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Abstract

A sizeable literature studies whether governments strategically interact with each other through policy-diffusion, learning, 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 and special interest groups of neighboring jurisdictions. Utilizing data on around 1,800 voter-petitions across over 12,000 German municipalities in 2002-09, we find that a jurisdiction's probability of hosting a petition is positively driven by the neighbors' direct democratic activity. These effects are persistent, and are stronger for more visible instruments of direct democracy. The interactions are also mostly driven by petitions in same or similiar policy areas, and are stronger in towns with relatively more per capita newspapers.

Keywords: Direct democracy, spatial spillovers, policy diffusion, citizen preferences

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 competition, coercion and learning. 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 local 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 municipalities when borders across municipalities are porous (Acemoglu et al., 2015). A particular economic constraint, however, may be due to *competition* for attracting mobile resources such as labor and capital through fiscal (including tax) competition (Tiebout, 1956; Wilson, 1999). In a principal-agent framework with incomplete information, a decision-maker is additionally subject to *yardstick competition* 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 (the success or failure of) neighbor's policies and mimic these accordingly. 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. The *first* contribution of this paper is to study 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 and special interest groups of other jurisdictions.

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 hypothesis we aim to test in this paper.

Regarding the relevance of the hypothesis, 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 hypothesis of

interactions through direct decision-making mechanisms seems timely. Moreover, 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 that rely on arguably more credible identification techniques by utilizing sources of exogenous variation (see, e.g. Lyytikäinen, 2012; Isen, 2014; Baskaran, 2014, 2015) find that some of the previously documented strong effects could be due to spurious correlations. The *second* contribution of our paper is to add to this dilemma by not only considering a possibly new mechanism for the effects, but also by departing from the standard approach to spatial econometrics and relying instead on quasi-exogenous variation in our treatment.

In particular, we focus on Germany, where since the mid-1990s citizens have the power to veto (some of) local governments' decisions and propose certain new policies by launching binding initiatives. Our empirical strategy is to test for spatial interactions in the occurence of these initiatives for a sample of around 12,000 German towns from 2002 to 2009. We apply spatial reaction functions and exploit a (quasi-random) exogenous instrument to identify interactions between municipalities. Our findings suggest that the probability of observing a petition or an initiative in a municipality is positively driven by its neighbors' activity in direct democracy. The response of an average municipality is strong with the probability of hosting a petition rising by about 26% if one of its neighbors hosts one more petition. Such effects are persistent but decreasing over time and space, and are stronger for institutions of direct democracy which are better observable. These spatial interactions are also mostly driven by petitions in same or similiar policy areas, and are stronger in towns with relatively more per capita newspapers. Our baseline results are robust to different definitions of neighborhood and weighting schemes.

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. Finally, 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.

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 Länder (henceforth: states) 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 since 1956. Berlin is the last state which introduced the possibility of petitions and initiatives in 2005.

These institutions enable the citizens to launch so-called *citizen petitions* ("Bürgerbegehren") which are divided into *innovative petitions* ("Initiativbegehren") and *corrective petitions* ("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 a petition to be successfully implemented, the initiators face several constrains. First, town- and state-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 negative decision, the next step of the procedure is reached, i.e. the petition is implemented as an *initiative* ("Bürgerentscheid"). Besides a simple majority, some states further require a certain quorum (minimum turnout relative to population) for the initiative to be accepted. Another limitation concerning local direct democracy in Germany are topic exclusions. In all states topics which directly concern the municipal budgets are not allowed for petitions (*fiscal taboo*). Moreover, each state has a list of other prohibited topics (*off-limits issues*) or even a narrow list of allowed topics (*positive catalogue*). All states except Bayern and Bremen also demand a cost-recovery proposal with the suggested petition.

Looking into the data we observe a higher activity of direct democracy in states with less strict institutions. For example, Bayern which has comparatively liberal institutions already launched around 2,500 petitions until 2013. On the contrary, Baden-Württemberg with

very rigorous regulations only launched less than 80 petitions until 2013.[1] The geographical distribution of the number of petitions is illustrated in a heat-map in Figure A1 of the appendix, and Table A1 summarizes some of 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. In more general terms, citizens may more effectively use their "voice" (Hirschman, 1970) when observing high democratic engagement in neighboring jurisdictions.[2] 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, since 2007 there have been several petitions which all aimed at stopping the project.[3] This direct democratic engagement by the population supposedly had spillover effects on the citizens of other municipalities, for example, in the close-by town of Leonberg where citizens launched a petition in 2011 against the demolition of the public indoor swimming pool and the related plans of building a new swimming pool. One of the initiators of the petition explicitly stated that their activities have been inspired by the Stuttgart 21 opposition.[4] Besides the diffusion

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

[2] In practice, this interaction can not only occur by mimicking of petitions and initiatives but also by less formal means, for example by demonstrations or informal initiatives. However, in our paper we focus on spillovers of official petitions and initiatives. In a sense, our results therefore constitute a lower bound estimate for spillover effects in direct democratic engagement of citizens.

