Unstable Partisanship during Scandals: Estimating the Causal Impact of Partygate on the British Conservative Party*

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Abstract

Partygate is one of the most significant political scandals to have taken place in the United Kingdom, with then Prime Minister Boris Johnson becoming the first to break the law while in office. However, the consequences of Partygate for British politics remain under-explored in political science research. In this paper, I shed light on the impact of Partygate on identification with and support for the Conservative Party through an Unexpected Event during Survey Design. As news of the scandals broke out among the British public in November 2021, collection of Wave 21 of the British Election Study Internet Panel was under way. This created a natural experiment where some respondents completed the survey "before" Partygate while others completed the survey "after", thus being as-if randomly assigned to a control and treatment group. Using a difference-in-differences estimator, I find that Partygate caused an immediate decline in both support for and identification with the Conservative Party. As expected from theories of stability of party identification, the decline in electoral support is smaller than the decline in identification. However, contrary to the hypotheses, I find no effect of Partygate on party identification strength among Conservatives: those who remained Conservative identifiers did not experience weakening of their identification. The results are robust to several alternative estimation approaches. These findings bear important implications for the study of political behavior: they shed light on the state of democratic accountability in the United Kingdom.

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1 Introduction

"Partygate" is a political scandal resulting from gatherings and parties that took place in governmental settings during the Covid-19 lockdowns of 2020 in the United Kingdom. With Boris Johnson becoming the first British Prime Minister to break the law while in office (Walker, 2022), it is arguably one of the worst scandals in modern British politics. Partygate is often thought to be the initial driver of the decline in support for the Conservative Party observed between 2021 and 2024 (Sir John Curtice in Ellyatt, 2024; Webber, 2024; Onward, 2024); yet, to the best of my knowledge, no formal research has corroborated this claim. This paper aims to address this gap by assessing the causal impact of Partygate on identification with and support for the Conservative Party.

This inquiry is important for three reasons. First, understanding whether and how the public reacts to scandals speaks to debates on the impact of events on political behavior. One line of research argues that individuals may have become less responsive to scandals in light of motivated reasoning (Wagner, Tarlov, and Vivyan, 2014; Lodge and Taber, 2000) and increasing affective polarization (Marchal and Watson, 2019). For example, because individuals increasingly like their partisan in-group and increasingly dislike the out-group, they may have become less likely to detach from their political identities by engaging in motivated reasoning when confronted with in-group wrongdoing (Bhatti, Hansen, and Leth Olsen, 2013). Second, in the British context claims of increased vote choice and partisan volatility are abundant (Fieldhouse, J. Green, Evans, Mellon, Prosser, Schmitt, et al., 2019), so understanding the sources of volatility constitutes an essential research endeavor. Scandals – and more broadly perceptions of distrust toward politicians – may shed light on this debate. Finally, this inquiry speaks to concerns about the state of democratic accountability in the UK, one of the world's oldest democracies. If voters fail to punish incumbents who are affected by scandals, the fundamental logic of democratic accountability is compromised (Costas-Pérez, Solé-Ollé, and Sorribas-Navarro, 2012).

This paper answers the research question through an Unexpected Event during Survey Design (UESD), which leverages the as-if random assignment of survey respondents to treatment and control conditions based on their survey completion date. In the case of Partygate, collection of Wave 22 of the British Election Study Internet Panel (Fieldhouse, J. Green, Evans, Mellon, Prosser, Bailey, et al., 2023) was under way when news of the scandal broke out among the public, thus providing a natural experiment where some respondents reported their party identification and vote intention before knowing about Partygate while other did so after. Analyses using both naive and difference-in-differences estimators show that Partygate caused a decline in both identification with and support for the Conservative Party. As expected from extensive literature on the stability of party identification, the decline in electoral support is larger in magnitude than the decline in party identification. These findings are confirmed through several robustness checks, all available in the Online Appendix.

The rest of the paper is structured as follows. First, I describe Partygate, with a particular focus on the timeline of events. Second, I briefly outline relevant literature on the impact of scandals for political behavior and outline four testable hypotheses. I focus in particular on the role of partisanship in moderating the potential electoral consequences of scandals. Subsequently, I illustrate the UESD and its assumptions, as well as the two estimators I utilize. I then present the results and describe key robustness checks. I conclude with a discussion of the findings, paying particular attention to their implications for relevant literature.

1.1 The timeline of Partygate

Partygate can be conceptualized as a gradual – rather than abrupt – scandal, insofar as the British public became aware of it slowly over the course of several months. Allegations of government staff breaking lockdown laws were first reported by the Daily Mirror on 30 November 2021 (Crerar, 2021). This was followed on 7 December 2021 by ITV's release of a leaked video showing then Press Secretary Allegra Stratton conducting a mock press conference and joking about parties taking place at 10 Downing Street (ITV News, 2021). The gatherings, which contravened social distancing regulations in place at the time, have since been subject to three separate investigations by the Cabinet Office (Gray, 2022), the Metropolitan Police (BBC News, 2022), and the House of Commons Privileges Committee (Committee of Privileges, 2023).

The Cabinet Office inquiry, led by Second Permanent Secretary Sue Gray, culminated in a report that was released on 25 May 2022 (Gray, 2022). The report described 16 events on 12 dates between May 2020 and April 2021, 8 of which were attended by Boris Johnson (ibid.). Many of these events breached Covid-19 lockdown regulations in place at the time (ibid.). The Metropolitan Police investigation culminated in 126 Fixed Penalty Notices [1] to 83 people (BBC News, 2022). Among these were Boris Johnson, his wife Carrie Johnson, and Chancellor (and future Prime Minister) Rishi Sunak (ibid.). Finally, the Privileges Committee inquiry was set up to determine whether Boris Johnson had knowingly misled Parliament (Committee of Privileges, 2023). Although Johnson denied lying throughout the investigation, the final report concluded that he was in fact guilty of

[1] This is a notice that provides someone with the option of paying a fee to avoid criminal prosecution for an alleged offense.

not only deliberately misleading Parliament, but also of contempt of Parliament (Committee of Privileges, 2023). The day the report was released, 9 June 2023, Johnson resigned as Member of Parliament, still maintaining his innocence.

