

Does Reducing Street-Level Bureaucrats' Workload Enhance Equity in Program Access? Evidence from Burdensome College Financial Aid Programs

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Abstract

Persistent disparities in program access jeopardize social equity and erode a key pillar of democratic governance. Scholars have uncovered the causes of these disparities, including administrative burden and front-line discrimination, but less attention has been devoted to identifying tools for reducing disparities. We build on this work by arguing that reducing street-level bureaucrats' workload may be a key lever for reducing disparities. We also argue that workload reductions will be especially effective at advancing equity when administrative burden is expanded and complexity in client cases could otherwise create room for racial discrimination. We leverage data on all high schools in Oklahoma from 2005 to 2014 ($n = 4,155$) to estimate the causal effects of a state policy that mandates a counselor-student ratio in a regression discontinuity design. In line with our hypotheses, we find that decreasing workload corresponds to an increase in access for intersectionally minoritized students—low-income Black, Native American, and Hispanic students. Moreover, we find that effects were concentrated in the years after administrative burden was expanded. Together, our findings suggest that reducing workload can alleviate longstanding disparities in program access.

For decades, public administration scholars have attempted to live up to the task of a design science to minimize illegitimate discrimination against marginalized communities and expand equity in access to public services (Lipsky 2010; Simon 1946). As part of this exploration, scholars have examined how street-level bureaucrats—as empowered citizen-agents on the front-lines of policy implementation—wield discretionary power in ways that either promote or hinder program access for clients seeking government assistance (Jilke and Tummers 2018; Keiser 1999; Tummers and Bekkers 2014; Watkins-Hayes 2011). While in some cases street-level bureaucrats “move toward” clients and serve as advocates (Bell and Smith 2021; Jilke and Tummers 2018; Tummers et al. 2015; Watkins-Hayes 2009), there is considerable evidence across policy domains that marginalized clients are not treated the same by street-level bureaucrats, resulting in disparities in access to public services (Bell and Smith 2021; Einstein and Glick 2017; Grohs, Adam, and Knill 2016; Jilke, van Dooren, and Rys 2018; Keiser, Mueser, and Choi 2004; Olsen, Kyhse-Andersen, and Moynihan 2020; Olson 2019; Paluck and Green 2009; Pedersen, Stritch, and Thuesen 2018; Schram et al. 2009; Soss, Fording, and Schram 2011; Tummers et al. 2015; Watkins-Hayes 2011; White, Nathan, and Faller 2015). This significant evidence of front-line discrimination beckons an important challenge for public administration scholars—identifying management strategies that can effectively mitigate discrimination (Blessett et al. 2019; Wright and Merritt 2020).

A key organizational lever that may shape the likelihood of discrimination is administrative capacity and

workload—defined as the relationship between tasks and resources (Andersen and Guul 2019; Assouline et al. 2021). Existing studies find some evidence that reducing workload mitigates the tendency of front-line workers to fall victim to implicit prejudice (Andersen and Guul 2019; Guul, Pedersen, and Petersen 2021; Guul, Villadsen, and Wulff 2019). However, extant literature has yet to consider how the policy environment, and specifically the level of administrative burden on clientele applying for public programs, may moderate the effects of reducing street-level bureaucrats' workload on program access. We build on these studies by arguing that reductions in street-level bureaucrats' workload will be especially impactful for clients facing multiple systems of marginalization when there is an increase in the level of administrative burden in the application process (Blessett et al. 2019; Cahalan et al. 2019; Crenshaw 2017; Hooks 1981; Pandey et al. 2022).

We first investigate whether decreasing school counselors' workload impacts low-income student access to two means-tested college financial aid programs, with varying levels of administrative burden. We examine financial aid access because it is a key steppingstone to college for low income and racially minoritized youth and the process involves significant administrative burdens (Dynarski and Scott-Clayton 2006, 2013; Rosinger, Meyer, and Wang 2021). In our first research question we investigate: *How does reducing school counselors' workload impact student access to burdensome means-tested college financial aid?* In our second question, we take this analysis a step further by examining not only how

counselor workload impacts outcomes for all low-income students, but specifically how counselor workload impacts outcomes for racially minoritized¹ low-income students (Cahalan et al. 2019; Crenshaw 2017; Hooks 1981). In this way, we utilize an intersectional equity lens which recognizes the overlapping and complex convergence of multiple systems of marginalization and oppression, which many prior studies on administrative burden and street-level bureaucracy have yet to effectively incorporate (Cahalan et al. 2019; Crenshaw 2017; Hooks 1981; Museus and Griffin 2011). Finally, we develop and test a novel theoretical hypothesis regarding the influence of administrative burden as a moderator in the relationship between changes in workload and equity in program access.

To test our theoretical propositions, we leverage a regression discontinuity design and an event-study analysis to investigate the impact of schools having an additional counselor by virtue of schoolwide enrollment being just above an arbitrary threshold in the state of Oklahoma (Angrist and Pischke 2009). We draw from rich administrative data from the Oklahoma State Regents for Higher Education, the Oklahoma State Board of Education, and the National Center for Education Statistics to construct our dataset on staffing levels, school characteristics, and student outcomes.

First, we find that reducing counselor workload has a positive and statistically significant effect on the level of program access to college financial aid for low-income students.² Our local average treatment effect estimates suggest that schools could nearly double financial aid receipt if they fully complied with state policy and hired an additional full-time counselor. Instead, many schools hired a part-time counselor, to remain compliant while balancing budgeting and personnel constraints. Second, we find that the effects were concentrated among low-income Black, Hispanic, and Native American students in more recent years where the program was changed to increase burdens on students.

Our findings make three important contributions to public administration research and theory. First, we employ an intersectional approach to our measurement of program access. Prior scholarship has focused on a single dimension of structural marginalization (i.e., racism, sexism, classism, ableism), whereas we are able to focus on whether decreases in street-level bureaucrats' workload reduces disparities for some marginalized groups while perpetuating others.³ Second, we analyze the causal impacts of a real policy change reducing the workload of street-level bureaucrats on program access for marginalized clientele. In this way, we measure downstream outcomes, rather than stated preferences or short-term behavior, which provides important insight on whether stated preferences of street-level bureaucrats translate to downstream client outcomes. Finally, we test a novel theoretical proposition on the potential for differential impacts

of reducing counselor workload depending on the level of administrative burden. Specifically, we provide evidence on whether reducing the workload of counselors produces particularly pronounced expansions in access for racially minoritized low-income students in years where there were expansions in the level of administrative burden, compared to previous years with less burden.

Administrative Burden, Counselor Capacity, and Access to College Financial Aid

State-imposed burdens that inflict learning, compliance, or psychological costs on clientele⁴ reduce equity in access to government programs (Bhargava and Manoli 2015; Deshpande and Li 2019; Gray 2019; Heinrich 2016, 2018; Herd and Moynihan 2018; Homonoff and Somerville 2020; Nisar 2017; Ray, Herd, and Moynihan 2022; Bell et al. forthcoming). In public administration literature, considerable effort has been dedicated to examining administrative burdens in health (Deshpande and Li 2019; Herd and Moynihan 2020), welfare (Barnes 2020; Keiser 1999; Schram et al. 2009; Soss, Fording, and Schram 2011), and immigration (Heinrich 2018; Herd and Moynihan 2018), with less attention on higher education (Bell and Smith 2021). Nevertheless, a striking example of a notoriously burdensome application is the Free Application for Federal Student Aid (FAFSA). In fact, 35–50 percent of high school students fail to complete the FAFSA prior to high school graduation due to administrative burdens (Bird et al. 2021; Kofoed 2017). Experiences of burden in the FAFSA leave lower to middle income students losing an estimated \$9,700 in grant and loan aid annually (Bird et al. 2021), which carries an aggregated 4 billion dollar price tag each year (Dynarski and Scott-Clayton 2006). Key street-level bureaucrats—such as school guidance counselors—may play an important role in helping students navigate this complexity, mitigating the negative impact of burdens on program access for students (Barnes 2020; Bell et al. 2020; Maynard-Moody and Musheno 2003; Mulhern 2019; Wiley and Berry 2018). School counselors play a crucial role in addressing the non-instructional needs of students and are often a resource for college intending students to learn about financial aid and college options (Carey and Harrington 2010a, 2010b; Lapan, Gysbers, Bragg, and Pierce 2012; Lapan, Whitcomb, and Aleman 2012; Pham and Jeenan 2011). Indeed, we argue that counselors may be able to leverage their discretionary power to promote program access to burdensome financial aid programs, if they have the capacity to do so (Andersen and Guul 2019; Bell and Smith 2021).

However, the capacity of counselors is often severely limited (Bell and Smith 2021).⁵ Building on prior literature on administrative burdens, we argue that a key solution to administrative burden could be the staffing levels and capacity of street-level bureaucrats (in this case, high school counselors). By increasing counselor capacity and reducing caseloads,

¹We use the term racially minoritized rather than “students of color” or “minorities” to describe the active process of minoritization that a socially constructed noun cannot capture (Benitez 2010).

²Importantly, this should be interpreted as the “intent-to-treat” estimate because we estimate the impacts of counselor capacity on all high school students rather than only among those who received the additional assistance provided by counselors.

³Though we are limited by our school level analysis, our findings do still reveal the effects of reducing counselor workload on students who are both low-income and racially minoritized. However, we do encourage future work to employ individual level data to provide even more nuanced analyses on intersectionality in program access.

⁴Learning costs involve information barriers such as understanding complex eligibility requirements; compliance costs are the hoops students have to jump through like filling out complex paperwork; and psychological costs are the stigma, loss of autonomy, and stress in the process (Herd and Moynihan 2018).

⁵While the American School Counselor Association (ASCA) recommends one counselor for every 250 students, the national average for the student/counselor ratio is about 490:1, and the 20% of public schools that do not employ a single school counselor are disproportionately likely to serve a large population of black students and low-income students (ASCA 2022).

organizations can enable street-level bureaucrats to develop better relationships with clients (Krueger and Whitmore 2001) and engage in more resource-intensive practices that may increase FAFSA completion and access to other college financial aid (Bettinger et al. 2012; Hurwitz and Howell 2014). Specifically, by expanding administrative capacity, counselors may be more able to take a hands-on approach to supporting students—what prior studies have called a “Student Advocate”—in which counselors “move toward” clients by taking on the burden of the application process (Bell and Smith 2021; Bell et al. 2020; Tummers et al. 2015; Watkins-Hayes 2009). Finally, additional counselors could allow for organizational changes including the specialization of duties (e.g., one counselor manage all college applications, while another manages behavioral management) (Bastian et al. 2019), and create peer effects—by which counselors learn from colleagues (Jackson and Bruegmann 2009) or increase productivity when being watched by a peer (Monsalve et al. 2014). New counselors could also induce shifts in the organizational culture and demographic representation of the student population, which could manifest into better outcomes for marginalized students (Keiser et al. 2002; Meier 2019; Portillo, Bearfield, and Humphrey 2020). Therefore, we predict that an increase in counselor capacity will reduce administrative burdens for low-income students and increase access to burdensome college financial aid.

