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 local officials in preserving social stability, and to screen them 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 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 often have repercussions for domestic political outcomes, and especially so when they adversely impact labor markets. How then do political leaders respond to the resulting citizen discontent and erosion in social stability that could 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 recent increase in the number of autocratic regimes around the world (Diamond 2015, Lührmann and Lindberg 2019; Hellmeier et al. 2021).

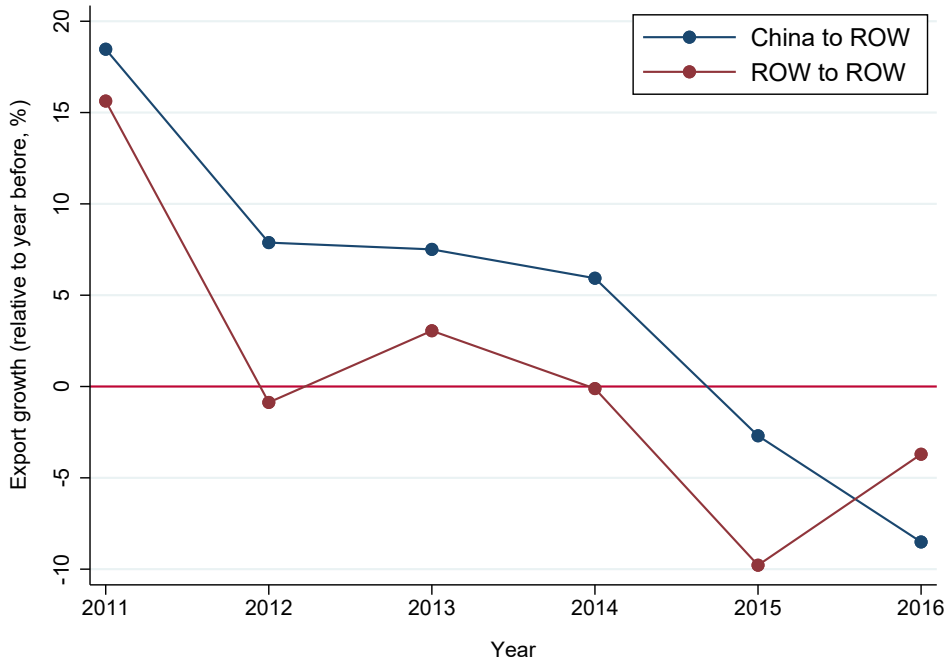
In this paper, we study the political impact of negative economic shocks in the context of a strong nondemocratic regime, using the setting afforded by the marked slowdown in China's export performance during the mid-2010s. While Chinese merchandize exports grew at a rapid average annual rate of 18% between 1992-2008 (Hanson 2012), this slowed considerably to a mere 0.6% between 2013-2016, even slipping into a contraction for two years (see Figure 1).[2] While these exports bounced back briefly in 2017, factors such as the onset of the US-China "tariff war" underscore the risk of this being an ongoing issue going forward. The slowdown 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.[3] Indeed, as export manufacturing orders declined in the mid-2010s, reports also emerged of a rise in layoffs and factory shutdowns, oftentimes setting

[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] 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.

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

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



off strikes and unrest over job losses and unpaid wage arrears.[4] This has led to concerns that the cumulation of such labor strikes could compromise domestic political stability.[5]

We make two contributions that shed light on the political economy consequences of China's export slowdown. First, we present formal evidence to link an adverse shock to exports to a rise in labor strikes, exploiting 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. To understand this fully, we argue that it is important to bear in mind the hierarchical nature of China's strong authoritarian regime: While the central government's position is not under imminent threat by localized strikes, it nevertheless holds local leaders closely to account for their performance in managing labor unrest in the face of these export headwinds, through the strategic use of decisions over their retention and replacement. This affects the career prospects for local incumbents, who respond accordingly by placing greater emphasis on measures to maintain social stability. To establish these points, we assemble systematic data at the local level on labor strikes and the political responses to these events,

[4]To give but one example, several hundred workers reportedly staged a peaceful march on 30 April and 1 May 2015 along the streets of Dongguan prefecture, a major manufacturing hub in Guangdong province, when the apparel factory where they were employed shuttered overnight and the factory manager became untraceable (see https://www.rfa.org/mandarin/yataibaodao/renquanfazhi/yf1-05012015100541.html). Dongguan was 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).

[5]This is aptly captured in the following quote from Eli Friedman, a Cornell University scholar on Chinese labor relations: "This is probably the thing that keeps Xi Jinping up at night. Governments are not swimming in money the way they used to be, and there's less room to compromise" (*New York Times*, 14 Mar 2016).

using novel data sources where necessary to help overcome the challenges posed by the tight state control of news and information within China.

Expanding on our substantive findings, 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 (a labor rights non-governmental organization), and build around this a prefecture-level panel dataset of annual observations from 2013-2016.[6] 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. As a result, there is variation in how inherently exposed each locality would be to product-level shocks in world trade flows, that largely reflect the weak and uneven recovery in external demand conditions in the aftermath of the global financial crisis (see for example the 2016 IMF World Economic Outlook).[7] 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 is driven by labor events where the reported cause was "wage arrears" and/or "layoffs", while also obtaining a consistent picture of the adverse impact of the export slowdown from other contemporaneous outcome variables, such as the manufacturing employment share, night lights intensity, and individual income.

We then turn to the implications of this labor unrest for the behavior and career prospects of local officials. At first glance, one might intuit that an increase in labor strikes would necessarily make a local leader more liable to replacement, given that the failure to uphold social stability has been widely seen as a veto criterion for career advancement within the Chinese political system (Edin 2003, Wang 2016). However, this logic has to be tempered against the need to properly incentivize local leaders – especially capable ones – to exert effort to curb social unrest, even in the face of plausibly exogenous economic shocks that are beyond their direct control. To think through this decision problem faced by the central government, we therefore develop a simple model of "political accountability with Chinese characteristics" that builds on Persson and Zhuravskaya (2016).

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 highlights how these predictions depend on whether the cen-

[6]Prefectures are a sub-provincial administrative unit. The analysis stops in 2016 due to the substantial change in the forces affecting China's exports following the start of the US-China "tariff war" (see Section 3.2 for further discussion).

[7]Our findings are robust under an extensive battery of checks, including recent recommendations advanced for validating the use of a Bartik IV (see Section 6). We show that the product-level trade shocks pass tests to verify that they are "as good as randomly assigned" to Chinese prefectures (Borusyak et al. 2022); 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).

tral government conditions the turnover decision on the observed severity of the local export shock ("sophisticated"), or instead adopts a uniform rule regardless of the extent of the local slowdown ("unsophisticated"). A "sophisticated" central government would not want to punish high-quality incumbents for export conditions that are outside their control. But precisely because the central government values social stability, it is able to use local leaders' performance in strike management amid the export slowdown to identify high-quality officials for retention and low-quality officials for replacement. This in turn enables Beijing to elicit effort from local leaders, specifically those of high-quality, to undertake costly measures to maintain social stability even when the export slowdown is very severe. This mechanism is in line with the accompanying political science scholarship, which has drawn attention to the role of both screening and incentives in China's political system (e.g., Heberer and Trappel 2013).

To confront the predictions on incumbent turnover with data, 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 adverse 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.

We argue that local leaders were replaced not just on account of a weak economic scorecard per se. Instead, these turnover decisions also appear to be tied to local officials' 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.

We then examine two sets of variables that speak to the predictions on local officials' use of stability-enhancing measures. First, we perform a textual analysis of prefecture work reports – an official speech delivered on an annual basis – 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 stress the importance of domestic law and order as a political priority. We construct measures 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 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 adverse economic shocks in China and, more broadly, in nondemocratic strong states. We find 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). A strong autocracy such as China is distinct, though, in the top-down nature of how political "accountability" is exercised: It is wielded by the central government, and so local officials need only worry about displeasing citizens insofar as that affects how their superiors evaluate them. We show that, in this context, the evaluation of lower-level officials takes on a more sophisticated guise than a simple veto criterion, let alone one based on "growth at all costs". Rather, the upper-level government seems to further require deft management of social tensions by local leaders, while itself being deft too in its evaluation of these officials: By taking into account the severity of exogenous economic shocks, they incentivize high-ability local officials to carry out the goals and objectives of the regime even in adverse circumstances.

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 then investigates the implications for incumbent turnover and on measures to bolster domestic stability. Section 6 reports on an extensive set of specification checks and robustness tests. 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, Dube and Vargas 2013, Bazzi and Blattman 2014, Sarsons 2015), coups (e.g., 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 (see also Heberer and Trappel 2013). This is consistent with the broader message painted in Martinez-Bravo et al. (2022) – of a central government that seeks local points of information to hold officials across its vast geography accountable – though their study focuses specifically on the role of local elections.

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 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 explore the effects of a negative shock to export opportunities.[11] We also contribute along these lines to work on how exposure to trade can affect political outcomes, such as legislative voting (Margalit 2011, Feingenbaum and Hall 2015), electoral voting (Jensen et al. 2017, Che et al. 2018, Iacoella et al. 2020, Dippel et al.

[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]This relationship between economic weakness and voting patterns can be affected by the presence of an independent media (Besley and Burgess 2002), by other local institutions (van der Brug et al. 2007), and by 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. For a broader survey article, see Lewis-Beck and Stegmaier (2018).

[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).

[11]See McCaig (2011) and Tian (2020) for exceptions in this regard; these explore respectively how Vietnam's and China's entry into export markets affected domestic economic outcomes.

2021, Ogeda et al. 2021), political polarization (Autor et al. 2020), and support for cross-border integration (Colantone and Stanig 2018).[12]

Third, our study contributes to the quantitative 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 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 performance in maintaining social stability can be a crucial determinant for their career prospects. In particular, we show that this is the case even holding local economic performance constant, underscoring the role of stability itself. In this regard, our paper connects to the vast political science literature on authoritarian controls in China (e.g., Edin 2003, Wang 2015, Chen et al. 2016, Mattingly 2019, Pan 2020), which has identified social stability as one of the veto criteria for career advancement. Our findings suggest though that the application of this criterion is more sophisticated in practice, in that it appears to draw a distinction between increases in social 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.[14] On a related note, Persson and Zhuravskaya (2016) and Chen and Kung (2016, 2018) have 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 evidence that economic shocks can induce a shift in fiscal resources towards uses aimed at maintaining social stability; this is similar to Wen (2020), who shows that state resources (specifically, employment in state-owned enterprises) are used to quell potential ethnic unrest.

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 5. 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

[12]On the flipside, there has also been work on the political impact of expanded export opportunities: Bustos and Morales-Arilla (2021) show reduced support for anti-trade political platforms in Mexico.

[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).

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

level of the province within China's administrative hierarchy. We include all prefectures across China, except Tibet due to data limitations.[15] 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.[16] 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 and without residency rights (hukou). By construction, $ExpShock_{it}$ measures the annual change in manufacturing exports in 1,000 USD per worker. Our regression analysis will be based on a panel that covers the period 2013-2016. We avoid the years prior to 2012; this is due both to data constraints, and the likelihood that any patterns could be confounded by the extraordinary stimulus measures that China adopted during the global financial crisis.[17] Our results continue to hold if we were to include 2017, though forces related to the US-China "tariff war" come into play shortly thereafter which preclude us from extending the sample further (see Section 3.2 below for additional discussion).

Figure 2 shows the distribution of $ExpShock_{it}$ across prefectures by year.[18] 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. There was moreover substantial variation in the severity of the export shock within any given year; in 2015, for example, the standard deviation of $ExpShock_{it}$ across

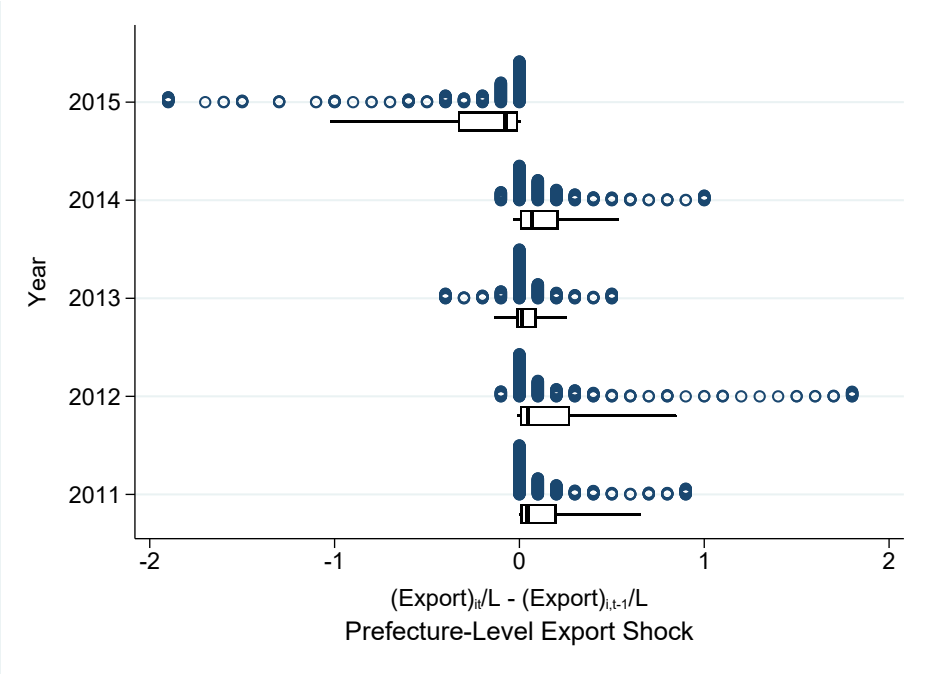
[15] We have constructed all our variables to be in accord with the 2010 administrative boundaries. With regard to Tibet, the China City Statistical Yearbooks contain only limited information on its prefecture-level characteristics; we were also not able to independently obtain fiscal data on its prefectures.

[16] 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. We set X_{fikt} equal to zero if firm-f, product-k exports are not observed in the customs data in year t.

[17] The CLB Strike Map data commence only in 2011, while we have had difficulty locating prefecture-level data on fiscal expenditures by uses for the global financial crisis years (specifically, for 2008-2010).

[18] 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)



prefectures was 948 USD per worker (see the summary statistics in Table C.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, or when examining exports by firm ownership types (e.g., privately-owned versus state-owned enterprises).

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 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 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 hosted on its CLB Strike Map, and the available data series commences in 2011. In the absence of official statistics, 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, and Giuntella and Wang 2019).[19] For the period of interest (up until end-2016), the

[19]The CLB Strike Map is at: https://maps.clb.org.hk/strikes/en. This data has been cited in the *Financial*

CLB 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.[20]

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 reports: (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).[21] 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 purely 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).[22]

Figure C.1 in the appendix maps out the distribution of CLB-recorded labor events across China. 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 C.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 this as best we can, we have compared the CLB data against official records on the number of labor dispute cases

Times (14 July 2016): https://www.ft.com/content/56afb47c-23fd-3bcd-a19f-bddab6a27883; *The Economist* (19 March 2016): https://www.economist.com/china/2016/03/19/deep-in-a-pit; and *The New York Times* (14 March 2016): https://www.nytimes.com/2016/03/15/world/asia/china-labor-strike-protest.html.

[20]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.

[21]While the CLB description of some events does contain short details on the industry involved (e.g., "a toy factory"), this is not systematically recorded for all entries. An estimate of the number of participants is also available, although these are reported in coarse bins (1-100, 101-1000, etc). Our baseline result from Table 1 – on the link between negative export shocks and labor strikes – continues to hold if we restrict the analysis to CLB events with more than 100 participants, i.e., larger labor events (available on request).

[22]Between 2012-2015, police involvement and arrests were reported in 1,415 cases, while mediation and negotiation (without any arrests) was the recorded outcome in 396 cases. We have refrained from using this dimension of the data in the regression analysis since it is reported for only about one-third of the 5,156 observations.

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 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.[23]

A more subtle concern is that the intensity of reporting on labor unrest could vary systematically with the extent of the economic shock 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.[24] 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.

[23]We have further confirmed that annual changes in the CLB strike data are positively correlated at the prefecture level with changes in counts of Sina Weibo posts with strike-related content drawn from Qin et al. (2017), once the strong tendency for mean reversion in the social media data is taken into account; see Appendix A.1 for details. Note though that the Qin et al. (2017) sample covers an earlier period, with just two years of overlap with the publicly-available CLB data (2011 and 2012).

[24]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.

Other Local Data: We put together a set of socioeconomic variables for use either as controls or as additional local outcomes to be explored; summary statistics for a selection of these variables are reported in Table C.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: gross industrial output per capita, the 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, namely night lights intensity from the VIIRS-DNB dataset, as well as with survey-based income data from the China Panel Family Studies (CPFS).

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:

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

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 defined in (1); both of these are constructed as changes between years $t-1$ and t.[25] The W_{it} term captures time-varying prefecture characteristics that are potential additional determinants of labor strikes.

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}'s capture any province-specific differences in changes in strike intensity over time. The D_i's in turn account for cross-prefecture differences in $\Delta(Events/L)_{it}$, or equivalently prefecture-specific linear time trends in $(Events/L)_{it}$; these help to control for possible pre-trends associated with prefecture characteristics, say if a greater presence of multinationals might be leading workers in a location to have more exposure to how labor relations are conducted abroad. Equation (2) is thus a relatively stringent specification – it

[25] 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).

is in effect a triple-difference specification in $(Events/L)_{it}$ – with the coefficient β_1 estimated off variation in export shifts across prefectures within provinces, as well as within prefectures over time. (We discuss some results later in Section 6 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; we will check that our key findings are not overly sensitive to these particular choices.

Though this is not a main focus of our exercise, we show in Appendix B that the regression specification in (2) can be rationalized from a simple model that builds on Campante and Chor (2012, 2014); see in particular Section B.6. 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 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 1,000 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 W_{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 approach would be to

establish 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. (2022) in Section 6.

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.[26] 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. Beyond our sample period, however, we should note that the shocks to external demand that beset the Chinese economy change substantially in their nature with the onset of the US-China "tariff war" in 2018. These discretionary tariffs were more targeted: for example, Ju et al. (2020) document that the early rounds of the Section 301 tariffs levied by the US disproportionately hit high-tech products from China, and so these would have impacted prefectures with a greater initial presence of such industries or of skilled workers.[27] We thus focus our analysis on the pre-tariff war years, for which our identification strategy is best suited, though it is worth noting that we have confirmed our findings when extending the sample up to 2017 (see Table C.11 in the appendix).

