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Vetoing and Inaugurating Policy Like Others Do: Evidence on Spatial Interactions in Voter Initiatives

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VETOING AND INAUGURATING POLICY LIKE OTHERS DO: EVIDENCE ON SPATIAL INTERACTIONS IN VOTER INITIATIVES

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Abstract

A sizeable literature studies whether governments strategically interact with each other through learning, coercion, fiscal and yardstick competition. This paper asks whether, in the presence of direct democratic institutions, spatial interactions additionally result from voters' direct actions. The proposed mechanism is that the voters' actions in vetoing a decision or inaugurating a preferred policy by a binding initiative in their jurisdiction can potentially have spillover effects on the actions of voters of neighboring jurisdictions. Utilizing data on around 3,200 voter-initiatives across all municipalities in 2002-14 and applying an instrumental variables approach, the paper finds that a jurisdiction's probability of hosting an initiative is positively driven by the neighbors' direct democratic activity. This effect persists over time, and is stronger in towns with relatively more information flows (measured by local newspaper consumption of citizens and total number of commuters across jurisdictions).

Keywords: Direct democracy, spatial spillovers, policy diffusion

JEL codes: D72, D78, R50

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1 Introduction

A sizeable literature in economics and political science studies the question of how strategic interactions among political jurisdictions affect their choice of public policies. Such interactions may occur horizontally or vertically and between or within countries, in general, because of learning, coercion, fiscal and yardstick competition. The fields of public finance and public economics have put forward several mechanisms that underlie such spatial relationships in the governments' spending and taxing decisions (Revelli, 2005) and in other public sector policies (Brueckner, 2003).

According to the *externality* mechanism, a government may find it optimal to internalize a policy set by another government, say in the field of education or health care, when making its own decision to build more or less schools and hospitals (Case et al., 1993). More generally, local state capacity building can be a strategic choice for jurisdictions when their borders are open (Acemoglu et al., 2015). A particular economic constraint may be due to the *competition* for attracting mobile resources such as labor and capital through fiscal competition (Tiebout, 1956; Wilson, 1999). In a principal-agent framework with incomplete information a decision-maker is additionally subject to *yardstick competition*. Such competition arises if the principals form certain *expectations* in regard to their jurisdiction's (not perfectly observable) performance, for example in the quality of public service provision, by relying on other jurisdictions' (again not perfectly observable, but comparable) performance as a yardstick (Besley and Case, 1995). Furthermore, representatives may *learn* from neighbor's policies and take them into account accordingly (see, e.g. Gilardi, 2010, for learning between OECD countries). Based on theoretical models of policy choice, Volden et al. (2008) formalize such learning-based *policy-diffusion* mechanisms and Mukand and Rodrik (2005) conceptualize the related idea of *policy experimentation*.

The outcomes of these often competing mechanisms can be similar, however with quite different implications for policy. Therefore, significant effort has been put to disentangle these mechanisms (Shipan and Volden, 2008), particularly with an empirical strategy of comparing sub-national jurisdictions within countries (Brueckner, 2003). However, what this literature has in common is that it almost exclusively focuses on economic systems based on a pure representative form of government. Our paper contributes to this literature by studying political systems where decisions can be made also directly by voters through initiatives or other direct democratic instruments. The basic idea is that the voters' actions in vetoing a decision or inaugurating a preferred policy by a binding initiative in their jurisdiction may potentially have spillover effects on the (direct democratic) actions of voters.

Theoretically, the proposed channel can be thought of (groups of) voters as collective decision-makers interacting with each other similar to individuals in the social interactions analysis (Manski, 2000). Of course, voters do have a role to play in a representative system, where, for example, they can “vote with their feet” affecting competition and the implied interactions. Voters can also influence political decisions outside of elections, such as through popular mobilization. In fact, a large literature in political science and sociology argues that such instances of collective action do not take place in isolation, but are often the result of significant spillovers across time and jurisdictions (Snow et al., 2004). Relatedly, our argument is that direct democracy provides a new and legitimate decision-making institution which may or may not be mimicked across-jurisdiction. This is the central question we aim to test in this paper.

Regarding the relevance of this question, most of the previous empirical contributions on spatial interaction in public policies concentrate on higher income countries with some level of autonomy in sub-national governance. Many of these countries by now have some kind of direct democratic institutions at the local level, therefore testing the proposed question of interactions through direct decision-making mechanisms seems timely.

Regarding the empirical design, a central concern with the observed spatial patterns in jurisdictions' policies has been the empirical difficulties in isolating possible common shocks or spatially correlated (unobservable) effects from the real effects of interest (Gibbons and Overman, 2012). Several recent papers rely on arguably more credible identification techniques by utilizing sources of exogenous variation (see, e.g. Lyytikäinen, 2012; Isen, 2014; Baskaran, 2014, 2015), and find that some of the previously documented strong effects could be due to spurious correlations.

Our design focuses on German municipalities from 2002 to 2014, where since the mid-1990s citizens have the power to veto (some of) local governments' decisions and propose certain new policies by launching initiatives (in total around 3,200 for the 13,000 municipalities in the study period). We apply spatial reaction functions, and exploit a plausibly exogenous instrument based on the states' differences in direct democratic laws to identify interactions between municipalities. Following Asatryan (2016), our main instrument for the number of initiatives in the neighboring municipalities is the amount of signatures required for the initiative to be successful. We use municipality fixed effects to control for unobserved constant spatial correlation across municipalities.

Our findings suggest that the probability of observing an initiative in a municipality is positively driven by its neighbors' activity in direct democracy. The results indicate that a one standard deviation increase in the average number of neighbor initiatives increases the probability of having an initiative by 2.13%. This effect is statistically significant over time, and is stronger in towns with relatively more information flows (measured by local newspaper consumption of citizens and total number of commuters across jurisdictions). We estimate the size of the average effects to peak in a 10-20 km neighborhood, then gradually declining and vanishing after around 60 km. Additionally, we find evidence that the results are driven by spillovers in similar policy areas.

