The Impact of Financial Stress on Workplace Harassment and Discrimination

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Abstract

I study the impact of financial stress on the incidence of harassment and discrimination using Equal Employment Opportunity (EEO) charges brought forward by United States Postal Service workers. An analysis of more than 800,000 EEO charges filed between 2004 and 2019 demonstrates that financial stress experienced in the second week of the pay cycle increases EEO incidents by about 5 percent compared with the first week. Further analyses suggest that the uncovered effects are driven by changes in the number of incidents rather than in their reporting.

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I. 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.[1] These data contain detailed information about each alleged incident such as the date the incident occurred, the date it was

[1] These numbers translate to roughly 3 complaints for every 100 workers per year.

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 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 2022). 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.

II. 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.[2] These papers show that life stress

[2] 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 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.

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, Hoober, 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 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.

III. 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 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).

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.[3]

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.

[3] These privacy protections prevent me from observing data fields like the name of the complainant or alleged offender.

Figure 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 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.

IV. 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 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 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 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 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 is 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 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 2 and 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:

$$(1) \quad \ln(I_{dwmy}) = \beta \text{week2} + \gamma_d + \theta_w + \lambda_m + \tau_y + \eta_{dmy} + \epsilon_{dwmy}$$

where $\ln(I_{dwmy})$ 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 β

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 γ_d, θ_w, λ_m, τ_y , and η_{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 2. ϵ_{dwmy} 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.

V. Results

The first column of Table 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 explore how heterogeneity in incident timing shown in Figure 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 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 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 A1 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 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 A2 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 A3, 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.[4] 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 A4, I conduct a similar exercise but scale the EEO incidents by two commonly used USPS productivity metrics

[4] 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.

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 A5. 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.[5] Appendix Table A5 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.[6]

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

[5] 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.
[6] 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 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.

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 2. Changes in reporting propensities thus do not appear to be driving the effects uncovered in my baseline estimates.

Even though Table 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 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 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.

Finally, I consider heterogeneity in the characteristics of the incidents that occur. I begin by looking at geographic heterogeneity in Appendix Table A6. 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 A7, 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 A8, 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.

