The Political Economy Consequences of China's Export Slowdown*

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Abstract

We study how adverse economic shocks influence political outcomes in strong authoritarian regimes, by examining the export slowdown in China during the mid-2010s. We first show that prefectures that experienced a more severe export slowdown witnessed a significant increase in incidents of labor strikes, using a shift-share instrumental variables strategy. The prefecture party secretary was subsequently more likely to be replaced by the central government, particularly if the rise in strikes was greater than in other prefectures that saw comparable export slowdowns. These patterns are consistent with a simple framework we develop, where the central government makes strategic use of a turnover decision to induce effort from and screen local officials for retention. In line with the framework's predictions, we find a heightened emphasis by local party secretaries – particularly younger officials whose career concerns are stronger – on upholding domestic stability following negative export shocks. This is evident in both words (from textual analysis of official speeches) and deeds (from expenditures on public security and social spending).

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

JEL codes: D73, D74, F10, F14, F16, H10, J52, P26

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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 a decrease in support for the government, and in extreme cases, even trigger 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 is singularly prominent in the context of China, where it has regularly been posited that high rates of economic growth are crucial to the stability of the regime.[1] At the same time, we know relatively little about how the modern-day Chinese political system would respond to weak economic conditions, since its incumbents do not need to face voters, as in democracies, nor need they imminently fear being removed by popular uprisings, as in weakly institutionalized polities. Instead, China is a leading example of a nondemocratic regime with relatively strong levels of state capacity. Understanding the political response of such regimes to negative economic shocks is of increasing relevance, not only because of the specific case of China, but also in light of the "democratic recession" that appears to have increased the number of authoritarian regimes around the world (Diamond 2015).[2]

In this paper, we study the political impact of negative economic shocks in the context of a strong nondemocratic regime, using the opportune setting afforded by the marked slowdown in China's export performance during the mid-2010s. While Chinese merchandize exports grew at a rapid average annual rate of 18% between 1992-2008 (Hanson 2012), this slowed considerably in tandem with the weak recovery of trade flows in the rest of the world following the global financial crisis (see Figure 1). China's manufacturing exports registered an average annual growth rate of just 0.6% between 2013-2016, even slipping into a contraction for two years.[3] While these exports bounced back briefly in 2017, the onset of the US-China "tariff war" threatens to be an ongoing drag on the outlook moving forward.

The slowdown has sparked concerns over the potential impact on domestic labor markets and workers, given the prominent role that exports have played in driving China's economic development and employment since the early 1990s.[4] Indeed, as export manufacturing orders

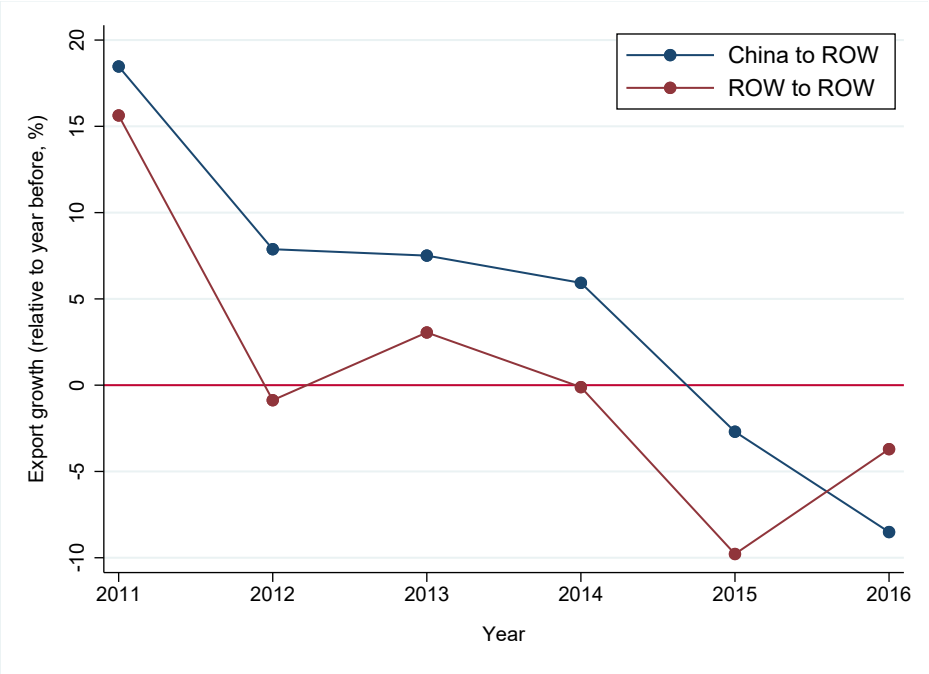
[1] As put by *The Economist* (1 Jun 2011): "The [Chinese] government cites stability as its source of legitimacy, and it draws a tight connection between stability and economic growth." Or, in the words of then-US Treasury Secretary Henry Paulson: "The Chinese see economic growth as essential to their stability" (Paulson 2008).

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

[3] Calculated from UN Comtrade data, as the average of the growth rates recorded in 2012-2013, 2013-2014, 2014-2015, and 2015-2016. For comparison, the nominal value of manufacturing exports from the rest of the world to all destinations excluding China decreased during this same period at an average annual rate of -2.6%. Note that while we refer to the years we study as 2013-2016, it should be understood that data for 2012 are used to calculate annual changes that we associate with 2013.

[4] 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

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



declined in the mid-2010s, reports also emerged of a rise in layoffs and factory shutdowns; in several instances, factory managers were even alleged to have fled and absconded with company funds. This in turn set off strikes over job losses and unpaid wage arrears.[5] While each labor grievance case has typically been localized in nature, the reports of these strikes have nevertheless been spread out geographically across China. This has led to concerns that the cumulation of such labor strikes could compromise domestic political stability.[6]

We make two contributions that shed light on the political economy consequences of China's export slowdown. First, we present formal evidence to link a negative shock in exports to a rise in labor strikes, using an empirical strategy that exploits variation in the extent of the slowdown across and within Chinese prefectures over time. Second, we trace out the impact of the export slowdown through the Chinese political system, specifically its implications for turnover in

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.

[5]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). 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).

[6]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).

local leaders. We show that the central government appears to hold local leaders closely to account for their performance in managing labor unrest and maintaining social stability in the face of these export headwinds, through the strategic use of decisions over their retention and replacement.

To establish these points, we marshal a combination of both conventional and novel data sources. A key part of our contribution here is to overcome the challenge posed by the tight state control of news and information within China, in order to introduce systematic data at the local level on labor strikes and the political responses to these events.

Specifically, we first establish that the slowdown in exports led to a rise in incidents of labor strikes. We do that by drawing on the records of strikes maintained by the China Labour Bulletin (CLB) – a labor rights non-governmental organization – and build around this a prefecture-level panel dataset of annual observations from 2013-2016.[7] To make the case that this relationship is causal in nature, we adopt a shift-share or Bartik instrumental variable (IV) for the severity of the export slowdown (c.f., Autor et al. 2013). This exploits the fact that prefectures differ in the initial mix of goods they export; this generates variation in how inherently exposed each locality would be to product-level shocks in world trade flows, that reflect (to a large extent) the weak and uneven recovery in external demand conditions in the aftermath of the global financial crisis (as documented for example in the 2016 IMF World Economic Outlook). Our findings are robust under an extensive battery of checks, including recent recommendations advanced for validating the use of a Bartik IV. We show in particular that the product-level trade shocks pass tests to verify that they are "as good as randomly assigned" to Chinese prefectures (Borusyak et al. 2020); that our results are not driven by a small number of products that may have seen large shocks (Goldsmith-Pinkham et al. 2020); and that they are robust to concerns about statistical inference (Adão et al. 2019).

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 0.15 more recorded labor events per million workers; this is 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 where the underlying cause was "wage arrears" and/or "layoffs", while also obtaining a consistent picture of the adverse impact of the export shock when examining other contemporaneous economic outcome variables, such as the manufacturing employment share at the prefecture level.

We then turn to the implications of this labor unrest for incumbent turnover. Given the distinctive features of China's hierarchical political system, we start by sketching out a model of "political accountability with Chinese characteristics", building on Persson and Zhuravskaya (2016), in which the threat of removal for local incumbents stems from personnel decisions

[7]Prefectures are a sub-provincial administrative unit. The analysis stops in 2016 due to limitations on the data on labor strikes thereafter (see Section 3.1), and also given the substantial change in the forces affecting China's exports following the start of the US-China "tariff war".

made in the upper-level of government. Following the scholarship that has emphasized the role of screening and incentives in the evaluation of local officials in China (e.g., Heberer and Trappel 2013), we posit that the central government can use this turnover decision to screen for high-capability local agents and to incentivize them to adopt measures that would bolster social stability. The model delivers predictions for how the local severity of the export shock would affect: (i) turnover of local incumbents; and (ii) the intensity of stability-enhancing measures they would adopt. In particular, it allows us to distinguish between the case of a "sophisticated" central government (who conditions the turnover decision on the observed severity of the local export shock), and that of an "unsophisticated" central government (who adopts a uniform rule regardless of the extent of the local slowdown).

It turns out that the predicted impact of export shocks on these political economy outcomes hinges crucially on that distinction. Specifically, the model predicts that both the probability of removal faced by local agents and their effort in maintaining stability increase monotonically with the severity of the export slowdown, when they are evaluated by a "sophisticated" upper-level government. Intuitively, when the export shock is more severe, high-capability local agents would intensify their effort to deliver better stability outcomes, in order to separate themselves from their low-capability peers; this is turn raises observed turnover, as low-capability incumbents in particular face a higher likelihood of removal. In contrast, the same is not true in the "unsophisticated" case: If the upper-level government disregards the severity of the shock and instead assesses local leaders on the absolute increase in labor unrest, even high-capability incumbents would exert little effort when faced with a very negative economic shock, as this would have minimal effect on improving their chances of retention; this in turn weakens the effectiveness of the screening mechanism.

Bearing these predictions on turnover in mind, we compile information on the career histories of prefecture-level party secretaries (the highest-ranking local official). Using a similar Bartik IV strategy as above, we find that prefectures that were hit by more negative export shocks also witnessed an increased likelihood of incumbent turnover. This effect is driven by movements where the incumbent was re-assigned to a position of lateral rank, often early in their tenure as party secretary; in the context of the Chinese political system, we show that this slows an official's career progression, leaving a dent in one's eventual promotion prospects. Our estimates suggest that a one-standard-deviation more severe export shock would increase an incumbent's likelihood of being laterally moved by 7.6 percentage points, which is consequential given that the average rate of such turnover was 16.3% in our sample period.

These turnover decisions appear to be tied to the local official's performance in managing labor unrest in the shadow of an export slowdown: Comparing prefectures where exports took a similar hit (specifically, looking within each tercile bin of our export shock IV), we find that party secretaries were more likely to be laterally moved if there was a larger increase in labor strikes under their watch. This suggests that the local leaders were evaluated relative to

their peers in prefectures that saw comparable export shocks, which aligns with the case of a "sophisticated" upper-level government.

Turning to the predictions on effort, we examine two dimensions of local officials' responses. First, we perform a textual analysis of prefecture work reports – an annual speech delivered by the party secretary – taking advantage of the observation that certain phrases – most notably, "weiwen" ("维稳") or "maintaining social stability" – have been adopted by the party establishment in China as watchwords to communicate the importance of domestic law and order as a political priority. We construct measures of the degree of "weiwen" emphasis in these policy speeches and find that a negative shock to a prefecture's exports is associated in the subsequent year with a discernible rise in these "weiwen" scores. Second, we show evidence of more concrete fiscal responses by local incumbents, using data on prefecture government expenditures that we collected from local statistical sources. We establish that a more severe export slowdown led to a subsequent increase in expenditures channelled towards public security uses (to safeguard law and order) and towards social spending (to potentially assuage worker grievances).

We moreover find that these political responses – a heightened "weiwen" emphasis, as well as increased public security and social spending – were strongest in the prefectures that experienced the most severe export contractions. This is in line with what our model predicts under a "sophisticated" central government, that seeks to incentivize effort to bolster stability even when economic conditions are very adverse. Consistent too with our modeling framework, these political responses to the export slowdown vary according to the strength of the official's career concerns: The effort exerted in stability measures was most intense among younger party secretaries, but this tapered off for officials nearing the mandatory retirement age of 60.

Our results thus paint a coherent picture of the political economy consequences of negative economic shocks in China and, more broadly, in nondemocratic strong states. Our model predicts that incumbents who have been hit by a negative economic shock face a greater likelihood of turnover; this is not unlike what has been observed in both democracies or weak autocracies (see the literature review in Section 2 below). Where the strong autocracy is distinct, though, lies in the nature of how political "accountability" is exercised, namely within the system from above. We show that the distinction has meaningful implications: It entails for instance that in strong autocracies, it is the local officials with brighter career prospects who will mount stronger responses to bolster regime stability. Moreover, a key takeaway is that a central government seeking to properly incentivize local officials in the face of negative economic shocks would indeed assess them by a relative standard, rather than an absolute one.

The paper proceeds as follows. Section 2 reviews the related literature. Section 3 describes our main data sources and the empirical strategy. Section 4 presents the findings on the effects of the export slowdown on labor strikes. Section 5 lays out a model linking export shocks, social stability, and incumbent responses. Section 6 then reports our empirical findings on incumbent turnover and on measures to bolster domestic stability. Section 7 concludes.

2 Related Literature

Our paper engages three strands in the literature. First and foremost, it connects with a broader set of studies on the political ramifications of negative economic shocks. In the context of democracies, it has been argued that an incumbent's response to such shocks can reveal information about his/her quality (e.g., Fearon 1999), and that the threat of electoral punishment for a bad response can be a powerful incentive that shapes a leader's behavior (e.g., Barro 1973, Ferejohn 1986).[8] This has motivated an extensive body of empirical work on "economic voting", to examine whether voters do in practice hold politicians accountable for a weak economy at the ballot box (e.g., Lewis-Beck 1998, Duch and Stevenson 2008).[9]

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 shocks often generate dissatisfaction ("grievances") against the incumbent, while at the same time reducing the opportunity cost of conflict, both of which can translate into political action that threatens the government of the day. In weak states, such negative shocks have indeed 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).[10]

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. 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 are aware, without a systematic quantitative assessment of the impact on incumbent behavior. 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 the central government for the purposes of evaluating local leaders. Our findings are consistent with this view; they further suggest that the higher-level decision-makers are fairly sophisticated, in that they appear to draw a distinction between increases in labor unrest that can be explained by adverse shocks beyond the local leader's control and increases which reflect on his/her ability (or lack thereof) to bolster stability.[11]

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

[9] 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.

[10] 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. 2021). See also Sarsons (2015), who studies the link between negative rainfall shocks and conflict in a democratic setting, albeit one with significant religious cleavages (India).

[11] This contrasts with the evidence surveyed in Healy and Malhotra (2013) on how voters in democracies can

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 of a negative shock to export opportunities.[12] We also contribute along these lines to work that has explored how exposure to trade can affect political outcomes, including legislative voting (Margalit 2011, Feingenbaum and Hall 2015), electoral voting (Jensen et al. 2017, Che et al. 2018), political polarization (Autor et al. 2020), support for extremist parties (Dippel et al. 2021), and support for cross-border integration (Colantone and Stanig 2018).

Last but not least, our study is related to the quantitative political economy literature on China's political system, specifically the management of its cadres.[13] The existing work has identified several key determinants of promotion within this system, including local economic performance (Li and Zhou 2005), political connections (Jia et al. 2015), social ties (Fisman et al. 2020), and factions (Francois et al. 2016, Shih and Lee 2018). We complement these studies by showing that an official's relative performance in maintaining social stability can be a crucial determinant for their career prospects. Persson and Zhuravskaya (2016) and Chen and Kung (2016, 2018) have moreover shown that the career concerns of Chinese politicians has swayed public spending towards uses that deliver a short-term boost to economic growth (such as construction projects). We find related evidence that economic shocks can induce a shift in fiscal resources towards uses aimed at bolstering social stability.

3 Data and Empirical Strategy

3.1 Data Sources and Measures

We turn now to our empirical setting. We describe in this section the data we use to first establish a relationship between the slowdown in exports and labor strikes, while postponing a description of the measures of incumbent turnover and other political responses to Section 6. Further details about the data construction are documented in Appendix A.

The unit of analysis throughout this paper is the prefecture, which is the division below the level of the province within China's administrative hierarchy. We include all prefectures across

mis-attribute negative economic shocks to poor performance, and thus end up voting out the incumbent for what amounts to bad luck. For specific examples, see Achen and Bartels (2004), Leigh (2009), and Cole et al. (2012), as well as the discussion between Achen and Bartels (2018) and Fowler and Hall (2018).

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

[13] There is an extensive literature on cadre management in the scholarship on Chinese politics, some examples of which are Gao (2009), Hu (2016) or Doyon and Keller (2020).

China, except Tibet due to data limitations.[14] 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.

Exports: We focus on the performance of manufacturing exports as our key local economic shock variable. For this, we draw on China's General Administration of Customs, which covers the universe of China's exporters and importers. For each trading firm, the customs data provides its location and a breakdown of its trade flows at the Harmonized System (HS) 6-digit product level. Let X_{fikt} denote the value of exports by firm f in year t, where i indexes the prefecture in which this firm is located and k indexes HS 6-digit products; we use in this study all products k – close to 4,500 HS6 codes – that map to the manufacturing sector.[15] Our main explanatory variable, the prefecture-level export shock, is then defined as:

$$ExpShock_{it} = \sum_{k} \sum_{f \in i} \frac{\Delta X_{fikt}}{L_{i,2010}}, \tag{1}$$

where $\Delta X_{fikt} = X_{fikt} - X_{fik,t-1}$. Note that $L_{i,2010}$ is 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). By construction, the $ExpShock_{it}$ variable measures the annual change in manufacturing exports in 1000 USD per worker. Our regression analysis will be based on a panel that covers the period $t \in \{2013, 2014, 2015\}$. We avoid the years prior to 2012, since this overlaps with the trade collapse during the global financial crisis. Our findings continue to hold if we were to extend the sample to include the export shock between 2015-2016 (available on request), but we do not use any years after 2016 as forces related to the US-China "tariff war" would come into play.

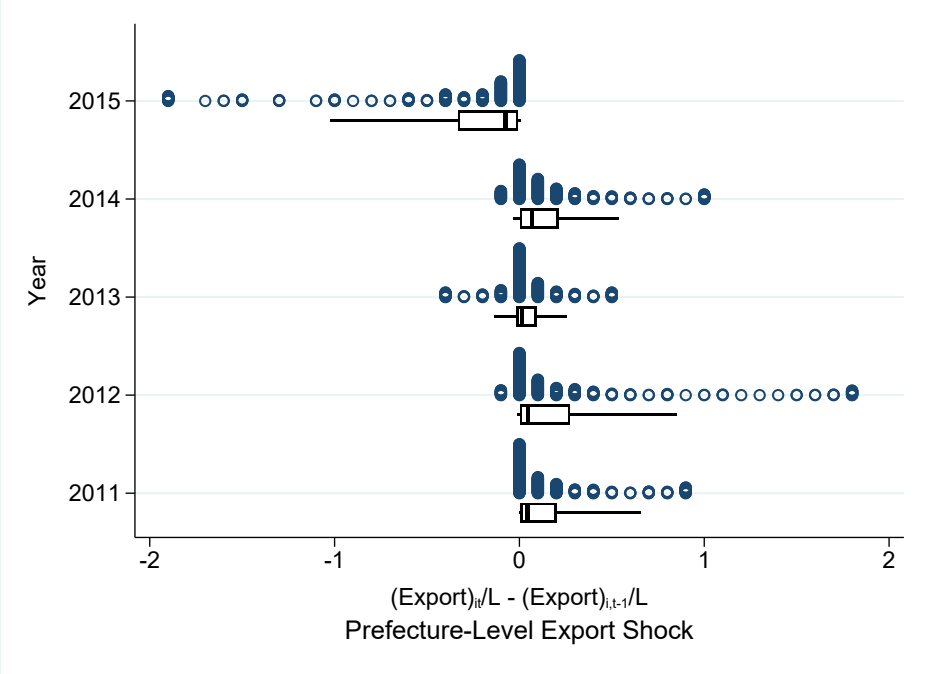
Figure 2 shows the distribution of $ExpShock_{it}$ across prefectures for each year in our sample.[16] There is considerable spatial and temporal variation in the per worker export shock. The export slowdown was especially marked in 2015, with a mean decline across prefectures of 372 USD per worker from the year before. More importantly, there was substantial variation in the severity of the export shock; in 2015, for example, the standard deviation of $ExpShock_{it}$ across prefectures was 948 USD per worker (see the summary statistics in Panel A, Table B.1, in the appendix). We will later verify the robustness of our results under alternative constructions of the export shock variable, such as when excluding firms that are pure intermediaries,

[14] While there have been some changes to prefecture boundaries over time, we have constructed all our variables to be in accord with the 2010 administrative divisions. With regard to Tibet, the China City Statistical Yearbooks contain only limited information on its prefecture-level characteristics; we were also not able to independently obtain fiscal data on its prefectures.

[15] We identify the relevant HS6 products on the basis of whether they map to SIC industry codes with leading digit equal to 2 or 3 (i.e., that are in the manufacturing sector). The mapping to SIC is from the World Integrated Trade Solutions (WITS) at: https://wits.worldbank.org/product_concordance.html.

[16] 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 later that our results are not driven by potential outliers.

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



or when examining exports by firm ownership types (e.g., privately-owned versus state-owned enterprises), these being distinctions that are relevant in the context of China's export activities.

Labor Strikes: Given the authoritarian nature of the political regime, it may come as a surprise that labor strikes even occur in China. In reality, collective actions and strikes – where workers purposefully hold back their labor to pursue specific workplace demands – are a fixture of China's industrial relations landscape. Although there are prohibitions against independent labor unions, strike actions themselves are not deemed illegal under the letter of the law. Strikes are thus often seen as an avenue for recourse over employment grievances, either to get firm managers to accommodate worker demands or to draw attention from the local authorities (China Labour Bulletin 2020).

We use data on strikes drawn from the China Labour Bulletin (CLB), a non-profit organization based in Hong Kong that has monitored and reported on labor relations in mainland China since 1994. The CLB's records of incidents of collective worker action are publicly hosted on its CLB Strike Map, and the available data series commences in 2011. In the absence of official statistics on strikes, this data has been used regularly by news media outside of China to examine trends in worker actions within China, and has also been applied in academic studies (see for example Qin et al. 2019).[17] For the period of interest (up until end-2016), the CLB

[17]The CLB Strike Map is at: https://maps.clb.org.hk/strikes/en. Media outlets that have cited this data include 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;

Strike Map was based on information compiled on a daily basis from online and media sources, including but not limited to Sina Weibo, WeChat, Tianya, Baidu, and Google.[18]

For each labor event, the CLB records the date, location (prefecture), and a short description of the incident. For the vast majority of observations (>98%), the CLB further records: (i) the broad sector in which the worker action occurred (e.g., manufacturing, construction, services); and (ii) the underlying cause (e.g., wage arrears, layoffs, work conditions). A total of 5,156 labor events were recorded over 2012-2015, with most of these 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. This dimension of the data is particularly useful, in that it confirms that the incidents recorded were triggered by specific employment grievances (rather than being acts of political protest or dissent). For about one-third of the observations, the CLB provides a brief account of how the worker action concluded. These point to substantial unevenness across China in the manner in which local authorities managed labor strikes in practice: the responses seen span the spectrum, from repression of the worker action (e.g., police arrests, use of pepper spray), to attempts at accommodation (e.g., mediation, negotiation, or even compensation).[19]

Figure 3 illustrates the distribution of CLB-recorded labor events across China during 2012-2015. As is clear from this map, the labor incidents are spread out geographically, though (not surprisingly) the density of events is higher in coastal manufacturing hubs such as the Yangtze River Delta and the Pearl River Delta. The summary statistics in Table B.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 age 15-64 population in the 2010 Census). While the average prefecture experienced an increase in 1.24 strikes per million workers between 2014-2015, the cross-prefecture standard deviation in this change was also large (1.77).

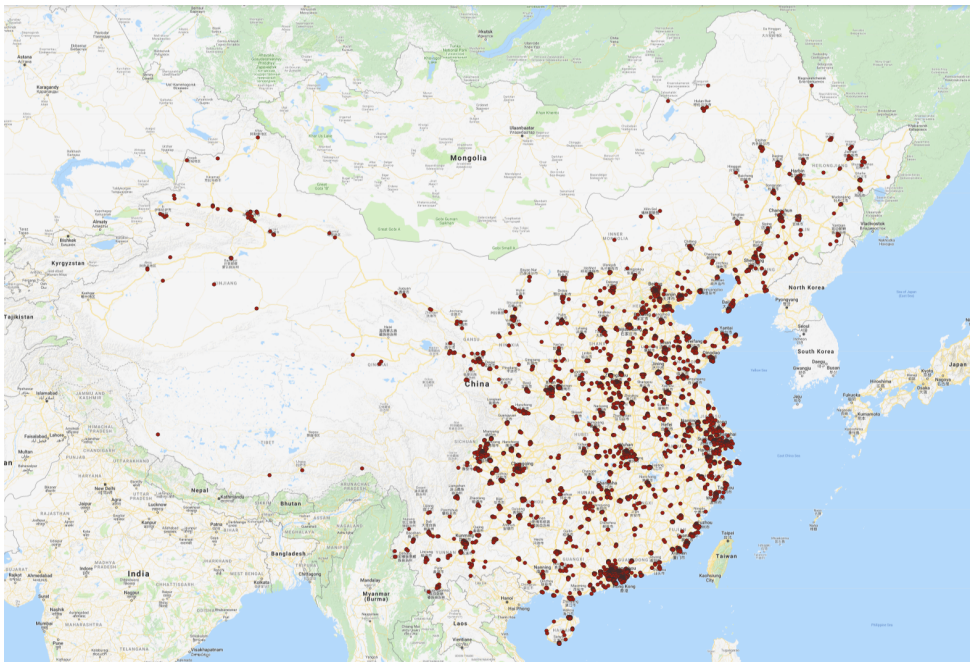
Given the manner in which the data are collected, the CLB are careful to acknowledge that they do not have a complete record of all labor incidents. For our empirical analysis though, what will be more crucial is whether the CLB data are adequately picking up trends over time and across locations in 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 submitted 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 our regression analysis), we nevertheless view it as a useful measure of

and *The New York Times* (14 March 2016): https://www.nytimes.com/2016/03/15/world/asia/china-labor-strike-protest.html.

[18]Based on a private email communication with the CLB. The CLB makes an effort to verify the veracity of each strike event that is identified, although details of this process are not publicly disclosed.

[19]Of the 5,156 observations from 2012-2015, police were involved and arrests made in 1,415 cases, while mediation and negotiation (without any arrests) was the outcome in 396 cases.

