has also uncovered evidence that voters can mis-attribute negative economic shocks to poor incumbent performance, resulting in their being punished by voters for what amounts to bad luck that is out of their control (see the survey by Healy and Malhotra 2013).[8]

On the other end of the spectrum, there is also a substantial literature on the impact of economic shocks in weakly institutionalized polities. Bad economic times are seen to reduce the opportunity cost of conflict, as well as generating dissatisfaction ("grievances") with incumbent performance, both of which can translate into anti-incumbent political action. In the context of weak states, such unrest can pose an immediate threat to the government of the day. Negative shocks have been linked to political instability (e.g., Haggard and Kaufman 1995, Alesina et al. 1996, Burke 2012), conflict (e.g., Miguel et al. 2004, Hendrix and Salehyan 2012, Bazzi and Blattman 2014), coups (e.g., Dube and Vargas 2013, Kim 2016), and even democratic change (e.g., Burke and Leigh 2010, Brückner and Ciccone 2011).[9]

Less is known, however, about contexts in which governments are authoritarian, and thus need not worry about electoral accountability, yet are sufficiently stable that they do not face an immediate existential threat. China is a paramount example, though not an isolated case in light of the recent "democratic recession" that appears to have increased the number of authoritarian regimes around the world (Diamond 2015).[10] The role of social protest in the Chinese system has been investigated in its many facets (e.g., Chen 2012), but as far as we can tell without a systematic quantitative assessment of the impact on incumbent behavior at the local versus upper levels of government. Related to this, Lorentzen (2013) has argued that protest in China, far from signaling regime weakness, is actually used as an information extraction device by higher levels of government. Our findings are consistent with this view, and moreover suggest that the higher-level decision-makers draw a distinction in their evaluation of local leaders between incumbent performance and the impact of negative shocks that are beyond the incumbent's control.

Our paper relates to a second strand of literature on the labor market and worker effects of exposure to international trade, on which we draw in our use of a shift-share IV strategy. While many of these existing studies have focused on the consequences of an increase in exposure to imports (Topalova 2010, Autor et al. 2013, Acemoglu et al. 2016, Dix-Carneiro and Kovak 2017, Dix-Carneiro et al. 2018, etc.), we instead study the effects that shocks to export opportunities can have on a key developing country.[11] Along these lines, we also contribute to a growing body of work that has explored how exposure to trade has affected political outcomes, including legislative voting (Feingenbaum and Hall 2015), electoral voting (Jensen et al. 2017, Che et al.

[8]For specific examples, see Achen and Bartels (2004), Leigh (2009), and Cole et al. (2012).

[9]Interestingly, this link from economic setbacks to civil conflict has been studied for several historical episodes in China when state institutions were weaker (see Jia 2014, Braggion et al. 2018).

[10]Cases like Russia provide illustrations of the broad relevance of strong-state authoritarianism. See Levitsky and Way (2015) though, for pushback on the extent of this democratic recession.

[11]See McCaig (2011) for an exception in this regard, that explores how Vietnam's entry into export markets affected poverty at the provincial level.

2018), political polarization (Autor et al. 2017), support for extremist parties (Dippel et al. 2018), and support for cross-border economic integration (Colantone and Stanig 2018).

Last but not least, our study is related to the literature on China's political system, specifically the management of its cadres. The existing work has identified several salient determinants of the promotion probability of government officials, including local economic performance (Li and Zhou 2015), political connections (Jia et al. 2015), social ties (Fisman et al. 2015), and factions (Francois et al. 2016, Shih and Lee 2018). We complement these studies by showing that the relative performance in maintaining social stability is a crucial determinant of the career prospects of local officials. Our paper is thus linked to the literature on the career concerns of politicians and how this can affect the allocation of public resources; see for example, Persson and Zhuravskaya (2016) and Chen and Kung (2016, 2018) who study this in the context of China. These papers focus on how promotion criterion based on economic outcomes has swayed the use of fiscal resources towards spending that has a short-term effect on boosting economic growth (such as construction projects). Our paper instead studies how economic shocks can induce a shift in fiscal resources towards uses aimed at bolstering social stability.

3 Data Sources and Measures

We turn now to describe our empirical setting and key data sources. We describe in this section the data on exports, labor strikes, and other local economic variables that we use to first establish a relationship between export performance and incidents of labor unrest. We postpone a discussion of the measures of political response and turnover to Sections 7-8, where we will encounter those variables at greater length. Further details about the data construction are provided in Appendix A.

Throughout this paper, the unit of our analysis is the prefecture; this is an administrative division in China that is smaller than a province, but larger than a county. All prefectures in China are included, with the exception of Tibet due to data limitations. There are 333 prefectures in our sample, with a median land area of 12,980km^2 and a median population of 3.25 million in 2010.[12]

3.1 Exports

We focus on the performance of manufacturing exports as our key local economic shock variable. For this, we draw on trade data from China's General Administration of Customs, which covers the universe of China's exporters and importers. For each trading firm, the customs data provides information on its location and a breakdown of its trade flows at the Harmonized

[12]While there have been some changes to administrative boundaries within China over time, we have constructed all our data variables to be in accord with the 2010 administrative divisions.

System (HS) 6-digit product level. We aggregate the data across all firms f located in prefecture i to construct a measure of manufacturing exports per worker in year t:

$$Export_{it} = \sum_k \sum_{f \in i} \frac{X_{fikt}}{L_{i,2010}}. \tag{1}$$

Note that k indexes HS 6-digit codes, and we include all products k that map to the manufacturing sector.[13] $L_{i,2010}$ denotes the working-age population (ages 15 to 64) in prefecture i and year 2010; this data are from the China Population Census, and includes all individuals both with or without residency rights (hukou). Our analysis will be based on a panel that covers the period $t \in \{2013, 2014, 2015\}$, and we will consider the annual change in exports – defined as $ExpShock_{it} = Export_{it} - Export_{i,t-1}$ – as our main explanatory variable. By construction, $ExpShock_{it}$ measures the change in manufacturing exports in 1000 USD per worker.

Figure 2: Prefecture-Level Annual Export Growth Rates
(Tail 5% top- and bottom-coded within each year)

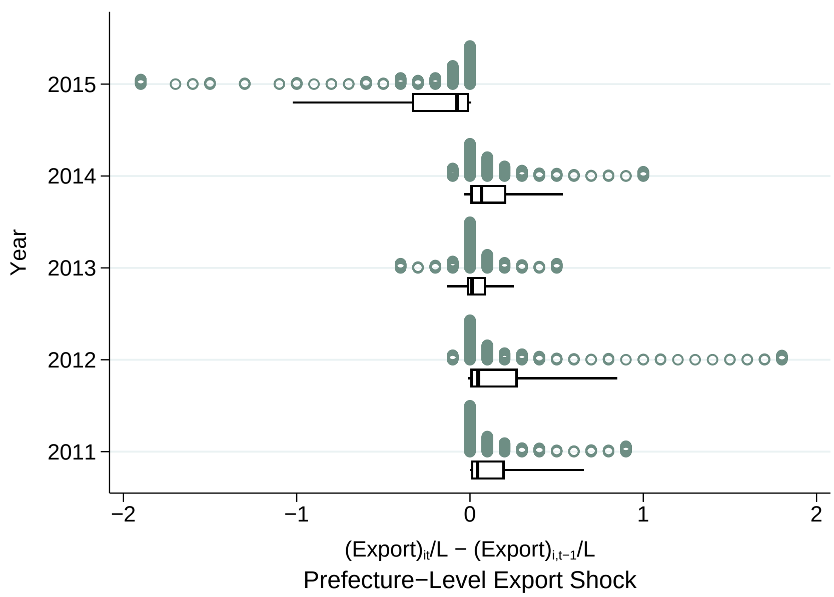


Figure 2 shows the distribution of $ExpShock_{it}$ across prefectures in the years leading up to 2015.[14] There is considerable spatial and temporal variation in this measure of the per worker export shock. The export slowdown was particularly marked in 2015, with a mean decline across prefectures of 373 USD per worker from the year before. More importantly, there was substantial variation in the severity of the export shock; in 2015, for example, the

[13]We use all HS product codes that map to SIC manufacturing industries, namely SIC industries with leading digit equal to 2 or 3. The mapping to SIC is from the World Integrated Trade Solutions (WITS) at: https://wits.worldbank.org/product_concordance.html.

[14]For the purposes of this figure, the data have been top- and bottom-coded at the 5th and 95th percentile values respectively across prefectures in any given year. Given the long tails in the export shock measure, we take care to verify in the later regression analysis that our results are not driven by potential outliers.

standard deviation of $ExpShock_{it}$ across prefectures was 949 USD per worker (as reported in the summary statistics in Panel A of Table 1).

We will later verify the robustness of the results to using alternative export shock variables, such as when excluding firms that are pure intermediaries, or when focusing on exports by firms across ownership types (namely, privately-owned versus state-owned enterprises), these being distinctions that are relevant in the context of China's export activities. Likewise, our findings continue to hold when we work with an export shock variable that is constructed as an export growth rate (rather than as a dollar value change).

3.2 Labor strikes

The data on labor strikes are drawn from the China Labour Bulletin (CLB), a non-profit organization based in Hong Kong which has monitored and logged incidents of collective actions by workers across mainland China since 2011. Up until 2017 (which includes our sample period), the CLB gathered this information on a daily basis from online and media sources, including but not limited to Sina Weibo, WeChat, Tianya, Baidu, and Google.[15] In the absence of official statistics on labor strikes, this data hosted on the CLB Strike Map have been used regularly by news media outside of China to examine trends in worker actions within China.[16] For each labor event, the CLB records the date, location (at the prefecture level), and a short description of the incident. For the vast majority of observations (>98%), the CLB provides further information on: (i) the sector in which the worker action occurred (e.g., manufacturing, construction, services); and (ii) its underlying cause (e.g., wage arrears, layoffs, work conditions). A total of 5,156 labor events were recorded for the 2012-2015 period, with most of these strikes occurring in the manufacturing sector (36%), followed by construction (26%). The most common cause cited – in about 60% of the cases – was employee demands over wage arrears.

Figure 3 illustrates the distribution of CLB-recorded labor events across China during 2012-2015. As is clear from this map, the labor events are spread out geographically, even while the density of events appears to be higher in coastal regions that are manufacturing hubs, such as the Yangtze River Delta and the Pearl River Delta. The summary statistics in Table 1 point to an increase over time in the occurrence of strikes, as measured by the number of labor events per million workers (with the denominator proxied by the population aged 15-64 in the 2010 Census). That said, there is substantial variation across prefectures in the extent of this increase: While the average prefecture experienced a rise in strikes per million workers of 1.24 in 2015, the standard deviation associated with this measure was 1.77.

[15] Starting in 2017, the CLB switched to updating on a weekly or twice-weekly basis, which potentially limits the comparability of the data pre- and post-2017.

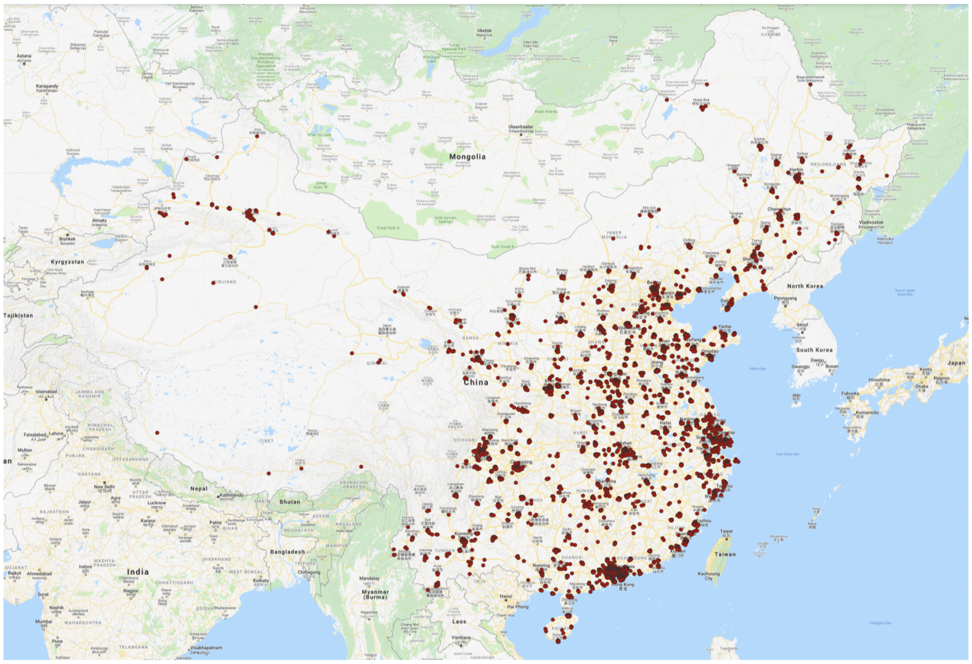
[16] See for example, the Financial Times (14 July 2016): https://www.ft.com/content/56afb47c-23fd-3bcd-a19f-bddab6a27883; The Economist (19 March 2016): https://www.economist.com/china/2016/03/19/deep-in-a-pit; and The New York Times (14 March 2016): https://www.nytimes.com/2016/03/15/world/asia/china-labor-strike-protest.html. The CLB Strike Map data is at: https://maps.clb.org.hk/strikes/en.

Table 1: Summary Statistics

Panel A: Labor Strikes and Economic Variables	2013	2014	2015	All Years
Δ Number of CLB events per million workers	0.208	0.742	1.239	0.730
	(0.647)	(1.075)	(1.769)	(1.320)
Export Shock (1000 USD per worker)	0.016	0.233	-0.372	-0.041
	(0.547)	(0.755)	(0.948)	(0.806)
Export Shock, Bartik IV (1000 USD per worker)	0.171	0.093	-0.659	-0.132
	(0.532)	(0.371)	(1.374)	(0.953)
Δ Log College-enrolled share of population	0.039	0.049	0.046	0.045
	(0.150)	(0.172)	(0.136)	(0.154)
Δ Log Mobile share of population	0.080	0.030	-0.004	0.035
	(0.110)	(0.085)	(0.097)	(0.104)
Δ Log Internet share of population	0.140	0.105	0.108	0.117
	(0.208)	(0.199)	(0.159)	(0.190)
Δ Log Average wage	0.096	0.080	0.103	0.093
	(0.068)	(0.051)	(0.065)	(0.062)
Δ Employment / Population	0.034	-0.010	0.000	0.008
	(0.192)	(0.180)	(0.021)	(0.153)
Δ Manufacturing employment / Population	0.011	-0.001	-0.001	0.003
	(0.069)	(0.007)	(0.009)	(0.041)
Δ Log Industrial output per capita	0.121	0.065	-0.008	0.059
	(0.097)	(0.193)	(0.220)	(0.186)
Panel B: Political Economy Response Measures	**2014**	**2015**	**2016**	**All Years**
Δ Log occurrences of "weiwen" related keywords	-0.131	0.086	-0.167	-0.070
	(0.757)	(0.782)	(0.757)	(0.773)
Δ Log "weiwen" score, MNB	-0.059	0.198	-0.426	-0.097
	(0.816)	(0.753)	(0.844)	(0.845)
Δ Log "weiwen" score, SVM	-0.090	-0.087	-0.271	-0.150
	(1.220)	(1.160)	(1.312)	(1.234)
Δ Log fiscal expenditure, Public security	0.050	0.114	0.128	0.097
	(0.083)	(0.110)	(0.118)	(0.110)
Δ Log fiscal expenditure, Social spending	0.077	0.134	0.080	0.098
	(0.074)	(0.080)	(0.074)	(0.080)
Δ Log fiscal expenditure, Total	0.077	0.136	0.066	0.093
	(0.067)	(0.114)	(0.075)	(0.093)
Party secretary Turnover	0.131	0.322	0.435	0.296
	(0.338)	(0.468)	(0.496)	(0.457)
Party secretary Turnover, Lateral	0.046	0.164	0.280	0.163
	(0.209)	(0.371)	(0.450)	(0.370)
Party secretary Turnover, Early lateral	0.024	0.066	0.060	0.050
	(0.153)	(0.249)	(0.238)	(0.218)

Notes: All annual changes are computed relative to the previous year. The mean across prefectures (excluding Tibet) is reported, with the standard deviation in parentheses. The "All Years" column reports the summary statistics pooled across all years and prefectures in the prior columns. The Δ Log College-enrolled share through Δ Log Industrial output per capita variables are computed from the annual City Statistical Yearbooks. The construction of the textual analysis measures, fiscal expenditure variables, and party secretary turnover records are described in Sections 7.1, 7.2, and 8 respectively.

Figure 3: CLB Map (2012-2015)



Given the manner in which the data are collected, the CLB themselves are careful to acknowledge that they do not have a complete record of all labor incidents. For our empirical analysis though, what will be more crucial is whether the CLB data are adequately picking up trends over time and across locations in the occurrence of labor strikes. To corroborate as best we can this dimension of the data, we have compared the CLB data against official records on the number of labor dispute cases where the involved parties have filed for mediation or arbitration, as reported in the China Labor Statistical Yearbooks published by the Ministry of Human Resources and Social Security (MOHRSS). While this MOHRSS data is only available at the more aggregate province level (and hence not ideal for a regression analysis), we nevertheless view it as a useful measure of the frequency of labor disputes that can serve as a cross-check to the CLB data.

Panel A of Figure A.1 in the appendix plots the number of CLB labor events and MOHRSS labor disputes aggregated at the national level between 2012-2015. Note that the total number of CLB events (right vertical axis) is smaller than the total number of MOHRSS labor disputes (left vertical axis).[17] This could be due to strikes being a more extreme and hence less frequent form of labor action; alternatively, this could simply reflect that the CLB do not capture all significant labor events that have taken place. More importantly for our purposes, the CLB strike data clearly follow a similar upward trend as incidents of officially-recorded labor disputes. Panel B of Figure A.1 further compares the annual changes in these two variables

[17]For these checks, we use the total number of labor dispute cases raised either collectively or by individuals; a very similar set of results is obtained when focusing only on collective labor disputes.

across provinces p and years t ($t \in \{2013, 2014, 2015\}$). This confirms that annual changes in the number of CLB-recorded strikes per million workers ($\Delta Events_{pt}^{CLB}$) are positively correlated with the corresponding changes in MOHRSS-recorded labor disputes per million workers ($\Delta Events_{pt}^{MOHRSS}$).

A more subtle concern is that the intensity of reporting on labor unrest could vary systematically with the extent of the economic shock experienced in a location. This would be a source of nonclassical measurement error that could lead to a spurious negative correlation between the number of CLB strikes and the change in exports, if internet sources were to intensify their efforts to report on labor unrest in locations where the export shock was more severe. Conversely, the reporting of labor disputes in the MOHRSS for such locations might have actually declined, if local officials there had a greater incentive to discourage the filing of labor disputes. We investigate this possibility by comparing the ratio of the number of CLB to MOHRSS events, $Events_{pt}^{CLB}/Events_{pt}^{MOHRSS}$, against the observed change in the value of exports per worker, where the latter are constructed using the analogue of (1) at the province level. The correlation coefficient between these two variables turns out to be small (0.0032) and not statistically significant.[18] While we are unable to conduct a similar analysis at the prefecture level due to data limitations, we take the above check as reassuring that such forms of systematic reporting bias are unlikely to be prevalent in the data.

3.3 Other Local Economic Data

We also collected a set of socioeconomic variables at the prefecture level to be used as controls or as additional local economic outcomes to be explored. These data were obtained from various official Chinese sources. Data on the breakdown of the population by age group (in particular, between the working ages of 15-64) and by internal migration status (hukou vs non-hukou) were drawn from the 2010 Population Census.

Data on other economic variables – such as the average wage level, gross industrial output per capita, manufacturing employment share, college educated share of the population, mobile and internet penetration rates – were computed from the China City Statistical Yearbooks. Note that these yearbooks report only on urban prefectures, which reduces our coverage to 290 prefectures when these variables are used. Moreover, the data in these yearbooks that pertain to labor markets (such as average wages) cover only the segment of the workforce with hukou rights; to the extent that the non-hukou workforce may bear the brunt of the impact of a negative export shock, the data would not directly pick up this effect. That said, this is the best publicly-available data (to the best of our knowledge) that provides updates at the prefecture level on an annual basis. (We present summary statistics for a selection of these prefecture-level variables in Panel A of Table 1.)

[18]The correlation between the annual change in this ratio of CLB to MOHRSS events and the province-level export shock is likewise small (0.0384) and statistically not significant.

4 Empirical Strategy

In this section, we elaborate on the regression model and identification strategy that we adopt to uncover the effects of the export slowdown on incidents of labor strikes. This forms the first piece of evidence in our discussion of broader political economy outcomes in China.

4.1 Estimating Equation

Our baseline regression specification is as follows:

$$\Delta(Event/L)_{it} = \beta_1 ExpShock_{it} + \beta_2 (Event/L)_{i,t-1} + \beta_X X_{it} + D_{pt} + D_i + \varepsilon_{it}, \tag{2}$$

where i denotes prefecture and t denotes year. The dependent variable $\Delta(Event/L)_{it}$ is the change in number of CLB-recorded labor events per million workers, while the key explanatory variable $ExpShock_{it}$ is the change in manufacturing exports per worker previously defined in (1); both of these variables are constructed as changes between year $t-1$ and t. The regression sample stacks the first differences of three periods, 2012-2013, 2013-2014, and 2014-2015, and includes province-by-year dummies, D_{pt}, and prefecture dummies, D_i. The first-differencing removes any time-invariant determinants of labor unrest that are specific to each prefecture. The D_{pt} and D_i further account for any province-specific shocks across different time periods, as well as any prefecture-specific linear time trends in strike intensity, respectively. As a result, the coefficient β_1 is being identified off variation in export shifts across prefectures within provinces, as well as within prefectures over time.

With regard to the other right-hand side variables, the X_{it} refers to a set of time-varying prefecture characteristics which we control for as potential alternative determinants of labor strikes. We also include the lagged number of CLB events per worker $(Event/L)_{i,t-1}$, to control for any tendency towards mean reversion in the occurrence of strikes; our findings are similar even if we were to drop this variable (see Table B.1). We cluster the standard errors by province to accommodate the possibility of unobserved correlated shocks across prefectures within a given provincial unit.[19] In practice, we run the regressions weighting each observation by the working-age population in 2010, although this is not material for our results (see Table B.1 for the unweighted regressions).

4.2 Instrumental Variable

An immediate concern that arises with ordinary least-squares estimates of (2) is the issue of reverse causality, namely that it could instead be the occurrence of labor strikes that is adversely

[19] In Table B.8, we demonstrate that the findings continue to hold under alternative clustering schemes that seek to account for the concern in Adão et al. (2018), namely that the regression error terms could be correlated across prefectures that need not be geographically proximate, yet feature a similar initial export product mix.

affecting export performance. We therefore construct a shift-share or Bartik IV for the export shock variable, to make a clearer case for a causal relationship running from a slowdown in exports to a rise in strikes. This IV combines information on the initial export mix within Chinese prefectures together with product-level shifts in world trade flows excluding China (henceforth, referred to as the "rest of the world" or ROW). To be more specific, we construct the following IV for $ExpShock_{it}$:

$$ExpShockROW_{it} = \sum_k \frac{X_{ik,2010}}{\sum_i X_{ik,2010}} \frac{\Delta X_{kt}^{ROW}}{L_{i,2000}}. \tag{3}$$

In the above, $\Delta X_{kt}^{ROW} \equiv X_{kt}^{ROW} - X_{k,t-1}^{ROW}$ is the change in the value of product-k trade flows from the ROW to the ROW, based on data on HS 6-digit product-level flows from UN Comtrade. Each product-k shift is then apportioned to prefectures within China using weights $X_{ik,2010}/\sum_i X_{ik,2010}$ that reflect the importance of each prefecture i as an exporter of product k in a pre-sample year (2010), as constructed from the Chinese customs data. We express the IV as an export shock in units of 1000 USD per worker, by dividing by the working-age population in prefecture i in the 2000 Census, $L_{i,2000}$; we use data from an earlier census since using the same denominator as in the construction of $ExpShock_{it}$ might artificially boost the first-stage correlation of the IV.

The validity of (3) as an instrument rests on the assumption that, conditional on the province-year and prefecture fixed effects, $ExpShockROW_{it}$ is uncorrelated with other time-varying, prefecture-specific determinants of the outcome variable that would be captured in the regression residual, ε_{it}, in (2). Given the Bartik-style construction, one would need to be reassured that the ε_{it} are uncorrelated with: (i) the initial export structure of the prefecture, and (ii) the product-specific export shocks observed at the national level. With regard to (i), a natural concern is that the initial export structure might directly drive prefecture-specific trends in labor strikes per capita. The inclusion of the D_i fixed effects helps precisely to guard against this concern, to the extent that the underlying trends are linear in nature.[20]

With regard to (ii), we view the rest-of-the-world trade shifts, ΔX_{kt}^{ROW}, as primarily picking up demand shocks that are external to China. This is supported by studies such as the IMF World Economic Outlook (Aslam et al. 2016), which found that about 60-80% of the global trade slowdown during this period was attributable to demand-side forces, specifically the weak recovery in world demand after the global financial crisis; these conclusions were reached via two separate methodologies, namely a reduced-form regression analysis and a model-based structural decomposition.[21] By contrast, supply-side forces and increases in trade frictions

[20]See for example McCaig (2011), who differences his outcome variable relative to pre-shock data to address this issue of confounding location-specific time trends that could be correlated with initial industry composition. With the inclusion of prefecture fixed effects, our empirical strategy is similar to his.

