

# Spending and voting under the term limits in the Missouri House of Representatives, 1996-2005: A pilot study.\*

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## Abstract

This paper proposes a new test of the agenda setter and experience effects in legislative bargaining. It is based on the natural experiment generated by the introduction of term limits in some US states. To implement this test we have, as a pilot study, collected detailed data on legislators, committee membership, votes on appropriation bills, and fiscal transfers from the state budget to each legislative district during the period 1996 to 2005 for the Missouri House of Representatives. Compared to the predictions of the legislative bargaining model [Baron and Ferejohn (1989)], we find a number of stark differences. The pilot study suggests that it is desirable and feasible to extend the approach to the remaining US states.

Key words: Legislative bargaining, agenda setter power, experience, comparative political economics, natural experiments.

## 1 Introduction

One of the central issues in public economics is how to allocate government resources across competing needs of different groups of citizens. In democracies by and large such decisions are made collectively by representatives elected by different voter constituencies and only very rarely by individual decision makers. This organizational structure of legislatures is intended to provide checks and balances on government policy, and to prevent hijacking of the public agenda by a particular group or individual.

Yet, understanding and predicting the outcomes of collective decision making processes has been a challenge. Game theoretic models have shown that these outcomes depend crucially on often unobservable details of how legislators interact (e.g. Persson and Tabellini, 2000). More specifically, these theoretical

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\*Very preliminary. Comments are welcome. Please do not quote without permission.

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studies highlight one particularly crucial feature: whether or not the power to propose the budget is concentrated in the hands of a few people (for example, legislative committees). Highly concentrated power leads to inefficiently high total spending (Baron, 1991), and causes the distribution of this spending to be skewed in favour of the committee members constituencies (Romer and Rosenthal, 1978; Baron and Ferejohn, 1989)<sup>1</sup>, while separation of powers can help provide checks and balances (Persson, Roland and Tabellini, 1997).<sup>2</sup> To date, there are no empirical tests of these crucial results, with the notable exception of Knight (2005) who shows a positive relationship between a state's transportation committee membership in the US Congress and its allocation of highway grants.<sup>3</sup> The major difficulty in such tests is disentangling committee power effects from characteristics of the legislative district, such as its demand for a particular type of spending, which may be determining the committee membership in the first place.

To overcome this problem, we propose to use the introduction of term limits (restrictions on how long a legislator can serve in office) as a natural experiment. Recent adoption of legislative term limits among 21 US states generates rich data that allow us to do precisely that. The key idea behind our approach is the following: once legislative term limits are introduced, their consequences for the way legislators behave and government spending is allocated depend crucially on whether agenda setter effects are present or not. Whilst without term limits these features are difficult to observe empirically, we can now identify their presence through changes that are induced by term limits. To see how this works, consider a district that loses a representative on the committee for, say, education due to term limits. This loss is uncorrelated with the preferences for education spending among the voters in the district that he represents. Therefore, by comparing the amount of spending before and after a district loses its committee representative, we can identify the effects of committee power on the allocation of spending separately from the effects of district characteristics, hence addressing the major challenge in empirical testing this theory.

Implementing this approach, however, requires detailed data not only on committee membership and legislative term limits for individual legislators, but also on spending targeted at specific legislative districts before and after the impact of term limits.

These data are not readily available, but can be put together by combining publicly available information on state legislators with data on state government transfers to local government units (available from the US Census Bureau). The spending data have to be matched to legislative districts using a geographical matching engine.

This paper reports on a pilot study that implements our approach using data

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<sup>1</sup>These contributions are essentially an application of Rubenstein (1982) sequential bargaining game.

<sup>2</sup>The legislative bargaining model is also an ingredient in models that study other political phenomena (e.g. Battaglini and Coate (2008)).

<sup>3</sup>The predictions have also been tested in a laboratory experiment by Frechette, Kagel and Morelli (2003).

for one of the 21 states that have introduced legislative term limits in recent years: Missouri. We have chosen Missouri because an appropriate matching engine is available both for the 1990 and 2000 Census (allowing us to take redistricting into account). More specifically, we have collected data on the legislators and committees of Missouri House of Representatives, the legislators' votes on budget bills and the allocation of state funding to local public good providers at the legislative district level between 1996 and 2005. Term limits were introduced in 1992 and became binding in 2002.

While this pilot study demonstrates that it is feasible to assemble appropriate data for the kind of test we propose, it is clear that similar data for more states are urgently needed to make the test convincing and conclusive. We also note that the data material thus collected can be productively employed to help settle other open questions. These include whether more experienced legislators are better at securing spending for their districts and whether term limits attract a new, better type of politician into the legislature.

We report on three sets of empirical results in what follows as well as describing how the data set was put together in detail. We start by directly testing the core predictions of the legislative bargaining model (Baron and Ferejohn (1989)). We analyze the **allocation of state funding** to legislative districts and test for the presence of the agenda setter effect. We find a strong positive correlation between funding received by a district and district representation on the committee that oversees the funding allocation. However, our tests show that the causality may not be running from committee representation to spending. Furthermore, contrary to common belief, we find that experienced legislators do not bring more spending to their districts than novices.