To put into context, this paper is related to the literature explaining (the extent of) spatial interactions by certain political-economy factors in general, and from the representative versus direct democracy angle in particular. In the theoretical framework of Hugh-Jones (2009) interactions may exist either for policy experimentation (citizens themselves observe the effects of policy) which is possible only in a direct democratic system, or for yardstick competition in representative systems. The theoretical paper by Boehmke (1999) argues that interactions can be more intensive between jurisdictions that have direct democratic systems compared to representative democracies, but this is explained primarily by informational advantages of the former system. In contrast, the empirical study of Schaltegger and Küttel (2002) with Swiss data argues that direct democracy (and fiscal autonomy) significantly increases the level of political competition and, therefore, reduces the scope of policy mimicking. The authors, however, do not analyze the channel that we propose here – that is the potential scope for spillovers through direct democratic institutions. Also, the focus is on referendums, thus, only on the veto-power of direct democracy, while the agenda-setting function of initiatives, which may actually enhance the policy space and not the opposite, is neglected. Hawley and Rork (2015) study spatial determinants of the property tax limit overrides in Massachusetts and demonstrate that a town’s likelihood of holding an initial vote increases by 10-15% if a neighboring town has already held a vote at some point in the past. This evidence combined with our findings reinforce the result of strong spatial interactions in direct democratic instruments in two different settings. In contrast to our paper, however, the focus of Hawley and Rork (2015) is again on referendums called by the government, which only allows studying government-level interactions. Furthermore, the referendums analyzed by Hawley and Rork (2015) are about a specific topic (i.e. the property tax limits) whereas our study covers a broader range of policy issues. Finally, Arnold et al. (2015) study the effect of citizen-initiatives on housing supply in the context of the German state Bayern. In order to estimate causal effects, they use a spatial lag of neighbor citizen-initiatives as

an instrument for a municipality’s likelihood of launching an initiative. Their first stage regressions are in line with our second stage results.

This paper is organized as follows. Section 2 briefly introduces the German institutions of direct democracy and presents some anecdotal evidence on spillover mechanisms. Section 3 describes our data and identification strategy, and Section 4 presents the results. Conclusions are discussed in Section 5.

2 Institutions and spillover mechanisms

Most German states (in German: Länder) introduced local-level direct democratic institutions in the 1990s after the German re-unification. Baden-Württemberg is an exception with institutions of direct democracy on the local level already established in 1956. Berlin is the last state which introduced laws of direct democracy in 2005.

These institutions enable citizens to launch so-called *citizen initiatives* (“Bürgerbegehren”) which are divided into *innovative initiatives* (“Initiativbegehren”) and *corrective initiatives* (“Korrekturbegehren”). The latter is used to veto on policies which have been adopted by the city council, while the former allows to launch new policies. For an initiative to be successfully implemented the initiators face several constraints. First, a town-specific amount of signatures has to be collected (*minimum signature requirement*) within a predefined time. If this is achieved, the city council will decide if it wants to realize the issue at hand or not. In case of a negative decision, the next step of the procedure is reached, i.e. citizens vote on the respective issue. Besides a simple majority, some states further require a certain quorum (minimum turnout relative to population) for the vote to be accepted. Another limitation are topic exclusions: topics which directly concern the municipal budgets are not allowed for initiatives in any state (*fiscal taboo*). Moreover, each state has a list of other prohibited

topics (*off-limits issues*) or a list of allowed topics (*positive catalogue*). All states except Bayern and Bremen also demand initiatives to be accompanied by a cost-recovery proposal.¹

Looking into the data we observe a higher activity of direct democracy in states with less strict institutions. For example, in Bayern, where there are comparatively liberal institutions, around 2,700 initiatives have been launched until 2015. On the contrary, in Baden-Württemberg, where very rigorous regulations are in place, only around 800 initiatives have been launched until 2015.² The geographical distribution of the number of initiatives is illustrated in a heat-map in Figure A1 of the appendix. Table A1 summarizes the state-level institutions of direct democracy.

With these direct democratic institutions in place, the argument is that there is an additional mechanisms at the hands of voters which may be used to (ban) mimic (non-)preferred policies across jurisdictions.³ Anecdotal evidence from the following cases helps to better understand the idea.

The construction of a new railway station in the city of Stuttgart is an example for direct democratic activity being contagious across jurisdictions. The so-called Stuttgart 21 project calls for deconstructing two wings of a century-old train station, and replacing above-ground tracks with a tunnel system which is supposed to speed up travel times. However, there have been several initiatives which all aimed at stopping the project.⁴ This direct democratic engagement by the population appears to have had spillover effects on the citizens of other

¹ For more detailed information on the history of German institutions of direct democracy see e.g. Asatryan (2016) and Rehmet et al. (2014).

² One of the regulations concern the number of signatures required for an initiative to be successful. Arnold and Freier (2015) and Asatryan et al. (2016) show that the signature requirement effects the number of initiatives.

³ In practice, these possible interactions can not only occur by mimicking of initiatives but also by less formal means, for example by demonstrations or informal initiatives. In a sense, our results therefore constitute a lower bound estimate for spillover effects in direct democratic activity of citizens.

⁴ In 2010, protests against this long-term project accumulated in large demonstrations. See for example an article published in the New York Times:

http://www.nytimes.com/2010/10/06/world/europe/06germany.html?_r=0 A version of this article appeared in print on October 6, 2010, on page A11 of the New York edition with the headline: Germany Pulls Back on Demolition of Stuttgart Rail Station.

municipalities, for example, in the close-by town of Leonberg where citizens launched an initiative in the same year against the demolition of a public indoor swimming pool and the related plans of building a new swimming pool. One of the initiators of the initiative explicitly stated that their activities have been inspired by the Stuttgart 21 opposition.⁵ Besides learning about the *possibility* of conducting initiatives, this case illustrates an additional notion of learning, namely learning about specific (optimal) *policies*.

A similar observation can be made for two other close-by municipalities, Denklingen and Seefeld in the state of Bayern. In Denklingen, there was a long-standing discussion whether to build a new city hall, or to renovate and extend the old one. This led to an initiative against building a new city hall which was accompanied by many newspaper articles about the topic.⁶ Shortly afterwards, a very similar discussion arose in Seefeld which then also led to an initiative.

These observations are related to the mechanisms described by the literature on diffusion processes within and across movements.⁷ Proximal models stipulate that actors mimic strategies of other people or groups which are spatially or culturally important to them (e.g. Soule, 1995, and Soule, 1997, in the context of student movements). In these settings, diffusion is promoted by *direct* and *indirect* channels. Direct channels refer to the existence of frequent contacts between the actors or even their overlapping engagement in more than one movement. These direct channels might also occur in the case of direct democracy when, for example, special interest groups spread to close-by municipalities. However, it is also well

⁵ For example in the regional newspaper “Stuttgarter Nachrichten”, which serves subscribers in both municipalities Stuttgart and Leonberg: <http://www.stuttgarter-zeitung.de/inhalt.buerger-begehren-die-sanierung-des-sportzentrums.4c32408a-5936-44dd-93b8-5bef9a6a138b.html>

⁶ For example, see the following reports from two regional newspapers which cover both municipalities: <http://www.augsburger-allgemeine.de/landsberg/754-Unterschriften-fuer-Rathaus-Stopp-id28639427.html> or <http://www.kreisbote.de/lokales/landsberg/buergerentscheid-ueber-rathaus-stopp-3354717.html>.