HYPOTHESIS 1: Increasing the number of school counselors will positively impact the proportion of low-income students receiving burdensome means-tested college financial aid.

Street-Level Bureaucrats’ Workload and Racial Discrimination

A key principle guiding public administration is the notion of universalistic treatment, which can eliminate prejudicial behavior that create inequities in program access and success (Lipsky 1984, 2010). However, street-level bureaucrats often engage in practices (whether consciously or unconsciously) that perpetuate existing racial equity gaps, which runs counter to the goal of many public programs to undo the inequities caused by market forces. In the context of K-12 schools in the United States, counselors often manage substantial student caseloads that make it challenging to address students’ needs and result in well-documented discrimination against racially minoritized students (Chetty et al. 2011; Hutchens, Block, and Young 2013; Krueger and Whitmore 2001; Linnehan, Weer, and Stonely 2011; Maynard-Moody and Musheno 2003; Thornhill 2019). While this type of racial discrimination is well-documented, the theories explaining the underlying causes of discrimination vary widely and the solutions to discrimination are relatively understudied.

While we cannot speak to the specific individual level mechanism of discrimination in this paper, we do consider it important to review the three main theoretical frameworks on racial discrimination in citizen-state interactions. First, some scholars, mostly in economics, have posited that discrimination is “taste-based,” arguing that bureaucrats harbor animosity toward outgroups and sympathy toward ingroups, which results in overt racism in decision-making (Becker

1971). This animosity is thought to be driven by the outgroup reflecting perceived threats to either material conditions or identity, which creates discriminatory intentions toward groups like immigrants or racially minoritized communities (Pereira, Vala, and Costa-Lopes 2010). On the other hand, in the theory of statistical discrimination, rational actors are acting under the constraints of imperfect information and are seeking to maximize economic utility (Phelps 1972). Therefore, when the cost of obtaining information to inform prioritization decisions is high and there are average differences in key outcomes across groups, street-level bureaucrats discriminate against clients to maximize utility (Jilke, van Dooren, and Rys 2018). Finally, rather than assuming “public officials were simply biased or racist”—echoing the tenants of taste-based discrimination—others argue that discrimination is more likely implicit, either activated by cognitive or affective forces (Assouline et al. 2021). In the context of bureaucratic organizations, scholars argue that these theories align well with the professionalization and meritocratic culture in which many bureaucrats strive for neutrality but end up unconsciously engaging in biased decision-making (Andersen and Guul 2019; Assouline, Gilad, and Ben-Nun Bloom 2021; Portillo, Bearfield, and Humphrey 2020).

Another causal mechanism of discrimination behavior is at the organizational, rather than the individual level. Indeed, regardless of the theoretical mechanism at play at the individual level, many scholars agree that stress in the organizational environment can undermine the ability of street-level bureaucrats to process information. In this way, these organizational stressors lead street-level bureaucrats to rely on stereotypes that alleviate the need for complex information processing when categorizing new cases and stimuli (Assouline, Gilad, and Ben-Nun Bloom 2021; Fiske 1998). Multiple organizational conditions contribute to implicit prejudice among street-level bureaucrats: 1) ambiguity⁶, 2) cognitive load, and 3) organizational performance (Burgess et al. 2014; Dovidio, Kawakami, and Gaertner 2000; Guul, Pedersen, and Petersen 2021; Guul, Villadsen, and Wulff 2018, 2019). Empirically, Dovidio et al. (2000) found support for the first mechanism in a study of undergraduates, where equal scores were given to White and African-American candidates for an education program when there was low ambiguity, but when there was increased ambiguity, respondents rated Whites higher than African-Americans. In the context of healthcare determinations in Israel, Jewish doctors were more likely to reject applications by Muslims when the application was for partial compensation for disability but not when the application was for full compensation, suggesting that ambiguity in client cases exacerbated bias against Muslim applications (Assouline, Gilad, and Ben-Nun Bloom 2021). Therefore, in cases with higher ambiguity, discrimination may arise more often than in cases with lower ambiguity where discrimination in decision-making is less defensible (Crandall and Eshleman 2003; Dovidio, Kawakami, and Gaertner 2000). For the second mechanism—cognitive load—scholars have found that when cognitive load is manipulated in lab or

⁶While some scholars have examined ambiguity from the perspective of anonymity, we conceptualize ambiguity as the uncertainty that accompanies cases that are less clear cut. In our context, interactions with counselors are not anonymous, and therefore while there may be uncertainty regarding whether certain documents or student conduct align with the extensive eligibility requirements, there is not likely to be anonymity in student–counselor interactions.

survey experiments, there is mixed evidence on the resulting levels of discrimination (Stepanikova 2012). However, recent evidence from field experiments does suggest that cognitive load and workload is related to discrimination among street-level bureaucrats across multiple policy contexts (Andersen and Guul 2019; Jilke, van Dooren, and Rys 2018; Pfaff et al. 2021). In a US-based audit study, public school principals were less likely to respond to parents who signaled intense religious beliefs, partially due to the perceived workload, resource burdens, and reputational risks of catering to religious priorities in traditionally secular school settings (Pfaff et al. 2021). In Flanders, scholars found that for-profit senior homes evade information requests from North African individuals because of the perceived increased cost in catering to individuals with specific language and religious needs (Jilke, van Dooren, and Rys 2018). Finally, discrimination may be especially likely in lower-performing organizations, as shown by Guul, Villadsen, and Wulff (2019) in the context of Danish public schools. Together, this literature makes it clear that organizational conditions influencing workload and the ambiguity/complexity of client cases could shape whether street-level bureaucrats engage in racial discrimination.

While existing studies have made significant strides in understanding the *theoretical mechanisms causing discrimination*, less attention has been paid to assessing the viability of policy solutions that impact the organizational environment of street-level bureaucrats, and under what conditions we can expect changes in workload to impact equity in program access. We build on recent scholarship from Denmark, where scholars combined evidence from a survey experiment and field experiment that explicitly manipulated teacher workload (Andersen and Guul 2019). The findings demonstrated that teachers were less likely to accept additional students with Middle Eastern aliases only when it impacted their workload or when they were not provided with additional resources, providing substantial support for the theory that reducing workload produces lower levels of discrimination (Andersen and Guul 2019). Therefore, we predict that increasing administrative capacity of counselors would reduce discrimination by decreasing workload, allowing more time to deal with complex or ambiguous cases, and reduce the need for prioritization of students (Andersen and Guul 2019; Auwarter and Arugete 2008; Francis, de Oliveira, and Dimmitt 2019; Welsch and Winden 2019). In turn, we predict that we will observe an increase in access for students that face intersectional disadvantages in schools that are above the policy threshold and are forced to hire additional counselors.

HYPOTHESIS 2: Increasing the number of school counselors will positively impact the number of low-income racially minoritized students receiving financial aid.

In this way, we overcome two important limitations in existing studies. First, we examine consequential downstream outcomes, rather than measuring stated bureaucratic preferences on a survey experiment (Andersen and Guul 2019; Guul, Pedersen, and Petersen 2021) or short term bureaucratic behavior like responses to an audit information request (Guul, Villadsen, and Wulff 2019; Olsen, Kyhse-Andersen, and Moynihan 2020). Second, by adopting an intersectional lens, we call attention to the multiple intersecting identities,

including race and socioeconomic status, that create power dynamics that result in marginalization for students in the context of college access and affordability (Crenshaw 2017; Hooks 1981; Woods, Benschop, and van den Brink 2022). For example, a recent study found that counselors were more likely to recommend college admission activity for low-income White students, but not for low-income Black students, revealing the importance of examining multiple axes of marginalization (Linnehan, Weer, and Stonely 2011). Therefore, by testing whether reductions in workload impact students facing intersectional disadvantage, we provide novel evidence on whether expansions of street-level bureaucrats' administrative capacity could reduce some disparities (for low-income students) while potentially perpetuating others (for low-income racially minoritized students) (Breslin et al. 2017; Butz and Gaynor, 2022). In doing so, we recognize the possibility that changes in workload may not be enough to produce changes in downstream outcomes for racially minoritized clients—it may be the case that extra capacity may still be unevenly distributed across clients, calling into question whether administrative capacity can impact downstream outcomes in the absence of other effective bias-reducing efforts.

Finally, we test a novel theoretical hypothesis that draws from the administrative burden and discrimination literature to provide insight on the conditions under which reductions in workload may be most impactful on downstream outcomes for marginalized clients. We posit that when administrative burden is expanded over time, complexity and ambiguity in client cases expands, which increases the likelihood of discrimination (either based on socioeconomic status or race/ethnicity) if there is no reduction in workload (Ray, Herd, and Moynihan 2022). Indeed, where eligibility requirements abound and paperwork is more complex, there may be more room for street-level bureaucrats to ration access for marginalized clientele, unless the organizational conditions are supportive, and workload is distributed across enough street-level bureaucrats. Therefore, as we illustrate in figure 1, we predict that when the level of administrative burden increases, the impacts of counselor workload reductions will be more pronounced on clients that would otherwise suffer from discrimination under conditions of enhanced complexity and ambiguity.

HYPOTHESIS 3: The impact of reducing counselors' workload on low-income students will be concentrated in the years following expansions in administrative burden.

Empirical Setting

We leverage a state-wide policy mandating a counselor-student ratio in Oklahoma to estimate the effects of workload changes on program access. In 1991, the Oklahoma State Board of Education mandated in the public-school Standards for Accreditation that high schools must maintain “at least one certified school counselor for every 450 students” (American School Counselor Association 2022). According to this policy, perfectly compliant schools would have one counselor in schools with 450 or fewer students, and then add a second counselor right at the enrollment threshold. We utilize this variation in workload above and below the

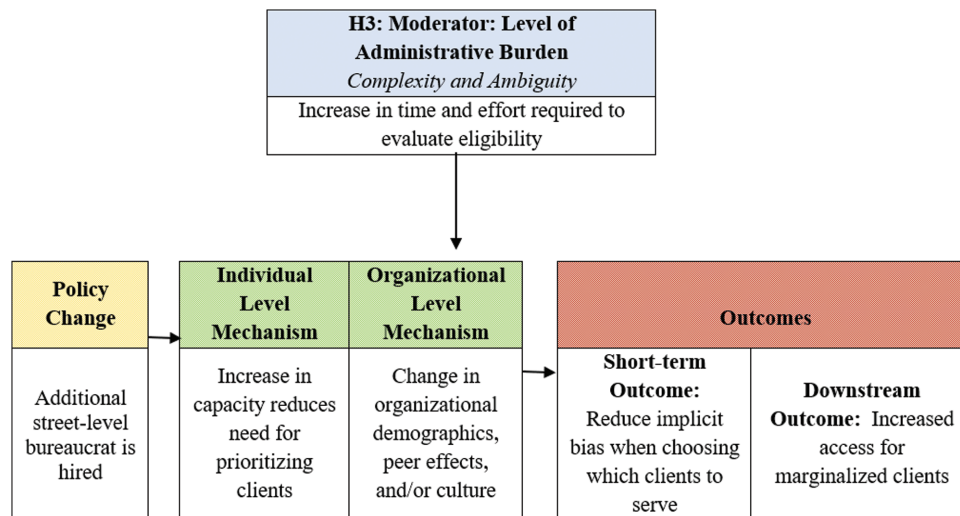


Figure 1. Logic Model for Key Theoretical Hypotheses.

