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What Reform of Labor Market Regulation in Denmark teaches us about the relationship between Political Responsibility and Electoral Accountability

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Abstract: Will voters hold an incumbent more electorally accountable for the quality of a policy outcome as the incumbent's political responsibility for this policy increases? To answer this question, we exploit a reform of labor market regulation in which some Danish municipal mayors were as-if randomly assigned more political responsibility for unemployment services. We find that the reform lead to immediate changes in how electorally accountable the mayor was held for unemployment services, but not in how electorally accountable the mayor was held for other policy outcomes. This suggests that the relationship between political responsibility and electoral accountability is causal, adaptive and tied to specific policies. Accordingly, voters seem to be quite judicious when assigning electoral credit or blame, moderating the extent to which an incumbent is held accountable for a specific outcome based on the extent to which this incumbent had a hand in crafting and implementing the policies which led to this outcome.

Voters hold incumbent politicians electorally accountable for how they perform in office by making electoral support for these incumbents contingent on the quality of policy outcomes (for a recent review see Healy and Malhotra, 2013). These outcomes include economic conditions (Kramer, 1971; Lewis-Beck, 1990; Van der Brug, Van der Eijk and Franklin, 2007), public service provision (Tilley and Hobolt, 2011; Burnett and Kogan, 2015) and management of other problems which might arise during an incumbents time in office (e.g., handling of natural disasters Healy and Malhotra, 2010). While this type of electoral accountability is widespread, it seems to be more prevalent in some elections than it is in others (Van der Brug, Van der Eijk and Franklin, 2007; Duch, Palmer and Anderson, 2000). In particular, a large number of studies have found that the extent to which a political system concentrates or disperses political responsibility, what is conventionally called clarity of responsibility, correlates with the extent to which voters hold governing politicians electorally accountable for the quality of policy outcomes (Powell Jr and Whitten, 1993; Whitten and Palmer, 1999; Duch and Stevenson, 2008).

This correlation between responsibility and accountability is strong and consistent across many different indicators of clarity of political responsibility. For instance, studies have shown that voters are less likely to hold incumbents accountable for the quality of the economic situation if the incumbent operates in an open economy (Hellwig, 2001; Fernández-Albertos, 2006; Hellwig and Samuels, 2007) if the incumbent is part of a government coalition (Nadeau, Niemi and Yoshinaka, 2002; Hobolt, Tilley and Banducci, 2013) or if the incumbent operates in a federal system (León, 2011; Cutler, 2008; Anderson, 2006).

Broadly speaking, these studies have been used to infer that electoral accountability will “strongly reflect the nature of policymaking in the society and the coherence and control the government can exert over that policy.” (Powell Jr and Whitten, 1993, 398). While this does tell us something important about how political responsibility relates to electoral accountability, it leaves several important aspects of the relationship underilluminated.

For one, it is not clear whether the correlation identified in the previous literature, reflects a causal or a spurious relationship between responsibility and accountability. We know from the political economy literature that political institutions, like those dispersing and concentrating political responsibility, are fundamentally endogenous (Acemoglu, 2005; Besley and Case, 2000). Accordingly, it is possible that the observational co-occurrence between responsibility and accountability is partly or completely driven by some extraneous underlying factor. Further, since previous literature has primarily focused on institutional differences in political responsibility which were established a long time ago, we do not know exactly how adaptive the relationship between electoral accountability and political responsibility is. That is, we do not know whether voters are quick to adjust to

changes in political responsibility, or whether the correspondence found in previous research is the result of a long-term process which slowly aligns responsibility and accountability. Finally, we know little about the extent to which the relationship is policy-specific: we do not know whether increasing political responsibility for an individual policy outcome leads voters to hold the incumbent more accountable for only this outcome, or whether increasing responsibility for an individual policy outcome leads voters to hold incumbents more accountable for other outcomes as well. We do not know this because previous research has primarily examined institutional differences which implicate changes in incumbents responsibility for a wide range of outcomes (e.g., federal contra unitary constitutions), making it impossible to discern whether voters are sensitive to policy-specific differences in political responsibility.

In this article, we re-examine the relationship between political responsibility and electoral accountability in a context which makes it possible to cast some light on these different and largely unexplored features of the relationship between accountability and responsibility: a reform of labor market regulation in Denmark. This reform presents a rare instance in which assignment of political responsibility for a specific policy outcome changed abruptly and exogenously. In particular, the reform increased municipal mayors' responsibility for unemployment services, and *only* for unemployment services, by making the municipalities responsible for the administration of active labor market policies – this makes it possible to examine whether the relationship between responsibility and accountability works at the level of specific policy outcomes. In 14 municipalities this reform was implemented three years before the 2009 municipal election and in the remaining 84 municipalities it was implemented after this election – this allows us to determine whether voters adapt to a recent change in political responsibility. In addition to this, the decision about which municipalities had to implement the reform before the 2009 election was taken practically unilaterally by the central government, and a closer examination of the selection process reveals that the central government plausibly chose municipalities as-if randomly – which makes it possible to recover the causal effect of this particular shock to political responsibility.

Using the Danish Municipal Election Survey (Elklit and Kjær, 2013), we show that voters in the municipalities where the reform was first implemented, the “treatment” municipalities, held the mayor more electorally accountable for the quality of unemployment services in the election following the reform. A finding which is robust to a number of different model specifications and a number of different estimation methods. Further, we find that the voters in the “treatment” municipalities did *not* hold their mayor more electorally accountable for the quality of services unaffected by the reform. This immediate adjustment in electorally accountability for unemployment services, and only these services, in response to an

exogenous and recent change in political responsibility, suggests that the relationship between political responsibility and electoral accountability is causal, adaptive and policy-specific. As such, our study provides a richer description of the relationship between political responsibility and electoral accountability than what extant literature has been able to provide.

The article extends the literature on how incumbent political responsibility shape voters assignment of electoral credit and blame for policy outcomes – a literature which has, broadly speaking, remained observational and paid little attention to changes in political responsibility for discrete policy outcomes (e.g. Powell Jr and Whitten, 1993; Whitten and Palmer, 1999; Anderson, 2000; Duch and Stevenson, 2008; Hobolt, Tilley and Banducci, 2013). Further, with a fairly consensual multi-party system (Houlberg and Pedersen, 2015), which garners less attention from voters than national politics (Elklit and Kjær, 2013), the Danish municipalities provide a hard case in a literature which is mostly focused on national politics, where sharp divides between opposition and government as well as higher levels of political attention potentially amplifies the relationship between responsibility and accountability (Duch and Stevenson, 2008; De Vries et al., 2011). By demonstrating that there is a close relationship between political responsibility and electoral accountability, this article also challenges the scope of studies showing that voters often punish politicians for outcomes they have had no or very little political responsibility for (e.g. Healy, Malhotra and Mo, 2010; Achen and Bartels, 2016; Sances, 2016). Instead, voters seem, on average, to be quite judicious when assigning credit or blame for the quality of policy outcomes.

Challenges in the Study of Responsibility and Accountability

When studying the relationship between political responsibility and electoral accountability, extant literature has typically: (1) indexed different elections according to how much political responsibility economic and political institutions assign to the incumbent up for re-election; (2) linked this index with an indicator of how electorally accountable the incumbent was held for a specific policy outcome – most often a correlation between support for the incumbent and either a subjective indicator (e.g., perceptions of the national economy) or an objective indicator (e.g. economic growth) of the quality of this outcome. Using this basic methodology, first sketched out by Powell and Whitten in their seminal 1993 article, a number of scholars have explored the relationship between responsibility and accountability using different indices of incumbent responsibility (e.g. Paldam, 1991; Whitten and Palmer, 1999; Nadeau, Niemi and Yoshinaka, 2002; Anderson, 2006; Hellwig and Samuels, 2007; Duch and Stevenson, 2008; Hobolt, Tilley and Banducci, 2013; Carlin

and Singh, 2015), different outcomes (Tavits, 2007; De Vries, Edwards and Tillman, 2010; Tilley and Hobolt, 2011) and different types of elections (Ebeid and Rodden, 2006; Berry and Howell, 2007). Broadly speaking, these studies have found that in elections where incumbents have more responsibility for policy outcomes they are held more electorally accountable for the quality of these outcomes.¹

This wealth of thorough and innovative studies have gotten us a long way when it comes to understanding how political responsibility shapes electoral accountability. However, to the extent that one wants to draw more detailed inferences about the relationship between political responsibility and electoral accountability from the extant literature, one faces several challenges. In particular, and as mentioned above, based on previous studies, one would have a hard time evaluating whether political responsibility has a causal effect on electoral accountability, a hard time evaluating how quickly voters adapt to changes in political responsibility, and a hard time evaluating the extent to which voters are sensitive to differences in how politically responsible incumbents are for specific policy areas. That is, one would have a hard time evaluating whether the relationship between responsibility and accountability is causal, adaptive and policy specific.

Below, we explicate, in turn, which particular challenges the existing literature would face if it were to make inferences about each of these three aspects of the relationship between responsibility and accountability. The reason we highlight these aspects, and not others, is that our study can contribute to the existing literature by helping us get at exactly these aspects. Accordingly, following our discussion of the challenges faced by the existing literature, we sketch out how our study meets these challenges, helping us characterize the relationship between responsibility and accountability more fully.

Causality

Previous studies have almost exclusively analyzed the relationship between responsibility and accountability by looking at the correlation between the presence of institutions which manipulate incumbent responsibility and the extent of electoral accountability (Duch and Stevenson, 2008). At the same time, however, most researchers agree that one can rarely estimate the causal effect of institutions using standard observational studies of institutional differences (Meyer, 1995; Besley and Case, 2000; Aghion, Alesina and Trebbi, 2004), because institutions are gen-

¹Another set of studies have examined which psychological processes lead voters to attribute certain outcomes to incumbent politicians (e.g. Gomez and Wilson, 2001; Tilley and Hobolt, 2011). While this literature also examines responsibility in relation to retrospective voting, it does so in a very different way than the literature discussed here. As such, in this more psychological literature, responsibility is a subjective belief voters hold, whereas in the literature described above, responsibility is an objective political condition, defined by the of mix political and economic institutions which define the nature of policy-making in a specific polity.

erally endogenous (Acemoglu, 2005; Przeworski, 2004). Accordingly, it is possible that the relationship between responsibility and accountability is confounded.