⁷ Please see Snow et al. (2004) in general, and Soule’s contribution in this book in particular for a summary on the diffusion research in the field of social movements.

possible that diffusion takes place by more indirect channels like media coverage (and the consequent informational flows between voters) as described by Snow et al. (2004, p. 295). In the context of race riots in the US, for example, Myers (2000) finds that wider media coverage increases the penetration of riots in neighboring areas. Similarly, Revelli (2008) shows that voters compare their jurisdiction's performance with jurisdictions they share the same local media with. We take such indirect channels into account and test if information flows play a role in mimicking direct democratic activity.

As explained above, initiatives in German towns may address different fields of public policies. Therefore, our empirical setup allows to test not only whether there are spillovers in direct democratic engagement, but also whether these spillovers are solely driven by initiatives within similar policy areas. Parallels can again be drawn with the government-level interaction channels. If governments search for better policies because of yardstick and fiscal competition due to voters looking across borders or threatening to exit, the question is why voters themselves cannot directly implement such policies when direct democratic rights are available.

Following the theoretical arguments and the anecdotal evidence discussed in the introduction and this section, we arrive to the main hypothesis of this work: Complementing a representative system of local governance with some institutions of direct democracy may open a new channel of policy-spillovers across jurisdictions that functions through interactions between (groups of) voters and their actions in exploiting their direct democratic rights. The next sections proceed to a formal analysis of this hypothesis.

3 Data and specification

Our data consists of an unbalanced panel of over 13,000 German municipalities across all German states for the years from 2002 to 2014 except of the city states Berlin, Bremen and

Hamburg.⁸ Table A1 of the appendix summarizes the data on: state-level institutions of direct democracy,⁹ municipality-level data on the frequency of observed initiatives as our dependent variable, and a number of control variables (unemployment rate, population, the share of population above 65 years old, and the sum of the vote shares for the Green Party (Bündnis90/Die Grünen), the Social Democratic Party (SPD) and the Left Party (Die Linke) in the federal elections (denoted by “left share” hereafter)).

The paper tests for spillovers in direct democratic activity by specifying a reaction function (spatial lag model) similar to the approach employed by the literature on tax competition and public budget spillovers (see e.g. Devereux et al., 2008; Redoano, 2014; Foucault et al., 2008).¹⁰ We specify the following linear probability model with municipality-panel fixed effects in order to test if citizens mimic their direct democratic activities across jurisdictions:

$$d_{-p_{it}} = \delta \sum_{j \neq i}^N w_j p_{jt} + X_{it} \beta_2 + \alpha_{2i} + \mu_{2t} + \varepsilon_{2it} \quad (1)$$

where the dependent variable $d_{-p_{it}}$ is a dummy which is one if there was at least one initiative launched by citizens of municipality i in year t . On the right hand side, X_{it} is the set of standard demographic and political controls on municipality level mentioned above; α_{2i} is a municipality fixed effect, μ_{2t} a year dummy, and ε_{2it} an unobserved error term. The spatial lag ($\sum_{j=1}^N w_j p_{jt}$) constitutes the variable of interest which is the average number of initiatives in the neighbor municipalities of i . The same weight w_j is attached to each neighbor municipality j of municipality i . We normalize the sum of these equal weights

⁸ We exclude these special “city states” since initiatives there are either implemented on the level of the state (same as city) or district, both being different than municipalities. The panel is unbalanced because of amalgamations of municipalities.

⁹ One of the main differences in direct democratic institutions are the amount of signatures which have to be collected within a predefined time period in order to get to the next step of the direct democratic process. This information is collected from states’ respective municipal codes.

¹⁰ More papers employing reaction functions are, e.g., Solé-Ollé (2001); Allers and Elhorst (2005); Leprince et al. (2007); Bordignon et al. (2003); Buettner (2001); Hauptmeier et al. (2012); Davies and Voget (2008); Devereux et al. (2008); Egger and Raff (2015); Overesch and Rincke (2011).

to one, thus the spatial lag is simply the average number of initiatives in the neighbor municipalities. In the main specification, municipalities within a 50 km radius of municipality i qualify as neighbors. This reflects the idea that spillovers in direct democratic actions are likely to be a rather regional phenomenon and that municipalities beyond 50 km may be on average too far away for having an effect on municipality i , for example due to limited information flows across regions.¹¹ Consistently, we also exclude municipalities which are close to a country border of Germany.¹² For robustness, we vary the definition of neighbor municipalities by varying the radius from 50 km to 30 and 70 km.

Reaction functions like specified in equation (1) may be subject to a major endogeneity concern: It is explicitly assumed that the likelihood of having an initiative in municipality i depends on the average number of initiatives in municipality j and vice versa - this makes the spatial lag endogenous by definition. The problem can be mitigated by applying appropriate instruments to the spatial lag. The literature cited above on budget and tax-setting spillovers uses the (weighted) averaged demographic and political control variables of the neighbor municipalities to instrument the spatial lag. However, as argued recently by Baskaran (2014, 2015) this is no golden way out since this approach is not robust to possible common shocks or spatially correlated (unobservable) effects.

Following Asatryan (2016), we address this problem by relying on a plausibly exogenous variable as our main instrument, namely the minimum requirement for the number of signatures which have to be collected within a predefined time. The first-stage specification

¹¹ With respect to *direct* exchange between individuals, Mok and Wellman (2007) show that distance matters for interpersonal contact. Also, regional newspapers make up almost 75% of the total sales of daily newspapers in Germany in 2014 (Bundesverband Deutscher Zeitungsverleger e.V., 2015, p. 5). These regional newspapers put a strong emphasis on regional news.

Similarly, the fiscal spillover literature also assumes geographically close jurisdictions to have a greater effect on each other than more remote jurisdictions (e.g. Foucault et al. (2008) in the context of spending interactions between French municipalities and Redoano (2014) with respect to tax competition among European countries.)

