

# The Effects of Social Movements: Evidence from #MeToo

Ro'ee Levy and Martin Mattsson\*

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## Abstract

Social movements are associated with large societal changes, but evidence of their causal effects is limited. We study the effect of the MeToo movement on reporting sex crimes to the police. We construct a new dataset of crimes reported in 31 OECD countries and employ a triple-difference strategy over time, across countries, and between crime types. The movement increased the reporting of sex crimes by 10%. Using rich US data, we find that in contrast to a common criticism of the movement, the effect is similar across socioeconomic groups, and that the movement also increased sexual assault arrests. The increased reporting reflects a higher propensity to report sex crimes and not an increase in crime incidence. The mechanism most consistent with our findings is that victims perceive sexual misconduct to be a more serious problem following the movement. Our results demonstrate that social movements can rapidly and persistently affect high-stakes decisions.

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\*Levy: Tel Aviv University, roee.levy@tauex.tau.ac.il; Mattsson: National University of Singapore, martin.mattsson@nus.edu.sg. We thank Olle Folke, Eduardo Fraga, Sahana Ghosh, Joni Hersch, Dean Karlan, Sarah Khan, Alexey Makarin, Costas Meghir, Mushfiq Mobarak, Yoko Okuyama, Johanna Rickne, Yogita Shamdasani, Joseph Shapiro, Ebonya Washington, Jaya Wen, David Yanagizawa-Drott, Mor Zoran, and seminar participants at the AEA Annual Meeting, Brown, the Behavioral Political Economy Workshop, Michigan, MIT, the Munich Lectures in Economics, NBER Gender in the Economy, NUS, Tel Aviv University, Yale, and the YES conference for helpful comments and suggestions. We also thank Calvin Jahnke and Kamila Janmohamed for their excellent research assistance. This project was approved by Yale University IRB (Protocol ID 2000023166). Funding for this project was provided by the Department of Economics at Yale University, the Tobin Center for Economic Policy, and Y-RISE.

# 1 Introduction

Societal changes are often associated with movements advocating for new norms and behaviors. For example, the increase in women’s labor force participation and increased concern for the environment happened in conjunction with the Women’s Liberation Movement and the environmental movement, respectively. However, establishing the causal effects of social movements has proven challenging. While social movements may affect behavior, they often coincide with external factors, such as rising incomes, that could result in behavioral changes regardless of the movements.

In this paper, we focus on the MeToo movement and estimate its causal effect on the reporting of sex crimes to the police, thus testing whether the movement addressed a major social problem—the under-reporting of sex crimes. The CDC estimates that 44% of American women experience sexual violence in their lifetime (Smith et al., 2018). Most of these cases are not reported to the police and in all countries reporting data to the UN, sexual assaults are underreported compared to other assaults (UNSDG, 2017). Because reporting is typically required to arrest an offender, underreporting hinders the justice system from preventing future crimes.

The MeToo movement, which started in October 2017, provides a well-suited setting to study the effects of social movements. The movement was exceptionally effective in rapidly increasing awareness around sexual misconduct. In the year after the movement began, the average OECD Google search interest in sexual misconduct increased by an unprecedented 85%. The movement quickly spread internationally but with large variation in its strength across countries. We use this variation and the fact that the movement took off almost instantly to identify its causal effect.

Even though the MeToo movement was prominent, its focus on high-profile individuals led to skepticism regarding the movement’s effect. One year after the MeToo movement started, a plurality of Americans agreed that it only changed things for famous people (Ipsos, 2018b). Similarly, a leading New York Times columnist argued that *“the movement has had little effect on the broader problem of sexual abuse, harassment and violence by men who are neither famous nor particularly powerful”* (Taub, 2019).

To estimate the effect of the MeToo movement, we construct a new dataset of the number of crimes reported to the police by collecting and harmonizing quarterly data from 31 OECD countries, covering 88% of the OECD population. We classify countries as having a strong or weak MeToo movement based on Google search interest for terms related to the movement and corroborate this measure using both survey and Twitter data.

Our primary empirical strategy is a triple-difference specification comparing countries with weak and strong movements, the reporting of sex crimes and non-sex crimes, and the pre- and post-periods. While countries with strong MeToo movements are different from countries with weak movements, we show that the two sets of countries have similar pre-trends in the difference between sex crimes and non-sex crimes reported. The triple-difference strategy allows us to avoid bias from other international events that could have affected sex crimes around the start of the movement, as we control for post-period by crime type fixed effects. Furthermore, by including fixed effects for the post-period by the strength of the movement, this strategy does not suffer from bias due to any event coinciding with the movement and changing the overall number of reported crimes in countries with strong MeToo movements.

We find that the MeToo movement substantially increased the number of reported sex crimes in countries with a strong movement. The Average Treatment effect on the Treated (ATT) is 10% during the first six months of the movement. A difference-in-differences estimate among countries that had a strong MeToo movement suggests that the movement's effect is persistent and lasts at least until the end of our data, 15 months after the movement started.<sup>1</sup>

The main result is consistent across a range of methods, data sets, and robustness tests. We find similar effects when constructing counterfactuals for the number of sex crimes reported had there been no MeToo movement using only non-sex crimes, using only countries with weak MeToo movements, and when employing the matrix completion method (Athey et al., 2021), which uses more flexible patterns in the data to create a counterfactual. Furthermore, we do not find an effect in additional placebo tests made possible by the triple-difference strategy: estimating the effect on non-sex crimes in countries with a strong MeToo movement and estimating the effect on sex crimes in countries with a weak movement.

We show that the result is robust to various specifications and alternative measures of the strength of the MeToo movement. We also find a similar effect when allowing the MeToo movement to start at different dates in different countries (a staggered intervention) and thus using additional variation in the data. Finally, we find that the movement increased the reporting of sex crimes in two independent US data sets, one based on crime data in eight large US cities and another based on a survey of university students. While every method we use has its own advantages and limitations, we interpret the consistency in the estimated effect as overwhelming evidence that the MeToo movement increased the number of reported sexual crimes.

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<sup>1</sup>Since the MeToo movement became stronger over time in many of the countries where it was initially weak, we estimate the long-run effect using a difference-in-differences approach between sex crimes and non-sex crimes in countries that had a strong movement, instead of using the triple-difference strategy.

The international dataset allows for the strongest identification strategy, but it lacks details on the crimes reported. To analyze heterogeneous effects, we use incident-level US data from the FBI National Incident-Based Reporting System (NIBRS) and the eight US cities. Since the US lacks substantial geographic heterogeneity in the strength of the MeToo movement, we employ a difference-in-differences strategy comparing sex crimes to all other crimes over time.

The MeToo movement had a larger effect on female victims and politically liberal counties. However, we do not find evidence for the claim, commonly made in media reports, that the MeToo movement mainly affected White women of high socioeconomic status. Instead, we find substantial effects across different income, racial, and educational groups.

To determine whether the movement mostly affected a stock of old crimes that would eventually be exhausted, we analyze the US city data because it records the date the crime occurred along with the date it was reported. We find that the movement affected both the flow of new crimes and the stock of crimes that are reported at least a month after they occur.

Using the NIBRS data, we find that the movement increased the number of arrests made for sexual assaults. The effect on arrests suggests that the MeToo movement may have prevented future crimes, although our data does not allow us to assess the overall welfare effect.

What were the main mechanisms behind the increase in reported crimes? Using multiple datasets, we conclude that the increased reporting of sex crimes is due to an increased propensity to report sex crimes, and not an increase in the incidence of these crimes. First, we analyze crimes *committed* before the start of the MeToo movement, but that could still have been *reported* after the movement started. We find a strong effect on the number of such crimes reported, even though their incidence could not have been affected by the movement. Second, we analyze the Campus Climate Survey (CCS), which allows us to separately estimate changes in the reporting rate and the incidence of sexual assaults among university students. We find that the reporting rate increased following the MeToo movement, while incidence decreased. Finally, the MeToo movement did not lead to major immediate changes in laws. Therefore, legal changes could not have led to a mechanical increase in incidence.

To explore why the reporting propensity increased, we analyze responses over time in multiple surveys conducted before and after the MeToo movement started. The mechanism most consistent with our data is that due to the movement, individuals believed that sexual misconduct is a more widespread social problem than they thought and considered specific acts of sexual misconduct to be more serious. Therefore, victims had stronger reasons to report sex crimes. We do not find evidence that stigma

decreased, nor do we find evidence that victims' expectations on how authorities would respond to a report changed following the movement.

The results relate to three different streams of literature. First, we contribute to a long debate among social scientists on whether social movements are influential (Burstein and Sausner, 2005). In a review of the topic, Amenta et al. (2010) state that “[t]he disagreement on this basic issue is wide. Some ... hold that social movements are generally effective and account for most important political change. Others ... argue that social movements are rarely influential.” Papers in this field often document a correlation between a movement's activity and an outcome, such as congressional attention (e.g., Baumgartner and Mahoney, 2005), but do not necessarily identify causal effects. A smaller literature focuses on the causal effects of political protest, a specific tactic often employed by social movements. This literature has shown that protests can mobilize people and change voting behavior, but that violent protests may also cause a political backlash (Madestam et al., 2013; Wasow, 2020; Bursztyn et al., 2021). We bridge these literatures by identifying the causal effect of a social movement as a whole. An additional contribution of our paper is that we estimate the effect of a movement on a costly personal decision instead of traditional political outcomes, such as voting. Understanding the effects on personal decisions is important because they are the focus of many social movements but may be especially difficult to change.

This paper also contributes to the literature on reporting gender-based violence by showing that social movements can have a large effect on the reporting of sex crimes, and analyzing the mechanism behind this change. Previous studies have shown that the election of female politicians and the integration of women into the police force increased the reporting of crimes against women (Iyer et al., 2012; Miller and Segal, 2019), and that a high-profile rape and murder case increased the reporting of sex crimes in India (Bhatnagar et al., 2019; McDougal et al., 2021). Public campaigns are a common strategy to increase reporting.<sup>2</sup> The MeToo movement can be seen as a particularly successful campaign focusing on discussing or reporting sexual misconduct. To the best of our knowledge, this paper presents the first rigorous evidence of the effects of the MeToo movement on reported sex crimes and thus demonstrates that such campaigns can be effective, even in the absence of changes to laws and government policies.<sup>3</sup>

Finally, the paper contributes to the literature on how norms evolve. It is well established that gender norms have strong effects on behavior (e.g., Alesina et al., 2013; Bertrand et al., 2015; Charles et al.,

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<sup>2</sup>For example, RAINN, the largest US-based anti-sexual violence organization, spends 21% of its budget on educating the public (RAINN, 2017).

<sup>3</sup>Rotenberg and Cotter (2018) present descriptive statistics showing that sex crimes reported increased in Canada after the MeToo movement started. Recent research has also examined the effect of the movement on firm profits, employment patterns in Hollywood, and quit rates from toxic workplaces in France (Lins et al., 2020; Luo and Zhang, 2021; Batut et al., 2021).

2018). While previous research has documented the importance and persistence of these norms, there is still a limited understanding of how they change (Giuliano, 2020). Several studies have shown that popular culture, motherhood, education, and programs empowering women can affect gender norms and behavior (e.g., Jensen and Oster, 2009; La Ferrara et al., 2012; Banerjee et al., 2019; Dhar et al., 2020; Green et al., 2020; Kuziemko et al., 2020; Field et al., 2021). A recent literature based on theory, as well as information interventions, argues that social norms can "unravel" when individuals start expressing their personal beliefs (Sunstein, 2019; Bursztyjn et al., 2020). We provide evidence that social movements can change the social norm for behavior under particular circumstances by changing perceptions of a societal problem.

The rest of the paper is organized as follows. Section 2 discusses the underreporting of sex crimes and describes the MeToo movement in more detail. Section 3 describes the international data, our primary identification strategy, and provides evidence for the effect of the movement. Section 4 describes the US data and provides results on heterogeneity in the effect as well as the effect on arrests. Section 5 describes survey data and provides evidence on the mechanisms through which the MeToo movement operated. Section 6 concludes.

## **2 Background**

### **2.1 Underreporting of Sexual Misconduct**

Underreporting of sexual misconduct is a serious global problem. Appendix Figure A.1 shows that among eight countries that provided data to the UN Sustainable Development Goals (SDG) Indicators Database, only 15% of sexual assaults were reported to the police in 2010-2017, compared to 35% of non-sexual assaults.

Underreporting can occur because reporting sexual crimes incurs high costs to the victims. First, attending hearings and police interviews has monetary costs such as lost income, childcare, and travel costs (Morabito et al., 2019). Second, reporting a sex crime forces the victim to repetitively relive the experience by giving detailed accounts of the crime, which may be especially hurtful for victims who are not believed by law enforcement officials (Spohn and Tellis, 2012). Third, reporting a crime may lead to retaliation by the offender or the community shared by the victim and the offender.<sup>4</sup>

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<sup>4</sup>According to the National Crime Victimization Survey (NCVS), 17% of sex crime victims who did not report the crime to the police cite fear of retaliation as a reason for not reporting the crime, while the same figure for victims of other violent crimes is 7%.

Reporting sex crimes also has important benefits, but most of these accrue to society and not directly to the victim. Reporting provides the police with information that is often crucial to arrest offenders. These arrests can prevent repeat offenders from committing additional crimes and may deter future offenders from committing crimes.<sup>5</sup> Given the potentially large positive externality from reporting, but the high personal cost of doing so, it is likely that sex crimes are reported at a rate that is below what is optimal from a social welfare perspective.

While we focus on the reporting of sex crimes to the police, the MeToo movement could also have affected other outcomes. In addition to sex crimes, the movement highlighted cases of sexual misconduct that do not constitute a criminal offense but still have serious negative welfare consequences, such as workplace sexual harassment (Hersch, 2011; Folke et al., 2020). Furthermore, a victim of a sex crime has a range of possible actions at her disposal. Reporting to the police is probably one of the actions with the greatest consequences. Therefore, the effects we find on crimes reported to the police are probably a subset of the effects of the movement on victims' behavior.

## 2.2 The MeToo Movement

The MeToo movement went viral on October 15, 2017, after the Harvey Weinstein sexual misconduct allegations were exposed, when a tweet by Alyssa Milano encouraged people who had been sexually harassed or assaulted to write "Me too" on social media.<sup>6</sup>

The movement quickly spread internationally. However, it did not spread everywhere and some countries initially had weak or non-existent movements. Appendix Table A.1 shows that on average, countries with stronger MeToo movements have higher incomes, larger shares of English speakers, and may exhibit greater gender equality.<sup>7</sup> As discussed in Section 3, we do not assume that the movement spread randomly, but rather that in the absence of the movement, reporting of sex crimes, compared to non-sex crimes, would have continued to evolve similarly in countries with strong and weak movements.

In the countries that had a strong MeToo movement, including the US, the movement typically involved a combination of the following components: accusations of sexual misconduct committed by

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<sup>5</sup>Indeed, Iyer et al. (2012) and Green et al. (2020) provide suggestive evidence that increases in reporting reduce the incidence of gender-based violence.

<sup>6</sup>The phrase "Me Too" was first used by Tarana Burke in 2006, but widespread usage only started after October 15, 2017.

<sup>7</sup>Other studies analyzing Twitter data also find that the strength of movements is associated with liberal democracies, more internet usage, higher income per capita, and more links to other countries (Mitra, 2019; Lee and Murdie, 2020). Idiosyncratic factors also influenced the spread of the movement. For example, in South Korea, the movement only took off in January 2018 after Prosecutor Seo Ji-hyeon discussed her experience of sexual assault on live television.

high-profile men which often ignited the local movement, victims sharing their experiences of sexual misconduct on social media, often with local hashtags, extensive media coverage and journalist investigations of sexual assault and sexual harassment, and in some cases local protests or petitions supporting the movement.<sup>8</sup>

While our identification strategy allows us to identify the effect of the movement as a whole, one limitation of this paper is that it cannot disentangle the effects of various components of the movement. For example, we cannot estimate the relative effects of high-profile cases compared to the personal stories shared by individuals on social media.<sup>9</sup>

Four factors make the MeToo movement a good setting to study the effects of social movements. First, the movement was very effective in drawing attention to sexual harassment and sexual misconduct. Figure 1 shows that in the OECD, mean Google search interest for MeToo and sexual misconduct dramatically increased immediately after the movement started. Second, there was large variation in the strength of the movement between countries, as shown in Figure 2. The OECD country in the 75th percentile in terms of MeToo search interest had a 454% larger interest in the MeToo movement in October 2017, compared to the country in the 25th percentile. Third, one of the main objectives of the MeToo movement, increasing reporting of sexual misconduct, is an outcome for which there is high-quality administrative data across many countries. Fourth, while the MeToo movement had a big impact on the public discourse, it did not result in immediate widespread changes to laws or government institutions. This allows us to attribute the short-run effect we find to changes in information or social norms.

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<sup>8</sup>These different aspects of the MeToo movement can be demonstrated with data from the US. First, during the first year of the movement, more than 200 high-profile men were ousted from positions of power following sexual misconduct allegations (Carlsen et al., 2018). Second, 65% of social media users stated that some or a great deal of the content they see on social media is about sexual harassment or assault (Anderson, 2018). Finally, *TIME Magazine's* 2017 person of the year were the "silence breakers" reporting sexual misconduct and Appendix Figure A.2 shows that among four major US newspapers, coverage related to sexual assault and sexual harassment increased substantially after the movement started.

<sup>9</sup>Even though we cannot separately estimate the effect of different aspects of the MeToo movement, we find it unlikely that the Weinstein scandal alone is driving our results. Many sexual scandals were uncovered before the MeToo movement and even though these scandals involved individuals at least as prominent as Weinstein (e.g., Donald Trump and Bill Cosby), they were not associated with large persistent increases in the interest in sexual misconduct, as demonstrated in Figure 1b.



## 3 Identifying the Effect of the MeToo Movement

### 3.1 International Crime Data

#### 3.1.1 Outcome: Reported Crimes

We build a new quarterly dataset with the number of crimes reported in 31 OECD countries representing 88% of the OECD population. We focus on the OECD because high-quality crime data is available for most OECD countries. The MeToo movement also began mostly among OECD countries, while there is still substantial variation in the strength of the movement between these countries.<sup>10</sup>

We separately obtain data for each country from 2010, or the first year for which data is available, until 2018.<sup>11</sup> We include in our sample countries that have quarterly, or more frequent, data available, disaggregated by sex crimes and non-sex crimes. For 25 of the countries, the data is based on the date the crime was reported to the police. For the remaining countries, the data is based on when the crime occurred or some combination of the occurrence and reported date. We harmonize the data by manually classifying offense categories as sex crimes or non-sex crimes for each country. Sex crimes are defined as all forms of sexual assault and sexual harassment. Non-sex crimes are defined as all other crimes. When possible, we exclude crimes that could have been indirectly affected by the MeToo movement, such as domestic violence.<sup>12</sup> For more details on the crime classification, see Appendix A.1. For more details on how we collected and processed each country's data, as well as a validation of the dataset using Eurostat annual estimates, see Appendix A.2.

#### 3.1.2 Measuring the Strength of the MeToo Movement

We use monthly Google Trends data on search behavior from 2010-2018 to create a proxy for the strength of the MeToo movement in each OECD country. The primary measure is based on the proportion of total Google searches for the "topic" of the MeToo movement. Google defines a search for a topic as any search query including a phrase directly linked to the topic in any language. Appendix A.3 provides more

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<sup>10</sup>Among the 30 countries with the highest number of tweets with MeToo related hashtags per Twitter user, in October 2017, 26 countries are OECD members. The Twitter data is described in Appendix A.4.

<sup>11</sup>We analyze data from 2010 due to data availability and since the beginning of a decade is a natural breakpoint in the time series. As discussed in Section 3.6.1, the main result is robust to other pre-period starting points.

<sup>12</sup>We exclude these crimes because non-sex crimes affected by the MeToo movement do not constitute a clean control group. Even after excluding these categories, it is possible that the MeToo movement still affected non-sex crimes, for example by increasing arrests of violent offenders. However, on average across countries the non-sex crimes category is 183 times larger than the sex crime category in the pre-MeToo period. Hence, any effect similar in absolute size across the two crime categories, would not substantially change our estimate. In other words, even if for every sex crime reported due to the MeToo movement, one non-sex crime was not reported, our results would remain virtually the same.

details on how the Google Trends data was processed and shows that our measure is highly correlated with an alternative search measure based on specific MeToo-related queries.

In our main specification, we use the interest in the MeToo movement during October 2017, the month the MeToo movement started. We categorize a country as having a *strong* MeToo movement if interest is above the OECD median and a *weak* MeToo movement if interest is below the OECD median. Figure 2 shows the interest of each OECD country.<sup>13</sup>

Appendix Figure A.3 confirms the validity of our primary measure for the strength of the MeToo movement by showing strong correlations with two alternative measures. First, we compare our search interest measure of the movement's strength with survey data from February-March 2019 on the fraction of the population that has heard of the MeToo movement (YouGov, 2019). The correlation between the two measures is 0.78. We also compare our search interest measure with the number of tweets with MeToo-related hashtags per 1,000 Twitter users in October 2017. The correlation between the measures is 0.83. The fact that Twitter activity was common in countries with a strong MeToo movement suggests that individuals in these countries did not merely search for information about the US movement, but took part in a local movement by amplifying its message. The Twitter measure is based on data collected by Morales Henry and Weintraub (2020) and is discussed in Appendix A.4.

We use Google as our main measure of the movement's strength since search activity reflects the interest of a much larger share of the population than tweets. Google is the dominant search engine in all of the countries in our sample, with a mean market share of search of 90%.<sup>14</sup> Therefore, search interest provides a consistent measure of the movement's strength across countries and over time. Section 3.6.1 shows that our result is robust to defining the strength of the movement using survey or Twitter data.

### 3.2 Empirical Strategy

We measure the causal effect of the MeToo movement on sex crimes reported to the police using a triple-difference strategy over time, across countries with strong and weak movements, and between

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<sup>13</sup>Sweden had an exceptionally high level of MeToo interest and is measured on a separate axis in Figure 2. There is no single explanation for why Sweden had such a high interest, but the combination of several high-profile sexual misconduct scandals, a strong and well-organized feminist movement, and previous movements discussing sexual misconduct are likely explanatory factors (Pollack, 2019). Our main result is virtually the same when removing Sweden from our sample.

<sup>14</sup>The mean market share is calculated based on data from [gs.statcounter.com](http://gs.statcounter.com) for October 2017. The minimum market share among all countries in our sample was 66%.

sex crimes and non-sex crimes:

$$y_{itc} = \beta_1 Post_t \times SexCrime_i \times StrongMeToo_c + \beta_2 Post_t \times SexCrime_i + \beta_3 Post_t \times StrongMeToo_c + \beta_4 Post_t + \delta_{ic} Trend_t + \gamma_{ic} Quarter_t + \varepsilon_{itc} \quad (1)$$

- $y_{itc}$  is the natural logarithm of the number of reported crimes of type  $i$ , in quarter  $t$ , in country  $c$ . We use the log transformation because it is plausible that the effect of the MeToo movement is relative to the number of sex crimes that would have been reported had there been no movement. However, using alternative outcome variables does not qualitatively change our results as shown in Table 2 and Appendix B.1
- $Post_t$  is an indicator for Q4 2017 (when the MeToo movement started) and later quarters
- $SexCrime_i$  is an indicator for whether crime category  $i$  is a sex crime
- $StrongMeToo_c$  is an indicator for whether country  $c$  had a strong MeToo movement
- $\delta_{ic} Trend_t$  and  $\gamma_{ic} Quarter_t$  control for differential linear time trends and seasonality by the full interaction of crime category and country fixed effects. Even though countries with strong and weak movements exhibit similar trends in the difference between sex crimes and non-sex crimes, we control for linear trends to rule out that differential country by crime type trends bias our estimate. We control for calendar quarter fixed effects because crime reports exhibit high seasonality (McDowall et al., 2012)<sup>15</sup>

The coefficient of interest,  $\beta_1$ , estimates the ATT. The counterfactual is a world in which the Weinstein sexual misconduct allegation had not been exposed and ignited a widespread social movement. In this counterfactual, the public discourse around sexual misconduct had continued similarly to the years prior.<sup>16</sup>Our identifying assumption is that without the MeToo movement, the difference between sex crimes and non-sex crimes would have changed in the same way from the pre-period to the post-period in the countries with strong and weak MeToo movements (after controlling for crime type and country-specific seasonality and linear time trends). For an omitted variable to explain the results, it would

<sup>15</sup>Section 3.6.2 shows that this specification is robust to a wide set of controls. In our main specification, we do not control for time by crime type or time by country fixed effects as there is no reason for such fixed effects to change our main estimate which is based on the relative change between crime types within countries. The results are almost identical when controlling for these fixed effects.

<sup>16</sup>Since we are estimating an ATT and not an Average Treatment Effect (ATE), the ideal experiment would be to ex-ante randomize the MeToo movement in countries that ex-post had a strong movement.

have to have a non-linear change after October 2017 that affects the number of reported sex crimes more than it affects non-sex crimes among countries where the MeToo movement was strong, as compared to countries where it was weak. While the strength of the MeToo movement is not random, we have no reason to believe it is correlated with an omitted variable affecting sex crimes differentially specifically in the post-period.

Observations are uniformly weighted in our main specification, allowing us to interpret the estimated effect as the average effect across countries with a strong MeToo movement. We cluster standard errors at the country by crime category level because that is where the MeToo movement varies. Appendix B.2 presents results using alternative statistical inference methods, including standard errors clustered at the country level and two-way clustered standard errors at the country by crime category and time level, and shows that using these methods decreases our standard errors.

For our primary specification in Section 3.3, we focus on the effects of the MeToo movement in the short run, defined as the first six months of the MeToo movement. In Sections 3.4 and 3.5, we estimate the effect for longer time periods. There are two main reasons for separately estimating the short-run effects. First, in the first six months there is a substantial difference in interest between countries with an initially strong movement and countries with an initially weak movement. Appendix Figure A.4 shows that after six months the gap in interest between the strong and weak countries declines. Therefore, we can only cleanly employ our triple-difference empirical strategy in the first six months. Second, during the initial six-month period there were, to the best of our knowledge, no major changes to laws governing sex crimes in any of the countries in our sample. Consequently, in this period, we can rule out that the effect is driven by a mechanical increase in the number of sex crimes due to a broader legal definition of what constitutes a sex crime.

### 3.3 Results

Table 1 shows that the MeToo movement increased the reporting of sex crimes. Column (1) uses data only on sex crimes to show a difference-in-differences estimate over time and between countries with strong and weak MeToo movements. Column (2) uses all 31 countries and shows a difference-in-differences estimate over time and between sex crime and non-sex crime. While the two columns use different sources of variation, they both find statistically significant effects of 11% and 8%, respectively.<sup>17</sup> Column

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<sup>17</sup>It is not surprising that Column (2) finds a smaller effect than Column (1) since it estimates the average effect for countries with both strong and weak MeToo movements.

(3) estimates the effect from Column (2) separately for countries with strong and weak movements and shows that the effect is driven by the countries that had a strong MeToo movement. These countries had an effect of 13%, while the point estimate is only 2% among countries with weak MeToo movements. Column (4) shows the results from our main triple-difference specification described in Equation 1. We find an effect of 10%, statistically significant at the 10% level ( $p\text{-value}=0.057$ ).<sup>18</sup> Finally, Column (5) presents our main specification with observations weighted by the country's population. Using these weights changes the interpretation of the estimate from the average effect of the MeToo movement on the number of sex crimes reported in countries that had a strong movement to the average effect of the movement on the population in these countries.<sup>19</sup> The estimated effect is similar, but more precisely estimated than Column (4) since we put more weight on countries with a large population that, on average, have less variation in the number of crimes reported.

In our main specification we assume that the MeToo movement occurred only in countries that had a strong movement. If the MeToo movement had some effect even in counties with weak movements, the triple-difference estimate is a lower bound for the effect in countries with a strong movement. Alternatively we could estimate the Intention to Treat effect of the global MeToo movement in all countries. Column (2) of Table 1 finds this effect to be 8% and significant at the 1% level. The difference between countries in this interpretation reflects heterogeneity in the effect of the global movement. We prefer the more demanding triple-difference estimator since it would only find an effect if the MeToo movement affected the countries that had strong local movements and not others.

Several coefficients in Table 1 can be interpreted as placebo tests. In Column (1), the coefficient on  $Post$  is the pre-post estimate for the effect of the MeToo movement on sex crime reported in countries with a weak MeToo movement. In Column (2), the coefficient on  $Post$  is the pre-post estimate for the effect on non-sex crimes in all countries. In Column (3), the coefficient on  $Post \times Sex\ Crime \times Weak\ MeToo$  is a difference-in-differences estimate, using variation over time and between crime types, for the effect on sex crimes reported in countries with a weak movement. In Columns (3)-(5), the coefficient on  $Post_t \times StrongMeToo_c$  is a difference-in-differences estimate of the effect on non-sex crimes in the post-period among countries with a strong movement. All four coefficients are close to zero. Because the placebo effects are close to zero, we receive similar estimates when we estimate the effect of the move-

<sup>18</sup>The estimate of the effect is 10.48 log points, which equals an 11.05% increase. For simplicity, we describe the effects in log points as percentage changes throughout the paper, although this slightly understates the magnitude of the effect.

<sup>19</sup>While both effects are of interest, we focus on the effect on the average country as it answers our main question about the effect of social moments on individual behavior. The population weighted analysis asks what was the effect of this particular movement on the average individual's behavior.

ment using only non-sex crimes or only countries with weak movements as the comparison group. We still prefer using the most demanding triple-difference estimator for our main specification since it relies on assumptions that are least likely to be violated.<sup>20</sup> The placebo estimates also confirm that our main estimate is driven by an increase in the number of sex crimes reported in countries with strong MeToo movements, and not by changes in any of the comparison groups.

### 3.3.1 Magnitude of The Effect

We provide intuition for the magnitude of the main effect by comparing it to the gap between the reporting of sexual and non-sexual assaults. If reporting of sexual assaults would have increased by 10% in countries included in the UN SDG database, the average reporting gap would have decreased by 8%.

To provide a more concrete point estimate, in Appendix B.1 we replicate Table 1 using crimes reported per 100,000 persons as the outcome variable.<sup>21</sup> We find that in the countries with a strong MeToo movement, the movement increased the reporting of sex crimes by 2.2 reports per 100,000 persons (p-value=0.012).

### 3.3.2 Visualizations of Results

We illustrate the triple-difference results in two ways: by presenting minimally processed data in Figure 3 and using an event study graph in Appendix Figure A.5.