Figure 3: CLB Map (2012-2015)



the frequency of labor disputes against which to cross-check the CLB data.

Panel A of Figure A.1 plots the number of CLB labor events and MOHRSS labor disputes aggregated at the national level, expressed in terms of events per million workers (see Appendix A.1). Note that the total number of CLB events (right vertical axis) is smaller than the total number of MOHRSS labor disputes (left vertical axis). This could be due to strikes being a more extreme and hence less frequent form of worker action; alternatively, this could simply reflect that the CLB do not capture all significant labor events that have taken place. Notwithstanding the difference in scale, the CLB strike data clearly follow a similar upward trend as the MOHRSS records on labor dispute cases; this is true both in levels (Panel A) and in annual changes (Panel B). Panel C further compares the two data sources at the province level, specifically the annual changes in event/case counts 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 are positively correlated with the corresponding changes in the number of officially-lodged labor dispute cases per million workers.

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 form of nonclassical measurement error that could generate 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 reported MOHRSS numbers might actually understate the extent of employer-worker conflict for such locations, if local officials 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.[20] 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 reporting bias are unlikely to be driving the observed variation in the CLB strike data.

Other Local Data: We put together a further set of socioeconomic variables at the prefecture level for use either as controls or as additional local outcomes to be explored; summary statistics for a selection of these variables are included in Panel A of Table B.1. Data on the working-age population and the population breakdown by internal migration status (hukou vs non-hukou) are drawn from the 2010 Census. We rely in turn on the China City Statistical Yearbooks to compute such variables as: the average wage level, gross industrial output per capita, manufacturing employment share of the population, college educated share of the population, as well as mobile and internet penetration rates. These Yearbooks report only on urban prefectures, which reduces our coverage to 290 prefectures when these latter variables are used. We supplement the above with a commonly-used proxy for economic activity based on night-lights intensity, constructed from the Visible Infrared Imaging Radiometer Suite Day/Night Band dataset (VIIRS-DNB, see Appendix A.2 for more details).

3.2 Empirical Strategy: Export Slowdown and Labor Unrest

We now describe the regression model and identification strategy that we adopt to uncover the effect of the export slowdown on incidents of labor strikes. This is a key relationship that will lead into our discussion of broader political economy outcomes in China.

Estimating Equation: Our baseline regression specification is as follows:

$$\Delta(Events/L)_{it} = \beta_1 ExpShock_{it} + \beta_X X_{it} + D_{pt} + D_i + \varepsilon_{it}, \tag{2}$$

where i denotes prefecture and t denotes year. The dependent variable $\Delta(Events/L)_{it}$ 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 as defined in (1); both of these are constructed as changes between years $t-1$ and t.[21] The X_{it} term captures time-varying prefecture characteristics that are potential additional determinants of labor strikes.

[20] 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.

[21] We express the labor strikes in per million worker terms, in order to normalize the measure to a reasonable scale. This is not critical for our findings: We continue to obtain a negative and significant β_1 coefficient if we were to replace $\Delta(Events/L)_{it}$ in (2) by $\Delta(Event)_{it}$. For example, the estimated β_1 for the IV specification in Column 3 of Table 1 would instead be -1.646 with a standard error of 0.657 (significant at the 5% level).

The regression in (2) 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} further capture any province-specific differences in changes in strike intensity across different time periods; the D_i's in turn account for cross-prefecture differences in $\Delta(Events/L)_{it}$, or equivalently prefecture-specific linear time trends in $(Events/L)_{it}$. Equation (2) is thus a relatively stringent specification, with the coefficient β_1 estimated off variation in export shifts across prefectures within provinces, as well as within prefectures over time. (We will discuss some results later in Section 4.2 based on less demanding specifications that leverage off the cross- rather than within-prefecture variation in the data.) In practice, we run (2) weighting each observation by the prefecture's working-age population in 2010, and report standard errors that are clustered by province. (In robustness checks, we will show that the key findings are not sensitive to these particular choices.)

Two further remarks are in order. First, our empirical setting works off a relatively short panel. This reflects a conscious decision to focus the study on a specific window in China's export experience, during which (as we will argue below) the main driving force behind the slowdown in exports was a decline in external demand from the rest of the world. This will be convenient from the perspective of providing a credible identification strategy. We have opted not to expand the analysis into the post-2016 years, as other forces related to the US tariffs imposed on China, as well as China's policy responses (including its retaliatory tariffs, adjustments to its MFN tariff rates, and the increase in fiscal stimulus), would need to be taken into account; these are arguably best left for future work to explore.

Second, though this is not a main focus of our exercise, we show in Appendix C.1 that the regression specification in (2) can be rationalized from a simple model that builds on Campante and Chor (2012, 2014). In that setting, workers face a stylized decision over how much time to allocate between production activities (that earn wage income) and strikes (to reduce potential expropriation of that income by factory managers). A negative economic shock such as an export slowdown that raises managers' attempts to withhold the payment of wages would then naturally raise workers' propensity to strike to retain their labor income.

Instrumental Variable: An immediate concern 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 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 product-k trade flows from the ROW to the ROW; we draw on UN Comtrade for this data, using once again only HS6 codes that map to the manufacturing sector. Each product-k shift is 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 in units of 1000 USD per worker, by dividing by the prefecture working-age population in the 2000 Census, $L_{i,2000}$; we draw on an earlier census to avoid using the same population data that already appears in the denominator of $ExpShock_{it}$.

The validity of (3) as an IV rests on the assumption that, conditional on X_{it} as well as 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 the product-specific export shocks observed at the national level. Toward this end, we will show that these product export shocks ΔX_{kt}^{ROW} pass a balance test, so that they can be seen to be as good as randomly assigned to Chinese prefectures; we discuss this and related checks recommended by Borusyak et al. (2020) in Section 4.2. More broadly, we view these ROW trade shifts as primarily picking up adverse external demand shocks experienced by China during the global trade slowdown. This position is supported by studies such as the IMF World Economic Outlook, which found using a range of methodologies that about 60-80% of the slowdown in trade flows during this period was attributable to the weak recovery in world demand after the global financial crisis (Aslam et al. 2016); the extent of the trade slowdown accounted for by supply-side forces and increases in trade frictions was smaller in comparison.[22] In our present empirical context, what this means is that the IV plausibly leverages on sources of variation in product-level trade flows that are driven by foreign demand conditions, and then projects these onto each prefecture on the basis of pre-determined weights.

While (3) serves as our baseline IV, we will explore alternative constructions to further isolate variation in the ROW trade flows that can be attributed to product-level demand shifts in foreign markets via a gravity-equation approach. We are also cognizant that the ΔX_{kt}^{ROW} terms

[22]The 2016 IMF World Economic Outlook implemented both a regression-based methodology and a model-based structural decomposition. The former approach – based on estimating an import demand system – delivered a 80% headline number for the contribution of aggregate demand forces to the global trade slowdown. On the other hand, the latter methodology – based on the multi-country model of production and trade of Eaton et al. (2016) – yielded a 60% figure for the contribution of aggregate demand forces to the decline in trade as a share of world GDP.

might be incidentally correlated with domestic demand or domestic supply shocks stemming from within China, and so will report robustness results in which we make an effort to control for these latter forces. A related concern for identification is that the initial export structure – the export share weights in (3) – might be directly driving 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.[23]

The construction of the IV in (3) contrasts with Autor et al. (2013) in several ways. At a basic level, 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 IV, instead of analogous employment share weights as in Autor et al. (2013). This reflects a practical consideration: Information on the employment associated with production at the level of detail of HS6 codes is not readily available. More substantively, as shown in Appendix A.3, the export shock defined in (3) that uses export-share weights can be directly rationalized by log-linearizing the relationship between exports and external demand shifts. If one were to instead apportion the export shocks ΔX_{kt}^{ROW} on the basis of employment shares in our setting, this could systematically over-state the importance of export exposure in prefectures where a greater share of output tends to be absorbed domestically, such as China's inland provinces. Last but not least, our identification strategy is based on a narrower window in China's export experience, during which we can plausibly associate product-level trade shocks to the slow and uneven recovery in external demand following the global financial crisis. While this allows us to establish a sharp set of findings on the short-run responses to the export slowdown of the mid-2010s, we should acknowledge that there are limits to how far we can extend the analysis to speak to the longer-term implications for China, particularly as forces related to the US-China "tariff war" enter the picture shortly after the end of our sample period.

4 Effects of Export Shocks on Labor Strikes

We present below our core findings on the relationship between the export slowdown and labor strikes at the prefecture level (Section 4.1). We include a discussion of robustness checks and validation exercises for the Bartik IV strategy (Section 4.2), as well as corroborating evidence from available data on other local economic and labor market outcomes (Section 4.3).

4.1 Baseline Results

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

[23]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.

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).[24] 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 $ExpShock_{it}$ 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 socioeconomic shifts that could concurrently affect labor unrest. We control for the change in the log college-enrolled share of the general population, motivated by work that has shown that individuals with higher levels of education have a greater propensity to engage in civic and even protest actions (e.g., Campante and Chor 2012, 2014). 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 information and communication technology (ICT) and its potential role in facilitating the mobilization of workers (Campante et al. 2018, Manacorda and Tesei 2020).[25] These two variables each exhibit a positive correlation with the occurrence of labor strikes, with the role of broader access to the internet even being statistically significant. That said, the estimated export shock coefficient, β_1, remains stable when these further controls are used. (We should stress that our conclusions on the effects of the export slowdown on labor strikes – and on the political outcomes we later explore – do not hinge on whether we include these auxiliary prefecture controls; see Table B.2 in the appendix.) 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, 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 in the Column 3 regression sample). The $\beta_1 = -0.1728$ point estimate from Column 3 translates this into 0.15 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.

[24] Although we do not report the estimates to save space, the shift-share IV is indeed positively correlated with the export shock. The table reports the accompanying Kleibergen-Paap F-statistics; these are all in excess of Stock-Yogo the 10 percent threshold for weak instruments, confirming the relevance of the instrument for explaining the variation in $ExpShock_{it}$.

[25] These auxiliary prefecture controls are each constructed as changes between year $t-1$ and t (i.e., contemporaneous with the dependent variable). Note that the mobile and internet usage measures could also help to capture differences across prefectures in the likelihood that a given labor strike might be recorded by the CLB, to the extent that such differences in reporting intensity are related to variation in the prevalence of ICT.

Table 1: Export Shocks and Labor Strikes

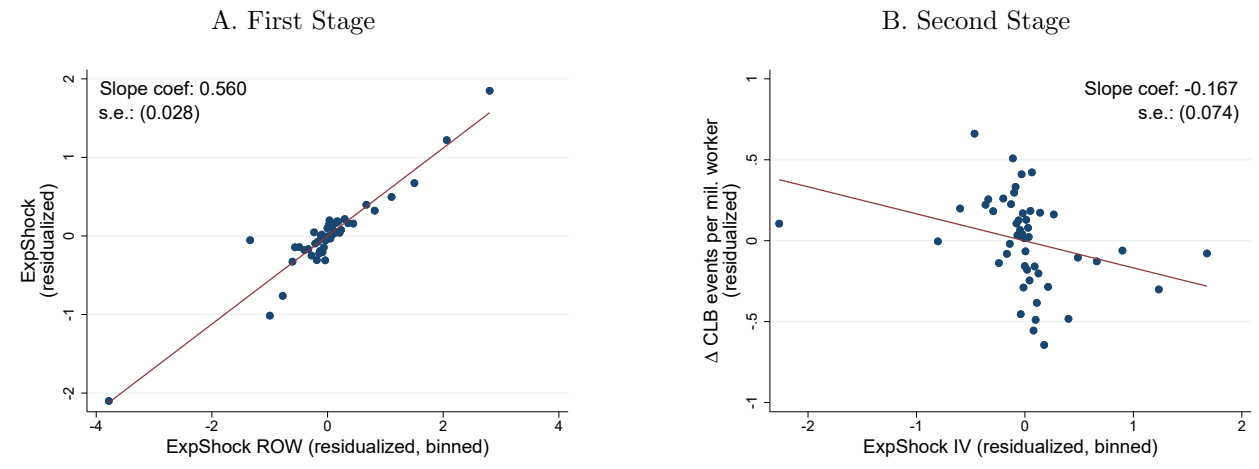
Dependent variable:	Δ **CLB Events per million**$_{it}$			
	(1) OLS	(2) IV	(3) IV	(4) OLS-RF
ExpShock$_{it}$	-0.1599***	-0.1822**	-0.1728**	
	(0.0346)	(0.0739)	(0.0746)	
ExpShockROW$_{it}$				-0.1035**
				(0.0477)
ΔLog College-enrolled share$_{it}$			-0.0679	-0.0930
			(0.2199)	(0.2249)
ΔLog Mobile share$_{it}$			0.8907	0.4951
			(0.8239)	(0.9744)
ΔLog Internet share$_{it}$			0.5258***	0.6325**
			(0.1840)	(0.2310)
Province-year dummies?	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y
Additional time-t controls?	N	N	Y	Y
First-stage F-stat	–	64.32	105.56	–
Observations	987	987	822	822
R^2	0.5023	0.5020	0.5264	0.5192

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.

To help with visualizing these relationships, Figure 4 presents binned-scatter plots of both the first- and second-stages from the IV specification in Column 3. (The axes variables have been residualized to remove variation that can be attributed to the fixed effects and auxiliary prefecture controls, as detailed in Appendix B.1.) Panel A confirms that there is a tight relationship between the (residualized) rest-of-the-world export shock IV and the prefecture $ExpShock_{it}$ variable, consistent with the high F-statistic that points to the explanatory power in this first-stage. Panel B in turn provides reassurance that the effect of the export slowdown on increases in labor strikes per worker is not driven by outliers; if the tail bins of $ExpShock_{it}$ were to be removed, this would in fact accentuate the negative slope we see in this panel.

We dive into the recorded nature and causes of the labor strikes in Table 2. We consider in particular whether "wage arrears" or "layoffs" was a cited cause for a strike (which was the case for about 60% of the CLB events), while also leveraging the information on the broad sector in which the strike occurred. The IV regressions in Table 2 follow the specification in Column

Figure 4: Binned Scatter Plots: Prefecture Export Shocks and Labor Strikes
(50 bins; based on Column 3, Table 1)



3 of Table 1, but use instead as the dependent variable the annual change in labor events under the respective cause and sector of activity (normalized by the 2010 prefecture working-age population). The results confirm that negative export shocks prompted an increase in labor strikes over wage arrears or layoffs (Column 1). This pattern holds within both the manufacturing (Column 3) and non-manufacturing (Column 5) sectors, though notably, the estimated effect is much larger in the former.[26] This is consistent with the broader narrative that the negative shock to manufacturing exports led to a rise in worker distress over unpaid wages or layoffs in that sector. In contrast, the estimates point to a positive correlation between the export shock and occurrences of strikes that are unrelated to wage arrears or layoffs (Column 2). This residual category includes a mix of strike causes, though about a third of these cases can be classified as being about demands for pay increases, overtime compensation, improvements in working conditions, or fairness in the workplace; labor activism due to these causes might plausibly rise when export growth is strong and driving up local labor demand.

In Table 3, we explore additional dimensions of the prefecture export shock variable. Column 1 considers the possibility of spatial spillovers in the export shock across prefectures. To address this, we construct the working-age population-weighted average of $ExpShock$ across all prefectures that share an administrative border with i; we instrument for this with an analogous neighboring-prefecture weighted-average measure of $ExpShockROW$. The results here indicate that the local export shock remains important for explaining the rise in labor strikes in prefecture i itself, even when we account for spillovers from neighboring locations in this way.[27]

[26] We include under "non-manufacturing" all sectors outside of manufacturing that are covered by the CLB Strike Map, namely: Mining; Transport and Logistics; Construction; Services; Education; Other.

[27] We have separately found evidence that the weighted-average neighboring prefecture export shock measure in time-$(t-1)$ has a negative association with the time-t increase in labor strikes per worker, consistent with spatial spillovers from the export slowdown being present with a lag. Note that including this one-period lagged

Table 2: Export Shocks and Labor Strikes: By Causes

Dependent variable:	Δ **CLB Events per million**$_{it}$					
Sector:	All	All	Mfg.	Mfg.	Non-Mfg.	Non-Mfg.
Cause: Wage Arrears, Layoffs?	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
	IV	IV	IV	IV	IV	IV
ExpShock$_{it}$	-1.3707**	0.1035**	-1.2700**	-0.0600	-0.1007***	0.1634***
	(0.4949)	(0.0410)	(0.4903)	(0.0423)	(0.0255)	(0.0546)
Province-year dummies?	Y	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	Y	Y
First-stage F-stat	105.56	105.56	105.56	105.56	105.56	105.56
Observations	822	822	822	822	822	822
R^2	0.5374	0.2850	0.5330	0.4009	0.1884	0.6009

Notes: The dependent variable is the change in CLB-recorded events per million workers in prefecture i between year $t-1$ and t; the odd-numbered columns count events where the recorded cause was wage arrears and/or layoffs, while the even-numbered columns count events related to other causes. Columns 1-2 include events across all sectors, Columns 3-4 include only events that occurred in the manufacturing sector, while Columns 5-6 include only events in non-manufacturing sectors. 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 1, 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.

In Column 2, we examine if the time-$(t+1)$ export shock might have explanatory power for strikes at time t, by replacing the contemporaneous export shock variable with $ExpShock_{i,t+1}$, and instrumenting for it with the time-$(t+1)$ Bartik variable. The export shock coefficient we obtain is not statistically significant, allaying the concern that our results could be driven by pre-determined trends in prefecture exports that might be co-moving with strike intensity.

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 during this period of slowing exports. We split $ExpShock_{it}$ into a component that reflects firm exit from exporting – defined here as firms that record positive exports in year $t-1$, but do not export in year t – and a remaining component that captures continuing or new exporters; we use the former as a proxy for the exit margin, in the absence of direct data on firm or plant closures. We present OLS estimates, *in lieu* of proposing two IVs for the separate components of $ExpShock_{it}$, so the patterns uncovered here should be viewed as descriptive rather than causal in nature. With this caveat in mind, we find a slightly larger effect associated with the exit margin, though both components of $ExpShock_{it}$ are relevant for explaining the overall negative correlation with the rise in strikes. Column 4 presents an alternative breakdown of $ExpShock_{it}$ into that accounted for by state-owned enterprises (SOEs) versus non-SOEs (i.e., private domestic and foreign-owned firms). We find

neighboring prefecture shock does not change the magnitude and significance of the own-prefecture export shock that is our main explanatory variable of interest (available on request).

Table 3: Export Shocks and Labor Strikes: Heterogeneous Effects

Dependent variable:	Δ **CLB Events per million**$_{it}$				
	(1) IV	(2) IV	(3) OLS	(4) OLS	(5) IV
ExpShock$_{it}$	-0.1771*				-0.6410**
	(0.0922)				(0.2808)
Neighboring ExpShock$_{it}$	0.0154				
	(0.1665)				
ExpShock$_{i,t+1}$		0.0613			
		(0.1065)			
ExpShock$_{it}^{Exit}$			-0.2152**		
			(0.0897)		
ExpShock$_{it}^{NonExit}$			-0.1515***		
			(0.0467)		
ExpShock$_{it}^{NonSOE}$				-0.2154***	
				(0.0726)	
ExpShock$_{it}^{SOE}$				0.8726	
				(1.1687)	
ln(Fiscal Pub. Security/L)$_{i,12}\times$ExpShock$_{it}$					0.7743***
					(0.2759)
Share of State Emp$_{i,10}\times$ExpShock$_{it}$					0.2059**
					(0.0831)
Share of Non-Hukou$_{i,10}\times$ExpShock$_{it}$					-0.0582
					(0.1887)
Share of College$_{i,10}\times$ExpShock$_{it}$					-14.9526***
					(3.2214)
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	Y
First-stage F-stat	31.85	23.94	–	–	6.81
Observations	822	822	822	820	813
R^2	0.5266	0.5176	0.5266	0.5248	0.5265

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 $t+1$ lead 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 variables (Fiscal Pub. Security/L)$_{i,12}$, Share of State Emp$_{i,10}$, Share of Non-Hukou$_{i,10}$ and Share of College$_{i,10}$ are demeaned; the main effects of these initial prefecture characteristics are absorbed by the prefecture dummies. The additional time-t controls are those used in Column 3 of Table 1, 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.

that the effect on labor strikes can be attributed to the non-SOE margin, suggesting that SOEs may have played a role during the export slowdown as a buffer for local employment.

Finally, we run an interaction specification in Column 5 to investigate several dimensions

along which the effect of the export shock on labor strikes might be heterogeneous. We find quite intuitively that the negative relationship between export performance and strikes was more muted where the local government: (i) exhibited a greater fiscal capacity to manage unrest (as proxied by the 2012 expenditure per worker on public security uses); and (ii) accounted for a larger share of employment (as measured by the share employed in government and party agencies, from the 2010 Census). Conversely, the effect of $ExpShock_{it}$ was exacerbated the greater the local population share with at least some college education (from the 2010 Census). This aligns with work showing that a weak economy is more liable to trigger a rise in protest activity when the local populace features higher levels of educational attainment (Campante and Chor 2014). Lastly, we obtain a negative interaction effect between $ExpShock_{it}$ and the initial share of non-hukou migrant workers (from the 2010 Census). Although this effect is not precisely estimated, the negative sign is consistent with the interpretation that, with restricted access to social benefits in the prefecture where they work, migrant workers without hukou were less protected from export shocks and hence more prone to strike when economic conditions worsened. We naturally caution against a causal reading of these results in Column 5, given that we do not propose an instrument for each initial prefecture characteristic. That said, we find it reassuring that the patterns are consistent with these forces that are relevant to the political and economic context of China.[28]

4.2 Validating the IV Strategy and Other Empirical Checks

In this subsection, we describe an extensive series of checks we have implemented, including several best-practice recommendations for validating the Bartik IV strategy. We keep the exposition relatively brisk here; a more detailed documentation of each test is provided in Appendix B. While the discussion below is written around the effect of the export slowdown on labor strikes, we should stress that the checks in each appendix table are performed too for the other key political outcome variables – i.e., the incumbent turnover, textual analysis, and fiscal spending measures – that will be the focus of the paper in Sections 5 and 6.

Specification Checks (Appendix B.1): At a basic level, we have verified that the effect of $ExpShock_{it}$ on labor strikes remains statistically significant and stable in magnitude when we: (i) drop the auxiliary prefecture controls, i.e., the change in college-enrolled, mobile-use, and internet-use shares (Panel A, Table B.2); or (ii) run unweighted regressions (Panel B).

We have further explored several alternative specifications that are commonly adopted for

[28]To gauge the magnitude of the heterogeneous effects, a standard deviation decrease ($\approx$ \$841) in exports per worker is associated with an increase in strike intensity by ($0.841 \times 0.6410 \approx$) 0.54 per million workers for a prefecture whose initial characteristics take on their in-sample mean values. This effect is reduced by 0.26 (respectively, 0.19) for a prefecture with one standard deviation higher ln(Fiscal Pub. Security/L)$_{i,10}$ (respectively, Share of State Emp$_{i,10}$), while it is increased by 0.32 (respectively, 0.01) for a prefecture with one standard deviation higher Share of College$_{i,10}$ (respectively, Share of Non-Hukou$_{i,10}$). (Note that each of these prefecture characteristics has been demeaned when included in Column 5 of Table 3).

panel data with a short time dimension (c.f., Angrist and Pischke 2009, Chapter 5.3). Toward this end, we drop the prefecture fixed effects from (2), and: (i) include instead the lagged dependent variable, i.e., $\Delta(Events/L)_{i,t-1}$, to control for prefecture-specific forces present in these pre-trends (Panel C); or (ii) control instead for the lagged level of strikes per worker, i.e., $(Events/L)_{i,t-1}$, to account for possible mean reversion in strike intensity (Panel D). We have also run a regression using just the subset of observations with $t = 2015$, the most severe year of the export slowdown, while controlling for province fixed effects (Panel E). Our findings across these three specifications indicate that the key relationship – between a slowdown in exports and a rise in labor strikes – is present too in the cross-sectional variation. While this complements our baseline results, we have opted to focus on the specification with D_i fixed effects since this controls more thoroughly for time-invariant prefecture characteristics. Moreover, the within-prefecture variation yields an $ExpShock_{it}$ coefficient (see Column 3 of Table 1) that is smaller in magnitude compared to these alternative specifications, so we are being more conservative in terms of the magnitude of the implied effect on labor strikes.[29]

Validating the Bartik Strategy (Appendix B.2): We carefully address a set of issues that may affect confidence in the Bartik identification approach. Following the formulation in Borusyak et al. (2020), the validity of a shift-share IV can be seen as stemming from the assumption that shocks – in our case, at the product level – are as good as randomly assigned. Dropping the time subscripts for clarity, the Bartik IV in (3) can be re-expressed as: $\sum_k s_{ik} g_k$, where $g_k = \Delta X_k^{ROW} / \sum_i X_{ik,2010}$ captures the ROW export shock experienced by product k, and $s_{ik} = X_{ik,2010}/L_{i,2000}$ measures the exposure of prefecture i to each product-level shock. Borusyak et al. (2020) show that the case for causal identification then rests on an orthogonality condition on the product-level shocks g_k. This can be formally written down as: $\sum_k s_k g_k \phi_k \xrightarrow{\text{p}} 0$, where: (i) $s_k = \sum_i e_i s_{ik}$ is a cross-prefecture weighted-average measure of exposure to product k, with e_i being the regression weights (in our setting, the prefecture working-age population); and (ii) $\phi_k = \sum_i (e_i s_{ik} \varepsilon_i) / \sum_i (e_i s_{ik})$ is an exposure-weighted expectation of untreated prefecture-level outcomes encapsulated in the residual term ε_i from the regression in (2). In 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 g_k's would be as good as randomly assigned. The associated effective sample size is then inversely related to the Herfindahl index, $E[\sum_k s_k^2]$, and the IV estimates are consistent when $E[\sum_k s_k^2] \to 0$.