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 employment weights as in Autor et al. (2013), since information on employment at the level of detail of HS6 codes is not readily available. As shown in Appendix A.3, the use of 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 overstate the importance of export exposure in prefectures where a greater share of output tends to be absorbed domestically, such as in China's inland provinces.

While (3) serves as our baseline IV, we will explore alternative constructions to further

[26]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. The latter approach – 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.

[27]Moreover, Chinese workers may have reacted differently to the US-China trade war, as this could have given rise to nationalistic sentiment, with less blame directed toward local leaders for the hit to exports.

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 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 forces. A related concern 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.[28]

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), as well as corroborating evidence from other local economic and labor market outcomes (Section 4.2). We discuss robustness checks and validation exercises for the Bartik IV strategy, covering all of our empirical results, later in Section 6.

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 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).[29] 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.[30] 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 showing that individuals with higher levels of education have a greater

[28]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.

[29]The table reports the accompanying Kleibergen-Paap F-statistics; these are in excess of the Stock-Yogo rule-of-thumb threshold of 10 for weak instruments, confirming the relevance of the IV for $ExpShock_{it}$.

[30]Giuntella and Wang (2019) investigate the impact of automation on labor markets and strikes in China.

Table 1: Export Shocks and Labor Strikes

Dependent variable:	Δ **CLB Events per million**$_{it}$				
	(1) OLS	(2) IV	(3) IV	(4) IV	(5) OLS-RF
ExpShock$_{it}$	-0.1599***	-0.1822**	-0.1728**		
	(0.0346)	(0.0739)	(0.0746)		
ExpShock$_{i,t+1}$				0.0613	
				(0.1065)	
ExpShockROW$_{it}$					-0.1035**
					(0.0477)
ΔLog College-enrolled share$_{it}$			-0.0679	-0.0906	-0.0930
			(0.2199)	(0.2243)	(0.2249)
ΔLog Mobile share$_{it}$			0.8907	0.5401	0.4951
			(0.8239)	(0.9264)	(0.9744)
ΔLog Internet share$_{it}$			0.5258***	0.6198**	0.6325**
			(0.1840)	(0.2309)	(0.2310)
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Additional time-t controls?	N	N	Y	Y	Y
First-stage F-stat	–	64.32	105.56	23.94	–
Observations	987	987	822	822	822
R^2	0.5023	0.5020	0.5264	0.5176	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 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 5 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 3-5 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.

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 these media technologies and their potential role in facilitating the mobilization of workers (Campante et al. 2018, Manacorda and Tesei 2020, Fergusson and Molina 2021, Campante et al. 2022).[31] 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

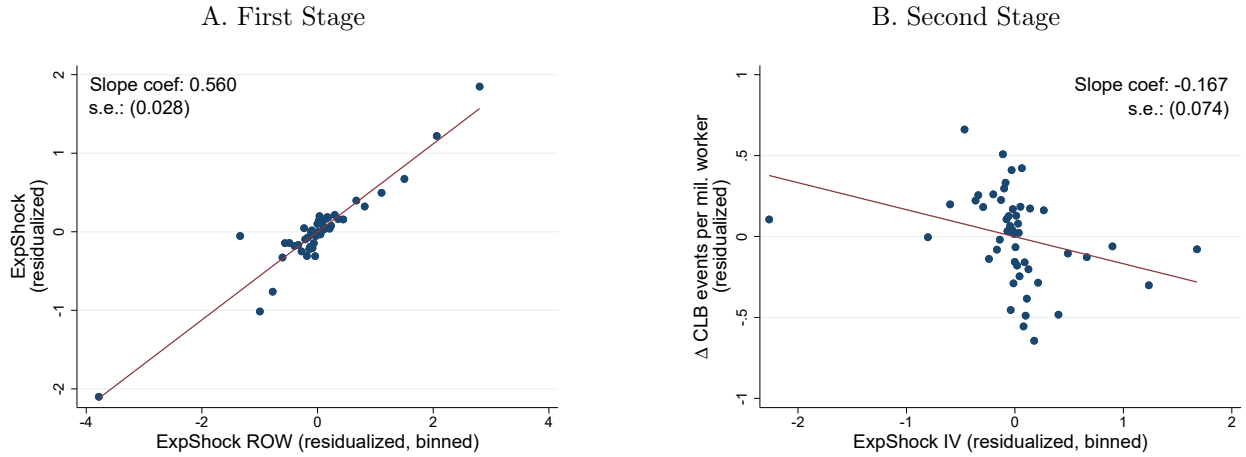
[31]These prefecture controls are each constructed as changes between year $t-1$ and t (i.e., contemporaneous with the dependent variable). The mobile and internet usage measures could also help to capture differences across prefectures in the likelihood that a given labor strike might be recorded by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

the political outcomes we later explore – do not hinge on whether we include these auxiliary prefecture controls; see Table C.10 in the appendix.) Column 4 presents a basic but important test related to the issue of pre-trends: We replace the contemporaneous export shock variable with $ExpShock_{i,t+1}$, and instrument for it with the time-$(t+1)$ Bartik variable, to see if this has explanatory power for the change in strike intensity at time t. The export shock coefficient we obtain is not statistically significant, allaying the concern that our results could reflect pre-determined trends in exports that might be co-moving with strikes. Lastly, reverting to the time-t export shock as the key explanatory variable, Column 5 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 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 sample). The $\beta_1 = -0.1728$ point estimate from Column 3 translates this into 0.15 more strike events per million workers, which is sizable considering that the median occurrence of strikes in our sample is 0.96 per million workers.

To help with visualizing these relationships, Figure 3 presents binned scatter plots of both the first- and second-stages from the IV specification in Column 3; the axes variables here are residualized to remove variation that can be attributed to the fixed effects and auxiliary prefecture controls, as detailed in Appendix C.5. 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 in this plot.

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



We dive into the 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. Table 2 follows the IV specification in Column 3 of Table 1, but uses 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 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 effect is much larger in the former.[32] 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.

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.

[32]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.

We briefly highlight several further results related to the impact on strikes; these are presented in more detail in Section C.1 in the appendix. First, we find evidence of spatial spillovers stemming from the export shock in neighboring prefectures. We should note though that the own-prefecture export shock remains negative and significant in explaining local labor strikes, even when we account for these spatial spillovers. Second, motivated by the anecdotes of factory closures, we have split $ExpShock_{it}$ into a component attributable to firm exit from exporting and a second component that captures changes in exports by continuing or new exporters. We find a slightly larger effect associated with the former (i.e., exit from exporting), though both components of $ExpShock_{it}$ are relevant for explaining the rise in strike intensity. Third, using an alternative breakdown of $ExpShock_{it}$, we find that the rise in strikes was driven by the export shock experienced by private-owned firms, which suggests that state-owned enterprises (SOEs) may have acted as a buffer for local employment that was being shed by these non-SOEs. Finally, using an interaction specification, we have uncovered several dimensions along which the effect of the export slowdown on strikes was heterogeneous. Quite intuitively, the impact on strikes was dampened where the local government: (i) was able to devote more fiscal resources to public security uses; and (ii) accounted for a larger share of employment. Conversely, the effect of $ExpShock_{it}$ was exacerbated in prefectures with a greater local population share with some college education; this is in line with prior work showing that a weak economy is more liable to trigger a rise in protest activity when the local populace is more highly educated (Campante and Chor 2014).[33]

4.2 Other Labor Market and Economic Outcomes

Our analysis to this point has focused on labor strikes. That said, if the export slowdown 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 3 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.[34]

The patterns in Table 3 are consistent with the broader narrative that the export shock had an adverse impact on local economic performance, particularly in the 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

[33]We also obtain a negative though statistically insignificant interaction effect between $ExpShock_{it}$ and the initial share of migrant workers. The negative sign is consistent with the interpretation that migrant workers without residential rights could be more prone to strike due to their limited access to social benefits.

[34]For each of the annual change outcome variables, we have dropped observations below the 1st percentile or above the 99th percentile, to reduce the influence of outliers on the regression estimates.

Table 3: 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 Individual Income**
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
ExpShock$_{it}$	0.0118**	0.0023**	0.0027	0.0237***	0.0612**
	(0.0054)	(0.0010)	(0.0123)	(0.0076)	(0.0237)
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y	N
Individual controls?	N	N	N	N	Y
First-stage F-stat	56.77	101.75	109.25	106.75	9.12
Observations	800	800	804	808	30,957
R^2	0.5341	0.5943	0.6257	0.8212	0.0387

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 in Columns 1-4, and as the change between year $t-2$ and t in Column 5; 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 in the prefecture-level regressions in Columns 1-4. The additional time-t controls in Column 1-4 are those used in Column 3 of Table 1, namely: the changes in the log college-enrolled, mobile-use, and internet-use shares. The individual controls in Column 5 include: six age group dummies (21-25, 26-30,31-35,36-40,41-45,46-50, and 51-55), five educational attainment group dummies (illiterate, primary school, middle school, high school, and college or above), a gender dummy, and an urban dummy. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

corresponding share of non-manufacturing employment (Column 2), this latter effect is much smaller in magnitude, 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 night lights intensity at the prefecture level (Column 4), as captured by the VIIRS-DNB satellite data, indicating that the export slowdown weighed negatively on economic activity measured via this broader proxy. Column 5 further explores individual-level panel data from the China Family Panel Studies (CFPS). For our purposes, we use the 2012, 2014 and 2016 waves of the survey, and relate individual income growth to the export shock constructed at the biennial frequency.[35] The result here reveals a slower growth in income induced by the export slowdown; this effect is more pronounced for low-skilled workers, as we show in Appendix C.2.[36]

[35]The change in exports per worker over two consecutive CFPS waves and the corresponding instrumental variable are constructed based on (1) and (3).

[36]The estimates in Table 3 imply effects that are fairly sizeable. A one-standard-deviation more severe contraction in exports ($\approx$ 841 USD per worker) would be associated with a 1.0 percentage point drop in the manufacturing share in prefecture population (Column 1), which is large when viewed against the in-sample standard deviation of 1.2 percentage points in the annual change in this 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), and a reduction in individual income of 5.1% (Column 5).

5 The Political Response to Export Shocks

Having established that the export slowdown prompted a rise in labor strikes, we turn now to the question of how the Chinese political system responded at the same time to this challenge to social stability. In particular, we are interested in how these developments affected prefecture party leaders and influenced their policy actions.

It is natural to expect that increased labor unrest would dent the career prospects of local leaders, given the importance of social stability as a key goal of the Chinese regime. In fact, social stability has been identified by numerous qualitative studies (e.g., Edin 2003, Wang 2016) as one of the veto targets upon which local officials are evaluated – that is to say, a target that weighs so heavily that, if it is missed, "all other achievements of the local leadership will be void" (Heberer and Trappel 2013).[37] Yet the very concern with social stability calls for properly incentivizing and retaining high-quality local leaders, so that they can effectively bolster it, even in the face of plausibly exogenous shocks that are beyond their direct control.

To help us think through how the Chinese government might deal with this challenge, we first sketch a simple conceptual framework. This will yield specific predictions to guide our empirical investigation into the effects on incumbent turnover and on local leaders' pursuit of stability-enhancing measures.

5.1 Conceptual Framework

We develop a simple model of "political accountability with Chinese characteristics," drawing on Persson and Zhuravskaya (2016), which captures features of China's administrative hierarchy and the career concerns of politicians within this system. While we leave the details and proofs of the model to Appendix B, it is worth going over its key features and intuition.

We consider a local (prefecture) incumbent who can engage in costly measures to bolster social stability when faced with an adverse export shock. Local incumbents differ in their competence in delivering social stability; for simplicity, we assume there are two categories of these local agents – high- versus low-quality types – where the former incur a lower marginal cost for enacting stability-enhancing measures. The performance of the local incumbent (henceforth "he") is in turn evaluated by an upper-level official in the central government (henceforth "she"). The upper-level official can either retain or sideline the local agent, and she exercises this decision with the objective of maximizing social stability, in line with the overriding importance placed on this goal as described above. (That said, in Appendix B.3, we show that the model

[37] In Chinese, the evaluation based on veto criteria is termed "一票否决"(*yipiaofoujue*). Based on interviews with government officials, Wang (2016) finds that, in general, four indicators are crucial for career advancement: the GDP growth rate, growth rate of per capita fiscal revenue, a social stability index, and the fertility rate; the latter two are seen as veto criteria. See also Pan (2020), which contains anecdotes of central government communication that confirm how failure to maintain social stability would translate into a veto on local leaders' career progression (p.44).

can be readily extended to allow for local economic performance or growth to also enter the upper-level government's objective function, without altering our key predictions.)

The information structure of the model is as follows: The upper-level official is able to observe the export shock affecting a prefecture, as she has knowledge of external conditions and the production structure of the local economy. She can also observe the level of social stability that is realized in the prefecture. This falls when export conditions worsen, but can be bolstered if the local leader undertakes costly measures. These encompass a broad range of potential actions, from channeling more resources to public security uses ("sticks"), to disbursing more social spending to quiet discontent ("carrots"). We assume that the effectiveness of these measures is subject to diminishing returns, and that such measures are less relevant when exports are healthy. However, the upper-level official is unable to directly observe the local leader's type, nor does she see the full extent of the stability-enhancing actions that the local leader has adopted; she is moreover unable to infer these perfectly from what she can observe (i.e., the export shock and the realized level of social stability), as there is an idiosyncratic stochastic component in the observed level of social stability.

The upper-level government therefore leverages the local leader's career concerns, in order to incentivize him to exert effort towards stability, as well as to screen for high-quality, competent types, in line with the 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). With this setup, we show formally in Appendix B that the upper-level government will choose to retain the local incumbent if and only if stability meets a certain minimal threshold, consistent with the narrative that maintaining social stability is a veto criterion for career advancement. Importantly, we consider a scenario in which this threshold rule is "sophisticated," in the sense that it can be explicitly conditioned on and adjusted to observed export conditions. In other words, local incumbents who face headwinds from external conditions will be judged more leniently when it comes to delivering stability, compared to those who experience favorable conditions.

This model delivers the following key predictions on the political economy consequences of export shocks for local incumbents:

Prediction 1 (Turnover) *Incumbent turnover increases in response to more negative export shocks.*

Under a "sophisticated" central government, high-quality incumbents always react to a more severe export slowdown by raising the effort they expend on stability measures. Intuitively, these high-quality types recognize that they can reasonably improve their chances of retention, as the upper-level government's threshold rule adjusts with export conditions to accommodate a lower level of social stability that she deems to be acceptable. We show in the appendix that this incentivizes the high-quality incumbent to raise his effort to such an extent that his

probability of removal is invariant to the severity of the export shock (although that probability remains positive as social stability has an idiosyncratic component). In other words, high-quality incumbents are not punished per se for the "bad luck" of being in a prefecture that underwent a negative external shock. On the other hand, low-quality incumbents do not alter how much they expend on stability-enhancing measures, given that they incur a high marginal cost of such effort. For the low-quality types, their likelihood of removal thus increases with the more negative export shock.

Taken together, this entails that more negative shocks will lead, on average, to greater turnover for incumbents whose types we ex ante do not observe. Intuitively, a "sophisticated" government would not want to punish a high-quality local official for a bad export shock that is known to be exogenous. However, since there is imperfect information about the quality of the incumbent, a more severe export shock provides a window for the upper-level government to better discriminate between whether a given local official is high- or low-quality, on the basis of his performance in strike management. Low-quality types are consequently more likely to be screened out, and the overall rate of turnover is higher, when the export shock is more severe. We should stress too that when we use the term "negative", including in the statement of our predictions, what matters is relative comparisons of changes in export conditions, and not whether there is a strict contraction in exports. Even export expansions can convey useful information: Given two prefectures that experience a similar export expansion due to favorable external conditions, the upper-level government would reasonably infer that the prefecture where strike intensity rose more (or decreased less) is being run by a local leader with weaker strike management ability. Our empirical approach – with the key specification being a difference-in-differences estimator – is entirely in line with this observation that it is relative differences that matter for our predictions.

The above discussion leads naturally into the following second prediction:

Prediction 2 (Stability Measures) *The use of stability measures increases in response to more negative export shocks, and more so for incumbents with higher expected future rents.*

This is driven by the responses of high-quality incumbents, who raise their use of stability measures with the severity of the export slowdown. The intuition is that increasing effort allows high-quality incumbents to differentiate themselves from the low-quality types, thereby avoiding what would otherwise be an elevated risk of removal. This increase will moreover be stronger for those incumbents who have higher expected rents from remaining in office, since this sharpens their incentives to exert costly effort. We will later use the age profile of local incumbents as a source of variation in these career concerns.

The model further allows us to distinguish between these predictions and what would emerge in the case of an "unsophisticated" upper-level government that failed to take into consideration the extent of the export shock. In that case, the impact of the shock on incumbent effort would

no longer be monotonic, as high-quality incumbents could respond to very severe slowdowns by instead reducing their effort: they might as well "give up", since stability-enhancing measures would not be enough to push social stability above an unadjusted threshold for performance. Similarly, the effect on incumbent turnover can also be non-monotonic, as the probability of removal for a high-quality incumbent is no longer invariant to the severity of the export shock (see the discussion in Appendix B.4).

5.2 Evidence on Incumbent Turnover

With these insights in mind, we now turn to the evidence, starting with 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 adverse 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.[38] 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 when each individual took and/or left office. We focus on the party secretary, as this is the top executive position at the prefecture level, with ultimate authority and substantial discretion over local fiscal, regulatory, and personnel policies (Persson and Zhuravskaya 2016).[39] 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 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 C.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

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

[39]The prefecture party secretary chairs the local party committee. The party committee is tasked with the leadership role in promoting economic and social development in the prefecture, as spelled out for example in the 2016 "Regulations on the Work of Local Committees of the Communist Party of China"; see: dangjian.people.com.cn/n1/2016/0105/c117092-28012181.html.

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.[40] 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*.[41] 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 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.[42] We thus associate an "early" lateral movement with a *de facto* demotion – being sidelined or replaced as per the language used in our model – as it typically slows down an official's career trajectory, denting their prospects of making it to the next tier in the administrative hierarchy.

