Sexual Misconduct and Scientific Production

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Abstract

While sexual misconduct in the workplace has complex and lasting consequences for directly affected individuals, its broader organizational implications remain less well understood. Using a novel dataset of over 1,000 documented sexual misconduct cases across U.S. universities, I examine how these publicly reported incidents affect departmental scientific productivity. Using the benefit of hindsight, I record the year sexual misconduct occurs and the year it becomes public. I employ coarsened exact matching and a staggered difference-in-differences design to compare control departments with those that experienced subsequently publicized misconduct incidents. Sexual misconduct shows no discernible effect on departmental productivity when it occurs, but public reporting reduces publications by 0.1 per faculty member annually—equivalent to nine fewer publications over five years for a median department of 18 members. These findings reveal that organizational costs arise specifically from public disclosure rather than from the misconduct itself. This distinction between occurrence and disclosure effects suggests that protecting victims and maintaining productivity may require differentiated policy approaches as institutions navigate competing demands from legal frameworks, ethical obligations, and performance concerns. These dynamics help explain both why social pressures transform misconduct from HR concerns into strategic organizational challenges and why firms may prioritize confidentiality strategies.

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Introduction

Sexual misconduct in the workplace — encompassing a spectrum of harmful behaviors including harassment, assault, and jokes or comments that devalue individuals based on their sex (Equal Employment Opportunity Commission, 1980) — is widespread and has complex and lasting consequences for affected individuals. Nearly 60% of women and 27% of men report having personally experienced unwanted sexual advances or sexual harassment, either verbal or physical, in workplace or non-workplace settings (Graf, 2018). Experiencing sexual misconduct can have severe personal consequences: it diminishes job satisfaction, heightens the desire to leave one's position, and undermines psychological well-being, potentially leading to anxiety, stress disorders, and depression (Schneider, Swan and Fitzgerald, 1997; Cortina and Berdahl, 2008; Fitzgerald and Cortina, 2018). Further research shows that it drives women away from organizations (Folke and Rickne, 2022; Adams-Prassl et al., 2024; Collis and Van Effenterre, 2024; Batut, Coly and Schneider-Strawczynski, 2022). Although researchers have made important strides in documenting the consequences of sexual misconduct on individual workers, we know much less about how such misconduct affects organizational-level outcomes such as productivity.

Organizations invest substantial resources in preventing sexual misconduct while simultaneously and presumably protecting perpetrators when incidents occur – a paradox that suggests competing organizational priorities. Many companies try to follow best practices by, for example, mandating anti-sexual harassment training (Antecol and Cobb-Clark, 2003; Kalev, Dobbin and Kelly, 2006; Dobbin and Kelly, 2007; Dobbin and Kalev, 2019). In fact, states such as New York, Massachusetts, and California require such programs by law. However, when misconduct occurs, organizations routinely implement non-disclosure agreements and retain accused employees (Adams-Prassl et al., 2024; Barmes, 2023).

One possible explanation for this paradox is that organizations are more concerned about the organizational consequences of public exposure than about the moral and legal risks of sexual misconduct itself. To explore this possibility, this study examines the productivity consequences of sexual misconduct in organizations both at the time the misconduct occurs and at the time an incident is publicly reported.

Studying sexual misconduct in organizations is challenging. Sexual misconduct is often not recorded or systematically underreported (Fitzgerald, Swan and Fischer, 1995; Boudreau et al., 2023; Dahl and Knepper, 2021; Cheng and Hsiaw, 2022), making data hard to obtain. Moreover, granular personnel and productivity data is required to study overall productivity.

To overcome these challenges I turn to university departments. This setting offers several key advantages. First, while imperfect, data about sexual misconduct cases exists. Second, one of university departments' key output — namely scientific publications — is an established measure and offers rich information about productivity. Moreover, university departments are central sites of scientific production and intellectual innovation, with far-reaching implications for knowledge creation and broader economic outcomes (Agrawal and Henderson, 2002; Azoulay, Graff Zivin and Wang, 2010; Stephan, 2010; Chandra and Xu, 2025).

In this empirical setting, I examine *how sexual misconduct incidents in university departments affect scientific production.* I build a panel dataset for over 5,000 university departments across nine disciplines between 1980 and 2024. To construct the treated departments, I use a comprehensive dataset of all sexual misconduct cases at universities in the United States that have been publicized between 1980 and 2024. To meaningfully study the relationship between sexual misconduct and productivity, I restrict my sample to research-focused universities where the accused individual was a faculty member, resulting in 359 treated university departments. To construct a set of comparable departments, I use the list of unique research-focused universities based on IPEDS data and the Carnegie Classification. I then randomly select nine departments per university as the comparison university departments. This results in a total of 5,076 control university departments.

For the 359 sexual misconduct incidents, I hand-collect over 1,300 lawsuits, news articles, or proprietary documents shared through online document libraries which allow me to establish two crucial points of time: the year when sexual misconduct starts to occur and the year when public reporting begins. I use these two points of time as two different "treatment" times to establish the causal impact on productivity of (1) the occurrence of sexual misconduct and (2) the public reporting of sexual misconduct.

To construct a suitable comparison group for departments where sexual misconduct was publicly reported, I pair Coarsened Exact Matching (CEM) with a staggered difference-in-differences event study design. University departments are matched on five covariates: discrimination and harassment state laws, whether the university is public or private, discipline, research intensity of the university department, and department size.

The results indicate that *public reporting* of sexual misconduct incidents decreases university departments' overall productivity, but that the *occurrence* of sexual misconduct does not. The public reporting of sexual misconduct reduces the average number of publications per department member per year by 0.1 (p-value < 0.05) within the first five years after the incident becomes public. In other words, for a median department of 18 faculty members, that results in nine fewer publications five years after the incident becomes public compared to its comparison departments. However, the average number of publications per department member remains indistinguishable from zero for departments where sexual misconduct occurred compared to control departments. This null effect at the average masks some heterogeneity, however. I find that in departments where withdrawal from research is possible and costless—for example, in clinical medicine, by switching to patient-focused activities without financial penalty—sorting does occur and department members shift away from research activities.

My main findings are consistent with stigma-based and cognitive mechanisms that reveal a crucial distinction between contained and spillover effects. When sexual misconduct remains private, stigma, fear of retaliation, and privacy concerns typically contain its organizational impact. Specifically, my findings that the occurrence of sexual misconduct has no discernible effect on productivity overall suggest that directly affected individuals may avoid sharing the stigmatized event with others due to self-blame, shame, and fear of negative judgment from others (Goffman, 1963, 1959). Thus, knowledge about misconduct incidents appears to remain limited to directly affected individuals and those with close ties to them.

However, when sexual misconduct becomes public, different mechanisms emerge. Public reporting creates reputational damage and productivity losses that extend beyond the direct victims, consistent with stigma by association (Lange, Lee and Dai, 2011), where publicized misconduct negatively affects individuals with observable ties to the accused through department membership (McDonnell and Werner, 2016; Barnett and King, 2017; Janney and Gove, 2017) or shared social group characteristics (Greve, Palmer and Pozner, 2010; Jensen, 2006).

Thus, certain organizations might find themselves in a double bind where they must balance demands for increased transparency while protecting the privacy of all parties involved against the organizational costs arising from public reporting about sexual misconduct incidents. These findings reveal that broad organizational costs emerge specifically from public disclosure rather than from the misconduct itself. This might explain both why social pressures transform misconduct from HR concerns into strategic organizational challenges and why firms may prioritize confidentiality strategies.

1 Sexual Misconduct in Organizations

Sexual misconduct refers to unwelcome sexualized conduct and consists of unwanted sexual attention and sexual coercion (Cortina and Areguin, 2021; Cheng and Hsiaw, 2022). Unwanted sexual attention includes unwelcome and sometimes traumatizing expressions of sexual interest. Examples include unwanted sexualized talk, nonconsensual touching, forcible kissing, and sexual assault (Fitzgerald et al., 1988; Fitzgerald, 2019). Sexual coercion, on the other hand, is the act of making employment (i.e., getting and keeping a job) and other professional rewards (i.e., promotions, grants, or social recognition) contingent on sexual cooperation (Cortina and Areguin, 2021). Sexual misconduct often goes hand in hand with gender harassment, which encompasses sexual jokes and acts that devalue individuals based on their sex (Leskinen, Cortina and Kabat, 2011; Leskinen and Cortina, 2014). This definition is more extensive than the legal definition oftentimes found in the literature (Folke and Rickne, 2022; Collis and Van Effenterre, 2024), as legal frameworks depend on local legislative frameworks and, more importantly, focus on the harms imposed on the victim, while in this study, we look at the effects of sexual misconduct on organizations (Cheng and Hsiaw, 2022).

Sexual misconduct remains prevalent in organizations. A nationally representative random-digit dial phone survey in the U.S. suggests that 47% of respondents have experienced sexual harassment at work (Rospenda, Richman and Shannon, 2009). The organizational impact of this prevalence is amplified by the fact that these incidents are typically not isolated events. In one survey, 66 percent of respondents who had recently experienced such misconduct reported that it happened to them more than once (Morral, Gore and Schell, 2015).

While many factors contribute to the occurrence of sexual misconduct in the workplace, such as individual characteristics (Pryor, 1987; Pryor, LaVite and Stoller, 1993) or status (Berdahl, 2007), a large body of work shows that organizational climate, gender imbalance, and power imbalance play critical roles. For instance, organizations that do not tolerate offensive behavior and maintain a climate of respect and fairness are significantly less likely to report cases of sexual harassment (Fitzgerald, Hulin and Drasgow, 1994; Pryor, LaVite and Stoller, 1993; Willness, Steel and Lee, 2007). Moreover, being in the gender minority within an occupation is strongly correlated with disproportionate levels of sexual misconduct (Mansfield et al., 1991; Welsh, 1999; Folke and Rickne, 2025; Subramani and Gorbatai, 2025). Both men and women experience heightened risks of sexual harassment from colleagues and supervisors in occupations numerically dominated by the opposite gender (Folke and Rickne, 2025). Similarly, asymmetries in power, whether stemming from differences in seniority or from economic vulnerability, further contribute to the risk of experiencing sexual misconduct (Fitzgerald, Hulin and Drasgow, 1994; Ilies et al., 2003; Popovich and Warren, 2010; Hershcovis et al., 2017; Adams-Prassl et al., 2024). Moreover, power can itself trigger misconduct against women who attain it, as harassment is often deployed as a sanction for violating prescriptive gender norms at work (McLaughlin, Uggen and Blackstone, 2012).

Sexual Misconduct Incidents and Organizational Performance

While research has documented the consequences of sexual misconduct for those directly targeted, there is reason to expect that its impact extends further. Misconduct is rarely experienced in isolation; it unfolds in organizational contexts where the consequences of its occurrence and handling may reverberate beyond the immediate victim (e.g., Acquadro Maran, Varetto and Civilotti, 2022; Trawalter et al., 2022). Moreover, if misconduct is revealed publicly, it may have additional implications for members of the focal organization (Cheng et al., 2024). By shifting attention to these broader potential dynamics, we can begin to capture the organizational consequences of sexual misconduct more comprehensively. This focus also resonates with the broader literature on organizational wrongdoing and deviance, which demonstrates that individual acts of misconduct can have significant implications for organizational performance that extend far beyond the directly affected parties and the immediate perpetrator (see Alexander, 1999; Karpoff, Lee and Vendrzyk, 1999; Karpoff, Lee and Martin, 2008).

As this section will discuss, some organizational consequences of sexual misconduct may arise directly from its occurrence. These may include increased employee sorting and internal disruptions associated with investigations, remedial actions, and related processes. Beyond these immediate effects, the public reporting of misconduct, often unexpected and abrupt, might generate additional ripple effects. Public reporting may compel the organization to focus on reputation management and organization members to grapple with the misconduct and the potential implications of its occurrence and its public reporting. In addition, when an incident of misconduct becomes public knowledge, external audiences such as suppliers, investors, and other exchange partners may distance themselves from the focal organization, with potentially adverse implications for performance.

Organizational Effects of Occurrence

There are reasons to believe that the occurrence of sexual misconduct might affect productivity through employee sorting and organizational measures. Evidence suggests that victims share their experiences with colleagues of the same gender and warn others of the same gender to keep them safe through "whisper networks" (Meza, 2017; Johnson, 2023). Learning about a colleague's experience with sexual misconduct at the workplace is shown to affect task conflict and performance (Raver and Gelfand, 2005). Additionally, while evidence is sparse, some research suggests that witnessing or being a bystander is associated with negative emotional and psychological consequences, and may even trigger physical harassment toward the bystander (Hitlan, Schneider and Walsh, 2006; Dionisi and Barling, 2018; Flecha, 2021; Acquadro Maran, Varetto and Civilotti, 2022). Given this body of evidence, there is a possibility that the occurrence of sexual misconduct shapes organizational performance via collaboration decisions and worker disruption.

Moreover, reports of sexual misconduct may trigger organizational investigations and audits, which may in turn lead to remedial measures. These investigations often involve interviewing the parties and witnesses involved (Bedera, 2024) and may culminate in organization-wide interventions, such as extensive sexual misconduct training programs, the introduction of new grievance policies and procedures, audits, and other bureaucratic measures (see, e.g., Cortina and Berdahl, 2008; Dobbin and Kelly, 2007). Taken together, processing and addressing sexual misconduct complaints may be disruptive and time-intensive and could divert attention and resources from organization members' core functions.

However, these effects may be constrained by barriers to the intra-organizational flow of information. Many sexual misconduct incidents remain unreported (Boudreau and Kaushik, 2023; Sockin and Sojourner, 2023), as affected individuals often prefer to keep information about the incidents to themselves (Rowe, 1996). Victims and witnesses may be particularly reluctant to share information or report incidents due to fear of stigma and retaliation (Cortina and Magley, 2003; Rehg et al., 2008; Dobbin and Kalev, 2019; Dahl and Knepper, 2021). Even if an incident is reported, organizational procedures are typically designed to maintain confidentiality and minimize the spread of information. For instance, the U.S. Equal Employment Opportunity Commission (EEOC) states that "employers will protect the confidentiality of harassment complaints to the extent possible" (Feldblum and Lipnic, 2016, p. 38). In addition, many organizations reinforce secrecy through formal confidentiality policies and non-disclosure agreements that prohibit parties from discussing incidents and investigations once concluded. Together, these barriers to information flow can limit collective awareness of a sexual misconduct incident within an organization, thereby muting the potential consequences of misconduct beyond those directly affected.

Indeed, even when some organization members eventually learn of a misconduct case, for instance, through whisper networks (Johnson, 2023; Meza, 2017), pluralistic ignorance may keep the information contained (Allport, 1924). Several organization members may know about a transgression, yet remain uncertain whether others are aware. Believing that they might be the only ones who know, individuals may hesitate to act, creating a barrier to coordination. In addition, the threat of a potentially damaging scandal may also discourage organization members from discussing incidents openly (see Adut, 2005). As a result, information about sexual misconduct incidents may not travel far within an organization, and thus their broader organizational consequences may remain constrained.

Organizational Effects of Public Reporting

Sexual misconduct incidents may become public through news coverage or whistleblower disclosures. In such cases, beyond the effects of the occurrence of sexual misconduct, public revelations may create distinct dynamics both inside and outside the organization. Internally, public reporting can alter organizational dynamics by lowering coordination barriers to collective action.