arbitrary threshold to test the impacts on our key outcome of interest—access to college financial aid. We choose to study this policy in the context of Oklahoma because it is a relatively understudied state with low levels of per pupil education funding and pronounced inequalities in school capacity and access to college financial aid (Bell 2019).⁷

For our outcome measures, we investigate student access to the largest federal and state financial aid programs—the Federal Pell Grant and the Oklahoma’s Promise—because school guidance counselors serve a key role in helping students overcome the many hurdles in the application processes for these programs. To gain access to the Federal Pell Grant, students must complete the FAFSA, which is a significant barrier for many disadvantaged students (Kofoed 2017; Schudde and Scott-Clayton 2016; Scott-Clayton and Schudde 2016). Previous studies have documented the difficulty disadvantaged students face in completing the FAFSA (Bettinger et al. 2012; Bird et al. 2021; Page, Castleman, and Meyer 2020), which often results in students losing access to means-tested financial aid. Therefore, despite the large benefits provided by the Federal Pell Grant (up to \$5,775 per year as of 2016), many students may not be able to access this potentially transformational financial aid without the personalized assistance of counselors in the FAFSA application process (Denning, Marx, and Turner 2017; Park and Scott-Clayton 2018). While gaining access to the Pell Grant is burdensome by virtue of having to complete the FAFSA, the level of administrative burden in Oklahoma’s Promise is significantly higher.

Oklahoma’s Promise was created in 1991 with the goal of expanding college affordability for low-income Oklahomans as a state-wide hybrid merit-based and need-based tuition-free college program. Oklahoma’s Promise provides students with the full cost of tuition at any public colleges, and partial coverage of tuition at private colleges—it does not cover other expenses including fees, books, housing, and transportation.

⁷Moreover, in Oklahoma the counselor staffing policy translates a pronounced difference in the number of counselors being hired by schools on the ground, unlike some of the other states with similar counselor ratio policies. Finally, in Oklahoma, there has been a steady decline in counselor-student ratio; in the 2008–09 school year, there were 1.17 counselors per 450 students and by 2016–17, there was only one counselor per 450 students.

In the first stage of enrollment, students in 8th, 9th, or 10th grade must enroll in the program by submitting a five-page application form that requires income documentation from parents. The State agency, the Oklahoma State Regents for Higher Education, does not do targeted outreach and relies on school counselors to disseminate information to their 8th–10th graders about the program and provide support in the application process. As part of the five-page application form, students must pledge to refrain from substance abuse and criminal delinquent acts, attend school regularly, do their homework, complete a 17-unit core curriculum, and make a 2.5 GPA overall and in the core curriculum specifically. Once students are enrolled initially in Oklahoma’s Promise, school counselors are responsible for monitoring student course taking and GPA to ensure students can access the program once they graduate. When it comes time for high school graduation, counselors play another critical role in implementation—they certify whether students have met all the requirements, ensure students fill out the FAFSA for other Federal aid, and report back to the state agency. Unsurprisingly, because there are significant information asymmetries, compliance hurdles, and psychological stigma of income documentation, many income-eligible students fail to enroll and maintain eligibility for the Oklahoma’s Promise program. When compared to other state promise programs, Oklahoma is more burdensome—for example, other programs like the Tennessee Promise and Oregon Promise only require students to fill out the FAFSA upon high school graduation along with some community service requirements without the long list of academic, income, and conduct requirements (Rosinger, Meyer, and Wang 2021). In addition to compliance hurdles, Oklahoma’s Promise has a higher-than-average learning cost score, and unlike other programs, such as the Tennessee Promise, there has been no engagement of community nonprofit organizations to increase the availability of mentoring to students applying for Oklahoma’s Promise (Kramer 2022). Therefore, it is not surprising that only a third of income-eligible students gain access to Oklahoma’s Promise program (Bell 2019), while other states like Tennessee have much higher participation rates (Spire 2022).

While the program has always included a long list of requirements, there was a pronounced expansion in the level

of administrative burden after 2007, when the legislature added FAFSA as a requirement (effectively restricting access to students that could prove legal citizenship status) and increased the number of income verification requirements (see Appendix table A4 for details). Before these changes, students were not required to fill out the FAFSA and only submitted income documentation once while in 8th, 9th, or 10th grade (Bell et al. 2021, 2023). After these changes, students had to submit a second income verification upon high school graduation and fill out the FAFSA in time to maintain eligibility for Oklahoma's Promise. We leverage this variation over time to provide insight into whether the impacts of workload are particularly pronounced when burden is expanded. Together, the Federal Pell Grant and Oklahoma's Promise programs provide ideal venues for examining our research questions because they both involve significant administrative burden and rely heavily on the work of school counselors to promote student access.

Research Design

Simply examining the relationship between the number of counselors in a school and student outcomes would produce biased estimates—while schools with more counselors may have more students receiving the Oklahoma Promise, that ignores other factors (such as the fact that schools with more counselors have more students). Understanding the causal relationship between counselors and student outcome requires a plausibly exogenous source of variation in counselor staffing that would only affect student outcomes through changes in counselor staffing.

To estimate this causal effect of reducing workload on student outcomes, we leverage the discontinuity in counselor staffing around Oklahoma's mandated enrollment thresholds. To the best of our knowledge, this threshold does not coincide with any other state policy, reducing the possibility that changes in staffing and outcomes that we observe at the threshold could be driven by another policy.⁸ We employ a “fuzzy” regression discontinuity analysis, or two-stage least squares model, that accounts for non-perfect compliance with state policy. In Oklahoma, which mandates no more than 450 students per school counselor, perfect compliance would mean schools with enrollments less than or equal to 450 students would have one school counselor ($x_i < c_i$), while a school with between 451–899 students would have two counselors ($c_1 \leq x_i < c_2$). Since schools are not perfectly compliant, school enrollments instead affect the probability of increased staffing (D_i) based on a school's enrollment (x_i), as a function of $g(x)$, where functions g_1 and g_0 can vary at cutpoint c_1 :

$$\Pr(D_i = 1 | x_i) = \begin{cases} g_1(x_i) & \text{if } x_i \geq c_1 \\ g_0(x_i) & \text{if } x_i < c_1 \end{cases}$$

⁸The only other school staffing policy that is close to, but does not coincide with the threshold, is the school librarian standards for accreditation. Schools in Oklahoma are required to have a part-time librarian if the school enrollment is less than 300 students, a full-time librarian (or a part-time librarian and a full-time librarian assistant) if the school enrollment is between 300 and 499, and at least one full-time librarian and a half-time librarian assistant if school enrollment is between 500 and 999 (Kachel and Lance 2021). We do not believe that this policy threatens the validity of our regression discontinuity design because the thresholds do not align with the counselor staffing threshold and librarians are not in charge of information dissemination and compliance certification in Oklahoma's Promise program or the Federal Pell Grant program. These tasks fall under the purview of school counselors and therefore this should not introduce bias in our estimation of the effects on outcomes such as student financial aid.

The first stage estimation process examines whether a given school, just above the enrollment threshold, hired an additional school counselor in a certain year. We examine the first stage compliance with the counselor staffing policies through the form:

$$\text{staffing}_{it} = \pi_0 + \pi_1(\text{distance}_{it}) + \pi_2(\text{above}_{it}) + \pi_3(\text{above}_{it} * \text{distance}_{it}) + \delta_t + \mu_{it} \quad (1)$$

which regresses school counselor staffing (staffing_{it}) for school i in year t of our panel on the distance between a given school's total enrollment and the enrollment threshold (where distance_{it} is equal to $x_{it} - c_1$), an indicator of whether enrollment is above or below the threshold, and the interaction of the above indicator with enrollment distance to allow for g_1 and g_0 to vary on either side of the enrollment threshold. We include a year fixed effect, δ_t .

The reduced form model is similar to the first-stage, replacing a measure of staffing for school-level outcomes Y_{it} :

$$Y_{it} = \gamma_0 + \gamma_1(\text{distance}_{it}) + \gamma_2(\text{above}_{it}) + \gamma_3(\text{above}_{it} * \text{distance}_{it}) + \delta_t + v_{it} \quad (2)$$

The reduced form analysis provides an estimate of how schools that enroll more than 450-students differ at the threshold in terms of student outcomes without explicitly connecting that to the number of counselors in the school (or any other differences in schools at the threshold). To capture the causal effect of counselor staffing on student outcomes, we run a 2SLS instrumental variable model, where counselor staffing is instrumented by a school having enrollment above the threshold as estimated in the first stage equation (1). The instrumental variables estimates are generated by the form:

$$Y_{it} = \beta_0 + \beta_1(\text{staffing}_{it}) + \beta_2(\text{distance}_{it}) + \beta_3(\text{above}_{it} * \text{distance}_{it}) + \delta_t + \varepsilon_{it} \quad (3)$$

The coefficient of interest, β_1 represents the causal estimate of increasing school counselor staffing on the outcomes described above in a given year. In accordance with best practices, we include robust standard errors clustered on the running variable—school enrollment (Lee and Lemieux 2010; Schochet et al. 2010).

Data Description

To investigate whether counselor staffing levels affect access to financial aid programs, we leverage multiple sources of unique administrative data. First, we requested detailed school level data from the Oklahoma Department of Education that captures the counselor staffing levels, demographic characteristics, test scores, and other observable characteristics that could increase precision. Next, we requested student-level data from the Oklahoma State Regents for Higher Education on Oklahoma's Promise enrollment and other postsecondary outcomes, which we aggregated to the school level based on the school in which the student graduated high school. The data was available from the 2005–06 through 2014–15 academic years.