[3] 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`

[4] 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`

in direct democratic *activity*, this case illustrates an additional notion of diffusion, namely the diffusion of *preferences*. With the petitions at hand, citizens wanted to preserve an already existing infrastructure instead of demolishing it and building a new one.

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 a petition against building a new city hall which was accompanied by many newspaper articles about the topic.[5] Shortly afterwards, a very similar discussion arose in Seefeld which then also led to a petition.

These observations are related to the mechanisms described by the literature on diffusion processes within and across movements.[6] Proximal models stipulate that actors mimic other people's or groups' strategies 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 for the case of direct democracy when, for example, special interest groups spread to close-by municipalities. However, it is also well 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) when the organizers are "unconnected". In the context of race riots in the US, for example, Myers (2000) finds that wider media coverage increases the penetration of riots in

[5] For example, see the following reports from the two newspapers which cover both of the 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`.

[6] Please see Snow et al. (2004) in general, and Soule's contribution in particular for a summary on the diffusion research in the field of social movements.

neighboring areas. 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 spillovers may be due to more 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.

In the case of yardstick competition, citizens evaluate their politicians' performance by comparing their jurisdiction's outcomes with the one's of their neighbors. [7]

3 Data and specification

We test for spillovers direct democratic activity by specifying a reaction function (spatial lag model) similar to the approach employed by the literature on public budget spillovers, e.g. Foucault et al. (2008) for public expenditure decisions in French municipalities and Solé-Ollé

[7] Besley and Case (1995) provide theoretical and empirical evidence on this in the context of US states's tax-setting and Rincke (2009) empirically identifies yardstick competition in the context of public sector technologies.

(2001) for local governments in Spain.[8] Our empirical specification is also in line with the tax reaction functions used by the sizable literature on tax interactions between municipalities (see, e.g. Allers and Elhorst, 2005; Leprince et al., 2007; Bordignon et al., 2003; Buettner, 2001; Hauptmeier et al., 2012) and between countries (see, e.g. Davies and Voget, 2008; Devereux et al., 2008; Egger and Raff, 2014; Overesch and Rincke, 2011; Redoano, 2014).

We specify the following linear probability model in order to test if citizens mimic their dire000ct democratic activities across jurisdictions:

$$d\_p_{it} = \delta \sum_{j \neq i}^{N} w_j p_{jt} + X_{it}\beta + \alpha_{1s} + \mu_{1t} + \varepsilon_{1it} \tag{1}$$

where the dependent variable d_p_{it} is a dummy which is one if there was at least one petition launched by citizens of municipality i in year t. On the right hand side, X_{it} is a set of standard demographic and political controls on municipality level; α_{1s} and μ_{1t} are state and year dummies, and ε_{1it} is an unobserved error term. The spatial lag ($\sum_{j=1}^{N} w_j p_{jt}$) constitutes the variable of interest which is the (weighted) sum of the number of initiatives in neighbor municipalities of i weighted by different schemes (see below). In the baseline specification municipalities within a 50 km radius of municipality i qualify as neighbors in our main specification. 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.[9]

[8] Please also see Besley and Case (1995) for public spending interactions.

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

Similarly, the fiscal spillover literature also assumes geographically close jurisdictions to have a greater effect on each other than more remote jurisdictions and therefore takes into account the distance between jurisdictions when setting up fiscal reaction functions, 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.

An ex-ante decision has to be made concerning the weight w_j which is attached to each neighbor municipality j of municipality i. For our baseline specification, all municipalities within 50 km radius of municipality i are perceived as equally important neighbors and receive the same weight. We normalize the sum of these contiguity weights to one, thus the spatial lag is simply the average number of petitions in the neighbor municipalities. For robustness, we vary the definition of neighbor municipalities by varying the radius to 30 and 70 km. For additional robustness consideration, we use population weights to give credit to the notion that large neighbor municipalities and their direct democratic activities are more visible for citizens than the ones of smaller municipalities.[10] This might be in particular true for large municipalities which are likely to compare themselves with other large municipalities rather than with smaller ones.[11]

Our underlying data consists of an unbalanced panel of 12,000 to 13,000 German municipalities across all German states for the years from 2002 to 2009 except of the city states Hamburg and Berlin.[12] Table A1 of the appendix summarizes the data on: state-level institutions of direct democracy;[13] municipality-level data on the frequency of observed petitions and initiative as our dependent variable; and a number of control variables such as 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).