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

$$Turnover_{i,t+1} = \theta_1 ExpShock_{it} + \theta_W W_{it} + D_{pt} + D_i + \varepsilon_{it}. \tag{4}$$

This investigates how the likelihood of replacement of the party secretary in year $t+1$ might

[40] 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.

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

[42] 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.

depend on the export shock experienced in prefecture i in year t. We use a one-period lead for the outcome variable, as decisions over turnover would in practice require time for the appropriate information to be gathered and for the institutional procedures of the Organization Department of the Chinese Communist Party to be implemented.[43] 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 W_{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 4 reports our results from estimating (4). 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.[44] (Figure C.3 presents binned scatter plots, analogous to Figure 3, that illustrate the relationship running from the export shock to incumbent lateral movement, as well as to the other political response measures we will examine in Sections 5.3 and 5.4. In Figure C.4, we further illustrate that the estimated negative impact of the export shock on these outcome variables is not overly sensitive to dropping observations that lie in the tail bins of each binned scatter plot.)

In the remaining columns of Table 4, we replace $Turnover_{i,t+1}$ 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 contrast, the export shock in the prefecture has no bearing on lateral movement after a full three-year tenure (Column 4), nor on promotion (Column 5). Negative export shocks are thus linked with an increased likelihood that the party secretary

[43]This specification is comparable to Li and Zhou (2005) and Jia (2017), in that a turnover dummy is regressed on an explanatory variable expressed in changes (respectively, changes in log income and changes in environmental quality). While there may appear to be a slight disconnect with the specification in (2) where the outcome variable is expressed in changes, we show in Table C.4 that our results are preserved if one were to instead use the first-difference of $Turnover_{i,t+1}$ as the dependent variable. The dummy lends itself to a more natural interpretation than its first-difference: for instance, turnover in two consecutive years would have been measured as the same first-difference as in a situation with no change in either year.

[44]Note that the regression explains around half of the variation in lateral movement during the sample period. Moreover, the OLS reduced-form regression that is the analogue of Column 2 – which uses the Bartik ROW export shock directly as an explanatory variable – has an R^2 of 0.5164; this falls to 0.5019 when the export shock is dropped, and to 0.4201 when the incumbent characteristics are further removed. Loosely speaking, the export shock therefore accounts for $(0.5164 - 0.5019)/(0.5164 - 0.4201)$ or about 15% of the variation that is explained by these observables (i.e., the export shock and incumbent characteristics) combined.

Table 4: 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.

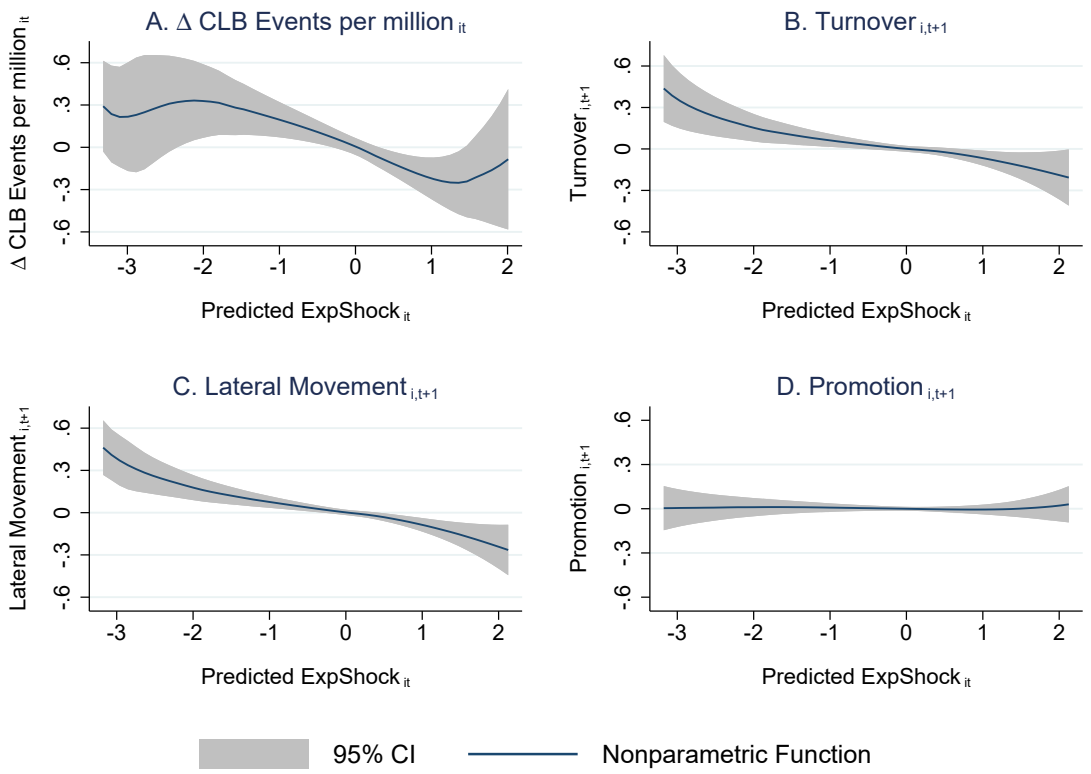
will be reshuffled in a manner detrimental to his/her career prospects. This is in line with the implications of our model, where the upper-level government screens out and sidelines local agents whose performance in the face of an export slowdown has been assessed to be subpar.[45]

We examine these patterns further in Figure 4, which illustrates the relationship between export shocks and incumbent turnover using a more flexible local polynomial plot. On the horizontal axis in each panel, we have plotted the export shock as predicted by our IV in the

[45]In Table C.5, we show that the findings on early lateral movement are robust when the regression is run using only the subsample of observations where the local incumbent has fewer than three years' tenure in their current position. We also present in that table suggestive evidence that negative export shocks over the duration of a local party secretary's tenure can significantly reduce his/her chances of promotion when he/she becomes eligible after crossing the three-year mark.

first-stage of (4).[46] Panel A confirms that it is the prefectures that were most exposed to an export slowdown that witnessed the largest increases in labor unrest events. 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). Through the lens of our conceptual framework, the monotonic patterns in Panels B and C are certainly consistent with outcomes under a "sophisticated" upper-level government, that strategically takes into account observable local export shocks when making decisions over personnel.

Figure 4: Prefecture Export Shocks and Incumbent Turnover
(Local polynomial regression)



Is this negative link between export performance and incumbent turnover mediated (at least in part) by labor strikes? In practice, the decisions over turnover exercised by the central government are likely to take into account other considerations related to economic performance (Li and Zhou 2005); this is in fact what prompted us to develop an extension in our model that accommodates this (see Appendix B.3). One can expect that an export slowdown would make it

[46]More precisely, the predicted export shock and the incumbent turnover variables that we use to plot Figure 4 have each been residualized of the variation that can be explained by the prefecture and incumbent controls W_{it} in (4) as well as the D_{pt} and D_i fixed effects.

more challenging for a prefecture party secretary to deliver a strong economic growth scorecard, which might hamper his/her chances of retention. In what follows though, we present several patterns in the data that indicate that turnover is not based solely on economic performance, as the local leader's management of potential labor unrest also influences whether he/she is ultimately replaced.

Figure 5 speaks to the above issue, by illustrating the probability of lateral movement in year $t+1$ across prefectures divided first by terciles of $ExpShockROW_{it}$ (i.e., the export shock IV), and then by terciles of $\Delta(Events/L)_{it}$.[47] Looking across the export shock terciles, Figure 5 underscores the previous finding that lateral movement was more likely in prefectures hit by worse export shocks; note that this approach of first binning the observations on the basis of $ExpShockROW_{it}$ allows us to (loosely) hold constant other channels through which export conditions might have affected turnover within each tercile, including that economic performance might have directly mattered for these decisions. Looking further then 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. The patterns here suggest that the turnover 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.[48]

Separately, we also consider in Appendix C.3 a series of regressions in which we include both $\Delta(Events/L)_{it}$ and a measure of contemporaneous economic performance. We find that increases in labor strikes are associated with a greater likelihood of an incumbent being sidelined, even when conditioning on the prefecture export shock, GDP growth, or changes in night lights intensity; the latter variables in turn typically exhibit negative coefficients, suggesting that a weaker economic performance is associated with more turnover (see Table C.6). Moreover, the implied magnitude of the coefficient estimate on $\Delta(Events/L)_{it}$ is similar to that for the economic performance measures, which is consistent with the existing qualitative evidence suggesting that both factors are given similar weight in the evaluation of local leaders in China's

[47]The latter $\Delta(Events/L)_{it}$ terciles are based on the full sample; see the Figure 5 notes for the mean values of $\Delta(Events/L)_{it}$ in each of the nine bins. We obtain a similar figure if we instead use $\Delta(Events/L)_{it}$ terciles computed separately within each $ExpShockROW_{it}$ tercile (available on request).

[48]Under the alternative approach of first binning by terciles of $\Delta(Events/L)_{it}$ and then by terciles of $ExpShockROW_{it}$, we find that turnover tends to be negatively correlated with the export shock within a given tercile of labor unrest. Note that this could reflect turnover driven directly by economic performance criteria. Indeed, if we were to residualize the lateral movement measure by variation that can be explained by prefecture GDP growth, a pattern akin to Figure 5 is restored, consistent with turnover being more likely for leaders with poorer strike management conditional on economic performance (available on request).

Figure 5: Export Performance, Strikes, and Incumbent Turnover

Notes: Within the Bottom tercile of $ExpSchockROW_{it}$, the averages of ΔCLB Events per million workers are -0.45, 0.57 and 2.41, respectively, for the Low, Medium and High groups; within the Middle tercile, the averages are -0.27, 0.54 and 1.93, respectively; within the Top tercile, the averages are -0.23, 0.52 and 1.79, respectively. The p-values reported are for t-tests of whether the probability of lateral movement is equal in the High versus Low bins of $\Delta(Events/L)_{it}$ (based on a regression of the lateral movement outcome variable on a set of 3×3 bin dummies, omitting one category, with province-clustered standard errors).

political system (Wang 2016, Pan 2020).[49,50] While we cannot disentangle the two channels more definitively (given the absence of multiple sources of plausibly exogenous variation), these findings nevertheless provide suggestive evidence that economic performance is not the only mediating variable behind the relationship between the export slowdown and political turnover we have detected.[51]

In sum, the body of evidence on incumbent turnover lends itself to the interpretation that

[49]Pan (2020) observes that "[s]ince the 2000s, avoiding large-scale collective action (social stability) and growing the economy have consistently been identified as the targets crucial for career advancement" (p.44); for example, based on internal documents she obtained from several central and eastern provinces between 2015 and 2017, prefecture and county leaders were evaluated annually on a 350 point scale, with 98 points scored based on economic development and 77 points based on social stability.

[50]In Figure C.2, we reproduce Figure 5 but first residualize the lateral movement outcome variable of variation that can be explained by prefecture GDP growth. This confirms that relative performance in strike management matters after stripping out variation that can be explained by this economic performance measure (see the accompanying discussion in Section C.3 in the appendix).

[51]An alternative approach would be to run specifications that interact the export shock with the observed increase in strikes, to explore if the interplay of the two might explain turnover. Through the lens of our conceptual framework, however, such a specification would be tricky to interpret in the absence of a second exogenous source of variation, as strikes would increase endogenously in response to the realized export slowdown.

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 over whether to retain an incumbent appear to account for the severity of the export slowdown that he/she was exposed to – in line with the upper-level government being "sophisticated" – rather than being determined solely by the absolute amount of labor unrest that was observed. Moreover, while achieving social stability is important for a local leader to keep his/her career on track, this is far from a guarantee of future advancement, as other criterion need to be factored in. After all, prefecture leaders who maintained a low occurrence of strikes during a severe export shock still faced a non-zero (and non-trivial) probability of lateral movement (as can be seen from Figure 5), and promotion rates do not appear to respond to the export shock in the prior year (Table 4, Column 5). More broadly, the narrative evidence on party guidelines on promotion suggests that success at curbing instability in any single year would be a necessary but insufficient condition for advancement: The annual evaluation of a party official reportedly needs to be deemed "excellent" for two consecutive years or at least "competent" for three consecutive years for him/her to be eligible for promotion (Edin 2003, Heberer and Trappel 2013).[52]

5.3 Stability Measures: "Weiwen" Textual Analysis

To further bolster the case that performance in managing labor unrest was indeed important amid the export slowdown, we present evidence that points to how local leaders were indeed cognizant of this and acted accordingly in their use of stability-enhancing measures. As discussed in Section 5.1, our conceptual framework predicts that more severe export slowdowns would 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 5.4 similar shifts using measures based on observed prefecture fiscal expenditures.[53] These results further help distinguish the social stability channel from the economic performance channel, since in the absence of stability considerations, we would not expect export shocks to induce the adoption of costly measures unlikely to have a direct positive effect on economic outcomes.

Data and Specification: We adopt a novel approach to measure the degree of attention

[52]In line with this, we find interestingly that there is a positive correlation between the cumulative export shock in the three prior years and the likelihood of promotion (c.f., Table C.5, Column 12, in the appendix).

[53]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 incumbents can enact 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.

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. As discussed in Jiang et al. (2019), the plans outlined in the annual work report should be seen as substantive statements of policy intent for the upcoming year: The content of each report is edited and vetted by the local party standing committee prior to the People's Congress meeting, and the report is then subject to approval by an anonymous vote during the Congress.[54]

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 occurrences of twelve keywords. This list 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.3.) The keyword count is then normalized by the total count of Chinese characters in the work report.

We also implement a more sophisticated machine-learning approach to compute "weiwen" scores for each report. For this, we first randomly selected 20 reports from a pre-sample year (2011), to mark out 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

[54]Although the work report speech is often delivered by mayors (who are in charge of the daily operations of the local government), the pre-eminent position of the party secretary in the local party hierarchy means that his/her policy directives would influence the content of these work reports. For example, Jiang (2018) demonstrates that the relative emphasis taken up by different "topics" in government work reports is correlated with party secretaries' political connections, a likely reflection of their career concerns.

document dated April 2015 on the topic of domestic security measures, as the training sample for the algorithms.[55] 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.[56,57]

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

$$\Delta y_{i,t+1} = \gamma_1 ExpShock_{it} + \gamma_W W_{it} + D_{pt} + D_i + \varepsilon_{it}. \tag{5}$$

This specification is similar to (2), with a textual analysis score (denoted by y) now being used in place of the CLB events variable. Note that (5) relates 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 lead the left-hand variable by one period, since the work reports are delivered at the start of each calendar year; the policy content and wording can thus be expected to be influenced by socioeconomic conditions over the preceding year. If there was a rise in strikes over the preceding year, this would likely raise the "weiwen" emphasis in the next year.

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,

[55]The State Council document 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

[56]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 US 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).

[57]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 were then computed. Reassuringly, the returned scores were very close to 1 (see Appendix A.6 for more details).

as in the regression model (4) from Section 5.2. This means in particular that we are accounting for potential confounding pre-trends in the use of stability measures, that may stem for example from differences across prefectures in pre-existing conditions related to ethnic tensions or the management of migrant worker populations. 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 Section 5.1 regarding the impact of expected future rents from holding office, we also consider whether the effect of the export shock on "weiwen" emphasis might vary with the career prospects of the local incumbent. We explore an interaction term with an indicator variable for whether the incumbent was 57 years-old or younger in year $t + 1$. Given that the mandated retirement age for prefecture party secretaries is 60, local leaders who are 57 years-old or younger would have at least a full three-year term of service ahead of them, and should in principle foresee higher future rents if they avoid an "early" lateral movement from their current positions.[58] In fact, as can be seen in Panel A of Figure 6, 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.[59]

Results: Table 5 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.[60] Our key finding is that negative export shocks induced greater emphasis by local officeholders on maintaining social stability, and especially so for the younger leaders, in line with Prediction 2 from Section 5.1. 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 prefecture with an average age party secretary, a one-standard-

[58]See: http://www.ccdi.gov.cn/djfg/fgsy/201312/t20131209_114257.html, for party guidelines on retirement. Jiang (2018) also adopts this approach of distinguishing between local leaders prior to and after the 57 year age mark. 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.

[59]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.

[60]We include the age dummy interacted with the Bartik variable in (3) as a second instrument in these even-numbered columns. Note that in Table C.12, 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.

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). As is indicated by the negatively significant coefficient for the interaction term, the response is stronger for prefectures with party secretaries under the age of 57.[61]

Table 5: 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 4. 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 variation with age is depicted in Figure 6, Panel B, for the MNB measure, in a manner that is less sensitive to the choice of a specific age cutoff for one's retirement horizons. We display here the coefficients on $ExpShock_{it}$ that we obtain when we run the specification (5) on rolling-window subsamples, each comprising those incumbents whose year-$(t+1)$ age was within a $\pm$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.

Panel A of Figure 7 illustrates local polynomial plots of the relationship between the MNB "weiwen" score and the export shock IV, in a manner analogous to Figure 4. We observe a

[61]Though the summary statistics in Table C.1 point to a decrease in average "weiwen" scores from 2015-2016, Table 5 – and the binned scatter plot in Figure C.3, Panel B – together confirm 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".

