UNIVERSITY OF CALIFORNIA SAN DIEGO

Essays in Applied Microeconomics

A dissertation submitted in partial satisfaction of the
requirements for the degree of Doctor of Philosophy

in

Economics

by

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The Dissertation of Jason Matthew Bigenho is approved and is acceptable in quality and form for publication on microfilm and electronically:

Chair

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DEDICATION

To my parents, Carl and Mary.

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ABSTRACT OF THE DISSERTATION

Essays in Applied Microeconomics

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This dissertation is composed of three papers on distinct topics, each using a different method from applied microeconomics. In chapter 1, I study how candidates' election results affect the future contribution behavior of their donors using a regression discontinuity design. I find that contributors to narrowly-winning House candidates are much more likely to contribute in the following cycle than contributors to narrow-losers. Much of this effect is driven by future giving to the same candidate, contrary to a "reinforcement learning" hypothesis. This has broader implications, as incumbents can rely more on contributions from past donors than can new challengers. I estimate that candidates from narrowly-winning parties receive \$130K more in individual contributions than those from narrowly-losing parties in the following cycle, almost

all of this coming from these repeat donors.

In chapter 2, we study the role of self- and social-image in social comparisons. We propose and test a theory that casts peer effects as the result of a signal extraction. The theory posits that individuals receive signals of their own attributes through completion of costly actions. Signal extraction is improved through social comparisons with other's actions. In experiments, we find that subjects choose to complete more real-effort tasks in exchange for charitable donations if they anticipate learning how their decisions compare to the choices of others. Further, differentiated responses to noisy or refined social information adhere to the dynamics of signal extraction.

In chapter 3, we characterize how municipal governments respond to economic fluctuations, using employment shocks as a proxy. We specifically study the role Tax and Expenditure Limitations play in this response. We find that, following a positive employment shock of one percent, limited municipalities persistently lag behind their unconstrained counterparts in capital-intensive spending, with little differential effect on public safety and administrative expenditures. Our findings illuminate an unintended consequence of fiscal responsibility measures in U.S. cities: limits designed to restrain the size of government may instead alter the government's spending mix, inducing investment cuts that allow a government to maintain patterns of administrative and public safety spending.

Chapter 1

Elections, Individuals and Incumbency: Campaign Contributions following Close Congressional Races

The "paradox of voting" is a seminal issue in economics and political science. A strategic-minded individual, weighing the numerous costs of voting against the near-infintessimal chance her vote decides an election, should *surely* choose not to cast a ballot. Yet, hundreds of millions vote in national elections world-wide. This ubiquitous revealed preference for voting has driven researchers to investigate motives beyond purely strategic or influential concerns.[1]

A number of behavioral models have been proposed to rationalize widespread political participation that assume a mechanism of "reinforcement learning," in which individuals adjust future behavior according to whether they receive a positive or negative outcome (i.e. their candidate of choice wins or loses) (Bendor, Diermeier and Ting, 2003; Fowler, 2006*b*). Individuals voting for the winning candidate are predicted to participate more in the future,

[1]There is a large literature investigating these alternative motivations. Past empirical work has focused on altruism (Fowler, 2006*a*; Fowler and Kam, 2007; Dawes, Loewen and Fowler, 2011), social considerations (Gerber, Green and Larimer, 2008; Dellavigna et al., 2017) and media influence (Gentzkow, 2006; DellaVigna and Kaplan, 2007; Enikolopov, Petrova and Zhuravskaya, 2011; Gentzkow, Shapiro and Sinkinson, 2011), among other factors.

while individuals voting for the losing candidate will participate less. While on its face such an assumption seems reasonable, there is little empirical evidence to support it. Much of the empirical work on political participation focuses on the act of participating in an election itself, offering few insights into the role of its results. These studies (Gerber, Green and Shachar, 2003; Meredith, 2009; Fujiwara, Meng and Vogl, 2016) have helped to build a consensus among turnout scholars that political behavior is habit forming (Denny and Doyle, 2009).

Whether election results influence future political behavior has implications beyond its place as a primitive in models of participation. If supporters of winning candidates are more politically active in the future than supporters of losing candidates, this differential participation will strongly favor incumbents. Thus, uncovering whether election results themselves influence political behavior can bolster understanding of what underlies the well-documented "incumbency advantage" that exists in U.S. legislatures (Parker, 1980; Cox and Katz, 1996; Ansolabehere and Snyder, 2002, 2004; Lee, 2008).

The main challenge to answering this question in the realm of voting is that, by design, voters' decisions in the ballot box are unobserved. Voting, however, is not the only form of political participation. Individuals can also support candidates by making monetary contributions to their campaigns, parties or other affiliated bodies. Campaign finance laws require that campaigns report itemized contributions for those gifts totalling more than $200. These reports are freely available from the Federal Election Commission, meaning that, unlike in the case of voting, the candidates individuals support are observed.

Observed contributions behavior, like widespread voting, cannot be fully rationalized by strategic motives. Many individuals make small contributions that have little chance of influencing electoral outcomes. Federal law also limits how much an individual can give to a campaign to $2700 per election (Federal Election Commission, 2017).[2] Thus, even the largest individual contributions are not expected to meaningfully affect elections. But while a single

[2]Historical limits going back to 2002 are available from the Center for Responsive Politics (2018*b*), here: https://www.opensecrets.org/overview/limits.php.

contribution may do little on the margin, in aggregate contributions from individuals play an outsize role in campaign finance. In 2008, for instance, more than 90% of contributions made to Federal candidates or parties, a total of $4.87 billion, came from individuals (Fremeth, Richter and Schaufele, 2013).

In this paper, I examine political contributions to study the relationship between election results and future political behavior of participants, here the contributors. Specifically, I ask whether contributors to winning candidates are more likely to make future contributions than contributors to losing candidates. A similar question is posed in Peskowitz (2017). Using fixed effects regressions and a minimum-distance regression discontinuity design, Peskowitz estimates that individuals contributing to winning candidates are 6 percentage points more likely to contribute in the next cycle than contributors to losing candidates. He interprets this result as evidence of reinforcement learning. However, this finding alone does not necessarily support a reinforcement learning hypothesis. As Lee (2008) highlights, winning candidates are much more likely than losing candidates to run in future elections. From the contributor's perspective, this amounts to a supply shock that affects contributors to winners and losers differentially.[3][4] Thus, a primary contribution of this paper is that I also examine the destination of future contributions, both in terms of which candidates receive these contributions and which localities these candidates represent, in order to tease apart these competing explanations.

Using contributions data from the Federal Election Commision, I estimate the causal effect of election results on future contribution behavior with a regression discontinuity design. I do this separately for contributors to candidates running for the U.S. House of Representatives and Senate. In the House, I find that, in the cycle following a win, contributors are nearly 10

[3]Here, the contributor is a "political consumer" and the candidate a firm in some sense. An alternative perspective would be to consider the contributor a supplier of contributions, which the candidate demands. In this latter framing, exit of losing candidates would amount to a demand shock.

[4]Peskowitz (2017) acknowledges this as a possible explanation of his results. However, he does not show how much of the measured discontinuities are attributable to giving to the same candidate. Rather, he estimates that the dollar amount of contributions to new candidates increases by 17%. The dependent variable used in this regression is $log(\text{New Contributions}+1)$, the log of one plus the dollar amount of contributions to new candidates. Individuals who do not make a reportable contribution thus receive a value of zero in this specification. He does not present extensive margin results for giving to new candidates.

percentage points more likely to make a donation. Significant discontinuities persist for at least two more electoral cycles. However, much of this is driven by discontinuous giving to the same candidate in future cycles. There is no evidence of increased giving to new candidates, new districts or donations to the Senate. Further, a descriptive analysis shows that contributors to winners and losers give at a similar rate when their candidate of choice runs again. In the Senate, I find a similar discontinuities in giving, again driven by continued gifts to the same candidate. Here too there is little to no evidence of spillovers to other candidates or states.

These results cast doubt on whether reinforcement learning is at play in political behavior. However, these findings still imply that incumbents have a fundraising advantage rising in part from a propensity for their donors to make future contributions. I test for this as well. Following Fouirnaies and Hall (2014), I show that House candidates from winning parties receive $130K more on average in future individual contributions than candidates from losing parties, almost all of which comes from individuals who had previously given to the district. This presents a significant advantage to incumbents, as the average general election House candidate raises just under $600K in total from individual contributions (Center for Responsive Politics, 2018*a*).

In highlighting the importance of candidates to contributions behavior, this paper also speaks to a literature on the determinants of political contributions. Previous studies have highlighted ideological (Ensley, 2009; Johnson, 2010; Barber, 2016; Barber, Canes-Wrone and Thrower, 2017), access-related (Snyder, 1990) and strategic (Gimpel, Lee and Pearson-Merkowitz, 2008; Hill and Huber, 2017) motivations for contributing. This study shows that separate of those concerns the candidates themselves are important in motivating contributions. In addition, this is one of only a handful of studies to examine variation in political contributions over time (Peskowitz, 2017; Fremeth, Richter and Schaufele, 2013). Lastly, the party-level incumbency results contribute to a large literature on the causes and consequences of the incumbency advantage (works cited above as well as: Levitt and Wolfram, 1997; Hirano and Snyder, 2009; Fowler, 2014; Fowler and Hall, 2017), specifically the financial incumbency advantage (Fouirnaies and Hall, 2014). Here, I show a previously unknown source of financial advantage in repeat donors.

The remainder of this paper proceeds as follows: Section 1.1 outlines the empirical strategy; Section 1.2 provides background information on individual campaign contributions and describes the data used for this paper; Section 1.3 presents results related to individual contribution behavior in the House (1.3.1) and Senate (1.3.2); Section 1.4 details the strategy and results relating to the financial incumbency advantage; Section 1.5 concludes.

1.1 Empirical Strategy

To estimate the causal effect of candidates' electoral outcomes on the future giving behavior of their donors, I employ a regression discontinuity design. I estimate regressions of the form:

$$\begin{aligned} y_{i,c,t+k} = \alpha + \tau \text{CandWin}_{c,t} + \beta_1 \text{Voteshare}_{c,t} \\ + \beta_2 (\text{CandWin}_{c,t} \times \text{Voteshare}_{c,t}) + \varepsilon_{i,c,t+k} \end{aligned} \tag{1.1}$$

where $y_{i,c,t+k}$ is a giving outcome of interest for individual i, who gives to candidate c during election cycle t, in future cycle $t+k$, $\text{Voteshare}_{c,t}$ is candidate c's two-party general election voteshare in cycle t, $\text{CandWin}_{c,t}$ is an indicator taking value 1 if candidate c wins in cycle t and $\varepsilon_{i,c,t+k}$ is the error term.[5] $\text{Voteshare}_{c,t}$ is normalized by subtracting 0.5 from the actual two-party voteshare of candidate c in cycle t, so that $\text{Voteshare}_{c,t}$ represents the distance from the plurality threshold.

The coefficient of interest here is τ, which respresents the discontinuity in giving outcome $y_{i,c,t+k}$ when the recipient candidate goes from barely losing (voteshare just below 50%) to barely

[5] "Two-party voteshare" is the candidate's vote total divided by the total number of votes received by the Democratic and Republican party candidates. In this study, only Democratic or Republican candidate recipients are considered. In most elections in the U.S., the winner and runner-up will be from the Democratic or Republican party. Thus, this is the relevant running variable. Use of the two-party voteshare rather than overall votershare is common in studies of this type.

winning (voteshare just above 50%). The primary identifying assumption is that the results of elections near the plurality threshold are sufficiently random to be considered exogenous. Further, I require that neither candidates nor contributors are able to predict or manipulate the results of these close elections.[6]

Individuals may contribute to more than one candidate. If more than one of these candidates participates in a general election, an individual could be assigned multiple running variables and treatment (candidate win or loss) indicators. To avoid this issue, I estimate equation (1.1) only for individuals that contribute to one candidate during a given election cycle. While this limits the external validity of findings, this strategy still addresses the vast majority of donors, as I discuss further in section 1.2.1. Lastly, I will estimate equation (1.1) separately for donors to the House and Senate. This is done because institutional differences between the houses (specifically, differing terms) potentially affect individual contribution behavior in different ways.

I concurrently estimate the likelihood that a candidate seeks future office following a close election to examine how this relates to any estimated discontinuities in giving behavior. To do this, I estimate a regression of the form:

$$\begin{aligned} \text{Candidate Runs}_{c,t+k} = \alpha + \tau \text{CandWin}_{c,t} + \beta_1 \text{Voteshare}_{c,t} \\ + \beta_2(\text{CandWin}_{c,t} \times \text{Voteshare}_{c,t}) + \varepsilon_{c,t+k} \end{aligned} \tag{1.2}$$

where Candidate $\text{Runs}_{c,t+k}$ is an indicator taking value 1 if candidate c runs a primary or general election campaign in future cycle $t+k$. Other terms are defined as they are in equation (1.1). The identifying assumptions here are analgous to those necessary to estimate equation (1.1).

In all regressions, I utilize a bandwidth of 10 percentage points on either side of the plurality threshold and employ triangular weights.[7]. Equations (1.1) and (1.2) are estimated with standard errors clustered at the Congressional district level for the House or Representatives and

[6]McCrary tests for manipulation are depicted in Appendix figure A.3

[7]Estimates with a rectangular kernel and other bandwidths can be found in Appendix tables A.10, A.11, A.12 and A.13.

at the state level for the Senate.

1.2 Background and Data

Section 1.2.1 provides background information regarding the significance of individual campaign contributions to Congressional fundraising, as well as the distribution of donors to Congressional candidates. Section 1.2.2 describes the data used for this project.

1.2.1 Individual Contributions to Federal Congressional Elections

Table 1.1: Congressional Candidate Funding Sources, 2013-2014

	Small Ind.	Large Ind.	PAC	Self-Funding	Other
***Panel A**: House*					
Democrat	10%	**48%**	34%	5%	3%
Republican	7.5%	**44.2%**	33.8%	9.9%	4.6%
***Panel B**: Senate*					
Democrat	17.9%	**55.9%**	16.7%	2.5%	7%
Republican	11.8%	**53.9%**	18.1%	7.5%	8.7%

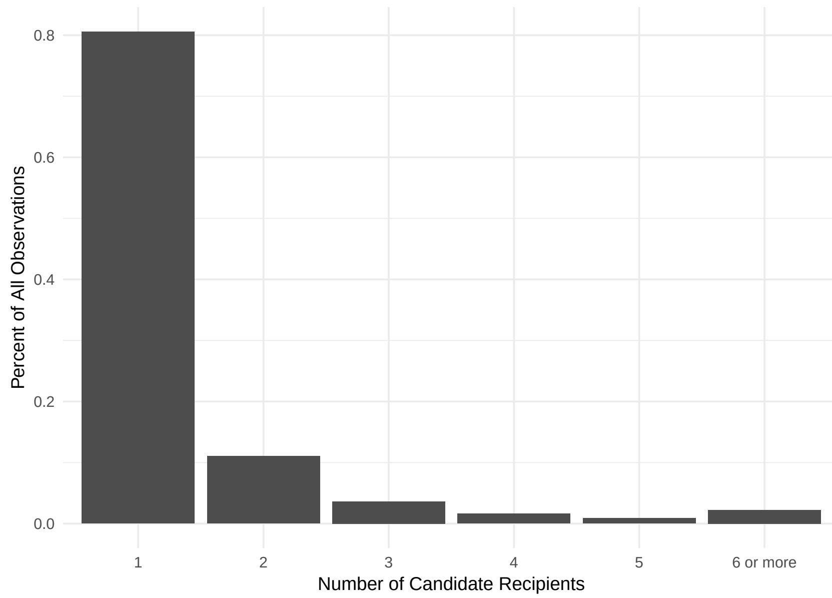
Notes: Entries are percentages of aggregate campaign funds from the sources in the column headings. Small individual contributions (Small Ind.) are contributions from individuals totaling less than $200. Large individual contributions (Large Ind.) are contributions from individuals totalling more than $200. *Source*: Center for Responsive Politics (2018*c*)

Contributions from individuals constitute a significant portion of all funds received by Congressional campaigns. Table 1.1 provides a breakdown of Congressional campaign funding by house, party and source during the 2013-2014 election cycle. There are five funding sources displayed in this table: “Small individual contributions” (Small Ind.), which are donations from individuals that total $200 or less, “Large individual contributions” (Large Ind., emphasized in bold), donations from individuals that exceed $200, contributions from Political Action Committees, candidate self-funding, and all other sources. Panel A shows percentages of total funds received from each funding source for candidates to the House or Representatives, while

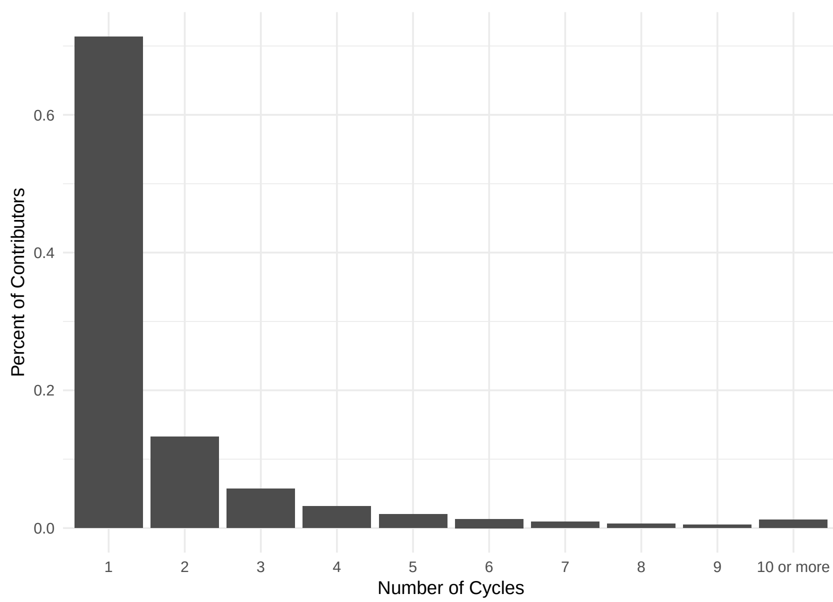
Panel B shows the corresponding information for candidates to the Senate. It is clear from table 1.1 that individual contributions dominate fundraising in both houses. The majority of funds received by Democratic and Republican House candidates come from individual contributions, with nearly 60% of House Democrats' and 52% of House Republicans' funds coming from individual contributions. Among individual contributions, the vast majority of funds come from large contributions. Large contributions make up 48% of all money raised by House Democrats and about 44% of all money raised by House Republicans, in both cases more than 80% of funds raised from individual contributions.

This pattern also exists in the Senate. Senate Democratic and Republican candidates both receive the majority of their funds from large individual contributions alone, accounting for 56% and 54% of their receipts, respectively. As with the House, large contributions make up the majority of funds from all individual contributions in the Senate, accounting for more than 75% of all individual contribution funds for both Democrats and Republicans.

The above discussion illustrates the central role individual contributions, especially large contributions, play in Congressional campaign fundraising. I now provide two stylized facts on the source of these funds, the donors themselves. Figure 1.1 is sourced from the Database on Ideology, Money in Politics, and Elections, a repository of campaign finance data that is the primary data source of this project. Panels (a) and (b) detail the distribution of donors making large contributions to Congressional candidates from 1979-2014. Panel (a) depicts a histogram of donors by the number of candidates they support during any one cycle. This chart show that the vast majority of donors (nearly 80%) make a large contribution to only one candidate in any given cycle. Of the remaining 20%, about half contribute to two candidates and the other half support three or more. Panel (b) depicts a similar histogram of donors by the number of cycles in which they make a large contribution. Again, there is significant mass at 1, with more than 70% of donors making a large contribution in only one cycle. There is a large drop-off after this, with a little more than 15% of donors participating in two cycles and the remainder participating in three or more.



(a) Histogram of Number of Recipients



(b) Histogram of Cycles Active

Figure 1.1: Distribution of Individual Contributors, 1979 - 2014

Notes: In panel (a), contribution records are collapsed to the contributor-candidate-cycle level. The histogram portrays the number of times a contributor-candidate appears during a given election cycle. In panel (b), contribution records are collapsed to contributor-cycle level. The histogram portrays the number of times a contributor appears. *Source*: Database on Ideology, Money in Politics, and Elections

Taken together, these histograms depict a donorate not composed of serial contributors, but rather individuals supporting single candidates. While this former category of donors certainly exists, they are not the typical contributor, even among those making large contributions. In addition, many donors choose not to contribute again after this intial donation.[8] Recall from section 1.1 that I estimate regression discontinuities only for individuals that contribute to one candidate. This discussion illustrates that this is not done purely for methodological convenience. Rather, this simply restricts analysis to the modal donor.

1.2.2 Data

Data Sources

This project uses two main sources of data. Data on political contributions by individuals from 1979-2014 are drawn from the Database on Ideology, Money in Politics, and Elections (DIME)[9]. DIME is composed of contribution records collected from the Federal Elections Commission (FEC). These data include the amounts, reporting date and recipient of donations as well as identifying information on contributors such as their names, addresses, occupations and employers. In constructing the dataset, Bonica created unique identifiers to track candidates and contributors over time.[10] These identifiers are used in this project and enable longitudinal analysis of individual contributors.

Election returns from U.S. House and Senate general elections from 1980-2012 come from the CQ Voting and Elections Collection[11]. These data contain the names, candidate status (incumbent/challenger/open seat) and Congressional district or state-level vote totals for major and third party candidates participating in House and Senate general elections.

[8]Because DIME only contains contribution records going back to the 1980 election cycle, it is possible that some individuals who make only one large contribution from 1979 to 2014 had also made a contribution before the 1980 cycle.

[9]Available at: https://data.stanford.edu/dime

[10]These "Bonica IDs" are constructed algorithmically. More information about the construction of these IDs is available in the DIME documentation, avaiable at: https://data.stanford.edu/dime.

[11]Available at: http://library.cqpress.com/elections/index.php

Data Construction and Sample Selection

Data construction for this project requires a number of important steps, which are outlined in this section. First, DIME contributions data are limited to only individuals giving to House or Senate candidates during the sample period. I aggregate all contributions from a contributor to a candidate in a given cyle into one observation. Campaign committees are required to report contributions totalling more than $200 per election cycle by the Federal Elections Commission. To isolate contributions that meet this criteria, I exclude observations where the total contribution does not exceed $200.

Because I have excluded contributions that total $200 or less, the external validity of results will be limited somewhat. However, the previous sections demonstrates that donations exceeding $200 make up a large fraction of the total contributions received by candidates and are economically relevant themselves. For the remainder of the paper I use phrases such as "make a contribution" or "give to the same candidate" as a shorthand for making a contribution exceeding $200. All dependent variables in the next section are indicators for making such contributions. Thus, it is possible that an individual that I classify as "not making a House contribution" could have given an amount totalling $200 or less to a House candidate. Some of these contributions are reported and appear in DIME, but it is unclear how many do not or what factors lead campaigns to not report such donations. Thus, I consider focusing only on reportable donations a more conservative, and cleaner, approach.

A number of additional data exclusions are made. A small number of observations appear to be PAC, union or corporate contributions miscoded as individual contributions after manual inspection of the data.[12] These observations are excluded. Similarly, contributions made from candidates to themselves are excluded. Of the remaining observations, about 1.5% of the contributor-candidate-cycle observations exceed federal individual contribution limits. These limits, however, are not per cycle but per election. This means that the limits apply

[12]Specifically, I drop observations where the Bonica contributor ID is less than 1,000,000.

separately to total contributions made during primary, general, special and runoff elections. Those observations exceeding the limit in at least one of these election types are flagged and all contributions by a contributor in the violating election cycle are removed.[13]

These contributions data are merged to general election returns from the CQ Press. I only match contributions to Democratic or Republican general election candidates. Matching is done at the candidate-district-cycle level. Candidates are first matched on correspondence of the last name in each dataset. The unmatched observations are then subject to a fuzzy matching procedure using the `R` package `stringdist` to account for slight differences in candidate names across the two datasets.[14] Both fuzzy-matched and unmatched observations are further subject to manual inspection to account for any mistakes or oversights in the matching procedure.

Districts that feature multiple elections in the same cycle are excluded.[15] Any election in which a Democrat and a Republican do not run against one another is excluded, as are elections in which a third party candidate wins.

The sample is then further restricted to those contributors giving to at most one general election candidate (House or Senate) in a given election cycle. One-candidate donors whose candidate runs for the House make up the estimation sample for the House of Representatives. This sample comprises 1,549,619 contributor-cycle observations, 818,141 of which represent contributions to close House elections. Likewise, one-candidate donors whose candidate runs for the Senate make up the estimation sample for the Senate. This sample comprises 964,436 contributor-cycle observations, 683,118 of which contribute to close Senate elections. These samples are summarized in Appendix section A.1.

[13]Results are nearly identical when these datapoints are included.

[14]This issue can arise for a number of different reasons. A candidate's last name may contain a hyphen in the CQ Press data that is missing in the DIME data, for example. Other times first or last names are misspelled or appear in the wrong order in the DIME data. `stringdist` package documentation: https://cran.r-project.org/web/packages/stringdist/stringdist.pdf

[15]This would be the case when there is a general election and also a runoff or special election during the same cycle. This is done because otherwise a contributor may be assigned multiple running variables for gifts to the same candidate in the same cycle.

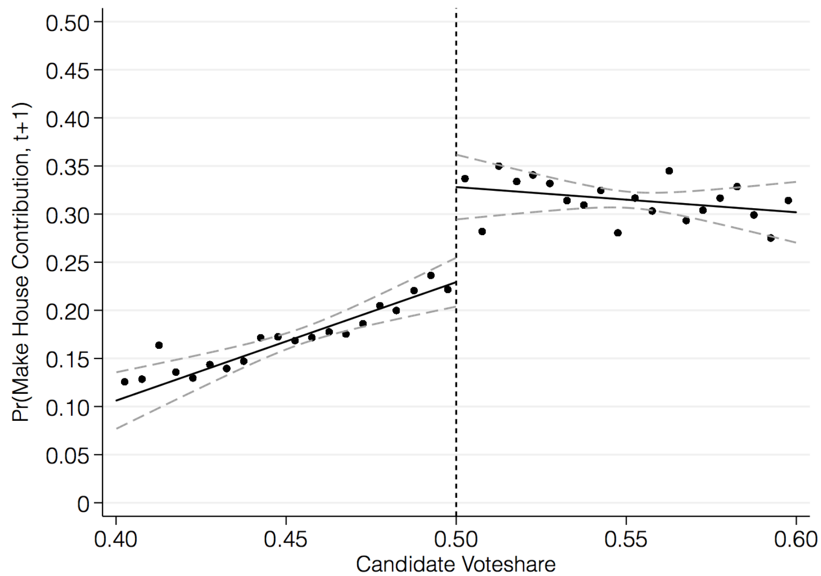
1.3 Main Results

This section details results from estimation of equation (1.1). Section 1.3.1 presents these results for the sample of House donors. Section 1.3.2 presents corresponding results for the sample of Senate donors.

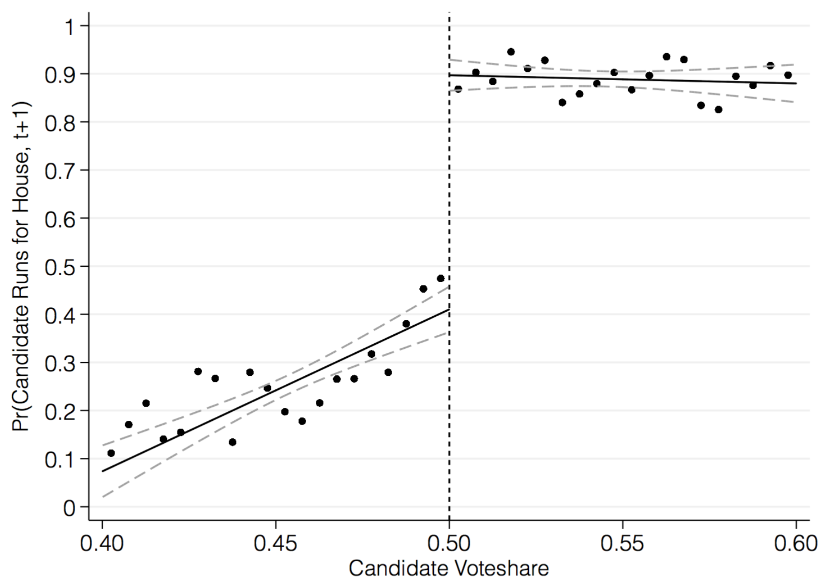
1.3.1 House of Representatives

Panel (a) of Figure 1.2 displays the fraction of donors making a contribution to the House of Representatives in the cycle following their initial contribution. The running variable is the recipient candidate's two-party voteshare. Visible is a large, sharp discontinuity in future giving at the 50% plurality threshold: individuals contributing to a narrowly-losing candidate contribute again at a rate of about 23%, while individuals contributing to narrowly-winning candidate contribute again at a rate of 33%, a 10 percentage point (or roughly 43%) increase. This result is presented in regression form in panel A, column 2 of Table 1.2.

It is clear from this result that there is a significant difference in the giving behavior of contributors to narrowly-winning and narrowly-losing candidates. A naive interpretation of this result could lead one to believe that there is learning among givers based on the results of their candidates' respective elections. However, panel (b) of Figure 1.2 complicates this story. Panel (b) shows the fraction of recipient candidates seeking office in the cycle following the close general election. Again, the running variable is the recipient candidate's two-party voteshare. This figure shows that there is a very large discontinuity in the likelihood of candidates running for future office at the 50% plurality threshold. Narrowly-losing candidates will run in the next cycle in about 41% of cases, while narrowly-winning candidates run again at a rate of almost 90%. This result is presented in regression form in Panel B, column 2 of Table 1.2. Thus, while giving behavior is discontinuous through the cutoff, so too is the likelihood of the receipient candidate seeking office. This is unsurprising, as incumbents generally seek (often succesfully) multiple terms in office. This does mean, however, that more careful analysis is



(a) Likelihood of Giving Next Cycle



(b) Likelihood That Candidate Seeks Office

Figure 1.2: Regression Discontinuity Estimates

Notes: Panel (a) displays the fraction of contributors in each bin making a donation exceeding \$200 in cycle $t+1$. Panel (b) displays the fraction of candidates in each bin running in a primary or general election in cycle $t+1$. The x-axis is the candidate recipient's two-party general election voteshare in cycle t. Solid lines are estimated via ordinary least squares using triangular weights. Standard errors clustered at the Congressional district level are represented by dashed lines. The bin width for both figures is 0.5 percentage points.

required to separate learning (if it exists) from candidate-specific effects. More precisely, because all contributors in the sample give to only one candidate, variation in the continued presence of that candidate could be sufficient to generate this discontinuity in giving. Donors to losing candidates may not find a new candidate they deem worthy of their donations if their original candidate does not run. On the other hand, if their candidate were to win, because they have already made a donation, donors may find the decision to continue to support the candidate a simple one.

Table 1.2: Regression Discontinuity Estimates: Effect of Close Candidate Victory on Overall House Giving and Likelihood of Running for House

	(1) $t-1$	(2) $t+1$	(3) $t+2$	(4) $t+3$
Panel A:				
Pr(Make House Contribution)				
Candidate Wins	-0.001	0.099***	0.058***	0.033***
	(0.014)	(0.020)	(0.009)	(0.008)
Constant	0.190***	0.229***	0.155***	0.132***
	(0.010)	(0.013)	(0.008)	(0.006)
Observations	807308	818141	702559	583824
Contributors	678182	687539	598615	502515
Elections	1854	1984	1843	1690
Panel B:				
Pr(Candidate Runs for House)				
Candidate Wins	-0.013	0.485***	0.482***	0.410***
	(0.039)	(0.029)	(0.036)	(0.037)
Constant	0.438***	0.412***	0.162***	0.122***
	(0.025)	(0.024)	(0.020)	(0.018)
Observations	3495	3726	3448	3146
Elections	1854	1984	1843	1690

Notes: Panel A displays results of estimation of equation (1.1) with the dependent variable being an indicator of an individual making a contribution to a House candidate exceeding $200 in cycle indicated in the column heading. Panel B displays results of estimation of equation (1.2) with the dependent variable being an indicator of a candidate running in a primary or general election campaign in the cycle indicated in the column heading. In both cases, regressions are estimated with triangular weights and slope coefficients are omitted. Robust standard errors for all regressions are clustered at the congressional district level and are presented in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Before diving deeper into this issue, I draw attention to the remaining entries of Table 1.2. These results reveal that both the discontinuity in giving and the discontinuity in running for office are not limited to the period immediately after the close election, but persist for at least two more election cycles. Panel A, column 3 of Table 1.2 shows that on average contributors to narrowly-losing candidates give again in $t+2$ about 16% of the time, while contributors to narrowly winning candidates give about 22% of the time. In $t+3$, contributors to narrowly-losing and -winning candidates give at rates of 13% and 16%, respectively. Both of these discontinuities are signficant at the 1% level. Panel B shows that the corresponding discontinuities in reelection seeking in cycles $t+2$ and $t+3$ are 48 percentage points (a jump from 16% to 64%) and 41 percentage points (a jump from 12% to 53%), respectively. These estimates are similarly highly significant. Also, note that neither House giving nor the propensity to seek election is discontinuous in the cycle before the close election, as seen in column 1 of Table 1.2.

To unpack the discontinuity in future giving, I turn to examining giving by recipient candidate. Panels A and B of Table 1.3 decompose the effects measured in Panel A of 1.2 by candidate. That is, in Panel A the dependent variable is an indicator for giving to the same candidate as in cycle t, while in Panel B the dependent variable is an indicator for giving to a candidate different from the recipient in cycle t. The estimates in Panel A show clearly that there are large and highly signficiant discontinuities in the propensity to give to the same candidate in cycles $t+1$ through $t+3$. In $t+1$, for example, donors to narrowly-losing candidates give to their candidate again about 12% of the time, while donors to narrowly-winning candidates give again nearly 28% of the time (a 16 percentage point, or 130%, increase). This coincides with a significant negative discontinuity in giving to new candidates. The second column of Panel B shows that donors to narrowly-losing candidates will give to new candidates in $t+1$ 12% of the time, while donors to narrow winners give to new candidates only about 8% of the time. Negative discontinuities also persist through $t+3$. None of these discontinuities exist in the cycle prior to the close election, so none of these results appear to be driven by past discrepencies in giving behavior.