We limit our sample to schools around the 450-student enrollment threshold, dropping schools enrolling 900 or more students (the next multiple of 450/threshold for hiring an

additional counselor).⁹ For our main analysis, we use a ± 225 policy-driven bandwidth local to the policy threshold that includes schools enrolling more than 225 students and schools enrolling fewer than 675 students. This choice of bandwidth is driven by the details of the counselor staffing policy—schools enrolling fewer than 225 students are exempt from the state counselor staffing ratio requirement and may pro-rate staffing hours based on student need. Additionally, schools enrolling more than 675 students would theoretically be within the bandwidth for the 900-student enrollment threshold, which we do not include in our analysis due to sample size restrictions. We also test the sensitivity of the results to different bandwidth choices, including data-driven bandwidths, to increase confidence in our estimates. [Table 1](#) highlights our relevant sample size—we start with 4,155 observations from 385 high schools in our panel; restricting this to schools ± 225 from the threshold (the policy-driven bandwidth) reduces the sample to 159 schools (1,369 observations). The MSE Bandwidth reduces the sample size even more than the policy bandwidth, depending on the covariate of interest.¹⁰ We note the limitations this smaller sample size imposes to the precision of our estimates, especially given the high sample size requirements for precise estimates in regression discontinuity designs ([Deke and Dragoset 2012](#); [Schochet 2009](#)).¹¹

Validity and Sample

Regression discontinuity designs provide a causal estimate of the effects of a policy under the assumption of as-good-as random sorting to either side of the running variable. Specifically, this assumes “imprecise control” over treatment—while individuals or units might work to change their value of the running variable (in this case, the number of students enrolled at a high school), regression discontinuity designs assume such control is sufficiently imprecise as to determine where they fall relative to a treatment cutoff ([Lee and Card 2008](#)). In the Oklahoma counselor policy, schools faced different staffing requirements depending on whether they enrolled more or fewer than 450 students. This is a case where we might be concerned about imprecise control—the policy threshold was well known, and enrollment is a variable which might be precisely controlled (either through falsification or creative counting) by school leaders with a vested interest in either evading needing to hire an additional school counselor or (perhaps less plausibly) to gain access to the opportunity to hire an additional school counselor.¹²

⁹There are few high schools in Oklahoma with enrollment around the 900-student enrollment threshold; given the low occurrence of large high schools and the challenge interpreting different relative staffing increases at higher thresholds, we focus on the 450-student enrollment threshold for this analysis.

¹⁰For example, the effective sample for examining the effect of staffing the number of Pell recipients leveraging the MSE bandwidth is 1,028 observations representing 121 unique schools in the panel.

¹¹We estimate that we require a sample size of 1,421 with power = 80% in order to detect a half-standard deviation effect on the count of students receiving Pell at the 5% significance level (estimated using the `rdsamps` package in Stata). Therefore, the analysis on the full sample is sufficiently powered, the policy bandwidth is slightly underpowered to detect a half-standard deviation effect, and the MSE bandwidth is significantly underpowered.

¹²School leaders have historically manipulated student characteristics—changing individual student disability classifications in response to accountability requirements ([Figlio and Getzler 2006](#)) or engaging in unofficial selective admissions to their schools in order to recruit students with higher academic performance, despite having official open enrollment policies ([Jabbar 2015](#)).

Thus, manipulating the enrollment count is not implausible. Investigating whether school leaders likely exercised “imprecise control” over enrollment or may have manipulated enrollment is critical to the interpretation of our estimates. We follow the standard approach of evaluating the density of the running variable at the threshold ([McCrary 2008](#)) and of examining non-treatment characteristics at the threshold ([Lee and Card 2008](#)); if either test fails, it suggests the possibility of precise manipulation ([Wong and Wing 2017](#)).

Appendix figure A1 illustrates the results from a McCrary density test of enrollment counts around the threshold to detect bunching; we find no evidence of a significant or large enrollment discontinuity, indicating that gaming at the threshold is unlikely a concern for the validity of our design. In [table 1](#), we examine whether school-level covariates or other inputs differ on either side of the cutoff to investigate whether there appears to be either (a) strategic manipulation around the RD threshold or (b) concurrent changes in inputs that would affect whether independence/continuity assumptions hold. [Table 1](#) includes the average student and school characteristics for our overall sample, average characteristics among the set of schools above and below the 450 student enrollment threshold, and the discontinuity in characteristics across the threshold (using the full sample, a policy-driven bandwidth of half the threshold in either direction, and a data-driven bandwidth). Examining student population characteristics, we find some small but statistically significant differences in enrollment compositive above and below the threshold that persist across bandwidths—of note, schools just above the threshold have more students receiving free or reduced-price lunch (five percentage points) as well as a higher Hispanic population (two percentage points). In smaller bandwidths, we do observe large differences in the share of Native and Black students, though we caution at the data-driven bandwidths the sample size diminishes considerably (for example, the difference in Black enrollment with an MSE-optimal bandwidth is estimated based on just 546 observations representing 75 schools in our panel).¹³

Given the absence of density bunching at the threshold, interpreting differences in non-treatment variables at the threshold suggests that schools naturally or by virtue of a co-existing policy have different characteristics at this threshold, and not that any differences in characteristics are a factor of precise manipulation. Under that assumption of naturally occurring differences at the threshold (absent evidence of any other policies occurring at this enrollment threshold), the appropriate recourse is to include those covariates in the analytic model to account for compositional differences; we include covariates in all our models and share a non-covariate adjusted model in the Appendix for reference.

As a robustness check, we also present a “donut regression discontinuity” analysis in [Supplementary Appendix table S2](#)—this robustness check assumes that even if manipulation on either side of the policy threshold occurred without a change in the density of observations, that only units very close to the threshold would have undertaken said manipulation and that omitting those observations would omit concerns about precise manipulation affecting the observed treatment effects ([Barreca](#)

¹³We also illustrate in [Supplementary Appendix Figure S1](#) how discontinuities in student characteristic occur throughout the enrollment distribution; with the exception of the share of free- or reduced-price lunch students, the discontinuities we observe at the policy threshold are observed at several other points in the enrollment distribution, speaking to the generally “noisy” nature of student characteristics.

Table 1 Sample

	Average across sample	Below	Above	Discontinuity at Threshold					
				Full Sample/No Bandwidth		Policy Bandwidth: $lsil$ 225		MSE Bandwidth: Varies	
				Estimate	<i>p</i> -value	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value
<i>Student characteristics</i>									
% Free or reduced lunch	0.57	0.58	0.51	0.05 (0.016)	0.002	0.05 (0.022)	0.018	0.06 (0.030)	0.037
% Special education	0.16	0.16	0.14	0.02 (0.004)	0.000	0.01 (0.006)	0.027	0.00 (0.011)	0.739
% Asian	0.01	0.01	0.01	0.00 (0.001)	0.063	0.00 (0.002)	0.121	0.00 (0.003)	0.409
% Black	0.04	0.04	0.08	-0.01 (0.015)	0.499	0.00 (0.023)	0.925	-0.11 (0.048)	0.020
% Hispanic	0.07	0.07	0.08	0.02 (0.007)	0.000	0.02 (0.008)	0.014	0.02 (0.012)	0.048
% Native	0.24	0.24	0.24	-0.01 (0.013)	0.325	0.03 (0.018)	0.063	0.11 (0.031)	0.001
% White	0.64	0.65	0.58	0.00 (0.016)	0.872	-0.05 (0.023)	0.024	-0.01 (0.046)	0.895
<i>School characteristics</i>									
Counselor FTE	0.77	0.54	2.13	0.16 (0.047)	0.001	0.26 (0.062)	0.000	0.24 (0.104)	0.020
Teacher FTE	15.25	11.84	35.78	1.56 (0.287)	0.000	1.41 (0.404)	0.000	0.93 (0.660)	0.157
Special education FTE	1.50	1.02	4.37	0.08 (0.139)	0.552	-0.14 (0.202)	0.497	-0.42 (0.286)	0.145
Professional staff FTE	0.79	0.62	1.86	-0.05 (0.084)	0.562	0.05 (0.116)	0.692	0.12 (0.174)	0.479
Administration FTE	1.26	0.97	3.04	0.14 (0.070)	0.041	0.04 (0.101)	0.674	-0.29 (0.172)	0.096
N school observations	4,155	3,563	592	4,155		1,369		Varies	
N unique schools	385	338	69	385		159		Varies	

Notes: Robust standard errors clustered on the running variable in parentheses. MSE bandwidth is typically about ± 100 but varies across covariates. MSE-optimal bandwidth calculated using Stata `rdrobust` command. Includes year fixed effects.

et al. 2011,2016). Finally, we include a robustness check in [Supplementary Appendix tables S6 and S7](#) that include school fixed effects where we identify the effect of a given school changing their counselor staffing (where the student characteristics year-to-year within a school appear to be balanced).

Table 1 also illustrates the first stage estimate—schools just above the 450-student enrollment threshold for hiring an additional counselor employ 0.16–0.26 additional FTE counselor hours. At the full bandwidth, we also observe a significant discontinuity on teacher and administrator FTE hours—that schools above the threshold employ an additional 1.6 teachers and 0.14 administrators—but no difference in the number of special education educators or professional staff. We posit that since many schools will staff counselor FTE through hiring individuals who split their FTE between counseling and other school responsibilities, that we might expect an increase in teacher or administrator staffing at the threshold as part of the policy response (e.g., a school might add a 1.0 FTE counselor by hiring two teacher-counselors, who each have 0.5 FTE dedicated to counseling). Further, although in absolute magnitude

the increase in teacher FTE is larger than the counselor FTE effect at the bandwidth, we note that schools employ a larger number of teachers than counselors, and the increase in teacher FTE is only eight percent higher than the average teacher FTE at schools below the threshold while the increase in counselor FTE staffing is 48 percent higher than the average counselor FTE at schools below the threshold). We further examine in [Appendix](#) school compliance with this policy in terms of counselor characteristics and their FTE allocations. For example, we find that counselors in schools just above the threshold have less FTE time allocated to counseling and are more likely to hold multiple positions in the school (e.g., working as a part-time counselor and a part-time teacher).

Results

First Stage: Compliance with the Counselor Staffing Policy

We first examine school compliance with the state policy—do schools with enrollments above the threshold for hiring

Table 2 First Stage Estimates, Overall, and by School FRL

		BW	Counselor FTE			Counselor Caseload		
			<i>n</i>	Estimate	<i>p</i> -value	<i>n</i>	Estimate	<i>p</i> -value
Panel A	Overall	Full	4,155	0.16 (0.047)	0.001	3,393	-80.68 (15.447)	0.000
		Policy	1,369	0.26 (0.062)	0.000	1,339	-39.13 (22.025)	0.076
		MSE	847	0.24 (0.104)	0.020	595	-21.92 (23.235)	0.346
Panel B: By FRL	Low-FRL schools	Full	2,123	0.17 (0.054)	0.002	1,748	-82.58 (16.680)	0.000
		Policy	853	0.25 (0.070)	0.000	830	-17.14 (19.959)	0.391
	High-FRL schools	Full	2,032	0.14 (0.085)	0.103	1,645	-68.30 (29.411)	0.020
		Policy	516	0.22 (0.113)	0.051	509	-65.89 (51.158)	0.198

Notes: Robust standard errors clustered on the running variable in parentheses. Policy bandwidth is ± 225 ; MSE bandwidth calculated using `rdrobust` in Stata. Counselor caseload represents the total enrollment in a school divided by number of counselors; schools with no counselors are therefore missing a counselor caseload value and dropped from caseload model. Includes year fixed effects.

an additional counselor actually do so? We present the first stage estimates in [table 2](#) and the visualization in [figure 2](#). We use two definitions of compliance in [table 2](#)—the total FTE counselors in a school, and the estimated counselor caseload, measured as the total school enrollment divided by school counselor FTE. The number of FTE individuals counseling provides a sense of overall staff *availability*, while the measure of counselor caseload provides a better conceptualization staff *capacity*.

