THE LONDON SCHOOL OF ECONOMICS
AND POLITICAL SCIENCE

Essays in Political Economics of Development

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Declaration

I certify that the thesis I have presented for examination for the MPhil/PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

I declare that my thesis consists of approximately 48,746 words.

Statement of conjoint work

I confirm that Chapter 2 was jointly co-authored with Professor Guy Michaels and I contributed 50% of this work.

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I can confirm that chapters 1 and 3 of my thesis were copy edited for conventions of language, spelling and grammar by Dr. Eve Richards.

Abstract

The collection of three essays study how political factors can shape economic outcomes, with a particular regard to developing countries. My exploration in this direction begins with the government-firm connections and extends to the cause and prevention of internal armed conflicts.

The first chapter examines the reciprocal relationship between governments and firms. The rent-seeking behavior of politically connected firms and its associated costs have long been recognized by economists. The existing literature has mainly focused on the favors that firms receive, but much less attentions has been paid to what politicians gain in return. In my first chapter, titled "**Can Governments Harvest Connections with Firms? Evidence from China**," I provide evidence on the reverse - firms providing favors to governments in a reciprocal relationship - exploiting a natural experiment in China. In October 2001, the tax revenue sharing rule between central and local governments was unexpectedly reformed: the higher the local tax revenue in 2001, the higher the share that local governments would get post-2001. From a newly collected dataset, I find that before the reform the governments that granted more favors to firms - access to credit and tax deductions - were able to mobilize more assistance from firms in order to raise the tax revenue in 2001. Furthermore, this reciprocation is not an institutional relationship, but hinges on a repeated interaction between firms and local leaders. Exploring the variation in leadership turnover, I find that firms who had previously received government favors provided no assistance to leaders who would soon leave office. These results are consistent with a theory of reciprocal relationships between governments and firms. My findings not only suggest that governments and firms can form dynamic relationships to exchange favors intertemporally, but also shed light on the government-business relationship in China.

The last two chapters focus on political violence, which has commonly been regarded as among the first-order issues in developing countries. The second chapter examines whether the endowment of natural resources might lead to internal armed conflict. To examine this question, we ideally require exogenous variation in resource windfalls. This is a challenging task as the quantity of natural resources extracted is a choice and oil prices may be affected by violent conflict. To address this issue, in a published paper co-authored with Guy Michaels at the London School of Economics, titled "**Do giant oilfield discoveries fuel internal armed**

conflicts?," we use new data to examine the effects of giant oilfield discoveries around the world since 1946. We show that the timing of giant oilfield discoveries is plausibly exogenous. We find that on average these discoveries increase per capita oil production and oil exports by up to 50 percent. But these giant oilfield discoveries also have a dark side: they increase the incidence of internal armed conflict by about 5-8 percentage points. This increased incidence of conflict due to giant oilfield discoveries is especially high for countries that had already experienced armed conflicts or coups in the decade prior to discovery.

In the last chapter, I focus on the prevention of internal armed conflict. In order to lessen the likelihood of conflict, one important precaution is to ensure that military resources do not fall into the hands of non-state groups. Observing the recent conflicts in the Middle East, and, in particular, the issue of wide spread of military resources well beyond governments control, one main contributing feature is that governments sponsor weapons to non-state agents (militias) to fight the states enemies, but after the war ends these weapons cannot be called back. In my third chapter, titled "**Proxy Warriors: A Theory of Military Assistance**", I develop a theory to study government's optimal strategy in sponsoring weapons to militias. Although giving militias more weapons may win the war more quickly, it puts more weapons into the hands of militias afterwards. Militias learn that they can benefit from holding more weapons when hostilities end. This incentivizes the militias to fight strategically in order to maximize their subsequent stock of weapons. Given this governmental dilemma, in a dynamic setting I rationalize a supply strategy featured with a stopgap proposed by the US security forces.

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I gratefully acknowledge the financial support from the London School of Economics.

Finally, I would like to dedicate this thesis to my grandparents. They have taught me to form a strong sense of purpose which leads me to the path of becoming an economist.

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Chapter 1:

Can Governments Harvest Connections with Firms? Evidence from China

Yu-Hsiang Lei

It is well-known that governments sometimes favor connected firms. This paper provides evidence on the reverse - firms providing favors to governments in a reciprocal relationship - exploiting a natural experiment from China. In October 2001, the tax revenue sharing rule between central and local governments was unexpectedly reformed: the higher the local tax revenue in 2001, the higher the share that local governments would get post-2001. From a newly collected dataset, I find that before the reform the governments that granted more favors to firms - access to credit and tax deductions - were able to mobilize more assistance from firms in order to raise the tax revenue in 2001. Furthermore, this reciprocation is not an institutional relationship, but hinges on a repeated interaction between firms and local leaders. Exploring the variation in leadership turnover, I find that firms who had previously received government favors provided no assistance to leaders who would soon leave office. These results are consistent with a theory of reciprocal relationships between governments and firms. My findings not only suggest that governments and firms can form dynamic relationships to exchange favors intertemporally, but also shed light on the government-business relationship in China.

1 Introduction

The rent-seeking behavior of politically connected firms and its associated costs have long been recognized by economists. Firms' rents are often generated through preferential treatment by governments, such as, better access to credit or lighter taxation (Fisman (2001), Johnson and Mitton (2003), Dinç (2005), Khwaja and Mian (2008), Ferguson and Voth (2008), Chen (2015)).[1] The existing literature has mainly focused on the favors that firms receive, but much less attentions has been given to what politicians gain in return. Beyond personal rents, such as corruption, there are also other forms of returns. For example, when governments are in need, firms that have received preferential treatments can take actions to help achieve certain policy objectives. The literature on developmental states shows that the assistance from firms to governments is widely observed in developing countries, including industrial development, increasing business investment, the absorption of unemployment and so on. (Evans (1995) and Woo-Cumings (1999)).

This study focuses on what governments get from firms. In the process, it shows that the government-firm relationship is dynamic and reciprocal. In other words, their reciprocal relationship is sustained by the future value of the relationship. Using a unique reform in China which enables me to quantify the value of firms' assistance to governments, I examine the question: do governments gain from firms' assistance through a reciprocal relationship?

To examine how governments can mobilize the informal assistance of firms is a challenging task. Not only are these returns being hard to observe and quantify, but also it is equally challenging to assess whether governments have any incentive to seek this assistance. Therefore, to test this question, ideally, I would focus on governments that share similar political institutions

[1] Furthermore, these rents are highly likely to cause efficiency loss and other social cost; for example, the efficiency losses of channeling business to less productive firms (Cingano and Pinotti (2013) and Amore and Bennedsen (2013)) and other social costs, such as workplace safety (Fisman and Wang (2015),Jia and Nie (2015)

and face a common shock that requires governments to seek assistance from firms. A tax-sharing reform in 2001 between central and local governments in China provides a unique set-up to examine this subject. First, the reform incentivized local governments to raise tax revenue, a quantifiable response that allows me to compare one with another, and its design leaves room for assistance from firms. Second, the reform was announced unexpectedly, which allows me to explore the pre-existing connections between governments and firms. Finally, exploring the variation within a country helps me to hold the underlying political institutions constant.

The central-local tax-sharing reform in question was announced in October 2001 by the central government, which specified that from 2002 local governments were required to share their corporate income tax with the central government.[2] In order to avoid a negative shock to local fiscal budgets, the 2001 local corporate income tax was designed as a benchmark — no local governments will ever receive less corporate income tax revenue than the revenue collected in 2001. That is, the higher the 2001 tax revenue, the more tax could be retained at the local level in future years. This design incentivized local governments to raise the benchmark (the 2001 corporate income tax revenue) during the 2-month window period after the announcement.[3] In both November and December 2001, abnormal tax growth was widely observed among local governments.[4]

How was this jump in revenue attained? One important issue to note is

[2]Here, the term 'local governments' means all non-central governments, including provincial, prefectural and county governments. My focus in this paper is county governments where most of the variation is coming from.

[3]Two incentives for local governments to raise benchmarks are discussed in the section of institutional background and here is the summary: first, the fiscal revenue is a main resource for local economic development, a dimension in which local governments are made to compete against each other for their leaders' career advancement; second, county leaders need to show that they are aligned with those in the prefectural or provincial governments who all intend to keep tax at the local level and also evaluate the county leaders' performance.

[4]A government paper published on January 1st, 2002 stated that corporate income tax increased nationwide by 139.4% for November and 187.1% for December, compared with the tax revenue for the same months in 2000.

that tax revenue needs to be remitted to the Treasury before being redistributed back to local governments. Therefore, the increase in tax revenue could not simply be a fake number. After careful auditing work done by central government, a government paper explicitly stated that local governments raised the 2001 tax revenue through two channels: (1) relabeling other tax revenues and (2) financing with the assistance of firms. In this paper, I first measure firms' assistance and then explore the determinants that explain the variation in firms' assistance.

In order to assist local governments, firms bear the cost of generating a large cash flow in a short time, as well as the risk of being punished by the central government. Given the risks and urgency, we hypothesize that governments are most likely to reach for assistance from firms which they have favored in the past. To test this, I trace the governmental favors to local firms before the reform and then to explore whether governments that granted favors to firms can mobilize firms' assistance in raising the 2001 tax revenue.

The first empirical challenge is to quantify the level of firms' assistance in raising the 2001 tax revenue. In order to do so, I assemble a panel dataset of the corporate income tax revenues between 1998 and 2003 from nearly 500 county governments from various provincial, prefectural and county fiscal or tax yearbooks. On average, the 2001 corporate income tax nearly doubled what it would have been if it had maintained its time trend. However, a great variation was found between county governments, reaching in some instances about 30 times the counterfactual.[5]

The next empirical challenge is to measure the governments' favors towards local firms. Guided by the literature, I focus on two of the most common favors that governments grant to firms in the Chinese context: access to credit and tax deductions. First, preferential access to credit is an impor-

[5]Aba county in Sichuan had the highest response in my sample, reaching almost 30 times what it should have been if it had stayed on its own trend.

tant favor from local governments. This is because all banks in China are state-owned and capital mobility across regions is low, therefore it gives local governments great influence over all decisions to grant loans. Furthermore, many evidences have pointed out that how loans commonly discriminate in favor of state-related firms (Boyreau-Debray and Wei (2005), Li, Meng, Wang and Zhou (2008), Firth, Lin, Liu and Wong (2009)). In addition, others discuss how private firms rely significantly less on loans and more on retained earnings and private lending (Allen, Qian and Qian (2005), Dollar and Wei (2007), Riedel and Gao (2007)). Given the data limitation I use total liability normalized by assets, called debt leverage, to proxy for access to credit. Since the composition of the liability for state-related firms is more likely to be favored loans from the governments, I use debt leverage to proxy a government favor to state-related firms.[6]

The second favor that I consider is preferential corporate income tax deduction. Chinese corporate income tax codes for domestic firms are identical nationwide. But, given its complex nature, the tax code is often manipulated by local governments, which offer tax deductions. Bai, Hsieh and (2014) offers a lively example and discussion. Accordingly, the effective tax rate, defined as the corporate income tax paid divided by the firms' reported tax base, is adopted as a proxy for tax deductions. The tax deduction is a sensitive favor for private firms, but it is not so much sensitive for state-related firms because both of their tax and revenue are part of local governments' fiscal budgets. To summarize, I have two measures for favors: (1) debt-leverage of state-related firms and (2) effective tax rate of private firms. Both measures for favors are constructed using data from China's Annual Surveys of Industrial Production and taking the average for 1999 to 2000, the years before the reform, to represent previous government favors to firms.

[6]State-related firms are defined to include both state-owned enterprises and collectively-owned enterprises which are owned by the rural community but under tight political controls. The firms' classification is discussed in detail in section 4.2.

I take the measure of firms' assistance to examine whether the governments that granted more favors to firms before the reform - access to credit and tax deductions - were able to mobilize more assistance from firms in order to raise the tax revenue in 2001. In a cross-sectional setup, I found that in counties where state-related firms had greater access to credit, county governments can mobilize more assistance from them in raising the 2001 benchmark. The estimated effect is large and statistically significant. A one standard deviation increase in the credit access measure of state-related firms leads to a 0.2 standard deviation increase in the measure for firms' assistance. I find no similar effect for private firms.[7] The results remain robust after controlling for the sales share of the state-related firms in local economy and other firm characteristics, including turnover ratio (firms' capacity to translate assets into sales income), profitability (a proxy for productivity) and local industrial composition. Turning to the second favor, tax deduction, similar results emerge. I find that in counties where private firms enjoyed greater tax deductions, county governments can mobilize more assistance from them in raising the 2001 benchmark. Moreover, no similar effect for state-related firms is found and the results remain robust after controlling for the share of private firms as well as other firm characteristics.

After showing that the governments can acquire more assistance from firms that received more favors in the past, I move on to examine whether this informal relationship is a personal or institutional one. To do so, I assemble another dataset comprising all county-level leadership turnover from all provincial and prefectural yearbooks between 1994 and 2008. This allows me to trace the year in which leaders take office and also the year in which they leave. Different from previous studies, such as Chen and Kung (2016), in which only the county party secretary is recorded, I also trace the county mayor who is directly responsible for local governance affairs, including man-

[7]As an important counterfactual, I show that the same measure of central-state-owned firms, which do not rely on local government favors, has no effect.

aging local fiscal planning. I find that when government leaders were soon to leave office in a few months, the firms that had previously received favors did not assist the government in raising the 2001 benchmark. This result confirms that this informal reciprocity between governments and firms is indeed a personal relationship. This finding is not because leaders lacked the incentive to respond, since county leaders need to show that they are aligned with those in the prefectural or provincial governments who all intend to keep tax at the local level and also evaluate the county leaders' performance. This is confirmed since leaving leaders are still found to relabel other taxes to finance their responses in the benchmark. In addition, I use leaders' tenure at the end of a term to proxy the likelihood of their leaving to show that the results for leaving leaders are not driven by reverse causality, i.e. leaders' leaving as a consequence of their responses to the reform. Finally, I find that firms that previously enjoyed greater favors from previous government are also more likely to take the initiative to assist new leaders in order to build up a new reciprocal relationship to gain future government favors.

To interpret the full set of results, I present a simple model of the reciprocal relationships between governments and firms in an infinite horizon. In my model, both governments and firms are forward-looking and they choose whether to exchange favors with each other. To align with the empirics, governments now request for favors from firms. Firms then need to decide whether to offer favors in exchange for future gain from reciprocity or not to offer favors and be punished by losing the reciprocal relationship with governments. The model suggests that if firms have previously received favors from governments, then a reciprocal relationship between governments and firms exists. In this case, firms would choose to return favors to governments. Furthermore, in a stationary environment the firms that have received more favors from governments previously would be the ones that also expect a higher value from future reciprocity. As a result, these firms are likely to

offer more favors to governments. Two predictions arise from the model: (1) that governments can mobilize more firms' assistance if firms have in the past received more favors from them. (2) when the probability of government turnover is high, previously favored firms do not return favors to governments since the current official will not be there in the future to return them.[8]

Literature review

The findings of this paper contribute to the literature on political connection by extending the discussion to include the perspective of governments or politicians. Due to its secrecy in nature we know very little about how much and what are the prices paid by the firms in exchange for benefits. These payments can be in simple forms of cash transfer that accrue to personal gain, such as corruption, but it can also be in forms to support connected politicians, such as through campaign contributions to provide electoral advantage (Sukhtankar (2012)) or through expanding employment to garner votes (Bertrand, Kramarz, Schoar and Thesmar (2006), Cole (2009), Dinç (2005)). The findings of this paper add to this small literature suggesting that the forms of payment do not have to be monetary but rather it can be all sorts of costly actions to meet governments' demand. Furthermore, this is the first study to provide the evidence in showing that the two-way reciprocal relationship can also exist in an authoritarian regime.

The findings of the informal and dynamic interaction between governments and firms also contribute to the empirical literature of informal relational contracts (see Gil and Zanarone (2016)). In particular, the breakdown of reciprocity due to leaders' leaving suggests that this informal contract is a long-term relationship that is enforced by anticipating future gain from staying in the reciprocity. There are a few empirical literatures that study the informal contracts between firms in the developing country context (Mac-

[8]I extend the theoretical framework in appendix A to understand firms' choice to assist local governments when leaders just arrived in office without pre-existing relationships with firms. It predicts that firms that relies more favors from governments would still assist new leaders in order to enter a new reciprocal relationship.

chiavello and Morjaria (2015), Banerjee and Duflo (2000), McMillan and Woodruff (1999)). This paper contributes to this growing literature in development and shows that the same informal relationship can also run between governments and firms.

Finally, this paper is related to the vast literature on fiscal federalism. While most studies (see Oates (1999)) focus on the benefits and the costs associated with decentralizing fiscal authorities. This paper depicts the political struggle between central and local governments when a recentralization needs to be reformed.

The remainder of the paper is organized as follows. Section 2 describes the institutional background to the central-local government tax-sharing reform, local government institutions and government favors to local firms. Section 3 provides a simple theoretical framework to organize the empirical results. In Section 4 I provide details of the data set and variable construction. In Section 5 I present the baseline empirical results and robustness checks. In Section 6 the heterogeneous effects of local leadership turnover is presented. In Section 7 I discuss how the findings can help us understand the government-business relationship in China and in section 7 I draw conclusions and policy implications.

2 Institutional Background

In this section, I begin by discussing the 2001 Chinese central-local tax-sharing reform (subsection 2.1). This is followed by introducing the underlying political institution (subsection 2.2), and lastly I discuss local governments' favors that go to firms (subsection 2.3).

2.1 The 2001 Central-local Govn't Tax-sharing Reform

Corporate income tax was an important local government fiscal resource before 2002. The tax rate is 33% on profit income and local governments do not have the authority to alternate the tax rate. All corporate income tax had been retained to local governments as a major fiscal resource, which accounted for about 11% of the local tax revenue in 2000, with a 25% annual growth rate.

In October 2001, central government unexpectedly announced a nation-wide reform to partially centralise income tax in order to develop the Western part of China. It states that from 2002 corporate income tax would be shared between central and local governments.[9] The central-local sharing ratio began at 50:50 in 2002 and changed to 60:40 from 2003 onwards.[10] To avoid a negative shock to local fiscal budgets, the 2001 tax revenue was designed to be a benchmark. Such that whenever the share that stayed at the local fell below the benchmark, the central government would refund to local governments the difference between the benchmark and the local shares. That is, the higher is the 2001 tax revenue, the more tax would remain at the local level post-2001. This specific design incentivized local governments to increase the 2001 tax revenue in November and December of 2001 right after the announcement of the policy. The local governments' responses to the reform was illustrated in a government paper published on January 1, 2002.[11]

"*After the announcement of the 2002 corporate income tax sharing reform in October this year, local corporate income tax had an* ***abnormal growth*** *... tax revenue of November 2001 increased by 139.4% compared with the same time last year ... and 187.1% for the first half of December... Areas*

[9]The term "local governments" here refers to all non-central government bodies, including provincial, prefectural and county governments (see Figure 11)

[10]The way in which the fiscal revenue-sharing system works can be found in the Han and Kung (2015).

[11]The same reform also applies to personal income tax with a similar reaction from local governments but smaller in extent. In this study I focus on the corporate income tax.

with more than 100% growth during mid-December are as follows: Jiangxi increased by 816%, Ningbo city increased by 708.7%, Henan increased by 609%... Do not raise the benchmark purposely..."

Due to the extreme responses from local governments, the central government abandoned the 2001 benchmark. This was replaced by a function of the corporate income tax revenue in year 2000. Using annual corporate income tax from about 500 county governments and including GDP per capita as controls, in Figure 1 I plot the estimates of year dummies from 1998 to 2003. It clearly shows that the 2001 estimate deviates from the time trend. However, there is no associated increase in local economic activities. This is shown in Figure 2.A in which the total value-added tax (VAT) in 2001 has no associated deviation, nor the local firms' aggregate sales income, shown in Figure 2.B. This confirms that the abnormal deviation in 2001 corporate income taxes is indeed a response to the tax-sharing reform.

An important issue to note is that all the tax revenue needs to be remitted to Treasury before being redistributed back to local governments. Therefore, local governments cannot simply fake the number. In fact, the central government sent auditors to check these local governments' abnormal responses.[12] They find out how local governments raised the tax revenue mainly through two channels which are stated in the same government paper: (1) relabeling tax revenue and (2) financing from firms' assistance. The first channel does not need the assistance of firms but simply relabeling other tax revenue as corporate income tax.[13] However, operations through the second channels require assistance from firms. The logic behind the scene can be best described as follows: in any other normal times, shown in Figure 3.A, firms

[12]Some local governments were fined for having raised tax revenues on purpose. Their fine was recorded in the provincial aggregates published in the 2002 National Tax Yearbook.

[13]Despite that it is a simple accounting exercise for governments, it can be very costly since this doing can easily be detected by the central government. Therefore, as I will show later in the empirical section it only explains about 18% of the rise in the benchmark and possibly leaders utilize this channel more only when firms are less willing to assist.

pay tax to local governments and the revenue is then remitted to Treasury before being redistributed back to the governments. However, in November and December 2001 as shown in Figure 3.B, firms paid tax and also other transfers. The money was again remitted to Treasury and then came back to the governments. Governments kept the tax and returned the extra money back to the firms. As shown in Figure 4, using firm survey data I find a consistent evidence that firms were not paying abnormally high corporate income tax in 2001. In sum, these patterns suggest that no real tax is being paid but simply move money around and relabel them as corporate income tax.

2.2 Political Institution

2.2.1 Using County as the Unit of Analysis

In this study, I use county governments as my unit of analysis for the following reasons: First, there is a great variation in how much the 2001 benchmarks are raised among county governments. Second, this is the lowest level of administration, therefore firms located in the county have to deal directly with this level of government.[14] Finally, I use firm location to match which governments firms deal with the most. However, this does not apply to firms registered under district governments because they are likely to operate in another district within the same prefecture. Furthermore, firms located in the district should value their relationship more with the prefectural governments, rather than with the district governments. This makes the county and district governments incompatible in many ways. Therefore, I only focus on county governments in this study. In the next, I discuss the governments' leadership and the sources of leaders' incentives to remain the tax at local.

[14]The governmental hierarchy is shown in Figure 11.

2.2.2 Government Leadership

Governments throughout the political hierarchy in China are running a dual-leadership system. In counties, the county mayor is the *de jure* leader and responsible for all governance affairs while the county party secretary leads the county to obey the Communist Party rule. There are no explicit rules on how their responsibilities should be distributed. In practice, the party secretary should be the *de facto* leader since the position enjoys a higher rank within the Party. Given the ambiguity in their roles in the government, when I examine the heterogeneous effects of leadership turnover, I take into account both county mayor and county party secretary. Another reason for this innovation is because when the party secretary leave the office, it is likely to promote county mayor to take over the party secretary position. In this case, there will be a continuity in the government leadership.

Regarding local leaders' turnover, both mayor and party secretary have a *de jure* term which is fixed for 5 years but their *de facto* terms are determined by bureaucratic assignments. Based on my constructed turnover data, their terms on average is about 4 years and more than 80% of them leave the office within 5 years with a peak around 4 to 5 years.

2.2.3 Promotion As An Incentive Driver

Both leaders of county governments who manage to show competence in developing local economies are rewarded by promotion within the hierarchical political system. This is done by making local government leaders compete against each other for career advancement; for example, county leaders compete against each other for promotion in entering prefectural government. This jurisdictional yardstick competition has long been recognized, as in Maskin, Qian and Xu (2000) and Xu (2011), and is supported by empirical evidence, such as Chen (2015), Jia, Kudamatsu and Seim (2015), and Li and Zhou (2005). It is this urge to develop economically which incentivizes

county leaders to raise the 2001 benchmarks to keep fiscal resources local. In addition, their personnel evaluations are usually compiled by governments in the ranks immediately above. For example, county leaders in my study are evaluated by prefectural governments, which also try to keep tax local. This puts in place another incentive for county leaders to respond to, as to cater for those who evaluate them.

2.3 Proxies for Government-Firm Relationship

To empirically identify firms that are connected to governments is particularly difficult. Most literature on political connections uses family ties or personal contacts to identify firm-specific connections. However, these methods do not apply here for two reasons: one, to conduct a business at the grassroots level, such as counties, some relationship (called *Guanxi* in Chinese) with government officials is necessary. Therefore, *Guanxi* is not binary but rather a relationship that varies in intensity. Second, I need an county-level measure for Guanxi in order to analyze county governments' aggregate responses. To address these two concerns, instead, I measure previous favors from governments to firms as a proxy for the value of the connection between them. This method gives a varying measure and enables me to aggregate at the county level. To do so, I focus on two of the most common and measurable favors that governments grant to firms in Chinese context - access to credit, and tax deductions.

I begin by discussing preferential access to credit. In China all banks are state-owned and capital mobility across regions is low. This gives local governments a great deal of power over decisions to grant loans. Therefore, as is widely recognized, capital in China is misallocated between firms. This has received significant attention in the literature, such as Hsieh and Klenow (2009) and Dollar and Wei (2007). In particular, literature, such as Firth et al. (2009), Li et al. (2008), Boyreau-Debray and Wei (2005), have pointed

out that state-related firms are much easier to access credit through external finance comparing with private firms which mostly rely on retained earnings and private lending (Dollar and Wei (2007), Riedel and Gao (2007) and Allen et al. (2005)). In addition, Song, Storesletten and Zilibotti (2011) also points out that private firms are financially repressed and as a result their capital-output and capital-labor ratios are substantially lower. These studies suggests that more government preferential loans would go to state-related firms. Therefore, I use the favor of granting access to credit as government favors for state-related firms.

The second favor to be discussed is preferential corporate income tax deduction. Chinese corporate income tax codes for domestic firms are identical nationwide. It had a flat rate at 33% before 2008 but a large grey area for deduction. Therefore, given the scope in which tax code can be manipulated, local governments often offer tax deductions. A lively discussion can be found in Bai et al. (2014). However, for state-related firms both their profit or tax need to be remitted to local governments, which makes state-related firms less sensitive to tax deduction as a favor. Therefore, I use corporate income tax deduction as government favors for private domestic firms.

3 Theoretical Framework

Political connections can take various forms; here, I define connected firms as those receiving preferential treatments from governments. Therefore, I model the governments' mobilization of firms' assistance in a reciprocal relationship, following Kranton (1996). Government (G) and Firm (F) are two risk-neutral agents exchanging favors in an infinite time horizon. For simplicity, I assume their favors to be identical and non-storable. Furthermore, favors are informal and no contractual agreements are possible. In each pe-

riod, agents take turns to offer and then to request for favors.[15] In this study, I examine the link between previous favors to firms and firms' help to governments in responding the reform. To align with the empirics, I assume that Firm is the first to request for favors, followed by Government. Firm needs a favor, x, from Government, where x is randomly drawn from a distribution $F(\cdot)$ with support $[0,\ \bar{x}]$.[16] Government who gives favors x incurs a private cost $c(x) > 0$, where $c'(x) > 0,\ c(0) = 0,\ and\ c(\bar{x}) < \bar{x}$.[17] The announcement of the reform serves as a realisation of favors needed by Government from Firm. Government therefore turns to Firm to request for favors as Firm previously did. Once a period is over and before a new period starts, nature determines that one of the two agents request for a favor before the other and then the game proceeds as in the previous period. I assume for probability $\theta > \frac{1}{2}$ Firm would be the first to request for favors and $(1-\theta) > 0$ for Government. Their per half period discount factor is $\delta > 0$. Furthermore, since this relationship between governments and firms is informal, it is more likely to be a personal rather than institutional one. Therefore, it is most likely that this relationship is a repeated interaction between government leaders and firms. That is, the leaders' leaving would affect how Firm expect its value from future of reciprocity. I assume the probability of leaders' staying is $\pi \in [0,\ 1]$ and it is only realized the half period before.[18] This suggests that whether leaders are leaving or not will only be known in the previous half period.

Let $V_R^F = \theta V_1 + (1-\theta)V_2$ and $V_R^G = (1-\theta)V_1 + \theta V_2$ be the expected lifetime

[15]There are various kinds of favors demanded by the local governments. The detailed discussion can be found in Section 7. Here in the framework I omit the discussion on the initial cause of the reciprocal relationship. Instead, I focus on this reform which would allow us to study this dynamic relationship.

[16]For simplicity, we assume that Government and Firm share the same distribution, $F(\cdot)$ with the same support $[0,\ \bar{x}]$. This can be easily to extend to have Government and Firm drawing their favor request from different distributions.

[17] I assume that the cost function is bounded above by the favor to ensure that it is beneficial in the expectation of staying in a reciprocal relationship. It is a reasonable assumption since many favors are costly because of the checks and balances by the institution rather than because of their inherent cost.

[18]This assumption is justified since county-level leaders' turnover are based on bureaucratic assignment without a fixed term. Furthermore, they often learn the decision at the very last minute.

discounted utility (continuation value) from their reciprocal relationship for Firm (F) and Government (G) respectively, where V_1 and V_2 are the expected value of those who request for favors first and second. V_1 is the expected utility when agent k requests for the favors first:

$$V_1 = \int_0^{\bar{x}} z dF(z) + \pi\delta \int_0^{\bar{x}} -c(\omega) dF(\omega) + (\pi\delta)^2 V_R^k,\ k \in \{G,\ F\} \qquad (1)$$

The first term is the expected favors received and the second term is the expected favors provided in the next half period before entering the next period to receive continuation value. V_2 is the expected utility when agent l requests for the favors second:

$$V_2 = \int_0^{\bar{x}} -c(\omega) dF(\omega) + \pi\delta \int_0^{\bar{x}} z dF(z) + (\pi\delta)^2 V_R^l,\ l \in \{G,\ F\} \qquad (2)$$

As a symmetry to the first agent to request for favors, the first term is the expected favors provided and the second term is the expected favors received in the next half period before entering the next period to receive continuation value. As I discuss earlier, here I assume that Firm is the first to request for a favor, I solve for $V_R^k,\ k \in \{G,\ F\}$, using (1) and (2). This gives

$$V_R^F(\bar{x}) = \frac{1}{2(1-\pi\delta)} M(\bar{x}) + \frac{(1-\pi\delta)(2\theta-1)}{2(1-\pi\delta^2(2\theta-1))} L(\bar{x}) \qquad (3)$$

$$V_R^G(\bar{x}) = \frac{1}{2(1-\pi\delta)} M(\bar{x}) - \frac{(1-\pi\delta)(2\theta-1)}{2(1-\pi\delta^2(2\theta-1))} L(\bar{x}) \qquad (4)$$

where $M(\bar{x}) = \int_0^{\bar{x}} (z - c(z)) dF(z)$ and $L(\bar{x}) = \int_0^{\bar{x}} (\omega + c(\omega)) dF(\omega)$. Since I assume $\theta > \frac{1}{2}$, then I have $V_R^F(\bar{x}) > V_R^G(\bar{x})$. This is mainly a result of time discounting, since Firm is always likely to be the earlier one to request for favors. The parameter θ thus captures the degree to which Firm is needy, in relation to Government. The continuation value for Firm, $V_R^F(\bar{x})$, is increasing in θ, while the continuation value for Government, $V_R^G(\bar{x})$, is decreasing

in θ.

3.1 A Reciprocal Relationship as An Equilibrium

Since a reciprocal exchange happens intertemporally, agents can choose whether to renege on offering favors. Therefore, I study conditions in which a reciprocal relationship is enforceable, a subgame perfect equilibrium, so that no one will have an incentive to renege on offering favors and to apply a punishment whenever someone does. First, a grim-trigger strategy can be a credible punishment: whenever an agent reneges on providing favors, the other agent will no longer provide favors. This is credible since when one agent stops offering favors, the best response for the other is to do the same. In this equilibrium, value for both agents will become 0. After establishing the outside option from the punishment, I turn to working out the conditions in which Government and Firm are willing to participate in a reciprocal relationship. Since I assume that Firm is the first to request for a favor, for Government to stay in a reciprocal relationship the following condition needs to be satisfied.

$$-c(\bar{x}) + \pi\delta \cdot 0 + (\pi\delta)^2 V_R^G \geq 0 \tag{5}$$

This condition states that if the worst possible payoff of Government, Firm request for $\bar{x}$ and then Government request for 0 favor and followed by the continuation value, is still greater than the payoff of never entering, a reciprocal relationship, 0, Government will choose to participate a reciprocal relationship and exchange favors with Firm. For Firm to stay in a reciprocal relationship, the following condition needs to be satisfied.

$$0 + \pi\delta \cdot -c(\bar{x}) + (\pi\delta)^2 V_R^F \geq 0 \tag{6}$$

If condition (5) for Government is satisfied, given that $V_R^F(\bar{x}) > V_R^G(\bar{x})$,

the condition (6) will also be satisfied since Firm is closer to requesting for a favor. This condition states that if the worst possible payoff of Firm, Firm requests for 0 favor and then Government requests for $\bar{x}$ and followed by the continuation value, is greater than the payoff of never entering a reciprocal relationship, 0, Firm will choose to participate a reciprocal relationship with Government. Therefore, I have the following prediction:

Prediction 1. *(Reciprocal Relationship) If governments have previously given favors to firms, the favored firms will return favors to the governments.*

If firms have previously received favors from governments, then a reciprocal relationship between governments and firms is an equilibrium. This gives rise to Prediction 1 which implies that county governments can find assistance from firms, which have previously received favors, to raise the 2001 benchmark. Furthermore, since local governments are raising as much resources as they can, this suggests that governments request for favors at its upper bound, i.e. $x = \bar{x}$. In the setup, I assume the distribution $F(\cdot)$ in which favors were drawn from is time-invariant. Therefore, the higher is *previous* favors from governments to firms, in a stationary environment, the higher is the expected value of *future* reciprocity for firms, which leads to a higher $\bar{x}$ to be requested by governments while the reciprocal relationship can still be sustained. As a result, the variation of previous favors from governments to firms can capture the variation in $\bar{x}$ across counties. This gives the following proposition.

Proposition 1. *County governments which previously granted more favors to local firms can now harvest more assistance from those previously favored firms*

Therefore, empirically I expect that the measures for favors to firms should be positively correlated with a firms' assistance in the 2001 benchmark.

When Firm decides whether to give favors to Government or to renege, it takes local leadership turnover into account, in particular when leaders are soon to leave. A reciprocal relationship is sustained because agents can gain from future reciprocity, as suggested in (5) and (6). If leaders are about to leave, the value of future reciprocity will not be received. This suggests that condition (6) will no longer hold and Firm will renege on its promise to return favors. This leads me to the following prediction.

Prediction 2. *(Reciprocity Without Future) When leaders are about to leave shortly, leaders will not be able to find assistance from previously favored firms.*

Empirically I expect that when leaders are leaving the office, previous favors to firms no longer have an effect on firms' assistance measure in the benchmark. At the same time, this prediction also suggests that the leaving leaders have to resort to its outside option - relabeling from other tax revenue.

Corollary 1. *(Resorting to Outside Option) When leaders are about to leave shortly, without the assistance from firms leaders can only relabel from other taxes.*

Empirically I expect that the leaving leaders would still relabel other tax to increase the benchmark and possibly do more with this channel. In the appendix A, I turn to look at leaders who have newly arrived in office to interpret my findings for new leaders. In the next section, I discuss the details of datasets and how I define and construct variables used in the empirical analysis.

4 Data on Fiscal, Economic, and Political Variables

In order to study local governments' mobilizing assistance from firms, I use data from 476 counties across China. In this section, I discuss my main data

sources and the details on variable construction. The descriptive statistics of variables are summarized in Table 1.

4.1 Corporate Income Tax and Other Fiscal Data

To measure local government responses in the 2001 benchmarks, I need a panel dataset of county-level corporate income tax revenue both pre- and post-2001. However, all the published sources are either aggregates at provincial level or have been openly available since 2001. Instead, I construct a dataset that uses first-hand data collected from various local tax and fiscal yearbook.[19] In total, I have a sample of 476 counties across China out of about 2,000.[20] The samples lie within 112 prefectures across 25 provinces. The map of counties, in which data is available, is shown in Figure 12. The reason why data for other counties are not available is because governments before 2001 combined the profits of state-owned enterprises (SOE) and corporate income tax as one bookkeeping item. Before 2001 it was only for idiosyncratic reasons that the public was able to observe corporate income tax, for example, if local tax authorities decided to publish their own Yearbooks or fiscal authorities published more detailed records for idiosyncratic reasons. The concern of sample selection will be addressed in the robustness check of section 5. In addition, all other county fiscal revenue, county-level nominal GDP and population are constructed from the *Provincial, Prefectural and County Fiscal Statistics*. To control for a possible difference in the incentive to respond caused by local fiscal burden, I construct fiscal pressure for each county which is defined as fiscal expenses divided by fiscal revenue. In the following section, I measure my main outcome variable, the firms' assistance in the 2001 benchmark.

[19]The data sources are listed in the Appendix.

[20] I exclude Tibet, Xinjiang and Qinghai from the analysis.

4.1.1 Measuring Assisted Corporate Income Tax Deviation (ATD)

In order to measure the assistance from firms to raise the 2001 corporate income tax, I take three steps: 1) I construct the counterfactual 2001 corporate income tax to measure the total deviation; 2) I then calculate the amount from relabeling other taxes and net out from the total deviation; 3) I finally normalized the measure with counterfactual corporate income tax. The details of each steps are explained in the following. While all other economic variables grew in a quadratic time trend, the deviation of 2001 corporate income tax from its own trend can be taken as a measure of the local governments' responses. In order to do so, using a panel data on corporate income tax from 476 counties from 1998 to 2003, I regress corporate income tax on individual quadratic time trends and include Ln GDP per capita as a control but exclude year 2001. For county i at time t,

$$y_{it} = b_0 + \omega_1 t + \omega_2 t^2 + \sum_i (\gamma_{1i} t + \gamma_{2i} t^2) \cdot \beta_i + \beta_i + \delta ln\_GDPPC_{it} + u_{it},$$
$$t = 1998....2003 \; but \; t \neq 2001$$

My specification is particularly flexible and allows each county to follow its own trend.[21] Using the above regression, I predict the counterfactual 2001 corporate income tax as if there were no distorted responses from local governments. The residuals for 2001 are my measure for the overall response. Using the residuals as an outcome variable, I plot the coefficient estimates on year dummies in Figure 5. This shows how large the 2001 deviation is in absolute terms. However, the governments' responses are from two channels as shown in Figure 6, relabeling other taxes and firms' transfer, as pointed

[21] One obvious alternative is log-linearity. There are two reasons why it is not prefered: first, I intend to use the deviation from trend as the governments' responses in absolute level. However, by taking log the absolute level of deviations will have their magnitude changed. Second, by taking log the variation of my main outcome variable, relative ATD, will be repressed because more than 50% of the observations have this measure exceeds 1.10, the threshold in which log transformation becomes to repress the variation greatly.

out by the central governmental paper. In order to construct a measure that is purely from firms' assistance, I proceed as follows: first, I show that governments indeed relabeled other taxes as part of the responses; second, I subtract the relabeled taxes from the total responses so that the remnants of responses are from firms' assistance.

As shown in Figure 7.A, I find that all other taxes, the sum of business tax, agricultural tax and other fees, experienced a negative deviation from trends.[22] Figure 7.B plots the relationship between the positive deviation in the 2001 benchmark and the negative deviation of other taxes. The negative correlation suggests that the government relabeled other taxes to finance the jump observed in the corporate income tax in 2001. Their significant correlation is also formally tested in a regression later in the robust section. The channel of relabeling other taxes accounts for about 18% of the average total corporate income tax deviation. I now construct my main outcome variable, a measure of relative ATD from firms' assistance, which is calculated by first subtracting relabeled tax revenue from 2001 corporate income tax and then divided by the predicted counterfactual. For county i,

$$\text{Relative ATD}_i = \frac{\text{2001 Corporate Income Tax}_i - \text{Relabeled Other Taxes}_i}{\text{Counterfactual 2001 Corporate Income Tax}_i}$$

If there were no firms' assistance, the relative ATD measure is expected to be distributed with a mean close to 1. As shown in Figure 8, the distribution has a long right tail and is skewed above 1 with a mean of 1.5 and standard deviation of 0.85. The measure ranges from 0 to almost 30 times the predicted counterfactual. In my sample, the largest relative ATD is from Aba County in Sichuan, as shown in Figure 9. It reaches almost 30 times the counterfactual. After constructing the main outcome of interested, relative ATD, in the next subsection I turn to explain how to construct measures for previous favors to

[22]Business tax is approximately 17% of local tax budgets. It is a tax that applies to taxable services, transferring intangible assets, or selling immovable properties. The sum of all these tax accounts for 64% of local tax revenue.

firms as my main regressors.[23]

4.2 Ownership and Measures for Favors

All firm-related variables are constructed using the Annual Surveys of Industrial Production. The dataset contains universal firms that have annual sales above 5 million RMB (eq. to 800,000 USD) from 1999 to 2003. Detailed information for each firm is recorded, including their location, industry code, paid-in capital composition, liability, assets, sales income, taxable income, payable income tax, and etc. Next, I begin by classifying each firm according to ownership, whether state-related or private. This is followed by constructing the measure for favors - debt leverage for credit access and effective tax rates for tax deduction.

4.2.1 Classifying Ownership

In my samples, the number of surveyed firms in each county ranges from 1 to 765 with a mean of about 73. I classify each firm into state-related and private domestic firms according to each firm's paid-in capital composition.[24] I define a firm to be local state-related if 50% or more of its paid-in capital is coming from the local state and collective capital.[25] Under this definition, state-related firms include both local state-owned enterprise and collectively-owned firms. On average, 50% of the surveyed firms are state-related under this classification. I apply the same method when I calculate for private firms. A firm is defined as private if 50% or more of the paid-in capital consists of

[23]According to the 2002 government paper, the reform was announced unexpectedly. However there is still concern for the information to be leaked before the announcement and so the local governments would raise the tax revenue long before October in 2001. But to bias the response estimate, the governments need to know the reform before 2001 since the fiscal data is annual. Given that there is a similar experience in local response in 1994 VAT sharing reform, the central government would only try harder to avoid leakage.

[24]There are 6 types of capital – local state, collective, private, legal person, central state, and foreign. Here I focus on local domestic firms which have more than 50% of their capital coming from the first 4 categories.

[25]Collective capital is a type of capital collectively owned by the residents of the town or village and managed by the local council. Therefore, the use of collective capital is tightly controlled by local politicians.

private and legal person capital.[26] On average, 43% of the surveyed firms are private according to the samples. The remaining 7% of firms are consist of central-state-owned firms and other foreign controlled firms.

After classifying firms by these two forms of ownership, I calculate their respective shares in the county. To assist governments in raising the 2001 benchmarks promptly, the firms' liquid assets matter more than their illiquid assets. Therefore, instead of using firms' total assets, I use their sales income to calculate the relative size for each ownership. The size of state-related firms in a county is dividing the total sales income of state-related firm by the county's total sales income. That is, for the surveyed firm j in county i,

$$\text{Size of state-related firms}_i = \frac{\sum_{j \in i} I(\text{State-related firms}_{ji}) \cdot \text{Sales income}_{ji}}{\sum_{j \in i} \text{Sales income}_{ji}}$$

Similarly, this method is also used to calculate the relative size of private firms. The average relative size of state-related firms is about 0.60 while private firms are smaller on average at about 0.32. Their variation is similar: private firms' standard deviation is at 0.23 and at 0.26 for state-related firms. Their shares sum up to 0.9 on average.[27] I next construct the measure for each favor for various forms of ownerships.

4.2.2 Measures for Favors

To measure the first favor - firms' access to credit - given the data limitation, I do not observe firms' total loans but only their balance sheets are available. Therefore, I take firms' liability to proxy their access to loan and normalized by their assets. This measure is to be called debt leverage in this paper. I calculate aggregate debt leverage for each county. This measure is constructed by summing up the total liabilities and then normalizing by the total assets of all the surveyed firms in the county. For all surveyed firms j

[26] The legal person capital is capital contributed by registered organizations.

[27] The remaining firms mainly consist of central-state-owned firms and foreign-owned firms.

in county i,

$$\text{Debt leverage}_i = \frac{\sum_{j \in i} \text{Liability}_{ji}}{\sum_{j \in i} \text{Assets}_{ji}}$$

The debt leverage measure has a mean of 0.7 with a standard deviation of 0.16. I repeat the same exercise and calculate the measure for each ownership. While the average debt leverage for state-related firms is 0.71, the same measure for private firms is lower at approximately 0.59. The variation is greater for private firms with standard deviation at 0.26 and at 0.18 for state-related firms. This measure has a very different meaning for state-related firms as compared to for private ones. As discussed in the institutional background, state-related firms are more likely to finance through bank loans while private firms have to rely on private lending. Therefore, the debt leverage ratio should capture the variation of the state-related firms' access to credit as a government favor but this is not the case for private firms.

The second favor is corporate income tax deduction and is measured using an effective corporate income tax rate, defined as the ratio of reported tax paid to a reported tax base. I calculate the average of the effective tax rate for each county. For the surveyed firm j in county i,

$$\text{Effective corporate income tax rate}_i = \frac{\sum_{j \in i} \frac{\text{reported tax paid}_{ji}}{\text{reported tax base}_{ji}}}{\#\text{ of surveyed firms}_i}$$

The effective tax rate is low: an average of about 5% with standard deviation at 0.05. I also calculate the average effective tax rate for each ownership. The mean of effective tax is 0.053 for private firms and 0.047 for state-related firms. Their standard deviations are 0.054 for private firms and 0.042 for state-related firms.