Figure 3 shows that countries with strong and weak MeToo movements had similar trends before the movement started. The figure shows the difference between the number of sex crimes reported and the number of non-sex crimes reported for each group of countries. To calculate the difference, we index both sex crimes and non-sex crimes to be 100 in Q3 2017, the period before the MeToo movement started. We then average the difference between the indexes for countries with strong and weak movements separately. The timelines validate our triple-difference strategy by showing that there are no differential pre-trends between these groups of countries. The figure also shows that there is a substantial divergence between countries with a strong and weak movement after the start of the MeToo movement. Appendix B.3 presents the components of the triple-difference estimate and confirms the results from Table 1 that changes to the comparison groups do not drive our main estimate.

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<sup>20</sup>While one can imagine other events that affected only sex crimes specifically in the post-period, or only strong countries, it seems less likely that an event unrelated to the MeToo movement, affected only sex crimes, in the strong countries, in the post-period.

<sup>21</sup>We use 2016 OECD population data from <https://data.oecd.org/pop/population.htm>.

Appendix Figure A.5 further confirms that there are no differential pre-trends using an event study graph based on a regression similar to Equation 1. Each point is a coefficient on the triple interaction between a specific quarter (excluding Q3 2017 as the reference period), whether a crime is a sex crime, and whether the country had a strong MeToo movement. The figure follows our main specification closely and uses an outcome variable that is detrended and deseasonalized by subtracting the country by crime type calendar quarter fixed effects and linear trends calculated in the pre-MeToo period. The regression controls for time fixed effects separately interacted with the crime being a sex crime and the country having a strong MeToo movement. The figure shows that there was no pre-trend before the movement started and that the estimate for Q1 2018 has the largest absolute magnitude of all estimates.

### 3.4 Allowing Staggered Start Dates for the MeToo Movement

In our main estimate, we use a single start date for the MeToo movement and classify countries as either having a strong or weak movement depending on search interest in October 2017. In terms of identification, this is the cleanest estimate since this measure of the strength of the movement does not depend on events that occurred after October 2017, which minimize the risk for reverse causality. However, this specification does not exploit all of the variation in the data as it does not allow movements to occur after Q4 2017. In this section, we allow the MeToo movement to start on different dates for different countries. For each country, we define the start of the movement as the first quarter when cumulative Google search interest was above the OECD median cumulative interest in Q4 2017. Based on this classification, the movement started between Q4 2017 and Q3 2018 in all the countries in our data.

A recent literature has shown that the two-way fixed effects estimator is not an unbiased estimator of the ATT when the intervention is staggered and treatment effects are heterogeneous or dynamic (Callaway and Sant’Anna, 2020; De Chaisemartin and d’Haultfoeuille, 2020). Therefore, in Column (1) of Appendix Table A.2, we use the estimator proposed by De Chaisemartin and d’Haultfoeuille (2021) (DCDH) as our main estimate of the effect of the MeToo movement when allowing for different start dates across countries.<sup>22</sup> We turn the DCDH estimator from a difference-in-differences estimator to a triple-difference estimator by estimating the effect on the difference between the log of sex crimes and non-sex crimes. For the DCDH estimator, only countries where the movement had yet to start serve as a control group. Therefore, we can only estimate the effect in the first three quarters of the movement since,

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<sup>22</sup>When the independent variable is staggered and binary, as in our setting, this estimator is numerically equivalent to the estimator proposed by Callaway and Sant’Anna (2020) when no control variables are used. These approaches estimate an average treatment effect of 0.18.

based on our definition, one year after the global start of the movement, there are no countries where the movement has not yet started. For reference, we also include the three-way fixed effects (3WFE) estimator, which uses countries where the MeToo movement already started as a comparison group and is therefore biased if the effect is heterogeneous or changes over time.

The first row of Column (1) of Appendix Table A.2 finds an average effect of 11% in the first three quarters after the movement started. The effect is similar to our main estimate and to the 3WFE estimate but less precisely estimated as fewer countries are used for comparison. In the subsequent rows the effect is shown separately by quarter. Finally, placebo estimates are shown for the four quarters before the start of the MeToo movement. The placebo tests estimate the deviation from the parallel trends assumption for two consecutive periods. For example, Placebo 1 tests if trends are parallel between countries for the penultimate and last period before the start of the movement. All the placebos are close to zero.<sup>23</sup>

### 3.5 Persistence of the Effect over Time

Was the effect of the MeToo movement driven by a short-term increase in the salience of sexual misconduct or did the movement change underlying social norms leading to a lasting effect on behavior? To estimate the long-term effects, we cannot use the triple-difference strategy because in some of the countries where the MeToo movement was initially weak, it gained traction and became stronger after October 2017, as shown in Appendix Figure A.4. Thus our counterfactual is contaminated in later periods. Instead, we focus only on countries that had a strong MeToo movement and estimate a difference-in-differences specification over time and by crime type.

Column (1) of Appendix Table A.3 shows that the average effect of the MeToo movement in the first five quarters, among countries with a strong movement, is 11%. Column (2) shows that the effect is relatively stable and varies between 9% and 13% until the end of our data, 15 months after the movement started. Appendix Figure A.6 visualizes the persistence of the effect over time by using detrended and deseasonalized data and plotting the interactions of whether a crime is a sex crime with binary variables for each of the quarters in 2010-2018. This figure also shows that there is a discontinuous increase in reporting between the pre- and post-periods. One caveat to these results is that our estimates become more sensitive to small deviations from the parallel trends assumption when estimating effects further away from the movement's start date.

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<sup>23</sup>In Column (2), we estimate a similar effect using 3WFE by removing any observations where the movement is active and estimating the effect of a placebo movement starting 1-4 periods before the MeToo movement started in each country.



### 3.6 Robustness Checks and Placebo Test

In this section, we confirm the reliability of our main result by first performing a battery of robustness checks and then using alternative time and fixed effects controls. In addition to the tests below, Appendix B.5 conducts placebo tests where we estimate the effects of fictional MeToo movements set in each of the six-month periods from 2010 until the start of the movement. Our main estimate is larger than all the placebo estimates.

#### 3.6.1 Robustness Checks

Table 2 shows that our primary triple-difference estimator is robust to using different time periods, alternative regression specifications, alternative empirical strategies, and most alternative definitions for the strength of the movement. Row (1) repeats the main estimate from Column (4) of Table 1. Row (2) shows the effect of the MeToo movement during its first three quarters by extending the sample to end in Q2 2018. Row (3) shows the estimate using only half of the pre-period data, starting from 2014 onwards. Both robustness tests find an effect similar to that estimated by our main specification.

Rows (4)-(8) estimate the effect with different measures of the strength of the MeToo movement. In Row (4), countries are classified as having a strong movement based on Google searches for the MeToo topic between October 2017 and March 2018 (instead of using only October 2017), the same period for which we measure the number of reported crimes. Row (5) uses the cumulative search interest and the DCDH estimator as described in Section 3.4, but restricts the calculation of the average effect to the first two quarters of the movement in each country. Row (6) uses the increase in search interest in the sexual assault and sexual harassment topics at the start of the movement.<sup>24</sup> In Row (7), the movement's strength is based on the number of October 2017 tweets containing a hashtag related to the MeToo movement per Twitter user (see Appendix A.4 for more details). Row (8) uses the fraction of the population that has heard of the MeToo movement in February-March 2019 (YouGov, 2019). The analysis is conducted for the 12 countries in our sample where the survey was conducted. While some of these specification are less precise, four of the five alternative measures of the movement's strength provide a result similar to our main specification.

Rows (9)-(12) focus on alternative samples and specification and all find a result at least as large as our main specification and significant at the 5% or 1% level. Row (9) shows the result when dropping

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<sup>24</sup>The increase is measured for October and November 2017 as search interest peaked in November 2017. We measure increase by regressing search interest on the post-period while controlling for linear trends and seasonality.

the largest outlier country in terms of the absolute effect separately for countries with weak and strong MeToo movements. The effect size is similar but more precisely estimated as two small countries with large variation in crimes reported are excluded from the sample.<sup>25</sup> In a minority of countries, the crime data is based on the date crimes occurred instead of the date crimes were reported. This may downward bias our results as the MeToo movement could have affected the reporting of crimes that occurred in the pre-period. Row (10) shows the results of our main specification including only countries where the data is based on the date crimes were reported and shows that the effect is only slightly larger among these countries. To ensure that our specification of the outcome variable is not driving the result, Row (11) shows the triple-difference estimate when the outcome variable is the number of crimes reported, instead of the log of crimes reported. We normalize the number of crimes to have an average of one in the year before the start of the MeToo movement in each country by crime type category. The estimated effect is an 11% increase over the baseline year (Q4 2016 - Q3 2017). Row (12) shows the result is robust to using a negative binomial regression with the count data of crimes reported as the outcome variable.

Row (13) analyzes the data using the matrix completion method (Athey et al., 2021). When estimating the effect using this method, we do not explicitly control for any trends or fixed effects. Instead, the method creates a counterfactual for the number of sex crimes that would have occurred in countries that had a strong MeToo movement based on flexible patterns in the data. The method is described in more detail in Appendix B.4. Despite using a very different empirical strategy, the estimated effect is similar to our main estimate.

A potential problem with our main specification is that reverse causality could bias the results if an exogenous increase in sex crime reporting in October 2017 increased the interest in the MeToo movement and thus affected the classification of strong and weak movements. To rule out such a mechanism we instrument having a strong MeToo movement with the fraction of the population speaking English.<sup>26</sup> Since an increase in reported sex crimes could not have affected the fraction of the population speaking English, this estimate should not suffer from reverse causality bias. Row (14) shows that the point estimate of this two-stage least squares (2SLS) regression is similar to that of our main specification.<sup>27</sup>

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<sup>25</sup>Running our main specification while excluding one country at the time results in estimates ranging from 0.08 to 0.14, and p-values ranging from 0.01 to 0.11.

<sup>26</sup>We use Ethnologue data on the share of the population speaking English. We instrument the interactions of  $Post \times Strong\ MeToo \times SexCrime$  and  $Post \times Strong\ MeToo$  with the same interactions, where Strong MeToo is replaced with the share of English speakers. See Appendix A.5 for a description of the processing of the Ethnologue data.

<sup>27</sup>The Kleibergen-Paap Wald F-statistic in the first stage of our 2SLS regression is 59. The conventional F-statistic when regressing the strong MeToo indicator on the fraction of the population speaking English at the country level is 41.

### 3.6.2 Alternative Controls

Appendix Table A.4 shows that the magnitude of the main estimate is robust to removing and adding fixed effects and time controls. In Column (2), we show that the controls are not driving the result by estimating the most parsimonious triple-difference specification without any fixed effects. In Column (3), we estimate the basic three-way fixed effects model using country by crime type, country by time period, and crime type by time period fixed effects. Column (4) estimates our main specification but without controlling for linear time-trends and calendar quarter fixed effects. Columns (5) and (6) gradually add controls and show the effect is robust to separately adding the country by crime type calendar-quarter fixed effects and linear trends. In Column (7), we allow for quadratic time trends interacted with country by crime type fixed effects to account for more complex secular trends. Finally, in Column (8) we include all the controls from Columns (3)-(7). While the standard errors vary across these seven different robustness tests, the coefficient of interest remains between 0.10 and 0.15.

## 4 Heterogeneity and Effect on Arrests in the US

To study heterogeneity in the effects of the MeToo movement and downstream outcomes, we focus on the US because it is the largest OECD country and has rich incident-level crime data.

### 4.1 US Crime Data

#### 4.1.1 The FBI National Incident-Based Reporting System

US law enforcement agencies voluntarily report incident-level data using the National Incident-Based Reporting System (NIBRS) for 52 specific crimes. A main advantage of using NIBRS data is that the crime categories and the variables describing each incident are harmonized across agencies. This allows us to test for heterogeneous effects by crime type, the victim and offender characteristics, and whether an arrest was made.

In our main specification, we analyze 2010-2018 NIBRS data aggregated at the state by crime category level for each month. The data includes more than 7,000 reporting agencies, covering approximately 30% of the US population.<sup>28</sup> We classify crimes into two main categories: sex crime and non-sex crime. The data does not include sexual harassment, and therefore, our estimates based on the NIBRS data measure

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<sup>28</sup>We exclude agencies that started reporting data using the NIBRS in 2018 to alleviate any concern that participation in the program was affected by the MeToo movement. Our main result is almost identical when including these agencies.

the effect only on sexual assaults. Appendix A.6 provides more details on how the data was processed.

#### 4.1.2 Incident-Level Data from Cities

We collect incident-level data from eight large US cities with a combined population of 17 million in 2017: Austin, Denver, Kansas City, Los Angeles, Louisville, New York City, Seattle, and Tucson. Our sample consists of all cities that are among the 40 largest, provide incident-level data on all crimes, provide the dates crimes occurred along with the dates they were reported, and provide the crimes' approximate location. In our main specification, we aggregate data at the city by crime category by month level. Appendix A.7 provides more details on how the data was processed for each city.

The city data complement our analysis in three ways. First, the data includes virtually all crimes reported to the police, and not only the relatively severe offenses covered by NIBRS. This allows us to analyze the effect of the MeToo movement on sexual harassment, in addition to sexual assault. Second, we use the difference between the crimes' reporting date and the date they occurred to analyze heterogeneous effects according to whether the crime was reported immediately or with a lag. Third, the crimes' location allows us to analyze heterogeneity in the effect of the movement by neighborhood demographics.

## 4.2 Empirical Strategy

We analyze the US data using a difference-in-differences specification over time and by crime type. We do not use a triple-difference strategy, as we do not observe meaningful variation in the strength of the MeToo movement across US states or demographics based on both search and survey data (see Appendix Figure A.8).<sup>29</sup> This is unsurprising as the national media covered the movement and the allegations related to it. Furthermore, the movement generated substantial discussion on social media, which is not limited to a specific media market.

We use the following regression as our primary specification:

$$y_{itc} = \beta_1 \text{SexCrime}_i \times \text{Post}_t + \beta_2 \text{Post}_t + \delta_{ic} \text{Trend}_t + \gamma_{ic} \text{Month}_t + \varepsilon_{itc} \quad (2)$$

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<sup>29</sup>While the OECD country in the 75th percentile in terms of search interest had a 454% larger interest in the MeToo movement, compared to the country in the 25th percentile, the same figure for US states is only 45%. Furthermore, the variation between OECD countries was relatively stable over time with a correlation of 0.95 between interest in October 2017 and interest in November 2017, while the same correlation for US states is just 0.39. The low correlation suggests that a large part of the variation in interest between US states is due to noise and not differences in the strength of the MeToo movement.

Where  $y_{itc}$  is the inverse hyperbolic sine transformation (IHS) of the number of reported crimes of type  $i$ , in month  $t$ , in location (state, county, city, or neighborhood)  $c$ . The specification is similar to our triple-difference specification described in Equation 1 with several differences. First, The IHS transformation is used instead of a log transformation because there are months when no crime is recorded for a specific location and crime category. Second, we aggregate the data at the monthly level, instead of the quarterly level.<sup>30</sup> Third, we use robust standard errors. Since our main specification includes only two crime categories, we cannot cluster the standard errors at the crime category level. Appendix Table A.5 uses the same specification, with a finer aggregation of crime categories, which allows us to cluster the standard errors at the crime category level, and shows that the point estimates and standard errors remain similar. Fourth, we weight regressions by the average number of crimes reported in a location in the pre-period because we are interested in the effect of the MeToo movement on the number of crimes reported and not in the effect on an average state or city.

### 4.3 Results

We find that the MeToo movement had a strong and statistically significant effect on sex crimes reported based on both the NIBRS and city datasets. Column (1) of Appendix Table A.5 uses NIBRS data to show that the MeToo movement increased the number of reported sexual assaults in the US by 9% in the six months after the movement started. The effect found is equivalent to closing 29% of the US gap between the reporting of sexual assault and non-sexual assaults.

Column (1) of Appendix Table A.6 finds an effect of 10% on sex crimes reported in our city sample. To ensure that the effect in one city is not driving the results, we run our main specification separately for each city. Columns (2)-(8) show that the point estimates are positive for six of the eight cities in our sample.

In Appendix B.4 we show that both the NIBRS and the city results are robust to an estimation using the matrix completion method (Athey et al., 2021). This method is especially useful when analyzing US data since we take advantage of the disaggregated crime data and allow the method to generate a counterfactual using the crime types that best predict sex crimes.

Appendix Figure A.9 confirms that there are no differential pre-trends in the US. It presents quarterly effects comparing sex crimes and non-sex crimes using the NIBRS data. The outcome variable is the log

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<sup>30</sup>For each location, we exclude months when no crimes of any type were reported in that location. In these months police agencies most likely did not provide data to the NIBRS.

of reported crimes, detrended and deseasonalized by removing state by crime category calendar quarter fixed effects and linear trends calculated in the pre-period. The figure shows an immediate increase in the number of sex crimes reported at the start of the MeToo movement. Because most crimes in the NIBRS data are dated by the time they occurred, the MeToo movement could affect some crimes that occurred before the movement started but were reported as a result of the movement (such a bias would go against the direction of our finding). Indeed, Sub-Figure A.9a shows that the estimates for the two quarters preceding the MeToo movement are somewhat larger than previous quarters. Sub-Figure A.9b only includes the crimes in the NIBRS data that are dated by the time they were reported, for which the pre-period could not be affected by the movement, and shows that among these crimes there are no deviations from the parallel trends assumption.

Appendix Figure A.9 also shows that there is no decline in the magnitude of the estimated effects over time. Appendix Table A.7 confirms that the effect was persistent using regression frameworks with both NIBRS and city data, and when restricting the data to crimes that were reported in the same month that they occurred.

#### 4.3.1 Effect on Arrests

Using NIBRS data, Table 3 shows that the MeToo movement increased the number of arrests in sexual assault cases, but that this increase is disproportionately smaller than the effect on reporting.<sup>31</sup> In Column (1), the short-run effect is estimated by aggregating reports into three categories: sexual assaults resulting in an arrest, sexual assaults not resulting in an arrest, and non-sex crimes, which is the control group. We find a 10% increase in sexual assault reports that did not result in an arrest, while reports resulting in an arrest increased by 5%. One concern with this specification is that the effect could be driven by a change in the arrest rate over time for all crimes. Columns (2) and (3) show that the results are similar when we run the regression separately for reports resulting in an arrest and reports not resulting in an arrest so that the comparison group is in the same arrest category. Columns (4)-(6) repeat the analysis for the long-run effect over 15 months and find similar results.

Why is the relative effect on arrests smaller than the effect on reports? Appendix Table A.9 provides suggestive evidence that the movement had a stronger effect on cases where the probability of arrest is low. We analyze sexual assaults at the incident level and show that controlling for observable character-

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<sup>31</sup>An arrest is defined as a case where a suspect is taken into custody based on a warrant or a previously submitted report, arrested on view (without a warrant), or summoned to court. Between January 2010 and September 2017, 20.2% of sexual assaults in the NIBRS data led to an arrest. If a case did not lead to an arrest for reasons outside the control of the police, it could still be classified as cleared. In Appendix Table A.8 we estimate the effect of the movement on cases cleared by the police.

istics of reported crimes can explain most of the decrease in the arrest *rate* after the movement started. Furthermore, as we discuss in the next section, the MeToo movement initially had a stronger effect on cases reported more than a month after they occurred and it may be more difficult to make an arrest in these cases.<sup>32</sup>

### 4.3.2 Heterogeneity

**Heterogeneous Effects by Demographics** The MeToo movement has been criticized for focusing on White victims of high socioeconomic status and ignoring the experiences of working-class women and women of color (Onwuachi-Willig, 2018). Based on the analysis of victim, offender, county, and neighborhood demographics, we find that the effect of the movement was larger for female victims, male offenders, and politically liberal counties. However, we do not find evidence that the MeToo movement mostly affected the reporting of Whites or those with high socioeconomic status. These results are not intended to capture causal relationships between demographic characteristics and the effect of the movement. Instead, the results show what societal groups the movement affected.

Figure 4 tests for heterogeneous effects among victims by separating sexual assault into sub-categories according to the victim demographics.<sup>33</sup> The first panel shows that the movement had a larger effect on female victims compared to male victims (the difference is statistically significant at the 5% level). This is consistent with the general narrative of the MeToo movement, which tended to focus on female victims. The second panel finds an almost identical effect on Black and White victims. The last panel repeats the analysis according to the offender's demographics and points to a similar effect among Black and White offenders.

Table 4 shows that the MeToo movement had a substantial effect across counties with different demographic profiles. To test for these heterogeneous effects, we run a regression similar to Equation 2 at the county level where we interact county demographics with the post-period by sex crime interaction, while controlling for demographics interacted with the post-period.<sup>34</sup>

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<sup>32</sup>Anecdotal evidence suggests that it was challenging for the police to make arrests in MeToo-related cases because they were reported long after they occurred. See, for example, Maddaus, Gene - Many Accused, None Prosecuted: Why #MeToo Hasn't Led to a Single Criminal Charge in L.A. *Variety*. September 25, 2019.

<sup>33</sup>For example, when estimating heterogeneous effects by race, the treated categories are sexual assaults of Black victims and sexual assaults of White victims, and the reference category is non-sex crimes.

<sup>34</sup>Each demographic variable is constant across time and its weighted mean is subtracted to keep the estimate for the effect of the MeToo movement consistent across specifications. Data on county-level income, education, race, and ethnicity is based on the American Community Survey 5-year 2016 estimates. The share of Trump voters in each county is based on the MIT Election Data and Science Lab (2018). To limit the number of cells with no crimes reported, we exclude counties with a population of less than 10,000. To ensure that the demographics of the populations covered by the agencies are similar to the demographics of counties where the agencies are located, we exclude county-years where the police agencies reporting data cover less than

Columns (2)-(7) present heterogeneity for each demographic variable and show that while some of the coefficients are statistically significant, the magnitudes of most of the effects are small relative to the total effect of the movement. For example, while counties with a larger share of college graduates are associated with a slightly larger effect, the difference in the effect on reporting between a county in the 75th percentile of college education and a county in the 25th percentile is only expected to be 2 percentage points, compared to the average effect of 9%. One exception to the relatively homogeneous effects is the difference between counties with different political ideologies, as measured by the share of Trump voters in the 2016 election. The difference in the expected effect between a county in the 25th percentile of Trump voters and a county in the 75th percentile is 7 percentage points. Since both Republicans and Democrats heard a lot about the movement as shown in Appendix Figure A.8b, our interpretation is that the different effect among Trump and Clinton voters is due to heterogeneous effects of the movement and not due to variation in exposure to the movement.

In Appendix B.6, we use the more detailed city data to analyze heterogeneity at the neighborhood level. In this data, we find a somewhat larger heterogeneity by income, but still show that the movement had an effect across neighborhoods with different demographic attributes.

It may seem surprising that we do not find a larger effect on Whites or those with high socioeconomic status, but the results are consistent with survey data. In a nationally representative sample of women conducted in March 2018, respondents were asked whether the MeToo movement represented their interests. Among Whites, there were 28 percentage points more respondents who felt the movement represented them than those who did not feel represented. Among Blacks and Hispanics, the corresponding figures were substantially higher at 40 and 45 percentage points, respectively (Morning Consult, 2018).<sup>35</sup>

**Heterogeneous Effects by Crime Type** In Appendix Figure A.10, we test for heterogeneity by crime type. The figure shows that the movement had a large effect on the number of rapes reported, the most severe sexual offense category in the NIBRS data, and on fondling cases. The figure also shows that the movement had a stronger effect on offenses where the victim was not physically injured (the difference between sexual assaults with and without injury is statistically significant at the 1% level). We do not

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85% of the population.

<sup>35</sup>We define respondents who felt the movement represented them as those who said the movement represented them 'very well' and 'somewhat well' and define respondents who were not represented as those who said 'not well' or 'not too well'. Consistent with our results, the survey found no substantial difference between those earning more or less than \$50,000. However, the net feeling of being represented among Clinton voters was +62 percentage points, while it was -8 percentage points for Trump voters.



find differential effects based on whether the victim knew the offender or whether the incident involved multiple victims. Finally, using the city data, we find a larger effect on sexual harassment compared to sexual assault. However, estimates using this dataset are less precise and the difference is not statistically significant.

**Effects on the Stock and Flow of Crimes** The effect we find on reporting could stem from victims reporting a stock of old crimes. Indeed, many of the high-profile cases discussed in the media following the MeToo movement occurred years before the movement started. Understanding whether the stock or flow of crimes was affected is important for our interpretation of the effect. If the movement mostly affected the stock of existing crimes, we would expect the effect to decline once this stock has been depleted.

Appendix Table A.10 shows that while the MeToo movement had a stronger effect on crimes reported at least 30 days after they occurred, the movement also affected the flow of crimes that were reported within 30 days. For this analysis, we use the city-level data and aggregate crime into three main categories: sex crimes reported more than 30 days after they occurred, sex crimes reported within 30 days, and non-sex crimes, which is the reference category. Column (1) shows that the movement increased the number of crimes reported within 30 days by 7% and increased the number of crimes reported more than 30 days after they occurred by 19%. Column (2) of Table A.10 shows that the long-run effects on the stock and flow of crimes are similar to the short-run estimates.

## 5 Mechanisms and Interpretation

In this section, we investigate potential mechanisms for how the MeToo movement increased the reporting of sex crimes. We begin by presenting two survey datasets which we analyze throughout the section. We then show that the effect on reporting reflects an increase in the propensity to report crimes, in contrast to an increase in the incidence of sex crimes or legal changes in what is considered a sex crime. Finally, we provide evidence for and against different mechanisms through which the MeToo movement may have increased the propensity to report.

## **5.1 Data**

### **5.1.1 AAU Campus Climate Survey on Sexual Assault and Sexual Misconduct**

The Campus Climate Survey (CCS) measures the prevalence of sexual misconduct in US universities (Association of American Universities, 2020). The survey was conducted in 2015 and 2019 with 21 universities participating in both surveys. The CCS data benefits our analysis in four ways. First, it measures both the incidence of sex crimes as well as the reporting of these crimes to university programs, such as the Title IX office. Second, it contains data on students' attitudes and beliefs regarding sexual misconduct, reporting, and university resources. Third, the survey clearly defines each type of sexual misconduct carefully, which minimizes the risk of effects being driven by changing interpretations of questions. Fourth, the large sample of both reporting and non-reporting victims provides sufficient power to disentangle changes in the incidence from changes in the propensity to report. One caveat is that the survey asks about reporting to university programs. If different mechanisms affect students reporting to the university compared to other victims reporting to the police, the channels we find when analyzing this data may not apply to the non-student population. Appendix A.8 provides more information on the CCS data processing.

### **5.1.2 Views of the Electorate Research Survey**

The Views of the Electorate Research Survey (VOTER) is a large panel of adults focusing on political attitudes, values, and affinities. We focus on questions related to sexual harassment asked in the July 2016, April-May 2018, and November 2018-January 2019 surveys. Compared to other surveys with questions on sexual misconduct, the VOTER data provides several advantages: it asks the same questions before and after the MeToo movement started, the panel allows us to control for individual fixed effects, and the timing of the survey was not affected by the movement.

## **5.2 Did the Movement Affect Incidence or the Propensity to Report Sex Crimes?**

The effect of the MeToo movement on the number of sex crimes reported could be driven by an increase in the incidence of crimes (a backlash effect) or an increase in the propensity to report crimes. We use two separate approaches to isolate the effect on reporting propensity from an effect on incidence: focusing on crimes that occurred before the movement started and estimating changes in incidence and reporting rates directly.

### 5.2.1 Crimes that Occurred Before the Movement Started

We rule out that an increase in incidence is driving the entire increase in reporting by restricting our analysis to crimes committed *before* the start of the MeToo movement. We use data from our sample of US cities since it includes both the date the crime occurred and the date it was reported. Appendix Table A.11 analyzes crimes that were reported at least 1-5 months after they occurred and that were reported before a certain date. For example, in Column (4), we only include crimes that were reported at least four months after they occurred and that were reported by January 2018, i.e., crimes that occurred before the start of the movement in October 2017. The estimated effect of the movement on the number of crimes reported is still strong and statistically significant and the result is robust to using different lags between a crime's occurrence and its reporting.<sup>36</sup> The incidence of these crimes could not have been affected by the movement. Thus, the effect is only driven by reporting propensity.

### 5.2.2 Self-Reported Incidence and Propensity to Report

Figure 5 analyzes the CCS data to show that the incidence of sexual assaults among students *decreased* after the start of the MeToo movement, while the reporting *rate* (the propensity to report) increased.

Sub-Figure 5a presents coefficients from a regression where the outcome is whether a student experienced a sexual assault in a particular year and the variables of interest are indicators for each of the five years leading up to the 2019 CCS survey. We arrange the data such that each observation is a student and the year when an incident could have occurred. To avoid confounding the effect of the MeToo movement with changes resulting from the lag between the incident occurrence and the year the survey was conducted (e.g., a recall bias), we include fixed effects for the number of years between the survey and the observation year. Hence, the regression is effectively comparing incidence in each school year preceding the survey with a school year that preceded the previous survey by the same number of years. For example, we compare answers regarding the 2017-2018 school year provided in the 2019 survey with answers regarding the 2013-2014 school year provided in the 2015 survey. We also control for student characteristics, university fixed effects, and the university-level survey response rate.

Column (2) of Appendix Table A.12 reports a difference-in-differences estimate using the difference between the last two years each survey asked about and other years, as well as the difference between

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<sup>36</sup>As expected, these effects are larger than our main estimates since the table focuses on crimes reported with a lag. These crimes were more strongly affected by the MeToo movement, as shown in Table A.10. The effects may even be downward biased if, due to the movement, some of the crimes that would otherwise be reported more than 1-5 months after they occurred were reported with a shorter lag.

the 2015 and 2019 surveys. Using this estimate, we find a decrease of 1.4 percentage points in the annual sexual assault rate after the start of the MeToo movement.<sup>37</sup> If the incidence of sexual crimes decreased as a result of the movement, our primary estimates of the effect on the number of crimes reported to the police should be interpreted as lower bounds for the increase in the propensity to report sexual crimes, as these estimates are reduced by a decrease in crime incidence.

Sub-Figure 5b analyzes a regression similar to the one presented in Sub-Figure 5a and shows that following the MeToo movement the reporting rate of unique sexual assaults increased.<sup>38</sup> Column (6) of Appendix Table A.12 uses the difference-in-differences design and shows that the propensity to report sexual assault increased by 5 percentage points, or 40% compared to the 2015 survey mean. This estimate is not directly comparable to the effects discussed in the previous sections, as it is estimating the effect on the reporting rate and not the total number of reported crimes. Appendix A.8 provides more information on the empirical specifications used throughout this section.