In short, the first evidence of Partygate was released among the British public on 7 December 2021, but the last investigation was not concluded until 9 June 2023. On the one hand, the trickle-down nature of the scandal makes it difficult to study, as there isn't a unique "big" event on which to focus. However, I argue that the release of the first video on 7 December 2021 constitutes a suitable proxy to study the consequences of Partygate on political behavior. First, the video constituted incontrovertible evidence of at least one breach of lockdown rules by the government. Second, the British media – both newspapers and the BBC – reported extensively on the video already on 8 December, thus spreading the news among the public (see Appendix A). Finally, focusing on the video leak as the treatment in this study constitutes a stringent test of any hypothesized consequence of Partygate; the video is arguably a weak, albeit diffuse, treatment because neither Boris Johnson nor other Cabinet members were portrayed in it. Hence, any effect of Partygate found using this treatment likely constitutes a lower bound of the overall effect.

2 The consequences of scandals for political behavior

Although the contribution of this paper is mainly empirical, it is important to situate it in the broader literature on scandals and political behavior. The question of whether scandals have consequences for political behavior and, if so, what these consequences are, speaks to the logic of democratic accountability (Costas-Pérez, Solé-Ollé, and Sorribas-Navarro, 2012). Since the number of political scandals is increasing worldwide (Kumlin and Esaiasson, 2012), understanding to what extent voters punish incumbents (and politicians more broadly) for wrongdoing is becoming ever more relevant.

Empirical evidence on the electoral consequences of scandals is mixed, with some scholars finding that scandals do have a substantial impact on political behavior (Dunlap and Wisniewski, 1978; Best, Ladewig, and Wong, 2013; Winters and Weitz-Shapiro, 2016), others others arguing for limited impact (Peters and Welch, 1980; Banducci and Karp, 1994; Dimock and Jacobson, 1995; Welch and Hibbing, 1997; Pattie and Ron Johnston, 2012; Vivyan, Wagner, and Tarlov, 2012; Basinger, 2013; Cobb and Taylor, 2015), and others still highlighting the conditionality of effects according to, *inter alia*, scandal type (Sikorski, 2018; Bhatti, Hansen, and Leth Olsen, 2013; McDermott, Schwartz, and Vallejo, 2015), media framing (Peterson and Vonnahme, 2014; Costas-Pérez, Solé-Ollé, and Sorribas-Navarro, 2012), affect toward the incumbent (Dimock and Jacobson, 1995; Fischle, 2000), and partisanship (Cobb and Taylor, 2015). After a brief discussion of this literature, I utilize it to derive my hypotheses on the impact of Partygate on identification with and support for the British Conservative Party.

2.1 Assessing the electoral impact of scandals

Most research finds that scandals have *some* electoral consequences, but the magnitude of these consequences is a source of contention in the literature. Starting with arguments on the limited impact of scandals, Basinger (2013) finds that most scandal-plagued candidates in the post-Watergate era in the US do manage to secure re-election, although they lose vote share. Overall, about 40% do not survive the scandal either due to electoral defeat or preemptive retirement from politics (ibid.). Similarly, candidates accused of corruption in the periods 1968-1978 and 1982-1990 perform worse in elections than those not subject to such accusations, although they are generally re-elected (Peters and Welch, 1980; Welch and Hibbing, 1997).

These findings based on aggregate evidence suggest that the electoral consequences of scandals may be limited. Even though some voters punish incumbents implicated in scandals, their numbers are not sufficient to result in removal from office. Studies that focus on single scandals support this conclusion. One of the most studied episodes is the US House of Representatives banking scandal, which broke out in 1992 when it was revealed that representatives were allowed to overdraw their House accounts without incurring any penalties. This scandal had a small negative impact on incumbents' re-election prospects by encouraging some to retire and reducing the electoral competitiveness of the others (Banducci and Karp, 1994). Dimock and Jacobson (1995) similarly find that those involved were evaluated more negatively by voters.

The 2009 House of Commons expenses scandal in the UK had similarly muted effects. One of the reasons is thought to be that MPs most gravely implicated in the scandal self-selected out of running for re-election or were *de facto* prevented from doing so (Pattie and Ron Johnston, 2012). Beyond this selection effect, MPs involved in the scandal who did seek re-election were only marginally punished by voters (Pattie and Ron Johnston, 2012; Vivyan, Wagner, and Tarlov, 2012). Vivyan and co-authors (2012) document a failure of perceptions to translate into action; the expenses scandal did increase perceptions of misbehavior among voters, but this translated into only marginal vote share losses. This discrepancy between attitudes and behavior vis à vis scandals is further bolstered by Cobbs and Taylor (Cobb and Taylor, 2015), who find that although corruption scandals in the US generally reduce favorability toward the affected party, this does not result in substantial changes in electoral outcomes.

However, the notion that scandals have limited electoral consequences has not remained unchallenged. Some argue that the negative consequences of scandals may not even be confined to those directly implicated in them. For example, the Abramoff lobbying scandal of 2005, which involved members of the US Congress accused of committing fraud against Native American tribes, affected all Republicans irrespective of actual wrongdoing (Best, Ladewig, and Wong, 2013). Indeed, Republican politicians with legal financial links to Abramoff were punished as harshly as those actually being investigated for fraud (ibid.). In a similar vein, a vignette experiment using as treatment a corruption scandal in a fictitious mayoral administration finds that voters punish all mayors linked to corruption scandals, irrespective of actual involvement (Winters and Weitz-Shapiro, 2016). However, the magnitude of the punishment was lower when the scandals were caused by *other* members of the mayoral administration (ibid.).

Most importantly for the present inquiry into the consequences of Partygate, Dunlap and Wisniewski (1978) find that Watergate led to a decrease in party identification among both Republicans and Democrats in favor of Independents. Comparatively, Republicans lost the largest share of identifiers, which in turn led to substantial electoral losses (ibid.). Although this study presents some clear limitations, not least the fact that the survey data comes only from the state of Washington, it suggests that scandals may not only affect volatile outcomes such as vote choice or incumbent favorability, but also deeply held social identities such as party identification (D. Green, Palmquist, and Schickler, 2002). At the same time, one explanation of this finding might be the sheer magnitude and relevance of Watergate in US politics.

