Ludwig-Maximilians-Universität München

NATIONAL DEFENSE, ECONOMIC GROWTH AND THE ROLE OF POLITICAL INSTITUTIONS

INAUGURAL-DISSERTATION

zur Erlangung des Grades Doctor oeconomiae publicae

an der Ludwig-Maximilians-Universität München

vorgelegt von

Johannes Hermann Eberhard Blum

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1. Introduction

National defense and economic growth are pivotal objects of investigation for scholars in the field of public economics. First, the capability to defend national sovereignty belongs to the main constitutional responsibilities and obligations of the nation state. National defense is a public good provided by the government. A wide range of needs and preferences among human beings (one of them being security), however, must be reconciled with restrictions in resources and budgets. This is where the fields of political economy and public finance interact. Second, economic growth reflects the rise of the common welfare of a society and the amount of resources that can be redistributed within a society. Understanding economic growth is a key challenge for scholars and among many other determinants, scholars have shown that political institutions influence economic growth.

This dissertation contributes to the literature on defense economics, economic growth and the role of political institutions. I examine how political institutions shape defense spending, how supply and demand for arms are related and how political instability influences economic growth. Specifically, my dissertation consists of four self-contained research papers, each of which is included as an individual chapter: **Chapter 2** examines the role of government changes for compliance with the NATO two percent spending target, **Chapter 3** examines how democratic transitions in the context of democracy's third wave have influenced national defense spending, **Chapter 4** investigates the relationship between national arms production, national defense spending and arms trade, and **Chapter 5** examines the effect of political instability on economic growth and uses coups d'état as one aspect of political instability.

The end of the Cold War did not mark the end of armed conflicts and the need for armed forces, which is the starting point for **Chapter 2**. Figure 1.1 shows world defense spending from 1988 to 2018 by region.[1] After the arms race between the United States and the Soviet

[1] Data is provided by the SIPRI Military Expenditure Database.

Union during the 1980s, world defense spending remarkably decreased after the Cold War until the late 1990s. The terrorist attacks of 9/11 unveiled new security threats through international terrorism and both the Georgia war in 2008 and the annexation of the Crimean peninsula in March 2014 unveiled Russia's striving for being perceived as a world power. The US campaigns in Afghanistan and Iraq starting 2001 and 2003 considerably increased the US defense budget while the defense budget of European NATO allies remained at low post-Cold War levels.[2] In Asia and in the Middle East, defense spending steadily increased from 1988 to 2018. The strong expansion in defense spending for Asia is attributed to the rise of China, which aims to increase its spheres of military influence particularly in the East and South China Sea. China accounted for almost half of the total defense spending in Asia in 2018. Other Asian countries like India and Pakistan, however, also considerably increased their defense budgets

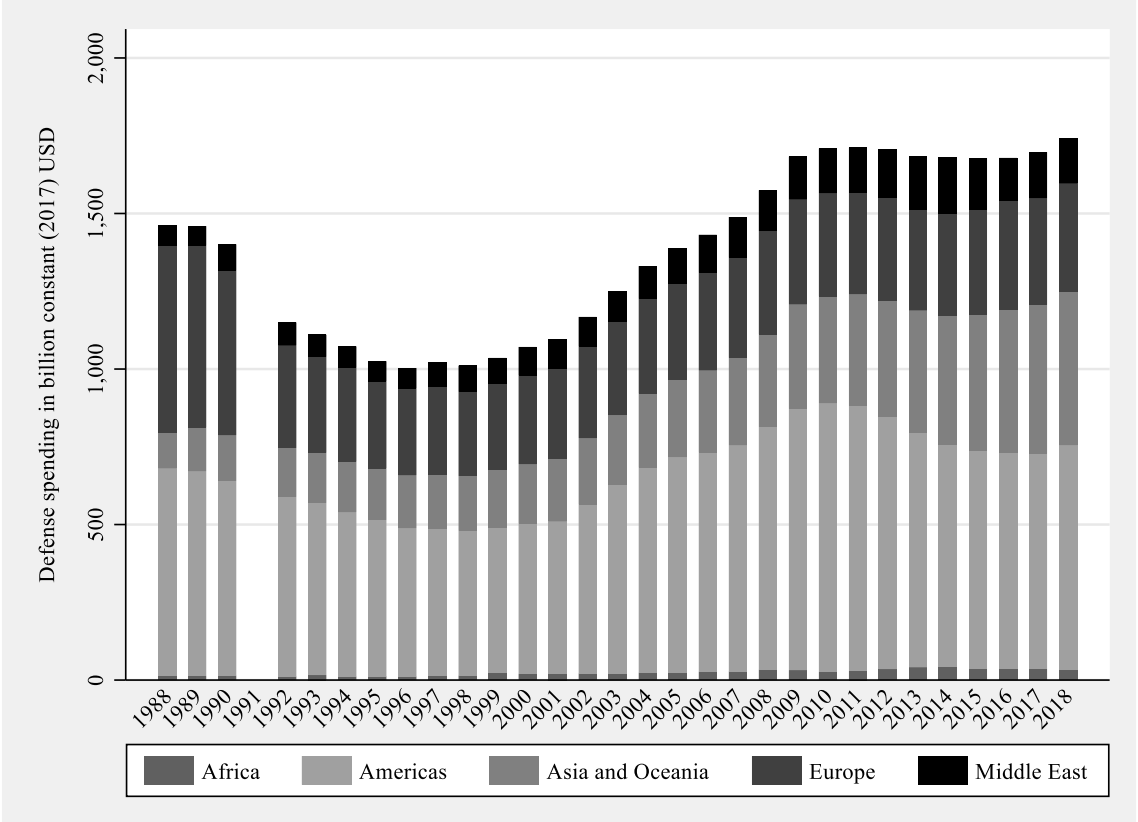

FIGURE 1.1: WORLD DEFENSE SPENDING 1988-2018[3]

[2] The United States accounted for around 90% of the total defense spending in North, Middle and South America during the depicted period.

[3] Figure 1.1 follows an illustration by SIPRI (https://www.sipri.org/media/press-release/2019/world-military-expenditure-grows-18-trillion-2018, accessed January 14, 2020). No totals for years prior to 1988 and for the year 1991 are shown because of missing data for the Soviet Union.

from the late 1990s onwards. Worldwide defense spending shows that military power—be it for deterrence or war—plays a larger role than expected after the fall of the Iron Curtain. European NATO countries, however, only dedicated around 1.5% of their GDP to national defense in the past decade, while the United States spent a GDP share of between 4.4% (2012) and 3.4% (2019) for national defense.

There is a long-lasting debate within NATO about burden sharing and free-riding within the alliance since NATO countries contribute to collective security which is nonrival within NATO and no NATO country can be excluded from it (Olson and Zeckhauser 1966). A spending target of 2% defense spending relative to GDP has been discussed since the early 2000s to counteract free-riding. At the NATO summit in Wales in September 2014, NATO countries with defense spending below 2% relative to GDP committed themselves to no longer decrease defense spending and to reach the two percent target by 2024. Countries, however, face a time inconsistency problem when international agreements are signed. A government which does no longer endorse the international agreement is inclined to no longer comply with it. Non-compliance is, thus, more likely when the government has changed after an agreement has been signed. The two percent target allows examining this commitment problem in the field of defense spending.

Chapter 2 is joint work with Niklas Potrafke (Blum and Potrafke 2019). We examine whether changes of government influence compliance with international agreements by means of the NATO two percent target. The empirical analysis covers the period 2010-2018 for 27 NATO countries. Countries below the two percent spending target show rising growth rates in military expenditure relative to GDP since the NATO summit in 2014. Countries which do not (yet) comply with the two percent target have, however, smaller growth rates in military expenditure relative to GDP after a large change of government (e.g. a change from a rightwing to a leftwing government) following the 2014 NATO summit than countries without such a large change of government. Countries with a large change of government after the NATO

summit are therefore less likely to comply with the two percent target by 2024 as they pursue the spending target with less effort.

The NATO two percent target as of 2014 only allows analyzing a rather short time period of defense spending in NATO countries after the military alliance has been confronted with new challenges in the 21st century. Apart from the power struggle between the United States and the Soviet Union that the fall of the Iron Curtain has ended, it also initiated the democratizations of the former Soviet countries and Soviet satellites in Eastern Europe and marked the peak of democracy's third wave (Huntington 1991a, 1991b), which sets the scene for **Chapter 3**. Democratic transitions in the past two centuries increased the level of political institutions worldwide. Figure 1.2 shows democratic and non-democratic governance according to the dichotomous democracy measure by Bjørnskov and Rode (2019) over the period 1950 to 2018. The second wave of democratizations after the Second World War stagnated during the 1960s, which marked the end of the second wave. The third wave started with the democratization of Portugal in 1974 and with Spain and Greece becoming democracies in the mid-1970s. The third wave particularly gained momentum in Latin America with the democratization of countries like Argentina and Chile in the 1980s and in Eastern Europe in the 1990s after the fall of the Iron Curtain.[4] The end of the Cold War marks the point at which more countries have been governed democratically than autocratically. In 2018, almost two thirds of all countries were described as democracies, which is a remarkable increase in the quality of political institutions.

This spread of democracy may have lowered national defense spending. With regard to government spending, autocratic regimes must ensure the loyalty of the military elite and often rely on the armed forces to preserve the regime (Kimenyi and Mbaku 1995, Bove and Brauner 2016, Geddes et al. 2018). In democracies, in turn, civilian public goods spending may crowd

[4] Democratizations in selected African and Asian-Pacific countries also belong to this third wave.

out defense spending to ensure the electorate's support (Dudley and Montmarquette 1981, Blum 2018). Political institutions are therefore likely to influence government spending and have shown to lower national defense spending (Dunne and Perlo-Freeman 2003, Collier and Hoeffler 2007, Albalate et al. 2012, Blum 2018).[5]

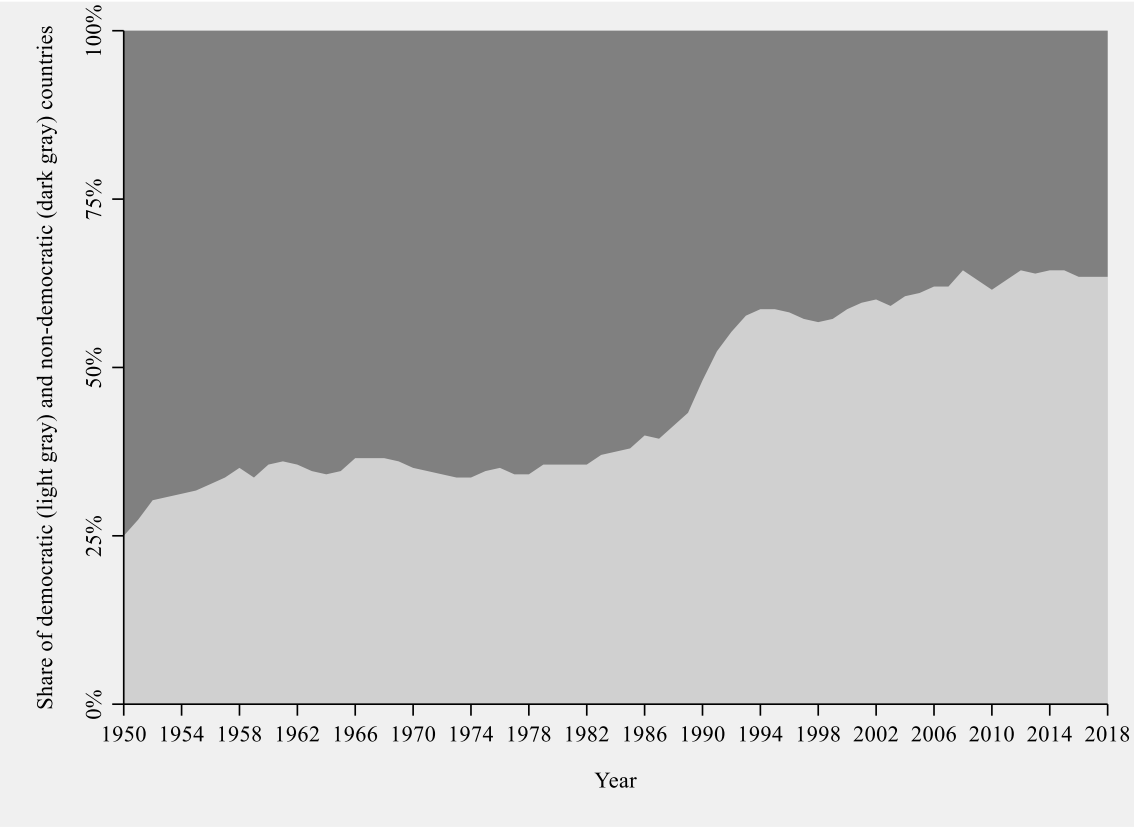

FIGURE 1.2: SHARE OF DEMOCRATIC AND NON-DEMOCRATIC COUNTRIES ACCORDING TO THE BJØRNSKOV AND RODE (2019) DEMOCRACY MEASURE

Chapter 3 investigates how the third wave of democracy influenced national defense spending (Blum 2020). The panel consists of 110 countries over the period 1972-2013. The empirical analysis for this time span is possible in the first place since the Stockholm International Peace Research Institute (SIPRI) has extended its data on military expenditure to years prior to 1988 for a large number of countries. Democracy is difficult to quantify and different democracy measures may give rise to different empirical results. I therefore apply four democracy

[5] Immanuel Kant (1795) argued in his seminal work "Perpetual Peace" that the spread of liberalism may foster peace and lower the relevance of armed forces. According to the Democratic Peace paradigm, political scientists argue that democracies even do not go to war with each other (Doyle 1983a, 1983b, Maoz and Russett 1993, Russett and O'Neal 2001).

measures in the empirical analysis. Estimation results provided by a dynamic panel data model suggest that the third wave of democracy decreased national defense spending relative to GDP by about 10% within countries that experienced democratization. Region-specific estimation results do not yield that the impact of democracy's third wave on national defense spending has been heterogeneous across world regions which the third wave reached in different sub-waves. To overcome endogeneity problems, I follow an instrumental variable (IV) approach which exploits the regional diffusion of democracy in the context of the third wave of democratizations. The IV estimates indicate that democratization in the context of democracy's third wave decreased national defense spending relative to GDP by about 20% within countries. OLS results thus underestimate the effect of democratization on defense spending. According to the estimated dynamics in defense spending, the cumulative long-run effect of democratization on national defense spending for both OLS and IV estimates is almost three times higher.[6]

The demand for defense spending has implications for other economic variables such as debt and economic growth (Dunne et al. 2004, Dunne et al. 2005, Alptekin and Levine 2012) as well as it contributes to the perceived threat originating from a country and the likelihood of arms races. The demand for defense spending, moreover, affects arms production, which **Chapter 4** investigates. Figure 1.3 shows arms sales by the top 100 arms-producing and military services companies. 43 of these 100 companies are US companies and the top five companies are all located in the United States.[7] The sales of arms and military services by US companies therefore even exceed the sales by the rest of the world (excluding China). The peak in the late 2000s for arms sales by US companies reflects the high level of US defense spending at that time and the latest increase reflects the risen US defense spending from 2017 to 2018.

[6] The topic of this chapter is related to earlier work I published based on my master's thesis in *Defence and Peace Economics* (Blum 2018), which has not been included in this dissertation. Chapter 3 reaches far beyond my earlier publication with regard to data, empirical strategy and results.
[7] Data is provided by the SIPRI Arms Industry Database. No data is available for Chinese companies.

The steady increase in arms sales in the rest of the world from 2002 to 2018 well corresponds with the increase in world defense spending (see Figure 1.1).

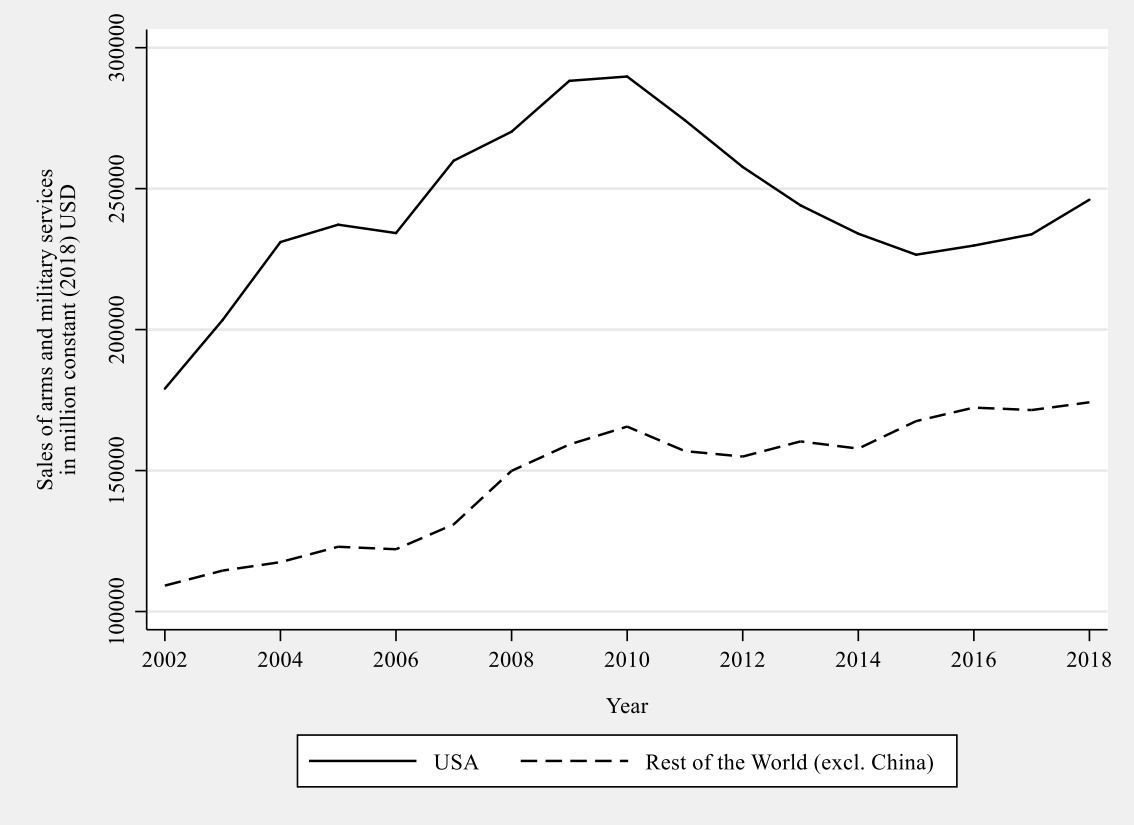

FIGURE 1.3: ARMS SALES BY THE TOP 100 ARMS-PRODUCING AND MILITARY SERVICES COMPANIES

Since arms and military equipment are not entirely sourced from domestic suppliers and the arms industry also strives for economic profits via arms exports, defense spending is also likely to affect the trade in arms. Many studies have estimated demand functions for national defense spending and international arms trade has been investigated by scholars, too. How supply and demand for military goods are related has, however, only been investigated in formal models or on a highly aggregated level yet.

Chapter 4 examines the relationship between arms production, national defense spending and arms trade (Blum 2019). It investigates the relationship between the supply of military goods by arms-producing companies and the demand for military goods by the national government and foreign governments, which involves national defense spending and arms trade. The empirical analysis is based on a panel of up to 195 of the world's largest arms-

producing companies in 21 countries over the period 2002-2016.[8] The results indicate that an increase in the demand for national defense spending by 1% increases the arms sales by a country's largest arms-producing companies by up to 1.2%. An increase in exports of major conventional weapons by 1% increases sales by up to 0.2%. The estimation results moreover show that arms imports do not affect domestic arms sales. This result reflects that imported and domestically produced arms are complements rather than substitutes because countries mainly import arms they do not produce themselves. Country-specific estimation results among countries with strong arms industries like the United States, the United Kingdom, France or Germany suggest that geopolitical conditions and international relations explain whether the arms industry of a country serves economic rather than own security purposes. The results reveal, for instance, that arms-producing companies in the United States primarily serve their own security purposes, i.e. the own armed forces, while arms-producing companies in Germany primarily serve the export market, i.e. the security interest of others.

Output in goods and services on the firm-level provides insights into the relevance and structure of an industry. Output in goods and services by an economy, in turn, reflects the welfare of a society. Scholars have investigated economic growth—i.e. how economic output evolves—for a long time, and **Chapter 5** contributes to its understanding. Figure 1.4 shows percentile ranks according to the "Political Stability and Absence of Violence/Terrorism" index from the World Bank. This index measures the perception on how likely political instability and politically motivated violence are. While the traditional view argues that political stability fosters economic growth (Barro 1991, Alesina et al. 1996), countries that are described as politically rather unstable are among the fastest growing economies (e.g. China, Indonesia and India). It is therefore a worthwhile endeavor to investigate whether stability in political institutions is indeed the breeding ground for economic growth.

[8] At the time this research paper has been written, the SIPRI Arms Industry Database did not yet extend to the year 2018 as in Figure 1.3.

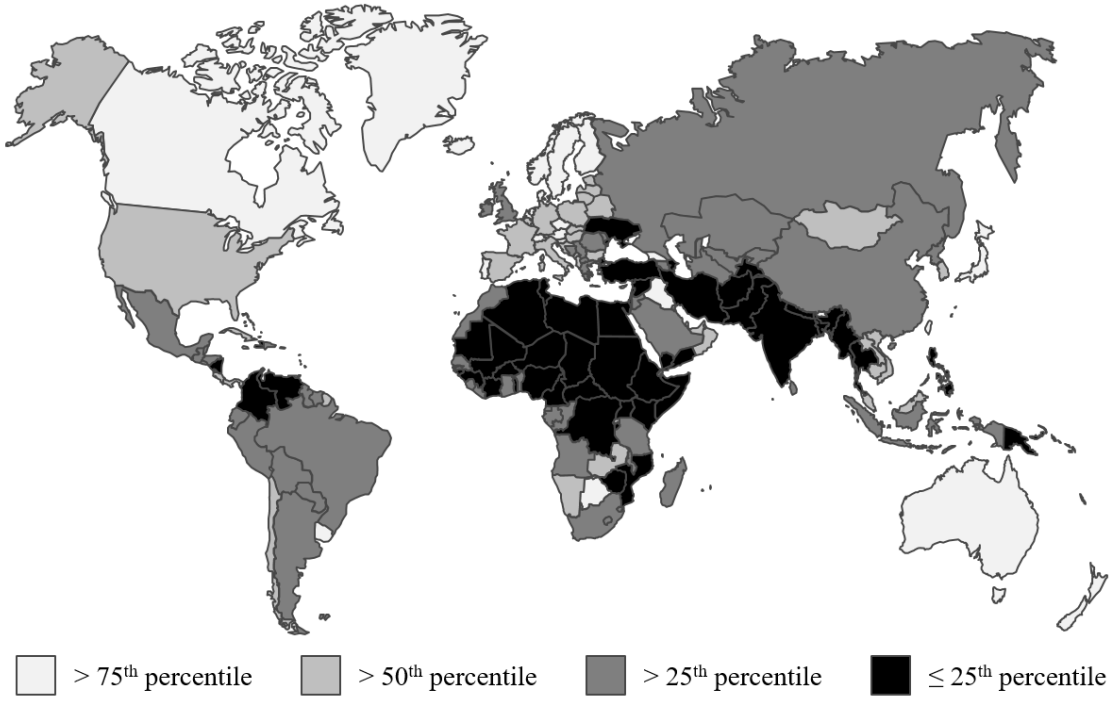

FIGURE 1.4: POLITICAL STABILITY ACCORDING TO THE WORLD BANK'S 'POLITICAL STABILITY AND ABSENCE OF VIOLENCE/TERRORISM' INDEX

Chapter 5 is joint work with Klaus Gründler (Blum and Gründler 2020). We examine how political instability influences economic growth by using coups d'état as a certain aspect of political instability. A novel dataset on coups d'état by Bjørnskov and Rode (2019) allows to cover 180 countries over the period 1950-2017 in the empirical analysis. Since coups are difficult to predict, they help overcoming identification problems when estimating the effect of political instability on growth. Results from panel difference-in-differences and dynamic panel data models indicate that coups have drastic consequences for economic growth and depress economic growth by about 2-3 percentage points. To overcome endogeneity problems, we first present case study results with synthetic control methods. Second, we exploit regional patterns of coup occurrence as an instrumental variable (IV) for coup occurrence. Third, we exploit the geospatial dimension of coups on the sub-national level by geocoding the Bjørnskov and Rode (2019) dataset for about 2,660 sub-national regions. The IV results as well as the estimation results on the sub-national level strongly support the baseline estimation results. With survey data for about 250,000 households in 85 countries, we also examine the effect of coups on the

household level. The micro-level results indicate that coups have adverse effects on employment and worsen the financial situation of households as well as health and life satisfaction of individuals. These adverse effects turn out to be stronger for women and for poorer households.

The four research papers included in this dissertation have either been published in refereed journals (**Chapter 2** and **Chapter 4**) or are in the process of publication at the time this dissertation is finalized (**Chapter 3** and **Chapter 5**). The four chapters are therefore almost identical to the published or latest submitted versions of each research paper. References and appendices to the individual chapters are separately included at the end of each chapter.

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2. Does a Change of Government Influence Compliance with International Agreements? Empirical Evidence for the NATO Two Percent Target

This chapter is joint work with Niklas Potrafke and has been published in *Defence and Peace Economics*.

(Blum, J., and N. Potrafke. 2019. "Does a change of government influence compliance with international agreements? Empirical evidence for the NATO Two Percent Target." *Defence and Peace Economics*, forthcoming.)

Abstract[*]

We examine whether changes of government influence compliance with international agreements. We investigate compliance with the NATO two percent target to which all NATO countries committed themselves during the NATO summit in Wales in 2014. The dataset includes the military expenditure by NATO countries over the period 2010-2018. The results suggest that countries that do not (yet) comply with the two percent target have smaller growth rates in military expenditure relative to GDP when they experienced a large change of government, e.g. a change from a rightwing to a leftwing government, than countries that did not experience such a large change of government since the NATO summit in 2014. Countries that experienced a large change of government are, thus, less likely to comply with the two percent target. Future research should examine the credibility problem of national governments in other international agreements too.

[*] We thank Raul Caruso, Martin Gassebner, Kai A. Konrad, the participants of the Silvaplana Political Economy Workshop 2018, and two anonymous referees for their helpful comments and Lisa Giani-Contini for proofreading.

2.1 Introduction

Countries sign international agreements to commit themselves to policy targets. Prominent examples are climate change agreements. When do countries, however, comply with international agreements? We examine a new aspect of when countries are not likely to comply with international agreements: when national government changes. It is conceivable that new governments are less willing to comply with international agreements signed by a previous government, especially when the new and the proceeding government have different platforms. We use the NATO two percent target to investigate empirically how changes of government influence compliance with an international agreement.

The two percent target has been discussed within NATO since the early 2000s to avoid problems of free-riding when defense burdens are shared. The United States have often criticized NATO allies for free-riding, meaning that it has long been an intriguing issue. The NATO countries agreed on the two percent target at the NATO summit in September 2014. Allies with military expenditure relative to GDP below 2% committed themselves to no longer decrease military expenditure and to reach the two percent target within the next ten years, i.e. by 2024. Decreasing military expenditure relative to GDP therefore means non-compliance with the two percent target. Because the target year is 2024, full compliance with this target cannot be confirmed yet. Countries that aim to meet the two percent target by 2024, however, will not meet the target overnight, but need to increase military expenditure for many years. We focus on efforts to increase military expenditure relative to GDP by those countries that have not met the two percent target yet. Whether countries reduce their efforts to increase military expenditure relative to GDP after government has changed has important implications for compliance with the two percent target in 2024 because many NATO countries are likely to experience at least one change of government within this ten-year period.

New governments are less willing to comply with international agreements signed by a previous government. The reason is a time inconsistency problem from a nation's perspective

related with international agreements. Compliance with international agreements depends on manifold issues such as agreement design, incentives, and cost-effectiveness. Empirical studies have investigated compliance with climate change agreements, anti-pollution standards, international monetary law and human rights treaties.

We use panel data for 27 NATO countries for the period 2010-2018 to examine how a change of government influences the growth rate in military expenditure for those countries that are below the two percent target. We observe rising growth rates in military expenditure for countries below the two percent target after the NATO summit in 2014. Countries that experienced a large change of government, e.g. a change from a rightwing to a leftwing government, after the summit, however, had lower growth rates in military expenditure relative to GDP than countries without such a change. Countries that experienced such a change of government after the NATO summit in 2014 are less likely to comply with the two percent target because they reduce their efforts to reach the target. Future research should investigate how changes of government influence compliance with other international agreements such as climate change and human rights treaties; and how to deal with the credibility problem of national governments when they commit themselves to international agreements.

2.2 Background

2.2.1 Time inconsistency problems and compliance with international agreements

Governments are less likely to comply with international agreements that a previous government has signed. A rational, forward-looking government will adjust policy decisions made in an earlier period because of restrictions the government now faces related to the expectations of optimizing agents (Kydland and Prescott 1977). Policies are likely to be time inconsistent and sticking to the initial policy would not yield an optimal outcome for a nation (see also Barro and Gordon 1983). A government that decides whether to comply with a non-binding agreement such as the NATO two percent target considers decisions of optimizing

agents both in the international (e.g. compliance of other governments) and in the domestic domain (e.g. voting behavior of the electorate). A government that does not agree with the international agreement is therefore inclined not to comply with the two percent target to which the country committed itself years ago. The risk of non-compliance is, thus, higher when government has changed after an agreement has been signed. In case of a change of government, the time inconsistency problem refers to the nation rather than to the policymaker who has been replaced. In any event, policymakers sign international agreements in the name of their nation, rather than in the name of themselves; a new government is equally tied to international agreements as the previous government was.

Scholars have examined compliance in the fields of climate policy, anti-pollution standards, monetary law and human rights treaties. International climate policy faces the same problems of free-riding associated with a public good as the NATO does with defense burden sharing. International climate change agreements attempt to deal with free-riding, but compliance is not certain: the complexity of domestic political processes and the challenge of design and enforcement of agreements in the international domain give rise to commitment problems, which induce governments to time inconsistent policies and non-compliance with earlier commitments (Hovi et al. 2009). Countries are also more likely to comply with climate change agreements the less cost-effective the measures necessary to comply with the agreements are (Barrett and Stavins 2003). Incentives play another important role for participation in and compliance with agreements to avoid free-riding behavior (Barrett and Stavins 2003). It is therefore important to investigate incentives in the context of the two percent target and to infer whether, and to what extent, NATO countries will be expected to comply with an agreement.

The design and acceptance of international treaties influence compliance. Treaties to prevent intentional oil pollution by tankers reveal that compliance with the requirement of rather costly additional equipment for tankers to reduce oil pollution is even higher than

compliance with a less cost-effective limit on oil discharge at sea. These differences in compliance are not substantiated in differences in cost-effectiveness. They are rather substantiated in both the design of such standards, which need to be transparent and verifiable, and in differences in the acceptance of these standards at the time they were introduced (Mitchell 1994).

Peer pressure and reputational concerns are other reasons for countries to comply with international treaties. An empirical study on Article VIII of the IMF's Articles of Agreement, which forbids restrictions on current international transactions, shows that peer pressure among countries located within the same region leads countries to comply with international monetary law. The probability of compliance with Article VIII is even higher in the first years after monetary restrictions have been abandoned since countries try to regain their international reputation (Simmons 2000). Both peer and reputational effects, as well as cost-effectiveness, influence compliance with international laws against human trafficking. Countries favor prevention measures against human trafficking over protection and prosecution measures because prevention satisfies those countries that are most affected by human trafficking inflows on the one hand, and induces less cost and effort on the other (Cho and Vadlamannati 2012). Ratification of international treaties on human rights, however, has not been shown to increase the respect of human rights in a country. An effect of ratification, however, is found for countries the more democratic they are and the stronger their civil society is (Keith 1999, Hathaway 2002, Neumayer 2005).

2.2.2 NATO's two percent target

NATO countries have committed themselves to the two percent target at the NATO summit in September 2014 to counteract free-riding. NATO countries contribute to collective security, a public good within NATO, since it is nonrival and no NATO country can be excluded (Olson and Zeckhauser 1966). Countries are therefore inclined to free-ride at the expense of those allies

which contribute a disproportionally larger share to the collective good.[1] NATO countries discuss defense burden sharing since the early years of NATO, and the United States have often criticized NATO allies for free-riding. NATO members discussed a target of 2% military expenditure relative to GDP as a prerequisite for candidate countries to join NATO at its summit in Prague in November 2002. Because many NATO countries had decreased military expenditure themselves, a target of 2% military expenditure relative to GDP has also been discussed for NATO countries. However, this target was non-binding for candidate countries and for NATO members. The figure of 2% was probably inspired by the level of military expenditure relative to GDP of candidate countries and new members at that time. The 2% figure may also reflect the experience that NATO allies fulfilled NATO obligations at the end of the Cold War when most of them spent at least a share of 2% of their GDP on defense. NATO countries agreed on a target of 2% military expenditure relative to GDP in 2006.[2] At the NATO summit in Riga in November 2006, however, it was not included in the final declaration by the heads of state and government. The NATO summit in Wales in September 2014 was heavily influenced by Russia's actions against Ukraine: Russia's increasingly aggressive behavior and its geopolitical actions in the last decade—including the annexation of Crimea in March 2014—have been a new challenge for NATO, making it important to counteract free-riding within the alliance. All 28 NATO countries at that time attended the NATO summit in September 2014 and the heads of state and government for the first time committed themselves to the two percent target: allies with military expenditure relative to GDP above 2% committed themselves to maintain military expenditure above this level. Allies with military expenditure relative to GDP

[1] By contrast, elements of military expenditure are at least private to some extent and defense burdens among countries have therefore been described as Hicksian complements rather than substitutes (Murdoch and Sandler 1984). Empirical studies arrive at manifold conclusions regarding free-riding within NATO (Murdoch and Sandler 1984, Oneal 1990, Sandler 1993, Hartley and Sandler 1999, Sandler and Murdoch 2000, Plümper and Neumayer 2015, George and Sandler 2018).
[2] See https://www.nato.int/cps/en/natohq/topics_67655.htm (accessed July 2, 2018).

below 2% committed themselves to no longer decrease defense spending and to reach the two percent target within the next ten years, i.e. by 2024.[3]

The official commitment to the two percent target in 2014 is legally not binding and sanctions in case of non-compliance with this agreement have not been defined. The same holds for the 2% target levels discussed at the NATO summits 2002 and 2006. The two percent target is therefore said to be more of a political commitment by the NATO countries.[4]

The two percent target is a quantitative indicator and military expenditure relative to GDP is tracked on a yearly basis. Compliance with this target is thus verifiable and transparent for all NATO countries. The two percent target is, however, often criticized within NATO: it expresses defense spending in terms of GDP, which implies that compliance with this target also depends on business cycles, leaving members shooting at a moving target in their efforts to comply.[5] NATO allies maintain that the capabilities needed to fulfill NATO obligations cannot simply be expressed by a spending target. Just meeting the two percent target might thus not give rise to an efficient outcome. The target does not reflect that larger countries have higher military expenditure—even as a share of GDP—than smaller ones because they also pursue interests outside of the NATO area. A considerable share of US military expenditure, for example, is attributed to the Pacific region.

2.2.3 Compliance with the NATO two percent target and changes of government

Though compliance with the two percent target is verifiable and transparent, compliance with it remains uncertain. Firstly, acceptance of the two percent target is mixed among NATO allies.

[3] "Allies currently meeting the NATO guideline to spend a minimum of 2% of their Gross Domestic Product (GDP) on defence will aim to continue to do so. […] Allies whose current proportion of GDP spent on defence is below this level will: (i) halt any decline in defence expenditure, (ii) aim to increase defence expenditures in real terms as GDP grows, (iii) aim to move toward the 2% guideline within a decade with a view to meeting their NATO Capability Targets and filling NATO's capability shortfalls." Wales Summit Declaration https://www.nato.int/cps/ic/natohq/official_texts_112964.htm (accessed July 2, 2018).

[4] See "Kurzinformation: Zur Entstehungsgeschichte und rechtlichen Bindungswirkung der Zwei-Prozent-Zielvorgabe der NATO für den Anteil der nationalen Verteidigungsausgaben am jeweiligen Bruttoinlandsprodukt", Wissenschaftliche Dienste, Deutscher Bundestag, March 21, 2017.

[5] See Lunn, S., and N. Williams. 2017. "NATO Defence Spending: The Irrationality of 2%." European Leadership Network.

Secondly, the two percent target is a solely cost-effective policy measure and compliance with it is likely to be at the expense of spending on civilian public goods. The marginal "political" cost of spending on the military instead of on civilian public goods is thus increasing. Countries below the two percent target will be less inclined to further increase military expenditure as they get closer to 2% military expenditure relative to GDP. Thirdly, incentives to comply are limited because no credible sanctioning or penalties exist if countries fail to reach the two percent target.

Fourthly, changes of government are likely to influence compliance. A new government is likely to have a political platform that deviates from the platform of the previous government that was in power during the 2014 NATO summit. Even in cases where the new government favors higher military expenditure than the previous government, the new government may have concerns with an agreement signed by the previous government because the new and the previous governments are domestic competitors. A new government is inclined to keep its costly pre-election promises at the expense of military spending, following the "guns vs. butter" trade-off (see also Bove et al. 2017). Peer pressure and reputational cost are, moreover, strong incentives to comply with international agreements, which are reduced for new governments. Peer pressure and reputational cost in case of non-compliance are high since NATO allies represent a fairly homogeneous group of countries, which collaborate in numerous fields other than defense policy. Maintaining reputation in the international domain is important for future collaboration with allied countries in other policy fields. However, reputational costs for not complying with the two percent target will be lower for a new government than for a government that signed the agreement in 2014. The credibility problem for new governments is reduced since it is not their own word they break.

Theory on the determinants of compliance does not clearly indicate whether NATO countries will comply with the two percent target; or whether incentives to free-ride prevail. We expect, however, that changes of government will have a negative effect on compliance.

For countries below the two percent target, we expect lower growth rates of military expenditure relative to GDP when these countries experienced a change of government after the NATO summit in 2014. In turn, we expect higher growth rates when the same government is still in power. We expect the effect of a change of government on compliance to be stronger the larger the change of government is.

The direction of a change in government ideology, i.e. whether the change occurs from leftwing to rightwing or from rightwing to leftwing, is also likely to influence compliance with the two percent target. Rightwing governments are expected to increase military expenditure because they endorse security and support the hierarchies and discipline that are associated with armed forces. Leftwing governments are expected to increase military expenditure because they favor a large government and increasing military expenditure may well increase public employment.[6] Empirical evidence on ideology-induced military expenditure for OECD countries is, however, mixed (Potrafke 2011, Whitten and Williams 2011, Kauder and Potrafke 2016, Bove et al. 2017, Nordvang 2018; on ideology-induced policies in OECD countries see also Potrafke 2017 and 2018). We examine whether compliance with the two percent target is higher when a rightwing government replaces a leftwing government than when a leftwing government replaces a rightwing government.

2.3 Empirical analysis

2.3.1 Data and descriptive statistics

Our dataset includes 27 NATO countries from 2010 to 2018. Two NATO countries are not included: Iceland because lack of data (Iceland has no own defense forces) and Montenegro, which joined NATO not before June 2017. Albania and Croatia joined NATO in 2009. By using data for the period 2010-2018, our sample includes countries that have been NATO members for the entire observation period. We use NATO data for military expenditure, which is the

[6] Scholars discuss the extent to which leftwing governments are pro-labor (see Vadlamannati and Tamazian 2017).

official data source on defense spending within NATO; figures for 2017 and 2018 are estimates. We use SIPRI data on military expenditure for a robustness test to confirm our inferences. By the time of the NATO summit in September 2014, military expenditure was at a historic low: the United States, Greece and the United Kingdom were the only NATO countries with military expenditure relative to GDP above 2% at that time. Figure 2.1 shows that the United States, Greece and the United Kingdom had military expenditure relative to GDP above 2%. The United States, however, has continuously decreased its military expenditure since 2011 after considerable increases in the years before.

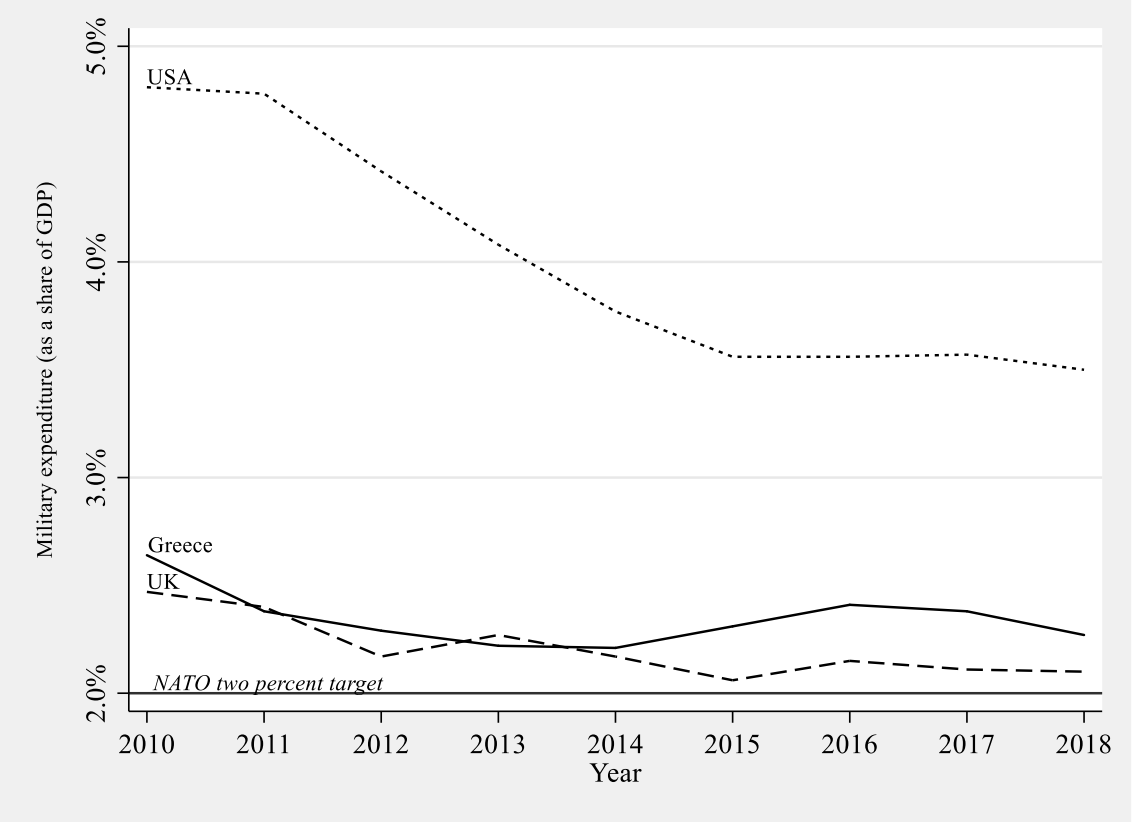

FIGURE 2.1: COUNTRIES CONTINUOUSLY ABOVE 2%

Figure 2.2 shows that Poland and Estonia increased military expenditure relative to GDP prior to the 2014 NATO summit and both reached the two percent target in 2015. Estonia maintained military expenditure relative to GDP above 2%, while Poland decreased military expenditure relative to GDP again in 2016 and 2017 and is estimated to be closely below the two percent target in 2018. Figure 2.3 shows countries with strong efforts towards the two percent target.

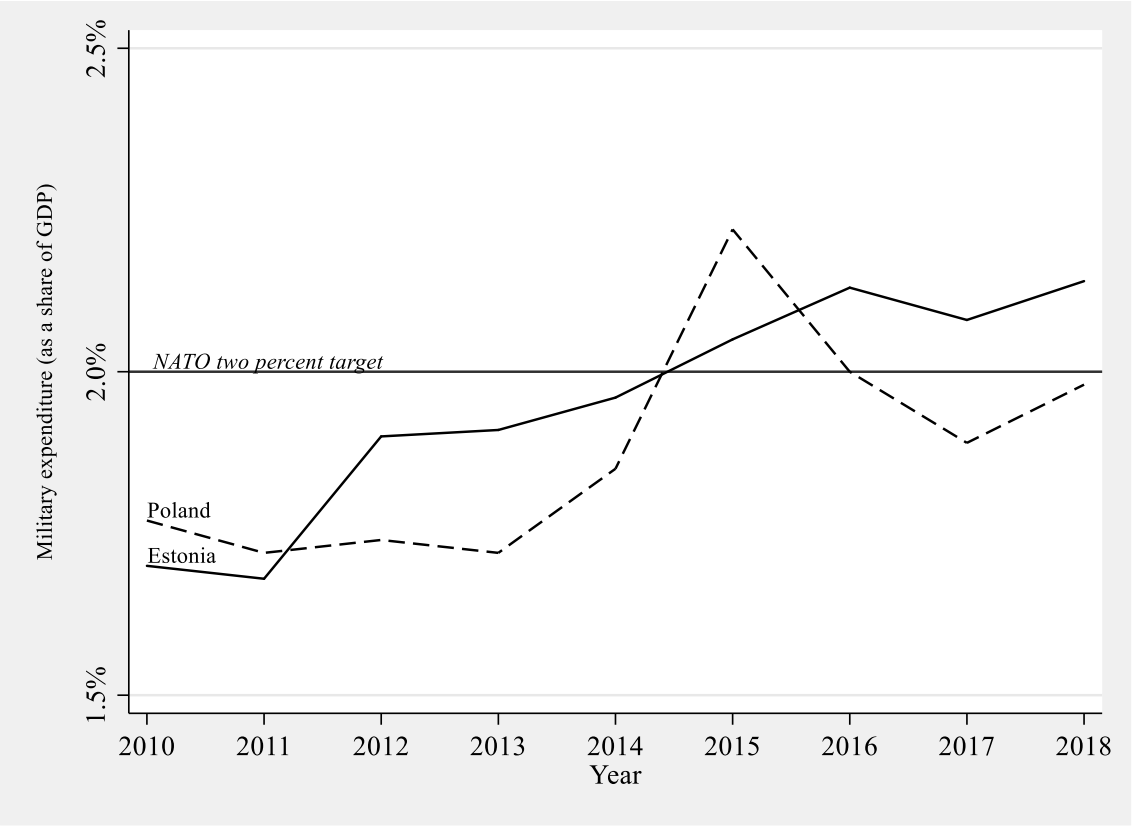

FIGURE 2.2: COUNTRIES OSCILLATING AROUND 2%

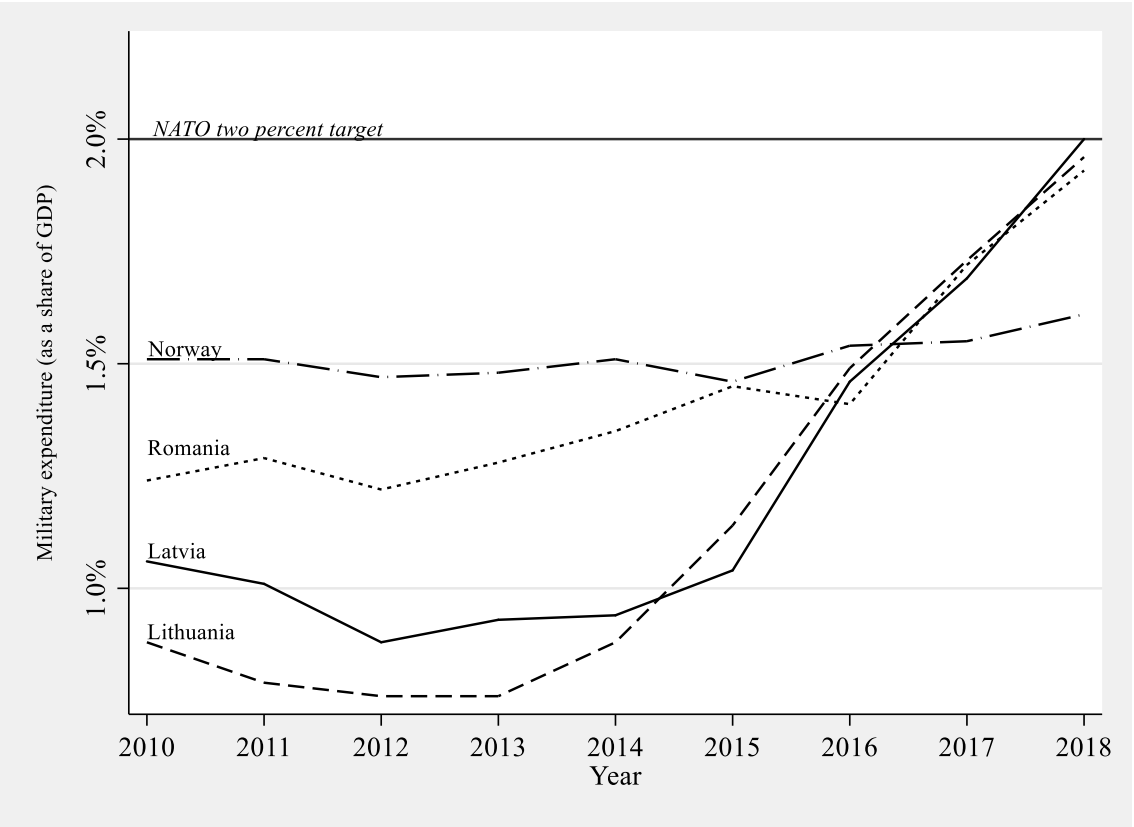

FIGURE 2.3: COUNTRIES WITH STRONG EFFORTS TOWARDS 2%

Lithuania and Latvia drastically increased military expenditure relative to GDP since 2013 and 2014. The increasing defense efforts made by the Baltic countries, however, are influenced by the annexation of Crimea by Russia rather than by the two percent target. Latvia is estimated to have reached exactly 2% military expenditure relative to GDP in 2018, while Lithuania and Romania are closely below. Only the United States, Greece, the United Kingdom, Estonia and Latvia thus managed to comply with the two percent target in 2018; Poland, Romania and Lithuania were closely below.

Figure 2.4 shows growth rates of military expenditure relative to GDP for the individual years. Both mean and median growth rates have increased since 2014 and turned positive in 2015 and 2016. Increasing growth rates since 2014 reflect NATO countries' efforts to comply with the two percent target. Growth rates in military expenditure relative to GDP after the NATO summit in 2014 reveal a clear pattern: in 2015, 13 NATO countries increased military expenditure relative to GDP compared to the previous year and 13 countries decreased military expenditure relative to GDP compared to the previous year. Germany neither increased nor decreased military expenditure relative to GDP. In 2016, the ratio of countries increasing military expenditure to countries decreasing military expenditure was 14 to 10, while three countries neither increased nor decreased military expenditure relative to GDP. This ratio was 17 to 10 for 2017 and 21 to 4 (with two countries neither increasing nor decreasing military expenditure relative to GDP) for 2018. The four countries that reduced military expenditure in 2018 compared to 2017 are Canada, Greece, the United Kingdom, and the United States, which—except of Canada—all complied with the two percent target the year before.

We use growth rates in military expenditure relative to GDP for the years after the NATO summit, i.e. 2015-2018, to show differences between countries that had military expenditure relative to GDP of above or below 2% in the previous year. For countries below 2% military expenditure relative to GDP, we distinguish between countries that experienced a

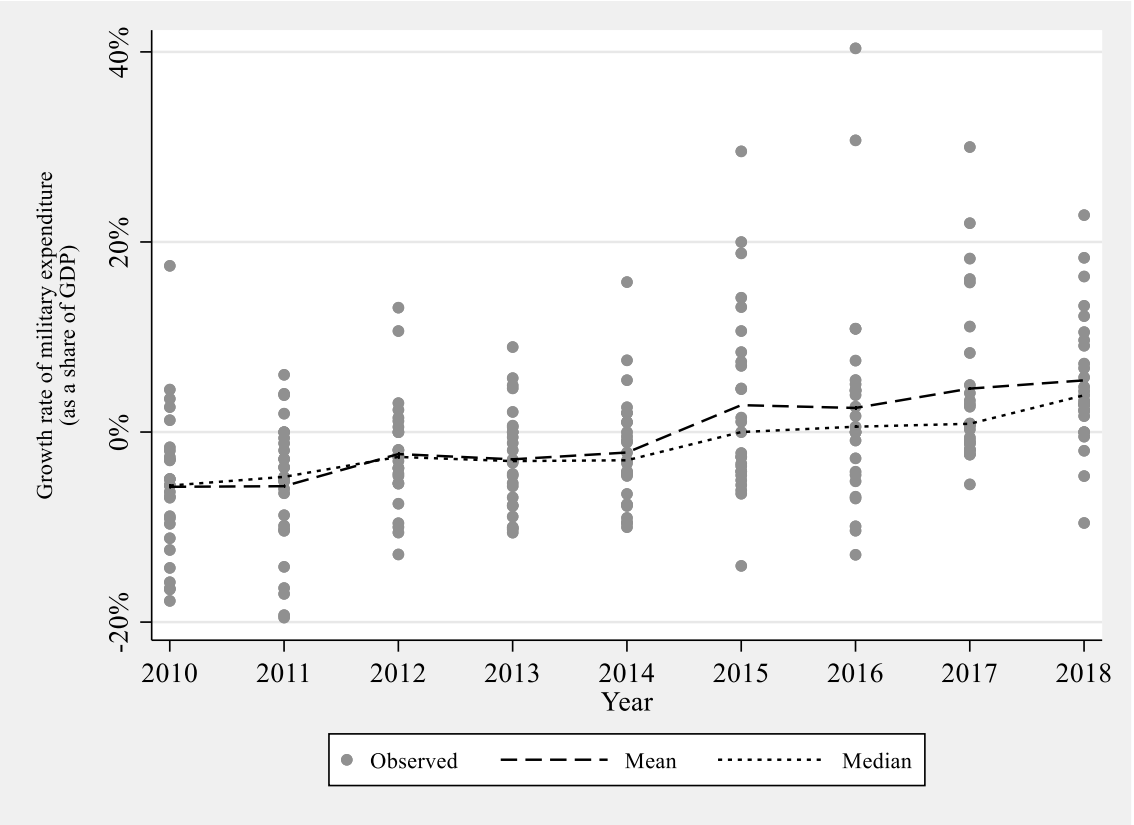

FIGURE 2.4: GROWTH RATES 2010-2018

change of government after the NATO summit in 2014 and those that did not. We consider two types of changes of government, which also include changes of the political platform: firstly, a *small* change of government describes a new personnel composition of the cabinet including a small change in government ideology, i.e. a change in the ideology score of one.[7] We measure government ideology by the index of Potrafke (2009), which is based on Budge et al. (1993) and update it for non-OECD NATO member countries. The index assumes values from 1 (rightwing) to 5 (leftwing). Changes in the government ideology index from year *t* to year *t + 1*, thus, describe a change in government ideology. A small change of government occurs when government changes and the government ideology index changes by one point, such as from a center to a leftwing government. Out of the 27 NATO countries considered, ten countries have experienced such a small change of government since the NATO summit in September 2014. Nine of these ten countries had military expenditure relative to GDP of below 2% in at

[7] We ignore changes of government without changes in government ideology, i.e. when members of the cabinet are replaced but the government party or coalition stays in power.

least one of the years from 2014 to 2016;[8] Greece was continuously above 2%. Secondly, *large* changes of government describe substantial changes in government ideology, i.e. changes in the ideology score of at least two. The large changes are almost exclusively changes from a leftwing to a rightwing government or vice versa. Seven countries experienced such a large change in government ideology since the NATO summit in September 2014: Canada, Portugal and Spain changed from a rightwing to a leftwing government and Croatia, Denmark, and the United States changed from a leftwing to a rightwing government. In Italy, Guiseppe Conte replaced the center-government of Paolo Gentiloni in June 2018. Though the Conte Cabinet and the government parties can hardly be described by rightwing-leftwing-schemes, this change of government was substantial and we indicate it as a large change too. Six of these seven countries which experienced a large change of government had military expenditure relative to GDP continuously below 2%; the United States were continuously above 2%.

A large change of government and a small change of government are mutually exclusive. We indicate both types of changes of government for years in which the change of government occurred in the first half of the calendar year and otherwise one year later. This time lag of at least half a year implies that changes in the defense budget by a new government become effective at earliest half a year after this new government assumed office.