¹² We define closeness analogous to the respective neighborhood definition in the different specifications. Border municipalities are not excluded in the spatial lag of municipalities not close to country borders.

therefore regresses the spatial lag on the averaged control variables of the neighbor municipalities (including the *signature requirement*) and all of the previous regressors and takes the following form:

$$\sum_{j \neq i}^N w_j p_{jt} = \beta \sum_{j \neq i}^N w_j X_{jt} + X_{it} \beta_1 + \alpha_{1i} + \mu_{1t} + \varepsilon_{1it} \quad (2)$$

Clearly, our main instrument, the signature requirement, is *relevant* for the frequency of initiatives in a municipality (non-zero covariance between $\sum_{j \neq i}^N w_j p_{jt}$ and $\sum_{j \neq i}^N w_j X_{jt}$). Furthermore, the exogeneity condition is fulfilled since the signature requirement in municipality j has no direct effect on the number of initiatives in municipality i (signature requirement in j is uncorrelated with the error term of the second stage ε_{2it}). This can be safely concluded since the signature requirements are determined by state-laws and not by the municipalities themselves and are, hence, exogenous to the single municipalities. Although set by the states, the instrument does not only vary across states but also by municipalities within states depending on population thresholds. It is measured in percent of a municipality's population and varies between 0.43% in a municipality in Sachsen-Anhalt and 15% in some municipalities in Rheinland-Pfalz, Sachsen and Sachsen-Anhalt (see Table A1).

To estimate our (second stage) specification we choose a linear probability model (LPM) which allows for a binary dependent variable. The advantages of LPM prevail in our setting, similar to Angrist and Pischke (2008) and Angrist (2001) who argue in favor of the LPM instead of alternative models for which the conditions are likely not to be fulfilled. A probit model would ensure that the fitted values fall between zero and one which is not always the case with LPM; however, curve-fitting grounds and predictions are not decisive in our context (Angrist and Pischke, 2008, p.80). Furthermore, including fixed effects would make probit estimates inconsistent (Fernández-Val, 2009). However, in our model it is crucial to include municipality and time-fixed effects in order to control for unobserved time-invariant

municipality factors and common dynamics in direct democratic activity over time.¹³ There is a crucial point why our identification strategy could fail if we were not including municipality fixed effects: The signature requirement in municipality j could be correlated with the initiatives in municipality i through a spatially correlated unobserved factor that determines both the minimum signature requirement and initiatives. Moreover, neighboring population sizes (which determine the neighbors' signature requirement) could be correlated with spatially correlated unobservables that also affect initiatives in i . We mitigate this potential endogeneity by including municipality-fixed effects.

There is another reason why we use LPM: Employing an instrumental variable approach within the framework of non-linear models would lead to severe additional complexity.¹⁴ However, instrumenting the spatial lag is indispensable for not being taken in by spurious correlation. Throughout our analysis, we cluster standard errors at the county level.

4 Results

4.1 Main results

Our baseline second stage results are collected in Table 1. The main explanatory variables of interest is the spatial lag, i.e. the average number of initiatives in neighbor municipalities within a 50 km radius. As specified above, we instrument this spatial lag on the (state-imposed) signature requirements and the covariates of the neighboring municipalities.

The first stage results are reported in Table A2 of the appendix. Our main instrument, the signature requirement, is statistically significant and negative. This evidence of a negative effect of signature requirements on direct democratic activity is consistent with the existing literature (Arnold and Freier, 2015; Asatryan et al., 2016).

¹³ Bazzi and Blattman (2014) and Friedman and Schady (2013) also use LPM in order to be able to include fixed effects.

¹⁴ Angrist and Pischke (2008, p. 80) also put this point forward when arguing in favor of LPM. Beck (2011) discusses the trade-off between LPM and non-linear models.

Table 1: Second stage effects of spillovers from neighbors' initiatives

	(1)	(2)	(3)	(4)	(5)
VARIABLE	Citizen initiative dummy				
Spatial lag (neighbor initiatives) in t	0.8714*** (0.2691)	0.6446* (0.3599)			
Spatial lag (neighbor initiatives) in $t - 1$			0.8838** (0.4189)		
Spatial lag (neighbor initiatives) in $t - 2$				0.9457** (0.4014)	
Spatial lag (neighbor initiatives) in $t - 3$					0.0363 (0.3263)
Signature requirement		-0.1335 (0.1067)	-0.1250 (0.1089)	-0.1728* (0.1000)	-0.2289** (0.0911)
Ln population		0.0075 (0.0064)	0.0072 (0.0077)	0.0092 (0.0106)	0.0062 (0.0153)
Unemployment rate		0.0435 (0.0272)	0.0656** (0.0296)	0.0636* (0.0350)	0.0137 (0.0389)
Share of population over 65		0.0217 (0.0292)	0.0118 (0.0308)	0.0144 (0.0323)	0.0000 (0.0352)
Left share		-0.0033 (0.0124)	-0.0093 (0.0130)	-0.0207* (0.0119)	-0.0253* (0.0136)
Observations	101,673	100,481	90,766	80,992	71,164
Number of municipalities	9,944	9,939	9,716	9,432	8,897
Hansen-J p-value	0.570	0.747	0.488	0.189	0.236
Kleibergen-Paap F	18.19	10.54	13.67	18.76	21.18
Kleibergen-Paap LM	49.42	35.97	45.49	59.56	74.83

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: The table presents results for the second stage estimation of the linear probability model specified in equation 1. All regressions include time and municipal fixed effects. Standard errors are clustered at the county level.

In Table 1 we report several tests for the validity of our specification and the strength of our instruments. The Hansen-J overidentification test checks if the instruments are uncorrelated with the error term. All specifications show large p-values which imply that our instruments are valid. The Kleibergen-Paap LM underidentification statistic tests whether the employed instruments are relevant, the null hypothesis being that they are irrelevant. In addition, the Kleibergen-Paap F statistic tests for weak identification, the null hypothesis

being that instruments are weak.¹⁵ The results of the Kleibergen-Paap statistics show that the instruments are sufficiently strong.

In the main results presented in Table 1, we obtain a significant and positive estimate for the spatial lag of initiatives which is robust to a number of specifications. After controlling for municipal characteristics and fixed effects, the probability of having an initiative in period t increases significantly when the neighbors host more initiatives (column 2). In column 3-5 of Table 1 we substitute the contemporaneous spatial lag by the first, second and third lagged values in order to study whether the spillover effects are persistent over time. Indeed, we observe that the effects hold over time. Spillovers from initiatives one and two years before appear to be even stronger than simultaneous spillovers. This is well possible given the long process of starting an initiative, collecting signatures and, if successful, eventually holding a vote. Also, knowledge about neighboring initiatives might not diffuse instantly and communities might take their time to organize an initiative after observing initiatives in neighbor municipalities. Consistently though, spillover effects vanish when moving even further into the past (column 5).