Table 1.3: Regression Discontinuity Estimates: Effect of Close Candidate Victory on Giving By Candidate (House)

	(1) $t-1$	(2) $t+1$	(3) $t+2$	(4) $t+3$
Panel A:				
Pr(Give to Same Candidate)				
Candidate Wins	0.005	0.156***	0.088***	0.057***
	(0.017)	(0.022)	(0.013)	(0.011)
Constant	0.119***	0.121***	0.044***	0.024***
	(0.011)	(0.014)	(0.008)	(0.005)
Panel B:				
Pr(Give to New Candidate)				
Candidate Wins	-0.006	-0.039***	-0.018***	-0.015***
	(0.006)	(0.007)	(0.006)	(0.005)
Constant	0.087***	0.123***	0.119***	0.113***
	(0.005)	(0.006)	(0.005)	(0.005)
Observations	807308	818141	702559	583824
Contributors	678182	687539	598615	502515
Elections	1854	1984	1843	1690

Notes: Panel A displays results of estimation of equation (1.1) with the dependent variable being an indicator of an individual making a contribution exceeding $200 to the same candidate from cycle t in the cycle indicated in the column heading. Panel B displays results of estimation of equation (1.1) with the dependent variable being an indicator of an individual making a contribution exceeding $200 to a candidate other than the cycle t candidate in the cycle indicated in the column heading. In both cases, regressions are estimated with triangular weights and slope coefficients are omitted. Robust standard errors for all regressions are clustered at the congressional district level and are presented in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.4: Regression Discontinuity Estimates: Effect of Close Candidate Victory on Giving Within District (House)

	(1) $t-1$	(2) $t+1$	(3) $t+2$	(4) $t+3$
Panel A:				
Pr(Give to Same District)				
Candidate Wins	-0.026	0.104***	0.063***	0.048***
	(0.019)	(0.027)	(0.011)	(0.012)
Constant	0.166***	0.186***	0.106***	0.074***
	(0.015)	(0.019)	(0.010)	(0.009)
Panel B:				
Pr(Give to Same Cand. in Same Dist.)				
Candidate Wins	-0.023	0.152***	0.093***	0.065***
	(0.024)	(0.027)	(0.015)	(0.013)
Constant	0.137***	0.123***	0.037***	0.022***
	(0.017)	(0.017)	(0.010)	(0.008)
Panel C:				
Pr(Give to New Cand. in Same Dist.)				
Candidate Wins	-0.002	-0.046***	-0.030***	-0.019**
	(0.007)	(0.009)	(0.009)	(0.008)
Constant	0.032***	0.066***	0.070***	0.052***
	(0.005)	(0.008)	(0.007)	(0.006)
Observations	393446	607370	343772	198553
Contributors	352191	533526	313348	188676
Elections	984	1497	1060	713

Notes: Panel A displays results of estimation of equation (1.1) with the dependent variable being an indicator of an individual making a contribution exceeding $200 to a candidate in the same district as the cycle t candidate in the cycle indicated in the column heading. Panel B displays results of estimation of equation (1.1) with the dependent variable being an indicator of an individual making a contribution exceeding $200 to the cycle t candidate in the same district as the cycle t candidate in the cycle indicated in the column heading. Panel C displays results of estimation of equation (1.1) with the dependent variable being an indicator of an individual making a contribution exceeding $200 to a canddiate other than the cycle t candidate in the same district as the cycle t candidate in the cycle indicated in the column heading. In all cases, regressions are estimated with triangular weights and slope coefficients are omitted. Robust standard errors for all regressions are clustered at the congressional district level and are presented in parantheses. *Significance*: * $p<0.1$, ** $p<0.05$, *** $p<0.01$

I now further restrict the analysis to the district of the initial contribution. Table 1.4 reports results for when the dependent variable is either an indicator of giving to a candidate in same district as the cycle t contribution in a future cycle (Panel A), giving to the same candidate in the same district (Panel B), or giving to a new candidate in the same district (Panel C).[16] Column 2 of Table 1.4 shows an estimated discontinuity in giving to the same district in cycle $t+1$ of 10.5 percentage points. This discontinuity is very close in magnitude to the overall discontinuity in giving from Table 1.2. Panels B and C show that this discontinuity is composed of a 15.2 percentage point jump in giving to the same candidate within the same district, and a 4.6 percentage point drop in giving to new candidates within the same district. As with the estimates of overall giving and giving by candidate from above, this pattern persists in cycles $t+2$ and $t+3$. Additionally, as with the previous results, no significant discontinuities are found in cycle $t-1$ giving.

Table 1.5 presents results for future giving to the Senate and new districts. In Panel A the dependent variable is an indicator for making a Senate contribution in a future cycle, while Panel B's dependent variable is an indicator for making a future contribution to candidate in a district different from the district of the recipient in cycle t. Recall that all contributors in the estimation sample only give to one candidate, with that candidate running for the House. Thus, none of the contributors in the sample make a Senate contribution in cycle t. In the two cycles following the close election, there is no estimated discontinuity in giving to the Senate and coefficients on candidate victory are very close to zero, as seen in columns 2 and 3 of Panel A of table 1.5. However, column 4 shows a marginally significant uptick in Senate giving among winning-candidate contributors in $t+3$. In this future cycle, contributors to winning

[16]Note that here the sample is smaller for all time periods than for those regressions in Table 1.2. This is because decenniel redistricting alters the boundaries of Congressional districts. Congressional district borders are redrawn following the decenniel Census. Thus, the years in which "new" Congresssional district borders come into effect during our sample period are years ending in 2: 1982, 1992, 2002 and 2012. This will naturally decrease the sample size, as it shortens the window over which analysis of district giving behavior is possible. This also results in discrepancies in the exact magnitudes of estimated effects across the two samples. However, these discrepencies are small. In no case is the direction or significance, and thus the interpretation, of estimated effects different. Results of all regressions estimated over a fully balanced panel are presented in appendix table A.8.

Table 1.5: Regression Discontinuity Estimates: Effect of Close Candidate Victory on Giving to Senate and to Other Districts (House)

	(1)	(2)	(3)	(4)
	$t-1$	$t+1$	$t+2$	$t+3$
Panel A:				
Pr(Make Senate Contribution)				
Candidate Wins	-0.001	0.002	-0.002	0.012*
	(0.008)	(0.005)	(0.005)	(0.006)
Constant	0.078***	0.079***	0.083***	0.067***
	(0.007)	(0.005)	(0.005)	(0.004)
Observations	807308	818141	702559	583824
Contributors	678182	687539	598615	502515
Elections	1854	1984	1843	1690
Panel B:				
Pr(Give to New District)				
Candidate Wins	-0.002	-0.003	-0.004	0.000
	(0.004)	(0.004)	(0.006)	(0.007)
Constant	0.053***	0.069***	0.072***	0.067***
	(0.003)	(0.004)	(0.005)	(0.006)
Observations	393446	607370	343772	198553
Contributors	352191	533526	313348	188676
Elections	984	1497	1060	713

Notes: Panel A displays results of estimation of equation (1.1) with the dependent variable being an indicator of an individual making a contribution exceeding $200 to a Senate candidate in the cycle indicated in the column heading. Panel B displays results of estimation of equation (1.1) with the dependent variable being an indicator of an individual making a contribution exceeding $200 to a candidate in a district other than that of the cycle t candidate in the cycle indicated in the column heading. In both cases, regressions are estimated with triangular weights and slope coefficients are omitted. Robust standard errors for all regressions are clustered at the congressional district level and are presented in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

candidates are 1.2 percentage points more likely to give to the Senate than their counterparts contributing to losing candidates. Interestingly, this result is potentially explained by a concurrent and statistically signficant uptick in candidates winning close elections in time t transitioning to running for the Senate in time $t+3$. This result is shown in Appendix table A.3. Panel B shows that no estimated discontinuity exists in future giving to new districts in any of the cycles examined. All coefficients on candidate victory are close to zero. Lastly, as with previous estimates, there appear to be no signficant differences in giving in either of these categories in the cycle preceding the close election.

The discontinuities in future giving to the same candidate presented above are strong evidence that the discontinuity in candidate office-seeking is a signficant driver of future contributions. However, this does not necessarily preclude behavioral effects from election results themselves. Recall that more than 40% of losing candidates in close elections seek reelection in the following cycle. It remains unclear if contributors to these candidates display different future contribution patterns than contributors to their winning counterparts. Because I do not have additional exogenous variation in whether a candidate chooses to run following a close election, I cannot answer this question causally. Instead, I undertake a descriptive analysis of behavior near the cutoff that suggests the absence of additional behavioral effects.

To do this, I estimate two additional sets of regression discontinuity regressions. In the first, I limit the sample to only those individuals contributing to candidates that choose to run again in the following cycle. In the second, I restrict the sample to only those individuals contributing to candidates that do not run again. Otherwise, the regressions are the same as those above and use the same dependent variables. Table 1.6 presents the results to these regressions translated into estimates of conditional means at cutoff. Column 1 reports the mean of each dependent variable for those individuals contributing to losing candidates that run again, column 2 reports these values for contributors to winning candidates that run again, while columns 3 and 4 report these values for contributors to losing and winning candidates that do not run again, respectively. In the first row of Table 1.6, the dependent variable is an indicator for making

Table 1.6: Descriptive Analysis: Conditional Means at the Cutoff

	(1)	(2)	(3)	(4)
	Candidate Runs, $t+1$		Does Not Run, $t+1$	
	After Loss	After Win	After Loss	After Win
Pr(Make House Contribution, $t+1$)	0.359	0.330	0.147	0.308
Pr(Give to Same Candidate, $t+1$)	0.307	0.286	0.005	0.197
Pr(Give to New Candidate, $t+1$)	0.088	0.078	0.144	0.136
Pr(Give to Same District, $t+1$)	0.331	0.292	0.093	0.281
Pr(Give to Same Cand. in Same Dist., $t+1$)	0.307	0.283	0.005	0.220
Pr(Give to New Cand. in Same Dist., $t+1$)	0.031	0.014	0.088	0.061
Pr(Give to New District, $t+1$)	0.064	0.065	0.071	0.069
Pr(Make Senate Contribution, $t+1$)	0.068	0.077	0.085	0.112

Notes: Entries in columns (1) and (2) are results from estimation of equation (1.1) conditioning on the recipient candidate running in a primary or general election in cycle $t+1$. Column (1) entries are the estimated constant terms and column (2) are the result of addition of the constant term and τ coefficient. Entries in columns (3) and (4) are results from estimation of equation (1.1) conditioning on the recipient candidate not running in a primary or general election in cycle $t+1$. Column (3) entries are the estimated constant terms and column (4) are the result of addition of the constant term and τ coefficient. The dependent variable in each regression is described in each row. No entries in column (1) and (2) are statistically different at the 5% level. All entries in columns (3) and (4) statistically different at the 5% level except Give to New Candidate, Give to New Cand. in Same Dist., Give to New District and Make Senate Contribution.

any house contribution in cycle $t+1$. Contributors to losing candidates that seek future office contribute again about 36% of the time, while contributors to winning candidates contribute again at a similar rate of 33%. When their candidate does not run again, contributors to losing candidates only give again about 14% of the time, a drop of 22 percentage points. Interestingly, in the cases where winning candidates do not seek reelection, their donors still contribute at a relatively high rate of 30%. The remaining rows of 1.6 show why this is the case. Row 2 shows results for when the dependent variable is giving again to the same candidate. As in row 1, regardless of whether the candidate won or lost, when the candidate runs again, about 30% of contributors will make another donation. When the candidate does not run, however, this changes. When a candidate loses and does not seek reelection, the percent of repeat contributors is virtually zero. However, winning candidates that do not run again will receive contributions from about 20% of their previous contributors. I interpret this as a result of the requirements of being a member of congress. Many members of congress (especially junior members) are under continual pressure to raise funds.[17] Thus, before they eventually make the decision not to run, sitting congressmen have likely made appeals to past donors for contributions for a prospective reelection campaign. It also appears that when candidates run again, their contributors are less likely to give to new candidates, specifically in their district, as evidenced by rows 3 and 6. Consistent with the previous results, giving to new districts appears largely orthogonal to whether or not the original recipient candidate seeks office. Lastly, there appears to be an uptick in giving to the Senate when winning candidates do not run for reelection in the House. About 23% of these candidates run for Senate, compared to less than 1% of losing House candidates that do not run another House campaign. This discontinuity seems sufficient to generate this jump in Senate giving.

Taken together, these results paint a clear picture of giving behavior following close House elections. A large discontinuity exists in future giving to the House between contributors

[17] Reports have said that the average congressman spends more than half of their time raising funds (O'Donnell, 2016).

to winning candidates and those to losing candidates. Much of this discontinuity is the result of increased giving to the same candidate in future cycles. Further, the candidate's continued presence seems to crowd out gifts to new candidates, specifically within the district of their original contribution. Contributors to candidates that seek future office, regardless of the results of the election, exhibit similar contribution patterns, specifically a higher likelihood of contributing to their candidate in the future. Further, any apparent spillovers to the Senate also seem to be driven by discontinuous Senate office-seeking by winners. Thus, it is likely that the discontinuity in overall giving is driven by the large discontinuity in the likelihood of candidates running again following close elections.

1.3.2 Senate

This section reports results for contributors to the Senate, which largely reaffirm the House results above. Again, note that time periods are defined here as they in the House analysis. Thus, a winning candidate will usually not run again following a close election in cycle t until cycle $t+3$, as Senate terms last 6 years. Table 1.7 reports regression discontinuity estimates for giving to the Senate in future cycles (Panel A) and the likelihood the candidate runs for office (Panel B). As expected, given the House results from above, there is significant positive jump in making a Senate contribution in cycle $t+3$, coinciding with a large jump in the likelihood the candidate seeks reelection (presented in column 4 of Panels A and B, respectively). Contributors to time t narrowly losing candidates contribute in time $t+3$ at a rate of 12%, while contributors to narrowly winning candidates contribute at a rate of about 19%. The candidates that narrowly lose in time t seek reelection in cycle $t+3$ about 8% of the time, while narrowly winning candidates, now incumbents, seek reelection about 80% of the time.

There are a small, but significant discontinuities in giving by contributors in cycles $t+1$ and $t+2$, while no such discontinuity exists for candidates seeking office in those cycles, as evidenced by columns 2 and 3 of table 1.7. Contributors to narrowly-winning candidates are 3.4

Table 1.7: Regression Discontinuity Estimates: Effect of Close Candidate Victory on Overall Senate Giving and Likelihood of Running for Senate

	(1) $t-1$	(2) $t+1$	(3) $t+2$	(4) $t+3$
Panel A:				
Pr(Make Senate Contribution)				
Candidate Wins	-0.002	0.034***	0.035***	0.066***
	(0.013)	(0.012)	(0.008)	(0.010)
Constant	0.105***	0.103***	0.091***	0.119***
	(0.012)	(0.009)	(0.008)	(0.007)
Observations	671230	683118	619836	549817
Contributors	603076	613520	558645	558645
Elections	256	279	262	262
Panel B:				
Pr(Candidate Runs for Senate)				
Candidate Wins	0.025	-0.022	-0.022	0.710***
	(0.038)	(0.027)	(0.033)	(0.070)
Constant	0.018	0.040*	0.049*	0.084***
	(0.014)	(0.022)	(0.026)	(0.028)
Observations	446	483	455	427
Elections	256	279	262	247

Notes: Panel A displays results of estimation of equation (1.1) with the dependent variable being an indicator of an individual making a contribution to a Senate candidate exceeding $200 in cycle indicated in the column heading. Panel B displays results of estimation of equation (1.2) with the dependent variable being an indicator of a candidate running in a Senate primary or general election campaign in the cycle indicated in the column heading. In both cases, regressions are estimated with triangular weights and slope coefficients are omitted. Robust standard errors for all regressions are clustered at the state level and are presented in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

and 3.5 percentage points more likely to contribute in cycles $t+1$ and $t+2$, respectively, than contributors to narrowly losing candidates. Narrowly winning candidates are less likely than narrowly losing candidates to run for Senate in cycles $t+1$ and $t+2$ by about 2.2 percentage points, though this results is not significant and losing candidates only seek office 4 to 5 five percent of the time during these cycles. While this appears consistent with a behavioral response outside of candidate effects, it could also be driven by Senators beginning to fundraise for a cycle $t+3$ campaign in the intervening cycles.

Table 1.8: Regression Discontinuity Estimates: Effect of Close Candidate Victory on Giving By Candidate (Senate)

	(1) $t-1$	(2) $t+1$	(3) $t+2$	(4) $t+3$
Panel A:				
Pr(Give to Same Candidate)				
Candidate Wins	-0.004	0.023**	0.038***	0.109***
	(0.014)	(0.009)	(0.008)	(0.016)
Constant	0.032***	0.013**	0.007**	0.016***
	(0.009)	(0.006)	(0.003)	(0.006)
Panel B:				
Pr(Give to New Candidate)				
Candidate Wins	0.002	0.016*	0.005	-0.033***
	(0.009)	(0.009)	(0.008)	(0.010)
Constant	0.079***	0.092***	0.085***	0.106***
	(0.009)	(0.006)	(0.008)	(0.007)
Observations	671230	683118	619836	549817
Contributors	603076	613520	558645	498400
Elections	256	279	262	247

Notes: Panel A displays results of estimation of equation (1.1) with the dependent variable being an indicator of an individual making a contribution exceeding $200 to the same candidate from cycle t in the cycle indicated in the column heading. Panel B displays results of estimation of equation (1.1) with the dependent variable being an indicator of an individual making a contribution exceeding $200 to a Senate candidate other than the cycle t candidate in the cycle indicated in the column heading. In both cases, regressions are estimated with triangular weights and slope coefficients are omitted. Robust standard errors for all regressions are clustered at the state level and are presented in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

To tease apart these competing explanations, I present results by candidate in table 1.8.

These results suggest a story in line with the House results. Column 4 shows results for cycle $t+3$. In cycles $t+3$, contributors to narrowly winning candidates in time t are 10.6 percentage points more likely to make a contribution to the same candidate than contributors to narrowly losing candidates. Further, in cycle $t+3$, contributors to narrowly winning candidates are 3.3 percentage points less likely to give to new candidates than contributors to narrowly losing candidates. Columns 2 and 3 show results for cycles $t+1$ and $t+2$. Panel A shows that, despite their candidates rarely actively seeking office in cycles $t+1$ and $t+2$, contributors to narrowly candidates make new contributions to these candidates. In cycle $t+1$, contributors to narrowly-winning candidates are 2.3 percentage points more likely to make a contribution to their candidate. In cycle $t+2$, they are 3.8 percentage points more likely to do so. There does not appear to much of an effect in terms of giving to new candidates. Panel B shows that contributors to narrowly winning candidates are 1.6 percentage points more likely to give to a new candidate in cycle $t+1$ than contributors to narrowly losing candidates, though this difference is only marginally significant.[18] In cycle $t+2$, these contributors are only 0.5 percentage points more likely to make such a contribution and the difference it not signficant.

The interpretation of these results is rather straightforward. While winning Senate candidates do not run for reelection until cycle $t+3$, they may still begin collecting donations for a prospective reelection campaign soon after their election.[19] This starts in the cycle immediately following the close election, seems to pick up somewhat in cycle $t+2$ and significantly intensifies in cycle $t+3$.

In Appendix section A.2, I report results for giving within the state of the original contribution, giving to the House and giving to new states. These results corroborate findings from the House: effects are concentrated within state and there are no estimated spillovers to other states or giving to the House.

[18]Further, this result is not significant when estimation is done using a balanced panel of contributors (Appendix table A.9) or using narrower bandwidths (Appendix tables A.12 and A.13).

[19]This echoes the result from above where Congressmen that eventually decide not to seek reelection still show receive significant contributions from their past donors.

1.4 Consequences for the Incumbency Advantage

While there is little evidence of behavioral effects in political contributions by individuals, the candidate effects demonstrated above have significant implications for campaign fundraising. Individuals respond when a candidate they have shown previous interest in seeks office again. When a candidate loses, while some of their donors will support a new candidate in the same district, many do not. This suggests that winning candidates have a pool of past donors from which they can draw new contributions that is larger than that of a prospective challenger. This implies an incumbency advantage in individual contributions. In this section, I show the existence and estimate the magnitude of such an advantage.

Past research has shown the existence of a financial incumbency advantage, driven largely by interest groups. Fouirnaies and Hall (2014) show that there is also an advantage among consumption-oriented donors such as individuals and ideological PACs, but their analysis does not separate these groups. Thus, it is unclear how much of this advantage comes specifically from an advantage among individuals. Further, the role of repeat donors in shaping this advantage is unknown.

To demonstrate and quantify a financial incumbency advantage from individuals, I follow Fouirnaies and Hall (2014) in undertaking a party-district-level analysis. I aggregate all contributions from individuals to the parties of their recipients (either Democratic and Republican) within a particular district.[20] I again use a regression discontinuity design to compare the totals and district shares of contributions to the two parties in the cycle following a close election.[21] I do this both for all donations to candidates within the district and those donations coming from individuals who previously contributed during the close election.

[20] Again, because I do not observe contributions that total less than $200, I can only aggregate contributions exceeding $200. Thus, all totals are totals of large contributions only

[21] Here, there are two observations per district, one for the Democratic party and one for the Republican party. Often analysis of this sort only utilizes observations from one of the parties per district. However, this only results in an estimate of a discontinuity for when that party wins, rather than a weighted average of the discontinuity estimates for both parties. The results to come are robust to making this decision. Estimates only for the Democratic party can be seen in appendix table A.7.

I estimate regressions of the form:

$$y_{p,d,t+1} = \alpha + \tau \text{PartyWin}_{p,d,t} + \beta_1 \text{PartyVoteshare}_{p,d,t}$$
$$+ \beta_2 (\text{PartyWin}_{p,d,t} \times \text{PartyVoteshare}_{p,d,t}) + \varepsilon_{p,d,t+1} \tag{1.3}$$

where $y_{p,d,t+1}$ is an outcome of interest for party p, in district d, in cycle $t+1$, $\text{PartyVoteshare}_{p,d,t}$ is the (normalized) two-party voteshare of party p's candidate, in district d, in election cycle t, and $\text{PartyWin}_{c,t}$ is an indicator taking value 1 if party p's candidate wins.

There are four outcomes of interest used for $y_{p,d,t+1}$. The first is "Party Share," which is the party's share of contributions to all candidates in district d in cycle $t+1$. Specifically, Party Share is defined as:

$$\text{Party Share}_{p,d,t+1} = \frac{\text{Party Total}_{p,d,t+1}}{\text{District Total}_{d,t+1}}$$

where "Party Total" is the total dollar amount of all contributions to candidates from party p in district d in cycle $t+1$ and "District Total" is the total dollar amount of all contributions to candidates in district d in cycle $t+1$, regardless of their party. The second outcome of interest is "Party Repeat Share," defined as:

$$\text{Party Repeat Share}_{p,d,t+1} = \frac{\text{Party Repeat Total}_{p,d,t+1}}{\text{District Total}_{d,t+1}}$$

where "Party Repeat Total" is the total dollar amount of all contributions to candidates from party p in district d in cycle $t+1$ that come from individuals who previously donated to district d in cycle t. The other two outcomes of interest are Party Total and Party Repeat Total, which are defined above.

I restrict the analysis to those districts where there are positive total contributions and to consecutive cycles not affected by redistricting.[22] As with previous regressions, I utilize a

[22]See footnote 16.

bandwidth of 10 percentage points and trangular weights.[23] Standard errors are clustered at the Congressional district level.

1.4.1 Incumbency Results

Figure 1.3 shows visually the results for Party Share and Party Repeat Share. Panel (a) shows a sharp discontinuity in Party Share following a close election. Candidates from narrowly-losing parties receive about 44% of the district's individual contributions in cycle $t+1$ while candidates from narrowly-winning parties receive 56% of future donations. This 12 percentage point jump is signficant at the 1% level. Panel (b) displays a similar sharp discontinuity in Party Repeat Share. Repeat contributions as a share of all district contributions are about 11% for narrowly-losing parties, while they are 21% for narrowly-winning parties. This 10 percentage point increase is also signficant at the 1% level.

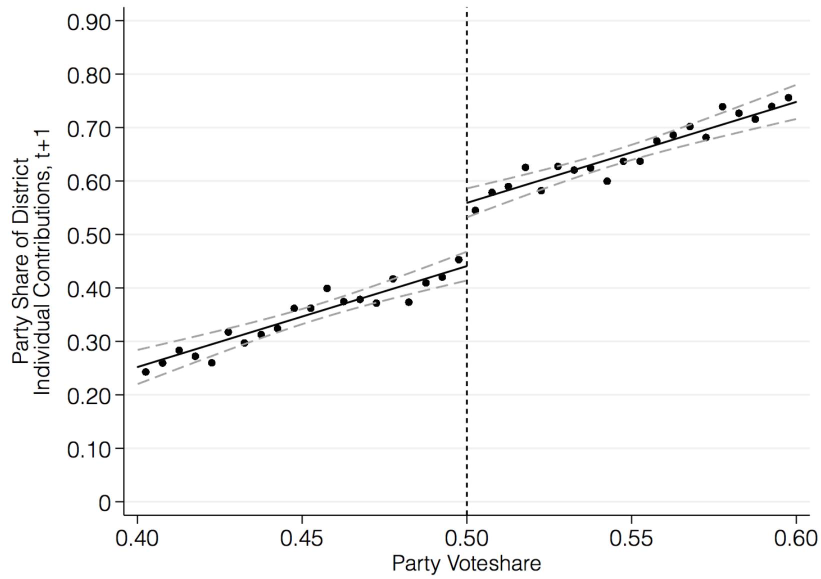
Table 1.9: Regression Discontinuity Estimates: Effect of Party Victory on Party Contributions Next Cycle

	(1) Party Share	(2) Party Repeat Share	(3) Party Total	(4) Party Repeat Total
Party Win	0.118***	0.104***	130.660***	124.375***
	(0.027)	(0.011)	(30.357)	(14.515)
Constant	0.441***	0.110***	580.785***	172.458***
	(0.014)	(0.005)	(38.724)	(14.997)
Observations	3004	3004	3004	3004
Elections	1502	1502	1502	1502
Districts	831	831	831	831

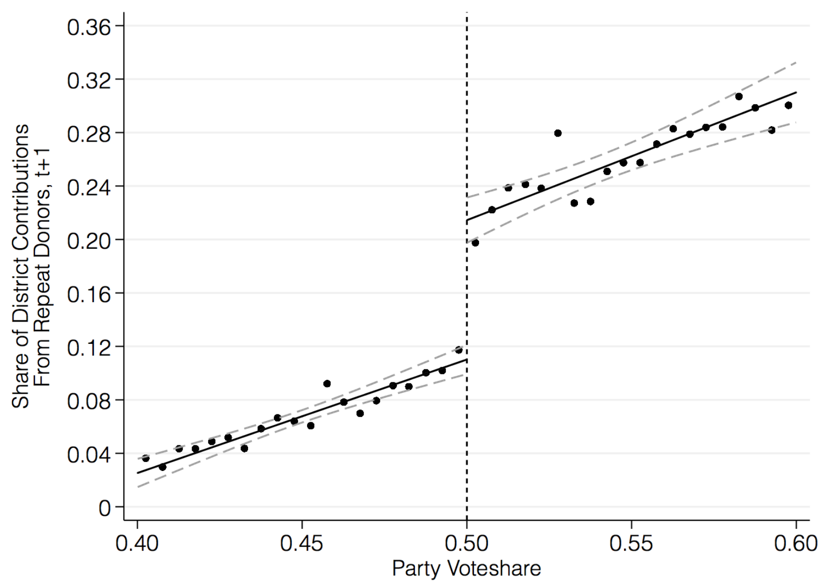
Notes: Entries are coefficients resulting from estimation of equation (1.3) with the dependent variables indicated by column headings, all of which are defined above. In all cases, regressions are estimated with triangular weights and slope coefficients are omitted. Robust standard errors for all regressions are clustered at the Congressional district level and are presented in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.9 shows these results in regression form in columns 1 and 2. Results for Party Total and Party Repeat Total are reported in columns 3 and 4. Column 3 shows candidates

[23] See appendix tables A.14 and A.15 for results from estimation using a rectangular kernel and other bandwidths.



(a) Share of District Contributions



(b) Share of District Repeat Contributions

Figure 1.3: Regression Discontinuity Estimates: Incumbency Advantage

Notes: Panel (a) displays the average value by bin of Party Share, defined above, in cycle $t+1$. Panel (b) displays the the average value by bin of Party Repeat Share, defined above, in cycle $t+1$. The x-axis is the two-party general election voteshare of the party's candidate in cycle t. Solid lines are estimated via ordinary least squares using triangular weights. Standard errors clustered at the Congressional district level are represented by dashed lines. The bin width for both figures is 0.5 percentage points.

from narrowly losing parties receive about \$580,000 in total contributions, while candidates from narrowly-winning parties receive \$710,000 (a \$130,000 or 22% increase). In 2014 the average Democratic general election candidate to the House of Representatives recieved about \$600,000 from individual contributions, so the estimates above constitute a sizable advantage. Column 4 reveals that almost all of this advantage is delivered by repeat donors. Candidates from narrowly-losing parties receive about \$172,000 from repeat contributors, while candidates in narrowly-winning parties receive about \$296,000 (a \$124,000 or 72% increase). Both of these discontinuities are significant at the 1% level. Appendix table A.6 shows that none of these variables are discontinuous through the cutoff during cycle t.

These results show that there exists a significant incumbency advantage in terms of individual contributions. Almost all of this advantage comes from an advantage among repeat donors. In light of the results in sections 1.3.1 and 1.3.2, it is clear that this is due to candidate effects. Winning parties have the advantage of continuity in candidates, which I have shown to be important in determining future contributions from individuals. Losing parties will often run a new candidate to challenge the incumbent, which seems to in itself guarantee drop off in the base of financial support.

1.5 Conclusion

In this paper, I provide strong evidence that election results have a significant effect on the future campaign contributions of individuals. Contributors to winning House candidates are nearly 10 percentage points more to make a contribution in the next election cycle than contributors to losing candidates. However, rather than confirming that reinforcement learning is to blame, I show that it is the effect election results have on candidates, and which candidates seek office, that drive contributions. This analysis in the House is corroborated by similar findings in the Senate.

While reinforcement learning appears not be at play, these results have important implications for the financial incumbency advantage. Incumbents can leverage prior connections to donors that fresh challengers do not possess. I estimate a significant financial incumbency advantage in terms of individual contributions, almost all of which comes from these previously involved donors.

Taken together these findings raise more questions about political participation and the incumbency advantage. Specifically, do these patterns carry over to other forms of political participation, particularly voting? There is some evidence suggesting that voting for a particular candidate engenders more positive future opinions of that candidate (Mullainathan and Washington, 2009). This could generate similar effects if past voters are more inclined to turnout simply by having voted for an incumbent in the past, especially if they face a new challenger. This would also imply that at least some of the incumbency advantage is due to candidate effects that induce differential turnout among previous voters.

Relatedly, while the motivations behind contributing and voting may be similar, contributors and voters have been shown, on average, to be very different groups. Donors are generally less diverse, older, wealthier and more educated than voters (Hill and Huber, 2017). Thus, the possibility remains that reinforcement learning could manifest in voting behavior, even if it does not appear to be present in contributions behavior.

Despite demographic differences between contributors and voters, however, the figures in section 1.2.1 detail a surprisingly “casual” donorate, in which individuals often only make one donation during their political life. This seems at odds with the common understanding of political contributors, especially considering that these individuals are making large enough contributions to require reporting. The results presented here can account for some, but not all, of this dropoff in giving. More research is required to understand why more donors do not continue to contribute after their initial donation.

Acknowledgements

Chapter 1, in full, is currently being prepared for submission for publication of the material. Bigenho, Jason. “Elections, Individuals and Incumbency: Campaign Contributions following Close Congressional Races.” The dissertation author was the sole investigator and author of this material.

Chapter 2

Social Comparisons in Peer Effects

The importance of social information in individual decision making is undeniable. Not only do individuals learn from their peers when making decisions over new and unfamiliar opportunities (Foster and Rosenzweig, 1995; Duflo and Saez, 2002; Bursztyn et al., 2014; Dahl, Løken and Mogstad, 2014), but they also converge to behavioral conformity in the work place (Mas and Moretti, 2009; Bandiera, Barankay and Rasul, 2010) and in the classroom (Hoxby, 2000; Sacerdote, 2001; Zimmerman, 2003; Bursztyn, Egorov and Jensen, 2017). While material incentives for coordination or reliance on social information for uncertainty resolution can explain some instances of social conformity (Katz and Shapiro, 1986; Banerjee, 1992; Foster and Rosenzweig, 1995), researchers have documented myriad examples of social influence that are not predicted by neo-classical theories (Frey and Meier, 2004; Falk and Ichino, 2006; Goette, Huffman and Meier, 2006; Alpizar, Carlsson and Johansson-Stenman, 2008; Mas and Moretti, 2009; Shang and Croson, 2009; Chen et al., 2010).

A common explanation for social conformity, absent material incentives, is adherence to social norms. Such theories are typically driven by imposing sanctions on deviating from a predetermined action (Akerlof, 1980; Jones, 1984; Akerlof and Kranton, 2000) or by individuals pooling at a common action to signal an optimal social type (Bernheim, 1994).[1] In this project we

[1] Bernheim and Exley (2015) explore an alternative explanation for social conformity that establishes preference

propose and experimentally test a theory of (partial) social conformity in the absence of a socially optimal action. Importantly, this theory produces novel predictions on how decision makers differentiate between peer behavior that is relevant or irrelevant to their own decisions. Further, we introduce a theoretical foundation and experimental test for self image as a mechanism underlying peer effects.

Our theory posits that individuals have an intrinsic desire to judge and evaluate themselves. That is, each person would like to perceive himself positively – e.g. diligent, intelligent, charitable, et cetera. However, individuals often lack a direct or objective means of self evaluation. Instead, they rely on their history of actions as a noisy signal of their attributes. This theory requires no predetermined socially optimal action. Rather, self-image is increasing in the performance of a costly action. Under these assumptions an individual faces intrinsic incentives to manipulate his personal image through his actions. Further, signal extraction is improved when he observes others performing similar tasks – i.e. social comparisons allow him to better understand the image implications of his own choices.

In the context of this signal extraction, we prove that if self image is decreasing in the observation of more costly peer-choice, then diminishing marginal utility over self image will produce positive peer effects. The intuition behind this result is simple. Suppose a decision maker trades marginal task cost for marginal image utility. If self image is decreasing in the costliness of peer choice, concavity of image utility implies that an individual will experience higher marginal utility for any fixed choice when he observes more costly peer choices. Therefore, a decision maker will choose a more costly action in response to more costly peer choice.[2] For example, consider a group of employees working on an unfamiliar task under a fixed-wage spot contract that offers no incentives for collusion on effort[3]. Why would an employee condition his own output on the observed output of a peer? Suppose that each employee wishes to perceive himself

mechanisms that drive instances of social conformity. Our project explores a belief mechanism.

[2]The intuition behind this result is shown graphically in section 2.1. See proposition 1 for the full signal extraction result.

[3]Previous experiments have documented peer effects under such conditions (Falk and Ichino, 2006)

as hard-working but is unsure how to judge his performance due to lack of experience. If each employee draws comparisons with peer output to better understand whether his output suggests diligence or laziness, then our theory establishes that a group of employees will conform in output when each individual is more afraid to learn that he is the laziest group member than he is eager to demonstrate that he is the most diligent.

Modeling social comparisons as self signaling also offers novel predictions on the determination of peer groups. Differential responsiveness to peer behavior is mediated by how informative observed behavior is to one's self image. The signal a peer's choice provides is refined in the similarity of a task's incentives and costs. Dissimilarity in peer environments acts as signal interference and diminishes behavioral convergence. For example, suppose an individual observes that his coworkers are donating some proportion of their holiday bonuses to charity. Then he will feel less compelled to donate to charity himself if he suspects that he received a smaller bonus than most of his colleagues.

The experiments in this project test our theory's predictions on self signaling produced conformity and peer-group formation. The first experiment tests a prediction of our model that is uniquely consistent with self-image signal extraction. In this experiment we test whether individuals are more willing to do a costly task in exchange for charitable donations if they anticipate learning how their decisions compare to the decisions of previous participants. Importantly, all participants commit to how many tasks they are willing to complete prior to learning any information on how their decisions compare to the decisions of others. Furthermore, both those who will and will not be shown the distribution of others' decisions must predict where their decision lies in the distribution of all previous choices. By drawing attention to the full set of information that the experimenter has in both treatment and baseline we ensure that any treatment effect is driven not by experimenter demand but by the anticipation of learning how one's decision compares to the decisions of others. In accordance with our theory's predictions we find that participants are willing to do more tasks when they know they will learn the distribution of previous participant choices.

The second experiment tests whether a noisier signal mitigates peer effects by statistically garbling the information peer behavior provides. Documenting that decision makers are less responsive to statistically garbled signals is an important result of this project. Our theory relies on individuals using peer behavior as part of a signal extraction to better understand what their own behavior says about them. Further, it posits that dissimilarity in the incentives and costs that decision makers face will cloud what one decision maker can learn from an other's choices – thereby reducing conformity in behavior. However, for this theory to be plausible the basic dynamics of signal extraction must hold. Our second experiment demonstrates that experiment participants are highly sensitive to the receipt of a statistically refined signal and unresponsive to a fully garbled signal. This presents a natural test for the signal extraction hypothesis, and our results lay the foundation for further investigation of what environmental factors produce signal interference in more natural decision making settings.

The economic scope of peer influence is vast. Previous work has documented that people's uptake of retirement savings programs, effort at work, decisions to invest in financial instruments, and participation in paternal leave are causally related to the observed decisions of their peers (Duflo and Saez, 2002; Mas and Moretti, 2009; Bandiera, Barankay and Rasul, 2010; Bursztyn et al., 2014; Dahl, Løken and Mogstad, 2014), and that students' academic achievements covary with those whom they share a classroom or dormitory (Hoxby, 2000; Sacerdote, 2001; Zimmerman, 2003). A careful investigation of how self-signaling and social comparisons may drive social conformity could not only contribute to our understanding of the existing empirical and theoretical literature, but also provide researchers and policy makers with greater predictive power over when peer effects are likely to exist in unexplored environments.

Section 2.1 presents a simplified version of our model that provides the formal intuition behind our results. Section 2.2 details our full model and endogenously derives all of the our results as part of a signal extraction problem. Section 2.3 details our experimental design, and section 2.4 discusses the results of our experiment. Finally, 2.5 concludes and discusses possible future work on this topic.

