

Voting at the Dining Table: How Mail-in Ballots Impact Couples' Political Behavior ^{*}

Thomas Taylor[†]

This version: October 9, 2026

[Link to latest version](#)

Abstract. This paper studies how mail voting reshapes the political behavior of couples in the United States. Leveraging differences in implementation across states, I study its effects throughout the political decision-making process, from take-up to views and actions, both across and within election cycles. Partners overwhelmingly adopt mail voting jointly, reshaping the household's political life: exploiting the varying length of mail voting periods across states, I show that couple members report significantly more political interactions during such periods, while singles do not. I then document three direct downstream consequences. The introduction of mail voting is associated with a leftward shift among married men, in both partisan ideology and policy views. This effect is primarily concentrated among independent and left-leaning men, while no effects are uncovered among Republicans. Using administrative data on the content and origin of actual ballots, I show that mail voting is associated with significant excess pairs of identical ballots within precincts, suggesting important coordination between partners. Finally, mail-voting periods are associated with a 1.8% increase in the daily number of reported violent incidents between partners, highlighting that increased observability can result in higher within-couple conflict.

JEL codes: D13, D72, D83, J12

Keywords: Mail voting, Voting technology, Couples, Political preferences, Mutual influence

^{*}I thank Mathieu Couttenier, Thomas F. Crossley, Francesco Drago, Sophie Hatte, Guido Imbens, Giovanni Mastrobuoni, Andrea Mattozzi, Matthew Mitchell, Elisa Mougin, Pietro Panizza, Vincent Pons, Jade Ponsard, Alessandro Tarozzi, Giulio Zanella, as well as conference and seminar participants at the European University Institute, ENS de Lyon, the 2nd ENS-Bologna workshop, 5th EUI Alumni Conference in Economics, 4th PhD and Post-Doc workshop in Naples, 18th UniTo-Collegio Carlo Alberto PhD workshop, 9th Economics and Politics Workshop at Sciences Po Grenoble, the 21st Doctorissimes at Paris I and the 2026 EEA-ESEM congress for helpful comments. Support from an EUI Early Stage Research grant is gratefully acknowledged.

[†]European University Institute, thomas.taylor@eui.eu

1 Introduction

Voting is widely considered a private act. Its secrecy, affirmed in many national constitutions and international conventions,¹ stands as a cornerstone of modern democracies. In practice, it has long rested on the fact that ballots are cast on election day, at the polling station. To foster participation, many countries have progressively relaxed this arrangement, allowing citizens to vote in their own time and place. Foremost among methods to do so lies the possibility of voting by mail. Yet, while polling stations are organized to guarantee that each voter marks their ballot in private, no such assurance exists once voting takes place outside of them. This potential loss of secrecy is especially salient in contexts where daily life is shared with others, first and foremost within couples.

By relocating the act of voting into the home, mail ballots alter the conditions under which couples vote along multiple margins. They create repeated occasions for partners to engage with the election together – when requesting, completing, and returning their ballots – and make each partner’s participation and choices potentially observable to the other. Moreover, they permit each of these steps to be at least partially carried out by one partner on behalf of the other. Under perfect assortative mating along political and ideological lines, these changes would be of little consequence. However, sorting is imperfect ([Easton and Holbein, 2021](#)). In three out of ten American married couples, partners do not share a party affiliation ([Hersh and Ghitza, 2018](#)), and disagreement on specific issues extends well beyond politics ([Ashraf et al., 2014](#); [D’Acunto et al., 2021](#)). Hence, the arrival of the ballot at home has the potential to reshape how partners engage with politics and with each other’s views.

This paper studies how mail voting changes the political behavior of American couples. Specifically, it documents the take-up of this technology and its impact on couples’ political interactions, before investigating three downstream consequences: political opinions, vote choices and within-couple conflict.

While postal voting is not specific to the American context,² the United States provides a particularly well-suited setting to address this question. First, availability and usage of postal ballots are widespread among American voters. In the 2024 general election, 76% of the voting-age population had the option to cast a ballot by mail without having to provide a reason, and close to a third of voters did so.³ Second, the autonomy conferred on states to regulate the conduct of general elections creates significant variation in the adoption and extent of mail voting availability, both across and within electoral cycles.

Starting in the 1970s, many American states gradually allowed all registered voters to cast ballots by mail, without needing to provide a reason for it, a regime known as no-excuse mail voting.⁴ At first a rare possibility, with only four states allowing it in 1990, it has been steadily

¹Such examples are numerous. The Universal Declaration of Human Rights for instance states that elections shall be “held by secret vote” (Article 21), the Italian constitution declares that “the vote is personal and equal, free and secret” (Article 48), the French constitution provides that suffrage shall “always be universal, equal and secret” (Article 3).

²Some other Western democracies allow for any voter to cast their ballot by mail in any election, such as Canada, Germany, Switzerland or the United Kingdom. Many more countries allow specific segments of the population to vote by mail (the elderly, citizens living abroad, etc.). See [Barrat et al. \(2023\)](#) for a review.

³Author’s computation based on CPS Voting and Registration supplement data.

⁴The action of voting by mail can be referred to as mail-in voting, mail voting, postal voting or absentee voting. Throughout this paper, these terms are used interchangeably. Vote-by-mail naturally refers also to the action of voting by mail, but is mainly used when referring only to systems where elections are run primarily by mail.

spreading since. In the 2024 general election, mail voting was an option for all registered voters in 36 states and the District of Columbia. I use this variation in two ways. Across electoral cycles, I exploit the staggered introduction of no-excuse mail voting to estimate the medium-run effects of mail voting availability. Some states later went further, implementing permanent absentee lists or all-mail elections, but my primary focus is on the first reform allowing any voter who wishes to do so to vote by mail. Within electoral cycles, I exploit the fact that each state sets its own mail-voting period. Ballots are sent out between 18 and 60 days before election day depending on the state, so that at the same distance from the election, ballots are already in voters' hands in some states and not yet in others. This lets me compare individuals, or administrative units, at the same point in the campaign with different access to mail ballots.

I begin by documenting the joint nature of mail voting take-up within married or cohabiting different-sex couples, on which I focus throughout this paper. I first leverage data from the Current Population Survey (CPS) Voting and Registration supplement from 1994 to 2024 and the staggered introduction of no-excuse mail voting across states in a difference-in-differences framework to show that partners adopt this voting method jointly. Among couples in which both partners vote, the share in which both do so by mail increases by 10.7 percentage points six elections after repealing the need to provide an excuse, against 2.2 percentage points for the share in which only one partner does so. From administrative data on the timing of mail ballot requests and returns in the state of Georgia, I document descriptively that both partners in over 70% of two-mail-voter households request their ballots on the same day, and over 80% return them on the same day. The latter figure is 17 percentage points higher than the corresponding share among early in-person voters, despite the longer window for returning mail ballots. Partners therefore not only adopt mail voting jointly but also act on it at the same time.

By physically entering the household, mail ballots have the potential to generate political interactions, particularly for jointly adopting couples. I therefore investigate whether mail ballots reshape political discussions in the run-up to elections. To do so, I exploit the fact that each state sends out its ballots on its own calendar, so that at any given distance from election day, ballots are already in circulation in some states and not yet in others. Using individual survey data from the 2000, 2004, and 2008 National Annenberg Election Survey (NAES), and making use of the timing of interviews and respondents' marital status, I compare the reported frequency of political discussions of respondents interviewed on the same day of the same campaign, across states where ballots have and have not yet gone out. Since campaigns might intensify once mail voting opens, estimating the differential response of individuals in couples relative to singles allows me to isolate the household component of mail voting's effect from other campaign factors.

Married or cohabiting respondents report significantly higher levels of political discussions with family and friends during the mail-voting period, while singles appear to be unaffected. The effect corresponds to an average increase of 0.12 days over the past week during which one discussed politics. Scaling by the share of couples with at least one mail voter in no-excuse states, this magnitude corresponds to an increase of 0.57 days over the past week, or 2.7 days over the full average mail voting period, assuming the effect is confined to households that receive a ballot. On the other hand, no effect on couple members' political discussions at work or online is observed, consistent with an effect that operates within the household. Decomposing this effect across intensity levels shows that mail voting periods appear to have little to no effect at the extensive

margin but instead increase political discussions of couples at all higher intensity levels. This highlights that postal voting affects not only already highly interested citizens but crucially also more moderately invested individuals.

Such increased interactions can, in turn, have important consequences for individual attitudes and behavior. Interpersonal exchanges, even of a brief and one-off nature, play an important role in shaping civic and political opinions (Bond et al., 2012; Broockman and Kalla, 2016; Pons, 2018). Moreover, these interactions can happen at the exact moment of voting, making disagreement between partners visible in a way voting at the polling booth never allows. This paper studies three consequences of such interactions: whether partners' opinions converge, whether this translates into ballot alignment, and whether disagreement spills over into conflict.

I examine how mail voting affects partisan identification and policy views using cross-sectional individual data since 2000 from the National Annenberg Election Survey (NAES) and the Cooperative Election Study (formerly Cooperative Congressional Election Study, CCES). Since such opinions are typically slow moving, I focus on changes across electoral cycles, leveraging again the staggered introduction of no-excuse mail voting in a dynamic difference-in-differences setting. If exchanges between couple members lead to some form of persuasion, one would expect expressed opinions to converge toward one another. The existing gender gap in partisan views in the United States implies that, on aggregate, such movements would push men leftward and women rightward (Edlund and Pande, 2002). Moreover, if an effect of mail voting on opinions goes through intra-household dynamics, analogous estimates for single respondents should yield no significant effects.

The introduction of no-excuse mail voting shifts married or cohabiting men leftward, in terms of both partisan self-identification and policy views, in the medium run. When respondents place themselves on a seven-point scale ranging from Strong Democrat to Strong Republican, this gradual effect amounts to an average leftward movement of 0.15 points, or 0.068 standard deviations, over the eight cycles (sixteen years) following introduction. In contrast, results document no impact on married or cohabiting women. Moreover, no such patterns are uncovered among singles, consistent with an effect that runs through the household. This shift is not uniform across the ideological distribution. Binary indicators based on this scale and which capture each party-strength reveal that this effect is concentrated among Democrats and Independents. The likelihood of identifying as a Republican, whether strongly or weakly, appears to be unaffected. Making use of Tausanovitch and Warshaw (2013)'s measure of ideological preferences, I find similar patterns when looking at policy views. Quantile regression estimates confirm the absence of effects on more conservative segments of the population. Moreover, such estimates document a U-shaped pattern on the more progressive half of the distribution, with the leftward shift strongest around the 30th percentile and gradually fading both at lower percentiles (more progressive) and toward the median (more conservative). These effects are consistent with men of moderate or already left-leaning ideology moving towards their partner. To make sense of this gender asymmetry, I document two further patterns. While campaign interest among women in couples rises significantly once mail ballots are sent out, it is men's political knowledge – measured by the ability to identify elected officials – that reacts positively to postal voting in the medium run. Taken together, these patterns are consistent with women taking the political initiative in couples that respond, with less engaged men learning from – and moving toward – their partner.

Previous work finds no effect of mail voting on partisan vote shares (Thompson et al., 2020; Barber and Holbein, 2020; Yoder et al., 2021). That the attitudinal effects are concentrated among already left-leaning men aligns with these results. Nonetheless, mail-voting-driven mutual influence may still extend to vote choices, especially when departing from the highly partisan races which constitute the focus of existing studies and electoral surveys. I therefore turn to local offices and ballot measures, which display significantly lower levels of partisanship (Kuriwaki, 2026), using cast vote records (CVRs) – transcriptions of every ballot cast – from two San Francisco elections. American ballots ask voters to decide many contests at once, which creates large numbers of possible choice combinations for each ballot. Leveraging the fact that CVRs record such choices jointly, I study how the distribution of identical ballot cards varies between mail and in-person ballots. Naturally, CVRs are fully anonymous and carry no individual information, making it impossible to link partners. I address this by asking whether two ballots with an identical and otherwise unique pattern of choices are cast from the same precinct more often than chance predicts and comparing this excess co-location between ballots cast by mail and in person. As these pairs constitute the rarest combinations of choices, their location in the same precinct, when not due to chance, comes from coordinated action or correlation of preferences. Since spatial correlation of preferences concerns both in-person and mail ballots and couples can align before going to the booth, differencing-out the estimated excess co-location across modes thus isolates the additional coordination enabled by mail voting. I estimate the net excess of co-located mail ballots to represent 15.5% of all unique identical pairs of ballot cards cast by mail in the 2019 municipal election and 24.7% in the 2020 presidential primary. While they should not be interpreted as the causal impact of mail voting availability, these figures point to substantial within-household coordination of mail ballots.

By bringing to light potential non-alignments between partners, whether revealed through increased interactions or direct voting observability, mail ballots might have consequences beyond political considerations. In particular, such non-alignments could lead to disagreements and act as a trigger for within-couple conflict. In the final section of this paper, I therefore study whether mail voting periods are associated with increased conflict between partners by focusing on the incidence of intimate partner violence. To do so, I use data on the universe of violent incidents between partners reported to law enforcement agencies in 28 states over the 2007-2016 period. From the roughly 3 million corresponding incidents, I construct a balanced panel at the county $\times$ day-level spanning the whole period and counting the daily number of such incidents. For identification, I leverage the varying length of mail voting periods across states and years, and the fact that the data covers non-electoral years. Importantly, the fixed holding of elections on the Tuesday-after-the-first-Monday of November allows me to also index dates in non-electoral years to their distance from what would be *Election Day*. Through a restrictive set of fixed effects, I estimate the within-county-year increase in reported intimate partner violence on days when postal voting is open, relative to counties and days where it is not, at the same distance from Election Day. Results show that mail voting periods are associated with a 1.2% increase in the daily likelihood of at least one incident occurring and a 1.8% increase in the total number of events at the county-day level. This amounts to roughly 500 additional incidents per election across the sample states. Increases are fully driven by incidents occurring at home and incidents involving a victim aged 40 to 59 see the highest surge. These results indicate that the mail-voting

period is one of heightened conflict within households, and suggest that the loss of ballot secrecy could enable pressure between partners.

Related literature. This paper provides, to the best of my knowledge, the first quantitative analysis of the impact of mail voting on couples’ joint political behavior. As such, it contributes to several strands of literature.

This paper first contributes to the literature on voting rules and technology. A large body of work shows that the rules governing the act of voting shape participation and, through it, political outcomes: registration requirements (Braconnier et al., 2017), identification laws (Cantoni and Pons, 2021), the accessibility of polling places (Cantoni, 2020; Alipour and Lindlacher, 2025), or compulsory voting (Hoffman et al., 2017).⁵ Within this literature, convenience voting – and mail voting in particular – has been considered almost exclusively as a change in the cost of participating, with effects measured on turnout or parties’ vote shares (Gerber et al., 2013; Barber and Holbein, 2020; Thompson et al., 2020; Yoder et al., 2021) and the composition of the electorate (Berinsky, 2005; Hodler et al., 2015).⁶ By showing that mail ballots eliminate the social rewards of being seen at the polls, through removing the community’s ability to observe who turns out, seminal work by Funk (2010) documents the importance of one’s voting environment for political behavior in the context of Switzerland. In the same context, recent work by Marchese and Schönenberger (2026) shows that this has important implications for gender participation as mail voting alleviates the negative pressure exerted by communities with regressive gender norms. In these papers, mail voting reduces the observability of one’s turnout decision by the community. My analysis offers a complementary view by documenting that the home is not a neutral environment either. Specifically, this paper shows that the increased within-household observability permitted by mail voting collectivizes the act of voting for cohabiting partners. In turn, this reshapes not only whether they vote, but also the political life of the household as a whole and, ultimately, the opinions each partner holds and expresses.

Second, this paper contributes to the understanding of the formation and transmission of political preferences within the household. Couples display, on average, strongly correlated political views (Alford et al., 2011). As assortative mating on partisanship and attitudes is an important component of couple formation, this is largely due to sorting (Alford et al., 2011; Huber and Malhotra, 2017; Iyengar et al., 2018). Yet, while showing that the correlation of attitudes within couple is high, these same studies highlight it is not perfect (Easton and Holbein, 2021), as further shown by the fact that members of three in ten married American couples have different political affiliations (Hersh and Ghitza, 2018). This imperfect sorting opens the door to significant influence and persuasion taking place, a driver of the within-couple correlation we know much less about. So far, causal evidence on partners’ influence on politics focuses extensively on turnout: men’s turnout increases when getting married (Stoker and Jennings, 1995; Bellettini et al., 2023), household members go to the polls together (Fieldhouse and Cutts, 2012; Bhatti et al., 2020) and stop going when one partner dies (Hobbs et al., 2014). By showing that a get-out-the-vote campaign spreads to the second household member, work by Nickerson (2008) pins down the role of couple socialization in joint political behavior adoption. By showing that

⁵See Cantoni et al. (2025) for a review.

⁶See Gronke et al. (2008) and Gronke et al. (2025) for extensive reviews of the effects of convenience and mail voting.

couples overwhelmingly adopt postal voting jointly and that this increases political discussions, this paper further contributes to this literature. While some longitudinal evidence showing that partners’ party support move together over time exists (Zuckerman et al., 2005; Stoker and Jennings, 2005), the endogeneity of political views makes it difficult to isolate the channel through which such shift operates. This paper contributes to the literature by exploiting a reform that changes the *exposure* to one another’s views, while leaving couple composition unchanged. As such, it shows that differences of opinion cohabit within couples and that they can significantly converge when confronted. This finding is consistent with evidence that even brief face-to-face conversations can shift vote choices and attitudes (Broockman and Kalla, 2016; Pons, 2018; Kalla and Broockman, 2020; Braghieri et al., 2026).

Moreover, the direction of the convergence is of significant importance. American women have consistently identified and voted to the left of men since the 1980s (Inglehart and Norris, 2000; Box-Steffensmeier et al., 2004; Gethin et al., 2022). Important work ties this gap to marriage itself, through its decline (Edlund and Pande, 2002) or the division of labor it organizes (Iversen and Rosenbluth, 2006). I show that the political interactions within couples partially close this gap, and that, in the context of mail voting, it is men who adjust. That men’s views shift with exposure to women’s interests and perspectives is well documented outside the couple. The birth of a daughter translates into more progressive positions on gender-related matters among legislators and judges (Washington, 2008; Glynn and Sen, 2015), into greater support for left-wing parties (Oswald and Powdthavee, 2010) and into improved female earnings and employment in the firms fathers manage (Ronchi and Smith, 2026). Similar shifts also follow exposure beyond the family structure (Dahl et al., 2021). This paper therefore documents a consistent but missing pattern: increased exposure to one’s partner’s views, through the political life of the household, can generate a progressive shift in men’s opinions. That this movement is concentrated among men of moderate views and limited political involvement, and likely responds to the political initiative of their partners, echoes Bellettini et al. (2023), who show that marriage raises men’s turnout toward women’s higher pre-marital levels. The existence of such an asymmetry further relates to work on gender imbalances in within-household information flows and belief updating documented in non-political contexts (Ashraf, 2009; Fehr et al., 2024; Delavande et al., 2025; Ashraf et al., 2026).

Finally, this paper contributes to the literature on conflict within couples and intimate partner violence. By documenting that mail-voting periods are associated with increased violence between partners, it relates to two strands of this literature. The first ties violence to short-lived stress and emotional cues: unexpected sport and betting losses (Card and Dahl, 2011; Arnesen and Matsuzawa, 2026), an additional day between paychecks (Masi and Santantonio, 2026), or forced proximity between partners (Leslie and Wilson, 2020).⁷ This paper shows that political disagreement, made recurrent and observable by the arrival of ballots at home, constitutes such a trigger. The second strand studies violence as an instrument of control within the couple (Bloch and Rao, 2002; Adams et al., 2024): with ballots completed at home, violence or its threat could serve to control the partner’s vote.⁸ While this paper cannot separate the two channels,

⁷These short-lived triggers complement a broader literature on economic hardship and domestic violence (Schneider et al., 2016; Arenas-Arroyo et al., 2021).

⁸The scope for such control, as for the escalation of cues into violence, is further studied by a rich literature on the role of within-couple bargaining power in driving intimate partner violence (Aizer, 2010; Anderberg et al.,

it highlights that electoral institutions carry consequences for household conflict that evaluations centered on electoral outcomes fail to capture.

The rest of this paper is organized as follows. Section 2 provides institutional background on postal voting in the United States. Section 3 describes the data. Section 4 documents patterns of mail voting take-up by couples, while section 5 studies how their political interactions are affected. Section 6 presents the consequences of mail voting on ideological views. Section 7 investigates whether mail ballots collectivize the act of voting. Section 8 studies how mail voting affects within-couple conflict. Section 9 concludes.

2 Setting and Conceptual framework

In this section, I provide further details on the gradual expansion of mail voting across the United States and its technical implementation. I then develop a conceptual framework presenting how mail voting can affect couples’ political behaviors.

2.1 Voting by mail in the United States

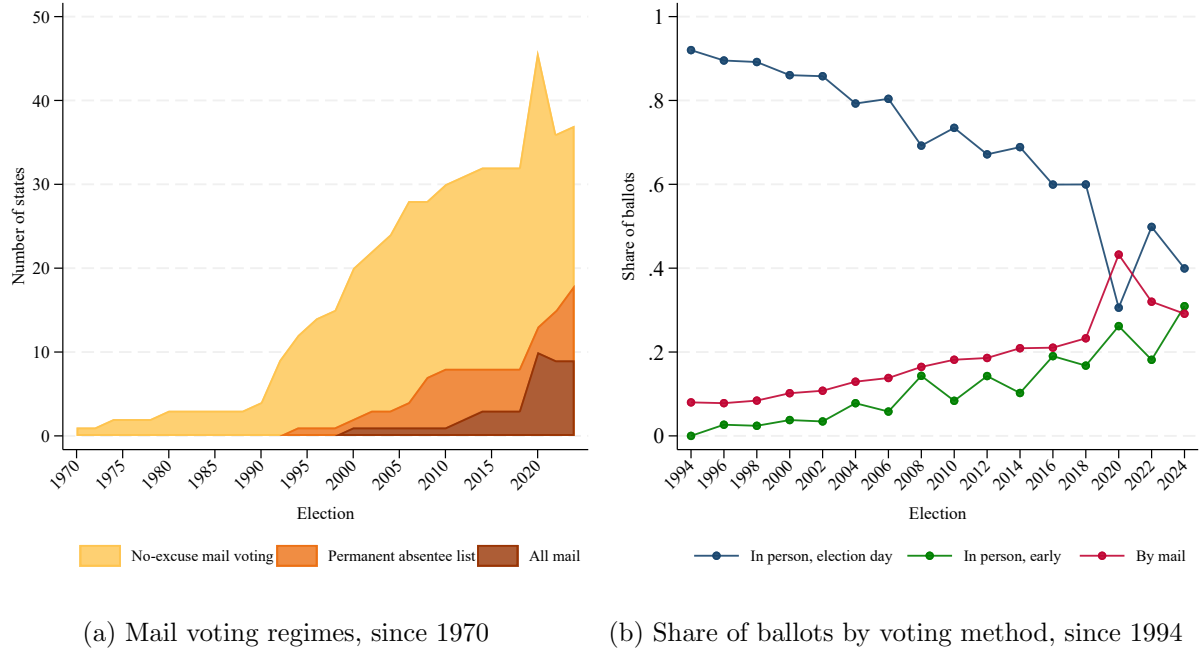
Mail-in voting first emerged in the United States during the Civil War, allowing soldiers serving far from home to cast ballots during elections (Inbody, 2016). In the first decades of the 20th century, most states enacted some absentee voting laws to accommodate voters unable to be physically present at the polls, such as those traveling for work-related reasons or ill. The introduction of “no-excuse” absentee voting in Idaho in 1970 represented a significant shift, allowing for the first time voters to request a mail-in ballot without providing a specific reason.

Since then, and especially since the early 1990s, other states gradually adopted similar policies, broadening access to mail-in voting. While the standard no-excuse mail voting regime requires voters to request a mail ballot in every election cycle, some states further facilitated postal voting. Permanent absentee lists were introduced, meaning that voters could opt into receiving a mail ballot in every future election without needing to make new requests. Following Oregon, which became in 2000 the first to do so, some states began running general elections primarily by mail, sending mail ballots to all registered voters.

In the 2024 general election, no-excuse mail voting was an option in 28 states and a further eight, as well as the District of Columbia, ran elections primarily by mail. Of the 28 states offering no-excuse mail voting, nine offered all voters the possibility to join a permanent absentee list. This expansion over the past decades reflects a broader trend toward enhancing voter convenience and accessibility (Gronke et al., 2008). The COVID-19 pandemic contributed to accelerating this shift, with many states temporarily expanding access to mail-in voting to ensure public safety during the 2020 elections. That year, 43% of voters cast their ballots by mail. This figure fell back to 29% in the 2024 election, in the midst of strong partisan conflict over its use. Nonetheless, this number remains above pre-pandemic levels, underscoring the significance of this voting method (U.S. Census Bureau, 2021; MIT Voting Lab, 2024). Importantly, the introduction of no-excuse mail voting has been shown not to be driven by partisan considerations (Biggers and Hanmer, 2015), a finding confirmed when analyzing adoption timing using a proportional hazards model (2016; Bhalotra et al., 2021).

(Cox, 1972) (Figure A1). Figure 1 presents descriptive evidence of the penetration of mail voting, documenting the evolution of the number of states allowing some form of no-excuse mail voting since 1970 and the share of ballots cast by mode since 1994.

Figure 1: Evolution of mail voting availability and take-up



Notes: This figure describes the expansion of mail voting in US general elections. Subfigure (a) reports the number of states offering some form of no-excuse mail voting since 1970, differentiating by mail voting regimes. Source: author's collection, based on state legislation. Subfigure (b) presents the share of ballots cast by voting mode in each general election since 1994. Source: Current Population Survey, Voting and Registration Supplement.

In practice, ballots are typically mailed to voters by election officials several weeks before Election Day to allow sufficient time for voters to receive, complete, and return them. As presented in Figure A3, timelines vary from one state to another, with ballots being sent out between 18 and 60 days ahead of Election Day. Voters may request a mail-in ballot through their local election office, typically either through an online portal, by email or in person.⁹ Voters registered on a permanent absentee list or voting in states running elections primarily by mail are sent ballots automatically, without any action on their part. Once the ballot is completed, it can be returned through multiple methods, including postal mail, official ballot drop boxes, or in-person submission at designated election offices. Deadlines for returning ballots also vary by state, with most requiring ballots to be received by Election Day, though some allow ballots postmarked by Election Day to be counted if they arrive within a specific period. Election officials verify the validity of returned ballots through processes such as signature matching or other voter identification methods before counting them.

The expansion of mail voting has been accompanied by recurring debate. While research finds modest turnout gains and no systematic partisan advantage (Thompson et al., 2020; Barber and Holbein, 2020; Yoder et al., 2021), public controversy has centered on electoral integrity and the

⁹Possible channels to request a mail ballot vary across states. A number of online platforms such as vote.org or absentee.vote411.org now also facilitate the process by providing voters with pre-filled applications when online portals are not available.

risk that ballots cast outside the supervision of the polling station could be manipulated (Gronke et al., 2008). The data offer little support for these concerns as switches to mail-based elections produce no detectable increase in reported fraud (Auerbach and Pierson, 2021). Instead, the challenges that persist are rather administrative: reliance on postal delivery, the risk of ballots being lost in transit or the resource intensity of signature verification (Meredith et al., 2024).

2.2 Conceptual framework

The possibility of voting by mail does not simply change the cost of participation. Instead, it changes when and where voting takes place. By voting ahead of Election Day, early and mail voters forgo information provided by the rest of the campaign and cast their votes in a different informational setting than had they waited. While voting at the booth, whether on election day or early, guarantees the individuality and secrecy of one’s vote, this is not necessarily the case once mail ballots relocate the act of voting to one’s home. While all voters are affected by the changing informational environment when deciding to vote early, the loss of individuality is particularly salient in contexts of multi-member households, and primarily so within the marital couple.

The administrative process to vote by mail can by itself alter how couples approach Election Day. Ballots need to be requested (in most states), before being filled and returned. As the cost of preparing a second request or carrying a second envelope to the mailbox is marginal, these steps exhibit economies of scale at the household level, pushing towards joint adoption of mail voting at the couple level.¹⁰ I document this empirically in section 4.

Such steps mechanically create occasions for discussions regarding the upcoming election. Both the request and receipt of ballots can mark an early start to individuals’ following of – or involvement in – the campaign. Completing them can become a domestic activity, performed at the same time and place, opening the door to increased political interactions. Moreover, even when individuals decide not to adopt mail voting, its mere availability raises the question of how to vote, potentially yielding increased political interactions in the absence of take-up. As such, investigating the intent-to-treat effect of mail voting availability appears particularly relevant.

Nonetheless, whether interactions actually increase, and whether such an increase has consequences, is not trivial. First, mail voting could merely shift political discussions forward in time rather than increasing them. In that case, interactions which would have occurred on or closer to Election Day are just spread out across time, with no effect on their total amount. Second, even if interactions do increase, pre-existing homophily in beliefs and policy views could limit their consequences (Alford et al., 2011; Iyengar et al., 2018). In other words, if couples already agree on everything, additional discussion will have no downstream consequences. While the first point remains an empirical question, which I investigate in section 5, the literature on assortative mating along political lines documents substantial misalignment within couples (Hersh and Ghitza, 2018; Easton and Holbein, 2021), leaving most with margins along which to converge.

More frequent discussions should, at a minimum, increase partners’ exposure to and knowledge of each other’s views. Several mechanisms can turn this exposure into convergence. First, through more frequent discussions, partners may inform and convince each other, generating gen-

¹⁰This is not fully specific to mail voting, with other voting modes themselves displaying lower costs when performed together. One can think for instance of driving together to the precinct when voting on Election Day.

uine updating of preferences and within-couple convergence (Zuckerman et al., 2007). Second, expressed disagreement is costly. A partner may report, or vote, closer to their spouse’s view than to their own for fear of conflict or disapproval. This mechanism strongly relates to the large literature on the role of social pressure in driving political decisions (Gerber et al., 2008; DellaVigna et al., 2017). Moreover, it extends the work of Funk (2010) who shows how mail ballots alleviate the pressure of being seen at the polls. While mail voting makes voting invisible to the community, it now makes participation and choices observable at home, potentially transferring the source of social pressure into the household. Third, a partner with little interest in politics can remain involved at almost no cost by following their spouse’s advice, potentially even letting them fill in the ballot. At the extreme, this potential delegation can even be non-consensual, with the loss of secrecy opening the door to coercion between couple members.

All of these mechanisms should compress within-couple gaps, whether in expressed positions or voting. None of them, however, needs to operate symmetrically. Imbalances within the couple can direct the convergence to one of the preexisting positions. In that case, couple averages should shift toward the partner with the higher cost of deviation: the more politically interested partner, the one with stronger convictions, or the one with greater bargaining power within the household.

These mechanisms differ, however, in the margin on which they operate and in their timing. Persuasion moves underlying views, and should therefore build up gradually with repeated interactions. Accommodation, delegation, and coercion operate on the expression of views and are more likely to resemble *one-shot* movements. These differences guide the empirical strategy: section 6 documents how views evolve across elections while section 7 and section 8 investigate the role of mail voting in coordinating choices on the ballot and within-couple conflict, respectively, within election campaigns.

3 Data

In this section, I describe the main data sources used throughout my analysis. I first present the collection and coding of mail voting rules in place across states. I then describe the surveys used to measure political behaviors before presenting administrative ballot-level data used to complement the electoral analysis. Finally, I describe the data on violent incidents between partners.

Mail voting rules. Central to this paper is the mapping of current and historical mail voting rules in place across states. Throughout my analysis, I am interested in the effect of postal voting on behaviors both across election cycles and within electoral campaigns, requiring two key pieces of information. First, from electoral legislation, I collect the date at which each state introduced no-excuse mail voting and its availability in every subsequent election. Table A1 presents each act of legislation which introduced mail voting rules. When studying consequences of postal voting across cycles, my main variable of interest is an indicator variable capturing the availability of no-excuse mail voting in the upcoming election, that is, the possibility for any voter who wishes to do so to cast a ballot by mail. Under the “basic” no-excuse absentee by mail regime, voters need to request a ballot and do so anew in every election cycle, which imposes a burden and might slow down its take-up. Two further mail regimes loosen this constraint: permanent absentee

lists, which allow voters to request a mail ballot once and then have it automatically sent in all subsequent elections; and all-mail elections, in which every registered voter receives a ballot automatically. I collect the introduction dates of these further extensions in a similar manner. Given the ordered nature of such rules, I also define a scaled treatment variable ranging from 0 (excuse required) to 3 (all-mail elections) used for the complementary analysis of the effects of all mail-voting expansions. I present in Figure 1 the overall evolution of states in each voting regime since 1970 while Figure A2 further describes the mail voting rule in place for every state-election pair since 1994, the earliest election in my analysis sample. That year, no-excuse mail voting was available in 12 states, a number which grew to 46 (including the District of Columbia) in the 2020 election, before several states that adopted in the wake of the COVID-19 pandemic reverted to requiring an excuse. In the 2024 election, 36 states and the District of Columbia offered some form of postal voting which did not require an excuse.

Beyond consequences across electoral cycles, I am also interested in studying how mail voting reshapes the way one approaches a given election. To do so, I leverage the fact that the dates at which mail ballots are sent out are set autonomously by states. At a given number of days prior to election day, voters in some states thus already have their ballot in hand while those in other states do not yet. I also collect this information from electoral legislation. Specifically, I record the earliest date explicitly stated by the electoral code at which ballots may be sent out to voters.¹¹ Figure A3 presents the distribution of identified mail voting periods for all state-election pairs between 2000 and 2016, a period which covers the two analysis samples on which I perform within-campaign analyses. These periods vary widely across states, ranging in the 2016 election from 18 days in Washington to 60 in North Carolina, with a median of 30.

Political behavior from survey data. Throughout this paper, I rely extensively on survey data to measure the political behavior and opinions of individuals in general elections. First, I make use of the Voting and Registration supplement of the Current Population Survey (CPS) in order to study the take-up of mail voting by couples between 1994 and 2024.¹² The CPS is one of the largest American household surveys, interviewing around 60,000 households every month, with a focus on labor force participation. In most months, a supplement is run covering other topics. The Voting and Registration supplement is run along the November wave of the CPS in even years, immediately following each general election. It asks respondents about their registration status, whether they voted in the last election and, importantly for my analysis, the method by which they voted. Compared to electoral surveys, discussed below, the CPS presents the crucial advantage of surveying all members of each household. This allows me to observe the turnout and vote mode choice of both members of each couple, and hence to study their joint

¹¹Each state’s legislation characterizes in its own wording the date at which ballots are sent out and mail voting opens, generating slight noise in the collection of this information. For instance this period is described in 2016 in Florida as “The supervisor of elections shall mail a vote-by-mail ballot to each absent qualified voter [...] between the 35th and 28th days before the [...] general election.” [FL Stat. §101.62 (2016)]; in Idaho as “shall be sent not later than forty-five (45) days before that election” [ID Code §34-1003 (2016)]; in Ohio “ballots shall be printed and ready for use on the thirty-fifth day before the day of the election” [Ohio Rev Code §3509.01 (2016)]; in Wyoming “the clerk [...] shall, not more than forty-five (45) days prior to the election, distribute to the applicant [...] the absentee ballot” [WY Stat §22-9-107 (2016)]. While in some states a range is given, in others only the earliest or the latest date is made explicit. For consistency, I code the earliest date which the legislation specifies. In such examples, the corresponding coding is therefore respectively 35, 45, 35 and 45.