We then examine the patterns of **voting** on budget proposals. First, we find that on average 85% of the legislators support budget bills instead of 50%+1 predicted by the legislative bargaining model. Second, belonging to the majority party of the House is the single most important determinant of how a legislator votes. Third, a legislator's voting decision depends on how long he expects to work together with the proposer in the future - a results which is at odds with predictions of the legislative bargaining model with a finite time horizon.

Finally, when we bring the **voting and the spending allocation decisions together**, we do not observe that legislators that are more willing to vote in favour of budget proposals get more money. This last finding goes against the main premise of the legislative bargaining model that votes are traded for budget allocations.

These results are suggestive and raise interesting questions about the empirical relevance of the legislative bargaining model. However, the results must be interpreted with caution and viewed as preliminary. They are based on information from one state only, and data from more states are needed to reach reliable and generalizable conclusions.

The paper is organized as follows. In section 2, we give a brief description of the Missouri House of Representatives and its budget procedure. In section 3, we describe in detail the data set and how it was compiled. In section 4,

we present the results, first on spending, then on voting, and finally we bring the two together. In section 5, we conclude with a discussion of how this pilot study can be extended to the 20 other states that have introduced term limits.

## 2 Missouri House of Representatives

Missouri House of Representatives is the lower house of Missouri state legislature. It is made up of 163 representatives elected by the same number of districts in biannual elections. The democrats have 53% of the seats between 1996 and 2002, while during the period 2002 to 2005, the republicans held the majority with a similar margin. The majority party appoints all the committee chairmen from its own ranks, but other committee members can come from either party. Under the democratic majority, the republicans held 46% of all committee seats, and under the republican majority, the democrats held 42% of committee seats.

In 2005, there were 24 committees in Missouri House of Representatives, but the exact number of committees and their mandate changes from year to year. For almost all categories of public services, such as health, education, transport, etc., the House of Representatives has both a regular committee and an appropriations committee. They divide the responsibilities into fiscal and non-fiscal responsibilities: for example, the Appropriations Committee on Health only deals with the budget for health provision and its allocation, while Health Committee deals with all other health policy related issues.

### 2.1 The Budget Process

The budget cycle is annual, except for capital expenses which have a biannual budget cycle. In March every year, the governor of Missouri submits a state budget proposal for the next fiscal year to the House of Representative's Budget Committee. The Budget Committee then assigns different parts of the budget to the relevant appropriations committees. There are between 6 and 8 such appropriations committees, depending on the year. The appropriations committees work on their part of the budget and then present their proposal to the House of Representatives as a separate bill. Some committees handle more than one bill: for example, the Appropriations Committee on Education produces two bills, one on primary and secondary education, and another on higher education. Each bill concerns one area of public spending, and allocates money within different programs in that area. The list of the appropriation bills that are drawn up every year is:

- Education, elementary and secondary
- Education, higher
- Transportation

- Health
- Economic development
- Natural resources
- Social services
- Corrections
- General government administration
- Top government administration
- Cost of bond financing
- Rent, lease and utilities of government offices
- Capital improvements
- Supplemental purposes<sup>4</sup>

There are three hearings and three rounds of votes for each bill in the House of Representatives. Once the bill is approved in the House, it is passed on to the Senate. If the Senate approves the House's bill, it goes to the Governor for signing.<sup>5</sup>

## 2.2 Term limits

Missouri is one of 21 US states that have recently imposed term limits on its legislators. In Missouri, this was done in 1992 through a citizens initiative. The adopted constitutional amendment limits each legislator to no more than eight years in total<sup>6</sup> in either of the two houses of the general assembly. When this change was introduced, the most senior member of the House had been there for 32 years. The first cohort of legislators affected by term limits were those elected in November 1994, forcing them to step down no later than the end of 2002.<sup>7</sup> Term limits reduced the average number of years of service in Missouri from 7 years in 1996 to 3 years in 2005. Out of 400 legislators that we track between 1996 and 2005, 237 stepped down at some point, and 97 of them did so at the end of the maximum allowed term.

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<sup>4</sup>In some years, there is more than one bill in the last two categories.

<sup>5</sup>If the Senate does not, the bill goes back to the House with the Senate's amendments. If the House fails to agree to the amendments, a special House-Senate conference committee is created to resolve this.

<sup>6</sup>This is, consecutive or not.

<sup>7</sup>Due to a technicality, some politicians elected in special elections had to step down in 2000.

### 3 The Data

It is useful to start by sketching the type of data required for our analysis before explaining in detail how we assembled the data for Missouri. In addition to institutional information on how the legislature and budget process work, we need three types of data.

1. Data on state budget transfers to local government offices, projects and branches located within the borders of legislative districts. Since our sample spans two census periods, we need to take into account the redistricting that takes place after each census.
2. Data on the identity of legislators, their legislative committee membership, their tenure and when, if at all, they were affected by a binding term limit.
3. Data on each appropriation bill (type of spending covered and proposing committee).