For the second lag, the estimated coefficient is 0.96, which indicates that the probability of having an initiative increases by 96 percentage points if – on average – all neighbor municipalities have one initiative more. Given that the average number of neighbor municipalities is 460, this implies an unrealistic increase in the number of neighbor initiatives by 460. To aid in interpretability, we compute results that show the effect of a one standard deviation increase in the number of neighbor initiatives. A one standard deviation increase in neighbor initiatives increases the probability of having an initiative by 2.13 percentage points.

¹⁵ The test replaces the Cragg-Donald weak identification test in the case of heteroskedastic standard errors.

4.2 Robustness tests

As robustness test, we depart from our neighborhood definition which considers municipalities within a radius of 50 km. We estimate the spillover effects from neighbor initiatives for radii in a range from 5 to 70 km, in steps of 5 km, and collect the results in Figure 1. Spatial lags containing municipalities within a radius of 5 and 10 km mostly yield insignificant effects. An explanation is that larger municipalities are dropped from this sample as they do not have neighbors within these radii.¹⁶ For lag 1 and 2, spillover effects decline for increasing radii from 20 km on. This confirms that spillover effects are regionally restricted, e.g. due to limited information flows across space, and close-by municipalities being more relevant for each other than further apart ones.

As a placebo test we regress the initiative dummy on forwarded spatial lags. The idea is that spillover effects from future initiatives on initiatives today cannot happen. As expected, the effects are insignificant. In Figure A2 we plot this placebo test together with the effects from Table 1.¹⁷

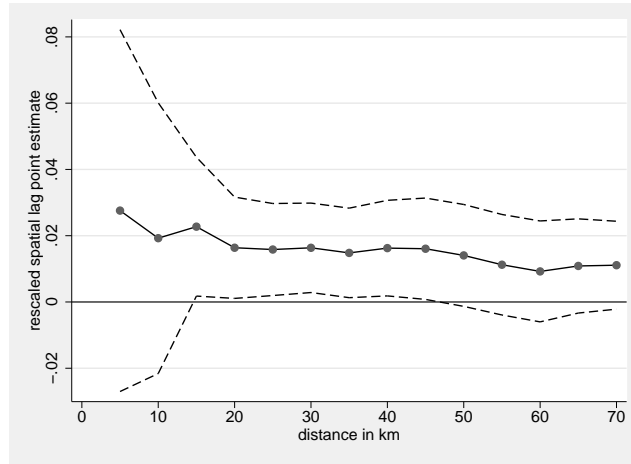
4.3 Extension of results

In this sub-section, we extend our main results by two important empirical tests to shed more light on the spillover mechanisms. Firstly, we ask whether the spillovers are due to learning about the possibility of holding initiatives or due to learning about specific (optimal) policies. Secondly, we are interested to see whether informational channels – such as newspaper circulation or direct personal contacts across municipalities – play a role in these patterns of interactions as it is the case for the diffusion of movements and the diffusion of policies due to yardstick competition (see section 2).

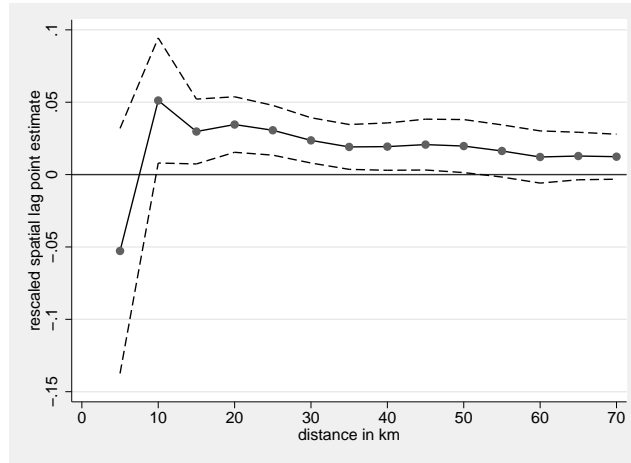
¹⁶ Note that distances are measured from the centroid of a town.

¹⁷ The effect in $t = 1$ corresponds to the spatial lag in $t - 1$, i.e. column 2 in Table 1, as we take the neighborhood's perspective in the graph and look how long it takes until the spillover effects happen.

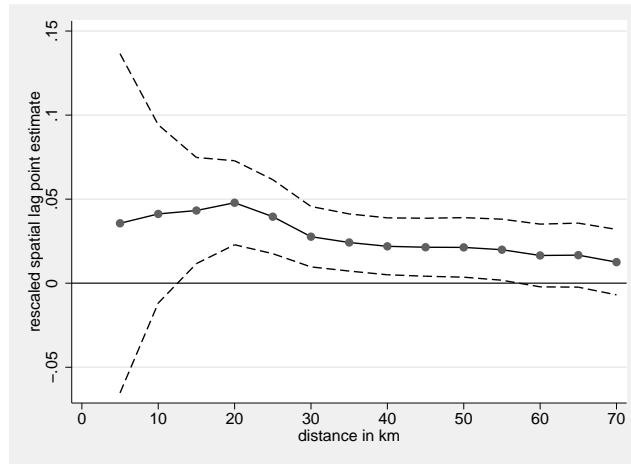
Figure 1: Robustness of main results to neighborhood-selection



(a) Lag 0



(b) Lag 1



(c) Lag 2

Note: These graphs show the point estimates with 95% confidence intervals of the spatial lag estimated from equation 1. The spatial lag contains municipalities within different radii. The point estimates and the standard error are rescaled by the respective standard deviation of each spatial lag. All regressions include time and municipality fixed effects as well as the control variables as defined before. Standard errors are clustered at the county level.

Up to now, we have demonstrated that all initiatives in the neighborhood positively affect the likelihood of hosting any initiative. This result allows arguing that the use of direct democratic instruments might have spillovers itself as voters learn about a new political tool they can exercise in general.¹⁸

¹⁸ Parallels can be drawn to the literature that studies the cross-border diffusion of democracy (Elkink, 2011), regime change (Brinks and Coppedge, 2006), and riots (Aidt and Franck, 2015).