Overall, the conflicting findings regarding the extent to which scandals affect political behavior suggest that other variables may moderate the relationship. I now turn to discussing the conditionality in the effects of scandals by focusing on three factors that may be particularly relevant to hypothesize the consequences of Partygate: type of scandal, media coverage, and motivated reasoning.

2.2 Conditionality in the effects of scandals

It has been proposed that the impact of scandals may be conditional on the type. A common distinction drawn in the literature is between morality scandals (often related to adultery) and financial ones, where the former are thought to be more consequential than the latter (Sikorski, 2018). Further, experimental evidence suggests that morality scandals characterized by political-ideological hypocrisy may be punished especially harshly (Bhatti, Hansen, and Leth Olsen, 2013; McDermott, Schwartz, and Vallejo, 2015). When politicians display ideological hypocrisy – for instance a left-wing politician making use of private healthcare – they lose trustworthiness among the electorate (Bhatti, Hansen, and Leth Olsen, 2013). Similarly, when a scandal contradicts politicians' openly stated values, as in the case of adultery when campaigning on Christian values, voters' perceptions are more negative than if values are not openly expressed (McDermott, Schwartz, and Vallejo, 2015).

Another key factor that influences the impact of scandals on political behavior is the volume and tone of media coverage. When voters acquire information on the gravity and illegality of a scandal through a high volume of coverage in the media, they are more likely to punish those involved (Costas-Pérez, Solé-Ollé, and Sorribas-Navarro, 2012). The mechanism is straightforward: media coverage helps voters discern the prevalence of corruption and reach conclusions on the legal validity of accusations (ibid.). Further, media choices can impact scandals' consequences: voters who watched cable television reacted differently than those who watched network news to the 2011 Herman Cain sexual harassment scandal (Peterson and Vonnahme, 2014).

Lodge and Taber's (Lodge and Taber, 2000) theory of opinion formation through motivated reasoning further corroborates the argument that the effects of scandals on political behavior may be conditional – specifically, it depends on the dominant cognitive mechanism in the electorate. In this framework, voters process new information driven by one of two goals: accuracy or direction (ibid.). The former refers to the drive to reach a correct conclusion, while the latter indicates the drive to reach a conclusion that is compatible with one's previous beliefs (ibid.). These two goals, which are not mutually exclusive, must be considered when determining the impact of scandals on political outcomes. If individuals are mostly driven by accuracy goals, we expect punishment of the scandal-ridden incumbent, while the effect is likely to be more muted if direction goals are stronger.

Strong party identification is a key predictor of direction goals in motivated reasoning (Taber and Lodge, 2006) because it can act as a perceptual screen through which information is filtered (Campbell et al., 1960; Richard Johnston, 2006), which thus affects political cognition. In other words, partisans overwhelmingly process information in way a that leads to confirmation of prior partisan beliefs (Taber and Lodge, 2006). For example, in corruption scandals those who identify with the political party involved are less likely to punish it for wrongdoing compared to those who do not identify with it (Cobb and Taylor, 2015).

Affect toward the incumbent may also encourage direction goals in post-scandal incumbent evaluations. Dimock and Jacobson (1995) show that voters' evaluations of the 1992 House banking scandal were heavily influenced by previous affect toward the incumbent. Faced with the choice of punishing or not punishing an incumbent they liked, voters generally opted for not punishing (Dimock and Jacobson, 1995). The same conditionality is found for reactions to the Clinton-Lewinsky affair (Fischle, 2000), where pre-existing positive affect toward Clinton reduce the impact of the scandal on voters' evaluations of the president.

2.3 Hypotheses on the consequences of Partygate

What, then, should be the consequences of Partygate on identification with and support for the Conservative Party? On the one hand, it is possible that the scandal had no consequences on political behavior because Conservative identifiers and voters simply pursued direction goals to minimize cognitive dissonance. On the other hand, the nature of Partygate – especially in its connection to the Covid-19 lockdowns – may result in prevalence of accuracy goals, as voters were confronted with a government that failed to abide by its own laws. Additionally, as previously argued, the volume of media coverage of the leaked video was quite high. In short, the characteristics of Partygate point toward a decline in both identification with and support for the Conservative Party.

A potential mechanism driving reactions to the scandal may be political-ideological hypocrisy (see Bhatti, Hansen, and Leth Olsen, 2013; McDermott, Schwartz, and Vallejo, 2015), in that the government broke the very same lockdown laws it had instituted. There is manifest hypocrisy in the government imposing (and enforcing tightly) a set of rules on citizens, while failing to abide by those rules themselves. This suggests that voters (likely already discontented from a year and a half of lockdowns and other restrictions) might have reacted strongly against the incumbent party in this instance. Hence, I formulate the following hypotheses:

H1: Partygate caused a decline in identification with the Conservative Party.

H2: Partygate caused a decline in intention to vote for the Conservative Party.

Since party identification is often thought to be a relatively stable social identity (D. Green, Palmquist, and Schickler, 2002; Richard Johnston, 2006), the decline in party identification is expected to be substantively smaller than the decline in intention to support the party in a future election. Therefore, I test a third hypothesis:

H3: The decline in electoral support for the Conservative Party is substantively larger than the decline in identification with the party.

Finally, to examine the consequences of Partygate on party identification in detail, I also test for whether the scandals caused a decline in party identification strength among Conservative identifiers.

In other words, I examine whether those who did not abandon their Conservative party identification in response to their scandal nevertheless exhibit some change in their identification:

H4: Partygate caused a decline in strength of identification with the Conservative Party.

3 Research design

I test the hypotheses using an Unexpected Event during Survey Design (UESD). As the name suggests, this research design leverages the unexpected occurrence of an event of interest while survey data is being collected, so that respondents are as-if randomly assigned to treatment and control conditions based on their survey completion time. The UESD is the most appropriate research design to study the causal impact of Partygate because, as news of Partygate broke out among the British public in the evening of 7 December 2021, Wave 22 of the British Election Study Internet Panel was being collected.

3.1 The Unexpected Event During Survey Design

The UESD leverages the unexpected occurrence of an event while a survey is being collected, thus enabling estimation of the causal effect of the event by comparing the outcome variable among respondents who complete the survey before (control group) and after (treatment group) the event (Muñoz, Falcó-Gimeno, and Hernández, 2020). The two foundational assumptions that enable causal identification in the UESD are excludability and (temporal) ignorability. The former requires that the timing of survey completion affects the outcome variable only through the event of interest, while the latter holds that the potential outcomes of respondents must be orthogonal to the time of survey completion. Here I provide some qualitative evidence that both of these assumptions hold in the case of Partygate, which I corroborate with more rigorous tests in the next section.