These data were compiled from several publicly available sources.

**Fiscal data** The source of the fiscal data is the State and Local Government Finances section of the Bureau of Census' Census of Governments and Survey of Governments from 1996 to 2005.<sup>8</sup> These data files itemize transfer payments from the state budget to 2,663 Missouri service providers, such as schools, hospitals, libraries, nursing homes, development agencies, etc. We classify the transfers into six categories defined by the underlying financed activity: education, health, transport, welfare, development and other. We then allocate each local government entity receiving a transfer to a legislative district. This is done by matching the zip code of the service provider to the zip codes covered by each legislative district. This takes into account the 2000 redistricting.<sup>9</sup> The zip codes of each service provider are taken from the Bureau of Census' Government Integrated Directories, 1997 and 2002. The zip codes covered by each legislative district are available from University of Missouri MABLE correspondence engine, which is then used to do the matching.<sup>10</sup> After matching the service providers to legislative districts,<sup>11</sup> we aggregated the funding received by each district under each category of spending.

We should stress that the transfer payments included in our data set are restricted to state budget transfers to designated local government entities only. The states spend more money than this on the six categories, but these funds

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<sup>8</sup><http://www.census.gov/govs/www/estimate.html>.

<sup>9</sup>The new legislative districts were created in 2001, but the first elections with these districts took place in 2002.

<sup>10</sup><http://mcde2.missouri.edu/websas/geocorr2k.html>

<sup>11</sup>When the zip code of a local government entity overlap more than one legislative district, we have divided the transfer in proportion to the overlap using the share of the zip code's population in the overlap as the weight.

are not channeled through these service providers and it is not possible to attribute them geographically. Table 1 shows the breakdown of per capita state transfers to legislative districts by category of spending, in 1996 US\$. Education is by far the largest category which reflects first, that it is the largest component of total state spending and, second, that our data include nearly all educational state spending, but only a small fraction of the spending in other categories. Primary and secondary education funding in the US is normally allocated according to a formulae that takes into account school district characteristics. During the period we study, Missouri's allocation of education funding was widely criticized for its lack of transparency and seemingly ad hoc nature. This is important because it would give legislators some leeway in allocating resources across districts in a discretionary manner.

**Data on legislators and committees** The source of the data on legislators, legislative committees, and their membership is the web site of Missouri House of Representatives and Missouri Manual. Our data cover the 400 representatives that served at some point between 1996 and 2005. We record the name and district of each legislator as well as the beginning and end of his or her term in office.

We focus on 14 committees only. These are the ones most relevant to provision of public services, and which are stable over time. Of these, 8 are appropriations, and 6 are non-appropriations committees. For each committee, we record the membership and who was the chair. Table 2 lists the names of the committees that we analyze together with information on average size, turnover rates, and average years of service.

We want to know whether the money that a district receives in a particular category is affected by whether that district's representative is on the relevant committee or not. Hence, we match each category of spending to the committee(s) with relevant mandate(s) as shown in Table 3.

To enable the analysis of experience effects, we define two variables related to each legislator's tenure in the House. 'Experience' is the number of years the legislators has spent in the House at a particular date. 'Remaining years' is the number of years remaining till the legislator must step down due to the term limit.<sup>12</sup> We compute these two variables for every legislator, as well as for the chairmen and members of the relevant appropriations committees. On average, a committee member spent six years in the House, which is more than the average legislator (4 years), but less than committee chairmen (8 years).<sup>13</sup> A summary of the data on legislators and committee membership is shown in Table 4.

**Data on appropriation bills** The information on each appropriation bill (type of spending covered and proposing committee) is taken from the House

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<sup>12</sup>This is the upper bound on how long the legislator can expect to serve in the House, since he may get ousted by the voters before his term limit is reached.

<sup>13</sup>'Remaining years' and 'experience' are correlated (correlation coefficient is 0.5) but not perfectly, since our sample starts with 160 legislators elected before the term limit was imposed.

and Senate Joint Bill Tracking Database.<sup>14</sup> The legislators' votes on each bill are collected from the Missouri House of Representatives Journal.

For each committee, we pair each of the members with the chairman and calculate the 'joint experience' and 'joint remaining years'. The former is the number of years the two have worked together in the past and the latter is the maximum number of years they can expect to work together in the future. These are computed as the smaller of the two legislators' experiences and their 'remaining years', respectively. These variables will be used to test some conjectures about intertemporal vote trading.

## 4 Preliminary Results

In this section, we present some preliminary results. We divide the discussion into three subsections: first, we discuss results related to the spending allocation; second, we discuss results related to the voting patterns of the legislators; and third, we combine the two.

### 4.1 The Allocation of Spending

The data on the allocation of spending across legislative districts allow for a test of agenda setter and experience effects, and, thus, for the core predictions of the legislative bargaining model.