¹² www.nber.org/research/data/current-population-survey-cps-supplements-voting-and-registration

voting behavior. I impose two sample restrictions. First, in many states requiring an excuse to vote by mail, being aged 65 or older is in itself a valid reason. I therefore restrict all analyses to individuals aged under 65. Second, so that the sample of singles is not itself exposed to within-household spillovers, I exclude single respondents living in households where a couple is present. I observe the turnout decisions of a resulting sample of 876,386 individuals aged 18 to 64, either in cohabiting relationships or single. At the couple level, this represents a sample of 278,053 different-sex cohabiting couples. I associate to each CPS respondent the mail voting rule in place in its state of residence at the time of the survey. Table A2 presents descriptive statistics both at the individual and couple level. No-excuse mail voting was available in the latest election to 50% of the respondents. On average, 61% of them report having voted in the corresponding general election and 9% of all respondents did so by mail. Conditional on having voted, 15% of in-sample voters thus cast their ballot by mail. Couples display a high level of turnout congruence, with 59% of couples seeing both partners vote and 25% where neither voted. As a result, just 16% of couples see only one member turn out. Mail voting take-up follows a similar pattern: conditional on both partners voting, joint adoption is more than three times as frequent as adoption by one partner alone.

Second, I use two sets of electoral surveys to measure political behaviors and opinions. In particular, I collect data from two large American electoral surveys: the Cooperative Election Study (formerly Cooperative Congressional Election Study, CCES)¹³ and the National Annenberg Election Survey (NAES).¹⁴ Running since 2006, the CCES is a yearly survey, collecting information on policy views and voting behaviors in general elections. In election years, on which I focus in this analysis, sample size ranges from 30,000 to 60,000 respondents and consists of a pre-election interview measuring, among others, respondents' views, partisanship, and political knowledge, and a post-election follow-up asking respondents about their vote choices. Most pre-election interviews take place during the month prior to the election and all of them within 50 days of the election. Similarly, the NAES surveys 50,000 to 80,000 individuals in three presidential election years (2000, 2004 and 2008)¹⁵ and collects very similar data. Unlike the CCES, the NAES consists of a single interview and typically runs throughout the whole election year, with the survey significantly scaling up in the last four months. Both surveys are individual-level cross-sections, only interviewing one household member. As such, I cannot directly study behavior of both couple members, as the CPS permits, but instead investigate how respondents' behaviors vary across marital status.

I make use of these surveys in two ways. The NAES' coverage of multiple months of electoral campaign and the comparability of respondents surveyed at any point in time allow me to study how mail ballots affect behaviors *within* electoral cycles. Specifically, I am interested in two types of outcomes capturing the frequency of individuals' political interactions and their interest in politics. I first make use of the number of days in the past week on which the respondent report having discussed politics with either family and friends (asked in 2000, 2004 and 2008), at work (2000 and 2008), or online (2004). Second, I leverage two scales measuring respondents' following of government affairs and interest in the current electoral campaign in the run-up to the

¹³tischcollege.tufts.edu/research-faculty/research-centers/cooperative-election-study

¹⁴www.annenbergpublicpolicycenter.org/data-access/

¹⁵The 2008 NAES survey consists of both a telephone and an internet study, differing both in design and content. I only make use of the telephone study.

2000 and 2004 elections. Based on the state of residence and date of interview, I code for each respondent whether mail voting was already open when surveyed. Table A3 provides descriptive statistics of all variables used in this analysis on the set of individuals interviewed at most 120 days prior to an election. This restriction, which defines my main sample, focuses the analysis on the period by which all presidential primaries have concluded. Before that point, the staggered primary calendar places states at very different moments of the political process, and the NAES itself scales up substantially only thereafter. The mail-voting period was open at the time of the interview for 10% of observations. On average, people report having discussed politics with their family and friends just over 3 days during the past week. This figure is much lower for discussions at work (1.49 days) and even more so online (0.50). When interviewed, the average respondent reported following government affairs “some of the time” and being “somewhat interested” in the ongoing campaign.

Second, I leverage both the NAES and CCES surveys to study how partisanship, policy views and political knowledge evolve across electoral cycles. Together, they allow me to measure a common set of political attitudes from roughly 430,000 individuals surveyed around general elections from 2000 to 2024, with the exception of 2002.¹⁶ My key outcomes of interest are partisan identification, measured on a 7-point scale,¹⁷ and policy views. While both surveys ask a wide array of policy questions on a broad range of topics, consistently measuring how these evolve across cycles is challenging as questions often differ across survey waves, both in topic and framing. To circumvent this limitation, I leverage work by [Tausanovitch and Warshaw \(2013\)](#). Through an item-response theory model, they estimate individual policy ideal points – ideological positioning on a one-dimensional progressive-conservative scale – for all CCES waves up until 2021 and the 2004 NAES. This is made possible by the inclusion of a so-called “super-survey” including all policy questions asked in previous NAES and CCES surveys when themselves fielding a module of the 2010 CCES wave. The overlapping set of questions this generates makes the computed measure comparable across survey waves. While this “super-survey” is not publicly shared as part of the CCES distribution, the authors make available the resulting ideal points ([Warshaw and Tausanovitch, 2022](#)).¹⁸ I therefore make use of this measure, which aggregates individual policy preferences into a one-dimensional scale where lower values correspond to more left-wing views and higher values to more right-wing views. Additionally, the CCES allows me to measure the level of political knowledge of respondents, specifically regarding their elected officials. I leverage four variables, available from 2006 to 2024, capturing whether respondents know the party holding the majority in their state’s lower and upper chambers, the party of their representative in Congress, and the party of their governor. Table A4 describes variables used throughout cross-cycle analyses. The average respondents leans slightly Democratic. Just over 60% of respondents know which party controls their state’s lower and upper chambers, and a similar share knows the party of their representative in Congress. Knowledge of the governor’s party is notably higher, with close to 80% answering correctly.

¹⁶I restrict the NAES observations to those surveyed in the last 50 days before the election so that included respondents were interviewed in a similar timeframe as CCES respondents.

¹⁷Specifically, the scale ranges as: 1=Strong Democrat, 2=Not strong Democrat, 3=Lean Democrat, 4=Independent, 5=Lean Republican, 6=Not strong Republican, 7=Strong Republican

¹⁸dataverse.harvard.edu/dataverse/americanideologyproject

Administrative ballot information. I complement the analysis of survey data with administrative ballot-level information. Specifically, I first make use of the universe of 2024 absentee-ballot applications in the states of Georgia and North Carolina in order to document descriptively whether couples request and return their ballots at similar times. In both states, voters who wish to vote by mail need to request a ballot. Since such records constitute public information, I observe the universe of requests for the 2024 general election. I am interested in ballots cast by couple members. To identify them, I start by matching requestors based on their shared last names and addresses. When more than two voters match within an address, I first apply one of two restrictions. In the absence of information on age, as in Georgia, I restrict the sample to two-individual matches, which represent 81% of all identified multi-voter households. When instead such information is available, as in North Carolina, I restrict observations to the two oldest in the household and further exclude pairs whose age difference is larger than 15 years, in order to mitigate the risk of capturing single parents and their children. In both cases, I then restrict the sample to different-sex pairs. Although the procedure is imperfect, the pairs it identifies correspond to couples in their vast majority. While other couples are missed, notably those who do not share a surname, this data is used solely as a descriptive tool whose informativeness survives their potential exclusion. Crucially, these files record both the date on which each ballot was requested and the date on which it was returned. For each pair, I compute the number of days separating the two partners' requests, and separating their returns, to assess whether couples take these steps together. Table A5 provides summary statistics.

Second, I use Cast Vote Records (CVRs) from the November 2019 municipal election and the March 2020 presidential primary in San Francisco County (California) to study whether mail voting is associated with higher ballot similarity.¹⁹ Now increasingly released by local American election offices (Kuriwaki et al., 2024), CVRs provide information on the actual content of ballots. Specifically, they record the full combination of choices across all contests in a given election, which is made possible by the processing of ballots by optical scanners. As such, they are the direct transcription of actual ballots. Crucially, CVRs provided by the San Francisco administration record the mode through which each given ballot was cast, distinguishing those cast by mail from in-person ballots. The presence of this information explains the focus on San Francisco over other jurisdictions making CVRs available. The restriction to the 2019 municipal election and 2020 presidential primary is explained by the fact that such data is not available beforehand and California began mailing ballots to all voters in the November 2020 general election, significantly shrinking and selecting the group of in-person voters after that.²⁰ While a majority of ballots were at the time already cast by mail, a significant share was still cast in person (28.8% in 2019, 35.1% in 2020 presidential primaries)²¹ allowing me to compare voting patterns across voting methods. As in most American elections, voters decide on a large number of offices and ballot measures simultaneously, some of which allowing ranked choices, creating a very large number of possible ballot patterns. Specifically, 14 contests were faced by most voters in the 2019 elections, while voters in the 2020 presidential primaries faced between 12 and 14 contests. Importantly,

¹⁹Results and data are accessible on the following links. 2019: sfelections.org/results/20191105w/detail.html; 2020: sfelections.org/results/20200303w/detail.html

²⁰Some small counties could run elections entirely by mail prior to that, due to their size, but this was not the case for San Francisco County.

²¹Source: Department of Elections reports for [2019 municipal](#) and [2020 presidential primary](#) elections.

San Francisco CVRs do not record full ballots but instead process each card separately, making ballot *cards* my object of interest. Figure E2 presents an example ballot from the 2019 election illustrating both the variety of contests and the division of the ballot into separate cards. In the 2019 election, the splitting of contests across cards could vary across districts, while in the 2020 primary the first card carried party-specific contests (presidential candidate and the Democratic County Central Committee) and the second carried 12 contests common to all voters. Party-specific cards in the 2020 primary are excluded from the analysis. Naturally, CVRs are fully anonymous and do not provide information on voters, making couples unidentifiable. To circumvent this, I leverage the rarity of voting patterns and their geographic spread across the city. In particular, I focus on ballot-card patterns that occur exactly twice and ask whether the two copies originate from the same precinct more often than chance would predict, and whether this excess differs between mail and in-person ballots. Table A6 presents summary statistics. The average ballot card shares its exact pattern with 636 other cards across the city in 2019 and 204 in the 2020 primary. In 2019, 3% of ballot cards have exactly one duplicate, a figure which goes up to 8% in 2020. Both the fall in the average number of duplicates and the rise in the share of single-duplicate cards between the two elections are largely explained by the higher number of contests per card (12 in 2020, against only up to 8 in 2019).

Violent incidents. In the latter part of this paper, I investigate how mail voting affects conflict within couples. To do so, I collect official records of violent incidents between partners from 2007 to 2016, drawn from the FBI’s National Incident-Based Reporting System (NIBRS).²² The NIBRS is a voluntary program through which US law enforcement agencies report crime data. For each reported incident, it provides detailed information on the nature of the incident (date, time, type of event, location) and on involved parties (age, gender, race, relationship between them). I identify violent events within couples as records where the offender is identified as the current partner of the victim (spouse, common-law spouse, or boyfriend/girlfriend) and the nature of the incident is one of the following: simple assault, aggravated assault, intimidation, murder, sexual assault, and rape. The sample is restricted to 28 states providing data at the county level,²³ from which I construct a balanced panel measuring the daily count of such events for 1,499 counties from January 1, 2007 to December 31, 2016. The outcomes of interest are (i) an indicator equal to one if at least one incident was reported in a county on a given day, capturing the extensive margin, and (ii) the daily number of recorded incidents, capturing the intensive margin. Leveraging characteristics of victims and offenders, I also decompose the daily number of events by demographic characteristics (age group, race) and location. Table A7 provides descriptive statistics for this data. Every day, at least one violent incident between partners is reported to the police in one in five counties, and the average county records 0.5 incidents per day. In over 80% of cases, the victim is a woman and the offender is her male partner.

²²bjs.ojp.gov/national-incident-based-reporting-system-nibrs

²³Specifically, included states are: Arkansas, Colorado, Connecticut, Delaware, Idaho, Iowa, Kansas, Kentucky, Louisiana, Massachusetts, Michigan, Montana, New Hampshire, North Dakota, Ohio, Oklahoma, Oregon, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, Washington, West Virginia, Wisconsin.

4 Mail voting take-up by American couples

With close to one third of ballots being cast by mail in the 2024 election, mail voting has seen widespread adoption over the past decades. Yet, this figure alone does not fully describe its penetration within American households, nor its potential to reshape political interactions. In this section, I describe patterns of mail voting take-up among American voters, with a particular focus on the joint behavior of partners.

4.1 Joint adoption of the technology: Evidence from household surveys

Few data sources make it possible to investigate the participation and voting mode of both partners. To study take-up of mail voting, I thus leverage the Voting and Registration supplement of the Current Population Survey. As a supplement to the CPS, it covers all household members and records basic electoral behavior information such as registration, participation and voting method.

Estimation. In order to document how voters take up mail voting following its opening to all voters, I estimate the following staggered difference-in-differences specification on CPS respondents from 1994 to 2024:

$$Y_{i,t} = \sum_{\substack{h=-4, \\ h \neq -1}}^5 \theta_h \mathbf{1}[h = t - G_s] + \mathbf{X}'_{s,t} \gamma + \mathbf{Z}'_i \delta + \alpha_s + \lambda_t + \varepsilon_{i,t} \quad (1)$$

where the outcome variable $Y_{i,t}$ is an indicator equal to 1 if respondent i , living in state s , reports having voted by mail in the general election of year t , and 0 otherwise. G_s denotes the first general election year during which no-excuse mail voting was available in state s . $t - G_s$ thus denotes the number of elections since its introduction in state s . $\mathbf{X}_{st}$ captures the availability of other early but in-person voting rules, while $\mathbf{Z}_i$ is a vector of individual characteristics.²⁴ α_s and λ_t respectively denote state and election-year fixed effects. θ_h captures the set of coefficients of interest, associated with each event-time dummy. Specifically, they measure the average change in mail-voting take-up at any given event-time, relative to the last election prior to state s 's introduction of no-excuse mail voting. Standard errors are clustered at the state level.

Because of mail voting's staggered introduction, estimation of Equation 1 using two-way fixed effect estimators can yield biased and inconsistent estimates in the presence of heterogeneous treatment effects (De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021; Borusyak et al., 2024). To address this concern, I employ the estimator proposed by De Chaisemartin and d'Haultfoeuille (2026). Two characteristics make it particularly appropriate for my empirical setting. First, it allows treatment to be non-absorbing, which accommodates the temporary adoption by some states in 2020 and their subsequent reversals. Second, treatment does not need to be binary. While my main specifications focus on the first no-excuse mail voting rule, the subsequent introductions, in some states,

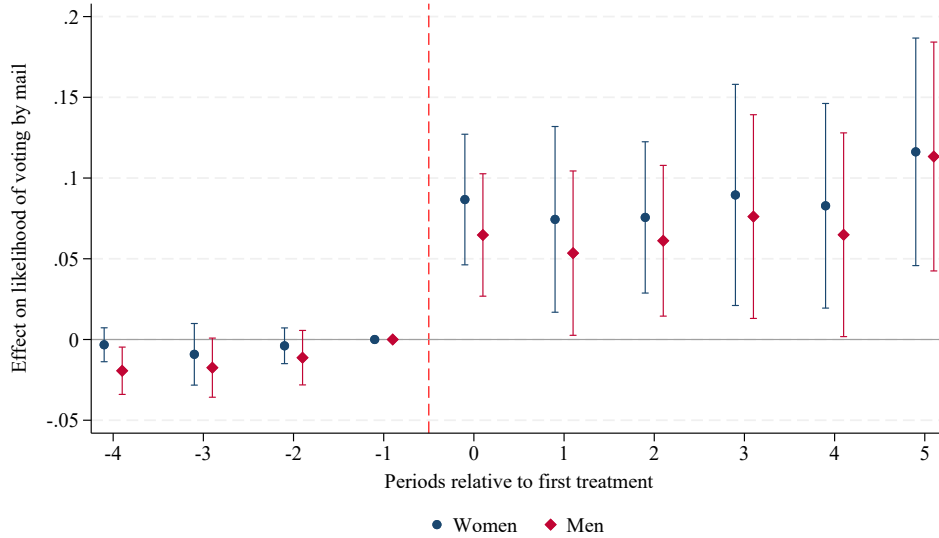
²⁴Individual characteristics included in vector $\mathbf{Z}_i$ are age, the squared value of age, race (as a four-category variable partitioned as *non-hispanic white*, *non-hispanic black*, *hispanic*, *other*), educational attainment (as four-category variable partitioned as: *less than high school*, *high school*, *some college or associate's degree*, *four-year college degree or more*) as well as indicator variables capturing the presence of children under 18 in the household and whether the respondent is employed.

of permanent absentee lists and all-mail elections create an ordered treatment measuring the accessibility of mail voting. I explore in Figure B5 the effects of such further expansions. In the baseline results presented in this section, I focus on the sample of voters in order to isolate the choice of how to vote from the decision of whether to vote. subsection B.2 presents analogous results including non-voters.

Individual take-up. Figure 2 reports the estimated effect of no-excuse mail voting availability on the likelihood of married or cohabiting voters of casting their ballot by mail. I observe a sharp and immediate increase in the likelihood of both female and male voters of voting by mail, with estimated coefficients of 8.6pp and 6.5pp, respectively, in the first election following the introduction. The estimated effect progresses to 11.6pp and 11.3pp by the sixth election cycle. On average, take-up is slightly larger among female married voters (average effect of +9.3pp) than males (+7.4pp).

I further report in Figure B2 results of the corresponding estimation for single voters. Interestingly, take-up among single female voters is very similar to that among married female voters (+9.2pp), while that of single male voters is notably lower (+4.6pp). This discrepancy hints at the role of joint adoption in driving male take-up.

Figure 2: Effects of no-excuse mail voting introduction on take-up by couples



Notes: This figure presents dynamic difference-in-differences estimates of the effect of no-excuse mail voting laws on mail voting take-up by married – or living as such – voters. Estimation is performed using the methodology of [De Chaisemartin and d'Haultfoeuille \(2026\)](#). The unit of observation is an individual aged 18 to 64 reporting having voted in the last November general election. The dependent variable is an indicator variable equal to 1 if the respondent declares having voted by mail. Estimation is performed separately for women (blue circular marker) and men (red diamond markers). All estimations control for race, age and its square, education, employment, the presence of an underage child at home, and the availability of in-person early voting. Standard errors are clustered at the state level. Bars denote 95% confidence intervals.

Adoption at the couple level. Having documented that mail voting sees a significant take-up among married voters, I ask whether such adoption occurs couple-wide or if instead partners tend to each adopt different voting methods, possibly at their own convenience. To do so, I aggregate the sample of CPS respondents at the couple level. Couples display a high degree of

congruence in how they vote. Figure B3 presents the joint distribution of voting modes within couples in sample, in state-elections where no-excuse mail voting was available. Conditional on the female partner’s mode, the male partner’s choice aligns closely: when she voted in person on Election Day, he did so too in 80% of cases; when she voted by mail, he voted by mail in 80% of cases; and when she voted early in person, he did so in 73% of cases. The congruence extends to participation itself: when she did not vote, he did not vote either in 77% of cases.

I confirm this pattern in a dynamic setting by estimating versions of Equation 1 at the couple level, where the outcomes are indicator variables respectively equal to 1 if zero, one, or two partners cast their ballots by mail. The estimation now includes a vector of couple-level characteristics measuring each partner’s age and its square, whether they have children under 18 in the household, attended college, are employed, and indicators for whether they are both non-hispanic black, both hispanic, both non-white of other races, or a mixed-race couple. Again, I focus in the baseline estimation on the sample of voters and thus restrict the sample to two-voter households.

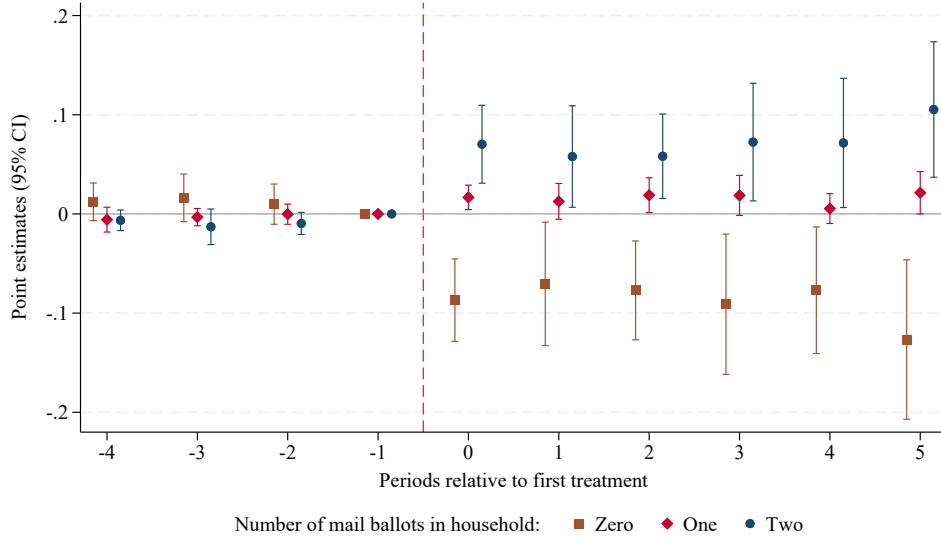
Figure 3 presents the corresponding estimates. Following the introduction of no-excuse mail voting, the likelihood that neither partner cast a ballot by mail significantly drops, by 9.3 percentage points on average over the six following elections. This decrease is almost fully driven by couples adopting jointly, as the likelihood of both couple members voting by mail increases by 7.6 percentage points on average. Adoption by one partner only is much more moderate, increasing by 1.7 percentage points, on average, over the period. This figure highlights the immediate and joint adoption of postal voting at the couple level. I further report in Figure B4 the corresponding figure including all couples, instead of only two-voter households. Beyond the similar pattern it presents, the higher rate of one-partner adoption in the full sample is concentrated in couples where only one partner votes – consistent with joint adoption being tied to joint participation.

Further mail voting expansions. In elections following the first introduction of no-excuse mail voting, multiple states further expanded its accessibility. While baseline legislation requires voters to request a mail ballot in every election, subsequent reforms relaxed this constraint through permanent absentee lists and all-mail elections. To explore whether the ease of access beyond the sheer possibility of voting by mail further expands take-up, I perform an analogous estimation using instead a four-level ordered treatment scale, defined as: 0=excuse required, 1=no-excuse mail voting, 2=permanent absentee list, 3=all-mail. The estimator proposed by [De Chaisemartin and d’Haultfoeuille \(2026\)](#) and used throughout this paper is particularly suited for this exercise, as it allows for non-binary discrete treatment without imposing equal effects for equal-sized changes. By comparing the observed treatment path to the counterfactual in which the state remains at its initial regime, it estimates the average effect of expansions in mail voting accessibility at each horizon. Figure B5 reports the corresponding couple-level estimates, and shows that expansions in mail voting accessibility further increase joint take-up by couples far beyond one-member adoption.

4.2 Voting in a shared time frame: Descriptive evidence from election ballots

Common adoption, however, can reflect other factors which correlate within couples – preferences, schedules on election day – rather than a genuinely collective activity. To move closer to the act

Figure 3: Effects of no-excuse mail voting on mail ballots cast by two-voter couples



Notes: This figure presents dynamic difference-in-differences estimates of the effect of no-excuse mail voting laws on the number of mail ballots cast among two-voter couples. Estimation is performed using the methodology of [De Chaisemartin and d'Haultfoeuille \(2026\)](#). The unit of observation is a couple in which both partners are aged 18 to 64 and report having voted in the last November general election. The dependent variable is an indicator variable equal to 1 if zero (brown square markers), one (red diamond markers) or two (blue circle markers) ballots were cast by mail. All estimations control for partners' race, age and their squares, education, employment, the presence of an underage child at home, and the availability of in-person early voting. Standard errors are clustered at the state level. Bars denote 95% confidence intervals.

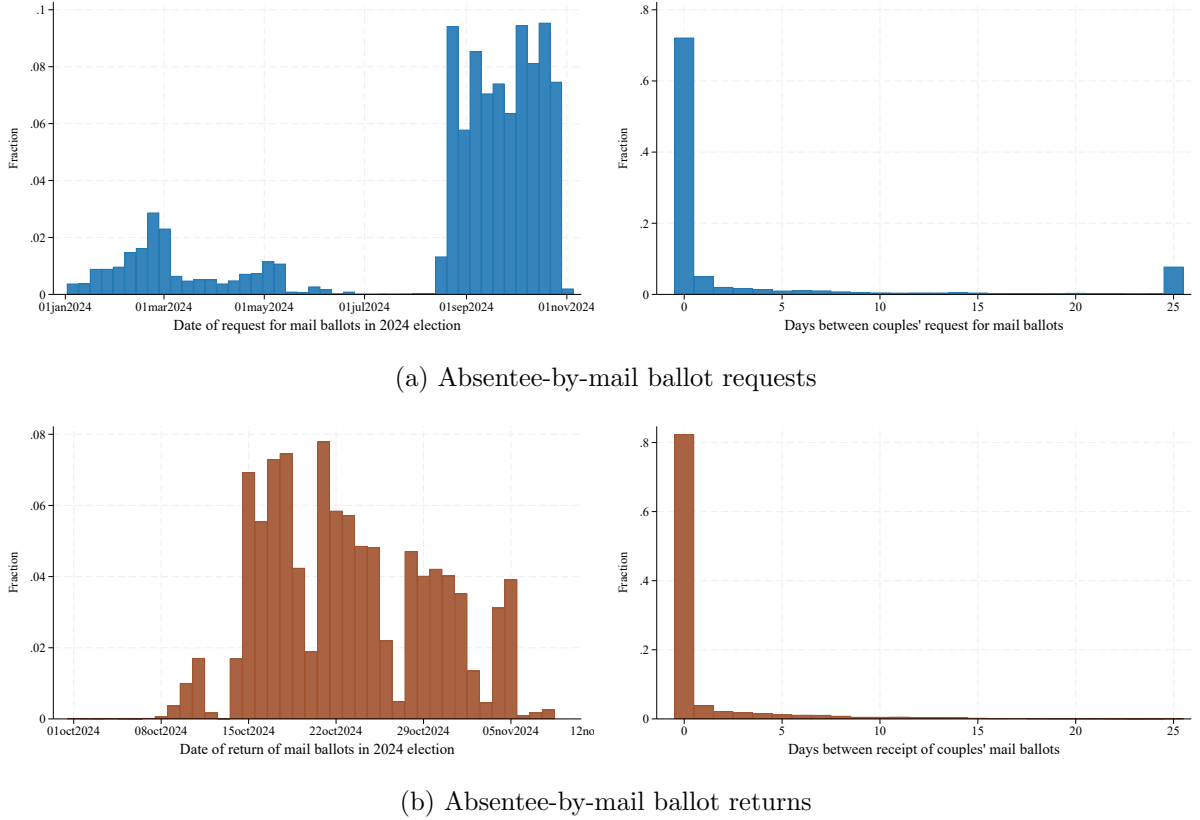
of voting itself, I use administrative records on all absentee ballots from the 2024 election in Georgia. This data reports the dates at which ballots were requested and returned, and whether this was done by mail or early in-person. As detailed in section 3, information on addresses and names allows me to identify households and likely couples.

To shed light on the collective nature of postal voting, I compute the distance between dates at which both identified couple members requested and returned their mail ballots. I report these figures in Figure 4. In addition, I show the distribution of all request and return dates. As shown in panel A, request dates are spread throughout the election year. A first batch of requests is received through the spring, before and just after primary elections, while they peak and plateau from mid-August until the week before the election. Despite this wide range of request dates, 72% of couples make their requests on the same date. In panel B, I show the corresponding pattern for return dates. Mail ballots are received by election officials starting on October 8 and flow in continuously until Election Day. Yet, when both couple members vote by mail, 82% of ballots are received on the same day. Figure B7 presents the analogous statistics for the state of North Carolina, which exhibits the same pattern.

I further report the corresponding figures for couples both opting for early in-person voting in Figure B6. For these voters, request and return coincide by construction with the day they appear at the polling station. While overall exhibiting a similar pattern, the share of couple members casting their ballots on the same day is only 62%, even though the shorter voting window makes coincidental same-day voting more likely.

Together, these figures highlight the likely joint nature of postal voting for partners. With over 7 in 10 couples requesting their postal ballots on the same day and over 8 in 10 returning

Figure 4: Timing of couples' mail ballots requests and returns during the 2024 general election in Georgia



Notes: This figure describes the timing of requests and returns of absentee ballots during the 2024 general election in the state of Georgia by identified couples. The unit of observation is either a mail ballot (left figures) or a pair of mail ballots identified as coming from a couple (right figures). Distances between partners' requests or returns are windorsized at 25 days.

them on the same day, it appears that, for many couples, all steps of the process are carried out together. Despite the high correlation between partners for all voting methods, comparison with early in-person ballots shows that such collective nature is strongest when voting by mail.

5 How mail voting impacts political interactions

By widening one's voting options and physically entering households, mail ballots have the potential to generate significant political interactions, particularly so for joint-adopting couples. This section attempts to quantify the extent to which mail voting reshapes political discussions in the run-up to elections.

Estimation. When measuring how mail voting availability affects political discussions in the run-up to elections, the natural intensification of campaigns as Election Day approaches can prove to be an important confounder. In order to address this, I leverage the fact that each state sets autonomously the date at which mail ballots are made available to its population. As described in Figure A3, mail voting periods range from 18 days in Washington to 60 days in North Carolina.

To measure political interactions, I make use of the 2000, 2004 and 2008 telephone NAES surveys. These cross-sectional studies present the advantage of sampling respondents through random telephone dialing throughout the corresponding presidential election years, making observations at any given date comparable. Crucially, they ask questions about political discussions with family and friends, at work or online. These questions, together with the important variation in mail voting periods, allow me to compare the political interactions of individuals in the same state prior to and during the mail voting period, as well as of individuals surveyed at the same distance from the general election across mail voting availability. The survey question closest to capturing couples' interactions asks only about discussions with "family and friends". To isolate as much as possible discussions with one's spouse, I further compare couples and singles. Specifically, I estimate the following two-way fixed effects model:

$$\begin{aligned} \# \text{ Discussed}_{i,t} = & \alpha + \beta_1 \text{Couple}_i + \beta_2 \text{Mail voting period}_{s,t} \\ & + \beta_3 \text{Couple}_i \times \text{Mail voting period}_{s,t} + X_i' \delta + \lambda_{s,y} + \gamma_{t,y} + \varepsilon_{i,t} \end{aligned} \quad (2)$$

where the outcome variable $\# \text{ Discussed}_{i,t}$ is equal to the number of days respondent i surveyed on date t reported having discussed politics in the past week with either "Family and friends", "At work" or "Online", each outcome being estimated separately. Couple_i is an indicator variable equal to 1 if respondent i is married or living as such. $\text{Mail voting period}_{s,t}$ is an indicator variable equal to 1 if mail voting is available on date t in state s . β_1 captures the baseline excess political discussions associated with being in a couple, outside of mail voting periods. β_2 measures the effect of mail voting being open on discussions for singles. β_3 measures the differential effect of mail voting availability for couples. Therefore, $\beta_2 + \beta_3$ quantifies the increased level of political discussion among couples explained by the availability of mail voting. X_i is a vector of individual characteristics measuring age, gender, educational attainment, race, full-time employment, the presence of children in the household, and monthly church attendance. The specification includes state $\times$ election-year fixed effects $\lambda_{s,y}$, which capture inherent state-level characteristics or local election specificities, as well as days-to-election $\times$ election-year fixed effects $\gamma_{t,y}$, which absorb daily conditions common to all states such as the general progression towards Election Day or nationwide campaign events or shocks. Standard errors are clustered at the state level.

Results. Table 1 presents the corresponding estimates. Column 1 reports coefficients for the baseline specification on the outcome of interest: discussions with family and friends. Outside of mail voting periods, respondents in relationships declare having discussed politics with family and friends almost half a day more than their single counterparts over the past week (+0.456). This figure, somewhat unsurprising, confirms that political discussions occur within the household. Turning to the main coefficients of interest, mail voting periods do not appear to be associated with increased political discussions for single respondents: the corresponding estimate is close to zero and not statistically significant (-0.020, p-value=0.721). On the other hand, such periods are associated with a highly significant increase in political discussions with family and friends for couple members, of 0.116 days in the past week. This effect is substantial, amounting to 25% of the excess discussions by couples compared to singles outside of mail voting periods. Scaling this effect by the share of couples receiving a mail ballot over the 2000-2008 period yields an increase

in political discussion among them of roughly 0.57 day in the past week, or 2.69 days over the full average mail voting period.²⁵ This back-of-the-envelope calculation provides an upper bound of the effect, as it need not be limited to voters actually taking up mail voting. Indeed, the sheer availability of mail voting can push voters to ask themselves, and discuss with each other, whether they should take it up or not.

Some demographic characteristics, such as age or having children, strongly correlate with being in a couple and can influence the take-up of postal voting. In order to measure the joint effect of marital status and mail voting net of such characteristics, I further estimate a specification which includes interactions between mail voting availability and all control variables. Obtained coefficients, reported in column 2, are very stable, supporting the finding that the impact of postal voting on political interactions runs primarily through the marital unit. One could also worry that the opening of postal voting reshapes states' local campaigns, thereby mechanically raising the political salience where mail ballots are available. To address this concern, I further run a specification including state $\times$ election linear time trends, which capture the natural progression of each state towards election date. I report the corresponding coefficients in column 3. Again, results are very stable to this specification, providing further support for the role of mail voting in shaping couples' interactions. Finally, one could worry that mail voting periods might increase political discussions of couples with everyone, making the uncovered effect on discussion independent of household-specific dynamics. I address this by leveraging questions in the NAES survey asking about discussions *At work* (asked in 2000 and 2008) and *Online* (2004). While mail voting periods are associated with significantly higher levels of political discussion at work for single respondents, this is not the case for couples (column 4). Furthermore, online interactions are unaffected, whether for singles or couples (column 5).

