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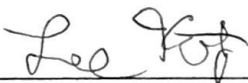
*"Essays in Labor Economics"*

presented by **Ayushi Narayan**

candidate for the degree of Doctor of Philosophy and hereby  
certify that it is worthy of acceptance.

Signature  \_\_\_\_\_

Typed name: Prof. Claudia Goldin

Signature  \_\_\_\_\_

Typed name: Prof. Lawrence Katz

Signature  \_\_\_\_\_

Typed name: Prof. Edward Glaeser

Signature  \_\_\_\_\_

Typed name: Prof. Katherine Coffman

Date: March 2, 2023

# Essays in Labor Economics

A dissertation presented

by

Ayushi Narayan

to

The Department of Economics

in partial fulfillment of the requirements

for the degree of

Doctor of Philosophy

In the subject of

Economics

Harvard University

Cambridge, Massachusetts

March 2023

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*Dissertation Advisor:*  
**Claudia Goldin**

*Author:*  
**Ayushi Narayan**

## **Essays in Labor Economics**

### **Abstract**

In this dissertation, I examine the determinants of workplace discrimination using the United States Postal Service (USPS) as a case study. The USPS is one of the largest employers in the country, and as part of the federal government, it must comply with the Freedom of Information Act (FOIA) and share privacy protected data with the public. Through FOIA requests, I have obtained data on roughly 250,000 Equal Employment Opportunity (EEO) complaints bringing more than 800,000 charges of harassment and other forms of discrimination at more than 12,000 USPS offices from fiscal years 2004 through 2019. I use this dataset in a series of investigations that examine the factors that contribute to increased discrimination at this important institution.

The first chapter of my dissertation, “The Impact of Financial Stress on Workplace Harassment and Discrimination,” explores how stress that stems from worries about finances influences discrimination and harassment at work. Stress can cause individuals to act out in unwanted ways, and I argue that one way this acting out can manifest itself is via discrimination and harassment. To isolate the role of financial stress, I study the financial stress that workers experience over the course of a fixed two-week pay period at the USPS. My empirical strategy is similar in spirit to the methods used in studies examining the effect of the monthly government benefit cycle on various outcomes. Consistent with a hypothesized increase in financial stress during the second week of the pay cycle, my results show that incidents of harassment and

discrimination rise by about 5 percent in the second week of the pay cycle as compared with the first week. Using data that differentiates when a discrimination incident occurred from when it was reported along with data on other characteristics of the complaint, I find that the uncovered effects are likely driven by changes in the number of incidents rather than in their reporting.

The second chapter, “The Impact of Extreme Heat on Workplace Harassment and Discrimination,” examines a different form of stress: heat stress. Using variation in extreme heat over space and time, I find that heat stress experienced on days when maximum temperatures exceed 90 degrees increases discrimination incidents by roughly 5 percent relative to days when temperatures are between 60 and 70 degrees. The effects are widespread across the USPS, and they also appear to be driven by changes in the number of incidents rather than in their reporting are experienced by workers. The results from this study expand our knowledge base on the potential economic implications of future climate change while also providing additional evidence on the role of stress in influencing harassment and discrimination at work.

The third chapter, “The Limits of Anti-Discrimination Workplace Policies” examines the impact of expanding specific policies already in place to combat discrimination. In particular, it studies the efficacy of increasing the use of grievance procedures and training, which are among the most common practices used to reduce workplace discrimination and harassment and protect firms against punitive damages in associated lawsuits. To understand the effect of grievance procedures, I use variation from a policy change that moved EEO reporting from the phone to online. This policy change led to a large increase in sex-based complaints in areas with greater access to broadband. Yet, I observe no commensurate change in sex gaps related to turnover, hiring, and promotions. To understand the effect of training, I combine data on the timing and location of USPS rights and remedies-based discrimination training with data on EEO charges

brought forward by USPS workers. I find no evidence of changes in the number of EEO incidents or reports following training. My analyses thus suggest that increasing the use of training or grievance procedures is unlikely to have large effects on workplace outcomes.

Taken together, my dissertation research shows that organizational design factors related to workplace stress, like pay and working conditions, can have important effects on experienced harassment and discrimination, while the additional use of policies commonly used to tackle the issue, like training or grievance procedures, may have limited impact.

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# **1. The Impact of Financial Stress on Workplace Harassment and Discrimination**

## **I.1. Introduction**

In 1993, the St. Petersburg Times covered a series of shootings that occurred at United States Postal Service (USPS) offices, writing that “the U.S. Postal Service ... has seen so many outbursts that in some circles excessive stress is known as ‘going postal’” (Steinmetz 2010). Today, the phrase “going postal” continues to evoke a strong image of how stress can influence a loss of control and aggression, and it applies to a variety of settings where stress can cause individuals to act out in unwanted ways. In this paper, I aim to answer the question: how does financial stress influence the propensity of individuals to commit acts of harassment and discrimination?

My analysis uses information from the high-pressure workplace that inspired the term “going postal”: the USPS. The USPS is the fourth largest employer in the United States, with roughly half a million employees working in tens of thousands of post office facilities across the United States. The vast size and scope of the USPS allow me to exploit variation in financial stress across space and time. In addition, as an independent agency of the federal government, the USPS must comply with the Freedom of Information Act (FOIA) and release personnel data – with privacy protected fields redacted – on all Equal Employment Opportunity (EEO) charges brought forward by its workers, allowing me to access data that is typically unattainable from private-sector firms.

Through FOIA requests, I have obtained data on roughly 250,000 EEO complaints bringing more than 800,000 charges of harassment and other forms of discrimination at more

than 12,000 USPS offices from fiscal years 2004 through 2019.<sup>1</sup> These data contain detailed information about each alleged incident such as the date the incident occurred, the date it was reported, and various characteristics of the incident. With these data fields, I can isolate changes in the timing of the actual incidents rather than their reporting.

I seek to understand how the financial stress of employees affects EEO incidents at the USPS. Financial stress impacts the lives of many Americans. Data collected by the Federal Reserve show that in 2018, about 40 percent of adults stated they would not be able to cover a \$400 unexpected expense with cash (Board of Governors of the Federal Reserve System 2019). Another study experimentally finds that Americans with household incomes of up to \$70,000 experience declines in cognitive capacity when responding to difficult financial problems (Mani et al. 2013). In 2019, 95 percent of USPS workers earned less than \$70,000 and that figure includes the majority of managers and supervisors (DataUniverse 2020). With modest incomes, many USPS employees may experience financial stress that could, in turn, affect the prevalence of EEO incidents.

To isolate the role of financial stress, I study the financial stress that workers experience over the course of a fixed two-week pay period at the USPS rather than the financial stress that varies with macroeconomic conditions to avoid confounding factors like competitive pressure faced by firms to treat workers equally, the number of opportunities to engage in discriminatory or harassing behaviors, or incentives to report unwanted actions (Becker 1957; Black and Brainerd 2004; Boulware and Kuttner 2019; Dahl and Knepper 2020; Donohue and Siegelman 2005; Siegelman and Donohue 1993). Intuitively, my empirical strategy assumes that the Monday following the receipt of the paycheck looks no different from the Monday preceding the

---

<sup>1</sup> These numbers translate to roughly 3 complaints for every 100 workers per year.

pay date, outside of the financial stress channel. The methodology is akin to that used by Evans and Moore (2011) to analyze the effect of the twice monthly military pay cycle on mortality, and it is similar in spirit to the methods used in studies examining the effect of the monthly government benefit cycle on various outcomes, including crime (e.g., Hastings and Washington 2010; Shapiro 2005; Carr and Packham 2021; Schnepel and Abdelrahman 2021; Watson, Guettabi and Reimer 2020).

Consistent with a hypothesized increase in financial stress during the second week of the pay cycle, my baseline results show that incidents of harassment and discrimination rise by about 5 percent in the second week of the pay cycle as compared with the first week. The number of incidents falls sharply just after employees receive their paychecks, in line with the existing literature on pay and government benefit cycles (Castner and Henke 2011; Evans and Moore 2011; Hamrick and Andrews 2016; Wilde and Ranney 2000), and then exhibits a steady drop-off. The finding is also in accordance with a small experimental literature showing marked improvements in decision-making and productivity immediately following paycheck receipt (Carvalho, Meier, and Wang 2016; Kaur et al. 2022).

My conclusions are robust across multiple specifications. In addition, I provide a series of results suggesting that the findings reflect changes in discriminatory and harassing behaviors rather than changes in their reporting. I show that the estimated effect on the number of incidents is largely unchanged when restricting to incidents reported more than three weeks after the incident date. Most important is that the impact based on the timing of the *incidents* is not replicated when using the timing of the *reports*. The timing of reports appears to have no robust relationship with the pay cycle, my measure of financial stress. Moreover, I observe significant effects of stress for both “formal” and “informal” complaints, where formality is a rough proxy

for how marginal the complaint is. More marginal complaints would more likely be resolved at the initial informal stage via mediation or counseling, while more serious complaints would more likely progress to the formal stage with an investigative process. If stressed workers changed their views on whether certain interactions were reportable for harassment or discrimination, these changes would likely occur for incidents just on the margin of being reported. The result that both “formal” and “informal” complaints rise with financial stress is consistent with an increase in incidents rather than an increase in reporting, where the effect would be loaded on more marginal informal complaints.

My findings thus suggest that greater financial stress increases incidents of harassment and discrimination, in line with concurrent research finding a similar impact of heat stress (Narayan 2022a). A direct implication for managers and policy makers is that stress reduction policies may have the added benefit of decreasing the prevalence of these unwanted employee behaviors. Additionally, the substantial impact of stress on workplace harassment and discrimination may have broader implications for our understanding of why these behaviors occur. The findings from this study can help policy makers, social scientists, and managers better identify the circumstances under which the labor market operates efficiently and fairly.

## **1.2. Theoretical Motivation and Contribution**

Research suggests that a wide range of factors, including demographics, workplace climate, and power structures, may contribute to harassment and discrimination in the workplace (Bergman et al. 2002; Bergman et al. 2012; Folke and Rickne 2022; Goldman et al. 2007; Harned et al. 2002; McCord et al. 2018; Neall and Tuckey 2014; Willness, Steel, and Lee 2002). Yet the role of stress remains underexplored. Studies show that emotional cues, like stress, can

influence behavior in a wide range of relevant settings (Card and Dahl 2011; Cesur and Sabia 2016; Loewenstein 2000).

A well-established literature illustrates how financial stress, in particular, can influence individuals. Research finds that scarcity changes how people allocate attention, causes them to focus on pressing needs, influences the content of cognition, and alters their perceptions of race (Krosch and Amodio 2014; Shah, Mullainathan, and Shafir 2012; Shah, Shafir, and Mullainathan 2015; Shah et al. 2018). A related literature, discussed in the introduction of the paper, shows that when individuals have not recently received money and finances are low, they change their consumption, decision-making, productivity, and time-use (Castner and Henke 2011; Carvalho, Evans and Moore 2011; Hamrick and Andrews 2016; Kaur et al. 2022; Meier, and Wang 2016; Wilde and Ranney 2000). These studies illustrate the myriad ways through which financial stress can influence emotions and cognitive functioning.

When employees have impaired emotions and cognitive functioning, they may fail to self-regulate. Inzlicht et al. (2021) define self-regulation as the dynamic process of determining a desired end state and taking action to move toward it while monitoring progress along the way. The interaction between conflict, emotions, and/or cognitive functioning forms the basis of many self-regulation theories (Inzlicht et al. 2021), though it is outside of the scope of this paper to comment on specific theories of self-regulation. The overall concept of self-regulation failure helps explain why impaired emotions and cognitive functioning can reduce worker' adherence to the laws, rules, and norms that seek to limit harassment and discrimination.

Self-regulation failure is at the core of a related body of work that studies how other forms of stress influence other forms of abusive supervision.<sup>2</sup> These papers show that life stress

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<sup>2</sup> These studies use the framework of ego depletion theory, which underpins one model of self-regulation (Tepper et al. 2017). Ego depletion theory argues that exertion in self-regulation can lead to self-regulation impairment and, in



that stems from a lack of quality sleep, limited exercise, or family-work conflict job can correlate with abusive supervision, as can on-the-job stress that stems from the content and difficulty of one's job (Barnes et al. 2015; Burton, Hooper, and Scheuer 2012; Collins and Jackson 2015; Courtright et al. 2016; Lin, Ma, and Johnson 2016; Mawritz, Folger, and Latham 2014). My research builds upon this literature and extends it two important ways.

One key contribution of my work is highlighting that organizational design can play an inadvertent role in triggering stress. I show that pay practices that contribute to financial scarcity can have significant effects on workplace misconduct. These practices can be altered by management, unlike other the life stressors studied in the literature, and they represent mutable characteristics of jobs that extend beyond the specific tasks required for given roles. My research therefore brings to light an important new mechanism through which management practices can influence misbehavior in the workplace. I demonstrate that managers' decisions related to organizational design might influence workers' overall emotional state and in turn workplace misbehavior. This novel pathway broadens our understanding of why misbehavior at work occurs.

In addition, this paper provides a contribution in terms of the type of workplace misconduct it examines. While workplace misbehavior is an interesting outcome on its own, it is particularly valuable to examine mistreatment that is targeted at individuals due to their protected classes in the forms of harassment and discrimination. This type of misconduct is often directed at individuals from historically disadvantaged backgrounds and can have significant negative impact on those who experience it. Research shows that individuals who experience workplace

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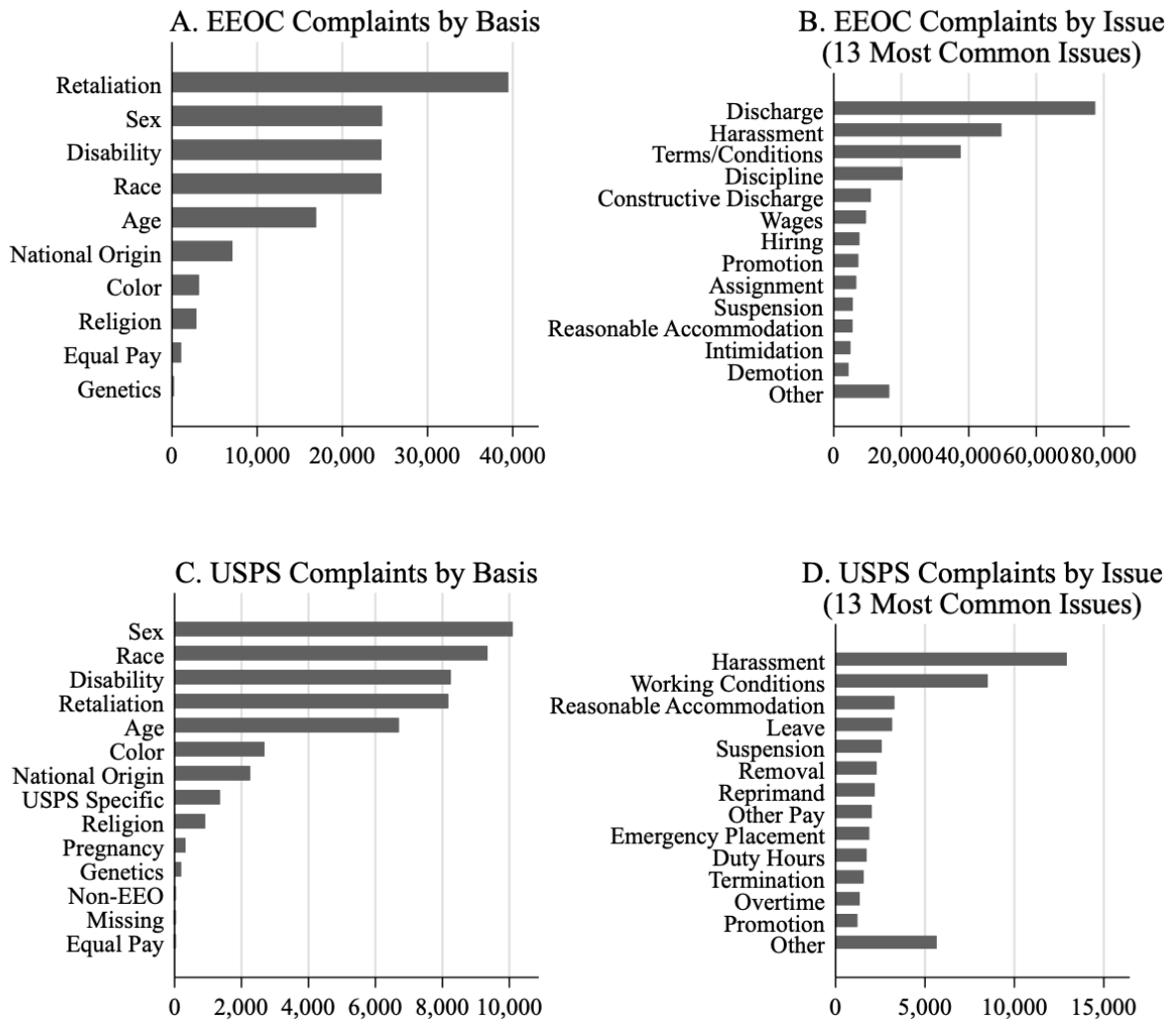
turn, to regulatory failure. The theory overall has received mixed empirical support (Inzlicht and Berkman 2015; Inzlicht et al. 2021), but the idea that stress influences self-regulation and abusive supervision nevertheless has some support in this literature.

harassment and discrimination report reduced levels of physical health, mental health, job satisfaction, work commitment, retention, and productivity (Bergman et al. 2002; Bergman et al. 2012; Folke and Rickne 2022; Goldman et al. 2007; Harned et al. 2002; Willness, Steel, and Lee 2002). Building our understanding of the circumstances under which harassment and discrimination occurs can help us reduce these behaviors and their negative impacts in the future.

### **1.3. Background on EEO Reporting**

In the United States, federal law makes it illegal to discriminate against a job applicant or an employee because of that person's race, color, religion, sex, national origin, age, disability status, or genetic information. It is also illegal to discriminate against a person in retaliation for their involvement in protected EEO activity. The U.S. Equal Employment Opportunity Commission (EEOC) is the governing body responsible for enforcing these laws. Workers who believe they have experienced discrimination on any of the bases listed above can file a complaint with the EEOC (EEOC 2020a). In fiscal year 2018, individuals brought forward 76,416 complaints covering issues spanning all types of work situations, including harassment, hiring, firing, promotions, training, wages, and benefits. Each complaint could contain multiple concurrent charges of discrimination. Figure 1-1 Panels A and B plot out the number of charges filed nationally at the EEOC (excluding those from the federal government workforce) in fiscal year 2018 by their basis and issue. The most prevalent bases of complaints filed at the EEOC were retaliation, sex, disability, race, and age. The most common issues related to discipline, harassment, and working conditions (EEOC 2020b).

**Figure 1-1 Number of EEOC and USPS Charges by Basis and Issue in FY 2018  
(Complaints Often Have Multiple Bases and Issues)**



Notes: Data on national EEOC charges were obtained from the EEOC. Data on USPS charges were obtained from the USPS.

Although the EEOC is perhaps the best-known body for reporting workplace harassment and other forms of discrimination, it is not the only option for workers who have experienced these unwanted behaviors. Individuals can also raise complaints to state or local Fair Employment Practices Agencies. These agencies often offer additional protections for workers and may have more flexible reporting policies (EEOC 2020c). Outside of formal legal channels at the federal, state, and local levels, workers can also bring forward complaints at their firms in

an attempt to resolve the issues internally. Firms typically have grievance procedures in place to reduce legal risk, though the specifics of the policies can vary (Dobbin and Kalev 2019; Dobbin and Kelly 2007).

Because of the diversity in reporting structures that allow workers to raise complaints at the firm, local, state, and federal levels and the different criteria for reporting at each of those levels, it is usually difficult to uncover a standard universe of reported incidents from a workplace. However, within the federal government, the EEOC directly provides leadership and guidance on all aspects of agencies' and departments' EEO programs. As a result, harassment and discrimination reporting policies at the USPS are consistent for all workers across both informal and formal complaint processes. In the dataset I have received, the same data fields are available for complaints resolved at the informal stage and those that are indicated to have progressed to the formal stage.

At the USPS, all complaints begin informally and are initiated as part of the pre-complaint process for formal complaints. To start the process, workers submit a written complaint to the Postal Service Equal Employment Opportunity Office, generally within 45 days of the alleged incident in order to comply with USPS EEO policy. The complaint can include alleged incidents that occurred on the bases and issues listed above as illegal types of discrimination under federal law. In line with general EEO complaints being filed against employers, USPS complaints can only be filed against managers and supervisors. These complaints can be in response to unwanted actions by coworkers that supervisors have inadequately addressed, and supervisors can themselves raise complaints against other supervisors. After USPS employees submit their complaint, they have the option to go through mediation or EEO counseling to help resolve the issue. Following this stage of the process, they

have the opportunity to withdraw or settle their complaint, or they can file a formal complaint. Once a formal complaint is filed, the USPS will begin an investigative process to determine whether any wrongdoing occurred and take corrective action (USPS 2018).

The USPS carefully tracks all EEO complaints – both informal and formal – to comply with the “No FEAR Act of 2002.” As part of the act’s requirements, agencies must publicly post quarterly statistics on EEO complaints, including data on the number of complaints filed, the bases and issues alleged in the complaints, the average length of time it takes to complete certain stages of the complaint process, as well as a handful of other complaint characteristics. USPS must release the data it has collected on EEO complaints, with privacy protected fields redacted, to comply with federal FOIA rules.<sup>3</sup>

The data I obtained via FOIA requests contain detailed information about each incident like the date the incident occurred, the date the incident was reported, the date the incident was closed, a numeric office identifier for where the incident occurred, whether it was resolved at the informal stage or the formal stage, and the basis and issue of each incident. They cover reports that were filed between fiscal years 2004 and most of 2019. Because my data were compiled on September 11, 2019, I restrict my sample to incidents that occurred before July 28, 2019, relying on the statutory 45-day deadline before which 70 percent of incidents in my data are reported. This sample restriction helps to make sure that I am only including days for which I am confident that most incidents have been reported. I likewise restrict my data to only include incidents that occurred after the start of the fiscal year on October 1, 2003.