Threats to excludability include collateral and simultaneous events (ibid.). Collateral events include all events related to the one of interest, such as the government's response to the leaked Partygate video. If such collateral events are present, any causal effect of Partygate on the outcome variables may be due not only to the event itself but also to these reaction events. Indeed, the day after the mock press conference video was leaked, then Prime Minister Boris Johnson announced a Cabinet inquiry into possible lockdown rules breaches (Hansard, 2021). However, although this may limit the generalizability of this paper's findings to other political scandals where the government reacted differently, the Cabinet inquiry announcement is a constitutive part of the scandals – it underscores the failure of Boris Johnson to acknowledge any wrongdoing in the immediate aftermath of the scandal becoming public, which subsequently resulted in parliamentary sanctions against him (Committee of Privileges, 2023).

Similarly, if other events that may affect public opinion happened on the same date, the UESD leads to estimates of the joint effect of these simultaneous events. In the case of Partygate, qualitative examination of newspaper coverage on 7 and 8 December 2021 suggests that no other significant events happened that should affect electoral support or, even less plausibly, party identification (see Appendix A).

Beyond collateral and simultaneous events, the most substantial threat to excludabilty in the context of Partygate would be endogenous timing of the video leak, that is, if it were planned by politically motivated actors. If this were the case, the treatment would be correlated with the error term and thus introduce bias into the estimation of causal effects. I contend that this is highly unlikely, given that ITV – the platform that first leaked the incriminating video on 7 December 2021 – does not exhibit partisan or ideological bias (Forsdick, 2018).

Turning to ignorability, the case in favor of orthogonality of the outcome variables with respect to the timing of survey completion is strong due to the unexpectedness of the event. Even though the Daily Mirror had reported allegations of lockdown breaches among the government already on 30 November, this did not gained traction: no other major news outlet reported on these allegations (BBC News and Wayback Machine, 2021). Appendix A provides further detail on the lack of media coverage of these preliminary allegations. Hence, the video leak of 7 December 2021 can be construed as unexpected, thus making it very implausible that party identification and vote intention of respondents would be correlated with survey completion times.

Since news of Partygate became public in the evening of 7 December 2021, I assign respondents who completed Wave 22 of the survey before 7 December to the control group and those who completed it on 8 December or after to the treatment group. I remove respondents who completed the survey on 7 December because it is more difficult to determine who was aware of the scandal when filling out the survey. Conversely, qualitative examination of the front pages of main newspapers and of BBC news coverage of the leaked video on 8 December shows that the scandal had already drawn attention of the media (see Appendix A).

Further, the video leak constitutes a sufficiently salient event, as evident from the sudden and marked increase in Google Trends searches for "Downing Street party" and "party video" between 5 and 11 December 2021 (Google Trends, 2021a; Google Trends, 2021b). In Appendix B, I bolster this claim by showing that treated respondents are more likely to label discontent toward politics in general and toward a specific political party as the most important issue facing Britain. Since no other significant political events occurred in the survey's time frame, this suggests that treated respondents were in fact aware of Partygate when completing the survey, that is, they complied with their treatment assignment.

Although I have argued that high news coverage, Google trend searches, and the "most important issue" analysis (Appendix B) suggest that treated respondents were most likely aware of Partygate already on 8 December, it remains possible that some did not comply with this "treatment encouragement". Given that I cannot measure directly respondents' awareness of the video leak, I choose to err on the side of caution and interpret all estimates as an Intent to Treat Effect (ITT). In short, I estimate the causal effect of treatment assignment, rather than treatment take-up.

3.2 Data and variables

The data utilized in this study come from Wave 21 and Wave 22 of the British Election Study (BES) Internet Panel (Fieldhouse, J. Green, Evans, Mellon, Prosser, Bailey, et al., 2023). This survey is collected by YouGov using an online sample of their panel members. Wave 21 was taken by 30,281 respondents between 7 and 25 May 2021. On the other hand, Wave 22 was taken by 28,113 respondents (19,241 of whom also took Wave 21, yielding a retention rate of 63.5%) between 26 November and 15 December 2021. The panel follows participants over time, but I choose to use an unbalanced panel for the difference-in-differences analysis in order to maximise external validity. Hence, I effectively treat the panel waves as two cross sections.

The dependent variables are vote intention in the next General Election, party identification, and party identification strength. Vote intention – which I also call electoral support – is measured by asking *If there were a UK General Election tomorrow, which party would you vote for?*, with 13 party categories as answer choices, plus the option of not voting at all. Party identification is measured using the standard question *Generally speaking, do you think of yourself as Labour, Conservative, Liberal Democrat or what?*, with 13 categorical options including no party identification. Both vote intention and party identification are re-coded as binary variables: they take value 1 if the answer choice is "Conservative" and 0 otherwise. Party identification strength is measured with the question *Would you call yourself very strong, fairly strong, or not very strong [party name]?*, which has the following answer choices: "Very strong" (1), "Fairly strong" (2), and "Not very strong" (3). I re-code this variable so that 1 denotes not very strong identifiers and 3 denotes very strong identifiers.

I utilize a standard set of socioeconomic covariates thought to affect political behavior to conduct balance tests: gender, age, ethnicity, educational attainment, household income, and socioeconomic status. These are complemented by a set of additional covariates that are theoretically relevant to the research question, which include left-right ideology, newspaper typically read, self-reported political attention, vote in the 2019 General Election, and personally knowing someone who died from Covid-19. Newspaper readership and political attention matter because they likely influence treatment uptake among treated respondents; those who read newspapers regularly and pay closer attention to politics are more likely to be aware of the Partygate video leak starting on 8 December 2021. Knowing someone who died from Covid-19 is particularly important because these individuals may perceive evident breaches of Covid-19 lockdown rules as a worse offense than others who had not been affected by Covid-19 in this way. More detailed variable descriptions and descriptive statistics for all covariates are presented in Appendix C and Appendix D.