[21]The regression-based analysis estimated an import demand system, which delivered the 80% headline number for the contribution of aggregate demand forces to the global trade slowdown. On the other hand, the

played a smaller role in comparison. In our present empirical context, what this means is that the shift-share IV we construct plausibly leverages on sources of variation in world trade flows that are driven by foreign demand conditions, and then projects these onto each prefecture on the basis of pre-determined product weights.

While (3) serves as our baseline IV, we will explore the use of alternative constructions to further isolate variation in the ROW product-level trade flows that is driven by foreign demand forces. This includes a version that exploits the variation across importing countries (to capture destination-specific demand forces), as well as measures that seek to filter out demand shifts in foreign markets via a gravity-equation approach. We are also cognizant of the concern that the ΔX_{kt}^{ROW} terms might be incidentally correlated with domestic demand or domestic supply shocks stemming from within China, and so we will report robustness results in which we make an effort to control for these latter forces. (We defer a more detailed discussion of these and other checks on the validity of the Bartik IV strategy to Section 5.2.)

It is helpful at this juncture to discuss how the manner of construction of the IV in (3) compares with the empirical approach in Autor et al. (2013). Our application studies the effects of export shocks, rather than a shock to import competition. In addition, we adopt export shares ($X_{ik,2010}/\sum_i X_{ik,2010}$) when building our instrument, instead of analogous employment share weights as in Autor et al. (2013). As shown in Appendix A.2, the export shock defined in equation (3) that uses export-share weights can be rationalized by log-linearizing the relationship between exports and external demand shifts. Moreover, if one were to instead apportion the export shocks ΔX_{kt}^{ROW} on the basis of employment shares, this could systematically over-state the importance of export exposure in prefectures that are on the whole less export-oriented (such as China's inland provinces). Last but not least, we have the benefit of the detailed Chinese customs data from which the HS 6-digit export weights can be readily computed.

5 Effects of Export Shocks on Labor Strikes

We turn now to present our core findings on the effect of export performance on labor strikes at the prefecture level (Section 5.1). We include in this section a discussion of robustness checks and validation exercises for the Bartik IV strategy (Section 5.2), as well as further corroborating evidence drawing on available data on other labor market and economic outcomes (Section 5.3).

5.1 Baseline Results

Table 2 first reports our baseline results. Column 1 presents the OLS estimates of the specification in equation (2), revealing that an export slowdown (i.e., a more negative $ExpShock_{it}$)

model-based decomposition built off the multi-country model of production and trade of Eaton et al. (2016); this approach yielded a 60% figure for the contribution of aggregate demand forces to the decline in trade as a share of world GDP.

Table 2: Export Shocks and Labor Strikes

Dependent variable:	Δ CLB Events per million$_{it}$			
	(1) OLS	(2) IV	(3) IV	(4) OLS-RF
ExpShock$_{it}$	-0.1603***	-0.3190***	-0.3207***	
	(0.0327)	(0.0560)	(0.0539)	
ExpShockROW$_{it}$				-0.2002***
				(0.0324)
Events per million workers$_{i,t-1}$	-0.9167***	-0.9169***	-1.0027***	-1.0935***
	(0.1608)	(0.1792)	(0.1610)	(0.1123)
Δ Log College-enrolled share$_{it}$			0.2623	0.2437
			(0.1887)	(0.1997)
Δ Log Mobile share$_{it}$			1.4753*	0.7634
			(0.7353)	(0.6254)
Δ Log Internet share$_{it}$			0.3462***	0.5363***
			(0.1184)	(0.1591)
Prefecture dummies?	Y	Y	Y	Y
Province-year dummies?	Y	Y	Y	Y
First-stage F-stat	–	49.20	69.74	–
Observations	987	987	822	822
R^2	0.6234	0.6105	0.6464	0.6719

Notes: The dependent variable is the change in CLB-recorded events per million workers in prefecture i between year $t-1$ and t. All regressions are weighted by the prefecture's working-age population in 2010. Column 1 reports OLS estimates, while Columns 2-3 are IV regressions. Column 4 reports the reduced-form where the Bartik IV is used directly in place of $ExpShock_{it}$ in an OLS regression. The additional control variables in Columns 3-4 are constructed as changes in log shares relative to prefecture population size, where the changes are taken between year $t-1$ and t. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

was indeed associated with a rise in CLB-recorded labor events per worker. We proceed to instrument in Column 2 for the export shock using the shift-share variable defined in (3).[22] The IV estimate points to a negative and statistically significant effect of the export shock in raising the occurrence of strikes, that is moreover larger in magnitude than the OLS estimate. This could be due to the standard attenuation bias arising from measurement error in the export shock variable. Alternatively, the OLS estimate in Column 1 may have been subject to omitted variables bias; for example, unobserved supply shocks due to automation could boost exports while also inducing more labor unrest from displaced workers, which would dampen the magnitude of the export shock coefficient, β_1. To the extent that the Bartik IV satisfies the exclusion restriction, it would leverage a component of $ExpShock_{it}$ that is orthogonal to such supply shocks to yield an estimate of β_1 that is not confounded by such forces.

We incorporate in Column 3 a set of concurrent socioeconomic shifts that could indepen-

[22]Although we do not report the estimates to save space, the shift-share IV is indeed positively correlated with the export shock. The table does report the F-stats; these are all in excess of the rule-of-thumb value of 10, indicating that the first stage is highly relevant for explaining the variation in $ExpShock_{it}$.

dently affect labor unrest. We control for the change in the log college-enrolled share of the general population, motivated by related work that has shown that individuals with higher levels of education have a greater propensity to engage in civic and even protest actions (see for example, Campante and Chor 2012). We further include the contemporaneous changes in the shares of mobile phone and internet subscribers in the prefecture population, to account for the diffusion of digital information and communication technology (ICT) and its potential role in facilitating the mobilization of workers (Manacorda and Tesei 2016, Campante et al. 2018). We find that these control variables each exhibit a positive correlation with the occurrence of labor strikes, with the role of broader access to ICT even being statistically significant.[23] That said, the estimated effect of the export shock on increases in strikes remains stable even when these further controls are used. Lastly, Column 4 reports the reduced-form effect of our shift-share variable on strike intensity in an OLS regression, to confirm that a decrease in the ROW export shock is directly relevant for explaining a rise in incidents of labor unrest.

To gauge the magnitude of the implied effects, we consider the differential change in strike intensity that would be induced by a one standard deviation shift in the export shock (about \$841 per worker). The β_1 point estimate from Column 3 translates this into 0.27 more strike events per million workers, which is fairly sizeable considering that the median occurrence of strikes in our sample is 0.96 per million workers.

We explore next the nature and causes of these labor strikes in more detail. Making use of the breakdown of these labor events by sector (as reported in the CLB), Table 3 confirms that the observed effect on labor strikes was indeed concentrated in the manufacturing sector (Column 1). Note that we run IV regressions in this table following the specification in Column 3 of Table 2, but use instead as the dependent variable the annual change in labor events in the respective sector (normalized once again by the 2010 prefecture working age population); when we do so, we also use a sector-specific measure of $(Event/L)_{i,t-1}$ on the right-hand side.[24] Bearing in mind that $ExpShock_{it}$ was constructed from manufacturing product trade flows, this finding verifies that the manufacturing sector did indeed bear the brunt of the labor market fallout. This also affected workers in construction and services (Columns 2 and 5), albeit with smaller estimated effects. On the other hand, the export slowdown was not systematically linked with labor unrest in the mining and transportation sectors (Columns 3 and 4).[25]

[23] These auxiliary control variables are constructed as changes between year $t-1$ and t (i.e., contemporaneous with the dependent variable). Our findings are also robust if we were to further include the changes in log average wage and log gross domestic product per capita from the China City Statistical Yearbooks; we however do not use these latter variables in the baseline tables as these are potentially direct outcomes of the export shock (results available on request).

[24] The five sectors considered in Table 3 account for about 90% of all CLB labor incidents. The sectors omitted from the table are "Education", "Retail", and "Others".

[25] There are two possible explanations for the larger spillover effects of the export shock in construction and services. First, the skills developed in the manufacturing sector could be less directly transferable to jobs in mining and transportation. Second, the state plays a relatively larger role in mining and transportation, and so could provide more of a buffer to cushion workers in these sectors from negative shocks. For example, according

Table 3: Export Shocks and Labor Strikes: By Sector

Dependent variable:	Δ **CLB Events per million**$_{it}$				
Sector:	**Manufacturing**	**Construction**	**Mining**	**Transportation**	**Services**
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
ExpShock$_{it}$	-0.1609***	-0.1104***	0.0062	0.0119	-0.0423***
	(0.0202)	(0.0321)	(0.0074)	(0.0254)	(0.0124)
Events per million workers$_{i,t-1}$	-0.8822***	-0.8231***	-1.1368***	-1.4104***	-1.3641***
	(0.1445)	(0.2366)	(0.1878)	(0.0576)	(0.0853)
Additional time-t controls?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Province-year dummies?	Y	Y	Y	Y	Y
First-stage F-stat	45.74	134.6	104.6	134.5	95.64
Observations	822	822	822	822	822
R^2	0.6424	0.6267	0.5375	0.7008	0.6543

Notes: The dependent variable is the change in CLB-recorded events per million workers in prefecture i between year $t-1$ and t, that occurred in the sector in question; the "Events per million workers$_{i,t-1}$" variable is the corresponding sector-specific level of CLB events per worker at time $t-1$. All columns report IV regressions, weighted by the prefecture's working-age population in 2010. The additional time-t controls are those used in Column 3 of Table 2, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

Table 4 further exploits the available information on the reported causes of the labor events. We consider three categories of employee demands, namely arising from: (i) wage arrears (the most commonly-cited cause); (ii) wage arrears and/or layoffs; and (iii) all other causes (*not* wage arrears or layoffs, which includes for example disputes over work conditions).[26] Using an IV specification analogous to that in Table 3, the results in Table 4 confirm that negative export shocks prompted an increase in labor strikes over wage arrears (Column 1), with an even larger estimated effect if strikes related to layoffs are also considered (Column 2). By contrast, we find no statistically significant effect of $ExpShock_{it}$ on labor events from other residual causes (Column 3). We obtain a similar set of findings when restricting the labor events to those strikes that occurred in the manufacturing sector: While we do now find a significant effect of an export shock on labor events associated with residual causes (Column 5), this effect is much smaller in magnitude compared to the rise in strikes over wage arrears or layoffs (Column 4). Viewed together, the patterns in Tables 3 and 4 are consistent with a broader narrative of weakening labor market outcomes particularly in the manufacturing sector, leading to an increase in expressions of worker distress arising from unpaid wages or layoffs.

In Table 5, we look into several facets of the prefecture-level export shock variable. Column 1 considers whether spatial correlation in the export shock experienced across prefectures

to the 2013 Economic Census, the share of employment by state-owned enterprises (SOEs) was only 4.7% in the construction sector, while the corresponding employment share was 27.5% for the transportation sector.

[26]Where multiple causes were cited for an event, we counted the incident as being about "wage arrears" (respectively, "layoffs") if the term appeared anywhere in the list of recorded employee demands.

Table 4: Export Shocks and Labor Strikes: By Causes

Dependent variable:	Δ **CLB Events per million**$_{it}$				
Cause:	**Wage Arrears**	**Wage Arrears and Layoffs**	**NOT Wage Arrears and Layoffs**	**Wage Arrears and Layoffs**	**NOT Wage Arrears and Layoffs**
Sector:	All	All	All	Mfg.	Mfg.
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
ExpShock$_{it}$	-0.3532***	-1.6156***	0.1116	-1.5647***	-0.1756***
	(0.0634)	(0.3574)	(0.0789)	(0.3576)	(0.0383)
Events per million workers$_{i,t-1}$	-0.6130***	-0.5709	-1.3832***	-0.6583	-1.0932***
	(0.1785)	(0.5130)	(0.0955)	(0.4441)	(0.1424)
Additional time-t controls?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Province-year dummies?	Y	Y	Y	Y	Y
First-stage F-stat	84.37	31.14	128.9	31.87	65.29
Observations	822	822	822	822	822
R^2	0.6312	0.5320	0.7149	0.5334	0.6713

Notes: The dependent variable is the change in CLB-recorded events per million workers in prefecture i between year $t-1$ and t, for which the recorded cause is as indicated in the column heading; Columns 1-3 include events across all sectors, while Columns 4-5 include only events that occurred in the manufacturing sector. The "Events per million workers$_{i,t-1}$" variable is the corresponding economy-wide or manufacturing-specific level of CLB events per worker by recorded cause at time $t-1$. All columns report IV regressions, weighted by the prefecture's working-age population in 2010. The additional time-t controls are those used in Column 3 of Table 2, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

might confound the interpretation of our regression findings. To address this, we construct the working-age population-weighted average of $ExpShock$ across all prefectures that share an administrative border with i; when we include this as an additional right-hand side variable, we further instrument for it with an analogous neighboring-prefecture weighted-average measure of $ExpShockROW$. While the estimates in Column 1 point to interesting evidence of spatial spillovers from negative shocks in neighboring prefectures, this does not detract from the importance of the local export shock for explaining the rise in labor incidents in prefecture i itself. In Column 2, we examine if the time-$(t+1)$ export shock might have explanatory power for the incidence of strikes at time t, by replacing the contemporaneous export shock variable in (2) with $ExpShock_{i,t+1}$, and instrumenting for it with the time-$(t+1)$ Bartik variable. Compared to the baseline results in Column 3 of Table 2, the export shock coefficient is smaller in magnitude and statistically insignificant. This helps to allay the concern that our results could be driven by pre-determined trends at the prefecture level in the evolution of exports that are in turn spuriously correlated with labor market outcomes.

Motivated by the anecdotal reports of factory closures, we examine in Column 3 whether the rise in labor incidents can be linked to firm exit induced by the slowdown in exports. To do so, we split $ExpShock_{it}$ into a component that reflects episodes of firm exit from exporting – here, defined as those firms that record positive exports in the customs data (in year $t-1$), but cease

Table 5: Export Shocks and Labor Strikes: Heterogeneous Effects

Dependent variable:	Δ **CLB Events per million**$_{it}$				
	(1)	(2)	(3)	(4)	(5)
	IV	IV	OLS	OLS	IV
ExpShock$_{it}$	-0.2477***				-1.5725***
	(0.0549)				(0.5536)
Neighboring ExpShock$_{it}$	-0.2999**				
	(0.1286)				
ExpShock$_{i,t+1}$		-0.1051			
		(0.0791)			
ExpShock$_{it}^{Exit}$			-0.5799*		
			(0.3261)		
ExpShock$_{it}^{NonExit}$			-0.0308		
			(0.0954)		
ExpShock$_{it}^{NonSOE}$				-0.1612**	
				(0.0742)	
ExpShock$_{it}^{SOE}$				-0.1495	
				(0.7838)	
(Fiscal Pub. Security/L)$_{i,12}\times$ExpShock$_{it}$					1.1844***
					(0.3334)
Share of SOE Emp$_{i,10}\times$ExpShock$_{it}$					0.2896**
					(0.1105)
Share of Non-Hukou$_{i,10}\times$ExpShock$_{it}$					-0.6645**
					(0.2415)
Share of College$_{i,10}\times$ExpShock$_{it}$					-11.5451***
					(3.7128)
Events per million workers$_{i,t-1}$	-1.0743***	-1.0667***	-1.0792***	-1.0309***	-0.9936***
	(0.1249)	(0.1162)	(0.1100)	(0.1345)	(0.1427)
Additional time-t controls?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Province-year dummies?	Y	Y	Y	Y	Y
First-stage F-stat	37.81	24.38	–	–	8.127
Observations	822	822	822	820	807
R^2	0.6568	0.6486	0.6661	0.6624	0.6404

Notes: The dependent variable is the change in CLB-recorded events per million workers in prefecture i between year $t-1$ and t. All regressions are weighted by the prefecture's working-age population in 2010. Columns 1, 2 and 5 report IV estimates, while Columns 3 and 4 are OLS regressions. Column 1 controls for a working-age population weighted-average export shock measure in neighboring prefectures; we use an IV that is the corresponding weighted-average Bartik variable across neighboring prefectures. Column 2 examines whether the time t to $t+1$ export shock has explanatory power for the increase in labor strikes between year $t-1$ and t. Column 3 breaks down the export shock into the contribution from firms that exit from exporting versus stayers/new entrants. Column 4 breaks down the contribution of SOEs versus non-SOEs. Column 5 studies heterogeneous effects across prefectures that differ along initial characteristics. The additional time-t controls are those used in Column 3 of Table 2, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

to export for two consecutive years (in years t and $t+1$) – and a remaining component that corresponds to continuing or new exporters. We use the former as a proxy for the exit margin of the export shock, in the absence of direct data on firm or plant closures; we also present OLS estimates, as it is not straightforward to propose two plausible IVs for the separate components

of $ExpShock_{it}$. We find in Column 3 that it is indeed the exit margin that drives the overall negative correlation between the export shock and a rise in labor strikes. Column 4 presents an alternative breakdown of $ExpShock_{it}$ into that attributable to state-owned enterprises (SOEs) versus non-SOEs (which includes private domestic and foreign-owned firms). That the non-SOE margin accounts for the export shock effect on labor incidents suggests that SOEs played a role during the export slowdown as a buffer for local employment conditions.

Finally, in Column 5, we investigate how the effects of the export shock varied across prefectures that differ along certain initial characteristics. We find quite intuitively that the negative effect of an export slowdown on labor strikes was more muted in prefectures where the local government: (i) exhibited a greater capacity to manage unrest (as proxied by the initial 2012 fiscal expenditure per worker on public security uses); and (ii) accounted for a greater share of employment (as proxied by the share of workers employed in government and party agencies, from the 2010 Census). Conversely, the effect of $ExpShock_{it}$ was exacerbated in prefectures with a larger initial share of migrant workers (as measured by the population share without hukou, from the 2010 Census). This would be consistent with the interpretation that, with restricted access to social security benefits in the prefecture where they work, migrant workers were less protected from export-induced income shocks and hence more prone to strike when economic conditions took a turn for the worse. Lastly, we also find a negative interaction effect between $ExpShock_{it}$ and the population share with at least some college education (from the 2010 Census); this aligns with existing cross-country evidence that periods of economic downturn are more strongly accompanied by rises in political protest in locations that feature higher levels of educational attainment (Campante and Chor 2012).

5.2 Further Robustness Checks

In this subsection, we report on an extensive series of checks, including several exercises that draw on recommendations in the recent literature pertaining to the use of a Bartik IV strategy. We keep the exposition brisk here in the interest of brevity, with details relegated to Appendix B. While the discussion is written around the effects of the export slowdown on the rise in labor strikes, note that the appendix tables report these checks too for the political response outcome variables (i.e., the textual analysis, fiscal spending, and incumbent turnover measures) that the paper will turn to after this section. (Readers who prefer not to dwell on these checks can proceed directly to that material starting in Section 6.)

Basic Checks: We start in Panel A of Table B.1 by replacing the province-year fixed effects in (2) with region-year fixed effects. This allows us to retain in the regression sample several large and economically important prefectures (namely, Beijing, Tianjin, Shanghai, and Chongqing) that would otherwise be dropped, as these are prefectures that make up their entire province. Reverting to province-year fixed effects, Panel B reruns the IV regressions without

weights, while Panel C drops the lag ($Events/L$) variable from the right-hand side. Our baseline results are not overturned by any of these specification checks.

We also address potential concerns about outlier observations. Figure B.1 in the appendix is a residual scatterplot, based on the estimates from Column 3 of Table 2, which provides reassurance that no observation is unduly influential for the negative slope coefficient of the export shock variable. In Table B.2, we further confirm that no individual province is driving the statistical significance of our results, by reporting the range of $ExpShock_{it}$ coefficient estimates when dropping one entire province at a time. (Please see Appendix B.1 for further details).

Other Prefecture-Level Shocks: The interpretation of our results could be undermined if the ROW demand shocks in the Bartik IV were incidentally correlated with demand shocks that originate from within China. If so, our baseline estimates may not be picking up the effects of export demand *per se*. We address this concern by adding a proxy for domestic demand shocks as a control variable. For this proxy, we draw on the China Industry Statistical Yearbooks to compute domestic absorption (i.e., $Absorption_{jt} = Output_{jt} - Export_{jt} + Import_{jt}$) for four-digit Chinese Standard Industrial Classification (CSIC) industries (indexed by j). We then map industry-level changes in absorption ($\Delta Absorption_{jt}$, between years $t-1$ and t) to each prefecture i with the following Bartik-style measure: $AbsorptionShock_{it} = \sum_j \frac{L_{ij,2010}}{\sum_i L_{ij,2010}} \frac{\Delta Absorption_{jt}}{L_{i,2000}}$. Here, $L_{ij,2010}/\sum_i L_{ij,2010}$ is prefecture i's share of industry-j employment (from the 2010 China Annual Survey of Industrial Firms), and $L_{i,2000}$ is the working-age population (from the 2000 Census); see Appendix B.2 for further details and related discussion.[27]

There is an analogous concern that the ROW demand shocks might be correlated with Chinese domestic supply shocks. We address this via a similar approach, by controlling for a Bartik-style measure of prefecture-level output shocks constructed as above, but with $\Delta Output_{jt}$ in place of $\Delta Absorption_{jt}$. Even with these controls, the estimated effect of the export shock remains robust; this holds regardless of whether we add the domestic demand and output proxies separately (Panels A and B, Table B.3), or jointly in the same regression (Panel C).[28] We have similarly constructed a prefecture-level import shock measure – by replacing $\Delta Absorption_{jt}$ with $\Delta Import_{jt}$ in the above definition of $AbsorptionShock_{it}$ – to control for changes in imports amidst the broader trade slowdown.[29] Including this measure of import shocks has little bearing

[27]One could in principle construct absorption at the prefecture level directly with the information on firm-level output and location that would be available in the Annual Survey of Industrial Firms. We instead adopted this approach through industry-level absorption, as the Annual Survey is not publicly accessible (to the best of our knowledge) for the years after 2013.

[28]Figure B.2 illustrates that the correlation between $Absorption_{jt}$ and $Output_{jt}$ on the one hand, and the CSIC industry-level export shock on the other hand, is low; the respective slope coefficients are not statistically different from zero. This provides further reassurance that our export shock variable is unlikely to be picking up the roles of domestic demand or supply shocks.

[29]In principle, an increase in imports may have two offsetting effects on labor unrest, and the overall effect is ex-ante ambiguous. On the one hand, imports could replace local production, which could induce more labor-related unrest. On the other hand, imported intermediate inputs may be complementary to domestic labor, and hence reduce strikes instead.

on the estimated export shock coefficient (Panel D).

Alternative Bartik IVs: We experiment with alternative constructions of the Bartik IV in Table B.4. In Panel A, we show that our results are similar when we exclude exports by intermediary firms from the prefecture-level *ExpShock* and *ExpShockROW* variables.[30] This allays the criticism that changes in exports recorded by intermediary firms may not reflect actual shocks to manufacturing production in the local labor market. In Panel B, we incorporate information on export shocks across destinations by constructing the Bartik IV as: $\sum_k \sum_{d \neq CHN} \frac{X_{idk,2010}}{\sum_i X_{idk,2010}} \frac{\Delta X^{ROW}_{dkt}}{L_{i,2010}}$. Here, ΔX^{ROW}_{dkt} denotes the change in exports of product k from the ROW to country d between years $t-1$ and t, while $X_{idk,2010}/\sum_i X_{idk,2010}$ is the share of China's exports of product k to destination d that accrue to prefecture i in the base year (2010). This in principle exploits variation across destinations in product-level demand shocks in the identification strategy. We next follow Redding and Venebles (2004) to back out importer product-specific demand shocks, recovering these off estimates of importer-year fixed effects from gravity equations that have been run separately for each product. We then compute the implied trade shifts that can be attributed to the evolution of these importer-by-product demand forces, and use these to build two Bartik measures: (i) one that is analogous to the baseline IV in (3); and (ii) a version that makes use of the variation across destinations d, that is analogous to the measure in Panel B above. The results when using these respective gravity-based measures as IVs are reported in Panels C and D. Last but not least for this table, we have also worked with an export shock measure that is based on product-level export growth rates, rather than on dollar changes per worker (Panel E). Our main findings remain unaffected under each of these alternatives to our Bartik IV; please see Appendix B.3 for additional details.

Validating the Bartik Strategy: We carefully address a series of issues that may affect confidence in the Bartik identification approach. Importantly, like all studies employing Bartik-style IVs, one has to establish that the results are not simply due to initial specialization in certain industries that display pre-determined trends, which then are driving the outcomes of interest. For example, labor unrest could be trending up in the textile industry, and hence prefectures specializing in textile products would experience more strikes even in the absence of export shocks. This issue is at the heart of Goldsmith-Pinkham et al. (2018), who emphasize how with Bartik-style IVs, one can view identification as stemming from the exogeneity of the initial shares. Note that the D_i fixed effects in specification (2) already account for prefecture-specific linear time trends in the outcome variable. To further alleviate concern about unobserved supply shocks with a non-linear pre-trend that are associated with certain products, we show that the results are robust to dropping each individual HS section – and reconstructing the $ExpShock_{it}$ measure and $ExpShockROW_{it}$ IV – one at a time (see Table B.5 and Appendix B.4 for details). In addition, we pick up on the test in Column 2 of Table 5,

[30]We follow the approach of Ahn et al. (2011) and drop firms with names containing Chinese characters that are the English-equivalent of "importer", "exporter", and/or "trading".

to show that future export shocks have little explanatory power for contemporaneous outcomes; this holds not just for labor strikes, but also for the set of political response variables we will study (see Table B.6 and Appendix B.5). This indicates that prefecture-specific pre-trends are unlikely to be at the root of our results.

