Do Direct Elections Matter? Quasi-experimental Evidence from Germany

Stefanie Gaebler, Felix Roesel

Do Direct Elections Matter?
Quasi-experimental Evidence from Germany*

Abstract

We estimate the causal effect of direct elections on the economic performance of politicians. Candidates running in direct elections to head local governments in the German state of Brandenburg need an absolute majority, and votes for the winner must represent at least 15% of eligible voters. If the quorum is not reached, direct elections are suspended, and local councils appoint the head of government. We examine election outcomes around the quorum, where the form of government is arguably exogenous. Event study results show that the public employment service becomes somewhat more effective under directly elected politicians. However, directly elected politicians do not seem to attract more businesses or expedite administrative acts.

Stefanie Gaebler**	Felix Roesel
ifo Institute – Leibniz Institute for Economic Research at the University of Munich, University of Munich Poschingerstr. 5 81679 Munich, Germany Phone: + 49 89 9224 1381 gaebler@ifo.de	ifo Institute – Leibniz Institute for Economic Research at the University of Munich Dresden Branch, Dresden University of Technology Einsteinstr. 3 01069 Dresden, Germany Phone: + 49 351 26476 28 roesel@ifo.de

1 Introduction

There is an ongoing debate whether directly elected politicians perform differently than politicians appointed by parliaments (Persson and Tabellini, 2003). From a pure median voter perspective, the form of government should not matter at all. If a parliament mirrors voters' preferences, appointed and directly elected politicians should equally represent the median voter. In reality, however, the form of government is a topic of interest and subject to strategic actions (Robinson and Torvik, 2016). Violent protests followed the Turkish constitutional referendum in 2017, when a close majority of 51.4% voted for a new constitution that included a directly elected president. Discussions also apply to the local level, where the mayor-council system competes with the council-manager system. There is still no dominant form of government. In 2014, approximately 50% of US municipalities reported a council-manager system; 40% had direct elections (mayor-council system).[1]

Empirical studies indicate that the form of government influences public finances.[2] Taxes tend to be lower if politicians are directly elected, and transfers from higher levels of government increase (Ade, 2014; Hessami, 2018). On the expenditure side, evidence is mixed. Studies have found that expenditures increase (Saha, 2011; Ade, 2014; Koethenbuerger et al., 2014; Garmann, 2015), decrease (Baqir, 2002; Coate and Knight, 2011; Lewis, 2018) or do not change (MacDonald, 2008) when politicians are directly elected. Koeppl-Turyna (2016) stratifies categories of expenditures and finds that directly elected politicians spend less on public administration and staff but more on infrastructure, which she assumes is more visible to voters. Enikolopov (2014), by contrast, shows that the number of public employees is higher if chief executives are directly elected.[3]

In this paper, we estimate the causal effect of direct elections on the administrative and economic performance of politicians beyond fiscal policy. A quorum applies to local elections in the German state of Brandenburg and determines the form of government. Like the USA, Brandenburg has two layers of local government: municipalities and counties. At both levels, candidates running to head local governments need an ab-

---

[1]The remaining 10% report mixed or other forms of government. See the ICMA Form of Government Statistics – Municipalities (2014), April 02, 2018.

[2]For an overview of countries, identification strategy, and results, see Table 7 in the Online Appendix.

[3]There are further papers showing that election rules matter to economic outcomes. See, for example, Ferraresi et al. (2015).

solute majority of votes in direct elections, and votes for the winner must also represent at least 15% of eligible voters. If the quorum is not reached, direct elections are suspended, and local councils appoint the head of government. All other rules and institutions are equal. At the municipality level, candidates always easily exceed the quorum. Elections at the county level, by contrast, scatter around the 15% threshold because voter turnout is low. We examine close outcomes around the quorum in 14 county elections. In Brandenburg, the head of county administration (*Landrat*) is a powerful player who heads and organizes a county administration with an average of 850 employees. In direct elections for the head of county administration between 2010 and 2017, nine counties missed and six counties exceeded the 15% quorum, some by only a few votes. In those cases, the form of government is arguably as good as exogenous. We employ difference-in-differences, event study, and synthetic control methods based on high-frequency monthly county data. Our results suggest that the form of government matters to the performance of politicians, but details are important. The public employment service for the long-term unemployed (*Jobcenter*), which is jointly administered by the county government and the federal public employment agency, becomes more effective. We find that long-term unemployment decreases under directly elected politicians, indicating that they are interested in delivering visible policies. By contrast, we do not find that directly elected politicians attract more businesses or expedite administrative acts.

Our study adds three novel aspects to the literature. First, our setting rules out self-selection of institutions to a large extent. Usually, the form of government is likely to be endogenous to political outcomes and strategies (Robinson and Torvik, 2016). Therefore, self-selection into forms of government is an issue with many prior studies. Governments may change the form of government for strategic reasons; the Turkish referendum in 2017 is an excellent case in point. Many empirical studies have exploited settings where jurisdictions self-select the form of government, which may lead to biased estimates. Other studies use temporal differences resulting from a gradual fade-in of direct elections. Direct elections were gradually introduced because election terms expired at different points in time. Even in those cases, governments are able to manipulate the time schedule (e.g., by enforcing retirements). Additionally, the pool of candidates may change when direct elections apply. Furthermore, changes in the form of government often coincide with additional overlapping reforms, for example, the duration of election terms or conditions for dismissing officials. We are able to abstract from self-selection and overlapping effects by simultaneous reforms. Using marginally

met or missed quorums enables us to identify causal effects more properly. Moreover, in our setting, all other rules (responsibilities, role of the local council, election terms, dismissing rules, and so on) remain equal; only the election mode changes with the quorum.

Second, against the background of broad evidence on fiscal policy, little is known about whether the direct election of politicians influences public services and economic outcomes. This is particularly surprising because local administrations control public services *directly*, and business-friendly administrations can set the stage for economic growth. By contrast, decisions on public finance often require approval by councils. We investigate the effect of direct elections on economic and administrative outcomes, which are at the discretion of the head of government and do not require further council approvals. To account for changes in office during the year, we use high-frequency (monthly) data on unemployment, business registrations, and building permits at the county level.[4] We find effects in policy areas where local officials have some discretion. County governments and the federal public employment agency are jointly responsible for public employment services for the long-term unemployed in Germany. The short-term unemployed, by contrast, are served by the federal employment agency alone. Accordingly, we find that direct county elections have effects on long-term unemployment but no effects on short-term unemployment. The number of business registrations and building permits do not change under directly elected heads of county administrations.

Third, our setting addresses some theoretical channels about *why* the form of government should matter to government performance. In our setting, directly elected and appointed politicians are equally accountable (same suspension rules). In most cases, the county council also appointed the candidate who has won the direct election when the quorum was not reached. Selection issues or information asymmetries among voters should therefore play a minor role. Neither directly elected nor appointed heads of government can be sure they will meet the 15% quorum in the next election. We can therefore also abstract from re-election motives. Finally, we compare directly elected politicians to council-appointed politicians who won the direct election but failed the quorum. Inferences do not change. Ruling out plenty of other channels, one possible interpretation of our findings is that direct elections create a different psychological climate between voters and politicians. Laboratory experiments have shown that followers accept deviating behavior from elected leaders rather from appointed leaders,

---

[4]We can show that our results do not change when we use data on an annual basis.

and elected leaders behave more socially responsibly than appointed ones (De Cremer and Van Dijk, 2008).

The remainder of this paper is structured as follows: Section 2 outlines theoretical arguments about why the form of government may matter to policy outcomes. Section 3 introduces the institutional background of county elections and local administration in the German state of Brandenburg. Section 4 explains our identification strategy and presents the data. The results, including robustness tests, are shown in section 5, and section 6 concludes.

2   Theoretical considerations

In a simple election model, both parliament and the president should equally represent the median voter. In this case, it should not matter to governmental outcomes whether a politician is appointed by the parliament or elected separately. In reality, however, there are large and emotional debates about whether to directly elect the head of government or not. Theoretical studies model differences across forms of government along five main dimensions: accountability, re-election motives, information asymmetries, selection of politicians, and psychological factors regarding self-conscious leadership.

First, accountability differs among different forms of government. Voters can hold a parliament or a president accountable if there are separate elections. In parliamentary systems, voters have to punish the parliament even if they (only) disagree with the appointed head of government. Maskin and Tirole (2004) argue that the "most important decisions should be taken by elected rather than nonaccountable officials" (p. 1050) in order to maximize welfare. Second, re-election motives may play a role. Elected politicians may face stronger incentives to gratify voters *directly* in order to increase re-election probabilities. Appointed politicians, by contrast, may reward the appointing institution rather than rewarding voters. Catering to the universe of voters or only a few councilors may well implicate different strategies and policies for the head of government. Third, Coate and Knight (2011) model information asymmetries among voters, which translate into different outcomes of governmental forms. If voters have only partial information about the policy preferences of local politicians, the form of government may matter to spending levels. Coate and Knight (2011) expect

lower spending under direct elections.[5] Fourth, direct elections may change the pool of candidates. Politicians appointed by councils are often described as managers or bureaucrats, while directly elected politicians are more likely to be charismatic leaders. Variations in characteristics are likely to translate into differences in policies. Fifth, the selection process itself may matter to the *perceived* backing of politicians. Direct elections and council elections "create [a] different psychological climate between leaders and follower[s]" (Hollander, 1992, p. 48). Direct election campaigns focus on personal characteristics and leadership, while council elections are dominated by political and party issues. Directly elected politicians are often said to feel more self-conscious because they know that they are backed by a majority of voters casting their votes explicitly for her name on the ballot sheet and not for an anonymous party organization.

However, even if one accepts that the form of government creates different political environments, consequences for policy outcomes are far from obvious. On the one hand, directly elected leaders feel more responsive to the interests and needs of their followers. Therefore, they might be more courageous in making difficult decisions, for example, when it comes to cut expenditures, reduce deficits, and implement reforms. On the other hand, directly elected politicians feel "closer" to the voters. They might have a stronger sense of social responsibility because followers put higher expectations on them to serve their interests (Julian et al., 1969; Hollander and Julian, 1970; Ben-Yoav et al., 1983; Kenney et al., 1996; Grossman et al., 2014). This, in turn, may induce incentives to run popular but less-sustainable policies. Appointed leaders are different. Voters perceive that they have less legitimacy and followers place fewer expectations on them. Appointed leaders themselves are expected to have less interest in the needs of their followers (Hollander and Julian, 1970; Hollander, 1992; De Cremer and Van Vugt, 2002). In laboratory experiments, De Cremer and Van Dijk (2008) show that followers accept deviating behavior by appointed rather than by elected leaders and that elected leaders exhibited more socially responsible behavior than their appointed counterparts.[6] Hollander (1985) concludes that "appointment or election [...] affects a leader's actions" (p. 507). The direction of the effects, however, is unclear and

---

[5]Besides heterogeneous preferences of politicians which are only partially observed by voters, further key assumptions leading to this result are: (i) under mayor-council systems projects need the support of the mayor and the council, (ii) under the council-manager system projects need the support of the council. Therefore, less projects are realized under the mayor-council system. Coate and Knight (2011) find support for their model prediction in cross-sectional and panel analysis.

[6]Dal Bó et al. (2010) and Grossman and Baldassarri (2012) show that voter also perceive differences between elections and appointments. For example, the results by Grossman and Baldassarri (2012) indicate that individuals who can elect a leader contribute more to public goods than individuals who cannot chose their leader.

remains an empirical issue that we aim to investigate in this paper. We will return to this issue in section 3.2.

3 Background

3.1 Institutions

In the German state of Brandenburg, outcomes in local elections determine the form of government in a specific way. Votes for the winner in direct elections must represent at least 15% of all eligible voters. Otherwise, the direct election is suspended, and the local council decides on the head of government. We will return to this setting in detail later.

Brandenburg has two layers of local government: municipalities (*Gemeinden*) and counties (*Landkreise*). We focus on the county level because direct elections often fluctuate around the 15% quorum in county elections, while the quorum is always clearly passed in municipal elections. The state of Brandenburg surrounds the German capital of Berlin. Brandenburg has 14 counties, which roughly correspond with US counties regarding population size (150,000 inhabitants on average; for a map, see Figure 1). There are also four consolidated city-counties (*kreisfreie Städte*, white shaded in Figure 1), which are hardly comparable to the more rural counties in scope and responsibilities. For this reason, we use only counties in this study.

[Figure 1 about here]

In Brandenburg, counties are responsible for numerous plenty of public services. Tasks include education (school buildings, school capacity planning), public transport, social care, county roads, development of the local economy by granting subsidies, and administrative tasks such as drivers' licenses or building permits (Roesel, 2017). Counties account for approximately 7 to 10% of total government spending in Germany.[7] Social care, however, is the main responsibility of German counties. By 2016, Brandenburg

---

[7]Excluding social insurance expenditures. Tasks and expenditure shares vary across German states.

county administrations spent some 1,400 Euro ($ 1,600) per capita on social care, which was approximately 75% of total county expenditures (1,900 Euro per capita).[8]

Among social care, the public employment service for persons who are unemployed for more than one year (long-term unemployment) is the major service. The county administration and the federal public employment agency (*Bundesagentur für Arbeit*) jointly organize the *Jobcenter*, which is the local public employment agency for the long-term unemployed. Additionally, the costs are shared. The county reimburses the accommodation costs of the long-term unemployed; unemployment benefits are paid by the federal public employment agency. In some counties, the *Jobcenter* is fully decentralized to the county administration (*Optionskommunen*, see Mergele and Weber (2017)). *Jobcenters* do not only provide job offers and qualifications but can also cut unemployment benefits if unemployed people are not willing to cooperate. County administrations thus have powerful measures but also plenty of discretion in designing labor market policies for the long-term unemployed. Services for short-term unemployed people (unemployed for less than one year), by contrast, are fully centralized and provided by the federal public employment agency. Counties do not have any influence on public employment services for the short-term unemployed; we will examine this difference later.