2.1 A Simplified Approach

Modeling social comparisons as self signaling offers three principal theoretical contributions. In this section, we will use a simple model to demonstrate the formal intuition behind each contribution. The simple model presented in this section exogenously assumes its results. In the next section, we present the full model that endogenously derives all dynamics presented in this section from a signal extraction problem.

The first result of our theory states that if an individual experiences positive image returns in the performance of a costly action and observing higher peer performance depresses self-image, then diminishing marginal utility over self image is a sufficient condition to generate convergence in group behavior. To see this, suppose that an individual 1 takes a costly action a_1 and observes the costly action of individual 2, a_2. Define an image function $I_1(a_1,a_2)$ for individual 1 such that I_1 is increasing in a_1 and decreasing in $a_2 - \frac{\partial I_1}{\partial a_1} > 0 > \frac{\partial I_1}{\partial a_2}$. Let person 1 experience utility U over image I_1. Assume that $U' > 0$ and $U'' < 0$. Lastly, let action a_1 and action a_2 incur costs $C_1(a_1)$ and $C_2(a_2)$ where we assume $C' > 0$ and $C'' > 0$. Then agent 1 maximizes $U(I_1(a_1,a_2)) - C_1(a_1)$ with first order condition $U'(I_1(a_1,a_2))\frac{\partial I_1}{\partial a_1} = C_1'(a_1)$.

The intuition behind our first result is shown in shown in figure 2.1. Let $a_2^* > a_2$, and, for simplicity, assume that $\frac{\partial^2 I_1}{\partial a_1 \partial a_2} = \frac{\partial^2 I_1}{\partial a_1^2} = 0$. Under these conditions we have that $U'(I_1(a_1,a_2^*))\frac{\partial I_1}{\partial a_1} > U'(I_1(a_1,a_2))\frac{\partial I_1}{\partial a_1}$ for all a_1 due to the strict concavity of U. Therefore, since the cost function $C(a_1)$ is not dependent on a_2, we have that $argmax_{a_1} U(I_1(a_1,a_2^*)) - C_1(a_1) > argmax_{a_1} U(I_1(a_1,a_2)) - C_1(a_1)$. In other words, person 1 chooses more (less) costly a_1 in response to a more (less) costly choice of a_2 because the marginal image utility at $I_1(a_1,a_2^*)$ is higher than the marginal image utility at $I_1(a_1,a_2)$.

The second theoretical contribution of our model states that responsiveness to peer behavior is mediated by how informative an other's behavior is to one's self image. In our full model, the signal a peer's choice provides is refined in the similarity of incentives and costs that decision makers face. This can be thought of simply by considering two cost functions for agent

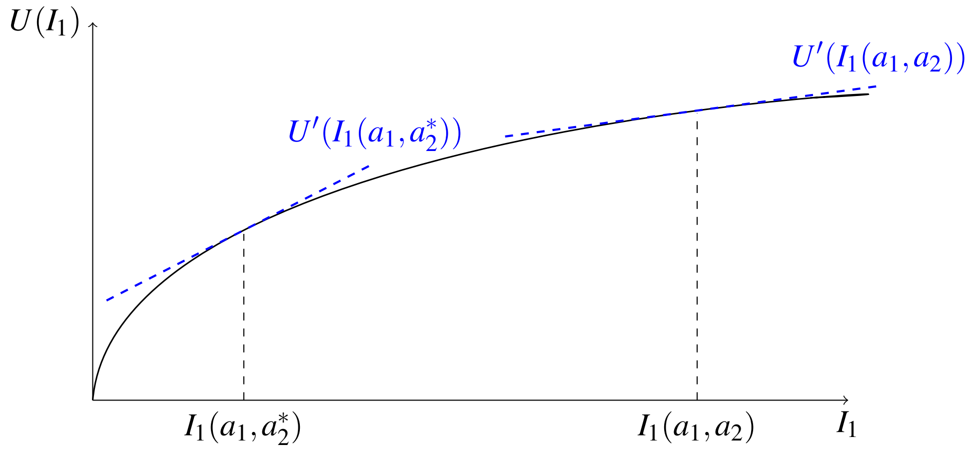


Figure 2.1: Concavity Induced Peer Effects

Notes: Here I_1 is the image obtained by person 1 after having performed action a_1 and having observed action a_2. U_1 is the utility that person 1 experiences over I_1. We assume that $a_2^* > a_2$.

2 – $C_2(a_2)$ and $\tilde{C}_2(a_2)$ – and two corresponding image functions for agent 1 – $I_1(a_1,a_2)$ and $\tilde{I}_1(a_1,a_2)$. Suppose that $\frac{\partial C_1}{\partial a}(a) = \frac{\partial C}{\partial a}(a) + \varepsilon_1$, $\frac{\partial C_2}{\partial a}(a) = \frac{\partial C}{\partial a}(a) + \varepsilon_2$, and $\frac{\partial \tilde{C}_2}{\partial a}(a) = \frac{\partial C}{\partial a}(a) + \tilde{\varepsilon}_2$, and that $cov(\varepsilon_1,\varepsilon_2) > cov(\varepsilon_1,\tilde{\varepsilon}_2)$. Under such conditions, the full model will derive a signaling equilibrium wherein $\frac{\partial I_1}{\partial a_2} < \frac{\partial \tilde{I}_1}{\partial a_2} < 0$. That is, a_2 is more informative to agent 1's image I_1 when the two decision makers' marginal costs are known to be more correlated.

The intuition behind why a more informative signal elicits a stronger behavioral response is illustrated in figure 2.2. Recall that decision maker 1 optimizes a_1 according to the first order condition $U'(I_1(a_1,a_2))\frac{\partial I_1}{\partial a_1} = C_1'(a_1)$. For simplicity, assume that $\frac{\partial I_1}{\partial a_1} = \frac{\partial \tilde{I}_1}{\partial a_1}$ for all (a_1,a_2) and that for some fixed $\hat{a}_2$, $I_1(a_1,\hat{a}_2) = \tilde{I}_1(a_1,\hat{a}_2)$.[4] As we can see from figure 2.2, since $U'' < 0$ and $\frac{\partial \tilde{I}_1}{\partial a_2} < \frac{\partial I_1}{\partial a_2}$, $U'(I_1(a_1,a_2^*)) > U'(\tilde{I}_1(a_1,a_2^*))$ for all $a_2^* > \hat{a}_2$. Therefore, since the cost function $C(a_1)$ is not dependent on a_2, we have that $argmax_{a_1} U(I_1(a_1,a_2^*)) - C_1(a_1) - \big(argmax_{a_1} U(I_1(a_1,\hat{a}_2)) - C_1(a_1)\big) > argmax_{a_1} U(\tilde{I}_1(a_1,a_2^*)) - C_1(a_1) - \big(argmax_{a_1} U(\tilde{I}_1(a_1,\hat{a}_2)) - C_1(a_1)\big)$. In other words, increases in a_2 depress I_1 more than $\tilde{I}_1$. Therefore, the increase in the marginal utility of image is greater

[4]We also again assume that $\frac{\partial^2 I_1}{\partial a_1 \partial a_2} = \frac{\partial^2 I_1}{\partial a_1^2} = 0$.

under information regime I_1 than $\tilde{I}_1$, and agent 1 responds more strongly to changes in a_2 under information regime I_1 than $\tilde{I}_1$.

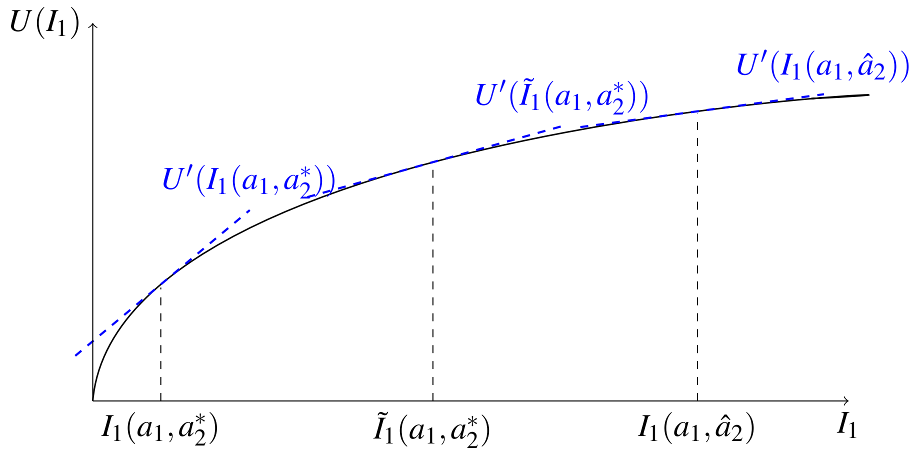


Figure 2.2: Differential Peer Response

Notes: Here I_1 is the image obtained by person 1 after having performed action a_1 and having observed action a_2 where a_2 incurs cost $C_2(a_2)$, and $\tilde{I}_1$ is the image obtained by person 1 having observed action a_2 where a_2 incurs cost $\tilde{C}_2(a_2)$.

Lastly, the predictions of our model allow us to empirically disentangle self-signaling from social signaling and experimenter demand. That is, our model predicts that self signaling will induce decision makers to choose more costly actions in anticipation of learning where their choice lies in the distribution of others' choices. This prediction crucially relies on the assumption that utility over self image is strictly increasing and diminishingly concave – i.e. $U' > 0$, $U'' < 0$ and $U''' > 0$.

The mechanism behind this result is shown in figure 2.3. Assume there are only two decision makers. Let $I_1(a_1|a_1 < a_2)$ be agent 1's image if he knows $a_1 < a_2$. Similarly, let $I_1(a_1|a_1 > a_2)$ be agent 1's image if he knows that agent 2 chose a less costly action. Further, assume that $I(a_1|a_1 < a_2) < I(a_1) < I(a_1|a_1 > a_2)$. Then define the optimization problem in which agent 1 anticipates learning whether $a_1 \lessgtr a_2$, but only after he makes his decision, as

$$max_{a_1} Pr(a_1 < a_2|a_1)U\left(I(a_1|a_1 < a_2)\right) + (1 - Pr(a_1 < a_2|a_1))U\left(I(a_1|a_2 > a_1)\right) - C(a_1)$$

Now let $Pr(a_1 < a_2|a_1)I(a_1|a_1 < a_2) + (1 - Pr(a_1 < a_2|a_1))I(a_1|a_2 > a_1) = I(a_1)$ for all a_1. Since we assume that $U''' > 0$, by Jensen's inequality we have that $Pr(a_1 < a_2|a_1)U'(I(a_1|a_1 < a_2)) + (1 - Pr(a_1 < a_2|a_1))U'(I(a_1|a_2 > a_1)) > U'(I(a_1))$ for all a_1.

As such,

$$\begin{aligned} argmax_{a_1} Pr(a_1 < a_2|a_1)U(I(a_1|a_1 < a_2)) + (1 - Pr(a_1 < a_2|a_1))U(I(a_1|a_2 > a_1)) - C(a_1) \\ > argmax_{a_1} U(I(a_1)) - C(a_1) \end{aligned}$$

That is, the decision maker will choose a more costly task when he knows that he will learn how his choice compares to the choices of others than when he is to remain ignorant to others choices. Intuitively, this occurs because convexity in marginal image utility implies that the potential opportunity cost of choosing too low of an action when $a_1 < a_2$ outweighs the opportunity cost of choosing too high of an action when $a_1 > a_2$.

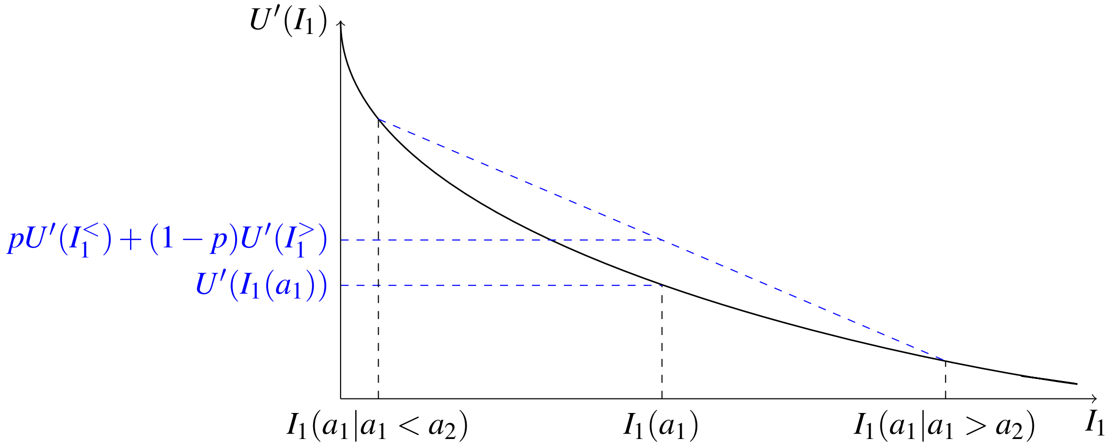


Figure 2.3: Social Information Anticipation

Notes: Here $U'(I_1^<) = U'(I_1(a_1|a_1 < a_2))$, $U'(I_1^>) = U'(I_1(a_1|a_1 < a_2))$, and $p = Pr(a_1 < a_2|a_1)$.

2.2 Social Comparisons in Peer Effects

2.2.1 Preliminaries

Building on Bénabou and Tirole (2006) suppose that agent 1 maximizes:

$$max_{a_1} \phi U(a_1) - C(a_1, \omega_1 - \theta_1) \tag{2.1}$$

Where $a_1 \in A$ is person 1's observable action, θ_1 is agent 1's level of ability and ω_1 represents the difficulty of the task. We assume $C(a_1, \omega_1 - \theta_1)$ is the "cost" of effort,[5] θ and ω are random variables, and ϕ is a known constant. The utility one holds over beliefs about his own ability is captured in $\phi U(a_1)$. Where

$$U(a_1) = V(E[\theta_1|a_1])$$

and V is a strictly increasing function.

[5]For notational simplicity we define the cost function $C(a, \omega - \theta)$ to be the negative net utility, apart from image returns, of taking action a under parameters ω and θ. As such, we can more generally interpret ω_1 and θ_1 as any environmental factor and any personal characteristic that mediate direct utility.

The interpretation of $C(a_1, \omega_1 - \theta_1)$ is straightforward; given a_1, the agent experiences task completion cost $C(a_1, \omega_1 - \theta_1)$. Understanding $U(a_1)$ is more nuanced. Generally speaking $\phi U(a_1)$ is the image utility function. When the decision maker is being observed by others U may serve as both a self and social image function. We assume that a decision maker observes a_1. Since U and C must be known functions for optimization, the decision maker will implicitly observe $\omega_1 - \theta_1$. However, we assume that he does not observe ω_1 independently of θ_1. Additionally, as per Bénabou and Tirole (2006), we must also assume that the decision maker suffers imperfect recall. That is, the decision maker may be able to remember the action – a_1 – he took, but will not be able to recall intangible influences.[6] In this interpretation we can think of U as a social image function and the future self as a third party observer. Therefore, the current self optimizes equation 2.1 with respect to the future self's inferred expectation of θ_1 given the observed a_1.

Next, we introduce the observation of a second agent's action – $a_2^* = argmax_{a_2} \phi U(a_2) - C(a_2, \omega_2 - \theta_2)$ – into agent 1's decision problem.[7]

$$max_{a_1} \phi U(a_1, a_2^*) - C(a_1, \omega_1 - \theta_1) \tag{2.2}$$

This model of image utility makes endogenous the desire to condition one's own decision on the decisions of others. To see this consider the first order condition of the above optimization problem:

$$\phi \frac{\partial U}{\partial a_1}(a_1, a_2) = \frac{\partial C}{\partial a_1}(a_1, \omega_1 - \theta_1) \tag{2.3}$$

From the decision maker's (or an observer's) perspective $\phi \frac{\partial U(a_1, a_2)}{\partial a_1}$ is a known function. However, this function must be consistent with the signal extraction of $E[\theta_1 | a_1, a_2]$. In other words, U is endogenously defined to satisfy equation 2.3. As such, person 1 will condition a_1 on person 2's

[6]Otherwise the decision maker could simply recall the marginal cost of doing one task.
[7]For simplicity, we will assume that agent 2 does not observe a_1.

decision a_2 only if V, C, and the distributions of (θ_1, θ_2) and (ω_1, ω_2) result in a signal extraction of $E[\theta_1|a_1, a_2]$ and image utility function, $U(a_1, a_2) = V(E[\theta_1|a_1, a_2])$, that predicts optimal a_1 changes with observed a_2^*. Note that this utility function does not adhere to an exogenously defined optimal action or social type. Rather, we will derive conditions on the strictly increasing image utility and cost functions, V and C, and the joint distribution of (ω_1, ω_2) that predict convergent behavior.

2.2.2 The Coefficient of Peer Effects

We first consider the problem of a single decision maker who does not observe other's choices. Recall that this agent solves equation 2.2 with first order equation 2.3. Rewrite the first order equation as $F(X,Y) = 0$ where $X = a_1$ and $Y = (\theta_1, \omega_1)$. Further, define $G(Y)$ such that $F(G(Y),Y) = 0$ for all Y. Then by the implicit function theorem, we know that $G'(Y) = -A_x^{-1}A_y$ where $A_x = F_X(X,Y)$ and $A_y = F_Y(X,Y)$. From equation 2.3 we get $A_x = \left[\frac{\partial F}{\partial a_1}\right]$ and $A_y = \left[\frac{\partial F}{\partial \theta_1}\ \frac{\partial F}{\partial \omega_1}\right]$. Notice that, by definition, $G(Y)$ is the optimal action, a_1^*, given a realization of (θ_1, ω_1). As such we now know that:

$$\frac{\partial a_1^*}{\partial \theta_1} = -\left(\frac{\partial F}{\partial a_1}\right)^{-1} \frac{\partial F}{\partial \theta_1} = \\ -\left(\phi V''(E[\theta_1|a_1]) \frac{\partial E[\theta_1|a_1]}{\partial a_1} - \frac{\partial^2 C}{\partial a_1^2}(a_1, \omega_1 - \theta_1)\right)^{-1} \frac{\partial^2 C}{\partial a_1 \partial(\omega_1 - \theta_1)}(a_1, \omega_1 - \theta_1) \quad (2.4)$$

$$\frac{\partial a_1^*}{\partial \omega} = -\left(\frac{\partial F}{\partial a_1}\right)^{-1} \frac{\partial F}{\partial \omega_1} = \\ \left(\phi V''(E[\theta_1|a_1]) \frac{\partial E[\theta_1|a_1]}{\partial a_1} - \frac{\partial^2 C}{\partial a_1^2}(a_1, \omega_1 - \theta_1)\right)^{-1} \frac{\partial^2 C}{\partial a_1 \partial(\omega_1 - \theta_1)}(a_1, \omega_1 - \theta_1) \quad (2.5)$$

We interpret these equations with lemma 1.

Lemma 1 *Suppose that agent 1 solves for:*

$$a_1^* = argmax_{a_1} \phi U(a_1) - C(a_1, \omega_1 - \theta_1)$$

Which has first order condition:

$$\phi \frac{\partial U}{\partial a_1}(a_1) - \frac{\partial C}{\partial a_1}(a_1, \omega_1 - \theta_1) = 0$$

If $\frac{\partial^2 C}{\partial a_1 \partial(\omega - \theta_1)}(a_1, \omega_1 - \theta_1) > 0$ *and* $\frac{\partial^2 C}{\partial a_1^2}(a_1, \omega - \theta_1) \geq 0$, *then* $\frac{\partial a_1^*}{\partial \theta_1} = -\frac{\partial a_1^*}{\partial \omega_1} > 0$.[8]

Lemma 1 simply states that if the marginal cost of a_1 is non-decreasing in a_1 and increasing in the difference between task difficulty and agent 1's ability, $\omega_1 - \theta_1$, then the optimal choice of a_1 will be increasing in agent 1's ability and decreasing in task difficulty. This lemma establishes the basic dynamics of choice on personal ability and task difficulty.

We now move to the case where agent 1 observes the actions of all other agents $i \in \{2, \ldots, N\}$. For simplicity we assume that all agents $i > 1$ have already made their decisions and cannot observe a_1. Now there are N equations to solve:[9]

$$max_{a_1} \phi U(a_2) - C(a_2, \omega_2 - \theta_2)$$

$$\vdots$$

$$max_{a_N} \phi U(a_N) - C(a_N, \omega_N - \theta_N)$$

$$max_{a_1} \phi U(a_1, a_2, \ldots, a_N) - C(a_1, \omega_1 - \theta_1)$$

[8] This of course assumes that $\frac{\partial E[\theta_1|a_1]}{\partial a_1} > 0$. This is established in the theoretical appendix.

[9] In this case we assume that the first N movers ignore the consequences of their actions on the second mover's action since they will not observe the second mover's action.

This results in the first order conditions:

$$\phi \frac{\partial U}{\partial a_2}(a_2) - \frac{\partial C}{\partial a_2}(a_1, \omega_1 - \theta_1) = 0$$

$$\vdots$$

$$\phi \frac{\partial U}{\partial a_N}(a_N) - \frac{\partial C}{\partial a_N}(a_N, \omega_N - \theta_N) = 0$$

$$\phi \frac{\partial U}{\partial a_1}(a_1, a_2, \ldots, a_N) - \frac{\partial C}{\partial a_1}(a_1, \omega_1 - \theta_1) = 0$$

Applying the implicit function theorem to this system of equations yields proposition 1.[10]

Proposition 1 *Define* $a_1^* = argmax_{a_1} \phi U(a_1, a_2, \ldots, a_N) - C(a_1, \omega_1 - \theta_1)$. *Then, for all* $i \in \{2, \ldots, N\}$

$$\frac{\partial a_1^*}{\partial a_i} = \frac{-\phi V''\left(E[\theta_1|a_1, a_i]\right) \frac{\partial E[\theta_1|a_1, a_i]}{\partial a_i}}{\phi V''\left(E[\theta_1|a_1, a_i]\right) \frac{\partial E[\theta_1|a_1, a_i]}{\partial a_1} - \frac{\partial^2 C}{\partial a_1^2}(a_1, \omega_1 - \theta_1)}$$

Proposition 1 states how individuals condition their action on the observed action of an other person.[11] That is, if $\frac{\partial E[\theta_1|a_1,a_i]}{\partial a_1} > 0$, $\frac{\partial E[\theta_1|a_1,a_i]}{\partial a_i} < 0$, $V''\left(E[\theta_1|a_1, a_i]\right) < 0$, and $\frac{\partial^2 C}{\partial a_1^2}(a_1, \omega - \theta_1) \geq 0$, then $\frac{\partial a_1^*}{\partial a_i} > 0$. Further, if $\frac{\partial E[\theta_1|a_1,a_i]}{\partial a_1} \geq -\frac{\partial E[\theta_1|a_1,a_i]}{\partial a_i}$, then $\frac{\partial a_1^*}{\partial a_i} \leq 1$. In other words, if the image utility function, V, is concave and the expectation of one's own θ is increasing in one's own action and decreasing in an other's action, then we will observe positive peer effects. Under these conditions the degree of conformity is mediated by the convexity of the cost function.

Proposition 1 tells us that, given two observed choices a_i and a_j, $\frac{\partial a_1^*}{\partial a_i} > \frac{\partial a_1^*}{\partial a_j}$ if $\frac{\partial E[\theta_1|a_1,a_2,\ldots,a_N]}{\partial a_i} < \frac{\partial E[\theta_1|a_1,a_2,\ldots,a_N]}{\partial a_j}$. In other words, agent 1 will best most responsive to the

[10]See theoretical appendix.

[11]Interestingly, the expression is invariant across unilateral and bilateral observation. That is, ignoring reflection effects, proposition 1 describes how a_1^* responds to a_2 when person 1 observes a_2 and person 2 *does not* observe a_1 and when person 1 observes a_2 and person 2 *does* observe a_1

choices of those whose decisions most impact his personal image. This leads to our group-formation result:

Corollary 1 *Suppose that θ_i is statistically independent of θ_j for all $i \neq j$, and $(i, j) \in \{1,\ldots,N\} \times \{1,\ldots,N\}$. Further, let $(\omega_1,\ldots,\omega_N) \sim F_\omega(\omega_1,\ldots,\omega_N)$. If ω_1 is more positive quadrant dependent with ω_i than with ω_j, then $\frac{\partial E[\theta_1|a_1,a_2,\ldots,a_N]}{\partial a_i} < \frac{\partial E[\theta_1|a_1,a_2,\ldots,a_N]}{\partial a_j} \implies \frac{\partial a_1^*}{\partial a_i} > \frac{\partial a_1^*}{\partial a_j}$.*

That is, social influence is increasing in the pairwise dependence of each individual's task "costliness".[12] Intuitively, this states that as the similarity between people's decision environments decreases, the signal that an other's action provides over one's own image-parameter, θ, becomes noisier. Therefore, one is less responsive to the actions of the other person.

2.2.3 Anticipation of Social Information

Our final result establishes the anticipation effect of self signaling. We change notation for this analysis. Let a_s be one's chosen action and let $n \leq N$ be the rank of one's action out of a population of N persons who each choose their own action a. That is, a_s is of rank n if and only if $n-1$ persons chose an action smaller than a_s and $N-n-1$ persons chose an action higher than a_s. For simplicity, we assume that equality with a_s is not possible.

Now suppose that the decision maker knows that we will not learn his rank n. Then even if he considers what his expected rank n is, he will choose a_s^* according to the usual first order condition.

$$max_{a_s} \phi U \left(E\left[E\left[\theta_s|a_s, E[n|a_s]\right]|a_s\right]\right) - C(a_s, \omega - \theta_s)$$
$$= max_{a_s} \phi U \left(E\left[\theta_s|a_s\right]\right) - C(a_s, \omega - \theta_s)$$

[12]Recall from footnote 5 that we define the cost function $C(a, \omega - \theta)$ to be the negative net utility, apart from image returns, of taking action a under parameters ω and θ. As such, we can more generally interpret ω as any environmental factor that mediates direct utility.

With first order condition

$$V'\left(E\left[\theta_s|a_s\right]\right)\frac{\partial E\left[\theta_s|a_s\right]}{\partial a_s}=\frac{\partial C}{\partial a_s}(a_s,\omega-\theta_s) \tag{2.6}$$

That is, he optimizes over $E\left[E\left[\theta_s|a_s,\sum_{n=1}^{N}P_n(a_s)*n\right]|a_s\right]=E\left[\theta_s|a_s\right]$ since a_s is the only information he will obtain. However, if he knows that we will learn his true relative rank, he solves

$$max_{a_s}\sum_{n=1}^{N}P_n(a_s)*V\left(E\left[\theta_s|a_s,n\right]\right)-C(a_s,\omega-\theta_s)$$

.

With first order condition

$$\sum_{n=1}^{N}P_n(a_s)*V'\left(E\left[\theta_s|a_s,n\right]\right)\frac{\partial E\left[\theta_s|a_s,n\right]}{\partial a_s}+\sum_{n=1}^{N}P_n'(a_s)V\left(E\left[\theta_s|a_s,n\right]\right)=\frac{\partial C}{\partial a_s}(a_s,\omega-\theta_s) \tag{2.7}$$

We prove that the left hand side of equation 2.7 is larger than the left hand side of equation 2.6 for all a_s in the theoretical appendix. That is, we show that:

$$\sum_{n=1}^{N}P_n(a_s)*V'\left(E\left[\theta_s|a_s,n\right]\right)\frac{\partial E\left[\theta_s|a_s,n\right]}{\partial a_s}+\sum_{n=1}^{N}P_n'(a_s)V\left(E\left[\theta_s|a_s,n\right]\right)>'\left(E\left[\theta_s|a_s\right]\right)\frac{\partial E\left[\theta_s|a_s\right]}{\partial a_s} \tag{2.8}$$

As such, since the cost function is not dependent on the relative rank of a_s, we know that a_s^* increases in anticipation of a decision maker learning his place in the distribution of other's choices.

2.3 Experimental Design

We conducted two experiments to directly test the predictions of our theory. The first experiment tests the relevance of self image in social comparisons, and the second experiment tests the relevance of signal extraction. In both experiments participants chose the maximum number of real-effort tasks they were willing to complete in exchange for a donation to charity. All experiment sessions were conducted at the University of California, San Diego Department of Economics. Subjects completed the experiment on individual computer terminals. Privacy screens were installed on each computer and barriers were placed between every subject. All participants were current undergraduates at U.C.S.D. Each subject was paid $15 dollars for their participation, and experiment sessions took approximately 50 minutes. Treatments were varied across sessions for a between-subjects design. Recruitment for each session was done by emailing a random sample of U.C.S.D. undergraduates. The first experiment contained nine to fourteen subjects per session, and the second experiment contained eight to twelve subjects per session.

Section 2.3.1 explains the real-effort task choice. Section 2.3.2 details the self-image experiment, and section 2.3.3 describes the test for signal extraction.

2.3.1 The Task Choice

In both experiments, participants were asked to select the maximum number of real-effort tasks they were willing to complete in exchange for donations to the Afghan Dental Relief Project. The A.D.R.P. is a charitable organization that provides free dental services and dental health education to the poorest families and individuals in Kabul, Afghanistan. 100% of the donations that were generated from the experiment were used to purchase dental supplies for A.D.R.P.'s free dental clinic. We collaborated with the A.D.R.P. because it is a deserving and relatively unknown charity. The founder, Dr. James Rolfe, agreed to monitor gifts made to the charity during the course of our experiment. Dr. Rolfe reports that no gifts were made to the

A.D.R.P. that could have been given by an U.C.S.D. undergraduate.[13]

The real-effort task was to transcribe captchas. Captchas are distorted images of a sequence of letters commonly used by website developers to distinguish between human users and bots. The task was deliberately made to be more frustrating and tedious than typical website captchas.[14] Our captchas consisted of capital and lowercase letters, numbers, and special characters. Only correctly transcribed captchas were considered completed tasks. Subjects were given three chances per randomly assigned captcha and they could not skip an assigned captcha except by deliberately entering three incorrect responses.[15] The typical participant was able to correctly transcribe 1-out-of-every-5 captchas. An example of the task is show in figure 2.4.[16]

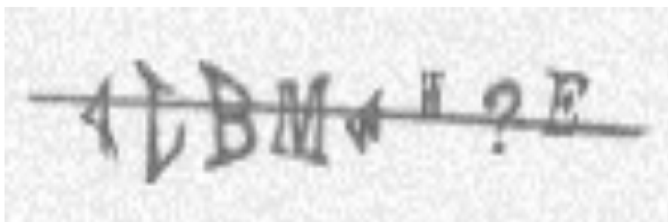


Figure 2.4: Example Task

In the self-image experiment subjects selected the maximum number of tasks they were willing to complete in exchange for donations of $2, $5, $10, $15, and $20. Each subject chose between 0 and 50 tasks for each possible donation amount. We ensured the incentive compatibility of this decision by randomly assigning each subject a donation and task amount after they had completed making their decisions. Each subject drew a ball out of an jar with a donation amount and corresponding 3-digit code. They then entered their codes into their computer terminals to confirm their assigned donations. A random number generator then assigned each subject a number of tasks (between 0 and 50). Both randomization devices were

[13]Specifically, all donations made to the A.D.R.P. during the course of our experiment came from us, the experimenters, or from members of Dr. Rolfe's local Santa Barbara community.

[14]Captchas were generated using the Python module `Claptcha`, available here: https://github.com/kuszaj/claptcha. All use of this module is in accordance with the license outlined at the previous link. The font used for captchas was "Mom's Typewriter," an open-source font available here: https://www.dafont.com/moms-typewriter.font.

[15]All captchas were randomly assigned out of a bank of 1000 captcha images.

[16]The correct transcription of the shown captcha is "4LBMwW?E"

fair – every donation and task combination had an equal probability of being assigned. If the assigned number of tasks was equal to or fewer than the maximum number they were willing to do for their assigned donation, they completed their assigned number of tasks and we gave the assigned donation to the A.D.R.P. For example, suppose that for a donation of $15 a participant chose to do at most 30 tasks. If he were randomly assigned $15 and 20 tasks, then he completed 20 tasks and $15 was given to the Afghan Dental Relief Project. On the other hand, if he was randomly assigned $15 and 40 tasks, then he did 0 tasks and no donation was given to the A.D.R.P. Subjects were offered the same choice in the signal-extraction experiment. However, only a donation of $20 was possible – therefore subjects in the signal extraction experiment only made one decision.

All experiment treatments followed a similar structure. Subjects were first introduced to the charity by reading a short news article. Second, all subjects completed sample captchas so that they were familiar with the task. Third, subjects received instruction and comprehension testing about the incentive compatible task choice. Lastly, subjects made their decisions under the conditions of the treatment they were assigned. In all treatments, subjects were free to leave as soon as they completed their assigned tasks. Importantly, individuals never observed the decisions of others in their own session, nor did they observe the randomly assigned donations or task amounts of other participants in their session.

2.3.2 Self Signalling and Social Comparisons

The first experiment tests our theory's prediction that individuals will choose more costly actions in anticipation of social information. In the context of our experiment this implies that individuals will be willing to complete more captchas when they will learn how their choice compares to the choices of previous participants. We use this prediction to identify the role of self image in social comparisons.

In both treatment and baseline, participants chose the maximum number of captchas they were willing to complete in exchange for donations of \$2, \$5, \$10, \$15, and \$20 – as described in section 2.3.1. In conjunction with their choices, subjects were also asked to predict the percent of all previous participants that were willing to complete more tasks than them for each of their decisions. This decision is depicted in figure 2.5. As figure 2.5 shows, subjects selected the maximum number of tasks they were willing to complete and guessed whether 95%, 75%, 50%, 25%, or 0% of all previous participants were willing to do more tasks them. For example, suppose that for a donation of \$10 dollars an individual chose to do a maximum of 12 tasks. Further suppose that this person thought that at least 50%, but fewer than 75%, of all previous participants were willing to do more than 12 tasks for a \$10 donation. This person was then instructed to indicate that he believes that at least 50% of all previous experiment participants were willing to do more tasks than him for a \$10 donation.

In treatment sessions, participants were given sealed envelopes that they opened after submitting their decisions and distributional guesses. These envelopes contained the true 5th, 25th, 50th, 25th, and 100th percentiles for task choice by donation amount. Treatment participants then submitted the correct answers for what percent of previous participants were willing to do more tasks than them for each possible donation.

While only treatment subjects ultimately learned the choices of previous participants, we deliberately asked all participants to consider how their choices compare to the choices of all previous participants. By doing so, we make subjects in both baseline and treatment cognizant of what the experimenter observes. Since the experimenter's information set is identical in treatment and baseline, any difference in subject behavior is solely due to subjects anticipating what they themselves will learn.[17][18]

[17]There is an second order equilibrium concern that we must address. Specifically, if all subjects in the baseline thought all previous participants were also in the baseline – and all treatment participants thought all previous participants were in the treatment – then subjects in the treatment may have had higher beliefs about what previous participants chose to do. However, we preclude this possibility by explaining to subjects in both treatments that 50% of all participants received social information and 50% did not.

[18]There is also a second order concern that potentially blurs the line between pure self signaling and experimenter driven social signaling. Specifically, while the experimenter's information set remains invariant between treatments, the subject's knowledge of the this information does not. In principle, differential behavior in treatment and baseline could arise from subjects' anticipation about what they will learn about what the the experimenter learns about

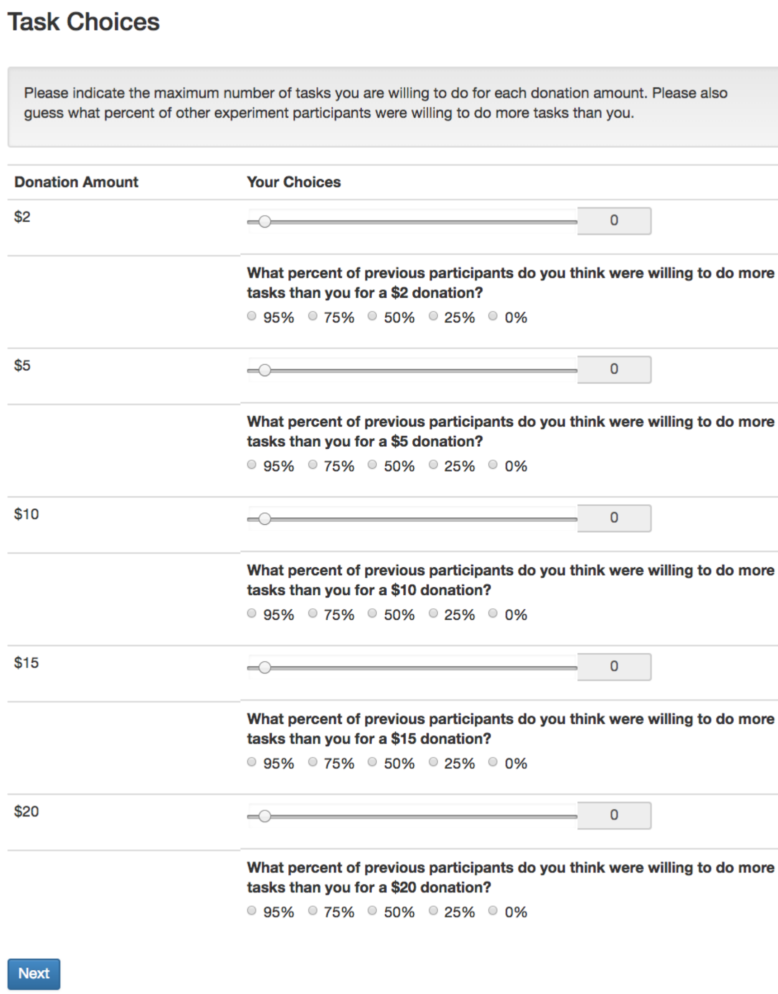


Figure 2.5: Decision Screen

Notes: This is the decision screen for participants in the self-image experiment.