4.2.3 Other Firm Characteristics

In order to control for local firms or industry characteristics, for each ownership I also construct their respective turnover ratio, defined as total sales income divided by total assets, to capture firms' efficiency associated with assets utilization; and their profitability, defined as profit divided by total assets, to proxy for productivity; and finally industry composition based on sales income from resources, manufacturing and utility industry. All the firm-related variables, including both measures of favors, are averaged from 1999 and 2000, before the reform in 2001. Not only this construction avoids reverse causality but it also captures the variation before the reform, i.e. the pre-existing government-firm relationship.[28]

4.3 Data on Local Political Leaders

In order to construct the tenure in office of local political leaders, I collect the names of local politicians from the Chinese provincial and prefectural Yearbook, which records the list of local politicians.[29] Tracing their names over time allows me to record the year in which they took office and the year in which they leave. Specifically, I collect the names of the top two leaders, the county party secretary and the county mayor, from 1994 to 2008.[30] In addition to these leaders' tenure, I can also learn if the county party secretary was promoted from mayor office in the same county.

To examine how the effect of favors changes with leadership turnover, I construct indicators for counties with leaders just starting office and those with leaders who are soon to leave. Given the unique dual-leadership system

[28] Another construction is to trace the leaders' tenure and take the average for the tenure period. The challenge here is that the tenure is often greater than 2 years, i.e. stating before 1999. Alternatively, I can use only values in 2000 since most leaders are unchanged and given that the relationship is a dynamic variable the most recent approximation makes even more sense. Despite the results are not reported in the paper, all the main results are robust to this alteration.

[29] The list of politicians names is based on those who are in position on the last day of the year.

[30] A caveat is that some Yearbooks, the earlier ones in particular, do not document the name list and therefore I do not have data on leaders' turnover for some counties.

in China as I discussed in the institutional background, I construct an indicator for counties with new leaders as follows. The indicator takes a value of 1 if both conditions are satisfied and 0 otherwise:

1. County party secretary began office in 2001, not promoted from mayor.
2. County mayor began office in 2001.

Out of 441 observations where the new leader indicator is not missing, about 34 (8%) county leaders had just taken office. Similarly, I construct an indicator for counties with both leaders leaving. This indicator takes a value of 1 if both conditions are satisfied and 0 otherwise:

1. County party secretary leaves office in 2002.
2. County mayor leaves office in 2002, not promoted to party secretary.[31]

Out of 440 observations where the indicator of leaving leaders is not missing, about 34 (8%) counties have leaders due to leave in the next few months. The indicator for leaders at the end of a term takes value of 1 if both leaders have stayed in the office for 4 years or more.[32] Out of 441 observations where the end-of-term leader indicator is not missing, about 40 (9%) both county leaders had stayed in the office for 4 years or more.

5 Empirical Analysis

This section begins by discussing my empirical specifications (subsection 5.1). I then discuss the estimates of each of the two favors. The first is state-related firms' access to credit, measured by debt leverage (subsection 5.2). This is followed by the second favor, private firms' tax deduction, measured by

[31]Long and Yang (2016) also follow this innovation in this paper and find that leaders' leaving has an negative effect on firms' charitable behaviour.

[32]Regarding local leaders' term, both mayor and party secretary have a *de jure* term which is fixed for 5 years but their *de facto* terms are determined by bureaucratic assignments. Based on my constructed turnover data, their terms on average is about 4 years and more than 80% of them leave the office within 5 years with a peak around 4 to 5 years.

effective tax rate (subsection 5.3). After the baseline estimation, robustness checks are provided (subsection 5.4).

5.1 Empirical Specification

In order to examine if county governments can mobilize connected firms' assistance in raising the 2001 tax benchmark, I use data from 476 counties in 2001 to estimate the following specification:

$$y_{ik} = \beta_0 + \gamma Z_{ik} + \beta_k + \beta' X_{ik} + u_{ik} \tag{7}$$

The outcome variable, y_{ik}, is the relative ATD of county i in prefecture k. Z_{ik} is the main regressor, the favors from local governments towards firms - credit access measured by debt leverage or tax deduction measured by effective tax rate. β_k is the prefectural fixed effects. A set of controls are included in X_{ik}. The first control is previous fiscal pressure which help to hold the governments' incentive to response fixed so that I can focus on governments' capacity to mobilize firms' assistance. Other controls include previous corporate income tax (normalized by GDP) and Ln GDP per capita in 2001.[33] Robust standard errors are clustered at the prefectural level. Given the concern over extreme outliers, I exclude observations with its measure of relative ATD at the top 1%. In the robustness checks, I also estimate the effects using quantile regressions.

5.2 Assistance from Favored Firms: Credit Access

In this subsection, I discuss the main results. I start by looking at whether counties with a higher local state-related firms' credit access, as measured in debt leverage, governments can find more assistance from state-related firms to raise the 2001 benchmark. That is, whether counties with higher previous

[33]The results are robust to include Ln GDP as a control instead.

debt leverage for firms would raise the 2001 benchmark more. As shown in column 1 of Table 2, the coefficient estimate for all firms' previous debt leverage is positive and significant. However, as pointing out earlier that not all bank loans should be treated as favors. Bank loans tend to favor local state-related firms. That is, empirically I expect that state-related firms' debt leverage should matter but not so for that of private firms. This conjecture is tested as shown in column 2 of Table 2. The coefficient estimate for state-related firms' previous debt leverage is positively significant and its magnitude is similar to the estimate in column 1 — one standard deviation increase in the firms' previous debt leverage increases the measure of relative ATD by about 0.2 standard deviation. This is a non-trivial effect and suggests that state-related firms which previously had more access to credit are more likely to assist governments. The same estimate for private firms' previous debt leverage is small and insignificant. This result confirms the first proposition proposed in the theoretical section: that governments which gave more favors to local firms in the past can *harvest* more of their assistance to respond in the reform. Furthermore, in column 3, I show that debt leverage matters only for local state-related firms and not for central-state ones, which have a small and insignificant estimate. This is important in that it suggests that only firms seeking to establish a relationship with local governments matter. In column 4, I show that this result is robust even after controlling for the relative sales share of state-related firms. This insignificant estimate for the relative size of state-related firms seems surprising and counterintuitive. One might expect that state-related firms would be under local government control and counties with relatively larger state-related firms would be able to mobilize more resources in raising the 2001 benchmark. This result suggests that the informal relationship is even more important than the given institutional bonding between governments and state-related firms.

Recognizing that there may be unobservables biasing my results which

cannot be addressed using a cross-sectional framework, I show that in Table 3 the point estimates starting from no controls in column 1 to full controls in column 4 stay significant and most importantly they all share similar magnitude. This suggests that concerns for bias due to unobservables perhaps are less of an issue. In the next subsection, I discuss my second favor, corporate income tax deduction.

5.3 Assistance from Favored Firms: Tax Deduction

In the previous subsection, I establish a positive relationship between state-related firm's access to credit and firms' assistance in the 2001 benchmark. Here I turn to another government favor: corporate income tax deduction. I expect private firms to be more sensitive to this favor and so in counties with more tax deduction for private firms, measured using an effective tax rate, governments can mobilize more private firms' assistance to raise the 2001 benchmark. Empirically, I expect a negative relationship between the previous effective tax rate for private firms and the relative ATD. In column 1 of Table 4, I show that the average effective tax rate has a negative effect on the relative ATD, but it is imprecisely estimated. Since private firms should be more responsive to this favor, I separate the tax rate calculation according to firms' ownership. The result is shown in column 2. The estimates of state-related and private firms are very different: while the significant estimate for private firms is large and negative, the same estimate for state-related firms is close to 0 and insignificant. The large estimate for tax rate of private firms suggests that one standard deviation decrease in effective tax rate is associated with a 0.14 standard deviation increase in the relative ATD. These results suggest that a lower corporate income tax rate for private firms has a positive effect on raising the 2001 benchmark. In column 3 it is shown that this result is robust after controlling for the relative sales share of private firms. Furthermore, when adding debt leverage for state-related

firms in column 4, the estimates of both favors are significant and the estimate for tax rate does not seem to change much. This result establishes that the two favors work independently. These results suggest that governments previously providing more corporate income tax deductions to private firms were able to receive more assistance from them.

As in the previous subsection, to address the concerns that unobservables may bias the results, in Table 5 I show the effect of effective tax rates from no control in column 1 to full control in column 4. The point estimates stay significant for all regressions and again share similar magnitude. These results again mitigate me the concerns of bias due to unobservables. To further validate the baseline results, I discuss robustness checks in the following subsection.

5.4 Robustness Checks

5.4.1 Characteristics of Local Firms

To capture government favors to firms, I use credit access and tax deductions. However, the variation of these two favors may be due to other unexplained characteristics of local firms or industries. In order to address this concern, I include additional controls of firms' characteristics to test the robustness of the baseline results.

The first one is the asset turnover ratio, defined as total sales divided by total assets, which is a measure for firms' ability to use their assets to generate sales or revenues. The second one is the profitability, defined as total profit divided by total assets, which not only measures firms' capacity to generate profit but it is also a widely used proxy for productivity. My last control is the industry composition based on three industries — resource, utility and manufacturing industry— following classification from the firm survey data. All three variables capture important characteristics of local firms that may be associated with credit access and tax deduction. When calculating

turnover ratio and profitability, I separate these variables according to their ownerships, state-related and private. The results are presented in Table 6.

I first examine the effect of debt leverage by adding each of these controls. In column 1 and 2, the estimates of state-related firms' debt leverage remain significant and the magnitude are very close to the baseline result in column 2 of Table 2. When controlling for industry shares as shown in column 3, the estimate remains significant but marginally smaller comparing with the one in the baseline. When turning to look at the effect of effective tax rate as shown in column 4 to 6, all three estimates are almost identical to the baseline estimate in column 2 of Table 4. Despite that the estimate in column 5 is imprecise estimated in column 5, the other estimates when controlling for turnover ratio in column 4 and industry composition in column 6 both remain significant. Overall these results suggest that my baseline findings are not driven by unexplained characteristics of local firms.[34]

5.4.2 Quantile Regressions

In the previous section, I excluded the observations with top 1% relative ATD to address the concern of outliers. Alternatively, I can estimate the effect using quantile regressions. In Table 7, I show the effects at the 25, 50 and 75 percentiles. In column 1 - 3, the estimates of the debt leverage of state-related firms are smaller than the baseline estimate which evaluates at mean but remain significant for 25 and 75 percentiles. Furthermore, the effect is much greater at the higher percentile – the estimate at the 75 percentile is more than double comparing with its counterpart at the 25 percentile. This result indeed suggests that the magnitude of the estimate at mean is somewhat driven by larger values but the coefficients remain significant even at a lower percentile. The results for effective tax rate give a very similar pattern as shown in column 4-6. The estimate is significant at the 75 percentile and

[34]The significantly negative estimates of utility industry will be explained in section 5.4.4.

more than double comparing with the estimate at the 25 percentile. This result suggests that even though the effect is much larger in counties with greater ATD but my baseline findings are not driven by the outliers.

5.4.3 2002 Tax Revenue and Selection Bias

One worry concerning about the relative ATD measure is whether the following year tax revenue were also affected by this reform and led to my findings. To address this issue, I repeat the same exercise as how I measure the outcome variable as regression (7) but this time I not only drop observations of 2001 but also those of 2002.[35] I then repeat the calculation in (8) to construct this alternative outcome variable. The results are shown in column 1 and 2 of Table 8. Despite that the effect is marginally smaller for debt leverage of state-relater firms and larger for effective tax rate of private firms, the results are consistent with my baseline findings.

My sample covers a third of all counties in China, 476 out of 1,600. Despite that the sample size is limited, it has a wide geographical distribution, including 112 prefectures across 25 provinces. I previously discussed that the limited data availability is due to idiosyncratic reasons, for example more detailed bookkeepings. But in order to fully address this concern for sample selection, I examine whether my baseline results can hold robust when conditioning only on 4 provinces that covers most or all counties, including Anhui, Hubei, Sichuan and Zhejiang. The results are shown in column 3 and 4 of Table 8. Using a subsample of only 209 counties, the effects are slightly larger for both debt leverage of state-relater firms and effective tax rate of private firms comparing with those in the baseline but without significant difference. These results relieve my concerns for selection bias.

[35] If I drop observations of both 2002 and 2003 when measuring relative ATD, the measure becomes incredibly noisy and therefore I compromise by dropping only those of 2002.

5.4.4 Firms That Need No Reciprocity Provide No Assistance

In the previous section, I discuss that firms provided assistance to governments when they rely on government favors. Another option to test this proposition is to check whether firms which do not rely on reciprocity to gain local government favors provide no assistance to raise the 2001 benchmark. In my firm survey data, firms are classified into resource, manufacturing and utility industries. The first two industries rely heavily on local governments' facilitations, such as targeted infrastructure, land and so on. Even though firms in the utility industries share same needs but they do not rely on reciprocal relationship with local governments to gain those favors.[36] For example, local electricity supply firms are managed by both a giant electricity agglomeration and a local government. However, the political power of the electricity agglomeration outweighs that of local governments since it enjoys a higher Party rank. Therefore, local governments need to cater for those firms in the utility industries rather than forming a reciprocal relationship with them. I expect that the larger is the utility industries, local governments would get less assistance of firms.

In column 5 of Table 8, I show that counties with greater sales share of utility industries have lower relative ATD. The estimate for the share of a utility industry, -1.024, is negative and significant. This magnitude suggests that when there is one standard deviation increase in the share of utility industries, it has a nontrivial effect, of 0.2 standard deviation decreases in the relative ATD. This allows me to conclude that when firms do not rely on reciprocal relationship to gain local government favors, they do not provide assistance to governments.

[36]This is because firms in the utility industries have a unique administrative structure, called "Tiao-Kuai" (in Chinese). Put simply, this means that firms under this administrative structure have two principles. Giant state-owned firms or bureaucracies of the central government which have vertical lines of authority to coordinate functions, while local governments share horizontal lines of authority to coordinate according to the needs of the locality being governed.

5.4.5 Firms' Assistance Or Strengthening Tax Enforcement?

In the previous section, I claimed that firms receiving tax deductions as a favor from governments will assist in raising the 2001 benchmark. Nonetheless, a similar result can also emerge if local governments renege on previously promised low tax rates and raise the tax to statutory level based on the legal tax code. If this alternative mechanism existed, the rise in the 2001 benchmark would be due to tax enforcement rather than a result of firms' assistance. However, given that governments collected more tax, one should observe an abnormal growth in the tax revenue reported by private firms. In Figure 10 I plot private firm tax revenue over time. It does not show that in 2001 private firms paid abnormally high tax and this result allows me to exclude this alternative explanation. After establishing the baseline results, in the next section, I discuss the heterogeneous effects of local leadership turnover.

6 Dynamics of Reciprocity

6.1 Previous Favored Firms under Leaving Leaderships

Following my baseline results and the associated robustness checks, I confirm the existence of reciprocal relationships between governments and firms. In this section I move on to test whether this informal reciprocal relationship is personal or institutional. In order to do so, I focus on the scenario in which leaders are soon to leave office as predicted by my theoretical framework — when leaders are leaving, in the absence of future benefit, previously favored firms will be reluctant to assist in raising the 2001 benchmark.[37]

[37]One alternative hypothesis is that because a leaving leader would more likely to appropriate due to the lack of accountability, similar to the term limit effect on the policy choices (Besley and Case (1995)). This is likely to be another channel - a channel of coercion. This is in fact an empirical question. Despite my results do not rule out this possible story but they seem to suggest that the reciprocity channel dominates the coercion one.

To start with, as discussed in the data section I define the counties with leaving leaders as counties with both county party secretaries and county mayors leaving office in 2002 and where the mayor is not promoted to party secretary in the same county. In testing this prediction, I assume that whether both county party secretary and mayor leave immediately in 2002 is predetermined and also public knowledge, in particular for local firms. This assumption is very likely to hold, since the reform was announced at the end of 2001 and an assigning order should already have been placed for those due to leave in a few months' time.[38] In Table 9, I show that counties with both leaders leaving and other counties are similar, except that the share of private firms is slightly larger for counties with leaving leaders.

In Table 10, I look at whether the effect of favors is diminished when leaders are soon to leave. In column 1, the estimate of the leaving-leader indicator is negative but imprecisely estimated. However, its non-trivial magnitude of the estimate suggests that a lower response may be due to limited firms' assistance. As results in column 2 and 3 shows, counties with their leaders soon to leave do not have assistance from state-related firms which previously received credit access as government favors. In fact, in these counties the effect of debt leverage is significantly lower than its effect in other counties and the sum of estimates for interaction and main effect for debt leverage is not significantly away from 0 with p-value around 11. The null effect of debt leverage for counties with leaving leaders suggests that governments cannot find assistance from previously favored state-related firms.

When I turn to the favor of tax deduction, the results are similar to those for the favor of access to credit. In column 4 and 5, the result also shows that counties with leaving leaders do not have private firms' assistance even if those firms have previously received favor of tax deduction. The estimate

[38]Leaders' term is 5 years but leaders can still be reassigned before the term finishes. More than 80% of local leaders leave within the 5-year term with a peak at 4 years. Unfortunately, I cannot explore the variation of leaders' assignment because the logic behind the personnel management inside the Party remains a black box.

of effective tax rate in these counties is significantly higher from the one for other counties. Furthermore, the insignificant sum of estimates for interaction and main effect for effective tax rate suggests that the favor of tax deduction has no effect in counties when leaders are soon to leave; that is, governments cannot reach for assistance from previously favored private firms. These results, along with those on the favor of credit access, suggest that leaving leaders are less likely to find assistance from previously favored firms, which is consistent with my second prediction.[39] Therefore, I can conclude that this informal reciprocal relationship is personal rather than institutional.

6.2 Robustness Checks

6.2.1 Lack of Incentive or No Assistance from Firms

In the previous section, I showed that firms previously receiving favors do not assist governments in return when leaders are soon to leave office. However, in addition to the explanation which states that firms choose not to provide assistance when they do not expect future returns, it could also be the case that leaving leaders lack the incentive to respond to the reform. To address this concern, I argue that there is an institutional reason on why leaders are still incentivized to respond even when they are leaving. This is because raising the 2001 benchmark shows their alignment with the leaders in the prefectural governments who evaluate the county leaders' performance for promotion and also intend to keep tax local.[40]

Furthermore, I use two observations to demonstrate that leaving leaders still have the incentive to respond. First, as shown in column 1 of Table 11, overall relative deviation from counties with leaders soon to leave office are no different from other counties.[41] Second, as suggested by the *Corollary*, I

[39]Furthermore, in the appendix B I find no evidence that my heterogeneous effects are driven by either of the leaders' leaving and suggest that both leaders matter.

[40] This is supported by the fact that prefectural governments also have abnormal responses in raising the 2001 benchmark.

[41]Overall relative deviation includes transfer from both channels - relabeling other tax resouces and finance

show that when leaders are soon to leave, other taxes were still relabeled as corporate income tax to be used to raise the 2001 benchmark .

In previous discussion on constructing the main outcome of interest, firms' assistance, in Figure 6 I show the strong negative correlation between relabeling other taxes and the overall deviations in the 2001 corporate income tax revenue. I now turn to formally testing this relationship using regression and the results are shown in Table 11. In column 2, this negative relationship is shown to be robust and significant after adding the full set of controls and prefectural fixed effects. On average, relabeling other taxes accounts for about 18% of the total deviations. Furthermore, in columns 3 to 4, I show that when leaders are leaving, this channel is still utilized because the interaction is negative and insignificant. More importantly, despite that the interaction is imprecisely estimated, the effect of deviation of other taxes when leaders are soon leaving is close to 1 ($\approx 0.144 + 0.850$) with a 0.49 p-value when testing the sum equals to 1. This result suggests that this channel becomes the dominate source to raise the benchmark when leaders are leaving. This result not only demonstrates leaders' incentive to respond even when they are leaving, but it also implies that, as suggested by the *Corollary*, the leaving leaders resort to relabel more from other taxes when the firms do not provide assistance.

6.2.2 Leaders' Leaving Could be Endogenous

In the above analysis, I use ex post information on whether leavers actually leave or not to construct indicators for leaders soon to leave. However, whether leaders leaving or not could be a result of raising the benchmark and so the use of ex post information may create a problem of endogeneity. To address this concern, I use the fact that the reform was unanticipated so that I can treat leaders' time in office in 2001 as an exogenous variation. I use

from firms.

the leaders who are close to the end of their *de jure* term, fixed at 5 years, to proxy the likelihood of their leaving.[42] I construct an indicator that takes a value of 1 if both county party secretary and mayor are at the end of their term by 2001, having served 4 years or more, and 0 otherwise.[43] Using this newly constructed indicator, I repeat the same exercise as those in Table 10. The results are shown in Table 12.

In column 1, counties with end-of-term leaders' relative ATD on average are similar to other counties. In column 2 and 3, I look at the heterogeneous effects of leaders' leaving on credit access for state-related firms, measured in debt leverage, on relative ATD. The results are similar to those in Table 10 in which the favor has no effect when leaders are expected to leave. Despite that the negative estimate of interaction is imprecisely estimated, its magnitude is close to the main effect for debt leverage and thus makes the sum almost 0. In column 4 and 5, a similar pattern is observed when looking at the effect of tax deductions for private firms, measured in the effective tax rate. While there is no effect from tax deduction when leaders are expected to leave judging by the sum of interaction and main effect for effective tax rate, other counties have a large and significant effect similar to those in column 5 of Table 10. My results show that the findings in the previous subsection are robust to this alternative definition of leaders due to leave. After testing my second prediction, I now turn to discuss my additional findings.

6.3 Previous Favored Firms under New Leaderships

In this section, I look whether firms that receive favors from previous governments will also assist local governments under new leadership. New

[42]The 5-year term is not fixed, for leaders can still be reassigned before the term finishes. More than 80% of local leaders leave within the 5-year term with a peak at 4 years. Unfortunately, I cannot explore the variation of leaders' assignment because the logic behind the personnel management inside the Party remains a black box.

[43]Since the mayor is likely to be promoted to party secretary, to proxy the likelihood of both leaders' leaving I also ensure the current party secretary was not previously promoted from local mayor.

leaders should have built limited reciprocal relationships with local firms. Therefore, this reform provides a unique opportunity to test if firms that were relied on favors from governments whether these firms are more likely to assist new leaders and thus to build up a relationship with leaders for future reciprocity. In the appendix A, based on my theory of reciprocal relationship, I modify the model and let firms to take the initiative and decide whether to assist local governments. The theory predicts that firms which rely more on government favors would be more willing to assist governments.

I first discuss the variation from an indicator for new leaders. It is likely that some unobservable factors which determine the turnover of local leadership also affect how much they the local leaders respond. However, as in my construction of an indicator for end-of-term leaders, I use the fact that the reform was unanticipated to treat those counties with new leaders as if they were determined exogenously. Here, counties with new leaders are defined as counties with both county party secretaries and mayors taking office in 2001 where the party secretary was not promoted from the mayor in the same county. In Table 13, I show that counties with both new leaders and other counties are very much balanced in all aspects. Now I turn to examine my empirical findings, shown in Table 14.

As shown in column 1, there is no significant difference in relative ATD between counties with new leaders and other counties. In column 2 and 3, I interact the state-related firms' debt leverage, as favors of credit access, with the indicator for new leaders. The result suggests that state-related firms which enjoyed credit access as favor under previous leadership will also assist new leaders. In column 4 and 5, I find that despite that the effect may be discounted but private firms that previously enjoyed greater tax deduction also assist new leaders. Overall my results suggest that firms that previously relied on government favors will also take the initiative to assist new leaders in order to build up a new reciprocal relationship to exchange for favors in

the future.[44]

7 Discussion: the government-firm relationship in China

The findings of this paper that local governments are in need of assistance from firms are especially important when it comes to explain the government-business relationship in China. I focus on three Chinese institutional features to discuss this implication. First, as previous papers (Maskin et al. (2000), Li and Zhou (2005), Xu (2011)) have pointed out, this is a country that uses many policy goals and quotas from the top to manage lower-tier governments and sets local governments to compete with each other to advance the careers of the leaders. Second, in order to achieve the sometimes unreasonable policy objectives or to compete against others, local governments often need to rely on informal assistance from firms, for example achieving economic indicators, absorbing unemployment and cutting down polluting emissions, and so on. In this paper, the need from firms' assistance to boost up corporate income tax revenue is another example of this kind. Third, not only local governments need assistance from firms, but they also have many resources to offer as an exchange. The decentralizing institution endows local governments with control over business resources, including capital, land and other public goods (Li et al. (2008), Firth et al. (2009), Bai et al. (2014)). Taking these features together, local governments are highly incentivized to build up crony economies due to the institutional design. While the previous literature mainly focused on the first and the third aspects, this paper is one of the first studies to offer systematic evidence on the second and thus to complete this picture in explaining the rise of institutionally-caused crony capitalism in China.

[44]Furthermore, in the appendix B I find no evidence that my heterogeneous effects are driven by either of the leaders' leaving and suggest that both leaders matter.

8 Conclusion

To better understand how connections work and thus their benefits and costs, in this study I use another perspective to look at government-firm connections through their two-way interactions. Using an event study in China, instead of focusing on firms' gain, I examine whether governments can mobilize more assistance from those firms to which they have already given favors. But it is not easy to do so, since favors are usually traded in secrecy or simply cannot be quantified. Yet this event study not only enabled me to measure the value that governments (or politicians) attribute to connections but also broadened my understanding of other possible forms of return. I summarize my findings as follows.

In order to respond to a central-local tax-sharing reform, I found a robust positive correlation between government favors to firms before the reform, credit access and tax deductions, and firms' assistance to governments in responding the reform. Furthermore, I found that this reciprocal relationship between governments and firms is personal rather than institutional. When leaders are about to leave office, I find that previously favored firms do not provide assistance, because no future gain can be expected from leaders once they have left. The results are consistent with the predictions derived from a theory of reciprocal relationships between governments and firms. As an additional result, I found evidence suggesting that private firms took the initiative to offer more assistance to new leaders in exchange for gains from future reciprocity.

The findings of this study have implications beyond the Chinese context. First, this unique setting allows me to be one of the very few studies to quantify that governments can gain from their connection with firms and to examine the two-way interaction between them. There are also other examples, such as campaign contribution (Claessens, Feijen and Laeven (2008)

and Sukhtankar (2012)) and lobby activities (Blanes i Vidal, Draca and Fons-Rosen (2012)). But here I stress that, as the second contribution, firms' favors to governments are not necessarily personal transfers, e.g. corruption; in some circumstances firms can contribute to achieving governmental objectives. This implication corresponds to the literature on developing states in Asia, including Japan, South Korea and Taiwan, where governments form close relationships with firms in order to seek formal or informal assistance to attain their overall developmental objectives (Woo-Cumings (1999) and Evans (1995)). Third, by revealing how connections work behind the scenes, I show that, unlike a spot transaction, connections between governments and firms can be dynamic relationships to exchange favors between each other intertemporally. This logic also brings in important policy implications. Policymakers should bear in mind that a policy that leaves room for governments to seek assistance from firms to fulfil their objectives would be highly likely to promote cronyism. Furthermore, even though a frequent turnover of leaders may hold back cronyism, it also has a downside – it costs firms more resources to build up new connections, which can be socially wasteful.

Finally, the findings in this study advance our understanding of the government-business relationship in China: first, even when state-related firms share institutional bonding with local governments, local governments cannot mobilize assistance from state-related firms without informal relationships. Second, as stated in the introduction, in authoritarian regimes, such as China's, higher tiers of government often dictate local government policy objectives which involve unreasonable missions or quotas. To carry out these tasks, the local governments often seek assistance of local firms. This gives local governments the incentive to invest in their relationships with local firms and to create local crony economy. Third, unlike other studies that focus on the party secretary, the findings about leadership turnover suggest when looking at connections with firms one should take the county mayor into account

together with the party secretary. Finally, the two measures for favors seem to capture the variation of government-firm connections and can be used for future study on cronyism in China.

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A Reciprocal Relationship Without Past

Now I look at leaders who have newly taken office. If leaders are new to office, it is likely that no previous reciprocal relationships exist with firms. This suggests that I should amend my framework to look at whether firms take the initiative in offering favors by raising the 2001 benchmark. Assuming that Government is the first to receive favors, I solve for $\bar{V_R^k}(\bar{x}),\ k \in \{G,\ L\}$. Similarly, I use (1) and (2), which gives

$$\bar{V_R^F}(\bar{x}) = \frac{1}{2(1-\pi\delta)}M(\bar{x}) + \frac{(1-\pi\delta)(2\theta-1)}{2(1+(\pi\delta)^2(2\theta-1))}L(\bar{x}) \tag{8}$$

$$\bar{V_R^G}(\bar{x}) = \frac{1}{2(1-\pi\delta)}M(\bar{x}) - \frac{(1-\pi\delta)(2\theta-1)}{2(1+(\pi\delta)^2(2\theta-1))}L(\bar{x}) \tag{9}$$

where $M(\bar{x}) = \int_0^{\bar{x}}(z-c(z))dF(z)$ and $L(\bar{x}) = \int_0^{\bar{x}}(\omega+c(\omega))dF(\omega)$. In this setting, since $\theta > \frac{1}{2}$, then I still have $\bar{V_R^F}(\bar{x}) > \bar{V_R^G}(\bar{x})$. But, compared with the previous case, in which Firm first requested for favors, the difference between $\bar{V_R^F}(\bar{x})$ and $\bar{V_R^G}(\bar{x})$ is smaller. To sustain a reciprocal relationship, the following condition for Firm needs to hold.

$$-c(\bar{x}) + \delta\cdot 0 + \delta^2\bar{V_R^F} \geq 0 \tag{10}$$

For Government to stay in a reciprocal relationship,

$$0 + \delta\cdot -c(\bar{x}) + \delta^2\bar{V_R^G} \geq 0 \tag{11}$$

All else being equal, $\bar{V_R^F}(\bar{x})$ increases in θ. This suggests that when Firm is needier, the value of its continuation is higher. Thus, under general conditions when $\pi\delta\bar{V_R^G} > (\pi\delta)^2\bar{V_R^F}$, a higher θ allows a greater $\bar{x}$ to satisfy both (10) and (11) conditions in order to sustain a reciprocal relationship; I have the following hypothesis.

Prediction 3. *(Reciprocity Without Past) When leaders are new, firms which*

rely more on governments' favors will take the initiative by offering more favors.

In counties with new leaders, previous favors no longer matter. Those new leaders cannot find assistance from firms which received favors from previous governments. However, firms that are sensitive to government favors might provide assistance in exchange for future reciprocity. While state-related firms share institutional bonding with governments, private firms rely more on this informal relationship. Empirically, I expect that private firms are more likely to offer assistance when leaders are new to office. Furthermore, previous favors should have no effect on relative ATD.

B Which Leader Matters?

B.1 Leaving Mayor or Leaving Party Secretary

In constructing an indicator for counties where leaders are due to leave, unlike the literature that mainly focuses on party secretary turnover, I take into account both the mayor and party secretary post. To test whether this innovation is valid, in theory I should include a quadruple interaction on the criteria for leaving leaders, a leaving mayor without promoting to party secretary and a leaving party secretary. In practice, however, the limited sample size sets a barrier against this practice. Instead, I look individually at the two leaders' leaving. I start by looking at mayors soon to leave office. The results are shown in Table A1.

In column 1 of panel A, counties with leaving mayors respond no differently from other counties. The estimates in column 2 and 3 do align with my prediction that the measure for the favor of credit access, debt leverage, of state-related firms has no effect in counties where mayors are soon to leave office but the effect exists for all other counties. However, the same pattern does not apply to the measure for tax deduction, the effective tax rate, of

private firms. As shown in column 4 and 5 the estimate for interaction is negative, an opposite sign that is predicted by the theory. When I turn to examine the effect of party secretaries soon to leave office, I use the variation of party secretaries leaving and no mayors promoted to fill the office. This is mainly to avoid the possibility that mayors soon to be promoted to party secretary may bias the effect of a leaving party secretary. In column 1 of panel B, similarly, counties with leaving party secretaries respond no differently from other counties. In column 2 and 3 the results suggest that a leaving party secretary may moderate the effect of debt leverage of state-related firms. However, I don't have a significant estimate for the interaction term. In column 4 and 5, the signs of the estimates do correspond to those in Table 10. However, the interaction term is imprecisely estimated.

To sum up, I find no evidence that my heterogeneous effects are driven by either of the leaders' leaving and this suggests that taking both leaders into account is a valid innovation when examining their relationships with firms.

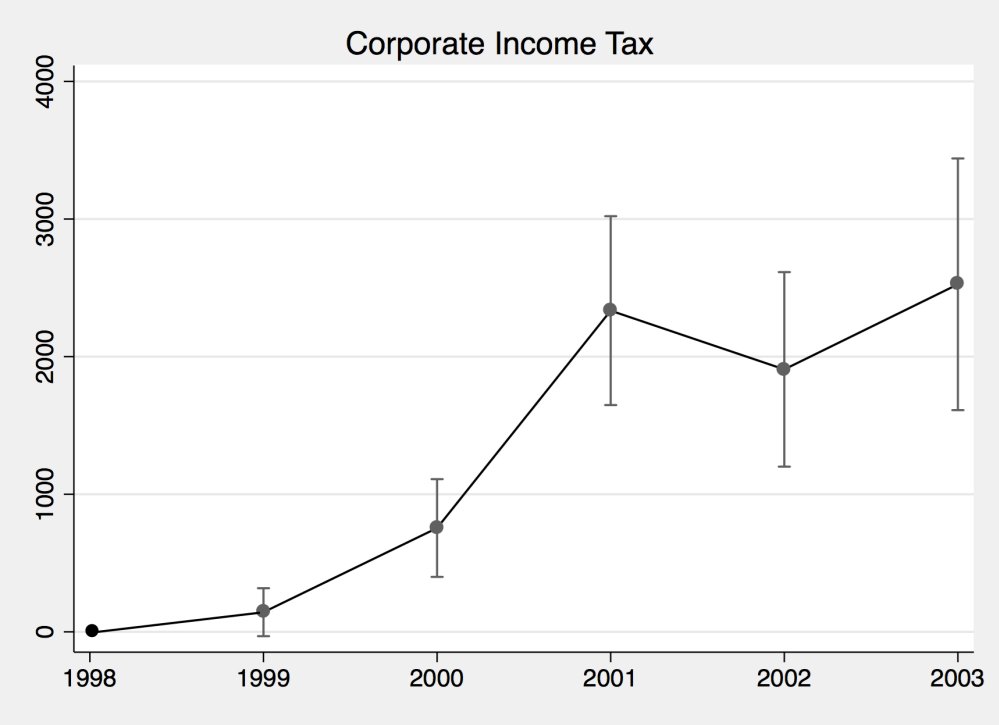
B.2 New Mayor or New Secretary

As in the last part of the previous section, I again look at which leader matters – mayors or party sectaries. I start by looking at the new mayor. To do this, I use the variation from counties with new mayors and party secretaries who were not promoted from the mayor office. This is to avoid the possibility that my estimates may be biased because the party secretaries promoted from mayor office still own mayor power. In column 1 of panel A in Table A2, I show that counties with new mayors do not respond differently from other counties. The results in column 2 and 3 suggest that state-related firms that were previously favored with credit access do not assist local governments differently when only the mayors are newly arrived. The same results can be found in column 4 and 5 when interacting the new mayor indicator and effective tax rate of private firms- private firms previ-

ously favored with tax deduction do not assist local governments differently when only the mayors are newly arrived. In panel B of Table A2, I repeat the same exercise as in panel A but this time I focus on the effects for new party secretary. The results are very similar to those in panel A. Firms that were previously favored through better credit access or greater tax deduction do not assist local governments differently when only the party secretaries are newly arrived. Overall these results suggest that no matter which leader, county party secretary or mayor, is new to office, previously favored firms will choose to assist no differently from their assistance to county governments without new leaderships.

C Figure

Figure 1: Corporate Income Tax (1998 - 2003)



Notes: Using observations from 476 counties, this figure plots the regression coefficients of year dummies from 1998 to 2003 (1998 as base year) with corporate income tax (in 10,000 RMB) as the outcome variables. The regression controls for Ln GDP per capita and county fixed effects. Robust standard errors are clustered at the county level. The corporate income tax data is collected from various local fiscal and tax yearbooks (see data appendix for the sources). GDP data is from Province, prefecture, and county fiscal statistics. Details on the data source can be found in the Data section.

Figure 2: Other Economics Indicators (1998 - 2003)

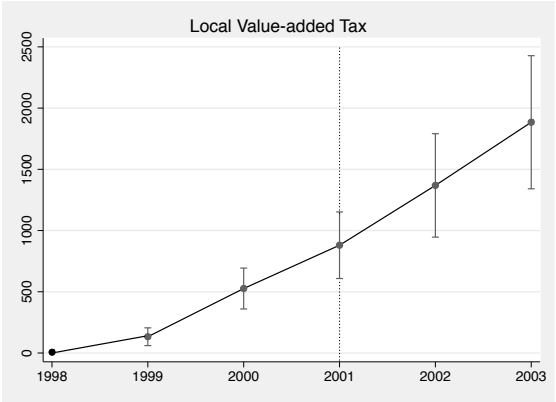


Figure 2.A

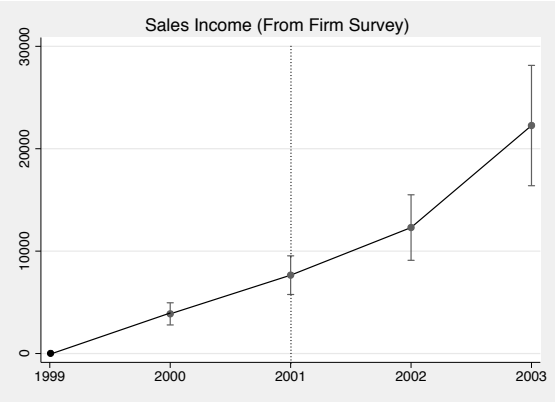


Figure 2.B

Notes: Figure 2.A plots the regression coefficients of year dummies from 1998 to 2003 (1998 as base year) with local value-added tax (in 10,000 RMB) from 476 counties. Figure 2.B plots the regression coefficients of year dummies from 1999 to 2003 (1999 as base year) with surveyed firms' aggregate sales income (in 10,000 RMB) from 476 counties. The regression controls for Ln GDP per capita and county fixed effects. Robust standard errors are clustered at the county level. Local GDP and VAT data are from Province, prefecture, and county fiscal statistics. Data on sales income is from Annual Surveys of Industrial Production. Details on the data source can be found in the Data section.

Figure 3: How Firms Assist Local Governments

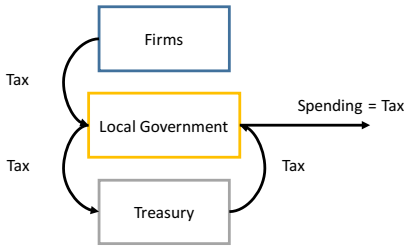


Figure 3.A

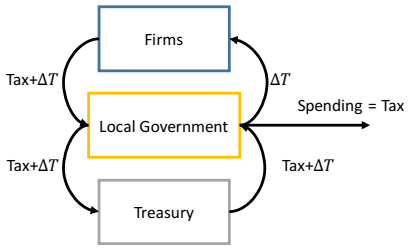
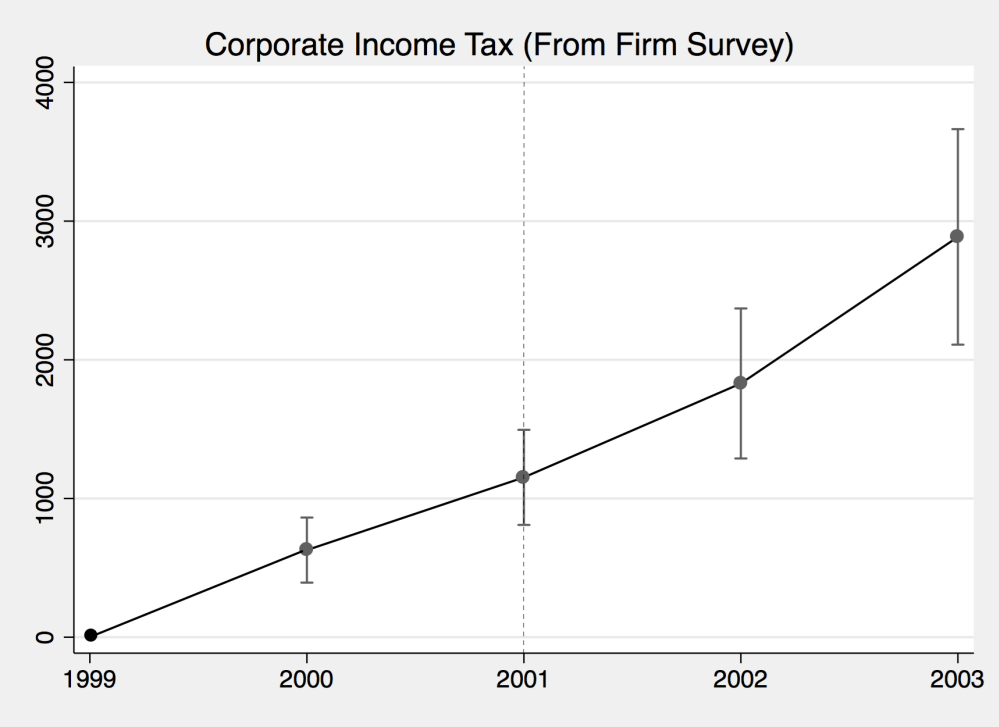


Figure 3.B

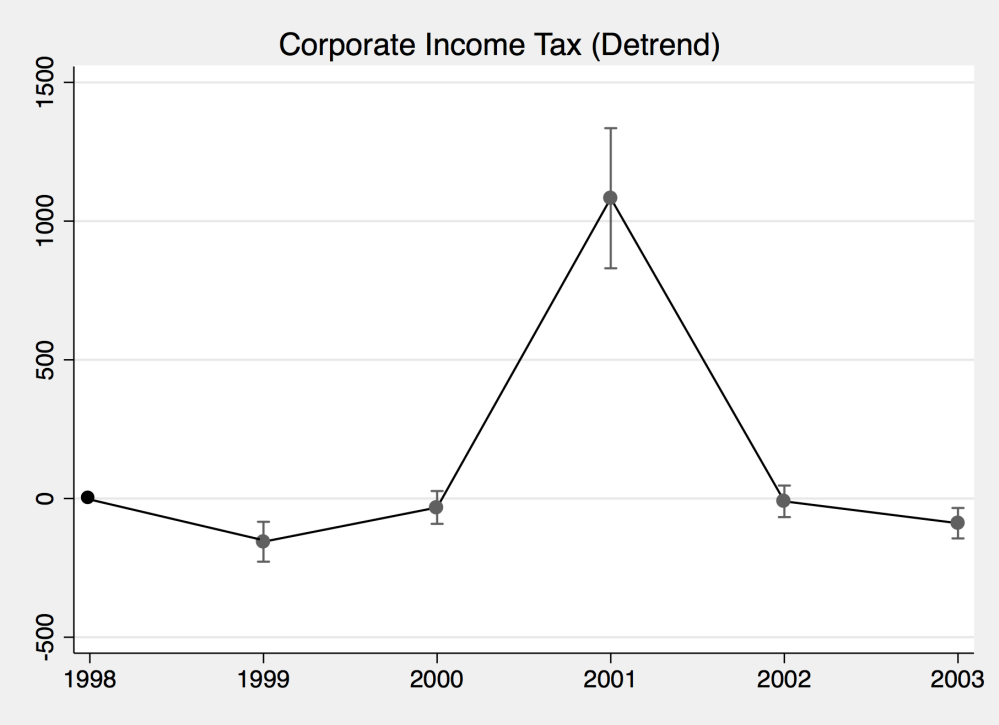
Notes: These figures show how firms may assist local governments in raising the 2001 tax revenue. In any other normal times, shown in Figure 3.A, firms pay tax to local governments and the revenue is then remitted to Treasury before being redistributed back to the governments. However, in November and December 2001 as shown in Figure 3.B, firms paid tax and also other transfers. The money was labelled as corporate income tax and remitted to Treasury before they came back to the local governments. Governments kept the tax and returned all the extra money back to the firms.

Figure 4: Corporate Income Tax (From A Large-scale Firm Survey)



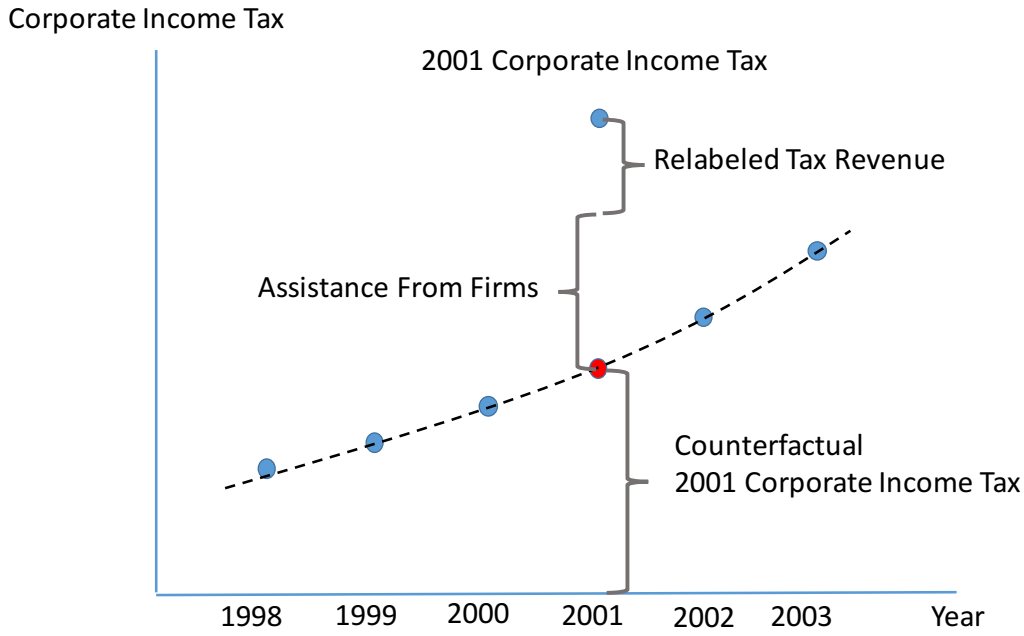
Notes: Using observations from 476 counties, this figure plots the regression coefficients of year dummies from 1999 to 2003 (1999 as base year) with corporate income tax from Annual Surveys of Industrial Production. The regression controls for Ln GDP per capita and county fixed effects. Robust standard errors are clustered at the county level. GDP data is from Province, prefecture, and county fiscal statistics. Details on the data source can be found in the Data section.