Responsibilities at the county level are shared between the powerful head of the county government (*Landrat*) and the local county council (*Kreistag*). Even though county politics sometimes lack public attention, the *Landrat* is considered a powerful political player, even sometimes described as a "regional prince"[9]. The *Landrat* in Brandenburg is the head of an administration with an average of 850 employees, he or she also has considerable discretion in designing county public services. First, the *Landrat* decides on the *location* of public services, which is likely to have implications for policy outcomes. For example, there have been heated debates in the Brandenburg county of Potsdam-Mittelmark on where the head of county government aims to concentrate public services from four branches to a single location in order to increase efficiency. Local politicians have discussed whether there should be exceptions for the public em-

---

[8]Consolidated city-counties spend some 3,600 Euro per capita because they also perform the tasks of municipalities. Given that (rural) counties spend some 1,900 Euro per capita, county administration accounts for around one half of total local government expenditure in Brandenburg.

[9]Zeit Online, "Wenn der Wahlverlierer gewinnt", April 23, 2018, `https://www.zeit.de/politik/deutschland/2018-04/landratswahlen-brandenburg-wahlbeteiligung-gueltigkeit`.

ployment agency for the long-term unemployed (*Jobcenter*).[10] Second, *employment and organization* of county administrations are at the full discretion of the head of county governments. In the Unstrut-Hainich-Kreis county in the German state of Thuringia, for example, the *Landrat* proposed reorganizing the county administration, hiring managers and introducing controlling tools for social expenditures that are far beyond the state average.[11] Third, the head of county government has some discretion in designing the *scope and form of public services*. For example, the *Landrat* of Hersfeld-Rotenburg county in the German state of Hesse has not only completely reorganized the administration but also changed the office hours of the county offices where citizens can access the public employment agency.[12] There are many more examples from different counties in Brandenburg and other German states showing that the heads of county governments can exercise discretion in organizing their administration.

We now turn to election rules. After Germany's reunification in 1990, the East German state of Brandenburg introduced a council-manager system at the county level. The head of the county government was appointed by the county council. By 2010, Brandenburg had modified the electoral rules for county elections and introduced the direct election of the heads of local governments in order to increase voter participation. Winning candidates, however, do not only require the majority of votes cast but votes also must represent at least 15% of the eligible voters. Such a quorum for elections is unique to Germany; quotas usually only apply to referendums. If the 15% threshold is not reached, the direct election is suspended and the local council decides on the head of government. Direct election outcomes are not binding. The local council can choose from one of the candidates running in the direct election but can also appoint someone else. The mode of election does not influence any further rule or function. The head of local government in Brandenburg is elected for eight years, but he or she can be recalled from office by the voters via referendum, independent of whether he or she was elected or appointed.[13] The 15% quorum in Brandenburg dates back to 1993, when direct elections for mayors of municipalities were implemented. Rules for direct elections, including the 15% quorum, were simply rolled over to the county level in

---

[10]Märkische Allgemeine, "Landrat will Verwaltung nach Beelitz umsiedeln", August 13, 2018, `http://www.maz-online.de/Lokales/Potsdam-Mittelmark/Mittelmarks-Landrat-blaest-zum-grossen-Umzug`.

[11]Thüringer Allgemeine, "Landrat will Kreisverwaltung neu strukturieren", August 13, 2018, `https://muehlhausen.thueringer-allgemeine.de/web/muehlhausen/startseite/detail/-/specific/Landrat-will-Kreisverwaltung-neu-strukturieren-1781514774`.

[12]Hersfelder Zeitung, "Das Landratsamt stellt sich neu auf", April 27, 2016, `https://www.hersfelder-zeitung.de/bad-hersfeld/zieht-einem-strang-6350821.html`.

[13]Similar rules apply to mayoral elections at the municipality level in Brandenburg. There was, however, not a single case of a missed quorum in mayoral elections.

2010. Thus, there is no link between voter turnout performance in county elections and the choice of a specific threshold.[14]

Direct elections have been gradually phased in since 2010 according to the remaining term of the head of local government in office. Temporal differences are a result of history. There were simultaneous elections in all counties in 1994. Afterward, however, not all heads of county governments served a full term because of, for example, retirement, dismissal, sickness, or death. Therefore, election schedules began to diverge across counties. The first direct election took place in Oberspreewald-Lausitz county in January 2010, while the counties of Oder-Spree and Potsdam-Mittelmark did not hold direct elections for the first time until January 2017. Despite the influential role of the head of county government, very low voter turnout in elections is widespread in all East German states, including Brandenburg (e.g., Mecklenburg-Western Pomerania 2018: 27.5% on average). Voter turnout in elections for the head of county government is often much higher in West German states (for example, Bavaria 2017: 61% on average). Therefore, since 2010, only five out of 14 counties in Brandenburg exceeded the quorum and elected a head of local government directly (blue shaded counties in Figure 1). In the other nine counties, by contrast, the 15% quorum was not reached, direct elections were suspended, and the county council decided on the head of government (gray-shaded counties in Figure 1). In six of the nine cases where the quorum was not reached, the county council elected the candidate who won the direct election. In two cases, the council appointed a candidate who was defeated in the direct election. In a single case, the council elected an external candidate who did not run in the direct election.

3.2 Hypotheses

How do we expect direct elections to influence policy outcomes in Brandenburg counties? County administrations have three main tasks that could be subject to changes: administrative services, local economic development, and the public employment service for the long-term unemployed. First, in section 2, we hypothesized that direct elections increase incentives to introduce popular policies. Expediting administrative acts is certainly popular. Heads of government may increase staff, reduce internal approvals, and reorganize employees in efficient team sizes. Reducing internal standards

---

[14]Accordingly, Table 1 shows that voter turnout in elections before 2010 did not predict "successful" direct elections after 2010.

in favor of the citizens may also increase, for example, the turnaround of building permits. However, discretion in administrative services is often very limited due to state and federal law. If anything, we would expect that the number of administrative acts should increase under directly elected politicians. Policy measures regarding economic development are even more restricted at the county level, and the effect of direct elections is not that clear. On the one hand, appointed officials typically appear to be managers rather than politicians. The term council-*manager* system already indicates the perceived role of the head of government. Council-appointed managers, for example, are said to act more professionally and deliver efficiency. One may therefore expect that direct elections reduce efficiency and decrease economic activities, which we proxy with new business registrations. On the other hand, directly elected politicians may be more responsive to the needs of their voters. Delivering on local economic growth and jobs is certainly among the main objectives of politicians seeking re-election. Thus, it is not clear from a theoretical point of view whether and how direct elections affect economic outcomes. Finally, the public employment service for the long-term unemployed is one of the main tasks of German counties. County administrations have considerable discretion on policy measures (for example, providing well-matching job offers and qualifications or cutting unemployment benefits). Unemployment is of the utmost importance to citizens of East Germany, including the state of Brandenburg, where unemployment rates increased to approximately 20% after reunification. One out of two East German citizens was unemployed for some time between 1990 and 2010 (25% in West Germany).[15] In 2009, 58% of East Germans worried about losing their job (West Germany: 46%).[16] In 2014, 43% of East Germans still feared becoming unemployed compared to 30% in West Germany.[17] Therefore, East German politicians who are responsive to their electorate are well advised to address the issue of unemployment. We would therefore expect that directly elected politicians are more inclined to reduce unemployment than appointed managers. For institutional reasons, county administrations can barely influence short-term unemployment but can impact long-term unemployment. We are therefore expecting differences between long-term and short-term unemployment and expect to see effects only for the former.

---

[15]Deutsche Welle, "Jeder zweite Ostdeutsche war schon arbeitslos", February 20, 2010, `https://www.dw.com/de/jeder-zweite-ostdeutsche-war-schon-arbeitslos/a-5265094`.

[16]FAZ, "Die größten Ängste der Deutschen", September 3, 2009, `https://www.faz.net/aktuell/gesellschaft/studie-die-groessten-aengste-der-deutschen-1855968.html`.

[17]Wirtschaftswoche, " Wovor wir uns fürchten", September 4, 2014, `https://www.wiwo.de/politik/deutschland/aengste-der-deutschen-wovor-wir-uns-fuerchten/10653642-all.html`.

4   Empirical analysis

4.1   Identification

Our main identification assumption is that counties with a directly elected head of county government will have evolved in a similar way to counties with an appointed head of government if there had been no (successful) direct election. Two conditions must be met to estimate a causal effect. First, both groups of counties should follow parallel trends absent of treatment. This counterfactual scenario cannot be tested, but counties should follow a parallel trend in the period before the direct elections to indicate that this assumption is fulfilled. We later show in our event studies that this condition is fulfilled (see section 5.2). For example, we do not see that counties with directly elected heads of government differ regarding long-term unemployment from counties with an appointed head of government prior to inauguration (see the right-hand graph in the upper panel of Figure 4). However, we also employ the synthetic control method to model common trends more explicitly.

Second, identification requires some exogenous source of variation in the form of government. Endogeneity of the form of government and policy outcomes is the main concern, and it is highly likely to bias regression results. We use both temporal variation in the introduction of direct elections and spatial variation resulting from the 15% quorum to identify the causal effect of direct elections. Figure 2 provides an overview of the timing and outcomes of the first direct elections in the counties of Brandenburg since 2010. Light gray bars represent periods without direct elections. In these periods, the head of local government (*Landrat*) was appointed by the county council. After 2010, direct elections were held when the term of the head of county government ended. The termination date is exogenous because, as explained in section 3, not all heads of county governments served a full term since 1994, inducing different timings to the elections. Ade (2014); Koethenbuerger et al. (2014); Martinez-Bravo et al. (2017) and Hessami (2018) examine differences in the timing of expiring election terms in a similar fashion.

[Figure 2 about here]

However, we can rely on a second source of arguably exogenous variation: a missed or reached 15% quorum. Examining close election outcomes is a well-established strat-

egy in the political economy literature (for example, Freier and Thomasius (2016); Pettersson-Lidbom (2008)). The blue bars in Figure 2 indicate countries where direct elections were held and the quorum was reached; i.e., at least 15% of all eligible voters cast their vote in favor of the winning candidate. Dark gray bars, by contrast, represent direct elections that were suspended because the quorum of 15% was not reached. In those cases, the county council appointed the head of local government. The numbers in each bar represent the share of eligible voters casting their vote for the winning candidate, which is the crucial condition for the quorum. In some cases, the quorum was only missed by a few votes (14.6% and 14.9%). For example, in the county of Havelland, the winning candidate missed the quorum by only 175 votes (0.13%). At the threshold, the form of government can be assumed to be essentially exogenous. We later compare trends in counties close to around the 15% quorum threshold (see section 4.3).

Previous voter behavior does not predict whether the 15% quorum will be reached. Table 1 shows that voter turnout in county council elections prior to the first direct elections of the head of local government is not correlated with reaching (or missing) the quorum. We use voter turnout in the last county council election before 2010 in 2008 (column (1)), average voter turnout in the elections in 2003 and 2008 (column (2)), and long-term average voter turnout for all county council elections since reunification (column (3)). Voter turnout in previous county elections does not predict whether the 15% quorum was reached or missed in either specification. Additionally, also the year of election does not predict whether the quorum was reached or not (column (4)). We conclude that neither previous turnout performance nor general time trends influence the propensity of reaching the 15% quorum.

[Table 1 about here]

Accordingly, figure 3 shows that our sample is also well-balanced over time. Between 2009 and 2010, the head of local government was appointed in all counties. The share of counties with direct elections steadily increased over time. The share of counties with directly elected politicians developed somewhat proportionally to the share of counties with direct elections, although our sample is comparably small. We can therefore rule out temporal clustering; having earlier or later elections does not predict the "success" of direct elections. The map in Figure 1 also does not suggest any spatial clustering of "successful" direct elections. We are confident that – at least in counties

scattering closely around the 15% quorum threshold – sorting into forms of government is essentially exogenous.

[Figure 3 about here]

4.2 Data

We use monthly performance data for the 14 counties of the state of Brandenburg for the time period 01/2009 to 12/2017,[18] leading to a total of 1,512 observations. Because elections and inaugurations of newly elected heads of local government take place during the year, high-frequency monthly data are well-suited to capture the short-term effects of direct elections. Data on an annual basis less precisely mirror changes in government during the year. However, we show that inferences do not change when we use annual data. Consistent and reliable budget data are not available because accounting standards changed in 2011.

We begin by presenting results for data in levels but will quickly move on to first differences. We conduct tests for stationarity and trend stationarity. The results clearly show that we cannot reject the hypothesis that data in levels have a unit root (see Table 8 in the Online Appendix for Breitung test results). Taking first differences is a straightforward way to eliminate autocorrelation. When we use first differences with respect to the same month of the previous year, we can clearly reject the null of a unit root. Thus, we use first differences for all variables in our baseline specification, dummy variables being the exception. However, using data in levels does not change any results; we will also use variables in levels when we apply the synthetic control method as a robustness check.[19]

The dependent variables under investigation cover all the main tasks of the head of local government: local public employment services, administrative services, and local economic development. To describe the performance of local public employment services, we use series on short-term and long-term unemployment rates and the overall unemployment rate. Building permits represent the day-to-day administrative tasks of county administration. Finally, we use data on gross business registrations and net

---

[18]Data cover the period 01/2008–12/2017; because we use first differences to the same month of the previous year in our analysis, first differences cover the period 01/2009–12/2017.

[19]Figure 6 in the Online Appendix shows the development of our dependent variables between 2009 and 2017 for all 14 counties in levels.

business registrations (registrations minus de-registrations) to proxy economic activity. As control variables, we include total population (log), the vote share for left-wing parties in the county council,[20] and a dummy variable indicating decentralized counties self-administering the public employment service (no joint administration with the federal public employment agency).