Figure 1-1 Panels C and D summarize the bases and issues of the charges brought forward at USPS in fiscal year 2018, the most recent fiscal year with complete data. The trends

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<sup>3</sup> These privacy protections prevent me from observing data fields like the name of the complainant or alleged offender.

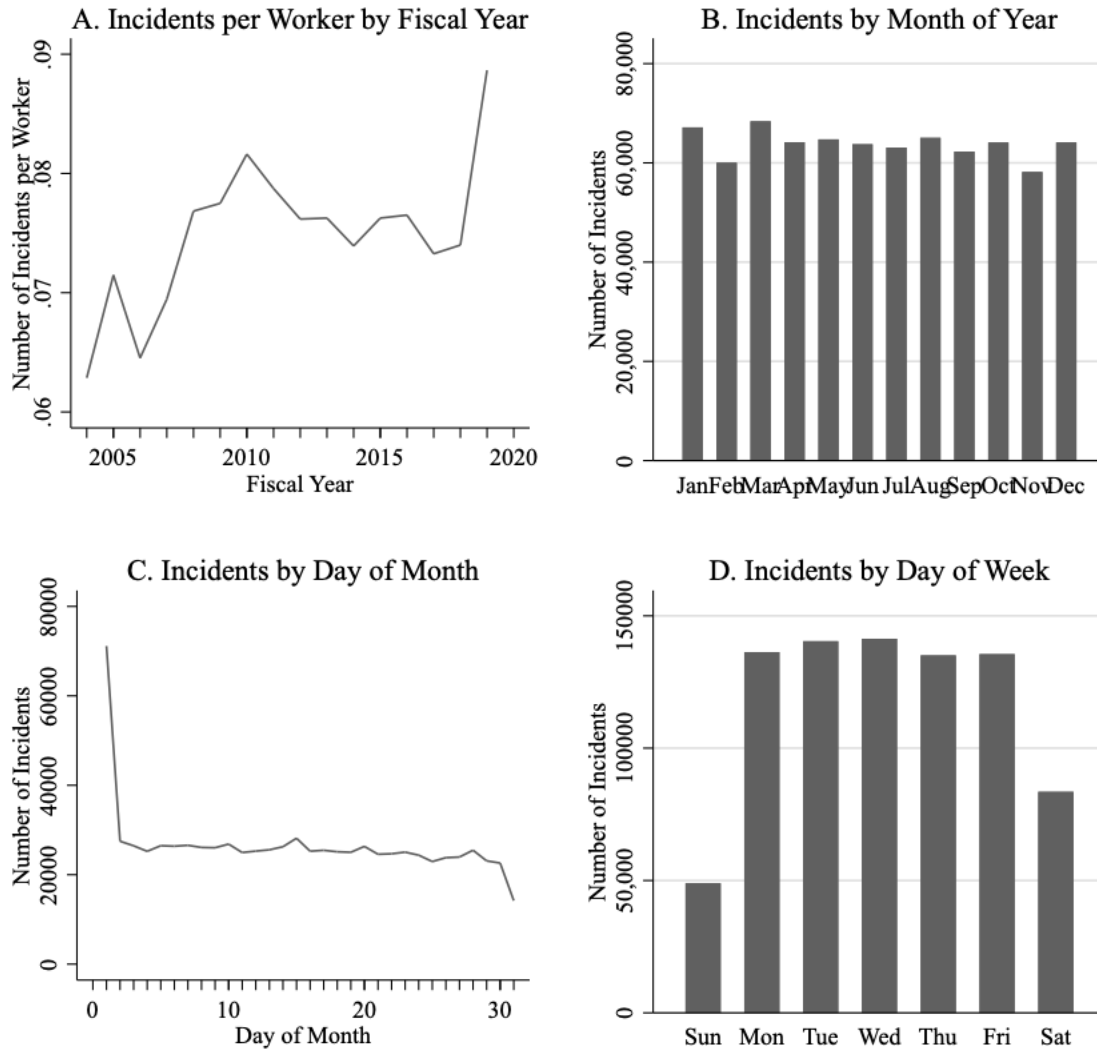
in these panels largely track those in Panels A and B that describe complaints brought forward to the EEOC, which do not include complaints raised at federal agencies like the USPS. Complaints on the bases of sex, race, retaliation, disability, and age continue to be the most common. Issues related to discipline, harassment, and working conditions are likewise reported most often both at the USPS and the EEOC, though the more specific issues tend to differ. The baseline regressions I estimate in the following sections of the paper include all of the listed bases and issues. I explore heterogeneity by basis and issue in robustness exercises.

#### **1.4. Empirical Strategy**

To identify the effect of financial stress, I exploit variation in stress that USPS employees experience over the pay cycle as their cash on hand lessens, either due to imperfect consumption smoothing or due to unexpected financial shocks. My empirical strategy is analogous to that used in Evans and Moore's (2011) analysis of twice monthly military pay cycles on mortality. USPS workers are paid bi-weekly on every other Friday. Publicly posted pay schedules allow me to determine the dates when workers receive these pay checks. Four pay dates in my sample frame were moved back by a day due to holidays. My pay cycle analyses use a restricted sample that excludes these pay periods, though the results are not sensitive to this choice. I combine the paycheck disbursement dates with EEO data on incident dates to understand the relationship between financial stress and harassment and discrimination. My prediction is that EEO incidents rise in the second week of the pay cycle when financial stress is greater.

To provide some information on the general timing of incidents, Figure 1-2 shows how incidents vary by fiscal year, month, day of the month, and day of the week. Panel A shows the number of incidents that occurred in a given fiscal year divided by the total number of career and

**Figure 1-2 Timing of USPS Incidents**



Notes: Data for FY 2019 are incomplete. Incidents by month exclude FY 2019.

non-career USPS employees employed during that year using employment statistics provided in annual USPS 10-K reports. The panel shows that incidents per worker rose between 2004 and 2008 and then moved in a somewhat procyclical pattern until 2019, when the number of incidents experienced a sharp increase due to a change in reporting procedures from phone reporting to online reporting.

Panel B shows limited variation in the number of incidents by the month of the year, with January, and March having relatively more complaints and February and November having

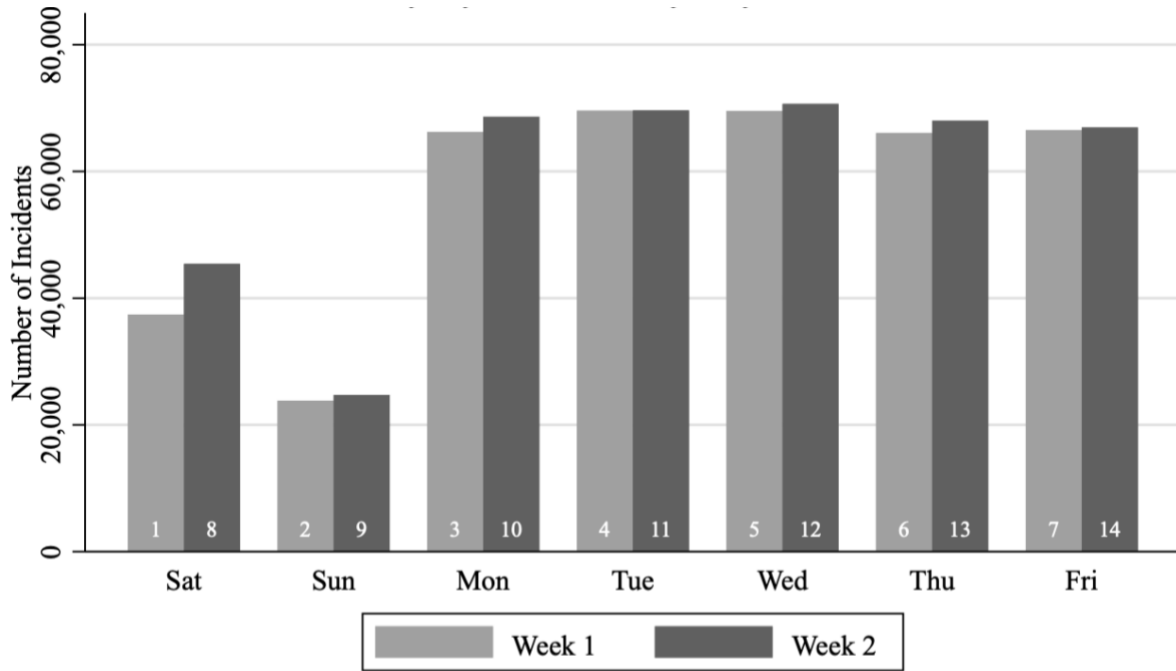
relatively fewer. In Panel C, we also see little variation in the number of incidents by the day of the month, with the exception of incidents on the first of the month. Because the excess mass of complaints occurring on the first of the month occurs for nearly every month in my sample, it seems that this spike reflects a coding of the first of the month for workers uncertain about the precise incident date. Incidents exhibit a sharp pattern over the days of the week in Panel D, with much fewer incidents occurring on Saturdays and Sundays when USPS employees are less likely to work.

The day of the week patterns are clear when I plot the number of incidents by the day of the pay cycle in Figure 1-3. In this figure and in the analyses that follow, I define the pay cycle to begin on the Saturday after pay checks are disbursed. Figure 1-3 provides evidence of a pay cycle effect. It shows a lower number of incidents on each day of the week in the first week of the pay cycle. There is an especially notable decline on Saturdays immediately after workers receive their paychecks, which is followed by a relatively stable decrease that persists through the end of the week and is significant even when the weekend effect is excluded.

The large decrease in incidents on the first day of the pay cycle might be consistent with the existing literature which shows sharp changes in behavior immediately following military pay dates (Evans and Moore 2011), pay dates in experimental studies (Carvalho, Meier, and Wang 2016; Kaur et al. 2022), and food stamp disbursement (Castner and Henke 2011; Hamrick and Andrews 2016; Wilde and Ranney 2000). The behavior changes in consumption, decision-making, productivity, and time-use found in this literature could also be a mechanism for my findings. In addition, the large decrease on Saturday could reflect some interaction between stress and EEO incidents that is unique to Saturdays, perhaps due to the mix of individuals who are working or the lack of extra eyes from other workers to hold individuals accountable. The



**Figure 1-3 Pay Cycle Pattern by Day of Week**



Notes: Sample contains over 800,000 incidents of alleged harassment and discrimination that occurred at USPS between FY 2004 and 2019. Cycles for four moved holiday pay dates are removed. Bar labels in white denote day of pay cycle.

available data do not allow me to distinguish between the possible explanations for the pattern I observe, so I allow for all of these possible pathways in the analyses that follow.

To formally estimate the effect of the pay cycle, I incorporate the heterogeneity in incident timing documented in Figures 1-2 and 1-3 by including fiscal year, month of the year, day of the month, and day of the week fixed effects, along with holiday fixed effects, in a regression. My primary treatment variable in the regression is an indicator that equals one when the incident date falls in the second week of the pay cycle. The regression I estimate can be characterized by the following equation:

$$(1.1) \ln(I_{dwm_y}) = \beta \text{week2} + \gamma_d + \theta_w + \lambda_m + \tau_y + \eta_{dmy} + \epsilon_{dwm_y}$$

where  $\ln(I_{dwm_y})$  is the log of the number of total incidents  $I$  that took place at USPS on the day of the month  $d$ , day of the week  $w$ , month of the year  $m$ , and fiscal year  $y$ ; week2 represents the

indicator for whether the incident occurred during the second week of the pay cycle ; and  $\beta$  stands for the coefficient of interest, approximating the percent change in the number of incidents associated with the second week of the pay cycle. The variables  $\gamma_d$ ,  $\theta_w$ ,  $\lambda_m$ ,  $\tau_y$  , and  $\eta_{dmy}$  represent the day of the month, day of the week, month of the year, fiscal year, and federal holiday fixed effects, respectively. These fixed effects help account for the daily, seasonal, and annual patterns of EEO incidents observed in Figure 1-2.  $\epsilon_{dwm_y}$  represents the unobserved determinants of the number of incidents occurring on a given day. I cluster my standard errors by week, the unit of variation in my treatment variable. Here, weeks are defined to start on Saturday and end on Friday to align with pay cycle timing.

## 1.5. Results

The first column of Table 1-1 presents the baseline results for the above specification, showing that USPS workers in the second week of the pay cycle experience a 4.94 percent increase in incidents compared with the first week of the pay cycle. This increase is significant at the 1 percent level. The subsequent columns make modifications to the baseline regression, but the findings are essentially unchanged. Column 2 adopts the same specification but reports standard errors clustered by pay period, a potentially more conservative unit of clustering. The result remains statistically significant.

I include in my sample only one incident per complaint in Column 3. In my baseline specification, it is possible that the same incident is counted multiple times if it can be classified as multiple types of discrimination. For example, if a black female employee experiences a single harassment outburst targeted at both her race and sex, she could file two separate charges in her complaint. Because I cannot distinguish between this scenario and one in which the

employee experiences recurring and separate outbursts, I opt to keep all charges in my baseline model. If I include only one charge per complaint and assign the charge to the earliest incident date reported in the complaint, my result remains almost identical to that obtained in the baseline regression.

Column 4 adopts the baseline specification but uses the number of incidents, rather than the log of incidents, as the outcome variable. Given an average number of 142 incidents per day, the coefficient in Column 4 suggests that the second week of the pay cycle sees a 3.4 percent increase in incidents as compared with the first week of the pay cycle, fairly similar to the baseline estimate.

The last four columns of Table 1-1 explore how heterogeneity in incident timing shown in Figure 1-2 affects the results. Column 5 excludes incidents taking place on the first of the month given the imperfect reporting for these dates suggested in Figure 1-2, Panel C, and the estimate remains little changed. Column 6 excludes incidents from November 2018 onward given the changes from phone to online reporting that led to an increase in charges visible in Panel A of Figure 1-2 and may have affected the timing and likelihood of submitting a complaint. The estimate remains unchanged. Column 7 excludes all fixed effects. Again there is limited impact on the findings. Because there is mechanically little correlation between the week of the pay cycle and the year, month, or day of the incident, this result is reassuring.

One might also wonder whether the impact of the pay cycle is significant if the large weekend effect is removed. To address this question, Column 8 includes only weekdays and continues to find a significant result, though the effect size drops by about half. Appendix Table A-1 conducts a similar exercise and estimates the baseline regression dropping one day of the week in each column. The effect estimated when excluding Saturdays is similar to that obtained

**Table 1-1 Effect of Pay Cycle on Incidents of Harassment and Discrimination**

	Baseline Results (1)	Cluster by Pay Period (2)	One Charge per Complaint (3)	Outcome not Logged (4)	Drop First of Month (5)	Drop Change in Reporting (6)	Drop All Fixed Effects (7)	Weekdays Only (8)
Coefficient on Week 2	0.0494*** (0.00680)	0.0494*** (0.00610)	0.0506*** (0.00696)	4.803*** (0.869)	0.0499*** (0.00660)	0.0482*** (0.00665)	0.0537*** (0.0118)	0.0167*** (0.00668)
Observations	5,720	5,720	5,720	5,720	5,532	5,358	5,720	4,084
R-Squared	0.804	0.804	0.821	0.813	0.797	0.808	0.003	0.655
Day of Week Fixed Effects	X	X	X	X	X	X	X	X
Day of Month Fixed Effects	X	X	X	X	X	X	X	X
Month of Year Fixed Effects	X	X	X	X	X	X	X	X
Fiscal Year Fixed Effects	X	X	X	X	X	X	X	X
Holiday Fixed Effects	X	X	X	X	X	X	X	X

Results

Specification Details

Notes: Unit of observation is nation-by-day. Sample contains over 800,000 incidents of alleged harassment and discrimination that occurred at USPS between FY 2004 and 2019. Cycles for four moved holiday pay dates are removed. Standard errors (in parentheses) are clustered on week except for Column 2 where they are clustered on pay period. \*\*\*p<.01, \*\*p<.05, \*p<.10.

when only looking at weekdays. A specification that includes two week 2 indicators, one for the weekend and one for weekdays, shows a significant increase in incidents for both treatment variables. The appendix table also shows that the baseline results are largely unchanged if I exclude Fridays or if I define the pay cycle to begin on Friday rather than Saturday, suggesting that the main finding is not sensitive to my choice of assigning the Friday of paycheck receipt to the second week of the pay cycle.

Appendix Table A-2 explores whether the uncovered effect is sensitive to the timing of pay disbursement. Column 1 presents the baseline results. In Column 2, I show that the result is the same when I include the four pay cycles which were moved by a day due to holidays falling on the Friday of paycheck disbursement. In Columns 3 and 4, I examine whether the effect differs for pay cycles with disbursement dates falling in the first half of the month versus the second half of the month. I find that the result is similar when comparing the first week in the two-week cycle to the second week, irrespective of when in the month the first week occurred.

Altogether the pattern of my results do not seem sensitive to the specification I adopt, but one concern could be that the pay cycle effect that I am observing is not reflecting a change in worker stress but instead a change in worker hours that coincides with the timing of the paycheck. In Appendix Table A-3, I analyze the relationship between the pay cycle, work hours, and EEO incidents for a restricted set of dates with available data on work hours.<sup>4</sup> The baseline results with the full and restricted samples are similar. The pay cycle appears to have a negligible effect on hours worked or number of employees working in the restricted sample, and scaling EEO incidents by hours worked or number of workers continues to yield estimates suggesting a statistically significant effect of about 5 percent in both cases. In Appendix Table A-4, I conduct

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<sup>4</sup> USPS only maintains daily records on the number of employees who work on a given day and the number of hours those employees work that day from January 20, 2018 onward.

a similar exercise but scale the EEO incidents by two commonly used USPS productivity metrics for which weekly data are available from fiscal year 2015 onward: the Distribution Productivity Index (DPI) and the Total Deliveries Per Hour (TDPH). Neither metric appears to covary with the pay cycle, and thus the results using scaled incidents remain unchanged.

As a final robustness check, I examine the effect of financial stress on union grievances, a second measure capturing workplace misconduct, in Appendix Table A-5. Most USPS workers are unionized, with the primary exception being managers and supervisors who cannot unionize under federal labor law. Unionized workers can file grievances on any dispute related to their conditions of employment, covering similar issues as under EEO policy but without a requirement for an allegation of discrimination. Even though grievances and EEO complaints can be filed concurrently, these dual submissions make up only a small share of union grievances.<sup>5</sup> Appendix Table A-5 thus looks at a very similar but generally distinct measure of misbehavior and replicates the primary findings of my EEO complaint analysis. It shows that union grievances filed by USPS workers increase by a statistically significant 4 percent in the second week of the pay cycle compared with the first week.<sup>6</sup>

Taken together, the analyses described above show that EEO incidents rise at in the second week of the pay cycle and that this result is not sensitive to the regression specification I adopt. The next set of analyses aim to more carefully explore whether my results reflect changes in the actual number of incidents that occurred or simply changes in reporting behavior. Even though the results presented above use variation in the incident date identified by the

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<sup>5</sup> Between 2004 and 2019, USPS workers filed roughly 3.2 million union grievances, about four times the number of EEO charges and more than ten times the number of unique EEO complaints filed over a similar time period.

<sup>6</sup> The table also replicates the EEO analyses showing that the baseline findings are driven by changes in the number of incidents rather than changes in reporting. The patterns for grievances are more prominent when looking at the timing of the incidents rather than the timing of the reports, and the effects are present for both easily resolved issues and those that progress to the more advanced stages of the grievance process.

complainant, the incident date and reporting date are closely linked. About a third of all incidents are reported within a week of when they occur.

In Table 1-2, I look explicitly at how financial stress relates to the timing of incidents compared with the timing of reports. Column 1 of the table repeats the baseline model using the date of the incident for reference. The analysis presented in Column 2 seeks to understand whether the baseline finding reflects changes in EEO processing capacity that may occur in the second week of the pay cycle. It uses the same regression specification as Column 1 but assigns each incident to the date the EEO office deemed it to be closed rather than the date it occurred. The small, statistically insignificant result demonstrates that it is unlikely that the increase in incidents is simply reflecting a rise in EEO processing, as this would also affect the number of charges closed on a given date.

**Table 1-2 Effect of Pay Cycle on Incidents or Reporting of Harassment and Discrimination**

	Baseline Results	Effect on Closed Date	Effect on Reports	3+ Weeks Reports	3+ Weeks Incidents
	(1)	(2)	(3)	(4)	(5)
	<u>Results</u>				
Coefficient on Week 2	0.0494*** (0.00680)	-0.0147 (0.0198)	0.0239* (0.0126)	0.0120 (0.0149)	0.0587*** (0.00898)
Observations	5,720	5,547	5,710	5,627	5,720
R-Squared	0.804	0.845	0.770	0.696	0.615
	<u>Specification Details</u>				
Day of Week Fixed Effects	X	X	X	X	X
Day of Month Fixed Effects	X	X	X	X	X
Month of Year Fixed Effects	X	X	X	X	X
Fiscal Year Fixed Effects	X	X	X	X	X
Holiday Fixed Effects	X	X	X	X	X

Notes: Unit of observation is nation-by-day. Sample contains over 800,000 incidents of alleged harassment and discrimination that occurred at USPS between FY 2004 and 2019. Cycles for four moved holiday pay dates are removed. Standard errors (in parentheses) are clustered on week. \*\*\*p<.01, \*\*p<.05, \*p<.10.

Column 3 does the same exercise but assigns each incident to the date the worker contacted the USPS EEO office about that incident. Here, the coefficient on week 2 is much smaller than the baseline estimate and less significant. Because a large share of incidents are

reported soon after they occur, to even more cleanly distinguish between the effect of financial stress on the number of incidents versus their reporting, I restrict my sample to the 50 percent of incidents that took at least three weeks for the worker to report. For this subset of observations, Column 5 continues to show no statistically significant relationship between the pay cycle and the reporting date. Meanwhile, Column 6 shows that the baseline result is largely unchanged when restricting to incidents that were reported three or more weeks later.