As discussed in Borusyak et al. (2018), the validity of a Bartik IV can be interpreted as relying instead on the assumption that shocks – in our case, at the product level – are as good as randomly assigned. This identification assumption may be violated if export demand decreases more in industries that tend to concentrate in prefectures with certain baseline characteristics that themselves have independent effects on local social stability. We therefore follow Borusyak et al. (2018) and test whether the export shocks are balanced across an exposure-weighted average of initial prefecture characteristics; the characteristics we consider are: the share of workers with college education, share of manufacturing employment, export-to-GDP ratio, share of population without hukou, log GDP per capita, and log fiscal revenue per capita. Table B.7 reports the results of the balance test, and more details are provided in Appendix B.6. It is reassuring that none of the estimated correlations is statistically significant at conventional levels. Moreover, the p-value for the joint test of significance across all six variables is 0.837.

Alternative Statistical Inference: As a baseline, we have reported standard errors clustered at the province level. This allows for arbitrary within-province correlation in the regression error terms, but no cross-province correlation. In the context of Bartik IVs though, Adão et al. (2018) have pointed out that prefectures located in different provinces could experience correlated shocks if they share a similar initial product-level export mix. We have thus verified that the statistical inference we draw is robust under alternative clustering protocols, including a two-way clustering by province and by a separate partitioning of the prefectures based on an export similarity index (see Appendix B.7 and Table B.8 for details).

5.3 Other Labor Market and Economic Outcomes

Our analysis to this point has focused on labor strikes as the key outcome variable. That said, if the effect of the export slowdown we have been describing is indeed operative, we should expect too to observe weakened outcomes in other measures related to employment and output, particularly in the manufacturing sector. Table 6 provides corroborating evidence on this front, with several alternative labor market and economic outcomes that could be constructed from the available data in the China City Statistical Yearbooks. (We adopt the same IV specification as in (2), but replace $\Delta(Event/L)_{it}$ and $(Event/L)_{i,t-1}$ respectively with the change in the economic outcome measure in question and its lag level.)

The message that emerges from Table 6 is consistent with the broader narrative that the export slowdown induced turbulence in the manufacturing sector. We find that a negative export shock was linked with a decline in the ratio of manufacturing employment to prefecture popu-

Table 6: Effect of Export Shocks on other Economic and Labor Market Outcomes

Dependent variable:	**Δ Economic outcome$_{it}$**				
	Share of Mfg. empl. in population	**Share of Non-Mfg. empl. in population**	**Log Industrial output per capita**	**Log Industrial output per worker**	**Log Average Wage**
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
ExpShock$_{it}$	0.0046**	-0.0004	0.0106†	0.0188**	0.0043
	(0.0018)	(0.0012)	(0.0062)	(0.0078)	(0.0036)
Economic outcome$_{i,t-1}$	-1.1614***	-1.4554***	-0.6574***	-1.0209***	-0.9447***
	(0.0861)	(0.0813)	(0.2330)	(0.1496)	(0.0734)
Additional time-t controls?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Province-year dummies?	Y	Y	Y	Y	Y
First-stage F-stat	52.07	101.6	77.14	94.86	155.1
Observations	819	819	822	822	809
R^2	0.9677	0.8052	0.7396	0.7768	0.6897

Notes: The dependent variables are the prefecture-level economic outcomes in the respective column headings; based on data from the City Statistical Yearbooks, these are computed as the change between year $t-1$ and t. All columns report IV regressions, weighted by the prefecture's working-age population in 2010. The additional time-t controls are those used in Column 3 of Table 2, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1, †p<0.15.

lation (Column 1), with no significant effect on the corresponding ratio for non-manufacturing employment (Column 2). Moreover, gross industrial output in both per capita and per worker terms declined in response to the slowdown in manufacturing exports (Columns 3 and 4). We obtain too an effect on average wages that is of the expected sign – a decrease in exports would reduce prefecture wages – although this point estimate is not statistically significant (Column 5). Note though that the average wage measure available in the statistical yearbooks covers in principle workers in all sectors, and so would be a noisy reflection of wage conditions that are specific to the manufacturing sector.

6 Political Response to Export Shocks: A Simple Model

Having established that prefecture-level export shocks prompt the emergence of labor unrest, we now turn to study the response of China's political system. We are interested in the reaction by local leaders who are most directly exposed to the events on the ground, as well as by decision-makers in the upper levels of government.

To organize our thinking on these fronts, we develop a simple model of career concerns following Persson and Zhuravskaya (2014). In the model, a local (prefecture) incumbent can engage in costly measures to bolster social stability when faced with a negative export shock. The local incumbent's performance is in turn evaluated by an upper-level decision-maker (the province or central government), who considers whether to retain or remove its local agent.

While the model is relatively stylized, it nevertheless yields predictions on the level of effort expended by the prefecture leader on stability measures that our subsequent empirical analysis can relate to. It moreover sheds light on the decision rule that would be adopted by the upper-level government in order to best incentivize the local incumbent's actions.

6.1 Setup

Consider a setting with two time periods. In the first period, the prefecture experiences an external export shock denoted by $x \in [0, 1]$; note that x is increasing in the export performance of the prefecture, so that we associate a *lower* value of x with an export slowdown. We thus assume that local stability y is given by:

$$y = x + (1 - x)s + \varepsilon,$$

where the first term, x, reflects the direct effect of the export shock on stability. The second term, $(1 - x)s$, captures how the local government's use of fiscal resources (denoted by s) can counteract the decline in stability that would accompany an export slowdown. We view s as encapsulating various measures that could contribute towards local stability. This includes public security spending to repress unrest ("sticks"), as well as social spending to soften the economic impact on workers ("carrots"), both of which we are able to observe in the prefecture-level fiscal data. We assume the measures s to be particularly effective at bolstering stability when export conditions are weak, or conversely, that these measures are less crucial from the standpoint of domestic stability when the economy is in healthy shape. For example, the general public might perceive social spending as less attractive, and might find the use of repression for its own sake uncalled for, when economic conditions are strong. The final term, $\varepsilon \sim N(0, \sigma^2)$, is an iid stochastic draw, which captures unobserved factors that influence the realized level of stability. We interpret those as encompassing, for instance, the way unobserved leader characteristics interact with local circumstances to affect political stability.

The local party secretary (henceforth, he/him) is appointed for one period, but can be reappointed or replaced for the second. Each period in office affords him rents R. In the first period, after observing the export shock, he decides how much prefecture fiscal resources s to devote to maintaining stability. At the end of the first period, the upper-level government (henceforth, she/her) observes the realized value of stability y, and evaluates the incumbent. We assume that she has complete information about the export shock x; in particular, the export shock itself will not be mis-attributed to the local incumbent's performance. The upper-level government will then follow a threshold strategy, keeping the incumbent if y exceeds $\bar{y}(x)$ – a threshold that depends on the observed shock – while replacing him otherwise from a pool of potential officeholders who are all *ex ante* identical. (For simplicity, the baseline model leaves aside the issue of selection on the basis of ability, and focuses instead on the provision

of incentives to incumbents. We show in Appendix B.8 that the insights still obtain when we allow incumbents to differ in their innate effectiveness in boosting stability, and incorporate formally a concern with screening out low-ability incumbents.)

The officeholder then chooses s in order to maximize:

$$\begin{aligned} U &= \Pr\left(y > \bar{y}(x)\right) R - g(s) \\ &= \left(1 - \Phi\left[\bar{y}(x) - x - (1-x)s\right]\right) R - g(s) \end{aligned}$$

where $\Phi(\cdot)$ denotes the cdf of the normal distribution for ε, and $g(s)$ is the cost of stability-enhancing measures. Note that this cost function satisfies: $g(0) = 0$, $g'(s) > 0$, and $g''(s) \geq 0$ (while being strictly positive over some range of the support of s). The first-order condition for an interior solution is then:

$$\phi\left[\bar{y}(x) - x - (1-x)s\right](1-x)R = g'(s), \tag{4}$$

where $\phi(\cdot)$ is the pdf of the $N(0, \sigma^2)$ normal distribution. Denote by s^* a solution to (4).

Turning to the strategic considerations of the upper-level government, we assume that she is concerned with maximizing the expected stability level, i.e., $E(y) = x+(1-x)s$, which increases with s. Therefore, she can optimize the incumbent's effort towards maintaining stability by suitably choosing $\bar{y}(x)$, such that the argument of $\phi(\cdot)$ is zero, i.e., $\bar{y}^*(x) = x + (1-x)s^*$. Plugging this into equation (4), that first-order condition can be rewritten as:

$$\frac{(1-x)R}{\sqrt{2\pi\sigma^2}} = g'(s^*). \tag{5}$$

6.2 Model Predictions

We consider the model's implications for two political outcomes which our data will speak to, namely: the effort expended by the local incumbent on stability measures, and the likelihood of replacement by the upper-level government.

Incumbent Effort: Let us assume for simplicity that $g(s) = \frac{\delta s^2}{2}$, where $\delta > 0$ is a parameter that governs the marginal cost of effort. This yields:

$$s^* = \frac{(1-x)R}{\delta\sqrt{2\pi\sigma^2}}, \tag{6}$$

which renders the following predictions. In response to negative export shocks:

(i) expenditure on stability measures increases ($\frac{ds^*}{dx} < 0$);

(ii) the increase in expenditure on stability measures is greater for incumbents who obtain a higher rent R ($\frac{d^2s^*}{dxdR} < 0$); and

(iii) the increase in expenditure on stability measures is greater for incumbents with a smaller cost of fiscal resources, δ ($\frac{d^2 s^*}{dx d\delta} > 0$).

Intuitively, a negative export shock increases the need for and effectiveness of spending to bolster social stability, which delivers (i). Moreover, an incumbent who can expect higher rents will have a magnified response to negative shocks, which yields prediction (ii). Similarly, incumbents who incur a lower fiscal cost of implementing stability measures will respond more strongly, as per (iii).[31] As we will see in Section 7.2, we will relate these latter two predictions to several observed prefecture and incumbent characteristics in our empirical exploration.

Turnover: Under what circumstances will the local incumbent be replaced? It is straightforward to see that $y - \bar{y}^*(x) = \varepsilon$, from which it follows that the likelihood of turnover is equal to $\Phi(-\varepsilon)$ and does not depend on the export shock x. This is because the threshold $\bar{y}^*(x)$ adjusts in response to the export shock, in order to properly incentivize the local incumbent to exert effort to curb instability when exports have been hit by a slowdown. In other words, the above framework implies that a local incumbent is removed not because of an adverse external export shock *per se*, but rather because of "excess" strikes above what the upper-level government adjudges to be tolerable, i.e., $y - \bar{y}^*(x)$. This distinction is particularly relevant in our empirical setting, since we seek to exploit variation from export shocks originating in the rest of the world, that would in principle be outside the control of local incumbents. We will thus take this notion of "excess strikes" seriously when we look empirically at the determinants of incumbent turnover in Section 8.

Note that this baseline model has the implication that the local leader's prospects for retention are driven entirely by the realization of a stochastic draw for ε. This is a feature that can be relaxed, as shown in the extension in Appendix B.8, where the upper-level government's desire to select high-ability incumbents will induce the latter to raise their effort on bolstering local stability, which in turn lowers their likelihood of being replaced. The crucial insight is preserved though, namely that the threshold for removal of an incumbent will take into account the circumstances under which he operates.[32]

7 Political Response: Preserving Social Stability

We now turn to the empirical investigation on the political reaction to the export slowdown and the consequent rise in labor unrest. We document here evidence confirming that these developments triggered a more acute concern over social stability, both from the public at large and from local officeholders. The latter was reflected in an increased emphasis on matters of

[31]Predictions (ii) and (iii) would hold under more general effort cost functions. In particular, it is sufficient (but not necessary) that the marginal cost $g'(s)$ be convex at the equilibrium effort level s^*.

[32]This relates to the literature that has investigated (in democracies) whether incumbents are punished for outcomes beyond their control. See the discussion in Fowler and Hall (2018) and Achen and Bartels (2018).

law and order in prefecture annual work reports. This was accompanied by actual increases in fiscal spending on stability measures, in line with the model's predictions.

7.1 Attention to Preserving Stability

We adopt a novel approach to measure the degree of attention paid at the prefecture level towards the issue of public security, that is based on the use of key political phrases – in particular, "weiwen" (in Chinese, "维稳") – in the public domain. The term "weiwen" is a contraction of "维护稳定", which literally translates as "maintaining stability"; it was reportedly first used in the official People's Daily newspaper in 2002, in an article that was accompanied by a photograph of armed police. Since then, the term "weiwen" has been adopted as a watchword by the political authorities, and is widely used to refer to actions to maintain law and order in the interest of preserving domestic stability (*New York Times*, 2012).

We will make use of the above observation in two ways, to construct measures that will be amenable to empirical analysis. First, we investigate the response of internet search volumes for the term "weiwen" at the prefecture level in the aftermath of a local strike event, as an indicator of the attention paid to domestic security issues by the general public. This will help validate the premise that the public associates the occurrence of labor strikes with concerns about stability. Second, we use this term as the basis for a textual analysis of prefecture annual work reports, to measure the degree to which preserving social stability features as a political priority of the local government; in particular, we explore whether there is a systematic shift in "weiwen" emphasis following a negative export shock.

7.1.1 Public Concern: Baidu Search Index

We follow a growing body of empirical work in the economics literature that has used data on the intensity of internet searches, based for example on such metrics as Google Trends, to gauge the pattern of internet users' interests and attitudes on various socioeconomic or political issues (Madestam et al. 2013; Stephens-Davidowitz 2014; Kearney and Levine 2015). For our purposes, Google Trends is unlikely to reflect the true search volume among domestic Chinese internet users, due to the fact that access to Google has been severely curtailed in mainland China since 2010. We thus turn instead to the counterpart of Google Trends on the largest search engine in China, Baidu. Note that Baidu's market share is typically estimated to be between 60-70% of internet users in China.[33]

The Baidu Index allows users to retrieve information at a weekly frequency on the volume of search queries for specific keywords, and can moreover distinguish searches by the prefectures from which they originate. Although Baidu does not publicly disclose the exact formula for

[33] For example, see: https://www.wsj.com/articles/bing-baidu-and-a-big-mess-for-chinese-search-engines-11548328142

its index, prior researchers have verified that the Baidu Index is likely to be linearly correlated with the volume of public searches recorded for a given keyword (Qin and Zhu 2017).[34] We therefore scrape the Baidu Index for the keyword "weiwen", both over time and by prefecture.[35]

We demonstrate that public attention to "weiwen" is indeed related to the occurrence of strikes, by exploiting the rich weekly dimension of the CLB strike data together with the above Baidu "weiwen" index. The structure of the merged data lend themselves to an event-study analysis, which we implement in the following regression:

$$\Delta \ln(SearchIndex)_{i,w} = \sum_{l=-2}^{6} \lambda_l \Delta(Events/L)_{i,w-l} + \lambda \ln(SearchIndex)_{i,w-1} + D_{p,w} + D_i + \varepsilon_{i,w}. \tag{7}$$

Here, $\Delta \ln(SearchIndex)_{i,w}$ is the change in the log Baidu "weiwen" index in prefecture i and week w (i.e., relative to week $w-1$). We regress this against a set of leads and lags of the change in CLB events per worker observed in that prefecture (where $\Delta(Events/L)_{i,w-l} = (Events/L)_{i,w-l} - (Events/L)_{i,w-l-1}$), as well as against the lag level of the search index itself in week $w-1$. The $D_{p,w}$ and D_i denote province-by-week and prefecture dummies respectively. The flexible lead-and-lag structure allows us to track the dynamic effects of incidents of labor strikes on public attention to "weiwen"-related issues. We estimate the above for a panel of weekly observations spanning 2012-2015, although the results are very similar if we were to expand the sample to 2011-2016 (available on request).

Figure 4 illustrates the estimates of the λ_l's from (7), together with the 90% confidence intervals; these are based on standard errors clustered by prefecture, to account for potential serial correlation in how the intensity of strikes might evolve in a given location. We find that the search volume for the term "weiwen" is statistically indistinguishable from zero in the weeks leading up to a labor strike. Of note, this search volume picks up a week after a strike incident is recorded in the prefecture, with the reaction in the Baidu index then persisting for up to six weeks. (The full set of coefficient estimates is reported in full in Table B.9 in the appendix. There, we show that the above pattern in the λ_l coefficients is robust even if one were to drop the prefecture fixed effects, or if one were to estimate the regressions with working-age population weights.)

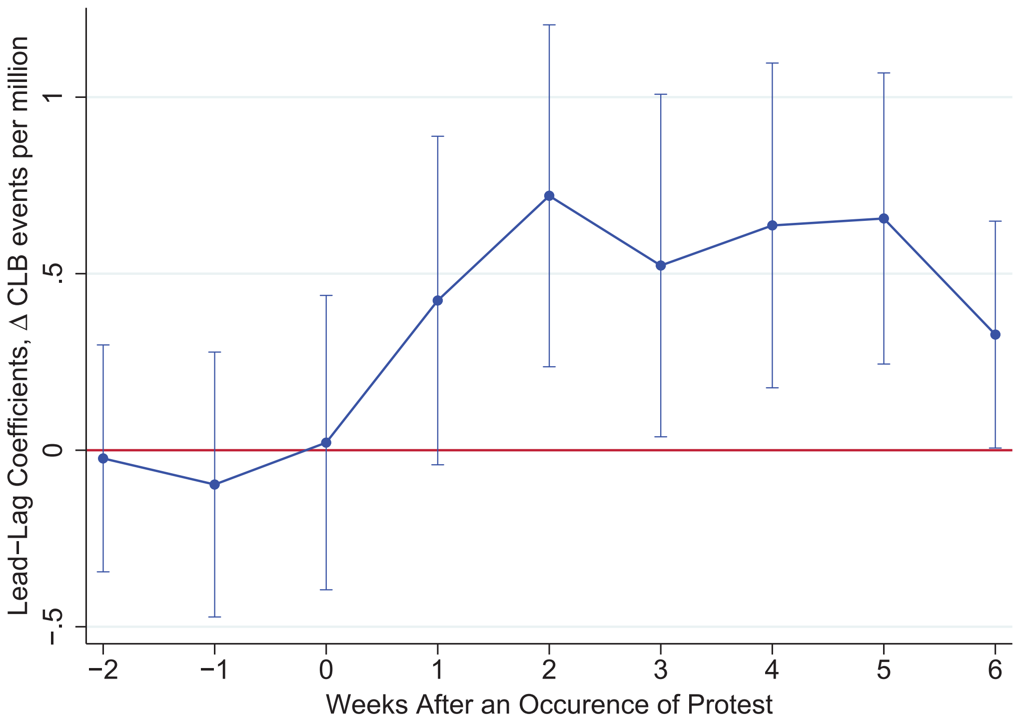
The above finding provides evidence of a significant response in terms of an increase in

[34] By contrast, Google Trends reports a "relative search index", which is the ratio of the search volume for a given term to the total number of searches conducted in a particular time/location. The index is then normalized with the most popular search term taking on a value of 100. The publicly-available information from Google Trends thus only has an ordinal interpretation (Kearney and Levine 2015). On the other hand, the Baidu Index appears to be a cardinal measure, which facilitates making comparisons across time and location.

[35] In practice, the process of retrieving the Baidu Index data was a laborious one. First, Baidu places limitations on the volume of information that can be accessed by a single user, so that the scraping process could not be fully automated and had to be reset periodically. Second, the Baidu Index comes in the form of a graphed time series, so an algorithm was written to read the value of the index off the pixels in the graphics file for each prefecture.

attention paid by netizens to the issue of public security following local incidents of labor strikes. This could reflect for example a rise in concern about the law and order situation in one's prefecture, or an increase in internet searches related to media coverage of a new "weiwen" policy to bolster stability. At a more basic level, this exercise also serves to validate the use of "weiwen" as a keyword, in that the frequency of its use can help shed light on the intensity of responses to local unrest events.

Figure 4: Temporal Correlation between Baidu "Weiwen" Search Index and CLB Events



7.1.2 Local Government Concern: Annual Work Reports

We next extract information on the political emphasis on maintaining social stability from an official document of the local government, namely the prefecture annual work report. Within China's political system, this is a report delivered as a speech at the prefecture-level People's Congress meeting usually held in January each year. The reports are relatively uniform in their format, which is helpful for our implementation of a textual analysis. Specifically, each report comes in two sections. The first section is a summary of socioeconomic conditions from the preceding year, together with a list of the government's accomplishments. On occasion, this material will mention instances of high-profile strikes or unrest events that drew the government's attention. The second section then lays out development policies for the year ahead. Apart from describing economic plans, this will include measures intended to check and mitigate social unrest (i.e., "weiwen" actions) in prefectures where this may be a relevant issue.

We use two different approaches to construct measures of a work report's emphasis on preserving social stability. Our more basic approach involves a simple count of "weiwen"-related keywords. For this, we scan each prefecture's work report for each year between 2013-2016, and count the number of occurrences of eleven keywords. This list of keywords naturally contains "weiwen" ("维稳"), its unabbreviated form ("维护稳定"), and several variants (e.g., "和谐稳定" or "harmony and stability", "安全稳定" or "safety and stability"); it also includes several synonyms for public security (e.g., "公共安全"). (The full list of keywords and their translation is in Table A.1.) The keyword count is then normalized by the total count of Chinese characters in the associated government work report.

We also implement a more sophisticated machine-learning approach to compute "weiwen" scores for each report. For this, we first randomly selected 20 reports from a pre-sample year (2011), to mark out all sentences as either being about "weiwen" or "not-weiwen". These labelled passages were then used, together with a paragraph from a national-level State Council document dated April 2015 on the topic of domestic security measures, as the training sample for the machine-learning algorithms.[36] We then tokenize the text of each annual work report using an online Chinese word library, before applying two machine-learning algorithms: (i) the Multinomial Naive Bayes (MNB), and (ii) the Support Vector Machine (SVM), to predict whether a paragraph is related to the topic of maintaining social stability. More specifically, the MNB model generates a posterior probability of a paragraph being "weiwen", from an underlying multinomial distribution model of token frequencies. The SVM model on the other hand is a binary classifier, that generates a 0-1 prediction for whether a paragraph is about "weiwen" or not, after partitioning out the observations in a high-dimensional metric space. (See Appendix A.4 for more technical details.) We then compute a report-level score, by taking the character-length weighted-average of the paragraph-specific scores. We view these "weiwen" scores as capturing the degree to which maintaining social stability is an active policy priority for the local government in the prefecture in question.[37] As a placebo test, we have checked the predictions that the machine-learning algorithms deliver for paragraphs that are related to tackling economic volatility (such as in the stock market or real estate prices), given that the same Chinese phrase ("稳定") is also used in references to economic stabilization policies. Both MNB and SVM models returned "weiwen" scores that were close to zero for such passages, verifying the algorithms' ability to discriminate between content related to economic versus political stability.

[36]The State Council document was entitled "Opinions on Strengthening Society's Public Security Prevention and Control System", and provides a set of recommendations on "weiwen" measures. See: http://www.gov.cn/xinwen/2015-04/13/content_2846013.htm

[37]Our approach thus makes use of "supervised" machine-learning algorithms, in that we train the algorithm to recognize "weiwen" versus "non-weiwen" passages instead of allowing it free rein to identify textual associations. This is similar to the approach in Gentzkow and Shapiro (2010), who use a keyword approach to identify the political slant of U.S. newspapers. For other applications of machine-learning methods to classify free text in empirical research in economics, see the survey article of Mullainathan and Spiess (2017).

With these textual analysis measures, we then estimate the following regression model to examine whether export shocks induced a political response in terms of "weiwen" emphasis:

$$\Delta y_{i,t+1} = \gamma_1 ExpShock_{it} + \gamma_2 y_{it} + \gamma_X X_{it} + D_{pt} + D_i + \varepsilon_{it}. \tag{8}$$

This specification is very similar to that in (2), with the key difference being that a textual analysis measure (denoted by y) is now being used in place of the CLB events variable. Note that the regression seeks to explain changes in this political response variable between years t and $t+1$, as a function of the export shock in the preceding year. We lead the response variable on the left-hand side by one period for two reasons. First, outcomes that involve political actions would naturally respond to adverse economic shocks with some lag. Second, the prefecture work reports are delivered at the start of each calendar year, with the content and wording influenced by socioeconomic conditions in the preceding year. We therefore relate political emphasis on stability that is quantified from work reports in year $t+1$ to export shocks in year t. As before, we instrument for the export shock with the Bartik IV from (3), while controlling for province-year and prefecture fixed effects; the regressions are weighted by the prefecture working-age population in 2010, with standard errors clustered by province.