Together, these results provide strong evidence that postal voting acts as a catalyst for political interactions within couples.

Robustness checks. To ensure the robustness of this result, I further perform a series of additional checks. Figure C1 reports the distribution of coefficients associated with 1,000 randomizations of the allocation of mail voting periods across states. The obtained distribution centers around 0, while the coefficient associated with the true distribution sits on the right tail. The obtained randomization p-value is 0.051. I then perform a leave-one-out exercise dropping successively one state out of the estimation. This exercise allows me to ensure that no single state's respondents, campaign conditions, or events are driving the measured effect. Figure C2 reports the corresponding coefficients, all of which are very close to that obtained on the full sample. The main specification focuses on NAES respondents surveyed within 120 days of the election in order to avoid including presidential primaries, whose dates vary across states. To ensure that results are not specific to this time frame, I report in Table C1 outcomes on samples ranging from 90 to 150 days prior to the election. The uncovered effect of mail voting on couples' political interaction remains very stable in all specifications, ranging from 0.111 to 0.126. A final concern could be directed at the bundling of discussions with friends and family into one

²⁵Between the 2000 and 2008 general elections, at least one mail ballot was cast in 20.3% of couples with both partners under 65 in states in which no-excuse mail voting was available. The scaled coefficient is thus obtained as: $0.116/.203 = 0.57$. The average mail voting length in the 2000, 2004 and 2008 elections is 33 days. The corresponding computation thus is: $0.57 \times \frac{33}{7} = 2.69$.

Table 1: Mail voting periods and political discussions

Outcome: Days discussed politics with	Family and friends			At work	Online
	(1)	(2)	(3)	(4)	(5)
Mail-voting period	-0.020 (0.055)	-0.011 (0.055)	0.058 (0.062)	0.149** (0.070)	0.000 (0.051)
Couple	0.456*** (0.021)	0.457*** (0.022)	0.456*** (0.021)	0.026 (0.020)	-0.107*** (0.023)
Couple $\times$ Mail-voting period	0.136** (0.060)	0.127** (0.060)	0.129** (0.062)	-0.159** (0.065)	0.047 (0.054)
Observations	70975	70975	70975	53880	23976
Mean dep. var.	3.078	3.078	3.078	1.486	0.499
Individual controls	✓	✓	✓	✓	✓
State $\times$ year and Days-to-elec $\times$ year FEs	✓	✓	✓	✓	✓
Controls $\times$ Mail period		✓			
State-year linear trends			✓		
Mail-voting period + Couple $\times$ Mail-voting period	0.116	0.116	0.187	-0.010	0.047
p-value	0.028	0.022	0.004	0.882	0.181

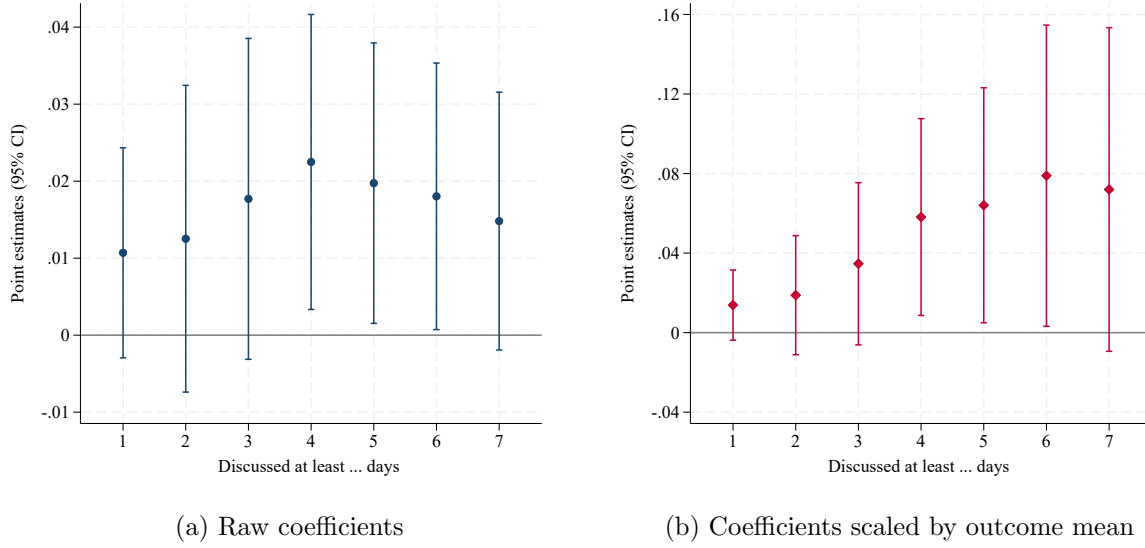
Notes: This table presents OLS estimates of the differential effect of mail voting on political discussions of couples and singles. The unit of observation is an individual. The dependent variable is equal to the number of days the respondent declares having discussed politics with family and friends (columns 1-3), at work (column 4, available only in 2000 and 2008) or online (column 5, available only in 2004). Mail-voting period is an indicator variable equal to 1 if mail ballots can have legally been sent out on that day. Couple is an indicator variable equal to 1 if the respondent is cohabiting with a partner. Individual controls include gender, race, age, education, full-time employment, the presence of an underage child at home and monthly church attendance. All estimations include state $\times$ year and days-to-Election $\times$ year fixed effects. Column 2 further includes the interaction between all controls and the indicator mail-voting period. Column 3 includes state $\times$ year daily linear trends. Standard errors are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

outcome. Although already strong evidence that the marital unit houses the core of this excess political discussion generated by mail voting, I attempt to provide further support for it. To do so, I make use of the three US waves of the Comparative National Election Project (CNEP),²⁶ which specifically ask about political discussions with one's spouse, as well as other interlocutors, during the 2012, 2016 and 2020 general election campaigns. While the small sample and static nature of this survey substantially limits its empirical usage in my setting, the interlocutor disambiguation make it possible to partially verify the patterns uncovered using NAES data. Details and estimation results are presented in Table C2. Despite limited precision due to sample size, this exercise confirms that mail voting availability is associated with higher frequency of discussions by married respondents with both their spouse and family, but at most marginally so with neighbors, friends, or co-workers.

Distributional heterogeneity. The uncovered average increase in political discussions generated by mail voting raises the question of where the distribution shifts. Does it mobilize individuals who were previously not discussing politics or, instead, already highly invested individuals? To answer this question, I create seven indicator variables based on the original NAES question, equal to 1 if the respondent reports having discussed politics with friends and family at least n days in the past week, n ranging from 1 to 7. I then re-estimate Equation 2 for each of these seven outcome variables. Figure 5 reports the estimated $\beta_2 + \beta_3$, corresponding to the increased political discussions of couples during mail voting periods. Panel A displays the raw coefficients while Panel B reports them scaled by the outcome mean. While mail voting appears to increase the likelihood of all discussion intensities, this effect is weakly significant for lower levels of dis-

²⁶ www.upf.edu/web/cnep

Figure 5: Mail voting periods and intensity of couples political discussions



Notes: This figure presents OLS estimates of the effect of mail voting on political discussions of couples. The unit of observation is an individual. The dependent variable is a binary indicator equal to 1 if the respondent declares having discussed politics with family and friends at least the corresponding number of days in the past week. Each coefficient is estimated through a separate regression with the corresponding dependent variable. Panel A reports the linear combination of coefficients $\text{mail-voting period} + \text{couple} \times \text{mail-voting period}$. Panel B scales the value reported in panel A by the mean of the corresponding dependent variable. Mail-voting period is an indicator variable equal to 1 if mail ballots can have legally been sent out on that day. Couple is an indicator variable equal to 1 if the respondent is cohabiting with partner. All estimations include race, age, education, full-time employment, the presence of an underage child at home and monthly church attendance. All estimations include state $\times$ year and days-to-Election $\times$ year fixed effects. Standard errors are clustered at the state level. Bars denote 95% confidence intervals.

cussions and of limited magnitude when scaled by the outcome mean. The associated increase in the likelihood of discussing politics at least once in the past week is 1.38% (p-value=0.126). This figure highlights that postal voting appears to have a limited impact on the extensive margin, with respondents reporting zero or very low levels of political discussion being marginally affected. This is consistent with the fact that such respondents are themselves less likely to vote, and thus somewhat mechanically less likely to see their behavior reshaped by a new voting technology. On the other hand, the magnitude and statistical significance of the effect grow substantially at higher intensities of discussions, reaching an increase of 7.89% (p-value=0.043) in the likelihood of reporting having discussed politics at least 6 days in the past week. This shows that mail voting has an important impact on already politically engaged individuals, further fueling interactions of both moderately and highly politicized couples.

6 Beyond discussions: mail ballots' impact on expressed opinions

By increasing political discussions, the availability of postal voting has the potential to foster exchanges of opinions between partners, in turn pushing couple members' positions towards each other. This section attempts to shed light on this by investigating how postal voting affects partisan identification and policy views of couple members.

6.1 Impact on opinions: partisanship and policy views

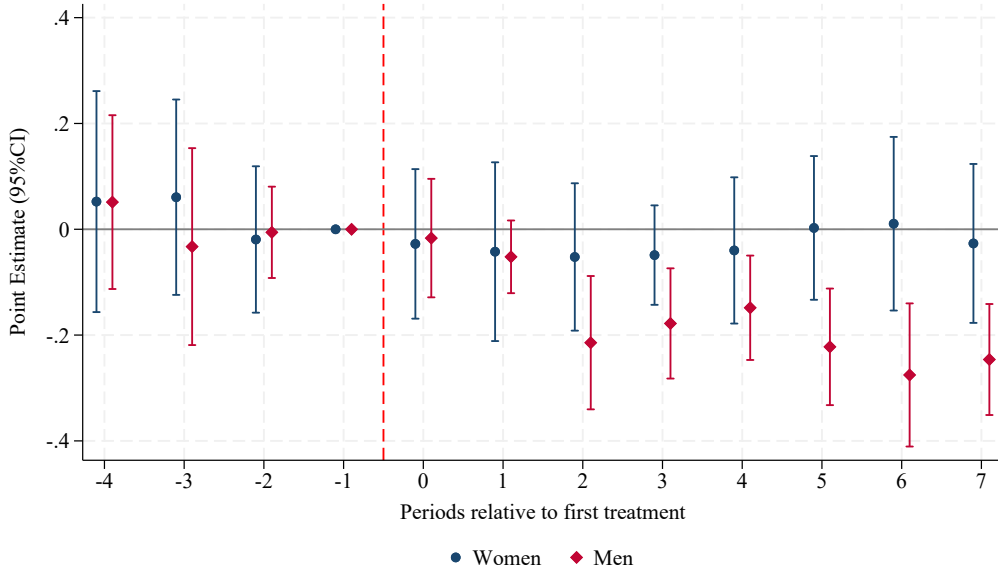
Partisan identification. To investigate how mail voting availability affects partisan identification, I estimate a version of Equation 1 on a sample of CCES and NAES respondents, where the outcome of interest is one’s self-declared partisan identification on a 7-point scale ranging from 1=Strong Democrat to 7=Strong Republican. As the outcome is a direct measure of partisanship, I limit my main specification to election years from 2000 to 2020 in order not to capture the post-2020 reversal of mail voting in some states, which occurred in a period of highly partisan attacks against postal voting.

If mail voting impacts opinions through within-household dynamics, I expect effects on partisanship to be concentrated among couples, leaving singles largely unaffected. Moreover, since women tend to express significantly more left-wing political views than men (Inglehart and Norris, 2000; Edlund and Pande, 2002), effects should be heterogeneous across gender and their direction insightful. Therefore, I estimate the model separately for each gender $\times$ marital-status pair. The specification controls for race, age and its square, college education, and full-time employment. Importantly, the strength of one’s partisanship can vary across the campaign as candidates attempt to mobilize voters, weakening the comparability of respondents surveyed at different points in time. To address this concern, I further control for the number of days separating one’s interview from Election Day. Because the NAES survey does not provide sampling weights, the main specification does not weight observations. I nonetheless show the robustness to weighting CCES observations in Figure D5. Thus, this specification identifies, within gender and marital status, the average shift in declared ideological feeling across electoral cycles. Estimated coefficients for respondents in a cohabiting couple are reported in Figure 6.

On average, the introduction of no-excuse mail voting had no impact on married women, with all post-treatment coefficients statistically indistinguishable from zero and very close to it. On the other hand, it is associated with a significant leftward shift in partisan ideology among married and cohabiting men. Over the 8 election cycles following the introduction, it moves partnered male respondents an average of -0.151 points on the ideological scale, corresponding to 0.068 of a standard deviation. This effect builds up over time, first materializing in the second election cycle and becoming highly significant from the third election on. This gradual increase in the measured effect is consistent both with the progressive penetration of mail voting among the population and with the slow moving nature of opinions and ideology. Moreover, many survey respondents have not yet entered the mail voting period nor received their ballots when interviewed, and thus, in the first election in which mail voting is available, have never yet experienced it. Importantly, estimated pre-mail-voting coefficients for both partnered men and women are not statistically different from zero and very similar, providing support for the parallel trends assumption. I further report in Figure D1 the results of the corresponding estimation on single respondents, whose average ideological evolution following the introduction of no-excuse mail voting does not present such a pattern. This absence of measured effect on singles is consistent with the finding that only couples see their political interactions affected by postal voting.

Robustness checks. To assess the robustness of this result, I perform a series of tests. First, I want to ensure that specific and localized conditions – such as a particular campaign, local state specificities, or even survey wave administration – are not driving the uncovered results. To

Figure 6: Impact of mail voting on partisan ideological feeling of couples



Notes: This figure presents dynamic difference-in-differences estimates of the effect of no-excuse mail voting introduction on partisan identification of married -or living as such- individuals. Estimation is performed using the methodology of [De Chaisemartin and d'Haultfoeuille \(2026\)](#). The unit of observation is an individual aged 18 to 64 and cohabiting with a partner. The dependent variable is a 7-point ordered-scale measuring self-declared partisan identification and ranging from 1=Strong Democrat to 7=Strong Republican. Estimation is performed separately for women (blue circular marker) and men (red diamond markers). All estimations control for race, age and its square, education, full-time employment, and the number of days separating the interview from election day. Standard errors are clustered at the state level. Bars denote 95% confidence intervals.

address this concern, I perform three leave-one-out tests, in which I successively exclude from the estimation one state, year, and adoption cohort. Coefficients associated with the corresponding average post-introduction effects are reported in Figures D2, D3, and D4, respectively. Coefficients are stable across all three tests, confirming that this effect is not driven by one-off temporal or local conditions.

I report in Figure D5 a series of further checks. Coefficients are robust to the exclusion of 2020 from the estimation, a year marked by mail voting adoptions driven by the COVID-19 pandemic and by politically charged debates over the technology. Including the 2022 and 2024 elections, which saw mail voting reversed in some states, yields very similar post-treatment estimates. The inclusion of these years nonetheless slightly affects pre-treatment coefficients, suggesting that partisanship could have played a role in post-2020 reversals and thereby strengthening the choice of restricting the analysis of partisan outcomes to the period through 2020. As mentioned previously, I choose for consistency not to use sampling weights in the baseline estimation as the NAES does not provide any. Results are robust to applying weights provided by the CCES. I also estimate the model without individual-level covariates as a sensitivity check on the conditional parallel trends assumption. Estimates are similar to the baseline specification, suggesting that allowing outcome trends to vary with observable differences in age, education, race, and employment status is not essential for the estimated treatment effects. Finally, I employ a stricter definition of couples, focusing only on married respondents instead of including also individuals living as such. Results are unchanged by this restriction.

One last concern is that findings are dependent on the difference-in-differences estimator used.

To alleviate this worry, Figure D6 reports the same estimation for alternative recently proposed event-study estimators ([Callaway and Sant’Anna, 2021](#); [Sun and Abraham, 2021](#); [Rambachan and Roth, 2023](#)) as well as the canonical two-way fixed effects estimator. Results are very stable across estimators. Moreover, the parallel trends assumption is supported by all.

Policy views. Beyond movements in expressed partisanship, highlighting within-couple convergence, one can wonder whether mail voting further shifts underlying views. While persuasion through increased political exchanges would push in that direction, a simple delegation of political initiative could translate into shifts in partisan identification without an adjustment of beliefs. Since comparing attitudes on individual topics is difficult due to the lack of comparability of survey questions across time, I leverage the policy ideal point measure computed by [Tausanovitch and Warshaw \(2013\)](#) for NAES and CCES surveys ([Warshaw and Tausanovitch, 2022](#)). Based on an item-response theory model and made possible by the inclusion of their own survey module in order to bridge questions and responses across survey waves, this measure aggregates individual policy preferences into a one-dimensional scale where lower values are associated with more left-wing views and higher values with more right-wing views.

To study how policy views have evolved following the introduction of mail voting, I then estimate the same version of Equation 1 as when investigating partisan identification, now using this policy preference index as the outcome. Results for married or cohabiting respondents are reported in Figure D7. The availability of this measure for 2004 to 2020 elections mechanically shortens the time frame of the analysis, with adoption cohorts between 2000 and 2004 no longer contributing to the estimated effects. Yet, estimated results display a very similar pattern to that of partisan identification. While women in couples do not exhibit signs of shifting policy views, men appear to shift leftward. Over the estimated period, this shift averages a decrease of 0.049 on the policy scale, corresponding to a leftward movement of 0.051 standard deviation. This magnitude is consistent with that of the partisanship shift. Moreover, although slightly noisier, this effect follows a similar trend, taking two cycles to emerge and building up with time. This pattern is again consistent with both the gradual take-up of mail voting and the slow-moving nature of policy preferences. Figure D8 reports the corresponding effects for single respondents. Again, singles do not appear to be significantly affected, further supporting the finding that mail voting’s impact on behaviors occurs within the household.

Together, these results highlight that, through increased and repeated political interactions, mail voting favors convergence among couples, in terms of both partisan identification and underlying beliefs. This finding moreover affirms the importance of the marital unit as a place of opinion exchanges, but one in which, absent a catalyst for interaction, information sharing can be imperfect.

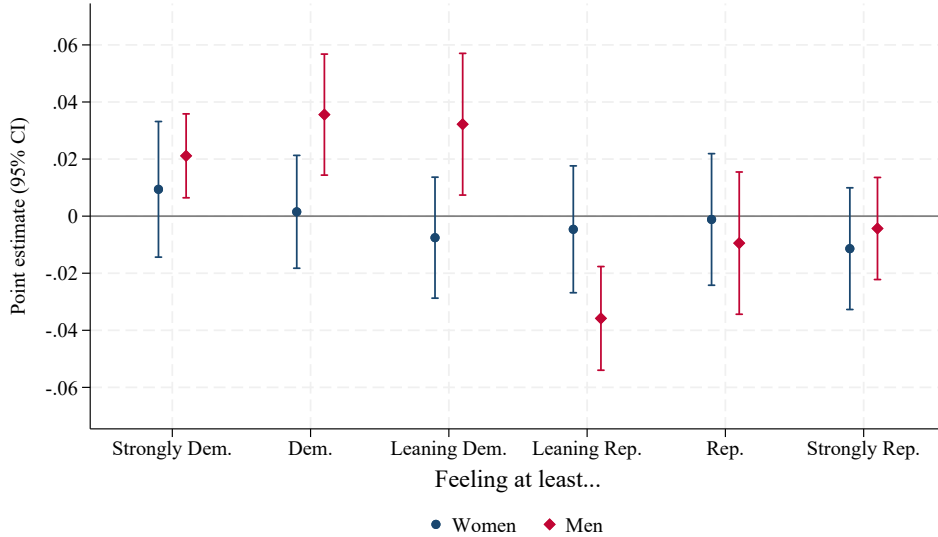
6.2 Heterogeneities across the ideological distribution

Mail voting appears to shift the average position of partnered men leftward while leaving that of women unaffected, reducing the gender gap in terms of partisanship and political preferences. Nonetheless, this finding might hide important heterogeneities along the ideological distribution. I now investigate such potential distributional effects.

Partisanship. To study how partisan identification shifts across the partisan spectrum, I first create a series of indicator variables corresponding to the different points of the partisan ideology scale. Specifically, I create six binary variables capturing different strengths of Democratic and Republican identification: Leaning Democrat or stronger, feeling weakly Democrat or stronger, feeling strongly Democrat, and the analogous Republican party counterparts. I then estimate the same event-study specification Equation 1 as previously presented in this section on these six outcomes for each gender $\times$ marital-status pair. Figure 7 reports the estimated average post-treatment effects for couples.

In line with estimations using the ordered scale as outcome, I find no effect on women’s partisan identification. Interestingly, men’s likelihood of identifying either strongly or weakly republican is unaffected by the introduction of mail voting, while that of leaning republican diminishes significantly. On the other side of the political spectrum, the likelihood of all strengths of democratic identification increases, with stronger effects recorded for leaning and identifying weakly democrat. These results document a striking finding: men identifying with the Republican party do not move, while the significant leftward shift in partisanship appears to be primarily concentrated among men with no or weak partisan attachment. This is consistent with the fact that such individuals are likely to be more ideologically “movable” or already aligned with more left-wing positions. Thus, interactions with a partner who sits, on average, to their left on the ideological spectrum can push them in that direction.

Figure 7: Impact of mail voting on partisan identification strength of couples along the distribution



Notes: This figure presents dynamic difference-in-differences estimates of the effect of no-excuse mail voting introduction on partisan identification of married -or living as such- individuals. Estimation is performed using the methodology of [De Chaisemartin and d’Haultfoeulle \(2026\)](#) and average post-treatment effects are reported. The unit of observation is an individual aged 18 to 64 and cohabiting with a partner. The dependent variable is an indicator variable equal to 1 if the respondent declares an ideological identification with the corresponding party of at least that given strength. Coefficients are each obtained through separate estimations. Estimation is performed separately for women (blue circular marker) and men (red diamond markers). All estimations control for race, age and its square, education, full-time employment, and the number of days separating the interview from election day. Standard errors are clustered at the state level. Bars denote 95% confidence intervals.

Policy views. In turn, I ask whether policy views follow similar distributional shifts. I address this question through quantile regressions in a difference-in-differences setting. While the incidental-parameters problem is of first-order concern in quantile regressions with fixed effects (Koenker, 2004; Canay, 2011), the cross-sectional nature of my setting departs from the canonical case of concern as the specification only calls for state and year fixed effects. Because the number of such parameters is small, does not grow with the sample, and each cell contains many observations, this threat is mitigated. Consistent with the rest of the paper, I cluster standard errors at the state level, using Parente and Santos Silva (2016)’s implementation of quantile regression for clustered data. Moreover, to partially mitigate the legitimate concerns related to TWFE difference-in-differences estimation in the presence of staggered treatment, I exclude from the sample states already treated at the beginning of my observation period.

Estimated results are reported in Figure D9. In line with previous results, policy views of women do not appear to shift at any point in the distribution. Men’s results, moreover, align strongly with those on partisan identification. While no significant effect is measured on the upper quantiles of the policy views index, corresponding to more conservative positions, the drop is largely concentrated in quantiles below the median. Specifically, the uncovered effect displays a U-shape: while the left-most side of the policy views scale exhibits smaller effects, the leftward shift intensifies at more moderate but still left leaning quantiles before converging back towards zero once entering more conservative leaning segments of the distribution. Figures D10 and D11 further report the analogous estimates to the implementation of two more recent developments of quantile regression with fixed effects (Machado and Santos Silva, 2019; Melly and Pons, 2025). Results align strongly with the pattern uncovered through canonical quantile regression, confirming that mail voting’s significant impact on partnered men’s policy views is concentrated among individuals with moderate or already left-leaning views.

6.3 Channel: from female initiative to male knowledge gains?

Previously presented results show how the availability of mail voting has partially closed the gender gap in partisan identification and policy views of couple members. Yet one can wonder why a voting technology generates such a one-directional movement on average. In this subsection, I attempt to document a channel through which the effect operates by showing how gender-imbalanced political involvement during the campaign is mirrored by unequal long-term political knowledge gains.

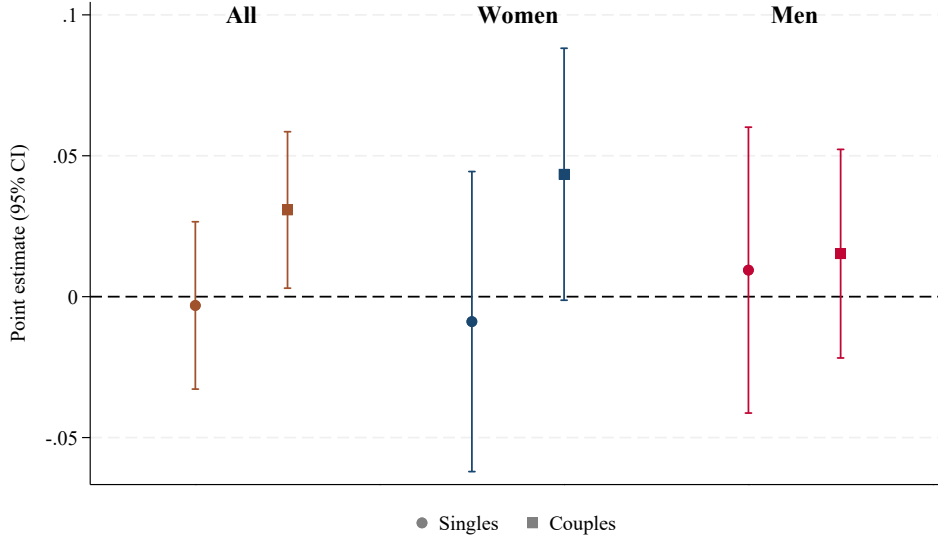
Short-term involvement in the campaign. Gendered effects in medium- and long-term opinions could first be traced back to short-term gendered imbalances in the consequences of mail voting availability. To study this, I leverage questions on one’s interest in the current campaign available in the 2000 and 2004 NAES surveys. Specifically, my variable of interest captures how closely individuals have been following the ongoing election campaign on a self-reported 3-point scale: Not much or less, somewhat, very closely.²⁷ I then estimate Equation 2 on this outcome

²⁷This variable is built based on two very close 2000 NAES questions and two 2004 NAES questions, each run on half the yearly sample. The 2000 questions and possible answers are “Would you say you have been [Not much/Somewhat/Very] interested in the political campaigns so far?” and “Would you say you have been [Not much/Somewhat/Very] interested in the presidential campaign so far?”. The 2004 questions and possible answers are “How closely are you following the campaign for president? [Not at all/Not too much/Somewhat/Very]” and

to measure how the following of the campaign of individuals surveyed at the same distance to the election varies depending on whether the mail-voting period is open on that day. I run this estimation both on the whole sample of respondents, and split by gender. Coefficients of interests are reported in Figure 8.

Mail-voting periods appear to be associated with a significant increase in couples' following of the campaign (+0.031) while leaving that of singles unaffected (-0.003). This result further extends the previous finding on political discussions: couple members not only discuss politics more, but also follow the ongoing campaign more closely. Yet, sub-sample analysis reveals a sharp gender heterogeneity. While neither male or female single respondents are affected by mail voting availability, the increase in couples' interest in the campaign is almost entirely driven by partnered women (+0.043, p-value=0.06). Partnered men, on the other hand, do not display any significant increase in campaign interest (+0.015, p-value=0.42). Table D1 presents the corresponding detailed regression table.

Figure 8: Within-campaign impact of mail-voting period on interest in the campaign



Notes: This figure presents OLS estimates of the differential effect of mail voting periods on campaign interest across marital status and gender. The unit of observation is an individual. The dependent variable is a 3-point scale measuring the level of interest in the ongoing electoral campaign. The explanatory variables of interest are an indicator equal to 1 if mail ballots can have legally been sent out on that day and its interaction with an indicator equal to 1 if the respondent is in a couple. Estimations are successively performed on the full sample (brown markers, left panel), women (blue markers, central panel) and men (red markers, right panel). The effect on singles (round markers) and couples (square markers) are obtained through a common estimation. All estimations control for race, age, education, full-time employment, the presence of an underage child at home and monthly church attendance and include state×year and days-to-Election×year fixed effects. Estimation on the whole sample additionally controls for gender. Standard errors are clustered at the state level. Bars denote 95% confidence intervals.

This pattern is consistent with one of two explanations. Either partnered women previously less interested in politics become more politically invested or, instead, mail voting requires a certain level of political involvement (finding out how to fill in the ballots, anticipating one's electoral choice, etc.) which is unevenly shared within the couple, weighing particularly on women. As such, this latter explanation aligns with patterns of unequal division of domestic tasks. I argue that the former does not appear to be a plausible explanation for these results. As suggestive

“Would you say you have been [not much/somewhat/very] interested in the campaign so far?”.

evidence, I run the same exercise leveraging information on one’s general interest in politics, instead of one’s following of the campaign.²⁸ Estimated coefficients are reported in Figure D12. Married women do not display a significant increase in their following of general government affairs, suggesting that their increase in campaign involvement is specific to the ongoing electoral process. Moreover, if such increased involvement were due to a “catching up” with one’s partner’s behavior, then long-term opinions should mimic this shift. As presented previously, this is not the case. Therefore, I argue that this pattern is instead consistent with mail ballots “entering” couples through women, taking the political initiative. This is further consistent with results presented in section 4 showing that while female take-up of mail voting is similar across marital status, that of men is notably higher when cohabiting with a partner.

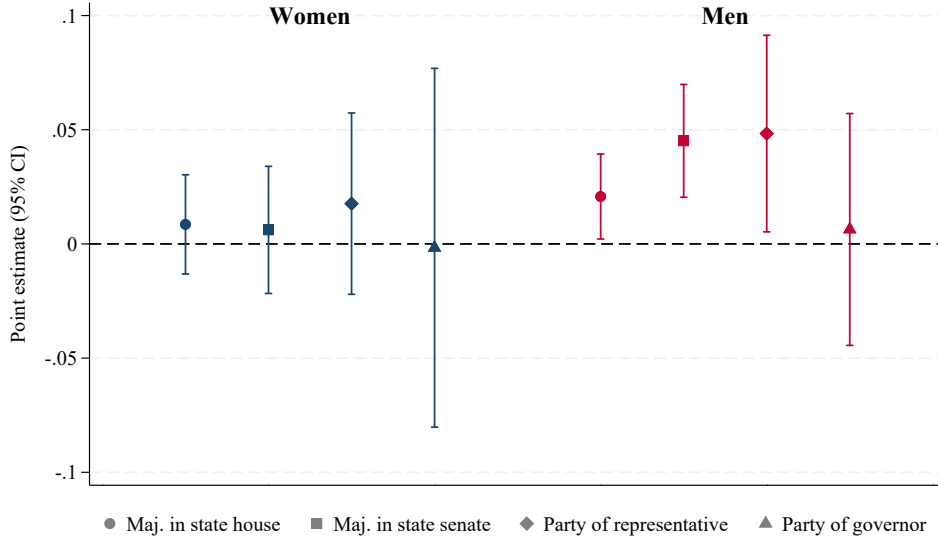
Long-term knowledge gains. In turn, I investigate how mail ballots impact political knowledge across electoral cycles. To do so, I leverage questions in the CCES measuring individuals’ knowledge of their elected officials, which I study using the same across-cycle estimation strategy as previous analyses, estimating Equation 1. Specifically, outcomes of interest are indicator variables measuring one’s knowledge of the majority in the state house, in the state senate, the party of one’s representative in Congress as well as of the governor. Because these questions are available only in the CCES, the estimation is limited to years 2006-2024, and I report the average treatment effect over four electoral cycles for couples in Figure 9.

Postal voting does not appear to be associated with any significant increase in partnered women’s political knowledge. This result supports the previous argument that the short-term increase in electoral involvement of women during the mail-voting period is not driven by an overall political “catch-up”. On the other hand, the likelihood that men correctly identify the majority party in state house and senate or know the party of their representative in Congress significantly increases, respectively by 2.0, 4.5, and 4.8 percentage points. Interestingly, the likelihood of knowing the party of one’s governor is unaffected. This is likely explained by the fact that individuals display significantly higher baseline knowledge of the governor’s party (77% correct identification in sample) relative to the other offices (62-65%). Results would thus be consistent with political knowledge increasing among individuals with non-zero knowledge. I confirm this finding by showing how many of the four offices respondents can correctly identify in Figure D13. Partnered men’s likelihood of correctly identifying none of these offices is unaffected, while that of identifying correctly one, two, or three of them all decrease. Conversely, the likelihood of identifying them all correctly significantly increases. Women also display an increase in the likelihood of identifying them all correctly, although smaller, but only at the expense of knowledge of three out of the four. Lower levels of knowledge are unaffected. This highlights that partnered men with low but non-zero political knowledge significantly gain from the introduction of mail voting, while only partnered women exhibiting already high levels of political knowledge further benefit.

Put together, such patterns are consistent with the argument that moderate or mildly politically involved men benefit from the political initiative of their partner, in turn gradually converging towards their partner’s position in terms of ideology and policy views.

²⁸Specifically, I make use of the following four-point scale question fielded identically in both the 2000 and 2004 NAES waves: “Would you say you follow what is going on in government and public affairs [most of the time/some of the time/now and then/hardly]”.

Figure 9: Across-campaign evolution of political knowledge of couple members



Notes: This figure presents dynamic difference-in-differences estimates of the effect of no-excuse mail voting introduction on partisan identification of married -or living as such- individuals. Estimation is performed using the methodology of [De Chaisemartin and d’Haultfoeuille \(2026\)](#) and average post-treatment effects are reported. The unit of observation is an individual aged 18 to 64 and cohabiting with a partner. The dependent variable is an indicator variable equal to 1 if the respondent correctly identifies the party with a majority in the state house (circle marker), in the state senate (square marker), of their representative in congress (diamond marker) or of their governor (triangle marker). Coefficient are each obtained through separate estimations. Estimations is performed separately for women (blue markers, left panel) and men (red markers, right panel). All estimations control for race, age and its square, education, full-time employment, and the number of days separating the interview from election day. Standard errors are clustered at the state level. Bars denote 95% confidence intervals.

7 From a collective action to a collective vote choice? Evidence from Cast Vote Records

The increased political interactions and the following convergence of political opinions generated by postal voting could in turn have important electoral consequences. Yet, recent work has uncovered that mail voting has no effect on partisan vote shares in general elections ([Thompson et al., 2020](#); [Yoder et al., 2021](#)), or at most a very mild and not statistically significant positive effect on Democratic vote shares ([Barber and Holbein, 2020](#)). I also uncover the same findings using the CCES sample, presented in subsection E.1. Nonetheless, the absence of measurable electoral effects in general elections does not necessarily mean that mutual influence does not translate all the way to ballots. As previously presented, most ideological movements happen within partisanship. Moreover, existing studies focus on national or high-level offices, positions which exhibit both few candidate options and high levels of partisan “stickiness” ([Conevska et al., 2026](#); [Kuriwaki, 2026](#)), making mutual influence difficult to observe. Similarly, since electoral surveys are usually restricted to such offices, survey-based evidence, as presented in subsection E.1, does not permit to go beyond this limitation. To address this and further document the impact of mail voting on individual decision making, I instead focus on lower-level electoral decisions, in the context of two elections in San Francisco. Specifically, I make use of administrative data on actual ballots to investigate how postal voting affects the level of resemblance of individual voting patterns.