Figure 2.5 shows that after the NATO summit in 2014, countries above 2% military expenditure relative to GDP in the previous year had growth rates of around 0%; and both the mean and median growth rate for these countries were even negative (mean: –0.8%; median: –0.8%). Low growth rates in military expenditure relative to GDP for countries above the two percent target mirror the course of military expenditure relative to GDP for the countries shown in Figure 2.1 from 2014 onwards. By contrast, many countries with military expenditure relative to GDP below 2% in the previous year had positive growth rates in military expenditure relative

[8] Governments might decide to increase military expenditure relative to GDP after the NATO summit in 2014, i.e. for the years 2015-2018, considering the level of military expenditure relative to GDP they observed for the respective previous year.

to GDP in the years from 2015 to 2018. Countries below 2% military expenditure relative to GDP that did not (yet) experience neither a small nor a large change of government after the NATO summit in 2014 had considerably larger growth rates (mean: 6.1%; median: 4.1%) than countries that already met the two percent target. In contrast, countries below 2% military expenditure relative to GDP that experienced a *small* change of government had growth rates (mean: 3.8%; median: 1.2%) that were smaller than those of countries without a small change of government.[9] The pattern is even clearer for countries below 2% military expenditure relative to GDP, which experienced a *large* change of government after the NATO summit in 2014: these countries had smaller growth rates (mean: 0.9%; median: 0%). Latvia and Norway, for

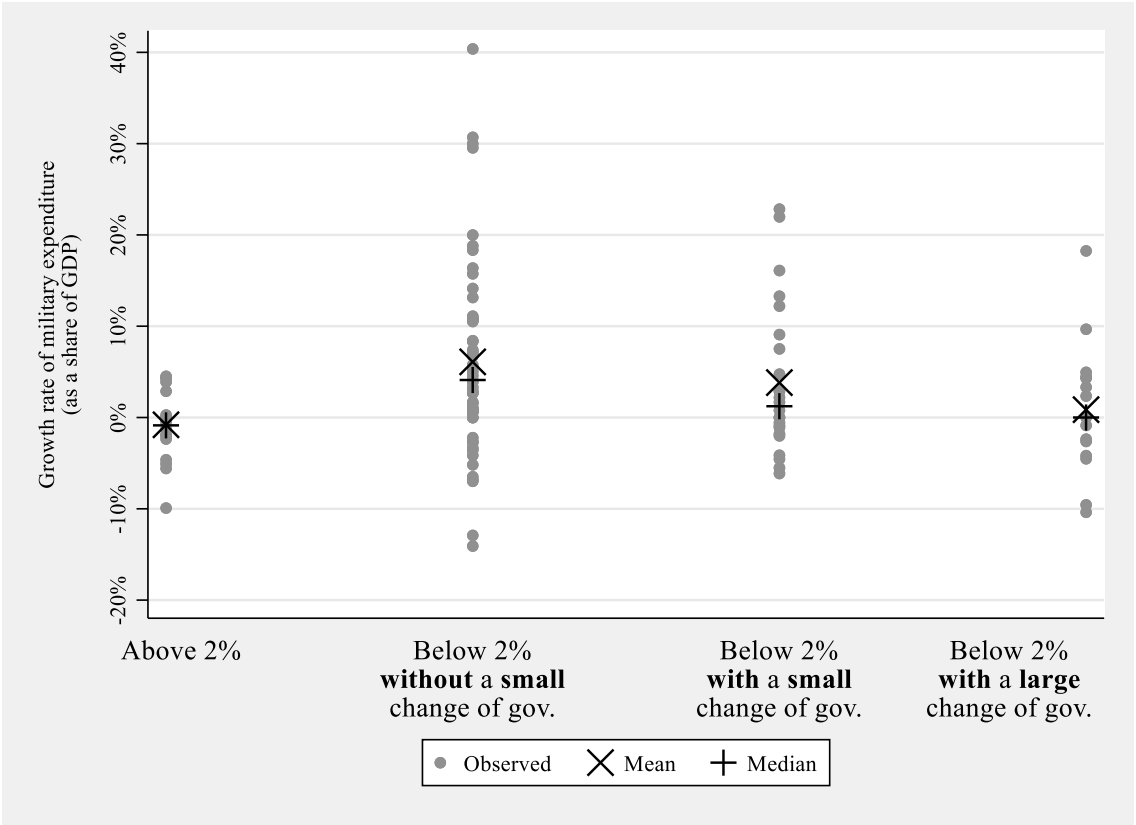

FIGURE 2.5: GROWTH RATES 2015-2018 FOR COUNTRIES ABOVE AND BELOW THE TWO PERCENT TARGET AND WITH AND WITHOUT A CHANGE OF GOVERNMENT

example, experienced no change of government in the years after the NATO summit in 2014 and increased military expenditure relative to GDP until 2018. By contrast, Croatia experienced

[9] Means in growth rates are not weighted according to the GDP of each country, i.e. this mean does not equal the growth rate of all countries' military expenditure relative to all countries' GDP.

a change from a leftwing to a rightwing government, i.e. a large change of government, in January 2016 and decreased military expenditure relative to GDP in 2016 by more than 10%. Portugal decreased military expenditure relative to GDP after the change from a rightwing to a leftwing government in November 2015. The growth rates in military expenditure relative to GDP for Canada and Portugal, which both changed from a rightwing to a leftwing government, and Croatia and Denmark, which both changed from a leftwing to a rightwing government, however, do not indicate that the direction of the government ideology change influences compliance with the two percent target differently.

The descriptive statistics indicate that a change of government is related to compliance with the two percent target depending on how strong the change of government is. Changes of government that include changes in government ideology, or even a change from a rightwing to a leftwing government or vice versa, corroborate that new national governments are less likely to comply with international agreements signed by previous national governments. We elaborate on conditional correlations between changes of government and compliance with the two percent target by estimating panel data models in the next sections.

2.3.2 Empirical strategy

The baseline panel data model has the following form:

$$\begin{aligned} &\textit{Military expenditure relative to GDP (Growth rate)}_{it} = \\ &\alpha_j \textit{ Two percent target}_{it} + \beta_j \textit{ Change of government since '14}_{ijt} + \\ &\gamma_j \textit{ Two percent target}_{it} * \textit{ Change of government since '14}_{ijt} + \\ &\Sigma_n \zeta_{jn} x_{int} + \eta_i + \tau_t + u_{ijt} \end{aligned}$$

$$\textit{with } i = 1, \ldots, 27;\ t = 1, \ldots, 9;\ j = 1, 2;\ n = 1, \ldots, 8 \qquad (1)$$

The dependent variable *Military expenditure relative to GDP (Growth rate)*$_{it}$ describes the growth rate in military expenditure relative to GDP for country *i* in year *t*. The dummy variable *Two percent target*$_{it}$ assumes the value 1 for the years after the NATO summit in 2014, i.e. for the years 2015-2018, if military expenditure relative to GDP was below the two percent target the year before, and value 0 otherwise. The variable *Two percent target*$_{it}$ reflects that a country which intends to comply with the two percent target is expected to increase military expenditure disproportionally compared to expected GDP growth when this country did not (yet) meet the two percent target in the previous year. The variable *Change of government since '14*$_{ijt}$ indicates small and large changes of government ($j = 2$) after the NATO summit in September 2014 in country *i* in year *t* ($t \geq 2015$), as described in Section 2.3.1. The dummy variables assume the value 1 when a change of government occurred after the NATO summit in September 2014 (as described in Section 2.3.1, the change of government needs to have occurred within the first half of the calendar year; otherwise the dummy variables indicate the change of government one year later). The variables for changes of government thus assume the value 1 in years in which the government that agreed to the two percent target in September 2014 has no longer been in office. We include the interaction term between the *Two percent target*$_{it}$ and the *Change of government since '14*$_{ijt}$ variable to examine whether countries below 2% military expenditure relative to GDP that experienced a change of government after the NATO summit in 2014 increased growth in military expenditure to a smaller extent than countries that did not (yet) experience a change of government.

We include eight control variables ($n = 8$). Firstly, we add variables measuring government ideology and parliamentary elections. *Rightwing*$_{it}$ is a dummy variable based on the government ideology index of Potrafke (2009) and assumes the value 1 for rightwing governments, i.e. a government ideology index with values 1 or 2, and value 0 otherwise. We control for government ideology because rightwing governments are likely to have higher military expenditure than leftwing governments have (Whitten and Williams 2011, Bove et al.

2017). The election dummy variable *Election (t + 1)*$_{it}$ is 1 in years which precede parliamentary elections. We control for elections because governments in times of elections are likely to shift public spending from military expenditure to social welfare to compete for votes (Bove et al. 2017). Secondly, we include variables describing conflicts, as well as internal and external threats. The dummy variable *War (t – 1)*$_{it}$ indicates whether a country has been involved in an interstate war (i.e. a war with another country) or an internal war (i.e. a war between a government and internal conflict groups) in year *t – 1* with at least 25 battle-related deaths. We consider wars in period *t – 1* because military expenditure is likely to increase with a time lag once a country gets involved in a conflict; in turn, military expenditure is likely to also decrease with a time lag once a conflict has ended because it takes time to demobilize and military resources need to be replenished. The data for armed conflicts is taken from the "UCDP/PRIO Armed Conflict Dataset" (Version 17.2) and defined according to Gleditsch et al. (2002). The variable *Internal threat*$_{it}$ proxies domestic conflict probability and a country's internal stability. The variable is defined as the average of an eleven-point index for internal violence over the past ten years. This index is taken from the "Major Episodes of Political Violence (MEPV) and Conflict Regions, 1946-2016" dataset by the Center for Systemic Peace. The definition of both variables *War (t – 1)*$_{it}$ and *Internal threat*$_{it}$ has been shown to explain variance in military expenditure relative to GDP (Blum 2018, 2020). Countries like Latvia, Lithuania, and Norway most probably increased military expenditure because of Russia's aggressions—peaking in the annexation of the Crimean peninsula in March 2014—rather than because of the two percent target. We therefore add a dummy variable *Crimea*$_{it}$ which assumes the value 1 for countries with a common border with Russia for the years 2014-2018, and value 0 otherwise.[10] This variable (jointly with fixed year effects) ensures that effects of the change in the security environment and effects of the two percent target are disentangled. Thirdly, we add three

[10] Poland and Lithuania are also labeled as having a common border with Russia because they are adjacent to the Russian exclave Kaliningrad.

socioeconomic variables: the growth rate of GDP in constant (2010) US dollars, the growth rate of population and the growth rate in government debt relative to GDP. The growth rates of GDP and population are included to investigate substitution effects when GDP and population increase (Dunne et al. 2008, Albalate et al. 2012, Blum 2018, Pamp et al. 2018, Langlotz and Potrafke 2019). The growth rate in the debt-to-GDP ratio accounts for a country's fiscal capacity; fiscal capacity and other macroeconomic factors have been shown to influence military expenditure in European countries and NATO countries (Odehnal and Neubauer 2018, Christie 2019). Data for GDP and population are taken from the World Bank; data for government debt to GDP are taken from the IMF. We include fixed country, η_i, and also fixed time effects, τ_t, and estimate the fixed effects model with ordinary least squares and standard errors robust to heteroskedasticity (Huber/White/sandwich standard errors; see Huber 1967 and White 1980).

Tables A2.1 and A2.2 in the Appendix show summary statistics and correlations of the variables. Data for the three socioeconomic variables are available for the period from 2010 to 2017 only. We therefore estimate balanced panels for the periods 2010-2018 and 2010-2017 and include the three socioeconomic variables in the panel for the period 2010-2017 only.

2.4 Empirical results

2.4.1 Baseline results

Table 2.1 shows the baseline results. Columns (4) and (5) relate to the period 2010-2017 and thus exclude the large changes of government in Italy and Spain and the small change of government in Hungary (the Fidesz party regained the two-third-majority in parliament) in 2018 but consider the three socioeconomic variables which are not yet available for 2018. We examine whether inferences regarding changes of government in countries that do not (yet) meet the two percent target change when individual control variables are included or excluded. The coefficient of the *Two percent target*$_{it}$ variable (countries which had military expenditure

relative to GDP after the NATO summit of below 2% in the previous year) is statistically significant at the 1% level. The numerical meaning of the coefficient is that after the NATO summit in 2014, countries which had military expenditure relative to GDP of below 2% in the previous year and did not experience a change of government increased the growth rate in military expenditure relative to GDP by 9.8 percentage points. This estimate corroborates that countries below the two percent target increased military expenditure relative to GDP, while countries that already comply with the two percent target did not further increase military expenditure relative to GDP. The estimate is quite large: the mean growth rate of military expenditure relative to GDP until the 2014 NATO summit was –3.8% (median: –3.7%; see Figure 2.4) and the coefficient estimate, thus, indicates that countries below the two percent target and without a change of government on average managed to turn growth rates for military expenditure relative to GDP into positive values. The coefficient estimates of the interaction term between countries below the two percent target and with a small change of government are negative in columns (1) to (5), but lack statistical significance.

The variable measuring a large change of government after the NATO summit in 2014 has a positive sign and is statistically significant at the 1% level in columns (1) to (4) and at the 10% level when year fixed effects are added in column (5). The positive estimate for the large change of government is attributed to the United States because it is the only country that experienced a large change of government without being below the two percent target. This estimate thus reflects that the United States' average growth rate of military expenditure relative to GDP was on average lower in the years before Donald Trump assumed office than after the Trump administration was in office.[11] The coefficient estimate of the interaction term between countries below the two percent target and with a large change of government is negative and

[11] No standard error of the estimate for the large change of government is reported in column (1) because the coefficient perfectly predicts the growth rate in military expenditure relative to GDP for the United States after the large change of government in 2017. Prediction is no longer perfect and standard errors are computed when the dummy variable for rightwing governments is added in column (2).

statistically significant at the 1% level in columns (1) to (4) and at the 5% significance level in column (5) when year fixed effects are added.

TABLE 2.1: BASELINE ESTIMATION RESULTS

NATO Military expenditure to GDP[a]	(1) 2010-18	(2) 2010-18	(3) 2010-18	(4) 2010-17	(5) 2010-17
Two percent target	0.098***	0.096***	0.082***	0.075***	0.075**
	(0.021)	(0.021)	(0.016)	(0.020)	(0.030)
Small change since '14	0.021	0.024	0.014	0.012	0.005
	(0.052)	(0.049)	(0.058)	(0.072)	(0.075)
Two percent target*Small change since '14	-0.040	-0.039	-0.024	-0.033	-0.036
	(0.049)	(0.048)	(0.054)	(0.068)	(0.069)
Large change since '14	0.046***	0.055***	0.064***	0.077***	0.057*
	(0.000)	(0.013)	(0.015)	(0.014)	(0.032)
Two percent target*Large change since '14	-0.094***	-0.103***	-0.101***	-0.114***	-0.102**
	(0.018)	(0.024)	(0.024)	(0.026)	(0.043)
Rightwing		-0.008	-0.017	-0.017	-0.014
		(0.012)	(0.014)	(0.015)	(0.015)
Election (t + 1)		0.003	0.003	0.009	0.010
		(0.009)	(0.009)	(0.009)	(0.009)
War (t – 1)			0.101***	0.084***	0.067***
			(0.007)	(0.010)	(0.015)
Internal threat			0.109***	0.100***	0.084**
			(0.017)	(0.027)	(0.039)
Crimea			0.072	0.081*	0.083
			(0.047)	(0.047)	(0.053)
GDP[a]				0.159	0.359
				(0.380)	(0.406)
Population[a]				0.348	0.034
				(2.341)	(2.248)
Debt to GDP[a]				0.018	0.031
				(0.116)	(0.112)
Marginal effect of Large change since '14 (if Two percent target=1)	-0.048***	-0.048**	-0.037**	-0.037*	-0.045**
	(0.018)	(0.019)	(0.017)	(0.021)	(0.022)
Country Fixed Effects	yes	yes	yes	yes	yes
Year Fixed Effects	no	no	no	no	yes
Observations	243	243	243	216	216
Countries	27	27	27	27	27
R^2 Overall	0.222	0.224	0.101	0.093	0.141
R^2 Within	0.246	0.247	0.285	0.260	0.287
R^2 Between	0.071	0.077	0.002	0.002	0.017

Notes: Standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively. All regressions apply standard errors clustered at the country level. [a] denotes variables which are expressed in year-on-year growth rates.

The marginal effect of a large change of government for countries below the two percent target (shown below the coefficient estimates in Table 2.1) is negative and statistically significant at the 1% level in column (1), at the 5% level in columns (2) and (3), at the 10% level in column (4) and again at the 5% level when year fixed effects are added in column (5). The size of the marginal effect indicates that countries below the two percent target in the previous year that experienced a large change of government decreased growth rates by up to 4.8 percentage points. The effect of 4.8 percentage points is quite large. It is almost half the size of the 9.8

percentage points increase for NATO countries which had military expenditure relative to GDP of below 2% in the previous year and did not experience a change in government ideology. And it is larger than the 3.8% average growth rate of military expenditure relative to GDP (median: 2.3%; see Figure 2.4) for all NATO countries in the years after the NATO summit.

The coefficient for rightwing governments, $Rightwing_{it}$, and the coefficient for an upcoming election, $Election\ (t + 1)_{it}$, do not turn out to be statistically significant in columns (3) to (5). The coefficients for $War\ (t - 1)_{it}$, i.e. an armed conflict in the previous year, and $Internal\ threat_{it}$ are statistically significant at the 1% level in columns (3) to (5), except for the coefficient for $Internal\ threat_{it}$ which is significant at the 5% significance level in column (5). A war in the previous year is associated with an increase in the growth rate of military expenditure relative to GDP by up to 10.1 percentage points. The positive relationship of both previous war and internal threat and the growth rate of military expenditure relative to GDP is in line with earlier findings (Collier and Hoeffler 2007, Dunne et al. 2008, Blum 2018, 2020). The coefficient of the dummy variable $Crimea_{it}$ is positive and statistically significant at the 10% level in column (4). The coefficients for the growth rates of GDP, population and government debt relative to GDP added in column (4) do not turn out to be statistically significant. These results do not support the findings of previous studies using military expenditure as the dependent variable. Studies which control for GDP and population use larger samples and include developing countries or both developing and developed countries (Dunne et al. 2008, Albalate et al. 2012, Blum 2018, Pamp et al. 2018). We focus, however, on the rather homogeneous group of NATO countries. Samples of studies which control for government debt include European countries or NATO members, i.e. rather homogeneous groups of countries. However, these empirical models are estimated in levels or first differences rather than in growth rates (Odehnal and Neubauer 2018, Christie 2019). Column (5) also includes fixed year effects but the results hardly differ from those in column (4). The year fixed effects in column (5) do not turn out to be statistically significant except for the years 2012 and

2013, for which the fixed effect is positive and significant at the 5% and the 10% level (not reported). An F-Test on the joint significance of the fixed year effects shows that the year fixed effects are jointly significant at the 10% level.

Estimation results for changes of government which distinguish between changes from leftwing to rightwing and changes from rightwing to leftwing do not indicate that the direction of the government ideology change influences compliance with the two percent target (results not shown).

2.4.2 Robustness tests

We examine the robustness of our results for large changes of government. Firstly, data on military expenditure published by NATO differ from data collected by SIPRI for some countries. The SIPRI data suggest that the United Kingdom did not comply with the two percent target in any of the years of the observation period, while France did comply with the two percent target in all years of the observation period. Turkey did comply with the two percent target in 2016 and 2017 and Poland did not comply with the two percent target in 2016 according to SIPRI data. Differences in figures for military expenditure between SIPRI, NATO and other data sources give rise to disagreements on compliance and non-compliance with the two percent target among NATO countries because countries will claim figures on own military expenditure that are favorable for them. When we use SIPRI instead of NATO data on military expenditure, inferences regarding changes of government for countries below the two percent target do not change (see Table A2.3 in the Appendix).[12] The coefficient for rightwing governments is negative and statistically significant at the 5% and 10% level in columns (3) to (5). This result is not in line with the findings of Bove et al. (2017), but it reflects the changes in growth rates of military expenditure relative to GDP for two countries: Canada had higher

[12] Out of the 216 observations in the panel, 13 observations are SIPRI estimates and one observation is described to be "highly uncertain".

average growth rates for military expenditure relative to GDP after the leftwing government of Justin Trudeau replaced the rightwing government of Stephen Harper in 2015. Poland also had on average lower growth rates for military expenditure relative to GDP after the rightwing government replaced the center government in 2015. The coefficient of the dummy variable $Crimea_{it}$ is positive and statistically significant at the 5% level in columns (3) to (5). Countries with a common border to Russia perceived a higher level of threat originating from Russia after Russia's annexation of Crimea in March 2014 and, therefore, increased military expenditure relative to GDP. On average, these countries increased growth rates in military expenditure relative to GDP by up to 12.7 percentage points in response to the annexation of Crimea.

Secondly, we apply a spatial lag model to account for spatial dependences in military expenditure among NATO allies. Following the Security Web concept of Rosh (1988), growth rates in military expenditure relative to GDP are likely to be influenced by neighboring countries with which a country shares a common border (Blum 2018). We therefore estimate a spatial autoregressive model (SAR) following Anselin (1988), which controls for growth rates in military expenditure relative to GDP of countries with a common land or sea border using maximum likelihood estimation.[13] Table A2.4 in the Appendix shows that the results hardly differ from the baseline estimation results. The coefficient estimate of the spatial lag of military expenditure relative to GDP is statistically significant at the 5% level in columns (1), (2) and (4) and indicates that countries increase the growth rate in military expenditure relative to GDP by almost 0.2 percentage points when a neighboring country increases its growth rate of military expenditure relative to GDP by 1.0 percentage points. The coefficient estimate of the spatial lag is statistically significant only at the 10% level in column (3) and does not turn out to be statistically significant in column (5).

[13] The binary contiguity matrix is row-standardized.

One may well want to control for time-invariant variables that are likely to be correlated with the growth in military expenditure relative to GDP: the geographical distance to Russia and the initial level of military expenditure relative to GDP. It is conceivable that the growth in military expenditure is larger, the closer a country is located to Russia. The growth in military expenditure is also likely to be smaller, the larger military expenditure relative to GDP in 2010 was. We include the inverse distance between capitals of NATO countries and Moscow and military expenditure relative to GDP in 2010 and exclude fixed country effects. Geographical closeness to Russia has the expected positive sign but lacks statistical significance. Military expenditure relative to GDP in 2010 has the expected negative sign and is statistically significant in individual specifications. Including the geographical closeness to Russia and military expenditure relative to GDP in 2010 does not change the inferences regarding effects of large changes of government on the growth in military expenditure (results not shown).

2.5 Conclusion

We have examined whether changes of government influence compliance with international agreements by means of the NATO two percent target. Whether countries reduce their efforts to increase military expenditure relative to GDP after a change of government has important implications for compliance with the two percent target in 2024 because many NATO countries are likely to experience changes of government within this ten-year period. We have used panel data for 27 NATO countries for the period 2010-2018 and found that countries that experienced a large change of government, e.g. a change of government from leftwing to rightwing or vice versa, are less likely to comply with the two percent target than countries that did not experience such a change of government after the NATO summit in 2014. Changes of government, however, do not turn out to be statistically significant when changes in government ideology are rather small, e.g. changes to or away from a center government.

The sample size with 27 NATO countries we observed from 2010-2018 is small and only a few countries experienced a large change of government after the NATO summit in 2014 for which we found the negative effect on growth rates in military expenditure relative to GDP. Investigating whether changes of government influence compliance with international agreements, however, is a worthwhile endeavor, even when samples are small.

We propose that international treaties and agreements need to be designed in a way that encourages compliance even when a government that has signed the treaty or agreement is no longer in power. In an intergovernmental organization such as the NATO alliance, member states might not commit themselves to binding agreements with impending sanctions in case of non-compliance. Agreement design should therefore consider positive incentives for governments to comply, irrespective of whether the incumbent or his or her predecessor committed to the agreement.

Future research should examine what determines compliance with international treaties and agreements in policy fields other than defense policy. Findings on the determinants of compliance should help to deal with the credibility problem of national governments when committing to international agreements and how to design future international treaties and agreements.

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Appendix

TABLE A2.1: SUMMARY STATISTICS

	Observations	Mean	Std. Dev.	Min	p25	p75	Max
NATO Mil. exp. to GDP[a]	243	-0.004	0.087	-0.195	-0.054	0.033	0.404
SIPRI Mil. exp. to GDP[a]	216	-0.008	0.087	-0.229	-0.054	0.025	0.409
Two percent target	243	0.379	0.486	0	0	1	1
Small change since '14	243	0.119	0.325	0	0	0	1
Large change since '14	243	0.070	0.256	0	0	0	1
Rightwing	243	0.428	0.496	0	0	1	1
Election (t + 1)	243	0.272	0.446	0	0	1	1
War (t – 1)	243	0.070	0.256	0	0	0	1
Internal threat	243	0.066	0.341	0	0	0	2
Crimea	243	0.103	0.304	0	0	0	1
GDP[a]	216	0.019	0.024	-0.091	0.010	0.031	0.111
Population[a]	216	0.002	0.009	-0.031	-0.003	0.007	0.030
Debt to GDP[a]	216	0.029	0.098	-0.318	-0.025	0.059	0.604

Notes: [a] denotes variables which are expressed in year-on-year growth rates.

TABLE A2.2: CORRELATIONS

	NATO Mil. exp. to GDP[a]	SIPRI Mil. exp. to GDP[a]	Two percent target	Small change since '14	Large change since '14	Rightwing	Election (t + 1)	War (t – 1)	Internal threat	Crimea	GDP[a]	Popul-ation[a]	Debt to GDP[a]
NATO Mil. exp. to GDP[a]	1.000												
SIPRI Mil. exp. to GDP[a]	0.910	1.000											
Two percent target	0.450	0.439	1.000										
Small change since '14	0.139	0.144	0.341	1.000									
Large change since '14	0.033	0.018	0.285	-0.101	1.000								
Rightwing	-0.065	-0.077	-0.024	-0.011	0.056	1.000							
Election (t + 1)	0.005	0.005	-0.038	-0.082	-0.059	0.014	1.000						
War (t – 1)	-0.087	-0.073	-0.114	-0.101	0.051	0.089	0.014	1.000					
Internal threat	-0.018	-0.000	0.048	-0.071	-0.053	0.224	0.023	0.611	1.000				
Crimea	0.370	0.396	0.183	0.084	-0.093	0.063	0.006	-0.093	-0.066	1.000			
GDP[a]	0.157	0.183	0.263	0.113	0.030	0.076	-0.012	0.280	0.377	0.116	1.000		
Population[a]	-0.038	-0.056	0.014	-0.117	0.030	-0.050	-0.018	0.303	0.315	-0.147	0.136	1.000	
Debt to GDP[a]	-0.110	-0.105	-0.315	-0.141	-0.146	-0.074	0.051	-0.115	-0.152	-0.087	-0.372	-0.134	1.000

Notes: [a] denotes variables which are expressed in year-on-year growth rates.

TABLE A2.3: ESTIMATION RESULTS WITH SIPRI DATA

SIPRI Military expenditure to GDP[a]	(1) 2010-17	(2) 2010-17	(3) 2010-17	(4) 2010-17	(5) 2010-17
Two percent target	0.087***	0.085***	0.065***	0.063***	0.062**
	(0.022)	(0.023)	(0.016)	(0.018)	(0.028)
Small change since '14	0.016	0.019	0.005	-0.006	-0.021
	(0.045)	(0.044)	(0.055)	(0.050)	(0.054)
Two percent target*Small change since '14	-0.007	-0.004	0.021	0.033	0.038
	(0.058)	(0.057)	(0.061)	(0.061)	(0.059)
Large change since '14	0.027***	0.041***	0.055***	0.057***	0.045
	(0.000)	(0.013)	(0.014)	(0.013)	(0.027)
Two percent target*Large change since '14	-0.078***	-0.090***	-0.086***	-0.089***	-0.087**
	(0.021)	(0.024)	(0.023)	(0.023)	(0.038)
Rightwing		-0.013	-0.026**	-0.026*	-0.027**
		(0.012)	(0.013)	(0.013)	(0.013)
Election (t + 1)		0.003	0.003	0.003	0.005
		(0.011)	(0.011)	(0.011)	(0.010)
War (t – 1)			0.111***	0.112***	0.089***
			(0.014)	(0.015)	(0.019)
Internal threat			0.152***	0.168***	0.168***
			(0.007)	(0.023)	(0.031)
Crimea			0.112**	0.115**	0.126**
			(0.046)	(0.042)	(0.048)
GDP[a]				0.454	0.784
				(0.465)	(0.470)
Population[a]				0.428	0.113
				(2.329)	(2.247)
Debt to GDP[a]				0.066	0.075
				(0.117)	(0.111)
Marginal effect of Large change since '14 (if Two percent target=1)	-0.051**	-0.049**	-0.031*	-0.032	-0.042*
	(0.021)	(0.023)	(0.018)	(0.019)	(0.023)
Country Fixed Effects	yes	yes	yes	yes	yes
Year Fixed Effects	no	no	no	no	yes
Observations	216	216	216	216	216
Countries	27	27	27	27	27
R^2 Overall	0.193	0.199	0.095	0.084	0.112
R^2 Within	0.213	0.216	0.303	0.311	0.346
R^2 Between	0.053	0.072	0.007	0.009	0.026

Notes: Standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively. All regressions apply standard errors clustered at the country level. [a] denotes variables which are expressed in year-on-year growth rates.

TABLE A2.4: SPATIAL AUTOREGRESSIVE MODEL (SAR) ESTIMATION RESULTS

NATO Military expenditure to GDP[a]	(1) 2010-18	(2) 2010-18	(3) 2010-18	(4) 2010-17	(5) 2010-17
Two percent target	0.088***	0.087***	0.078***	0.071***	0.081***
	(0.017)	(0.017)	(0.015)	(0.019)	(0.031)
Small change since '14	0.009	0.012	0.008	0.008	0.011
	(0.053)	(0.050)	(0.057)	(0.070)	(0.072)
Two percent target*Small change since '14	-0.031	-0.030	-0.020	-0.030	-0.043
	(0.049)	(0.047)	(0.052)	(0.067)	(0.067)
Large change since '14	0.036***	0.044***	0.056***	0.057***	0.048
	(0.004)	(0.014)	(0.013)	(0.014)	(0.031)
Two percent target*Large change since '14	-0.084***	-0.092***	-0.095***	-0.094***	-0.093**
	(0.015)	(0.021)	(0.022)	(0.025)	(0.041)
Rightwing		-0.008	-0.016	-0.016	-0.014
		(0.012)	(0.013)	(0.014)	(0.014)
Election (t + 1)		0.003	0.003	0.009	0.010
		(0.008)	(0.008)	(0.008)	(0.008)
War (t – 1)			0.098***	0.082***	0.065***
			(0.007)	(0.009)	(0.014)
Internal threat			0.095***	0.088***	0.075*
			(0.019)	(0.028)	(0.039)
Crimea			0.061	0.071	0.076
			(0.045)	(0.046)	(0.051)
GDP[a]				0.175	0.353
				(0.366)	(0.387)
Population[a]				0.213	-0.062
				(2.228)	(2.107)
Debt to GDP[a]				0.029	0.037
				(0.112)	(0.107)
Spatial ρ	0.159**	0.159**	0.102*	0.098**	0.078
	(0.064)	(0.064)	(0.053)	(0.043)	(0.051)
Error variance σ^2	0.005***	0.005***	0.005***	0.005***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Country Fixed Effects	yes	yes	yes	yes	yes
Year Fixed Effects	no	no	no	no	yes
Observations	243	243	243	216	216
Countries	27	27	27	27	27
R^2 Overall	0.223	0.225	0.116	0.109	0.158
R^2 Within	0.243	0.245	0.287	0.263	0.290
R^2 Between	0.086	0.091	0.003	0.004	0.025

Notes: Standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively. All regressions apply standard errors clustered at the country level. [a] denotes variables which are expressed in year-on-year growth rates.

3. Democracy's Third Wave and National Defense Spending

Abstract[*]

I investigate how the third wave of democracy influenced national defense spending by using a panel of 110 countries for the period 1972-2013. I use new SIPRI data on military expenditure, which has been extended to years prior to 1988, and four democracy measures to address differences among democracy indices. The results from a dynamic panel data model suggest that democracy's third wave decreased defense spending relative to GDP by about 10% within countries that experienced democratization. This result does not show to be heterogeneous across world regions which the third wave reached in different sub-waves. I exploit the regional diffusion of democracy in the context of the third wave of democratizations as an instrumental variable (IV) for democracy in order to overcome endogeneity problems. The IV estimates indicate that democracy decreased national defense spending relative to GDP by about 20% within countries, demonstrating that OLS results underestimate the effect of democracy on national defense spending. The cumulative long-run effect of democratization resulting from the dynamics in defense spending is almost three times higher for both OLS and IV estimates.

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3.1 Introduction

Defense spending follows budget decisions taken by national governments and has been shown to be influenced by political institutions (e.g. Dunne and Perlo-Freeman 2003a, Collier and Hoeffler 2007, Albalate et al. 2012, Blum 2018). Against the backdrop of the worldwide decrease in popularity for democracy, and drastically reduced defense budgets in Western democracies since the end of the Cold War (which NATO's two percent target for defense spending relative to GDP is supposed to counteract), patterns of defense spending and the underlying policy decisions in democracies and autocracies are relevant for scholars and political decision-makers. On the one hand, defense spending describes—among others—one measure for the military capabilities of a nation state. Defense spending thus indicates how militarily powerful a country is. If democratization decreases national defense spending, democracies might also be less threatening, and democratization may alleviate arms races and help to solve security dilemmas. On the other hand, defense spending is one element of government spending and reveals different preferences for public spending between democracies and autocracies. National defense spending is, in turn, likely to give rise to indirect effects because it has been shown that defense spending affects other economic variables like economic growth (Dunne et al. 2005, Alptekin and Levine 2012), debt (Dunne et al. 2004) or productivity (Caruso and Francesco 2012) as well as national arms production (Blum 2019).

This chapter contributes to the literature on how political institutions influence national defense spending. The third wave of democratizations as defined by Huntington (1991a) provides a unique setting to investigate how the spread of democracy influenced national defense spending.[1] Democracy's third wave doubled the number of democracies until 1990 and proceeded in regional sub-waves: it describes the democratizations in Southern Europe in the

[1] Huntington (1991a, 1991b: 13-26) defines three waves of democratization to describe the global expansion of democracy: the introduction of male suffrage in the United States and in European countries describes the first wave lasting from the 1820s until 1926. The second wave accounts for democratizations after the Second World War until the 1960s in the former fascist European countries, countries like Japan, Korea and Turkey as well as in some Latin American countries which, however, quickly relapsed into autocratic regimes.

mid-1970s, in Latin America in the 1980s, in Eastern Europe in the early 1990s and in some African and Asian-Pacific countries. I investigate how political institutions in the context of the third wave of democratization influenced national defense spending for 110 countries over the period 1972-2013. I use new data on military expenditure by the Stockholm International Peace Research Institute (SIPRI), which has been extended to years prior to 1988, and estimate the impact of democracy's third wave by means of four democracy measures. I apply a dynamic panel data model and an instrumental variable (IV) approach in order to deal with endogeneity problems associated with the relationship between political institutions and defense spending. I find that democracy's third wave decreased national defense spending within countries that experienced democratization by about 10% according to OLS and by about 20% according to IV estimates.

Democracies are supposed to spend a smaller share of their GDP for defense than autocracies as a result of their government spending policies and their role within the international community. National defense spending represents a budget decision on the spending of public funds: a government chooses how much public funds it spends for defense and for civilian public goods—the so-called "guns versus butter" trade-off. In a democracy, an electorate votes the government and the median voter as the representative in a majority voting with single-peaked preferences is decisive for the outcome. The party or candidate that most closely reflects the preferences of the median voter takes over the government. The median voter in a democracy prefers spending on civilian public goods to defense spending; he regards defense spending as a necessity to ensure national security, while levels above the necessary level do not further increase his utility. Civilian public goods, in contrast, have a higher probability to further increase the median voter's utility. The median voter prefers "butter" to "guns". Civilian public goods spending, thus, crowds out defense spending because a government wants to ensure the support from the electorate (Dudley and Montmarquette 1981, Blum 2018). In an autocracy, in contrast, the government depends on the loyalty of the elites.

Governments in autocratic regimes must therefore allow these elites to extract rents to ensure political support. Rents for the military elite are particularly decisive in autocratic regimes: the lack of legitimation by an electorate requires financially well-endowed military personnel (including paramilitary forces upon which autocrats often rely) to ensure loyalty towards the government and avoid military coups (Kimenyi and Mbaku 1995, Bove and Brauner 2016). Moreover, strong armed forces are required if the autocratic leader has to use military force against an opposition to stay in power and preserve the regime (Geddes et al. 2018, Blum 2018). Scholars have found empirical evidence for high levels of government spending in perfect democracies and perfect autocracies to finance either public goods for the electorate, or rents for the elite (Plümper and Martin 2003, Hausken et al. 2004). Democracies thus sustain high levels of civilian public goods spending at the expense of defense spending to ensure political support by the electorate, while non-democratic regimes finance rents for the (military) elites to ensure loyalty among the (military) elite.[2]

Apart from the differences in the government spending policies of democracies and autocracies, scholars in the field of International Relations have extensively discussed whether democracies are more peaceful and, thus, the perceived threat originating from democracies—especially towards other democracies—is reduced compared to autocracies. Immanuel Kant's (1795) work "Perpetual Peace" represents an early political and philosophical reasoning that the spread of liberalism fosters peace and lowers the relevance of armed forces. According to the Democratic Peace paradigm, democracies do not go to war against each other. Empirical evidence for this theory exists (Doyle 1983a, 1983b, Maoz and Russett 1993, Russett and O'Neal 2001); the relationship between democracy and conflict is more complex, however (see the reviews of Hegre 2014 and Gates et al. 1996).

[2] See also Wintrobe (1998) on the economics of autocratic regimes.

Scholars have estimated demand functions for national defense spending and found that strategic, political and socio-economic factors determine a country's demand for defense spending (Dunne and Perlo-Freeman 2003a, 2003b, Dunne et al. 2008, 2009, Fordham and Walker 2005, Collier and Hoeffler 2007, Goldsmith 2007, Rota 2011, Albalate et al. 2012, Brauner 2015, Töngür et al. 2015, Skogstad 2016, Yesilyurt and Elhorst 2017, George and Sandler 2018, Blum 2018).[3] Many of those studies control for democracy by means of the Polity IV index, and find a negative and statistically significant correlation between a country's Polity IV score and defense spending relative to GDP.

I estimate static and dynamic panel data models for a rich sample of 110 countries for the period 1972-2013, thus including the entire third wave of democratization. Democracy is hard to quantify, and different measures are not simply interchangeable and may well give rise to changing empirical results. I therefore apply four democracy measures to provide a comprehensive view on how political institutions influence national defense spending: the dichotomous democracy measure by Bjørnskov and Rode (2019), the Polity IV index by Marshall et al. (2018) and the dichotomous and continuous democracy measure by Gründler and Krieger (2016, 2018). The coefficient estimates for all four democracy measures indicate a significant negative impact of democracy on defense spending relative to GDP. The dichotomous democracy measures indicate that democracy's third wave decreased national defense spending relative to GDP by about 10% within countries that experienced democratization. Region-specific estimation results accounting for the sub-waves which reached different regions at different points in time, do not indicate that the impact of democracy's third wave on national defense spending relative to GDP has been heterogeneous across world regions. In a further step, I apply an IV strategy that exploits the regional diffusion of democracy in the context of the third wave of democratizations to overcome endogeneity

[3] See Blum (2018) for a detailed discussion of the cited studies.

problems. Apart from internal instruments applied in a GMM model (Dunne and Perlo-Freeman 2003b), this is the first IV approach estimating the effect of democracy on national defense spending. The IV estimates indicate an effect of democracy on national defense spending of about 20%. The OLS estimates resulting from non-instrumented democracy measures thus underestimate the effect of democracy on national defense spending. The results for the lagged values of the dependent variable in the dynamic panel data model yield a multiplier for the cumulative long-run effect of democracy which is 2.9. This long-run multiplier indicates that established democracies have about 30% less defense spending relative to GDP according to OLS results and more than 50% less defense spending relative to GDP according to IV results than if the respective nations were under autocratic rule.

In line with previous studies, I estimate the demand for national defense spending as a share of GDP to measure the financial endowment and capabilities of the armed forces relative to the size of a country. This measure does not account for differences in total government spending and budget composition between democracies and autocracies. Data on defense spending as a share of total government spending has, however, not been extended by SIPRI to years prior to 1988. If democratization increases total government spending (see Aidt and Jensen 2013) and simultaneously reduces defense spending relative to GDP, it is likely that the impact of democracy on defense spending relative to total government spending exceeds the impact of democracy on defense spending relative to GDP.

3.2 Data and descriptive findings

3.2.1 SIRPI data for defense spending 1972-2013

Data for military expenditure relative to GDP is provided by the Stockholm International Peace Research Institute (SIPRI). The figures are defined for calendar years. SIPRI has extended data on military expenditure to years prior to 1988 for a large number of countries. The within-variation of the data is much more reliable than the between-variation due to differences among

countries in accounting rules and items included in the calculation of the figures. A large share of these new observations for years prior to 1988 are, however, SIPRI estimates. SIPRI states that these estimates are constructed to match official data to the SIPRI definition if necessary, and "to combine overlapping sources of data that do not agree with each other". To construct more consistent time series, "one series is generally raised or lowered by a fixed percentage so as to make it consistent with the other in the year at which they are joined. In a few cases, estimates of the whole series are made, usually based on expert analyses, to obtain a series more consistent with SIPRI's definition."[4] Therefore, estimated data is not fully reliable for the empirical analysis. Countries for which data on military expenditure are estimates for more than half of the observation period, i.e. for more than 21 years, are therefore excluded from the sample, which halves the share of estimates. Observations that are labeled as being "highly uncertain" are excluded from the analysis.[5] The sample includes all countries from the first year onwards for which continuous data for military expenditure relative to GDP and continuous data for the explanatory variables is available. A total of 110 countries for the period 1972-2013 are thus included in the empirical analysis, resulting in 2,978 observations. The sample includes the United States, Canada, and Western European countries, which all experienced almost no variation in democracy, but decreased defense spending after the end of the Cold War, as a control group.

[4] See https://www.sipri.org/databases/milex/sources-and-methods and https://www.sipri.org/databases/milex/frequently-asked-questions, both accessed November 9, 2019.

[5] Pakistan, Bangladesh, India, Ethiopia, Rwanda, Nigeria, Nepal, Jordan and Ghana are main troop contributors to UN peacekeeping missions as of 2013. UN peacekeeping missions represent important sources of income for these countries and increase defense expenditure as described by SIPRI data. Since no data on troop contributions by country is available for the period 1972-2013 to explain peacekeeping-induced variation in defense spending, these countries are excluded. Furthermore, India and Pakistan became nuclear powers at some unobserved point in time during the observation period, which might have caused these countries to considerably decrease defense spending relative to GDP.

3.2.2 Democracy measures

I apply four democracy measures in the empirical analysis to address differences among democracy indices. First, the dichotomous democracy measure by Bjørnskov and Rode (2019), which is an update of the Democracy-Dictatorship dataset by Cheibub et al. (2010). Second, the Polity IV index by Marshall et al. (2018), which is an update of the Polity III dataset by Jaggers and Gurr (1995). Third, the Dichotomous Support Vector Machines Democracy Index (DSVMDI) and, fourth, the Continuous Support Vector Machines Democracy Index (CSVMDI) by Gründler and Krieger (2016, 2018), which are both based on machine learning techniques. The dichotomous democracy measure by Bjørnskov and Rode (2019) classifies a country as a democracy if both chief executive and legislature are popularly elected, more than one party competes in the elections, and the power has alternated under identical electoral rules to those when the predecessor assumed office (Cheibub et al. 2010: 69-71). Bjørnskov and Rode (2019) label regime changes to democracy or dictatorship for the respective year in case the change occurred in the first half of the year, and for the subsequent year otherwise. The Polity IV index by Marshall et al. (2018) is a composite index measuring autocracy and democracy on a scale ranging from –10 to +10. It combines two composite indices for autocracy and democracy, both ranging between zero and ten and both consisting of sub-indices. These sub-indices account for the competitiveness and openness of executive recruiting, the constraints on the chief executive and the competitiveness and regulation of political participation. The Polity IV index and its components are coded as of the end of the year. The dichotomous and the continuous democracy measures by Gründler and Krieger (2016, 2018) both range within an interval of zero and one. The two democracy measures are calculated by means of machine learning algorithms for pattern recognition, which learn from example inputs without being explicitly programmed. The underlying input attributes to describe democracy are political participation and political competition as core elements of democracy, as well as independence of the judiciary and freedom of the press (Gründler and Krieger 2016).

Democracy is difficult to quantify because of challenges in "conceptualization, measurement, and aggregation" (Munck and Verkuilen 2002), i.e. democracy measures are different in the elements they regard as crucial to describe a democracy, the way of measuring these elements and the way of aggregating them to one quantitative measure. Democracy measures are not simply interchangeable, and the choice of democracy measure can considerably affect empirical results (Cheibub et al. 2010).[6] Selecting the right democracy measure for a quantitative analysis is therefore of essential methodological interest. Huntington (1991b: 11) prefers a dichotomous approach to describe the third wave of democracy. A dichotomous democracy measure provides a clear-cut definition and is easy to interpret in the empirical analysis. A continuous democracy measure, in turn, may be more precise in measuring democracy, and accounts for the process in which political institutions develop and democracies emerge. In terms of measurement error in democracy measures, a continuous measure implies many small errors, while a dichotomous measure implies few large errors (Alvarez et al. 1996).

Measurement error gives rise to biased estimates and if the bias is caused by a systematic, non-random measurement error, it cannot be solved by instrumental variables (Gründler and Krieger 2018, 2019). Such systematic measurement error is supposed to be ruled out if machine learning algorithms are applied for measuring democracy as stated by Gründler and Krieger (2016, 2018). The alternation rule in the measurement of democracy by Bjørnskov and Rode (2019) (i.e. that the power needs to have alternated in accordance with democratic rules before a country is described as a democracy) implies a non-random measurement error rooted in the conceptualization of democracy. The strength of political institutions is

[6] The differences among the four democracy measures are tangible: a comparison between the dichotomous democracy measure by Bjørnskov and Rode (2019) and the dichotomous democracy measure by Gründler and Krieger (2016, 2018) shows considerable deviations especially for African and Asian-Pacific countries. Contingent on a threshold Polity IV score above which a country is indicated as a democracy, about 10% of the information from the Polity IV measure disagrees with the dichotomous democracy measure by Bjørnskov and Rode (2019). See Potrafke (2012, 2013) on how using the measure by Cheibub et al. (2010) changes established results and Gründler and Krieger (2016) on how machine learning based democracy measures resolve ambiguity about the relationship between democracy and economic growth.

underestimated for young democracies that did not yet experience a change of government, thus leading to an underestimation of the effect of democracy (Knutsen and Wig 2015). The most common source of a systematic, non-random measurement error lies in the aggregation of additive sub-components of democracy, particularly if the sub-components receive equal weights. Hence, most measurement errors lead to an underestimation of changes in political institutions and to an overestimation of the effect of democracy (Gründler and Krieger 2016, 2018, 2019). This systematic measurement error applies to the Polity IV index by Marshall et al. (2018). The reliability of the Polity IV index is, however, also contested for other reasons: the index is neither continuous nor cardinal, and owing to its composite nature, identical scores can result from numerous different combinations of the democracy and the autocracy index and their sub-indices. The bimodal distribution of the Polity IV index with peaks at very high and very low levels, moreover, casts doubt on whether this index contains more information than a dichotomous index (Vreeland 2008, Cheibub et al. 2010).[7] A threshold value for the Polity IV index above which a country is described as a democracy is, in turn, always arbitrary. Most studies estimating demand functions for national defense spending apply the Polity IV index to control for political institutions. I therefore apply the Polity IV index as a measure of democracy to ensure that the empirical results can be compared with previous research.

3.2.3 Descriptive findings on democracy's third wave and defense spending

The third wave of democracy started with the democratization of Portugal after the Carnation Revolution in 1974, and with Spain and Greece becoming democracies in the mid-1970s. It continued in Latin America in the 1980s with the democratization of countries like Argentina and Chile. The fall of the Iron Curtain triggered democratization in Eastern European countries such as Hungary and Poland in the early 1990s. Democratizations in African countries like

[7] Following the findings of Vreeland (2008) that sub-indices of the Polity IV index are closely related to civil war, the Polity IV index might even confound the effect of democracy on defense spending with the effect of civil war on defense spending.

Kenya and Senegal as well as in Asian-Pacific countries like the Philippines and South Korea are also attributed to the third wave (Huntington 1991a, 1991b). Figure A3.1 in Appendix I shows two maps which indicate how the spread of democracy during the third wave changed the world's political landscape between 1972 and 2013. Reverse transitions during which some democracies relapsed into autocratic regimes marked the end of the first and second wave of democratization. However, by the time Huntington defined the three waves of democracy, the third wave was at its peak and it was not possible to foresee reverse transitions to mark the end of this third wave. The relapses of democracies such as Turkey and Venezuela into autocratic regimes in the mid-2010s might also mark the end of this third wave. The observation period from 1972 to 2013 thus ensures that the entire third wave of democracy is covered in the empirical analysis.

Figure 3.1 shows national defense spending relative to GDP, the dichotomous democracy measure by Bjørnskov and Rode (2019) and the Polity IV index for selected countries. Portugal remarkably decreased defense spending relative to GDP after the coup d'état and the subsequent transition to democracy from about 5% to less than 3%. Argentina reached defense spending relative to GDP of almost 5% during the rule of Perón and the subsequent military regime which lasted until 1983; defense spending decreased following democratization. The negative impact of democracy on defense spending is also visible for Chile following the end of the military dictatorship of Pinochet and for Uruguay after 1985; during the military regime in both countries, defense spending relative to GDP had reached values above 5% with a maximum of almost 9% for Chile in 1982. Albania, Hungary and Romania decreased defense spending relative to GDP after becoming democracies. Indonesia already decreased defense spending in the run-up to democratic transition and retained low levels after becoming a democracy. Malawi retained low levels of defense spending relative to GDP after democratic transition, too. For the discussed countries, periods of democratic regime

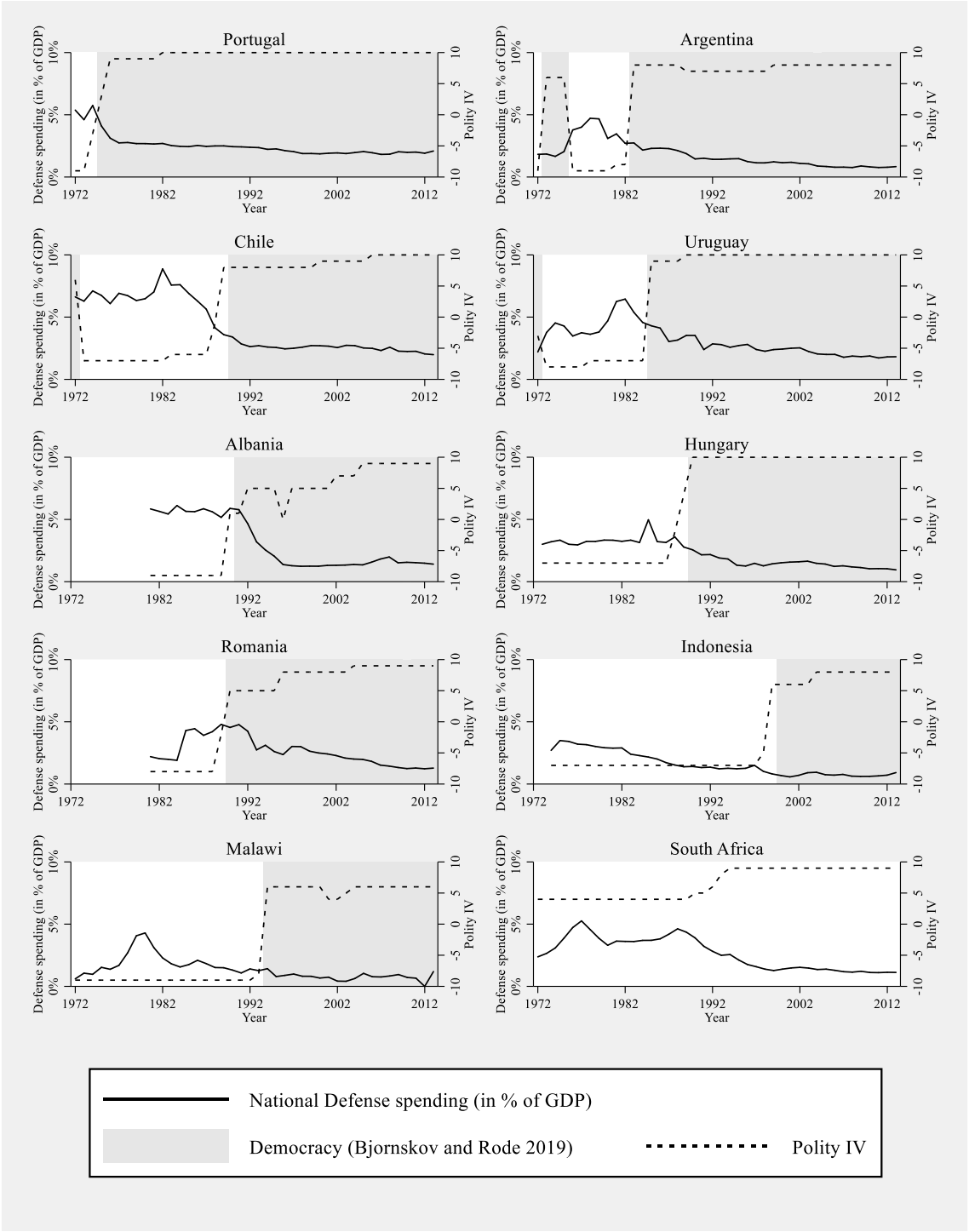

FIGURE 3.1: NATIONAL DEFENSE SPENDING AND DEMOCRACY IN SELECTED COUNTRIES

according to the dichotomous democracy measure are accompanied with considerably high levels of the Polity IV index and vice versa. South Africa is, however, not classified as a democracy according to the dichotomous measure despite the increase to a Polity IV score of +9 which coincided with a decrease in defense spending. Figures for North American and

Western European countries as well as for countries in the Middle East are not shown because these countries experienced almost no variation in the two democracy measures.

Table A3.1 in Appendix I lists the 110 countries in the sample, shows the first and last year of the continuous time series for each country and indicates whether a country experienced variation in the four democracy measures during the observation period.

3.3 Empirical analysis

3.3.1 Dynamic panel data model

I estimate a dynamic panel data model to examine how national defense spending is influenced by democracy. Persistence in defense spending and potential correlation between democracy and national defense spending prior to democratic transition require a panel model which includes dynamics of the dependent variable and thus accounts for pre-transition dynamics.[8] For instance, the denominator of the dependent variable, GDP, has been shown to experience a dip prior to democratization (Brückner and Ciccone 2011), which might give rise to pre-transition correlation between democracy and defense spending relative to GDP. The baseline model looks as follows:

$$y_{it} = \sum_{j=2}^{j=5} \beta_j \, y_{it-j} + \, \mu \, D_{it-1} + \boldsymbol{x_{it-1}\delta} + \, \eta_i + \gamma_t + \varepsilon_{it} \qquad (1)$$

The dependent variable y_{it} describes the natural logarithm of national defense spending relative to GDP for country i in year t. Using the natural logarithm of defense spending relative to GDP as the dependent variable allows to interpret the estimated coefficients as elasticities. The model follows Hamilton (2018) and Acemoglu et al. (2019) and includes four lags of the dependent

[8] Acemoglu et al. (2019) employs a similar dynamic panel data model to estimate the effect of democracy on economic growth.

variable before regime transition. These four lags describe national defense spending relative to GDP for the period from $t-5$ to $t-2$ before regime transition. The lag structure of the dependent variable starts with the second instead of the first lag because the four democracy measures are all lagged by one year. An equivalent one year time lag of the dependent variable as a regressor would otherwise (depending on the timing of the transition within the year and the adjustment duration of the defense budget) be likely to correlate with the one year time lag of regime transition. The dynamic panel data model includes four lags of the dependent variable for two reasons: First, sequential exogeneity—the standard assumption for linear dynamic panel data models—needs to be fulfilled. This assumption is less demanding than strict exogeneity, which is violated once a lagged dependent variable is included in the regression. Sequential exogeneity requires that democracy and past levels of defense spending are orthogonal to current and future shocks to national defense spending as well as that the error term ε_{it} is not serially correlated. A lag structure of national defense spending that includes a sufficiently long pre-transition period accounts for dynamics in national defense spending which may influence the likelihood of regime transition. A sufficiently large lag structure of the dependent variable, moreover, rules out serial correlation in the error term. Second, consistent estimates require that conditional on fixed effects and control variables, national defense spending relative to GDP and each of the democracy measures follow stationary processes. National defense budgets are likely to be persistent and scholars have shown that past values of defense spending relative to GDP explain their current values (Dunne and Perlo-Freeman 2003b, Rota 2011). Including four lags of the dependent variable creates stationary time series with high probability (Hamilton 2018). The inclusion of lagged dependent variables gives rise to biased within-estimates with an asymptotic bias of order $1/T$ since the strict exogeneity assumption does not hold in dynamic panel data models (Nickell 1981). However, with an average of 31 year-observations per country estimated in the dynamic panel data model, this bias is supposed to be rather small.