3.3 The difference-in-means and difference-in-differences estimators

I estimate the ITT using both a difference-in-means and difference-in-differences estimator. The difference-in-means estimator subtracts the mean value of the outcome variable in the control group from the mean value of the outcome variable in the treatment group. It can be calculated by regressing each of the dependent variables on the treatment. Even though the two core dependent variables in this study – party identification and electoral support – are binary, I choose an OLS regression specification. This choice is informed by the higher ease of interpretation of OLS coefficients compared to logit ones, but in Appendix E, I show that all results are robust to a logistic specification. Hence, I estimate:

$$Y_i = \alpha + \beta T_i + \varepsilon_i \tag{1}$$

where Y_i is the dependent variable – vote intention or party identification – of unit i, which takes value $Y_i = 1$ if unit i is Conservatives and $Y_i = 0$ otherwise; T_i is the treatment dummy, which equals 1 if unit i is assigned to the treatment group and 0 if it is assigned to the control group; α is the intercept and ε is the random error component. I use the difference-in-means estimator to test H1 and H2. To test H4, I first subset the data to include only Conservative identifiers and then use this estimator.

The difference-in-means produces an unbiased estimate of the causal effect of Partygate on identification with and support for the Conservative Party if and only if the assumptions of the UESD are met. To relax both the assumptions of excludability and temporal ignorability, I also test the hypotheses with a difference-in-differences estimator. The only assumption required to conduct a difference-in-differences analysis is the parallel trends assumption, which I provide evidence for in the next section.

The difference-in-differences setup for this study is as follows. The control group is defined as units who completed Wave 22 of the BES Internet Panel before 7 December 2021, while units who completed it on 8 December or after are assigned to the treatment group. Again, I eliminate observations from 7 December to reduce potential issues of non-compliance in the treatment group. The pre-treatment period is defined as Wave 21, where both the control and treatment group are untreated. The post-treatment period is Wave 22, where the treatment group is treated and the control group is not. I choose an OLS specification due to higher interpretability, but the results are robust to estimation using logistic regression Appendix D. I estimate the following difference-in-differences equation:

$$Y_{igt} = \alpha_{igt} + \beta Treat_g + \gamma Post_t + \delta(Treat_g \times Post_t) + \varepsilon_{igt} \tag{2}$$

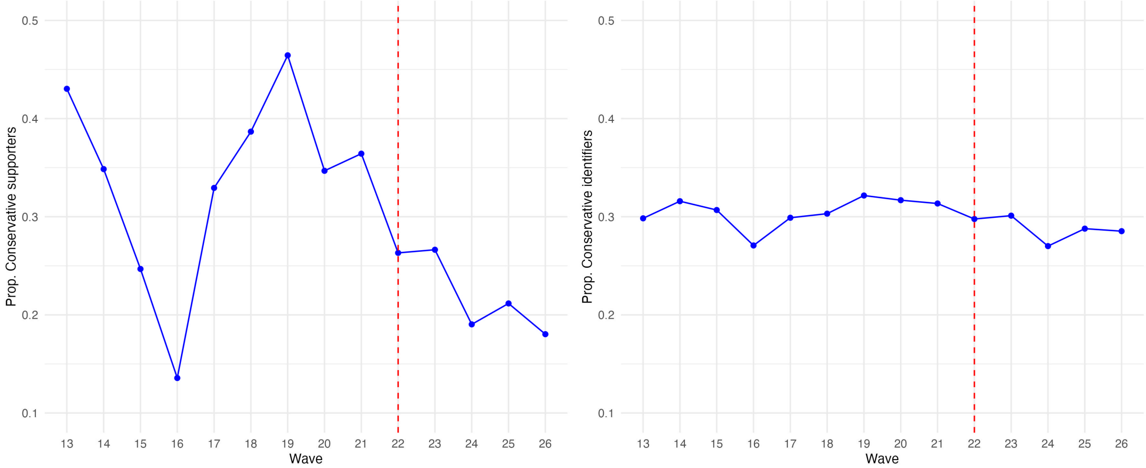
where Y_{igt} is the dependent variable for unit i in treatment group g at time t; α_{igt} and ε_{igt} are the intercept and random error terms, respectively; $Treat_g$ is a dummy variable indicating the treatment assignment of each unit: $Treat_g = 1$ for units in the treatment group and $Treat_g = 0$ for units in the control group. $Post_t$ is another dummy indicating the time period, either pre- ($Post_t = 0$) or post-treatment ($Post_t = 1$). The coefficient δ of the interaction term between $Treat_g$ and $Post_t$ is the coefficient of interest. H1 and H2 are tested using this specification with party identification and vote intention as the dependent variables, respectively. H4 is tested using the same specification among the subset of Conservative identifiers with party identification strength as the outcome.

4 Analysis and results

To begin, Figure 1 shows the proportion of survey respondents who intend to vote for the Conservative Party and the proportion of Conservative Party identifiers across waves. Three patterns emerge. First, it is evident that vote intention is much more volatile than party identification; the former ranges from 0.14 in Wave 16 to 0.46 in Wave 19, while the latter from 0.27 in Wave 16 and 24 to 0.32 in Wave 19. Second, even though the swings in party identification are smaller than those in vote intention, the two variables follow a similar pattern over time. Third, a sharp decline in support for the Conservative Party can be detected between Wave 21 and Wave 22, the latter being in part a

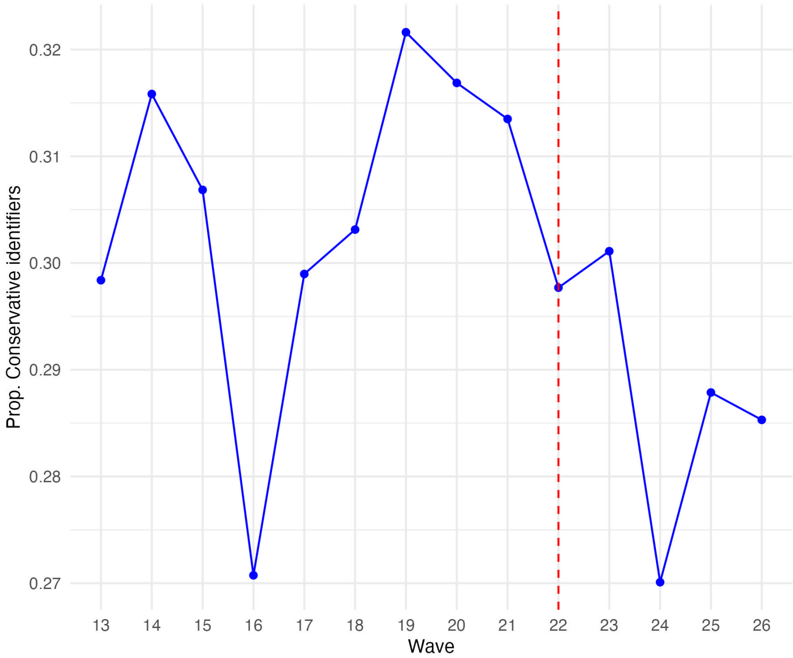
"Partygate wave". This lends preliminary support for H2.