Setting and data. As presented in section 3, I leverage *Cast Vote Records* (CVRs) – transcripts of actual ballots – from two elections in San Francisco County (California): the November 5, 2019 municipal election and the March 3, 2020 presidential primary. Three key features of these records matter for the analysis. Each ballot card reports (i) the full combination of choices across its contests, (ii) the mode through which it was cast, by mail or in person, and (iii) the precinct in which it was cast. Crucially, precincts are very small units. San Francisco had 474 precincts in 2019 and 609 in 2020, with on average 1,044 and 827 registered voters, respectively. Voters are assigned a precinct based on their residential address, and those registered at the same address therefore vote in the same precinct.

Strategy. I use these data to study whether ballots cast by mail exhibit more similar voting patterns than those cast in person. Linking such a finding to partners’ mutual influence, however, faces two challenges. First, as direct transcriptions of ballots, CVRs contain no information on voters: ballots cannot be paired, and couples cannot be identified directly. Second, parties and candidates issue voting recommendations. Some patterns are therefore very frequent and may simply reflect voters following a common recommendation, which can moreover be easier to apply at home than in the booth.

I confront these challenges by exploiting the rarity of voting patterns and their geographic distribution across the city. With voters facing over a dozen contests, many of which carry low partisan polarization, the number of possible choice combinations is very large. I choose to study how very rare ballot cards co-occur in the same precinct and voting mode. Specifically, I focus on unique pairs of identical ballot cards – patterns recorded only twice citywide among all cards of identical layout. My measure of interest is then the *excess* co-location among unique pairs of mail ballots relative to other types of pairs (both cast in person, or mixed). This choice provides three advantages. First, identical cards can capture the most advanced form of couple alignment: both members making the exact same choices. Second, focusing on very rare choice combinations allows me to abstract from party or candidate recommendations. Finally, given the overall number of precincts, these patterns are unlikely to originate from the same one by chance, which makes any excess co-location particularly meaningful.

Importantly, patterns can only be compared among ballots offering the same set of choices, with two implications for the analysis. First, as each card in multi-card ballots is recorded separately and cannot be linked back to its ballot (see section 3), the analysis is conducted at the card level. Second, ballots are printed according to a fixed catalog of ballot types defined by the election administration: a type fully determines the set of contests on the ballot and the way it is divided into cards, so that all ballots of a given type are split into cards identically. The pagination of cards within a ballot type thus defines a card type, identifying a common layout and contest set. Because some contests are citywide and others district-level, the ballot type faced by voters is thus determined by their precinct. All comparisons are made within card type.

Computation. Consider cards of type t , cast by one of two modes $v \in \{M, E\}$, by mail or on election day. Drawing two cards at random and without replacement from the N_v^t cards of type t cast by mode v across the city, the probability that both originate from the same precinct writes as:

$$p_{vv}^{0,t} = \frac{\sum_p \binom{n_{v,p}^t}{2}}{\binom{N_v^t}{2}} = \frac{\sum_p n_{v,p}^t \times (n_{v,p}^t - 1)}{N_v^t \times (N_v^t - 1)} \quad (3)$$

where $n_{v,p}^t$ denotes the number of such cards cast in precinct p . The analogous probability for a mixed pair, drawing one card of each mode, is $p_{ME}^{0,t} = \sum_p n_{M,p}^t n_{E,p}^t / (N_M^t N_E^t)$.

Let T_{vv}^t denote the number of unique pairs of type t with mode composition $vv \in \{MM, EE, ME\}$. If the two cards of every pair were cast by unrelated voters, the number of pairs expected to co-locate in the same precinct would be, by linearity of expectation, $E_{vv} = \sum_t T_{vv}^t p_{vv}^{0,t}$. Comparing this benchmark with the number of pairs whose two cards are actually observed in the same precinct, O_{vv} , yields the excess co-location rate:

$$\Delta_{vv} = \frac{O_{vv} - E_{vv}}{\sum_t T_{vv}^t}, \quad (4)$$

This figure measures the share of unique pairs found in the same precinct beyond what independent location within their card type would produce. To test whether this excess differs from zero, I compute the associated Z-score, $z_{vv} = (O_{vv} - E_{vv}) / \sqrt{E_{vv}}$.²⁹

A positive excess among mail pairs, however, cannot be directly attributed to joint completion. Voters from the same precinct share similar preferences and may independently arrive at the same rare combination of choices, generating co-location of identical cards without any household link. Moreover, couples who align perfectly can also vote identically at the booth. Netting excess rates across mode compositions addresses both concerns, as joint completion does not affect all compositions equally. Since in-person ballots are completed alone in the booth, both members of an EE pair filled in their card in isolation. The EE excess thus captures the spatial clustering of preferences, together with any choices that partners settle in advance and later reproduce separately. The object of interest is therefore the *net* excess co-location rate

$$\Delta^{net} = \Delta_{MM} - \Delta_{EE}, \quad (5)$$

which differences out both components and isolates what is specific to ballots that could physically be completed together. Mixed pairs, in which only one card was completed at home, provide a complementary benchmark. Since couples overwhelmingly vote through the same mode (see Figure B3), few ME pairs correspond to couples, and their excess should mostly reflect the spatial clustering of preferences, placing Δ_{ME} below both within-mode excesses. For the same reason, subtracting the cross-mode excess can be seen as netting out non-household drivers of co-location, with the net excess then written, equivalently, as $(\Delta_{MM} - \Delta_{ME}) - (\Delta_{EE} - \Delta_{ME})$.

Results. Table 2 reports, for each mode composition and election, the number of unique pairs together with their observed, expected, and excess co-location rates. I identify 6,630 unique pairs in the 2019 municipal election and 18,542 in the 2020 presidential primary, the majority of which

²⁹Formally, the null holds the observed precinct counts $n_{v,p}^t$ fixed and assumes that a card's content is unrelated to its location, so that the two cards of any pair are drawn at random from their pools' spatial distributions. Co-location indicators are then independent Bernoulli variables with probabilities $p_{vv}^{0,t}$, and O_{vv} is approximately Poisson-distributed with mean E_{vv} .

(56.6 and 64.8%, respectively) consist of two mail cards. While expected co-location rates are low and similar across compositions (2.8-3.4% in 2019, around 1 percent in 2020), observed rates are not. Mail pairs co-locate in 24.6% of cases in 2019 and 32.9% in 2020, implying excess rates Δ_{MM} of 21.8 and 31.8%. Pairs cast in person co-locate far less: Δ_{EE} amounts to 6.3% in 2019 and 7.1% in 2020 — a still significant excess, consistent with in-person pairs capturing the local correlation of preferences and fully aligned couples voting in person. Cross-mode co-location is lowest, with Δ_{ME} at 1.3 and 1.1%, in line with the ordering predicted above. A word of caution is warranted about cross-mode pairs in the 2020 primary: in a large number of precincts, mail and in-person ballots were assigned different ballot types, preventing the identification of ME pairs in these cases.

The net excess co-location rate of Equation 5 amounts to 15.5% of unique pairs in 2019 and 24.7% in the 2020 primary. Under the (strong) assumption that mail pairs would behave like in-person pairs absent the possibility of filling ballots side by side, these figures indicate that roughly one in six and one in four unique identical pairs are attributable to mail voting.

Extending this figure from unique pairs to the full distribution of votes requires co-completion to be unrelated to the frequency of the voting pattern. That is, that couples co-complete or influence each other to the same extent whether their choices are unique or common. To assess this assumption, I extend the analysis to card patterns appearing exactly $m \in \{3, \dots, 25\}$ times citywide and report in Figure E3 the net excess scaled by $\frac{m-1}{2}$, which expresses it per card and makes estimates comparable across m .³⁰ In 2019, the scaled net excess is flat in m , with an estimated slope not statistically different from zero. In 2020, the very rarest patterns co-locate somewhat more, but the profile flattens beyond patterns occurring four times, at a net excess rate of about 14 percent – in line with the 2019 estimates. The “popularity” of a voting pattern thus at most marginally affects couples’ propensity to co-complete it, and extending the unique-pair benchmark to the full voting population indicates that co-completion through mail ballots can account for roughly 15 percent of identical pairs cast by mail.

Naturally, these figures should not be interpreted as the *causal* impact of mail voting on couples’ ballot co-completion. Couples are not jointly observed: the focus on unique pairs approximates couple behavior only on average, and pairs may capture relations other than couples. Selection into voting mode is also possible, with couples of higher baseline alignment perhaps selecting into mail voting more. Nonetheless, this exercise provides strong evidence that, even among very rare voting patterns, mail voting is associated with significantly higher levels of voting congruence than in-person voting, in line with the existence of substantial ballot co-completion, and offers a benchmark for the magnitude of this behavior. Moreover, identical cards capture the extreme case of perfect alignment, while mutual influence need not run across all offices. This exercise should thus be read as evidence of significant ballot co-completion when voting by mail, rather than as a causal quantification of couples’ mutual influence.

³⁰A co-completing couple adds exactly one co-located pair to its pattern, whatever m . A pattern appearing m times, however, generates $\binom{m}{2}$ pairs, so that the same behavior translates into a per-pair excess that mechanically shrinks as m grows. Expressing the excess per card (that is, per occurrence of the pattern) rather than per pair removes this dilution: with $\binom{m}{2}$ pairs and m cards per pattern, the per-card excess equals the per-pair excess multiplied by $\binom{m}{2}/m = \frac{m-1}{2}$.

Table 2: Observed and Expected co-location of ballots in two San Francisco elections

Election	Mode	Total pairs	Co-location of ballots						
			Observed (O)		Expected (E)		Excess ($\Delta = O - E$)		
			#	%	#	%	#	%	Z-score
Nov 2019	MM	3,753	924	24.6%	106.9	2.8%	817.1	21.8%	79.0
	EE	638	62	9.7%	21.7	3.4%	40.3	6.3%	8.6
	ME	2,239	92	4.1%	61.9	2.8%	30.1	1.3%	3.8
	Total	6,630							
Mar 2020	MM	12,021	3,952	32.9%	133.7	1.1%	3,818.3	31.8%	330.2
	EE	5,462	445	8.1%	55.5	1.0%	389.5	7.1%	52.3
	ME	1,059	22	2.1%	10.6	1.0%	11.4	1.1%	3.5
	Total	18,542							

Notes: This table reports the observed and theoretically expected number of unique identical pairs, and their excess co-location rate in the 2019 municipal election and 2020 presidential primary in San Francisco. MM, EE, and ME denote pairs of ballots respectively both cast by mail, both cast in-person on election day, and mixed.

8 Mail ballots and within-couple conflict: the case of intimate partner violence

By increasing political interactions and reducing the individuality of one’s vote, mail voting might have consequences beyond direct political outcomes. While political discussions might, in themselves, be prone to disagreement, the potential loss of secrecy generated by mail ballots further reduces significantly the sheer possibility of political nonalignment going unnoticed. Through this section, I therefore investigate how postal voting affects levels of conflict between partners, focusing specifically on intimate partner violence.

Estimation. As described in section 3, I collect from the NIBRS all reported violent events between partners in 28 states over the 2007-2016 period. This corresponds to roughly 3 million incidents, from which I construct a panel by counting incidents at the county-day level. I estimate the following baseline equation:

$$IPV_{c,t} = \beta \text{Mail voting period}_{s,t} + \alpha \text{Mail persistence}_{s,t} + \mathbf{X}'_{c,t} \delta + \lambda_{c,y} + \gamma_t + \epsilon_{c,t} \quad (6)$$

where $IPV_{c,t}$ denotes one of two outcome variables characterizing the occurrence of violent events on date t in county c : (i) an indicator variable equal to 1 if at least one incident is reported, and (ii) the total number of reported incidents. $\text{Mail voting period}_{s,t}$ is an indicator variable equal to 1 if mail voting is open (and available without an excuse) in county c ’s state s on date t . β is therefore my coefficient of interest. To account for the fact that mail voting might have persistent effects, I allow for states which offered postal voting to differ in the weeks following elections by including a vector of indicators $\text{Mail persistence}_{s,t}$ respectively equal to 1 if state s offered mail voting and date t falls [0,13], [14,27], [28-41] or [42,55] days after an election. $\mathbf{X}_{c,t}$ include a set of time-varying controls capturing daily weather conditions (temperature, rainfall, and their squares) in county c , which have been shown to affect levels of interpersonal and domestic

violence (Blakeslee et al., 2021; Cohen and Gonzalez, 2024; Habets, 2025; Rizzotto et al., 2025). $\lambda_{c,y}$ is a vector of county-by-year fixed effects, which accounts for county-specific conditions and allows them to vary across years. This forces comparison within the same county-year, netting out confounders such as local economic shocks, potential changes in the county’s incident reporting practices or general differences associated with election years. I also estimate a looser specification in which I include county and year fixed effects separately. This latter estimation compares mail voting periods to all other days in the sample, documenting how mail-voting days differ from a county’s overall (conditional) average level. Reporting both estimations further allows me to assess how conflict levels might differ between electoral and non-electoral years. γ_t denotes a vector of temporal fixed effects of two kinds. Since US elections are consistently held on the Tuesday after the first Monday of November, I index dates according to this anchor and include a set of days-to-Election-Day fixed effects. Importantly, this anchoring based on a constant common calendar rule permits such indexing in non-election years too. Thus, it captures within-year day-to-day variation and allows to compare days at same distance from the election. Moreover, since any given distance from Election Day always falls on the same day of the week, it accounts for day-of-the-week variation, which can affect intimate partner violence, such as weekends versus workdays. Additionally, the estimation includes day-of-the-month fixed effects, which capture within-month cycles such as paydays (Masi and Santantonio, 2026). Put together, in the stricter specification, β identifies the average within-county-year increase in reported intimate partner violence on days when postal voting is open, relative to unaffected days at the same distance from Election Day. The counterfactual profile of these days is estimated from non-election years and from counties in states without an open no-excuse mail voting period. Identification therefore rests on the assumption that, conditional on covariates, reported violence would have followed parallel paths over the electoral calendar absent postal voting. As mail voting periods are decided statewide, treatment does not vary across counties within a state. Standard errors are therefore clustered at the state level (Abadie et al., 2023). Estimations using an indicator for the occurrence of *any* event as outcome are performed through Ordinary Least Squares regression, while those explaining the *number* of events use a Poisson pseudo-maximum-likelihood model. This choice allows me to take into account the non-linearity of count data and handle over-dispersion while circumventing the incidental parameters problem that fixed-effects would cause in other non-linear models (Cameron and Trivedi, 2005). Estimated coefficients are reported in Table 3.

Results. Across specifications, mail-voting periods appear to be associated with significantly higher levels of intimate partner violence. Column 1 reports that mail voting periods exhibit a 2.1% larger likelihood that at least one violent incident is reported and 3.8%³¹ more overall incidents than other days. My preferred specification, which relies on within-county-year comparisons and is reported in column 2, displays somewhat smaller but still substantial results: mail voting periods are associated with a 1.2% increase in the likelihood of any incident occurring and a 1.8% increase in the total number of events. Scaled to the full mail-voting period, this daily increase corresponds to 0.31 excess incidents in the average county over the average window length, or roughly 500 excess incidents per election across the full sample of counties.

Due to the sharp gender imbalance in the direction of intimate partner violence, women

³¹Conversions of Poisson regression coefficients into percentages are computed as: $(e^\beta - 1) \times 100$.

overwhelmingly suffering from male-perpetrated violence, I report in columns 3 to 6 results from the analog estimation separating incidents by the gender of the victim. Effects are present across both genders in similar magnitudes when scaled to the mean: Mail-voting periods are associated with increases of 1.6% (respectively 2.3%) in the likelihood of at least one event with a female (resp. male) victim occurring, and of 1.6% (resp 2.5%) in the total number of such events. I further present in Figure F1 the corresponding estimates across demographic characteristics of victims and offenders. Effects are strongest among victims aged 40 to 59, while no effect is detected when the victim is over 60. Estimates are very similar when incidents are decomposed by the race of either the victim or the offender. While the extensive margin rises similarly across locations, the increase on the intensive margin is fully driven by incidents occurring at the couple's home.

Table 3: Evolution of intimate partner violence during mail voting periods

	All incidents		Female victim		Male victim	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: At least one incident						
Mail-voting period	0.0045** (0.0020)	0.0026* (0.0014)	0.0050** (0.0019)	0.0030** (0.0014)	0.0020** (0.0009)	0.0016* (0.0008)
Observations	5475847	5475847	5475847	5475847	5475847	5475847
Mean dep. var.	0.209	0.209	0.185	0.185	0.071	0.071
Panel B: Total number of incidents						
Mail-voting period	0.0370*** (0.0131)	0.0175** (0.0083)	0.0363*** (0.0131)	0.0159** (0.0075)	0.0407** (0.0203)	0.0244 (0.0192)
Observations	5475847	4796782	5468541	4759158	5326074	4279900
Mean dep. var.	0.520	0.594	0.422	0.485	0.102	0.127
All panels						
County FE + year FE	✓		✓		✓	
County × year FE		✓		✓		✓
Days-to-election date FE	✓	✓	✓	✓	✓	✓
Day-of-the-month FE	✓	✓	✓	✓	✓	✓

Notes: This table reports OLS (Panel A) and PPML (Panel B) estimates for the effect of mail voting periods on intimate partner violence. The unit of observation is a county×day. The dependent variable is either an indicator variable equal to 1 if at least one incident was reported (Panel A) or the total number of incidents (Panel B). Columns 1 and 2 considers all such events, while the dependent variable in subsequent columns are restricted to incidents with either a female (columns 3 and 4) or male (columns 5 and 6) victim. Mail-voting period is an indicator variable equal to 1 if mail ballots can have legally been sent out on that day. All estimations control for maximum daily temperature and its square, total daily precipitation and its square, national holidays and 4 indicator variables equal to 1 in post-election days [0,13], [14,27], [28,41] or [42,55] in mail states. All estimations include days-to-Election Day (Tuesday after the first Monday) and day-of-the-month fixed effects, as well as either county and year (columns 1, 3, 5) or county×year (columns 2, 4, 6) fixed effects. Standard errors are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

Notably, the decreased magnitude across specifications suggests that electoral years exhibit higher baseline levels of intimate partner violence than non-electoral years, or that mail voting has effects beyond the corresponding voting period. This, in itself, is consistent with my findings. While mail voting appears to act as a trigger, politically driven within-couple conflict can arise from other sources of political interactions. As such, election years display higher levels of political salience throughout the year than non-electoral years, and particularly so once primaries are underway. Finally, electoral motives could themselves contribute to increasing figures in election years if incumbents enhance policing and reporting capacities when approaching re-election.

Robustness checks. I assess the robustness of these results through a number of tests. I first perform an analogous estimation on the incidence of other types of crime: vehicle theft, drug-related offenses, property damage, shoplifting and assaults not between partners. Estimated coefficients are reported in Table F1. Mail voting periods are not associated with a significant increase in any of these types of crimes. This finding supports the conclusion that the observed effects on intimate partner violence are not due to a general increase in criminality or particular crime-reporting conditions during such periods. Second, I ensure that results are not driven by a specific state or year by performing two leave-one-out exercises in which I, respectively, exclude one state or year in each estimation. Results are reported in Figure F2 and Figure F3. Both exercises yield very stable results, confirming that the uncovered pattern is not due to particular conditions in a single state or year.

9 Conclusion

This paper studies how mail voting reshapes the political behavior of couples in the United States. By relocating it from the polling station to the home, mail ballots collectivize the act of voting: partners not only overwhelmingly adopt this technology jointly, but also request and return ballots together. This transformation is associated with a significant increase in couples' political discussions throughout the mail-voting period. Such increased interactions have important downstream consequences.

First, married men display a leftward shift in partisan ideology and policy views. This effect, concentrated among independent and left-leaning men, and absent among Republicans, suggests that increased interactions foster convergence of partners' opinions. Second, by documenting that mail ballots carry excess pairs of identical ballots originating from the same precinct, this paper shows that mail ballots are associated with significant coordination between partners, whether ideological or logistical. Finally, mail-voting periods are associated with a higher incidence of intimate partner violence: on an issue prone to disagreement, increased interaction and the new observability of one's choices can also heighten conflict.

The collective engagement of partners fostered by mail voting generates important civic gains as knowledge and opinions diffuse between partners. This is particularly the case since less engaged couple members benefit from the involvement of their spouse. These gains, however, are not evenly distributed. The uncovered gender imbalance in take-up and campaign involvement suggests that the burden of the ballot falls unequally across partners: what constitutes a reduction in the cost of voting for the individual can amount, within the household, to a reallocation of electoral work. At the extreme, the lack of secrecy can have serious implications as it raises concerns about potential losses of political autonomy and can put the physical integrity of voters at risk.

Ultimately, this paper highlights that voting rules are not neutral in the environment they create for voters, and that considerations beyond turnout and vote shares ought to be taken into account in their design and evaluation.

References

- Abadie, A., S. Athey, G. W. Imbens, and J. M. Wooldridge (2023). When should you adjust standard errors for clustering? *The Quarterly Journal of Economics* 138(1), 1–35.
- Adams, A., K. Huttunen, E. Nix, and N. Zhang (2024). The dynamics of abusive relationships. *The Quarterly Journal of Economics* 139(4), 2135–2180.
- Aizer, A. (2010). The gender wage gap and domestic violence. *American Economic Review* 100(4), 1847–1859.
- Alford, J. R., P. K. Hatemi, J. R. Hibbing, N. G. Martin, and L. J. Eaves (2011). The politics of mate choice. *The Journal of Politics* 73(2), 362–379.
- Alipour, J.-V. and V. Lindlacher (2025). No surprises, please: Voting costs and electoral turnout. *Journal of Political Economy Microeconomics* 3(1), 59–97.
- Anderberg, D., H. Rainer, J. Wadsworth, and T. Wilson (2016). Unemployment and domestic violence: Theory and evidence. *The Economic Journal* 126(597), 1947–1979.
- Arenas-Arroyo, E., D. Fernandez-Kranz, and N. Nollenberger (2021). Intimate partner violence under forced cohabitation and economic stress: Evidence from the COVID-19 pandemic. *Journal of Public Economics* 194, 104350.
- Arnesen, E. and K. Matsuzawa (2026). Sports betting legalization amplifies emotional cues and intimate partner violence. *Review of Economics and Statistics*, 1–39.
- Ashraf, N. (2009). Spousal control and intra-household decision making: An experimental study in the Philippines. *American Economic Review* 99(4), 1245–1277.
- Ashraf, N., M. Bakhtin, E. Field, A. Voena, and R. Ziparo (2026). Gendered spheres of learning and household decision-making over fertility. *Review of Economic Studies*, rdag020.
- Ashraf, N., E. Field, and J. Lee (2014). Household bargaining and excess fertility: an experimental study in Zambia. *American Economic Review* 104(7), 2210–2237.
- Auerbach, J. and S. Pierson (2021). Does voting by mail increase fraud? Estimating the change in reported voter fraud when states switch to elections by mail. *Statistics and Public Policy* 8(1), 18–41.
- Barber, M. and J. B. Holbein (2020). The participatory and partisan impacts of mandatory vote-by-mail. *Science Advances* 6(35), eabc7685.
- Barrat, J., A. Ellis, G. Orr, V. Vashchanka, and P. Wolf (2023). Special voting arrangements: The international IDEA handbook.
- Bellettini, G., C. B. Ceroni, E. Cantoni, C. Monfardini, and J. Schafer (2023). Modern family? the gendered effects of marriage and childbearing on voter turnout. *British Journal of Political Science* 53(3), 1016–1040.
- Berinsky, A. J. (2005). The perverse consequences of electoral reform in the United States. *American Politics Research* 33(4), 471–491.
- Bhalotra, S., U. Kambhampati, S. Rawlings, and Z. Siddique (2021). Intimate partner violence: The influence of job opportunities for men and women. *The World Bank Economic Review* 35(2), 461–479.
- Bhatti, Y., E. Fieldhouse, and K. M. Hansen (2020). It’s a group thing: How voters go to the polls together. *Political Behavior* 42(1), 1–34.

- Biggers, D. R. and M. J. Hanmer (2015). Who makes voting convenient? Explaining the adoption of early and no-excuse absentee voting in the American states. *State Politics & Policy Quarterly* 15(2), 192–210.
- Blakeslee, D., R. Chaurey, R. Fishman, D. Malghan, and S. Malik (2021). In the heat of the moment: Economic and non-economic drivers of the weather-crime relationship. *Journal of Economic Behavior & Organization* 192, 832–856.
- Bloch, F. and V. Rao (2002). Terror as a bargaining instrument: A case study of dowry violence in rural India. *American Economic Review* 92(4), 1029–1043.
- Bond, R. M., C. J. Fariss, J. J. Jones, A. D. Kramer, C. Marlow, J. E. Settle, and J. H. Fowler (2012). A 61-million-person experiment in social influence and political mobilization. *Nature* 489(7415), 295–298.
- Borusyak, K., X. Jaravel, and J. Spiess (2024). Revisiting event-study designs: robust and efficient estimation. *Review of Economic Studies* 91(6), 3253–3285.
- Box-Steffensmeier, J. M., S. De Boef, and T.-M. Lin (2004). The dynamics of the partisan gender gap. *American Political Science Review* 98(3), 515–528.
- Braconnier, C., J.-Y. Dormagen, and V. Pons (2017). Voter registration costs and disenfranchisement: experimental evidence from France. *American Political Science Review* 111(3), 584–604.
- Braghieri, L., P. Schwardmann, and E. Tripodi (2026). Talking across the aisle. *CEPR DP19753*.
- Broockman, D. and J. Kalla (2016). Durably reducing transphobia: A field experiment on door-to-door canvassing. *Science* 352(6282), 220–224.
- Callaway, B. and P. H. Sant’Anna (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics* 225(2), 200–230.
- Cameron, A. and P. Trivedi (2005). Microeconometrics: methods and applications. *Cambridge University Press*.
- Canay, I. A. (2011). A simple approach to quantile regression for panel data. *The Econometrics Journal* 14(3), 368–386.
- Cantoni, E. (2020). A precinct too far: Turnout and voting costs. *American Economic Journal: Applied Economics* 12(1), 61–85.
- Cantoni, E. and V. Pons (2021). Strict ID laws don’t stop voters: Evidence from a US nationwide panel, 2008–2018. *The Quarterly Journal of Economics* 136(4), 2615–2660.
- Cantoni, E., V. Pons, and J. Schafer (2025). Voting rules, turnout, and economic policies. *Annual Review of Economics* 17(1), 217–239.
- Card, D. and G. B. Dahl (2011). Family violence and football: The effect of unexpected emotional cues on violent behavior. *The Quarterly Journal of Economics* 126(1), 103–143.
- Cohen, F. and F. Gonzalez (2024). Understanding the link between temperature and crime. *American Economic Journal: Economic Policy* 16(2), 480–514.
- Conevska, A., S. Hirano, S. Kuriwaki, J. B. Lewis, C. Mutlu, and J. M. Snyder Jr (2026). How partisan are US local elections? evidence from 2020 cast vote records. *American Political Science Review* 120(2), 564–581.
- Cox, D. R. (1972). Regression models and life-tables. *Journal of the Royal Statistical Society: Series B (methodological)* 34(2), 187–202.

- D’Acunto, F., U. Malmendier, and M. Weber (2021). Gender roles produce divergent economic expectations. *Proceedings of the National Academy of Sciences* 118(21), e2008534118.
- Dahl, G. B., A. Kotsadam, and D.-O. Rooth (2021). Does integration change gender attitudes? the effect of randomly assigning women to traditionally male teams. *The Quarterly Journal of Economics* 136(2), 987–1030.
- De Chaisemartin, C. and X. d’Haultfoeuille (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review* 110(9), 2964–2996.
- De Chaisemartin, C. and X. d’Haultfoeuille (2026). Difference-in-differences estimators of intertemporal treatment effects. *Review of Economics and Statistics* 108(4), 863–880.
- Delavande, A., G. Kosar, and B. Zafar (2025). Information-sharing within couples: Evidence from a sequential survey of spouses. *Working Paper*.
- DellaVigna, S., J. A. List, U. Malmendier, and G. Rao (2017). Voting to tell others. *The Review of Economic Studies* 84(1), 143–181.
- Easton, M. J. and J. B. Holbein (2021). The democracy of dating: How political affiliations shape relationship formation. *Journal of Experimental Political Science* 8(3), 260–272.
- Edlund, L. and R. Pande (2002). Why have women become left-wing? The political gender gap and the decline in marriage. *The Quarterly Journal of Economics* 117(3), 917–961.
- Fehr, D., J. Mollerstrom, and R. Perez-Truglia (2024). Listen to her: Gender differences in information diffusion within the household. *Journal of Public Economics* 239, 105213.
- Fieldhouse, E. and D. Cutts (2012). The companion effect: household and local context and the turnout of young people. *The Journal of Politics* 74(3), 856–869.
- Funk, P. (2010). Social incentives and voter turnout: evidence from the Swiss mail ballot system. *Journal of the European Economic Association* 8(5), 1077–1103.
- Gerber, A. S., D. P. Green, and C. W. Larimer (2008). Social pressure and voter turnout: Evidence from a large-scale field experiment. *American Political Science Review* 102(1), 33–48.
- Gerber, A. S., G. A. Huber, and S. J. Hill (2013). Identifying the effect of all-mail elections on turnout: Staggered reform in the evergreen state. *Political Science Research and Methods* 1(1), 91–116.
- Gethin, A., C. Martínez-Toledano, and T. Piketty (2022). Brahmin left versus merchant right: Changing political cleavages in 21 western democracies, 1948–2020. *The Quarterly Journal of Economics* 137(1), 1–48.
- Glynn, A. N. and M. Sen (2015). Identifying judicial empathy: does having daughters cause judges to rule for women’s issues? *American Journal of Political Science* 59(1), 37–54.
- Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics* 225(2), 254–277.
- Gronke, P., E. Galanes-Rosenbaum, P. A. Miller, and D. Toffey (2008). Convenience voting. *Annual Review of Political Science* 11(1), 437–455.
- Gronke, P., M. S. Romero, E. Shino, and D. M. Thompson (2025). Vote-by-mail in the United States. *Journal of Election Administration, Research & Practice* 3, 35–44.
- Habets, M. (2025). Temperature variations and domestic violence. *Available at SSRN 5598432*.

- Hersh, E. and Y. Ghitza (2018). Mixed partisan households and electoral participation in the United States. *PloS One* 13(10), e0203997.
- Hobbs, W. R., N. A. Christakis, and J. H. Fowler (2014). Widowhood effects in voter participation. *American Journal of Political Science* 58(1), 1–16.
- Hodler, R., S. Luechinger, and A. Stutzer (2015). The effects of voting costs on the democratic process and public finances. *American Economic Journal: Economic Policy* 7(1), 141–171.
- Hoffman, M., G. León, and M. Lombardi (2017). Compulsory voting, turnout, and government spending: Evidence from Austria. *Journal of Public Economics* 145, 103–115.
- Huber, G. A. and N. Malhotra (2017). Political homophily in social relationships: Evidence from online dating behavior. *The Journal of Politics* 79(1), 269–283.
- Inbody, D. S. (2016). *The soldier vote: War, politics, and the ballot in America*. Springer.
- Inglehart, R. and P. Norris (2000). The developmental theory of the gender gap: Women’s and men’s voting behavior in global perspective. *International Political Science Review* 21(4), 441–463.
- Iversen, T. and F. Rosenbluth (2006). The political economy of gender: Explaining cross-national variation in the gender division of labor and the gender voting gap. *American Journal of Political Science* 50(1), 1–19.
- Iyengar, S., T. Konitzer, and K. Tedin (2018). The home as a political fortress: Family agreement in an era of polarization. *The Journal of Politics* 80(4), 1326–1338.
- Kalla, J. L. and D. E. Broockman (2020). Reducing exclusionary attitudes through interpersonal conversation: Evidence from three field experiments. *American Political Science Review* 114(2), 410–425.
- Koenker, R. (2004). Quantile regression for longitudinal data. *Journal of Multivariate Analysis* 91(1), 74–89.
- Kuriwaki, S. (2026). Ticket splitting in a nationalized era. *The Journal of Politics* 88(1), 47–62.
- Kuriwaki, S., M. Reece, S. Baltz, A. Conevska, J. R. Loffredo, C. Mutlu, T. Samarth, K. E. Acevedo Jetter, Z. Djanogly Garai, K. Murray, et al. (2024). Cast vote records: A database of ballots from the 2020 US election. *Scientific Data* 11(1), 1304.
- Leslie, E. and R. Wilson (2020). Sheltering in place and domestic violence: Evidence from calls for service during COVID-19. *Journal of Public Economics* 189, 104241.
- Machado, J. A. and J. Santos Silva (2019). Quantiles via moments. *Journal of Econometrics* 213(1), 145–173.
- Marchese, A. and F. Schönenberger (2026). Gender norms, social pressure, and the gender gap in turnout: Evidence from Swiss elections. *Working paper*.
- Masi, O. and C. Santantonio (2026). Overstretched: Financial distress and intimate partner violence in the US. *Journal of Health Economics*, 103124.
- Melly, B. and M. Pons (2025). Minimum distance estimation of quantile panel data models. *arXiv preprint arXiv:2502.18242*.
- Meredith, M., M. Morse, A. Madarang, and K. Steele (2024). Measuring lost votes by mail. *Science Advances* 10(51), eadr2225.
- MIT Voting Lab (2024). Voting by mail and absentee voting. Accessed: 2025-01-12.