Such reaction functions may be subject to a major endogeneity concern: we explicitly assume that the likelihood of a petitions in municipality i depends on the number of petitions

[10] We normalize the sum of the neighbors' population to one.

[11] Population weights are also used by the more classical spillover literature, for example by Brueckner and Saavedra (2001) and Baskaran (2014) in a tax competition setting with US and German municipalities respectively.

[12] We exclude these special "city states" since initiatives there are either implemented on the state- (same as city) or district level, both being something different than municipalities.

[13] 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 respective state constitutions.

in municipality j and vice versa - this makes the spatial lag endogenous by definition. The problem can be circumvented by applying appropriate instruments to the spatial lag. The literature on budget and tax-setting spillovers cited above 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 (2014), we mitigate 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 as described above. The first-stage specification therefore regresses the spatial lag on the (weighted) averaged control variables of the neighbor municipalities (including the *signature requirement*) and all of the previous regressors (including the fixed effects) and takes the following form:

$$\sum_{j \neq i}^{N} w_j p_{jt} = \beta \sum_{j \neq i}^{N} w_j X_{jt} + X_{it} + \alpha_{2s} + \mu_{2t} + \varepsilon_{2jt} \tag{2}$$

Clearly, our main instrument, the signature requirement, is *relevant* for the frequency of petitions 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 petitions in municipality i (signature requirement is uncorrelated with the error term of the second stage (ε_{it})). This can be safely concluded since the signature requirements are determined by state-laws and not by the municipalities themselves. In addition, these state-laws have been implemented well before the period of analysis mainly in the mid-1990s, but in some cases as early as 1956 (Table A1). Although set by the states, the instrument do not only vary across states but also by municipalities within states depending on population thresholds.

To estimate our (second stage) specification we choose a linear probability model (LPM) which allows for a binary dependent variable. We think that the advantages of LPM prevail in our stetting, 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 to not be fulfilled. Certainly, 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 but marginal effects which, in turn, prove to be quite similar across the models (Angrist and Pischke, 2008, p.80). Furthermore, including fixed effects makes probit estimates inconsistent (Fernéndez-Val, 2009). However, in our model it is crucial to include state-fixed and time-fixed effects in order to control for unobserved time-invariant regional factors and the dynamics in direct democratic activity over time.[14] In addition, we necessarily need to employ an instrumental variable approach to deal with the endogeneity of the spatial lag. Doing this within the framework of non-linear models would lead to severe additional complexity.[15] Throughout our analysis, we use robust standard errors to account for heteroskedasticity, and cluster the standard errors by municipality.

4 Results

4.1 Main results

Our baseline second stage results are collected in Table 1. The main explanatory variables of interest are the spatial lag of the sum of citizen-petitions (columns: 1-4) and citizen-initiatives (columns: 5-8) of neighbor municipalities within a 50 km radius. As specified above, in the first stage we instrument this spatial lag on the (state-imposed) signature requirements and

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

[15] 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' petitions and initiatives

	(1)	(2)	(3)	(4)
VARIABLE	Citizen petition dummy			
Spatial lag citizen petitions in t	0.1114***	0.0453***		
	(0.0977)	(0.0973)		
Spatial lag citizen petitions in $t-1$			0.0417**	
			(0.1066)	
Spatial lag citizen petitions in $t-2$				0.0388**
				(0.1089)
Signature requirement		-0.3562***	-0.3605***	-0.3601***
		(0.2443)	(0.2733)	(0.2975)
Ln population		0.1031***	0.1031***	0.1000***
		(0.0007)	(0.0008)	(0.0008)
Unemployment share		0.0085*	0.0083	0.0040
		(0.0133)	(0.0156)	(0.0186)
Share of population over 65		0.0219***	0.0210***	0.0206***
		(0.0141)	(0.0151)	(0.0164)
Left share		0.0172***	0.0144***	0.0164***
		(0.0048)	(0.0052)	(0.0060)
Observations	97,581	85,499	71,136	57,656
R-squared	0.0203	0.0436	0.0429	0.0439
	(5)	(6)	(7)	(8)
Spatial lag citizen initiatives in t	0.1394***	0.0592**		
	(0.3270)	(0.3093)		
Spatial lag citizen initiatives in $t-1$			0.0461*	
			(0.3348)	
Spatial lag citizen initiatives in $t-2$				0.0456*
				(0.3497)
Signature requirement		-0.3581***	-0.3635***	-0.3622***
		(0.2440)	(0.2726)	(0.2972)
Ln population		0.1029***	0.1033***	0.1002***
		(0.0007)	(0.0008)	(0.0008)
Unemployment share		0.0077	0.0068	0.0033
		(0.0133)	(0.0155)	(0.0185)
Share of population over 65		0.0217***	0.0205***	0.0201***
		(0.0140)	(0.0150)	(0.0163)
Left share		0.0183***	0.0158***	0.0174***
		(0.0048)	(0.0052)	(0.0059)
Observations	97,581	85,499	71,136	57,656
R-squared	0.0163	0.0434	0.0423	0.0423