Figure 5: Corporate Income Tax Deviation from Trend



Notes: Taking corporate income tax to remove individual county quadratic trend (excluding 2001), in this figure I plot the regression coefficients of the detrend residuals on year dummies from 1998 to 2003 (1998 as base year). The regression controls for Ln GDP per capita and county fixed effects. Robust standard errors are clustered at the county level. Corporate income tax data is collected from various local fiscal and tax yearbooks. GDP data is from Province, prefecture, and county fiscal statistics. Details on the data source can be found in the Data section.

Figure 6: Decomposition of 2001 Corporate Income Tax



Notes: This figure shows that the deviation of 2001 corporate income tax consists of transfers from two different channels: (1) relabeling other tax revenue and (2) transfer from firms.

Figure 7: Relabeling Other Taxes — A Channel to Response

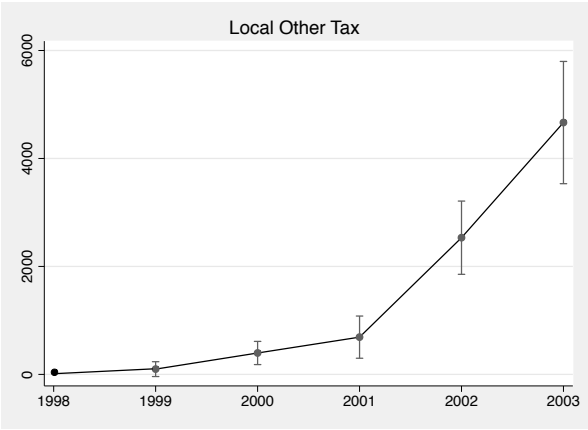


Figure 7.A

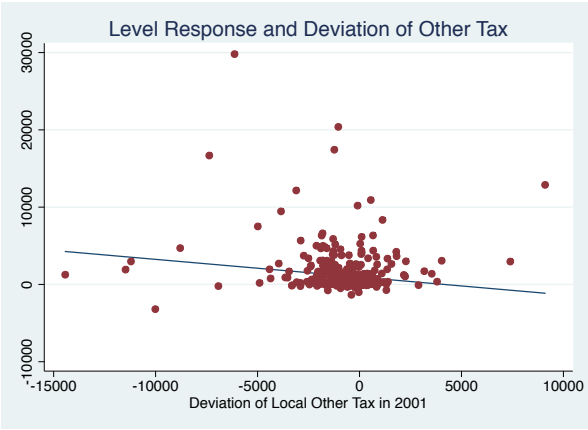
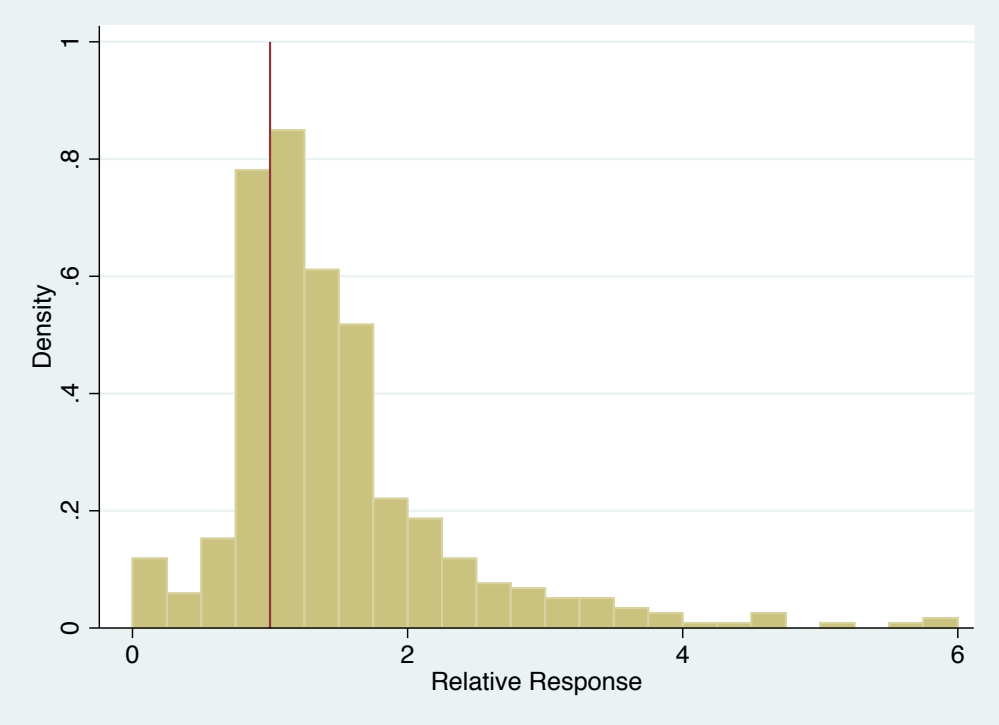


Figure 7.B

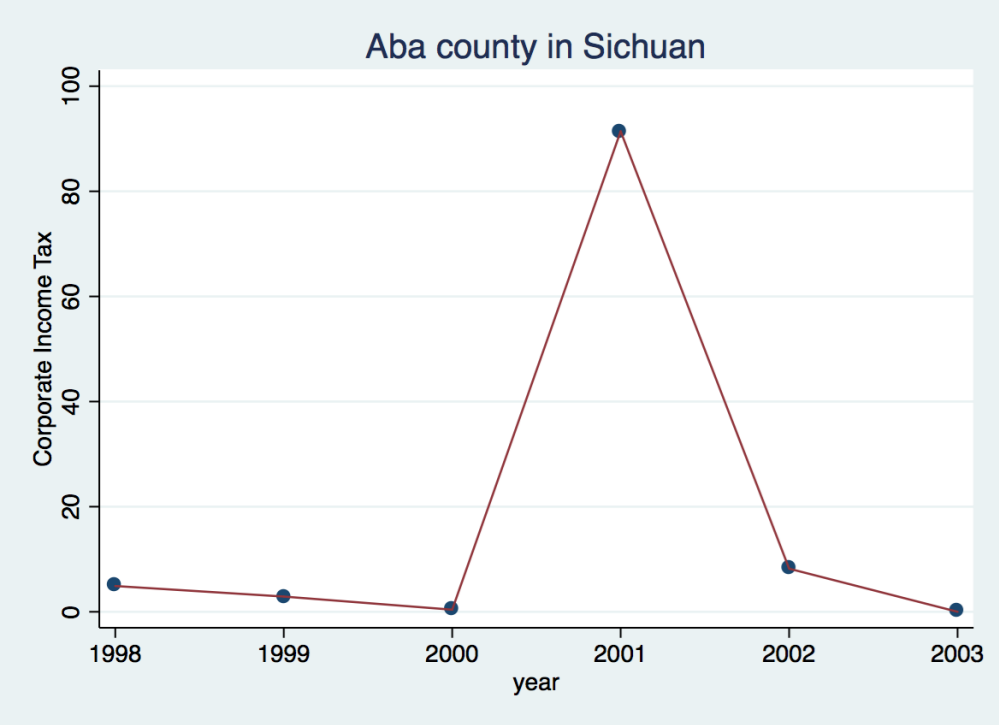
Notes: Figure 7.A plots the regression coefficients of year dummies from 1998 to 2003 (1998 as base year) with local other tax from 476 counties, including business tax, agriculture tax and other fees. The regression control for Ln GDP per capita and county fixed effects. Robust standard errors are clustered at the county level. In Figure 7.B, I scatterplot the absolute deviations in the 2001 corporate income tax and off-trend deviation of the other taxes. Business tax, agriculture tax, other fees, and GDP data are from Province, prefecture, and county fiscal statistics. Details on the data source can be found in the Data section.

Figure 8: Distribution of Relative ATD Measure



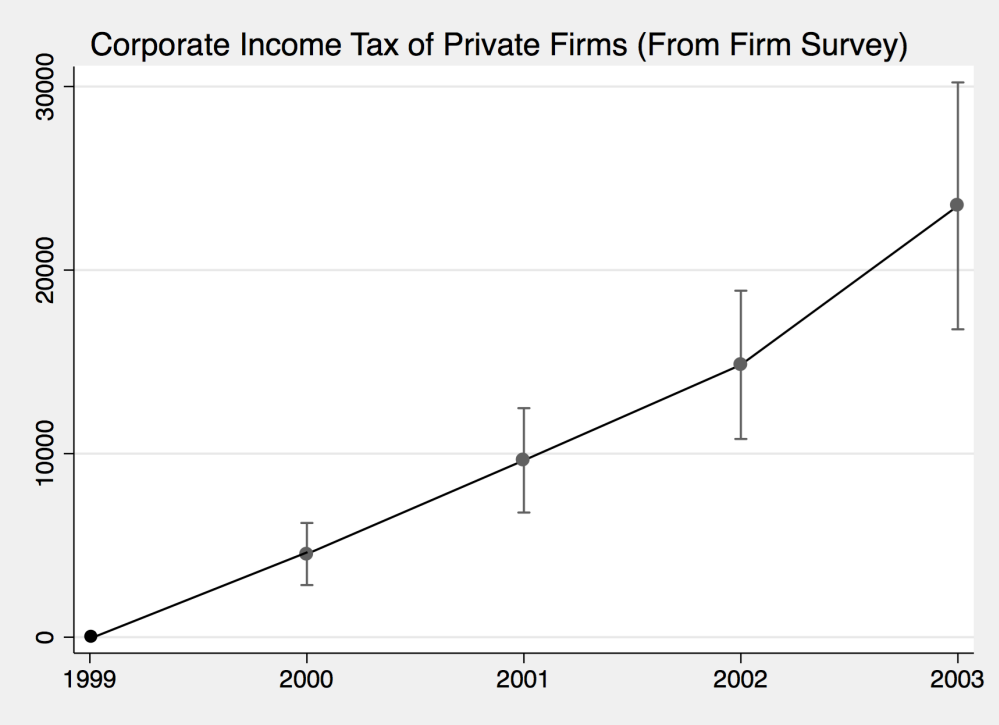
Notes: This figure shows the distribution of my main outcome variable, the relative ATD in the 2001 benchmarks. Red line indicates the value of relative ATD being 1. My observations are 473 counties across 112 prefectures and 25 provinces in China. Details of constructing relative ATD can be found in the text of data section.

Figure 9: Relative ATD for Aba County in Sichuan



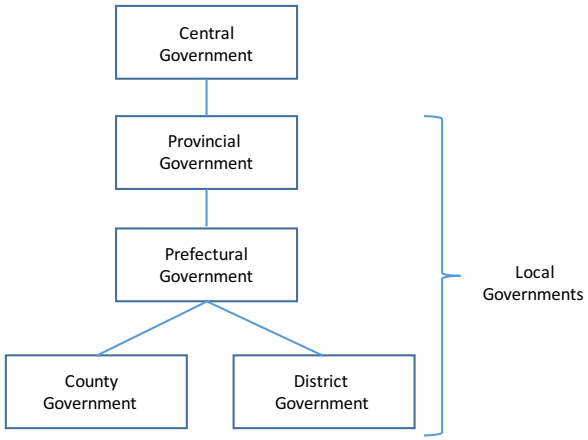
Notes: This figure I plot the corporate income tax revenue of Aba county in Sichuan from 1998 to 2003. The data is collected from Aba County Tax Authority Year Books.

Figure 10: Corporate Income Tax of Private Firms



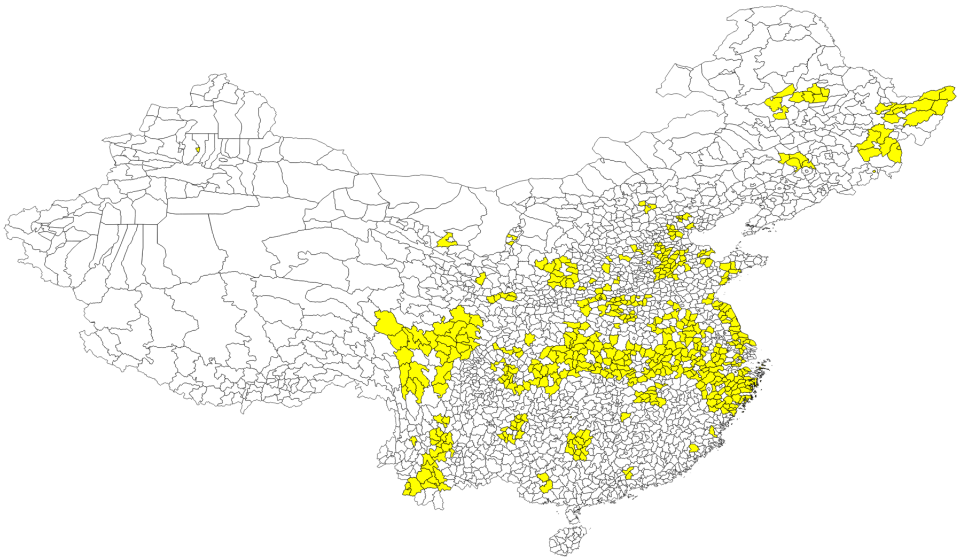
Notes: Using observations from 476 counties, this figure plots the regression coefficients of year dummies from 1998 to 2003 (1998 as base year) with corporate income tax of private firms from Annual Surveys of Industrial Production. The regression controls for Ln GDP per capita and county fixed effects. Robust standard errors are clustered at the county level. GDP data is from Province, prefecture, and county fiscal statistics. Details on the data source can be found in the Data section.

Figure 11: Government Hierarchy in China



Notes: Hierarchy of Chinese government: starting from the central government, provincial governments, prefecture governments, and followed by county and district governments.

Figure 12: Counties in which data are available



Notes: This map show counties (in yellow) in which data on corporate income taxes is available.

D Tables

Table 1A: Descriptive statistics

Variables	Obs.	Mean	Std Dev.	Max	Min
Relative ATD	471	1.47	0.85	5.99	0
Debt leverage (All firms)	471	0.69	0.16	1.31	0.02
Debt leverage (State-related)	471	0.71	0.18	2.03	0.02
Debt leverage (Private)	471	0.59	0.26	1.62	0
Debt leverage (Central-state)	471	0.14	0.30	1.67	0
Debt leverage (Utility industry)	471	0.50	0.23	1.38	0
Effective tax rate (All firms)	471	0.05	0.05	0.67	0
Effective tax rate (Private)	471	0.05	0.05	0.45	0
Effective tax rate (State-related)	471	0.05	0.04	0.36	0
Sales share (State-related)	471	0.59	0.26	1.00	0.02
Sales share (Private)	471	0.32	0.23	0.92	0
Sales share (Utility industry)	471	0.12	0.17	1.00	0
Ln GDP per capita	471	8.54	0.72	10.99	5.55
Normalized corp. income tax	471	0.00	0.01	0.07	0
Previous fiscal pressure	471	2.45	3.22	44.26	0.84
Absolute deviation	471	1,087	2,575	29,801	-3,200
Deviation of other taxes	471	-574	1,655	9,103	-14,433

Notes: This table presents descriptive statistics for variables used in the analysis. The unit of observation is county in year 2001. *Relative ATD* is a measure for the extent of firms' assistance in the 2001 corporate income tax; *Debt leverage* variables measures the amount of loans that go to different type of firms (normalized by total assets); *Effective tax rate* captures the extent of tax deduction for firms of different ownerships; *Sales share* measures the size of firms for each type; *Normalized corporate income tax* is the previous corporate income tax normalized by GDP; *Fiscal pressure* captures the extent of tightness in fiscal budget; *Absolute deviation* is a measure on the absolute deviations in the 2001 corporate income tax from trend; *Deviation of other tax* measures how much fiscal resources were transferred to raise the 2001 corporate income tax. All firm-related variables are taken average from 1999 to 2000 and constructed from Annual Surveys of Industrial Production. Corporate Income tax are collected from various local fiscal or tax yearbooks. All other taxes and GDP data are from Province, prefecture, and county fiscal statistics. Details on the data source can be found in the Data section.

Table 1B: Descriptive statistics (continue)

Variables	Obs.	Mean	Std Dev.	Max	Min
Indicator for leaving leaders	440	0.08	0.27	1	0
Indicator for end-of-term leaders	440	0.09	0.29	1	0
Indicator for leaving mayor	440	0.21	0.41	1	0
Indicator for leaving party secretary	440	0.14	0.35	1	0
Indicator for new leaders	441	0.08	0.27	1	0
Indicator for leaving mayor	441	0.15	0.36	1	0
Indicator for leaving party secretary	441	0.14	0.35	1	0

Notes: This table presents descriptive statistics for variables used in the analysis. The unit of observation is county in year 2001. *Indicator for new leaders* is an indicator that takes value of 1 if both county mayor and party secretary are new to office and 0 otherwise; *Indicator for leaving leaders* is an indicator that takes value of 1 if both county mayor and party secretary are soon to leave office and 0 otherwise; *Indicator for new mayor* is an indicator that takes value of 1 if county mayor just arrive in office and without party secretary promoted from mayor and 0 otherwise. *Indicator for new party secretary* is an indicator that takes value of 1 if county party secretary just arrive in office and without being promoted from mayor and 0 otherwise; *Indicator for end-of-term leaders* is an indicator that takes value of 1 if both county mayor and party secretary are at the end of term in office and 0 otherwise; *Indicator for leaving mayor* is an indicator that takes value of 1 if county mayor is soon to leave and without promoting to party secretary and 0 otherwise. *Indicator for leaving party secretary* is an indicator that takes value of 1 if county party secretary is soon to leave and without mayor promoting to party secretary and 0 otherwise. Data on local political leaders are collected from Provincial Yearbooks. Details on the data source can be found in the Data section.

Table 2: Relative ATD and credit access

VARIABLES	(1)	(2)	(3)	(4)
	Relative ATD			
Debt leverage (All firms)	0.759**			
	(0.353)			
Debt leverage (State-related)		0.816**	0.819**	0.817**
		(0.333)	(0.337)	(0.330)
Debt leverage (Private)		-0.211	-0.211	-0.204
		(0.183)	(0.184)	(0.187)
Debt leverage (Central-state)			0.0316	
			(0.144)	
Sales share (State-related)				0.0750
				(0.215)
Prefecture FE	✓	✓	✓	✓
All Controls	✓	✓	✓	✓
Obs.	471	471	471	471
R^2	0.415	0.422	0.422	0.422

Notes: This table presents estimates of debt leverage (a measure for credit access) on relative ATD in the 2001 benchmark. *Relative ATD* is a measure for the extent of firms' assistance in the 2001 corporate income tax; *Debt leverage* variables measures the amount of loans that go to different type of firms (normalized by total assets); *Sales share* measures the size of firms for each type. All regressions control for previous fiscal pressure, previous corporate income tax (normalized by GDP), Ln GDP per capita and prefecture fixed effects. Robust standard errors are clustered at prefecture level. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Robustness check: credit access as a favor

	(1)	(2)	(3)	(4)
VARIABLES	Relative ATD			
Debt leverage (State-related)	0.976***	0.941***	0.874**	0.816**
	(0.354)	(0.334)	(0.348)	(0.333)
Debt leverage (Private)	-0.175	-0.157	-0.171	-0.211
	(0.215)	(0.197)	(0.195)	(0.183)
Ln GDP per capita		-0.286**	-0.299***	-0.338***
		(0.112)	(0.107)	(0.110)
Normalized corp. income tax			-12.15*	-13.70**
			(6.627)	(6.272)
Previous fiscal pressure				-0.0280
				(0.0209)
Prefecture FE	✓	✓	✓	✓
All Controls	✓	✓	✓	✓
Obs.	471	471	471	471
r^2	0.395	0.412	0.418	0.422

Notes: This table presents robustness check on estimates of debt leverage of state-related firms on relative ATD in the 2001 benchmark. *Relative ATD* is a measure for the extent of firms' assistance in the 2001 corporate income tax; *Debt leverage* variables measures the amount of loans that go to different type of firms (normalized by total assets); *Normalized corporate income tax* is the previous corporate income tax normalized by GDP; *Fiscal pressure* captures the extent of tightness in fiscal budget. All regressions control for prefecture fixed effects. Robust standard errors are clustered at prefecture level. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Relative ATD and tax deduction

VARIABLES	(1)	(2)	(3)	(4)
	Relative ATD			
Effective tax rate (All firms)	-0.509			
	(1.512)			
Effective tax rate (Private)		-2.246*	-2.217*	-2.003*
		(1.198)	(1.241)	(1.128)
Effective tax rate (State-related)		0.144	0.157	0.188
		(1.139)	(1.148)	(1.104)
Sales share (Private)			-0.0565	
			(0.248)	
Debt leverage (State-related)				0.754**
				(0.322)
Prefecture FE	✓	✓	✓	✓
All Controls	✓	✓	✓	✓
Obs.	471	471	471	471
R^2	0.405	0.416	0.416	0.429

Notes: The table presents estimates of effective corporate income tax rate (a measure for tax deduction) on relative ATD in the 2001 benchmark. *Relative ATD* is a measure for the extent of firms' assistance in the 2001 corporate income tax; *Debt leverage* variables measures the amount of loans that go to different type of firms (normalized by total assets); *Effective tax rate* captures the extent of tax deduction for firms of different ownerships; *Sales share* measures the size of firms for each type. All regressions control for previous fiscal pressure, previous corporate income tax (normalized by GDP), and Ln GDP per capita. Robust standard errors are clustered at prefecture level. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Robustness check: tax deduction as a favor

	(1)	(2)	(3)	(4)
VARIABLES	Relative ATD			
Effective tax rate (Private)	-2.944***	-2.504**	-2.289*	-2.246*
	(1.100)	(1.150)	(1.197)	(1.198)
Effective tax rate (State-related)	-0.157	0.0839	0.473	0.144
	(1.314)	(1.302)	(1.401)	(1.139)
Ln GDP per capita		-0.243**	-0.265**	-0.307**
		(0.122)	(0.118)	(0.121)
Normalized corp. income tax			-12.53**	-13.67**
			(5.835)	(5.899)
Previous fiscal pressure				-0.0329
				(0.0245)
Prefecture FE	✓	✓	✓	✓
All Controls	✓	✓	✓	✓
Obs.	471	471	471	471
R^2	0.393	0.405	0.411	0.416

Notes: This table presents robustness check on estimates of effective tax rate of private firms on relative ATD in the 2001 benchmark. *Relative ATD* is a measure for the extent of firms' assistance in the 2001 corporate income tax; *Effective tax rate* captures the extent of tax deduction for firms of different ownerships; *Normalized corporate income tax* is the previous corporate income tax normalized by GDP; *Fiscal pressure* captures the extent of tightness in fiscal budget. All regressions control for provincial fixed effects. Robust standard errors are clustered at prefecture level. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Robustness check: other firm characteristics

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Relative ATD					
Debt Leverage (State-related)	0.817**	0.830**	0.692*			
	(0.335)	(0.333)	(0.349)			
Debt Leverage (Private)	-0.220	-0.218	-0.233			
	(0.188)	(0.179)	(0.200)			
Effective tax rate (Private)				-2.238*	-2.123	-2.334*
				(1.201)	(1.292)	(1.263)
Effective tax rate (State-related)				0.161	0.157	-0.361
				(1.120)	(1.081)	(0.869)
Turnover ratio (State-related)	✓			✓		
Turnover ratio (Private)	✓			✓		
Profitability (State-related)		✓			✓	
Profitability (Private)		✓			✓	
Sales share (Utility industry)			✓			✓
Sales share (Resource industry)			✓			✓
Prefecture FE	✓	✓	✓	✓	✓	✓
All Controls	✓	✓	✓	✓	✓	✓
Obs.	471	471	471	471	471	471
R^2	0.422	0.424	0.436	0.416	0.417	0.436

Notes: The table presents robustness checks on estimates of favors on the relative ATD. Columns (1) - (3) present estimates on debt leverage when controlling for turnover ratio, profitability or industry shares. Columns (4) - (6) present estimates on effective tax rate when controlling for turnover ratio, profitability or industry shares. *Relative ATD* is a measure for the extent of firms' assistance in the 2001 corporate income tax; *Debt leverage* variables measures the amount of loans that go to different type of firms (normalized by total assets); *Effective tax rate* captures the extent of tax deduction for firms of different ownerships; *Sales share* measures the size of firms for each type; *Turnover ratio* measures the efficiency on how assets are used to generate sales income; *Profitability* tries to capture the variation of productivity of local firms, defined by profit normalizing by assets. All regressions control for previous fiscal pressure, previous corporate income tax (normalized by GDP), Ln GDP per capita and prefecture fixed effects. Robust standard errors are clustered at prefecture level. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Robustness check: effects of favors in quantile regression

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Relative ATD					
Percentile	0.25	0.50	0.75	0.25	0.50	0.75
Panel A: Quantile regression with prefecture fixed-effects						
Debt leverage (State-related)	0.304**	0.425	0.788**			
	(0.132)	(0.407)	(0.314)			
Debt leverage (Private)	-0.0659	-8.19e-06	-0.264			
	(0.111)	(0.372)	(0.277)			
Effective tax rate (Private)				-0.970	-1.592	-2.120***
				(0.998)	(1.185)	(0.760)
Effective tax rate (State-related)				0.469	-0.283	-1.130*
				(1.076)	(1.195)	(0.640)
Prefecture FE	✓	✓	✓	✓	✓	✓
All Controls	✓	✓	✓	✓	✓	✓
Obs.	471	471	471	471	471	471
R^2	0.287	0.351	0.338	0.272	0.344	0.329

Notes: The table presents estimates of two favors on the relative ATD using quantile regressions. Columns (1) - (3) present estimates of debt leverage (measure for favor of credit access) in 25th, 50th, and 75th percentile. Columns (4) - (6) present estimates of effective tax rate (measure for favor of tax deduction) in 25th, 50th, and 75th percentile. *Relative ATD* is a measure for the extent of firms' assistance in the 2001 corporate income tax; *Debt leverage* variables measures the amount of loans that go to different type of firms (normalized by total assets); *Effective tax rate* captures the extent of tax deduction for firms of different ownerships; All regressions control for previous fiscal pressure, previous corporate income tax (normalized by GDP), and Ln GDP per capita. Robust standard errors are clustered at prefecture level. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Robustness check: sample selection

VARIABLES	(1)	(2)	(3)	(4)	(5)
	Relative ATD				
	Excluding 2002		Only 4 Provinces		
Debt leverage (State-related)	0.595*		1.174*		
	(0.335)		(0.662)		
Debt leverage (Private)	-0.191		-0.0621		
	(0.263)		(0.478)		
Effective tax rate (Private)		-3.506**		-4.163***	
		(1.421)		(1.512)	
Effective tax rate (State-related)		-0.657		1.134	
		(1.156)		(2.404)	
Sales share (Utility Industry)					-1.024**
					(0.416)
Prefecture FE	✓	✓	✓	✓	✓
All Controls	✓	✓	✓	✓	✓
Obs.	470	470	209	209	471
R^2	0.437	0.449	0.390	0.397	0.422

Notes: The table presents robustness checks on estimates of favors on the relative ATD. Columns (1) - (2) present estimates of favors when conditioning only on 4 provinces — Anhui, Hubei, Sichuan and Zhejiang. Columns (3) - (4) present estimates of favors but using an alternative outcome measures that exclude 2002 when estimating. *Relative ATD* is a measure for the extent of firms' assistance in the 2001 corporate income tax; *Debt leverage* variables measures the amount of loans that go to different type of firms (normalized by total assets); *Effective tax rate* captures the extent of tax deduction for firms of different ownerships; *Sales share* measures the size of firms for each type of firms; All regressions control for previous fiscal pressure, previous corporate income tax (normalized by GDP), Ln GDP per capita and prefecture fixed effects. Robust standard errors are clustered at prefecture level. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Summary statistics: counties with & without leaving leaders

Variable	Counties with leaving leaders	Counties without leaving leaders	Difference	(Stand Error)
Debt leverage (All firms)	0.710	0.703	0.007	(0.027)
Debt leverage (State-related)	0.740	0.721	0.029	(0.034)
Debt leverage (Private)	0.615	0.621	-0.070	(0.052)
Debt leverage (Utility industry)	0.549	0.532	0.017	(0.037)
Effective tax rate (All firms)	0.042	0.051	-0.009	(0.008)
Effective tax rate (Private)	0.044	0.059	-0.015*	(0.009)
Effective tax rate (State-related)	0.035	0.047	-0.012	(0.007)
Sales share (State-related)	0.511	0.582	-0.071	(0.045)
Sales share (Private)	0.421	0.330	0.091**	(0.041)
Sales share (Utility industry)	0.110	0.097	0.013	(0.023)
Ln GDP per capita	8.489	8.558	-0.069	(0.128)
Normalized corp. income tax	0.004	0.005	-0.001	(0.001)
Previous fiscal pressure	1.866	1.920	-0.054	(0.176)
Deviation of other tax	-718.892	-591.698	-127.194	(298.658)

Notes: This table presents descriptive statistics between counties with leaving leaders and counties without. *Debt leverage* variables measures the amount of loans that go to different type of firms (normalized by total assets); *Effective tax rate* captures the extent of tax deduction for firms of different ownerships; *Sales share* measures the size of firms for each type; *Normalized corporate income tax* is the previous corporate income tax normalized by GDP; *Fiscal pressure* captures the extent of tightness in fiscal budget; *Absolute deviations* is a measure on the absolute deviation in the 2001 corporate income tax from trend; *Deviation of other tax* measures how much fiscal resources were transferred to raise the 2001 corporate income tax.All firm-related variables are taken average from 1999 to 2000 and constructed from Annual Surveys of Industrial Production. Corporate Income tax are collected from various local fiscal or tax yearbooks. All other taxes and GDP data are from Province, prefecture, and county fiscal statistics. Details on the data source can be found in the Data section.

Table 10: Leaving leaders and assistance from previously favored firms

VARIABLES	(1)	(2)	(3)	(4)	(5)
	Relative ATD				
Indicator for leaving leaders	-0.117	-0.130	0.941*	-0.127	-0.394**
	(0.115)	(0.118)	(0.540)	(0.118)	(0.186)
Debt leverage (State-related)		0.580**	0.679**		
		(0.292)	(0.313)		
Debt leverage × Leaving Leaders			-1.482**		
			(0.738)		
Effective tax rate (Private)				-2.264*	-2.441**
				(1.169)	(1.197)
Effective tax rate × Leaving Leaders					5.841**
					(2.811)
Prefecture FE	✓	✓	✓	✓	✓
All Controls	✓	✓	✓	✓	✓
Obs.	440	440	440	440	440
R^2	0.438	0.446	0.450	0.451	0.454

Notes: The table presents heterogeneous effects of leaving leaders for each of the favors. *Relative ATD* is a measure for the extent of firms' assistance in the 2001 corporate income tax; *Debt leverage* variables measures the amount of loans that go to different type of firms (normalized by total assets); *Effective tax rate* captures the extent of tax deduction for firms of different ownerships; *Indicator for leaving leaders* is an indicator that takes value of 1 if both county mayor and party secretary are soon to leave office and 0 otherwise. All regressions control for previous fiscal pressure, previous corporate income tax (normalized by GDP), Ln GDP per capita and prefecture fixed effects. Robust standard errors are clustered at prefecture level. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 11: Government responses and relabeling other taxes

VARIABLES	Total Relative Responses	Level Responses		
	(1)	(2)	(3)	(4)
Indicator for leaving leaders	-0.0600		-142.5	-714.2
	(0.154)		(404.7)	(767.4)
Deviation of other tax		-0.178**	-0.176**	-0.144*
		(0.0794)	(0.0814)	(0.0803)
Deviation of other tax × Leaving leaders				-0.850
				(0.706)
Prefecture FE	✓	✓	✓	✓
All Controls	✓	✓	✓	✓
Obs.	440	471	440	440
R^2	0.426	0.591	0.587	0.591

Notes: The table presents the negative correlation between the deviation of other tax and the deviation of corporate income tax in 2001. *Total relative deviation* is a measure for the extent of total deviations in the 2001 corporate income tax; *Absolute deviation* is a measure on the absolute deviation in the 2001 corporate income tax from trend; *Deviation of other tax* measures how much fiscal resources were transferred to raise the 2001 corporate income tax; *Indicator for leaving leaders* is an indicator that takes value of 1 if both county mayor and party secretary are soon to leave office and 0 otherwise. All regressions control for previous fiscal pressure, previous corporate income tax (normalized by GDP), Ln GDP per capita and prefecture fixed effects. Robust standard errors are clustered at prefecture level. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 12: Effects of favors when leaders are at the end of term

VARIABLES	(1)	(2)	(3)	(4)	(5)
	Relative ATD				
Indicator for end-of-term leaders	0.0125	0.0225	0.563	0.0270	-0.157
	(0.143)	(0.145)	(0.528)	(0.144)	(0.184)
Debt leverage (State-related)		0.572*	0.632*		
		(0.297)	(0.324)		
Debt leverage × End-of-term leaders			-0.784		
			(0.738)		
Effective tax rate (Private)				-2.254*	-2.738**
				(1.167)	(1.191)
Effective tax rate × End-of-term leaders					3.525*
					(2.084)
Prefecture FE	✓	✓	✓	✓	✓
All Controls	✓	✓	✓	✓	✓
Obs.	440	440	440	440	440
R^2	0.437	0.445	0.446	0.450	0.454

Notes: The table presents heterogeneous effects of end-of-term leaders for each of the favors. *Relative ATD* is a measure for the extent of firms' assistance in the 2001 corporate income tax; *Debt leverage* variables measures the amount of loans that go to different type of firms (normalized by total assets); *Effective tax rate* captures the extent of tax deduction for firms of different ownerships; *Indicator for end-of-term leaders* is an indicator that takes value of 1 if both county mayor and party secretary are at the end of term in office and 0 otherwise. All regressions control for previous fiscal pressure, previous corporate income tax (normalized by GDP), Ln GDP per capita and prefecture fixed effects. Robust standard errors are clustered at prefecture level. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 13: Summary statistics: counties with & without new leaders

Variable	Counties with new leaders	Counties without new leaders	Difference	(Stand Error)
Debt leverage (All firms)	0.699	0.704	-0.004	(0.027)
Debt leverage (State-related)	0.710	0.723	-0.013	(0.031)
Debt leverage (Private)	0.680	0.615	0.064	(0.040)
Debt leverage (Utility industry)	0.521	0.534	-0.013	(0.041)
Effective tax rate (All firms)	0.055	0.050	0.004	(0.008)
Effective tax rate (Private)	0.060	0.057	0.003	(0.009)
Effective tax rate (State-related)	0.052	0.046	0.006	(0.007)
Sales share (State-related)	0.545	0.579	-0.034	(0.045)
Sales share (Private)	0.347	0.336	0.011	(0.041)
Sales share (Utility industry)	0.068	0.101	-0.033	(0.023)
Ln GDP per capita	8.630	8.547	0.083	(0.128)
Normalized corp. income tax	0.005	0.005	0.000	(0.001)
Previous fiscal pressure	1.747	1.930	-0.183	(0.175)
Deviation of other tax	-597.291	-601.856	4.566	(298.719)

Notes: This table presents descriptive statistics between counties with leaving leaders and counties without. *Debt leverage* variables measures the amount of loans that go to different type of firms (normalized by total assets); *Effective tax rate* captures the extent of tax deduction for firms of different ownerships; *Sales share* measures the size of firms for each type; *Normalized corporate income tax* is the previous corporate income tax normalized by GDP; *Fiscal pressure* captures the extent of tightness in fiscal budget; *Absolute deviation* is a measure on the absolute deviation in the 2001 corporate income tax from trend; *Deviation of other tax* measures how much fiscal resources were transferred to raise the 2001 corporate income tax.All firm-related variables are taken average from 1999 to 2000 and constructed from Annual Surveys of Industrial Production. Corporate Income tax are collected from various local fiscal or tax yearbooks. All other taxes and GDP data are from Province, prefecture, and county fiscal statistics. Details on the data source can be found in the Data section.

Table 14: New leaders and assistance from previously favored firms

VARIABLES	(1)	(2)	(3)	(4)	(5)
	Relative ATD				
Indicator for new leaders	-0.065	-0.064	-0.347	-0.064	-0.296
	(0.124)	(0.121)	(0.721)	(0.127)	(0.212)
Debt leverage (State-related)		0.586*	0.580*		
		(0.296)	(0.297)		
Debt leverage × New leaders			0.387		
			(0.960)		
Effective tax rate (Private)				-2.206*	-2.403**
				(1.178)	(1.180)
Effective tax rate × New leaders					3.962
					(3.078)
Prefecture FE	✓	✓	✓	✓	✓
All Controls	✓	✓	✓	✓	✓
Obs.	441	441	441	441	441
R^2	0.438	0.446	0.446	0.450	0.452

Notes: The table presents heterogeneous effects of new leaders for each of the favors. *Relative ATD* is a measure for the extent of firms' assistance in the 2001 corporate income tax; *Debt leverage* variables measures the amount of loans that go to different type of firms (normalized by total assets); *Effective tax rate* captures the extent of tax deduction for firms of different ownerships; *Indicator for new leaders* is an indicator that takes value of 1 if both county mayor and party secretary are new to office and 0 otherwise. All regressions control for previous fiscal pressure, previous corporate income tax (normalized by GDP), Ln GDP per capita and prefecture fixed effects. Robust standard errors are clustered at prefecture level. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table A1: Which leaving leader matters? mayor or party secretary?

VARIABLES	(1)	(2)	(3)	(4)	(5)
	Relative ATD				
Panel A: Heterogeneous effect of leaving mayor					
Indicator for leaving mayor	-0.108	-0.111	0.937*	-0.0808	0.0852
	(0.107)	(0.105)	(0.553)	(0.101)	(0.160)
Debt leverage (State-related)		0.620**	0.800**		
		(0.299)	(0.344)		
Debt leverage × Leaving mayor			-1.462*		
			(0.798)		
Effective tax rate (Private)				-2.206*	-1.172
				(1.142)	(1.037)
Effective tax rate × Leaving mayor					-2.944*
					(1.774)
Prefecture FE	✓	✓	✓	✓	✓
All Controls	✓	✓	✓	✓	✓
Obs.	447	447	447	447	447
R^2	0.446	0.455	0.462	0.458	0.463
Panel B: Heterogeneous effect of leaving party secretary					
Indicator for leaving party secretary	-0.0625	-0.0980	0.0997	-0.0782	-0.172
	(0.111)	(0.108)	(0.348)	(0.110)	(0.150)
Debt leverage (State-related)		0.645**	0.724*		
		(0.297)	(0.381)		
Debt leverage × Leaving party secretary			-0.264		
			(0.504)		
Effective tax rate (Private)				-2.301*	-2.420**
				(1.165)	(1.216)
Effective tax rate × Leaving party secretary					2.080
					(2.150)
Prefecture FE	✓	✓	✓	✓	✓
All Controls	✓	✓	✓	✓	✓
Obs.	447	447	447	447	447
R^2	0.444	0.454	0.454	0.457	0.458

Notes: The table presents heterogeneous effects of leaving leaders on each of the favors. Panel A presents heterogeneous effects of leaving mayor. Panel B presents heterogeneous effects of leaving party secretary. *Relative ATD* is a measure for the extent of firms' assistance in the 2001 corporate income tax; *Debt leverage* variables measures the amount of loans that go to different type of firms (normalized by total assets); *Effective tax rate* captures the extent of tax deduction for firms of different ownerships; *Indicator for leaving mayor* is an indicator that takes value of 1 if the county mayor is soon to leave and without promoting to party secretary and 0 otherwise. *Indicator for leaving party secretary* is an indicator that takes value of 1 if the county party secretary is soon to leave and without the mayor promoting to party secretary and 0 otherwise. All regressions control for previous fiscal pressure, previous corporate income tax (normalized by GDP), Ln GDP per capita and prefecture fixed effects. Robust standard errors are clustered at prefecture level. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table A2: Which new leader matters? mayor or party secretary?