Figure 6: Heterogeneous Responses to Export Shocks by Incumbent Age

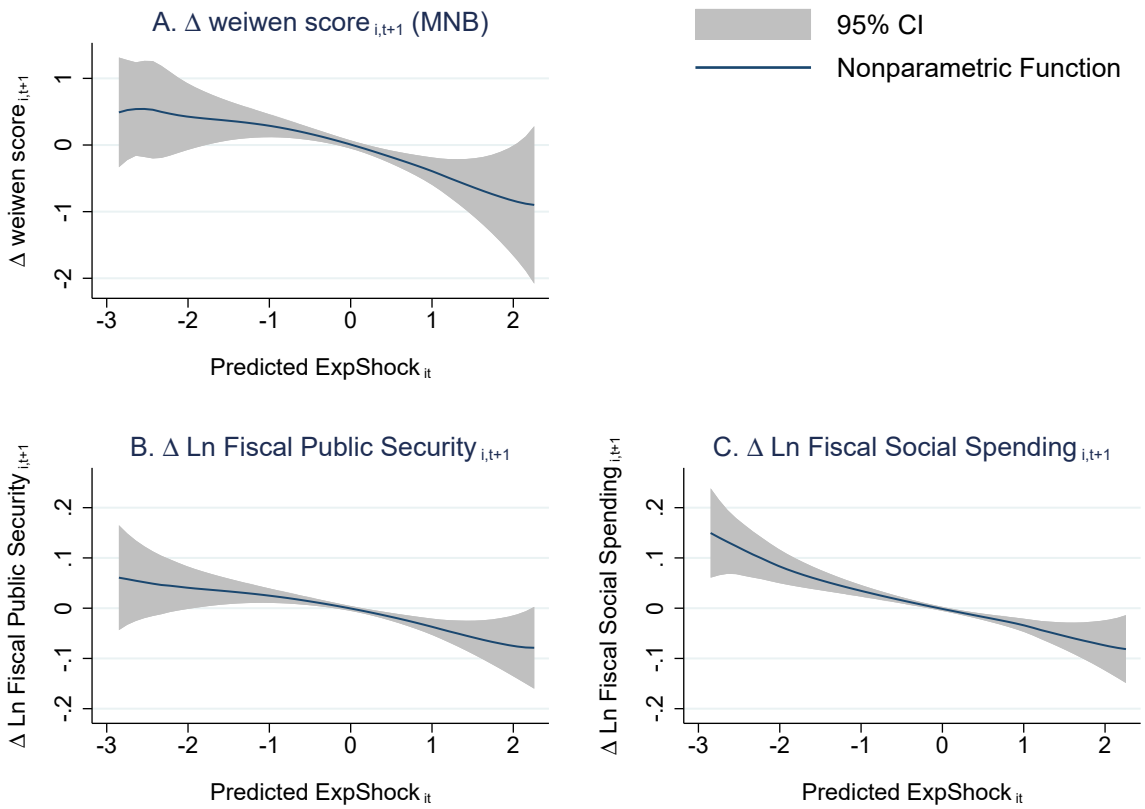
monotonic relation: Incumbents who were exposed to very negative export shocks (as predicted by our instrument) on average display notably larger increases in their concern with stability, as captured by the MNB "weiwen" measure. As discussed in Section 5.1, this is suggestive evidence of sophisticated behavior by the upper-level government, in the sense of it adjusting the criteria for evaluating performance in order to keep providing incentives to (high-quality) local officials for at least the range of negative export shocks that we are able to observe.

5.4 Stability Measures: Fiscal Expenditure

The analysis from the previous subsection captures 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 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

Figure 7: Prefecture Export Shocks and Stability Measures
(Local polynomial regression; Incumbents aged < 57)



Yearbooks published by provincial Bureaus of Statistics; prefecture statistical yearbooks; as well as balance sheets from prefecture government websites. 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%).[62]

[62]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.

We follow the IV specification in (5) 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 (5). Once again, we lead the outcome variable by one period, to reflect an underlying rigidity in the budget process that is relevant in practice: the budget that is approved by the standing committee of the prefecture-level People's Congress at the start of each year should not be altered without going through a set of procedures prescribed by law.[63] Due to this institutional set-up, local conditions in year t can be expected to form the basis on which budget plans are drafted for year $t+1$, which then gets reflected in actual expenditures in year $t+1$.

Results: Panel A of Table 6 reports these regression results. 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 when we consider public security (Column 1a) and social spending (Column 1b) separately. In terms of implied magnitudes, a one-standard-deviation worse export shock 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 C.7 in the appendix, we present the results when the spending items are expressed instead as shares of total expenditure; we arrive at a similar set of conclusions that moreover suggests that the increase in public security or social spending in response to the export shock is not merely picking up a level increase in countercyclical government spending, but reflecting a rise in the relative importance of these uses in the overall budget.

Coming back to Table 6, 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, across the full sample, neither total expenditures (Column 3) nor total revenues (Column 4) appear to respond to the export slowdown. On a separate note, Table C.8 in the appendix breaks down social spending further into several components; the findings show that an export slowdown prompts increases specifically on education (which is the largest component of social spending) and medical services.[65]

[63] See: http://www.npc.gov.cn/zgrdw/englishnpc/Law/2007-12/12/content_1383623.htm.

[64] These patterns are consistent with Wen (2020), who uncovers evidence pointing to the use of employment in Chinese state-owned enterprises as a means to curb potential ethnic unrest. On the other hand, 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.

[65] See Lee and Zhang (2013) and Wang (2014) for anecdotal examples of how the Chinese government has expanded school enrollment during economic downturns to preempt potential unrest among the youth. It is also useful to bear in mind that local governments are in principle responsible for providing education and medical services to migrant workers and their children, as re-emphasized in a State Council document entitled "Opinions of the State Council on Further Improving Migrant Worker Services", dated 12 September 2014; see: http://www.moe.gov.cn/jyb_xxgk/moe_1777/moe_1778/201612/ t20161213_291790.html. Note that 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.

Table 6: 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 4. 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 countercyclical 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 6: 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.[66,67]

In Panels B and C in Figure 7, 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 adverse export shocks in fact undertook 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.

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. In fact, although social spending may also serve as an automatic fiscal stabilizer, the emphasis on "weiwen" and the fiscal spending on public security indicate that the response is not solely driven by an effort to boost economic growth. Hence, the screening and selection mechanism on the basis of relative performance in delivering social stability seems to be an important driving factor in the responses of the prefecture party secretaries. Moreover, 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.

6 Robustness

In this section, we describe the 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 in the interest of space. A more detailed documentation of each test is provided in Appendix C, where we report the respective checks for five key prefecture-level outcome variables, namely: labor strikes, lateral incumbent turnover, the MNB "weiwen" measure, public security spending, and social spending.

Specification Checks (Appendix C.5): At a basic level, we have verified that our results remain stable when we: (i) drop the auxiliary prefecture controls, i.e., the change in college-

[66]Table C.9 uncovers several other dimensions of heterogeneity. In response to an export slowdown, 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.

[67]We have also found using an interaction specification that incumbents who expanded the use of stability measures to a greater extent amid an export slowdown were less likely to experience a lateral movement (available on request), though these results are subject to the caveat that the use of stability measures should be seen as endogenous variables in the context of our model, and we lack a second source of exogenous variation for, say, the composition of fiscal spending across prefectures.

enrolled, mobile-use, and internet-use shares (Panel A, Table C.10); or (ii) run unweighted regressions (Panel B).

We have further explored several alternative specifications that are commonly adopted for 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, e.g., $\Delta(Events/L)_{i,t-1}$, to control for prefecture-specific forces present in these pre-trends (Panel C); or (ii) control instead for the lagged dependent variable in levels, e.g., $(Events/L)_{i,t-1}$, to account for possible mean reversion (Panel D); or (iii) simply drop the prefecture fixed effects (Panel E).[68] We have also run regressions using just the subset of observations with $t = 2015$, the most severe year of the export slowdown, while controlling for province effects (Panel F), as well as a long-difference specification that explores the impact of the cumulative export shock over 2012-2015 (Panel G). Our findings across these specifications indicate that the effects of the export shock on the range of outcome variables are 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.[69]

Validating the Bartik Strategy (Appendix C.6): We carefully address a set of issues that may affect confidence in the Bartik identification approach. 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 (Borusyak et al. 2022). This assumption would be violated in practice if more severe export shocks tend to occur in products concentrated in prefectures with certain baseline characteristics, that themselves have independent effects on local labor strikes or political outcomes. We therefore follow Borusyak et al. (2022) and test whether the product-level export shocks, $g_k = \Delta X_k^{ROW} / \sum_i X_{ik,2010}$, are balanced across an exposure-weighted average of initial prefecture characteristics.[70] We consider two types of prefecture variables: (i) baseline 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 C.12 reports

[68] Angrist and Pischke (2009, Chapter 5.4) argue that the fixed effects specification (our baseline) and the alternative that controls instead for the lagged dependent variable (Panel C, Table C.10) 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 C.10, to avoid Nickell bias.

[69] In Panel H of Table C.10, we further show that the coefficient estimates are virtually unaffected when contemporaneous (annual) changes in prefecture GDP are added as a control.

[70] More formally, define: $s_{ik} = X_{ik,2010}/L_{i,2000}$ to be the exposure of prefecture i to the product-level shock g_k; e_i to be the regression weights (i.e., prefecture working-age population), and $s_k = \sum_i e_i s_{ik}$. Borusyak et al. (2022) show that the relevant exposure-weighted average of prefecture characteristic v_i that should be used for this balance test is $s_k \phi_k$, where $\phi_k = \sum_i (e_i s_{ik} v_i) / \sum_i (e_i s_{ik})$.

the results of this balance test.[71] 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 considered 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.[72]

Borusyak et al. (2022) 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.[73] We report the results from these product-level regressions in Panel B of Table C.12; by construction, the export shock coefficient is equal to that in the analogous prefecture-level regressions (Column 3, Table 1; Column 2, Table 4; Column 3, Table 5; Columns 1a and 1b, Table 6). Moreover, the smaller standard errors we obtain here (even when clustering by broad HS 2-digit headings) suggest that the statistical inference drawn in our baseline prefecture-level regressions was relatively conservative.

Returning to the prefecture-level specification, 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 C.13). In addition, we pick up on the test in Column 2 of Table C.2, to show that future export shocks have little explanatory power for contemporaneous outcomes; this holds not just for labor strikes, but also for the political response variables we study in Section 5 (see Table C.14). This indicates that prefecture-specific pre-trends are unlikely to be driving our results.

A related concern is that our results might be driven by initial specialization in certain industries that display pre-determined trends. For example, 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 sets of 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 C.15). Note that this check also helps to reassure that our results

[71] 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}.

[72] 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.

[73] 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 (W_{it}) and the fixed effects (D_i and D_{pt}) on the right-hand side of (2).

are not driven by individual HS sections which may have experienced particularly large shocks (Goldsmith-Pinkham et al. 2020).[74,75]

Alternative Statistical Inference (Appendix C.7): 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 presented in Panel B of Table C.12 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 C.16).[76] We also adopt the wild cluster bootstrap-t procedure proposed by Cameron et al. (2008) as an alternative method of inference. This is to address the concern that with a relatively small number of clusters, the cluster-robust standard errors are downwards biased.[77]

Alternative Bartik IVs (Appendix C.8): We experiment with several variants of the Bartik IV in Table C.17. 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.[78] 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 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 C.9): 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

[74] 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).

[75] 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).

[76] 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).

[77] The number of clusters in our baseline regressions is 30. The Monte Carlo simulations in Cameron et al. (2008) show that when the number of clusters is 30, the problem of over-rejection is relatively minor.

[78] 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".

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 C.18); 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). 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).[79]

7 Conclusion

In this paper, we have documented how the slowdown in world trade in the years following the global financial crisis triggered an increase in labor strikes in China. To make the case that our estimates reflect a causal impact of this export shock, we applied a shift-share IV strategy and performed extensive checks related to the validity of this approach. We further showed that there was an elevated likelihood of turnover for the local party secretary in prefectures that experienced a more severe export slowdown, particularly when this was accompanied by larger increases in labor unrest relative to other prefectures that saw comparable export shocks. Local leaders appeared to be cognizant of this threat of replacement, as declining exports led to a rising use of "weiwen" language in annual work reports, as well as increased expenditures on both public security and social spending. Such responses were notably more pronounced for younger leaders whose career incentives would in principle be strongest.

That Beijing's decisions over the replacement of local officials accords more with that of a "sophisticated" upper-level government in our model of "political accountability with Chinese characteristics" is perhaps not that surprising, but useful to document systematically nevertheless. While the central government does place a lot of weight on local leaders' performance in delivering social stability, our analysis suggests that this is implemented in a more subtle way than an absolute "veto criterion"; instead, the central government appears to condition its assessment on the severity of the export shock, in order to make strategic use of this information to incentivize and screen local leaders.

The findings here raise pertinent questions on the political repercussions for China moving forward. At a basic level, there is certainly much more to be learnt about the medium- to long-run effects of the government actions and fiscal spending that have been enacted in response to

[79]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.

the export slowdown, 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). More broadly, since the period under study in this paper, China's exports have been confronted by further challenges, including an apparent slowdown in economic growth stemming from the "Trump tariffs" (see for example, Chor and Li 2021), as well as the supply chain and port disruptions brought about by the Covid-19 pandemic. Our results suggest that the more tepid outlook for Chinese exports will likely challenge the abilities of local leaders to navigate the resulting labor market fallout and work-related grievances. What remains to be seen is whether the strains to social stability could cumulate to such an extent that it might test the status quo of unquestioned stability for the central government.

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A Data Appendix

A.1 Corroborating Data for Labor Strikes and Disputes

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). Note that the MOHRSS labor dispute counts exceed the CLB strike counts, since the MOHRSS in principle covers a wider set of disputes while the CLB might be expected to pick up only on more severe disagreements that escalate into publicly-visible strikes. We nevertheless view the MOHRSS data as a useful source of corroboration, since these labor dispute counts should be expected to rise too if there is an increase in wage arrears or worker layoffs (these being the most common causes of strikes in the CLB data).

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.)

Sina Weibo Strike Posts. Qin et al. (2017) constructed count measures of social media (Sina Weibo) posts related to strikes, as identified from keywords linked to strike activity. The data they have made available in their replication package covers a shorter and earlier time frame, 2010 to 2012. Given that the publicly-available CLB data commences only in 2011, this gives two years of overlap (2011 and 2012) with which to perform a basic comparison.

In Column 1 of Table A.1, we regress the change between 2011 and 2012 in the Qin et al. (2017) measure of strike posts expressed in per million worker terms against the contemporaneous change in CLB events per million workers. The negative correlation obtained here may come across as somewhat surprising initially. That said, when we further control in Column 2 for the initial level of strike post counts per million workers (in 2011), we find strong evidence of mean reversion, while the partial correlation with the change in CLB events per million workers is now positive and significant. In other words, once the strong tendency toward mean reversion in the Weibo posts data is taken into account, both the strike post and CLB event data appear to be picking up similar movements over time in the occurrence of strikes. There are moreover

natural reasons why one might expect mean reversion to be a strong feature of the social media data: A surge in Weibo strike posts in one year due say to a high-profile strike event might lead the internet censors to clamp down on posts in the next year. (Note that since we have one data point per prefecture, we run the regressions in Table A.1 with province fixed effects, while reporting robust standard errors.)

In Columns 3-4, we regress the change in Weibo strike posts measure against the Bartik rest-of-the-world export shock. We find that we can recover our baseline result of a negative export shock coefficient with this alternative measure of labor market conflict instead, but again only if we control for the initial level of Weibo strike posts per million workers to account for mean reversion (compare Columns 3 versus 4). (Note that we have run these last two columns as OLS reduced-form regressions, as the corresponding IV specifications have F-statistics that are slightly lower than the rule-of-thumb threshold value of 10.)

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 VIIRS-DNB, we calculate the average night lights intensity across observation grids that overlap with each Chinese prefecture's territory. VIIRS-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 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 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.

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.2, 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.2 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.[80] 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.[81] 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.[82]

To operationalize these supervised machine learning algorithms, we put together a training dataset comprising: (i) 20 prefecture annual work reports selected at random from a pre-sample year (2011); and (ii) the State Council document of 13 April 2015 on: "Opinions on Strengthening Society's Public Security Prevention and Control System" (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

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

[81] 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

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

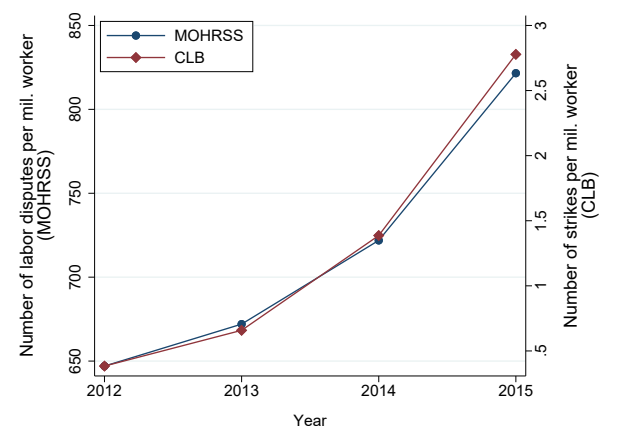
zero correlation in the joint occurrence of any pair of tokens. The SVM model on the other hand transforms the passages from the training dataset into points in a high-dimensional metric space, and then partitions these in a binary fashion via a hyperplane that seeks to maximize the distance between 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.