I separately include four democracy measures D_{it-1} in the dynamic panel data model: the Bjørnskov and Rode (2019) dichotomous democracy measure *Democracy*$_{it-1}$, the Polity IV index *Polity IV*$_{it-1}$ by Marshall et al. (2018), the Dichotomous Support Vector Machines Democracy Index *DSVMDI*$_{it-1}$ and the Continuous Support Vector Machines Democracy Index *CSVMDI*$_{it-1}$—both by Gründler and Krieger (2016, 2018).[9] Each of the democracy measures enters the regression with a one year time lag to take account of a reaction time until budgetary adjustments become effective, as well as to mitigate possible problems of reverse causality between democracy and national defense spending. Given the coding rule of the dichotomous democracy measure by Bjørnskov and Rode (2019), this one year time lag effectively allows for a transition phase between more than half a year and less than one and a half years after the regime change until defense spending is adjusted. The time lag of the Polity IV variable effectively codes the Polity IV index as of the beginning of the year, and allows for at least one calendar year after a change in the Polity IV score until budgetary adjustments become effective.

The dynamic panel data model includes both country fixed effects η_i and year fixed effects γ_t. The empirical model thus exploits the within variation in national defense spending. The within-analysis is favorable because, as mentioned earlier, SIPRI data is more reliable over time than across countries. Year fixed effects account for global trends in defense spending which are particularly pronounced from the Cold War to the post-Cold War era. The standard errors ε_{it} are clustered at the country level and robust to serial correlation and heteroskedasticity (Huber/White/sandwich standard errors; see Huber 1967 and White 1980).

The $1 \times K$ vector $\boldsymbol{x_{it-1}}$ contains five control variables, which are all lagged by one year. The parameters of the control variables are included in the $K \times 1$ vector $\boldsymbol{\delta}$. The set of control variables includes two strategic and three socio-economic variables: *War*$_{it-1}$ is a dummy

[9] I apply the variable "Polity2" from the Polity IV dataset which prorates the Polity IV index for the duration of interregnum periods.

variable which indicates whether a country has been involved in an interstate war (i.e. a war with another country), or an internal war (i.e. a war between a government and internal conflict groups) in year $t-1$ with at least 25 battle-related deaths. The time lag for measuring the impact of war reflects that national defense spending increases with a time lag once a country is involved in a violent conflict, and decreases with a time lag once a conflict has ended because the country then needs to demobilize and replenish military resources. Data for armed conflicts is taken from the "UCDP/PRIO Armed Conflict Dataset" by Gleditsch et al. (2002) (Version 17.2). *Internal threat*$_{it-1}$ describes a country's internal stability and the probability of a domestic conflict, which is proxied by means of an eleven-point index for internal violence that is lagged by one year. Data on internal violence is taken from the "Major Episodes of Political Violence (MEPV) and Conflict Regions, 1946-2016" dataset (Version July 25, 2017). Few studies have controlled for internal threat but both variables *War*$_{it-1}$ and *Internal threat*$_{it-1}$ have shown to explain variance in national defense spending relative to GDP (Blum 2018, Blum and Potrafke 2019). *GDP*$_{it-1}$ describes the natural logarithm of GDP in constant (2010) US dollars in year $t-1$ to investigate possible income or substitution effects (Dunne et al. 2008, Albalate et al. 2012, Blum 2018). GDP also needs to be controlled for because transitions to democracy can cause or at least be accompanied by higher economic growth (Acemoglu et al. 2019, Papaioannou and Siourounis 2008). *Population*$_{it-1}$ describes the natural logarithm of a country's population in year $t-1$. The data for GDP and Population is taken from the World Development Indicators of the World Bank.[10] Scholars have controlled for trade to detect peaceful effects of economic integration or increased defense spending to protect trade routes. The results on trade are, however, mixed (Dunne and Perlo-Freeman 2003b, Dunne et al. 2008, Blum 2018). *Trade globalization*$_{it-1}$ controls for the level of trade in year $t-1$ to detect possible

[10] Data for GDP is not available for the entire observation period for Hungary, Poland and Romania. SIPRI data for military expenditure in levels and in shares of GDP have therefore been used to construct GDP figures for those countries. This procedure is intended to accurately capture variation in GDP over time for those countries. GDP data from the World Bank should, however, not be compared with GDP data compiled from SIPRI figures; this issue is, however, mitigated because the fixed effects model exploits the within-variation of countries only.

attenuation effects of trade on national defense spending. This variable measures the de facto globalization in terms of international trade and considers trade in goods, trade in services and trade partners' diversification. The data on trade globalization is taken from the KOF Globalization Index (Dreher 2006, Gygli et al. 2019). Table A3.2 and A3.3 in Appendix I show summary statistics and correlations for all variables applied in the estimation.

3.3.2 Estimation results

I estimate the panel model in three steps. First, I estimate equation (1) without lags of the dependent variable, i.e. a static panel data model, and without control variables. The static panel data model allows to include 110 countries with a total of 2,978 observations. Panel A of Table 3.1 shows estimation results for the four democracy measures with country and year fixed effects only. The correlation of the four democracy measures conditioned on country and year fixed effects yields negative estimates for all democracy measures. The Bjørnskov and Rode (2019) dichotomous democracy measure (*Democracy*) in column (1) is statistically significant at the 5% level and indicates that democracy decreases national defense spending relative to GDP by 18% within countries.[11] The three other democracy measures in columns (2) to (4) are statistically significant at the 1% level. A one-point change in a country's *Polity IV* score is associated with a decrease in national defense spending relative to GDP by 2.2%. The impact of democracy is 22% for the Dichotomous Support Vector Machines Democracy Index (*DSVMDI*) and—assuming a hypothetical change from zero to one—30% for the Continuous Support Vector Machines Democracy Index (*CSVMDI*).

Second, I add control variables to the static panel data model in panel B of Table 3.1. The coefficient estimates for the four democracy measures do hardly change in both size and statistical significance. A war turns out with a positive coefficient estimate which is significant

[11] Note that the percentage impact of democracy, i.e. when the democracy dummy switches to one, is calculated as $100[\exp(-0.196) - 1] = -17.8$.

at the 1% level in columns (1) through (4). The increase in defense spending relative to GDP during wartime—an impact of 26% according to the coefficient estimate in column (1)—shows the economic significance of the impact of democracy: the negative impact of democracy on national defense spending relative to GDP according to the two dichotomous democracy measures in columns (1) and (3) is almost as large as the positive impact of a war. The impact of war on national defense spending further illustrates the "peace dividend", i.e. the decreased level of defense spending during the absence of armed conflicts. Internal threat and the coefficients of the three socio-economic variables for GDP, population and trade globalization

TABLE 3.1: ESTIMATION RESULTS FOR THE STATIC PANEL DATA MODEL

National defense spending (in % of GDP)[a]	(1)	(2)	(3)	(4)
Panel A: Without control variables				
Democracy (t – 1)	-0.196**			
	(0.095)			
Polity IV (t – 1)		-0.022***		
		(0.007)		
DSVMDI (t – 1)			-0.250***	
			(0.069)	
CSVMDI (t – 1)				-0.350***
				(0.098)
Country Fixed Effects	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes
Observations	2,978	2,978	2,978	2,978
Countries	110	110	110	110
R^2 Overall	0.125	0.158	0.134	0.143
R^2 Within	0.197	0.210	0.207	0.206
R^2 Between	0.061	0.098	0.113	0.121
Panel B: With control variables				
Democracy (t – 1)	-0.216**			
	(0.098)			
Polity IV (t – 1)		-0.023***		
		(0.007)		
DSVMDI (t – 1)			-0.240***	
			(0.071)	
CSVMDI (t – 1)				-0.358***
				(0.098)
War (t – 1)	0.235***	0.233***	0.211***	0.229***
	(0.062)	(0.066)	(0.060)	(0.063)
Internal threat (t – 1)	0.021	0.013	0.018	0.016
	(0.024)	(0.022)	(0.021)	(0.022)
GDP[a] (t – 1)	-0.136	-0.164	-0.152	-0.166
	(0.163)	(0.162)	(0.160)	(0.164)
Population[a] (t – 1)	0.138	0.193	0.196	0.210
	(0.272)	(0.272)	(0.271)	(0.274)
Trade globalization (t – 1)	0.003	0.002	0.003	0.002
	(0.004)	(0.004)	(0.004)	(0.004)
Country Fixed Effects	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes
Observations	2,978	2,978	2,978	2,978
Countries	110	110	110	110
R^2 Overall	0.060	0.064	0.063	0.060
R^2 Within	0.217	0.227	0.223	0.224
R^2 Between	0.002	0.005	0.009	0.008

Notes: Standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively. All regressions apply standard errors clustered at the country level. [a] denotes variables which are expressed in their natural logarithm.

do not turn out to be statistically significant. The lack of statistical significance for GDP and population contradicts earlier findings (Collier and Hoeffler 2007, Dunne et al. 2008, Blum 2018). The result for GDP suggests that no substitution effect exists when GDP increases, which means that defense spending increases in proportion to GDP.

Third, I include the four lags of the dependent variable and thus estimate the dynamic panel data model as described by equation (1). The lag structure reduces the number of countries from 110 to 95 (the number of countries is reduced by those countries with too short observation periods) and the total number of observations from 2,978 to 2,455. Table 3.2 shows that compared to Table 3.1, the coefficient estimates of all democracy measures are about half the size, but still statistically significant at the same levels once dynamics of the dependent variable are included in the model. The impact of democracy on national defense spending relative to GDP is reduced to 9% (*Democracy*), 1.1% (for a one-point increase in *Polity IV*), 12% (*DSVMDI*) and 16% (*CSVMDI*; for a hypothetical change from zero to one). Likewise, the coefficient estimates for a war are approximately halved in size. A Wald test does not reject the null hypothesis of equality of the two dichotomous democracy measures (columns 1 and 3). In columns (1) through (4), the second lag of the dependent variable is positive and statistically significant at the 1% level and the fourth and fifth lag of the dependent variable are statistically significant at the 5% level; however, the fourth lag has a negative sign. The results for the second lag of the dependent variable—a coefficient of 0.672 in column (1)—indicate considerable persistence in national defense spending relative to GDP and show that empirical models estimating demand functions for national defense spending should account for the dynamics in national defense spending. The results for the lag structure of the dependent variable yield a multiplier for the cumulative long-run effect of democracy of 2.9.[12] The implied

[12] Note that the cumulative long-run effect of democracy is calculated as $\hat{\mu} * (1 - \sum_{j=2}^{5} \widehat{\beta_j})^{-1}$ with $\hat{\mu}$ being the parameter estimate for the democracy measure, $\hat{\beta}_j$ being the parameter estimate for the j[th] lag of the dependent variable and $(1 - \sum_{j=2}^{5} \widehat{\beta_j})^{-1}$ describing the long-run multiplier for parameter estimates in a dynamic panel data model (see Acemoglu et al. 2019). The long-run multiplier according to column (1) is thus calculated as $(1 - (0.672 - 0.011 - 0.029 + 0.027))^{-1} = 2.9$

long-run effect of democracy in the dynamic panel data model of about 30% according to the two dichotomous democracy measures thus even exceeds the coefficient estimates for democracy in the static panel data model (Table 3.1).[13]

TABLE 3.2: ESTIMATION RESULTS FOR THE DYNAMIC PANEL DATA MODEL

National defense spending (in % of GDP)[a]	(1)	(2)	(3)	(4)
Democracy (t – 1)	-0.097**			
	(0.047)			
Polity IV (t – 1)		-0.011***		
		(0.003)		
DSVMDI (t – 1)			-0.123***	
			(0.033)	
CSVMDI (t – 1)				-0.175***
				(0.038)
War (t – 1)	0.123***	0.125***	0.115***	0.124***
	(0.034)	(0.036)	(0.034)	(0.035)
Internal threat (t – 1)	0.001	-0.003	-0.001	-0.001
	(0.006)	(0.005)	(0.005)	(0.005)
GDP[a] (t – 1)	0.066	0.050	0.054	0.046
	(0.052)	(0.053)	(0.053)	(0.054)
Population[a] (t – 1)	-0.025	0.010	0.008	0.018
	(0.134)	(0.132)	(0.134)	(0.135)
Trade globalization (t – 1)	0.000	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
National defense spending (% of GDP)[a] (t – 2)	0.672***	0.660***	0.661***	0.660***
	(0.059)	(0.058)	(0.058)	(0.059)
National defense spending (% of GDP)[a] (t – 3)	-0.011	-0.013	-0.008	-0.009
	(0.057)	(0.057)	(0.056)	(0.057)
National defense spending (% of GDP)[a] (t – 4)	-0.029**	-0.030**	-0.030**	-0.030**
	(0.013)	(0.013)	(0.013)	(0.013)
National defense spending (% of GDP)[a] (t – 5)	0.027**	0.028**	0.028**	0.028**
	(0.011)	(0.011)	(0.011)	(0.011)
Country Fixed Effects	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes
Observations	2,455	2,455	2,455	2,455
Countries	95	95	95	95
R^2 Overall	0.856	0.854	0.849	0.850
R^2 Within	0.643	0.647	0.647	0.646
R^2 Between	0.932	0.928	0.922	0.924

Notes: Standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively. All regressions apply standard errors clustered at the country level. [a] denotes variables which are expressed in their natural logarithm.

3.3.3 Robustness tests

Several robustness tests confirm the empirical results on democracy and national defense spending. First, the lags of the dependent variable from $t-5$ to $t-2$ reduce the number of observations per country, and eliminate countries with too short observation periods in the dynamic panel data model. I therefore replicate Table 3.1 with the same 95 countries employed

[13] A Hausman test for a fixed effects versus a random effects model confirms that the fixed effects model is the proper model of choice as opposed to a random effects model.

in the dynamic panel data model in Table 3.2. Table A3.4 in Appendix II shows that the estimation results are almost unchanged.[14]

Second, the observation period includes the period after the 2007/2008 financial crisis, which has shown to considerably influence empirical results (Eberhardt 2019). I therefore estimate the dynamic panel data model for years until 2006, i.e. exclude years after 2006, to rule out that the empirical results are sensitive to sample selection. Table A3.5 in Appendix II shows that the empirical results for the dynamic panel data model hardly change though the number of observations is considerably reduced from 2,455 to 1,828.

Third, countries did not exclusively experience transition from autocracy to democracy; they also experienced transition from democracy to autocracy or even several transitions from and to democracy and autocracy. I therefore exclude countries with reverse transitions, i.e. transition from democracy to autocracy. For countries with multiple transitions which later turned to a democracy, I exclude the period of the reverse transition and only include the period from the last autocratic regime onwards. This sample with fewer countries and observations allows to confirm that the effect of democracy on defense spending is indeed an effect of transition towards democracy, rather than an effect of increased defense spending after transition to autocracy. Table A3.6 in Appendix II shows that the estimation results for the dynamic panel data model hardly change.

Fourth, the type of autocratic regime prior to democratization might influence the estimation results because of considerable differences among different types of autocratic regimes. Two kinds of autocratic regimes need to be examined in more detail because most countries during the third wave experienced transition towards democracy initiating from these regime types: military regimes and communist regimes. Military dictatorships have shown to voluntarily hand over their power to civilian governments and—unlike civilian autocracies—

[14] The number of observations in Table A3.4 in Appendix II is larger than in Table 3.2 because the absence of lagged dependent variables allows to include more year observations for each country.

may have even planned democratization (Wintrobe 1990, Bjørnskov 2019). Military regimes may well influence the development of political institutions on the road towards democracy and ensure that the military elite maintains its rents and privileges. The impact of democratization on national defense spending is thus likely to be reduced for military autocracies compared to civilian autocracies which experience democratization. I therefore estimate the dynamic panel data model excluding all countries that have ever been military dictatorships during the observation period. Table A3.7 in Appendix II shows that the dichotomous democracy measure by Bjørnskov and Rode (2019) does no longer turn out to be statistically significant. The coefficient estimates for the other three democracy measures are larger compared to the baseline results in Table 3.2 and confirm that military dictatorships which turn into a democracy decrease national defense spending to a lesser extent than civilian autocracies experiencing democratization. Communist regimes might have higher government spending in general compared to other autocratic regimes because of a larger government size and the government's interference with the economy. The effect of democracy on defense spending relative to GDP might therefore be driven by a decline in government spending in general. I therefore estimate the dynamic panel data model excluding all countries that have ever been communist autocracies during the observation period. Table A3.8 in Appendix II shows that the dichotomous democracy measure by Bjørnskov and Rode (2019) is statistically significant at the 10% level only, however, the coefficient estimates for the other three democracy measures do hardly change.[15] This result complies with the finding that government size in communist countries is not likely to differ considerably from other autocratic regimes (Kammas and Sarantides 2019).[16]

[15] Note that military dictatorships and communist regimes are not mutually exclusive. Countries like Albania, Poland and Laos were military and communist regimes at the same time according to Bjørnskov and Rode (2019).
[16] Government ideology has been shown to influence defense spending (Whitten and Williams 2011, Bove et al. 2017, Potrafke 2020). Government ideology of the chief executive (taken from the Database of Political Institutions) as an additional control variable does, however, not turn out to be statistically significant and does not change the results on democracy. Controlling for government ideology implies a considerably limited sample since many governments cannot be categorized by means of leftwing-rightwing patterns (results not reported).

Fifth, the dynamic panel data model includes four lags of the dependent variable, thus following Acemoglu et al. (2019) and Hamilton (2018), to account for possible pre-transition correlation between democracy and defense spending. The time horizon of such pre-transition correlation might, however, be differently specified with regard to defense spending dynamics. I therefore examine whether the estimation results change once dynamics of defense spending relative to GDP are increased or reduced. The estimation results for the four democracy measures hardly change for extended dynamics in defense spending up to the tenth lag. When the lag structure is reduced to one, two, or three lags, the dichotomous democracy measure by Bjørnskov and Rode (2019) no longer turns out to be statistically significant, while the results for the remaining democracy measures remain robust. This result also holds once the lag structure of defense spending starts with the first instead of the second lag (results not reported).

3.3.4 Regional effect heterogeneity

Section 3.3.3 shows that military regimes and communist regimes experiencing transition towards democracy only slightly differ from other pre-transition regime types regarding how democratization affects national defense spending. The impact of democracy on national defense spending has, however, been shown to be heterogeneous across countries (Blum 2018). With regard to the third wave, the effect of democracy on defense spending might be heterogeneous across the sub-waves that reached different regions at different points in time. I estimate interaction terms of the four democracy measures with seven geopolitical world regions in the dynamic panel data model jointly with the respective non-interacted democracy measure for all countries. The results thus allow to infer whether the effect of democracy for one of the seven regions deviates from the democracy effect estimated across all countries in the sample, i.e. whether region-specific heterogeneity exists.

Table A3.9 in Appendix II shows the estimation results. The dynamic panel data models estimated in panels A through G include all 95 countries that can be included in the dynamic

analysis. In each panel, the interaction between one world region with the four democracy measures is estimated. The coefficient estimates for the non-interacted four democracy measures in panels A through G are similar to the results shown in Table 3.2. For countries in Latin America and Eastern Europe, however, the Bjørnskov and Rode (2019) dichotomous democracy measure does no longer turn out to be statistically significant (column 1 of panels C and D). This result indicates that the statistical significance of this democracy measure shown in Table 3.2 is partially attributed to democratizations in Latin American and Eastern European countries. The negative though not statistically significant interaction terms for all four democracy measures in panel D, moreover, indicate that the negative impact of democracy on national defense spending is somewhat more pronounced for Eastern European countries. The positive though only weakly statistically significant interaction terms for the democracy measures in panel E, in turn, indicate that the negative impact of democracy is somewhat less pronounced for Southern European countries.[17] For the other world regions, however, the interaction terms of the four democracy measures with each region have both positive and negative signs (panels A, B, C, and G) from columns (1) to (4). Yet the interaction terms almost never reach statistical significance. The results do not show a distinct pattern in region-specific deviations from the estimated general effect of democracy shown in Table 3.2. Considerable region-specific heterogeneity in the effect of democracy on national defense spending is, therefore, unlikely to exist for the third wave of democratization.

[17] The positive and statistically significant interaction terms for countries in Western Europe and North America in columns (2) and (4) of panel F are attributed to minor changes in the Polity IV score for Belgium and the United States, and minor changes in the continuous democracy measure by Gründler and Krieger (2016, 2018). No coefficient estimates for the dichotomous democracy measures are estimated for countries in Western Europe and North America (columns 1 and 3 of panel F), because these countries did not experience any variation in these two democracy measures between 1972 and 2013.

3.4 Instrumental variable (IV) approach

3.4.1 IV strategy and exclusion restriction

The results for all four democracy measures in Tables 3.1 and 3.2 corroborate the considerations from Section 3.1 that the third wave of democratizations decreased national defense spending relative to GDP within countries that experienced democratization. The estimated impact of democracy on national defense spending might, however, be biased if democracy is endogenous. First, unobserved developments prior to democratic transition might drive both democratization and defense spending cuts within a country and give rise to omitted variable bias. Second, reverse causality might further give rise to endogeneity if the size of the military sector influences the chances for a regime change. The dynamic panel data model which includes country fixed effects and dynamics of the dependent variable accounts for time-invariant country characteristics and possible pre-transition correlation between democracy and defense spending. However, a remaining source of endogeneity bias that the dynamic panel data model cannot rule out relates to time-variant unobservables. Third, measurement error in democracy indices is likely because—as discussed in Section 3.2.2—democracy is difficult to quantify. To overcome these endogeneity concerns and yield consistent estimates for the effect of democracy, I apply an IV strategy that exploits regional sub-waves in the context of democracy's third wave as an instrumental variable for democracy. This IV strategy alleviates concerns regarding omitted variable bias and reverse causality as well as biases resulting from random measurement error. Nonetheless, biases resulting from systematic, non-random measurement error are not remedied by means of an instrumental variable.

The third wave of democracy offers a suitable setting for this IV strategy because the third wave proceeded in regional waves from Southern Europe in the mid-1970s via Latin America in the 1980s to Eastern Europe in the early 1990s, also hitting countries in Africa and Asia in cohesive patterns. Acemoglu et al. (2019) use this IV strategy to estimate the causal effect of democracy on growth and argue that "this regional pattern reflects the diffusion of the

demand for democracy […] across countries within a region, which tend to have similar histories, political cultures, practical problems, and close informational ties." I therefore treat the regional sub-waves of democracy's third wave as a "source of exogenous variation in democracy" (Acemoglu et al. 2019). I construct jackknifed democracy scores for the four democracy measures in order to describe democracy's regional diffusion. Each country i is therefore allocated to a geopolitical region R_i together with other countries $\tilde{\imath}$ which are in geographic proximity and share similar cultures and histories. For each country i, the set $L_i = \{\tilde{\imath} : \tilde{\imath} \neq i, R_{\tilde{\imath}} = R_i\}$ describes all other countries $\tilde{\imath}$ in the same region whose democracy is likely to influence democracy in country i. The jackknifed democracy instrument JDI_{it} for country i in year t is calculated as the average democracy score of the countries in set L_i, i.e. of all other countries $\tilde{\imath}$ in country i's region R_i except the democracy score of country i itself ("jackknifed" averages):

$$JDI_{it} = \frac{1}{|L_i|} \sum_{\tilde{\imath} \in L_i} D_{it} \tag{2}$$

The just-identified two-stage-least-squared (2SLS) model follows the dynamic panel data model and applies the jackknifed democracy score as an instrument for democracy:

$$D_{it-1} = \sum_{j=2}^{j=5} \lambda_j \, y_{it-j} + \, \theta \, JDI_{it-2} + \boldsymbol{x_{it-1}\pi} + \, \sigma_i + \tau_t + \nu_{it} \tag{3}$$

$$y_{it} = \sum_{j=2}^{j=5} \beta_j \, y_{it-j} + \, \mu \, \widehat{D}_{it-1} + \boldsymbol{x_{it-1}\delta} + \, \eta_i + \gamma_t + \varepsilon_{it} \tag{4}$$

Equation (3) describes the first-stage regression which applies the jackknifed democracy score of one of the four democracy measures as an instrumental variable for the respective democracy score. The jackknifed democracy instrument is lagged by one year behind the democracy measure which is instrumented (i.e. democracy in $t-1$ is predicted by means of jackknifed democracy in $t-2$) because an increase in the regional diffusion of democracy is unlikely to instantly translate into an increased demand for democracy in a nearby located autocracy. Equation (4) describes the second stage that employs the instrumented democracy measure.

A valid instrumental variable needs to be relevant for describing the instrumented variable and has to fulfill the exclusion restriction. Though the relevance of the instrumental variable can be confirmed in the first-stage regression, the exclusion restriction cannot be empirically tested. The exclusion restriction is not fulfilled if the jackknifed democracy instrument influences national defense spending in country i through channels other than democracy in country i. In the following, I address two channels which are the most severe threats to the exclusion restriction: direct effects on defense spending stemming from democracy's regional diffusion and spatial dependence in defense spending.

The most obvious threat to the excludability of the instrumental variable is that the regional average level of democracy directly influences a country's defense spending relative to GDP because the perceived threat originating from democracies in the neighborhood is lower than the perceived threat originating from autocracies. According to the Democratic Peace paradigm, however, democracies are only less likely to wage war against each other—not against autocracies. The Democratic Peace paradigm does therefore not apply to autocracies prior to their democratic transition even if they are surrounded by democracies. The effect of democratic peace is a downstream effect after democratization and does—from a theoretical viewpoint—not violate the exclusion restriction. Once included in the dynamic panel data model, the jackknifed democracy scores for each democracy measure do not turn out to be statistically significant (results not reported).

Spatial dependences in defense spending would furthermore clearly violate the exclusion restriction as the regional diffusion of democracy would influence defense spending in country i not exclusively via the channel of democracy in country i itself, but also via decreased defense spending within the respective region. Scholars have shown that defense spending of neighboring countries or countries located in the same region as well as the defense spending of rivals influence a country's own level of defense spending (Dunne and Perlo-Freeman 2003a, 2003b, Dunne et al. 2008, 2009, Collier and Hoeffler 2007, Albalate et al. 2012).[18] This largely spatial relationship motivated the use of spatial lag models for estimating demand functions for defense spending. Scholars found empirical evidence for spatial dependence of national defense spending relative to GDP among countries both in cross-country analyses (Goldsmith 2007) and panel data models (Skogstad 2016, Yesilyurt and Elhorst 2017, George and Sandler 2018, Blum 2018). Though spatial dependence has been found in spatial panel data models, the model specifications in these studies differ from the model applied in my analysis: except of the analysis by Yesilyurt and Elhorst (2017), the empirical models neither include dynamics of the dependent variable to account for persistence in defense spending, nor fixed year effects to absorb common shocks. I therefore augment my dynamic panel data model by spatial dependences among countries and examine whether spatial correlation in national defense spending exists in a dynamic panel data model with both country and year fixed effects, and conditioned on strategic and socio-economic control variables. I apply a spatial autoregressive (SAR) model, which in a Bayesian model comparison has shown to be superior to other spatial lag models when demand functions for national defense spending are estimated (Yesilyurt and Elhorst 2017). The SAR model has also been applied in most previous studies in this field (Goldsmith 2007, Skogstad 2016, George and Sandler 2018). Unlike the Spatial Durbin Model (SDM), the SAR model assumes that the

[18] These findings go back to the security web concept of Rosh (1988).

spatial lags of the explanatory variables do not turn out to be jointly significant.[19] Previous research corroborates this assumption because spatial lags of the determinants of defense spending have shown to be hardly significant (Blum 2018). Since a spatial panel model requires a strongly balanced panel, I employ two balanced panels: one for 40 countries for the entire observation period 1972-2013 and one for the period 1981-2013, which allows to include 53 countries. The 13 additional countries include further Eastern European countries since only data for Hungary is available from 1972 onwards. Due to the limited number of countries included in the spatial analysis, I apply an inverse distance matrix for the spatial weighting of observations. This matrix describes the inverse distance between the capitals of all countries included in the sample. The matrix, thus, relates all countries to one another according to their distance from each other, and carries more spatial information than a binary contiguity matrix would. The weighting matrix is row-standardized, i.e. each row sums up to one, and the model is estimated using maximum likelihood.[20] Clustered standard errors turn maximum likelihood into a pseudo maximum likelihood because the computation of clustered standard errors follows a corrected assumption about the sample distribution (Cameron and Trivedi 2009: 316-317). Likelihood-ratio tests to compare among specifications are therefore unfeasible. Table A3.10 in Appendix II shows the estimation results. Democracy is measured by means of the Bjørnskov and Rode (2019) dichotomous democracy measure and all columns include the full set of control variables. Columns (1) and (5) neither include country nor year fixed effects and columns (2) and (6) include country fixed effects only. The spatial autoregressive coefficient ρ is significant at the 1% level in columns (1), (2), (5) and (6) and indicates spatial dependence in defense spending relative to GDP among the countries in the sample. The spatial autoregressive coefficient does, however, no longer turn out to be statistically significant at any significance level once year fixed effects are included in columns (3) and (7); including lags of

[19] LeSage and Pace (2009, 32-33 and 155-158) provide an overview of different spatial lag models.
[20] LeSage and Pace (2009, chapter 3) discuss maximum likelihood estimation in spatial lag models.

the dependent variable in columns (4) and (8) does not change this result. Once the spatial panel data model is fully specified and accounts for worldwide trends in defense spending from the Cold War to the post-Cold War period, spatial correlation does not further explain variance in defense spending among countries. Spatial dependences are thus unlikely to violate the exclusion restriction of the instrumental variable. The results support that the regional diffusion of democracy—measured by means of the jackknifed democracy instrument—influences national defense spending through the channel of political institutions rather than directly or through spatial dependences.

3.4.2 2SLS and first-stage estimation results

Table 3.3 shows 2SLS estimation results in panel A and first-stage results of the jackknifed democracy instrument for each of the four democracy measures in panel B. The jackknifed democracy scores for all democracy measures in panel B are statistically significant at the 1% level and the Kleibergen-Paap F-Statistics for the excluded instrument are above the 10%-critical value suggested by Stock and Yogo (2005). The first-stage results thus indicate that the jackknifed democracy scores serve as a highly relevant instrument for the respective democracy measures.[21] The 2SLS results show negative IV estimates for all four democracy measures. The Bjørnskov and Rode (2019) dichotomous democracy measure and the Gründler and Krieger (2016, 2018) dichotomous and continuous democracy measures are statistically significant at the 5% level; the Polity IV index is statistically significant at the 1% level. The IV estimates for all four democracy measures are larger and in a closer range to each other compared with the OLS estimates from the baseline dynamic panel data model. The IV estimates indicate an effect of democracy on national defense spending relative to GDP of 21% (*Democracy*), 1.4% (for a one-point increase in *Polity IV*), 17% (*DSVMDI*) and 17% (*CSVMDI*; for a hypothetical

[21] Further lags of the jackknifed democracy instruments did not turn out to be statistically significant in the first-stage regression.

change from zero to one). A Wald test does not reject the null hypothesis of equality of the parameter estimates for the dichotomous democracy measure by Bjørnskov and Rode (2019) and the two democracy measures by Gründler and Krieger (2016, 2018). The virtually identical effect size of the dichotomous and the continuous democracy measure by Gründler and Krieger (2016, 2018) supports their credibility because both indicators are "conceptually equivalent" and supposed to yield similar estimation results (Gründler and Krieger 2018).

The larger size of the IV estimates compared to the OLS estimates shown in Table 3.2 indicates that OLS estimation results for the non-instrumented democracy measures underestimate the effect of democracy on national defense spending. The downward bias is likely to be attributed to the development of political institutions in the run-up of a democratization which influences national defense spending before a country is described as a democracy. Since dichotomous democracy measures are a rather rough measure of democracy, this bias is larger for the two dichotomous democracy measures (columns 1 and 3) than for the Polity IV index and the continuous democracy measure (columns 2 and 4). A second source of this downward bias is measurement error in democracy measures. The downward bias of the OLS estimates is consistent with the findings of Acemoglu et al. (2019), whose IV estimates for the effect of democracy on growth also exceed the OLS estimates.

The results for the control variables and the lags of the dependent variable hardly differ from the estimation results shown in Table 3.2. Again, according to the multiplier for the cumulative long-run effect of democracy, the estimated impact of democracy on national defense spending is almost three times higher in the long run. Given the IV estimates for the Bjørnskov and Rode (2019) dichotomous democracy measure and the Gründler and Krieger (2016, 2018) dichotomous and continuous democracy measures shown in Table 3.3, the implied long-run effect of more than 50% indicates that defense spending relative to GDP in established democracies is less than half as high as if the respective nations were under autocratic rule. The selected examples shown in Figure 3.1 indicate that such a long-run effect size is well plausible.

TABLE 3.3: TWO-STAGE-LEAST-SQUARES AND FIRST-STAGE ESTIMATION RESULTS

National defense spending (in % of GDP)[a]	(1)	(2)	(3)	(4)
Panel A: Two-stage-least-squares estimates				
Democracy (t – 1)	-0.234**			
	(0.104)			
Polity IV (t – 1)		-0.014***		
		(0.005)		
DSVMDI (t – 1)			-0.183**	
			(0.090)	
CSVMDI (t – 1)				-0.192**
				(0.095)
War (t – 1)	0.130***	0.126***	0.113***	0.125***
	(0.033)	(0.037)	(0.035)	(0.035)
Internal threat (t – 1)	-0.002	-0.005	-0.003	-0.001
	(0.007)	(0.005)	(0.006)	(0.005)
GDP[a] (t – 1)	0.034	0.038	0.037	0.042
	(0.055)	(0.054)	(0.053)	(0.057)
Population[a] (t – 1)	0.007	0.027	0.035	0.025
	(0.128)	(0.131)	(0.131)	(0.138)
Trade globalization (t – 1)	0.000	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
National defense spending (% of GDP)[a] (t – 2)	0.659***	0.654***	0.651***	0.658***
	(0.059)	(0.057)	(0.059)	(0.059)
National defense spending (% of GDP)[a] (t – 3)	-0.008	-0.013	-0.005	-0.008
	(0.056)	(0.056)	(0.055)	(0.056)
National defense spending (% of GDP)[a] (t – 4)	-0.030**	-0.030**	-0.030**	-0.030**
	(0.013)	(0.013)	(0.013)	(0.013)
National defense spending (% of GDP)[a] (t – 5)	0.026**	0.027**	0.028**	0.028**
	(0.011)	(0.011)	(0.011)	(0.011)
Panel B: First-stage estimates *(excluded instruments only)*				
Jackknifed democracy measure (t – 2)	0.779***	1.138***	0.636***	0.801***
	(0.147)	(0.180)	(0.126)	(0.096)
Country Fixed Effects	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes
Observations	2,455	2,455	2,455	2,455
Countries	95	95	95	95
R^2	0.639	0.646	0.646	0.646
Kleibergen-Paap F-Statistic	27.98	40.05	25.44	68.95
Stock-Yogo (10% rel. bias)	16.38	16.38	16.38	16.38

Notes: Standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively. All regressions apply standard errors clustered at the country level. [a] denotes variables which are expressed in their natural logarithm.

3.5 Conclusion

I investigated how political institutions in the context of the third wave of democratization influenced national defense spending. New SIPRI data on military expenditure for years prior to 1988 allowed to examine the impact of democracy's third wave for the period 1972-2013 for 110 countries, thus including the entire third wave of democratization. Since democracy is hard to quantify and different democracy measures can yield different results, I applied four democracy measures: the dichotomous democracy measure by Bjørnskov and Rode (2019), the Polity IV index by Marshall et al. (2018) and the dichotomous and continuous democracy

measure by Gründler and Krieger (2016, 2018). The coefficient estimates for all four democracy measures in the dynamic panel data model indicated a significant negative impact of democracy on defense spending relative to GDP, which is about 10% according to the two dichotomous democracy measures. Region-specific estimation results accounting for the sub-waves that reached different regions at different points in time did not provide evidence for effect heterogeneity across world regions. I applied an IV strategy that exploits the regional diffusion of democracy in the context of the third wave of democratizations to overcome endogeneity problems. The IV estimates indicated an effect of democracy on national defense spending of about 20%. The OLS estimates resulting from non-instrumented democracy measures thus underestimate the effect of democracy on national defense spending. For both OLS and IV estimates, the cumulative long-run effect of democratization is almost three times higher according to the dynamics in defense spending. Differences in government spending policies between democracies and autocracies and a decrease in mutually perceived threat among democratic conspecifics might lead countries to decrease defense spending after transition to democracy. The results are in line with other studies that estimate demand functions for national defense spending.

It remains open as to whether the first and second wave of democracy reduced defense spending as was the case with the third wave of democracy. The first wave took place in the era of imperialism, industrialization and the rise of the nation state in the Western world, i.e. under conditions of rivalry. The second wave occurred parallel with the onset of the Cold War, an era of system competition. Both world powers struggled for spheres of influence in a divided Europe as well as in Latin America, Africa and Asia, where they fought proxy wars (e.g. in Vietnam, Afghanistan, Angola and Ethiopia). The third wave, however, incorporates the decline and end of this bipolar system, which allowed countries to develop political institutions and liberal societies without external compulsions. The emerging democracies could reduce defense spending and avoid rent-seeking within the armed forces. Apart from its intensity and

regional diffusion, the state of the world during the third wave of democratization further makes the third wave unique compared to the first and second wave. Investigating whether and to which extent democratizations in the context of the first and the second wave of democracy decreased national defense spending would therefore be a useful starting point for future research.

The measurement of democracy remains a challenge for scholars. Political institutions are too diverse to be easily compared both among countries and over time by means of a single numeric measure. The conceptualization, i.e. the institutions defined as critical for a democratic regime, the measurement of these institutions, and the aggregation to one single measure, e.g. dichotomous or continuous, determine whether a country is described as a democracy or autocracy and often give rise to measurement error in democracy indices. Future empirical research should therefore continue to apply various democracy measures.

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Appendix I

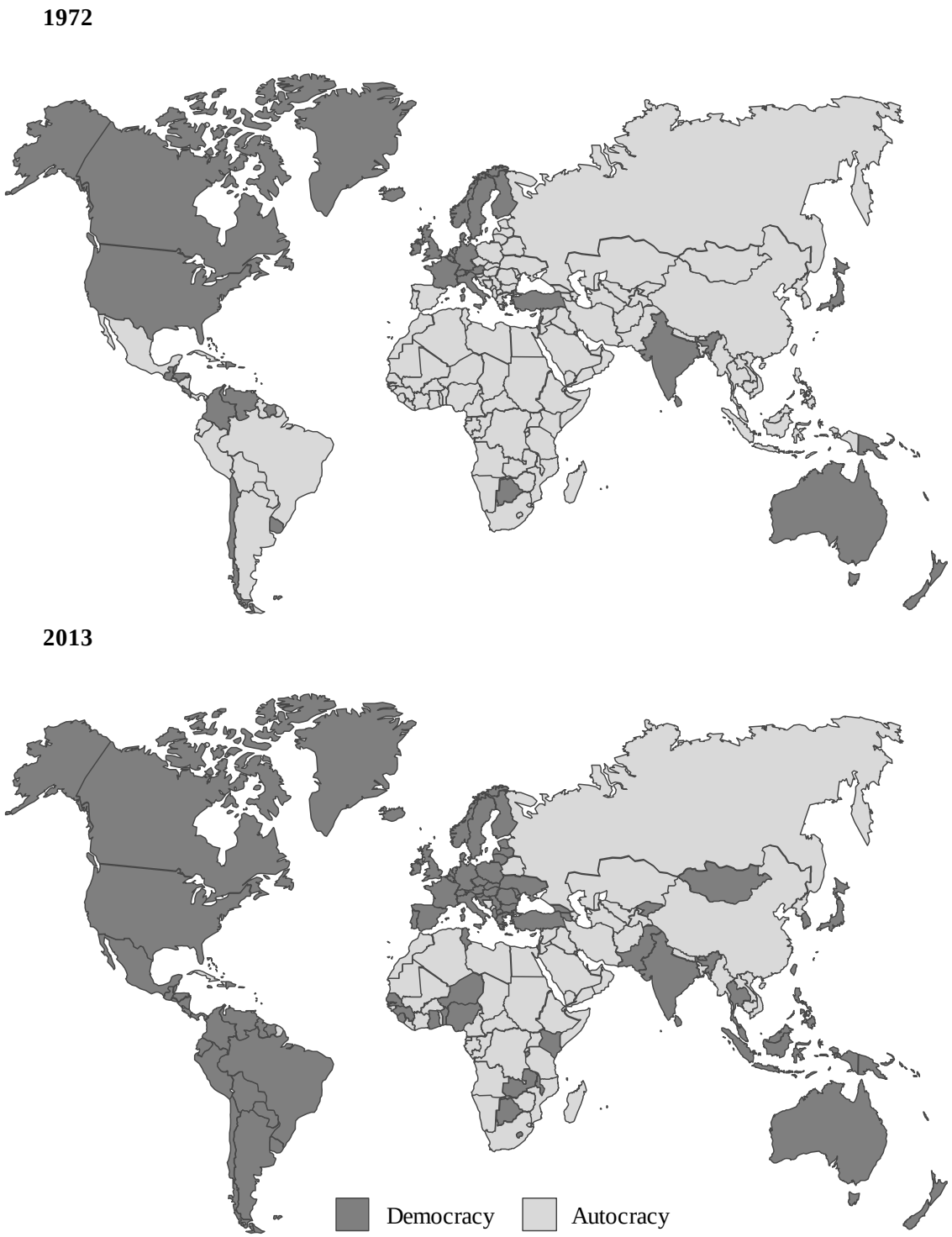

FIGURE A3.1: DEMOCRACIES 1972 AND 2013 ACCORDING TO THE DICHOTOMOUS DEMOCRACY MEASURE BY BJØRNSKOV AND RODE (2019)

Notes: The 1972 map is a contemporaneous political map and does not reflect countries and borders as of 1972. The entire territory of Germany, for example, is therefore labeled as a democracy in 1972.

TABLE A3.1: LIST OF COUNTRIES

ID	Country	First year	Last year	Variation in democracy measures			
				Democracy	Polity IV	DSVMDI	CSVMDI
1	Albania	1981	2013	yes	yes	yes	yes
2	Algeria	1972	2013	no	yes	yes	yes
3	Angola	1985	2013	no	yes	yes	yes
4	Argentina	1972	2013	yes	yes	yes	yes
5	Armenia	1995	2013	no	yes	no	yes
6	Azerbaijan	1992	2013	no	yes	yes	yes
7	Belarus	1992	2013	no	yes	yes	yes
8	Belgium	1972	2013	no	yes	no	yes
9	Benin	2012	2013	no	no	no	yes
10	Botswana	1977	2013	no	yes	no	yes
11	Bulgaria	1989	2013	yes	yes	yes	yes
12	Burkina Faso	1972	2013	no	yes	yes	yes
13	Burundi	2012	2013	no	no	no	yes
14	Cambodia	1994	2013	no	yes	yes	yes
15	Cameroon	1972	2013	no	yes	yes	yes
16	Canada	1972	2013	no	no	no	yes
17	Central African Republic	2007	2013	no	no	yes	yes
18	Chad	2013	2013	no	no	no	no
19	Chile	1972	2013	yes	yes	yes	yes
20	Colombia	1972	2013	no	yes	no	yes
21	Croatia	1996	2013	no	yes	no	yes
22	Cuba	2009	2013	no	no	no	yes
23	Cyprus	1985	2013	no	no	no	yes
24	Czech Republic	1994	2013	no	yes	no	yes
25	Denmark	1972	2013	no	no	no	yes
26	Dominican Republic	1972	2013	no	yes	yes	yes
27	East Timor	2005	2013	no	yes	no	yes
28	Ecuador	1972	2013	yes	yes	yes	yes
29	Egypt	1972	2013	no	yes	yes	yes
30	Equatorial Guinea	2007	2009	no	no	no	yes
31	Eritrea	1994	2003	no	yes	no	yes
32	Estonia	1996	2013	no	yes	no	yes
33	Fiji	1972	2013	yes	yes	yes	yes
34	Finland	1972	2013	no	no	no	yes
35	France	1972	2013	no	yes	no	yes
36	Gabon	2010	2013	no	no	no	yes
37	Gambia	2012	2013	no	no	no	yes
38	Georgia	1996	2013	yes	yes	yes	yes
39	Germany	1972	2013	no	no	no	yes
40	Greece	1972	2013	yes	yes	yes	yes
41	Guatemala	1972	2013	yes	yes	yes	yes
42	Guinea	2012	2013	no	no	no	yes
43	Guinea-Bissau	2009	2013	yes	yes	yes	yes
44	Guyana	2000	2013	yes	no	no	yes
45	Haiti	2013	2013	no	no	no	no
46	Honduras	2000	2013	no	no	yes	yes
47	Hungary	1973	2013	yes	yes	yes	yes
48	Indonesia	1974	2013	yes	yes	yes	yes
49	Iran	1980	2013	no	yes	yes	yes
50	Ireland	1972	2013	no	no	no	yes
51	Israel	1972	2013	no	yes	no	yes
52	Italy	1972	2013	no	no	no	yes
53	Jamaica	1981	2013	no	yes	yes	yes
54	Japan	1972	2013	no	no	no	yes
55	Kenya	1972	2013	yes	yes	yes	yes
56	Kuwait	1996	2013	no	no	no	yes
57	Kyrgyzstan	1992	2013	yes	yes	yes	yes
58	Laos	1992	2013	no	no	no	yes
59	Latvia	1996	2013	no	no	no	yes
60	Lesotho	1976	2013	yes	yes	yes	yes
61	Liberia	2004	2013	yes	yes	yes	yes
62	Libya	2012	2013	no	no	yes	yes

TABLE A3.1 CONTINUED: LIST OF COUNTRIES

	Country	First year	Last year	Variation in democracy measures			
				Democracy	Polity IV	DSVMDI	CSVMDI
63	Lithuania	1996	2013	no	no	no	yes
64	Luxembourg	1972	2013	no	no	no	yes
65	Macedonia	1996	2013	no	yes	no	yes
66	Malawi	1972	2013	yes	yes	yes	yes
67	Malaysia	1972	2013	yes	yes	yes	yes
68	Mali	1993	2013	yes	yes	yes	yes
69	Mauritania	2012	2013	no	no	no	yes
70	Mauritius	1977	2013	no	yes	no	yes
71	Moldova	1996	2013	no	yes	no	yes
72	Mongolia	1987	2013	yes	yes	yes	yes
73	Montenegro	2007	2013	no	no	no	yes
74	Morocco	1972	2013	no	yes	yes	yes
75	Mozambique	1981	2013	no	yes	yes	yes
76	Namibia	1991	2013	no	no	no	yes
77	Netherlands	1972	2013	no	no	no	yes
78	Nicaragua	1991	2013	no	yes	no	yes
79	Niger	2008	2013	yes	yes	yes	yes
80	Norway	1972	2013	no	no	no	yes
81	Oman	1972	2013	no	yes	no	no
82	Panama	1987	1999	yes	yes	yes	yes
83	Papua New Guinea	1985	2013	no	yes	no	yes
84	Poland	1981	2013	yes	yes	yes	yes
85	Portugal	1972	2013	yes	yes	yes	yes
86	Qatar	2002	2010	no	no	no	no
87	Romania	1981	2013	yes	yes	yes	yes
88	Saudi Arabia	1987	2013	no	no	no	yes
89	Senegal	1979	2013	yes	yes	yes	yes
90	Sierra Leone	2000	2013	no	yes	yes	yes
91	Singapore	1972	2013	no	no	no	yes
92	Slovak Republic	1994	2013	no	yes	no	yes
93	Slovenia	1996	2013	no	no	no	yes
94	South Africa	1972	2013	no	yes	yes	yes
95	Spain	1972	2013	yes	yes	yes	yes
96	Sudan	1990	2009	no	yes	no	yes
97	Sweden	1972	2013	no	no	no	yes
98	Switzerland	1981	2013	no	no	no	yes
99	Tajikistan	2008	2012	no	no	no	yes
100	Tunisia	1972	2013	yes	yes	yes	yes
101	Turkey	1972	2013	yes	yes	yes	yes
102	Turkmenistan	1994	1999	no	no	no	yes
103	UAE	1997	2013	no	no	no	no
104	UK	1972	2013	no	no	no	yes
105	USA	1972	2013	no	yes	no	yes
106	Uganda	1983	2013	yes	yes	yes	yes
107	Ukraine	1993	2013	no	yes	no	yes
108	Uruguay	1972	2013	yes	yes	yes	yes
109	Yemen	1992	2013	no	yes	yes	yes
110	Zambia	2004	2013	yes	yes	no	yes

TABLE A3.2: SUMMARY STATISTICS

	Observations	Mean	Std. Dev.	Min	p25	p75	Max
National defense spending (in % of GDP)	2978	0.029125	0.033041	1.72e-07	0.013710	0.032367	0.343764
Democracy (t – 1)	2978	0.607790	0.488325	0	0	1	1
Polity IV (t – 1)	2978	3.810947	6.919581	-10	-3	10	10
DSVMDI (t – 1)	2978	0.691404	0.461992	0	0	1	1
CSVMDI (t – 1)	2978	0.664054	0.358472	0.005817	0.346033	0.949886	0.973161
War (t – 1)	2978	0.156817	0.363689	0	0	0	1
Internal threat (t – 1)	2978	0.503358	1.429624	0	0	0	9
GDP (t – 1)	2939	4.42e+11	1.40e+12	4.57e+08	1.07e+10	2.43e+11	1.55e+13
Population (t – 1)	2978	2.37e+07	4.11e+07	342421	3960612	2.57e+07	3.14e+08
Trade globalization (t – 1)	2978	49.73571	20.54514	8.55423	32.94455	66.17088	99.55211

Notes: National Defense Spending in % of GDP, GDP (t – 1) and Population (t – 1) are expressed in their absolute values.

TABLE A3.3: CORRELATIONS

	National defense spending (in % of GDP)[a]	Democracy (t – 1)	Polity IV (t – 1)	DSVMDI (t – 1)	CSVMDI (t – 1)	War (t – 1)	Internal threat (t – 1)	GDP[a] (t – 1)	Population[a] (t – 1)	Trade globali-zation (t – 1)
National defense spending (in % of GDP)[a]	1.000									
Democracy (t – 1)	-0.282	1.000								
Polity IV (t – 1)	-0.344	0.831	1.000							
DSVMDI (t – 1)	-0.317	0.769	0.854	1.000						
CSVMDI (t – 1)	-0.338	0.821	0.915	0.936	1.000					
War (t – 1)	0.228	-0.123	-0.172	-0.162	-0.165	1.000				
Internal threat (t – 1)	0.143	-0.166	-0.233	-0.202	-0.226	0.694	1.000			
GDP[a] (t – 1)	0.127	0.341	0.335	0.242	0.308	0.054	0.007	1.000		
Population[a] (t – 1)	0.114	0.011	-0.009	-0.020	0.011	0.269	0.243	0.689	1.000	
Trade globalization (t – 1)	-0.104	0.005	0.092	0.063	0.070	-0.268	-0.271	-0.259	-0.543	1.000

Notes: [a] denotes variables which are expressed in their natural logarithms.

Appendix II

TABLE A3.4: ESTIMATION RESULTS FOR THE STATIC PANEL DATA MODEL FOR 95 COUNTRIES

National defense spending (in % of GDP)[a]	(1)	(2)	(3)	(4)
Panel A: Without control variables				
Democracy (t – 1)	-0.196**			
	(0.096)			
Polity IV (t – 1)		-0.023***		
		(0.007)		
DSVMDI (t – 1)			-0.254***	
			(0.069)	
CSVMDI (t – 1)				-0.356***
				(0.099)
Country Fixed Effects	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes
Observations	2,930	2,930	2,930	2,930
Countries	95	95	95	95
R^2 Overall	0.129	0.163	0.138	0.146
R^2 Within	0.198	0.211	0.208	0.207
R^2 Between	0.132	0.198	0.147	0.169
Panel B: With control variables				
Democracy (t – 1)	-0.217**			
	(0.099)			
Polity IV (t – 1)		-0.023***		
		(0.007)		
DSVMDI (t – 1)			-0.245***	
			(0.072)	
CSVMDI (t – 1)				-0.365***
				(0.099)
War (t – 1)	0.244***	0.242***	0.220***	0.239***
	(0.063)	(0.067)	(0.061)	(0.064)
Internal threat (t – 1)	0.021	0.012	0.018	0.016
	(0.024)	(0.022)	(0.021)	(0.022)
GDP[a] (t – 1)	-0.139	-0.167	-0.158	-0.173
	(0.164)	(0.162)	(0.161)	(0.165)
Population[a] (t – 1)	0.139	0.194	0.200	0.214
	(0.273)	(0.273)	(0.271)	(0.275)
Trade globalization (t – 1)	0.003	0.002	0.003	0.002
	(0.004)	(0.004)	(0.004)	(0.004)
Country Fixed Effects	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes
Observations	2,930	2,930	2,930	2,930
Countries	95	95	95	95
R^2 Overall	0.062	0.066	0.063	0.060
R^2 Within	0.218	0.229	0.225	0.226
R^2 Between	0.018	0.029	0.022	0.023

Notes: Standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively. All regressions apply standard errors clustered at the country level. [a] denotes variables which are expressed in their natural logarithm.

TABLE A3.5: ESTIMATION RESULTS FOR THE DYNAMIC PANEL DATA MODEL UNTIL THE 2007/2008 FINANCIAL CRISIS

National defense spending (in % of GDP)[a]	(1)	(2)	(3)	(4)
Democracy (t – 1)	-0.086*			
	(0.046)			
Polity IV (t – 1)		-0.010***		
		(0.003)		
DSVMDI (t – 1)			-0.106***	
			(0.032)	
CSVMDI (t – 1)				-0.159***
				(0.038)
War (t – 1)	0.129***	0.129***	0.118***	0.127***
	(0.032)	(0.033)	(0.034)	(0.033)
Internal threat (t – 1)	0.014	0.010	0.012	0.012
	(0.009)	(0.009)	(0.010)	(0.009)
GDP[a] (t – 1)	0.139**	0.107	0.122*	0.107
	(0.069)	(0.068)	(0.065)	(0.067)
Population[a] (t – 1)	-0.147	-0.100	-0.099	-0.087
	(0.133)	(0.136)	(0.131)	(0.134)
Trade globalization (t – 1)	-0.001	-0.001	-0.001	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)
National defense spending (% of GDP)[a] (t – 2)	0.602***	0.591***	0.593***	0.592***
	(0.053)	(0.052)	(0.053)	(0.054)
National defense spending (% of GDP)[a] (t – 3)	-0.001	-0.003	-0.000	-0.001
	(0.046)	(0.045)	(0.046)	(0.046)
National defense spending (% of GDP)[a] (t – 4)	-0.029**	-0.028**	-0.028**	-0.029**
	(0.014)	(0.013)	(0.013)	(0.013)
National defense spending (% of GDP)[a] (t – 5)	0.029***	0.029**	0.030***	0.029**
	(0.011)	(0.011)	(0.011)	(0.011)
Country Fixed Effects	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes
Observations	1,828	1,828	1,828	1,828
Countries	89	89	89	89
R^2 Overall	0.802	0.844	0.807	0.830
R^2 Within	0.692	0.696	0.695	0.695
R^2 Between	0.828	0.876	0.833	0.859

Notes: Standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively. All regressions apply standard errors clustered at the country level. [a] denotes variables which are expressed in their natural logarithm.

