Can Bureaucrats Manipulate Policy Outcomes?

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Abstract

This paper empirically tests whether the theoretically established concern that bureaucrats can affect policy outcomes is justified, using US state government directed Unemployment Insurance (UI) policies. We show that overpayments in UI benefits from policy audits proxy for bureaucratic tastes. This allows us to estimate regression-discontinuity models based on narrowly decided US Governor elections. We reject the hypothesis that bureaucrats push budget maximization. Instead, we find evidence that bureaucrats sympathize with their "clients," consistent with the model in Prendergast (2007), as bureaucracies appear to work to increase UI benefits per recipient. Furthermore, consistent with this hypothesis, we do not find that overpayments in UI benefits respond to policy changes, indicating that bureaucrats do not have a "preferred" policy.

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I. Introduction[1]

This paper investigates whether non-elected bureaucrats are capable of changing government policies that are designed by elected officials (Maskin and Triole, 2004; Enikolopov, 2014). Standard economic theory suggests that politicians only hire bureaucrats when the rate of return to improved policy implementation overcomes the potential loss of control over policy outcomes, depending on both information flows and policy complexity (Alesina and Tabellini, 2007, 2008; Ting, 2012). Our paper examines whether this concern is warranted using state government Unemployment Insurance policies.

There are two paths by which bureaucrats might influence policy, either the implementation of the policy is sufficiently complex so that how the rules are implemented is in fact the policy, or bureaucrats are able to influence policymakers. For example, the regulatory literature is concerned with whether regulators are 'captured' by either producer or consumer interests, with the implication that the implementation of the policy rules is in fact the policy (Besley and Coate, 2003). An alternative is that administrators can influence politicians directly, so that the policies enacted by politicians are altered. A necessary condition for bureaucrats to be able to change policy, however, is that they would want to change policy, which would indicate that the bureaucracy has a taste over policy outcomes. Based on our finding that the bureaucracy has the power to change policy outcomes, we attempt to infer their implied preferences.

[1] The work in this paper has benefitted from comments received at the Regional Science Meetings, the Urban Economics Association Meetings, the Public Choice Meetings, and the Western Regional Science Meetings. We have also benefitted from comments by our colleagues at a brown bag, and especially Willa Friedman.

Our empirical test is therefore a joint test; the first is to discern the tastes of bureaucrats, and the second is to determine whether policies enacted by elected government officials reflects those tastes. Our ability to evaluate this joint test is possible because of the availability of unique administrative data on the Unemployment Insurance Program (UI). The UI program in the US is administered by each state, and each state has considerable discretion in choosing a vector of program parameters (Craig and Palumbo, 1999; US DOL, 2012). The US Department of Labor (DOL) audits each state according to the state's own rules. This oversight program, called the Benefit Accuracy Management program (BAM), was initiated by the DOL in 1996. The error rate report includes, based on a relatively small but representative sample, both overpayments and underpayments in UI benefits. The results indicate the amounts workers should have been paid compared to the amounts actually paid.according to the state's own rules. We argue below that the error rate resulting from the DOL audits is, at least in part, an indicator of the policy preferences of the UI bureaucracy.

Unsurprisingly, the reported errors are asymmetric, where overpayments are much larger than underpayments. To control for potential random error, our measure of overpayments in UI benefits in this paper is referred to as *Net Overpayments*, which is constructed by subtracting underpayments in UI benefits from benefit overpayments. *Net Overpayments* data is then used as a signal of bureaucratic tastes, to measure the potential differences between bureaucratic preferences compared to payments the program design is supposed to dispense. Our hypothesis, for which we provide substantial evidence below, is that Net Overpayments suggest bureaucrats will attempt to influence policy changes in the UI program.

Our empirical test arises because if UI payment errors made by administrators are indeed random, we should not find policy responses by politicians. Similarly, if bureaucrats do not have policy preferences, we should not expect to observe any influence. In contrast, if the administrative errors are systematic and reflect how bureaucrats believe policy should be conducted, and if bureaucrats have any influence, then the errors will lead to policy changes by state governments. We are not suggesting that the errors themselves are the influence, simply that errors signal that bureaucrats are dissatisfied, and thus will attempt to influence policy in some way. For the policy change to occur bureaucrats would need a mechanism in place by which they can convince politicians to enact their desired changes. Thus, our empirical test is of the joint hypothesis that bureaucrats have tastes over policy outcomes, and that they have the ability to communicate their preferences to politicians in a compelling manner.

We model the joint test of bureaucratic tastes and influence by constructing an unusual regression-discontinuity (RD) design based on state governor elections. Specifically, we find a discontinuous jump in the administrative error rate when the political party of the governor changes from a Republican to a Democrat. Such a jump is consistent with the possibility that bureaucrats believe they will be more influential over policy change due to the political change. We therefore use a narrow bandwidth around governor elections with a change in party to test how UI policies change as a result of the discontinuity in error rates.

The empirical test uses state level panel data that covers 50 US states and the period 1996-2018.[2] Specifically, using a relatively narrow window of state governor election outcomes, we are

[2] The availability of the UI policy audit data starts in 1996.

able to test whether bureaucratic errors in UI benefits influence UI policy choices in three dimensions, which are (1) UI benefits per recipient[3], (2) UI recipients per capita, and (3) UI taxes per capita.[4] The RD specification is consistent with a view that governors choose administrative leaders, and those leaders would be expected to be crucial in the "conversation" between the bureaucrats and the politicians.[5] Thus a change in the governor's political party suggests a change in the communication framework between politicians and bureaucrats without a corresponding change in either the composition of the bureaucracy or of changes in the economy.

Within the RD framework, we use an instrumental variable strategy on administrative error rates. This strategy acknowledges that there is an error production function that would be expected to depend on program outcomes. For example, a period of high unemployment may cause higher error rates simply due to congestion from increased recipients.[6]

Given our desire to understand the motivation for both bureaucrats and politicians, we examine the theoretical literature for empirical expectations. There are few extant theories about the motivation of bureaucrats for attempting to manipulate policy outcomes.[7] One direct expression of bureaucratic tastes comes from Niskanen (1971), who posits that workers within the

[3] Our measure of the policy outcome, UI benefits per recipient, is measured as benefits per recipient less overpayments per recipient to reflect the level of payments as designed by the politicians.

[4] UI is financed by an earmarked tax on employers, which in most cases amounts to an annual lump sum tax per worker.

[5] Of the 44 governor elections with a political party change for which we could find information, 83% of them choose a new head administrator of the agency with UI policy.

[6] Unfortunately, we do not have sufficiently detailed data by which we could estimate the administrative error production function.

[7] This statement ignores the perspective of some of the older work that assumes without an incentive structure that the goal of government is to maximize social welfare.

government will desire to maximize total public expenditure, as this will increase the demand for workers in the bureaucracy. This theory is closely linked to rent-seeking theory, suggesting bureaucrats create private gains for themselves (Krueger, 1974). In the UI context, we interpret this theory to suggest bureaucrats advocating for a greater number of recipients, conditional on the unemployment rate, to increase the size and pay of the bureaucracy.

Outside of these narrowly "selfish" theories, however, there is little other work that explicitly ascribes "tastes" to bureaucrats. McFadden (1975, 1976), empirically tests the tastes of the bureaucracy, but does not compare those tastes to those of the policymakers. Alesina and Tabellini (2007, 2008) essentially argue that bureaucrats will implement a prescribed policy, but only if they can be monitored closely. Thus, they model a tension between bureaucrats and politicians consistent with potential manipulation, but are not explicitly given preferences in their work. Furthermore, the Alesina and Tabellini framework does not model a feedback between policy choices of politicians and bureaucratic behavior.

An exception in the theoretical literature that attributes explicit tastes to bureaucrats is the recent hypothesis advanced in Prendergast (2007). The Prendergast model suggests that politicians will select administrators in part based on the tastes of the administrators. In the empirical example we utilize, we believe that consistent with the program content, administrators would be selected that have sympathies for recipients. In the Prendergast framework, sympathetic bureaucrats are chosen by the politicians because there are asymmetric political costs to committing errors. Specifically, if a deserving recipient is denied benefits, there will be a much higher political cost than if an underserving person receives more resources than is justified. If as a result politicians

hire program administrators that sympathize with recipients, those administrators might be expected to advocate for greater benefits per recipient.

The empirical results from our panel estimates shows that bureaucrats indeed appear to have influence on political choices. Surprisingly, perhaps, we find that bureaucratic influence is equal in regimes with Republican governors as with Democratic governors. Furthermore, the particular pattern of influence suggests the Prendergast model of sympathetic bureaucrats is important because we find that only UI benefits per recipient are influenced by bureaucrats. More specifically, our results reject the rent seeking hypothesis where administrators expand the program to increase demand for administration. We find no influence of the error rate on the number of UI recipients per capita, or on UI taxes per capita.

Our paper proceeds by outlining the characterizations of bureaucratic preferences in Section II, and illustrates our empirical interpretation of those theories. In Section III, we discuss the data used to test for bureaucratic influence on UI policy outcomes. The panel data uses policy outcomes and audited error rates from all 50 US states, so we start our period with the initiation of the audit data from 1996-2018. We also discuss the RD specification, and the instruments we construct to control that potentially the error rates respond to policy choices. Section IV presents our empirical results using the RD approach with instrumented administrative errors. Specifically, we find that the main policy target of bureaucratic tastes is UI benefits per recipient. We also find our result is robust to an IV specification without the RD. And, we find that bureaucrats do not demonstrate tastes for an optimal policy, as we find that administrative errors do not respond to policy changes.

II. Theories of Preferences by Administrators

Our goal is to estimate the joint hypothesis of whether bureaucracies can be characterized as having policy tastes, which will only be discernable if the bureaucracies are also interested in changing policy based on their tastes. Our test of whether bureaucracies are able and interested in changing policy relies on the results of the policy audit by DOL. The policy audit reports, based on sampling, the rate at which overpayments in benefits (Overpayments) are made to UI recipients. If the Overpayment rate is not random, but rather reflects in part the explicit behavior of UI administrators, then it is an excellent method by which we can test the joint hypothesis of administrative tastes and influence. The substantial literature on bureaucratic behavior is not generally direct about the policy objectives of government administrators, but nonetheless provides considerable structure concerning the interaction between bureaucrats and policymakers.

Perhaps the clearest bureaucratic objective is conjectured in Niskanen (1971), who posits that administrators want to increase the demand for their services. Consistent with rent seeking by the policy administrators (Krueger, 1974), his hypothesis is that bureaucrats benefit from greater opportunity for career advancement caused by a continual growth in the workforce.[8] While the motivation is slightly different, this theory is broadly consistent with the work by Ting (2012), who develops a more explicit model to describe the allocation of the supply of publicly provided goods. Ting suggests that the quality of a program or policy enacted by the legislature matters for the bureaucrat. If the policy is of high quality, it is more efficient for the legislature to allow the

[8] Consistent with rent seeking, administrators in this view do not have tastes over policy outcomes in the sense of good public policy, only in the attributes of policy administrators that would benefit the administrators directly.

bureaucrat to implement the program because inefficiencies associated with the extraction of rents in legislative bargaining are removed by the bureaucrats' endorsement of the policy. In the UI policy context, we interpret both of these works as saying that the bureaucrats would be supportive of an expansion in the number of UI recipients, as this would result in a higher demand for program administrators.[9]

The model developed in Prendergast (2007) suggests a more nuanced view of bureaucratic preferences.[10] This model carefully considers the loss function to politicians from administrative errors. If the loss function is asymmetric, Prendergast posits that policy-makers will want to hire bureaucrats with policy preferences that reflect the politicians' loss function. Pertinent to the case of UI, it is reasonable to assume that the political loss function is asymmetric. Specifically, denying UI benefits to a person perceived to be deserving of public assistance may be viewed as much more costly than granting extra benefits to a person that would not be perceived as being deserving. If this is indeed the case, the Prendergast model suggests politicians will desire to hire bureaucrats who strongly sympathize with potential recipients. In this way, the possibility of the

[9] Also consistent with this framework is that bureaucrats would support a more complex policy, as that would also entail an increase in demand for administrators. We do not have an empirical test specifically to capture policy complexity except to the extent complexity results in greater outcomes such as recipients or annual benefits per recipient.