If financial stress were primarily influencing workers' choices to report rather than the prevalence of the underlying incidents, we would see bigger effects when assigning incidents to their report date, and we would observe smaller impacts when using the baseline model for incidents that took longer to report. Yet this is not the pattern of results presented in Table 1-2. Changes in reporting propensities thus do not appear to be driving the effects uncovered in my baseline estimates.

Even though Table 1-2 suggests no direct effect of stress on reporting, it could still be that workers perceive situations as harassing and discriminatory during times of stress in ways that they otherwise would not. In order for this hypothesis to be consistent with the results in Table 1-2, it would have to be that changes in worker perceptions during times of stress continue to color their perceptions of those situations weeks later. Although this seems unlikely, I cannot directly test for whether or not this is the case.

Instead, I provide suggestive evidence against a story of perception changes by examining whether the effect of financial stress is concentrated among more marginal cases. If in response to stressful situations, workers changed their perceptions of behaviors or their desire to report unwanted actions, we would expect that these changes would occur for the situations in which the workers were just on the margin of reporting the actions. These more marginal cases



would likely be easier to resolve at the informal stage of the complaint process, and we would expect to see the effect of the pay cycle loaded on these types of incidents. The results in Table 1-3 show this is not the case. Incidents that were reported in complaints that were ultimately formalized also saw similar effects of financial stress. The findings are thus not consistent with my baseline estimates reflecting strong selection effects in reporting.

**Table 1-3 Effect of Pay Cycle on Incidents of Harassment and Discrimination by Complaint Formality**

	Informal (1)	Formal (2)
	<u>Results</u>	
Coefficient on Week 2	0.0634*** (0.00863)	0.0425*** (0.0137)
Observations	5,720	5,697
R-Squared	0.797	0.621
	<u>Specification Details</u>	
Day of Week Fixed Effects	X	X
Day of Month Fixed Effects	X	X
Month of Year Fixed Effects	X	X
Fiscal Year Fixed Effects	X	X
Holiday Fixed Effects	X	X

Notes: Unit of observation is nation-by-day. Sample contains over 800,000 incidents of alleged harassment and discrimination that occurred at USPS between FY 2004 and 2019. Cycles for four moved holiday pay dates are removed. Standard errors (in parentheses) are clustered on week. \*\*\*p<.01, \*\*p<.05, \*p<.10.

Finally, I consider heterogeneity in the characteristics of the incidents that occur. I begin by looking at geographic heterogeneity in Appendix Table A-6. I compare the average county-level USPS pay derived from publicly posted payroll rosters (DataUniverse 2020) to the average county-level overall pay from the Quarterly Census of Employment and Wages (QCEW). I find that the pay cycle effect is minimal in areas where USPS pay is relatively high for the local labor market. This finding is consistent with a smaller effect in areas where USPS employee’s relative cost of living is lower.

Next, in Appendix Table A-7, I examine whether there is any heterogeneity in the effect of stress by basis type. For example, are charges on the basis of sex more responsive to financial stress than charges on the basis of race? I observe similar effects sizes for the five most common bases of sex, race, disability, retaliation, and age, consistent with no ex-ante hypothesized heterogeneity by basis type. And in Appendix Table A-8, I examine whether harassment and non-harassment incidents are affected by financial stress. I find that harassment and non-harassment incidents both increase with financial stress.

## **1.6. Conclusion**

In this paper, I show that financial stress can increase incidents of workplace harassment and discrimination, and my findings are robust across a variety of regression specifications. Although my analyses rely on data that only contain incidents that were ultimately reported, I use detailed information on the timing of the incidents and the timing of the reports along with data fields describing the characteristics of the incidents to show that the effects I'm uncovering are most likely in response to changes in actual behavior rather than changes in reporting. The findings are consistent with the idea that financial stress may lead to regulatory failure in workplace interactions. My study builds upon existing theory by showing that organizational design can contribute to stress and associated harassment and discrimination. The result that visceral factors influence harassment and discriminatory behavior stands counter to standard economic models which would suggest that individuals' choices to discriminate should not vary with emotional cues (Becker 1957).

My findings have potential policy relevance. At a narrow level, they suggest that firms can benefit from adopting practices that reduce stress through the added value of reducing the

number of EEO incidents that workers experience. At a broader level, they imply that stressful circumstances could cause labor markets to function in ways that are less fair and efficient and that policy makers may want to more carefully monitor workplace harassment and discrimination when employee stress levels are likely to be high. I show that important, but ultimately relatively modest increases in financial stress may lead to increases in harassing and discriminatory behavior. High stress conditions that stem from events like major economic downturns, natural disasters, or pandemics could spur even larger increases in these behaviors.

More research is needed to understand in detail how different firm practices lower stress and associated instances of harassment and discrimination. Although this study demonstrates that financial stress influences harassment and discrimination, it is unable to identify the mechanisms of regulatory failure that occur with financial stress, how financial stress influences workers in other organizations, or what the expected effect sizes would be for different pay policy changes. Additional research could help uncover the mechanisms behind and potential policy solutions for the effect of financial stress on harassment and discrimination. Better understanding the circumstances under which harassment and discrimination occur and the firm practices that can reduce these unwanted behaviors can help create more equitable and inclusive work environments in the future.

## **2. The Impact of Extreme Heat on Workplace Harassment and Discrimination**

### **2.1. Introduction**

A broad literature studies the relationship between environmental factors and economic outcomes, building our knowledge base on the potential economic implications of future climate change (Dell, Jones, and Olken 2014). One growing strand of this literature examines how temperature affects labor productivity. Observational, quasi-experimental, and experimental studies show that extreme heat can lower labor supply hours, reduce worker productivity, increase injury rates, and influence decision-making on the job (e.g., Adhvaryu et al. 2020; Cachon, Galliano, and Olivares 2012; Dillender 2021; Graff Zivin and Neidell 2014; Heal and Park 2016; Heyes and Saberian 2019; Niemela et al. 2002; Park, Pankratz, and Behrer 2021; Somanathan et al. 2021; Sudarshan and Tewari 2013). However, despite new evidence on the increasing importance of interpersonal interactions in the modern labor market (Deming 2017), there is limited research examining how heat stress influences workplace interactions. This paper fills the gap in the literature by studying the effect of extreme heat on workplace harassment and discrimination.

It builds upon an emerging body of work that explores inequality in workplace environmental exposure by directly examining how discrimination and harassment charges respond to extreme heat (Park and Stainier 2021; Park, Pankratz, and Behrer 2021). Studies shows that individuals who experience workplace harassment and discrimination have lower reported levels of physical health, mental health, job satisfaction, work commitment, retention, and productivity (Bergman et al. 2002; Bergman et al. 2012; Folke and Rickne 2022; Goldman et al. 2007; Harned et al. 2002; Willness, Steel, and Lee 2002). Yet researchers have made little

progress on understanding how working conditions like extreme heat affect discrimination and harassment, in part due to data limitations. To get around the issue, this study uses privacy protected data obtained via the Freedom of Information Act (FOIA) on Equal Employment Opportunity (EEO) charges brought forward by public sector workers at the US Postal Service (USPS).

The data contain detailed information on 250,000 EEO complaints bringing over 800,000 charges of harassment and discrimination at more than 12,000 USPS offices from fiscal years 2004 through 2019.<sup>7</sup> I geocode the roughly 12,000 post office identifiers in my dataset and merge these with localized daily weather data provided by the National Oceanic and Atmospheric Administration's Daily Global Historical Climatology Network (NOAA GCHN). A data field that identifies the date each EEO charge occurred allows me to relate the timing of EEO incidents with the timing of heat stress. The variation across space and over time in EEO incidents and heat stress forms the basis of my empirical strategy.

In my preferred empirical strategy, I group temperatures above 50 degrees in 10-degree bins and include controls for precipitation and snowfall along with fixed effects for the office of the incident, week of the year, fiscal year, day of the week, and holidays. I find that extremely hot days with temperatures exceeding 90 degrees experience roughly 5 percent more EEO incidents than cooler days when the highs are between 60-70 degrees. Days with pleasant weather between 50-60 and 70-80 degrees do not experience the same increase in incidents. My findings are robust across multiple specifications. Heterogeneity analyses show that both indoor

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<sup>7</sup> These numbers translate to roughly 3 complaints for every 100 workers per year. The USPS is the fourth largest employer in the United States, with roughly half a million employees working at tens of thousands of post office facilities.

and outdoor USPS workers are affected by heat stress and that direct effects of extreme heat on working conditions do not explain the observed results.

I also perform a series of analyses that suggest the results reflect a change in the underlying number of incidents rather than a change in their reporting. I begin by contrasting my baseline results which study heat stress on the date the incident occurred with an examination of heat stress on the date the incident was reported. I show that the patterns I observe based on the timing of the incidents are not replicated when using the timing of the reports. The timing of reports appears to have no relationship with the timing of extreme heat. In addition, I observe significant effects of heat stress for both “formal” and “informal” complaints, with formality roughly proxying for how marginal the complaint is. This result is consistent with heat stress contributing to an increase in incidents rather than a change in reporting since the uncovered effect is not loaded on marginal informal complaints.

Taken together, my findings show that heat stress can increase incidents of workplace harassment and discrimination. The results complement concurrent work showing that financial stress likewise raises EEO incidents at the USPS (Narayan 2022b), and the findings are consistent with a literature examining the effect of extreme heat on decision making in non-work settings (Cheema and Patrick 2012; Park 2020; Park, Hurwitz, and Smith 2020). This paper is also closely related to the literature examining the relationship between climate and aggression (Behrer and Bolotnyy 2022; Heilmann, Kahn, and Tang 2021; Hsiang, Burke, and Miguel 2013; Larrick et al. 2011; Muckerjee and Sanders 2021). My research builds upon this literature, which largely documents how violent crime and conflict rise at high temperatures, by demonstrating that heat-induced aggression can emerge in more ordinary contexts. I examine instances of misconduct that typically do not rise to the level of a criminal report or visible violence, and my

finding that both informal and formal complaints rise in response to heat stress further suggests that incidents of a range of severity respond to high temperatures. In addition, my analysis sheds light on the incidence of heat-induced outbursts, showing that high temperatures can increase mistreatment directed at often disadvantaged members of our society.

My study provides some of the first causal evidence on how environmental conditions can influence workplace inequality via harassment and discrimination. The results suggest that policies that make environmental working conditions less stressful may have the added benefit of reducing the prevalence of labor market discrimination and associated workplace inequality.

## **2.2. Materials and Methods**

### **2.2.1. EEO Data**

My study relies on data obtained via FOIA requests on EEO harassment and discrimination charges brought forward by USPS workers. As part of the federal government, the USPS EEO program is governed by federal law.<sup>8</sup> Federal law makes it illegal to discriminate against a job applicant or an employee because of that person's race, color, religion, sex, national origin, age, disability status, or genetic information. It is also illegal to discriminate against a person in retaliation for their involvement in protected EEO activity. All types of work situations are covered under the law and are categorized into issue types. Appendix Figure B-1 Panels A and B summarize the bases and issues of the EEO charges brought forward by USPS workers in fiscal year 2018. Complaints on the bases of sex, race, retaliation, disability, and age and on issues related to discipline, harassment, and working conditions were the most common.

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<sup>8</sup> This stands in contrast private sector discrimination and harassment reporting which is governed by various policies at the firm, local, state, and federal levels.

To raise an EEO complaint at the USPS, workers submit a written complaint to the Postal Service Equal Employment Opportunity Office, generally within 45 days of the alleged incident in order to comply with USPS EEO policy. USPS EEO complaints can only be filed against managers and supervisors. These complaints can be in response to unwanted actions by coworkers that supervisors have inadequately addressed, and supervisors can themselves raise complaints against other supervisors. After USPS employees submit their complaint, they have the option to go through mediation or EEO counseling to help resolve the issue. They can then withdraw or settle their complaint, or they can file a formal complaint. After a formal complaint is filed, the USPS begins an investigative process to determine whether any wrongdoing has occurred and take corrective action (USPS 2018).

The data I obtained via FOIA requests contain detailed information on all informal and formal charges covering every basis and issue brought forward between fiscal years 2004 and most of 2019.<sup>9</sup> I observe roughly 250,000 EEO complaints carrying over 800,000 charges of harassment and discrimination at more than 12,000 offices during this time frame. These EEO incidents serve as my primary outcome variable in the analyses that follow.<sup>10</sup>

### **2.2.2. Estimating the Effect of Extreme Heat**

To assess the effect of heat stress on EEO incidents, I begin by exploring the relationship between the number of incidents on a given day in a given state and the daily maximum temperature in that state. Figure 2-1 Panel A shows binned scatter plot of this relationship,

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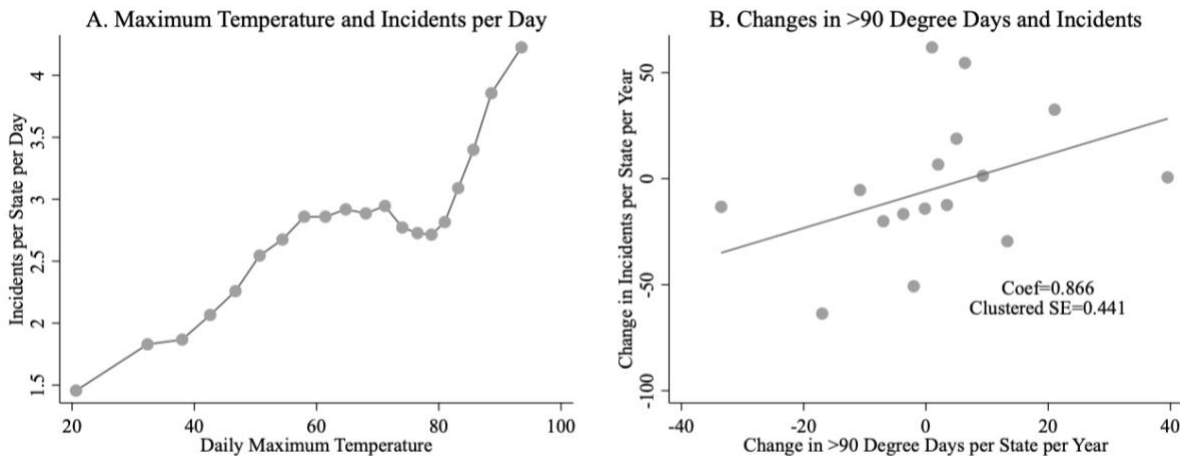
<sup>9</sup> Because my data were compiled on September 11, 2019, I restrict my sample to incidents that occurred before July 28, 2019, relying on the statutory 45-day deadline before which 70 percent of incidents in my data are reported. This sample restriction helps to make sure that I am only including days for which I am confident that most incidents have been reported. I likewise restrict my data to only include incidents that occurred after the start of the fiscal year on October 1, 2003.

<sup>10</sup> For more information on the USPS EEO data and reporting process, see (19).



displaying a sharp increase in daily EEO incidents after temperatures exceed 80 degrees Fahrenheit, with particularly high incident counts above 90 degrees. However, part of this relationship could be due to the mix of states that have hotter (or colder) temperatures on average. Figure 2-1 Panel B therefore shows a binned scatter plot using annual differences in the number of days when daily maximum temperatures exceeded 90 degrees. After differencing out state specific attributes that contribute to the prevalence of EEO incidents, I continue to observe a positive relationship between incidents of harassment and discrimination and hot weather.

**Figure 2-1 Binned Scatters of Relationship Between Temperature and Incidents of Harassment and Discrimination**



Notes: Unit of observation is state-by-day for the 50 states in Panel A and state-by-year in Panel B. Data span fiscal years 2004 through 2018. Maximum temperature is derived from NOAA's GHCN data.

To more precisely measure the effect of heat stress on EEO incidents, I use information provided in my dataset on the office where each incident occurred. The EEO incidents reported in my dataset come from over 12,000 USPS offices across the country. For 95 percent of the numeric office identifiers in my dataset, I could obtain information on the zip code of the facility from the USPS. I impute the zip code for the remaining 5 percent using the zip code of the nearest numeric office identifier, as office identifiers appear to be assigned somewhat

geographically. Altogether, I am able to associate the EEO incidents in my dataset with over 10,000 zip codes across the country. Then, using the latitude and longitude associated with the center point of each zip code, I match each office with the nearest weather station in NOAA's GHCN database. I restrict my search to the 4,798 weather stations in the United States that report data for at least 95 percent of the days in my sample frame, in line with Park et al. (2020). On average, the nearest weather station is about 6 miles away from the center point of the zip code associated with the office of the incident.

For my baseline specification, I use a Poisson pseudo-maximum likelihood estimation of the following equation:

$$(2.1) \ln(I_{osdwy}) = \sum_{\omega \neq 60-70} \beta_{\omega} temp_{\omega s d w y} + \phi X_{s d w y} + \alpha_o + \theta_d + \lambda_w + \tau_y + \eta_{d w y} + \epsilon_{osdwy}$$

where  $I_{osdwy}$  is the number of total incidents  $I$  that took place at USPS in office  $o$ , associated with weather station  $s$ , on the day of the week  $d$ , week of the year  $w$ , and fiscal year  $y$ .

$temp_{\omega s d w y}$  is an indicator equal to one if the maximum temperature at weather station  $s$  falls within temperature band  $\omega$  on a given day. I include five temperature bands in my baseline specification: less than 50 degrees, 50 to 60 degrees, 70 to 80 degrees, 80 to 90 degrees, and 90 degrees plus. My omitted category contains temperatures between 60 and 70 degrees. Because the outcome variable is logged,  $\beta_{\omega}$  approximately captures the percent change in the number of incidents associated with temperature band  $\omega$  as compared with the omitted category of  $\omega = 60 - 70$ . My coefficient of interest will be  $\beta_{\omega=>90}$ , in line with the recent literature examining the effects of extreme heat in the United States (e.g., Cachon, Galliano, and Olivares 2012; Deschenes and Greenstone 2011; Park 2020; Park et al. 2020; Ranson 2014). I incorporate precipitation and snowfall controls for a given day at weather station  $s$  in  $\phi X_{s d w y}$ . The variables  $\alpha_o$ ,  $\theta_d$ ,  $\lambda_w$ ,  $\tau_y$ , and  $\eta_{d w y}$  represent office, day of the week, week of the year, fiscal year, and

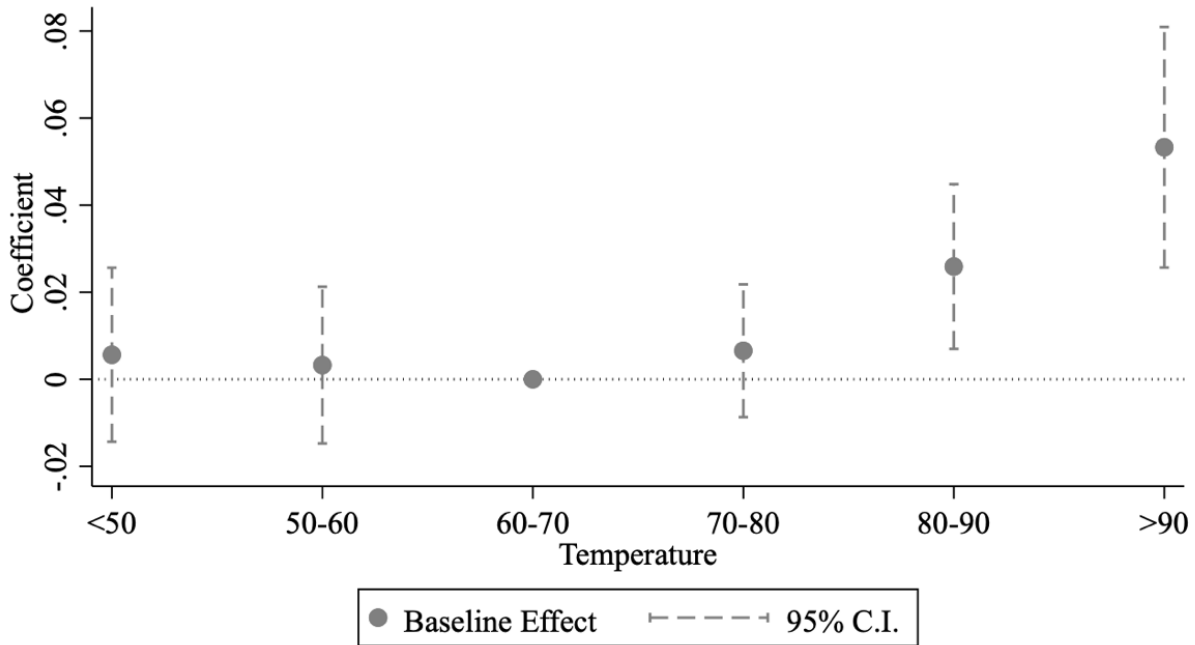
federal holiday fixed effects, respectively.  $\epsilon_{osdwy}$  represents the unobserved determinants of the number of incidents occurring on a given day. I cluster my standard errors on weather station, the unit at which the treatment varies, as in Park et al. (2020), unless otherwise noted.

## **2.3. Results**

### **2.3.1. Baseline Results**

Figure 2-2 presents the results for this baseline regression, showing a statistically significant 5.3 percent increase in incidents when the daily maximum temperature is greater than 90 degrees. It also shows a smaller 2.6 percent increase in incidents when maximum temperatures fall between 80 and 90 degrees. The effect sizes for the cooler temperature bands are all small and statistically insignificant in comparison to the omitted 60 to 70 degree band. The corresponding point estimates and standard errors are provided in Column 1 of Appendix Table B-1.

**Figure 2-2 Effect of Heat on Incidents of Harassment and Discrimination**



Notes: Unit of observation is office-by-day. Baseline sample contains over 800,000 incidents of alleged harassment and discrimination that occurred at USPS between FY 2004 and 2019. Maximum temperature is derived from NOAA's GHCN data. Standard errors are clustered on weather station. Dashed lines represent 95% confidence intervals.