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

Notes: The table presents beta coefficients for the second stage estimation of the linear probability model specified in equation 1. All regressions include time and state fixed effects as well as border dummies which identify municipalities with a distance of max. 50 kilometers to the respective border. Standard errors are robust to heteroskedasticity and are clustered at the municipality level.

the control variables of neighboring municipalities.[16][17] The first stage results are reported in Table A2 of the appendix and show statistically significant coefficients for all instruments. For the main instrument, as expected, a higher signature requirement is strongly associated with fewer petitions. Such evidence of a causal negative effect of signature requirements on direct democratic activity is also demonstrated by Arnold and Freier (2015).

In the main results presented in Table 1, we obtain a significant and positive estimate for the spatial lag of citizen petitions which is robust to a number of specifications. After controlling for municipal characteristics, the probability of having a petition increases by 0.045 standard deviations, on average, if the spatial lag is one standard deviation higher. This means that the probability of hosting a petition rises by about 26% if the neighbors host one petition more, on average. In columns 3-4 of Table 1 we substitute the spatial lag by the first and second lagged values of the spatial lag in order to study whether the spillover effects are persistent over time. Indeed, we observe that the effects hold over time and, as expected, decrease in size for both of the lagged-effects compared to the simultaneous-effect. In all three specifications with control variables included, larger population, higher share of the over 65 years old population and higher left share are all associated with more frequent petitions, while a higher signature requirement is again correlated with lower direct democratic activity.

As a next step we include citizen-initiatives in the spatial lag instead of petitions holding other details of the specification the same. The idea behind this exercise is that initiatives are likely to have a greater impact on the preferences and behavior of neighbors' citizens than petitions since the former are more salient. To remind, initiatives are defined as those petitions that have already successfully passed the stage of signature collection and have been voted upon, while petitions may merely be in the initial stages of the process and

[16] In order to compare the size of the point estimates across models, we report *beta* coefficients.

[17] To control for the boundary value problem (e.g., Geys and Osterloh, 2013), in all regressions we include neighbor-country-dummies for municipalities within a 50 km distance to Germany's nine neighbors.

thus not known to the greater public. In columns 5-8 of Table 1, in accordance with our expectations, we observe that the spatial lag in each specification is statistically significant and is larger than the size of the point estimates of the previous specifications.

4.2 Robustness tests

As robustness test, we check the sensitivity of our results to different radii in defining neighbors. First, we restrict the choice of neighbor municipalities within a radius of 30 km and, second, we increase the radius to 70 km from the baseline of 50 km. The results reported in columns 1-6 of Table 2 largely confirm the main results. These also demonstrate a stronger spatial effect in close relative to large neighborhoods.

As an additional placebo test, we generate random petitions for all municipalities and regress them on the true spatial lags of neighbor petitions. The placebo petition dummy has the same mean as the true petition dummy. The results are also reported in columns 7-9 of Table 2. As expected, the spatial lags and all other variables are not significant.

Next, we test whether our main results are sensitive to the choice of the weighting scheme of the spatial lag variable. Table 3 replicates the main results by substituting the main explanatory variables of interest with the spatial lag of the *population-weighted* citizen-petitions (columns: 1-4) and citizen-initiatives (columns: 5-8) of neighbor municipalities within a 50 km radius. The results are largely robust.

4.3 Extension of results

In this final sub-section we extend our main results by two important empirical tests to shed more light on the spillover mechanisms. In the first set of regressions we ask whether the spillovers are due to the mimicking of general direct democratic activity or due to the diffusion of specific public policies through petitions on these policies, perhaps accompanied