2.3.3 Signal Extraction and Peer-Group Formation

All of the predicted dynamics of our proposed theory rely on signal extraction. Therefore, peer effects are predicted to arise only if group behavior provides a sufficiently refined signal. In the context of our experiment, our theory states that decision makers may refer to others' choices to better understand whether their own choices reveal selfishness or task difficulty. As such, we

them. We argue that this is still a matter of self image since any difference in subject choice could only arise out of a desire to manipulate what the subject knows about the experimenters perception of the subject. The manipulation of the experimenter's perception of the subject would only be incidental.

use statistically garbled and refined social information to directly test the relevance of signal extraction dynamics to our decision environment.

As described in section 2.3.1, participants in this experiment chose the maximum number of captchas they were willing to complete in exchange for a $20 donation to the A.D.R.P. Prior to their decision, each person was informed that they would receive one of the two following statements on the choices of all previous participants.

1. *More than* ***50%*** of all previous participants were willing to complete at least 20 tasks for a donation of $20.

2. *Less than* ***25%*** of all previous participants were willing to complete at least 20 tasks for a donation of $20.

Subjects received their signals by drawing one of sixteen available envelopes at random. 8-out-of-16 envelopes contained the true statement in the baseline, while 15-out-of-16 envelopes contained the true statement in the treatment.[19] After reading their signals and choosing how many tasks they were willing to complete, subjects also indicated which signal they believed to be true.[20]

Signal extraction has a specific hypothesis in this context. Task choice and beliefs should be highly dependent on the obtained signal in the treatment. However, in baseline, subjects should understand that variation in the obtained signal is pure noise. Therefore, task choice and beliefs should be independent of the obtained signal in baseline.

2.4 Results

Section 2.4.1 corroborates our theory's prediction that individuals will choose more costly actions to bolster self image in anticipation of social information. Section 2.4.2 supports

[19]The high signal – signal 1 – is the true signal.

[20]Subjects were not informed that they would be asked which signal they believed until after they made their decisions.

the relevance of image signal extraction in our experiment.

2.4.1 Self-Image Results

Figure 2.6 plots the average task choice by donation amount of those in the baseline and anticipation treatments. Those in the anticipation treatment were, on average, willing to complete more captchas than those in the baseline. We also observe that task choice is increasing in the donation amount,[21] and that the treatment effect increases in the donation amount.

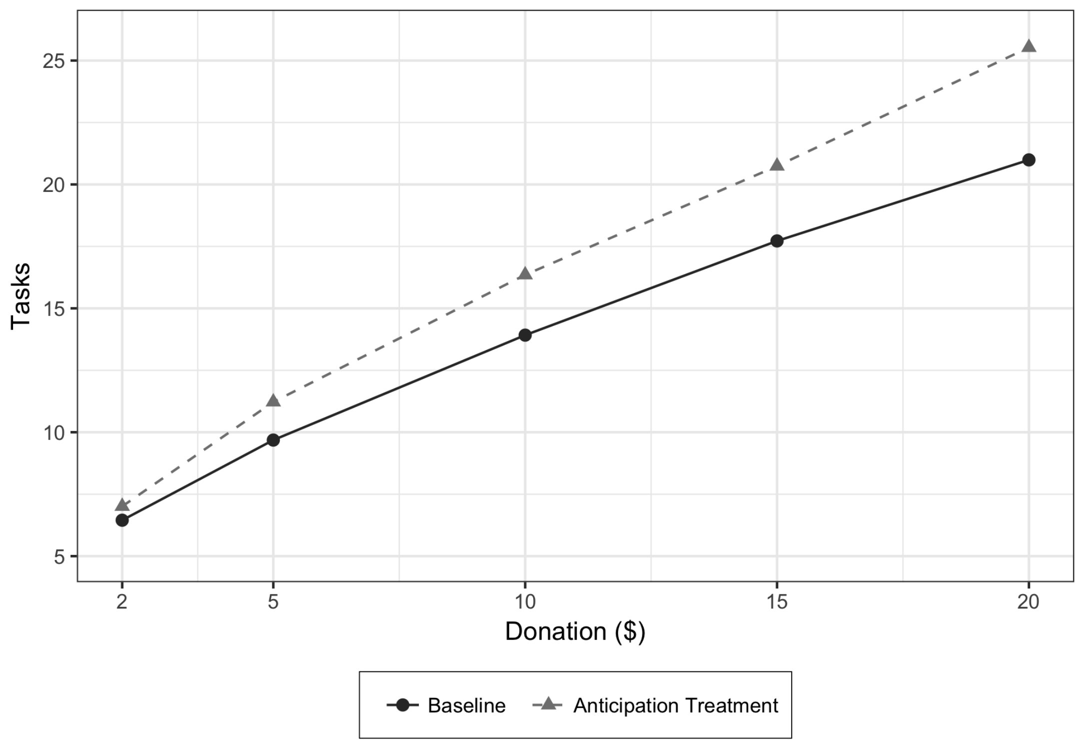


Figure 2.6: Mean Task Choice by Donation

Notes: This figure plots the average number of tasks subjects were willing to complete for each possible donation. In concordance with self signaling, subjects choose to do more tasks if they anticipate learning how their choices compare to the choices of previous participants. This effect is increasing in the value of the gift to charity.

[21] In fact, 94% of all participants displayed within-subject monotonicity in task choice across the donation amounts.

Table 2.1: Effect of Treatment on Tasks Allocation

	DV: Task Choice	
	(1)	(2)
Donation	0.903***	0.802***
	(0.045)	(0.064)
Treatment	2.417*	0.296
	(1.452)	(1.152)
Treat × Donation		0.204**
		(0.089)
Constant	4.367***	5.413***
	(0.928)	(0.817)
R-Squared	0.208	0.211
Subjects	219	219
Observations	1095	1095

Notes: This table quantifies the results shown in figure 2.6. Robust standard errors, clustered at the subject level, are presented in parentheses. There are 108 participants in the treatment and 111 in the baseline. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.1 presents these results via estimation of the following regressions:

$$y_{i,d} = \alpha_0 + \alpha_1 Donation_{i,d} + \alpha_2 Treat_i + \varepsilon_{i,d} \tag{2.9}$$

$$y_{i,d} = \beta_0 + \beta_1 Donation_{i,d} + \beta_2 Treat_i + \beta_3 Treat_i \times Donation_{i,d} + \varepsilon_{i,d} \tag{2.10}$$

where $y_{i,d}$ is the maximum number of tasks subject i was willing to complete for donation d. Regression 2.10 is more informative. From this regression we can see that for every additional \$5 donated to the A.D.R.P., baseline participants were willing to do an additional four tasks while treatment participants were willing to do an additional five tasks. This result, significant at the 5% level, is useful for two principle reasons. First, strict monotonicity in task performance over donation size demonstrates the costliness of captcha transcription. Secondly, we find no evidence of any fixed treatment effect – the estimation of β_2 is indistinguishable from 0. Rather, the anticipatory image effect is entirely tied to the size of the gift given to the A.D.R.P. For a near-meaningless gift of \$2, subjects are largely unconcerned with how their decisions compare to peer decisions. However, they choose to do 20% more tasks when the gift is \$15 or \$20. In other words, image concerns only exist when sufficient charitable stakes are attached to an otherwise vacuous task.[22]

Table 2.2 confirms that the linearly interacted regression is the correct specification. The corresponding regression for table 2.2 is:

$$y_{i,d} = \delta_0 + \sum_{d \in \{2,5,10,15,20\}} \delta_{1,d} 1_{i,donation=d} + \sum_{d \in \{2,5,10,15,20\}} \delta_{2,d} Treat_i \times 1_{i,donation=d} + \varepsilon_{i,d} \tag{2.11}$$

From this regression we can not only see that task choices and the treatment effect are increasing

[22]This finding also further corroborates our interpretation that the treatment effect is driven by self-image concerns. While no subjects learn the decisions, donation assignments or task allocations of others in their session, it is plausible that earlier experiment departures were taken as a signal of lower task choice. Of course, this feature is consistent across both treatments. Therefore, social image concerns could only enhance our treatment effect if subjects thought that learning the true distribution of previous choices would induce harsher judgment of early departure. However, because no one learned the donation assignment of others, such a hypothesis would be more consistent with the existence of a fixed treatment effect across all possible donations.

Table 2.2: Effect of Treatment on Tasks Allocation, by Donation

	DV: Tasks Choice
\$5	3.234***
	(0.472)
\$10	7.468***
	(0.741)
\$15	11.270***
	(0.957)
\$20	14.541***
	(1.185)
Treat x \$2	0.559
	(1.098)
Treat x \$5	1.538
	(1.293)
Treat x \$10	2.433
	(1.572)
Treat x \$15	3.020*
	(1.779)
Treat x \$20	4.537**
	(2.038)
Constant	6.450***
	(0.815)
R-Squared	0.212
Subjects	219
Observations	1095

Notes: This table individually presents the results of figure 2.6 for each donation amount. Robust standard errors, clustered at the subject level, presented in parentheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

in the donation, but also that a linear approximation of these effects fits well. For example, regression 2.10 predicts that the total average treatment effect for donations of \$15 and \$20 will be 3 and 4 tasks while the indicator regression finds the effects to be 3 and 4.5 tasks.

2.4.2 Signal Extraction Results

In accordance with our theory, we find that participants' choices are highly sensitive to the receipt of a statistically refined signal and unresponsive to a fully garbled signal. As seen in figure 2.7, subjects who received the true signal with 15-out-of-16 probability chose to do approximately ten more tasks if they drew the high signal – "More than 50% of all previous participants were willing to complete at least 20 tasks for a donation of \$20" – than if they recieved the low signal – "Less than 25% of all previous participants were willing to complete at least 20 tasks for a donation of \$20." However, subjects only chose a statistically insignificant two more tasks if they received the high signal in the baseline treatment where they received the true signal 8-out-of-16 times. Additionally, subjects who received the high signal in treatment also chose, on average, 5.5 more tasks than those who received the high signal in baseline, and an average of 5.8 more tasks than all subjects in the baseline. These results are quantified in tables 2.3 and 2.4.

Additionally, we find that 53% of individuals believe signals received in baseline, while 87% of individuals believe signals received in treatment.[23] Notably, individual beliefs are highly predictive of task choice. We find that those who believe the high signal is true are willing to do an average of 12 more tasks than those who believe the low signal. As table 2.5 shows, this result holds true controlling for signal and treatment.

Lastly, we examine the distributional shift in task choice from baseline to treatment. Figure 2.9 shows that the treatment effect is delivered by those who would have otherwise chosen to do fewer than 20 tasks in the baseline. This is reflected in tables 2.6 and 2.7. Table 2.6

[23] This difference is significant at the 1% level. See appendix table B.2. Beliefs by signal and treatment are shown in figure 2.8.

Table 2.3: Effect of Signal on Task Choice, by Treatment

	DV: Task Choice	
	(1) Baseline	(2) Treatment
High Signal	2.006	9.608***
	(3.407)	(3.078)
Constant	17.091***	15.000***
	(2.172)	(2.547)
R-Squared	0.006	0.043
Observations	64	55

Notes: Column 1 regresses task choice on receiving the high signal in the baseline; Column 2 does the same for those in the treatment. There are four low-signal observations in the treatment. The shown result is robust to a small sample t-test – shown in appendix table B.1. Robust standard errors presented in parentheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.4: Task Choice by Received Signal

	DV: Task Choice	
	(1) Both Signals	(2) High Signal
Treatment	5.847**	5.511*
	(2.347)	(3.129)
Constant	18.062***	19.097***
	(1.686)	(2.616)
R-Squared	0.050	0.040
Observations	119	82

Notes: Column 1 regresses task choice on treatment for all those in the experiment. Column 2 restricts the same regression to only those who received the high signal. Robust standard errors presented in parentheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

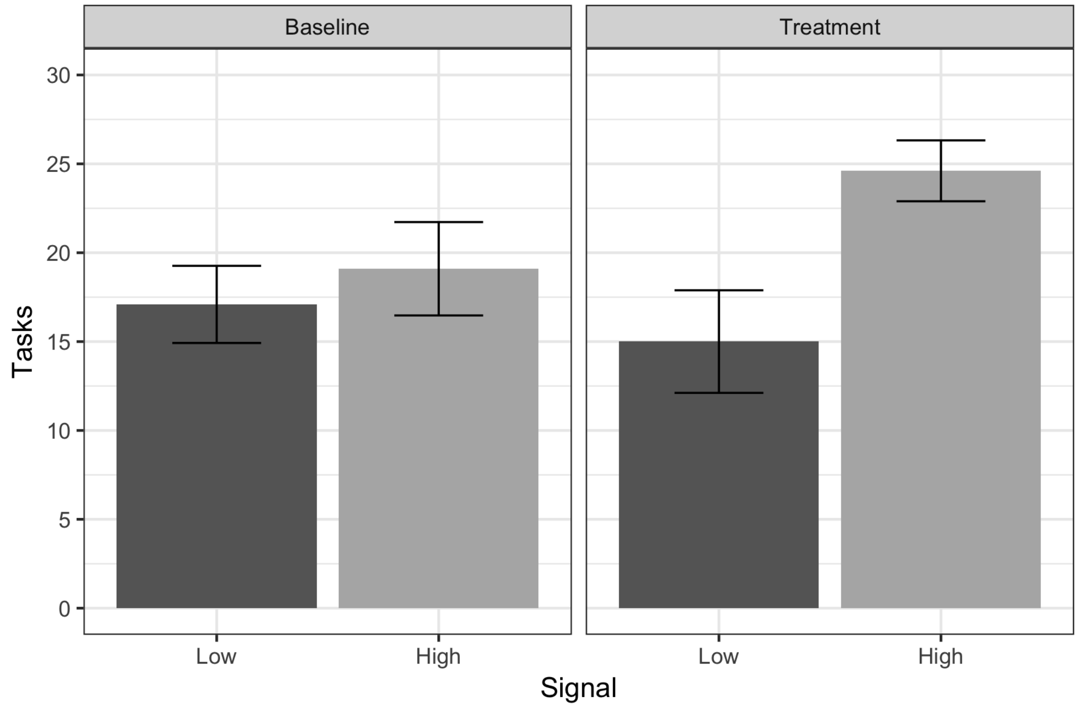


Figure 2.7: Average Tasks Choice by Signal, Treatment

Notes: This figure plots the average number of tasks subjects were willing to complete in exchange for a $20 donation by treatment and signal. The high signal stated that more than 50% of previous participants were willing to complete at least 20 tasks while the low signal stated less than 25% were willing to complete 20 tasks. In concordance with image signal extraction, subjects chose do significantly more tasks when they receive the high signal in treatment – where there is a 15-out-of-16 chance of receiving the true signal – than in the baseline – where there is an 8-out-of-16 chance of receiving the true signal. Brackets represent standard errors.

demonstrates that 73% of treatment participants choose to do at least 20 tasks while only 52% of baseline participants choose to do 20 or more tasks. Further, quantile regression results in table 2.7 shows that those below the median chose to significantly more tasks in response to the treatment, while those above the median do not.[24] Through the lens of our theory, these results, in conjunction with the results shown in table 2.5, suggest that those who choose to do fewer than 20 tasks in the baseline do so under the belief that most others made the same decision. However, in the treatment, the refined signal corrects their beliefs and they choose to complete

[24]The remaining quantiles are show in appendix table B.3.

more captchas.[25]

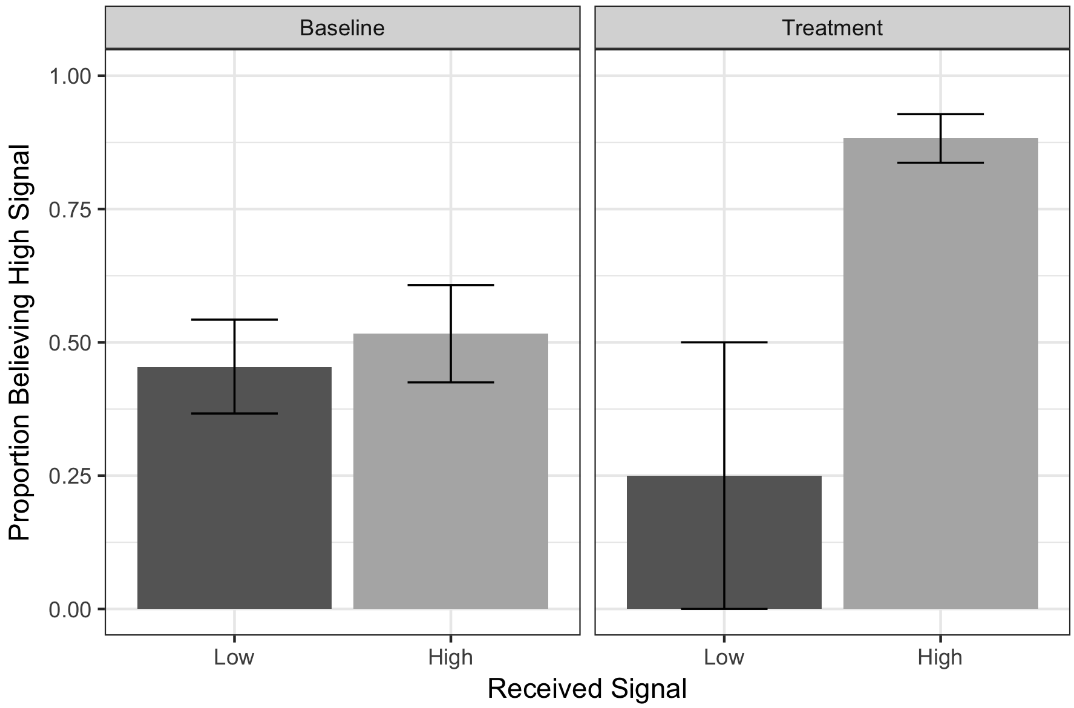


Figure 2.8: Distribution of Beliefs by Signal, Treatment

Notes: This figure plots the proportion of subjects who believed that more than 50% of previous participants were willing to complete at least 20 tasks by treatment and signal. In concordance with image signal extraction, participant beliefs are highly dependent on their signals in treatment, where there was a 15-out-of-16 chance of observing the true signal, and unresponsive in baseline where there was a 8-out-of-16 chance of observing the true signal. Brackets represent standard errors.

Our theory relies on signal extraction as the mechanism underlying behavioral changes following the receipt of social information. Thus, establishing that signal extraction occurs in practice is vital to demonstrating the viability of our theory as predictive of behavior resulting from social interactions. Not only should the receipt of more reliable social information alter behavior, but it should alter individuals' underlying beliefs which induce this change in behavior. The results outlined above show that the receipt of a refined signal affects both beliefs and

[25]A plausible alternative story is that moral wiggle room/wishful thinking leads those who would like to do fewer tasks to believe that everyone else also did few tasks. However, this story cannot explain why a more refined signal would correct deliberately chosen beliefs. Such a story would also have to incorporate the dynamics of signal extraction wherein wishful think is easier to abide by when the received social information is less informative.

Table 2.5: Task Choice by Belief, Signal, Treatment

	DV: Task Choice	
	(1)	(2)
High Belief	12.736***	11.931***
	(2.151)	(2.599)
High Signal		1.420
		(2.504)
Treatment		1.018
		(2.642)
Constant	12.524***	11.596***
	(1.638)	(1.673)
R-Squared	0.216	0.221
Observations	119	119

Notes: We regress task choice on an indicator that the subject believes the high signal is true. Robust standard errors presented in parentheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

actions. Further, our results suggest that those individuals whose beliefs change are those that drive changes in average behavior across treatments.

Table 2.6: Effect of Treatment on Likelihood of Choosing At Least Twenty Tasks

	DV: $1\{\text{Tasks} \geq 20\}$
Treatment	0.212**
	(0.087)
Constant	0.516***
	(0.063)
R-Squared	0.047
Observations	119

Notes:We regress an indicator for choosing to do 20 or more tasks on treatment. Robust standard errors in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

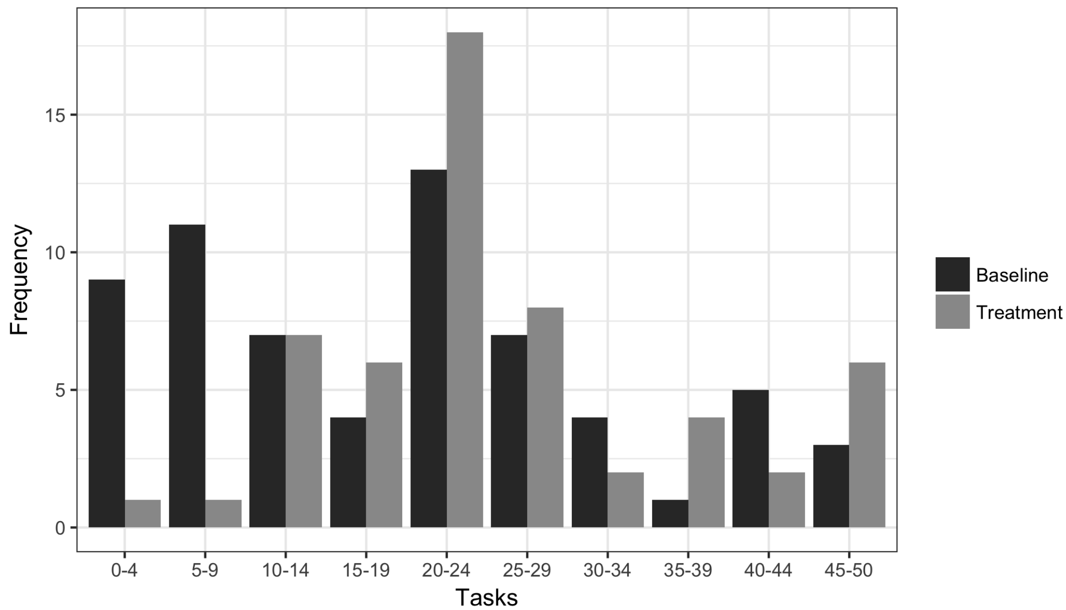


Figure 2.9: Distribution of Task Choice by Treatment

Notes: This figure shows that the difference in task choice between treatment and baseline arises from subjects who were willing to do 20 or fewer tasks. Given the established correlation between choice and beliefs, this figure suggests that the refined signal corrected beliefs so that more subjects believed the high signal and, consequently, were wiling to do more tasks.

2.5 Conclusion

Our primary objective was to propose and test a theory of self signaling in peer effects. In accordance with our theory, experiment participants chose costlier actions in anticipation of social information, and their choices adhered to basic signal extraction dynamics. These results establish a foundation for further exploration of the role of self and social signaling in peer effects. Specifically, our theory predicts that differences in extrinsic incentives and costs will act as signal interference and diminish the impact of social comparisons, thereby reducing observed peer effects and social conformity. However, our theory remains largely agnostic as to the scope of peer differences and environmental considerations that may be interpreted as signal interference. For example, while our theory clearly predicts that an individual who performs a task for charity will react less to decisions made by someone who is paid a wage for the task,

Table 2.7: Quantile Regressions

	DV: Task Choice				
	(1)	(2)	(3)	(4)	(5)
Quantiles	20th	40th	50th	60th	80th
Treatment	10.000***	10.000***	0.000	3.000	5.000
	(2.434)	(3.099)	(3.045)	(3.068)	(6.612)
Constant	5.000***	10.000***	20.000***	20.000***	30.000***
	(1.864)	(2.438)	(2.365)	(2.502)	(3.523)
R-Squared	0.050	0.050	.	0.050	0.050
Observations	119	119	119	119	119

Notes: This table quantifies the distributional difference in task choice shown in figure 2.9. Standard errors in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

it may also explain why a graduate student feels more compelled to donate to the Salvation Army when observing others do so at Aldi than at Whole Foods. That is, the relevance of image signal extraction introduces the possibility that differential peer effects between groups with differentiated observable personal traits is explained by subjectively perceived signal interference.

A related literature documents an important relationship between outcome uncertainty and self-serving preferences (Dana, Weber and Kuang, 2007; Andreoni and Bernheim, 2009; Exley, 2016). Excuse driven preferences may exacerbate the effect of perceived signal interference in image signaling. Therefore, exploring this relationship may provide better predictive power over the environmental conditions and policy interventions that will catalyze or mitigate social conformity. Such a research agenda may also lend insight on when individuals will seek or avoid social information.

Acknowledgements

Chapter 2, in full, is currently being prepared for submission for publication of the material. Bigenho, Jason; Martinez, Seung-Keun. "Social Comparisons in Peer Effects." The dissertation author was one of two primary investigators and authors of this material.

Chapter 3

Institutional Determinants of Municipal Fiscal Dynamics

As of 2008, local governments in the United States spend an amount roughly equal to one-eighth of national gross domestic product (GDP)—accounting for one-fourth of total government spending—and employ more than 14 million people (Glaeser, 2013). In light of these facts, it is clear that local governments, and municipal governments in particular, account for a substantial amount of economic activity in the U.S. and their spending and revenue decisions have the potential to affect a large portion of the population. Municipal governments are charged with funding a number of essential services, public goods and capital projects and often face fiscal limitations imposed on them by higher levels of government. It may, therefore, come as a surprise that the existing literature has not explored how these institutional limitations affect cities over the business cycle.

The particular limitations we study are a subset of what are referred to as "Tax and Expenditure Limitations" (henceforth TELs) and are imposed on municipalities by their state of residence. Specifically, we examine limits on growth in general expenditures or general revenues. One example of such a TEL is a limit in New Jersey on general expenditure increases, which states that increases in appropriations are restricted to rise by no more than five percent or the change in the CPI, whichever is smaller and applies to all municipalities within the state. Our analysis aims to help us better understand some of the forces shaping the evolution of city economies over time. In doing so, we provide evidence on whether or not these TELs achieve their desired effect of limiting the size of local governments—a question which has received differing answers in the literature. Beyond this, we also illuminate a likely unintended consequence of these limitations by widening the scope of the analysis to examine the dynamic response of disaggregated spending categories to shocks to local area employment. This allows us to provide a comprehensive examination of how these limitations interact with economic fluctuations, which can be used to both better evaluate the effects of these constraints and inform the design of future fiscal responsibility measures at the local level.

We measure economic fluctuations using instrumented log changes in commuting zone employment, where our instrument stems from the shift-share decomposition of employment growth developed in Bartik (1991). These employment changes are then mapped to municipalities within a given commuting zone. Using these estimates in conjunction with disaggregated municipal spending data, we construct a Jordà (2005) local projections specification to estimate the impact of the aforementioned TELs on municipal fiscal behavior in response to an employment shock contemporaneously and over time. We find that limitations on increases in general expenditures or revenues have strong effects. In response to a positive employment growth shock of one percent, we estimate a large, persistent, negative and statistically significant effect on growth in capital outlays that reaches a peak of roughly -3.5% per-capita one year after a shock. That is, spending growth on capital outlays falls by 3.5% per-capita in TEL-constrained municipalities relative to those unconstrained the year following a shock. A similar pattern emerges for transportation and public maintenance spending—relatively capital-intensive spending categories—suggesting they are absorbing the brunt of the capital response.

There is little differential effect on public safety and administrative expenditures and only a modest negative effect on general expenditures, indicating that TELs do not seem to be constraining the overall size of municipal governments much following an expansionary shock. This finding falls somewhere between that of Kousser, McCubbins and Moule (2008) and those ofearlier studies by Misiolek and Elder (1988), Elder (1992) and Shadbegian (1998). Taken together, our results suggest that broad fiscal responsibility interventions, such as the imposition of limits on general expenditures or revenues, may be ineffective in reducing the size of government. Rather, they may instead prompt governments to reduce investment in order to maintain their current levels of public safety and administrative spending.

A number of early papers sought to characterize the impact of TELS on local finances.[1] However, the effects captured in this literature represent average effects of the implementation of TELs on fiscal variables; how these limitations impact the ability of local governments to respond to economic fluctuations—and the dynamic adjustment induced by the interaction of these two forces—has gone largely unstudied. Buettner and Wildasin (2006) study dynamic municipal government adjustment to fiscal shocks, though no attention is paid to the impact of TELs on adjustment and the analysis focuses on more aggregated fiscal categories. At the state level, Poterba (1994), Bohn and Inman (1996) and Clemens (2012) study the impact of strict balanced-budget requirements on state finances in the face of economic downturns. This literature focuses on describing how state budgets respond to economic fluctuations, given the presence of binding limits (e.g., documenting whether adjustment has occurred largely through the revenue or expenditure side of the budget and what the composition of budget cuts induced by recessionary shocks is). We seek to answer a similar question at the municipal level, though with a decidedly different methodological approach in addition to consideration of expansionary shocks.

The rest of the paper is organized as follows. Section 3.1 provides the requisite background information on municipal governments and TELs. Section 3.2 discusses the data used in this project and how we construct the employment shocks mentioned above. Section 3.3 details our empirical strategy. Section 3.4 presents our results and section 3.5 concludes.

[1]See, for example, Joyce and Mullins (1991), Elder (1992), Mullins and Joyce (1996), Shadbegian (1998), Shadbegian (1999) and Skidmore (1999).

3.1 Background

3.1.1 Municipal Governments

We motivated our examination of municipal governments by noting that local government spending amounts to a significant fraction of GDP and that municipal governments feature prominently in this spending total. State and local government spending over time, as a share of GDP, is plotted in Figure 3.1.[2]

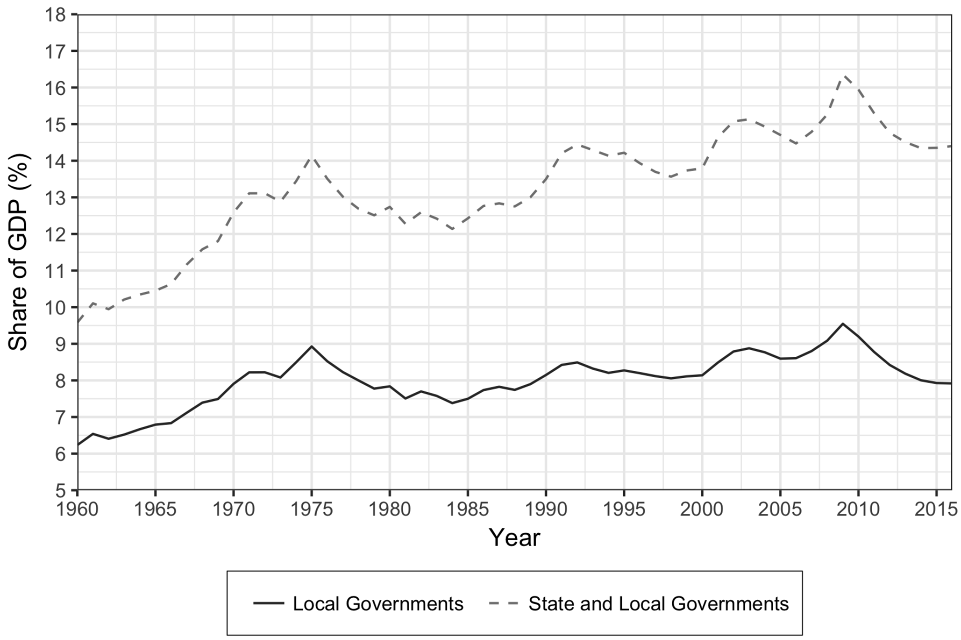


Figure 3.1: State and Local Government Spending, 1960-2016

Source: *Federal Reserve Economic Data*, Federal Reserve Bank of St. Louis, 2018

Municipal governments are but one form of local government—the other major forms being county and town governments and school and special districts—so it is worth discussing why we choose to focus on cities as opposed to another form of local government. Our reasons are twofold. First, municipal governments spend and raise more money than any other form of local government. Second, municipal governments are more numerous than are the other forms of local governments and the number of municipalities has been fairly constant over time.

[2]This figure does not include spending on social welfare.

Given that municipal governments account for a substantial amount of economic activity, we are particularly concerned with examining how institutional limitations affect a city's ability to respond to business cycle movements—making our focus on municipal governments a natural one. The particulars of TELs are the focus of the following subsection; here we focus on the functions of municipal governments.

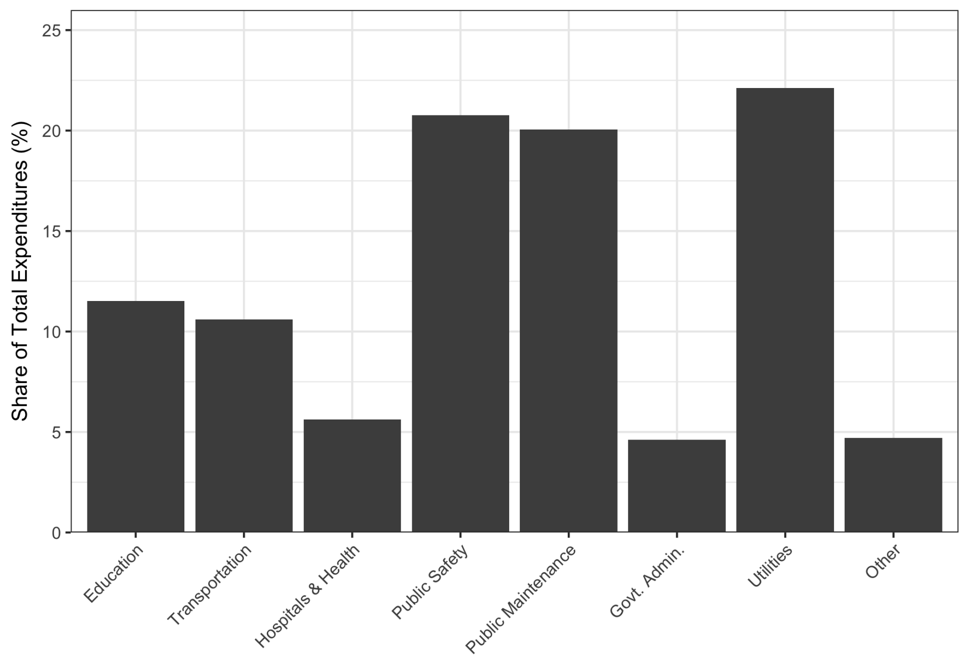


Figure 3.2: Municipal Government Spending, 2007

Source: *Annual Survey of State and Local Government Finances*, U.S. Census Bureau, 2007

There were 19,492 municipal governments as of 2007, a number that has been relatively stable over the past 50 years. Glaeser (2013) notes that municipal government spending can be broadly grouped into three main categories: basic city services, social welfare spending and education. Examples of basic city services include police, fire and waste management, while social welfare spending includes explicit social welfare spending as well as spending on hospitals and housing services. Virtually all city governments provide basic services like those listed above, but that is not the case for the latter two categories. Large cities tend not to spend much, if at all, on either of these categories—education spending, for instance, often falls under the directive of independent school boards in these places. Another type of spending that we will be especially concerned with in this paper, given the incentives provided by local fiscal institutions, is spending on capital outlays. Capital outlays comprise a portion of spending in many of the categories and examples listed above, but more generally include spending on construction and infrastructure pertaining to highways, buildings and bridges. Specifically, the expenditure categories we study are general expenditures, capital outlays, transportation, public safety, public maintenance and government administration. Figure 3.2 shows the percentage breakdown of municipal spending by function for 2007, a year in which all city governments were sampled as a part of the Census of Governments.