[10] This model is broadly consistent with the view of bureaucratic behavior proposed in Leaver (2009), except that in Leaver bureaucrats attempt to avoid complaints by the public, rather than have explicit tastes. The difference in the two models is that there is no reason to assume bureaucrats would want to change the policy choices in the Leaver model, while it would be perfectly consistent given the underlying assumed preferences in the Prendergast framework.

denial of benefits to the deserving is minimized, which therefore minimizes the potential loss to the politician.[11]

The process that is not considered in Prendergast, however, is the future impact on the program of an administration that is sympathetic toward program recipients. That is, if bureaucrats are primarily sympathetic for recipients, they may attempt to influence policy to be more generous. If there are paths by which bureaucrats can influence policy, then if the preferences are consistent with the view advanced in Prendergast, policy may be pushed in a direction that politicians may not have originally intended. The model in Gailmard and Patty (2007) is consistent with this view, where in their model development of policy preferences by bureaucrats allows the overall administrative quality to rise. Our work will test the proposition that bureaucrats have a view of the appropriate policy, which will only become apparent if bureaucrats have any success in pushing policy in their desired direction.[12] We find, however, that administrators push for a policy more generous to recipients, and we do not find any evidence of a "bliss point" beyond which UI policy is too generous.

Empirical papers, such as Chang and Turnbull (2002), Dahlberg and Mork (2006), and Gains and John (2010), have explored bureaucratic preferences in the context of maximizing public spending, increasing the size of the bureaucracy, and the tasks and jobs which bureaucrats would like to perform. These papers generally find evidence consistent with potential bureaucratic

[11] The loss function for all programs is not expected to show the same asymmetry. Politicians might want to hire IRS bureaucrats with no sympathy for taxpayers, for instance.

[12] See Gailmard and Patty (2012) for a review of models showing the effect of bureaucratic behavior on political and economic outcomes.

influence over policy, consistent with our efforts to test policy preferences as revealed by this influence.

The unique contribution of the work here is that the UI audited error rate offers a window on expressed tastes by the bureaucracy. In the context of the theoretical work above, this taste variable allows distinction between the theoretical predictions. Specifically, we believe that demand for bureaucrats will depend on number of UI recipients, in which case rent seeking bureaucrats will be found to push program expansion, at least in part through commission of errors, resulting in more recipients. In some sense, it would be consistent with some views that a high quality program is one for which continuing eligibility is easy.[13] What we find, however, is a push toward program generosity in administrations headed by both Democratic governors, and Republican governors.

Data and Econometric Specification

We construct a panel data set for the 50 US states and for the years 1996-2011.[14] UI program information by state is published by the U.S. Department of Labor (DOL) each year (annual).[15] The primary program outcomes are annual benefits per recipient (*UI Ben*), number of

[13] Clearly, the wage replacement rate is another measure of program quality. Our assumption is that the size of the benefit check does not impact demand for administrative workers compared to the number of recipients.

[14] There are seven missing observations in the UI audit data for benefits in the first two years of the audits.

[15] Our data only pertains for regular UI benefits, as we exclude Extended and Emergency benefits which are generally paid for by the federal government during periods of high unemployment.

recipients per capita (*UI Rec*), and total annual taxes per capita (*UI Tax*) for each state and year.[16] *UI Ben* can be further decomposed into the average weekly benefit amount (*AWBA*), and the average duration of UI eligibility (*UI Dur*). The constraint on the beginning of the panel is the availability of the DOL audited error rates by state, which starts in 1996.[17] The UI program outcome variables are choice variables by each state within the federal programmatic constraints (DOL, 2012). States are required to totally fund their program as outside of administrative costs, there are no federal subsidies.[18] Thus on average, total UI expenditures per capita will equal the number of annual recipients per capita times annual benefits per recipient. As a result, UI taxes per capita are also a policy choice by states.[19] We deflate the dollar values by the CPI with 2011 as the base year, and use the Census population estimates to construct per capita values.

One key feature of the UI program is that there is significant variation by state in the criteria for both eligibility, and for annual benefits per recipient. For example, while eligibility for the program would seem to be straightforward for full time workers with a long history of work experience, the eligibility criteria for people with short work spells, who are seasonal, and/or who are part-time workers can vary widely between states. And, even for workers well attached to the labor force a determination of the motivation for work separation has many potential dimensions

[16] Recipients per capita are the number of new UI recipients, measured by the DOL as the number of first payments. This indicates a worker has initiated a new unemployment spell.

[17] While the number of cases audited is relatively small, the DOL claims that they are a representative sample of each state's caseload.

[18] The federal government, however, pays 100% of approved administrative costs.

[19] Exceptions to total expenditures equaling UI taxes occur due to business cycles, as there are also savings accounts for each state, see Craig, Hemissi, Mukherjee, and Sorensen (2016).

with important consequences for program eligibility and benefit amounts.[20] While the calculation of benefits for full-time salaried workers might similarly seem straightforward, hourly workers do not always work the same number of hours every week nor always at the same wage rate. Further, there can be incentive compensation such as tips, overtime and bonuses that are not constant at all times. As a result of these programmatic details, in addition to straightforward variation in generosity, variation between states in the level of weekly benefits per recipient can be considerable.[21]

We use program outcomes to measure the impact of the myriad administrative complexities. UI benefits (*UI Ben*) is the annual dollars received on average for each UI recipient. In addition, we use the DOL designation of the number of people that receive first payments as a share of the population as the recipient measure (*Rec*), so that

$$Exp = Ben \times Rec,$$

where Exp is annual UI expenditure per capita.

We model UI taxes per capita (*UI Tax*) as a third program outcome. UI taxes are an earmarked tax that essentially are lump sum taxes per annual employee.[22] In part, the tax level is

[20] This discussion does not do full justice to the variety of potential policy differences between states, including the range of training and search activities required of unemployed workers to maintain their eligibility.

[21] Further, variation in benefits per recipient could depend on the industrial composition of the unemployed. Our point, however, is that the program outcomes are the result of a myriad set of small policy choices within the program details.

[22] The earmarked state UI taxes are based generally on, for example, the first $9,000 in annual wages (the amounts by state vary between $7,000 and $16,000). While the tax rate on each firm varies depending on the extent to which states use experience ratings based on the firm's past unemployment experience, UI taxes generally amount to a lump sum tax per full time annual worker since the base is so small.

dependent on the overall program generosity. Another choice, however, we exploit to construct an instrumental variable is that UI expenditures are not required to be equal to taxes each year. Instead there is a UI trust fund savings account that aids smoothing across years (Craig, Hemisi, Satadru, and Sorensen, 2016). While borrowing from the federal government is possible, over time the state government will have to balance UI taxes and expenditures.

In our estimation process we explore including state level control variables. Data for the unemployment rate, the share of workers in the manufacturing industry, and the share of workers who work for the government are collected from the Bureau of Labor Statistics; Gross State Product per capita is gathered from the Bureau of Economic Analysis; and the poverty rate is taken from the Census Bureau. Additional demographic variables include the share of the population that is white, the share that is under 18 years old and the share that is over 64 years old. Finally, the share of the population at least 25 years old with a high school degree is obtained from the Current Population Survey. Information on the political party of the governor comes from the *Book of the States* (annual).

If the administrative net error rate is an indicator of bureaucratic preferences, it is possible that the error rate will change when the political party of the governor changes. One reason for such an expectation is that if bureaucrats believe their policy preferences will be more likely to result in policy change, bureaucrats may attempt to influence a perceived sympathetic governor. Further, with a new governor, it is extremely likely that the administrative agency head will change, which may stimulate a change in bureaucratic behavior. To test for this possibility, Figure 1 shows the net error rate as a function of the margin of victory (MOV) for the state governor. In this figure, we define a positive MOV as indicating a Democrat has won the gubernatorial election,

and that correspondingly a negative MOV indicates a Republican has won the election. The figure shows that the MOV causes a significant jump in the administrative error rate when a Democrat wins the governorship. Such a jump is consistent with bureaucrats expecting that Democratic governors or their appointed agency heads may be more sympathetic to their policy views, again conditional on the error rate indicating an expression of those views. .

In contrast, Figures 2-4 indicate that UI annual benefits per recipient, recipients per capita, and taxes per capita, do not exhibit a discontinuous jump with a change in governor's party. This result is not surprising, as UI policy is rarely an important issue in governors' elections and is rather less controversial than many other potential policy choices.[23] Further, UI policy outcomes are a result of the actions not only of the governor, but of the legislature.

Based on the discontinuity in apparent bureaucratic behavior, we therefore identify the impact of bureaucratic behavior from policy choices by examining instances of relatively close elections where the governor's political party changes in an RD framework.[24] Our empirical test will be whether the discontinuous changes in the net administrative error rate results in changes in policy outcomes. Because of the potentially long lags in the political choice process, we look for changes throughout the first eight years of a governors' term, generally covering two terms.[25]

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[23] The lack of a change suggests bureaucrats may not have rational expectations regarding actions of politicians. We leave this research for another project.

[24] We use a difference on each side of elections where the party of the governor changes equal to 7.5%. Sensitivity analysis shows the results are preserved with even smaller margins.

[25] There are a few governors that serve two year terms, and we have re-classified them all to be equivalent to four-year terms. Most states allow a governor to serve up to eight years (there are at least two exceptions), but not all impose a term limit. We stop our examination, however, after the first eight years.

Using an RD strategy on a subsample of narrowly decided gubernatorial elections makes it less likely that election outcomes are endogenous on UI outcomes. This does not, however, fully account for the potential simultaneity between the UI policy outcomes and *Net Overpayments per recipient*. In particular, we recognize there is an underlying production function process of administrative errors. While we do not have data in sufficient detail to estimate the production function, we note that there may be two influences on errors unrelated to policy outcomes. Specifically, we use as instrumental variables for the net error rate the share of the public workforce that is unionized, and the share of UI applications made in-person rather than using the telephone or computer.[26].

The model underlying the IVs is that general public sector unionization may impact the relationship between the state government bureaucracy and the politicians that hire them. Among the dimensions that might be important are that communication may be affected by unionization, or even the policy views of the bureaucracy. Application for UI may be different in-person compared to on-line, since the collection of information is less personal on-line.[27] Finally, it is possible that whether the UI savings account has a high or low balance impacts the degree to which errors are tolerated, consistent with Alesina and Tabelini (2007, 2008).[28] We present statistical

[26] The union variable is taken from ***www.unionstats.com***, which is a website described by Barry Hirsch and David Macpherson (2003). The share of applications on-line are obtained from DOL (annual).

[27] In fact, we find there is a positive correlation between the error rate and the share of UI applicants that apply on-line.