VARIABLES	(1)	(2)	(3)	(4)	(5)
	Relative ATD				
Panel A: Heterogeneous effect of new mayor					
Indicator for new mayor	-0.0472	-0.0489	-0.411	-0.0453	-0.0428
	(0.108)	(0.105)	(0.704)	(0.112)	(0.260)
Debt leverage (State-related)		0.586**	0.562*		
		(0.296)	(0.299)		
Debt leverage × New mayor			0.508		
			(1.036)		
Effective tax rate (Private)				-2.205*	-2.200*
				(1.179)	(1.261)
Effective tax rate × New mayor					-0.0423
					(3.515)
Prefecture FE	✓	✓	✓	✓	✓
All Controls	✓	✓	✓	✓	✓
Obs.	441	441	441	441	441
R^2	0.438	0.446	0.446	0.450	0.450
Panel B: Heterogeneous effect of new party secretary					
Indicator for new party secretary	0.128	0.135	-1.512	0.126	0.192
	(0.147)	(0.147)	(1.233)	(0.143)	(0.238)
Debt leverage (State-related)		0.595**	0.391		
		(0.298)	(0.293)		
Debt leverage × New party secretary			2.235		
			(1.747)		
Effective tax rate (Private)				-2.202*	-2.083
				(1.195)	(1.288)
Effective tax rate (State) × New party secretary					-1.297
					(2.854)
Prefecture FE	✓	✓	✓	✓	✓
All Controls	✓	✓	✓	✓	✓
Obs.	441	441	441	441	441
R^2	0.440	0.448	0.461	0.452	0.453

Notes: The table presents heterogeneous effects of new leaders on each of the favors. Panel A presents heterogeneous effects of new mayor. Panel B presents heterogeneous effects of new party secretary. *Relative ATD* is a measure for the extent of firms' assistance in the 2001 corporate income tax; *Indicator for new mayor* is an indicator that takes value of 1 if the county mayor just arrived in office and without the party secretary being promoted from mayor and 0 otherwise. *Indicator for new party secretary* is an indicator that takes value of 1 if the county party secretary just arrived in office and without being promoted from mayor and 0 otherwise. All regressions control for previous fiscal pressure, previous corporate income tax (normalized by GDP), Ln GDP per capita and prefecture fixed effects. Robust standard errors are clustered at prefecture level. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

E Data Sources for Corporate Income Tax

Province	Prefecture	County	Data Sources	Province	Prefecture	County	Data Sources
Anhui	Anqing	Huaining	Tax Yeabook	Anhui	Wuhu	Nanling	Tax Yeabook
Anhui	Anqing	Susong	Tax Yeabook	Anhui	Wuhu	Wuhu	Tax Yeabook
Anhui	Anqing	Taihu	Tax Yeabook	Anhui	Xuancheng	Guangde	Tax Yeabook
Anhui	Anqing	Tongcheng	Tax Yeabook	Anhui	Xuancheng	Jingde	Tax Yeabook
Anhui	Anqing	Wangjiang	Tax Yeabook	Anhui	Xuancheng	Jingxian	Tax Yeabook
Anhui	Anqing	Yuexi	Tax Yeabook	Anhui	Xuancheng	Jixi	Tax Yeabook
Anhui	Anqing	Zongyang	Tax Yeabook	Anhui	Xuancheng	Langxi	Tax Yeabook
Anhui	Bengbu	Guzhen	Tax Yeabook	Anhui	Xuancheng	Ningguo	Tax Yeabook
Anhui	Bengbu	Huaiyuan	Tax Yeabook	Chongqing	Chongqing	Bishan	Fiscal Yearbook
Anhui	Bengbu	Wuhe	Tax Yeabook	Chongqing	Chongqing	Dazu	Fiscal Yearbook
Anhui	Bozhou	Guoyang	Tax Yeabook	Chongqing	Chongqing	Dianjiang	Fiscal Yearbook
Anhui	Chaohu	Hanshan	Tax Yeabook	Chongqing	Chongqing	Fengdu	Fiscal Yearbook
Anhui	Chaohu	Hexiang	Tax Yeabook	Chongqing	Chongqing	Fengjie	Fiscal Yearbook
Anhui	Chaohu	Lujiang	Tax Yeabook	Chongqing	Chongqing	Hechuan	Fiscal Yearbook
Anhui	Chaohu	Wuwei	Tax Yeabook	Chongqing	Chongqing	Jiangjin	Fiscal Yearbook
Anhui	Chuzhou	Dingyuan	Tax Yeabook	Chongqing	Chongqing	Kaixian	Fiscal Yearbook
Anhui	Chuzhou	Fengyang	Tax Yeabook	Chongqing	Chongqing	Liangping	Fiscal Yearbook
Anhui	Chuzhou	Lai'an	Tax Yeabook	Chongqing	Chongqing	Nanchuan	Fiscal Yearbook
Anhui	Chuzhou	Mingguang	Tax Yeabook	Chongqing	Chongqing	Pengshui	Fiscal Yearbook
Anhui	Chuzhou	Quanjiao	Tax Yeabook	Chongqing	Chongqing	Qijiang	Fiscal Yearbook
Anhui	Chuzhou	Tianchang	Tax Yeabook	Chongqing	Chongqing	Rongchang	Fiscal Yearbook
Anhui	Fuyang	Funan	Tax Yeabook	Chongqing	Chongqing	Shizhu	Fiscal Yearbook
Anhui	Fuyang	Jieshou	Tax Yeabook	Chongqing	Chongqing	Tongliang	Fiscal Yearbook
Anhui	Fuyang	Linquan	Tax Yeabook	Chongqing	Chongqing	Tongnan	Fiscal Yearbook
Anhui	Fuyang	Taihe	Tax Yeabook	Chongqing	Chongqing	Wulong	Fiscal Yearbook
Anhui	Fuyang	Yingshang	Tax Yeabook	Chongqing	Chongqing	Wushan	Fiscal Yearbook
Anhui	Hefei	Changfeng	Tax Yeabook	Chongqing	Chongqing	Wuxi	Fiscal Yearbook
Anhui	Hefei	Feidong	Tax Yeabook	Chongqing	Chongqing	Yongchuan	Fiscal Yearbook
Anhui	Hefei	Feixi	Tax Yeabook	Chongqing	Chongqing	Youyang	Fiscal Yearbook
Anhui	Huaibei	Suixi	Tax Yeabook	Chongqing	Chongqing	Yunyang	Fiscal Yearbook
Anhui	Huainan	Fengtai	Tax Yeabook	Chongqing	Chongqing	Zhongxian	Fiscal Yearbook
Anhui	Huangshan	Qimen	Tax Yeabook	Fujian	Fuzhou	Minhou	Fiscal Yearbook
Anhui	Huangshan	Shexian	Tax Yeabook	Fujian	Quanzhou	Anxi	Tax Yeabook
Anhui	Huangshan	Xiuning	Tax Yeabook	Gansu	Jinchang	Yongchang	Fiscal Yearbook
Anhui	Huangshan	Yixian	Tax Yeabook	Gansu	Lanzhou	Yuzhong	Fiscal Yearbook
Anhui	Lu'an area	Huoqiu	Tax Yeabook	Gansu	Tianshui	Gangu	Fiscal Yearbook
Anhui	Lu'an area	Huoshan	Tax Yeabook	Gansu	Tianshui	Qinan	Fiscal Yearbook
Anhui	Lu'an area	Jinzhai	Tax Yeabook	Gansu	Tianshui	Qingshui	Fiscal Yearbook
Anhui	Lu'an area	Shouxian	Tax Yeabook	Gansu	Tianshui	Wushan	Fiscal Yearbook
Anhui	Lu'an area	Shucheng	Tax Yeabook	Gansu	Tianshui	Zhangchuan	Fiscal Yearbook
Anhui	Ma'anshan	Dangtu	Tax Yeabook	Guangdong	Guangzhou	Conghua	Fiscal Yearbook
Anhui	Suzhou	Dangshan	Tax Yeabook	Guangdong	Guangzhou	Zengcheng	Fiscal Yearbook
Anhui	Suzhou	Lingbi	Tax Yeabook	Guangxi	Guilin	Gongcheng	Fiscal Yearbook
Anhui	Suzhou	Sixian	Tax Yeabook	Guangxi	Guilin	Guanyang	Fiscal Yearbook
Anhui	Suzhou	Xiaoxian	Tax Yeabook	Guangxi	Guilin	Lingchuan	Fiscal Yearbook
Anhui	Tongling	Dongzhi	Tax Yeabook	Guangxi	Guilin	Lingui	Fiscal Yearbook
Anhui	Tongling	Qingyang	Tax Yeabook	Guangxi	Guilin	Lipu	Fiscal Yearbook
Anhui	Tongling	Shitai	Tax Yeabook	Guangxi	Guilin	Longsheng	Fiscal Yearbook
Anhui	Tongling	Tongling	Tax Yeabook	Guangxi	Guilin	Pingle	Fiscal Yearbook

Province	Prefecture	County	Data Sources	Province	Prefecture	County	Data Sources
Guangxi	Guilin	Quanzhou	Fiscal Yearbook	Heilongjiang	Jiamusi	Huanan	Fiscal Yearbook
Guangxi	Guilin	Xing'an	Fiscal Yearbook	Heilongjiang	Jiamusi	Tangyuan	Fiscal Yearbook
Guangxi	Guilin	Yangshuo	Fiscal Yearbook	Heilongjiang	Jiamusi	Tongjiang	Fiscal Yearbook
Guangxi	Guilin	Yongfu	Fiscal Yearbook	Heilongjiang	Mudanjiang	Dongning	Fiscal Yearbook
Guangxi	Guilin	Ziyuan	Fiscal Yearbook	Heilongjiang	Mudanjiang	Hailin	Fiscal Yearbook
Guangxi	Nanning	Paning	Fiscal Yearbook	Heilongjiang	Mudanjiang	Linkou	Fiscal Yearbook
Guangxi	Nanning	Wuming	Fiscal Yearbook	Heilongjiang	Mudanjiang	Muling	Fiscal Yearbook
Guizhou	Anshun	Guanling	Tax Yeabook	Heilongjiang	Mudanjiang	Ning'an	Fiscal Yearbook
Guizhou	Anshun	Pingba	Tax Yeabook	Heilongjiang	Mudanjiang	Suifenhe	Fiscal Yearbook
Guizhou	Anshun	Puding	Tax Yeabook	Heilongjiang	Qiqihar	Baiquan	Fiscal Yearbook
Guizhou	Anshun	Zhenning	Tax Yeabook	Heilongjiang	Qiqihar	Fuyu	Fiscal Yearbook
Guizhou	Anshun	Ziyun	Tax Yeabook	Heilongjiang	Qiqihar	Gannan	Fiscal Yearbook
Guizhou	Guiyang	Baiyun	Tax Yeabook	Heilongjiang	Qiqihar	Kedong	Fiscal Yearbook
Guizhou	Guiyang	Huaxi	Tax Yeabook	Heilongjiang	Qiqihar	Keshan	Fiscal Yearbook
Guizhou	Guiyang	Kaiyang	Tax Yeabook	Heilongjiang	Qiqihar	Longjiang	Fiscal Yearbook
Guizhou	Guiyang	Nanming	Tax Yeabook	Heilongjiang	Qiqihar	Tailai	Fiscal Yearbook
Guizhou	Guiyang	Qingzhen	Tax Yeabook	Heilongjiang	Qiqihar	Yian	Fiscal Yearbook
Guizhou	Guiyang	Udang	Tax Yeabook	Heilongjiang	Shuangyashan	Baoqing	Fiscal Yearbook
Guizhou	Guiyang	Xiaohe	Tax Yeabook	Heilongjiang	Shuangyashan	Jixian	Fiscal Yearbook
Guizhou	Guiyang	Xifeng	Tax Yeabook	Heilongjiang	Shuangyashan	Raohe	Fiscal Yearbook
Guizhou	Guiyang	Xiuwen	Tax Yeabook	Henan	Kaifeng	Kaifeng	Fiscal Yearbook
Guizhou	Guiyang	Yunyan	Tax Yeabook	Henan	Kaifeng	Lankao	Fiscal Yearbook
Hebei	Hengshui	Anping	Fiscal Yearbook	Henan	Kaifeng	Sixian	Fiscal Yearbook
Hebei	Hengshui	Fucheng	Fiscal Yearbook	Henan	Kaifeng	Tongxu	Fiscal Yearbook
Hebei	Hengshui	Gucheng	Fiscal Yearbook	Henan	Kaifeng	Weishi	Fiscal Yearbook
Hebei	Hengshui	Jinxiang	Fiscal Yearbook	Henan	Luohe	Linying	Fiscal Yearbook
Hebei	Hengshui	Jizhou	Fiscal Yearbook	Henan	Luohe	Wuyang	Fiscal Yearbook
Hebei	Hengshui	Raoyang	Fiscal Yearbook	Henan	Luohe	Yancheng	Fiscal Yearbook
Hebei	Hengshui	Shenzhou	Fiscal Yearbook	Henan	Luoyang	Songxian	Fiscal Yearbook
Hebei	Hengshui	Wuqiang	Fiscal Yearbook	Henan	Luoyang	Xin'an	Fiscal Yearbook
Hebei	Hengshui	Wuyi	Fiscal Yearbook	Henan	Luoyang	Yanshi	Fiscal Yearbook
Hebei	Hengshui	Zaoqiang	Fiscal Yearbook	Henan	Luoyang	Yichuan	Fiscal Yearbook
Hebei	Langfang	Bazhou	Fiscal Yearbook	Henan	Pingdingshan	Baofeng	Fiscal Yearbook
Hebei	Langfang	Dachang	Fiscal Yearbook	Henan	Pingdingshan	Jiaxian	Fiscal Yearbook
Hebei	Langfang	Dacheng	Fiscal Yearbook	Henan	Pingdingshan	Lushan	Fiscal Yearbook
Hebei	Langfang	Guan	Fiscal Yearbook	Henan	Pingdingshan	Ruzhou	Fiscal Yearbook
Hebei	Langfang	Sanhe	Fiscal Yearbook	Henan	Pingdingshan	Wugang	Fiscal Yearbook
Hebei	Langfang	Wenan	Fiscal Yearbook	Henan	Pingdingshan	Yexian	Fiscal Yearbook
Hebei	Langfang	Xianghe	Fiscal Yearbook	Henan	Sanmenxia	Lingbao	Fiscal Yearbook
Hebei	Langfang	Yongqing	Fiscal Yearbook	Henan	Sanmenxia	Lushi	Fiscal Yearbook
Hebei	Shijiazhuang	Gaoyi	Fiscal Yearbook	Henan	Sanmenxia	Mianchi	Fiscal Yearbook
Hebei	Shijiazhuang	Jingxing	Fiscal Yearbook	Henan	Sanmenxia	Shanxian	Fiscal Yearbook
Hebei	Shijiazhuang	Yuanshi	Fiscal Yearbook	Henan	Sanmenxia	Sheqi	Fiscal Yearbook
Hebei	Xingtai	Pingxiang	Fiscal Yearbook	Henan	Sanmenxia	Yima	Fiscal Yearbook
Hebei	Zhangjiakou	Wanquan	Fiscal Yearbook	Henan	Zhengzhou	Dengfeng	Fiscal Yearbook
Hebei	Zhangjiakou	Xuanhua	Fiscal Yearbook	Henan	Zhengzhou	Gongyi	Fiscal Yearbook
Heilongjiang	Jiamusi	Fujin	Fiscal Yearbook	Henan	Zhengzhou	Rongyang	Fiscal Yearbook
Heilongjiang	Jiamusi	Fuyuan	Fiscal Yearbook	Henan	Zhengzhou	Xinmi	Fiscal Yearbook
Heilongjiang	Jiamusi	Huachuan	Fiscal Yearbook	Henan	Zhengzhou	Xinzheng	Fiscal Yearbook

Province	Prefecture	County	Data Sources	Province	Prefecture	County	Data Sources
Henan	Zhengzhou	Zhongmou	Fiscal Yearbook	Hubei	Xiantao	Xiantao	Tax Yeabook
Henan	Zhumadian	Xiping	Fiscal Yearbook	Hubei	Xiaogan	Anlu	Tax Yeabook
Hubei	Enshi	Badong	Tax Yeabook	Hubei	Xiaogan	Dawu	Tax Yeabook
Hubei	Enshi	Hefeng	Tax Yeabook	Hubei	Xiaogan	Hanchuan	Tax Yeabook
Hubei	Enshi	Jianshi	Tax Yeabook	Hubei	Xiaogan	Xiaochang	Tax Yeabook
Hubei	Enshi	Laifeng	Tax Yeabook	Hubei	Xiaogan	Yingcheng	Tax Yeabook
Hubei	Enshi	Lichuan	Tax Yeabook	Hubei	Xiaogan	Yunmeng	Tax Yeabook
Hubei	Enshi	Xianfeng	Tax Yeabook	Hubei	Yichang	Changyang	Tax Yeabook
Hubei	Enshi	Xuan'en	Tax Yeabook	Hubei	Yichang	Dangyang	Tax Yeabook
Hubei	Forest zone	Linqu	Tax Yeabook	Hubei	Yichang	Genggui	Tax Yeabook
Hubei	Huanggang	Hongan	Tax Yeabook	Hubei	Yichang	Wufeng	Tax Yeabook
Hubei	Huanggang	Huangmei	Tax Yeabook	Hubei	Yichang	Xingshan	Tax Yeabook
Hubei	Huanggang	Loushui	Tax Yeabook	Hubei	Yichang	Yidu	Tax Yeabook
Hubei	Huanggang	Luotian	Tax Yeabook	Hubei	Yichang	Yuanan	Tax Yeabook
Hubei	Huanggang	Macheng	Tax Yeabook	Hubei	Yichang	Zhijiang	Tax Yeabook
Hubei	Huanggang	Tuanfeng	Tax Yeabook	Hunan	Huaihua	Hongjiang	Fiscal Yearbook
Hubei	Huanggang	Weichun	Tax Yeabook	Hunan	Zhuzhou	Youxian	Fiscal Yearbook
Hubei	Huanggang	Wuxue	Tax Yeabook	Jiangsu	Changzhou	Jintan	Fiscal Yearbook
Hubei	Huanggang	Yingshan	Tax Yeabook	Jiangsu	Changzhou	Piaoyang	Fiscal Yearbook
Hubei	Huangshi	Daye	Tax Yeabook	Jiangsu	Lianyungang	Donghai	Fiscal Yearbook
Hubei	Huangshi	Yangxin	Tax Yeabook	Jiangsu	Lianyungang	Ganyu	Fiscal Yearbook
Hubei	Jingmen	Jingshan	Tax Yeabook	Jiangsu	Lianyungang	Guanna	Fiscal Yearbook
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Hubei	Jingmen	Zhongxiang	Tax Yeabook	Jiangsu	Nantong	Hai'an	Fiscal Yearbook
Hubei	Jingzhou	Gong'an	Tax Yeabook	Jiangsu	Nantong	Haimen	Fiscal Yearbook
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Hubei	Jingzhou	Jianli	Tax Yeabook	Jiangsu	Nantong	Rugao	Fiscal Yearbook
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Hubei	Qianjiang	Qianjiang	Tax Yeabook	Jiangsu	Suzhou	Kunshan	Fiscal Yearbook
Hubei	Suizhou	Guangshui	Tax Yeabook	Jiangsu	Suzhou	Taicang	Fiscal Yearbook
Hubei	Ten Kansas	Fangxian	Tax Yeabook	Jiangsu	Suzhou	Wujiang	Fiscal Yearbook
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Hubei	Xianning	Chongyang	Tax Yeabook	Jiangxi	Yichun	Fengcheng	Fiscal Yearbook
Hubei	Xianning	Jiayu	Tax Yeabook	Jiangxi	Yichun	Fengxin	Fiscal Yearbook
Hubei	Xianning	Tongcheng	Tax Yeabook	Jiangxi	Yichun	Gaoan	Fiscal Yearbook
Hubei	Xianning	Tongshan	Tax Yeabook	Jiangxi	Yichun	Jing'an	Fiscal Yearbook

Province	Prefecture	County	Data Sources	Province	Prefecture	County	Data Sources
Jiangxi	Yichun	Shanggao	Fiscal Yearbook	Shanxi	Linfen	Xiangfen	Fiscal Yearbook
Jiangxi	Yichun	Tonggu	Fiscal Yearbook	Shanxi	Yuncheng	Wanrong	Fiscal Yearbook
Jiangxi	Yichun	Wanzai	Fiscal Yearbook	Shanxi	Yuncheng	Yuanqu	Fiscal Yearbook
Jiangxi	Yichun	Yifeng	Fiscal Yearbook	Shanxi	Yan'an	Ansai	Fiscal Yearbook
Jiangxi	Yichun	Zhangshu	Fiscal Yearbook	Shanxi	Yan'an	Fuxian	Fiscal Yearbook
Jilin	Siping	Gongzhuling	Fiscal Yearbook	Shanxi	Yan'an	Ganquan	Fiscal Yearbook
Jilin	Siping	Lishu	Fiscal Yearbook	Shanxi	Yan'an	Huangling	Fiscal Yearbook
Jilin	Siping	Shuangliao	Fiscal Yearbook	Shanxi	Yan'an	Huanglong	Fiscal Yearbook
Jilin	Siping	Yitong	Fiscal Yearbook	Shanxi	Yan'an	Luochuan	Fiscal Yearbook
Jilin	Yanbian	Yanji	Fiscal Yearbook	Shanxi	Yan'an	Wuqi	Fiscal Yearbook
Ningxia	Yinchuan	Helan	Fiscal Yearbook	Shanxi	Yan'an	Yanchuan	Fiscal Yearbook
Ningxia	Yinchuan	Yongning	Fiscal Yearbook	Shanxi	Yan'an	Yanzhang	Fiscal Yearbook
Shandong	Dezhou	Leling	Tax Yeabook	Shanxi	Yan'an	Yichuan	Fiscal Yearbook
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Shandong	Dezhou	Yucheng	Tax Yeabook	Sichuan	Aba	Li	Tax Yeabook
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Shandong	Dongying	Kenli	Tax Yeabook	Sichuan	Aba	Maoxian	Tax Yeabook
Shandong	Dongying	Lijin	Tax Yeabook	Sichuan	Aba	Rangtang	Tax Yeabook
Shandong	Jinan	Jiyang	Fiscal Yearbook	Sichuan	Aba	Ruoergai	Tax Yeabook
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Shanxi	Jinzhong	Qixian	Fiscal Yearbook	Sichuan	Nanchong	Yingshan	Tax Yeabook

Province	Prefecture	County	Data Sources	Province	Prefecture	County	Data Sources
Sichuan	Panzhihua	Miyi	Fiscal Yearbook	Zhejiang	Jinhua	Wuyi	Fiscal Yearbook
Sichuan	Panzhihua	Yanbian	Fiscal Yearbook	Zhejiang	Jinhua	Yiwu	Fiscal Yearbook
Sichuan	Suining	Daying	Fiscal Yearbook	Zhejiang	Jinhua	Yongkang	Fiscal Yearbook
Sichuan	Ziyang	Lezhi	Tax Yeabook	Zhejiang	Lishui	Jingning	Fiscal Yearbook
Tianjin	Tianjin	Jinghai	Fiscal Yearbook	Zhejiang	Lishui	Jinyun	Fiscal Yearbook
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Tianjin	Tianjin	Ninghe	Fiscal Yearbook	Zhejiang	Lishui	Qingtian	Fiscal Yearbook
Xinjiang	Shihezi	Shihezi	Fiscal Yearbook	Zhejiang	Lishui	Qingyuan	Fiscal Yearbook
Yunnan	Chuxiong	Chuxiong	Tax Yeabook	Zhejiang	Lishui	Songyang	Fiscal Yearbook
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Zhejiang	Hangzhou	Chunan	Fiscal Yearbook	Zhejiang	Wenzhou	Pingyang	Tax Yeabook
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Zhejiang	Heng	Changshan	Tax Yeabook	Zhejiang	Wenzhou	Yongjia	Tax Yeabook
Zhejiang	Heng	Jiangshan	Tax Yeabook	Zhejiang	Wenzhou	Yueqing	Tax Yeabook
Zhejiang	Heng	Kaihua	Tax Yeabook	Zhejiang	Zhoushan	Daishan	Fiscal Yearbook
Zhejiang	Heng	Longyou	Tax Yeabook	Zhejiang	Zhoushan	Shengsi	Fiscal Yearbook
Zhejiang	Huzhou	Anji	Tax Yeabook				
Zhejiang	Huzhou	Changxing	Tax Yeabook				
Zhejiang	Huzhou	Deqing	Tax Yeabook				
Zhejiang	Jiaxing	Haining	Tax Yeabook				
Zhejiang	Jiaxing	Haiyan	Tax Yeabook				
Zhejiang	Jiaxing	Jiashan	Tax Yeabook				
Zhejiang	Jiaxing	Pinghu	Tax Yeabook				
Zhejiang	Jiaxing	Tongxiang	Tax Yeabook				
Zhejiang	Jinhua	Dongyang	Fiscal Yearbook				
Zhejiang	Jinhua	Lanxi	Fiscal Yearbook				
Zhejiang	Jinhua	Pan'an	Fiscal Yearbook				
Zhejiang	Jinhua	Pujiang	Fiscal Yearbook				

Chapter 2:

Do giant oilfield discoveries fuel internal armed conflicts?

Yu-Hsiang Lei and Guy Michaels

We use new data to examine the effects of giant oilfield discoveries around the world since 1946. On average, these discoveries increase per capita oil production and oil exports by up to 50%. But these giant oilfield discoveries also have a dark side: they increase the incidence of internal armed conflict by about 5–8 percentage points. This increased incidence of conflict due to giant oilfield discoveries is especially high for countries that had already experienced armed conflicts or coups in the decade prior to discovery.

1. Introduction

Do natural resource windfalls, such as those arising from the discovery of giant oilfields, increase the risk of internal armed conflict? Anecdotal evidence from Nigeria, Angola, and Iraq leads us to suspect that they may, and recent research (Acemoglu et al., 2010; Besley and Persson, 2009, 2011; Dal Bó and Dal Bó, 2011) even sheds light on the mechanisms that underlie some of these conflicts over resources. But as Norway, Canada, and Brazil show, not all oil rich countries experience conflict. Careful surveys of the literature on conflicts and natural resources (e.g. Blattman and Miguel, 2010; Ross, 2004, 2006) show how difficult it has been to estimate the causal effect of oil on armed conflict in all but a handful of countries.[1] The goal of this paper is to examine whether giant oilfield discoveries really do fuel internal armed conflicts around the world, and if so — in which settings.

We begin with a simple model, following Besley and Persson (2009), which guides our empirical analysis. In this model, giant oilfield discoveries increase oil revenues, generating windfall income for the incumbent. When the incumbent cannot credibly commit to share this windfall, the opposition may mobilize to challenge him, and this may lead to an internal armed conflict. Such conflicts over resources are especially likely in countries where political violence tends to translate into political and economic gains.

To investigate this model's predictions, we ideally require exogenous variation in resource windfalls. Finding such variation in multiple countries is challenging, since cross-country (or cross-conflict) comparisons may be contaminated by omitted variables bias. Using panel data to absorb country fixed effects is not straightforward either, because the quantity of natural resources extracted is a choice and oil prices may be affected by violent con-

[1] Studies of the causal effect of natural resources on conflict tend to focus on specific countries. For example, Angrist and Kugler (2008) and Dube and Vargas (2013) study the effect of resource windfalls on conflict in Colombia, and Bellows and Miguel (2009) study this effect in Sierra Leone. Also closely related is contemporaneous work by Cotet and Tsui (2013) on oil and conflict, which we discuss below.

flict. To overcome this difficulty, we focus on the discovery of giant oilfields, each of which contained ultimate recoverable reserves (URR) of 500 million barrels (bbl) equivalent or more before extraction began (data on these giant oilfields are reported in Horn, 2004).[2] Of the 910 giant oilfields that were known as of 2003, we focus on the 782 giants that were discovered since 1946 in 65 different countries.

We show evidence that in a panel of countries, controlling for country and year fixed effects, the timing of giant oilfield discoveries is plausibly exogenous, at least in the short-medium run. To see why, consider how important giant oilfields are as a global source of hydrocarbons. Horn (2007) concludes that giant oilfields account for over 40% of the world's URR of oil and gas. Discoveries of these giant fields are therefore economically important events, which are rare in all but a handful of countries: in less than 5% of the country–year observations in our global dataset was one or more giant oilfield discovered. It is true that countries can influence the prospecting efforts within their territory, and thus affect the discovery rate. But prospecting for oil is highly uncertain, and the odds of finding a giant oilfield are usually low, so countries have little control over the timing of such finds. Below we discuss a wide range of empirical tests, which support our interpretation that of the events that follow giant oilfield discoveries as causal. But before we further discuss our causal interpretation of the findings, we first describe them.

We find using a panel of 193 countries from1946 to 2008 that on average oil production increases by about 35–50 percentage points within 4–10 years of a giant discovery.[3] Giant oilfield discoveries similarly increase oil exports

[2]Unless otherwise specified, we use "oil" as a shorthand that also includes condensate and natural gas. To determine whether an oilfield has estimated ultimate recoverable reserves of 500 million bbl of oil equivalent or more, the estimated reserves of oil and condensate are summed up. These are then added to the amount of natural gas, which is converted to oil at a ratio of 6000 cu ft/bbl (Horn, 2004). Note that ultimate recoverable reserves include the amount already extracted and the amount that has not yet been extracted.

[3]We use all the countries in the world, even those that do not discover giant oilfields. This allows us to control for countries where non-giant discoveries are made (as discussed below), and for variation in countries that do not discover oil, and which may affect the estimated year effects in the panel regressions.

by about 20–50% within 6–10 years.

Having found evidence suggesting a large impact of giant oilfield discoveries on oil output, we next examine their impact on conflict. We find that on average giant oilfield discoveries increase the incidence of internal armed conflicts (measured as a year with 25 or more conflict casualties) by about 5–8 percentage points within 4–8 years of discovery, compared to a baseline probability of about 10 percentage points.

We also find that the discovery of giant oilfields is especially likely to fuel internal conflicts in countries with recent histories of political violence. For example, giant oilfield discoveries increase the incidence of internal armed conflict by about 11–18 percentage points (compared to a baseline probability of about 37–39%) when a country experienced at least one such conflict in the decade prior to discovery. Giant oilfield discoveries similarly increase the odds of internal armed conflict by 11–14 percentage points (compared to a baseline probability of about 19– 20%) in countries that experienced at least one coup in the decade prior to discovery. In contrast, in countries that experienced no internal conflicts or coups in the decade before a discovery, there is no significant effect of giant oilfield discoveries on the incidence of internal armed conflicts.

Turning to the effect of giant oilfield discoveries on economic outcomes, we find suggestive evidence that per capita GDP and government spending may have increased by about 4–6% within the decade following a giant discovery. But unlike our results on conflict, these estimates are not robust to the different specifications that we consider. Moreover,we find no evidence that giant oilfield discoveries significantly affect private consumption or spending.

To support our interpretation that the findings described above are the causal consequences of giant oilfield discoveries, we report results from a number of robustness checks. First, we address the concern that giant oilfield discoveries may have resulted from economic or political changes that

preceded them. Reassuringly, we find no evidence of significant economic or political changes in the five years leading up to giant oilfield discoveries. We also test whether giant oilfield discoveries follow lulls in previous conflicts, and find no evidence to support this hypothesis. Second, we tackle the concern that giant oilfield discoveries are serially correlated over time, because some oilfields are close together, so one finding one may lead to another. While it is true that giant oilfield discoveries in a country's recent past increase the odds that it finds a giant oilfield in a given year, controlling for these past discoveries does not change our estimates by much. Our results are also robust to excluding country–year observations within a decade or less of previous giant discoveries. Observations with giant oilfield discoveries account for only about 1% of the remaining sample, making them especially difficult to anticipate. Third, we address concerns that economic or political conditions shortly before discovery may affect our estimates, by showing that our results are robust to controlling for (instrumented) lagged dependent variables, lagged institutional quality (polity 2), and lagged aggregate private investment. Fourth, we tackle the concern that observations with oil discoveries are different from others in ways that we cannot measure and control for directly. To do so, we use the Oil and Gas Journal Data Book (2008) to restrict our sample to observations where at least one oil discovery – not necessarily of a giant oilfield –was made. Regressions using this sample compare the effect of giant oilfield discoveries to the effect of smaller oilfield discoveries. Remarkably, even in this restricted sample we find that our results hold.

Our finding that giant oilfield discoveries fuel internal conflicts in countries prone to violence has policy implications. Those who strive to reduce armed conflict should be concerned about oil rents that incumbents obtain in conflict-prone areas, especially if those rents encourage challenges to the incumbents' power. And firms that prospect for oil in conflict-prone areas and

those who regulate them ought to be concerned about negative externalities for many locals,who have little to gain from giant oilfield discoveries but may suffer from conflicts over the oil.

The remainder of the paper is organized as follows. Section 2 discusses the related literature, Section 3 presents a model of conflict over oil revenues, Section 4 discusses the data, Section 5 presents our results, and Section 6 concludes.

2. Related Literature

Concerns that some natural resources – including oil – may fuel internal armed conflicts arise from observing at oil-rich countries, such as Angola, Colombia, Iraq, Sudan, and Indonesia. A number of influential papers including Collier and Hoeffler (1998, 2004) and Reno (1999) have investigated the relationship between natural resources and conflict, sparking considerable interest among social scientists and policy makers. Surveys of the developing literature on this topic, including Ross (2004, 2006), Humphreys (2005), and Blattman and Miguel (2010), conclude that there is evidence linking oil to some instances of internal armed conflict. At the same time, not all oil-rich countries experience internal armed conflict, so conflicts over resources are clearly not inevitable.[4]

Theoretical studies of the links between natural resource rents and conflict have focused on the possibility that these conflicts are the result of competition over resources. Summarizing this literature, Blattman and Miguel (2010) point out that models of armed conflict typically consider the cases where property rights are not well-protected, contracts are imperfectly enforced, and rulers are not always replaced by fair elections. Recent contributions to the literature on conflicts over resources include Garfinkel and Skaperdas

[4]For example, Michaels (2011) and Caselli and Michaels (2013) find no evidence of armed conflict in the U.S. South and in Brazil.

(2007), Dal Bó and Dal Bó (2011), Besley and Persson (2009, 2011), Caselli and Cunningham (2009), Acemoglu et al. (2010), Miguel and Satyanath (2011), Harari and La Ferrara (2013), and Caselli et al. (2013). Recent evidence on the effect of U.S. food aid on civil conflict (Nunn and Qian, 2014) is also highly relevant.

But despite all this research on the relation between natural resources and armed conflict, establishing the causal effect of resource windfalls on conflict around the world has been difficult. Some of the best-identified studies examine causality using regional variation within countries. For example, Bellows and Miguel (2009) find that chiefdoms with more diamond wealth in Sierra Leone experienced more armed clashes, and studies of Colombia find that high coca prices increase conflict in coca producing regions (Angrist and Kugler, 2008) and high oil prices increase conflict in areas where oil is extracted from or shipped through in pipelines (Dube and Vargas, 2013).

Taken together, the evidence from within-country studies suggests that natural resource windfalls can fuel armed conflicts, at least in some countries and settings. But in order to generalize these findings to the rest of the world and to better understand in what settings natural resource windfalls are more likely to cause armed conflict, it seems useful to look beyond the boundaries of specific nations. It turns out, however, that using variation from multiple countries to identify the effect of natural resource on conflict is not straightforward. To see why this is a challenge, consider first comparisons of resource rich countries with resource scarce ones, or of conflicts that take place in resource rich and resource scarce parts of the world. The main concern about this approach is that resource-rich areas might differ from others in ways that are difficult to measure and control for. For example, the Middle East is not only rich in oil but it also differed from other parts of the world in important ways before oil was discovered. These differences, which are notoriously hard to quantify, along with oil abundance, may have caused

subsequent conflicts, and telling apart the causes is difficult.

To overcome the problem of fixed differences between countries, we could consider a second approach, which interacts country-specific measures of oil abundance with variation over time in oil prices. But this approach suffers from concerns about reverse causality, since conflicts may raise oil prices, as they probably did during the Arab-Israeli War in 1973, the Iranian Revolution in 1979, the Kuwait War in 1990, and the Libyan Civil War in 2011, making the direction of causality between conflict and resource revenues difficult to ascertain.[5]

A third approach we could have pursued uses time-varying measures of oil production or exports in each country. But this approach also has problems in shedding light on causality, since countries choose the amount of oil they extract, and potential buyers may also choose how much to buy from whom. These choices may respond, directly or indirectly, to armed conflicts or their underlying causes.[6]

Since identifying the causal effect of natural resources on conflict using the approaches described above is difficult, our paper focuses on the discovery of giant oilfields as a more plausibly exogenous source of variation. A closely related study in this respect is contemporaneous work by Cotet and Tsui (2013), which concludes that while the defense burden increases following oil discoveries, conflict does not increase significantly. There are several differences in the implementation of their paper and ours. First, our data, unlike theirs, cover the entire world, focus only on giant oilfield discoveries, and measure not only oil deposits, but also gas and condensate. Second, we report large and significant effects of giant discoveries on oil output and oil

[5]The possibility that internal conflict in Libya increased oil prices was discussed by the media. See for example: http://www.bbc.co.uk/news/business-12522291. When we regress an indicator for internal conflict on an interaction of an indicator for countries with at least one giant oilfield and the log of inflation-adjusted oil price, controlling for country and year fixed effects, we get a coefficient of 0.044 (s.e. 0.024), suggesting a positive and marginally significant relation between the two.

[6]For example, the recent internal armed conflict in Syria appears to have reduced its oil production: http://www.ft.com/cms/s/0/c9d67952-e823-11e0-9fc7-00144feab49a.html#axzz1aOqrle6u.

exports (both measured per capita), while they do not. The giant discoveries which we study, most likely reflect larger prizes over which rivals may fight. Third, while Cotet and Tsui (2013) choose to emphasize instrumental variables estimates where the relationship between oil discoveries and conflict is positive but imprecisely estimated, some of the other estimates that they report (e.g. in Table 10) actually are positive and significant. Finally, we have incorporated data used by Cotet and Tsui (2013) into our robustness checks. We show that applying our methodology to their data yields estimates that are quite similar to ours. In other words, even using their data, major oil discoveries are followed by increases in internal armed conflict.

3. A model of conflict for resources

To guide our empirical analysis, we begin with a simple model of conflict over resources, following Besley and Persson (2009).[7] The model focuses on two potentially conflicting groups denoted by J: an incumbent I and an opposition O. Each group makes up half of the population and can mobilize a fraction A^J of its citizens to serve in its army. The decision of each group whether or not to mobilize an army is discrete, and is denoted by $\delta^J \in 0,\ A^J$. The probability that power transitions from the incumbent to the opposition is determined by a conflict function: Prob(change of power)$= \frac{1}{2} + \frac{1}{\mu}\{\delta^O - \delta^I\}$. The parameter μ captures the degree to which the country can resist political violence, and low values of μ mean that i political violence is a more practical means of transferring power. We assume that $\frac{A^I}{\mu} \leq \frac{1}{2} \leq 1 - \frac{A^O}{\mu}$, which holds as long as μ is sufficiently large.

The winning group has access to government revenue denoted by R, which comes from natural resources. These resources must be shared according to an institutional rule, which stipulates that the incumbent gets $(1-\theta)2R$

[7] As we explain below, we depart from their model only in relatively minor details.

and the opposition gets $2\theta R$, where $\theta \in [0, \frac{1}{2}]$. In other words, we consider sharing rules that range from institutions that lead to complete equality $\theta = \frac{1}{2}$ to institutions where the winner takes all ($\theta = 0$).

In addition to any revenues they may receive from the government, each citizen supplies one unit of labor to the market, earning a real wage w. A group that wants to finance its army does so by taxing its population. Since we are interested primarily in bilateral internal conflicts (as opposed to one-sided conflicts), we depart from Besley and Persson (2009) by assuming that the opportunity cost of fighting is equal for the opposition and the incumbent.[8]

The timing of events within each period is as follows. First the amount of resources at the government's disposal, R, is determined randomly. We assume that if a giant oilfield is discovered then $R = RH$, and otherwise $R = 0$.[9] Second, the opposition decides whether to mobilize its army to fight the incumbent. Third, the government decides whether to mobilize its own army to fight the opposition. We assume that both the opposition and the incumbent only mobilize if the net expected returns to mobilization are strictly positive, and an internal conflict takes place if at least one party mobilizes an army. Fourth, these choices and the probabilistic conflict technology then determine who wins power. Finally, the winner allocates the resources R.

Given our assumptions, the expected per capita payoff to incumbent members is:

$$w(1 - \delta^I) + \{\frac{1}{2} - \frac{1}{\mu}(\delta^O - \delta^I)(1 - 2\theta)\}2R \tag{1}$$

[8]Besley and Persson (2009) study repression as one-sided violence by an incumbent, which has lower opportunity cost of fighting than the opposition since he can finance part of his army by taxing that opposition. In our empirical analysis (Subsection 5.3) we therefore examine the possibility of repression in the aftermath of giant oil discoveries.

[9]Besley and Persson (2009) do not focus on oil discoveries but on rents in general. Our assumption of two states of the world – with and without giant oil discoveries – makes the model more closely related to our empirical analysis.

where the first term wages net of taxes, and the second is the expected size of the transfer. Similarly, the expected payoff to opposition members is:

$$w(1-\delta^O) + \{\frac{1}{2} - \frac{1}{\mu}(\delta^O - \delta^I)(1-2\theta)\}2R \tag{2}$$

To solve for the equilibrium we identify the sub-game perfect Nash equilibrium in the sequential game where the opposition moves first. It turns out that this game has two equilibria:

- Peace (when neither side mobilizes): $\delta^O = \delta^l = 0$, which occurs when $2R(1-2\theta)/w \leq \mu$.
- Internal conflict (when both sides mobilize): $\delta^I = A^I$ and $\delta^O = A^O$, which occurs when $2R(1-2\theta)/w > \mu$.

This model guides our empirical analysis of the effect of giant oilfields in a number of ways. First, the model assumes that giant oilfield discoveries increase oil revenues. While this assumption seems very plausible, it may take time to start generating revenues from newly discovered oilfields, especially if it is difficult to extract the oil or if the discovering country lacks the appropriate technology, capital, or infrastructure. While we cannot measure oil revenues, we can measure oil production and oil exports, and our first empirical challenge is to determine whether they increase significantly within a few years of discovery, and if so - by how much.

Second, we investigate the effect of oil discovery on internal armed conflict. The model predicts that in countries where $2R^H(1-2\theta)/w > \mu$, the discovery of a giant oilfield ends peace and sets off an internal conflict. This can happen when the incumbent receives most of the oil, and cannot commit to sharing them with the opposition. If conditions are otherwise ripe for conflict, a giant oilfield discovery can fuel conflict over the oil.

Third, giant oilfield discoveries are likely to set off conflict only in countries where political violence is seen as effective, namely where μ is sufficiently

low. Empirically, we identify countries with low μ as those with a history of internal conflicts or coups in the decade prior to the discovery of a giant oilfield. It is in those countries that we expect giant oilfield discoveries to trigger armed conflicts over the control of the oil. The model also allows for the possibility that giant oilfield discoveries fuel conflicts in countries with low wages (which imply a low opportunity cost of fighting), poor institutions that increase inequality (θ close to zero represents "winner takes all" societies, where it pays to fight for control), or ethnic fractionalization that creates conflicting groups to begin with. In practice, however, underlying characteristics such as income, institutions, and ethnic fractionalization may be interrelated with each other and with the degree to which political violence pays off (μ). In the empirical analysis below (Section 5) we focus not only on the interaction of giant oilfield discoveries with empirical measures of μ, but we also examine other possible interactions related to the model.

Finally, the discovery of a giant oilfield increases government revenues, R, and total per capita GDP, $R+w$. The increase of log per capita GDP in this model is $\partial ln(R+w)/\partial ln(R) = R/(R+w)$, or in other words the proportional increase in GDP as a result of an oilfield discovery is less than the proportional increase in oil revenues as a result of this discovery. Moreover, as we discussed, in some cases oil discoveries cause mobilization, and this may reduce civilian per capita GDP. Any additional factors which are not modeled, such as the cost of conflict or any distortionary effect of oil on the rest of the economy, may further reduce the net benefits of giant oilfield discovery. Given these caveats, Subsection 5.3 investigates the effect of giant oil discoveries on GDP and its components.

4. Data on oil, conflicts, and economic outcomes

To analyze the effects of giant oilfield discoveries we require panel data on the timing of these discoveries in addition to outcome measures and control variables. Since country definitions differ over time and usage, we use the country definitions from the Penn World Table (Heston et al., 2009), a commonly used dataset, as the basis for our analysis.[10] The Penn World Table reports data on countries from 1950 to 2007, but we examine all the conflicts that took place after the end of the Second World War (see below), so some of the variables we match in from other sources span the years from 1946 to 2008, which is our period of analysis.

4.1. Data on oil discovery and production

Our main regressor of interest is an indicator for the discovery of (at least one) giant oil field in a given country in a given year. We use data from Horn (2003, 2004), which reports the date of discovery, the name of the discovering country, and a number of other variables, for 910 giant oilfields discovered both onshore and offshore from 1868 to 2003. This dataset builds on previous datasets (e.g. Halbouty et al., 1970), and attempts to include every giant oilfield discovered around the world. To qualify as a giant (and thus be included in the dataset), an oilfield must have contained ultimate recoverable reserves (URR) of at least 500million barrels of oil equivalent (MMOBE). One limitation of these data is that the oilfields it describes differ considerably in the identity of those who estimated the URR and in the way the URR was estimated. Moreover, the estimated URR of various oilfields was gradually updated, depending on the estimators and their methods.[11] Since

[10]We add three Communist countries which existed until the early 1990s: the USSR (until 1991), Yugoslavia (until 1991), and Czechoslovakia (until 1992); the countries that emerged from these three are covered in our dataset from the year following the corresponding collapse. We also add North Korea,Myanmar, and Netherlands Antilles. Our results are robust to excluding these countries.

[11]For example, some oilfields' URR was updated from an earlier version of the dataset we use (compare Horn, 2004, 2003).

this process may induce measurement error issues across oilfields, we simply construct an indicator for whether a country is mentioned in the dataset as having discovered at least one giant oilfield in each given year. This does not avoid all forms of measurement error, as some oilfields may have been incorrectly included in the dataset or excluded from it, but we consider this a reasonable compromise given the limitations of the data.[12]

Of the 910 giant oilfields covered in Horn (2004), 782were discovered from 1946 onwards, and these discoveries took place in 65 different countries. The 461 country–year observations with giant discoveries account for less than 5% of all the observations in our data, and in all but a few countries giant oilfield discoveries are rare events (Table A1 lists the number of observations with discoveries in each discovering country). The rate of giant oilfield discoveries peaked during the 1960s and 1970s, and country–year pairs with discoveries were most common in Asia (41%), followed by Europe (18%), Africa (16%), North America (12%), South America (9%), and Oceania (4%).[13] Our dataset contains 285 country–year observations with giant onshore discoveries and 213 country–year observations with giant offshore discoveries. These figures include 37 country–year observations with both onshore and offshore giant discoveries. Table 1 reports summary statistics for our measure of giant oilfield discoveries and for other variables that we describe below.

We complement our data on giant oilfield discoveries with data on the timing of other oilfield discoveries from the Oil and Gas Journal Data Book (2008). This source reports more discoveries than our main dataset, since it is not limited to giant oilfield discoveries, but its main drawback is that the quantity of oil discovered is not reported for most oilfields. In addition, these data seem to focus on oil-producing fields, so they may exclude some gas

[12]Nonetheless, in some robustness checks below we report separate estimates for giant oilfields of different sizes.

[13]The continent classification follows that of the United Nations (U.N. Statistics Division, 2013). The country–year distribution of discovery by decades is 3% for 1946–1949, 15% for 1950s, 22% for 1960s, 22% for 1970s, 14% for 1980s, 17% for 1990s, and 7% for 2000–2003.

fields.[14] But these data are still useful, since they allow us to restrict parts of our analysis to observations with oil discoveries, and compare the effect of giant oilfield discoveries to discoveries of smaller fields.

In our robustness checks we incorporate into our dataset two variables from the dataset constructed by Cotet and Tsui (2013).[15] Both variables are measured by country-year observations, for 63 countries from 1946-2003.[16] The first variable is the number of "wildcat" (exploratory) oil wells drilled, which is a proxy for oil exploration efforts. In addition to using this variable directly, we also construct an indicator for a positive number of wildcat wells. The second variable is the quantity of oil discovered, which we use to construct indicators for years with "giant" discoveries (years with total URR of at least 500 million barrels) and for years with smaller discoveries. These variables differ from those that we use in our main analysis in several ways. First, the sources of data that Cotet and Tsui (2013) use are different from ours, and they differ in the way they estimate ultimate recoverable reserves (URR). Second, their measure includes only oil discoveries, while ours includes not only oil but also natural gas and condensate. Finally, the measure that Cotet and Tsui (2013) use aggregates the URR over all discoveries within a country in a given year, while our measure effectively uses only the largest single discovery. The correlation between our indicator for giant oilfield discoveries and an indicator for years where the Cotet and Tsui (2013) report discoveries with URR of at least 500 million barrels, is around 0.55.

Our final source for data on oil is Ross (2011), which reports the value of production of oil and gas by country and year from 1932 onwards.[17] These

[14]Some fields covered in Horn (2004) do not appear in the Oil and Gas Journal Data Book (2008), even though this latter source covers smaller fields, so it reports more fields overall. This may be because the coverage of the Oil and Gas Journal Data Book is uneven across countries, whereas Horn (2004) attempts to cover all giant oilfields in all countries.

[15]Their dataset is available at: http://www.aeaweb.org/aej/mac/data/2010-0022_data.zip, and is discussed in Cotet and Tsui (2013) and Tsui (2011).

[16]The dataset that Cotet and Tsui (2013) construct includes Papua New Guinea, which is nonetheless excluded from their econometric analysis of the remaining 62 countries.

[17]Details of data construction can be found in Ross (2010).

data allow us to examine whether giant oilfield discoveries affect the value of oil and gas which a country produces. We convert this variable into US$2005, in line with our other variables below, using the CPI index from US Bureau of Labor Statistics.