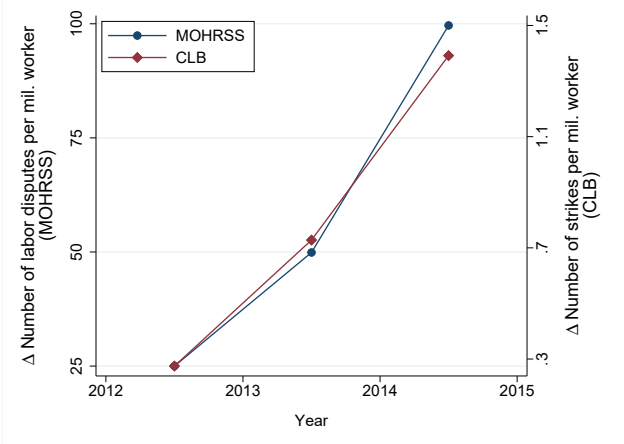
We subsequently applied these two models to the prefecture annual work reports in our sample period of interest (2012-2016). The "weiwen" score under each machine learning model for a given work report was computed by first calculating the "weiwen" score for each paragraph in the report, and then taking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

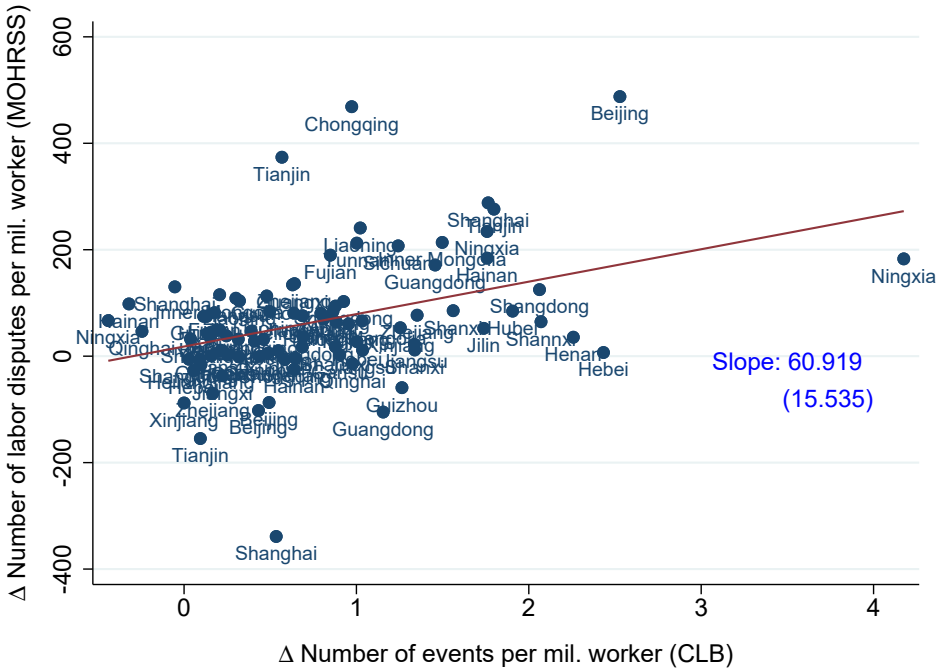


Table A.1: Comparing CLB Labor Events versus Qin et al. (2017) Weibo Strike Posts

Dependent variable:	Δ **Weibo Strike Posts per million**$_i$			
	(1) OLS	(2) OLS	(3) OLS-RF	(4) OLS-RF
(Initial Weibo Strike Posts/L)$_i$		-0.5992***		-0.6037***
		(0.0624)		(0.0722)
Δ (CLB Events/L)$_i$	-207.4004**	65.4421**		
	(94.3582)	(25.9100)		
ExpShockROW$_i$			104.6184	-52.8545*
			(88.8377)	(26.8829)
Province dummies?	Y	Y	Y	Y
Observations	310	310	310	310
R^2	0.2655	0.9354	0.2684	0.9265

Notes: The dependent variable is the change in Weibo strike posts per million workers in prefecture i between 2011 and 2012. All columns run OLS regressions with province fixed effects, with the prefecture's working-age population in 2010 used as regression weights. Robust standard errors are reported. *** p<0.01, ** p<0.05, * p<0.1.

Table A.2: Future Promotion Probability of Lateral Movers

Dependent variable:	**Promotion: in the next movement** (1) OLS	**Promotion: ever in the future** (2) OLS	**Highest rank: sub-province level or above** (3) OLS	**Highest rank: province level or above** (4) 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.3: 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"

Notes: The above is the list of keywords adopted for the basic "weiwen"-related keyword count measure (c.f., Columns 1-2 of Table 5).

B Political Response to Export Shocks: A Simple Model

We develop in this Appendix 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 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. 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 local leaders on fostering stability, that we will then confront with data in Section 5.

B.1 Setup

We consider a setting in which a prefecture faces external demand conditions for its exports, denoted by $x \in [\underline{x}, \overline{x}]$, that are exogenous from the perspective of the local leader. Changes in x thus correspond to what we have been referring to in the main paper as an export shock. Note that x is increasing in the exports of the prefecture, so that we associate decreases in x with an export slowdown.

Export conditions affects social stability, y, at the prefecture level, which we specify to be:

$$y = h(x, m) + \varepsilon. \tag{B.1}$$

The $h(x, m)$ function captures how export conditions 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 is an independent draw from a mean-zero normal distribution, $N(0, \sigma^2)$, which reflects further idiosyncratic forces that influence social stability.[83] (We will shortly see that this upper-level government objective function can be readily extended to incorporate direct concerns about economic performance/growth, in addition to concerns about social stability, without detracting from the model's predictions on the relationship between export shocks and incumbent turnover.)

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 export conditions are at their most severe ($x \longrightarrow \underline{x}$), social stability deteriorates substantially, to such

[83] 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.

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 conditions 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 adverse export conditions, 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 export conditions are more adverse. Put otherwise, additional "sticks" or "carrots" are less relevant when the export performance of the prefecture is healthy.[84]

The upper-level government 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 to mount a "weiwen" 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" or high-quality) and $\ell = B$ ("bad" or low-quality) – who differ in their innate competency in delivering social stability. We 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 local export conditions 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 these conditions. 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 the local incumbent based on information of x and y. 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.[85]

[84] We show in Appendix B.6 how this formulation of the social stability function can be justified in a worker-level model of strike intensity. 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).

[85] 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.

B.2 Optimal Strategies of Governments at Different Levels

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). 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 given the selection rule administrated by the upper-level government.

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. Specifically, let: $g'_\ell(s) = a_\ell + \delta m$, where $\delta > 0$, $a_G = 0$ and $a_B > R\bar{h}/\sqrt{2\pi\sigma^2}$. By invoking this parameter restriction, we establish a key lemma:

Lemma 1: *In equilibrium, the upper-level government adopts a threshold rule in which she will retain the incumbent if and only if y exceeds $\bar{y}(x) = h(x, m_G(x))$, while sidelining him otherwise. B-type local leaders choose $m_B = 0$, while G-type local leaders' optimal level of effort is the interior solution to the following first-order condition:*

$$\phi(0)h_m(x, m_G)R = \delta m_G. \tag{B.2}$$

Note that (B.2) expresses m_G as an implicit function of observed export conditions x. Thus, the $\bar{y}(x)$ stability cutoff that the upper-level government adopts for its threshold rule is entirely a function of observed export conditions, and can be implemented in particular without precise knowledge of whether the local leader is a G- or B-type.

The proof of Lemma 1 is detailed in Appendix B.5.1, while the intuition is as follows. The condition on a_B implies that for a B-type leader, the marginal cost of enacting stability measures exceeds the marginal benefit for all $m > 0$, regardless of the selection rule adopted by the upper-level government. Hence, $m_B = 0$. With the goal of maximizing expected stability, the upper-level government thus has an interest in eliciting as high a level of m_G as possible. She sets a threshold rule $\bar{y}(x)$ that adjusts with the realized export shock. This ensures that the expected gain of enhancing stability measures increases for the G-type leaders when prefectures are hit by a more severe export slowdown. As for G-type leaders, with the cutoff rule of retention in mind, they would choose m in order to maximize his expected rents, less the costs borne from enacting stability measures:

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

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:

$$\phi(\bar{y}(x) - h(x, m))h_m(x, m)R = g'_G(m) = \delta m, \tag{B.3}$$

where $\phi(\cdot)$ is the pdf associated with $\Phi(\cdot)$. We show in Appendix B.5.1 that the first-order condition (B.3) implies a strictly interior solution for m_G (i.e., with $m_G > 0 = m_B$). Moreover, since $h_{mm} \leq 0$, the upper-level government elicits the highest level of m_G by setting $\bar{y}(x) = h(x, m_G)$. m_G hence is the solution to Equation (B.2).

B.3 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 explore empirically in Section 5.

B.3.1 Party Secretary Turnover

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 export conditions 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 when there is an 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 x, 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.[86]

Last but not least, we consider the implications for the probability of turnover for a prefecture leader whose type we do not directly observe; this is closely connected to the turnover dummy variable we use in the empirical work in Section 5. Given that the share of G-type leaders is p, this probability is: $\pi = \frac{p}{2} + (1-p)\Phi(\bar{y}(x) - h(x, 0)) = \frac{p}{2} + (1-p)\Phi(h(x, m_G) - h(x, 0))$. The model yields the following prediction.

[86] 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 $\bar{y}(x)$.

Prediction 1 (Turnover): *The probability of turnover for a prefecture leader whose type is unobserved, π, increases in response to more negative export shocks.*

The proof of Prediction 1 is in Appendix B.5.2. Prediction 1 indicates that, when looking across a broad set of local leaders, one should expect that adverse export shocks increase the likelihood of incumbent turnover. This 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. In other words, a more negative export shock strengthens the signal of ability relative to the noise ε, which improves the upper-level government's ability to screen and retain the G-type local leaders.

In emphasizing the social stability channel, the baseline model abstracts from other potential determinants of local political turnover. In particular, an export shock could affect economic growth, which has been shown elsewhere to also influence local political turnover Li and Zhou (2005). We can readily incorporate this by extending the model, so that the upper-level government not only cares about social stability, but also economic performance/growth.

This can be done by modifying the upper-level government's objective function to: $y = h(x, m) + q(x, e) + \varepsilon$, where $e \geq 0$ denotes the efforts taken by incumbents to improve local economic performance, $q(x, e)$. Written in this way, the non-idiosyncractic component of the government's objective function can be viewed as a weighted average of strike management $h(x, m)$ and economic performance $q(x, e)$, and thus consistent with a multi-tasking interpretation; the weights are implicitly folded into the $h(\cdot)$ and $q(\cdot)$ functions. We adopt an analogous set of assumptions about how $q(\cdot)$ varies with x and e. In particular, we assume that: (i) $q_x > 0$, so it is easier for the prefecture to deliver strong economic performance/growth if external export conditions are favorable; (ii) $q \longrightarrow -\infty$ as $x \longrightarrow \underline{x}$; (iii) $q \longrightarrow +\infty$ as $x \longrightarrow \overline{x}$; (iv) $q_e > 0$, and $\tilde{q}_{ee} \leq 0$, so that economic policy effort raises economic performance, but with diminishing returns to ensure an interior solution; and (v) $\tilde{q}_{xe} < 0$, so that these policy measures matter less when exports are healthy.

With the above formulation, notice that defining $\tilde{h}(x, m, e) \equiv h(x, m) + q(x, e)$, we have that $\tilde{h}(\cdot)$ inherits the following properties from $h(\cdot)$: (i) $\tilde{h}_x > 0$; (ii) $\tilde{h} \longrightarrow -\infty$ as $x \longrightarrow \underline{x}$; (iii) $\tilde{h} \longrightarrow +\infty$ as $x \longrightarrow \overline{x}$; (iv) $\tilde{h}_m > 0$, $\tilde{h}_{mm} \leq 0$, with $\tilde{h}_m < \overline{h}$ being bounded; and (v) $\tilde{h}_{xm} < 0$. Given that $\tilde{h}(\cdot)$ inherits these key properties, it follows that Prediction 1 – that incumbent turnover increases the lower is x – continues to hold even with this modified objective function that incorporates direct concerns about economic performance. Moreover, it is straightforward to see that the optimal level of stability measures adopted will continue to be pinned down by equation (B.2); in particular, a high-quality local leader would choose a combination of m and e to ensure that his probability of replacement is invariant to export conditions, x. It then follows that Prediction 2 below – that the use of stability measures increases the lower is x – would also continue to hold.

In Appendix C.3, we report on some descriptive regressions that explore whether both local labor unrest and economic performance have explanatory power for party secretary turnover.

We find there that increases in labor strikes are positively associated with the likelihood that a local incumbent is laterally moved (i.e., sidelined), even when conditioning on prefecture GDP growth or changes in night lights intensity. The latter variables in turn typically exhibit negative coefficients, suggesting that a weaker economic performance is associated with more turnover (see Table C.6). These findings provide suggestive evidence that economic performance is unlikely to be the sole mediating variable that drives the relationship between export shocks and political turnover detected in Section 5.2.

What may more definitely distinguish the social stability channel from the economic performance channel are the responses of stability-enhancing measures that are presumably unproductive in promoting economic growth (e.g., emphasis on "weiwen" and fiscal spending on public security). More specifically, in the absence of the social stability channel, we should not expect an export shock to have an impact on the components of m that are orthogonal to e.

B.3.2 Stability Measures

We thus turn to the baseline model's predictions on the responses of stability measures:

Prediction 2 (Stability Measures): *The expected level of stability measures, $m = pm_G + (1 - p)m_B$, increases in response to more negative export shocks.*

The proof of Prediction 2 is in Appendix B.5.3. 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. For the analysis in Section 5, we proxy for m using two sets of measures: (i) emphasis on"weiwen" inferred from prefecture annual work reports; and (ii) fiscal expenditure on public security and social spending.

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. 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: $h(x, m) = h^{(1)}(x) + h^{(2)}(x)m$, such that conditions (i)-(v) are satisfied.[87] With this functional form assumption, we can unambiguously sign the cross derivative $\frac{d^2m}{dxdR}$.

[87]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 $h^{(2)}_x(x) = 1 - x$, 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 (B.8) that any efforts to sign $\frac{d^2m_G}{dxdR}$ under a general $h(x, m)$ function will require making assumptions about the third-derivatives of $h(x, m)$, which can be unintuitive.

Prediction 3 (Heterogeneous Responses of Stability Measures): *Following a negative export shock, the expected level of stability measures increases more in regions where local incumbents face high expected future rents. That is,* $\frac{d^2m}{dxdR} < 0$.

The proof of Prediction 3 is in Appendix B.5.4. In this particular case, for any given level of external export conditions x, $h(x, m)$ is linear in the stability measures m that are implemented. In general, Prediction 2 holds true as long as diminishing returns to the use of stability measures do not set in too quickly.

B.4 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 observed export conditions 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 find in Section 5.

Suppose then that the upper-level government picks an "unsophisticated" cutoff rule where $\bar{y}$ does not depend on x. 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, the first-order condition in (B.3) is now:

$$\phi(\bar{y} - h(x, m))h_m(x, m)R \;=\; \delta m. \tag{B.4}$$

The case of an "unsophisticated" upper-level government yields a distinct set of implications, which are summarized in the following statement:

Prediction 4: *In the "unsophisticated" case, the use of stability measures does not necessarily increase with a slowdown in exports, especially when export shocks are very negative. The relationship between export performance and turnover may also exhibit non-monotonicities.*

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 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 increases their use in a monotonic way – as the export shock worsens. These contrasting predictions are useful to bear in mind for interpreting the empirical results.

With an "unsophisticated" government and a fixed threshold, the probability of turnover changes with x for the G-type leaders as well. There is a possibility that an export slowdown can be associated with a fall in turnover probability if G-type leaders respond to a negative shock with an elevated use of m and such stability measures are very efficacious in enhancing h. 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.

B.5 Detailed Proofs

B.5.1 Proof of Lemma 1

We first establish that for an upper-level government whose objective is to maximize expected stability, $E(y) = ph(x, m_G) + (1-p)h(x, m_B)$, the optimal decision rule given export conditions x takes the form of a single threshold, with the local leader 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 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 are associated with different courses of action; in other words, if $(y_j, y_{j+1}]$ is an interval 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 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:

$$\left(\sum_{i=\ldots-2,0,2,\ldots} Pr(y_i < y < y_{i+1}) \right) R - g_\ell(m)$$

$$= \left(\sum_{i=\ldots-2,0,2,\ldots} \Phi(y_{i+1} - h(x, m)) - \Phi(y_i - h(x, m)) \right) R - g_\ell(m),$$

and the associated first-order condition is:

$$\left(\sum_{i=\ldots-2,0,2,\ldots} \phi(y_i - h(x,m)) - \phi(y_{i+1} - h(x,m))\right) h_m(x,m)R \;=\; g'_\ell(m). \tag{B.5}$$

Here, $\Phi(\cdot)$ is the cdf of the $N(0,\sigma^2)$ normal distribution, and $\phi(\cdot)$ is the associated pdf.

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 (B.5) 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 (B.5) that pre-multiplies $h_m(x,m)R$ can be written as:

$$\begin{aligned}&\left(\sum_{k=1}^{\infty} \phi(y_{j-2k} - h(x,m)) - \phi(y_{j-2k+1} - h(x,m))\right) + \phi(y_j - h(x,m)) \\ &+ \left(\sum_{k=1}^{\infty} \phi(y_{j+2k} - h(x,m)) - \phi(y_{j+2k-1} - h(x,m))\right)\end{aligned} \tag{B.6}$$

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 (B.6) 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 (B.5) is not larger than $R\overline{h}/\sqrt{2\pi\sigma^2}$. Thus, (B.5) 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:

$$\left(\sum_{i=\ldots-2,0,2,\ldots} \phi(y_i - h(x,m)) - \phi(y_{i+1} - h(x,m))\right) R \;=\; \frac{\delta m}{h_m(x,m)}. \tag{B.7}$$

Bearing in mind that $h_{mm} \leq 0$, the right-hand side of (B.7) is increasing in m. The upper-level government will thus seek a decision rule that pushes up the left-hand side of (B.7) 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 (B.7) 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 (B.7). It follows that the optimal decision rule features a unique cutoff $y_j = h(x, m_G)$, where the local incumbent is replaced when stability y falls below y_j and is retained when y is larger than y_j. In the main paper, we denote the threshold rule by $\bar{y}(x, m_G)$.

Note that m_G is in turn determined by solving the G-type incumbent's first-order condition given the threshold rule that the upper-level government will adopt. At $m = 0$, the marginal cost of enacting stability 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 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 $\overline{h}R/\sqrt{2\pi\sigma^2}$. The intermediate value theorem then implies the existence of an interior solution to the G-type leader's first-order condition, which satisfies $\phi(0)h_m(x, m_G)R = \delta m_G$.

B.5.2 Proof of Prediction 1

Differentiating the expression for π 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) $\frac{dm_G}{dx} < 0$.

B.5.3 Proof of Prediction 2

Log differentiating (B.2), we obtain:

$$\frac{dm_G}{dx} = \frac{h_{xm} m_G}{h_m - m_G h_{mm}} < 0, \tag{B.8}$$

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. 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.

B.5.4 Proof of Prediction 3

With $h(x,m) = h^{(1)}(x) + h^{(2)}(x)m$, the expression in (B.8) 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 $h_x^{(2)} < 0$. Moreover, log-differentiating (B.3), 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: $\frac{d^2 m_G}{dxdR} < 0$; in words, the incentive to raise stability measures following a negative export shock would be stronger for G-type local incumbents who face high expected future rents. Since $m_B = 0$, this statement holds too in expectation for a local leader of ex-ante unknown type.

B.5.5 Proof of Prediction 4

For G-type leaders, 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 the first-order condition (B.4), the level of stability measures adopted by the G-type leader m_G^u would then tend to 0 too.[88] Therefore, m_G^w changes with x in a non-monotonic way. Since $m_B^u = 0$, these implications also apply to the expected level of stability measures, $pm_G^u + (1-p)m_B^u$.