As a result of public reporting, a sexual misconduct incident becomes common knowledge within the organization. Awareness is no longer confined to a subset of members but extends to everyone, both internally and externally. Given the barriers to information flow discussed earlier, many organization members may learn of the case for the first time through such disclosure. Thus, public revelations not only eliminate ignorance of the incident but also resolve the pluralistic ignorance surrounding it. In addition, public revelation can lead to disruptive publicity (Adut, 2005), making inaction within the organization difficult to sustain.

Regardless of prior knowledge, the public disclosure of sexual misconduct incidents is likely to trigger sensemaking among organization members as they seek to interpret and process the event. Sensemaking, defined as the effort to understand issues or events that are novel, ambiguous, or otherwise violate expectations (Maitlis and Christianson, 2014), is a common response to crisis situations (Wiesenfeld, Wurthmann and Hambrick, 2008) and arises when unexpected events disrupt routine activities. Through this process, individuals construct plausible explanations by selecting and interpreting cues through existing mental frameworks (Weick, 1988, 1995; Weick, Sutcliffe and Obstfeld, 2005; Maitlis and Sonenshein, 2010). Because sensemaking is an inherently social process (Allport, 1924; Maitlis and Christianson, 2014), public disclosure of sexual misconduct may produce complex and disruptive cognitive and social processes across the organization. Such disruptions, in turn, may be especially consequential in knowledge-intensive settings, where organizational outcomes depend directly on cognitive performance (Blackler, 1995).

Furthermore, public revelation stigmatizes the organization itself (Adut, 2005; Hudson, 2008), as seen in other domains such as bankruptcy (Sutton and Callahan, 1987; Neu and Wright, 1992; McKinley, Ponemon and Schick, 1996) or doping scandals in sports (Yenkey and Palmer, 2025). Through association, organization members also become tainted (McDonnell and Werner, 2016; Barnett and King, 2017; Janney and Gove, 2017; Zhang et al., 2021). Stigmatization, particularly when morally grounded, often provokes strong emotions and social punishment, though the extent of taint and sanctioning depends on perceived blameworthiness (Jones, 1984; Gomulya and Boeker, 2016; Bruyaka, Philippe and Castañer, 2018).

Organizations and their members may respond to stigmatizing events through strategies such as covering (controlling the narrative), withdrawing from relationships, or passing (maintaining normalcy; Goffman, 1963; Page, 1984; Sutton and Callahan, 1987). In the case of publicized sexual misconduct, however, passing and withdrawal are often untenable, leaving narrative control as the primary option — one that typically requires extensive communication with collaborators and partners. While individual members engage in sensemaking and narrative management at the personal level, organizations simultaneously confront the imperative of managing reputational crisis. This task is fraught: traditional strategies for addressing taint can prove inadequate or even counterproductive (Page, 1984; Sutton and Callahan, 1987; Cheng et al., 2024). Concealment or deflection may appear as complicity, reframing is implausible, and denying responsibility risks being perceived as victim-blaming. As a result, transparency and the acceptance of responsibility often emerge as the most viable strategies, though these approaches are time-consuming and resource-intensive, potentially diverting attention and resources away from core organizational functions.

At the same time, externally, audiences that serve as key gatekeepers to resources and organizational success often respond to publicized sexual misconduct by distancing themselves from the stigmatized organization. Because stigma spreads through visible ties, suppliers, investors, collaborators, and customers may withdraw or downgrade their support (Goffman, 1963; Pontikes, Negro and Rao, 2010; McDonnell and Werner, 2016; Barnett and King, 2017; Janney and Gove, 2017). Such distancing has been documented across diverse contexts: firms entering Chapter 11 bankruptcy experience withdrawal from suppliers and degraded product quality Sutton and Callahan (1987); major adverse events reduce collaborations (McDonnell, Odziemkowska and Pontikes, 2021); public reporting in stigmatized industries increases divestiture (Durand and Vergne, 2015); and stigmatizing revelations diminish customer engagement (Piazza and Jourdan, 2018). Stigma-by-association can also operate at highly granular levels: blacklisted workers depress their colleagues' employment prospects (Pontikes, Negro and Rao, 2010), and stigmatized events can even curtail "ceremonial" citations in academia (Azoulay, Bonatti and Krieger, 2017). Taken together, these findings suggest that external distancing may be one way through which publicized misconduct affects organizational outcomes, likely unfolding simultaneously with the internal processes described above.

In summary, while prior research has emphasized the individual consequences of sexual misconduct, this study focuses on its organizational implications for productivity and performance. It distinguishes between effects that stem from the occurrence of misconduct within organizations and those that follow its public revelation. This distinction anchors the analysis and shifts attention from individual harms to broader organizational consequences.

2 Empirical Design, Data, and Estimation

Studying sexual misconduct comes with its challenges. Sexual misconduct is often not recorded and is systematically underreported (Fitzgerald, Swan and Fischer, 1995; Boudreau et al., 2023; Dahl and Knepper, 2021; Cheng and Hsiaw, 2022), making data hard to obtain. Moreover, granular personnel and productivity data is required to study organizational outcomes and internal dynamics. To overcome these challenges and test the idea that stigma associated with sexual harassment shapes firm behavior, I turn to university departments. This setting offers several key advantages. First, while imperfect, data about sexual misconduct cases exit. Second, one of university departments' key output – namely scientific publications – are an established measure and offer rich information about productivity and collaboration patterns. Moreover, university departments are central sites of scientific production and intellectual innovation, with far-reaching implications for knowledge creation and broader economic outcomes (Agrawal and Henderson, 2002; Azoulay, Graff Zivin and Wang, 2010; Stephan, 2010; Chandra and Xu, 2025).

To study the impact of sexual misconduct incidents on organizational and societal outcomes in academia, I construct a unique dataset that looks at the productivity outcomes.[1] The departments are listed under Appendix D. Treated departments have experienced a sexual misconduct case that became public.

2.1 Treated Research Departments

To construct the dataset of treated research departments, I use data from the Academic Sexual Misconduct Database (ASMD; Libarkin, 2024) to define a list of academics who have been linked to an exposed sexual misconduct incident. The database accessed on November 19, 2024

[1] I define departments by following Clarivate's Core Collection containing 256 categories (https://mjl.clarivate.com/help-center and supplement these with SCImago categories when necessary (https://www.scimagojr.com/journalrank.php). Note, that for the purpose of this work, *departments* are defined as scholars who publish in a list of journals that corresponds to their department. Clarivate and SCImago call them categories.

contains 1,294 incidents[2]. These are all sexual misconduct incidents in academia which became public either through a lawsuit or media reporting and include resolved and ongoing incidents. Note, that when I say *publicized*, then I mean the newspapers have reported about the start of the investigation or lawsuit, any updates, or the conclusion of such. The incidents involve faculty members, administrators, coaches, and other staff. These cases were resolved between 1980 and 2024. For the purpose of this study, I focus on *resolved incidents* at *research-focused* institutions committed by *faculty*. This removes cases which may have ongoing investigations and for which only the overall university is known.This also removes technical colleges or U.S. Military or Navy Academies. It also excludes cases which were committed a coach, administrator - such as a dean, an athletic director, or an executive director - or where we have no information. [3] This reduces the sample to 359 incidents.

To construct the outcome variables, I use the institution and department information, at which the respective incidents occurred and collect the publications, citation information, reference information, and abstracts for each institution x department pair. I collect publication data from OpenAlex. OpenAlex is an open-source database providing access to over 200 million publication records by combining databases such as Microsoft Academic Graph (MAG), Crossref, ORCID, Unpaywall, Pubmed, Pubmed Central, ISSN, and others (Priem, Piwowar and Orr, 2022).[4] OpenAlex claims to have superior linkage of author, institution, and publication information compared to alternative data providers such as Web of Science, Lens.org, or Dimensions.

I manually assign the department by examining each of the 359 incidents in the dataset. I assign the department based on four pieces of information: Libarkin's assignment of discipline, online news articles, an algorithm based on the faculty's publications (in cases where they could

[2]Note that the raw dataset contains two duplicates which I removed

[3]A university is considered research-focused if it has ever in its history been classified by Carnegie Classifications as one of these three categories: Research 1 - Very High Research Spending and Doctorate Production, Research 2 - High Research Spending and Doctorate Production, Research Colleges and University. I conducted a manual search and classification for the 57 universities without a Carnegie Classification. This removes 575 universities which are mostly technical colleges or U.S. Military/Navy Academies. Among the research-active universities, I remove ongoing cases (173) and cases for which both the individual and the discipline were unknown (83). Lastly, I remove all incidents which were committed by a coach, administrator, or unknown (126).

[4]More information can be found here: https://openalex.org/about

be linked to OpenAlex), and the department affiliation reported on their publications (in cases where they could be linked to OpenAlex).[5]

The departments are constructed through topic categories defined by Clarivate and SJR where each topic category is linked to a corresponding journal list.[6]. Examples of topic categories are "Political Science," "Geology," or "Marine Biology." There are a total of 527 distinct topic categories which are listed under Appendix D. My dataset consists of 142 unique departments. Each of these departments is linked to a journal list, which will be used to track publications for a given department.

The 359 incidents took place at 139 unique institutions, one institution is anonymous. All 139 institutions could be matched to OpenAlex.[7] Of the 359 incidents, I can link 347 accused and publicly exposed faculty to an OpenAlex profile. This contains four names and eight incidents where we have enough information to assign a department and university but not enough information to identify the accused individual.

2.2 Identification Strategy and the Construction of Control Research Departments

My empirical strategy focuses on changes in published research output after the incident (A) occurred and (B) was reported on by the media, relative to before the incident occurred, was reported on by the media, or led to the departure of the faculty facing sexual misconduct allegations, respectively.[8] To ensure that I estimate the causal effect of interest and not some other influence that is correlated with the passage of time, my specifications include time-fixed

[5] I hired research assistants who have experience in the life sciences and assigned the individuals in the social sciences myself.

[6] The SJR topic category "Economics and Econometric"'s corresponding journal list, for example, consists of 166 journals ranging from "American Economic Review" to"Economics of Energy and Environmental Policy" or "NBER Macroeconomics Annual book series"

[7] I employed automatic matching by institution name. Each institution that did not return a 1:1 match was matched manually where I search for the correct match if no potential match has been returned by the search algorithm and remove false positives. Note that I also remove universities which are abroad. For example, Duke University (https://openalex.org/I170897317) has a partner university with Wuhan University. That partner university is called Duke Kunshan University and is located in China (https://openalex.org/I4210159968). I remove that university since it is not located in the US and most likely unaware of inter-personal dynamics at the department of interest.

[8] Note that in approximately 65% of the reported cases, the accused faculty left the institution.

effects. However, there may still be time-related confounders such as policies or event-related trends. To mitigate this threat to identification, I implement a coarse exact matching approach described below.

To construct the control research departments, I collect all U.S. based universities which have been classified as research-focused by the Carnegie Classification over the course of the time period of interest, which is 1980 until 2024, including. I construct this list of universities in a three-step process. First, I collect the complete sample of universities that have existed in the United States between 1980 until the most recently available year, 2023, using IPEDS data. IPEDS is a set of surveys conducted annually by the U.S. Department of Education's National Center for Education Statistics (NCES). Institutions that participate in a federal student aid program are required to respond according to the Higher Education Act of 1965.

After that, I keep universities which have been categorized as research-active[9] according to the Carnegie Classification at least once in the history of Carnegie Classification. For that, I construct a panel dataset of the Carnegie Classifications between 1974 and 2023. I keep all university names which have ever been designated as a "Research University" during this time period. I then link the IPEDS database with the selection of research-active universities which results in a dataset of the universe of research-focused universities between 1980 and 2023. This resulted in 1,152 research universities.

In a last step, I link these universities to OpenAlex. Out of the 1,152 universities, 582 could not be matched. I manually inspected all 582 universities which revealed that these are not research-intensive universities, likely explaining why they cannot be linked to OpenAlex. I exclude these universities which leaves 564 control universities.

To construct control research *departments*, I take each of the 564 control universities and assign them nine randomly selected departments. More specifically, I randomly select one department for each of the nine disciplines defined by Clarivate's Core Collection. In total, I construct 5,076 control departments, 564 control departments per discipline. Note, since this

[9]Defined by Carnegie Classifications as one of these three categories: Research 1 - Very High Research Spending and Doctorate Production, Research 2 - High Research Spending and Doctorate Production, Research Colleges and University.

is the universe of research-active universities x discipline pairs, a subset of these university x department pairs is, by definition, treated. Instead of removing them from the control set, I will account for that using a staggered treatment design. This will be discussed in Section 3.

2.3 Variable Construction

Outcome Variables

Number of Publications per Department Member The publication data will be assembled at the department x year x publication level and then aggregated at the department x year or department x year x subgroup (for example, women faculty) level. To capture the productivity of all department members equally, I will count within-department co-authored publications multiple times. For example, if two department members co-author together, their publication will count as two publications.

I measure department x year productivity by counting the number of publications published every year by a given university department divided by the number of department members. This measure provides us with two properties. First, this linear transformation of the number of publications in a given department into a "per capita" makes the measure independent of department size and allows for easy comparison across departments. Second, publication count data is often characterized by a power distribution, with a tail of zeros. Such data structure weights a switch from zero to one more heavily than a shift across non-zero real numbers (Chen and Roth, 2024). Because my dataset is assembled at the university department level, the power distribution is less pronounced, as is shown in Figure 2. The transformation into a per capita unit removes the concern entirely, as is illustrated in Figure 3.

Gender I will infer the department members' *gender* based on their first and, if available, middle name using genderize.io. I will use a statistical probability cut-off point of 90% for either the first or middle names or 75% for first and middle names jointly.

Treatment Indicators I collect all publicly available materials on the set of publicly known sexual misconduct cases in the U.S. Academy (Libarkin, 2024). This includes news articles, lawsuit documents, proprietary documents posted on digital document libraries, and information shared in the Academic Sexual Misconduct Database. I have over 1,300 pieces of information for the 359 incidents. Based on this rich set of information, I code up the treatment indicators for *occurrence of a sexual misconduct*, and *public reporting of a sexual misconduct incident* at the year level.

Spillover Effects by Gender I investigate whether the impact of sexual misconduct is different for men and for women department members, where I will infer their *gender* based on their first and, if available, middle name using genderize.io.

Ability to withdraw from research activities I investigate whether the ability to shift away from research could shape the effect of sexual misconduct occurrences on productivity. This is an ex-post analysis which allows us to understand whether in the extreme case we would see differential productivity effects. To do so, I create two groups of departments. The first group consists of departments where faculty cannot remain employed while simultaneously stopping research and maintaining the same earnings. These include departments such as biology. The second group consists of departments where faculty can easily remain employed while simultaneously stopping research and maintaining the same earnings. These include departments in clinical medicine, where one can shift to patient-focused work, or architecture, where one can work with clients on projects.

This measure captures three dimensions of a given department: the availability of alternative tasks (such as patient-focused work), the financial reward for such alternative tasks, and the ease of withdrawing from research activities while remaining on the faculty. I asked two LLMs (a model by Anthropic and a model by OpenAI) to generate categories ranging from "very low" to "high." I then aggregated these scores and assigned a binary variable of either "no easy switch" or "clinical medicine/professional." While this is admittedly a crude measure of the ability to withdraw from research activities, manual inspection suggests that the measure is sensible. For example, departments in life sciences are categorized as difficult to withdraw from, while departments in clinical medicine, law, or architecture are categorized as easy to withdraw from.