In contrast to the analysis in Sections 3 and 4, our analysis of the CCS data relies mostly on variation over time, and we do not have a clean comparison group. Therefore, the results should be interpreted more carefully. Still, the sharp increase in the reporting rate around the MeToo movement is consistent with our previous results and with responses to hypothetical survey questions—in October 2018, 62% of Americans stated that if it happened to them, they would be more likely to report sexual harassment now, compared to a year ago (Ipsos, 2018a).

### 5.2.3 Changes to Laws and Government Policy

The MeToo movement could also have increased incidence by changing the laws governing sex crimes, for example, by expanding the types of behavior classified as illegal. We find evidence against this mechanism, at least in the short term. A report by the International Lawyers Network (2019) shows that among the 11 OECD countries covered by the report, no country made changes to laws governing sexual misconduct between the start of the MeToo movement and the end of Q1 2018.<sup>39</sup> The lack of

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<sup>37</sup>In Appendix B.7, we analyze changes in respondents' stating that they were a victim of a crime using NCVS data. Complementing our main results, we find that these changes are unlikely to be driven by a change in incidence but are instead a consequence of the MeToo movement increasing the willingness to report sex crimes in a survey or expanding respondents' definitions of what behavior constitutes a sex crime.

<sup>38</sup>We focus on unique sexual assaults, defined as cases where the victim did not experience any other incident of physical sexual misconduct, since it is not possible to determine which incident was reported in cases where a student was a victim of multiple incidents. Columns (3) and (4) of Appendix Table A.12 show that our conclusion that incidence did not increase holds when focusing on unique sexual assaults. In addition to the controls used in Figure 5a, in Figure 5b we also control for the type of sexual assault since all our observations are now cases of sexual assault.

<sup>39</sup>A few countries changed laws concerning sex crimes later in 2018, such as Iceland and the US, where the earliest changes took effect in the second quarter of 2018, and Sweden, where the change took effect in the third quarter of 2018. These changes

legal changes in the immediate aftermath of the MeToo movement is not surprising given that passing legislation is a lengthy process, often taking more than a year.

### 5.3 How did the MeToo Movement Affect the Propensity to Report Crimes?

This section examines three broad mechanisms for the increased propensity to report crime. First, the MeToo movement could have increased the benefits victims receive from reporting if victims perceived sexual assaults to be a more important social problem (Battaglini et al., 2020). Second, the movement could have decreased costs associated with reporting, such as stigma (Bursztyn et al., 2020). Finally, the movement could have changed the beliefs about how the authorities would respond to a report.

#### 5.3.1 Increased Benefit from Reporting

Following the MeToo movement, victims may have learned that sexual misconduct is a larger social problem than they thought. Similarly, victims could have changed their perception of the severity of specific cases of sexual misconduct. These changes could increase reporting if they increased victims' perceptions of how much society would benefit from reporting a crime or if they resulted in victims receiving greater benefit from taking part in a movement normalizing the reporting of sexual crimes (Bursztyn et al., 2021).

Table 5 uses survey data to provide evidence consistent with this explanation. Column (1) of Sub-Table 5a shows that the public's agreement with the statement "*sexual harassment against women in the workplace is no longer a problem in the United States*" decreased in the 2018-2019 VOTER surveys, compared to 2016. Column (1) of Sub-Table 5b provides similar evidence using the CCS data and shows that more students believe that sexual assault or sexual misconduct is problematic at their university in the 2019 survey, compared to 2015. Furthermore, Column (2) shows that the *perceived* likelihood of experiencing sexual assault increased. Appendix Table A.13 repeats the analysis of all these questions among women, who are more likely to be victims of sexual misconduct, and finds similar effects.

To further explore why reporting rates increased, we analyze a CCS question asking students who experienced sexual assault, and did not report it, to provide all the reasons for why they did not report. The explanations that would have been given by students who *did* report as a result of the MeToo movement should be less common in the 2019 survey (the post-period). Thus, we can explore which ex-  

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could have affected the long-run estimates but could not have directly influenced reporting in the first six months of the movement.

planations substantially decreased between 2015 and 2019 to better understand the mechanisms driving the effect of the movement. This analysis should be interpreted cautiously since the possible answers provided to respondents are not identical in the two surveys. We still choose to analyze this question because it is rare to observe victims' explanations for not reporting before and after a sharp change in the reporting rate.

Appendix Figure A.11 shows that substantially fewer students felt that the sexual assault was not serious enough to report, consistent with the hypothesis that due to the movement, victims considered some sexual assault cases to be more severe. The share of students choosing the 'not serious enough to report' explanation decreased by 13 percentage points from 2015 to 2019.<sup>40</sup> Among all the potential answers provided in the 2015 and 2019 surveys, this explanation saw the largest absolute change.

US national surveys conducted after the MeToo movement started are also consistent with victims considering specific acts of sexual misconduct to be more serious. In October 2018 Ipsos surveys, 54% of respondents agreed with the statement "*my views on what constitutes sexual harassment have become more clear*" (Ipsos, 2018a), and 24% of employed respondents agreed that "*The #MeToo movement has made me realize now that I may have been a victim of sexual harassment in the workplace*" (Ipsos, 2018b). Finally, this is consistent with Batut et al. (2021) who find that the MeToo movement increased French women's propensity to quit toxic work environments by changing "*what is acceptable in the workplace for women*", without changing actual workplace practices.

### 5.3.2 Decreased Costs

The MeToo movement could also have decreased the perceived cost associated with reporting. Specifically, the movement may have reduced the stigma and shame associated with being a sexual assault victim. However, we do not find evidence that the movement affected stigma. Table 5a shows that agreement with the statement "*women who complain about harassment often cause more problems than they solve*" did not decrease in the 2018-2019 VOTER survey data, compared to 2016.<sup>41</sup>

Costs could also have decreased if victims felt less shame following the MeToo movement. However, Appendix Figure A.11 shows that there was no decrease in the share of students not reporting sexual assaults because they felt ashamed or because it was emotionally difficult.

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<sup>40</sup>The decrease is robust to controlling for student characteristics, university fixed effects, response rate, and total enrollment, as well as the type of sexual assault.

<sup>41</sup>An important caveat in this analysis is that we do not observe second-order beliefs. While we do not find evidence for decreased stigma, victims may still have believed that stigma decreased.

### 5.3.3 Beliefs about the Response of Authorities

Reporting may have increased if the MeToo movement led victims to believe that other victims of the same offender are more likely to also report to the police, and thus corroborate a pattern of behavior (Cheng and Hsiaw, 2021). In Appendix Figure A.10, we use the NIBRS data to estimate the effect of the movement on incidents with a single victim and multiple victims. We do not find differential effects on these groups, suggesting that the movement did not operate mostly by increasing coordination between victims of the same incident (it could still have increased coordination across incidents).

Even without expecting more corroborating evidence, victims may have believed that there is a greater probability that their report would be taken seriously following the movement. However, using the CCS data, we do not find evidence for such a change in beliefs. Sub-Table 5b shows that between 2015 and 2019 there was no increase in students' perceived likelihood that "*campus officials would take the report seriously*" nor is there an increase in the perceived likelihood that "*campus officials would conduct a fair investigation*". Appendix Table A.13 shows that among women, there was actually a decrease in both perceived likelihoods. Furthermore, Column (5) of Appendix Table A.12 shows that the MeToo movement not only affected the reporting rate of sexual assault, it also increased the rate at which victims tell friends and family about sexual assaults. If the main mechanism affecting reporting was the perceived response of authorities, we would not expect to find a substantial effect on victims discussing the assaults with people close to them.

Finally, students' self-reported explanations also suggest that the perceived response from the university is not the driving mechanism. In Appendix Figure A.11, we do not find a decrease in the share of students stating that they did not report to the university because no one would believe them or since they feared the report would not be kept confidential.

To conclude, the mechanisms most consistent with the survey data is that the MeToo movement increased victims' motivation to report crimes because individuals perceived sexual misconduct to be more widespread and considered specific acts of sexual misconduct to be more serious. This suggests that social movements can be effective by increasing the perceived importance of taking certain actions (i.e., reporting a crime to the police) or changing the social norms of what is the appropriate action in particular circumstances (i.e., reporting an incident that previously would not be considered serious enough to report). In these cases, social movements can affect behavior even without changing expectations regarding society's response to the behavior or the stigma associated with the behavior.

## 6 Conclusions

This study shows that the MeToo movement led to a substantial increase in the number of sex crimes reported to the police. This result is consistent across multiple datasets from independent sources and robust to various estimation techniques. Using US data, we show that the effect reflects an increase in the propensity to report crime and is not driven by an increase in incidence.

We estimate that in its first six months the MeToo movement led to the reporting of 26,371 additional sex crimes among the 16 countries with strong MeToo movements for which we have data. In the first 15 months, 69,738 sex crimes were reported as a result of the movement.<sup>42</sup> 33,963 of these crimes were sexual assaults reported in the US.

We also find that in this period the movement led to 4,647 arrests in the US. Arrests may prevent repeat offenders from committing more crimes and deter potential offenders from committing sex crimes. Therefore, the movement may have had a positive effect on welfare, but an assessment of the overall welfare consequences is outside the scope of this paper.

Our finding that the MeToo movement affected individual behavior may be relevant for other social movements as well. The MeToo movement is an example of a modern social movement gaining prominence in recent years. Social media has enabled these new movements to raise awareness at a larger scale, within shorter time spans, and with limited organizing structures (Enikolopov et al., 2020). Several other social movements, such as Black Lives Matter and March for Our Lives, have had similar success in raising awareness for their causes, often using new digital technologies (Pew Research Center, 2018). We leave it to future research to determine whether these movements also strongly affected behavioral outcomes.

This paper suffers from several limitations. First, our dataset is composed of 31 countries. This limits our power and does not allow us to test for complicated functional forms in the relationship between the strength of the MeToo movement and reporting sex crimes. Second, our analysis of heterogeneity and mechanisms focuses on the US due to data availability. Third, while we find that the effects persist over 15 months, our data does not allow us to estimate longer-term effects and our parallel trends assumption is more likely to be violated for longer-term estimates. Finally, while we provide evidence consistent with specific mechanisms, our analysis of why the propensity to report increased is not conclusive, and

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<sup>42</sup>We use the difference-in-differences specification to estimate the effect of the MeToo movement for each country with a strong movement separately. We then compare the actual number of reported sex crimes with the predicted number of reported sex crimes if the MeToo movement had not taken place. The calculation for the countries where we have partial police data (Australia, the UK, and the US) is based on the assumption that the MeToo movement had the same effect per capita on areas for which we obtained data as in other areas in the country.



the mechanisms we suggest should be further explored in future studies.

Overall, our findings show that social movements can have large effects on meaningful and costly personal decisions. This effect may occur almost immediately and can persist over time.

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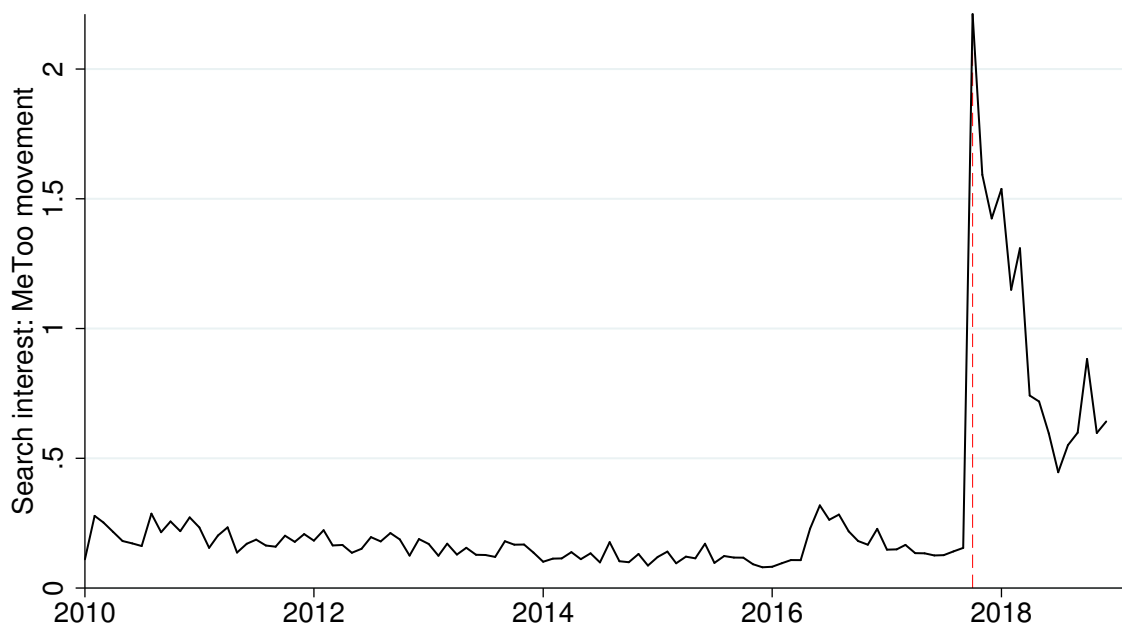
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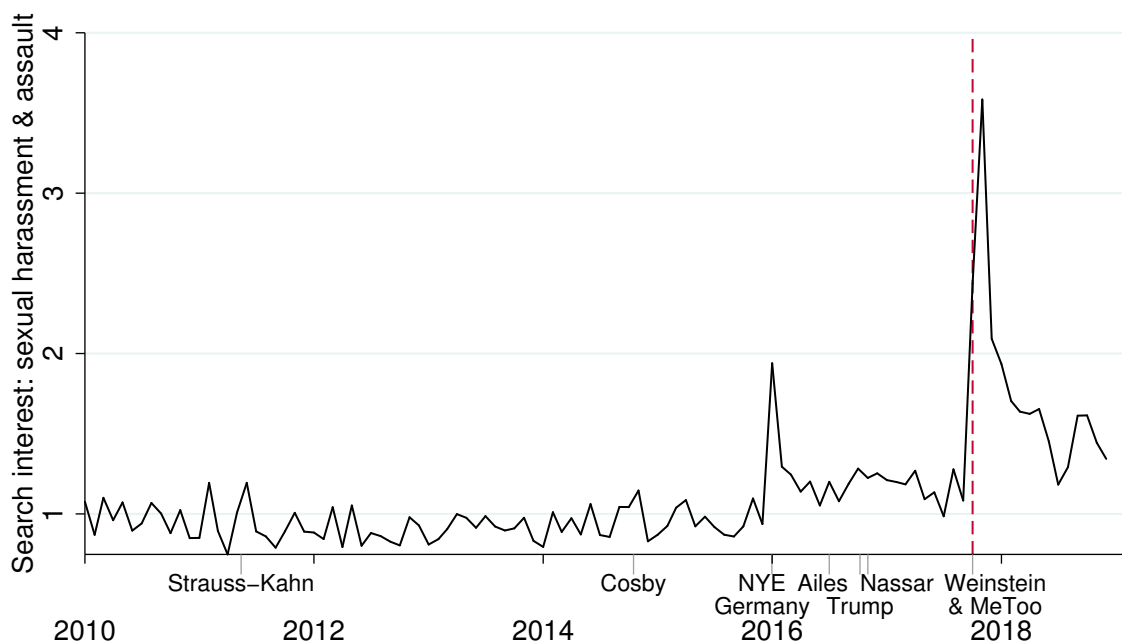
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Figure 1: MeToo-Related Google Search Interest in the OECD

(a) Interest in the MeToo Movement

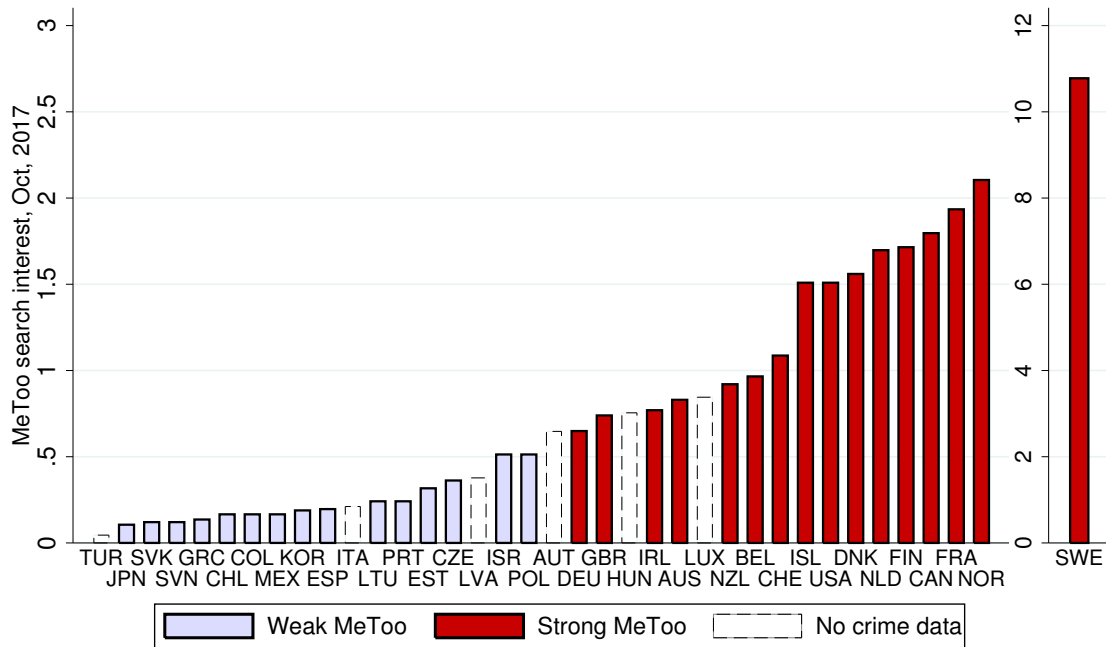


(b) Interest in Sexual Harassment and Sexual Assault



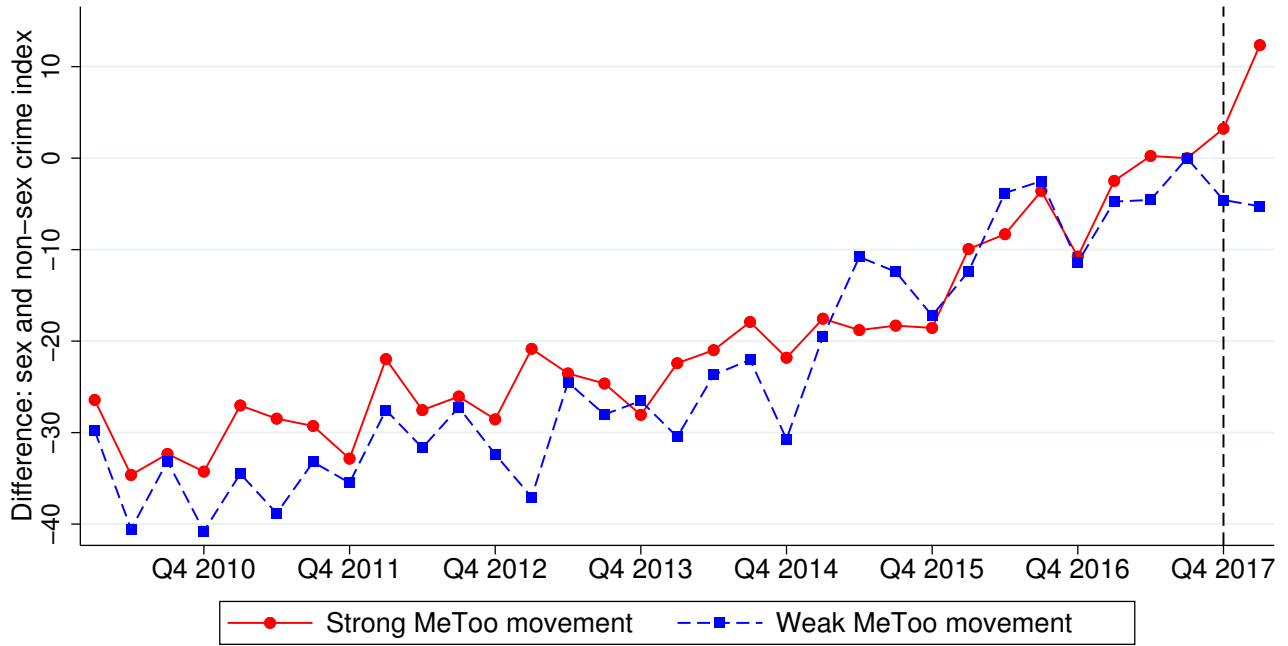
These figures show the OECD mean monthly search interest in the MeToo movement and in sexual harassment and sexual assault from 2010 to 2018. Data is from Google Trends. Sub-Figure (a) presents the search interest in the topic of the MeToo movement. The variable is normalized so that the post-MeToo OECD mean equals 1. Sub-Figure (b) presents the search interest in the topics of sexual harassment and sexual assault. The variable is normalized so that the pre-MeToo mean equals 1 for each country. The dates when six high-profile sexual misconduct cases were first widely reported are indicated below the time axis. The cases are those involving: Dominique Strauss-Kahn, Bill Cosby, the 2015–16 New Year’s Eve sexual assaults in Germany, Roger Ailes, the Donald Trump Access Hollywood tape, Larry Nassar, and Harvey Weinstein. The vertical dashed lines represent the start of the MeToo movement (October 2017).

Figure 2: Immediate Search Interest in the MeToo Movement



This figure shows the strength of the MeToo movement in OECD countries based on Google search interest in the topic of the MeToo movement during October 2017. Search interest is normalized so that the OECD average is one. The Weak MeToo group of countries have below-median interest, the Strong MeToo group of countries have above-median interest, and the rest of the countries are not included in our sample since we have not obtained access to their reported crime data.

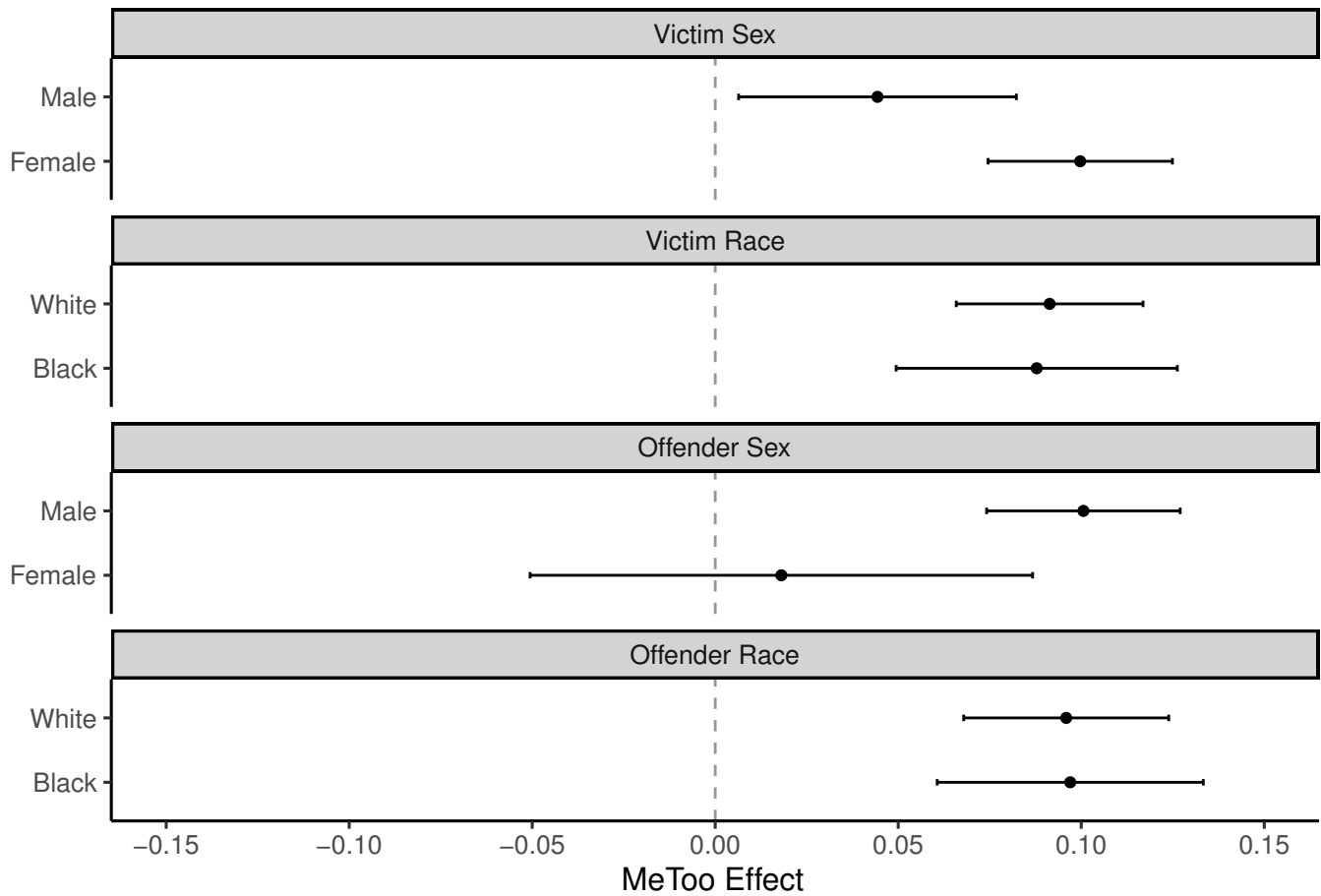
Figure 3: Difference in Sex and Non-Sex Crimes Reported by MeToo Movement Strength



This figure visualizes the triple-difference estimate by showing the difference between the number of sex crimes reported and the number of non-sex crimes reported for countries with strong and weak MeToo movements. Both crime categories are normalized to 100 in the period before the start of the MeToo movement (Q3 2017) for each country, causing the difference in that period to be zero by construction. The differences are averaged separately for the countries with strong and weak MeToo movements. The vertical dashed line represents the start of the MeToo movement. Data includes all 31 countries in our sample. For 7 countries, data is available for only part of the period; see Appendix A.2 for details.

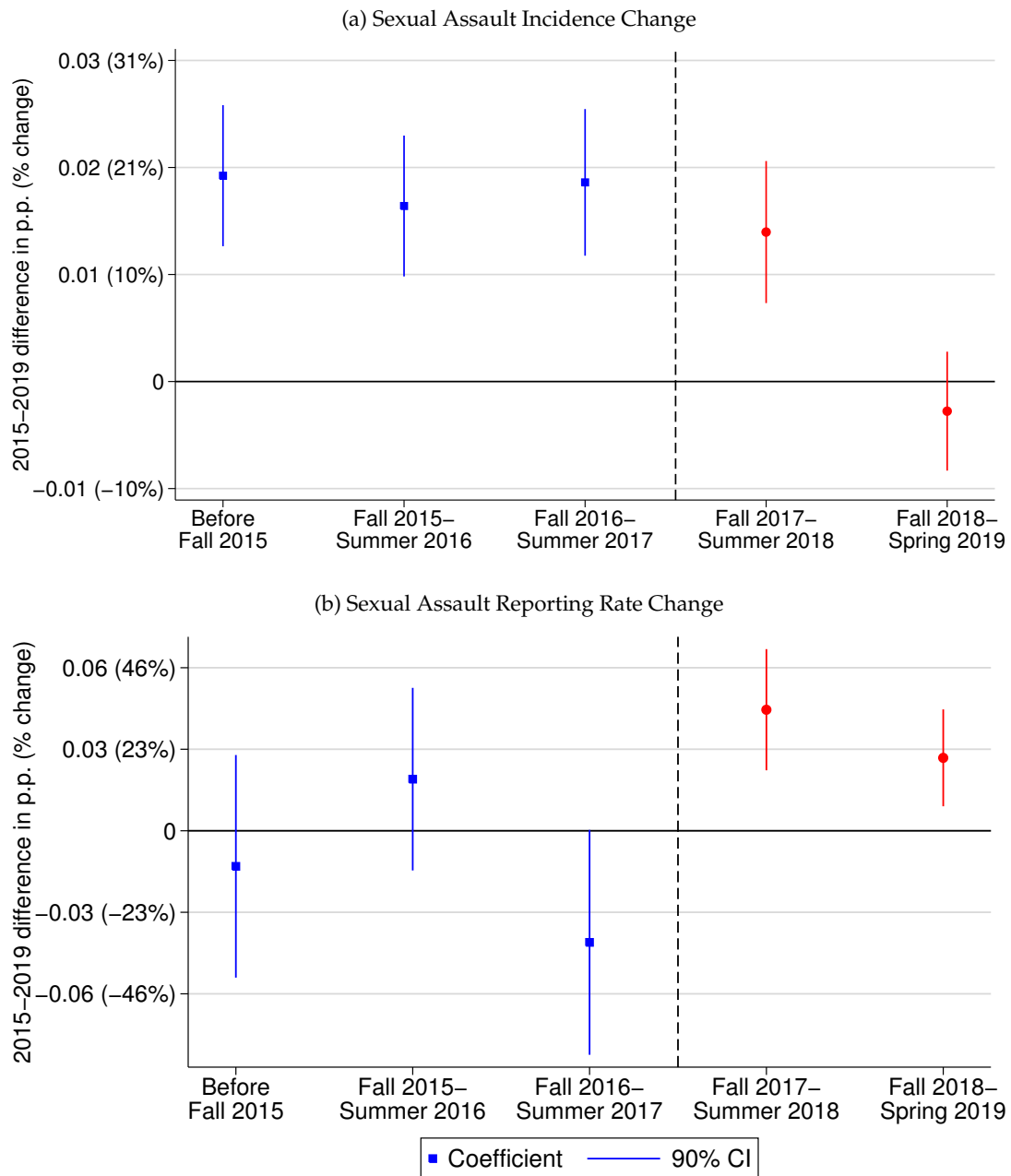


Figure 4: Effect of the MeToo Movement by Victim and Offender Demographics



This figure shows the effect of the MeToo movement by victim and offender demographics. Each panel presents the results from a regression where crimes are aggregated into different categories and the reference group is all non-sex crimes. Sex crimes with multiple victims or offenders are excluded. All regressions are weighted by the number of crimes in each state before the MeToo movement started. 2010-2018 NIBRS data.