[28] Alesina and Tabellini model bureaucrats as needing to be monitored, but without an explicit agenda of their own. Their model is consistent with our presentation here, except that we add the possibility of an agenda, in which case there might potentially be feedback between bureaucratic behavior and politician preferences.

results below that suggest all three IV variables assist at statistical identification of the impact of administrative error rates on the resulting policy decisions.

Table 1 presents the mean of the variables we use in the analysis over the full sample of the 50 US states during the years 1996-2011. The net error rate data is available annually by state, although seven states are missing in the first year of the data and are dropped. Table 1 also presents the cross-sectional means at both ends of our sample. The overpayment rate is about 10% of the total UI benefits paid out, so is substantial with considerable cross state and cross time variation.

To allow focus on whether bureaucrats can influence political policy choices, we define the programs after subtracting the net policy errors. Since the net errors are total overpayments minus underpayments, our assumption is that net overpayments represent "intentional" errors by the bureaucracy. Thus random errors, indicated by underpayments, remain in the policy outcome definitions, but deviation from policy expressed by the excess errors we assume was not part of the political choice process. The resulting UI policy outcomes therefore represent those expected by the legislature when the policies are enacted.

A. Test of Overpayments in Benefits as an Expression of Policy Preferences

This subsection presents a test of whether it is likely that administrative errors are expressions of policy preferences by bureaucrats. Specifically, given any set of policies, it is expected that the unemployment rate determines the policy outcomes. We take the residuals of such a regression, and show they are correlated with the net overpayment error rate. While only suggestive, such a demonstration is consistent with policy expressions to the extent that attributes outside of the unemployment rate are partial determinants of policy outcomes.

Table 2 presents the regression results showing our three outcome measures, annual UI benefits per recipient, UI recipients per capita, and annual UI taxes per capita, as functions of the unemployment rate, as[29]

$$UI\ Outcome_{st}^{j} = \gamma^{j} UR_{st} + X_{st}\beta^{j} + \alpha_{s}^{j} + \phi_{t}^{j} + \varepsilon_{st}^{j} \qquad (1)$$

where UI Outcome refers to the natural logarithm of each of the *j* policy outcomes: (1) UI annual benefits per recipient (UI Ben), (2) the number of first-time UI recipients per capita (UI Rec), and (3) annual UI taxes per capita (UI Tax); *s* indexes states and *t* indexes years. Each policy outcome is regressed on *UR,* which is the state level unemployment rate, and X, which is a vector of state level controls; state (α_s) and year (ϕ_t) fixed effects are included in the regressions.[30]

The residuals ($\hat{\varepsilon}$) from the above equations are then used as a measure of "surprise" outcomes over and above what is expected, conditional on the unemployment rate. If our supposition that administrative errors are a measure of bureaucratic intentions is correct, it is likely that they would be correlated with the surprise residuals from equations (1). If so, *Net overpayments per capita*, defined as the natural logarithm of the difference between overpayments per capita and underpayments per capita, should be correlated with the residuals, as:

$$Net\ Overpayments\ per\ capita_{st} = \omega^{j}\widehat{\varepsilon_{st}^{j}} + X_{st}\delta^{j} + \sigma_{s}^{j} + \theta_{t}^{j} + \nu_{st}^{j}, \qquad (2)$$

where j indexes the UI program outcomes of UIBen, UIRec, and UITax. *Net Overpayments per capita* is the dependent variable and the residuals obtained from Equations 1 are the main

[29] Benefits per recipient times Recipients per capita is not strictly taxes per capita because there are savings accounts associated with UI. Thus properly the product is total expenditures per capita. We estimate taxes per capita since it is an important policy choice for UI.

[30] Omitting the control variables leaves the results essentially unchanged.

independent variables; state level control variables and fixed effects are also included in the regressions.

Table 2 shows the results of comparing the policy residuals for ln UI Ben to the ln of *net overpayment rate*. Panel B shows that the residuals from the regression are highly correlated with the *net overpayment rate*, and as well with the gross overpayment rate. The pattern is similar for the other two outcome variables. Table 3 Panel B shows the residuals from the UI Rec relationship are almost as highly correlated with *Net Overpayments per capita*. Finally, taxes per capita are illustrated in Tablel 4, and again Panel B shows the residuals of the unemployment regression are correlated with *Net Overpayments per capita*.[31]

B. Estimation

To ascertain whether bureaucrats can influence UI policy, we estimate the following three equations using a regression discontinuity framework:

$$UI\ \ln(Outcome)_{st} = \eta_1^j DEM\ 1^{st} Term\ Net\ OP_{st} + \eta_2^j DEM\ 2^{nd}\ Term\ Net\ OP_{st} + \eta_3^j REP\ 1^{st}\ Term\ Net\ OP_{st} + \eta_4^j REP\ 2^{nd} Term\ Net\ OP_{st} + X_{st}\beta^j + \mu_s^j + \rho_t^j + \pi^j MOV_{st} + \tau_{st}^j, \quad (3)$$

where UI Outcome indicates an equation for each of the UI program outcomes UIBen, UIRec, and UITax, and the coefficients are indexed by j. The subscripts *s* indexes state and *t* indexes year. The dependent variables are the natural logarithm of UI Benefits per recipient (*UI Ben*), the

[31] The final issue in the residuals of the regressions presented in Tables 2-4 is that the residuals themselves may represent policy tastes by bureaucrats. We explore this possibility but find that the residuals are uncorrelated with policy outcomes. Our conclusion is that the production function is more complex than simply a function of congestion (the unemployment rate), which is the specification used here. For example, we do not have a measure of policy complexity. Therefore we focus on the net overpayment variable as our ongoing measure of policy tastes.

natural logarithm of UI Recipients per capita (*UI Rec*), and the natural logarithm of UI Taxes per capita (*UI Tax*). The main independent variables is the natural logarithm of *Net Overpayments per recipient* (*Net OP*), which is defined as the natural logarithm of overpayments in annual benefits per recipient net of underpayments in benefits per recipient. To test for evolution in the relationship between politicians and the bureaucracy, we allow a slope dummy variable to differentiate the first from the second governor's term.[32] We further separately test for the influence of the bureaucracy in states with a Democrat as the governor from states with a Republican governor.[33] The vector, X_{st}, contains state level demographic control variables, and we include fixed effects for states (α_s) and years (ϕ_t).[34] Finally, in the RD estimation, we include the margin of victory (*MOV*) for the most recent gubernatorial elections within 7.5 percentage points.

By using the RD design, we statistically separate citizen preferences as expressed in voting from governmental demand. We further attempt to differentiate two hypotheses as to how the political party might reflect the actions of the bureaucracy. One is that if the bureaucracy

[32] We initially thought that if the bureaucracy was successful in the first term, there may be no further influence in the second. We also hypothesized there may be differential effects changing throughout the governors' terms. The empirical work clearly shows, however, no differential effects throughout the eight years we test.

[33] As discussed briefly earlier, differential influence might exist because of similarity in tastes, if for example the bureaucracy is generally Democratic. Or differential influence might exist because of differences in the production function of influence. As discussed above, Figure 2 suggests the bureaucracy has differential expectations. Our empirical work will show, however, that neither of these influences apparently prevail.

[34] The control variables are the percentage of the workforce in the manufacturing sector, the natural logarithm of Gross State Product per capita, the poverty rate, the percentage of high school graduates, the percentage of the population that is white, the percentage of the population that is under the age of 18, and the percentage of the population over the age of 64

identifies with one party, the communication function between policy makers and the bureaucrats may differ. Second, if the bureaucracy has policy preferences, it may be that the bureaucracy may exercise its discretion differently depending on the party in power because the policy choices differ.

A shortcoming in using the RD design in our context is that even though within a relatively narrow interval around the threshold, there are unobserved factors that may cause biased estimates of the effect of *Net OP*. For example, a potential problem that impacts estimation is that there may be a fundamental production function relationship, where the level of overpayments in benefits depends on the UI policy choices. We attempt to control for this process by using overpayments in benefits net of underpayments in benefits, as presumably underpayments represent true mistakes. This data adjustment, however, might not fully capture the potential simultaneity issue because of the input of recipients who have an incentive to control underpayments rather than overpayments.[35] An additional concern is that there may be aspects of the private sector labor force that are not fully accounted for in the RD design. For example, if employees from certain industries are more difficult to process than other industries, and if those industrial differences are reflected in pay and hence in UI benefit levels, then it is possible we would find a spurious relationship between overpayments in benefits and the policy outcomes.

In our context, we control for these potential sources of bias through variable definitions, and by using instrumental variables in our RD framework. *Net Op* is used as the primary

[35] That is, recipients will attempt to avoid underpayments. Thus Underpayments may under-estimate unintentional errors, because bureaucrats will not have the advantage of recipient assistance when making Overpayments.

measure of bureaucratic influence because its construction potentially controls for the random component of errors in benefits by subtracting underpayments from overpayments. In addition, we also instrument for *Net OP* in our estimation process with three variables. Specifically, an important change in the technology of administering UI is the opportunity for individuals to apply for UI on-line or by phone, instead of going to an office and having an interview in person. There are several aspects where this change might impact the error rate, but not linked to policy outcomes. Among them are that potential UI recipients may tend to exaggerate more without a personal contact, and that it may be difficult for potential recipients to navigate the forms.[36] Therefore, we use the share of recipients using an UI office to apply as an instrument for *Net OP*.

A second instrument we employ is the extent of public sector unionization. While the public sector has become an important outpost of unionization, our variable is averaged over all government workers, it is not specific to the UI administration (Hirsch and McPherson, 2003). Nonetheless, unionization may impact UI errors in several dimensions, all of which are most likely uncorrelated with UI program outcome variables. Unionization may impact the relationship between workers and their supervisors. For example, the error rate may rise to the extent employees are more insulated from oversight, or it may fall if unionization fosters a greater sense of collaboration. A second unrelated dimension is that unionization may impact the communication process between workers and policy-makers, so that workers' views on policy are expressed differently with different impacts.[37]

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[36] We find that the average error rate is higher when a higher proportion of recipients applies on-line.

[37] This process could go either way. Unions might provide for more frequent communication with less encumbered information exchange, or instead may codify hostility to make communication in the policy dimension less effective.

Levels of the UI trust fund savings account is the final IV we employ. We hypothesize that high levels of savings relative to annual expenses may lead to more relaxed oversight than low levels of saving outside of the policy determination process.

A final test we present is that of reversed causality, which is whether *Net OP* responds to UI policy outcomes. This test will answer the question as to whether the bureaucracy has an "optimal" policy. That is, if policy moves closer to an ideal point, *Net OP* would be expected to fall. Conversely, if policy moves away from an ideal point, the *Net OP* rate would be expected to change as bureaucrats respond. We use the full sample to conduct this test using a 2 stage least squares instrumental variables strategy. The RD graph of Figure 2 justifies the use of an IV model rather than an RD design because there are no discontinuities in UI policy when the party affiliation of the governor changes. Perhaps this is not surprising, since UI is a policy dictated by pragmatism rather than ideology.[38]

IV. Estimation Results Showing Bureaucratic Influence on Policy

Table 5 presents the RD results, using the IV estimates for *Net OP*, using a subsample of gubernatorial elections with a margin of victory within 7.5 percentage points.[39] The RD illustrates the local average treatment effect on UI policy around a discontinuous jump in *Net OP* when the political party of the governor changes. In either term, first or second, for Democrat and

[38] In theory, UI policy only exists because workers do not behave as our simple models suggests, in that a considerable share of people do not have sufficient precautionary savings.

[39] Wider margins of victory generally have smaller coefficients with larger standard errors, while smaller margins of victory produce very similar coefficients with proportionally smaller standard errors.