This is a possibility, which looms large if one takes a close look at some of the specific institutions, which have been used to get at the relationship between responsibility and accountability. For instance, a number of studies have shown that a country's economic openness is negatively correlated with how electorally accountable its executive is held for the economic situation (Hellwig, 2001; Fernández-Albertos, 2006; Hellwig and Samuels, 2007; Duch and Stevenson, 2008). These studies argue that this makes sense because more economic openness also means less executive responsibility for economic outcomes. However, economic openness is also known to be correlated with the extent to which countries provide social protection to those who are unemployed (Cameron, 1978; Rodrik, 1996), and we know from studies of economic voting, that economically vulnerable voters are more likely to punish (or reward) governing politicians for the quality of the economic situation (Singer, 2013; Fossati, 2014). Accordingly, when researchers find that voters are less likely to hold their government electorally accountable for the economic situation in countries with an open economy, this might be due to the fact that in open economies voters usually have extensive social protection, and therefore the voters do not need to worry that much about short-run economic fluctuations.

Another example of a potentially endogenous institution can be found in studies demonstrating that single party governments are more likely to be held electorally accountable for the economic situation than multi-party governments (Anderson, 2000; Nadeau, Niemi and Yoshinaka, 2002; Hobolt, Tilley and Banducci, 2013). This might be because it is harder to assess who is responsible for economic outcomes in a coalition government, but it might also be a result of another difference between coalition and single party governments. A number of studies have documented that partisans generally refrain from holding their own party electorally accountable for economic outcomes (Bisgaard, 2015; Kayser and Wlezien, 2011; Rudolph, 2006), attributing any poor performance to some other factor than the quality of their preferred party (Tilley and Hobolt, 2011). Coalition governments are typically larger, electorally speaking, than single party governments. Accordingly, there will probably also be more voters who feel attached to a government party in a coalition government. When comparing the level of electoral accountability for single and multi-party governments, one may therefore be picking up the effect of differences in the number of government partisans rather than differences in levels of incumbent responsibility.

These examples are not exhaustive in the sense that they cover all the types of institutions which have been used to index how politically responsible the incumbent is for policy outcomes. Even so, we believe these examples illustrate that there is a more general challenge in the existing literature, when it comes to identifying

the causal effects of political responsibility on electoral accountability.

Adaptiveness

Another interesting feature of the relationship between responsibility and accountability is how adaptive it is. That is, whether voters respond swiftly to short-run changes in political responsibility, continually adjusting how accountable incumbents are held for various outcomes, or whether this adjustment process works more slowly.

Previous studies have not paid much attention to the question of adaptiveness, mainly focusing on differences in incumbent responsibility which rarely change or which change slowly and incrementally (e.g., Ebeid and Rodden, 2006; Duch, Palmer and Anderson, 2000). This makes it hard to know how voters respond to sudden shifts in political responsibility. Some studies do examine more dynamic aspects of political responsibility, focusing on institutions which allocate different degrees of responsibility for policy outcomes to incumbent politicians over time, within the same political unit (e.g. Nadeau, Niemi and Yoshinaka, 2002; Carlin and Singh, 2015). By focusing on this type of time-sensitive variation in assignment of political responsibility, such studies could potentially tell us something about how adaptive the relationship between responsibility and accountability is. Yet in spite of this, these studies have rarely leveraged the dynamic nature of these institutions, when examining how they affect electoral accountability. Instead they pool, either completely or partially, the within and between unit variation, making it impossible to get at whether the time-sensitive (within unit) variation in political responsibility correlates with electoral accountability.

Adaptiveness is potentially quite important, because if voters are not adaptive, they risk holding their incumbent accountable for the quality of some outcome the incumbent is no longer responsible for – or they risk failing to hold the incumbent to account for the quality of an outcome the incumbent has recently become responsible for. Also a lack of adaptiveness can give incumbents an incentive to neglect policy areas where they have recently become more politically responsible, because they know they will not be held immediately accountable for performance in this area by the electorate.

Policy-specificity

Extant literature has primarily examined the relationship between responsibility and accountability in terms of economic and political institutions which affect how politically responsible incumbents are for a large set of policy outcomes. For instance, constitutional design (Anderson, 2006; Carlin and Singh, 2015), which broadly shapes incumbents' ability to affect economic and social outcomes, or different

parliamentary practices (Powell Jr and Whitten, 1993; Whitten and Palmer, 1999; Nadeau, Niemi and Yoshinaka, 2002), which shape incumbents' executive and legislative discretion in all policy areas.

This focus on responsibility for a diverse and not clearly demarcated set of outcomes makes it difficult to assess how policy-specific voters are as they link political responsibility to electoral accountability. That is, we do not know whether voters link responsibility to accountability at the level of individual policy outcomes, weighing each outcome according to how responsible the government is for this particular outcome, or whether voters link responsibility and accountability at a more aggregate level, using different policy outcomes to form an overall evaluation of how the country is doing, and then weighing this evaluation based on how responsible the incumbent typically is for policy outcomes in general.²

Previous literature cannot discriminate between a policy-specific and a more general relationship, because it looks at differences in incumbent responsibility for a diffuse set of policy outcomes. Instead, if one wants to make inferences about policy-specificity, one would need to examine a difference in political responsibility which only covered a discrete set of policy outcomes. Then it would be possible to examine policy-specificity empirically by detailing whether voters *only* differed in how electorally accountable they held the incumbent for the specific policy outcomes for which there was a difference in the incumbents' political responsibility, or whether electoral accountability for other outcomes was affected as well.

The Contribution of This Study

By focusing on a reform which changed political responsibility for a specific policy, our study enables us to get at whether the relationship between responsibility and accountability is *policy-specific*. That is, we can examine whether voters only hold their incumbent more electorally accountable for the policy outcome affected by the reform, or whether voters hold their incumbent more electorally accountable for other policy outcomes as well. Further, since we examine the effect of this reform on electoral accountability at the election immediately following its implementation, any effect that we do find will suggest that the relationship between responsibility and accountability is reasonably *adaptive*. Finally, as we discuss in more detail below, the change in political responsibility which followed from this reform was exogenous to existing and expected levels of electoral accountability,

²It is not theoretically straightforward which of these approaches to linking responsibility and accountability voters will adopt. On the one hand, adopting a policy-specific strategy seems to be more rational if one simply wants to learn more about the incumbents competence (for evidence of this see appendix of Achen and Bartels, 2016). On the other hand, voters are often interested in employing heuristics and mental shortcuts (Downs, 1957; Kuklinski, Quirk et al., 2000). One such mental shortcut might be to link responsibility and accountability at an aggregate rather than a policy-specific level.

making it possible to identify the *causal* effect of this change in political responsibility.

Analyzing the extent to which the relationship between responsibility and accountability can be described as causal, adaptive and policy-specific is important because it tells us something about how adept voters are at electing competent politicians, who are willing and able to provide the kind of policy outcomes voters prefer. As such, we know that, in general, voters have an easier time electing more competent politicians if they only hold these politicians electorally accountable for the outcomes which they are actually responsible for (Anderson, 2006; Duch and Stevenson, 2008; Ashworth, 2012; Achen and Bartels, 2016).³ If the relationship between responsibility and accountability is causal, adaptive and policy-specific then it will be more likely that voters only hold their incumbent electorally accountable for things their incumbent was, in fact, politically responsible for. If the relationship is causal, then voters respond to changes in responsibility by holding incumbents more electorally accountable. If the relationship is policy-specific, then voters are more likely to shift their attention away from policy outcomes which incumbents have little responsibility for, and towards outcomes incumbents have more responsibility for. If the relationship is adaptive, then voters are more likely to act on the current distribution of political responsibility when holding incumbents accountable. In summary, our study contributes by examining aspects of the relationship between political responsibility and electoral accountability, which existing literature has not been able to study effectively, and which tells us something interesting about the quality of electoral decision making.

Research Design: Reform of Labor Market Regulation

In 2006 the administrative boundaries of Denmark were fundamentally redrawn. Both in terms of geography as 271 municipalities became 98, but also in terms of policy responsibilities, as the municipalities gained new responsibilities and gave a few away. Unfortunately, from a research standpoint, most of this extensive reform was implemented in all municipalities at the same time, making it hard to test how it affected the municipalities (although aspects of the reform have been leveraged in other contexts: Lassen and Serritzlew 2011; Bhatti and Hansen 2011; Blom-Hansen, Houlberg and Serritzlew 2014).

One part of the reform, however, was not implemented at once, but in two steps: a reform of labor market regulation, which transferred political responsibility for unemployment-services from various agencies to municipal mayors. This reform was first implemented in 14 municipalities (out of 98) in the beginning of 2007 and

³This is not to say that a singular focus on what the incumbent is responsible for is a panacea for effective democratic control. A point we return to in the conclusion.

then implemented fully in 2010 (Ministry of Employment 2010; see figure 1 for a time-line).⁴.

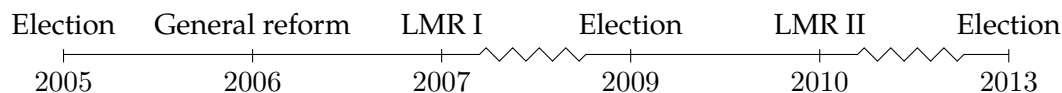


Figure 1: Labor Market Reform (LMR) timeline. LMR I was in the 14 treatment municipalities, LMR II was the remaining municipalities.

This reform provides a unique opportunity to investigate how voters react when incumbents get more political responsibility, as we can use the municipal election which took place in November 2009 to compare the beliefs and behavior of the voters in the 14 municipalities where the mayor got more responsibility for unemployment services before this election – ‘treatment’ municipalities – with the beliefs and behavior of the voters in the 84 municipalities where the mayor did not get more responsibility until 2010 – ‘control’ municipalities.

Following the discussion above about the relationship between responsibility and accountability, we are interested in finding out whether voters in the treatment municipalities held their mayor more electorally accountable for unemployment services than voters in the control municipalities. As detailed above, this will help us understand the extent to which the relationship between political responsibility and electoral accountability is causal, adaptive and policy-specific.

To do this, however, we need to make two assumptions about the reform. One is about the nature of the reform: that the reform exclusively affected municipal mayors political responsibility for unemployment services. The other is about the assignment of municipalities to early implementation of the reform (i.e. assignment to treatment): that assignment was independent of extant and potential levels of electoral accountability.⁵ Below we explain why both of these assumptions are reasonable to make when analyzing the effect of the reform on electoral accountability and then we present the data used to estimate this effect.

⁴The municipalities were: Odense, Esbjerg, Vejle, Næstved, Herning, Sønderborg, Holbæk, Helsingør, Skanderborg, Høje-Taastrup, Thisted, Brønderslev-Dronninglund, Rudersdal and Rebild (Order 1400, 2006).