Figure 1: Proportion of Conservative Party supporters and identifiers across waves.



Changes in the proportion of Conservative identifiers across waves are shown in Figure 2 in greater detail. Although the magnitude of the fluctuations in this variable is not substantively large, there is a clear acceleration in the decline in Conservative identification between Wave 21 and Wave 22. This corroborates the intuition of H1, i.e. that Partygate pushed individuals to de-identify from the Conservative Party.

Figure 2: Proportion of Conservative Party identifiers across waves.



4.1 Identifying assumptions of the UESD

Before presenting the estimation results, I attempt to show that the core identifying assumptions of the UESD are in fact met in the case of Partygate. This should improve confidence in the difference-in-means estimates, which I accompany nonetheless with a difference-in-differences analysis. I first assess covariate balance across the treatment and control groups: if covariates are balanced, we can be more confident in the assumption of temporal ignorability because there are no systematic differences on observables across treatment conditions. Therefore, I conduct balance tests on left-right ideology, age, gender, ethnicity, education, household income, socioeconomic status, newspaper read, political attention, vote in the 2019 General Election, and knowing someone who died from Covid-19. Due to space constraints, I cannot report the balance tests in full here (see Appendix D). The variables that show statistically significant and sizable imbalances are gender, ethnicity, and education. Hence, I estimate the ITT both naively and controlling for the imbalanced covariates.

Following (Muñoz, Falcó-Gimeno, and Hernández, 2020), I examine pre-treatment trends in the outcome variables to strengthen the argument in favor of the excludability assumption. Further, examination of pre-treatment trends is also customary to bolster the credibility of the parallel trends assumption, without which the difference-in-differences estimator is biased. In short, this assumption requires that the potential outcomes of the treatment group, had it not received the treatment, would have followed the same trend as potential outcomes in the control group (i.e., the outcomes would have remained parallel post-treatment). Due to the fundamental problem of causal inference, this assumption cannot be tested. However, if the treatment and control group maintained parallel trends pre-treatment, we can reasonably assume that they would have maintained parallel trends also post treatment, had the treatment not occurred.

The quasi-experimental setting of the UESD already increases the likelihood that pre-treatment trends are parallel. Nevertheless, I also assess this formally by conducting placebo difference-in-differences estimations using each previous wave as a treatment cut-off, keeping the real treatment and control groups constant. Figure 3 and Figure 4 illustrate the results of these placebo estimations for party identification and vote intention, respectively. For both outcomes, the confidence intervals include zero in all cases except when Partygate occurs (and thereafter, but this is not an issue). Vote intention comes close to showing a significant placebo estimate in Wave 20, but the confidence interval ranges includes zero. Therefore, there is reason to believe that, had Partygate not occurred, the difference in party identification and vote intention between the treatment and control group would have remained constant. Any observed treatment effect is thus due to Partyagte.

Figure 3: Placebo difference-in-differences on party identification.

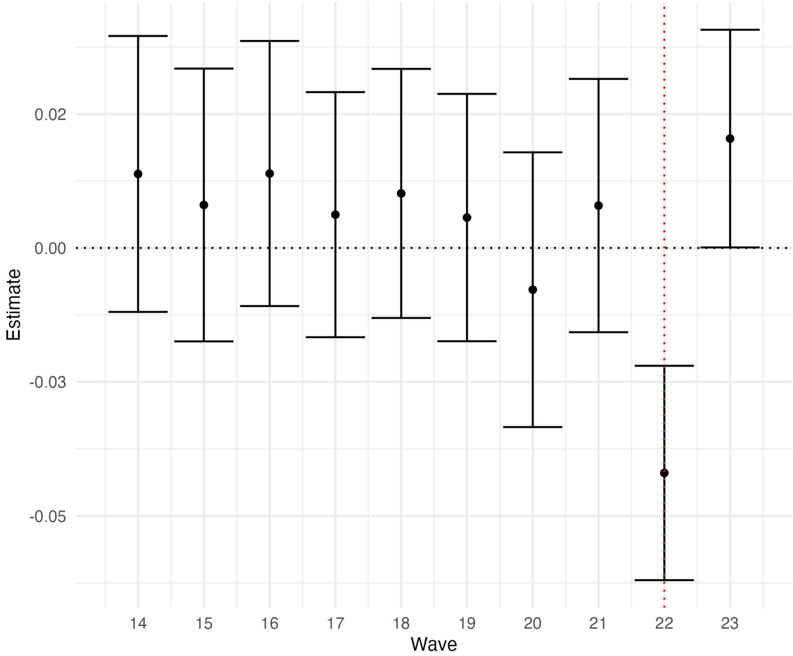
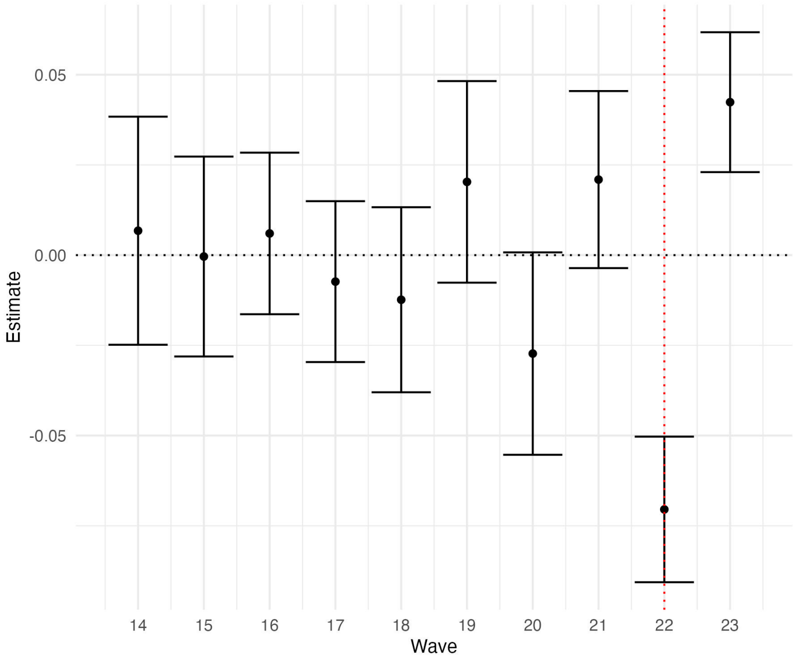