Table 2 shows the descriptive statistics of our data. On average, unemployment rates (first differences to the same month in the previous year) decreased in Brandenburg, but long-term unemployment rates decreased more than short-term unemployment rates. The number of building permits increased in our sample period by 10.4 per million capita and per year. Gross and net business registrations decreased on average. In 14% of all observations, a directly elected head of government served in office. Total population and vote shares for left-wing parties in county council elections barely changed over time. Finally, in 45% of all observations, counties run a decentralized public employment service.

[Table 2 about here]

4.3   Regression specifications

We estimate difference-in-differences models, which we later extend to event studies. Our baseline model takes the following form:

$$\Delta Y_{itm} = \alpha_i + \theta_t \cdot \gamma_m + \beta Direct_{itm} + \Delta X'_{itm}\lambda + \epsilon_{itm} \tag{1}$$

where ΔY_{itm} describes our performance variables of interest in county i, year t, and month m. We use six different labor market but also administrative and economic outcomes on a monthly basis (first differences to the previous year) as performance variables. $Direct_{itm}$ is a dummy variable and refers to our difference-in-differences estimator of interest. It takes on the value of one for counties with a directly elected head of local government, and zero if there was no directly elected head of local government (either there were no direct elections or the quorum was not reached). We include interacted year and month fixed effects ($\theta_t \cdot \gamma_m$), county fixed effects (α_i), and a vector of control variables, $\Delta X'_{itm}$ as described in section 4.2. ϵ_{itm} denotes the error term. We

---

[20]See Mechtel and Potrafke (2013); Hibbs (1977).

cluster standard errors at the county level. Because the number of clusters (14) is rather low, we also report wild bootstrapped p-values.[21]

As we discussed in section 4.1, we can rule out endogeneity to a reasonable extent. Election terms expired for historical reasons, inducing exogeneity in the timing of direct elections. However, we already described that we cannot fully rule out that election terms are manipulated according to county government performance. Therefore, we also investigate two subsamples of counties within a window of $\pm 3.0\%$ and $\pm 4.5\%$ around the 15% threshold. In counties close to the 15% threshold, reaching or failing to reach the quorum, and therefore the form of government, depends on factors that are arguably beyond political and economic outcomes. Some voters, for example, may have not cast their vote because of bad weather, holidays, or local festivals. Taking up the idea of regression discontinuity designs (RDD) (Lee and Lemieux, 2010; Ferreira and Gyourko, 2009), election outcomes near the 15% threshold can be treated as exogenous. Counties that barely reached the quorum should be similar in terms of unobservables to the counties that just marginally missed the quorum. Because we have a low number of observations, we restrict our sample to counties around the threshold and cannot estimate RDD.

We also estimate event studies where we replace $Direct_{itm}$ with a vector of dummies measuring the months before and after a directly elected head of government takes office. We include 11 dummies for the months before taking office (-12 and less to -2) and 36 dummies for the months after taking office (1 to 36 and later). The month before inauguration serves as the base category. Our high-frequency data allow us a more precise picture of the effects around changes in office. This also enables us to test the validity of the parallel trend assumption that our main specification in equation (1) rests on, because the event study design allows visualizing whether counties with directly elected politicians perform differently than counties with appointed politicians both before and after a new politician enters office. Our event study takes the following form:

$$\Delta Y_{itm} = \alpha_i + \theta_t \cdot \gamma_m + \sum_{j=c}^{C} \beta_j (Direct_{itm}^j) + \Delta X_{itm}' \lambda + \epsilon_{itm} \qquad (2)$$

where similar to equation (1), ΔY_{itm} denote our monthly performance variables in first differences to the same month in the previous year, α_i, θ_t, and γ_m are county, year, and

---

[21]Inferences do not change when we use robust standard errors instead of clustering.

month fixed effects, $\Delta X'_{itm}$ is a vector of control variables, and ϵ_{itm} captures the error term. The vector of coefficients of interest is described by $\sum_{j=c}^{C} \beta_j$. $Direct_{itm}$ takes on the value of 1 if a directly elected politician enters office in county i in $(t+j)$ years and 0 otherwise. j ranges from $c = -12$ and less to $C = +36$ and more, excluding -1 (base category).

Finally, to account for the low number of treated units, we apply the synthetic control method developed by Abadie and Gardeazabal (2003) and Abadie et al. (2010, 2015) as a robustness test. We construct a synthetic counterfactual for all five Brandenburg counties surpassing the 15% quorum from a donor pool of the nine counties that failed the 15% quorum. The counterfactual is a weighted average of donor pool counties. Weights are derived in such a way that the synthetic counterfactual matches the pre-direct election period of the treated county best. This enables us to compare the development of all counties with "successful" direct elections with its synthetic counterpart before and after the inauguration of the politician.

5   Results

5.1   Difference-in-differences

Table 3 shows the results of our difference-in-differences estimations, where we move stepwise from annual data in levels to our preferred baseline specification using monthly data in first differences and control variables. We start with the most intuitive and basic specification in panel A, using annual data in levels and without any control variable. We then turn to first differences and estimate equation (1) using annual data in first differences (to the previous year) in panel B. We showed in section 4.2 that our level data are likely to have a unit root, which can be fixed by first differencing. In Panel C, we employ monthly data in first differences (to the same month in the previous year) instead of annual data to capture the timing of inaugurations during the year more precisely. Finally, we derive the most sophisticated specification in panel D, where we include control variables as discussed in section 4.2. This is our baseline specification.

Turning to the regression outcomes, we find a negative and significant effect of a directly elected head of government on the overall unemployment rate in all specifica-

16

tions (column (1)). County administrations have substantial discretion in organizing the public employment service for the long-term unemployed (*Jobcenter*) but cannot directly influence services for short-term unemployment. Accordingly, we observe that the effect of directly elected politicians on overall unemployment rates is entirely driven by long-term unemployment (column (3)). By contrast and as expected, we find no effect on short-term unemployment rates (column (2)), an area where county administrations have hardly any influence. The effect on long-term unemployment is equivalent to around one third of a standard deviation in long-term unemployment changes and is therefore also economically substantial. We do not find systematic effects of direct elections on administrative acts (in this case: building permits, column (4)) or business activity, which we proxy with business registrations (columns (5)).

[Table 3 about here]

Our results are robust to different specifications. First, we substitute interacted month-year fixed effects with separate fixed effects for years and months. The results shown in panel A of Table 4 fairly reproduce our baseline findings in Table 3 panel D. Clustering standard errors by county-per-year (panel B) or excluding the month of inauguration from our analysis (panel C) does not change any inferences. We also resample our dataset in a manual jackknife procedure by leaving out each of the 14 counties. Inferences barely change when we exclude individual counties. In 14 (13) out of 14 jackknife-like regressions, we find a statistically significant effect of direct elections on total unemployment (long-term unemployment). Net and gross business registrations, building permits, and short-term unemployment rates, by contrast, turn out to be barely significant (only in 1, 4, 0 and 1 out of 14 regressions).[22] Thus, our results are robust to technical modifications.

[Table 4 about here]

A potential concern might be that the selection of candidates may drive the results, if the councils appoint individuals systematically differing from the winners in direct elections.[23] To rule out the concern of selection, we therefore exclude all three counties where the council did *not* appoint the candidate who won the direct election but

---

[22]Results are available upon request.

[23]However, one may also take this as one potential channel explaining differences across forms of government. See, section 2.

failed to achieve the 15% quorum (panel A in Table 5). The only systematic difference between directly elected politicians surpassing the 15% quorum and winners of direct elections who missed the 15% quorum but were appointed by the local council is the mode of election. Table 5 shows that the results do not change when we adjust the control group as described. Another issue might be the presence of direct elections. In panel B, we include a dummy taking the value of one after the first direct election was held. Inferences do not change. Thus, not having yet held direct elections does not explain the differences between directly elected and council-appointed politicians in regard to long-term unemployment rates.

[Table 5 about here]

Finally, there is concern whether counties easily surpassing the quorum and counties clearly failing the quorum may also be different in terms of unobservables. We therefore restrict our sample to counties close to the 15% quorum threshold. Counties in this subsample should be even more comparable in unobservable characteristics because assignment into different forms of government is essentially exogenous. Table 6 reports the results for two different bandwidths, which provide a sufficient number of observations. In the upper panel, we reduce the bandwidth around the 15% threshold to ±4.5%. Accordingly, the number of observations decreases from 14 to 12 counties, but the results remain robust. In the lower panel of Table 6, we further reduce the bandwidth to ±3.0%, which roughly halves the dataset and leaves us with eight counties. Inferences do not change; in fact, point estimates increase in this homogenous sample, and we find effects that are statistically significant at the 1% level. In conclusion, we find robust and economically substantial effects of directly elected politicians on long-term unemployment but not on short-term unemployment or other administrative outcomes.

[Table 6 about here]

5.2 Event studies

Because elections and inaugurations took place at different points in time across counties, we re-estimate our baseline specification using event studies as described in equation (2). Event studies allow for a more precise picture of the months before and af-

ter entering office. Event studies also allow us to indicate whether the difference-in-differences common trend assumption is met and to investigate the timing of the effects. We denote the first month of a directly elected head of local government in office (first month after inauguration) in each county by 1; the last month before she or he enters office is defined by -1 and serves as the base category. Vertical dashed lines indicate the moment of inauguration and therefore the transition from council-manager to mayor-council system.

Figure 4 shows the results. We do not observe significant differences in unemployment rates between counties with directly elected heads of government and appointed heads of government in the months ahead of inauguration; 90% confidence intervals always include the zero (see upper panel of graphics in Figure 4). The results, however, change for the period after a directly elected head of county administration comes into office. Total unemployment rates decrease some months after inauguration. This effect is mainly driven by short-term unemployment, which also decreases. However, the effects are fairly small in size and become insignificant after few months. After a period of around two and a half years (30 months), long-term unemployment begins to decrease sharply and remains significantly lower under directly elected heads of government. Overall unemployment rates decrease accordingly. Thus, it takes around two and a half years until changes in the form of government translate into substantial labor market effects. There are no striking patterns to report for building permits or business registrations. Coefficients fluctuate around zero, confirming difference-in-differences results that are not statistically significant.

[Figure 4 about here]

5.3   Synthetic control

Inferences do not change when we include or exclude individual counties (see section 5.1). However, due to the comparably low number of observations, we also use the synthetic control method to model counterfactuals for all five counties surpassing the 15% quorum for direct elections. The nine counties failing to reach the 15% quorum are the donor pool.[24] The results are in line with our difference-in-differences estimations

---

[24]We cannot include counties from other states because all other comparable federal states have direct elections at the county level. The only German state not holding direct elections at the county level in our period of interest is Baden-Wuerttemberg, which differs drastically from Brandenburg (average unemployment rates 2009—2017: 4% vs. 10%).

when trends before a direct election do not differ among counties and their synthetic counterfactual; however, we observe some diverging trends afterward.

Figure 5 shows the results of the synthetic control approach for short- and long-term unemployment rates in our five counties with "successful" direct elections using monthly data in levels.[25] Vertical lines indicate the month when a directly elected politician was inaugurated. First, we turn to long-term unemployment rates (right-hand side). In the case of the counties of Maerkisch-Oderland, Oberspreewald-Lausitz, and Prignitz, pre-inauguration trends of the synthetic counterparts reproduce the "real" counties very well. In Dahme-Spreewald and Potsdam-Mittelmark, the synthetic control method does not deliver overlapping but somewhat parallel trends. After the directly elected head of county government has been inaugurated, trends clearly start diverging in the cases of Maerkisch-Oderland and Oberspreewald-Lausitz. Additionally, in Prignitz and Dahme-Spreewald, real outcomes in long-term unemployment rates decline compared to the synthetic counterfactual. The post-election period in Potsdam-Mittelmark is too short to infer any trend. In any event, none of our synthetic control findings contradict our difference-in-difference results; some are strongly supportive. Patterns change when we turn to short-term unemployment (left-hand side of Figure 5). The short-term unemployment rates of our treated counties and their synthetic counterparts follow very similar pre- and post-election trends. As expected, and in line with all previous findings, short-term unemployment rates do not change with the form of government.

[Figure 5 about here]

Figures 7 to 11 in the Online Appendix show the full results for all five treated counties and for all performance variables, including overall unemployment rates, building permits, and business registrations. For building permits and business registrations, we do not find differences between treated counties and their synthetic counterfactual corroborating difference-in-differences and event study findings. Overall unemployment rates diverge after inauguration, which is driven by the decrease in long-term unemployment rates. In conclusion, the synthetic control approach supports all the main results. In at least three out of five counties with "successful" direct elections, long-term unemployment clearly decreases after a directly elected politician enters of-

---

[25]We cannot include annual data because our data start in 2009 and we have only one observation for elections held in 2010.

fice. By contrast, we do not find an effect on short-term unemployment rates, building permits or business registrations.

5.4 Mechanisms

What are the driving forces behind our findings, and how do politicians change unemployment rates? County administrations account for only a small proportion of total county employment; hiring unemployed workers in the county administration is therefore hardly an option. There are two other, more reasonable ways. First, county administrations can increase subsidies, become more business-friendly and thus boost the economy and employment. Second, county administrations can change the incentives to work, i.e., the public employment service may become more efficient. For example, the public employment service (*Jobcenter*) has substantial discretion in sanctioning unemployed people who do not comply with job offers (Mergele and Weber, 2017).