Covariates To assign a research discipline to each department, I use the nine primary disciplines based on Clarivate's Core Collection (https://mjl.clarivate.com/help-center): Agriculture, Biology & Environmental Sciences; Arts & Humanities; Business Collection; Clinical Medicine; Electronics & Telecommunication Collection; Engineering, Computing & Technology; Life Sciences; Physical, Chemical & Earth Sciences; Social And Behavioral Sciences.

To determine whether the university is a public or a private university, I use IPEDS' institutional datasets. I am able to match all 139 unique treated institutions with IPEDS data. The 564 control universities are part of the IPEDS dataset by design.

The research intensity at a given university is assigned using the Shanghai Ranking's *Global Ranking of Academic Subjects (GRAS)* which contains rankings of universities in 55 subjects across Natural Sciences, Engineering, Life Sciences, Medical Sciences, and Social Sciences (Shanghai Consultancy, 2024). The ranking is useful for the purpose of this research because it ranks universities based on research output and quality, faculty quality, and research collaborations.[10] In contrast, U.S. News Education Ranking focuses on research output *and* student experience, and the Carnegie Classifications are based on the number of doctoral degrees granted and the amount of research expenditure. I assign the GRAS at the university x subject level.

An institution's behavior is likely shaped by the laws under which it operates. To account for that, I construct a categorical variable at the state level that captures the extent to which state-level legal framework deviate from federal-law related to four categories: discrimination, workplace hostility, sexual harassment, and retaliation. To do so, I hand-collect the extent of legal deviations for each category.

Department size is determined by the number of publishing researchers in a given year.

[10] More information about their methodology can be found here: `https://www.shanghairanking.com/methodology/arwu/2024`

The ideal measure for department size would be based on administrative data. In the absence of that, I use the number of active scholars in the department as a proxy for department size. I chose this approximation for department size because the sample in this study centers around research-focused universities. Therefore, scholars in these institutions are typically expected to maintain active research agendas as a core component of their responsibilities. This makes the number of publishing researchers a reasonable proxy for overall department size, as non-publishing faculty members are likely to be rare exceptions rather than the norm in such settings. Additionally, this measure captures the intellectually active portion of the department, which is particularly relevant when examining research productivity and scholarly output.

A future iteration will approximate the number of employees by capturing the universe of published researchers for each university department and use their first and last publications under the respective university affiliation as the duration of employment. If an individual publishes only once, then I will assume they stayed at the department for that given year. Data collection for this alternative measure is currently in progress.

3 Empirical Strategy

To estimate the causal effect of publicly exposed sexual misconduct incidents on academic department outcomes, I implement a staggered Difference-in-Differences (DiD) design. My approach incorporates Coarsened Exact Matching (CEM) as a preprocessing step to improve covariate balance before estimating treatment effects, allowing for causal inference under more credible assumptions. This section addresses empirical challenges, the matching design, and the regression specifications.

Departments experience the two "treatments" — occurrence of the misconduct and public reporting of it, respectively — at different points in time. The treatment effect is identified by comparing outcomes in treated departments before and after exposure, relative to matched control departments that have not yet been treated or will never be treated.

A department is classified as a valid control in a given treatment window if it did not experience a public sexual misconduct case within a symmetric 5-year window around the treatment year. I conduct robustness checks that tighten this requirement in two ways: (i) requiring that no other department in the same university was treated during the window, and (ii) restricting controls to departments in universities that are never treated.

The treatment in my setting is not randomly assigned. For a case to become public, it requires either involved parties that are willing to file a lawsuit or go public or local or national news media that is motivated and able to obtain evidence. This is more likely the case for sexual misconduct cases that are more severe or persistent and take place at public universities where information is easier to access, for example via freedom of information requests. To account for potential differences in observed covariates, I use a multi-stage coarse exact matching approach.

This multi-stage coarse exact matching approach directly addresses several causal challenges. First, the CEM pre-processing helps mitigate bias by ensuring that treatment and control departments are balanced on key covariates that might drive treatment effect heterogeneity. Second, by pre-processing data through CEM before parametric analysis, I reduce model dependence and sensitivity to specification choices (Ho et al., 2007). The matching procedure creates more comparable treatment and control groups, addressing the fundamental concern that departments experiencing misconduct incidents may systematically differ from those that do not.

Additionally, the inclusion of university x department fixed effects (ψ_u) in all specifications provides additional protection against potential confounding from institution-level factors. By differencing out all time-invariant university department characteristics, as well as any university-wide shocks that affect all university departments equally, this approach isolates the department-level impact of sexual misconduct incidents. This is particularly important given that universities may differ systematically in their reporting procedures, institutional cultures, and responses to misconduct cases. The university department fixed effects ensure that our estimates reflect the causal impact of misconduct at the department level rather than capturing university department-level heterogeneity.

Overall, I estimate the average treatment effect (ATE) at the department level. These estimates capture the collective impact on all department members, including both direct effects on those most immediately affected and any spillover effects on their colleagues.

The nature of my comparison group leads me to estimate a lower-bound effect when examining the impact of occurrence of sexual misconduct. This is because I compare departments where sexual misconduct took place and became public to a mix of departments where sexual misconduct did not take place and departments where sexual misconduct took place but did not become public. I probe robustness to my results with a set of empirical and statistical tests outlined in Section C.

My baseline model will be a simple difference-in-difference estimator which I will build out into a staggered Difference-in-Difference design (often called two-way fixed effects (TWFE)). Recent methodological developments allow me to address empirical challenges associated with staggered Difference-in-Difference design (de Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021; Baker, Larcker and Wang, 2022). Specifically, standard TWFE estimators can produce biased estimates in settings with heterogeneous treatment effects over time because they implicitly use already-treated units as controls for newly-treated units. This creates a weighted average of treatment effects where some effects receive negative weights, potentially leading to estimates with incorrect signs or magnitudes. Because my data has treatment points that span across over 40 years, likely creating heterogeneity in treatment effects across cohorts, and I use coarsened exact matching, my preferred specification uses the Sun and Abraham (2021) estimator. This estimation method handles staggered Difference-in-Difference designs with treatment effect heterogeneity across cohorts and allows for matching weights.

To account for uncertainty across all stages of analysis, I employ cluster-robust standard errors at the university department level, addressing potential correlation in outcomes within departments over time.

3.1 Stage 1: Coarsened Exact Matching (CEM)

I employ a multi-stage approach that combines preprocessing through Coarsened Exact Matching (CEM) (Iacus, King and Porro, 2011, 2012) with subsequent regression-based analyses. This approach pairs departments that experienced sexual misconduct incidents with similar departments that did not experience such incidents based on pre-specified covariates. One of the key advantages of this approach is that it reduces model dependence by first addressing selection bias through matching before conducting parametric analysis (Ho et al., 2007). Note that for simplicity, I use *publication* as the outcome variable Y moving forward.

In the first stage, I implement CEM to create appropriate comparison groups by matching departments that experienced sexual misconduct incidents with similar departments that did not. To satisfy the conditional independence assumption, I match on covariates that simultaneously predict both the likelihood of experiencing sexual misconduct incidents or the public reporting of it and departmental research productivity.

Specifically, I match on research discipline, legal framework at the state level, department size, and research intensity. These covariates block the main confounding pathways between misconduct occurrence and productivity. For example, larger departments have both more opportunities for misconduct incidents and greater research output By matching on these variables, I ensure that treated and control departments differ primarily in their treatment status rather than in other characteristics that could confound the relationship.

Research Discipline. Research disciplines strongly influence publication patterns (due to field-specific characteristics and norms). Thus, I stratify the matching process by research discipline. I use Clarivate's primary nine research categories defined in its Core Collection, where each of the 142 disciplines of the treated dataset is linked to one primary research discipline.

Legal Framework at the State Level. Institution's behavior is likely influenced by the law they are governed by. To account for that, I construct a categorical variable at the state level that captures deviation of state-level legal framework compared to federal-law related to discrimination, workplace hostility, sexual harassment, and retaliation. To do so, I hand-collect the legal deviations and classify each state by the extent to which it deviates from federal law. I produce three coarsed strata: States compliant with federal law, States that provide minor expansions compared to federal law, States that provide wider or significant expansions compared to the federal law.

Research Intensity. The research intensity at a given university is assigned using the ShanghaiRanking's *Global Ranking of Academic Subjects (GRAS)* which contains rankings of universities in 55 subjects across Natural Sciences, Engineering, Life Sciences, Medical Sciences, and Social Sciences (Shanghai Consultancy, 2024).

Stage 2: Staggered Difference-in-Difference Estimation

Analysis 1: Overall Treatment Effect Analysis Using the matched sample from Stage 1, I estimate the overall effect of sexual misconduct incidents on department productivity. I apply the following weighted regression model to the matched data:

$$Y_{it} = \alpha_i + \lambda_t + \beta_1 Post_{it} + \tau(Treat_i \times Post_{it}) + \gamma DeptSize_{i,t-7} + \varepsilon_{it} \tag{1}$$

where Y_{it} is the number of publications for university department i at time t, α_i are university department fixed effects, λ_t are time fixed effects, $Treat_i$ is a binary indicator equal to 1 for departments that experience a misconduct incident (and 0 otherwise), $Post_{it}$ is a binary indicator equal to 1 for periods t after department i experiences a misconduct incident or the public reporting of it (and 0 otherwise), τ is the coefficient of interest representing the causal effect of sexual misconduct exposure on departmental productivity, γ controls for lagged department size, and ε_{it} is the error term.

Standard errors are clustered at the university x department level, and control university x departments are weighted using the CEM stratum-specific weights:

$$w_i = \begin{cases} 1 & \text{if } i \text{ is treated} \\ \frac{m_T^s}{m_C^s} & \text{if } i \text{ is a control unit in stratum } s \end{cases} \tag{2}$$

where m_T^s is the number of treated units in stratum s and m_C^s is the number of control units in stratum s.

I will report the average treatment effect over the five post-treatment periods for which I will take the simple average of τ (Equation 3) with a standard error of the mean of the post-treatment coefficients. I will then continue with reporting the full event study in standard event-study plots.

$$\bar{\tau} = \frac{\sum_{t=1}^{5} \tau_{it}}{5} \tag{3}$$

Analysis 2: Heterogeneity Analysis by Gender Continuing with the matched sample, I investigate how the impact of misconduct incidents varies across different demographic groups within departments. I analyze:

1. **Gender**: Separate analyses for publications by women and men department members.

For each subgroup analysis, I estimate:

$$Y_{it}^g = \alpha_i^g + \lambda_t^g + \beta_2^g Post_{it} + \tau^g (Treat_i \times Post_{it}) + \gamma^g DeptSize_{i,t-7} + \varepsilon_{it}^g \tag{4}$$

where Y_{it}^g represents the productivity measure for subgroup g in university department i at time t, τ^g captures the causal effect of sexual misconduct exposure specific to subgroup g, and γ^g controls for department size.

3.2 Staggered Event Study Design

To investigate the time-dynamic of the treatment effect, I estimate an event-study specification:

$$Y_{it} = \alpha_i + \lambda_t + \sum_{k \neq -1} \beta_k \cdot \mathbb{1}(r_{it} = k) + \varepsilon_{it} \tag{5}$$

where $r_{it} = t - T_i$ is time relative to the treatment year. The year prior to the treatment is the omitted category ($k = -1$). I plot the coefficients β_k to visually assess pre-trends and time-dynamic treatment effects. My preferred specification uses the Sun and Abraham (2021) estimator since it allows for matching weights and takes into account heterogeneous treatment effects over time.

4 Results

To start, I describe the sexual misconduct cases that take place in research-focused universities, are committed by faculty, and become public. These misconduct cases take place in all disciplines of academia, from Arts & Humanities (34.8% of all cases) and Social & Behavioral Sciences (26.2%) to Engineering, Computer & Technology (4.2%) or Business (1.1%). The cases also range in severity. Out of all 359 misconduct cases, 132 involved sexual assault, 228 involved sexual harassment, 55 involved sexual advances, and 101 involved sexualized comments. Note, that these categories are not mutually exclusive and that several cases involve multiple types of sexual misconduct. These actions are mostly taken by tenured faculty (in 67% of the cases).[11] In over half of the cases, there is a repeated offender. Specifically, 12.5% of the cases, the accused faculty receives two allegations. In 39.8% of the cases, the accused faculty commits sexual misconduct three or more times.

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Table 1: Characteristics of Sexual Misconduct Cases in University Departments
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The search of publications for the 5,076 universities from 1980 until 2024 yields a total of 1,374,089 publications and 7,820,515 authors. Gender could be assigned to 80.26 % of the

[11]Note, that this number is likely slightly upward biased since in some cases, it was unclear whether the stated rank was at time of misconduct or time of reporting.

authors. Out of all authors, 56.49% are algorithmically determined to be men and 23.78% are determined to be women. The remaining authors (19.74%) either carry a gender-ambiguous name or have a name that is unknown to the database. This includes authors for whom we only have the first initials.

Table 2 shows the summary statistics of the treated and the control departments in the year used for the matching. After matching, there are 201 universities in the treatment department and 1,982 universities in the control department. The two groups look descriptively and unconditioned similar when we compare their legal environment. What is noticeable, however, is that group of treated departments consists of a higher share of public universities (73.63% versus 46.92%), a higher level of total publications (average 61 versus 18), and more department members (average 90 versus 31).

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Table 2: Summary Statistics of Treated and Control Departments

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The Coarsened Exact Matching (CEM) reduces the differences between the treated and the matched control university departments noticably from a multivariate L1 distance of 0.71 to 0.5 as shown in Table A2. The L1 distance is reduced by 54% for *discipline* and reduced to virtually zero for the type of institution (public or private), the matching based on state laws for hostility and harassment, and the Shanghai ranking. The L1 distance for department size is improved with a 67% reduction, though some imbalance remains.

Table A1 shows that control departments have higher baseline publication rates than treated departments both before (2.18 vs 1.53) and after matching (2.98 vs 1.51). This persistent difference in outcome levels does not compromise identification because the matched control departments serve as valid counterfactuals for treated departments despite different productivity levels. The identifying assumption is that matched departments would follow parallel trends absent treatment, not that they have identical baseline outcomes. The difference-in-differences design with department fixed effects eliminates time-invariant level differences, identifying the causal effect through within-department variation in productivity following misconduct incidents.

Next, I turn to the main results. Recall, that I simultaneously study the effect of occurrence of sexual misconduct and the effect of public reporting of sexual misconduct on scientific production. Figure 4 plots the results of the former in an event study format. The overall finding is that occurrence of sexual misconduct has no discernible effect on the average number of publications per department member. Column 1 uses the full sample. These cases will be publicly reported on later. The average time that passes between the occurrence and public reporting of misconduct is 7 years and the median is 3 years, the full distribution is shown in Figure 5a. This means that these results include potential productivity effects of the public disclosure of sexual misconduct. To probe robustness into my results, I remove all incidents where public reporting occurred within the five-year window of this analysis. Note, that this subsample analysis presents a tradeoff. While we have a cleaner estimation since there will be no public reporting events within the five years of this analysis, we are likely selecting on a type of incidence. Sexual misconduct incidents that take longer to become public may be more complex and thus require a longer time window for the investigation. They may also be more severe. The result is robust to this subsample as presented in Figure 5b.