Table 7 presents the results from this analysis of the prefecture annual work reports. For each measure, the odd-numbered columns report a basic specification without prefecture time-varying controls (X_{it}), while the even-numbered columns include the prefecture-level changes in the college-enrolled, mobile-use and internet-use shares, as seen earlier in Column 3 of Table 2. We obtain a consistent pattern regardless of the textual-analysis dependent variable or auxiliary controls adopted, namely that a negative export shock raises the emphasis placed by local officeholders on maintaining social stability, to the extent that these are reflected in the annual work reports they deliver. (We have also repeated the full set of checks described earlier in Section 5.2 to assess the robustness of these effects of the export shock on "weiwen" emphasis; these are reported using the MNB "weiwen" measure in Column 2 of the corresponding Appendix B tables.) The above findings therefore underscore the political importance that the prefecture governments attach to upholding public security and stability in response to the export slowdown.

7.2 Fiscal Expenditure

The analysis from the previous subsection is useful for identifying the announced intentions of the local government, but does this translate into the allocation of tangible resources towards maintaining social stability, as suggested by our framework from Section 6? We turn to this issue now by studying how the export slowdown affected the use of fiscal resources at the prefecture level.

Toward this end, we collected data on realized fiscal expenditures and their detailed structure

Table 7: Export Shocks and "Weiwen" Emphasis

Dependent variable:	Δ **Textual "weiwen" score**$_{i,t+1}$					
	Share of keywords	**Share of keywords**	**Log MNB**	**Log MNB**	**Log SVM**	**Log SVM**
	(1)	(2)	(3)	(4)	(5)	(6)
	IV	IV	IV	IV	IV	IV
ExpShock$_{it}$	-0.0023*	-0.0021†	-0.1600**	-0.1904**	-0.1535***	-0.1714***
	(0.0012)	(0.0012)	(0.0772)	(0.0725)	(0.0545)	(0.0577)
Textual "weiwen" score$_{it}$	-1.2997***	-1.3176***	-29.6643***	-31.3318***	-41.1034***	-41.7952***
	(0.0421)	(0.0360)	(3.6840)	(4.1088)	(3.0013)	(3.0813)
Additional time-t controls?	N	Y	N	Y	N	Y
Prefecture dummies?	Y	Y	Y	Y	Y	Y
Province-year dummies?	Y	Y	Y	Y	Y	Y
First-stage F-stat	64.41	103.2	52.63	77.54	61.85	97.54
Observations	923	802	923	802	923	802
R^2	0.7706	0.7671	0.5022	0.5146	0.5938	0.6022

Notes: The dependent variable is the change in textual "weiwen" score in prefecture i between year t and $t+1$ (i.e., one year after the export shock). All columns report IV regressions, weighted by the prefecture's working-age population in 2010. Columns 1 and 2 regress the change in "weiwen" keyword share against the initial level of the keyword share. Columns 3 and 4 regress the change in log Multinomial Naive Bayes (MNB) score against the initial level of the MNB score. Columns 5 and 6 regress the change in log Support Vector Machine (SVM) score against the initial level of the SVM score. The additional time-t controls in even-numbered columns are those used in Column 3 of Table 2, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1, † p<0.15.

by spending categories. There is no one-stop repository of local-level fiscal data for China (to the best of our knowledge), and so these data were gathered from several sources. The majority of the data are from the Fiscal Statistical Yearbooks published by the provincial Bureau of Finance, and the Statistical Yearbooks published by the provincial Bureau of Statistics, from provinces across China. These are supplemented with information from prefecture statistical yearbooks, as well as balance sheets from prefecture government websites. In all, we were able to gather data for up to 95% of the prefecture-year observations in our sample. Note that subnational governments in China are responsible for 85% of government spending (Wingender 2018), and so are a meaningful locus of decision-making over the use of fiscal resources.

We focus our attention on two broad categories of spending that capture measures to bolster local stability. The first is spending on public security uses. This includes all expenses by the People's Armed Police, public security organs, court system, judicial system, and prosecutorial system. On the other end of the spectrum, we consider other forms of expenditures – which we place under the label of "social spending" – that could in principle assuage citizens' discontent and thus bolster stability. These include: public services, education, social security, medical services, and public housing. To give a sense of how these spending items compare against each other, the share of prefecture fiscal expenditure on public security averaged 5.13% during the period 2013-2016. By contrast, the average share on social spending was 54.24%, with the largest components of this being spending on education (17.8%), social security (12.59%), and

public services (10.10%).[38]

We follow the IV specification in (8) to assess the impact of export performance on patterns of fiscal spending at the prefecture level. Specifically, we regress changes in log fiscal spending in year $t+1$ on export shocks in the prior period (year t); in other words, we use the log of each expenditure item in turn as the variable y in equation (8). We report the estimates from these in Panel A of Table 8.

In Column 1, we demonstrate that total spending on stability measures – summed over public security and social spending – indeed rises in response to a negative export shock. This increase is moreover statistically significant for each component, when we consider public security (Column 1a) and social spending (Column 1b) separately. In terms of magnitude, the estimated coefficients imply that a one standard deviation worse export shock ($\approx$841 USD per worker) would prompt a 1.8% increase in public security spending, which is slightly larger than the corresponding 1.3% increase in social spending. We further find in Column 2 that the response of other forms of spending (i.e., summing over all categories not related to public security or social spending) is less pronounced, even while Column 3 confirms that a bad export shock would induce a significant rise in total prefecture fiscal expenditures.[39] Put otherwise, fiscal policy at the local level is on the whole counter-cyclical with respect to the prefecture's export performance, and this is mostly driven by the increase in spending on items that can be associated with measures to raise stability.[40]

Panel B of Table 8 delves into whether the effects of export shocks on fiscal spending patterns might differ systematically across prefectures. We focus our discussion here on the findings in Columns 1a and 1b for public security and social spending respectively. We consider first an interaction between the export shock and the increase in CLB-recorded strikes per worker observed in the previous year, $\Delta(Events/L)_{it}$, in order to explore whether the fiscal spending effects might have been prompted (at least in part) by concerns over labor disputes.

[38] Public security and social spending thus constituted around 60% of the total expenditures for the average prefecture. The main remaining expenditure items are: on agriculture, forestry, and water conservancy; on transport; and on urban and rural community affairs. These spending categories are arguably less relevant for mitigating labor unrest. Moreover, production in the transport sector in principle complements export activities, and hence fiscal expenditure on these items may increase mechanically when the export sector expands.

[39] In the tables in Appendix B, specifically in Columns 3 and 4, we document the robustness of these findings – for log changes in spending on public security and log changes in social spending – to the various concerns discussed in Section 5.2. Separately, Table B.10 presents the results when we work instead with the spending items expressed as shares of total expenditure; we arrive at a similar set of conclusions. Table B.11 in turn breaks down social spending further into its individual components; we find here that an export slowdown tends to raise most forms of social spending, with the exception of expenditures on social security.

[40] In relation to this, we have found that locally-raised fiscal revenues (i.e., excluding transfers from the central government) also increase during an export slowdown (results available on request). This suggests that the local authorities in China do possess the fiscal tools and capacity to increase revenues, in support of a rise in discretionary spending. See for example Chen (2017), who presents evidence from an earlier episode showing that Chinese counties were able to clamp down on tax evasion or undertake land sales, in the face of a potential revenue shortfall. This is in contrast to the situation in the United States highlighted by Feler and Senses (2017), where negative trade shocks tightened local budgets and thus hurt the provision of local public goods.

Table 8: Export Shocks and Prefecture Fiscal Expenditure

Dependent variable:	Δ **Log Fiscal measure**$_{i,t+1}$				
Fiscal measure:	**Stability Measures**	**Public Security**	**Social Spending**	**Other Spending**	**Total Expenditure**
	(1)	(1a)	(1b)	(2)	(3)
	IV	IV	IV	IV	IV
	Panel A: Average Effects				
ExpShock_{it}	-0.0163***	-0.0214***	-0.0160**	-0.0103	-0.0114***
	(0.0054)	(0.0069)	(0.0059)	(0.0063)	(0.0040)
Log Fiscal Measure_{it}	-0.9701***	-0.9356***	-0.9590***	-0.7744***	-0.7446***
	(0.0493)	(0.0551)	(0.0492)	(0.0516)	(0.0727)
First-stage F-stat	189.1	117.9	183.7	133.1	98.01
Observations	755	812	760	755	817
R^2	0.7801	0.7747	0.7805	0.8103	0.8050
	Panel B: Heterogeneous Effects				
ExpShock_{it}	0.0633***	-0.0518**	0.0815***	0.0840**	0.0608**
	(0.0143)	(0.0231)	(0.0166)	(0.0348)	(0.0232)
$\Delta(\text{Events}/L)_{it} \times \text{ExpShock}_{it}$	-0.0149***	-0.0100***	-0.0160***	-0.0186***	-0.0171***
	(0.0023)	(0.0020)	(0.0026)	(0.0062)	(0.0045)
$(\text{FiscalRev}/L)_{i,2012} \times \text{ExpShock}_{it}$	-0.0335***	0.0337**	-0.0440***	-0.0392**	-0.0262**
	(0.0082)	(0.0134)	(0.0093)	(0.0182)	(0.0114)
$(49 \leq \text{Age} \leq 53)_{it} \times \text{ExpShock}_{it}$	-0.0228***	-0.0231*	-0.0229***	-0.0164	-0.0157
	(0.0055)	(0.0126)	(0.0055)	(0.0178)	(0.0109)
$\Delta(\text{Events}/L)_{it}$	0.0009	-0.0001	0.0013	0.0005	0.0010
	(0.0021)	(0.0039)	(0.0021)	(0.0055)	(0.0031)
$(49 \leq \text{Age} \leq 53)_{it}$	-0.0132**	-0.0106	-0.0128*	-0.0167	-0.0135*
	(0.0062)	(0.0102)	(0.0069)	(0.0131)	(0.0071)
Log Fiscal Measure_{it}	-0.9663***	-0.9665***	-0.9554***	-0.7954***	-0.7823***
	(0.0435)	(0.0565)	(0.0446)	(0.0594)	(0.0667)
First-stage F-stat	17.03	18.36	18.08	12.93	14.01
Observations	755	812	760	755	817
R^2	0.7841	0.7647	0.7832	0.8002	0.7944
Additional time-t controls?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Province-year dummies?	Y	Y	Y	Y	Y

Notes: The dependent variable is the change in log fiscal expenditure under the respective column headings in prefecture i between year t and $t+1$ (i.e., one year after the export shock). All columns report IV regressions, weighted by the prefecture's working-age population in 2010. Panel A reports the average effects of the export shock on the respective fiscal spending measures. Panel B explores heterogeneous effects: The $\Delta(\text{Events}/L)_{it}$ variable is the change in CLB-recorded events per million between year $t-1$ and t. $(\text{FiscalRev}/L)_{i,2012}$ is the local fiscal revenue per worker in 2012. $(49 \leq \text{Age} \leq 53)_{it}$ is a dummy variable for whether the prefecture party secretary is between ages 49 and 53 (inclusive) in year t. The additional time-t controls are those used in Column 3 of Table 2, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

The estimated interaction coefficients are negative and significant in both Columns 1a and 1b, indicating that fiscal responses to a negative export shock are stronger for both "stick" and "carrot" measures when there is a more severe increase in labor strikes, and hence (potentially) a greater threat to social stability.

Motivated by our framework from Section 6, we also consider whether the impact of the

export shock might vary with the career prospects of the local incumbent and with local fiscal capacity. On the former, we explore an interaction term involving a dummy variable for whether the prefecture party secretary is between ages 49-53 in year t. As established in Appendix A.5, the likelihood of promotion for a local party leader peaks between these ages, and we thus take this dummy as a proxy for officeholders who have higher expected rents (R). We find that its interaction coefficient with the export shock is significantly negative for both public security and social spending. Politicians with greater promotion prospects are thus more likely to increase spending on measures to bolster stability following a bad export shock, in line with the logic of our model.

On initial fiscal capacity, we use as a proxy the level of fiscal revenue per worker in 2012. We find that this interaction effect with negative export shocks is negative and significant for total spending on stability measures (Column 1). Prefecture governments who face fewer fiscal constraints thus raise their spending on stability measures more strongly, consistent with the implications of our model (associated in particular with a lower δ). Interestingly, we also find that local governments with deeper fiscal pockets are less inclined to raise spending on public security following a negative export shock (Column 1a), and more inclined instead to raise social spending (Column 1b). While our stylized model does not fully capture this heterogeneous response across different forms of spending, we hypothesize that this pattern could be related to social spending requiring a more sustained or intensive use of fiscal resources than a short-term ramping up of public security.

8 Political Response: Incumbent Turnover

As a final piece of empirical evidence, we examine the response of the upper-level government, in the decisions that it makes over whether to retain or replace local party leaders. The model in Section 6 would imply that prefecture party secretaries would be assessed on their relative performance in maintaining social stability when faced with a negative export shock. We seek to understand the extent to which the observed career paths of party secretaries during the export slowdown period is in line with these predictions.

8.1 Data and Specification

We collected information on the biographic characteristics and career histories of local party secretaries from the curricula vitae of these officeholders. These were compiled primarily from the database of Chinese party and government leaders maintained by People.cn, an official website affiliated with the Chinese government.[41] The data cover 544 individuals who held the

[41]See: http://ldzl.people.com.cn/dfzlk/front/firstPage.htm. We supplement this with information from Wikipedia where necessary.

position of prefecture party secretary over the period 2013-2016, and allows us to track the month and year in which each individual took and/or left office.

Our empirical analysis focuses on turnover of party secretaries. This is because the party secretary is the top executive position at the prefecture level, with ultimate authority and substantial discretion over local fiscal and regulatory policies (Persson and Zhuravskaya 2016). In particular, the party secretary is in charge of personnel and other political duties such as maintaining social stability, while the mayor (the second in rank) is in charge of the daily operations of the government (Yao and Zhang 2015). To the extent that the party secretary bears greater responsibility for local political stability, his/her career trajectory could be more susceptible to any social unrest associated with negative economic shocks.

We define $Turnover_{it}$ to be an indicator variable equal to 1 when there is a change in party secretary in prefecture i within a given calendar year t. Over 2014-2016, the average annual turnover rate for prefecture party secretaries was 29.6% (see Table 1). We further classified the nature of each instance of turnover as: a promotion, a lateral movement, or being due to other causes (e.g., corruption, retirement, movement to an honorary position). We are helped here by the fact that China's political system has a clear hierarchy of positions that can be ranked in terms of political importance. This starts at the top with national-level positions, followed in descending order by positions at the sub-national, province, sub-province, prefecture, and sub-prefecture levels. In our coding, we define a promotion as a move by a prefecture party secretary to a post that is at the sub-provincial level or above, while a lateral movement is a transfer to a different prefecture-level position.[42] Based on this criterion, 25% of the instances of turnover during 2014-2016 are promotions, and 55.1% are lateral movements. The remaining cases are a combination of retirements or terminations of political career (e.g., due to corruption). There were in fact no cases where an individual was demoted from prefecture party secretary to a position at the sub-prefecture level or below.

The fact that there are no observed demotions (and relatively few cases of termination) suggests that punishment for weak performance takes a different form within China's political system. To capture this, we further break down the nature of the lateral movements in our data. Existing guidelines indicate that prefectural officials should typically have served at least three years in a position, before being promoted to the next level in the political hierarchy; this

[42]There are a number of exceptions to this coding rule, as we detail in Appendix A.5. First, there are a handful of more prominent prefectures designated as provincial-level or sub-provincial-level administrative units; the party secretary positions in these locations are thus of higher rank, and we take care to define movements into and out of these positions with this higher rank in mind. Second, we do not categorize appointments to several honorary positions as promotions, even though these are nominally of sub-province rank. This follows Li and Zhou (2005) and Yao and Zhang (2015), who categorize appointments to the position of chairman or vice-chairman of the province-level People's Congress or People's Political Consultative Committee to be akin to "consolation prizes" or retirement posts; our results are robust if we were instead to treat these movements as promotions. Third, some prefecture party secretaries simultaneously hold positions that rank at the sub-provincial level (e.g., member of the provincial standing committee); for such cases, we consider a movement to another position at the sub-provincial level (e.g., a vice-provincial governor) as a lateral movement.

is announced for example in the *Regulations for the Selection and Appointment of Party Cadres*, by the Organizational Department of the Chinese Communist Party.[43] We therefore label lateral moves that occurred prior to the three-year mark in the prefecture party secretary's tenure as cases of "early" lateral movement. In our sample period, 31.1% of the lateral movements are classified as "early". In Appendix A.5, we provide empirical evidence confirming that among officeholders who had been moved laterally, those who were moved early had a lower likelihood of future promotion compared to those who had served in their prior positions for the requisite three years.[44] We therefore adopt this definition of a premature lateral move as a proxy for a *de facto* demotion, since it tends to blemish an official's career trajectory.

Using this data on incumbent turnover, we estimate the following:

$$Turnover_{i,t+1} = \theta_1 ExpShock_{it} + \theta_2 Turnover_{it} + \theta_X X_{it} + D_{pt} + D_i + \varepsilon_{it}. \tag{9}$$

This follows closely the specification in (8), in that we investigate how the political response in year $t+1$ – in this case, the decision of an upper-level government over whether or not to replace a local officeholder – might depend on the export shock seen in the prefecture in year t. As before, we instrument $ExpShock_{it}$ with the Bartik IV from (3), while controlling throughout for province-year and prefecture fixed effects. We additionally include a set of incumbent characteristics that we are able to extract from their curricula vitae, namely: gender, age, education (whether he/she possesses a Masters degree or higher), tenure in the position (in years), as well as province of birth. We use this last piece of information to construct a dummy variable for whether the incumbent's current appointment is in her birth province, as a proxy for the strength of his/her ties with the local political networks.

To better understand the links between negative export shocks and political turnover, we also study the differential effects across prefectures that experienced high versus low "excess strikes". Recall that in the theoretical framework presented in Section 6, the upper-level government is cognizant that an export slowdown would induce a rise in worker-related unrest. In order to properly incentivize local leaders to exert effort to bolster stability even when there has been a bad export shock, the upper-level government's decision over whether to replace the incumbent would have to depend not on the absolute level of strikes observed, but on the extent to which the increase can be seen as excessive. To capture this, we compute the residuals from a reduced-form regression of $\Delta(Events/L)_{it}$ on the Bartik-style ROW export shock variable; to be more specific, we take these residuals from the regression in Column 4 of Table 2. We then split prefectures into high and low "excess strike" groups based on whether the associated regression residual is respectively above or below its median value. This provides us with a proxy for whether the local authorities have respectively under- or over-performed

[43] See: http://www.people.com.cn/GB/shizheng/16/20020723/782504.html.

[44] This analysis is based on a sample of prefecture party secretaries who experienced a lateral movement during 2007-2012, and considers their observed career histories up until 2016 where our data end.

in their handling of the labor strike situation, relative to a benchmark that takes into account observed ROW export shocks. We then augment the regression model in (9) as follows:

$$\begin{aligned} Turnover_{i,t+1} &= \sum_{g=H,L} \theta_g^{Exp} \mathbf{1}(i \in g) \times ExpShock_{it} + \theta_2 Turnover_{it} + \theta_H \mathbf{1}(i \in H) \\ &\quad + \theta_X X_{it} + D_{pt} + D_i + \varepsilon_{it}, \end{aligned} \tag{10}$$

so that the coefficients θ_H^{Exp} and θ_L^{Exp} pick up the differential effects of the export shock across these two categories of prefectures (as captured by the dummy variables $\mathbf{1}(i \in g)$, with H for the high and L for the low excess strike groups respectively).

8.2 Results

Table 9 reports our key results on incumbent turnover. Column 1 presents the estimates from (9), showing that the incumbent party secretary was indeed more likely to be replaced following a downturn in prefecture exports. In particular, a one standard deviation more negative export shock would raise the likelihood of turnover by 6.2 percentage points, a fairly sizeable effect when compared against the average turnover rate of around 29.6% in our sample period. (As reported in Column 5 of the Appendix B tables, this link from the export shock to incumbent turnover is robust under the alternative specifications and checks discussed in Section 5.2.)

The results from estimating (10) are reported in Column 2. In prefectures with low excess strikes, an export slowdown has a negative but statistically insignificant effect on political turnover. Importantly, the estimated effect of the export shock is much larger in magnitude and highly significant in prefectures with high excess strikes. This is consistent with our model's prediction that an incumbent would be replaced not because of the exogenous export shock *per se*, but rather on the basis of his/her *relative* performance in maintaining social stability.[45] This stands in interesting contrast with evidence from democratic countries where voters have been seen to punish politicians for economic circumstances that are outside their control (Achen and Bartels 2004, Cole et al. 2012). One possible explanation is that relative to the voters in a democracy, the upper-level government in China may hold more precise information or be more able to process that information to evaluate the performance of its officeholders.

Column 3 augments the model with an interaction term between the export shock and the indicator for whether the incumbent is between 49 and 53 years of age. The estimated coefficient is positive and significant, implying that young and potentially more politically-motivated incumbents are less likely to be replaced after an adverse export shock, possibly

[45] We have separately run a regression similar to (10), but in which $\Delta(Events/L)_{it}$ was itself interacted with $ExpShock_{it}$, in lieu of the interaction terms involving the high versus low "excess strike" dummy variables. We do not obtain a significant interaction coefficient under this alternative specification, which underscores the importance of considering "excess strikes" rather than just looking at the raw increase in labor unrest (results available on request).

Table 9: Export Shocks and Party Secretary Turnover

Dependent variable:	**Party Secretary Turnover**$_{i,t+1}$				
	(1) IV	(2) IV	(3) IV	(4) IV	(5) IV
ExpShock$_{it}$	-0.0742***				
	(0.0192)				
ExcessStrike$^L_{it}$×ExpShock$_{it}$: θ^{Exp}_L		-0.0034	-0.3657**	-0.0012	-0.3637**
		(0.0734)	(0.1473)	(0.0737)	(0.1441)
ExcessStrike$^H_{it}$×ExpShock$_{it}$: θ^{Exp}_H		-0.0983***	-0.2944**	-0.0995***	-0.2874*
		(0.0236)	(0.1352)	(0.0238)	(0.1482)
(49≤Age≤53)$_{it}$×ExpShock$_{it}$			0.2904*		0.2949*
			(0.1520)		(0.1660)
(FiscalRev/L)$_{i,12}$×ExpShock$_{it}$			0.1427*		0.1384
			(0.0809)		(0.0866)
Turnover$_{it}$	-0.6844***	-0.6860***	-0.6959***	-0.6862***	-0.6973***
	(0.0515)	(0.0505)	(0.0613)	(0.0502)	(0.0624)
ExcessStrike$^H_{it}$: θ_H		0.0730*	0.0764*	0.0706*	0.0775*
		(0.0407)	(0.0436)	(0.0384)	(0.0444)
Incumbent Characteristics:					
Tenure$_{it}$	0.1866***	0.1875***	0.1964***	0.1857***	0.1930***
	(0.0154)	(0.0138)	(0.0251)	(0.0145)	(0.0244)
(Age≤48)$_{it}$	0.0204	-0.0109	0.0927	-0.0095	0.0764
	(0.1369)	(0.1259)	(0.1815)	(0.1212)	(0.1911)
(49≤Age≤53)$_{it}$	-0.0841	-0.0807	-0.0621	-0.0798	-0.0607
	(0.0551)	(0.0563)	(0.0575)	(0.0554)	(0.0571)
Born in the same province$_{it}$	0.0524	0.0639	0.0423	0.0639	0.0488
	(0.0804)	(0.0636)	(0.0576)	(0.0652)	(0.0620)
Master degree or above$_{it}$	-0.1448	-0.1491*	-0.1784**	-0.1550*	-0.1834**
	(0.0865)	(0.0812)	(0.0796)	(0.0801)	(0.0787)
Female$_{it}$	0.1347	0.1505	0.0965	0.1550	0.1025
	(0.1895)	(0.1999)	(0.1883)	(0.2001)	(0.1848)
Additional time-t controls?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Province-year dummies?	Y	Y	Y	Y	Y
First-stage F-stat	94.38	23.33	10.03	26.33	5.465
Level effect: $\hat{\theta}^{Exp}_L \times \overline{ExpShock}_{it}$	–	0.0001	0.0139**	0.0000	0.0139**
Level effect: $\hat{\theta}^{Exp}_H \times \overline{ExpShock}_{it} + \hat{\theta}_H$	–	0.0767*	0.0877**	0.0744*	0.0885**
p-value: $(\hat{\theta}^{Exp}_H - \hat{\theta}^{Exp}_L) \times \overline{ExpShock}_{it} + \hat{\theta}_H$	–	[0.0718]	[0.0996]	[0.0651]	[0.0963]
Observations	822	822	822	822	822
R^2	0.5472	0.5742	0.5092	0.5757	0.5054

Notes: The dependent variable is a dummy for whether there was a change in prefecture party secretary in year $t+1$ (i.e., one year after the export shock). All columns report IV regressions, weighted by the prefecture's working-age population in 2010. The ExcessStrike$^H_{it}$ variable in Columns 2-3 is an indicator variable for whether the predicted residual obtained from running the reduced-form regression from Column 4 of Table 2 has an above-median value. The ExcessStrike$^H_{it}$ variable in Columns 4-5 is similarly obtained from the predicted residual when run additionally with incumbent time-t controls. Where relevant, the level effect of being in the low (respectively, high) excess strike bin is reported, together with the p-value of the test with null hypothesis that the difference in these level effects is zero. (FiscalRev/L)$_{i,2012}$ is the initial local fiscal revenue per worker in 2012. (Age≤48)$_{it}$ and (49≤Age≤53)$_{it}$ are dummy variables for whether the prefecture party secretary is of the respective ages in year t. The additional time-t controls are those used in Column 3 of Table 2, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

because they are more inclined to increase spending on stability measures (as seen earlier in Column 1 of Panel B, Table 8). We also find here that the incumbent is less likely to be replaced when he/she has more fiscal resources (initial fiscal revenues per worker in 2012) to wield against negative shocks. Columns 4-5 repeat the exercise from the preceding two columns, with a version of the excess strike dummy that is constructed from the regression in Column 4 in Table 2 after further controlling for the set of incumbent characteristics used in Column 1 of Table 9; the results we obtain are very similar. Note that near the bottom of the table, we have computed for each column the implied effects on turnover for prefectures in the low and high excess strike groups respectively, after accounting for the main effect of the group dummy and setting $ExpShock_{it}$ at its mean value. The reported p-values confirm that there is a positive gap (significant at least at the 10% level) in the estimated effects on incumbent turnover in high relative to low excess strike prefectures.