The subsequent columns in Appendix Table B-1 show that the baseline result is robust to changes in the regression specification. In Column 2, I re-estimate my baseline regression with my standard errors clustered by state rather than weather station. The standard errors remain similar to those when I cluster by weather station. The regression in Column 3 modifies the baseline specification by adopting state by week of the year fixed effects in place of the national week of the year fixed effects. Here, the estimated effects for extreme heat are largely unchanged, though I now observe some decline in incidents at temperatures below 50 degrees. The specification in Column 4 includes only one weather band for when temperatures exceed 90 degrees and continues to find an increase in incidents compared to when temperatures are below 90 degrees. The magnitude of the effect is smaller, as might be expected with the 80- to 90-degree band included in the omitted category. In Column 5, I drop the precipitation controls, and

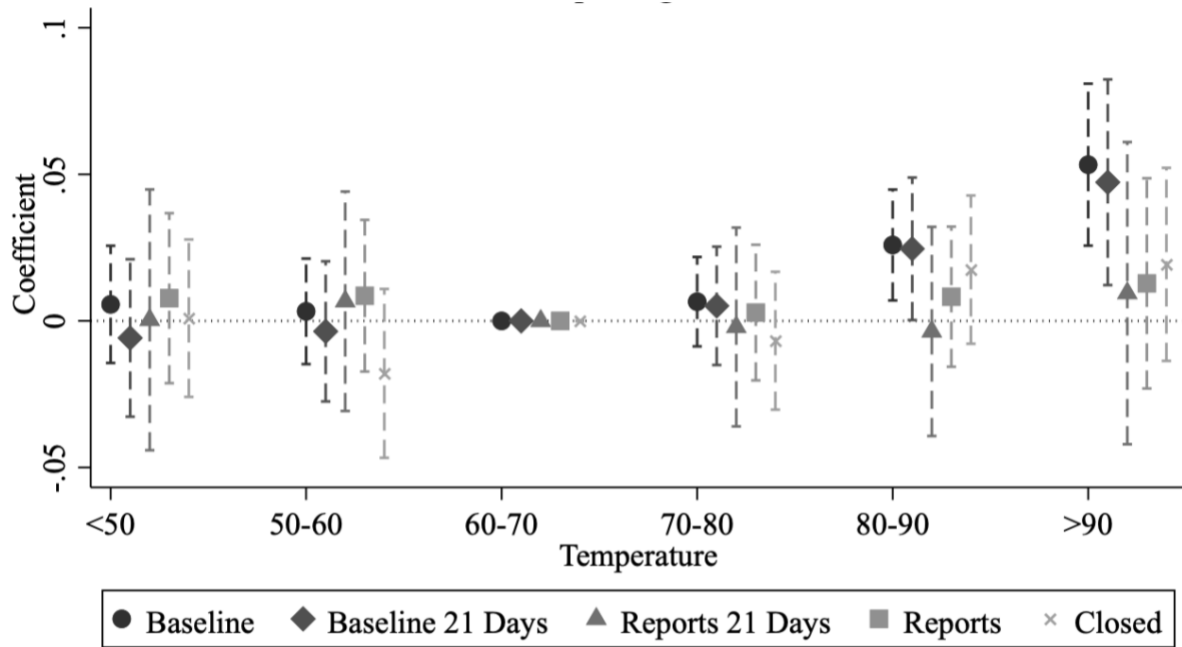
my results remain essentially unchanged, consistent with the fact that precipitation appears to have no relationship with my outcome variable.

I include in my sample only one incident per complaint in Column 6. In my baseline specification, it is possible that the same incident is counted multiple times if it can be classified as multiple types of discrimination. For example, if an older female employee experiences a single harassment event targeted at both her age and sex, she could file two separate charges in her complaint. Because I cannot distinguish between this scenario and one in which the employee experiences separate discriminatory or harassing events, I choose to keep all charges in my baseline model. If I include only one charge per complaint and assign the charge to the earliest incident date reported in the complaint, my results remain nearly identical to the baseline.

### **2.3.2. Incidents vs. Reporting**

The analyses described above show that EEO incidents rise at very high temperatures and that this result is not sensitive to the regression specification I use. The next set of analyses aim to more carefully explore whether my results reflect changes in the number of incidents that occurred or changes in reporting behavior. The results presented above use temperature variation for the incident date identified by the complainant, but the incident date and reporting date are closely linked. About 10 percent of incidents are reported on the same day the incident occurred, and a third of all incidents are reported within a week.

**Figure 2-3 Effect of Heat on Incidents or Reporting of Harassment and Discrimination**



Notes: Unit of observation is office-by-day. Baseline sample contains over 800,000 incidents of alleged harassment and discrimination that occurred at USPS between FY 2004 and 2019. Maximum temperature is derived from NOAA's GHCN data. Standard errors are clustered on weather station. Dashed lines represent 95% confidence intervals.

In Figure 2-3, I look specifically at how heat stress relates to the timing of incidents compared with the timing of reports. The point estimates and standard errors associated with the figure are available in Appendix Table B-2. The first set of lines in Figure 2-3 marked with dark gray circles repeats the baseline specification. In the second set of slightly lighter gray lines marked with a diamond, I restrict my sample to the 50 percent of incidents that took at least three weeks for the worker to report. For this subset of observations with distinct incident and reporting dates, the baseline result continues to show a sharp increase in incidents at high temperatures. The third set of gray lines marked with a triangle uses the same subset of reports but assigns each incident to the date the worker contacted the USPS EEO office about that incident rather than the date the worker said the incident occurred. Here, the coefficient on the greater than 90 degrees indicator is small and statistically insignificant. The subsequent fourth

line marked with a square uses the full sample and the same exercise of assigning each incident to the date it was reported. Again, I observe no increase in reports with temperature, and a 95 percent confidence interval would rule out the effect size obtained in the baseline estimate.

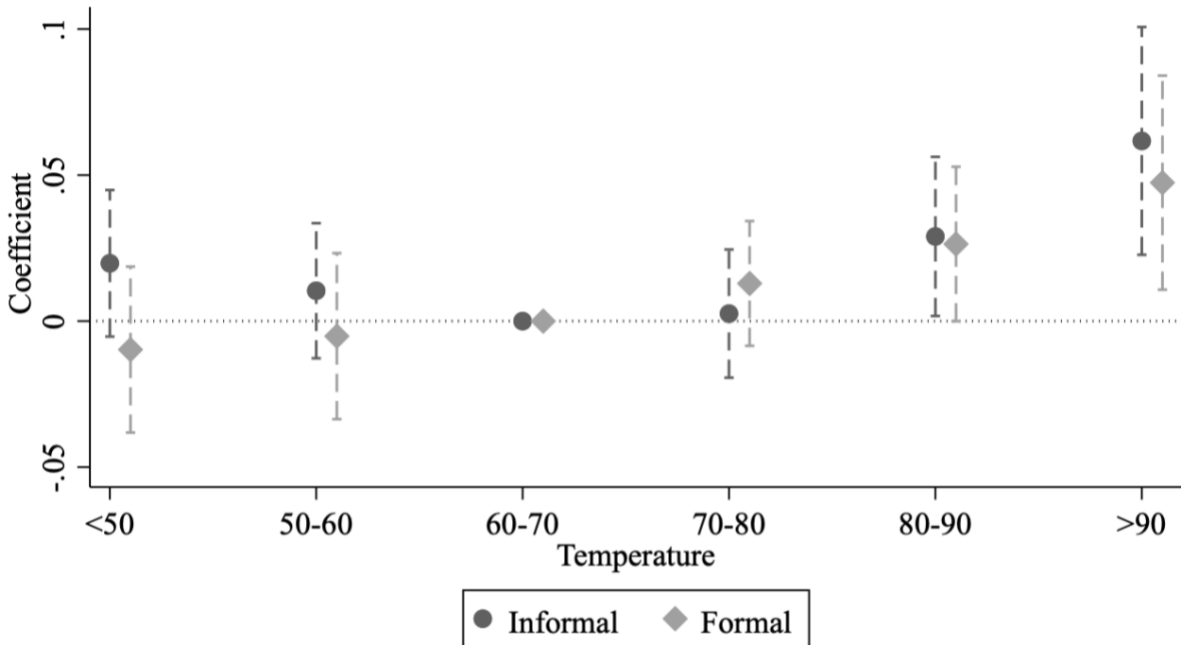
In the final light gray line marked with an X, I seek to understand whether the baseline findings reflect changes in EEO processing capacity that occur on hot weather days. The specification is akin to the baseline specification but assigns each incident to the date the EEO office deemed it to be closed rather than the date it occurred. The small, statistically insignificant result demonstrates that it is unlikely my findings are capturing a rise in EEO processing.

The pattern of results in Figure 2-3 is not consistent with heat stress leading to an increase in EEO reporting. If this were the case, we would expect to see larger effect sizes when assigning incidents to their report date and smaller effect sizes when using the baseline model for incidents that took longer to report. Even so, it is still possible that workers perceive otherwise similar situations as more harassing and discriminatory during times of stress and that my estimates are reflecting this change in perceptions. For this hypothesis to be consistent with the results in Figure 2-3, the changes in worker perceptions during times of stress would need to continue to influence their perceptions of those situations weeks later. Although this seems unlikely, it is not possible to directly test whether this is the case.

Instead, I provide suggestive evidence against a story of perception changes by showing that the effect of heat stress is not concentrated among more marginal cases. If workers change whether they consider certain interactions harassing or discriminatory when stressed, we expect that these changes would occur for incidents just on the margin of being reported. These more marginal cases would likely be easier to resolve at the informal stage of the complaint process, so we would predict that the effect of extreme heat would be concentrated on informal incidents.

The results in Figure 2-4 and the corresponding Appendix Table B-3 show this is not the case. Incidents that were reported in complaints that were ultimately formalized also saw similar effects of high temperatures.

**Figure 2-4 Effect of Heat on Harassment and Discrimination by Complaint Formality**



Notes: Unit of observation is office-by-day. Baseline sample contains over 800,000 incidents of alleged harassment and discrimination that occurred at USPS between FY 2004 and 2019. Maximum temperature is derived from NOAA's GHCN data. Standard errors are clustered on weather station. Dashed lines represent 95% confidence intervals.

### 2.3.3. Robustness of Findings

Appendix Tables B-4 and B-5 show that the primary findings in the above tables and figures are robust to alternate models and datasets. In Appendix Table B-4, I adopt an ordinary least squares regression rather than a Poisson regression. I keep the same fixed effects, controls, and standard error clustering level as in my baseline specification. I provide outcome means and scaled results for ease of interpretation. Very hot temperatures above 90 degrees continue to



increase the number of EEO incidents by roughly 5 percent but do not appear to influence reporting. In each case the results are not sensitive to the functional form of the regression.

As second robustness check, I examine the effect of heat stress on union grievances, an alternate measure of workplace misconduct, in Appendix Table B-5. Most USPS workers are unionized, and unionized workers can file grievances on any dispute related to their conditions of employment, covering similar issues as under EEO policy but without a requirement for an allegation of discrimination. Although grievances and EEO complaints may be filed concurrently, these dual submissions make up just a small share of union grievances.<sup>11</sup> Appendix Table B-5 thus looks at a similar but distinct measure of misconduct and replicates the primary findings of the EEO complaint analysis. It shows that union grievances filed by USPS workers increase by a statistically significant 3.6 percent when temperatures exceed 90 degrees. The table also suggests a rise in grievances at cooler temperatures in comparison to 60-to-70-degree days, which differs somewhat from the EEO results. Although it is not obvious what is causing this discrepancy at lower temperatures, it is reassuring that this second measure of workplace misconduct sees an uptick at very high temperatures. The other columns of the table continue to suggest that incidents rather than reports rise with heat stress.

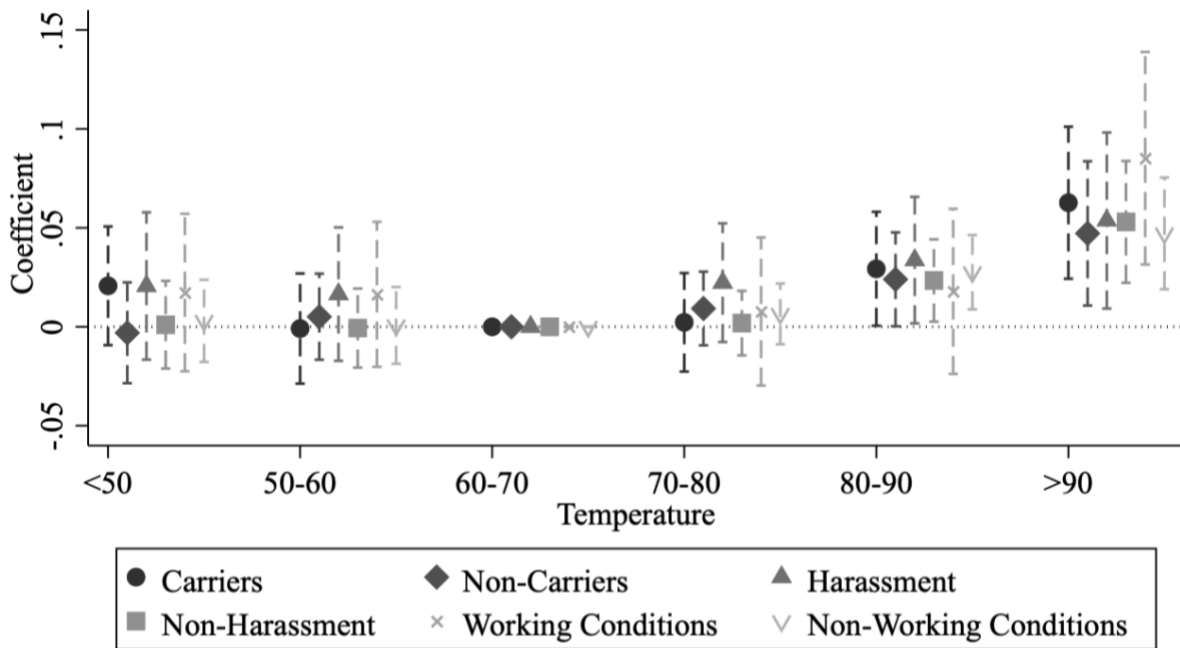
#### **2.3.4. Heterogeneity Analysis**

I next consider heterogeneity in the characteristics of the incidents that occur in Figure 2-5, with corresponding point estimates provided in Appendix Table B-6. In the first two lines of

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<sup>11</sup> Between 2004 and 2019, USPS workers filed roughly 3.2 million union grievances, about four times the number of EEO charges and more than ten times the number of unique EEO complaints filed over a similar time period.

**Figure 2-5 Effect of Heat on Harassment and Discrimination by Occupation or Issue Type**



Notes: Unit of observation is office-by-day. Baseline sample contains over 800,000 incidents of alleged harassment and discrimination that occurred at USPS between FY 2004 and 2019. Maximum temperature is derived from NOAA's GHCN data. Standard errors are clustered on weather station. Dashed lines represent 95% confidence intervals.

Figure 2-5, I examine differences in the effect of heat stress by the complainant's occupation, comparing effects for letter carriers and non-letter carriers. There may be some hypothesized heterogeneity by occupation as letter carriers spend more time outside and thus might be more sensitive to heat stress. However, it is worthwhile to note that EEO complaints can only be raised against supervisors, and supervisors of letter carriers and non-letter carriers may be similarly exposed to the heat. Moreover, letter carriers may have limited interactions with their supervisors during the hottest parts of the day when they are out delivering mail. Both of these factors could potentially dampen the observed amount of heterogeneity by occupation. Indeed, the results in Figure 2-5, marked with a dark gray circle and diamond, show a significant effect of heat stress for both letter carriers and non-letter carriers, respectively. The results suggest that heat stress influences behaviors even among workers not directly exposed to the heat during the workday, in

line with existing research uncovering effects of extreme heat on judges, manufacturing workers, and warehousing employees who work indoors (Behrer and Bolotnyy 2022; Cachon, Gallino, and Olivares 2012; Heyes and Saberian 2019; Park, Pankratz, and Behrer 2021).<sup>12</sup> Still, the effect of heat stress appears to be somewhat larger in magnitude for letter carriers, which could be consistent with greater heat exposure and consequently more EEO incidents among carriers.

To further probe the role of climate adaptation, Appendix Table B-7 explores whether the effect of heat stress varies based on how common extreme heat is in an area. In places where extreme heat is rare, there may be fewer adaptations to the heat available to USPS workers. The effect of heat stress may therefore be higher in these areas. Consistent with this hypothesis, in Appendix Table B-7, I find that the effect of heat stress is roughly twice as large in offices experiencing extreme heat for seven days or less a year as compared to offices in areas with more than seven days of greater than 90-degree weather. However, my estimates are imprecise when disaggregating the data in this way, so I cannot reject that the effect sizes are similar between the two groups. The estimates thus provide suggestive, but not definitive, evidence on the potential role of adaptation in mitigating the effect of extreme heat on discrimination and harassment.

The remaining lines in Figure 2-5 examine heterogeneity by issue type. The third and fourth lines marked by a gray square and triangle show that both harassment and non-harassment incidents are affected by heat stress. The last two light gray lines marked with an X and a V show that discrimination in both working conditions and non-working conditions rise with

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<sup>12</sup> Directly testing the role of air conditioning is not feasible. Nearly all USPS facilities have some form of air conditioning (USPS 1998). Although many USPS trucks do not have air conditioning (Jameel 2019), given the mobile nature of trucks, it is not clear which areas have trucks with air conditioning and which do not. Both of these factors make it challenging to directly study the role air conditioning at the USPS. Residential air conditioning likely also contributes to the heat stress experienced by USPS workers as they arrive at work, but quality local estimates of air conditioning that span the United States are not available.

extreme heat. The point estimate for working conditions reflects an effect of heat stress nearly double of that found at the baseline, as might be expected since extreme heat influences the working environment. Even so, the effect for non-working conditions is similar to the baseline estimate, suggesting that heat stress is influencing misconduct more generally as well.

One might be also curious whether there is any heterogeneity in the effect of heat stress by basis type. For example, are charges on the basis of sex more responsive to extreme heat than charges on the basis of age? Appendix Table B-8 estimates the effect of heat stress for each of the five most common bases of sex, race, disability, retaliation, and age. It shows no substantial heterogeneity across these bases, consistent with no ex-ante hypothesized heterogeneity by basis type. Altogether, the heterogeneity analyses suggest that heat stress impacts harassment and discrimination for a broad range of workers at the USPS.

## **2.4. Conclusion**

In this paper, I show that extreme heat can increase incidents of workplace harassment and discrimination for a range of employees across the USPS. The results are robust across a variety of regression specifications and are consistent with changes in actual behavior rather than changes in reporting. How might the effects observed in this paper translate to other work settings? My study relies on data from the USPS, which is a unique workplace that is large, geographically dispersed, highly unionized, and part of the federal government. These distinctive attributes of the USPS relate to competitive forces and worker and citizen voice, which existing economic theory and empirical evidence show can influence working conditions.

There are two types of competition faced by an employer that might influence employee mistreatment: product market competition and labor market competition. In terms of product

market competition, the USPS is a very large legal monopoly with just Federal Express (FedEx) and United Parcel Service (UPS) as its primary competitors. A longstanding literature has suggested that reduced product market competition can increase discrimination (Becker 1957; Black and Brainerd 2004). Limited competitive forces in the product market might also affect other negative workplace attributes like those that relate to heat stress or harassment. Employers often need to pay higher wages to compensate for negative attributes in order to attract labor (Basu 2003; Folke and Rickne 2022; Brown 1980; Hersch 2011; Rosen 1974). Workplaces with proclivities for harassment or other contributors to poor working conditions may thus be put out of business when there is sufficient competition. Because the USPS product market has limited competition, it may face less competitive pressure than others in addressing issues related to heat stress and discrimination and harassment.

In terms of labor market competition, however, the USPS is unlikely to wield more monopsony power than other types of employers. Monopsony power, which allows workplaces to offer lower wages and worse working conditions, can arise from the concentration of employers, frictions that limit mobility between jobs, and informational asymmetries between workers and firms about working conditions (U.S. Department of Treasury 2022). Because the USPS is geographically dispersed, it does not typically contribute to significant concentration in the local labor market despite its size. Public sector accountability and transparency requirements limit information asymmetries at the USPS, and it does not use non-poach or non-compete agreements that seek to limit mobility. Overall, the limited monopsony power at the USPS might increase the quality of its working conditions.

In addition, the USPS is also a workplace with a high level of worker and citizen voice, which directly pressures it to address the issues I examine in this paper. Studies show that the

USPS has provided its workers with strong rents and has a long history of serving diverse employees (Boustan and Margo 2009; Hirsch, Wachter, and Gillula 2000). USPS unions include specific provisions related to heat stress and harassment and discrimination in bargaining agreements, and the unions repeatedly raise concerns on these topics to USPS leadership (Cothran 2016; Robinson 2020; Szeredy 2022). The USPS thus may be more responsive to its workers' needs than other workplaces that do not share these features. Future research examining different workplaces as well as specific environmental policies would further build our understanding of the environmental determinants of workplace harassment and discrimination.

### **3. The Limits of Popular Anti-Discrimination Workplace Policies**

#### **3.1. Introduction**

Grievance procedures and training are among the most common practices used to combat workplace discrimination and harassment, and they can protect firms against punitive damages in associated lawsuits (Bisom-Rapp 2001; Dobbin and Kalev 2016; Dobbin and Kalev 2019). But is further investing in existing grievance procedures and training effective for combatting discrimination? In this paper, I find limited empirical evidence to support the view that the increased use of these practices has large effects on behavior in a workplace. To carry out this analysis, I use a large and detailed dataset from the United States Postal Service (USPS) that contains information on Equal Opportunity (EEO) grievances reported by workers, anti-discrimination trainings, and career outcomes

I begin by investigating the role that grievance procedures can play in influencing broader workplace outcomes. In theory, the heightened use of grievance procedures could reduce the prevalence of discrimination and harassment by providing accountability, increase misbehavior via backlash as bad actors resent and respond negatively to the heightened attention towards the topic, or have little influence when most workers remain unaffected. The limited causal inference and related research on the topic finds evidence in support of each of these hypotheses. One study examining the effect of EEO litigation on workforce composition finds beneficial impact of accountability (Knight, Dobbin, and Kalev 2022), and other research finds most USPS employees are satisfied with the USPS grievance system (Bingham 2003). Alternatively, two studies show grievance procedures lower the share of minorities and women in management positions (Dobbin and Kalev 2016; Dobbin and Kalev 2019). The findings of these studies are consistent with fears of backlash noted in various news articles following the

rise of sexual harassment reporting with the 2017 #MeToo movement (e.g., Bower 2019; The Economist 2018; Tolentino 2018). Yet other research shows that a small fraction of those who experience harassment and discrimination report the misbehavior (Boudreau et al.; Feldblum and Lipnic 2016), limiting the scope for positive or negative response to reporting changes.