⁵These assumptions roughly correspond to the exclusion and independence (or as-if randomness) assumptions laid out by Dunning (2012) and Gerber and Green (2012). Along with the assumption of non-interference between units, which is less relevant here because political responsibility could not spillover to neighboring municipalities, they constitute the central assumptions needed to make causal inferences.

The Reform only Affected Responsibility for Unemployment Services

The reform of labor market regulation made municipalities politically responsible for helping so-called “insured workers” get back to work if they lost their job (i.e. the three-fourths of all workers who were members of an unemployment insurance fund). Before the reform was implemented the national government, along with the unemployment insurance funds, were responsible for the insured workers, whereas the municipalities were responsible for uninsured workers. The reform removed the unemployment insurance funds and the government from the equation, and gave each municipality unilateral responsibility for helping all those who were out of a job in that particular municipality (Order 1400, 2006; Eskelinen 2008). In particular, after the reform, municipalities were expected to help employers looking for employees, find and finance retraining for all unemployed workers, assist unemployed workers with special needs, and they were expected to do so while maintaining an efficient organization.

While the reform ostensibly only had an effect on who was politically responsible for unemployment services, reforms tend to be messy and have a very diverse set of long and short term consequences. In light of this, one might suspect that assignment to implementation of the labor market reform had important side-effects which could pose threats to the inferences we want to make below. As one examines the nature of the reform in more detail, however, potential side-effects are not forthcoming. For one, the reform presents a very “clean” change in political responsibility for a specific policy; no responsibilities outside the area of unemployment services were conferred and no alternative regulation was implemented as part of the reform (Ministry of Employment 2010; Act 483, 2009). Further, unemployment services constitutes an important part of public service provision in Denmark, whose labor market policy has long been premised on the idea that the day to day interaction with the unemployed individual is important for reducing structural unemployment (Torfing, 1999). An idea which is mirrored in spending priorities (OECD, 2016). For instance, according to the OECD, expenditures towards unemployment services (i.e. active labor market policies) represented 1.82 percent of the Danish GDP in 2013 compared to just 0.23 percent in the United Kingdom. Accordingly, as we study differences between voters in municipalities which either did or did not implement the reform, it seems more likely that any differences we do find is the result of a change in political responsibility for an important and expensive policy area, and less likely that any such differences is the result of some small, not apparently observable, change which accompanied the reform.⁶

⁶See section S1 of the supplementary materials for some additional evidence of this ‘exclusion’ assumption.

The Change in Responsibility Was Exogenous

If we want to draw causal inferences based on the selection of some municipalities to early implementation of the labor market reform, then this selection process should be independent of existing and potential levels of electoral responsibility (Gerber and Green, 2012). If it is not, we risk confounding the effect of the reform with the effect of being a ‘type’ of municipality which is more likely to get selected for early implementation. Three factors surrounding the selection process makes it likely that the selection process was, in fact, independent of existing and potential levels of electoral accountability.

First, the selection process was confined to municipalities within a single country at a single time. This makes it possible to rule out a host of possible confounders, such as macro-social developments and country-specific factors like culture and history.

Second, the decision about which municipalities were assigned to implement the reform early was made by the central government rather than the municipalities themselves, eliminating a substantial threat to causal inference: self-selection. In particular, employees at the ministry as well as the Minister prepared a list of municipalities, which was then approved by the parties which voted for the reform in Parliament (Ministry of Employment, 2006).

Third, and perhaps most important, it seems likely that the Ministry’s assignment of municipalities to early implementation was independent of the municipalities’ existing or potential levels of electoral accountability for unemployment services. For one, it is not clear that the Ministry would have known what the level of electoral accountability for unemployment services were in the individual municipalities. In addition to this, even if the Ministry knew the levels, it is not clear that they would have had an incentive to assign municipalities to early implementation based on their level of accountability for unemployment services. There could feasibly have been an incentive to pick municipalities which generally fared better when it came to handling unemployment services, picking municipalities which were likely to make the reform look like a success (although we do not find any such imbalances between early and late implementers, cf. table 1), but it is unclear why the Ministry should be interested in implementing the reform in places where the level of electoral accountability for unemployment services was higher (or lower) than average. Finally, even if the ministry did know, and for some reason favored a particular type of municipality, which for some reason had higher levels of electoral accountability, there were political forces at work which would arguably have muted any political favoritism.

When the reform was being negotiated, several actors were highly critical of giving the municipalities responsibility for unemployment services. As such, both

large unions, employer organizations as well as the minority government's usual ally in parliament, the Danish People's Party, were doubtful that the municipalities were up to the task (Kristensen, 2008, 88). Accordingly, there was pressure on the Ministry of Employment not to 'cherry-pick' municipalities based on past performance. As a person close to the selection process expressed it: "We were allowed to send up a test-balloon, but it was extremely important that they [the municipalities] were balanced."⁷ This sentiment is also mirrored in a press statement published by the Ministry of Employment, explaining how the 14 municipalities had been selected. In the statement, the Minister of Employment was quoted as saying that the goal had been to select "big as well as small municipalities, in cities as well as in rural areas". More generally, the Minister said "that the goal was to spread them out across the country" (Ministry of Employment 2006, author's translation). As such, specific types of municipalities were not targeted in the selection process. This is confirmed by looking at how the chairman of the organization Local Government Denmark, an organization representing the municipalities, reacted to the selection process. He said that the ministry's decision insured that "a broad cross section of municipalities [are represented], both size-wise and geographically." (Ritzau, 2006, author's translation). Additional evidence suggesting that the selection process was not politically charged can be found if one looks at the reaction to the ministry's decision among those who were very critical of the reform; the unions and the employer organizations. As far as we have been able to determine, none of these political organizations officially criticized the government for having selected a biased or in another way problematic set of municipalities for early implementation.⁸

Taken together, these factors suggest that the selection of the 14 early implementing municipalities was *independent* of existing patterns of accountability. That is, based on the evidence presented here, there is reason to believe that the change in political responsibility for unemployment services was exogenous or as-if random. An assertion we revisit below, presenting quantitative evidence to supplement the more qualitative evidence presented here.

Data and Measuring Electoral Accountability

To analyze the consequences of the reform we utilize the Danish municipal election survey (Elklit and Kjær, 2013). The 2009 election survey is of special interest, since this is where electorates in the treatment and control municipalities were governed by mayors with different levels of responsibility for unemployment services

⁷Interview with Jan Handeliowitz, former employee at the Ministry of Employment, authors translation.

⁸To determine this we examined all newspaper stories mentioning the reform in the month following the announcement of the assignment of municipalities to early-implementer status in the three major Danish broadsheets (Jyllands Posten, Politiken and Berlingske).

(cf. figure 1). However, we also use the 2013 and 2005 surveys to test whether the electorates of the treatment and control municipalities differed before treatment (2005), and after they both were treated (2013). Respondents to the municipal election survey were recruited within six weeks of the municipal election using stratified random sampling in order to ensure that at least 30 respondents in each of the 98 Danish municipalities participated in the survey. The surveys were conducted partly via a web-survey and partly over the phone (for details about the surveys see Elklit and Kjær, 2013).

To measure the extent to which the mayor was held electorally accountable for unemployment services we examine the correlation between voters evaluation of unemployment services and their propensity to support the municipal mayor (a typical measure of electoral accountability cf. Stevenson and Duch, 2013; Duch and Stevenson, 2008; Carlin and Singh, 2015). We interpret a higher correlation as evidence that the mayor is being held more electorally accountable for the quality of unemployment services. To assess voters' evaluation of unemployment services we use the following survey item: "How satisfied or unsatisfied are you in general with the municipality's efforts towards the unemployed?" Answers were recorded on a five-point Likert scale going from "very unsatisfied" to "very satisfied". To measure support for the incumbent mayor we look at whether respondents said they voted for the incumbent mayor's party at the municipal election.⁹ We exclude respondents who did not vote and respondents who could not remember which party they voted for from the analysis. We use electoral support for the mayoral party to measure support for the mayor, because voters do not elect mayors directly in Denmark, but elect members of a city council, and the city council then appoints a mayor right after the election (Houlberg and Pedersen, 2015).¹⁰

This measure is not perfect, and will probably contain some measurement error. In particular, the measure might also capture, at least in part, the extent to which voters form beliefs about unemployment services based on who they vote for (so called motivated reasoning, cf. Tilley and Hobolt, 2011). Accordingly, our measure might overestimate the *level* of electoral of accountability in each municipality. Even so, we will still be able to get an unbiased estimate of the *difference* between treatment and control (i.e. the difference in the correlation between support for the mayor and evaluation of unemployment services), since this measurement error does not vary across treatment and control municipalities (King, Keohane

⁹The survey-item on unemployment services was not included in the 2013 survey. The structural reform in 2005 meant that a majority of municipalities had been amalgamated, and therefore most voters did have an incumbent mayor in the 2005 election. As such, we can only effectively measure electoral accountability for unemployment services in the 2009 election, and not at the 2005 and 2013 elections.

¹⁰Municipal elections in Denmark are held every four years in November, the electoral system is proportional representation, and in most municipalities there is a multi-party system which mirrors the national party system.

and Verba, 1994, chap. 5).¹¹

We also use a number of other survey items in our analysis, which index, among other things, voters' beliefs about the clarity of political responsibility in the municipality. All survey items used in the analysis are described in section S2 of the supplementary materials. Descriptive statistics on all variables can be found in section S3 of the supplementary materials.

Analysis

The key part of our analysis will be estimating the effect of being in a treatment municipality, where the mayor has recently gotten more political responsibility for unemployment services, on how electorally accountable voters hold their mayor for the quality of unemployment services. In addition to this, we will also explore whether mayors in the treatment municipalities are held more electorally accountable for the quality of other services like housing policy and elderly care. We will examine the robustness of these findings as we go along, and after the main analysis we will interrogate the viability of an alternative explanation for our findings: that differences in political attention, not political responsibility, is what is actually driving our results.

Before engaging in these analyses, however, we do a balance-test and a manipulation check, demonstrating that assignment to early implementation of the reform (i.e. treatment) was plausibly as-if random, and that being assigned to early implementation had an impact on voters' beliefs about the distribution of political responsibility.

Balance Test and Manipulation Check

In table 1 we compare treatment and control municipalities, before they were treated, on a number of individual-level and municipality-level variables. For the individual-level variables there are no statistically meaningful differences. Most important, there are no differences across treatment and control on several variables, which are likely to be highly correlated with the extent of electoral accountability, such as knowledge about municipal powers and interest in local politics (Vries and Giger, 2014). This is consistent with the qualitative evidence suggesting that implementation of the reform was assigned to municipalities as-if randomly. As such, if particular types of municipalities had a higher probability of being assigned to early implementation it seems likely that we would be able to identify systematic dif-

¹¹In the section 'Alternative Explanations' we empirically examine the assumption that this type of measurement error is likely to vary across treatment and control municipalities, and find no evidence of this.

ferences across treatment and control municipalities. We find practically no such differences.