4.2. Data on economic outcomes

The Penn World Table (PWT 6.3, 2009) is our source for GDP-related measures and population from 1950-2007. We use this dataset to construct Purchasing Power Parity (PPP) adjusted per-capita GDP in constant US$2005, and to decompose it into private consumption, private investment, and government expenditure.[18] We also construct a measure of the real exchange by taking the ratio of the nominal exchange rate (XRAT, which measures dollars per local currency unit) to PPP. Using this definition, a decrease in the real exchange rate corresponds to a real exchange rate appreciation. In addition, we supplement the PWT data with International Monetary Fund (IMF) data (Abbas et al. 2010) on public debt as a percentage of GDP.

To measure countries' international trade, we use the NBER-UN trade data (Feenstra et al. 2005), which reports trade outcomes from 1962-2000. We construct per capita measures of oil exports and non-oil exports. This last measure is constructed by summing up the exports in SITC Revision 2 categories 33 (Petroleum, petroleum products and related materials) and 34 (Gas, natural and manufactured). We convert all these measures into US$2005 as described above.

[18]PPP-adjusted GDP per capita is constructed using rgdpl (real GDP per capita, Laspeyres) and the components of GDP are constructed by multiplying each share, kc (private consumption), ki (private investment), kg (government spending), to rgdpl. All these variables are from PWT 6.3.

4.3. Data on political violence

We use the UCDP/PRIO dataset (Gleditsch et al. 2002) to measure the incidence of internal armed conflicts from 1946-2008.[19] One of our main outcomes of interest is an indicator for whether a given country experiences an internal conflict, which claims the lives of 25 people or more, in each given year. About 10 percent of our country-year observations involve such conflicts, and these conflicts take place in 97 different countries. Almost half of the internal conflicts in our data took place during the 1980s and 1990s, and the continent with the most conflicts was Asia (47% of conflict observations), followed by Africa (33%), South America (8%), North America (7%), Europe (6%), and Oceania (1%).[20]

For our robustness checks, we construct five other measures of internal armed conflict. The first is an intensity-scaled measure of internal armed conflicts, which takes on the value of one if the internal conflict's death toll in a given year was 25-999, two if it was 1000 or more, and zero otherwise. The second is an indicator for having either an internal or an internationalized internal conflict, since conflicts may switch from one type to the other. The third is an indicator for having any type of armed conflict (internal or not). The final two measures, following Collier and Hoeffler (2004) and Cotet and Tsui (2013), are: an indicator for onset of internal armed conflict (having an internal conflict and no internal armed conflict in the preceding year); and a measure of internal armed conflict transitions (an indicator for an internal armed conflict in the current year minus the indicator for the previous year).

Another measure of political violence that we use is an indicator for having at least one coup in a given year, based on data from the Polity IV project (Marshall and Marshall 2011). A coup is defined as a forceful seizure of

[19]Conflicts are classified into four types in the UCDP/PRIO dataset: interstate, internal, internationalized internal, and extra-systemic (conflicts between a state and a non-state group outside its territory). Our main incidence measure is constructed using internal conflicts, but we consider others below.

[20]The country-year distribution of conflict incidence by decades is 2% for 1946-1949, 5% for 1950s, 10% for 1960s, 15% for 1970s, 22% for 1980s, 27% for 1990s, 17% for 2000-2008.

executive authority and office by a dissident or opposition faction within the country's ruling or political elites that results in a substantial change in the executive leadership and the policies of the prior regime, or an attempt to do so (we do not distinguish between successful or unsuccessful coups). About 5.5 percent of our observations are classified as having at least one coup, and coups thus defined took place in 116 different countries from 1946-2008. Coups were fairly evenly distributed from the 1960s onwards (and rarer before), and the continent with the most country-year observations with coups is Africa (51%), followed by Asia (25%), South America (9%), North America (9%), Europe (5%), and Oceania (1%).[21]

As an indicator for repression, we use a measure for purges from Banks (2010). This indicator takes on a value of one if a country experiences at least one purge in a given year, and zero otherwise. A purge is defined as systematic murder and elimination of political opponents by incumbent regimes. About 8.6 percent of our observations are classified as having involved repression, and repression thus defined took place in 112 countries from 1946-2008. Repressions peaked during the beginning of the sample period - the1940s and 1950s - and gradually declined over time.[22]

We also follow Besley and Persson (2011) in constructing an indicator for countries with strong institutions. They use the fraction of time spent having the highest score for executive constraints variable (XCONST) from Polity IV project (Marshall et al. 2010) as the criterion for having strong institutions.[23] In our analysis, we also use the Polity 2 score from the Polity IV project as a measure for institutional quality. This is a common measure of a country's

[21]The country-year distribution of coup incidence by decades is 4% for 1946-1949, 7% for 1950s, 19% for 1960s, 20% for 1970s, 20% for 1980s, 18% for 1990s, 12% for 2000-2008.

[22]The country-year distribution of repression incidence by decades is 32% for 1946-1949, 30% for 1950s, 17% for 1960s, 11% for 1970s, 3% for 1980s, 2% for 1990s, and 1% for 2000-2008.

[23]Details can be found in Besley and Persson (2011, pp. 1430-1431). There are 26 countries they define as having strong institutions: Australia, Austria, Belgium, Canada, Costa Rica, Denmark, Estonia, Finland, Germany, Greece, Iceland, Ireland, Israel, Italy, Japan, Latvia, Lithuania, Luxembourg, Netherlands, New Zealand, Norway, Sweden, Switzerland, South Africa, United Kingdom, and the United States.

political institutions, taking on values from -10 (strongly autocratic) to 10 (strongly democratic). Finally, we use the ethnic fractionalization measure from Alesina et al. (2003).

5. Results

This section begins by discussing our baseline empirical specifications and estimates of the effect of giant oilfield discoveries on oil production and exports and on internal armed conflicts (Subsection 5.1). We then discuss the robustness of our estimates using a number of alternative specifications (Subsection 5.2). We conclude this section by discussing the estimates of giant oilfield discoveries on other economic and political outcomes (Subsection 5.3).

5.1. Baseline Specifications and Results

In order to examine the effect of giant oilfield discoveries, we use our panel data on countries over time to estimate the following specification:

$$Y_{it+j} = \beta_{1j} Disc_{it} + Country_i + Year_t + \varepsilon_{it} \tag{3}$$

where Y_{it+j} is the outcome in country i in year $t+j$, $Disc_{it}$ is an indicator for the discovery of a giant oilfield in country i in year t, $Country_i$ and $Year_t$ are country and year fixed effects, and ε_{it} is a stochastic error. We begin by estimating this specification for different lags j, where in most cases $j \in \{2, 4, 6, 8, 10\}$. This allows us to non-parametrically trace the effect of discovery on outcomes over a decade.[24] In addition, some of our specifications add controls, as explained in below and in the various tables.

As we later discuss, we find that after controlling for country and year fixed effects, the timing of giant oilfield discoveries is largely uncorrelated with countries' economic and political performance in the five preceding years.

[24]Below we report estimates for other values of j, including odd and negative values.

One notable exception to this, however, is that giant oilfield discoveries in a country's recent past raise the odds of additional discoveries in its near future. Specifically, we find that the unconditional probability of a giant discovery in year t increases from about 1 percent when there were no giant oilfield discoveries from $t-10$ to $t-1$ to 87 percent if there was a giant discovery in every year from $t-10$ to $t-1$. Controlling for country and (year fixed effects) significantly reduces the predictive power of past discoveries, though it remains statistically significant. In a regression where the dependent variable is $Disc_it$ and the regressor of interest is the number of years with giant oilfield discoveries from $t-10$ to $t-1$, controlling for country and year fixed effects, the estimated coefficient is 0.032 (s.e. 0.004).[25]

These results suggest that giant oilfield discoveries in a country's recent past have some predictive power for whether a subsequent discovery is made.[26] We account for this serial correlation in the timing of giant oilfield discoveries by repeating our estimates of specification (1) with another specification, which we call (1a), and which includes the number of years with giant oilfield discoveries from $t-10$ to $t-1$ (labeled PDiscit) as a control. In addition to reporting estimates from specifications (1) and (1a) in tables, we also plot the regression estimates and 95 percent confidence intervals for $jin\{-5,-4,\ldots,9,10\}$ using figures. These figures allow us to economize on space when we examine whether pre-discovery years differed from post-discovery years in terms of the outcomes of interest, and to display outcomes in years $t+j$ where j is zero or positive and odd.

Having explained our baseline estimation strategy, we now examine the model's first prediction (or rather its assumption) that giant oilfield discoveries increase income from oil. As Panel A of Table 2 shows, oil production increases by about 25-30 percentage points within two years of a giant discov-

[25]One implication of this is that part of the effect of giant discoveries on subsequent outcomes may operate through a (slightly) increased probability of making further discoveries.

[26]Interestingly, we find no significant correlation between the number of giant oilfields discovered in a year and the inflation-adjusted price of oil in that year from 1946-2003.

ery. This effect of discovery on oil production rises to about 40-50 percentage points within four years, and remains stable (at least) until 10 years after discovery.[27] Past discoveries also matter, and their effect declines from about 26 percentage points two years after discovery to about 16 percentage points ten years after discovery. These large and precise estimates confirm that giant oilfield discoveries have an important economic impact on the discovering countries, as we can expect from the sheer size of these oilfields.

Panel B of Table 2 reports the effect of giant oilfield discoveries on oil exports. These discoveries increase oil exports by about 20-30 percentage points after six years, and this rises to about 40-50 percentage points after ten years. Past discoveries again matter, increasing oil exports by about 10-20 percent. These estimates are similar to the effects we find on oil production, although oil exports appear to take a bit longer to respond to giant discoveries.

Our finding that giant oilfield discoveries increase per capita production and exports of oil lead us to investigate the second prediction of the model, that internal armed conflict increases after giant oilfields are discovered. Panel A of Table 3 documents the effect of giant oilfield discoveries on the subsequent incidence of internal armed conflict. In line with the second prediction of the model, we find that giant oilfield discoveries increase the incidence of internal armed conflict by about 5-8 percentage points within 4-8 years of discovery. This effect is sizeable, since the mean of the conflict variable is just 10 percent, as we report in Table 1. Table 3 also shows that our estimates of the effect of giant oilfield discoveries on internal armed conflict do not change much when we control for the number of discoveries in $t-10$ to $t-1$.

Our finding that giant oilfield discoveries increase the incidence of internal conflicts is also robust to alternative ways of measuring conflict. For example,

[27]The outcomes in the tables are in logs, and in the text we convert them into percentage changes. For example, the 3rd-5th columns of Panel A of Table 2 show that log oil production increases by 0.39-0.41 within 6-10 years of giant discoveries, which corresponds to an increase of about 48-50 percentage points.

Panel B of Table 3 shows that giant oilfield discoveries have a similar impact on an intensity-scaled measure of armed conflict, which gives more weight to conflict years with 1,000 casualties or more, as described in Section 4. Giant discoveries increase this scaled measure of internal conflict by about 6-9 percentage points within 4-6 years of discovery. Panel C of Table 3 shows an increase of about 5-8 percentage points after discovery in a measure of conflict, which includes both internal and internationalized internal armed conflicts.[28]

Given this evidence that giant oilfield discoveries increase the incidence of internal armed conflict, we now ask: which countries are particularly likely to experience internal conflicts after giant oilfield discoveries? According to Prediction 3 of our model, armed conflict over oil is prevalent in countries where political violence pays off. In order to identify these conflict-prone countries, we use past violence as an indicator. As Panel A of Table 4 shows, countries that experienced at least one coup from $t-10$ to $t-1$ were more likely to plunge into internal conflict following giant oilfield discoveries. In fact, in these countries giant oilfield discoveries raised the incidence of an internal conflict by as much as 11-14 percentage points from $t+4$ to $t+8$. This figure is high, but we should bear in mind that the mean incidence of an internal conflict following a coup is about 19-20 percent. By contrast, in countries that experienced no coups from $t-10$ to $t-1$, oil discoveries have no significant effect on the incidence of internal conflict, again consistent with the model's predictions.

Another indicator that political violence pays off is that a country already experienced internal conflict at some point from $t-10$ to $t-1$. In those countries, giant oilfield discoveries raise the probability of conflict by as much as 11-18 percentage points. The baseline level of violence in these cases is

[28]We also estimate similar regressions for the onset of internal armed conflict and for internal armed conflict transitions, find no contemporaneous relation between these and giant oilfield discoveries, a finding that is similar to Cotet and Tsui (2013). These measures increase significantly 4 years after discovery.

also very high, with a mean of about 37-39 percent. Panel D of the table shows that there is no significant effect of giant oilfield discoveries on internal conflict in countries with no recent history of internal conflicts.

5.2. Robustness of Our Main Results

The results discussed so far indicate that giant oilfield discoveries increase oil output and the incidence of internal armed conflict, and that the latter increases particularly for countries with recent histories of violence. We now examine the robustness of these results, and we begin by looking at what happens in the years leading up to giant oilfield discoveries.

Sub-Figure A of Figure 1 shows estimates of specification (1) for our measure of oil production before and after discovery. The figure suggests that oil production did not change much in the years leading up to giant oilfield discoveries. Similarly, Sub-Figure B of Figure 1 shows that oil exports also did not increase during the lead-up to the discovery of giant oilfields, and we can again see that oil exports took longer to respond to giant oilfield discoveries than oil production. Sub-figure C of Figure 1 shows that the probability of internal armed conflicts also did not change much in the years leading up to giant discoveries. Finally, Sub-Figure D of Figure 1 shows that in countries that experienced at least one internal armed conflict from $t-10$ to $t-1$, conflicts did not systematically flare up in the years prior to giant oilfield discoveries.

The four sub-figures of Figure 2 show estimates similar to the corresponding sub-figures of Figure 1, except that this time we control for the number of years with giant oilfield discoveries from $t-10$ to $t-1$ (the estimates for Figure 2 are generated using specification (1a) instead of specification 1). The results once again show no evidence of significant trends before giant discoveries. Moreover, the estimates are quantitatively very similar to those in Figure 1. From this point on, to economize on space, we focus primarily

on estimates that control for discoveries before t, as in specification (1a).

In Figure 3 we examine the changes before and after discovery in some of the alternative measures of conflict discussed above. Sub-Figures A and B of this figure correspond to Panels B and C of Table 3, using as outcomes internal armed conflicts scaled by intensity and internal armed conflict including ones that were internationalized. These outcomes, like our main measure of armed conflict, show little change in the years leading up to discovery, and become positive and significant within 4-8 years after discovery. Sub-Figure C of the figure corresponds to panel A of Table 4, showing that in countries that had at least one coup from $t-10$ to $t-1$, internal armed conflicts increase more with giant oilfield discoveries, and there were no significant changes in the years leading up to discovery. Sub-Figure D shows similar results for countries that experienced any type of armed conflict from $t-10$ to $t-1$.

The finding that our key variables of interest do not change systematically in the years leading up to giant oilfield discoveries supports our interpretation that our estimated effects of giant oilfield discoveries are plausibly causal. In the following paragraphs we address further potential concerns regarding this interpretation.

One concern that may linger, for example, is that there may be serial correlation not only in the timing of giant oilfield discoveries but also in the outcomes we examine. To address this concern, panel B of Table 5 re-estimates specification (1a), but this time also controlling for the dependent variable in $t-1$, which is instrumented by the dependent variable in $t-2$. The outcome here is our measure of oil production, and the estimates are smaller than the baseline, but still positive and significant.

Another related concern is that political conditions in the discovering country may have changed shortly before discovery. But Panel C of the table adds to specification (1a) a control for polity 2 (a common measure of institutional quality) in $t-1$ and this does not change the estimates much. Since we do

not have a measure of investment in the oil sector, Panel D reports estimates of specification (1a) with a control for log PPP-adjusted per capita private investment in 2005 US dollar in $t-1$, and again the estimates remain statistically significant. Panel E adds together all the controls from Panels B-D, and again the estimates remain significant for $t+2$ through to $t+10$, this time with the exception of the estimate for $t+8$, which is marginally significant.

While the results discussed so far include all discoveries of giant oilfields since 1946 and control for discoveries in countries' recent past, a concern remains that the odds of discovery are not the same in all countries and in all years. More specifically, the regressions discussed so far include country-year observations where the probability of discovery was relatively high given the history of past discovery, along with many (most) observations where the odds of discovery were low. Panel F of Table 5 focuses on giant oilfield discoveries that were especially surprising, since no giant oilfield was discovered in the country from $t-10$ to $t-1$. When we focus only on observations for which no giant discoveries were made in the prior decade, the odds of a giant discovery fall to just over 1 percent, so these discoveries were in all likelihood highly unexpected. The results show that the effect of these unexpected discoveries on oil production are about twice as large as in the baseline, and precisely estimated. This is probably because in the countries that make these discoveries, oil production prior to the giant discovery was usually very low.

Another potential concern regarding our identification is that the countries that discover giant oilfields differ from others in ways that change over time and are therefore not fully controlled for by country fixed effects. To address this concern, Panel G re-estimates the baseline specification using only countries that make at least one giant discovery in the period we analyze (from 1946 onwards). The estimates in this specification are similar to those in the baseline, although slightly larger.

Finally, we address the concern that country-year observations with oil discoveries differ from others not only across countries, but also within countries, and in ways that we cannot observe and control for directly. To mitigate this concern, we use data from the Oil and Gas Journal Data Book (2008), which records country-year pairs where some oil discoveries, not necessarily giant, were made. Estimating specification (1a) using only these country-year observations, we essentially compare instances of giant oilfield discoveries to instances of smaller discoveries. As Panel H of the table shows, even when we restrict ourselves to these cases, the effect of giant oilfield discoveries on oil production remains positive and significant, albeit smaller, for $t + 6$ to $t + 10$.

Table 6 repeats the robustness checks described above for our main result, that giant oilfield discoveries increase the probability of internal armed conflict from $t + 4$ to $t + 8$. Controlling for the (instrumented) lagged dependent variable, lagged polity 2 score and lagged investment, or all of these together, tends to increase the coefficients very slightly, and they remain statistically significant. Excluding observations that follow one or more discoveries in $t - 10$ to $t - 1$ makes the estimate for $t + 4$ imprecise, but the coefficients for $t + 6$ and $t + 8$ are still precise – the latter is even larger than in the baseline specification. Restricting the sample to countries with giant oilfield discoveries leaves the baseline coefficients almost unchanged. And using only observations with some oil discoveries tends to increase both the point estimates and the standard errors, leaving the estimates for $t + 4$ and $t + 6$ positive and statistically significant.

Table 7 reports estimates for the same robustness checks as in Tables 5 and 6, but this time for the effect of giant oilfield discoveries on internal armed conflicts in countries that experienced at least one year of conflict from $t-10$ to $t - 1$. As before the controls we include make little difference to our estimates when they are included separately or simultaneously: the estimates for

$t+4$ to $t+8$ remain significant and change little in magnitude. Excluding observations with recent past discoveries makes the estimate for $t+4$ imprecise, but the estimates for $t+6$ and $t+8$ are still precise. Restricting our analysis to the set of countries with giant oilfield discoveries again makes almost no difference relative to the baseline. Finally, using only observations with some oil discoveries, while restricting our sample to about 400 observations, still results in positive and significant estimates for $t+4$ and $t+6$.

The estimates reported thus far show that the effect of oil discovery on conflict are larger in countries with a history of conflict. We now compare the interaction of giant discoveries and recent conflicts with interactions of giant discoveries with other features of the discovering country. To do so, we begin by estimating the following equation:

$$Y_{it+j} = \beta_{2j} Disc_{it} + \gamma_{2j} PConf_{it} + \delta_{2j} PDisc_{it} + \theta_{2j} Disc_{it} \times PConf_{it} + Country_i + Year_t + \varepsilon_{it} \quad (4)$$

where $PConf_{it}$ measures the number of years from $t-10$ to $t-1$ in which country i experienced internal armed conflict. Panel A of Table 8 reports estimates of β_{2j} and θ_{2j} for $j \in \{2, 4, 6, 8, 10\}$. As the table shows, θ_{2j} is positive and significant for 4, 6, 8, and even 10 years after discovery, confirming again that giant oil discoveries spell trouble in countries with recent histories of violence.

We now add to this specification interactions of giant discoveries with other country characteristics, following our discussion in Section 3. First, we consider the possibility that in countries with strong institutions, giant oilfield discoveries lead to less conflict. To test this, we add to specification (2) an interaction of giant discoveries with strong institutions, which may proxy for an institutionalized commitment to share revenues with the opposition (θ close to $\frac{1}{2}$). Second, much of the literature (see survey in Blattman and

Miguel 2010) finds that conflicts are more prevalent in poor countries. In the model this corresponds to low-wage countries, and given our data limitations we proxy this using lagged per capita GDP. Specifically, we examine whether giant oilfield discoveries are more likely to tip poor countries into internal conflict by further adding to specification (2) controls for log per capita GDP in $t-1$ (as discussed in the data section) and its interaction with our indicator for giant discoveries, $Disc_it$. Finally, we consider the possibility that in countries with higher ethnic fractionalization, giant discoveries are more likely to cause conflict, possibly because those countries are more prone to be divided into opposing factions that willing to fight each other. We test this hypothesis by further adding to specification (2) an interaction of our measure of ethnic fractionalization (again see data section) with $Disc_it$. Panel B of Table 8 shows that none of the three interactions we added is statistically significant in any of the regressions, while the interaction of giant discoveries and past conflict is still positive and significant from 4 years after discovery onwards. This suggests that the countries that should be most concerned about tipping into violent conflict over resources are those with recent histories of conflict.

We also examine whether giant oilfield discoveries might themselves take place during periods of lull following conflicts. We estimate a regression where the dependent variable is an indicator for giant discovery and the regressor of interest is an indicator for having no internal armed conflict in periods $t-j$ to $t-1$ and conflict in period $t-j-1$, for $j \in \{1, 2, 3, 4, 5\}$, controlling for country and year fixed effects. The coefficient of interest in all these regressions is small and imprecise (results available from the authors), providing evidence that lulls in fighting do not predict the timing of giant oilfield discoveries.

But while lulls in conflict do not predict giant oilfield discoveries, do they affect search effort to discover new oilfields more generally? To answer this question, we use data from Cotet and Tsui (2013) on the number of "wildcat" wells, which are wells exploratory wells drilled outside known oil-producing

areas. Appendix Table A2 shows that the number of wildcat wells drilled (and an indicator for any wildcat drilling) does not change significantly in the years following a lull of any length from 1-5 years. These results are robust to measuring the outcome during the final year of the lull or in the year after the lull.

Our finding that wildcat drilling does not increase following lulls in conflict differs from the finding that Cotet and Tsui (2013) report in columns 4-6 of Table 9 of their paper, since they report a negative association between wildcat drilling and conflict. Their regressions, however, do not control for country fixed effects, so they effectively use cross-sectional variation, whereas we consistently use panel variation within countries and over time.

We further compare our results to those of Cotet and Tsui (2013) in Appendix Table A3. Panel A of the table reports estimates as in Table 3 of our paper using only the observations for which we have non-missing wildcat data from Cotet and Tsui (2013). While the sample is considerably smaller (since Cotet and Tsui 2013 have data on fewer countries than we do) the coefficients and their precision are quite similar to our baseline estimates. Panel B of the same table re-estimates these regressions controlling for the number of wildcat wells drilled, and the results are almost unchanged.

We next use the data from Cotet and Tsui (2013) to construct an indicator for years with oil discoveries whose total estimated URR is at least 500 million barrels of oil. This measure is still different from our measure of giant discoveries, not only because the sources are different, but also because their measure excludes natural gas and condensate. In addition, their measure use aggregates the URR over all discoveries within a country in a given year, while our measure effectively uses only the largest single discovery. As Panel C of Appendix Table A3 shows, estimates using this new measure are still positive and similar in magnitude to our baseline estimates, although only the estimated effect on conflict 6 years after a major discovery is signif-

icant. Finally, the last panel of the table shows that years where Cotet and Tsui (2013) report smaller discoveries (which add up to less than 500 million barrels of oil) are not followed by an increase in internal armed conflict.

We further explore the relationship between the size of giant oilfields discovered and internal armed conflict using our main data. Specifically, we divide the giant oilfield discoveries into four quartiles by the size of the estimated Ultimate Recoverable Reserves (URR). In Panel A of Appendix Table A4 we report estimates as in Table 3 of the paper, but this time allowing for differential effects of discoveries of different quartiles. The effects of giant oilfield discoveries at all sizes are generally positive. Although only some of the estimates are statistically significant, we do find a positive and significant effect for at least some lag between 4-8 years after giant oilfield discoveries of all sizes, although the strongest effects are concentrated in the 2nd and 3rd quartiles. One might (cautiously) interpret this finding as suggesting an inverted U-shape effect of giant oilfield discoveries, whereby the very largest giant oilfield discoveries might not have as strong an effect as mere giant discoveries. This result may be somewhat related to the pattern documented in Collier and Hoeffler (2004), where high levels of primary commodity exports are associated with more conflict, but in the case of the very highest levels, "as in Saudi Arabia, the government is so well-financed that rebellion is militarily infeasible." At the same time, in our analysis we find that even the largest discoveries still increase the odds of conflict.[29]

Panel B of the same table repeats the exercise, but this time includes an indicator for smaller (non-giant) oilfield discovery years, based on the Oil and Gas Journal Data Book. These smaller (non-giant) discoveries have small and insignificant effects on conflict, unlike the giant discoveries.

Another question that we examine is whether giant oilfields discovered

[29]Panel B of the same table repeats the exercise, but this time includes an indicator for smaller (non-giant) oilfield discovery years, based on the Oil and Gas Journal Data Book. These smaller (non-giant) discoveries have small and insignificant effects on conflict, unlike the giant discoveries.

onshore have a different effect on conflict from those made offshore. Panel A of Appendix Table A5 reports estimates of specifications as in Table 3 of our paper, but this time using separate indicators for giant onshore and offshore discoveries instead of our usual indicator for all giant discoveries. The estimates show that onshore discoveries significantly increase the odds of internal armed conflict within a few years of discovery. The estimated effect of offshore discoveries on subsequent conflict is also positive, but somewhat smaller and imprecisely estimated, except in one case. At the same time, a one-sided hypothesis test of whether the effect of onshore discoveries is significantly larger than that of offshore discoveries cannot reject the null. Taken together, our findings are broadly consistent with Lujala (2010) and Ross (2006, 2012), who conclude that onshore oil increases conflict more than offshore oil, but in the specifications that we estimate the difference between onshore and offshore giant discoveries is imprecisely estimated.

5.3. Additional Results

The results discussed so far suggest that giant oilfield discoveries have two opposing effects on the discovering countries' economy: they increase oil income, but also the incidence of a costly internal conflict. We now turn our attention to the fourth and last outcome that we discuss in the model section – whether these discoveries have a positive or a negative effect on per capita GDP and its components. Using these as outcomes, Table 9 reports estimates of specification (1a), and an augmented specification, which includes controls as in Panel E of Tables 5-7, namely the dependent variable in $t-1$ (instrumented by that same variable in $t-2$) and polity 2 and log PPP-adjusted per capita private investment, both also measured in $t-1$.

Panel A of Table 9 suggests that giant oilfield discoveries increase per capita GDP by about 4-6 percent. But as Panel B shows, this estimate is imprecise when more controls are added. Having also experimented with sim-

ilar specifications with various controls, we conclude that the positive effect we find in Panel A is not very robust, so we are unable to say conclusively whether giant oilfield discoveries have a small positive effect on per capita GDP, or whether this effect is zero.

The next two panels of Table 9 show similar results for the effect of giant oilfield discoveries on per capita government spending. Once again the effect is either positive (around 4-6 percent in Panel C), or insignificantly different from zero (Panel D) when more controls are added.

The remainder of Table 9 shows that giant oilfield discoveries have no significant effect on per capita private consumption and (with the exception of one negative estimate for $t + 6$ in panel H), also no effect on per capita private investment.[30]

We conclude this section of the paper with an investigation of other possible economic and political consequences of giant oilfield discoveries in Table 10. In Panels A and B of this table we test one of the mechanisms often discussed in the "Dutch Disease" literature (e.g. Corden and Neary 1982), whereby natural resource booms may cause a real exchange rate appreciation. This may happen, for example, if an oil-producing country spends some of its proceeds from oil on local non-tradable goods. As a result of such spending, the nominal exchange rate may appreciate (if the exchange rate is flexible) or local prices may rise. Either (or both) of these can cause real exchange rate appreciation, which can hurt the non-oil exporting industries. Panels A and B of Table 10, however, show that giant oilfield discoveries decrease the real exchange rate only for some years after discovery, and even then the effect is quite small and imprecisely estimated. Panels C and D similarly show that non-oil exports are not significantly reduced by giant oilfield discoveries. A more thorough investigation of various related "Dutch Disease" mechanisms

[30]We do not report figures for the effect of giant oilfield discoveries on per capita GDP and its components, but these are available on request from the authors, and they also suggest that the changes before and around discoveries are small and imprecisely estimated.

is, however, outside the scope of this paper and we leave it for future work.

We next examine an alternative hypothesis on a potential cost of oil production, namely that it may lead, in some cases, to over-spending and indebtedness by the government (for related discussions see Tornell and Lane 1999 and Manzano and Rigobón 2008). As Panels E and F of Table 10 show, we find no support for this hypothesis using our global dataset. Again, we leave further investigations of this issue for particular countries or regions for future work. Turning to other political economy hypotheses on the effect of natural resources, Panels G and H of Table 10 examine whether competition over oil takes the form of coups to replace the incumbent. As the table shows, we find no evidence that giant oilfield discoveries increase the odds of coups in the subsequent decade. Finally, the last two panels of Table 10 test the prediction of Besley and Persson (2009, 2011), that resource windfalls increase repression. Our estimates show no significant increase in repression in the aftermath of giant oil discoveries.

In sum, the results discussed in this subsection suggest that while the economic gains from giant oilfield discoveries to the local population may be limited, we do not identify other costs to from discovery, except for our main result of an increased risk of internal armed conflict.

6. Conclusion

We began this paper by asking whether natural resource windfalls fuel internal armed conflicts, and if so – in which settings. To answer this question, we use new data on giant oilfield discoveries to identify the effect of oil on economic and political outcomes around the world. We find that within a few years of giant oilfield discoveries, per capita oil production and oil exports in discovering countries increase by up to 50 percent. But we also find that discovering giant oilfields increases the incidence of internal armed conflict

by about 5-8 percentage points. This increase is driven predominantly by countries with recent histories of political violence – those that experienced coups or armed conflicts during the decade prior to discovery. We show that these findings are robust to a wide range of specification checks.

Our findings shed light on the questions we began with. Giant oil and gas field discoveries in Norway, Canada, and Australia, are unlikely to fuel internal armed conflicts, since these countries' recent histories include little political violence. But in countries where political disputes are often resolved by violence (or remain unresolved despite violence), giant oilfield discoveries can fuel the flames of internal conflicts.

Our finding that giant oilfield discoveries fuel internal conflicts in countries that are prone to violence has policy implications. Those who strive to reduce armed conflict should be concerned about oil rents that incumbents obtain in conflict-prone areas, especially if those rents encourage challenges to the incumbents' power. At the same time, the firms that prospect for oil in conflict-prone areas and those who regulate them ought to be concerned about negative externalities for many locals, who have little to gain from giant oilfield discoveries but may suffer from conflicts over the oil.

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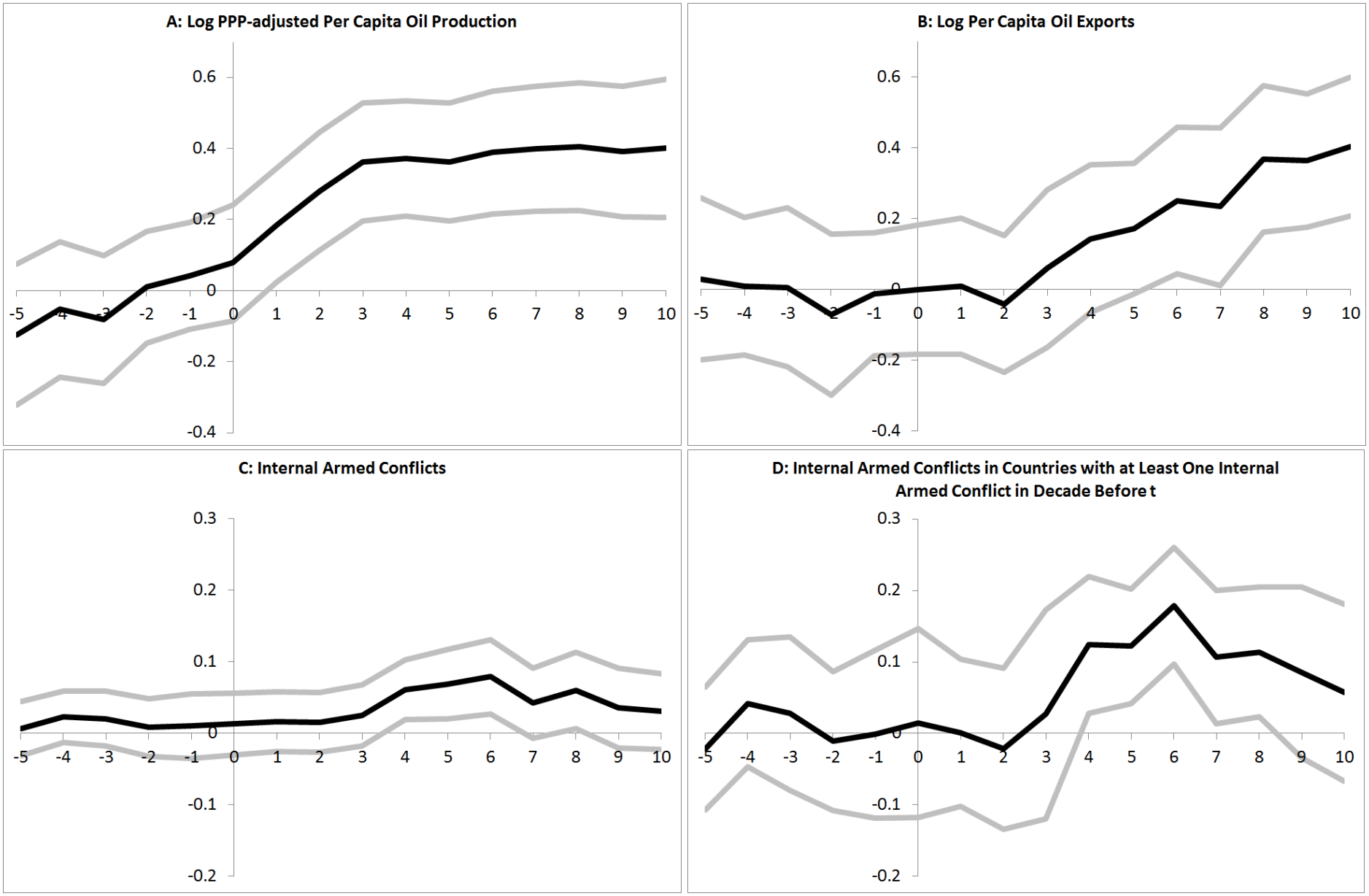


Figure 1: Effect of giant oilfield discovery on oil production, oil export, and internal armed conflicts. Sub-figure A shows the effect of giant oil discovery on PPP-adjusted per capita oil production in US$2005. Sub-figure B shows the effect of giant oil discovery on per capita oil export in US$2005. Sub-figure C shows the effect of giant oil discovery on internal armed conflicts. Sub-figure D is as sub-figure C but only with those having at least one or more years experienced internal armed conflict from t-10 to t-1. The x-axes report the number of years before or after t, ranging from t-5 to t+10. The black lines show the estimated coefficients and the grey lines show the 95% confidence intervals based on robust standard errors, which are clustered by country. All regressions include country and year fixed effects. Details on variable construction can be found in the data section of the paper.

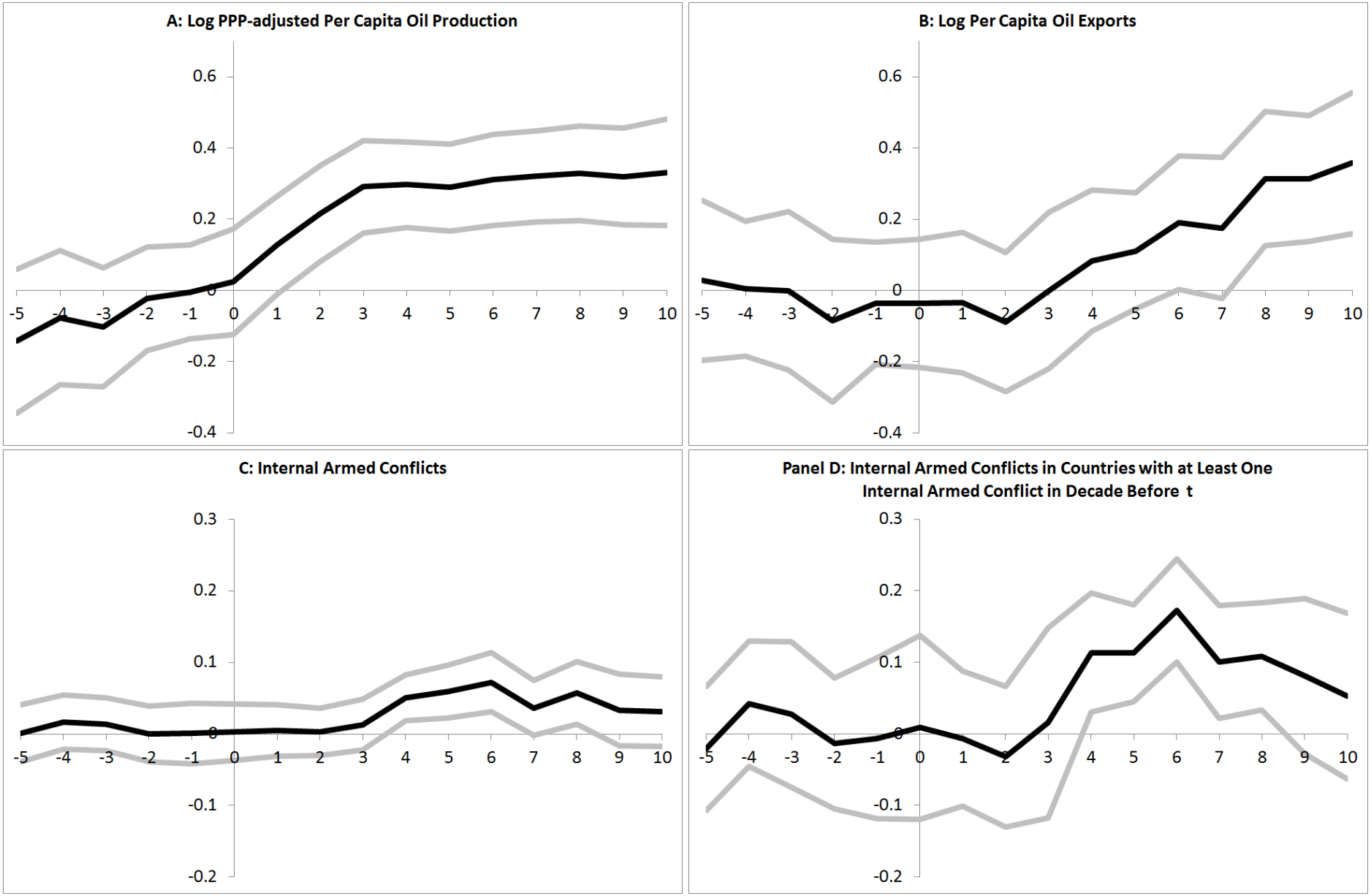


Figure 2: Effect of giant oilfield discovery on oil production, oil export, and internal armed conflicts with controls for the number of years with discoveries from t-10 to t-1. Sub-figure A shows the effect of giant oil discovery on PPP-adjusted per capita oil production in US$2005. Sub-figure B shows the effect of giant oil discovery on per capita oil export in US$2005. Sub-figure C shows the effect of giant oil discovery on internal armed conflicts. Sub-figure D is as sub-figure C but only with those having at least one or more years experienced internal armed conflict from t-10 to t-1. The x-axes report the number of years before or after t, ranging from t-5 to t+10. The black lines show the estimated coefficients and the grey lines show the 95% confidence intervals based on robust standard errors, which are clustered by country. All regressions include country and year fixed effects and control for the number of years with discoveries from t-10 to t-1. Details on variable construction can be found in the data section of the paper.

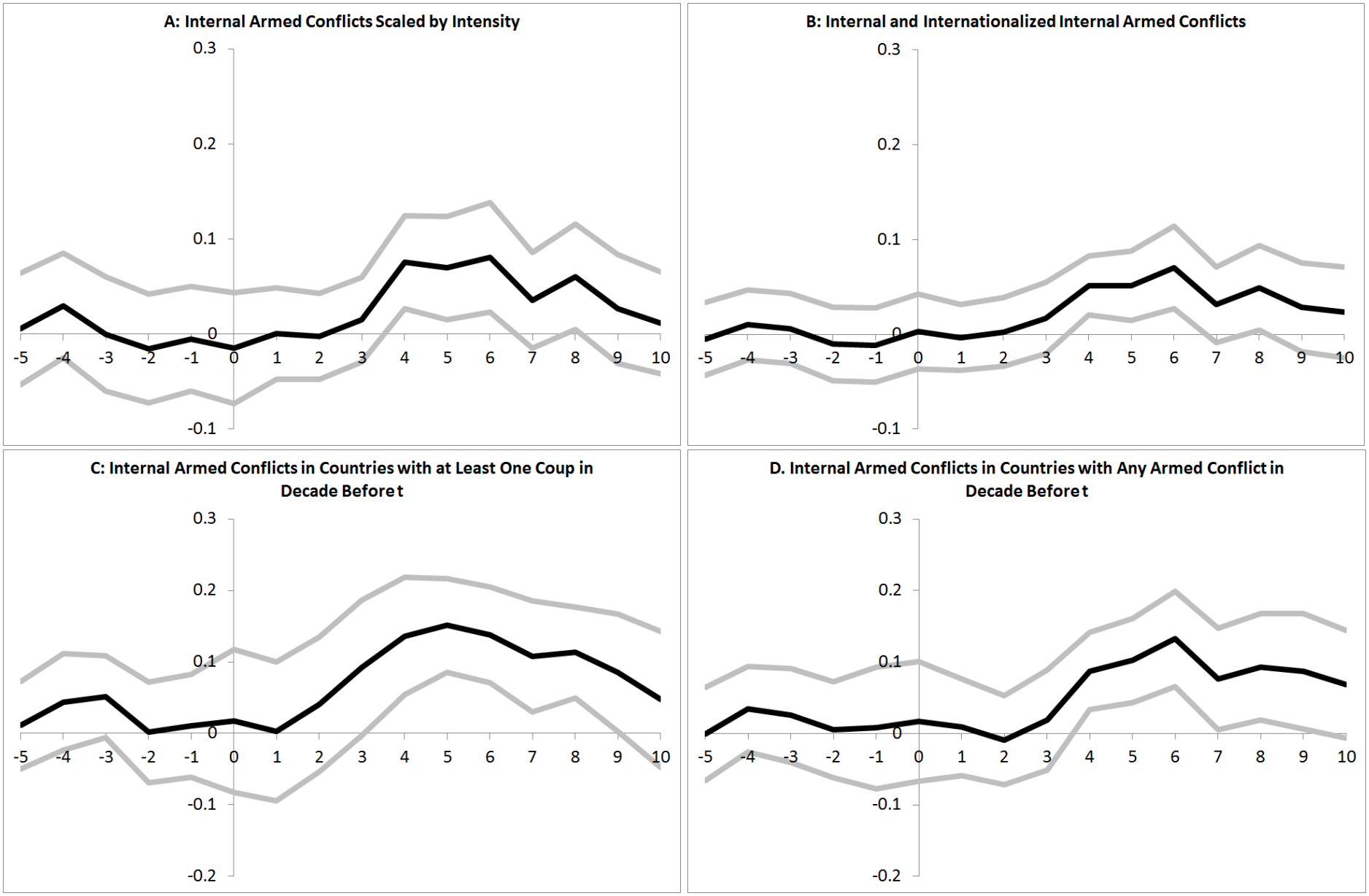


Figure 3: Effect of giant oilfield discovery on various measures of internal armed conflicts. Sub-figure A shows the effect of giant oil discovery on internal armed conflict scaled by intensity. Sub-figure B shows the effect of giant oil discovery on internal and internationalized internal armed conflicts. Sub-figure C shows the effect of giant oil discovery on internal armed conflicts but only for countries experienced at least one coup from t-10 to t-1. Sub-figure D shows the effect of giant oil discovery on internal armed conflicts but only for countries experienced any armed conflicts from t-10 to t-1. The x-axes report the number of years before or after t, ranging from t-5 to t+10. The black lines show the estimated coefficients and the grey lines show the 95% confidence intervals based on robust standard errors, which are clustered by country. All regressions include country and year fixed effects and control for the number of years with discoveries from t-10 to t-1. Details on variable construction can be found in the data section of the paper.