Finally, in Table 10, we make use of the information we gathered on the nature of each instance of incumbent turnover. The dependent variables in this table are dummies for turnover under the stated cause; we control for the respective lagged dependent variables, and use here the version of the excess strike dummy from Columns 4-5 of Table 9. The results in Columns 1-2 show that the export shock has small and insignificant effects on promotion, regardless of whether or not we consider the possibility of heterogeneous effects across prefectures that experienced high versus low excess strikes. Columns 3-4 confirm that a negative shock induces more lateral movements, with this effect being concentrated moreover in prefectures that saw high excess strikes. Lastly, as shown in Columns 5-6, the preceding results on lateral movement are largely driven by the effects of export shocks on the "early" lateral movements (i.e., that occur before the incumbent has accrued three years of service time as a local party secretary); notice that the export shock coefficients of interest are larger in magnitude than the corresponding point estimates in Columns 3-4.[46] The contrasting effects of export shocks on promotion versus early lateral movements are consistent with our observations that the latter movements are akin to a *de facto* demotion, that send a negative signal about the incumbent's future career path.

In sum, the evidence on incumbent tenure lends itself to the interpretation that the central government in China makes active decisions about the retention or replacement of local incumbents in response of negative economic shocks. These decisions moreover appear to be based on an evaluation of the local leader's performance in maintaining social stability, that accounts for the severity of the economic conditions that the incumbent faced.

[46] We have checked that the results are similar when defining lateral movements to be early if they occur within the first two years (as opposed to three) of a prefecture party secretary's tenure (available on request).

Table 10: Export Shocks and Party Secretary Turnover: By Causes

Dependent variable:	**Promotion**$_{i,t+1}$		**Lateral Movement**$_{i,t+1}$		**Early Lateral Movement**$_{i,t+1}$	
	(1) IV	(2) IV	(3) IV	(4) IV	(5) IV	(6) IV
ExpShock$_{it}$	-0.0025		-0.0579*		-0.0895***	
	(0.0119)		(0.0314)		(0.0162)	
ExcessStrike$^L_{it}$×ExpShock$_{it}$: θ^{Exp}_L		-0.0124		-0.0321		-0.0226
		(0.0125)		(0.0769)		(0.0257)
ExcessStrike$^H_{it}$×ExpShock$_{it}$: θ^{Exp}_H		0.0023		-0.0632**		-0.1159***
		(0.0170)		(0.0267)		(0.0190)
Turnover Outcome$_{it}$	-0.2790***	-0.2773***	-0.4607***	-0.4634***	-0.3675***	-0.3697***
	(0.0636)	(0.0631)	(0.0584)	(0.0620)	(0.0500)	(0.0487)
ExcessStrike$^H_{it}$: θ_H		0.0095		0.0704*		0.0218
		(0.0095)		(0.0343)		(0.0159)
Incumbent Characteristics:						
Tenure$_{it}$	0.0142**	0.0141**	0.0620***	0.0613***	-0.0136	-0.0144
	(0.0052)	(0.0053)	(0.0128)	(0.0127)	(0.0102)	(0.0102)
(Age≤48)$_{it}$	0.0095	0.0131	0.0627	0.0520	-0.0170	-0.0440
	(0.0681)	(0.0710)	(0.1184)	(0.1126)	(0.0816)	(0.0782)
(49≤Age≤53)$_{it}$	0.0430*	0.0433*	-0.0922	-0.0883	-0.0709	-0.0695
	(0.0249)	(0.0253)	(0.0637)	(0.0635)	(0.0477)	(0.0483)
Born in the same province$_{it}$	0.0477**	0.0479**	-0.1168**	-0.1085**	-0.0021	0.0043
	(0.0221)	(0.0214)	(0.0522)	(0.0452)	(0.0585)	(0.0521)
Master degree or above$_{it}$	-0.0302	-0.0292	-0.2259***	-0.2308***	-0.0974*	-0.1059**
	(0.0337)	(0.0337)	(0.0586)	(0.0572)	(0.0562)	(0.0509)
Female$_{it}$	-0.1369*	-0.1380*	0.1519	0.1626	0.1473	0.1623
	(0.0739)	(0.0737)	(0.1401)	(0.1451)	(0.1125)	(0.1231)
Additional time-t controls?	Y	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y	Y
Province-year dummies?	Y	Y	Y	Y	Y	Y
First-stage F-stat	84.33	28.47	83.64	28.29	85.57	28.49
Level effect: $\hat{\theta}^{Exp}_L \times \overline{ExpShock}_{it}$	–	0.0005	–	0.0012	–	0.0009
Level effect: $\hat{\theta}^{Exp}_H \times \overline{ExpShock}_{it} + \hat{\theta}_H$	–	0.0094	–	0.0728**	–	0.0262
p-value: $(\hat{\theta}^{Exp}_H - \hat{\theta}^{Exp}_L) \times \overline{ExpShock}_{it} + \hat{\theta}_H$	–	[0.346]	–	[0.0461]	–	[0.120]
Observations	822	822	822	822	822	822
R^2	0.4899	0.4911	0.5093	0.5223	0.5242	0.5824

Notes: The dependent variable is a dummy for whether there was a change in prefecture party secretary in year $t+1$ (i.e., one year after the export shock), that is classified as a promotion (Columns 1-2), lateral movement (Columns 3-4), or early lateral movement (Columns 5-6); the Turnover Outcome$_{it}$ variable is the one-year lag of the dependent variable in each column. All columns report IV regressions, weighted by the prefecture's working-age population in 2010. The ExcessStrike$^H_{it}$ variable is an indicator for whether the predicted residual obtained from running the reduced-form regression from Column 4 of Table 2 with additional incumbent time-t controls has an above-median value. Where relevant, the level effect of being in the low (respectively, high) excess strike bin is reported, together with the p-value of the test with null hypothesis that the difference in these level effects is zero. (Age≤48)$_{it}$ and (49≤Age≤53)$_{it}$ are dummy variables for whether the prefecture party secretary is of the respective ages in year t. The additional time-t controls are those used in Column 3 of Table 2, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

9 Conclusion

We have undertaken a wide-ranging investigation into the political economy consequences of a slowdown in exports for China. We have documented how negative shocks to exports have been associated with an increase in labor-related strikes in Chinese prefectures. Using a shift-share instrumental variables strategy, we have argued that our estimates reflect a causal impact of these export shocks.

In order to better understand the political responses to this unfolding dynamic, we study a simple model that seeks to capture essential features of China's hierarchical political system. In the wake of a negative export shock, the model predicts that "excess" strikes are associated with a greater likelihood that the upper-level government will replace the local incumbent; the threat of removal in turn induces the local incumbent to increase the effort and resources channelled towards bolstering domestic stability.

These political responses are evident in the novel data that we gathered. Our textual analysis of prefecture annual work reports shows that declining exports led to a rising use of "weiwen" phrases, signaling a heightened emphasis on preserving stability as a political priority. More directly, prefecture-level fiscal expenditures were increased, on both public security measures (to safeguard law and order) and social spending (to potentially assuage worker grievances). Last but not least, we find that severe export slowdowns accompanied by an excessively high level of labor-related incidents – in excess of what would be predicted by the extent of the shock to exports – help to explain subsequent turnover of local party secretaries.

These patterns are useful for understanding the political response within China to the recent decline in exports, a topic of obvious importance given China's role in global trade and the world economy. More broadly though, they shed light on how economic shocks impact political outcomes in the context of an autocratic regime with high levels of state capacity: Local incumbents can be removed if they under-perform, but this accountability is exercised within the political system from above, rather than stemming from the ballot box (in the case of democracies) or the threat of political violence (in weak autocracies). With a large and arguably increasing share of the world's population living under strong autocratic regimes, understanding how such political systems function and cope with economic challenges is more relevant than ever.

References

Acemoglu, Daron, David Autor, David Dorn, Gordon H. Hanson, and Brendan Price. 2016. "Import Competition and the Great US Employment Sag of the 2000s." *Journal of Labor Economics*, 34(S1): S141 - S198.

Achen, Christopher H., and Larry M. Bartels. 2004. "Blind Retrospection: Electoral Responses to Drought, Flu, and Shark Attacks." Working Paper.

Achen, Christopher H., and Larry M. Bartels. 2018. "Statistics as If Politics Mattered: A Reply to Fowler and Hall." *Journal of Politics*, 80(4).

Adão, Rodrigo, Michal Kolesár, and Eduardo Morales. 2018. "Shift-Share Designs: Theory and Inference." NBER Working Paper No. 24944.

Aghion, Philippe, Antonin Bergeaud, Matthieu Lequien, and Marc J. Melitz. 2018. "The Impact of Exports on Innovation: Theory and Evidence." NBER Working Paper No. 24600.

Ahn, JaeBin, Amit K. Khandelwal, and Shang-Jin Wei. 2011. "The Role of Intermediaries in Facilitating Trade." *Journal of International Economics* 84(1): 73-85.

Alesina, Alberto, Sule Özler, Nouriel Roubini and Phillip Swagel. 1996. "Political Instability and Economic Growth." *Journal of Economic Growth* 1(2):189-211.

Aslam, Aqib, Emine Boz, Eugenio Cerutti, Marcos Poplawski-Ribeirom, and Petia Topalova. 2016. "Global Trade: What's Behind the Slowdown?" *IMF World Economic Outlook*

Autor, David H., David Dorn, and Gordon H. Hanson. 2013. "The China Syndrome: Local Labor Market Effects of Import Competition in the United States." *America Economic Review* 103(6): 2121-2168.

Autor, David H., David Dorn, ,Gordon H. Hanson, and Kaveh Majlesi. 2017. "Importing Political Polarization? The Electoral Consequences of Rising Trade Exposure." NBER Working Paper No. 22637.

Barro, Robert J. 1973. "The control of politicians: An economic model." *Public Choice* 14(1):19-42.

Bazzi, Samuel and Christopher Blattman. 2014. "Economic Shocks and Conflict: Evidence from Commodity Prices." *American Economic Journal: Macroeconomics* 6(4):1-38.

Besley, Timothy and Robin Burgess. 2002. "The Political Economy of Government Responsiveness: Theory and Evidence from India." *Quarterly Journal of Economics* 117(4):1415-1451.

Borusyak, Kirill, Peter Hull, and Xavier Jaravel. 2018. "Quasi-Experimental Shift-Share Research Designs." NBER Working Paper No. 24977.

Braggion, Fabio, Alberto Manconi, and Haikun Zhu. 2018. "Credit and Social Unrest: Evidence from 1930s China." Mimeo.

Brückner, Markus, and Antonio Ciccone. 2011. "Rain and the Democratic Window of Opportunity." *Econometrica* 79(3): 923-947.

Burke, Paul J., and Andrew Leigh. 2010. "Do Output Contractions Trigger Democratic Change?" *American Economic Journal: Macroeconomics* 2(4): 124-157.

Burke, Paul J. 2012. "Economic Growth and Political Survival." *B.E. Journal of Macroeconomics* 12(1), Article 5.

Campante, Filipe R., and Davin Chor. 2012. "Why was the Arab World Poised for Revolution? Schooling, Economic Opportunities, and the Arab Spring" *Journal of Economic Perspectives* 26(2): 167-188.

Campante, Filipe R., Ruben Durante, and Francesco Sobbrio. 2018. "Politics 2.0: The Multifaceted Effect of Broadband Internet on Political Participation." *Journal of the European Economic Association* 16(4): 1094-1136.

Che, Yi, Yi Lu, Justin R. Pierce, Peter K. Schott, and Zhigang Tao. 2018. "Did Trade Liberalization with China Influence U.S. Elections?" NBER Working Paper No. 22178.

Chen, Xi. 2012. "Social Protest and Contentious Authoritarianism in China." New York: Cambridge University Press.

Chen, Ting, and James Kung. 2016. "Do Land Revenue Windfalls Create a Political Resource Curse? Evidence from China." *Journal of Development Economics* 123: 86-106.

Chen, Ting, and James Kung. 2018. "Busting the 'Princelings': The Campaign Against Corruption in China's Primary Land Market." *Quarterly Journal of Economics*, forthcoming.

Chen, Shawn Xiaoguang. 2017. "The Effect of a Fiscal Squeeze on Tax Enforcement: Evidence from a Natural Experiment in China." *Journal of Public Economics* 147: 62-76.

Colantone, Italo, and Piero Stanig. 2018. "Global Competition and Brexit." *American Political Science Review* 112(2): 201-218.

Cole, Shawn, Andrew Healy, and Eric Werker. 2012. "Do Voters Demand Responsive Governments? Evidence from Indian Disaster Relief." *Journal of Development Economics* 97(2): 167-181.

Dippel, Christian, Robert Gold, Stephan Heblich, and Rodrigo Pinto. 2018. "Instrumental Variables and Causal Mechanisms: Unpacking the Effect of Trade on Workers and Voters." NBER Working Paper No. 23209.

Diamond, Larry. 2015. "Facing Up to the Democratic Recession." *Journal of Democracy* 26(1):141-155.

Dix-Carneiro, Rafael, and Brian K. Kovak. 2017. "Trade Liberalization and Regional Dynamics." *American Economic Review* 107(10): 1908-2946.

Dix-Carneiro, Rafael, Rodrigo R. Soares, and Gabriel Ulyssea. 2018. "Economic Shocks and Crime: Evidence from the Brazilian Trade Liberalization." *American Economic Journal: Applied Economics* 10(4): 158-195.

Dube, Oeindrila, and Juan F. Vargas. 2013. "Commodity Price Shocks and Civil Conflict: Evidence from Colombia." *Review of Economic Studies* 80(4): 1384-1421.

Duch, Raymond M., and Randolph T. Stevenson. 2008. *The Economic Vote: How Political and Economic Institutions Condition Election Results.* Cambridge: Cambridge University Press.

Eaton, Jonathan, Samuel Kortum, Brent Neiman, and John Romalis. 2016. "Trade and the Global Recession." *American Economic Review* 106 (11): 3401-38.

Fearon, James D. 1999. "Electoral accountability and the control of politicians: selecting good types versus sanctioning poor performance." In Democracy, Accountability, and Representation, edited by Adam Przeworski, Susan C. Stokes, Bernard Manin. New York: Cambridge University Press.

Feenstra, Robert C., and Chang Hong. 2010. "China's Exports and Employment." In China's Growing Role in World Trade, edited by Robert Feenstra and Shang-jin Wei. Chicago: University of Chicago Press.

Feigenbaum, James J., and Andrew B. Hall. 2015. "How Legislators Respond To Localized Economic Shocks." *Journal of Politics* 77(4): 1012-1030.

Feler, Leo, and Mine Z. Senses. 2019. "Trade Shocks and the Provision of Local Public Goods." *American Economic Journal: Economic Policy* 9(4): 101-143.

Ferejohn, John. 1986. "Incumbent Performance and Electoral Control." *Public Choice* 50(1-3):5-25.

Finger, Joseph M., and Mordechai E. Kreinin. 1979. "A Measure of 'Export Similarity' and Its Possible Uses." *Economic Journal* 89(356): 905-912.

Fisman, Raymond, Jing Shi, Yongxiang Wang, and Weixing Wu. 2018. "Soical Ties and the Selection of China's Elite." Working Paper.

Fowler, Anthony H., and Andrew B Hall. 2018. "Do Shark Attacks Influence Presidential Elections? Reassessing a Prominent Finding on Voter Competence." *Journal of Politics*, 80(4).

Francois, Patrick, Francesco Trebbi, and Kairong Xiao. 2016. "Factions in Non-democracies: Theory and Evidence from Chinese Communist Party" NBER Working Paper No. 22755.

Gentzkow, Matthew, and Shapiro, Jesse M. 2010. "What Drives Media Slant? Evidence From U.S. Daily Newspapers." *Econometrica* 78(1): 35-71.

Goldsmith-Pinkham, Paul, Isaac Sorkin, and Henry Swift. 2018. "Bartik Instruments: What, When, Why, and How." NBER Working Paper No. 24408.

Haggard, Stephan, and Robert R. Kaufman. 1995. *The Political Economy of Democratic Transitions.* Princeton: Princeton University Press

Hanson, Gordon H. 2012. "The Rise of Middle Kingdoms: Emerging Economies in Global Trade. *Journal of Economic Perspectives* 26(2): 41-64.

Healy, Andrew J., and Neil Malhotra. 2013. "Retrospective Voting Reconsidered." *Annual Review of Political Science* 16: 285-306.

Healy, Andrew J., Mikael Persson, and Erik Snowberg. 2017. "Digging into the Pocketbook: Evidence on Economic Voting from Income Registry Data Matched to a Voter Survey." *American Political Science Review* 111(4): 771-785.

Hendrix, Cullen S., and Idean Salehyan. 2012. "Climate Change, Rainfall, and Social Conflict in Africa." *Journal of Peace Research* 49(1): 35-50.

Jia, Ruixue. 2014. "Weather Shocks, Sweet Potatoes and Peasant Revolts in Historical China." *Economic Journal* 124(575): 92-118.

Jensen, J. Bradford, Dennis P. Quinn, and Stephen Weymouth. 2017. "Winners and Losers in International Trade: The Effects on US Presidential Voting." *International Organization* 71(3): 423-457.

Jia, Ruixue, Masa Kudamatsu, and David Seim. 2015. "Political Selection in China: Complementary Roles of Connections and Performance." *Journal of the European Economic Association* 13(4): 631-668.

Kearney, Melissa S., and Phillip B. Levine. 2015. "Media Influences on Social Outcomes: The

Impact of MTV's 16 and Pregnant on Teen Childbearing." *American Economic Review* 105(12): 3597-3632.

Kim, Nam Kyu. 2016. "Revisiting Economics Shocks and Coups." *Journal of Conflict Resolution* 60(1): 3-31.

Kovak, Brian K. 2013. "Regional Effects of Trade Reform: What is the Correct Measure of Liberalization?" *American Economic Review* 103(5): 1960-1976.

Leigh, Andrew. 2009. "Does the World Economy Swing National Elections?" *Oxford Bulletin of Economics and Statistics* 71(2): 163-181.

Levitsky, Steven, and Lucan Way. 2015. "The Myth of Democratic Recession." *Journal of Democracy*, 26(1):45-58.

Lewis-Beck, Michael S. 1988. *Economics and Elections. The Major Western Democracies.* University of Michigan Press.

Li, Hongbin, and Li-An Zhou. 2005. "Political Turnover and Economic Performance: the Incentive Role of Personnel in China." *Journal of Public Economics* 89(9): 1743-1762.

Lorentzen, Peter. 2013. "Regularizing Rioting: Permitting Public Protest in an Authoritarian Regime." *Quarterly Journal of Political Science* 8(2): 127-158.

Los, Bart, Marcel P. Timmer, and Gaaitzen J. de Vries. 2015. "How Important are Exports for Job Growth in China? A Demand Side Analysis." *Journal of Comparative Economics* 43(1): 19-32.

Madestam, Andreas, Daniel Shoag, Stan Veuger, and David Yanagizawa-Drott. 2013. "Do Political Protest Matter? Evidence From The Tea Party Movement." *Quarterly Journal of Economics* 128(4): 1633-1685.

Manacorda, Marco, and Andrea Tesei. 2016. "Liberation Technology: Mobile Phones and Political Mobilization in Africa." Working Paper

McCaig, Brian. 2011. "Exporting Out of Poverty: Provincial Poverty in Vietnam and U.S. Market Access." *Journal of International Economics* 85: 102-113.

Miguel, Edward, Shanker Satyanath, and Ernest Sergenti. 2004. "Economic Shocks and Civil Conflict: An Instrumental Variables Approach." *Journal of Political Economy* 112(4): 725-753.

Mullainathan, Sendhil, and Jann Spiess. 2017. "Machine Learning: An Applied Econometric Approach." *Journal of Economic Perspectives* 31(2): 87-106.

New York Times. 2012. "Preserving Stability." 19 Sep 2012.

New York Times. 2016a. "China's Fading Factories Weigh on an Already Slowing Economy." 20 Jan 2016.

New York Times. 2016b. "Labor Protests Multiply in China as Economy Slows, Worrying Leaders." 14 Mar 2016.

Nunn, Nathan, Nancy Qian, and Jaya Wen. 2018. "Distrust and Political Turnover." NBER Working Paper No. 24187.

Persson, Petra, and Ekaterina Zhuravskaya. 2016. "The Limits of Career Concerns in Federalism: Evidence from China." *Journal of the European Economic Association* 14(2): 338-374.

Persson, Torsten, and Guido Tabellini. 2000. "Political Economics: Explaining Economic Policy." Cambridge, MA: MIT Press.

Qian, Gang. 2012. "Preserving Stability." *The New York Times.* https://cn.nytimes.com/china/20120919/cc19qiangang2/

Qin, Yu, and Hongjia Zhu. 2017. "Run Away? Air Pollution and Emigration Interests in China." *Journal of Population Economics* 31(1): 235-266.

Redding, Stephen J., and Anthony J. Venables. 2004. "Economic Geography and International Inequality." *Journal of International Economics* 62(1): 53-83.

Shih, Victor, and Jonghyuk Lee. 2018. "Locking in Fair Weather Friends: Assessing the Fate of Chinese Communist Elite when Their Patrons Fall from Power." Mimeo.

Stephens-Davidowitz, Seth. 2014. "The Cost of Racial Animus on a Black Presidential Candidate: Using Google Search Data to Find What Surveys Miss." *Journal of Public Economics* 118: 2640.

Tanner, Murray Scot. 2014. "China's Social Unrest Problem." Testimony before the U.S.-China Economic and Security Review Commission. See: https://www.uscc.gov/sites/default/files/Tanner_Written Testimony.pdf

Topalova, Petia. 2010. "Factor Immobility and Regional Impacts of Trade Liberalization: Evidence on Poverty from India." *American Economic Journal: Applied Economics* 2(4): 1-41.

Wingender, Philippe. 2018. "Intergovernmental Fiscal Reform in China." IMF Working Paper.

van der Brug, Wouter, Cees van der Eijk, and Mark Franklin. 2007. *The Economy and the Vote.* Cambridge University Press.

Yao, Yang, and Muyang Zhang. 2015. "Subnational Leaders and Economic Growth: Evidence from Chinese Cities." *Journal of Economic Growth* 20: 405-436.

A Data Appendix (FOR ONLINE PUBLICATION)

A.1 Labor Disputes Data from MOHRSS

The data on the number of labor dispute cases are from the China Labor Statistical Yearbook, published by the Ministry of Human Resources and Social Security (MOHRSS). These record labor dispute cases that have been officially submitted for mediation or arbitration to "employment dispute arbitration committees" (劳动争议仲裁委员会) at the county level. The count is aggregated at the province level when reported in the statistical yearbooks.

Panel A in Figure A.1 demonstrates that at the national level, the trends over time in the total number of MOHRSS labor dispute cases and the total number of CLB-reported labor events are highly correlated. Panel B in the same figure confirms that over the 2012-2015 period, the annual changes in both series are positively correlated across provinces. There is one observation for Ningxia that appears to be an outlier to the right in Panel B, but removing this point would further strengthen the positive correlation.

A.2 Rationalizing the Export-share Weights in the Bartik IV

We provide a brief justification for the use of weights based on initial export shares in the construction of the Bartik IV in (3). Let X_{iR}^k denote the value of exports of product k from prefecture i in China to the ROW. We have:

$$X_{iR}^k = \lambda_{iR}^k Y_R^k,$$

where Y_R^k is the total expenditure in the ROW on product k, while λ_{iR}^k is the corresponding expenditure share (out of Y_R^k) that is allocated to those products that originate from prefecture i in China. The value of product-k exports from China as a whole to the ROW, X_{CR}^k, is given by a similar relation:

$$X_{CR}^k = \lambda_{CR}^k Y_R^k,$$

where λ_{CR}^k denotes the expenditure share on those products that originate from China.