Turning to the municipal-level variables, these paint roughly the same picture. Across the different variables only one shows significant differences between the two groups – the treatment municipalities had a slightly larger number of inhabitants than the control municipalities. Even so, examining the standardized differences for the other municipal-level variables there does seem to be some substantial, though statistically insignificant, differences across treatment and control. This is not that surprising, since the number of observations at the municipal level is relatively low, which means that the random variation between treatment and control could be relatively high. Nonetheless, these random imbalances might skew the results one way or another. Accordingly, when analyzing differences between treatment and control municipalities, we will look at whether controlling for the municipal-level variables, which have the largest standardized differences, changes our results (i.e. proportion of national government voters/mayors, female office seekers, non-western citizens and number of inhabitants).

Table 1: Were treatment and control municipalities different?

Variable	Treatment	Control	Std. dif.	p-value	n
<i>Individual-level variables (2005)</i>					
Informed	0.44	0.45	-0.07	0.16	1996
Interested	0.63	0.64	-0.06	0.31	1884
Unemployment performance	0.47	0.48	-0.03	0.68	1454
Knowledge about municipal powers	0.70	0.71	-0.05	0.37	2011
Elderly performance	0.64	0.67	-0.09	0.15	1534
Housing performance	0.74	0.75	-0.05	0.40	1944
Ideology	0.69	0.70	-0.00	0.97	2011
Apathy	0.14	0.15	-0.00	0.93	1988
Obligation	0.95	0.96	-0.07	0.25	2000
Satisfaction with municipal democracy	0.52	0.52	-0.02	0.71	1975
Pivotality	0.45	0.46	-0.00	0.95	1875
<i>Municipality-level variables (2006)</i>					
Population density (log)	2.18	2.25	-0.12	0.60	98
Citizens with more than high-school education (pct.)	23.10	21.54	0.19	0.51	98
Unemployment rate (pct.)	2.16	2.31	-0.14	0.48	98
Citizens with non-western origins (log)	2.39	2.32	0.27	0.27	98
Female municipal office-seekers (pct.)	28.13	30.39	-0.46	0.13	98
Municipal tax rate (pct.)	24.55	24.77	-0.23	0.31	98
Social transfers (log)	4.13	4.13	0.03	0.91	98
Services contracted out (0-100 scale)	21.90	22.78	-0.22	0.35	98
Spending on active labor market policies (log)	3.21	3.20	0.06	0.77	98
Inhabitants (log)	4.82	4.58	0.69	0.00	98
Proportion of national government voters	0.42	0.38	0.31	0.17	98
Proportion of national government mayors	0.64	0.45	0.38	0.20	98

Individual-level variables from the 2005 municipal election survey. See Appendix A for detailed description.

Municipal-level variables taken from Statistics Denmark. p-values from difference in means test.

National government voters are voters who voted for parties in government at the municipal election in 2005.

Standardized difference computed as difference in means divided by standard deviation in the control group.

Another relevant issue is whether voters in the treated municipalities actually

updated their beliefs about the extent of their mayors responsibility for unemployment services. That is, whether the “manipulation” actually registered with the voters. Unfortunately, there is no question in the municipal election survey which directly probes voters’ beliefs about the extent of their mayor’s responsibility for unemployment services. Instead, we use two questions which ask respondents about how much political responsibility local politicians have for conditions in the municipality in general. The first of these question asks voters whether the mayor and other local officials (rather than national politicians) had the primary responsibility for how the municipality had developed in the last four years. The second question asked voters about the extent to which the mayor had an effect on the well-being of the municipality. Analyzing voters’ responses to these questions, we find that the voters in the treatment municipalities believed that local politicians were more responsible for and had greater a influence on conditions in the municipality ($p < 0.05$; see section S4 of the supplementary materials for details on this analysis).

Electoral Accountability for Unemployment Services

In order to identify the effect the reform had on electoral accountability for unemployment services, we zoom in on the 2009 municipal election, comparing the extent to which voters evaluation of unemployment services shaped the propensity to vote for the mayor’s party in the treatment and the control municipalities.

We begin by specifying a regression model, which sets the probability that the respondent voted for the mayoral party as a logistic function of the respondent’s evaluation of the municipality’s performance in the area of unemployment services, an indicator variable determining whether the respondent lived in a treatment or a control municipality as well as an interaction between the two. We also include a small number of other variables in the model: performance towards the elderly, performance towards housing management, as well as beliefs about how powerful the municipal administration is vis-a-vis the local politicians. These variables are meant to reduce the error term of the model, and meant to control for trends in performance and beliefs about how responsible local politicians are for economic and social outcomes. Since the treatment was assigned to municipalities, we cluster standard errors at the municipal level when estimating the model. In the first column of table 2, we present the key estimates from the logistic regression.

The primary estimate of interest is the interaction between treatment and unemployment performance. This coefficient indicates whether voters’ evaluation of the quality of unemployment services was more or less closely tied to the propensity to vote for the mayoral party in the treatment municipalities. The logit-coefficient of the interaction is statistically significant ($p < 0.05$) and positive. This means that

unemployment performance mattered more in the municipalities where the mayor had more political responsibility for unemployment services. The remaining coefficients in the model have the expected sign, and, apart from housing performance, are all statistically significant.

A similar picture emerges if one derives the average marginal effects (AMEs) of unemployment performance for treatment and control municipalities (for a description of the statistical properties of AMEs see Hanmer and Ozan Kalkan, 2013). The AMEs are reported at the bottom of column one in table 2, and they reveal that in the control municipalities, the result of going from one end of the unemployment performance scale to the other end is an average increase in the probability of voting for the mayoral party of 17 percentage points. In the treatment group the same change leads to an increase of 36 percentage points. A difference which is statistically significant ($p < 0.05$) and quite large: the AME for a voter in the treatment municipalities is twice that for a voter in the control municipalities.

Figure 2 graphs the interaction by deriving predicted probabilities of voting for the mayor from the logistic regression across treatment status and unemployment performance. In the left panel we plot the relationship between unemployment performance and the probability of voting for the mayoral party in the control municipalities. In the right panel we plot the same relationship for the treatment municipalities, where they mayor had more responsibility for unemployment services. These graphs tell the same story as the regression model; the quality of unemployment services had a larger role to play in the treatment municipalities.

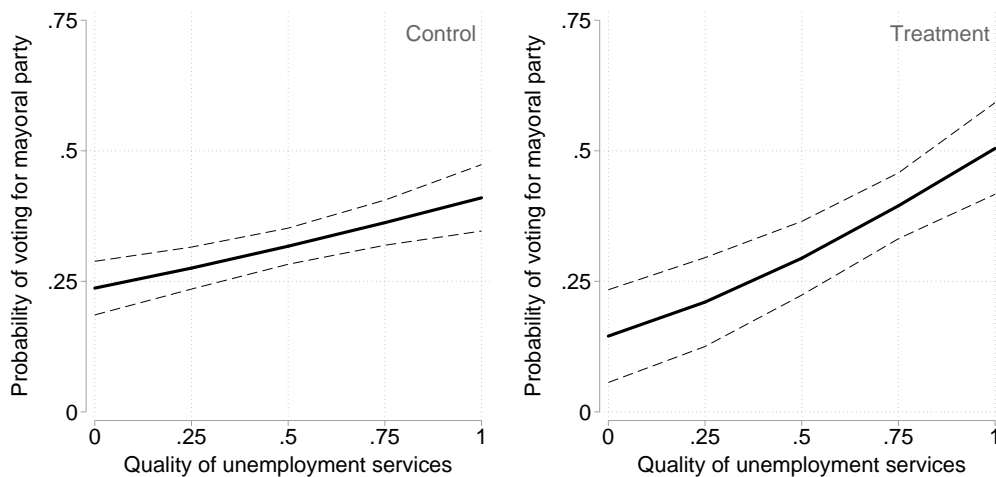


Figure 2: Predicted probability of voting for the mayoral party across beliefs about the quality of unemployment services (i.e. unemployment performance) in treatment and control municipalities. The predicted probabilities are derived from the logistic regression model displayed in column one of table 2. The dashed lines are 95 pct. confidence intervals.

Table 2: Logistic regression of probability of voting for the mayoral party

	(1)	(2)	(3)	(4)
Unemployment performance	0.82*	0.83*	0.94*	0.85*
	(0.23)	(0.23)	(0.36)	(0.35)
Treatment	-0.61	-0.59	-0.52	-0.63
	(0.39)	(0.40)	(0.43)	(0.41)
Treatment * Unemployment performance	1.00*	0.98 ⁺	1.25*	1.33*
	(0.51)	(0.52)	(0.62)	(0.58)
Administration controls municipality	-0.36 ⁺	-0.46*	-0.48*	-0.43 ⁺
	(0.21)	(0.21)	(0.24)	(0.24)
Elderly performance	0.76*	0.81*	1.03*	1.03*
	(0.23)	(0.25)	(0.33)	(0.30)
Housing performance	0.38	0.31	-0.13	0.04
	(0.29)	(0.30)	(0.38)	(0.39)
Sociodemographic controls		✓	✓	✓
Political controls			✓	✓
Municipal level variables				✓
AME (Control)	0.17	0.17	0.10	0.09
	(0.05)	(0.05)	(0.04)	(0.04)
AME (Treatment)	0.36	0.35	0.26	0.25
	(0.08)	(0.08)	(0.06)	(0.05)
Difference (T-C)	0.19	0.18	0.15	0.15
<i>p-value of difference</i>	0.03	0.04	0.04	0.01
Pseudo R ²	0.04	0.05	0.41	0.42
Log likelihood	-921	-894	-561	-552
Observations	1522	1500	1500	1500

Robust standard errors clustered on municipality in parentheses. ⁺ $p < 0.1$ * $p < 0.05$

These analyses indicate that there is a causal relationship between political responsibility and electoral accountability. As such, in the municipalities where the mayor was exogenously assigned more responsibility for unemployment services, the mayor's party was held more electorally accountable for the quality of these services. The analyses also indicate that there is quite an adaptive relationship between responsibility and accountability. As such, the reform which changed how responsible the municipal mayor was for unemployment services was implemented just three years prior to the election analyzed here but in spite of this, voters did adjust how accountable they held their mayor.

