Do Public Officials Exhibit Social Class Biases when they Handle Casework? Evidence from Multiple Correspondence Experiments

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Are public officials more responsive to requests from affluent or poor constituents? A growing body of evidence suggests that lawmakers are more responsive to the rich when they craft policy. However, some scholars theorize that officials also exhibit a corresponding bias *in favor of the poor* when they handle casework, essentially giving policy to the rich and services to the poor. In this paper, we test this casework prediction using four experiments in which confederates sent simple requests to state or local officials. In each, our confederates' reported social classes were randomly assigned and signaled with a brief introductory statement mentioning the sender's occupation or economic situation. Across our samples, we find precisely-estimated null effects of social class biases: the officials we studied were equally likely to respond regardless of the constituent's class. These findings raise doubts about whether casework is really a class-biased process.

A growing body of evidence suggests that politicians in the United States are far more responsive to the preferences of affluent Americans than to the views of middle- and working-class citizens. When legislators cast roll call votes, their choices are more strongly associated with the views of higher-income citizens than with those of the less fortunate (Bartels 2008; Jacobs and Druckman 2011). When laws change, they tend to move toward outcomes that more privileged Americans favor, regardless of what less affluent citizens want (Gilens 2012; Hill and Leighley 1992; Rigby and Wright 2013; Schumaker and Getter 1977).[1]

If the privileged tend to get their way in the legislative process, do they also tend to get their way in the many other important stages of the political process? Are the federal agencies that implement new laws more responsive to the views of the rich, too? Do the street-level bureaucrats who carry out the day-to-day work of federal, state, and local governments exhibit the same kind of unequal responsiveness scholars have observed among members of Congress? When citizens reach out to public officials for help with day-to-day needs—with *casework* requests—are the rich favored in that process, too? In short, just how deep does the unequal responsiveness scholars have observed in the passage of state and federal laws really run?

This paper focuses on casework requests at the state and local levels. Every day, thousands of citizens contact state and local officials to ask for help with various government programs, agencies, and benefits. These more granular interactions are far more common and perhaps more consequential than interactions federal government officials (e.g., Soss 1999).

These direct constituent services, moreover, may be one site where the less fortunate are actually *better* represented than the affluent. One emerging school of thought (e.g., Foster-Molina 2015, 1) argues that more affluent Americans tend to have more clear and forceful policy preferences, while less affluent Americans tend to have more concerns about the basic demands of

[1] But see Erikson and Bhatti (2011) and Soroka and Wlezien (2010).

day-to-day life. In this view, politicians seeking to keep their constituents happy might essentially give policy to the affluent and services to the less fortunate, giving rise to a system in which "the privileged are disproportionately influential with respect to legislation, [while] the less privileged are disproportionately served through constituent service."

Do the less fortunate really receive preferential treatment when they put in constituent service or casework requests? Or, does the unequal responsiveness scholars have observed in federal and state lawmaking extend to casework, too? To find out, we conducted four randomized-control trials on state and local public officials. In each experiment, we sent the kinds of requests that officials frequently receive from constituents seeking their help. Experiments 1 and 2 focused on *state legislators*, a group of politicians that collectively has a powerful influence over distributional outcomes and that has been found to be unequally responsive when it comes to legislation (Kelley and Witko 2012; Rigby and Wright 2013). In these experiments, a confederate emailed legislators with simple requests—the first asking for help registering to vote and the second requesting a brief in-person meeting—and randomly varied the confederate's stated occupation. Experiment 3 focused on *public school principals*, consequential local "street-level bureaucrats" who also have frequent contact with citizens. In this larger-scale experiment, confederates asked principals for information on school music and art programs. The last experiment, experiment 4, focused on *mayors* in the United States—important elected officials who influence public policy and constituent-government interactions in important ways.

In all of these experiments, we randomly varied whether public officials received correspondence from a more or less affluent person by varying the individual's stated occupation (Experiments 1, 2, and 4) or biographical narrative (Experiment 3). This approach follows the approach of similar recent audit studies on racial, ethnic, age, and gender bias (Adida, Laitin, and

Valfort 2010; Butler and Broockman 2011; Butler, Karpowitz, Pope 2011; Kalla, Rosenbluth, and Teele 2017; Yinger 2001).