Table 1: Summary Statistics

Outcome in year:	Obs	Mean	Std. Dev.	First year of data	Last year of data
Discovery (indicator for giant oilfield discovery)	10,141	0.05	0.21	1946	2003
Indicator for discovery size in quartile 4, URR ϵ (2733, 160673]	10,141	0.01	0.11	1946	2003
Indicator for discovery size in quartile 3, URR ϵ (1180, 2733]	10,141	0.01	0.11	1946	2003
Indicator for discovery size in quartile 2, URR ϵ (658, 1180]	10,141	0.01	0.11	1946	2003
Indicator for discovery size in quartile 1, URR ϵ [500, 658]	10,141	0.01	0.11	1946	2003
Onshore discovery (indicator for giant onshore oil discovery)	10,141	0.03	0.17	1946	2003
Offshore discovery (indicator for giant offshore oil discovery)	10,141	0.02	0.14	1946	2003
Indicator for any oilfield discovery, not necessarily of a giant	11,091	0.12	0.32	1946	2008
Number of wildcats drilled	2,951	128	790	1946	2003
Indicator for positive wildcats drilled	2,951	0.86	0.35	1946	2003
Giant-equivalent discovery (As discovery indicator, but using data from Cotet and Tsui 2013)	2,951	0.15	0.36	1946	2003
Non-giant discovery (As indicator for any oil discovery, but using data from Cotet and Tsui 2013)	2,951	0.52	0.50	1946	2003
Log PPP-adjusted per capita oil and gas production (US$2005)	3,759	5.33	2.92	1950	2007
Log per capita oil exports (US$2005)	4,599	2.64	3.48	1962	2000
Log per capita non-oil export (US$2005)	5,562	5.69	1.92	1962	2000
Internal armed conflict indicator	11,091	0.10	0.30	1946	2008
Internal armed conflict indicator scaled by intensity	11,091	0.13	0.41	1946	2008
Internal or internationalized internal armed conflict indicator	11,091	0.11	0.32	1946	2008
Armed conflict indicator	11,091	0.14	0.34	1946	2008
Coup indicator	11,091	0.05	0.23	1946	2008
Repression indicator	8,497	0.09	0.28	1946	2008
Polity 2 score (between -10 and 10)	7,831	0.17	7.49	1946	2008
Ethnic fractionalization (time-invariant, between 0 and 1)	10,650	0.44	0.27	1946	2008
Log real exchange rate	8,362	0.71	0.55	1950	2007
Log public debt as percentage of GDP	5,698	3.75	0.89	1946	2008
Log PPP-adjusted per capita GDP (US$2005)	8,342	8.46	1.13	1950	2007
Log PPP-adjusted per capita government spending (US$2005)	8,342	6.63	1.18	1950	2007
Log PPP-adjusted per capita private consumption (US$2005)	8,342	7.97	0.99	1950	2007
Log PPP-adjusted per capita private investment (US$2005)	8,338	6.69	1.59	1950	2007
PPP-adjusted per capita military expenses (US$2005)	6,119	0.34	1.10	1950	2001
Ratio of PPP-adjusted military expenses to GDP	6,115	0.04	0.08	1950	2001

Notes: This table reports summary statistics for a panel of 193 countries from 1946-2008. Giant oilfields are those having an estimated ultimate recoverable reserves (URR) of oil, including gas and condensate equivalent, of at least 500 million barrels (Horn 2004). URR figures in this table are in million of barrels of oil equivalent. There are 461 observations with at least one giant oilfield discovery from 1946-2003.

Table 2: Effect of Giant Oil Discoveries on Oil Production and Oil Export

Outcome in year:	t+2	t+4	t+6	t+8	t+10	t+2	t+4	t+6	t+8	t+10
Panel A. Dependent variable: Log PPP-adjusted per capita oil and gas production in US$2005										
Discovery	**0.28**	**0.37**	**0.39**	**0.41**	**0.40**	**0.22**	**0.30**	**0.31**	**0.33**	**0.33**
	(0.08)	(0.08)	(0.09)	(0.09)	(0.10)	(0.07)	(0.06)	(0.07)	(0.07)	(0.08)
Years with discoveries from t-10 to t-1						**0.23**	**0.22**	**0.20**	**0.18**	**0.15**
						(0.05)	(0.05)	(0.05)	(0.05)	(0.06)
Observations	3,535	3,705	3,629	3,551	3,470	3,535	3,705	3,629	3,551	3,470
Panel B. Dependent variable: Log oil and gas exports per capita in US$2005										
Discovery	-0.04	0.14	**0.25**	**0.37**	**0.40**	-0.09	0.08	**0.19**	**0.31**	**0.36**
	(0.10)	(0.11)	(0.11)	(0.11)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)
Years with discoveries from t-10 to t-1						**0.17**	**0.18**	**0.18**	**0.15**	**0.11**
						(0.05)	(0.05)	(0.04)	(0.04)	(0.05)
Observations	4,563	4,530	4,492	4,453	4,436	4,563	4,530	4,492	4,453	4,436

Notes: This table reports the effect of discovering at least one giant oilfield in a panel of country-year observations. The panel includes 193 countries and uses data from 1946-2008. All regressions control for country and year fixed effects. Giant oilfields are those having an estimated ultimate recoverable reserves of oil, including gas and condensate equivalent, of at least 500 million barrels (Horn 2004). Robust standard errors in parentheses are clustered by country.

Table 3: Effect of Giant Oil Discoveries on Internal Armed Conflicts

Outcome in year:	t+2	t+4	t+6	t+8	t+10	t+2	t+4	t+6	t+8	t+10
Panel A. Dependent variable: Internal armed conflict										
Discovery	0.015	**0.061**	**0.079**	**0.060**	0.031	0.003	**0.050**	**0.072**	**0.057**	0.031
	(0.021)	(0.021)	(0.026)	(0.027)	(0.027)	(0.017)	(0.016)	(0.021)	(0.022)	(0.025)
Years with discoveries from t-10 to t-1						0.020	0.018	0.011	0.005	0.000
						(0.012)	(0.011)	(0.010)	(0.010)	(0.009)
Observations	10,135	10,129	9,933	9,547	9,161	10,135	10,129	9,933	9,547	9,161
Panel B. Dependent variable: Internal armed conflict scaled by intensity										
Discovery	0.009	**0.084**	**0.085**	0.060	0.009	-0.002	**0.076**	**0.081**	**0.060**	0.012
	(0.028)	(0.030)	(0.034)	(0.034)	(0.029)	(0.023)	(0.025)	(0.029)	(0.028)	(0.027)
Years with discoveries from t-10 to t-1						0.018	0.013	0.007	0.000	-0.005
						(0.015)	(0.013)	(0.013)	(0.013)	(0.014)
Observations	10,135	10,129	9,933	9,547	9,161	10,135	10,129	9,933	9,547	9,161
Panel C. Dependent variable: Internal and internationalized internal armed conflict										
Discovery	0.014	**0.060**	**0.076**	0.050	0.021	0.003	**0.051**	**0.070**	**0.049**	0.023
	(0.022)	(0.020)	(0.027)	(0.027)	(0.026)	(0.019)	(0.016)	(0.022)	(0.023)	(0.024)
Years with discoveries from t-10 to t-1						0.019	0.015	0.008	0.002	-0.004
						(0.012)	(0.011)	(0.010)	(0.010)	(0.010)
Observations	10,135	10,129	9,933	9,547	9,161	10,135	10,129	9,933	9,547	9,161

Notes: This table reports the effect of discovering at least one giant oilfield in a panel of country-year observations. The panel includes 193 countries and uses data from 1946-2008. All regressions include country and year fixed effects. Giant oilfields are those having an estimated ultimate recoverable reserves of oil, including gas and condensate equivalent, of at least 500 million barrels (Horn 2004). Robust standard errors in parentheses are clustered by country.

Table 4: Heterogeneous Effect of Giant Oil Discoveries on Internal Armed Conflicts

Outcome in year:	t+2	t+4	t+6	t+8	t+10	t+2	t+4	t+6	t+8	t+10
Panel A. Countries that experienced at least one coup from t-10 to t-1										
Discovery	0.044	**0.139**	**0.140**	**0.114**	0.047	0.041	**0.136**	**0.138**	**0.113**	0.048
	(0.050)	(0.044)	(0.037)	(0.033)	(0.049)	(0.048)	(0.042)	(0.034)	(0.032)	(0.049)
Years with discoveries from t-10 to t-1						**0.070**	**0.055**	0.034	0.009	-0.006
						(0.016)	(0.015)	(0.017)	(0.019)	(0.020)
Observations	2,605	2,605	2,557	2,457	2,347	2,605	2,605	2,557	2,457	2,347
Panel B. Countries that experienced no coups from t-10 to t-1										
Discovery	0.010	0.039	0.057	0.037	0.022	-0.001	0.026	0.046	0.025	0.012
	(0.024)	(0.021)	(0.033)	(0.034)	(0.034)	(0.016)	(0.014)	(0.024)	(0.026)	(0.029)
Years with discoveries from t-10 to t-1						0.016	0.019	0.016	0.015	0.013
						(0.014)	(0.014)	(0.012)	(0.011)	(0.010)
Observations	7,337	7,331	7,183	6,897	6,621	7,337	7,331	7,183	6,897	6,621
Panel C. Countries that experienced at least one internal armed conflict from t-10 to t-1										
Discovery	-0.021	**0.124**	**0.179**	**0.114**	0.057	-0.032	**0.113**	**0.172**	**0.108**	0.053
	(0.058)	(0.049)	(0.042)	(0.046)	(0.063)	(0.050)	(0.042)	(0.037)	(0.038)	(0.059)
Years with discoveries from t-10 to t-1						0.040	**0.042**	0.026	0.016	0.011
						(0.026)	(0.021)	(0.023)	(0.021)	(0.019)
Observations	1,958	1,958	1,907	1,797	1,679	1,958	1,958	1,907	1,797	1,679
Panel D. Countries that experienced no internal armed conflicts from t-10 to t-1										
Discovery	0.019	0.024	0.018	0.018	0.000	0.016	0.022	0.019	0.022	0.005
	(0.014)	(0.017)	(0.019)	(0.020)	(0.018)	(0.011)	(0.014)	(0.018)	(0.019)	(0.017)
Years with discoveries from t-10 to t-1						0.006	0.003	-0.002	-0.006	-0.009
						(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
Observations	7,984	7,978	7,833	7,557	7,289	7,984	7,978	7,833	7,557	7,289

Notes: This table reports the effect of discovering at least one giant oilfield in a panel of country-year observations. The panel includes 193 countries and uses data from 1946-2008. All regressions include country and year fixed effects. Giant oilfields are those having an estimated ultimate recoverable reserves of oil, including gas and condensate equivalent, of at least 500 million barrels (Horn 2004). Robust standard errors in parentheses are clustered by country.

Table 5: Robustness of Effect of Giant Oil Discoveries on Oil Production

Outcome in year:	t+2	t+4	t+6	t+8	t+10
Panel A. Baseline - as in Panel A of Table 2					
Discovery	**0.215**	**0.297**	**0.310**	**0.329**	**0.332**
	(0.069)	(0.061)	(0.065)	(0.068)	(0.076)
Observations	3,535	3,705	3,629	3,551	3,470
Panel B. As baseline, but controlling for the dependent variable in t-1, instrumented by the dependent variable in t-2					
Discovery	**0.136**	**0.172**	**0.160**	0.135	**0.164**
	(0.050)	(0.057)	(0.067)	(0.074)	(0.076)
Observations	3,134	3,121	2,919	2,718	2,525
Panel C. As baseline, but controlling for polity2 score in t-1					
Discovery	**0.215**	**0.279**	**0.259**	**0.254**	**0.266**
	(0.072)	(0.062)	(0.072)	(0.075)	(0.082)
Observations	3,290	3,400	3,296	3,185	3,068
Panel D. As baseline, but controlling for log PPP-adjusted per capita private investment in US$2005 in t-1					
Discovery	**0.144**	**0.221**	**0.222**	**0.231**	**0.242**
	(0.070)	(0.066)	(0.077)	(0.078)	(0.084)
Observations	3,356	3,426	3,291	3,151	3,005
Panel E. As baseline, but with all controls from panels B-D					
Discovery	**0.139**	**0.173**	**0.162**	0.133	**0.168**
	(0.050)	(0.058)	(0.068)	(0.075)	(0.075)
Observations	2,977	2,964	2,772	2,579	2,394
Panel F. As baseline, but excluding observations with one or more discoveries from t-10 to t-1					
Discovery	0.384	**0.574**	**0.555**	**0.627**	**0.672**
	(0.197)	(0.189)	(0.173)	(0.184)	(0.195)
Observations	2,202	2,337	2,302	2,269	2,226
Panel G. As baseline, but using only countries that discovered at least one giant oilfield					
Discovery	**0.239**	**0.320**	**0.338**	**0.355**	**0.352**
	(0.071)	(0.064)	(0.067)	(0.070)	(0.079)
Observations	2,570	2,686	2,637	2,587	2,535
Panel H. As baseline, but using only countries*year observations with one or more discoveries in the 2008 Oil and Gas Journal Data Book.					
Discovery	0.023	0.086	**0.144**	**0.172**	**0.170**
	(0.080)	(0.068)	(0.070)	(0.058)	(0.063)
Observations	1,107	1,138	1,142	1,140	1,119

Notes: This table reports the effect of discovering at least one giant oilfield in a panel of country-year observations. The panel includes 193 countries and uses data from 1946-2008. All regressions include country, year fixed effects, and control for the number of years with discoveries from t-10 to t-1. Giant oilfields are those having an estimated ultimate recoverable reserves of oil, including gas and condensate equivalent, of at least 500 million barrels (Horn 2004). Robust standard errors in parentheses are clustered by country.

Table 6: Robustness of Effect of Giant Oil Discoveries on Internal Armed Conflicts

Outcome in year:	t+2	t+4	t+6	t+8	t+10
Panel A. Baseline - as in Panel A of Table 3					
Discovery	0.003	**0.050**	**0.072**	**0.057**	0.031
	(0.017)	(0.016)	(0.021)	(0.022)	(0.025)
Observations	10,135	10,129	9,933	9,547	9,161
Panel B. As baseline, but controlling for the dependent variable in t-1, instrumented by the dependent variable in t-2					
Discovery	0.005	**0.053**	**0.072**	**0.055**	0.029
	(0.016)	(0.019)	(0.023)	(0.022)	(0.025)
Observations	9,749	9,743	9,547	9,161	8,775
Panel C. As baseline, but controlling for polity2 score in t-1					
Discovery	0.009	**0.060**	**0.086**	**0.065**	0.036
	(0.020)	(0.020)	(0.025)	(0.025)	(0.028)
Observations	6,894	6,888	6,726	6,407	6,087
Panel D. As baseline, but controlling for log PPP-adjusted per capita private investment in US$2005 in t-1					
Discovery	0.015	**0.060**	**0.084**	**0.064**	0.025
	(0.026)	(0.024)	(0.026)	(0.028)	(0.036)
Observations	7,404	7,404	7,218	6,845	6,471
Panel E. As baseline, but with all controls from panels B-D					
Discovery	0.013	**0.058**	**0.092**	**0.067**	0.027
	(0.023)	(0.024)	(0.029)	(0.028)	(0.037)
Observations	5,942	5,942	5,789	5,481	5,171
Panel F. As baseline, but excluding observations with one or more discoveries from t-10 to t-1					
Discovery	0.031	0.036	**0.068**	**0.098**	0.057
	(0.030)	(0.029)	(0.027)	(0.039)	(0.040)
Observations	8,590	8,586	8,424	8,111	7,791
Panel G. As baseline, but using only countries that discovered at least one giant oilfield					
Discovery	0.007	**0.052**	**0.072**	**0.058**	0.026
	(0.018)	(0.016)	(0.022)	(0.022)	(0.024)
Observations	3,572	3,570	3,504	3,374	3,244
Panel H. As baseline, but using only countries*year observations with one or more discoveries in the 2008 Oil and Gas Journal Data Book.					
Discovery	0.011	**0.066**	**0.100**	0.067	0.040
	(0.033)	(0.030)	(0.038)	(0.036)	(0.035)
Observations	1,311	1,307	1,298	1,276	1,247

Notes: This table reports the effect of discovering at least one giant oilfield in a panel of country-year observations. The panel includes 193 countries and uses data from 1946-2008. All regressions include country, year fixed effects, and control for the number of years with discoveries from t-10 to t-1. Giant oilfields are those having an estimated ultimate recoverable reserves of oil, including gas and condensate equivalent, of at least 500 million barrels (Horn 2004). Robust standard errors in parentheses are clustered by country.

Table 7: Robustness of Effect of Giant Oil Discoveries on Internal Armed Conflicts in Countries with at Least One Internal Armed Conflict in Decade Before Discovery

Outcome in year:	t+2	t+4	t+6	t+8	t+10
Panel A. Baseline - as in Panel C of Table 4					
Discovery	-0.032	**0.113**	**0.172**	**0.108**	0.053
	(0.050)	(0.042)	(0.037)	(0.038)	(0.059)
Observations	1,958	1,958	1,907	1,797	1,679
Panel B. As baseline, but controlling for the dependent variable in t-1, instrumented by the dependent variable in t-2					
Discovery	-0.029	**0.115**	**0.176**	**0.110**	0.050
	(0.044)	(0.043)	(0.037)	(0.036)	(0.057)
Observations	1,948	1,948	1,898	1,788	1,670
Panel C. As baseline, but controlling for polity2 score in t-1					
Discovery	-0.025	**0.109**	**0.181**	**0.111**	0.054
	(0.050)	(0.044)	(0.036)	(0.039)	(0.062)
Observations	1,879	1,879	1,830	1,723	1,607
Panel D. As baseline, but controlling for log PPP-adjusted per capita private investment in US$2005 in t-1					
Discovery	-0.012	**0.125**	**0.196**	**0.128**	0.066
	(0.060)	(0.048)	(0.037)	(0.040)	(0.068)
Observations	1,753	1,753	1,704	1,597	1,481
Panel E. As baseline, but with all controls from panels B-D					
Discovery	-0.019	**0.109**	**0.198**	**0.130**	0.066
	(0.052)	(0.049)	(0.039)	(0.038)	(0.067)
Observations	1,708	1,708	1,662	1,558	1,444
Panel F. As baseline, but excluding observations with one or more discoveries from t-10 to t-1					
Discovery	-0.031	0.154	**0.131**	**0.118**	-0.009
	(0.057)	(0.082)	(0.058)	(0.057)	(0.096)
Observations	1,437	1,437	1,400	1,323	1,236
Panel G. As baseline, but using only countries that discovered at least one giant oilfield					
Discovery	-0.026	**0.113**	**0.172**	**0.108**	0.049
	(0.051)	(0.041)	(0.038)	(0.038)	(0.059)
Observations	986	986	964	916	864
Panel H. As baseline, but using only countries*year observations with one or more discoveries in the 2008 Oil and Gas Journal Data Book.					
Discovery	-0.028	0.052	**0.215**	**0.129**	0.087
	(0.066)	(0.049)	(0.056)	(0.046)	(0.066)
Observations	406	406	403	398	384

Notes: This table reports the effect of discovering at least one giant oilfield in a panel of country-year observations. The panel includes 193 countries and uses data from 1946-2008. All regressions include country, year fixed effects, and control for the number of years with discoveries from t-10 to t-1. Giant oilfields are those having an estimated ultimate recoverable reserves of oil, including gas and condensate equivalent, of at least 500 million barrels (Horn 2004). Robust standard errors in parentheses are clustered by country.

Table 8: Effect of Giant Oil Discoveries and Interactions on Internal Armed Conflicts

Outcome in year:	t+2	t+4	t+6	t+8	t+10
Panel A. As Panel A of Table 3, but including discovery interaction with number of years with internal conflicts from t-10 to t-1					
Discovery	0.012	0.030	**0.037**	0.030	0.007
	(0.018)	(0.017)	(0.018)	(0.021)	(0.020)
Years with internal armed conflicts from t-10 to t-1	**0.044**	**0.028**	**0.015**	0.005	-0.006
	(0.004)	(0.005)	(0.006)	(0.006)	(0.006)
Discovery x (Years with internal armed conflicts from t-10 to t-1)	-0.007	0.013	**0.023**	**0.017**	**0.016**
	(0.009)	(0.008)	(0.006)	(0.007)	(0.007)
Observations	10,135	10,129	9,933	9,547	9,161
Panel B. As Panel A, but including various interactions of discovery					
Discovery	0.085	0.191	0.079	0.071	-0.051
	(0.252)	(0.197)	(0.188)	(0.193)	(0.169)
Years with internal armed conflicts from t-10 to t-1	**0.038**	**0.019**	0.003	-0.010	**-0.023**
	(0.005)	(0.006)	(0.007)	(0.007)	(0.008)
Log ppp-adjusted per capita GDP in t-1	-0.014	-0.010	-0.012	-0.013	-0.012
	(0.020)	(0.021)	(0.023)	(0.025)	(0.027)
Discovery x (Years with internal armed conflicts from t-10 to t-1)	-0.002	0.015	**0.029**	**0.025**	**0.029**
	(0.012)	(0.011)	(0.007)	(0.009)	(0.009)
Discovery x (Log ppp-adjusted per capita GDP in t-1)	-0.003	-0.015	-0.009	-0.007	0.007
	(0.029)	(0.021)	(0.019)	(0.021)	(0.018)
Discovery x (Countries with strong institution)	0.028	0.029	0.077	0.088	0.102
	(0.072)	(0.062)	(0.080)	(0.086)	(0.083)
Discovery x (Ethnic fractionalization)	-0.102	-0.080	0.031	-0.018	-0.119
	(0.121)	(0.110)	(0.097)	(0.100)	(0.118)
Observations	7,209	7,209	7,028	6,666	6,304

Notes: This table reports the effect of discovering at least one giant oilfield in a panel of country-year observations. The panel includes 193 countries and uses data from 1946-2008. All regressions include country, year fixed effects, and control for the number of years with discoveries from t-10 to t-1. Giant oilfields are those having an estimated ultimate recoverable reserves of oil, including gas and condensate equivalent, of at least 500 million barrels (Horn 2004). Robust standard errors in parentheses are clustered by country.

Table 9: Effect of Giant Oil Discoveries on Per Capita GDP and its Components

Outcome in year:	t+2	t+4	t+6	t+8	t+10
Panel A. Dependent variable: Log PPP-adjusted per capita GDP in US$2005					
Discovery	**0.040**	**0.048**	**0.036**	**0.059**	**0.059**
	(0.018)	(0.019)	(0.018)	(0.020)	(0.024)
Observations	7,937	8,266	8,108	7,940	7,760
Panel B. As Panel A, but controlling for the dependent variable in t-1, instrumented by the dependent variable in t-2, polity2 score in t-1, and log PPP-adjusted per capita private investment in US$2005 in t-1					
Discovery	0.016	0.014	0.002	0.004	-0.001
	(0.010)	(0.014)	(0.014)	(0.017)	(0.021)
Observations	5,839	5,838	5,532	5,222	4,912
Panel C. Dependent variable: Log PPP-adjusted per capita government spending in US$2005					
Discovery	0.017	**0.041**	**0.048**	**0.053**	**0.062**
	(0.016)	(0.018)	(0.021)	(0.024)	(0.029)
Observations	7,937	8,266	8,108	7,940	7,760
Panel D. As Panel C, but including controls as Panel B					
Discovery	0.017	0.026	0.035	0.035	0.023
	(0.012)	(0.018)	(0.026)	(0.027)	(0.026)
Observations	5,839	5,838	5,532	5,222	4,912
Panel E. Dependent variable: Log PPP-adjusted per capita private consumption in US$2005					
Discovery	-0.004	-0.002	-0.009	-0.001	0.013
	(0.016)	(0.018)	(0.020)	(0.019)	(0.021)
Observations	7,937	8,266	8,108	7,940	7,760
Panel F. As Panel E, but including controls as Panel B					
Discovery	0.015	0.017	0.003	0.010	0.015
	(0.010)	(0.014)	(0.012)	(0.015)	(0.017)
Observations	5,839	5,838	5,532	5,222	4,912
Panel G. Dependent variable: Log PPP-adjusted per capita private investment in US$2005					
Discovery	0.038	0.026	0.004	0.049	0.051
	(0.032)	(0.026)	(0.025)	(0.036)	(0.038)
Observations	7,933	8,262	8,104	7,936	7,756
Panel H. As Panel G, but including controls as Panel B					
Discovery	0.008	-0.019	**-0.053**	-0.004	-0.005
	(0.016)	(0.021)	(0.025)	(0.043)	(0.042)
Observations	5,834	5,833	5,527	5,217	4,907

Notes: This table reports the effect of discovering at least one giant oilfield in a panel of country-year observations. The panel includes 193 countries and uses data from 1946-2008. All regressions include country, year fixed effects, and control for the number of years with discoveries from t-10 to t-1. Giant oilfields are those having an estimated ultimate recoverable reserves of oil, including gas and condensate equivalent, of at least 500 million barrels (Horn 2004). Robust standard errors in parentheses are clustered

Table 10: Effect of Giant Oil Discoveries on Other Outcomes

Outcome in year:	t+2	t+4	t+6	t+8	t+10
Panel A. Dependent variable: Log real exchange rate					
Discovery	0.045	0.021	0.020	-0.014	-0.027
	(0.033)	(0.028)	(0.026)	(0.026)	(0.029)
Observations	7,957	8,286	8,126	7,956	7,774
Panel B. As panel A, but controlling for the dependent variable in t-1, instrumented by the dependent variable in t-2, polity2 score in t-1, and log PPP-adjusted per capita private investment in US$2005 in t-1					
Discovery	0.036	0.017	0.014	-0.035	-0.048
	(0.028)	(0.026)	(0.026)	(0.028)	(0.037)
Observations	5,840	5,839	5,533	5,223	4,913
Panel C. Dependent variable: Log per capita non-oil export in US$2005					
Discovery	-0.053	0.020	0.017	0.001	0.030
	(0.069)	(0.070)	(0.057)	(0.065)	(0.061)
Observations	5,519	5,475	5,431	5,387	5,367
Panel D. As Panel C, but including controls as Panel B					
Discovery	-0.036	-0.008	-0.011	0.024	0.051
	(0.027)	(0.027)	(0.035)	(0.049)	(0.068)
Observations	4,134	3,846	3,540	3,292	3,045
Panel E. Dependent variable: Log Public debt (as percentage of GDP)					
Discovery	0.046	0.041	0.045	-0.022	-0.067
	(0.033)	(0.039)	(0.050)	(0.048)	(0.043)
Observations	5,144	5,424	5,517	5,438	5,337
Panel F. As Panel E, but including controls as Panel B					
Discovery	0.021	0.034	0.011	0.009	-0.024
	(0.020)	(0.035)	(0.043)	(0.045)	(0.035)
Observations	3,733	3,725	3,577	3,305	3,038
Panel G. Dependent variable: Coup					
Discovery	-0.009	0.005	0.008	0.018	-0.005
	(0.010)	(0.013)	(0.013)	(0.013)	(0.012)
Observations	10,135	10,129	9,933	9,547	9,161
Panel H. As Panel G, but including controls as Panel B					
Discovery	-0.007	0.002	0.014	0.009	-0.018
	(0.011)	(0.014)	(0.018)	(0.012)	(0.017)
Observations	5,942	5,942	5,789	5,481	5,171
Panel I. Dependent variable: Repression					
Discovery	0.007	-0.022	-0.019	-0.020	-0.003
	(0.024)	(0.019)	(0.019)	(0.017)	(0.015)
Observations	7,778	7,974	7,987	7,805	7,611
Panel J. As Panel I, but including controls as Panel B					
Discovery	-0.004	**-0.032**	-0.014	-0.009	-0.007
	(0.020)	(0.016)	(0.015)	(0.013)	(0.013)
Observations	5,694	5,693	5,542	5,238	4,932

Notes: This table reports the effect of discovering at least one giant oilfield in a panel of country-year observations. The panel includes 193 countries and uses data from 1946-2008. All regressions include country, year fixed effects, and control for the number of years with discoveries from t-10 to t-1. Giant oilfields are those having an estimated ultimate recoverable reserves of oil, including gas and condensate equivalent, of at least 500 million barrels (Horn 2004). Robust standard errors in parentheses are clustered by country.

Appendix Table A1: Number of Years (from 1946-2003) with One or More Giant Oilfield Discoveries, by Country

Outcome in year:	Years (from 1946-2003) with at least one giant oilfield discovery	country	Years (from 1946-2003) with at least one giant oilfield discovery	country	Years (from 1946-2003) with at least one giant oilfield discovery
Former USSR	41	Angola	7	Albania	1
Saudi Arabia	29	Malaysia	6	Austria	1
Iran	27	Colombia	5	Azerbaijan	1
United States	25	Pakistan	5	Bangladesh	1
China	21	Qatar	5	Côte d'Ivoire	1
Iraq	20	Argentina	4	Denmark	1
Nigeria	19	Congo, Republic of	4	Ecuador	1
Australia	18	Netherlands	3	Equatorial Guinea	1
Libya	16	Peru	3	Gabon	1
Norway	15	Thailand	3	Germany	1
Canada	14	Trinidad and Tobago	3	Hungary	1
Indonesia	14	Tunisia	3	Morocco	1
Mexico	14	Bolivia	2	Namibia	1
United Arab Emirates	14	Brunei	2	New Zealand	1
Brazil	13	France	2	Papua New Guinea	1
United Kingdom	12	Italy	2	Philippines	1
Venezuela	12	Kazakhstan	2	Romania	1
Egypt	11	Myanmar	2	Russian Federation	1
Oman	10	Sudan	2	Spain	1
Kuwait	9	Viet Nam	2	Syria	1
Algeria	8	Yemen	2	Turkmenistan	1
India	8	Afghanistan	1		

Notes: This table reports the number of country-year cells with one or more discovery of a giant oilfield from 1946-2003, by country.

Appendix Table A2: Oil Explorations During Lull Periods Following Internal Armed Conflict

Dependent variable	Number of wildcats drilled					Indicator for positive wildcats drilled				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Panel A. Indicator for conflict followed by the number of years without conflict before t										
Conflict in t-2, no conflict in t-1	-7.126					0.014				
	(8.192)					(0.031)				
Conflict in t-3, no conflict in t-2 & t-1		-8.544					0.011			
		(7.873)					(0.035)			
Conflict in t-4, no conflict in t-3, t-2 & t-1			-1.785					0.003		
			(7.726)					(0.035)		
Conflict in t-5, no conflict in t-4, t-3, t-2 & t-1				-0.895					-0.043	
				(9.069)					(0.046)	
Conflict in t-6, no conflict in t-5, t-4, t-3, t-2 & t-1					-0.137					-0.038
					(8.493)					(0.040)
Observations	2,951	2,951	2,951	2,951	2,951	2,951	2,951	2,951	2,951	2,951
Panel B. As Panel A, but using indicator for conflict followed by the number of years without conflict including t										
Conflict in t-1, no conflict in t	-2.562					0.014				
	(8.455)					(0.029)				
Conflict in t-2, no conflict in t-1 & t		-8.027					-0.009			
		(7.828)					(0.040)			
Conflict in t-3, no conflict in t-2, t-1 & t			-6.666					0.011		
			(8.048)					(0.037)		
Conflict in t-4, no conflict in t-3, t-2, t-1 & t				-1.573					-0.017	
				(7.822)					(0.031)	
Conflict in t-5, no conflict in t-4, t-3, t-2, t-1 & t					-0.415					-0.050
					(9.290)					(0.048)
Observations	2,951	2,951	2,951	2,951	2,951	2,951	2,951	2,951	2,951	2,951

Notes: This table reports the association of oil explorations and the lull period of conflict in a panel of country-year observations. The panel includes 63 countries and uses data from 1946 - 2003 respectively. All regressions include country and year fixed effects. Wildcats drilled, a direct measure of oil exploration effort, are constructed using Cotet and Tsui (2013) online published dataset. Robust standard errors in parentheses are clustered by country.

Appendix Table A3: Effect of Giant Discoveries on Internal Armed Conflicts: Comparing with Cotet and Tsui (2013)

Outcome in year:	t+2	t+4	t+6	t+8	t+10	t+2	t+4	t+6	t+8	t+10
Panel A. As in Panel A of Table 3, but using only observations for which wildcat data from Cotet and Tsui (2013) are available										
Discovery	0.012	**0.068**	**0.089**	**0.060**	0.037	0.002	**0.059**	**0.083**	**0.057**	0.036
	(0.025)	(0.024)	(0.030)	(0.030)	(0.029)	(0.021)	(0.019)	(0.026)	(0.025)	(0.028)
Years with discoveries from t-10 to t-1						0.021	0.018	0.011	0.007	0.003
						(0.013)	(0.012)	(0.011)	(0.010)	(0.009)
Observations	2,948	2,944	2,880	2,755	2,629	2,948	2,944	2,880	2,755	2,629
Panel B. As Panel A, but controlling for the number of wildcats drilled										
Discovery	0.012	**0.068**	**0.089**	**0.060**	0.037	0.002	**0.059**	**0.083**	**0.057**	0.036
	(0.025)	(0.024)	(0.030)	(0.030)	(0.029)	(0.021)	(0.019)	(0.026)	(0.025)	(0.028)
Wildcats drilled (in thousands)	**0.014**	0.012	0.005	0.000	-0.005	0.002	0.001	-0.002	-0.005	-0.007
	(0.006)	(0.007)	(0.006)	(0.006)	(0.007)	(0.010)	(0.011)	(0.011)	(0.012)	(0.012)
Years with discoveries from t-10 to t-1						0.021	0.018	0.011	0.007	0.003
						(0.013)	(0.012)	(0.011)	(0.010)	(0.010)
Observations	2,948	2,944	2,880	2,755	2,629	2,948	2,944	2,880	2,755	2,629
Panel C. As Panel B, but using an indicator for years with giant-equivalent discoveries using Cotet and Tsui (2013) data										
Giant-equivalent discovery (Cotet and Tsui 2013)	0.022	0.043	**0.057**	0.049	0.034	0.018	0.036	**0.051**	0.045	0.032
	(0.035)	(0.029)	(0.026)	(0.031)	(0.027)	(0.032)	(0.025)	(0.021)	(0.026)	(0.025)
Wildcats drilled (in thousands)	0.012	0.008	0.000	-0.003	-0.006	0.011	0.006	-0.002	-0.004	-0.006
	(0.007)	(0.008)	(0.007)	(0.008)	(0.008)	(0.008)	(0.009)	(0.008)	(0.009)	(0.008)
Years with giant-equivalent discovery from t-10 to t-1						0.004	0.007	0.006	0.004	0.002
						(0.010)	(0.009)	(0.010)	(0.008)	(0.008)
Observations	2,948	2,944	2,880	2,755	2,629	2,948	2,944	2,880	2,755	2,629
Panel D. As Panel C, but controlling for non-giant oil discoveries from Cotet and Tsui (2013)										
Giant-equivalent discovery (Cotet and Tsui 2013)	0.019	0.054	**0.065**	0.056	0.024	0.015	0.046	**0.058**	0.052	0.022
	(0.039)	(0.032)	(0.030)	(0.039)	(0.034)	(0.036)	(0.027)	(0.024)	(0.033)	(0.032)
Non-giant discovery (Cotet and Tsui 2013)	-0.004	0.016	0.012	0.010	-0.014	-0.004	0.015	0.011	0.009	-0.015
	(0.018)	(0.016)	(0.017)	(0.017)	(0.020)	(0.018)	(0.016)	(0.017)	(0.017)	(0.020)
Wildcats drilled (in thousands)	0.012	0.006	-0.001	-0.004	-0.005	0.011	0.004	-0.003	-0.004	-0.005
	(0.007)	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)
Years with giant-equivalent discovery from t-10 to t-1						0.004	0.007	0.006	0.004	0.002
						(0.010)	(0.009)	(0.010)	(0.008)	(0.008)
Observations	2,948	2,944	2,880	2,755	2,629	2,948	2,944	2,880	2,755	2,629

Notes: This table reports the reconciliation between our findings and Cotet and Tsui (2013) in a panel of country-year observations. The panel includes 63 countries and uses data from 1946 - 2003 respectively. All regressions include country and year fixed effects. Giant oilfields are those having an estimated ultimate recoverable reserves of oil, including gas and condensate equivalent, of at least 500 million barrels (Horn 2004). Giant-equivalent discovery and non-giant discovery are constructed using Cotet and Tsui (2013) data. Details can be found in section 4. Robust standard errors in parentheses are clustered by country.

Appendix Table A4: Effect of Oil Discovery Size on Internal Armed Conflicts

Outcome in year:	t+2	t+4	t+6	t+8	t+10	t+2	t+4	t+6	t+8	t+10
Panel A. As in Panel A of Table 3, but including indicators for each quartile of discoveries size in a given country-year observations										
Discovery size in quartile 4	-0.017	-0.016	**0.057**	0.050	0.026	-0.036	-0.032	**0.046**	**0.045**	0.026
	(0.030)	(0.034)	(0.026)	(0.028)	(0.047)	(0.030)	(0.036)	(0.020)	(0.022)	(0.046)
Discovery size in quartile 3	0.041	**0.114**	**0.116**	0.065	0.009	0.021	**0.097**	**0.105**	**0.061**	0.009
	(0.033)	(0.031)	(0.036)	(0.033)	(0.033)	(0.030)	(0.026)	(0.029)	(0.028)	(0.030)
Discovery size in quartile 2	0.002	0.058	**0.097**	**0.079**	0.071	-0.004	0.053	**0.094**	**0.078**	**0.071**
	(0.039)	(0.037)	(0.036)	(0.033)	(0.037)	(0.036)	(0.035)	(0.033)	(0.031)	(0.036)
Discovery size in quartile 1	0.028	**0.069**	0.043	0.044	0.011	0.020	**0.062**	0.038	0.042	0.011
	(0.031)	(0.028)	(0.029)	(0.034)	(0.031)	(0.029)	(0.025)	(0.026)	(0.031)	(0.030)
Years with discoveries from t-10 to t-1						0.021	0.018	0.011	0.005	0.000
						(0.012)	(0.011)	(0.010)	(0.010)	(0.010)
Observations	10,135	10,129	9,933	9,547	9,161	10,135	10,129	9,933	9,547	9,161
Panel B. As Panel A, but controlling for non-giant oil discovery dummy from Oil and Gas Journal Databook										
Discovery size in quartile 4	-0.011	-0.016	**0.056**	0.051	0.024	-0.033	-0.035	**0.043**	**0.045**	0.024
	(0.031)	(0.035)	(0.027)	(0.029)	(0.047)	(0.029)	(0.037)	(0.020)	(0.023)	(0.047)
Discovery size in quartile 3	0.048	**0.113**	**0.114**	0.066	0.008	0.026	**0.094**	**0.102**	**0.060**	0.007
	(0.036)	(0.033)	(0.037)	(0.034)	(0.032)	(0.032)	(0.027)	(0.029)	(0.028)	(0.029)
Discovery size in quartile 2	0.008	0.058	**0.095**	**0.080**	0.070	0.000	0.050	**0.090**	**0.077**	**0.069**
	(0.039)	(0.038)	(0.036)	(0.034)	(0.036)	(0.036)	(0.035)	(0.033)	(0.031)	(0.034)
Discovery size in quartile 1	0.034	**0.068**	0.041	0.044	0.010	0.024	**0.060**	0.035	0.041	0.010
	(0.032)	(0.028)	(0.030)	(0.034)	(0.031)	(0.029)	(0.025)	(0.026)	(0.030)	(0.030)
Non-giant discovery	0.018	-0.002	-0.005	0.001	-0.004	0.010	-0.008	-0.009	-0.001	-0.004
	(0.016)	(0.015)	(0.017)	(0.016)	(0.017)	(0.015)	(0.015)	(0.017)	(0.016)	(0.018)
Years with discoveries from t-10 to t-1						0.020	0.018	0.012	0.005	0.000
						(0.012)	(0.011)	(0.011)	(0.010)	(0.010)
Observations	10,135	10,129	9,933	9,547	9,161	10,135	10,129	9,933	9,547	9,161

Notes: This table reports the effect of discovering oilfield in size in a panel of country-year observations. The panel includes 193 countries and uses data from 1946-2008. All regressions include country and year fixed effects. Giant oilfields are those having an estimated ultimate recoverable reserves of oil, including gas and condensate equivalent, of at least 500 million barrels (Horn 2004). Discovery size in each quartile is constucted based on the estimated Ultimate Recoverable Reserves (URR, in million barrels) of all giant discoveries in a given year and each quartile has the following range of URR: URR in quartile 1 ϵ [500, 658], URR in quartile 2 ϵ (658, 1180], URR in quartile 3 ϵ (1180, 2733], and URR in quartile 4 ϵ (2733, 160673]. Robust standard errors in parentheses are clustered by country.

Appendix Table A5: Effect of Onshore and Offshore Giant Oil Discoveries on Internal Armed Conflicts

Outcome in year:	t+2	t+4	t+6	t+8	t+10	t+2	t+4	t+6	t+8	t+10
Panel A. As in Panel A of Table 3, but using separate indicators for at least one onshore or offshore discoveries in a given country-year observation										
Onshore discovery	0.009	**0.070**	**0.083**	**0.069**	0.030	-0.003	**0.061**	**0.077**	**0.066**	0.030
	(0.021)	(0.026)	(0.025)	(0.026)	(0.024)	(0.021)	(0.024)	(0.023)	(0.024)	(0.023)
Offshore discovery	0.031	0.034	0.065	0.028	0.004	0.017	0.022	0.057	0.024	0.004
	(0.030)	(0.028)	(0.038)	(0.042)	(0.048)	(0.024)	(0.023)	(0.032)	(0.037)	(0.047)
Years with discoveries from t-10 to t-1						0.020	0.018	0.011	0.005	0.000
						(0.012)	(0.011)	(0.010)	(0.010)	(0.010)
Observations	10,135	10,129	9,933	9,547	9,161	10,135	10,129	9,933	9,547	9,161
H0: Onshore discovery = Offshore discovery (p-value) H1: Onshore discovery > Offshore discovery	0.242	0.166	0.336	0.206	0.323	0.262	0.144	0.316	0.196	0.322
Panel B. As Panel A, but controlling for the number of years with onshore and offshore discoveries from t-10 to t-1 separately										
Onshore discovery						-0.003	**0.060**	**0.077**	**0.067**	0.031
						(0.019)	(0.023)	(0.022)	(0.023)	(0.022)
Offshore discovery						0.022	0.027	**0.063**	0.027	0.004
						(0.022)	(0.018)	(0.028)	(0.032)	(0.043)
Years with onshore discoveries from t-10 to t-1						0.021	0.018	0.012	0.004	-0.002
						(0.016)	(0.013)	(0.013)	(0.012)	(0.012)
Years with offshore discoveries from t-10 to t-1						0.007	0.006	0.000	0.000	0.000
						(0.021)	(0.022)	(0.021)	(0.019)	(0.013)
Observations						10,135	10,129	9,933	9,547	9,161
H0: Onshore discovery = Offshore discovery (p-value) H1: Onshore discovery > Offshore discovery						0.143	0.126	0.332	0.159	0.300

Notes: This table reports the effect of discovering at least one giant onshore or offshore oilfield in a panel of country-year observations. The panel includes 193 countries and uses data from 1946-2008. All regressions include country and year fixed effects. Giant oilfields are those having an estimated ultimate recoverable reserves of oil, including gas and condensate equivalent, of at least 500 million barrels (Horn 2004). Robust standard errors in parentheses are clustered by country.

Chapter 3:

Proxy Warriors: A Theory of Military Assistance

Yu-Hsiang Lei

One main feature contributing to the latest conflicts in the Middle East, and, in particular, to the issue of the wide spread of military resources well beyond governmental control, is that governments sponsor weapons to non-state militias to fight the state's enemies, but after the fighting ends these weapons cannot be called back. I develop a theory to study government strategy in supplying weapons to militias. When many weapons are supplied a war can be won more quickly but leaving too many weapons beyond control. Militias learn that they can benefit from retaining these extra weapons when hostilities end. This incentivizes the militias to fight strategically in order to maximize their subsequent stock of weapons. Given this governmental dilemma, in a dynamic setting I rationalize a supply strategy with a stopgap proposed by the US security forces. I find that the government would prefer to adopt a stopgap strategy when the weapons left with the militia are particularly harmful and when the government is impatient. I also find that the effect of a stopgap strategy is more useful to the government when the militia is sufficiently patient and when the weapons depreciate at a medium rate.

1. Introduction

Mercenary captains are either excellent men of arms or not: if they are, you cannot trust them because they always aspire to their own greatness, either by oppressing you, who are their patron, or by oppressing others contrary to your intention; but if the captain is not virtuous, he ruins you in the ordinary way.

– Machiavelli, The Prince, XII

Establishing a monopoly of violence is one of the essential features of a modern state, in the Weberian view. However, in many modern civil wars, the government military is no longer the only war agent. In fact, due to limited state capacity many countries often rely on non-state militias to fight national enemies (e.g. Bates (2015) and Kaldor (2013)). According to Carey, et al. (2013), between 1981 and 2007 there were 332 identified pro-government militias working in about 50 countries in all regions of the world.[1] Governments recruit these allies to fight the governments' enemies by supplying military resources and wage compensation.[2] However, at the end of the war much of the weaponrys were left held in the hands of the militias, which poses additional security concerns.[3] This concern also has received much attention in the debate whether or not to provide weapons to local militias in the recent conflict in Syria.[4] In order to address this issue, many programs were introduced by the UN and the World Bank to buy up weapons after the war (Harris, 2002) or to provide conditional aid when the community surrenders its weapons (Mugumya, 2005).

This concern not only brings in measures to deal with the issue *ex post* but also reflects the way in which a government should supply weapons to a militia *ex ante*. In Kimberly Marten's recent book on warlords (Marten (2012)), she mentions how this concern affects the way in US arming the militia. In 2005-2006 during the US mission in Iraq, the Sons of Iraq (SOI), a Arab Sunni tribal militias, were working with US troops and Shia-led government of Iraq in fighting against Al Qaeda in Iraq. She cited from a war journalist Jim Michaels (Michaels, 2010) who observed on how the supply of weapons was implement in practice:

[1]The definition of a pro-government militia is: 1) one which is identified as pro-government or sponsored by the government (national or subnational); 2) one which is identified as not being part of the regular security forces; 3) one which is armed; 4) one which has some level of organization. More details can be found in Carey, et al. (2013).