3.1.2 Tax and Expenditure Limitations (TELs)

Municipal governments face unique institutional limitations, often imposed on them by higher levels of government. The purpose of these limits is largely to constrain the size of local governments. Much like their state counterparts, city governments often face balanced-budget requirements and are formally restricted from running operating expenditure deficits. Additionally, many municipal governments are limited in their ability to borrow, with these limitations written into their state's constitution. Fiscal restrictions at the municipal level, however, usually take a more disaggregated form relative to state-level measures, where restrictions largely apply to the budget deficit as a whole. Mullins and Wallin (2004) catalogue such restrictions, known as "Tax and Expenditure Limitations" (TELs), drawing on the classification system developed in Joyce and Mullins (1991). The seven basic forms of TELs are listed below; these measures are state policies that apply to all municipalities within the state.

1. Overall property tax rate limits applying to all local governments
2. Specific property tax rate limits applying to specific types of local government (municipalities, counties, school districts, and special districts) or specific functions

3. Property tax levy (revenue) limits

4. General revenue increase limits

5. General expenditure increase limits

6. Limits on assessment increases

7. Full disclosure (truth in taxation)

Oftentimes, an explicit goal of imposing property tax TELs is to diversify revenue streams by inducing a shift in revenue away from property taxes and towards sources such as charges, user fees and utilities.[3] For this reason, we restrict attention to limits on increases in general expenditures and revenues, which more accurately reflect constraints on the government as a whole. In our empirical analysis, we group general expenditure and revenue growth limits into one category since they serve similar functions in principle. Figure 3.3 shows where these limits exist and table 3.1 provides further information as to when they were put into (and in some cases, taken out of) place. We ignore full disclosure (truth in taxation) since, in practice, this does not represent a binding limitation on municipal governments.

Table 3.1: States Implementing General Expenditure and Revenue TELs

State	Type of Limit	Passed	Repealed
Arizona	General Expenditure	1921	
California	General Expenditure	1979	
Colorado	General Expenditure	1992	
Minnesota	General Revenue	1971	1993
Nebraska	General Expenditure	1996	
Nevada	General Revenue	1984	1989
New Jersey	General Expenditure	1976	

Source: Adapted from Mullins and Wallin (2004)

[3] This effect has been documented in a number of studies, including Mullins and Joyce (1996), Shadbegian (1999) and Skidmore (1999).

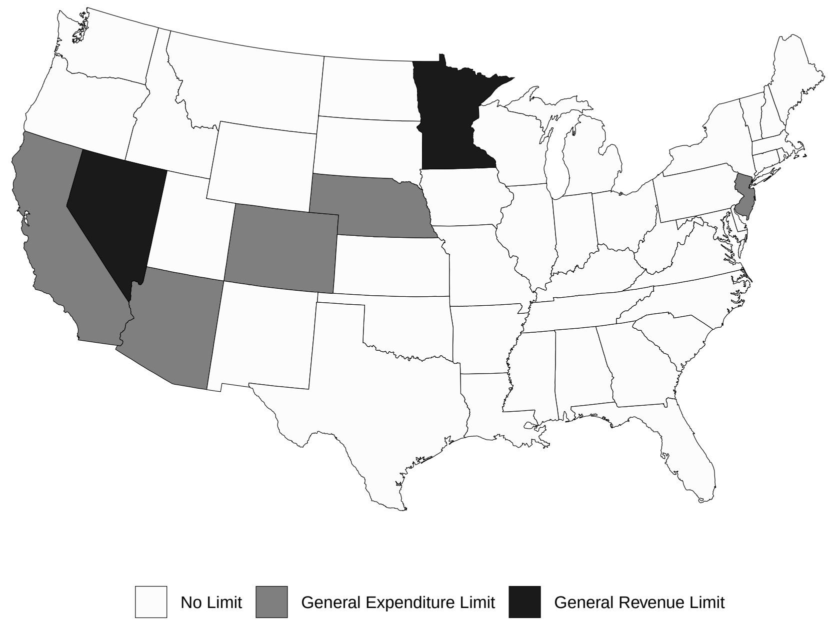


Figure 3.3: States Implementing General Expenditure and Revenue TELs

Source: Adapted from Mullins and Wallin (2004)

3.2 Data

Annual, disaggregated expenditure data for a large number of U.S. municipalities spanning the years 1974-2004 come via the *Annual Survey of State and Local Government Finances*, courtesy of the U.S. Census Bureau. Annual county-level employment data by both North American Industry Classification System (NAICS) and Standard Industrial Classification (SIC) industry codes for the same time frame also comes from the U.S. Census Bureau, through their data product titled *County Business Patterns*. We follow Autor, Dorn and Hanson (2013) in mapping and aggregating county-level employment data to the corresponding commuting zones.

3.2.1 Shock Construction

In order to examine how TELs affect municipalities' ability to respond to economic fluctuations, we must first define what we mean by "economic fluctuations." We choose to measure economic fluctuations with changes to the local level of employment. Changes in the local level of employment would undoubtedly be an endogenous regressor in any regression in which a municipal fiscal variable appears as the dependent variable, biasing our estimates. To overcome this problem, we instrument for changes in employment following Bartik (1991). Specifically, the employment growth predictions used to form the instrument can be written as:

$$Bartik_{c,s,t} = \sum_{j} Share_{j,c,s,1974} \times \Delta log(Emp_{j,n,t}) \tag{3.1}$$

where *Emp* represents the absolute level of employment, j indexes industries, c indexes commuting zones, s indexes states, t indexes time (annual) and n indicates a national total. We take 1974, the first year of our sample, as the base year and utilize SIC three-digit industry codes when estimating (3.1). Predicted employment growth in commuting zone c at time t is the sum across industries of national employment growth in each industry at time t, weighted by industry j's share of employment in commuting zone c in 1974. The employment growth prediction is a function of initial industry composition and industry-specific national growth rates. Thus, it removes the idiosyncratic time-varying components of growth. This leaves our employment growth prediction as being a function of the growth predicted by the all-industry national average and industry-specific national growth rates, hence removing the endogenous component of the regressor.[4] We then assign the same commuting zone-level shock to each municipality within a given commuting zone.

Note that in the equation given above, c indexes *commuting zones*, whereas our analysis concerns *municipalities*. Due to data limitations, our Bartik shocks are constructed at the commuting zone level, the reason being that employment data by industry code (a necessary component of the Bartik instrument) do not exist at the municipal level, or any level more disaggregated than the county level. We choose to construct our shocks at the commuting

[4]This may not be obvious from the structure of equation (3.1), which is the result of terms canceling in the shift-share decomposition.

zone-level since we believe commuting zones more accurately describe the relevant labor market, though our results are robust to using county-level shocks.[5]

3.2.2 Descriptive Statistics

In constructing our final sample, we choose to keep municipalities with no more than two non-surveyed years during our sample period and no more than ten years of missing data in any fiscal category we examine. For municipalities with missing values in one or more fiscal category, we impute values linearly using surrounding years' values. The end result is a "relatively balanced" panel of 1,150 municipalities from 1974-2004; table 3.2 presents summary statistics for our entire sample for the years 1974 and 2004.[6] The summary statistics for our sample in 2004 align closely with the spending breakdown displayed in figure 3.2 for the universe of municipal governments in 2007.

General expenditures are defined as "all city expenditure other than the specifically enumerated kinds of expenditure classified as *Utility Expenditure*, *Liquor Stores Expenditure*, and *Employee-Retirement* or other *Insurance Trust Expenditure*." Transportation is the sum of direct expenditures on highways, airports and parking. Public safety is the sum of direct expenditures on police, fire, corrections and protective inspection and regulation. Public maintenance is the sum of direct expenditures on parks and recreation, housing and community development, solid waste management and sewerage. Government administration is the sum of direct expenditures on financial administration, judicial and legal matters and general public buildings. These definitions follow the annual summary report for the *Annual Survey of State and Local Government Finances*, issued by the U.S. Census Bureau.

[5] We follow a similar approach when using county-level shocks; we assign the same county-level shock to each municipality within a given county.

[6] Sample sizes differ slightly from the total number of observations (1,150) when there are missing values that cannot be imputed.

Table 3.2: Summary Statistics

	N	Mean	Std. Dev.	Min	Max
***Panel 1**: 1974 Summary Statistics*					
Population	1149	75972.68	283806.3	1402	7646818
Exp./Rev. TEL	1150	0.047	0.213	0	1
Spending Variables					
General Expenditures	1149	909.405	695.867	108.438	8554.54
Capital Outlays	1135	207.870	314.486	0.930	8220.699
Transporation	1148	113.759	108.540	8.529	2505.386
Public Maintenance	1145	188.107	160.451	1.112	1643.869
Public Safety	1149	160.330	86.359	0.143	1351.332
Government Administration	1136	33.516	35.463	0.263	427.921
***Panel 2**: 2004 Summary Statistics*					
Population	1086	99401.15	319603.3	1312	8084316
Exp./Rev. TEL	1150	0.135	0.342	0	1
Growth Variables					
CZ Employment Growth	1150	0.011	0.025	-0.162	0.266
Predicted Growth	1150	0.004	0.011	-0.078	0.064
Spending Variables					
General Expenditures	1086	1706.146	2497.151	452.96	73165.72
Capital Outlays	1066	276.510	504.984	0.610	13539.95
Transporation	1086	171.878	217.111	6.093	5594.251
Public Maintenance	1086	330.227	235.43	18.703	3480.346
Public Safety	1085	337.078	175.591	0.056	2751.601
Government Administration	1074	74.625	70.307	0.044	946.524

Notes: Summary statistics for cities in our sample for years 1974 and 2004. Exp./Rev. TEL is an indicator taking value one if a city is limited by a general expenditure or general revenue TEL in that year. All spending variables are in real, per-capita 2004 U.S. dollars. CZ employment growth is the first difference of the natural logarithm of commuting zone employment. Predicted growth is the Bartik growth prediction as defined in equation (3.1). Note that these quantities are undefined for 1974, as we take that as the base year. Alaska and Hawaii have been excluded from the sample, as are cities with ten or more zero values (which could denote missing data or a true zero) for any spending variable. Remaining zero values are imputed linearly from the surrounding years' values for that city. Any remaining missing values arise when a city did not appear in the sample at all in that year. Values for these years were not imputed.

3.3 Empirical Strategy

Broadly, our goal is to estimate the effect of local fiscal institutions on municipal governments' response to cyclical movements. Specifically, we examine the effect of general expenditure and revenue TELs on disaggregated city spending categories in response to a local employment shock. The question our statistical model speaks to is, what is the differential response in growth in a given spending category following a shock to local area employment growth between a municipality subject to a limit on increases in general expenditures or revenues and one which is not? Our baseline specification draws on the local projections method formulated in Jordà (2005) and used similarly in Leduc and Wilson (2013). Our estimating equation is given by:

$$
\begin{aligned}
\log(y_{i,c,s,t+h}) - \log(y_{i,c,s,t-1}) =& \alpha_i^h + \alpha_t^h + \sum_{q=1}^{p+1} \beta_q^h \log(y_{i,c,s,t-q}) \\
&+ \delta_1^h 1\{TEL_{s,t}\} + \delta_2^h \Delta \log(\widehat{Emp}_{c,s,t}) \\
&+ \delta_3^h 1\{TEL_{s,t}\} \times \Delta \log(\widehat{Emp}_{c,s,t}) + \varepsilon_{i,c,s,t+h},
\end{aligned}
\tag{3.2}
$$

where $y_{i,c,s,t}$ is per-capita spending in a category y in municipality i in commuting zone c in state s at time t. t indexes years and $h = 0, 1, \ldots, 5$ denotes the horizon of the forecast. The expenditure categories we examine are general expenditures, capital outlays, transportation, public safety, public maintenance and government administration. $\Delta \log(\widehat{Emp}_{c,s,t})$ is the instrumented log change in commuting zone employment, where employment growth is instrumented for using $Bartik_{c,s,t}$ as in equation (3.1) in our first-stage. $1\{TEL_{s,t}\}$ equals one if state s has a general expenditure or revenue TEL at time t and zero otherwise. We set $p = 2$ and cluster standard errors at the commuting zone level. α_i^h and α_t^h are municipality and year fixed-effects, respectively.

Our coefficient of interest is δ_3^h; its interpretation is the following. If employment growth increases by one percent at time t, spending growth in category y in a TEL-constrained municipality changes by δ_3^h percent relative to a municipality without either TEL at horizon

h—i.e., spending growth in category y in a municipality with a general expenditure or revenue TEL is δ_3^h percent higher/lower than that in a municipality without one h years after the shock. At horizon $h = 0$, this model reduces to a fairly standard static specification. For $h = 1, \ldots, 5$, this model produces forecasts of the effect of a shock to employment growth at time t on category y at time $t + h$, conditional on information through time t. Plotting δ_3^h for $h = 0, 1, \ldots, 5$ therefore provides impulse responses for the relative effect of the shock across municipalities with and without general expenditure or revenue TELs on impact through five years afterwards. We feel that it is crucial to examine dynamic effects for a complete analysis of how institutional limitations affect municipalities—only by doing so can we get a sense of how institutions influence these governments' behavior over the business cycle. By coupling dynamic analysis with disaggregation, our estimation strategy allows us to see precisely where the shocks are felt within constrained governments, in addition to the magnitude and persistence of the effects.

3.4 Results

3.4.1 Main Results

This section presents estimates from the baseline specification given in equation (3.2). The spending categories we examine are general expenditures, capital outlays, transportation, public maintenance, public safety and government administration. Coefficient estimates and standard errors are reported in tables C.6 and C.7. Our coefficient of interest is δ_3^h, which corresponds to the coefficient on the variable TEL$\times \Delta log(Emp)$ in the table. Figure 3.4 plots the impulse response coefficients δ_3^h for $h = 0, 1, \ldots, 5$, along with accompanying 90% confidence bands.

Our results show that following a positive shock to employment growth of one percent, growth in general expenditures in municipalities faced with a limit on increases in general expenditures or revenues only modestly falls behind that in municipalities with no such limits.

Table 3.3: Baseline Results

	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$	$h=5$
***Panel 1**: Expenditures*						
$\Delta log(Emp)$	-0.189	-0.237	-0.011	0.044	0.410**	0.565***
	(0.155)	(0.154)	(0.131)	(0.175)	(0.192)	(0.162)
TEL $\times$ $\Delta log(Emp)$	-0.340*	-0.815***	-0.410*	-0.078	0.343**	-0.020
	(0.184)	(0.292)	(0.214)	(0.214)	(0.169)	(0.258)
TEL	0.037*	0.070***	0.078***	0.076***	0.073***	0.093***
	(0.020)	(0.024)	(0.021)	(0.023)	(0.027)	(0.035)
N	34153	33003	31853	30703	29553	28403
***Panel 2**: Capital Outlays*						
$\Delta log(Emp)$	-0.148	-0.280	1.067**	-0.087	0.130	1.663***
	(0.496)	(0.506)	(0.526)	(0.569)	(0.651)	(0.498)
TEL $\times$ $\Delta log(Emp)$	-2.406***	-3.527***	-2.515***	-0.248	1.073*	0.309
	(0.631)	(0.784)	(0.756)	(0.702)	(0.551)	(0.621)
TEL	0.126***	0.198***	0.185***	0.128**	0.112**	0.137***
	(0.025)	(0.036)	(0.049)	(0.050)	(0.047)	(0.050)
N	33971	32821	31671	30521	29371	28221
***Panel 3**: Transportation*						
$\Delta log(Emp)$	-0.217	-0.098	0.241	0.577**	0.323	0.756***
	(0.212)	(0.238)	(0.277)	(0.264)	(0.530)	(0.285)
TEL $\times$ $\Delta log(Emp)$	-0.320	-1.672***	-1.465***	-0.427*	0.227	-0.019
	(0.261)	(0.465)	(0.355)	(0.253)	(0.318)	(0.269)
TEL	0.026*	0.089***	0.096***	0.086***	0.075***	0.086***
	(0.014)	(0.027)	(0.030)	(0.028)	(0.024)	(0.027)
N	34148	32998	31848	30698	29548	28398

Notes: Dependent variables are given by each panel heading. Units of all dependent variables are real, per-capita 2004 U.S. dollars. h represents the forecast horizon, as outlined in equation (3.2). TEL is an indicator taking value one if a city faces a general expenditure or general revenue TEL during the year in which the shock occurs. *Emp* is employment in the commuting zone in which a city resides. Municipality and year fixed effects omitted. Standard errors are clustered at the commuting zone level and presented in parentheses. *Significance*: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 3.4: Baseline Results (Continued)

	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$	$h=5$
***Panel 4**: Public Maintenance*						
$\Delta log(Emp)$	-0.412	-0.258	-0.285	-0.227	0.684***	0.411
	(0.261)	(0.273)	(0.295)	(0.303)	(0.264)	(0.252)
TEL $\times$ $\Delta log(Emp)$	-0.563*	-1.456***	-0.962***	-0.521	-0.225	-0.778*
	(0.328)	(0.355)	(0.366)	(0.400)	(0.317)	(0.429)
TEL	0.051***	0.096***	0.095**	0.090**	0.099**	0.131***
	(0.016)	(0.026)	(0.039)	(0.046)	(0.043)	(0.037)
N	34132	32982	31832	30682	29532	28382
***Panel 5**: Public Safety*						
$\Delta log(Emp)$	-0.053	0.019	0.151	0.232*	0.507***	0.252
	(0.126)	(0.126)	(0.114)	(0.134)	(0.166)	(0.177)
TEL $\times$ $\Delta log(Emp)$	0.124	-0.131	0.129	-0.067	-0.054	-0.211
	(0.150)	(0.389)	(0.412)	(0.217)	(0.267)	(0.229)
TEL	0.006	0.020	0.019	0.031*	0.037*	0.046***
	(0.013)	(0.018)	(0.020)	(0.016)	(0.019)	(0.017)
N	34137	32987	31837	30687	29537	28387
***Panel 6**: Government Administration*						
$\Delta log(Emp)$	0.156	0.411	0.280	0.617**	0.791***	0.560*
	(0.291)	(0.295)	(0.280)	(0.265)	(0.275)	(0.294)
TEL $\times$ $\Delta log(Emp)$	0.763*	0.663*	1.169*	-0.123	0.585**	0.412
	(0.415)	(0.398)	(0.679)	(0.419)	(0.265)	(0.254)
TEL	-0.046	-0.048	-0.060	-0.031	-0.054	-0.043
	(0.044)	(0.059)	(0.072)	(0.066)	(0.056)	(0.057)
N	34020	32870	31720	30570	29420	28270

Notes: Dependent variables are given by each panel heading. Units of all dependent variables are real, per-capita 2004 U.S. dollars. h represents the forecast horizon, as outlined in equation (3.2). TEL is an indicator taking value one if a city faces a general expenditure or general revenue TEL during the year in which the shock occurs. *Emp* is employment in the commuting zone in which a city resides. Municipality and year fixed effects omitted. Standard errors are clustered at the commuting zone level and presented in parentheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

This indicates that TELs do not seem to be constraining the size of municipal governments much following an expansionary shock, which contributes to the outstanding debate on whether or not TELs achieve their intended effect of constraining the size of local governments. Specifically, general expenditures growth falls by 0.815% in constrained municipalities relative to unconstrained municipalities one year after a shock, but rebounds to grow 0.343% quicker four years following a shock. The latter result may reflect "catch up" spending that occurs once the binding constraint has slackened.

There are two primary takeaways from our findings for the disaggregated spending categories listed above. First, there is by and large no differential effect on public safety and administrative expenditures. The only significant result at any horizon for either of these two categories is the 0.585% relative increase in administrative spending growth four years after an initial shock. The timing of this effect comports with the result for general expenditures and is again likely reflective of catch up spending occurring once the constraint has slackened, particularly given the nature of the shock we consider.[7] Second, capital-related spending in municipalities facing a general expenditure or revenue TEL falls substantially and persistently relative to spending in municipalities without either of these limits. Significant effects on capital outlays persist for two years following a shock, reaching a peak relative reduction in growth of 3.527% per-capita after one year. Transportation and public maintenance expenditures—two capital-intensive spending categories—track this pattern, indicating they are absorbing the bulk of the capital response. Maximum relative reductions in transportation and public maintenance spending growth are 1.672% and 1.456% per-capita, respectively; both occur one year after a shock.

[7] If administrative staffing and spending roughly follow a step-function based on population, per-capita administrative spending would be expected to increase following a positive shock to employment growth. This could appear as a significant relative increase in administrative spending at longer horizons if initial desired spending increases must be postponed due to the presence of a binding constraint on increases in general expenditures or revenues.

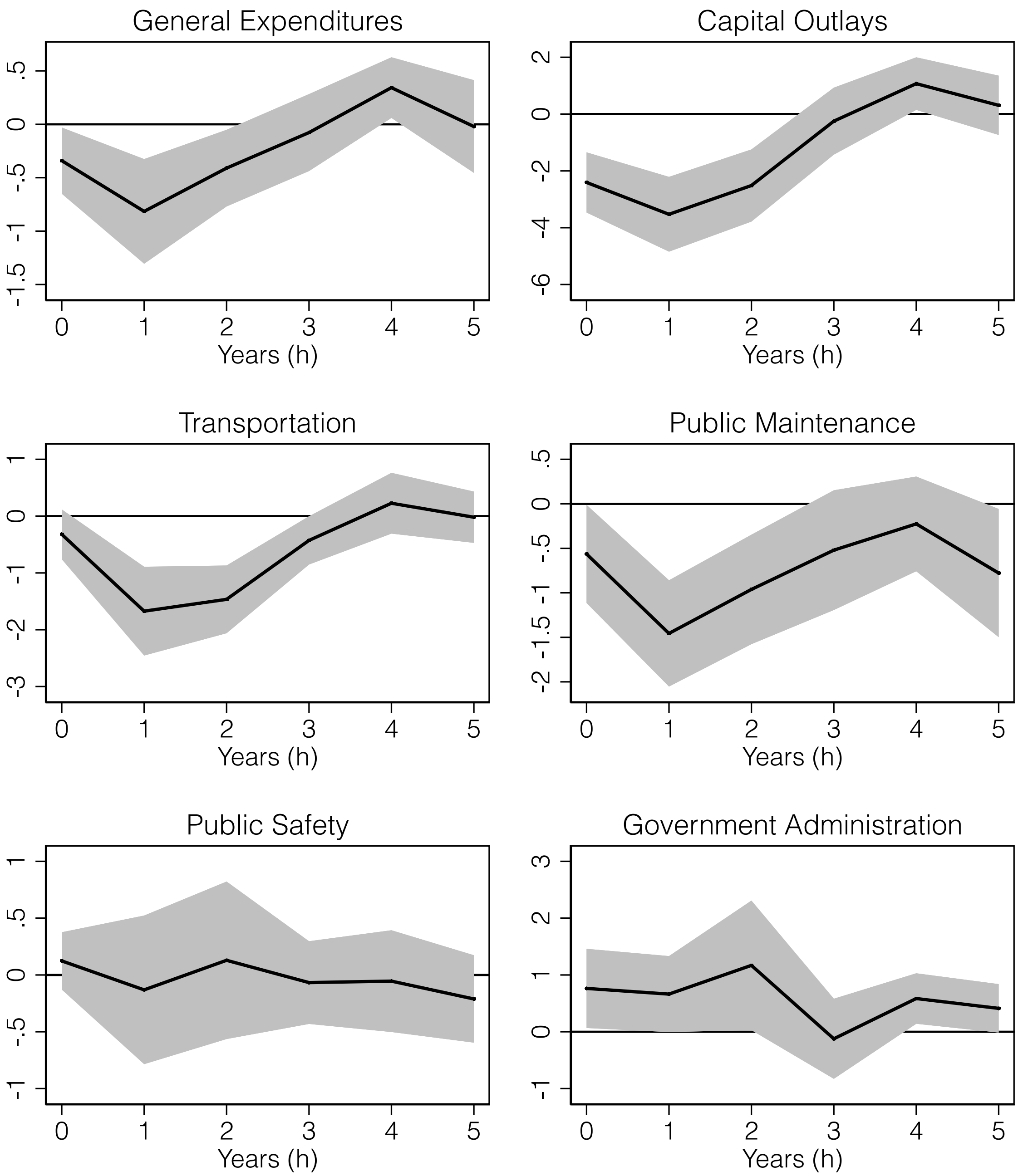


Figure 3.4: Baseline Results - Impulse Response Functions

Notes: Impulse response functions for δ_3^h, as specified by equation (3.2). The x-axis represents the forecast horizon h, *i.e.*, the number of years following an employment shock. Point estimates are represented by the thick black line, with 90 percent confidence bands represented by the shaded gray area.

3.4.2 Threats to Identification

In this section, we outline two potential threats to identification and present evidence that neither is a concern in our case. First, the implementation of a TEL is potentially endogenous. Specifically, state governments may respond to large increases in local government expenditures by passing a TEL in order to curb what they consider to be "out of control" spending. This would introduce bias into our estimates.

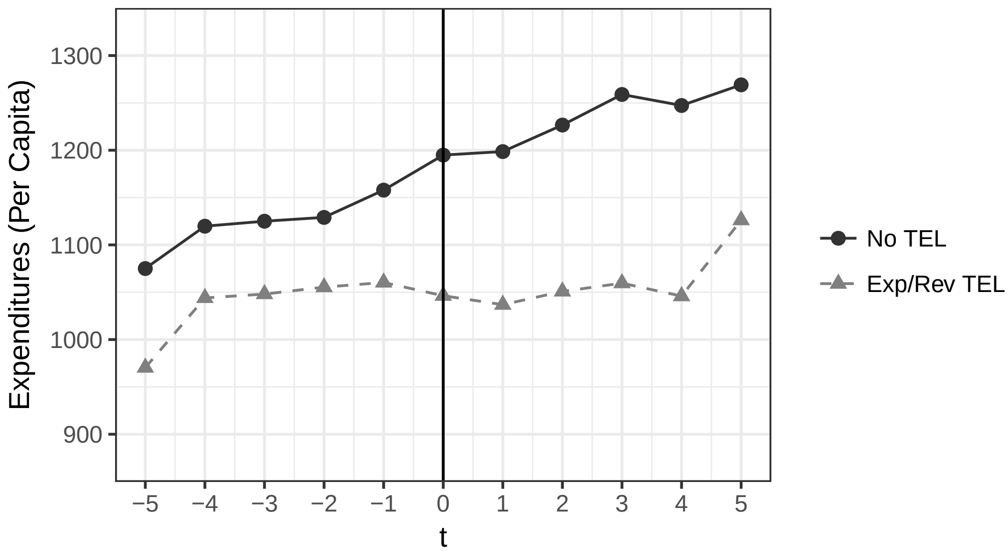


Figure 3.5: Comparison of Trends

Notes: Average per-capita expenditures for municipalities in states implementing general expenditure or general revenue TELs (gray dashed lines and triangles) and municipalities in all other states (black solid lines and points). The *x*-axis represents the number of years before or after a TEL is implemented.

To address this concern, we compare spending patterns of municipalities in states implementing general expenditure or general revenue TELs with those in states that do not in the years immediately before and after the limits are passed. Figure 3.5 plots average per-capita expenditures for municipalities in TEL-implementing states and in other states in order to examine expenditure trends surrounding the five TELs passed during our sample period.[8] The *x*-axis represents the number of years before or after the implementation of a TEL, with $t = 0$ being the year of implementation. This figure shows clear parallel trends in expenditures between the

[8] These are the TELs passed in 1976, 1979, 1984, 1992 and 1996.

two groups. Only when the TEL is implemented do the trends begin to diverge. This shows that, on average, states do not respond specifically to upticks in spending by local governments by passing TELs, and that before TELs are passed, the dynamics of municipal spending are similar in TEL and non-TEL states.

An additional concern is that local governments in TEL states receive systematically different shocks than those in non-TEL states. Particularly, we might be concerned that because there are more non-TEL states than TEL states, the distribution of shocks between these groups differs, either from an underlying difference in the economies of the states within

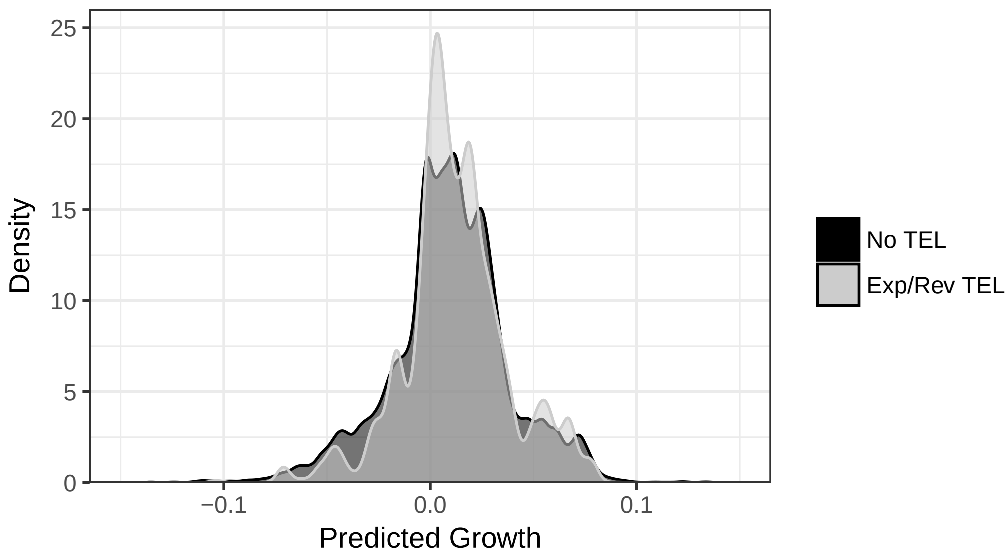


Figure 3.6: Comparison of Shock Densities

Notes: Overlaid densities of predicted growth (calculated according to equation (3.1)) in states with (light, in front) and without (dark, behind) general expenditure or general revenue TELs.

those groups, or simply as a consequence of the few observations for TEL-states. In either case, our estimates could be biased. To test for this, in figure 3.6 we plot the distribution of growth predictions, as calculated by equation (3.1), for municipalities in states with general expenditure or general revenue TELs (light, in front) and those in states without TELs (dark, behind). The distributions are overlaid so that they may be more easily compared. The general shapes, centers and spreads of the two distributions align closely. The distribution for TEL states is more uneven, but this is likely attributable to the sample size. This figure should confirm that, on average, TEL-facing municipalities and their counterparts in other states face similar shocks.

3.5 Conclusion

Municipal governments account for a substantial amount of economic activity in the United States and are entrusted with funding a number of essential services, public goods and capital projects. Furthermore, they often face fiscal limitations imposed on them by higher levels of government. In this paper, we study how limits on general expenditures or revenues growth affect cities in response to business cycle movements. These limits are state-level policies that apply to all municipalities within a state and comprise a subset of what are referred to more generally as "Tax and Expenditure Limitations" (TELs).

We measure economic fluctuations using instrumented log changes in commuting zone employment, drawing on the methodology developed in Bartik (1991). We then use these estimates, along with disaggregated municipal spending data, to formulate a local projections impulse response specification to study the effect these limits have on municipalities in response to an employment shock within-period and over time. Our findings are summarized as follows. Following an expansionary shock, the overall size of municipal governments (as measured by general expenditures) faced with one of these limits grows only slightly more slowly than it does in those without either limit. Moreover, we find largely no differential effect for public safety and administrative expenditures. The primary effect of the TELs is on capital spending; cities faced with general expenditure or revenue TELs lag substantially and persistently behind their unconstrained counterparts in capital outlays growth following a positive shock to employment growth. Relative reductions in capital outlays occur predominantly through transportation and public maintenance expenditures, specifically.

TELs are fiscal responsibility measures intended to constrain the size of local governments. What we have shown in this paper is that the most restrictive of these limits are only moderately successful in doing so. Rather, as the economy expands, such limits appear to induce reductions in public investment in order to sustain prior levels of public safety and administrative spending. We hope our findings will be used to inform the design of future fiscal responsibility measures at the local level. Our results suggest that targeting aggregate expenditure or revenue categories may lead to potentially undesirable changes in the underlying spending mix. As a result, directing these limits instead towards more disaggregated spending categories may prove to be more effective in achieving the underlying policy intent. One possibility would be coupling a limit on general expenditure increases with an explicit limit on increases in administrative spending.

Acknowledgements

Chapter 3, in full, is currently being prepared for submission for publication of the material. Bigenho, Jason; Johnson, Grant. "Institutional Determinants of Municipal Fiscal Dynamics." The dissertation author was one of two primary investigators and authors of this material.

Appendix A

Elections, Individuals and Incumbency

A.1 Summary Statistics

Summary statistics for all one-candidate House contributors and those giving to close elections are presented in Table A.1. For the most part, these two samples look similar in terms of their average characteristics. In both samples, the average total contribution is about $1000 (real 2014 USD). About 25% of donors make a contribution in the next election cycle (two years later). The majority of these donors give to their time t recipient candidate in the next cycle (about 70% of repeat givers and 17% of the overall sample), while 40% of repeat donors (10% overall) give to a new candidate in the next cycle. The distribution of subsequent giving, also, is similar across these samples. The vast majority of next cycle donations go to the same district. 21% of individuals give to the same district in the next cycle (more than 80% of repeat givers), while 7% of individuals give to a new district (about 30% or repeat givers).

The main differences across these samples appear in the latter half of Table A.1. First, average recipient candidate voteshare is 57% in the sample of givers to all elections, while only 51% in the sample of close-election contributors. Similarly, the likelihood of recipient candidate victory is higher in the larger sample (66% compared to 55%). Recipients in the larger sample are more often Republican, though only negligibly so (48% compared to 47%). The larger

Table A.1: Summary Statistics for House Donors

	All Elections	Close Elections
Contribution Amount ($)	1006.53	1016.24
	[892.54]	[925.17]
Make House Contribution, $t+1$	0.26	0.25
	[0.44]	[0.43]
Give to Same Candidate, $t+1$	0.18	0.17
	[0.39]	[0.38]
Give to New Candidate, $t+1$	0.10	0.10
	[0.29]	[0.30]
Give to Same District, $t+1$	0.21	0.21
	[0.41]	[0.41]
Give to New District, $t+1$	0.07	0.07
	[0.26]	[0.26]
Candidate Voteshare	0.57	0.51
	[0.12]	[0.05]
Candidate Wins	0.68	0.55
	[0.47]	[0.50]
Democrat Recipient	0.48	0.47
	[0.50]	[0.50]
Incumbent Recipient	0.58	0.45
	[0.49]	[0.50]
Candidate Runs for House, $t+1$	0.67	0.60
	[0.47]	[0.49]
Observations	1549619	818141

Notes: Contribution records are collapsed to the contributor-candidate-cycle level. Those observations where the total contribution does not exceed $200 are excluded. Observations with Bonica CID less than 1,000,000 are excluded (potential PAC, union or firm contributions). Contributions from candidates to their own campaigns are excluded. Contributions from contributors where total contributions to any single election exceed Federal limits are excluded. Candidates matched to CQ Press general election returns by last name, then fuzzy matched on last name and inspected manually for errors. Sample restricted to only individuals donating to one candidate in a cycle and that candidate seeks election in the House. "Close Elections" are those elections where candidate voteshares are between 0.4 and 0.6. Contribution amount is the total amount of all donations to the recipient candidate, in real 2014 US dollars. *Sources*: Database on Ideology, Money in Politics, and Elections; CQ Voting and Elections Collection.

pool of donors more often give to incumbent candidates. About 58% of donations in the larger sample go to incumbents, compared to 45% going to incumbents in close elections. Finally, the likelihood of the recipient candidate running again in the next cycle is higher in the larger sample at 67% compared to the 60% figure in close elections.

Summary statistics for Senate contributors are presented in Table A.2. The average contribution here, in both the larger sample and donors to close elections, is higher than that of the House samples at about \$1130. The propensity to contribute in cycle $t+1$ is significantly lower in the Senate samples, with Senate contributors participating about half as much as House contributors at 12%. Note, however, that time periods here are defined as they are in the House. So, while there always be a House election in an individual's district in period $t+1$, there will not necessarily be a Senate election in an individual's state in $t+1$. Further, given that a large fraction of repeat donors in our House samples continue to give to the same candidate, the fact that few candidates run for Senate in consecutive election cycles (2-3%, from the last row of Table A.2) helps explain this difference. This also explains why Senate contributors give more often to new candidates than their House counterparts. Senate contributors give to the same candidate in $t+1$ only about 2% of the time, while giving to new candidates at a rate of 10%. Future giving is also less focused in the same locality as the initial gift. Senate contributors give again to the same state 7% of the time (about 60% of repeat givers), while giving to new states 5% of the time (about 40% of repeat givers).