Our evidence is more in line with the second channel: County administrations can become more efficient. First, our labor market effects are entirely driven by long-term unemployment rates, which can be influenced by the county administration to some extent. If a booming economy drives the results, we would expect to see short-term unemployment declining as well. Short-term unemployment, however, barely changes. Evidence on local economic activity supports this finding. Because there are no monthly GDP data available at the county level, we proxy the local economic performance by registrations of new businesses. Difference-in-differences results (see columns (5) in Tables 3, 4, 5 and 6) indicate that gross business registrations go up while de-registrations also increase. Net business registrations (column (6)) therefore reveal barely any significant effect; event study evidence (Figure 4) is also inconclusive. Thus, we have little evidence that economic performance improves under directly elected politicians. Second, we have shown that it takes several months to observe any labor market effect. Reorganizing an administration takes time. In the case of Potsdam-Mittelmark (see section 3), for example, the directly elected head of county administration introduced reform proposals some two years after the election, which coincides with our findings.

However, increasing efficiency in one administrative task might be offset by decreases in efficiency in other tasks if politicians simply shift resources. We investigate whether

the number of building permits changes in counties with a directly elected head of government. Building permits are one of the most demanding and important administrative services delivered by German county administrations. We find some weak negative effects on the number of building permits only in the subsample that is close to the 15% threshold (see column (4) in Table 6). Against the background of robust labor market effects, this may indicate that personnel resources were moved to the social administration. Unemployment is likely to be more visible to the media and the voters than other administrative tasks. However, the effects on building permits are comparably weak, indicating that increases in public employment services do not necessarily come at the cost of other services.

We now return to the mechanisms we discussed in section 2. Our specific setting allows the discussion of some of the theoretical channels that may explain differences across forms of government. First, in our setting, directly elected and council-appointed heads of government are equally accountable for their decisions because the same suspension rules apply. Directly elected politicians cannot be held "more accountable" than their appointed counterparts. Thus, accountability cannot drive the results. Second, we can also widely rule out re-election motives playing a major role. Almost all election results are around the 15% quorum; the head of local government cannot be sure whether she or he will be re-elected in the next election, even if their political performance is good and there is no serious challenger. Third, information asymmetries, as modeled by Coate and Knight (2011), cannot account for the differences we revealed in our setting. In the majority of cases, the county council elected the candidate winning the direct election even when the 15% quorum was not reached. Therefore, the selection of candidates rarely changed when direct elections were replaced by council appointment. Finally, the selection of candidates should also not play a major role. We compare directly elected politicians surpassing the 15% quorum and winners of direct elections who miss the 15% quorum but were appointed by the local council afterward. The only difference between both groups of winners is that one group directly enters office while the other is appointed by the council a few weeks later. Because direct elections always apply and candidates do not change, the selection of candidates should not drive the results.

Thus, accountability, re-election motives, information asymmetries, and the selection of candidates are not able to explain our findings. We conclude that the only remaining theory, factors attributed to the self-consciousness of political leaders, is the most likely to explanation off differences across forms of government. Elected leaders feel more

socially responsible in serving their followers, while followers place higher expectations on them. Appointed leaders are said to be somewhat less interested in the needs of their followers and are perceived to have less legitimacy. Moreover, followers accept deviating behavior more from an appointed than from an elected leader (De Cremer and Van Dijk, 2008; Kenney et al., 1996; Ben-Yoav et al., 1983; Hollander and Julian, 1970; Hollander, 1985, 1992; De Cremer and Van Vugt, 2002; Grossman et al., 2014). Direct elections create stronger ties between politicians and voters that in turn can translate in differences into policy outcomes.

6   Conclusion

We examine a quasi-experimental setting in the German state of Brandenburg, where a quorum applies to direct elections of the head of local governments. Votes for the winning candidate must represent at least 15% of all eligible voters; otherwise, the direct election (mayor-council or presidential system) is suspended and replaced by council appointment (council-manager or parliamentary system). We use election outcomes of county elections around the 15% threshold where the form of government is arguably exogenous.

Our results show that the public employment service for long-term unemployment operates more efficiently under directly elected politicians, but it takes several months to reorganize the administration. We find little evidence that directly elected politicians attract more businesses or expedite administrative acts such as building permits. Our findings are in line with the recent literature showing that directly elected politicians enact more visible policies.

Our setting allows us to rule out some theoretical explanations of why direct elections matter. We find differences between directly elected politicians surpassing the 15% quorum and winners of direct elections who miss the 15% quorum but were appointed by the local council afterward. The only systematic difference between both groups is the mode of election. The literature has shown that follower-leader relations are different under direct elections compared to council appointments. Directly elected politicians seem to be more self-conscious and are therefore more likely to implement reforms. However, more research is required to examine the mechanisms at work more explicitly. Future studies may also focus more on administrative efficiency, which is widely considered one major reason to change the form of local government.

References

Abadie A, Gardeazabal J (2003) The economic costs of conflict: A case study of the Basque country. American Economic Review 93(1):113–132

Abadie A, Diamond A, Hainmueller J (2010) Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program. Journal of the American Statistical Association 105(490):493–505

Abadie A, Diamond A, Hainmueller J (2015) Comparative politics and the synthetic control method. American Journal of Political Science 59(2):495–510

Ade F (2014) Do constitutions matter? Evidence from a natural experiment at the municipality level. Public Choice 160(3-4):367–389

Baqir R (2002) Districting and government overspending. Journal of Political Economy 110(6):1318–1354

Ben-Yoav O, Hollander EP, Carnevale PJ (1983) Leader legitimacy, leader-follower interaction, and followers' ratings of the leader. Journal of Social Psychology 121(1):111–115

Coate S, Knight B (2011) Government form and public spending: Theory and evidence from US municipalities. American Economic Journal: Economic Policy 3(3):82–112

Dal Bó P, Foster A, Putterman L (2010) Institutions and behavior: Experimental evidence on the effects of democracy. American Economic Review 100(5):2205–29

De Cremer D, Van Dijk E (2008) Leader-follower effects in resource dilemmas: The roles of leadership selection and social responsibility. Group Processes & Intergroup Relations 11(3):355–369

De Cremer D, Van Vugt M (2002) Intergroup and intragroup aspects of leadership in social dilemmas: A relational model of cooperation. Journal of Experimental Social Psychology 38(2):126–136

Enikolopov R (2014) Politicians, bureaucrats and targeted redistribution. Journal of Public Economics 120:74–83

Ferraresi M, Rizzo L, Zanardi A (2015) Policy outcomes of single and double-ballot elections. International Tax and Public Finance 22(6):977–998, DOI 10.1007/s10797-014-9344-x, URL https://doi.org/10.1007/s10797-014-9344-x

Ferreira F, Gyourko J (2009) Do political parties matter? Evidence from US cities. Quarterly Journal of Economics 124(1):399–422

Freier R, Thomasius S (2016) Voters prefer more qualified mayors, but does it matter for public finances? Evidence for Germany. International Tax and Public Finance 23(5):875–910, DOI 10.1007/s10797-015-9382-z, URL `https://doi.org/10.1007/s10797-015-9382-z`

Garmann S (2015) Elected or appointed? How the nomination scheme of the city manager influences the effects of government fragmentation. Journal of Urban Economics 86(3):26–42

Grossman G, Baldassarri D (2012) The impact of elections on cooperation: Evidence from a lab-in-the-field experiment in Uganda. American Journal of Political Science 56(4):964–985

Grossman G, et al. (2014) Do selection rules affect leader responsiveness? Evidence from rural Uganda. Quarterly Journal of Political Science 9(1):1–44

Hessami Z (2018) Accountability and incentives of appointed and elected public officials. Review of Economics and Statistics 100(1):51–64

Hibbs DA (1977) Political parties and macroeconomic policy. American Political Science Review 71(4):1467–1487

Hollander EP (1985) Leadership and power. In: Lindzey G, Aronson E (eds) Handbook of Social Psychology, vol 2, 2nd edn, Random house, New York, pp 485–537

Hollander EP (1992) Leadership, followership, self, and others. Leadership Quarterly 3(1):43–54

Hollander EP, Julian JW (1970) Studies in leader legitimacy, influence, and innovation. Advances in Experimental Social Psychology 5:33–69

Julian JW, Hollander EP, Regula CR (1969) Endorsement of the group spokesman as a function of his source of authority, competence, and success. Journal of Personality and Social Psychology 11(1):42

Kenney RA, Schwartz-Kenney BM, Blascovich J (1996) Implicit leadership theories: Defining leaders described as worthy of influence. Personality and Social Psychology Bulletin 22(11):1128–1143

Koeppl-Turyna M (2016) Opportunistic politicians and fiscal outcomes: the curious case of Vorarlberg. Public Choice 168(3-4):177–216

Koethenbuerger M, Egger P, Smart M (2014) Proportional influence? Electoral rules and special interest spending. Working papers, Beiträge zur Jahrestagung des Vereins für Sozialpolitik

Lee DS, Lemieux T (2010) Regression discontinuity designs in economics. Journal of Economic Literature 48(2):281–355

Lewis BD (2018) Local government form in Indonesia: Tax, expenditure, and efficiency effects. Studies in Comparative International Development 53(1):25–46

MacDonald L (2008) The impact of government structure on local public expenditures. Public Choice 136(3-4):457–473

Martinez-Bravo M, Mukherjee P, Stegmann A (2017) The non-democratic roots of elite capture: Evidence from Soeharto mayors in Indonesia. Econometrica 85(6):1991–2010

Maskin E, Tirole J (2004) The politician and the judge: Accountability in government. American Economic Review 94(4):1034–1054

Mechtel M, Potrafke N (2013) Electoral cycles in active labor market policies. Public Choice 156(1-2):181–194

Mergele L, Weber M (2017) Public employment services under decentralization: Evidence from a natural experiment. Working papers 2017002, Berlin Doctoral Program in Economics and Management Science (BDPEMS), URL https://ideas.repec.org/p/bdp/wpaper/2017002.html

Persson T, Tabellini G (2003) The economic effects of constitutions: What do the data say. Cambridge, Massachusetts: MIT Press

Pettersson-Lidbom P (2008) Do parties matter for economic outcomes? A regression-discontinuity approach. Journal of the European Economic Association 6(5):1037–1056

Robinson J, Torvik R (2016) Endogenous presidentialism. Journal of the European Economic Association 14(4):907–942

Roesel F (2017) Do mergers of large local governments reduce expenditures? – Evidence from Germany using the synthetic control method. European Journal of Political Economy 50(C):22–36

Saha S (2011) City-level analysis of the effect of political regimes on public good provision. Public Choice 147(1):155–171

Figure 1: Counties in the German state of Brandenburg
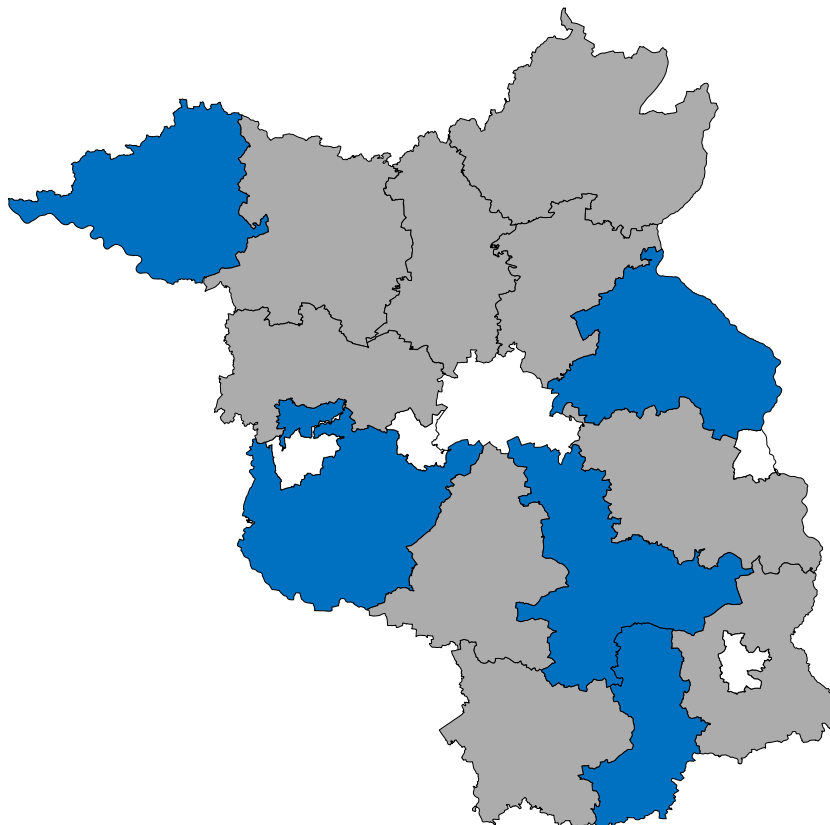
Notes: The map shows the 14 counties of the German state of Brandenburg. In blue shaded counties, the 15% quorum was reached and the head of local government was directly elected. In gray shaded counties, the winner of direct elections missed the quorum of 15% and the head of local government was appointed by the local council. Four consolidated city-counties (Brandenburg an der Havel, Cottbus, Frankfurt/Oder, and Potsdam) and the state of Berlin (in the very center) are excluded (white shaded).