- Nickerson, D. W. (2008). Is voting contagious? Evidence from two field experiments. *American Political Science Review* 102(1), 49–57.
- Oswald, A. J. and N. Powdthavee (2010). Daughters and left-wing voting. *The Review of Economics and Statistics* 92(2), 213–227.
- Parente, P. M. and J. M. Santos Silva (2016). Quantile regression with clustered data. *Journal of Econometric Methods* 5(1), 1–15.
- Pons, V. (2018). Will a five-minute discussion change your mind? A countrywide experiment on voter choice in france. *American Economic Review* 108(6), 1322–1363.
- Rambachan, A. and J. Roth (2023). A more credible approach to parallel trends. *Review of Economic Studies* 90(5), 2555–2591.
- Rizzotto, J. S., K. M. Sims, and H. K. Gibbs (2025). Hot tempers: Differential effects of heat and drought on domestic violence. *Review of Economics of the Household*, 1–26.
- Ronchi, M. and N. Smith (2026). Daddy’s girl: Daughters, managerial decisions, and gender inequality. *The Review of Economic Studies Forthcoming*.
- Schneider, D., K. Harknett, and S. McLanahan (2016). Intimate partner violence in the great recession. *Demography* 53(2), 471–505.
- Stoker, L. and M. K. Jennings (1995). Life-cycle transitions and political participation: The case of marriage. *American Political Science Review* 89(2), 421–433.
- Stoker, L. and M. K. Jennings (2005). Political similarity and influence between husbands and wives. In A. S. Zuckerman (Ed.), *The Social Logic of Politics: Personal Networks as Contexts for Political Behavior*, pp. 51–74. Philadelphia: Temple University Press.
- Sun, L. and S. Abraham (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics* 225(2), 175–199.
- Tausanovitch, C. and C. Warshaw (2013). Measuring constituent policy preferences in Congress, state legislatures, and cities. *The Journal of Politics* 75(2), 330–342.
- Thompson, D. M., J. A. Wu, J. Yoder, and A. B. Hall (2020). Universal vote-by-mail has no impact on partisan turnout or vote share. *Proceedings of the National Academy of Sciences* 117(25), 14052–14056.
- U.S. Census Bureau (2021). Voting and registration in the election of November 2020. Accessed: 2025-01-12.
- Warshaw, C. and C. Tausanovitch (2022). Subnational ideology and presidential vote estimates (v2022). *Harvard Dataverse*.
- Washington, E. L. (2008). Female socialization: How daughters affect their legislator fathers’ voting on women’s issues. *American Economic Review* 98(1), 311–332.
- Yoder, J., C. Handan-Nader, A. Myers, T. Nowacki, D. M. Thompson, J. A. Wu, C. Yorgason, and A. B. Hall (2021). How did absentee voting affect the 2020 US election? *Science Advances* 7(52), eabk1755.
- Zuckerman, A. S., J. Dasovic, and J. Fitzgerald (2007). *Partisan families: The Social Logic of Bounded Partisanship in Germany and Britain*. Cambridge University Press.
- Zuckerman, A. S., J. Fitzgerald, and J. Dasović (2005). Do couples support the same political parties? Sometimes: Evidence from British and German household panel surveys. In A. S. Zuckerman (Ed.), *The Social Logic of Politics: Personal Networks as Contexts for Political Behavior*, pp. 75–94. Philadelphia: Temple University Press.

Appendix

A Background information and summary statistics

A.1 Mail-voting systems across the United States

Table A1: History of mail-voting legislation across states, until 2024 elections - 1/2

State	Mail voting current system	First election-year implemented	Legislation
Alabama	Excuse needed	2020 only	Sec. of State Emergency Rule 7/17/2020
Alaska	No-excuse absentee	1996	1995 Alaska HB 21
Arizona	No-excuse absentee	1992 (no-excuse) 2008 (permanent absentee list)	1991 Ariz. SB 1320 2007 Ariz. HB 2106
Arkansas	Excuse needed	2020 only	Ark. Exec. Order No. 20-40 8/7/2020
California	All-mail elections	1980 (no-excuse) 2002 (permanent absentee list) 2018 (all-mail, opt-in), 2020 - temporary, 2022 (all-mail, statewide)	1978 Cal AB 1699 2001 Cal AB 1520 2016 Cal SB 450, 2020 Cal AB 860, 2021 Cal AB 37
Colorado	All-mail elections	1992 (no-excuse), 2007 (permanent absentee list) 2013 (all-mail)	1992 Colo. HB 92-1004 2007 Colo. SB 234 2013 Colo. HB 1303
Connecticut	Excuse needed	2020 only	2020 Conn. HB 6002
Delaware	Excuse needed	2020 only	2020 Del. HB 346
Florida	No-excuse absentee	2002	2001 Fla. SB 1118
Georgia	No-excuse absentee	2006	2005 Ga. HB 244
Hawaii	All-mail elections	1994 (no-excuse) 2008 (permanent absentee list) 2020 (all-mail)	1993 Haw. HB 620 2008 Haw. SB 156 2019 Haw. HB 1248
Idaho	No-excuse absentee	1970	1970 Idaho Sess. Laws 140
Illinois	No-excuse absentee	2010 (no-excuse) 2022 (permanent absentee list)	2009 Ill. SB 2022 2021 Ill. SB 825
Indiana	Excuse needed		
Iowa	No-excuse absentee	1990	1990 Iowa HF 2329
Kansas	No-excuse absentee	1996	1995 Kan. SB 232
Kentucky	Excuse needed	2020 only	Ky. Exec. Order No. 2020-688
Louisiana	Excuse needed		
Maine	No-excuse absentee	2000	PL 1999, c. 645, Sec. 4
Maryland	No-excuse absentee	2006 (no-excuse) 2022 (permanent absentee list)	2005 Md. HB 622 2021 Md. SB 683
Massachusetts	No-excuse absentee	2020 - temporary, 2022 (no-excuse)	2020 Mass. HB 4820, 2022 Mass. S. 2924
Michigan	No-excuse absentee	2020 (no-excuse) 2024 (permanent absentee list)	2018 Mich. Proposal 18-3 2023 Mich. SB 370
Minnesota	No-excuse absentee	2014 (no-excuse) 2024 (permanent absentee list)	2013 Minn. HF 894 2023 Minn. HF 3
Mississippi	Excuse needed		
Missouri	Excuse needed	2020 only	2020 Mo. SB 631

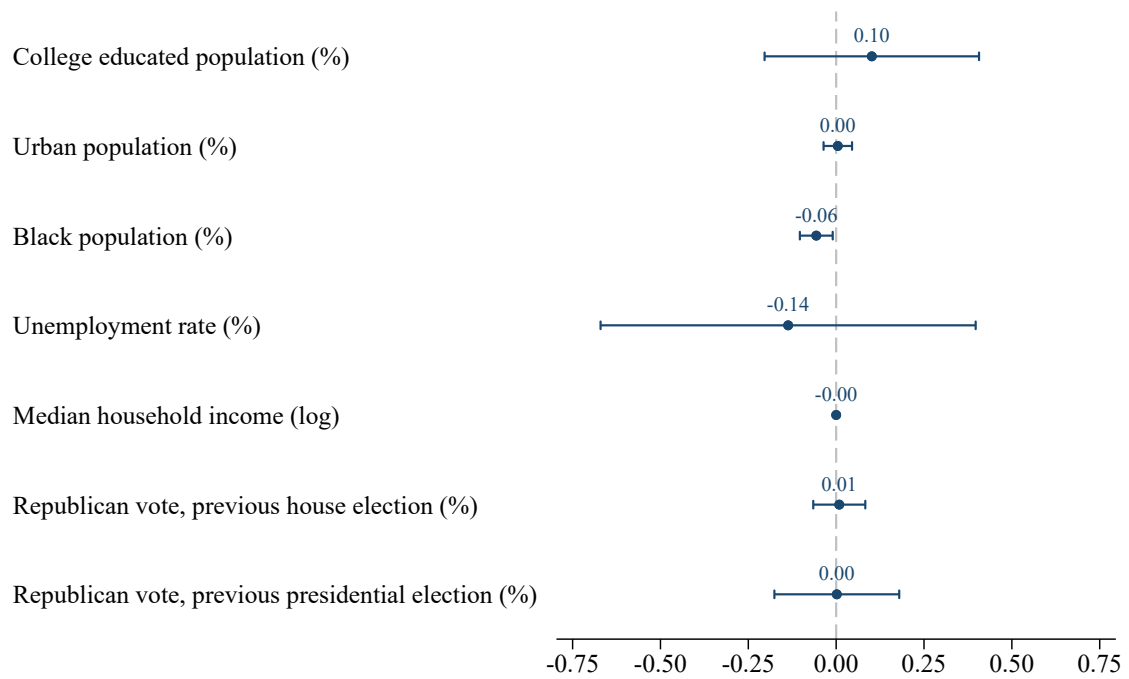
Notes: This table records information on mail voting regimes in the 2024 elections across US states and reports the implementing legislative act for each change of mail voting regime.

Table A1: History of mail-voting legislation across states, until 2024 elections - 2/2

State	Mail voting current system	First election-year implemented	Legislation
Montana	No-excuse absentee	2000 (no-excuse)	1999 Mont. SB 151
		2006 (permanent absentee list)	2005 Mont. SB 88
Nebraska	No-excuse absentee	2000	1999 Neb. LB 571
Nevada	All-mail elections	1992 (no-excuse)	1991 Nev. AB 652
		2020-temporary, 2022 (all-mail)	2020 Nev. AB 4; 2021 Nev. AB 321
New Hampshire	Excuse needed	2020 only	2020 N.H. HB 1266
New Jersey	No-excuse absentee	2005 (no-excuse)	2004 N.J. A. 35
		2009 (permanent absentee list)	2008 N.J. A. 2451
		2020 (all-mail, temporary)	2020 N.J. A. 4475
New Mexico	No-excuse absentee	1994 (no-excuse)	1993 N.M. SB 51
		2024 (permanent absentee list)	2023 N.M. HB 4
New York	No-excuse absentee	2020-temporary, 2024 (no-excuse)	2020 N.Y. SB 8015D, 2023 N.Y. S. 7394-A
North Carolina	No-excuse absentee	2002	2001 N.C. HB 977
North Dakota	No-excuse absentee	1998	1997 N.D. SB 2151
Ohio	No-excuse absentee	2006	2005 Ohio HB 234
Oklahoma	No-excuse absentee	1992	1992 Okla. SB 653
Oregon	All-mail elections	2000	1998 Or. Ballot Measure 60
Pennsylvania	No-excuse absentee	2020	2019 Pa. SB 421
Rhode Island	No-excuse absentee	2012	2011 R.I. HB 5748
South Carolina	Excuse needed	2020 only	2020 S.C. HB 5305
South Dakota	No-excuse absentee	2004	2003 S.D. HB 1176
Tennessee	Excuse needed		
Texas	Excuse needed		
Utah	All-mail elections	2004 (no-excuse)	2004 Utah HB 9
		2012 (all-mail, opt-in) 2020 (all-mail, statewide)	2012 Utah HB 172, 2020 Utah HB 36
Vermont	All-mail elections	1994 (no-excuse)	1993 Vt. Acts No. 79
		2020-temporary, 2022 (all-mail)	2020 Vt. H. 681, 2021 Vt. S. 15
Virginia	No-excuse absentee	2020 (no-excuse)	2020 Va. HB 1
		2020 (permanent absentee list)	2020 Va. HB 207
Washington	All-mail elections	1974 (no-excuse)	1974 Wash. Sess. Laws 1st Ex. Sess. ch. 35
		1994 (permanent absentee list)	1993 Wash. HB 1646
		2005 (all-mail, opt-in) 2012 (all-mail, statewide)	2005 Wash. ESSB 5499, 2011 Wash. SB 5124
Washington D.C.	All-mail elections	2010 (no-excuse)	2009 D.C. Law 18-103
		2020 & 2022-temporary, 2024 (all-mail)	D.C. Act 23-422, 2022 D.C. Law 24-342
West Virginia	Excuse needed	2020 only	Sec. of State Emergency rule 7/27/2020
Wisconsin	No-excuse absentee	2000	1999 Wis. Act 182
Wyoming	No-excuse absentee	1992	1991 Wyo. SF 118

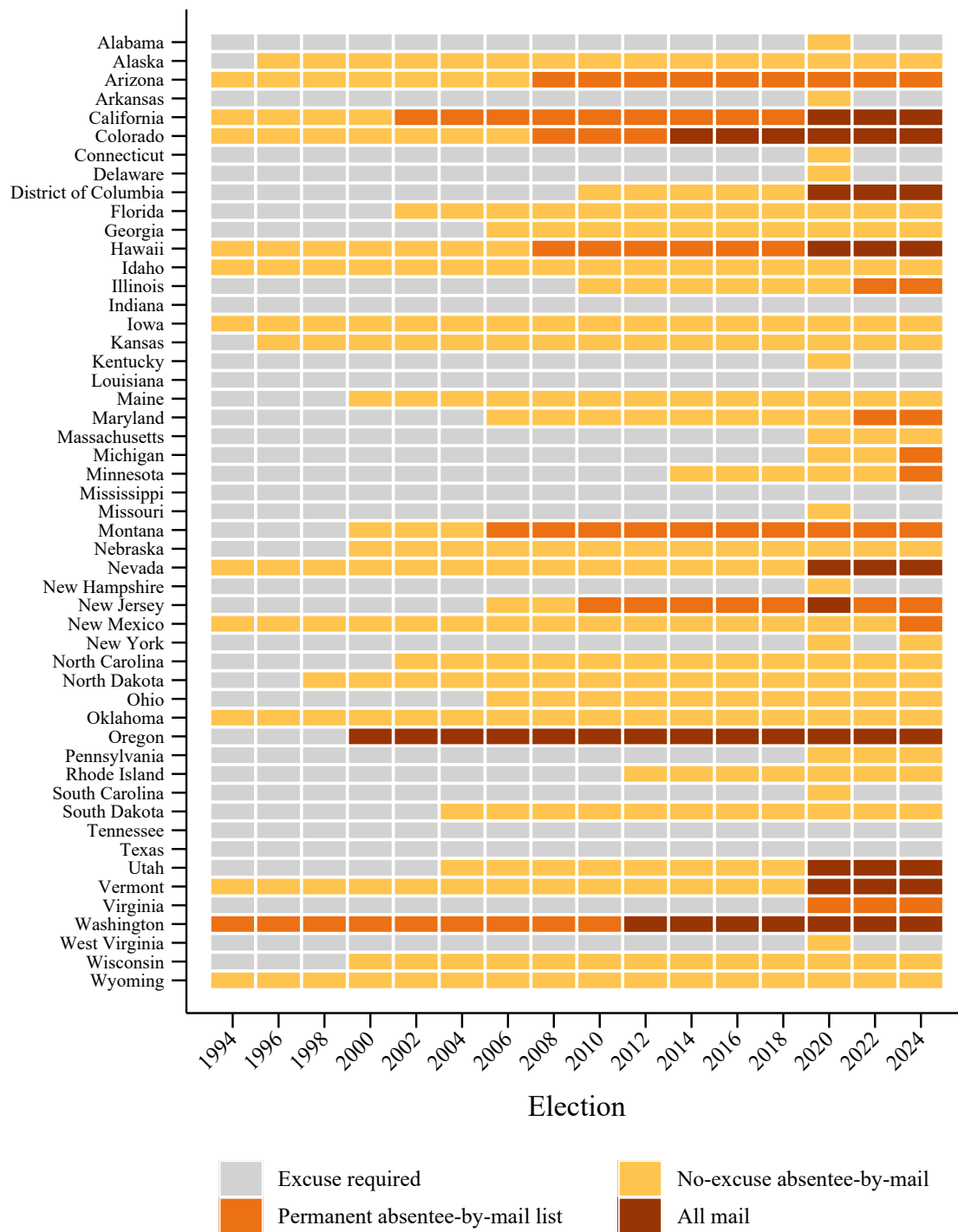
Notes: This table records information on mail voting regimes in the 2024 elections across US states and reports the implementing legislative act for each change of mail voting regime.

Figure A1: Survival analysis of drivers of no-excuse mail voting adoption



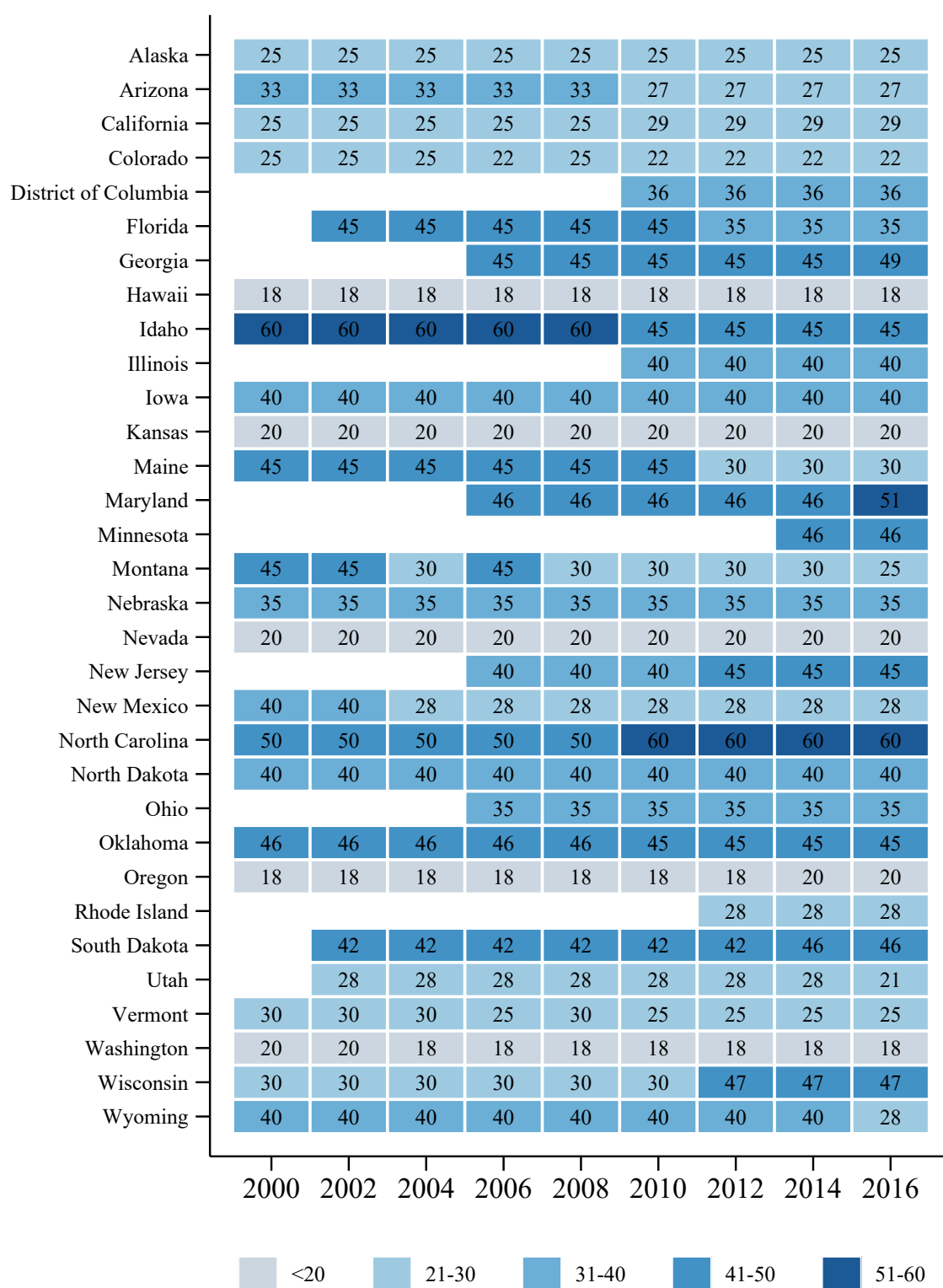
Notes: This figure presents Cox proportional-hazards estimates of drivers of no-excuse mail voting adoption. The unit of analysis is a state $\times$ election-year. Failure corresponds to adopting no-excuse mail voting. Standard errors are clustered at the state level. Bars denote 95% confidence intervals.

Figure A2: Mail voting rules in place by state: 1994-2024



Notes: This figure plots the evolution of the mail voting regime in each state between 1994 and 2024. States are coded as all mail once legislation has been adopted requiring all mail elections statewide.

Figure A3: Length of mail voting period by state: 2000-2016



Notes: This figure reports the evolution of the length of the mail period in general elections between 2000 and 2016. The figure is limited to states offering no-excuse mail voting over the period. Numbers in cells denote the earliest date mentioned in electoral legislation at which ballots can be distributed to voters. Empty cells indicate that no-excuse mail voting was not available in the given state-election.

A.2 Summary statistics of analysis variables

Table A2: Summary statistics of CPS data and mail voting take-up

Variable	Obs.	Mean	S.D.	Min	Max
Individual-level					
No-excuse mail voting is available	876386	0.50	0.50	0	1
Early in-person voting is available	876386	0.63	0.48	0	1
Voted	876386	0.61	0.49	0	1
Voted by mail	876386	0.09	0.29	0	1
In a couple	876386	0.67	0.47	0	1
Woman	876386	0.52	0.50	0	1
Age	876386	42.19	12.19	18	64
White (non-hispanic)	876386	0.77	0.42	0	1
Black (non-hispanic)	876386	0.10	0.30	0	1
Hispanic	876386	0.08	0.27	0	1
Of other ethnicity	876386	0.05	0.23	0	1
Education: no high school degree	876386	0.08	0.28	0	1
Education: high school degree	876386	0.30	0.46	0	1
Education: some college education	876386	0.30	0.46	0	1
Education: 4-year college degree or more	876386	0.32	0.47	0	1
At least one under 18 child in household	876386	0.44	0.50	0	1
Currently employed	876386	0.77	0.42	0	1
Couple-level					
No-excuse mail voting is available	278053	0.50	0.50	0	1
Early in-person voting is available	278053	0.63	0.48	0	1
Number who voted	278053	1.34	0.85	0	2
Neither voted	278053	0.25	0.43	0	1
One voted	278053	0.16	0.37	0	1
Both voted	278053	0.59	0.49	0	1
Number who voted by mail	278053	0.19	0.55	0	2
Neither voted by mail	278053	0.89	0.32	0	1
One voted by mail	278053	0.04	0.20	0	1
Both voted by mail	278053	0.07	0.26	0	1
Neither voted by mail (cond. both voted)	163491	0.83	0.37	0	1
One voted by mail (cond. both voted)	163491	0.04	0.20	0	1
Both voted by mail (cond. both voted)	163491	0.13	0.33	0	1
Age of male partner	278053	44.67	11.18	18	64
Age of female partner	278053	42.66	11.07	18	64
Male partner has some college education	278053	0.62	0.49	0	1
Female partner has some college education	278053	0.65	0.48	0	1
Both are white (non-hispanic)	278053	0.79	0.41	0	1
Both are black (non-hispanic)	278053	0.05	0.22	0	1
Both are hispanic	278053	0.04	0.20	0	1
Both are of other ethnicities	278053	0.03	0.18	0	1
Partners are of mixed ethnicities	278053	0.08	0.28	0	1
At least one under 18 child in household	278053	0.53	0.50	0	1
Both are currently employed	278053	0.66	0.47	0	1

Notes: This table presents descriptive statistics of all variables used in the analysis of mail voting take-up, based on CPS data. The unit of observation is either an individual (top panel) or a couple (bottom panel).

Table A3: Summary statistics of variables used in within-campaign analysis

Variable	Obs.	Mean	S.D.	Min	Max
Mail period	70975	0.10	0.31	0	1
Days to election	70975	-58.41	33.07	-120	-1
Days discussed politics, with family and friends	70975	3.08	2.55	0	7
Days discussed politics, at work	53880	1.49	2.06	0	7
Days discussed politics, online	23976	0.50	1.40	0	7
Interest in government affairs (4-point scale)	43479	1.94	0.97	0	3
Interest in current campaign (3-point scale)	50759	1.08	0.75	0	2
In a couple	70975	0.64	0.48	0	1
Woman	70975	0.55	0.50	0	1
Age	70975	42.92	12.29	18	64
White (non hispanic)	70975	0.78	0.42	0	1
Black (non hispanic)	70975	0.09	0.28	0	1
Hispanic	70975	0.08	0.28	0	1
Of other ethnicity	70975	0.05	0.22	0	1
Education: high school degree or less	70975	0.31	0.46	0	1
Education: associate's degree (2 years) or some college	70975	0.29	0.45	0	1
Education: bachelor's degree (4 years)	70975	0.25	0.43	0	1
Education: postgraduate degree	70975	0.15	0.36	0	1
Currently in full-time employment	70975	0.66	0.47	0	1
Children under 18 in household	70975	0.45	0.50	0	1
Attends church monthly	70975	0.45	0.50	0	1

Notes: This table presents descriptive statistics of all variables used in the analysis of political interactions and interest, based on NAES data. The unit of observation is an individual surveyed within 120 days to the election.

Table A4: Summary statistics of variables used in across-electoral cycles analysis

Variable	Obs.	Mean	S.D.	Min	Max
Mail vote	428773	0.61	0.49	0	1
Partisan identification (7-point scale)	428773	3.64	2.17	1	7
Policy views index	329398	-0.05	0.93	-2.39	3.50
Knows majority in state lower chamber	407547	0.64	0.48	0	1
Knows majority in state upper chamber	407547	0.63	0.48	0	1
Knows party of representative in congress	407547	0.64	0.48	0	1
Knows party of governor	407547	0.78	0.42	0	1
Days to election	428773	-19.90	9.45	-50	-1
In a couple	428773	0.57	0.50	0	1
Woman	428773	0.54	0.50	0	1
Age	428773	44.06	13.40	18	64
White (non hispanic)	428773	0.71	0.45	0	1
Black (non hispanic)	428773	0.12	0.33	0	1
Hispanic	428773	0.09	0.29	0	1
Of other ethnicity	428773	0.08	0.27	0	1
Education: high school degree or less	428773	0.28	0.45	0	1
Education: associate's degree (2 years) or some college	428773	0.35	0.48	0	1
Education: bachelor's degree (4 years)	428773	0.24	0.43	0	1
Education: postgraduate degree	428773	0.13	0.33	0	1
Currently in full-time employment	428773	0.50	0.50	0	1

Notes: This table presents descriptive statistics of all variables used in the analysis of political attitudes across election cycles, based on CCES and NAES data. The unit of observation is an individual.

Table A5: Summary statistics of couples' absentee ballots in the 2024 general election in Georgia

Variable	Obs.	Mean	S.D.	Min	Max
Days between request dates, both by mail	42472	8.87	33.30	0	289
Days between return dates, both by mail	39909	0.99	2.96	0	49
Days between request dates, both in person	714300	1.89	3.58	0	238
Days between return dates, both in person	714297	1.89	3.57	0	31
Days between request dates, mixed	35957	44.25	63.01	0	305
Days between return dates, mixed	32360	6.24	5.84	0	48

Notes: This table presents descriptive statistics of absentee ballots request and return dates in the 2024 general election in Georgia. The unit of observation is a pair of absentee ballots identified as a couple.

Table A6: Summary statistics of Cast Vote Records

Variable	Obs.	Mean	S.D.	Min	Max
2019 Municipal elections					
Cast by mail	408269	0.72	0.45	0	1
Cast in person on election day	408269	0.28	0.45	0	1
Number of contests on card	408269	7.13	1.20	6	15
Number of identical cards	408269	636.46	1314.06	0	6872
Has a single duplicate citywide	408269	0.03	0.18	0	1
Has a single duplicate citywide which is in same precinct	408269	0.01	0.07	0	1
2020 Presidential Primaries					
Cast by mail	304940	0.65	0.48	0	1
Cast in person on election day	304940	0.35	0.48	0	1
Number of contests on card	304940	12.00	0.00	12	12
Number of identical cards	304940	203.87	426.87	0	2169
Has a single duplicate citywide	304940	0.08	0.27	0	1
Has a single duplicate citywide which is in same precinct	304940	0.01	0.11	0	1

Notes: This table presents descriptive statistics of Cast Vote Records from the 2019 municipal and 2020 presidential primary elections in San Francisco county (California). The unit of observation is a ballot card.

Table A7: Summary statistics on violent incidents between partners and covariates used in the analysis

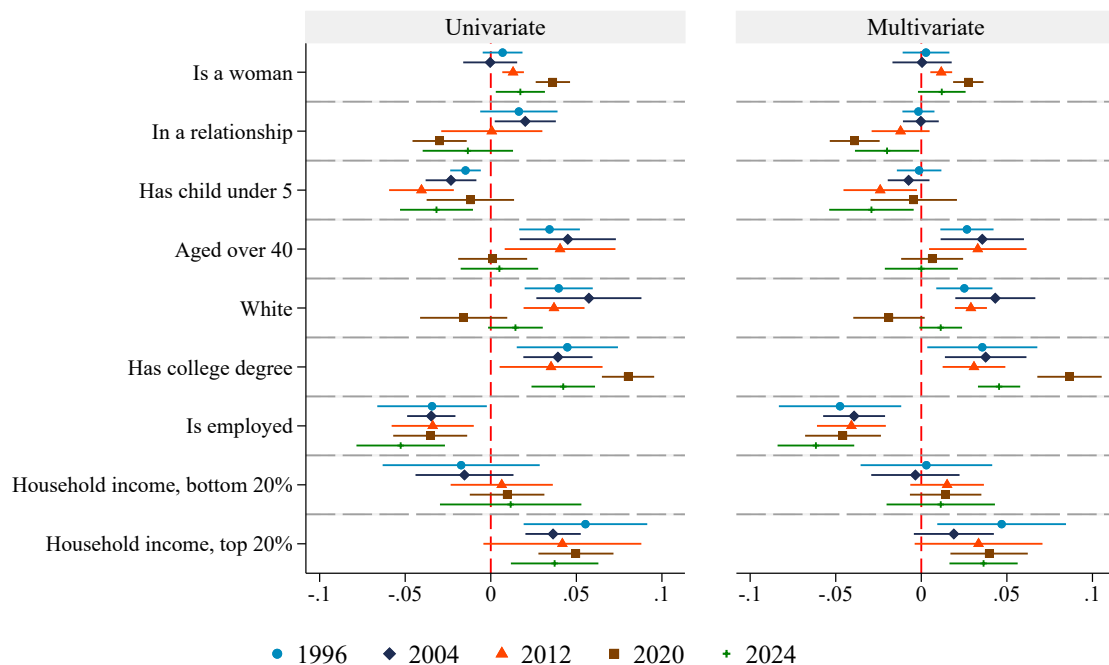
Variable	Obs.	Mean	S.D.	Min	Max
At least one incident					
Any	5475847	0.21	0.41	0	1
Victim is a woman	5475847	0.19	0.39	0	1
Victim is a man	5475847	0.07	0.26	0	1
Victim is aged 18 to 29	5475847	0.13	0.34	0	1
Victim is aged 30 to 39	5475847	0.09	0.29	0	1
Victim is aged 40 to 49	5475847	0.06	0.24	0	1
Victim is aged 50 to 59	5475847	0.03	0.17	0	1
Victim is aged 60 or more	5475847	0.01	0.09	0	1
Victim is white	5475847	0.18	0.39	0	1
Victim is not white	5475847	0.07	0.26	0	1
Offender is white	5475847	0.18	0.38	0	1
Offender is not white	5475847	0.08	0.28	0	1
Both are white	5475847	0.17	0.38	0	1
Neither are white	5475847	0.06	0.24	0	1
Incident occurent at home	5475847	0.19	0.39	0	1
Total number of incidents					
Any	5475847	0.52	1.87	0	70
Victim is a woman	5475847	0.42	1.57	0	62
Victim is a man	5475847	0.10	0.44	0	18
Victim is aged 18 to 29	5475847	0.25	1.01	0	44
Victim is aged 30 to 39	5475847	0.14	0.60	0	24
Victim is aged 40 to 49	5475847	0.09	0.40	0	16
Victim is aged 50 to 59	5475847	0.03	0.21	0	9
Victim is aged 60 or more	5475847	0.01	0.10	0	5
Victim is white	5475847	0.35	1.04	0	32
Victim is not white	5475847	0.17	1.18	0	60
Offender is white	5475847	0.33	0.97	0	30
Offender is not white	5475847	0.19	1.25	0	63
Both are white	5475847	0.31	0.92	0	29
Neither are white	5475847	0.15	1.15	0	60
Incident occurent at home	5475847	0.42	1.54	0	60
Other variables					
During mail voting period	5475847	0.02	0.15	0	1
Maximum daily temperature (celcius)	5475847	17.86	11.60	-29	46
Daily precipitation (mm)	5475847	2.67	7.21	0	331
Date is a holiday	5475847	0.03	0.18	0	1

Notes: This table presents descriptive statistics on violent incidents within couples and other variables used in the analysis from 01/01/2007 to 31/12/2016. The unit of observation is a county-day.

B Mail voting take-up: details and additional analyses

B.1 Characteristics of mail voters

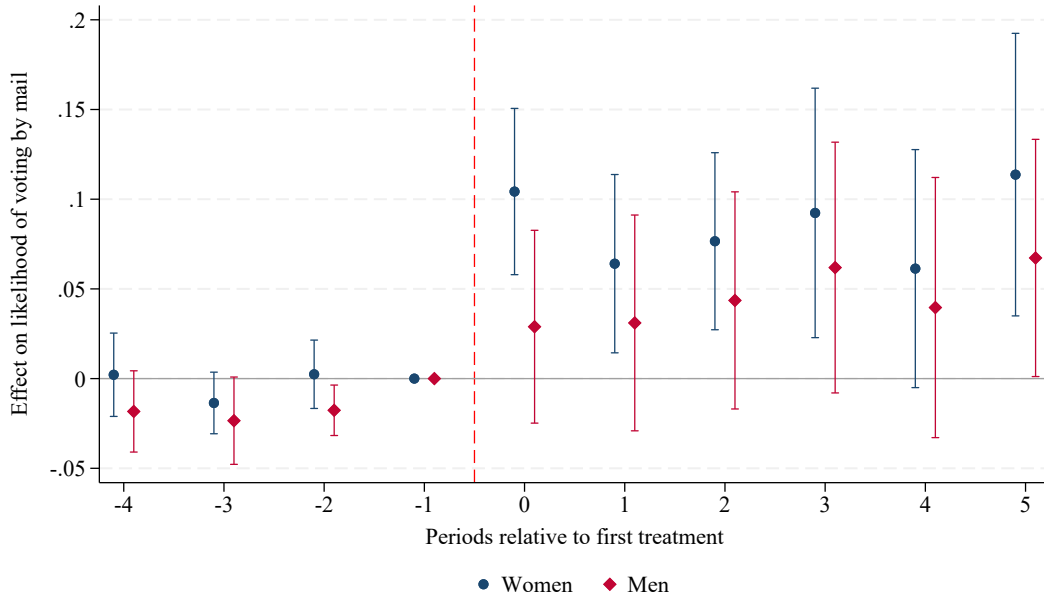
Figure B1: Characteristics of mail voters across elections



Notes: This figure presents OLS estimates of the association between demographic characteristics and voting by mail. The unit of observation is a CPS respondent aged 18 to 64 who reports having voted in the corresponding election. The outcome is an indicator variable equal to 1 if the respondent declares having voted by mail. The left panel reports coefficients of univariate regressions in which each corresponding characteristics enters alone. The right panel reports coefficients of multivariate regression where all reported characteristics are included together. Estimation is successively restricted to years 1996 (light blue circle markers), 2004 (dark blue diamonds), 2012 (orange triangles), 2020 (brown squares) and 2024 (green cross). All estimations include state fixed effects. Standard errors are clustered at the state-level. Bars denote 95% confidence intervals.