#### 4.1.1 Agenda setter effect

We start by asking whether the legislative districts that have representatives on the relevant committees get more money. Table 5 shows the average transfers from the state budget broken down by spending category. For all types of spending except health and welfare, the districts that have a representative on a relevant committee receive more. This effect is larger (and more statistically significant) when we look at appropriations committee membership alone.

Table 6 shows that the positive relationship between committee membership and transfers remains significant when we control for the party of affiliation of the legislator, year and transfer type fixed effects in a linear regression framework. First, we use any relevant committee membership as our explanatory variable (first column) and find that districts whose representative serves on one of the committees receive \$4 per capita more than the rest. In the estimation reported in the second column, we add a separate variable for the appropriation committee membership. Appropriation committee membership is associated with an average \$7.7 increase in spending per capita, which is significantly different from zero at the 1% level. The point estimate of general committee membership is now negligible in size and no longer statistically significant. Columns three and four show the same estimation for the sub-sample

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<sup>14</sup><http://www.house.mo.gov>

without education and for education spending separately. Appropriations committee membership is significant in both.

These findings by themselves do not confirm the existence of the agenda setter effect for the members of appropriations committees. The allocation of legislators to committees is not random. If there are district characteristics that are correlated both with the likelihood of having a representative on a committee, and with the amount of funding the district gets, our committee membership variable may simply be proxying for these characteristics.

To address the potential endogeneity of committee membership, and hence to isolate any causal agenda setter effect on spending, we include district fixed effects into our estimations. Now the effect of committee membership is identified from changes in transfers received by a legislative district after its representative resigns from the relevant committee. The estimates in Table 7 show that the appropriations committee effect is identified in the entire sample, and in the sub-sample without education spending. Somewhat surprisingly the sign of the effect changes when we limit our attention to education spending, implying that districts whose representatives resign from the education appropriations committee get more funding. However, this effect is significant only at the 10% level.

As we already discussed, we can claim that the relationship we estimated in Table 7 is causal only if the characteristics of a district that affect both its representation on an appropriations committee and the transfers it receives are fixed over time. In other words, if over-time changes in representation are uncorrected with spending allocations. This is not necessarily the case: for example, a district's demographic composition can change over time, affecting both its need for, say, health spending and its representative's willingness to serve on a health-related committee. When, however, the district loses its representation on a committee because its legislator steps down when the term limit is reached, this change in representation is *not* correlated with districts characteristics. If the money that the district receives falls after such event, we have identified the causal effect of committee membership.

To explore this exogenous variation in membership, we construct a discontinuity regression. That is, we compare the amount of spending that a district receives two years before and two years after it loses representation on a committee because its representative steps down at the term limit. Table 8 shows the results. We do find that districts get less money after an exogenous loss of committee membership, but the effect is not statistically significant.<sup>15</sup>

In conclusion, then, we are not able to identify any agenda setter effect when we focus on purely exogenous loss of committee membership. On the one hand, this suggests that the estimates shown in previous tables are contaminated by endogenous selection into committees, and therefore do not estimate correctly the effect of the committee power on the spending allocation. On the other hand, we must be careful in drawing conclusions from our discontinuity regres-

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<sup>15</sup>This result does not change if i) we control for aggregate changes by estimating year fixed effects or ii) we control for the effect of having a new legislator using the rest of the districts that did not go through these episodes (not reported).

sion. The number of ‘episodes’ is small – only in 32 (out of 163) districts the representative is forced to step down because of term limits – and this is reflected in a several-fold increase in standard errors. Even the party affiliation which had a significant effect in most regressions reported earlier lost its significance in the discontinuity regression.

There are two reasons why our sample shrinks so much when we focus on the loss of committee representation due to term limits. First, our sample contains only three years after term limits came into effect. Second, when a legislator steps down at the term limit, his district does not always lose representation on all committees on which the retiring legislator used to sit. Hence, further progress in identifying the committee power or agenda setter effect should come from expanding our sample with data from other states.

#### **4.1.2 Experience effect**

It is often argued that experienced legislators are better at bargaining for a bigger allocation of resources towards their districts. Such ‘bargaining experience’ has been cited as the explanation for often-observed incumbency advantage in elections (see, for instance, evidence from Ansolabehere and Snyder, 2002), as well as the reason for the introduction of legislative term limits (Dick and Lott, 1993). Yet, experience effects are difficult to identify empirically because legislative turnover is endogenous. Term limits generate an exogenous source of turnover in legislatures, which allows us to test this theory.

To this end, we estimate discontinuity regressions in which we compare state transfers to a district two years after and two years before it loses a legislator due to the term limit. In our sample this happens in 97 instances. We do not find significant experience effects in any category of expenditure (not reported), but, as above, the small number of cases limits the power of this test and information from other states are needed to confirm or disconfirm the rejection of the experience hypothesis.