Similar to the House samples, the differences in the Senate samples mainly occur in candidate-level characteristics. Average recipient candidate voteshare is 54% in the sample of all elections, while only 51% in the sample of close elections. The likelihood of recipient candidate victory is higher in the larger sample (61% compared to 53%). Recipients in the larger sample are more often Republican (50% compared to 49%) and incumbents (50% compared to 42%). As mentioned above, the likelihood of the recipient candidate running again in the next cycle is negligible in both samples: 2% in the larger sample compared to 3% in close elections.

Table A.2: Summary Statistics for Senate Donors

	All Elections	Close Elections
Contribution Amount ($)	1131.49	1131.06
	[938.46]	[932.88]
Make Senate Contribution, $t+1$	0.11	0.12
	[0.32]	[0.32]
Give to Same Candidate, $t+1$	0.02	0.02
	[0.14]	[0.14]
Give to New Candidate, $t+1$	0.10	0.10
	[0.30]	[0.30]
Give to Same State, $t+1$	0.07	0.07
	[0.25]	[0.25]
Give to New State, $t+1$	0.05	0.05
	[0.22]	[0.22]
Candidate Voteshare	0.54	0.51
	[0.10]	[0.05]
Candidate Wins	0.61	0.53
	[0.49]	[0.50]
Democrat Recipient	0.50	0.49
	[0.50]	[0.50]
Incumbent Recipient	0.50	0.42
	[0.50]	[0.49]
Candidate Runs for Senate, $t+1$	0.02	0.03
	[0.15]	[0.18]
Observations	964436	683118

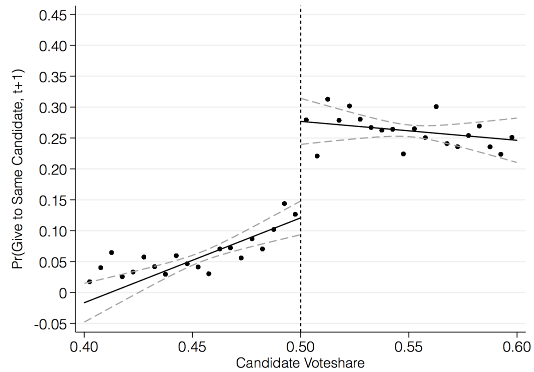
Notes: Contribution records are collapsed to the contributor-candidate-cycle level. Those observations where the total contribution does not exceed $200 are excluded. Observations with Bonica CID less than 1,000,000 are excluded (potential PAC, union or firm contributions). Contributions from candidates to their own campaigns are excluded. Contributions from contributors where total contributions to any single election exceed Federal limits are excluded. Candidates matched to CQ Press general election returns by last name, then fuzzy matched on last name and inspected manually for errors. Sample restricted to only individuals donating to one candidate in a cycle and that candidate seeks election in the Souse. “Close Elections” are those elections where candidate voteshares are between 0.4 and 0.6. Contribution amount is the total amount of all donations to the recipient candidate, in real 2014 US dollars. $t+1$ is the election cycle following the close election. *Sources*: Database on Ideology, Money in Politics, and Elections; CQ Voting and Elections Collection.

A.2 Additional Results

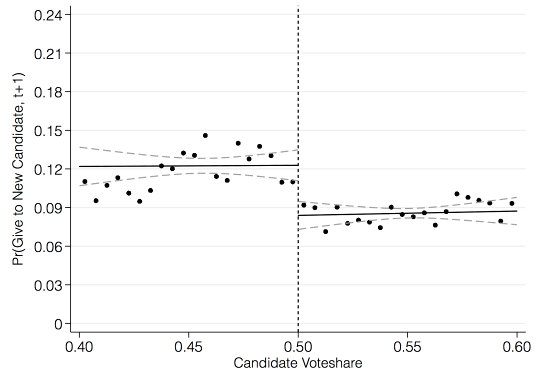
Table A.3: Regression Discontinuity Estimates: Effect of Close Candidate Victory on House Candidates Running for Senate

	(1) $t-1$	(2) $t+1$	(3) $t+2$	(4) $t+3$
Candidate Wins	-0.003	0.004	0.024**	0.027**
	(0.003)	(0.008)	(0.009)	(0.011)
Constant	0.003	0.011**	0.006	0.007*
	(0.003)	(0.005)	(0.004)	(0.004)
Observations	3495	3726	3448	3146
Elections	1854	1984	1843	1690

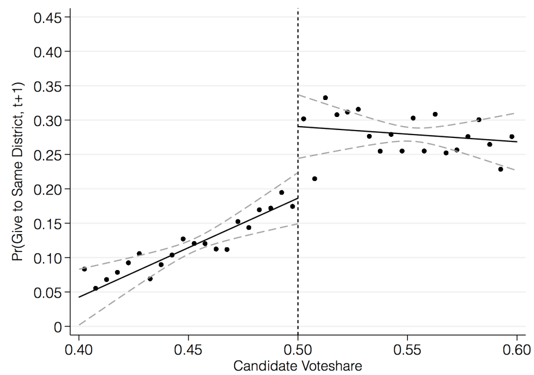
Notes: Entries are results of estimation of equation (1.2) with the dependent variable being an indicator of a candidate running in a primary or general election Senate campaign in the cycle indicated in the column heading. Regressions are estimated with triangular weights and slope coefficients are omitted. Robust standard errors for all regressions are clustered at the congressional district level and are presented in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ *** $p < 0.01$



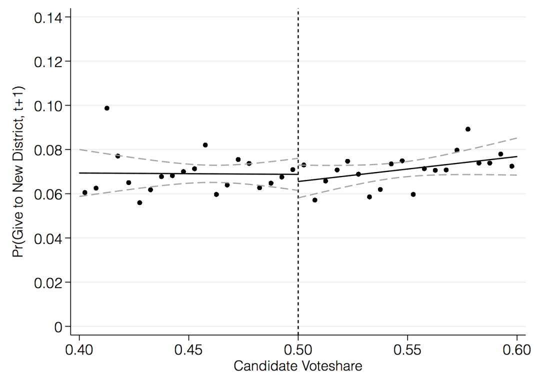
(a) Give to Same Candidate



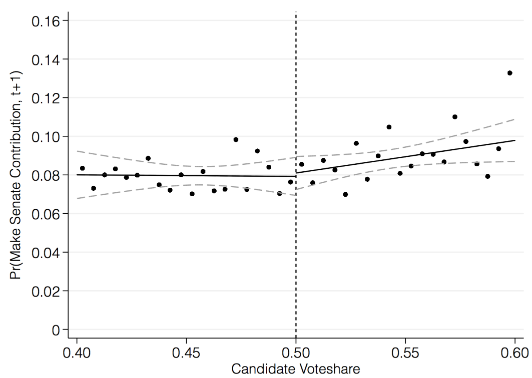
(b) Give to New Candidate



(c) Give to Same District



(d) Give to New District



(e) Give to Senate

Figure A.1: Additional RD Figures (House)

Notes: Panel headings denote dependent variables. All dependent variables measured in cycle $t+1$. The x-axes represent the two-party general election voteshare of the candidate recipient in time t. Solid lines are estimated via ordinary least squares using triangular weights. Standard errors clustered at the Congressional district level are represented by dashed lines. The bin width for all figures is 0.5 percentage points.

Table A.4: Regression Discontinuity Estimates: Effect of Close Candidate Victory on Giving Within State (Senate)

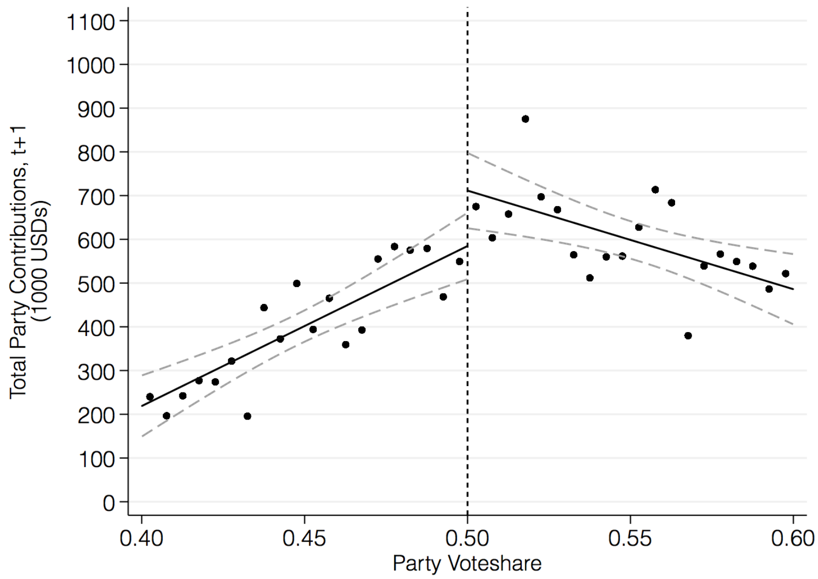
	(1) $t-1$	(2) $t+1$	(3) $t+2$	(4) $t+3$
Panel A:				
Pr(Give to Same State)				
Candidate Wins	0.001	0.036***	0.038***	0.072***
	(0.013)	(0.012)	(0.006)	(0.012)
Constant	0.065***	0.050***	0.045***	0.083***
	(0.013)	(0.007)	(0.009)	(0.008)
Panel B:				
Pr(Give to Same Cand. in Same State)				
Candidate Wins	-0.004	0.023**	0.038***	0.109***
	(0.014)	(0.009)	(0.008)	(0.016)
Constant	0.032***	0.012**	0.007**	0.016***
	(0.009)	(0.006)	(0.003)	(0.006)
Panel C:				
Pr(Give to New Cand. in Same State)				
Candidate Wins	0.005	0.016	0.005	-0.035***
	(0.008)	(0.011)	(0.005)	(0.009)
Constant	0.036***	0.038***	0.038***	0.069***
	(0.009)	(0.006)	(0.009)	(0.008)
Observations	671230	683118	619836	549817
Contributors	603076	613520	558645	498400
Elections	256	279	262	247

Notes: Panel A displays results of estimation of equation (1.1) with the dependent variable being an indicator of an individual making a contribution exceeding $200 to a Senate candidate in the same state as the cycle t candidate in the cycle indicated in the column heading. Panel B displays results of estimation of equation (1.1) with the dependent variable being an indicator of an individual making a contribution exceeding $200 to the cycle t candidate in the same state as the cycle t candidate in the cycle indicated in the column heading. Panel C displays results of estimation of equation (1.1) with the dependent variable being an indicator of an individual making a contribution exceeding $200 to a Senate candidate other than the cycle t candidate in the same state as the cycle t candidate in the cycle indicated in the column heading. In all cases, regressions are estimated with triangular weights and slope coefficients are omitted. Robust standard errors for all regressions are clustered at the state level and are presented in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

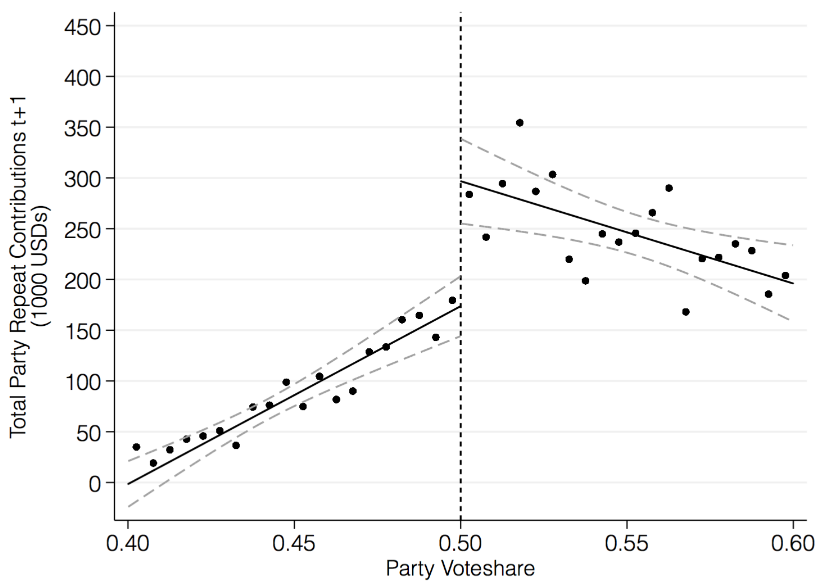
Table A.5: Regression Discontinuity Estimates: Effect of Close Candidate Victory on Giving to House and Giving to Other States (Senate)

	(1) $t-1$	(2) $t+1$	(3) $t+2$	(4) $t+3$
Panel A:				
Pr(Make House Contribution)				
Candidate Wins	-0.009	-0.004	0.000	0.006
	(0.006)	(0.006)	(0.005)	(0.004)
Constant	0.078^{***}	0.101^{***}	0.100^{***}	0.092^{***}
	(0.005)	(0.007)	(0.005)	(0.003)
Panel B:				
Pr(Give to New State)				
Candidate Wins	-0.002	0.001	0.000	-0.000
	(0.006)	(0.008)	(0.006)	(0.006)
Constant	0.046^{***}	0.057^{***}	0.051^{***}	0.045^{***}
	(0.005)	(0.006)	(0.004)	(0.003)
Observations	671230	683118	619836	549817
Contributors	603076	613520	558645	498400
Elections	256	279	262	247

Notes: Panel A displays results of estimation of equation (1.1) with the dependent variable being an indicator of an individual making a contribution exceeding \$200 to a House candidate in the cycle indicated in the column heading. Panel B displays results of estimation of equation (1.1) with the dependent variable being an indicator of an individual making a contribution exceeding \$200 to a Senate candidate in a state other than that of the cycle t candidate in the cycle indicated in the column heading. In both cases, regressions are estimated with triangular weights and slope coefficients are omitted. Robust standard errors for all regressions are clustered at the state level and are presented in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$



(a) Total Party Contributions



(b) Total Party Repeat Contributions

Figure A.2: Additional Incumbency RD Figures

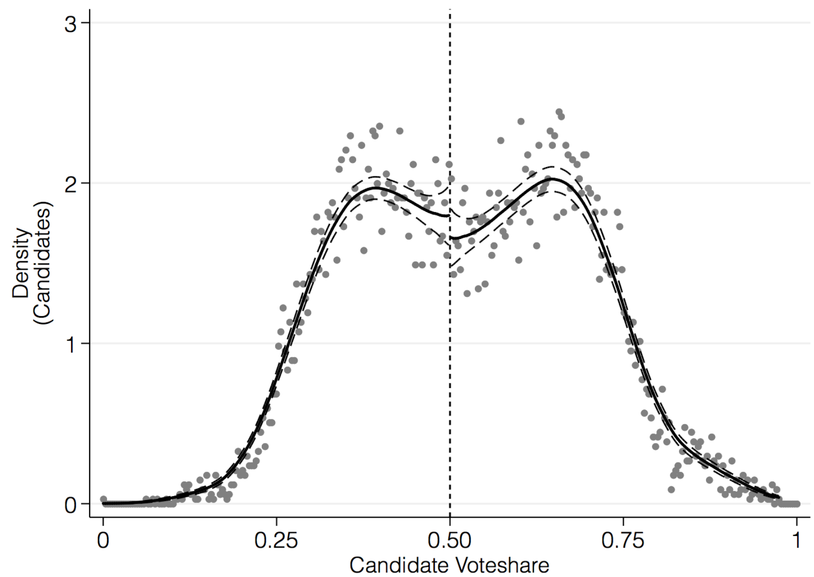
Notes: Panel (a) displays the average value by bin of Party Total, defined above, in cycle $t+1$. Panel (b) displays the the average value by bin of Party Repeat Total, defined above, in cycle $t+1$. The x-axis is the two-party general election voteshare of the party's candidate in cycle t. Solid lines are estimated via ordinary least squares using triangular weights. Standard errors clustered at the Congressional district level are represented by dashed lines. The bin width for both figures is 0.5 percentage points.

A.3 Robustness

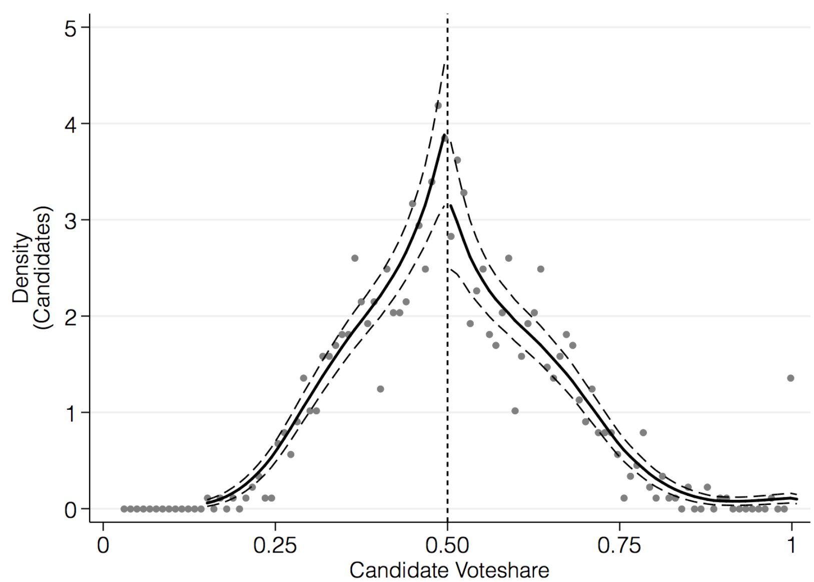
Table A.6: Robustness: Lagged Party Totals and Shares of Contributions

	(1) Party Share	(2) Party Repeat Share	(3) Party Total	(4) Party Repeat Total
Party Win	0.013	0.005	39.571	8.151
	(0.023)	(0.008)	(41.498)	(12.904)
Constant	0.493***	0.093***	705.721***	150.953***
	(0.012)	(0.005)	(50.422)	(14.586)
Observations	2732	2732	2732	2732
Elections	1366	1366	1366	1366
Districts	773	773	773	773

Notes: Entries are coefficients resulting from estimation of equation (1.3) with the dependent variables indicated by column headings, analogous to those defined in section 1.4 except measured in cycle t. In all cases, regressions are estimated with triangular weights and slope coefficients are omitted. Robust standard errors for all regressions are clustered at the Congressional district level and are presented in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$



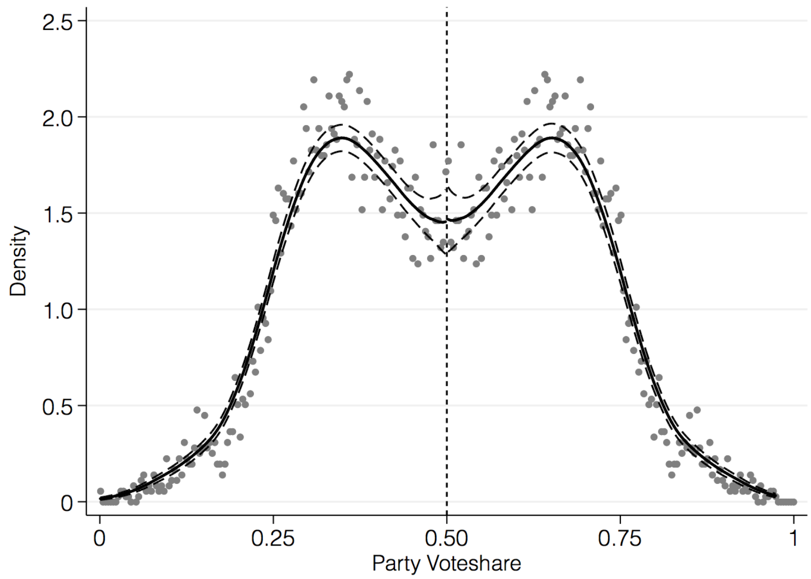
(a) House Candidates



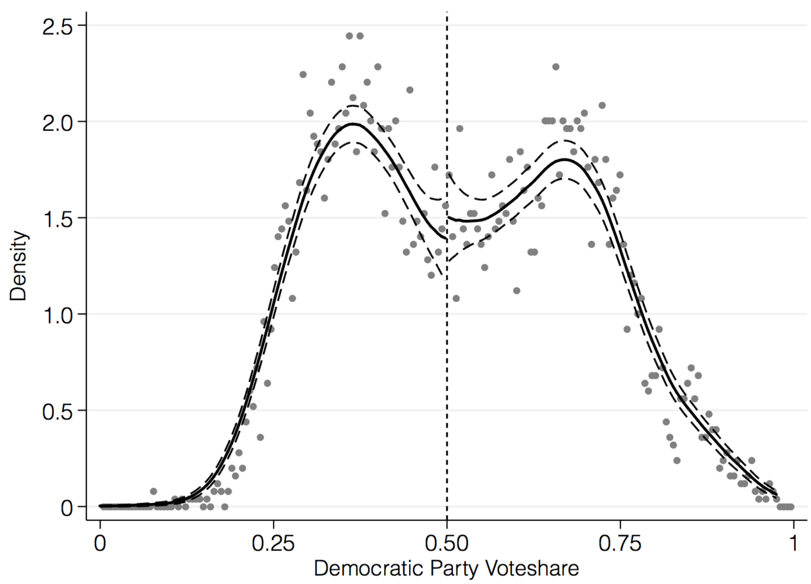
(b) Senate Candidates

Figure A.3: McCrary Tests (Main Results)

Notes: Panel (a) displays density of vote shares for recipient House candidates in the main sample. Panel (b) displays deensity of vote shares for recipient Senate candidates in the main sample. Based on McCrary (2008).



(a) Both Parties



(b) Democratic Party

Figure A.4: McCrary Tests (Incumbency)

Notes: Panel (a) displays density of vote shares for parties used in the financial incumbency analysis. Panel (b) displays density of vote shares for only the Democratic party observations used in the financial incumbency analysis. Based on McCrary (2008).

Table A.7: Regression Discontinuity Estimates: Effect of Party Victory on Democratic Party Contributions Next Cycle

	(1) Party Share	(2) Party Repeat Share	(3) Party Total	(4) Party Repeat Total
Party Win	0.122***	0.091***	210.813***	142.185***
	(0.026)	(0.013)	(79.064)	(34.903)
Constant	0.375***	0.092***	515.397***	157.357***
	(0.019)	(0.007)	(54.847)	(21.975)
Observations	1502	1502	1502	1502
Elections	1502	1502	1502	1502
Districts	831	831	831	831

Notes: Entries are coefficients resulting from estimation of equation (1.3) with the dependent variables indicated by column headings, all of which are defined above. Only observations from the Democratic Party are included. In all cases, regressions are estimated with triangular weights and slope coefficients are omitted. Robust standard errors for all regressions are clustered at the Congressional district level and are presented in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.8: Robustness: Discontinuity Estimates Using Balanced Panel (House)

	(1) $t-1$	(2) $t+1$	(3) $t+2$	(4) $t+3$
Make House Contribution	-0.006	0.085***	0.048***	0.031*
	(0.026)	(0.024)	(0.016)	(0.017)
Give to Same Candidate	-0.005	0.136***	0.089***	0.057***
	(0.029)	(0.032)	(0.018)	(0.019)
Give to New Candidate	-0.003	-0.036***	-0.030***	-0.015
	(0.006)	(0.013)	(0.011)	(0.011)
Make Senate Contribution	0.008	-0.003	-0.000	0.002
	(0.011)	(0.008)	(0.010)	(0.009)
Give to Same District	-0.016	0.091***	0.054***	0.036**
	(0.025)	(0.027)	(0.017)	(0.016)
Give to Same Cand. in Same Dist.	-0.016	0.134***	0.089***	0.057***
	(0.028)	(0.032)	(0.018)	(0.019)
Give to New Cand. in Same Dist.	-0.002	-0.044***	-0.034***	-0.024**
	(0.005)	(0.011)	(0.011)	(0.011)
Give to New District	0.008	0.002	-0.001	-0.001
	(0.009)	(0.009)	(0.008)	(0.008)
Observations	100211	100211	100211	100211
Candidate Runs for House	-0.063	0.422***	0.499***	0.370***
	(0.093)	(0.070)	(0.069)	(0.078)
Observations	631	631	631	631

Notes: All regressions employ a triangular kernel and 10 percentage point bandwidth. Each entry is the regression coefficient on whether the recipient candidate wins in time t. All other coefficients are excluded. Dependent variables for each regression are described in each row. Timing of past and future donations are given in the column headings. Robust standard errors, clustered at the Congressional district level, presented in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.9: Robustness: Discontinuity Estimates Using Balanced Panel (Senate)

	(1) $t-1$	(2) $t+1$	(3) $t+2$	(4) $t+3$
Make Senate Contribution	0.003	0.027**	0.033***	0.065***
	(0.014)	(0.013)	(0.009)	(0.010)
Give to Same Candidate	0.001	0.025**	0.041***	0.109***
	(0.016)	(0.010)	(0.009)	(0.016)
Give to New Candidate	0.003	0.008	0.000	-0.034***
	(0.010)	(0.010)	(0.009)	(0.010)
Make House Contribution	-0.007	-0.010	-0.004	0.004
	(0.005)	(0.007)	(0.006)	(0.004)
Give to Same State	0.008	0.032**	0.037***	0.071***
	(0.014)	(0.013)	(0.007)	(0.012)
Give to Same Cand. in Same State	0.001	0.025**	0.041***	0.109***
	(0.016)	(0.010)	(0.009)	(0.016)
Give to New Cand. in Same State	0.009	0.010	0.002	-0.036***
	(0.008)	(0.012)	(0.006)	(0.010)
Give to New State	-0.004	-0.001	-0.001	-0.000
	(0.006)	(0.009)	(0.006)	(0.006)
Observations	537929	537929	537929	537929
Candidate Runs for Senate	0.027	-0.013	-0.019	0.746***
	(0.041)	(0.029)	(0.035)	(0.069)
Observations	390	390	390	390

Notes: All regressions employ a triangular kernel and 10 percentage pint bandwidth. Each entry is the regression coefficient on whether the recipient candidate wins in time t. All other coefficients are excluded. Dependent variables for each regression are described in each row. Timing of past and future donations are given in the column headings. Robust standard errors, clustered at the state level, presented in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.10: Robustness: Discontinuity Estimates with Alternative Bandwidths and Triangular Kernel (House)

	$t-1$			$t+1$			$t+2$			$t+3$		
	10	5	2	10	5	2	10	5	2	10	5	2
Make House Contribution	-0.001	0.014	0.030	0.099***	0.089***	0.076*	0.058***	0.056***	0.039**	0.033***	0.027**	0.017
	(0.014)	(0.018)	(0.029)	(0.020)	(0.030)	(0.039)	(0.009)	(0.014)	(0.019)	(0.008)	(0.012)	(0.018)
Give to Same Candidate	0.005	0.024	0.046	0.156***	0.129***	0.112***	0.088***	0.069***	0.033	0.057***	0.047***	0.027
	(0.017)	(0.023)	(0.033)	(0.022)	(0.033)	(0.040)	(0.013)	(0.019)	(0.029)	(0.011)	(0.015)	(0.023)
Give to New Candidate	-0.006	-0.008	-0.013	-0.039***	-0.024**	-0.014	-0.018***	-0.003	0.015	-0.015***	-0.011	-0.007
	(0.006)	(0.009)	(0.013)	(0.007)	(0.010)	(0.013)	(0.006)	(0.008)	(0.010)	(0.005)	(0.007)	(0.011)
Make Senate Contribution	-0.001	-0.002	-0.004	0.002	0.004	0.009	-0.002	-0.006	0.003	0.012*	0.015	0.020
	(0.008)	(0.012)	(0.017)	(0.005)	(0.006)	(0.009)	(0.005)	(0.006)	(0.010)	(0.006)	(0.009)	(0.015)
Observations	807308	442425	197865	818141	449327	201054	702559	383398	166673	583824	314640	137246
Give to Same District	-0.026	-0.011	0.014	0.104***	0.088**	0.077	0.063***	0.058***	0.044*	0.048***	0.050***	0.026
	(0.019)	(0.028)	(0.043)	(0.027)	(0.041)	(0.050)	(0.011)	(0.016)	(0.025)	(0.012)	(0.016)	(0.024)
Give to Same Cand. in Same Dist.	-0.023	-0.000	0.031	0.152***	0.118***	0.097**	0.093***	0.066***	0.025	0.065***	0.058***	0.025
	(0.024)	(0.036)	(0.054)	(0.027)	(0.040)	(0.049)	(0.015)	(0.023)	(0.036)	(0.013)	(0.018)	(0.026)
Give to New Cand. in Same Dist.	-0.002	-0.008	-0.014	-0.046***	-0.029***	-0.020	-0.030***	-0.006	0.023	-0.019**	-0.010	-0.004
	(0.007)	(0.010)	(0.016)	(0.009)	(0.011)	(0.017)	(0.009)	(0.011)	(0.016)	(0.008)	(0.009)	(0.016)
Give to New District	-0.002	-0.004	-0.002	-0.003	-0.001	-0.002	-0.004	-0.006	-0.005	0.000	-0.001	0.008
	(0.004)	(0.005)	(0.006)	(0.004)	(0.006)	(0.009)	(0.006)	(0.007)	(0.011)	(0.007)	(0.010)	(0.017)
Observations	393446	207625	94710	607370	329156	153564	343772	192581	91946	198553	107521	47336
Candidate Runs for House	-0.013	0.022	0.086	0.485***	0.409***	0.340***	0.482***	0.430***	0.384***	0.410***	0.369***	0.316***
	(0.039)	(0.056)	(0.088)	(0.029)	(0.042)	(0.065)	(0.036)	(0.053)	(0.085)	(0.037)	(0.056)	(0.088)
Observations	3495	1677	672	3726	1803	724	3448	1684	678	3146	1523	616

Notes: All regressions employ a triangular kernel. Each entry is the regression coefficient on whether the recipient candidate wins in time t. All other coefficients are excluded. Dependent variables for each regression are described in each row. Timing of past and future donations and bandwidths are given in the column headings. Robust standard errors, clustered at the Congressional district level, presented in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.11: Robustness: Discontinuity Estimates with Alternative Bandwidths and Rectangular Kernel (House)

	$t-1$			$t+1$			$t+2$			$t+3$		
	10	5	2	10	5	2	10	5	2	10	5	2
Make House Contribution	-0.009	0.010	0.032	0.104***	0.099***	0.075*	0.060***	0.065***	0.039**	0.037***	0.031***	0.020
	(0.012)	(0.017)	(0.025)	(0.016)	(0.025)	(0.039)	(0.008)	(0.012)	(0.019)	(0.007)	(0.010)	(0.016)
Give to Same Candidate	-0.005	0.017	0.050*	0.170***	0.147***	0.103**	0.097***	0.088***	0.035	0.062***	0.055***	0.034*
	(0.015)	(0.021)	(0.029)	(0.018)	(0.028)	(0.043)	(0.011)	(0.016)	(0.025)	(0.009)	(0.013)	(0.021)
Give to New Candidate	-0.006	-0.006	-0.014	-0.046***	-0.029***	-0.016	-0.024***	-0.010	0.008	-0.014***	-0.014**	-0.010
	(0.005)	(0.008)	(0.011)	(0.006)	(0.009)	(0.012)	(0.006)	(0.007)	(0.010)	(0.005)	(0.006)	(0.010)
Make Senate Contribution	-0.001	-0.002	-0.000	0.000	0.001	0.012	0.002	-0.005	0.006	0.011**	0.011	0.019
	(0.007)	(0.010)	(0.017)	(0.005)	(0.006)	(0.008)	(0.005)	(0.006)	(0.009)	(0.005)	(0.008)	(0.013)
Observations	807308	442425	197865	818141	449327	201054	702559	383398	166673	583824	314640	137246
Give to Same District	-0.030*	-0.021	0.018	0.111***	0.101***	0.070	0.070***	0.071***	0.045**	0.046***	0.052***	0.036
	(0.016)	(0.025)	(0.040)	(0.022)	(0.035)	(0.054)	(0.010)	(0.014)	(0.022)	(0.011)	(0.015)	(0.022)
Give to Same Cand. in Same Dist.	-0.030	-0.014	0.039	0.167***	0.140***	0.090*	0.107***	0.093***	0.037	0.063***	0.072***	0.032
	(0.020)	(0.032)	(0.050)	(0.022)	(0.035)	(0.053)	(0.013)	(0.019)	(0.031)	(0.012)	(0.017)	(0.024)
Give to New Cand. in Same Dist.	0.000	-0.005	-0.017	-0.054***	-0.038***	-0.019	-0.037***	-0.021**	0.009	-0.019***	-0.022**	-0.001
	(0.006)	(0.009)	(0.015)	(0.008)	(0.010)	(0.015)	(0.008)	(0.010)	(0.015)	(0.007)	(0.009)	(0.014)
Give to New District	-0.002	-0.002	-0.011	-0.003	-0.002	-0.007	-0.005	-0.005	-0.011	-0.000	0.002	-0.002
	(0.004)	(0.005)	(0.008)	(0.004)	(0.005)	(0.008)	(0.005)	(0.007)	(0.010)	(0.006)	(0.009)	(0.015)
Observations	393446	207625	94710	607370	329156	153564	343772	192581	91946	198553	107521	47336
Candidate Runs for House	-0.012	0.016	0.099	0.509***	0.435***	0.336***	0.500***	0.466***	0.367***	0.417***	0.386***	0.326***
	(0.034)	(0.052)	(0.079)	(0.026)	(0.037)	(0.059)	(0.031)	(0.045)	(0.076)	(0.032)	(0.048)	(0.079)
Observations	3495	1677	672	3726	1803	724	3448	1684	678	3146	1523	616

Notes: All regressions employ a rectangular kernel. Each entry is the regression coefficient on whether the recipient candidate wins in time t. All other coefficients are excluded. Dependent variables for each regression are described in each row. Timing of past and future donations and bandwidths are given in the column headings. Robust standard errors, clustered at the Congressional district level, presented in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.12: Robustness: Discontinuity Estimates Using Alternative Bandwidths and Triangular Kernel (Senate)

	$t-1$			$t+1$			$t+2$			$t+3$		
	10	5	2	10	5	2	10	5	2	10	5	2
Make Senate Contribution	-0.002	-0.002	-0.037***	0.034***	0.031*	0.026	0.035***	0.028***	0.015	0.066***	0.053***	0.033*
	(0.013)	(0.015)	(0.012)	(0.012)	(0.016)	(0.025)	(0.008)	(0.010)	(0.016)	(0.010)	(0.013)	(0.018)
Give to Same Candidate	-0.004	0.010	-0.021	0.023**	0.023*	0.008	0.038***	0.037***	0.032***	0.109***	0.092***	0.060
	(0.014)	(0.016)	(0.016)	(0.009)	(0.013)	(0.026)	(0.008)	(0.008)	(0.009)	(0.016)	(0.024)	(0.039)
Give to New Candidate	0.002	-0.010	-0.021*	0.016*	0.013	0.023	0.005	-0.000	-0.010	-0.033***	-0.029*	-0.019
	(0.009)	(0.011)	(0.013)	(0.009)	(0.012)	(0.015)	(0.008)	(0.011)	(0.018)	(0.010)	(0.015)	(0.021)
Make House Contribution	-0.009	-0.020**	-0.023	-0.004	-0.009	-0.014	0.000	-0.007	-0.005	0.006	0.006	-0.003
	(0.006)	(0.009)	(0.015)	(0.006)	(0.009)	(0.013)	(0.005)	(0.005)	(0.009)	(0.004)	(0.005)	(0.005)
Give to Same State	0.001	0.003	-0.039***	0.036***	0.031**	0.023	0.038***	0.027***	0.008	0.072***	0.055***	0.033
	(0.013)	(0.017)	(0.014)	(0.012)	(0.013)	(0.019)	(0.006)	(0.008)	(0.009)	(0.012)	(0.014)	(0.020)
Give to Same Cand. in Same State	-0.004	0.010	-0.021	0.023**	0.022*	0.008	0.038***	0.037***	0.032***	0.109***	0.092***	0.060
	(0.014)	(0.016)	(0.016)	(0.009)	(0.013)	(0.026)	(0.008)	(0.008)	(0.009)	(0.016)	(0.024)	(0.039)
Give to New Cand. in Same State	0.005	-0.005	-0.022***	0.016	0.012	0.018	0.005	-0.005	-0.021*	-0.035***	-0.036**	-0.027
	(0.008)	(0.008)	(0.008)	(0.011)	(0.013)	(0.020)	(0.005)	(0.007)	(0.010)	(0.009)	(0.016)	(0.024)
Give to New State	-0.002	-0.004	-0.001	0.001	0.003	0.006	0.000	0.004	0.008	-0.000	0.004	0.005
	(0.006)	(0.008)	(0.013)	(0.008)	(0.010)	(0.016)	(0.006)	(0.007)	(0.012)	(0.006)	(0.006)	(0.007)
Observations	671230	396592	175825	683118	405864	181402	619836	361184	177218	549817	326250	163895
Candidate Runs for Senate	0.025	0.087	0.101	-0.022	-0.009	-0.014	-0.022	-0.029	0.007	0.710***	0.641***	0.612***
	(0.038)	(0.054)	(0.086)	(0.027)	(0.037)	(0.025)	(0.033)	(0.037)	(0.016)	(0.070)	(0.110)	(0.191)
Observations	446	253	117	483	282	134	455	265	130	427	254	125