VI. 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.

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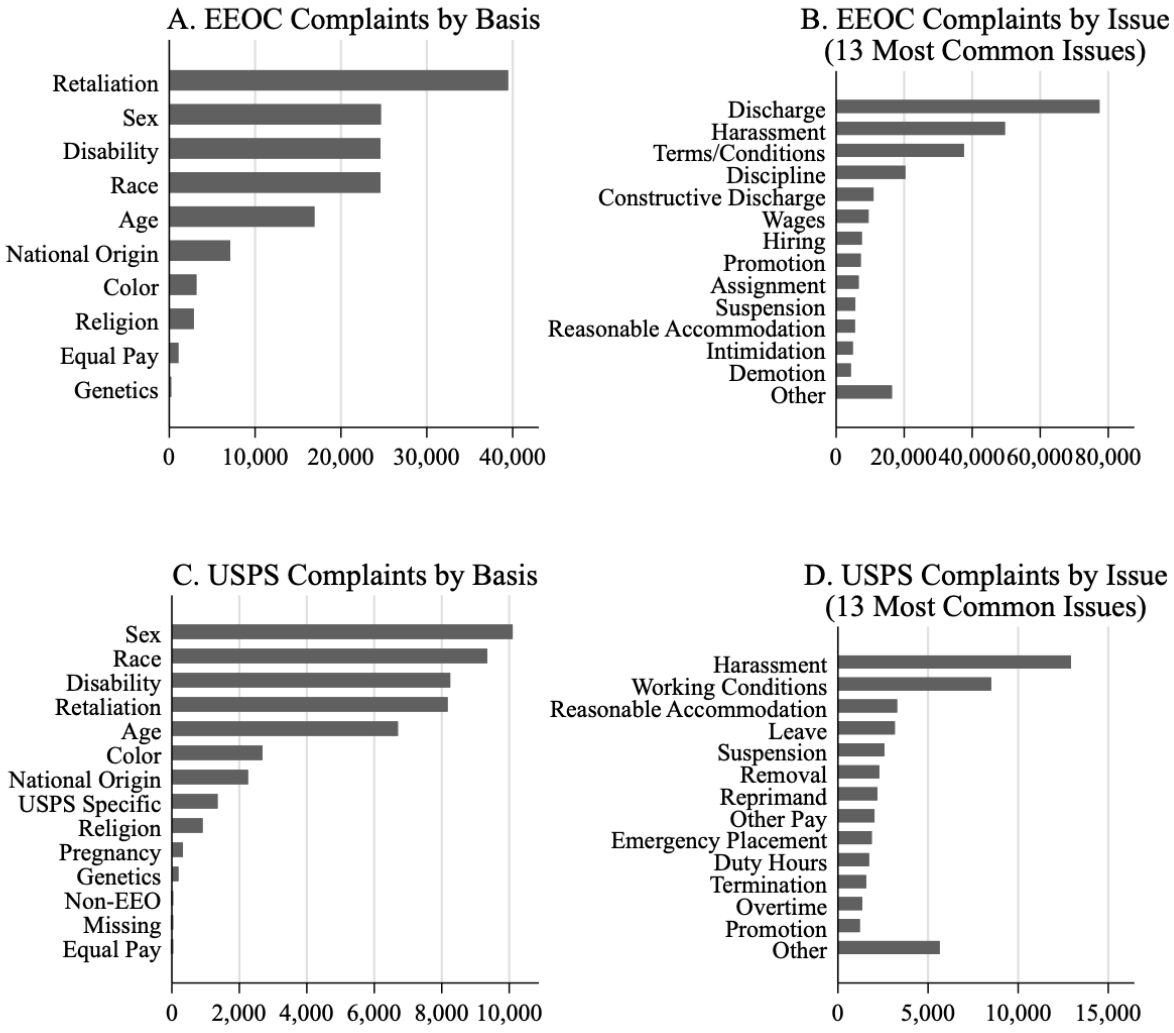
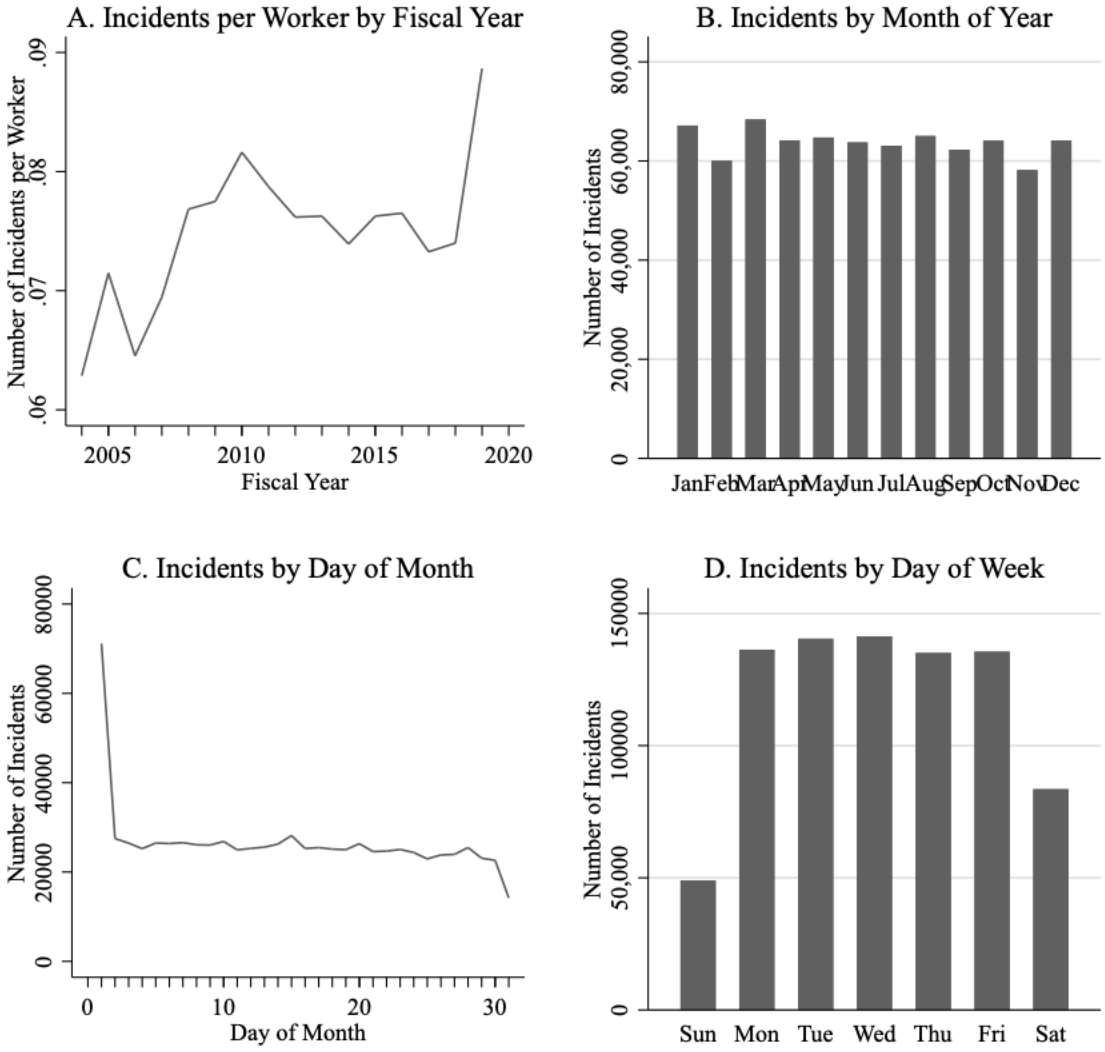
Figure 1 -
Number of EEOC and USPS Charges by Basis and Issue in FY 2018
(Complaints Often Have Multiple Bases and Issues)