As previewed in [table 1](#), in panel A of [table 2](#), we show that schools hire about an additional 0.16–0.26 FTE counselors at the policy threshold, which corresponds to an average reduction in counselor caseload of 40–80 students.¹⁴

We then separately estimate the first stage for schools with below- or above-median shares of students on free or reduced-price lunch in panel B (median FRL share is about 53 percent of students) to test whether compliance varies by the student population a school serves. We do not find evidence of differential compliance. Low-FRL schools employ an additional 0.17–0.25 FTE counselors at the threshold and high-FRL schools employ an additional 0.14–0.22 FTE counselors at the threshold; estimates for either the full or policy bandwidth are not statistically different from each other. Overall, our first stage analysis highlights that schools are responsive to the counselor staff policy on average, but do not fully comply—hiring an average of 0.16 FTE counselors when the policy would suggest an additional 1.0 FTE counselor.

In Appendix table A1, we examine the extent to which counselor demographics differ at the policy discontinuity. We see no difference in the share of female, Black, White, or Native counselors in a school or in the share of counselors who hold a master's degree. Overall, 99% of counselors have a master's degree, about 90% are female, and about 88% are white. Next, we examined how counselors' appointments

varied above and below the threshold. We see no discontinuity in counselor salary, but counselors at schools just above the state threshold for hiring an additional counselor spend 17% less of their FTE on counseling duties and on average hold an additional 0.36 positions in their schools. That is, schools just above the threshold for hiring an additional counselor do employ more counseling FTE (as noted in [Table 2](#) of our main analysis), but in doing so employ counselors who are more likely to hold multiple jobs (e.g., as a counselor and as a teacher) and thus have a smaller share of their overall FTE allocation focused on counseling duties than schools that employ fewer counselors just below the policy threshold.¹⁵

Main Results: The Impact of Counselor Workload on Program Access

In [Figure 3a](#) and [3b](#) and [Table 3](#), we present our reduced form and regression discontinuity estimates. In [Table 3](#) for each outcome, we note the reduced form estimate of crossing the threshold and then the 2SLS estimates (using the full sample, the policy-driven bandwidth, and the data-driven MSE bandwidth calculated using the `rdrobust` package in Stata) to estimate the projected effect of full compliance (e.g., hiring a full additional counselor at the threshold). We examine both the *share* of 12th grade students receiving Pell or Promise, and then the raw *count* of students receiving each financial aid program. The results in [Table 3](#) do provide some support for Hypothesis 1 when we examine the reduced form and the 2SLS estimates for the full sample, but the estimates in the policy bandwidth and MSE bandwidth do not reach conventional thresholds for statistical significance. At schools enrolling fewer than 450 students, about 27% of 12th

¹⁴This measure of counselor caseload drops schools from analysis with zero counselors, where there is no computable ratio.

¹⁵For this analysis, we merge our sample to publicly available counselor demographic and staffing data from the Oklahoma Department of Education (OKDOE). Of the 4,155 school-by-year observations in our main analytic sample, 3,034 observations merge with the OKDOE counselor characteristics data, representing about 73% of the school-by-year observations in our analysis.

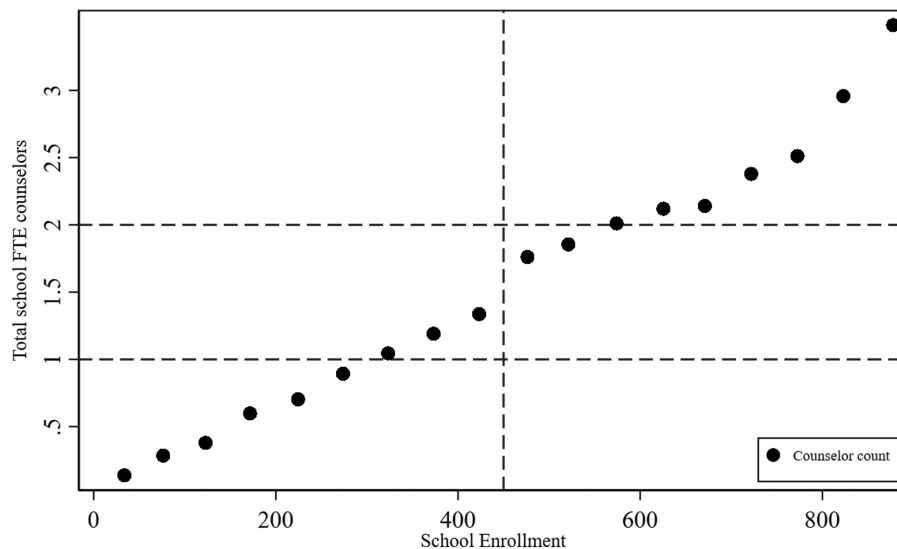


Figure 2. First stage compliance. Notes: The Above Figure Shows School Counselor Staffing Levels in Oklahoma Public Schools (BW: ± 225). Binsize = 50. First-Stage Linear Discontinuity, Counselor Count: 0.264 (0.062)^{***}, $N = 1,369$.

graders receive Pell. The proportion of students receiving Pell increases by 3.6 percentage points at the threshold (about a 13% increase). Fewer students in schools below the threshold receive the Oklahoma Promise scholarship—12.7 percent—and Promise receipt increases by about 1.7 percentage points at the threshold (about a 13% increase). The reduced form estimates for the raw count of recipients is statistically significant for Pell (an additional 4.3 students relative to a control mean of about 11 students) but not statistically significant for Promise (an additional 0.9 students relative to a control mean of about 4.8 students).

Given imperfect compliance with the policy, the 2SLS estimates are large—for example, a 32-percentage point increase in Pell receipt and 15-percentage point increase in Promise receipt from hiring a full 1.0 FTE counselor, estimated with the full sample. However, the 2SLS estimates in both the policy and MSE bandwidth do not reach conventional levels of statistical significance when we look across all years of data. The results in Figure 4a and 4b which employ an event study approach, on the other hand, do suggest support for Hypothesis 3—the effects of reducing counselor workload are significant in the time period with greater administrative burden. Therefore, we conclude that increasing counselor capacity does appear to increase access to both the Federal Pell Grant and Oklahoma’s Promise among socioeconomically disadvantaged student clientele (Hypothesis 1), depending on the time period we analyze.

The Moderating Role of Administrative Burden

Next, we examine differential effects of counselor staffing on by student race and by cohort to test our predictions in Hypotheses 2 and 3. Given the small number of students receiving Promise and Pell at each school-year we observe, we report results by race using the count of students receiving the program rather than the percent of students of a given race receiving the program. We also cut the results by graduation year in Table 4; students in the graduating cohort of 2008 and beyond were subjected to much more stringent Oklahoma Promise access rules and a more complex application process

that provided more discretion to counselors. Descriptively, we calculate that program complexity reduced program access across these two time periods—on average, 7.4 students per school received the Oklahoma Promise program in pre-2008 cohorts, which decreased to an average of 5.6 students in post-2008 cohorts.

Despite little variance in compliance with the policy in the two time periods (the first-stage estimate for pre-2008 cohorts is a 0.18 FTE increase in staffing relative to a 0.16 FTE increase in post-2008 cohorts), we do see evidence of a stronger effect of counselors for cohorts experiencing a higher level of administrative burden.¹⁶ We find no statistically significant increases in Pell or Promise receipt cohorts prior to the change in Oklahoma’s Promise program requirements in 2007, but positive effects on program receipt in more recent years.¹⁷ We estimate that among more recent graduating cohorts, schools above the policy threshold saw an additional 6.2 students receiving Pell (relative to an average of 12 students receiving Pell in below-threshold schools, a 50% increase) and an additional 1.5 students receiving Promise (relative to about five students receiving Promise in below-threshold schools, about a 30% increase).

As the Promise program increased in administrative burden and complexity, additional staff capacity appears to be a way to overcome a declining trend in program access, in line with Hypothesis 3. Looking by student race we see no significant effect on White student Pell or Promise receipt and statistically significant increases in financial aid receipt for Black, Hispanic, and Native students at the policy threshold, suggesting additional counseling capacity benefited traditionally underrepresented students, particularly during eras of increased financial aid access complexity.

¹⁶Results are robust to inclusion or exclusion of covariates; Appendix Table A2 shows the effect without covariates.

¹⁷Results are robust to restricting the post-period to the three cohorts immediately affected by the shift in administrative burden; results available upon request.