In this study, I seek to understand whether an increase in harassment and discrimination reporting always leads to backlash, whether it can lead to improvements, or whether it has no significant impact. I use variation from a November 2018 policy at the USPS that shifted the reporting format from over the phone and via the mail to primarily online. In response to this policy shift, the number of complaints containing at least one sex-based allegation rose by about 40 percent. The increase was concentrated in counties with above median broadband access. Leveraging data on workplace outcomes by sex from the names and occupations listed in public payroll rosters, I compare high and low broadband access counties before and after the policy change. I find the sex gaps in turnover, hiring, or promotion outcomes remain unchanged despite the sharp increases in sex-based grievance rates. The confidence intervals suggest that a 10 percent increase in sex-based EEO reporting would change the female share in separations or recent hires by less than roughly 1 percent. My findings thus suggest a null effect of EEO grievance reporting on downstream workplace outcomes.

I then turn to analyzing the effect of training, which likewise has a theoretically ambiguous effect on workplace outcomes. The literature often finds a substantial impact of training on individuals' behavior, both positive and negative. For example, Sharma (2021) studies the impact of sexual harassment training in an educational environment in India and finds that training reduces extreme harassment by 100 percent. Devine et al. (2017) observe a roughly 50 percent increase in female hiring in STEM departments at the University of Wisconsin



Madison in the two years following bias training, and Dobbin and Kalev (2019) find that sexual harassment training for managers increases the share of female managers by about 5 percent. Chang et al. (2019) see somewhat more mixed results. They observe that diversity training changes attitudes but not behaviors among less inclusive groups and behaviors but not attitudes among more inclusive groups. The results from Chang et al. (2019) also highlight that the frequently observed null effect of training on learning or attitudinal measures in the literature may not translate to behavioral outcomes (e.g., Bezrukova et al. 2016; Kalinoski et al. 2013; Paluck and Green 2009; Paluck et al. 2021; Roehling and Huang 2020). Conversely, a series of studies finds evidence of backlash, with certain training types reducing the share of diverse individuals in management positions (Kalev, Dobbin, and Kelly 2006; Dobbin and Kalev 2016; Dobbin and Kalev 2019).

My paper contributes to the literature by using rigorous methods to show that training can have limited impact on discriminatory and harassing behavior or reporting. I show that the null effect often observed for knowledge and attitudinal measures can extend to real behavioral outcomes, and my findings are consistent with descriptive survey research by the U.S. Merit Systems Protection Board showing no change in reported harassment among the federal workforce despite an increase in training over time (USMSPB 1995). My empirical strategy compares outcomes for offices that recently had training to offices that did not, controlling for fixed office and timing effects. I focus my analysis on the study of USPS's Notification and Federal Employee Antidiscrimination and Retaliation Act (No FEAR Act) Training which is a rights-and-remedies-based training that has been legally mandated for all USPS employees to take every two years since the early 2000s. The No FEAR Act training I study is not the gold standard training that builds empathy, encourages interactions across groups, discusses how

behaviors can be changed, or occurs in tandem with other organizational changes. However, it is similar to the definitions-based training commonly provided at many workplaces (Gino and Coffman 2021), making it of particular relevance to study.

I find no evidence that No FEAR Act training influences EEO reporting or the underlying prevalence of EEO incidents in my analysis of over 400,000 instances of No FEAR Act training between October 2017 and September 2019. The baseline point estimates suggest training neither increased nor decreased reported EEO complaints in an office by more than roughly 5 percent in the 4 weeks following training, my standard errors rule out potential effects sizes of roughly 15 percent. I am also able to disentangle counteracting effects of training on the number of incidents and reporting by disaggregating the reporting date from the incident date, looking at whether training for supervisors influenced reports of subordinates, and exploring whether retaliation concerns rose in response to the training. Although my estimates from these analyses are somewhat imprecise, the confidence intervals exclude existing estimates from the literature examining how training influences reported misconduct, and when combined with the elasticity estimates from the first part of the paper, the confidence intervals exclude some of the estimates examining broader workplace outcomes.

Overall, the findings of this paper suggest that simply doing more of the same legally motivated grievance and training policies can have limited impact on unwanted discriminatory and harassing behaviors. The findings are specific to the USPS, which is a highly unionized and regulated public-sector workplace. Similar policies might have a different impact in workplaces with fewer protections. The results from this study also may not translate to related but different policies, such as higher quality training or the use of an alternate grievance procedure. Still, this

study highlights that new policies will likely be needed to make progress on the issues of discrimination and harassment in workplaces like the USPS.

### **3.2. Details about the USPS and Its Policies**

The USPS is the fourth largest employer in the United States, with roughly half a million employees working in tens of thousands of post office facilities across the United States. Its large size allows me to examine variation in the use workplace discrimination policies across geographies and over time. In addition, because the USPS is part of the federal government, it must comply with the Freedom of Information Act (FOIA) and release personnel data, with privacy protected fields redacted, allowing me to access data that is typically unattainable from private-sector firms. The variation in the use workplace discrimination policies and the data collected via FOIA requests form the basis of my analysis.

Through FOIA requests, I have obtained data on over 65,000 EEO complaints bringing roughly 125,000 charges of harassment and other forms of discrimination at thousands of USPS offices between 2015 and 2019.<sup>13</sup> USPS EEO complaints can only be filed against managers and supervisors. The USPS EEO data contain detailed characteristics of each complaint such as when it was raised, the office where it originated, the occupation category of the complainant, and the bases and issues of the contained charges. For reference, Appendix Figure C-1 shows the most common bases (sex, race, retaliation, disability, and age) and issues (those related to discipline, harassment, and working conditions) of the EEO charges brought forward by USPS workers in fiscal year 2018.

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<sup>13</sup> Here and throughout the paper, charges refer to individual allegations of discrimination because of race, color, religion, sex, national origin, age, disability status, genetic information, or retaliation. Complaints refer to the collection of charges brought forward by a USPS employee at a given point of time. The numbers referenced above translate to roughly 3 complaints for every 100 workers per year.

The USPS EEO grievance procedure begins with an informal pre-complaint process. USPS workers initiate the process by submitting a written complaint to the Postal Service Equal Employment Opportunity Office, generally within 45 days of the alleged incident in order to comply with USPS EEO policy. Prior to November 2018, workers would receive the requisite paperwork via the mail after calling an automated phone number. Since November 2018, the same form has been completed online.

After submitting the informal complaint, USPS workers can complete mediation or EEO counseling to help resolve the issue. The USPS mediation process uses transformative model of dispute resolution that emphasizes hearing voices from both sides of the dispute and generating dialogue. The USPS model has been carefully studied as an alternative to the standard legalistic grievance procedure, with evidence suggesting it yields satisfactory outcomes for participants over 90 percent of the time (e.g., Bingham et al. 2003; Dobbin and Kalev 2020). At the end of this informal stage, USPS workers can withdraw or settle their complaint, or they can file a formal complaint.

Once a formal complaint is filed, the USPS begins an investigative process to determine whether any wrongdoing occurred and take corrective action (USPS 2018). The data I collected via FOIA requests covers the universe of informal and formal complaints raised by USPS workers. To carry out my research, I combine the EEO data I collected with data on policy changes and broader workplace outcomes.

### **3.3. The Downstream Impact of Grievance Reporting**

This section of the paper asks whether reporting EEO grievances has downstream effects on broader workplace outcomes at the USPS, either positively as intended, negatively via

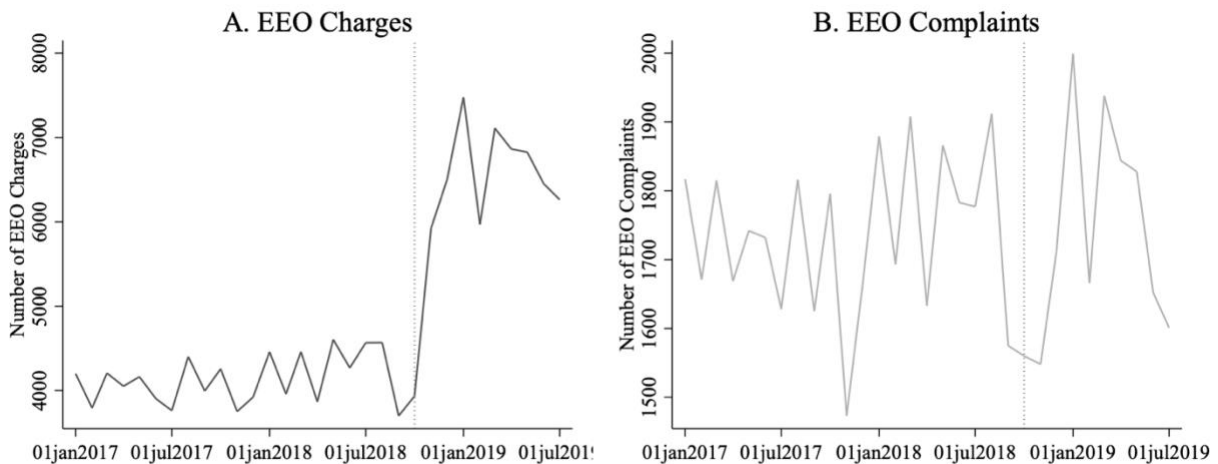
backlash, or not at all due to a limited impact of limited reporting. It focuses on outcomes related to turnover, hiring, and promotions that I construct from publicly posted USPS payroll rosters. These rosters show which employees worked at which post offices on January 1 between 2015 and 2020 (DataUniverse 2020). The rosters do not contain any demographic information but allow me to infer the sex of employees from their first names. I merge this payroll roster data with my dataset on reported USPS EEO grievances and aggregate the data to the county level to facilitate merging between the USPS data and data from the Census I add later.

I begin by examining the cross-sectional relationship between grievance reporting and turnover in Appendix Table C-1. I define turnover as the names on January 1 in year  $y$  and office  $o$  that do not show up in office  $o$  on January 1 of year  $y+1$  divided by the year  $y$  number of names. I compare turnover in base year  $y$  to the number of EEO charges in the first six months of year  $y$ . I restrict my EEO data to the first six months of the year to better capture how current EEO charges relate to future attrition. Column 1 presents estimates for a bivariate regression examining the relationship between EEO charges and turnover. The point estimate suggests that one extra EEO charge per 100 USPS employees is associated with 0.06 additional separations for every 100 USPS employees, a very small null effect. The standard errors clustered by county would rule out effect sizes greater than roughly -0.1 to 0.15 separations for every 100 employees. The result is replicated when adding county and year fixed effects to a panel regression in Column 2. Columns 3 and 4 focus on sex-based charges and their relationship with female turnover as a robustness check. The precise null effect remains.

Appendix Table C-1 thus suggests no relationship between EEO complaints and turnover. This result could occur both if there were a true null effect or if there were offsetting management effects. For example, if good management were associated with higher reporting

rates and lower turnover while bad management led to higher underlying discrimination rates and higher turnover, the two effects could cancel out in the cross section. I therefore seek to more rigorously identify the effect of EEO reporting on worker outcomes by studying a change in USPS EEO policy that theoretically only affected reporting and not underlying manager behavior. In particular, I examine a change from phone to internet EEO reporting that occurred in November 2018. Prior to November 2018, USPS workers had to call an automated phone number after which they would receive paperwork via the mail to complete and return. Since November 2018, workers have submitted the same form online instead.

**Figure 3-1 USPS EEO Charges and Complaints Over Time**

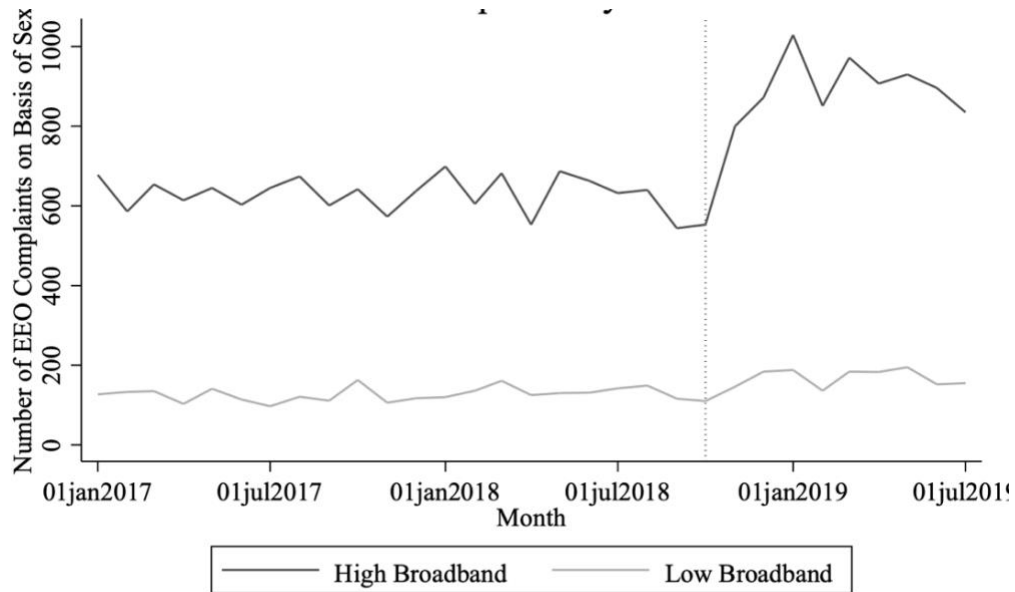


Note: Unit of observation is nation-by-month.

This policy shift held the overall number of complaints fixed but increased the number of allegations in each complaint, as demonstrated in Figure 3-1. Alongside the increase in the number of charges per complaint was a change in the composition of charges included in the complaints, as shown in Appendix Figure C-2. There were more complaints containing at least one charge on the bases of sex and race, and fewer complaints containing a charge on the basis of retaliation. There were also fewer complaints containing a charge on the bases of disability or age, a pattern consistent with older employees engaging less with a more technologically

complex reporting platform. The issues included in the complaints changed as well, with a rise in complaints containing at least one charge with a harassment or disciplinary issue and a drop in complaints containing at least one charge with a working condition issue, for example. I use the change in the composition of charges included in the EEO complaints to identify the effect of EEO grievance reporting on worker outcomes.

**Figure 3-2 EEO Sex-Based Complaints by Broadband Access**



Note: Unit of observation is broadband category-by-month.

Because my outcome variables can only be disaggregated by sex in the payroll rosters, I focus my analysis on the number of complaints that included at least one sex-based charge, a variable which rose by roughly 40 percent in response to the policy change. These complaints drew additional attention to harassment and other forms of discrimination on the basis of sex in the places they were raised as managers had to respond to the sex-based allegations they contained. Some counties were more affected by the reporting policy change from phone to online than others. Notably, I find that counties with above median broadband coverage in the 2015-2019 five-year American Community Survey estimates can account for the entire increase

in complaints containing a sex-based charge while counties with below median coverage see no increase over time, as shown in Figure 3-2.

This pattern is in line with the hypothesis that broadband coverage is correlated with internet use and online reporting, but it is worthwhile to note that the broadband itself may not be responsible for the changes in reporting. As Appendix Figure C-3 and Appendix Table C-2 show, broadband coverage is higher on the East and West coasts of the United States, and it is higher in counties with larger populations, greater shares of college educated adults, smaller unemployment rates, bigger incomes, and lower poverty rates. Still, as long as I can assume broadband coverage is unrelated to other underlying changes in discrimination and harassment reporting trends during this time period, the mechanism through which it influences EEO reporting because of the policy change is irrelevant for my estimation strategy.

The parallel trends in reporting between high and low broadband areas before the policy change in Figure 3-2 support making this assumption, as the two groups follow similar patterns of reporting behavior except for after the policy change. Moreover, there were no other major events or changes in policy that happened around November 2018 when the reporting change came into effect that might affect high and low broadband areas differently. The #MeToo movement reached its height in late 2017, and political events that drew attention to the topic of sexual harassment like President Donald Trump's election or the confirmation of Justice Brett Kavanaugh occurred before November 2018 as well. Appendix Figure C-4 depicts the Google Trends patterns for the search phrases "#MeToo" and "harassment" from 2015 to 2020, showing that the large rise in attention to these terms occurred before November 2018.

I am thus able to compare outcomes for high and low broadband counties before and after the policy change using a differences-in-differences style regression to estimate the effect of



grievance reporting on workplace outcomes. The regression can be characterized by the following equation, with my unit of data at the county-by-year level for the years 2015 to 2019:

$$(3.1) Y_{ot} = \beta \text{post} \times \text{broadband}_{ot} + \theta_o + \lambda_t + X_{ot} + \epsilon_{ot}$$

where  $Y_{ot}$  represents outcome  $Y$  at office  $o$  in year  $t$ ;  $\text{post} \times \text{broadband}_{ot}$  is indicator for broadband coverage interacted with post at office  $o$  in year  $t$ , and  $\beta$  is my coefficient of interest. I use two definitions for broadband coverage. One is a continuous variable that provides more variation and power, and another is a binary variable for above median coverage that may be easier to interpret and potentially less likely to be correlated with omitted variables given its coarseness.  $\theta_o$  and  $\lambda_t$  are office and year fixed effects, respectively, and  $X_{ot}$  represents a quadratic in time varying employment controls.  $\epsilon_{ot}$  represents the unobservable determinates of my outcome variable. I cluster my standard errors on county, the unit of variation in my data.

Table 3-1 shows the effect of broadband coverage on EEO complaints containing at least one sex-based charge. The first two columns use monthly data from 2017 through 2019 to match the data in Figure 3-2, and the last two columns follow the primary specification listed above. In each case, the coefficients represent a large and statistically significant increase in complaints from the baseline mean. The point estimates in Columns 1 and 3 using the binary instrument are easier to interpret and suggest an increase in sex-based complaints of roughly 50 to 60 percent following the policy change, in line with the pattern visible in Figure 3-2. The T-statistic for the estimated coefficient is roughly 6 to 7 depending on the specification, corresponding with an F-statistic of over 30. The strong relationship between the policy shift and sex-based EEO

complaints allows me to use a two-stage least squares (2SLS) research design with the estimates in Columns 3 and 4 of Table 3-1 making up the first stage of the estimation strategy.<sup>14</sup>

**Table 3-1 Relationship Between Sex-Based EEO Complaints and Instrument**

	Binary Broadband Monthly	Continuous Broadband Monthly	Binary Broadband Yearly	Continuous Broadband Yearly
	(1)	(2)	(3)	(4)
<u>Results</u>				
Coefficient on Post Interaction	0.252*** (0.0419) [6.004]	0.0137*** (0.00201) [6.828]	1.702*** (0.282) [6.027]	0.0918*** (0.0129) [7.108]
Observations	55,428	55,428	8,940	8,940
R-Squared	0.767	0.767	0.925	0.925
<u>Specification Details</u>				
County Fixed Effects	X	X	X	X
Time Fixed Effects	X	X	X	X
Broadband Instrument Used	Binary	Continuous	Binary	Continuous
Unit of Observation	County-Month	County-Month	County-Year	County-Year
Data Range	2017-2019	2017-2019	2015-2019	2015-2019
Outcome Variable Mean	0.473	0.473	2.730	2.730

Notes: Unit of observation is noted in table. EEO complaints reflect those filed during the first six months of the year from 2015-2019 in yearly regressions. Standard errors (in parentheses) are clustered on county; corresponding t-statistics in brackets below. \*\*\*p<.01, \*\*p<.05, \*p<.10.

The 2SLS strategy can be characterized by the following set of equations that are analogous to Equation 3.1:

$$(3.2) \widehat{reports}_{ot} = \beta post \times broadband_{ot} + \theta_o + \lambda_t + X_{ot} + \epsilon_{ot}$$

$$(3.3) Y_{ot} = \delta \widehat{reports}_{ot} + \varphi_o + \zeta_t + Z_{ot} + \nu_{ot}$$

with Equation 3.2 representing the first stage that obtains predicted values of sex-based reports and Equation 3.3 representing the second stage that then estimates how the predicted measure of sex-based reports affects the outcomes of interest. In essence, the 2SLS strategy allows me to estimate the effect of grievance reporting using variation that stems only from the change in

<sup>14</sup> Appendix Table A3 shows a similar pattern for EEO complaints on the basis of sex specifically for female workers and for charges of any basis overall.

reporting policy captured in the  $post \times broadband_{ot}$  instrument. The standard errors are adjusted to account for the first stage estimation, so I can use them to bound the likely effect of reporting on my outcomes of interest.

I use this research design to study the effect of sex-based EEO reports on three outcomes related to sex gaps in turnover, hiring, and promotions. The first outcome measure is the female share among separated employees, with separations defined as names on January 1 in year  $y$  and office  $o$  that do not show up in office  $o$  on January 1 of year  $y+1$ . The second outcome measure is the female share among recent hires, with recent hires defined as names on January 1 in year  $y+1$  and office  $o$  that did not show up in office  $o$  on January 1 of year  $y$ . The third outcome measure is the female share among new supervisors, with new supervisors defined as names of supervisors or managers on January 1 in year  $y+1$  and office  $o$  that did not show up as supervisors or managers in office  $o$  on January 1 of year  $y$ .