Whereas other recent audit studies find clear evidence that politicians discriminate on the basis of race, ethnicity, age, and religion when responding to constituent requests, we find no evidence of an analogous social class bias in any of our experiments. In Experiments 1 and 2, state lawmakers were just as likely to grant our confederate's requests regardless of whether they perceived the confederate as an HR professional or a dishwasher in a restaurant. In Experiment 3, public school principals were just as likely to respond to request for information from a citizen who described financial hardships and a citizen who did not. Experiment 4—which magnified the class divisions between constituent social classes even further by making comparisons between jobs at the absolute top of the income distribution to those at the absolute bottom—likewise found (precisely-estimated) non-significant and substantively trivial differences. When pooled together, the 95% confidence interval from these experiments allow us to confidently rule out differences in favor of the more affluent confederate as small as 1.32 percentage points—an effect that is statistically and substantively smaller than previous audit studies of race, ethnicity, age, and religion.[2]

These null findings represent a significant departure, both from the growing research on demographic biases in political casework, and from the research on unequal responsiveness among politicians in the United States. The non-differences we observe do not appear to be the result of a lack of statistical power, nor can we attribute them to the level or type of office studied or the type of social class manipulation being used. Casework simply may not be as biased towards the rich as other aspects of the government, as biased towards the poor as scholars theorize, or as biased along social class lines as it is on other demographic dimensions.

[2] On the flip side, our effects are precise enough to rule out effects as small as 2.8 percentage points in favor of the less affluent confederate.

Past Research

Most recent work on constituent services has found clear evidence that politicians prioritize requests from some social groups over others. The basic theoretical logic is straightforward: politicians usually receive more requests than they can answer, so they prioritize requests from social groups that could benefit them or that they have some personal connection to. In one of the most notable recent studies on this topic, Butler and Broockman (2011) emailed roughly 5,000 state legislators across the country posing as a constituent who wanted help registering to vote. They signed half the emails "Jake Mueller," a name they had previously determined was almost always the name of a white person, and signed the other half "DeShawn Jackson," a name that was almost always that of a black person (based on data from the U.S. Census). DeShawn received significantly fewer responses (the gap observed between the two treatment conditions was 5.1 percentage points, p=0.04). Even when legislators had strategic incentives to help the constituent—when the citizen added that he was a member of the legislator's party—lawmakers often discriminated on the basis of race, suggesting the bias was a matter of more than just electoral strategy. Other audit studies have reached similar conclusions: officeholders have been found to be less responsive to requests from Hispanics, Muslims, and the elderly.

Do politicians similarly prioritize the rich—or the poor—when answering casework requests? There are reasons to expect that they might, as the last section noted. To date, however, experimental research on this point has been rare. We know of just one study on this topic: Butler (2014, Ch. 5) finds that mayors in the United States are more likely to answer an email from an out-of-town citizen who claimed to be a homeowner and who asked about the town's advanced placement programs than an out-of-town citizen who claimed to be a renter and who asked whether the town's high schools offered a free lunch program. In Butler's study, however, the constituent's economic circumstances were conveyed somewhat indirectly (through homeownership and the nature of the request) and the more and less affluent constituents were requesting slightly different services (information about different programs). Our experiments cue class more directly and allow us to compare how politicians respond to the same request when it comes from more and less affluent constituents.

Research Design

To determine whether state and local officials exhibit class bias in their responses to casework requests, we conducted four audit experiments. Following the design of previous casework studies, Experiments 1 and 2 involved a confederate sending requests (for help registering to vote and for an in-person meeting) to all members of the North Carolina state legislature. Experiment 3 involved a confederate emailing public school principals with a request for information about art and music programs at the school. Experiment 4 involved emailing a nationwide sample of mayors in the United States with a question about who to call in the city before digging for a landscaping project.[3] In all of our experiments, we randomized whether the confederates portrayed themselves as more or less affluent.