As for the implications for political 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:

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

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 $\bar{y}$.

[88] 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:

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

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.

B.6 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 their framework 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 predictions provide a justification for the regression specification we run in Section 4. At the same time, we show how we can map the predictions for the level of strikes to the stability function $h(x, m)$ adopted in the setup of our model of political responses developed earlier in Appendix B.1.

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 decreases in x corresponding to adverse export shocks. 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:

$$-\tau_s(1-s) - (1-\tau) - c_s \;\; = \;\; 0. \tag{B.9}$$

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 (B.9), 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 (B.9) 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 changes in 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 W_{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

(B.9) 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 B), 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 C.14). 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.

We are nevertheless still able to recover unbiased estimates of the effect of the export shock on changes in strikes per capita, insofar as the product-level export shocks in our Bartik instrumental variable are uncorrelated with weighted-averages of pre-determined prefecture characteristics, including the intensity of "weiwen" measures; this would alleviate omitted variables bias concerns associated with these prefecture characteristics. This is a balance condition that we have verified in Panel A of Table C.12, where we show that the product-level export shocks are uncorrelated with weighted-average measures of prefecture pre-trends in the "weiwen" textual analysis score (multinomial Bayes version), log fiscal spending on public security, as well as log social spending.

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 (B.1) that was laid out at the start of Appendix B. 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 (B.9) 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 (B.1), in particular: $h_m > 0$, $h_x > 0$, and $h_{mx} < 0$. Consider:

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

Note that $\breve{h}(x,m) \longrightarrow -\infty$ as $x \longrightarrow -\infty$, while $\breve{h}(x,m) \longrightarrow +\infty$ 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 $\breve{h}(x,m)$ will inherit these derivative properties, i.e., $\breve{h}_m > 0$ and $\breve{h}_x > 0$. With regard to the cross-derivative, direct differentiation yields: $\breve{h}_{mx} = \frac{f(m)+2a(x)}{((1-2a(x))+(f(m)+1))^3}(-2a_x f_m)$, which is negative so long as $f(m) > -2a(x)$. (Note that the denominator of $\breve{h}_{mx}$ 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 $\breve{h}_{mx} < 0$. 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 $\breve{h}(x,m)$ function that meets the required conditions for the $h(x,m)$ term in the expression for social stability in equation (B.1). 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 Additional Empirical Results and Checks

We present additional results and 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 C.1.

C.1 Export Shocks and Labor Strikes: Heterogeneous Effects

In Table C.2, we expand on our baseline result on the negative link between $ExpShock_{it}$ and a rise in labor strikes, by exploring additional dimensions of the prefecture export shock. Column 1 considers the possibility of spatial spillovers from the export slowdown. 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 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.[89]

We examine in Column 2 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 3 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 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.

We run an interaction specification in Column 4 to investigate several dimensions along which the effect of the export shock on labor strikes might be heterogeneous. We find 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

[89] We have separately found 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 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).

the 2010 Census). Conversely, the effect of $ExpShock_{it}$ was exacerbated in prefectures with a greater 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 4, given that we do not propose an instrument for each initial prefecture characteristic.[90]

C.2 Export Shocks and Labor Market Outcomes in the CFPS

We elaborate on the evidence presented in Column 5 of Table 3 in the main paper, on the impact of export shocks on individual income. The individual-level information on income are obtained from the China Family Panel Studies (CFPS). The CFPS is a biennial survey designed to be China's equivalent of the US Panel Study of Income Dynamics. For our purpose, we use the 2012, 2014 and 2016 waves of the CFPS. The individual-level data are drawn from the module on adults, and the sample is restricted to the working-age population between 21 and 55 years old. Our sample contains 30,721 unique individuals located across 128 prefectures in 25 provinces. We can trace 44% of the individuals for 3 waves, and 27% of the individuals for 2 waves. The total number of observations is 66,259.

To estimate the impact of export slowdowns on individual income, we estimate:

$$IncomeGrowth_{jt} = \alpha_1 ExpShock_{it} + \alpha_W W_{jt} + D_{pt} + D_i + \varepsilon_{jt}, \tag{C.1}$$

where $IncomeGrowth_{jt} = \frac{Income_{jt} - Income_{i,t-2}}{Income_{jt} + Income_{i,t-2}}$ is the Davis-Haltiwanger-Schuh approximation of the log growth rate of the income of individual j between two consecutive waves of the survey. $ExpShock_{it}$ is the change in exports per worker in prefecture i over two consecutive waves; it is defined as in equation (1) in the main text but with the changes in product-level exports computed over two years instead. W_{jt} is a vector of individual characteristics, including six age group dummies (21-25, 26-30, 31-35, 36-40, 41-45, 46-50, and 51-55), five educational attainment group dummies (illiterate, primary school, middle school, high school, and college or above), a gender dummy, and a dummy indicating urban areas. D_i and D_{pt} denote prefecture dummies

[90] To gauge the magnitude of these 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 4 of Table C.2.)

and province-year dummies, respectively. Time-invariant individual characteristics that might be determinants of income are netted out in this difference-in-differences specification. We use $ExpShockROW_{it}$, i.e., the change in per worker exports to the ROW over two consecutive waves, as an instrument for $ExpShock_{it}$. Standard errors are clustered at the province level.[91]

Panel A of Table C.3 reports the regression results. The estimated coefficient in Column 1 implies that a one-standard-deviation decline in exports per worker ($\approx$ \$841) reduces income growth by 5.1%. In Columns 2-4, we estimate equation (C.1) for workers with different skill levels. We define high-skilled workers as those with college education or above, mid-skilled workers as those with high-school education, and low-skilled workers as those with middle-school education or below. The effect of export shocks appears to be more pronounced for low-skilled workers. Panel B investigates whether export shocks affect the likelihood of having a wage-earning job. Here, we replace the dependent variable with the change in consecutive waves in the individual's wage-earning job status, where $Wage\text{-}Earning\ Employment_{jt}$ is equal to one if individual j has a wage-earning job, and is zero if the individual is in the agricultural sector, self-employed, unemployed, or does not participate in the labor market. The results echo those in Panel A.

C.3 Additional Results: Incumbent Turnover

Export Shocks, Lateral Movement and Promotion: In Table 4 in the main paper, we have worked with an incumbent turnover dummy as the dependent variable when exploring how this is affected by export shocks. In Table C.4, we demonstrate that our results are robust across a range of specifications where the dependent variable is instead a turnover dummy expressed in first-differences.

For ease of reference, Column 1 presents our baseline result from the main text, with $Lateral\ Turnover_{i,t+1}$ as the outcome of interest. Column 2 replaces $Lateral\ Turnover_{i,t+1}$ with $\Delta Lateral\ Turnover_{i,t+1}$ in our baseline specification. Column 3 drops the prefecture fixed effects. Relative to Column 3, Column 4 then additionally controls for the one-period lagged outcome variable in changes (i.e., $\Delta Lateral\ Turnover_{i,t}$), while Column 5 controls for the one-period lagged outcome in levels (i.e., $Lateral\ Turnover_{i,t}$). Across all columns, we see that the coefficient of ExpShock_{it} remains negative and significant even when we use $\Delta Lateral\ Turnover_{i,t+1}$ as the dependent variable. Moreover, the magnitude of this coefficient is relatively stable regardless of the stance that one might take with regard to the specification.

Next, in Table C.5, we present additional results on the relationship between export performance and incumbent movement at the prefecture level. Panel A focuses on how export shocks impact the likelihood of lateral movement of the local party secretary, while Panel B examines how this influences promotion.

[91] A concern is that migrant workers observed in year $t-2$ and prefecture i may attrit in year t due to adverse employment outcomes induced by an export slowdown. In this case, the estimate of α_1 renders a lower bound of the true effect.

Starting with Panel A, Columns 1 and 2 reproduce Columns 3 and 4 in Table 4 in the main paper; these show that in the full sample of observations, a more severe contraction in exports raises the likelihood that the prefecture party secretary is laterally moved, particularly before the three-year mark in their tenure in that position ("early"), which can be viewed as a dent on his/her career trajectory. Columns 3 and 4 in Table C.5 re-run the prior two columns, but limit the sample to the subset of incumbents with fewer than three years' tenure (respectively, at least three years' tenure) in that position. The negative and significant export shock coefficient in Column 3 confirms the finding that weaker exports raise the likelihood of an early lateral movement. In Columns 5-6, we replace the year-on-year export shock variable ($ExpShock_{it}$) with the cumulative export shock over a three-year window centered on year t (i.e., by the sum of $ExpShock_{i,t-1}$, $ExpShock_{it}$ and $ExpShock_{i,t+1}$); we instrument for this with the corresponding cumulative Bartik IV summed over the same three-year window. We find that a negative cumulative export shock appears to raise the prospects of an early lateral movement, although this effect is not statistically significant (Column 5).

Panel B presents an analogous set of regressions to explore how export shocks affect an incumbent's promotion prospects. The export shock in the prior year does not display a significant effect on incumbent promotion, either prior to the three-year mark in one's tenure as prefecture party secretary (Column 7), or once the three-year threshold for eligibility for promotion has been reached (Column 8). This holds both in the pooled sample of all incumbents over the sample period (Columns 7-8), as well as when the sample is limited to local leaders with $Tenure_{i+1,t} < 3$ (Column 9) or $Tenure_{i+1,t} \geq 3$ (Column 10) respectively. When we instead use the three-year cumulative export shock as the key explanatory variable, we uncover a positive and significant effect on promotion at or after the three-year mark (Column 12), but not before (Column 11). The result in Column 12 is consistent with the interpretation that a weak track record with regard to export performance over the course of an incumbent's tenure as prefecture party secretary significantly reduces the likelihood of promotion once he/she becomes eligible (in accord with party guidelines) at the three-year mark.

Lateral Movement, Labor Unrest and Economic Growth: We next explore the relevance of labor unrest for turnover, even when we directly condition on measures of prefecture economic performance. To do so, we consider regressions in which the dependent variable is the indicator for whether the incumbent in prefecture i was laterally moved in year $t+1$.

In Panel A of Table C.6, we relate lateral movement to both the increase in CLB labor strikes, $\Delta(Events/L)_{it}$, and the rest-of-the-world export shock, $ExpShockROW_{it}$, in the same specification. Conditional on the export shock, increases in labor unrest are associated with a rise in the probability that the local incumbent is sidelined (Column 1). When we control for prefecture fixed effects (Column 2), the point estimate remains similar though it becomes marginally insignificant (with a p-value of 0.107). As the number of CLB events could be measured with noise, we instead rank the prefecture-year observations of $\Delta(Events/L)_{it}$, and normalize these ranks so that they are in the range of (0,1]. The estimated coefficients for

the normalized rank measure are stable and statistically significant regardless of whether or not we include prefecture fixed effects (Columns 3-4). We do find throughout Panel A that the export shock coefficients are negative and statistically significant, indicating that weak economic performance can be associated with incumbent turnover. However, the patterns are also consistent with the interpretation that this is not the sole criterion considered by the upper-level government when evaluating prefecture leaders, as the rise in labor unrest continues to explain incumbent turnover even when we condition on the export shock.

In Panels B and C, we repeat the analysis but replace the $ExpShockROW_{it}$ variable on the right-hand side with alternative measures of economic growth, namely: the change in prefecture log GDP (Panel B), and the change in night lights intensity (Panel C). To reduce the noise that could be embedded in these variables, we adopt the normalized rank versions of these measures in Columns 7-8 and 11-12. These broad measures of economic performance are negatively correlated with the likelihood of lateral movement. The implied magnitudes moreover suggest that both weak economic performance and rising labor unrest have a similar importance in explaining turnover. As an example, the estimates in Column 12 indicate that moving a prefecture from the 25th to the 75th percentile of $\Delta(Events/L)_{it}$ is associated with an increase in the probability of lateral movement by 8.2 percentage points. On the other hand, moving a prefecture from the 75th to the 25th percentile of night lights growth is associated with an increase in this probability of 9.8 percentage points. Interestingly, there is narrative evidence from other sources that corroborates this view that social stability and economic growth have a similar degree of importance in determining local political turnover. For example, Pan (2020) describes internal documents that specify the criteria for political advancement from several provinces between 2015 and 2017: prefecture governments can earn a maximum of 350 points in their annual performance evaluations, among which 98 are assigned to economic development, and 77 points are assigned to social stability. (The latter includes 18 points for ensuring social stability and preventing collective action, and 5 points for managing petitions.)

In Figure C.2, we reproduce Figure 5, but using instead a residualized measure of incumbent turnover; this is obtained from a regression of the lateral movement dummy on the normalized rank measure of GDP growth (used in Panel B of Table C.6) and province-year fixed effects, with the 2010 working age population in the prefecture as regression weights. Recall that we first group prefecture-year observations by $ExpShockROW_{it}$ terciles, then by terciles of $\Delta(Events/L)_{it}$. The patterns here resemble that in the original Figure 5. Aligned with the model's prediction, we find that it is the relative performance in managing labor unrest that drives the patterns in the data, after this attempt to strip out variation in turnover that can be explained by economic performance (specifically, GDP growth) per se. In particular, looking within the Bottom export shock tercile, incumbents in the Low strike bin were significantly less likely to be replaced compared to their peers in the High strike bin, which is consistent with their being retained for a good performance in strike management in spite of a bad export shock. Likewise, looking within the Top export shock tercile, incumbents in the High strike bin were significantly more likely to be removed compared to their peers in the Low strike bin,

which is consistent with their being sidelined for poor strike management in spite of being the beneficiaries of a good export shock. Note too that after accounting for this variation that can be explained by GDP growth, the residualized lateral turnover rate for incumbents in the Top export shock and High strike bin is now significantly higher than for incumbents in the Bottom export shock and Low strike intensity bin (p-value= 0.007).

C.4 Additional Results: "Weiwen" and Fiscal Expenditures

Tables C.7-C.9 present additional results on the relationship between export shocks and the intensity of stability-enhancing measures. Table C.7 works with the fiscal measures when these are expressed as shares of total fiscal expenditure, instead of in levels. Table C.8 breaks down social spending into various sub-components. Table C.9 explores for further dimensions of heterogeneity in the response of "weiwen" emphasis and related expenditures to an export shock. Please see Section 5.4 of the main paper for a discussion of these results.

C.5 Robustness: Basic Specification Checks and Sample Period

To ensure that the baseline findings are not driven by the possible presence of influential observations, Figure 3 and Figure C.3 present residual binned scatter plots for the main specifications of interest.[92] Take Panel B in Figure 3 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 W_{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 3 (for the change in labor strikes) and Figure C.3 (for the political outcome variables) confirm the negative relationships detected vis-à-vis the export shock by the baseline regression analysis. It 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 -0.051 and standard error of 0.043). We have furthermore re-run the baseline regressions for each of the outcome variables (i.e., the specifications listed in footnote 92), after dropping the corresponding residualized

[92] Figure 3 is based on the IV specification in Column 3 in Table 1. Panels A-D in Figure C.3 follow Column 2 in Table 4, Column 3 in Table 5, and Columns 1a and 1b of Panel A in Table 6, respectively.

export IV observations that belong in the top and bottom bins, or even in the top and bottom two bins, out of the 50 bins used in the respective binned scatter plots in Figures 3 and C.3. Figure C.4 illustrates the export shock coefficients that are obtained, together with 90% confidence intervals. These confirm that our baseline results are not overly sensitive to these tail observations; the exception here is the "weiwen" emphasis outcome measure, where the export shock coefficient is slightly more imprecisely estimated.

Table C.10 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 C.10 columns should be compared against the baseline IV specifications reported in Column 3 in Table 1, Column 2 in Table 4, Column 4 in Table 5, and Columns 1a and 1b of Panel B in Table 6, 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.[93]

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

[93]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).

variable is Δy_{it}).[94] 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 simply drops the prefecture fixed effects from the baseline specification, without controlling for the lagged outcome variable in either changes or levels. Note that this stacked first-difference model without prefecture fixed effects is similar to a four-period model with prefecture fixed effects where the variables are included in levels. All our results from the baseline specification continue to hold here.

Panel F 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).

Panel G reports a long-difference specification, in which the export shock and the corresponding Bartik IV are constructed as changes over the period 2012 to 2015 (rather than year-to-year changes). The regressions that are run here therefore exploit cross-sectional variation, while controlling for province fixed effects (in lieu of province-year and prefecture fixed effects). Note that the dependent variable in Column 1 is the change in CLB events per million workers between 2012 and 2015; that in Column 2 is an indicator variable for whether the party secretary was laterally moved in 2016; while that in Columns 3-5 is the change in the respective political response measure between 2013 and 2016. We continue to find that the export shock has a negative effect on these outcomes of interest, though the impact on "weiwen" emphasis is now marginally insignificant.

Last but not least, Panel H reverts to our baseline specification in (2), but includes prefecture GDP growth between year $t-1$ and year t as an additional control. This has little effect on both the magnitudes and significance of the estimated export shock coefficients. Though this is reassuring, it should be viewed strictly as a robustness check given that local GDP growth is potentially endogenous.

Turning to the issue of the sample period, we demonstrate that our baseline regression findings continue to hold if we were to extend the time frame under study to span 2012-2017, instead of 2013-2016. (To be clear, since we work with variables in changes, the earliest observation in our sample is now the change between 2011-2012, instead of 2012-2013; the latest observation in our sample is now the change between 2016-2017, instead of 2015-2016. We refer to the years of the analysis as 2012-2017, though it should be understood that data for 2011 are used to calculate annual changes associated with 2012.)

We are unable to extend the sample even further back in time for two reasons. First, the

[94] 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., $y_{i,t-1}$.

data that the CLB has made publicly available on its Strike Map only commences from 2011. In addition, we have had difficulties locating information on local fiscal expenditures on public security and social spending for the period 2008-2010. Coverage of these earlier years is patchy in local statistical yearbooks or government statistical websites, and we can speculate that this opaqueness is not accidental given that these were the global financial crisis years during which various extraordinary budget measures were adopted. Second, by restricting the sample period to the aftermath of the global financial crisis, our estimates are less likely to be confounded by forces associated with the economic stimulus plan implemented during 2009-2010.