TABLE A3.6: ESTIMATION RESULTS FOR THE DYNAMIC PANEL DATA MODEL EXCLUDING REVERSE TRANSITIONS

National defense spending (in % of GDP)[a]	(1)	(2)	(3)	(4)
Democracy (t – 1)	-0.111**			
	(0.055)			
Polity IV (t – 1)		-0.011***		
		(0.003)		
DSVMDI (t – 1)			-0.126***	
			(0.039)	
CSVMDI (t – 1)				-0.178***
				(0.043)
War (t – 1)	0.126***	0.127***	0.114***	0.126***
	(0.038)	(0.041)	(0.039)	(0.039)
Internal threat (t – 1)	0.000	-0.004	-0.001	-0.002
	(0.006)	(0.005)	(0.005)	(0.005)
GDP[a] (t – 1)	0.076	0.059	0.062	0.052
	(0.055)	(0.055)	(0.056)	(0.057)
Population[a] (t – 1)	-0.057	-0.027	-0.035	-0.019
	(0.142)	(0.141)	(0.142)	(0.144)
Trade globalization (t – 1)	0.000	0.000	0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
National defense spending (% of GDP)[a] (t – 2)	0.679***	0.671***	0.669***	0.669***
	(0.063)	(0.062)	(0.061)	(0.062)
National defense spending (% of GDP)[a] (t – 3)	-0.028	-0.028	-0.023	-0.024
	(0.059)	(0.059)	(0.059)	(0.060)
National defense spending (% of GDP)[a] (t – 4)	-0.031**	-0.031**	-0.031**	-0.031**
	(0.012)	(0.012)	(0.012)	(0.012)
National defense spending (% of GDP)[a] (t – 5)	0.024*	0.026**	0.025*	0.025**
	(0.013)	(0.012)	(0.013)	(0.012)
Country Fixed Effects	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes
Observations	2,326	2,326	2,326	2,326
Countries	91	91	91	91
R^2 Overall	0.860	0.866	0.862	0.865
R^2 Within	0.652	0.654	0.655	0.654
R^2 Between	0.935	0.940	0.939	0.942

Notes: Standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively. All regressions apply standard errors clustered at the country level. [a] denotes variables which are expressed in their natural logarithm.

TABLE A3.7: ESTIMATION RESULTS FOR THE DYNAMIC PANEL DATA MODEL EXCLUDING MILITARY DICTATORSHIPS

National defense spending (in % of GDP)[a]	(1)	(2)	(3)	(4)
Democracy (t – 1)	-0.088			
	(0.085)			
Polity IV (t – 1)		-0.017***		
		(0.006)		
DSVMDI (t – 1)			-0.159***	
			(0.060)	
CSVMDI (t – 1)				-0.265***
				(0.071)
War (t – 1)	0.107***	0.101***	0.083**	0.099***
	(0.027)	(0.025)	(0.032)	(0.025)
Internal threat (t – 1)	-0.005	-0.011	-0.007	-0.010
	(0.011)	(0.009)	(0.011)	(0.009)
GDP[a] (t – 1)	0.076	0.027	0.041	0.014
	(0.065)	(0.067)	(0.066)	(0.069)
Population[a] (t – 1)	-0.046	0.024	0.003	0.033
	(0.163)	(0.152)	(0.162)	(0.162)
Trade globalization (t – 1)	0.001	0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.001)
National defense spending (% of GDP)[a] (t – 2)	0.648***	0.631***	0.629***	0.627***
	(0.075)	(0.073)	(0.073)	(0.074)
National defense spending (% of GDP)[a] (t – 3)	0.000	-0.005	0.005	0.003
	(0.071)	(0.070)	(0.068)	(0.070)
National defense spending (% of GDP)[a] (t – 4)	-0.031**	-0.031**	-0.032**	-0.032**
	(0.013)	(0.012)	(0.013)	(0.012)
National defense spending (% of GDP)[a] (t – 5)	0.015	0.016	0.016	0.016
	(0.016)	(0.016)	(0.016)	(0.016)
Country Fixed Effects	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes
Observations	1,712	1,712	1,712	1,712
Countries	70	70	70	70
R^2 Overall	0.854	0.870	0.868	0.871
R^2 Within	0.589	0.596	0.596	0.595
R^2 Between	0.928	0.948	0.948	0.956

Notes: Standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively. All regressions apply standard errors clustered at the country level. [a] denotes variables which are expressed in their natural logarithm.

TABLE A3.8: ESTIMATION RESULTS FOR THE DYNAMIC PANEL DATA MODEL EXCLUDING COMMUNIST DICTATORSHIPS

National defense spending (in % of GDP)[a]	(1)	(2)	(3)	(4)
Democracy (t – 1)	-0.100*			
	(0.055)			
Polity IV (t – 1)		-0.011***		
		(0.003)		
DSVMDI (t – 1)			-0.113***	
			(0.040)	
CSVMDI (t – 1)				-0.153***
				(0.041)
War (t – 1)	0.132***	0.134***	0.124***	0.133***
	(0.037)	(0.040)	(0.038)	(0.038)
Internal threat (t – 1)	-0.000	-0.004	-0.002	-0.001
	(0.006)	(0.006)	(0.006)	(0.005)
GDP[a] (t – 1)	0.026	0.009	0.019	0.010
	(0.068)	(0.068)	(0.068)	(0.070)
Population[a] (t – 1)	-0.045	0.010	-0.008	0.007
	(0.154)	(0.150)	(0.155)	(0.156)
Trade globalization (t – 1)	0.000	0.000	0.000	0.000
	(0.002)	(0.002)	(0.002)	(0.002)
National defense spending (% of GDP)[a] (t – 2)	0.717***	0.706***	0.712***	0.712***
	(0.065)	(0.063)	(0.062)	(0.063)
National defense spending (% of GDP)[a] (t – 3)	-0.080	-0.078	-0.079	-0.079
	(0.076)	(0.075)	(0.075)	(0.076)
National defense spending (% of GDP)[a] (t – 4)	0.013	0.013	0.013	0.014
	(0.037)	(0.036)	(0.036)	(0.037)
National defense spending (% of GDP)[a] (t – 5)	0.003	0.001	0.002	0.003
	(0.043)	(0.041)	(0.041)	(0.041)
Country Fixed Effects	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes
Observations	2,143	2,143	2,143	2,143
Countries	82	82	82	82
R^2 Overall	0.884	0.876	0.882	0.880
R^2 Within	0.623	0.626	0.626	0.625
R^2 Between	0.977	0.966	0.973	0.971

Notes: Standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively. All regressions apply standard errors clustered at the country level. [a] denotes variables which are expressed in their natural logarithm.

TABLE A3.9: ESTIMATION RESULTS FOR THE DYNAMIC PANEL DATA MODEL ALLOWING FOR REGION-SPECIFIC EFFECT HETEROGENEITY

National defense spending (in % of GDP)[a]	(1) Democracy (t – 1)	(2) Polity IV (t – 1)	(3) DSVMDI (t – 1)	(4) CSVMDI (t – 1)
Panel A				
Democracy measure	-0.085**	-0.011***	-0.133***	-0.173***
	(0.038)	(0.002)	(0.045)	(0.045)
Democracy measure * *Africa*	-0.045	0.002	0.023	-0.005
	(0.132)	(0.007)	(0.069)	(0.095)
Panel B				
Democracy measure	-0.137**	-0.011***	-0.119***	-0.167***
	(0.057)	(0.003)	(0.032)	(0.040)
Democracy measure * *Asia & Pacific*	0.155*	0.001	-0.019	-0.050
	(0.093)	(0.005)	(0.106)	(0.103)
Panel C				
Democracy measure	-0.094	-0.010***	-0.125***	-0.190***
	(0.063)	(0.004)	(0.040)	(0.050)
Democracy measure * *Latin America*	-0.010	-0.002	0.007	0.057
	(0.073)	(0.004)	(0.056)	(0.079)
Panel D				
Democracy measure	-0.077	-0.010***	-0.116***	-0.164***
	(0.050)	(0.003)	(0.036)	(0.040)
Democracy measure * *Eastern Europe*	-0.116	-0.002	-0.058	-0.056
	(0.093)	(0.005)	(0.083)	(0.099)
Panel E				
Democracy measure	-0.098**	-0.011***	-0.124***	-0.175***
	(0.047)	(0.003)	(0.034)	(0.038)
Democracy measure * *Southern Europe*	0.055	0.017*	0.079*	0.071
	(0.048)	(0.009)	(0.042)	(0.086)
Panel F				
Democracy measure	-0.097**	-0.011***	-0.123***	-0.174***
	(0.047)	(0.003)	(0.033)	(0.038)
Democracy measure * *Western Europe & North America*	-	0.039***	-	0.661***
		(0.011)		(0.058)
Panel G				
Democracy measure	-0.097**	-0.011***	-0.126***	-0.177***
	(0.048)	(0.003)	(0.034)	(0.039)
Democracy measure * *Middle East*	-0.026	0.005	0.068	0.079
	(0.058)	(0.006)	(0.064)	(0.100)
Control Variables Included	yes	yes	yes	yes
National Defense Spending (% of GDP)[a] (t – 2)…(t – 5) included	yes	yes	yes	yes
Country Fixed Effects	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes
Observations	2,455	2,455	2,455	2,455
Countries	95	95	95	95

Notes: Standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively. All regressions apply standard errors clustered at the country level. [a] denotes variables which are expressed in their natural logarithm. The dynamic panel data models estimated in panels A through G include all 95 countries which can be included in the dynamic analysis. In each panel, the interaction term of one world region and the four democracy measures is estimated. Regional dummy variables are dropped because of multicollinearity with the country fixed effects. All regressions include the full set of control variables, include four lags of the dependent variable and account for both country and year fixed effects.

TABLE A3.10: ESTIMATION RESULTS FOR THE SPATIAL PANEL DATA MODEL

National defense spending	1972-2013 for 40 countries				1981-2013 for 53 countries			
(in % of GDP)[a]	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democracy (t – 1)	-0.230**	-0.231**	-0.243***	-0.130**	-0.291***	-0.301***	-0.279***	-0.084
	(0.096)	(0.094)	(0.091)	(0.055)	(0.089)	(0.094)	(0.090)	(0.055)
War (t – 1)	0.250***	0.245***	0.248***	0.112***	0.235***	0.229**	0.223**	0.108***
	(0.084)	(0.087)	(0.089)	(0.035)	(0.089)	(0.089)	(0.089)	(0.041)
Internal threat (t – 1)	0.056***	0.057***	0.046***	0.008	0.050**	0.047**	0.047**	-0.000
	(0.016)	(0.017)	(0.017)	(0.006)	(0.021)	(0.022)	(0.022)	(0.008)
GDP[a] (t – 1)	-0.194	-0.273**	-0.314	-0.006	-0.130	-0.228**	-0.185	0.000
	(0.125)	(0.134)	(0.214)	(0.064)	(0.089)	(0.103)	(0.127)	(0.059)
Population[a] (t – 1)	0.073	0.134	0.159	-0.047	0.017	0.037	0.136	-0.001
	(0.201)	(0.252)	(0.254)	(0.148)	(0.177)	(0.284)	(0.287)	(0.169)
Trade globalization (t – 1)	-0.002	-0.001	-0.001	0.001	-0.004	-0.003	-0.002	0.000
	(0.003)	(0.004)	(0.004)	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)
Nat. def. spend. (% of GDP)[a] (t – 2)				0.846***				0.728***
				(0.079)				(0.067)
Nat. def. spend. (% of GDP)[a] (t – 3)				-0.146				-0.085
				(0.137)				(0.106)
Nat. def. spend. (% of GDP)[a] (t – 4)				0.002				0.052
				(0.064)				(0.063)
Nat. def. spend. (% of GDP)[a] (t – 5)				0.050				-0.030
				(0.056)				(0.051)
Spatial ρ	0.491***	0.441***	0.157	-0.062	0.533***	0.452***	0.070	0.036
	(0.076)	(0.086)	(0.116)	(0.083)	(0.067)	(0.087)	(0.138)	(0.101)
Error variance σ^2	0.099***	0.097***	0.095***	0.049	0.080***	0.078***	0.077***	0.051*
	(0.034)	(0.033)	(0.032)	(0.031)	(0.028)	(0.027)	(0.026)	(0.030)
Country Fixed Effects	no	yes	yes	yes	no	yes	yes	yes
Year Fixed Effects	no	no	yes	yes	no	no	yes	yes
Observations	1,680	1,680	1,680	1,480	1,749	1,749	1,749	1,484
Countries	40	40	40	40	53	53	53	53
R^2 Overall	0.037	0.023	0.020	0.866	0.058	0.022	0.069	0.875
R^2 Within	0.372	0.374	0.422	0.678	0.466	0.469	0.494	0.581
R^2 Between	0.004	0.001	0.000	0.956	0.008	0.000	0.007	0.985
Log-Likelihood	-568.3	-437.7	-405.6	126.1	-435.8	-264.2	-238.9	99.43

Notes: Standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively. All regressions apply standard errors clustered at the country level. The model applies an inverse distance matrix as spatial weighting matrix. [a] denotes variables which are expressed in their natural logarithm.

4. Arms Production, National Defense Spending and Arms Trade: Examining Supply and Demand

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Abstract[*]

Scholars have estimated demand functions for national defense spending and investigated international arms trade for a long time. The relationship between supply and demand for military goods has, however, only been examined on aggregate level or in formal models yet. I investigate how the supply of military goods by arms-producing companies and the demand for military goods by both the national government and foreign governments are related by using a panel of up to 195 arms-producing companies in 21 countries for the period 2002-2016. The results show that if the demand for national defense spending increases by 1%, the arms sales by a country's largest arms-producing companies increase by up to 1.2%. If exports of major conventional weapons increase by 1%, sales increase by up to 0.2%. Arms imports do not affect domestic arms sales because imported and domestically produced arms are complements, and countries mainly import those arms they do not produce themselves. Country-specific estimation results suggest that differences among countries in geopolitical conditions and international relations determine whether a country's arms industry serves economic rather than security purposes.

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4.1 Introduction

Scholars have estimated demand functions for national defense spending (Dunne and Perlo-Freeman 2003a, 2003b, Collier and Hoeffler 2007, Albalate et al. 2012, Blum 2018, 2020, George and Sandler 2018, Blum and Potrafke 2019) and examined determinants of international arms trade (Smith and Tasiran 2005, Comola 2012, Akerman and Seim 2014, Kinne 2016, Brender 2018, Thurner et al. 2019) for a long time. The effect of arms trade on national defense spending has been investigated, too (Pamp and Thurner 2017, Pamp et al. 2018). Both national defense spending and arms trade express the demand for military goods by the national government and foreign governments while the supply of military goods is provided by the arms industry.[1] The market for military goods with arms-producing companies on the supply side and domestic and foreign governments on the demand side has, however, only been considered in formal models yet (Glismann and Horn 1992, Levine et al. 1994, Levine and Smith 1997, Dunne et al. 2007).[2] This chapter is original in establishing the link between supply and demand for military goods and estimates the effect of national defense spending and arms transfers on the sales of military goods by arms-producing companies.

The empirical analysis applies three databases from the Stockholm International Peace Research Institute (SIPRI): the SIRPI Arms Industry Database containing the sales of arms and military services by the worlds' top 100 arms-producing and military services companies, the SIPRI Arms Transfers Database containing trend indicator values (TIV) for the exports and imports of major conventional weapons and the SIPRI Military Expenditure Database containing data on national defense spending. These three databases have not been linked to each other yet. Smith and Dunne (2018), however, provide a parsimonious model describing world arms sales as a function of world defense spending; they find an elasticity of arms sales

[1] Military goods include military equipment (both arms and other equipment) and military services.
[2] Scholars often have regarded arms exports as the supply of arms in the international arms market. This view, however, considers the country level only and ignores the company level (see, e.g., Smith et al. 1985).

with respect to defense spending of 1.5. The authors conclude that "it is surprising that there is not more quantitative work using arms industry data" and encourage scholars to investigate the economics of arms at the firm level (Smith and Dunne 2018).

I examine how a country's demand for defense spending and the foreign demand for military goods from that country relate to the sales of military goods by its arms-producing industry. The sample includes up to 195 arms-producing and military services companies in 21 countries for the period 2002-2016. Arms orders for the own armed forces and arms exports determine the sales of military goods by arms-producing companies. In industrialized countries, arms orders are placed well in advance (especially larger procurement projects have long lead times) and exports previously need to be approved by the government. Companies thus produce "on demand" rather than stockpiling arms, and company sales describe the outcome of orders by the domestic government and by foreign governments. This chronological order between orders and production of military goods makes reverse causality in this supply-demand model less likely. I use panel data models with fixed effects and in first differences. The fixed effects results indicate that if national defense spending increases by 1%, arms sales by a country's largest arms-producing and military services companies increase by up to 1.2%. If exports of major conventional weapons increase by 1%, arms sales for these companies increase by up to 0.2%. Estimation results in first differences are similar for national defense spending, however, estimates for exports of major conventional weapons are considerably smaller. Arms imports are not shown to affect domestic arms sales, because countries mainly import arms they do not produce themselves. Imported and domestically produced arms are, thus, complements rather than substitutes. Country-specific estimation results suggest differences among countries in how the arms industry serves security or economic purposes. These differences are likely to be based on geopolitical conditions and international relations: the results suggest that, for instance, the United States' arms-producing companies primarily serve security purposes, while arms-producing companies in Germany primarily serve the export market.

4.2 Supply and demand for military goods

Supply and demand for military goods describe an imperfect market with few suppliers and few customers. The arms industry in most industrialized countries has an oligopolistic structure: the capital (including human capital) intensity in production, the high cost for military R&D, and strong confidentiality standards in procurement projects—which impair the diffusion of know-how and enhance long-term dependencies from arms suppliers in terms of training, maintenance and possible reorders—give rise to a market structure in which a few large arms manufacturers develop and produce arms and, thus, dominate the market for military goods (Glismann and Horn 1992, Levine et al. 1994). Military arms are highly differentiated products and companies, thus, operate under monopolistic competition. The demand side in industrialized countries is described by a monopsony in which the domestic government is the only domestic customer and even decides on arms exports to other countries (Glismann and Horn 1992). In an open economy model, the output of arms by domestic suppliers in equilibrium equals domestic arms demand, i.e. the demand for a country's own armed forces, plus arms exports less arms imports:

$$\textit{Domestic arms supply} = \textit{Domestic arms demand} + \textit{Arms exports} - \textit{Arms imports} \qquad (1)$$

Equation (1) can be approached with data provided by the Stockholm International Peace Research Institute (SIPRI). SIPRI provides data on the sales of military goods by the world's top 100 arms-producing companies, data on national defense spending and data on exports and imports of major conventional weapons (data is described in Section 4.3 in detail). Sales of military goods can thus be described as a function of national defense spending and international arms transfers. Reverse causality between the sales of military goods and national defense spending or arms transfers in such a supply-demand equation is unlikely because defense budgets in industrialized countries are commonly adopted before the beginning of a fiscal year (and include orders which have been placed well in advance or even long-term

procurement projects with long lead times) and arms exports undergo thorough approval processes in advance.[3] Arms-producing companies thus produce arms "on demand" contingent to domestic orders and approved exports rather than stockpiling arms. National defense budgets and approved arms exports determine the output of military goods by arms-producing companies. Defense spending and arms exports are, in turn, also the outcome of demand functions, which describe, for example, increased defense spending during wartime or increased arms exports when an ally is involved in an armed conflict. Regarding arms imports, it is important to examine whether imported arms and domestically produced arms are complements or substitutes.

Equation (1) allows to derive hypotheses on how supply and demand for military goods are related. Countries have built up domestic arms industries to provide their armed forces with military goods. Despite the increasing role of arms trade and joint procurement projects among allies, it is reasonable to assume that governments still source arms from their domestic arms-producing companies if possible to ensure security of supply, which is particularly important during wartime. These companies are often even partly or fully owned by the national government and have privileged status with respect to take-overs and foreign ownership to ensure control over domestic arms production. Variation in national defense spending over time is often driven by equipment spending—even though national defense spending is not limited to cost elements related to the arms industry such as procurement and maintenance but also includes large cost elements like personnel cost.[4] The first hypothesis to be examined is:

[3] Reverse causality might, however, arise if governments absorb cost-overruns for large-scale procurement projects.

[4] NATO figures show that the increase in national defense spending for NATO countries after NATO member states have committed themselves to the two percent spending target in 2014 disproportionately increased equipment expenditure compared to personnel expenditure (see "Defence Expenditure of NATO Countries (2011-2018)." NATO Press Release (2018)091).

Hypothesis 1: *If national defense spending increases, the sales of arms and military services by a country's largest arms-producing and military services companies increase.*

Military arms include small arms, major conventional weapons and weapons of mass destruction. Transfers of small arms are difficult to track and small arms do not exclusively serve military purposes. Transfers of weapons of mass destruction are strongly monitored and internationally heavily regulated. Exports of major conventional weapons are, however, likely to be positively related to the sales of military goods by domestic arms-producing companies. A country's largest arms-producing and military services companies which belong to the world's top 100 defense companies are likely to contribute disproportionately more to the production of major conventional weapons and disproportionately less to the production of small arms and weapons of mass destruction. Arms exports also include used major conventional weapons, though arms-producing companies are only involved in the production of new and the modernization of used arms. Export deals of used weapons, however, only represented 12% of the export deals of all major conventional weapons between 2002 and 2016 (2% were export deals of used but modernized major conventional weapons and 86% were export deals of new major conventional weapons). The second hypothesis to be examined is:

Hypothesis 2: *If exports of major conventional weapons increase, the sales of arms and military services by a country's largest arms-producing and military services companies increase.*

National defense spending includes—among other cost buckets—both domestically produced arms on the left-hand side of equation (1) and imported arms on the right-hand side of equation (1); arms imports therefore have a negative sign in equation (1). A negative relationship between the sales of military goods by domestic arms-producing companies and arms imports

would imply a "make or buy" decision by the government, i.e. that a government decides whether it buys a military good from a domestic arms-producing company or whether it imports such a good from abroad. In an environment of monopolistic competition with differentiated goods, however, domestically produced and imported military goods are likely not to be substitutes. Arms for military purposes are differentiated because of both product properties and origin: with regard to security of supply, domestically produced arms differ from imported arms because imported arms imply a strategic dependency from other countries. A country therefore seeks to be self-reliant in the production of military goods fundamental for national defense and restricts arms imports to those arms it does not produce itself (Glismann and Horn 1992). The Berry Amendment, for example, requires the United States Department of Defense to prefer the procurement of domestically produced military goods.[5,6] Countries therefore need to import only those military goods which are not produced by domestic arms-producing companies. Domestically produced arms and imported arms are, thus, complements rather than substitutes. Because of the complementarity between domestically produced and imported arms, increases in arms imports might even coincide with increases in arms sales by domestic arms-producing companies without any explicit causal link—for example, during large procurement activities. The complementarity might also give rise to reverse causality if components for domestically produced arms are sourced from foreign suppliers. The third hypothesis to be examined is:

[5] See Grasso, V.B. 2014. "The Berry Amendment: Requiring Defense Procurement to Come from Domestic Sources." Congressional Research Service (RL31236).

[6] In 2017, Donald Trump took measures to enforce source restrictions by means of the Buy American Act and the Berry Amendment (see Gregg, A. 2017. "Pentagon moves to shut foreign firms out of its supply chain." The Washington Post. Available at: https://www.washingtonpost.com/business/capitalbusiness/pentagon-moves-to-shut-foreign-firms-out-of-its-supply-chain/2017/07/06/37bc7498-60f6-11e7-8adc-fea80e32bf47_story.html?noredirect=on&utm_term=.443c0ac4e1c8, accessed January 9, 2019).

Hypothesis 3: *If imports of major conventional weapons increase, the sales of arms and military services by a country's largest arms-producing and military services companies do not decrease. Imports are rather unrelated or even positively related to sales.*

Scholars have described the trade-off between security considerations and economic returns in the context of arms trade (Levine et al. 1994, Thurner et al. 2019). This trade-off also applies to the importance governments attribute to the cost of defense spending and the returns from arms trade. A country's geopolitical role and its position within the international community are, among others, likely to determine the extent to which the arms industry serves security or economic purposes: the United States, for example, act as a world power and have quite often been engaged in military activities within the last decades to pursue national interest or the interest of the Western world. This role is likely to support a strong domestic arms industry which provides the armed forces with military goods and guarantees security of supply. Germany, in contrast, has been much more reserved in engaging in international conflicts and continuously decreased defense spending after the Cold War. The strong German defense industry, however, exports arms to numerous countries around the world. The examples for the United States and Germany support the conjectures stated by Levine et al. (1994) that "there might be a Stackelberg leader, the US, who has world-wide security concerns, and a number of 'small' followers (Britain and France) who are purely motivated by the economic return." The fourth and last hypothesis to be examined is:

Hypothesis 4: *Countries differ in whether and to what extent national defense spending and arms transfers explain the sales of arms and military services by a country's largest arms-producing and military services companies.*

4.3 Data and descriptive statistics

Data on arms sales, national defense spending and arms trade is provided by means of different SIPRI databases. The SIRPI Arms Industry Database contains information on the sales of arms and military services of the top 100 arms-producing and military services companies of each year in OECD countries and developing countries (no data is available for Chinese firms). Military goods are supposed to explicitly serve military purposes and military services include—among others—IT, maintenance, repair, logistics, training, intelligence and armed security services. The sales of military goods include "both sales for domestic procurement and sales for exports."[7] The database covers the period 2002-2016 and indicates the country of each company. Sales figures are reported in million constant (2016) US dollars and reflect each company's financial year.[8] As companies drop out or enter the list of the top 100 companies over the years, the number of countries for which data is available from 2002 through 2016 is less than 100. The database is subject to noteworthy shortcomings which have not been improved yet: for instance, the definition of military goods and the information provided on arms sales is not standardized among companies, arms sales might be double-counted because of intra-industry trade in intermediate products and components, Chinese companies are not covered at all and information on merger and acquisition activities and divestments of companies are only selectively available for 2015 and 2016. Since quality and consistency of sales data are the better, the larger the companies considered are, the top 100 companies describe the most reliable data available. The companies included are, however, not representative for the entire arms industry.[9]

[7] See https://www.sipri.org/databases/armsindustry/sources-and-methods, accessed December 3, 2018.
[8] Data is collected in local currency and converted into constant (2016) US dollars using average exchange rates. Domestic sales of military goods are, however, more likely to be conducted in local currency whereas international sales are conducted in US dollars. As the data does not reflect a company's domestic and international sales share and timing of these sales within a year, sales figures cannot be interpreted exactly if intra-annual exchange rate fluctuations are high.
[9] See Fleurant and Tian (2018) and Smith and Dunne (2018) for a discussion of the SIPRI Arms Industry Database.

The SIPRI Arms Transfers Database includes trend indicator values (TIV) for a country's exports and imports of major conventional weapons such as aircrafts, ships, tanks, or missiles. The TIV are supposed to describe "actual deliveries of major conventional weapons" per year in units which are comparable among countries and show trends in arms trade. The TIV constructed by SIPRI are therefore "based on the known unit production costs of a core set of weapons and is intended to represent the transfer of military resources rather than the financial value of the transfer."[10] Trend indicator values are expressed in millions. Used weapons are valued with 40% and used but modernized weapons are valued with two thirds of a weapon's initial value.

The SIPRI Military Expenditure Database provides data on national defense spending for a given calendar year. National defense spending is defined in million constant (2016) US dollars, i.e. in absolute terms. Defense spending in absolute terms better reflects the demand for security the arms industry must meet than defense spending as a share of GDP, which is the measure commonly applied when demand functions for national defense spending are estimated (see Dunne and Perlo-Freeman 2003a, 2003b, Blum 2018, 2020). Using defense spending in absolute terms is consistent with data on arms sales and TIV for arms trade, which are both also expressed in absolute terms.

The SIRPI Arms Industry Database also considers large foreign subsidiaries of international defense corporations which as an independent company would rank among the top 100.[11] Subsidiaries are specified by the country in which they are located. Since sales figures of subsidiaries are included in the sales figures of the parent company, including both subsidiaries and parent companies into one panel would result in double-counting.[12] I therefore

[10] See https://www.sipri.org/databases/armstransfers/sources-and-methods, accessed December 3, 2018.
[11] E.g. subsidiaries of BAE Systems (United Kingdom) like BAE Systems Inc. (United States) and BAE Systems Australia, or other subsidiaries of international corporations such as Airbus and Thales.
[12] It is not possible to subtract subsidiary figures from parent company figures, because time series of both subsidiary and parent in most of the cases do not have the same length and would result in inconsistent time series of the parent companies.

employ three panel data sets: i) a balanced panel of arms-producing and military services companies, ii) an unbalanced panel of arms-producing and military services companies and iii) an unbalanced panel of large subsidiaries of arms-producing companies. The first two panels include the large international corporations but exclude subsidiaries as further elements of national arms production; the third panel, in turn, does not consider any independent company. Given the data availability for national defense spending and arms transfers, the balanced company panel contains 44 companies in nine countries during the period 2002-2016.[13] These nine countries include the strongest arms industries like the United States, the United Kingdom, France and Germany and six of the top ten arms exporting countries. The unbalanced company panel (which the balanced company panel is a subset of) contains 195 companies in 21 countries and the unbalanced subsidiary panel contains 74 subsidiaries in 12 countries. Companies in the balanced company panel belonged to the top 100 arms-producing and military services companies from 2002 through 2016 while companies and subsidiaries in the unbalanced panels belonged to these top 100 according to their sales in at least one of the years from 2002 to 2016. Table A4.1 in Appendix I shows a list of countries with the number of companies by country included in each panel as well as country ranks in national defense spending, exports of major conventional weapons and imports of major conventional weapons as of 2016.

Tables A4.2 and A4.3 in Appendix I show summary statistics and correlations of arms sales, national defense spending and arms exports and imports for each of the three panels. Summary statistics and correlations for the sales of arms and military services are based on company-level data, thus including more observations than summary statistics and correlations for country-level data like national defense spending, exports and imports. The unconditional correlations of the sales of arms and military services with national defense spending and with exports of major conventional weapons are positive and of similar size: more than 30% in the

[13] In three cases, company names have been unified in the dataset after name changes: the Israeli aerospace company Israel Aircraft Industries changed name to Israel Aerospace Industries in 2006, EADS changed name to Airbus Group in 2014 and to Airbus SE in 2017 and Finmeccanica changed name to Leonardo in 2016.

balanced company panel and more than 20% in the unbalanced company panel. The correlation of imports with the sales of arms and military services is positive but only 5% in the unbalanced company panel. The high unconditional correlation between national defense spending and the exports of major conventional weapons of more than 60% in each of the three panels reflects that countries with high levels of national defense spending are also strong in arms exports, and vice versa.

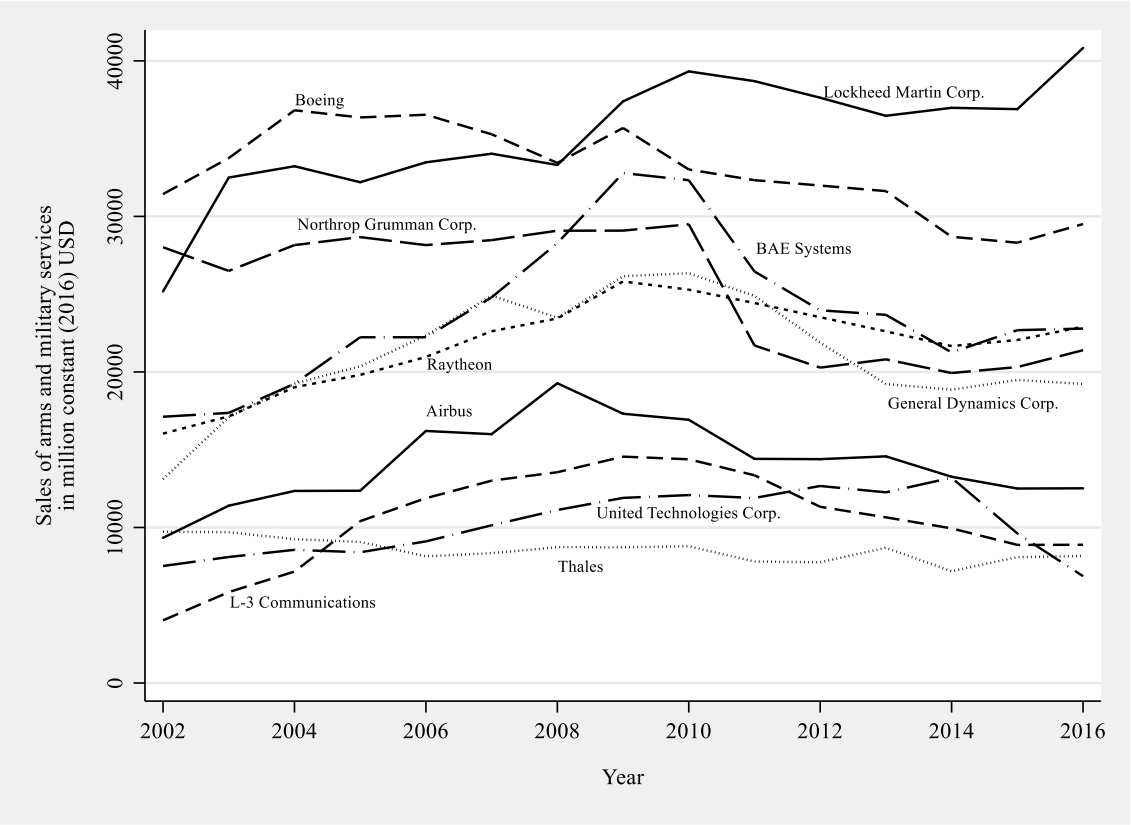

FIGURE 4.1: ARMS SALES BY THE TOP 10 ARMS-PRODUCING AND MILITARY SERVICES COMPANIES

Figure 4.1 shows the sales of arms and military services by the ten largest arms-producing companies from the balanced company panel (i.e. with time series available from 2002 through 2016). Seven of these companies are in the United States, one in France, one in the United Kingdom and another one—Airbus—is trans-European.[14] The sales of arms and military

[14] The "main engineering and production facilities" of Airbus Defence and Space—the division for equipment and services in the fields of aerospace and defense—are in France, Germany, Spain and the United Kingdom. For the further analysis, Airbus sales are allocated to France and Germany with a share of 40% each and to Spain and the United Kingdom with a share of 10% each, reflecting the employee shares of Airbus SE in these four countries (see: Airbus SE. 2017. "Annual Report 2017." Available at https://www.airbus.com/investors/financial-results-

services differ among companies over time: sales figures of Boing, United Technologies and Thales hardly varied over time while sales figures of Lockheed Martin, BAE Systems and Northrop Grumman have been rather volatile during the observation period. A considerable share of these ten companies, however, experienced peaks in sales during the late 2000s while sales decreased in the early 2010s; this development is concurrent with national defense spending in the United States, the United Kingdom and France. Figures 4.2 and 4.3 show how time series of national defense spending and exports of major conventional weapons are related to the total arms sales by the largest arms-producing companies in the United States (Figure 4.2) and in France, Germany, Israel and the United Kingdom (Figure 4.3; axes are removed and figures are not sized to scale to enhance readability). Companies do not drop out during the observation period because Figures 4.2 and 4.3 contain companies of the balanced company panel only; time series for total sales within one country thus describe the same set of companies from 2002 to 2016. For the United States and the United Kingdom, the similarity in time series for the sales of arms and military services and national defense spending is much more pronounced than for arms sales and the exports of major conventional weapons. For France and Germany, however, the sales of arms and military services and the exports of major conventional weapons show a pronounced similarity rather than arms sales and national defense spending do. The sales of arms and military services by Israeli companies have been rather constant since the late 2000s; national defense spending remarkably dropped in 2016 while the exports of major conventional weapons increased after 2014, thus allowing to hold arms sales at rather constant levels. The time series drawn for the five illustrated countries support the hypotheses stated in Section 4.2 that national defense spending and arms exports are positively

and-annual-reports.html#annualreports). Almost all employees of MBDA—the second trans-European company in the sample—work in France (45%), the United Kingdom (31%), Italy (12%) and Germany (12%). For the further analysis, MBDA sales are allocated to these four countries according to the employee shares (see: MBDA. 2016. "Corporate and Social Responsibility Report 2016." Available at https://www.mbda-systems.com/wp-content/uploads/2015/04/csr_report_2016.pdf). Comparison of prorated sales data and sales data available for MBDA subsidiaries in France and Italy supports the approach of prorating sales figures according to employee shares.

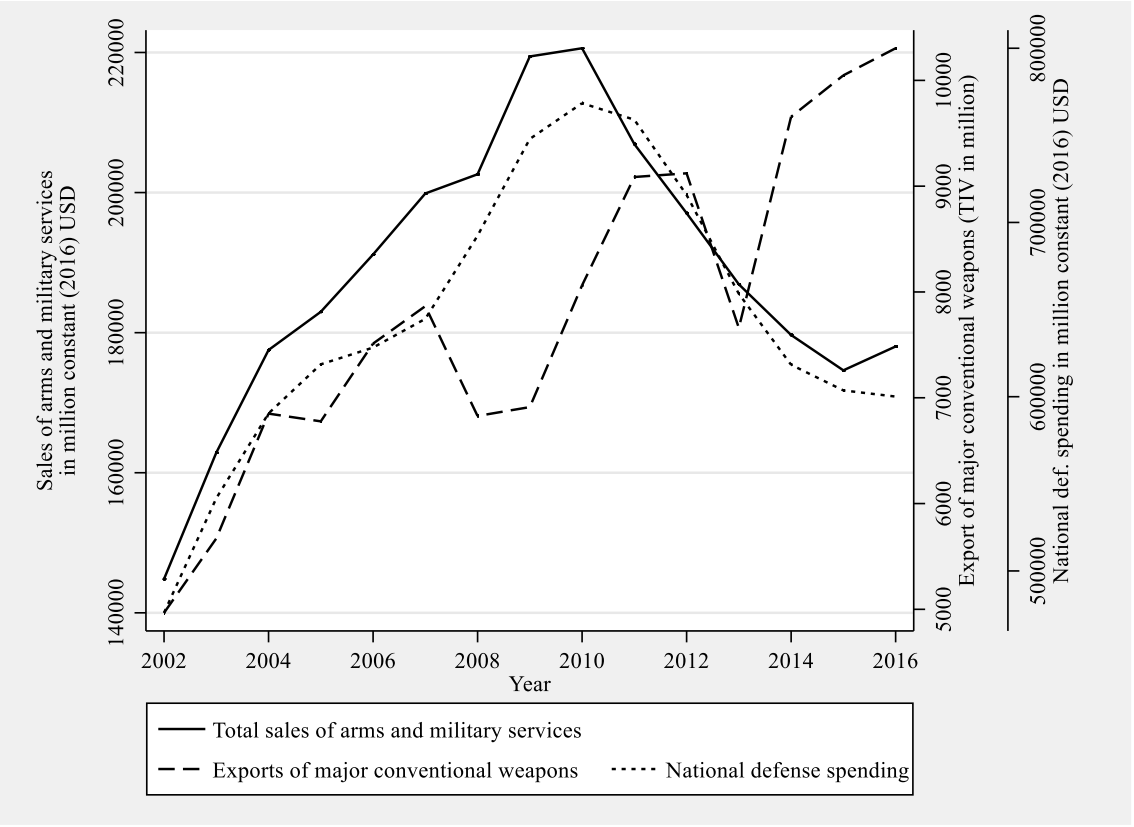

FIGURE 4.2: UNITED STATES TOP COMPANIES' ARMS SALES, NATIONAL DEFENSE SPENDING AND EXPORTS OF MAJOR CONVENTIONAL WEAPONS

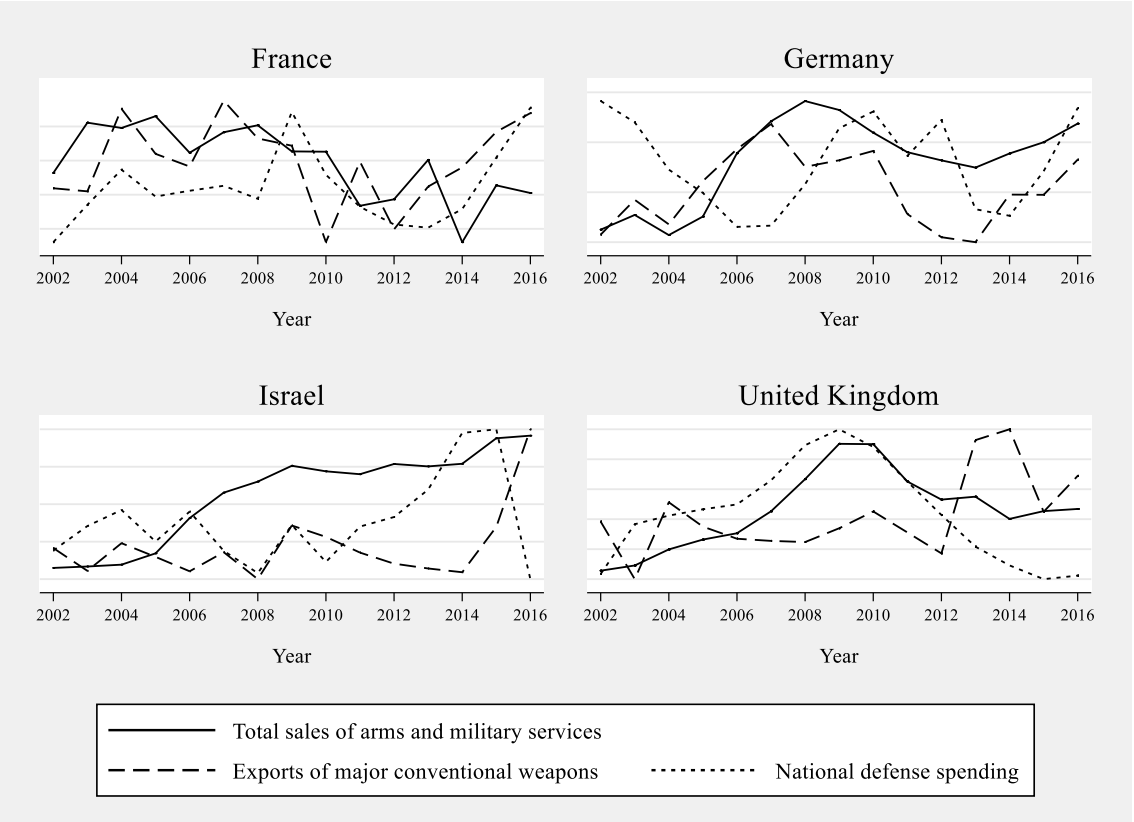

FIGURE 4.3: TOP COMPANIES' ARMS SALES, NATIONAL DEFENSE SPENDING AND EXPORTS OF MAJOR CONVENTIONAL WEAPONS

correlated with the sales by arms-producing and military services companies and that country-specific differences exist.

4.4 Empirical strategy

The baseline panel data model to estimate how national defense spending and arms transfers affect the sales of military goods by arms-producing companies has the following form:

$$ln(Sales\ of\ arms\ and\ military\ services)_{ijt} = \beta_1\ ln(National\ defense\ spending)_{jt} + \beta_2\ ln(Arms\ exports)_{jt} + \beta_3\ ln(Arms\ imports)_{jt} + \alpha_{ij} + u_{ijt} \quad (2)$$

The dependent variable $Sales\ of\ arms\ and\ military\ services_{ijt}$ describes the sales of arms and military services by company *i* in country *j* in year *t*. The considered companies belonged to the worlds' top 100 arms-producing and military services companies in at least one of the years during the observation period ($t = 2002,\ldots, 2016$). The number of companies *i* and countries *j* (in which these *i* companies are located) differs among the three assembled panels (see Section 4.3). The variable $National\ defense\ spending_{jt}$ describes national defense spending in country *j* in year *t*. The variables $Arms\ exports_{jt}$ and $Arms\ imports_{jt}$ describe trend indicator values for the exports and the imports of major conventional weapons from and to country *j* in year *t*. Both dependent and explanatory variables are expressed in their natural logarithms, which allows to interpret estimated coefficients as elasticities. The coefficient α_{ij} describes company fixed effects to account for unobserved characteristics of individual companies. The empirical model thus exploits the within-variation of the variables for companies and countries and allows to investigate how trends in the sales by individual arms-producing companies are influenced by trends in national defense spending and arms transfers. The within-interpretation is, moreover, favorable because—due to differences in how the figures are collected for different companies and countries—SIPRI data for company sales and national defense spending is more reliable

over time than across companies and countries. The standard error u_{ijt} is clustered at the company level and robust to serial correlation and heteroskedasticity (Huber/White/sandwich standard errors; see Huber 1967 and White 1980).

Time series of the sales of arms and military services and the explanatory variables might be non-stationary for the period 2002-2016 and give rise to spurious estimation results. Temporary trends might exist in national defense spending for individual countries and in the total sales of arms and military services by arms-producing companies (see Figures 4.2 and 4.3). I therefore also estimate the model in first differences (i.e. log-differences), thus eliminating company fixed effects α_{ij}, to alleviate possible problems resulting from non-stationary time series. The panel model in first differences looks as follows:

$$\Delta ln(\textit{Sales of arms and military services})_{ijt} = \delta_1 \, \Delta ln(\textit{National defense spending})_{jt} + \delta_2 \, \Delta ln(\textit{Arms exports})_{jt} + \delta_3 \, \Delta ln(\textit{Arms imports})_{jt} + \varepsilon_{ijt} \quad (3)$$

Standard errors ε_{ijt} are again clustered at the company level and robust to serial correlation and heteroscedasticity. The fixed effects and the first differences log-log panel data models are both estimated with ordinary least squares. The estimated coefficients δ_1, δ_2, and δ_3 are interpreted like the coefficients of the panel fixed effects model. Hypotheses 1 to 3 imply that the estimated coefficients yield $\beta_1 > 0$, $\beta_2 > 0$ and $\beta_3 \geq 0$ for the fixed effects model and $\delta_1 > 0$, $\delta_2 > 0$ and $\delta_3 \geq 0$ for the first differences model.

Since—as discussed in Section 4.2— companies produce arms "on demand" contingent to defense budgets which are adopted before the beginning of the fiscal year (and include orders which have been placed well in advance or even long-term procurement projects with long lead times) and arms exports which have been approved in advance, reverse causality is less likely in this empirical supply-demand model. Omitted variable bias cannot be ruled out though factors influencing the sales of arms and military services both directly and indirectly through

the channels of defense spending and arms trade are unlikely to exist on a large scale and to substantially bias the results. In any event, I include further control variables on the country-level in a robustness test.

4.5 Empirical results

4.5.1 Baseline results

Table 4.1 shows the baseline estimation results for the fixed effects and the first differences model.[15] The 44 and the 195 companies of the balanced and the unbalanced company panel reported in Table 4.1 indicate six companies more than included in the panels because sales figures for the two trans-European companies Airbus and MBDA have been allocated to individual countries according to employee shares (see footnote 14). The results for the fixed effects model indicate that if national defense spending increases by 1%, the sales by domestic arms-producing and military services companies increase by 1.1% for the balanced company panel and by 1.2% for the unbalanced company panel. The estimates of the first differences model are similar in size and statistically significant at the 1% level, too. An elasticity above one is plausible following the assumption stated in Section 4.2 that increases in national defense spending in the short run influence equipment expenditure more than they influence personnel expenditure. If total national defense spending increases by 1%, equipment spending is therefore likely to increase by more than 1%. If exports of major conventional weapons increase by 1%, the sales by domestic arms-producing and military services companies increase by almost 0.2% for the balanced company panel and by almost 0.1% for the unbalanced company panel; both estimates are statistically significant at the 1% level. Estimation results in first differences are considerably smaller (0.05% and 0.03%), but statistically significant at the 1% level, too. Imports of major conventional weapons do not turn out to be statistically significant

[15] The number of observations and the number of companies and subsidiaries in the unbalanced panels is reduced in the first differences model because of the first differenced time series.

for both the balanced and the unbalanced company panel neither in the fixed effects model nor in the first differences model. The empirical results of both empirical models for the two company panels, i.e. that $\beta_1 > 0$, $\beta_2 > 0$, $\beta_3 \geq 0$ and $\delta_1 > 0$, $\delta_2 > 0$, $\delta_3 \geq 0$, support hypotheses 1 to 3.

TABLE 4.1: BASELINE ESTIMATION RESULTS

Fixed effects model	(1) Balanced Company	(2) Unbalanced Company	(3) Unbalanced Subsidiary
Sales of arms and military services[a]	Panel	Panel	Panel
National defense spending[a]	1.087***	1.208***	0.908**
	(0.234)	(0.137)	(0.417)
Exports of major conventional weapons[a]	0.194***	0.089***	-0.004
	(0.048)	(0.024)	(0.064)
Imports of major conventional weapons[a]	0.014	0.016	-0.034*
	(0.015)	(0.011)	(0.019)
Company Fixed Effects	yes	yes	yes
Observations	660	1,460	252
Companies (for column (3): Subsidiaries)	44	195	74
R^2 Overall	0.106	0.045	0.447
R^2 Within	0.203	0.234	0.094
R^2 Between	0.108	0.057	0.391

First differences model	(1) Balanced Company	(2) Unbalanced Company	(3) Unbalanced Subsidiary
Sales of arms and military services[a]	Panel	Panel	Panel
National defense spending[a]	0.977***	1.202***	1.064**
	(0.155)	(0.123)	(0.491)
Exports of major conventional weapons[a]	0.051***	0.027***	-0.025
	(0.015)	(0.010)	(0.051)
Imports of major conventional weapons[a]	0.008	0.002	-0.029*
	(0.008)	(0.007)	(0.015)
Observations	616	1,238	170
Companies (for column (3): Subsidiaries)	44	162	44
R^2	0.117	0.117	0.061

Notes: Standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively. All regressions apply standard errors clustered at the country level. [a] denotes variables which are expressed in their natural logarithms.

Results are different in column (3) which shows results for the unbalanced subsidiary panel. If national defense spending increases by 1%, the sales by subsidiaries located in this country increase by 0.9% according to the fixed effects model; the first differences estimate for national defense spending is slightly larger. Compared to columns (1) and (2), the coefficients are statistically significant only at the 5% level because, in terms of national defense, countries might rely less on arms produced by foreign subsidiaries than on arms produced by domestic companies. Exports of major conventional weapons do not turn out to be statistically significant neither in the fixed effects model nor in the first difference model, because foreign companies

might place their subsidiaries in countries which domestically absorb large shares of the subsidiaries' production rather than producing military goods for export (e.g. the BAE subsidiary in the United States). Arms-producing companies are often blamed to circumvent arms export bans (e.g. to conflict countries or autocracies) by using foreign subsidiaries or licensed arms production abroad. The results on export effects for the unbalanced subsidiary panel, however, at least do not indicate that these are large-scale practices. Imports are negative and statistically significant at the 10% level for both empirical models because a government's rationale not to import military goods which are also domestically produced (see Hypothesis 3) is likely to be reduced for military goods provided by a foreign company's subsidiary. The results for the unbalanced subsidiary panel need to be interpreted with due caution because—as the number of observations and the number of subsidiaries indicate—the average time series for subsidiaries are quite short.

4.5.2 Robustness tests

I examine the robustness of the empirical results in several ways. Given the market structure for military goods, national defense spending and arms exports describe the two channels national arms production flows into. It cannot, however, be ruled out that other factors at the country level contribute to both the sales by arms-producing and military services companies and to national defense spending or arms exports. I therefore include five control variables at the country level to reduce possible omitted variables bias: a war dummy for involvement in an internal or interstate war, a proxy variable for internal stability and domestic conflict probability, the natural logarithm of GDP in constant (2010) US dollars to capture business cycle effects and to account for the presumably positive relationship between defense spending and economic growth (see Alptekin and Levine 2012), an index for trade globalization accounting for the trade in goods and services and for trade partners diversification and a

continuous democracy measure describing political institutions.[16] Including these control variables does not change the inferences from the baseline results (see Table A4.4 in Appendix II). Estimates of the added control variables are only rarely significant because these country-level variables explain arms sales mainly via the channels of defense spending and arms trade rather than directly influencing arms sales. In column (2) of the fixed effects model, the war dummy is negative and statistically significant at the 5% level. This result might indicate that after controlling for defense spending arms sales are lower during wartime when defense spending is commonly high, i.e. that the effect of national defense spending might otherwise overestimate arms sales in periods of armed conflict. War does, however, not turn out to be statistically significant neither in the other fixed effects nor in the first differences models. Internal threat does not turn out to be statistically significant in the unbalanced company panel, which is the only panel with within-country variation for this variable. GDP has positive but only rarely significant estimates in the fixed effects and the first differences model for the company panels and shows that economic growth positively influences arms sales by arms-producing companies. Trade globalization is statistically significant in column (3) only and indicates that trade integration is positively related to the amount of military goods produced by subsidiaries of foreign firms. The continuous democracy measure is statistically significant only in column (2) for the fixed effects model.[17]

Arms sales figures of one year might not exclusively contain sales volumes of military goods delivered in the respective year. It is likely that sales figures also include advance payments, especially for major orders which might be paid in several installments rather than

[16] Data for armed conflicts is taken from the "UCDP/PRIO Armed Conflict Dataset" by Gleditsch et al. (2002). Data for the proxy variable for internal stability and domestic conflict probability is taken from the "Major Episodes of Political Violence (MEPV) and Conflict Regions, 1946-2016" dataset. Data for GDP is taken from the World Development Indicators of the World Bank. The index for trade globalization is a subset of the KOF Globalization Index (Dreher 2006, Gygli et al. 2019, see Potrafke 2015 for a survey). The continuous democracy index (CSVMDI) is based on machine learning techniques and provided by Gründler and Krieger (2016, 2018).

[17] Controlling for government ideology (see Comola 2012 and Brender 2018) using data from the Database of Political Institutions does not change the inferences either, however, the number of observations is reduced for governments which cannot be categorized by means of leftwing-rightwing patterns (results not reported).

upon delivery. National defense spending may reflect such payment smoothing, however, export TIV for major conventional weapons reflect actual deliveries irrespective of the actual payment flow. I therefore estimate the model including lead values for the exports of major conventional weapons to allow for less contemporaneity in the payments for sold arms (i.e. the sales by arms-producing companies) and their actual delivery. Lead values for the exports of major conventional weapons are thus supposed to capture the effects of advance payments. Inferences from the baseline results in Table 4.1 do not change when lead values of up to three years for the exports of major conventional weapons are added to the regression (see Table A4.5 in Appendix II). An increase in approved exports of major conventional weapons in the subsequent year ($t + 1$) is significantly positively related to an increase in the arms sales by arms-producing companies according to both fixed effects and first differences results. Further lead values for the exports of major conventional weapons are only rarely statistically significant.[18]

It is a worthwhile endeavor to more specifically delimit elements of domestic and foreign demand which determine the sales of military goods. First, NATO provides data on national defense spending and on the equipment spending share of total defense spending, which allows to construct figures for military equipment spending of NATO countries in million constant (2015) US dollars.[19] Equipment spending (as a subset of overall defense spending) might more accurately approximate the domestic demand arms-producing companies have to meet. The fixed effects estimates for military equipment spending of NATO countries are less than half the size compared to the coefficients for overall defense spending (see Table A4.6 in Appendix II). The first differences estimates for military equipment spending are also considerably smaller and statistically significant only at the 5% level for the balanced and at

[18] The number of observations decreases from column to column because the latest year is dropped from the sample for each additional lead value of the export variable. The number of companies and subsidiaries is reduced by those companies and subsidiaries for which no further lead values are available.

[19] NATO reports do not include military expenditure in constant US dollars for years prior to 2010. I therefore deflated military expenditure in current US dollars by using the US GDP deflator from the World Bank.

the 10% level for the unbalanced company panel. It is reasonable that coefficients for overall defense spending are larger because—as mentioned earlier—increases in overall defense spending often imply that equipment spending increases disproportionately. Imports are now statistically highly significant according to fixed effects estimation results in columns (1) and (2) and have a positive sign. This result is in line with hypothesis 3 that arms sales by a country's largest arms-producing companies do not decrease if arms imports increase. The positive sign for imports even confirms that imported and domestically produced arms are complements rather than substitutes: increasing arms imports might coincide with increasing national arms production because of, for example, larger procurement activities, and imported components for domestically produced arms might give rise to reverse causality which further explains the positive sign for arms imports. Equipment spending does not turn out to be statistically significant for subsidiaries in column (3)—neither for the fixed effects nor for the first differences model.