Republican governors, we find that an increase in *Net OP* causes an increase in the natural logarithm of UI Benefits per recipient (*UI Ben*). In addition, we find no significant effect on the natural logarithm of UI Recipients capita (*UI Rec*), and also much smaller elasticities. Further, there is an insignificant effect on the natural logarithm of UI Taxes per capita (*UI Tax*).

These results illustrate that when the bureaucracy increases the net error rate, state governments with governors from either political party tend to increase annual benefits per recipient, and of virtually the same magnitude. This result is found when there is an exogenous increase in the net overpayment rate. Such an increase does not occur all the time, of course, and in fact annual UI benefits do not appear to increase over time in real terms.

One striking attribute of these results is that we find they are empirically robust to alternative specifications in many dimensions. Table 6 presents RD results without using the IVs, and finds a very similar coefficient on benefits. This specification further finds a small impact on recipients which is only marginally significant in the second term of the governor. Table 7 presents IV estimates but without the RD specification, so uses all of the data in the sample. These results are statistically insignificant from zero or from our RD estimates, but primarily because of the large estimated standard errors. Table 8 shows results using gross Overpayment errors as the left-hand side variable. These results suggest that defining Overpayments net of Underpayments is not an important attribute of our empirical structure. Table 9 adds additional slope dummies for each pair of years of a governor's term, and finds virtually identical coefficients to the main specification. Table 10 collapses the distinction between governors' terms altogether, so that the only difference is between governors from the Democratic and Republican parties. Again, the primary coefficients are essentially preserved.

A. Discussion of the Motivation for Bureaucratic Influence

The results reported in Table 5 appear to be robust. Bureaucrats are shown to influence the level of UI policy outcomes in a manner consistent with what might be called "intentional errors." Given our discussion of the potential theories of the motivation for this behavior, we believe our results are quite supportive of the theory as advanced in Prendergast (2007).

Specifically, we show that bureaucrats support higher benefits per recipient in an environment consistent with sympathetic administrators. That is, if the asymmetric loss function analysis is pertinent for UI, as seems quite likely, Prendergast posits that bureaucrats will be hired that are sympathetic with the program recipients. We find bureaucrats appear to be willing to act on this sympathy, by being ready to support higher payments per recipient. Given the coefficients found in our estimation, it appears bureaucrats are equally able to motivate governments irrespective of the Governor's political party.

Also consistent with the Prendergast hypothesis, we find that bureaucrats do not appear to be motivated by narrow rent seeking returns. That is, as opposed to the hypothesis in Niskanen (1971) and Krueger (1974), we do not find evidence that administrators seek to expand the recipient base of UI. Since it would appear that greater recipients would increase demands for the administrators, our finding that there is no impact of administrative errors on recipiency decisions would appear to be a clear rejection of this view.[40]

The model by Alesina and Tabellini (2007) suggests bureaucrats are important in implementing policy, and further suggests significant but one-way interaction between bureaucrats

[40] These results refer to the preferences of the bureaucrats, not of the government in its entirety.

and politicians through what is termed as monitoring. Our results in Table 6 find that the conversation between bureaucrats and politicians is not only a result of politicians ensuring bureaucrats implement the desired policy but is also a two-way conversation. The other side is where bureaucratic errors put pressure on politicians to change policy choices. If, for example, bureaucratic errors were only a symptom of monitoring by politicians, there is no mechanism in the Alesina Tabellini framework where that would be expected to change policy outcomes.

B. Test for Preferences for an Optimal Policy by Bureaucrats

An alternative to the Prendergast model of sympathy for recipients is to consider whether bureaucrats have a preferred policy. That is, if current UI benefits are too low, errors would be expected to grow as bureaucrats put pressure on the policy choice environment to have them increased. Conversely, however, if benefits were considered overly generous, bureaucrats would instead be expected to attempt to get them lowered to their optimal policy. To test this conjecture, we estimate an instrumental variables regression using the full sample of data, as we do not find that there is a discontinuity in the UI policy outcomes when the Governor margin of victory is at 0% to justify the estimation of a RD model in this context.

Table 10 presents the test of whether the *Net OP* responds to changes in the UI policy outcome variables. If bureaucrats have a preferred policy, it would be reasonable to expect the response of *Net Op* to be sensitive to changes in UI policy outcomes because policies that are closer to the position preferred by the bureaucrats would cause *Net Op* to fall. Similarly, policy changes that increased the distance between the preferred policy and the actual policy would result in an increase in *Net Op*. We employ an instrumental variables design because *Net OP* may be

endogenous to programmatic outcomes, such as *UI Ben*, *UI Rec* and *UI Tax*.[41] In essence, our model assumes that *Net OP* are chosen by the bureaucrats, and UI policy outcomes are chosen by politicians. Nonetheless, to the extent they respond to each other they will be simultaneously chosen.[42]

As such, to examine the response of *Net Op*, we estimate three equations showing the impact on Net Overpayments of the three UI outcome variables using instrumental variables:

$$\begin{aligned} Net\ OP_{st} = {} & \eta_1^j DEM\ 1^{st} Term \ln(UI\ Outcome^j) + \eta_2^j DEM\ 2^{nd}\ Term \ln(UI\ Outcome^j) \\ & + \eta_3^j REP\ 1^{st}\ Term \ln(UI\ Outcome^j) + \eta_4^j REP\ 2^{nd} Term \ln(UI\ Outcome^j) \\ & + X_{st}\beta^j + Z_{st}\partial^j + \alpha_s^j + \phi_t^j + \epsilon_{st}^j, \end{aligned} \quad (4)$$

where j indicates that (4) consists of three equations, one each for UI Ben, UI Rec, and UI Tax. As throughout, the policy outcomes variables are defined as net of administrative errors to reflect political preferences. X is the vector of control variables as before, and Z are the two variables we used as IVs earlier, the percent of the public workforce unionized, and the share of UI applicants who use an office to apply. The instrument for the policy outcome is the UI trust fund. We argue that the trust fund is an appropriate instrument because it has been explicitly designed within the UI program to smooth financial outcomes over the business cycle (Craig, Hemissi, Satadru, and Sorensen, 2011). It is possible that a relatively low level of savings would induce the legislature

[41] Because the *Net Overpayments* residuals may depend on the policy outcomes, we instrument outcomes using as an IV the share of UI applications that were in-person. The first stage estimates show that the IV is important for benefits per recipient and for UI taxes, although not for recipients per capita.

[42] We also experimented with a ratchet model. The difficulty with such a strategy is that it is difficult to specify an ideal point. We tried a wide range of alternatives, however, and were unable to find any evidence of a reversal in the signs of the coefficients at any point.

to somewhat restrict UI policies or raise taxes, and conversely a relatively high savings level may induce UI policy expansions or tax reductions.[43]

The Table 10 results show, for all three UI policy outcomes, that *Net OP* is unresponsive to changes in the three UI outcomes. Our empirical estimates suggest that bureaucrats do not have some sort of "optimal" policy that they are trying to implement because policy outcomes seem to have no feedback effects on *Net Op*. We believe these findings demonstrate that the argument proposed in Prendergast is most consistent with the evidence found here. Specifically, bureaucrats behave as if they want to improve payments to the unemployed irrespective of current policies. Furthermore, we believe there is significant evidence that they do so using the policy error rates that result in overpayments of UI benefits.

V. Summary and Conclusions

Our paper has attempted to discern the validity of the assumption that administrative bureaucrats are able to influence actual policy. We test this idea because we are able to use data on the UI program in US states which differentiates policy outcomes as assigned by policy makers from "mistakes" as determined by DOL audits. We thus differentiate state UI spending into benefits per recipient net of mistakes, and the level of mistakes. We perform our empirical test using a panel of UI states from 1996, the start of the audit program, though 2011. We conduct two tests. First, we find evidence in a regression discontinuity framework that state government policies are affected by bureaucratic preferences. These preferences suggest bureaucrats are

[43] The Craig, Hemissi, Satadru, and Sorensen (2011) work explicitly recognizes these possibilities by estimating a buffer stock model to explain political choices.

sympathetic towards their recipients, and attempt to improve especially benefit levels for recipients. It is interesting that bureaucrats seem much less interested in expanding the recipient base, which might be consistent with an expansion of work opportunities for bureaucrats. Also, in our regression discontinuity framework where unemployment insurance policies are the dependent variable, we find that coefficients on the Democratic Governor terms are of the same sign and similar in magnitudes as the coefficients of the Republican Governor terms, thus rejecting Alesina-Tabellini type bureaucrats looking for an optimal policy. Further evidence that bureaucrats do not have an "optimal" policy is that we do not find evidence that policy outcomes feed-back upon *Overpayment* rates. We thus conclude that the theory offered by Prendergast is the most consistent with our empirical findings about bureaucratic behavior in the context of unemployment insurance.

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Table 1: Summary Statistics

	Full Sample $n = 1076$		1996 $n = 45$		2018 $n = 48$	
Variables	Mean	Std	Mean	Std	Mean	Std
UI Gross Overpayments (2005$, millions)	66.11	86.79	41.72	53.61	57.87	77.29
UI Net Overpayments (2005$, millions)	61.99	82.09	36.61	47.77	55.41	74.63
UI Overpayment Rate (%)	10.80	6.46	8.52	4.63	12.34	7.39
UI Annual Benefits (2005$) per recipient	3,362	1,264	2,894	1,525	3,441	1,121
UI Recipients per capita	0.03	0.01	0.03	0.01	0.02	0.01
UI Taxes (2005$) per capita	95.73	53.29	93.51	56.61	83.01	46.82
UI Trust Fund (2005$) per capita	160.25	128.65	215.21	103.64	206.36	147.48
UI Average Weekly Benefits (2005$)	269.20	68.77	185.65	31.35	341.86	76.13
UI Average Duration (weeks)	14.94	2.59	13.94	2.52	14.44	2.45
UI Office Claims (%)	26.32	36.61	89.00	13.81	2.91	9.99
Public Sector Unionization (%)	32.55	17.63	33.97	18.48	31.83	17.80
Manufacturing Workers (%)	10.35	4.34	13.62	5.18	8.75	3.55
Democratic Governor Dummy (%)	43.04	49.54	36.96	48.80	36.17	48.57
GSP (2005$) per capita	41,486	8,329	35,636	6,106	46,046	8,543
Poverty Rate (%)	12.56	3.36	13.09	4.03	11.45	3.06
Pop w/ HS Educ (%)	87.65	4.22	82.84	4.36	91.22	2.67
Pop White (%)	73.09	15.27	78.35	14.13	68.27	15.98
Pop Under 18 (%)	24.28	2.18	26.21	2.01	22.14	1.61
Pop Over 64 (%)	13.58	2.18	12.72	2.04	16.72	1.62

Figure 1

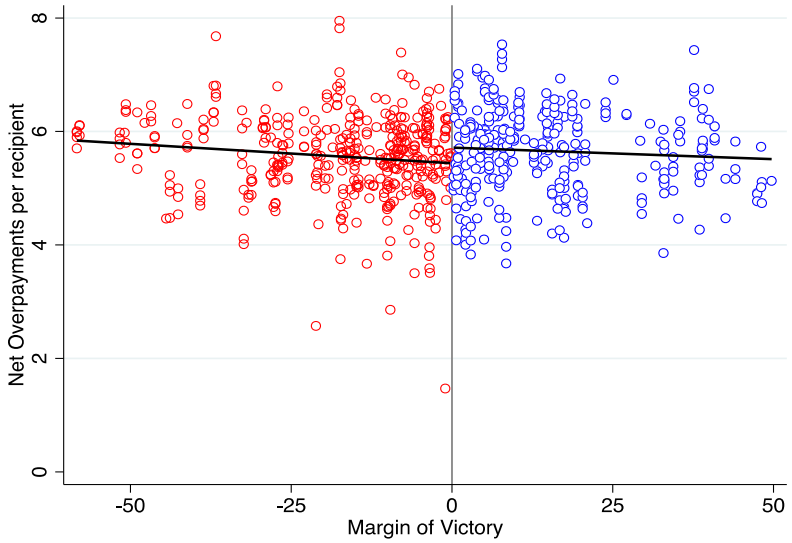

Figure 1 shows strong evidence of a discontinuity in the natural logarithm of Net Overpayments per recipient around the threshold. Local linear regressions are estimated on both sides of the threshold. The margin of victory represents the difference between Democratic and Republican share of the vote in the most recent gubernatorial election; the margin of victory is positive when a Democratic governor is elected and is negative when a Republican governor is elected. For gubernatorial elections with margin of victory within 7.5 percentage points, the estimated discontinuity = 0.003, T-stat = 2.52, $N = 244$.