Table C.11 reports on the results from the extended sample. Column 1 in Panel B relates the changes in labor strike intensity to export shocks over 2012-2017. As we do not have access to the detailed Chinese firm-level customs data for 2017, we are unable to construct $ExpShock_{it}$ for 2016-2017; that said, we can construct the Bartik rest-of-the-world export shock variable $ExpShockROW_{it}$ for this last year, so we instead run these regressions as OLS reduced-form specifications. To provide a fair point of comparison, we report the corresponding OLS reduced-form regression results for our shorter baseline sample period in Panel A. In Column 1 of Panel B, the estimated coefficient for the effect of the export shock on the change in strike intensity remains negative and significant, albeit with a smaller magnitude. In Columns 2-5, we regress the political outcome variables – lateral turnover, as well as the respective changes in the "weiwen" score, public security spending, and social spending – against the export shock variable; since we use one-year leads for the dependent variable, the sample period is now 2013-2017. Reassuringly, our results on the impact of adverse export shocks on these political responses are broadly preserved.

We do not extend the analysis beyond 2017 for two reasons. First, China's export slowdown after 2017 was largely driven by tariffs levied by a single major trade partner, the US. While we show that the product-level demand shocks in our Bartik variable are as good as randomly assigned in terms of their impact on prefecture locations over the 2013-2016 period (c.f., Table C.12), the discretionary tariffs imposed by the US starting in 2018 are more likely to be "targeted". As a result, the identification assumption we defended could be violated in this more recent period. For example, Ju et al. (2020) have found that the earlier waves of Section 301 US tariff actions against China targeted high-tech products, and we can expect that prefectures with greater concentrations of such industries or of skilled workers might be disproportionately affected. Second, Chinese workers may have reacted differently to the export shock brought about by the US-China trade war: the US tariffs may have been seen as punitive, giving rise to nationalistic sentiment in China, with less blame directed toward local leaders.

C.6 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. (2022), 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. (2022) 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 C.12 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": 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 pre-determined 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 C.13 reports the results of IV regressions with these additional controls.[95] 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.[96] 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 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 C.14 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

[95] 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.

[96] 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.

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.[97] 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 C.15. 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).

C.7 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.

[97] 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.

First, in Panel B of Table C.12, 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. (2022), this delivers consistent standard errors that are not subject to 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 C.16, 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.)

On a separate note, the number of province clusters in our regression is 30, which is the

borderline case discussed in Cameron et al. (2008): When the number of clusters is small, the use of conventional cluster-robust standard errors could lead to over-rejection. To address the concern, Table C.16 also reports the p-value of the two-sided tests based on the wild cluster bootstrap-t procedure detailed in Appendix B of Cameron et al. (2008). In particular, we simulate the distribution of the Wald test statistics based on 999 pseudo-samples that are block bootstrapped at the province level. The p-value is defined as the probability that a random draw from the simulated distribution is more extreme in absolute value than the t-ratio obtained from our baseline clustering protocols (i.e., the cluster-robust variance estimator). It is reassuring to find that the baseline findings are robust under this correction procedure.[98]

C.8 Alternative Bartik Shocks

In this next set of checks reported in Table C.17, 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:

$$\sum_{k} \sum_{d \neq CHN} \frac{X_{idk,2010}}{\sum_i X_{idk,2010}} \frac{\Delta X_{dkt}^{ROW}}{L_{i,2000}}. \tag{C.2}$$

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 (C.2) 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:

$$\ln X_{odkt} = \alpha_1 \ln Dist_{od} + \alpha_2 B_{od} + \alpha_3 Col_{od} + \alpha_3 Lang_{od} + \varphi_{okt} + \varphi_{dkt} + \varepsilon_{odkt}, \tag{C.3}$$

[98]The Monte Carlo simulations in Cameron et al. (2008) show that when the number of clusters is 30, the problem of over-rejection is relatively minor. For example, the rejection rates for tests of nominal size 0.05 is around 0.068 (see their Tables 2 and 3.)

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 (C.3) 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:

$$\sum_k \sum_{d \neq CHN} \frac{X_{idk,2010}}{\sum_i X_{idk,2010}} \frac{\Delta \hat{X}^{ROW}_{dkt}}{L_{i,2000}}, \tag{C.4}$$

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 (C.4).

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

$$\sum_k \frac{X_{ik,2010}}{\sum_i X_{ik,2010}} \frac{\Delta \hat{X}^{ROW}_{kt}}{L_{i,2000}}. \tag{C.5}$$

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 (C.5).

C.9 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 C.18. (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 C.18; 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 C.5, 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 C.18 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 C.1: CLB Map (2012-2015)

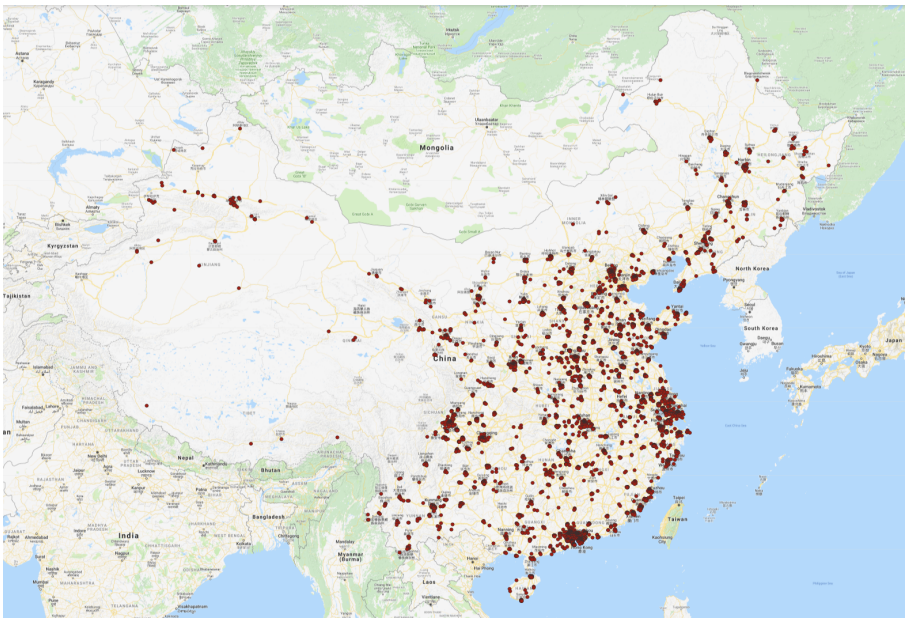
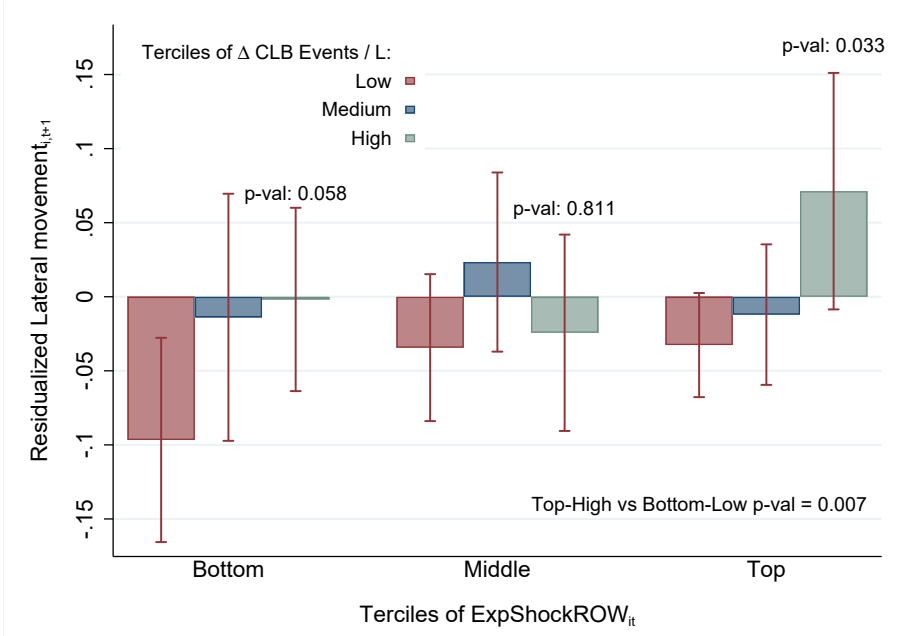


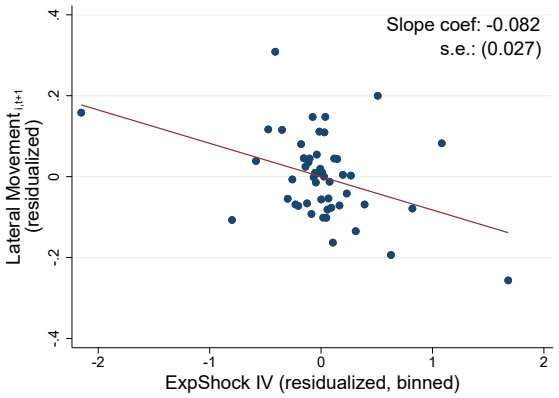
Figure C.2: Export Performance, Strikes, and Incumbent Turnover



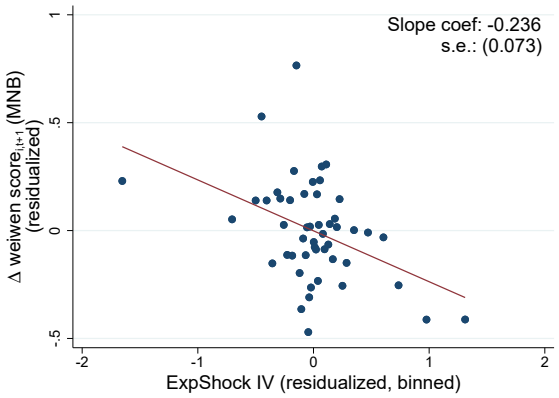
Notes: The lateral movement residuals are obtained from a regression of this outcome variable against the normalized rank measure of prefecture GDP growth and province-year fixed effects (weighted by initial prefecture working-age population). Within the Bottom tercile of $ExpSchockROW_{it}$, the averages of ΔCLB Events per million workers are -0.45, 0.57 and 2.41, respectively, for the Low, Medium and High groups; within the Middle tercile, the averages are -0.27, 0.54 and 1.93, respectively; within the Top tercile, the averages are -0.23, 0.52 and 1.79, respectively. The p-values reported are for t-tests of whether the residualized lateral movement variable is equal in the High versus Low bins of $\Delta(Events/L)_{it}$ (based on a regression of this outcome variable on a set of 3×3 bin dummies, omitting one category, with province-clustered standard errors).

Figure C.3: Binned Scatter Plots: Prefecture Export Shocks, Turnover, and Incumbent Responses (50 bins)

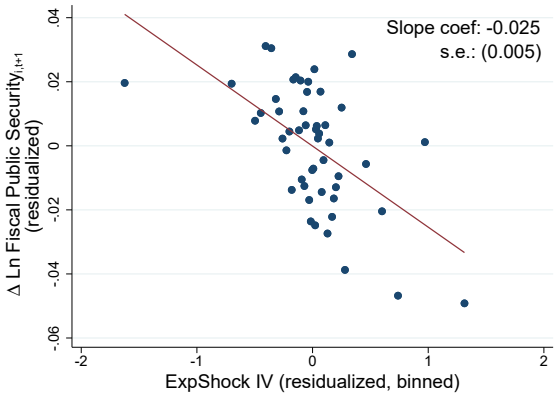
A. Lateral turnover
(based on Column 2, Table 4)



B. Weiwen score, MNB
(based on Column 3, Table 5)



C. Public Security Spending
(based on Column 1a, Panel A, Table 6)



D. Social Spending
(based on Column 1b, Panel A, Table 6)

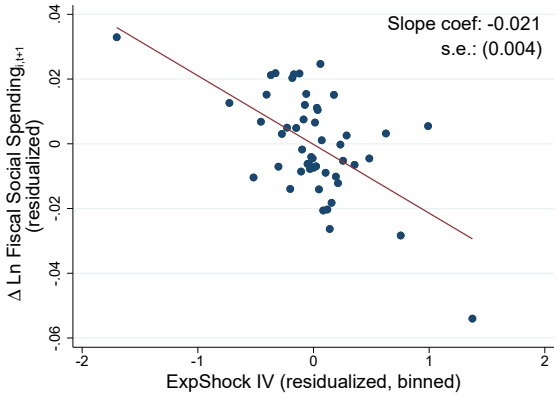
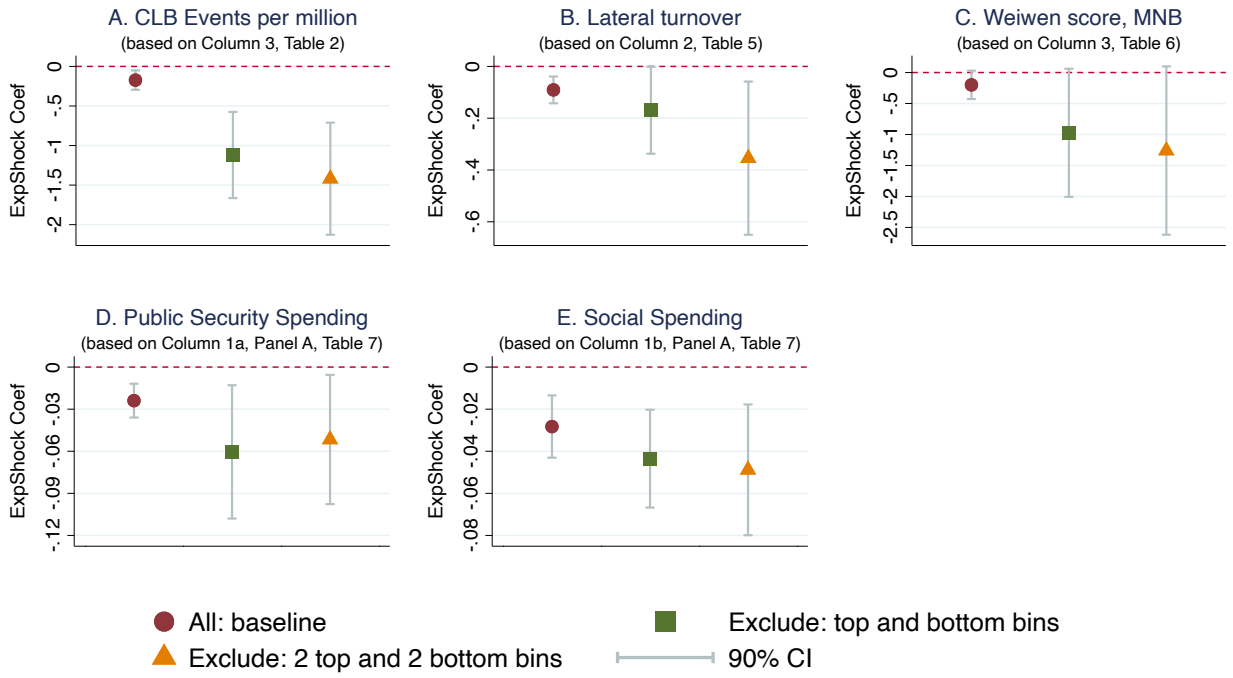


Figure C.4: Robustness: Dropping Observations in the Tail Bins of the Residualized Export Shock IV



Notes: Each panel illustrates the $ExpShock_{it}$ coefficient and corresponding 90% confidence interval that are obtained when observations in tail bins are excluded. Panels A through E re-run respectively the baseline specifications from Column 3, Table 1 (dependent variable: CLB events per million workers); Column 2, Table 4 (incumbent lateral movement); Column 3, Table 5 (MNB "weiwen" measure); Column 1a, Table 6 (public security spending); and Column 1b, Table 6 (social spending). The red circles illustrate the estimates and 90% confidence intervals from the baseline sample, the green squares illustrate these when observations in the top and bottom residualized export shock IV bins (from Figures 3 and C.3) are dropped, and the orange triangles illustrate these when observations in the top and bottom 2 residualized export shock IV bins are dropped.

Figure C.5: Cross-Industry Correlation between Domestic Demand, Domestic Output and Export Shocks

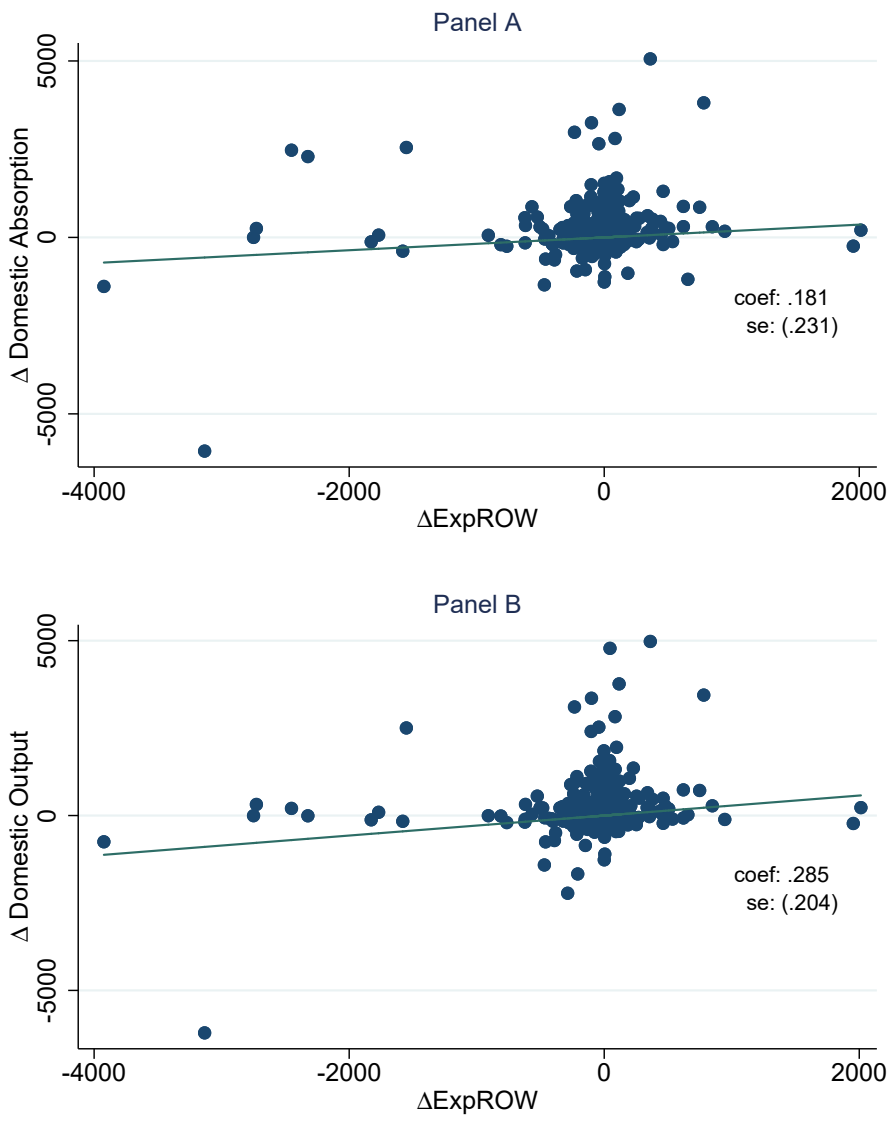


Table C.1: Summary Statistics

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 5.2, 5.3, and 5.4 respectively.