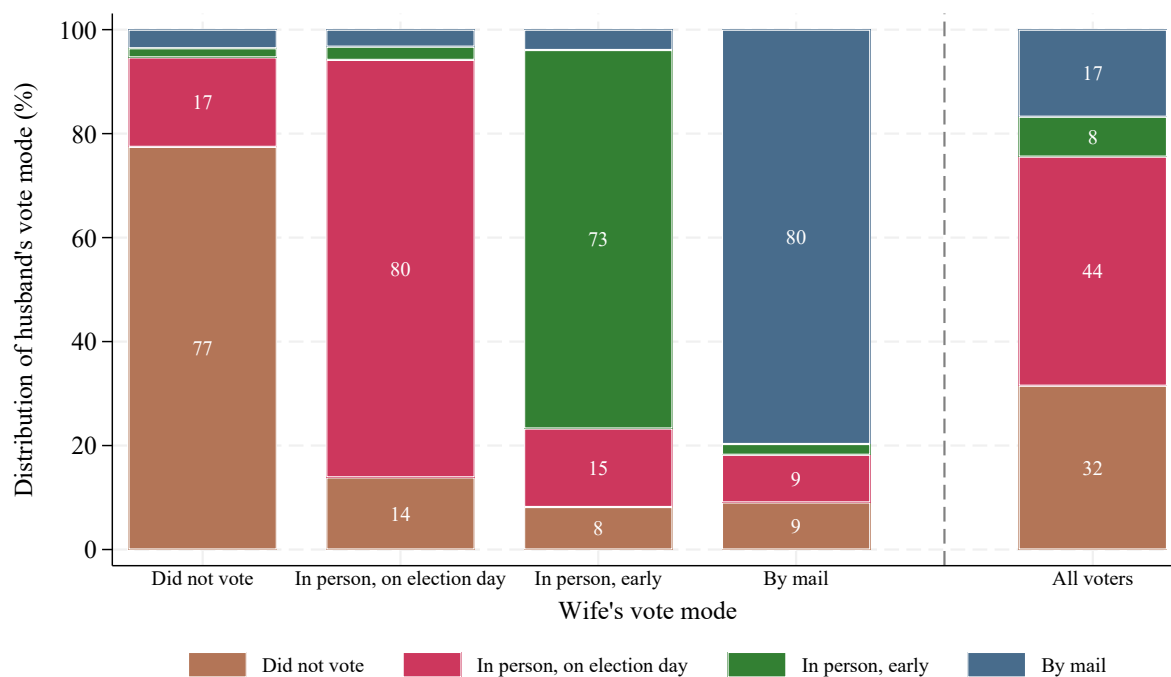
B.2 Additional results from the Current Population Survey

Figure B2: Effects of no-excuse mail voting introduction on take-up by singles



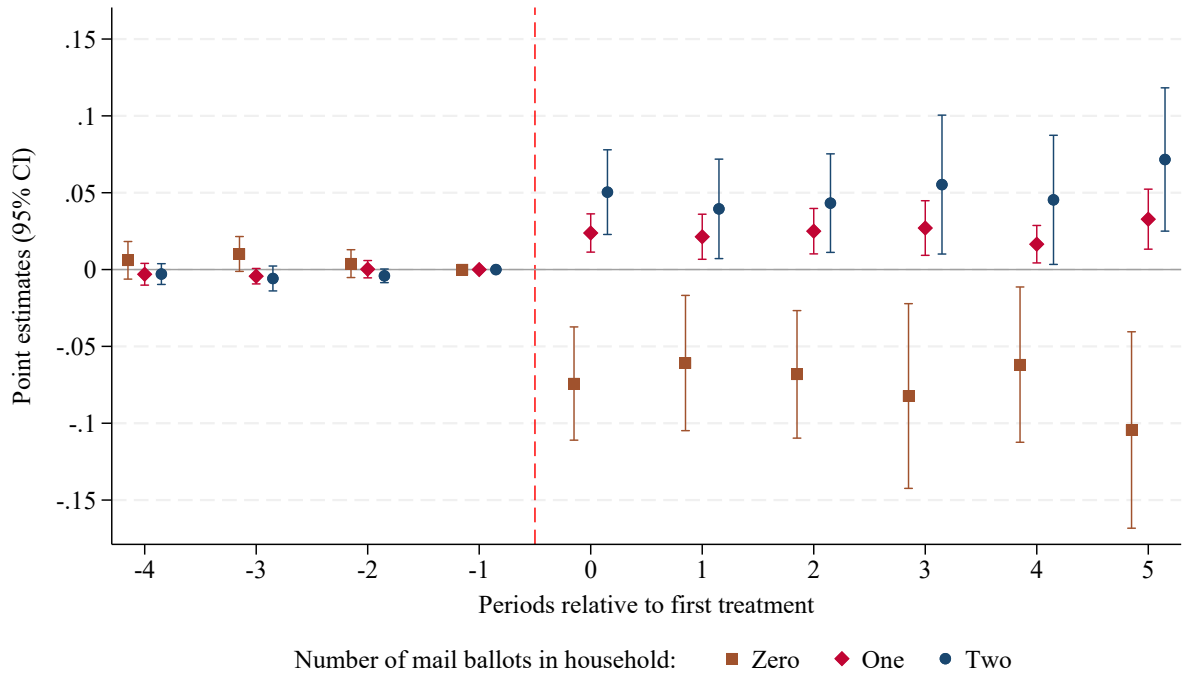
Notes: This figure presents dynamic difference-in-differences estimates of the effect of no-excuse mail voting introduction on mail voting take-up by married -or living as such- voters. Estimation is performed using the methodology of [De Chaisemartin and d'Haultfoeuille \(2026\)](#). The unit of observation is a single individual aged 18 to 64. The dependent variable is an indicator variable equal to 1 if the respondent declares having voted by mail. Estimation is performed separately for women (blue circular marker) and men (red diamond markers). All estimations control for race, age and its square, education, employment, the presence of an underage child at home, and the availability of in-person early voting. Standard errors are clustered at the state level. Bars denote 95% confidence intervals.

Figure B3: Distribution of couples' joint vote mode



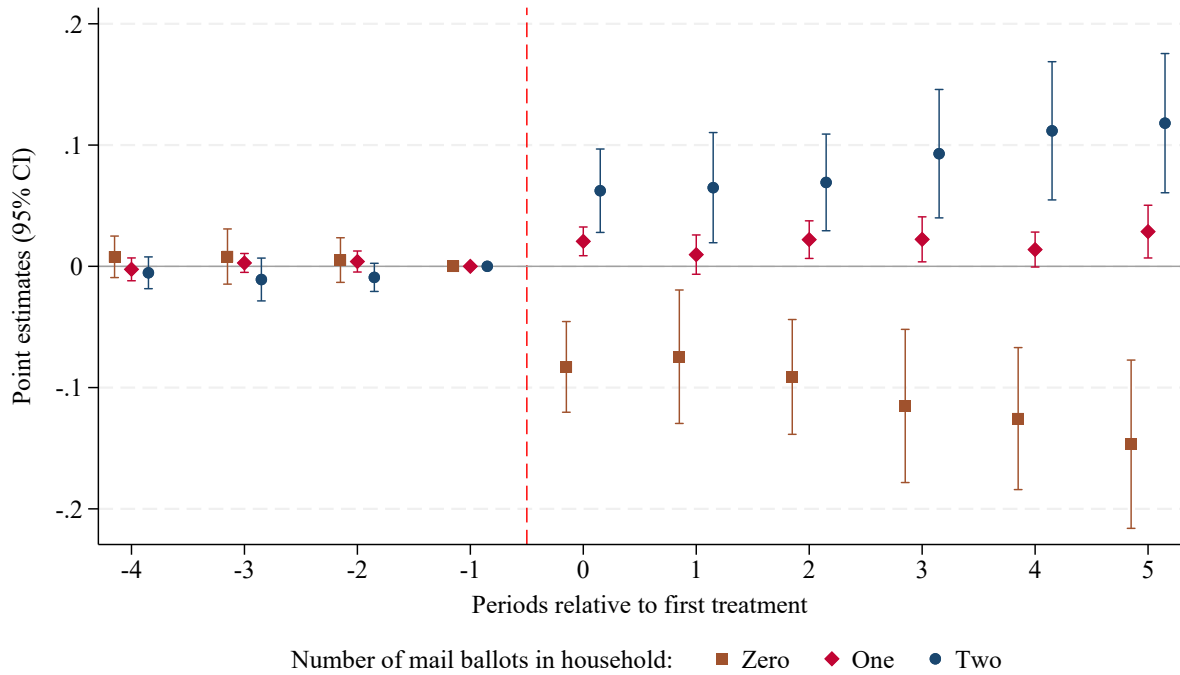
Notes: This figure presents the joint distribution of vote modes of couples. The unit of observation is a couple in which both partners are aged 18 to 64. For each voting method used by the female partner (x-axis), the y-axis reports the share of couples in which the male partner used the corresponding voting method. Voting methods are either: did not vote (brown), in-person on election day (red), in-person early (green), by mail (blue). Bar labeled “All voters” reports the distribution of voting method among all in-sample individuals.

Figure B4: Effects of no-excuse mail voting on mail ballots cast by couples



Notes: This figure presents dynamic difference-in-differences estimates of the effect of no-excuse mail voting introduction on the number of mail ballots cast among couples. Estimation is performed using the methodology of [De Chaisemartin and d'Haultfoeuille \(2026\)](#). The unit of observation is a couple in which both partners are aged 18 to 64. The dependent variable is an indicator variable equal to 1 if zero (brown square markers), one (red diamond markers) or two (blue circle markers) ballots were cast by mail. All estimations control for partners' race, age and their squares, education, employment, the presence of an underage child at home, and the availability of in-person early voting. Standard errors are clustered at the state level. Bars denote 95% confidence intervals.

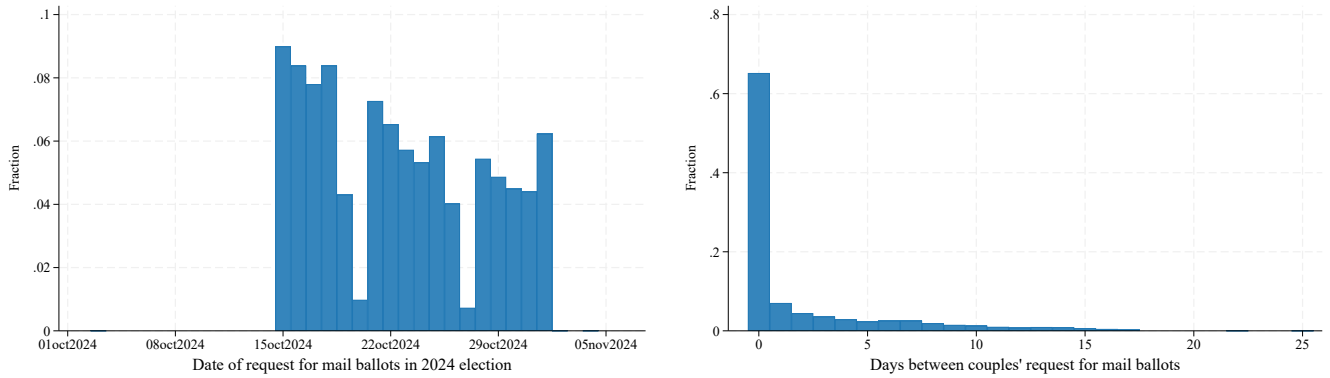
Figure B5: Effects of mail voting rules on mail ballots cast by two-voter couples: accounting for expansions beyond the first law



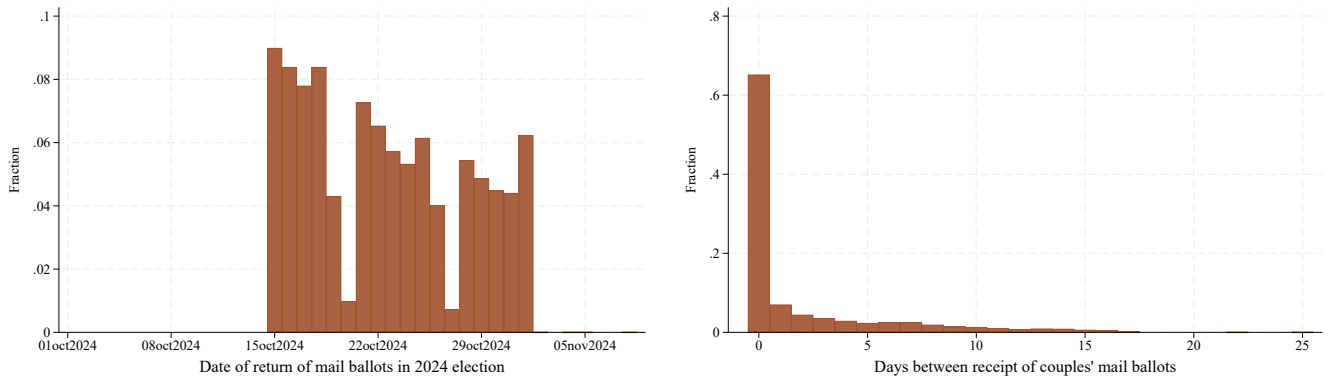
Notes: This figure presents dynamic difference-in-differences estimates of the effect of no-excuse mail voting laws on the number of mail ballots cast among two-voter couples. Estimation is performed using the methodology of [De Chaisemartin and d'Haultfoeuille \(2026\)](#). The unit of observation is a couple in which both partners are aged 18 to 64 and report having voted in the last November general election. Treatment is defined as a scale capturing the accessibility of mail ballots, where 1 corresponds to allowing no-excuse absentee voting, 2 if a permanent absentee list exists and 3 if mail-ballots are sent to all voters. State-years which allow none of these are coded as 0. The dependent variable is an indicator variable equal to 1 if zero (brown square markers), one (red diamond markers) or two (blue circle markers) ballots were cast by mail. All estimations control for partners' race, age and their squares, education, employment, the presence of an underage child at home, and the availability of in-person early voting. Standard errors are clustered at the state level. Bars denote 95% confidence intervals.

B.3 Absentee ballots in the states of Georgia and North Carolina

Figure B6: Timing of couples' in-person early ballots requests and returns during the 2024 general election in Georgia



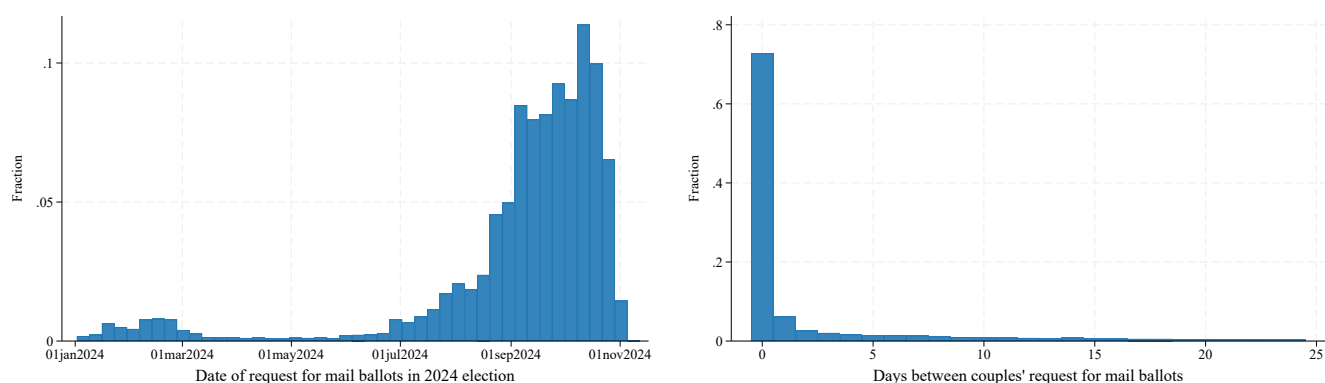
(a) Absentee in-person ballot requests



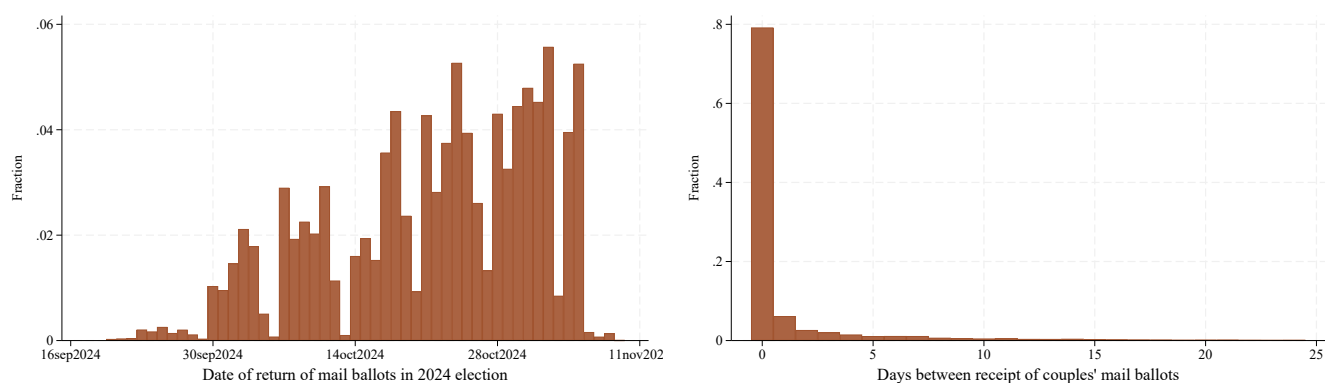
(b) Absentee in-person ballots returns

Notes: This figure describes the timing of requests and returns of in-person absentee ballots during the 2024 general election in the state of Georgia by identified couples. The unit of observation is either an in-person early ballot (left figures) or a pair of in-person early ballots identified as coming from a couple (right figures). Both panels are identical since in-person requests and returns are filed together when present at the poll.

Figure B7: Timing of couples' mail ballots requests and returns during the 2024 general election in North Carolina



(a) Absentee-by-mail ballot requests



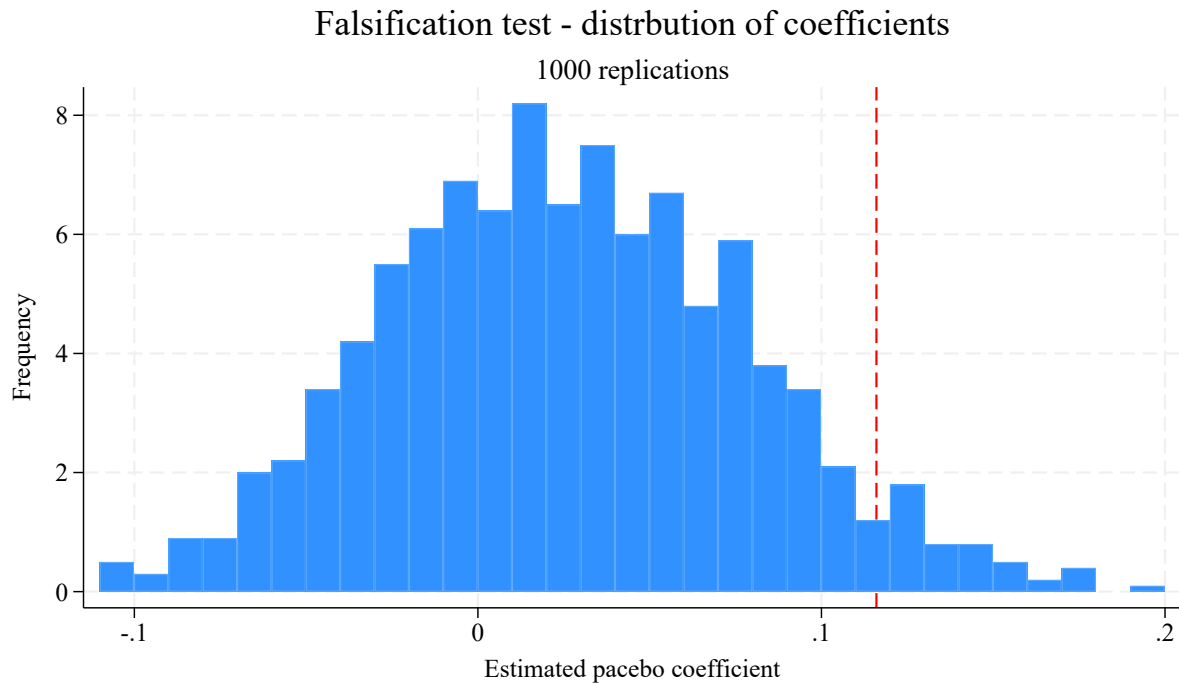
(b) Absentee-by-mail ballots returns

Notes: This figure describes the timing of requests and returns of absentee ballots during the 2024 general election in the state of North Carolina by identified couples. The unit of observation is either a mail ballot (left figures) or a pair of mail ballots identified as coming from a couple (right figures).

C Mail voting and couples' political interactions: additional analyses

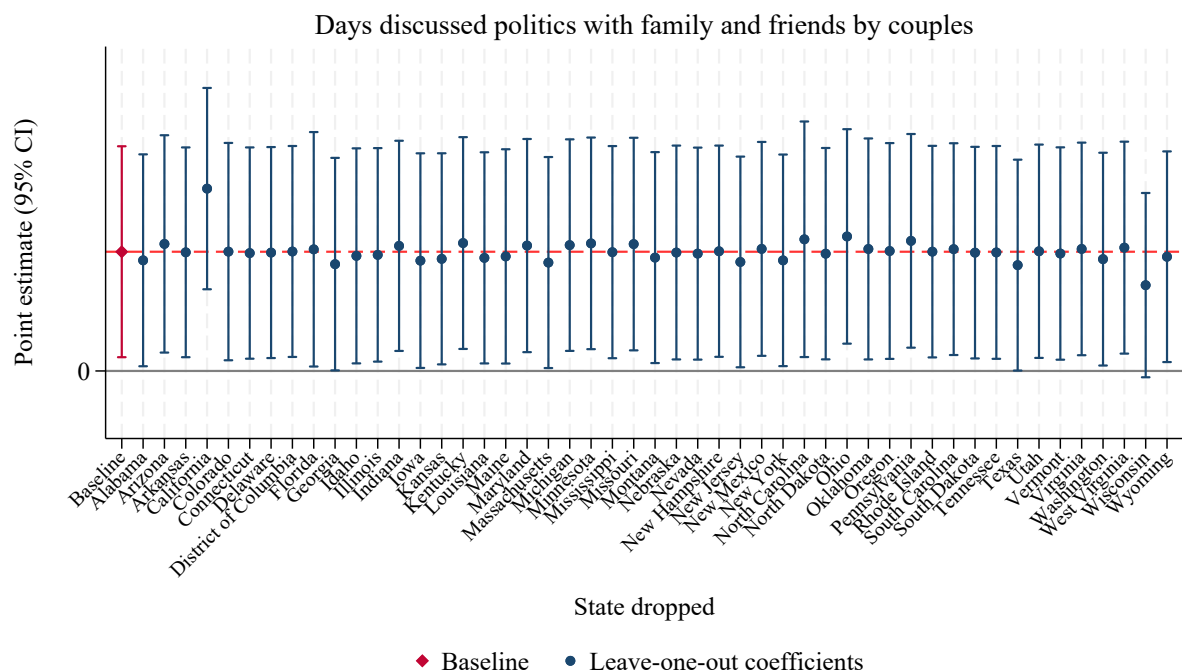
C.1 Robustness checks

Figure C1: Coefficient associated with: Mail-voting period + Couple $\times$ Mail-voting period. Falsification test with 1000 permutations. Red bar denotes the true estimate. Randomization p-val = .05



Notes: This figure presents the distribution of OLS coefficients obtained through 1000 random permutations of states' mail voting profile (availability across all years in sample and length when available). The reported coefficient is the linear combination of coefficients *mail-voting period* + *couple* $\times$ *mail-voting period*, corresponding to the effect of mail voting periods on couples. The red dashed line corresponds to the true coefficient reported in Table 1.

Figure C2: Coefficient associated with: $\text{Mail-voting period} + \text{Couple} \times \text{Mail-voting period}$. Leave-one-out test, dropping one state at a time.



Notes: This figure presents OLS estimates of the differential effect of mail voting on political discussions of couples and singles, successively excluding one state. The unit of observation is an individual. The dependent variable is equal to the number of days the respondent declares having discussed politics with family and friends. Red markers and horizontal lines correspond to the baseline estimate of the coefficient associated with *Mail voting period* + *couple* × *Mail voting period*. Blue markers correspond to the analogous estimates obtained when excluding the corresponding state from the same estimation. Mail-voting period is an indicator variable equal to 1 if mail ballots can have legally been sent out on that day. Couple is an indicator variable equal to 1 if the respondent is cohabiting with a partner. All estimations control for race, age, education, full-time employment, the presence of an underage child at home and monthly church attendance, and include state×year and days-to-Election×year fixed effects. Standard errors are clustered at the state level. 95% confidence intervals are reported.

Table C1: Mail voting and political discussions: robustness to alternative time frames

Outcome:	Days discussed politics with Family and friends						
Time window (days):	150	140	130	120	110	100	90
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mail-voting period	-0.026 (0.055)	-0.029 (0.054)	-0.035 (0.054)	-0.020 (0.055)	-0.018 (0.057)	0.009 (0.057)	0.019 (0.058)
Couple	0.440*** (0.022)	0.444*** (0.021)	0.445*** (0.021)	0.456*** (0.021)	0.460*** (0.023)	0.483*** (0.023)	0.486*** (0.024)
Couple $\times$ Mail-voting period	0.152** (0.061)	0.147** (0.060)	0.146** (0.060)	0.136** (0.060)	0.129** (0.061)	0.102* (0.061)	0.096 (0.062)
Observations	76918	74734	73289	70975	66645	62078	56403
Mean dep. var.	3.034	3.045	3.055	3.078	3.134	3.204	3.257
Individual controls	✓	✓	✓	✓	✓	✓	✓
State $\times$ year and Days-to-elec $\times$ year FEs	✓	✓	✓	✓	✓	✓	✓
Mail-voting period + Couple $\times$ Mail-voting period	0.126	0.118	0.111	0.116	0.111	0.111	0.115
p-value	0.023	0.028	0.037	0.028	0.036	0.034	0.035

Notes: This table presents OLS estimates of the differential effect of mail voting on political discussions of couples and singles, for different temporal restriction. The unit of observation is an individual. Baseline sample is extended to respondents surveyed up to 150 (column 1), 140 (column 2) or 130 days (column 3) to Election day, or restricted to respondent surveyed up to 110 (column 5), 100 (column 6) or 90 days (column 7) to Election day. The dependent variable is equal to the number of days the respondent declares having discussed politics with family and friends. Mail-voting period is an indicator variable equal to 1 if mail ballots can have legally been sent out on that day. Couple is an indicator variable equal to 1 if the respondent is cohabiting with partner. Individual controls include race, age, education, full-time employment, the presence of an underage child at home and monthly church attendance. All estimations include state $\times$ year and days-to-Election $\times$ year fixed effects. Standard errors are clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

C.2 Political discussions in the Comparative National Elections Project

Table C2: Mail voting and political discussions: using CNEP survey

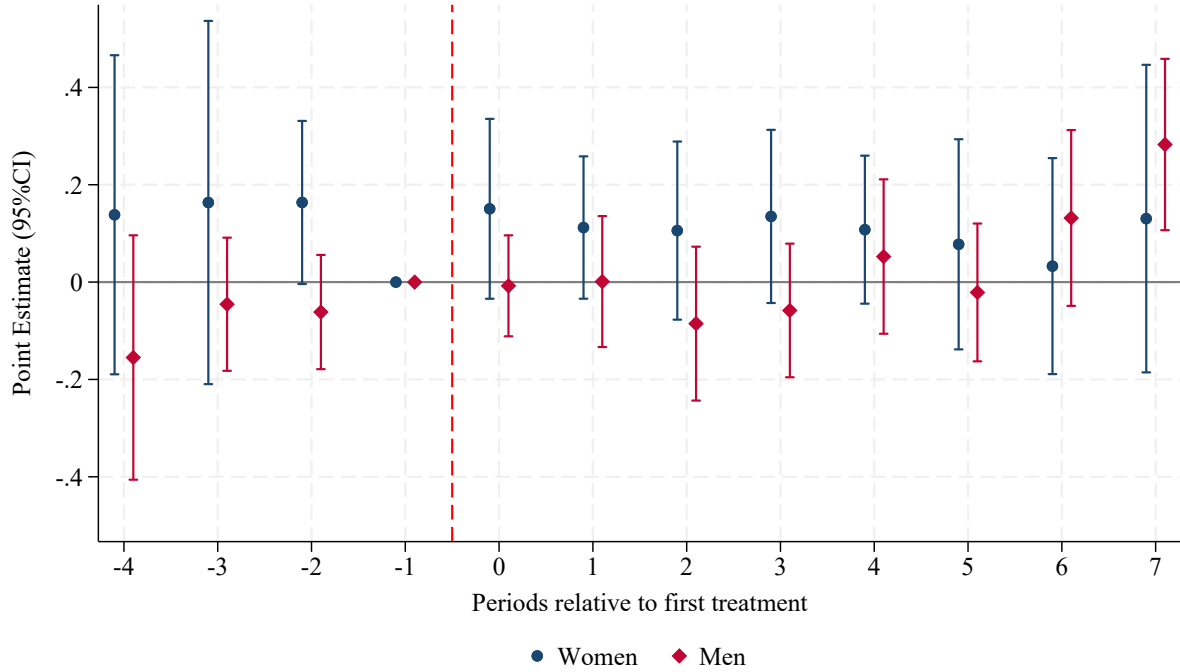
Outcome:	During the campaign, frequency of discussion with:				
	Spouse	Family	Neighbors	Friends	Co-workers
	(1)	(2)	(3)	(4)	(5)
No-excuse mail voting	0.177 (0.115)	0.246** (0.109)	0.099 (0.078)	0.090 (0.093)	-0.015 (0.085)
All-mail election	0.356* (0.185)	0.066 (0.154)	0.058 (0.066)	-0.002 (0.111)	-0.074 (0.088)
Observations	2097	2097	2066	2097	2097
Mean dep. var.	2.299	2.036	0.691	1.685	0.999
Year & State FE	✓	✓	✓	✓	✓

Notes: This table reports OLS estimates of the association between mail voting rules and the frequency of campaign discussions with different interlocutors in the CNEP data. The unit of observation is an individual aged 18 to 64 and cohabiting with a partner. The dependent variable is a 4-point ordered scale measuring the frequency of discussion through the past campaign with one's spouse (column 1), family (column 2), neighbors (column 3), friends (column 4), co-workers (column 5) and ranging from 0=Never to 3=Often. No-excuse mail voting is an indicator equal to 1 if no-excuse mail voting is available in that state-year. All-mail election is an indicator variable equal to 1 if the election was run primarily by mail in that state-year. All estimations control for gender, age, race, education and include year and state fixed effects. Standard errors are clustered at the state level. *** p<0.01, ** p<0.05, * p<.1.

D The impact of mail voting on political opinions: additional analyses

D.1 Mail voting and partisan identification: singles

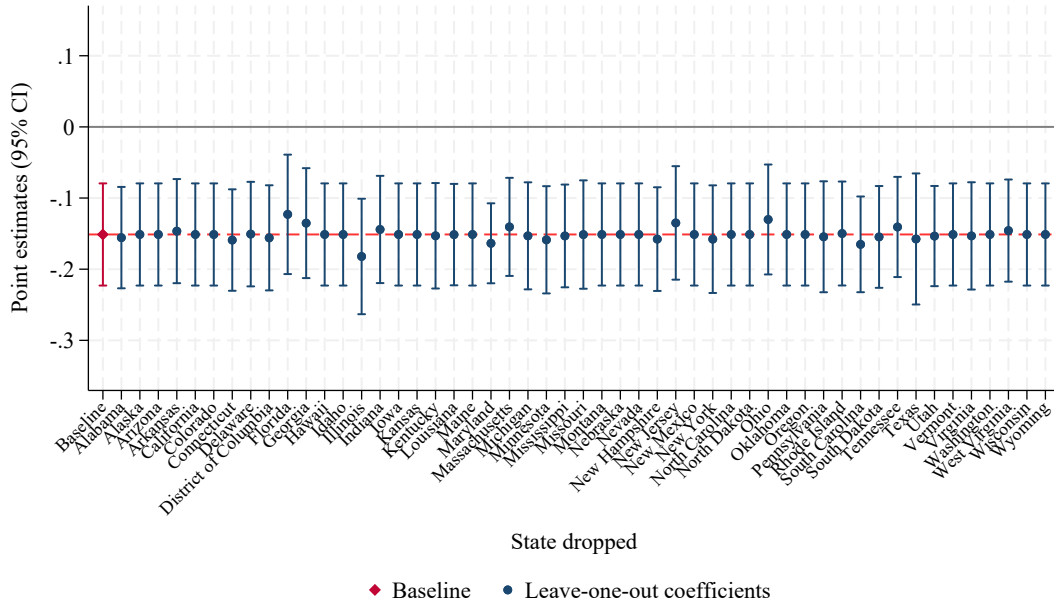
Figure D1: Impact of mail voting on partisan ideological feeling of singles



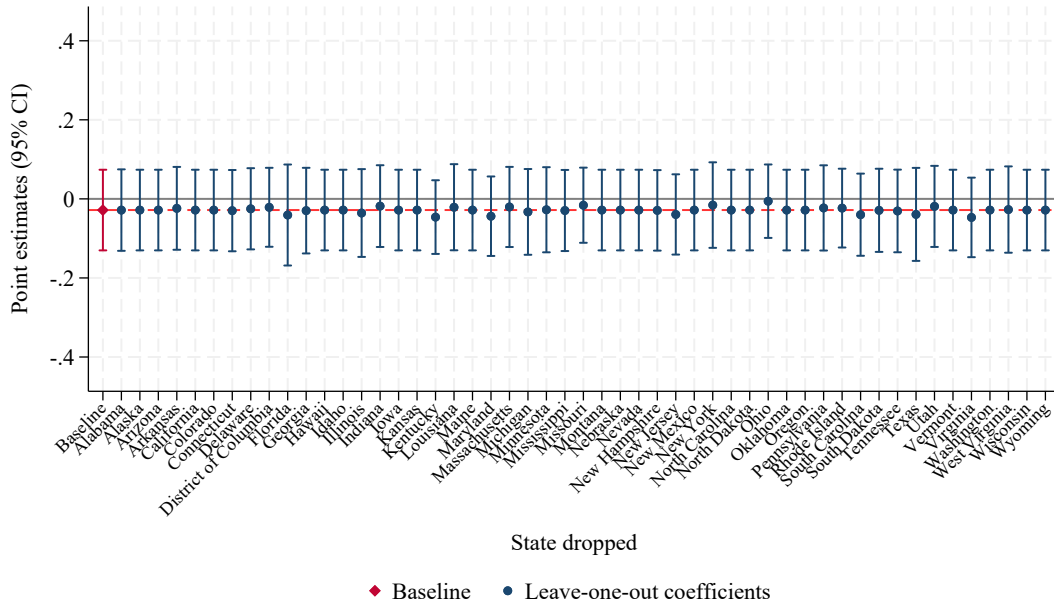
Notes: This figure presents dynamic difference-in-differences estimates of the effect of no-excuse mail voting introduction on partisan identification of single individuals. Estimation is performed using the methodology of [De Chaisemartin and d'Haultfoeuille \(2026\)](#). The unit of observation is a single individual aged 18 to 64. The dependent variable is a 7-point ordered-scale measuring self-declared partisan identification and ranging from 1=Strong Democrat to 7=Strong Republican. Estimation is performed separately for women (blue circular marker) and men (red diamond markers). All estimations control for race, age and its square, education, full-time employment, and the number of days separating the interview from election day. Standard errors are clustered at the state level. Bars denote 95% confidence intervals.