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Figure 4: Effect of Occurrence of Sexual Misconduct on Average Number of Publications per Department Member

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Figure 5: Effect of Occurrence of Sexual Misconduct on Average Number of Publications per Department Member (Restricted to cases where it didn't get publicly reported on within five years after occurrence

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Estimation of the effect of the occurrence of sexual misconduct on productivity may be attenuated toward zero due to potential contamination in the control group—departments that experienced unreported misconduct would dilute any true negative effects. Therefore, there is a possibility that the true effect may be non-negligible. I address this through both empirical and statistical approaches.[12] First, I create a control group in which we would expect a lower level of contamination by restricting to departments with above-median representation of women for a given discipline at time $t-1$, since research shows that higher representation of women is associated with decreased likelihood of sexual misconduct occurrence (Mansfield et al., 1991; Welsh, 1999; Folke and Rickne, 2025; Subramani and Gorbatai, 2025). Figure 6 shows that even with this refined control group, I find no discernible effect of sexual misconduct occurrence on departmental productivity.[13]

Second, I conduct statistical exercises to assess the plausibility of meaningful negative effects being masked by contamination. This approach is in spirit of Manski (2003), Altonji, Elder and Taber (2005), and Oster (2019), who consider how selection into control or treatment affects stability of the observed treatment effect. I calculate the contamination levels required to reconcile the observed effect of 0.022 with a meaningfully negative true effect. I define a meaningful negative effect as -0.056 publications per faculty-year, which represents one full publication per year in a median-sized department of 18 faculty members.[14] Using the contamination adjustment formula True Effect = Observed Effect$/(1-c)$, where c represents the contamination rate, I find that even accounting for statistical uncertainty, contamination would need to exceed 59% of control departments for such an effect to be plausible. This would imply a total prevalence of sexual misconduct across all departments of at least 64%—far exceeding the 38% documented by Boudreau et al. (2023).

Beyond this contamination bounds analysis, I conduct additional robustness tests examining four scenarios that vary both contamination levels (10% vs. 29%) and true effect sizes (negligible vs. meaningfully negative). These exercises, detailed in Appendix C, consistently support the interpretation that the observed null effect likely represents a genuine absence of productivity impacts. While I cannot definitively rule out all alternative explanations, the convergence of evidence—the null finding with a refined control group, requiring implausibly high contamination rates for meaningful effects, modest adjusted effects under realistic con-

[12]Note, that this is not pre-registered.

[13]While we still have a null effect, we can observe that the sign flips from positive to negative. This is in line with my prior that the effect may be negative but likely limited, outlined in Section 1.

[14]My rational is that one publication is large enough to show up in the statistics of a department.

tamination, and statistical rejection of scenarios with meaningful negative effects—suggests that sexual misconduct has negligible effects on departmental research productivity at the occurrence stage.

Most victims of sexual misconduct in academia are women. Informal information flow via whisper networks may suggest that there are sorting and disruptions that may not be gender neutral. Specifically, it may be possible that women adjust their work patterns in some form, for example via collaboration networks, as suggested by Gertsberg (2022). This, in turn may have downstream consequences on their productivity. Therefore, Column 3 and 4 of Table A3 report the results split by gender. The estimates are comparable and I don't observe a noticeable difference on women (nor men).

There may be particular instances where productivity may be dampened to a notable extent, however. For example, some organizational regimes allow for faculty to shift away from their core activity, research Cortina and Berdahl (2008). As an exploratory analysis,[15] I analyze whether withdrawal from research activities can be observed in departments where switching to non-research activities are easy and costless in the sense that it is not effortful and does not necessarily come with a financial penalty. To explore this possibility, I split the sample into two groups. The first group consists of departments where switching is easy and costless. This includes departments in clinical medicine and professional departments such as architecture, law, physical therapy, or veterinary sciences. The second group consists of departments where it is hard to remain on the faculty and switch away from research activities. Examples include biology, astronomy, chemistry, or education.

Figure 7 presents the results. Panel 7a reports the event study estimates for departments where the switch away from research is easy. Panel 7b reports the event study estimates for departments where the switch away from research is relatively hard. While there remains no discernible effect on the research output for departments, where it is hard to move away for research, I document significant withdrawal in departments where withdrawal is possible. On average, a department member in this category reports 0.12 fewer publications for the five

[15]In that this analysis is not pre-registered.

years after a sexual misconduct occurs, the p-value is below 0.01. As Panel 7a shows, the effect size is somewhat increasing over time, starting at -0.07 in the year of the event, and reaching -0.18 in the fifth year after the event.

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Figure 7: Comparison of Departments with Varying Degrees of Ability to Switch
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The main findings of this first set of analyses reveal a null effect: the occurrence of sexual misconduct has no discernible impact on average departmental publication rates, whether examining all faculty, women, or men separately. Appendix Section C thoroughly probes our confidence in this null effect, addressing potential contamination in the control group and confirming that the negligible productivity effects are genuine. Having said that, there is a limited set of departments — perhaps a form of edge case — where I do find some noticeable decrease in productivity. This occurs specifically in departments where it is relatively easy for faculty to shift away from research activities, such as clinical medicine and professional schools, where affected departments show a decline of the five years following the occurrence of sexual misconduct. Taken together, occurrence of sexual misconduct has no discernible effect on organizational productivity.

Next, I turn to the next set of analyses which look at the effect of public reporting of sexual misconduct on university department productivity. Figure 8 shows the plot of the event study. The plot shows that the public reporting of sexual misconduct has a negative effect on the average number of publications per department member. My preferred estimate combines Coarsened Exact Matching weights with cohort-weights to adjust for heterogeneous treatment effects. It shows that sexual misconduct has an overall negative effect on the number of publications per department member, ranging from -0.04 to -0.13 publications per year. In other words, over the course of five years after the misconduct case becomes public, for the median department of 18 faculty members, that results in nine fewer publications five years after the incident becomes public compared to its comparison departments. Figure A5 shows that this result is robust to alternative specification.

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Figure 8: Effect of Public Reporting of Sexual Misconduct on Average Number of Publications per Department Member

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The result is robust to various approaches to difference-in-difference design, as Table A5, columns one to five show. Specifically, I start with an analysis in the form of a simple difference-in-difference design. Column 1 reports the average treatment effect over the five years following the public reporting of the sexual misconduct incident. Column 2 reports the same estimates but in the form of an event study. The average annual treatment effect of the five years after the public reporting of sexual misconduct is -0.04 publications per department member. The p-value is below the common alpha threshold of 0.05. This compares to an annual treatment effect ranging from -0.01 to -0.07 when we look at a simple event-study in column 2.

The estimates based on the matched sample are qualitatively similar but noticeably larger. Column 3 reports the average treatment effect over the five years following the public reporting of the sexual misconduct incident. Column 4 reports the same estimates but in the form of an event study. Column 5 reports the matched event study using the Sun and Abraham (2021) estimator. The average annual treatment effect of the five years after the public reporting of sexual misconduct is -0.1 publication per department member. This estimate is directionally significant. This compares to an annual treatment effect range from -0.05. to -0.14 and from -0.04 and -0.13, respectively, when we look at the event study estimates in column 4 and 5 of Table A5. The estimates from the event study suggest that the productivity effect appears over time, with the highest decrease in productivity in year three.

The effect varies somewhat for men and women department members. Table A6 shows that men who were members of a department where sexual misconduct occurred and was publicly reported on write 0.11 (p-value <0.10) fewer publications per year, on average, compared to men in comparable departments without publicized sexual misconduct case. For women, the estimated effect is -0.06 (p-value <0.05).

5 Discussion and Conclusion

Through an empirical investigation of the occurrence and public reporting of sexual misconduct incidents, I demonstrate that sexual misconduct incidents bear organizational consequences and affect organizational productivity negatively. My staggered difference-in-difference analysis of 347 sexual misconduct incidents reveals nuanced effects on departmental research output. The occurrence of sexual misconduct in university departments has no discernible effect on the overall productivity in research output. However, professional departments, such as law, architecture, or clinical medicine — where switching to non-research activities is relatively easy and without financial penalty — experience a decline in research output following such incidents. Moreover, the public reporting of sexual misconduct precipitates a substantial decline in scientific production that persists for five years. These findings align with theoretical predictions that sexual misconduct may impact organizational performance through two distinct pathways: the occurrence of incidents through employee sorting and internal disruptions, although this impact may be limited due to information barriers, and public disclosure through sensemaking disruptions, stigmatization, and external stakeholder distancing.

These estimates should be interpreted as intention-to-treat effects that capture department-wide impacts rather than individual-level consequences for directly affected parties. This ITT approach reveals that sexual misconduct incidents create organizational externalities extending well beyond victims and perpetrators, affecting the productivity of entire academic units. While individual victims undoubtedly experience severe personal and professional consequences, my results demonstrate that the organizational costs are substantially broader, providing an economic rationale for institutional investment in prevention and response systems.

Implications for Research on Organizational Misconduct

Finally, this study contributes to the organizational misconduct literature. A large body of work has examined publicly revealed misconduct. Recent examples include the investigation of the Nyaga stockbrokerage fraud and its effects on subsequent market participation (Yenkey, 2018), the spillover effects on teammates and managers of publicly known doping cases (Yenkey and Palmer, 2025), and the study of child abuse scandals on the engagement in rites (Stroube and Zavyalova, 2024). For a more holistic understanding of organizational misconduct and its implications for organizations, we need to study different points in time and study these within-misconduct events simultaneously.

This study demonstrates that misconduct affects organizations not only upon public revelation but also at the time of occurrence, with distinct mechanisms operating at each stage. I find that these two points in time involve different channels through which they operate in an organization. I theorize that the occurrence of sexual misconduct triggers sorting mechanisms, as organization members reallocate their efforts, while public reporting activates cognitive processes and reputational management strategies. By examining misconduct at both temporal points simultaneously, this study provides a more nuanced understanding of the organizational sociology and economics of misconduct.

This dual-temporal approach challenges existing theoretical frameworks that treat misconduct as a single-point phenomenon. By demonstrating that organizational responses operate through fundamentally different mechanisms at the time of occurrence versus revelation, this study opens new avenues for understanding how organizations respond to internal crises more broadly. The findings suggest that theories focusing solely on post-revelation effects may miss critical organizational dynamics that shape long-term outcomes.

Moreover, the approach employed in this study—capturing both occurrence and revelation effects within the same organizational context—offers a methodological framework that could be applied to other forms of organization misconduct or crises. This within-event temporal analysis provides researchers with tools to examine the full lifecycle of organizational disruptions, rather than focusing on single moments in time.

These findings have important implications for regulatory approaches, internal governance structures, and industry-wide standards for handling misconduct. The evidence that misconduct triggers immediate sorting effects suggests that organizations and regulators should develop monitoring systems capable of detecting behavioral changes even before misconduct becomes public. Furthermore, the distinct mechanisms operating at occurrence versus revelation indicate that organizations need differentiated response strategies—immediate interventions to address sorting behaviors and longer-term strategies focused on cognitive and reputational management.

These findings inform both theoretical understanding and practical management of organizational misconduct. For scholars, they highlight the importance of temporal sequencing in misconduct research and suggest that single-event studies may underestimate the full organizational impact. For practitioners, they underscore the need for proactive monitoring systems that can detect behavioral changes even before misconduct becomes public, and for developing differentiated response strategies that address both immediate sorting effects and longer-term reputational concerns.

Implications for Research on Sexual Misconduct in the Workplace

A core implication of this research is that sexual misconduct affects organizations more broadly than previously recognized. Sexual misconduct remains a pervasive issue in organizations. A robust body of literature examines how workplace sexual misconduct affects victims and other directly involved individuals (Schneider, Swan and Fitzgerald, 1997; Sims, Drasgow and Fitzgerald, 2005; Cortina and Berdahl, 2008; McLaughlin, Uggen and Blackstone, 2017; Adams-Prassl et al., 2024). The question of its effects on organizations more broadly is not well understood.

This study extends this important work by demonstrating that sexual misconduct incidents bear organization-wide consequences. Although no discernible effects on overall productivity emerge at the time of occurrence, I find that sexual misconduct incidents meaningfully influence organization members' decisions to shift away from research activities when alternative activities are available. These findings contrast with the outcomes when sexual misconduct incidents become public, which substantially reduces overall productivity for five years following the announcement.

These findings inform ongoing debates about organizational responsibilities and strategies as society demands increased transparency and accountability in workplace sexual misconduct cases. While organizations, stakeholders, policymakers, and scholars debate interventions and policies such as sexual harassment training or non-disclosure agreements, the evidence from the organizational perspective is lacking.

This work suggests that organizations taking a legal perspective on how to mitigate and handle sexual misconduct incidents might find themselves in a double bind where they have to balance responding to calls for increased transparency while being required to protect the privacy of all parties involved against the organizational costs arising from public discourse about sexual misconduct incidents. Although only suggestive, this work offers empirical evidence that the costs of publicly reporting sexual misconduct incidents may be an important channel through which sexual misconduct risks shift from an HR issue to a strategic one.

Implications for Research on Science of Science

A substantial body of work examines factors that shape the rate of scientific production. These include institutional factors such as tenure status (Tripodi et al., 2025), resource-based factors like access to frontier science (Iaria, Schwarz and Waldinger, 2018) and industry partnerships (Bikard, Vakili and Teodoridis, 2019), or moves between universities of different ranks (Deville et al., 2014), and combinatorial concerns via interdisciplinary collaborations (Leahey, Beckman and Stanko, 2017). This study extends this literature by demonstrating that social processes, specifically sexual misconduct, also shape scientific productivity.

This contribution builds on emerging research examining how heightened societal awareness of sexual misconduct affects scientific production. Recent work has shown that the #MeToo movement alters the collaboration structures for women (Gertsberg, 2022) and that accusations of harassment affect collaboration patterns and recognition for accused faculty members (Maimone et al., 2025; Widmann, Rose and Chugunova, 2022). I advance this conversation by shifting the analytical focus from individual-level effects to department-level organizational disruptions, examining how sexual misconduct incidents reverberate throughout entire academic units—using a novel methodological approach that captures effects both at the time of incident occurrence and upon public revelation. I show that sexual misconduct incidents influence scientists' decisions to reallocate effort away from research when organizational structures permit such transitions and create department-wide productivity disruptions that extend far beyond the immediate parties involved.

The theoretical implications extend beyond the immediate context of sexual misconduct. By documenting how disruptions related to organizational climate translate into measurable changes in scientific output, this study contributes to a broader understanding of how social dynamics within academic institutions affect knowledge production. The findings reveal that factors traditionally considered peripheral to scientific productivity — such as organizational climate, interpersonal dynamics, and workplace disruptions — may play more central roles in shaping research outcomes than previously recognized.

More broadly, this study contributes to debates about gender and science by highlighting an indirect pathway through which gender inequality may hinder scientific progress. The underrepresentation of women in academic departments — a documented risk factor for sexual misconduct — may create conditions that ultimately reduce overall scientific productivity through the spillover effects documented here, suggesting that gender equality and the effort to mitigate cases of sexual misconduct from happening are relevant both for universities and society.