Consider now a set of exogenous shocks that shifts the foreign demand for good k. Let X_{iR} denote total exports from prefecture i to the ROW. The change in these total exports is then given by:

$$dX_{iR} = \sum_k \lambda_{iR}^k dY_R^k + d\lambda_{iR}^k Y_R^k = \sum_k \left(\frac{\lambda_{iR}^k}{\lambda_{CR}^k} X_{CR}^k \frac{dY_R^k}{Y_R^k} + \frac{d\lambda_{iR}^k}{\lambda_{iR}^k} X_{iR}^k \right) = \sum_k \left(\frac{X_{iR}^k}{X_{CR}^k} d\tilde{X}_{CR}^k + \frac{d\lambda_{iR}^k}{\lambda_{iR}^k} X_{iR}^k \right).$$

where $d\tilde{X}_{CR}^k = X_{CR}^k \frac{dY_R^k}{Y_R^k}$ is the change in product-k exports from China induced by the demand shock in the ROW. In our empirical approach, we focus on sources of variation in prefecture-i exports to the ROW that stem from shifts in foreign demand conditions. This corresponds

precisely to the first set of terms in the above expression for dX_{iR}, namely: $\sum_k \frac{X_{iR}^k}{X_{CR}^k} d\tilde{X}_{CRk}$. The construction of the Bartik IV thus adopts as weights the initial share of prefecture i in China's total exports of product k (i.e., $\frac{X_{iR}^k}{X_{CR}^k}$); in practice, we also replace $d\tilde{X}_{CR}^k$ by the corresponding change in product-k exports from the ROW to the ROW.

A.3 An Example of a "Weiwen" Paragraph

The following is an example of a "weiwen" paragraph that was included in our training sample for the machine learning algorithms. This paragraph is from the State Council document of 13 April 2015, entitled: "Opinions on Strengthening Society's Public Security Prevention and Control System". The extracted paragraph in Chinese and its English translation (lightly edited by Google Translate) are included.

Original:

"健全社会治安形势分析研判机制。政法综治机构要加强组织协调，会同政法机关和其他有关部门开展对社会治安形势的整体研判、动态监测，并提出督办建议。公安机关要坚持情报主导警务的理念，建立健全社会治安情报信息分析研判机制，定期对社会治安形势进行分析研判。加强对社会舆情、治安动态和热点、敏感问题的分析预测，加强对社会治安重点领域的研判分析，及时发现苗头性、倾向性问题，提升有效应对能力。建立健全治安形势播报预警机制，增强群众自我防范意识。"

Translation:

"[We shall] improve the analysis and evaluation system on public security. The procuratorial office, judicial administrative department, and public security department shall work collectively and, in accordance with other departments, carry out all-round dynamic monitoring, and put forward suggestions and advice. The public security department shall uphold intelligence-led policing, establish and enhance the mechanism for analyzing, inspecting, and reviewing criminal intelligence on social stability. [We shall] regularly examine and monitor the public security situation. [We shall] improve the system of analyzing and predicting the trend of social opinions, hotspot security problems, and sensitive issues. [We shall] strengthen the analysis and examination of the major aspects of social stability in order to uncover in a timely manner the emerging and hidden risks that endanger social stability, and to improve the ability to cope with such issues. [We shall] establish and improve the monitoring and early-warning mechanisms for public security, and enhance people's awareness for self-protection."

A.4 Machine Learning Models and Packages

Our machine learning models require inputs of words, commonly known as tokens in the field of natural language processing, for training and classification purposes. Unlike English, where tokenization simply involves splitting the text at white spaces and punctuation marks, Chinese

text tokenization is more complicated due to the lack of delimiters such as spaces between words. We employed an open source software library called *jieba* to perform this task; this library contains a large dictionary of Chinese words, along with their relative positions and their respective frequencies.[47] When the software scans through a sentence, it builds a directed acyclic graph (DAG) for all possible word combinations, and then identifies the most probable combination based on the word-position frequency from its dictionary.

For both the Multinomial Naive Bayes (MNB) and Support Vector Machine (SVM) models, we adopted packages from the open source *scikit-learn* library.[48] This is a well-tested and well-supported machine learning software library, with packages written in Python. For the MNB, we used a "term frequency-inverse document frequency" (TFIDF) construction tool to compute the frequencies of word tokens, as a first step in preparing the text documents for analysis.[49]

To operationalize these supervised machine learning algorithms, we put together a training dataset comprising: (i) 20 prefecture annual work reports selected at random from a pre-sample year (2011); and (ii) the State Council document of 13 April 2015 on: "Opinions on Strengthening Society's Public Security Prevention and Control System". For (i), we manually identified the sentences and phrases in each of the 20 reports that were on the topic of maintaining social stability ("weiwen"); for (ii), we classified the entire report as being about "weiwen". The MNB model uses this training dataset as the basis for computing a posterior probability that an unseen text passage is about "weiwen", based on a multinomial probability distribution model for the occurrence of tokens; the model is "naive", in that it assumes a zero correlation in the joint occurrence of any pair of tokens. The SVM model on the other hand transforms the passages from the training dataset into points in a high-dimensional metric space, and then partitions these in a binary fashion via a hyperplane that seeks to maximize the distance between itself and the nearest observation that lies on either "side" of it; unseen text passages are then mapped into this same metric space, and classified as "weiwen" or not on the basis of which side of the hyperplane they are located.

In line with common practice, we performed a cross-validation of the 20 pre-sample work reports at the training stage as follows. We divided these into four subsets of 5 reports each, and then trained the machine learning model using the first three of these subsets together with the State Council document from (ii). The trained models were then used to score the passages in the omitted subset of 5 reports that had been marked out as being about "weiwen". We repeated the above procedure a further three times, omitting in turn the second, third and fourth subsets of 5 reports. From this exercise, the simple average of the scores obtained for the passages in the omitted subset of reports was 0.98 for the MNB and 0.97 for the SVM models respectively, providing validation of the internal consistency of the training sample in identifying "weiwen" passages.

[47]Available at: https://github.com/fxsjy/jieba

[48]See: http://scikit-learn.org/stable/modules/generated/sklearn.naive_bayes.MultinomialNB.html, and http://scikit-learn.org/stable/modules/generated/sklearn.svm.SVC.html

[49]From: http://scikit-learn.org/stable/modules/generated/sklearn.feature_extraction.text.TfidfVectorizer.html.

We subsequently applied these two models to the prefecture annual work reports in our sample period of interest (2012-2016). The "weiwen" score under each machine learning model for a given work report was computed as the character-length weighted-average of the paragraph scores from that report.

A.5 Classification of Incumbent Turnover

We classify each instance of incumbent turnover as a promotion, a lateral movement, or being due to other causes (corruption, demotion, retirement, movement to an honorary position). This coding is in turn based on a comparison of the political rank of the individual's new position relative to the old position that he/she vacated.

For most prefectures, the position of party secretary is considered to be at the prefecture (or bureau) level in terms of political rank ("Tingju Ji", 厅局级 in Chinese). We consider a movement to be a promotion if the new position is at the sub-provincial ministerial level ("Fusheng Ji", 副省级; or "Fubu Ji", 副部级) or above. To give some examples of sub-provincial level positions, these include: the provincial vice-governor; provincial vice-party secretary; provincial standing committee member; head of People's Procuratorate and People's Court at the provincial level; etc. Some examples of provincial ministerial level ("Fusheng Ji", 副省级; or "Fubu Ji", 副部级) positions are: the provincial governor; provincial party secretary; head of different ministries at the central level; etc.

There are a number of key exceptions to the above coding rules. First, there are 4 prefectures that are also province-level municipalities (Beijing, Shanghai, Tianjin, Chongqing), so the party secretary position is considered a rank at the provincial ministerial level; for these, we consider their movement as a promotion if the new position is at the sub-national level ("Fuguo Ji", or 副国级) or above. Second, there are 15 prefectures that are also sub-province-level municipalities (Changchun, Chengdu, Dalian, Guangzhou, Hangzhou, Harbin, Jinan, Nanjing, Ningbo, Qingdao, Shenyang, Shenzhen, Wuhan, Xi'an, Xiamen), where the party secretary is a rank at the sub-provincial ministerial level; for these, we consider a movement to be a promotion if the new position is at the provincial ministerial level or above. Third, we do not consider movements to positions in the province-level People's Congress or province-level People's Political Consultative Committee to be promotions, since these are viewed as honorary positions akin to "consolation prizes" in China's political hierarchy; this follows Li and Zhou (2005) and Yao and Zhang (2015).

During the period 2014-2016, there were 292 instances of local party secretary turnover, out of 987 available prefecture-year observations. Of these, 73 (or 25%) were classified as promotions and 161 (or 55.1%) as lateral movements. The latter include 50 instances of early lateral movements, that occurred before the incumbent had accrued three years in that position.

Promotion age profile: Figure A.2 presents the age distribution of the prefecture party secretaries (right vertical axis), as well as the observed promotion probability at different ages (left vertical axis). The figure is constructed using the sample of prefecture party secretaries

from 2012-2016; note that the promotion probability is computed simply as the share of incumbents promoted at each given age. The figure shows that the promotion likelihood peaks between ages 49-53 and declines steadily afterward. Likewise, the frequency of observing a party secretary at a given age declines after age 53. In our empirical analysis, we thus consider the age range between 49-53 as that in which a prefecture party secretary would see his/her expected future rents from holding office to be at its largest.

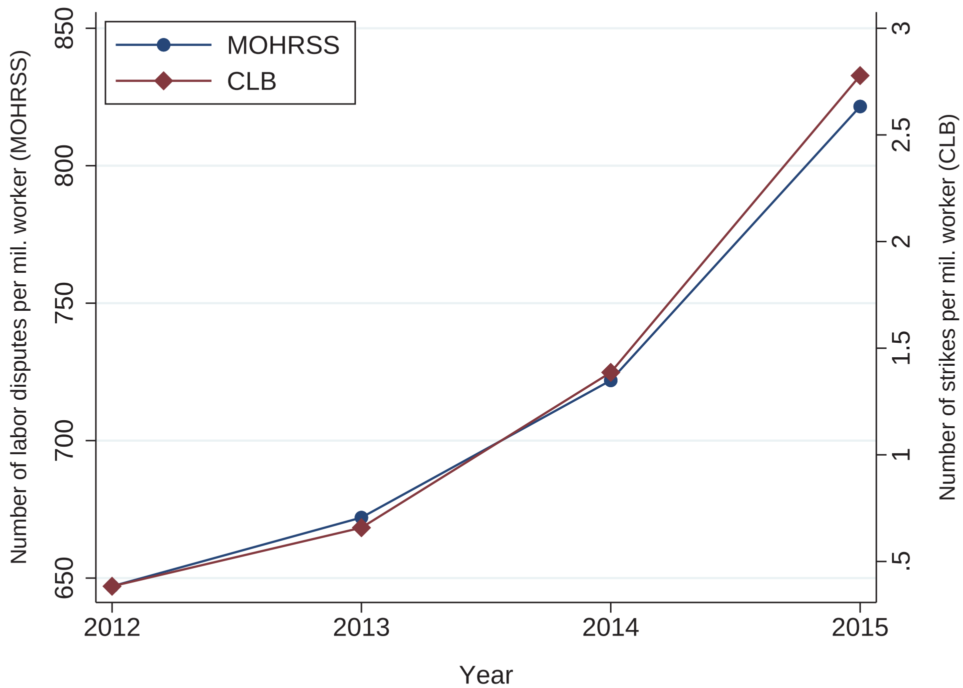
Early lateral movement and career trajectory: We investigate the implications of early lateral movement on an official's career path, to show that this lowers his/her probability of future promotion. Toward this end, we use the data on political turnover of prefecture party secretaries and restrict the sample to those officials who experienced a lateral movement during 2007-2012; we then examine the career path of these officials up until 2016 where our data end.

For each official, we consider the first lateral move he/she experienced in 2007-2012. Let P_0 denote the position that the official held prior to this move, and let P_1 be the position to which he/she was moved laterally. Let P_2 then denote the position that he/she moved to in his/her next subsequent move, if any. We code up a dummy variable equal to 1 if P_2 is a higher political rank relative to P_1; the dummy is equal to 0 otherwise, including in situations where the official did not experience a subsequent move P_2. Figure A.3 illustrates the future promotion probability among these lateral movers, grouped by bins according to their tenure duration in P_0 at the time they were moved to P_1. Notice that the subsequent promotion probability of early lateral movers, i.e., those who were in position P_0 for fewer than three years, is lower than that of lateral movers with a tenure of 3-6 years.

This finding is further substantiated by the regressions reported in Table A.2. Using the same sample of prefecture party secretaries as in Figure A.3, we regress various outcome measures related to future promotion on: (i) dummy variables for the official's years of tenure in P_0 at the time of the lateral move to P_1; and (ii) a set of officeholder control variables, as listed in the Table A.2 footnote. The dependent variables are dummies for whether: (i) the move to P_2 was a promotion relative to P_1 (Column 1); (ii) the official was ever promoted in any moves including and subsequent to P_2 (Column 2); (iii) the highest rank he/she occupied was at the sub-provincial level or higher (Column 3); and (iv) the highest rank he/she occupied was at the provincial level or higher (Column 4). Columns 1 and 2 confirm that early lateral movers (the omitted category) have lower future promotion prospects relative to lateral movers who had spent between 3-6 years in their prior positions. The effect is statistically significant for promotion during one's next move (Column 1); it may even affect one's prospects of ever being promoted (Column 2), at least to the extent observable by 2016, although this coefficient is not statistically significant. Early lateral movers are also less likely to make it to positions higher up the political ranking, specifically to provincial-level positions (Column 4).

Figure A.1: Comparing CLB Labor Events versus MOHRSS Labor Disputes

A. Number of Events (national level)



B. Changes in Number of Events (across provinces)

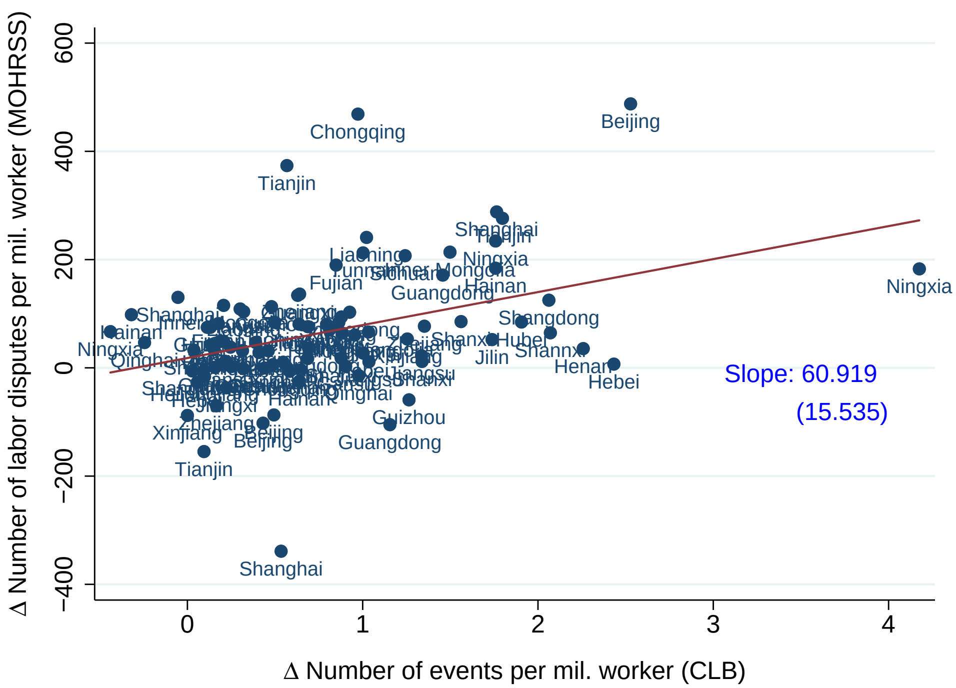


Figure A.2: Promotion Age Profile

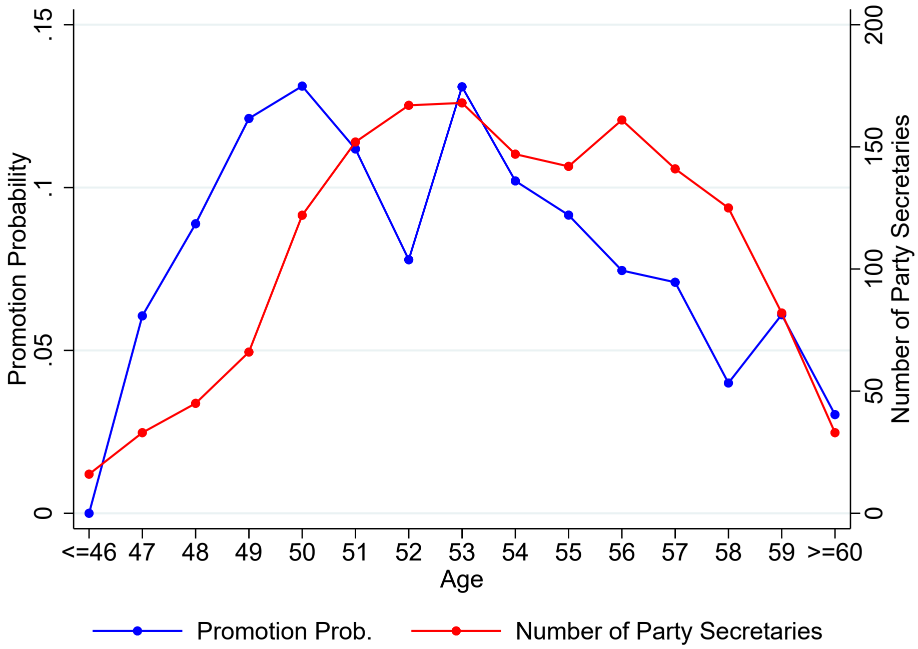


Figure A.3: Future Promotion Probability of Lateral Movers

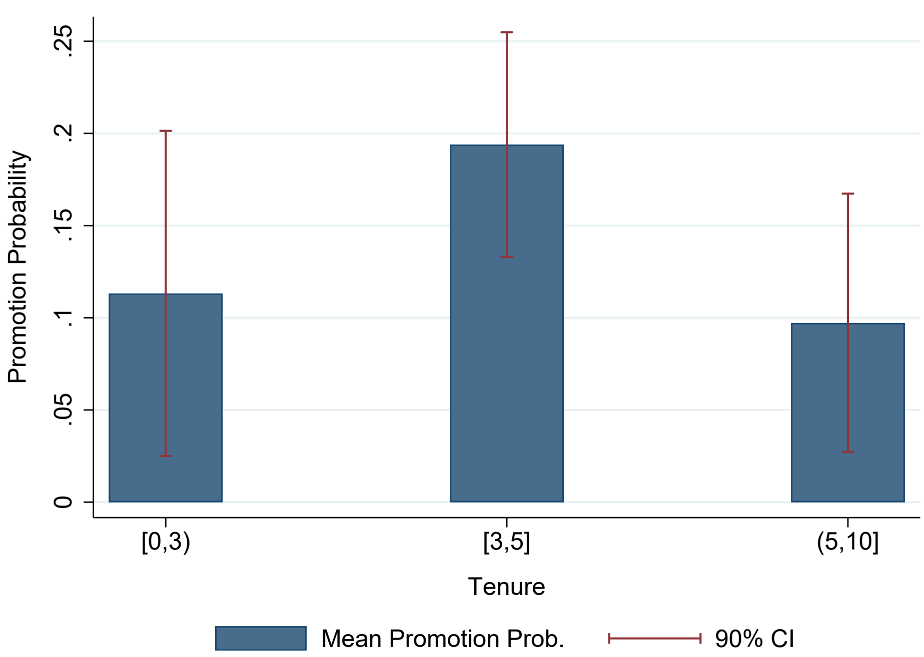


Table A.1: Keywords Related to “preserving stability”

Chinese	English
维稳	a shorthand term for “preserving stability”
维护稳定	preserving stability
保持稳定	maintaining stability
社会稳定	social stability
和谐稳定	harmony and stability
安全稳定	safety and stability
安定和谐	safety and harmony
社会和谐	social harmony
公共安全	public security
和谐平稳	harmony and stability
维稳处突	a shorthand term for “preserving stability and handling sudden-breaking incidents”

Table A.2: Future Promotion Probability of Lateral Movers

Dependent variable:	**Promotion: in the next movement**	**Promotion: ever in the future**	**Highest rank: sub-province level or above**	**Highest rank: province level or above**
	(1)	(2)	(3)	(4)
	OLS	OLS	OLS	OLS
Tenure∈[3,6)	0.1505***	0.0872	0.0119	0.0803*
	(0.0510)	(0.0890)	(0.0847)	(0.0438)
Tenure∈[6,10)	0.0112	-0.0261	0.0460	0.0441
	(0.0705)	(0.1023)	(0.1142)	(0.0358)
Incumbent characteristics?	Y	Y	Y	Y
Year dummies?	Y	Y	Y	Y
Province dummies?	Y	Y	Y	Y
Observations	276	276	276	276
R^2	0.2121	0.1989	0.2601	0.1498

Notes: The sample comprises all prefecture party secretaries who recorded a lateral move during 2007-2012. The incumbent characteristics included as controls are dummy variables for whether (in the lateral-move year) the official: was aged ≤48; was aged 49-53; held a masters degree or higher; is female; held the party secretary position in a prefecture within the same province as his/her birth. All columns also use turnover year dummies and province dummies. Robust standard errors clustered by province are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

B Additional Results & Checks (FOR ONLINE PUBLICATION)

B.1 Basic Robustness

Table B.1 presents a set of basic checks on our findings linking a slowdown in exports at the prefecture level to increases in labor strikes (Column 1) and responses by the political authorities (Columns 2-5). The dependent variables in this table (and in other robustness tables that follow) are in column order: (i) the time-t change (relative to the previous year) in the number of CLB-recorded strikes per worker; (ii) the time-$(t+1)$ change in the log Multinomial Naive Bayes (MNB) "weiwen" score; (iii) the time-$(t+1)$ change in log fiscal expenditure on public security; (iv) the time-$(t+1)$ change in log fiscal expenditure on social spending; and (v) an indicator variable for party secretary turnover in time $(t+1)$. The IV specifications that are estimated follow equation (2) for Column 1, equation (8) for Columns 2-4, and equation (9) for Column 5.

Panel A in Table B.1 shows that the results remain intact if the province-year fixed effects are replaced by region-year fixed effects.[50] This specification allows us to retain several large prefectures (Beijing, Shanghai, Tianjin, Chongqing) that comprise their entire province, that would otherwise be dropped from the sample when province-year fixed effects are used. Panel B reports unweighted regressions, to demonstrate that the findings do not depend on the decision to weight the regressions by prefecture initial workforce size. Panel C shows that dropping the lag level of the response variable of interest from the right-hand side controls is immaterial for our findings.

To address potential concerns about influential observations, Figure B.1 presents a residual scatterplot based on the specification reported in Column 3 of Table 2. For the horizontal axis variable, we take the predicted export shock that emerges from running the first-stage of the IV regression; we then regress this predicted variable against the right-hand side variables in equation (2) excluding $ExpShock_{it}$, while weighting the observations by $L_{i,2010}$, in order to extract an export shock residual. The vertical axis variable is analogously constructed, as the residual from regressing the change in CLB events per million workers against all right-hand side variables in (2) while weighting the observations by $L_{i,2010}$, once again excluding $ExpShock_{it}$. The residual scatterplot reveals a downward-sloping relationship, and moreover provides reassurance that no single observation appears to be driving the negative slope.

In Table B.2, we demonstrate that the findings are robust to dropping each province in turn, so that there is no particular province that is driving the results. For each column, the table reports the largest and smallest $ExpShock_{it}$ coefficients obtained from this exercise of dropping one province at a time, together with the associated statistical significance levels.

[50] The seven regions are: Northeast (Heilongjiang, Jilin, and Liaoning), North (Beijing, Tianjin, Hebei, Shanxi, and Inner Mongolia), Central (Henan, Hubei, and Hunan), East (Shandong, Jiangsu, Anhui, Shanghai, Zhejiang, Jiangxi, and Fujian), South (Guangdong, Guangxi, and Hainan), Northwest (Shannxi, Gansu, Ningxia, Qinghai, and Xinjiang), and Southwest (Sichuan, Guizhou, Yunnan, Chongqing, and Tibet).

B.2 Controlling for other Domestic Shocks

A potential concern is that demand shocks from the ROW could be correlated with shocks that originate from within China's prefectures. The estimated export shock coefficient in our regressions may thus be picking up the effects of these domestic demand or supply shocks, rather than the effect of export demand *per se*.

Consider first the possible role of domestic demand shocks. We construct a measure of domestic demand, in order to directly control for it in the regressions. We build this measure from information on absorption (i.e., domestic output less net exports) at the industry level. For each four-digit Chinese Standard Industrial Classification (CSIC) industry (indexed by j) and year (indexed by t), we compute first the output of that industry that is absorbed in the Chinese economy as: $Absorption_{jt} = Output_{jt} - Export_{jt} + Import_{jt}$; in particular, the data on output are from the China Industry Statistical Yearbooks. We then project the annual change in $Absorption_{jt}$ onto Chinese prefectures i using a Bartik-style construction as follows:

$$AbsorptionShock_{it} = \sum_j \frac{L_{ij,2010}}{\sum_i L_{ij,2010}} \frac{\Delta Absorption_{jt}}{L_{i,2010}}.$$

In words, this is a weighted-average measure of the industry-level absorption shocks, where the weights used are the initial shares of prefecture i in China-wide employment in industry j (i.e., $L_{ij,2010}/\sum_i L_{ij,2010}$); these weights are computed from the 2010 China Annual Survey of Industrial Firms. The variable is further normalized by the working age population in prefecture i, $L_{i,2010}$ (from the 2010 Census). This is the proxy for domestic demand shocks at the prefecture level which we control for in Panel A of Table B.3. (We build this measure from industry-level data for China as a whole, as detailed data on industry-level output by prefecture are not yet publicly available for the years in our sample, to the best of our knowledge.)