D.2 Mail voting and partisan identification: robustness checks

Figure D2: Mail voting and partisan ideological feeling of couples: Leave-one-out coefficients - states



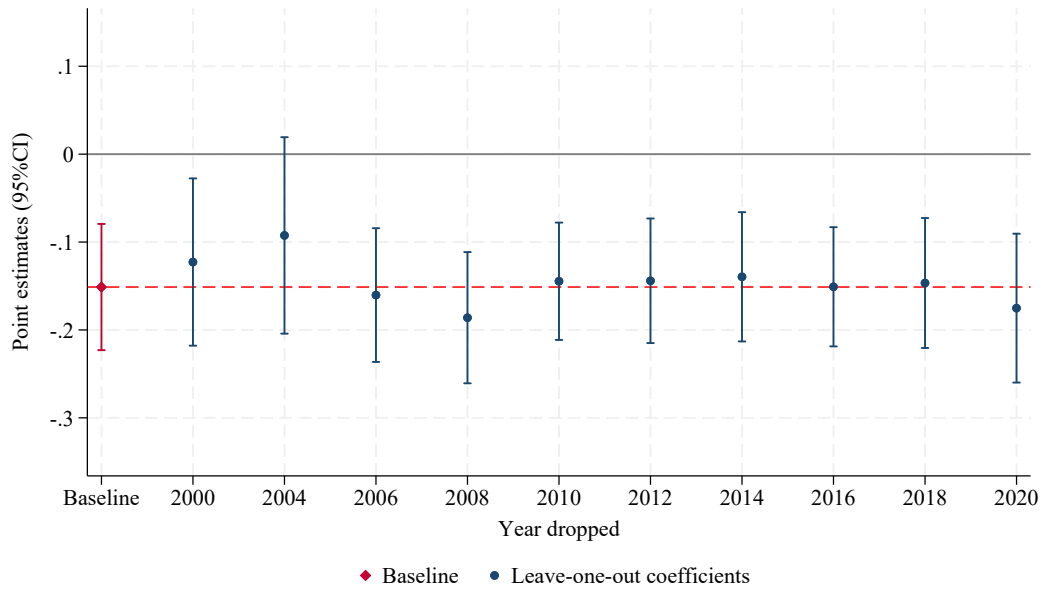
(a) Men



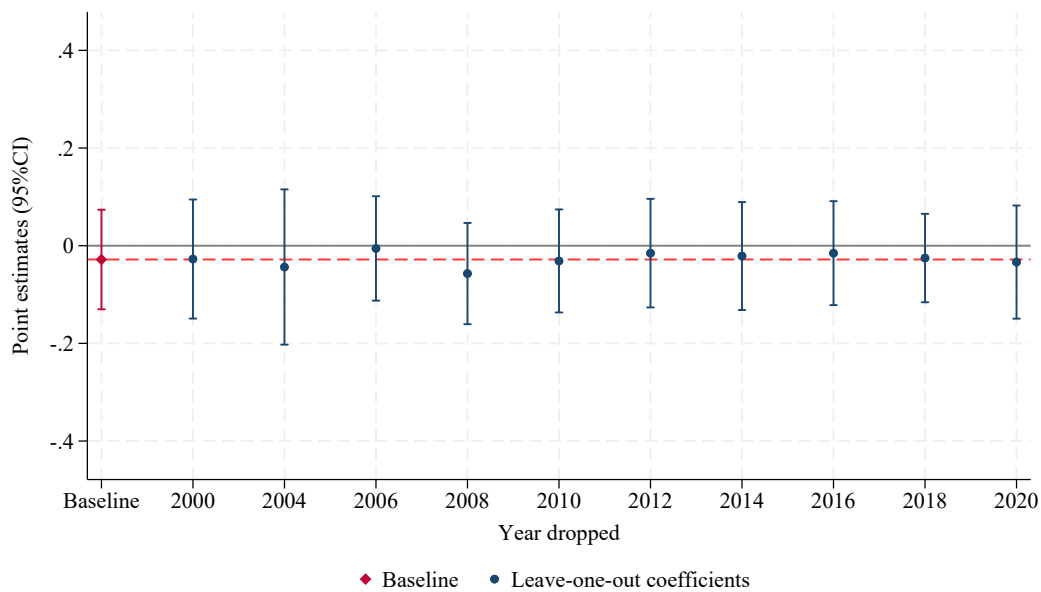
(b) Women

Notes: This figure presents dynamic difference-in-differences estimates of the effect of no-excuse mail voting introduction on partisan identification of married -or living as such- individuals, successively excluding one state. Estimation is performed using the methodology of [De Chaisemartin and d'Haultfoeuille \(2026\)](#) and average post-treatment estimates are reported. The unit of observation is an individual aged 18 to 64 and cohabiting with a partner. The dependent variable is a 7-point ordered-scale measuring self-declared partisan identification and ranging from 1=Strong Democrat to 7=Strong Republican. Estimation is performed separately for men (panel a.) and women (panel b.). All estimations control for race, age and its square, education, full-time employment, and the number of days separating the interview from election day. Standard errors are clustered at the state level. Bars denote 95% confidence intervals.

Figure D3: Mail voting and partisan ideological feeling of couples: Leave-one-out coefficients - years



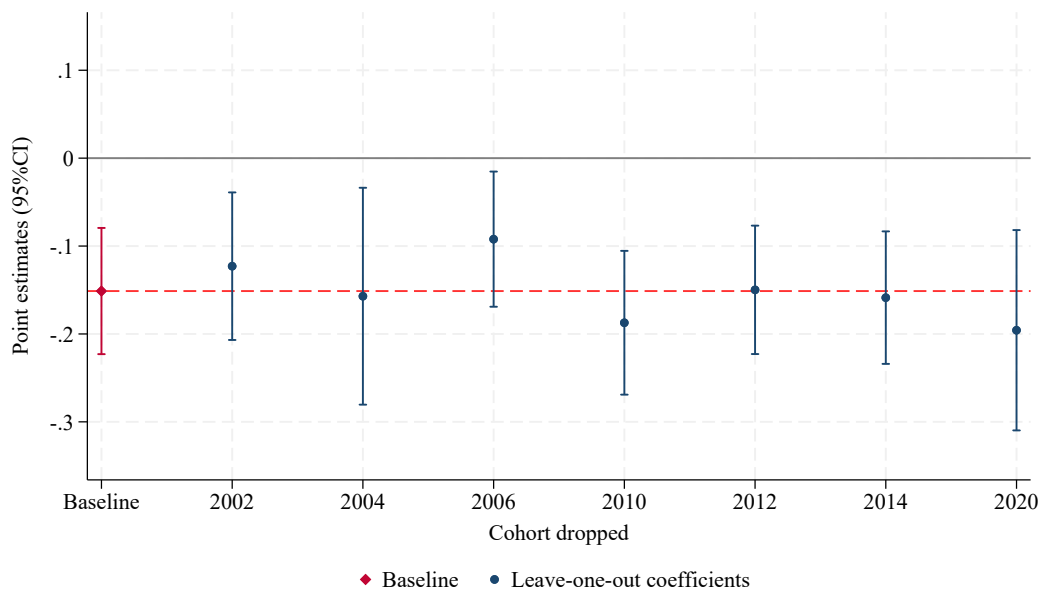
(a) Men



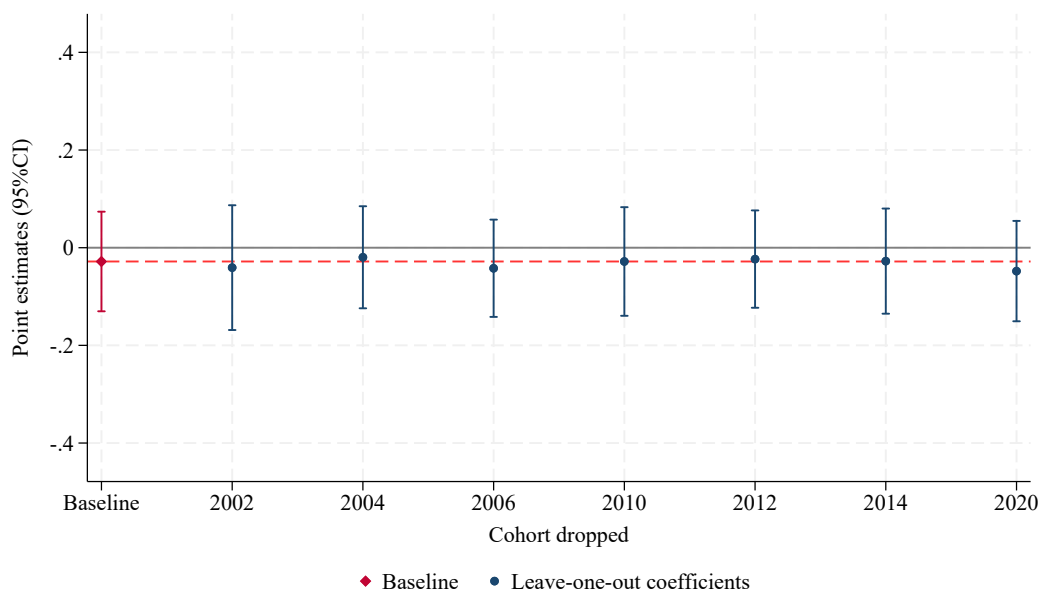
(b) Women

Notes: This figure presents dynamic difference-in-differences estimates of the effect of no-excuse mail voting introduction on partisan identification of married -or living as such- individuals, successively excluding one year. Estimation is performed using the methodology of [De Chaisemartin and d'Haultfoeuille \(2026\)](#) and average post-treatment estimates are reported. The unit of observation is an individual aged 18 to 64 and cohabiting with a partner. The dependent variable is a 7-point ordered-scale measuring self-declared partisan identification and ranging from 1=Strong Democrat to 7=Strong Republican. Estimation is performed separately for men (panel a.) and women (panel b.). All estimations control for race, age and its square, education, full-time employment, and the number of days separating the interview from election day. Standard errors are clustered at the state level. Bars denote 95% confidence intervals.

Figure D4: Mail voting and partisan ideological feeling of couples: Leave-one-out coefficients - cohorts



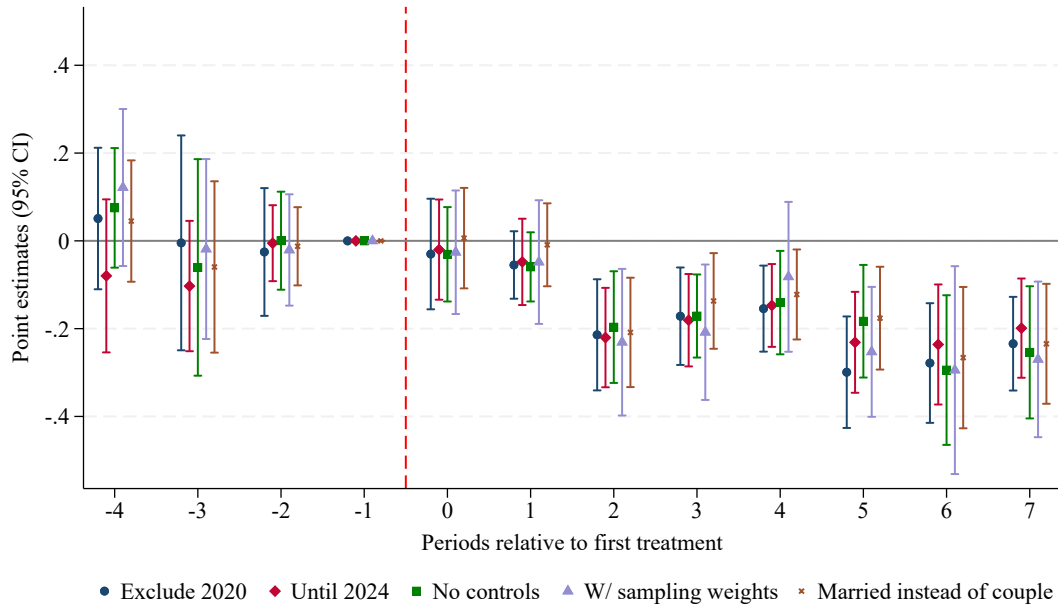
(a) Men



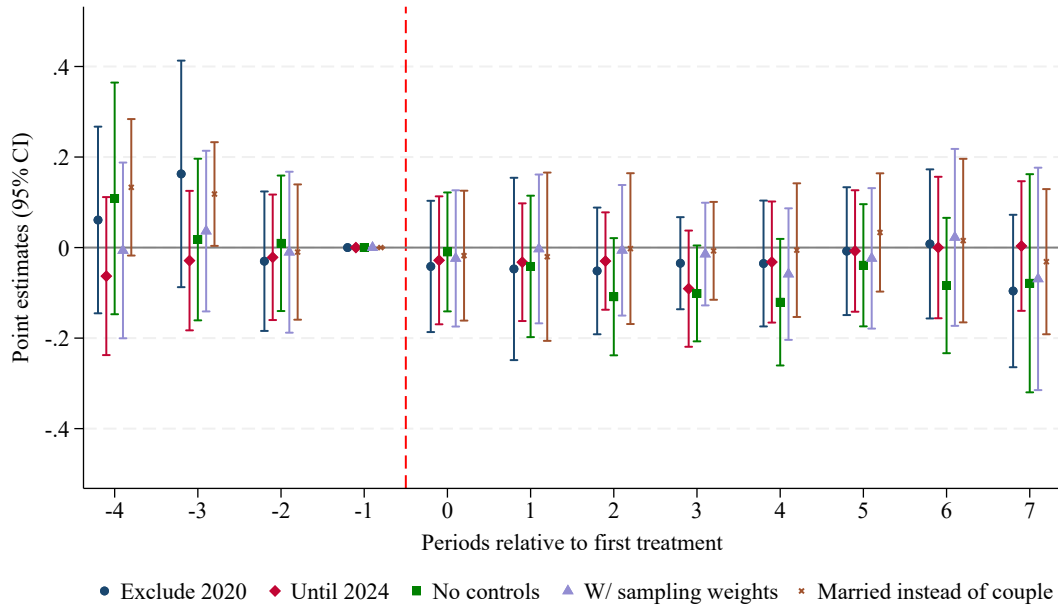
(b) Women

Notes: This figure presents dynamic difference-in-differences estimates of the effect of no-excuse mail voting introduction on partisan identification of married -or living as such- individuals, successively excluding one treatment cohort. Estimation is performed using the methodology of [De Chaisemartin and d'Haultfoeuille \(2026\)](#) and average post-treatment estimates are reported. The unit of observation is an individual aged 18 to 64 and cohabiting with a partner. The dependent variable is a 7-point ordered-scale measuring self-declared partisan identification and ranging from 1=Strong Democrat to 7=Strong Republican. Estimation is performed separately for men (panel a.) and women (panel b.). All estimations control for race, age and its square, education, full-time employment, and the number of days separating the interview from election day. Standard errors are clustered at the state level. Bars denote 95% confidence intervals.

Figure D5: Mail voting and partisan ideological feeling of couples: Robustness checks



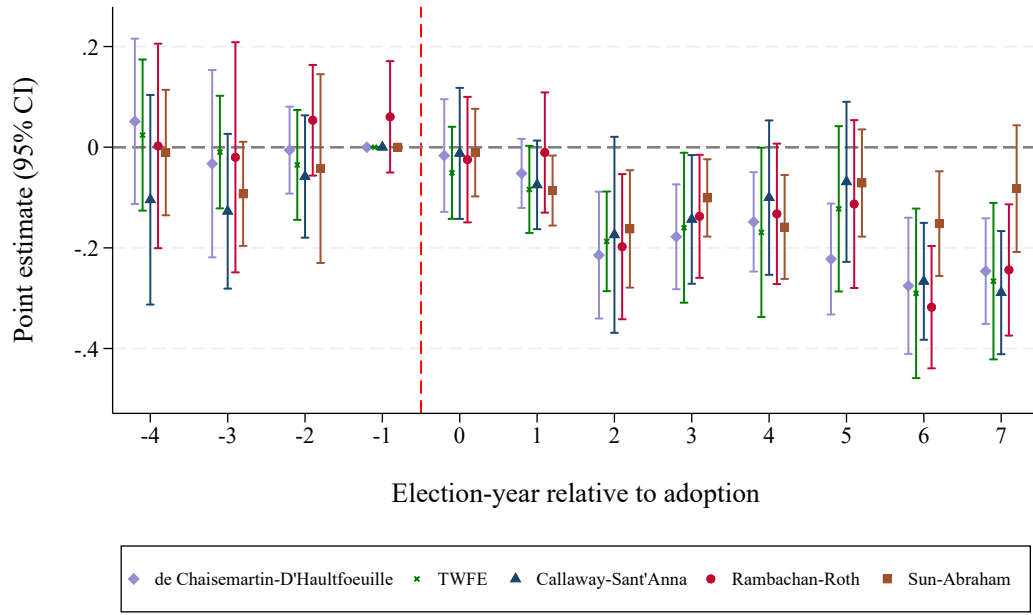
(a) Men



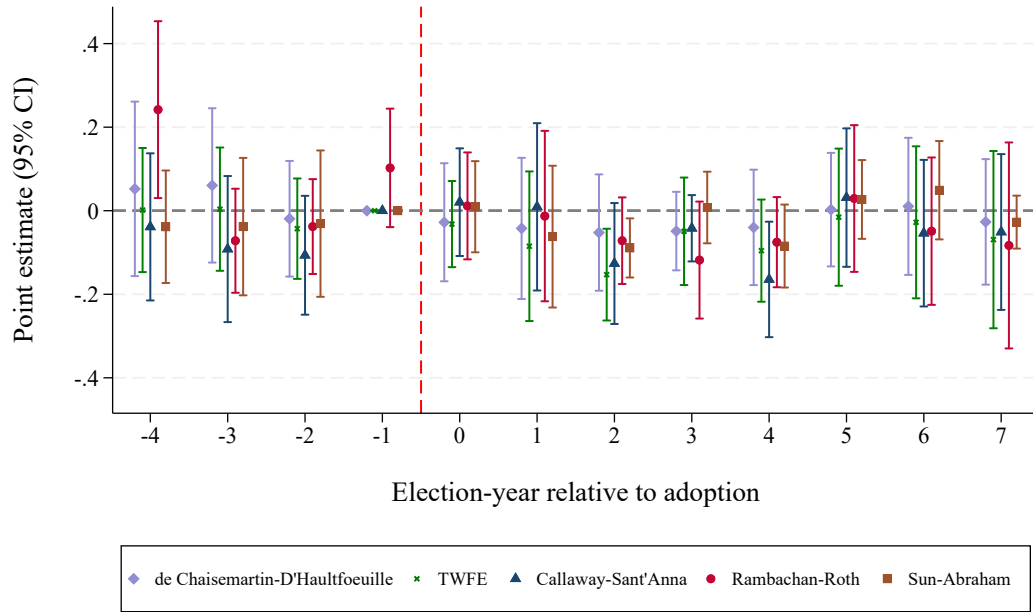
(b) Women

Notes: This figure presents dynamic difference-in-differences estimates of the effect of no-excuse mail voting introduction on partisan identification of married -or living as such- individuals, through different robustness checks. Estimation is performed using the methodology of [De Chaisemartin and d'Haultfoeuille \(2026\)](#). The unit of observation is an individual aged 18 to 64 and cohabiting with a partner. The dependent variable is a 7-point ordered-scale measuring self-declared partisan identification and ranging from 1=Strong Democrat to 7=Strong Republican. Blue circle markers exclude the year 2020. Red diamond markers include 2022 and 2024 elections. Green square markers drop all individual controls included in the baseline specification. Purple triangles weight CCES observations using the provided sampling weights. Brown cross restrict couples to married individuals. Estimation is performed separately for men (panel a.) and women (panel b.). Standard errors are clustered at the state level. Bars denote 95% confidence intervals.

Figure D6: Impact of mail voting on partisan ideological feeling of couples - alternative estimators



(a) Men

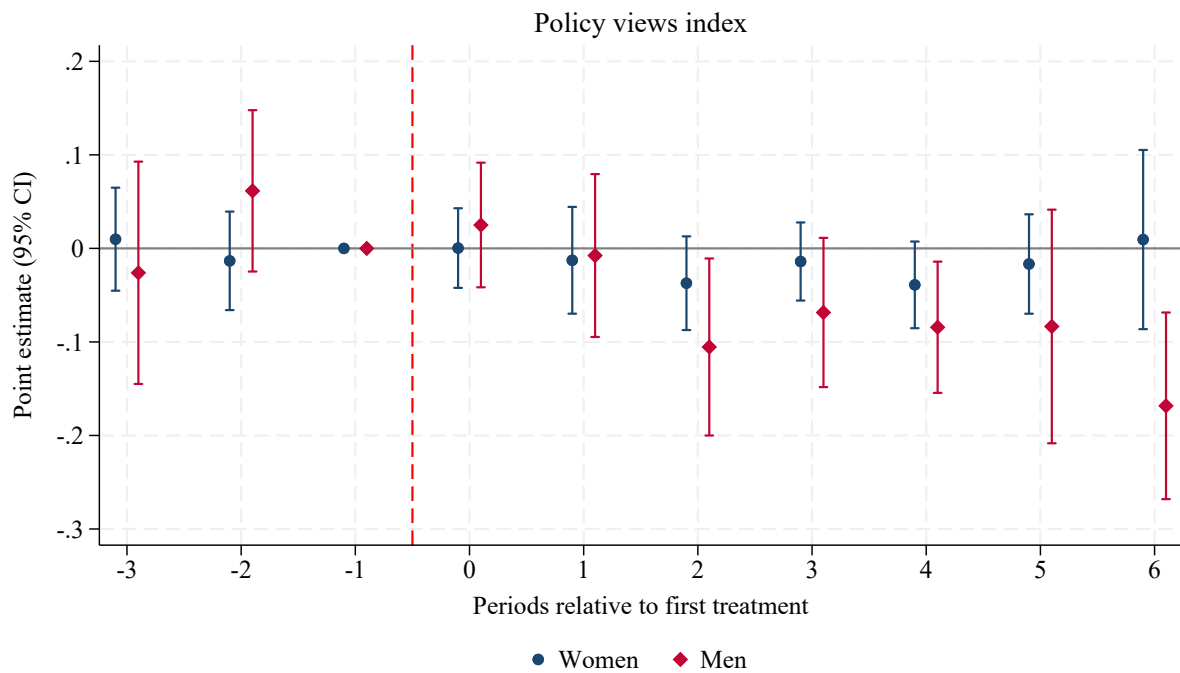


(b) Women

Notes: This figure presents dynamic difference-in-differences estimates of the effect of no-excuse mail voting introduction on partisan identification of married -or living as such- individuals, estimated using different estimators. The unit of observation is an individual aged 18 to 64 and cohabiting with a partner. The dependent variable is a 7-point ordered-scale measuring self-declared partisan identification and ranging from 1=Strong Democrat to 7=Strong Republican. Purple diamond markers denote the baseline coefficients estimated using [De Chaisemartin and d'Haultfoeuille \(2026\)](#). In turn, the estimation is performed using two-way fixed effects (green cross markers), [Callaway and Sant'Anna \(2021\)](#) (blue triangle markers), augmented inverse-probability weighting implemented by [Rambachan and Roth \(2023\)](#), and [Sun and Abraham \(2021\)](#) (brown square markers). Estimation is performed separately for men (panel a.) and women (panel b.). Standard errors are clustered at the state level. Bars denote 95% confidence intervals.

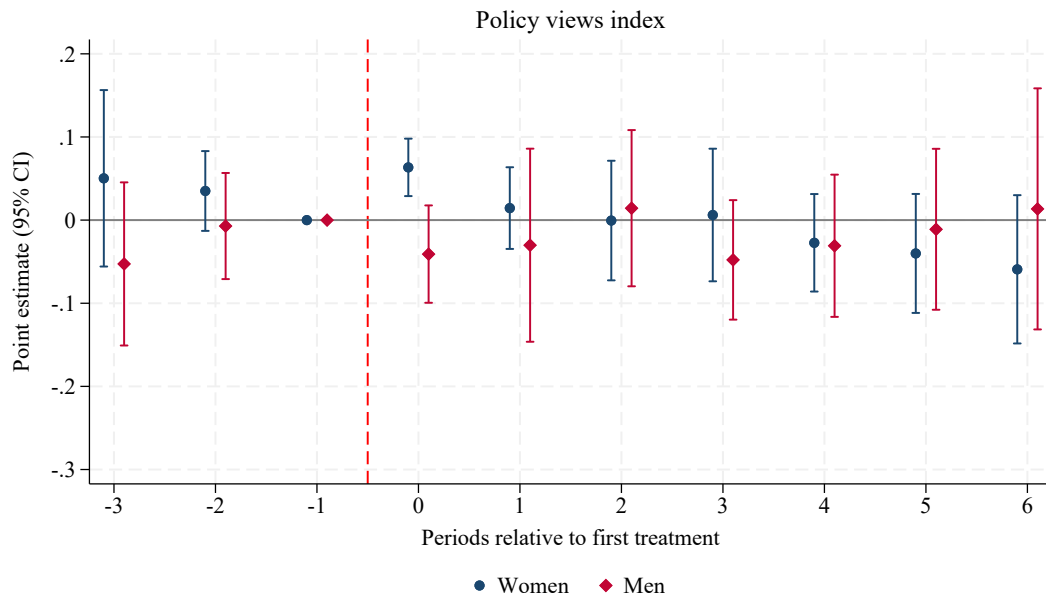
D.3 Mail voting and policy views

Figure D7: Impact of mail voting on policy views of couples



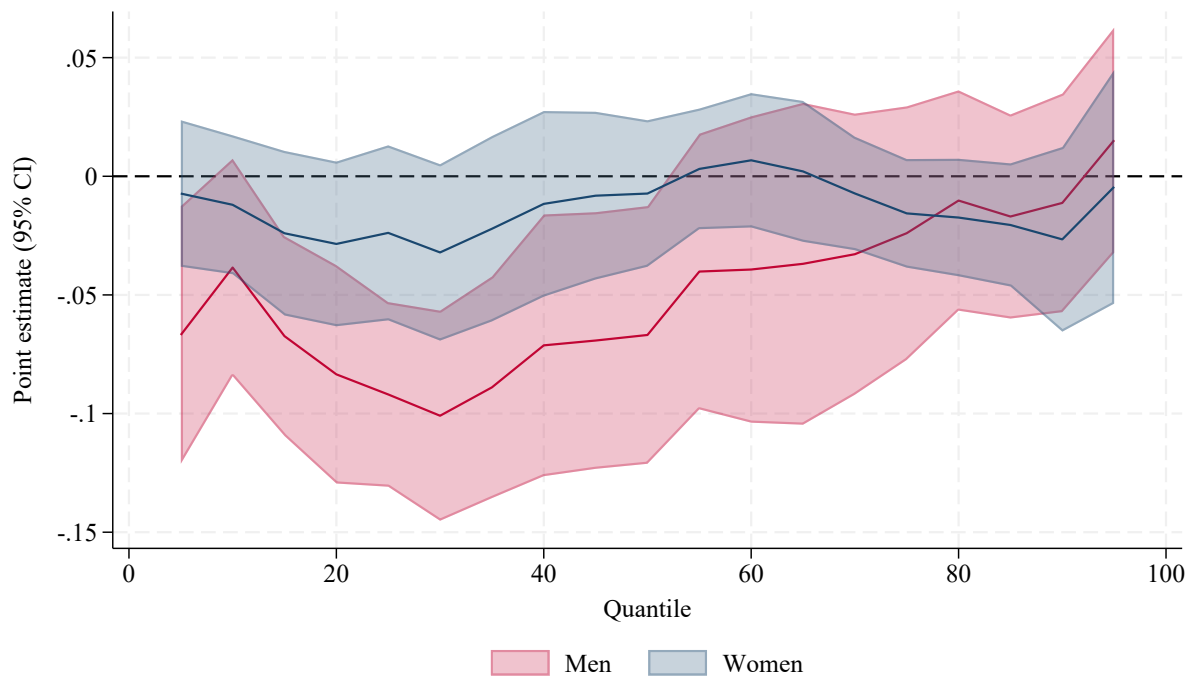
Notes: This figure presents dynamic difference-in-differences estimates of the effect of no-excuse mail voting introduction on policy views of married -or living as such- individuals. Estimation is performed using the methodology of [De Chaisemartin and d'Haultfoeuille \(2026\)](#). The unit of observation is an individual aged 18 to 64 and cohabiting with a partner. The dependent variable is the policy ideal point scale developed by [Warshaw and Tausanovitch \(2022\)](#), where negative values capture more progressive views while positive values correspond to more conservative ones. Estimation is performed separately for women (blue circular marker) and men (red diamond markers). All estimations control for race, age and its square, education, full-time employment, and the number of days separating the interview from election day. Standard errors are clustered at the state level. Bars denote 95% confidence intervals.

Figure D8: Impact of mail voting on policy views of singles



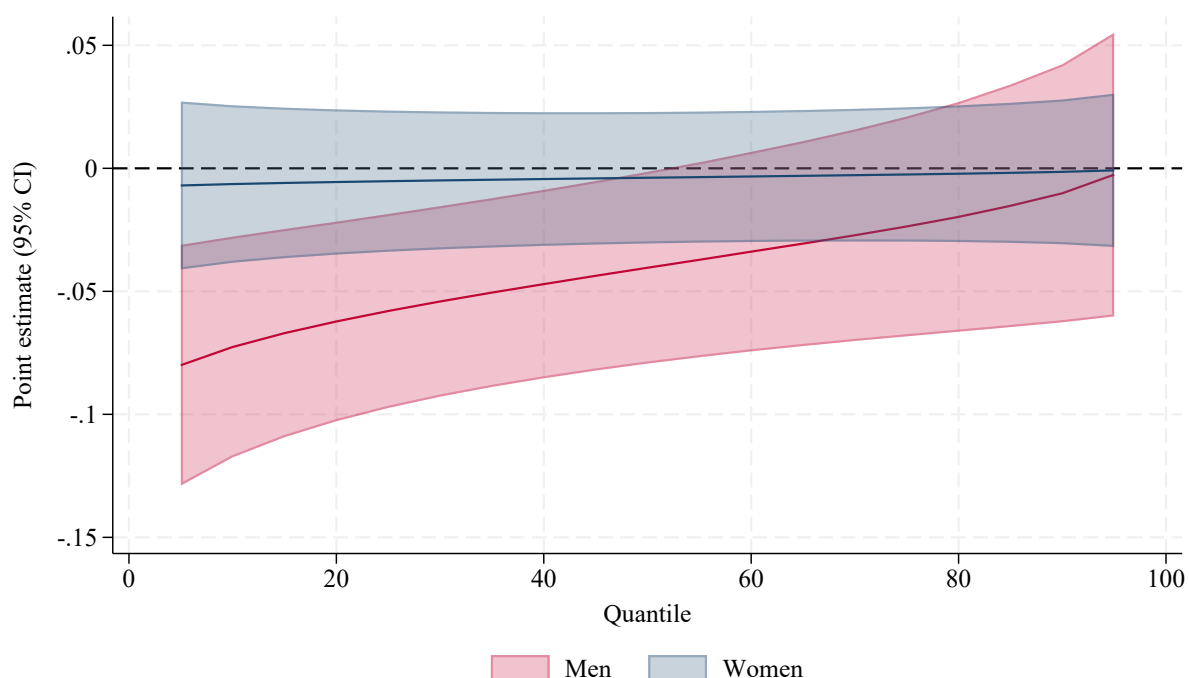
Notes: This figure presents dynamic difference-in-differences estimates of the effect of no-excuse mail voting introduction on policy views of single individuals. Estimation is performed using the methodology of [De Chaisemartin and d'Haultfoeuille \(2026\)](#). The unit of observation is a single individual aged 18 to 64. The dependent variable is the policy ideal point scale developed by [Warshaw and Tausanovitch \(2022\)](#), where negative values capture more progressive views while positive values correspond to more conservative ones. Estimation is performed separately for women (blue circular marker) and men (red diamond markers). All estimations control for race, age and its square, education, full-time employment, and the number of days separating the interview from election day. Standard errors are clustered at the state level. Bars denote 95% confidence intervals.