Table 2: Robustness of main results to neighborhood-selection and placebo petitions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLE	30km			70km			50 km: placebo petition dummy		
Spatial lag citizen petitions in t	0.0527**			0.0384**			-0.0110		
	(0.0955)			(0.1264)			(0.0718)		
Spatial lag citizen petitions in $t-1$		0.0468**			0.0373*			-0.0011	
		(0.1035)			(0.1383)			(0.0822)	
Spatial lag citizen petitions in $t-2$			0.0282			0.0268			-0.0722
			(0.1039)			(0.1473)			(0.0844)
Signature requirement	-0.3549***	-0.3592***	-0.3626***	-0.3569***	-0.3612***	-0.3626***	0.0751	0.1232	0.0683
	(0.2446)	(0.2742)	(0.2992)	(0.2438)	(0.2724)	(0.2966)	(0.0774)	(0.0878)	(0.1015)
Ln population	0.1004***	0.1008***	0.0987***	0.1045***	0.1045***	0.1013***	-0.0003	0.0001	0.0000
	(0.0007)	(0.0008)	(0.0008)	(0.0007)	(0.0008)	(0.0008)	(0.0005)	(0.0006)	(0.0006)
Unemployment share	0.0089*	0.0084	0.0031	0.0086*	0.0084	0.0028	0.0126	0.0219	0.0152
	(0.0131)	(0.0154)	(0.0186)	(0.0135)	(0.0157)	(0.0188)	(0.0171)	(0.0191)	(0.0221)
Share of population over 65	0.0219***	0.0209***	0.0202***	0.0217***	0.0207***	0.0201***	-0.0017	-0.0127	-0.0110
	(0.0139)	(0.0149)	(0.0162)	(0.0143)	(0.0153)	(0.0166)	(0.0146)	(0.0161)	(0.0179)
Left share	0.0149***	0.0123**	0.0158***	0.0228***	0.0200***	0.0227***	0.0010	-0.0002	-0.0026
	(0.0048)	(0.0052)	(0.0059)	(0.0050)	(0.0055)	(0.0062)	(0.0057)	(0.0063)	(0.0071)
Observations	85,492	71,130	57,651	85,499	71,136	57,656	85,499	71,136	57,656
R-squared	0.0446	0.0426	0.0439	0.0429	0.0426	0.0441	0.0005	0.0005	0.0005

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

Notes: The table presents beta coefficients. The variable "placebo petition dummy" has the same mean as the true variable and represents randomly generated petitions. All regressions include time and state fixed effects as well as border dummies which identify municipalities with a distance of max. 30, 50 or 70 kilometers to the respective border. Standard errors are robust to heteroskedasticity and are clustered at the municipality level.

Table 3: Robustness: Second stage effects of spillovers from neighbors' weighted petitions and initiatives

	(1)	(2)	(3)	(4)
VARIABLE	Citizen petition dummy			
Spatial lag citizen petitions in t	0.0545***	0.0253**		
	(0.0115)	(0.0121)		
Spatial lag citizen petitions in $t-1$			0.0283**	
			(0.0129)	
Spatial lag citizen petitions in $t-2$				0.0312**
				(0.0140)
Signature requirement		-0.3663***	-0.3645***	-0.3645***
		(0.2468)	(0.2709)	(0.2951)
Ln population		0.1032***	0.1036***	0.1001***
		(0.0007)	(0.0008)	(0.0008)
Unemployment share		0.0097*	0.0103*	0.0045
		(0.0141)	(0.0162)	(0.0191)
Share of population over 65		0.0219***	0.0212***	0.0214***
		(0.0142)	(0.0153)	(0.0167)
Left share		0.0240***	0.0212***	0.0237***
		(0.0050)	(0.0055)	(0.0062)
Observations	99,458	85,515	71,136	57,656
R-squared	0.0154	0.0422	0.0425	0.0436
	(5)	(6)	(7)	(8)
Spatial lag citizen initiatives in t	0.1141***	0.0693**		
	(0.1004)	(0.0938)		
Spatial lag citizen initiatives in $t-1$			0.0683**	
			(0.0875)	
Spatial lag citizen initiatives in $t-2$				0.0687**
				(0.0915)
Signature requirement		-0.3650***	-0.3631***	-0.3623***
		(0.2476)	(0.2719)	(0.2964)
Ln population		0.1032***	0.1041***	0.1015***
		(0.0007)	(0.0008)	(0.0008)
Unemployment share		0.0085	0.0096	0.0046
		(0.0137)	(0.0161)	(0.0193)
Share of population over 65		0.0217***	0.0206***	0.0202***
		(0.0142)	(0.0152)	(0.0166)
Left share		0.0239***	0.0209***	0.0234***
		(0.0050)	(0.0055)	(0.0062)
Observations	99,458	85,515	71,136	57,656
R-squared	0.0071	0.0390	0.0388	0.0391

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

Notes: The table presents beta coefficients for the second stage estimation of the linear probability model specified in equation 1. All regressions include time and state fixed effects as well as border dummies which identify municipalities with a distance of max. 50 kilometers to the respective border. Standard errors are robust to heteroskedasticity and are clustered at the municipality level.

by shifts in local voters' preferences. Secondly, we are interested to see whether informational channels – such as newspaper circulation – play a role in these patterns of interactions as it is the case for the diffusion of movements.