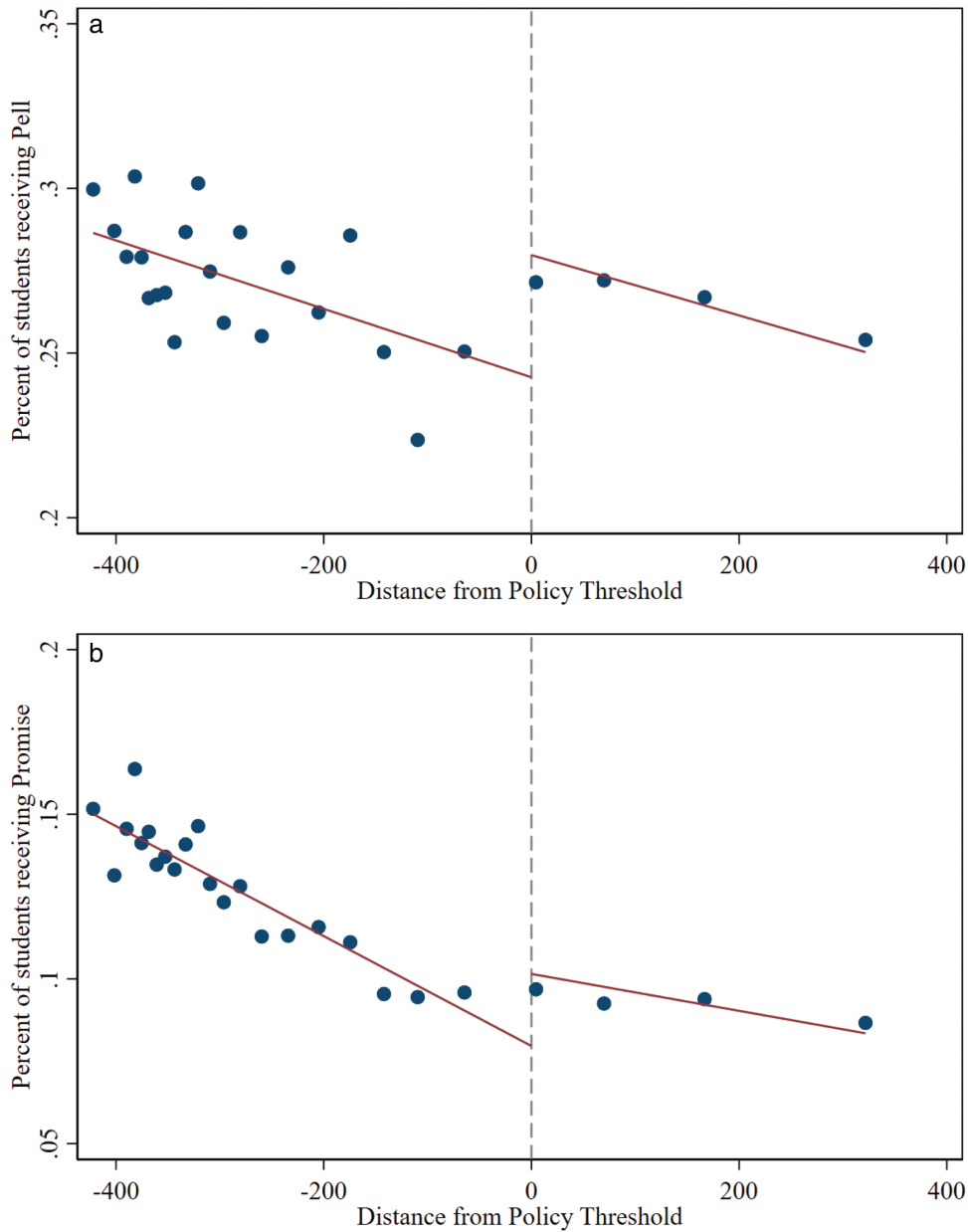


Figure 3. (a) Reduced Form Effect, Pell Receipt. Notes. Reduced Form Discontinuity, Percent Receiving Pell: 0.036 (0.012)**. $N = 4,155$. (b) Reduced Form Effect, Promise Receipt. Notes. Reduced Form Discontinuity, Percent Receiving Promise: 0.017 (0.005)**. $N = 4,155$.

Indeed, while the number of additional students receiving Pell in the post-period may be higher for white students than Hispanic students (e.g., a reduced form effect of 0.93 additional white Pell recipients compared with 0.54 additional Hispanic Pell recipients), the magnitude of the effect is greater for Hispanic students (a 90% increase relative to 0.6 Hispanic Pell recipients below the threshold) than for white students (a 15.5% increase relative to 5.97 White Pell recipients below the threshold). We also find the post-2008 Promise effect is greater for Hispanic (a 92.8% increase relative to 0.28 recipients below the threshold), Black (a 181.8% increase relative to 0.22 recipients below the threshold), and Native (a 67% increase relative to 0.88 recipients below the threshold) students than for white students (a non-statistically significant 8.9% increase relative to 2.78 recipients below the threshold). This provides support for Hypothesis 2 that

benefits would be concentrated among marginalized students facing multiple systems of marginalization.^{18,19}

Given concerns about discontinuities in school characteristics observed in Table 1, we also run a “donut” regression estimate where we drop schools with enrollments closest to

¹⁸We do not have 12th grade enrollment by race to calculate accurate percent program receipt measures. As a robustness check to ensure our results are not skewed by sample size, we run a variant of Table 4 in Supplementary Appendix Table S1 that restricts the analysis to schools with non-zero enrollments for each student race observed in our data. That analysis reduces our analytic sample (e.g., for the post-2008 sample that reduces sample from 3,022 observations for 383 unique schools to 1,899 observations for 336 unique schools), but despite the reduction in precision we still estimate significant effects that mirror trends in Table 4.

¹⁹Appendix Table A3 reports the same results as Table 4 but using the ± 225 enrollment policy bandwidth instead; the sample size is smaller but the same trend of larger and more precisely estimated post-2008 effects holds.

Table 3 Effect of Counselor Staffing on Financial Aid Receipt

	Below Mean	2SLS							
		Reduced Form		Full Sample/No Bandwidth		Policy Bandwidth		MSE Bandwidth	
		Estimate	<i>p</i> -value	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value
Received Pell, percent	0.272	0.036 (0.012)	0.003	0.318 (0.167)	0.067	0.125 (0.099)	0.215	0.103 (0.185)	0.578
Received Pell, count	10.886	4.331 (1.529)	0.005	38.335 (19.373)	0.058	12.965 (10.738)	0.237	10.782 (19.251)	0.575
Received promise, percent	0.127	0.017 (0.005)	0.002	0.151 (0.078)	0.062	0.041 (0.040)	0.314	0.031 (0.057)	0.588
Received promise, count	4.765	0.861 (0.000)	0.000	7.622 (5.815)	0.201	2.125 (4.056)	0.604	3.534 (5.911)	0.550
N observations		4,155		4,155		1,369		Varies	

Notes: Robust standard errors clustered on the running variable in parentheses. Includes school-level demographics (enrollment by race, share of students receiving special education, share of students on free or reduced price lunch) and district labor market indicators (district unemployment rate, share in poverty, and share of adults with a college education) as well as year fixed effects. MSE bandwidth calculated using *rdrobust* package in Stata. Effective sample size when calculating effect within MSE bandwidths ranges between 758 and 875, depending on the outcome.

the policy threshold; if we suspected precise manipulation around the policy threshold we would suspect this to only be a concern for schools very close to the threshold (assuming it is easier to manipulate school enrollment by ~10 students than ~50 students). Our donut regression as reported in [Supplementary Appendix Table S2](#) yields remarkably consistent results, whether we drop schools with enrollments ± 10 , ± 20 , or ± 30 students around the threshold.²⁰

Event Study Analysis

In addition to our main regression discontinuity estimates, we further explore the extent to which the effects of additional counseling vary by administrative burden in an event study framework. We run a model where we separately include year indicators and interact them with the treatment effect of whether or not a school was above or below the enrollment threshold in that year; by then graphing those estimates we can visualize the extent to which the effects of additional counselors vary before and after the administrative burden of accessing Promise changed in 2008. As [Figure 4a](#) (Pell receipt) and [Figure 4b](#) (Promise receipt) illustrate, we see little to no effect of additional counselors in pre-2008 years, but a large and persistent difference in Pell and Promise receipt by whether a school was above or below the policy threshold in post-2008 years.

Robustness Checks: School Fixed Effects

Our main analytic approach compares schools above and below the threshold in a given year. Given the panel structure of our data, we can run a modification of this analysis where we add school fixed effects and a time trend to our models. This enables us to compare a school's compliance and outcomes in years when it has fewer than 450 schools to years when it has more than 450 students. The main advantage of this approach is that within-school comparisons

do not suffer from the same potential endogeneity concerns that schools below and above the threshold are fundamentally different in ways that may affect the outcome other than through counselor hiring. However, the main disadvantage of this approach is that the outcomes are only identified off schools with variation in whether their enrollment is above or below the 450-enrollment threshold for hiring an additional counselor. In practice, very few schools cross the enrollment threshold ($N = 22$, as opposed to $N = 69$ schools ever above the threshold in our main analysis), and we therefore view the school fixed effects model as a robustness check to our main model.

In [Supplementary Appendix Table S4](#), we first illustrate that schools are similar to themselves in years with enrollments above and below 450 students, with the exception of the policy treatment effect of employing more school counselor FTE hours (and marginally more FTE teachers) when their enrollment crosses the policy threshold. [Supplementary Appendix table S5](#) replicates [table 2](#) from our main analysis illustrating the first stage compliance; we observe similar first stage compliance when incorporating school fixed effects, estimating schools hire an additional 0.15–0.19 counselors when they cross the enrollment threshold, resulting in a counselor caseload reduction of about 50–100 students per FTE counselor. We then report a replication of our main effects in [Supplementary Appendix tables S6 and S7a](#) replication of the results by race and by the administrative burden each cohort faced accessing Oklahoma's Promise. We observe a reduced form 6.8 percentage point increase in Pell receipt and 2.6 percentage point increase in Promise receipt at the policy threshold when using school fixed effects. Looking by race and cohort we also see similar trends as in the main analysis, estimating a reduced form increase of about 14–15 Pell recipients and an additional 3–4 Promise recipients in the post-2008 cohorts.²¹ Overall, we view the results from models

²⁰As an additional check, we examine whether we observe a treatment effect at “placebo” policy thresholds—whether there is a significant difference at the 350-student or 550-student thresholds where there is not observed difference in counselor staffing. As we report in [Supplementary Appendix Table S3](#) we do not observe a significant effect at either placebo threshold.

²¹We note that pre-2008 estimates are only identified off schools that have enrollments on either side of the 450 enrollment threshold during the three years of the pre-period and therefore we encourage caution in the interpretation of the pre-2008 estimates.