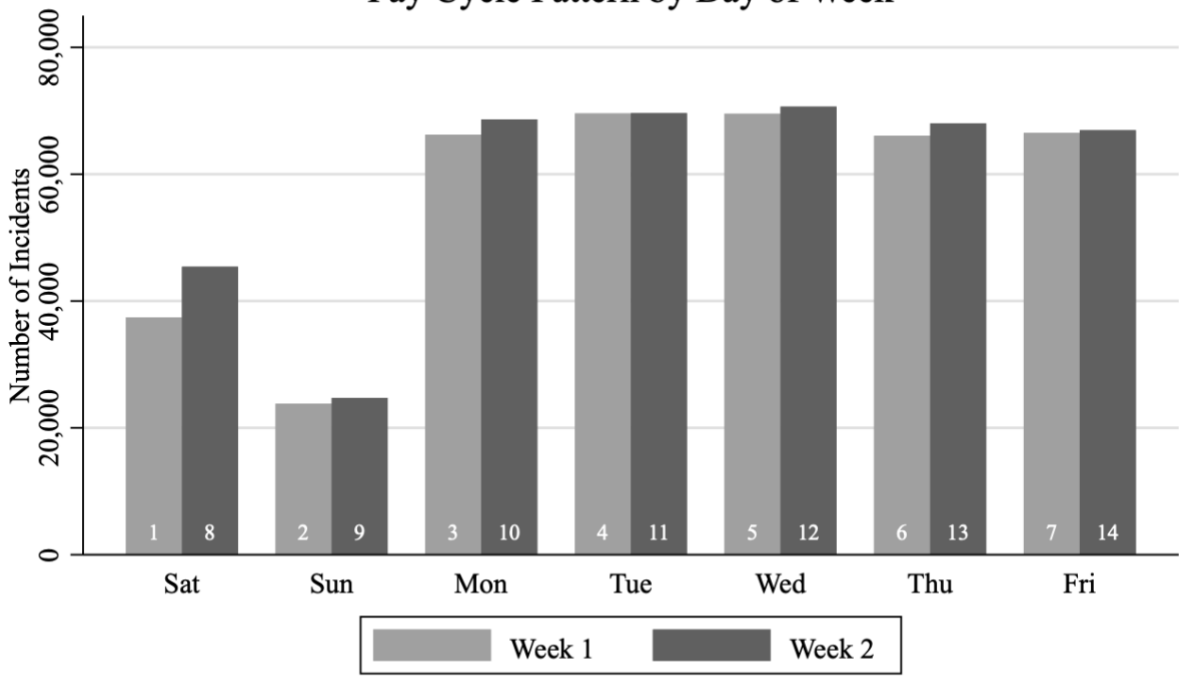
Notes: Data on national EEOC charges were obtained from the EEOC. Data on USPS charges were obtained from the USPS.

Figure 2 -
Timing of USPS Incidents



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

Figure 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.