Figure 2

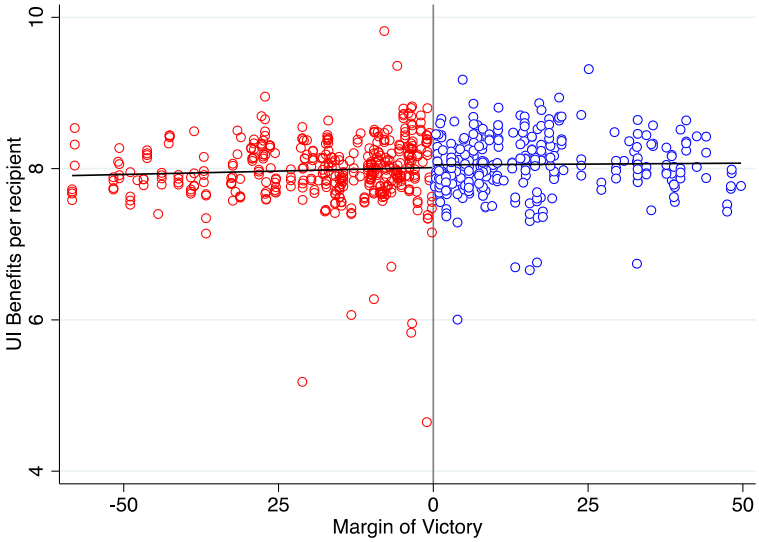

Figure 2 shows no strong evidence of a discontinuity in the natural logarithm of UI Benefits per recipient around the threshold. Local linear regressions are estimated on both sides of the threshold. Local linear regressions are estimated

on both sides of the threshold. The margin of victory represents the difference between Democratic and Republican share of the vote in the most recent gubernatorial election; the margin of victory is positive when a Democratic governor is elected and is negative when a Republican governor is elected. For gubernatorial elections with margin of victory within 7.5 percentage points, the estimated discontinuity = 0.0002, T-stat = 0.22, $N = 244$.

Figure 3

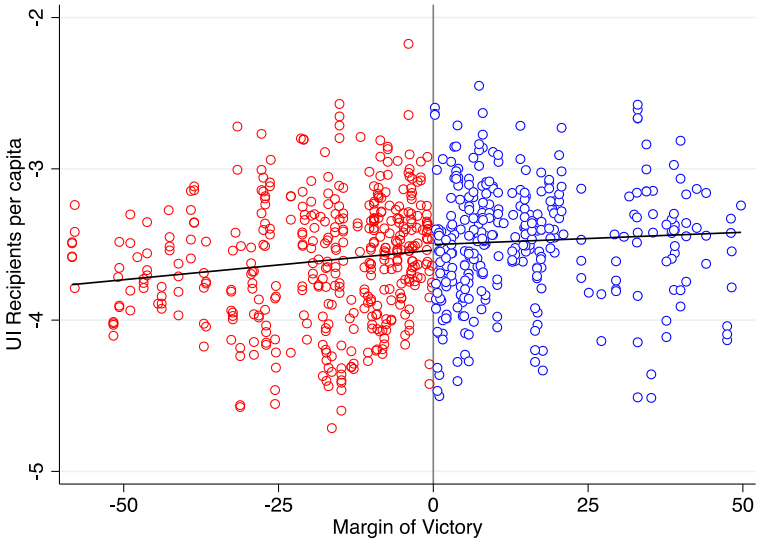

Figure 3 shows no strong evidence of a discontinuity in the natural logarithm of UI Recipients per capita around the threshold. Local linear regressions are estimated on both sides of the threshold. Local linear regressions are estimated on both sides of the threshold. The margin of victory represents the difference between Democratic and Republican share of the vote in the most recent gubernatorial election; the margin of victory is positive when a Democratic governor is elected and is negative when a Republican governor is elected. For gubernatorial elections with margin of victory within 7.5 percentage points, the estimated discontinuity = -0.0003, T-stat = -0.60, $N = 244$.

Figure 4

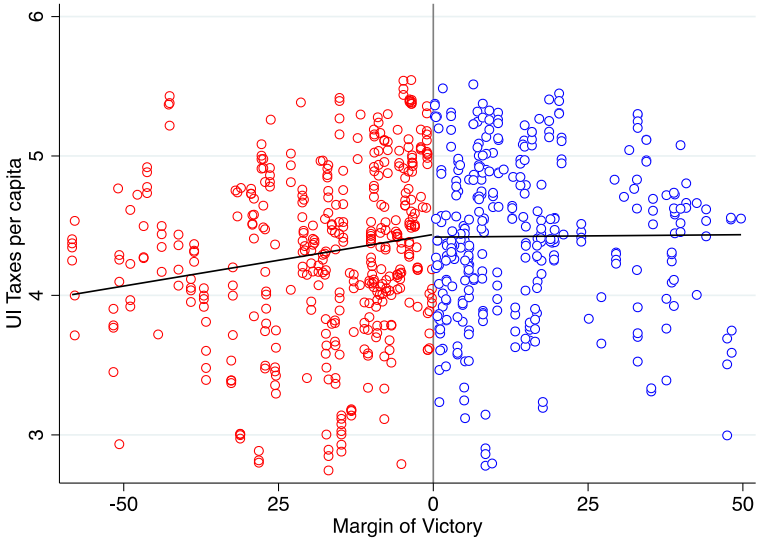

Figure 4 shows no strong evidence of a discontinuity in the natural logarithm of UI Taxes per capita around the threshold. Local linear regressions are estimated on both sides of the threshold. Local linear regressions are estimated on both sides of the threshold. The margin of victory represents the difference between Democratic and Republican share of the vote in the most recent gubernatorial election; the margin of victory is positive when a Democratic governor is elected and is negative when a Republican governor is elected. For gubernatorial elections with margin of victory within 7.5 percentage points, the estimated discontinuity = -0.001, T-stat = -1.76, $N = 244$.

Table 2: UI Overpayments reflect "Surprises" in UI Benefits
Panel A

	Dependent Variable
Independent Variables	ln(UI Benefits per recipient)
Unemployment rate	0.061***
	(0.019)
State Fixed Effects	Yes
Year Fixed Effects	Yes
Controls	Yes
Observations	745
R-squared	0.495

Notes: This table shows the results from a regression of the natural logarithm UI Benefits per recipient on the unemployment rate. Data includes 50 US states and spans the period 1996-2011. State level control variables include: the percentage of the workforce in the manufacturing sector, the natural logarithm of Gross State Product per capita, the poverty rate, the percentage of high school graduates, the percentage of the population that is white, the percentage of the population that is under the age of 18, and the percentage of the population over the age of 64. State and year fixed effects are included. Standard errors are clustered at the state level. Significance levels: *, 10%; **, 5%; ***, 1%.

Panel B

	Dependent Variables	
Independent Variables	ln(Overpayments per capita)	ln(Net Overpayments per capita)
Residuals from Panel A	0.699***	0.672***
	(0.148)	(0.160)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Controls	Yes	YEs
Observations	745	745
R-squared	0.752	0.737

Notes: This table shows the results from regressions of the natural logarithm of UI Overpayments per capita and the natural logarithm of UI Net Overpayments per capita (defined as $\ln(\frac{Overpayments - Underpayments}{State\ population})$) on the residuals obtained in panel A. Data includes 50 US states and spans the period 1996-2011. State level control variables include: the percentage of the

workforce in the manufacturing sector, the natural logarithm of Gross State Product per capita, the poverty rate, the percentage of high school graduates, the percentage of the population that is white, the percentage of the population that is under the age of 18, and the percentage of the population over the age of 64. State and year fixed effects are included. Standard errors are clustered at the state level. Significance levels: *, 10%; **, 5%; ***, 1%.

Table 3: UI Overpayments reflect "Surprises" in UI Recipients
Panel A

	Dependent Variable
Independent Variables	ln(UI Recipients per capita)
Unemployment rate	0.068***
	(0.016)
State Fixed Effects	Yes
Year Fixed Effects	Yes
Controls	Yes
Observations	745
R-squared	0.909

Notes: This table shows the results from a regression of the natural logarithm UI Recipients per capita on the unemployment rate. Data includes 50 US states and spans the period 1996-2011. State level control variables include: the percentage of the workforce in the manufacturing sector, Gross State Product per capita, the poverty rate, the percentage of high school graduates, the percentage of the population that is white, the percentage of the population that is under the age of 18, and the percentage of the population over the age of 64. State and year fixed effects are included. Standard errors are clustered at the state level. Significance levels: *, 10%; **, 5%; ***, 1%.

Panel B

	Dependent Variables	
Independent Variables	ln(Overpayments per capita)	ln(Net Overpayments per capita)
Residuals from Panel A	0.610***	0.568**
	(0.208)	(0.216)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Controls	Yes	Yes
Observations	745	745
R-squared	0.693	0.686

Notes: This table shows the results from regressions of the natural logarithm of UI Overpayments per capita and the natural logarithm of UI Net Overpayments per capita (defined as $\ln(\frac{Overpayments - Underpayments}{State\ population})$) on the residuals obtained in panel A. Data includes 50 US states and spans the period 1996-2011. State level control variables include: the percentage of the workforce in the manufacturing sector, the natural logarithm of Gross State Product per capita, the

poverty rate, the percentage of high school graduates, the percentage of the population that is white, the percentage of the population that is under the age of 18, and the percentage of the population over the age of 64. State and year fixed effects are included. Standard errors are clustered at the state level. Significance levels: *, 10%; **, 5%; ***, 1%.

Table 4: UI Overpayments reflect "Surprises" in UI Taxes

Panel A

	Dependent Variable
Independent Variables	ln(UI Taxes per capita)
Unemployment rate	0.014
	(0.024)
State Fixed Effects	Yes
Year Fixed Effects	Yes
Controls	Yes
Observations	745
R-squared	0.831

Notes: This table shows the results from a regression of the natural logarithm UI Taxes per capita on the unemployment rate. Data includes 50 US states and spans the period 1996-2011. State level control variables include: the percentage of the workforce in the manufacturing sector, the natural logarithm of Gross State Product per capita, the poverty rate, the percentage of high school graduates, the percentage of the population that is white, the percentage of the population that is under the age of 18, and the percentage of the population over the age of 64. State and year fixed effects are included. Standard errors are clustered at the state level. Significance levels: *, 10%; **, 5%; ***, 1%.