Misconduct More Broadly

An intriguing question that emerges is whether sexual misconduct is conceptually equivalent to other forms of organizational misconduct, such as financial fraud (Kang, 2008; Stuart and Wang, 2016; Wang, Stuart and Li, 2021), doping (Yenkey and Palmer, 2025), or scientific fraud (Furman, Jensen and Murray, 2012; Lu et al., 2013; Azoulay et al., 2015; Gross, 2016; Azoulay, Bonatti and Krieger, 2017). There are reasons to believe that they are similar and comparable. For example, when becoming public, they oftentimes operate as a scandal and inflict negative stigma on organizations (Hudson, 2008). However, as Greve, Palmer and Pozner

(2010, p. 54) note, “acts are labeled as misconduct whenever they are harmful or morally objectionable,” suggesting that there may be more nuance to organizational misconduct than currently acknowledged and that further evidence may be needed to answer this question.

For example, there are reasons to believe that misconduct may relate to organizational performance and reward structures in distinct ways. Both financial fraud and scientific fraud, for example, could be understood as, in part, corrupted forms of performance enhancement. While they are associated with an apparent increase in organizational performance, they potentially exploit legitimate organizational activities to secure personal gains like bonuses, promotions, funding, or status. However, these fraud types may differ in their systemic impact: financial fraud might produce more contained, short-term effects, while scientific fraud could redirect entire research fields and shape the future direction of scientific inquiry for decades.

Sexual misconduct might operate through somewhat different mechanisms. Rather than representing a corrupted version of normal organizational processes, sexual misconduct might involve the abuse of organizational power structures to inflict direct interpersonal harm. That is, the offender isn’t simply transgressing in core organizational tasks—they are actively exploiting institutionally legitimized hierarchies and authority as tools to cause harm to others. The benefits to perpetrators may be unrelated to — and often counterproductive to — organizational performance metrics.

These potential distinctions suggest that sexual misconduct may be conceptually different from other forms of organizational misconduct, despite sharing certain organizational consequences such as creating scandals and reputational damage. While all forms of misconduct violate organizational norms, the underlying mechanisms, motivational structures, and relationships to organizational performance may vary substantially across misconduct types. Future research should systematically examine these potential conceptual differences to determine whether misconduct types require distinct theoretical and practical approaches.

Limitations and Future Research

As with all empirical work, this study operates within certain data constraints. The sexual misconduct cases examined here are those that eventually became public, creating a sample conditional on later revelation. While a complete census of all incidents would be ideal, this study represents an important first step in understanding the temporal dynamics of organizational misconduct. By demonstrating distinct effects at occurrence versus revelation even within this constrained sample, the findings establish both the theoretical importance and empirical feasibility of examining these dynamics. Future research can build on this framework as data systems evolve to capture a broader range of incidents.

Furthermore, the intent-to-treat nature of my estimates may mask important heterogeneity in individual-level impacts. While I capture average department-level effects, the consequences for direct victims, accused faculty, and bystanders likely vary substantially. Future research with access to individual-level data could decompose these department-wide effects to better understand the distribution of impacts across different groups within affected organizations.

Moreover, several avenues warrant future investigation to refine our understanding of organizational misconduct. Systematic reviews of literature or empirical comparisons of conceptually distinct types of misconduct in similar contexts would provide valuable insights into the distinctiveness of the different variations of organizational misconduct. Moreover, while this paper advances our understanding of spillovers associated with sexual misconduct, the boundaries of these effects remain unclear. Particularly important is understanding how sexual misconduct affects junior talent and their career trajectories, and exploring its explanatory power for the gender underrepresentation in certain fields.

Another important avenue for future work concerns organizational responses to sexual misconduct. One open question remains: why do organizations often retain perpetrators rather than terminate them? Adams-Prassl et al. (2024) find that women supervisors are more likely to dismiss perpetrators, while Collis and Van Effenterre (2024) provide exploratory evidence that individuals averse to hostile environments preferentially sort into departments with women supervisors. Understanding these dynamics could inform more effective organizational policies for addressing sexual misconduct while minimizing productivity losses.

Lastly, we have a surprisingly sparse understanding of the process within an organization after a sexual misconduct incident occurs. Scholars and policy makers alike would greatly benefit from a richer understanding of the processes, activities, and consequences that the occurrence and processing of sexual misconduct has on organizations.

Practical Implications

This study reveals that sexual misconduct incidents create organizational disruptions that operate through distinct mechanisms at occurrence versus public revelation. While misconduct occurrence triggers selective behavioral responses in departments where alternative activities are available, public revelation creates widespread productivity declines that persist for years across entire academic units.

These findings challenge common assumptions about organizational responses to sexual misconduct. The evidence that public revelation — rather than the misconduct itself — creates the most severe and persistent organizational disruptions suggests that institutional concerns about transparency may be more complex than typically understood. Organizations facing calls for increased transparency may find themselves managing a fundamental tension. While ethical—and often legal—obligations require addressing misconduct, public disclosure creates substantial institutional costs that extend far beyond the immediate parties involved. This dynamic may help explain why organizations often appear reluctant to pursue complete transparency, as they balance moral imperatives against significant organizational consequences.

As organizations and institutions grapple with increasing demands for transparency and accountability, understanding these temporal dynamics becomes crucial for developing effective responses that address both immediate behavioral disruptions and longer-term reputational and productivity consequences. This study provides a foundation for such understanding while highlighting the substantial work that remains in building safer, more productive organizational environments.

Implications and Conclusion

This study reveals an important distinction in how organizations experience sexual misconduct. While the occurrence of misconduct shows no discernible effect on departmental productivity, public disclosure appears to trigger substantial declines—approximately 10 fewer publications over five years for a median-sized department. This finding suggests several considerations for organizational policy and practice.

First, the different mechanisms operating at occurrence versus disclosure indicate that protecting victims and maintaining productivity may benefit from differentiated approaches. Prevention efforts naturally focus on addressing initial misconduct, while the organizational disruptions following public revelation may require distinct management strategies. Policies that treat these phases as a single phenomenon might not fully address the complexity of these situations.

Second, these dynamics may create challenging organizational incentives. Legal frameworks such as EEOC procedures require confidentiality during investigations, which happens to align with avoiding the productivity declines documented here. Organizations may therefore face incentives to maintain confidentiality—adhering to legal protocols while limiting performance impacts—even when greater transparency might serve important accountability goals. This could help explain institutional tendencies toward limited disclosure despite increasing calls for openness.

Third, these findings suggest that efforts to increase accountability for sexual misconduct may be more complex than initially apparent. While stakeholders understandably seek transparency, and victims deserve support and justice, the evidence that public disclosure but not occurrence of sexual misconduct affects organizational costs suggests that resistance to openness may stem partly from performance concerns. Effective policy might therefore benefit from recognizing that organizations often navigate multiple considerations: legal requirements for confidentiality, ethical imperatives for transparency, and potential consequences for productivity.

These insights may inform the development of approaches that address misconduct while acknowledging the multifaceted challenges institutions encounter. Recognizing how public disclosure can transform misconduct from an HR matter into a broader organizational concern may help in designing reforms that advance the goal of creating safer, more accountable workplaces.

6 Figures and Tables

Figures

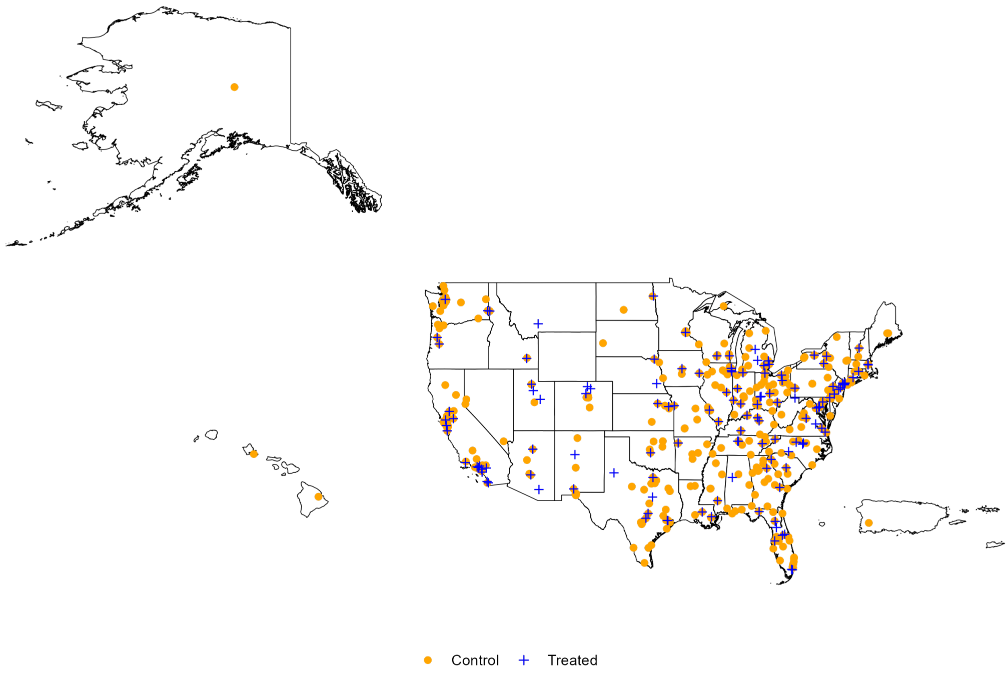
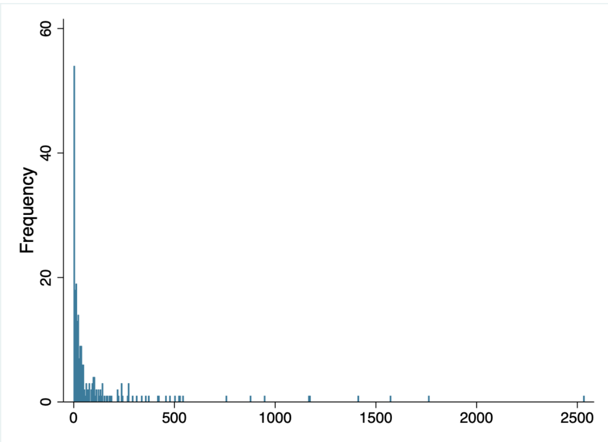
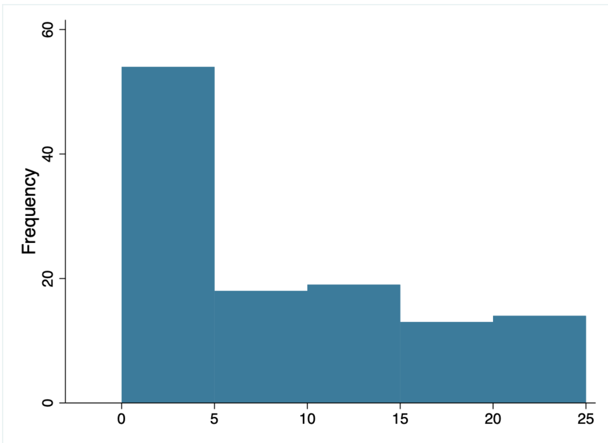


Figure 1: Geographic Distribution of Treated and Control Universities



(a) Distribution of publication count of treated departments



(b) Distribution of publication count of treated departments, restricted to the bottom 50 percentile

Figure 2: Histogram of publication count by department of treated departments at time period $t-1$ of the occurrence of sexual misconduct.

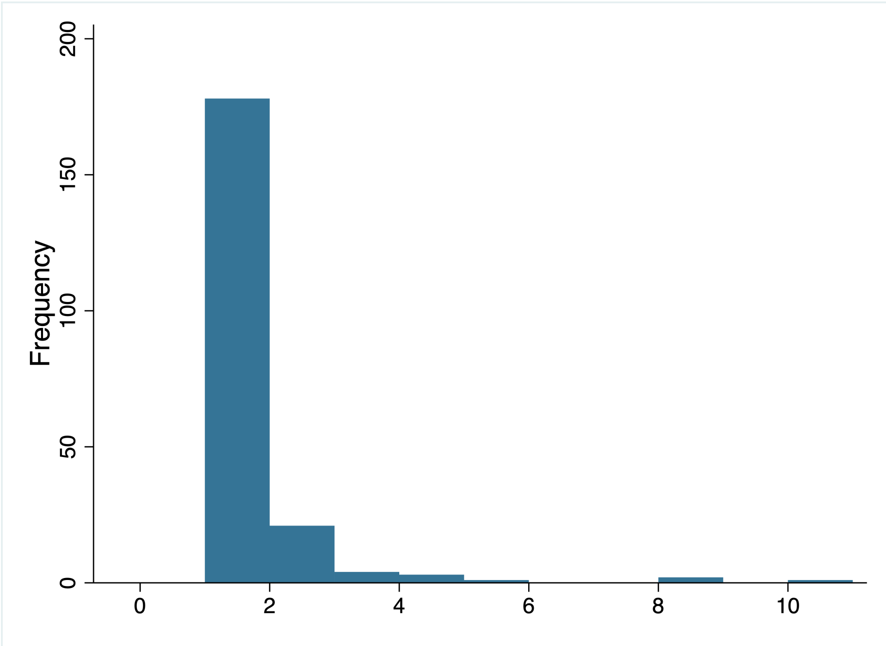


Figure 3: Distribution of average number of publications per department member at time period $t-1$ of the occurrence of sexual misconduct

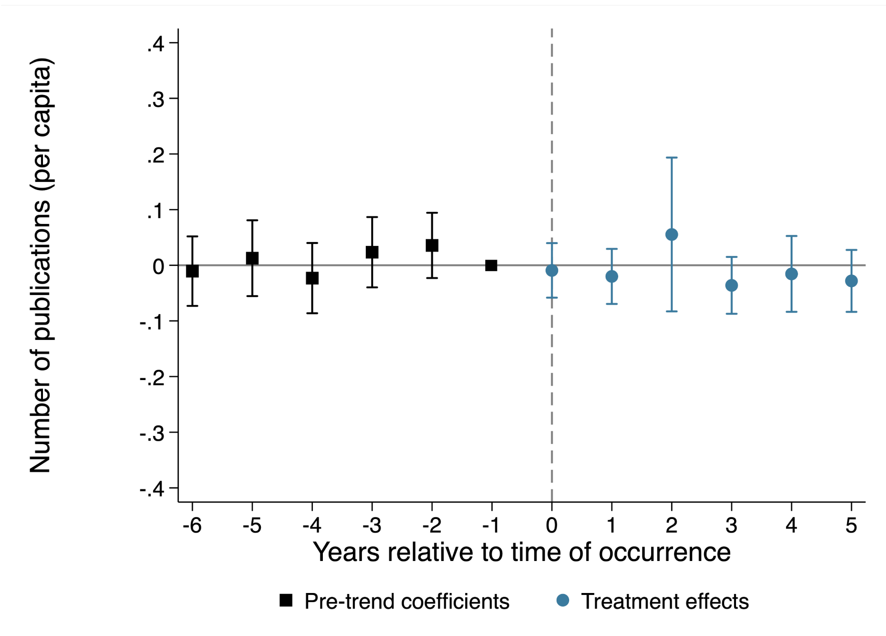
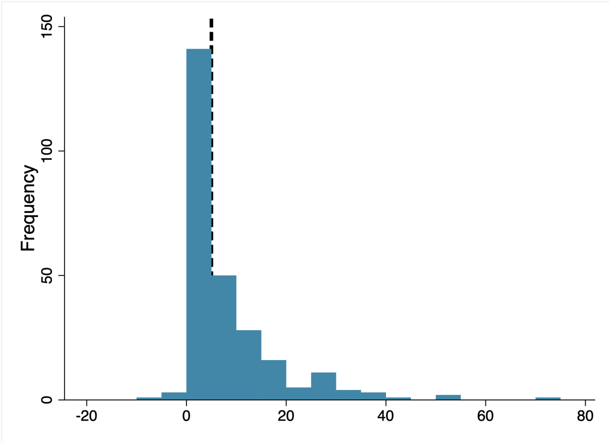
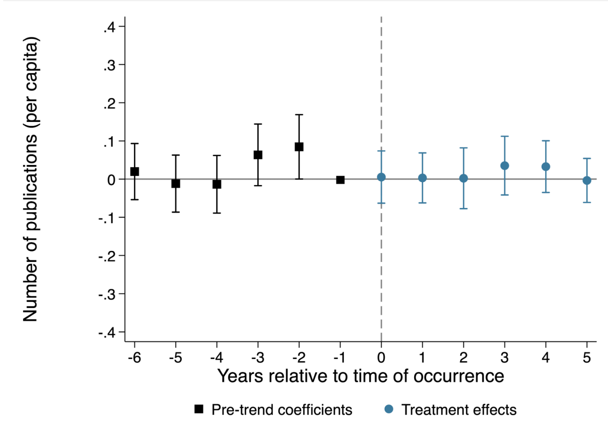


Figure 4: Effect of Occurrence of Sexual Misconduct on Average Number of Publications per Department Member

Note: The table output can be found in Table A3.