How sensitive are these conclusions to the specific statistical procedures used to obtain them? To answer this question, we run some additional analyses, adding more controls to the regression model estimated above, trying out different ways of estimating the model, and re-estimating the model using a difference-in-difference approach.

Controls: We begin by extending the set of control variables used in our logistic regression model. First, we include a number of socio-demographic variables – age, gender education and occupational status (including whether the respondent was unemployed) as well as two variables which measure the extent to which people used local media during the election campaign. The controls are included in the model estimated in the second column of table 2. Introducing these controls only

change our estimates slightly, and the difference in AMEs between the treatment and control municipalities remains statistically significant ($p < 0.05$).

Next, we include a set of political variables. To gauge partisanship, we include a control for whether the respondent would vote for the mayor's party if a *national* election was held tomorrow, and a variable indicating whether the respondent shares the ideological orientation of the mayor. We also include a variable indicating whether the mayor was from the same party as one of the governing parties, something which might make the mayor susceptible to blame for unemployment services (Cutler, 2008). These political controls are included in the third column of table 2. Introducing the political controls reduces the *overall* AME of unemployment performance, but does not substantially reduce the *difference* between the treatment and control municipalities, which remains statistically significant.

Finally, we include a battery of municipal-level control variables, for which there were some statistically insignificant, but substantial imbalances across treatment and control. These are the proportion of national government voters, the proportion of female office seekers, log number of non-western citizens and the log number of inhabitants. We include these controls in column four of table 2. Including these controls do not change the result in important ways, as the interaction effect and difference in AMEs remain statistically significant. A more detailed descriptions of the variables included as controls can be found in section S2 of the supplementary materials.

Alternative Estimation Methods: We also examined whether the results are sensitive to alternative ways of estimating the interaction effect and its sampling variability. Specifically, we implemented a multilevel logit model and a form of randomization inference. We present these analyses in section S5 of the supplementary materials. Using these alternative estimation methods the difference in the AME of unemployment performance between treatment and control municipalities remains statistically significant ($p \approx 0.05$ for multi-level results, $p < 0.1$ for randomization inference).

A Difference in Difference approach: We also examined whether similar results could be obtained using a difference-in-difference approach. Here we pooled data from the 2005 and 2009 municipal election survey, estimating a model which examines whether the difference between treatment and control municipalities with respect to accountability for unemployment performance is different across the two elections. In effect this controls for any pre-treatment differences there might have been in levels of electoral accountability for unemployment services.

Conducting this analysis is complicated by a few factors. Even though the key unemployment performance and vote intention questions was asked in both surveys, there is not a large overlap between the datasets when it comes to the control variables used in the analyses of the 2009 election. As such, we cannot estimate

a model with as large a number of controls, however, this problem is somewhat offset by the fact that the difference-in-difference approach does control for any pre-treatment differences between treatment and control municipalities. Another and more serious challenge to including the 2005 data relates to the fact that some municipalities were in the process of being amalgamated due to the larger structural reform which was implemented in 2006 (cf. figure 1). As such, almost half of the respondents voted in a new amalgamated municipality, which was different from the one where their incumbent mayor had been elected, blurring patterns of accountability. We address this by defining the dependent variable in '05 as voting for the party which had the mayoralty in the voter's existing (old) municipalities, however, the amalgamations do impede the strength of the analysis.

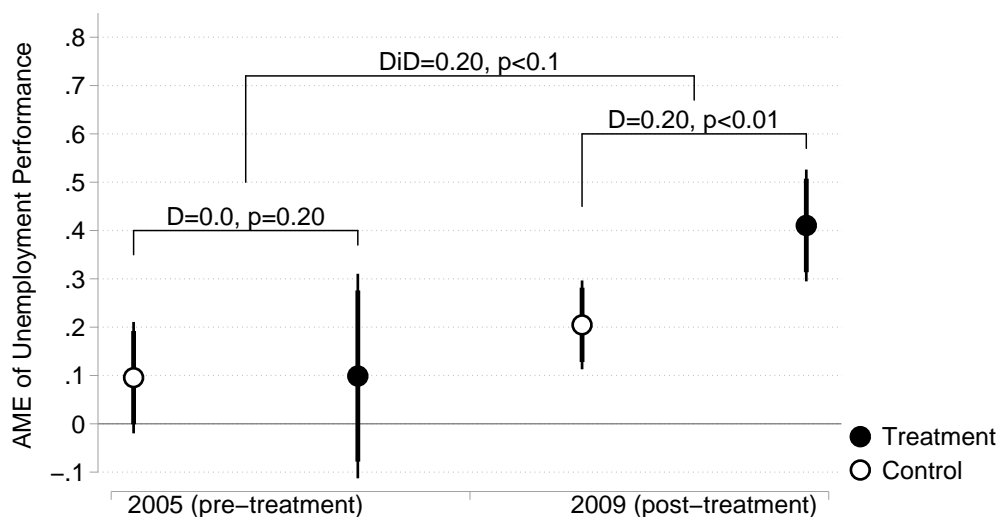


Figure 3: Average Marginal Effects of unemployment performance on probability of voting for the mayoral party across treatment status and time period. Derived from logistic regression model described in the text; McFadden $R^2 = 0.032$, $n = 2,582$. Wald tests used to compare the different AMEs. Vertical lines are 90 pct. (thick) and 95 pct. (thin) confidence intervals.

In figure 3, we show the AMEs of unemployment performance on support for the mayoral party in treatment and control municipalities in both 2009 and 2005. The AMEs are derived from a logistic model estimated on the pooled dataset. The model sets voting for the mayoral party as a function of a three-way interaction between unemployment performance, treatment status and time period ('09 vs. '05). In the model we also control for housing and elderly care performance as well as for whether the mayor is of the same party as the national government. To take the different patterns of accountability across amalgamated and continuing municipalities into account (cf. above), we allow all performance variables to have

different slopes depending on whether voters lived in a municipality which was amalgamated.

As can be seen from figure 3 there was no difference in the effect of voters evaluation of unemployment services on support for the mayoral party across treatment and control municipalities in 2005. Accordingly, before the reform of labor market regulation was implemented, and the change in political responsibility had materialized, there was no apparent difference in electoral accountability across treatment and control municipalities. In 2009, however, when the treatment municipalities had gotten more political responsibility for unemployment services, there is a statistically significant difference; just as we saw above. Looking at the difference in differences, we also find a significant difference, however, it is only statistically significant at the ten percent level. The slight drop in statistical significance can be explained by the extra error introduced by including the more noisy 2005 data.

Electoral Accountability for Other Outcomes

Above we suggested that changes in political responsibility for one policy outcome might lead voters to hold elected officials more electorally accountable for other policy outcomes as well, because voters might link responsibility and accountability at a more general level, rather than at the level of each specific policy. To investigate whether this is the case, we look at how electorally accountable voters in the treatment and control municipalities held their mayor for the quality of two types of public services not affected by the labor market reform: housing and care for the elderly. These two types of services are privileged because they represent a key area which local governments typically deal with, housing, and an area which shares several features of the unemployment services examined above, elderly care.¹² In addition to this we look at ideological congruence with the mayoral party to see if voters are more electorally responsive to the mayors ideological orientation in the treatment municipalities.

To examine electoral accountability for these other policy areas, we estimate an extended version the model presented in the fourth column of table 2; extended to include interactions between the treatment variable and voters' evaluation of their municipality's housing performance, their municipality's elderly performance and voters' ideological congruence with the mayor. Figure 4 graphs the key estimates derived from this model – the AMEs of the policy-variables among voters living in the treatment municipalities and among voters living in the control municipalities. For comparison, we also include the AMEs of unemployment performance, derived from the same model.

¹²In particular elderly care only directly affects a certain target population (i.e. the elderly), similar to how unemployment services only affect the unemployed. Elderly care is also a public service which consists of direct contact with municipal employees, similarly to unemployment services.

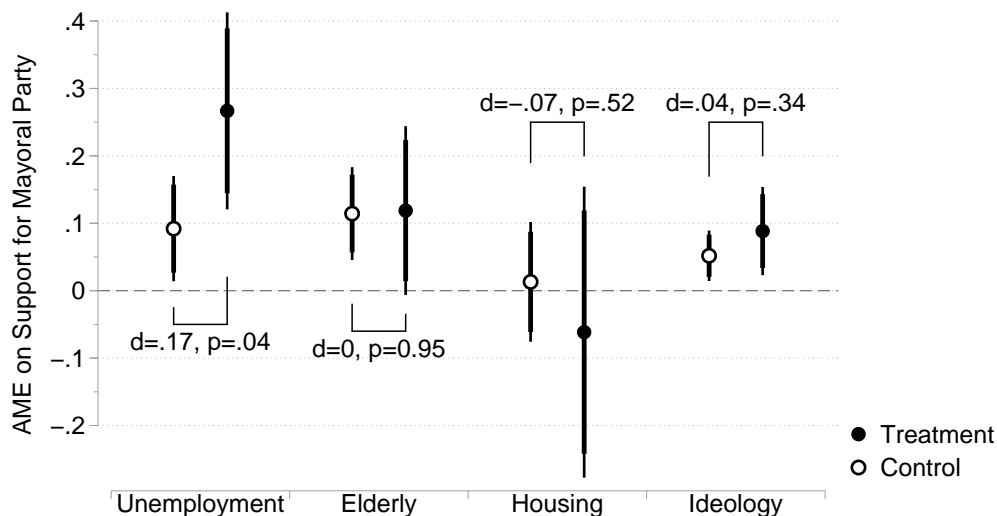


Figure 4: Average marginal effects of different variables on probability of voting for the mayoral party in treatment and control municipalities. All average marginal effects derived from an augmented version of the model presented in the fourth column of table 2 which includes interactions between each policy-variable and treatment status; McFadden $R^2 = 0.41$, $n = 1,500$. Wald tests used to compare the different AMEs. Vertical lines are 90 pct. (thick) and 95 pct. (thin) confidence intervals.

As can be seen from figure 4 there are no statistically discernible differences across treatment and control municipalities for the AMEs of elderly care, housing or ideology. This is in contrast to unemployment services, where there is a substantial and statistically significant differences.

To analyze the robustness of these results, we run similar analyses for a number of other policy areas. The results of these analyses are laid out in section S6 of the supplementary materials. Among the seven additional areas which we examine, we do not identify a single statistically significant difference in electoral accountability between treatment and control municipalities.

In sum, there is no sign that voters held the mayor more electorally accountable for other policy outcomes than the one for which additional political responsibility was conferred, suggesting that the relationship between responsibility and accountability was policy specific.