Figure 4: Placebo difference-in-differences on vote intention.



4.2 Testing the hypotheses

Having provided evidence in favor of the identifying assumptions of the estimators, I now test the hypotheses. H1, which predicts a decline in identification with the Conservative Party caused by Partygate, is tested with a naive difference-in-means estimator, a difference-in-means estimator that controls for imbalanced covariates (gender, ethnicity, and education), and finally with a difference-in-differences estimator. Table 1 summarizes the findings. All three estimators show that Partygate caused a statistically significant decline in identification with the Conservative Party, thus confirming H1. However, the coefficients are substantively small: the largest one – estimated using a difference-in-means without controls – shows a decline of 0.054 in Conservative identification. The smallest coefficient shows that Partygate reduced the share of Conservative identifiers by 0.027, while the difference-in-differences analysis estimates a 0.042 decline. In standard deviation units, the difference-in-differences estimate shows an effect of -0.008.

Table 1: ITT of Partygate on identification with the Conservative Party

	Diff-in-means		Diff-in-diff
	(1)	(2)	(3)
Treated	-0.054^{***}	-0.027^{***}	-0.012
	(0.007)	(0.008)	(0.008)
Post			-0.004
			(0.004)
Treated*Post			-0.042^{***}
			(0.010)
Constant	0.311^{***}	0.371^{***}	0.315^{***}
	(0.003)	(0.014)	(0.003)
Covariates	No	Yes	No
Observations	27,782	19,044	58,063
R^2	0.002	0.040	0.001
Adjusted R^2	0.002	0.038	0.001
Residual Std. Error	0.457 (df = 27780)	0.451 (df = 19007)	0.461 (df = 58059)
F Statistic	65.115^{***} (df = 1; 27780)	21.997^{***} (df = 36; 19007)	26.831^{***} (df = 3; 58059)
Note:			*p<0.1; **p<0.05; ***p<0.01

Turning to electoral support for the Conservatives, H2 proposes that Partygate caused a decline in this variable too. The results illustrated in Table 2 confirm this hypothesis. Estimation using a difference-in-means, difference-in-means with covariates (again, gender, ethnicity, and education as they are imbalanced), and difference-in-differences estimators produces a negative and statistically significant ITT. The magnitude of the decline in intention to support the Conservative Party in the next General Election ranges between 0.081 and 0.062, with the difference-in-differences estimate placed in-between at 0.072. After standardizing the difference-in-differences coefficient, the magnitude of the decline in electoral support for the Conservative Party is of 0.013 standard deviation units.

Table 2: ITT of Partygate on support for the Conservative Party

	Diff-in-means		Diff-in-diff
	(1)	(2)	(3)
Treated	-0.081^{***}	-0.062^{***}	-0.010
	(0.007)	(0.008)	(0.008)
Post			-0.084^{***}
			(0.004)
Treated*Post			-0.072^{***}
			(0.011)
Constant	0.282^{***}	0.373^{***}	0.366^{***}
	(0.003)	(0.014)	(0.003)
Covariates	No	Yes	No
Observations	27,287	18,688	57,040
R^2	0.006	0.037	0.014
Adjusted R^2	0.006	0.035	0.014
Residual Std. Error	0.440 (df = 27285)	0.437 (df = 18651)	0.462 (df = 57036)
F Statistic	156.619^{***} (df = 1; 27285)	19.813^{***} (df = 36; 18651)	269.260^{***} (df = 3; 57036)

Note: *p<0.1; **p<0.05; ***p<0.01

Taken together, the effects of Partygate on Conservative party identification and vote intention also corroborate H3, given that the decline in vote intention is substantively larger than the decline in party identification. Comparing the standardized coefficient, we see that the decline in party identification amounts to 0.008 standard deviation units, while the decline in support is 0.013. The effect is larger for vote intention across all estimated coefficients.

I now focus on the H4, which predicts a decline in party identification strength among Conservative identifiers caused by Partygate. As outlined above, I test this hypothesis by repeating the estimation procedure for H1, but only among the subset of Conservative identifiers. Table 3 shows that this hypothesis does not find empirical support in the data. Both difference-in-means coefficients – with an without covariates – are statistically insignificant with large standard errors. Similarly, the ITT estimated through difference-in-differences fails to reach conventional statistical significance levels, although it comes closer than the others with a p-value smaller than 0.10.

A potential explanation for this Null finding is that those who de-identified from the Conservative party were only weak "new" (e.g., post-2019) identifiers with uncrystallized attachment to the party. In other words, the absence of effect on party identification strength suggests that those who remained Conservative after Partygate may exhibit higher stability in the strength of their partisan attachment, which in turn prevents it from changing due to the treatment. However, it is important to stress again that the treatment, as operationalized in this paper, is weak. It is not implausible, especially given statistical significance at the 0.10 alpha level, that even those who remained Conservative in December 2021 are affected by Partygate later on, when the full extent of the government's breaches became apparent.

Table 3: ITT of Partygate on strength of identification with the Conservative Party.