Table 3-2 presents the 2SLS and reduced form results for these three outcomes for both the binary and continuous broadband instruments, as well as the OLS relationship between sex-based reports and my outcomes. The reduced form estimates show the effect of a one-unit change in the instrument on the outcomes, while the 2SLS and OLS estimates show the effect of a one unit change in sex-based reports on the outcomes. The point estimates are insignificant across all specifications. When scaled by the outcome means for the separations and recent hires outcomes, the 95 percent confidence intervals constructed from the 2SLS estimates generally rule out positive or negative elasticities of 0.1 or larger. In other words, the results would suggest that a 10 percent increase in sex-based EEO reporting would change the female share in separations or recent hires by less than roughly 1 percent.<sup>15</sup> The estimated elasticity range for the

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<sup>15</sup> As context, a 10 percent change in reporting is consistent with the literature examining how the #MeToo movement impacted reporting (Levy and Mattson 2021)

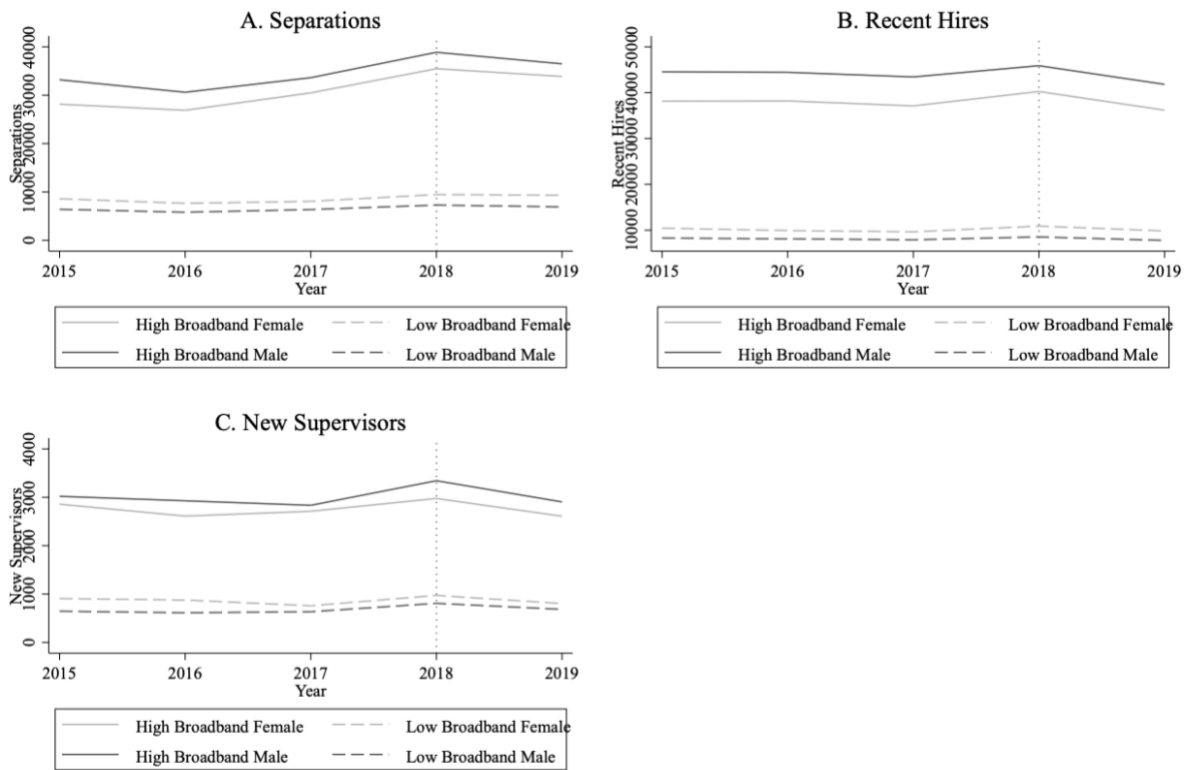
**Table 3-2 Effect of Policy Change on Share Female in Workplace Outcomes**

	Separations (1)	Separations (2)	New Hires (3)	New Hires (4)	Recent Supervisors (5)	Recent Supervisors (6)
			<u>2SLS Results</u>			
Coefficient on Post X Complaints	-0.00114 (0.00755)	-0.00259 (0.00591)	0.00835 (0.00643)	0.00402 (0.00556)	0.0170 (0.0140)	-0.00685 (0.0137)
Observations	8,810	8,810	8,849	8,849	6,694	6,694
			<u>Reduced Form Results</u>			
Coefficient on Post X Broadband	-8.78e-05 (0.000584)	-0.00365 (0.00829)	0.000636 (0.000482)	0.00563 (0.00774)	0.00161 (0.00131)	-0.0110 (0.0220)
Observations	8,810	8,810	8,849	8,849	6,694	6,694
			<u>OLS Results</u>			
Coefficient on Post X Complaints		0.000281 (0.000200)	-0.000234 (0.000177)		0.000135 (0.000511)	
Observations		8,827	8,864		6,706	
			<u>Specification Details</u>			
Instrument Type	Continuous	Binary	Continuous	Binary	Continuous	Binary
County Fixed Effects	X	X	X	X	X	X
Fiscal Year Fixed Effects	X	X	X	X	X	X
Quadratic Employment Controls	X	X	X	X	X	X
Outcome Mean	0.580	0.580	0.555	0.555	0.545	0.545
			<u>Implied Elasticity Range</u>			
Upper Bound (from 2SLS)	0.0653	0.0429	0.104	0.0741	0.285	0.129
Lower Bound (from 2SLS)	-0.0761	-0.0676	-0.0211	-0.0342	-0.0668	-0.217

Notes: Unit of observation is county-by-year. EEO complaints reflect those filed during the first six months of the year from 2015-2019. The implied elasticity range is calculated by using the 95 percent confidence interval suggested by the 2SLS results scaled by the outcome and complaint means. Standard errors (in parentheses) are clustered on county. \*\*\*p<.01, \*\*p<.05, \*p<.10.

new supervisors variable is somewhat noisier due to the relatively small number of new supervisors each year at the USPS but continues to suggest a rather inelastic response. The findings in Table 3-2 are reflected in Figure 3-3, which shows parallel trends in male and female outcomes for high and low broadband areas over time despite the big increase in sex-based EEO complaints that occurs in high broadband areas during this time.

**Figure 3-3 USPS Worker Outcomes by Broadband Access and Sex**



The results suggest that an increase in discrimination and harassment reporting has a negligible effect on workplace outcomes related to turnover, hiring, and promotions. The findings do not, however, rule out significant positive or negative effects for the specific individuals who raised the complaints, as the study relies on aggregate data. Rather the study implies that additional discrimination and harassment grievance reporting alone is unlikely to

generate large spillovers onto downstream workplace outcomes via accountability effects or backlash.

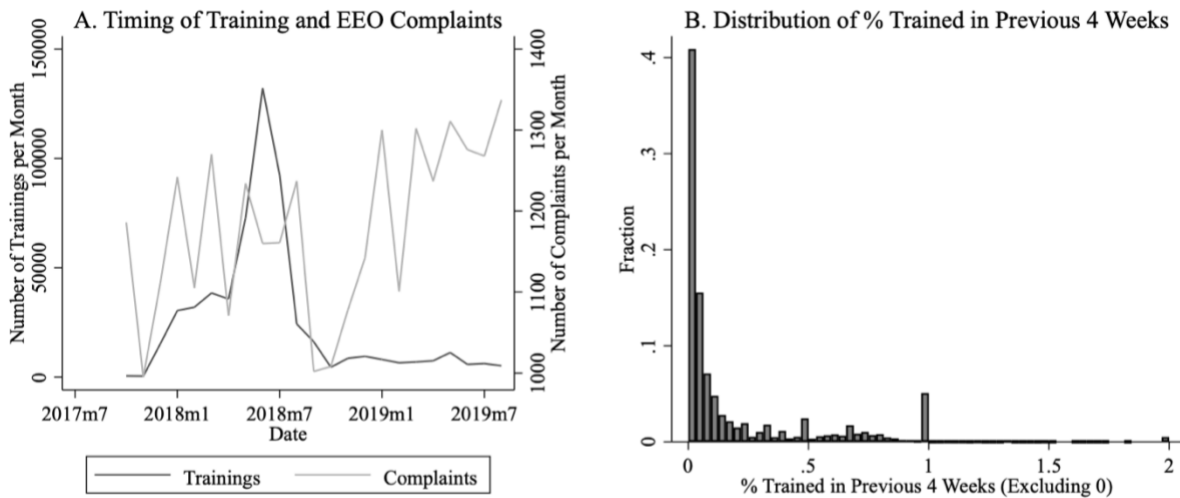
### **3.4. The Impact of Training on EEO Outcomes**

I next examine the impact of training at the USPS on outcome variables derived from roughly 30,000 EEO complaints that were raised between October 2017 and September 2019. Like many organizations, the USPS uses training to combat discrimination and harassment. It has various types of training it uses for this purpose. For example, some training instructs managers how to respond to discrimination and harassment allegations, other training aims to correct discriminatory or harassing behaviors, and the training examined in this paper, No FEAR Act training, seeks to inform all employees about the rights and remedies available to them by law. No FEAR Act training takes about 30 minutes to complete. It is usually completed online, but it can also be provided as a PowerPoint presentation when online training is not feasible. In the training, employees learn about discrimination being illegal, their rights, relevant policies and procedures, and protections for individuals who report, in line with recommendations by the EEOC of what discrimination training should entail (EEOC 2022).

I choose to focus my analysis on No FEAR Act training for three reasons. First, it covers topics recommended by the EEOC to include in anti-discrimination training. Second, it is similar in content to the training offered at many organizations and allows me to assess the effect of incremental training in line with the type of training most people receive at work. And third, it is unlikely to be correlated with other changes in the timing and location of discrimination or harassment due to its legal mandate.

The No FEAR Act training is mandated by law to be completed by all USPS employees at least once every two years. The timing structure of the No FEAR Act training is advantageous from a research perspective, as the training occurs at various times of the year across offices in a manner theoretically uncorrelated with underlying discriminatory behavior. My empirical strategy is thus able to compare offices that recently had a No FEAR Act training to offices that did not. In my dataset, I observe the office and date of over 400,000 instances of No FEAR Act training between October 2017 and September 2019. Figure 3-4, Panel A shows the distribution of these trainings over this time frame. A cluster of the trainings are completed before June 30, 2018, in line with the USPS timeline for completing trainings, but continue to occur, albeit at a lower frequency, throughout my study period. Since USPS employees within the same office take the No FEAR Act training at different times and since I do not observe the names of USPS workers who received a training or submitted an EEO complaint due to privacy protections in the data, I define my main training treatment variable as the share of USPS workers in an office who received a No FEAR Act training in the previous 4 weeks. Figure 3-4, Panel B shows the distribution of this variable for non-zero values, which make up 86 percent of my observations.

**Figure 3-4 Timing of Training at the USPS**



To construct the denominator for this variable, I rely on data from publicly posted payroll rosters which show the number of employees who worked at each post office on January 1, 2018 and January 1, 2019 (DataUniverse 2020). I take the average of these two values per office to construct a proxy for the number of employees over my sample period. Because the payroll roster data do not contain the office finance number to facilitate merging with the training and outcomes data, I match the payroll roster data to the training data by the post office name in both datasets. About 85 percent of the trainings match directly to an office name, and I use a fuzzy matching algorithm to merge most of the remaining observations. I exclude about 5 percent of observations that I am unable to confidently match which the merging algorithm, including those for which the total number of trainings during my sample period exceeds 200 percent of the employment in the payroll data. I include in my sample all other post offices I observe in my training and EEO data. About 20 percent of these offices received at least one EEO complaint between October 2017 and September 2019. Table 3-3 presents the summary statistics for my analysis sample.

**Table 3-3 Summary Statistics of Training Data and Sample**

	Mean	SD
	(1)	(2)
	<u>Summary Stats</u>	
Number of Employees	54.29	106.02
Share Existing Employees	0.72	0.31
No Fear Act Trainings	20.82	64.42
Complaints	1.00	4.97
Complaints per Worker	0.02	0.08

Notes: Table presents summary statistics for sample of 27,531 USPS facilities over study period of October 2017 to September 2019.

For my analysis, I use a Poisson pseudo-maximum likelihood estimation of the following equation, with my unit of data at the office-by-day level:

$$(3.4) \ln(Y_{odwy}) = \beta pct\_train_{odwy} + \theta_o + \tau_y + \lambda_w + \gamma_d + \eta_{dwy} + \epsilon_{odwy}$$



where  $\ln(Y_{odwy})$  is the log of my outcome variable, the number of EEO complaints, in office  $o$  on a given day  $d$ ;  $pct\_train_{odwy}$  represents the share of employees in office  $o$  who received a training in the past 4 weeks, and  $\beta$  stands for the coefficient of interest. Because the outcome variable is logged,  $\beta$  approximately captures the percent change in the number of complaints associated with training. The variables  $\theta_o$ ,  $\tau_y$ ,  $\lambda_w$ ,  $\gamma_d$ , and  $\eta_{dwy}$  represent office, year, week-of-year, day-of-week, and holiday fixed effects, respectively.  $\epsilon_{odwy}$  represents the unobserved determinants of my outcome variable occurring on a given day. I cluster my standard errors on post office, which is the unit of variation in my data.

Table 3-4 presents the results for the above specification examining the effect of training on EEO reporting and incidents. The first column examines complaints raised by USPS workers based on the date they were reported. The second column examines them based on the date they occurred. For complaints with multiple incident dates, I use the first incident date in the complaint. About 10 percent of incidents are reported on the same day the incident occurred, and a third of all incidents are reported within a week, so although the reporting and incident effects are closely related, the results might differ based on the date the charges and complaints are assigned. In both Columns 1 and 2, I observe a null effect. The point estimates suggest at most a 5 percent negative impact, and the 95 percent confidence intervals constructed from the standard errors generally rule out a positive or negative effect size of about 15 percent. The results thus suggest a limited effect of training on harassment and discrimination incidents and their reporting.

Columns 3 and 4 further explore how the timing of the No FEAR Act training corresponds with the timing of EEO reports and incidents by adding lags for the previous 24 weeks of training. Observing how additional lags of training influence EEO outcomes could help

**Table 3-4 Effect of No FEAR Act Training on EEO Reports**

	<u>Baseline Regression</u>		<u>Additional Lags</u>		<u>Threshold Analysis</u>	
	<u>Complaint Date</u> (1)	<u>Incident Date</u> (2)	<u>Complaint Date</u> (3)	<u>Incident Date</u> (4)	<u>Complaint Date</u> (5)	<u>Incident Date</u> (6)
Coefficient on % Trained in Past 4 Weeks	-0.0522 (0.0582)	-0.0356 (0.0596)	-0.0205 (0.0615)	-0.00945 (0.0639)		
Coefficient on % Trained in Past 8 Weeks			0.0244 (0.0593)	0.107* (0.0624)		
Coefficient on % Trained in Past 12 Weeks			0.107* (0.0588)	0.249*** (0.0642)		
Coefficient on % Trained in Past 16 Weeks			0.0362 (0.0633)	0.0766 (0.0656)		
Coefficient on % Trained in Past 20 Weeks			-0.0160 (0.0631)	-0.0424 (0.0658)		
Coefficient on % Trained in Past 24 Weeks			0.0391 (0.0604)	0.0794 (0.0613)		
Coefficient on 1% to 50% Trained in Past 4 Weeks					-0.0586 (0.0446)	0.00519 (0.0442)
Coefficient on >50% Trained in Past 4 Weeks					0.0127 (0.0160)	0.0215 (0.0173)
Observations	4,063,279	3,827,182	2,988,388	2,777,289	4,071,140	3,836,130
			<u>Specification Details</u>			
Office Fixed Effects	X	X	X	X	X	X
Year Fixed Effects	X	X	X	X	X	X
Week of Year Fixed Effects	X	X	X	X	X	X
Day of Week Fixed Effects	X	X	X	X	X	X
Holiday Fixed Effects	X	X	X	X	X	X

Notes: Unit of observation is office-by-day. Sample contains roughly 30,000 EEO complaints that were raised at USPS between October 2017 and September 2019. Standard errors (in parentheses) are clustered on office. \*\*\*p<.01, \*\*p<.05, \*p<.10.

us explore whether there is evidence of fadeout in the observed impact of training if we observe a monotonic decline in effect size with time. The lags could also shed light on whether short-term correlates with the timing of training like workload or stress (e.g., Narayan 2022a; Narayan 2022b) are biasing the results if we observe consistently stronger effects for the lags. Although somewhat noisy, the results in Columns 3 and 4 generally follow no observable pattern, thus providing limited evidence that fadeout effects or short-term confounders are influencing the baseline findings.

Columns 5 and 6 of Table 3-4 examine whether the results are sensitive to the share of workers trained recently. It could be that training is more effective after a critical threshold of workers receives the training. The columns use indicators for the whether the share of workers trained falls between 0 and 50 percent or greater than 50 percent to see if there is a stronger impact when more than 50 percent of workers receive training. The results show a null effect for regardless of the threshold used when looking both at reports and incidents.

Table 3-5 examines more carefully whether the main results are reflecting a true null effect or counteracting effects of training on the number of incidents and on reporting. The regressions in Table 3-4 all look at both the reporting date and the incident date to help separate the two potential channels. However, because the incident and reporting dates are closely connected, it could be that training both increases (decreases) reporting and decreases (increases) the number of incidents in a way that offsets each other in my findings. Both of these alternative explanations seem unlikely given that the results in Table 3-4 are very similar when the EEO outcomes are assigned to the date they were reported and the date they occurred. Still, the alternative hypotheses warrant careful consideration, and Table 3-5 more formally tests whether reporting and incident effects are offsetting each other.

**Table 3-5 Effect of No FEAR Act Training on EEO Reporting Behavior**

	<u>Supervisor Training</u>		<u>Reporting Timing</u>		<u>Retaliation Complaints</u>	
	(1)	(2)	(3)	(4)	(5)	(6)
	Supervisor Training		Complaints >28		Complaints <28	
	Controlling for		Days		Days	
	Subordinate Training		Complaints >28		Complaints <28	
	Subordinates	Training	Days	Days	Complaints	Complaints
Coefficient on % Trained in Past 4 Weeks	0.00425 (0.0573)	0.176** (0.0835)	0.0312 (0.0891)	-0.109 (0.0671)	-0.0649 (0.110)	-0.0692 (0.0602)
Observations	3,309,612	3,302,522	2,742,412	3,545,709	2,069,571	3,976,072
			<u>Results</u>			
			<u>Specification Details</u>			
Office Fixed Effects	X	X	X	X	X	X
Year Fixed Effects	X	X	X	X	X	X
Week of Year Fixed Effects	X	X	X	X	X	X
Day of Week Fixed Effects	X	X	X	X	X	X
Holiday Fixed Effects	X	X	X	X	X	X

Notes: Unit of observation is office-by-day. Sample contains roughly 30,000 EEO complaints that were raised at USPS between October 2017 and September 2019. Standard errors (in parentheses) are clustered on office. \*\*\*p<.01, \*\*p<.05, \*p<.10.

The first two columns in Table 3-5 examine how subordinate reports of harassment and discrimination change with manager training. Because the training should only affect manager behavior and not subordinate reporting tendencies, there is no scope for a null result to reflect offsetting effects. If training reduced manager misconduct, we would uncover a negative effect here. Column 1 replicates the baseline specification with the main treatment variable defined as the share of managers who received training in the past 4 weeks and the outcome variables defined as the number of charges or complaints reported by subordinates on a given day. The results continue to show a small, statistically insignificant effect, supporting the view that training has a limited effect on reported incidents. Column 2 adds in a control for the share of subordinates who received a training in the past 4 weeks since the trainings in an office may occur at similar times for both managers and subordinates. Here, the coefficient is significant at the 5 percent level in the opposite direction as would be hypothesized, meaning that the 95 percent confidence interval excludes even small declines in subordinate reporting in response to manager training.

The remaining columns return to the baseline sample combining managers and subordinates. Column 3 restricts the sample to incidents that took at least 4 weeks to report. The underlying behavior in these incidents would not have been affected by the training as the incidents occurred before the training took place, so we should only expect the estimates to reflect an effect on reporting. We continue to observe a null result, with the point estimate suggesting an effect size of negative 6 percent. In comparison, Column 4 restricts the sample to incidents that took fewer than 4 weeks to report for comparison. It also displays a null result, though this result could be influenced by offsetting reporting and behavior effects.

In Columns 5 and 6, I probe more deeply into the alternate hypothesis that training both decreases reporting and increases incidents in a way that offsets each other. The likely mechanism through which this would occur is via an increased recognition of retaliation. Individuals who experienced harassment and discrimination may limit their reporting when they realize they may face retaliation, while offenders might increase their likelihood of retaliation once they learn this may limit future reporting. This behavior would likely lead to an increase in reporting of retaliation. However, in Columns 5 and 6, we see that the effect of training on complaints containing a retaliation charge and complaints not containing a retaliation charge are very similar, suggesting retaliation concerns are not behind the null finding. Altogether, the results presented in Tables 3-4 and 3-5 find no evidence of No FEAR Act training influencing discrimination and harassment outcomes at the USPS.

### **3.5. Conclusion**

Using rigorous empirical methods, this study finds that an increase in harassment and discrimination reporting has little effect on downstream career outcomes related turnover, hiring, and promotions. It then finds no evidence that an increase in commonly used rights-and-remedies training influences the number of reported harassment and discrimination incidents. The paper thus suggests a limited impact on the margin for two practices that are commonly used to combat workplace discrimination and harassment and that provide firms with protection against associated lawsuits. The results from the study would support policies that reduce the legal protections offered to organizations that rely on grievance procedures and training to combat discrimination and harassment, that change federal guidance for the content of anti-

discrimination training,<sup>16</sup> and that provide protections for organizations to study new solutions for these issues.

It is important to acknowledge that although this study suggests a null effect of increasing the use of grievance procedures and rights-and-remedies training, the results are particular to the policies I study. They do not rule out an impact at the extensive margin of providing any form of grievance procedure or training. The results may also not extend to other forms of grievance reporting or training. For example, the #MeToo movement or gold standard diversity training may have a different impact. Still, the results from this study would suggest that if those other policies generated more benefit or backlash, the impacts would likely come from channels separate from those studied in this study.