In addition to policy specificity, these findings also speak to the question of whether the relationship between responsibility and accountability is causal, in the sense that the analysis of the other policy areas can be interpreted as a placebo test. If we had found that there were differences in how accountable voters held the mayor for some other type of performance, then this might have been because the electorates in the municipalities assigned to treatment were simply more likely to

attribute political responsibility for all types of outcomes to the mayoral party in the 2009 election – irrespective of any objective differences in political responsibility.

Taken together with the findings presented above, these analyses therefore suggests that the relationship between political responsibility and electoral accountability identified here is both causal, adaptive and policy-specific.

Alternative Explanations: Political Attention and Priming

When a policy area is subject to increased political subjugation it seems natural that it will become more politically contested. Accordingly, increased local political responsibility for unemployment services in a municipality could have lead to increased local political attention to this issue in the campaign, priming the issue in the minds of the voters, making it more important for them when deciding whom to vote for (Krosnick and Kinder, 1990). Accordingly, one might wonder: is the change in electoral accountability for unemployment services really based on the fact that these unemployment services were primed in treatment municipalities?

This might be the case. More generally, priming may help explain why political responsibility is related to electoral accountability: responsibility leads to attention, attention leads to accountability. There is some evidence which suggests that priming plays such a role (Ruder et al., 2014; Johns, 2011; Hart, 2016) It is important to note, however, that our investigation does not hinge upon figuring this out. We remain agnostic about the mechanism underlying the relationship between political responsibility and electoral accountability. As such, our case is not necessarily well suited to figure out whether priming is what is driving the responsibility-accountability relationship. Instead, we focus on whether the relationship can be characterized as causal, adaptive and policy-specific. Even so, if we accept that priming of unemployment services is the mechanism, which it might be, then the increased attention to unemployment services, which any priming would presuppose, might have other side-effects that challenge inferences made above.

For one, increased attention to unemployment services might have lead to increased polarization between mayoral-partisans and non-partisans in beliefs about unemployment services. In particular, if unemployment services was framed by elite actors as a more important issue in the treatment municipalities, then mayoral partisans would have a strong motive to engage in partisan rationalization when forming their beliefs about unemployment services (for evidence of such a mechanism see Parker-Stephen, 2013). This is potentially problematic, because this might mean that the effect of the reform identified above was not a result of voters holding the mayor more electorally accountable for unemployment services. Instead, our results could be interpreted as voters in the treatment municipalities being more inclined to form their beliefs about unemployment services based on their partisan

preconceptions (i.e. reverse causation from voting to beliefs about unemployment services being stronger in the treatment municipalities). To explore the empirical viability of this alternative explanation, we examined mayoral partisans and non-partisans beliefs about the quality of unemployment services. Specifically, we looked for any evidence that partisan polarization in beliefs about unemployment services was larger in the treatment municipalities. As is detailed in section S7 of the supplementary material, we find no evidence of this. We find no evidence that voters' partisan preconceptions had a greater bearing on their beliefs about the quality of unemployment services in the treatment municipalities, making it very unlikely that increased partisan polarization can explain the identified differences across treatment and control municipalities.

A related concern is that the increase in electoral responsibility we do find is not simply the result of a change in political responsibility but a result of a *recent* change in political responsibility. That is, changes in the distribution of political responsibility might have short lived priming-effects, which moderate voters attention to the policy area for which responsibility is changed in the immediate aftermath of this change. The implication being that there was no permanent shift in how accountable the mayor was held for unemployment services, and accordingly no lasting relationship between responsibility and electoral accountability. This alternative explanation is hard to test in the present context, as we cannot look at the long term differences between the reformed (i.e. treated) and unreformed (i.e. control) municipalities. As such, in the election following the one examined above all municipalities had implemented the reform, which increased political responsibility for unemployment services. Even so, we do at least know that if the results obtained above is the consequence of a priming effect, then this priming effect cannot have been *very* short-lived. After all, the election examined above occurred a few years after the reform was first implemented in the treatment municipalities, and while this is a short period of time compared to the rate at which other political institutions change (cf. the section on adaptiveness), it is not a short period of time compared to the news cycle of typical elections (Rosenberg and Feldman, 2008). In addition to this more theoretical consideration, there is one feasible empirical test of the short lived priming explanation. This test is based on the 2013 municipal election. As such, in the 2013 election, responsibility for unemployment services was constant across treatment and control municipalities since all municipalities had implemented the labor market reform, however, time since implementation varied. In the late-implementing control municipalities, the reform was relatively new (implemented three years before the election), in the early-implementing treatment municipalities the reform was not new (implemented roughly seven years before the election). In section S8 of the supplementary materials we examine whether this difference in time since implementation affected the extent to which unem-

ployment services was on the voter's agenda in the 2013 election (i.e. whether the reform primed voters to focus on unemployment services). We find no evidence of such a priming effect.

In conclusion, we find no evidence that any side-effects of increased political attention – such as increased partisan polarization or short-lived priming – can explain the findings laid out above. This reaffirms our initial interpretation; that the findings can be explained by a relationship between political responsibility and electoral accountability which is causal, adaptive and policy-specific.

Conclusion

Our study examined a reform of Danish labor market regulation in which some municipal mayors were assigned more political responsibility for a specific policy. We found that in the election following this reform, voters in the municipalities which got more political responsibility for this policy held their mayor more electorally accountable for the outcomes of the policy. This is especially noteworthy because this change in political responsibility was as-if random, because the change happened just a few years prior to the election, and because we also find that voters *only* held their mayor more electorally accountable for the outcomes of the policy affected by the reform. As such, our findings suggest that when an incumbent's political responsibility for a specific policy or set of policies change this has a practically immediate causal effect on how electorally accountable voters hold this incumbent for the outcomes of this specific policy or set of policies. This empirical characterization of the relationship between political responsibility and electoral accountability considerably advances our understanding of this relationship, since we knew little about the extent to which the relationship was causal, adaptive or policy-specific, prior to this study.

How generalizable is this characterization of the relationship between political responsibility and electoral accountability? We examined a change in political responsibility which was abrupt and exogenous – this makes our case inherently special. Nonetheless, there are reasons to believe our findings do generalize. As such, several features of the elections studied here are relatively widespread. For instance, this study focused on a multi-party system with proportional representation. Most countries have multi-party systems with proportional representation. Some might argue that Denmark is a most likely case for identifying a relationship between power and electoral responsibility, as the relatively high levels of political knowledge and interest among Danish voters (Hansen and Pedersen, 2014) might make them more adept at holding politicians electorally accountable than other voters. However, this potential threat to generalizability is arguably ameliorated by the fact that we focus on local elections, which are generally followed less closely

(Elklit and Kjær, 2013). That is, a Danish voter in a local election might be more like an average voter in a national election when it comes to political engagement. Even so, more studies in other contexts investigating the extent to which the relationship between responsibility and accountability can be characterized as causal, adaptive and policy-specific is needed to pin down the exact scope of our findings.

Turning to implications, a more causal, adaptive and policy-specific relationship between political responsibility and electoral accountability should, all else equal, imply that voters elect more competent politicians. That is, if voters are able to focus their calculus of electoral reward and punishment on policy areas which the incumbent is primarily responsible for, then it will be more likely that voters select politicians based on the quality of outcomes which accurately reflect the politicians' effort and abilities (Duch and Stevenson, 2008; Achen and Bartels, 2016). Even so, a relationship between political responsibility and electoral accountability which is causal, adaptive and policy-specific is no panacea for effective democratic control. In particular, as Ashworth (2012) and others have argued, voters may end up holding politicians accountable for policy outcomes which voters think are relatively unimportant, but which politicians are clearly responsible for. Nonetheless, our results suggest that to the extent that it is possible to sort competent politicians from incompetent ones, voters will do so with a lot of dexterity, finding out how responsible politicians are for specific policy outcomes, and electorally punishing or rewarding them accordingly.

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Supplementary materials for:

“What Reform of Labor Market Regulation in Denmark teaches us about the relationship between Political Responsibility and Electoral Accountability”

S1: The broader Legislative Context of the Reform

Above we argued that the reform of labor market regulation changed how politically responsible municipalities were for unemployment services. A concern one might have with this argument, is that the national government somehow undid the effects of the reform by introducing detailed legislation instructing municipalities on how they should administer unemployment services, leaving the municipalities with no real administrative discretion. If this is the case, then implementing the reform would simply have meant trading a clear limit to the municipalities political responsibility for an opaque limit. However, if one studies the reform legislation (Order 1400, 2006), there is no sign of any such detailed regulation instructing municipalities on how to administer unemployment services. Further, if one explores the amount of enacted national legislation related to labor market regulation around the implementation of the reform, one does not find any marked increase. On the contrary, an examination of the legislation coming from the ministry of employment between 2005 and 2011 reveals, that while additional statutes and laws were being instated, fewer were instated in this period than between 1998 and 2004 and between 1991 and 1997 (to examine this we used data from Jakobsen and Mortensen, 2014). As such, we find no evidence suggesting that the national government tried to take back some or all of the political responsibility for unemployment services delegated to the municipalities as part of the labor market reform.

S2: Variable Descriptions

Table 3 presents a short description of the different survey items used in the analysis.

Table 3: Description of survey items from the municipal election surveys

Variable name	Question	Coding
Reelect mayor	'Who did you vote for in the previous election?'	1 is for Mayoral party voters, 0 is for other party's voters.
Treatment	'What municipality do you live in?'	1 indicates 14 treatment municipalities, 0 the 84 control municipalities.
Unemployment performance	'How satisfied or unsatisfied are you in general with your municipality's efforts towards the unemployed?'	Five point scale going from 0 "Very unsatisfied" to 1 "Very satisfied".
Elderly performance	'How satisfied or unsatisfied are you in general with your municipality's efforts towards the elderly?'	Five point scale going from 0 "Very unsatisfied" to 1 "Very satisfied".
Housing performance	'How satisfied or unsatisfied are you in general with your municipality's efforts towards private and public housing?'	Five point scale going from 0 "Very unsatisfied" to 1 "Very satisfied".
Administration controls municipality	'In reality the administration controls the municipality, not the politicians'	Five point scale going from 0 "Completely disagree" to 1 "Completely agree".
Partisanship	'Who would you vote for if a national election was held tomorrow?'	1 if respondent voted for mayoral party, 0 otherwise.
Mayoral party in government	Indicator variable of whether the mayor's party is in government	1 for mayors from the two governing parties in 2009, 0 for other mayors.
Ideology	Measures congruence between respondent's ideology (left or right-wing) and the ideology of the mayor. Mayoral ideology determined based on party (Conservative and Liberal party as right wing), respondents ideology based on question about self-reported ideology.	Coded 1 if you share ideology with mayor, coded 0 if you do not.
News consumption - local	'Thinking back, how important was local media as a source of knowledge about the municipal election campaign?'	Four point scale going from 1 "Not at all" to 5 "Very important".