Figure 5: Changes in the Incidence and Reporting Rates of Sexual Assault, 2015-2019 CCS Data



These figures plot regression coefficients on variables for a school year preceding the 2019 CCS. In Sub-Figure (a), each observation is a year by an enrolled student. The outcome variable is whether the student was the victim of a sexual assault in that year. In Sub-Figure (b), each observation is a unique sexual assault, defined as an assault for a student who did not experience any other incident of physical sexual misconduct. The outcome variable is whether the student reported the sexual assault to a university program. The regressions control for years between the observation year and the survey, university fixed effects, survey response rate, total enrollment, and student characteristics. Sub-Figure (b) also controls for sexual assault type. Samples weighted to be representative of the student population in surveyed universities. The vertical axes show the 2015-2019 change and the percentage change from the 2015 mean in parenthesis. 90% confidence intervals are created using standard errors clustered at the university level. The vertical dashed line indicates the start of the MeToo movement. Appendix A.8 provides more information on the empirical specifications and Appendix Table A.12 shows the results in a table format.

Table 1: The Effect of the MeToo Movement

	ln(crime)				
	(1)	(2)	(3)	(4)	(5)
Post * Strong MeToo	0.113** (0.047)		0.008 (0.028)	0.008 (0.028)	0.004 (0.030)
Post * Sex crime		0.077*** (0.029)		0.023 (0.041)	0.013 (0.032)
Post * Sex crime * Strong MeToo			0.128*** (0.035)	0.105* (0.054)	0.103** (0.042)
Post * Sex crime * Weak MeToo			0.023 (0.041)		
Post	0.032 (0.037)	0.014 (0.014)	0.010 (0.018)	0.010 (0.018)	0.013 (0.022)
Country * Crime type * Lin. trend	X	X	X	X	X
Country * Crime type * Quarter	X	X	X	X	X
Crime data used	Sex crimes	All crimes	All crimes	All crimes	All crimes
Weights	Uniform	Uniform	Uniform	Uniform	Population
Final quarter	Q1 2018	Q1 2018	Q1 2018	Q1 2018	Q1 2018
Observations	913	1,826	1,826	1,826	1,826
Clusters	31	62	62	62	62

This table shows the effect of the MeToo movement on sex crimes reported during the first six months of the movement. The outcome variable is the log of the number of reported crimes in a specific category, in a country, and in a quarter. Column (1) analyzes only sex crime using a difference-in-differences estimate over time and across countries. Columns (2)-(5) include data on both sex crimes and non-sex crimes. Column (2) presents a difference-in-differences estimate over time and between crime types. Column (3) presents the estimate from Column (2) separately for countries with strong and weak MeToo movements. Column (4) presents our primary triple-difference estimate over time, across countries, and between crime types. Column (5) shows the triple-difference estimate weighted by the 2016 population of each country. Using these weights changes the interpretation of the estimate from the average effect of the MeToo movement on the number of sex crimes reported in countries that had a strong movement to the average effect of the movement on the population in these countries. A country is categorized as having a strong MeToo movement if search interest for the topic of the MeToo movement was above the OECD median in October 2017. Data from 31 OECD countries from Q1 2010 to Q1 2018. Standard errors clustered at the country by crime level in parenthesis. \*\*\*p<0.01, \*\*p<0.05, \*p<0.1.

Table 2: Robustness Checks

(1)	Preferred specification	0.105* (0.054)
Length of measurement period:		
(2)	9 month effect	0.096* (0.053)
(3)	Pre-period start in 2014	0.105*** (0.039)
Different measures of MeToo strength:		
(4)	6 month MeToo search interest	0.102* (0.057)
(5)	Cumulative search interest (DCDH estimator)	0.085 (0.070)
(6)	SA/SH immediate search interest	0.027 (0.056)
(7)	Tweets per twitter users	0.107* (0.054)
(8)	% heard of MeToo movement (12 countries)	0.087 (0.078)
Alternative samples and specifications:		
(9)	Excluding outliers	0.112** (0.046)
(10)	Only data based on date crimes were reported	0.127** (0.062)
(11)	Outcome variable: normalized number of crimes	0.112** (0.054)
(12)	Negative binomial regression	0.116*** (0.045)
Alternative empirical strategies:		
(13)	Matrix completion method	0.108** (0.049)
(14)	2SLS: fraction English speakers as IV	0.093 (0.066)

This table shows robustness checks for our main triple-difference estimate. Row (1) repeats the main estimate from Column (4) of Table 1. Row (2) measures the effect over 9 months. Row (3) only uses pre-period data from 2014 onwards. Rows (4)-(8) use different measures of the strength of the MeToo movement. Row (9) shows the result when dropping the two outlier countries with the largest absolute effects among the countries with weak and strong MeToo movements separately. Row (10) only includes data from the 25 countries basing their statistics on the date the crimes were reported to the police. In Row (11) crimes are normalized to equal one on average for each country by crime type group, in the year leading up to the start of the MeToo movement (instead of using the log of crimes as the outcome). Row (12) shows the result of a negative binomial regression using the count data of crimes reported as the outcome variable. Row (13) shows the result of the matrix completion method, described in Appendix B.4. Row (14) shows the result of a two-stage least squares regression where having a strong MeToo movement is instrumented by the fraction of English speakers. All rows except Rows (8) and (10) use data from 31 OECD countries. Row (8) uses data from the 12 OECD countries surveyed in the 2019 YouGov survey. Standard errors clustered at the country by crime type level are in parenthesis. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table 3: Effect of the MeToo Movement on Arrests

	ihs(crime)					
	(1)	(2)	(3)	(4)	(5)	(6)
Post * Sexual Assault, No Arrest	0.097*** (0.015)			0.101*** (0.011)		
Post * Sexual Assault, Arrest	0.045* (0.026)			0.062*** (0.017)		
Post * Sexual Assault		0.096*** (0.016)	0.060** (0.027)		0.104*** (0.011)	0.079*** (0.019)
Difference	0.052*			0.040**		
State * Crime Type * Lin. Trend	X	X	X	X	X	X
State * Crime Type * Month	X	X	X	X	X	X
Post	X	X	X	X	X	X
Final Month	Mar 18	Mar 18	Mar 18	Dec 18	Dec 18	Dec 18
Crimes	All	No Arrest	Arrest	All	No Arrest	Arrest
Observations	10,314	6,876	6,876	11,259	7,506	7,506

This table shows the effect of the MeToo movement on sexual assaults by whether an arrest was made. An arrest is defined as an incident where a suspect is taken into custody based on a warrant or previously submitted report, arrested on view without a warrant, or summoned to court. In Columns (1) and (4), the crimes are aggregated into three separate crime categories: sexual assaults for which an arrest was made, sexual assaults for which no arrest was made, and non-sex crimes, which are the control group. In Columns (2) and (5), only crimes where no arrest was made are included and Columns (3) and (6) include only crimes where an arrest was made. Columns (1)-(3) focus on the short-run effect and Columns (4)-(6) focus on the long-run effect. 2010-2018 NIBRS data. Regressions are weighted by the number of crimes in each state before the MeToo movement started. Robust standard errors in parenthesis. \*\*\*p<0.01; \*\*p<0.05; \*p<0.1

Table 4: Effect of the MeToo Movement by County Demographics

	ihs(crime)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post * Sexual Assault	0.094*** (0.012)	0.094*** (0.012)	0.094*** (0.012)	0.094*** (0.012)	0.094*** (0.012)	0.094*** (0.012)	0.094*** (0.012)
Post * Sexual Assault * Med. Income (std. dev.)		0.013 (0.009)					
Post * Sexual Assault * % College			0.125 (0.112)				
Post * Sexual Assault * % Blacks (Compared to Whites)				0.081 (0.077)			
Post * Sexual Assault * % Other Race (Compared to Whites)					0.561*** (0.218)		
Post * Sexual Assault * % Hispanics						0.296*** (0.112)	
Post * Sexual Assault * % Vote Trump							-0.272*** (0.088)

Interquartile Range of Demographic Diff. in Effect, 75th-25th Pct.		1.249 0.016	0.134 0.017	0.194 0.016	0.055 0.031	0.062 0.019	0.265 -0.072
County * Crime Type * Lin. Trend	X	X	X	X	X	X	X
County * Crime Type * Month	X	X	X	X	X	X	X
Post	X	X	X	X	X	X	X
Post * Demographic							
Final Month	Mar 18	Mar 18	Mar 18	Mar 18	Mar 18	Mar 18	Mar 18
Observations	174,004	174,004	174,004	174,004	174,004	174,004	174,004

This table shows the effect of the MeToo movement based on county-level data and tests for heterogeneous effects by county demographics. 2010-2018 NIBRS data. All regressions are weighted by the number of crimes in each county before the MeToo movement started. The demographic variables are first subtracted by their weighted mean. Robust standard errors in parenthesis. \*\*\*p<0.01; \*\*p<0.05; \*p<0.1

Table 5: Changes in Perceptions of Sexual Misconduct Following the MeToo Movement (in Standard Deviations)

(a) Perceptions of Problem and Individuals Reporting, VOTER Data

	Workplace sexual harassment is no longer a problem (1)	Accusers often cause more problems than they solve (2)
2018-2019 surveys	-0.128*** (0.026)	0.029 (0.024)
Ref. Group	2016 Survey	2016 Survey
Respondent FE	X	X
Observations	11,010	10,993

(b) Perceptions of Size of Problem and University Response to Reporting, CCS Data

	Scale of problem		University response	
	How problematic? (1)	Will you experience? (2)	Officials take seriously (3)	Fair investigation (4)
2019 survey	0.151** (0.069)	0.068** (0.032)	-0.000 (0.026)	-0.006 (0.040)
Ref. Group	2015 survey	2015 survey	2015 survey	2015 survey
Controls	X	X	X	X
University FE	X	X	X	X
Clusters	21	21	21	21
Observations	237,712	239,149	226,149	224,918

Sub-Table (a) shows changes in VOTER respondents' agreement with: "sexual harassment against women in the workplace is no longer a problem in the United States" and "women who complain about harassment often cause more problems than they solve". Answers are coded on a four-point scale between "strongly disagree" and "strongly agree". The post-period is the 2018 and 2019 surveys and the reference period is the 2016 survey. The regressions are weighted to make the sample representative of the US population. Robust standard error in parenthesis. Columns (1) and (2) of Sub-Table (b) show changes in University students' perception of "how problematic is sexual assault or sexual misconduct at [university]" and how likely it is that they "will experience sexual assault or sexual misconduct on campus". Columns (3) and (4) of Sub-Table (b) present students' beliefs on how likely it is that "campus officials would take the report seriously" and "campus officials would conduct a fair investigation" if someone were to report a sexual assault or sexual misconduct. In all columns of Sub-Table (b), answers are coded on a five-point scale between "not at all" to "extremely". Standard errors clustered at the university level are in parenthesis. In both tables, outcomes are recast as standard deviations away from the sample mean. \*\*\*p<0.01; \*\*p<0.05; \*p<0.1

# Appendix For Online Publication

## A Data Processing

### A.1 Crime Classification

We classify each crime as belonging to one of the following categories: sexual assault, defined as a sex crime that includes physical contact; sexual harassment, defined as a sex crime that does not include physical contact (e.g., stalking and indecent exposure); non-sex crimes; and crimes which are not directly affected by the MeToo movement but could be indirectly related to it. Crimes indirectly related to the MeToo movement include bestiality, bigamy, sex crimes against children (when victims are all 15 years of age or younger), domestic assault, harassment where it is not clear if the harassment is of sexual nature, incest, pornography, prostitution, and registration of sexual offenders. When possible, we exclude these crimes from the analysis since spillovers from the MeToo movement can affect this group of crimes, and therefore, they are not a suitable control group. We also exclude cases appearing in police records that are not related to any specific crime (e.g., missing person investigations) and traffic tickets. Throughout most of the analysis, we aggregate the sexual assault and sexual harassment crimes into one category, defined as sex crime.

### A.2 OECD Crime Data Collection and Processing

To collect high-frequency crime data from as many OECD countries as possible, we first downloaded the data available on the websites of the statistics agencies and the police. If no data was available online, we contacted both the main statistics agency as well as the national police requesting data on the number of crimes reported at a monthly or quarterly level. Finally, if these contacts did not yield the required data, we filed the equivalent of a Freedom of Information Act request or purchased data specifically aggregated for our project from the statistics agency.

To quality control the international data, we crosschecked our data with the 2017 Eurostat data on sexual violence for the 20 countries in both samples. Reassuringly, the correlation in the number of sex crimes per capita is 0.97.<sup>43</sup> The average percentage difference between the number of crimes in the two datasets is -2% corroborating that the data we collected is in line with EU estimates. Finally, the average absolute percentage difference between the datasets is 26%, showing that the two numbers are similar in magnitude for most countries. The difference could be explained by the fact that we excluded specific sex crimes that are not directly related to the MeToo movement (such as crimes against children) and since we include crimes that can appear outside the sexual assault category, such as stalking.

In Australia, the United Kingdom, and the United States, high-frequency data on the number of crimes reported are not available for the whole country.<sup>44</sup> For Australia, we have data for New South Wales, Queensland, Victoria, and Western Australia, covering 88% of the population, but not for the

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<sup>43</sup>We could not use the Eurostat data in our analysis because it is only available at an annual level.

<sup>44</sup>In the US, crime data for many agencies is also available through the Summary Report System. We do not use that dataset since the definition for rape has changed in 2013 and agencies are gradually changing their reports based on the new definition. Furthermore, this system only collects data on the most severe crimes, and therefore it does not include data on sexual assaults besides rapes.



Australian Capital Territory, Northern Territory, South Australia, and Tasmania. For the United Kingdom, we have data for England, Northern Ireland, and Wales, covering 92% of the population, but not for Scotland. For the United States, we use the NIBRS data, which is described in Appendix A.6.

For most countries in the data, crimes are aggregated into quarters based on the date the crime was reported.<sup>45</sup> For Belgium, Colombia, Germany, and Iceland, crimes are aggregated based on the date when they occurred. For the UK and US, some crimes are aggregated based on the quarter they were reported, while other crimes are aggregated based on the quarter they occurred.

### A.2.1 Data Source and Processing by Country

**Australia** We received data separately for each state from the following sources: the Crime Statistics Agency of Victoria, the New South Wales Bureau of Crime Statistics and Research, the Queensland Police<sup>46</sup>, and the Western Australia Police Force. All the data was downloaded or received by email in June-July 2019.

For Victoria, all sexual offenses are classified as sex crimes. The stalking, harassment and threatening behaviour category is excluded (we exclude this category because we do not know whether the harassment is sexual in nature). For New South Wales, sexual assault and "indecent assault, acts of indecency, intimidation" are classified as sex crimes. Assaults related to domestic violence, pornography, prostitution offenses, offensive conduct, and "intimidation, stalking, and harassment" are excluded. For Queensland, rape and attempted rape, other sexual offenses, and stalking are classified as sex crimes. Breach of domestic violence protections and prostitution are excluded. For Western Australia, sexual assault, history of sexual assault, and non-assaultive sexual offenses are classified as sex crimes. Common family assault, serious family assault, and threatening behaviour by a family member are excluded. All other crime categories are classified as non-sex crimes. After classifying crimes for each state separately, we add the crime categories together to get totals for Australia.

**Belgium** We received a spreadsheet by email of reported offenses for each quarter in 2010-2018 on July 8, 2019 from the Federal Police of Belgium. Rape, sexual assault, voyeurism, public decency actions (exhibitionism, etc.), and public decency - obscenities in public are classified as sex crimes. Child pornography, grooming, pornography, incitement to fornication, sexual assault of a minor, exploitation of fornication, public decency (other than exhibitionism and obscenities), and offenses against the family are excluded. All other crime categories are classified as non-sex crimes.

**Canada** After a formal application, we received a spreadsheet by email of monthly incident-based crime statistics by offense types for 2010-2018 on August 12, 2019 from the Canadian Centre for Justice Statistics. Sexual assault (levels 1, 2, and 3) and voyeurism are classified as sex crimes. Sexual interference, invitation to sexual touching, luring a child via computer, or agreement/arrangement - sexual offenses against child, other sexual offenses against children, criminal harassment, commodification of

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<sup>45</sup>For Switzerland, the crime is aggregated based on the quarter the case was transmitted to the Federal Statistical Office. For the vast majority of crimes, this quarter is the quarter when the crime was reported to the police, and therefore, we group Switzerland with other countries using the reporting date when analyzing these countries separately in the robustness section.

<sup>46</sup><https://www.police.qld.gov.au/maps-and-statistics>

sexual activity violations, indecent/harassing communications, non-consensual distribution of intimate images, and other violent crimes (as this category includes both sex crimes and non-sex crimes) are excluded. All other crime categories are classified as non-sex crimes.

**Chile** We received a spreadsheet via email of monthly reported offenses for 2010-2018 on September 13, 2019 from *Policía de Investigaciones (PDI)*. *Delitos sexuales* (sex offenses) are classified as sex crimes. *Violencia intrafamiliar* (domestic violence) is excluded. All other crime categories are classified as non-sex crimes.

**Colombia** We downloaded spreadsheets of incident-level crime data for 2010-2018 on April 19, 2021 from *Policía Nacional*.<sup>47</sup> *Delitos sexuales* (sex offenses) are classified as sex crimes. We exclude *violencia intrafamiliar* (domestic violence) and crime categories that are not available for all years: *homicidios* (homicides), *terrorismo* (terrorism) and extortion. All other crime categories are classified as non-sex crimes.

**Czech Republic** We downloaded spreadsheets of reported offenses for each month and year for the period 2010-2018 on December 14, 2018 from *Policie České republiky*.<sup>48</sup> Paragraphs 201, 202, 211, and 212 (rape, sexual pressure, sexual abuse in dependence, and other sexual abuse) are classified as sex crimes. Paragraphs 174, 186, 190, 213, 214, 231, 251, 271, 280, 281, 290, 633, and 890 (torture of a person living in a common dwelling, dangerous persecution, commercial form of sexual abuse in dependence, other sex deviations, pimping, incest, trafficking in humans, other moral offenses, and trafficking in children) are excluded. All other crime categories are classified as non-sex crimes.

**Denmark** We downloaded a spreadsheet of reported criminal offenses for each quarter for 2010-2018 on May 30, 2019 from Statistics Denmark.<sup>49</sup> Rape, any kind of sexual offense (heterosexual, homosexual, and other), and offenses against public decency (grouping, indecent exposure, and other) are classified as sex crimes. Any kind of sexual offenses against a child under 12 or under 15, prostitution, and family relation offenses are excluded. All other crime categories are classified as non-sex crimes.

**Estonia** We received a spreadsheet by email of registered crimes for each month for 2010-2018 on July 12, 2019 from the Estonian Ministry of Justice. Paragraphs 141-143 and 153'1 (rape, acts of sexual nature against will, compelling a person to engage in sexual intercourse or other act of sexual nature, compelling a person to satisfy sexual desire, sexual intercourse or other acts of sexual nature using influence and sexual harassment) are classified as sex crimes. Paragraphs 118'1, 133, 144-147, 157'3 and 175-179 (disabling female genital mutilation, pimping, aiding prostitution, sexual intercourse with descendant, sexual intercourse or other act of a sexual nature with a child, buying sex from minors, satisfaction of sexual desire with a child, inability of person of less than ten years to comprehend, harassing pursuit, requesting access to child pornography and watching thereof, aiding prostitution involving minors, use

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<sup>47</sup><https://www.policia.gov.co/grupo-informacion-criminalidad/estadistica-delictiva>

<sup>48</sup><https://www.policie.cz/clanek/statisticke-prehledy-kriminality-za-rok-YYYY.aspx>

<sup>49</sup><https://www.statbank.dk/statbank5a/SelectVarVal/Define.asp?MainTable=STRAF10&PLanguage=1&PXSID=0&wsid=cftree>

of minors in manufacture of pornographic works, use of minors in manufacture of erotic works, manufacture of works involving child pornography or making child pornography available, agreement of sexual purpose for meeting with child and sexual enticement of children) are excluded. All other crime categories are classified as non-sex crimes.

**Finland** We downloaded a spreadsheet of reported crimes for each month for 2010-2018 on May 30, 2019 from *Tilastokeskuksen*.<sup>50</sup> Paragraphs 20:1-2 (rape, aggravated rape) and paragraphs 20:4-5, 5a, 8-9 (other sex crimes) are classified as sex crimes. Paragraphs 20:6-7, 7b (Sexual abuse of a child, aggravated sexual abuse of a child, aggravated rape of a child) and traffic offenses are excluded. All other crime categories are classified as non-sex crimes.

**France** We downloaded a spreadsheet of monthly reported crimes for 2010-2018 on July 23, 2019 from *Ministère de l'Intérieur*.<sup>51</sup> *Atteintes sexuelles* (Sexual abuse), *Viols sur des majeur(e)s* (rape of adult), and *Harcèlements sexuels et autres agressions sexuelles contre des majeur(e)s* (sexual harassment and other sexual assault against adult) are classified as sex crimes. *Proxénétisme* (pimping), *Viols sur des mineur(e)s* (rape of minors), *Harcèlements sexuels et autres agressions sexuelles contre des mineur(e)s* (sexual harassment and, other sexual assault against minors), and *Vols à main armée contre des particuliers à leur domicile* (violent unarmed flights against individuals at home) are excluded. All other crime categories are classified as non-sex crimes.

**Germany** We downloaded spreadsheets of monthly reported crimes for each year for 2012-2018 on March 18, 2019 from *Bundeskriminalamt*.<sup>52</sup> We classify all crimes under *Schlüssel* (crime number) 100000 (offenses against sexual self-determination) as sex crimes except crime number 140000 (exploiting sexual inclinations) as it is related to prostitution and pornography. We also classify crime number 232400 (stalking) as a sex crime. We exclude crime numbers 012000, 236000, 238100, 238300, 239110, 239200, 611000, 624010, 670013, 670014, 673140, and 673100 (sexual murder, trafficking in human beings with intent to sexual exploitation, promotion of trafficking in human beings, promotion of trafficking in human beings in connection with trafficking in human beings with intent to sexual exploitation, trafficking in human beings with intent to sexual exploitation, forced prostitution, blackmail on a sexual basis, simulating a crime against sexual self-determination, bigamy, incest, denigrating the memory of the deceased on a sexual basis, and insult/defamation on a sexual basis). All other crime categories are classified as non-sex crimes.

**Greece** We downloaded data on monthly reported crimes for the years 2010-2018 on July 29, 2019 from the Hellenic Statistical Authority (ELSTAT).<sup>53</sup> Rape and attempted rape are classified as sex crimes. All other crime categories are classified as non-sex crimes, excluding the broad category "crimes of a sexual

<sup>50</sup>[https://pxnet2.stat.fi/PXWeb/pxweb/en/StatFin/StatFin\\_oik\\_rpk\\_tiet/statfin\\_rpk\\_pxt\\_117t.px/](https://pxnet2.stat.fi/PXWeb/pxweb/en/StatFin/StatFin_oik_rpk_tiet/statfin_rpk_pxt_117t.px/)

<sup>51</sup><https://www.data.gouv.fr/en/datasets/chiffres-departementaux-mensuels-relatifs-aux-crimes-et-delits-enregistres-par-les-services-de-police-et-de-gendarmerie-depuis-MONTH-YYYY/>

<sup>52</sup>[https://www.bka.de/DE/AktuelleInformationen/StatistikenLagebilder/PolizeilicheKriminalstatistik/pks\\_node.html](https://www.bka.de/DE/AktuelleInformationen/StatistikenLagebilder/PolizeilicheKriminalstatistik/pks_node.html)

<sup>53</sup><http://archive.data.gov.gr/dataset/statistikh-epethrida>

nature" as it includes several crime types not directly related to the MeToo movement, such as child pornography, sexual exploitation of children, pimping, etc.

**Iceland** We received a spreadsheet by email of reported crimes for each month for 2010-2018 on December 12, 2020 from *Ríkislögreglustjóri*. Rape, sexual exploitation, sexual coercion, sexual harassment, sexual offenses against children under 18 years of age, child abuse (including individuals up to 18 years of age), violations of modesty, negligence regarding age according to Article 201 or 202 (including individuals up to 18 years of age), sexual intercourse of an employee with a resident of an institution, and requesting sex with another person/offering it in a public advertisement are classified as sex crimes. Grooming, incest, sexual intercourse with or sexual harassment of a foster child, sexual intercourse with or sexual harassment of a child under 15 years, abusing facilities for sex, as well as all pornography and prostitution related crimes are excluded. All other crime categories are classified as non-sex crimes.

**Ireland** We downloaded a spreadsheet of reported crimes for each quarter for 2010-2018 on May 31, 2019 from the Central Statistics Office.<sup>54</sup> Sexual offenses are classified as sex crimes. All other crime categories are classified as non-sex crimes.

**Israel** We downloaded a spreadsheet of reported crimes for each month for 2010-2018 on July 3, 2021 from the Central Bureau of Statistics.<sup>55</sup> We classify sexual offenses as sex crimes. We exclude prostitution. All other crime categories are classified as non-sex crimes. Crime statistics from each month are reported from the start of the year until the end of the month. Monthly statistics are obtained by deducting the previous month's yearly total from each month's yearly total. Each data file reports each month's yearly total for a given year and updated statistics for the equivalent time period from the preceding year. Monthly data for the years 2010-2013 and 2015-2018 are extracted from data files for the years 2011-2014 and 2016-2019. Monthly data for the year 2014 is extracted from the 2014 data files as updated data was not released in most of the 2015 files.

**Japan** We downloaded spreadsheets with monthly crimes reported for 2016-2018. We downloaded the data in January-May 2019 from the National Statistics Center website.<sup>56</sup> We classify rape, sexual assault, and publicly obscene acts as sex crimes. We exclude the category Other: Obscene, sexual assault, crime of marriage, etc. Each file covers crimes from the start of the year to the end of a particular month. To obtain monthly data for the calendar months February - December, we take the difference between each month's total and the previous month's total. Data for the year 2015 comes from the 2016 data files as 2015 was the comparison year in 2016.

**South Korea** We downloaded 2014-2018 crime data from the Korean Institute of Criminology and Prosecution Service (Quarterly Crime Trend Reports) and downloaded 2010-2013 data from the Prosecution Service and Police aggregated by the Korean Statistical Information Service. The datasets were retrieved

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<sup>54</sup><https://data.cso.ie/>

<sup>55</sup><https://www.cbs.gov.il/en/publications/Pages/default.aspx>. At least one month was missing and we received the data through an email exchange with the Central Bureau of Statistics

<sup>56</sup><https://www.e-stat.jp/stat-search/files?page=2&toukei=00130001&kikan=00130&second=1&second2=1>

between March and May 2019.<sup>57</sup> We classify all types of sexual violence (rape, indecent act by compulsion, rape causing bodily injury/death, taking sexual photographs, home intrusion of sexual nature, obscene acts using communication technology, and indecent acts in crowded places, rapes, and violations of the "Act On The Protection of Children and Juveniles Against Sexual Abuse") as sex crimes. We exclude traffic violations from the data.

**Lithuania** We downloaded monthly reported crimes for each quarter in 2012-2015 and 2017-2018 on July 15, 2019 from *Informatikos ir Rysiu Departamentas*.<sup>58</sup> Data for 2016 is not available. Sexual assault and rape are classified as sex crimes. All other crime categories are classified as non-sex crimes.

**Mexico** We downloaded a spreadsheet of monthly reported crimes for 2015-2018 on May 24, 2021 from *Instituto Nacional de Estadística y Geografía*.<sup>59</sup> *Abuso sexual, violación simple, violación equiparada, acoso sexual, hostigamiento sexual, Otros delitos que antentan contra la libertad y la seguridad sexual* (sexual abuse, rape, sexual harassment, and other sex crimes) are classified as sex crimes. *Incesto* and *violencia familiar* (incest and domestic violence) are excluded. All other crime categories are classified as non-sex crimes.

**The Netherlands** We downloaded a spreadsheet of monthly reported crimes for 2012-2018 on July 7, 2019 from *Korps Nationale Politie*.<sup>60</sup> All crimes in the sex crimes category are classified as sex crimes. Child pornography and child prostitution are excluded. All other crime categories are classified as non-sex crimes.

**New Zealand** We downloaded a spreadsheet of monthly reported crimes from July 2014 to December 2018 on June 21, 2019 from the New Zealand Police.<sup>61</sup> Aggravated sexual assault and non-aggravated sexual assault are classified as sex crimes. Incest is excluded. All other crime categories are classified as non-sex crimes.

**Norway** We received spreadsheets by email of incident level data for reported sex crimes and quarterly data for other crime types on July 8, 2021 from *Politidirektoratet*. We analyze data from 2016-2018 because a change in legal definitions in 2015 caused a break in the sex crime time series. We classify statistical groups 1401, 1406, 1412, 1413, 1414, 1416, 1419, 1420, 1422, 1423, 1424, 1425, 1426, 1451-1459, 1468, 1498, and 1499 (rape, attempted rape, sexual abuse of incapacitated person, sexual abuse by misusing position, sexual abuse of person with mental disorder, sexual abuse of person in institutional care, sexual act without consent, other sex crimes, sexual harassment, sexually harassing behavior and exposure, sexual act by threat, digital sexual harassment) as sex crimes. We exclude statistical groups 1402-1405, 1407, 1408, 1415, 1417, 1421, 1427-1429, 1460-1477, 3899 (sex acts with child, rape of child, pornography, child pornography, incest, pimping, promoting prostitution, purchase of sexual acts, grooming, other moral crimes). All other crime categories are classified as non-sex crimes.

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<sup>57</sup><https://www.spo.go.kr/site/spo/ex/board/List.do?cbIdx=1301>

[https://kosis.kr/eng/statisticsList/statisticsListIndex.do?menuId=M\\_01\\_01&staId=1976029](https://kosis.kr/eng/statisticsList/statisticsListIndex.do?menuId=M_01_01&staId=1976029)

<sup>58</sup><https://osp.stat.gov.lt/statistiniu-rodikliu-analize#/>

<sup>59</sup><https://www.gob.mx/sesnsp/acciones-y-programas/datos-abiertos-de-incidencia-delictiva>

<sup>60</sup><https://data.politie.nl/#/Politie/nl/dataset/47013NED/table?dl=21672>

<sup>61</sup><https://www.police.govt.nz/about-us/publications-statistics/data-and-statistics/policedatanz>

**Poland** We received spreadsheets of reported crimes for each month and year for 2010-2018 on Apr 3, 2020 from *Gabinet Informacja Publiczna*. Articles 197-199 (rape, sexual activity that takes advantage of the vulnerability of another person, abusing dependence relationship or using a critical position to lead another person to sexual intercourse) are classified as sex crimes. Articles 200-204 (sexual activity with a minor, incest, distribution of pornography without consent, subjecting another person to prostitution by taking advantage of a dependent relationship, force, illegal deceit or threat and inducing another person to practice prostitution for monetary gain) and domestic violence are excluded. All other crime categories are classified as non-sex crimes.