Table C.2: Export Shocks and Labor Strikes: Heterogeneous Effects

Dependent variable:	Δ **CLB Events per million**$_{it}$			
	(1) IV	(2) OLS	(3) OLS	(4) IV
ExpShock$_{it}$	-0.1771*			-0.6410**
	(0.0922)			(0.2808)
Neighboring ExpShock$_{it}$	0.0154			
	(0.1665)			
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)$_{i,12}$×ExpShock$_{it}$				0.7743***
				(0.2759)
Share of State Emp$_{i,10}$×ExpShock$_{it}$				0.2059**
				(0.0831)
Share of Non-Hukou$_{i,10}$×ExpShock$_{it}$				-0.0582
				(0.1887)
Share of College$_{i,10}$×ExpShock$_{it}$				-14.9526***
				(3.2214)
Province-year dummies?	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y
Additional time-t controls?	Y	Y	Y	Y
First-stage F-stat	31.85	–	–	6.81
Observations	822	822	820	813
R^2	0.5266	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 and 4 report IV estimates, while Columns 2 and 3 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 breaks down the export shock into the contribution from firms that exit from exporting versus stayers/new entrants. Column 3 breaks down the contribution of SOEs versus non-SOEs. Column 4 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.

Table C.3: Export Shocks and other Local Economic Outcomes: the CFPS

Dependent variable:	**Income Growth**$_{jt}$			
Sample:	All	High-skill	Mid-skill	Low-skill
	(1)	(2)	(3)	(4)
	IV	IV	IV	IV
ExpShock$_{it}$	0.0612**	-0.0402	0.0571	0.0542***
	(0.0237)	(0.0475)	(0.0396)	(0.0188)
First-stage F-stat	9.12	7.33	5.81	9.68
Observations	30,957	1,821	2,146	26,990
R^2	0.0387	0.2376	0.1941	0.0301
Dependent variable:	**ΔWage-Earning Employment**$_{jt}$			
Sample:	All	High-skill	Mid-skill	Low-skill
	(1)	(2)	(3)	(4)
	IV	IV	IV	IV
ExpShock$_{it}$	0.0841***	0.0360	0.0496	0.0780***
	(0.0259)	(0.0421)	(0.0423)	(0.0217)
First-stage F-stat	9.12	7.33	5.81	9.68
Observations	30,957	1,815	2,141	26,990
R^2	0.0954	0.3488	0.2538	0.0707
Province-year dummies?	Y	Y	Y	Y
Prefecture dummies?	Y	Y	Y	Y
Individual controls?	Y	Y	Y	Y

Notes: Individual controls include six age group dummies (21-25, 26-30,31-35,36-40,41-45,46-50, and 51-55), five educational attainment group dummies (illiterate, primary school, middle school, high school, and college or above), a gender dummy, and an urban dummy. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

Table C.4: Robustness: Change in the Rate of Lateral Turnover

Dependent variable:	**Party Sec. Lateral Turnover**$_{i,t+1}$	**ΔParty Sec. Lateral Turnover**$_{i,t+1}$			
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
ExpShock$_{it}$	-0.0907***	-0.1435***	-0.1082***	-0.0813*	-0.0730***
	(0.0314)	(0.0271)	(0.0242)	(0.0415)	(0.0230)
Province-year dummies?	Y	Y	Y	Y	Y
Prefecture dummies?	Y	Y	N	N	N
One-Period Lagged Δ Lateral Turnover	N	N	N	Y	N
One-Period Lagged Lateral Turnover	N	N	N	N	Y
Additional time-t controls?	Y	Y	Y	Y	Y
Incumbent controls?	Y	Y	Y	Y	Y
First-stage F-stat	116.76	116.77	136.81	135.69	135.85
Observations	821	819	823	823	823
R^2	0.4987	0.4955	0.3343	0.5204	0.6178

Notes: The dependent variable in Column 1 is an indicator variable for whether the party secretary in i was laterally moved in year $t+1$; while that in Columns 2-5 is the change in the indicator variable between year t and $t+1$. Column 4 includes the one-period lag of the dependent variable on the right-hand side (i.e., ΔParty Sec. Lateral Turnover$_{it}$), while dropping the prefecture fixed effects. Column 5 includes the one-period lag of lateral turnover on the right-hand side (i.e., Party Sec. Lateral Turnover$_{it}$), while dropping the prefecture fixed effects. All regressions are weighted by the prefecture's working-age population in 2010. 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. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

Table C.5: Additional Results on Export Shocks, Lateral Movement, and Promotion

Dependent variable:	**Party Secretary Lateral Movement$_{i,t+1}$**					
	Early	**Regular**	**Early**	**Regular**	**Early**	**Regular**
Sample:	All	All	Tenure$_{i,t+1}$ <3	Tenure$_{i,t+1}$ ≥3	Tenure$_{i,t+1}$ <3	Tenure$_{i,t+1}$ ≥3
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A	IV	IV	IV	IV	IV	IV
ExpShock$_{it}$	-0.0834***	-0.0073	-0.1134**	0.0556		
	(0.0205)	(0.0408)	(0.0486)	(0.0947)		
ExpShock$_{it}$: 3-year window					-0.0460	0.0407
					(0.0690)	(0.0391)
First-stage F-stat	116.76	116.76	44.00	3.80	18.76	32.52
Observations	821	821	351	273	351	273
R^2	0.4759	0.5545	0.6341	0.6715	0.6350	0.6763
Dependent variable:	**Party Secretary Promotion$_{i,t+1}$**					
	Early	**Regular**	**Early**	**Regular**	**Early**	**Regular**
Sample:	All	All	Tenure$_{i,t+1}$ <3	Tenure$_{i,t+1}$ ≥3	Tenure$_{i,t+1}$ <3	Tenure$_{i,t+1}$ ≥3
	(7)	(8)	(9)	(10)	(11)	(12)
Panel B	IV	IV	IV	IV	IV	IV
ExpShock$_{it}$	0.0093	-0.0141	-0.0182	-0.0051		
	(0.0060)	(0.0088)	(0.0231)	(0.0231)		
ExpShock$_{it}$: 3-year window					-0.0071	0.0214***
					(0.0154)	(0.0067)
First-stage F-stat	116.76	116.76	44.00	3.80	18.76	32.52
Observations	821	821	351	273	351	273
R^2	0.4759	0.4368	0.6630	0.6124	0.6615	0.6143
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

Notes: In Panel A, the dependent variable for the odd-numbered columns is a dummy for whether the prefecture party secretary experienced an early lateral movement in year $t+1$ having spent less than three years in that position (as of year $t+1$); that for the odd-numbered columns is a dummy for regular lateral movement in year $t+1$ having spent at least three years in that position. In Panel B, the dependent variable for the odd-numbered columns is a dummy for whether the prefecture party secretary experienced an early promotion in year $t+1$ having spent less than three years in that position (as of year $t+1$); that for the odd-numbered columns is a dummy for regular promotion in year $t+1$ having spent at least three years in that position. Regressions are run using all observations in Columns 1-2 and 7-8; Columns 3, 5, 9, 11 restrict the sample to incumbents with less than three years tenure in their current position (as of year $t+1$), while Columns 4, 6, 10, 12 restrict the sample to incumbents with at least three years tenure in their current position. The export shock measure used in Columns 5-6 and 11-12 is the cumulative export shock over the three-year window from year $t-2$ to $t+1$; this is instrumented by the corresponding three-year cumulative Bartik export shock variable. 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 4. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

Table C.6: Lateral Movement, Labor Strikes, and Economic Performance

Dependent variable:	**Party Secretary Lateral Movement**$_{i,t+1}$			
	(1)	(2)	(3)	(4)
Panel A	OLS	OLS	OLS	OLS
Δ CLB Events per million$_{it}$	0.0362*	0.0377		
	(0.0180)	(0.0227)		
Normalized rank: Δ CLB Events per million$_{it}$			0.1474**	0.1644**
			(0.0675)	(0.0773)
ExpShockROW$_{it}$	-0.0560***	-0.0596***	-0.0567***	-0.0605***
	(0.0120)	(0.0203)	(0.0127)	(0.0213)
Observations	839	839	839	839
R^2	0.3190	0.5386	0.3181	0.5390
	(5)	(6)	(7)	(8)
Panel B	OLS	OLS	OLS	OLS
Δ CLB Events per million$_{it}$	0.0395**	0.0409*		
	(0.0181)	(0.0230)		
Normalized rank: Δ CLB Events per million$_{it}$			0.1570**	0.1681**
			(0.0687)	(0.0792)
Δ Log GDP$_{it}$	-0.1878	-0.0932		
	(0.2786)	(0.6294)		
Normalized rank: Δ Log GDP$_{it}$			-0.1255	-0.1414
			(0.0763)	(0.1082)
Observations	839	839	839	839
R^2	0.3008	0.5194	0.3021	0.5205
	(9)	(10)	(11)	(12)
Panel C	OLS	OLS	OLS	OLS
Δ CLB Events per million$_{it}$	0.0390**	0.0402*		
	(0.0183)	(0.0234)		
Normalized rank: Δ CLB Events per million$_{it}$			0.1530**	0.1631**
			(0.0691)	(0.0793)
Δ Log Night Lights intensity$_{it}$	-0.0486	-0.0618		
	(0.0816)	(0.0858)		
Normalized rank: Δ Log Night Lights intensity$_{it}$			-0.1489*	-0.1967**
			(0.0786)	(0.0795)
Observations	839	839	839	839
R^2	0.3008	0.5197	0.3014	0.5221
Province-year dummies?	Y	Y	Y	Y
Prefecture dummies?	N	Y	N	Y
Additional time-t controls?	Y	Y	Y	Y

Notes: The dependent variable is a dummy for whether there was a lateral movement of prefecture party secretary in year $t+1$ (i.e., one year after the export shock). This is regressed against the change in CLB-recorded events per million workers in prefecture i between year $t-1$ and t, as well as the respective measures of changes in prefecture economic performance between year $t-1$ and t. All regressions are 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.

Table C.7: Export Shocks and Fiscal Expenditure Shares

Dependent variable:	Δ **Log (Share of Total Fiscal Expenditure)**$_{i,t+1}$			
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 4. 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 C.8: 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 (5). 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 4. 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 C.9: 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 4. 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.

Table C.10: 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 C.10: 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: Drop Prefecture FEs				
ExpShock$_{it}$	-0.2533***	-0.0728***	-0.1968*	-0.0216**	-0.0263***
	(0.0718)	(0.0227)	(0.1049)	(0.0093)	(0.0082)
(Age≤57)$_{i,t+1}$×ExpShock$_{it}$			-0.2460***	-0.0117*	-0.0250***
			(0.0707)	(0.0058)	(0.0083)
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 dummies?	N	N	Y	Y	Y
First-stage F-stat	127.70	136.79	21.95	21.76	26.35
Observations	825	824	806	818	773
R^2	0.3294	0.2718	0.2045	0.4970	0.4542
	Panel F: 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

Table C.10: Robustness: Basic Specification Checks (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 G: Long-Difference				
ExpShock$_{it}$	-0.6223***	-0.0623*	-0.1742	-0.0309**	-0.0288***
	(0.1626)	(0.0364)	(0.1333)	(0.0120)	(0.0078)
(Age≤57)$_{i,t+1}$×ExpShock$_{it}$			-0.1194	-0.0346**	-0.0431***
			(0.1121)	(0.0166)	(0.0068)
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 dummies?	N	N	Y	Y	Y
First-stage F-stat	22.53	24.44	11.27	8.35	5.52
Observations	272	272	260	269	242
R^2	0.2802	0.3948	0.1840	0.5603	0.5958
	Panel H: Controlling for GDP growth				
ExpShock$_{it}$	-0.1777**	-0.0917***	-0.2461*	-0.0243***	-0.0238***
	(0.0752)	(0.0317)	(0.1257)	(0.0071)	(0.0056)
(Age≤57)$_{i,t+1}$×ExpShock$_{it}$			-0.3135***	-0.0126*	-0.0246**
			(0.0897)	(0.0065)	(0.0089)
$\Delta \log(GDP_{it})$	-1.8071	-0.3461	0.1416	0.2472*	0.1120
	(2.1263)	(0.6032)	(1.7075)	(0.1211)	(0.2287)
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	104.77	116.01	14.25	14.42	19.93
Observations	822	821	801	813	762
R^2	0.5273	0.4985	0.2979	0.6156	0.6144

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, (4) for Column 2, and (5) 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 4, while Columns 3-5 also control for prefecture-tier-by-year dummies as in Tables 5 and 6. 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 drops the prefecture fixed effects. Panel F reports a cross-sectional regression using data from 2015 only; province fixed effects are used in lieu of province-year and prefecture fixed effects. Panel G reports long-difference specifications, in which the export shock and the corresponding instrumental variable is constructed over the period 2012 to 2015; the dependent variable in Column 1 is the change in CLB-recorded events per million workers between 2012 and 2015; that in Column 2 is an indicator variable for whether the party secretary was laterally moved in year 2016; while that in Columns 3-5 is the change in the respective political response measure between 2013 and 2016. All columns in Panel G use province fixed effects in lieu of province-year and prefecture fixed effects. Panel H uses $\Delta \ln(GDP_{it})$ as a further control in the baseline specification. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

Table C.11: Robustness: Different Sample Periods

Dependent variable:	**Δ CLB Events per million**$_{it}$ (1) OLS-RF	**Party Sec. Lateral Turnover**$_{i,t+1}$ (2) OLS-RF	**Δ Log MNB "weiwen" score**$_{i,t+1}$ (3) OLS-RF	**Δ Log Fiscal Public Security**$_{i,t+1}$ (4) OLS-RF	**Δ Log Fiscal Social Spending**$_{i,t+1}$ (5) OLS-RF
			Panel A: Baseline Sample Period		
ExpShockROW$_{it}$	-0.1035**	-0.0541**	-0.1633**	-0.0161**	-0.0160***
	(0.0477)	(0.0222)	(0.0710)	(0.0068)	(0.0056)
(Age≤57)$_{i,t+1}$×ExpShockROW$_{it}$			-0.2124***	-0.0086	-0.0153*
			(0.0387)	(0.0074)	(0.0077)
Data period for outcome variable	2013-15	2014-16	2014-16	2014-16	2014-16
Province 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 dummies?	N	N	Y	Y	Y
Observations	822	821	801	813	762
R^2	0.5192	0.5164	0.3273	0.6134	0.6014
			Panel B: Extended Sample Period		
ExpShockROW$_{it}$	-0.0432**	-0.0489**	-0.0863*	-0.0122***	-0.0135***
	(0.0159)	(0.0231)	(0.0488)	(0.0037)	(0.0038)
(Age≤57)$_{i,t+1}$×ExpShockROW$_{it}$			-0.1129**	-0.0085	-0.0114*
			(0.0410)	(0.0068)	(0.0063)
Data period for outcome variable	2012-17	2013-17	2013-17	2013-17	2013-17
Province 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 dummies?	N	N	Y	Y	Y
Observations	1,621	1,346	1,309	1,331	1,244
R^2	0.4810	0.4294	0.2402	0.5127	0.5241

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. Panel A shows the results of the OLS reduced-form specifications for the baseline sample period, while Panel B reports the reduced-form estimation results for the extended sample period. (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 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 C.12: 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. (2022). 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 4, Column 3 in Table 5, and Columns 1a and 1b of Table 6. Robust standard errors are clustered by HS 2-digit codes. *** p<0.01, ** p<0.05, * p<0.1.

Table C.13: Robustness: Controlling for Time Varying Effects of the "Incomplete Share" (IV specifications)

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: 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 C.13: 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 C.14: Effects of Future Export Shocks

Dependent variable:	**Δ CLB Events per million**$_{i,t}$ (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$_{i,t+1}$	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, (4) for Column 2, and (5) 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 4, while Columns 3-5 also control for prefecture-tier-by-year dummies as in Tables 5 and 6. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

Table C.15: Robustness: Dropping One HS Section at a Time

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
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, (4) for Column 2, and (5) 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 4, while Columns 3-5 also control for prefecture-tier-by-year dummies as in Tables 5 and 6. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1.

Table C.16: 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⟧***
Wild cluster bootstrap-t (Cameron et al. ,2008):					
p-value of two-sided tests:	0.0178	0.0031	0.0451	0.0003	0.0000
(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⟧***
Wild cluster bootstrap-t (Cameron et al. ,2008):					
p-value of two-sided tests			0.0003	0.0550	0.0038
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, (4) for Column 2, and (5) 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 4, while Columns 3-5 also control for prefecture-tier-by-year dummies as in Tables 5 and 6. Robust standard errors are clustered as described in each respective row. *** p<0.01, ** p<0.05, * p<0.1.

Table C.17: 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 C.17: 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 (C.4)				
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 (C.5)				
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, (4) for Column 2, and (5) for Columns 3-5. The alternative Bartik IVs in each Panel are constructed as described in Section C.8. (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 4, while Columns 3-5 also control for prefecture-tier-by-year dummies as in Tables 5 and 6. Robust standard errors are clustered at the province level. *** p<0.01, ** p<0.05, * p<0.1, † p<0.15.

Table C.18: 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 C.18: 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, (4) for Column 2, and (5) 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 4, while Columns 3-5 also control for prefecture-tier-by-year dummies as in Tables 5 and 6. 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.