The above identification assumption would be violated in practice if more severe export

[29]This is in line with the discussion in Angrist and Pischke (2009, Chapter 5.4), whose argument suggests that the fixed effects specification (our baseline) and the alternative that controls instead for the lagged dependent variable (Panel C, Table B.2) would yield coefficient estimates that bracket the true magnitude of the effect of $ExpShock_{it}$, under some reasonable assumptions. Note that we do not include both prefecture fixed effects and $\Delta(Events/L)_{i,t-1}$ (or $(Events/L)_{i,t-1}$) simultaneously on the right-hand side in Panel C (respectively, Panel D) of Table B.2, as this exposes the specification to Nickell bias.

shocks tend to occur in products concentrated in prefectures with certain baseline characteristics, that themselves have independent effects on local labor strikes. We therefore follow Borusyak et al. (2020) and test whether the product-level export shocks, g_k, are balanced across an exposure-weighted average of initial prefecture characteristics, $s_k\phi_k$. We consider two types of variables which the ε_i residuals could be picking up: (i) baseline prefecture characteristics in 2010, namely: the college-educated share, manufacturing employment share, export-to-GDP ratio, non-hukou share of population, log GDP per capita, party secretary age, and log fiscal revenue per capita; and (ii) pre-period trends in the main prefecture outcomes of interest, including $\Delta(Events/L)$. Panel A in Table 4 reports the results of this balance test.[30] It is reassuring that none of these correlations is statistically significant at conventional levels. Moreover, the p-value for a joint test of significance across all prefecture variables is 0.9996. In our data, the Herfindahl index based on the initial export structure in 2010 is 0.0098, which implies an effective sample size of 306.1(=3/0.0098) in our panel.[31]

We have further assessed the robustness of our findings in more stringent specifications that include the initial export exposure of the prefecture (manufacturing exports per worker) interacted with year fixed effects (Table B.3). The results help to alleviate concerns that our findings may be confounded with unobserved time-varying forces linked to the initial export exposure of the manufacturing sector within a prefecture. In addition, we pick up on the test in Column 2 of Table 3, 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 in Section 6 (see Table B.4). This indicates that prefecture-specific pre-trends are unlikely to be at the root of our results.

Borusyak et al. (2020) also discuss how the estimating equation can be recast as a product-level regression that delivers an export shock effect of identical magnitude. To do so, we run a regression of $\Delta(Events/L)_k^{\perp}$ on $ExpShock_k^{\perp}$, with the latter instrumented by $\Delta X_k^{ROW} / \sum_i X_{ik,2010}$, where the '$\perp$' superscript refers to the suitably-transformed product-level analogue of the variable in question.[32] We report the results from these product-level regressions in Panel B of Table 4; by construction, the export shock coefficient is equal to that in the earlier prefecture-level regression (Column 3, Table 1). More importantly, the implied effect is highly significant even after adjusting the standard errors for potential common shocks across products within a broad HS 2-digit heading; the smaller standard errors we obtain here suggest that the statistical

[30]Specifically, we regress the exposure-weighted initial prefecture characteristic against g_{kt} and year fixed effects, using the average exposure variable s_k as regression weights, while clustering the standard errors at the HS 4-digit level; the reported coefficient in Panel A is that obtained on g_{kt}.

[31]To calculate the Herfindahl index (HHI), we re-normalize the pre-period exposure shares s_k to sum to one. Since our panel spans three years, the effective sample size is given by 3/HHI.

[32]Specifically: $\Delta(Events/L)_k^{\perp} = \sum_i e_i s_{ik} \Delta(Events/L)_i^{\perp}$ and $ExpShock_k^{\perp} = \sum_i e_i s_{ik} ExpShock_i^{\perp}$, where $\Delta(Events/L)_i^{\perp}$ and $ExpShock_i^{\perp}$ are respectively the residualized prefecture outcome and export shock variables, after accounting for the variation due to the auxiliary controls (X_{it}) and the fixed effects (D_i and D_{pt}) on the right-hand side of (2).

Table 4: Balance Tests and Product-Level Analysis

Panel A: Balance Test of Industry Shocks	Coef.	SE
Predetermined Prefecture Characteristics:		
Share of college educated (%)	0.0068	(0.0051)
Manufacturing employment share (%)	0.0396	(0.0293)
Export to GDP ratio (%)	0.1427	(0.1090)
Share of population without Hukou (%)	0.0961	(0.0712)
Log GDP per capita	0.0011	(0.0009)
Party secretary age	0.0062	(0.0046)
Party secretary age$\leq$ 57 (indicator variable)	-0.0002	(0.0002)
Log fiscal revenue per capita	0.0019	(0.0014)
Pre-trends in Outcomes:		
Δ Event per mill., 2011-12 ($\times$100)	0.0145	(0.0354)
Δ Event per mill., 2012-13 ($\times$100)	0.0349	(0.0504)
Δ Party secretary turnover, Lateral, 2011-12 ($\times$100)	0.0027	(0.0029)
Δ Party secretary turnover, Lateral, 2012-13 ($\times$100)	0.0060	(0.0060)
Δ Log "weiwen" score, MNB, 2011-12 ($\times$100)	-0.0026	(0.0066)
Δ Log "weiwen" score, MNB, 2012-13 ($\times$100)	-0.0046	(0.0165)
Δ Log fiscal expenditure, Public Security, 2011-12 ($\times$100)	-0.0027	(0.0034)
Δ Log fiscal expenditure, Public Security, 2012-13 ($\times$100)	-0.0025	(0.0025)
Δ Log fiscal expenditure, Social Spending, 2011-12 ($\times$100)	0.0012	(0.0012)
Δ Log fiscal expenditure, Social Spending, 2012-13 ($\times$100)	-0.0015	(0.0015)
Joint significance test:	$\chi^2(18)$=4.29, p-value=0.9996	

Panel B: Product-Level Regressions

Dependent variable:	**Δ CLB Events per million$^{\perp}_{kt}$**	**Party Sec. Lateral Turnover$^{\perp}_{k,t+1}$**	**Δ Log MNB "weiwen" score$^{\perp}_{k,t+1}$**	**Δ Log Fiscal Public Security$^{\perp}_{k,t+1}$**	**Δ Log Fiscal Social Spending$^{\perp}_{k,t+1}$**
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
ExpShock$^{\perp}_{kt}$	-0.1728***	-0.0907***	-0.1988**	-0.0234***	-0.0208***
	(0.0285)	(0.0124)	(0.0776)	(0.0019)	(0.0030)
First-stage F-stat	33.36	34.55	29.97	29.97	29.30
Observations	13,197	13,197	13,197	13,197	13,197
R^2	0.1721	0.0723	0.0570	0.2292	0.4312

Notes: Panel A reports coefficients from regressing product-specific weighted averages of beginning-of-period prefecture characteristics on HS6 product-level export shocks, as recommended by Borusyak et al. (2020). Standard errors are clustered by HS 4-digit codes. The regressions are weighted by the average HS6 product-level export exposure across prefectures. A subset of these coefficients and their standard errors are multiplied by 100 for readability; none of the estimates are significant at the 10% level. Panel B reports the results of product-level IV regressions that yield export shock coefficients equivalent to the prefecture-level specifications. Columns 1 through 5 in this panel are the product-level analogues of respectively, Column 3 in Table 1, Column 2 in Table 6, Column 3 in Table 7, and Columns 1a and 1b of Table 8. Robust standard errors are clustered by HS 2-digit codes. *** p<0.01, ** p<0.05, * p<0.1.

inference drawn in our baseline prefecture-level regressions was relatively conservative.

Returning to the prefecture-level specification, we perform a set of checks to establish that the results are not being driven by initial specialization in certain industries that display predetermined trends. In our context, one might be concerned that labor unrest could be trending up in say the textile industry, and hence prefectures specializing in textile products would experience more strikes even in the absence of export shocks. Our use of prefecture fixed effects in (2) helps to partially guard against this, since these capture prefecture-specific linear time trends in $(Events/L)_{it}$. To further allay concern about unobserved shocks with a non-linear pre-trend that are associated with certain products, we show that the results are robust to dropping each HS section – and reconstructing the $ExpShock_{it}$ measure and $ExpShockROW_{it}$ IV – one at a time (Table B.5). Note that this check also helps to reassure that our results are not driven by individual HS sections which may have experienced particularly large shocks (Goldsmith-Pinkham et al. 2020).[33,34]

Alternative Statistical Inference (Appendix B.3): Adão et al. (2019) register the concern that prefectures located in different provinces could experience correlated shocks if they share a similar initial product-level export mix, so that clustering by province alone could be inadequate. The product-level regressions just presented in Panel B of Table 4 help in addressing this issue, since these allow for correct statistical inference after collapsing out the prefecture dimension in the data. We have moreover verified that if one were to continue working with the prefecture-level regression in (2), these results are robust under a variety of alternative clustering protocols; this includes a two-way clustering by province and by a separate partitioning of the prefectures based on an export similarity index (see Table B.6).[35]

Alternative Bartik IVs (Appendix B.4): We experiment with several variants of the Bartik IV in Table B.7. In Panel A, we exclude exports by intermediary firms from $ExpShock$ and $ExpShockROW$, since such exports may not reflect actual shocks experienced by the manufacturing sector in the local labor market.[36] In Panel B, we use a Bartik IV that is constructed as a weighted-average of ROW product-by-destination trade shocks; this goes beyond the product-level shocks in (3), by introducing variation that in principle captures destination-specific demand forces. As a refinement, we follow Redding and Venables (2004) to infer these destination-specific demand forces from importer-year fixed effects which we estimate in product-level gravity equations. This allows us to build a pair of Bartik IVs based respectively

[33]We have also verified that the trends in labor unrest over the pre-sample period (i.e., 2011-12) are uncorrelated with the export shares of the 15 HS sections (available upon request).

[34]As an alternative approach to demonstrate that the results are not driven by potential outlier industries, we reconstruct the instrument by dropping products whose shifters appear below the 5th or above the 95th percentile. The results remain stable (available upon request).

[35]Our data setting does not allow us to directly implement the Adão et al. (2019) correction procedure, as we have more product-level trade shocks ($> 4,000$ HS6 codes) than geographic units (333 prefectures).

[36]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".

on: (i) gravity-implied product-by-destination trade shocks (Panel C); and (ii) gravity-implied product-level trade shocks (after aggregating across destination countries, Panel D). Our main findings remain unaffected under each of these alternatives.

Other Prefecture-Level Shocks (Appendix B.5): The interpretation of our results could be undermined if the ROW demand shocks in the Bartik IV were incidentally correlated with other contemporaneous shocks that originate from within China. If so, our regressions may not be picking up the effects of export demand *per se*. We seek to address this by controlling directly for a proxy for domestic demand shocks (Panel A, Table B.8); this is constructed as a shift-share variable, using changes in domestic absorption computed for Chinese Standard Industrial Classification (CSIC) industries at the four-digit level, which we then project onto prefectures using pre-period employment shares in the respective CSIC industries as weights. We also account for possible Chinese domestic supply shocks, by including a similarly-constructed shift-share measure of prefecture output shocks (Panel B). Even when the domestic demand and output shock proxies are used jointly, the estimated effect of $ExpShock_{it}$ remains robust (Panel C).[37] We have likewise constructed a prefecture import shock measure; when included in the regressions, this has little bearing on the export shock coefficient (Panel D).[38]

4.3 Other Labor Market and Economic Outcomes

Our analysis to this point has focused on labor strikes. That said, if the export slowdown indeed affected prefectures in the manner described, we should expect to observe effects on other outcomes related to employment and output, particularly in the manufacturing sector. Table 5 provides corroborating evidence on this front. We explore here a set of relevant prefecture outcomes constructed from the China City Statistical Yearbooks (unless otherwise stated); using the IV specification in (2), we estimate the effect of $ExpShock_{it}$ on the change (between years $t-1$ and t) in each of these other economic outcomes of interest.[39]

The patterns across the columns in Table 5 are consistent with the broader narrative that the export shock had an adverse impact on China's manufacturing sector. We find that a decrease in $ExpShock_{it}$ was linked with a fall in manufacturing employment expressed as a share of prefecture population (Column 1); while there was a decline too in the corresponding share of non-manufacturing employment (Column 2), this latter effect is much smaller in magnitude,

[37]Figure B.2 illustrates that the correlations between the CSIC industry-level domestic demand and domestic output shocks on the one hand, and the CSIC industry-level export shock on the other hand, are low; the respective slope coefficients are not significantly different from zero. This provides reassurance that the export slowdown is unlikely to be picking up the roles of domestic demand or supply shocks.

[38]The effect of an increase in imports on labor unrest is in principle 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.

[39]For each of the annual change outcome variables, we have dropped observations that are smaller than the 1st percentile or that exceed the 99th percentile, to reduce the influence of outliers on the regression estimates.

Table 5: Export Shocks and other Local Economic Outcomes

Dependent variable:	Δ **Economic outcome**$_{it}$				
	Share of Mfg. empl. in population	**Share of Non-Mfg. empl. in population**	**Log Industrial output per worker**	**Log Night Lights intensity**	**Log Average Wage**
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
ExpShock$_{it}$	0.0118**	0.0023**	0.0027	0.0237***	0.0018
	(0.0054)	(0.0010)	(0.0123)	(0.0076)	(0.0019)
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	Y
First-stage F-stat	56.77	101.75	109.25	106.75	92.14
Observations	800	800	804	808	792
R^2	0.5341	0.5943	0.6257	0.8212	0.4325

Notes: The dependent variable is the prefecture-level economic outcome in the respective column heading, computed as the change between year $t-1$ and t; observations at or below the 1st percentile and at or above the 99th percentile in each year are dropped as outliers. 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 1, 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.

implying that the manufacturing sector experienced a disproportionately larger employment hit. Where exports declined, gross industrial output per worker tended to move in tandem (Column 3), although this effect is not precisely estimated. Interestingly, we find a strong association between weak export performance and a drop in VIIRS-DNB night-lights intensity at the prefecture level (Column 4), indicating that the export slowdown cast a dampener on economic activity as measured via this broader proxy. We obtain an effect on average wages that is of the expected sign – suggesting that a decrease in $ExpShock_{it}$ would lower prefecture wages – although this point estimate is not statistically significant (Column 5). Note that the wage measure in the city yearbooks is an average across all sectors but covers only the segment of the workforce with hukou rights; to the extent that migrant workers bore the brunt of a negative export shock, this would not be directly picked up in this data.[40]

5 Political Response to Export Shocks: A Simple Model

Having established that the export slowdown prompted a rise in labor strikes, we turn now to an analysis of how these developments affected the stability of prefecture party leaders and influenced their policy actions. Tracing out these political economy implications of the export

[40]The coefficient estimates in Table 5 imply effects that are fairly sizeable. Consider for example a one-standard deviation more severe contraction in exports ($\approx$ 841 USD per worker). This would be associated with a 1.0 percentage point drop in the manufacturing share of prefecture population (Column 1), which is large when viewed against the in-sample standard deviation of 1.2 percentage points in this annual change variable. Similarly, a decrease in $ExpShock_{it}$ of this magnitude would be associated with a within-prefecture fall in night-lights intensity of 2.0% (Column 4).

slowdown will be the goal of the remainder of this paper.

To organize our thinking, we first develop in this section a simple model drawing on Persson and Zhuravskaya (2016), that is set up to capture features of China's administrative hierarchy and the career concerns of politicians within this system. 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 official (in the central government), who considers whether to retain or sideline its local agent. The upper-level official has full knowledge of both the extent of the export shock and the realized level of social stability in the prefecture.

In this stylized model of "political accountability with Chinese characteristics", we derive the optimal decision rule by which the central government can incentivize the prefecture leader's actions while screening out low-ability incumbents; this is in line with how such concerns – over incentive-provision and screening – are seen by China scholars to play a critical role in the evaluation process of local officials.[41] The model delivers predictions on how the severity of the export shock would affect patterns of turnover for local leaders, as well as the level of effort expended by them on fostering stability, that we will then confront with data in Section 6.

5.1 Setup

We consider a setting in which a prefecture experiences an export shock, denoted by $x \in [\underline{x}, \overline{x}]$. To fix ideas and to connect this to the empirical setting laid out earlier, this export shock is driven by external demand conditions that are exogenous from the perspective of the local leader. 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.

This export shock in turn affects social stability, y, at the prefecture level, which we specify to be:

$$y = h(x, m) + \varepsilon. \tag{4}$$

The $h(x, m)$ function captures how the export shock and the actions of the local leader jointly affect social stability y. Here, $m \geq 0$ denotes the level of stability-enhancing measures that the local leader adopts. We view m as encapsulating both public security measures to repress unrest ("sticks"), as well as social spending to soften the economic impact on workers ("carrots"). The ε term on the other hand is an independent draw from a mean-zero normal distribution, $N(0, \sigma^2)$, which reflects further idiosyncratic forces that influence social stability.[42]

[41] Heberer and Trappel (2013), for instance, note: "Career advancement is the central point of motivation for local cadres to achieve positive evaluations. Arguably, the evaluation of policy implementation plays a prominent role in promotions and career advancement. *Guanxi* alone, as a rule, are no longer sufficient" (p.1054).

[42] We can generalize this distribution to any smooth pdf function $\phi(\cdot)$ that: (i) features a finite mode; and (ii) where the probability mass vanishes to zero in the tails of the distribution (i.e., $\phi(z) \longrightarrow 0$ as $z \longrightarrow \pm\infty$). For example, the logistic distribution would be an alternative that satisfies these conditions.

We place some additional structure on $h(x, m)$ by requiring that: (i) $h_x > 0$; (ii) $h \longrightarrow -\infty$ as $x \longrightarrow \underline{x}$; and (iii) $h \longrightarrow +\infty$ as $x \longrightarrow \overline{x}$; for all $m \geq 0$. In words, a more negative export shock lowers social stability, since labor unrest would intensify. We assume that when the export shock is at its most severe ($x \longrightarrow \underline{x}$), social stability deteriorates substantially, to such an extent that any finite stochastic draw ε would be inconsequential for the overall value of y; we make an analogous assumption about how export shocks that are exceedingly favorable ($x \longrightarrow \overline{x}$) would bolster social stability. We further stipulate that: (iv) $h_m > 0$, $h_{mm} \leq 0$, with $h_m < \overline{h}$ being bounded; and (v) $h_{xm} < 0$. In the face of an export shock x, the use of stability measures m can therefore raise overall stability, although this is (weakly) subject to diminishing returns. Finally, we assume that such measures are more effective at raising social stability when the export shock is more adverse. Put otherwise, additional "sticks" or "carrots" are less relevant when the export performance of the prefecture is healthy.[43]

The upper-level government in our model seeks to leverage local leaders' career concerns, in order to incentivize the latter to undertake costly measures to bolster social stability when necessary. Local leaders bear a cost $g_\ell(m)$ of implementing stability measures, where $g_\ell(m)$ is increasing and convex in m; this comprises for example the locally-borne costs of diverting fiscal and organizational resources towards mounting a public security response. In line with related work that has highlighted how China's hierarchical system has been designed (at least in part) to facilitate the selection of more capable politicians (e.g., Edin 2003, Heberer and Trappel 2013, Lorentzen 2013), the model incorporates two types of prefecture leaders – indexed by $\ell = G$ ("good") and $\ell = B$ ("bad") – who differ in their innate competency in delivering social stability. We will adopt a functional form below that features: $g'_B(m) > g'_G(m)$ for all $m > 0$, so that G-type leaders have a lower marginal cost of implementing a given level of stability measures m, this being the key dimension that distinguishes the two leader types.

The timing of events is as follows. The party secretary in the prefecture (henceforth, he/him) first observes the local export shock x, and decides on the level of stability measures m to implement. The idiosyncratic term ε is then realized. The upper-level government (henceforth, she/her) does not directly observe ε; she instead observes the overall value of social stability y, and proceeds to evaluate the local incumbent. We assume that she has complete information about x and the exogenous nature of this shock; in particular, this means that the export slowdown itself will not be mis-attributed to the local incumbent's handling of economic matters. At the same time, she is unable to perfectly observe the local incumbent's actions m nor directly see his ability type, though she is aware that there is a share $p \in (0, 1)$ of G-type leaders in the large pool of officeholders. The upper-level government then decides either to retain or sideline

[43]We show in Appendix C.1 how this formulation of the social stability function can be linked with the worker-level model of strike intensity that we develop in that same appendix section. Specifically, we interpret the level of strike intensity chosen by the representative worker as an inverse measure of social stability, and provide an illustrative example which generates a $h(x, m)$ functional form that satisfies the required limit value and derivative conditions (i)-(v).

the local incumbent. If he is retained, he obtains a payoff of R, which captures the present discounted value of future rents from holding office (including built-in expectations about rents from possible subsequent promotions). If he is instead sidelined, we normalize his payoff to 0.[44]

We consider an upper-level government who is concerned with maximizing expected social stability, which has consistently been a foremost objective of China's ruling party (e.g., Edin 2003, Chen et al. 2016).[45] The upper-level government's objective function is therefore given by: $E(y) = ph(x, m_G) + (1-p)h(x, m_B)$, where m_G and m_B denote the levels of m chosen by each respective type of local leader. In Appendix C.2, we show formally that this implies a threshold rule in which she will retain the incumbent if and only if y exceeds $\bar{y}(x)$, while sidelining him otherwise. As the notation anticipates, the optimal threshold will be a function of the observed export shock, x.

With this in mind, a local leader of type $\ell \in \{G, B\}$ would choose m in order to maximize his expected rents, less the costs borne from enacting stability measures:

$$\Pr\left(y > \bar{y}(x)\right) R - g_\ell(m) \;=\; \left[1 - \Phi(\bar{y}(x) - h(x,m))\right] R - g_\ell(m).$$

Here, $\Phi(\cdot)$ is the cdf of the $N(0, \sigma^2)$ normal distribution for ε. The first-order condition with respect to m for an interior solution is therefore:

$$\phi(\bar{y}(x) - h(x,m))h_m(x,m)R \;=\; g'_\ell(m), \tag{5}$$

where $\phi(\cdot)$ is the pdf associated with $\Phi(\cdot)$. For concreteness, we will consider a marginal cost function that implies an equilibrium in which the two leader types choose distinct levels of stability measures, and use this to convey the essential intuition. Specifically, let: $g'_\ell(s) = a_\ell + \delta m$, where $\delta > 0$, $a_G = 0$ and $a_B > R\bar{h}/\sqrt{2\pi\sigma^2}$. Since $\phi(\cdot)$ achieves a maximum value of $1/\sqrt{2\pi\sigma^2}$ at $\phi(0)$, this last condition on a_B implies that for a B-type leader, the marginal cost of enacting stability measures exceeds the marginal benefit for all $m > 0$. We thus have $m_B = 0$ regardless of $\bar{y}(x)$, as the stability measures are too costly to B-type leaders.

As for G-type leaders, we show in Appendix C.2 that the first-order condition (5) implies a strictly interior solution for m_G (i.e., with $m_G > 0 = m_B$); the proof is a straightforward application of the intermediate value theorem. Since the upper-level government's goal is to maximize expected stability, she will have an interest in eliciting as high a level of m_G as possible. With $\phi(0)$ being the modal value of $\phi(\cdot)$, and bearing in mind that $h_{mm} \leq 0$, this is

[44] While we do not develop this explicitly in the model for the sake of simplicity, the expected future rents would be higher (all else equal) for G-type leaders in a fully dynamic setting, given that their probability of retention is higher in equilibrium. Intuitively, this would reinforce the implications of the model, as it would further incentivize G-type leaders to intensify their use of measures to bolster stability.

[45] Local officials can at times be evaluated on the basis of "一票否决" ("yipiao foujue", literally: "one item veto rule"). This means that particular goals are heavily weighted to the extent that if one of these targets is missed, "all other achievements of the local leadership will be void" (Heberer and Trappel 2013). The objective function we adopt in the model is consistent with social stability being designated as one such overriding target.

achieved by the value of m_G that satisfies:

$$\phi(0)h_m(x, m_G)R \quad = \quad \delta m_G. \tag{6}$$

Moreover, the stability threshold that the upper-level government adopts for retaining the local incumbent is that which sets the argument of $\phi(\cdot)$ to zero, namely: $\bar{y}(x) = h(x, m_G)$.

5.2 Model Predictions

We consider the model's implications for two political economy outcomes, namely: the likelihood the local incumbent is sidelined (turnover), and the resources the local incumbent expends to maintain stability (stability measures). We derive predictions on how each of these outcome variables would respond to a more severe export slowdown, these being relationships which we will be able to explore empirically in Section 6.

Stability Measures: Log differentiating (6), we obtain:

$$\frac{dm_G}{dx} \quad = \quad \frac{h_{xm}m_G}{h_m - m_G h_{mm}} < 0, \tag{7}$$

since $h_m > 0$, $h_{mm} \leq 0$ and $h_{xm} < 0$. Thus, for G-type leaders, the use of stability measures – and presumably, the expenditure on such efforts – would increase in response to a negative export shock. Intuitively, a slowdown in exports raises the need for and effectiveness of measures to bolster social stability; G-type leaders would respond by intensifying their use of stability measures, in order to better separate themselves from their low-capability peers. Since $m_B = 0$, these implications also apply to the expected level of stability measures, $pm_G + (1-p)m_B$, for a prefecture whose leader's type we are not able to directly observe.

We further consider how the intensity of the above response – the increase in stability measures when exports weaken – might depend on the size of the rents that the local incumbent can expect should he be retained (i.e., $\frac{d^2 m_G}{dxdR}$). This is a relevant dimension of heterogeneity that we will be able to explore with plausible empirical proxies for these expected rents R. To study this cross-derivative, it will be helpful to examine the particular case where the systematic component of the stability function takes the form: $h(x, m) = h^{(1)}(x) + h^{(2)}(x)m$, so that for any given level of the export shock x, $h(x, m)$ is linear in the stability measures m that are implemented.[46] In this case, the expression in (7) simplifies to: $\frac{dm_G}{dx} = \frac{h_x^{(2)}}{h^{(2)}} m_G$, so that: $\frac{d^2 m_G}{dxdR} = \frac{h_x^{(2)}}{h^{(2)}} \frac{dm_G}{dR}$. Note that the underlying requirement that $h_{xm} < 0$ implies that $h_x^{(2)} < 0$.

[46] One can readily find examples of $h^{(1)}(x)$ and $h^{(2)}(x)$ functions for which the limit value and derivative conditions (i)-(v) required of $h(x, m)$ would be satisfied; for instance, consider: $h^{(1)}(x) = \ln(x/(1-x))$ and $h_x^{(2)}(x) = 1 - x$, with $x \in [0, 1]$ and $m \in [0, 1]$. More broadly, it should be clear from inspecting the expression for $\frac{dm_G}{dx}$ in (7) that any efforts to sign $\frac{d^2 m_G}{dxdR}$ under a general $h(x, m)$ function will require making assumptions about the third-derivatives of $h(x, m)$, which can be unintuitive.

Moreover, log-differentiating (5), we get: $\frac{dm_G}{dR} = \frac{h_m m_G}{h_m - m_G h_{mm}} \frac{1}{R}$, which is clearly positive (since $h_m > 0$ and $h_{mm} \leq 0$). We thus have: $\frac{d^2 m_G}{dx dR} < 0$; in words, the incentive to raise stability measures following a negative export shock would be stronger for local incumbents who face high expected future rents, so long as diminishing returns to the use of stability measures do not set in too quickly. (Since $m_B = 0$, this statement holds too in expectation for a local leader of ex-ante unknown type.)