To control for the role of domestic supply shocks, we construct an analogous Bartik-style measure of prefecture-level shifts in output, using the same data sources as above:

$$OutputShock_{it} = \sum_j \frac{L_{ij,2010}}{\sum_i L_{ij,2010}} \frac{\Delta Output_{jt}}{L_{i,2010}}.$$

We control for this proxy for domestic supply shocks in Panel B of Table B.3; in Panel C, we control for it together with the domestic absorption shock.

Throughout Panels A-C, we find that the estimated effect of the export shock on labor strikes and political responses is similar to the baseline results in the main paper, suggesting that domestic shocks are not influencing our findings. (Interestingly, Column 1 indicates that a weakening in domestic demand and output would be associated with increases in labor strikes, although this does not detract from the strong findings on the role of the export shock.) In Figure B.2, we illustrate the cross-industry correlation between $\Delta Absorption_{jt}$ and $\Delta Output_{jt}$ on the one hand, and the CSIC industry-level export shock on the other. These partial scat-

terplots are based on data from 2013-2015, and are obtained after residualizing $\Delta Absorption_{jt}$, $\Delta Output_{jt}$, and the CSIC industry-level export shock for the role of year fixed effects. The slope coefficients in the figure are slightly positive, but not different from zero in a statistically significant way. This provides further reassurance that the export shock is not likely to be picking up an incidental correlation with domestic demand or supply shifts.

To assess the potential confounding effect of imports, we construct a Bartik-style measure of prefecture-level import shocks as:

$$ImpShock_{it} = \sum_j \frac{L_{ij,2010}}{\sum_i L_{ij,2010}} \frac{\Delta M_{jt}}{L_{i,2010}},$$

where ΔM_{jt} is the change in imports of industry j in year t, computed from the China customs data. While we are reasonably confident about the exogeneity of external demand shocks faced by Chinese exporters, it is more challenging to propose exogenous import supply shocks to instrument for changes in imports at the prefecture level. With this caveat in mind, Panel D of Table B.3 presents a specification where we introduce the above $ImpShock_{it}$ variable. The estimated export shock coefficient resembles that from the baseline estimates, while the coefficient on the import shock is not statistically significant.

B.3 Alternative Bartik Shocks

In this next set of checks reported in Table B.4, we confirm the robustness of the findings under alternative constructions of the Bartik IV.

Excluding intermediary firms: In Panel A, we drop firms f that are trade intermediaries, identifying these on the basis of their Chinese character firm names, following Ahn et al. (2011). We remove these intermediaries from the construction of the $ExpShock_{it}$ variable in (1) and the ROW Bartik IV in (3).

Destination-specific demand shocks: In Panel B, we use information on the composition of exports across destination markets, to construct the following alternative Bartik IV:

$$\sum_k \sum_{d \neq CHN} \frac{X_{idk,2010}}{\sum_i X_{idk,2010}} \frac{\Delta X^{ROW}_{dkt}}{L_{i,2010}}. \tag{B.1}$$

Here, ΔX^{ROW}_{dkt} denotes the change in exports of product k from the ROW (excluding China) to country d in year t. $X_{idk,2010}/\sum_i X_{idk,2010}$ is the share of exports of product k from China to the ROW that originate from prefecture i in the base year (2010); specifically, we apportion destination-specific demand changes to each prefecture according to the initial distribution of exports across source prefectures. The apportioned export shocks are summed across products and destination markets, and then normalized by the local working age population. The variation in (B.1) thus stems from cross-destination-by-product differences in demand shocks, and cross-prefecture differences in initial specialization patterns in producing for different markets.

(We exclude exports to Hong Kong and Macau for this exercise.)

Gravity-based Demand Shocks: In Panels C and D, we use an empirical gravity model of trade, in order to extract a component of the shift in trade flows that can be attributed to foreign demand forces. Following Redding and Venables (2004), we first estimate:

$$\ln X_{odkt} = \alpha_1 \ln Dist_{od} + \alpha_2 B_{od} + \alpha_3 Col_{od} + \alpha_3 Lang_{od} + \varphi_{okt} + \varphi_{dkt} + \varepsilon_{odkt}, \tag{B.2}$$

where X_{odkt} denotes the trade flow of product k from country o to country d in year t. On the right-hand side, $Dist_{od}$ is the bilateral distance between o and d; B_{od} is an indicator variable for whether the two countries share a common border; Col_{od} is an indicator variable for shared colonial ties; and $Lang_{od}$ is a common language dummy. (Both the data on bilateral trade flows and distance variables are from the CEPII; we use in particular the BACI database for trade flows.) In the above, φ_{okt} denotes exporter-by-product-by-year fixed effects, while φ_{dkt} denotes importer-by-product-by-year fixed effects; the estimation thus separates import demand from export supply forces, and we consider the φ_{dkt}'s as capturing demand shifters in the ROW that would be faced by Chinese exporters. We estimate (B.2) separately for each HS6-digit product, while excluding trade flows associated with China. We then construct the following measure of exposure to demand shocks in the ROW:

$$\sum_k \sum_{d \neq CHN} \frac{X_{idk,2010}}{\sum_i X_{idk,2010}} \frac{\Delta \hat{X}^{ROW}_{dkt}}{L_{i,2010}}, \tag{B.3}$$

where $\Delta \hat{X}^{ROW}_{dkt} = X^{ROW}_{dk,t-1} \Delta \varphi_{dkt}$. Note that by multiplying the change (in log form) in the product-specific demand shock in d ($\Delta \varphi_{dkt}$) with lagged product-k exports from the ROW to country d ($X^{ROW}_{dk,t-1}$), we obtain the change in exports from the ROW to d as predicted by a gravity-based estimate of the change in market capacity of importer d. Panel C makes use of this gravity-based Bartik IV from (B.3).

We also construct a second gravity-based measure that is analogous to our baseline IV from equation (3) in the main paper:

$$\sum_k \frac{X_{ik,2010}}{\sum_i X_{ik,2010}} \frac{\Delta \hat{X}^{ROW}_{kt}}{L_{i,2010}}. \tag{B.4}$$

Here, $\Delta \hat{X}^{ROW}_{kt} = \sum_{d \neq CHN} X^{ROW}_{dk,t-1} \Delta \varphi_{dkt}$ captures the implied demand shock for product k summed across all destination countries d in the ROW. Panel D makes use of this alternative gravity-based Bartik IV defined in (B.4).

Based on Export Growth Rates: In Panel E, we experiment with a measure of the ROW demand shock that is based on product-level export growth rates, as opposed to being

denominated in dollar units per worker. This is constructed as:

$$\kappa_i \sum_k \frac{X_{ik,2010}}{\sum_k X_{ik,2010}} \Delta \ln X_{kt}^{ROW}, \tag{B.5}$$

where $\Delta \ln X_{kt}^{ROW}$ is the product-level growth rate of exports from the ROW to the ROW. Note that the weights $X_{ik,2010}/\sum_k X_{ik,2010}$ are now the share of product k in total exports from prefecture i, and thus capture how important product k is for the prefecture. The term κ_i is a scaling term to capture the importance of exports for economic outcomes in the prefecture; in particular, we set κ_i to be total prefecture exports in 2012 divided by the working age population in 2000 ($X_{i,2012}/L_{i,2000}$). The above variable is in the same spirit as Aghion et al. (2018), who construct a Bartik-style export demand shock measure at the firm level. (There are a large number of zero trade observations in the product-level trade data we work with. We thus use in practice the Davis-Haltiwanger-Schuh approximation of the log growth rate, i.e., $\Delta \ln X_{kt}^{ROW} \approx 2\left(\frac{X_{kt}^{ROW} - X_{k,t-1}^{ROW}}{X_{kt}^{ROW} + X_{k,t-1}^{ROW}}\right)$, to avoid dropping observations where X_{kt}^{ROW} or $X_{k,t-1}^{ROW}$ is a zero.) The results from using (B.5) as a Bartik IV for $ExpShock_{it}$ are reported in Panel E. There is a decrease in the first-stage F-statistic to levels that are just above the rule-of-thumb value of 10 for instrument relevance; that said, the effects of the export shock on labor strikes and in inducing political responses from the local government remain statistically significant.

B.4 Dropping One HS Section at a Time

We assess whether our results hinge on the variation in export patterns inherent in any particular segment of products. To do so, we reconstruct both the export shock in (1) and the Bartik IV in (3), but leaving out the products from one HS section at a time. Bear in mind that the HS sections are broad – there are only 15 HS sections – so that the number of products dropped each time is large; there is thus a meaningful amount of variation left out with each iteration of this check.[51] If our baseline results are driven by endogeneity or pre-trend concerns that are associated with a particular sector – a concern articulated by Goldsmith-Pinkham et al. (2018) related to the use of Bartik instruments – one should expect the regression estimates to be sensitive when we drop all products from the corresponding HS section. For each dependent variable, we obtain 15 estimates of the export shock coefficient; we report the range of these coefficients in Table B.5. Across the columns, we always find that the largest and smallest coefficients obtained are negative and significantly different from zero. These findings alleviate the concern that there may be particularly pivotal or influential product segments for which the orthogonality conditions required for identification may be more questionable.

[51] The HS sections are: (i) Animal & Animal Products; (ii) Vegetable Products; (iii) Foodstuffs; (iv) Mineral Products; (v) Chemical & Allied Industries; (vi) Plastics/Rubbers; (vii) Raw Hides, Skins, Leather & Furs; (viii) Wood & Wood Products; (ix) Textiles; (x) Footwear/Headgear; (xi) Stone/Glass; (xii) Metals; (xiii) Machinery/Electrical; (xiv) Transportation; and (xv) Miscellaneous.

B.5 Effects of Future Export Shocks

To address the possibility that the results might be driven by pre-trends in the key variables, we examine in Table B.6 whether the export shock at time $t+1$ (as opposed to time t) has explanatory power over the outcomes of interest. In particular, we adopt the same IV specifications as in equations (2), (8) and (9), but replace $ExpShock_{it}$ by $ExpShock_{i,t+1}$, while instrumenting for the latter with the time-$(t+1)$ Bartik variable. In Column 1, this means that we examine whether the annual change in strikes per worker in year t (for the sample period 2013-2015) can be explained by the future export shock in year $(t+1)$; in Columns 2-5, we are exploring whether the political response measures observed in year t (for the sample period 2014-2016) respond with no lag to the contemporaneous year-t export shock. Across the columns, the export shock coefficient that we now estimate is smaller in magnitude than in the baseline results and typically not statistically significant. (The only exception is in Column 4, which reports a mildly significant but positive effect on social spending; if anything, one would need a reversion in pre-trends to rationalize the pattern for this particular outcome variable.) In sum, these findings suggest that prefectures hit by more negative exports shocks were not already experiencing faster deterioration in labor market conditions and social stability.

B.6 Balance Test of Product-level Export Shocks

The Bartik IV can be formulated more generally as $\sum_k s_{ik} g_k$, where g_k denotes the export shock experienced by product k and s_{ik} measures the exposure of location i to each product-level shock. (In our context, based on equation (3), we have: $g_k = \Delta_{kt}^{ROW} / \sum_i X_{ik,2010}$, and $s_{ik} = X_{ik,2010}/L_{i,2010}$.) As discussed in Borusyak et al. (2018), the validity of the instrument relies on the assumption that $\sum_k s_k g_k \phi_k \xrightarrow{\text{p}} 0$, where $s_k = E(s_{ik})$ measures the expected exposure to product k, and $\phi_k = E(s_{ik}\varepsilon_i)/E(s_{ik})$ is an exposure-weighted expectation of untreated potential prefecture-level outcomes. Put in other words, the identification relies on the assumption that, weighted by s_k, the correlation between product-level shocks g_k and unobservables ϕ_k approaches zero in large sample; this is the sense in which the shocks would then be as good as randomly assigned. In our context, this assumption could be violated if say export demand decreased more in products that happen to be produced in prefectures that were hit by other unobserved shocks that also affect social stability.

To allay this concern, we follow Borusyak et al. (2018) to test for whether the export shocks are balanced with respect to various initial prefecture characteristics that could in principle enter the ε_i. In particular, we regress g_{kt} on the empirical counterpart of $\phi_k = E(s_{ik}\varepsilon_i)/E(s_{ik})$, where ε_i comprises a set of various prefecture characteristics from 2010, namely: the share of workers with college education, share of manufacturing employment, export to GDP ratio, share of population without hukou rights, log GDP per capita, log fiscal revenue per capita. (The data are drawn from the Census, the China City Statistical Yearbook, and the prefecture-level yearbooks.) Table B.7 reports the results of this balance test. We report here the coefficient

estimates from regressing the g_{kt}'s (at the HS 6-digit level) against each of the weighted-average prefecture characteristics and year fixed effects (with the sample period being 2013-2015). Each regression is weighted by average industry exposure s_k, and the standard errors are clustered by 4-digit HS codes. The lack of statistical significance of the coefficients, both individually and jointly, provides supportive evidence that our empirical setting – and in particular, the HS 6-digit product-level ROW export shocks – meets the requirements for treatment balance.

B.7 Alternative Clustered Standard Errors

As pointed out in Adão et al. (2018), the regression residuals in shift-share empirical specifications would be correlated across regions that are similar in their sectoral composition, regardless of their geographic proximity, in the presence of unobserved sectoral shifters that affect the outcome of interest. As a result, standard errors that are clustered by geographic unit (in our context, by province) are likely biased downward. To address this potential problem, we construct alternative clusters based on the similarity of prefectures' export structure. For each prefecture, we calculate an index of the similarity of its initial vector of product-level export shares to that of each of the 30 provincial capitals. The index we use is based on Finger and Kreinin (1979):

$$SimilarityIndex_{ij}^{ROW} = \sum_k \min\left\{\frac{X_{ik}^{ROW}}{X_i^{ROW}}, \frac{X_{jk}^{ROW}}{X_j^{ROW}}\right\},$$

where X_{ik}^{ROW}/X_i^{ROW} (respectively, X_{jk}^{ROW}/X_j^{ROW}) denotes product k's share in the total exports of prefecture i (respectively, j) to the ROW. By construction, the index ranges between 0 to 1. If i's and j's export patterns are totally dissimilar, in that i only exports products that j does not (and vice versa), then the index takes on a value of 0. On the other extreme, if the export shares of the two prefectures are identical, then the index is equal to 1. We used the 2010 China customs data to construct this index, and then assigned each prefecture to an export-similarity cluster corresponding to the provincial capital with which its export profile was most similar.

In Table B.8, we report the robust standard errors under different modes of clustering. Row (i) reproduces our baseline standard errors, that are clustered at the province level. Row (ii) reports the standard errors clustered instead by export-similarity group. Row (iii) then presents standard errors that are two-way clustered by province and by export-similarity group. In Rows (iv) and (v), we repeat the exercise in Rows (ii) and (iii), but modify how the export-similarity groups are constructed; specifically, we group each prefecture with the provincial capital outside of its own province with which its export-similarity index is highest. With this, there is no overlap in the clusters at the province level and the export-similarity groups. The statistical inference that we draw is robust regardless of the mode of clustering.

As discussed in Adão et al. (2018), the spatial correlation of regression residuals induced by similarity in sectoral composition will be less of a concern when the number of industries

(in our case, products) in the shift-share IV is large, and when the shifter (in our case, export demand from the ROW) soaks up most of the sectoral shocks affecting the outcomes of interest. For our analysis, the number of products is more than 4,000. At the same time, the *annual* product-level export shocks that we exploit can be relatively large in magnitude. These features of our data potentially explain why our statistical inference is robust under alternative ways of clustering the standard errors. (Note that we cannot directly apply the standard-error correction approach proposed in Adão et al. (2018), since the number of products is larger than the number of prefectures (333) in our setting.)

B.8 Political Response to Export Shocks: Extension

In this appendix section, we consider an extension to our framework in which we explicitly model the upper-level government's concern with selecting high-quality local incumbents, in addition to properly incentivizing them. We have the same basic two-period model, but now there are two types of incumbents, $\ell \in \{H, L\}$, with the H-type being more capable than the L-type in terms of delivering social stability, holding all else constant. Specifically, local stability is given by:

$$y = \theta_\ell + x + (1-x)s + \varepsilon,$$

with $\theta_H > \theta_L$; recall that $\varepsilon \sim N(0, \sigma^2)$ is an iid draw from the normal distribution with mean 0 and variance σ^2. Again the upper-level government will follow a threshold strategy, keeping the incumbent if y exceeds $\bar{y}(x)$, and replacing him otherwise from an *ex ante* identical pool of potential officeholders. Note that ℓ is private information to each local officeholder, that is not directly observed by the upper-level government.

The officeholder of type ℓ would then choose s in order to maximize utility, U, given by:

$$\begin{aligned} U &= \Pr\left(y > \bar{y}(x)\right) R - g_\ell(s) \\ &= \left(1 - \Phi\left[\bar{y}(x) - \theta_\ell - x - (1-x)s\right]\right) R - g_\ell(s), \end{aligned}$$

where $\Phi(\cdot)$ denotes the cdf of the $N(0, \sigma^2)$ distribution, and $g_\ell(s)$ is a type-specific cost function. The first-order condition is then:

$$\phi\left[\bar{y}(x) - \theta_\ell - x - (1-x)s\right](1-x)R = g'_\ell(s), \tag{B.6}$$

where $\phi(\cdot)$ is the pdf of the $N(0, \sigma^2)$ distribution. Denote by s^*_ℓ the solution to equation (B.6). Let us further assume that $g'_\ell(s) = a_\ell + \delta s$, where $\delta > 0$, $a_H = 0$ and $a_L > R/\sqrt{2\pi\sigma^2}$, so that the H-type incurs a lower marginal cost of effort when implementing stability measures. In this case, $s^*_L = 0$ regardless of $\bar{y}(x)$, as the marginal cost of effort is always higher than the marginal benefit for type L. This means that the low type will always choose zero effort, as stability measures are too costly to him.

Now we turn to the strategy of the upper-level government. Recall that she can fully observe

x, and her utility is given by the expected stability level:

$$\begin{aligned} E(y) &= (1-x)(\pi_H s_H^* + \pi_L s_L^*) + (\pi_H \theta_H + \pi_L \theta_L) + x \\ &= (1-x)\pi_H s_H^* + (\pi_H \theta_H + \pi_L \theta_L) + x, \end{aligned}$$

where π_H and π_L denote respectively the shares of H-type and L-type among incumbents. The second equality follows because $s_L^* = 0$. Therefore, the upper-level leader can optimize the expected stability level by maximizing a H-type incumbent's effort towards maintaining stability s_H, by suitably choosing $\bar{y}(x)$ such that $\bar{y}^*(x) = \theta_H + x + (1-x)s_H^*$. Equation (B.6) for type H can thus be written as:

$$\frac{(1-x)R}{\delta\sqrt{2\pi\sigma^2}} = s_H^*, \tag{B.7}$$

which characterizes the equilibrium effort level of the H-type.

Equation (B.7) renders the following predictions on the incumbent's response to export shocks: (i) $\frac{ds_H^*}{dx} < 0$; (ii) $\frac{d^2 s_H^*}{dx dR} < 0$; and (iii) $\frac{d^2 s_H^*}{dx d\delta} > 0$, exactly as in the baseline model. On the probability of turnover, this is once again not a function of the export shock *per se*; in particular, the threshold $\bar{y}^*(x)$ adjusts in response to the export shock x. Moreover, a L-type incumbent would be more likely to be replaced than a H-type given the same observed level of the export shock x. To see this, with $\theta_H > \theta_L$ and $s_H^* > 0$, we have:

$$\Phi\left[\bar{y}(x) - \theta_L - x\right] > \Phi\left[\bar{y}(x) - \theta_H - x - (1-x)s_H^*\right].$$

Therefore, the cutoff rule also acts to screen out the L-type incumbents.

Figure B.1: Residual Scatterplot
(based on Column 3, Table 2)

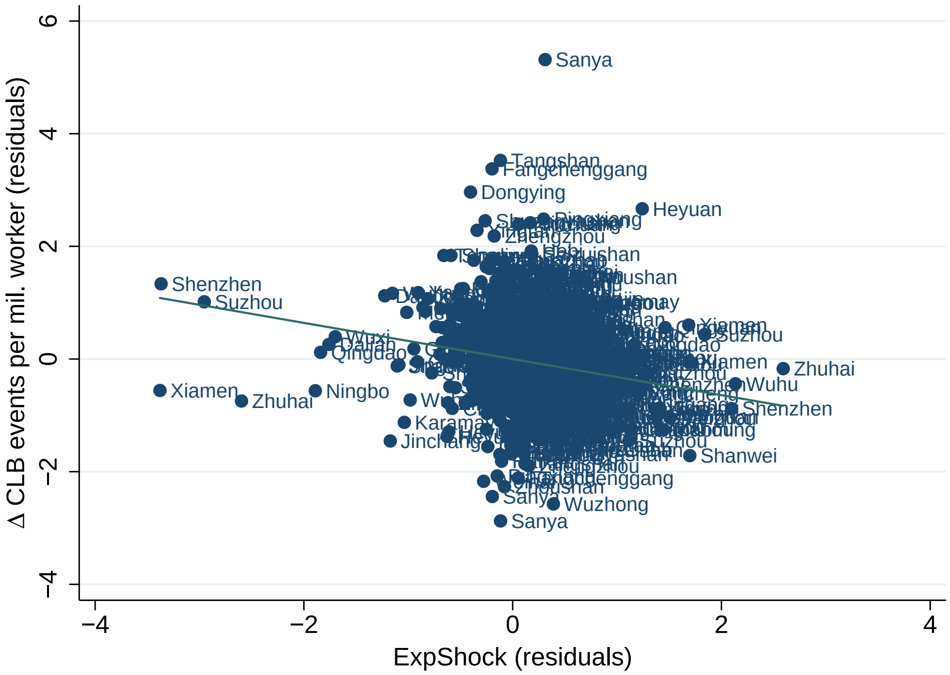


Figure B.2: Cross-Industry Correlation between Domestic Demand, Domestic Output and Export Shocks

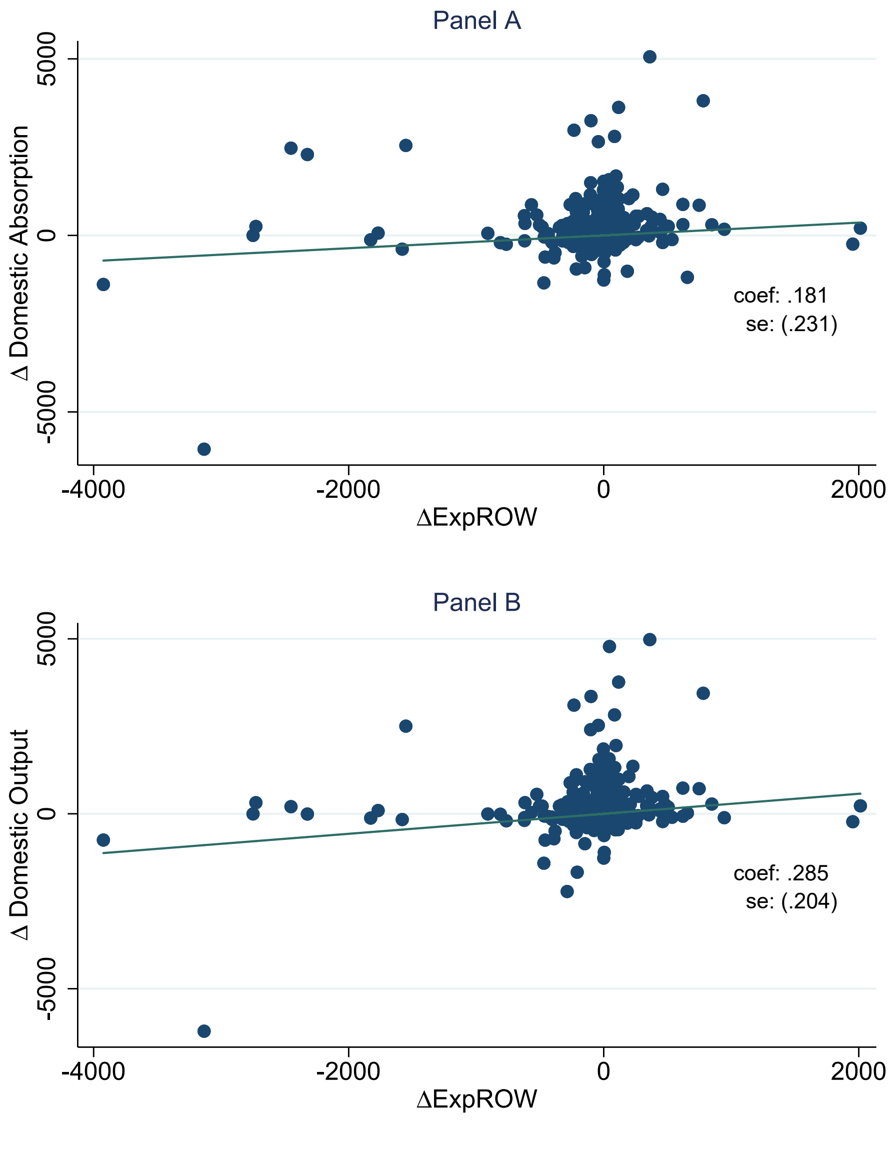


Table B.1: Robustness: Alternative FEs, Unweighted Regressions, and Dropping Lag Controls