Experiments 1 and 2: State Legislators

In Experiments 1 and 2, our sample consisted of all state legislators in North Carolina in 2012 and 2013. In both, we partnered with a confederate who was an actual North Carolina voter and whose job duties entailed both washing dishes in a restaurant and working as an HR professional. In each experiment, the confederate emailed every member of the North Carolina state legislature with a

[3] So as to not signal homeownership, all emails simply stated that the constituent was about to do some landscaping work.

simple request, and began each email by mentioning his name and a randomly-assigned occupation: "My name is Joey, and I'm [a dishwasher / an HR professional]."

We expected these two occupations to convey very different information to the recipient. In general, occupations provide a strong signal of a person's position in a society's economic or social class structure—as Donald Matthews (1954, 23) put it, what we do for a living is "[p]robably the most important single criterion for social ranking in the United States." These two occupations, moreover, should evoke very different expectations about the sender's income, social status, and so on (Hout 2008). According to the BLS, a dishwasher's average salary in 2010 was $18,930, putting the occupation around the 20th percentile of the individual income distribution; whereas an HR manager's average salary was $52,690 per year, placing them around the 60th percentile. (We draw an even greater distinction between the two occupational conditions in Experiment 4 below.) When legislators saw "dishwasher" vs. "HR manager," there are strong reasons for them to perceive clear differences in the affluence of the sender.

In Experiment 1, we worked with our confederate to email every member of the North Carolina General Assembly to ask for *information about registering to vote*. (Requests like these are extremely routine, and North Carolina legislators do not share or "pool" staff, so we did not have any reason to expect that staffers or legislators would discuss the requests with one another, nor did we ever encounter any evidence that email recipients were aware that their behavior was being studied.) The body of the email was modeled after previous audit studies (see Box A1 in the Supplemental Materials). 84 legislators received an email from the dishwasher, and 86 received an email from the HR professional. We assigned legislators to treatment groups using block randomization by party and chamber. Our randomization yielded nearly perfect balance on other observable characteristics, like the legislator's race and gender and legislative background.

Six months later (in early 2013), we ran Experiment 2, again working with our confederate. We expected that this would be long enough that there was little chance that legislators or staffers would remember the first email and, indeed, none who responded to the email in the second experiment mentioned any previous correspondence. We also used a different email address, in case some legislators maintained databases or email archives. Here we emailed state legislators to ask for something slightly costlier: *time to meet*. With our help, our confederate emailed every state legislator to ask to meet or speak on the phone about a recent voter ID bill. The subject line read, "meeting to discuss voter registration" (for the text of this email see Box A2 in the Supplemental Materials).

With both experiments, we recorded politicians' responses for two weeks, including the length of the response, whether it was helpful (meaning it clearly explained how to register to vote, or offered to set up a meeting), and whether the email was signed by a staffer or the legislator. If state lawmakers prioritized requests from affluent citizens, Experiments 1 and 2 should have detected it.

Experiment 3: Public School Principals

Because these experiments had relatively small samples, in Experiment 3, we focused on a larger sample of public officials: public school principals in North Carolina and Kentucky. School principals are what Lipsky refers to as street-level bureaucrats, local government officials who receive numerous casework requests and make many consequential decisions. Unlike state legislators, principals are not subject to direct elections, which significantly diminishes any electoral incentives that might compel them to respond more to the rich or the poor. In that sense, principals are useful because they provide a window into the non-electoral (e.g., social or personal) incentives public officials might have to prioritize the rich or the poor.

We first randomly selected 719 principals, then randomly assigned half to receive a *less affluent* treatment email and half to receive a *more affluent* email. The subject of both emails was

"Music and Art Programs," and like the first two experiments, we recruited confederates who were state residents and who were interested in learning more about the subject at hand.

In addition to increasing our sample size and statistical power, another goal of Experiment 3 was to make the social class signals even more pronounced. Rather than just signaling class by mentioning a job, in the *less affluent* treatment, the email included a paragraph that described how the confederate had struggled financially, had been on food stamps, and had a son who was on free/reduced price lunches at school (see Box A3 in the Supplemental Materials)—clear markers of a less affluent person. In the *more affluent* treatment, we simply omitted this paragraph (as it is hard to imagine analogous language that would convey affluence and that would not sound artificial—e.g., "my child is *not* on free/reduce price lunches.") As in Experiments 1 and 2, we recorded how the officials in question responded for two weeks.