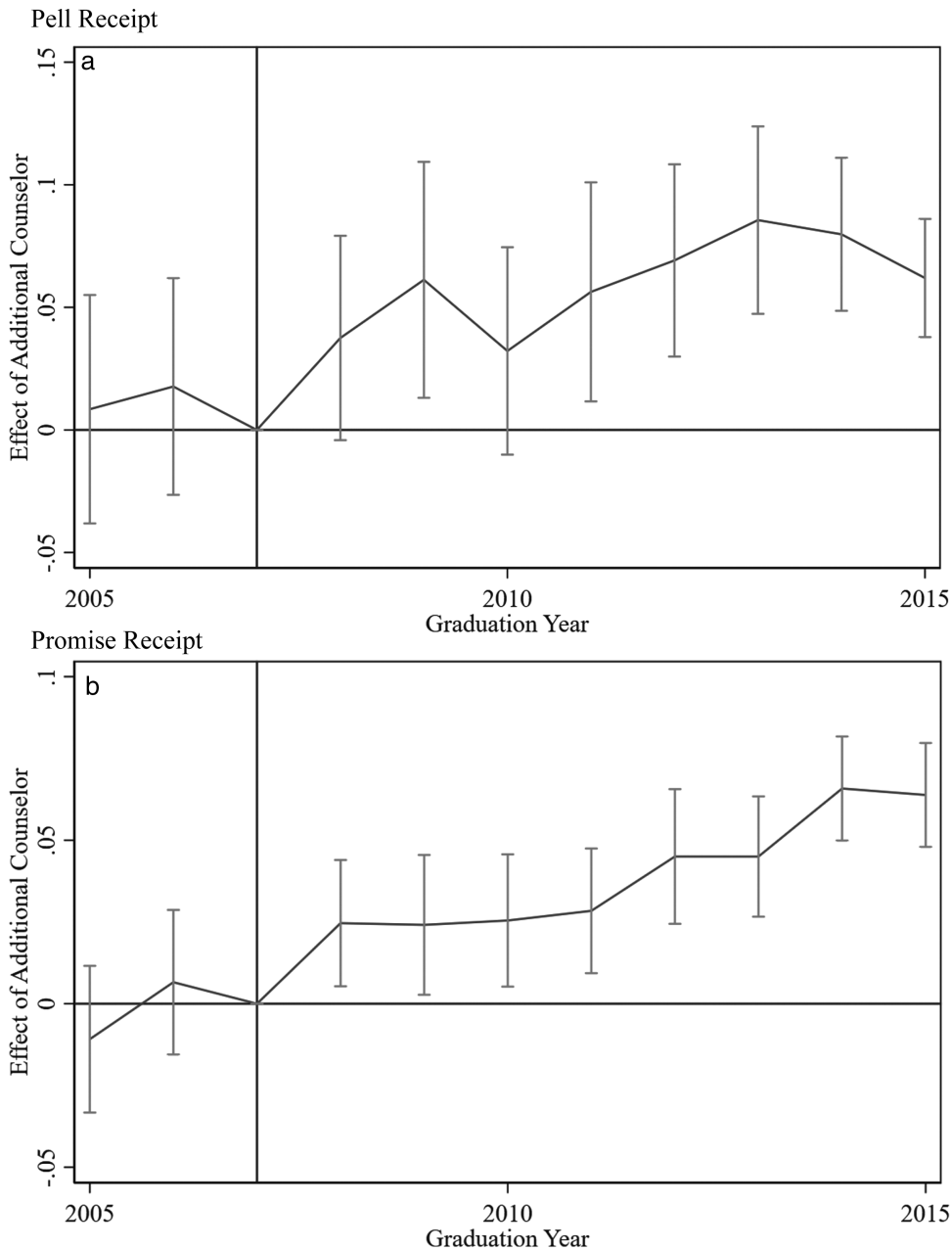


Figure 4. (a) Event Study Plot of Pell Receipt. *Notes:* Plots the Every Student Estimates of Additional Counselor Effect Pre- and Post-promise Administrative Burden Increase in 2008. Pre-policy Year (2007) Effect Set to Zero. (b) Event Study Plot of Promise Receipt. *Notes:* Plots the Every Student Estimates of Additional Counselor Effect Pre- and Post-promise Administrative Burden Increase in 2008. Pre-policy Year (2007) Effect Set to Zero.

incorporating school fixed effects as quite similar in terms of direction, magnitude, and precision as our main findings.

Conclusion

While scholars have invested significant effort into diagnosing the causes and consequences of administrative burdens and racial discrimination in public services, the tools public managers can use to reduce disparities in program access have been relatively understudied. Building on recent scholarship on the organizational factors that drive implicit bias among street-level bureaucrats, we provide causal evidence on the conditions under which reducing street-level bureaucrats' workload can impact key client outcomes for individuals

facing intersectional systems of oppression. Our study leverages unique administrative data and a regression discontinuity design to explore the impacts of reducing counselor workload on program access to burdensome college financial aid for low-income and racially minoritized students.

First, even when we did not account for race/ethnicity and variation over time in the level of burden in Oklahoma's Promise program, reducing counselor workload increased the number of low-income students accessing the Oklahoma's Promise and Federal Pell grant programs. Though there is some imprecision in the 2SLS estimates in the policy and MSE bandwidth, our robustness analysis including school fixed effects confirms the positive relationship between counselor capacity and the proportion of low-income students accessing

Table 4. Effect of Counselor Staffing on Financial Aid Receipt, by Cohort

		Pre-2008					Post-2008						
		Reduced Form	First Stage	2SLS		Reduced Form	First Stage	2SLS					
Received Pell, count	All students	-0.367 (2.852)	0.898 (0.098)	0.178	0.069	-5.151 (40.370)	0.900	6.233 (1.764)	0.000 (0.053)	0.155	0.004	50.791 (23.341)	0.039
	White	-0.526 (1.910)	0.783			-7.368 (28.996)	0.802	0.931 (0.984)	0.344			7.568 (8.492)	0.381
	Black	0.518 (0.909)	0.569			7.228 (17.106)	0.677	1.908 (0.813)	0.019			15.554 (8.885)	0.092
	Hispanic	-0.403 (0.357)	0.259			-5.631 (9.216)	0.548	0.543 (0.233)	0.020			4.425 (2.494)	0.088
	Native	0.879 (0.956)	0.358			12.262 (19.695)	0.541	2.627 (0.693)	0.000			21.418 (9.209)	0.028
Received promise, count	All Students	-0.586 (1.185)	0.621			-8.202 (19.605)	0.680	1.518 (0.631)	0.016			12.364 (6.980)	0.089
	White	-0.251 (0.950)	0.792			-3.520 (14.127)	0.806	0.247 (0.419)	0.557			2.006 (3.545)	0.576
	Black	0.051 (0.242)	0.834			0.708 (3.683)	0.849	0.398 (0.194)	0.041			3.240 (2.004)	0.118
	Hispanic	0.215 (0.180)	0.233			2.994 (4.974)	0.554	0.258 (0.124)	0.038			2.099 (1.289)	0.116
	Native	-0.163 (0.393)	0.679			-2.271 (6.571)	0.733	0.590 (0.243)	0.016			4.814 (2.586)	0.074
	N observations	1,133					3,022						
N schools	381					383							

Notes: Robust standard errors clustered on the running variable in parentheses. Includes school-level demographics (urbanicity, enrollment by race, share of students receiving special education, share of students on free, or reduced price lunch), staffing information (teacher and administrator count), and district labor market indicators (district unemployment rate, share in poverty, and average income) and year fixed effects. Estimated off of full sample.

both means-tested financial aid programs. These findings support Hypothesis 1, suggesting that street-level bureaucrats' workload may be an organizational lever that can reduce burdens and increase access to college financial aid for socioeconomically disadvantaged students.

Second, we test a novel theoretical hypothesis—that draws from administrative burden literature—to predict the conditions under which reductions in street-level bureaucrats' workload can enhance equity in program access. We predict that street-level bureaucrats' workload reductions will be particularly important for marginalized clientele when administrative burdens are expanded and there are increases in the complexity and ambiguity in client cases. We find support for this hypothesis—the impacts of reducing workload on program access are concentrated in years following an expansion in administrative burden in the Oklahoma's Promise program (after the legislature added FAFSA as a requirement for Oklahoma's Promise as well as additional income checks).

Despite some imprecision in the 2SLS estimates, which is standard for power hungry fuzzy regression discontinuity designs, the reduced form shows positive and statistically significant increases in program access for Black, Hispanic, and Native American students in the post-2008 cohorts, but not in the pre-2008 cohorts. We find positive effects for Black, Native American, and Hispanic low-income students in the years with additional burdens for *both* the Oklahoma's Promise program and the Federal Pell Grant program. This indicates a potential spillover effect from

the Oklahoma's Promise requirements, which implicated access to the Federal Pell Grant program by requiring the FAFSA. The primary way that burdens were expanded in the Promise program linked the only document necessary for receiving the Pell Grant—the FAFSA application. Therefore, while students have many other avenues through which they may lose access to the Promise program, either due to the academic, conduct, or income verification requirements, Pell Grant students only have to fill out the FAFSA. FAFSA submission (the only requirement for Pell receipt) may be an easier task for counselors to support than full application to the Oklahoma Promise program (which requires FAFSA as well as additional forms and certifications that require coordination between the student and parent). In essence, Pell falls more within the realistic purview of a school counselor's duties, while Promise requirements may fall outside of the counselors' control. Therefore, it is not altogether surprising that we see a greater expansion in access to the Pell Grant after the Promise program change, which required the submission of an FAFSA as part of the expansion in eligibility requirements after 2007. Together, these findings support the notion that expansions in the administrative capacity of street-level bureaucrats can reduce bias and mitigate the negative impacts of administrative burden on equity in program access. These findings have numerous important insights for future research at the intersection of street-level bureaucracy, administrative burden, and racial discrimination on the frontlines of policy implementation.

First, administrative burdens could contribute to racial disparities not only by the direct imposition of learning, compliance, and psychological costs on clients, but also by enhancing complexity and ambiguity in client cases for street-level bureaucrats. Building on recent work demonstrating that burdens are racialized tools that serve to perpetuate inequities (Ray, Herd, and Moynihan 2022), our findings demonstrate how street-level organizations can potentially mitigate the negative effects of administrative burden for low-income and racially minoritized clients. This provides promising new evidence that can inform theory and practice at the intersections of street-level bureaucracy and administrative burden.

Our study also has important limitations that could be built upon in future research. First, a key limitation of our data is that we are measuring outcomes at the school level when interactions with counselors happen at the individual level. While prior research leverages research design strategies such as survey experiments and audits that observe stated preferences and preliminary revealed preferences such as the response rate and friendliness, our research captures consequential downstream client outcomes. In this way, we acknowledge the limitations based on the unit of analysis and inability to measure individual citizen-state interactions, but we argue that the added benefit of examining downstream outcomes outweighs these limitations. Future studies that could combine data capturing individual interactions with street-level bureaucrats and the measurement of downstream outcomes would be particularly fruitful for the development of this literature, especially in teasing out the causal mechanism underlying our findings.

Second, it is possible that there were other changes that occurred during the post-2008 period that could be influencing the results other than the level of administrative burden in the Oklahoma's Promise program. As a result of the program changes, the state agency had to update the counselors on the rules for the program, and in doing so they could have forged a stronger connection between front-line implementation and the state agency priorities—one of those priorities has been to expand access to the Oklahoma's Promise among income eligible students as a result of an external audit (Southern Regional Education Board 2018). This communication could have also clarified program rules and goals, which could have spillover effects on program access. If the communication from the state agency was driving the changes we observe in the post-2008 period, we might expect that all schools experienced increases in Pell and Promise receipt after the program changes, but this was not the case. In fact, when we examine the share of 12th grade students receiving Pell and Promise in non-treated schools (school enrollment below the counselor threshold) before and after the change in program burden in 2008, we see that the share of Pell recipients declined from 33.4% to 24.8% and the share of Promise recipients declined from 16.7% of students to 11.2%. We take this as evidence that the effect of the scholarship reform in 2007 was not to refocus time to supporting students' applications across all schools. Rather, schools that had additional counselors due to the counselor-student ratio policy were better able to serve students who faced additional burdens in the application process after the 2007 legislative reforms. However, we also acknowledge that this is one of multiple interpretations of the results and we encourage future research to further test our hypothesis regarding the moderating impact of administrative burdens.

Another potential confounder were the efforts under the Obama Administration to increase FAFSA applications, which could be influencing the results (US Department of Education 2014). However, we do not consider this a concern for our analysis for two reasons. First, the largest federally funded programs aimed at providing personalized assistance in FAFSA completion (including GEAR up and TRIO) started in Oklahoma prior to the years in our analysis. Second, any Federal or State government efforts to increase FAFSA applications should have impacted schools above and below the threshold equally.

One other potential confounder could be the economic recession, which did shift college going behavior and caused widespread unemployment that may have increased the number of students eligible for means-tested financial aid programs (Barr and Turner 2015). However, given that we control for the income-eligible population of students empirically and include a time trend and year fixed effects, we consider this is less of a concern for our analysis. Together, to the best of our knowledge, the most important change during this time period was the expansion of burden, but we do acknowledge that there could also be other confounders that are unobserved. Ultimately, our article is but the first test of our hypothesis regarding the moderating effects of expanding burden on the relationship between street-level bureaucrats' workload and program access, and we encourage future research to test whether our results hold in other contexts where burdens have expanded.