Panel B

	Dependent Variables	
Independent Variables	ln(Overpayments per capita)	ln(Net Overpayments per capita)
Residuals from Panel A	0.173**	0.159*
	(0.078)	(0.081)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Controls	Yes	Yes
Observations	745	745
R-squared	0.688	0.682

This table shows the results from regressions of the natural logarithm of UI Overpayments per capita and the natural logarithm of UI Net Overpayments per capita (defined as $\ln(\frac{Overpayments - Underpayments}{State\ population})$) on the residuals obtained in panel A. Data includes 50 US states and spans the period 1996-2011. State level control variables include: the percentage of the

workforce in the manufacturing sector, the natural logarithm of Gross State Product per capita, the poverty rate, the percentage of high school graduates, the percentage of the population that is white, the percentage of the population that is under the age of 18, and the percentage of the population over the age of 64. State and year fixed effects are included. Standard errors are clustered at the state level. Significance levels: *, 10%; **, 5%; ***, 1%.

Table 5: IV-RD Regressions of Policy Outcomes on Net OP

	Coefficient	Standard Error
Instrumented Variables:	**Dependent Variable: UI Ben**	
DEM 1st Term *Net OP	0.515**	(0.217)
DEM 2nd Term *Net OP	0.501**	(0.218)
REP 1st Term *Net OP	0.506**	(0.207)
REP 2nd Term *Net OP	0.478**	(0.234)
Instrumented Variables:	**Dependent Variable: UI Rec**	
DEM 1st Term *Net OP	0.085	(0.093)
DEM 2nd Term *Net OP	0.083	(0.095)
REP 1st Term *Net OP	0.072	(0.087)
REP 2nd Term *Net OP	0.053	(0.093)
Instrumented Variables:	**Dependent Variable: UI Tax**	
DEM 1st Term *Net OP	0.116	(0.121)
DEM 2nd Term *Net OP	0.111	(0.120)
REP 1st Term *Net OP	0.123	(0.116)
REP 2nd Term *Net OP	0.099	(0.125)
Number of Observations	244	

Notes: This table shows the results from RD regressions using Instrumental Variables (IV). The regressions use a subsample of gubernatorial elections with a margin of victory within 7.5 percentage points of zero. The dependent variables are: (1) the natural logarithm of UI Benefits per recipient, (2) the natural logarithm of UI Recipients per capita, and (3) the natural logarithm of UI Taxes per capita. Net Overpayments (Net OP) are interacted with the first and second term of states with a Democrat elected as governor, or a Republican elected as governor. These interaction variables are instrumented using (1) UI claims filed at the UI office as a percentage of total UI claims filed, (2) the public sector unionization rate. Data includes 50 US states and spans the period 1996-2011. Governor margin of victory is included in the regressions. State level control variables include: the unemployment rate, the percentage of the workforce in the manufacturing sector, the natural logarithm of Gross State Product per capita, the poverty rate, the percentage of high school graduates, the percentage of the population that is white, the percentage

of the population that is under 18 years old, and the percentage of the population over the age of 64. State and year fixed effects are included. Standard errors are clustered at the state level. Significance levels: *, 10%; **, 5%; ***, 1%.

Table 5A: IV-RD Regressions of Net OP on Policy Outcomes

	Coefficient	**Standard Error**
Instrumented Variables:	**Dependent Variable: Average Duration**	
DEM 1st Term *Net OP	0.029	(0.038)
DEM 2nd Term *Net OP	0.031	(0.036)
REP 1st Term *Net OP	0.034	(0.036)
REP 2nd Term *Net OP	0.039	(0.039)
Number of Observations	244	

Notes: This table and the 5B show the breakdown with UI Ben, where average annual benefits equals the average weekly benefit amount times average duration of benefits. These tables show that UI Ben cannot be characterized by either aspect.

This table shows the results from RD regressions using Instrumental Variables (IV). The regressions use a subsample of gubernatorial elections margin of victory within 7.5 percentage points of zero. The dependent variable is the natural logarithm of average duration of UI benefits. Net Overpayments (Net OP) are interacted with the first and second term of states with a Democrat elected as governor, or a Republican elected as governor. These interaction variables are instrumented using (1) UI claims filed at the UI office as a percentage of total UI claims filed, (2) the public sector unionization rate. Data includes 50 US states and spans the period 1996-2011. Governor margin of victory is included in the regressions. State level control variables include the unemployment rate, the percentage of the workforce in the manufacturing sector, the natural logarithm of Gross State Product per capita, the poverty rate, the percentage of high school graduates, the percentage of the population that is white, the percentage of the population that is under 18 years old, and the percentage of the population over the age of 64. State and year fixed effects are included. Standard errors are clustered at the state level. Significance levels: *, 10%; **, 5%; ***, 1%.

Table 5B: IV-RD Regressions of Net OP on Policy Outcomes

	Coefficient	Standard Error
Instrumented Variables:	**Dependent Variable: Avg Weekly Ben**	
DEM 1st Term *Net OP	-0.019	(0.028)
DEM 2nd Term *Net OP	-0.021	(0.028)
REP 1st Term *Net OP	-0.011	(0.027)
REP 2nd Term *Net OP	-0.012	(0.029)
Number of Observations	244	

Notes: This table shows the results from RD regressions using Instrumental Variables (IV). The regressions use a subsample of gubernatorial elections margin of victory within 7.5 percentage points of zero. The dependent variable is the natural logarithm of average weekly UI benefits. Net Overpayments (Net OP) are interacted with the first and second term of states with a Democrat elected as governor, or a Republican elected as governor. These interaction variables are instrumented using (1) UI claims filed at the UI office as a percentage of total UI claims filed, (2) the public sector unionization rate. Data includes 50 US states and spans the period 1996-2011. Governor margin of victory is included in the regressions. State level control variables include the unemployment rate, the percentage of the workforce in the manufacturing sector, the natural logarithm of Gross State Product per capita, the poverty rate, the percentage of high school graduates, the percentage of the population that is white, the percentage of the population that is under 18 years old, and the percentage of the population over the age of 64. State and year fixed effects are included. Standard errors are clustered at the state level. Significance levels: *, 10%; **, 5%; ***, 1%.

Table 6: RD Regressions of Policy Outcomes on Net OP
Without IVs

	Coefficient	Standard Error
Independent Variables:	**Dependent Variable: UI Ben**	
DEM 1st Term *Net OP	0.495***	(0.106)
DEM 2nd Term *Net OP	0.473***	(0.108)
REP 1st Term *Net OP	0.497***	(0.106)
REP 2nd Term *Net OP	0.491***	(0.125)
Independent Variables:	**Dependent Variable: UI Rec**	
DEM 1st Term *Net OP	-0.080	(0.049)
DEM 2nd Term *Net OP	-0.084*	(0.048)
REP 1st Term *Net OP	-0.068	(0.043)
REP 2nd Term *Net OP	-0.104*	(0.053)
Independent Variables:	**Dependent Variable: UI Tax**	
DEM 1st Term *Net OP	0.008	(0.019)
DEM 2nd Term *Net OP	-0.000	(0.021)
REP 1st Term *Net OP	0.026	(0.018)
REP 2nd Term *Net OP	-0.015	(0.024)
Number of Observations	244	

Notes: This table shows the results from Regression Discontinuity (RD) regressions. The regressions use a subsample of gubernatorial elections with margin of victory within 7.5 percentage points of zero. The dependent variables are: (1) the natural logarithm of UI Benefits per recipient, (2) the natural logarithm of UI Recipients per capita, and (3) the natural logarithm of UI Taxes per capita. Net Overpayments (Net OP) are interacted with the first and second term of states with a Democrat elected as governor, or a Republican elected as governor. Net overpayments is defined as overpayments per recipient less underpayments per recipient. Data includes 50 US states and spans the period 1996-2011. Governor margin of victory is included in the regressions. State level control variables include the unemployment rate, the percentage of the workforce in the manufacturing sector, the natural logarithm of Gross State Product per capita, the poverty rate, the percentage of high school graduates, the percentage of the population that is white, the percentage of the population that is under 18 years old, and the percentage of the population

over the age of 64. State and year fixed effects are included. Standard errors are clustered at the state level. Significance levels: *, 10%; **, 5%; ***, 1%.

Table 7: IV-RD Regressions of Policy Outcomes on OP
(Gross not Net OP)

	Coefficient	Standard Error
Instrumented Variables:	**Dependent Variable: UI Ben**	
DEM 1st Term *OP	0.610***	(0.203)
DEM 2nd Term *OP	0.594***	(0.205)
REP 1st Term *OP	0.594***	(0.192)
REP 2nd Term *OP	0.577***	(0.220)
Instrumented Variables:	**Dependent Variable: UI Rec**	
DEM 1st Term *OP	0.096	(0.105)
DEM 2nd Term *OP	0.094	(0.105)
REP 1st Term *OP	0.082	(0.099)
REP 2nd Term *OP	0.066	(0.103)
Instrumented Variables:	**Dependent Variable: UI Tax**	
DEM 1st Term *OP	0.126	(0.131)
DEM 2nd Term *OP	0.121	(0.130)
REP 1st Term *OP	0.132	(0.126)
REP 2nd Term *OP	0.110	(0.134)
Number of Observations	244	

Notes: This table shows the results from RD regressions using Instrumental Variables (IV). The regressions use a subsample of gubernatorial elections margin of victory within 7.5 percentage points of zero. The dependent variables are: (1) the natural logarithm of UI Benefits per recipient, (2) the natural logarithm of UI Recipients per capita, and (3) the natural logarithm of UI Taxes per capita. The dummy variables for first and second governor terms for Democrats and Republicans are interacted with overpayments per capita. These variables are instrumented using: (1) UI claims filed at the UI office as a percentage of total UI claims filed, (2) the public sector unionization rate. Data includes 50 US states and spans the period 1996-2011. Governor margin of victory is included in the regressions. State level control variables include the unemployment rate, the percentage of the workforce in the manufacturing sector, the natural logarithm of Gross State Product per capita, the poverty rate, the percentage of high school graduates, the percentage of the population that is white, the percentage of the population that is under 18 years old, and the percentage of the

population over the age of 64. State and year fixed effects are included. Standard errors are clustered at the state level. Significance levels: *, 10%; **, 5%; ***, 1%.