(a) Number of years passed between occurrence and public reporting s



(b) Estimation based on cases with > 5 delay in public reporting

Figure 5: Effect of Occurrence of Sexual Misconduct on Average Number of Publications per Department Member (Restricted to cases where it didn't get publicly reported on within five years after occurrence

Note: The table output can be found in Table A3.

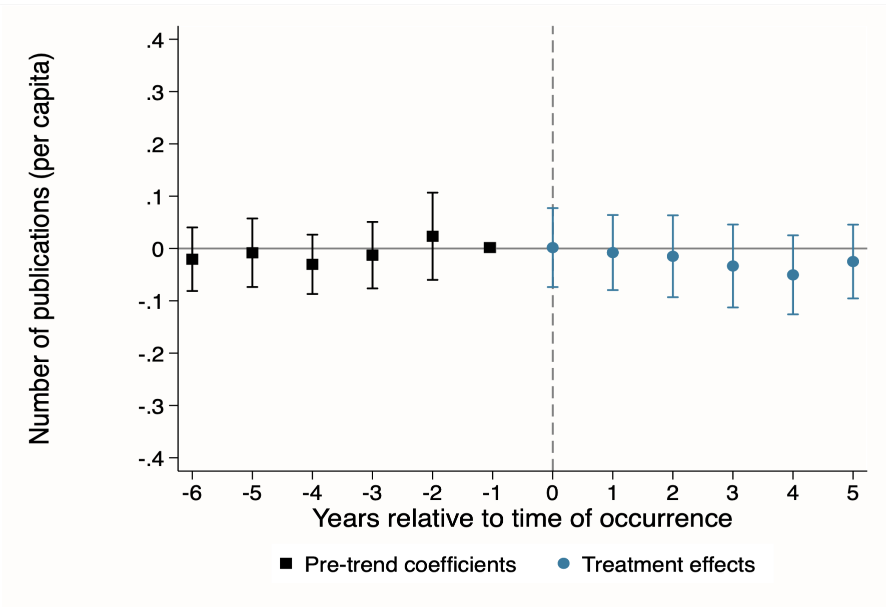
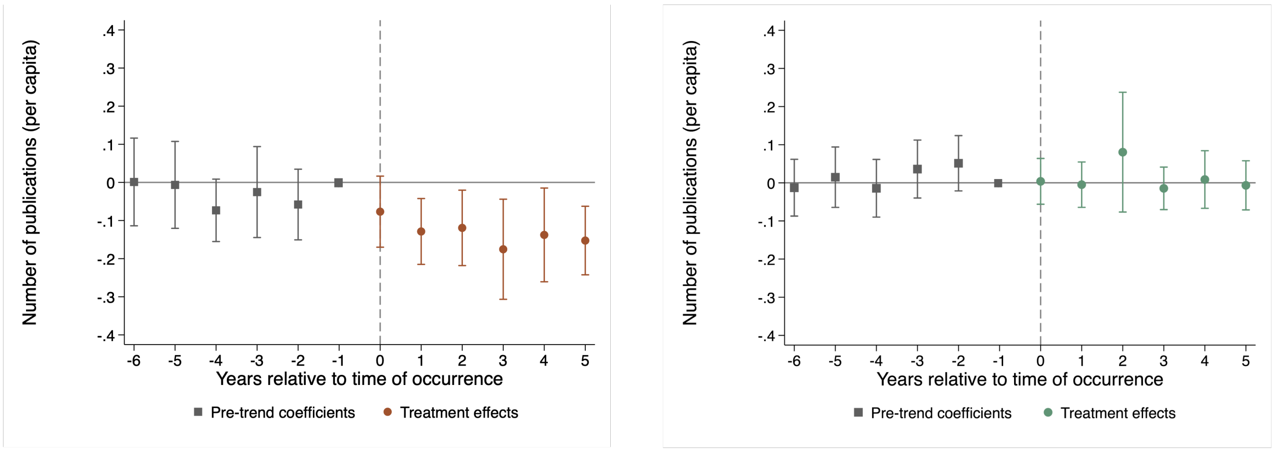


Figure 6: Effect of Occurrence of Sexual Misconduct on Average Number of Publications per Department Member (Control restricted to departments with representation of women above the median)

Note: The table output can be found in Table A3.



(a) Departments where it is easy to withdraw from research activities

(b) Departments where it is hard to withdraw from research activities

Figure 7: Comparison of Departments with Varying Degrees of Ability to Switch

Note: The table output can be found in Table A4.

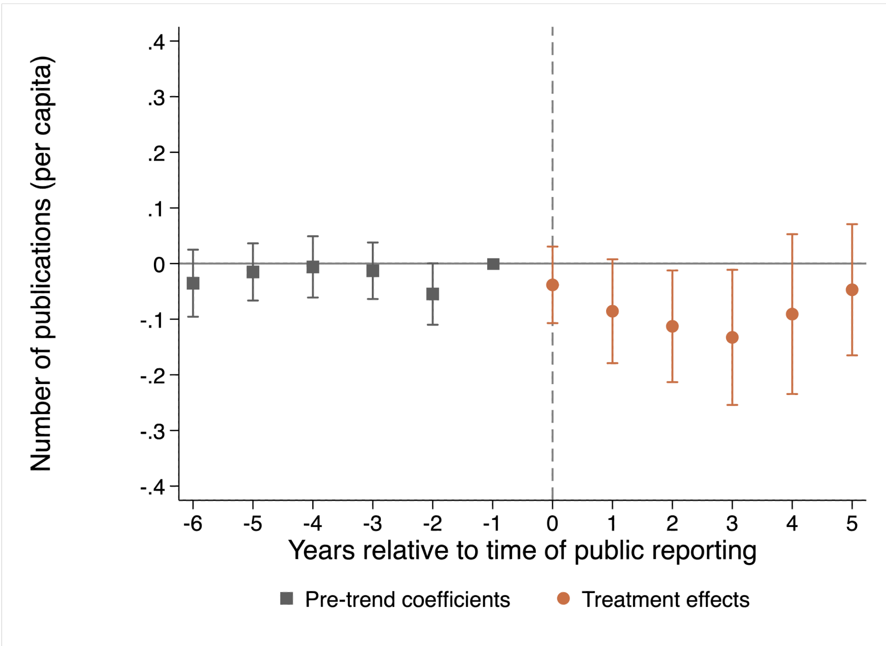


Figure 8: Effect of Public Reporting of Sexual Misconduct on Average Number of Publications per Department Member

Note: The table output can be found in Table A5.

Tables

Table 1: Characteristics of Sexual Misconduct Cases in University Departments

	Mean	Median	SD	Min.	Max.
Year of occurrence	2008	2010	9.65	1950	2025
Year of public reporting	2014	2017	7.27	1995	2025
Years of delay	6.96	3	8.80	0	74
Disciplines					
Agriculture, Biology & Environmental Sciences	7.26%	0	0.26	0	1
Arts & Humanities	31.68%	0	0.46	0	1
Business Collection	1.15%	0	0.11	0	1
Clinical Medicine	9.54%	0	0.29	0	1
Electronics & Telecommunication Collection	0.38%	0	0.06	0	1
Engineering, Computing & Technology	3.44%	0	0.18	0	1
Life Sciences	7.6%	0	0.27	0	1
Physical, Chemical & Earth Sciences	9.54%	0	0.29	0	1
Social And Behavioral Sciences	29.01%	0	0.45	0	1
Role/Position of Accused					
Assistant Professor	7.25%	0	0.26	0	1
Associate/Established/Tenured Professor	40.46%	0	0.49	0	1
Full Professor	29.39%	0	0.46	0	1
Emeritus	3.05%	0	0.17	0	1
Not Specified	19.85%	0	0.40	0	1
Type of misconduct					
Abuse of Power & Professional Misconduct	5.34%	0	0.23	0	1
Hostile Environment	5.73%	0	0.23	0	1
Retaliation	4.12%	0	0.20	0	1
Inappropriate Relationship	6.49%	0	0.25	0	1
Sexualized/Inappropriate Comments	3.82%	0	0.19	0	1
Sexual Harassment & Misconduct	26.71%	0	0.44	0	1
Stalking & Intimidation	5.34%	0	0.23	0	1
Sexual Violence & Assault	11.07%	0	0.31	0	1
Substance-related Harassment & Assault	0.76%	0	0.09	0	1
Number of allegations					
Once	35.88%	0	0.48	0	1
Twice	12.60%	0	0.33	0	1
Serial	41.22%	0	0.49	0	1
Unknown	10.31%	0	0.31	0	1
Role/Position of victim					
Peer (Faculty, Researcher, Colleague)	8.02%	0	0.27	0	1
Graduate or Postdoctoral Student	26.34%	0	0.44	0	1
Research or Teaching Assistant	2.29%	0	0.15	0	1
Undergraduate Student	3.05%	0	0.17	0	1
Student (level unknown)	44.27%	0	0.51	0	1
Other/Unknown	16.03%	0	0.37	0	1

Notes: Sample consists of 262 sexual misconduct cases at US research universities that became publicly known between 1980 and 2024 and could be matched to control departments. Years of delay represents the time elapsed between the occurrence of misconduct and its public reporting. Type of misconduct and role/position of victim categories are not mutually exclusive, as cases may involve multiple types and victims. Abuse of power & professional misconduct, hostile environment, and retaliation are coded for context but are not counted as sexual misconduct for the main analysis in this paper.

Table 2: Summary Statistics of Treated and Control Departments

	Mean	Median	SD	Min.	Max.
Control Departments (Never-treated)					
University Characteristics (N = 1,982)					
Public Universities	46.92%	0	0.50	0	1
State Law compared to Federal Law					
Compliant	43.90%	0	0.50	0	1
Slightly Expanded	23.36%	0	0.42	0	1
Significantly Expanded	32.04%	0	0.47	0	1
Department Characteristics (N = 1,038)					
Total publications	18.09	3	49.88	0	969
Articles Share	0.68	0.95	0.43	0	1
Department member count	30.61	1	5	0	2,129
Female member count	8.08	1	26.68	0	565
Male member count	16.71	3	54.42	0	1,178
Treated Departments					
University Characteristics (N = 201)					
Public Universities	73.63%	1	0.44	0	1
State Law compared to Federal Law					
Compliant	46.27%	0	0.50	0	1
Slightly Expanded	13.93%	0	0.35	0	1
Significantly Expanded	38.81%	0	0.49	0	1
Department Characteristics (N = 180)					
Total publications	60.96	26.5	98.53	0	742
Articles Share	0.89	0.97	0.21	0	1
Department member count	89.52	38.5	131.10	0	743
Female member count	26.91	13	41.90	0	313
Male member count	45.82	18	68.11	0	351

Notes: This table presents summary statistics for treated departments (those experiencing publicly reported sexual misconduct) and matched control departments. University characteristics are stable over time. Department characteristics are measured at baseline (t-1).

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Appendix to

Sexual Misconduct and Scientific Production

Manuela R. Collis

List of Appendices

A Tables

Table A1: Outcome Descriptive Statistics Before and After Matching

	Before Matching		After Matching	
Variable	**Control**	**Treated**	**Control**	**Treated**
Publications per Capita Total				
Mean (SD)	2.18 (2.65)	1.53 (1.11)	2.98 (3.53)	1.51 (1.02)
N	50,188	210	4,759	202
Publications per Capita Women				
Mean (SD)	2.28 (2.73)	1.51 (1.11)	2.99 (3.47)	1.48 (0.94)
N	33,632	188	3,492	181
Publications per Capita Men				
Mean (SD)	2.33 (2.94)	1.64 (1.42)	3.14 (3.76)	1.59 (1.28)
N	44,991	198	4,268	190

Note: Control group statistics after matching use analytic weights from CEM.

Table A2: L1 Imbalance Before and After Matching at time period $t-1$

Variable	Before Matching	After Matching	Reduction (%)
Multivariate L1 distance	0.710	0.501	29.44
Univariate L1 distance			
Discipline	0.293	0.135	53.92
Public Institution	0.112	2.2e-15	100.00
State Law for Hostility & Harassment	0.081	4.1e-15	100.00
Shanghai Ranking (GRAS)	0.032	5.3e-16	100.00
Department Size	0.445	0.147	66.93

Note: The variables *Discipline*, *Public Institution* and *State Laws for Discrimination & Harassment* are categorical.

Table A3: Effect of Occurrence of Sexual Misconduct on Average Number of Publications per Department Member

	Average number of publications per department member				
	(1) Full Sample	(2) 5 yr delay	(3) Adj. control	(4) Women	(5) Men
5 Year Average TE	0.022	0.013	-0.019	0.018†	-0.032
	(0.023)	(0.020)	(0.029)	(0.024)	(0.023)
6 periods before treatment	-0.017	0.015	-0.019	-0.024	-0.011
	(0.021)	(0.037)	(0.030)	(0.037)	(0.046)
5 periods before treatment	0.019	-0.013	-0.008	0.031	0.028
	(0.025)	(0.038)	(0.033)	(0.049)	(0.048)
4 periods before treatment	-0.022	-0.012	-0.030	-0.035	-0.029
	(0.021)	(0.038)	(0.028)	(0.030)	(0.044)
3 periods before treatment	0.022	0.066	-0.011	-0.009	0.018
	(0.026)	(0.042)	(0.032)	(0.033)	(0.043)
2 periods before treatment	0.051†	0.083†	0.026	0.033	0.023
	(0.029)	(0.044)	(0.043)	(0.029)	(0.039)
1 periods before treatment	(.)	(.)	(.)	(.)	(.)
Event period (t=0)	0.007	0.001	0.004	0.009	-0.037
	(0.026)	(0.036)	(0.038)	(0.032)	(0.033)
1 periods after treatment	0.002	0.001	-0.004	0.010	-0.013
	(0.025)	(0.033)	(0.036)	(0.037)	(0.033)
2 periods after treatment	0.079	0.003	-0.013	0.093	-0.012
	(0.067)	(0.041)	(0.039)	(0.077)	(0.037)
3 periods after treatment	0.002	0.035	-0.031	-0.015	-0.053†
	(0.027)	(0.040)	(0.040)	(0.031)	(0.031)
4 periods after treatment	0.020	0.032	-0.046	-0.008	-0.067
	(0.031)	(0.035)	(0.038)	(0.030)	(0.042)
5 periods after treatment	0.013	-0.002	-0.021	-0.012	-0.061
	(0.022)	(0.030)	(0.036)	(0.033)	(0.039)
Observations	53,841	35,544	12,691	29,623	35,880
R-squared	0.9385	0.9360	0.9767	0.9465	0.8907

† $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors clustered at university x department level. Fixed effects for university x department and years. Column 2 restricts the sample to incidents for which public reporting occurs after more than five years. Column 3 restricts the control group to departments which have a representation of women that is above the median for a given discipline.