[2]For example, Janjaweed militia supported by Sudanese government. (http://www.theguardian.com/world/2004/aug/01/sudan.jeevanvasagar) or Iran backed Taliban. (http://www.wsj.com/articles/iran-backs-taliban-with-cash-and-arms-1434065528) and also many others, such as Latin America (Mazei 2009), Indonesia (Cribb 2001), Africa, and elsewhere (Reno 2002; Mitchell 2004).

[3]For example, paramilitary in Columbia (Grajales, 2011).

[4]"It would have been nearly impossible for the United States to ensure its arms were ending up in the right hands." (http://www.usnews.com/debate-club/should-obama-have-armed-syrian-rebels-sooner

"... US commanders did not consciously support warlordism....When the US gave them [SOI] weapons it was always presented as a temportary stopgap."[5]

Based on with this policy suggestion, it is clear that when the US security forces work with militias in Iraq, the weapons left beyond US control are a major concern, one main contributor to this being that the weapons are supplied without careful consideration. What the US commander proposed was to supply weapons with a brief stopgap before another supply. However, it is unclear what effects result from using a supply strategy featuring stopgaps. The strategic consideration behind it is unclear, since another obvious choice is to replenish the stock of weapons so that the militia maintained enough capacity to fight. Therefore, the question is what the benefits associated with the temporary stopgap strategy are, so that governments may reasonably prefer it to any other course of action.

The focus of this paper is to understand the characteristics of supply strategies with stopgap features and to clarify the conditions in which governments prefer to use them. In order to do so, I develop a model in which the government and the militia interact in an infinite horizon. In each period, the government supplies weapons to the militia in order to fight the enemy. Apart from the weapons in hand, the militia also inputs costly efforts to fight. The effort is a private decision that cannot be observed by the government. The weapon holdings by the militia depreciate over time. The war could be won with a higher probability if the militia holds more weapons or inputs more effort. The government can benefit from winning the war while the militia can gain from the weapons left in their hands after the war. But the militia's gain through holding weapons is at the expense of the government's utility. Therefore, the government in supplying many weapons faces a trade-off: on the one hand, the war can be won more quickly but on the other hand too many weapons are held by the militia. In addition, the militia also strategically chooses how much costly effort to input.

Given the strategic concerns from both the government and the militia, I try to rationalize the use of a supply strategy with stopgap features. Before the analysis starts, I first define this supply strategy: the government first decides how much weaponry to supply and then stops supplying for a while before resuming. During the stopgap periods, the weapons held by the militia depreciate. Given this definition, my first finding is that if the government supplies with limited commitment, stopgap periods are never part of an equilibrium. This is because, without commitment, the government would always supply additional weapons to the militia if too few weapons are available. That means that the government will not

[5]This quote is from chapter 6 p.142 of Marten (2012).

choose not to supply weapons as part of an equilibrium. This result leads me to examine the situation in which the government can supply with commitment.

When the government can supply with commitment, I first show that an equilibrium exists for the government to use a temporary stopgap strategy. In order to rationalize the choice of stopgap strategy, I use it to compare with another commonly observed strategy - replenish weapons in every period so that the militia has a constant amount of weaponry available. If the government's welfare under the strategy with stopgaps is greater than that under the benchmark strategy, then the stopgap strategy is preferred.

In order to compare these different equilibrium, I characterize their differences in the militia's input of effort and the government's expected loss from leaving weapon beyond its control. I show that the militia's effort is constant over time when weapon holding is constant, while the effort associated with a stopgap strategy has a dynamic pattern. The militia's efforts are highest in periods with a supply of weapons and then start to trend down once entering the stopgap periods when the weapon holdings start to diminish due to depreciation. The dynamic pattern is due to the changes in the held weapons and in the the militia's continuation value. While the effort is greater in supply periods than is the effort under constant holding, the effort is less in the stopgap periods. Furthermore, the government can save losses if the war is won in a stopgap period because the militia will be holding fewer weapons.

In sum, there are three differences between the two equilibrium. First, under the stopgap strategy the effort is always higher in supply periods than is the effort under constant weapon holding. Second, the expected losses from weapons beyond government control are lower because the war is likely to be won in a stopgap period when fewer weapons are being held by the militia. Finally, both weaponry and effort are lower in stopgap periods, which suggests that the war is less likely to be won in these periods. Given these differences, using a one-period-stopgap strategy as an example, I look for sufficient conditions in which the supply strategy with stopgap produces greater welfare for the government than would accrue from a strategy of constant supply.

The main result of this paper can be summarized in two propositions: first, that when leaving weapons in the hands of a militia after a war is particularly costly for the government, the supply strategy with a stopgap is to be preferred. This is because it lowers the expected weapon holdings at the end of the war. Second, when the government wants to win a war now without delay, the strategy of supplying as a stopgap is again to be preferred. This is because the effort is greater in the periods with supply, thus raising the odds of ending the war.

In addition, for the second result to hold, I find that the militia should take future values

sufficiently into account. This is because it is only when the militia is patient enough to take its future weapon supply into account that it responds strategically to the stopgap strategy. Furthermore, when the weapons depreciate at a medium rate, the government prefers the supply strategy with stopgap. This is because the dynamic incentive effect is limited when the weapons do not depreciate much. Equally, when the weapons depreciate too fast, the dynamic incentive effect cannot compensate for the loss of holding too few weapons in periods with a stopgap strategy.

Literature Review

This paper is related to several different areas of study. First, it relates closely to the literature on the military and the potential to stage a coup (e.g. Acemoglu, Vindigni and Ticchi (2010a, 2010b), Besley and Robinson (2010), Leon (2014), Collier and Hoeffler (2006)). These papers discuss the dilemma facing governments between the strength of the military and the concern at the prospect of coups; Papers of this theme study how the government may control their own armed forces through size and budgets. In this paper, governments which employ militias share these concerns - supplying many weapons to win the war but also increasing the security threat from the militias. Given such dilemmas, this paper extends the discussion on the strength of the armed group from a static relationship to a dynamic interaction.

Second, this paper also contributes to the literature on the monopoly of violence. Researchers take the strength of non-governmental groups as exogenously given and focus on negotiation or power-sharing as ways of settling incipient conflict (e.g. Powell, 2013). In this paper, the strength of the militia is endogenously determined through the government's military sponsorship.

Third, as Blattman and Miguel (2010) and others (e.g. Dani Rodrik (1999), Cerra and Saxena (2008), Abadie and Gardeazabal (2003), and Weinstein (2005)) point out, many political and economic legacies are conferred by wars. Besides the impact of these, this paper along with others (e.g. Kurtenbach and Wulf, 2012) emphasizes security concerns as another legacy of war. In particular, it studies the how governments can strategically supply their allies in order to reduce the risk of leaving too many weapons outside their control after a war.

Fourth, the commitment problem has been the leading explanation for the cause of civil wars (Blattman and Miguel, 2010) and their long duration (Walter 1997, 2002, 2009). This paper also contributes to this literature by introducing another contracting problem between the government and its allies. The present paper takes the incomplete contracting problem between the government and a militia as exogenously given. Without commitment, the militia which was an ally during the war can turn itself into another security threat

afterwards.

Finally, this paper also relates to the discussion of ways to control war agents. Padro-i-Miquel and Yared (2012) claim that the government can choose to directly intervene to punish its agents. This paper introduces another instrument: the control of the weapon supply. This can be used to build up the strength of the militias to fight the enemy but it also raises security concerns after the war.

The next section presents the model. Section 3 characterizes the equilibria when the government has limited commitment, and Section 4 studies two government strategies with commitment. In the first one the government supplies weapons to keep a militia's holding constant. In the second, the government do not supply weapons in stopgap periods. Section 5 characterizes the differences in these two equilibria. Section 6 compares the government's welfare in the former with that in the latter supply strategy. Section 7 considers the comparative effects of stopgap strategies. Section 8 concludes. Proofs of the results are in the Appendix.

2. Model

I consider a dynamic environment in which a government supplies military weapons to a militia for fighting an enemy. In every period, the government chooses how many weapons to supply, or to supply none. The militia uses the weapons-in-hand, defined as weapons given in the past subject to depreciation, and the weapon supply in this period, together with its unobservable effort in fighting. In this scenario, if the war continues, the government cannot determine whether it does so due to the militia's shirking or to bad luck. If the government wins the war, the militia is not committed to returning the weapons. Instead, it can choose to hold these weapons to make later gains at the expense of the government's utility. This is the source of incentive misalignment between the government and the militia. I study the conditions in order to rationalize the use of the stopgap strategy. For expositional simplicity, I assume there are no other transfers from the government to the militia and focus simply on the government's weapon supply strategies.[6]

2.1. Setup

There are infinite time periods, $t = \{1, ..., \infty\}$. In every period the government (g) and the militia (m) repeat the following interaction. The government chooses $w_t \geq 0$ to give

[6]Even if the government uses transfer to exchange for the weapons after winning the war is committed, the militia can always use weapon-in-hand to renegotiate with the government. This assumption also helps the paper to focus on comparing weapon-supply strategies of the government.

to the militia. In order to focus on ways in which to allocate weapons, I assume that it costs nothing for the government to supply weapons. The weapons held by the militia are subjected to depreciation over time, leaving $\delta \in [0,\ 1]$ proportion of weapons in the next period. The higher δ is, the more slowly the weapons depreciate. The weapons-in-hand at time t is equal to $\bar{w}_t = \sum_{s=1}^{t} \delta^{t-s} w_s$. Given the value of the weapons held, the militia privately chooses the amount of effort $e_t \geq 0$ in fighting the enemy at a cost of $c(e_t) = \frac{c}{2}e^2$, $c > 0$. Nature determines whether the war is won with a probability of $p(\bar{w}_t + e_t)$. Otherwise, the war continues with a probability of $1 - p(\bar{w}_t + e_t)$. When the war is won, the government receives a payoff Δ^g and a loss depending on the value of the weapons held by the militia $G(\bar{w}_t) = G\bar{w}_t$, $G > 0$, while the militia receives a payoff of $M(w_t) = Mw_t$, $M > 0$.[7] I let the maximal weapons supplied be $\hat{w} > 0$ and the maximal effort inputs be $\hat{e} > 0$. The $p(\cdot)$ is assumed to be linear. Given a small $p > 0$, $p(w + e) = pw + pe \in [0, 1)$.[8] The government and the militia discount future values with a discount factor of $\beta_g \in [0,\ 1]$ and $\beta_m \in [0,\ 1]$, respectively. All the parameters are common knowledge for both the government and the militia. The only information asymmetry is that the government does not observe how much effort invested by the militia. The timing of the game is shown in the Figure 1.

2.2. Equilibrium Definition

Next, I present the equilibrium concept to characterize different supply strategies. The government maximizes its utility function, V_g, by choosing the amount of weapons to supply in every period, w_t, subjected to the militia's participation constraint, incentive compatibility constraint, and finally the feasibility constraints.

$$V_g = \max_{\{w_t\}_{t=1}^{\infty}} \sum_{t=1}^{\infty} \beta_g^{t-1} \Pi_{\gamma=1}^{t-1} (1 - p(\bar{w}_\gamma + e_\gamma)) p(\bar{w}_t + e_t) \{\Delta^g - G(\bar{w}_t)\} \tag{1}$$

subjected to

$$V_m^1 = \max_{\{e_t\}_{t=1}^{\infty}} \sum_{t=1}^{\infty} \beta_m^{t-1} \Pi_{\gamma=1}^{t-1} (1 - p(\bar{w}_\gamma + e_\gamma)) \{p(\bar{w}_t + e_t) M(\bar{w}_t) - c(e_t)\} \geq 0 \tag{2}$$

$$\beta_m^{t-1} \Pi_{\gamma=1}^{t-1} (1 - p(\bar{w}_\gamma + e_\gamma)) \big\{ - c(e_t) + p'(\bar{w}_t + e_t) \{M(\bar{w}_t) - V_m^{t+1}\} \geq 0,\ \forall t \in [1, ..., \infty] \tag{3}$$

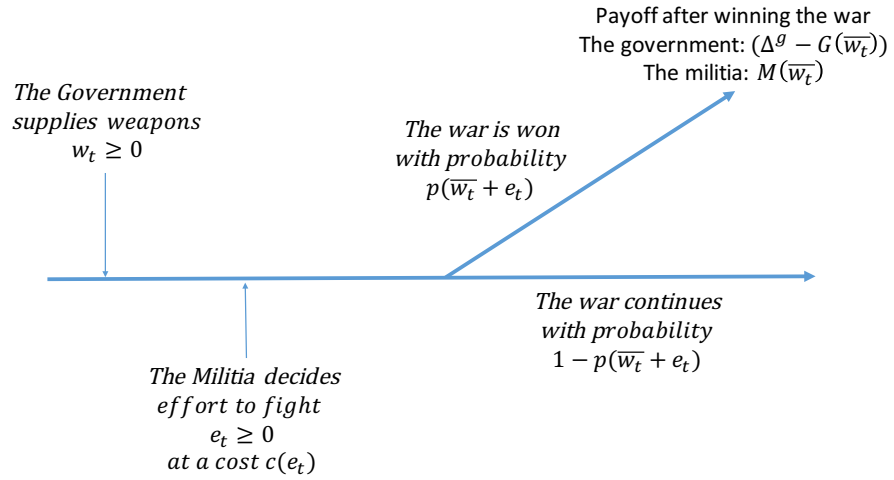
$$w_t \geq 0 \text{ and } e_t \geq 0,\ t \in [1, ..., \infty] \tag{4}$$

[7] I assume that the militia can only gain from weapons after winning the war. This assumption is not as strong as it sounds. When the government chooses which militia to work with, it is usually those who are also affected by the enemy and therefore there is an intrinsic value attached to winning for the militia. The setup can easily accommodate this intrinsic value without any qualitative differences in the main results.

[8] I assume 0 effort is a normalized benchmark and that means the minimal amount of efforts. Therefore, when the effort inputs are 0, the probability of winning the war can still be positive.

Constraint (1) is the government's utility function, V_g, which is the sum of the government's expected payoff from winning the war at a given time. I assume that Δ_g is large enough to keep the government's net payoff always positive. The cost G(w) is assumed to be incurred only when the war is won and the militia uses the weapons to threaten. Constraint (2) is the militia's participation constraint, which is the sum of the militia's expected payoff from winning the war at a given time and the cost of its effort. Constraint (3) is the militia's incentive compatibility constraint, which is derived from the first-order condition of the militia's effort-choosing problem. The choice of effort must balance three factors - the expected value of winning the war, $p'(\bar{w}_t + e_t)M(\bar{w}_t)$, the expected marginal costs of additional effort, $-c'(e_t)$, and a lower continuation value in expectation, $-p'(\bar{w}_t + e_t)V_m^{t+1}$. Constraint (4) ensures that there is no borrowing of weapons and that effort is non-negative.

Figure 1: Timing of The Game

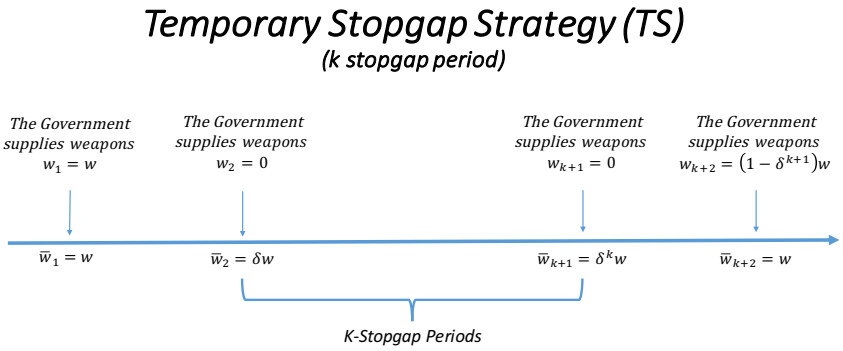


2.3. Supply With Temporary Stopgaps

After setting up the model, in this subsection, I now define the temporary stopgap strategy based on the model. A temporary stopgap strategy with k stopgap periods is defined as follows: to begin with, the government chooses to supply $w_1 > 0$ and then leaves k periods without any supply, $w_2 = w_3 = ... = w_{k+1} = 0$. After a k-period of no supply, the government supplies weapons again to the militia, $w_{(k+1)+1} > 0$, before entering a stopgap period. During all stopgap periods, the weapons held by the militia depreciate, $\bar{w}_s = \delta^{s-1} w_1$, for $s \in [2, ..., k+1]$. This pattern then repeats itself as long as the war continues. This strategy

is illustrated in Figure 2, below. In order to rationalize the use of a temporary stopgap strategy, in the next section, I start by examining whether a supply strategy with stopgaps would be adopted by a government without commitment.

Figure 2: *Temporary Stopgap Strategy*



3. Government Without Commitment

In this section, I characterize the equilibrium which takes into account a government supplying weapons with limited commitment. When the government cannot supply with commitment, it may face a time inconsistency problem. This means that if the government chooses a supply strategy ahead of time, it may renege upon it. Therefore, its decision becomes state contingent. In the following proposition, I show that, in this scenario, the government would not choose not to supply any weapons.

Proposition 1. *If the government supplies weapons without commitment, the temporary stopgap strategy is never part of an equilibrium,* $w_t > 0$ *for all* $t > 0$.

The proof of Proposition 1 is shown in Appendix section A. Proposition 1 states that if the government has no commitment, then it is never a rational choice for the government to leave gaps in its supply of weapons. The intuition behind this is that without commitment the government can in every period decide whether to supply weapons, given the existing weapon holdings. The government would have to use a threshold strategy. If the weapon stock is lower than the optimal level, the government would have an incentive to top it up to the desired level at no extra cost. However, if the weapon stocks are higher than the optimal level, the government would choose not to supply any weapons. But, since the supply relationship starts from no weapon holdings, the government begins by simply supplying weapons to the desired level. If the war continues, the weapons depreciate. The

government faces the same problem and then supplies again to the desired level. Since weapons will never be supplied beyond the optimal level, the government will not stop supplying weapons. Therefore, supplying no weapon should never be a possibility in the equilibrium when the government cannot commit.

In the next section, I move on to discuss the scenario when the government can supply weapons with commitment.

4. Government With Commitment

In the previous section, I showed that the government without commitment would not adopt a supply strategy with temporary stopgap supplies. In this section, I turn to examining the equilibrium in which the government supplies weapons with commitment; that is, it chooses the weapon supply strategy at the beginning of the game and does not renege upon it. Therefore, I apply the Nash equilibrium concept in the following analysis.

In order to rationalize the use of a weapons supply strategy with stopgaps, I focus on comparing the stopgap strategy with another strategy commonly used by the government – the supply-upto-level (SUL) strategy – in which the government supplies weapons in every period in order to keep the militia's weapon holdings constant. I illustrate these two strategies in Figure 3.

As the first example shown in Figure 3, the SUL strategy is meant to keep the weapon holdings constant, $\bar{w}_t = w$. After the first government supply, in every period the government simply supplies the weapons to make up for the depreciation $w_t = (1-\delta)w$. The temporary stopgap strategy, in contrast, leaves periods in which no weapons are supplied and lets the weapon holdings depreciate. In Figure 3, the second example is the one-stopgap-period strategy. In period 2, the government stops supplying and the weapon holdings depreciate at a rate of $1-\delta$, $\bar{w}_2 = \delta w$. In period 3, the government supplies weapons, $w_3 = (1-\delta^2)w$, to make up for the depreciation and reach the level set in period 1 once more. Below I first establish the Nash equilibrium associated with the SUL strategy and then proceed to the equilibrium associated with the temporary stopgap strategy.

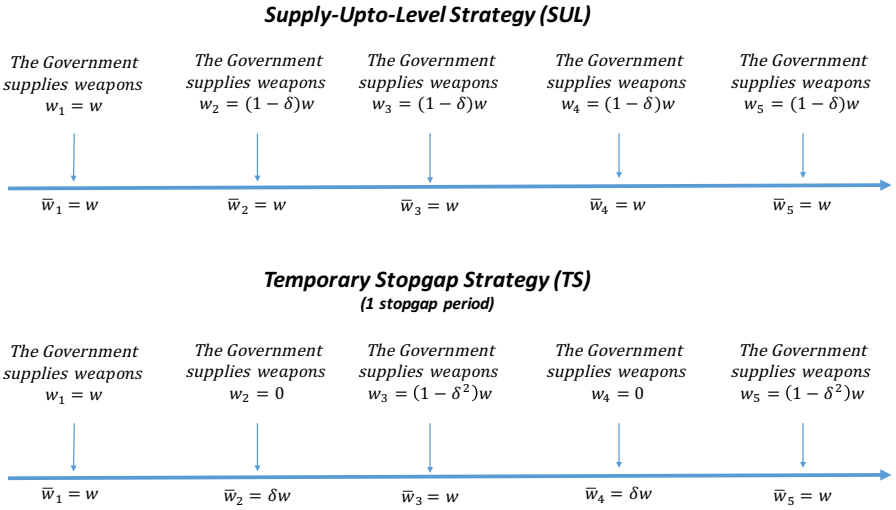
Proposition 2. ***(Supply-Upto-Level)*** *When the government can commit to supplying weapons which will to keep the weapon holdings constant, the equilibrium exists in which:*

1. The government supplies $w_1 = w^{SUL}$ followed by $w_t = (1-\delta)w^{SUL}$, for all $t \geq 2$.
2. In every period the militia exerts the same effort $e_t = e^{SUL}$, for all $t \geq 1$.

The proof of Proposition 2 is shown in Appendix section B. In the equilibrium associated

with the SUL strategy, the government in every period tops up the weapons to keep the weapon holdings constant, $\bar{w}_t = w^{SUL}$, for all $t \geq 1$.[9]

Figure 3: *Supply-Upto-Level (SUL)* vs. *Temporary Stopgap (TS, one-period)*



Since the weapon holdings are constant, the militia's optimization problem is recursive and stationary. Therefore, I can easily compute the militia's choice of effort. I find that in the equilibrium the militia uses the same effort $e_t = e^{SUL}$ in every period. The intuition behind this is straightforward: since the weapon holdings are fixed, the militia's problem is identical in each period and the choice of effort does not change conditions for later periods either. As a result, the militia chooses the same level of effort inputs in each period so long as the war continues.

In the next proposition, I establish the equilibrium associated with the temporary stopgap strategy.

Proposition 3. ***(Temporary Stopgap)*** *When the government can commit to stop supplying weapons for k periods, the equilibrium exists in which*

1. *The government supplies* $w_1 = w^{TS}$ *and* $w_2 = ... = w_{k+1} = 0$.
2. *This weapon supply strategy is repeated with the following pattern:*
   $(w_{s(k+1)+1},\ w_{s(k+1)+2},\ ...\ w_{s(k+1)+k+1}) = (\ (1-\delta^{(k+1)})w^{TS},\ 0,\ ...\ 0),\ s \in [1,\ 2,\ ...\ \infty]$.

[9] I assume for simplicity that the government knows the rate of weapon depreciation and is able to use the knowledge to control the level of weapons held by the militia.

3. *The militia's efforts conform to following pattern:*
$(e_{s(k+1)+1},\ ...,\ e_{s(k+1)+k+1}) = (e_1^k,\ ...,\ e_{k+1}^k),\ s \in [0,\ 1,\ 2,\ ...\ \infty]$

The proof of Proposition 3 is shown in Appendix section C. This equilibrium associated with a k-period-stopgap strategy is characterized with k periods of no supply after each supply period. During those stopgap periods, the weapon holdings are subjected to a depreciation at a rate of $1-\delta$. After k periods of no supply, the government supplies $w = (1-\delta^{(k+1)})w^{TS}$ again to replenish the holdings to a given level $\bar{w}_t = w^{TS}$. This gives the following weapon holding pattern which repeats itself every $k+1$ periods:

$$(\bar{w}_{s(k+1)+1},\ \bar{w}_{s(k+1)+2},\ ...\ \bar{w}_{s(k+1)+k+1}) = (\ w^{TS},\ \delta w^{TS},\ ...\ \delta^k w^{TS}),\ s \in [0,\ 1,\ 2,\ ...\ \infty]$$

Since the weapon holding pattern repeats itself in every k+1 period, the militia's effort choosing problem remains the same. Therefore, the militia's equilibrium efforts also follow a fixed pattern over k+1 periods and then repeat themselves. After establishing the equilibrium associated with each of the two supply strategies, in the next section, I characterize these two equilibria and discuss their differences.

5. Characterizing The Equilibria

In this section, in order to characterize the two equilibria, I focus on two aspects of them that prepare us to compare them in the next section. The first one to be discussed in subsection 5.1 is the militia's effort choices. Then I move on in subsection 5.2 to examining the government's losses from the militia's weapon holdings after winning the war.

5.1. The Militia's Effort Choices

In this subsection, I first discuss the militia's effort choice under the SUL strategy as the benchmark and then proceed to study the effort choices under the temporary stopgap strategy; finally I examine their differences and how their differences are affected by various government and militia characteristics.

5.1.1. Supply-Upto-Level Strategy

I start by examining how the various characteristics of effort choice change under the SUL strategy. The results are summarized in the following lemma.

Lemma 5.1. *The following statements hold true for the militia's effort choice under the SUL strategy,* e^{SUL}*:*

1. $\frac{\partial e^{SUL}}{\partial \beta_m} < 0$: *$e^{SUL}$ is decreasing in the militia's patience, β_m.*
2. $\frac{\partial e^{SUL}}{\partial \bar{w}} > 0$: *$e^{SUL}$ is increasing in weapon holdings, $\bar{w}$.*
3. $\frac{\partial e^{SUL}}{\partial \bar{w}} > 0$: *$e^{SUL}$ is increasing in gain from holding weapons after the war, M.*
4. $\frac{\partial e^{SUL}}{\partial \delta} = 0$: *$e^{SUL}$ does not change with the depreciation factor, δ.*

The proof of Lemma 5.1 is shown in Appendix section D. The first part of Lemma 5.1 states that the militia chooses to make fewer efforts when it is more patient, with a greater β_m. The intuition behind this is straightforward: when the militia discount future value less, the continuation value increases. Therefore, it reduces the militia's incentive to exert costly effort in order to win the war sooner. The second part of Lemma 5.1 states that when there are more weapon holdings, the militia is willing to put more effort into fighting the war. The intuition behind this is that the militia can gain from weapons after the war is over and thus holding more weapons will encourage the militia to put more effort into fighting. This also suggests that the weapon holdings not only increase the chance to win the war but also function similarly as a gain by rewarding the militia after the war is won. Following the same logic, if the militia could benefit more from the weapon holdings, such as earning a higher unit benefit from doing so (M), it would exert more effort in fighting. The final part of Lemma 5.1 states that the weapons' depreciation does not affect the militia's choice of effort. This is because, under the SUL strategy, the government is committed to supplying weapons to a fixed level. Therefore, the militia's weapon holdings are constant at any given period. Since the militia's effort choices are determined by the amount of weapon holdings over time and not by the amount supplied, the militia's choice of effort will not be affected by the weapons' depreciation. In the next lemma, I show that the militia is better off when it holds more weapons.

Lemma 5.2. $\frac{\partial V_m}{\partial \bar{w}} \geq 0$*: the militia's expected utility increases with its weapon holdings, $\bar{w}$.*

The proof of Lemma 5.2 is shown in Appendix section E. As discussed earlier, the weapon holdings serve as a reward for the militia to gain after winning the war. Therefore, Lemma 5.2 states that if more weapons are held by the militia, it can expect higher utility from them.

5.1.2. Temporary Stopgap Strategy

In this subsection, I turn to characterizing the militia's efforts under the temporary stopgap strategy. For expositional simplicity, I focus first on the one-stopgap-period strategy, k=1, and at the end of the section will generalize the result to a k-stopgap-period strategy at the end of the section. The following corollary describes the effort choices under the one-stopgap-period strategy.

Corollary 1. *Under the one-stopgap-period strategy ($k = 1$), there are two equilibrium efforts: $(e_{2s+1},\ e_{2s+2}) = (e_1^1,\ e_2^1),\ for\ all\ s \in [0,\ 1,\ 2,\ ...\ \infty]$.*

On the basis of Proposition 3, the equilibrium efforts under the one-stopgap-period strategy repeat a fixed pattern every two periods. When the government supplies weapons, the militia exerts effort, e_1^1. When the government supplies no weapons, the militia exerts efforts, e_2^1. Since the stopgap is one period and whenever the government supplies weapon the amount is fixed, the pattern of the militia's efforts repeats itself every two periods. Next, I characterize these two effort choices, summarizing the results in the following lemma.

Lemma 5.3. *The following statements hold true for the militia's choice of efforts under the one-stopgap-period strategy:*

1. $\frac{\partial e_1^1}{\partial \beta_m} < 0$ *and* $\frac{\partial e_2^1}{\partial \beta_m} < 0$*: e_1^1 and e_2^1 are decreasing in the militia's patience, β_m.*
2. $\frac{\partial e_1^1}{\partial \bar{w}} > 0$ *and* $\frac{\partial e_2^1}{\partial \bar{w}} > 0$*: e_1^1 and e_2^1 are increasing in weapon holding, $\bar{w}$.*
3. $\frac{\partial e_1^1}{\partial M} > 0$ *and* $\frac{\partial e_2^1}{\partial M} > 0$*: e_1^1 and e_2^1 are increasing in gain from weapons after the war, M.*
4. $\frac{\partial e_1^1}{\partial \delta} < 0$ *for* $\delta > \hat{\delta}$ *and* $\frac{\partial e_1^1}{\partial \delta} \geq 0$ *for* $\delta \leq \hat{\delta}$*: e_1^1 is decreasing in δ when δ is sufficiently large and increasing in δ otherwise.*
5. $\frac{\partial e_2^1}{\partial \delta} < 0$ *for* $\delta > \bar{\delta}$ *and* $e_2^1 = 0$ *for* $\delta \leq \bar{\delta}$*: e_2^1 is decreasing in δ and is equal to 0 when δ is small.*
6. $e_1^1 \geq e_2^1$.

The proof of Lemma 5.3 is shown in the Appendix section F. Lemma 5.3 establishes how the militia's effort choices under the one-stopgap-period strategy are affected by various factors. The first three statements concern the same properties as those in Lemma 5.1 and therefore the same intuitions apply. The militia chooses to make a greater effort if he discounts the future more, holds more weapons, and can gain more from holding weapons. The next two statements refer to the properties that fundamentally distinguish the two strategies. Unlike the effort choice under the SUL strategy, the effort choices under the stopgap strategy are affected by weapon depreciation, δ. In particular, the two efforts respond in different directions. First, there is a non-monotonic effect of the weapons' depreciation, δ, on the militia's effort choice in the supply periods, e_1^1. If the weapon depreciation is rapid ($\delta \leq \hat{\delta}$), the militia chooses to put in more effort for a slower weapon depreciation. Conversely, if the weapons depreciate slowly ($\delta > \hat{\delta}$), the militia chooses to make less effort with slower weapon depreciation. In other words, e_1^1 has a concave relationship with δ. Second, the militia chooses not to exert any effort in periods without weapon supply, $e_2^1 = 0$, if the weapons depreciate fast ($\delta \leq \bar{\delta}$). But the militia chooses greater effort for a slower weapon depreciation when the weapons depreciate slowly ($\delta > \bar{\delta}$). In other words, e_2^1 increases with δ when δ is large enough.

In order to trace the intuition behind the last two statements, I start from the effort made in periods without supply, e_2^1, and then move to discussing the effort made in periods with supply, e_1^1. Previously in Lemma 5.2 I mentioned that the weaponry held by the militia can be treated as a reward for winning the war. Therefore, more weaponry would encourage the militia to make more effort to fight the enemy. When the weapons depreciate fast, in periods without supply the weapon holdings become fewer, to the point where it is no longer worth investing any effort in fighting. This is because it would imply that too few weapons would be gained from winning the war and also that a further supply would arrive by the time the war had been delayed for one period. Both factors drive the militia's incentive not to make an effort to fight in periods without supply. Therefore, if δ is small, the militia chooses not to invest any effort in periods without supply, $e_2^1 = 0$. However, if the weapon depreciate slowly enough, e_2^1 becomes positive. And the slower the weaponry depreciates, the more weapons are held by the militia in a period of no supply; thus the militia will put in more effort. Therefore when δ is sufficiently large, the militia chooses to input greater effort in periods without supply, e_2^1 for a higher δ.

Now I resume the discussion on the effect that weapons depreciation has on the effort choice in periods with supply, e_1^1. The militia in supply periods chooses to make less effort if the weapons depreciate more slowly because in periods without supply the weaponry does not depreciate so much. The militia's continuation value is higher for a even greater δ. However, if the weapons depreciate sufficiently fast, the militia chooses to make more effort when the weapon depreciation is slower. This result seems counter-intuitive. The intuition behind this is as follows: in periods without supply, when the weapons depreciate fast, fewer weapons remain and thus the reward for winning the war is also small. This is true to the point where the militia no longer finds it worthwhile to input any effort, $e_2^1 = 0$. If in periods without supply the weapons depreciation becomes slightly slower, it will increase the probability of winning the war. This means that there is a higher chance of winning the war when the reward is small. Therefore, the militia will have an incentive to make more effort in supply periods and this will lower the chance that the war will continue and be won in periods without a weapons supply.

The last property in Lemma 5.3 states that the militia will choose to make a greater effort in supply periods than in periods without supply, i.e. $e_1^1 > e_2^1$. There are two main reasons for asserting this. First, the militia in supply periods holds more weapons than it does in periods without supply. Earlier in the discussion, I showed that the weapons serve as a reward that the militia can gain if the war is won. Therefore, holding more weapons will incentivize the militia to make more effort. This is the first reason why the militia is expected to input more effort in supply periods. Second, for the militia each supply period is

immediately followed by a period without supply, while a periods without supply is followed by a period with supply. In Lemma 5.2, I show that the militia's utility is higher when it holds more weapons. This suggests that the militia in supply periods has a lower continuation value than in periods without supply. Therefore, the militia has another incentive to make a greater effort in supply periods.

After establishing the comparative statics for the militia's effort choices under the two strategies, in the next subsection I compare how the differences between them change with various characteristics.

5.1.3. Effort Choices in Comparison

Given Lemma 5.1 and 5.3, I can now compare the effort choices that the militia makes between these two strategies. The results are summarized in the following lemma.

Lemma 5.4. *Given a fixed level of weapon holdings whenever there is a supply, w,*

1. *During the weapon supply period: $e_1^1(w) > e^{SUL}(w)$.*
2. *During the period when no weapon is supplied: $e_2^1(w) < e^{SUL}(w)$.*

The proof of Lemma 5.4 is shown in Appendix section G. The first part of Lemma 5.4 states that for a given weapon holding, w, the militia will choose greater effort in periods with weapon supply under the temporary stopgap strategy, $e_1^1(w)$, than it will under the SUL strategy, $e^{SUL}(w)$. The intuition behind this is that if the weapon holdings are fixed whenever replenishments are supplied, w, the militia under the SUL strategy always holds w, while under the one-stopgap-period strategy it holds w when the government supplies weapons and δw in stopgap periods. This suggests that the militia under the SUL strategy holds no fewer weapons than when it is under the one-stopgap-period strategy. As previously discussed, the weapon holdings can be treated as a reward for winning the war. As a result, the militia's continuation value is higher with the SUL strategy than it is with the one-stopgap-period strategy. This incentivizes the militia in supply periods under the one-stopgap-period strategy to invest more effort in fighting and reduces the likelihood that the war will continue. Therefore, e_1^1 is greater than e^{SUL}.

The second statement in Lemma 5.4 suggests that in stopgap periods, the militia will choose to invest less effort under the temporary stopgap strategy, $e_2^1(w)$, than under the SUL strategy, $e^{SUL}(w)$. As previously discussed, the continuation value under the temporary stopgap strategy is lower than it is under the SUL strategy. This incentivizes the militia to exert more effort under the SUL strategy. However, despite a lower continuation value, fewer weapon holdings can be gained from winning the war in periods without supply. Furthermore,

knowing that the supply will arrive in the next period, the militia, if patient enough, might be better off if it delayed the war for another period and did so by making less effort. This would incentivize the militia to make less of an effort. In the proof, I show that the effect of the latter dominates the former. Therefore, the militia would choose $e_2^1(w)$ smaller than $e^{SUL}(w)$.

In sum, Lemma 5.4 suggests that, taking the effort choice under the SUL strategy as a benchmark, the government when using the stopgap strategy is making a trade-off between benefiting from a greater effort in the weapon supply periods at the expense of having fewer weapons and a lower effort in periods without supply. Since the purpose of this paper is to compare these two strategies, the natural next step is to investigate how these effort differences change with other various factors, in particular, the militia's patience, β_m, and the weapons' depreciation, δ. The first result is summarized in the following lemma.

Lemma 5.5. *Given a fixed level of weapon holdings whenever there is a supply, w,*

- $\frac{\partial|e_1^1(w)-e^{SUL}(w)|}{\partial\beta_m} > 0$*:* $|e_1^1(w) - e^{SUL}(w)|$ *is increasing in* β_m*.*
- $\frac{\partial|e_2^1(w)-e^{SUL}(w)|}{\partial\beta_m} < 0$*:* $|e_2^1(w) - e^{SUL}(w)|$ *is decreasing in* β_m*.*

The proof of Lemma 5.5 is shown in Appendix section H. Lemma 5.5 shows how the differences in efforts between the two strategies change with the militia's patience, β_m. The first part of Lemma 5.5 states that, if the militia is more patient, it invests even greater effort in supply periods under the stopgap strategy than under the SUL strategy. The reason behind this is as follows: if the militia discounts future value fully, it will care only about its current weapon holdings. Regardless of the supply strategies, if the weapon holdings are identical the effort choices are no different. But once the militia cares enough about future values, the difference in the continuation value starts to matter. As Lemma 5.1 and 5.3 suggest, both effort choices are decreasing in β_m because the militia value the future more. Since the slope is steeper for e^{SUL} than for e_1^1 when facing a greater value in β_m, the difference is greater when the militia is more patient.

The second part of Lemma 5.5 states that, in periods without supply under the one-stopgap-period strategy, the militia invests an amount of effort closer to the amount under the SUL strategy, when the militia is more patient. The intuition behind this is similar to the first statement that although both efforts are less when the militia is more patient, the rising slope of the continuation value is steeper for the SUL strategy and therefore the associated effort falls faster than the effort in periods without supply under the stopgap strategy. Since $e_2^1 < e^{SUL}$, the difference is smaller when the militia is more patient. In sum, these results suggest that, if the militia is more patient, it will input more effort in supply periods than it would under SUL strategy, and it will input more or less the same effort in no-supply

periods. In the following lemma, I show how these differences in effort choices between the two strategies change with the rate of weapons depreciation.

Lemma 5.6. *Given a fixed level of weapon holdings whenever there is a supply, w,*

- $\frac{\partial|e_1^1(w)-e^{SUL}(w)|}{\partial\delta} < 0$ *for* $\delta > \hat{\delta}$ *and* $\frac{\partial|e_1^1(w)-e^{SUL}(w)|}{\partial\delta} \geq 0$ *otherwise:* $|e_1^1(w) - e^{SUL}(w)|$ *is decreasing in* δ *when* δ *is sufficiently large and increasing in* δ *otherwise.*
- $\frac{\partial|e_2^1(w)-e^{SUL}(w)|}{\partial\delta} \leq 0$*:* $|e_2^1(w) - e^{SUL}(w)|$ *is non-increasing in* δ*.*

The proof of Lemma 5.6 is shown in Appendix section I. As suggested by Lemma 5.1, the effort choice under the SUL strategy, e^{SUL}, is not affected by weapons depreciation. Therefore, the effect of weapons depreciation on the differences in efforts is dominated by the effect on the effort choices under the one-stopgap-period strategy. As a result, Lemma 5.6 is a restatement of the fourth and the fifth parts in Lemma 5.3. The intuition behind this is therefore no different from that behind Lemma 5.3. In sum, these results suggest that for a greater δ both efforts under the stopgap strategy converge the effort choice under the SUL strategy. But if δ is small enough, both efforts in the stopgap strategy will become greater with a greater δ. In the next Corollary, I extend some of the results in previous lemmas to the strategy with k stopgap periods.

Corollary 2. *Given a fixed level of weapon holdings whenever there is a supply, w,*

1. *During the weapon supply period:* $e_1^k(w) > e^{SUL}(w)$*.*
2. *In the last no weapon supply period:* $e_{k+1}^k(w) < e^{SUL}(w)$*.*
3. *The efforts have the following pattern:* $e_1^k(w) \geq e_2^k(w) \geq ... \geq e_{k+1}^k(w)$*.*
4. *The difference of efforts in (3) is decreasing in* δ*.*

The first part of Corollary 2 states that the militia always invests greater effort in supply periods under the temporary stopgap strategy than under the SUL strategy. The second part of Corollary 2 states that the militia always invests less effort in the last stopgap period under the temporary stopgap strategy than under the SUL strategy. These results are similar to Lemma 5.4. The third part of Corollary 2 states that the militia's effort choices decrease over the stopgap periods. The intuition behind this is similar to that behind the last statement in Lemma 5.3 that the militia's continuation values grow over the stopgap periods as they get closer to the supply period while the weapon holdings are depreciating over the stopgaps. The two factors incentivize the militia to exert less effort. The final part of Corollary 2 states that the differences in the militia's effort choices are smaller when the weapons depreciate more slowly, a higher δ. The intuition is also straightforward: as the weapons depreciate

more slowly, the relative differences in weapon holdings in the stopgap periods is smaller. Therefore, the militia is given a limited incentive to differentiate between its inputs of effort. After studying the effort choices under the two supply strategies, I turn to look at the way in which the stopgap strategy may affect the government's losses from weapons beyond its control after the war.

5.2. The Losses when Weapons are Left beyond Government Control

Finally, in this subsection, I compare the government's losses due to weapons being permanently held by the militia after the war ends. The differences in the government's losses between the two strategies are documented in the following lemma.

Lemma 5.7. *Given a fixed level of weapon holdings whenever the supply, w:*

1. *Supply-upto-level strategy: $G(\bar{w}_t) = G(w)$, for $t \geq 1$.*
2. *One-stopgap-period strategy: $\{G(\bar{w}_s), G(\bar{w}_{s+1})\} = \{G(w),\ G(\delta w)\}$ for odd number s.*

The first part of Lemma 5.7 states that under the SUL strategy the government suffers a fixed amount of losses due to the constancy of the militia's weapon holdings. The losses under the one-stopgap-period strategy, stated in the second part of Lemma 5.7, follow the pattern of weapon holdings that repeats itself every two periods. In supply periods, the losses are the same as those in the SUL strategy, $G(w)$. But in periods without supply, the government's losses are smaller because of fewer holdings, $G(\delta w) < G(w)$. This suggests that whenever the war is won in periods without weapon supply the government can save $G(w) - G(\delta w)$ in losses. The likelihood of winning the war in periods without supply lowers the expected losses for the government. This is another factor that incentivizes the government to use the one-stopgap-period strategy. After characterizing and discussing the two equilibrium, in the next section I calculate and compare the government's welfare associated with the two equilibrium in order to rationalize the adoption of the stopgap strategy.

6. Rationalizing The Stopgap Strategy

In this section, I compare the government's welfare associated with each of the two strategies in order to rationalize the adoption of the temporary stopgap strategy. For expositional simplicity, I continue to focus on the equilibrium with one-stopgap-period strategy when considering the temporary stopgap strategy. To undertake this exercise, I explore conditions in which the government would prefer the one-stopgap-period strategy. In particular, I focus on two government characteristics - the losses due to the militia's retention of weapons after

the war, G, and the patience of the government, β_g. The government's incentive to reduce the losses due to weapons remaining beyond its control leads to the following proposition:

Proposition 4. *If* $G > \bar{G}$, *then* $V_g^{TS1} > V_g^{SUL}$.

The proof of Proposition 4 is shown in Appendix section J. Proposition 4 states that if it is very costly for the government to leave weapons in the hands of the militia after the war is over, $G > \bar{G}$, then the temporary stopgap strategy gives a greater government welfare than the SUL strategy, $V_g^{TS1} > V_g^{SUL}$. The intuition behind this can be derived straight from Lemma 5.7 that the government can reduce the expected losses so long as it is probable that the war will be won in a period without a weapon supply (fewer weapon holdings). This implication is important for it suggests that a government that may suffer substantial threats from non-state agents holding weapons after the war will prefer the temporary stopgap strategy. This intuitive result supports the rationale behind the US commander's proposition at the beginning of this paper. However, this is not the only way that the government can gain from the temporary stopgap strategy. As discussed in the previous section, a government using the stopgap strategy may also benefit from the higher effort made by the militia in a supply period. This factor leads to the next proposition discussing the way in which the government discounts future value can affect its preference for one of the two strategies.

Proposition 5. *If* $G < \bar{G}$ *and* $\beta_g < \bar{\beta}_g^L$, *then* $V_g^{TS1} > V_g^{SUL}$.