Notes: All regressions employ a triangular kernel. Each entry is the regression coefficient on whether the recipient candidate wins in time t. All other coefficients are excluded. Dependent variables for each regression are described in each row. Timing of past and future donations and bandwidths are given in the column headings. Robust standard errors, clustered at the state level, presented in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.13: Robustness: Discontinuity Estimates Using Alternative Bandwidths and Rectangular Kernel (Senate)

	$t-1$			$t+1$			$t+2$			$t+3$		
	10	5	2	10	5	2	10	5	2	10	5	2
Make Senate Contribution	0.000	-0.002	-0.034**	0.030***	0.039**	0.020	0.037***	0.030***	0.012	0.064***	0.061***	0.038*
	(0.014)	(0.016)	(0.015)	(0.009)	(0.017)	(0.024)	(0.007)	(0.011)	(0.015)	(0.009)	(0.014)	(0.019)
Give to Same Candidate	-0.006	0.001	-0.010	0.018**	0.033**	0.011	0.038***	0.042***	0.030***	0.108***	0.104***	0.065*
	(0.014)	(0.016)	(0.018)	(0.007)	(0.016)	(0.019)	(0.008)	(0.009)	(0.010)	(0.014)	(0.020)	(0.037)
Give to New Candidate	0.006	-0.002	-0.026*	0.016*	0.012	0.013	0.007	-0.002	-0.012	-0.033***	-0.033**	-0.020
	(0.009)	(0.011)	(0.014)	(0.008)	(0.009)	(0.019)	(0.007)	(0.010)	(0.016)	(0.009)	(0.012)	(0.020)
Make House Contribution	-0.004	-0.017**	-0.024*	-0.003	-0.009	-0.015	0.002	-0.004	-0.008	0.007	0.006	-0.002
	(0.005)	(0.008)	(0.013)	(0.006)	(0.008)	(0.014)	(0.005)	(0.005)	(0.010)	(0.004)	(0.005)	(0.006)
Give to Same State	0.002	0.002	-0.028*	0.031***	0.039**	0.025	0.039***	0.031***	0.012	0.070***	0.065***	0.044**
	(0.014)	(0.016)	(0.016)	(0.011)	(0.017)	(0.019)	(0.007)	(0.009)	(0.010)	(0.011)	(0.016)	(0.021)
Give to Same Cand. in Same State	-0.006	0.001	-0.010	0.019***	0.032*	0.011	0.038***	0.042***	0.030***	0.108***	0.104***	0.065*
	(0.014)	(0.016)	(0.018)	(0.007)	(0.016)	(0.019)	(0.008)	(0.009)	(0.010)	(0.014)	(0.020)	(0.037)
Give to New Cand. in Same State	0.009	0.002	-0.019*	0.014	0.010	0.015	0.006	-0.005	-0.015*	-0.036***	-0.038***	-0.022
	(0.008)	(0.008)	(0.009)	(0.011)	(0.011)	(0.021)	(0.005)	(0.006)	(0.009)	(0.008)	(0.012)	(0.023)
Give to New State	-0.001	-0.003	-0.008	0.002	0.003	-0.003	0.001	0.002	0.001	0.000	0.002	-0.000
	(0.005)	(0.008)	(0.013)	(0.007)	(0.009)	(0.016)	(0.005)	(0.007)	(0.012)	(0.005)	(0.007)	(0.009)
Observations	671230	396592	175825	683118	405864	181402	619836	361184	177218	549817	326250	163895
Candidate Runs for Senate	0.007	0.059	0.122	-0.033	-0.002	-0.003	-0.020	-0.027	-0.003	0.715***	0.660***	0.571***
	(0.034)	(0.046)	(0.075)	(0.025)	(0.037)	(0.046)	(0.030)	(0.045)	(0.033)	(0.060)	(0.091)	(0.178)
Observations	446	253	117	483	282	134	455	265	130	427	254	125

Notes: All regressions employ a rectangular kernel. Each entry is the regression coefficient on whether the recipient candidate wins in time t. All other coefficients are excluded. Dependent variables for each regression are described in each row. Timing of past and future donations and bandwidths are given in the column headings. Robust standard errors, clustered at the state level, presented in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.14: Robustness: Estimates Using Alternative Bandwidths and Triangular Kernel (Incumbency)

	(1) 10	(2) 5	(3) 2
Panel A: *Party Share*			
Party Win	0.118***	0.121***	0.071
	(0.027)	(0.039)	(0.057)
Constant	0.441***	0.440***	0.465***
	(0.014)	(0.019)	(0.028)
Panel B: *Party Repeat Share*			
Party Win	0.104***	0.097***	0.072***
	(0.011)	(0.015)	(0.023)
Constant	0.110***	0.115***	0.126***
	(0.005)	(0.008)	(0.012)
Panel C: *Party Total*			
Party Win	130.660***	160.808***	154.588**
	(30.357)	(43.250)	(69.397)
Constant	580.785***	556.264***	521.811***
	(38.724)	(46.706)	(67.166)
Panel D: *Party Repeat Total*			
Party Win	124.375***	118.609***	101.404***
	(14.515)	(19.534)	(32.199)
Constant	172.458***	180.364***	184.613***
	(14.997)	(19.621)	(28.574)
Observations	3004	1433	580
Elections	1502	717	290
Districts	831	477	242

Notes: All regressions employ a triangular kernel. Slope coefficients are excluded. Dependent variables for each regression are described in panel headings. Bandwidths given in column headings. Robust standard errors, clustered at the Congressional district level, presented in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.15: Robustness: Estimates Using Alternative Bandwidths and Rectangular Kernel (Incumbency)

	(1) 10	(2) 5	(3) 2
Panel A: *Party Share*			
Party Win	0.111***	0.137***	0.072
	(0.026)	(0.036)	(0.053)
Constant	0.444***	0.432***	0.465***
	(0.013)	(0.018)	(0.026)
Panel B: *Party Repeat Share*			
Party Win	0.108***	0.105***	0.075***
	(0.010)	(0.014)	(0.021)
Constant	0.108***	0.112***	0.122***
	(0.005)	(0.007)	(0.011)
Panel C: *Party Total*			
Party Win	117.860***	164.220***	110.876*
	(27.033)	(39.705)	(64.175)
Constant	590.165***	576.609***	490.417***
	(36.456)	(46.227)	(65.426)
Panel D: *Party Repeat Total*			
Party Win	129.379***	126.583***	81.544***
	(13.187)	(17.971)	(29.685)
Constant	166.737***	180.249***	168.582***
	(13.374)	(18.034)	(27.682)
Observations	3004	1433	580
Elections	1502	717	290
Districts	831	477	242

Notes: All regressions employ a rectangular kernel. Slope coefficients are excluded. Dependent variables for each regression are described in panel headings. Bandwidths given in column headings. Robust standard errors, clustered at the Congressional district level, presented in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix B

Social Comparisons in Peer Effects

B.1 Theoretical Appendix

Define $a_1^* = \text{argmax}_{a_1} \phi U(a_1, a_2, \ldots, a_N) - C(a_1, \omega_1 - \theta_1)$. Then, for all $i \in \{2, \ldots, N\}$

$$\frac{\partial a_1^*}{\partial a_i} = \frac{-\phi V''(E[\theta_1|a_1, a_i]) \frac{\partial E[\theta_1|a_1, a_i]}{\partial a_i}}{\phi V''(E[\theta_1|a_1, a_i]) \frac{\partial E[\theta_1|a_1, a_i]}{\partial a_1} - \frac{\partial^2 C}{\partial a_1^2}(a_1, \omega_1 - \theta_1)}$$

Proof.

$$max_{a_1} \phi U(a_2) - C(a_2, \omega_2 - \theta_2)$$

$$\vdots$$

$$max_{a_N} \phi U(a_N) - C(a_N, \omega_N - \theta_N)$$

$$max_{a_1} \phi U(a_1, a_2, \ldots, a_N) - C(a_1, \omega_1 - \theta_1)$$

This results in the first order conditions:

$$\phi\frac{\partial U}{\partial a_2}(a_2)-\frac{\partial C}{\partial a_2}(a_1,\omega_1-\theta_1)=0$$

$$\vdots$$

$$\phi\frac{\partial U}{\partial a_N}(a_N)-\frac{\partial C}{\partial a_N}(a_N,\omega_N-\theta_N)=0$$

$$\phi\frac{\partial U}{\partial a_1}(a_1,a_2,\dots,a_N)-\frac{\partial C}{\partial a_1}(a_1,\omega_1-\theta_1)=0$$

Rewrite these equations as $F_1(X,Y)=0,\dots,F_N(X,Y)=0,F_s(X,Y)=0$ where $X=(a_1,\dots,a_s)$ and $Y=(\theta_1,\omega_1,\dots,\theta_s,\omega_s)$. Further define $F(X,Y)=(F_1,\dots,F_s)$ and define $G(Y)$ such that $F(G(Y),Y)=(0,\dots,0)$ for all Y. Again define $A_x=F_X(X,Y)$ and $A_y=F_Y(X,Y)$.

$$A_x=\begin{bmatrix}\frac{\partial F_1}{\partial a_1} & \frac{\partial F_1}{\partial a_2} & \dots & \frac{\partial F_1}{\partial a_N}\\ 0 & \frac{\partial F_2}{\partial a_2} & \dots & 0\\ \vdots & \vdots & & \vdots\\ 0 & 0 & \dots & \frac{\partial F_N}{\partial a_N}\end{bmatrix} A_y=\begin{bmatrix}\frac{\partial F_1}{\partial \theta_1} & \frac{\partial F_1}{\partial \omega_1} & 0 & 0 & \dots & 0 & 0\\ 0 & 0 & \frac{\partial F_2}{\partial \theta_2} & \frac{\partial F_2}{\partial \omega_2} & \dots & 0 & 0\\ \vdots & \vdots & & & & \vdots & \vdots\\ 0 & 0 & 0 & 0 & \dots & \frac{\partial F_N}{\partial \theta_N} & \frac{\partial F_N}{\partial \omega_N}\end{bmatrix}$$

Now

$$A_x^{-1}=\begin{bmatrix}\left(\frac{\partial F_1}{\partial a_1}\right)^{-1} & -\frac{\partial F_1}{\partial a_2}\left(\frac{\partial F_2}{\partial a_2}\frac{\partial F_1}{\partial a_1}\right)^{-1} & \dots & -\frac{\partial F_s}{\partial a_N}\left(\frac{\partial F_N}{\partial a_N}\frac{\partial F_s}{\partial a_s}\right)^{-1}\\ 0 & \left(\frac{\partial F_2}{\partial a_2}\right)^{-1} & \dots & 0\\ \vdots & \vdots & & \vdots\\ 0 & 0 & \dots & \left(\frac{\partial F_N}{\partial a_N}\right)^{-1}\end{bmatrix}$$

$G'(Y) = -A_x^{-1} A_y =$

$$\begin{bmatrix} \left(\frac{\partial F_1}{\partial a_1}\right)^{-1} \frac{\partial^2 C}{\partial a_1 \partial \theta_1} & \left(\frac{\partial F_1}{\partial a_1}\right)^{-1} \frac{\partial^2 C}{\partial a_1 \partial \omega_1} & \frac{-\frac{\partial F_1}{\partial a_2} \frac{\partial^2 C}{\partial a_2 \partial \theta_2}}{\frac{\partial F_2}{\partial a_2} \frac{\partial F_1}{\partial a_1}} & \frac{-\frac{\partial F_1}{\partial a_2} \frac{\partial^2 C}{\partial a_2 \partial \omega_2}}{\frac{\partial F_2}{\partial a_2} \frac{\partial F_1}{\partial a_1}} & \cdots & \frac{-\frac{\partial F_1}{\partial a_N} \frac{\partial^2 C}{\partial a_N \partial \theta_N}}{\frac{\partial F_N}{\partial a_N} \frac{\partial F_1}{\partial a_1}} & \frac{-\frac{\partial F_1}{\partial a_N} \frac{\partial^2 C}{\partial a_N \partial \omega_N}}{\frac{\partial F_N}{\partial a_N} \frac{\partial F_1}{\partial a_1}} \\ 0 & 0 & \frac{\frac{\partial^2 C}{\partial a_2 \partial \theta_2}}{\frac{\partial F_2}{\partial a_2}} & \frac{\frac{\partial^2 C}{\partial a_2 \partial \omega_2}}{\frac{\partial F_2}{\partial a_2}} & \cdots & 0 & 0 \\ \vdots & \vdots & \vdots & \vdots & & \vdots & \vdots \\ 0 & 0 & 0 & 0 & \cdots & \frac{\frac{\partial^2 C}{\partial a_N \partial \theta_N}}{\frac{\partial F_N}{\partial a_N}} & \frac{\frac{\partial^2 C}{\partial a_N \partial \omega_N}}{\frac{\partial F_N}{\partial a_N}} \end{bmatrix}$$

Therefore,

$$\frac{\partial a_1^*}{\partial a_i} = \frac{-\phi V''\left(E[\theta_1|a_i,a_1]\right) \frac{\partial E[\theta_1|a_i,a_1]}{\partial a_i}}{\left(\phi V''\left(E[\theta_i|a_i]\right) \frac{\partial E[\theta_i|a_i]}{\partial a_i} - \frac{\partial^2 C}{\partial a_i^2}\right) * \left(\phi V''\left(E[\theta_1|a_i,a_1]\right) \frac{\partial E[\theta_1|a_i,a_1]}{\partial a_1} - \frac{\partial^2 C}{\partial a_1^2}\right)} \left(\frac{\partial^2 C}{\partial a_i \partial \theta_i} + \frac{\partial^2 C}{\partial a_i \partial \omega_i}\right) * \left(\frac{1}{\phi V''\left(E[\theta_i|a_i]\right) \frac{\partial E[\theta_i|a_i]}{\partial a_i} - \frac{\partial^2 C}{\partial a_i^2}(a_i, \omega_i - \theta_i)} \left(\frac{\partial^2 C}{\partial a_i \partial \theta_i} + \frac{\partial^2 C}{\partial a_i \partial \omega_i}\right)\right)^{-1}$$

$$\frac{\partial a_1^*}{\partial a_i} = -\frac{\phi V''\left(E[\theta_1|a_i,a_1]\right) \frac{\partial E[\theta_1|a_i,a_1]}{\partial a_i} \left(\phi V''\left(E[\theta_i|a_i]\right) \frac{\partial E[\theta_i|a_i]}{\partial a_i} - \frac{\partial^2 C}{\partial a_i^2}(a_i, \omega_i - \theta_i)\right)}{\left(\phi V''\left(E[\theta_i|a_i]\right) \frac{\partial E[\theta_i|a_i]}{\partial a_i} - \frac{\partial^2 C}{\partial a_i^2}\right) * \left(\phi V''\left(E[\theta_1|a_i,a_1]\right) \frac{\partial E[\theta_1|a_i,a_1]}{\partial a_1} - \frac{\partial^2 C}{\partial a_1^2}\right)}$$

Which yields

$$\frac{\partial a_1}{\partial a_i} = \frac{-\phi V''\left(E[\theta_1|a_1,a_i]\right) \frac{\partial E[\theta_1|a_1,a_i]}{\partial a_i}}{\phi V''\left(E[\theta_1|a_i,a_1]\right) \frac{\partial E[\theta_1|a_i,a_1]}{\partial a_1} - \frac{\partial^2 C}{\partial a_1^2}(a_1, \omega_1 - \theta_1)}$$

■

We know prove a_1^* increases in anticipation of social information. **Proof.**

Now the left hand side of 2.7 is larger than 2.6 by Jensen's inequality (since $L''' > 0$) if $\frac{\partial E[\theta_s|a_s,n+1]}{\partial a_s} - \frac{\partial E[\theta_s|a_s,n]}{\partial a_s} \leq 0$ (since $L'' < 0$) and $\sum_{n=1}^{N} P_n'(a_s) L\left(E\left[\theta_s|a_s,n\right]\right) > 0$.

We first establish that $\sum_{n=1}^{N} P_n'(a_s) L\left(E\left[\theta_s|a_s,n\right]\right) > 0$. Note that $\sum_{n=1}^{N} P_n'(a_s) = 0$. Therefore we can establish $\sum_{n=1}^{N} P_n'(a_s)L(E[\theta_s|a_s,n]) > 0$ by demonstrating that $\frac{\partial}{\partial a_s}\frac{P_{n+1}(a_s)}{P_n(a_s)} > 0$ and $L(E[\theta_s|a_s,n+1]) < L(E[\theta_s|a_s,n])$.

$$\frac{P_{n+1}(a_s)}{P_n(a_s)} = \frac{P_{n+1}(K_s(a_s))}{P_n(K_s(a_s))}$$

$$\frac{\partial}{\partial a_s}\frac{P_{n+1}(a_s)}{P_n(a_s)} = \frac{\partial}{\partial K_s}\frac{P_{n+1}(K_s(a_s))}{P_n(K_s(a_s))}\frac{\partial K_s}{\partial a_s}$$

Now,

$$\frac{P_{n+1}(K_s|\omega)}{P_n(K_s|\omega)} = \frac{F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}}{F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}} = \frac{F_\theta(\omega-K_s)}{1-F_\theta(\omega-K_s)}$$

.

And

$$\frac{\partial}{\partial K_s}\frac{P_{n+1}(K_s|\omega)}{P_n(K_s|\omega)} = \frac{-f_\theta(\omega-K_s)}{(1-F_\theta(\omega-K_s)^2} < 0$$

.

But if $\frac{\partial}{\partial K_s}\frac{P_{n+1}(K_s|\omega)}{P_n(K_s|\omega)} < 0$ for all ω then we know that $\frac{\partial}{\partial K_s}\frac{P_{n+1}(K_s)}{P_n(K_s)} < 0$, and $\frac{\partial}{\partial a_s}\frac{P_{n+1}(a_s)}{P_n(a_s)} = \frac{\partial}{\partial K_s}\frac{P_{n+1}(a_s)}{P_n(K_s(a_s))}\frac{\partial K_s}{\partial a_s} > 0$ because $\frac{\partial K_s}{\partial a_s} < 0$.

We next show that $E[\theta_s|a_s,n+1] > E[\theta|a_s,n]$.

$$E[\theta_s|a_s,n] = \theta_s - \omega + \frac{1}{\int_{\mathbb{R}} f_\theta(\omega-K_s)\Pi_{m<n}F_\theta(\omega-K_s)\Pi_{m>n}(1-F_\theta(\omega-K_s))f_\omega(\omega)d\omega} \int_{\mathbb{R}} \omega f_\theta(\omega-K_s,\omega-K_i)\Pi_{m<n}F_\theta(\omega-K_s)\Pi_{m>n}(1-F_\theta(\omega-K_s))f_\omega(\omega)d\omega$$

Then

$$E[\theta_s|a_s,n+1]-E[\theta_s|a_s,n]$$
$$=\frac{1}{\int_{\mathbb{R}} f_\theta(\omega-K_s)\Pi_{m<n+1}F_\theta(\omega-K_s)\Pi_{m>n+1}(1-F_\theta(\omega-K_s))f_\omega(\omega)d\omega}$$
$$\int_{\mathbb{R}}\omega f_\theta(\omega-K_s,\omega-K_i)\Pi_{m<n+1}F_\theta(\omega-K_s)\Pi_{m>n+1}(1-F_\theta(\omega-K_s))f_\omega(\omega)d\omega$$
$$-\frac{1}{\int_{\mathbb{R}} f_\theta(\omega-K_s)\Pi_{m<n}F_\theta(\omega-K_s)\Pi_{m>n}(1-F_\theta(\omega-K_s))f_\omega(\omega)d\omega}$$
$$\int_{\mathbb{R}}\omega f_\theta(\omega-K_s,\omega-K_i)\Pi_{m<n}F_\theta(\omega-K_s)\Pi_{m>n}(1-F_\theta(\omega-K_s))f_\omega(\omega)d\omega$$

But

$$\frac{1}{\int_{\mathbb{R}} f_\theta(\omega-K_s)\Pi_{m<n+1}F_\theta(\omega-K_s)\Pi_{m>n+1}(1-F_\theta(\omega-K_s))f_\omega(\omega)d\omega}$$
$$\int_{\mathbb{R}} f_\theta(\omega-K_s,\omega-K_i)\Pi_{m<n+1}F_\theta(\omega-K_s)\Pi_{m>n+1}(1-F_\theta(\omega-K_s))f_\omega(\omega)d\omega$$
$$=\frac{1}{\int_{\mathbb{R}} f_\theta(\omega-K_s)\Pi_{m<n}F_\theta(\omega-K_s)\Pi_{m>n}(1-F_\theta(\omega-K_s))f_\omega(\omega)d\omega}$$
$$\int_{\mathbb{R}} f_\theta(\omega-K_s,\omega-K_i)\Pi_{m<n}F_\theta(\omega-K_s)\Pi_{m>n}(1-F_\theta(\omega-K_s))f_\omega(\omega)d\omega$$

So the sign of $E[\theta_s|K_s,n+1]-E[\theta_s|K_s,n]$ can be judged by which values of ω receive or lose relative probability density.

Abusing notation,

$$Pr[\omega_s|K_s,n+1]-Pr[\omega_s|K_s,n]$$
$$=\frac{f_\theta(\omega-K_s)\Pi_{m<n+1}F_\theta(\omega-K_s)\Pi_{m>n+1}(1-F_\theta(\omega-K_s))f_\omega(\omega)}{\int_{\mathbb{R}} f_\theta(\omega-K_s)\Pi_{m<n+1}F_\theta(\omega-K_s)\Pi_{m>n+1}(1-F_\theta(\omega-K_s))f_\omega(\omega)d\omega}$$
$$-\frac{f_\theta(\omega-K_s)\Pi_{m<n}F_\theta(\omega-K_s)\Pi_{m>n}(1-F_\theta(\omega-K_s))f_\omega(\omega)}{\int_{\mathbb{R}} f_\theta(\omega-K_s)\Pi_{m<n}F_\theta(\omega-K_s)\Pi_{m>n}(1-F_\theta(\omega-K_s))f_\omega(\omega)d\omega}$$

$$Pr[\omega_s|K_s,n+1]-Pr[\omega_s|K_s,n]$$
$$=\frac{f_\theta(\omega-K_s)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega)}{\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega)d\omega} - \frac{f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}f_\omega(\omega)}{\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}f_\omega(\omega)d\omega}$$

$$Pr[\omega_s|K_s,n+1]-Pr[\omega_s|K_s,n]>0$$
$$\iff f_\theta(\omega-K_s)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega)$$
$$-\frac{\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega)d\omega}{\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}f_\omega(\omega)d\omega}$$
$$\times f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}f_\omega(\omega)>0$$

$$Pr[\omega_s|K_s,n+1]-Pr[\omega_s|K_s,n]>0$$
$$\iff f_\omega(\omega)f_\theta(\omega-K_s)$$
$$\left[F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}-C*F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}\right]>0$$

Where $C=\frac{\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega)d\omega}{\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}f_\omega(\omega)d\omega}$

$$Pr[\omega_s|K_s, n+1] - Pr[\omega_s|K_s, n] > 0$$

$$\iff f_\omega(\omega) f_\theta(\omega - K_s)$$

$$\left[F_\theta(\omega - K_s)^{n-1}(1 - F_\theta(\omega - K_s))^{N-n-1}(F_\theta(\omega - K_s) - C + CF_\theta(\omega - K_s))\right] > 0$$

$$Pr[\omega_s|K_s, n+1] - Pr[\omega_s|K_s, n] > 0$$

$$\iff f_\omega(\omega) f_\theta(\omega - K_s)$$

$$\left[F_\theta(\omega - K_s)^{n-1}(1 - F_\theta(\omega - K_s))^{N-n-1}((1+C)F_\theta(\omega - K_s) - C)\right] > 0$$

Therefore, iff $\omega > F_\theta^{-1}(\frac{C}{1-C}) + K_s$ it receives greater probability weight under rank $n+1$ than under rank n. The opposite is true for $\omega < F_\theta^{-1}(\frac{C}{1-C} + K_s$. Therefore, $E[\theta_s|K_s(a_s), n+1] > E[\theta_s|K_s(a_s), n]$.

Lastly, we will demonstrate $\frac{\partial E[\theta_s|a_s, n+1]}{\partial a_s} - \frac{\partial E[\theta_s|a_s, n]}{\partial a_s} \leq 0$. **Note:** Through all of this we are assuming $K_s(a_s)$ is not rank dependent because a_s is chosen prior to the rank being revealed. Further, K_s is observable to the decision maker at the moment he is making his decision through the marginal cost of his action.

$$\frac{\partial E[\theta_s|K_s, n]}{\partial K_s} \;=\; 1 \;+\; \frac{\partial}{\partial K_s} \frac{\int_{\mathbb{R}} \omega f_\theta(\omega - K_s) F_\theta(\omega - K_s)^{n-1}(1 - F_\theta(\omega - K_s))^{N-n} f_\omega(\omega) d\omega}{\int_{\mathbb{R}} f_\theta(\omega - K_s) F_\theta(\omega - K_s)^{n-1}(1 - F_\theta(\omega - K_s))^{N-n} f_\omega(\omega) d\omega}$$

$$\frac{\partial E[\theta_s|K_s,n+1]}{\partial K_s} = 1 + \frac{\partial}{\partial K_s}\frac{\int_{\mathbb{R}} \omega f_\theta(\omega-K_s)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega)d\omega}{\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega)d\omega}$$

$$\begin{aligned}
&\frac{\partial E[\theta_s|K_s,n+1]}{\partial K_s} - \frac{\partial E[\theta_s|K_s,n]}{\partial K_s}\\
&\qquad = \frac{\partial}{\partial K_s}\frac{\int_{\mathbb{R}} \omega f_\theta(\omega-K_s)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega)d\omega}{\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega)d\omega}\\
&\qquad\ - \frac{\partial}{\partial K_s}\frac{\int_{\mathbb{R}} \omega f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}f_\omega(\omega)d\omega}{\int_{\mathbb{R}} f_\theta(\omega_i-K_s)f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}f_\omega(\omega)d\omega}
\end{aligned}$$

But

$$\begin{aligned}
&\frac{\partial}{\partial K_s}\frac{\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega)d\omega}{\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega)d\omega}\\
&\qquad = \frac{\partial}{\partial K_s}\frac{\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}f_\omega(\omega)d\omega}{\int_{\mathbb{R}} f_\theta(\omega_i-K_s)f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}f_\omega(\omega)d\omega}
\end{aligned}$$

So we consider probability weight given an ω:

$$
\begin{aligned}
&\frac{\partial}{\partial K_s}\frac{f_\theta(\omega-K_s)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega)d\omega}{\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega)d\omega} \\
&\qquad= \frac{-1}{\left(\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega)d\omega\right)^2} \\
&\qquad\Bigg(\bigg[F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}\left[f_\theta'(\omega_i-K_s)f_\theta(\omega-K_s)f_\omega(\omega)\right] \\
&\qquad\quad+nF_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n-1}\left[f_\theta(\omega-K_s)^2f_\omega(\omega)\right] \\
&\qquad-(N-n-1)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-2}\left[f_\theta(\omega-K_s)^2f_\omega(\omega)\right]\bigg] \\
&\qquad\quad\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega)d\omega \\
&\qquad\quad-f_\theta(\omega-K_s)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega) \\
&\qquad\int_{\mathbb{R}}\bigg[F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}\left[f_\theta'(\omega_i-K_s)f_\theta(\omega-K_s)f_\omega(\omega)\right] \\
&\qquad\quad+nF_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n-1}\left[f_\theta(\omega-K_s)^2f_\omega(\omega)\right] \\
&\qquad\quad-(N-n-1)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-2}\left[f_\theta(\omega-K_s)^2f_\omega(\omega)\right]\bigg]\Bigg)
\end{aligned}
$$

$$
\begin{aligned}
&\frac{\partial}{\partial K_s}\frac{f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}f_\omega(\omega)d\omega}{\int_{\mathbb{R}} f_\theta(\omega_i-K_s)f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}f_\omega(\omega)d\omega}\\
&\qquad\qquad=\frac{-1}{\left(\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}f_\omega(\omega)d\omega\right)^2}\\
&\qquad\qquad\Bigg(\Bigg[F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}\left[f'_\theta(\omega_i-K_s)f_\theta(\omega-K_s)f_\omega(\omega)\right]\\
&\qquad\qquad\quad+(n-1)F_\theta(\omega-K_s)^{n-2}(1-F_\theta(\omega-K_s))^{N-n}\left[f_\theta(\omega-K_s)^2f_\omega(\omega)\right]\\
&\qquad\qquad-(N-n)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n-1}\left[f_\theta(\omega-K_s)^2f_\omega(\omega)\right]\Bigg]\\
&\qquad\qquad\quad\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}f_\omega(\omega)d\omega\\
&\qquad\qquad\quad-f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}f_\omega(\omega)\\
&\qquad\qquad\int_{\mathbb{R}}\Bigg[F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}\left[f'_\theta(\omega_i-K_s)f_\theta(\omega-K_s)f_\omega(\omega)\right]\\
&\qquad\qquad\quad+(n-1)F_\theta(\omega-K_s)^{n-2}(1-F_\theta(\omega-K_s))^{N-n}\left[f_\theta(\omega-K_s)^2f_\omega(\omega)\right]\\
&\qquad\qquad\quad-(N-n)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n-1}\left[f_\theta(\omega-K_s)^2f_\omega(\omega)\right]\Bigg]d\omega\Bigg)
\end{aligned}
$$

Then

$$
\begin{aligned}
&\frac{\partial Prob[\theta_s|K_s,n+1]}{\partial K_s} - \frac{\partial Prob[\theta_s|K_s,n]}{\partial K_s} > 0 \\
&\iff \frac{1}{\left(\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}f_\omega(\omega)d\omega\right)^2} \\
&\Bigg(\Bigg[F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}\left[f'_\theta(\omega_i-K_s)f_\theta(\omega-K_s)f_\omega(\omega)\right] \\
&+(n-1)F_\theta(\omega-K_s)^{n-2}(1-F_\theta(\omega-K_s))^{N-n}\left[f_\theta(\omega-K_s)^2f_\omega(\omega)\right] \\
&-(N-n)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n-1}\left[f_\theta(\omega-K_s)^2f_\omega(\omega)\right]\Bigg] \\
&\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}f_\omega(\omega)d\omega \\
&-f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}f_\omega(\omega) \\
&\int_{\mathbb{R}}\Bigg[F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}\left[f'_\theta(\omega_i-K_s)f_\theta(\omega-K_s)f_\omega(\omega)\right] \\
&+(n-1)F_\theta(\omega-K_s)^{n-2}(1-F_\theta(\omega-K_s))^{N-n}\left[f_\theta(\omega-K_s)^2f_\omega(\omega)\right] \\
&-(N-n)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n-1}\left[f_\theta(\omega-K_s)^2f_\omega(\omega)\right]\Bigg]d\omega\Bigg) \\
&-\frac{1}{\left(\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n}(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega)d\omega\right)^2} \\
&\Bigg(\Bigg[F_\theta(\omega-K_s)^{n}(1-F_\theta(\omega-K_s))^{N-n-1}\left[f'_\theta(\omega_i-K_s)f_\theta(\omega-K_s)f_\omega(\omega)\right] \\
&+nF_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n-1}\left[f_\theta(\omega-K_s)^2f_\omega(\omega)\right] \\
&-(N-n-1)F_\theta(\omega-K_s)^{n}(1-F_\theta(\omega-K_s))^{N-n-2}\left[f_\theta(\omega-K_s)^2f_\omega(\omega)\right]\Bigg] \\
&\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n}(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega)d\omega \\
&-f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n}(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega) \\
&\int_{\mathbb{R}}\Bigg[F_\theta(\omega-K_s)^{n}(1-F_\theta(\omega-K_s))^{N-n-1}\left[f'_\theta(\omega_i-K_s)f_\theta(\omega-K_s)f_\omega(\omega)\right] \\
&+nF_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n-1}\left[f_\theta(\omega-K_s)^2f_\omega(\omega)\right] \\
&-(N-n-1)F_\theta(\omega-K_s)^{n}(1-F_\theta(\omega-K_s))^{N-n-2}\left[f_\theta(\omega-K_s)^2f_\omega(\omega)\right]\Bigg]d\omega\Bigg) > 0
\end{aligned}
$$

$$\iff \frac{F_\theta(\omega-K_s)^{n-2}(1-F_\theta(\omega-K_s))^{N-n-1}}{\left(\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n} f_\omega(\omega)d\omega\right)^2}$$

$$\Bigg(\Bigg[F_\theta(\omega-K_s)(1-F_\theta(\omega-K_s))\left[f'_\theta(\omega_i-K_s)f_\theta(\omega-K_s)f_\omega(\omega)\right]$$

$$+(n-1)(1-F_\theta(\omega-K_s))\left[f_\theta(\omega-K_s)^2 f_\omega(\omega)\right]$$

$$-(N-n)F_\theta(\omega-K_s)\left[f_\theta(\omega-K_s)^2 f_\omega(\omega)\right]\Bigg]$$

$$\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n} f_\omega(\omega)d\omega$$

$$-f_\theta(\omega-K_s)F_\theta(\omega-K_s)(1-F_\theta(\omega-K_s))f_\omega(\omega)$$

$$\int_{\mathbb{R}}\Bigg[F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n}\left[f'_\theta(\omega_i-K_s)f_\theta(\omega-K_s)f_\omega(\omega)\right]$$

$$+(n-1)F_\theta(\omega-K_s)^{n-2}(1-F_\theta(\omega-K_s))^{N-n}\left[f_\theta(\omega-K_s)^2 f_\omega(\omega)\right]$$

$$-(N-n)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n-1}\left[f_\theta(\omega-K_s)^2 f_\omega(\omega)\right]\Bigg]d\omega\Bigg)$$

$$-\frac{F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n-2}}{\left(\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n}(1-F_\theta(\omega-K_s))^{N-n-1} f_\omega(\omega)d\omega\right)^2}$$

$$\Bigg(\Bigg[F_\theta(\omega-K_s)(1-F_\theta(\omega-K_s))\left[f'_\theta(\omega_i-K_s)f_\theta(\omega-K_s)f_\omega(\omega)\right]$$

$$+n(1-F_\theta(\omega-K_s))\left[f_\theta(\omega-K_s)^2 f_\omega(\omega)\right]$$

$$-(N-n-1)F_\theta(\omega-K_s)\left[f_\theta(\omega-K_s)^2 f_\omega(\omega)\right]\Bigg]$$

$$\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n}(1-F_\theta(\omega-K_s))^{N-n-1} f_\omega(\omega)d\omega$$

$$-f_\theta(\omega-K_s)F_\theta(\omega-K_s)(1-F_\theta(\omega-K_s))f_\omega(\omega)$$

$$\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n}(1-F_\theta(\omega-K_s))^{N-n-1} f_\omega(\omega)d\omega$$

$$-f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n}(1-F_\theta(\omega-K_s))^{N-n-1} f_\omega(\omega)$$

$$\int_{\mathbb{R}}\Bigg[F_\theta(\omega-K_s)^{n}(1-F_\theta(\omega-K_s))^{N-n-1}\left[f'_\theta(\omega_i-K_s)f_\theta(\omega-K_s)f_\omega(\omega)\right]$$

$$+nF_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n-1}\left[f_\theta(\omega-K_s)^2 f_\omega(\omega)\right]$$

$$-(N-n-1)F_\theta(\omega-K_s)^{n}(1-F_\theta(\omega-K_s))^{N-n-2}\left[f_\theta(\omega-K_s)^2 f_\omega(\omega)\right]\Bigg]d\omega\Bigg)>0$$

$$
\begin{aligned}
\iff 1 > \frac{F_\theta(\omega-K_s)}{1-F_\theta(\omega-K_s)} &\times \frac{\left(\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n} f_\omega(\omega)d\omega\right)^2}{\left(\int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^{n}(1-F_\theta(\omega-K_s))^{N-n-1} f_\omega(\omega)d\omega\right)^2} \\
&\times \Bigg(\Bigg[F_\theta(\omega-K_s)(1-F_\theta(\omega-K_s))\left[f'_\theta(\omega_i-K_s)f_\theta(\omega-K_s)f_\omega(\omega)\right] \\
&\quad + n(1-F_\theta(\omega-K_s))\left[f_\theta(\omega-K_s)^2 f_\omega(\omega)\right] \\
&\quad - (N-n-1)F_\theta(\omega-K_s)\left[f_\theta(\omega-K_s)^2 f_\omega(\omega)\right]\Bigg] \\
&\quad \int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega)d\omega \\
&\quad - f_\theta(\omega-K_s)F_\theta(\omega-K_s)(1-F_\theta(\omega-K_s))f_\omega(\omega) \\
&\quad \int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega)d\omega \\
&\quad - f_\theta(\omega-K_s)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega) \\
&\quad \int_{\mathbb{R}} \Bigg[F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}\left[f'_\theta(\omega_i-K_s)f_\theta(\omega-K_s)f_\omega(\omega)\right] \\
&\quad + nF_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n-1}\left[f_\theta(\omega-K_s)^2 f_\omega(\omega)\right] \\
&\quad - (N-n-1)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-2}\left[f_\theta(\omega-K_s)^2 f_\omega(\omega)\right]\Bigg]d\omega\Bigg)^{-1} \\
&\times \Bigg(\Bigg[F_\theta(\omega-K_s)(1-F_\theta(\omega-K_s))\left[f'_\theta(\omega_i-K_s)f_\theta(\omega-K_s)f_\omega(\omega)\right] \\
&\quad + n(1-F_\theta(\omega-K_s))\left[f_\theta(\omega-K_s)^2 f_\omega(\omega)\right] \\
&\quad - (N-n-1)F_\theta(\omega-K_s)\left[f_\theta(\omega-K_s)^2 f_\omega(\omega)\right]\Bigg] \\
&\quad \int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega)d\omega \\
&\quad - f_\theta(\omega-K_s)F_\theta(\omega-K_s)(1-F_\theta(\omega-K_s))f_\omega(\omega) \\
&\quad \int_{\mathbb{R}} f_\theta(\omega-K_s)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega)d\omega \\
&\quad - f_\theta(\omega-K_s)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}f_\omega(\omega) \\
&\quad \int_{\mathbb{R}} \Bigg[F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-1}\left[f'_\theta(\omega_i-K_s)f_\theta(\omega-K_s)f_\omega(\omega)\right] \\
&\quad + nF_\theta(\omega-K_s)^{n-1}(1-F_\theta(\omega-K_s))^{N-n-1}\left[f_\theta(\omega-K_s)^2 f_\omega(\omega)\right] \\
&\quad - (N-n-1)F_\theta(\omega-K_s)^n(1-F_\theta(\omega-K_s))^{N-n-2}\left[f_\theta(\omega-K_s)^2 f_\omega(\omega)\right]\Bigg]d\omega\Bigg)
\end{aligned}
$$

Since $\frac{F_\theta(\omega-K_s)}{1-F_\theta(\omega-K_s)}$ is the only term in the expression where ω is neither integrated out or identically placed in the numerator and denominator, we know that $\frac{\partial Prob[\theta_s|K_s,n+1]}{\partial K_s} - \frac{\partial Prob[\theta_s|K_s,n]}{\partial K_s} > 0$ holds for smaller values of ω. And as such $\frac{\partial E[\theta_s|a_s,n+1]}{\partial a_s} - \frac{\partial E[\theta_s|a_s,n]}{\partial a_s} \leq 0$.