Experiment 4: Mayors

In the final experiment, we conducted a similar audit of a random sample of just over 3,400 mayors in the United States. This sample for this experiment was drawn from the American Municipal Officials Survey (AMOS)—which consists of the largest database of elected municipal officials (Butler et al. 2017). We focus on mayors given their direct role in many constituent services.

As in Experiment 3, our goal in this experiment was to increase the statistical power of our test and, more importantly, to increase the treatment intensity, both by increasing the apparent economic differences between our more and less affluent treatment categories, and by increasing the fraction of the sample exposed to treatment (a commonly used experimental technique to try and detect small effect sizes).[4] One potential criticism of experiments 1 and 2 is that the differences

[4] We assigned just under 60% of the sample to the more affluent treatment condition given theoretical predictions of bias towards this group. To see if we could detect any small biases in favor of the more affluent constituent.

between a dishwasher and an H.R. professional (i.e. a comparison of someone at the 20th and the 60th percentiles) might not be large enough for elected officials to notice, or that elected officials might think both groups were in the middle- to lower-classes, not the *upper* class. To increase the salience of class via our occupational treatments, we selected three less affluent professions and four even more affluent professions: mayors were randomly assigned to receive an email from a grocery store clerk ($22,130 per year), an auto mechanic ($29,700), or a farm worker ($25,070)—occupations in the bottom decile of the income distribution—or from a pharmacist ($121,710), a dentist ($180,010), a software developer ($111,780), or a personal finance advisor ($124,140)—occupations in the top 5% of the income distribution. These occupations provide a much larger degree of separation than the occupations used in Experiments 1 and 2.[5]

In Experiment 4, we sent emails posing as hypothetical constituents requesting information about who to call before digging for a landscaping project. This common request is within the purview of local officials, like mayors, and is a constituent request that takes little time to answer. It also is a request that one from various class backgrounds might feasibly ask. For the text of the email for experiment 4, see Box A4 below.

Results

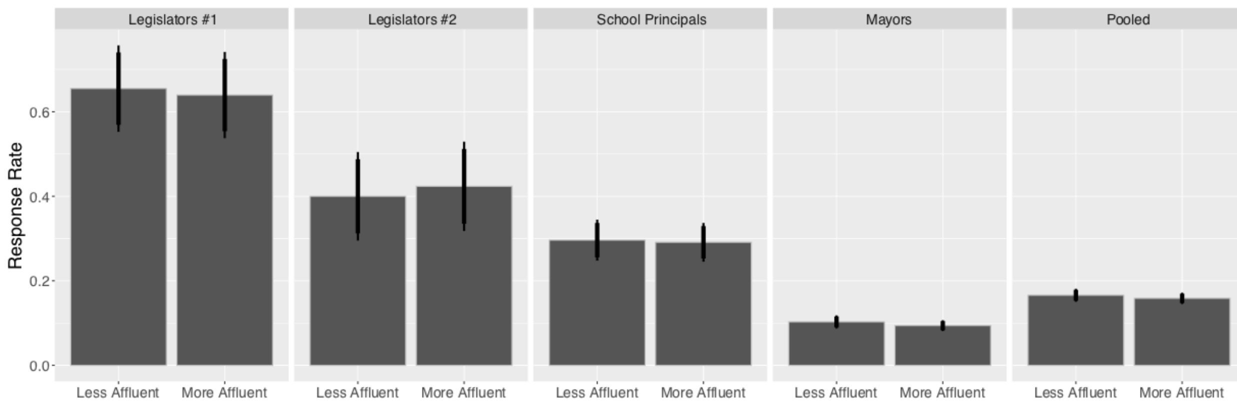
Across our three experiments, our confederates received responses from 16.2% of public officials (64.7% in Experiment 1, 41.2% in 2, 29.4% in 3, and 9.8% in 4).[6] Figure 1 plots the response rate broken down by our class treatments, with the pooled estimates shown on the far right.