The proof of Proposition 5 is shown in Appendix section K. Proposition 5 states that the government when it is less patient prefers the temporary stopgap strategy, $\beta_g < \bar{\beta}_g^L$. The intuition behind this is as follows: the government begins by supplying weapons and then moving onto stopgap periods. Lemma 5.4 suggests the efforts are in this case higher in periods with supply than corresponding ones would be under the SUL strategy. Therefore, the effort made is higher at the beginning when using the stopgap strategy than it is when using the SUL strategy. Since greater efforts at the beginning increase the chance of winning the war without delay, this makes the stopgap strategy preferable. This proposition suggests that a government which hopes to win the war without delay should adopt the temporary stopgap strategy. Propositions 4 and 5 are the main rationales behind the government's adopting the stopgap strategy. However, the effect of the stopgap strategy of raising the amount of effort in supply periods would depend on the militia's patience, β_m, and the rate at which the weapons depreciate, δ. Therefore, in the next section, I move to a discussion of the way in which these elements would shape the effects of the stopgap strategy.

7. Comparative Statics

In the previous section, I showed that when the government is hoping to win a war as soon as possible, the stopgap strategy is preferred. This is because it makes the militia put in more effort in the supply periods. However, the effect of the stopgap strategy to bring forward the effort will depend on the militia's characteristics and the quality of the weapons. In this section, I examine how the militia's patience and the weapons' depreciation rate affect the result of the stopgap strategy.

7.1. The Militia's Characteristics

In this subsection, I examine how the militia discounts of future value will affect the amount of effort that it invests, which then determines the government's preference for one or other of the two strategies. In particular, I look for conditions in which the e_1^1 is greater than e^{SUL}, and e_2^1 is closer to e^{SUL}. This means that the government can gain from the greater efforts in supply periods without losing too much efforts in periods without supply. The result is summarized in the following proposition.

Proposition 6. *If $G < \bar{G}$ and $\beta_m > \bar{\beta}_m^L$, then $V_g^{TS1} > V_g^{SUL}$.*

The proof of Proposition 6 is shown in Appendix section L. Proposition 6 states that the government would prefer the temporary stopgap strategy when the militia is sufficiently patient, $\beta_m > \bar{\beta}_m^L$. This result can be easily shown using Lemma 5.5. The intuition behind this is twofold: first, as stated in Lemma 5.5, the effort in supply periods, e_1^1, and the effort under the SUL strategy, e^{SUL}, diverges (with the e_1^1 being greater) when the militia is more patient. Second, also stated in Lemma 5.5, the effort in periods without supply, e_2^1, is closer to the effort inputs in the SUL strategy, e^{SUL} with a greater β_m. Therefore, when the militia is patient enough, comparing with the SUL strategy, the militia under the stopgap strategy inputs greater effort in periods with supply and invests similar effort in periods without supply. This condition relaxes the requirement on the government's impatience to prefer the stopgap strategy. Therefore, greater patience from the militia makes the stopgap strategy more likely to be the government's preference. This result does not imply that the government should work with a militia which is more patient. In fact, the opposite is what actually works: - the government should choose to work with a militia which is less patient and consequently will make more effort. What this result suggests is that when the government has to work with a militia which is very patience, the government should adopt the stopgap strategy, which will produce more welfare for itself.

7.2. The Weapon's Depreciation

Finally, in this subsection I discuss how weapons' depreciation will determine the government's preference between the two strategies.

Proposition 7. *If* $G < \bar{G}$ *and* $\bar{\delta}^U > \delta > \bar{\delta}^L$*, then* $V_g^{TS1} > V_g^{SUL}$.

The proof of Proposition 7 is shown in Appendix section M. Proposition 7 states that when the weapons depreciation is at an intermediate level, by the government prefers the stopgap strategy to the SUL strategy. The intuition behind this is as follows: when the weapons depreciate slowly, $\delta > \bar{\delta}^U$, the extra effort in periods with supply will not be large enough to compensate for the losses from both lower weapon holdings and less effort invested during periods without supply. Conversely, when the weapons depreciate rapidly, $\delta < \bar{\delta}^L$, the weapon holdings and also the effort invested in periods without supply will be too low to compensate for it by extra effort in the periods with supply. These conditions also relax the requirement on the government's impatience, β_g, to prefer the stopgap strategy. Therefore, a government that supplies weapons with a medium rate of depreciation will be more likely to prefer the stopgap strategy.

8. Conclusions

In order to lessen the likelihood of conflict, one important precaution is to ensure that military resources do not fall into the hands of non-state groups. However, due to limited state capacity, countries often rely on many non-state militias to fight their enemies. Governments recruit these allies for this purpose by offering to supply military resources and wage compensation. Their sponsorship seems to explain why so many weapons are left beyond governments' control. In order to deal with this dilemma, a weapon-supply strategy with stopgaps is suggested – the militia holds fewer weapons in stopgap periods due to depreciation. In this paper, I sought to understand why this strategy might be preferred by governments which supply arms to non-state militias. I find that this strategy is sustained only if the government can commit itself to a given supply strategy. Compared with another commonly observed supply strategy, in which the stock of weapons is kept constant by government replenishment, the stopgap strategy is the one that governments prefer when leaving the weapons in the hands of a militia is particularly harmful and also when the government hopes to win the war very soon afterwards.

There are a few avenues for future development on this subject. First, in this paper, I have been using the one-stopgap-period strategy as an example. It would be very useful also to know how different conditions might shape the number of stopgap periods chosen.

Second, today's conflicts all seem to rely heavily on the effort invested by the militia, e.g. in guerrilla warfare. Therefore, it would be useful to know whether and how the result would differ if the militia's effort mattered more in relation to the weapons. Third, I assume the government is fully informed about the rate of the weapons' depreciation and I also set the rate to be constant. However, in many situations, this information is also secretly known to the militia and the rate of deprecation may be affected by the effort invested by the militia. This may generate different dynamics and results. Finally, the central purpose of this paper is to rationalize the use of stopgap strategy, but it would also be essential to learn what the optimal strategy would be for a government to employ.

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Appendix A. Proof of Proposition 1

In this section, I show that the government without commitment cannot have supply stopgaps as part of equilibrium. Because the government supplies without commitment, in every period the government would make supply decision based on current weapon holdings. This makes the current weapon holdings a state variable. In every period the government faces the same problem. Therefore, its problem can be rearranged into a recursive structure:

$$V^G(w) = \max_{w' \geq w} \; p(w'+e)(\Delta^g - G(w')) + \beta_g(1-p(w'+e))V^G(\delta w') \tag{5}$$

The government uses a threshold strategy: if the current holdings is below the optimal level, then the government would top it up to the desired level without any cost. However, if the current holdings is above the optimal level, the government will choose not to supply any weapons. Since there were no weapons held by the militia initially, in the equilibrium, the weapon supply will be $w_1 = w*$ and then fixed at $w_t = \delta w^*$ for $t > 1$. The weapon holdings by the militia are $w*$ for all $t \geq 1$. The militia also learns that weapons will always be top up to a certain level. Therefore it generates a constant level of efforts, e.

$$V^M(w^*) = \max_{e \geq 0} \; p(w^*+e)M(w^*) - c(e) + \beta_g(1-p(w^*+e))V^M(w^*) \tag{6}$$

The next step is to take the first-order-condition:

$$p'(w'+e)(1+\frac{\partial e}{\partial w'})\{\Delta^g - G(w') - \beta_g V^M(\delta w')\} - p(w'+e)G + \beta_g(1-p(w'+e))\delta\frac{\partial V^M(\delta w')}{\partial \delta w'} \tag{7}$$

If $\delta w'$ is below the w^*, then it does not affect the value of V^M. Since $\frac{\partial V^M(\delta w')}{\partial \delta w'} = 0$ when $\delta w < w^*$, the optimal level of w is determined by

$$p(1+\frac{\partial e}{\partial w^*})\{\Delta^g - G(w^*) - \beta_g V^M\} - p(w^*+e)G = 0 \tag{8}$$

This equation along with the following first-order condition characterize the equilibrium

$$p'(w^*+e)M(w^*) - c'(e) - \beta_g p'(w^*+e)V^M(w^*) = 0 \tag{9}$$

In every period, the government supplies $(1-\delta)w^*$ and the militia inputs effort $e_t = e^*$. The equilibrium does not include periods with no weapon supplies.

Appendix B. Proof of Proposition 2

In this section, I characterize the equilibrium associated with the supply-upto-level strategy. Under SUL strategy, the government supplies weapons $w_1 = w$ and then followed by $w_t = (1-\delta)w$, for all $t \geq 2$. This ensures $\bar{w}_t = w$, for all $t \geq 1$. The government chooses w to maximize welfare, V_g.

I start by solving the militia's response function when the government commits to have the militia holding $\bar{w}_t = w$, for all $t \geq 1$. Then I solve for the w that maximize the government's welfare. Since the militia faces the same problem in every period with the same amount of weapon-in-hand. The militia's equilibrium effort choice is fixed as a function of w, $e = e^{SUL}(w)$. Therefore, the militia's problem is stationary and reduced to a recursive structure as follows:

$$V_m^{SUL} = \max_e p(w+e)M(w) - c(e) + \beta_g(1-p(w+e))V_m^{SUL} \tag{10}$$

To find the militia's participation constraint, the equation 10 is rearranged and gives

$$V_m^{SUL} = \frac{p(w+e^{SUL}(w))M(w) - c(e^{SUL}(w))}{1-\beta_g(1-p(w+e^{SUL}(w)))} \geq 0 \tag{11}$$

This can always be achieved since the militia can simply input no efforts and no other costs will occur. To find the militia's incentive compatibility constraint, I simply look for the first-order condition:

$$-c'(e) + p'(w+e)\{M(w) - V_m\} = 0 \tag{12}$$

Substituting V_m^{SUL} from equation 11 into equation 12, the militia's incentive compatibility constraint is as follows:

$$\frac{p'(w+e)[(1-\beta_m)M(w) + \beta_m c(e)]}{1-\beta_m(1-p(w+e))} = c'(e) \tag{13}$$

Therefore the equilibrium associated with the supply-upto-level strategy is characterized as follows:

$$V_g^{SUL} = \max_{\{w\}} p(w+e)\{\Delta^g - G(w)\} + \beta_g(1-p(w+e))V_g^{SUL} \tag{14}$$

$$s.t.$$

$$V_m^{SUL} = \frac{p(w + e^{SUL}(w))M(w) - c(e^{SUL}(w))}{1 - \beta_m(1 - p(w + e^{SUL}(w)))} \geq 0 \tag{15}$$

$$-c(e) + p'(w + e)\{M(w) - V_m\} = 0, \ \forall t \in [1, ..., \infty] \tag{16}$$

$$w \geq 0 \ and \ e \geq 0 \tag{17}$$

This equilibrium can be summarized and characterized by two equations: 1) the militia's incentive compatibility constraint 13. 2) the government's first-order condition which pins down the optimal choice of weapon supply level, w^{SUL}, shown below:

$$p'(w + e^{SUL}(w))(1 + \frac{\partial e^{SUL}(w)}{\partial w})\{\Delta^G - G(w) - \beta_g V_g^{SUL}\} - p(w + e^{SUL}(w))G'(w) = 0 \tag{18}$$

$$\text{where } V_g = \frac{p(w + e^{SUL}(w))\{\Delta^g - G(w)\}}{1 - \beta_g(1 - p(w + e^{SUL}(w)))}$$

To ensure the existence of interior solution, two conditions are assumed:

Assumption B.1. *More weapons encourage fighting:* $\frac{\partial e^{SUL}(w)}{\partial w} > 0$

Assumption B.2. *Diminishing effect of additional weapon on effort:* $\frac{\partial^2 e^{SUL}(w)}{\partial w^2} < 0$

While Assumption B.1 ensures the government's first-order condition is held, Assumption B.2 ensures the second-order condition is satisfied as well as the uniqueness of the solution.

Appendix C. Proof of Proposition 3

Under the temporary stopgap strategy, the government supplies weapons as follows: $w_1 = w^{TS}$ and $w_2 = ... = w_{k+1} = 0$, then followed by
$(w_{s(k+1)+1}, \ w_{s(k+1)+2}, \ ... \ w_{s(k+1)+k+1}) = (\ (1 - \delta^{(k+1)})w^{TS}, \ 0, \ ... \ 0), \ s \in [1, \ 2, \ ... \ \infty]$.
Therefore the militia's weapon-in-hand has the following pattern:
$(\bar{w}_{s(k+1)+1}, \ \bar{w}_{s(k+1)+2}, \ ... \ \bar{w}_{s(k+1)+k+1}) = (\ w^{TS}, \ \delta w^{TS}, \ ... \ \delta^k w^{TS}), \ s \in [0, \ 1, \ 2, \ ... \ \infty]$
Because the weapon-in-hand has a fixed pattern that itself repeats every $k+1$ period, the militia's problem also repeats every $k+1$ period: $(e_{s(k+1)+1}, \ ..., \ e_{s(k+1)+k+1}) = (e_1^k, \ ..., \ e_{k+1}^k), \ s \in [0, \ 1, \ 2, \ ... \ \infty]$. This reduces the militia's problem to a recursive structure with $k+1$ effort

choices.[10]

$$V_m^k = \max_{\{e_t^k\}_{t=1}^{k+1}} \sum_{l=0}^{k} \beta_m^l \Pi_{\gamma=1}^{l}(1-p(\delta^{\gamma-1}w+e_\gamma^k))[p(\delta^l w+e_{l+1}^k)M(\delta^l w)-c(e_{t+1}^k)] + \beta_m^{k+1}\Pi_{\gamma=1}^{k+1}(1-p(\delta^{\gamma-1}w+e_\gamma^k))V_m^k \tag{19}$$

To find the militia's participation constraint, the equation 19 is rearranged and gives

$$V_m^k = \frac{\sum_{l=0}^{k} \beta_m^l \Pi_{\gamma=1}^{l}(1-p(\delta^{\gamma-1}w+e_\gamma^k))[p(\delta^l w+e_{l+1}^k)M(\delta^l w)-c(e_{t+1}^k)]}{1-\beta_m^{k+1}\Pi_{\gamma=1}^{k+1}(1-p(\delta^{\gamma-1}w+e_\gamma^k))} \geq 0 \tag{20}$$

To find the militia's incentive compatibility constraint, I take the first-order condition for e_t^k, for all $t \in [1, ..., k+1]$:

$$\begin{aligned} &-c'(e_t^k)+p'(\delta^{t-1}w+e_t^k)\{M(\delta^{t-1}w)- \\ &\sum_{l=t}^{k} \beta_m^l \Pi_{\gamma=t+1}^{l}(1-p(\delta^{\gamma-1}w+e_\gamma^k))[p(\delta^l w+e_{l+1}^k)M(\delta^l w)-c(e_{l+1}^k)] \\ &\qquad -\beta_m^{k+2-t}\Pi_{\gamma=t+1}^{k+1}(1-p(\delta^{\gamma-1}w+e_\gamma^k))V_m^k\} = 0 \end{aligned} \tag{21}$$

Therefore the equilibrium associated with the temporary stopgap strategy is characterized as follows:

$$V_g^k = \max_{\{w\}} \sum_{l=0}^{k} \beta_g^l \Pi_{\gamma=1}^{l}(1-p(\delta^{\gamma-1}w+e_\gamma^k))[p(\delta^l w+e_{l+1}^k)[\Delta^g - G(\delta^l w)]] + \beta_g^{k+1}\Pi_{\gamma=1}^{k+1}(1-p(\delta^{\gamma-1}w+e_\gamma^k))V_g^k \tag{22}$$

$$s.t.$$

$$V_m^k = \frac{\sum_{l=0}^{k} \beta_m^l \Pi_{\gamma=1}^{l}(1-p(\delta^{\gamma-1}w+e_\gamma^k))[p(\delta^l w+e_{l+1}^k)M(\delta^l w)-c(e_{t+1}^k)]}{1-\beta_m^{k+1}\Pi_{\gamma=1}^{k+1}(1-p(\delta^{\gamma-1}w+e_\gamma^k))} \geq 0 \tag{23}$$

$$\begin{aligned} &-c'(e_t^k)+p'(\delta^{t-1}w+e_t^k)\{M(\delta^{t-1}w)- \\ &\sum_{l=t}^{k} \beta_m^l \Pi_{\gamma=t+1}^{l}(1-p(\delta^{\gamma-1}w+e_\gamma^k))[p(\delta^l w+e_{l+1}^k)M(\delta^l w)-c(e_{l+1}^k)] \\ &\qquad -\beta_m^{k+2-t}\Pi_{\gamma=t+1}^{k+1}(1-p(\delta^{\gamma-1}w+e_\gamma^k))V_m^k\} = 0 \end{aligned} \tag{24}$$

$$w \geq 0 \ and \ e_t^k \geq 0, \ for \ t \in [1, ..., k+1] \tag{25}$$

[10]From section 5 onwards, I use one-stopgap-period strategy as an example to show a more complete characterization.

Appendix D. Proof of Lemma 5.1

Under the supply-upto-level strategy, the militia's effort choice for a given weapon-in-hand, w, is determined by the following equation:

$$-\frac{c'(e^{SUL})}{p'(w+e^{SUL})}+\frac{(1-\beta_m)M(w)+\beta_m c(e^{SUL})}{1-\beta_m(1-p(w+e^{SUL}))}=0 \tag{26}$$

The proof of first part of Lemma, the effort is decreasing in the militia's patience, β_m, is shown below:

$$\frac{\partial e^{SUL}}{\partial \beta_m}=\frac{\Omega_1}{\{c''(e^{SUL})-p''(w+e^{SUL})\frac{c'(e^{SUL})}{p'(w+e^{SUL})}\}(1-\beta_m(1-p(w+e^{SUL})))}<0 \tag{27}$$

$$\begin{aligned}\Omega_1=-p'(w+e^{SUL})(p(w+e^{SUL})M(w)-c(e^{SUL}))/1-\beta_m(1-p(w+e^{SUL}))=\\ -\frac{1}{\beta_m}(p'(w+e^{SUL})M(w)-c'(e^{SUL}))<0\end{aligned} \tag{28}$$

The proof of second part of Lemma, the effort is increasing in weapon holding, $\bar{w}$, is shown below,

$$\frac{\partial e^{SUL}}{\partial w}=\frac{\Omega_2}{\{c''(e^{SUL})-p''(w+e^{SUL})\frac{c'(e^{SUL})}{p'(w+e^{SUL})}\}(1-\beta_m(1-p(w+e^{SUL})))} \tag{29}$$

$$\Omega_2=p'(w+e^{SUL})\{(1-\beta_m)M-\beta_m c'(e^{SUL})\}+p''(w+e^{SUL})\{(1-\beta_m)M(w)+\beta_m c(e^{SUL})\} \tag{30}$$

Since $p(w+e^{SUL})$ is linear, the term $p''(w+e^{SUL})=0$. The Ω_2 can be rearranged into $p\{(1-\beta_m)M-\beta_m c e^{SUL}\}$ which is positive because $e^{SUL}<\frac{(1-\beta_m)M}{\beta_m}$.
The proof of third part of Lemma, the effort is increasing in gain from holding weapons after the war, M, is shown below.

$$\frac{\partial e^{SUL}}{\partial M}=\frac{\Omega_3}{\{c''(e^{SUL})-p''(w+e^{SUL})\frac{c'(e^{SUL})}{p'(w+e^{SUL})}\}(1-\beta_m(1-p(w+e^{SUL})))}>0 \tag{31}$$

$$\Omega_3=(1-\beta_m)wp'(w+e^{SUL}) \tag{32}$$

Finally, the proof of last part of Lemma, the effort does not change with depreciation rate, δ, is trivial since the δ does not enter the militia's optimization problem under the supply-upto-level equilibrium.

Appendix E. Proof of Lemma 5.2

In this proof, I show that $\frac{\partial V_m^{SUL}}{\partial w} > 0$.

$$\frac{\partial V_m^{SUL}}{\partial w} = \frac{\Omega}{(1-\beta_m(1-p(w+e)))^2} \tag{33}$$

$$\Omega = p'(w+e)(1+\frac{\partial e}{\partial w})\{(1-\beta_m)M(w)+\beta_m c(e^{SUL})\}+\{p(w+e)M-c'(e)\frac{\partial e}{\partial w}\}(1-\beta_m(1-p(w+e))) \tag{34}$$

Substituting in equation 26 and gives

$$= p'(w+e)(1+\frac{\partial e}{\partial w})\{\frac{c'(e)}{p'(w+e)}(1-\beta_m(1-p(w+e)))\}+\{p(w+e)M-c'(e)\frac{\partial e}{\partial w}\}(1-\beta_m(1-p(w+e))) \tag{35}$$

$$= \{c'(e)+p(w+e)M\}(1-\beta_m(1-p(w+e))) > 0 \tag{36}$$

This result suggests that the more weapons are supplied to the militia, the higher the utility that stays with the militia. Intuitively since the weapon serves as a resource for both fighting the war and to gain, more weapons given relaxed the constraints the militia has and thus should generate more values to the milita. I also apply the same intuition for later proofs.

Appendix F. Proof of Lemma 5.3

In this section, I characterize the effort choices under the one-stopgap-strategy. Let $k = 1$ and in the equilibrium there are two different level of efforts: e_1^1 and e_2^1. These efforts are jointly determined by the following equations for a given w:

$$-\frac{c'(e_1^1)}{p'(w+e_1^1)}+\frac{M(w)-\beta_m^2(1-p(\delta w+e_2^1))\{-c(e_1^1)+M(w)\}-\beta_m\{-c(e_2^1)+p(\delta w+e_2^1)M(\delta w)\}}{1-\beta_m^2(1-p(\delta w+e_2^1))(1-p(w+e_1^1))} = 0 \tag{37}$$

$$-\frac{c'(e_2^1)}{p'(\delta w+e_2^1)}+\frac{M(\delta w)-\beta_m^2(1-p(w+e_1^1))\{-c(e_2^1)+M(\delta w)\}-\beta_m\{-c(e_1^1)+p(w+e_1^1)M(w)\}}{1-\beta_m^2(1-p(\delta w+e_2^1))(1-p(w+e_1^1))} = 0 \tag{38}$$

Equation (37) is the first-order condition of e_1^1 for the militia's utility function. Equation (38) is the first-order condition of e_2^1 for the militia's utility function. In particular, when δ is sufficiently small, the second term of (38) can be negative and increasing in δ. In this case, I can conclude that $e_2^1 = 0$ when $\delta < \bar{\delta} = \delta(\beta_m|w)$. Now I start by showing the proof

of the first part of Lemma, both effort choices are decreasing in the militia's patience, β_m.

$$\frac{\partial e_1^1}{\partial \beta_m} = \frac{\Omega_1}{\{c''(e_1^1) - p''(w+e_1^1)\frac{c'(e_1^1)}{p'(w+e_1^1)}\}(1-\beta_m^2(1-p(w+e_1^1))(1-p(\delta w+e_2^1)))} < 0 \quad (39)$$

$\Omega_1 = -p'(w+e_1^1)\{2\beta_m(1-p(\delta w+e_2^1))[p(w+e_1^1)M(w)-c(e_1^1)]+[1+\beta_m^2(1-p(w+e_1^1))(1-p(\delta w+e_2^1))](p(\delta w+e_2^1)M(\delta w)-c(e_2^1))\}/(1-\beta_m^2(1-p(w+e_1^1))(1-p(\delta w+e_2^1)))$
$= -\frac{1}{\beta_m}(p'(w+e_1^1)M(w)-c'(e_1^1)) - p'(w+e_1^1)\beta_m(1-p(\delta w+e_2^1))V_m^{TS} < 0$

$$\frac{\partial e_2^1}{\partial \beta_m} = \frac{\Omega_2}{\{c''(e_2^1) - p''(\delta w+e_2^1)\frac{c'(e_2^1)}{p'(\delta w+e_2^1)}\}(1-\beta_m^2(1-p(w+e_1^1))(1-p(\delta w+e_2^1)))} < 0 \quad (40)$$

$\Omega_2 = -p'(\delta w+e_2^1)\{2\beta_m(1-p(w+e_1^1))[p(\delta w+e_2^1)M(\delta w)-c(e_2^1)]+[1+\beta_m^2(1-p(w+e_1^1))(1-p(\delta w+e_2^1))](p(w+e_1^1)M(w)-c(e_1^1))\}/(1-\beta_m^2(1-p(w+e_1^1))(1-p(\delta w+e_2^1)))$
$= -\frac{1}{\beta_m}(p'(\delta w+e_2^1)M(\delta w)-c'(e_2^1)) - p'(\delta w+e_2^1)(V_m^{TS}-(p(w+e_1^1)M(w)-c(e_1^1)) < 0$

The proof of second part of Lemma, both effort choices are increasing in gain from holding weapons after the war, M, is shown below.

$$\frac{\partial e_1^1}{\partial M} = \frac{w(1-\beta_m)(1+\beta_m(1-p(\delta w+e_2^1)))+(1-\delta)\beta_m p(\delta w+e_2^1)}{\{c''(e_1^1) - p''(w+e_1^1)\frac{c'(e_1^1)}{p'(w+e_1^1)}\}(1-\beta_m^2(1-p(w+e_1^1))(1-p(\delta w+e_2^1)))} > 0 \quad (41)$$

$$\frac{\partial e_2^1}{\partial M} = \frac{\delta w(1-\beta_m)(1+\beta_m(1-p(w+e_1^1)))+\delta\beta_m p(w+e_1^1)}{\{c''(e_1^1) - p''(w+e_1^1)\frac{c'(e_1^1)}{p'(w+e_1^1)}\}(1-\beta_m^2(1-p(w+e_1^1))(1-p(\delta w+e_2^1)))} > 0 \quad (42)$$

The proof of third part of Lemma, the effort e_1^1 decreases in the depreciation factor, δ, is shown below. If $e_2^1 > 0$, then

$$\frac{\partial e_1^1}{\partial \delta} = \frac{-w\beta_m p'(w+e_1^1)\{p(\delta w+e_2^1)M'(\delta w)+c'(e_2^1)\}}{\{c''(e_1^1) - p''(w+e_1^1)\frac{c'(e_1^1)}{p'(w+e_1^1)}\}(1-\beta_m^2(1-p(w+e_1^1))(1-p(\delta w+e_2^1)))} < 0 \quad (43)$$

Otherwise, if $e_2^1 = 0$, then

$$\frac{\partial e_1^1}{\partial \delta} = \frac{\Omega_5}{\{c''(e_1^1) - p''(w+e_1^1)\frac{c'(e_1^1)}{p'(w+e_1^1)}\}(1-\beta_m^2(1-p(w+e_1^1))(1-p(\delta w+e_2^1)))} \tag{44}$$

$\Omega_5 = -wc'(e_1^1)\beta_m^2 p'(\delta w+e_2^1)(1-p(w+e_1^1)) + p'(w+e_1^1)\{w\beta_m^2 p'(\delta w+e_2^1)(M(w)-c(e_1^1)) - w\beta_m(p'(\delta w+e_2^1)M(\delta w)+p(\delta w+e_2^1))M'(\delta w)\}$
$= -wce_1^1\beta_m^2 p(1-p(w+e_1^1)) + p\{w\beta_m^2 p(M(w)-c(e_1^1)) - w\beta_m(pM(\delta w)+p(\delta w))M\}$

The Ω_5 is decreasing in δ and when δ is sufficiently small, $\Omega_5 > 0$. The proof of last part of Lemma, the effort e_1^2 increases in the depreciation factor, δ, when $\delta \geq \delta(\beta_m|w)$ is shown below.

$$\frac{\partial e_2^1}{\partial \delta} = \frac{\Omega_6}{\{c''(e_2^1) - p''(\delta w+e_2^1)\frac{c'(e_2^1)}{p'(\delta w+e_2^1)}\}(1-\beta_m^2(1-p(w+e_1^1))(1-p(\delta w+e_2^1)))} \geq 0 \tag{45}$$

$\Omega_6 = wp''(\delta w+e_2^1)\frac{c'(e_2^1)}{p'(\delta w+e_2^1)}(1-\beta_m^2(1-p(w+e_1^1))(1-p(\delta w+e_2^1))) + wp'(\delta w+e_2^1)\{(1-\beta_m^2(1-p(w+e_1^1)))M'(\delta w) - \beta_m^2(1-p(w+e_1^1))c'(e_2^1)\} = wp\{(1-\beta_m^2(1-p(w+e_1^1)))M - \beta_m^2(1-p(w+e_1^1))ce_2^1\} > 0$

Appendix G. Proof of Lemma 5.4

In this section, I first show that $e_1^1 > e^{SUL}$ and then followed by $e_2^1 < e^{SUL}$. To prove that $e_1^1 > e^{SUL}$, I compare the first-order condition for both effort choices, the equation 37 and 26. I do this by first assuming that $e_1^1 = e^{SUL}$ and study the sign of equation 37. If the sign is positive, then

$$\begin{aligned} &-\frac{(1-\beta_m)M(w)+\beta_m c(e^{SUL})}{1-\beta_m(1-p(w+e^{SUL}))} \\ &+\frac{M(w)-\beta_m^2(1-p(\delta w+e_2^1))[-c(e^{SUL})+M(w)]-\beta_m[-c(e_2^1)+p(\delta w+e_2^1)M(\delta w)]}{1-\beta_m^2(1-p(\delta w+e_2^1))(1-p(w+e^{SUL}))} \end{aligned} \tag{46}$$

which is determined by

$$\frac{p(w+e^{SUL})M(w)-c(e^{SUL})}{1-\beta_m(1-p(w+e^{SUL}))} - \frac{p(\delta w+e_2^1)M(\delta w)-c(e_2^1)}{1-\beta_m(1-p(\delta w+e_2^1))} > \tag{47}$$

$$\frac{p(\delta w + e^{SUL}(\delta w))M(\delta w) - c(e^{SUL}(\delta w))}{1-\beta_m(1-p(w+e^{SUL}(\delta w)))} - \frac{p(\delta w + e_2^1)M(\delta w) - c(e_2^1)}{1-\beta_m(1-p(\delta w + e_2^1))} > 0 \tag{48}$$

Lemma 5.2 suggests that (47) is greater than (48) because more weapon-in-hands gives a greater utility to the militia. This result implies that $e_1^1 > e^{SUL}$. Next I turn to compare e_2^1 and e^{SUL} which is determined below:

$$p'(\delta w + e^{SUL})\{m\delta w - \beta_m V_m^{SUL}(\delta w)\} - c'(e^{SUL}) = 0 \tag{49}$$

$$p'(\delta w + e_2^1)\{m\delta w - \beta_m V_m^{TS1}\} - c'(e_2^1) = 0 \tag{50}$$

The continuation value of period without weapon supply, V_m^1, is greater than the continuation value of supply-upto-level strategy, V_m^{SUL}. This is because the weapon-in-hand for the first case is greater or at least the same as those in the later. This suggest $e_2^1 \leq e^{SUL}(\delta w) \leq e^{SUL}(w)$.

Appendix H. Proof of Lemma 5.5

In this section, I show how the difference in effort choices change with the militia's patience, β_m, and the weapon depreciation factor, δ. Below I show that the effort differences are all increasing in β_m.

$$\frac{\partial e_1^1}{\partial \beta_m} - \frac{\partial e^{SUL}}{\partial \beta_m} = \frac{\Omega_1}{c\{(1-\beta_m^2(1-p(w+e_1^1))(1-p(\delta w+e_2^1))\}\{1-\beta_m(1-p(w+e^{SUL}))\}} > 0 \tag{51}$$

$\Omega_1 = \beta_m^2(1-p(\delta w+e_2^1))\{(1-p(w+e^{SUL}))(pM\delta w - ce_2^1) - (1-p(w+e_1^1))(pMw - ce_1^1)\}$
$+\beta_m\{(1-p(w+e^{SUL}))(pMw - ce_1^1) - (1-p(\delta w+e_2^1))(pM\delta w - ce_2^1)\}$
$+(pMw - ce^{SUL}) - (pMw - ce_1^1)$ is convex in β_m.

$$\frac{\partial e_2^1}{\partial \beta_m} - \frac{\partial e^{SUL}}{\partial \beta_m} = \frac{\Omega_2}{c\{(1-\beta_m^2(1-p(w+e_1^1))(1-p(\delta w+e_2^1))\}\{1-\beta_m(1-p(w+e^{SUL}))\}} > 0 \tag{52}$$

$\Omega_2 = \beta_m^2(1-p(w+e_1^1))\{(1-p(w+e^{SUL}))(pMw - ce_1^1) - (1-p(\delta w+e_2^1))(pMw - ce^{SUL})\}$
$+\beta_m\{(1-p(w+e^{SUL}))(pM\delta w - ce_2^1) - (1-p(w+e_1^1))(pMw - ce_1^1)\}$
$+(pMw - ce^{SUL}) - (pM\delta w - ce_2^1)$ is convex in β_m.

Appendix I. Proof of Lemma 5.6

In this section, I show how the effort differences change with δ. Lemma 5.1 suggests $\frac{\partial e^{SUL}}{\partial \delta} = 0$. Therefore, we have the following:

$$\frac{\partial e_1^1}{\partial \delta} - \frac{\partial e^{SUL}}{\partial \delta} = \frac{\partial e_1^1}{\partial \delta} \tag{53}$$

$$\frac{\partial e_2^1}{\partial \delta} - \frac{\partial e^{SUL}}{\partial \delta} = \frac{\partial e_2^1}{\partial \delta} \tag{54}$$

The results should follow those in Lemma 5.3.

Appendix J. Proof of Proposition 4

In this section, I compare the government's welfare between supply-upto-level and 1-period stopgap strategy by first establishing a sufficient condition as follows.

$V_g^{TS}(w^{TS}) - V_g^{SUL}(w^{SUL}) > V_g^{TS}(w^{SUL}) - V_g^{SUL}(w^{SUL}) =$

$$\frac{p(w+e_1^1)(\Delta^g - G(w)) + \beta_g(1-p(\delta w+e_1^1))p(\delta w+e_2^1)(\Delta^g - G(\delta w))}{1-\beta_g^2(1-p(\delta w+e_2^1))(1-p(w+e_1^1))} - \frac{p(w+e^{SUL}(w))(\Delta^g - G(w))}{1-\beta_g(1-p(w+e^{SUL}(w)))} \tag{55}$$

$$= \frac{\Omega}{\{1-\beta_g^2(1-p(\delta w+e_2^1))(1-p(w+e_1^1))\}\{1-\beta_g(1-p(w+e^{SUL}(w)))\}} \tag{56}$$

$$\begin{aligned}\Omega = &\{p(w+e_1^1) - p(w+e^{SUL})\}(\Delta^g - G(w)) \\ &+ \beta_g\{(1-p(w+e_1^1))p(\delta w+e_2^1)(\Delta^g - G(\delta w)) - (1-p(w+e^{SUL}))p(w+e_1^1)(\Delta^g - G(w)\} \\ &+ \beta_g^2(1-p(w+e_1^1))\{(1-p(\delta w+e_2^1))p(w+e^{SUL})(\Delta^g - G(w)) - (1-p(w+e^{SUL}))p(\delta w+e_2^1)(\Delta^g - G(\end{aligned} \tag{57}$$

Since the denominator is positive, this sign is determined by Ω and it is quadratic in β_g. Furthermore, Ω is positive when $\beta_g = 1$ and $\beta_g = 0$. I check the coefficient of β_g^2 which determines the convexity or concavity of Ω. I find that if G is large, Ω is concave and is always positive. If G is sufficiently small, Ω is convex in β_g. The next step is to check if Ω ever goes below 0. To do so I first let $\Omega_2 =$

$$\begin{aligned}&\{(1-p(w+e_1^1))p(\delta w+e_2^1)(\Delta^g - G(\delta w)) - (1-p(w+e^{SUL}))p(w+e_1^1)(\Delta^g - G(w)\}^2 \\ &-4\{p(w+e_1^1) - p(w+e^{SUL})\}(\Delta^g - G(w))\}(1-p(w+e_1^1))\{(1-p(\delta w+e_2^1))p(w+e^{SUL})(\Delta^g -\end{aligned}$$

$G(w)) - (1 - p(w + e^{SUL}))p(\delta w + e_2^1)(\Delta^g - G(\delta w))\}$

Ω_2 is consisted of the square of the coefficient for β_g minus 4 times the coefficient of β_g^2 and the constant term. If $\Omega_2 < 0$, then the function is always larger than 0 and $V_g^{TS} > V_g^{SUL}$. If $\Omega_2 > 0$, then $V_g^{TS} > V_g^{SUL}$ when β_g is small or when β_g is large. To show the sign of Ω_2, first let $\Theta_1 = p(w + e_1^1) - p(w + e^{SUL})$ and $\Theta_2 = p(\delta w + e_2^1) - p(w + e^{SUL})$. I find that the Ω_2 can be rearranged as follows:

$$(\Delta^g - G(w))\Big\{\{(\Theta_2(1 - p(w + e_1^1) + \Theta_1)) + (1 - p(w + e_1^1))p(\delta w + e_2^1)\frac{G(1-\delta)w}{\Delta^g - Gw}\}^2$$
$$-4\Theta_1 p(w + e^{SUL})(1 - p(w + e_1^1))p(\delta w + e_2^1)\frac{G(1-\delta)w}{\Delta^g - Gw}\Big\}$$

The first term, $(\Delta^g - G(w))$ is decreasing in G. For the second term, the first one is also decreasing in G since the $\Theta_2(1 - p(w + e_1^1) + \Theta_1)$ is negative and $(1 - p(w + e_1^1))p(\delta w + e_2^1)\frac{G(1-\delta)w}{\Delta^g - Gw}$ is increasing G. A larger G will lower the difference. The second one is $-4\Theta_1 p(w+e^{SUL})(1-p(w+e_1^1))p(\delta w+e_2^1)\frac{G(1-\delta)w}{\Delta^g - Gw}$ which is also decreasing in G. Therefore Ω_2 is decreasing in G and it can go below zero. As a result is sets an upper bound, $\bar{G}$, for G. Whenever G is greater than $\bar{G}$, Ω_2 is negative. This suggests large enough G would make temporary stopgap strategy always superior to supply-upto-level strategy.

Appendix K. Proof of Proposition 5

In this section, I show that the $V_g^{TS1} > V_g^{SUL}$ when β_g is sufficiently small. Following previous section, I show that the comparison is determined by the sign of Ω, Equation (57). As previously discussed when G is small, Equation (57) is convex in β_m. Furthermore if $G < \bar{G}$, $\bar{\beta_g}^L > 0$ and $\bar{\beta_g}^U < 1$ exist and $V_g^{TS1} > V_g^{SUL}$ when $\beta_g < \bar{\beta_g}^L$ or $\beta_g > \bar{\beta_g}^U$. However, given that governments are generally not patient, in this paper I focus on discussing the impatient governments.

Appendix L. Proof of Proposition 6

In this section, I show that the $V_g^{TS1} > V_g^{SUL}$ when β_m is sufficiently large. In order to compare the government's welfare, I take the difference of the government's utility:

$$V_g^{TS}(w^{TS}) - V_g^{SUL}(w^{SUL}) > V_g^{TS}(w^{SUL}) - V_g^{SUL}(w^{SUL}) > \bar{V}_g^{TS}(w^{SUL}) - V_g^{SUL}(w^{SUL}) =$$

$$\frac{p(w+e_1^1)(\Delta^g - G(w)) + \beta_g(1-p(\delta w+e_1^1))p(\delta w+e_2^1)(\Delta^g - G(w))}{1-\beta_g^2(1-p(\delta w+e_2^1))(1-p(w+e_1^1))} - \frac{p(w+e^{SUL}(w))(\Delta^g - G(w))}{1-\beta_g(1-p(w+e^{SUL}(w)))} \tag{58}$$

$$= \frac{\Omega}{\{1-\beta_g^2(1-p(\delta w+e_2^1))(1-p(w+e_1^1))\}\{1-\beta_g(1-p(w+e^{SUL}(w)))\}} \tag{59}$$

$$\begin{aligned}\Omega = {} & (\Delta^g - G(w))\{p(w+e_1^1) - p(w+e^{SUL})\} \\ & + \beta_g\{(1-p(w+e_1^1))p(\delta w+e_2^1) - (1-p(w+e^{SUL}))p(w+e_1^1)\} \\ & + \beta_g^2(1-p(w+e_1^1))\{(1-p(\delta w+e_2^1))p(w+e^{SUL}) - (1-p(w+e^{SUL}))p(\delta w+e_2^1)\}\end{aligned} \tag{60}$$

In equation (58), the first inequality holds since $V_g^{TS}(w^{TS}) > V_g^{TS}(w^{SUL})$. The second inequality holds because $\bar{V}_g^{TS}(w^{SUL})$ is $V_g^{TS}(w^{SUL})$ but replacing the $G(\delta w)$ with $G(w)$. Therefore if Ω is positive, then $V_g^{TS}(w^{TS}) > V_g^{SUL}(w^{SUL})$.

Equation (60) can be arranged into a quadratic and convex function of β_g and therefore one-period-stopgap equilibrium is associated with a higher government welfare comparing with those associated with supply-upto-level equilibrium if the following condition holds:

$$\beta_g < \bar{\beta}_g = \frac{p(w+e_1^1) - p(w+e^{SUL}(w))}{\{p(w+e^{SUL}(w)) - p(\delta w+e_2^1)\}(1-p(w+e_1^1))} \tag{61}$$

This result suggests that one-period-stopgap equilibrium is preferred if:

1. the government is less patient, a small β_g, which aligns with Proposition 5.
2. the militia exerts more effort during the weapon supply period than the effort choice under the supply-upto-level strategy and the effort choice during the stopgap periods is also sufficiently large.

The later one can be achieved through conditions on β_m and δ. Here I focus on the condition on β_m while the condition on δ is shown in the next section. To do so, I examine how $\bar{\beta}_g$ change with a larger β_m. If $\bar{\beta}_g$ is increasing in β_m, then when the β_m is sufficiently large so that $\beta_g < \beta_g^L$. This suggests $V_g^{TS1} > V_g^{SUL}$. To do so, I take the derivative over β_m.

$$\frac{\partial \bar{\beta}_g}{\partial \beta_m} = \frac{\Omega_1}{(1-p(w+e^{SUL}))(p(w+e^{SUL}) - p(w+e_2^1)))} \tag{62}$$

$$\Omega_1 = p\{p(w+e^{SUL}) - p(\delta w + e_2^1)\}\{(1-p(w+e^{SUL}))\frac{\partial e_1^1}{\partial \beta_m} - (1-p(w+e_1^1))\frac{\partial e^{SUL}}{\partial \beta_m}\}$$
$$- p(1-p(w+e_1^1))\{p(w+e_1^1) - p(w+e^{SUL})\}(\frac{\partial e^{SUL}}{\partial \beta_m} - \frac{\partial e_2^1}{\partial \beta_m}) > 0 \tag{63}$$

The first term of Ω_1 is positive since $\{(1-p(w+e^{SUL}))\frac{\partial e_1^1}{\partial \beta_m} - (1-p(w+e_1^1))\frac{\partial e^{SUL}}{\partial \beta_m}\}$ is greater than $(\frac{\partial e_1^1}{\partial \beta_m} - \frac{\partial e^{SUL}}{\partial \beta_m})$ which is positive shown in Lemma 5.5. The second term of Ω_1 is positive since $(\frac{\partial e^{SUL}}{\partial \beta_m} - \frac{\partial e_2^1}{\partial \beta_m})$ is negative shown in Lemma 5.5. Therefore, when $\beta_m > \beta_m(\beta_g, \delta|w)$, $V_g^{TS1} > V_g^{SUL}$.

Appendix M. Proof of Proposition 7

In this section, I show that $V_g^{TS1} > V_g^{SUL}$ when $\delta^L(\beta_m, \beta_g|w) < \delta < \delta^U(\beta_m, \beta_g|w)$. As shown in the Appendix section L, the sufficient condition, $\beta_g < \bar{\beta}_g$, implies that $V_g^{TS1} > V_g^{SUL}$, where

$$\bar{\beta}_g = \frac{p(w+e_1^1) - p(w+e^{SUL}(w))}{\{p(w+e^{SUL}(w)) - p(\delta w + e_2^1)\}(1-p(w+e_1^1))} \tag{64}$$

In order to prove the proposition that $\delta^L(\beta_m, \beta_g|w) < \delta < \delta^U(\beta_m, \beta_g|w)$, I show that the $\bar{\beta}_g$ has a concave relationship with δ.

$$\frac{\partial \bar{\beta}_g}{\partial \delta} = \frac{\Omega_1}{(p(w+e^{SUL}) - p(\delta w + e_2^1))^2(1-p(w+e_1^1))^2} \tag{65}$$

$$\Omega_1 = p'(w+e_1^1)\frac{\partial e_1^1}{\partial \delta}(1-p(w+e^{SUL}))(p(w+e^{SUL}) - p(\delta w + e_2^1))$$
$$+p'(\delta w + e_2^1)(w + \frac{\partial e_2^1}{\partial \delta})(1-p(w+e_1^1))(p(w+e_1^1) - p(w+e^{SUL}))$$

Given Lemma 5.3, when δ is small, both $\frac{\partial e_1^1}{\partial \delta}$ and $\frac{\partial e_2^1}{\partial \delta}$ are positive. Therefore Ω_1 is positive and so is $\frac{\partial \bar{\beta}_g}{\partial \delta}$. This suggests that there is $> 1\delta^L > 0$ such that $V_g^{TS1} > V_g^{SUL}$ when $\delta^L(\beta_m, \beta_g|w) < \delta$. On another hand, given Lemma 5.3 when δ is large, $\frac{\partial e_1^1}{\partial \delta}$ is negative and $\frac{\partial e_2^1}{\partial \delta}$ is positive. But both $\frac{\partial e_1^1}{\partial \delta}$ and $\frac{\partial e_2^1}{\partial \delta}$ are both decreasing in δ (second-order condition) and therefore $\frac{\partial \bar{\beta}_g}{\partial \delta}$ is decreasing in δ. Therefore, there exists $0 < \delta^U < 1$ such that the

$V_g^{TS1} > V_g^{SUL}$ when $\delta < \delta^U(\beta_m, \beta_g|w)$.