Age	'How old are you?'	Measured in years.
Employment status	'Where are you currently employed?'	11 different categories including student, unemployed and retiree.
Knowledge about municipal powers	Five different questions about who has responsibility for various policy areas.	Proportion of correct answers.
Interest	'How interested would you say you are in politics?'	Four point scale going from 0 "Not at all" to 1 "Very".
Informed	'How informed would you say you are about municipal politics in your own municipality?'	Five point scale going from 0 "Not at all informed" to 1 "Very informed".
Influence	'The mayor has a great deal of influence on how the municipality develops'	Five point scale going from 0 "Completely disagree" to 1 "Completely agree".
Responsible	'Who do you think has the main responsibility for things going like they have in the past four years in your municipality?'	Respondents answering "Local politicians" or "The Mayor" coded 1. Respondents answering "National politicians" coded 0.
Apathy	'I cannot be bothered with the municipal election'	Five point scale going from 0 "Completely disagree" to 1 "Completely agree".
Obligated	'I feel obligated to vote at the municipal election'	Five point scale going from 0 "Completely disagree" to 1 "Completely agree".
Satisfied with democracy	'How satisfied are you with the local democracy?'	Four point scale going from 0 "Not at all satisfied" to 1 "Very satisfied"
Pivotality	'How likely is it that your vote will be pivotal?'	Five point scale going from 0 "Basically zero" to 1 "Very probable"

S3: Descriptive Statistics

Tables 4, 5 and 6 present descriptive statistics on the survey items used in the analysis of the 2005, 2009 and 2013 municipal election surveys.

Table 4: Descriptive statistics 2005

	Mean	SD	Min	Median	Max	n
Informed	0.45	0.19	0.00	0.40	1.00	1996
Interested	0.64	0.26	-0.33	0.67	1.00	1884
Unemployment performance	0.47	0.25	0.00	0.60	1.00	1454
Knowledge about municipal powers	0.71	0.28	0.00	0.80	1.00	2011
Elderly performance	0.66	0.31	0.00	0.75	1.00	1534
Housing performance	0.75	0.26	0.00	0.75	1.00	1944
Ideology	0.70	0.46	0.00	1.00	1.00	2011
Apathy	0.15	0.27	0.00	0.00	1.00	1988
Obligation	0.96	0.16	0.00	1.00	1.00	2000
Satisfaction with municipal democracy	0.52	0.17	0.00	0.50	1.00	1975
Pivotality	0.45	0.28	0.00	0.50	1.00	1875

Table 5: Descriptive statistics 2009

	Mean	SD	Min	Median	Max	n
Vote for mayoral party	0.31	0.46	0.00	0.00	1.00	2742
Vote for mayorak party at national elections	0.23	0.42	0.00	0.00	1.00	3199
Voted for mayoral party at regional election	0.25	0.43	0.00	0.00	1.00	3199
Voted for mayoral party at last municipal election	0.30	0.46	0.00	0.00	1.00	3199
Voted for mayoral party at last national election	0.26	0.44	0.00	0.00	1.00	3199
Influence	0.74	0.25	0.00	0.75	1.00	3175
Responsibility	0.67	0.47	0.00	1.00	1.00	2998
Unemployment performance	0.51	0.29	0.00	0.50	1.00	2296
treatment	0.15	0.36	0.00	0.00	1.00	3336
Housing performance	0.71	0.23	0.25	0.75	1.00	2920
Elderly performance	0.57	0.31	0.00	0.50	1.00	2771
Administration controls municipality	0.50	0.31	0.00	0.50	1.00	2895
Local media consumption	3.14	0.92	1.00	3.00	5.00	3336
Age	54.53	13.79	18.00	55.50	91.00	3272
Shares ideology with mayoral party	0.44	0.50	0.00	0.00	1.00	3336
Mayor is from the same party as national government	0.49	0.50	0.00	0.00	1.00	3199
Elementary school	0.18	0.38	0.00	0.00	1.00	3336
High school	0.07	0.26	0.00	0.00	1.00	3336
Vocational high school	0.04	0.19	0.00	0.00	1.00	3336
Vocational school	0.25	0.44	0.00	0.00	1.00	3336
Shorter tertiary education	0.08	0.27	0.00	0.00	1.00	3336
Tertiary education	0.23	0.42	0.00	0.00	1.00	3336
Graduate degree	0.03	0.17	0.00	0.00	1.00	3336
Postgraduate degree	0.12	0.33	0.00	0.00	1.00	3336
Untrained worker	0.05	0.21	0.00	0.00	1.00	3336
Skilled worker	0.11	0.31	0.00	0.00	1.00	3336
Blue collar worker	0.13	0.34	0.00	0.00	1.00	3336
White collar worker	0.20	0.40	0.00	0.00	1.00	3336
Self employed	0.06	0.24	0.00	0.00	1.00	3336
Home maker	0.00	0.05	0.00	0.00	1.00	3336
Student	0.03	0.16	0.00	0.00	1.00	3336
Not looking for work	0.00	0.06	0.00	0.00	1.00	3336
Unemployed	0.04	0.19	0.00	0.00	1.00	3336
Retiree	0.34	0.47	0.00	0.00	1.00	3336
Will not say	0.01	0.10	0.00	0.00	1.00	3336
Other	0.03	0.18	0.00	0.00	1.00	3336

Table 6: Descriptive statistics 2013

	Mean	SD	Min	Median	Max	n
Responsibility	0.78	0.41	0.00	1.00	1.00	3968
Influence	0.72	0.22	0.00	0.75	1.00	4254
Unemployment services: most important	0.13	0.34	0.00	0.00	1.00	3205
Unemployment services: should be most important	0.09	0.29	0.00	0.00	1.00	3205

S4: A Manipulation Check

Mean responses for the two ‘manipulation check’ questions are presented in the two leftmost columns of table 7. It is important to note, that while the differences between treatment and control are not very large, these questions ask about conditions in the municipality in general, not just unemployment services. As such, we would expect voters in the treatment municipalities to think the mayor is substantially more responsible for unemployment services, and accordingly, slightly more responsible for the overall conditions in the municipality.

Table 7: Manipulation check

	2009		2013	
	Responsible	Influence	Responsible	Influence
Control	0.66 (0.01)	0.74 (0.01)	0.78 (0.01)	0.72 (0.00)
Treatment	0.71 (0.02)	0.76 (0.02)	0.78 (0.01)	0.72 (0.01)
p-value	0.03	0.03	0.39	0.25
Observations	2998	3175	3968	4254

Standard errors in parentheses, one-sided p-value from difference in means test.

Was the 2009 difference due to pre-treatment differences in voters’ beliefs? This seems unlikely given the extensive balance-test presented in table 1, but we cannot be sure since these questions were not posed in the 2005 survey. However, these questions were part of the 2013 municipal election survey and we can utilize the 2013 data to conduct a post-treatment balance test. Recall, that when the 2013 election came about, the reform was implemented in all municipalities. As such, if the differences in the 2009 survey were due to the asymmetry in political responsibility caused by the reform, these differences should have disappeared in 2013. In the two rightmost columns of table 7, we report means across treatment and control municipalities. As expected, once all municipalities had implemented the reform, there was no longer any difference in mean responses to the two ‘manipulation check’ questions.

S5: Alternative Estimation Methods

In the analyses conducted below we show that the key findings presented in table 2 are robust to employing two alternative estimation methods. These methods relax some of the assumptions made in order to estimate the models in 2, and accordingly, they provide a more complete picture of the statistical evidence for the key conjecture of our analysis: that voters in the treatment municipalities held the mayoral party more electorally accountable for unemployment services than voters in the control municipalities.

The models estimated above did not take the hierarchical structure of the data – individual voters nested within municipalities – fully into account. In order to do this, we estimate a set of mixed effects multilevel logit models with the same configuration of variables as we used in table 2. Estimates from these models are presented in table 8. The important estimates remain practically unchanged, although the standard error of the estimates increase slightly. Most importantly the difference in AMEs remain statistically significant ($p \approx 0.05$). The logit interaction coefficients are also statistically significant, although only at the ten percent level.

Table 8: Multi-level logistic regression of probability of voting for the mayoral party

	(1)	(2)	(3)	(4)
Unemployment performance	0.84* (0.24)	0.84* (0.25)	0.92* (0.37)	0.88* (0.36)
Treatment	-0.60 (0.41)	-0.58 (0.44)	-0.49 (0.45)	-0.64 (0.43)
Treatment * Unemployment performance	1.02+ (0.55)	1.02+ (0.59)	1.28+ (0.66)	1.36* (0.61)
Administration controls municipality	-0.31 (0.21)	-0.42+ (0.21)	-0.44+ (0.24)	-0.41+ (0.24)
Housing performance	0.54+ (0.30)	0.53+ (0.32)	-0.01 (0.40)	0.07 (0.40)
Elderly performance	0.83* (0.23)	0.92* (0.25)	1.06* (0.31)	1.05* (0.30)
Sociodemographic controls		✓	✓	✓
Political controls			✓	✓
Municipal-level variables				✓
AME (Control)	0.17 (0.05)	0.17 (0.05)	0.10 (0.04)	0.09 (0.04)
AME (Treatment)	0.36 (0.08)	0.35 (0.09)	0.23 (0.06)	0.24 (0.05)
Difference (T-C)	0.18	0.18	0.14	0.15
<i>p-value of difference</i>	0.05	0.07	0.05	0.02
Log likelihood	-911.88	-881.73	-553.65	-550.06
Observations	1522	1504	1504	1504

Robust standard errors clustered on municipality in parentheses. + $p < 0.1$ * $p < 0.05$

The tests used to assess the statistical significance of the interaction terms and differences in AMEs in table 2 rely on a number of parametric assumptions. To get around these assumptions, we tried to derive the statistical significance using a

form of randomization inference; a non-parametric method (cf. Gerber and Green, 2012). In particular, we used the following procedure.

1. Drew a random sample of 14 municipalities, creating a dummy which was equal to one if the respondent lived in one of these randomly drawn municipalities.
2. Estimated the models reported in column 1-4 of table 2, but substituting the actual treatment variable for the dummy variable created in (1).
3. Stored the estimated interaction effect between the simulated treatment dummy and unemployment performance obtained for each logit model estimated in (2).
4. Derived the the average marginal effect (AME) of unemployment performance in the simulated treatment and control municipalities for each of the models estimated in (2) and stored the difference in AME's.
5. Repeated (1)-(4) 10.000 times resulting in 10.000 unique interaction coefficients and AME-differences for each model.
6. Calculated *p-values* by looking at the proportion of simulated logit coefficients and AME estimates which were *larger* than the ones estimated for the actual treatment and control municipalities.