Figure 2: Direct elections in Brandenburg counties
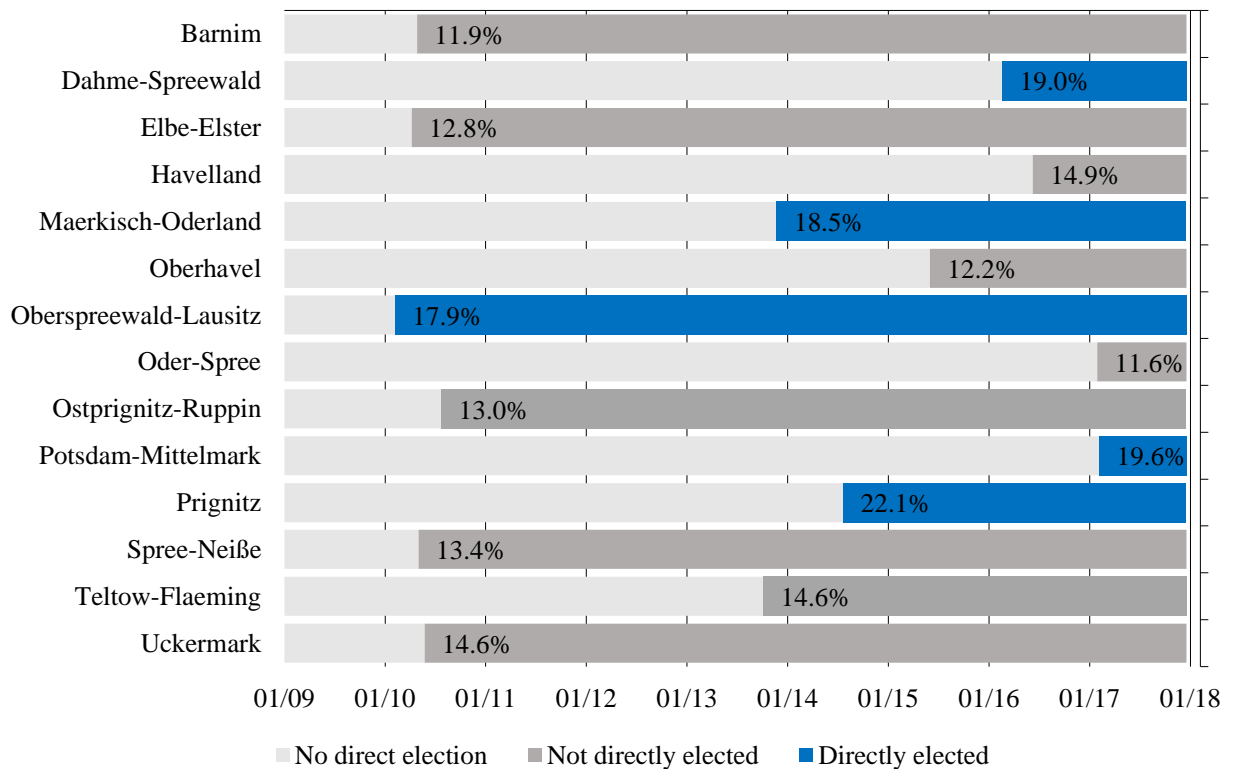
Notes: The figure shows the timing and the outcomes of direct elections for the head of local government (*Landrat*) in the 14 counties of Brandenburg. Transitions represent the day of inauguration, numbers in bars are the share of eligible voters casting their vote for the winning candidate. Light gray bars represent times of no direct elections (election by the local council). Dark gray bars represent direct elections that were suspended because the 15% quorum was not reached; the local council decided on the head of government. Blue bars show "successful" direct elections when the votes for the winning candidate represent at least 15% of the eligible voters.

Figure 3: Sample balancedness
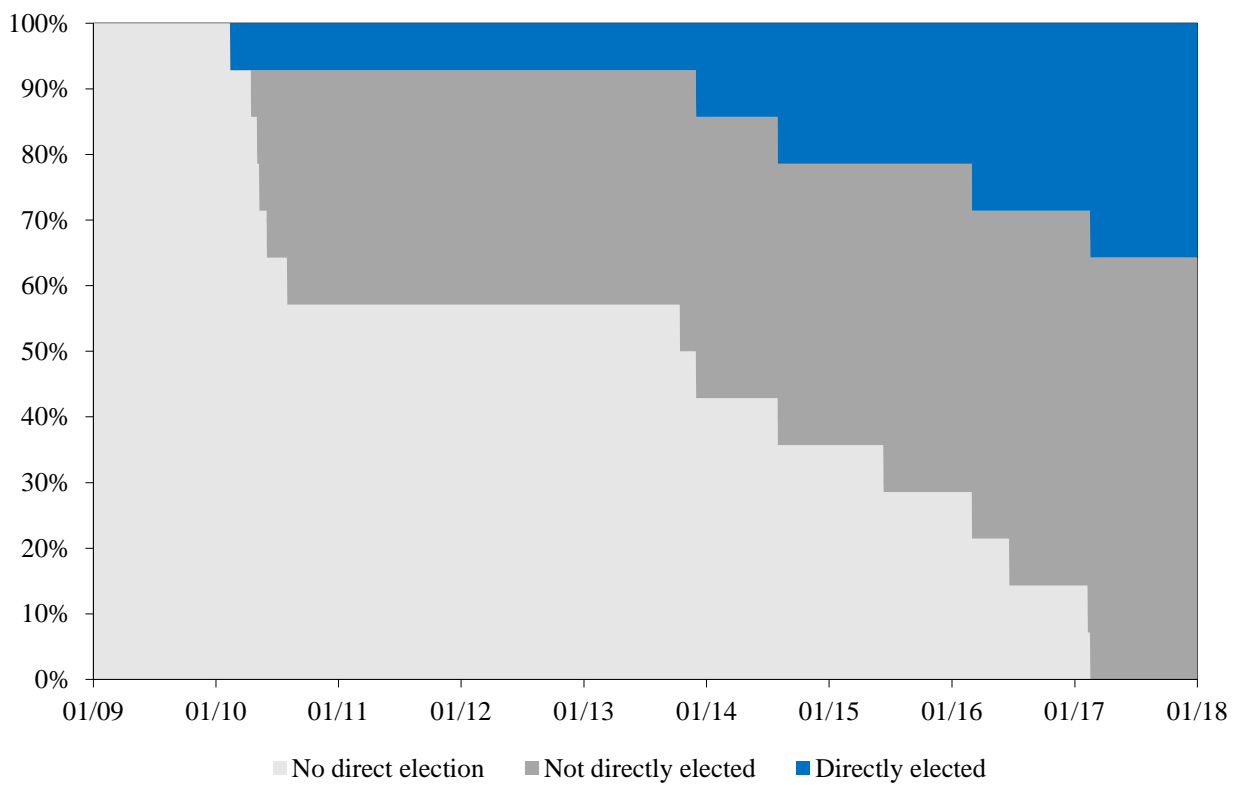
Notes: The figure shows the cumulative shares for no direct elections (light gray), direct elections that were suspended because the 15% quorum was not reached (dark gray) and "successful" direct elections when the votes for the winning candidate represent at least 15% of the eligible voters.

Figure 4: Event studies
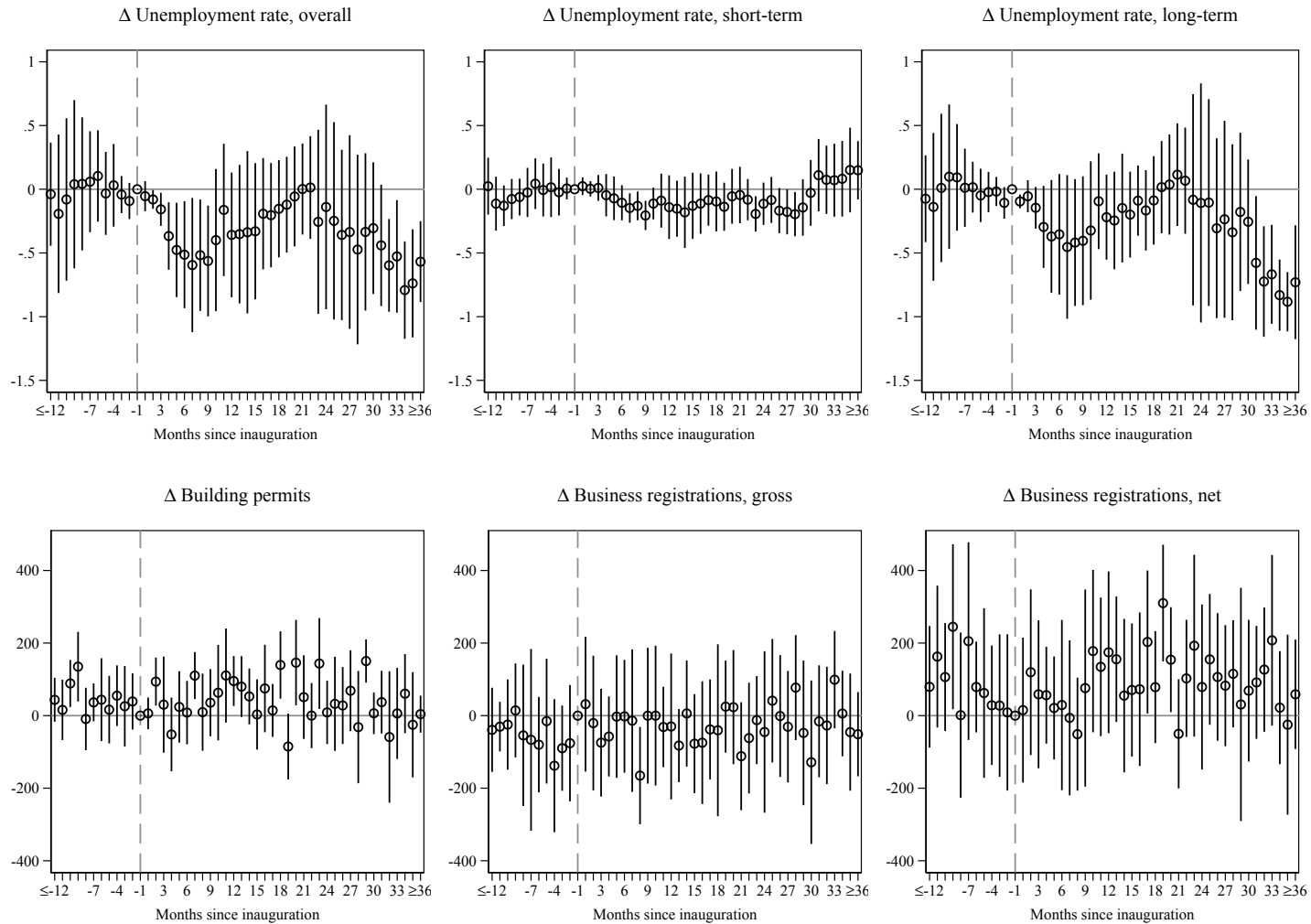
Notes: The figures shows the results of event study estimations. Vertical dashed lines represent the inauguration of a directly elected politician. The 14 counties of the German state of Brandenburg are our units of observation. We use monthly data in first differences to the previous year over the period 01/2009 to 12/2017. Circles are point estimates, black lines represent the 90% confidence interval. We include year-month and county fixed effects and control variables (see notes for table 3, panel D). Month −1 is the base category. We cluster standard errors at the county level.

Figure 5: Synthetic control method
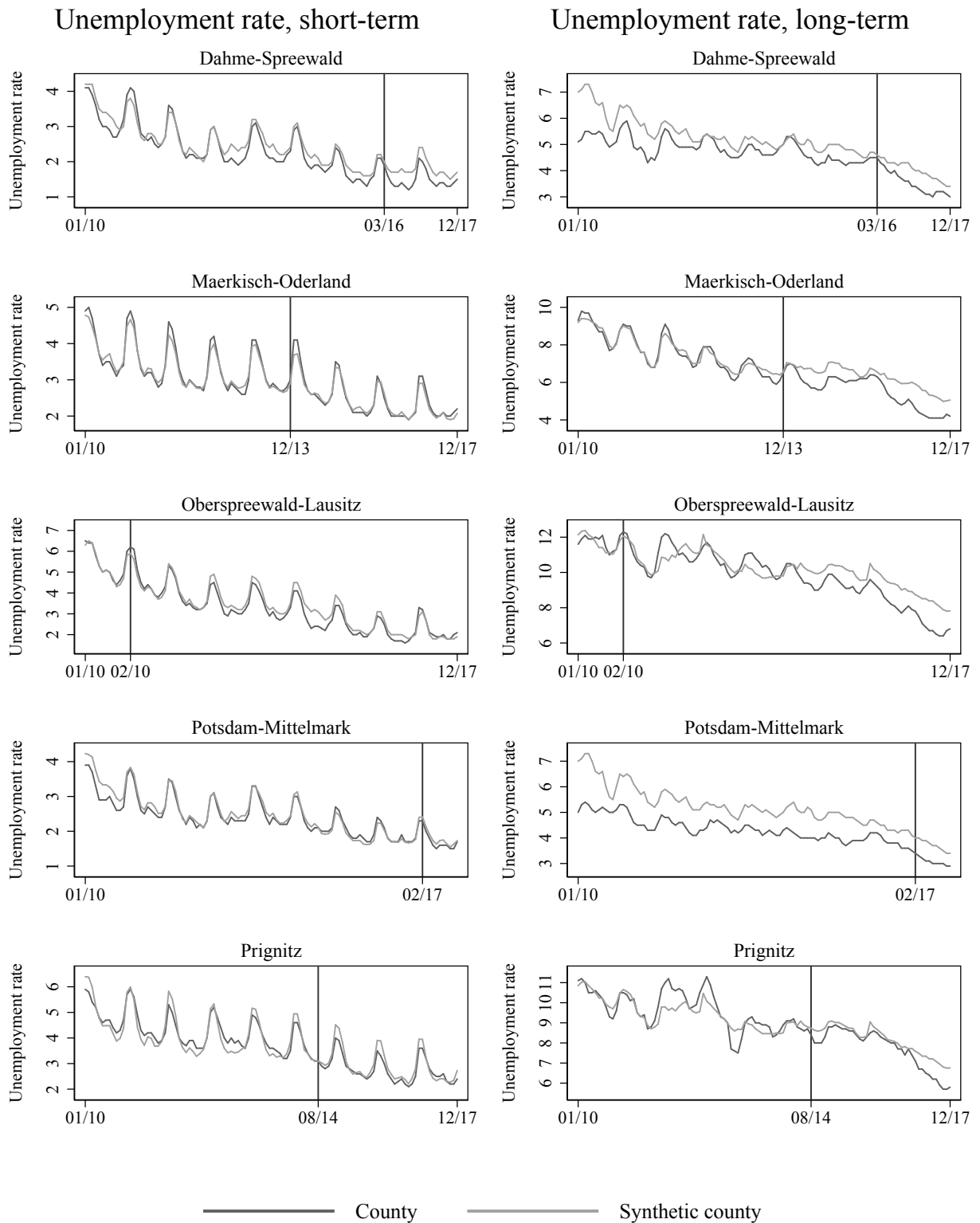
Notes: The figure shows short-term and long-term unemployment rates in five counties where the head of local government was directly elected (black) and their synthetic counterparts (gray). The vertical lines represents the month of inauguration of the directly elected head of local government. The donor pool consists of nine counties in the German state of Brandenburg where the 15% quorum was not reached.