The results are also specific to the USPS context. The USPS is a unionized, highly regulated, public-sector workplace. Regulations and union contracts have put in place clear policies and procedures at the USPS related to many aspects of employment. The impact of increasing the use of similar grievance procedures or trainings may be different in workplaces that do not have such explicit policies and procedures. For example, the use of grievance procedures might influence hiring, promotions, and terminations might more heavily in organizations that do not have as many policies related to these actions. The response to training might be larger in organizations that do not have EEO policies that are as transparent as those at the USPS. Alternatively, the response to training might be smaller in workplaces where employees do not have the same employment protections that USPS workers have. Additional research examining non-unionized, less regulated, private sector workplaces and different types of training and grievance procedures would help build our understanding of these policies.

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<sup>16</sup> Policymakers might consider changing federal policy on training to align with more comprehensive state laws, which research suggests might have greater impact than current federal policies (Kmec, Hirsh, and Skaggs 2016) .

Even though the results presented in this paper are specific to the context of this study, they reflect popular policies used in many organizations (Dobbin and Kalev 2016; Dobbin and Kalev 2019) and are relevant to a very large workplace in the United States. The findings are somewhat noisy, especially in the analysis of training, but they still suggest that these widespread policies have smaller impacts in this important workplace than other estimates and hypotheses in the literature. For example, research finds that the #MeToo movement led to a roughly 10 percent increase in reporting (Levy and Mattson 2021). The elasticity estimates from my study of roughly 0.1 would suggest that such a change in reporting would have less than a 1 percent change in the female of in separations or recent hires, a figure that stands in contrast to much of the popular press on the backlash of #MeToo (e.g., Bower 2019; The Economist 2018; Tolentino 2018).

In terms of the training literature, my confidence intervals of roughly plus or minus 15 percent exclude some existing estimates from the literature examining how training influences reported misconduct, such as those finding a 100 percent decline extreme harassment in Sharma (2021). If combined with the elasticity estimates from the grievance reporting analysis, the 15 percent bound would correspond with at most a 1.5 percent change in the share of female separations or recent hires, corresponding with a 0.1 bound on the elasticity estimates for these outcomes, or a 3 percent change in the share of female new supervisors, corresponding with a 0.2 bound on the elasticity estimates for this outcome. These values are smaller than the roughly 15 percent increase in the female share of hired employees in Devine et al. (2017) or the 5 to 10 percent change in the share of diverse managers observed in Dobbin and Kalev (2016) and Dobbin and Kalev (2019).



The variation in findings across studies and contexts highlights the need for additional research examining policies like grievance procedures and training. Moreover, the results from this study exemplify how it can be difficult to obtain very precise estimates even with very large data and careful research methods. Further research on the impact of grievance procedures and training would not only shed light on how context influences the impact of these policies but would also increase the sample of individuals studied to tighten our understanding of them. Research on more novel anti-discrimination and harassment solutions would further develop our knowledge base of how to address these issues in the workplace.

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# A. Appendix to Chapter 1

## Appendix Table A - 1 Effect of Pay Cycle on Incidents of Harassment and Discrimination Dropping One Day of Week

	Drop Saturday (1)	Drop Sunday (2)	Drop Monday (3)	Drop Tuesday (4)	Drop Wednesday (5)	Drop Thursday (6)	Drop Friday (7)	Separate Indicators (8)	Alt Fri Definition (9)
Coefficient on Week 2	0.0221*** (0.00712)	0.0496*** (0.00646)	0.0549*** (0.00726)	0.0551*** (0.00735)	0.0541*** (0.00737)	0.0552*** (0.00740)	0.0545*** (0.00747)		0.0441*** (0.00657)
Coefficient on Week 2*Weekend								0.128*** (0.0160)	
Coefficient on Week2*Weekday								0.0179** (0.00706)	
Observations	4,902	4,902	4,903	4,903	4,902	4,902	4,906	5,720	5,720
R-Squared	0.819	0.715	0.808	0.805	0.805	0.808	0.809	0.806	0.804
Day of Week Fixed Effects	X	X	X	X	X	X	X	X	X
Day of Month Fixed Effects	X	X	X	X	X	X	X	X	X
Month of Year Fixed Effects	X	X	X	X	X	X	X	X	X
Fiscal Year Fixed Effects	X	X	X	X	X	X	X	X	X
Holiday Fixed Effects	X	X	X	X	X	X	X	X	X

Notes: Unit of observation is nation-by-day. Sample contains over 800,000 incidents of alleged harassment and discrimination that occurred at USPS between FY 2004 and 2019. Cycles for four moved holiday pay dates are removed. Standard errors (in parentheses) are clustered on week. \*\*\*p<.01, \*\*p<.05, \*p<.10.

**Appendix Table A - 2 Effect of Pay Cycle on Incidents of Harassment and Discrimination by Cycle Timing**

	Baseline	With Moved Pay Dates	Pay Dates Before 15th	Pay Dates After 15th
	(1)	(2)	(3)	(4)
<u>Results</u>				
Coefficient on Week 2	0.0494*** (0.00680)	0.0502*** (0.00681)	0.0501*** (0.0112)	0.0570*** (0.0145)
Observations	5,720	5,780	2,799	2,921
R-Squared	0.804	0.803	0.813	0.802
<u>Specification Details</u>				
Day of Week Fixed Effects	X	X	X	X
Day of Month Fixed Effects	X	X	X	X
Month of Year Fixed Effects	X	X	X	X
Fiscal Year Fixed Effects	X	X	X	X
Holiday Fixed Effects	X	X	X	X

Notes: Unit of observation is nation-by-day. Sample contains over 800,000 incidents of alleged harassment and discrimination that occurred at USPS between FY 2004 and 2019. Cycles for four moved holiday pay dates are removed except in Column 2. Standard errors (in parentheses) are clustered on week. \*\*\*p<.01, \*\*p<.05, \*p<.10.

**Appendix Table A - 3 Effect of Pay Cycle on Incidents of Harassment and Discrimination Scaled by Work Hours**

	Baseline	Baseline with Limited Sample	Effect on Hours	Effect on Number Workers	Incidents per Hour	Incidents per Worker
	(1)	(2)	(3)	(4)	(5)	(6)
<u>Results</u>						
Coefficient on Week 2	0.0494*** (0.00680)	0.0593** (0.0225)	0.00531 (0.00623)	0.00678 (0.00470)	0.0540** (0.0214)	0.0525** (0.0217)
Observations	5,720	555	555	555	555	555
R-Squared	0.804	0.836	0.970	0.970	0.604	0.604
<u>Specification Details</u>						
Day of Week Fixed Effects	X	X	X	X	X	X
Day of Month Fixed Effects	X	X	X	X	X	X
Month of Year Fixed Effects	X	X	X	X	X	X
Fiscal Year Fixed Effects	X	X	X	X	X	X
Holiday Fixed Effects	X	X	X	X	X	X
Years in Sample	All	2018-2019	2018-2019	2018-2019	2018-2019	2018-2019

Notes: Unit of observation is nation-by-day. Baseline sample contains over 800,000 incidents of alleged harassment and discrimination that occurred at USPS between FY 2004 and 2019. Data on daily hours and workers are only available from January 20, 2018 onward. Cycles for four moved holiday pay dates are removed. Standard errors (in parentheses) are clustered on week. \*\*\*p<.01, \*\*p<.05, \*p<.10.

### Appendix Table A - 4 Effect of Pay Cycle on Incidents of Harassment and Discrimination Scaled by Productivity

	Baseline with					
	Baseline Results	Limited Sample	Effect on DPI	Effect on TDPH	Incidents per DPI	Incidents per TDPH
	(1)	(2)	(3)	(4)	(5)	(6)
	<u>Results</u>					
Coefficient on Week 2	0.0443*** (0.0131)	0.0570*** (0.0181)	-0.00471 (0.00780)	0.00533 (0.00747)	0.0617*** (0.0198)	0.0516*** (0.0194)
Observations	819	259	259	259	259	259
R-Squared	0.014	0.037	0.001	0.002	0.036	0.027
	<u>Specification Details</u>					
<u>Years in Sample</u>	All	2015-2019	2015-2019	2015-2019	2015-2019	2015-2019

Notes: Unit of observation is nation-by-week. Baseline sample contains over 800,000 incidents of alleged harassment and discrimination that occurred at USPS between FY 2004 and 2019. Data on productivity are only available from FY 2015 onward. Cycles for four moved holiday pay dates are removed. Robust standard errors (in parentheses). \*\*\*p<.01, \*\*p<.05, \*p<.10.

### Appendix Table A - 5 Effect of Pay Cycle on Union Grievances

	Baseline Results	3+ Weeks Incidents	3+ Weeks Reports	Effect on Reports	More Easily Resolved	Less Easily Resolved
	(1)	(2)	(3)	(4)	(5)	(6)
	<u>Results</u>					
Coefficient on Week 2	0.0396*** (0.00567)	0.0578*** (0.00641)	0.00462 (0.0103)	0.0113 (0.0101)	0.0383*** (0.00537)	0.0425*** (0.00864)
Observations	5,784	5,784	5,760	5,781	5,784	5,784
R-Squared	0.902	0.874	0.905	0.917	0.898	0.861
	<u>Specification Details</u>					
Day of Week Fixed Effects	X	X	X	X	X	X
Day of Month Fixed Effects	X	X	X	X	X	X
Month of Year Fixed Effects	X	X	X	X	X	X
Fiscal Year Fixed Effects	X	X	X	X	X	X
Holiday Fixed Effects	X	X	X	X	X	X

Notes: Unit of observation is nation-by-day. Sample contains roughly 3.2 million grievances brought forward by unionized USPS workers between calendar years 2004 and 2019. Cycles for four moved holiday pay dates are removed. Less easily resolved grievances are those that reach the last two steps of the grievance process. Standard errors (in parentheses) are clustered on week. \*\*\*p<.01, \*\*p<.05, \*p<.10.



**Appendix Table A - 6 Effect of Pay Cycle on Incidents of Harassment and Discrimination by Geographic Variation in Ratio of Average USPS Pay to Average County Pay**

	Low Salary Ratio (1)	Medium Salary Ratio (2)	High Salary Ratio (3)
Coefficient on Week 2	0.0508*** (0.00731)	0.0439*** (0.0153)	0.0189 (0.0218)
Observations	5,720	5,633	4,938
R-Squared	0.779	0.474	0.159
Day of Week Fixed Effects	X	X	X
Day of Month Fixed Effects	X	X	X
Month of Year Fixed Effects	X	X	X
Fiscal Year Fixed Effects	X	X	X
Holiday Fixed Effects	X	X	X

Notes: Unit of observation is nation-by-day. Sample contains over 800,000 incidents of alleged harassment and discrimination that occurred at USPS between FY 2004 and 2019. Cycles for four moved holiday pay dates are removed. Average USPS pay is constructed from public DataUniverse payroll rosters, and average county pay is constructed from the QCEW. Standard errors (in parentheses) are clustered on week. \*\*\*p<.01, \*\*p<.05, \*p<.10.

**Appendix Table A - 7 Effect of Pay Cycle on Incidents of Harassment and Discrimination by Basis**

	Sex (1)	Race (2)	Disability (3)	Retaliation (4)	Age (5)
	<u>Results</u>				
Coefficient on Week 2	0.0516*** (0.00788)	0.0482*** (0.00809)	0.0503*** (0.0102)	0.0421*** (0.00895)	0.0525*** (0.00981)
Observations	5,719	5,720	5,711	5,719	5,714
R-Squared	0.742	0.742	0.695	0.682	0.708
	<u>Specification Details</u>				
Day of Week Fixed Effects	X	X	X	X	X
Day of Month Fixed Effects	X	X	X	X	X
Month of Year Fixed Effects	X	X	X	X	X
Fiscal Year Fixed Effects	X	X	X	X	X
Holiday Fixed Effects	X	X	X	X	X

Notes: Unit of observation is nation-by-day. Sample contains over 800,000 incidents of alleged harassment and discrimination that occurred at USPS between FY 2004 and 2019. Cycles for four moved holiday pay dates are removed. Standard errors (in parentheses) are clustered on week. \*\*\*p<.01, \*\*p<.05, \*p<.10.

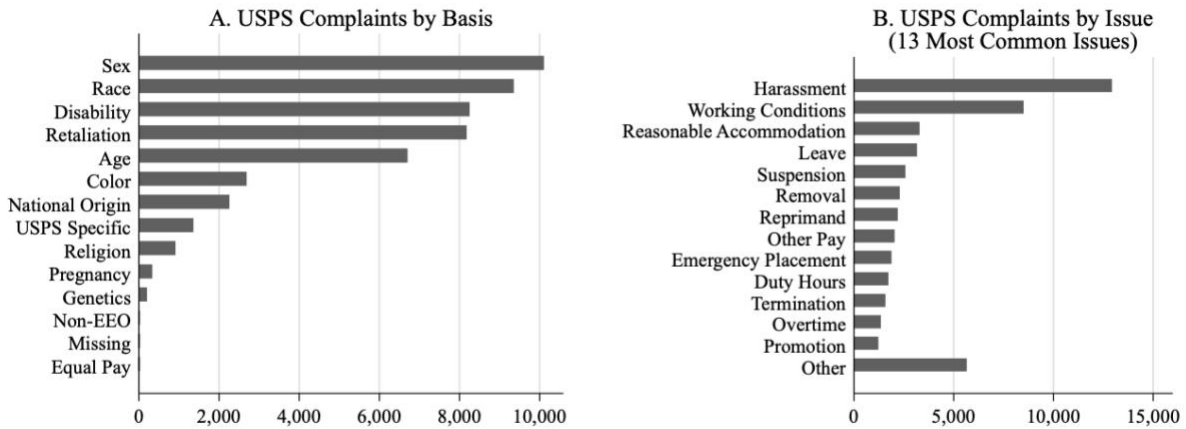
**Appendix Table A - 8 Effect of Pay Cycle on Incidents of Harassment and Discrimination by Issue Type**

	Harassment	Non-Harassment
	(1)	(2)
	<u>Results</u>	
Coefficient on Week 2	0.0301*** (0.0116)	0.0545*** (0.00729)
Observations	5,706	5,719
R-Squared	0.634	0.775
	<u>Specification Details</u>	
Day of Week Fixed Effects	X	X
Day of Month Fixed Effects	X	X
Month of Year Fixed Effects	X	X
Fiscal Year Fixed Effects	X	X
Holiday Fixed Effects	X	X

Notes: Unit of observation is nation-by-day. Sample contains over 800,000 incidents of alleged harassment and discrimination that occurred at USPS between FY 2004 and 2019. Cycles for four moved holiday pay dates are removed. Standard errors (in parentheses) are clustered on week. \*\*\*p<.01, \*\*p<.05, \*p<.10.

## B. Appendix to Chapter 2

**Appendix Figure B - 1 Number of USPS Charges by Basis and Issue in FY 2018  
(Complaints Often Have Multiple Bases and Issues)**



Notes: Data on USPS charges were obtained from the USPS.

**Appendix Table B - 1 Effect of Heat on Incidents of Harassment and Discrimination**

	Baseline Results	Cluster by State	State by Week of Year Effects	Only >90 Degrees	Drop Precipitation	One Charge per Complaint
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Results</b>						
Coefficient on >90 Degrees	0.0533*** (0.0141)	0.0533*** (0.0142)	0.0521*** (0.0153)	0.0322*** (0.0104)	0.0540*** (0.0140)	0.0675*** (0.0130)
Coefficient on 80-90 Degrees	0.0259*** (0.00965)	0.0259** (0.0102)	0.0327*** (0.0101)		0.0263*** (0.00961)	0.0277*** (0.00861)
Coefficient on 70-80 Degrees	0.00656 (0.00778)	0.00656 (0.00752)	0.0128 (0.00792)		0.00675 (0.00780)	0.00787 (0.00762)
Coefficient on 50-60 Degrees	0.00326 (0.00919)	0.00326 (0.00809)	-0.00256 (0.00935)		0.00314 (0.00918)	0.00481 (0.00949)
Coefficient on <50 Degrees	0.00564 (0.0102)	0.00564 (0.0146)	-0.0230** (0.0106)		0.00590 (0.0102)	0.00390 (0.0109)
Observations	70,576,131	70,576,131	69,939,697	70,576,131	70,576,131	70,570,542
<b>Specification Details</b>						
Precipitation Controls	X	X	X	X		X
Office Fixed Effects	X	X	X	X	X	X
Week of Year Fixed Effects	X	X	X	X	X	X
(by State)						
Fiscal Year Fixed Effects	X	X	X	X	X	X
Day of Week Fixed Effects	X	X	X	X	X	X
Holiday Fixed Effects	X	X	X	X	X	X

Notes: Unit of observation is office-by-day. Baseline sample contains over 800,000 incidents of alleged harassment and discrimination that occurred at USPS between FY 2004 and 2019. Maximum temperature is derived from NOAA's GHCN data. Standard errors (in parentheses) are clustered on weather station, except for in Column 2 where they are clustered on state. \*\*\*p<.01, \*\*p<.05, \*p<.10.

**Appendix Table B - 2 Effect of Heat on Incidents or Reporting of Harassment and Discrimination**

	Baseline Results	3+ Week Incidents	3+ Weeks Reports	Effect on Reports	Effect on Closed Date
	(1)	(2)	(3)	(4)	(5)
	<u>Results</u>				
Coefficient on >90 Degrees	0.0533*** (0.0141)	0.0473*** (0.0179)	0.00947 (0.0263)	0.0128 (0.0183)	0.0193 (0.0168)
Coefficient on 80-90 Degrees	0.0259*** (0.00965)	0.0246** (0.0124)	-0.00360 (0.0182)	0.00826 (0.0122)	0.0175 (0.0129)
Coefficient on 70-80 Degrees	0.00656 (0.00778)	0.00513 (0.0103)	-0.00208 (0.0173)	0.00284 (0.0118)	-0.00676 (0.0120)
Coefficient on 50-60 Degrees	0.00326 (0.00919)	-0.00357 (0.0122)	0.00670 (0.0191)	0.00859 (0.0132)	-0.0179 (0.0147)
Coefficient on <50 Degrees	0.00564 (0.0102)	-0.00582 (0.0137)	0.000367 (0.0227)	0.00776 (0.0148)	0.000929 (0.0137)
Observations	70,576,131	57,474,368	57,313,717	70,450,692	69,576,132
	<u>Specification Details</u>				
Precipitation Controls	X	X	X	X	X
Office Fixed Effects	X	X	X	X	X
Week of Year Fixed Effects	X	X	X	X	X
Fiscal Year Fixed Effects	X	X	X	X	X
Day of Week Fixed Effects	X	X	X	X	X
Holiday Fixed Effects	X	X	X	X	X

Notes: Unit of observation is office-by-day. Baseline sample contains over 800,000 incidents of alleged harassment and discrimination that occurred at USPS between FY 2004 and 2019. Maximum temperature is derived from NOAA's GHCN data. Standard errors (in parentheses) are clustered on weather station. \*\*\*p<.01, \*\*p<.05, \*p<.10.

**Appendix Table B - 3 Effect of Heat on Incidents of Harassment and Discrimination by Complaint Formality**

	Informal (1)	Formal (2)
	<u>Results</u>	
Coefficient on >90 Degrees	0.0617*** (0.0199)	0.0474** (0.0187)
Coefficient on 80-90 Degrees	0.0290** (0.0139)	0.0264* (0.0135)
Coefficient on 70-80 Degrees	0.00258 (0.0112)	0.0129 (0.0109)
Coefficient on 50-60 Degrees	0.0104 (0.0118)	-0.00515 (0.0145)
Coefficient on <50 Degrees	0.0198 (0.0128)	-0.00975 (0.0145)
Observations	63,457,855	48,527,768
	<u>Specification Details</u>	
Precipitation Controls	X	X
Office Fixed Effects	X	X
Week of Year Fixed Effects	X	X
Fiscal Year Fixed Effects	X	X
Day of Week Fixed Effects	X	X
Holiday Fixed Effects	X	X

Notes: Unit of observation is office-by-day. Baseline sample contains over 800,000 incidents of alleged harassment and discrimination that occurred at USPS between FY 2004 and 2019. Maximum temperature is derived from NOAA's GHCN data. Standard errors (in parentheses) are clustered on weather station. \*\*\*p<.01, \*\*p<.05, \*p<.10.

**Appendix Table B - 4 Effect of Heat on Incidents of Harassment and Discrimination with OLS Model**

	Baseline Results	per Complaint	3+ Weeks Incidents	3+ Weeks Reports	Effect on Reports	Informal	Formal
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	<u>Results</u>						
Coefficient on >90 Degrees	0.000573*** (0.000182)	0.000226*** (5.07e-05)	0.000241** (0.000118)	3.27e-05 (0.000167)	9.20e-05 (0.000235)	0.000297** (0.000123)	0.000279** (0.000120)
Coefficient on 80-90 Degrees	0.000269** (0.000114)	9.07e-05*** (3.05e-05)	0.000129* (7.55e-05)	-4.55e-05 (0.000111)	5.84e-05 (0.000148)	0.000113 (7.81e-05)	0.000171** (8.14e-05)
Coefficient on 70-80 Degrees	7.71e-05 (9.20e-05)	2.68e-05 (2.66e-05)	3.01e-05 (6.24e-05)	-2.37e-06 (0.000107)	5.08e-05 (0.000144)	-1.05e-06 (6.54e-05)	9.49e-05 (6.58e-05)
Coefficient on 50-60 Degrees	9.86e-06 (0.000103)	1.26e-05 (3.20e-05)	-3.34e-05 (7.17e-05)	3.50e-05 (0.000108)	8.42e-05 (0.000148)	3.36e-05 (6.75e-05)	-3.32e-05 (8.14e-05)
Coefficient on <50 Degrees	-1.55e-05 (0.000111)	4.45e-07 (3.52e-05)	-9.10e-05 (8.15e-05)	-9.04e-07 (0.000125)	7.61e-05 (0.000160)	3.53e-05 (7.15e-05)	-5.97e-05 (7.83e-05)
Observations	70,711,756	70,711,756	70,711,756	70,711,756	70,711,756	70,711,756	70,711,756
R-Squared	0.030	0.030	0.015	0.006	0.013	0.017	0.015
	<u>Scaled Results</u>						
Mean of Outcome Variable	0.0116	0.00340	0.00584	0.00580	0.0115	0.00566	0.00579
<i>90 Degrees Scaled by Mean</i>	<i>0.0494</i>	<i>0.0665</i>	<i>0.0413</i>	<i>0.0056</i>	<i>0.0080</i>	<i>0.0525</i>	<i>0.0482</i>
	<u>Specification Details</u>						
Precipitation Controls	X	X	X	X	X	X	X
Office Fixed Effects	X	X	X	X	X	X	X
Week of Year Fixed Effects	X	X	X	X	X	X	X
Fiscal Year Fixed Effects	X	X	X	X	X	X	X
Day of Week Fixed Effects	X	X	X	X	X	X	X
Holiday Fixed Effects	X	X	X	X	X	X	X

Notes: Unit of observation is office-by-day. Baseline sample contains over 800,000 incidents of alleged harassment and discrimination that occurred at USPS between FY 2004 and 2019. Maximum temperature is derived from NOAA's GHCN data. Standard errors (in parentheses) are clustered on weather station. \*\*\*p<.01, \*\*p<.05, \*p<.10.