Table 8: IV-RD Regressions: Governor Pair Years Of Policy Outcomes on Net OP

	Coefficient	Standard Error
Instrumented Variables	**Dependent Variable: UI Ben**	
DEM[1,2]*Net OP	0.664***	(0.106)
DEM[3,4]*Net OP	0.657***	(0.108)
DEM[5,6]*Net OP	0.644***	(0.115)
DEM[7,8]*Net OP	0.617***	(0.114)
REP[1,2] *Net OP	0.635***	(0.086)
REP[3,4] *Net OP	0.625***	(0.089)
REP[5,6] *Net OP	0.621***	(0.111)
REP[7,8] *Net OP	0.618***	(0.127)
Instrumented Variables	**Dependent Variable: UI Rec**	
DEM[1,2]*Net OP	-0.009	(0.033)
DEM[3,4]*Net OP	-0.015	(0.033)
DEM[5,6]*Net OP	-0.015	(0.036)
DEM[7,8]*Net OP	-0.013	(0.036)
REP[1,2] *Net OP	-0.014	(0.028)
REP[3,4] *Net OP	-0.020	(0.027)
REP[5,6] *Net OP	-0.044	(0.028)

REP[7,8] *Net OP	-0.061	(0.049)

Instrumented Variables	**Dependent Variable: UI Tax**	
DEM[1,2]*Net OP	0.025	(0.048)

Table 8: IV-RD Regressions: Governor Pair Years Of Policy Outcomes on Net OP (cont)

	Coefficient	**Standard Error**
Instrumented Variables	**Dependent Variable: UI Tax**	
DEM[3,4]*Net OP	0.034	(0.048)
DEM[5,6]*Net OP	0.024	(0.053)
DEM[7,8]*Net OP	0.034	(0.051)
REP[1,2] *Net OP	0.044	(0.039)
REP[3,4] *Net OP	0.039	(0.037)
REP[5,6] *Net OP	-0.003	(0.039)
REP[7,8] *Net OP	0.009	(0.050)
Observations	244	

Notes: This table shows the results from IV-RD; the first and second governor terms are decomposed into pairs of years. The regressions use a subsample of gubernatorial elections margin of victory within 7.5 percentage points of zero. The dependent variables are: (1) the natural logarithm of UI Benefits per recipient, (2) the natural logarithm of UI Recipients per capita, and (3) the natural logarithm of UI Taxes per capita. The dummy variables for each pair of years for Democrats and Republicans are interacted with net overpayments per capita. These variables are instrumented using: (1) UI claims filed in an office as a percentage of total UI claims filed, (2) the public sector unionization rate. Data includes 50 US states and spans the period 1996-2011. Governor margin of victory is included in the regressions. State level control variables include the unemployment rate, the % of the workforce in the manufacturing sector, the natural logarithm of Gross State Product per capita, the poverty rate, the % of high school graduates, the % of the population that is white, the % of the population that is under 18 years old, and the % of the population over the age of 64. State and year fixed effects are included. Standard errors are clustered at the state level. Significance levels: *, 10%; **, 5%; ***, 1%.

Table 9: IV-RD Regressions: Dummy Variables for Governor
Collapsing All Terms

	Coefficient	Standard Error
Instrumented Variables:	**Dependent Variable: UI Ben**	
DEM *Net OP	0.435*	(0.255)
REP *Net OP	0.424*	(0.239)
Instrumented Variables:	**Dependent Variable: UI Rec**	
DEM *Net OP	-0.129	(0.178)
REP *Net OP	-0.142	(0.175)
Instrumented Variables:	**Dependent Variable: UI Tax**	
DEM *Net OP	0.266	(0.267)
REP *Net OP	0.259	(0.258)

Notes: This table shows the results from RD regressions using Instrumental Variables (IV). The regressions use a subsample of gubernatorial elections margin of victory within 7.5 percentage points of zero. The dependent variables are: (1) the natural logarithm of UI Benefits per recipient, (2) the natural logarithm of UI Recipients per capita, and (3) the natural logarithm of UI Taxes per capita. Net Overpayments (Net OP) are interacted with a dummy variable indicating a Democrat is elected as governor, or a Republican is elected as governor. These interaction variables are instrumented using (1) UI claims filed at the UI office as a percentage of total UI claims filed, (2) the public sector unionization rate. Data includes 50 US states and spans the period 1996-2011. Governor margin of victory is included in the regressions. State level control variables include the unemployment rate, the percentage of the workforce in the manufacturing sector, the natural logarithm of Gross State Product per capita, the poverty rate, the percentage of high school graduates, the percentage of the population that is white, the percentage of the population that is under 18 years old, and the percentage of the population over the age of 64. State and year fixed effects are included. Standard errors are clustered at the state level. Significance levels: *, 10%; **, 5%; ***, 1%.

Table 10: IV Regressions of Net OP on Policy Outcomes
Test of Optimal Policy by Bureaucrats

	Coefficient	Standard Error
Instrumented Variables:	**Dependent Variable: Net OP**	
DEM 1st Term UI Ben	1.040	(1.095)
DEM 2nd Term UI Ben	1.034	(1.105)
REP 1st Term UI Ben	1.034	(1.102)
REP 2nd Term UI Ben	1.049	(1.104)
Instrumented Variables:	**Dependent Variable: Net OP**	
DEM 1st Term UI Rec	-8.914	(27.101)
DEM 2nd Term UI Rec	-8.910	(27.186)
REP 1st Term UI Rec	-8.896	(27.124)
REP 2nd Term UI Rec	-8.894	(27.036)
Instrumented Variables:	**Dependent Variable: Net OP**	
DEM 1st Term UI Tax	5.628	(21.304)
DEM 2nd Term UI Tax	5.526	(21.008)
REP 1st Term UI Tax	5.565	(21.167)
REP 2nd Term UI Tax	5.583	(21.156)
Number of Observations	745	

Notes: This table shows the results from IV regressions. The dependent variables are: (1) the natural logarithm of UI Benefits per recipient, (2) the natural logarithm of UI Recipients per capita, and (3) the natural logarithm of UI Taxes per capita. The policy outcome variables are instrumented using interactions of first and second governor terms for Democrats and Republicans with UI Trust Fund per capita. Data includes 50 US states and spans the period 1996-2011. State level control variables include: the unemployment rate, the percentage of the workforce in the manufacturing sector, the natural logarithm of Gross State Product per capita, the poverty rate, the percentage of high school graduates, the percentage of the population that is white, the percentage of the population that is under 18 years old, and the percentage of the population over the age of 64. State and year fixed effects are included. Standard errors are clustered at the state level. Significance levels: *, 10%; **, 5%; ***, 1%.

APPENDIX

Table A1: First Stage Regressions

	Dependent Variables			
Instrumental Variables	**DEM 1st Term NET OP**	**DEM 2nd Term NET OP**	**REP 1st Term NET OP**	**REP 2nd Term NET OP**
DEM 1st Term *%Office	0.182	-0.028	0.349	0.272
	(0.672)	(0.128)	(0.611)	(0.319)
DEM 2nd Term *%Office	0.771	-0.955	-0.755	0.300
	(1.060)	(0.853)	(0.748)	(0.376)
REP 1st Term *%Office	-1.116	-0.041	1.196*	0.167
	(0.751)	(0.147)	(0.651)	(0.271)
REP 2nd Term *%Office	-0.878	0.324	0.237	1.727
	(0.864)	(0.284)	(0.889)	(1.714)
DEM 1st Term *%Union	0.067***	-0.016**	-0.064***	-0.013
	(0.017)	(0.008)	(0.015)	(0.011)
DEM 2nd Term *%Union	-0.049**	0.092***	-0.046***	-0.012
	(0.017)	(0.012)	(0.015)	(0.012)
REP 1st Term *%Union	-0.032*	-0.006	0.023	-0.006
	(0.016)	(0.006)	(0.019)	(0.009)
REP 2nd Term *%Union	-0.034*	-0.012	-0.060***	0.065**
	(0.018)	(0.008)	(0.017)	(0.028)
***F*-statistic**	62.31	151.36	173.38	433.65
Number of Observations	244	244	244	244

Notes: This table shows the first stage regressions for Table 5. The estimation uses a subsample of races with governor elections with a margin of victory within 7.5 percentage points of zero. Data includes 50 US states and spans the period 1996-2011. State level control variables include: the unemployment rate, the percentage of the workforce in the manufacturing sector, the natural logarithm of Gross State Product per capita, the poverty rate, the percentage of high school graduates, the percentage of the population that is white, the percentage of the population that is under 18 years old, and the percentage of the population over the age of 64. State and year fixed

effects are included. Standard errors are clustered at the state level. Significance levels: *, 10%; **, 5%; ***, 1%.

Table A2: First Stage Regressions

	Dependent Variables			
Instrumental Variables	**DEM 1st Term *UI Tax**	**DEM 2nd Term *UI Tax**	**REP 1st Term *UI Tax**	**REP 2nd Term *UI Tax**
DEM 1st Term *Trust Fund	-0.908***	0.226***	0.392***	0.300***
	(0.094)	(0.050)	(0.064)	(0.059)
DEM 2nd Term *Trust Fund	0.449***	-1.105***	0.354***	0.293***
	(0.068)	(0.115)	(0.058)	(0.065)
REP 1st Term *Trust Fund	0.353***	0173***	-0.857***	0.331***
	(0.045)	(0.044)	(0.084)	(0.067)
REP 2nd Term *Trust Fund	0.319***	0.194***	0.440***	-0.952****
	(0.063)	(0.048)	(0.077)	(0.143)
***F*-statistic**	92.39	39.93	79.45	107.40
Number of Observations	244	244	244	244

Notes: This table shows the first stage regressions for Table 7. The estimation uses a subsample of races with governor elections with a margin of victory within 7.5 percentage points of zero. Data includes 50 US states and spans the period 1996-2011. State level control variables include the unemployment rate, the percentage of the workforce in the manufacturing sector, the natural logarithm of Gross State Product per capita, the poverty rate, the percentage of high school graduates, the percentage of the population that is white, the percentage of the population that is under 18 years old, and the percentage of the population over the age of 64. State and year fixed effects are included. Standard errors are clustered at the state level. Significance levels: *, 10%; **, 5%; ***, 1%.

Table A3: IV-RD Regressions: Margin of Victory 3%

	Coefficient	Standard Error
Instrumented Variables:	**Dependent Variable: UI Ben**	
DEM 1st Term *Net OP	0.707***	(0.170)
DEM 2nd Term *Net OP	0.803***	(0.196)
REP 1st Term *Net OP	0.717***	(0.167)
REP 2nd Term *Net OP	0.742***	(0.192)
Instrumented Variables:	**Dependent Variable: UI Rec**	
DEM 1st Term *Net OP	0.076	(0.086)
DEM 2nd Term *Net OP	0.096	(0.088)
REP 1st Term *Net OP	0.034	(0.092)
REP 2nd Term *Net OP	0.041	(0.097)
Instrumented Variables:	**Dependent Variable: UI Tax**	
DEM 1st Term *Net OP	0.140	(0.112)
DEM 2nd Term *Net OP	0.208*	(0.125)
REP 1st Term *Net OP	0.114	(0.116)
REP 2nd Term *Net OP	0.097	(0.115)
Number of Observations	95	

Notes: This table shows the results from RD regressions using Instrumental Variables (IV). The regressions use a subsample of gubernatorial elections margin of victory within 3 percentage points of zero. The dependent variables are: (1) the natural logarithm of UI Benefits per recipient, (2) the natural logarithm of UI Recipients per capita, and (3) the natural logarithm of UI Taxes per capita. Net Overpayments (Net OP) are interacted with the first and second term of states with a Democrat elected as governor, or a Republican elected as governor. These interaction variables are instrumented using (1) UI claims filed at the UI office as a percentage of total UI claims filed, (2) the public sector unionization rate. Data includes 50 US states and spans the period 1996-2011. Governor margin of victory is included in the regressions. State level control variables include: the unemployment rate, the percentage of the workforce in the manufacturing sector, the natural logarithm of Gross State Product per capita, the poverty rate, the percentage of high school graduates, the percentage of the population that is white, the percentage of the population that is under 18 years old, and the percentage of the population over the age of 64. State and year fixed effects are included. Standard errors are clustered at the state level. Significance levels: *, 10%; **, 5%; ***, 1%.