Table 1: Previous voter turnout and year of election do not predict direct election outcomes

	Quorum reached ($= 1$)			
	(1) 2008	(2) 2003-2008	(3) 1993-2008	(4)
Turnout	0.110 (0.122)	0.102 (0.081)	0.007 (0.014)	
Election year				0.040 (0.145)
Pseudo R^2	0.046	0.045	0.004	0.005
Observations	14	28	56	14

Notes: The table shows the results of probit regressions where the 14 counties of the state of Brandenburg are the units of observation. The dependent variable is the dummy *Direct*, which is one if the 15% quorum in the direct election for the head of local government was reached and zero otherwise. In column (1) to (3) voter turnout in previous county council elections is the regressor. We average over the elections in 2008 (column (1)), 2003 and 2008 (column (2)), and 1993, 1998, 2003, 2008 (column (3)). In column (4) the year of the election is the regressor.

Table 2: Descriptive statistics

	Obs.	Mean	SD	Min	Max
Dependent variables					
Δ Unemployment rate, overall	1,512	-0.698	0.607	-2.800	1.500
Δ Unemployment rate, short-term	1,512	-0.240	0.283	-1.400	0.600
Δ Unemployment rate, long-term	1,512	-0.457	0.578	-2.600	2.000
Δ Building permits	1,512	10.365	134.911	-811.870	1,053.247
Δ Business registrations, gross	1,512	-23.732	157.175	-891.189	802.746
Δ Business registrations, net	1,512	-9.341	159.714	-824.572	711.153
Directly elected politician in office					
Direct (yes = 1)	1,512	0.144	0.351	0.000	1.000
Control variables					
Δ Population (log)	1,512	-0.002	0.008	-0.020	0.025
Δ Left-wing vote share	1,512	0.058	2.234	-7.724	7.778
Decentralized (yes = 1)	1,512	0.452	0.498	0.000	1.000

Note: The 14 counties of the German state of Brandenburg are our units of observation. We use monthly data over the period 01/2009 to 12/2017. All variables are in first differences with respect to the previous year, dummy variables being the exception. Long-term and short-term unemployment rate refer to unemployed under *SGB II* and *SGB III*. Decentralized counties are fully responsible for the public employment service for long-term unemployed, not decentralized counties have joint public employment service for long-term unemployed with the federal public employment agency. Data on building permits and business registration are per million capita.

Table 3: Baseline

	Unemployment rate			Building permits	Business registration	
	(1) Overall	(2) Short-term	(3) Long-term	(4) All	(5) Gross	(6) Net
A: Levels, year						
Direct	-0.533*	-0.252	-0.286	18.540	6.684	17.030
	(0.257)	(0.248)	(0.215)	(21.850)	(12.395)	(13.351)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	No	No	No	No	No	No
Within R^2	0.917	0.905	0.842	0.542	0.862	0.659
Wild bootstrapped p-value	0.081	0.435	0.255	0.440	0.615	0.265
Observations	126	126	126	126	126	126

	Δ Unemployment rate			Δ Building permits	Δ Business registration	
	(1) Overall	(2) Short-term	(3) Long-term	(4) All	(5) Gross	(6) Net
B: First differences, year						
Direct	-0.270**	-0.010	-0.261**	5.874	8.123	7.124
	(0.095)	(0.036)	(0.112)	(11.454)	(8.593)	(8.145)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	No	No	No	No	No	No
Within R^2	0.409	0.571	0.515	0.233	0.321	0.307
Wild bootstrapped p-value	0.028	0.784	0.046	0.729	0.372	0.524
Observations	126	126	126	126	126	126
C: First differences, month						
Direct	-0.304***	-0.048	-0.249**	-3.650	8.460	1.051
	(0.099)	(0.035)	(0.106)	(8.906)	(4.934)	(10.245)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	No	No	No	No	No	No
Within R^2	0.349	0.541	0.393	0.122	0.158	0.164
Wild bootstrapped p-value	0.004	0.163	0.023	0.691	0.154	0.915
Observations	1,512	1,512	1,512	1,512	1,512	1,512
D – Baseline: First differences, month						
Direct	-0.320**	-0.058	-0.254**	-5.427	10.102	2.331
	(0.108)	(0.038)	(0.110)	(8.668)	(6.193)	(10.089)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.357	0.550	0.397	0.123	0.160	0.165
Wild bootstrapped p-value	0.008	0.166	0.032	0.544	0.211	0.800
Observations	1,512	1,512	1,512	1,512	1,512	1,512

Notes: The table shows the results of difference-in-differences estimations. The 14 counties of the German state of Brandenburg are our units of observation. We use data over the period 01/2009 to 12/2017. Our variable of interest (*Direct*) takes on the value of 1 for directly elected heads of county government, and zero otherwise. The first panel (A) shows regression results for data in levels and annual data; the second panel (B) for first differences (to the previous year) and annual data; the third panel (C) for first differences (to the same month in the previous year) and monthly data; the fourth panel (D) for first differences (to the same month in the previous year) and monthly data with control variables. Additional control variables: total population (log), left-wing vote share in county council elections, dummy for decentralized public employment service for long-term unemployed. Data on building permits and business registration are per million capita. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10.

	Δ Unemployment rate			Δ Building permits	Δ Business registration	
	(1) Overall	(2) Short-term	(3) Long-term	(4) All	(5) Gross	(6) Net
A: Year FE, month FE						
Direct	-0.330***	-0.057	-0.266**	-7.019	7.405	-0.914
	(0.107)	(0.039)	(0.110)	(8.842)	(7.229)	(10.790)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.254	0.391	0.321	0.023	0.036	0.040
Wild bootstrapped p-value	0.005	0.210	0.030	0.436	0.428	.939
Observations	1,512	1,512	1,512	1,512	1,512	1,512
B: County-year cluster						
Direct	-0.320***	-0.058	-0.254**	-5.427	10.102	2.331
	(0.116)	(0.053)	(0.126)	(14.538)	(14.185)	(19.000)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.357	0.550	0.397	0.123	0.160	0.165
Wild bootstrapped p-value	0.015	0.338	0.091	0.730	0.523	0.904
Observations	1,512	1,512	1,512	1,512	1,512	1,512
C: W/o inauguration month						
Direct	-0.325**	-0.059	-0.257**	-5.448	8.488	4.942
	(0.110)	(0.037)	(0.114)	(8.100)	(7.062)	(10.433)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.351	0.551	0.395	0.124	0.159	0.165
Wild bootstrapped p-value	.01	.137	.036	.502	.367	.631
Observations	1,498	1,498	1,498	1,498	1,498	1,498

Notes: The table shows the results of difference-in-differences estimations. The 14 counties of the German state of Brandenburg are our units of observation. We use monthly data in first differences to the previous year over the period 01/2009 to 12/2017. Our variable of interest (*Direct*) takes on the value of one for directly elected heads of county government, and zero otherwise. The first panel (A) shows regression results with year fixed effects and month fixed effects (instead of their interaction); the second panel (B) shows regression results with standard errors clustered at the county-year level; the third panel (C) shows regression results excluding the month of inauguration. Additional control variables: total population (log), left-wing vote share in county council elections, dummy for decentralized public employment service for long-term unemployed. Data on building permits and business registration are per million capita. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10.

Table 5: Robustness (II)

	Δ Unemployment rate			Δ Building permits	Δ Business registration	
	(1) Overall	(2) Short-term	(3) Long-term	(4) All	(5) Gross	(6) Net
A: Smaller sample						
Direct	-0.315**	-0.035	-0.271**	1.116	14.233**	5.732
	(0.111)	(0.038)	(0.112)	(7.898)	(6.271)	(10.481)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.375	0.575	0.410	0.134	0.189	0.195
Wild bootstrapped p-value	0.007	0.358	0.026	0.901	0.104	0.556
Observations	1,188	1,188	1,188	1,188	1,188	1,188
B: Election dummy						
Direct	-0.546***	-0.104	-0.443***	-1.888	7.334	4.217
	(0.150)	(0.074)	(0.124)	(14.338)	(8.573)	(20.471)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.378	0.554	0.412	0.123	0.160	0.165
Wild bootstrapped p-value	0.000	0.214	0.000	0.905	0.409	0.849
Observations	1,512	1,512	1,512	1,512	1,512	1,512

Notes: The table shows the results of difference-in-differences estimations. The 14 counties of the German state of Brandenburg are our units of observation. We use monthly data in first differences to the previous year over the period 01/2009 to 12/2017. Our variable of interest (*Direct*) takes on the value of one for directly elected heads of county government, and zero otherwise. The first panel (A) shows regression results for a smaller sample excluding counties where the council did *not* appoint the candidate winning the (suspended) direct election; the second panel (B) shows regression results when we include a dummy taking the value of one after the first direct election was hold. Additional control variables: total population (log), left-wing vote share in county council elections, dummy for decentralized public employment service for long-term unemployed. Data on building permits and business registration are per million capita. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10.

Table 6: Subsamples around the 15% quorum

	Δ Unemployment rate			Δ Building permits	Δ Business registration	
	(1) Overall	(2) Short-term	(3) Long-term	(4) All	(5) Gross	(6) Net
A: Bandwidth 4.5%						
Direct	-0.257*	0.007	-0.258	-11.540	15.171*	15.674
	(0.142)	(0.034)	(0.162)	(8.808)	(7.444)	(13.007)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.399	0.570	0.404	0.129	0.168	0.166
Wild bootstrapped p-value	0.081	0.834	0.091	0.262	0.223	0.341
Observations	1,296	1,296	1,296	1,296	1,296	1,296
B: Bandwidth 3.0%						
Direct	-0.819***	0.063	-0.880***	-35.899*	-1.313	-27.213
	(0.145)	(0.085)	(0.146)	(16.031)	(21.888)	(34.428)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.452	0.577	0.424	0.158	0.194	0.175
Wild bootstrapped p-value	0.234	0.547	0.164	0.414	0.945	0.508
Observations	864	864	864	864	864	864

Notes: The table shows the results of difference-in-differences estimations. The 14 counties of the German state of Brandenburg are our units of observation. We use monthly data in first differences to the previous year over the period 01/2009 to 12/2017. Our variable of interest (*Direct*) takes on the value of 1 for directly elected heads of county government, and zero otherwise. The upper panel refers to a subsample of counties within a bandwidth of ±4.5% around the 15% quorum, the lower panel refers to a subsample within a bandwidth of ±3.0% around the 15% quorum. Additional control variables: total population (log), left-wing vote share in county council elections, dummy for decentralized public employment service for long-term unemployed. Data on building permits and business registration are per million capita. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10.

Online Appendix

For online publication only.

Table 7: Literature overview

| Paper	Election	Country	Identification strategy	Counterfactual	Outcomes	Results
Ade (2014)	Mayor (municipal level)	German states (Hesse, Saarland, Rhineland-Palatinate)	Reform (introduction of direct election by citizens); Different timing in elections – phasing in of the reform; Difference-in-differences estimation, Event Study approach	Municipalities which not yet had a direct election	Property tax rates; Trade tax rate; Total expenditure per capita; Personnel expenditure per capita	Reduced tax rates; Increased spending on government employees (under direct elections)
Baqir (2002)	City governments	U.S.	Ordinary Least Square Regression	Cities with council-manager system	Government expenditure per capita and as share of total income; Government employment	Reduced expenditures (under mayor-council system)
Coate and Knight (2011)	City governments	U.S.	Cross-sectional and panel analysis	Cities with council-manager system	Government spending per capita	Reduced spending (under mayor-council system)
Enikolopov (2014)	Chief executive officer (local communities)	U.S.	Fixed effects estimation	Local communities with appointed chief executive officers	Number of public employees	Increased number of public employees (under direct elections)
Garmann (2015)	Mayors (municipal level)	German state (Hesse)	Reform (introduction of direct election by citizens); Different timing in elections – phasing in of the reform; Difference-in-differences estimation	Municipalities which not yet had a direct election	Government expenditures per capita (Total, Personnel, Material, Investment)	Increased total, personnel and material expenditure (under direct elections)
Hessami (2018)	Mayors (municipality level)	German state (Hesse)	Reform (introduction of direct election by citizens); Different timing in elections – phasing in of the reform; Difference-in-differences estimation	Municipalities which not yet had a direct election	Investment transfers per capita	Increased investment grants in election years (under direct elections)
Koeppl-Turyna (2016)	Majors (municipality level)	Austrian state (Vorarlberg)	Reform (introduction of direct election by citizens); Each municipality can choose electoral rules for each election separately; Difference-in-differences estimation, propensity score matching	Municipalities without directly elected mayors	Municipal expenditure per capita (Total, Public Administration, Security and Public Order, Education and Sport, Culture and Religion, Social Support and Housing, Health Protection, Transportation, Promotion of the Economy (i.e., subsidies and grants to industries and agriculture), Services, Finance (e.g., debt payments), Public Personnel)	Reduced expenditure on public administration and public personnel; Increased expenditure in transportation infrastructure and economic subsidies to firms and individuals; No effect on total expenditure (under direct elections)
Koethenbuerger et al. (2014)	Mayor (municipality level)	German state (Lower Saxony)	Reform (introduction of direct election by citizens); Different timing in elections – phasing in of the reform; Difference-in-differences estimation	Municipalities which not yet had a direct election	Municipal expenditure per capita (Total, Social Security, General Administration	Increase in local government expenditure (under direct elections)
Lewis (2018)	Local government (district level)	Indonesia	Reform (introduction of direct election by citizens); Different timing in elections – phasing in of the reform; Fixed effects estimation	Districts without direct elections	Tax, expenditures, efficiency effects	Reduced spending, increased saving and efficiency, no effect on taxes (under direct elections)
MacDonald (2008)	City councilors	U.S.	Fixed effects estimation	Cities with mayor-council system	Government expenditure per capita; Core government expenditure per capita	No effect
Saha (2011)	City councilors	U.S.	Seemingly unrelated regressions system	Cities with council-manager system	Government expenditure per capita on different public services (highways, police protection, fire protection, sewerage, and parks and recreational activities)	Increased police expenditure and highways expenditure (under mayor-council form)