Party Secretary Turnover: What does the model predict for a local incumbent's likelihood of being sidelined? We have derived the upper-level government's cutoff rule to be: $\bar{y}(x) = h(x, m_G)$. For a G-type leader, it is straightforward to see that the probability of turnover is $\Phi(0) = 1/2$ and does not depend on the export shock x; in particular, this means that the G-type leader is not penalized if he should suffer the "bad luck" of being dealt with a severe export slowdown. This is because the threshold $\bar{y}(x)$ adjusts with the observed export shock: the incumbent is evaluated on a relative benchmark, with the upper-level government accommodating a higher level of instability – i.e., setting a lower $\bar{y}(x)$ – when prefecture exports are hit by a more severe slowdown. Without this, say if $\bar{y}(x)$ were set at an absolute level instead, the G-type leader would not be properly incentivized in the event of a very severe export slowdown to incur costs on stability measures that do not strongly improve his prospects for retention. (That said, the presence of the stochastic term ε prevents the central government from perfectly retaining G-type leaders with probability 1.)

The corresponding turnover probability for a B-type leader is $\Phi(\bar{y}(x) - h(x, 0))$, since $m_B = 0$. Note that a B-type incumbent is more likely to be replaced than a G-type for a given level of the export shock, as: $\Phi(\bar{y}(x) - h(x, 0)) > \Phi(\bar{y}(x) - h(x, m_G)) = \Phi(0)$. Therefore, in addition to incentivizing G-type leaders to implement stability measures, the cutoff rule acts as a screen in that more capable incumbents are retained with a higher probability.[47]

Last but not least, we consider the implications for the probability of turnover for a prefecture leader whose type we do not directly observe. Given that the share of G-type leaders is p, this probability is: $\frac{p}{2} + (1-p)\Phi(\bar{y}(x) - h(x, 0)) = \frac{p}{2} + (1-p)\Phi(h(x, m_G) - h(x, 0))$. Differentiating this expression with respect to x yields:

$$(1-p) \cdot \phi(h(x, m_G) - h(x, 0)) \cdot \left(h_x(x, m_G) - h_x(x, 0) + h_m(x, m_G) \frac{dm_G}{dx} \right).$$

(Note in particular that the B-type leader does not alter his choice of $m_B = 0$ for small changes in x.) The above derivative has a negative sign, as: (i) $h_x(x, m_G) - h_x(x, 0) < 0$ (since $h_{xm} < 0$); (ii) $h_m(x, m_G) > 0$; and (iii) $\frac{dm_G}{dx} < 0$. Thus, when looking across a broad set of local leaders, one should expect that negative export shocks increase the likelihood of incumbent turnover; this

[47]Note though that the screen does not achieve a perfect separation of G-type from B-type leaders. This is because it is possible for a B-type leader to obtain a highly favorable stochastic draw ε that raises the realized level of stability y above the threshold $\bar{y}(x)$.

is driven in particular by the fact that low-capability leaders find themselves more vulnerable to being sidelined when the export slowdown is more severe.

Comparison with an "unsophisticated" central government: To this point, we have been adopting the baseline assumption that the upper-level government is "sophisticated", in that she explicitly conditions the cutoff rule for the retention of the local leader on the observed export shock x. It is instructive now to contrast this case against the predictions that would emerge if instead the upper-level government does *not* take into account the realized level of x. This will provide an alternative benchmark against which to assess the empirical patterns on political economy outcomes that we will turn to in Section 6.

Suppose then that the upper-level government picks an "unsophisticated" cutoff rule where $\bar{y}$ does not depend on x. The first-order condition in (5) with respect to m is now:

$$\phi(\bar{y} - h(x,m))h_m(x,m)R \quad = \quad a_\ell + \delta m.$$

Maintaining the assumption that a_B is sufficiently large, we continue to have $m_B^u = 0$, with B-type leaders unwilling to incur costs on stability measures. (The superscript u refers to outcomes in this "unsophisticated" case.) For G-type leaders on the other hand, should the export shock become very adverse and $x \longrightarrow \underline{x}$, we would have: $\bar{y} - h(x,m) \longrightarrow -\infty$ and hence $\phi(\bar{y} - h(x,m)) \longrightarrow 0$. Given that $a_G = 0$, the level of stability measures adopted by the G-type leader m_G^u would then tend to 0 too.[48] The underlying intuition is that a G-type leader would have no incentive to put in positive effort on stability measures when x is very low, since the "fundamentals" from the bad export shock dwarf the stochastic term and there is no chance that social stability will exceed the $\bar{y}$ cutoff. In other words, a G-type incumbent may as well "give up" under a sufficiently negative shock, since he understands he will be punished regardless by the "unsophisticated" upper-level government. This stands in stark contrast to the predictions derived earlier for the case of a "sophisticated" central government, where the G-type leader is incentivized (via the cutoff rule that adjusts with x) to continue to adopt stability measures – and in fact increase their use – as the export shock worsens.

As for the implications for turnover, the relevant probability for a given local leader is now: $p\Phi(\bar{y} - h(x, m_G^u)) + (1-p)\Phi(\bar{y} - h(x,0))$, and its derivative with respect to x is:

$$p\phi(\bar{y} - h(x, m_G^u))\left(-h_x(x, m_G^u) - h_m(x, m_G^u)\frac{dm_G^u}{dx}\right) - (1-p)\phi(\bar{y} - h(x,0))h_x(x,0).$$

[48] As an extension of this logic, one can show that if x is in a sufficiently low range, m_G^u would actually be increasing in x. Log differentiating the first-order condition for the G-type leader yields:

$$\frac{dm_G^u}{dx} \quad = \quad \frac{h_{xm}m_G^u - (\phi'/\phi)h_x h_m m_G^u}{h_m - m_G^u h_{mm} - (\phi'/\phi)(h_m)^2 m_G^u}.$$

From the pdf of the normal distribution, note that: $\phi'(\bar{y} - h)/\phi(\bar{y} - h) = -(1/\sigma^2)(\bar{y} - h)$. As $x \longrightarrow \underline{x}$, we have $h \longrightarrow -\infty$ and hence $\frac{\phi'}{\phi} \longrightarrow -\infty$. Thus, $\frac{dm_G^u}{dx} > 0$ for very low values of x.

The above effect of x on turnover cannot be signed unambiguously, even though we know that $-h_x(x, m_G^u) < 0$ and $-h_x(x, 0) < 0$. Suppose in particular that we are in a range of values of x where $\frac{dm_G^u}{dx} < 0$, this being the more well-behaved case where a slowdown in exports raises the G-type leader's use of stability measures. We would then have: $-h_m(x, m_G^u)\frac{dm_G^u}{dx} > 0$, and this could dominate the entire derivative if stability measures are at the same time very efficacious in enhancing h (i.e., $h_m(x, m_G^u)$ is large). Put otherwise, there is now the possibility that an export slowdown can be associated with a fall in turnover probability if G-type leaders respond with an elevated use of stability measures to try to raise y above the absolute cutoff $\bar{y}$. This is quite different from the behavior of G-type leaders seen under a "sophisticated" central government: There, the cutoff adjusts smoothly as a function of x, so that the turnover probability for a G-type leader remains constant at 1/2; instead, as exports worsen, it is the turnover probability of B-type leaders that rises as they become more likely to be screened out. It is this latter effect that generates a strict downward-sloping relationship between turnover probability and export performance under a "sophisticated" central government.

The key takeaway then is that the case of an "unsophisticated" upper-level government yields a distinct set of predictions, especially when export shocks are very negative: The use of stability measures would be low, and moreover unresponsive to small changes in the severity of the export shock. The relationship between export performance and turnover may also exhibit non-monotonicities. These contrasting predictions are useful to bear in mind for interpreting the empirical results below.

6 Political Response to Export Shocks: Evidence

6.1 Incumbent Turnover

Guided by our model, we now turn to the evidence on the response of the upper-level government vis-à-vis whether to retain or sideline prefecture party leaders. To cut to the punchline, we find that negative export shocks were associated with increased incumbent turnover; the patterns we uncover are moreover in line with the interpretation that the turnover decision is being used strategically by a "sophisticated" central government to incentivize and screen local officials.

Data and Specification: We collected information on the biographic characteristics and career histories of local party secretaries from their curricula vitae. These were compiled from the database of leaders maintained by People.cn, an official website affiliated with the Chinese government.[49] The data cover 544 individuals who held the position of prefecture party secretary at any point during the period 2013-2016, and allows us to track the month and year in which each individual took and/or left office. We focus on the party secretary, as this is the

[49]See: http://ldzl.people.com.cn/dfzlk/front/firstPage.htm.

top executive position at the prefecture level, with ultimate authority and substantial discretion over local fiscal, regulatory, and personnel policies (Persson and Zhuravskaya 2016). As the official directly responsible for political duties such as maintaining social stability, his/her career trajectory would in principle be most susceptible among local officeholders to any labor unrest associated with negative economic shocks (Yao and Zhang 2015).

We define $Turnover_{it}$ to be an indicator variable equal to 1 when there is a change in party secretary in prefecture i in a given calendar year t. Over 2014-2016, the average annual turnover rate for prefecture party secretaries was 29.6% (see Table B.1). We further classify the nature of each instance of turnover as: a promotion, a lateral movement, or 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 administrative hierarchy of positions. This starts at the top with national-level appointments, 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-province level or above, while a lateral movement is a transfer to a different prefecture-level position. There are a number of key exceptions though to this coding rule, which we elaborate on in Appendix A.4. The most pertinent of these is that several high-profile prefectures are officially designated as province-level or sub-province-level administrative units (e.g., Beijing); the party secretary positions in these locations are thus of higher rank, and we classify movements into and out of these positions on the basis of this higher rank.[50] 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 a prefecture party secretary was demoted to a position at the sub-prefecture level or below.

The fact that there are no observed demotions in rank indicates that punishment for weak performance takes a different form in China's political system. We therefore examine more closely the nature of the lateral movements. Existing guidelines indicate that prefectural officials should have served at least three years in a position, before being eligible for promotion to the next level in the hierarchy; see for example the *Regulations for the Selection and Appointment of Party Cadres*.[51] Based on this, we 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.4, we

[50] In addition, we do not categorize appointments to several honorary positions as promotions (e.g., chairman of the province-level People's Congress); even though these are nominally of sub-province rank, the positions are viewed as "consolation prizes" or retirement posts (Li and Zhou 2005, Yao and Zhang 2015). Some prefecture party secretaries simultaneously hold positions that rank at the sub-province level (e.g., member of the provincial standing committee); for such cases, we consider a movement to another position at the sub-province level (e.g., a vice-provincial governor) as a lateral movement.

[51] Issued by the Organizational Department of the Chinese Communist Party. See: http://www.china.org.cn/english/congress/226530.htm.

provide regression-based 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.[52] We thus associate an "early" lateral movement with a *de facto* demotion – or being sidelined (as per the language used in the model) – as it typically slows down an official's career trajectory.

Using this data on incumbent turnover, we estimate regressions that follow closely the earlier specification in (2):

$$Turnover_{i,t+1} = \theta_1 ExpShock_{it} + \theta_X X_{it} + D_{pt} + D_i + \varepsilon_{it}. \tag{8}$$

This investigates how the likelihood of replacement of the party secretary in year $t+1$ might depend on the export shock experienced in prefecture i 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 weight the observations by prefecture working-age population, and cluster standard errors by province. In addition to the prefecture time-varying controls from (2), we include in X_{it} a set of incumbent characteristics extracted from their CVs: a gender dummy, age, education (a dummy variable for Masters degree or higher), and tenure in current position (in years). We also construct a dummy variable for whether the incumbent's appointment is in their province of birth, as a proxy for the strength of their ties with local political networks.

Results: Table 6 reports our results from estimating (8). Column 1 demonstrates that the incumbent party secretary was indeed more likely to be replaced following a downturn in prefecture exports. This effect is present when we focus our dependent variable on movements of a lateral nature in Column 2; a one-standard-deviation more negative export shock would raise the likelihood of lateral movement by $0.841 \times 0.0907 \approx 7.6$ percentage points, a sizeable effect when compared against the average rate of such turnover of 16.3% in our sample period. (As reported in Column 2 of the Appendix B tables, this link from the export shock to incumbent lateral movement is robust under the many checks discussed in Section 4.2; Columns 3-5 of these appendix tables likewise report the analogous checks for the textual analysis scores and fiscal measures that we consider in Sections 6.2 and 6.3. Figure B.1 presents binned scatter plots, analogous to Figure 4 above, to illustrate the relationship running from the export shock to each of these political response variables, including incumbent lateral movement.)

In the remaining columns of Table 6, we replace $Turnover_{i,t+1}$ in (8) with indicator variables that break down further the nature of incumbent movement. We find more specifically that it is the "early" cases of lateral movement – indicative of a *de facto* demotion – that rise in response to a bad export shock (Column 3). In sharp contrast, the export shock in the prefecture has no bearing on lateral movement after a full three-year tenure (Column 4), nor on promotion

[52] 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.

Table 6: Export Shocks and Party Secretary Turnover

Dependent variable:	**Party Secretary Turnover**$_{i,t+1}$				
	Turnover	**Lateral**	**Lateral**	**Lateral**	**Promotion**
			Tenure$_{i,t+1} < 3$	Tenure$_{i,t+1} \geq 3$	
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
ExpShock$_{it}$	-0.0750***	-0.0907***	-0.0834***	-0.0073	-0.0048
	(0.0222)	(0.0314)	(0.0205)	(0.0408)	(0.0102)
Incumbent Characteristics:					
Tenure$_{i,t+1}$	0.2090***	0.1138***	-0.0236*	0.1374***	0.0344***
	(0.0167)	(0.0192)	(0.0119)	(0.0147)	(0.0105)
Age$_{i,t+1}$	0.0365**	0.0244*	0.0215**	0.0029	0.0094
	(0.0154)	(0.0136)	(0.0100)	(0.0075)	(0.0068)
Born in the same province$_{i,t+1}$	-0.0125	-0.0917*	-0.0241	-0.0676*	0.0151
	(0.1095)	(0.0524)	(0.0497)	(0.0329)	(0.0415)
Master degree or above$_{i,t+1}$	0.1142	-0.0712	-0.0481	-0.0231	0.0693
	(0.1097)	(0.0858)	(0.0537)	(0.0593)	(0.0734)
Female$_{i,t+1}$	0.1732	0.3353	0.2175	0.1177	-0.0368
	(0.1996)	(0.2187)	(0.1488)	(0.0871)	(0.0714)
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	Y
First-stage F-stat	116.76	116.76	116.76	116.76	116.76
Observations	821	821	821	821	821
R^2	0.5568	0.4987	0.4759	0.5545	0.4660

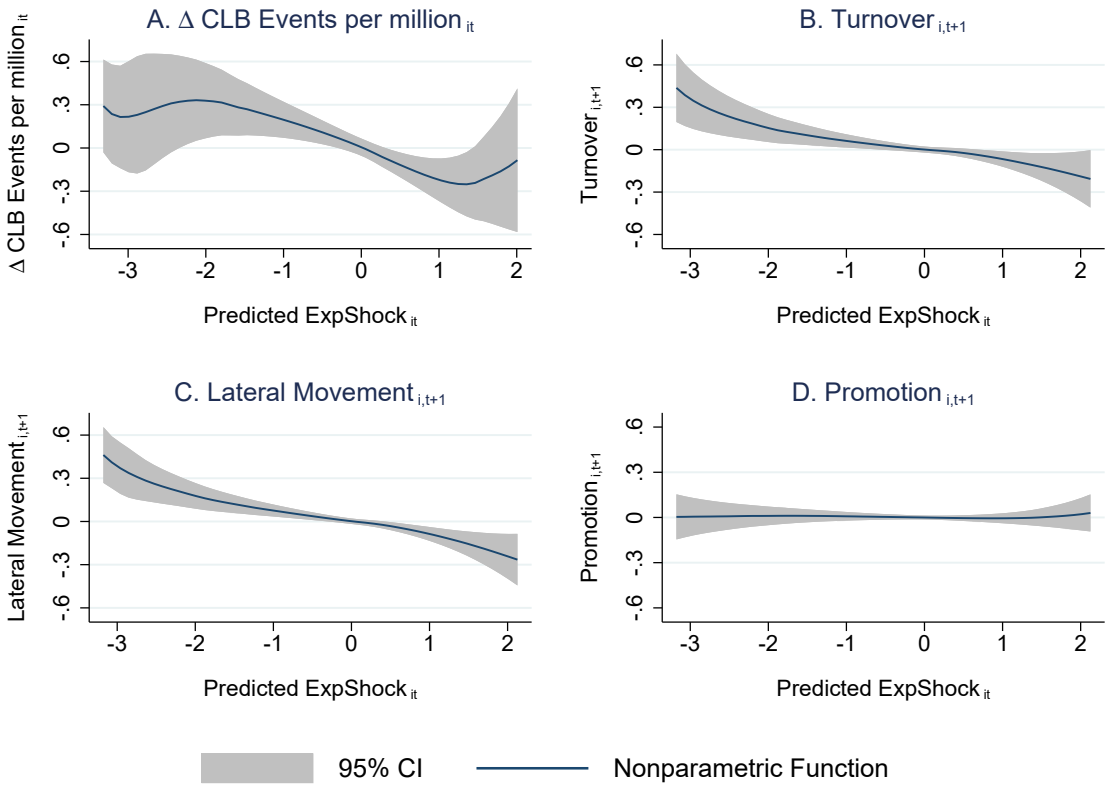
Notes: The dependent variable in each column is a dummy for whether there was a change in prefecture party secretary in year $t+1$ (i.e., one year after the export shock); this is for all forms of turnover (Column 1), lateral movements (Columns 2), lateral movements when the incumbent had a tenure of < 3 years (Columns 3), lateral movements when the incumbent had a tenure ≥ 3 years (Columns 4), and promotions (Columns 5), respectively. 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 1, 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.

(Column 5). Negative export shocks are thus linked with an increased likelihood that the party secretary will be reshuffled in a manner detrimental to his/her career prospects. This is in line with the implications of our model, where the upper-level government screens out and sidelines local agents whose performance in the face of an export slowdown has been assessed to be subpar.

We examine these patterns further in Figure 5, which illustrates the relationship between export shocks and incumbent turnover using a more flexible local polynomial plot. This is motivated by the earlier discussion that non-monotonicities in the turnover probability are possible if the upper-level government were adopting an "unsophisticated" decision rule. On the horizontal axis in each panel, we have plotted the export shock as predicted by our IV in

the first-stage of (8).[53] Panel A confirms that it is the prefectures that were most negatively exposed to an export slowdown that witnessed the largest increases in labor unrest events. Most interestingly, we find a distinct downward-sloping relationship between the likelihood of incumbent turnover and a prefecture's export performance (Panel B), and specifically when the movement considered is of the lateral variety (Panel C). We instead obtain slope coefficients that are indistinguishable from zero – a flat relationship – when promotions are considered (Panel D). The monotonic patterns in Panels B and C are certainly more consistent with – even if not fully definitive evidence of – outcomes under a "sophisticated" upper-level government, that strategically takes into account observable local export shocks when making decisions over personnel.

Figure 5: Prefecture Export Shocks and Incumbent Turnover
(Local polynomial regression)

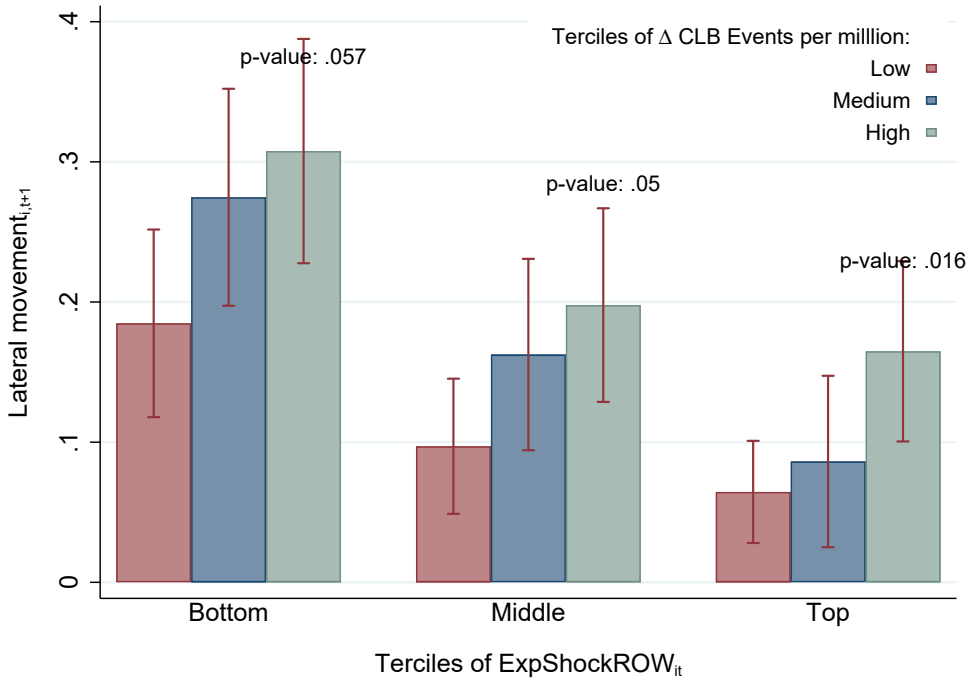


To what extent is this negative link between export performance and incumbent turnover mediated by labor strikes? Figure 6 speaks to this issue, by illustrating the probability of lateral movement in year $t+1$, broken down first by terciles of $ExpShockROW_{it}$ (i.e., the export shock

[53] More precisely, the predicted export shock and the incumbent turnover variables that we use to plot Figure 5 have each been residualized of the variation that can be explained by the prefecture and incumbent controls X_{it} in (8) as well as the D_{pt} and D_i fixed effects.

IV), and then by terciles of $\Delta(Events/L)_{it}$ (i.e., the observed change in CLB labor strikes per million workers in year t) within each of the export shock bins. Comparing across the export shock terciles, Figure 6 underscores the previous finding that lateral movement was more likely in prefectures hit by worse export shocks. Looking further within *each* of the three export shock bins, we uncover a distinct pattern wherein party secretaries who saw a larger increase in labor unrest under their watch were more likely to be laterally moved.[54] This is suggestive that the lateral movement was in part a consequence of a negative evaluation of the local leader's handling of the labor strike situation relative to their peers who were subject to a comparable export shock. In fact, prefectures that were in the worst-hit export tercile, yet saw relatively little unrest, actually experienced a similar amount of lateral turnover as those prefectures that were in the best-performing export tercile, but saw the largest increases in labor turmoil.

Figure 6: Export Performance, Strikes, and Incumbent Turnover



In sum, the body of evidence on incumbent turnover lends itself to the interpretation that the higher levels of government in China made active decisions about the replacement of local party secretaries in response to the export slowdown. The labor strike situation mattered for incumbent turnover, but in a subtle way: Decisions made over whether to retain an incumbent appear to account for the severity of the export slowdown that he/she was exposed to – in line

[54] Within each tercile of $ExpShockROW_{it}$, we conduct a t-test for whether the probability of lateral movement is the same in the first and the last bins of $\Delta(Events/L)_{it}$. The corresponding p-values are reported in the figure.

with the upper-level government being "sophisticated" – rather than being determined solely by the absolute amount of labor unrest that was observed.

6.2 Stability Measures: "Weiwen" Textual Analysis

We now turn to investigate the responses of the prefecture party secretaries. As we have argued in the model in Section 5, more severe export slowdowns can be expected to induce a greater use of stability measures, especially if the local incumbent sees larger rents from retaining office. We first document such patterns in this section using a textual analysis measure that reflects the emphasis on law and order in prefecture annual work reports; we will show in Section 6.3 similar shifts using measures based on observed prefecture fiscal expenditures.[55]

Data and Specification: We adopt a novel approach to measure the degree of attention paid to 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 translates literally as "maintaining stability". It was reportedly first used in the official People's Daily newspaper in 2002, in an article accompanied by a photograph of armed police. Since then, the term "weiwen" has been adopted as a political watchword by the authorities, to refer to actions to maintain law and order in the interest of preserving domestic stability (*New York Times*, 2012).

We draw on the above observation to construct measures of the degree of emphasis on social stability exhibited by the local government in its annual work report. Within China's political system, this work report is 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. Each report comes in two sections. The first section is a summary of socioeconomic conditions from the preceding year, often rendered as a list of the local government's accomplishments. On occasion, this material mentions instances of high-profile strikes or unrest events that drew the government's attention. The second section lays out development policies for the year ahead. Apart from describing economic plans, this includes measures intended to mitigate social unrest ("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 work report in our sample period, and count the number of

[55] We should note that we view the "weiwen" emphasis in speeches and the uses of fiscal expenditure as only a subset of a broad range of measures that local incumbents can enact in order to tackle a decrease in social stability. There are in principle other measures that local leaders could implement (e.g., internet surveillance, coordination with factory managers, side payments to striking workers, etc.) that are not fully observed even by the central government, so that the latter cannot condition its turnover decision on the efficacy of the full set of stability-enhancing measures that the local leaders have adopted.

occurrences of twelve 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 is in Table A.2.) The keyword count is then normalized by the total count of Chinese characters in the work report.

We also implement a more advanced 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 manually all sentences as either being about "weiwen" or "not weiwen". These labelled passages were used, together with paragraphs from a national-level State Council document dated April 2015 on the topic of domestic security measures, as the training sample for the algorithms.[56] 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). The MNB generates a posterior probability that a paragraph is on the topic of "weiwen", using an underlying multinomial distribution model of token frequencies. The SVM on the other hand is a binary classifier, that generates a 0-1 prediction for whether a paragraph is about "weiwen", after partitioning the observations in a high-dimensional metric space. (See Appendix A.6 for more technical details.) We compute a report-level score, by taking the character-length weighted average of the paragraph scores. We view these "weiwen" scores as capturing the degree to which maintaining social stability was an announced policy priority for the prefecture government in the year in question.[57,58]

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

$$\Delta y_{i,t+1} = \gamma_1 ExpShock_{it} + \gamma_X X_{it} + D_{pt} + D_i + \varepsilon_{it}. \tag{9}$$

This specification is similar to (2), with a textual analysis score (denoted by y) now being used

[56]The State Council document in question is: "Opinions on Strengthening Society's Public Security Prevention and Control System", which provides a set of recommendations on "weiwen" measures; a training paragraph is reproduced in Appendix A.5. For the full document, see: http://www.gov.cn/xinwen/2015-04/13/content_2846013.htm

[57]Our approach makes use of "supervised" machine-learning algorithms, in that the algorithm is trained to recognize "weiwen" versus "not weiwen" passages instead of being tasked 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).