Table 2: Second stage effects of spillovers from neighbors' initiatives by topic

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	initiative traffic			initiative economy			initiative infrastructure		
Spatial lag traffic	-0.6820 (0.8684)			-0.0739 (1.1306)			-0.4833 (1.0842)		
Spatial lag economy		0.2073 (0.2129)			0.8442** (0.4062)			-0.0195 (0.4293)	
Spatial lag infrastructure			-0.1266 (0.2587)			0.0984 (0.3272)			0.5985* (0.3496)
Signature requirement	-0.1132 (0.0973)	-0.1158 (0.0946)	-0.1154 (0.1025)	-0.1359 (0.0970)	-0.1598* (0.0943)	-0.1312 (0.1030)	-0.2093** (0.0963)	-0.2063** (0.0956)	-0.1806* (0.1043)
Ln population	-0.0033 (0.0035)	-0.0044 (0.0033)	-0.0036 (0.0039)	0.0074 (0.0081)	0.0010 (0.0073)	0.0081 (0.0082)	-0.0081 (0.0070)	-0.0076 (0.0064)	-0.0039 (0.0075)
Unemployment rate	0.0168 (0.0159)	0.0227 (0.0217)	0.0207 (0.0177)	0.0032 (0.0218)	-0.0033 (0.0251)	0.0070 (0.0224)	-0.0102 (0.0219)	-0.0046 (0.0261)	0.0131 (0.0233)
Share of population over 65	0.0090 (0.0174)	0.0011 (0.0144)	0.0064 (0.0157)	0.0042 (0.0233)	-0.0155 (0.0216)	0.0035 (0.0221)	0.0482** (0.0229)	0.0464** (0.0213)	0.0433** (0.0217)
Left share	-0.0031 (0.0060)	-0.0040 (0.0050)	-0.0050 (0.0048)	-0.0071 (0.0102)	-0.0022 (0.0092)	-0.0076 (0.0091)	-0.0234*** (0.0079)	-0.0251*** (0.0076)	-0.0263*** (0.0071)
Observations	80,992	80,992	80,992	80,992	80,992	80,992	80,992	80,992	80,992
Number of ags	9,432	9,432	9,432	9,432	9,432	9,432	9,432	9,432	9,432
Hansen-J p-value	0.361	0.424	0.349	0.0825	0.243	0.0664	0.0638	0.0604	0.261
Kleibergen-Paap F	7.552	13.54	25.28	7.552	13.54	25.28	7.552	13.54	25.28
Kleibergen-Paap LM	43.27	42.17	73.47	43.27	42.17	73.47	43.27	42.17	73.47

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table presents second stage estimates of the linear probability model specified in equation 1. Spatial lags contain municipalities within a radius of 50 km. We use the spatial lag in $t - 2$. All regressions include time and municipal fixed effects as defined before. Standard errors are clustered at the county level.

Table 3: Effects of spillovers from neighbors' initiatives by number of newspapers per household and commuters per capita

VARIABLE	(1)	(2)	(3)	(4)
	Citizen initiative dummy			
	local newspapers below median above median		commuters p.c. below median above median	
Spatial lag (neighbor initiatives)	0.7041 (0.6145)	1.1689*** (0.4409)	0.4290 (0.5886)	0.8436* (0.5073)
Signature requirement	-0.1959 (0.1407)	-0.0461 (0.0734)	-0.0905 (0.1203)	-0.2067** (0.0951)
Ln population	-0.0023 (0.0130)	0.0114 (0.0156)	-0.0026 (0.0178)	0.0019 (0.0192)
Unemployment share	0.0293 (0.0530)	0.0454 (0.0534)	0.0426 (0.0537)	0.0654 (0.0769)
Share of population over 65	0.0196 (0.0476)	0.0142 (0.0566)	0.0602 (0.0578)	0.0670 (0.0700)
Left share	-0.0205 (0.0167)	-0.0210 (0.0207)	-0.0619*** (0.0230)	-0.0031 (0.0236)
Observations	37,093	37,109	37,006	36,906
Number of municipalities	3,787	3,637	7,209	6,210
Hansen-J p-value	0.272	0.793	0.763	0.377
Kleibergen-Paap F	19.11	14.50	6.375	27.27
Kleibergen-Paap LM	48.69	40.46	38.91	48.05

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table presents second stage estimates of the linear probability model specified in equation 1. All spatial lags contain municipalities within a radius of max. 50 km. We use the spatial lag in $t - 2$. All regressions include time and municipal fixed effects. Standard errors are clustered at the county level.

In Table 2 we advance a step further by dividing the initiatives into three main public policy areas – traffic, economy and infrastructure – over which initiatives can be held. These topics are by far the most frequent ones and jointly cover 83% of all. We then estimate the baseline regressions with controls and fixed effects both within and across these topics. Two out of three within-topic coefficients reported on the diagonal of Table 2 show significant effects. These results broadly support the hypothesis that the interaction effects are largely driven by spillovers in specific public policies. We do not detect evidence for spillovers across different topics.¹⁹

Next, we test whether the cross-municipal spillovers in initiatives are conditional on the availability and exchange of information. One of the important spillover mechanisms we have in mind is one that functions through popular media. Vetoing or inaugurating a certain policy in one municipality may have the most impact on neighboring town’s voters when they are sufficiently informed. To test this hypothesis we divide the municipalities according to whether on a per household basis these purchase daily regional newspapers above or below the median town. Regional newspapers are a primary source of information for citizens on local issues.²⁰ Results for the baseline specifications for the two sub-samples are collected in Table 3 (column 1-2). Conditional on the town-size and a number of further controls, we find robust evidence of significant spatial interactions in towns with above-median number of newspapers but not for those with below-median newspapers.²¹ This exercise reveals that information is one important transmission channel through which spillovers in initiatives spread.

¹⁹ Due to different topic restrictions depending on the respective municipal codes, there cannot be any mimicking in some specific public policies across neighboring municipalities. Thus, in more homogeneous settings than this one, the mimicking in direct democratic institutions would probably be higher.

²⁰ We rely on the data on 2008’s newspaper circulation by Falck et al. (2014) which in turn rely on data from the German Audit Bureau of Circulations (Informationsgemeinschaft zur Feststellung der Verbreitung von Werbeträgern, IVW, 2011). The data provides the annual number of daily newspapers per household for each municipality in 2008.

²¹ The number of newspapers is not correlated with other variables that affect the probability of hosting an initiative. Means and standard deviations of the control variables are similar for the two sub-samples.

Finally, we test whether more direct channels of information diffusion play a role for direct democratic activities having effects on neighbors. Direct channels refer to personal contacts of citizens from different municipalities. To test this hypothesis we divide our sample according to whether a municipality ‘hosts’ incoming commuters above or below the median town.²² Results for the baseline specification for the two sub-samples are collected in Table 3 (column 3-4). Conditional on the town-size and a number of further controls, we find robust evidence of significant spatial spillovers into towns with above-median number of commuters per capita but not for those with below-median commuters.²³ This indicates that direct channels of information diffusion between municipalities play a role for spillovers in direct democracy.