[5] In theory, one could place even more separation between treatment conditions by using an even more affluent occupation like a Chief Executive of a large corporation. However, we deemed that this treatment might raise suspicions.

[6] Experiment 1's overall response rate was only slightly higher than (and not statistically distinct from) the response rate in Butler and Broockman's (2011) national study (57%). The decline in response rates in lower levels of government is consistent with our expectations, especially given that local officials are less likely to have staffs that help respond to emails.

In all four experiments, the officials we studied were equally likely to respond to our confederates' emails regardless of how they described their social classes. In Experiment 1, 64.0% of North Carolina legislators (or their staffs) responded to a request for voter registration information from a white-collar professional, and 65.5% responded to a request from a working-class citizen (Figure 1, first panel). This difference was substantively small and not statistically significant (p = 0.84). In Experiment 2, the response rates were 40.0% for a white-collar professional requesting a meeting and 42.4% for a blue-collar worker (Figure 1, second panel). Again, this difference was small, and not close to being statistically significant (p = 0.76). In our more high-powered Experiment 3, the patterns were the same (Figure 1, third panel). In the less affluent treatment, the principal responded 29.1% of the time; in the more affluent treatment, the principal responded to 29.6% of emails. Again, this difference was substantively small and statistically insignificant (p = 0.88). In Experiment 4, where the statistical power was even higher and the treatment administered to a higher proportion of the sample with a greater treatment intensity, the response rates were also statistically and substantively equal. Those in the more affluent condition received responses from 9.4% of the sample; those in the less affluent sample received responses from 10.3% of the sample. This difference was substantively small and was not statistically significant (p=0.39).

Figure 1: Response Rates by Treatment Condition



Notes: Each panel reports the results of one experiment. Bars plot average response rates with corresponding 90% (wide) and 95% (narrow) confidence intervals. *N*=170 (first), *N*=170 (second), *N*=719 (third), *N*=3433 (fourth), *N*=4492 (fifth; pooled).

The same was true when we pooled the results from our experiments (Figure 1, far right panel).[7] Altogether, the emails in our less affluent treatment received responses about 16.6% of the time, those in our more affluent treatment received responses about 15.8% of the time; this substantive small difference was not statistically significant (p=0.49), and the 95% confidence intervals were precise enough to allow us to rule out anything larger than a 1.32-percentage point bias in favor of one class or another.[8] Our null effects are not for a lack of statistical power. To benchmark, our *upper bound effect* is only 54% of the *average* difference Butler and Broockman (2011) observe across different racial treatments sent to state legislators (5.1 percentage points).[9] Simply

[7] Our pooled estimates include a study fixed effect. This does little to change the results.

[8] These null findings were also evident for numerous subgroups of cases. In Experiments 1 and 2, the results were the same for Republicans and Democrats, women and men, upper and lower chambers, and legislators from more and less affluent backgrounds. Sadly, we do not have as much comparable background information about school principals or mayors.

[9] Maybe, though, *how* public officials responded depended on the class of the person making the request. Answering this question is difficult due to the potential for post-treatment bias. To test this possibility without doing so, we examined the length of each reply (Kalla, Rosenbluth, and Teele 2017). Results based on these measures (available on request) were once again null. In all of our experiments, we failed to find evidence of any social class bias in responses to simple casework requests.

put, we find no evidence of class-based discrimination in our constituent request experiments regardless of sample, request, or treatment intensity.

Discussion

In this paper, we presented the results from four different audit studies seeking to ascertain the degree of class-bias in constituency requests. The results of these experiments represent a significant departure from past research. In contrast to the literature on racial, religious, and age-based biases in constituent services, we find no evidence of a social class bias in how officeholders respond to the less affluent, regardless of whether the officials in question are elected state legislators and mayors or appointed public school principals. In contrast to research on unequal responsiveness in the policymaking process, we find no evidence that the affluent get preferential treatment in the basic casework process. And in contrast to the emerging idea that politicians give policy to the rich and services to the poor, we find no evidence that the less fortunate get preferential treatment, either. In our experiments, casework simply doesn't seem to have much to do with social class.