**Portugal** We received a spreadsheet via email of monthly reported crimes for 2010-2018 on July 11, 2019 from *Instituto Nacional de Estatística*. Rape and "Other crimes against freedom and sexual self-determination" are classified as sex crimes. Sexual abuse of children, pandering of child pornography, domestic violence against spouse/kin, and domestic violence against children are excluded. All other crime categories are classified as non-sex crimes.

**Slovakia** We downloaded spreadsheets of crime statistics by paragraph (podľa §) for 2010-2018 on June 28, 2019 from *Ministerstvo vnútra Slovenskej republiky*.<sup>62</sup> Paragraphs 199 and 200 (rape and sexual violence) are classified as sex crimes. Paragraphs 167, 201, 202, 203, 204, 208, 367, 368, 369, 370, 371, and 372 (exposing others to venereal disease, rape of minor, sexual abuse of minor, incest, bigamy, battering a close person and a person entrusted into one's care, procuring and soliciting prostitution, pornography, domestic violence) are excluded from the analysis. All other crime categories are classified as non-sex crimes. Crime statistics from each month are reported from the start of the year until the end of the month. The monthly statistics are obtained by deducting the previous month's yearly total from each month's yearly total.

**Slovenia** We received a spreadsheet by email of monthly reported crimes for 2010-2018 on June 28, 2019 from *Statistični Urad*. Rape, sexual violence, violation of sexual integrity by abuse of position, and sexual abuse of a defenseless person are classified as sex crimes. Criminal offenses against marriage, family, and youth are excluded. All other crime categories are classified as non-sex crimes.

**Spain** We received a spreadsheet by email of quarterly reported crimes for 2010-2018 on June 2, 2021 from *Ministerio del Interior*. Sexual violence is classified as a sex crime. All other crimes are classified as non-sex crimes.

**Sweden** We downloaded a spreadsheet of monthly reported crimes for 2010-2018 on May 28, 2019 from *Brottsförebyggande rådet*.<sup>63</sup> Paragraphs 9603-9622 and 9627-9673 (all levels of sexual harassment, sexual assault, and rape except those against children) are classified as sex crimes. Paragraphs 0356, 0376, 0412, 9349, 9351, 9353, 9355, 9357, 9359, 9360, 9365, 9367, 9401, 9622-9626 (all levels of assault of woman or elder known to the offender or in close relationship to the offender, violation of the rights /

<sup>62</sup><https://www.minv.sk/?statistika-kriminality-v-slovenskej-republike-csv>; <https://www.minv.sk/?statistika-kriminality-v-slovenskej-republike-za-rok-YYYY>

<sup>63</sup><http://statistik.bra.se/solwebb/action/index>

women's rights of a person known to the offender, human trafficking for sexual purposes, illegal threats against girls and women, offensive photography, forced marriage, pimping, prostitution, and incest) are excluded. All other crime categories are classified as non-sex crimes.

**Switzerland** After a formal application, we received a spreadsheet of monthly reported crimes via email for 2010-2018 on September 24, 2019 from *Bundesamt für Statistik*. Articles 188-194 and 198 (sexual acts with dependent persons, indecent assault, rape, sexual acts with persons incapable of judgment or resistance, sexual acts with persons in institutional care, prisoners and persons on remand, exploitation of a person in a position of need or dependency, indecent conduct, and sexual harassment) are classified as sex crimes. Articles 124, 181, 187, 195-197, 199, 213, and 215 (female genital mutilation, coercion, sexual activity with a child under 16 years, encouraging prostitution, sexual acts with minors against payment, pornography, unauthorized practice of prostitution, incest, and bigamy) are excluded. All other crime categories are classified as non-sex crimes.

**United Kingdom** After a formal application, we received spreadsheets via email of monthly reported crimes by offense for 2010-2018 for England and Wales on June 25, 2019 from the Home Office: Crime and Policing Analysis Unit. We downloaded another spreadsheet of reported crimes by crime type for each month for 2010-2018 for Northern Ireland on July 8, 2019 from Open Data Northern Ireland.<sup>64</sup>

For England and Wales, sexual assault of victims aged 13 and over, rape of victims aged 16 and over, causing sexual activity without consent, sexual activity with a person with a mental disorder, other miscellaneous sexual offenses, abuse of position of trust of a sexual nature, exposure and voyeurism and stalking are classified as sex crimes. Sexual assault of a child under 13, rape of a child under 16, bigamy, soliciting for purposes of prostitution, exploitation of prostitution, abuse of children through sexual exploitation, incest or familial sexual offenses, sexual activity involving a child under 13, sexual activity involving a child under 16, sexual grooming, trafficking for sexual exploitation, unnatural sexual offenses, harassment, and malicious communications are excluded. All other crime categories are classified as non-sex crimes. For Northern Ireland, sexual offenses are classified as sex crimes. All other crime categories are classified as non-sex crimes. After classifying crimes for England and Wales and Northern Ireland separately, we add the crime categories together to get totals for the United Kingdom.

**United States** We use the NIBRS data described in Appendix A.6.

### A.3 Google Search Data

As our primary measure of the MeToo movement's strength, we use the search interest in the topic of the MeToo movement in October 2017. Our search interest data is scraped from Google trends and contains monthly search interest figures for all of the OECD from 2010-2018.<sup>65</sup> To simplify the interpretation of this measure, we normalize the magnitude of the interest so that the average interest in the OECD is one in the post-period.

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<sup>64</sup><https://www.opendatani.gov.uk/dataset/police-recorded-crime-in-northern-ireland/resource/6fd51851-df78-4469-98c5-4f06953621a0>

<sup>65</sup>For scraping, we used the R package *gtrendsR* written by Philippe Massicotte and Dirk Eddebuettel. The data was scraped on October 26, 2020.

Google defines a search for a topic as any search query including a phrase directly linked to the topic in any language but does not provide information on the phrases linked to the MeToo movement topic. Therefore, we also create our own definition of the MeToo movement topic in all of the languages used in the OECD for which we could find a phrase related to the MeToo movement. We restrict our measure to phrases with search interest in their country of origin of at least 1% of the search interest for "me too" in the US. These terms are: "me too", "balance ton porc", "moi aussi", "quella volta che", and "yo tambien", as well as these terms written without spaces.

In October 2017, searches for these phrases had a 0.995 correlation with the MeToo movement topic defined by Google across countries. We prefer to use the search for the MeToo movement topic instead of our list of exact phrases since it is more likely that the topic search includes searches for additional phrases related to the MeToo movement in other languages.

In Figure 1b, Table 2, and Appendix Figure A.4 we also use an alternative measure of search interest based on searches for the topics of sexual harassment and sexual assault.<sup>66</sup> We create the interest for these two topics by normalizing the two interest measures onto the same scale and taking the sum of the two search interests.

#### **A.4 Tweets Related to the MeToo Movement**

To calculate the number of tweets related to the MeToo movement, we use the Schlesinger Library #MeToo Digital Media Collection Twitter Dataset (Morales Henry and Weintraub, 2020). The dataset contains the ids of tweets with 71 MeToo-related hashtags from October 15, 2017, until March 31, 2020. We focus on tweets from October 2017.

To determine the country of a tweet we use the `user_location` field. We exclude tweets that were deleted and cannot be accessed. We further exclude approximately one-quarter of tweets that do not contain the location field. We assign tweets to a country by matching them to country names, US states, and cities. We manually include or exclude additional strings, such as regions to improve the matching process, and fix common mistakes, such as matching the term "New England" with the UK instead of the US. We determine the country of 74% of the tweets where the location field exists. Our final dataset contains 798,636 tweets from October 2017 to which we can assign a country. Our method determining the location of tweets is probably more likely to detect US-based tweets, both because we carefully match US state names and because we match locations using Latin script. Any bias this creates would go against our argument that tweets related to the MeToo movement were common in many countries and did not mostly originate from the US.

We determine the number of Twitter users per country based on the number of people that could be reached in the country with Twitter advertisements in 2019. The data was collected by Hootsuite (2019).

#### **A.5 Fraction of English Speakers**

We use data on the fraction of English speakers from the 23rd edition of the Ethnologue Global Dataset (Lewis et al., 2020). The data contains estimates for the population using English as their first language,

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<sup>66</sup>Caputi et al. (2019) show that the MeToo movement affected search interest for the terms 'sexual' along with 'harassment' and 'assault' in the US.



the population using English but for whom English is not a native language, and the total population. We take the sum of the population using English as a first language and the population using English as a non-native language and divide it by the total population to get the fraction of the population who uses English.

For five countries (Chile, Colombia, France, Japan, and Slovakia) we use values from the 24th edition of the Ethnologue Global Dataset to replace missing values in the 23rd edition data. For Slovenia, we impute the fraction using English as their first language using the median fraction of first-language English users for the country's region (Eastern Europe).

## A.6 NIBRS Crime Data

We classify NIBRS offenses as either sexual assaults or non-sex crimes. The sexual assault offenses are fondling, rape, sexual assault with an object, sodomy, and statutory rape. We exclude incest, human trafficking, and the pornography/obscene material crime categories. All other 43 offense types form the non-sex crimes category. Domestic assault is not a separate offense type in the NIBRS dataset. To exclude domestic violence crimes which may have been affected by the MeToo movement, we exclude all aggravated assaults where the circumstances of the assault are defined in the NIBRS as a "lovers quarrel" and all assaults or aggravated assaults for which the relationship between the offender and victim is defined in the NIBRS as one of the following: victim was ex-spouse, victim was spouse, homosexual relationship, victim was boyfriend/girlfriend, victim was common-law spouse.

In the NIBRS data, an incident can include multiple crimes if they occurred in concert, at the same time and place. We define an incident as a sexual assault if one of the offenses which occurred as part of the incident is a sexual assault. Similarly, if the incident is not a sexual assault, we exclude it if one of the offenses which occurred as part of the incident should be excluded (e.g., if an incident includes both a pornography/obscene material offense and a weapon law violations offense, it is excluded).

One potential concern with the NIBRS data is that police agencies started reporting sexual assaults through the NIBRS as a result of the MeToo movement. However, we find no evidence that the movement affected whether agencies include sexual assault in their reports. We check whether agencies participating in the NIBRS system started reporting sexual assaults in a specific month. Since there is natural variation in reporting, we focus on cases where agencies did not report any sexual assaults in twelve consecutive months and then reported at least four assaults. This occurred in only seven agencies out of over 2,000. Even in those agencies, the increase in reporting is minimal and the increase does not occur after the MeToo movement started. Therefore, this increase probably represents noise and not a decision of an agency to start reporting sexual assaults.

When analyzing data at the state level, we exclude state-years with months when fewer than 100 crimes were reported in total.

## A.7 City Crime Data

City-level crime data was obtained from the cities' open data websites. For each city, we first categorize a crime as a sexual assault, sexual harassment, non-sex crime, or a crime that should be excluded since it is indirectly related to the MeToo movement (as explained in Section A.1). We define each month as

spanning from the 15th day of the calendar month to the 14th day of the next calendar month. By defining months in this way, we can cleanly categorize each observation in the aggregated data as occurring before or after the start of the MeToo movement, since the movement started on October 15, 2017.<sup>67</sup>

### A.7.1 Data Source and Processing by City

**Austin** We downloaded 2010-2018 crime data from the Austin Open Data Portal on June 29, 2021.<sup>68</sup> We classify crimes based on the category description and highest offense description. Indecent exposure, stalking, window peeping, and public lewdness are classified as sexual harassment. Rape, sodomy, sexual assault with an object, and sexual assault are classified as sexual assault. We exclude all crimes related to domestic violence, dating violence, harassment, sex crimes against children, prostitution, sex offender registration, online harassment, pornography, prostitution, and bestiality. We define neighborhoods based on police sectors.

**Denver** We downloaded 2013-2018 crime data from the Denver Open Data Catalog on March 18, 2019.<sup>69</sup> We use the offense category and offense type to classify crimes. Sexual harassment, window peeping, and indecent exposure are defined as sexual harassment. All subcategories under sexual assault are defined as sexual assault. We exclude domestic violence crimes, prostitution-related crimes, crimes related to obscene materials, and crimes related to sexual offenders. We also exclude traffic cases and cases that are not defined as crimes. Each observation in Denver is defined as an offense. Therefore, we aggregate offenses to incidents based on the incident number as several offenses can be associated with one incidence if they occur in concert. If an incident includes multiple offense types, we define the crime category based on the following hierarchy: sexual assault, sexual harassment, excluded crime, other crime. We define neighborhoods based on police districts.

**Kansas City** We downloaded 2010-2017 crime data from Open Data Kansas City on June 27 2018 and 2018 data on March 10, 2020.<sup>70</sup> We use the NIBRS offense name and crimes description to classify crimes. Window peeping, indecent conduct, indecent exposure, and stalking are defined as sexual harassment. Fondling, rape, and sexual assault are defined as sexual assault. We exclude human trafficking, commercial sex acts, incest, prostitution-related crimes, crimes related to obscene materials, crimes related to harassment that are not necessarily related to sexual harassment, and assaults where the category name mentions domestic violence. We also exclude crimes that do not have an offense code, typically suicides and auto thefts, along with cases of a dead body. We aggregate offenses to incidents as explained in the description for Denver. When aggregating offenses, we keep the observation related to the victim if such an observation exists and define the reported date and the occurrence date as the minimum reported and occurrence dates, respectively, among the relevant observations.<sup>71</sup> We define neighborhoods based

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<sup>67</sup>We do not use a similar definition when analyzing the international data or the NIBRS data since most international data we collect is already aggregated at the month or quarter level, and since we want to keep the NIBRS results consistent with the international analysis.

<sup>68</sup><https://data.austintexas.gov/Public-Safety/Crime-Reports/fdj4-gpfu>

<sup>69</sup><https://www.denvergov.org/opendata/dataset/city-and-county-of-denver-crime>

<sup>70</sup><https://data.kcmo.org/browse?category=Crime>

<sup>71</sup>In a very small share of cases where there is a gap of more than one day between the occurrence dates defined for the same incident, we define the occurrence date as missing.

on police divisions.

**Los Angeles** We downloaded 2010-2018 crime data from Los Angeles Open Data on March 10, 2020.<sup>72</sup> We use the crime description to classify crimes. Window peeping, stalking, indecent exposure, lewd conduct, and child annoying (17 years and under) are defined as sexual harassment. Battery with sexual contact, oral copulation, attempted rape, forcible rape, sexual penetration with a foreign object, and sodomy are defined as sexual assault. We exclude incest, bigamy, human trafficking, all crimes related to pornography, lewd acts with children, and intimate partner violence. We define neighborhoods based on police divisions.

**Louisville** We downloaded 2010-2018 crime data from Open Data Louisville on June 27, 2019.<sup>73</sup> We classify crimes based on the crime type and description. Indecent exposure, stalking and voyeurism are defined as sexual harassment. Rape, sexual abuse, sexual assault, sexual misconduct, sodomy, and sexual offense are defined as sexual assault. We exclude all crimes related to domestic or dating violence, bigamy, child pornography, child prostitution, death investigation, distribution of obscene material, fire investigation, failure to comply with sex offender registration, incest, missing or injured persons, non-criminal death, prostitution, pending SVU investigation, suicide, sex crimes against children, and harassment that is not necessarily sexual in nature. We also exclude very few cases with no reported date or an inactive incident number. Each observation in Louisville is defined as an offense. Therefore, we aggregate offenses to incidents based on the incident number as explained for Denver. We define neighborhoods based on police divisions.

**New York City** We downloaded 2010-2018 crime data from Open Data NYC on June 26, 2019.<sup>74</sup> We use the offense description and police department description to classify crimes. Indecent exposure, lewdness, and unlawful disclosure of indecent images are defined as sexual harassment. Rape, sex crimes, sexual abuse, sexual assault, sexual offense with a controlled substance, sexual misconduct, and sodomy are defined as sexual assault. We exclude all crimes related to prostitution, bigamy, obscenity, pornography, sex trafficking, incest, and sex crimes endangering child welfare. We define neighborhoods based on police precincts.

**Seattle** We downloaded 2010-2018 crime data from Open Data Seattle on March 18, 2019.<sup>75</sup> We classify crimes based on the crime subcategory and the primary offense description. Indecent exposure, lewd conduct, and window peeing are defined as sexual harassment. Rape, sodomy, indecent liberties, and other sex offenses are classified as sexual assault. We exclude all crimes related to domestic violence, pornography, and prostitution. We define neighborhoods based on police precincts.

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<sup>72</sup><https://data.lacity.org/Public-Safety/Crime-Data-from-2010-to-2019/63jg-8b9z>

<sup>73</sup><https://data.louisvilleky.gov/dataset/crime-data>

<sup>74</sup><https://data.cityofnewyork.us/Public-Safety/NYPD-Complaint-Data-Historic/qgea-i56i>, <https://data.cityofnewyork.us/Public-Safety/NYPD-Complaint-Data-Current-Year-To-Date-/5uac-w243>

<sup>75</sup>The data was downloaded from <https://data.seattle.gov/Public-Safety/Crime-Data/4fs7-3vj5>. The Seattle Police Department has changed its records management system in May 2019 and therefore the data may no longer be available online.

**Tucson** We downloaded 2010-2018 crime data from the City of Tucson Open GIS Data on June 29, 2021.<sup>76</sup> We classify crimes based on the statute description and crime subcategory. Indecent exposure, lewd and lascivious acts, window peeing, obscene phone calls, and stalking are classified as sexual harassment. Rape and other sex crimes are classified as sexual assault. The Tucson data includes police activities that are not necessarily related to specific crimes. We exclude those by excluding all incidents related to accidents, animal bites, criminal warrants, deaths, disturbances, explosions, false alarms, firearm accidents, fires, flooding, gas leaks, immigration-related custody transfers, institutional escapees, licenses and registration, lost animals, missing persons, moving violations, point control and other miscellaneous activities, property damage or loss, public hazards, restoring public peace, suicides, suspicious activities, transportation to treatment facilities, unfounded incidents, and welfare checks. We also exclude crime related to domestic violence, sex offenses against children, prostitution, and human trafficking. We define neighborhoods based on police divisions and assign crimes to divisions based on the 'neighborhd' variable. While the classification is not perfect, especially in the downtown district, this variable allows us to correctly assign the vast majority of incidents to police divisions.

## **A.8 Campus Climate Survey**

The AAU Campus Climate Survey (CCS) was conducted in 2015 and 2019 (Association of American Universities, 2020). We limit our analysis to the 21 universities conducting both the 2015 and 2019 surveys. The CCS was offered to all graduate and undergraduate students and the average response rate was 24% in 2015 and 23% in 2019. When weighting the sample to make it representative of the student population, as is done in our analysis, there were no statistically significant differences between the surveys in the sample demographics except for the race of the students.<sup>77</sup>

### **A.8.1 Sexual Assault Incidence and Reporting**

Our analysis relies both on variation across the 2015 and 2019 survey waves and on variation in the year when an incident occurred. Each survey covered incidents occurring in all of the students' previous school years. For the 2019 survey, the last two school years preceding the survey occurred after the MeToo movement started (Fall 2018 - Spring 2019, Fall 2017 - Summer 2018), and earlier years mentioned in the survey occurred before the movement started (Fall 2016 - Summer 2017, Fall 2015 - Summer 2016, and before Fall 2015). We rearrange the data so that each observation is an academic year when a specific student was enrolled at the university.

When analyzing the effect on incidence, the number of observations is the sum of all the academic years the interviewed students have attended the universities where they were enrolled at the time of the interview. The number of students is 331,383, generating a total of 811,214 observations.

We use the following specification to generate the results in Figure 5 and Columns (1), (3), (5), and (7) in Appendix Table A.12:

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<sup>76</sup><https://gisdata.tucsonaz.gov/search?groupIds=b6a49faa168647d8b56e1a06bd53600f&tags=incidents>

<sup>77</sup>The weighted 2019 survey had 2% fewer Asian students and 2% more students identifying as mixed or not being White, Black, or Asian.

$$y_{iuts} = \sum_{j=1}^5 \beta_j \text{YearsBeforeSurvey}_t \times 2019 \text{ survey}_s + \sum_{j=1}^5 \beta_j \text{YearsBeforeSurvey}_t + \gamma \text{ResponseRate}_{us} + \delta X_i + \zeta_u + \varepsilon_{iuts} \quad (3)$$

When analyzing incidence in Sub-Figure 5a and Columns (1)-(4) of Appendix Table A.12, the unit of observation is a year that a student was enrolled in a university. The regression is effectively comparing incidence in each school year preceding the survey with a school year that preceded the previous survey by the same number of years. For example, we compare Fall 2017-Summer 2018 in the 2019 survey with Fall 2013-Summer 2014 in the 2015 survey.

When analyzing reporting in Sub-Figure 5b and Columns (5)-(8) of Appendix Table A.12, the unit of observation is a unique sexual assault. In Columns (3)-(8), we exclude students who were victims of multiple incidents of physical sexual misconduct because it is not possible to determine which incident of physical sexual misconduct was reported when multiple incidents were described in the survey.<sup>78</sup>

$y_{iuts}$  is the outcome variable for student  $i$ , at university  $u$ , taking survey  $s$  (2015 or 2019), about academic year  $t$ . The three outcome variables are: having experienced a sexual assault defined as sexual penetration, oral sex, kissing, or sexual touching by physical force, threat of force, or while the victim was unable to consent; having reported the sexual assault to a university program; and having told a friend of family member about the sexual assault.

$j$  years before survey $_t$  is an indicator variable for the question being about the academic year  $j$  years before the survey took place. 2019 survey $_s$  is an indicator for the survey taking place in 2019. Response rate $_{us}$  is the response rate for survey  $s$  at university  $u$ .  $X_i$  is a vector of student characteristics: fixed effects for each level of age (18-25+), year in undergraduate program (1-4+), year in graduate program (1-4+), and fixed effects for each of four gender categories and four race categories.  $\zeta_u$  are university fixed effects. In Sub-Figure 5b and Columns (5)-(8) in Appendix Table A.12, where the unit of observation is a sexual assault, we also control for fixed effects for the type of sexual assault.<sup>79</sup>

Columns (2), (4), (6), and (8) of Appendix Table A.12 use a similar specification but with just one indicator variable for whether the academic year asked about was two years or less before the survey (*Within 2y of survey $_t$* ) instead of indicator variables for each academic year. The coefficient of interest is the coefficient on the interaction between *within 2y of survey $_t$*  and 2019 survey $_s$ . This is a difference-in-differences estimate using the differences between the years before and after the MeToo movement started and the 2015 and 2019 surveys.

<sup>78</sup>Physical sexual misconduct includes sexual assaults as well as sexual coercion without the threat of force and sexual acts without active ongoing consent.

<sup>79</sup>Five types of sexual assaults are defined in the CCS survey: sexual penetration or oral sex by physical force or threat of physical force, attempted sexual penetration or attempted oral sex by physical force or threat of physical force, kissing or sexual touching by physical force or threat of physical force, sexual penetration or oral sex while victim was unable to consent, and kissing or sexual touching while victim was unable to consent.

## A.8.2 Reasons for Not Reporting

An additional benefit of analyzing the CCS data is that it asks students why they did not report sexual assaults to any university program. Victims of sexual assaults who did not report were asked "were any of the following reasons why you did not contact anyone at [University]?" in 2015 and "why did you decide not to contact any of these programs or resources?" in 2019. A list of potential reasons were then presented and the students could choose all options that applied. The options changed from the 2015 survey to the 2019 survey but the first eight options in Appendix Figure A.11 were sufficiently similar that they can be compared directly.<sup>80</sup>

## B Additional Analysis

### B.1 Effect on Reported Crimes per 100,000 Persons

In our main specification, we estimate the effect of the MeToo movement on the natural logarithm of reported sex crimes. This generates an estimate for the effect in terms of a relative change from the counterfactual number of reported crimes. We believe it is likely that the effect of the MeToo movement is relative to the number of crimes that would have been reported had there been no movement. Using the natural logarithm also provides an intuitive interpretation of the effect as a percentage increase.

However, it is possible that the MeToo movement instead affected the absolute number of sex crimes reported per person, irrespective of the counterfactual number of sex crimes reported. Table A.14 uses the same specifications as Table 1 but with the number of crimes per 100,000 persons as the outcome variable. As the number of non-sex crimes reported is more than two orders of magnitudes larger than the number of sex crimes reported, using the number of non-sex crimes as the counterfactual would cause the estimate to be completely dominated by relatively small changes in the number of non-sex crimes reported.<sup>81</sup> Therefore, for each country, we normalize the number of non-sex crimes reported to be comparable in magnitude to the number of sex crimes reported. We divide the number of non-sex crimes by its mean in the year before the start of the MeToo movement (Q4 2016 - Q3 2017) and multiply it by the mean number of sex crimes reported in that year.

Overall, the results in Table A.14 are qualitatively similar to the results in Table 1. Column (4) shows the results from our main triple-difference specification. We find an effect of 2.2 more sex crimes being reported per 100,000 population (p-value=0.012).

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<sup>80</sup>The exact options in 2015 were: "Did not know where to go or who to tell", "Felt embarrassed, ashamed or that it would be too emotionally difficult", "I did not think anyone would believe me", "I did not think it was serious enough to report", "I did not want the person to get into trouble", "I feared negative social consequences", "I feared it would not be kept confidential", "Incident did not occur while attending school", "I did not think anything would be done", "Incident was not on campus or associated with the school", and "Other". In 2019, the exact options were: "I did not know where to go or who to tell", "I felt embarrassed, ashamed or that it would be too emotionally difficult", "I did not think anyone would believe me", "I did not think it was serious enough to contact any of these programs or resources", "I did not want the person to get into trouble", "I feared negative academic, social or professional consequences", "I feared it would not be kept confidential", "Incident occurred while school was not in session", "I could handle it myself", "I didn't think these resources would give me the help I needed", "I feared retaliation", and "Other".

<sup>81</sup>On average across countries the mean quarterly number of non-sex crimes is 183 times larger than the mean quarterly number of sex crimes reported.

## B.2 Alternative Statistical Inference Methods

In our main triple-difference specification in Section 3.3, we use standard errors clustered at the country by crime type level. This is our preferred inference method as the treatment occurs at this level. Table 1 shows the robustness of these standard errors to alternative methods for statistical inference. All figures relate to our triple-difference estimator from Column (4) of Table A.14. Row (1) displays our preferred standard errors clustered at the country by crime type level as well as the corresponding p-value. To avoid a downward bias in the conventional p-values caused by a small number of clusters we calculate wild bootstrap p-values and present these in Row (2) (Roodman et al., 2019). The wild bootstrap p-values are similar to the conventional p-values suggesting that there is no substantial bias. Row (3) displays non-clustered robust standard errors and the corresponding p-value. Row (4) displays standard errors clustered at the country level and the corresponding p-value. Row (5) displays standard errors and p-value calculated using two-way clustering at the country by crime type and time period level. Row (6) displays standard errors clustered at the country by crime type level where the regression includes country by post-period fixed effects absorbing more of the unexplained variation. Including these fixed effects does not change the point estimate.

## B.3 Components of the Triple-Difference Estimate

Appendix Figure A.7 shows the timelines for the components of the visualization of the triple-difference estimate in Figure 3. Sub-Figure A.7a shows the number of sex crimes reported, indexed to be 100 in Q3 2017, and averaged across the countries with strong and weak MeToo movements. A clear seasonality is observed as the fourth quarter of each year tends to see a decrease in the number of sex crimes reported. This is true for both strong and weak MeToo movement countries until Q4 2017, when the number of reported sex crimes stays flat in the countries with a strong MeToo movement, while the countries with a weak movement experience the typical decline. In Q1 2018, the number of reported sex crimes in countries with strong and weak MeToo movements continues to diverge. Sub-Figure A.7b confirms that this differential increase in reported crimes for the countries with strong MeToo movements did not happen for non-sex crimes.

The figure also shows that while countries with strong and weak movements have similar pre-trends in the difference between sex crimes and non-sex crimes, they have different pre-trends for each crime category. Therefore, in our main specification, we control for country by crime type linear time trends and seasonality, and hence, these differential trends do not drive the effects presented in Table 1.

## B.4 Matrix Completion

Our main specifications assume that after controlling for seasonality and time trends, non-sex crimes are a suitable control group for sex crimes, or that countries with a weak MeToo movement are a suitable control group for countries with a strong movement. In this section, we relax those assumptions, and instead of estimating an effect based on the standard triple difference or difference-in-differences specifications, we use the matrix completion method. The results are robust to the method used.

The matrix completion method (Athey et al., 2021) is based on a matrix where each row is a unit and each column is a time period. The method attempts to predict the counterfactual outcome for treated

units in the post-period. This exercise is similar to creating synthetic controls, with the added benefit of exploiting time variation in the data. We use the method to create a counterfactual for the expected number of sex crimes that would have been reported in the post-period if there was no MeToo movement. The counterfactual matrix is created for all observations and values are chosen to minimize the sum of squared differences between the actual outcomes and the predicted counterfactual outcomes for non-treated observations, with penalization according to the nuclear norm of the predicted matrix. The average treatment effect is the weighted difference between the actual outcomes and counterfactual outcomes for the treated units in the post-periods. A main advantage of the matrix completion approach is that it is *“able to model more complex patterns in the data, while allowing the data (rather than the analyst) to indicate whether time-series patterns within units, or cross-sectional patterns within a period, or a more complex combination, are more useful for predicting counterfactual outcome”* (Athey, 2018).

The results are summarized in Table A.16.<sup>82</sup> In Column (1), we allow the algorithm to choose the variables used to create the counterfactual matrix without including any explicit controls. In Column (2), we explicitly control for each matrix row, in addition to any other variables chosen by the algorithm. For example, when analyzing the NIBRS data, the second column controls for state by crime category fixed effects. Both columns use bootstrapped standard errors. The rest of this section describes in more detail the estimation for each sample.

#### **B.4.1 International Data**

We define each row in the matrix as a crime by country category and each column as a quarter. We define two categories for each country - sex crimes and non-sex crimes. We do not use more detailed categories since we only have aggregated data for some countries in our sample. A cell is defined as treated if it occurred in a country with a strong movement, in the sex crime category, in the post-period (the last quarter of 2017 or the first quarter of 2018). An important distinction from our main estimate is that we are no longer estimating a triple interaction. Instead, to work with the matrix completion framework, we combine the country and crime categories into one variable. The result in Column (1) shows that without including any explicit controls, the estimated effect is similar to our main estimate from Column (4) of Table 1. In Column (2), we explicitly control for country by crime type fixed effects. This generates a larger point estimate for the effect of the MeToo movement.