Table 8: Unit roots tests

A: Levels	Unemployment rate			Building permits	Business registration	
	(1) Overall	(2) Short-term	(3) Long-term	(4) All	(5) Gross	(6) Net
P–value (Breitung test)	1.000	1.000	1.000	0.550	0.992	0.092
P–value (Breitung test, time trend)	0.379	0.443	0.986	0.178	0.262	0.581

| B: First differences	Δ Unemployment rate			Δ Building permits	Δ Business registration	
	(1) Overall	(2) Short-term	(3) Long-term	(4) All	(5) Gross	(6) Net
P–value (Breitung test)	0.000	0.000	0.001	0.000	0.000	0.000

Notes: The table shows results of the Breitung test. The null hypothesis is that all panels have a unit root. The 14 counties of the German state of Brandenburg are our units of observation. We use annual data over the period 2009 to 2017. The first panel (A) shows p-values of the Breitung test using level data and excluding and including a time trend to consider trend stationarity; the second panel (B) shows the p-value of the Breitung test using data in first differences (to the previous year).

Figure 6: Raw data plots
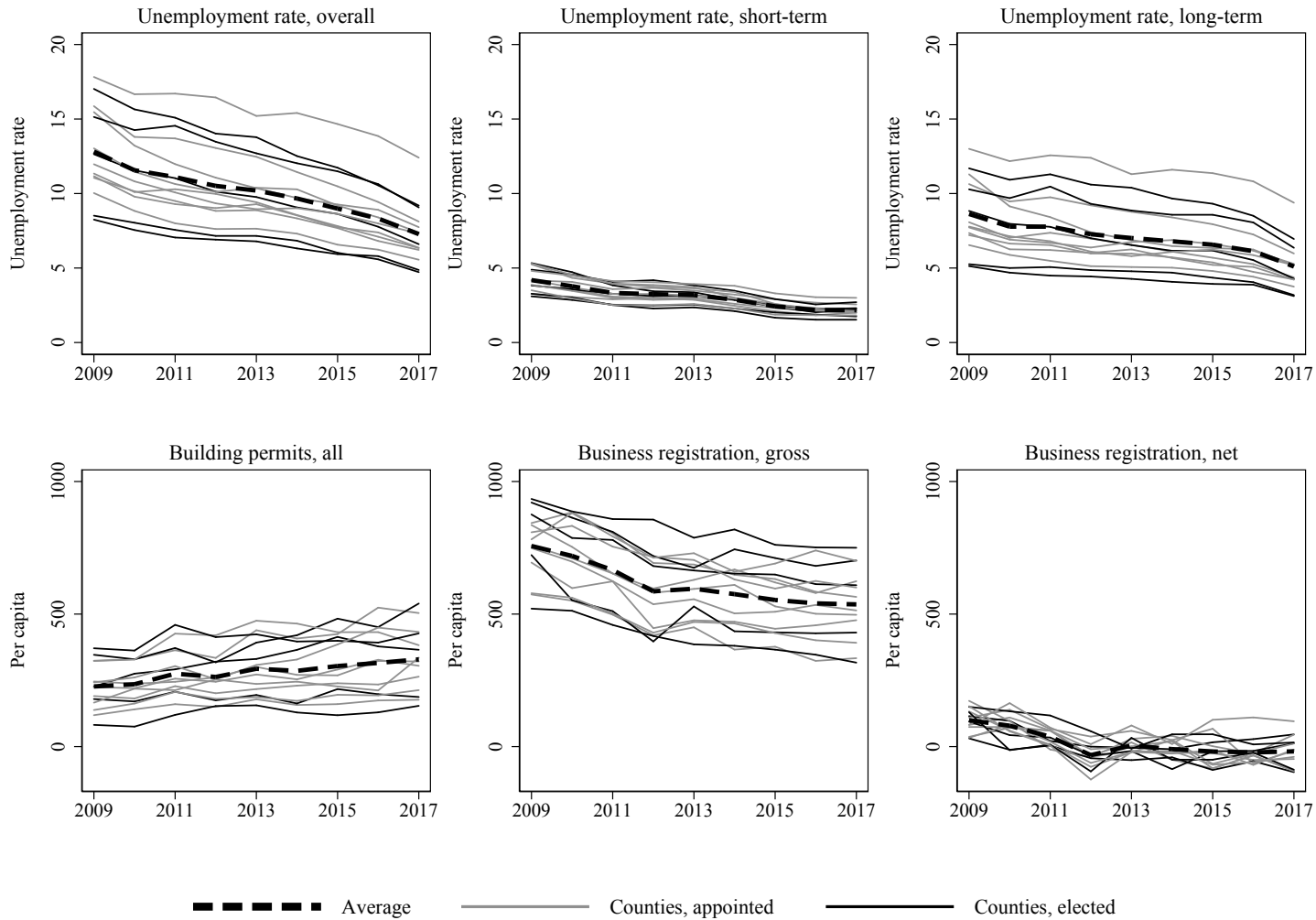
Notes: The figure shows how our six dependent variables evolve over the period 2009 to 2017 (annual data). The 14 counties of the German state of Brandenburg are our units of observation. Black dashed lines represent the average over all counties, solid lines represent individual counties. Gray lines show counties where the direct elections where suspended because the 15% quorum was not reached and the council decided on the head of government. Black lines show counties with "successful" direct elections where votes for the winning candidate represent at least 15% of the eligible voters. Data on building permits and business registration are per million capita.
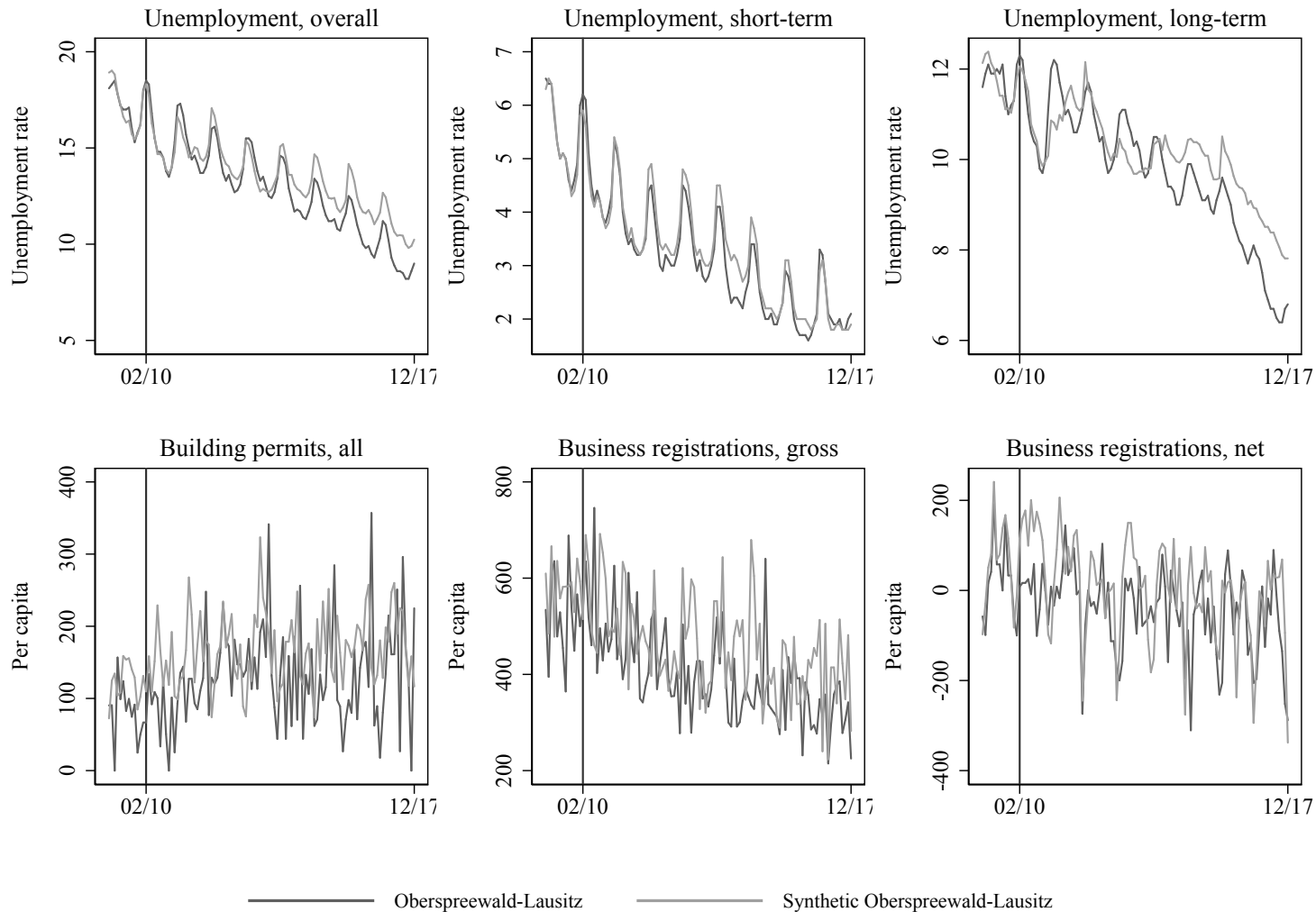
Figure 7: Dahme-Spreewald
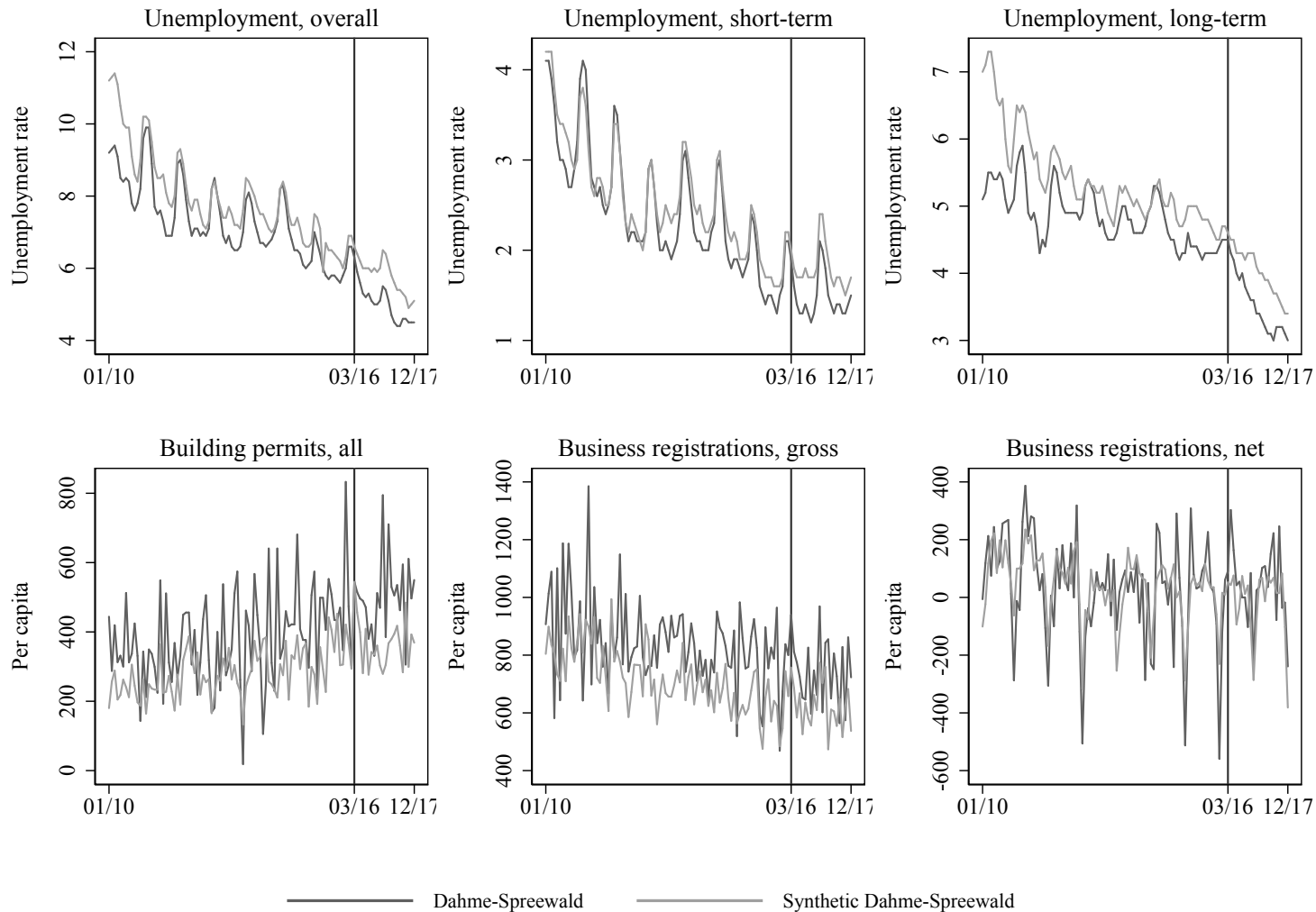
Notes: The figure shows our six dependent variables in the county Dahme–Spreewald and its synthetic counterpart (monthly data in levels). The vertical lines represents the month of inauguration of the directly elected head of local government. The donor pool consists of the counties in the German state of Brandenburg where the head of local government was never "successfully" elected and always appointed by the council. Data on building permits and business registration are per million capita.
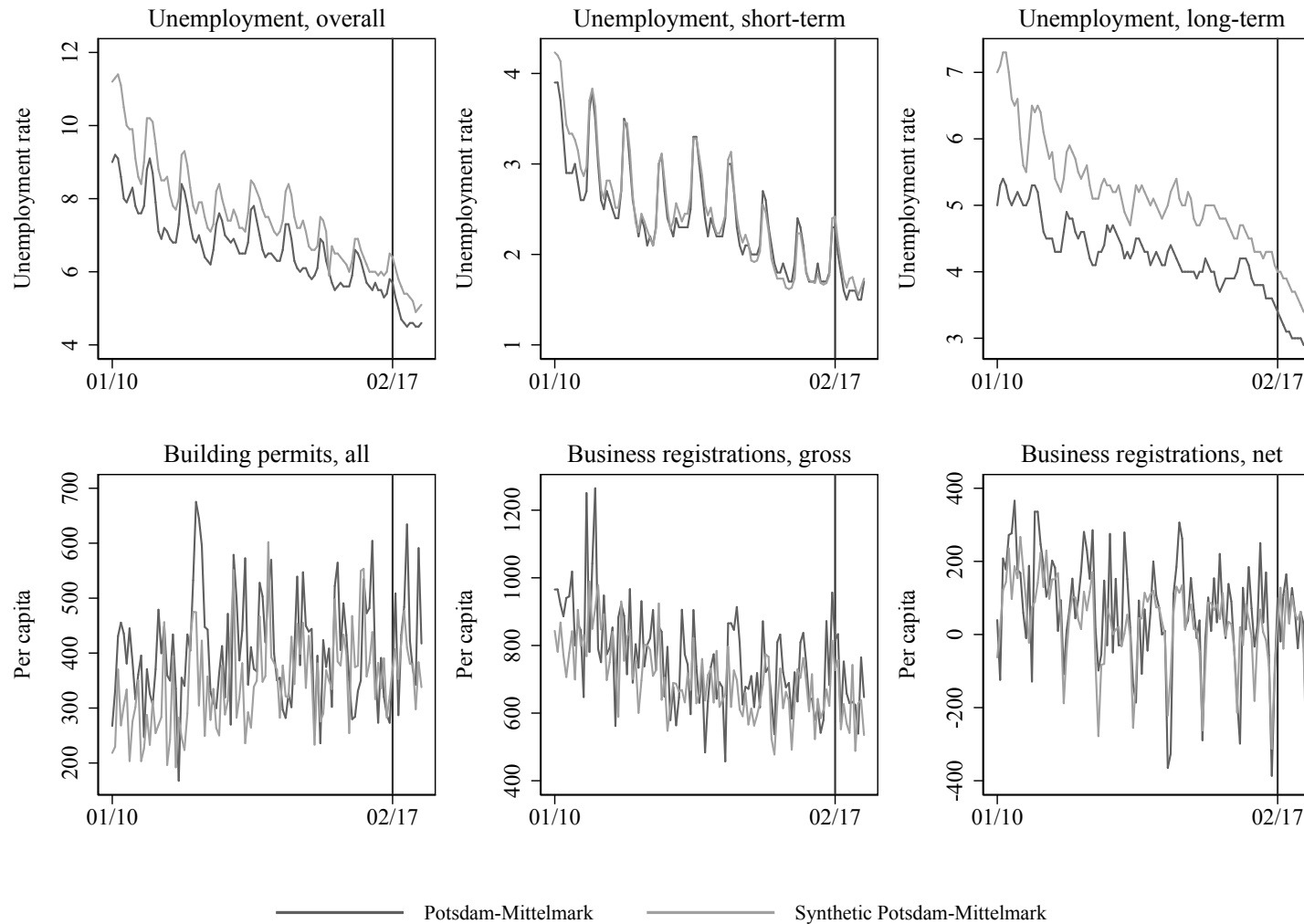