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

	Baseline Results	Cluster by Pay Period	One Charge per Complaint	Outcome not Logged	Drop First of Month	Drop Change in Reporting	Drop All Fixed Effects	Weekdays Only
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Results				
Coefficient on Week 2	0.0494***	0.0494***	0.0506***	4.803***	0.0499***	0.0482***	0.0537***	0.0167**
	(0.00680)	(0.00610)	(0.00696)	(0.869)	(0.00660)	(0.00665)	(0.0118)	(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
				Specification Details				
Day of Week Fixed Effects	X	X	X	X	X	X		X
Day of Month Fixed Effects	X	X	X	X	X	X		X
Month of Year Fixed Effects	X	X	X	X	X	X		X
Fiscal Year Fixed Effects	X	X	X	X	X	X		X
Holiday Fixed Effects	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 except for Column 2 where they are clustered on pay period. ***p<.01, **p<.05, *p<.10.

Table 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)
			Results		
Coefficient on Week 2	0.0494***	-0.0147	0.0239*	0.0120	0.0587***
	(0.00680)	(0.0198)	(0.0126)	(0.0149)	(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
			Specification Details		
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.

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

	Informal	Formal
	(1)	(2)
	Results	
Coefficient on Week 2	0.0634***	0.0425***
	(0.00863)	(0.0137)
Observations	5,720	5,697
R-Squared	0.797	0.621
	Specification Details	
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.

Table A1 -
Effect of Pay Cycle on Incidents of Harassment and Discrimination Dropping One Day of Week

	Drop Saturday	Drop Sunday	Drop Monday	Drop Tuesday	Drop Wednesday	Drop Thursday	Drop Friday	Separate Indicators	Alt Fri Definition
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Results				
Coefficient on Week 2	0.0221***	0.0496***	0.0549***	0.0551***	0.0541***	0.0552***	0.0545***		0.0441***
	(0.00712)	(0.00646)	(0.00726)	(0.00735)	(0.00737)	(0.00740)	(0.00747)		(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
					Specification Details				
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.

Table A2 -
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)
		Results		
Coefficient on Week 2	0.0494***	0.0502***	0.0501***	0.0570***
	(0.00680)	(0.00681)	(0.0112)	(0.0145)
Observations	5,720	5,780	2,799	2,921
R-Squared	0.804	0.803	0.813	0.802
		Specification Details		
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.

Table A3 -
Effect of Pay Cycle on Incidents of Harassment and Discrimination Scaled by Work Hours

	Baseline Results	Baseline with Limited Sample	Effect on Hours	Effect on Number Workers	Incidents per Hour	Incidents per Worker
	(1)	(2)	(3)	(4)	(5)	(6)
			Results			
Coefficient on Week 2	0.0494***	0.0593**	0.00531	0.00678	0.0540**	0.0525**
	(0.00680)	(0.0225)	(0.00623)	(0.00470)	(0.0214)	(0.0217)
Observations	5,720	555	555	555	555	555
R-Squared	0.804	0.836	0.970	0.970	0.604	0.604
			Specification Details			
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.

Table A4 -
Effect of Pay Cycle on Incidents of Harassment and Discrimination Scaled by Productivity

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

Table A5 -
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)
			Results			
Coefficient on Week 2	0.0396***	0.0578***	0.00462	0.0113	0.0383***	0.0425***
	(0.00567)	(0.00641)	(0.0103)	(0.0101)	(0.00537)	(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
			Specification Details			
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.

Table A6 -
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	Medium Salary Ratio	High Salary Ratio
	(1)	(2)	(3)
Coefficient on Week 2	0.0508***	0.0439***	0.0189
	(0.00731)	(0.0153)	(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.

Table A7 -
Effect of Pay Cycle on Incidents of Harassment and Discrimination by Basis

	Sex	Race	Disability	Retaliation	Age
	(1)	(2)	(3)	(4)	(5)
			Results		
Coefficient on Week 2	0.0516***	0.0482***	0.0503***	0.0421***	0.0525***
	(0.00788)	(0.00809)	(0.0102)	(0.00895)	(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
			Specification Details		
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.

Table A8 -
Effect of Pay Cycle on Incidents of Harassment and Discrimination by Issue Type

	Harassment	Non-Harassment
	(1)	(2)
	Results	
Coefficient on Week 2	0.0301***	0.0545***
	(0.0116)	(0.00729)
Observations	5,706	5,719
R-Squared	0.634	0.775
	Specification Details	
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.