#### **B.4.2 US NIBRS Data**

Using the matrix completion method to analyze the US data allows us to exploit detailed crime categories instead of aggregating all crimes as sex crimes and non-sex crimes. The method flexibly determines the most suitable control group. For example, violent crimes may be a good predictor of sex crimes.

We define each row in the matrix as a state by category combination and each column as a month. For example, one row is stolen property offenses in West Virginia. This row is always considered untreated. The sexual assault category is considered treated from October 2017 for all states. We use the main NIBRS offense categories and define a new category for incidents that include crimes in multiple categories. We

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<sup>82</sup>The method was estimated using the R package `gsynth` by Yiqing Xu and Licheng Liu. Available online: [https://yiqingxu.org/software/gsynth/gsynth\\_examples.html](https://yiqingxu.org/software/gsynth/gsynth_examples.html)



weight each observation by the mean number of reported crimes of the same category that were reported in the state in the pre-period. We find an effect of 7%-12%, similar to our main estimate.

### **B.4.3 US City Data**

When analyzing the US city data, we define each row as a crime category by city combination and each column as a month. We use the original crime categories defined for each city and do not aggregate crimes to broader categories. All sexual assault or sexual harassment crimes that occurred on or after October 2017 are considered treated. For example, indecent exposure in Los Angeles is a row in the matrix and is considered treated for time periods (matrix columns) on or after October 2017. First-degree assault in Louisville is an example of a row in the matrix which is untreated in all time periods. To prevent excess zeros, we exclude categories for which there was at least one month with no crimes reported. In total, we have 40 treated groups and 482 control groups. We weight each observation by the mean number of reported crimes of the same category that were reported in the city in the pre-period. We find an effect of approximately 9%, similar to our main estimate.

### **B.5 Additional Placebo Test**

We conduct a set of placebo tests to further assure that the MeToo movement is driving our result and not some other mechanism, such as non-linear differential trends between countries with strong and weak movements. Appendix Figure A.12 presents placebo tests setting the start of the MeToo movement in every second quarter from Q2 2010 to Q4 2017 and assuming the movement persists for six months.<sup>83</sup> We estimate the effect of these placebo MeToo movements using the triple-difference specification from Equation 1. Of the 15 placebo tests, only one is statistically significant at the 10% level. The actual effect of the MeToo movement (Q4 of 2017) has a larger absolute coefficient than any of the 15 placebo tests.

### **B.6 Neighborhood-Level Heterogeneity**

We analyze the heterogeneous effects of the MeToo movement by neighborhood demographics in the US city data. We define the neighborhood where each crime occurs using the most coarse definition of police administrative areas available in the dataset. The definition allows us to cleanly assign most crimes to a neighborhood. We use the most coarse definition (e.g., a police division instead of a police beat) to ensure that the number of crimes is positive for most observations. The jurisdictions are detailed in Section A.7. We use the shapefiles for the police boundaries of each city to identify the geographical boundaries of each neighborhood.<sup>84</sup>

The neighborhood demographics are determined by spatially matching the neighborhoods with census block groups. We calculate each neighborhood's demographics as the weighted average of the demographic covariates among overlapping block groups, where the weight of each block group is the

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<sup>83</sup>We set the start date in every second period to avoid having two adjacent estimates using overlapping data and thereby introducing a mechanical autocorrelation. When estimating the placebo effect for every quarter, we find that only four placebo periods have a statistically significant effect at the 10% level and that the actual effect of the MeToo movement has a larger absolute coefficient than any of the 31 placebo tests.

<sup>84</sup>For most cities we use the most recent shapefile available. For Seattle, where changes in the shapefiles are clearly defined, we use different shapefiles for different years and determine the boundaries of each police precinct according to the year when the crime occurred.

population of the block group multiplied by the share of the block group's area overlapping with the neighborhood. The block group demographics are based on the American Community Survey 5-year 2016 estimates. Neighborhoods where demographic data does not exist, such as an airport, are excluded.

Table A.17 does not find evidence for strong heterogeneity by the neighborhood demographics. While some of the point estimates are consistent with heterogeneous effects, the estimated heterogeneity is relatively small, with the possible exception of income. For example, the difference in the expected effect on reporting between a neighborhood in the 75th percentile of the share of individuals with a college education and a neighborhood in the 25th percentile is expected to be 3.2 percentage points, compared to the average effect of 11%. Similarly, the difference between neighborhoods in the 75% and 25% percentile of median income, the share of Blacks, the share of Asians and other races, and the share of Hispanics, is 5.6, -0.2, 3.7, and -1.0 percentage points, respectively.

## **B.7 Incidence of Sex Crimes in the National Crime Victimization Survey**

In this section, we use data from the National Crime Victimization Survey (NCVS) to investigate if the MeToo movement had an effect on the incidence of sex crimes in the US. We find that while the fraction of respondents who report being a victim of a sex crime increased in interviews conducted after the start of the MeToo movement, the timing of the increase suggests that it is driven by a change in how respondents answered the survey questions and not an increase in the underlying incidence of sex crimes. This is consistent with our suggested mechanisms for increased reporting, that the MeToo movement affected individuals' definition of sex crimes and increased the perceived importance of sexual misconduct as a societal problem, thus increasing the motivation to report such conduct.

The NCVS is a rotating panel survey continuously conducted by the US Bureau of Justice Statistics (US Bureau of Justice Statistics, 2020). Respondents are randomly selected to be representative of the US population above 12 years of age. Respondents are interviewed every six months regarding their experiences of criminal victimization in the six calendar months preceding the calendar month of the interview. We use NCVS data from 2010-2018 interviews to document changes in the incidence of sexual crimes after the start of the MeToo movement.

During this period there were 2,627,762 NCVS interviews, but only 1,380 of these interviews mention a sex crime, limiting our ability to precisely estimate changes in incidence. The NCVS asks if the interviewee has experienced an attack or threat of "rape, attempted rape, or other type of sexual attack" or been "forced or coerced to engage in unwanted sexual activity". Another drawback of the NCVS is that "sexual attack" and "sexual activity" are not defined in the survey. We define any of these activities as a sex crime, regardless of whether the respondent provides additional details about the incident.

We construct a dataset at the interview level with an indicator variable for whether the respondent was a victim of a sex crime in the six calendar months preceding the survey. We also create an indicator variable for whether the interview took place after the start of the MeToo movement and we construct a continuous variable for the fraction of the interview's *incident period* that took place after the start of the MeToo movement. The incident period covers the six calendar months preceding the calendar month of the interview, but it does not cover the calendar month in which the interview happened. Survey weights are used to adjust the survey sample to the US population above 12 years of age.

Table A.18 shows the results of a regression estimating the effect of the MeToo movement on the

propensity of NCVS respondents to state that they were victims of sex crimes after controlling for regional linear trends and seasonality. Column (1) shows that in the six months after the start of the MeToo movement, respondents were 0.0004 percentage points more likely to report being a victim of a sex crime in the past six months, a 68% increase over the pre-MeToo mean. Column (2) finds a similar effect in the 15 months after the start of the MeToo movement. This effect could be driven either by an increase in the incidence of sex crimes or a change in individuals' survey response conditional on incidence. Responses conditional on incidence may change due to increased willingness to report sex crimes in a survey or due to a change in the perception of what types of incidents constitute a sex crime.

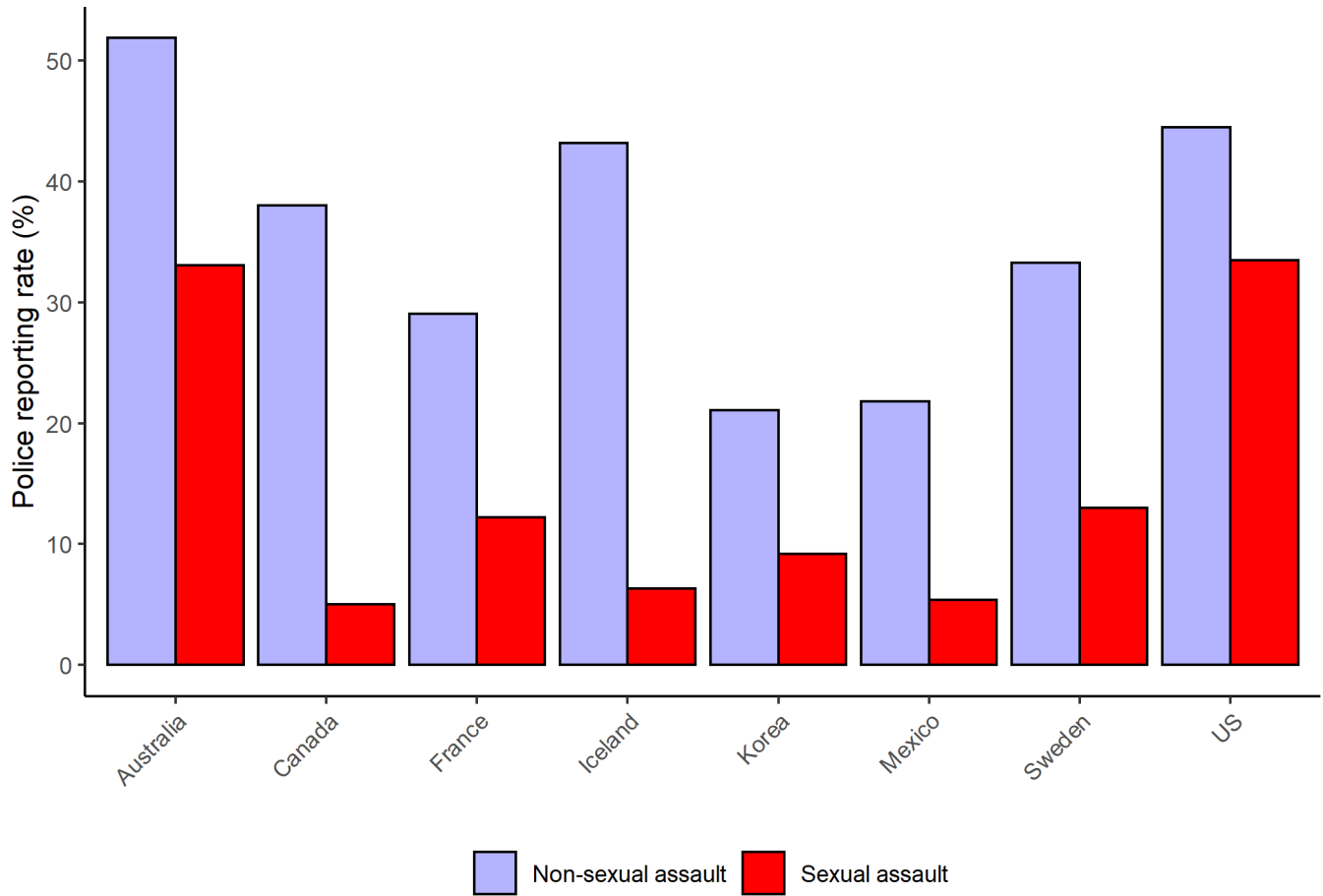
To disentangle the incidence and response mechanisms, we use the timing of the increase in sex crimes. If the MeToo movement increased the incidence of sex crimes, the estimated effect is expected to increase gradually as the incident period respondents are asked about includes more months after the movement started. For example, an interview conducted in April 2018 asking about October 2017-March 2017 should find a larger effect than an interview conducted in November 2017 and asking about May 2017-October 2017. Conversely, if the MeToo movement increased respondents' willingness to discuss sex crime incidents with the NCVS enumerators or expanded the respondents' definition of the type of incidents that constitute a sex crime, then an instantaneous increase should occur after the start of the MeToo movement and is not expected to necessarily increase over time.

Column (3) of Table A.18 provides evidence consistent with the latter explanation and thus suggests that incidence did not increase. We control for the percentage of the incident period that occurred after the start of the MeToo movement and for whether the interview was conducted after the movement started. The coefficient on the percentage of the incident period after the MeToo movement started is close to zero suggesting that there was no substantial increase in the underlying incidence of sex crimes. The coefficient on whether the interview was conducted after the movement started is similar to that in Columns (1) and (2), suggesting that the effect is driven by changes in the respondents' answers conditional on the occurrence of incidents.

Figure A.13 further investigates the timing of the increase in sex crimes. The horizontal axis is the interview date. The vertical axis on the left-hand side shows the coefficients from a regression where the outcome is whether a respondent stated she was a victim of a sex crime in the past six calendar months. The coefficients are on fixed effects for each calendar month, except for October 2017 which we split into two time periods as the movement started on October 15, 2017. The vertical axis on the right-hand side shows the fraction of the incident period occurring after the start of the MeToo movement. The figure shows that the change in sex crimes does not increase with the fraction of the incident period occurring after the start of the MeToo movement, suggesting that the increase is not driven by an increase in the incidence of sex crimes.

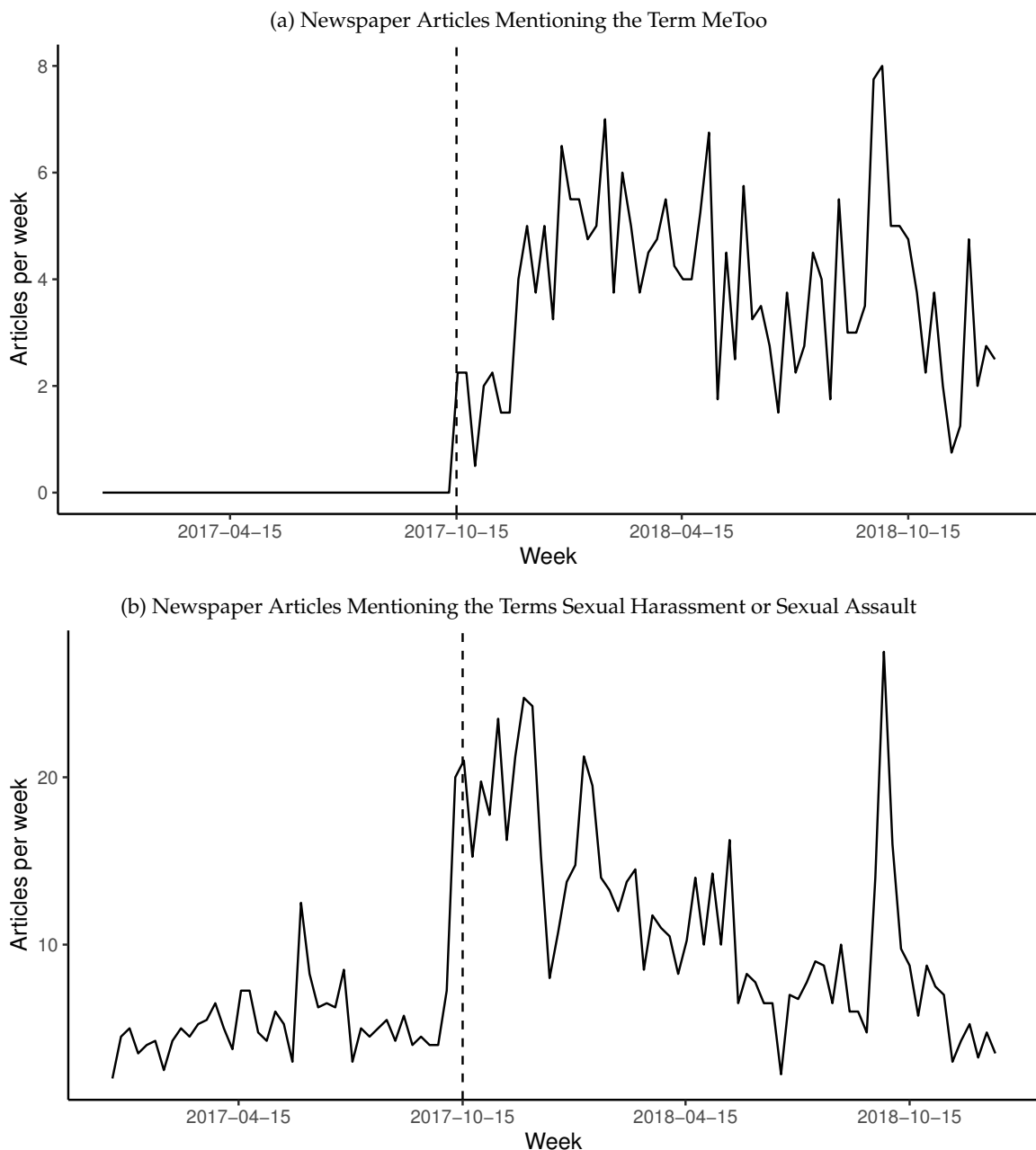
## C Additional Figures and Tables

Figure A.1: Share of Assaults Reported to the Police



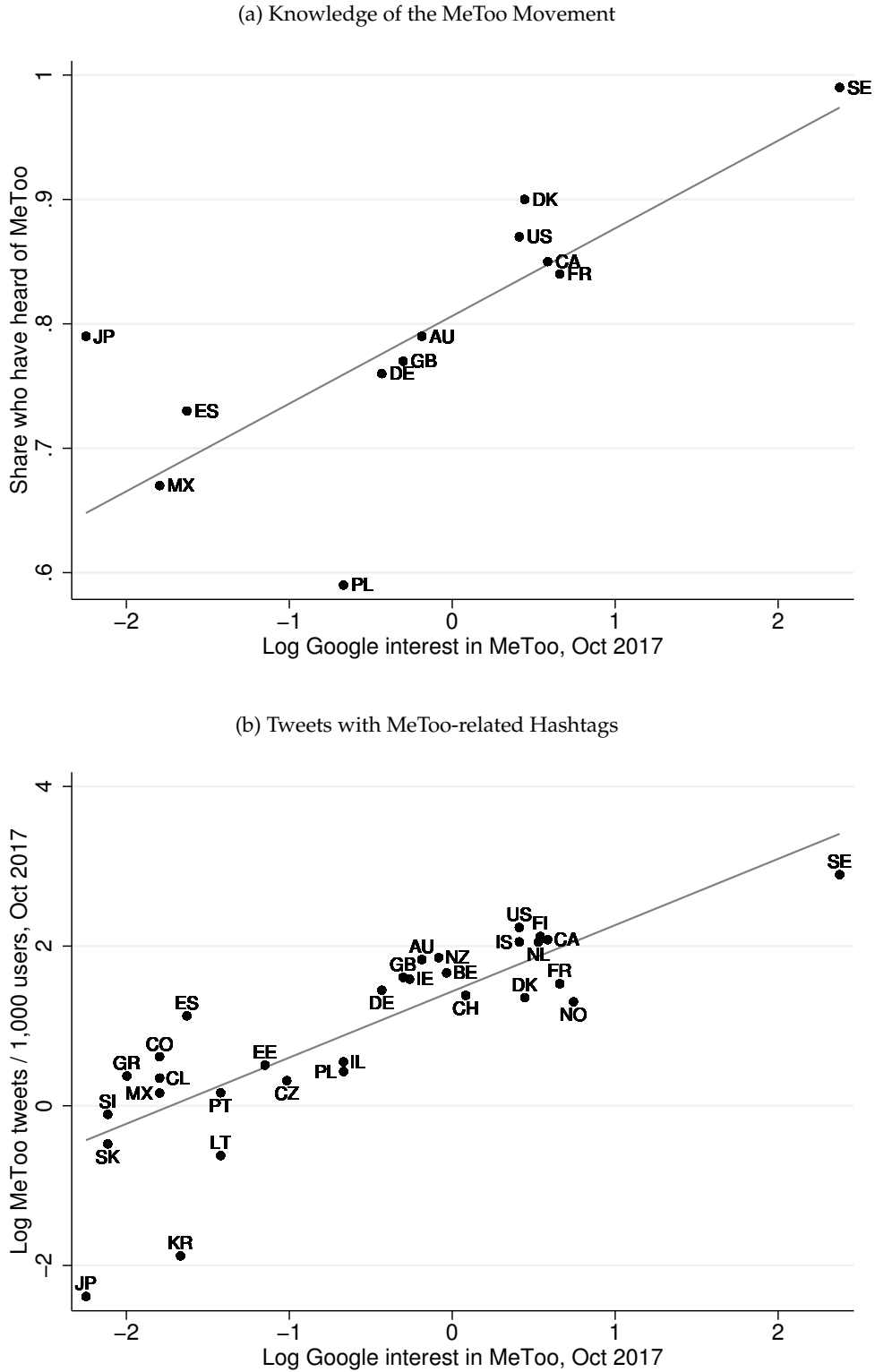
This figure shows the average share of sexual assaults and other physical assaults reported to the police in the years 2010-2017 (UNSDG, 2017).

Figure A.2: Newspaper Coverage Following the MeToo movement



This figure shows how newspaper coverage evolved after the MeToo movement started. Sub-Figure (a) shows the weekly average number of articles mentioning the term “metoo” in the newspapers Chicago Sun-Times, Denver Post, New York Post, and USA Today. Sub-Figure (b) presents the weekly average number of articles mentioning the terms “sexual assault” or “sexual harassment” in those newspapers (articles mentioning both terms are counted twice). The vertical dashed line represents the start of the MeToo movement. The newspapers were chosen based on circulation and data availability. The number of articles is determined using the website [newslibrary.com](http://newslibrary.com).

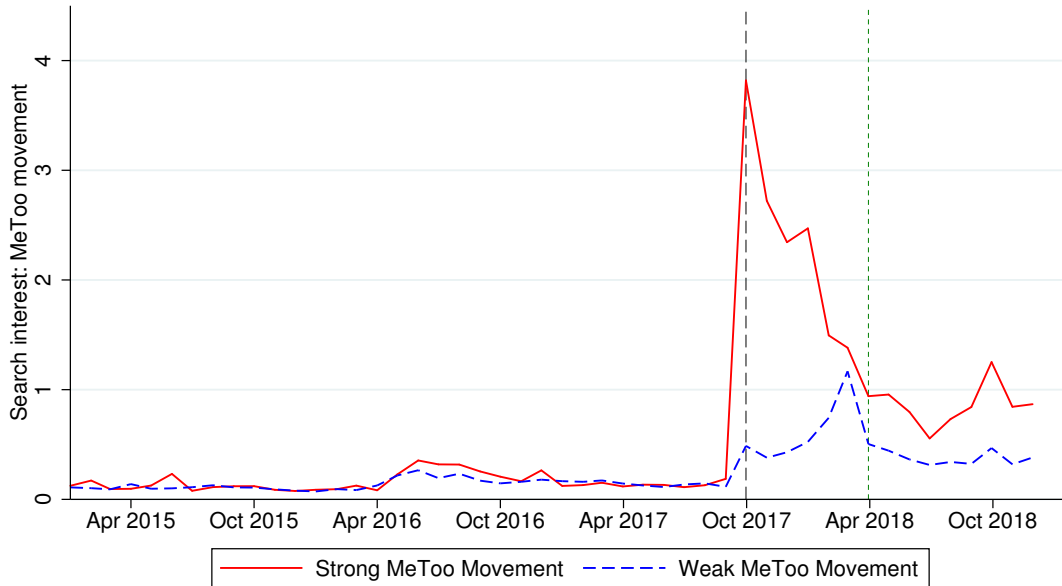
Figure A.3: Relationship Between Google Search Interest and Other Measures of the MeToo Movement's Strength



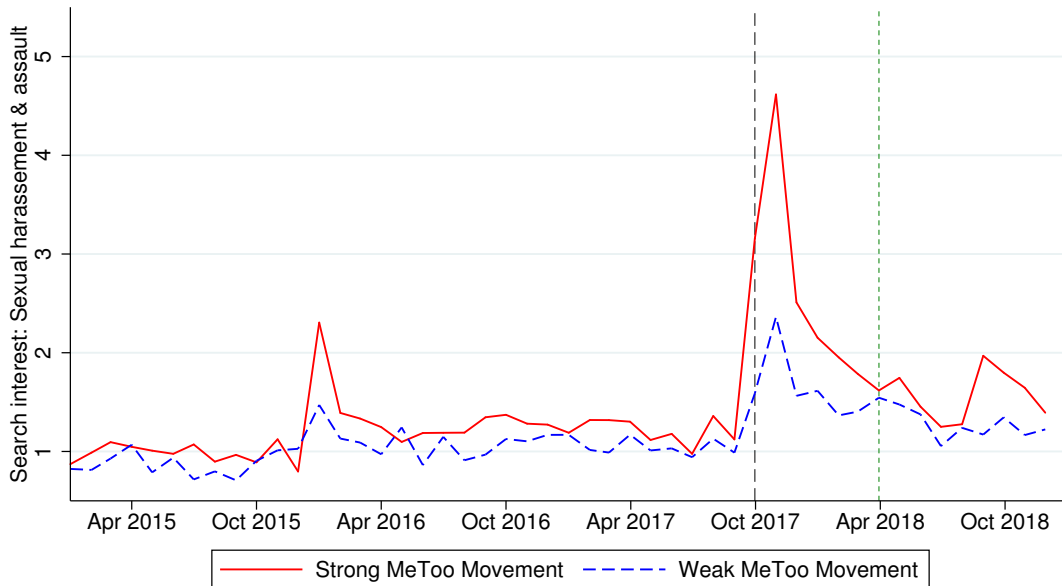
These figures show the relationships between the log of Google search interest for terms related to the MeToo movement in October 2017 and two other measures of the movement's strength. Sub-Figure (a) compares the search interest to the fraction of respondents who had heard about the MeToo movement in a YouGov survey conducted in February-March 2019 (YouGov, 2019). Sub-Figure (b) compares search interest to the log of the number of tweets related to the movement, per 1,000 Twitter users, in October 2017.

Figure A.4: Search Interest by the Initial Strength of the MeToo Movement

(a) Search Interest in the Topic of the MeToo Movement

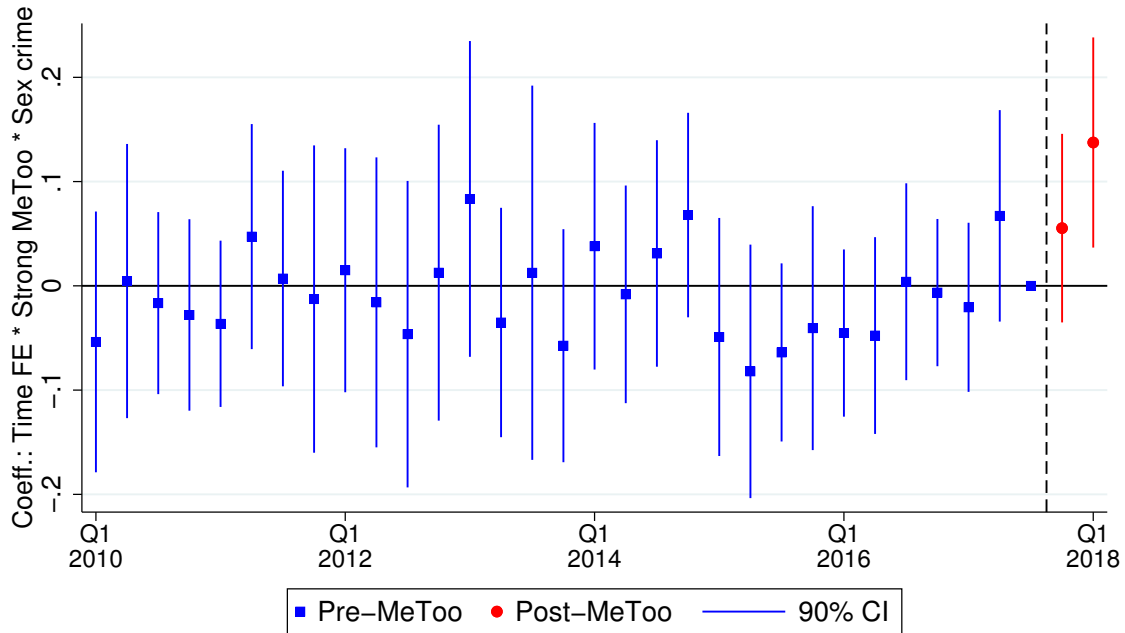


(b) Search Interest in the Topics of Sexual Harassment and Sexual Assault



These figures show monthly search interest related to the MeToo movement among OECD countries with strong and weak movements. Countries are classified as weak or strong by search interest in the MeToo topic in October 2017. Data is from Google Trends. The first vertical line represents the start of the MeToo movement, the second vertical line represents the end of the six-month period we use to measure short-term effects. Sub-Figure (a) shows search interest in the topic of the MeToo movement. The data is normalized so that the post-MeToo OECD mean equals 1. Sub-Figure (b) shows search interest in the topics of sexual harassment and sexual assault. The data is normalized so that the pre-MeToo mean equals 1 for each country. The sudden increase in Sub-Figure (b) around January 2016 is caused by the 2015-16 New Year's Eve sexual assaults in Germany.

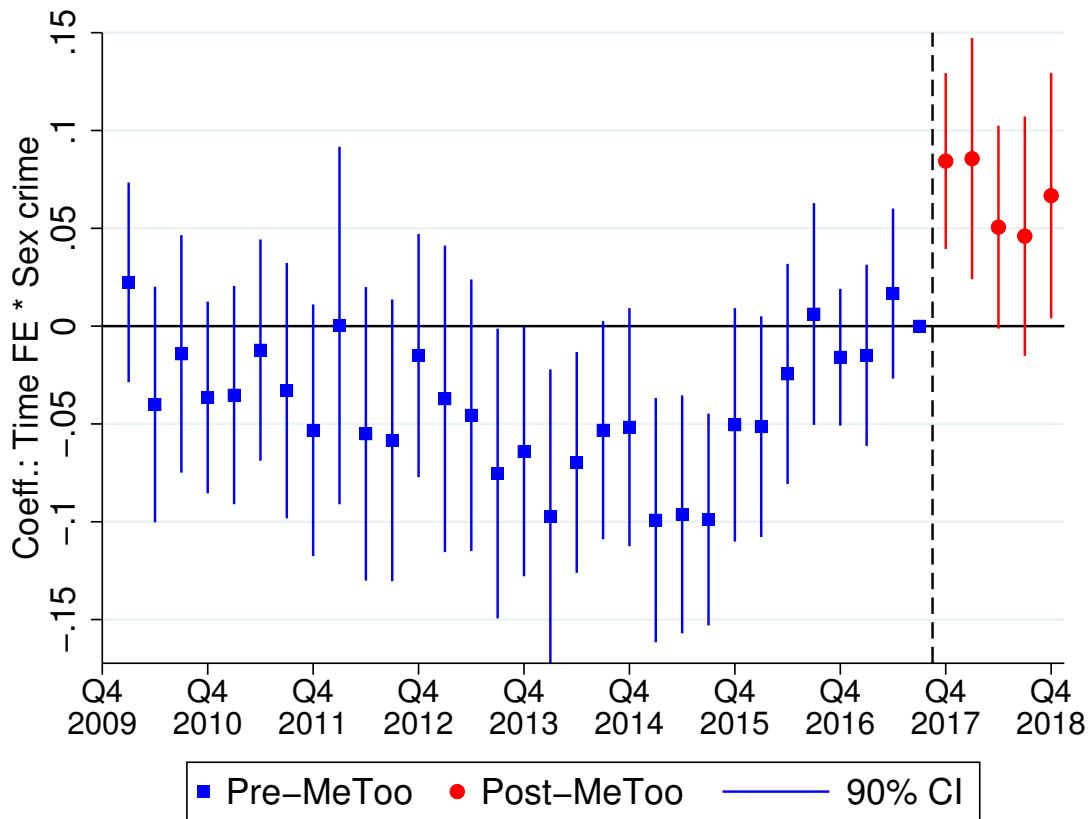
Figure A.5: Detrended and Deseasonalized Timeline of Triple-Difference Estimates



This figure shows triple-difference estimates for each quarter. The figure is based on one regression where the outcome variable is the log of crimes reported, detrended and deseasonalized by subtracting the country by crime type calendar quarter fixed effects and linear trends. The fixed effects and trends are calculated using data from before the start of the MeToo movement. The coefficients shown are those on each quarter interacted with whether the crime type is a sex crime and whether the country has a strong MeToo movement. The regression controls for time fixed effects separately interacted with the strong movement and sex crime variables. The quarter before the start of the MeToo movement is normalized to zero. Data from 31 OECD countries. 90% confidence intervals constructed using standard errors clustered at the country by crime type level.