Dependent variable:	**Δ CLB Events per million**$_{it}$ (1) IV	**Δ Log MNB "weiwen" score**$_{i,t+1}$ (2) IV	**Δ Log Fiscal Public Security**$_{i,t+1}$ (3) IV	**Δ Log Fiscal Social Spending**$_{i,t+1}$ (4) IV	**Party Secretary Turnover**$_{i,t+1}$ (5) IV
	Panel A: Region×Year FEs				
ExpShock$_{it}$	-0.2210***	-0.2181**	-0.0159***	-0.0292***	-0.0713***
	(0.0740)	(0.0793)	(0.0054)	(0.0087)	(0.0222)
Additional time-t controls?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Region-year dummies?	Y	Y	Y	Y	Y
First-stage F-stat	50.05	50.97	75.24	75.06	71.78
Observations	837	817	827	778	834
R^2	0.5783	0.4616	0.6839	0.7063	0.4994
	Panel B: Unweighted Regressions				
ExpShock$_{it}$	-0.2504***	-0.1484*	-0.0212**	-0.0085**	-0.0711***
	(0.0818)	(0.0753)	(0.0079)	(0.0032)	(0.0224)
Additional time-t controls?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Province-year dummies?	Y	Y	Y	Y	Y
First-stage F-stat	52.13	45.59	53.61	65.36	52.79
Observations	822	802	812	760	822
R^2	0.6513	0.5395	0.7654	0.7309	0.5660
	Panel C: Drop Lag Level Controls				
ExpShock$_{it}$	-0.1728**	-0.0358	-0.0235***	-0.0293***	-0.0461**
	(0.0746)	(0.0936)	(0.0069)	(0.0093)	(0.0189)
Additional time-t controls?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Province-year dummies?	Y	Y	Y	Y	Y
First-stage F-stat	105.6	106.4	107.1	134.2	85.54
Observations	822	802	812	760	822
R^2	0.5264	0.2800	0.6095	0.5879	0.3919

Notes: The dependent variable in Column 1 is the change in CLB-recorded events per million workers in prefecture i between year $t-1$ and t, while that in Columns 2-5 is the change in the respective political response measure between year t and $t+1$ (i.e., one year after the export shock). All columns report IV regressions, based on the specification in (2) for Column 1, (8) for Columns 2-4, and (9) for Column 5. Panel A uses region-year instead of province-year fixed effects. Panel B reports unweighted regressions, instead of weighting by the prefecture 2010 working-age population as in the rest of the table. Panel C drops the time-$(t-1)$ level of CLB events per million workers or the corresponding time-t level of the political response measures from the right-hand side controls. The additional time-t controls are those used in Column 3 of Table 2, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. Column 5 further includes the incumbent characteristics used as controls in Column 1 of Table 9. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

Table B.2: Robustness: Dropping One Province at a Time

Dependent variable:	**Δ CLB Events per million**$_{it}$ (1) IV	**Δ Log MNB "weiwen" score**$_{i,t+1}$ (2) IV	**Δ Log Fiscal Public Security**$_{i,t+1}$ (3) IV	**Δ Log Fiscal Social Spending**$_{i,t+1}$ (4) IV	**Party Secretary Turnover**$_{i,t+1}$ (5) IV
Range of Estimates:					
Min ExpShock$_{it}$ coef.	-0.3650***	-0.2436†	-0.0276***	-0.0191***	-0.0864***
	(0.1095)	(0.1514)	(0.0063)	(0.0063)	(0.0157)
Max ExpShock$_{it}$ coef.	-0.3022***	-0.1319***	-0.0164†	-0.0072*	-0.0666***
	(0.0476)	(0.0453)	(0.0102)	(0.0041)	(0.0242)
Additional time-t controls?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Province-year dummies?	Y	Y	Y	Y	Y

Notes: The dependent variable in Column 1 is the change in CLB-recorded events per million workers in prefecture i between year $t-1$ and t, while that in Columns 2-5 is the change in the respective political response measure between year t and $t+1$ (i.e., one year after the export shock). All columns report IV regressions, weighted by the prefecture's working-age population in 2010, based on the specification in (2) for Column 1, (8) for Columns 2-4, and (9) for Column 5. For each dependent variable, the regressions are run dropping each province in turn; the smallest and largest export shock coefficients with associated standard errors are reported. The additional time-t controls are those used in Column 3 of Table 2, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. Column 5 further includes the incumbent characteristics used as controls in Column 1 of Table 9. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1, † p<0.15.

Table B.3: Robustness: Controlling for Other Prefecture-Level Shocks

Dependent variable:	**Δ CLB Events per million**$_{it}$	**Δ Log MNB "weiwen" score**$_{i,t+1}$	**Δ Log Fiscal Public Security**$_{i,t+1}$	**Δ Log Fiscal Social Spending**$_{i,t+1}$	**Party Secretary Turnover**$_{i,t+1}$
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
	Panel A: Domestic Absorption Shocks				
ExpShock$_{it}$	-0.2303***	-0.2393**	-0.0232***	-0.0186**	-0.0746**
	(0.0679)	(0.1045)	(0.0060)	(0.0071)	(0.0272)
AbsorptionShock$_{it}$	-0.2399*	0.1206	0.0043	0.0071	0.0009
	(0.1299)	(0.0982)	(0.0055)	(0.0063)	(0.0367)
First-stage F-stat	51.12	47.93	64.79	95.50	54.46
Observations	822	802	812	760	822
R^2	0.6655	0.5096	0.7726	0.7787	0.5470
	Panel B: Domestic Output Shocks				
ExpShock$_{it}$	-0.2062**	-0.1740***	-0.0224***	-0.0213***	-0.0504†
	(0.0837)	(0.0583)	(0.0045)	(0.0051)	(0.0328)
OutputShock$_{it}$	-0.2075*	-0.0278	0.0016	0.0096**	-0.0398
	(0.1059)	(0.0543)	(0.0077)	(0.0041)	(0.0550)
First-stage F-stat	24.71	22.02	24.57	34.37	21.45
Observations	822	802	812	760	822
R^2	0.6671	0.5167	0.7735	0.7756	0.5564
	Panel C: Domestic Absorption & Domestic Output Shocks				
ExpShock$_{it}$	-0.2190***	-0.1436***	-0.0217***	-0.0216***	-0.0420
	(0.0768)	(0.0489)	(0.0044)	(0.0041)	(0.0295)
AbsorptionShock$_{it}$	-0.1943	0.5172	0.0100	-0.0051	0.1293
	(0.1535)	(0.3564)	(0.0148)	(0.0140)	(0.0965)
OutputShock$_{it}$	-0.0517	-0.4355	-0.0062	0.0134	-0.1423
	(0.0857)	(0.3024)	(0.0175)	(0.0109)	(0.1173)
First-stage F-stat	22.79	21.09	23.12	34.07	20.25
Observations	822	802	812	760	822
R^2	0.6669	0.5339	0.7748	0.7752	0.5620
	Panel D: Import Shocks				
ExpShock$_{it}$	-0.3151***	-0.1947**	-0.0215***	-0.0152**	-0.0798***
	(0.0550)	(0.0803)	(0.0077)	(0.0058)	(0.0249)
ImpShock$_{it}$	-0.1259	0.0888	0.0033	-0.0148	0.1174
	(0.3743)	(0.2260)	(0.0179)	(0.0138)	(0.1178)
First-stage F-stat	118.0	135.1	239.0	224.3	180.6
Observations	822	802	812	760	822
R^2	0.6476	0.5142	0.7746	0.7821	0.5463
Additional time-t controls?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Province-year dummies?	Y	Y	Y	Y	Y

Notes: The dependent variable in Column 1 is the change in CLB-recorded events per million workers in prefecture i between year $t-1$ and t, while that in Columns 2-5 is the change in the respective political response measure between year t and $t+1$ (i.e., one year after the export shock). All columns report IV regressions, weighted by the prefecture's working-age population in 2010, based on the specification in (2) for Column 1, (8) for Columns 2-4, and (9) for Column 5. The prefecture-level absorption, output, and import shocks are constructed as described in Section B.2. The additional time-t controls are those used in Column 3 of Table 2, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. Column 5 further includes the incumbent characteristics used as controls in Column 1 of Table 9. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1, † p<0.15.

Table B.4: Robustness: Alternative Bartik Measures

Dependent variable:	Δ CLB Events per million$_{it}$	Δ Log MNB "weiwen" score$_{i,t+1}$	Δ Log Fiscal Public Security$_{i,t+1}$	Δ Log Fiscal Social Spending$_{i,t+1}$	Party Secretary Turnover$_{i,t+1}$
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
	Panel A: Excluding Trade by Intermediary Firms				
ExpShock$_{it}$	-0.3205***	-0.1854**	-0.0193*	-0.0129**	-0.0636***
	(0.0652)	(0.0727)	(0.0094)	(0.0058)	(0.0188)
First-stage F-stat	122.4	156.1	193.7	84.60	177.5
Observations	822	802	812	760	822
R^2	0.6512	0.5137	0.7751	0.7793	0.5584
	Panel B: Destination-specific Demand Shocks				
ExpShock$_{it}$	-0.3093***	-0.1508*	-0.0180**	-0.0142*	-0.0952***
	(0.0600)	(0.0789)	(0.0072)	(0.0074)	(0.0207)
First-stage F-stat	28.75	28.46	37.86	47.85	35.40
Observations	822	802	812	760	822
R^2	0.6483	0.5183	0.7782	0.7818	0.5376
	Panel C: Gravity-based Instrument – Equation (B.3)				
ExpShock$_{it}$	-0.4145***	-0.1969*	-0.0173*	-0.0162†	-0.0750†
	(0.0675)	(0.1143)	(0.0089)	(0.0101)	(0.0480)
First-stage F-stat	51.43	63.91	90.69	69.34	104.7
Observations	822	802	812	760	822
R^2	0.6255	0.5137	0.7788	0.7803	0.5468
	Panel D: Gravity-based Instrument – Equation (B.4)				
ExpShock$_{it}$	-0.3536***	-0.1911*	-0.0214**	-0.0156*	-0.0823**
	(0.0559)	(0.0942)	(0.0088)	(0.0079)	(0.0323)
First-stage F-stat	272.1	290.3	300.1	117.6	287.3
Observations	822	802	812	760	822
R^2	0.6400	0.5145	0.7748	0.7808	0.5438
	Panel E: Growth-rate Instrument – $\kappa_i = X_{i,2012}/L_{i,2000}$				
ExpShock$_{it}$	-0.2199***	-0.1716**	-0.0177***	-0.0160***	-0.1078***
	(0.0348)	(0.0638)	(0.0042)	(0.0049)	(0.0183)
First-stage F-stat	11.32	12.32	12.16	13.07	12.79
Observations	822	802	812	760	822
R^2	0.6589	0.5167	0.7785	0.7805	0.5304
Additional time-t controls?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Province-year dummies?	Y	Y	Y	Y	Y

Notes: The dependent variable in Column 1 is the change in CLB-recorded events per million workers in prefecture i between year $t-1$ and t, while that in Columns 2-5 is the change in the respective political response measure between year t and $t+1$ (i.e., one year after the export shock). All columns report IV regressions, weighted by the prefecture's working-age population in 2010, based on the specification in (2) for Column 1, (8) for Columns 2-4, and (9) for Column 5. The alternative Bartik IVs are constructed as described in Section B.3. The additional time-t controls are those used in Column 3 of Table 2, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. Column 5 further includes the incumbent characteristics used as controls in Column 1 of Table 9. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1, † p<0.15.

Table B.5: Robustness: Dropping One HS Section at a Time

Dependent variable:	**Δ CLB Events per million$_{it}$** (1) IV	**Δ Log MNB "weiwen" score$_{i,t+1}$** (2) IV	**Δ Log Fiscal Public Security$_{i,t+1}$** (3) IV	**Δ Log Fiscal Social Spending$_{i,t+1}$** (4) IV	**Party Secretary Turnover$_{i,t+1}$** (5) IV
Range of Estimates:					
Min ExpShock$_{it}$ coef.	-0.6442***	-0.3864**	-0.0426***	-0.0292***	-0.1113**
	(0.1525)	(0.1877)	(0.0139)	(0.0112)	(0.0476)
Max ExpShock$_{it}$ coef.	-0.3086***	-0.1387***	-0.0208***	-0.0155***	-0.0711***
	(0.0530)	(0.0508)	(0.0068)	(0.0057)	(0.0189)
Additional time-t controls?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Province-year dummies?	Y	Y	Y	Y	Y

Notes: The dependent variable in Column 1 is the change in CLB-recorded events per million workers in prefecture i between year $t-1$ and t, while that in Columns 2-5 is the change in the respective political response measure between year t and $t+1$ (i.e., one year after the export shock). All columns report IV regressions, weighted by the prefecture's working-age population in 2010, based on the specification in (2) for Column 1, (8) for Columns 2-4, and (9) for Column 5. For each dependent variable, the regressions drop trade flows from one HS section at a time from $ExpShock_{it}$ and the construction of the $ExpShockROW_{it}$ IV; the smallest and largest export shock coefficients with associated standard errors are reported. The additional time-t controls are those used in Column 3 of Table 2, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. Column 5 further includes the incumbent characteristics used as controls in Column 1 of Table 9. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

Table B.6: Effects of Future Export Shocks

Dependent variable:	**Δ CLB Events per million$_{it}$** (1) IV	**Δ Log MNB "weiwen" score$_{i,t+1}$** (2) IV	**Δ Log Fiscal Public Security$_{i,t+1}$** (3) IV	**Δ Log Fiscal Social Spending$_{i,t+1}$** (4) IV	**Party Secretary Turnover$_{i,t+1}$** (5) IV
ExpShock$_{i,t+1}$	-0.1051	-0.0378	-0.0002	0.0031*	-0.0336
	(0.0791)	(0.0729)	(0.0034)	(0.0016)	(0.0298)
Additional time-t controls?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Province-year dummies?	Y	Y	Y	Y	Y
First-stage F-stat	24.38	24.69	24.98	26.46	22.33
Observations	822	802	812	760	822
R^2	0.6486	0.5045	0.7678	0.7681	0.5601

Notes: The dependent variable in Column 1 is the change in CLB-recorded events per million workers in prefecture i between year $t-1$ and t, while that in Columns 2-5 is the change in the respective political response measure between year t and $t+1$ (i.e., one year after the export shock). All regressions are weighted by the prefecture's working-age population in 2010, based on the specification in (2) for Column 1, (8) for Columns 2-4, and (9) for Column 5, but with $ExpShock_{it}$ replaced by $ExpShock_{i,t+1}$ and instrumented with by the time-$(t+1)$ Bartik variable. The additional time-t controls are those used in Column 3 of Table 2, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. Column 5 further includes the incumbent characteristics used as controls in Column 1 of Table 9. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

Table B.7: Balance Test of Industry Shocks

	(1) Coef.	(2) SE
Share of college educated (%)	0.0010	(0.0027)
Manufacturing employment share (%)	0.0723	(0.0460)
Export to GDP ratio (%)	0.1892	(0.1235)
Share of population without Hukou (%)	0.0839	(0.0562)
Log GDP per capita	0.0001	(0.0005)
Log fiscal revenue per capita	0.0006	(0.0007)

Joint significance test: $\chi^2(6)$=2.77, p-value=0.8367

Notes: This table reports coefficients from regressing product-specific weighted averages of beginning-of-period prefecture characteristics on HS6 product-level export shocks and year fixed effects. Standard errors are clustered by HS 4-digit codes. The regressions are weighted by the average HS6 product-level export exposure across prefectures. Coefficients are multiplied by 100 for readability; none of the coefficient estimates are significant at the 10% level.

Table B.8: Robustness: Alternative Clustered Standard Errors

Dependent variable:	**Δ CLB Events per million**$_{it}$ (1) IV	**Δ Log MNB "weiwen" score**$_{i,t+1}$ (2) IV	**Δ Log Fiscal Public Security**$_{i,t+1}$ (3) IV	**Δ Log Fiscal Social Spending**$_{i,t+1}$ (4) IV	**Party Secretary Turnover**$_{i,t+1}$ (5) IV
ExpShock$_{it}$	-0.3207	-0.1904	-0.0214	-0.0160	-0.0742
Robust Standard Errors Clustered at:					
(i) province	(0.0539)***	(0.0725)**	(0.0069)***	(0.0059)**	(0.0192)***
(ii) export similarity	[0.0589]***	[0.0615]***	[0.0051]***	[0.0045]***	[0.0299]**
(iii) two-way clustering: (i) and (ii)	{0.0547}***	{0.0572}***	{0.0064}***	{0.0026}***	{0.0217}***
(iv) export similarity: outside prov.	⟨0.0827⟩***	⟨0.0546⟩***	⟨0.0050⟩***	⟨0.0047⟩***	⟨0.0277⟩***
(v) two-way clustering: (i) and (iv)	⟦0.0730⟧***	⟦0.0510⟧***	⟦0.0066⟧***	⟦0.0029⟧***	⟦0.0223⟧***
Additional time-t controls?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Province-year dummies?	Y	Y	Y	Y	Y
Observations	822	802	812	760	822
R^2	0.6464	0.5146	0.7747	0.7805	0.5472

Notes: The dependent variable in Column 1 is the change in CLB-recorded events per million workers in prefecture i between year $t-1$ and t, while that in Columns 2-5 is the change in the respective political response measure between year t and $t+1$ (i.e., one year after the export shock). All regressions are weighted by the prefecture's working-age population in 2010, based on the specification in (2) for Column 1, (8) for Columns 2-4, and (9) for Column 5. The additional time-t controls are those used in Column 3 of Table 2, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. Column 5 further includes the incumbent characteristics used as controls in Column 1 of Table 9. Robust standard errors are clustered as described in each respective row. *** p<0.01, ** p<0.05, * p<0.1.

Table B.9: Temporal Correlation between Baidu "Weiwen" Search Index and CLB Events

Dependent variable:	**Δ Log Baidu "weiwen" search index**$_{i,w}$			
	(1) OLS	(2) OLS	(3) OLS	(4) OLS
Δ CLB Events per million workers$_{i,w-6}$	0.3207	0.5673**	0.3275*	0.5071**
	(0.2123)	(0.2696)	(0.1953)	(0.2409)
Δ CLB Events per million workers$_{i,w-5}$	0.7145***	0.9824***	0.6563***	0.7871**
	(0.2340)	(0.3061)	(0.2506)	(0.3367)
Δ CLB Events per million workers$_{i,w-4}$	0.7669***	1.1771***	0.6367**	0.7774**
	(0.2562)	(0.3115)	(0.2796)	(0.3662)
Δ CLB Events per million workers$_{i,w-3}$	0.6308**	0.9596***	0.5231*	0.4811
	(0.2585)	(0.3042)	(0.2949)	(0.3697)
Δ CLB Events per million workers$_{i,w-2}$	0.9440***	1.1208***	0.7206**	0.4865
	(0.2569)	(0.3351)	(0.2944)	(0.4031)
Δ CLB Events per million workers$_{i,w-1}$	0.7674***	0.9930***	0.4240	0.1984
	(0.2540)	(0.3279)	(0.2828)	(0.3724)
Δ CLB Events per million workers$_{i,w}$	0.3650	0.4586	0.0216	-0.2494
	(0.2248)	(0.3199)	(0.2534)	(0.3503)
Δ CLB Events per million workers$_{i,w+1}$	0.1573	0.2432	-0.0972	-0.2595
	(0.2002)	(0.2858)	(0.2280)	(0.3184)
Δ CLB Events per million workers$_{i,w+2}$	0.1358	0.1424	-0.0231	-0.1362
	(0.1957)	(0.2537)	(0.1953)	(0.2419)
Log Baidu "weiwen" search index$_{i,w-1}$	-0.6120***	-0.5687***	-0.9053***	-0.9132***
	(0.0260)	(0.0307)	(0.0056)	(0.0065)
Weighted?	N	Y	N	Y
Prefecture dummies?	N	N	Y	Y
Province-week dummies?	Y	Y	Y	Y
Observations	63,232	63,232	63,232	63,232
R^2	0.3840	0.3698	0.5144	0.5217

Notes: The dependent variable is the change in the prefecture log Baidu index score between week $w-1$ and w. The sample comprises all weeks in the years 2012-2015. All regressions are estimated by OLS, with six lags and two leads of the weekly change in CLB-recorded events per million workers included as right-hand side variables. All columns include province-by-week fixed effects, while Columns 3-4 further include prefecture fixed effects. Columns 2 and 4 use the prefecture working-age population from the 2010 Census as regression weights. Robust standard errors are clustered at the prefecture level. *** p<0.01, ** p<0.05, * p<0.1.

Table B.10: Export Shocks and Fiscal Expenditure Shares

Dependent variable:	Δ 100 $\times$ (**Share of Fiscal measure**)$_{i,t+1}$			
Fiscal measure:	**Stability Measures**	**Public Security**	**Social Spending**	**Other Spending**
	(1)	(1a)	(1b)	(2)
	IV	IV	IV	IV
ExpShock$_{it}$	-0.1500*	-0.0677**	-0.0878	0.1500*
	(0.0845)	(0.0266)	(0.0757)	(0.0845)
100$\times$Share Fiscal Measure$_{it}$	-0.8772***	-0.8745***	-0.8735***	-0.8772***
	(0.0478)	(0.0655)	(0.0453)	(0.0478)
Additional time-t controls?	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y
Province-year dummies?	Y	Y	Y	Y
First-stage F-stat	127.8	109.5	126.3	127.8
Observations	755	812	760	755
R^2	0.7815	0.8094	0.7633	0.7815

Notes: The dependent variable is the change in the share of fiscal expenditure under the respective column headings in prefecture i between year t and $t+1$ (i.e., one year after the export shock). All columns report IV regressions, weighted by the prefecture's working-age population in 2010, based on the specification in (8). The additional time-t controls are those used in Column 3 of Table 2, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

Table B.11: Export Shocks and Prefecture Fiscal Expenditure by Social Spending Categories

Dependent variable:	Δ **Log Fiscal measure**$_{i,t+1}$				
Fiscal measure:	**Public Services**	**Education**	**Social Security**	**Medical Services**	**Public Housing**
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
	Panel A: Average Effects				
ExpShock$_{it}$	-0.0087*	-0.0121***	0.0058	-0.0076*	-0.0524**
	(0.0046)	(0.0034)	(0.0083)	(0.0039)	(0.0238)
Log Fiscal Measure$_{it}$	-0.9770***	-0.8676***	-1.0897***	-0.9575***	-1.1611***
	(0.0546)	(0.0671)	(0.1004)	(0.0620)	(0.1170)
First-stage F-stat	104.8	166.5	76.06	118.8	160.1
Observations	814	817	816	817	764
R^2	0.7606	0.8088	0.6791	0.7847	0.7403
	Panel B: Heterogeneous Effects				
ExpShock$_{it}$	0.0338	0.0452***	0.1015	-0.0761***	0.2498**
	(0.0308)	(0.0147)	(0.0600)	(0.0198)	(0.0986)
Δ(Events/L)$_{it}$×ExpShock$_{it}$	-0.0132***	-0.0041	-0.0048	0.0045	-0.0817***
	(0.0045)	(0.0033)	(0.0081)	(0.0030)	(0.0099)
(FiscalRev/L)$_{i,2012}$×ExpShock$_{it}$	-0.0120	-0.0318***	-0.0544	0.0405***	-0.1007
	(0.0192)	(0.0092)	(0.0447)	(0.0103)	(0.0682)
(49≤Age≤53)$_{it}$×ExpShock$_{it}$	-0.0100	-0.0165	-0.0314	0.0029	-0.0506
	(0.0170)	(0.0101)	(0.0202)	(0.0139)	(0.0649)
Δ(Events/L)$_{it}$	-0.0037	0.0012	0.0129**	0.0029	-0.0150
	(0.0038)	(0.0033)	(0.0058)	(0.0044)	(0.0148)
(49≤Age≤53)$_{it}$	-0.0119	-0.0099	0.0019	-0.0077	-0.0441
	(0.0118)	(0.0097)	(0.0126)	(0.0149)	(0.0289)
Log Fiscal Measure$_{it}$	-0.9861***	-0.8713***	-1.0548***	-0.9964***	-1.1692***
	(0.0610)	(0.0695)	(0.0886)	(0.0668)	(0.1144)
First-stage F-stat	18.55	22.80	20.28	21.50	17.31
Observations	814	817	816	817	764
R^2	0.7582	0.8139	0.6744	0.7710	0.7354
Additional time-t controls?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Province-year dummies?	Y	Y	Y	Y	Y

Notes: The dependent variable is the change in log fiscal expenditure by social spending categories under the respective column headings in prefecture i between year t and $t+1$ (i.e., one year after the export shock). All columns report IV regressions, weighted by the prefecture's working-age population in 2010, based on the specification in (8). Panel A reports the average effects of the export shock on the respective fiscal spending measures. Panel B explores heterogeneous effects: The $\Delta(\text{Events}/L)_{it}$ variable is the change in CLB-recorded events per million between year $t-1$ and t. $(\text{FiscalRev}/L)_{i,2012}$ is the initial local fiscal revenue per worker in 2012. $(49{\leq}\text{Age}{\leq}53)_{it}$ is a dummy variable for whether the prefecture party secretary is between ages 49 and 53 (inclusive) in year t. The additional time-t controls are those used in Column 3 of Table 2, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.