Table A4: Comparison of Departments with Varying Degrees of Ability to Switch

	Occurrence of Sexual Misconduct		
	(1) Full Sample	(2) Hard to switch	(3) Professional / Clinical medicine
5 Year Average TE	0.022	0.037	-0.12**
	(0.023)	(0.025)	(0.034)
6 periods before treatment	-0.017	-0.018	-0.012
	(0.021)	(0.023)	(0.055)
5 periods before treatment	0.019	0.023	-0.032
	(0.025)	(0.027)	(0.053)
4 periods before treatment	-0.022	-0.017	-0.079*
	(0.021)	(0.022)	(0.034)
3 periods before treatment	0.022	0.024	-0.017
	(0.026)	(0.028)	(0.051)
2 periods before treatment	0.051†	0.060†	-0.040
	(0.029)	(0.032)	(0.040)
1 periods before treatment	(.)	(.)	(.)
Event period (t=0)	0.007	0.015	-0.067
	(0.026)	(0.028)	(0.044)
1 periods after treatment	0.002	0.011	-0.091*
	(0.025)	(0.028)	(0.040)
2 periods after treatment	0.079	0.099	-0.113*
	(0.067)	(0.075)	(0.052)
3 periods after treatment	0.002	0.018	-0.143*
	(0.027)	(0.028)	(0.063)
4 periods after treatment	0.020	0.036	-0.131*
	(0.031)	(0.034)	(0.059)
5 periods after treatment	0.013	0.031	-0.178**
	(0.022)	(0.024)	(0.062)
Observations	53,841	42,122	11,719
R-squared	0.9385	0.9397	0.9327

† $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors clustered at university x department level. Fixed effects for university x department and years.

Table A5: Effect of Public Reporting on Number of Publications per Department Member

	Simple DiD (1)	Simple Event-study (2)	Matched DiD (3)	Matched Event-study (4)	Sun & Abraham (5)
Treatment Post	-0.0402*		-0.0950†		
	(0.0186)		(0.0521)		
5 years before treatment		0.0166		-0.0117	-0.0154
		(0.0207)		(0.0259)	(0.0276)
4 years before treatment		0.0231		-0.0075	-0.0086
		(0.0227)		(0.0287)	(0.0290)
3 years before treatment		0.0084		-0.0187	-0.0134
		(0.0223)		(0.0265)	(0.0263)
2 years before treatment		-0.0104		-0.0460	-0.0598*
		(0.0247)		(0.0304)	(0.0287)
1 years before treatment		(.)		(.)	(.)
Event period (t=0)		-0.0107		-0.0502	-0.0386
		(0.0245)		(0.0397)	(0.0339)
1 year after treatment		-0.0394		-0.0983†	-0.0905†
		(0.0250)		(0.0519)	(0.0477)
2 years after treatment		-0.0523*		-0.1050†	-0.1142*
		(0.0232)		(0.0544)	(0.0509)
3 years after treatment		-0.0742**		-0.1440*	-0.1327*
		(0.0247)		(0.0656)	(0.0612)
4 years after treatment		-0.0421		-0.1156	-0.0969
		(0.0390)		(0.0802)	(0.0764)
5 years after treatment		-0.0137		-0.0766	-0.0517
		(0.0390)		(0.0671)	(0.0638)
Dept. Member Count (lagged)	0.0008*	0.0008*	0.0003	0.0003	
	(0.0003)	(0.0003)	(0.0004)	(0.0004)	
Observations	53,481	53,481	39,381	39,381	39,484
Clusters	2,086	2,086	1,485	1,485	1,494
R-squared	0.938	0.938	0.936	0.936	0.936
Unit FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Cohort-Specific FE	No	No	No	No	No

†$p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Dependent variable is publications per capita at the department level. Standard errors clustered at the department level in parentheses. Never-treated departments used as control group. Columns (3)-(5) use CEM-matched sample with analytical weights. Event study columns show dynamic treatment effects. Sun & Abraham column uses interaction-weighted estimator. All specifications include department and year fixed effects.

Table A6: The Effect of Public Reporting on Publication Output by Gender

	Public Reporting of Sexual misconduct		
	(1) Total	(2) Women	(3) Men
5 Year Average TE	-0.095*	-0.06**	-0.11†
	(0.048)	(0.016)	(0.025)
6 periods before treatment	-0.035	-0.037	-0.041
	(0.032)	(0.032)	(0.044)
5 periods before treatment	-0.017	-0.048	-0.030
	(0.026)	(0.034)	(0.035)
4 periods before treatment	-0.011	-0.050	0.002
	(0.032)	(0.026)	(0.49)
3 periods before treatment	-0.011	-0.017	-0.013
	(0.027)	(0.034)	(0.032)
2 periods before treatment	-0.063	-0.012	-0.091**
	(0.029)	(0.036)	(0.033)
1 periods before treatment	0.000	0.000	0.000
	(.)	(.)	(.)
Event period (t=0)	-0.043	-0.018	-0.044
	(0.034)	(0.033)	(0.04)
1 periods after treatment	-0.091†	-0.048	-0.087
	(0.047)	(0.037)	(0.054)
2 periods after treatment	-0.113*	-0.096***	-0.125*
	(0.049)	(0.031)	(0.062)
3 periods after treatment	-0.132*	-0.099***	-0.157†
	(0.061)	(0.029)	(0.085)
4 periods after treatment	-0.094	-0.059***	-0.159
	(0.075)	(0.046)	(0.104)
Observations	39,484	29,623	38,240
R-squared	0.9361	0.9448	0.8311

† $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors clustered at university x department level. Fixed effects for university x department and years.

B Tables

Table B7: Effect of Sexual Misconduct on Publication per Department Member - Delay in Public Reporting

	(1) Same-year	(2) within 0-5 yrs	(3) after 6-10 yrs
5 Year Average TE	-0.058	0.016	0.012
	(0.070)	(0.033)	(0.043)
6 periods before treatment	-0.100	-0.012	-0.056
	(0.084)	(0.031)	(0.056)
5 periods before treatment	-0.020	$0.079^{\dagger}$	-0.060
	(0.084)	(0.041)	(0.068)
4 periods before treatment	-0.010	0.007	-0.093
	(0.108)	(0.028)	(0.069)
3 periods before treatment	0.039	0.026	0.079
	(0.092)	(0.038)	(0.073)
2 periods before treatment	$-0.136^{\dagger}$	0.042	0.026
	(0.076)	(0.042)	(0.068)
1 periods before treatment	(.)	(.)	(.)
Event period (t=0)	-0.008	0.021	-0.049
	(0.100)	(0.038)	(0.051)
1 periods after treatment	-0.112	-0.000	0.001
	(0.115)	(0.042)	(0.058)
2 periods after treatment	$-0.196^{\dagger}$	0.123	0.019
	(0.107)	(0.123)	(0.066)
3 periods after treatment	-0.176	-0.052	0.055
	(0.119)	(0.038)	(0.070)
4 periods after treatment	0.205	-0.009	0.036
	(0.266)	(0.051)	(0.063)
5 periods after treatment	0.032	0.009	-0.036
	(0.085)	(0.037)	(0.036)
Observations	668	4,091	1,317
R-squared	0.9747	0.8978	0.8896

$^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Standard errors clustered at university x department level. Fixed effects for university x department and years. Column 3 (Poisson model) reports Pseudo R-squared in place of R-squared.

C Robustness of the Null Finding - Full Outline

As reported in Figure 4, the occurrence of sexual misconduct shows no discernible effect on the productivity of university departments. This is a lower bound effect since the sampling process is such that only cases that eventually become public are in my sample of treated departments. This means I have a contaminated control group and this likely biases my estimates downwards. In other words, the treated group consists of departments where sexual misconduct occurred (identified ex post via public reporting). The control group includes departments with no known misconduct and departments with misconduct that never became public. Therefore, contamination in the control group would attenuate any true negative effects toward zero.

This leaves room for the possibility, in the extreme case, that we compare departments where sexual misconduct occurred and became public, with departments where sexual misconduct occurred but never became public. Intuitively, we would compare two very similar groups of departments with each other—at time of occurrence—and thus, the null effect may seem sensible, but potentially not estimating the treatment effect of interest, which is the effect of the occurrence of sexual misconduct in university departments on department-level productivity.

Theoretically, we would expect that the occurrence of sexual misconduct in university departments has either a negative effect or a null effect on the productivity of departments. On one hand, there are reasons to believe sexual misconduct could negatively affect organizational productivity through several mechanisms: victims share experiences through whisper networks which can affect task conflict and performance, bystanders may experience psychological consequences that impact their work, and organizational responses like investigations and remedial measures (training programs, new policies, audits) can be disruptive and divert resources from core functions. On the other hand, there are reasons to expect no discernible effects due to significant barriers to information flow within organizations: many incidents go unreported due to fear of stigma and retaliation, organizational procedures emphasize confidentiality and often include non-disclosure agreements, and even when some members learn of incidents through informal channels, pluralistic ignorance and fear of scandal may prevent open discussion or coordinated action, ultimately containing the impact to those directly involved rather than affecting broader organizational performance.

Therefore, the interpretation of the estimated effect depends on two factors: the contamination level in the control group and the true effect size. As a starting point, we may want to ask how high contamination has to be in order for the results to be non-negligible. Learning more about the level of contamination required, will give us a better sense of whether the observed negligible effect is sensible or not. What we will see, is that the required levels of contamination are far higher than what empirical evidence would suggest.

While the inquiry of the required level of contamination provides us with useful insights, we can also investigate the following four scenarios related to the contamination levels and the true effect size. This allows us to evaluate how possible it is that there is a true null effect: (A) low contamination with negligible true effects, (B) low contamination with meaningfully negative true effects, (C) high contamination with negligible true effects, and (D) high contamination with meaningfully negative true effects. Below, I formally test which scenarios are consistent with the observed data, using a contamination adjustment model to evaluate whether the null finding reflects a genuine absence of productivity impacts or merely contamination-induced attenuation. This approach is in spirit of Manski (2003), Altonji, Elder and Taber (2005), and Oster (2019), who consider how selection into control or treatment affects stability of the observed treatment effect.

Conceptual Framework

The interpretation of the null finding for occurrence effects depends critically on the level of contamination in the control group—that is, the proportion of control departments that experienced unreported sexual misconduct. To establish plausible bounds for this parameter, I need to carefully distinguish between two related but distinct concepts: the total prevalence of misconduct across all departments, and the contamination rate within the control group specifically.

Boudreau et al. (2023) document that 38% of teams in their study experienced at least one case of sexual harassment. This represents the *total prevalence*—the proportion of all departments experiencing misconduct, whether reported or not. In my sample, this total prevalence includes both the 261 treated departments (where misconduct became public) and any control departments with unreported misconduct. To translate between total prevalence and contamination rates, I use the relationship: Total Prevalence $= P(\text{Treated}) + P(\text{Control}) \times c$, where $P(\text{Treated}) = 261/2,101 = 0.124$ and $P(\text{Control}) = 1,840/2,101 = 0.876$.

If the total prevalence in my setting matches Boudreau et al. (2023)'s 38%, this implies a contamination rate of $c = (0.38 - 0.124)/0.876 = 0.29$, or approximately 29% of control departments having unreported misconduct. For this analysis, I define "low contamination" as $c = 0.10$, representing a scenario where institutional reporting mechanisms function effectively and most serious misconduct either becomes public or does not occur. In contrast, "high contamination" is set at $c = 0.29$, matching the level implied by empirical evidence. To put this in perspective, a contamination rates of 14% in our context would mean unreported cases roughly equal reported ones, while the 29% contamination implied by empirical evidence suggests unreported cases outnumber reported ones by two-to-one across all research universities.

To distinguish economically meaningful from negligible effects, I pre-specify a smallest effect size of interest (SESOI) of 0.027 publications per department and year (≈0.49 papers per year in a median-sized department of 18 members). I classify effects at or below -0.056 as meaningfully negative for departmental productivity (one full publication per year in an 18-faculty department). With 261 treated and 1,840 control departments and a standard error of 0.023, the minimum detectable effect (MDE) at 80% power is 0.064 publications per faculty-year.

The contamination adjustment model makes several key assumptions: (1) contamination affects only control departments, not treated ones; (2) the occurrence effect is homogeneous across departments; and (3) there are no other sources of bias such as baseline imbalances, differential time trends, or measurement lags. Under these assumptions, contamination mechanically attenuates the observed treatment-control contrast. When a fraction c of the control departments experienced unreported misconduct, the observed difference is diluted relative to the true effect. To see why, consider what we're actually comparing:

Treated departments: All experienced a case of sexual misconduct. If the true effect of misconduct is τ (average change in publications per department over five years after treatment), these departments have an average outcome of $Y_0 + \tau$, where Y_0 is the baseline productivity.

Control departments: A mixture of:

- Fraction $(1 - c)$: Truly unaffected departments with outcome Y_0

- Fraction c: Contaminated departments (unreported misconduct) with outcome $Y_0 + \tau$

The control group's average outcome is $(1-c) \times Y_0 + c \times (Y_0 + \tau) = Y_0 + c\tau$, where fraction $(1-c)$ are truly unaffected departments with outcome Y_0 and fraction c are contaminated departments with outcome $Y_0 + \tau$. The observed treatment-control difference is therefore $(Y_0 + \tau) - (Y_0 + c\tau) = \tau - c\tau = \tau(1-c)$. This yields the contamination adjustment formula:

$$\text{True Effect} = \frac{\text{Observed Effect}}{1-c} \tag{6}$$

This formula shows that as contamination increases, the observed effect shrinks toward zero, potentially masking real impacts. A concrete illustration clarifies the logic under empirically-grounded contamination with a meaningful negative effect. Consider 100 control departments where 29% have unreported misconduct ($c = 0.29$) and the true effect is -0.056 publications per faculty member. The 71 departments without misconduct experience no change (0), while the 29 departments with unreported misconduct experience the full effect (-0.056). The average change in the control group becomes $(71 \times 0 + 29 \times (-0.056))/100 = -0.016$. Comparing treated departments (all at -0.056) to this mixed control group (averaging -0.016) yields an observed difference of $-0.056 - (-0.016) = -0.040$, not the true -0.056. The effect is compressed by 29%. Under this contamination-only model, negative true effects would produce negative expected observed effects (unless contamination exceeds 100%, which is outside of possible levels of contamination).

What level of contamination will generate a non-negligible effect?

Given the observed effect of 0.022, what levels of contamination would we need to assume for there to be a true significant negative effect (-0.056)? We can use our contamination adjustment formula, True Effect $\approx$ Observed Effect / $(1-c)$, and rearrange it to solve for c. Starting with $-0.056 \approx 0.022/(1-c)$, we multiply both sides by $(1-c)$ to get $-0.056 \times (1-c) \approx 0.022$. Expanding this gives us $-0.056 + 0.056c \approx 0.022$. Adding 0.056 to both sides yields $0.056c \approx 0.078$. Finally, dividing both sides by 0.056 gives us $c \approx 0.078/0.056 \approx 1.393$.