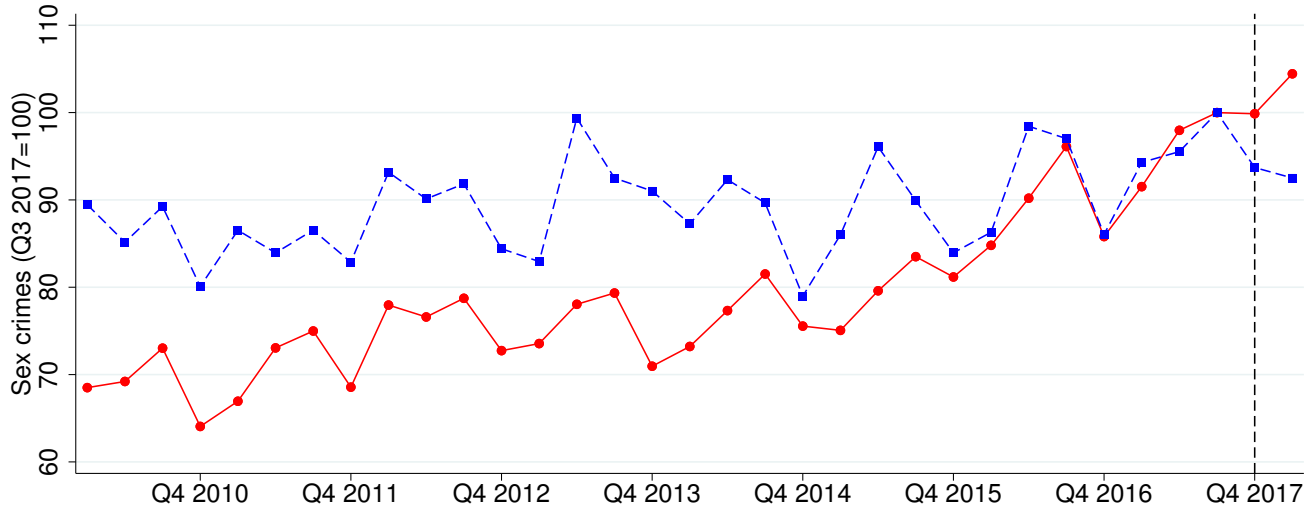
Figure A.6: Long-Term Effect: Detrended and Deseasonalized Timeline for Strong MeToo Countries



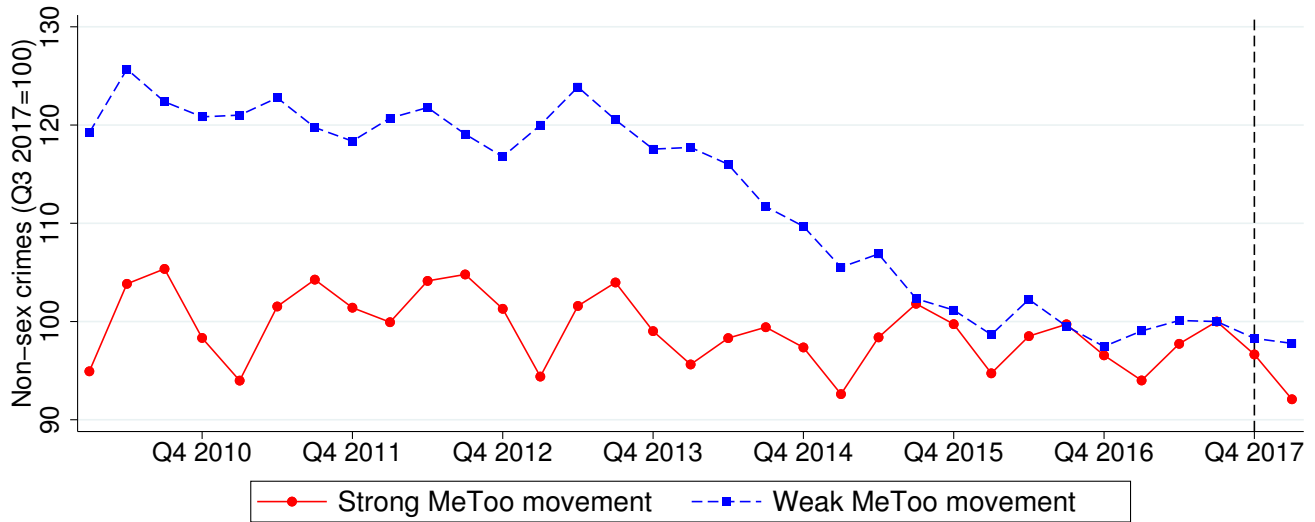
This figure shows difference-in-differences estimates for each quarter in our sample of 16 OECD countries with a strong MeToo movement. The figure is based on a regression where the outcome variable is the log of the number of crimes of a specific type, in a quarter, and in a country. The coefficients are point estimates of the quarter fixed effects interacted with whether a crime is a sex crime. The outcome variable is detrended and deseasonalized by subtracting the country by crime type calendar quarter fixed effects and linear trends. The fixed effects and trends are calculated using data from before the start of the MeToo movement. The coefficient for the quarter before the start of the MeToo movement is normalized to zero. The 90% confidence intervals are constructed using standard errors clustered at the country by crime type level.

Figure A.7: Crimes Reported in Countries with Strong and Weak Move-  
ments

(a) Sex Crimes Reported in Countries with Strong and Weak MeToo Movements



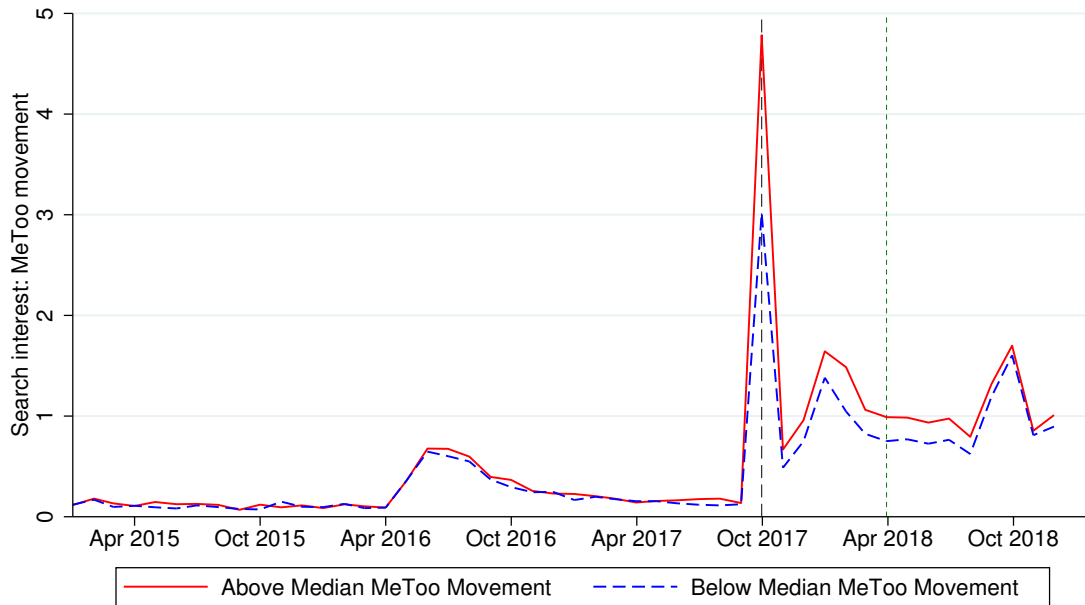
(b) Non-Sex Crimes Reported in Countries with Strong and Weak MeToo Movements



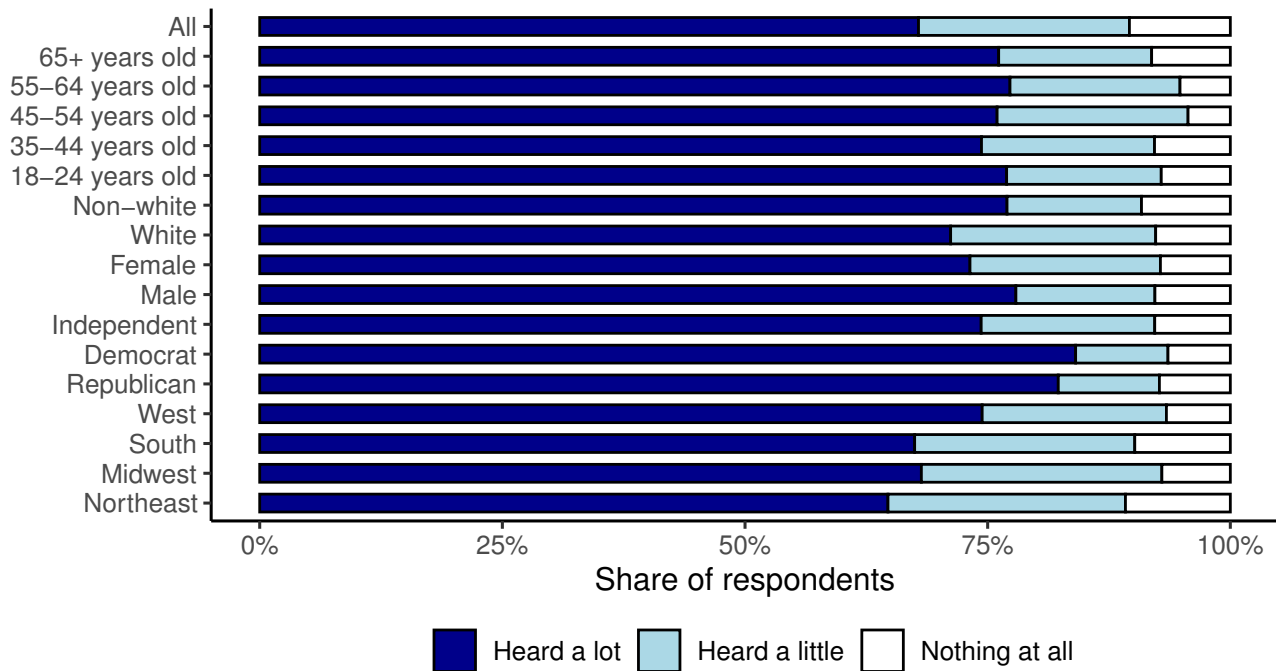
This figure shows the timelines of the four components of the triple-difference estimate, sex and non-sex crimes reported separately for countries with strong and weak MeToo movements. All crime categories are normalized to 100 in the period before the start of the MeToo movement (Q3 2017) for each country. Sub-Figure (a) shows sex crimes reported and Sub-Figure (b) shows non-sex crimes reported. The vertical dashed lines represent the start of the MeToo movement. Data includes all 31 countries in our sample. For 7 countries, data is available for only part of the period; see Appendix A.2 for details on each country.

Figure A.8: Variation in the MeToo Movement in the US

(a) MeToo Interest Across US States

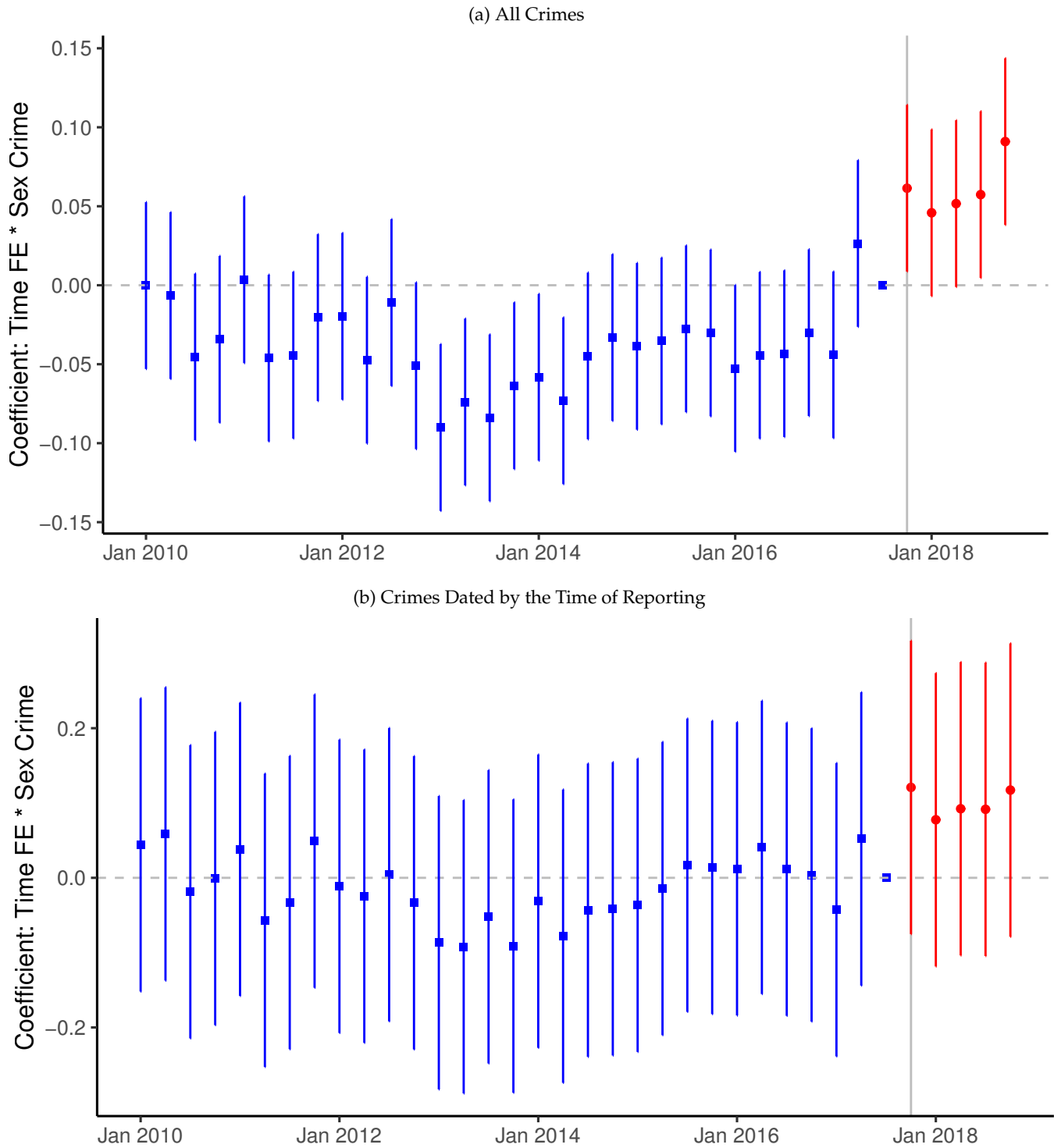


(b) Hearing about the MeToo Movement, Across Demographics



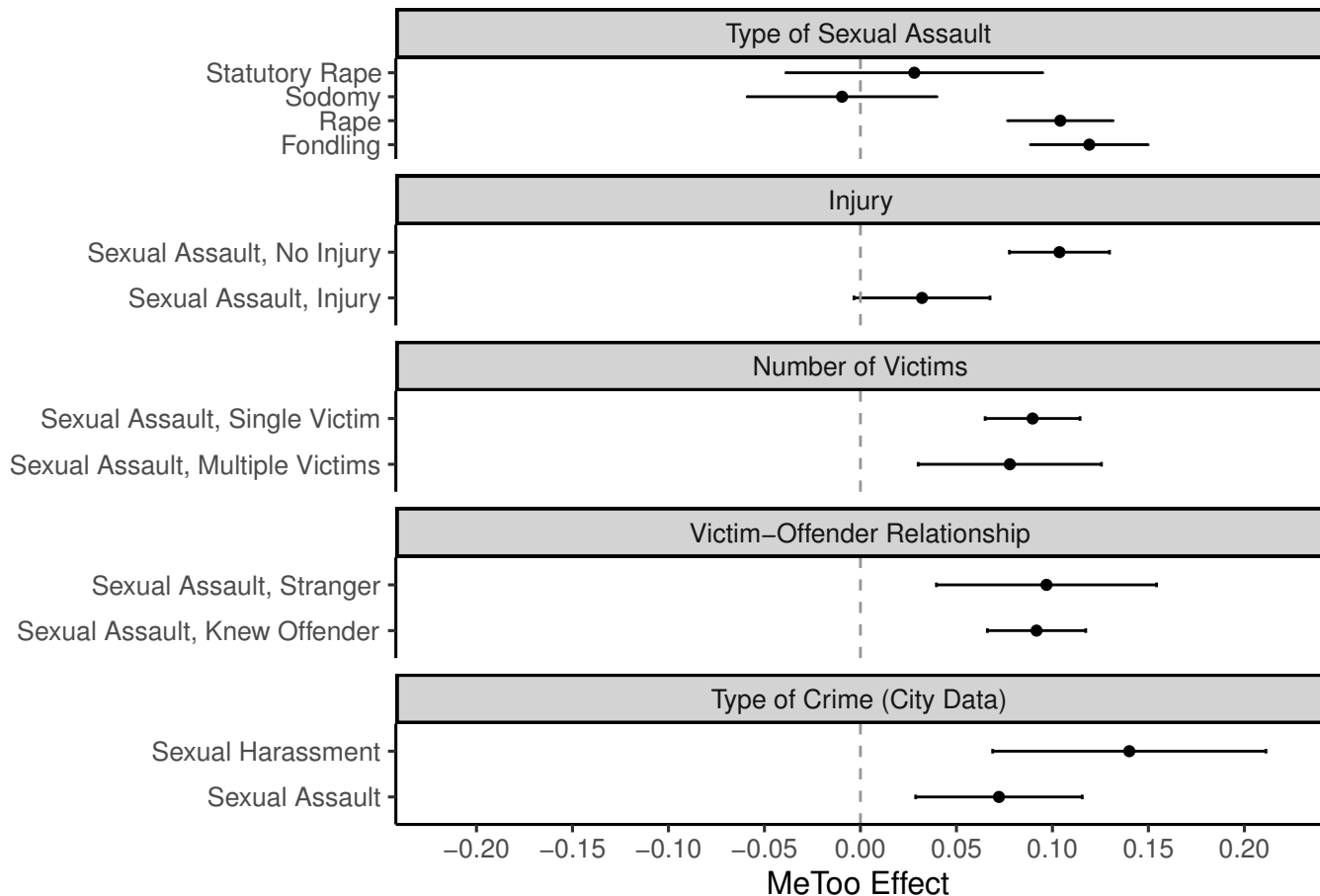
These figures show that interest in and knowledge about the MeToo movement was not concentrated among some US states or demographic groups. Sub-Figure (a) replicates Appendix Figure A.4a for US states and shows the average monthly search interest in states that had above and below median search interest in October 2017. Data is from Google Trends. The first vertical line represents the start of the MeToo movement, the second vertical line represents the end of the six-month period we use to measure short-term effects. The increase around May 2016 is due to the release of the Meghan Trainor song "Me Too". Sub-figure (b) shows the share of Americans who heard a lot, a little, or nothing at all about the MeToo movement according to a December 2017 survey (Pew Research Center, 2017).

Figure A.9: Long-Term Effect of the MeToo Movement in the US



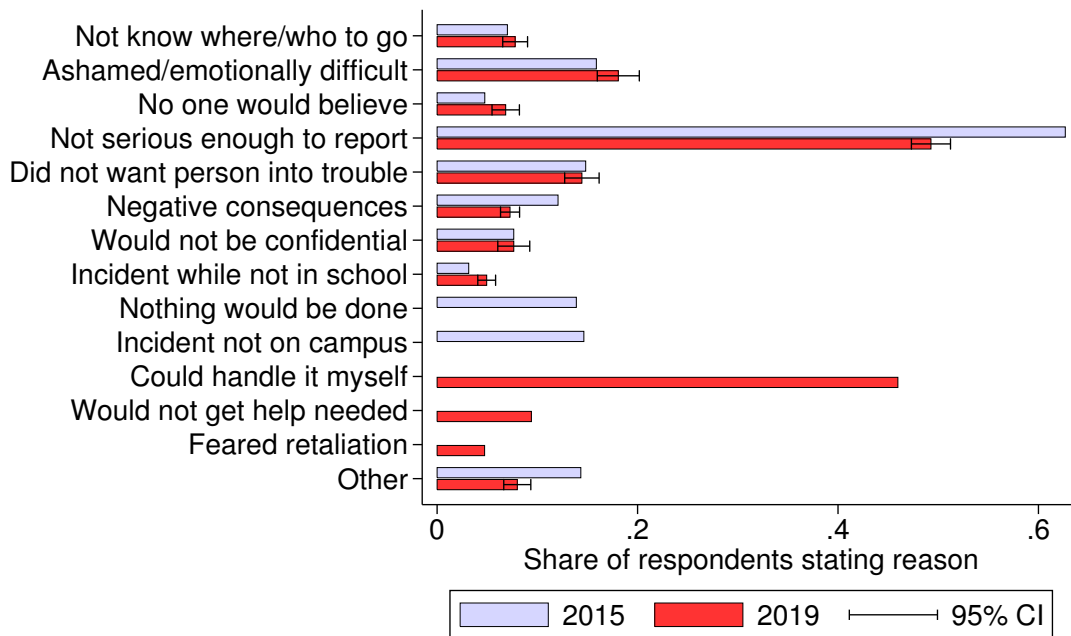
This figure shows difference-in-differences estimates for each quarter in the NIBRS US crime data. Sub-Figure (a) includes all crimes and Sub-Figure (b) includes only crimes where the crime date refers to the date the crime was reported and not the date it occurred. The figures are based on a regression where the outcome variable is the log of the number of crimes of a specific type, in a quarter, and in a state. The coefficients are point estimates of each quarter interacted with whether a crime is a sex crime. The outcome variable is detrended and deseasonalized by subtracting the pre-period state by crime type calendar quarter fixed effects and linear trends. The coefficient for the quarter before the start of the MeToo movement is normalized to zero. 90% confidence intervals are constructed using robust standard errors.

Figure A.10: Effect of the Movement by Relationship and Crime Type



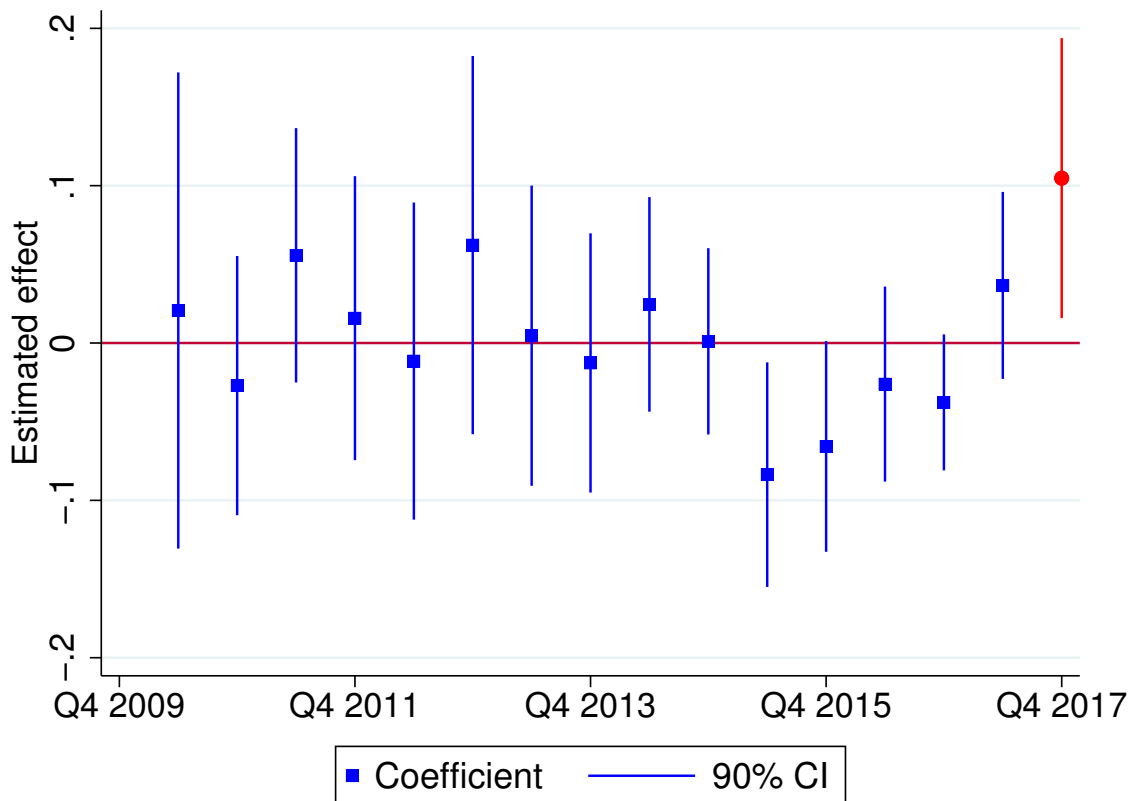
This figure shows the effect of the MeToo movement on different crime types. Each panel presents the results from a regression where crimes are aggregated into different categories and the reference group is all non-sex crimes. In the first panel, the category Sexual Assault With An Object is excluded since approximately one-third of state-by-month observations had zero crimes reported. Incidents related to multiple sexual offense crime categories are also excluded. The second panel excludes cases where it is unknown if a victim was injured. In the fourth panel, cases where the relationship between the victim and offender was not reported or where the relationship is unknown are excluded. Regressions are weighted by the number of crimes in each state before the MeToo movement started.

Figure A.11: Stated Reasons for not Reporting Sexual Assaults



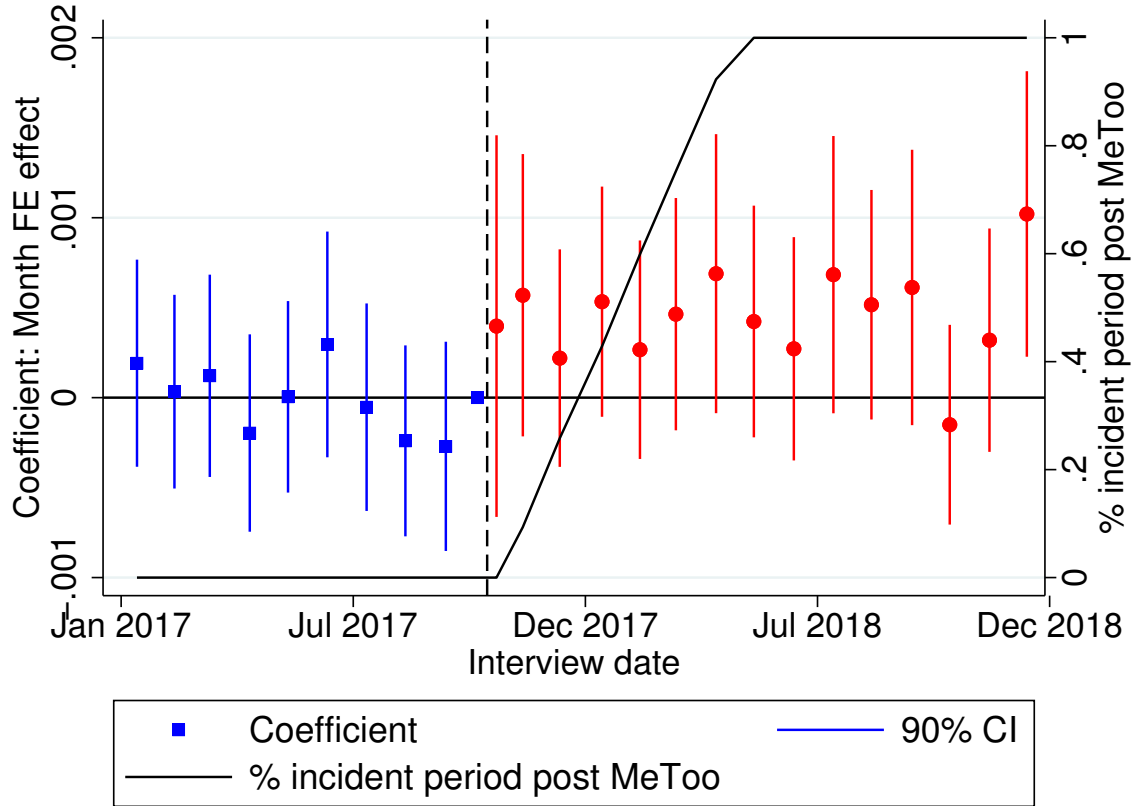
This figure shows the answers to the question "were any of the following reasons why you did not contact anyone at [university]?" (2015) and "why did you decide not to contact any of these programs or resources?" (2019) among victims of unique sexual assaults who did not report to the university. Respondents could choose multiple answers. The data is based on 10,042 observations from the 2015 and 2019 CCS. Sample weighted to be representative of the student population in surveyed universities. Confidence intervals are constructed using the standard errors, clustered at the university level, of the difference between the two estimates. The five answers for which there is only an estimate for one year were only asked in that year. More details on the possible answers are provided in Appendix A.8.