Second, SIPRI provides data on arms transfers at the level of individual deals which allows to distinguish between new, used and used but modernized major conventional weapons. Excluding exports of used major conventional weapons from the estimation might more accurately approximate the foreign demand companies face because arms-producing companies are involved in the production of new arms or the modernization of used arms only. As described in Section 4.2, aggregated data for the exports of new and used but modernized weapons is similar to the data for the exports of all weapons: only 12% of the tracked arms export deals of all major conventional weapons between 2002 and 2016 have been exports of used weapons (2% were export deals of used but modernized weapons and 86% were export deals of new weapons) and used and used but modernized weapons, moreover, only receive 40% and 66% of a new weapon's trend indicator value. I do not distinguish between new and used imported weapons, because procurement projects for the armed forces can include both new and used imported weapons and both are able to substitute domestically produced weapons.

The results for approved exports of new and used but modernized major conventional weapons confirm the previous inferences from Table 4.1 regarding the effect of arms exports on companies' sales of military goods (see Table A4.7 in Appendix II).[20]

4.6 Country-specific results

Differences in country-specific results might indicate the extent to which an arms industry serves security or economic purposes. Figures 4.2 and 4.3 showed that in the United States and the United Kingdom, sales figures follow national defense spending rather than exports of major conventional weapons; for Germany, however, sales figures seem to follow the exports of major conventional weapons rather than national defense spending. I estimate country-specific coefficients for the explanatory variables to examine this heterogeneity among countries. Figure 4.4 illustrates coefficient estimates and their 95% confidence intervals according to the fixed effects and the first differences model for the balanced panel. Results for countries with at least three companies in the panel are shown, which includes those countries where the majority of the top 100 arms-producing and military services companies is located. National defense spending is significantly and positively related to the sales by arms-producing and military services companies in Israel, the United Kingdom and the United States (in Israel according to the fixed effects model only); this result reflects the baseline estimation results. For France, Germany and Italy, however, national defense spending does not turn out to be statistically significant. Arms exports surprisingly do not turn out to be statistically significant for France, one of the world's largest arms-producing and arms-exporting countries. In Germany, Israel, Italy and the United Kingdom, in turn, exports of major conventional weapons are significantly positively related to the sales by arms-producing and military services companies (in Israel and Italy according to the fixed effects model only). Arms exports from the United States do not

[20] As a further robustness test, all three panels are estimated excluding the trans-European companies Airbus (former EADS) and MBDA, for which sales figures have been allocated according to the employee share in individual countries. Inferences from the baseline results do not change (results not reported).

turn out to be significantly correlated with the sales by US arms-producing and military services companies. Imports of major conventional weapons do not turn out to be statistically significant according to both the fixed effects and the first differences model for any of the six countries.

Figure 4.5 illustrates country-specific results of the fixed effects and the first differences model for the unbalanced company panel, thus including more countries. The results confirm

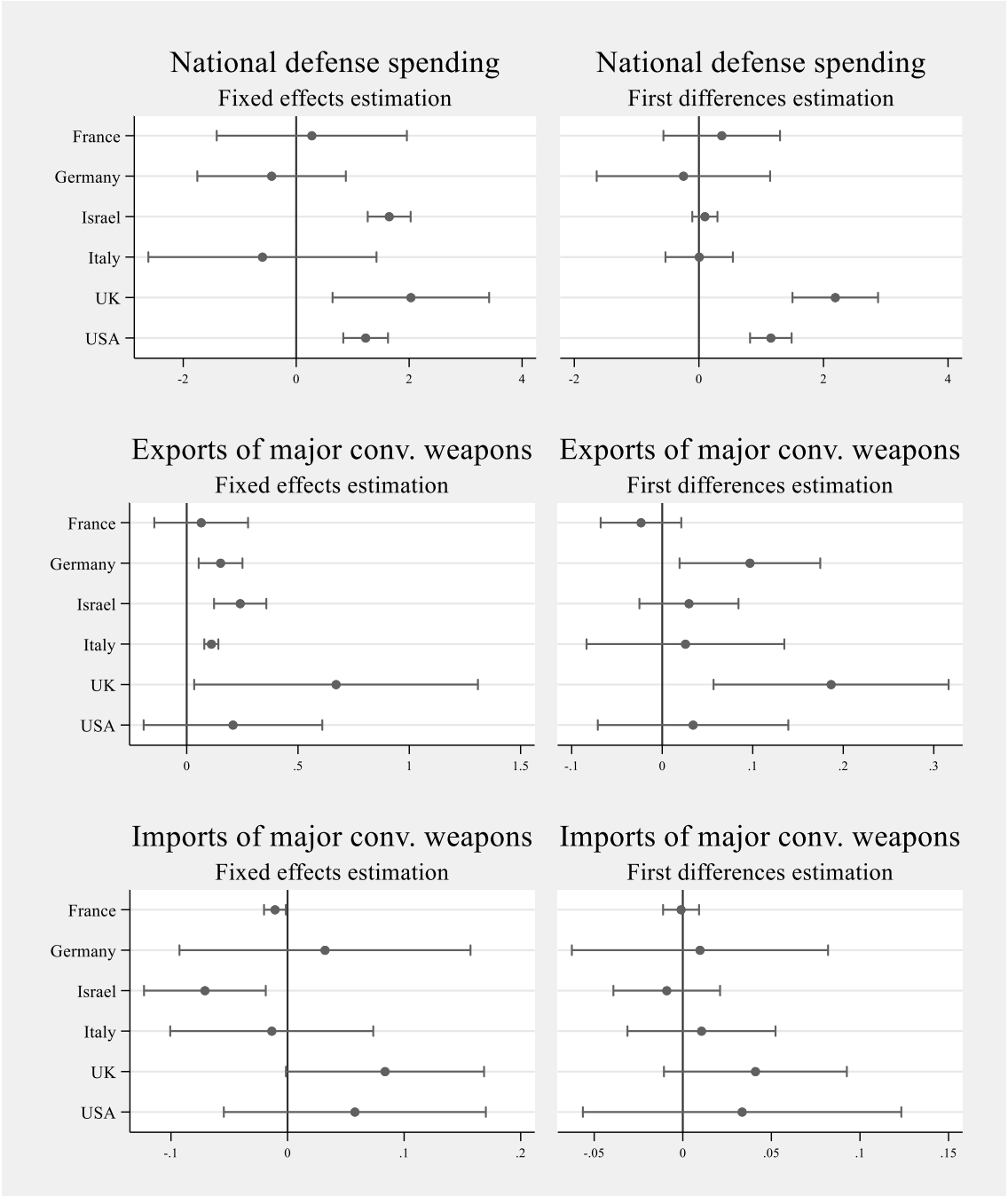

FIGURE 4.4: COUNTRY-SPECIFIC COEFFICIENTS AND 95% CONFIDENCE INTERVALS FROM FIXED EFFECTS AND FIRST DIFFERENCES ESTIMATION FOR THE BALANCED PANEL

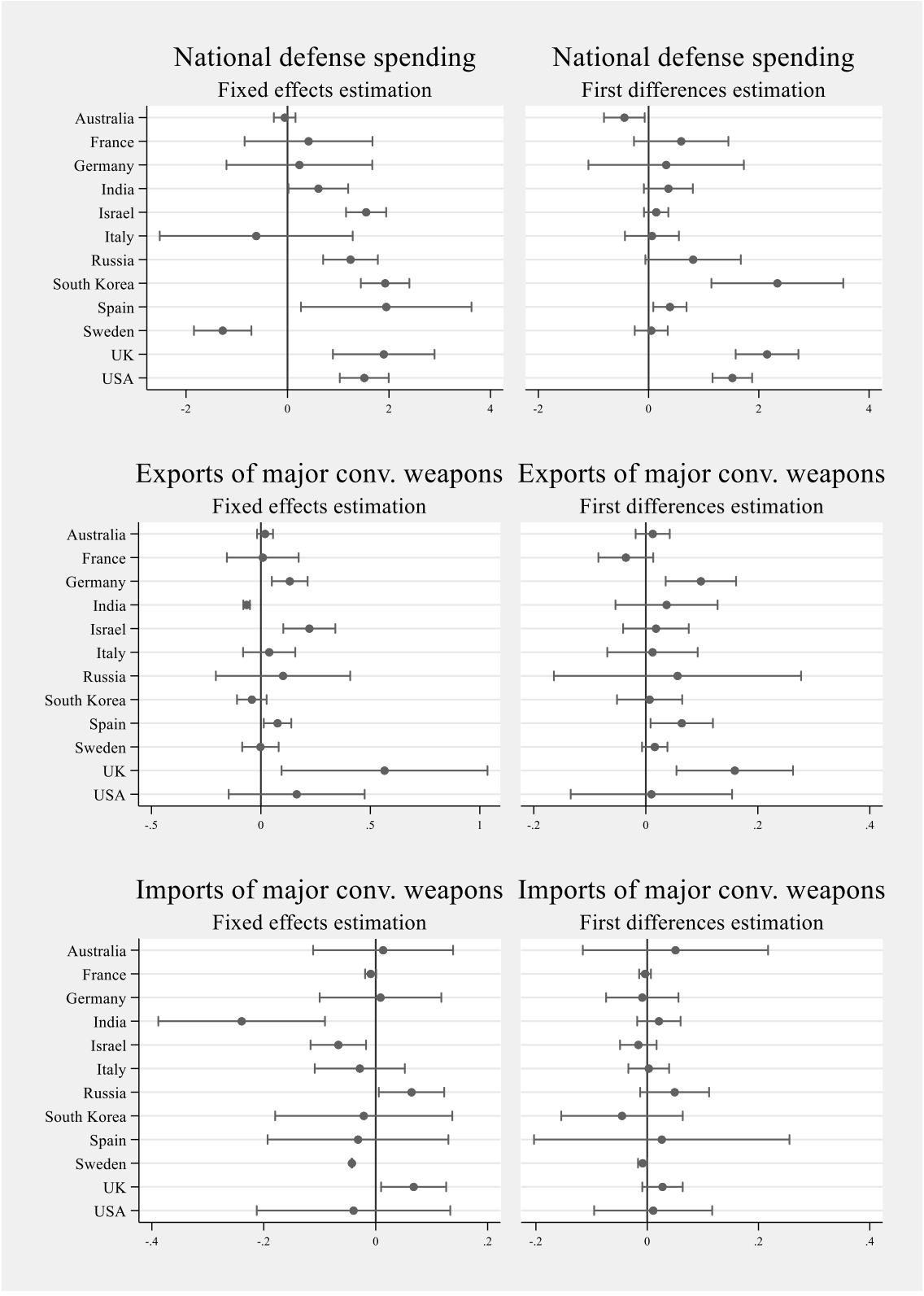

FIGURE 4.5: COUNTRY-SPECIFIC COEFFICIENTS AND 95% CONFIDENCE INTERVALS FROM FIXED EFFECTS AND FIRST DIFFERENCES ESTIMATION FOR THE UNBALANCED PANEL

the inferences for the six countries shown in Figure 4.4. National defense spending in India, Russia, South Korea and Spain is significantly and positively related to the sales by arms-producing and military services companies in these countries according to fixed effects results

(according to the results in first differences, this holds for South Korea and Spain only). Exports of major conventional weapons are significantly and positively related to the sales by arms-producing and military services companies in Spain.

The results support hypothesis 4 and suggest differences among countries in the extent to which the arms industry serves security or economic purposes. Arms-producing companies in the United States serve the superpower's security concerns and develop and produce arms primarily for the country's own armed forces. Arms exports by the United States are likely to be solely an externality of the strong domestic demand for military goods. The same holds for strong military powers like India, Israel, Russia and South Korea as well as for Spain and the United Kingdom. In Israel, Spain and the United Kingdom, however, arms exports significantly determine the sales of military goods by domestic arms-producing companies, too. In Germany, a country with low levels of defense spending, arms exports and the economic returns they generate primarily determine the sales of military goods by German defense companies; national defense spending only seems to subordinately contribute to the sales of military goods. The lack of statistical significance of national defense spending for the sales of military goods reflects that countries like Germany and France have—among other countries—often been criticized by the United States for free-riding on the United States' defense burden within NATO. In times of low levels of defense spending and, thus, low domestic demand for arms, an orientation towards arms exports might ensure the survival of the domestic arms industry and its innovative capacity. Arms exports thus might work like a subsidy the government does not need to pay for, safeguard jobs and ensure that defense capacities can later be increased if necessary. Considering the tremendous R&D cost for new weapon systems and high cost of manufacturing, arms exports are often necessary to realize economies of scale and to reduce procurement cost, thus making national security even affordable. Export-oriented arms industries like Germany, however, might be less effective in meeting the requirements of the own armed forces when it comes to domestic orders.

Table 4.2 shows estimation results when the large share of US-companies and subsidiaries located in the United States is excluded from the three panels. Compared to the baseline estimation results of the fixed effects model, national defense spending does no longer turn out to be statistically significant in columns (1) and (3), however, inferences for the unbalanced company panel in column (2) are unchanged. Inferences regarding the estimation results of the first differences model are—apart from smaller estimates for national defense spending—unchanged for any of the three panels.

TABLE 4.2: ESTIMATION RESULTS EXCLUDING COMPANIES AND SUBSIDIARIES IN THE UNITED STATES

Fixed effects model	(1)	(2)	(3)
	Balanced Company	Unbalanced Company	Unbalanced Subsidiary
Sales of arms and military services[a]	Panel	Panel	Panel
National defense spending[a]	0.364	0.929***	0.128
	(0.514)	(0.210)	(0.325)
Exports of major conventional weapons[a]	0.157***	0.076***	-0.015
	(0.036)	(0.022)	(0.050)
Imports of major conventional weapons[a]	0.010	0.019	-0.011
	(0.015)	(0.012)	(0.014)
Company Fixed Effects	yes	yes	yes
Observations	405	838	186
Companies (for column (3): Subsidiaries)	27	110	58
R^2 Overall	0.045	0.068	0.132
R^2 Within	0.054	0.150	0.003
R^2 Between	0.045	0.066	0.149

First differences model	(1)	(2)	(3)
	Balanced Company	Unbalanced Company	Unbalanced Subsidiary
Sales of arms and military services[a]	Panel	Panel	Panel
National defense spending[a]	0.619**	0.798***	0.675**
	(0.241)	(0.153)	(0.316)
Exports of major conventional weapons[a]	0.048***	0.028***	-0.024
	(0.016)	(0.010)	(0.052)
Imports of major conventional weapons[a]	0.008	0.005	-0.027*
	(0.008)	(0.007)	(0.016)
Observations	378	710	120
Companies (for column (3): Subsidiaries)	27	88	35
R^2	0.039	0.057	0.035

Notes: Standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively. All regressions apply standard errors clustered at the country level. [a] denotes variables which are expressed in their natural logarithms.

4.7 Conclusion

This chapter presented new empirical evidence on how supply and demand in the market for military goods are related. I examined how national defense spending and arms transfers relate to the sales of military goods by arms-producing companies. The sample included data for up

to 195 arms-producing and military services companies in 21 countries for the period 2002-2016. The results of the fixed effects model indicated that if national defense spending increases by 1%, the arms sales by a country's largest arms-producing and military services companies increase by up to 1.2%. If exports of major conventional weapons increase by 1%, arms sales by these companies increase by up to 0.2%. Estimation results in first differences were similar for national defense spending, however, the elasticity of companies' arms sales with regard to the exports of major conventional weapons was considerably smaller. Arms imports were not shown to affect domestic arms sales, because countries mainly import arms they do not produce themselves. Imported arms and arms produced by domestic arms manufacturers are, thus, complements rather than substitutes.

Country-specific estimation results suggest differences among countries in the extent to which an arms industry serves security or economic purposes. The differences allow to draw inferences regarding the structure of a country's arms industry. In the United States and Russia, the arms industry's purpose is to provide the own armed forces and to guarantee self-reliance in the production of military goods to maintain the role as independent world powers. In Germany, a NATO ally surrounded by closely aligned partners and under the security umbrella of the United States, the arms industry primarily served economic purposes during the observation period. The insights into supply and demand for military goods are derived from a positive analysis and do not describe a normative claim or policy recommendations. The findings contribute to the literature on the arms industry and have implications for scholars investigating into arms trade and defense spending: differences among arms industries, for example, reflect the long-lasting debate on burden sharing within NATO.

Future research should examine the supply side of the market for military goods in more detail. Arms exports have shown to imply positive externalities and serve as substitutes for defense spending: democracies decrease national defense spending in response to increases in their arms exports to other democracies (Pamp and Thurner 2017, Pamp et al. 2018).

Investigating the extent to which arms-producing companies benefit or suffer from this shift from defense spending to arms exports is a worthwhile endeavor. Another related question is whether governments even balance out domestic arms orders and arms exports to smooth national arms production. Governments might, for instance, approve arms exports in times of decreased defense spending—meaning the relationship between defense spending and arms exports was reversed—to ensure the survival of the domestic arms industry.

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Appendix I

TABLE A4.1: LIST OF COUNTRIES, NUMBER OF COMPANIES AND SUBSIDIARIES AND COUNTRY RANKS IN DEFENSE SPENDING, EXPORTS AND IMPORTS

Country	**Companies**		**Subsidiaries**	**Rank: National defense spending (2016)**	**Rank: Exports of major conventional weapons (2016)**	**Rank: Imports of major conventional weapons (2016)**
	Balanced company panel	*Unbalanced company panel*	*Unbalanced subsidiary panel*			
Australia	-	4	3	12	20	8
Brazil	-	1	-	13	22	44
Canada	-	1	2	14	19	34
Finland	-	1	-	47	26	37
France	5	14	9	5	4	62
Germany	5	8	5	9	3	59
India	-	4	-	6	30	1
Israel	3	6	-	16	6	15
Italy	3	5	14	11	8	13
Japan	-	4	-	8	-	23
Netherlands	-	-	1	25	12	47
Norway	-	1	-	30	18	42
Poland	-	2	-	24	43	49
Russia	-	20	9	3	2	41
South Korea	-	10	2	10	9	7
Spain	1	5	1	17	11	54
Sweden	1	3	3	33	14	55
Switzerland	1	2	-	37	15	57
Turkey	-	2	-	15	13	22
Ukraine	-	1	-	46	10	-
United Kingdom	8	16	9	7	7	30
United States	17	85	16	1	1	16
Total # of companies	**44**	**195**	**74**			
Total # of countries	**9**	**21**	**12**			

TABLE A4.2: SUMMARY STATISTICS

	Observations	Mean	Std. Dev.	Min	p25	p75	Max
Balanced Company Panel							
Sales of arms and military services	660	6,291	8,871	328	1,345	6,125	40,830
National defense spending	135	96,069	197,148	4,220	14,783	52,739	768,466
Exports of major conventional weapons	135	1,676	2,333	16	422	1,697	10,304
Imports of major conventional weapons	135	290	286	1	74	451	1,196
Unbalanced Company Panel							
Sales of arms and military services	1,460	3,683	6,480	328	835	2,945	40,830
National defense spending	333	83,444	179,179	3,063	15,030	51,763	768,466
Exports of major conventional weapons	333	1,513	2368	2	175	1,474	10,304
Imports of major conventional weapons	333	444	604	1	99	563	5,322
Unbalanced Subsidiary Panel							
Sales of arms and military services	252	2,281	3,060	395	757	2,543	22,261
National defense spending	89	139,987	231,175	4,882	26,383	55,922	768,466
Exports of major conventional weapons	89	2,536	3,053	14	512	4,967	10,304
Imports of major conventional weapons	89	410	374	2	104	572	1,574

Notes: Sales of arms and military services and national defense spending are in million constant (2016) US dollars. Exports and imports of major conventional weapons are trend indicator values (TIV) in million.

TABLE A4.3: CORRELATIONS

	Sales of arms and military services[a]	National defense spending[a]	Exports of major conventional weapons[a]	Imports of major conventional weapons[a]
Balanced Company Panel				
Sales of arms and military services[a]	1.000			
National defense spending[a]	0.326	1.000		
Exports of major conventional weapons[a]	0.302	0.839	1.000	
Imports of major conventional weapons[a]	0.193	0.494	0.292	1.000
Observations	660	135	135	135
Unbalanced Company Panel				
Sales of arms and military services[a]	1.000			
National defense spending[a]	0.209	1.000		
Exports of major conventional weapons[a]	0.223	0.619	1.000	
Imports of major conventional weapons[a]	0.051	0.357	-0.180	1.000
Observations	1,460	333	333	333
Unbalanced Subsidiary Panel				
Sales of arms and military services[a]	1.000			
National defense spending[a]	0.667	1.000		
Exports of major conventional weapons[a]	0.457	0.743	1.000	
Imports of major conventional weapons[a]	0.243	0.292	-0.166	1.000
Observations	252	89	89	89

Notes: [a] denotes variables which are expressed in their natural logarithms.

Appendix II

TABLE A4.4: ESTIMATION RESULTS WITH ADDITIONAL CONTROL VARIABLES AT THE COUNTRY-LEVEL

Fixed effects model	(1)	(2)	(3)
	Balanced Company	Unbalanced Company	Unbalanced Subsidiary
Sales of arms and military services[a]	Panel	Panel	Panel
National defense spending[a]	0.933***	1.108***	0.973**
	(0.212)	(0.133)	(0.401)
Exports of major conventional weapons[a]	0.120***	0.069***	0.016
	(0.040)	(0.018)	(0.053)
Imports of major conventional weapons[a]	0.024	0.014	-0.042*
	(0.016)	(0.011)	(0.023)
War	-0.094	-0.130**	0.042
	(0.104)	(0.066)	(0.059)
Internal threat	-	-0.040	-
		(0.037)	
GDP[a]	0.558*	0.198	-0.195
	(0.316)	(0.254)	(0.811)
Trade globalization	0.008	0.007	0.029***
	(0.007)	(0.005)	(0.009)
Continuous democracy measure (CSVMDI)	2.477	1.574**	-0.307
	(1.874)	(0.735)	(1.551)
Company Fixed Effects	yes	yes	yes
Observations	660	1,460	252
Companies (for column (3): Subsidiaries)	44	195	74
R^2 Overall	0.109	0.050	0.419
R^2 Within	0.264	0.259	0.155
R^2 Between	0.110	0.064	0.360

First differences model	(1)	(2)	(3)
	Balanced Company	Unbalanced Company	Unbalanced Subsidiary
Sales of arms and military services[a]	Panel	Panel	Panel
National defense spending[a]	0.943***	1.078***	1.083**
	(0.167)	(0.140)	(0.482)
Exports of major conventional weapons[a]	0.046***	0.021**	-0.022
	(0.017)	(0.010)	(0.051)
Imports of major conventional weapons[a]	0.008	0.000	-0.028
	(0.008)	(0.006)	(0.017)
War	0.008	0.011	0.012
	(0.011)	(0.010)	(0.020)
Internal threat	-	-0.030	-
		(0.038)	
GDP[a]	0.399	0.518**	-0.176
	(0.340)	(0.208)	(0.860)
Trade globalization	0.000	-0.003	0.011*
	(0.003)	(0.002)	(0.006)
Continuous democracy measure (CSVMDI)	0.265	0.232	-0.855
	(0.557)	(0.259)	(1.301)
Observations	616	1,238	170
Companies (for column (3): Subsidiaries)	44	162	44
R^2	0.125	0.129	0.076

Notes: Standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively. All regressions apply standard errors clustered at the country level. [a] denotes variables which are expressed in their natural logarithms.

TABLE A4.5: ESTIMATION RESULTS INCLUDING LEADS FOR THE EXPORTS OF MAJOR CONVENTIONAL WEAPONS

Fixed effects model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Balanced Company Panel	Balanced Company Panel	Balanced Company Panel	Unbalanced Company Panel	Unbalanced Company Panel	Unbalanced Company Panel	Unbalanced Subsidiary Panel	Unbalanced Subsidiary Panel	Unbalanced Subsidiary Panel
Sales of arms and military services[a]									
National defense spending[a]	1.138***	1.215***	1.240***	1.266***	1.246***	1.249***	1.252**	1.322**	1.054*
	(0.230)	(0.226)	(0.229)	(0.144)	(0.156)	(0.162)	(0.474)	(0.533)	(0.590)
Exports of major conventional weapons[a]	0.136***	0.135***	0.133***	0.069***	0.090***	0.112***	0.019	0.097	0.054
	(0.040)	(0.038)	(0.040)	(0.022)	(0.024)	(0.027)	(0.067)	(0.078)	(0.161)
Imports of major conventional weapons[a]	0.004	0.001	-0.004	0.008	0.011	0.004	-0.061**	-0.056**	-0.043
	(0.015)	(0.016)	(0.018)	(0.011)	(0.012)	(0.013)	(0.026)	(0.023)	(0.026)
Exports of major conventional weapons[a] (t + 1)	0.126***	0.112***	0.123***	0.069***	0.059***	0.088***	0.133	0.129	0.274*
	(0.039)	(0.034)	(0.035)	(0.021)	(0.020)	(0.025)	(0.122)	(0.138)	(0.154)
Exports of major conventional weapons[a] (t + 2)		0.039	0.014		0.031	-0.003		0.036	0.006
		(0.031)	(0.026)		(0.022)	(0.021)		(0.106)	(0.104)
Exports of major conventional weapons[a] (t + 3)			0.055			0.052*			0.007
			(0.044)			(0.028)			(0.173)
Company Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	616	572	528	1,238	1,061	911	170	125	94
Companies (for columns (7) - (9): Subsidiaries)	44	44	44	162	142	128	44	31	21
R^2 Overall	0.108	0.109	0.107	0.047	0.045	0.051	0.455	0.485	0.462
R^2 Within	0.239	0.272	0.305	0.259	0.274	0.303	0.173	0.201	0.209
R^2 Between	0.109	0.110	0.108	0.058	0.029	0.044	0.316	0.417	0.372

First differences model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Balanced Company Panel	Balanced Company Panel	Balanced Company Panel	Unbalanced Company Panel	Unbalanced Company Panel	Unbalanced Company Panel	Unbalanced Subsidiary Panel	Unbalanced Subsidiary Panel	Unbalanced Subsidiary Panel
Sales of arms and military services[a]									
National defense spending[a]	1.045***	1.043***	1.016***	1.224***	1.248***	1.164***	1.346**	1.538*	1.278
	(0.159)	(0.161)	(0.159)	(0.131)	(0.137)	(0.134)	(0.574)	(0.758)	(0.762)
Exports of major conventional weapons[a]	0.069***	0.071***	0.089***	0.035**	0.034**	0.058***	0.026	0.034	0.089
	(0.015)	(0.016)	(0.019)	(0.015)	(0.015)	(0.019)	(0.052)	(0.064)	(0.123)
Imports of major conventional weapons[a]	0.007	0.004	-0.001	0.002	0.002	-0.001	-0.039*	-0.035*	-0.027
	(0.009)	(0.009)	(0.011)	(0.007)	(0.007)	(0.008)	(0.023)	(0.020)	(0.023)
Exports of major conventional weapons[a] (t + 1)	0.063***	0.070***	0.088***	0.032**	0.043***	0.058***	0.123	0.137	0.278*
	(0.017)	(0.019)	(0.023)	(0.013)	(0.014)	(0.018)	(0.081)	(0.118)	(0.133)
Exports of major conventional weapons[a] (t + 2)		0.009	0.025		0.026	0.025		0.028	0.119
		(0.015)	(0.020)		(0.017)	(0.016)		(0.050)	(0.088)
Exports of major conventional weapons[a] (t + 3)			0.034*			0.047***			0.180
			(0.019)			(0.016)			(0.121)
Observations	572	528	484	1,061	911	779	125	94	73
Companies (for columns (7) - (9): Subsidiaries)	44	44	44	142	128	111	31	21	17
R^2	0.144	0.152	0.161	0.128	0.148	0.155	0.111	0.131	0.164

Notes: Standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively. All regressions apply standard errors clustered at the country level. [a] denotes variables which are expressed in their natural logarithms.

TABLE A4.6: ESTIMATION RESULTS FOR THE EQUIPMENT SPENDING OF NATO COUNTRIES

Fixed effects model	(1)	(2)	(3)
	Balanced Company	Unbalanced Company	Unbalanced Subsidiary
Sales of arms and military services[a]	Panel	Panel	Panel
Equipment spending[a]	0.380***	0.413***	0.180
	(0.111)	(0.084)	(0.284)
Exports of major conventional weapons[a]	0.220***	0.174***	0.078
	(0.050)	(0.037)	(0.099)
Imports of major conventional weapons[a]	0.042**	0.041***	-0.000
	(0.019)	(0.014)	(0.024)
Company Fixed Effects	yes	yes	yes
Observations	585	1,162	197
Companies (for column (3): Subsidiaries)	39	139	57
R^2 Overall	0.043	0.013	0.398
R^2 Within	0.155	0.127	0.018
R^2 Between	0.041	0.008	0.353

First differences model	(1)	(2)	(3)
	Balanced Company	Unbalanced Company	Unbalanced Subsidiary
Sales of arms and military services[a]	Panel	Panel	Panel
Equipment spending[a]	0.094**	0.093*	-0.098
	(0.038)	(0.051)	(0.162)
Exports of major conventional weapons[a]	0.057***	0.060***	-0.023
	(0.017)	(0.015)	(0.055)
Imports of major conventional weapons[a]	0.014	0.010	-0.023
	(0.009)	(0.006)	(0.015)
Observations	546	1,006	138
Companies (for column (3): Subsidiaries)	39	124	36
R^2	0.030	0.018	0.016

Notes: Standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively. All regressions apply standard errors clustered at the country level. [a] denotes variables which are expressed in their natural logarithms.

TABLE A4.7: ESTIMATION RESULTS FOR THE EXPORTS OF NEW AND USED BUT MODERNIZED MAJOR CONVENTIONAL WEAPONS

Fixed effects model	(1)	(2)	(3)
	Balanced Company	Unbalanced Company	Unbalanced Subsidiary
Sales of arms and military services[a]	Panel	Panel	Panel
National defense spending[a]	1.064***	1.194***	0.913**
	(0.232)	(0.137)	(0.416)
Exports of *new* and *modernized* major conventional weapons[a]	0.201***	0.100***	-0.011
	(0.048)	(0.026)	(0.066)
Imports of major conventional weapons[a]	0.019	0.018	-0.035*
	(0.016)	(0.011)	(0.018)
Company Fixed Effects	yes	yes	yes
Observations	660	1,453	252
Companies (for column (3): Subsidiaries)	44	191	74
R^2 Overall	0.107	0.045	0.448
R^2 Within	0.215	0.239	0.094
R^2 Between	0.109	0.056	0.391

First differences model	(1)	(2)	(3)
	Balanced Company	Unbalanced Company	Unbalanced Subsidiary
Sales of arms and military services[a]	Panel	Panel	Panel
National defense spending[a]	0.976***	1.231***	1.071**
	(0.154)	(0.123)	(0.491)
Exports of *new* and *modernized* major conventional weapons[a]	0.048***	0.027***	-0.038
	(0.014)	(0.010)	(0.049)
Imports of major conventional weapons[a]	0.008	0.002	-0.029*
	(0.008)	(0.007)	(0.015)
Observations	616	1,232	170
Companies (for column (3): Subsidiaries)	44	162	44
R^2	0.116	0.118	0.063

Notes: Standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively. All regressions apply standard errors clustered at the country level. [a] denotes variables which are expressed in their natural logarithms.

5. Political Stability and Economic Prosperity: Are Coups Bad for Growth?

This chapter is joint work with Klaus Gründler.

Abstract[*]

We examine how political instability influences economic growth. We use a novel dataset on coups d'état provided by Bjørnskov and Rode (2019) to model political instability (180 countries, 1950-2017). Usage of coups helps to overcome identification problems in the study of the instability-growth nexus, as coups are difficult to anticipate. We employ panel difference-in-differences and dynamic panel data models and find that coups depress economic growth by about 2-3 percentage points. To tackle endogeneity, we follow three paths. First, we document and discuss case studies using synthetic control methods. Second, we use spatial patterns to construct instrumental variables. Third, we geocode the Bjørnskov and Rode (2019) database and exploit the geospatial dimension of coup activity based on about 2,660 sub-national regions. We also examine the effect of coups on household level outcomes using micro data for about 250,000 households in 85 countries. The results show that coups increase unemployment and have negative effects on the financial situation, health, and life satisfaction. The effects are stronger for women and for poorer households.

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5.1 Introduction

"In revolutions the occasions may be trifling but great interests are at stake."

Aristotle

Do countries need a stable political environment to prosper? With the political turmoil caused by the rise of populist politics and the downfall of established parties in many Western countries, the question of how political stability influences economic growth has become increasingly popular in both academia and the public discourse. This discussion is fueled by the observation that many countries with low political stability scores are among the most fast-growing economies on the globe, including China (rank 115 in the World Bank's 2017 political stability ranking), Indonesia (135), India (160), and Bangladesh (174).[1] First concerns are raised about the importance of stability for economic growth and development. In a 2014 World Bank column, for example, lead World Bank economist Hussain (2014) asks "can political stability hurt economic growth?".

In this chapter, we provide strong evidence against this view. Using panel data from 180 countries and 2,660 sub-national regions, our results show that political instability has negative effects on economic growth. We use coups d'états as a source of exogenous variation in political instability and find that periods of instability reduce growth by 2-3 percentage points. This result is very stable across numerous empirical specifications and occurs both on the country level and the sub-national level. We start by examining panel difference-in-differences and dynamic panel data models on the country level and discuss our general findings in case studies for which we use synthetic control estimations. In the next step, we use spatial variation in coup occurrence to estimate a causal effect of political instability on growth. First, we construct instrumental variables based on geospatial correlations on the country-level and, second, we

[1] Data comes from the World Bank's "Political Stability and Absence of Violence/Terrorism" index, which is measured annually and part of the Worldwide Governance Indicators (WGI) dataset. The country ranks are taken from the most recent version of the WGI at the time this paper is written.

exploit a newly compiled georeferenced database on coup activity on the sub-national level. The sub-national strategy provides a powerful tool to identify the effect of political instability on economic growth because it allows us to estimate the effect of a coup in regions without direct coup activity. Thus, we separate the effect of political instability from that of coup-induced violence. The parameter estimates for each of these models are very similar and support our baseline finding of a negative growth effect of 2-3 percentage points. To dig deeper into the consequences of political instability for the living conditions of individuals, we use micro data for roughly 250,000 individuals and find that instability has devastating effects on the economic situation of households.

Estimating the effect of political instability on growth is afflicted with four key challenges. First, the term "political (in)stability" is not clearly defined. The seminal paper of Alesina et al. (1996) approximates political instability with the propensity of government changes. Other studies use composite measures such as the "Worldwide Governance Indicators" (WGI) dataset from the World Bank, which consolidates multiple data series on conflict, violence, protests and terrorism into a single index of political instability (Kaufmann et al. 2010). Drawing evidence based on such variables is difficult, as they mix several forms of government changes and political violence. The growth effect of government changes may depend on whether power is transferred regularly or irregularly, and it is unclear whether political violence is the cause or the result of political instability. Second, regular government changes and lasting periods of political violence are predictable by economic agents. It is therefore challenging to identify an effect on growth based on such variables, as economic agents account for predictable changes in the political environment in their decision making. Third, violence and conflicts have direct negative effects on economic growth, and it is difficult to separate these effects from an unstable political environment. Fourth, researchers estimating the effect of political instability on growth face an inevitable endogeneity problem, because political tensions may have their roots in unfavorable economic conditions.

To tackle these challenges, we use a new dataset on coups d'état from Bjørnskov and Rode (2019) to measure political instability. The dataset provides the largest and most detailed compilation of coups and coup attempts, including 208 countries for the period 1950-2018. We follow Powell and Thyne (2011) and define coups as illegal attempts by the military or other elites within the state apparatus to unseat the sitting executive. By using coups d'états as measures for political instability, we focus on a certain aspect of political instability that is straightforward to measure and to interpret. This strategy allows us to address important problems accompanied by the measurement of political instability. Specifically, the focus on coups (i) circumvents the problem of anticipation effects, as coups are extremely difficult to predict (Zolberg 1968, Bazzi and Blattman 2014, Gassebner et al. 2016), (ii) avoids problems afflicted with the selection and aggregation of country attributes into an index of political stability, (iii) enables a clear definition of political (in)stability, which facilitates the interpretation of empirical results, and (iv) allows us to distinguish between the effect of violence and the effect of instability.

We use panel difference-in-differences models and dynamic panel data models to estimate the effect of coups d'états on economic growth. Although coups are difficult to predict with time-varying factors, our analysis shows that the ex ante probability of coups varies systematically across countries because of distinct time-invariant geospatial patterns in the occurrence of coups. We control for spatial dependency and other time-invariant factors that may confound the estimated relationship between coups and growth in a fixed effects model. To further alleviate concerns about endogenous selection into coups initiated by unfavorable economic conditions, we model pre-coup dynamics in GDP. To tackle the possibility that the relationship between coups and growth is confounded by time-varying unobservables, we use three strategies. First, we provide case study evidence using synthetic control methods. Second, we use the geospatial correlation of coups by constructing jackknifed spatial instruments that use coup occurrences in neighboring countries as instruments for domestic coups. Third, we

examine the growth effect of coups on a sub-national level, constructing a dataset of coup occurrence for 2,660 sub-national units between 1992 and 2012. We analyze each coup listed in the Bjørnskov and Rode (2019) database and geocode the coups based on multiple scholarly articles, books, and newspaper articles. To separate the effects of political instability from those of violence, we use our georeferenced coup dataset and estimate the effect of coups on growth for sub-national regions without direct coup involvement. In the last step, we estimate the effect of coups on household-level outcomes. We first provide a stylized theoretical model of labor supply in which political instability increases uncertainty about future wage payments. The model also suggests that labor supply depends on productivity, which can be affected by coups via a decrease in health and life satisfaction. We then use data from roughly 250,000 households in 85 countries (about 13,000 of which have experienced a coup d'état) to estimate the effect of coups on household-level outcomes.

Our empirical results suggest that coups have drastic consequences for economic growth. Our estimates show that coup d'états decrease economic growth by 2-3 percentage points. These results are remarkably stable across various estimation techniques and model specifications. We examine the robustness of our empirical results, accounting for regime transitions in the aftermath of coups, political institutions and their dynamics prior to coups, potential confounding factors, and different sample compositions that focus on individual continents, countries with higher ex ante probability of coups, and coups experience. In each of these models, the effect of coups d'état on economic growth is negative, similar in size, and highly statistically significant. The estimated parameters of coups in our sub-national analysis are virtually identical to our country-level outcomes, even if we control for the spatial distribution of conflict and human capital on the sub-national level. Finally, we find that coups have negative effects on the economic situation of households, increasing unemployment and decreasing financial capacities of households. The adverse effect on employment is particularly pronounced for women and is less prevalent among men. We also find that coups decrease

health and life satisfaction. The adverse effects are stronger for poorer households, while richer households are less affected by coup activities. We further document that coups depress individuals' expectations about the future and decrease the perceived importance of democracy.

Contribution to the existing literature: This chapter contributes to the literature examining the growth effect of political instability. From a theoretical viewpoint, the direction of this effect is not clear-cut. On the one hand, the traditional perspective is that political stability fosters investment (Alesina et al. 1996, Alesina and Perotti 1996). On the other hand, the Oi-Hartman-Abel effect posits that uncertainty increases investment when firms can insure against bad outcomes (Bloom 2014, Li et al. 2019). Political instability can also boost growth if the incumbent is unable or unwilling to provide property rights, an efficient legal system, or growth-increasing economic policies (Acemoglu and Robinson 2000). In a similar vein, long regime duration may increase the pervasiveness of interest-group policies and corruption (Olson 1982), which is negative for economic growth (e.g. Gründler and Potrafke 2019). Empirical evidence on the stability-growth nexus is also undetermined so far. While some studies support the pessimistic view of political instability (Barro 1991, Alesina et al. 1996, Aisen and Veiga 2013), others find indefinite relationships (Sala-i- Martín 1997, Jong-A-Pin 2009) or positive effects of instability on growth (Campos and Nugent 2003, Berggren et al. 2012). A key reason for the inconclusiveness of these studies is that they use different definitions and measures of political stability, which mix regular and irregular government changes with information on protests, violence, and civil conflict. This chapter also contributes to the literature on the relationship between coups d'états and economic growth. There is surprisingly little evidence on the political and economic consequences of coups (Lachapelle 2020). Early studies in the empirical growth literature report negative correlations between coups and economic growth (Barro 1991, Levine and Renelt 1992, Alesina et al. 1996). These studies have pioneered empirical growth research during the 1990s, but restricted

computational capacity has left important econometric concerns unconsidered, and coups mainly serve as vehicles for robustness analyses. Using the database of Bjørnskov and Rode (2019), our study substantially exceeds the number of included countries and years compared with previous studies. Exploiting our georeferenced sub-national regional dataset, we are the first to explore the effect of coups on the sub-national level.

Organization: This chapter is organized as follows. In Section 5.2, we describe our data, show how coups have developed over the past six decades, and present our georeferenced dataset on sub-national coups d'état. In Section 5.3, we report the results of our country-level analysis, relating coups to economic growth. In Section 5.4, we apply our IV approach and exploit our sub-national dataset for causal identification. Section 5.5 examines the consequences of coups for household-level outcomes. Section 5.6 summarizes our findings and discusses avenues for future research.

5.2 Data and descriptive evidence

5.2.1 Data on coups d'états

We measure coups d'état employing a novel dataset on regime types and regime changes compiled by Bjørnskov and Rode (2019). The dataset covers all coup attempts from 1950 to 2018 and indicates whether a coup has been successful or whether it failed. The dataset also includes the group which led the coup (e.g. civilian or military), the name of the coup leaders, and their military or civilian rank. There have been multiple coups in some of the countries included in the dataset, and the dataset covers detailed information also for second or third coup attempts. In total, the dataset includes 537 coups or coup attempts that took place in 498 country-year observations, 34 of which included a second or third coup.[2]

[2] Unless indicated otherwise, we use the term "coup" in the remainder of this article for all coup attempts irrespective of whether the coup was successful or whether it failed.

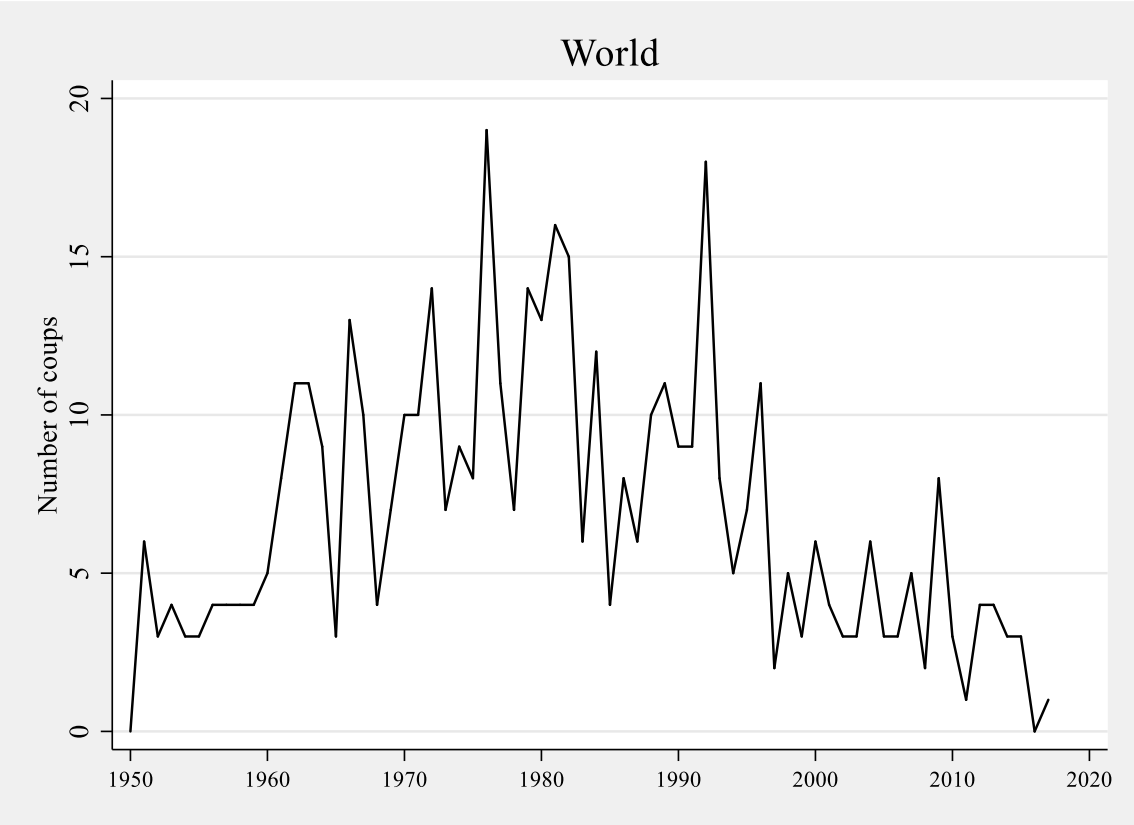

FIGURE 5.1: DEVELOPMENT OF THE TOTAL NUMBER OF COUPS IN THE WORLD PER YEAR

A coup is assigned to year *t* if the coup occurred in the first half of year *t* (i.e. for coups between January and June) or in the second half of the previous year $t - 1$ (i.e. for coups between July and December). For our empirical analysis, this temporal assignment of coups defines a time window between 6 and 18 months after a coup until changes in per capita GDP become effective. This coding is important because coups are almost evenly distributed over months. Coding on an annual basis from January to December would yield downward biased estimates when coups take place at the end of a year.

A concern may be that failed coups are underreported in the Bjørnskov and Rode (2019) dataset because they may attract less public attention. Two arguments speak against this concern: first, the number of failed coups ($N = 294$) in the sample exceeds the number of successful coups ($N = 243$). Second and more importantly, governments which (politically) survive a coup attempt have little reason to hide it. Coup attempts can be politically exploited to strengthen the own power (e.g. by persecuting opposition members) and allows the head of government to stage himself as a strong ruler.

We include all countries in our sample for which data on GDP per capita and coups is available. Our panel consists of 180 countries and covers the period 1950 to 2017, which results in more than 9,000 country-year observations, 432 of which saw coups or coup attempts (402 include a single coup, 27 include two coups, and 3 include three coups). The success rate of coups in our sample is 46%. 102 of the countries in the sample experienced at least one coup, 78 did not experience any coup since 1950. Table A5.1 in the Appendix provides detailed information on data availability and coup occurrence for all countries in our sample.

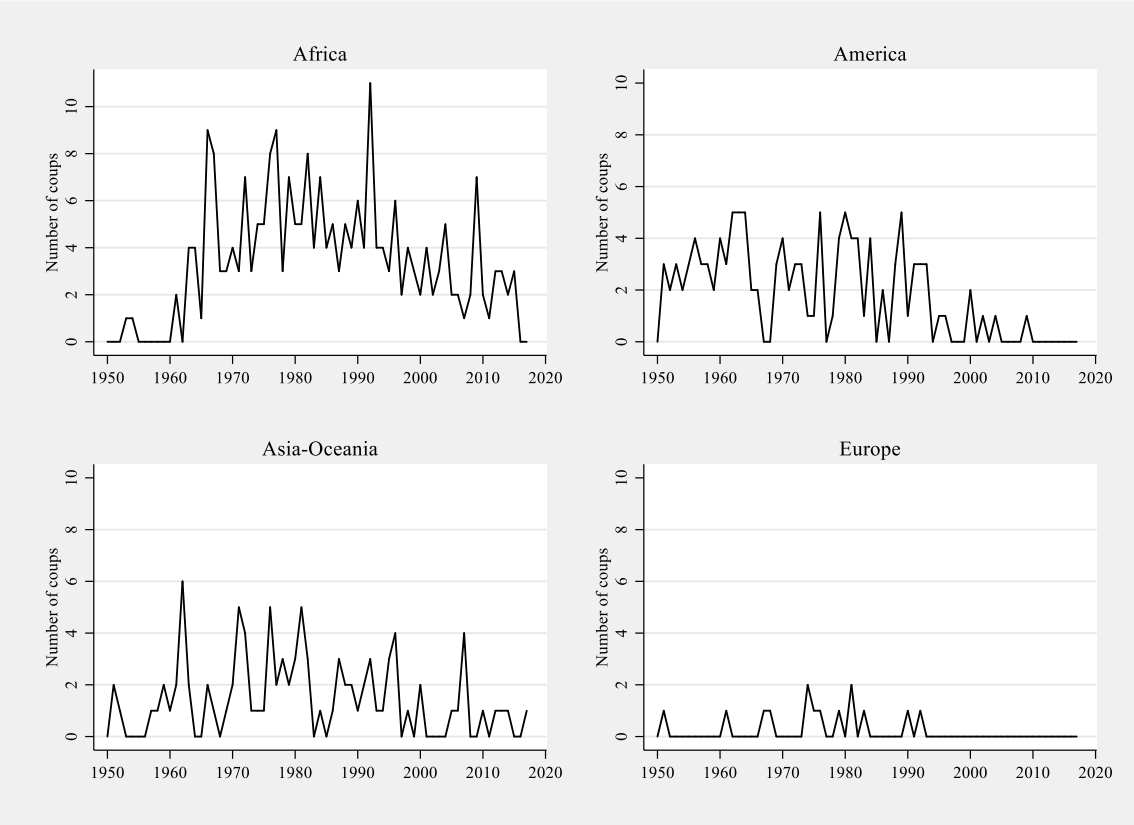

FIGURE 5.2: DEVELOPMENT OF COUPS D'ÉTAT ACROSS CONTINENTS

Figure 5.1 shows the total number of coups per year that occurred between 1950 and 2018. The numbers reveal distinct temporal patterns in coup occurrence. There have been roughly 5 coups per year during the 1950s and the early 1960s, but coup activity rose considerably during the 1960s and the 1970s, reaching its peak in 1976 with a total of 19 coups. With a brief interruption in the early 1990s, the number of coups declined since the early 1980s and reached its all-time low in the post-2010 period. Figure 5.2 shows differences in coup occurrence across continents.

With an average of 3.39 coups per year, coup activity is strongest in Africa, followed by America (1.81) and Asia (1.42). In contrast, coups are rare events in Europe (0.21 per year), and almost all coups have occurred between the mid-1970s and the mid-1980s. Consistent with the trend observable for all countries, coup activity has substantially declined in Africa and America during the past three decades. The decline is, however, less pronounced in Asia.

5.2.2 The geospatial dimension of coups

While there are some themes common to almost all coups and coup attempts, each coup has his own history. To establish a more complete picture of this history and the heterogeneity of coups, we analyze each coup of the Bjørnskov and Rode (2019) database and geocode the coups based on multiple sources that provide information about the regions in which the coups took place, including many books, scholarly articles, and newspaper articles.

Figure 5.3 shows the geographic pattern of coups and coup attempts based on our geocoded data for the successful coups in Pakistan 1999 and the Central African Republic in 2003, as well as the coup attempts in Venezuela 1992 and in Turkey 2016. The countries are representative for some distinct differences in the geospatial dimension of coups in our dataset.

First, the 1999 Pakistan coup d'état was a military takeover initiated by General Pervez Musharraf, which unseated the publicly elected civilian government of Prime Minister Nawaz Sharif. The coup was relatively bloodless and took place only in the Prime Minister's Secretariat in Islamabad (Hossain 2000). In a similar vein, General François Bozizé marched on the Central African Republic's capital Bagui in March 2003 while then President Ange-Félix Patassé (after surviving seven previous coup attempts) stayed in Niger for a regional conference. Bozizé captured the presidential palace and the international airport, with little resistance from government troops and CEMAC peacekeepers, which allowed Bozizé to suspend the constitution and to seize power (The Economist 2003). Both the 1999 Pakistan

coup and the 2003 Central African Republic coup are exemplary for military takeovers that take place in the capital.

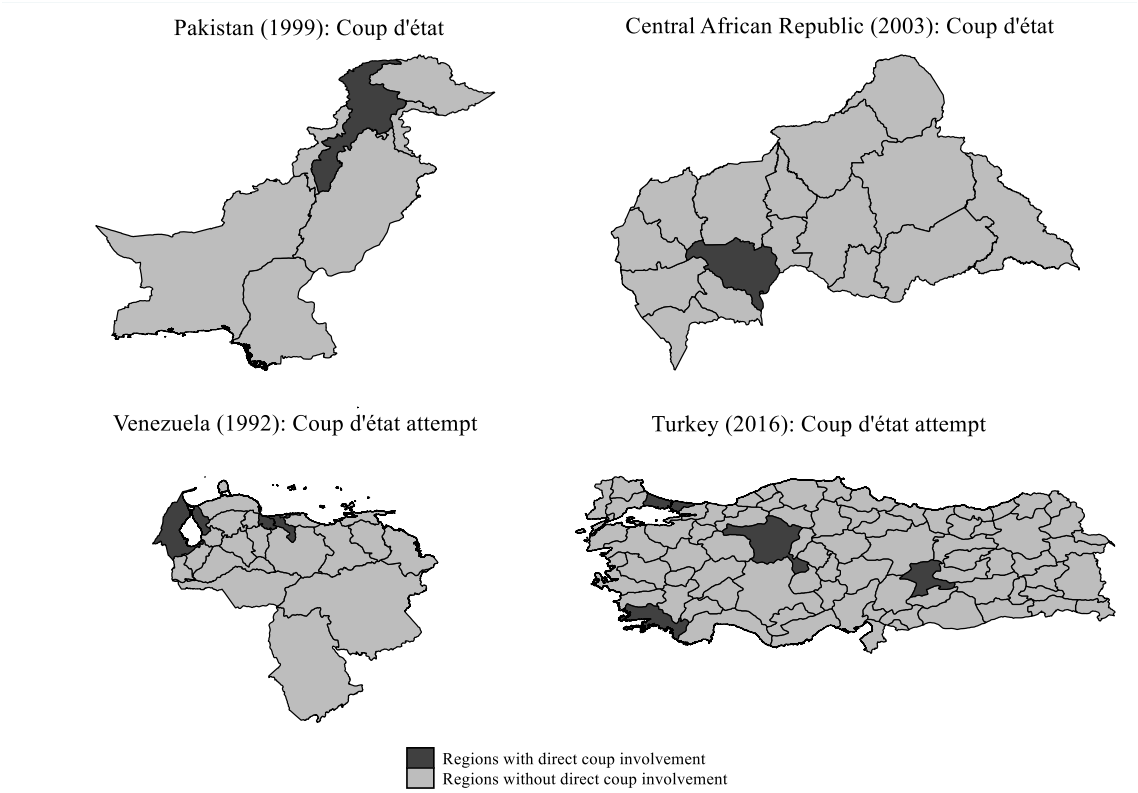

FIGURE 5.3: SPATIAL DIMENSION OF COUPS

Notes: The figure shows coups in Pakistan 1999, the Central African Republic 2003, as well as coup attempts in Venezuela 1992 and Turkey 2016. Regions in dark gray mark sub-national entities with direct coup involvement, regions in light gray mark sub-national units without direct coup involvement. Data is a georeferenced version of the Bjørnskov and Rode (2019) dataset.

In contrast to these successful and relatively non-violent coups, Venezuela saw two violent and unsuccessful coup attempts in 1992, which involved several regions in the country. The 1992 Venezuelan coups took place in February and November and were attempts to seize control by the Revolutionary Bolivarian Movement-200. The first attempt was led by Hugo Chávez, the second attempt was directed by a group of young military officers while Chávez was in prison. Both attempts were directed against President Carlos Andrés Pérez and demanded about 300 casualties and 95 injuries. In the February 1992 coup, Chávez failed to take Caracas, whereas other rebel forces took control of Valencia, Maracaibo, and Maracay. In the November coup attempt, rebelling air force officers were able to take over a state-run TV station in Caracas, broadcasting a video that was filmed in prison and in which Chávez called for a popular

uprising. Before the rebellion was crushed, the putschists were able to gain control over several military bases in the country.

Finally, the 2016 coup attempt in Turkey was carried out by a faction within the Turkish Armed Forces, which attempted to seize control in many key regions, including Ankara, Istanbul, Marmaris, Malatya, and Kars. The coup had devastating consequences: during violent clashes, over 300 people were killed and more than 2,000 were injured. After the government defeated the rebellion, more than 40,000 people were detained, including soldiers, judges, and teachers. Another 75,000 people were arrested and over 160,000 were fired from their job on accusation of connections to Fethullah Gülen, the alleged coup leader. Gülen, however, denied being behind the attempt and accused President Recep Tayyip Erdogan of a self-coup ("autogolpe") conducted to cement his political power. The coups in Pakistan, the Central African Republic, Venezuela and Turkey show that there are substantial differences in coup attempts with regard to the geographical reach, the degree of violence, and the political consequences. Common to these coups is that they decrease political stability.