A value of $c > 1$ suggests that the contamination would have to exceed 100% for there to be possibly a significant negative effect. Before drawing any conclusions, recall that our estimate is not a precisely estimated zero. Instead, the observed effect of 0.022 is somewhat noisy. We can take this into account and compute a lower-bound and upper-bound contamination level.

The calculation of contamination bounds incorporates the uncertainty in the estimate. We will use it to determine the range of contamination levels that could potentially reconcile the observed estimate with a meaningful negative true effect. That is, instead of simply relying on the estimate, we can take advantage of the known standard errors (0.023) to compute the confidence interval and use the full range of plausible values. The 95% confidence interval for the observed effect spans from -0.023 to 0.067. We can use this information to calculate a contamination bound. We can assume the observed effect is -0.023 and again compute the contamination levels required for such an effect size. And we can do the same, assuming the observed effect size is the upper level of the confidence interval, 0.067.

At the lower bound of -0.023, the required contamination would be $c = 1 - (-0.023/-0.056) = 0.589$, or about 59%. This means that even if the true observed effect were at the very bottom of what's statistically plausible, we would still need nearly 60% of the control departments to have experienced unreported misconduct for the data to be consistent with meaningful productivity losses. At the upper bound of 0.067, the calculation yields $c = 2.196$. Since a contamination level of above 1 is outside of the possible levels of contamination, this scenario can be ruled out. Therefore, we can say that for there to plausibly be a meaningful negative effect, the contamination levels will have to be at least 59%.

To evaluate how plausible this required contamination level is, we can translate it to total prevalence and compare with empirical evidence. If 59% of our 1,840 control departments have unreported misconduct, this represents 1,086 departments. Adding our 261 treated departments yields 1,347 total departments with misconduct out of 2,101 departments—a total prevalence of 64%. This far exceeds the 38% prevalence documented by Boudreau et al. (2023). Conversely, if we accept their 38% prevalence as accurate for our setting, the implied contamination in our control group would be only $(0.38-0.124)/0.876 = 29\%$—well below the 59% threshold needed for meaningful negative effects.

This analysis offers us two insights. First, even though the estimated effect is somewhat noisy, the contamination bounds allow us to take that into account and provide us with a useful bound of contamination level c under which the true effect size would be meaningfully negatively affecting productivity. That contamination bound ranges from 59% to 100%.

Second, this range implies that the total prevalence of sexual misconduct would need to be at least 64%. However, work by Boudreau et al. (2023) suggests that the true prevalence is closer to 38%, which would correspond to only 29% contamination in our control group. This suggests that the observed negligible effect may indeed be a true negligible effect.

Testing whether the true effect could be negligible (Scenarios A & C)

Given the observed effect of 0.022, what would the true effect be under different contamination levels? Contamination attenuates the observed effect according to the formula: Observed Effect = True Effect $\times$ $(1 - c)$. Rearranging this gives us True Effect = Observed Effect / $(1 - c)$, which allows us to recover the true effect from our observed estimate.

Scenario A: Low contamination ($c = 0.10$)

If only 10% of control departments have unreported misconduct, we can calculate the adjusted effect:

$$\text{True Effect} = \frac{0.022}{1 - 0.10} = \frac{0.022}{0.90} = 0.024 \tag{7}$$

This adjusted effect of 0.024 publications per faculty-year falls below our pre-specified SESOI threshold of 0.027. In a median department of 18 faculty, this translates to less than half a publication per year (0.43 papers)—a change that would be difficult to detect in practice and unlikely to meaningfully affect departmental operations.

To account for statistical uncertainty, we can also compute the 95% confidence interval for the adjusted effect. Our original confidence interval spans from -0.023 to 0.067. Since we treat c as known and not as a random variable, we are able to adjust the confidence interval of the initial result linearly. That is, we can divide the lower bound and upper bound of the confidence interval by $1 - c$ to obtain the confidence interval for the estimated effect under the scenario where the contamination level is 10%. We compute $-0.023/0.90$ and $0.067/0.90$ which gives us a 95% confidence interval of $[-0.026, 0.074]$ for this scenario.

Scenario C: High contamination ($c = 0.29$)

If 29% of control departments have unreported misconduct—matching the contamination level implied by Boudreau et al. (2023)'s documented prevalence—the adjustment becomes:

$$\text{True Effect} = \frac{0.022}{1 - 0.29} = \frac{0.022}{0.71} = 0.031 \tag{8}$$

This adjusted effect of 0.031 publications per faculty-year marginally exceeds our pre-specified SESOI threshold of 0.027. In practical terms, this represents 0.56 publications per year in a median 18-faculty department—approximately one paper every two years. While this effect exceeds the SESOI threshold, it remains modest in magnitude and may not be economically significant for departmental decision-making or resource allocation.

To account for statistical uncertainty, we compute the 95% confidence interval for the adjusted effect. Our original confidence interval spans from -0.023 to 0.067. Since we treat c as known and not as a random variable, we can adjust the confidence interval of the initial result linearly by dividing the lower and upper bounds by $1 - c$. We compute $-0.023/0.71$ and $0.067/0.71$, which gives us a 95% confidence interval of $[-0.032, 0.094]$ for this scenario. Notably, this confidence interval includes zero, suggesting the adjusted effect is not statistically distinguishable from null.

Testing whether meaningful negative effects are plausible (Scenarios B & D)

If the true effect were meaningfully negative ($\tau = -0.056$), what would we expect to observe? We can work backwards from this assumption to calculate what we should have seen in our data, then test whether our actual observation is consistent with these predictions.

To test whether our observed value is statistically consistent with each scenario's prediction, I need to determine how likely it is to observe our estimate (0.022) if the scenario were true. Since our observed effect is a sample estimate with standard error 0.023, it follows approximately a normal distribution. Under the null hypothesis that a given scenario is correct (e.g., the expected value is -0.050), the difference between our observed and expected values, standardized by the standard error, follows a standard normal distribution: $z = \frac{\text{Observed}-\text{Expected}}{SE}$. This z-statistic measures how many standard errors our observation lies from the scenario's prediction. Large absolute values of z (typically $|z| > 1.96$) indicate that our observation would be unlikely if the scenario were true, allowing us to assess each scenario's plausibility.

Scenario B: Low contamination ($c = 0.10$)

If the true effect is -0.056 and only 10% of control departments have unreported misconduct, we can calculate what we would expect to observe. Recall that contamination attenuates the observed effect by a factor of $(1 - c)$, so our expected observation would be $-0.056 \times (1 - 0.10) = -0.056 \times 0.90 = -0.050$. This means we should observe departments with public misconduct having 0.050 fewer publications per faculty-year compared to the control group. Our actual observation is 0.022. To test whether this discrepancy could arise by chance, we calculate:

$$z = \frac{0.022 - (-0.050)}{0.023} = \frac{0.072}{0.023} = 3.13 \tag{9}$$

which yields $p < 0.01$. This indicates that our observation is highly unlikely if this scenario were true.

Scenario D: High contamination ($c = 0.29$)

If the true effect is -0.056 and 29% of control departments have unreported misconduct, the expected observation becomes $-0.056 \times (1 - 0.29) = -0.056 \times 0.71 = -0.040$. Even with empirically-grounded contamination diluting the effect, we should still observe treated departments being less productive by 0.040 publications per faculty-year. Our actual observation is 0.022. To test whether this discrepancy could arise by chance, we calculate:

$$z = \frac{0.022 - (-0.040)}{0.023} = \frac{0.062}{0.023} = 2.70 \tag{10}$$

which yields $p < 0.01$, indicating that our observation is statistically inconsistent with this scenario.

The analysis addresses two key questions that together determine the interpretation of our null finding. First, can the true effect be negligible given what we observed? Scenarios A and C examined this question. After adjusting for contamination, the true effects would be 0.024 (low contamination) or 0.031 (high contamination). In Scenario A, the adjusted effect falls below the SESOI threshold of 0.027. In Scenario C, while the point estimate marginally exceeds SESOI, the confidence interval $[-0.032, 0.094]$ includes zero. Both scenarios remain consistent with negligible to modest true effects.

Second, are meaningful negative effects plausible given what we observed? Scenarios B and D examined this question. If misconduct truly reduced productivity by 0.056 publications per faculty-year, we would expect to observe values of -0.050 under low contamination or -0.040 under high contamination. Our observed estimate of 0.022, while somewhat noisy (SE = 0.023), is 3.13 and 2.70 standard errors away from these predictions, respectively. These statistical tests suggest that meaningful negative effects are unlikely given our data ($p < 0.01$ and $p < 0.01$).

Summary

This multi-pronged analysis offers evidence that the observed effect is a true null effect and that sexual misconduct has, indeed, negligible effects on departmental productivity. Three complementary approaches converge on this conclusion. First, the contamination bounds analysis shows that even accounting for the noisiness in our data, meaningful negative effects would require at least 59% contamination in control departments—corresponding to 64% total prevalence of misconduct across all departments, far exceeding the 38% documented by Boudreau et al. (2023). Moreover, if we accept their 38% prevalence as accurate, this implies only 29% contamination in our control group, well below the required threshold. Second, when we adjust for plausible contamination levels (10–29%), the true effects remain modest, ranging from 0.024 to 0.031 publications per faculty-year, with confidence intervals that include zero and exclude meaningful productivity losses. Third, statistical tests reject scenarios where true effects are meaningfully negative: if misconduct truly reduced productivity by 0.056 publications per faculty-year, we would expect to observe negative values between -0.050 and -0.040, yet we observed $+0.022$ ($p < 0.02$ for all scenarios). The convergence of these approaches—each addressing different aspects of the possibility that contamination may be present—strengthens our conclusion that the null finding reflects a genuine absence of productivity impacts rather than measurement limitations or contamination-induced attenuation.

D Clarivate Core Collection

I use nine primary departments which are based on Clarivate's Core Collection (`https://mjl.clarivate.com/help-center`): Agriculture, Biology & Environmental Sciences; Arts & Humanities; Business Collection; Clinical Medicine; Electronics & Telecommunication Collection; Engineering, Computing & Technology; Life Sciences; Physical, Chemical & Earth Sciences; Social And Behavioral Sciences. Below, I list for the research communities defined by Clarivate and SJR for each of the nine disciplines.

Disciplines and Communities

Agriculture, Biology & Environmental Sciences
- Agriculture/Agronomy
- Agricultural Chemistry
- Animal Sciences
- Aquatic Sciences
- Biology
- Biodiversity
- Biophysics
- Biotechnology
- Botany
- Conservation
- Developmental Biology
- Ecology/Environmental Sciences
- Entomology
- Evolutionary Biology
- Fisheries
- Food Science
- Forestry
- Horticulture
- Marine Biology
- Molecular Biology
- Mycology
- Paleontology
- Parasitology
- Plant Sciences
- Soil Science
- Veterinary Sciences
- Wildlife Management
- Zoology

Arts & Humanities
- Architecture
- Art
- Asian Studies
- Classical Studies
- Dance
- Film, Radio & TV
- Folklore
- History
- Humanities (General)
- Language & Linguistics
- Literary Criticism
- Literature
- Medieval Studies
- Music
- Philosophy
- Poetry
- Religion
- Renaissance Studies
- Theater

Business Collection
- Accounting
- Advertising
- Banking
- Business
- E-commerce
- Economics
- Finance
- Hospitality Industry
- Human Resources
- Insurance
- International Business
- Logistics
- Management
- Marketing
- Nonprofit Organizations
- Operations Research
- Real Estate

Clinical Medicine
- Allergy
- Anesthesiology
- Audiology
- Cardiology
- Critical Care
- Dentistry
- Dermatology
- Emergency Medicine
- Endocrinology
- Gastroenterology
- General Medicine
- Geriatrics
- Health Policy
- Hematology
- Immunology
- Infectious Diseases
- Medical Ethics
- Medical Informatics
- Medical Technology
- Neurology
- Nursing
- Nutrition
- Obstetrics & Gynecology
- Oncology
- Ophthalmology
- Orthopedics
- Pathology
- Pediatrics
- Pharmacology
- Physical Therapy
- Preventive Medicine
- Psychiatry
- Radiology
- Rheumatology
- Sports Medicine
- Surgery
- Toxicology
- Transplantation
- Tropical Medicine

- Urology

Electronics & Telecommunications Collection
- Artificial Intelligence
- Automation
- Computer Hardware
- Computer Science
- Control Systems
- Cybernetics
- Digital Signal Processing
- Electrical Engineering
- Electronics
- Embedded Systems
- Information Systems
- Machine Learning
- Mobile Communications
- Network Security
- Robotics
- Software Engineering
- Telecommunications

Engineering, Computing & Technology
- Acoustics
- Aerospace Engineering
- Architectural Engineering
- Automotive Engineering
- Biomedical Engineering
- Ceramics
- Chemical Engineering
- Civil Engineering
- Composite Materials
- Construction
- Energy & Fuels
- Environmental Engineering
- Fluid Dynamics
- Industrial Engineering
- Manufacturing
- Marine Engineering
- Materials Science
- Mechanical Engineering
- Metallurgy
- Mining Engineering
- Nanotechnology
- Nuclear Engineering
- Petroleum Engineering
- Polymers
- Remote Sensing
- Thermodynamics
- Transportation
- Water Resources

Life Sciences
- Biochemistry
- Biomedical Research
- Biometrics
- Cancer Research
- Cell Biology
- Computational Biology
- Genetics
- Genomics
- Immunobiology
- Limnology
- Microbiology
- Microscopy
- Molecular Biology
- Neuroscience
- Physiology
- Proteomics
- Stem Cell Research
- Structural Biology
- Systems Biology
- Tissue Engineering
- Toxinology
- Virology

Physical, Chemical & Earth Sciences
- Analytical Chemistry
- Applied Mathematics
- Astronomy & Astrophysics
- Atmospheric Science
- Atomic Physics
- Catalysis
- Chemistry
- Computational Physics
- Condensed Matter Physics
- Crystallography
- Electrochemistry
- Fluid Dynamics
- Geochemistry
- Geology
- Geophysics
- Inorganic Chemistry
- Mathematics
- Meteorology
- Mineralogy
- Nanotechnology
- Nuclear Physics
- Oceanography
- Optics
- Organic Chemistry
- Particle Physics
- Physical Chemistry
- Polymer Science
- Quantum Physics
- Seismology
- Spectroscopy
- Statistical Physics
- Thermodynamics

Social And Behavioral Sciences
- Anthropology
- Applied Linguistics
- Archaeology
- Area Studies
- Behavioral Sciences
- Child Development
- Clinical Psychology
- Cognitive Science
- Communication
- Criminology
- Cultural Studies
- Demography
- Developmental Psychology
- Economic Geography
- Education
- Educational Psychology
- Environmental Studies
- Ergonomics
- Ethics
- Ethnic Studies
- Experimental Psychology
- Family Studies
- Geography
- Gerontology
- Health Education
- Human Geography
- Industrial Psychology
- Information Science
- International Relations
- Law
- Library Science
- Political Science
- Psychoanalysis
- Psychology
- Public Administration
- Social Psychology
- Social Work
- Sociology
- Special Education
- Sports Science
- Substance Abuse
- Urban Studies
- Women's Studies