5 Conclusions

To the best of our knowledge, previous literature has not yet tested or conceptualized the proposed hypothesis that voters of related jurisdictions may mimic each others behavior through the means of direct democracy. The channels of such interactions may be quite different and complex, going from spillovers in specific policies to the process of learning to exploit direct democratic rights. Further research on the behavior of individuals and, especially, organized groups of individuals in the social interaction analysis may help to shed more light into these and perhaps more interaction mechanisms. However, what this analysis adds to the literature is that it is plausible that mimicking between jurisdictions takes place not only through government-level interactions (learning, coercion, fiscal and yardstick competition), but also through voters’ direct actions in vetoing and inaugurating policies through binding-initiatives. Such interactions are, of course, conditional on the

²² We use commuters data from the Federal Statistical Office (“Regionaldatenbank”).

²³ The number of commuters is not correlated with other variables that affect the probability of hosting an initiative. Means and standard deviations of the control variables are similar for the two sub-samples.

existence of some institutions of direct democracy, which are currently not any more rare especially in sub-national levels of high-income countries.

If this reasoning is true, then it is important to recognize and quantify such interactions, because direct democracy matters for policy-outcomes. Studies traditionally concentrating on US and Switzerland, but more recently also extending to Germany and other countries, find empirical support that direct democratic instruments affect policies in the public sector in general, and fiscal decisions in particular. Although, we find evidence for spatial spillovers in initiatives, it is left to future work to assess to what extent such mimicking behavior drives policy outcomes.

Acknowledgement

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Figure A1: Geographical distribution of initiatives in German towns from 2002 to 2014

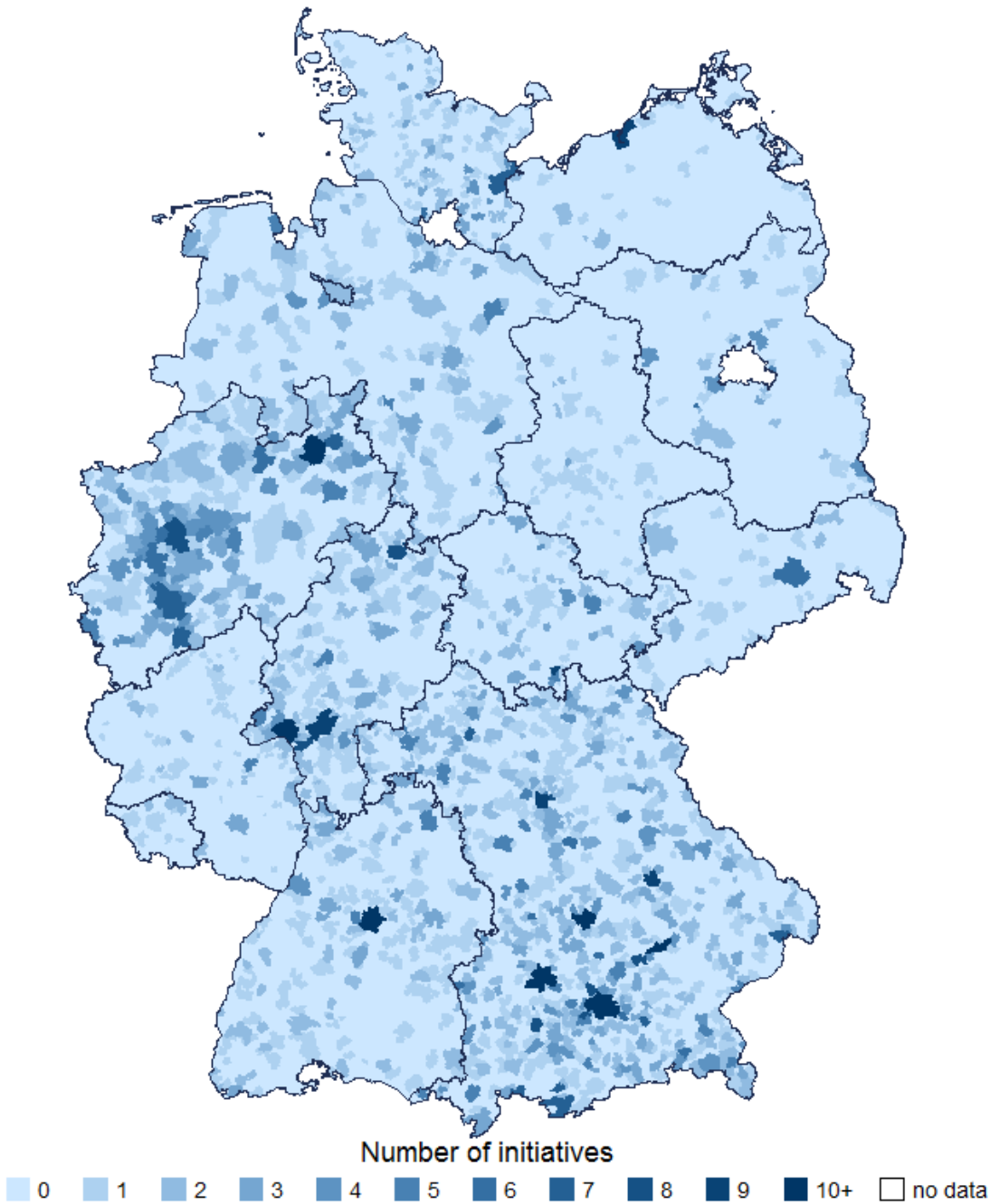


Table A1: Institutions and summary statistics across states, 2002-2014

STATE	Year	Signature direct democracy adopted	Signature requirement (mean)	(min-max)	Number of initiatives	Total annual population (mean)	Unemployment rate (mean)	Share of population over 65 (mean)	Left share (mean)	Number of towns in the sample
Baden-Württemberg	1956	0.0983	(0.0333-0.1)	256	9671	0.0335	0.181	0.378	1112	
Bayern	1995	0.0983	(0.03-0.1)	1272	6083	0.0350	0.181	0.295	2056	
Brandenburg	1993	0.1	(0.1)	80	5514	0.120	0.206	0.602	929	
Hessen	1993	0.0985	(0.03-0.1)	250	14160	0.0492	0.201	0.484	430	
Mecklenburg-Vorpommern	1993	0.0997	(0.0196-0.1)	49	1975	0.134	0.188	0.507	1829	
Niedersachsen	1996	0.0999	(0.0708-0.1)	223	7671	0.0563	0.200	0.487	1061	
Nordrhein-Westfalen	1994	0.080	(0.03-0.1)	426	45121	0.0577	0.191	0.446	406	
Rheinland-Pfalz	1994	0.130	(0.0601-0.15)	102	1748	0.0412	0.201	0.434	2311	
Saarland	1997	0.1	(0.1)	13	19777	0.0532	0.217	0.527	52	
Sachsen	1990	0.1433	(0.1-0.15)	116	8552	0.108	0.225	0.428	1042	
Sachsen-Anhalt	1990	0.1417	(0.0043-0.15)	109	3215	0.137	0.205	0.565	2311	
Schleswig-Holstein	1990	0.0999	(0.04-0.1)	232	2519	0.0504	0.192	0.468	1142	
Thüringen	1993	0.0887	(0.034-0.1)	131	2396	0.091	0.202	0.511	1032	

Notes: The data on observed initiatives (column: 5) is available at <http://www.mehr-demokratie.de/bb-datenbank.html>. The data on the signature requirement (columns: 3-4) is collected from the state's codes. The control variables (columns: 6, 8 and 9) come from the Federal Statistical Office ("Regionaldatenbank"). The data on unemployment is taken from the Bundesagentur für Arbeit.