**Appendix Table B - 5 Effect of Heat on Union Grievances**

	Baseline Results (1)	3+ Weeks Incidents (2)	3+ Weeks Reports (3)	Effect on Reports (4)	More Easily Resolved (5)	Less Easily Resolved (6)
	<u>Results</u>					
Coefficient on >90 Degrees	0.0355*** (0.0106)	0.0360*** (0.0111)	0.00456 (0.0145)	0.00834 (0.0126)	0.0294*** (0.00927)	0.0480** (0.0230)
Coefficient on 80-90 Degrees	0.0140** (0.00698)	0.0244*** (0.00841)	0.00167 (0.0105)	-0.00217 (0.00810)	0.00969 (0.00629)	0.0233* (0.0133)
Coefficient on 70-80 Degrees	-0.00397 (0.00712)	6.08e-05 (0.00769)	-0.0149* (0.00794)	-0.0198*** (0.00590)	-0.00930 (0.00592)	0.00630 (0.0135)
Coefficient on 50-60 Degrees	0.0219** (0.0107)	0.0152 (0.0107)	0.0277*** (0.0107)	0.0140* (0.00735)	0.0135* (0.00816)	0.0369* (0.0195)
Coefficient on <50 Degrees	0.0561*** (0.00854)	0.0716*** (0.0109)	0.0376*** (0.0111)	0.0275*** (0.00812)	0.0510*** (0.0103)	0.0681*** (0.0136)
Observations	103,765,782	96,134,978	95,949,630	103,655,668	92,534,853	85,068,275
	<u>Specification Details</u>					
Precipitation Controls	X	X	X	X	X	X
Zip Code Fixed Effects	X	X	X	X	X	X
Week of Year Fixed Effects	X	X	X	X	X	X
Fiscal Year Fixed Effects	X	X	X	X	X	X
Day of Week Fixed Effects	X	X	X	X	X	X
Holiday Fixed Effects	X	X	X	X	X	X

Notes: Unit of observation is zip code-by-day. Sample contains roughly 3.2 million grievances brought forward by unionized USPS workers between calendar years 2004 and 2019. Maximum temperature is derived from NOAA's GHCN data. Less easily resolved grievances are those that reach the last two steps of the grievance process. Standard errors (in parentheses) are clustered on weather station. \*\*\*p<.01, \*\*p<.05, \*p<.10.

**Appendix Table B - 6 Effect of Heat on Incidents of Harassment and Discrimination by Occupation or Issue Type**

	Letter Carrier	Non-Letter Carrier	Non- Harassment	Non- Harassment	Working Conditions	Non-Working Conditions
	(1)	(2)	(3)	(4)	(5)	(6)
<u>Results</u>						
Coefficient on >90 Degrees	0.0627*** (0.0196)	0.0472** (0.0186)	0.0537** (0.0227)	0.0530*** (0.0157)	0.0852*** (0.0274)	0.0472*** (0.0144)
Coefficient on 80-90 Degrees	0.0293** (0.0147)	0.0240** (0.0121)	0.0337** (0.0163)	0.0234** (0.0106)	0.0179 (0.0213)	0.0276*** (0.00958)
Coefficient on 70-80 Degrees	0.00226 (0.0127)	0.00923 (0.00950)	0.0223 (0.0153)	0.00186 (0.00832)	0.00774 (0.0191)	0.00651 (0.00784)
Coefficient on 50-60 Degrees	-0.000898 (0.0142)	0.00508 (0.0111)	0.0165 (0.0172)	-0.000660 (0.0102)	0.0164 (0.0187)	0.000706 (0.00990)
Coefficient on <50 Degrees	0.0207 (0.0153)	-0.00305 (0.0130)	0.0206 (0.0190)	0.00103 (0.0113)	0.0173 (0.0203)	0.00300 (0.0106)
Observations	47,767,950	59,992,549	50,062,829	66,527,444	44,310,656	67,993,732
<u>Specification Details</u>						
Precipitation Controls	X	X	X	X	X	X
Office Fixed Effects	X	X	X	X	X	X
Week of Year Fixed Effects	X	X	X	X	X	X
Fiscal Year Fixed Effects	X	X	X	X	X	X
Day of Week Fixed Effects	X	X	X	X	X	X
Holiday Fixed Effects	X	X	X	X	X	X

Notes: Unit of observation is office-by-day. Baseline sample contains over 800,000 incidents of alleged harassment and discrimination that occurred at USPS between FY 2004 and 2019. Maximum temperature is derived from NOAA's GHCN data. Standard errors (in parentheses) are clustered on weather station. \*\*\*p<.01, \*\*p<.05, \*p<.10.



**Appendix Table B - 7 Effect of Heat on Incidents of Harassment and Discrimination by  
Number of Average >90 Degree Days**

	7 Days or Less (1)	More than 7 Days (2)
	<u>Results</u>	
Coefficient on >90 Degrees	0.125** (0.0586)	0.0522*** (0.0150)
Coefficient on 80-90 Degrees	0.0348 (0.0335)	0.0256** (0.0102)
Coefficient on 70-80 Degrees	-0.0207 (0.0227)	0.0109 (0.00822)
Coefficient on 50-60 Degrees	-0.000731 (0.0227)	0.00330 (0.00999)
Coefficient on <50 Degrees	-0.00220 (0.0277)	0.00668 (0.0112)
Observations	12,974,753	57,601,378
	<u>Specification Details</u>	
Precipitation Controls	X	X
Office Fixed Effects	X	X
Week of Year Fixed Effects	X	X
Fiscal Year Fixed Effects	X	X
Day of Week Fixed Effects	X	X
Holiday Fixed Effects	X	X

Notes: Unit of observation is office-by-day. Baseline sample contains over 800,000 incidents of alleged harassment and discrimination that occurred at USPS between FY 2004 and 2019. Maximum temperature is derived from NOAA's GHCN data. Standard errors (in parentheses) are clustered on weather station. \*\*\*p<.01, \*\*p<.05, \*p<.10.

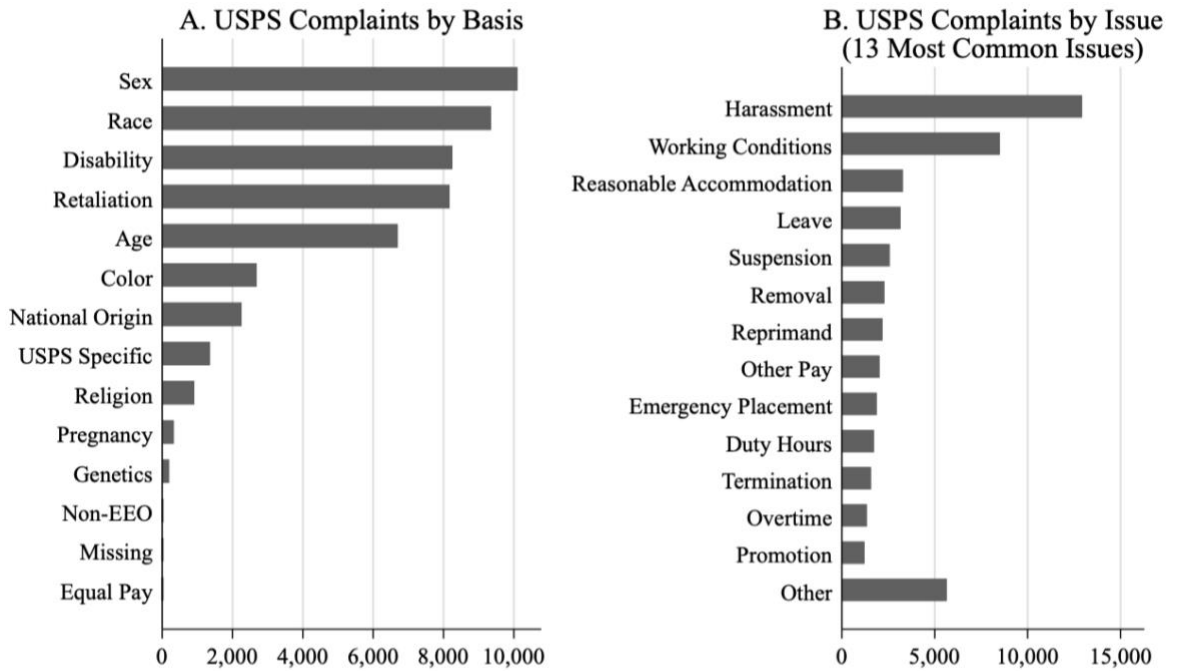
**Appendix Table B - 8 Effect of Heat on Incidents of Harassment and Discrimination by Basis**

	Sex	Race	Disability	Retaliation	Age
	(1)	(2)	(3)	(4)	(5)
	<u>Results</u>				
Coefficient on >90 Degrees	0.0456*** (0.0158)	0.0567*** (0.0161)	0.0501** (0.0217)	0.0436*** (0.0163)	0.0702*** (0.0216)
Coefficient on 80-90 Degrees	0.0292*** (0.0104)	0.0220* (0.0113)	0.0130 (0.0150)	0.0144 (0.0115)	0.0385*** (0.0139)
Coefficient on 70-80 Degrees	0.00129 (0.00969)	0.00644 (0.0103)	0.00237 (0.0117)	0.00868 (0.0106)	0.0188 (0.0119)
Coefficient on 50-60 Degrees	-0.00442 (0.0111)	0.00702 (0.0120)	-0.00520 (0.0134)	0.00315 (0.0130)	0.0150 (0.0137)
Coefficient on <50 Degrees	0.00752 (0.0122)	0.0110 (0.0125)	-0.00211 (0.0139)	0.00404 (0.0137)	0.00829 (0.0155)
Observations	53,531,101	46,175,056	46,437,574	45,040,711	47,873,881
	<u>Specification Details</u>				
Precipitation Controls	X	X	X	X	X
Office Fixed Effects	X	X	X	X	X
Week of Year Fixed Effects	X	X	X	X	X
Fiscal Year Fixed Effects	X	X	X	X	X
Day of Week Fixed Effects	X	X	X	X	X
Holiday Fixed Effects	X	X	X	X	X

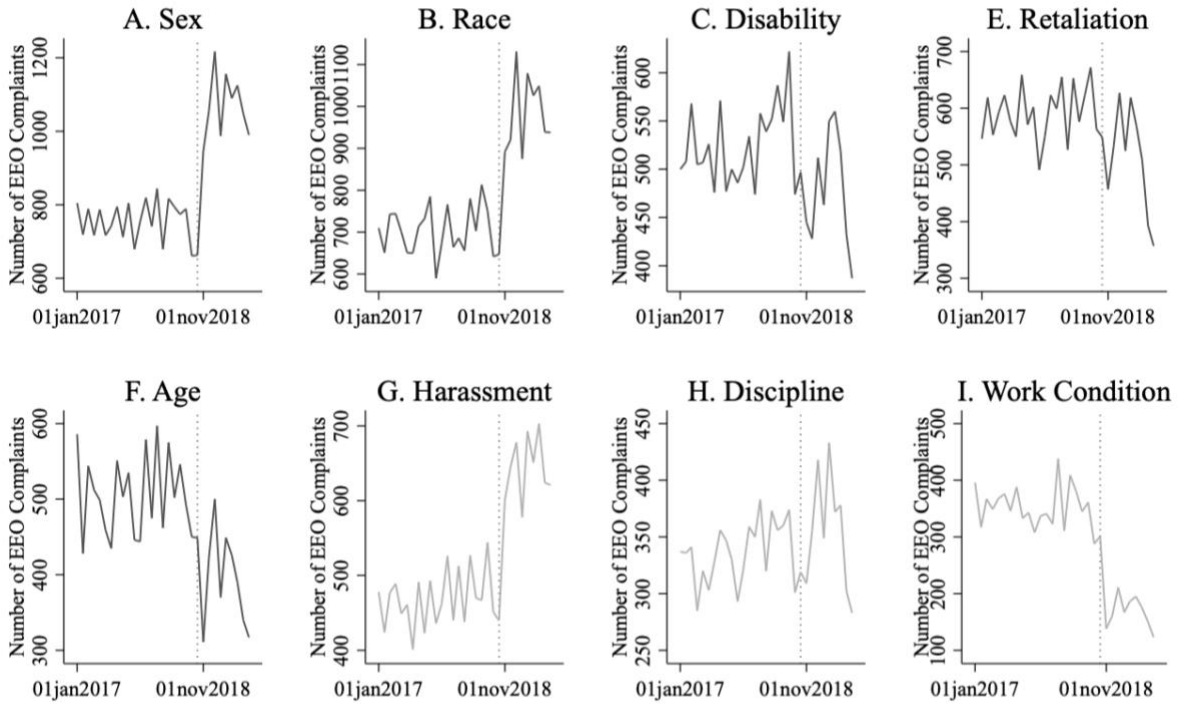
Notes: Unit of observation is office-by-day. Baseline sample contains over 800,000 incidents of alleged harassment and discrimination that occurred at USPS between FY 2004 and 2019. Maximum temperature is derived from NOAA's GHCN data. Standard errors (in parentheses) are clustered on weather station. \*\*\*p<.01, \*\*p<.05, \*p<.10.

### C. Appendix to Chapter 3

**Appendix Figure C - 1 Number of USPS Charges by Basis and Issue in FY 2018  
(Complaints Often Have Multiple Bases and Issues)**

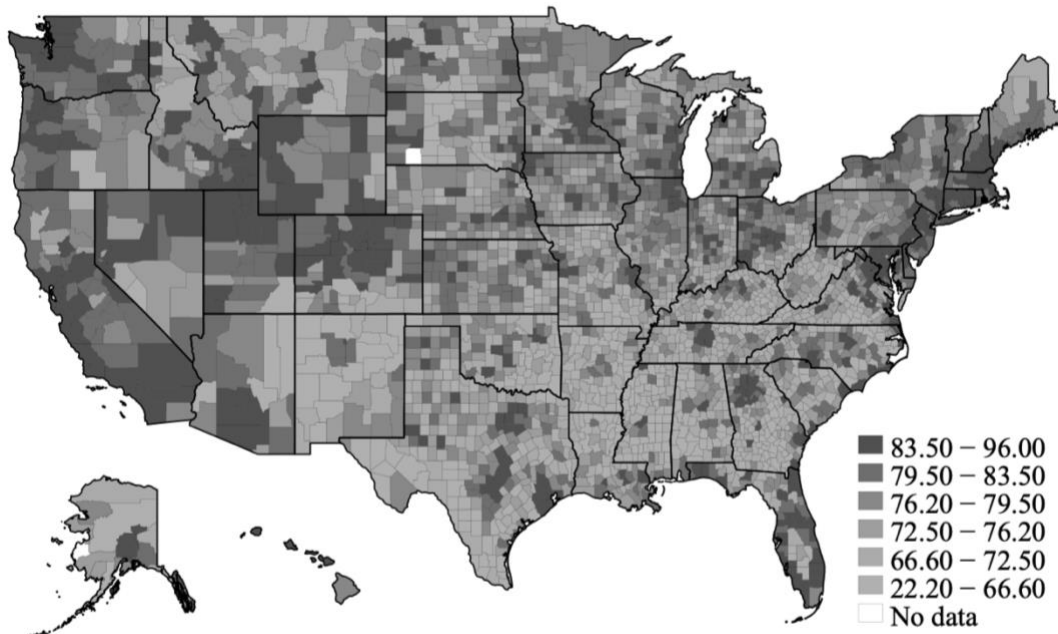


**Appendix Figure C - 2 USPS EEO Complaints by Basis and Issue Over Time**



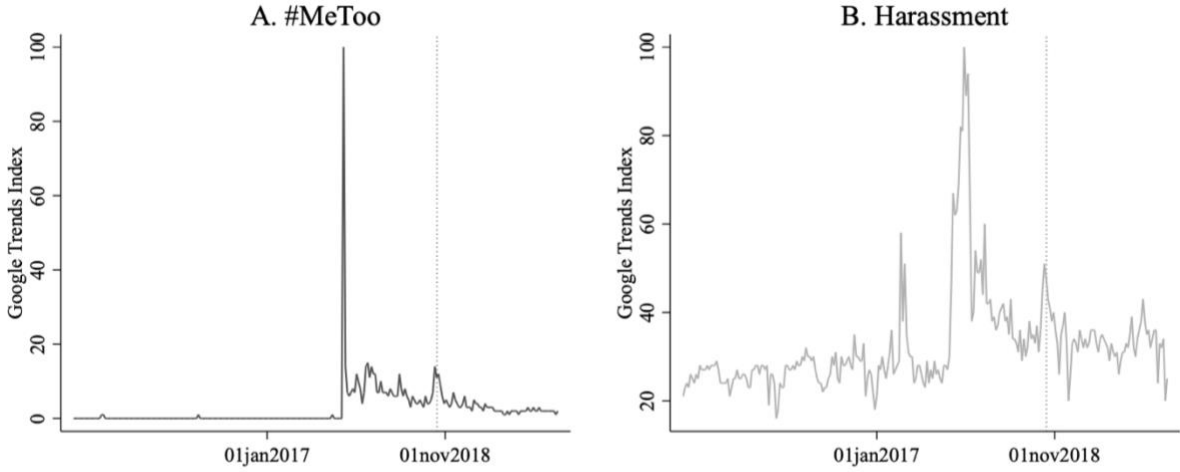
Note: Unit of observation is nation-by-month.

**Appendix Figure C - 3 Broadband Coverage by County**



Source: ACS 2015-2019 5-Year Estimates.

**Appendix Figure C - 4 Google Trends of #MeToo and Harassment from 2015 to 2020**



Source: Google Trends.

**Appendix Table C - 1 Relationship Between Turnover and EEO Charges**

	OLS Reg for All Charges	Panel Reg for All Charges	OLS Reg for Sex Charges	Panel Reg for Sex Charges
	(1)	(2)	(3)	(4)
	<u>Results</u>			
Coefficient on EEO Charges	0.0636 (0.0474)	0.0465 (0.0565)	0.0190 (0.0151)	0.00953 (0.0176)
Observations	8,959	8,959	8,941	8,941
R-Squared	0.008	0.396	0.002	0.353
	<u>Specification Details</u>			
County Fixed Effects		X		X
Fiscal Year Fixed Effects		X		X

Notes: Unit of observation is county-by-year. EEO charges reflect those filed during the first six months of the year from 2015-2019 scaled by worker counts. Turnover defined as names on January 1 in year y and office o that do not show up in office o on January 1 of year y+1 divided by the year y number of names. Standard errors (in parentheses) are clustered on county. \*\*\*p<.01, \*\*p<.05, \*p<.10.

**Appendix Table C - 2 Characteristics of High Broadband Counties**

	Share with Broadband	Population	Share with College	Unemployment Rate	Median Income	Poverty Rate
	(1)	(2)	(3)	(4)	(5)	(6)
	<u>High Broadband</u>					
Mean (Standard Deviation)	83.8 (3.8)	276046 (562454)	30.1 (10.4)	4.8 (1.5)	65173 (14978)	11.8 (4.1)
	<u>Low Broadband</u>					
Mean (Standard Deviation)	70.2 (7.4)	56417 (148048)	18.1 (5.6)	6.5 (3.6)	45479 (9949)	19.0 (8.5)

Source: ACS 2015-2019 5-Year Estimates. High broadband counties are those with above median broadband levels.

**Appendix Table C - 3 Relationship Between Sex-Based EEO Complaints and Alternate Instruments**

	Binary Broadband Monthly (1)	Continuous Broadband Monthly (2)	Binary Broadband Yearly (3)	Continuous Broadband Yearly (4)
<u>Results for Female-Based Complaints</u>				
Coefficient on Post Interaction	0.132*** (0.0311)	0.00700*** (0.00139)	0.918*** (0.203)	0.0479*** (0.00883)
Observations	55,428	55,428	8,940	8,940
R-Squared	0.677	0.677	0.894	0.894
Outcome Variable Mean	0.302	0.302	1.781	1.781
<u>Results for All Charges</u>				
Coefficient on Post Interaction	1.893*** (0.297)	0.100*** (0.0133)	11.79*** (1.841)	0.626*** (0.0802)
Observations	55,428	55,428	8,940	8,940
R-Squared	0.769	0.769	0.917	0.917
Outcome Variable Mean	2.709	2.709	15.58	15.58
<u>Specification Details</u>				
County Fixed Effects	X	X	X	X
Time Fixed Effects	X	X	X	X
Broadband Instrument Used	Binary	Continuous	Binary	Continuous
Unit of Observation	County-Month	County-Month	County-Year	County-Year
Data Range	2017-2019	2017-2019	2015-2019	2015-2019

Notes: Unit of observation is noted in table. EEO complaints reflect those filed during the first six months of the year from 2015-2019 in yearly regressions. Standard errors (in parentheses) are clustered on county; corresponding t-statistics in brackets below. \*\*\*p<.01, \*\*p<.05, \*p<.10.