	Diff-in-means		Diff-in-diff
	(1)	(2)	(3)
Treated	0.007	0.019	-0.041^{*}
	(0.018)	(0.022)	(0.022)
Post			0.075^{***}
			(0.011)
Treated*Post			0.049^{*}
			(0.029)
Constant	2.196^{***}	2.119^{***}	2.122^{***}
	(0.008)	(0.033)	(0.007)
Covariates	No	Yes	No
Observations	8,095	5,630	17,473
R^2	0.00002	0.013	0.004
Adjusted R^2	-0.0001	0.007	0.004
Residual Std. Error	0.641 (df = 8093)	0.637 (df = 5595)	0.645 (df = 17469)
F Statistic	0.158 (df = 1; 8093)	2.235^{***} (df = 34; 5595)	23.840^{***} (df = 3; 17469)

Note: *p<0.1; **p<0.05; ***p<0.01

5 Robustness checks

I conduct several additional analyses to gauge the robustness of these results. First, I re-estimate the difference-in-differences coefficients both for party identification and vote intention using a logistic regression specification. The results, reported in full in Appendix E, confirm that there is a negative and statistically significant change in both outcome variables, thus providing further evidence in favor of H1 and H2. Again, the decline in vote intention is larger than the decline in party identification, which corroborates H3.

Appendix F reports the results of placebo difference-in-differences analyses on dependent variable unrelated to the event. I choose authoritarian-libertarian values, which are measured through a 5-item battery, because one of the items provides a further "sanity check" for my estimates. The first four items – young people's respect for British tradition, support for the death penalty, teaching children the value of authority, and censorship of film and magazines – are all unaffected by the treatment; this is expected because these items capture values that should not be affected by a political scandal such as Partygate. However, the fifth item measures support for harsher sentences for people who break the law. As one might expect, this variable is positively affected by the treatment: being exposed to the government's law-breaking behavior plausibly caused individuals to support tougher sentences. The results are presented in full in Appendix F and overall confirm that Partygate had an effect only on variables that are related to the event, thus producing evidence in favor of the exclusion restriction.

Finally, I analyze the frequency of item non-response in the outcome variables across the treatment and control group to determine whether there are non-random patterns of variable-specific attrition. Such violations would threaten the assumption of ignorability because they would introduce systematic differences across treatment conditions. I find some evidence of non-randomness in item non-response mostly according to ethnicity, as shown in Appendix G. This may limit the validity of these findings for specific ethnic groups which seem to have modified their non-response patterns in vote intention and party identification due to Partygate.

6 Discussion and conclusion

In this paper, I employed an Unexpected Event during Survey Design to estimate the causal impact of Partygate on identification with and support for the British Conservative Party. This is possible due to the fact that when news of the scandal first broke out among the public on 7 December 2021, Wave 22 of the British Election Study Internet Panel was being collected. This created a natural experiment whereby individuals who completed the survey "before" Partygate are as-if randomly assigned to a control condition and those who completed the survey "after" Partygate are as-if randomly assigned to a treatment condition.

The empirical analyses, which use both difference-in-means and difference-in-differences estimators, found that Partygate caused a decrease in both identification with and support for the Conservative Party, which confirms theoretical expectation. Similarly, as predicted by the hypotheses, the magnitude of the decrease is larger for vote intention than party identification. Contrary to expectations, however, I find no effect of Partygate on party identification strength among Conservative identifiers.

Two considerations are necessary to understand these results – in particular those concerning party identification – in the context of wider literature on partisanship. First, party identification is generally thought to be among the most stable variables (see Richard Johnston, 2006, for a reviews). Second, the treatment employed in this study is arguably quite weak. If we do not consider the nature and timing of Partygate, both of these considerations would point toward finding no effect of the scandal on party identification. This is even more true if we take into account evidence of increasing affective polarization in Britain (Marchal and Watson, 2019) and direction goals when forming opinions on political wrongdoing (Wagner, Tarlov, and Vivyan, 2014).

The fact that, despite all of this, party identification was affected by the scandals can be interpreted in two ways. On the one hand, it is possible that party identification declined because it has decreased in stability; this interpretation corroborates notions of increased political volatility in Britain (Fieldhouse, J. Green, Evans, Mellon, Prosser, Schmitt, et al., 2019). Another interpretation of this result is that Partygate constituted one of the major shocks that can move even stable social identities (D. Green, Palmquist, and Schickler, 2002). This is entirely plausible, given the heightened social tensions associated with the Covid-19 pandemic and the arguably widespread sense of betrayal by the government felt among the public (Catterall, 2022). I cannot adjudicate between these explanations here, but the weakness of the treatment utilized may offer some tentative evidence for the former mechanism.

Turning to the literature on political scandals, the findings of this paper constitute evidence in favor of the claim that scandals do have consequences on political behavior. Further, they tentatively corroborate evidence of the role of perceived hypocrisy in fostering harsher punishment of politicians implicated in scandals (Bhatti, Hansen, and Leth Olsen, 2013; McDermott, Schwartz, and Vallejo, 2015). Partygate was characterized by high levels of hypocrisy from the incumbent Conservative government, which enforced social distancing tightly among the public but failed to behave according to the same rules.

Importantly, this paper presents some limitations. First, it remains unclear if this preliminary negative impact of Partygate on vote intention actually contributed to the electoral demise of the Conservative Party, which culminated in their historic defeat in the 2024 General Election (Onward, 2024). In order to assess this claim, one would have needed to evaluate whether Partygate results in persistently lower support for the Conservative Party. This is beyond the scope of the current paper and may be difficult to achieve regardless, given that as the scandal quickly gained prominence in British politics, everyone is "treated" after Wave 22 of the panel survey.

Second, the generalizabilty of these findings may be limited. Partygate was in many ways a unique political event, especially because it took place in the context of a global pandemic. The anger felt by voters toward Boris Johnson and his government was deeply personal because social distancing rules prevented people from visiting friends, attending funerals, celebrating birthday, and so on (Catterall, 2022). In short, Partygate is likely in the upper echelon of emotional intensity and ability to elicit outrage when it comes to political scandals.

Finally, a qualification should be made regarding the magnitude of the coefficients. Although Partygate did cause de-identification from the Conservative Party and led to fewer people intending to support it at the polls, the coefficient sizes estimated here remain small. Given the weakness of the treatment, however, I argue that this should not cause too much worry. In fact, as stated above, the estimated ITTs are perhaps best interpreted as lower bounds of the cumulative effects of Partygate – the overall effect of the scandal is likely larger.

7 Appendix

The Online Appendix and all materials to reproduce the empirical analyses are available in my GitHub repository: https://github.com/benegiocoli/partygate.

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