5.2.3 Data on economic development and growth

Data on GDP per capita is taken from Penn World Table (PWT) version 9.1, which was released in April 2019 (Feenstra et al. 2015). Version 9.1 covers data on prices, output, and productivity for 182 countries between 1950 and 2017. The PWT is often considered the "gold standard" in providing harmonized cross-country measures of GDP. As our main outcome variable, we use the log of per capita GDP, measured in constant (2011) US dollars. To assess the consistency of our results, we use GDP data from the World Development Indicators (WDI) database of the World Bank (2019) in our robustness tests. Summary statistics for growth-related variables, coup-related variables and control variables (which are applied in the robustness tests) are shown in Table A5.2 in the Appendix.

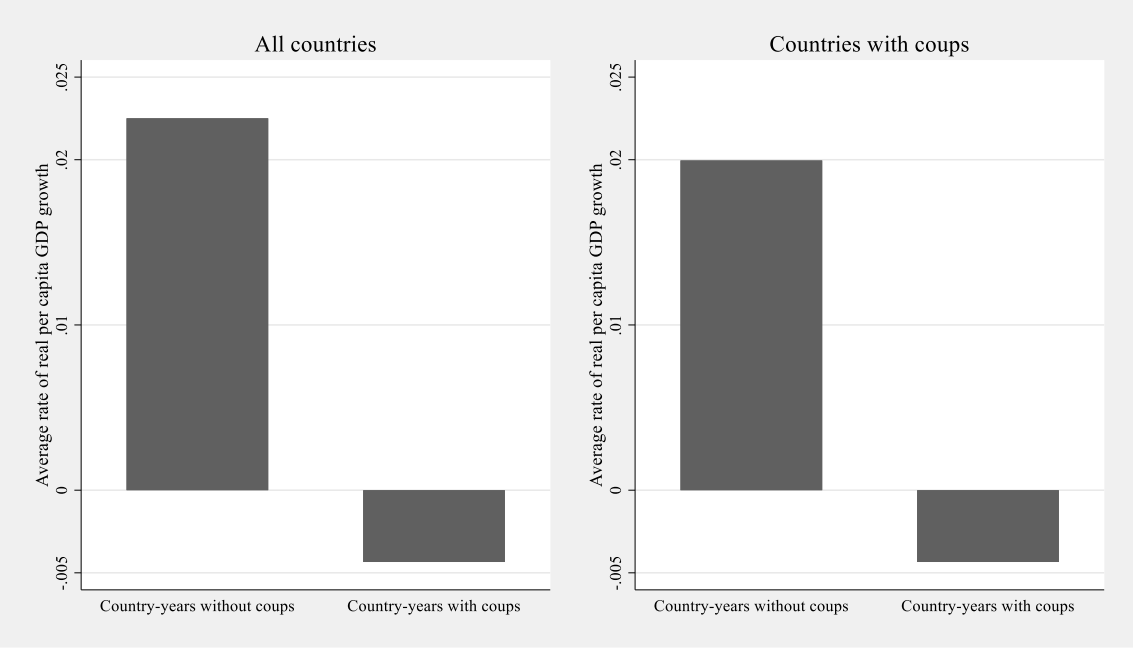

FIGURE 5.4: COUP OCCURRENCE AND MEAN GROWTH RATES IN THE SAMPLE

Notes: The figure shows growth rates for country-years observations with and without coups for the whole sample of country-years (left panel) and the sample of countries that have experienced at least one coup (right panel).

5.2.4 Descriptive evidence on coups and growth

Figure 5.4 shows the unconditional correlation between real per capita GDP growth and the occurrence of coups. The figure shows correlations based on (i) our full sample of country-year observations and (ii) a sample that only considers the 102 countries that experienced at least one coup between 1950 and 2017. We might expect that the second group of countries is politically more unstable in general and hence features lower per capita growth rates than countries without coup activity. The figure does not point to substantial differences between the full sample and the sample of countries with at least one coup. For both samples, however, the figure reveals striking differences in growth rates between country-year observations with and without coups. In the full sample, the average growth rate for country-year observations without coups is 2.3%, but it is –0.4% in years when a coup took place. When focusing on countries that experienced at least one coup, the average growth rate for country-years without coups is 2.0% compared to –0.4% in years during which a coup took place. The differences are even more remarkable when we restrict the sample to the post-1990s period, the period during

which coups became less frequent. In this case, the average growth rate is 2.1% for country-years without coups and –1.0% for those in which a coup occurred (not illustrated).

5.2.5 Can coups be anticipated?

Figure 5.2 shows that the probability of coup occurrence exhibits distinct spatial patterns. An important conclusion is that time-invariant factors influence the ex ante probability of coups. Much less clear, however, is whether coups can systematically be predicted by time-varying factors. There is a large literature studying the determinants of coups, both theoretically and empirically (for an overview, see Singh 2014). This literature quarrels over the question of whether coups can be anticipated or predominantly contain random elements. A prominent argument is that coups occur more often in times when the cost of coups are low (see, e.g., Aidt and Albornoz 2011). For a given cost of coups, however, innate personal characteristics of potential coup leaders (such as their risk aversion, charisma, or ambition) can tip the balance to execute a coup or not (Collier and Hoeffler 2007). In a similar vein, studies analyzing the micro-dynamics of coups argue that elites face a daunting coordination problem when contemplating a coup d'état (Casper and Tyson 2014), and many officers in military coups only want to join a coup if others join as well (Little 2017). Ultimately, the decision to eventually execute a coup may feature an important element of chance. Summarizing the literature on the determinants of coups d'état, Lachapelle (2020) concludes that "although coups have been extensively studied, current scholarship lacks a robust model of the determinants of military coups".

The reason for why there is no conclusive theory of coup occurrence may either be that coup attempts are not systematically caused by time-varying factors, or that these factors have not yet been identified. Using an extreme bounds analysis based on more than three million regressions, Gassebner et al. (2016) demonstrate that most of the proposed variables are unsuccessful in describing the occurrence of coup attempts. Their results show that from 66 variables proposed in the empirical literature, unfavorable economic conditions, previous coups

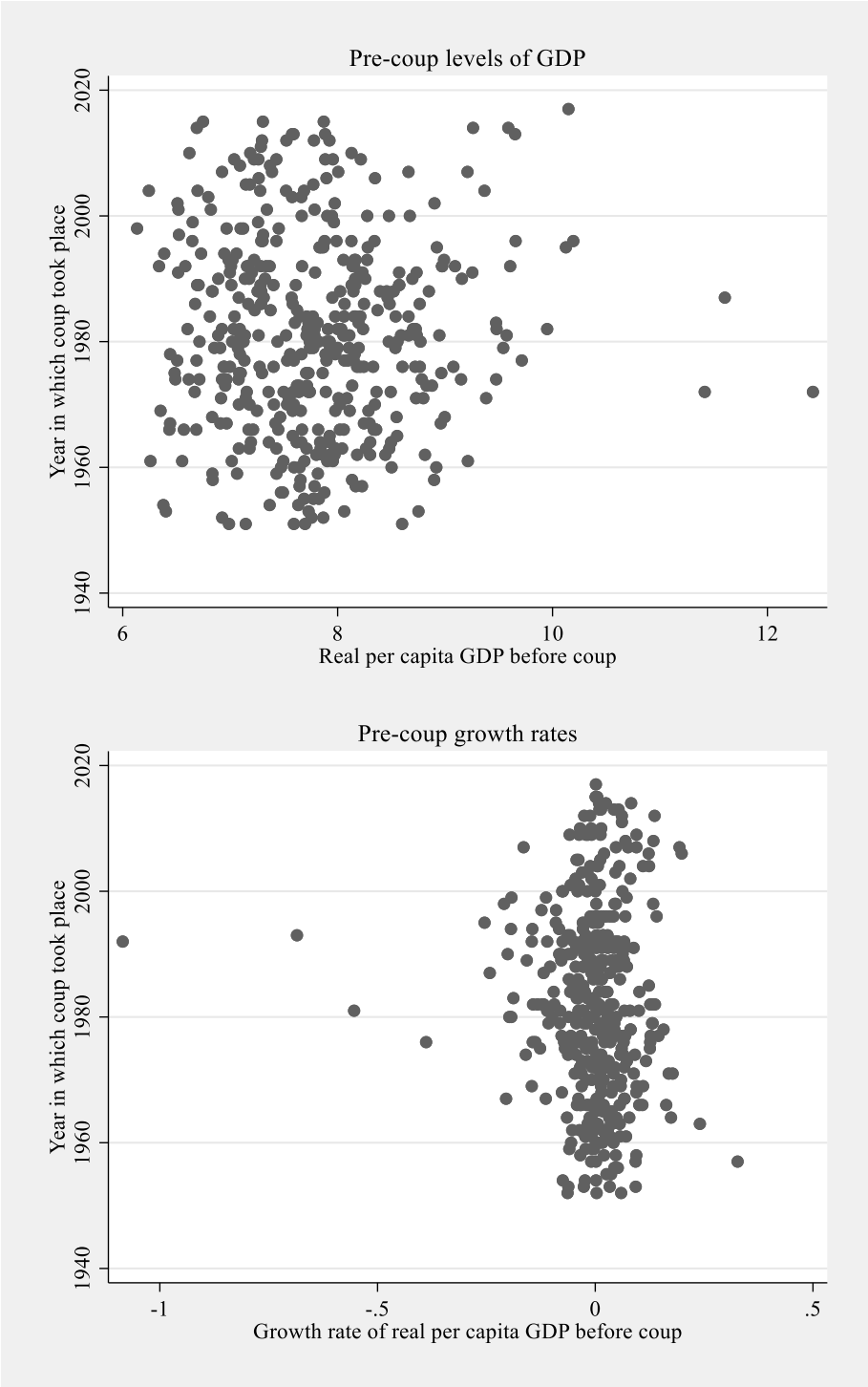

FIGURE 5.5: TIMING OF COUPS AND PRE-COUP LEVELS OF ECONOMIC DEVELOPMENT: GRAPHICAL ANALYSIS.

Notes: The figure shows a scatter plot that relates the timing of coup occurrence to the level of real per capita GDP (in log terms, upper graph) and the growth rate of real per capita GDP (lower graph). The correlations are 0.0008 for GDP levels and –0.076 for growth rates.

experiences, and other forms of political violence are the only factors that are correlated with coup attempts. Other studies confirm that coups are difficult to predict and question the conventional wisdom that per capita income is an important source of coup occurrence. Powell (2012) and Svolik (2013) show that coup attempts and incomes per capita are not statistically

significantly correlated.[3] Supporting evidence comes from the study of Bazzi and Blattman (2014), who cast doubt on the effect of economic shocks on conflict and coups d'états. These recent results are consistent with the classical perspective that coups are random phenomena that cannot be systematically explained (Zolberg 1968, Decalo 1976).

Figure 5.5 provides a graphical analysis of the correlation between pre-coup economic conditions and coup attempts. The figure relates the year in which coups took place to per capita GDP (upper graph) and its growth rate (lower graph) prior to the coup attempts. This analysis suggests that there is no systematical pattern between coup occurrence and the level of per capita GDP or its rate of change prior to coups. In both cases, the correlation is weak (0.001 for levels and –0.076 for growth rates). Also, we do not observe changes in the relationship between economic conditions and coup occurrence over time.

5.3 Country-level results

5.3.1 Panel difference-in-differences model

Our first approach to estimate the effect of coups on economic development is a panel difference-in-differences model. Our specification follows a standard set-up (see, e.g., Beck et al. 2010)

$$\Delta y_{it} = \delta \text{Coup}_{it} + \boldsymbol{C}_i \boldsymbol{\gamma} + \boldsymbol{T}_t \boldsymbol{\lambda} + \varepsilon_{it}, \qquad (1)$$

where the dependent variable Δy_{it} is the growth rate of real per capita GDP of country i at time t. To eliminate cross-country differences in the propensity of coups occurrence, equation (1) includes country fixed effects, implemented by a full set of country dummies $\boldsymbol{C}_i$. The country fixed effects account for any cross-country heterogeneity in time-invariant characteristics that

[3] Likewise, in the study of Gassebner et al. (2016), neither the pre-coup level of per capita GDP nor the growth rate passes the critical CDF threshold for robustness in extreme bounds analyses proposed by Sturm and De Haan (2005).

may influence the probability of coups, such as institutions, natural resources, and cultural or ethnic factors (see Figure 5.2). Fixed effects also eliminate cross-country differences in climatic factors (Masters and McMillan 2001) and natural resources (Rodríguez and Sachs 1999), which have been shown to influence political instability. We also include a vector of year fixed effects $\boldsymbol{T}_t$ to absorb cross-national shocks and trends in coup activity (see Figure 5.1). Our variable of interest $Coup_{it}$ is one if a coup has occurred in a given country-year, and zero otherwise.

Table 5.1 reports variations of the difference-in-differences model of equation (1). Column (1) shows the results of a reduced specification without country or year fixed effects, columns (2) and (3) gradually introduce country and year fixed effects. Column (4) presents results of the standard difference-in-differences specification described in equation (1). Each of these models suggests a negative effect of coup activity on economic growth that is very robust across specifications. The genuine difference-in-differences model in column (4) shows that compared with periods without coup activity, the growth rate of real per capita GDP declines by about 2.2 percentage points when a coup takes place. The effect is statistically significant at the 1% level. Economically, the negative effect of coups is sizeable: the average growth rate of real per capita GDP for observations without any coup in our sample is 2.3%. This implies that, on average, a coup almost offsets economic growth.

TABLE 5.1: COUPS D'ÉTAT AND ECONOMIC GROWTH—BASELINE RESULTS, PANEL DIFFERENCE-IN-DIFFERENCES MODEL

GDP per capita growth	(1)	(2)	(3)	(4)	(5)	(6)
$Coup_{it}$	-0.027***	-0.022***	-0.026***	-0.022***	-0.021***	
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	
Single $coup_{it}$						-0.021***
						(0.003)
Multiple $coups_{it}$					-0.003	-0.024
					(0.023)	(0.023)
Country Fixed Effects	no	yes	no	yes	yes	yes
Year Fixed Effects	no	no	yes	yes	yes	yes
Observations	9,709	9,709	9,709	9,709	9,709	9,709
Countries	180	180	180	180	180	180
R^2	0.008	0.059	0.058	0.109	0.109	0.109

Notes: The table reports the results of panel difference-in-differences estimations on the effect of coups d'état on economic growth (equation 1). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. Per capita GDP growth is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The variable "Coup" denotes whether a coup has taken place at a given year, "Single Coup" only considers country-year observations with one coup in a given year, and "Multiple Coups" considers country-year observations with multiple coups in a given year. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

Column (5) estimates whether there is an additional effect of a second or third coup in the event of multiple coups in one year. There is virtually no change in the effect of our measure of coups occurrence, which remains negative and statistically significant at the 1% level. The coefficient estimate for a second or third coup (*Multiple Coups*$_{it}$) also has a negative sign, but the parameter estimate is far from statistical significance (p = 0.891). Column (6) compares the effect of multiple coups with the effect of a single coup. i.e. country-year observations with no more than one coup. The results corroborate the outcomes of column (5). Taken together, columns (5) and (6) indicate that the adverse effect of political instability on economic growth fully materializes with a single coup or the first of multiple coups in a given year. The observation that further coups do not seem to matter for economic growth suggests that a first of multiple coups in a given year entails political instability to an extent which subsequent coups cannot increase further. We interpret this result as a sign that the estimated parameter for *Coup*$_{it}$ reflects the effect of political instability after a coup, rather than capturing other factors—such as violence and conflict—that may directly accompany coups.

Our panel difference-in-differences model rests on the identifying assumption that the timing of coups is unaffected by economic development prior to the coup. This is a strong assumption, but it is not implausible. First, the recent literature has cast doubt on the conventional wisdom that economic conditions trigger coup d'états and political violence (Bazzi and Blattman 2014, Svolik 2013, Powell 2012). Second, Figure 5.2 shows distinct geospatial correlation of coups, suggesting that the propensity is influenced by more fundamental long-term roots rather than by short-term fluctuations in economic growth. Third, Figure 5.5 provides a graphical analysis of our key identifying assumption. The figure suggests that neither the level nor the growth rate of GDP prior to coup attempts can predict the occurrence of coups.

5.3.2 Dynamic panel data model

To further alleviate concerns about endogenous treatment effects of coups and to account for a potential pre-treatment correlation between economic development and coup occurrence, our second empirical strategy augments equation (1) by pre-coup GDP dynamics (see, e.g., Acemoglu et al. 2019)

$$y_{it} = \sum_{j=1}^{J} \beta_j \, y_{it-j} + \, \mu \, \text{Coup}_{it} + \, \eta_i + \zeta_t + \varepsilon_{it}. \qquad (2)$$

The dependent variable y_{it} is the log of real per capita GDP in country i in year *t*. The specification includes four lags of GDP per capita prior to the period in which a coup takes place. We include four lags of GDP for two reasons: first, the standard assumption of linear dynamic panel models requires that the error term ε_{it} is serially uncorrelated and that coups and past levels of GDP are orthogonal to current and future shocks to GDP (*sequential exogeneity*):

$$E\left(\varepsilon_{it} \middle| y_{it-1}, \dots, y_{it_0}, \text{Coup}_{it}, \dots, \text{Coup}_{it_0}, \eta_i, \zeta_t\right) = 0, t = 1, \dots, T. \qquad (3)$$

This assumption is less demanding than strict exogeneity, under which the parameter μ in equation (2) would be identified, but which is always violated when (2) contains lagged variables. To fulfill *sequential exogeneity*, it is required to include a sufficiently long pre-coup time period to account for GDP dynamics that may influence the probability of coup occurrence.

Second, another important assumption of the dynamic panel data model is that conditional on fixed effects, GDP and coups follow stationary processes. This assumption ensures consistent parameter estimates and well-behaved limit distributions. Hamilton (2018) shows that the inclusion of four lags of the dependent variable creates stationary series with

very high probability.[4] When we include four lags of the log value of GDP per capita, we can directly compare the coefficient size of the parameter estimates with that of our panel difference-in-differences model (equation 1). Under the assumptions of sequential exogeneity and stationarity, we estimate equation (2) with the standard within-group estimator.

TABLE 5.2: COUPS D'ÉTAT AND ECONOMIC GROWTH—BASELINE RESULTS, FULL DYNAMIC PANEL DATA MODEL

Logarithm of GDP per capita	(1)	(2)	(3)	(4)	(5)	(6)
$Coup_{it}$	-0.039***	-0.035***	-0.037***	-0.030***	-0.031***	
	(0.008)	(0.007)	(0.007)	(0.007)	(0.008)	
Single $coup_{it}$						-0.031***
						(0.008)
Multiple $coups_{it}$					0.018	-0.013
					(0.013)	(0.012)
$Log(GDP^{pc})$ (t – 1)	0.887***	0.851***	0.873***	0.827***	0.827***	0.827***
	(0.075)	(0.075)	(0.074)	(0.072)	(0.072)	(0.072)
$Log(GDP^{pc})$ (t – 2)	0.148**	0.146**	0.161**	0.155**	0.155**	0.155**
	(0.068)	(0.066)	(0.068)	(0.066)	(0.066)	(0.066)
$Log(GDP^{pc})$ (t – 3)	0.003	0.007	-0.000	0.002	0.002	0.002
	(0.044)	(0.044)	(0.047)	(0.047)	(0.047)	(0.047)
$Log(GDP^{pc})$ (t – 4)	-0.044	-0.030	-0.039	-0.029	-0.029	-0.029
	(0.040)	(0.043)	(0.042)	(0.044)	(0.044)	(0.044)
Country Fixed Effects	no	yes	no	yes	yes	yes
Year Fixed Effects	no	no	yes	yes	yes	yes
Observations	9,169	9,169	9,169	9,169	9,169	9,169
Countries	180	180	180	180	180	180
R^2 Overall	0.990	0.990	0.990	0.990	0.990	0.990
R^2 Within	0.946	0.946	0.948	0.949	0.949	0.949
R^2 Between	0.999	0.999	0.999	0.999	0.999	0.999

Notes: The table reports the results of dynamic panel data estimations on the effect of coups d'état on economic growth (equation 2). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. The log of per capita GDP is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The variable "Coup" denotes whether a coup has taken place at a given year, "Single Coup" only considers country-year observations with one coup in a given year, and "Multiple Coups" considers country-year observations with multiple coups in a given year. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

Table 5.2 shows the baseline estimation results. Column (1) shows the results of reduced specification without country or year fixed effects, columns (2) and (3) gradually introduce country and year fixed effects. Column (4) shows the results of the full dynamic panel data model described in equation (2). The first and second lag of the dependent variable is positive and statistically significant at the 1% and the 5% significance level. The occurrence of at least one coup ($Coup_{it}$) shows a positive effect on GDP per capita which is statistically highly significant at the 1% level. According to column (4), a coup thus reduces GDP per capita by about 3.0%. This result is in line with the descriptive results shown in Figure 5.4. Given that

[4] The series is stationary in case that the fourth differences of GDP are stationary, an assumption which is very likely to be fulfilled.

the average growth rate of GDP per capita in our sample for observations without any coup is 2.3%, an effect of 3.0% reflects a high economic significance. Column (5) estimates whether there is an additional effect of a second or third coup in the event of multiple coups in one year. The variable for a second or third coup (*Multiple Coups*$_{it}$) does, however, not turn out to be statistically significant. Column (6) shows the estimation results for single coups (*Single Coup*$_{it}$), i.e. country-year observations with not more than one coup, and multiple coups. The result for single coups is identical to the result for at least one coup while the effect of multiple coups does not turn out to be statistically significant. The results thus indicate that the adverse effect of political instability on economic growth fully materializes with a single coup or the first of multiple coups in a given year. Further coups do not seem to matter for economic growth after a first coup has occurred.

5.3.3 Event-study analysis

Our results rest on the assumption that there are no systematic differential trends in GDP of countries with and without coups d'état. To assess the plausibility of this assumption, we examine the dynamics between coups and economic growth by using a flexible event study. The flexible event study illustrates the effect of a coup in years before and years after the coup. Results for the pre-coup years thus allow to infer whether the common trends assumption holds, while results for the post-coup years show how persistent the growth effect of a coup is. The event study is flexible in the sense that all countries which experienced a coup can be included irrespective of the year in which the coup occurred. We extend our panel difference-in-differences model and our full dynamic panel data model by estimating year-specific dummy variables for years before and after a coup. The sample for the flexible event study is, however, restricted to single coups (denoted *Single Coup*), i.e. each event window includes one coup—not a second or third coup in the same year and no other single coups or multiple coups in the years around the event. Additional coups, either in the same year or in other years of the event

window, would bias the estimation results. The empirical models for the flexible event study are

$$\Delta y_{it} = \sum_{T=t-4}^{T=t+3} \delta_T \, (\text{Single Coup})_{it}^{T} + \boldsymbol{C}_i\boldsymbol{\gamma} + \boldsymbol{T}_t\boldsymbol{\lambda} + \varepsilon_{it}, \tag{4}$$

$$\mathrm{y}_{it} = \sum_{j=1}^{j=4} \beta_j \mathrm{y}_{it-j} + \sum_{T=t-4}^{T=t+3} \delta_T \, (\text{Single Coup})_{it}^{T} + \, \eta_i + \zeta_t + \varepsilon_{it}. \tag{5}$$

Bjørnskov and Rode (2019) code the year of a coup depending on the month in which the coup occurred (see Section 5.2.1), and the year of a coup is either described by year t or by year $t-1$. Hence, we chose $t-2$ as the reference year for this event study. The remaining T years describe years prior to and after the coup ($T = [t-4;\ t+3]$). The coefficients δ_T thus estimate the effect of a coup on GDP per capita from $t-4$ years before the coup to $t+3$ years after the coup. The remaining coefficients in equations (4) and (5) are identical to equations (1) and (2). We chose an event window of eight years to include a sufficiently large number of countries in the sample ($N = 80$), but the results are not sensitive to changes in the window size.

Figures 5.6 and 5.7 illustrate the event study results following equations (4) and (5) (see Tables A5.3 and A5.4 in the Appendix for the numeric estimation results). Coefficient estimates for both event study models are not statistically significant at the 5% level for years prior to a coup, suggesting that the common trends assumption is fulfilled. In the year of the treatment, the parameter estimates are negative and statistically significant, both in year $t-1$ (i.e. the year of a coup in case the coup occurred between July and December) and in year t (i.e. the year of a coup in case the coup occurred between January and June). In the panel difference-in-differences model, the effects are statistically significant at the 1% level and amount to a reduction in growth rates by, respectively, 2.0 and 3.1 percentage points. The results are similar

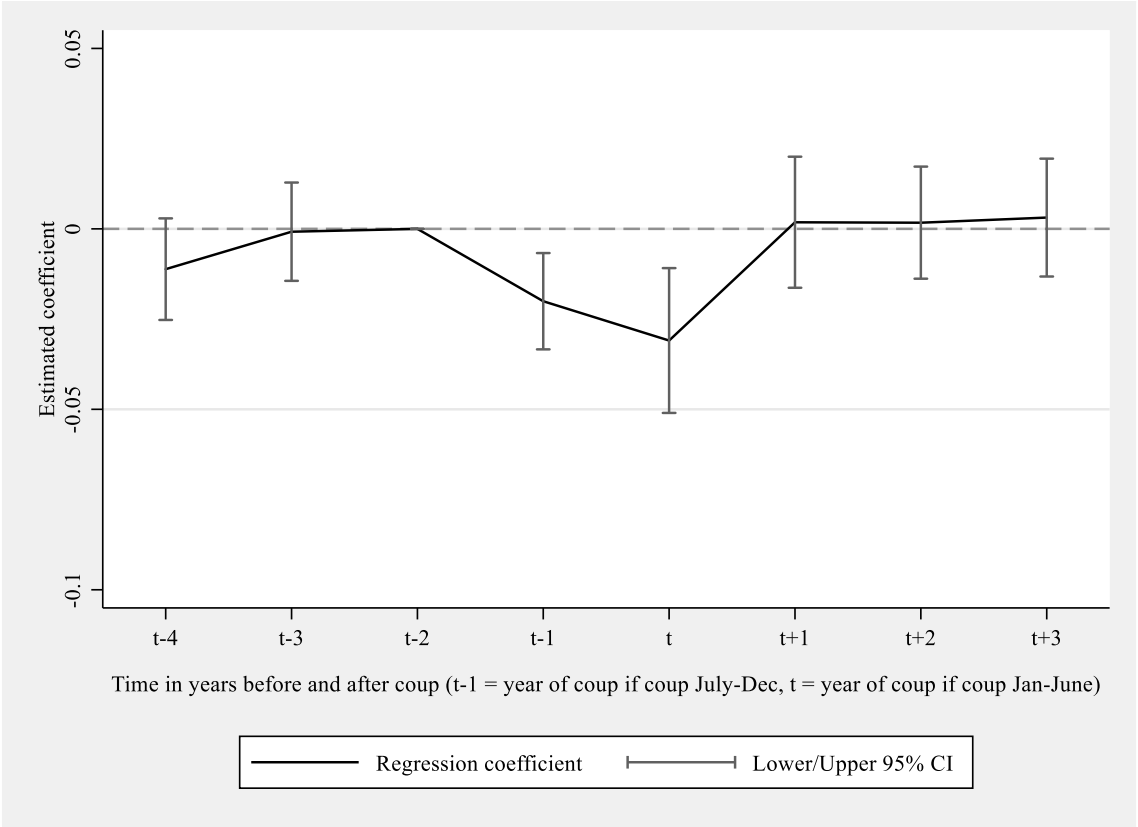

FIGURE 5.6: EVENT STUDY RESULTS FOR THE OCCURRENCE OF A SINGLE COUP, PANEL DIFFERENCE-IN-DIFFERENCES MODEL (RESULTS IN GROWTH RATES)

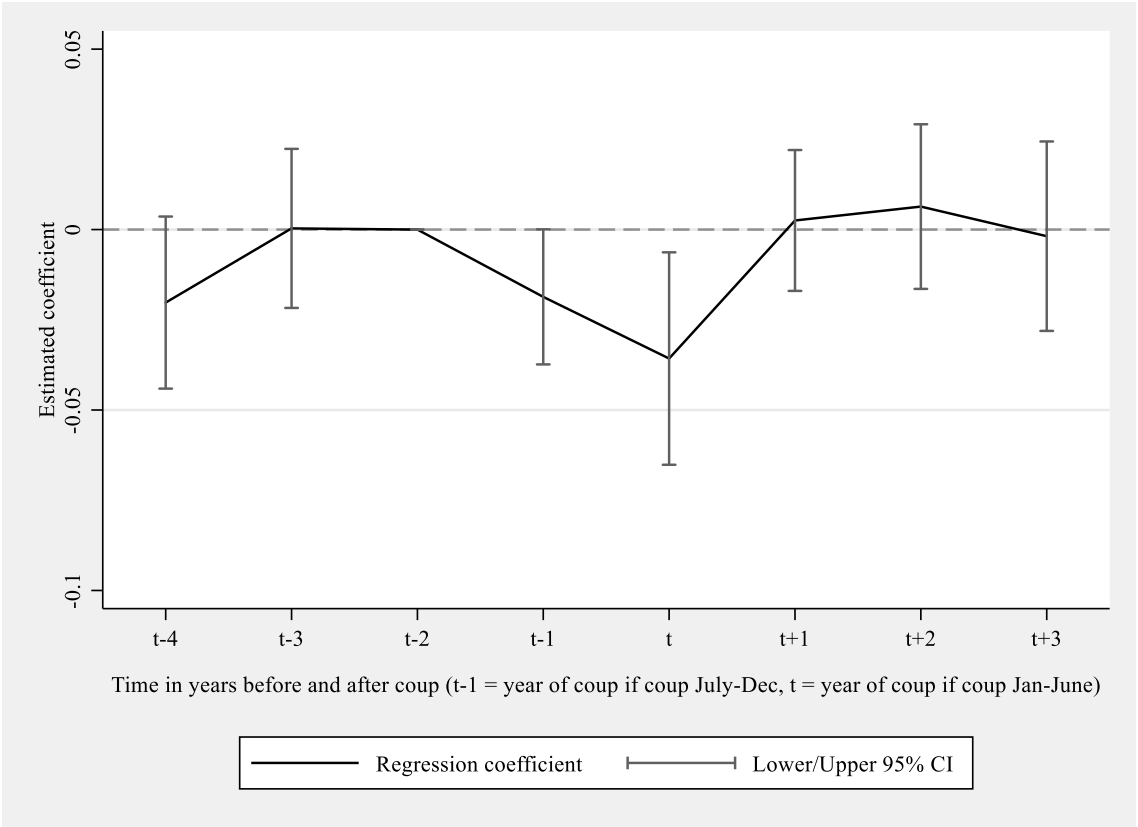

FIGURE 5.7: EVENT STUDY RESULTS FOR THE OCCURRENCE OF A SINGLE COUP, FULL DYNAMIC PANEL DATA MODEL

Notes: Bjørnskov and Rode (2019) assign a coup to year *t* if the coup either occurred in the first half of year *t* or in the second half of the previous year $t - 1$.

for the dynamic panel data model. Given the coding rule of the coup variable, the negative coefficients for year $t-1$ and year t are consistent with our baseline estimates.

The estimated parameters are not statistically significant in years after the treatment, indicating that coups directly influence growth rates for a maximum of two periods. The event study does not reveal a catch-up effect in years after the coup. This suggests that treated countries grow at rates similar to those of untreated countries two years after a coup, but they do not compensate for the loss during the coup period. The absence of a catch-up effect suggests that the adverse effect of a coup can have long-lasting economic consequences.

5.3.4 Robustness tests

The validity of our estimates depends on some important assumptions underlying our empirical model. In this section, we investigate threats to the validity of our results caused by potential violations of these assumptions.

First, we examine the robustness of our results to changes in the control group. In our baseline models, we exploit all available information in our dataset to arrive at the broadest possible sample of coups d'état and economic growth. A concern may be, however, that the control group is not accurately specified, as (i) the ex ante probability of coups differs between the group of countries with and without coups because of systematic differences in time-invariant factors and (ii) these factors may be correlated with economic growth, i.e. countries that experienced coups in their younger economic history may have lower growth rates in general. Our baseline model includes a full set of country dummy variables to account for these concerns. Also, Figure 5.4 suggests that the differences in growth rates between countries with and without coup experience in the sample are small. To further tackle the possibility that the results are driven by an inadequate control group, we re-estimate our baseline model using only country-year observations from countries that experienced at least one coup (see Tables A5.5 and A5.6 in the Appendix). This adjustment reduces the number of countries from 180 to 102

and the number of observations from 9,709 to 5,854. The parameter estimates in this reduced model are practically identical to the full sample. In the difference-in-differences model, a coup reduces GDP by 2.1 percentage points (compared to 2.2 percentage points in the baseline model).

Second, we assess the stability of our results when we adjust the structure of our dataset. In Tables A5.7 and A5.8 in the Appendix, we alter the time dimension of our sample and re-estimate our model using non-overlapping five-year averages. Doing so has little effect on our results. In our preferred specification, a coup is assigned to year t if it occurred in the first half of year t (i.e. for coups between January and June) or in the second half of the previous year $t - 1$ (i.e. for coups between July and December). This coding scheme is important to ensure that the coup effect can materialize in the data, but a concern may be that this coding results in a temporal bias. In Tables A5.9 and A5.10 in the Appendix, we test for an alternative coding of coup occurrence where we re-code coups to match calendar years. We do not observe any changes in the growth effect of coups d'état when we alter the coding scheme.

Third, we restrict the sample to country-year observations in which a coup took place and examine whether multiple coups or successful coups have additional adverse effects on GDP per capita. If the negative effect found in our previous estimates reflects political instability, then we would expect that neither additional coups nor coup success directly influence GDP growth. Tables A5.11 and A5.12 in the Appendix report the results for multiple coups in a sample including all observations with coup occurrence. Tables A5.13 and A5.14 in the Appendix report the results for successful coups in a sample including single coups only. The results show that (i) the negative effect of coups is independent of coup success or failure, and (ii) the adverse effect of coups sets in with the first coup, and there is no further detrimental effect of a second or third coup.

Fourth, we use alternative measures of GDP per capita from the World Bank (World Bank 2019). The number of countries and country-year observations is slightly reduced when

using the World Bank data, but the estimates are similar to our baseline results (see Tables A5.15 and A5.16 in the Appendix).

The occurrence of coups may be driven by exogenous shocks that also affect growth directly. As a fifth robustness test, we account for time-varying factors that possibly confound our parameter estimates. The selection of potential confounding factors refers to variables that pass the critical CDF threshold of 0.95 in the Extreme Bounds Analysis of Gassebner et al. (2016). We include dummy variables for interstate and internal war from the "UCDP/PRIO Armed Conflict Dataset" by Gleditsch et al. (2002) (Version 17.2), a score for civil and ethnic violence from the "Major Episodes of Political Violence (MEPV) and Conflict Regions, 1946-2016" dataset (Version July 25, 2017), a variable for coup experience which describes the cumulative number of coups in a country since 1950, the dichotomous democracy indicator of Bjørnskov and Rode (2019), the KOF Globalization Index (see Dreher 2006, Potrafke 2015, Gygli et al. 2019), an index for ethnic fractionalization from the "Historical Index of Ethnic Fractionalization Dataset" (HIEF) (see Drazanova 2019) and—for the panel difference-in-differences model—the second lag of GDP per capita to account for the current level of economic development. It is worth mentioning that control variables are redundant in a correctly specified difference-in-differences model with randomly assigned treatments. In such a setup, covariates may even be "bad controls" (see Angrist and Pischke 2009). Control variables for internal war or civil violence are likely to be an outcome of a coup which influences GDP per capita. Nevertheless, our inferences from the baseline estimation results do not change once these control variables are added to the models (see Tables A5.17 and A5.18 in the Appendix).

Sixth, we estimate the preferred specifications of our models (including country and year fixed effects) separately for geographic regions. Figure 5.2 shows that the number of coups

differs between continents, and it is a concern that our baseline results may be driven by individual geographic regions. The results, reported in Tables A5.19 and A5.20 in the Appendix, show that coups reduced GDP growth regardless of the geographic region.

Seventh, we examine whether certain characteristics of a coup influence the growth effect of coups d'état. We test for the type of coup (i.e. whether the coup was led by the military, by civilians, or by members of the royal family) and for biographic information of the coup leader, including the age of the coup leader, the civil rank of the coup leader (in case of a civilian coup), and the military rank of the coup leader (in case of a military coup). None of these variables influences the growth effect of coups. The generality of the coup effect bolsters our argument that the adverse growth effect of coups is caused by an environment of political instability (see Tables A5.21 and A5.22).

Eighth, an additional source of bias in our estimates would come from differential trends in GDP among countries with coup attempts. To investigate the extent to which differences in trends influence our results, we follow Acemoglu et al. (2019) by interacting dummies for the quintile of per capita GDP of countries in 1960 with a full set of period effects. The rationale for this strategy is to identify the effect of coups by comparing countries that were similarly developed at the start of our sample.[5] Columns (1)-(2) of Table A5.23 in the Appendix show that differences in GDP trends have no impact on the effect of coups on GDP.

Our dynamic panel data model allows us to remove the potentially confounding influence of any pre-coup trend in GDP. To specify the time horizon of pre-coup GDP dynamics, we follow Acemoglu et al. (2019) and Hamilton (2018) in using four periods prior to coups. In columns (3)-(5) of Table A5.23 in the Appendix, we examine the sensitivity of the results to changes in the specification of GDP dynamics. We reduce the time horizon before the

[5] To construct the quintiles, we use data from the Maddison database to maximize the number of included countries. The Maddison dataset compiles historical GDP data for a large number of countries.

treatment to one, two, and three lags, with little effect on inferences. Differences in the lag structure also do not influence the size of the estimated parameter: In each case, the Wald test does not reject the null of equality of the parameter estimates for coups in the baseline specification (four lags) compared to specifications with alternative lag structures presented in columns (3)-(5). We also run models with richer GDP dynamic including up to ten lags, with no effect on inferences (not reported).

The within group estimates of our dynamic panel data models have an asymptotic bias of order $1/T$ (Nickell 1981). This bias is caused by the failure of strict exogeneity and is mitigated for large T. As our sample includes a total of 68 periods, we expect the "Nickell-bias" to be small, which motivates usage of the within estimator as our baseline approach. Column (6) of Table A5.23 in the Appendix reports the results from a GMM estimation that yields consistent estimates of the dynamic panel data model for finite T. From the sequential-exogeneity condition, we can derive the following moment condition for the GMM framework

$$E[(\varepsilon_{it} - \varepsilon_{it-1})(y_{is}, Coup_{is+1})'] = 0 \; \forall s \geq t-2, \tag{6}$$

which can be employed using the "difference-GMM estimator" (Arellano and Bond 1991). Intuitively, the Arellano-Bond estimator accounts for correlations of our coup variable with past and current realizations of the error term. The difference-GMM results are very similar to those of our baseline dynamic panel data model, which corroborates our expectation that the Nickell-bias of our baseline models is small.

A disadvantage of the difference-GMM estimator is that it is designed for "large N, small T" settings. For large T, the number of moment conditions is of order T^2, which can lead to instrument proliferation (Roodman 2009) and causes an asymptotic bias of order $1/N$. Our specification of the difference-GMM estimator uses a weighting matrix proposed by Alvarez and Arellano (2003), which delivers consistent estimates even when T is large. To address the

problem of instrument proliferation more directly, column (7) of Table A5.23 in the Appendix shows the results when we estimate our baseline model using the dynamic panel data estimator of Han and Phillips (2010). The Han-Phillips estimator imposes no restriction on the number of cross-sectional units and the time span other than $NT \longrightarrow \infty$, and Gaussian asymptotics apply irrespective of the composition of NT. Again, there is virtually no change in the growth effect of coups.

Finally, our baseline models rely on the assumption that the relationship between coups and economic growth is linear. The last column in Table A5.23 in the Appendix presents results of nonparametric kernel regressions with Li-Racine kernel and bootstrapped standard errors following Cattaneo and Jansson (2018). Nonparametric regressions make no assumption on the functional form of the relationship between coups and growth.[6] The reported effects in column (8) are averages of contrasts of factor covariates and are strongly comparable to our parametric specifications.

5.3.5 Coups and political transitions

Marinov and Goemans (2014) argue that coups are the single most important factor for the downfall of democratic governments. Hence, a threat to the validity of our results is that our coup variable may capture the effect of transitions to autocracy rather than the coup effect per se. In our sample, the majority of coups took place within certain regime types and did not lead to political transitions: of the 402 single coup attempts in our sample (185 of which have been successful), 45 successful coups led to a transition from democracy to autocracy, 7 successful coups led to a transition from autocracy to democracy, and 128 (5) successful coups took place in autocracies (democracies) without regime transitions.

[6] Our nonparametric estimator takes averages of the local-linear estimates (see Li et al. 2003 and Cattaneo and Jansson 2018).

To rule out that our results are driven by regime transitions, we estimate the effect of a transition towards autocracy or democracy after a coup. Tables A5.24 and A5.25 in the Appendix show that our baseline estimates are not driven by regime transitions towards autocracy. A coup with a subsequent transition to autocracy is negatively related to growth. The correlation is statistically significant in the unconditional model (column 1) but ceases to be significant once we control for the general occurrence of a coup (column 2). Consistent with our previous results, the growth effect of coups is negative and statistically significant. The coup effect also remains unchanged when we control for democracy (column 3) or exclude country-year observations with transition to autocracy (column 4).

We also test for potential effects of transitions towards democracy initiated by coups to account for the argument that coup leaders have incentives to democratize in order to establish political legitimacy (see, e.g., Thyne and Powell 2016). Consistent with the results for a transition to autocracy, there is no direct growth effect from a coup-led transition towards democracy, but the negative effect of the coup persists (Tables A5.26 and A5.27 in the Appendix).

In Tables A5.13 and A5.14, we tested whether successful coups, i.e. coups which overthrew the incumbent government, have different effects on growth than failed coups. A related potential source of bias is that our estimates may capture the effect of a government change rather than the stability effect of coups. We account for this concern in Tables A5.28 and A5.29 in the Appendix, augmenting our baseline models by an interaction term between government change and coups d'état.[7] The results show that the negative effect of coups is not driven by changes in government. The effect of a coup d'état remains negative and statistically significant in each model, also when we exclude country-year observations with government changes in column (4). The Wald test suggests that the parameter estimates are not significantly

[7] Since the Database of Political Institutions (DPI) does not cover years prior to 1975, the sample for this analysis is restricted to years from 1975 onwards.

different from the baseline outcomes. Consistent with previous studies (see, e.g., Alesina et al. 1996), government changes exert additional adverse effects on growth *on top* of the coup effect.

Overall, the results provide support for our argument that the negative effect of coups stems from political instability initiated by coups d'état rather than from regime transitions or government changes.

5.3.6 Coups and political institutions

A concern may be that the institutional environment of countries and, in particular, institutional changes influence the likelihood of a coup d'état. Countries with underdeveloped institutions may be more politically unstable and hence more prone to coups. Established institutions may increase the hurdles of a coup, while coups are less costly when institutions are underdeveloped. In particular, institutional changes may influence the occurrence of coups, as (i) the cost of coups is low in times when institutions are vulnerable, which is typically the case when newly formed institutional environments are not yet established (Aidt and Albornoz 2011) and (ii) institutional changes may have detrimental effects for parts of the elite, increasing incentives to conduct a coup to preserve the status quo. A related literature argues that states "are a prize that can be seized, especially when the institutions that constrain power are weak" (Bazzi and Blattman 2014; see also Besley and Persson 2010, 2011).

To rule out that the estimated relationship between coups and economic growth is confounded by institutional dynamics, we augment our empirical models by the quality of political institutions. As we are not interested in regime transitions (which are examined in Section 5.3.5), we cannot use dichotomous democracy indicators for our analysis. Instead, we use the Continuous Support Vector Machines Democracy Index (CSVMDI) compiled by Gründler and Krieger (2016, 2018, 2019), which is based on machine learning algorithms to classify the extent of democratization on a continuous scale between 0 and 1. To model institutional dynamics prior to coups, we include four lags of the CSVMDI in our baseline

models. The results, presented in Tables A5.30 and A5.31 in the Appendix, are very similar to our baseline estimates.

5.3.7 Case study evidence

Our results so far reflect average growth effects of coups. In the next step, we examine in more detail the anatomy of coups d'état by analyzing case studies of specific coups. This analysis is motivated by Bazzi and Blattman (2014), who argue that systematic selection of country cases is essential to assess robustness of studies that deal with political instability. A demanding requirement for the selection of country cases is that there is a sufficiently long pre-treatment period that is not interrupted by any coup (usually of ten years or more), along with a post-treatment period of at least three years without additional coups or coup attempts. This requirement leaves us with a handful of coups that can be used to draw case study evidence. We carefully examine all of these events.

Figure 5.8 shows the results of a synthetic control analysis on coups d'état in four countries. The figure shows the logarithm of GDP per capita for the treated country and its synthetic twin, which consists of a weighted average of up to 69 eligible countries without any coup activity. The coup in the Democratic Republic of Congo in 1960 took place during the Congo Crisis after independence from Belgium and was led by Joseph-Désiré Mobutu, who finally became president after a second successful coup attempt in 1965. The coup can be interpreted as a symptom of high political instability after the Congo became independent (Haskin 2005). The coup in Thailand 1971 was a self-coup by the prime minister to gain support for the suppression of communist tendencies. At that time, Thailand experienced peasant revolts and student protests that were inspired by its neighbors Laos and Cambodia, which had come under communist rule one year earlier (Mezey 1973). The 1979 military coup in South Korea led by Major General Chun Doo-Hwan ended the Fourth Republic of Korea. It took Chun several months to finally gain control over most government apparatuses, resulting in high

political instability (Hyun-Hee et al. 2005). The coup in Cameroon 1984 was an unsuccessful attempt by presidential palace guards to unseat President Paul Biya and involved armed fights in Cameroon's capital Yaoundé with several casualties (Randal 1984).

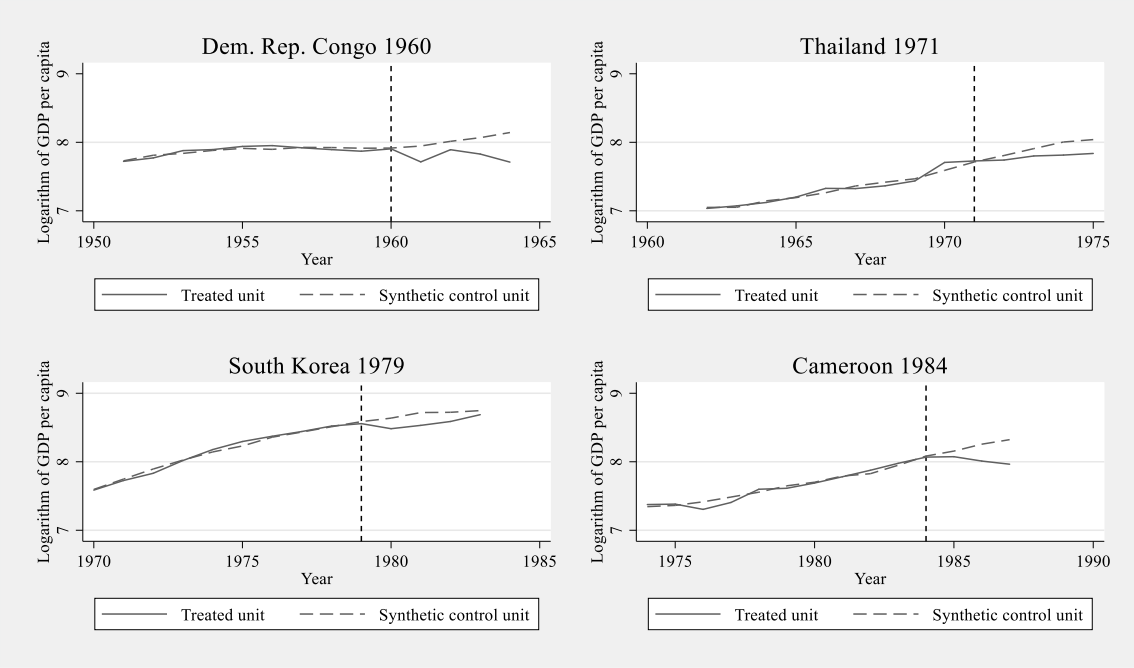

FIGURE 5.8: SYNTHETIC CONTROL ANALYSIS FOR SELECTED COUP EXAMPLES

Notes: The solid line represents the development of per capita GDP (log scale) of the treated country, the dashed line is the counterfactual development suggested by the synthetic control group. All countries only feature one coup during the time period used for the synthetic control analysis. The donor pool includes countries without coups or coup attempts. Weights: Democratic Republic of Congo: India 32.5%, Nicaragua 67.5%; Thailand: Botswana 33.5%, Cape Verde 3%, China 25%, Malta 3.2%, Singapore 35.2%; South Korea: Botswana 48.4%, Malta 36%, Saudi Arabia 15.6%; Cameroon: Antigua and Barbuda 10.1%, Botswana 76.6%, Cape Verde 13.4%.

For the majority of coups with a sufficiently large pre- and post-treatment period without further coups, we observe that per capita GDP develops unfavorable relative to the synthetic control group after the treatment. In some (relatively rare) cases, there are no statistically significant differences between the treated country and the control group. An important conclusion that we draw based on a detailed analysis of these growth-neutral coups is that there are no systematic patterns connecting these coups. Rather, there are specific circumstances that mitigate the detrimental growth effects that are unique to each coup d'état. A prominent example is the "constitutional coup" in Tunisia 1987, in which the new Prime Minister Zine El Abidine Ben Ali made a group of seven doctors sign a medical report, attesting that the aging and sick President Habib Bourguiba was mentally incapable. This coup, allegedly backed by Article 57

of the Tunisian Constitution, did not affect political stability. Ben Ali continued Bourguibas policies, positioning himself as his spiritual successor (see Figure A5.1 in the Appendix).

To alleviate concerns about strategic selection of the country case studies, Appendix I provides synthetic control estimates and accompanying descriptions for additional country cases.

5.4 The geospatial dimension of coups

5.4.1 IV estimates: Coup contagion hypothesis

The key identifying assumption of our baseline regressions is that coups are difficult to predict with time-varying factors. We now relax this assumption and develop an IV approach that accounts for time-varying unobservables that may confound the relationship between coups and growth. The descriptive analysis of coups d'états in Section 5.2.1 shows that the ex ante probability of coups depends on the geographic region in which a country is located. We control for country fixed effects in our baseline model to account for time-invariant geographic confounders. In the next step, we exploit the geographical pattern for causal identification.

The political science literature has intensely studied the geographic patterns of coups. The most prominent explanation for the observed spatial dependency is the "coup contagion hypothesis", which was first raised by Li and Thompson (1975) and later re-evaluated by numerous scholars. Based on stochastic statistical models, Li and Thompson (1975) find a correlation of military coups in a country and the occurrence of coups in neighboring countries. The work was the foundation for the discussion of a "coup contagion" phenomenon. Li and Thompson (1975) explain the spatial correlation by a behavioral reinforcement process: successful coups in one country inspire and encourage military leaders in geographically close countries to follow the example. Consistent with our argument that coups are difficult to predict, a recent study by Miller et al. (2018) challenges the view of a direct causal spread of coup attempts across country borders. However, in line with earlier studies on the coup contagion

hypothesis, they also report a strong spatial correlation in coup occurrence. A similar correlation can be found in our data. Figure 5.9 shows the total number of coups for each country between 1950 and 2017, pointing to a strong geospatial pattern in coup occurrence.

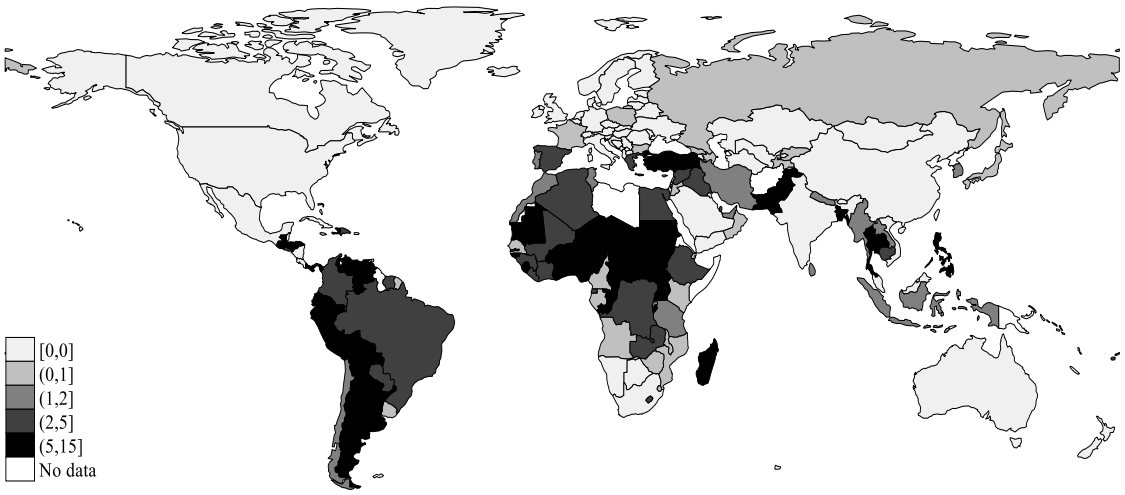

FIGURE 5.9: NUMBER OF COUPS PER COUNTRY, 1950-2017

Notes: The numbers are calculated using the Bjørnskov and Rode (2019) dataset.

We exploit the geographic correlation of coup occurrence to construct an instrumental variable for coups. For each country i, we first define a set $\mathfrak{S}_i \equiv \{\tilde{\imath}: \tilde{\imath} \neq i, L_{\tilde{\imath}} = L_i\}$ of other countries $\tilde{\imath}$ in which coup occurrence may be correlated with coups attempts in i. We use the classification of the World Bank for the specification of the relevant peer group of countries, which consolidates countries that share a common political and economic history into regions L_i. As coups are rare events (we observe in about 5% of our country-year observations), we define a time-window $t' = t - \tau$ to be relevant for coup occurrence in t. Our baseline IV uses $\tau = 5$. Based on $\mathfrak{S}_i$, we compute averages for L_i, leaving out i to not violate the exclusion restriction ("jackknifed" averages)

$$Z_{it} = |\mathfrak{S}_i|^{-1} \sum_{\tilde{\imath} \in \mathfrak{S}_i} \text{Coup}_{\text{i}t'}. \tag{7}$$

A similar logic is used to construct instruments in the democracy literature (Acemoglu et al. 2019, Gründler and Krieger 2016, Madsen et al. 2015). We posit that jackknifed regional

averages are even better suited to identify the effect of coups, as (i) coups are more difficult to predict than democratization events and (ii) in the majority of cases, coups can unequivocally be assigned to a given time period, while it takes several periods to cultivate a democracy, and mistiming in the coding of democratization is likely to bias the instrument.

The corresponding empirical model is identical to the models in equations (1) and (2) except that coups are treated as endogenous variables, which yields (panel difference-in-differences model equivalently)

$$y_{it} = \sum_{j=1}^{J} \beta_j \, y_{it-j} + \mu \, \text{Coup}_{it} + \eta_i + \zeta_t + \varepsilon_{it}$$

$$Coup_{it} = \sum_{j=1}^{J} \pi_j \, Z_{it-j} + \sum_{j=1}^{J} \lambda_j \, y_{it-j} + \psi_i + \varphi_t + \nu_{it}. \tag{8}$$

The key identifying assumption is that, conditional on GDP dynamics and country and year fixed effects, coups in countries $\tilde{\imath} \in \mathfrak{S}_i$ do not influence GDP in i via channels other than the encouragement of coups in i ("exclusion restriction"). This assumption is plausible, but it may be violated if coups increase the probability of violent conflict with neighboring countries or lead to a decrease in trade. We control for these potential threats to the validity of our IV strategy.