■

B.2 Results Appendix

Table B.1: Small Sample t-test: Effect of High Signal on Task Choice

	Task Difference
High Signal	9.608^{**}
	(3.357)
N High Signal	51
N Low Signal	4
N Overall	55

Notes: Standard errors in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.2: Effect of Treatment on Belief of Received Signal

	DV: $1\{\text{Believe Signal}\}$
Treatment	0.341^{***}
	(0.078)
Constant	0.531^{***}
	(0.063)
R-Squared	0.135
Observations	119

Notes: Standard errors in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.3: Quantile Regressions

	DV: Task Choice				
	(1)	(2)	(3)	(4)	(5)
Quantiles	10th	30th	50th	70th	90th
Treatment	8.000**	12.000***	0.000	0.000	5.000
	(3.761)	(2.967)	(3.045)	(3.376)	(6.777)
Constant	2.000	8.000***	20.000***	25.000***	40.000***
	(1.981)	(2.046)	(2.365)	(2.621)	(5.485)
R-Squared	0.050	0.050	.	.	0.050
Observations	119	119	119	119	119

Notes: Standard errors in parantheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

B.3 Experiment Instructions

All experiment sessions were run using oTree. The instructions were shown on a step-by-step basis with each feature of the experiment explained on a sequence of screens.

Instructions: Anticipation Experiment

Screen 1

Hello and welcome, today you will be participating in an experiment on economic decision making. Funds for this experiment have been provided by the University of California. You will be paid for your participation in this experiment. Your final payment will consist of a fixed $5 show-up payment and a $10 completion bonus. You will be paid privately in cash after the experiment has concluded. We ask that you please silence and put away all cell phones and any other personal electronic devices now and for the remainder of the experiment.

In this experiment you will be presented with an opportunity to complete a task for charity. For this project, we are partnering with Dr. James Rolfe – founder of the Afghan Dental Relief Project – to bring modern dental care to the very poor in Afghanistan. The A.D.R.P. is a charitable organization that provides free dental services and dental health education to the poorest families and individuals in the city of Kabul, the capital of Afghanistan. 100% of the donations that you generate from this experiment will be used to purchase dental supplies and ship them to A.D.R.P.'s free dental clinic. Please take the time to carefully read the news article below that details the work Dr. Rolfe and the A.D.R.P. have accomplished and can continue to accomplish with your help.

Screen 2

Today, you will have the opportunity to complete tasks to generate donations to A.D.R.P.'s free dental clinic. The task will be to transcribe captchas. For each task, you will be shown an image of text. Your objective is to correctly type the text that is shown to you in the space provided. Each correctly transcribed captcha will be counted as one completed task. You will have 3 chances to correctly transcribe each captcha. If you fail to correctly transcribe a captcha within three tries you will simply be shown a different captcha. You will now be shown 5 captchas to transcribe so that you are familiar with the task. Please note that each captcha may consist of both capital and lower case letters, the numbers 2-9, and special characters !, %, &, ?.

Screen 3

5 Sample Captchas

Screen 4: Tasks for Donations

For this experiment you will decide how many captchas you are willing to complete in order to generate donations for the A.D.R.P.'s free dental clinic. That is, you will choose the maximum number of captchas you are willing to do for donations of $2, $5, $10, $15, and $20. You will be able to select between 0 and 50 tasks for each possible donation amount. For example, if for a donation of $5 dollars you select 12 tasks, then you are indicating that you are willing to do a maximum of 12 tasks in exchange for a $5 donation to the A.D.R.P.

After you have made your decisions we will randomly assign each person a donation amount and a number of tasks. If the the number of tasks you are assigned is fewer than the maximum number you selected for yourself, then you will complete your assigned number of tasks and we will give the assigned donation to the A.D.R.P. If the randomly assigned number of tasks is higher than your chosen number, then you will complete no tasks and no donation will be given to the A.D.R.P.

For example, suppose that for a donation of $15 you choose to do at most 30 tasks. Then if you are randomly assigned $15 and 20 tasks, then you will complete 20 tasks and $15 will be given to the Afghan Dental Relief Project. On the other hand if you are randomly assigned $15 and 40 tasks, then you will do 0 tasks and no donation will be given to the A.D.R.P.

Each person will be assigned their own number of tasks and their own donation amount. No one will learn your assigned donation amount or number of tasks. Please note that the randomization device is "fair" – all possible donation and task amounts will have equal probability of being assigned. So it is in your best interest to treat each decision as if it is the one that counts.

You will be free to leave the experiment as soon as you complete your tasks. You will be free to go immediately if you have no tasks to complete. Your completion bonus will be $10 regardless of the number of tasks you have to do.

Comprehension Questions

Suppose you choose to do a maximum of 30 tasks for a $10 donation. Under which of the following conditions would you complete tasks for a $10 donation?

- If I am randomly assigned a $10 donation and 20 tasks.
- If I am randomly assigned a $10 donation and 40 tasks.

Suppose you are assigned a $10 donation and 21 tasks. How many tasks will you be asked to do?

- 21 if I choose more than 21 tasks for a $10 donation.
- 21 if I choose fewer than 21 tasks for a $10 donation.

Suppose you are assigned a $5 donation and 15 tasks. How many tasks will you be assigned to do?

- 0 if I choose more than 15 tasks for a $10 donation.
- 0 if I choose fewer than 15 tasks for a $10 donation.

Screen 5: Guessing other's choices

In addition to selecting how many tasks you are willing to do for each possible donation amount, you will guess how your decisions compare to the decisions of previous participants.

That is, you will guess what percent of previous participants were willing to do a larger number of tasks than you for each possible donation. Specifically, you will say whether you believe that at least 95%, 75%, 50%, 25%, or 0% of all previous participants were willing to do more tasks than you.

As an example, suppose that for a donation of $5 dollars you choose to do a maximum of 12 tasks and think that at least 50%, but fewer than 75%, of previous participants were willing to do more than 12 tasks for a $5 donation. Then you will indicate that you believe that at least 50% of all previous experiment participants were willing to do more tasks than you for a $5 donation.

Comprehension Question

Suppose that you choose to do at most 10 tasks for a donation of $15 dollars and that you believe that at least 75%, but fewer than 95%, of previous participants were willing to do more than 10 tasks for a $15 donation. How would you answer the question "What percent of previous participants were willing to do more tasks that you?"

- 95%
- 75%
- 50%
- 25%
- 0%

Screen 5B: Seeing other's choices (Treatment Only)

Lastly, you will learn how your decisions actually compare to the decisions of all those who participated in this experiment before you. After everyone has made their decisions and guesses, you will open the yellow envelope on your desk. The contents of this envelope will show you how many tasks at least 95%, 75%, 50%, 25% and 0% of all previous participants willing to do. Therefore, you will learn the true proportion of previous participants that were willing to do more tasks than you. You will then verify that you understand this information by answering a series of multiple choice questions. Please do not open these envelopes until you are instructed to do so.

For example, suppose for a donation of $5 dollars you were willing to do 12 tasks. And suppose that the information in the yellow envelope shows you that 50% of previous participants were willing to do at least 15 tasks for a $5 donation and 75% were willing to do at least 10 tasks for $5 donation. This would mean that at least 50%, but less than 75%, of previous participants were willing to do more tasks than you for a $5 donation.

Comprehension Question

Suppose that you choose to do at most 35 tasks for a donation of $15 dollars. Also suppose you learn from the information in the yellow envelope that 50% percent of other participants were willing to at least 30 tasks for a $15 donation and 25% were willing to at least 40 tasks for $15 donation. Then what percent of previous experiment participants were actually willing to do more tasks than you?

- 95%
- 75%
- 50%
- 25%
- 0%

Screen 6: Summary

To sum up, the experiment proceeds as follows.

Step 1 You will choose how many tasks you are willing to do for each possible donation amount and you will guess what percent of people were willing to do more tasks than you for each of your decisions. You will do this with sliders and multiple choice questions as shown below. *In Baseline:*In this session – as with 50% of all sessions – you *will not* learn any information on what others actually chose. (*In Treatment:*In this session – as with 50% of all sessions – you *will* learn any information on what others actually chose.)

Step 2 Each person will be randomly assigned a donation amount by drawing a ball out of a jar. A random number generator will then assign you a number of tasks. If this number of tasks is lower than the amount you were willing to do, then you will complete those tasks and we will donate to the A.D.R.P. on your behalf. The experiment will conclude after you finish your tasks (if any). We will hand you your show-up fee and completion bonus ($15) as you exit the lab.

Instructions: Signal Extraction

Screen 1

Hello and welcome, today you will be participating in an experiment on economic decision making. Funds for this experiment have been provided by the University of California. You will be paid for your participation in this experiment. Your final payment will consist of a fixed $5 show-up payment and a $10 completion bonus. You will be paid privately in cash after the experiment has concluded. We ask that you please silence and put away all cell phones and any other personal electronic devices now and for the remainder of the experiment.

In this experiment you will be presented with an opportunity to complete a task for charity. For this project, we are partnering with Dr. James Rolfe – founder of the Afghan Dental Relief Project – to bring modern dental care to the very poor in Afghanistan. The A.D.R.P. is a charitable organization that provides free dental services and dental health education to the poorest families and individuals in the city of Kabul, the capital of Afghanistan. 100% of the donations that you generate from this experiment will be used to purchase dental supplies and ship them to A.D.R.P.'s free dental clinic. Please take the time to carefully read the news article below that details the work Dr. Rolfe and the A.D.R.P. have accomplished and can continue to accomplish with your help.

Screen 2

Today, you will have the opportunity to complete tasks to generate donations to A.D.R.P.'s free dental clinic. The task will be to transcribe captchas. For each task, you will be shown an image of text. Your objective is to correctly type the text that is shown to you in the space provided. Only correctly transcribed captchas will be counted as completed tasks. You will have 3 chances per captcha. You will now be shown 5 captchas to transcribe so that you are familiar with the task. Please note that each captcha may consist of both capital and lower case letters, only the numbers 2-9, and special characters !, %, &, ?.

Screen 3

5 Sample Captchas

Screen 4: Tasks for Donations

For this experiment you will decide how many captchas you are willing to complete in order to generate a donation for the A.D.R.P.'s free dental clinic. That is, you will choose the maximum number of captchas you are willing to complete in exchange for a donation of $20. You will be able to select between 0 and 50 tasks.

After you have made your decisions we will randomly assign each person a number of tasks. If the the number of tasks you are assigned is fewer than the maximum number you selected for yourself, then you will complete your assigned number of tasks and we will donate to A.D.R.P. on your behalf. If the randomly assigned number of tasks is higher than your chosen number, then you will complete no tasks and no donation will be given to the A.D.R.P.

For example, suppose that you choose to do at most 25 tasks. Then if you are randomly assigned 20 tasks, then you will complete 20 tasks and $20 will be given to the Afghan Dental Relief Project. On the other hand if you are randomly assigned 40 tasks, then you will do 0 tasks and no donation will be given to the A.D.R.P.

Each person will be assigned their own number of tasks. No one will learn your assigned number of tasks. Please note that the randomization device is "fair" – all possible task amounts will have equal probability of being assigned.

You will be free to leave the experiment as soon as you complete your tasks. You will be free to go immediately if you have no tasks to complete. Your completion bonus will be $10 regardless of the number of tasks you have to do.

Identical Comprehension Questions for this section as the Anticipation Experiment

Screen 5: Seeing Other's Choices

In addition to making your own choices, you will receive information about how your decisions compare to the decisions of the over 200 UCSD students who have already participated in this experiment. Before making your decisions, you will each draw an envelope at random. In each of the envelopes will be one of the two following statements on how many tasks previous participants were willing to complete in exchange for a $20 donation to the A.D.R.P. The statements you could receive are:

- More than 50% of all previous participants were willing to do at least 20 tasks.
- Less than 25% of all previous participants were willing to do at least 20 tasks.

In Baseline: Only one of these statements is true. There are a total of 16 available envelopes at the front of the room. 8 of these envelopes contain the true statement and 8 envelopes contain the false statement. Therefore, when you pick an envelope, there will be a 50% chance that you will receive the true statement and a 50% chance that you will receive the false statement.

In Treatment: Only one of these statements is true. There are a total of 16 available envelopes at the front of the room. 15 of these envelopes contain the true statement and 1 envelope contains the false statement. Therefore, when you pick an envelope, there will be a 94% chance that you will receive the true statement and a 6% chance that you will receive the false statement.

Comprehension Questions, Set A

If 8-out-of-16 (*In Treatment:*15-out-of-16) available envelopes contain the true statement, which of the following is most likely going to happen?

- I will receive the true statement.
- I will receive the false statement.
- I will receive the true or false statement with equal probability.

Suppose that you receive the statement that "more than 50% of all previous participants were willing to do at least 20 tasks." What is the probability that this statement is TRUE and that *more than 50%* of all previous participants were willing to do at least 20 tasks?

- 94%
- 50%

Suppose that you receive the statement that "more than 50% of all previous participants were willing to do at least 20 tasks." What is the probability that this statement is FALSE and that *less than 25%* of all previous participants were willing to do at least 20 tasks?

- 50%
- 6%

Comprehension Questions, Set B

If 8-out-of-16 (*in treatment:*15-out-of-16) available envelopes contain the true statement, what is the probability you will receive a true statement?

- 94%
- 50%

If 8-out-of-16 (*in treatment:*15-out-of-16) available envelopes contain the true statement, what is the probability you will receive a false statement?

- 50%
- 6%

If 8-out-of-16 (*in treatment:*15-out-of-16) available envelopes contain the true statement, what is the maximum number of true statements 12 participants could pick?

- 8
- 12

If 8-out-of-16 (*in treatment:*15-out-of-16) available envelopes contain the true statement, what is the maximum number of false statements 12 participants could pick?

- 8
- 1

We switched from comprehension question set B to A after the first few sessions of the signal extraction experiment. We believed set A would be more revealing of subject comprehension. We found no difference in task choice dynamics between comprehension set A and B.

B.3.1 News Article about the A.D.R.P.

Afghan Dental Relief Project Ready for Next Level[1]

By Kelsey Abkin

In 2003, Santa Barbara dentist James Rolfe came across an article about three women going to Afghanistan to treat victims of PTSD (post-traumatic stress disorder). Instead of putting the article down and going on with his day, Dr. Rolfe picked up the phone and asked to go along. The poverty and vulnerability he saw upon arriving in Afghanistan affected him immediately. Within a country of some 30 million people, Rolfe said he saw "resources in manpower and [natural] resources but no infrastructure to really use it." Eleven years later, thanks to Rolfe and his Afghanistan Dental Relief Project, this is changing. The project started the Kabul Dental Clinic and Training Center, which offers basic dental services, and Rolfe is on the verge of expanding to a permanent dental clinic, hoping to extend nonbasic, often life-saving dental procedures.

The mission began when Rolfe returned to Afghanistan, this time with a homemade, portable dentists office and base camp. Before that, what passed for dental care in the war-torn country often amounted to a barber ripping out sore teeth without anesthetics. With 90 percent of Afghans having never seen a real dentist and 70 percent malnourished, dental problems were extreme. Abscesses were not uncommon and often led to septicemia, which can be lethal without antibiotics. Word that an American dentist had come offering free dental care spread fast among rural communities, and soon Rolfe was helping more than 60 people a day. Many of the orphaned boys whom Rolfe treated would become his assistants, thus leading to the Kabul School of Dental Technology.

After a car bomb, two scams, 100,000 patients treated, and 11 years since the birth of the

[1] This is an abridged version of an article in the *Santa Barbera Independent* from 2014. This the the text that was shown to subjects. The full-legnth article can be found here: https://www.independent.com/news/2014/jul/12/afghan-dental-relief-project-ready-next-level/

Afghanistan Dental Relief Project, Rolfe is on the brink of taking it to a new level. He recently worked with Afghanistans Ministry of Public Health to obtain permits to provide more complex, nonbasic dental services to Afghans for a small fee, such as endodontic treatment or prosthetic restorations. For non-Afghans, however, the fee is equivalent to what they would pay in Dubai, and the treatment of only 16 non-Afghans covers the clinics entire monthly operating expenses.

Today, Rolfe can be found in his successful dentistry clinic near the Lobero Theatre. His self-built office surrounds his patients with the sounds of nature and artifacts reminiscent of a cultured life. He has managed to live a life performing dentistry for no cost in a Santa Barbara commune and now helping thousands in Afghanistan that intertwines his passion to help with his skills as a dentist. Rolfe, who is 75 years old, continues to work 115 hours a week and lives well below the poverty line. Except for his basic needs, he gives all he makes to his Afghanistan project, and hes nowhere near ready to slow down. In the future, he sees a first-rate dental infrastructure providing Afghans with health care, jobs and education. “We need to be more active,” Rolfe said. “If we feel something in our heart, we need to act on that, and that needs to form the basis of our existence.”

Appendix C

Institutional Determinants of Municipal Fiscal Dynamics

C.1 Additional Results

Table C.1: First Stage Results

	(1) $\Delta log(Emp)$	(2) $\Delta log(Emp)$	(3) $\Delta log(Emp)$	(4) $\Delta log(Emp)$
Bartik	0.887***	0.677***	0.893***	0.663***
	(0.044)	(0.121)	(0.045)	(0.138)
Year FE	No	Yes	No	Yes
Muni FE	No	No	Yes	Yes
Observations	34500	34500	34500	34500
Adjusted R^2	0.353	0.379	0.374	0.402
F	410.64	31.19	388.19	23.16

Notes: Entries are coefficients from first-stage regressions. In all cases, the dependent variable is the actual change in log commuting zone employment and independent variables are the predicted change in log employment, calculated according to (3.1), and fixed effects, if any. Fixed effects coefficients are omitted. Standard errors, clustered at the commuting zone level, are presented in parentheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.2: Ordinary Least Squares Results (Reduced Form)

	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$	$h=5$
***Panel 1**: Expenditures*						
$\Delta log(Emp)$	-0.089**	-0.022	0.080*	0.135***	0.243***	0.299***
	(0.037)	(0.044)	(0.047)	(0.050)	(0.051)	(0.052)
TEL $\times$ $\Delta log(Emp)$	-0.261**	-0.495**	-0.317*	-0.007	0.025	-0.037
	(0.111)	(0.216)	(0.173)	(0.141)	(0.116)	(0.129)
TEL	0.035*	0.062***	0.075***	0.074***	0.083***	0.094***
	(0.018)	(0.021)	(0.021)	(0.024)	(0.027)	(0.031)
N	34153	33003	31853	30703	29553	28403
***Panel 2**: Capital Outlays*						
$\Delta log(Emp)$	-0.172	0.262*	0.483***	0.609***	0.636***	0.918***
	(0.136)	(0.140)	(0.150)	(0.157)	(0.162)	(0.173)
TEL $\times$ $\Delta log(Emp)$	-1.336***	-1.794***	-0.994**	0.326	0.680	0.162
	(0.399)	(0.563)	(0.418)	(0.503)	(0.691)	(0.591)
TEL	0.097***	0.150***	0.143***	0.110**	0.124**	0.141***
	(0.024)	(0.034)	(0.045)	(0.047)	(0.049)	(0.048)
N	33971	32821	31671	30521	29371	28221
***Panel 3**: Transportation*						
$\Delta log(Emp)$	-0.068	0.036	0.362***	0.526***	0.437***	0.525***
	(0.068)	(0.079)	(0.093)	(0.111)	(0.096)	(0.095)
TEL $\times$ $\Delta log(Emp)$	-0.074	-0.723**	-0.740**	0.024	0.176	0.212
	(0.206)	(0.350)	(0.310)	(0.221)	(0.237)	(0.211)
TEL	0.019	0.063**	0.076**	0.073**	0.076***	0.079***
	(0.014)	(0.026)	(0.031)	(0.029)	(0.023)	(0.022)
N	34148	32998	31848	30698	29548	28398

Notes: Dependent variables are given by each panel heading. Units of all dependent variables are real, per-capita 2004 U.S. dollars. h represents the forecast horizon, as outlined in equation (3.2). TEL is an indicator taking value one if a city faces a general expenditure or general revenue TEL during the year in which the shock occurs. *Emp* is employment in the commuting zone in which a city resides. Municipality and year fixed effects omitted. Standard errors are clustered at the commuting zone level and presented in parentheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.3: Ordinary Least Squares Results (Reduced Form, Continued)

	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$	$h=5$
***Panel 4**: Public Maintenance*						
$\Delta log(Emp)$	-0.171**	-0.069	-0.051	0.063	0.152**	0.359***
	(0.068)	(0.078)	(0.081)	(0.084)	(0.077)	(0.077)
TEL $\times \Delta log(Emp)$	-0.460**	-0.751***	-0.554**	-0.244	-0.059	-0.324
	(0.192)	(0.223)	(0.258)	(0.298)	(0.285)	(0.286)
TEL	0.049***	0.077**	0.083**	0.081*	0.094**	0.117***
	(0.018)	(0.030)	(0.039)	(0.044)	(0.045)	(0.043)
N	34132	32982	31832	30682	29532	28382
***Panel 5**: Public Safety*						
$\Delta log(Emp)$	-0.051*	0.012	0.156***	0.162***	0.219***	0.201***
	(0.031)	(0.037)	(0.039)	(0.047)	(0.054)	(0.055)
TEL $\times \Delta log(Emp)$	-0.041	-0.215	-0.051	-0.030	-0.291**	-0.225
	(0.170)	(0.343)	(0.296)	(0.212)	(0.130)	(0.146)
TEL	0.010	0.022	0.024	0.030*	0.044***	0.046***
	(0.013)	(0.017)	(0.018)	(0.018)	(0.016)	(0.016)
N	34137	32987	31837	30687	29537	28387
***Panel 6**: Government Administration*						
$\Delta log(Emp)$	-0.068	0.036	0.362***	0.526***	0.437***	0.525***
	(0.068)	(0.079)	(0.093)	(0.111)	(0.096)	(0.095)
TEL $\times \Delta log(Emp)$	-0.074	-0.723**	-0.740**	0.024	0.176	0.212
	(0.206)	(0.350)	(0.310)	(0.221)	(0.237)	(0.211)
TEL	0.019	0.063**	0.076**	0.073**	0.076***	0.079***
	(0.014)	(0.026)	(0.031)	(0.029)	(0.023)	(0.022)
N	34148	32998	31848	30698	29548	28398

Notes: Dependent variables are given by each panel heading. Units of all dependent variables are real, per-capita 2004 U.S. dollars. h represents the forecast horizon, as outlined in equation (3.2). TEL is an indicator taking value one if a city faces a general expenditure or general revenue TEL during the year in which the shock occurs. *Emp* is employment in the commuting zone in which a city resides. Municipality and year fixed effects omitted. Standard errors are clustered at the commuting zone level and presented in parentheses. *Significance*: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

C.2 Robustness

Table C.4: Baseline Results (Counties)

	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$	$h=5$
***Panel 1**: Expenditures*						
$\Delta log(Emp)$	0.027	-0.228	-0.017	-0.083	0.247	0.152
	(0.128)	(0.158)	(0.175)	(0.218)	(0.211)	(0.166)
$\text{TEL} \times \Delta log(Emp)$	-0.295*	-0.650***	-0.371*	0.047	0.440***	0.184
	(0.156)	(0.215)	(0.204)	(0.183)	(0.169)	(0.174)
TEL	0.037***	0.068***	0.078***	0.072***	0.069***	0.086***
	(0.010)	(0.013)	(0.017)	(0.019)	(0.020)	(0.021)
N	34153	33003	31853	30703	29553	28403
***Panel 2**: Capital Outlays*						
$\Delta log(Emp)$	0.821*	-0.327	1.019*	-0.384	0.552	0.947**
	(0.442)	(0.433)	(0.546)	(0.547)	(0.496)	(0.435)
$\text{TEL} \times \Delta log(Emp)$	-2.236***	-2.917***	-2.307***	0.183	1.258**	0.789**
	(0.514)	(0.582)	(0.685)	(0.505)	(0.565)	(0.402)
TEL	0.131***	0.191***	0.189***	0.114**	0.103**	0.121***
	(0.029)	(0.041)	(0.056)	(0.051)	(0.044)	(0.039)
N	33971	32821	31671	30521	29371	28221
***Panel 3**: Transportation*						
$\Delta log(Emp)$	-0.159	-0.100	0.147	0.380	0.545*	0.635**
	(0.199)	(0.235)	(0.309)	(0.255)	(0.290)	(0.277)
$\text{TEL} \times \Delta log(Emp)$	-0.213	-1.682***	-1.481***	-0.435	0.132	0.188
	(0.212)	(0.431)	(0.428)	(0.336)	(0.208)	(0.375)
TEL	0.023*	0.095***	0.102***	0.089***	0.078***	0.081**
	(0.013)	(0.021)	(0.023)	(0.024)	(0.027)	(0.034)
N	34148	32998	31848	30698	29548	28398

Notes: Dependent variables are given by each panel heading. Units of all dependent variables are real, per-capita 2004 U.S. dollars. h represents the forecast horizon, as outlined in equation (3.2). TEL is an indicator taking value one if a city faces a general expenditure or general revenue TEL during the year in which the shock occurs. Emp is employment in the county in which a city resides. Municipality and year fixed effects omitted. Standard errors are clustered at the county level and presented in parentheses. *Significance*: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table C.5: Baseline Results (Counties, Continued)

	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$	$h=5$
***Panel 4**: Public Maintenance*						
$\Delta log(Emp)$	-0.119	-0.135	0.001	-0.185	0.413	0.211
	(0.188)	(0.220)	(0.292)	(0.272)	(0.252)	(0.234)
TEL $\times$ $\Delta log(Emp)$	-0.403*	-0.950***	-0.735**	-0.171	0.151	-0.369
	(0.245)	(0.235)	(0.340)	(0.389)	(0.263)	(0.263)
TEL	0.048**	0.086***	0.091**	0.080**	0.087***	0.120***
	(0.022)	(0.030)	(0.040)	(0.039)	(0.033)	(0.033)
N	34132	32982	31832	30682	29532	28382
***Panel 5**: Public Safety*						
$\Delta log(Emp)$	0.022	0.010	0.107	0.120	0.364***	0.085
	(0.111)	(0.133)	(0.136)	(0.134)	(0.120)	(0.112)
TEL $\times$ $\Delta log(Emp)$	0.115	-0.137	0.106	-0.056	-0.101	-0.131
	(0.162)	(0.321)	(0.329)	(0.148)	(0.141)	(0.162)
TEL	0.006	0.020	0.020	0.031*	0.039*	0.044**
	(0.010)	(0.016)	(0.015)	(0.016)	(0.021)	(0.020)
N	34137	32987	31837	30687	29537	28387
***Panel 6**: Government Administration*						
$\Delta log(Emp)$	0.010	-0.113	0.276	0.360	0.466*	0.254
	(0.226)	(0.316)	(0.258)	(0.318)	(0.265)	(0.308)
TEL $\times$ $\Delta log(Emp)$	0.747**	0.787**	1.099**	-0.229	0.587	0.417
	(0.328)	(0.351)	(0.450)	(0.333)	(0.456)	(0.336)
TEL	-0.048**	-0.055*	-0.062*	-0.026	-0.055	-0.044
	(0.021)	(0.028)	(0.037)	(0.038)	(0.041)	(0.042)
N	34020	32870	31720	30570	29420	28270

Notes: Dependent variables are given by each panel heading. Units of all dependent variables are real, per-capita 2004 U.S. dollars. h represents the forecast horizon, as outlined in equation (3.2). TEL is an indicator taking value one if a city faces a general expenditure or general revenue TEL during the year in which the shock occurs. *Emp* is employment in the county in which a city resides. Municipality and year fixed effects omitted. Standard errors are clustered at the county level and presented in parentheses. *Significance*: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table C.6: Baseline Results (Excluding Missing Data)

	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$	$h=5$
***Panel 1**: Expenditures*						
$\Delta log(Emp)$	-0.181	-0.019	-0.082	0.092	0.356*	0.707***
	(0.170)	(0.199)	(0.147)	(0.179)	(0.185)	(0.202)
TEL $\times$ $\Delta log(Emp)$	-0.030	-0.234	0.279	0.134	0.117	-0.063
	(0.145)	(0.215)	(0.176)	(0.265)	(0.212)	(0.283)
TEL	-0.000	0.019	0.019	0.033*	0.047**	0.056**
	(0.012)	(0.016)	(0.015)	(0.020)	(0.021)	(0.023)
N	13170	12731	12292	11853	11414	10975
***Panel 2**: Capital Outlays*						
$\Delta log(Emp)$	-0.235	-0.864	-0.126	-0.163	0.168	1.815***
	(0.593)	(0.684)	(0.708)	(0.610)	(0.537)	(0.560)
TEL $\times$ $\Delta log(Emp)$	-1.626**	-2.047**	-1.399**	-0.375	0.374	0.275
	(0.793)	(0.902)	(0.638)	(0.839)	(0.482)	(1.005)
TEL	0.078**	0.139***	0.130**	0.127**	0.133**	0.125*
	(0.037)	(0.052)	(0.052)	(0.058)	(0.054)	(0.065)
N	13170	12731	12292	11853	11414	10975
***Panel 3**: Transportation*						
$\Delta log(Emp)$	-0.450	-0.448	0.184	0.968***	0.451	0.706*
	(0.334)	(0.352)	(0.354)	(0.360)	(0.337)	(0.412)
TEL $\times$ $\Delta log(Emp)$	-0.301	-1.242*	-0.989**	-0.184	-0.638*	-0.014
	(0.385)	(0.664)	(0.462)	(0.288)	(0.384)	(0.380)
TEL	0.005	0.049	0.053	0.055*	0.083**	0.082**
	(0.022)	(0.032)	(0.032)	(0.030)	(0.036)	(0.034)
N	13170	12731	12292	11853	11414	10975

Notes: Regressions estimated on those municipalities without missing data in any spending category. Dependent variables are given by each panel heading. Units of all dependent variables are real, per-capita 2004 U.S. dollars. h represents the forecast horizon, as outlined in equation (3.2). TEL is an indicator taking value one if a city faces a general expenditure or general revenue TEL during the year in which the shock occurs. *Emp* is employment in the commuting zone in which a city resides. Municipality and year fixed effects omitted. Standard errors are clustered at the commuting zone level and presented in parentheses. *Significance*: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table C.7: Baseline Results (Excluding Missing Data, Continued)

	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$	$h=5$
***Panel 4**: Public Maintenance*						
$\Delta log(Emp)$	-0.328	-0.315	-0.447	-0.477	0.358	0.635*
	(0.347)	(0.344)	(0.403)	(0.465)	(0.367)	(0.350)
TEL $\times$ $\Delta log(Emp)$	-0.548	-1.148**	-0.507	-0.463	-0.587	-0.955
	(0.369)	(0.462)	(0.585)	(0.527)	(0.427)	(0.591)
TEL	0.036	0.076*	0.073	0.079	0.101**	0.138***
	(0.024)	(0.039)	(0.049)	(0.051)	(0.044)	(0.041)
N	13170	12731	12292	11853	11414	10975
***Panel 5**: Public Safety*						
$\Delta log(Emp)$	-0.006	0.084	0.234*	0.326**	0.515**	0.528***
	(0.130)	(0.126)	(0.127)	(0.160)	(0.211)	(0.188)
TEL $\times$ $\Delta log(Emp)$	0.091	0.059	0.239	0.180	-0.206	-0.339**
	(0.136)	(0.185)	(0.274)	(0.198)	(0.154)	(0.141)
TEL	-0.003	0.003	0.003	0.012	0.036*	0.040**
	(0.010)	(0.016)	(0.016)	(0.018)	(0.019)	(0.020)
N	13170	12731	12292	11853	11414	10975
***Panel 6**: Government Administration*						
$\Delta log(Emp)$	0.930**	0.657*	0.987***	0.664**	1.055***	1.088***
	(0.393)	(0.392)	(0.344)	(0.272)	(0.340)	(0.383)
TEL $\times$ $\Delta log(Emp)$	0.749*	0.528	1.158	0.450	0.499	0.719*
	(0.434)	(0.516)	(0.888)	(0.366)	(0.381)	(0.398)
TEL	-0.082*	-0.094	-0.132*	-0.114**	-0.105**	-0.101*
	(0.047)	(0.061)	(0.075)	(0.056)	(0.052)	(0.059)
N	13170	12731	12292	11853	11414	10975

Notes: Regressions estimated on those municipalities without missing data in any spending category. Dependent variables are given by each panel heading. Units of all dependent variables are real, per-capita 2004 U.S. dollars. h represents the forecast horizon, as outlined in equation (3.2). TEL is an indicator taking value one if a city faces a general expenditure or general revenue TEL during the year in which the shock occurs. *Emp* is employment in the commuting zone in which a city resides. Municipality and year fixed effects omitted. Standard errors are clustered at the commuting zone level and presented in parentheses. *Significance*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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