Up to now, we have demonstrated that all petitions and initiatives in the neighborhood positively affect the likelihood of hosting any petition. 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]

Table 4: Second stage effects of spillovers from neighbors' petitions by topic

	(1)	(2)	(3)	(4)	(5)
Citizen petition dummy	culture	traffic	land use plan	economy	infrastructure
Spatial lag culture	0.0369	-0.0240	0.0289	-0.0565**	0.0180
	(0.3407)	(0.5208)	(0.1860)	(0.6103)	(0.9537)
Spatial lag traffic	0.0187	0.0422*	0.0520***	-0.0116	0.0717***
	(0.1340)	(0.2183)	(0.0958)	(0.2696)	(0.3686)
Spatial lag land use plan	-0.0043	0.0491**	0.0963***	-0.0187	0.0472**
	(0.2597)	(0.4508)	(0.2712)	(0.6459)	(0.8366)
Spatial lag economy	0.0274	0.0131	0.0270	0.0521*	0.0610**
	(0.1156)	(0.2003)	(0.1010)	(0.2475)	(0.3275)
Spatial lag infrastructure	0.0174	0.0143	0.0256*	-0.0056	0.0467***
	(0.0493)	(0.0744)	(0.0291)	(0.0899)	(0.1277)

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

Notes: The table presents second stage estimates as beta coefficients of the linear probability model specified in equation 1. Spatial lags are not lagged and contain municipalities within a radius of 50 kilometers. All regressions include time, state fixed effects, the control variables as defined before and border dummies. The border dummies identify municipalities with a distance of max. 50 kilometers to the respective border. Standard errors are robust to heteroskedasticity and clustered at the municipality level.

[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 5: Effects of spillovers from neighbors' petitions by number of local newspapers per household

VARIABLE	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	citizen petition dummy							
	equally weighted				population-weighted			
No. of newspapers per household	below median		above median		below median		above median	
Spatial lag citizen petitions in t	0.0408		0.0437*		0.0064		0.0365*	
	(0.1495)		(0.1366)		(0.0180)		(0.0207)	
Spatial lag citizen petitions in $t-1$		0.0332		0.0415*		0.0086		0.0412*
		(0.1673)		(0.1453)		(0.0201)		(0.0219)
Signature requirement	-0.4647***	-0.4584***	-0.2357***	-0.2359***	-0.4775***	-0.4615***	-0.2399***	-0.2391***
	(0.3604)	(0.4058)	(0.3848)	(0.4279)	(0.3634)	(0.3999)	(0.3852)	(0.4294)
Ln population	0.1027***	0.1001***	0.1022***	0.1005***	0.1039***	0.1023***	0.1011***	0.0990***
	(0.0011)	(0.0012)	(0.0009)	(0.0010)	(0.0011)	(0.0012)	(0.0009)	(0.0010)
Unemployment share	-0.0029	-0.0044	0.0019	0.0024	-0.0049	-0.0054	0.0035	0.0054
	(0.0195)	(0.0223)	(0.0265)	(0.0302)	(0.0209)	(0.0233)	(0.0268)	(0.0302)
Share of population over 65	0.0254***	0.0249***	0.0253***	0.0245***	0.0250***	0.0246***	0.0267***	0.0263***
	(0.0205)	(0.0214)	(0.0266)	(0.0285)	(0.0203)	(0.0212)	(0.0273)	(0.0295)
Left share	0.0097	0.0086	0.0248***	0.0217***	0.0171***	0.0155**	0.0291***	0.0256***
	(0.0070)	(0.0076)	(0.0077)	(0.0082)	(0.0077)	(0.0083)	(0.0078)	(0.0084)
Observations	37,039	31,287	37,091	31,215	37,053	31,287	37,093	31,215
R-squared	0.0660	0.0654	0.0311	0.0302	0.0657	0.0647	0.0294	0.0295

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

Notes: The table presents beta coefficients for the second stage estimation of the linear probability model specified in equation 1. Beta coefficients are reported. All spatial lags contain municipalities within a radius of max. 50 kilometers. All regressions include time, state fixed effects and border dummies. Standard errors are robust to heteroskedasticity and are clustered at the municipality level.

In Table 4 we advance a step further by dividing the petitions into five main public policy areas over which petitions can be held – culture, traffic, land use, economy and infrastructure – and then estimating the baseline simultaneous regressions with controls both within and across these topics. All five within-topic coefficients except for culture reported on the diagonal of Table 4 show significant effects. These results broadly support the hypothesis that the interaction effects are largely driven by spillovers in specific public policies. Petitions on land use, traffic and infrastructure – topics that are related to each other – are also spatially correlated in the bilateral regressions.