## **4.2 Voting Patterns**

Our data allow us to analyze the votes of the legislators on appropriation bills. Table 4 shows that on average, 85% legislators vote ‘yes’ in the third and final reading of the bills covered by our sample. This is significantly more than the 50%+1 vote predicted by the legislative bargaining model. When we look at how the percentage of legislators willing to support the appropriation bills changes over time, we find that it declines with a particularly large drop after the republicans won the majority of the House (Table 9). The change is due to the votes of the minority: when the republicans were in the minority, they voted ‘yes’ 82% of the time, but when the democrats are in the minority, they vote ‘yes’ 41% of the time. Notice that this coincides with the slight decline in the share of minority legislators on appropriations committees during the years of the republican majority.

Table 10 illustrates the variation in support across different types of bills: the proposals for spending on administration receive less votes than other types of proposals.

To gain a better understanding of these patterns, we estimate the relationship between the probability that a legislator votes ‘yes’ and the characteristics of the legislator. The results of a simple linear probability model are reported in Table 11. We find that whether the legislator’s party has the majority in the House has a large and statistically significant effect on his support for appropriation bills: the legislator is 28 percentage points more likely to vote ‘yes’ if he is a member of the majority party (columns 1 - 3). We also find that members of appropriations committees are more likely to support the bills, and that the legislators elected later are less likely to do so (column 1). However, the latter two results disappear once we introduce year effects (column 2). Hence, we cannot identify any effect of the election year separately from the spurious negative correlation between voting ‘yes’ and the election year which arises because the share of ‘yes’ votes falls while the election year of the average legislator rises over time.

When we include experience and the variable counting the remaining years of service of the legislator into our regression equation (column 3), neither of these variables are significant.<sup>16</sup> The identification of these two variables separately comes from the legislators that were elected before 1994, since for all those elected in 1994 or afterwards, the eight year term limit implies a perfect correlation between experience and remaining years.

We now ask whether a legislator’s vote on a particular bill is affected by the amount of time he spends working with the chairman of the committee that has proposed the bill. We distinguish between two main hypotheses. First, a legislator may be more inclined to vote in favour of a proposal, if he has worked longer with the chairman in the past. This may be due to loyalty, reputation, or because people develop similar views when they work together. Second, a legislator may be more willing to cooperate with a chairman when the legislator expects to work longer with him in the future. This could be happen when a longer future together means that there is a greater chance that the legislator will be rewarded for his support.

Theoretically, the presence of term limits means that the game between the legislators and the chairmen is finite. In simple finite bargaining games, the vote of the legislator in each period should be independent of both past history and the number of remaining periods. Hence, the legislative bargaining model predicts that we should find no effect of either joint past or joint future of the legislator and the chairman on the legislator’s vote.

The results reported in Table 12 show that the data are inconsistent with this prediction. Column 1 reports the results of a linear regression with year effects. First, we find that the number of years the legislator and the chairman spent together in the past indeed does not affect the likelihood of a ‘yes’ vote.

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<sup>16</sup>Year elected is perfectly correlated with experience, and hence we exclude it from column 3 estimation.

However, we see that the maximum number of the future years the legislator and the chairman can look forward to working together increases the probability that the legislator votes in favour of the chair’s proposal today. This result is statistically significant at 1% level. It amounts to a 3.5 percentage points increase in the probability that the legislator votes yes for each additional joint remaining year. This effect is coming primarily from the minority legislators, who on average vote yes 73% of the time. This result is remarkably robust: it remains significant and similar in magnitude after we add bill type effects (column 2), control for the legislator’s remaining years (column 3), experience (column 4), and legislative district fixed effects (column 5). Since all legislators elected before 1994 have the same remaining years till 2002, the identification of this effect comes from legislator-chairman pairs where one of the two or both were elected after 1994.

Before we draw conclusions from these results, we turn to three potentially important changes that happened to the House of Representatives in 2002. First, the term-limit clock started in 1994 claimed its first victims at the end of 2002: 71 legislators had to step down that year generating an unusually high turnover in the House. Second, the first redistricting since 1992 took effect in 2002 elections. Third, possibly related to the first two shifts, the majority of the house changed after the 2002 elections from the democrats to the republicans. These changes may have led to a shift in the way legislators interacted after 2002.

We modify our regression equation to allow for these changes to affect the two determinants of voting we have identified: belonging to the majority party and joint remaining years. Column 1 in table 13 reports the results of a model in which we interact these two variables with a dummy that is equal to 1 after 2002, and zero otherwise. First, the effect of belonging to the majority party rises significantly after 2002: the result we have already seen in the raw data when we noticed that democrats are more reluctant to support the proposals of the republican majority proposals than when the roles are reversed. Second, we find that the effect of the joint remaining years remains positive, but is significantly larger before than after 2002: five and two percentage points respectively. Still, both effects, including the weaker post-2002 effect are significant.<sup>17</sup>

We also allow for all coefficients of our model to vary before and after 2002 by estimating our model separately for the two periods (columns 2 and 3). The results for 1996-2002 are very similar to what we have seen before. In particular, the joint remaining years is positive, significant and three percentage points a year in magnitude. The results for 2002-2005 are more surprising: joint remaining years is now negative and significant, indicating a two percentage points reduction in the probability of a ‘yes’ vote for every additional year the legislator and the chair expect to work together. We note that once we have accounted for the break in 2002, we also observe that members of the Houses appropriation committees are more likely to vote ‘yes’ (columns 1 -3).<sup>18</sup>

<sup>17</sup>That is the sum of the coefficients on the joint remaining life and its interaction with the dummy for after 2002 is significantly greater than zero.