Table A4: IV-RD Regressions: Margin of Victory 5%

	Coefficient	Standard Error
Instrumented Variables:	**Dependent Variable: UI Ben**	
DEM 1st Term *Net OP	0.707***	(0.170)
DEM 2nd Term *Net OP	0.803***	(0.196)
REP 1st Term *Net OP	0.717***	(0.167)
REP 2nd Term *Net OP	0.742***	(0.192)
Instrumented Variables:	**Dependent Variable: UI Rec**	
DEM 1st Term *Net OP	0.076	(0.086)
DEM 2nd Term *Net OP	0.096	(0.088)
REP 1st Term *Net OP	0.034	(0.092)
REP 2nd Term *Net OP	0.041	(0.097)
Instrumented Variables:	**Dependent Variable: UI Tax**	
DEM 1st Term *Net OP	0.140	(0.112)
DEM 2nd Term *Net OP	0.208*	(0.125)
REP 1st Term *Net OP	0.114	(0.116)
REP 2nd Term *Net OP	0.097	(0.115)
Number of Observations	166	

Notes: This table shows the results from RD regressions using Instrumental Variables (IV). The regressions use a subsample of gubernatorial elections margin of victory within 5 percentage points of zero. The dependent variables are: (1) the natural logarithm of UI Benefits per recipient, (2) the natural logarithm of UI Recipients per capita, and (3) the natural logarithm of UI Taxes per capita. Net Overpayments (Net OP) are interacted with the first and second term of states with a Democrat elected as governor, or a Republican elected as governor. These interaction variables are instrumented using (1) UI claims filed at the UI office as a percentage of total UI claims filed, (2) the public sector unionization rate. Data includes 50 US states and spans the period 1996-2011. Governor margin of victory is included in the regressions. State level control variables include: the unemployment rate, the percentage of the workforce in the manufacturing sector, the natural logarithm of Gross State Product per capita, the poverty rate, the percentage of high school graduates, the percentage of the population that is white, the percentage of the population that is under 18 years old, and the percentage of the population over the age of 64. State and year fixed effects are included. Standard errors are clustered at the state level. Significance levels: *, 10%; **, 5%; ***, 1%.

Table A5: IV-RD Regressions: Margin of Victory 10%

	Coefficient	Standard Error
Instrumented Variables:	**Dependent Variable: UI Ben**	
DEM 1st Term *Net OP	0.071	(0.242)
DEM 2nd Term *Net OP	0.057	(0.241)
REP 1st Term *Net OP	0.054	(0.237)
REP 2nd Term *Net OP	0.036	(0.248)
Instrumented Variables:	**Dependent Variable: UI Rec**	
DEM 1st Term *Net OP	-0.037	(0.092)
DEM 2nd Term *Net OP	-0.041	(0.093)
REP 1st Term *Net OP	-0.044	(0.091)
REP 2nd Term *Net OP	-0.057	(0.097)
Instrumented Variables:	**Dependent Variable: UI Tax**	
DEM 1st Term *Net OP	0.217	(0.248)
DEM 2nd Term *Net OP	0.222	(0.252)
REP 1st Term *Net OP	0.200	(0.233)
REP 2nd Term *Net OP	0.218	(0.252)
Number of Observations	333	

Notes: This table shows the results from RD regressions using Instrumental Variables (IV). The regressions use a subsample of gubernatorial elections margin of victory within 10 percentage points of zero. The dependent variables are: (1) the natural logarithm of UI Benefits per recipient, (2) the natural logarithm of UI Recipients per capita, and (3) the natural logarithm of UI Taxes per capita. Net Overpayments (Net OP) are interacted with the first and second term of states with a Democrat elected as governor, or a Republican elected as governor. These interaction variables are instrumented using (1) UI claims filed at the UI office as a percentage of total UI claims filed, (2) the public sector unionization rate. Data includes 50 US states and spans the period 1996-2011. Governor margin of victory is included in the regressions. State level control variables include: the unemployment rate, the percentage of the workforce in the manufacturing sector, the natural logarithm of Gross State Product per capita, the poverty rate, the percentage of high school graduates, the percentage of the population that is white, the percentage of the population that is under 18 years old, and the percentage of the population over the age of 64. State and year fixed effects are included. Standard errors are clustered at the state level. Significance levels: *, 10%; **, 5%; ***, 1%.

Table A6: IV-RD Regressions: Margin of Victory 25%

	Coefficient	Standard Error
Instrumented Variables:	**Dependent Variable: UI Ben**	
DEM 1st Term *Net OP	-0.308	(0.280)
DEM 2nd Term *Net OP	-0.314	(0.279)
REP 1st Term *Net OP	-0.336	(0.288)
REP 2nd Term *Net OP	-0.363	(0.295)
Instrumented Variables:	**Dependent Variable: UI Rec**	
DEM 1st Term *Net OP	0.079	(0.079)
DEM 2nd Term *Net OP	0.076	(0.079)
REP 1st Term *Net OP	0.071	(0.080)
REP 2nd Term *Net OP	0.067	(0.079)
Instrumented Variables:	**Dependent Variable: UI Tax**	
DEM 1st Term *Net OP	0.138	(0.210)
DEM 2nd Term *Net OP	0.140	(0.210)
REP 1st Term *Net OP	0.147	(0.213)
REP 2nd Term *Net OP	0.155	(0.214)
Number of Observations	561	

Notes: This table shows the results from RD regressions using Instrumental Variables (IV). The regressions use a subsample of gubernatorial elections margin of victory within 25 percentage points of zero. The dependent variables are: (1) the natural logarithm of UI Benefits per recipient, (2) the natural logarithm of UI Recipients per capita, and (3) the natural logarithm of UI Taxes per capita. Net Overpayments (Net OP) are interacted with the first and second term of states with a Democrat elected as governor, or a Republican elected as governor. These interaction variables are instrumented using (1) UI claims filed at the UI office as a percentage of total UI claims filed, (2) the public sector unionization rate. Data includes 50 US states and spans the period 1996-2011. Governor margin of victory is included in the regressions. State level control variables include: the unemployment rate, the percentage of the workforce in the manufacturing sector, the natural logarithm of Gross State Product per capita, the poverty rate, the percentage of high school graduates, the percentage of the population that is white, the percentage of the population that is under 18 years old, and the percentage of the population over the age of 64. State and year fixed effects are included. Standard errors are clustered at the state level. Significance levels: *, 10%; **, 5%; ***, 1%.

Table A7: IV-RD Regressions: Full sample

	Coefficient	Standard Error
Instrumented Variables:	**Dependent Variable: UI Ben**	
DEM 1st Term Net OP	-0.714	(0.178)
DEM 2nd Term Net OP	-0.180	(0.178)
REP 1st Term Net OP	-0.186	(0.175)
REP 2nd Term Net OP	-0.196	(0.177)
Instrumented Variables:	**Dependent Variable: UI Rec**	
DEM 1st Term Net OP	0.066	(0.069)
DEM 2nd Term Net OP	0.068	(0.069)
REP 1st Term Net OP	0.056	(0.069)
REP 2nd Term Net OP	0.052	(0.068)
Instrumented Variables:	**Dependent Variable: UI Tax**	
DEM 1st Term Net OP	0.060	(0.176)
DEM 2nd Term Net OP	0.073	(0.177)
REP 1st Term Net OP	0.063	(0.173)
REP 2nd Term Net OP	0.061	(0.171)
Number of Observations	745	

Notes: This table shows the results from RD regressions using Instrumental Variables (IV). The regressions use the full data sample. The dependent variables are: (1) the natural logarithm of UI Benefits per recipient, (2) the natural logarithm of UI Recipients per capita, and (3) the natural logarithm of UI Taxes per capita. Net Overpayments (Net OP) are interacted with the first and second term of states with a Democrat elected as governor, or a Republican elected as governor. These interaction variables are instrumented using (1) UI claims filed at the UI office as a percentage of total UI claims filed, (2) the public sector unionization rate. Data includes 50 US states and spans the period 1996-2011. Governor margin of victory is included in the regressions. State level control variables include: the unemployment rate, the percentage of the workforce in the manufacturing sector, the natural logarithm of Gross State Product per capita, the poverty rate, the percentage of high school graduates, the percentage of the population that is white, the percentage of the population that is under 18 years old, and the percentage of the population over the age of 64. State and year fixed effects are included. Standard errors are clustered at the state level. Significance levels: *, 10%; **, 5%; ***, 1%.

Table A8: OLS Regressions of Policy Outcomes on Net OP

	Coefficient	Standard Error
Independent Variables:	**Dependent Variable: UI Ben**	
DEM 1st Term *Net OP	0.295***	(0.086)
DEM 2nd Term *Net OP	0.289***	(0.085)
REP 1st Term *Net OP	0.295***	(0.085)
REP 2nd Term *Net OP	0.287***	(0.083)
Independent Variables:	**Dependent Variable: UI Rec**	
DEM 1st Term *Net OP	-0.026	(0.019)
DEM 2nd Term *Net OP	-0.026	(0.019)
REP 1st Term *Net OP	-0.030	(0.020)
REP 2nd Term *Net OP	-0.031	(0.019)
Independent Variables:	**Dependent Variable: UI Tax**	
DEM 1st Term *Net OP	0.022	(0.023)
DEM 2nd Term *Net OP	0.033	(0.022)
REP 1st Term *Net OP	0.021	(0.019)
REP 2nd Term *Net OP	0.019	(0.021)
Number of Observations	745	

This table shows the results from OLS regressions. The estimation uses the full sample. The dependent variables are (1) the natural logarithm of UI Benefits per recipient, (2) the natural logarithm of UI Recipients per capita, and (3) the natural logarithm of UI Taxes per capita. The independent variables are interactions of first and second governor terms for Democrats and Republicans with net overpayments, which is defined as overpayments per recipient less underpayments per recipient. State level control variables are include the unemployment rate, the percentage of the workforce in the manufacturing sector, the natural logarithm of Gross State Product per capita, the poverty rate, the percentage of high school graduates, the percentage of the population that is white, the percentage of the population that is under 18 years old, and the percentage of the population over the age of 64. State and year fixed effects are included. Standard errors are clustered at the state level. Significance levels: *, 10%; **, 5%; ***, 1%.

Table A9: OLS Regressions of Net OP on Policy Outcomes

	Coefficient	Standard Error
Independent Variables:	**Dependent Variable: Net OP**	
DEM 1st Term UI Ben	0.721***	(0.142)
DEM 2nd Term UI Ben	0.724***	(0.143)
REP 1st Term UI Ben	0.719***	(0.142)
REP 2nd Term UI Ben	0.732***	(0.140)
Independent Variables:	**Dependent Variable: Net OP**	
DEM 1st Term UI Rec	-0.445*	(0.093)
DEM 2nd Term UI Rec	-0.444**	(0.095)
REP 1st Term UI Rec	-0.426*	(0.087)
REP 2nd Term UI Rec	-0.447**	(0.093)
Independent Variables:	**Dependent Variable: Net OP**	
DEM 1st Term UI Tax	0.085	(0.072)
DEM 2nd Term UI Tax	0.072	(0.072)
REP 1st Term UI Tax	0.077	(0.071)
REP 2nd Term UI Tax	0.093	(0.077)

This table shows the results from OLS regressions. The estimations use the full sample. The dependent variables are (1) the natural logarithm of UI Benefits per recipient, (2) the natural logarithm of UI Recipients per capita, and (3) the natural logarithm of UI Taxes per capita. The independent variables are interactions of first and second governor terms for Democrats and Republicans with net overpayments, which is defined as overpayments per recipient less underpayments per recipient. State level control variables include the unemployment rate, the percentage of the workforce in the manufacturing sector, the natural logarithm of Gross State Product per capita, the poverty rate, the percentage of high school graduates, the percentage of the population that is white, the percentage of the population that is under 18 years old, and the percentage of the population over the age of 64. State and year fixed effects are included. Standard errors are clustered at the state level. Significance levels: *, 10%; **, 5%; ***, 1%.