[58]As a placebo test, we have checked the predictions that the trained algorithms deliver on paragraphs that are related to tackling economic volatility (such as in stock or real estate prices), given that the Chinese phrase ("稳定") is also used in references to economic stabilization policies. Both MNB and SVM models returned "weiwen" scores close to zero for such passages, verifying the algorithms' ability to discriminate between content related to economic versus political stability. Separately, we also implemented a cross-validation of our training paragraphs, by omitting a subset (one quarter) of the 20 pre-sample work reports at a time when training the machine-learning algorithms; the "weiwen" scores of the omitted work reports was then computed. Reassuringly, the returned scores were very close to 1 (see Appendix A.6 for more details).

in place of the CLB events variable. Note that (9) seeks to explain changes in the political response variable y between years t and $t+1$, as a function of the export shock in the preceding year. We lead the left-hand variable by one period for two reasons. First, this accommodates a lag in how quickly political actions would respond to an adverse economic shock whose severity may not be fully anticipated. 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 the change in political emphasis on stability $\Delta y_{i,t+1}$ between year t and $t+1$ (i.e., 2013-2014, 2014-2015, 2015-2016) to the export shock experienced between year $t-1$ and t (i.e., 2012-2013, 2013-2014, 2014-2015, respectively). We instrument for the export shock with the Bartik IV from (3), while controlling for prefecture and incumbent characteristics, as well as province-year and prefecture fixed effects, as in the regression model (8) from Section 6.1. The regressions are likewise weighted by the 2010 working-age population, with standard errors clustered by province. When examining local leaders' responses, we further include a set of "prefecture tier" dummy variables, namely whether i is a regular prefecture or an administrative unit at the sub-province-level (or higher); we interact these with year dummies to capture the possibility that party leaders in higher-profile appointments may be more obligated to fall in line with political directives on "weiwen" that are being set over time at the national level.

Motivated by the predictions from our framework in Section 5 regarding the impact of future office rents (R), we also consider whether the effect of the export shock on "weiwen" emphasis might vary with the career prospects of the local incumbent. For that, we explore an interaction term with an indicator variable for whether he/she was 57 years-old or younger in year $t+1$. Given that the mandated retirement age for prefecture party secretaries is 60, these are local leaders who have at least a full three-year term of service ahead of them, and who should in principle foresee higher future rents if they avoid an "early" lateral movement from their current positions.[59] In fact, as can be seen in Panel A of Figure 7, a local officeholder is distinctly less likely to be promoted after the age of 57. Moreover, the retirement age appears to be a binding constraint, as there are no leaders in our sample who continue to serve in prefecture-level party secretary positions beyond the age of 61.[60]

Results: Table 7 presents the regressions from this analysis of the prefecture annual work reports. For each "weiwen" measure, the odd-numbered columns report a basic specification without the interaction term between $ExpShock_{it}$ and the party secretary age dummy, while the even-numbered columns include this interaction term.[61] Our key finding is that negative

[59]Persson and Zhuravskaya (2016) work with a variable that is in a similar spirit, to capture incumbents in their final term of office prior to retirement age.

[60]Out of 13 prefecture party secretaries who were active after age 60, eight were officeholders in Beijing and Shanghai (province-level municipalities where the retirement age is 65), while the remaining five all served only a few months at age 61 before being moved to an honorary position or retired from politics. The party guidelines on retirement can be found at: http://www.ccdi.gov.cn/djfg/fgsy/201312/t20131209_114257.html

[61]We include the age dummy interacted with the Bartik variable in (3) as a second instrument in these even-

export shocks induced greater emphasis by local officeholders on maintaining social stability, and especially so for the younger leaders, in line with the predictions of our model. This holds across the three different measures of "weiwen" emphasis. As we have demeaned the age dummy before using it in the regression, the point estimate for the main effect term in $ExpShock_{it}$ implies that for the typical incumbent, a one standard deviation increase in the severity of the export shock ($\approx$ 841 USD per worker) would raise the MNB "weiwen" score by about 21% (Column 4); this is driven by the response of prefecture party secretaries under the age of 57, as there is a $0.3133 \times 0.841 \approx 26\%$ larger increase in "weiwen" emphasis for these younger leaders relative to those within three years of retirement age.[62]

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$_{it}$	-0.0032	-0.0049*	-0.1988	-0.2463*	-0.2681	-0.3260**
	(0.0034)	(0.0027)	(0.1393)	(0.1264)	(0.1684)	(0.1397)
(Age$\leq$57)$_{i,t+1}\times$ExpShock$_{it}$		-0.0084**		-0.3133***		-0.3504***
		(0.0036)		(0.0889)		(0.1101)
(Age$\leq$57)$_{i,t+1}$		0.0248**		-0.0310		0.2764
		(0.0095)		(0.1350)		(0.5967)
Province-year dummies?	Y	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	Y	Y
Incumbent controls?	Y	Y	Y	Y	Y	Y
Prefecture-tier-by-year dummies?	Y	Y	Y	Y	Y	Y
First-stage F-stat	54.95	14.27	54.95	14.27	54.95	14.27
Observations	801	801	801	801	801	801
R^2	0.2483	0.2587	0.3082	0.2979	0.2817	0.2893

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); this is the change in "weiwen" keyword share (Columns 1-2), the change in log Multinomial Naive Bayes (MNB) score (Columns 3-4), and the change in log Support Vector Machine (SVM) score (Columns 5-6), respectively. (Age$\leq$57)$_{i,t+1}$ is a dummy variable (demeaned) for whether the incumbent prefecture party secretary is at age 57 or younger in year $t+1$. 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 1, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. The incumbent controls are those used in Table 6. The prefecture-tier-by-year dummies control for separate time trends respectively for ordinary and sub-province-level prefectures. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

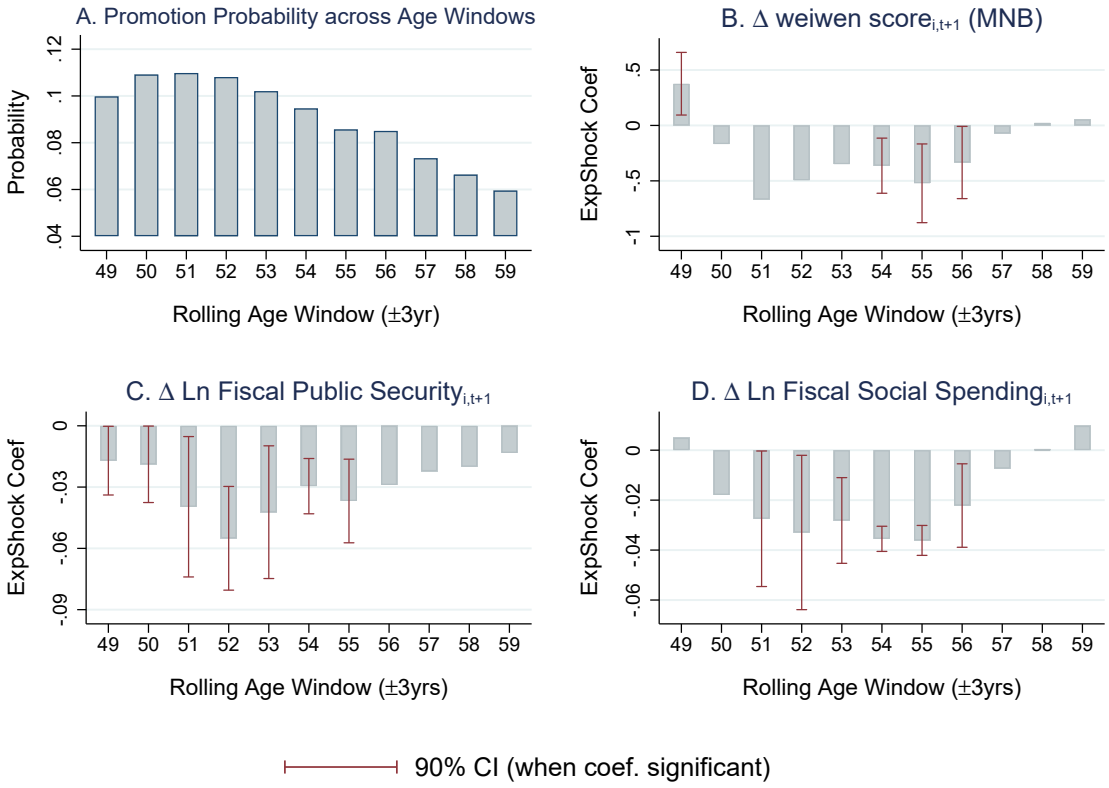
This age pattern is illustrated in Figure 7, specifically in Panel B, for the MNB measure.

numbered columns. Note that in Table 4, we have verified that this incumbent age dummy variable passes the requisite balance test, so that the export shock was not disproportionately bigger (or smaller) in prefectures with incumbents aged 57 or younger.

[62]Though the summary statistics in Table B.1 point to a decrease in average "weiwen" scores from 2015-2016, Table 7 confirms that in the residual variation after accounting for the controls (including province and prefecture-tier time trends), there is a significant negative relationship running from the export shock to an increased emphasis on "weiwen". In addition, we have performed the full set of checks described earlier in Section 4.2 to assess the robustness of the effect of the export shock on "weiwen" emphasis; these are reported for the MNB measure in Column 3 of the Appendix B tables.

We display here the coefficients on $ExpShock_{it}$ that we obtain when we run the specification (9) on rolling-window subsamples, each comprising those incumbents whose year-$(t+1)$ age was within a ±3-year band centered at the ages labeled on the horizontal axis. The negative effect of the export shock on "weiwen" emphasis emerges for the incumbent age windows in the early to mid-50s, but is notably more muted at the older end of the age spectrum.

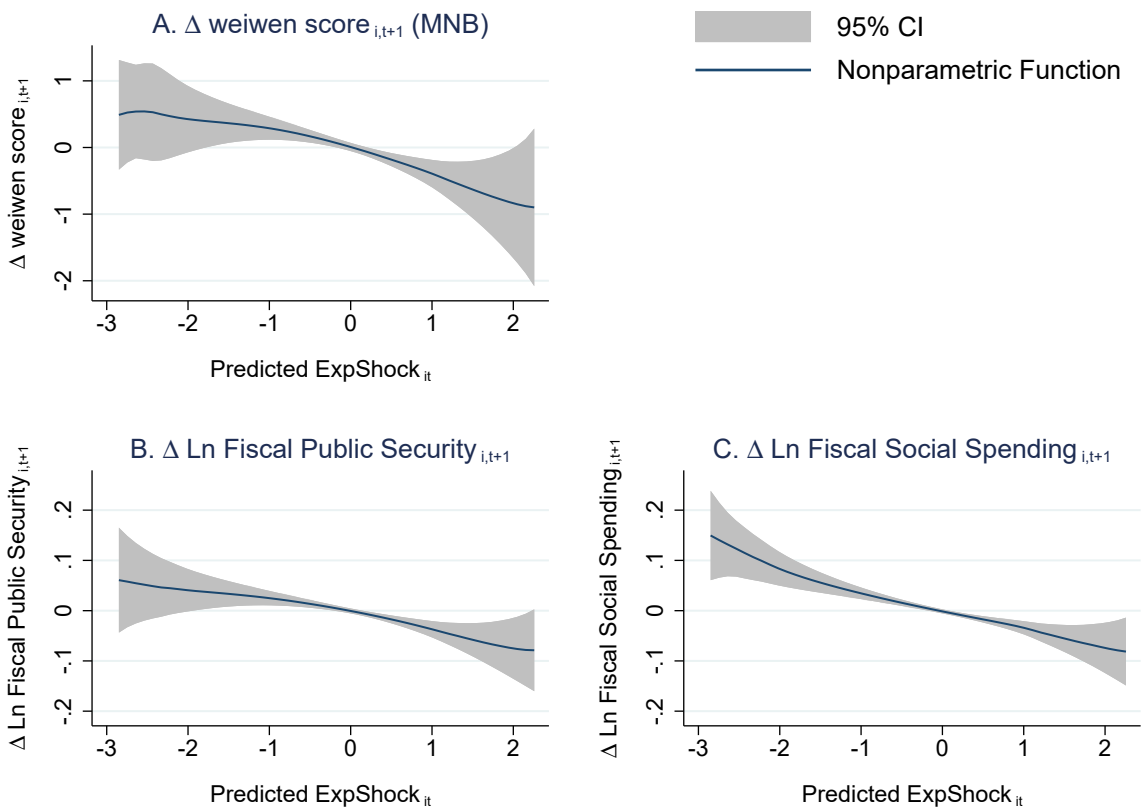
Figure 7: Heterogeneous Responses to Export Shocks by Incumbent Age



We shed light on one further implication of the model from Section 5, specifically whether the profile of stability measures with respect to the severity of the export shock is more consistent with a "sophisticated" versus "unsophisticated" upper-level decision-maker. Towards this end, Panel A of Figure 8 illustrates local polynomial plots of the relationship between the MNB "weiwen" score and the export shock IV, in a manner analogous to Figure 5. Recall that in the model, an "unsophisticated" upper-level government would induce little effort from all incumbents (including those who are high-capability) when the prefecture is subject to an extremely bad export shock, as any stability measures used by the incumbent would be insufficient to prevent his removal. The actual profile of stability measures we see in the data is instead very different: Incumbents who were exposed to very negative export shocks (as predicted by our instrument) on average display notably larger increases in their concern with stability, as captured by the MNB "weiwen" measure. Once again, this is suggestive that the

upper-level government is operating under a more "sophisticated" decision rule that conditions its evaluation of leaders on the information contained in the export shocks, so that the party secretaries most adversely affected by the shocks continue to be incentivized to adopt "weiwen" measures.

Figure 8: Prefecture Export Shocks and Stability Measures
(Local polynomial regression; Incumbents aged < 57)



6.3 Stability Measures: Fiscal Expenditure

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

Data and Specification: We collected data on realized fiscal expenditures and their detailed structure 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 had to be gathered from several sources, namely: Fiscal Statistical Yearbooks published by provincial Bureaus of Finance; Statistical

Yearbooks published by provincial Bureaus of Statistics; prefecture statistical yearbooks; as well as balance sheets from prefecture government websites (when necessary). In all, we were able to gather this 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 encompass 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. Second, we consider forms of expenditure – which we place under the label of "social spending" – that could in principle assuage citizens' discontent. These include: public services, education, social security, medical services, and public housing. The share of prefecture fiscal expenditure on public security averaged 5.1% during 2013-2016. By contrast, the average share on social spending was 54.2%, with its largest components being education (17.8%), social security (12.6%), and public services (10.1%).[63]

We follow the IV specification in (9) 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 (9).

Results: Panel A of Table 8 reports these estimates. In Column 1, we demonstrate that total spending on stability measures – the sum of public security and social spending – indeed rises in response to an export slowdown. This increase is 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 (≈841 USD per worker) would prompt a 2.0% increase in public security spending, which is slightly larger than the corresponding 1.7% increase in social spending.[64] In Table B.10 in the appendix, we break down social spending further into several sub-components; the findings here show that an export slowdown prompts increases specifically on education (which is the largest component of social spending) and medical services.[65] Coming back to Table 8, we find in Column 2 that the response of other forms of spending (i.e., all categories not related to public security or social spending) is less pronounced and not statistically significant. Similarly,

[63]Public security and social spending thus constituted around 60% of the total expenditures for the average prefecture. The main remaining expenditure items are arguably less relevant for mitigating labor unrest, namely: agriculture, forestry, and water conservancy; transport; and urban and rural community affairs.

[64]Our findings are 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. In the tables in Appendix B, specifically in Columns 4 and 5, 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 4.2. Separately, Table B.9 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.

[65]We do not explore the components of spending on public security, as the underlying data do not provide a breakdown across comparable sub-categories for a large enough number of prefectures.

across the full sample, neither total expenditures (Column 3) nor total revenues (Column 4) appear to respond to the export slowdown.

Table 8: Export Shocks and Prefecture Fiscal Measures

Dependent variable:	Δ **Log Fiscal measure**$_{i,t+1}$					
Fiscal measure:	**Stability Measures**	**Public Security**	**Social Spending**	**Other Spending**	**Total Expenditure**	**Total Revenue**
	(1)	(1a)	(1b)	(2)	(3)	(4)
	IV	IV	IV	IV	IV	IV
	Panel A: Average Effect					
ExpShock$_{it}$	-0.0208**	-0.0234**	-0.0208**	-0.0049	-0.0024	-0.0103
	(0.0083)	(0.0094)	(0.0085)	(0.0083)	(0.0046)	(0.0085)
First-stage F-stat	61.72	55.16	61.31	61.72	54.77	54.47
Observations	759	813	762	759	816	821
R^2	0.5999	0.6103	0.6084	0.7019	0.7118	0.7617
	Panel B: Differential Effect by Age Group					
ExpShock$_{it}$	-0.0238***	-0.0248***	-0.0241***	-0.0090	-0.0044	-0.0087
	(0.0056)	(0.0071)	(0.0055)	(0.0076)	(0.0052)	(0.0075)
(Age$\leq$57)$_{i,t+1}\times$ExpShock$_{it}$	-0.0221***	-0.0123*	-0.0244***	-0.0268***	-0.0143**	0.0086
	(0.0078)	(0.0066)	(0.0087)	(0.0080)	(0.0051)	(0.0063)
(Age$\leq$57)$_{i,t+1}$	-0.0235**	-0.0299*	-0.0222**	0.0006	-0.0097	-0.0202
	(0.0095)	(0.0157)	(0.0096)	(0.0173)	(0.0101)	(0.0123)
First-stage F-stat	20.00	14.42	19.95	20.00	14.36	14.28
Observations	759	813	762	759	816	821
R^2	0.6042	0.6123	0.6131	0.7012	0.7114	0.7668
Province-year dummies?	Y	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	Y	Y
Incumbent controls?	Y	Y	Y	Y	Y	Y
Prefecture-tier-by-year dummies?	Y	Y	Y	Y	Y	Y

Notes: The dependent variable is the change in log fiscal measure under the respective column headings in prefecture i between year t and $t+1$ (i.e., one year after the export shock). Panel A reports the average effects of the export shock on the respective fiscal measures, while Panel B reports the differential effects across prefectures by the incumbent party secretary's age group. (Age$\leq$57)$_{i,t+1}$ is a dummy variable (demeaned) for whether the incumbent prefecture party secretary is at age 57 or younger in year $t+1$. 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 1, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. The incumbent controls are those used in Table 6. The prefecture-tier-by-year dummies control for separate time trends respectively for ordinary and sub-province-level prefectures. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

Panel B shows, however, that the effects again differ according to the party secretary's age group, our proxy for the future rents that he/she anticipates from retaining office. Local officials under age 57 respond more strongly to negative export shocks in terms of both "sticks" (public security) and "carrots" (social spending). These younger leaders appear to adopt a fiscal policy stance that is more counter-cyclical with respect to the prefecture's export performance, as total expenditures rise during a slowdown without a corresponding increase in fiscal revenues (Columns 3-4). This heterogeneity by incumbent age is also evident in the rolling-window $ExpShock_{it}$ coefficients, illustrated in Panels C and D in Figure 7: Younger incumbents are more inclined to respond to an export slowdown by ramping up public security and social

spending, but the incentives appear to peter out as leaders approach retirement age.[66]

On a related note, the patterns of fiscal spending are once again more in line with the interpretation that local leaders are aware that they are being evaluated by a "sophisticated" central government. In Panels B and C in Figure 8, we use the same flexible, non-parametric approach to illustrate that there is a monotonic, downward-sloping relationship between increases in public security spending (respectively, social spending) and shifts in local export performance. Local officeholders in prefectures faced with the most extreme negative export shocks in fact responded with markedly larger increases in spending on stability measures. While one might have argued that the analogous pattern in Panel A based on "weiwen" emphasis could simply reflect rhetoric, Panels B and C indicate that this response was backed by the use of costly resources as well. The evidence is thus consistent with the case of a "sophisticated" upper-level government that takes into account the severity of the export slowdown when evaluating its local agents, justifying effort by local incumbents even under very negative shocks.

Taken together, the findings we have uncovered using the textual analysis "weiwen" scores and the fiscal spending data underscore the political importance that local incumbents attach to upholding stability when economic conditions deteriorate. The heterogeneity with respect to career incentives and the continued use of stability measures during extreme negative shocks, in particular, are consistent with their response being driven by the type of incentive and screening mechanism administered by a "sophisticated" central government as highlighted in our model. At the same time, we should acknowledge that the effects we have uncovered speak directly to the short-term responses that local governments enacted to the export slowdown. There is certainly much to be learnt about the medium- to long-run effects of these government actions and fiscal spending, particularly as more data becomes available on socioeconomic outcomes such as internal migration, job security, and even crime (Dix-Carneiro et al., 2018; Che et al., 2018).

7 Conclusion

In this paper, we documented how the slowdown in world trade in the years following the global financial crisis was associated with an increase in labor strikes in China. Using a prefecture-level panel dataset, and applying a shift-share instrumental variables strategy, we argued that our estimates reflect a causal impact of this export shock on public expressions of labor-related grievances.

[66]Table B.11 uncovers several other dimensions of heterogeneity in fiscal spending patterns. In response to a negative export shock, there is a greater increase in spending on stability measures in prefectures: (i) that experienced larger increases in labor events per capita in year t; and (ii) with a larger initial fiscal capacity, as captured by log fiscal revenues per worker in 2012. Interestingly, prefectures with larger initial fiscal capacity were more inclined to raise social spending rather than public security. These patterns should be viewed strictly as correlations though, since we do not propose a separate instrument for lagged increases in labor strikes nor for initial fiscal resources.

We presented further evidence on leaders' responses to this unfolding dynamic, by bringing together novel data on local political outcomes. Severe export slowdowns raised the likelihood of subsequent replacement for the prefecture party secretary, particularly when this was accompanied by larger increases in labor unrest relative to other prefectures that saw comparable export shocks. Local leaders appear to be cognizant of this threat of replacement, as declining exports led to a rising use of "weiwen" phrases in annual work reports, as well as increased expenditures on both public security and social spending. Such responses were notably more pronounced for younger local leaders whose career incentives would in principle be strongest. These patterns are aligned with the model of "political accountability with Chinese characteristics" that we developed, in which the central government makes strategic use of information about the severity of the export shock in order to screen and incentivize local leaders.

More broadly, the findings in this paper shed light on how economic shocks can impact political outcomes in autocratic regimes with high levels of state capacity, such as China: 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 headwinds is more relevant than ever.

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A Data Appendix (ONLINE ONLY)

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. We use the total number of labor dispute cases raised either collectively or by individuals; a very similar set of cross-check patterns is obtained when dropping the labor disputes raised by individuals (available on request).

Panel A in Figure A.1 demonstrates that at the national level, the total number of MOHRSS labor dispute cases and the total number of CLB-reported labor events are highly correlated over time. Panel B shows that this is true too when comparing annual changes in these two variables. Panel C in the same figure considers the same data series, but aggregated at the province level instead. We find here that over the 2013-2015 period, the annual changes in MOHRSS labor dispute cases and CLB strikes are positively correlated across provinces. (There is one observation for Ningxia that appears to be an outlier to the right, but removing this point would further strengthen the positive correlation.)

A.2 Night-Lights Data

The night-lights measures are constructed from the Visible Infrared Imaging Radiometer Suite Day/Night Band (VIIRS-DNB) dataset. This provides a monthly average of night-lights intensity in 15 arc-second geographic grids, corresponding to a physical distance of approximately 463 meters. The VIIRS-DNB dataset commences in April 2012, and is based on raw readings obtained and processed from the Suomi National Polar-orbiting Partnership (Suomi-NPP) satellite launched in 2011. The sensors onboard represent an advancement in night-time imaging capacity, that surpasses its predecessor – the Defense Meteorological Program Operational Line-Scan System (DMSP-OLS) – in radiometric accuracy, spatial resolution and geometric quality (Jing et al. 2015). Most existing studies use night-lights intensity data from the DMSP-OLS (see Henderson et al. 2012, for example), but this data is only available up till 2013. Unlike the DMSP-OLS, the data from the VIIRS-DNB is not top-coded. Using a cross-country panel dataset, Hu and Yao (2019) find a linear relation between night-lights intensity based on the VIIRS-DNB and real GDP per capita; by contrast, the relationship between night-lights intensity based on the DMSP-OLS and real GDP per capita exhibits concavity at the highest values of real GDP per capita, which reflects the top-coding in this older satellite data. With the VIRRS-DNB, we calculate the average night-lights intensity across observation grids that overlap with each Chinese prefecture's territory. VIRRS-DNB does not provide data for Northern

China during the summer time as a result of the stray light problem, and so we exclude observations across all prefectures from May to August. Lastly, we aggregate the prefecture-monthly data to the prefecture-annual level.

A.3 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.4 Classification of Incumbent Turnover

We classify each instance of incumbent turnover as a promotion, a lateral movement, or 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 ("Sheng Ji", 省级; or "Bu Ji", 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 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.

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 (at least to the extent observed by 2016). 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 we do not observe a subsequent move P_2.

In Column 1 of Table A.1, we regress this indicator variable (for whether P_2 was a promotion relative to P_1) on a set of categorical dummy variables reflecting the official's years of tenure in P_0 at the time of his/her lateral move to P_1. Specifically, the dummies we include reflect: (i) whether the years of tenure was equal to 3 (this being in principle the first year of "eligibility" for a promotion from P_0); and (ii) whether the years of tenure was greater than or equal to

4. The omitted category is thus whether the years of tenure was shorter than 3, this being the case of an early lateral movement. We include as controls dummy variables for a set of officeholder characteristics, namely: a full set of dummies for his/her age in the year of the lateral move from P_0 to P_1; gender (female); whether he/she held a master degree or higher; and whether the party secretary position P_0 was in a prefecture located in the same province as his/her birth (as a proxy for possible local political connections). The regressions also include year-of-turnover fixed effects (for the move from P_0 to P_1) and province fixed effects (for the party secretary position P_0). Table A.1 reports OLS regressions, with standard errors clustered by province; the results under probit regressions are similar and available on request.

Relative to the early lateral movers, i.e., those who were in position P_0 for fewer than three years, we find in Column 1 that the politicians who moved immediately after the three-year threshold was passed have on average a higher subsequent promotion probability. This is consistent with a career history of early lateral movements leaving a relative dent in an officeholder's future promotion prospects. In contrast, an immediate lateral movement upon the completion of the stipulated minimum service period of 3 years potentially signals that one's career trajectory is still on track for a possible future promotion.

The finding is further substantiated by the regressions reported in the remaining columns, where the dependent variables are dummies for whether: (i) the official was ever promoted in any moves including and subsequent to P_2 (Column 2); (ii) the highest rank he/she occupied was at the sub-provincial level or higher (Column 3); and (iii) the highest rank he/she occupied was at the provincial level or higher (Column 4). Column 2 confirms that early lateral movers (the omitted category) have lower future promotion prospects relative to lateral movers who had spent the required 3 years in their prior position, at least to the extent observable by 2016. Early lateral movers are also less likely to make it to positions higher up the political ranking: this effect is not precisely estimated for promotion to sub-provincial-level positions (Column 3), but is statistically significant at the 10% level for promotion to provincial-level positions (Column 4).