Finally, there could be multiple mechanisms driving the results that we are not able to observe that should be tested further in future research. For instance, it could be the case that after counselors experience a reduced workload, they could be better able to devote more time and attention to higher needs students who may require more time-consuming hands-on support than other students with more supportive parents. Alternatively, it could be that the additional counselors hired shift the organizational culture when it comes to interacting with "needy" students in ways that reduce bias in client prioritization. If prior practices were resulting in bias against marginalized students, and the new counselor challenges those practices, it is possible that the hiring of counselors is both influencing workload and organizational culture (Cohen 2018; Lavee, Cohen, and Nouman 2018). Finally, it could also be the case that the counselors hired share the demographic identity of students and therefore engage in symbolic or active representation (Headley, Wright, and Meier 2021; Meier 2019). These possibilities provide fruitful directions for future researchers looking to advance theory on the organizational levers public managers can utilize to reduce discrimination at the front-lines, and the conditions under which those levers will impact downstream outcomes for intersectionally minoritized populations.

Supplementary Material

Supplementary data is available at the *Journal of Public Administration Research and Theory* online.

Data Availability

The data underlying this article are available at the Open Science Framework repository doi: 10.17605/OSF.IO/RSYU3.

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Appendix

Table A1. Counselor Characteristics Above and Below Threshold

Variable	Below	Above	Discontinuity	
			Estimate	<i>p</i> -value
Has masters	0.989	0.990	-0.006 (0.006)	0.314
Female	0.877	0.897	-0.011 (0.022)	0.609
Black	0.011	0.035	-0.011 (0.013)	0.413
White	0.898	0.857	-0.008 (0.025)	0.761
American Indian/native	0.070	0.068	-0.027 (0.018)	0.133
Average salary	\$40,977	\$43,763	-1,030.434 (730.733)	0.159
Share FTE for counselling	0.852	0.962	-0.171 (0.013)	0.000
Average number of schools served	1.684	1.097	0.002 (0.049)	0.964
Average number of positions	1.362	1.109	0.359 (0.030)	0.000
N observations	2,549	485	3,034	
N schools	334	67	384	

Notes: Robust standard errors clustered on the running variable in parentheses. Reports on school counsellor characteristics from the Oklahoma Department of Education, reporting average characteristics for schools above and below the threshold and differences in characteristics at the threshold. Does not include schools with zero counsellors.

Table A2. Effect of Counselor Staffing on Financial Aid Receipt, by Cohort Without Covariates

		Pre-2008				Post-2008							
		Reduced Form		First Stage		2SLS		Reduced Form		First Stage		2SLS	
		Estimate	<i>p</i> -value	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value
Received Pell, count	All students	0.697 (2.918)	0.811	0.178 (0.098)	0.069	3.907 (16.387)	0.821	5.382 (1.819)	0.003	0.155 (0.053)	0.004	34.721 (16.227)	0.058
	White	-2.467 (2.044)	0.228			-13.836 (13.300)	0.346	1.434 (1.070)	0.180			9.254 (7.611)	0.252
	Black	3.364 (1.874)	0.073			18.867 (13.920)	0.233	0.415 (1.109)	0.709			2.675 (6.953)	0.708
	Hispanic	0.134 (0.445)	0.763			0.753 (2.495)	0.775	0.651 (0.238)	0.006			4.202 (2.291)	0.097
	Native	0.507 (1.095)	0.643			2.842 (6.256)	0.669	2.677 (0.728)	0.000			17.272 (7.416)	0.042
Received promise, count	All students	-0.554 (1.187)	0.641			-3.107 (6.703)	0.662	1.598 (0.616)	0.010			10.307 (5.481)	0.089
	White	-1.054 (1.005)	0.294			-5.910 (6.167)	0.382	0.611 (0.447)	0.172			3.939 (3.316)	0.262
	Black	0.684 (0.396)	0.084			3.835 (2.970)	0.253	0.044 (0.266)	0.870			0.281 (1.686)	0.871
	Hispanic	0.471 (0.222)	0.034			2.639 (1.942)	0.232	0.318 (0.132)	0.016			2.050 (1.163)	0.108
	Native	-0.255 (0.441)	0.563			-1.431 (2.626)	0.609	0.615 (0.271)	0.023			3.965 (2.180)	0.099
	N observations	1,133				3,022							
	N schools	381				383							

Notes: Robust standard errors clustered on the running variable in parentheses. Includes school-level demographics (urbanicity, enrollment by race, share of students receiving special education, share of students on free, or reduced price lunch), staffing information (teacher and administrator count), and district labor market indicators (district unemployment rate, share in poverty, and average income) and year fixed effects. Estimated off of full sample.

Table A3. Effect of Counselor Staffing on Financial Aid Receipt, Policy Bandwidth

		Pre-2008						Post-2008					
		Reduced Form		First Stage		2SLS		Reduced Form		First Stage		2SLS	
		Estimate	p-value	Estimate	p-value	Estimate	p-value	Estimate	p-value	Estimate	p-value	Estimate	p-value
Received Pell, count	All students	-1.419	0.711	0.428	0.001	-6.177	0.711	4.380	0.034	0.205	0.003	27.196	0.112
		(3.827)		(0.128)		(16.427)		(2.056)		(0.069)		(16.518)	
	White	-0.696	0.795			-3.061	0.792	0.792	0.453			4.918	0.478
		(2.676)				(11.470)		(1.056)				(6.823)	
	Black	0.816	0.466			3.510	0.528	1.491	0.164			9.253	0.220
		(1.118)				(5.460)		(1.070)				(7.361)	
Hispanic	-0.122	0.753			-0.526	0.753	0.243	0.371			1.509	0.402	
	(0.389)				(1.650)		(0.272)				(1.770)		
Native	-1.639	0.164			-7.075	0.300	2.233	0.014			13.871	0.079	
	(1.174)				(6.646)		(0.902)				(7.579)		
Received promise, count	All students	-0.803	0.644			-3.517	0.646	1.064	0.175			6.602	0.240
		(1.735)				(7.534)		(0.782)				(5.479)	
	White	0.414	0.768			1.721	0.772	0.268	0.624			1.662	0.633
		(1.403)				(5.864)		(0.547)				(3.433)	
	Black	-0.162	0.616			-0.690	0.607	0.413	0.117			2.566	0.174
		(0.323)				(1.321)		(0.263)				(1.834)	
Hispanic	0.130	0.659			0.558	0.667	0.334	0.023			2.070	0.082	
	(0.296)				(1.275)		(0.146)				(1.144)		
Native	-1.168	0.029			-5.035	0.189	0.197	0.524			1.227	0.544	
	(0.534)				(3.706)		(0.309)				(1.995)		
N observations		389						980					
N schools		142						145					

Notes: Robust standard errors clustered on the running variable in parentheses. Includes school-level demographics (urbanicity, enrollment by race, share of students receiving special education, share of students on free, or reduced price lunch), staffing information (teacher and administrator count), and district labor market indicators (district unemployment rate, share in poverty, and average income) and year fixed effects. Estimated off of policy bandwidth (-225 enrollment).

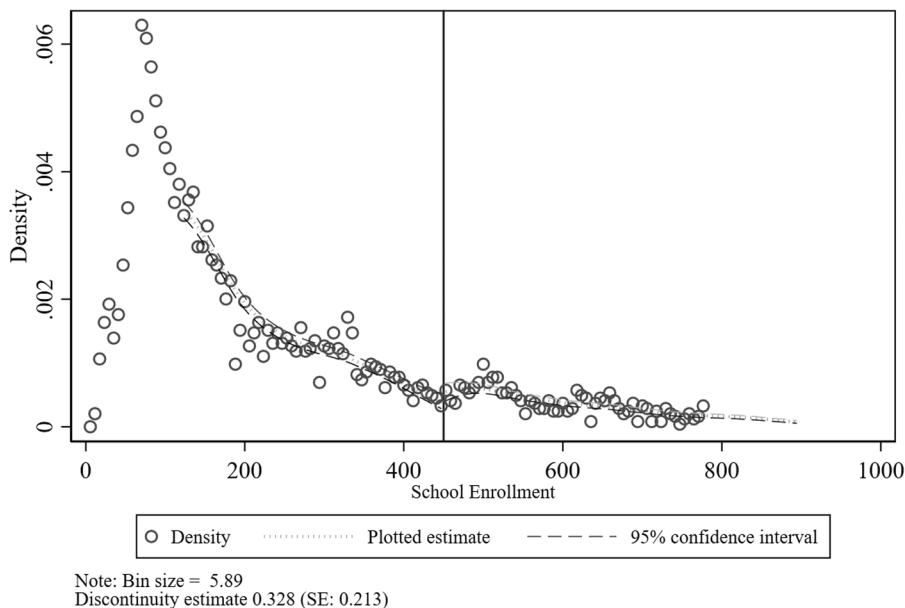


Figure A1. McCrary Density Test. Note: Bin Size = 5.89. Discontinuity Estimate 0.328 (SE: 0.213).

Table A4. Changes in Administrative Burden Over Time

Stage in process	Before 2007	2007–2015
Middle/ High School	<ol style="list-style-type: none"> 1. Must submit the five-page OP application form by the deadline in 8th, 9th, or 10th grade. 2. Must submit income documentation (tax returns) with the application in 8th, 9th, or 10th grade proving that the family makes below \$50,000 a year. 3. Must agree to complete the 17-unit core curriculum. 4. Must make a 2.5 overall GPA. 5. Must make a 2.5 GPA in the core curriculum. 6. Must attend school regularly. 7. Must do homework regularly. 8. Must refrain from substance abuse. 9. Must refrain from criminal or delinquent acts. 10. Must be certified by counselor that they met all of the academic and conduct requirements upon graduation from high school. 	<ol style="list-style-type: none"> 1. Must submit the five-page OP application form by the deadline in 8th, 9th, or 10th grade. 2. Must submit income documentation (tax returns) with the application in 8th, 9th, or 10th grade proving that the family makes below \$50,000 a year. 3. Must agree to complete the 17-unit core curriculum. 4. Must make a 2.5 overall GPA. 5. Must make a 2.5 GPA in the core curriculum. 6. Must attend school regularly. 7. Must do homework regularly. 8. Must refrain from substance abuse. 9. Must refrain from criminal or delinquent acts. 10. Must be certified by counselor that they met all of the academic and conduct requirements upon graduation from high school. 11. Upon high school graduation, must submit income documentation at the time the student begins college to prove family income is below \$100,000 a year. 12. Must submit Free Application for Federal Student Aid as documentation proving that they are a US citizen or lawfully present in the United States
College	<ol style="list-style-type: none"> 1. Must start college within 3 years of high school graduation 2. May not receive the award for more than 5 years (consecutive) or the completion of a baccalaureate degree. 	<ol style="list-style-type: none"> 1. Must start college within 3 years of high school graduation 2. May not receive the award for more than 5 years (consecutive) or the completion of a baccalaureate degree. 3. Must refrain from being expelled for more than one semester for conduct reasons. Otherwise, permanently lose eligibility.