Panel A of Table 5.3 shows the second-stage results of our IV estimates, with first-stage results reported in panel B. Columns (1)-(3) show the outcomes for the difference-in-differences setting and columns (4)-(6) report the results for the dynamic panel data model. For both estimation techniques, the first specification (columns 1 and 4) presents estimates without country and year fixed effects, the second specification (columns 2 and 5) includes country and year fixed effects, and the third specification (columns 3 and 6) adds variables that potentially

TABLE 5.3: COUPS D'ÉTAT AND ECONOMIC GROWTH—INSTRUMENTAL VARIABLE ESTIMATIONS

Growth rate and logarithm of GDP per capita	Panel Diff-in-Diff Model			Dynamic Panel Data Model		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Second-stage results					
$Coup_{it}$	-0.059***	-0.037**	-0.037*	-0.077***	-0.029*	-0.041**
	(0.018)	(0.019)	(0.020)	(0.025)	(0.016)	(0.020)
$Log(GDP^{pc})$ (t – 1)				0.884***	0.827***	0.834***
				(0.074)	(0.072)	(0.076)
$Log(GDP^{pc})$ (t – 2)				0.149**	0.155**	0.143**
				(0.068)	(0.065)	(0.061)
$Log(GDP^{pc})$ (t – 3)				0.004	0.002	-0.017
				(0.044)	(0.047)	(0.038)
$Log(GDP^{pc})$ (t – 4)				-0.043	-0.029	-0.016
				(0.040)	(0.044)	(0.030)
$Imports_{it}$			0.001			0.093***
			(0.001)			(0.028)
$Exports_{it}$			-0.000			0.100***
			(0.009)			(0.031)
Interstate war_{it}			-0.025**			-0.032**
			(0.012)			(0.015)
	Panel B: First-stage results					
Z (t – 1)	24.214	15.112	14.950	21.276	15.110	14.955
	(22.269)	(17.950)	(17.792)	(21.125)	(17.893)	(17.744)
Z (t – 2)	23.037**	17.137*	17.155*	20.609**	17.029*	17.048*
	(10.11)	(10.155)	(10.159)	(10.063)	(10.224)	(10.222)
Z (t – 3)	8.840	2.746	2.755	6.675	2.776	2.788
	(7.565)	(5.268)	(5.631)	(6.912)	(5.571)	(5.577)
Z (t – 4)	37.100***	30.206***	30.233***	34.770***	30.113***	30.143***
	(4.555)	(4.838)	(4.849)	(4.590)	(4.815)	(4.830)
Country Fixed Effects	no	yes	yes	no	yes	yes
Year Fixed Effects	no	yes	yes	no	yes	yes
Observations	9,169	9,169	9,169	9,169	9,169	9,169
Countries	180	180	180	180	180	180
R^2 Overall	0.113	0.050	0.052	0.982	0.981	0.959
Equality with baseline (Wald)	0.038	0.621	0.437	0.129	0.944	0.583
Kleibergen-Paap F	42.36	18.77	18.56	34.31	18.54	18.35
Stock-Yogo (10% rel. bias)	10.27	10.27	10.27	10.27	10.27	10.27
Hansen J p-val	0.302	0.305	0.320	0.511	0.514	0.767
SW χ^2p-val	0.000	0.000	0.000	0.000	0.000	0.000

Notes: The table reports the results of panel difference-in-differences models (columns 1-3) and dynamic panel data estimations (columns 4-6) on the effect of coups d'état on economic growth. Robust standard errors (adjusted for clustering by countries) are reported in parentheses. The log of per capita GDP is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The instrumental variable captures spatial correlations of coups measured by Equation (7). *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

violate the exclusion restriction. The exclusion restriction may be violated if coups exert direct effects on neighboring countries when they initiated interstate war activity or influence trade between states. We account for these effects by controlling for interstate war of i as well as for exports and imports. In each model, the effect of coups on GDP growth is negative and statistically significant. The effect size is somewhat larger than in the baseline results, indicating a downward bias in the baseline estimates due to time-variant unobservables. However, once we account for country and year fixed effects, the parameter estimates are similar to those of

the baseline estimates. Except for column (1), the Wald test cannot reject the null hypothesis that the IV estimates are statistically equal to the baseline estimates.

The validity of our IV results depends on the suitability of regional coup activity to instrument national coup occurrence. The test statistics reported in Table 5.3 give us confidence that our IV strategy is valid: the Kleibergen-Paap test clearly rejects the possibility of weak identification, the Sanderson-Windmeijer test and Hansen's J test provide no sign of misspecification due to under- or overidentification. Also, the first-stage results reported in panel B show that coup occurrence is significantly correlated with regional coup occurrence within our five-year time window. We also test for different lag structures of Z_{it}, with little effect on inferences.

5.4.2 Effects of coups on the sub-national level

We next develop a different and complementary approach to tackle endogeneity exploiting sub-national data. To this end, we construct a new geospatial dataset of coup occurrence. We analyze each coup of the Bjørnskov and Rode (2019) database and geocode the coups based on multiple books, scholarly articles, and newspaper articles (see Section 5.2.2 for a detailed description of the geospatial dimension of coups). We then merge the georeferenced data to sub-national income levels computed by Lessmann and Seidel (2017).

Examining the geospatial dimension of coups allows us to separate the effect of political instability from that of violent actions and conflict. We might expect that the negative effect of coups may depress growth in the geospatial area where it was initiated. However, a negative parameter estimate may be due to (i) the effect of political instability or (ii) the direct adverse effect on growth initiated by coup-induced violence and the destruction of infrastructure. The geospatial analysis allows us to disentangle these effects. If regions without direct coup involvement would be affected by coups that take place in other sub-national entities within the same country, this would provide strong evidence for the hypothesis that it is the instability and

uncertainty caused by coups that decreases growth rather than the direct effect of violence. This argument is a sub-national version of the coup contagion hypothesis exploited in the previous section. Across sub-national units, coups are contagious by definition, as sub-national entities share a common national government that is attempted to be unseated by a coup.

The sub-national dataset of Lessmann and Seidel (2017) is computed based on nighttime lights collected from satellite data provided by the National Oceanic and Atmospheric Administration (NOAA). The data is available for the period between 1992 and 2012. The collected data on coups and regional incomes allows us to analyze 2,660 sub-national regions in 168 countries (due to territorial changes, there are changes in the total number of sub-national units over time). Our georeferenced data on coup occurrence shows that coups often occur in the capital, but there are many instances in which coups took place in multiple regions or in sub-national units outside the capital.

The coups in Pakistan, the Central African Republic, Venezuela and Turkey discussed in Section 5.2.2 show that there are substantial differences in coup attempts with regard to the geographical reach, the degree of violence, and the political consequences. Common to these coups is that they reflect political instability. Looking at the geospatial dimension of coups allows us to separate this type of instability from direct effects caused by violent actions. We follow a two-step approach to examine the geospatial dimension of coups. First, we estimate the effect of coups in the region where the coup takes place via (panel difference-in-differences model equivalently)

$$y_{irt}^{\text{reg}} = \sum_{j=1}^{\text{J}} \beta_j y_{irt-j}^{\text{reg}} + \mu\, \text{Coup}_{irt} + \eta_r + \zeta_t + \varepsilon_{irt}, \tag{9}$$

where y_{irt}^{reg} is the log of real per capita gross regional product (GRP) of sub-national region r of country i at time t, η_r and ζ_t are regional and year fixed effects, and ε_{irt} is the idiosyncratic error.

Second, we generate a new variable $\text{Coup}^{\text{c}}{}_{irt,r\neq\tilde{r}}$ that assumes a value of 1 (and zero otherwise) for region r when two criteria are fulfilled: (1) a coup took place in one or more regions $\tilde{r}$ at time t in the country to which region r belongs, and (2) the coup did not take place in region r itself, i.e. $r \neq \tilde{r}$. The newly constructed variable measures *indirect* involvement in coup activity: regions r are not directly affected by violent actions that may have direct effects on economic growth. Hence, $\text{Coup}^{\text{c}}{}_{irt,r\neq\tilde{r}}$ only captures the effect of political instability rather than that of coup-induced violence. We estimate the empirical model (panel difference-in-differences model equivalently)

$$y_{irt}^{\text{reg}} = \sum_{j=1}^{\text{J}} \beta_j y_{irt-j}^{\text{reg}} + \theta\ \text{Coup}^{\text{c}}{}_{irt} + \ \eta_r + \zeta_t + \varepsilon_{irt}\ \ \forall r \neq \tilde{r}, \qquad (10)$$

where we exclude regions $\tilde{r}$, i.e. regions in which coups took place, to ensure that the model only captures indirect coup involvement.

The results are presented in Table 5.4. Columns (1) and (2) show the results of the panel difference-in-differences setting with (column 1) and without (columns 2) regions with direct coup involvement. Columns (3) and (4) use the same specifications for our dynamic panel data setting. The results strongly coincide with our baseline results obtained with country-level data. In the panel difference-in-differences setting, a coup lowers GDP growth by 2.1 percentage points, which is almost identical to the country-level estimate of 2.2 percentage points. The parameter estimate is somewhat smaller in the dynamic panel data model, but it is still not statistically distinguishable from the country-level estimate (p = 0.150).

TABLE 5.4: COUPS D'ÉTAT AND ECONOMIC GROWTH—RESULTS ON THE SUB-NATIONAL LEVEL

Growth rate and logarithm of GDP per capita	Panel Diff-in-Diff Model		Dynamic Panel Data Model	
	(1) *All regions*	(2) *Coup regions excluded*	(3) *All regions*	(4) *Coup regions excluded*
$Coup_{irt}$	-0.021***		-0.018**	
	(0.007)		(0.008)	
$Coup^{c}_{irt,r\neq\tilde{r}}$		-0.023***		-0.020***
		(0.009)		(0.007)
Log(GRP^{pc}) (t – 1)			0.816***	0.815***
			(0.075)	(0.075)
Log(GRP^{pc}) (t – 2)			0.168**	0.169**
			(0.074)	(0.074)
Log(GRP^{pc}) (t – 3)			-0.043	-0.042
			(0.045)	(0.045)
Log(GRP^{pc}) (t – 4)			-0.065	-0.066
			(0.040)	(0.040)
Sub-National Unit Fixed Effects	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes
Observations	51,727	51,655	43,707	43,659
Sub-National Units	2,660	2,660	2,659	2,659
R^2 Overall	0.297	0.298	0.935	0.935
F-Statistic	86.66	82.64	833.1	844.3

Notes: The table reports the results of panel difference-in-differences models (columns 1-2) and dynamic panel data models (columns 3-4) on the effect of coups d'état on economic growth at the sub-national level. Robust standard errors (adjusted for clustering by countries) are reported in parentheses. The log of per capita GRP is measured in real terms, data on coups d'état is geocoded by sub-national units using the coups listed in Bjørnskov and Rode (2019). Due to restrictions availability of sub-national GRP estimates, the models include the period 1992-2012. Sub-national regions are first-level administrative areas (ADM1). *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

The parameter estimates are unaffected if we identify the effect of political instability by restricting the sample to sub-national units without direct coup involvement. If anything, the parameter estimates are larger in the models that estimate indirect effects of coups. Taken together, the sub-national results allow us to draw two conclusions: (i) The strong similarity between the estimation results on the sub-national level and the country level indicates that the strong negative correlation found at the national level is not mediated by time-varying unobservables at the country level. (ii) The fact that the parameter estimates of the country-level and the sub-national-level analyses are identical even when we exclude regions with direct coup involvement suggests that the country-level estimates are not biased by direct growth effects of violent actions that may accompany coups. This finding also provides support for our hypothesis that it is political instability that initiates the negative growth effect, rather than direct effects of violent actions. This finding is plausible, as many coups in our dataset have been relatively unbloody, and we would not expect these events to exert effects as large as our estimated parameters.

Still, violent conflicts that accompany coups may be stronger on the sub-national level, because the spatial distribution of conflicts is asynchronous across countries. Hence, a concern may be that our sub-national parameter estimates are confounded by regional conflicts. To alleviate these concerns, we re-estimate equations (9) and (10) by including data on sub-national conflicts. We construct a conflict dummy variable using the UCDP Georeferenced Event Dataset of Sundberg and Melander (2013). The geo-coded data allows us to compute conflict measures that coincide with our ADM1 regions. The results, shown in Table A5.32 in the Appendix, illustrate that conflict goes in tandem with weak economic growth. The estimate on coups is stable in all models and the size of the estimated coefficient is unaffected from the inclusion of regional conflict. In particular, the parameter estimate is larger for coups than for conflict, suggesting that political instability is the relatively stronger negative correlate of development. In Table A5.33 in the Appendix, we provide additional robustness checks on the sub-national levels, where we account for cross-regional differences in human capital (measured via the georeferenced data provided by Gennaioli et al. 2013). Although restrictions in data availability reduce the number of included sub-national units, there is little impact on inferences, and the parameter estimates are robust in both their size and significance levels.

5.5 Household-level results

5.5.1 Theory

We now shift the focus from the macro perspective to the micro level and examine how coups influence household-level outcomes. Previous studies mainly focus on the microeconomic effect of violent civil conflict: Dupas and Robinson (2010) observe a sizable decrease in income, expenditure, and food consumption in Kenya in the aftermath of the 2007 presidential election, which led to a two month period of civil conflict. In a similar vein, empirical studies investigate micro effects of civil conflict in Sierra Leone (Bellows and Miguel 2006), civil war in Rwanda (Serneels and Verpoorten 2013), and political violence in Perú (Léon 2012). The

underlying argument of these studies is that causes and consequences of civil conflict tend to be visible mostly at the micro level (Balcells and Justino 2014), and that violent civil conflict may disrupt productive activity (Dupas and Robinson 2010). It is, however, unclear whether the effects identified in these studies are driven by violence or by political instability.

We study the household-level impact of coup d'états to examine the micro effects of political instability. Compared to violent civil conflict or war, much less individuals are directly affected by a coup d'état. Potential effects of a coup should hence much more reflect the influence of political instability than the effect of violence.

To date, very little is known about the household-level effects of coups, both from a theoretical and an empirical perspective. We posit that coups influence household-level outcomes by affecting labor markets. On the demand side, the arguments are similar to those regarding the micro effect of conflict: labor demand may decrease if firms are hesitant to hire employees in times when political instability makes it difficult to anticipate future market potentials. Also, unemployment may rise as a consequence of violence that accompanies coups, i.e. when infrastructure and production plants are destroyed. On the supply side, the effects are less clear-cut. We study labor supply effects of coups d'état based on a simple stylized model in the spirit of Keane and Rogerson (2012). Consider a T-period lived household with preferences

$$\sum^{T} \beta^{t} \left[\frac{1}{1-\left(\frac{1}{\zeta}\right)} c_t^{1-\left(\frac{1}{\zeta}\right)} \frac{1}{1-\left(\frac{1}{\delta}\right)} h_t^{1+\left(\frac{1}{\delta}\right)} \right], \tag{11}$$

where c_t and h_t denote consumption and working hours at age t, ζ and δ are preference parameters for consumption and working hours, and β is the discount factor. Each individual has productivity θ_t, so that a supply of h_t units of time results in $\theta_t h_t$, which will be rewarded

with ω. In steady state, a newly born individual with zero initial wealth faces the maximization problem

$$\sum^{T} \beta^t c_t = \sum^{T} \beta^t \theta_t h_t (1-\rho)\omega, \tag{12}$$

where $(1-\rho)$ denotes the probability that ω will be received, and ρ is a risk factor. There may be many reasons for why $\rho > 0$, for instance when firms are insolvent and cannot pay wages. In our model, we argue that political instability increases ρ. Now consider the effects of a change in ρ on working hours h_t. Keane and Rogerson (2015) analyze a similar problem based on the elasticity of labor supply to changes in the tax and transfer scheme. We can use the same logic to derive the first-order condition for h_t

$$\log h_t = a\left[(\zeta-1)\log(\omega) - \zeta \log(t) - \log(\bar{c}) - \delta \log(\theta_0)\right] + a\zeta \log(1-\rho) + \delta \log(\theta_t), \tag{13}$$

where $\bar{c}$ is a constant and $a = \delta/(\zeta+\delta)$.[8] Equation (13) helps us to arrive at two important conclusions. First, labor supply depends on the productivity in t relative to the initial productivity in 0. This implies that factors which decrease productivity have a negative effect on labor supply. Second, labor supply declines for increasing ρ. This result indicates that individuals decrease their labor supply when the political instability initiated by coups increases the risk of defaulting wage payments.

An important question is whether the effects differ between women and men. We might expect that the elasticity is smaller for chief income earners of households, a role which—in countries with a high frequency of coups—is mostly occupied by men.

[8] See Keane and Rogerson (2012, 2015) for a detailed derivation.

5.5.2 Empirical analysis on the household level

To estimate the effect of coups on household-level outcomes, we use micro data from the World Value Survey (WVS). The WVS is the most extensive cross-country collection of micro data intended to measure individuals' beliefs, values, and well-being. The WVS also measures a wide range of socio-economic characteristics that we can use to evaluate the influence of coups. At the time we conduct our study, the WVS provides data from 341,271 individuals in 97 countries that are representative for about 90% of the world population. The key advantage of the WVS is its unparalleled coverage that includes both developed and developing countries. The data spans the period 1981-2016 and includes multiple observations of households living in countries during years in which a coup takes place. To analyze the microeconomic effects of coups, we combine our data on coup attempts with the individual-level data of the WVS.

Data for our variables of interest are available for a maximum of about 254,000 individuals in 85 countries. About 13,000 of the surveyed households experienced a coup, which is about 5% of all included observations. This proportion resembles the share of country-year observations with coups in the data used for our baseline model on the growth effect of coup attempts (4.4%). We estimate empirical models of the form

$$m_{ith} = \delta \mathrm{Coup}_{it} + \boldsymbol{X}_{ith}\boldsymbol{\beta} + \eta_i + \varphi_t + \varepsilon_{ith}, \qquad (14)$$

where m_{ith} is the relevant outcome for individual h living in country i at time t. In accordance with our stylized model, we analyze the effect of a coup in country i at t (denoted with Coup_{it}) on employment and the financial situation of individuals. Consistent with the implications of the model, we also look at factors that influence the productivity of individuals, namely health and life satisfaction. Both variables arguably influence the ability of individuals to produce output and may be influenced by political instability and the accompanying tensions and violence.

The matrix $\boldsymbol{X}_{ith}$ controls for individual socio-economic characteristics, including age, age squared, education, the decile on the national income distribution, a dummy variable for retired individuals, and a dummy variable for individuals that are students or on educational training. To account for unobserved time-invariant heterogeneity in the form of institutions, culture, geography, and national coup history, we include a country fixed effect η_i in the regression. We also include a wave fixed effect φ_t to account for cross-national trends in coup occurrence documented in Section 5.2.1.

Our outcome variables are measured based on different questions of the WVS. To assess the financial situation of households, we use question V59 of the sixth wave of the WVS (V64, V132, V80, and V68 in Waves 1-5), which asks respondents: "*How satisfied are you with the financial situation of your household? If '1' means you are completely dissatisfied on this scale, and '10' means you are completely satisfied, where would you put your satisfaction with your household's financial situation?*". The employment status is recovered from question V229 of Wave 6 (V220, V358, V229, and V241 in previous waves). Health is measured based on question V11 (alternative numbering: V82 in Wave 2 and V12 in Wave 4): "*All in all, how would you describe your state of health these days? Would you say it is very good, good, fair, or poor?*". Finally, life satisfaction refers to question V10 (alternative numbering: V18 in Wave 2 and V11 in Wave 4): "*Taking all things together, would you say you are: Very happy, quite happy, not very happy, or not at all happy?*".

Table 5.5 presents our results on the effects of coups on the household level. The table reports estimates of two model specifications for each outcome variable (financial situation of households, unemployment, health, life satisfaction): (i) a specification that only includes fixed effects and our coup variable (labeled "reduced") and (ii) a fully-specified model that includes individual control variables. The results show that coups worsen the financial situation of households, increase unemployment, and decrease health and life satisfaction. Each effect is

statistically significant at the 1% level and relatively unaffected by the introduction of individual controls.

TABLE 5.5: EFFECTS OF COUPS D'ÉTAT ON THE HOUSEHOLD-LEVEL

	Financial situation		Unemployment		Health		Life satisfaction	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Reduced	*Controls*	*Reduced*	*Controls*	*Reduced*	*Controls*	*Reduced*	*Controls*
$Coup_{it}$	-0.300***	-0.387***	0.010***	0.019***	-0.197***	-0.173***	-0.289***	-0.294***
	(0.037)	(0.034)	(0.004)	(0.004)	(0.012)	(0.012)	(0.012)	(0.011)
$Income\ decile_{ith}$		0.342***		-0.013***		0.047***		0.049***
		(0.002)		(0.000)		(0.001)		(0.001)
Age_{ith}		-0.057***		-0.013***		-0.017***		-0.010***
		(0.002)		(0.000)		(0.001)		(0.001)
$Age\ squared_{ith}$		0.001***		0.000***		0.000***		0.000***
		(0.000)		(0.000)		(0.000)		(0.000)
$Student_{ith}$		-0.006		-0.195***		-0.030***		-0.013**
		(0.020)		(0.002)		(0.007)		(0.006)
$Retired_{ith}$		-0.030		-0.109***		-0.170***		-0.051***
		(0.020)		(0.002)		(0.007)		(0.006)
$Education_{ith}$		0.055***		-0.003***		0.032***		0.008***
		(0.002)		(0.000)		(0.001)		(0.001)
$Unemployed_{ith}$		-0.475***				-0.081***		-0.142***
		(0.018)				(0.006)		(0.006)
Country Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes
Wave Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes
Households	249,231	249,231	254,079	254,079	246,880	246,880	248,953	248,953
Countries	85	85	85	85	85	85	85	85
R^2	0.146	0.249	0.049	0.105	0.105	0.211	0.117	0.151
F Statistic	525.241	979.195	124.733	278.151	340.778	726.276	384.855	469.743

Notes: The table reports estimations on the effect of coup d'états on the household level, with robust standard errors (adjusted for heteroskedasticity) in parentheses. Household-level data is taken from the World Value Survey (WVS). We include all observations for which data on coups and data on household characteristics are available. Our combined dataset covers a maximum of 254,079 households in 85 countries observed between 1981 and 2016. The financial situation is measured with (referring to the last wave; alternative questions of earlier waves in parentheses) question V59 (V64, V132, V80, and V68 in earlier waves), where respondents are asked to classify their satisfaction with their household's financial situation on a scale running from 1 to 10. Employment status is measured with question V229 (V220, V358, V229, and V241 in previous waves). Health is measured based on question V11 (V82 and V12 in earlier waves), where respondents classify their health level as "very good", "good", "fair", or "poor". Life satisfaction refers to question V10 (V18 and V11 in earlier waves), where respondents classify their life satisfaction on a 4-scale index. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

Possible threats to the identification of the coup effect on the individual-level outcomes come from differences across age cohorts or sub-national regions. To examine the influence of these factors on our results, we re-estimate our micro-level models with cohort fixed effects and region fixed effects, with very little impact on inferences: while the model on the effect of coup activity on unemployment in column (3) of Table 5.5 yields an estimate of 0.10, the effect is 0.11 and remains statistically significant at the 1% level when we include cohort and region fixed effects. The same applies to the other outcomes and model specifications (not reported).

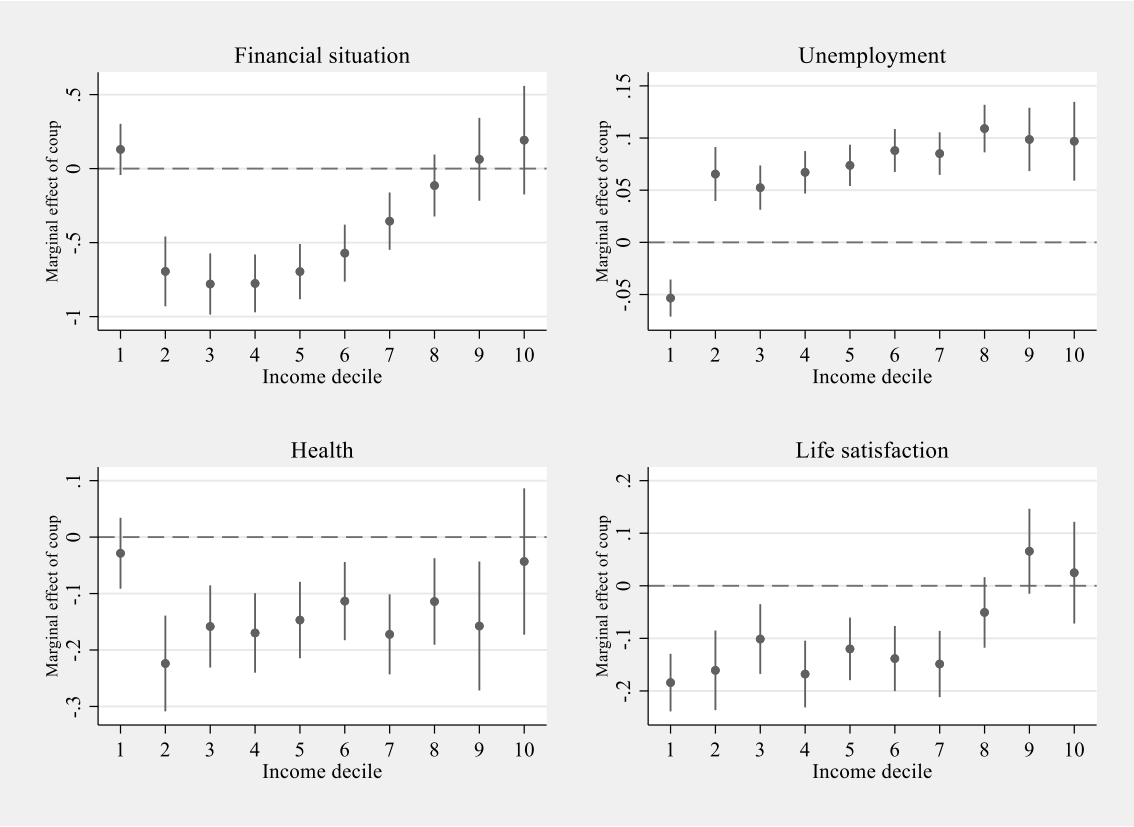

FIGURE 5.10: EFFECT OF COUPS ON INDIVIDUAL CHARACTERISTICS DEPENDENT ON THE INCOME DECILE OF HOUSEHOLDS

Notes: The figure shows the effect of coups on household characteristics dependent on the income decile of households in the national income distribution. The results are derived based on approximately 250,000 households in 85 countries (see Table 5.5). Vertical lines represent the 95% confident intervals.

It is conceivable that the effect of coups on individual-level outcomes varies across income groups. In particular, the effects may be different between the elite and the working class. To examine differences in the coup effect relative to the position of the household on the national income ladder, we re-estimate our models with interaction terms that account for the income decile of the respondent. The results are visualized in Figure 5.10 and indicate distinct pattern of the coup effect relative to the income level. The figure suggests that the financial situation and the health level of the poorest 10% is relatively unaffected by coups. However, coups substantially decrease the financial situation and the health level of individuals from the second income decile to the upper middle class. Top-income earners on average are not affected by coups. While the employment effect is negative for all income groups except for the poorest 10%, coups influence life satisfaction of the poor and the middle class, but have little effect on life satisfaction of top-income earners.

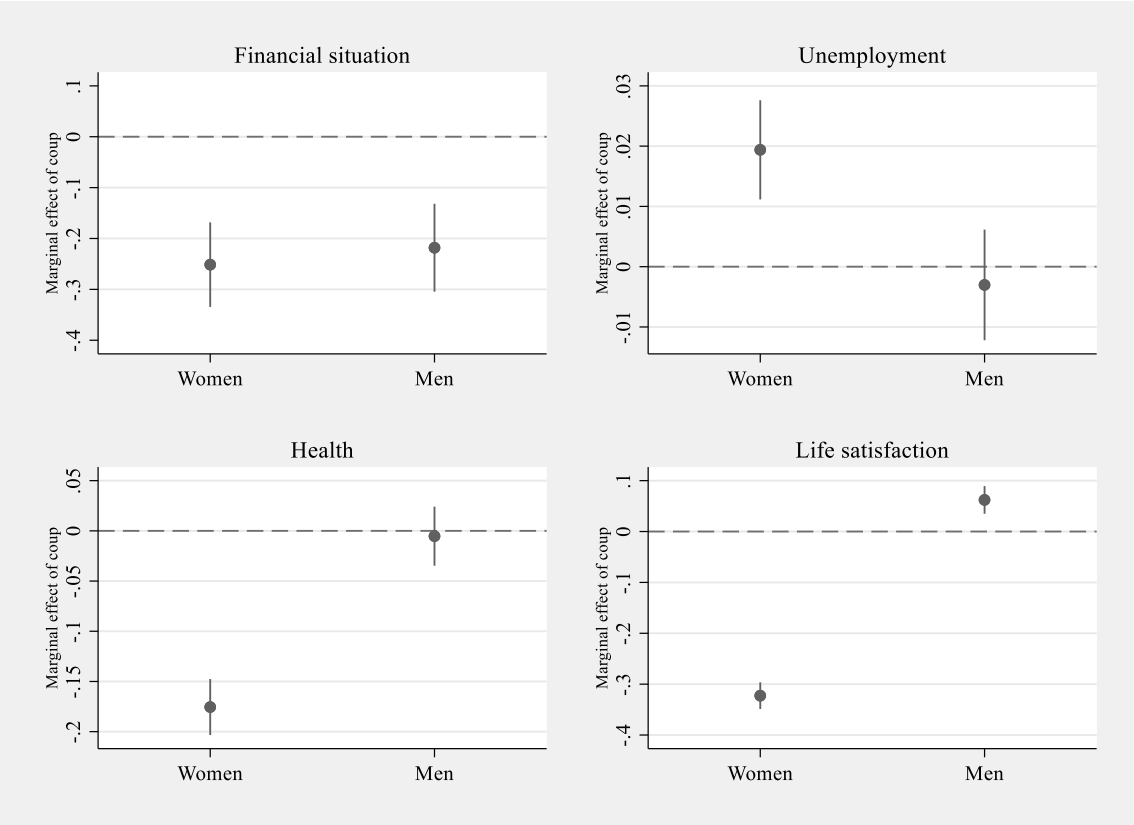

FIGURE 5.11: GENDER DIFFERENCES IN THE EFFECT OF COUPS

Notes: The figure shows the effect of coups on the financial situation, unemployment, health and life satisfaction conditional on the gender of the respondents. The results are derived based on approximately 250,000 households in 85 countries (see Table 5.5). Vertical lines represent the 95% confident intervals.

Figure 5.11 examines gender differences in the coup effect. While we do not find large differences between women and men regarding the financial situation, the employment effect seems predominately caused by an adverse employment effect for women, while employment of men on average remains unaffected by coups. One interpretation of this result may be that in countries with higher exposure to coups, the elasticity of labor supply is lower for men than for women. We also observe that the negative effects of coups on health and life satisfaction are almost only driven by an adverse effect on women.

Finally, in Table A5.34 in the Appendix, we examine whether coups influence individuals' expectations and preferences. We associate the experience of a coup with expectations about the future, measured by question V50, where respondents are asked to classify their view on the statements *"humanity has a bright future"* versus *"humanity has a bleak future"*. We use this data to construct a dummy for negative future expectations. We also

examine the extent to which individuals have confidence in their government (V115, measured on a four-scale ladder) and attitudes towards democracy (V140, measured on a ten-scale ladder). Again, we report unconditional correlations and estimates conditioned on socio-economic characteristics. The results show that the experience of a coup d'état depresses individuals' expectations about the future. Coups also decrease confidence in the government and lower the subjective importance of democracy. Given the importance of expectations and preferences for decision making (Falk et al. 2018), the results of Table A5.34 suggest that coups can also have economically important psychological effects that go beyond proximate socio-economic factors.

5.6 Conclusion

Motivated by the growing interest in and lack of evidence for the economic effects of political instability, we study how coups d'état influence economic growth. Our results show that there is a statistically and economically significant negative effect of coups on per capita GDP growth. Across manifold model specifications on the country-level and the sub-national level, a coup is associated with a decrease in per capita GDP of 2-3 percentage points. The abundance of evidence, drawn from manifold empirical techniques and all leading to very similar results, gives us confidence that there is a causal effect of coups d'état on future GDP growth.

Our focus on coups d'état highlights a particular aspect of political instability, one that mirrors the *zeitgeist* of countries' political environment. Against the backdrop of increasing instability tendencies in the Western world, our results paint a pessimistic picture but advocate for the establishment of a stable political environment. We propose several directions for future research. First, more quantitative country case studies are needed to better understand the political instability caused by coups. Our synthetic control analyses provide a first step in this direction, but the specific circumstances are yet to explore. Second, the mechanisms through which coups d'état and political instability influence economic development are still poorly

understood. Third, our microeconomic results show how socio-economic characteristics of individuals react to coups in the short-run, but more research should be conducted on the long-run effects of political instability.

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Appendix I: Additional country case studies

This section provides accompanying case study evidence to Section 5.3.7. We show the result of synthetic control methods for coups with the typical pattern of a decrease in the development of GDP relative to the synthetic control group (Argentina 1988, Thailand 2006, and Egypt 2013), and also show results for the 1987 coup in Tunisia, for which this pattern cannot be observed.

The coup in Tunisia 1987 is said to be a "constitutional coup" in which Prime Minister Zine El Abidine Ben Ali replaced the aging and sick President Habib Bourguiba, backed by the Tunisian Constitution. The synthetic control analysis for this successful coup shows no effect on GDP per capita. This is not surprising, given that there was no change in policies and no increase in uncertainty after the unseating of Bourguiba.

Argentina experienced several uprisings by the Argentine Army from 1987 to 1990 of which, however, none overturned the government. The synthetic control analysis shows the typical pattern of a reduction in GDP after the coup. In contrast to many other coups, however, there was a catch-up effect in GDP per capita in the aftermath of the 1988 Argentinian coup that was initiated in the early 1990s.

Thailand experienced a successful coup in 2006, which was led by the Royal Thai Army. The military unseated Prime Minister Thaksin Shinawatra after a year of political crises and unrest associated with his government. Under military rule, human rights and freedom of expression have been restricted, Thailand received a new constitution and the party of Thaksin Shinawatra was banned from the elections in 2007. The turmoil that accompanied the 2006 coup in Thailand led to a period of high political instability.

In a similar vein, the successful military coup in Egypt 2013 overthrew the democratically elected President Morsi and suspended the Egyptian constitution. The coup was justified with the repression of the opposition by Morsi's Muslim Brotherhood and disappointment about the democratization process in Egypt.

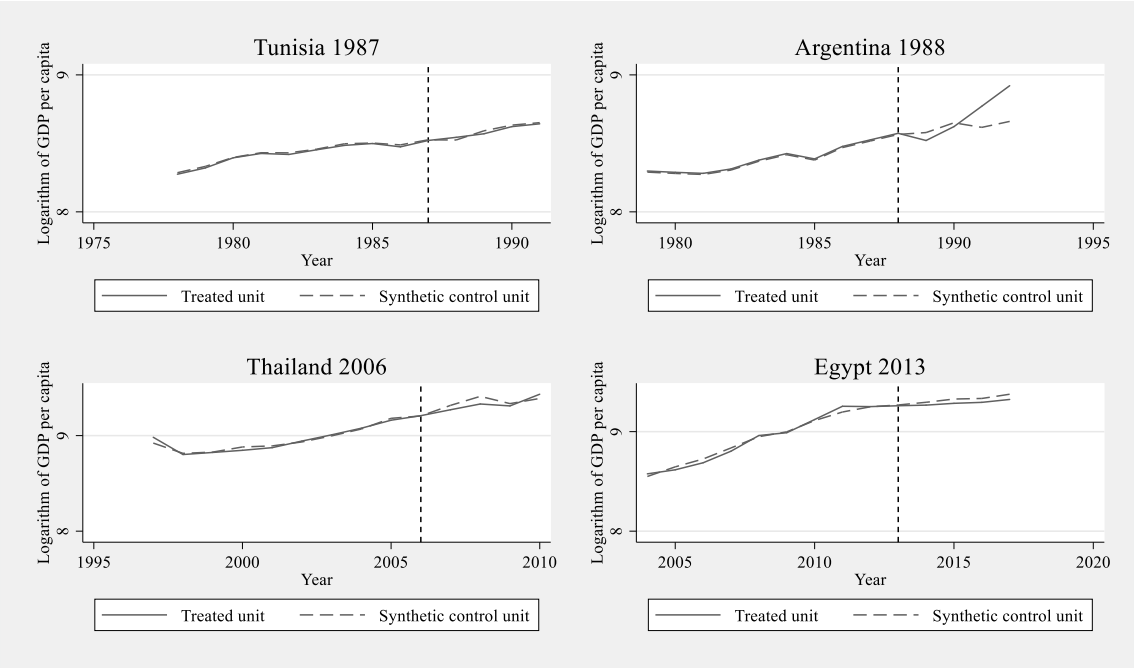

FIGURE A5.1: SYNTHETIC CONTROL ANALYSIS FOR SELECTED COUP EXAMPLES

Notes: The solid line represents the development of per capita GDP (log scale) of the treated country, the dashed line is the counterfactual development suggested by the synthetic control group. All countries only feature one coup during the time period used for the synthetic control analysis. The donor pool includes countries without coups or coup attempts. Weights: Tunisia: Belize 4.7%, Botswana: 21.8%, Cape Verde 17.8%, Malaysia 5.6%, Mexico 16.5%, Nicaragua 5.1%, Singapore 4.2%, Vietnam 14.4%; Argentina: Antigua and Barbuda 13.9%, Botswana: 4.2%, China 4.6%, Costa Rica 15.6%, Kuwait 0.9%, Mauritius 1.3%, Mongolia 18.3%, St. Lucia 12.8%, St. Vincent and Grenadines 25.5%, Vietnam 2.8%; Thailand: Barbados 1.2%, Malaysia 50%, Ukraine 31.2%, Uzbekistan 17.6%; Egypt: Albania 42%, India 13.7%, Kazakhstan 21.8%, St. Lucia 0.6%, Uzbekistan 21.9%.

Appendix II: Supplementary tables

TABLE A5.1: LIST OF COUNTRIES

ID	Country	Coups	Years	From	To	Included in event study
1	Albania	-	48	1970	2017	no
2	Algeria	3	58	1960	2017	yes
3	Angola	1	48	1970	2017	yes
4	Anguilla	-	48	1970	2017	no
5	Antigua and Barbuda	-	48	1970	2017	no
6	Argentina	10	68	1950	2017	yes
7	Armenia	-	28	1990	2017	no
8	Aruba	-	48	1970	2017	no
9	Australia	-	68	1950	2017	no
10	Austria	-	68	1950	2017	no
11	Azerbaijan	1	28	1990	2017	yes
12	Bahamas	-	48	1970	2017	no
13	Bahrain	2	48	1970	2017	yes
14	Bangladesh	10	59	1959	2017	yes
15	Barbados	-	58	1960	2017	no
16	Belarus	-	28	1990	2017	no
17	Belgium	-	68	1950	2017	no
18	Belize	-	48	1970	2017	no
19	Benin	11	59	1959	2017	yes
20	Bermuda	-	48	1970	2017	no
21	Bhutan	-	48	1970	2017	no
22	Bolivia	17	68	1950	2017	yes
23	Bosnia-Herzegovina	-	28	1990	2017	no
24	Botswana	-	58	1960	2017	no
25	Brazil	5	68	1950	2017	yes
26	British Virgin Islands	-	48	1970	2017	no
27	Brunei	-	48	1970	2017	no
28	Bulgaria	1	48	1970	2017	yes
29	Burkina Faso	7	59	1959	2017	yes
30	Burundi	12	58	1960	2017	yes
31	Cambodia	5	48	1970	2017	yes
32	Cameroon	1	58	1960	2017	yes
33	Canada	-	68	1950	2017	no
34	Cape Verde	-	58	1960	2017	no
35	Cayman Islands	-	48	1970	2017	no
36	Central African Republic	8	58	1960	2017	yes
37	Chad	14	58	1960	2017	yes
38	Chile	2	67	1951	2017	no
39	China	-	66	1952	2017	no
40	Colombia	4	68	1950	2017	no
41	Comoros	14	58	1960	2017	yes
42	Congo, Dem. Rep.	4	68	1950	2017	yes
43	Congo, Republic of	6	58	1960	2017	no
44	Costa Rica	-	68	1950	2017	no
45	Cote d'Ivoire	4	58	1960	2017	yes
46	Croatia	-	28	1990	2017	no
47	Curacao	-	13	2005	2017	no
48	Cyprus	1	68	1950	2017	yes
49	Czech Rep.	-	28	1990	2017	no
50	Denmark	-	68	1950	2017	no
51	Djibouti	1	48	1970	2017	yes
52	Dominica	2	48	1970	2017	no
53	Dominican Republic	3	67	1951	2017	no
54	Ecuador	10	68	1950	2017	yes
55	Egypt	3	68	1950	2017	yes
56	El Salvador	3	68	1950	2017	yes
57	Equatorial Guinea	5	58	1960	2017	yes
58	Estonia	-	28	1990	2017	no
59	Ethiopia	5	68	1950	2017	yes
60	Fiji	4	58	1960	2017	yes
61	Finland	-	68	1950	2017	no
62	France	1	68	1950	2017	yes
63	Gabon	1	58	1960	2017	yes
64	Gambia, The	5	58	1960	2017	yes
65	Georgia	2	28	1990	2017	no
66	Germany	-	68	1950	2017	no
67	Ghana	10	63	1955	2017	yes
68	Greece	4	67	1951	2017	yes
69	Grenada	2	48	1970	2017	yes
70	Guatemala	10	68	1950	2017	yes
71	Guinea	4	59	1959	2017	yes
72	Guinea-Bissau	9	58	1960	2017	yes
73	Haiti	9	58	1960	2017	yes
74	Honduras	8	68	1950	2017	yes
75	Hong Kong	-	58	1960	2017	no

TABLE A5.1 CONTINUED: LIST OF COUNTRIES

ID	Country	Coups	Years	From	To	Included in event study
76	Hungary	-	48	1970	2017	no
77	Iceland	-	68	1950	2017	no
78	India	-	68	1950	2017	no
79	Indonesia	2	58	1960	2017	no
80	Iran	2	63	1955	2017	no
81	Iraq	5	48	1970	2017	yes
82	Ireland	-	68	1950	2017	no
83	Israel	-	68	1950	2017	no
84	Italy	-	68	1950	2017	no
85	Jamaica	1	65	1953	2017	yes
86	Japan	1	68	1950	2017	yes
87	Jordan	1	64	1954	2017	no
88	Kazakhstan	-	28	1990	2017	no
89	Kenya	1	68	1950	2017	yes
90	Kuwait	-	48	1970	2017	no
91	Kyrgyzstan	1	28	1990	2017	yes
92	Laos	2	48	1970	2017	yes
93	Latvia	-	28	1990	2017	no
94	Lebanon	1	48	1970	2017	yes
95	Lesotho	4	58	1960	2017	yes
96	Liberia	4	54	1964	2017	no
97	Lithuania	-	28	1990	2017	no
98	Luxembourg	-	68	1950	2017	no
99	Macao	-	48	1970	2017	no
100	Macedonia	-	28	1990	2017	no
101	Madagascar	7	58	1960	2017	no
102	Malawi	1	64	1954	2017	yes
103	Malaysia	-	63	1955	2017	no
104	Maldives	4	48	1970	2017	yes
105	Mali	5	58	1960	2017	yes
106	Malta	-	64	1954	2017	no
107	Mauritania	10	58	1960	2017	no
108	Mauritius	-	68	1950	2017	no
109	Mexico	-	68	1950	2017	no
110	Moldova	-	28	1990	2017	no
111	Mongolia	-	48	1970	2017	no
112	Montenegro	-	28	1990	2017	no
113	Morocco	2	66	1950	2015	no
114	Mozambique	1	58	1960	2017	yes
115	Myanmar	2	56	1962	2017	yes
116	Namibia	-	58	1960	2017	no
117	Nepal	2	58	1960	2017	yes
118	Netherlands	-	68	1950	2017	no
119	New Zealand	-	68	1950	2017	no
120	Nicaragua	-	68	1950	2017	no
121	Niger	7	58	1960	2017	yes
122	Nigeria	10	68	1950	2017	no
123	Norway	-	68	1950	2017	no
124	Oman	1	48	1970	2017	no
125	Pakistan	7	68	1950	2017	yes
126	Panama	8	68	1950	2017	yes
127	Paraguay	5	67	1951	2017	yes
128	Peru	7	68	1950	2017	yes
129	Philippines	6	68	1950	2017	yes
130	Poland	1	48	1970	2017	yes
131	Portugal	2	68	1950	2017	no
132	Qatar	3	48	1970	2017	no
133	Romania	-	58	1960	2017	no
134	Russia	1	28	1990	2017	no
135	Rwanda	2	58	1960	2017	yes
136	Sao Tome and Principe	2	48	1970	2017	yes
137	Saudi Arabia	-	48	1970	2017	no
138	Senegal	1	58	1960	2017	no
139	Serbia	-	28	1990	2017	no
140	Seychelles	2	58	1960	2017	yes
141	Sierra Leone	12	57	1961	2017	yes
142	Singapore	-	58	1960	2017	no
143	Sint Maarten	-	13	2005	2017	no
144	Slovak Republic	-	28	1990	2017	no
145	Slovenia	-	28	1990	2017	no
146	South Africa	-	68	1950	2017	no
147	South Korea	2	65	1953	2017	yes
148	Spain	3	68	1950	2017	no
149	Sri Lanka	2	68	1950	2017	yes
150	St. Kitts & Nevis	-	48	1970	2017	no
151	St. Lucia	-	48	1970	2017	no
152	St.Vincent & Grenadines	-	48	1970	2017	no
153	Sudan	7	48	1970	2017	yes

TABLE A5.1 CONTINUED: LIST OF COUNTRIES

ID	Country	Coups	Years	From	To	Included in event study
154	Surinam	7	48	1970	2017	yes
155	Swaziland	1	48	1970	2017	yes
156	Sweden	-	68	1950	2017	no
157	Switzerland	-	68	1950	2017	no
158	Syria	7	58	1960	2017	no
159	Taiwan	-	67	1951	2017	no
160	Tajikistan	1	28	1990	2017	no
161	Tanzania	2	58	1960	2017	yes
162	Thailand	13	68	1950	2017	yes
163	Togo	9	58	1960	2017	yes
164	Trinidad &Tobago	1	68	1950	2017	yes
165	Tunisia	2	58	1960	2017	yes
166	Turkey	6	68	1950	2017	yes
167	Turkmenistan	-	28	1990	2017	no
168	Turks and Caicos	-	48	1970	2017	no
169	UAE	2	48	1970	2017	yes
170	UK	-	68	1950	2017	no
171	USA	-	68	1950	2017	no
172	Uganda	7	68	1950	2017	yes
173	Ukraine	-	28	1990	2017	no
174	Uruguay	1	68	1950	2017	yes
175	Uzbekistan	-	28	1990	2017	no
176	Venezuela	8	68	1950	2017	yes
177	Vietnam	-	48	1970	2017	no
178	Yemen	-	29	1989	2017	no
179	Zambia	3	63	1955	2017	yes
180	Zimbabwe	1	64	1954	2017	yes

TABLE A5.2: SUMMARY STATISTICS

	Observations	Mean	Std. Dev.	Min	p25	p75	Max
Growth-related variables							
GDP growth (PWT)	9709	0.021327	0.062933	-0.670144	-0.002638	0.048140	0.941375
GDP per capita (PWT)	9889	12965.8	30798.17	131.3002	2274.093	14473.53	792461.3
GDP growth (WB)	7846	0.021397	0.057925	-0.649924	-0.00096	0.046086	1.403708
GDP per capita (WB)	8019	10965.81	16168.53	132.3032	1099.682	14061.38	116232.8
Coup-related variables							
Coup	9889	0.043685	0.204403	0	0	0	1
Single coup	9889	0.040651	0.197491	0	0	0	1
Multiple coups	9889	0.003034	0.054998	0	0	0	1
Successful coup	9889	0.018708	0.135497	0	0	0	1
Failed coup	9889	0.021944	0.146507	0	0	0	1
Civilian coup	9889	0.010719	0.102981	0	0	0	1
Military coup	9889	0.031955	0.175888	0	0	0	1
Royal coup	9889	0.001011	0.031785	0	0	0	1
Transition to autocracy	9889	0.004449	0.066558	0	0	0	1
Transition to aemocracy	9889	0.000708	0.026597	0	0	0	1
Government change	9889	0.005663	0.075042	0	0	0	1
Age of coup leader	432	38.11806	21.32986	0	32	52	89
Civilian rank index	432	0.277778	0.718768	0	0	0	3
Military rank index	432	6.643519	4.16401	0	4	11	12
Control variables							
Interstate war	9889	0.018505	0.134777	0	0	0	1
Internal war	9889	0.130549	0.336924	0	0	0	1
Civil and ethnic violence	7852	0.623026	1.581165	0	0	0	10
Coup experience	9889	1.746183	2.809321	0	0	2	17
Democracy	9889	0.499848	0.500025	0	0	1	1
Government change	9889	0.095864	0.294420	0	0	0	1
KOF Globalization Index	7152	51.08496	17.29259	14.2923	38.23158	63.45823	91.16795
Ethnic Fractionalization Index	7175	0.433986	0.270748	0	0.187	0.669	0.89

Notes: The table reports summary statistics of the variables used in the empirical estimations. GDP growth reports GDP per capita growth. Data refers to WB = World Band and PWT = Penn World Tables. GDP is measured in constant 2011 US dollars (PWT) and constant 2010 US dollars (WB) and expressed in absolute values. "Transition to Autocracy" and "Transition to Democracy" show regime transitions after a coup d'état. "Government Change" reports changes in government after a coup d'état. A description of the variables and corresponding data sources can be found in Sections 5.2 and 5.3.4. Column labeled "Std. Dev." reports the standard deviation, "p25" gives the 25th percentile, "p75" reports the 75th percentile.