Next, we test whether the cross-municipal spillovers in petitions 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 local newspapers above or below the median town.[19] Results for both the baseline and population-weighted spatial lag indicators for the two sub-samples are collected in Table 5. 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.[20] This exercise reveals that information is one important transmission channel through which spillovers in petitions spread.

[19] We rely on the definition of local newspapers and data on 2008's local newspaper circulation by Falck et al. (2014). The data provides the annual number of daily local newspapers per household for each municipality in 2008.

[20] The number of newspapers does not seem to be correlated with other variables that affect the probability of hosting a petition. Means and standard deviations of the control variables are similar for the two sub-samples.

5 Conclusions

To the best of our knowledge, previous literature has not yet tested or conceptualized the proposed hypothesis that voters and interest groups 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 policy-preferences 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 (e.g., Manski, 2000) may help to shed more light into these and perhaps more interaction mechanisms. However, what this analysis adds to the literature is that mimicking between jurisdictions takes place not only through government-level interactions (learning, diffusion, 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 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 do such mimicking behavior drive policy outcomes.

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Figure A1: Geographical distribution of petitions in German towns from 2002 to 2009

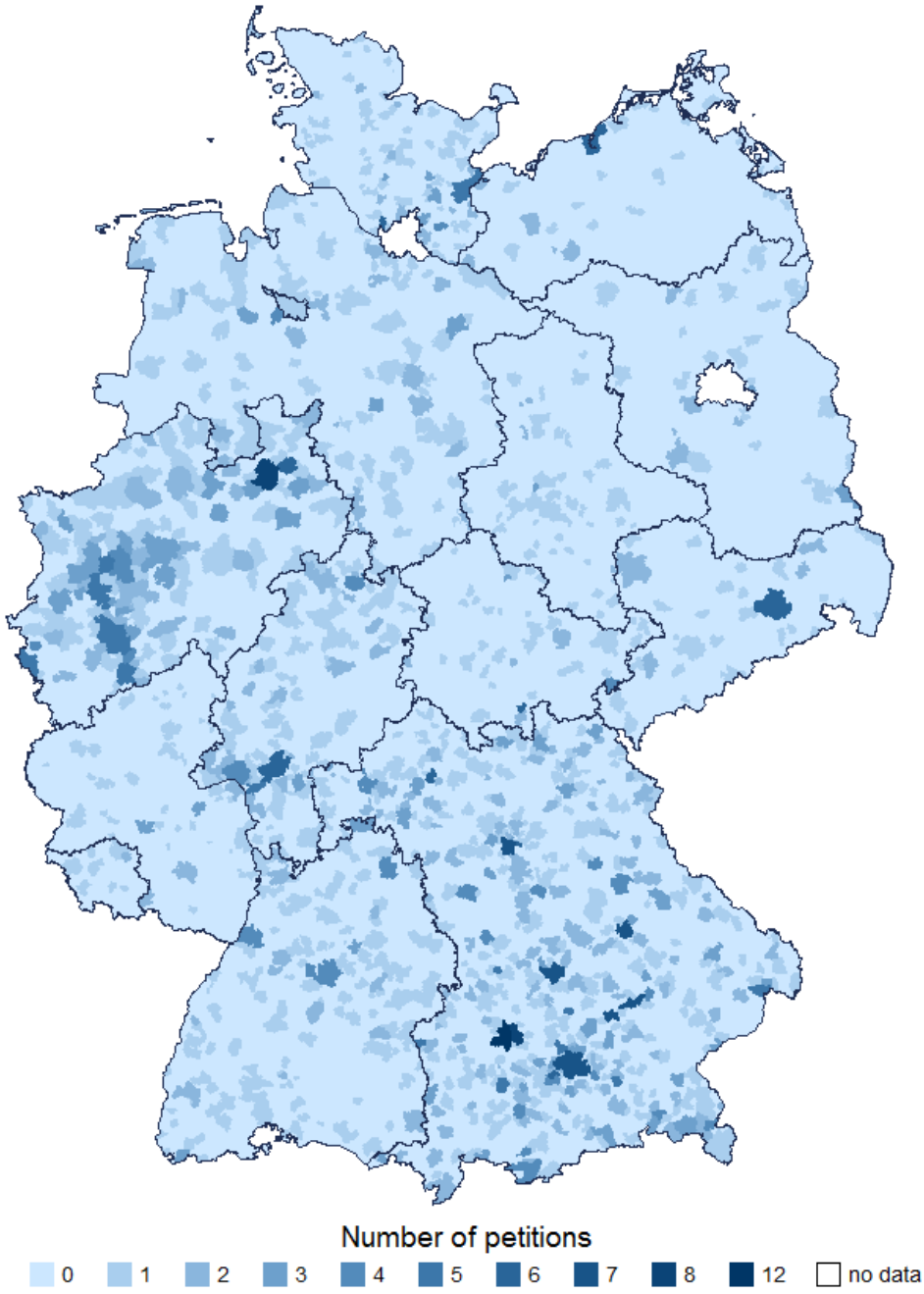

Table A1: Institutions and summary statistics across states, 2002-2009

STATE	Year direct democracy adopted	Signature requirement (mean)	(min-max)	Number of petitions	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)	159	61	9947	0.0378	0.174	0.404	1111
Bayern	1995	0.0982	(0.03-0.1)	712	306	6300	0.0429	0.174	0.303	2056
Berlin	2005									0
Brandenburg	1993	0.1	(0.1)	37	11	5425	0.142	0.195	0.647	925
Bremen	1994	0.1	(0.1)	1	0	3E+05	0.118	0.206	0.644	2
Hamburg	1998									0
Hessen	1993	0.0982	(0.03-0.1)	134	31	14722	0.0565	0.193	0.193	426
Mecklenburg-Vorpommern	1993	0.0997	(0.0199-0.1)	34	4	2008	0.154	0.18	0.557	1012