Of course, these experiments have limitations. Our work includes just a few kinds of public officials and cannot rule out discriminatory differences that are very small. That said, our work makes a important contribution—to date, there have been few experiments on class and casework—but a great deal more could still be done.

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Supplemental Materials

Box A1: Experiment 1 Email Text

Dear [Representative/Senator] [legislator's name],

My name is Joey, and I am [**a dishwasher / an HR professional**]. I'm trying to figure out how to register to vote for the upcoming election. I heard that the voter registration deadline is soon.

Who should I call in order to register? Also, is there anything special I need to do when I register so that I can vote in future elections?

Thanks,

Joey

Box A2: Experiment 2 Email Text

Dear [Representative/Senator] [legislator's name],

My name is Joey, and I am [**a dishwasher / an HR professional**]. I've been following the recent debates about voter registration and House Bill 351, and I wanted to share my opinion with you.

Do you have any time in the next couple of weeks to meet or speak on the phone briefly?

Thanks,
Joey

Box A3: Experiment 3 Email Text

Dear [Mr./Ms.] [Principal's Last Name],

My name is Jessica. I'm emailing you on behalf of my son Joseph. My family is thinking about moving soon and would like to know more about your school. Specifically, I would like to know what music and art programs your school offers.

[**To share a bit about our family, we have struggled financially for the past few years. We've been on food stamps and Joseph has had to receive free/reduced lunches at school**.]

I'm emailing a few other schools in your area to see what they have to offer as well. I'd really appreciate hearing from you.

Thanks again,

Jessica

Box A4: Experiment 4 Email Text

> To whom it may concern,
>
> My name is Joey, and I am a [[**insert rich or poor profession**]] here in town. I'm emailing you because I'm planning to do some landscaping work, but I wanted to know who I should call before I dig.
>
> Is there a town office that I should call? I'd really appreciate hearing from you.
>
> Thanks, Joey

To check that imbalances in confounding variables weren't driving these results, we also estimated logistic regression models that controlled for several additional factors. When we randomized our sample, the resulting treatment groups were well-balanced on a wide range of observable variables. Still, as a simple robustness check, we estimated the models reported in Table A1 in the Supplementary Information, which controlled for the legislator's previous occupation, party, and chamber; whether the legislator was on the state elections committee; the legislator's race and gender; how long the legislator had been in office; whether the legislator was a party or committee leader; and the number of staffers the legislator employed.

None of these controls changed our findings. The response rates for lawmakers in the blue-collar treatment and the white-collar treatment were nearly identical, and the modest differences between them were nowhere near statistically significant (the odds ratios were statistically indistinguishable from 1). Only one control variable in each model predicted significant differences in response rates, about what we would expect by chance alone. With or without controls, lawmakers appeared equally likely to respond to a request from a constituent regardless of the constituent's class.

Table A1: Logit Models Relating Whether Legislators Responded (Experiments 1 and 2) Treatment Variable, and Additional Controls

	Request for Information	Request for a Meeting
Blue-collar constituent treatment	**0.878**	**1.116**
	(0.293)	**(0.365)**
Private-sector professional legislator	0.901	0.726
	(0.320)	(0.254)
Republican legislator	0.695	0.630
	(0.295)	(0.251)
Lower chamber legislator	1.258	1.384
	(0.479)	(0.522)
Elections committee legislator	0.755	0.604
	(0.365)	(0.297)
White legislator	3.294*	2.141
	(1.717)	(1.133)
Male legislator	0.974	0.815
	(0.412)	(.337)
Legislator's prior terms (#)	1.015	0.902*
	(0.416)	(0.045)
Committee leader	1.038	0.804
	(0.490)	(0.375)
Staff size (#)	1.009	1.211
	(0.129)	(0.184)
Intercept	0.786	0.671
	(0.536)	(0.464)
n	170	170
Pseudo R^2	0.029	0.044

Notes: Cells report odds ratios (with standard errors in parentheses) from logit models relating whether each legislator responded to the variables listed above. Except for "Prior Terms" and "Staff Size," all variables are indicators.
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, two-tailed.