A random sample of the 10,000 simulations are plotted in figure 5 along with the calculated p-values. These p-values signify how likely it is to get an interaction or difference in AMEs of the size estimated in table 2 or larger *if* there was no effect of being assigned to implement the labor market reform for any of the municipalities (a sharp null). The p-values do become slightly larger using this method, however, the p-values are still below 0.1 and thus reflect that the observed difference in the weight voters put on unemployment service between treatment and control municipalities is unlikely to have occurred by chance.

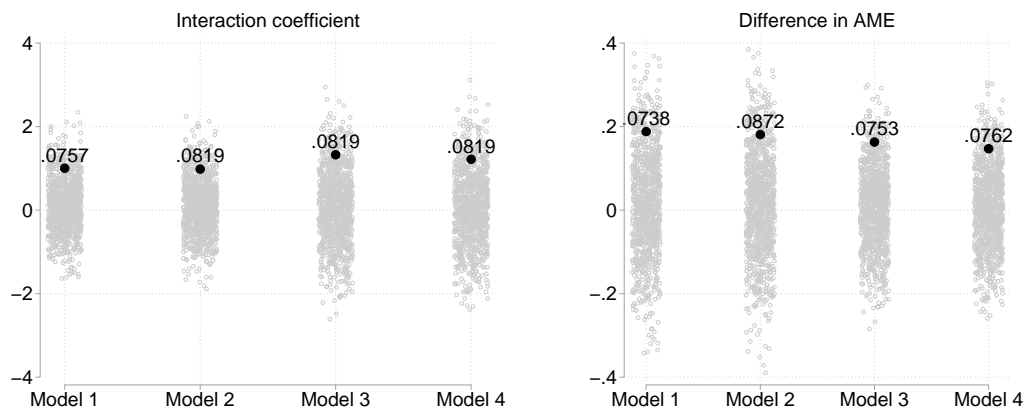


Figure 5: A sample ($n=1,000$) of the simulated differences in AMEs and interaction effects from each of the four different logit models estimated in table 2. Computed using randomization inference (RI). Black dot signifies realized outcome, taken from table 2, and the number attached to it is the RI p-value.

S6: Analyzing Additional Policy Areas

In this section we examine differences in electoral accountability across treatment and control municipalities for some additional policy outcomes. As such, we investigate whether voters in the treatment municipalities were more likely to electorally punish and reward the mayor for performance in nine different policy areas, which were not affected by the reform of labor market regulation (including the two examined in figure 4). In particular, we use the logit model presented in column 4 of table 2 as a template, switching the unemployment performance variable for one of the other policy variables. We do this for all the different policy variables. For each of these nine new policy-models, we then estimated the AME of performance in the policy area on propensity to vote for the mayoral party for voters living in both the treatment and in the control municipalities. Finally, we test the AME in the treatment municipalities against the AME in the control municipalities using a Wald test. The result of these analyses are reported in table 9.

As is revealed by looking at the right-most column of table 9, the AME of voters' assessment of the quality of the services provided in these nine different policy areas do not significantly differ across treatment and control municipalities. As such, unemployment performance is the only policy outcome which voters held their mayor more electorally accountable for in the treatment municipalities.

Table 9: Differences across treatment and control for other policy outcomes

Policy Area	Treatment	Control	Standard error	p-value
Unemployment performance	0.25	0.09	0.07	0.01
Housing	0.05	0.04	0.09	0.85
Daycare	0.08	0.08	0.10	0.94
Recreation	0.01	0.08	0.08	0.44
Schools	0.08	0.03	0.08	0.56
Library	0.04	0.09	0.07	0.47
Culture	0.19	0.09	0.09	0.28
Business	0.18	0.10	0.10	0.43
Elderly Services	0.14	0.11	0.09	0.79
Health Services	0.17	0.08	0.09	0.29
Total	0.13	0.08	0.09	0.47

The models from which the average marginal effects are derived include the full set of controls.

S7: Partisan Polarization

In this section we look for signs that mayoral-partisans did in fact differ more from non-mayoral partisans in their beliefs about the quality of unemployment services in the treatment municipalities. That is, we examine whether the increase in political responsibility for unemployment services had an effect on partisan polarization in voters' beliefs about the quality of unemployment services.

The key measure in this regard is the net mayoral partisan satisfaction with unemployment services (i.e. mean response to unemployment performance question among mayoral partisans subtracted from mean response among non-mayoral partisans). This measure tells us how much more satisfied mayoral partisans were with unemployment services than non-mayoral partisans. We can split respondents into mayoral partisans and non-partisans in four different ways; based on whether the respondents voted for the mayor's party in the last national election (1), whether the respondents voted for the mayor's party in the last municipal election (2), at the regional election (3), and based on whether respondents said they would vote for the mayoral party if a national election was held tomorrow (4). In figure 6 we show the net mayoral partisan satisfaction for mayoral partisans and non-partisans in treatment and control municipalities using each of these four empirical definitions.

If increased political responsibility lead to increased partisan polarization, we would expect the difference between mayoral partisans and non-mayoral partisans to be greater in the treatment municipalities. We find no evidence of this. Instead, no matter what metric of partisanship is used, net mayoral partisan satisfaction with unemployment services seem to be roughly similar across treatment and control municipalities. As such, there is no evidence that mayoral partisans reacted

to the increase in mayoral responsibility for unemployment services by adopting a more positive view of these services.

This means that our finding in the main analysis, that there is a closer connection between voters' support for the mayor and their evaluation of unemployment services in the treatment municipalities, is not a result of those predisposed to support the mayor in the treatment municipalities being more likely to evaluate unemployment services more favorably than those predisposed to support the mayor in the control municipalities.

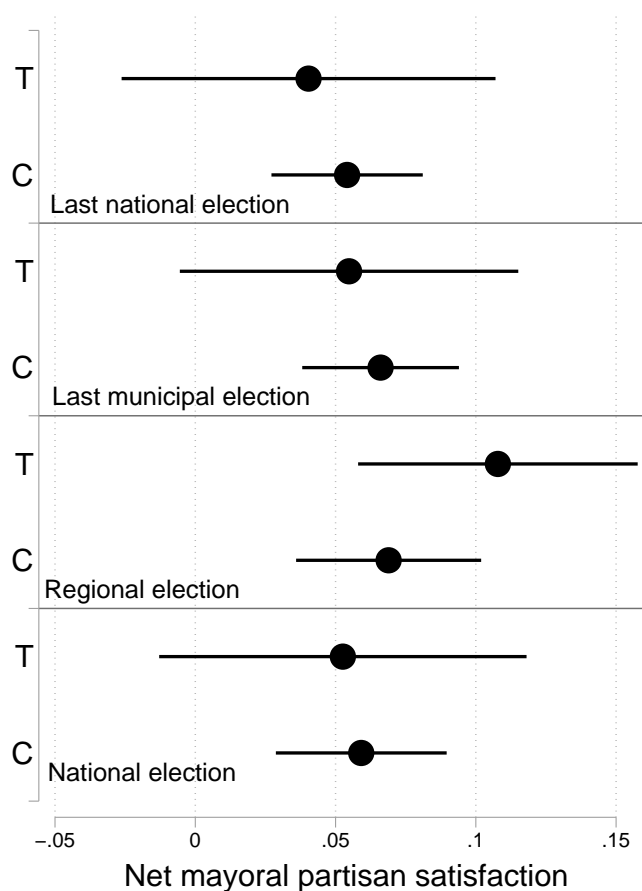


Figure 6: Dots signify the average net mayoral partisan satisfaction with unemployment services across four different empirical definitions of mayoral partisanship. The average net mayoral partisan satisfaction is estimated for both treatment (T) and control (C) municipalities. Horizontal lines are 95 pct. confidence intervals.

S8: Short-lived Priming

An alternative explanation for why voters increased the weight they put on unemployment services in the election following the reform of labor market regulation is that the reform briefly primed this issue (Krosnick and Kinder, 1990). That is, the reform could have lead to a brief period of increased political attention to unemployment services in the treatment municipalities, which, in turn, could have lead voters to rely more on their evaluation of the quality of these services, when deciding whom to vote for. We offer two pieces of evidence against this alternative explanation.

First, we know from the analysis conducted in section S4 (cf. table 7), that voters in the treatment municipalities, where the mayor got more responsibility for unemployment services, did in fact believe the mayor was more responsible for what happened in the municipality. This should increase our confidence that the difference in behavior between the treatment and control electorates was, at least in part, due to differences in beliefs about political responsibility, and therefore not solely due to short-lived priming.

The second piece of evidence against the short-lived priming explanation can be found by examining voters' agenda in the 2013 municipal election. In 2013 all municipalities had implemented the reform, leaving responsibility balanced across treatment and control municipalities, but the control municipalities differed from the treatment municipalities in that they had implemented the reform recently (in 2010). As such, an empirical implication of the short-lived priming explanation is that unemployment services should be higher on the voters' agenda in the 84 control municipalities in 2013.

To examine this empirical implication, we measured voters' agenda in the different municipalities at the 2013 election. Specifically, we examined open-ended answers given to two questions: "When thinking back on the campaign in your own municipality, what was the most debated issue?" and "What did you think should have been the most debated issue?" Arguably, the content of these open-ended responses should give us an indication of whether unemployment services was higher on the agenda in some municipalities than in others. To analyze the responses, we coded whether they mentioned at least one of four stemmed key words related to unemployment-services.¹³ We use this indicator as a rough measure of the extent to which unemployment services were on the voters' agenda in the different municipalities. Recall, that if the short-lived priming explanation is accurate, we would expect unemployment services to be higher on the agenda of voters in the control municipalities vis-a-vis voters in the treatment municipalities.

¹³The four key-words were: *arbedj*, *job*, *erhverv*, *beskæf*. The key words are stemmed versions of different synonyms for work (i.e. work, job, employ, occupation). For each of the open ended answers, about ten percent of respondents used at least one key word.

This does not seem to be the case. We find very small and statistically insignificant differences in the percentage which used at least one key word for both the question about what voters believed was on the agenda ($d = 0.07$ pct., $p = 0.34$), and the question about what voters thought should have been on the agenda ($d = 0.04$ pct., $p = 0.37$). In sum, we find no evidence of the short-lived priming explanation.