A.5 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 (from Google Translate, lightly edited) 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.6 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.[67] 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.[68] 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.[69]

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

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

[68] 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

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

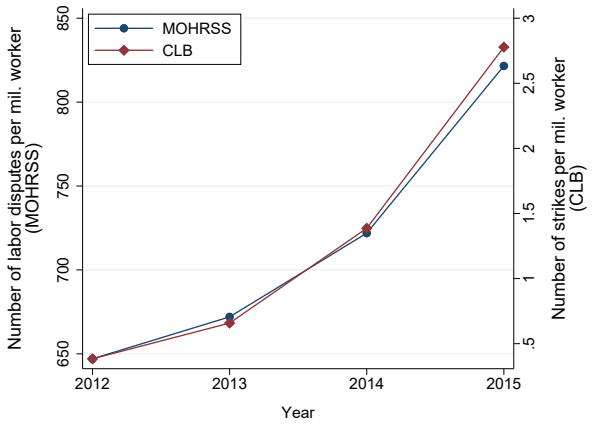
Strengthening Society's Public Security Prevention and Control System" (see Appendix A.5). For (i), we manually identified the sentences in each of the 20 reports that were on the topic of maintaining social stability ("weiwen"); for (ii), we classified the entire report as being about "weiwen". The 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 prediction accuracy rates 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.

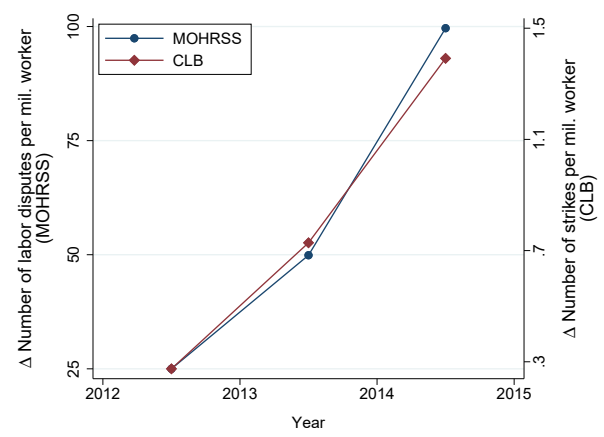
We subsequently applied these two models to the prefecture annual work reports in our sample period of interest (2012-2016). The "weiwen" score under each machine learning model for a given work report was computed by first calculating the "weiwen" score for each paragraph in the report, and then taking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

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

A. Number of Events (national)



B. Changes in Number of Events (national)



C. Changes in Number of Events (across provinces)

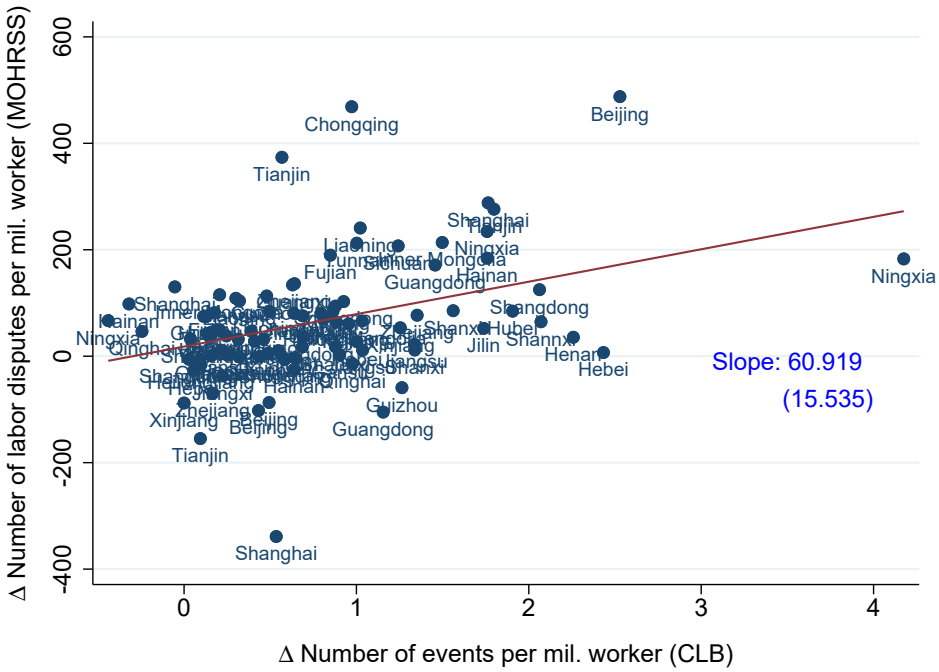


Table A.1: Future Promotion Probability of Lateral Movers

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	0.3149***	0.2937***	0.1525	0.1280*
	(0.0603)	(0.1004)	(0.1054)	(0.0714)
Tenure≥4	0.0925	0.0126	0.0210	0.0473
	(0.0573)	(0.0892)	(0.0833)	(0.0404)
Incumbent characteristics?	Y	Y	Y	Y
Year dummies?	Y	Y	Y	Y
Province dummies?	Y	Y	Y	Y
Observations	275	275	275	275
R^2	0.3139	0.2838	0.3595	0.2106

Notes: The sample comprises all prefecture party secretaries who recorded a lateral move during 2007-2012, with "Tenure" being the number of years that he/she had been in that position up to the time of the lateral move. The dependent variables are respectively indicator variables for whether: the official was promoted at his/her next career movement (Column 1); he/she was eventually promoted (Column 2); he/she was eventually promoted to a position of sub-provincial rank or higher (Column 3); he/she was eventually promoted to a position of provincial rank or higher (Column 4); as of the end of our sample in 2016. The incumbent characteristics included as controls are dummy variables for: age (in the lateral-move year); gender (female); whether he/she held a master degree or higher; and whether his/her party secretary position was 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.

Table A.2: Keywords Related to "preserving stability"

Chinese	English
维稳	a shorthand term for "preserving stability"
治安	law and order
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"

B Additional Empirical Results & Checks (ONLINE ONLY)

We present robustness checks in this appendix. The summary statistics for key variables – for each year in our sample, as well as averaged over the sample period – are reported in Table B.1.

B.1 Basic Specification Checks

To ensure that the baseline findings are not driven by the possible presence of influential observations, Figure 4 and Figure B.1 present residual binned scatter plots for the main specifications of interest.[70] Take Panel B in Figure 4 for example. 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) – the D_i and D_{pt} fixed effects, as well as the auxiliary prefecture controls X_{it} – but excluding $ExpShock_{it}$, while weighting the observations by $L_{i,2010}$, in order to extract an export shock residual. Prefecture-year observations are then grouped into 50 bins based on their residual export shock, with each bin representing around 2% of total working age population. The vertical axis variable is analogously constructed, with the residuals obtained from regressing the change in CLB events per million workers against all right-hand side variables in equation (2) – once again excluding $ExpShock_{it}$ – while weighting the observations by $L_{i,2010}$.

The residual binned scatter plots in Figure 4 (for the change in labor strikes) and Figure B.1 (for the political outcome variables) confirm the negative relationships detected vis-à-vis the export shock by the baseline regression analysis. It moreover provides reassurance that no single group of observations appears to be driving the negative slopes. We have verified that the downward-sloping relationship remains statistically significant when we drop the smallest and largest bins from each of these figures; the only exception is when lateral turnover is the dependent variable, where the slope is slightly less precisely estimated (with a slope of -0.51 and standard error of 0.043).

Table B.2 presents a set of specification 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) an indicator variable for party secretary lateral movement in time $(t+1)$; (iii) the time-$(t+1)$ change in the log Multinomial Naive Bayes (MNB) "weiwen" score; (iv) the time-$(t+1)$ change in log fiscal expenditure on public security; and (v) the time-$(t+1)$ change in log fiscal expenditure on social spending. The results reported in the Table B.2 columns should be compared against the baseline IV specifications reported

[70] Figure 4 is based on the IV specification in Column 3 in Table 1. Panels A-D in Figure B.1 follow the specifications in Column 2 in Table 6, Column 3 in Table 7, and Columns 1a and 1b of Panel A in Table 8, respectively.

in Column 3 in Table 1, Column 2 in Table 6, Column 4 in Table 7, and Columns 1a and 1b of Panel B in Table 8, respectively.

In Panel A, we drop the additional time-t control variables, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. The estimates here confirm that our main results are not sensitive to the use of these auxiliary controls. 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 adopts an alternative specification that drops the prefecture fixed effects (D_i), but includes instead the one-period lag of the dependent variable (i.e., $\Delta y_{i,t-1}$ if the dependent variable is Δy_{it}) to control for prefecture-specific pre-trends in the respective outcomes of interest. In the context of panel data settings, Angrist and Pischke (2009, Chapter 5.3) provide an argument that a specification that controls for the lagged dependent variable (but not fixed effects) and a specification that controls for fixed effects (but not the lagged dependent variable) would bracket the true magnitude of the effect of interest – in our case, $ExpShock_{it}$ – under some reasonable assumptions. In Column 1 of Panel C, we obtain an $ExpShock_{it}$ coefficient that is larger in magnitude than in our baseline regression in Table 1. This suggests that if the baseline prefecture fixed effects model in Table 1 is misspecified, it is nevertheless delivering us a conservative estimate of the size of the true effect of the export shock on an increase in labor strikes. More broadly too, looking across the remaining columns of Panel C, our findings for the political response variables remain largely robust under this alternative specification.[71]

Panel D repeats the analysis in Panel C, but replaces the lagged dependent variable by the one-period lag level of the outcome variable under consideration (i.e., $y_{i,t-1}$ if the dependent variable is Δy_{it}).[72] This specification accommodates the possibility of a tendency towards mean reversion in the outcome variables. The results we obtain here resemble those in Panel C.

Panel E runs regressions on a cross-section of observations from the year 2015 only; prefecture fixed effects are thus dropped, but province fixed effects are included. The relationships that we have uncovered remain stable, which implies that the baseline findings do not hinge on exploiting the within-prefecture variation over a short panel. That said, we should note that our main results are not simply driven by this single year, as we continue to obtain similar patterns when we run our baseline specification in (2) on the subsample that pools the observations from 2013 and 2014 (available on request).

[71]With a short panel, the regression model with both prefecture fixed effects and lagged dependent variable is subject to the concern of Nickell bias. For this consideration, we do not report the robustness checks based on this specification, although the results are qualitatively similar (available upon request).

[72]Note that when the outcome variable is the indicator variable for party secretary lateral movement, the regression result is omitted. This is because the turnover outcome is a flow variable analogous to Δy_{it}, and there is no corresponding empirical counterpart for the lagged outcome in level, i.e., $y_{i,t-1}$.

B.2 Validating the Bartik IV Strategy

Balance Tests: The Bartik IV can be formulated more generally as $\sum_k s_{ik} g_k$, where g_k denotes the export shock experienced by HS 6-digit 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: $g_k = \Delta X_k^{ROW} / \sum_i X_{ik,2010}$, and $s_{ik} = X_{ik,2010}/L_{i,2000}$. As discussed in Borusyak et al. (2020), the validity of the instrument relies on the assumption that $\sum_k s_k g_k \phi_k \xrightarrow{\text{p}} 0$, where: (i) $s_k = \sum_i e_i s_{ik}$ is the cross-prefecture weighted-average measure of exposure to product k, with e_i being the weights in the prefecture-level regression model; and (ii) $\phi_k = \sum_i (e_i s_{ik} \varepsilon_i) / \sum_i (e_i s_{ik})$ is an exposure-weighted expectation of untreated prefecture-level outcomes encapsulated in the residual term ε_i. In 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 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 manufactured in prefectures that were hit by other unobserved shocks that also affect social stability. (For clarity, we have dropped the time subscripts in this exposition; it is straightforward to generalize the framework to a panel setting, by relabeling i by $\tilde{i} = (i, t)$ and k by $\tilde{k} = (k, t)$.)

We follow Borusyak et al. (2020) 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 consider a set of various prefecture characteristics from 2010, namely: the share of workers with college education, manufacturing employment share, export-to-GDP ratio, share of population without hukou rights, log GDP per capita, party secretary age, an indicator for whether the party secretary is aged 57 years-old or younger, log fiscal revenue per capita; the data are drawn from the 2010 Population Census, the China City Statistical Yearbook, and prefecture-level statistical yearbooks. We also included a set of variables that capture the trends of the main outcomes of interest in the pre-slowdown period, namely: the change in the number of CLB-recorded strikes per worker, party secretary lateral movement, the change in the log Multinomial Naive Bayes (MNB) "weiwen" score, the change in log fiscal expenditure on public security, the change in log fiscal expenditure on social spending; we considered both changes from 2011-2012 and 2012-2013.

Panel A of Table 4 reports the balance test results. We regress each of the above weighted-average prefecture characteristics – the empirical counterpart of $\phi_k = \sum_i (e_i s_{ik} \varepsilon_i) / \sum_i (e_i s_{ik})$ – against g_{kt} 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 – in particular, the HS 6-digit product-level ROW export shocks – meets the requirements for treatment balance.

"Incomplete share": We turn next to address what is referred to as the "incomplete share" problem in Borusyak et al. (2020). Since a part of the variation in the Bartik-style

instrument stems from the manufacturing export exposure per worker at the start of the period, i.e., $\sum_k s_{ik} = \sum_k X_{ik,2010}/L_{i,2000} = X_{i,2010}/L_{i,2000}$, one may be concerned that prefecture-specific linear time trends in labor unrest (respectively, other outcome variables) could be correlated with the initial export exposure. Note first that the prefecture fixed effects (D_i) in equation (2) absorb the possible role of initial export exposure on the average level of $\Delta(Events/L)_{it}$, and thus help already to guard against this potential threat to identification. The primary remaining concern is the possibility that initial export exposure could be correlated with predetermined trends in the *growth* of labor unrest (respectively, other outcome variables). To mitigate this, we augment the baseline model by controlling for $X_{i,2010}/L_{i,2000}$ interacted with year dummies.

Panel A of Table B.3 reports the results of IV regressions with these additional controls.[73] Although we obtain qualitatively similar results, the estimates become much more imprecise. The first-stage Kleibergen-Paap F-statistics drop below the Stock-Yogo 10 percent threshold for weak instruments. While we do not report it in the table, we can confirm that the first-stage estimate of the coefficient of the IV in the Column 1 regression is 0.196 and this is statistically significant at the 5% level. This finding indicates that the additional controls soak up too much variation in the instrument, and hence hinder the identification of the effects of export shocks. Due to this consideration, in Panel C, we assess the robustness of the main findings based on the reduced-form regressions that directly relate the outcomes of interest to the instrument. Despite the reduction in the residual variation in $ExpShockROW_{it}$ with the additional controls, the reduced-form estimates should still remain consistent. This reduced-form analysis when controlling for $X_{i,2010}/L_{i,2000}$ interacted with year dummies yields a broadly similar set of coefficient estimates, with a slight loss of statistical significance.

Since $X_{i,2010}/L_{i,2000}$ tends to be relatively skewed, we have implemented a less parametric approach that is similar in spirit, in which we take indicator variables for the terciles of $X_{i,2010}/L_{i,2000}$ and interact each of these with year fixed effects instead. The results are reported in Panel B.[74] The first-stage Kleibergen-Paap F-statistics remain above the Stock-Yogo 10 percent threshold. Moreover, the estimates resemble closely the baseline findings. (The only exception is the result in Column 1 for labor strikes. The estimate is marginally insignificant with a p-value of 0.146.) For completeness, we also report the corresponding reduced-form results from this set of regressions in Panel D. Overall, given the stringent nature of the specification as it stands, the data do reasonably well when we seek to control for the possible role of the "incomplete share", particularly when we take a more flexible approach to control for

[73] In Columns 3-5, we further control for a three-way interaction of $(Age \leq 57)_{i,t+1}$, $X_{i,2010}/L_{i,2000}$, and year dummies (while controlling also for the double-interaction terms between $(Age \leq 57)_{i,t+1}$ and $X_{i,2010}/L_{i,2000}$, as well as between $(Age \leq 57)_{i,t+1}$ and the year dummies). Hence, the identification of the heterogeneous effects by age group also stems from the variation of $ExpShock_{it}$ that is independent of the initial export exposure.

[74] Analogously, in Columns 3-5, we further control for the triple interaction of $(Age \leq 57)_{i,t+1}$, the indicator variables for the terciles of $X_{i,2010}/L_{i,2000}$, and year dummies; we are careful to also control for the double-interaction terms between $(Age \leq 57)_{i,t+1}$ and the initial export exposure tercile dummies, as well as between $(Age \leq 57)_{i,t+1}$ and the year fixed effects.

the role of time trends in $X_{i,2010}/L_{i,2000}$ across its different terciles.

Further Pre-Trend Tests: To address the possibility that the results might be driven by pre-trends in the key variables, we examine in Table B.4 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 the baseline analysis, 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 much smaller in magnitude than in the baseline results and statistically indifferent from zero. 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.

Dropping HS Sections: 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.[75] 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. (2020) – 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.

Separately, we have checked that our findings remain robust if we were to drop HS 6-digit product codes that experienced particularly large shocks. In particular, we have reconstructed the Bartik IV variable after dropping HS 6-digit codes where the product-level shock was less than the 1st percentile or greater than the 99th percentile in any year in our sample. Our results are largely unaffected, and remain robust even if we were to expand the set of HS 6-digit codes dropped to all codes where the product-level shock was smaller than the 5th or larger than the 95th percentile in any year (results available on request).

[75]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.3 Alternative Clustered Standard Errors

As pointed out in Adão et al. (2019), 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) could be biased downward. We take two approaches to address the problem. First, in Panel B of Table 4, we estimate the product-level analogue of our baseline regressions; here, we cluster the standard errors at the two-digit HS level, which accommodates the possibility of unobserved correlated shocks across products within a broad sector. As is discussed in Borusyak et al (2020), this delivers a convenient implementation of the standard error adjustment to alleviate the concern in Adão et al. (2019).

As a second approach, 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, 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.6, 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 the 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. (2019), 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. (2019), since the number of products is larger than the number of prefectures (333) in our setting.)

B.4 Alternative Bartik Shocks

In this next set of checks reported in Table B.7, 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 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:

$$\sum_k \sum_{d \neq CHN} \frac{X_{idk,2010}}{\sum_i X_{idk,2010}} \frac{\Delta X_{dkt}^{ROW}}{L_{i,2000}}. \tag{B.1}$$

Here, ΔX_{dkt}^{ROW} 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 destination d 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:

$$\sum_k \sum_{d \neq CHN} \frac{X_{idk,2010}}{\sum_i X_{idk,2010}} \frac{\Delta \hat{X}^{ROW}_{dkt}}{L_{i,2000}}, \tag{B.3}$$

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:

$$\sum_k \frac{X_{ik,2010}}{\sum_i X_{ik,2010}} \frac{\Delta \hat{X}^{ROW}_{kt}}{L_{i,2000}}. \tag{B.4}$$

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).

B.5 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, so that the estimated export shock coefficient in our regressions may not be picking up the effects of shifts in 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 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:

$$AbsorptionShock_{it} = \sum_j \frac{L_{ij,2010}}{\sum_i L_{ij,2010}} \frac{\Delta Absorption_{jt}}{L_{i,2000}}.$$

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, $L_{i,2000}$ (from the 2000 Census). This is the proxy for domestic demand shocks at the prefecture level which we control for in Panel A of Table B.8. (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:

$$OutputShock_{it} = \sum_j \frac{L_{ij,2010}}{\sum_i L_{ij,2010}} \frac{\Delta Output_{jt}}{L_{i,2000}}.$$

We control for this proxy for domestic supply shocks in Panel B of Table B.8; 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. 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 scatterplots 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.

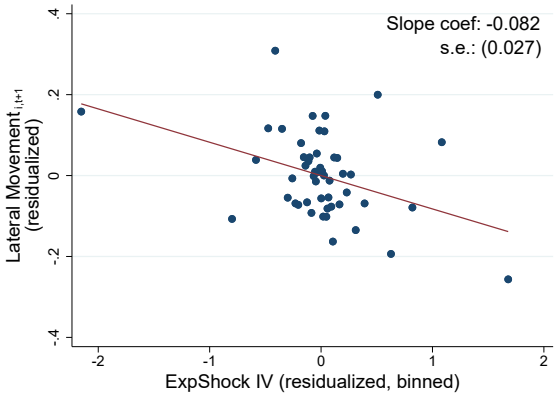
To assess the potential confounding effect of imports, we construct a Bartik-style measure of prefecture-level import shocks as:

$$ImpShock_{it} = \sum_j \frac{L_{ij,2010}}{\sum_i L_{ij,2010}} \frac{\Delta M_{jt}}{L_{i,2000}},$$

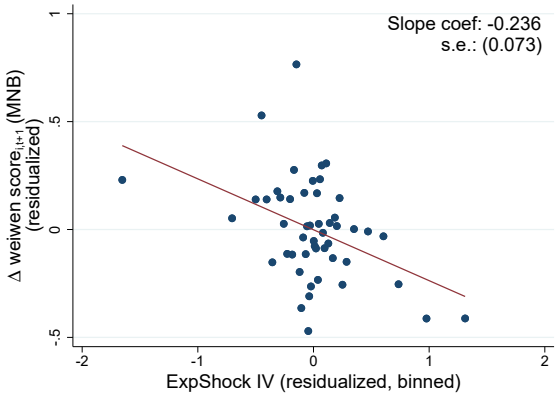
where ΔM_{jt} is the change in imports of industry j in year t, computed from the China customs data. While we are reasonably confident about the exogeneity of external demand shocks faced by Chinese exporters during our sample period, it is more challenging to propose exogenous import supply shocks to instrument for changes in imports at the prefecture level. With this caveat in mind, Panel D of Table B.8 presents a specification where we introduce the above $ImpShock_{it}$ variable. The estimated export shock coefficients resemble that from the baseline estimates, while the coefficients on the import shock are not statistically significant.

Figure B.1: Binned Scatter Plots: Prefecture Export Shocks, Turnover, and Incumbent Responses (50 bins)

A. Lateral turnover
(based on Column 2, Table 6)



B. Weiwen score, MNB
(based on Column 3, Table 7)



C. Public Security Spending
(based on Column 1a, Panel A, Table 8)

D. Social Spending
(based on Column 1b, Panel A, Table 8)

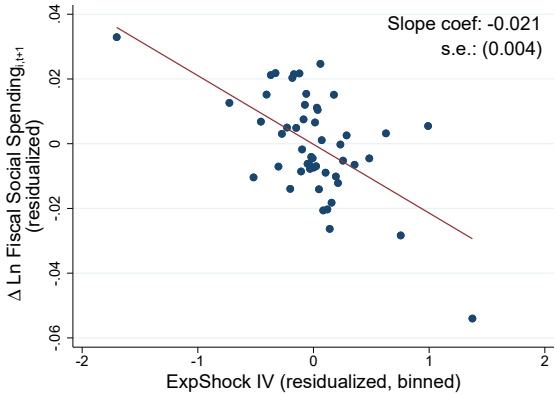


Figure B.2: Cross-Industry Correlation between Domestic Demand, Domestic Output and Export Shocks

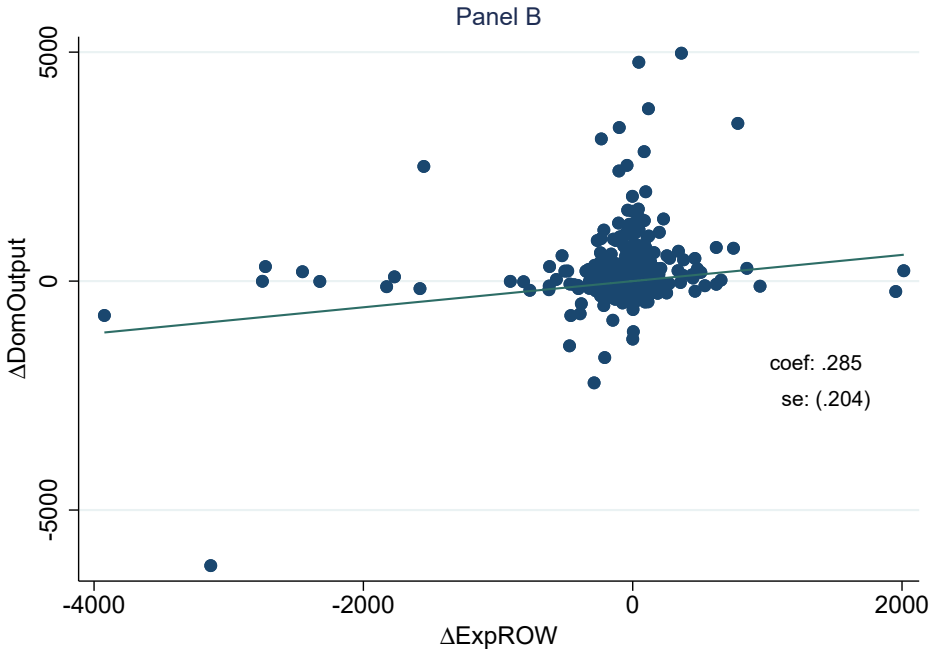


Table B.1: Summary Statistics

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
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	0.067	0.061	0.051
	(0.154)	(0.250)	(0.239)	(0.220)
Δ Log "weiwen" keyword occurrence	-0.008	0.008	-0.013	-0.004
	(0.045)	(0.051)	(0.046)	(0.048)
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	-0.092	-0.096	-0.282	-0.158
	(1.371)	(1.312)	(1.508)	(1.401)
Δ Log fiscal expenditure, Public security	0.051	0.113	0.128	0.097
	(0.083)	(0.110)	(0.118)	(0.110)
Δ Log fiscal expenditure, Social spending	0.078	0.133	0.081	0.098
	(0.074)	(0.080)	(0.074)	(0.080)
Δ Log fiscal expenditure, Total	0.077	0.136	0.066	0.093
	(0.066)	(0.114)	(0.075)	(0.093)

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 below. 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 party secretary turnover, "weiwen" textual analysis, and fiscal expenditure measures is described in Sections 6.1, 6.2, and 6.3 respectively.