Figure A.12: Placebo Tests, Setting the Start Date of the MeToo Movement in Every Second Quarter from Q2 2010 to Q4 2017



This figure shows the results from 15 placebo triple-difference regressions (Q2 2010-Q2 2017) and our main triple-difference result (Q4 2017). Each line is from a separate regression using the full Q1 2010 - Q1 2018 dataset, but with different six-month periods for when the placebo MeToo movement occurred. The corresponding 90% confidence intervals are constructed using standard errors clustered at the country by crime type level.

Figure A.13: Changes in the Fraction of NCVS Respondents Describing Themselves as Victims of Sex Crimes



This figure shows the share of respondents in the NCVS survey who stated that they were a victim of a sex crime in interviews between January 2017 and December 2018. Each NCVS interview covers the "incident period" of the six calendar months before the calendar month of the interview. All coefficients are on month fixed effects from one regression. The outcome variable is whether a sex crime was mentioned in the interview, where region-specific linear time trends and seasonality have been subtracted. The regression estimates one fixed effect for each of the 108 months in 2010-2018, except for October 2017 for which there are two fixed effects, one for interviews before October 15, 2017 and one for interviews after October 15, 2017. The coefficient on the binary variable for the first half of October is normalized to zero. The figure displays all of the post-period coefficients and ten pre-period coefficients. The coefficients are measured on the vertical axis on the left. The solid line shows the fraction of the incident period that occurred before the start of the MeToo movement on the vertical axis on the right. The figure shows that the increase in sex crimes is equally large for all interviews after the start of the MeToo movement. The effect does not increase with the fraction of the incident period occurring after the start of the MeToo movement suggesting that the increase is not driven by an increase in the incidence of crime. The observations are weighted such that the sample is representative of the US population above 12 years of age. The 90% confidence intervals are constructed using robust standard errors.



Table A.1: Summary Statistics for Countries with Strong and Weak MeToo Movements

	Weak MeToo Movement Mean (SD)	Strong MeToo Movement Mean (SD)
Immediate MeToo Interest, October 2017	0.237 (0.138)	1.910 (2.411)
GDP per capita, PPP	27,772 (6,847)	47,105 (8,417)
Population (thousands)	35,821 (41,685)	41,919 (79,336)
Global freedom score	0.87 (0.11)	0.97 (0.03)
Percent internet use	77.1 (10.6)	88.8 (6.3)
Percent English-speaking population	29.1 (21.9)	76.2 (18.9)
Percent women in parliament	25.2 (9.8)	33.6 (8.0)
WVS: Men make better political leaders than women (1-4)	2.0 (0.2)	1.7 (0.2)
Baseline searches for sexual harassment and sexual assault	15.8 (20.0)	15.4 (9.4)
Number of non-sexual crimes reported per capita	2,045 (1,180)	6,556 (2,488)
Number of sexual crimes reported per capita	22.6 (22.9)	78.4 (43.3)
Sex crimes as a percent of all crimes reported	1.6 (2.1)	1.2 (0.6)

This table compares countries with strong and weak MeToo movements as measured by Google search interest in October 2017. Search interest is based on Google Trends data and is described in Section 3.1.2. GDP, internet use and percent of women in parliament are taken from the World Development Indicators. Population is taken from the OECD population indicator. Freedom scores are taken from the Freedom House global freedom scores. Share of English speakers is based on the Ethnologue 23rd and 24th editions (2019-2020) and is described in Appendix A.5. Attitudes toward political leaders are taken from the most recent World Values Survey taken before October 2017. Possible answers are 'strongly agree' (4), 'agree' (3), 'disagree' (2), 'strongly disagree' (1). Number of crimes reported is based on data we collected and is described in Section 3.1.1. Unless noted otherwise, all data is from 2016.

Table A.2: Effect of the MeToo Movement Estimated with Staggered Start Dates

	DCDH	3WFE
	(1)	(2)
Average effect (Q1-Q3)	0.11 ( 0.11)	0.09*** ( 0.03)
Q1 effect	0.04 ( 0.06)	0.08** ( 0.04)
Q2 effect	0.14 ( 0.10)	0.16** ( 0.07)
Q3 effect	0.19 ( 0.26)	0.15* ( 0.09)
Q4 effect		0.16 ( 0.10)
Q5 effect		0.13 ( 0.11)
Placebo 1	-0.01 ( 0.05)	0.02 ( 0.03)
Placebo 2	0.02 ( 0.04)	0.03 ( 0.03)
Placebo 3	0.01 ( 0.04)	0.01 ( 0.03)
Placebo 4	0.00 ( 0.02)	0.06 ( 0.04)
Country * Lin. trend	X	
Country * Quarter	X	
Country * Crime type * Lin. trend		X
Country * Crime type * Quarter		X

This table shows the estimate of the effect of the MeToo movement when allowing the movement to have a different start date in each country. We define the start date as the first quarter when cumulative Google search interest at the end of the quarter reached the OECD median cumulative interest at the end of Q4 2017. Column (1) uses the estimator proposed by De Chaisemartin and d’Haultfoeuille (2021) (DCDH). This is a difference-in-differences estimator, but by using the difference between the log of sex crimes and non-sex crimes as the outcome variable, it estimates the triple-difference effect. The first row shows the treatment effect averaged across the treated country-quarter observations. The next three rows show this effect estimated separately for each quarter since the start of the MeToo movement. The final four rows show the results of placebo tests estimating the deviation from the parallel trends assumption for two consecutive periods, one to four periods before the start of the MeToo movement. Column (2) uses a conventional three-way fixed effects (3WFE) estimator as a reference and should be interpreted carefully as it is a biased estimator in the presence of heterogeneous or dynamic treatment effects. Data from 31 OECD countries for the period from 2010 to 2018. Standard errors are clustered at the country by crime level in parenthesis. In Column (1) the standard errors were computed using 500 bootstrap replications. \*\*\*p<0.01, \*\*p<0.05, \*p<0.1.

Table A.3: Persistence of the Effect in Countries with a Strong MeToo Movement

	ln(crime)	
	(1)	(2)
Post * Sex crime	0.108*** (0.036)	
2017 Q4 * Sex crime		0.126*** (0.033)
2018 Q1 * Sex crime		0.127** (0.050)
2018 Q2 * Sex crime		0.091** (0.037)
2018 Q3 * Sex crime		0.087** (0.039)
2018 Q4 * Sex crime		0.110** (0.045)
Country * Crime type * Lin. trend	X	X
Country * Crime type * Quarter	X	X
Post	X	
Q4 2017-Q4 2018 FE		X
Final quarter	Q4 2018	Q4 2018
Observations	1,036	1,036
Clusters	32	32

This table shows the effect of the MeToo movement over time using data from the 16 OECD countries with a strong MeToo movement in October 2017. The outcome variable is the log of the number of reported crimes in a specific category, in a country, and in a quarter. Standard errors clustered at the country by crime level in parenthesis. \*\*\*p<0.01, \*\*p<0.05, \*p<0.1.

Table A.4: Robustness to Alternative Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(crime)							
Post * Sex crime * Strong MeToo	0.105* (0.054)	0.113 (0.226)	0.150** (0.060)	0.146 (0.103)	0.139** (0.059)	0.118 (0.108)	0.095* (0.051)	0.097** (0.048)
Post	X	X		X	X	X	X	
Strong MeToo		X						
Sex crime		X						
Post * Sex crime	X	X		X	X	X	X	
Post * Strong MeToo	X	X		X	X	X	X	
Sex crime * Strong MeToo		X						
Country * Crime type	X		X	X	X	X	X	X
Time * Crime type			X					X
Time * Country			X					X
Country * Crime type * Lin. trend	X				X		X	X
Country * Crime type * Quarter	X					X	X	X
Country * Crime type * Quadratic trend							X	X
Final quarter	Q1 2018	Q1 2018	Q1 2018	Q1 2018	Q1 2018	Q1 2018	Q1 2018	Q1 2018
Observations	1,826	1,826	1,826	1,826	1,826	1,826	1,826	1,826
Clusters	62	62	62	62	62	62	62	62

This table shows the robustness of our main estimate of the effect of the MeToo movement to alternative fixed effects and time controls. Column (1) shows our main estimate from Table 1 Column (4). Column (2) does not use any fixed effects. Column (3) uses country by crime type, country by time period, and crime type by time period fixed effects. Column (4) shows the main estimate without controlling for calendar quarter fixed effects and linear time trends. Column (5) shows the main estimate without calendar fixed effects and Column (6) shows the estimate without linear time trends. Column (7) uses our main specification but also controls for quadratic time trends interacted with each crime type by country combination. Column (8) uses all the controls from Column (3)-(7). Data from 31 OECD countries for the period from Q1 2010 to Q1 2018. A country is categorized as having a strong MeToo movement if search interest for the topic of the MeToo movement was above the OECD median in October 2017. Standard errors clustered at the country by crime level in parenthesis. \*\*\*p<0.01, \*\*p<0.05, \*p<0.1.

Table A.5: Effect of the MeToo Movement in the US with Crime Aggregated by Offense Types

	ihs(crime)		
	(1)	(2)	(3)
Post * Sexual Assault	0.091*** (0.015)	0.106*** (0.017)	0.106*** (0.025)
State * Crime Type * Lin. Trend	X	X	X
State * Crime Type * Month	X	X	X
Post	X	X	X
Agg Crimes	Sexual/Other	Offense Type	Offense Type
S.E	Robust	Cluster by Offense Type	Cluster by Type*State
Num of Clusters		21	756
Final Month	Mar 18	Mar 18	Mar 18
Observations	6,876	70,500	70,500

This table shows the effect of the MeToo movement using different crime aggregation and inference methods. Column (1) is our main estimate where crimes are categorized as either sex crimes or non-sex crimes, and robust standard errors are used. In Columns (2)-(3), crimes are aggregated according to the NIBRS offense types. The outcome is the number of crimes of a specific type in a state and month. The coefficient of interest is that on the interaction of the post-period and sexual assault. Incidents that include multiple offense types are excluded along with state by offense types that reported no crimes in the pre-period. Column (2) clusters standard errors by the offense type and Column (3) clusters at the state by offense type level. In Column (1), observations are weighted by the mean number of crimes in each state in the pre-period. In Columns (2) and (3), observations are weighted by the number of crimes in each state and offense type in the pre-period. 2010-2018 NIBRS data. \*\*\*p<0.01; \*\*p<0.05; \*p<0.1.

Table A.6: Effect of the MeToo Movement by City

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	ihs(crime)								
Post * Sex Crimes	0.098*** (0.024)	0.144*** (0.041)	0.085*** (0.033)	0.106 (0.075)	0.189*** (0.074)	0.084 (0.076)	-0.074 (0.083)	-0.043 (0.087)	0.093 (0.071)
City * Crime Type * Lin. Trend	X								
City * Crime Type * Month	X	X	X	X	X	X	X	X	X
Crime Type * Lin. Trend		X	X	X	X	X	X	X	X
Crime Type * Month		X	X	X	X	X	X	X	X
Post	X	X	X	X	X	X	X	X	X
Final Month	Mar 18	Mar 18	Mar 18	Mar 18	Mar 18	Mar 18	Mar 18	Mar 18	Mar 18
City	All	NYC	LA	Austin	Seattle	Denver	Louisville	Tucson	Kansas City
Observations	1,512	198	198	198	198	126	198	198	198

This table shows the estimated effect of the MeToo movement on sex crimes for each city separately. Robust standard errors in parenthesis. \*\*\*p<0.01; \*\*p<0.05; \*p<0.1

Table A.7: Persistence of the Effect in the US

	ihs(crime)				
	(1)	(2)	(3)	(4)	(5)
Post * Sex Crimes	0.099*** (0.011)		0.094*** (0.018)		
2017 Q4 * Sex Crimes		0.099*** (0.016)		0.116*** (0.030)	0.104*** (0.035)
2018 Q1 * Sex Crimes		0.083*** (0.019)		0.081** (0.032)	0.038 (0.036)
2018 Q2 * Sex Crimes		0.091*** (0.017)		0.077*** (0.025)	0.060** (0.024)
2018 Q3 * Sex Crimes		0.096*** (0.020)		0.120*** (0.032)	0.111*** (0.031)
2018 Q4 * Sex Crimes		0.131*** (0.028)		0.075 (0.055)	0.059 (0.055)
Location * Crime Type * Lin. Trend	X	X	X	X	X
Location * Crime Type * Month	X	X	X	X	X
Post	X	X	X	X	X
Data	NIBRS	NIBRS	Cities	Cities	Cities
Crimes	All	All	All	All	Reported Within 1 M
Observations	7,506	7,506	1,656	1,656	1,656

This table shows the effect of the MeToo movement on sex crimes by quarter. Data is aggregated at the monthly state/city by crime category level. Columns (1) and (2) are based on 2010-2018 NIBRS data. Columns (3)-(5) are based on the sample of US cities. Columns (1) and (3) report the long-run effects until December 2018. Columns (2), (4), (5) report the effect by quarter. Column (5) includes only crimes that were reported within 30 days of their occurrence. Regressions are weighted by the number of crimes in each state/city before the MeToo movement started. Robust standard error in parenthesis. \*\*\*p<0.01; \*\*p<0.05; \*p<0.1

Table A.8: Effect of the MeToo Movement on Clearance

	ihs(crime)					
	(1)	(2)	(3)	(4)	(5)	(6)
Post * Sexual Assault, Not Cleared	0.097*** (0.016)			0.107*** (0.011)		
Post * Sexual Assault, Cleared	0.073*** (0.023)			0.074*** (0.016)		
Post * Sexual Assault		0.098*** (0.016)	0.078*** (0.024)		0.112*** (0.012)	0.075*** (0.016)
Difference	0.024			0.033**		
State * Crime Type * Lin. Trend	X	X	X	X	X	X
State * Crime Type * Month	X	X	X	X	X	X
Post	X	X	X	X	X	X
Final Month	Mar 18	Mar 18	Mar 18	Dec 18	Dec 18	Dec 18
Crimes	All	Not Cleared	Cleared	All	Not Cleared	Cleared
Observations	10,314	6,876	6,876	11,259	7,506	7,506

This table shows the effect of the MeToo movement on sexual assaults by whether a case was cleared. A case is cleared if an arrest was made or if the police have sufficient probable cause to arrest a suspect but could not make an arrest for reasons outside their control including the victim refusing to cooperate, the death of the offender, the prosecutor declining prosecution for a reason other than lack of probable cause, the offender being in the custody of another jurisdiction, and the offender being a juvenile. In Columns (1) and (4), the crimes are aggregated into three separate crime categories: sexual assaults that were cleared, sexual assaults that were not cleared, and non-sex crimes, which are the control group. In Columns (2) and (5), only crimes that were not cleared are included and Columns (3) and (6) include only crimes that were cleared. Columns (1)-(3) show short-run effects and Columns (4)-(6) show long-run effects. 2010-2018 NIBRS data. Regressions are weighted by the number of crimes in each state before the MeToo movement started. Robust standard errors in parenthesis. \*\*\*p<0.01; \*\*p<0.05; \*p<0.1



Table A.9: Effect on Sexual Assault Arrest Rate Conditional on Covariates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post	-0.0041** (0.0021)	-0.0031 (0.0021)	-0.0039* (0.0021)	-0.0040* (0.0021)	-0.0043** (0.0021)	-0.0030 (0.0021)	-0.0032 (0.0021)	-0.0041* (0.0021)	-0.0015 (0.0020)
Agency		X							X
Injury			X						X
Location				X					X
Relationship					X				X
Type						X			X
Weapon							X		X
Victim								X	X
State * Month	X	X	X	X	X	X	X	X	X
State * Lin. Trend	X	X	X	X	X	X	X	X	X
Final Month	Mar 18	Mar 18	Mar 18	Mar 18	Mar 18	Mar 18	Mar 18	Mar 18	Mar 18
Observations	643,168	643,168	643,168	643,168	643,168	643,168	643,168	643,168	643,168

This table shows the association between the post-period and arrests related to sexual assault when controlling for incident details. Each observation is a sexual assault crime reported between Jan 2010 and March 2018 and the outcome is whether the report resulted in an arrest. All columns control for a linear trend and calendar month fixed effects. Column (1) shows that the arrest rate for sexual assault decreased in the post-period. Column (2)-(9) control for additional covariates. Column (2) controls for agency fixed effects. Column (3) controls for whether the incident resulted in an injury. Column (4) controls for the location type (residence, outside residence, unknown or multiple locations). Column (5) controls for the relationship between the victim and the offender (offender known to the victim, offender is a stranger, unknown relationship, or missing data). Column (6) controls for the type of sexual assault and whether the incident is associated with multiple offenses. Column (7) controls for whether and which weapon was used. Column (8) controls for the victim's race, sex, and age group. Column (9) controls for all the covariates. Robust standard errors in parenthesis. \*\*\*p<0.01; \*\*p<0.05; \*p<0.1

Table A.10: Effect of the MeToo Movement by the Lag Between the Occurrence and Reporting Dates

	(1)	(2)
Post * Sex Crimes, Lag<=30 Days	0.070*** (0.026)	0.075*** (0.019)
Post * Sex Crimes, Lag>30 Days	0.191*** (0.049)	0.176*** (0.038)
City * Crime Type * Lin. Trend	X	X
City * Crime Type * Month	X	X
Post	X	X
Final Month	Mar 2018	Dec 2018
Pre period mean Lag<=30	152	152
Pre period mean Lag>30	36	36
Observations	2,268	2,484

This table shows the effect of the MeToo movement on sex crimes according to when the crime was reported, relative to when it occurred. In all columns, the data is aggregated into three categories: sex crimes reported within 30 days, sex crimes reported after more than 30 days, and non-sex crimes. Non-sex crimes is the reference category. Column (1) focuses on the primary short-term effect and includes data until March 2018 and Column (2) focuses on the long-term effect and includes data until December 2018. The regressions are weighted by the number of crimes in each city before the MeToo movement started. City crime data 2010-2018. Robust standard errors in parenthesis. \*\*\*p<0.01; \*\*p<0.05; \*p<0.1

Table A.11: Effect on Crimes that Occurred Before the Movement Started

	ihc(crime) (1)	ihc(crime) (2)	ihc(crime) (3)	ihc(crime) (4)	ihc(crime) (5)
Post * Sex Crimes	0.211* (0.116)	0.152 (0.111)	0.131 (0.107)	0.216** (0.095)	0.191** (0.087)
City * Crime Type * Lin. Trend	X	X	X	X	X
City * Crime Type * Month	X	X	X	X	X
Post	X	X	X	X	X
Final Month	Oct 2017	Nov 2017	Dec 2017	Jan 2018	Feb 2018
Crimes Included	1 Month <= Lag	2 Month <= Lag	3 Month <= Lag	4 Month <= Lag	5 Month <= Lag
Observations	1,432	1,448	1,464	1,480	1,496

This table shows the effect of the MeToo movement on the reporting of sex crimes that occurred before the start of the movement. Column (1) includes only crimes that were reported at least one month after they occurred and restricts data to crimes reported by October 2017. Therefore, all crimes included occurred before the MeToo movement started. Columns (2)-(5) gradually expand the data to include crimes reported two to five months after the movement started (by November 2017 to February 2018) while gradually restricting the data to crime reported at least two to five months after they occurred so that all crimes included still occurred before the MeToo movement started. 2010-2018 city crime data. Regressions are weighted by the number of crimes in each city before the MeToo movement started. Robust standard errors in parenthesis. \*\*\*p<0.01; \*\*p<0.05; \*p<0.1

Table A.12: Change in the Incidence and Reporting Rate of Sexual Assaults, CCS data

	Sexual assault (1)	(2)	Unique sexual assault (3)	(4)	Assault reported (5)	Told friend/family (6)	(7)	(8)
1y before survey * 2019 (Post MeToo)	-0.003 (0.003)		0.000 (0.001)		0.027** (0.011)		0.037*** (0.011)	
2y before survey * 2019 (Post MeToo)	0.014*** (0.004)		0.005*** (0.002)		0.045*** (0.014)		0.037* (0.018)	
3y before survey * 2019 (Pre MeToo)	0.019*** (0.004)		0.006*** (0.001)		-0.041 (0.025)		0.019 (0.027)	
4y before survey * 2019 (Pre MeToo)	0.016*** (0.004)		0.005*** (0.002)		0.019 (0.020)		-0.043 (0.037)	
5y before survey * 2019 (Pre MeToo)	0.019*** (0.004)		0.004 (0.002)		-0.013 (0.025)		-0.045 (0.041)	
Within 2y of survey * 2019 survey		-0.014*** (0.002)		-0.003** (0.001)		0.052** (0.022)		0.048** (0.020)
Mean outcome 2015 survey	0.07	0.07	0.02	0.02	0.13	0.13	0.74	0.74
Demographic controls	X	X	X	X	X	X	X	X
University FE	X	X	X	X	X	X	X	X
Response rate control	X	X	X	X	X	X	X	X
Years since survey FE	X	X	X	X	X	X	X	X
2019 survey FE		X		X		X		X
Assault type FE					X	X	X	X
Clusters	21	21	21	21	21	21	21	21
Observations	588,832	588,832	502,245	502,245	11,148	11,148	11,148	11,148

This table shows the change in the incidence of sexual assault, the propensity to report sexual assaults to university programs, and the propensity to tell a friend or family member about sexual assaults after the start of the MeToo movement. In Columns (1) and (2), the unit of observation is every academic year when the student was enrolled in the university. Columns (3) and (4) show the changes in the share of enrolled students who experienced a sexual assault in an academic year. Columns (5) and (6) show the changes in the share of enrolled students who experienced a sexual assault in an academic year, excluding any student who experienced multiple incidents of physical sexual misconduct. In Columns (7)-(8), the unit of observation is a unique sexual assault. Columns (5) and (6) show the change in the reporting rate of unique sexual assaults. Columns (7) and (8) show the change in the share of students who told a friend or family member about the unique sexual assault. Data is from the universities conducting both the 2015 and 2019 CCS. Sample weighted to be representative of the student population in surveyed universities. Standard errors are clustered at the university level. \*\*\*p<0.01; \*\*p<0.05; \*p<0.1.

Table A.13: Change in Attitudes Related to the MeToo Movement Among Women Respondents (in Standard Deviations)

(a) Perceptions of Problem and Individuals Reporting, Women in VOTER data

	Workplace sexual harassment is no longer a problem (1)	Accusers often cause more problems than they solve (2)
2018-2019 surveys	-0.068** (0.034)	0.047 (0.034)
Ref. Group	2016 Survey	2016 Survey
Respondent FE	X	X
Observations	5,838	5,819

(b) Perceptions of Size of Problem and University Response to Reporting, Women in CCS data

	Scale of problem		University response to reports	
	How problematic? (1)	Will you experience? (2)	Officials take seriously (3)	Fair investigation (4)
2019 survey	0.207** (0.086)	0.106* (0.060)	-0.079** (0.037)	-0.092* (0.047)
Ref. Group	2015 survey	2015 survey	2015 survey	2015 survey
Controls	X	X	X	X
University FE	X	X	X	X
Clusters	21	21	21	21
Observations	142,828	143,621	135,038	134,334

This table is similar to Table 5 but focuses only on women respondents instead of the entire sample since the vast majority of those experiencing and reporting sexual crimes to the police are women. In the CCS data, 84% of sexual assault victims identified as women. We exclude respondents who identify as a man or as neither a man or a woman, as well as those not reporting a gender.

Table A.14: The Effect of the MeToo Movement on Sex Crimes Reported per 100,000 Persons

	Crime per 100k persons				
	(1)	(2)	(3)	(4)	(5)
Post * Strong MeToo	2.736*** (0.607)		0.513 (0.616)	0.513 (0.616)	0.501 (0.677)
Post * Sex crime		1.078** (0.516)		-0.067 (0.221)	0.073 (0.350)
Post * Sex crime * Strong MeToo			2.155** (0.833)	2.223** (0.862)	2.116** (0.920)
Post * Sex crime * Weak MeToo			-0.067 (0.221)		
Post	0.076 (0.147)	0.408 (0.320)	0.143 (0.166)	0.143 (0.166)	0.084 (0.273)
Country * Crime type * Lin. trend	X	X	X	X	X
Country * Crime type * Quarter	X	X	X	X	X
Crime data used	Sex crimes	All crimes	All crimes	All crimes	All crimes
Weights	Uniform	Uniform	Uniform	Uniform	Population
Final quarter	Q1 2018	Q1 2018	Q1 2018	Q1 2018	Q1 2018
Observations	913	1,826	1,826	1,826	1,826
Clusters	31	62	62	62	62

This table shows the effect of the MeToo movement on the number of sex crimes reported per capita during the first six months of the movement. The outcome variable is the number of reported crimes per 100,000 persons in a specific crime category, in a country, and in a quarter. The numbers of non-sex crimes reported have been rescaled so that the quarterly mean equals that of sex crimes reported for the four quarters preceding the MeToo movement (Q4 2016 - Q3 2017). Column (1) analyzes only sex crime using a difference-in-differences estimate over time and across countries. Columns (2)-(5) include data on both sex crimes and non-sex crimes. Column (2) presents a difference-in-differences estimate over time and between crime types. Column (3) presents the estimate from Column (2) separately for countries with strong and weak MeToo movements. Column (4) presents our primary triple-difference estimate over time, across countries, and between crime types. Column (5) shows the triple-difference estimate weighted by the 2016 population of each country. A country is categorized as having a strong MeToo movement if search interest for the topic of the MeToo movement was above the OECD median in October 2017. Data from 31 OECD countries from Q1 2010 to Q1 2018. Standard errors clustered at the country by crime level in parenthesis. \*\*\*p<0.01, \*\*p<0.05, \*p<0.1.

Table A.15: Alternative Statistical Inference Methods

Point estimate	0.105
Inference methods:	
(1) SEs clustered at country by crime type(*) P-value	(0.054) [ 0.057]
(2) Wild bootstrap p-value(*)	[ 0.062]
(3) Robust standard errors(**) P-value	(0.046) [ 0.022]
(4) SEs clustered by country(*) P-value	(0.052) [ 0.051]
(5) Two-way clustered SEs: Country by crime and time(**) P-value	(0.049) [ 0.041]
(6) Regression including country by post FEs(***) P-value	(0.037) [ 0.006]

This table displays standard errors and p-values using alternative statistical inference methods. Standard errors are displayed in parentheses and p-values are displayed in square brackets. Row (1) displays our preferred standard error clustered at the country by crime type level and corresponding p-value. Row (2) shows the p-value calculated with wild bootstrap (Roodman et al., 2019). Row (3) displays the non-clustered, robust standard error and the corresponding p-value. Row (4) displays the standard error clustered at the level of the country and the corresponding p-value. Row (5) displays the standard error and p-value calculated using two-way clustering at the country by crime type and time period level. Row (6) displays the standard error clustered at the country by crime type level where the regression includes country by post-period fixed effects. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table A.16: The Effect of the MeToo Movement, Matrix Completion Estimates

	(1)	(2)
Sample	Estimate	Estimate With Row Fixed Effects
International data	0.108*** (0.026)	0.173*** (0.038)
US data - NIBRS	0.090*** (0.022)	0.093*** (0.03)
US data - large cities	0.121*** (0.017)	0.072*** (0.021)

This table displays the result of the matrix completion estimates for each sample. The first column shows the result without including any explicit controls and in the second column, we control for row fixed effects. When analyzing the international data, each row is a country by aggregate crime category (sex crimes or non-sex crimes). When analyzing the NIBRS and US city data, each row is a state by crime category or city by crime category, respectively. The method is described in Appendix B.4. Standard errors generated by bootstrapping.



Table A.17: Effect of the MeToo Movement by Neighborhood

	ihs(crime)					
	(1)	(2)	(3)	(4)	(5)	(6)
Post * Sex Crimes	0.111*** (0.018)	0.111*** (0.018)	0.111*** (0.018)	0.112*** (0.018)	0.112*** (0.018)	0.111*** (0.018)
Post * Sex Crimes * Med. Income (std. dev.)		0.043*** (0.017)				
Post * Sex Crimes * % College			0.134 (0.088)			
Post * Sex Crimes * % Blacks (Compared to Whites)				-0.008 (0.081)		
Post * Sex Crimes * % Other Race (Compared to Whites)					0.142 (0.116)	
Post * Sex Crimes * % Hispanics						-0.026 (0.080)

Interquartile Range of Demographic		1.301	0.243	0.277	0.264	0.384
Diff. in Effect, 75th-25th Pct.		0.056	0.032	-0.002	0.037	-0.01
Neighborhood * Crime Type * Lin. Trend	X	X	X	X	X	X
Neighborhood * Crime Type * Month	X	X	X	X	X	X
Post	X	X	X	X	X	X
Post * Demographic	X	X	X	X	X	X
Final Month	Mar 18	Mar 18	Mar 18	Mar 18	Mar 18	Mar 18
Observations	25,750	25,750	25,750	25,750	25,750	25,750

This table shows the effect of the MeToo movement based on neighborhood-level data and tests for heterogeneous effects by neighborhood demographics. 2010-2018 city crime data. All regressions are weighted by the number of crimes in each neighborhood before the MeToo movement started. The income variable is standardized so its weighted mean equals zero and weighted standard deviation equals one. All other demographic variables have their weighted mean subtracted. Neighborhoods where demographic data does not exist, such as an airport, are excluded from all columns. Robust standard errors in parenthesis. \*\*\*p<0.01; \*\*p<0.05; \*p<0.1

Table A.18: Change in NCVS Respondents Reporting Being the Victim of a Sex Crime

	% Sex crime victim		
	(1)	(2)	(3)
Interview post MeToo	0.00044*** (0.00014)	0.00049*** (0.00010)	0.00043* (0.00023)
% incident period post MeToo			0.00007 (0.00027)
Region * Linear trend	X	X	X
Region * Month FE	X	X	X
Observations	1,431,240	1,613,292	1,613,292
Pre-MeToo mean	0.00065	0.00065	0.00065
Last interview date in data	31/03/2018	31/12/2018	31/12/2018
Ave. % incident period post MeToo	0.40	0.77	0.77

This table shows the effect of the MeToo movement on the propensity of NCVS respondents to report being a victim of a sex crime. Column (1) measures the effect of whether the interview was conducted after the start of the MeToo movement and estimates the effect over the first six months after the movement started. Column (2) estimates the effect over 15 months after the movement started. Column (3) separately measures the effect of whether the interview was conducted after the start of the MeToo movement and the share of the incident period covered by the interview that occurred after the start of the MeToo movement. Each NCVS interview covers the "incident period" of the six calendar months before the calendar month of the interview. Data from 2010-2018 NCVS. Robust standard errors in parenthesis. \*\*\*p<0.01; \*\*p<0.05; \*p<0.1.