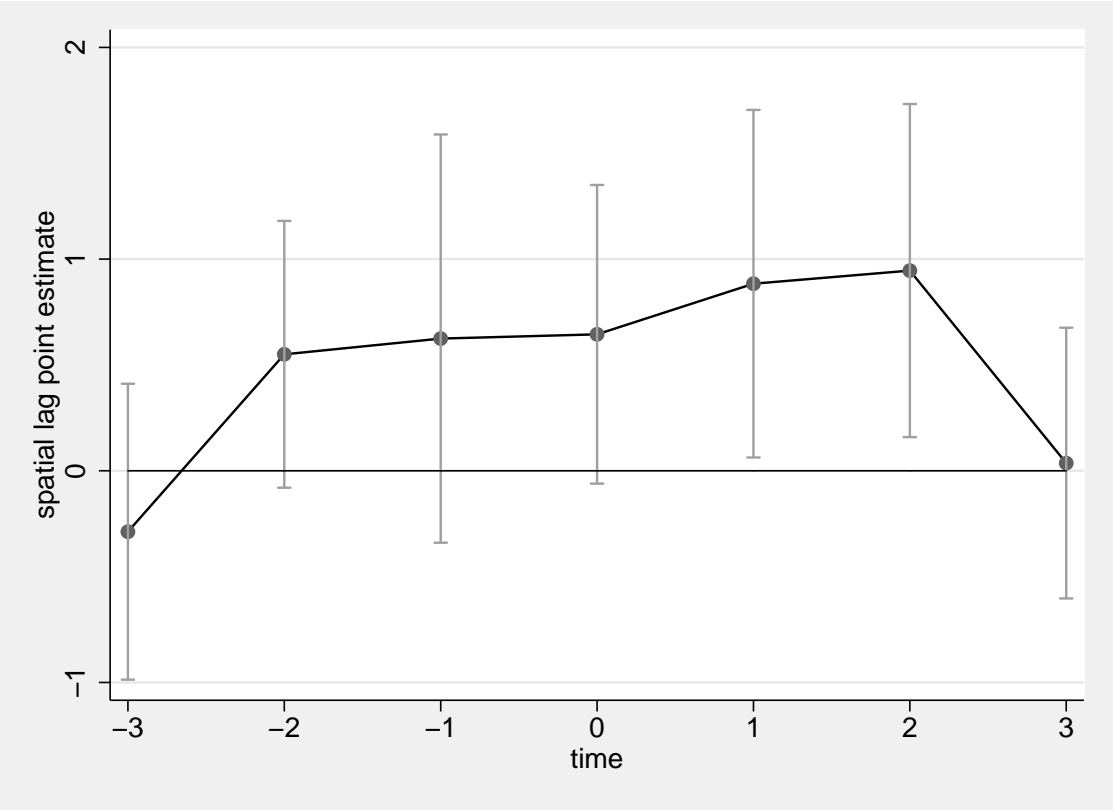
Table A2: First-stage results of Table 1 - determinants of citizen-initiatives

VARIABLE	(1)	(2)	(3)	(4)	(5)
	Spatial lag citizen initiatives				
	t	$t - 1$	$t - 2$	$t - 3$	
$\sum_{j \neq i}^N (w_j \times \text{signature requirement } j)$	-0.1233*** (0.0169)	-0.1366*** (0.0341)	-0.0677*** (0.0245)	-0.1374*** (0.0185)	-0.1876*** (0.0306)
$\sum_{j \neq i}^N (w_j \times \ln \text{ population } j)$	0.0022 (0.0032)	0.0051* (0.0030)	0.0017 (0.0032)	-0.0017 (0.0025)	-0.0014 (0.0026)
$\sum_{j \neq i}^N (w_j \times \text{unemployment share } j)$	-0.0876*** (0.0308)	-0.0812** (0.0315)	-0.1363*** (0.0310)	-0.1095*** (0.0331)	-0.1706*** (0.0419)
$\sum_{j \neq i}^N (w_j \times \text{share of population over 65 } j)$	0.1997** (0.0866)	0.1423 (0.0872)	0.1669* (0.0950)	0.3585*** (0.1021)	0.3222** (0.1426)
$\sum_{j \neq i}^N (w_j \times \text{left share } j)$	-0.0125 (0.0107)	-0.0149 (0.0135)	-0.0032 (0.0108)	0.0121 (0.0132)	-0.0092 (0.0139)
Signature requirement		0.0222 (0.0320)	-0.0319 (0.0253)	0.0007 (0.0223)	-0.0007 (0.0265)
Ln population		0.0025 (0.0019)	0.0040* (0.0024)	0.0022 (0.0027)	-0.0006 (0.0035)
Unemployment rate		-0.0044 (0.0085)	0.0092 (0.0093)	-0.0036 (0.0115)	0.0457*** (0.0150)
Share of population over 65		0.0037 (0.0048)	0.0063 (0.0063)	0.0119 (0.0077)	0.0081 (0.0087)
Left share		-0.0048* (0.0026)	-0.0041 (0.0033)	0.0061 (0.0052)	0.0009 (0.0052)
Observations	102,878	101,670	90,990	81,283	71,701
R-squared	0.0763	0.0790	0.0811	0.0844	0.0964
Number of municipalities	11,149	11,128	9,940	9,723	9,435

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table presents the results for the OLS first stage estimation of the model specified in equation 2. All regressions include time and municipal fixed effects. Standard errors are clustered at the county level.

Figure A2: Spillover effects over time



This graph shows the point estimates with 95% confidence intervals of the spatial lag estimated from equation 1. Spatial lags contain municipalities within a radius of 50 km. All regressions include time and municipality fixed effects and the control variables as defined before. Standard errors are clustered at the county level.