Figure 8: Maerkisch-Oderland
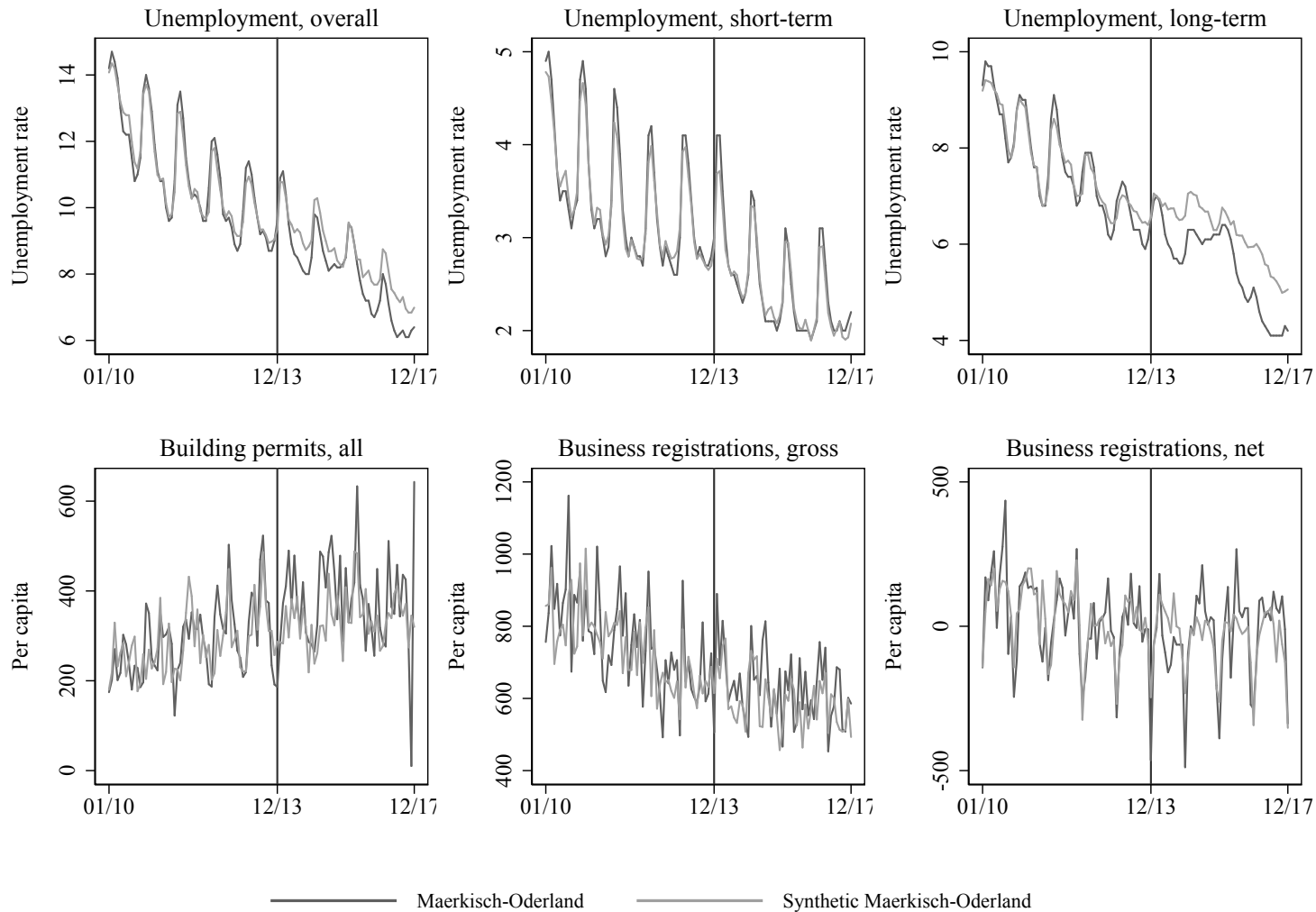
Notes: The figure shows our six dependent variables in the county Maerkisch–Oderland and its synthetic counterpart (monthly data in levels). The vertical lines represents the month of inauguration of the directly elected head of local government. The donor pool consists of the counties in the German state of Brandenburg where the head of local government was never "successfully" elected and always appointed by the council. Data on building permits and business registration are per million capita.
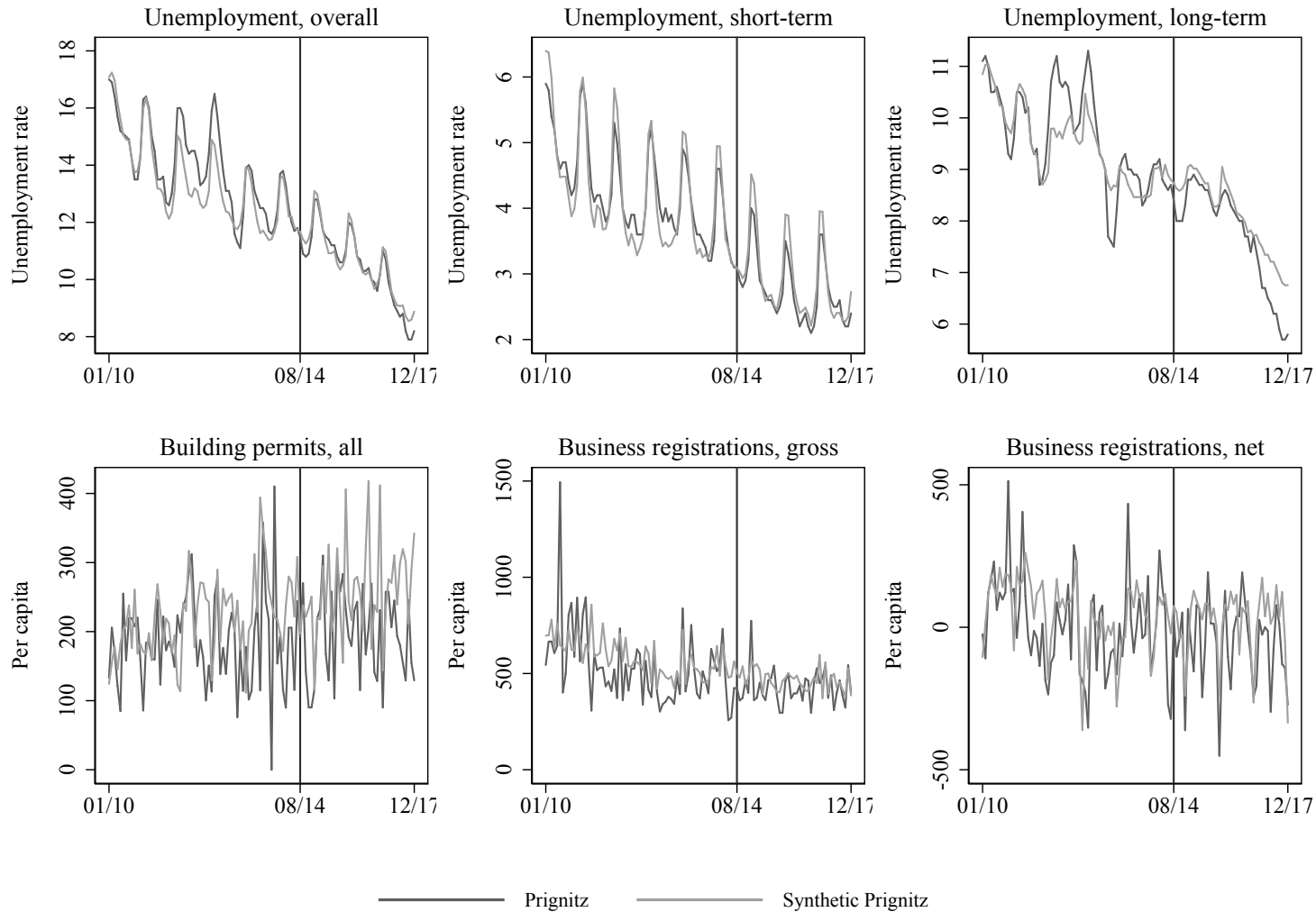
Figure 9: Oberspreewald-Lausitz

Notes: The figure shows our six dependent variables in the county Oberspreewald–Lausitz and its synthetic counterpart (monthly data in levels). The vertical lines represents the month of inauguration of the directly elected head of local government. The donor pool consists of the counties in the German state of Brandenburg where the head of local government was never "successfully" elected and always appointed by the council. Data on building permits and business registration are per million capita.

Figure 10: Potsdam-Mittelmark

Unemployment, overall

Unemployment, short-term

Unemployment, long-term

Building permits, all

Business registrations, gross

Business registrations, net

Potsdam-Mittelmark   Synthetic Potsdam-Mittelmark

Notes: The figure shows our six dependent variables in the county Potsdam–Mittelmark and its synthetic counterpart (monthly data in levels). The vertical lines represents the month of inauguration of the directly elected head of local government. The donor pool consists of the counties in the German state of Brandenburg where the head of local government was never "successfully" elected and always appointed by the council. Data on building permits and business registration are per million capita.

Figure 11: Prignitz

Notes: The figure shows our six dependent variables in the county Prignitz and its synthetic counterpart (monthly data in levels). The vertical lines represents the month of inauguration of the directly elected head of local government. The donor pool consists of the counties in the German state of Brandenburg where the head of local government was never "successfully" elected and always appointed by the council. Data on building permits and business registration are per million capita.