<sup>18</sup>The results for all the variables of interest remain the same when we introduce district

### 4.3 Spending and voting

We can now ask whether the patterns of voting we have found above translate into patterns of spending. We do not report these results formally, but instead summarize them below.

First we begin with the result that legislators from the majority party vote in favour of budget bills more often. If they are doing this in return for more spending, we should then observe that their districts get more than the districts of the minority legislators. However, when we analyze the spending data we find no difference of the kind.

Second, the positive effect of the joint remaining years on the legislator's willingness to vote 'yes' suggests that it may also have an effect on spending transfers. However, we find no significance there as well. The only exception is education, where we see a negative coefficient, i.e. the opposite of what we had expected, significant at the 10% level.

Finally, if votes are exchanged for transfers, there should be a positive correlation between the amount of money a district gets and the probability that the legislator votes 'yes'. We find no such relationship, either contemporaneously or when we lag the vote by two years to allow for a delayed reward. However, when we look at education alone we find that there is a strong negative correlation between money and probability of support, even after legislative district fixed effects have been taken out.

These results of putting voting and spending patterns together suggest that contrary to the main premise of the legislative bargaining model, the votes are not being traded for spending, at least not in an identifiable manner.

## 5 Discussion

Our pilot study of Missouri has shown two main things. Firstly, it is feasible to collect the type of data required to explore the natural experiment generated by the introduction of term limits to test for agenda setter and experience effects. However encouraging this is, it is clear that convincing and conclusive tests require data from more states than one. In fact, only 97 legislators stepped down due to term limits in Missouri between 1995 and 2006, which leaves us with a very small sample for our discontinuity analysis. Ideally, similar data should be collected for the 20 other states that have introduced term limits. The major challenge in doing so is to match spending data to the legislative district boundaries that existed prior to 2002. While the University of Missouri MABLE correspondence engine can be used to match fiscal data for all these states post-2000 in the same way as it was done for Missouri, a new matching engine must be programmed for the pre-2000 period. Another issue that needs to be taken into account is that Missouri's proliferation of appropriations committees enabled us

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fixed effects into our regressions (not reported). This is with the exception of the coefficient on joint experience, which is positive and significant before 2002, and negative and significant after 2002. The size of it is small: 0.5 and 1 percentage points respectively.

to consider different spending types separately. While many other states have a similar proliferation of committees, some operate a system in which there is only one appropriations committee for the entire budget. In these states, it is not possible to study individual items and the key issue is whether being on the appropriations committee matters or not. Data on legislators and committees, on the other hand, can be collected from online or other published sources along with the relevant institutional information.

Secondly, our preliminary analysis of the Missouri data raises a number of interesting questions in need of additional research. We find that legislative districts that have representatives on appropriations committees receive more transfers than districts without such representation. This relationship is robust to inclusion of district fixed effects. However, we did not find that districts that suffer an exogenous loss of a committee representative due to a binding term limit get less money afterwards. We also did not find that experienced legislators bring more state funds to their districts. These results are surprising and run counter to the predictions of the legislative bargaining model. This motivates further research with a particular emphasis on getting data from other states and on developing a better understanding of how committee membership is allocated both across legislators and across districts.

Our analysis of the voting data also generates new and surprising results. Firstly, we find that party affiliation explains a lot of the variation in how legislators vote. This suggests that the proposer's ability to persuade others to vote for his bill is significantly stronger among members of his own party than among legislators from outside his party. This raises an interesting question of why this is the case, what the role of the parties is, and how political parties affect legislative bargaining.

Secondly, we find that the number of votes cast in support of most budget bills is much higher than the 50% needed to pass them. This finding should be viewed together with the observation that the members of the majority party vote for these bills 98% of the time. This implies that the committees can pass the bills with the support of their party alone, albeit with a small margin given the size of Missouri's majorities. This observation does not square with the logic of the legislative bargaining model: the bills appear to appeal to the members of the minority which is suboptimal given their votes are not required to pass the bills. This raises a number of questions and possibilities: Are the interests of legislators less in conflict than expected? Do committees benefit from having large rather than small majorities approve their bills (for example, for publicity or to persuade the senate and the governor to sign them)?