Niedersachsen	1996	0.0999	(0.0708-0.1)	123	32	7922	0.0658	0.19	0.519	1026
Nordrhein-Westfalen	1994	0.0789	(0.03-0.1)	242	62	47742	0.0635	0.184	0.472	396
Rheinland-Pfalz	1994	0.0998	(0.0601-0.1)	43	9	1916	0.0485	0.196	0.463	2300
Saarland	1997	0.138	(0.0986-0.15)	9	0	20174	0.0585	0.21	0.556	52
Sachsen	1990	0.1	(0.1)	45	11	8311	0.129	0.214	0.462	1022
Sachsen-Anhalt	1990	0.0997	(0.0416-0.1)	86	35	2311	0.14	0.2	0.581	2268
Schleswig-Holstein	1990	0.1	(0.1)	126	29	2860	0.0582	0.181	0.514	1119
Thüringen	1993	0.0993	(0.05-0.1)	51	7	2432	0.113	0.194	0.557	1013

Notes: The data on observed petitions and initiatives (columns: 5-6) 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 constitutions. The control variables (columns: 7-11) come from the "Statistik Lokal" database.

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

VARIABLE	(1)	(2)	(3)	(4)
	Spatial lag citizen petitions			
	t		$t-1$	$t-2$
$\sum_{j\neq i}^{N}(w_j \times$signature requirement $j)$	-0.2738***	-0.2487***	-0.2157***	-0.2104***
	(0.0159)	(0.0157)	(0.0165)	(0.0193)
$\sum_{j\neq i}^{N}(w_j \times$ln population $j)$	0.4308***	0.4401***	0.4409***	0.4916***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
$\sum_{j\neq i}^{N}(w_j \times$unemployment share $j)$	0.0732***	0.0813***	0.0236**	0.0113
	(0.0039)	(0.0054)	(0.0056)	(0.0062)
$\sum_{j\neq i}^{N}(w_j \times$population share over 65 $j)$	-0.0499***	-0.0468***	-0.0444***	0.0101
	(0.0122)	(0.0127)	(0.0148)	(0.0168)
$\sum_{j\neq i}^{N}(w_j \times$left share $j)$	0.0017	-0.0039	0.0137	0.0010
	(0.0020)	(0.0024)	(0.0025)	(0.0026)
Signature requirement		-0.1284***	-0.1329***	-0.1615***
		(0.0205)	(0.0217)	(0.0254)
Ln population		-0.0114***	-0.0137***	-0.0141***
		(0.0001)	(0.0001)	(0.0001)
Unemployment share		0.0015	0.0197***	0.0082
		(0.0021)	(0.0023)	(0.0026)
Share of population over 65		0.0010	0.0006	0.0054*
		(0.0018)	(0.0019)	(0.0020)
Left share		0.0434***	0.0418***	0.0546***
		(0.0009)	(0.0009)	(0.0010)
Observations	97,581	85,499	71,136	57,656
R-squared	0.6118	0.6144	0.6009	0.6219
F-test first stage	1145	1042	964.5	844.8

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

Notes: The table presents beta coefficients for the OLS first stage estimation of the model specified in equation 2. All regressions include time and state fixed effects as well as border dummies which identify municipalities with a distance of max. 50 kilometers to the respective border. Standard errors are robust to heteroskedasticity and are clustered at the municipality level.