TABLE A5.3: FLEXIBLE EVENT STUDY ANALYSIS—PARAMETER ESTIMATES FOR THE PANEL DIFFERENCE-IN-DIFFERENCES MODEL

GDP per capita growth

Treatment variable at the corresponding time pre- and post coups	*Parameter estimates*
Single coup ($T \leq t - 4$)	-0.011
	(0.007)
Single coup ($T = t - 3$)	-0.001
	(0.007)
Single coup ($T = t - 2$)	-
Single coup ($T = t - 1$)	-0.020***
	(0.007)
Single coup ($T = t$)	-0.031***
	(0.010)
Single coup ($T = t + 1$)	0.002
	(0.009)
Single coup ($T = t + 2$)	0.002
	(0.008)
Single coup ($T \geq t + 3$)	0.003
	(0.008)
Country Fixed Effects	yes
Year Fixed Effects	yes
Observations	1,058
Countries	80
R^2	0.228

Notes: The table reports the results of the flexible event study analysis (equation 4), which is shown graphically in Figure 6. Standard errors in parentheses. All standard errors account for heteroskedasticity and are clustered at the country level. Standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.4: FLEXIBLE EVENT STUDY ANALYSIS—PARAMETER ESTIMATES FOR THE FULL DYNAMIC PANEL DATA MODEL

Logarithm of GDP per capita

Treatment variable at the corresponding time pre- and post coups	*Parameter estimates*
GDP per capita, first lag[a]	0.775***
	(0.064)
GDP per capita, second lag[a]	0.056
	(0.046)
GDP per capita, third lag[a]	0.063
	(0.058)
GDP per capita, fourth lag[a]	0.047
	(0.060)
Single coup ($T \leq t - 4$)	-0.020*
	(0.012)
Single coup ($T = t - 3$)	0.000
	(0.011)
Single coup ($T = t - 2$)	-
Single coup ($T = t - 1$)	-0.019*
	(0.009)
Single coup ($T = t$)	-0.036**
	(0.015)
Single coup ($T = t + 1$)	0.003
	(0.010)
Single coup ($T = t + 2$)	0.006
	(0.011)
Single coup ($T \geq t + 3$)	-0.002
	(0.013)
Country Fixed Effects	yes
Year Fixed Effects	yes
Observations	1,030
Countries	80
R^2 Overall	0.919
R^2 Within	0.919
R^2 Between	0.996

Notes: The table reports the results of the flexible event study analysis (equation 5), which is shown graphically in Figure 7. Standard errors in parentheses. All standard errors account for heteroskedasticity and are clustered at the country level. Standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.5: COUPS D'ÉTAT AND ECONOMIC GROWTH—SAMPLE OF COUNTRIES WITH AT LEAST ONE COUP BETWEEN 1950 AND 2017, PANEL DIFFERENCE-IN-DIFFERENCES MODEL

GDP per capita growth	(1)	(2)	(3)	(4)	(5)	(6)
$Coup_{it}$	-0.024***	-0.022***	-0.023***	-0.021***	-0.021***	
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	
Single $coup_{it}$						-0.021***
						(0.003)
Multiple $coups_{it}$					-0.003	-0.024
					(0.023)	(0.023)
Country Fixed Effects	no	yes	no	yes	yes	yes
Year Fixed Effects	no	no	yes	yes	yes	yes
Observations	5,854	5,854	5,854	5,854	5,854	5,854
Countries	102	102	102	102	102	102
R^2	0.009	0.050	0.051	0.092	0.092	0.092

Notes: The table reports the results of panel difference-in-differences estimations on the effect of coups d'état on economic growth (equation 1). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. Per capita GDP growth is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The variable "Coup" denotes whether a coup has taken place at a given year, "Single Coup" only considers country-year observations with one coup in a given year, and "Multiple Coups" considers country-year observations with multiple coups in a given year. The sample is restricted to country-year observations of countries that experienced at least one coup in the sample period between 1950 and 2017. For details, see Table A5.1. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.6: COUPS D'ÉTAT AND ECONOMIC GROWTH—SAMPLE OF COUNTRIES WITH AT LEAST ONE COUP BETWEEN 1950 AND 2017, FULL DYNAMIC PANEL DATA MODEL

Logarithm of GDP per capita	(1)	(2)	(3)	(4)	(5)	(6)
$Coup_{it}$	-0.027***	-0.029***	-0.025***	-0.024***	-0.025***	
	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	
Single $coup_{it}$						-0.025***
						(0.007)
Multiple $coups_{it}$					0.018	-0.007
					(0.014)	(0.013)
$Log(GDP^{pc})$ (t – 1)	1.103***	1.075***	1.083***	1.039***	1.039***	1.039***
	(0.025)	(0.025)	(0.025)	(0.026)	(0.026)	(0.026)
$Log(GDP^{pc})$ (t – 2)	-0.029	-0.024	-0.011	-0.007	-0.007	-0.007
	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)
$Log(GDP^{pc})$ (t – 3)	-0.009	-0.006	-0.015	-0.011	-0.011	-0.011
	(0.028)	(0.028)	(0.028)	(0.027)	(0.027)	(0.027)
$Log(GDP^{pc})$ (t – 4)	-0.068***	-0.069***	-0.059***	-0.063***	-0.063***	-0.063***
	(0.017)	(0.018)	(0.017)	(0.017)	(0.017)	(0.017)
Country Fixed Effects	no	yes	no	yes	yes	yes
Year Fixed Effects	no	no	yes	yes	yes	yes
Observations	5,548	5,548	5,548	5,548	5,548	5,548
Countries	102	102	102	102	102	102
R^2 Overall	0.992	0.992	0.993	0.993	0.993	0.993
R^2 Within	0.963	0.963	0.966	0.966	0.966	0.966
R^2 Between	0.999	0.999	0.999	0.999	0.999	0.999

Notes: The table reports the results of dynamic panel data estimations on the effect of coups d'état on economic growth (equation 2). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. The log of per capita GDP is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The variable "Coup" denotes whether a coup has taken place at a given year, "Single Coup" only considers country-year observations with one coup in a given year, and "Multiple Coups" considers country-year observations with multiple coups in a given year. The sample is restricted to country-year observations of countries that experienced at least one coup in the sample period between 1950 and 2017. For details, see Table A5.1. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.7: COUPS D'ÉTAT AND ECONOMIC GROWTH—SAMPLE OF NON-OVERLAPPING FIVE-YEAR AVERAGES, PANEL DIFFERENCE-IN-DIFFERENCES MODEL

GDP per capita growth	(1)	(2)	(3)	(4)
$Coup_{it}$	-0.012***	-0.009***	-0.013***	-0.009***
	(0.003)	(0.003)	(0.003)	(0.003)
Country Fixed Effects	no	yes	no	yes
Year Fixed Effects	no	no	yes	yes
Observations	2,051	2,051	2,051	2,051
Countries	180	180	180	180
R^2	0.013	0.139	0.088	0.212

Notes: The table reports the results of panel difference-in-differences estimations on the effect of coups d'état on economic growth (equation 1). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. Per capita GDP growth is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The variable "Coup" denotes whether a coup has taken place at a given year. Estimates are based on non-overlapping five year averages (1960-1964; 1965-1969; ...), where each five-year interval serves as a unit of observation. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.8: COUPS D'ÉTAT AND ECONOMIC GROWTH—SAMPLE OF NON-OVERLAPPING FIVE-YEAR AVERAGES, FULL DYNAMIC PANEL DATA MODEL

Logarithm of GDP per capita	(1)	(2)	(3)	(4)
$Coup_{it}$	-0.105***	-0.094***	-0.102***	-0.076***
	(0.020)	(0.020)	(0.019)	(0.019)
$Log(GDP^{pc})$ (t – 1)	0.966***	0.911***	0.967***	0.860***
	(0.007)	(0.013)	(0.007)	(0.026)
Country Fixed Effects	no	yes	no	yes
Year Fixed Effects	no	no	yes	yes
Observations	1,881	1,881	1,881	1,881
Countries	180	180	180	180
R^2 Overall	0.967	0.967	0.970	0.968
R^2 Within	0.847	0.847	0.863	0.866
R^2 Between	0.993	0.993	0.993	0.992

Notes: The table reports the results of dynamic panel data estimations on the effect of coups d'état on economic growth (equation 2). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. Per capita GDP growth is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The variable "Coup" denotes whether a coup has taken place at a given year. Estimates are based on non-overlapping five year averages (1960-1964; 1965-1969; ...), where each five-year interval serves as a unit of observation. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.9: COUPS D'ÉTAT AND ECONOMIC GROWTH—ALTERNATIVE CODING SCHEME OF COUPS (CALENDAR YEARS), PANEL DIFFERENCE-IN-DIFFERENCES MODEL

GDP per capita growth	(1)	(2)	(3)	(4)	(5)	(6)
$Coup_{it}$	-0.026***	-0.021***	-0.026***	-0.021***	-0.021***	
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	
Single $coup_{it}$						-0.021***
						(0.004)
Multiple $coups_{it}$					0.006	-0.015
					(0.017)	(0.016)
Country Fixed Effects	no	yes	no	yes	yes	yes
Year Fixed Effects	no	no	yes	yes	yes	yes
Observations	9,709	9,709	9,709	9,709	9,709	9,709
Countries	180	180	180	180	180	180
R^2	0.007	0.058	0.058	0.109	0.109	0.109

Notes: The table reports the results of panel difference-in-differences estimations on the effect of coups d'état on economic growth (equation 1). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. Per capita GDP growth is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The variable "Coup" denotes whether a coup has taken place at a given year, "Single Coup" only considers country-year observations with one coup in a given year, and "Multiple Coups" considers country-year observations with multiple coups in a given year. The coding scheme of coups d'état differs from the original coding of Bjørnskov and Rode (2019) and re-codes coups to match calendar years. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.10: COUPS D'ÉTAT AND ECONOMIC GROWTH—ALTERNATIVE CODING SCHEME OF COUPS (CALENDAR YEARS), FULL DYNAMIC PANEL DATA MODEL

Logarithm of GDP per capita	(1)	(2)	(3)	(4)	(5)	(6)
$Coup_{it}$	-0.035***	-0.030***	-0.033***	-0.025***	-0.026***	
	(0.007)	(0.006)	(0.007)	(0.006)	(0.007)	
Single $coup_{it}$						-0.026***
						(0.007)
Multiple $coups_{it}$					0.005	-0.021*
					(0.012)	(0.011)
$Log(GDP^{pc})$ (t – 1)	0.888***	0.852***	0.874***	0.828***	0.828***	0.828***
	(0.075)	(0.075)	(0.075)	(0.072)	(0.072)	(0.072)
$Log(GDP^{pc})$ (t – 2)	0.148**	0.145**	0.161**	0.155**	0.155**	0.155**
	(0.068)	(0.066)	(0.068)	(0.066)	(0.066)	(0.066)
$Log(GDP^{pc})$ (t – 3)	0.004	0.007	-0.000	0.002	0.002	0.002
	(0.044)	(0.044)	(0.047)	(0.047)	(0.047)	(0.047)
$Log(GDP^{pc})$ (t – 4)	-0.044	-0.030	-0.039	-0.029	-0.029	-0.029
	(0.040)	(0.043)	(0.043)	(0.044)	(0.045)	(0.045)
Country Fixed Effects	no	yes	no	yes	yes	yes
Year Fixed Effects	no	no	yes	yes	yes	yes
Observations	9,169	9,169	9,169	9,169	9,169	9,169
Countries	180	180	180	180	180	180
R^2 Overall	0.990	0.990	0.990	0.990	0.990	0.990
R^2 Within	0.946	0.946	0.948	0.949	0.949	0.949
R^2 Between	0.999	0.999	0.999	0.999	0.999	0.999

Notes: The table reports the results of dynamic panel data estimations on the effect of coups d'état on economic growth (equation 2). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. The log of per capita GDP is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The variable "Coup" denotes whether a coup has taken place at a given year, "Single Coup" only considers country-year observations with one coup in a given year, and "Multiple Coups" considers country-year observations with multiple coups in a given year. The coding scheme of coups d'état differs from the original coding of Bjørnskov and Rode (2019) and re-codes coups to match calendar years. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.11: COUPS D'ÉTAT AND ECONOMIC GROWTH—EFFECT OF TWO OR MORE COUPS, SAMPLE OF COUNTRY-YEARS WITH COUPS, PANEL DIFFERENCE-IN-DIFFERENCES MODEL

GDP per capita growth	(1)	(2)	(3)	(4)
Multiple coups$_{it}$	-0.009	0.014	0.006	0.025
	(0.022)	(0.018)	(0.027)	(0.022)
Country Fixed Effects	no	yes	no	yes
Year Fixed Effects	no	no	yes	yes
Observations	427	427	427	427
Countries	102	102	102	102
R^2	0.001	0.410	0.163	0.547

Notes: The table reports the results of panel difference-in-differences estimations on the effect of coups d'état on economic growth (equation 1). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. Per capita GDP growth is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The variable "Multiple Coups" considers country-year observations with multiple coups in a given year. The sample is restricted to country-year observations in which a coup has taken place. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.12: COUPS D'ÉTAT AND ECONOMIC GROWTH—EFFECT OF TWO OR MORE COUPS, SAMPLE OF COUNTRY-YEARS WITH COUPS, FULL DYNAMIC PANEL DATA MODEL

Logarithm of GDP per capita	(1)	(2)	(3)	(4)
Multiple coups$_{it}$	0.017	0.021*	0.027	0.024
	(0.013)	(0.013)	(0.019)	(0.016)
Log(GDP^{pc}) (t – 1)	1.091***	0.959***	1.041***	0.921***
	(0.129)	(0.094)	(0.121)	(0.080)
Log(GDP^{pc}) (t – 2)	-0.127	-0.084	-0.128	-0.133
	(0.168)	(0.152)	(0.154)	(0.152)
Log(GDP^{pc}) (t – 3)	0.104	0.152	0.090	0.163*
	(0.097)	(0.096)	(0.093)	(0.087)
Log(GDP^{pc}) (t – 4)	-0.091	-0.061	-0.028	0.004
	(0.072)	(0.076)	(0.074)	(0.076)
Country Fixed Effects	no	yes	no	yes
Year Fixed Effects	no	no	yes	yes
Observations	398	398	398	398
Countries	96	96	96	96
R^2 Overall	0.980	0.979	0.982	0.981
R^2 Within	0.929	0.930	0.944	0.946
R^2 Between	0.983	0.980	0.984	0.978

Notes: The table reports the results of dynamic panel data estimations on the effect of coups d'état on economic growth (equation 2). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. The log of per capita GDP is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The variable "Multiple Coups" considers country-year observations with multiple coups in a given year. The sample is restricted to country-year observations in which a coup has taken place. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.13: COUPS D'ÉTAT AND ECONOMIC GROWTH—EFFECT OF COUP SUCCESS, SAMPLE OF COUNTRY-YEARS WITH SINGLE COUPS, PANEL DIFFERENCE-IN-DIFFERENCES MODEL

GDP per capita growth	(1)	(2)	(3)	(4)
Successful $coup_{it}$	-0.010	-0.009	-0.013	-0.013
	(0.008)	(0.009)	(0.009)	(0.012)
Country Fixed Effects	no	yes	no	yes
Year Fixed Effects	no	no	yes	yes
Observations	397	397	397	397
Countries	101	101	101	101
R^2	0.005	0.378	0.181	0.533

Notes: The table reports the results of panel difference-in-differences estimations on the effect of coups d'état on economic growth (equation 1). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. Per capita GDP growth is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The variable "Successful Coup" considers country-year observations with coups that were successful in unseating the ruling government in a given year. The sample is restricted to country-year observations in which a single coup has taken place. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.14: COUPS D'ÉTAT AND ECONOMIC GROWTH—EFFECT OF COUP SUCCESS, SAMPLE OF COUNTRY-YEARS WITH SINGLE COUPS, FULL DYNAMIC PANEL DATA MODEL

Logarithm of GDP per capita	(1)	(2)	(3)	(4)
Successful $coup_{it}$	-0.012	-0.012	-0.014	-0.016
	(0.013)	(0.013)	(0.014)	(0.013)
$Log(GDP^{pc})$ (t – 1)	1.105***	0.958***	1.060***	0.932***
	(0.143)	(0.104)	(0.135)	(0.093)
$Log(GDP^{pc})$ (t – 2)	-0.133	-0.090	-0.133	-0.157
	(0.181)	(0.161)	(0.169)	(0.166)
$Log(GDP^{pc})$ (t – 3)	0.105	0.159	0.084	0.167*
	(0.104)	(0.106)	(0.097)	(0.096)
$Log(GDP^{pc})$ (t – 4)	-0.100	-0.064	-0.037	0.004
	(0.077)	(0.077)	(0.079)	(0.079)
Country Fixed Effects	no	yes	no	yes
Year Fixed Effects	no	no	yes	yes
Observations	370	370	370	370
Countries	96	96	96	96
R^2 Overall	0.979	0.978	0.982	0.980
R^2 Within	0.928	0.929	0.943	0.946
R^2 Between	0.983	0.980	0.985	0.979

Notes: The table reports the results of dynamic panel data estimations on the effect of coups d'état on economic growth (equation 2). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. The log of per capita GDP is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The variable "Successful Coup" considers country-year observations with coups that were successful in unseating the ruling government in a given year. The sample is restricted to country-year observations in which a single coup has taken place. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.15: COUPS D'ÉTAT AND ECONOMIC GROWTH—BASELINE SPECIFICATION WITH WORLD BANK GDP DATA, PANEL DIFFERENCE-IN-DIFFERENCES MODEL

GDP per capita growth	(1)	(2)	(3)	(4)	(5)	(6)
$Coup_{it}$	-0.025***	-0.020***	-0.024***	-0.019***	-0.019***	
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	
Single $coup_{it}$						-0.019***
						(0.004)
Multiple $coups_{it}$					-0.003	-0.022
					(0.018)	(0.018)
Country Fixed Effects	no	yes	no	yes	yes	yes
Year Fixed Effects	no	no	yes	yes	yes	yes
Observations	7,846	7,846	7,846	7,846	7,846	7,846
Countries	171	171	171	171	171	171
R^2	0.008	0.086	0.069	0.148	0.148	0.148

Notes: The table reports the results of panel difference-in-differences estimations on the effect of coups d'état on economic growth (equation 1). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. Per capita GDP growth is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The variable "Coup" denotes whether a coup has taken place at a given year, "Single Coup" only considers country-year observations with one coup in a given year, and "Multiple Coups" considers country-year observations with multiple coups in a given year. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.16: COUPS D'ÉTAT AND ECONOMIC GROWTH—BASELINE SPECIFICATION WITH WORLD BANK GDP DATA, FULL DYNAMIC PANEL DATA MODEL

Logarithm of GDP per capita	(1)	(2)	(3)	(4)	(5)	(6)
$Coup_{it}$	-0.017***	-0.015***	-0.016***	-0.013***	-0.014***	
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	
Single $coup_{it}$						-0.014***
						(0.004)
Multiple $coups_{it}$					0.015*	0.001
					(0.009)	(0.008)
Log(GDP^{pc}) (t – 1)	1.252***	1.191***	1.243***	1.173***	1.173***	1.173***
	(0.065)	(0.065)	(0.067)	(0.067)	(0.067)	(0.067)
Log(GDP^{pc}) (t – 2)	-0.185**	-0.164**	-0.175**	-0.153*	-0.153*	-0.153*
	(0.085)	(0.080)	(0.084)	(0.079)	(0.079)	(0.079)
Log(GDP^{pc}) (t – 3)	0.017	0.017	0.021	0.022	0.022	0.022
	(0.026)	(0.024)	(0.025)	(0.023)	(0.023)	(0.023)
Log(GDP^{pc}) (t – 4)	-0.085***	-0.063***	-0.091***	-0.070***	-0.070***	-0.070***
	(0.016)	(0.016)	(0.017)	(0.017)	(0.017)	(0.017)
Country Fixed Effects	no	yes	no	yes	yes	yes
Year Fixed Effects	no	no	yes	yes	yes	yes
Observations	7,333	7,333	7,333	7,333	7,333	7,333
Countries	171	171	171	171	171	171
R^2 Overall	0.999	0.999	0.999	0.999	0.999	0.999
R^2 Within	0.982	0.982	0.983	0.984	0.984	0.984
R^2 Between	0.999	0.999	0.999	0.999	0.999	0.999

Notes: The table reports the results of dynamic panel data estimations on the effect of coups d'état on economic growth (equation 2). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. The log of per capita GDP is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The variable "Coup" denotes whether a coup has taken place at a given year, "Single Coup" only considers country-year observations with one coup in a given year, and "Multiple Coups" considers country-year observations with multiple coups in a given year. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.17: COUPS D'ÉTAT AND ECONOMIC GROWTH—CONTROLLING FOR POTENTIAL CONFOUNDING FACTORS, PANEL DIFFERENCE-IN-DIFFERENCES MODEL

GDP per capita growth	(1)	(2)	(3)	(4)	(5)	(6)
$Coup_{it}$	-0.027***	-0.020***	-0.028***	-0.021***	-0.020***	
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
Single $coup_{it}$						-0.020***
						(0.005)
Multiple $coups_{it}$					-0.006	-0.026
					(0.022)	(0.021)
Interstate war_{it}	-0.020	-0.038**	-0.020	-0.038**	-0.038**	-0.038**
	(0.016)	(0.019)	(0.015)	(0.018)	(0.018)	(0.018)
Internal war_{it}	-0.003	-0.010**	-0.004	-0.009**	-0.009**	-0.009**
	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
Civil and ethnic $violence_{it}$	-0.001	-0.005***	-0.001	-0.004***	-0.004***	-0.004***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Coup $experience_{it}$	-0.001	-0.003**	-0.001	-0.002	-0.002	-0.002
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
$Democracy_{it}$	-0.003	-0.004	-0.002	-0.002	-0.002	-0.002
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
KOF $Globalization\ Index_{it}$	0.001***	0.002***	0.001***	0.002***	0.002***	0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Ethnic $Fractionalization\ Index_{it}$	-0.016**	-0.118**	-0.017**	-0.113**	-0.112**	-0.112**
	(0.007)	(0.049)	(0.007)	(0.047)	(0.046)	(0.046)
$Log(GDP^{pc})$ (t – 2)	-0.018***	-0.058***	-0.016***	-0.056***	-0.056***	-0.056***
	(0.002)	(0.008)	(0.002)	(0.008)	(0.008)	(0.008)
Country Fixed Effects	no	yes	no	yes	yes	yes
Year Fixed Effects	no	no	yes	yes	yes	yes
Observations	5,514	5,514	5,514	5,514	5,514	5,514
Countries	138	138	138	138	138	138
R^2	0.053	0.175	0.096	0.209	0.209	0.209

Notes: The table reports the results of panel difference-in-differences estimations on the effect of coups d'état on economic growth (equation 1). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. Per capita GDP growth is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The variable "Coup" denotes whether a coup has taken place at a given year, "Single Coup" only considers country-year observations with one coup in a given year, and "Multiple Coups" considers country-year observations with multiple coups in a given year. Dummy variables for interstate and internal war are constructed based on data from the UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002, Version 17.2). Scores for civil and ethnic violence are taken from the Major Episodes of Political Violence (MEPV) and Conflict Regions, 1946-2016 dataset (Version July 25, 2017). Coup experience is measured with the cumulative number of coups in a country since 1950. Democracy is measured via the dichotomous democracy indicator of Bjørnskov and Rode (2019). Globalization is measured with the KOF Globalization Index of Dreher (2006) and Gygli et al. (2019). Data on ethnic fractionalization is from the Historical Index of Ethnic Fractionalization Dataset (HIEF) (see Drazanova 2019). The second lag of GDP per capita accounts for the current level of economic development. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.18: COUPS D'ÉTAT AND ECONOMIC GROWTH—CONTROLLING FOR POTENTIAL CONFOUNDING FACTORS, FULL DYNAMIC PANEL DATA MODEL

Logarithm of GDP per capita	(1)	(2)	(3)	(4)	(5)	(6)
$Coup_{it}$	-0.032***	-0.026***	-0.031***	-0.026***	-0.028***	
	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)	
Single $coup_{it}$						-0.028***
						(0.009)
Multiple $coups_{it}$					0.035**	0.006
					(0.016)	(0.013)
Interstate war_{it}	-0.023	-0.039*	-0.023	-0.040**	-0.040**	-0.040**
	(0.018)	(0.021)	(0.017)	(0.020)	(0.020)	(0.020)
Internal war_{it}	-0.001	-0.008	-0.001	-0.008	-0.008	-0.008
	(0.005)	(0.007)	(0.005)	(0.006)	(0.006)	(0.006)
Civil and ethnic $violence_{it}$	-0.001	-0.005***	-0.001	-0.004**	-0.004**	-0.004**
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Coup $experience_{it}$	-0.000	-0.003***	-0.000	-0.004***	-0.004***	-0.004***
	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
$Democracy_{it}$	-0.002	0.000	-0.001	0.002	0.002	0.002
	(0.003)	(0.006)	(0.003)	(0.006)	(0.006)	(0.006)
KOF Globalization $Index_{it}$	0.001***	0.003***	0.001***	0.001	0.001	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Ethnic Fractionalization $Index_{it}$	-0.013**	-0.042	-0.017***	-0.091*	-0.092*	-0.092*
	(0.005)	(0.053)	(0.005)	(0.052)	(0.052)	(0.052)
$Log(GDP^{pc})$ (t – 1)	1.107***	1.043***	1.101***	1.032***	1.032***	1.032***
	(0.028)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)
$Log(GDP^{pc})$ (t – 2)	-0.053*	-0.046	-0.037	-0.030	-0.031	-0.031
	(0.032)	(0.031)	(0.032)	(0.031)	(0.031)	(0.031)
$Log(GDP^{pc})$ (t – 3)	0.014	0.018	-0.001	0.004	0.005	0.005
	(0.032)	(0.030)	(0.031)	(0.029)	(0.029)	(0.029)
$Log(GDP^{pc})$ (t – 4)	-0.085***	-0.084***	-0.077***	-0.075***	-0.075***	-0.075***
	(0.016)	(0.017)	(0.016)	(0.016)	(0.016)	(0.016)
Country Fixed Effects	no	yes	no	yes	yes	yes
Year Fixed Effects	no	no	yes	yes	yes	yes
Observations	5,442	5,442	5,442	5,442	5,442	5,442
Countries	138	138	138	138	138	138
R^2 Overall	0.995	0.994	0.995	0.994	0.994	0.994
R^2 Within	0.952	0.952	0.955	0.956	0.956	0.956
R^2 Between	0.999	0.999	0.999	0.999	0.999	0.999

Notes: The table reports the results of dynamic panel data estimations on the effect of coups d'état on economic growth (equation 2). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. Per capita GDP growth is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The variable "Coup" denotes whether a coup has taken place at a given year, "Single Coup" only considers country-year observations with one coup in a given year, and "Multiple Coups" considers country-year observations with multiple coups in a given year. Dummy variables for interstate and internal war are constructed based on data from the UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002, Version 17.2). Scores for civil and ethnic violence are taken from the Major Episodes of Political Violence (MEPV) and Conflict Regions, 1946-2016 dataset (Version July 25, 2017). Coup experience is measured with the cumulative number of coups in a country since 1950. Democracy is measured via the dichotomous democracy indicator of Bjørnskov and Rode (2019). Globalization is measured with the KOF Globalization Index of Dreher (2006) and Gygli et al. (2019). Data on ethnic fractionalization is from the Historical Index of Ethnic Fractionalization Dataset (HIEF) (see Drazanova 2019). *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.19: COUPS D'ÉTAT AND ECONOMIC GROWTH—REGIONAL DIFFERENCES, PANEL DIFFERENCE-IN-DIFFERENCES MODEL

GDP per capita growth	(1) Africa	(2) Africa	(3) Africa	(4) America	(5) America	(6) America	(7) Asia-Oceania	(8) Asia-Oceania	(9) Asia-Oceania	(10) Europe
$Coup_{it}$	-0.020***	-0.019***		-0.023***	-0.023***		-0.024*	-0.024**		-0.055***
	(0.005)	(0.004)		(0.004)	(0.005)		(0.012)	(0.011)		(0.018)
Single $coup_{it}$			-0.019***			-0.023***			-0.024**	
			(0.004)			(0.005)			(0.011)	
Multiple $coups_{it}$		-0.009	-0.028		0.004	-0.020***		-0.011	-0.035	
		(0.025)	(0.025)		(0.008)	(0.006)		(0.141)	(0.141)	
Country Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	2,910	2,910	2,910	2,311	2,311	2,311	2,503	2,503	2,503	1,985
Countries	50	50	50	41	41	41	49	49	49	40
R^2	0.103	0.103	0.103	0.176	0.176	0.176	0.101	0.101	0.101	0.322

Notes: The table reports the results of panel difference-in-differences estimations on the effect of coups d'état on economic growth (equation 1). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. Per capita GDP growth is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The variable "Coup" denotes whether a coup has taken place at a given year, "Single Coup" only considers country-year observations with one coup in a given year, and "Multiple Coups" considers country-year observations with multiple coups in a given year. The regional classification refers to the classification of the World Bank. There have been no multiple coups in Europe. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.20: COUPS D'ÉTAT AND ECONOMIC GROWTH—REGIONAL DIFFERENCES, FULL DYNAMIC PANEL DATA MODEL

Logarithm of GDP per capita	(1) Africa	(2) Africa	(3) Africa	(4) America	(5) America	(6) America	(7) Asia-Oceania	(8) Asia-Oceania	(9) Asia-Oceania	(10) Europe
$Coup_{it}$	-0.023**	-0.025**		-0.042***	-0.043***		-0.027	-0.030		-0.046**
	(0.010)	(0.011)		(0.008)	(0.009)		(0.018)	(0.018)		(0.021)
Single $coup_{it}$			-0.025**			-0.043***			-0.030	
			(0.011)			(0.009)			(0.018)	
Multiple $coups_{it}$		0.020	-0.005		0.002	-0.041**		0.094***	0.064**	
		(0.022)	(0.020)		(0.021)	(0.019)		(0.030)	(0.029)	
Log(GDP^{pc}) (t – 1)	1.012***	1.012***	1.012***	0.615***	0.615***	0.615***	1.052***	1.052***	1.052***	0.821***
	(0.033)	(0.033)	(0.033)	(0.075)	(0.075)	(0.075)	(0.044)	(0.044)	(0.044)	(0.133)
Log(GDP^{pc}) (t – 2)	-0.001	-0.002	-0.002	0.215**	0.215**	0.215**	-0.015	-0.014	-0.014	0.084
	(0.040)	(0.040)	(0.040)	(0.091)	(0.091)	(0.091)	(0.035)	(0.035)	(0.035)	(0.068)
Log(GDP^{pc}) (t – 3)	0.004	0.005	0.005	0.042	0.042	0.042	-0.037	-0.036	-0.036	0.145**
	(0.038)	(0.038)	(0.038)	(0.082)	(0.082)	(0.082)	(0.037)	(0.037)	(0.037)	(0.070)
Log(GDP^{pc}) (t – 4)	-0.054**	-0.054**	-0.054**	0.102	0.102	0.102	-0.059**	-0.059**	-0.059**	-0.153***
	(0.026)	(0.026)	(0.026)	(0.075)	(0.075)	(0.075)	(0.023)	(0.023)	(0.023)	(0.021)
Country Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	2,760	2,760	2,760	2,188	2,188	2,188	2,356	2,356	2,356	1,865
Countries	50	50	50	41	41	41	49	49	49	40
R^2 Overall	0.988	0.988	0.988	0.972	0.972	0.972	0.994	0.994	0.994	0.984
R^2 Within	0.954	0.954	0.954	0.880	0.880	0.880	0.978	0.978	0.978	0.979
R^2 Between	0.999	0.999	0.999	0.999	0.999	0.999	0.999	0.999	0.999	0.997

Notes: The table reports the results of dynamic panel data estimations on the effect of coups d'état on economic growth (equation 2). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. Per capita GDP growth is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The variable "Coup" denotes whether a coup has taken place at a given year, "Single Coup" only considers country-year observations with one coup in a given year, and "Multiple Coups" considers country-year observations with multiple coups in a given year. The regional classification refers to the classification of the World Bank. There have been no multiple coups in Europe. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.21: COUPS D'ÉTAT AND ECONOMIC GROWTH—COUP CHARACTERISTICS AND BIOGRAPHIC INFORMATION OF THE COUP LEADER, PANEL DIFFERENCE-IN-DIFFERENCES MODEL

GDP per capita growth	(1) Type of coup	(2) Type of coup	(3) Type of coup	(4) Age of coup leader	(5) Civil rank of coup leader	(6) Military rank of coup leader
Civilian $coup_{it}$	-0.186	0.001				
	(0.139)	(0.010)				
Military $coup_{it}$	-0.187		-0.001			
	(0.138)		(0.010)			
Royal $coup_{it}$		0.187	0.186			
		(0.138)	(0.139)			
Age of coup $leader_{it}$				-0.000		
				(0.000)		
Civil rank $index_{it}$					-0.006	
					(0.007)	
Military rank $index_{it}$						0.000
						(0.001)
Country Fixed Effects	yes	yes	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes	yes	yes
Observations	397	397	397	397	397	397
Countries	101	101	101	101	101	101
R^2	0.538	0.538	0.538	0.530	0.531	0.529

Notes: The table reports the results of panel difference-in-differences estimations on the effect of coups d'état on economic growth (equation 1). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. Per capita GDP growth is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The sample is restricted to country-year observations in which a coup has taken place. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.22: COUPS D'ÉTAT AND ECONOMIC GROWTH—COUP CHARACTERISTICS AND BIOGRAPHIC INFORMATION OF THE COUP LEADER, FULL DYNAMIC PANEL DATA MODEL

Logarithm of GDP per capita	(1) Type of coup	(2) Type of coup	(3) Type of coup	(4) Age of coup leader	(5) Civil rank of coup leader	(6) Military rank of coup leader
Civilian $coup_{it}$	-0.129	0.014				
	(0.128)	(0.015)				
Military $coup_{it}$	-0.143		-0.014			
	(0.123)		(0.015)			
Royal $coup_{it}$		0.143	0.129			
		(0.123)	(0.128)			
Age of coup $leader_{it}$				0.000		
				(0.000)		
Civil rank $index_{it}$					-0.002	
					(0.008)	
Military rank $index_{it}$						-0.000
						(0.002)
$Log(GDP^{pc})$ (t – 1)	0.945***	0.945***	0.945***	0.937***	0.936***	0.937***
	(0.092)	(0.092)	(0.092)	(0.092)	(0.091)	(0.092)
$Log(GDP^{pc})$ (t – 2)	-0.163	-0.163	-0.163	-0.166	-0.163	-0.163
	(0.161)	(0.161)	(0.161)	(0.161)	(0.160)	(0.164)
$Log(GDP^{pc})$ (t – 3)	0.164*	0.164*	0.164*	0.167*	0.167*	0.167*
	(0.098)	(0.098)	(0.098)	(0.097)	(0.096)	(0.096)
$Log(GDP^{pc})$ (t – 4)	-0.003	-0.003	-0.003	0.006	0.005	0.004
	(0.072)	(0.072)	(0.072)	(0.077)	(0.079)	(0.079)
Country Fixed Effects	yes	yes	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes	yes	yes
Observations	370	370	370	370	370	370
Countries	96	96	96	96	96	96
R^2 Overall	0.980	0.980	0.980	0.980	0.980	0.980
R^2 Within	0.946	0.946	0.946	0.946	0.946	0.946
R^2 Between	0.979	0.979	0.979	0.979	0.979	0.979

Notes: The table reports the results of dynamic panel data estimations on the effect of coups d'état on economic growth (equation 2). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. Per capita GDP growth is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The sample is restricted to country-year observations in which a coup has taken place. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.23: COUPS D'ÉTAT AND ECONOMIC GROWTH—ALTERNATIVE ESTIMATION STRATEGIES

Growth rate and logarithm of GDP per capita	GDP in 1960 Quintiles × Year Effects		Alternative Lag Structure			Difference GMM	Han and Phillips Estimator	Nonparametric Estimations
	(1) *Panel DiD*	(2) *Dynamic Panel*	(3) *One Lag*	(4) *Two Lags*	(5) *Three Lags*	(6) *Dynamic Panel*	(7) *Dynamic Panel*	(8)
$Coup_{it}$	-0.021***	-0.026***	-0.030***	-0.032***	-0.030***	-0.028***	-0.020***	-0.023***
	(0.004)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.005)	(0.006)
$Log(GDP^{pc})$ (t – 1)		1.034***	0.953***	0.839***	0.840***	0.805***	0.869***	
		(0.025)	(0.006)	(0.071)	(0.073)	(0.076)	(0.167)	
$Log(GDP^{pc})$ (t – 2)		-0.014		0.117	0.141**	0.152**		
		(0.027)		(0.117)	(0.063)	(0.064)		
$Log(GDP^{pc})$ (t – 3)		-0.006			-0.024	0.005		
		(0.028)			(0.049)	(0.046)		
$Log(GDP^{pc})$ (t – 4)		0.060***				-0.025		
		(0.017)				(0.044)		
Country Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes
GDP (1960) × Year effects	yes	yes	no	no	no	no	no	no
Observations	6,415	6,178	9,709	9,529	9,349	8,989	9,709	9,889
Countries	144	144	180	180	180	180	180	180
R^2 Overall	0.175	0.994	0.990	0.990	0.990		0.999	
R^2 Within		0.960	0.951	0.951	0.950			
R^2 Between		0.999	0.999	0.999	0.999			

Notes: The table reports the results on the effect of coups d'état on economic growth estimated with alternative estimation strategies. Columns (1)-(2) interact the quintile of the distribution of per capita GDP in 1960 with year effects to account for differences in the growth path of GDP. Columns (3)-(5) use alternative specifications to model GDP dynamics in the full dynamic panel data model. Robust standard errors (adjusted for clustering by countries) are reported in parentheses. Columns (6)-(8) employ alternative estimating strategies, including difference GMM (column 6), the Han and Phillips estimator (Han and Phillips 2010, column 7), and nonparametric kernel regressions with Li-Racine kernel and bootstrapped standard errors (column 8). The reported effects in column (8) are averages of contrasts of factor covariates. Per capita GDP growth is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The variable "Coup" denotes whether a coup has taken place at a given year. Standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.24: COUPS D'ÉTAT AND ECONOMIC GROWTH—CONTROLLING FOR TRANSITIONS INTO AUTOCRACY, PANEL DIFFERENCE-IN-DIFFERENCES MODEL

GDP per capita growth	All observations (incl. Transitions)			Transitions excluded
	(1)	(2)	(3)	(4)
Transition to autocracy (after coup)	-0.020***	-0.000	0.001	
	(0.007)	(0.008)	(0.008)	
$Coup_{it}$		-0.021***	-0.021***	-0.022***
		(0.004)	(0.004)	(0.004)
$Democracy_{it}$			0.003	
			(0.003)	
Country Fixed Effects	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes
Observations	9,709	9,709	9,709	9,665
Countries	180	180	180	180
R^2	0.105	0.109	0.110	0.109

Notes: The table reports the results of panel difference-in-differences estimations on the effect of coups d'état on economic growth (equation 1). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. Per capita GDP growth is measured in 2011 US dollars, data on coups d'état and democracy is from Bjørnskov and Rode (2019). The variable "Coup" denotes whether a coup has taken place at a given year. The variable "Transition to Autocracy (after Coup)" is a dummy variable that is 1 if a democratic country becomes autocratic after a coup d'état and 0 otherwise. Column "Transition excluded" excludes country-year observations in which coups led to a transition to autocracy. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.25: COUPS D'ÉTAT AND ECONOMIC GROWTH—CONTROLLING FOR TRANSITIONS INTO AUTOCRACY, FULL DYNAMIC PANEL DATA MODEL

Logarithm of GDP per capita	All observations (incl. Transitions)			Transitions excluded
	(1)	(2)	(3)	(4)
Transition to autocracy (after coup)	-0.028**	-0.001	0.002	
	(0.011)	(0.012)	(0.012)	
$Coup_{it}$		-0.030***	-0.029***	-0.030***
		(0.007)	(0.007)	(0.008)
$Democracy_{it}$			0.008	
			(0.006)	
$Log(GDP^{pc})$ (t – 1)	0.829***	0.827***	0.827***	0.827***
	(0.072)	(0.072)	(0.072)	(0.072)
$Log(GDP^{pc})$ (t – 2)	0.155**	0.155**	0.155**	0.155**
	(0.066)	(0.066)	(0.066)	(0.066)
$Log(GDP^{pc})$ (t – 3)	0.002	0.002	0.002	0.002
	(0.047)	(0.047)	(0.047)	(0.047)
$Log(GDP^{pc})$ (t – 4)	-0.030	-0.029	-0.029	-0.029
	(0.045)	(0.044)	(0.044)	(0.045)
Country Fixed Effects	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes
Observations	9,169	9,169	9,169	9,126
Countries	180	180	180	180
R^2 Overall	0.990	0.990	0.990	0.990
R^2 Within	0.949	0.949	0.949	0.948
R^2 Between	0.999	0.999	0.999	0.999

Notes: The table reports the results of dynamic panel data estimations on the effect of coups d'état on economic growth (equation 2). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. Per capita GDP growth is measured in 2011 US dollars, data on coups d'état and democracy is from Bjørnskov and Rode (2019). The variable "Coup" denotes whether a coup has taken place at a given year. The variable "Transition to Autocracy (after Coup)" is a dummy variable that is 1 if a democratic country becomes autocratic after a coup d'état and 0 otherwise. Column "Transition excluded" excludes country-year observations in which coups led to a transition to autocracy. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.26: COUPS D'ÉTAT AND ECONOMIC GROWTH—CONTROLLING FOR TRANSITIONS INTO DEMOCRACY, PANEL DIFFERENCE-IN-DIFFERENCES MODEL

GDP per capita growth	All observations (incl. Transitions)			Transitions excluded
	(1)	(2)	(3)	(4)
Transition to democracy (after coup)	-0.026*	-0.006	-0.008	
	(0.016)	(0.016)	(0.015)	
$Coup_{it}$		-0.021***	-0.021***	-0.021***
		(0.004)	(0.004)	(0.004)
$Democracy_{it}$			0.003	
			(0.003)	
Country Fixed Effects	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes
Observations	9,709	9,709	9,709	9,702
Countries	180	180	180	180
R^2	0.105	0.109	0.110	0.109

Notes: The table reports the results of panel difference-in-differences estimations on the effect of coups d'état on economic growth (equation 1). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. Per capita GDP growth is measured in 2011 US dollars, data on coups d'état and democracy is from Bjørnskov and Rode (2019). The variable "Coup" denotes whether a coup has taken place at a given year. The variable "Transition to Democracy (after Coup)" is a dummy variable that is 1 if an autocratic country becomes democratic after a coup d'état and 0 otherwise. Column "Transition excluded" excludes country-year observations in which coups led to a transition to democracy. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.27: COUPS D'ÉTAT AND ECONOMIC GROWTH—CONTROLLING FOR TRANSITIONS INTO DEMOCRACY, FULL DYNAMIC PANEL DATA MODEL

Logarithm of GDP per capita	All observations (incl. Transitions)			Transitions excluded
	(1)	(2)	(3)	(4)
Transition to democracy (after coup)	-0.010	0.018	0.013	
	(0.017)	(0.018)	(0.018)	
$Coup_{it}$		-0.030***	-0.029***	-0.030***
		(0.007)	(0.007)	(0.007)
$Democracy_{it}$			0.008	
			(0.005)	
$Log(GDP^{pc})$ (t – 1)	0.829***	0.827***	0.827***	0.827***
	(0.072)	(0.072)	(0.072)	(0.072)
$Log(GDP^{pc})$ (t – 2)	0.155**	0.155**	0.155**	0.155**
	(0.066)	(0.066)	(0.066)	(0.066)
$Log(GDP^{pc})$ (t – 3)	0.002	0.002	0.002	0.002
	(0.047)	(0.047)	(0.047)	(0.047)
$Log(GDP^{pc})$ (t – 4)	-0.030	-0.029	-0.029	-0.029
	(0.045)	(0.044)	(0.044)	(0.044)
Country Fixed Effects	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes
Observations	9,169	9,169	9,169	9,162
Countries	180	180	180	180
R^2 Overall	0.990	0.990	0.990	0.990
R^2 Within	0.948	0.949	0.949	0.949
R^2 Between	0.999	0.999	0.999	0.999

Notes: The table reports the results of dynamic panel data estimations on the effect of coups d'état on economic growth (equation 2). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. Per capita GDP growth is measured in 2011 US dollars, data on coups d'état and democracy is from Bjørnskov and Rode (2019). The variable "Coup" denotes whether a coup has taken place at a given year. The variable "Transition to Democracy (after Coup)" is a dummy variable that is 1 if an autocratic country becomes democratic after a coup d'état and 0 otherwise. Column "Transition excluded" excludes country-year observations in which coups led to a transition to democracy. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.28: COUPS D'ÉTAT AND ECONOMIC GROWTH—CONTROLLING FOR GOVERNMENT CHANGE, PANEL DIFFERENCE-IN-DIFFERENCES MODEL

GDP per capita growth	All observations (incl. Government Changes)			Gov. changes excluded
	(1)	(2)	(3)	(4)
Government change$_{it}$ × coup$_{it}$	-0.043***	-0.025*	-0.013	
	(0.012)	(0.013)	(0.014)	
Coup$_{it}$		-0.019***	-0.019***	-0.014***
		(0.006)	(0.006)	(0.004)
Government change$_{it}$			-0.015***	
			(0.003)	
Country Fixed Effects	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes
Observations	7,295	7,295	7,295	6,318
Countries	180	180	180	180
R^2	0.125	0.127	0.133	0.133

Notes: The table reports the results of panel difference-in-differences estimations on the effect of coups d'état on economic growth (equation 1). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. Per capita GDP growth is measured in 2011 US dollars, data on coups d'état and democracy is from Bjørnskov and Rode (2019). The variable "Coup" denotes whether a coup has taken place at a given year. The variable "Government Change" is a dummy variable that is 1 if there was a (regular or irregular) change in government in a particular year, and 0 otherwise. Column "Gov. Changes excluded" excludes country-year observations in which a government change (regular or irregular) took place. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.29: COUPS D'ÉTAT AND ECONOMIC GROWTH—CONTROLLING FOR GOVERNMENT CHANGE, FULL DYNAMIC PANEL DATA MODEL

Logarithm of GDP per capita	All observations (incl. Government Changes)			Gov. changes excluded
	(1)	(2)	(3)	(4)
Government change$_{it}$ × coup$_{it}$	-0.046***	-0.019	-0.007	
	(0.017)	(0.019)	(0.019)	
Coup$_{it}$		-0.029***	-0.029***	-0.026**
		(0.011)	(0.011)	(0.011)
Government change$_{it}$			-0.014***	
			(0.004)	
Log(GDP^{pc}) (t – 1)	0.823***	0.822***	0.821***	0.805***
	(0.077)	(0.076)	(0.076)	(0.076)
Log(GDP^{pc}) (t – 2)	0.159**	0.159**	0.159**	0.170**
	(0.072)	(0.072)	(0.072)	(0.073)
Log(GDP^{pc}) (t – 3)	-0.020	-0.020	-0.020	-0.015
	(0.052)	(0.052)	(0.052)	(0.053)
Log(GDP^{pc}) (t – 4)	-0.024	-0.023	-0.023	-0.020
	(0.048)	(0.048)	(0.048)	(0.051)
Country Fixed Effects	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes
Observations	7,217	7,217	7,217	6,263
Countries	180	180	180	180
R^2 Overall	0.990	0.990	0.990	0.989
R^2 Within	0.918	0.919	0.919	0.915
R^2 Between	0.999	0.999	0.999	0.999

Notes: The table reports the results of dynamic panel data estimations on the effect of coups d'état on economic growth (equation 2). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. Per capita GDP growth is measured in 2011 US dollars, data on coups d'état and democracy is from Bjørnskov and Rode (2019). The variable "Coup" denotes whether a coup has taken place at a given year. The variable "Government Change" is a dummy variable that is 1 if there was a (regular or irregular) change in government in a particular year, and 0 otherwise. Column "Gov. Changes excluded" excludes country-year observations in which a government change (regular or irregular) took place. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.30: COUPS D'ÉTAT AND ECONOMIC GROWTH—PRE-COUP DYNAMICS IN POLITICAL INSTITUTIONS, PANEL DIFFERENCE-IN-DIFFERENCES MODEL

GDP per capita growth	(1)	(2)	(3)	(4)	(5)	(6)
$Coup_{it}$	-0.026***	-0.022***	-0.025***	-0.021***	-0.021***	
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	
Single $coup_{it}$						-0.021***
						(0.004)
Multiple $coups_{it}$					-0.001	-0.022
					(0.016)	(0.016)
Political Institutions (t – 1)	-0.008	-0.009	-0.004	-0.004	-0.004	-0.004
	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)
Political Institutions (t – 2)	0.014	0.013	0.012	0.010	0.010	0.010
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Political Institutions (t – 3)	-0.003	-0.004	-0.004	-0.004	-0.004	-0.004
	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)
Political Institutions (t – 4)	0.006	0.007	0.002	0.003	0.003	0.003
	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)
Country Fixed Effects	no	yes	no	yes	yes	yes
Year Fixed Effects	no	no	yes	yes	yes	yes
Observations	7,784	7,784	7,784	7,784	7,784	7,784
Countries	170	170	170	170	170	170
R^2	0.012	0.078	0.056	0.122	0.122	0.122

Notes: The table reports the results of panel difference-in-differences estimations on the effect of coups d'état on economic growth (equation 1). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. Per capita GDP growth is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The variable "Coup" denotes whether a coup has taken place at a given year, "Single Coup" only considers country-year observations with one coup in a given year, and "Multiple Coups" considers country-year observations with multiple coups in a given year. The quality of political institutions is measured based on the continuous democracy indicator compiled by Gründler and Krieger (2016, 2018, 2019). *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.31: COUPS D'ÉTAT AND ECONOMIC GROWTH—PRE-COUP DYNAMICS IN POLITICAL INSTITUTIONS, FULL DYNAMIC PANEL DATA MODEL

Logarithm of GDP per capita	(1)	(2)	(3)	(4)	(5)	(6)
$Coup_{it}$	-0.035***	-0.031***	-0.032***	-0.027***	-0.028***	
	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)	
Single $coup_{it}$						-0.028***
						(0.008)
Multiple $coups_{it}$					0.021	-0.008
					(0.015)	(0.013)
Political Institutions (t – 1)	0.000	0.004	0.005	-0.005	-0.005	-0.005
	(0.012)	(0.011)	(0.013)	(0.011)	(0.011)	(0.011)
Political Institutions (t – 2)	0.006	0.005	0.001	-0.001	-0.001	-0.001
	(0.013)	(0.012)	(0.013)	(0.012)	(0.012)	(0.012)
Political Institutions (t – 3)	-0.005	-0.005	-0.007	-0.009	-0.009	-0.009
	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)
Political Institutions (t – 4)	0.017	0.025**	0.013	0.008	0.008	0.008
	(0.011)	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)
$Log(GDP^{pc})$ (t – 1)	1.057***	1.015***	1.043***	0.984***	0.984***	0.984***
	(0.041)	(0.043)	(0.041)	(0.044)	(0.044)	(0.044)
$Log(GDP^{pc})$ (t – 2)	-0.003	0.001	0.014	0.015	0.015	0.015
	(0.034)	(0.032)	(0.033)	(0.030)	(0.030)	(0.030)
$Log(GDP^{pc})$ (t – 3)	0.019	0.022	0.010	0.011	0.012	0.012
	(0.022)	(0.022)	(0.024)	(0.023)	(0.023)	(0.023)
$Log(GDP^{pc})$ (t – 4)	-0.080***	-0.077***	-0.072***	-0.068***	-0.068***	-0.068***
	(0.014)	(0.015)	(0.014)	(0.014)	(0.014)	(0.014)
Country Fixed Effects	no	yes	no	yes	yes	yes
Year Fixed Effects	no	no	yes	yes	yes	yes
Observations	7,784	7,784	7,784	7,784	7,784	7,784
Countries	170	170	170	170	170	170
R^2 Overall	0.994	0.994	0.994	0.994	0.994	0.994
R^2 Within	0.957	0.958	0.960	0.961	0.961	0.961
R^2 Between	0.999	0.999	0.999	0.999	0.999	0.999

Notes: The table reports the results of dynamic panel data estimations on the effect of coups d'état on economic growth (equation 2). Robust standard errors (adjusted for clustering by countries) are reported in parentheses. Per capita GDP growth is measured in 2011 US dollars, data on coups d'état is from Bjørnskov and Rode (2019). The variable "Coup" denotes whether a coup has taken place at a given year, "Single Coup" only considers country-year observations with one coup in a given year, and "Multiple Coups" considers country-year observations with multiple coups in a given year. The quality of political institutions is measured based on the continuous democracy indicator compiled by Gründler and Krieger (2016, 2018, 2019). *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.32: COUPS D'ÉTAT AND ECONOMIC GROWTH—RESULTS ON THE SUB-NATIONAL LEVEL, ACCOUNTING FOR SUB-NATIONAL CONFLICT

Growth rate and logarithm of GDP per capita	Panel Diff-in-Diff Model		Dynamic Panel Data Model	
	(1) *All regions*	(2) *Coup regions excluded*	(3) *All regions*	(4) *Coup regions excluded*
$Coup_{irt}$	-0.018***		-0.015***	
	(0.006)		(0.008)	
$Coup^{c}_{irt,r\neq\tilde{r}}$		-0.022***		-0.019***
		(0.006)		(0.007)
$Conflict_{irt}$	-0.008**	-0.008**	-0.006*	-0.006*
	(0.003)	(0.003)	(0.003)	(0.003)
$Log(GRP^{pc})$ (t – 1)			0.815***	0.814***
			(0.075)	(0.076)
$Log(GRP^{pc})$ (t – 2)			0.168**	0.169**
			(0.073)	(0.073)
$Log(GRP^{pc})$ (t – 3)			-0.043	-0.042
			(0.045)	(0.045)
$Log(GRP^{pc})$ (t – 4)			-0.065	-0.065
			(0.040)	(0.040)
Sub-National Unit Fixed Effects	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes
Observations	51,727	51,655	43,707	43,650
Sub-National Units	2,660	2,660	2,659	2,659
R^2 Overall	0.300	0.301	0.935	0.935
F-Statistic	83.23	79.77	810.2	811.5

Notes: The table reports the results of panel difference-in-differences models (columns 1-2) and dynamic panel data estimations (columns 3-4) on the effect of coups d'état on economic growth at the sub-national level. Robust standard errors (adjusted for clustering by countries) are reported in parentheses. The log of per capita GRP is measured in real terms, data on coups d'état is geocoded by sub-national units using the coups listed in Bjørnskov and Rode (2019). Due to restrictions availability of sub-national GRP estimates, the models include the period 1992-2012. Sub-national regions are first-level administrative areas (ADM1). The dummy variable for conflict is constructed using the UCDP Georeferenced Event Dataset of Sundberg and Melander (2013). *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.33: COUPS D'ÉTAT AND ECONOMIC GROWTH—RESULTS ON THE SUB-NATIONAL LEVEL, ACCOUNTING FOR SUB-NATIONAL HUMAN CAPITAL

Growth rate and logarithm of GDP per capita	Panel Diff-in-Diff Model		Dynamic Panel Data Model	
	(1) *All regions*	(2) *Coup regions excluded*	(3) *All regions*	(4) *Coup regions excluded*
$Coup_{irt}$	-0.022***		-0.022***	
	(0.006)		(0.006)	
$Coup^{c}_{irt,r\neq\tilde{r}}$		-0.019***		-0.020***
		(0.006)		(0.006)
$Human\ capital_{irt}$	0.036	0.036	0.028	0.028
	(0.043)	(0.043)	(0.024)	(0.024)
$Log(GRP^{pc})$ (t – 1)			0.896***	0.895***
			(0.041)	(0.041)
$Log(GRP^{pc})$ (t – 2)			0.112**	0.112**
			(0.052)	(0.052)
$Log(GRP^{pc})$ (t – 3)			-0.073	-0.072
			(0.051)	(0.051)
$Log(GRP^{pc})$ (t – 4)			-0.045	-0.045
			(0.042)	(0.042)
Sub-National Unit Fixed Effects	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes
Observations	45,646	45,594	38,660	38,619
Sub-National Units	2,315	2,315	2,315	2,315
R^2 Overall	0.342	0.343	0.948	0.948
F-Statistic	96.83	91.80	1244.8	1306.5

Notes: The table reports the results of panel difference-in-differences models (columns 1-2) and dynamic panel data estimations (columns 3-4) on the effect of coups d'état on economic growth at the sub-national level. Robust standard errors (adjusted for clustering by countries) are reported in parentheses. The log of per capita GRP is measured in real terms, data on coups d'état is geocoded by sub-national units using the coups listed in Bjørnskov and Rode (2019). Due to restrictions availability of sub-national GRP estimates, the models include the period 1992-2012. Sub-national regions are first-level administrative areas (ADM1). Human capital is constructed using the UCDP Georeferenced Event Dataset of Sundberg and Melander (2013). *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

TABLE A5.34: EFFECTS OF COUPS D'ÉTAT ON THE HOUSEHOLD-LEVEL—THE INFLUENCE ON PERCEPTIONS AND PREFERENCES

	Expectations: Future will be bleak		Confidence in government		Attitudes towards democracy	
	(1) *Reduced*	(2) *Controls*	(3) *Reduced*	(4) *Controls*	(5) *Reduced*	(6) *Controls*
$Coup_{it}$	0.446***	0.447***	-0.055***	-0.052***	-0.134**	-0.111**
	(0.027)	(0.027)	(0.014)	(0.014)	(0.053)	(0.053)
$Income\ decile_{ith}$		-0.001		0.003***		0.012***
		(0.001)		(0.001)		(0.003)
Age_{ith}		-0.001		-0.004***		0.013***
		(0.001)		(0.001)		(0.002)
$Age\ squared_{ith}$		0.000		0.000***		-0.000***
		(0.000)		(0.000)		(0.000)
$Student_{ith}$		-0.031***		0.004		0.079***
		(0.010)		(0.008)		(0.024)
$Retired_{ith}$		0.035***		-0.021***		-0.049**
		(0.010)		(0.008)		(0.022)
$Education_{ith}$		0.003***		-0.018***		0.089***
		(0.001)		(0.001)		(0.003)
$Unemployed_{ith}$		0.013		-0.054***		-0.053**
		(0.009)		(0.007)		(0.021)
Country Fixed Effects	yes	yes	yes	yes	yes	yes
Wave Fixed Effects	yes	yes	yes	yes	yes	yes
Households	41,390	41,390	226,995	226,995	131,820	131,820
Countries	45	85	85	85	79	79
R^2	0.172	0.173	0.130	0.135	0.075	0.085
F Statistic	303.3	257.3	532.1	515.1	149.7	161.2

Notes: The table reports estimations on the effect of coup d'états on the household level, with robust standard errors (adjusted for heteroskedasticity) in parentheses. Household-level data is taken from the World Value Survey (WVS). We include all observations for which data on coups and data on household characteristics are available. Our combined dataset covers a maximum of 254,079 households in 85 countries observed between 1981 and 2016. Expectations about the future are taken from question V50, where individuals are asked *"For each of the following pairs of statements, please tell me which one comes closest to your own views: Humanity has a bright future versus humanity has a bleak future"*. We construct a dummy that assumes a value of 1 if individuals answer that the future will be bleak, and zero otherwise. Confidence in government is measured using question V115 (V138, V153, V142, and V289 in earlier waves). The question asks people *"I am going to name a number of organizations. For each one, could you tell me how much confidence you have in them: is it a great deal of confidence, quite a lot of confidence, not very much confidence or none at all?"*. We re-code the variable so that higher values reflect greater confidence in government. Attitudes towards democracy are measured on a ten-scale ladder running from 1 to 10. Information stems from question V140 (V162 in earlier waves) where respondents are asked *"How important is it for you to live in a country that is governed democratically? On this scale where 1 means it is 'not at all important' and 10 means 'absolutely important' what position would you choose?"*. *, **, and *** indicate significance at the 10, 5, and 1% significance level, respectively.

6. Conclusion

Introduction and conclusion to each chapter have extensively framed the individual research topics by motivating the research question and by providing an overview on content and results. This concluding chapter therefore briefly reviews and highlights how this dissertation contributes to the literature on defense economics, economic growth and the role of political institutions as well as which implications for politics and public debate can be drawn.

Chapter 2 contributes to the scarce literature on compliance with international agreements. The chapter is the first to examine government changes as one potential reason for non-compliance by using the NATO two percent target. The credibility problem when national governments commit themselves to international agreements may well apply to other policy fields and may have implications for the design of future treaties or agreements in terms of incentive compatibility and enforcement. **Chapter 3** builds on the large existing literature on the determinants of national defense spending. The chapter is the first to cover the entire third wave of democracy with new SIPRI data on defense spending as well as uses state-of-the-art estimation techniques. Both chapters estimated demand functions for national defense spending, which contributes to scholars' knowledge about government spending and the allocation of defense capabilities. Specifically, national defense spending reflects the outcome of a public choice which is channeled via political institutions and the decision-making of governments. The first two chapters thus showed the challenges and restrictions democracies face when political turnarounds are required. The quantitative analysis of national defense is of particular interest after the bipolar world with two opposed blocks has been replaced by multifaceted security matters, e.g. through international terrorism, the rise of China as both an economic and military power or new tensions between Russia and the Western world which remind of the Cold War.

While **Chapter 2** and **Chapter 3** explained the spending of public funds, which is primarily a result of political decision-making, **Chapter 4** and **Chapter 5** explained output in goods and services, which is the result of a value-creation process. **Chapter 4** estimated firm-level output in arms and used defense spending, i.e. the demand for security from external threats, as an explanatory variable. **Chapter 5** estimated the output of an economy and examined the effect of coups d'état, i.e. one aspect of internal insecurity and political instability, on economic growth. All four chapters, thus, dealt with questions of security and stability from different perspectives.

Chapter 4 is the first empirical work relating supply and demand for military goods by means of arms production, national defense spending and arms trade. The chapter showed how the demand for defense spending by the national government and foreign governments has shaped a country's defense industry structure and contributes to the debate on burden sharing and armament on a more abstracted level than the simple view on defense spending levels can provide. **Chapter 5** showed that coups d'état have drastic consequences for economic growth. The chapter contributes to the large literature on economic growth and fills a gap which in this level of detail has previously been left blank in the literature. It furthermore anticipates consequences from increasing political instability in the Western world, as we observe the rise of populism and signs of decay of international institutions (e.g. the Brexit) since the late 2010s.

This dissertation primarily contributes to the literature on defense economics, economic growth, and the role of political institutions. It thereby also contributes to the literature in related fields such as compliance with international agreements, free-riding, or government spending. For several reasons, the prospects for research in these fields are promising: first, defense economics will be of increasing interest in times of rising worldwide defense budgets and new international powers like China, which strive to extend their geopolitical influence. Second, understanding economic growth will with high probability remain an evergreen in economic research. Third, the role of political institutions will be of further interest in times of political

turbulences in the Western world and the declining victory march of democracy worldwide. All of these developments add further complexity to the topics discussed in this dissertation and serve as fruitful starting points for future research.