Thirdly, we find that voting is forward looking, with the number of years the legislator expects to work with the agenda setter in the future increases the probability of a 'yes' vote. This hints at over-time cooperation between legislators. Another interesting aspect of this result is that it is at odds with the theory of finitely repeated games. At the same time, there is a number of unanswered questions associated with it, such as why the effect is different before and after 2002? The change of leadership and redistricting that took place in 2003 in Missouri make it difficult for us to identify the dynamic aspect

of legislators' decisions separately from these changes. The data suggest that these changes may be interacting with the effects we are interested in a complex way. To take the investigation of dynamic cooperation between legislators further, we need to gather data on a number of states to be able to isolate the effects of interest from state-specific events.

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**Table 1 State transfers to legislative districts, 1996 US\$ per capita**

	Mean	Standard error	Min	Max
Education	1,625	46	0	20,858
Health	16	2	0	1,413
Transport	87	5	0	2,916
Welfare	7	1	0	569
Development	2	0	0	334
Other	78	6	0	3,826
Total	1,817	55	0	27,685
Total without education	192	13	0	6,828

**Table 2 Committees of Missouri House of Representatives**

Committee	Number of members	Turnover	Years of service of committee members
Appropriations			
Budget	32	0.18	7.8
Education	23	0.07	5.0
Health	18	0.13	5.0
Transport	16	0.09	3.3
Natural and economic resources	24	0.11	5.8
Social services	22	0.11	6.6
Corrections	15	0.10	4.5
General administration	19	0.10	6.2
Non-appropriations			
Education	23	0.064	4.3
Health	14	0.20	4.9
Transport	21	0.14	5.2
Commerce	23	0.11	5.1
Children, youth and families	16	0.13	4.5
Ways and means	19	0.16	5.8
Average	21	0.121	5.3

**Table 3 Matching committees to spending**

Category of spending	Committees	
	Appropriation	Non-appropriation
Education	Education	Education
Health	Health	Health*
Transport	Transport*	Transport
Welfare	Social services; Corrections; Health	Children, youth & families
Development	Natural & economic resources*	Commerce; Children, youth & families
Other	All of the above + Ways and means	All of the above

\* part of another committee in some of the years

**Table 4 Legislators and voting statistics**

	Mean	SE	N	Min	Max
<b>Characteristics of legislators</b>					
Party (1 - democrat, 0 - republican)	0.50	0.01	1,627	0	1
Member of majority party (1 - member)	0.55	0.01	1,627	0	1
Year elected first time	1994.8	0.16	1,630	1960	2005
Year cannot run again	2004.8	0.08	1,630	2002	2012
Member of an appropriation committee (1 - member)	0.62	0.01	1,630	0	1
Experience	4.8	0.1	1,630	0	37
Remaining years	5.3	0.1	1,630	1	9
Legislator's vote (1 "yes", 0 "no")	0.85	0.00	26,739	0	1
<b>Characteristics of the proposing committee</b>					
Party composition (% democrats)	0.51	0.00	172	0.33	0.58
Experience of the chairman	8.1	0.4	172	2	23
Remaining years of the chairman	3.4	0.1	172	0	6
Average experience of committee members	6.0	0.2	172	1.2	9.4
Average remaining years of committee members	4.3	0.1	172	1.2	6.5
<b>Joint characteristics</b>					
Joint experience of the legislator and the proposing committee chair	3.86	0.02	28,036	0	23
Joint remaining years of legislator and the proposing committee chair	3.26	0.01	28,036	0	6

**Table 5 Transfers from state budget to legislative districts (1996 US\$ per capita)**

Transfer category	No committee	Any committee	Appropriation committee
Education	1,577 (55)	1,768* (84)	1,953** (128)
Health	15.5 (2.3)	19.3 (7.3)	12.7 (5.0)
Transport	83.6 (5.4)	98.6 <sup>+</sup> (11)	142.5** (19.0)
Welfare	8.1 (1.3)	5.1 (0.9)	4.7 (0.9)
Development	1.6 (0.2)	2.6* (0.7)	3.6** (0.1)
Other	42 (5.0)	87** (7.9)	96** (9.4)
Total	1,647 (81)	1,876 <sup>+</sup> (65)	1,895* (77)
Total less education	124 (13)	208* (15)	224** (19)

*Standard errors in parentheses*

*\*\* significantly different from no committee at 1% level, \* at 5% level, <sup>+</sup> 10% level*

**Table 6 Committee membership and spending**

	All	All	Without education	Education
Committee	0.039* (0.018)	-0.008 (0.019)	-0.006 (0.005)	-0.069 (0.150)
Appropriation committee		0.077** (0.021)	0.026** (0.007)	0.446* (0.185)
Democrat	0.039** (0.016)	0.039** (0.016)	0.026** (0.003)	0.107 (0.092)
Year effects	Y	Y	Y	Y
Transfer type effects	Y	Y	Y	N
N	9,930	9,930	7,825	1,565

*Standard errors in parentheses*

*\*\* significant at 1% level, \* at 5% level*

**Table 7 Committee membership effect (district fixed effects)**

	All	Without education	Education
Appropriation committee	0.053* (0.022)	0.011** (0.004)	-0.153+ (0.080)
Democrat	0.017 (0.013)	0.020** (0.005)	-0.010 (0.060)
Year effects	Y	Y	Y
Transfer type effects	Y	Y	N
District effects	Y	Y	Y
N	9,930	7,825	1,565