Table B.2: Robustness: Basic Specification Checks

Dependent variable:	**Δ CLB Events per million**$_{it}$ (1) IV	**Party Sec. Lateral Turnover**$_{i,t+1}$ (2) IV	**Δ Log MNB "weiwen" score**$_{i,t+1}$ (3) IV	**Δ Log Fiscal Public Security**$_{i,t+1}$ (4) IV	**Δ Log Fiscal Social Spending**$_{i,t+1}$ (5) IV
	Panel A: Drop Additional Time-t Controls				
ExpShock$_{it}$	-0.1822**	-0.0768***	-0.2105	-0.0241***	-0.0248***
	(0.0739)	(0.0243)	(0.1351)	(0.0069)	(0.0052)
(Age≤57)$_{i,t+1}$×ExpShock$_{it}$			-0.3000***	-0.0108	-0.0237***
			(0.0969)	(0.0066)	(0.0082)
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Additional time-t controls?	N	N	N	N	N
Incumbent controls?	N	Y	Y	Y	Y
Prefecture-tier-by-year dummies?	N	N	Y	Y	Y
First-stage F-stat	64.32	71.38	9.88	9.61	12.56
Observations	987	974	919	956	905
R^2	0.5020	0.4899	0.2894	0.6036	0.5866
	Panel B: Unweighted Regressions				
ExpShock$_{it}$	-0.1744**	-0.0689***	-0.1277	-0.0198*	-0.0151***
	(0.0787)	(0.0189)	(0.1374)	(0.0110)	(0.0043)
(Age≤57)$_{i,t+1}$×ExpShock$_{it}$			-0.2150*	-0.0092	-0.0177*
			(0.1085)	(0.0091)	(0.0093)
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	Y
Incumbent controls?	N	Y	Y	Y	Y
Prefecture-tier-by-year dummies?	N	N	Y	Y	Y
First-stage F-stat	53.44	51.11	9.79	10.23	12.93
Observations	822	821	801	813	762
R^2	0.4481	0.5080	0.2936	0.5850	0.5534
	Panel C: Include One-Period Lagged Dependent Variable and Drop Prefecture FEs				
ExpShock$_{kt}$	-0.2494***	-0.0728***	-0.2071	-0.0189*	-0.0257***
	(0.0555)	(0.0227)	(0.1281)	(0.0093)	(0.0081)
(Age≤57)$_{i,t+1}$×ExpShock$_{it}$			-0.1928**	-0.0082	-0.0259***
			(0.0876)	(0.0049)	(0.0070)
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	N	N	N	N	N
Additional time-t controls?	Y	Y	Y	Y	Y
Incumbent controls?	N	Y	Y	Y	Y
Prefecture-tier-by-year dummies?	N	N	Y	Y	Y
First-stage F-stat	122.20	135.99	22.51	24.31	59.59
Observations	825	823	788	814	739
R^2	0.3522	0.2721	0.3490	0.5292	0.4594

Table B.2: Robustness: Basic Specification Checks (cont.)

Dependent variable:	**Δ CLB Events per million**$_{it}$ (1) IV	**Party Sec. Lateral Turnover**$_{i,t+1}$ (2) IV	**Δ Log MNB "weiwen" score**$_{i,t+1}$ (3) IV	**Δ Log Fiscal Public Security**$_{i,t+1}$ (4) IV	**Δ Log Fiscal Social Spending**$_{i,t+1}$ (5) IV
	Panel D: Include One-Period Lagged Outcome in Levels and Drop Prefecture FEs				
ExpShock$_{it}$	-0.2495***	—	-0.1877*	-0.0218**	-0.0248***
	(0.0824)		(0.0989)	(0.0090)	(0.0078)
(Age≤57)$_{i,t+1}$×ExpShock$_{it}$			-0.1513*	-0.0119**	-0.0237***
			(0.0747)	(0.0058)	(0.0079)
Province-year dummies?	Y	—	Y	Y	Y
Prefecture dummies?	N	—	N	N	N
Additional time-t controls?	Y	—	Y	Y	Y
Incumbent controls?	N	—	Y	Y	Y
Prefecture-tier-by-year dummies?	N	—	Y	Y	Y
First-stage F-stat	116.46	—	22.90	19.77	22.61
Observations	825	—	806	818	773
R^2	0.3297	—	0.4888	0.4968	0.4641
	Panel E: Cross-section (2015 only)				
ExpShock$_{it}$	-0.3456***	-0.0503**	-0.1702*	-0.0256***	-0.0208***
	(0.0925)	(0.0231)	(0.0882)	(0.0081)	(0.0053)
(Age≤57)$_{i,t+1}$×ExpShock$_{it}$			-0.1977***	-0.0200***	-0.0269***
			(0.0405)	(0.0067)	(0.0088)
Province dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	N	N	N	N	N
Additional time-t controls?	Y	Y	Y	Y	Y
Incumbent controls?	N	Y	Y	Y	Y
Prefecture-tier-by-year dummies?	N	N	Y	Y	Y
First-stage F-stat	177.65	128.77	22.71	22.79	23.73
Observations	277	277	275	275	266
R^2	0.2330	0.3729	0.1671	0.4529	0.4156

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; that in Column 2 is an indicator variable for whether the party secretary in i was laterally moved in year $t+1$; while that in Columns 3-5 is the change in the respective political response measure between year t and $t+1$. All regressions are weighted by the prefecture's working-age population in 2010, based on the specification in (2) for Column 1, (8) for Column 2, and (9) for Columns 3-5. (Age≤57)$_{i,t+1}$ is a dummy variable (demeaned) for whether the incumbent party secretary is at age 57 or younger in year $t+1$; the main effect of (Age≤57)$_{i,t+1}$ is included in Columns 3-5 though its coefficient is not reported. The additional time-t controls are those used in Column 3 of Table 1, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. Columns 2-5 further control for the incumbent characteristics used in Table 6, while Columns 3-5 also control for prefecture-tier-by-year dummies as in Tables 7 and 8. Panel A drops the additional time-t controls. Panel B runs unweighted regressions. Panel C includes the one-period lag of the dependent variable on the right-hand side (i.e., Δy_{t-1} if the dependent variable is Δy_t), while dropping the prefecture fixed effects. Panel D includes the one-period lag level of the prefecture outcome variable under consideration (i.e., y_{t-1} if the dependent variable is Δy_t), while dropping the prefecture fixed effects. Panel E reports a cross-sectional regression using data from 2015 only; province fixed effects are used in lieu of province-year and prefecture fixed effects. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

Table B.3: Robustness: Controlling for Time Varying Effects of the "Incomplete Share"
(IV specifications)

Dependent variable:	**Δ CLB Events per million**$_{it}$	**Party Sec. Lateral Turnover**$_{i,t+1}$	**Δ Log MNB "weiwen" score**$_{i,t+1}$	**Δ Log Fiscal Public Security**$_{i,t+1}$	**Δ Log Fiscal Social Spending**$_{i,t+1}$
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
	Panel A: Controlling for Initial Export Exposure × Year FEs				
ExpShock$_{it}$	-1.2946**	-0.1585	-1.0474*	-0.0134	-0.0040
	(0.5627)	(0.1907)	(0.5869)	(0.0184)	(0.0163)
(Age≤57)$_{i,t+1}$×ExpShock$_{it}$			-1.9762*	-0.1130**	-0.0368
			(1.0570)	(0.0438)	(0.0443)
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	Y
Incumbent controls?	N	Y	Y	Y	Y
Prefecture-tier-by-year dummies?	N	N	Y	Y	Y
First-stage F-stat	6.97	8.19	2.76	2.86	4.44
Observations	822	821	801	813	762
R^2	0.3093	0.4653	-0.0226	0.6131	0.6319
	Panel B: Controlling for Terciles of Initial Export Exposure × Year FEs				
ExpShock$_{it}$	-0.1016	-0.0695**	-0.2286*	-0.0250***	-0.0236***
	(0.0677)	(0.0332)	(0.1154)	(0.0069)	(0.0052)
(Age≤57)$_{i,t+1}$×ExpShock$_{it}$			-0.3327**	-0.0167**	-0.0261**
			(0.1250)	(0.0074)	(0.0100)
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	Y
Incumbent controls?	N	Y	Y	Y	Y
Prefecture-tier-by-year dummies?	N	N	Y	Y	Y
First-stage F-stat	108.45	123.78	14.82	15.08	21.15
Observations	822	821	801	813	762
R^2	0.5461	0.5229	0.3095	0.6253	0.6203

Table B.3: Robustness: Controlling for Time Varying Effects of the "Incomplete Share" (cont.)
(Reduced Form Specifications)

Dependent variable:	**Δ CLB Events per million$_{it}$**	**Party Sec. Lateral Turnover$_{i,t+1}$**	**Δ Log MNB "weiwen" score$_{i,t+1}$**	**Δ Log Fiscal Public Security$_{i,t+1}$**	**Δ Log Fiscal Social Spending$_{i,t+1}$**
	(1)	(2)	(3)	(4)	(5)
	RF	RF	RF	RF	RF
	Panel C: Controlling for Initial Export Exposure × Year FEs				
ExpShockROW$_{it}$	-0.2518***	-0.0321	-0.2879**	-0.0029	-0.0014
	(0.0792)	(0.0380)	(0.1380)	(0.0076)	(0.0063)
(Age≤57)$_{i,t+1}$×ExpShock$_{it}$			-0.3455*	-0.0413**	-0.0144
			(0.1983)	(0.0173)	(0.0142)
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	Y
Incumbent controls?	N	Y	Y	Y	Y
Prefecture-tier-by-year dummies?	N	N	Y	Y	Y
Observations	822	821	801	813	762
R^2	0.5461	0.5229	0.3095	0.6253	0.6203
	Panel D: Controlling for Terciles of Initial Export Exposure × Year FEs				
ExpShockROW$_{it}$	-0.0610	-0.0415*	-0.1533**	-0.0163**	-0.0163***
	(0.0442)	(0.0229)	(0.0627)	(0.0063)	(0.0056)
(Age≤57)$_{i,t+1}$×ExpShock$_{it}$			-0.2285***	-0.0117	-0.0196*
			(0.0588)	(0.0072)	(0.0101)
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	Y
Incumbent controls?	N	Y	Y	Y	Y
Prefecture-tier-by-year dummies?	N	N	Y	Y	Y
Observations	822	821	801	813	762
R^2	0.5228	0.5157	0.3336	0.6307	0.6288

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; that in Column 2 is an indicator variable for whether the party secretary in i was laterally moved in year $t+1$; while that in Columns 3-5 is the change in the respective political response measure between year t and $t+1$. All regressions are weighted by the prefecture's working-age population in 2010. (Age≤57)$_{i,t+1}$ is a dummy variable (demeaned) for whether the incumbent party secretary is at age 57 or younger in year $t+1$. Panels A and C control for initial export exposure per worker interacted with year fixed effects, while Panels B and D control for initial export exposure tercile dummies interacted with year fixed effects. Panels A and B are IV specifications, while Panels C and D are reduced-form regressions. Columns 3-5 further control for the interaction term of (Age≤57)$_{i,t+1}$ with year dummies, the interaction between (Age≤57)$_{i,t+1}$ and initial export exposure (whether in levels or tercile dummies), and a three-way interaction of (Age≤57)$_{i,t+1}$ with initial export exposure and year dummies. The additional time-t controls include the changes in the log college-enrolled, mobile-use, and internet-use shares. Columns 2-5 further control for the incumbent characteristics, while Columns 3-5 also control for prefecture-tier-by-year dummies. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

Table B.4: Effects of Future Export Shocks

Dependent variable:	**Δ CLB Events per million**$_{i,t}$	**Party Sec. Lateral Turnover**$_{i,t+1}$	**Δ Log MNB "weiwen" score**$_{i,t+1}$	**Δ Log Fiscal Public Security**$_{i,t+1}$	**Δ Log Fiscal Social Spending**$_{i,t+1}$
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
ExpShock$_{i,t+1}$	0.0613	-0.0197	0.0054	0.0039	-0.0002
	(0.1065)	(0.0295)	(0.0689)	(0.0050)	(0.0031)
(Age≤57)$_{i,t+1}$×ExpShock$_{i,t+1}$			0.0395	0.0092	-0.0025
			(0.1360)	(0.0068)	(0.0061)
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	Y
Incumbent controls?	N	Y	Y	Y	Y
Prefecture-tier-by-year dummies?	N	N	Y	Y	Y
First-stage F-stat	23.94	22.54	6.88	6.82	9.43
Observations	822	821	801	813	762
R^2	0.5176	0.4984	0.3047	0.6097	0.5789

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; that in Column 2 is an indicator variable for whether the party secretary in i was laterally moved in year $t+1$; while that in Columns 3-5 is the change in the respective political response measure between year t and $t+1$. All regressions are weighted by the prefecture's working-age population in 2010, based on the specification in (2) for Column 1, (8) for Column 2, and (9) for Columns 3-5. The dependent variable in each column is regressed against $ExpShock_{i,t+1}$ (as opposed to $ExpShock_{it}$), and instrumented for by $ExpShockROW_{i,t+1}$. (Age≤57)$_{i,t+1}$ is a dummy variable (demeaned) for whether the incumbent party secretary is at age 57 or younger in year $t+1$; the main effect of (Age≤57)$_{i,t+1}$ is included in Columns 3-5 though its coefficient is not reported. The additional time-t controls are those used in Column 3 of Table 1, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. Columns 2-5 further control for the incumbent characteristics used in Table 6, while Columns 3-5 also control for prefecture-tier-by-year dummies as in Tables 7 and 8. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

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

Dependent variable:	**Δ CLB Events per million**$_{it}$	**Party Sec. Lateral Turnover**$_{i,t+1}$	**Δ Log MNB "weiwen" score**$_{i,t+1}$	**Δ Log Fiscal Public Security**$_{i,t+1}$	**Δ Log Fiscal Social Spending**$_{i,t+1}$
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
Range of Estimates:					
Min ExpShock$_{it}$ coef.	-0.3908***	-0.1594**	-0.5220*	-0.0422***	-0.0375***
	(0.1288)	(0.0618)	(0.3038)	(0.0179)	(0.0131)
Max ExpShock$_{it}$ coef.	-0.1615**	-0.0890***	-0.1500**	-0.0240***	-0.0237***
	(0.0724)	(0.0296)	(0.0756)	(0.0071)	(0.0054)
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	Y
Incumbent controls?	N	Y	Y	Y	Y
(Age≤57)$_{i,t+1}$, (Age≤57)$_{i,t+1}$×ExpShock$_{it}$?	N	N	Y	Y	Y
Prefecture-tier-by-year dummies?	N	N	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; that in Column 2 is an indicator variable for whether the party secretary in i was laterally moved in year $t+1$; while that in Columns 3-5 is the change in the respective political response measure between year t and $t+1$. All regressions are weighted by the prefecture's working-age population in 2010, based on the specification in (2) for Column 1, (8) for Column 2, and (9) for Columns 3-5. For each column, 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 are reported with the associated standard errors. Columns 3-5 include (Age≤57)$_{i,t+1}$, a dummy variable (demeaned) for whether the incumbent party secretary is at age 57 or younger in year $t+1$, as well as its interaction with $ExpShock_{it}$; the $ExpShock_{it}$ coefficients reported in these columns are thus the effects of the export shock evaluated at the mean value of (Age≤57)$_{i,t+1}$. The additional time-t controls are those used in Column 3 of Table 1, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. Columns 2-5 further control for the incumbent characteristics used in Table 6, while Columns 3-5 also control for prefecture-tier-by-year dummies as in Tables 7 and 8. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

Table B.6: Robustness: Alternative Clustered Standard Errors

Dependent variable:	**Δ CLB Events per million**$_{it}$ (1) IV	**Party Sec. Lateral Turnover**$_{i,t+1}$ (2) IV	**Δ Log MNB "weiwen" score**$_{i,t+1}$ (3) IV	**Δ Log Fiscal Public Security**$_{i,t+1}$ (4) IV	**Δ Log Fiscal Social Spending**$_{i,t+1}$ (5) IV
ExpShock$_{it}$	-0.1728	-0.0907	-0.2463	-0.0248	-0.0241
Robust Standard Errors Clustered at:					
(i) province	(0.0746)**	(0.0314)***	(0.1264)*	(0.0071)***	(0.0055)***
(ii) export similarity	[0.0681]***	[0.0276]***	[0.0930]***	[0.0074]***	[0.0072]***
(iii) two-way clustering: (i) and (ii)	{0.0591}***	{0.0233} ***	{0.1060}***	{0.0059}***	{0.0063}***
(iv) export similarity: outside prov.	⟨0.0714⟩***	⟨0.0338⟩**	⟨0.1057⟩***	⟨0.0075⟩***	⟨0.0071⟩***
(v) two-way clustering: (i) and (iv)	⟦0.0557⟧***	⟦0.0227⟧***	⟦0.1063⟧**	⟦0.0074⟧***	⟦0.0061⟧***
(Age≤57)$_{i,t+1}$×ExpShock$_{it}$			-0.3133	-0.0123	-0.0244
Robust Standard Errors Clustered at:					
(i) province			(0.0889)***	(0.0066)*	(0.0087)***
(ii) export similarity			[0.0960]***	[0.0076]	[0.0042]***
(iii) two-way clustering: (i) and (ii)			{0.0978}***	{0.0070}*	{0.0067}***
(iv) export similarity: outside prov.			⟨0.0881⟩***	⟨0.0087⟩	⟨0.0049⟩***
(v) two-way clustering: (i) and (iv)			⟦0.0816⟧***	⟦0.0058⟧**	⟦0.0049⟧***
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	Y
Incumbent controls?	N	Y	Y	Y	Y
Prefecture-tier-by-year dummies?	N	N	Y	Y	Y
Observations	822	821	801	813	762
R-squared	0.5264	0.4987	0.2979	0.6123	0.6131

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; that in Column 2 is an indicator variable for whether the party secretary in i was laterally moved in year $t+1$; while that in Columns 3-5 is the change in the respective political response measure between year t and $t+1$. All regressions are weighted by the prefecture's working-age population in 2010, based on the specification in (2) for Column 1, (8) for Column 2, and (9) for Columns 3-5. (Age≤57)$_{i,t+1}$ is a dummy variable (demeaned) for whether the incumbent party secretary is at age 57 or younger in year $t+1$; the main effect of (Age≤57)$_{i,t+1}$ is included in Columns 3-5 though its coefficient is not reported. The additional time-t controls are those used in Column 3 of Table 1, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. Columns 2-5 further control for the incumbent characteristics used in Table 6, while Columns 3-5 also control for prefecture-tier-by-year dummies as in Tables 7 and 8. Robust standard errors are clustered as described in each respective row. *** p<0.01, ** p<0.05, * p<0.1.

Table B.7: Robustness: Alternative Bartik Measures

Dependent variable:	**Δ CLB Events per million**$_{it}$	**Party Sec. Lateral Turnover**$_{i,t+1}$	**Δ Log MNB "weiwen" score**$_{i,t+1}$	**Δ Log Fiscal Public Security**$_{i,t+1}$	**Δ Log Fiscal Social Spending**$_{i,t+1}$
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
	Panel A: Excluding Trade by Intermediary Firms				
ExpShock$_{it}$	-0.1620†	-0.0705*	-0.2720*	-0.0258***	-0.0233***
	(0.0996)	(0.0344)	(0.1487)	(0.0091)	(0.0070)
(Age≤57)$_{i,t+1}$×ExpShock$_{it}$			-0.3414***	-0.0165**	-0.0253**
			(0.1156)	(0.0069)	(0.0101)
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	Y
Incumbent controls?	N	Y	Y	Y	Y
Prefecture-tier-by-year dummies?	N	N	Y	Y	Y
First-stage F-stat	178.59	123.33	12.13	12.22	22.59
Observations	822	821	801	813	762
R^2	0.5220	0.5129	0.2960	0.6099	0.6189
	Panel B: Destination-specific Demand Shocks				
ExpShock$_{it}$	-0.1630***	-0.1004***	-0.1591*	-0.0236**	-0.0237***
	(0.0536)	(0.0318)	(0.0808)	(0.0084)	(0.0075)
(Age≤57)$_{i,t+1}$×ExpShock$_{it}$			-0.2237***	-0.0231*	-0.0256***
			(0.0751)	(0.0112)	(0.0084)
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	Y
Incumbent controls?	N	Y	Y	Y	Y
Prefecture-tier-by-year dummies?	N	N	Y	Y	Y
First-stage F-stat	35.46	40.63	10.68	10.74	12.22
Observations	822	821	801	813	762
R^2	0.5265	0.4937	0.3154	0.6108	0.6134

Table B.7: Robustness: Alternative Bartik Measures (cont.)

Dependent variable:	**Δ CLB Events per million**$_{it}$	**Party Sec. Lateral Turnover**$_{i,t+1}$	**Δ Log MNB "weiwen" score**$_{i,t+1}$	**Δ Log Fiscal Public Security**$_{i,t+1}$	**Δ Log Fiscal Social Spending**$_{i,t+1}$
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
	Panel C: Gravity-based Instrument – Equation (B.3)				
ExpShock$_{it}$	-0.1609**	-0.0766†	-0.1864	-0.0229†	-0.0454***
	(0.0764)	(0.0503)	(0.1522)	(0.0146)	(0.0125)
(Age≤57)$_{i,t+1}$×ExpShock$_{it}$			-0.3142**	-0.0168†	-0.0403***
			(0.1153)	(0.0107)	(0.0089)
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	Y
Incumbent controls?	N	Y	Y	Y	Y
Prefecture-tier-by-year dummies?	N	N	Y	Y	Y
First-stage F-stat	79.76	94.33	16.77	17.11	23.34
Observations	822	821	801	813	762
R^2	0.5265	0.5042	0.3087	0.6140	0.5019
	Panel D: Gravity-based Instrument – Equation (B.4)				
ExpShock$_{it}$	-0.1404*	-0.0904*	-0.1419	-0.0247***	-0.0305***
	(0.0686)	(0.0473)	(0.1163)	(0.0070)	(0.0058)
(Age≤57)$_{i,t+1}$×ExpShock$_{it}$			-0.2416***	-0.0118*	-0.0307***
			(0.0828)	(0.0062)	(0.0084)
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	Y
Incumbent controls?	N	Y	Y	Y	Y
Prefecture-tier-by-year dummies?	N	N	Y	Y	Y
First-stage F-stat	298.71	190.43	74.81	75.31	75.11
Observations	822	821	801	813	762
R^2	0.5263	0.4988	0.3162	0.6125	0.5919

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; that in Column 2 is an indicator variable for whether the party secretary in i was laterally moved in year $t+1$; while that in Columns 3-5 is the change in the respective political response measure between year t and $t+1$. All regressions are weighted by the prefecture's working-age population in 2010, based on the specification in (2) for Column 1, (8) for Column 2, and (9) for Columns 3-5. The alternative Bartik IVs in each Panel are constructed as described in Section B.4. (Age≤57)$_{i,t+1}$ is a dummy variable (demeaned) for whether the incumbent party secretary is at age 57 or younger in year $t+1$; the main effect of (Age≤57)$_{i,t+1}$ is included in Columns 3-5 though its coefficient is not reported. The additional time-t controls are those used in Column 3 of Table 1, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. Columns 2-5 further control for the incumbent characteristics used in Table 6, while Columns 3-5 also control for prefecture-tier-by-year dummies as in Tables 7 and 8. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1, † p<0.15.

Table B.8: Robustness: Controlling for Other Prefecture-Level Shocks

Dependent variable:	**Δ CLB Events per million**$_{it}$	**Party Sec. Lateral Turnover**$_{i,t+1}$	**Δ Log MNB "weiwen" score**$_{i,t+1}$	**Δ Log Fiscal Public Security**$_{i,t+1}$	**Δ Log Fiscal Social Spending**$_{i,t+1}$
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
			Panel A: Domestic Absorption Shocks		
ExpShock$_{it}$	-0.1556†	-0.0906**	-0.2695*	-0.0258***	-0.0226***
	(0.0915)	(0.0380)	(0.1417)	(0.0072)	(0.0066)
(Age≤57)$_{i,t+1}$×ExpShock$_{it}$			-0.3161***	-0.0124*	-0.0245**
			(0.0975)	(0.0065)	(0.0091)
AbsorptionShock$_{it}$	-0.0696	-0.0005	0.1134	0.0049	-0.0081
	(0.1615)	(0.0613)	(0.1107)	(0.0097)	(0.0104)
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	Y
Incumbent controls?	N	Y	Y	Y	Y
Prefecture-tier-by-year dummies?	N	N	Y	Y	Y
First-stage F-stat	71.85	80.40	12.65	12.85	16.85
Observations	822	821	801	813	762
R^2	0.5268	0.4987	0.2937	0.6111	0.6170
			Panel B: Domestic Output Shocks		
ExpShock$_{it}$	-0.2024**	-0.0896***	-0.2344*	-0.0226***	-0.0185**
	(0.0905)	(0.0307)	(0.1212)	(0.0070)	(0.0070)
(Age≤57)$_{i,t+1}$×ExpShock$_{it}$			-0.3183***	-0.0132*	-0.0281***
			(0.0950)	(0.0070)	(0.0094)
OutputShock$_{it}$	0.0794	-0.0031	-0.0332	-0.0060	-0.0167**
	(0.1445)	(0.0526)	(0.1194)	(0.0118)	(0.0075)
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	Y
Incumbent controls?	N	Y	Y	Y	Y
Prefecture-tier-by-year dummies?	N	N	Y	Y	Y
First-stage F-stat	31.91	34.71	8.43	8.54	10.38
Observations	822	821	801	813	762
R^2	0.5262	0.4992	0.3003	0.6154	0.6251

Table B.8: Robustness: Controlling for Other Prefecture-Level Shocks (cont.)

Dependent variable:	**Δ CLB Events per million**$_{it}$	**Party Sec. Lateral Turnover**$_{i,t+1}$	**Δ Log MNB "weiwen" score**$_{i,t+1}$	**Δ Log Fiscal Public Security**$_{i,t+1}$	**Δ Log Fiscal Social Spending**$_{i,t+1}$
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
	Panel C: Domestic Absorption & Domestic Output Shocks				
ExpShock$_{it}$	-0.2347***	-0.0891***	-0.1650*	-0.0177**	-0.0157**
	(0.0691)	(0.0249)	(0.0881)	(0.0071)	(0.0064)
(Age≤57)$_{i,t+1}$×ExpShock$_{it}$			-0.4155**	-0.0201**	-0.0327**
			(0.1496)	(0.0088)	(0.0125)
AbsorptionShock$_{it}$	-0.5048**	0.0077	0.6206	0.0441*	0.0258
	(0.2074)	(0.1577)	(0.4974)	(0.0226)	(0.0226)
OutputShock$_{it}$	0.5022***	-0.0095	-0.5808	-0.0450*	-0.0397**
	(0.1606)	(0.1448)	(0.4912)	(0.0248)	(0.0180)
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	Y
Incumbent controls?	N	Y	Y	Y	Y
Prefecture-tier-by-year dummies?	N	N	Y	Y	Y
First-stage F-stat	27.43	30.94	7.40	7.48	8.65
Observations	822	821	801	813	762
R^2	0.5296	0.4994	0.3178	0.6243	0.6292
	Panel D: Import Shocks				
ExpShock$_{it}$	-0.1675**	-0.0915**	-0.2462*	-0.0248***	-0.0243***
	(0.0784)	(0.0345)	(0.1275)	(0.0076)	(0.0060)
(Age≤57)$_{i,t+1}$×ExpShock$_{it}$			-0.3106***	-0.0140*	-0.0266**
			(0.0969)	(0.0074)	(0.0110)
ImpShock$_{it}$	-0.1669	0.0246	-0.0455	0.0289	0.0265
	(0.3400)	(0.1100)	(0.2086)	(0.0271)	(0.0291)
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	Y
Incumbent controls?	N	Y	Y	Y	Y
Prefecture-tier-by-year dummies?	N	N	Y	Y	Y
First-stage F-stat	166.23	190.30	17.64	17.72	22.41
Observations	822	821	801	813	762
R^2	0.5267	0.4984	0.2981	0.6131	0.6134

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; that in Column 2 is an indicator variable for whether the party secretary in i was laterally moved in year $t+1$; while that in Columns 3-5 is the change in the respective political response measure between year t and $t+1$. All regressions are weighted by the prefecture's working-age population in 2010, based on the specification in (2) for Column 1, (8) for Column 2, and (9) for Columns 3-5. (Age≤57)$_{i,t+1}$ is a dummy variable (demeaned) for whether the incumbent party secretary is at age 57 or younger in year $t+1$; the main effect of (Age≤57)$_{i,t+1}$ is included in Columns 3-5 though its coefficient is not reported. The additional time-t controls are those used in Column 3 of Table 1, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. Columns 2-5 further control for the incumbent characteristics used in Table 6, while Columns 3-5 also control for prefecture-tier-by-year dummies as in Tables 7 and 8. Panels A and B control directly for a prefecture-level shift-share measure of shocks to domestic absorption and shocks to domestic output respectively, while Panel C includes both of these shocks simultaneously. Panel D controls for a prefecture-level shift-share measure of shocks to imports. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1, † p<0.15.