*Standard errors in parentheses*

*\*\* significant at 1% level, \* at 5% level, + 10% level*

**Table 8 Loss of committee representation due to term limits**

	All	Without education	Education
Appropriation committee	0.01 (0.09)	-0.01 (0.06)	-0.00 (0.47)
Democrat	0.18 (0.28)	0.01 (0.21)	0.70 (1.15)
Year effects	N	N	N
Transfer type effects	Y	Y	Y
District effects	Y	Y	Y
N	181	157	24
R-squared	0.85	0.45	0.84

*Standard errors in parentheses*

*\*\* significant at 1% level, \* at 5% level, + 10% level*

**Table 9 The size of the majority falls over time**

Year	% Yes (average across all bills)
1996	0.94
1997	0.94
1998	0.92
1999	0.91
2000	0.89
2001	0.89
2002	0.87
2003	0.71
2004	0.68
2005	0.77

**Table 10 The size of the majority varies by type of appropriation bill**

Type of bill	% Yes (average across all years)
Top administration	0.69
Administration	0.74
Social services	0.8
Health	0.81
Natural resources	0.83
Transportation	0.84
Elementary and secondary education	0.85
Higher education	0.85
Corrections	0.87
Economic development	0.87
Supplemental purposes	0.87
Public safety	0.89
Property, leases and utilities	0.94
Capital improvements	0.95
Cost of bonds	0.99

**Table 11 Voting and legislator characteristics**

Dependent variable is the vote: 'Yes = 1, 'No' = 0			
	(1)	(2)	(3)
Majority party	0.281 (0.016)**	0.286 (0.014)**	0.288 (0.014)**
Appropriation committee	0.028 (0.014)*	0.022 (0.013)	0.024 (0.013)
Year elected	-0.010 (0.001)**	-0.001 (0.002)	
Experience			0.000 (0.002)
Remaining years			-0.006 (0.004)
Constant	20.689 (2.512)**	2.815 (3.071)	0.822 (0.039)**
Year effects	N	Y	Y
Bill type effects	N	N	N
Legislative district effects	N	N	N
Observations	26,739	26,739	26,739
R-squared	0.19	0.22	0.22

*In parentheses are the standard errors clustered at district-year level.*

*\* significant at 5%; \*\* significant at 1%*

**Table 12 Joint past and joint future: the legislator and the chairman**

Dependent variable is the vote: 'Yes = 1, 'No' = 0					
	(1)	(2)	(3)	(4)	(5)
Majority party	0.284 (0.014)**	0.284 (0.014)**	0.287 (0.014)**	0.284 (0.014)**	0.348 (0.012)**
Appropriation committee	0.020 (0.013)	0.019 (0.013)	0.023 (0.013)	0.019 (0.013)	0.022 (0.012)
Experience				-0.000 (0.002)	
Remaining years			-0.012 (0.004)**		
Joint experience	0.003 (0.002)	0.003 (0.002)	-0.002 (0.003)	0.003 (0.003)	0.003 (0.002)
Joint remaining years	0.035 (0.003)**	0.031 (0.003)**	0.037 (0.003)**	0.031 (0.003)**	0.029 (0.003)**
Constant	0.563 (0.026)**	0.467 (0.025)**	0.529 (0.036)**	0.468 (0.025)**	0.405 (0.024)**
Year effects	Y	Y	Y	Y	Y
Bill type effects	N	Y	Y	Y	Y
Legislative district effects	N	N	N	N	Y
Observations	26,739	26,739	26,739	26,739	26,739
R-squared	0.23	0.28	0.28	0.28	0.38

*In parentheses are the standard errors clustered at district-year level.*

*\* significant at 5%; \*\* significant at 1%*

**Table 13 Before and after 2002**

Dependent variable is the vote: 'Yes = 1, 'No' = 0			
	1996-2005	1996-2002	2002-2005
Majority party	0.162 (0.019)**	0.162 (0.019)**	0.576 (0.010)**
Appropriation committee	0.024 (0.010)*	0.029 (0.014)*	0.021 (0.010)*
Joint experience	0.004 (0.002)	0.004 (0.002)	-0.001 (0.004)
Joint remaining years	0.053 (0.008)**	0.030 (0.008)**	-0.021 (0.005)**
After 2002	-0.232 (0.053)**		
After 2002 * Majority party	0.409 (0.021)**		
After 2002 * Joint remaining years	-0.034 (0.009)**		
Constant	0.399 (0.049)**	0.547 (0.054)**	0.302 (0.028)**
Year effects	Y	Y	Y
Bill subject effects	Y	Y	Y
Observations	26,739	18,639	8,100
R-squared	0.35	0.14	0.56

*In parentheses are the standard errors clustered at district level.*

*\* significant at 5%; \*\* significant at 1%*