Table B.9: Export Shocks and Fiscal Expenditure Shares

Dependent variable:	Δ **Log (Share of Fiscal share)**$_{i,t+1}$			
Fiscal measure:	**Stability Measures**	**Public Security**	**Social Spending**	**Other Spending**
	(1)	(1a)	(1b)	(2)
	IV	IV	IV	IV
	Panel A: Average Effect			
ExpShock$_{it}$	-0.0184**	-0.0210**	-0.0184**	-0.0024
	(0.0080)	(0.0095)	(0.0083)	(0.0052)
First-stage F-stat	61.72	55.16	61.31	61.72
Observations	759	813	762	759
R^2	0.6616	0.6975	0.6436	0.6214
	Panel B: Differential Effect by Age Group			
ExpShock$_{it}$	-0.0193**	-0.0203**	-0.0197**	-0.0045
	(0.0073)	(0.0094)	(0.0073)	(0.0042)
(Age$\leq$57)$_{i,t+1}\times$ExpShock$_{it}$	-0.0073*	0.0022	-0.0099*	-0.0120***
	(0.0042)	(0.0066)	(0.0049)	(0.0043)
(Age$\leq$57)$_{i,t+1}$	-0.0115	-0.0201*	-0.0105	0.0125
	(0.0079)	(0.0104)	(0.0087)	(0.0101)
First-stage F-stat	20.00	14.42	19.95	20.00
Observations	759	813	762	759
R^2	0.6379	0.6956	0.6100	0.6379
Province-year dummies?	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y
Incumbent controls?	Y	Y	Y	Y
Prefecture-tier-by-year dummies?	Y	Y	Y	Y

Notes: The dependent variable is the change in log fiscal measure – expressed as a share of total fiscal expenditures – under the respective column headings in prefecture i between year t and $t+1$ (i.e., one year after the export shock). Panel A reports the average effects of the export shock on the respective fiscal measures, while Panel B reports the differential effects across prefectures by the incumbent party secretary's age group. (Age$\leq$57)$_{i,t+1}$ is a dummy variable (demeaned) for whether the incumbent prefecture party secretary is at age 57 or younger in year $t+1$. 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 1, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. The incumbent controls are those used in Table 6. The prefecture-tier-by-year dummies control for separate time trends respectively for ordinary and sub-province-level prefectures. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

Table B.10: 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$_{it}$	-0.0074	-0.0238**	0.0082	-0.0150***	-0.0442
	(0.0126)	(0.0089)	(0.0099)	(0.0047)	(0.0392)
First-stage F-stat	54.78	54.77	54.76	54.77	61.35
Observations	813	816	815	816	766
R^2	0.5479	0.6747	0.4517	0.6207	0.4111
	Panel B: Heterogeneous Effects				
ExpShock$_{it}$	-0.0075	-0.0264***	0.0040	-0.0147***	-0.0476
	(0.0123)	(0.0070)	(0.0104)	(0.0042)	(0.0363)
(Age$\leq$57)$_{i,t+1}\times$ExpShock$_{it}$	0.0013	-0.0221**	-0.0249	-0.0028	-0.0294
	(0.0101)	(0.0086)	(0.0169)	(0.0060)	(0.0310)
(Age$\leq$57)$_{i,t+1}$	0.0152	-0.0403**	0.0263	-0.0382*	-0.0605
	(0.0154)	(0.0148)	(0.0322)	(0.0202)	(0.0659)
First-stage F-stat	14.36	14.36	14.36	14.36	19.95
Observations	813	816	815	816	766
R^2	0.5484	0.6853	0.4611	0.6256	0.4122
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	Y
Incumbent controls?	Y	Y	Y	Y	Y
Prefecture-tier-by-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 (9). Panel A reports the average effects of the export shock on the respective fiscal measures, while Panel B reports the differential effects across prefectures by the incumbent party secretary's age group. (Age$\leq$57)$_{i,t+1}$ is a dummy variable (demeaned) for whether the incumbent prefecture party secretary is at age 57 or younger in year $t+1$. 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 1, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. The incumbent controls are those used in Table 6. The prefecture-tier-by-year dummies control for separate time trends respectively for ordinary and sub-province-level prefectures. 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 Party Secretary Responses: More Heterogeneous Effects

Dependent variable:	**Panel A: Δ Textual "weiwen" score$_{i,t+1}$**		
	Share of keywords	**Log MNB**	**Log SVM**
	(1)	(2)	(3)
	IV	IV	IV
ExpShock$_{it}$	-0.0019	-0.7277	0.2761
	(0.0188)	(0.6458)	(0.7164)
Δ(Events/L)$_{it}$×ExpShock$_{it}$	0.0014	-0.0469	0.0360
	(0.0060)	(0.0928)	(0.1014)
ln(FiscalRev/L)$_{i,2012}$×ExpShock$_{it}$	-0.0029	0.3560	-0.4291
	(0.0112)	(0.3567)	(0.4195)
(Age≤57)$_{i,t+1}$×ExpShock$_{it}$	-0.0079**	-0.3578***	-0.3244**
	(0.0031)	(0.1138)	(0.1501)
Province-year dummies?	Y	Y	Y
Prefecture dummies?	Y	Y	Y
Additional time-t controls?	Y	Y	Y
Incumbent controls?	Y	Y	Y
Prefecture-tier-by-year dummies?	Y	Y	Y
First-stage F-stat	6.33	6.33	6.33
Observations	801	801	801
R^2	0.2574	0.2680	0.3018
Dependent variable:	**Panel B: Δ Log Fiscal measure$_{i,t+1}$**		
Fiscal measure:	**Stability Measures**	**Public Security**	**Social Spending**
	(1)	(1a)	(1b)
	IV	IV	IV
ExpShock$_{it}$	0.0168	-0.0911***	0.0347*
	(0.0142)	(0.0282)	(0.0173)
Δ(Events/L)$_{it}$×ExpShock$_{it}$	-0.0140***	-0.0053	-0.0156***
	(0.0021)	(0.0033)	(0.0023)
ln(FiscalRev/L)$_{i,2012}$×ExpShock$_{it}$	-0.0195**	0.0484***	-0.0310***
	(0.0087)	(0.0168)	(0.0106)
(Age≤57)$_{i,t+1}$×ExpShock$_{it}$	-0.0174***	-0.0190**	-0.0179***
	(0.0042)	(0.0077)	(0.0040)
Province-year dummies?	Y	Y	Y
Prefecture dummies?	Y	Y	Y
Additional time-t controls?	Y	Y	Y
Incumbent controls?	Y	Y	Y
Prefecture-tier-by-year dummies?	Y	Y	Y
First-stage F-stat	7.11	6.32	7.22
Observations	759	813	762
R^2	0.6052	0.5781	0.6098

Notes: The dependent variable in Panel A is the change in the respective textual "weiwen" score in prefecture i between year t and $t+1$ (i.e., one year after the export shock), while that in Panel B is the change in the log fiscal expenditure under the respective column headings in prefecture i between year t and $t+1$. All columns report IV regressions, weighted by the prefecture's working-age population in 2010. (Age≤57)$_{it}$ is a dummy variable for whether the prefecture party secretary is at age 57 or younger in year $t+1$. The Δ(Events/L)$_{it}$ variable is the change in CLB-recorded events per million between year $t-1$ and t. ln(FiscalRev/L)$_{i,2012}$ is log of the local fiscal revenue per worker in 2012. The variables interacted with ExpShock$_{it}$ are each demeaned; the regressions include the level effects of these variables, even though the coefficients are not reported. The additional time-t controls are those used in Column 3 of Table 1, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. The incumbent controls are those used in Table 6. The prefecture-tier-by-year dummies control for separate time trends respectively for ordinary and sub-province-level prefectures. Robust strandard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

C Model Appendix (ONLINE ONLY)

C.1 Modeling the Relationship between Export Shocks and Strikes

We lay out a model below that rationalizes the key empirical relationship – between export shocks and worker strikes – that is the focus of the first half of our paper. The model builds on Campante and Chor (2012, 2014), specifically the setup they consider in which a representative worker decides on the optimal allocation of time/effort between: (i) production activities that yield labor income; and (ii) activities to curb the potential expropriation of that income. The model's predictions provide a justification for the regression specification we run in Section 4. At the same time, we show how we can map the model's predictions for the level of strikes to the stability function $h(x, m)$ adopted in Section 5 in the main paper, where we consider the upper-level government's decision problem.

Consider a representative worker, who has an endowment of labor which we will normalize to 1; one can think of this as the total amount of time that the worker possesses. This is to be divided between production uses l and strike activity s. The worker's objective is to maximize his/her expected income, subject to the time constraint $l + s = 1$.

The worker's full labor income is wl, where w is the prevailing wage per unit time that the worker takes as given. However, the worker receives only a fraction $1 - \tau(x, s) \leq 1$ of wl, as the manager of the factory in which he/she is employed expropriates a fraction $\tau(x, s) \in [0, 1]$ of it. Here, x denotes prevailing export conditions, with a lower value of x corresponding to a more adverse export shock. We will make the assumption that $\tau_x < 0$, so that the manager withholds a smaller fraction of the labor income due to workers when the local economy's export performance is strong. (For example, when x is high, there is a healthy demand for labor in the prefecture, and workers can credibly threaten to leave for an outside firm, constraining the manager's ability to expropriate labor income.)

On the other hand, workers can respond to the threat of expropriation by increasing their strike intensity s, in order to raise their retained share of labor income, $1 - \tau(x, s)$. We thus assume that: $\tau_s < 0$ and $\tau_{ss} \geq 0$; the latter condition allows for diminishing returns in the effectiveness of strikes. We further stipulate that $\tau_{sx} \geq 0$, so that strikes are (weakly) more effective as a means for workers to recover wages when the export shock is more adverse (i.e., when x is lower).

However, workers incur direct costs from participating in strikes, for example due to possible detention by the local authorities. We specify the pecuniary equivalent of this cost to be $c(s, m)$ units of labor, with $c_s > 0$, $c_{ss} > 0$ (i.e., increasing and convex in the intensity of strikes). Moreover, this direct cost to workers depends on the stringency of "weiwen" measures – denoted by m – that the local government has in place to bolster social stability; we make the natural assumptions here that $c_m > 0$ and $c_{ms} > 0$, so that both the total cost and the marginal cost of raising strike intensity are higher if the prefecture government is adopting tougher "weiwen" measures.

The worker's decision problem (to maximize expected income) can then be stated as follows:

$$\max_{l,s} \; (1-\tau(x,s))wl - c(s,m)w$$
$$\text{s.t.} \qquad l + s = 1.$$

Restricting our attention to interior solutions (i.e., $s \in (0,1)$), the first-order condition with respect to s is:

$$-\tau_s(1-s) - (1-\tau) - c_s \;\; = \;\; 0. \tag{C.1}$$

It is straightforward to check that the associated second-order condition is: $-\tau_{ss}(1-s) + 2\tau_s - c_{ss} < 0$. That this is negative follows from $\tau_{ss} \geq 0$, $\tau_s < 0$ and $c_{ss} > 0$, so the objective function is in fact globally concave in s; any interior solution, if it exists, will be a unique maximum. We thus have an implicit expression, from (C.1), for the strike intensity s^* that will be chosen by the representative worker in the prefecture in question.

To understand how s^* is affected by export conditions, we totally differentiate (C.1) to obtain:

$$\frac{ds^*}{dx} \;\; = \;\; \frac{\tau_{sx}(1-s^*) - \tau_x}{-\tau_{ss}(1-s^*) + 2\tau_s - c_{ss}}.$$

The denominator of this last expression is negative as a consequence of the second-order condition, while the numerator is positive since $\tau_{sx} \geq 0$ and $\tau_x < 0$. We thus have $\frac{ds^*}{dx} < 0$; intuitively, the share of wages withheld by the manager rises when export conditions are weak, and so it becomes relatively more important for workers to allocate effort towards strike activities to push back against this expropriation.

Taking stock, the above provides a justification for our empirical specifications exploring a relationship running from prefecture-level export shocks to workers' strike intensity. Through the lens of this framework, the prefecture fixed effects which we include in our baseline estimating equation – (2) in the main paper – serve to capture time-invariant local characteristics that might be correlated with the $\tau(x,s)$ and $c(s,m)$ functions. (For example, the overall tone of firm-employer relations might differ across locations for longstanding historical reasons, reflecting itself in cross-prefecture differences in the average level of τ_s – the marginal effectiveness of strikes in curbing expropriation of wages – over time.) Similarly, the vector X_{it} in (2) would control for possible time-varying prefecture characteristics that could shift the $\tau(x,s)$ and $c(s,m)$ functions. (For example, a more educated workforce, or higher mobile and internet penetration rates, might affect the ease with which strikes can be organized, thus shifting τ_s downward – i.e., making it more negative – for any given values of x and s.)

An additional implication from this simple framework is that a greater use of "weiwen" measures by the local government will discourage strikes. This can be seen by totally differentiating

(C.1) with respect to m, which yields:

$$\frac{ds^*}{dm} = \frac{c_{ms}}{-\tau_{ss}(1-s^*)+2\tau_s - c_{ss}}.$$

Since $c_{ms} > 0$, we have: $\frac{ds^*}{dm} < 0$; in particular, this comparative static is a meaningful one, to the extent that workers take the level of "weiwen" measures implemented by the local government as given when deciding upon their strike intensity. Note though that we do not control directly for the level of "weiwen" measures in our regressions explaining strike outcomes in Section 4, as we conceptually view m itself as an outcome variable decided upon by the local party secretary. As a matter of interpretation for our empirical results, the export shock coefficient that we estimate in Table 1 should thus be seen as capturing both a direct effect of the export slowdown on strikes, as well as an indirect effect arising from possible contemporaneous responses in the use of "weiwen" measures. Since any adverse export shock would tend to raise the use of stability-enhancing measures (based on the model in Section 5), this latter effect would tend to offset the direct effect of the export slowdown on workers' strike intensity, and so would handicap us against finding a negative and significant effect of the export shock on strike outcomes. It bears pointing out that in practice, what we find is that a negative export shock affects the use of "weiwen" measures with a one-year lag: $ExpShock_{it}$ is negatively correlated with the "weiwen" textual analysis measures and the log fiscal spending measures in year $t+1$, but the contemporaneous correlation within the same calendar year is not statistically significant (see the results in Table B.4). As argued in the main paper, prefecture work reports are typically released in the first months of a calendar year, and would contain policy announcements or shifts that are a reaction to conditions in the preceding year. Also, the year-$(t+1)$ fiscal measures are more likely to reflect the full set of spending responses enacted to cope with social instability arising from a year-t export shock.

As a final exercise, it is useful to consider how the solution to the worker strike decision problem can be connected to social stability at the prefecture level, and more specifically, to the $h(x,m)$ term in the upper-level government's objective function in equation (4) in Section 5 of the main paper. We illustrate this for concreteness (and convenience) with a particular functional form. Consider: $\tau(x,s) = (1-s)(1-a(x))$ and $c(s,m) = \frac{1}{2}f(m)s^2$. This choice of $\tau(x,s)$ and $c(s,m)$ satisfies the derivative properties $\tau_s < 0$, $\tau_x < 0$, $\tau_{sx} \geq 0$, $c_s > 0$, $c_{ss} > 0$ and $c_{sm} > 0$ so long as $a(x) \in [0,1]$, $a_x > 0$, $f(m) > 0$ and $f_m > 0$. The solution to (C.1) is then:

$$s^* = \begin{cases} \frac{1-2a(x)}{(1-2a(x))+(f(m)+1)} & \text{if } a(x) < \frac{1}{2}, \\ 0 & \text{if } a(x) \geq \frac{1}{2}. \end{cases}$$

We focus on the more interesting case where $a(x) < \frac{1}{2}$. We interpret s^* as an inverse measure of social stability, and so will illustrate how one can map $1 - s^* = \frac{f(m)+1}{(1-2a(x))+(f(m)+1)}$ to a stability function which has the properties required of $h(x,m)$ in (4), in particular: $h_m > 0$, $h_x > 0$, and

$h_{mx} < 0$. Consider:

$$\tilde{h}(x,m) \;\equiv\; x+1-s^* \;=\; x + \frac{f(m)+1}{(1-2a(x))+(f(m)+1)}.$$

Note that $\tilde{h}(x,m) \longrightarrow -\infty$ as $x \longrightarrow -\infty$, while $\tilde{h}(x,m) \longrightarrow +\infty$ as $x \longrightarrow +\infty$. Moreover, $\frac{d(1-s^*)}{dm} > 0$ (since $f_m > 0$), and $\frac{d(1-s^*)}{dx} > 0$ (since $a_x > 0$); it is then straightforward to see that $\tilde{h}(x,m)$ will inherit these derivative properties, i.e., $\tilde{h}_m > 0$ and $\tilde{h}_x > 0$. With regard to the cross-derivative, direct differentiation yields: $\tilde{h}_{mx} = \frac{f(m)+2a(x)}{((1-2a(x))+(f(m)+1))^3}(-2a_x f_m)$, which is negative so long as $f(m) > -2a(x)$. (Note that the denominator of $\tilde{h}_{mx}$ is positive since we are in the $a(x) < \frac{1}{2}$ case.) Intuitively, if "weiwen" measures inflict a sufficiently high cost on workers (i.e., $f(m)$ is sufficiently large), then "weiwen" measures would be particularly effective in promoting social stability when x is low and workers are inclined to increase their strike intensity, i.e., we have $\tilde{h}_{mx} < 0$. To be concrete, one can consider an $a(x)$ function such as $a(x) = \frac{1}{2}\frac{e^x}{1+e^x}$, which is increasing in x and which moreover is bounded with $0 < a(x) < \frac{1}{2}$ for all real values of x. It follows that this particular $a(x)$ satisfies $f(m) + 2a(x) > 0$, and yields a $\tilde{h}(x,m)$ function that meets the required conditions for the $h(x,m)$ term in the expression for social stability in equation (4). It bears repeating though that the above choice of functional form for $\tau(x,s)$ and $c(m,s)$ is meant to be purely illustrative, rather than invite a structural interpretation of what constitutes social stability.

C.2 Political Response to Export Shocks: Details

In this appendix section, we fill in a key detail related to the solution of the model from Section 5 of the main paper. Specifically, we prove that for an upper-level government whose objective is to maximize expected stability, i.e., $E(y) = ph(x,m_G) + (1-p)h(x,m_B)$, the optimal decision rule given the export shock x takes the form of a single threshold, with the local leader being retained if and only if the observed y exceeds a cutoff $\bar{y}(x)$.

Recall that the stability function is: $y = h(x,m) + \varepsilon$. As ε is an iid $N(0,\sigma^2)$ stochastic term, the realized values of y span the real line. We thus consider decision rules that partition the real line into measurable subsets, and that specify one course of action ("retain" or "replace") that would apply to all stability values that fall within each respective subset. Let y_i where $i \in \{\ldots,-1,0,1,\ldots\}$ denote the sequence of points on the real line that partition out these subsets, such that y_i is increasing in i and adjacent intervals on the real line are associated with different courses of action; in other words, if $(y_j, y_{j+1}]$ is an interval of stability values where the upper-level government decides to retain the local incumbent (where j is an integer), then $(y_{j-1}, y_j]$ and $(y_{j+1}, y_{j+2}]$ are intervals in which the local incumbent will be replaced. Note that the y_i's are each in principle functions of the observed export shock x, but we have suppressed this in the notation. We adopt the convention that if there are only a countably finite number I of cutoffs with index $i < 0$, then $y_{-I-1}, y_{-I-2}, \ldots = -\infty$; likewise, if there are only a countably finite number I of cutoffs with index $i > 0$, then $y_{I+1}, y_{I+2}, \ldots = \infty$. Without loss of generality,

we fix $(y_0, y_1]$ to be an interval in which the upper level government decides to retain the local incumbent.

The objective function of a local leader of type ℓ is thus to maximize:

$$\begin{aligned} & \left(\sum_{i=\ldots-2,0,2,\ldots} Pr(y_i < y < y_{i+1}) \right) R - g_\ell(m) \\ = & \left(\sum_{i=\ldots-2,0,2,\ldots} \Phi(y_{i+1} - h(x,m)) - \Phi(y_i - h(x,m)) \right) R - g_\ell(m), \end{aligned}$$

and the associated first-order condition is:

$$\left(\sum_{i=\ldots-2,0,2,\ldots} \phi(y_i - h(x,m)) - \phi(y_{i+1} - h(x,m)) \right) h_m(x,m)R \;=\; g'_\ell(m). \tag{C.2}$$

We show first that a type-B local incumbent would have no incentive to choose a positive level of stability-enhancing measures. Recall that $g'_B(m) = a_B + \delta m$ and $a_B > R\overline{h}/\sqrt{2\pi\sigma^2}$, where $\overline{h}$ is the upper bound of $h_m(x,m)$. The type-B leader's marginal cost of effort thus always exceeds $R\overline{h}/\sqrt{2\pi\sigma^2}$. We in turn show that the marginal benefit on the left-hand side of (C.2) is always smaller than $R\overline{h}/\sqrt{2\pi\sigma^2}$. Note that given x and m, there exists an integer j such that $y_j - h(x,m) \leq 0 < y_{j+1} - h(x,m)$. We will lay out the proof for the case that j is even, but it should be clear that we have an analogous proof for the case where j is odd since the $\phi(\cdot)$ function is symmetric about 0. The term on the left-hand side of (C.2) that pre-multiplies $h_m(x,m)R$ can be written as:

$$\begin{aligned} & \left(\sum_{k=1}^{\infty} \phi(y_{j-2k} - h(x,m)) - \phi(y_{j-2k+1} - h(x,m)) \right) + \phi(y_j - h(x,m)) \\ & + \left(\sum_{k=1}^{\infty} \phi(y_{j+2k} - h(x,m)) - \phi(y_{j+2k-1} - h(x,m)) \right) \end{aligned} \tag{C.3}$$

For each $k = 1, 2, \ldots$, we have $\phi(y_{j-2k} - h(x,m)) - \phi(y_{j-2k+1} - h(x,m)) \leq 0$, since $y_{j-2k} - h(x,m) \leq y_{j-2k+1} - h(x,m) \leq 0$; note that all these weak inequalities bind as equalities if and only if $y_{j-2k} = y_{j-2k+1} = -\infty$. Also, $\phi(y_{j+2k} - h(x,m)) \leq \phi(y_{j+2k-1} - h(x,m))$, since $y_{j+2k} - h(x,m) \geq y_{j+2k-1} - h(x,m) \geq 0$; once again, this all holds with equality if and only if $y_{j+2k} = y_{j+2k-1} = \infty$. (In particular, the proof as written up admits for the possibility that $(y_j, y_{j+1}] = (y_j, \infty]$ or $(y_j, y_{j+1}] = (-\infty, y_{j+1}]$.) It follows that the expression in (C.3) is less than or equal to $\phi(y_j - h(x,m))$, with equality holding if and only if j is the only cutoff. But $\phi(y_j - h(x,m))$ achieves a maximum value of $1/\sqrt{2\pi\sigma^2}$ precisely when $y_j - h(x,m) = 0$. Since $h_m(x,m)R \leq \overline{h}R$, it follows that the left-hand side of (C.2) is indeed not larger than $R\overline{h}/\sqrt{2\pi\sigma^2}$. Thus, (C.2) is never satisfied with equality for $\ell = B$, and we have a corner solution at $m^*_B = 0$.

The upper-level government's problem therefore boils down to enacting a decision rule to

elicit as high a level of m_G from G-type leaders as possible, since this is clearly what would maximize $E(y)$. The first-order condition for G-type leaders is given more specifically by:

$$\left(\sum_{i=\ldots-2,0,2,\ldots} \phi(y_i - h(x,m)) - \phi(y_{i+1} - h(x,m)) \right) R \;=\; \frac{\delta m}{h_m(x,m)}. \tag{C.4}$$

Bearing in mind that $h_{mm} \leq 0$, the right-hand side of (C.4) is increasing in m. The upper-level government will thus seek a decision rule that pushes up the value of the left-hand side of (C.4) as much as possible, so that the G-type incumbent will in turn set a high value of m_G. As we have seen in the preceding argument for the B-type incumbent, the left-hand side of (C.4) achieves a maximum value of $\phi(0)R = R/\sqrt{2\pi\sigma^2}$, with equality if and only if there is a unique cutoff level of stability y_j with $y_j - h(x,m) = 0$; the presence of any other cutoffs would lower the left-hand side of (C.4). It follows that the optimal decision rule features a unique cutoff $y_j = h(x, m_G)$, where the upper-level government replaces the local incumbent when stability y falls below y_j and retains him when y is larger than y_j.

Note that m_G is in turn determined by solving the G-type incumbent's first-order condition bearing in mind the nature of the threshold rule that the upper-level government will adopt. At $m = 0$, the marginal cost of enacting stability-enhancing measures is 0; so long as the marginal benefit for the G-type incumbent at $m = 0$ is positive, this implies that he will have an incentive to enact a positive level of stability-enhancing measures. On the other hand, as m tends to infinity, the marginal cost increases without bound, while the marginal benefit term is bounded above by $\overline{h}R/\sqrt{2\pi\sigma^2}$. This implies the existence of an interior solution to the G-type leader's first-order condition, which satisfies (6) in the main paper.