Figure D9: Quantile regression of the effect of mail voting on couples' policy views



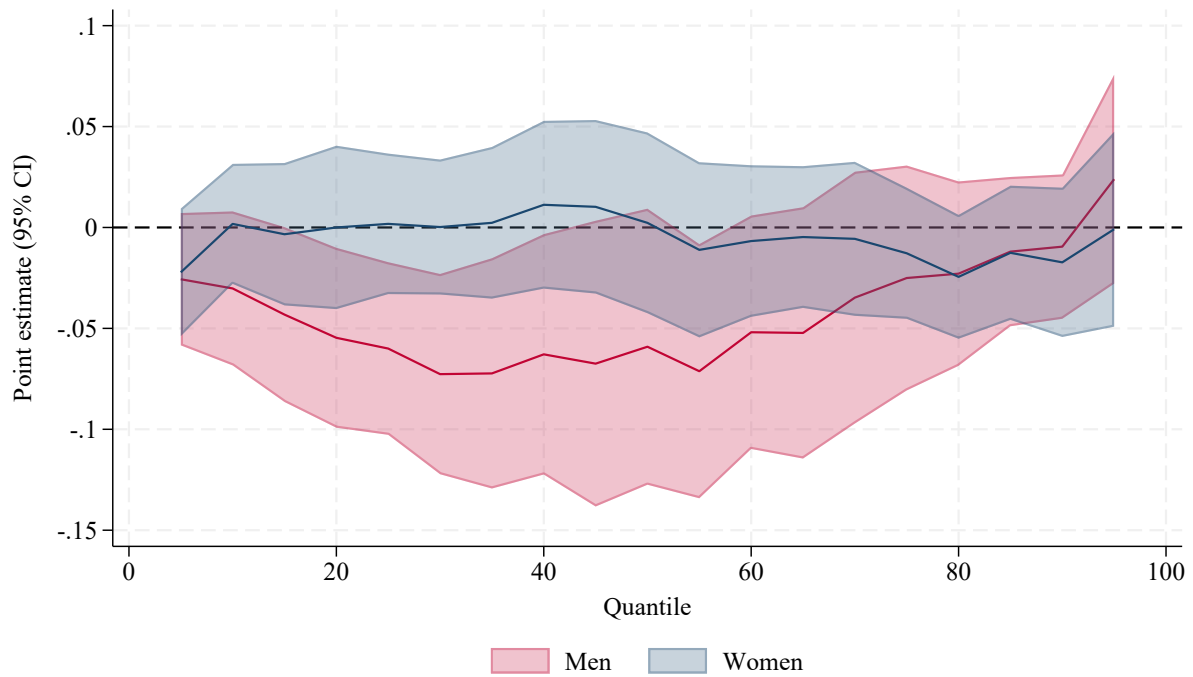
Notes: This figure presents quantile regression estimates of the effect of no-excuse mail voting introduction on policy views of married -or living as such- individuals. The unit of observation is an individual aged 18 to 64 and cohabiting with a partner. Sample is restricted to states which are not already treated at the beginning of the observation period. The dependent variable is the policy ideal point scale developed by [Warshaw and Tausanovitch \(2022\)](#), where negative values capture more progressive views while positive values correspond to more conservative ones. Effects on both gender are obtained through a common estimation. Effects are computed for every fifth quantile between the 5th and 95th. The estimation controls for race, age and its square, education, full-time employment, the number of days separating the interview from election day and include state and year fixed effects. Standard errors are clustered at the state level. Shaded areas denote 95% confidence intervals.

Figure D10: Quantile regression of the effect of mail voting on couples' policy views: Machado and Santos Silva (2019)



Notes: This figure presents quantile regression estimates of the effect of no-excuse mail voting introduction on policy views of married -or living as such- individuals using the methodology of [Machado and Santos Silva \(2019\)](#). The unit of observation is an individual aged 18 to 64 and cohabiting with a partner. Sample is restricted to states which are not already treated at the beginning of the observation period. The dependent variable is the policy ideal point scale developed by [Warshaw and Tausanovitch \(2022\)](#), where negative values capture more progressive views while positive values correspond to more conservative ones. Effects on both gender are obtained through a common estimation. Effects are computed for every fifth quantile between the 5th and 95th. The estimation controls for race, age and its square, education, full-time employment, the number of days separating the interview from election day and include state and year fixed effects. Standard errors are clustered at the state level. Shaded areas denote 95% confidence intervals.

Figure D11: Quantile regression of the effect of mail voting on couples' policy views: Melly and Pons (2025)



Notes: This figure presents quantile regression estimates of the effect of no-excuse mail voting introduction on policy views of married -or living as such- individuals using the methodology of [Melly and Pons \(2025\)](#). The unit of observation is an individual aged 18 to 64 and cohabiting with a partner. Sample is restricted to states which are not already treated at the beginning of the observation period. The dependent variable is the policy ideal point scale developed by [Warshaw and Tausanovitch \(2022\)](#), where negative values captures more progressive views while positive values correspond to more conservative ones. Effects on both gender are obtained through a common estimation. Effects are computed for every fifth quantile between the 5th and 95th. The estimation controls for race, age and its square, education, full-time employment, the number of days separating the interview from election day and include state and year fixed effects. Standard errors are clustered at the state level. Shaded areas denote 95% confidence intervals.

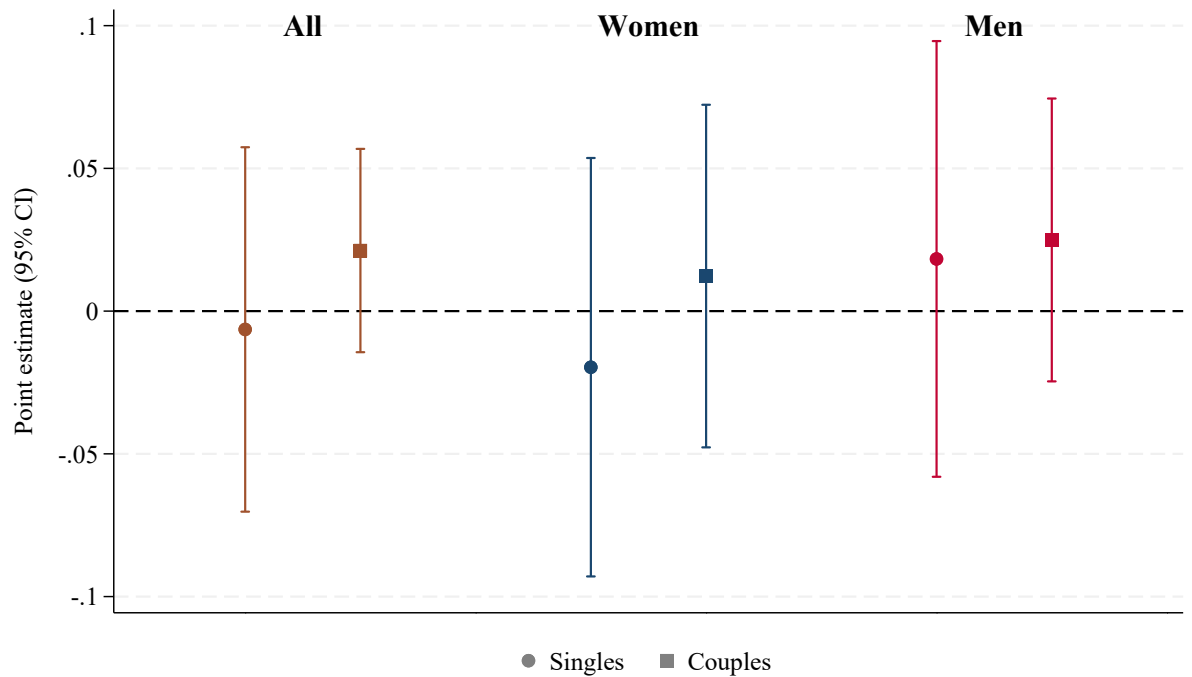
D.4 Mail voting and political involvement: campaign interest and political knowledge

Table D1: Within-campaign impact of mail-voting period on interest in the campaign

Sample:	All	Women	Men
	(1)	(2)	(3)
Mail-voting period	−0.003 (0.015)	−0.009 (0.027)	0.009 (0.026)
Couple	0.070*** (0.006)	0.060*** (0.009)	0.084*** (0.013)
Couple × Mail-voting period	0.034* (0.017)	0.052 (0.034)	0.006 (0.028)
Observations	55451	30225	25226
Mean dep. var.	1.079	1.044	1.121
Individual controls	✓	✓	✓
State × year and Days-to-elec × year FEs	✓	✓	✓
Mail-voting period + Couple × Mail-voting period	0.031	0.043	0.015
p-value	0.032	0.060	0.421

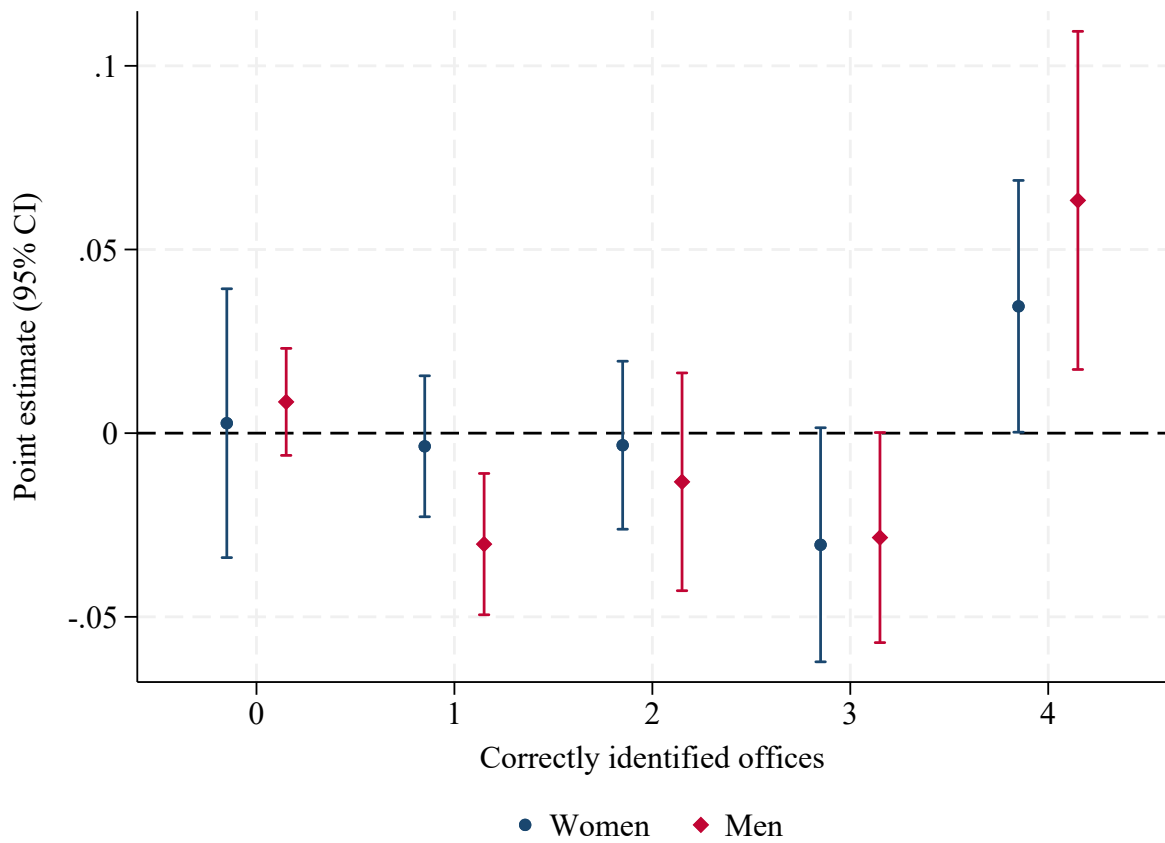
Notes: This tables presents OLS estimates of the differential effect of mail voting on campaign interest of couples and singles. The unit of observation is an individual. The dependent variable is a 3-point scale measuring the level of interest in the ongoing electoral campaign. Mail-voting period is an indicator variable equal to 1 if mail ballots can have legally been sent out on that day. Couple is an indicator variable equal to 1 if the respondent is cohabiting with a partner. Individual controls include gender, race, age, education, full-time employment, the presence of an underage child at home and monthly church attendance. All estimations include state×year and days-to-Election×year fixed effects. Standard errors are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

Figure D12: Within-campaign impact of mail voting availability on interest in government affairs



Notes: This table presents OLS estimates of the differential effect of mail voting on following of government affairs across marital status and gender. The unit of observation is an individual. The dependent variable is a 4-point scale measuring the level of interest in government affairs. The explanatory variable of interest is Mail-voting period, an indicator variable equal to 1 if mail ballots can have legally been sent out on that day. Estimations are successively performed on the full sample (brown markers, left panel), women (blue markers, central panel) and men (red markers, right panel). The effect on singles (round markers) and couples (square markers) are obtained through a common estimation. All estimations control for race, age, education, full-time employment, the presence of an underage child at home and monthly church attendance and include state×year and days-to-Election×year fixed effects. Estimation on the whole sample additionally controls for gender. Standard errors are clustered at the state level. Bars denote 95% confidence intervals.

Figure D13: Number of correctly identified offices



Notes: This figure presents dynamic difference-in-differences estimates of the effect of no-excuse mail voting introduction on partisan identification of married -or living as such- individuals. Estimation is performed using the methodology of [De Chaisemartin and d'Haultfoeuille \(2026\)](#) and average post-treatment effects are reported. The unit of observation is an individual aged 18 to 64 and cohabiting with a partner. The dependent variable is an indicator variable equal to 1 if the respondent correctly identifies the corresponding number of offices, when successively asked the party with a majority in the state house, in the state senate, of their representative in congress or of their governor. Coefficient are each obtained through separate estimations. Estimations is performed separately for women (blue circle markers) and men (red diamond markers). All estimations control for race, age and its square, education, full-time employment, and the number of days separating the interview from election day. Standard errors are clustered at the state level. Bars denote 95% confidence intervals.

E Coordination and mutual influence on the ballot

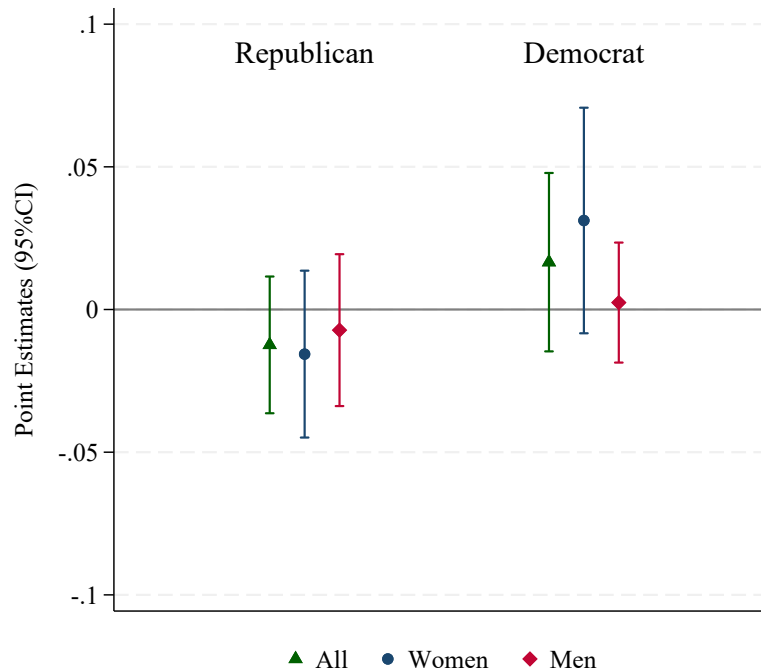
E.1 Party vote shares in survey data

An investigation of the effect of mail voting on partisan vote shares based on the electoral surveys used through this analysis faces some limitations and empirical challenges. In particular, declared turnout rates in both NAES and CCES surveys are extremely high, reaching up to 90% of respondents declaring they voted in some election years. These figures, on top of not matching official records, can carry significant gender bias. The CCES address this problem by validating respondents answers against voting registers. Over the 2006-2024 period, average declared turnout among the population in the CCES is 88% while average validated turnout goes down to 57%, a figure much more in line with official numbers. Due to the huge gap, I limit the analysis of the impact of no-excuse mail voting on vote shares to the CCES. While this significantly improves the accuracy of partisan vote measure, it reduces the time frame to years from 2006 onward and thereby the identifying variation.

In line with previously presented analysis, I estimate the effect of no-excuse mail voting on partisan votes using Equation 1. Figure E1 presents the average effect of no-excuse mail voting on the likelihood of declaring either having voted for the Republican or Democratic party in the House elections by married or cohabiting respondents.

No significant effect is uncovered, especially among men for whom the estimated coefficients are very close to zero.

Figure E1: Impact of no-excuse absentee on partisan voting - House



Notes: This figure presents dynamic difference-in-differences estimates of the effect of no-excuse mail voting introduction on partisan voting of married -or living as such- individuals. Estimation is performed using the methodology of [De Chaisemartin and d'Haultfoeuille \(2026\)](#) and average post-treatment estimates are reported. The unit of observation is an individual aged 18 to 64 and cohabiting with a partner. The dependent variable is an indicator variable equal to 1 if the respondent declares having voted for the Republican candidate in the latest house election (Left panel) or for the Democratic candidate (Right panel). Estimation is performed successively pooling all respondents (green triangle markers), on the sample of women (blue circle markers), and on the sample of women (red diamond markers). All estimations control for race, age and its square, education, full-time employment, and the number of days separating the interview from election day. Standard errors are clustered at the state level. Bars denote 95% confidence intervals.

E.2 Evidence from Cast Vote Records: additional information and extension

Figure E2: Example ballot in 2019 San Francisco Municipal elections

BT 2 PCT

OFFICIAL BALLOT
Consolidated Municipal Election
City and County of San Francisco
November 5, 2019

INSTRUCTIONS TO VOTERS:
Use a pen with dark ink.
Rank candidates in the order of your choice.
Fill in the oval:
• In the 1st column for your 1st choice.
• In the 2nd column for your 2nd choice.
• In the 3rd column for your 3rd choice, and so on.
Do not fill in more than one oval.
• If a candidate is not listed, write the candidate's name in the space at the end of the candidate list and fill in the oval for the rank.
If you make a mistake, you may request a new ballot.

CITY AND COUNTY
市長

	1st Choice	2nd Choice	3rd Choice	4th Choice	5th Choice	6th Choice
MAYOR 市長						
PAUL YEAMBA ROBERTSON / 羅曼 尹巴瑟 Mayor, Democratic	☐	☐	☐	☐	☐	☐
ELLEN LEE ZHOU / 李潔儀 Mayor, Republican	☐	☐	☐	☐	☐	☐
LONDON L. BIRNEY / 畢希 布萊恩 Mayor of San Francisco Democratic	☐	☐	☐	☐	☐	☐
JOEL VENTRECA / 羅曼 布蘭斯 Mayor, Republican	☐	☐	☐	☐	☐	☐
WILMA FANG / 彭麗華 Mayor, Republican	☐	☐	☐	☐	☐	☐
ROBERT L. JORDAN, JR. / 小羅伯特 L. 喬丹 Mayor	☐	☐	☐	☐	☐	☐

CITY ATTORNEY
市務律師

	1st Choice	2nd Choice	3rd Choice
DENNIS J. HERRERA / 丹尼絲 J. 赫雷拉 City Attorney	☐	☐	☐

DISTRICT ATTORNEY
地方檢察官

	1st Choice	2nd Choice	3rd Choice	4th Choice
LEF CAUTION / 黎少東 District Attorney	☐	☐	☐	☐
NANCY TUNG / 湯麗華 District Attorney	☐	☐	☐	☐
CHELA BOUVON / 布魯文 District Attorney	☐	☐	☐	☐
DAVID LOFFUS / 羅曼 路夫斯 District Attorney	☐	☐	☐	☐

PUBLIC DEFENDER
公共辯護律師

	1st Choice	2nd Choice	3rd Choice
MANOHAR "MANO" SAJJU / 薩吉 Public Defender	☐	☐	☐

SHERIFF
警長

	1st Choice	2nd Choice	3rd Choice
PAUL MIYAMOTO / 藤田 廣平 San Francisco Sheriff	☐	☐	☐

TREASURER
財政局

	1st Choice	2nd Choice	3rd Choice
JOSE CISNEROS / 希斯羅 Treasurer	☐	☐	☐

VOTE BOTH SIDES OF BALLOT
請在兩面投票

(a) Card 1

BT 2 PCT

OFFICIAL BALLOT
Consolidated Municipal Election
City and County of San Francisco
November 5, 2019

INSTRUCTIONS TO VOTERS:
Use a pen with dark ink.
Fill in the oval to the right of your choice, as shown in the picture.
To vote for a qualified write-in candidate, write the candidate's name in the space at the end of the candidate list and fill in the oval.
If you make a mistake, you may request a new ballot.

SCHOOL
學校

MEMBER, BOARD OF EDUCATION
教育局委員

Vote for One

選一人

JENNY LAM / 林潔儀
Board of Education

KRISTEN STROGER / 史都華
Mayor, Republican

ROBERT L. COLEMAN / 羅曼 柯利曼
Mayor

MEMBER, COMMUNITY COLLEGE BOARD
社區大學董事會成員

Vote for One

選一人

VIVY LEE / 李麗儀
Community College Trustee

MEASURES SUBMITTED TO THE VOTERS
提交市民投票的措施

CITY AND COUNTY PROPOSITIONS
市縣提案

A SAN FRANCISCO AFFORDABLE HOUSING BONDS. To finance the construction, development, acquisition, and preservation of housing affordable to extremely low, low and middle-income households through programs that will prioritize vulnerable populations such as San Francisco's working families, veterans, seniors, and persons with disabilities, to assist in the acquisition, rehabilitation, and preservation of existing affordable housing to prevent the displacement of residents, to repair and reconstruct damaged and dilapidated public housing developments and their surrounding infrastructure, to assist the City's middle-income residents or workers in obtaining affordable rental or home ownership opportunities including down payment assistance and support for new construction of affordable housing for San Francisco's working families and City College of San Francisco employees, and to pay related costs, shall the City and County of San Francisco issue \$600,000,000 in general obligation bonds with a duration of up to 30 years from the time of issuance, an estimated average tax rate of \$0.195000 of assessed property value, and projected average annual revenue of \$10,000,000, subject to independent citizen oversight and regular audits?

YES / 贊成 ☐ NO / 反對 ☐

B Shall the City amend the Charter to change the name of the Department of Aging and Adult Services to the Department of Disability and Aging Services, to change the name of the Aging and Adult Services Commission to the Disability and Aging Services Commission, and to add new qualifications for those of the seven seats on this Commission?

YES / 贊成 ☐ NO / 反對 ☐

C Shall the City overturn the law passed by the Board of Supervisors suspending the sale of electronic cigarettes until they receive FDA authorization, and adopt new regulations on the sale, manufacture, distribution and advertising of electronic cigarettes in San Francisco?

YES / 贊成 ☐ NO / 反對 ☐

D Shall the City impose a 1.5% business tax on shared rides and a 3.5% business tax on private rides for taxis charged by commercial ride-sharing and on-demand vehicle companies until November 5, 2046, raising an estimated \$20.5 million annually in bond improvements in transit service and safety and performance safety?

YES / 贊成 ☐ NO / 反對 ☐

E Shall the City amend the Planning Code to allow 100% Affordable Housing Projects and Educator Housing Projects in public zoning districts and to require approval of these projects?

YES / 贊成 ☐ NO / 反對 ☐

F Shall the City establish new restrictions on campaign contributions to local elected officials and candidates, and apply new disclaimer requirements to campaign advertisements?

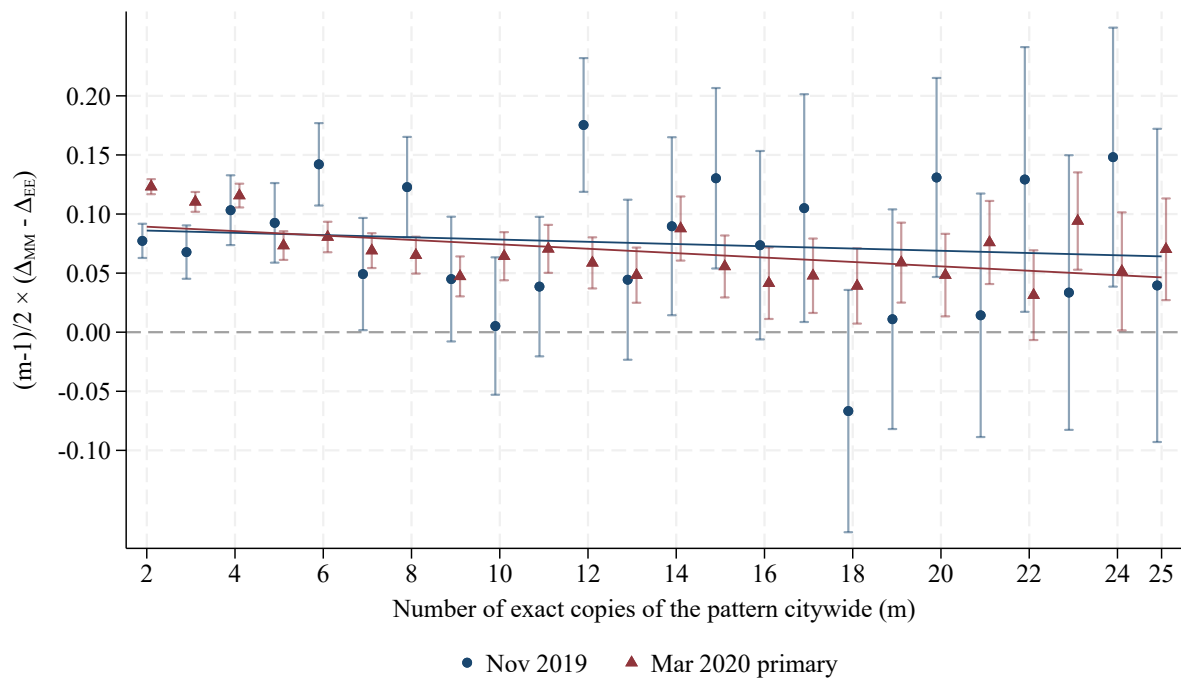
YES / 贊成 ☐ NO / 反對 ☐

VOTE BOTH SIDES OF BALLOT
請在兩面投票

(b) Card 2

Notes: This figure provides an example of the layout of a ballot of VBM BT 2-type in 2019 San Francisco Municipal elections.

Figure E3: Per card excess ballot co-location rate: patterns occurring up to 25 times

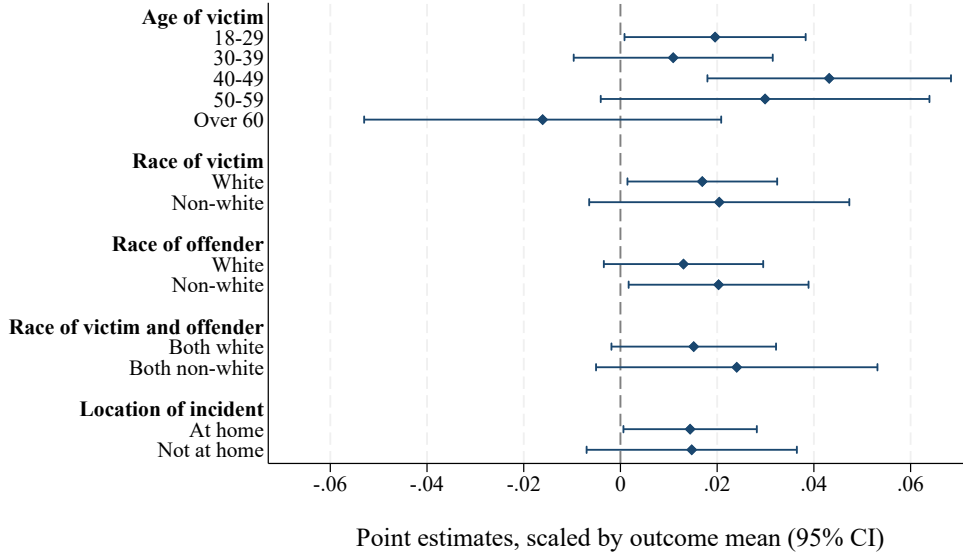


Notes: This figure reports the excess co-location rate of ballots for patterns occurring between 2 and 25 times across the city, scaled by pattern occurrence. Blue circular markers denote figures computed for the 2019 municipal elections, while red triangle markers denote the 2020 presidential primary. For each election, a linear trend is fitted. Bars denote 95% confidence intervals.

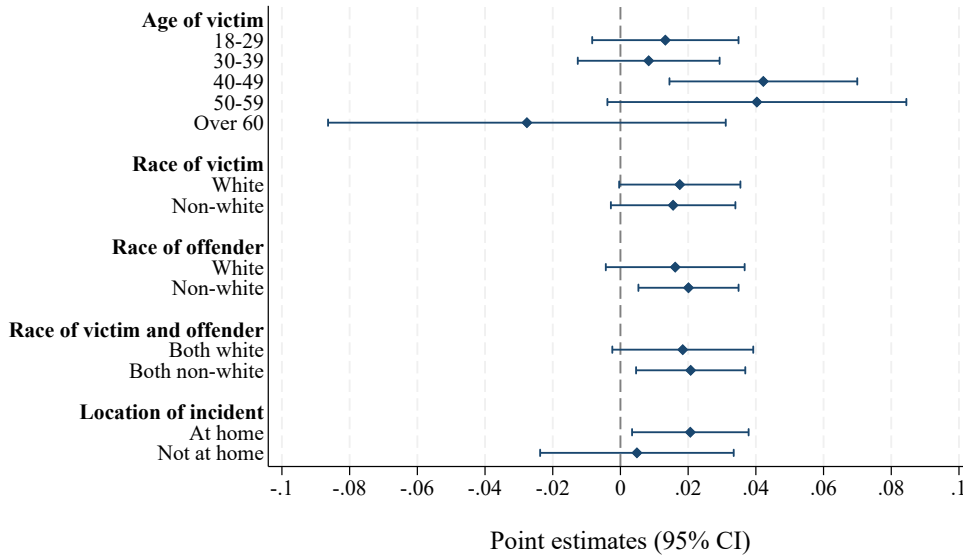
F Mail-voting period and within-couple conflict: additional analyses

F.1 By demographics

Figure F1: Effect of mail voting periods on intimate partner violence, by demographic characteristic



(a) At least one incident



(b) Total number of incidents

Notes: This figure reports OLS (Panel A) and PPML (Panel B) estimates for the effect of mail voting periods on intimate partner violence, by incident characteristic. The unit of observation is a county×day. The dependent variable is either an indicator variable equal to 1 if at least one incident was reported (Panel A) or the total number of incidents (Panel B) matching the corresponding characteristics. All coefficients are estimated from separate regressions. For comparability, OLS coefficients in panel A have been scaled to the outcome mean. All estimations control for maximum daily temperature and its square, total daily precipitation and its square, national holidays and 4 indicator variables equal to 1 in post-election days [0-13],[14-27],[28-41] or [42,55] in mail states. All estimations include county×year, days-to-Election Day (Tuesday after the first Monday) and day-of-the-month fixed effects. Bars represent 95% confidence intervals.

F.2 Other types of crimes

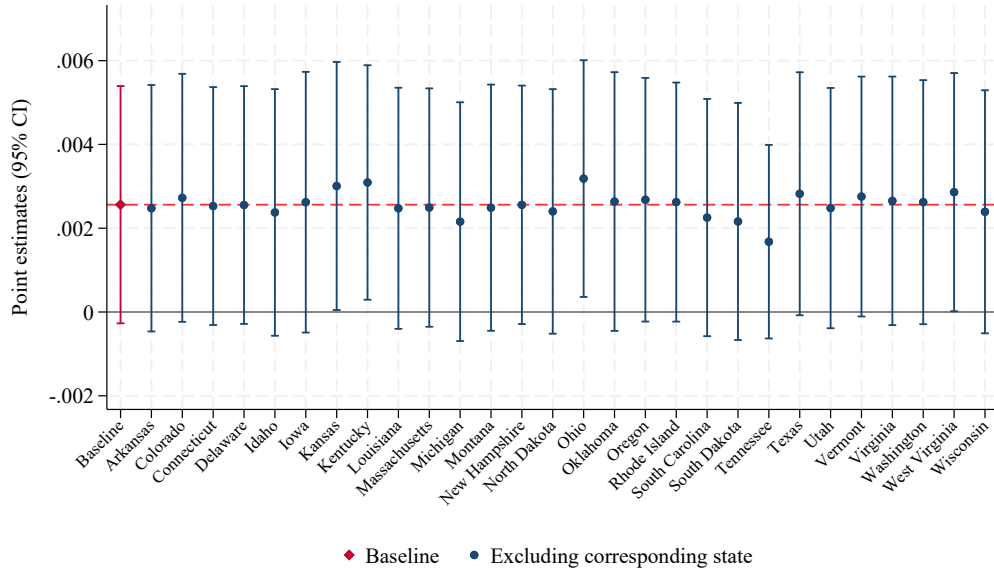
Table F1: Evolution of 5 types of crime during mail voting periods

	Vehicle theft	Drug offenses	Property damage	Shop- lifting	Other assaults
	(1)	(2)	(3)	(4)	(5)
Panel A: At least one incident					
Mail-voting period	0.0006 (0.0010)	0.0020 (0.0019)	-0.0009 (0.0022)	-0.0033** (0.0014)	0.0015 (0.0017)
Observations	5475847	5475847	5475847	5475847	5475847
Mean dep. var.	0.118	0.260	0.327	0.187	0.325
Panel B: Total number of incidents					
Mail-voting period	0.0120 (0.0253)	0.0159 (0.0195)	0.0185 (0.0123)	-0.0096 (0.0115)	0.0094 (0.0122)
Observations	4648476	4830029	4901620	4246637	4917315
Mean dep. var.	0.364	0.773	1.480	0.681	1.383
All panels					
County $\times$ year FE	✓	✓	✓	✓	✓
Days-to-Election Day FE	✓	✓	✓	✓	✓
Day-of-the-month FE	✓	✓	✓	✓	✓

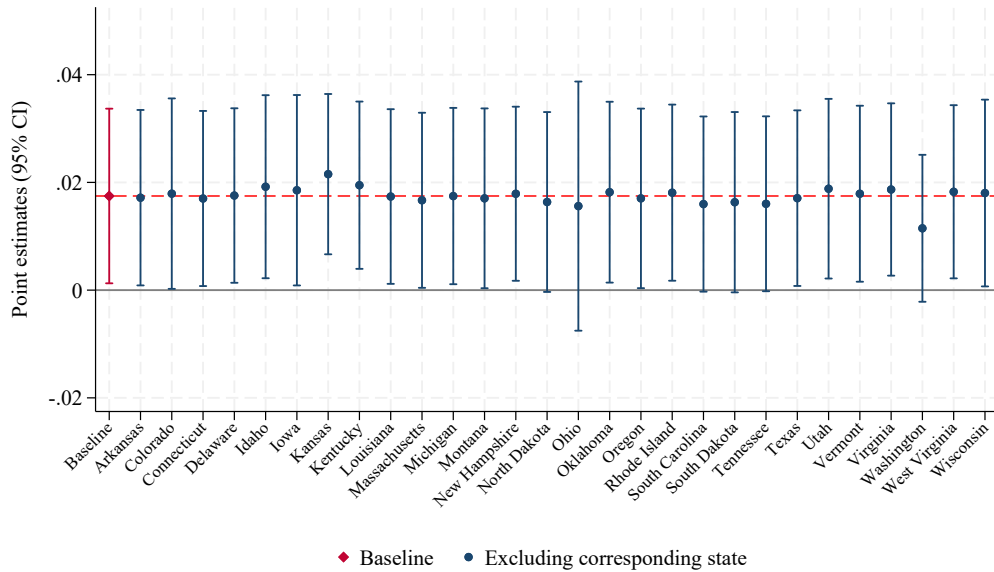
Notes: This table reports OLS (Panel A) and PPML (Panel B) estimates for the effect of mail voting periods on 5 types of crime. The unit of observation is a county $\times$ day. The dependent variable is either an indicator variable equal to 1 if at least one incident of that type was reported (Panel A) or the total number of incidents (Panel B). The corresponding types of crime are: Vehicle theft (column 1), Drug offenses (column 2), property damage (column 3), shoplifting (column 4), simple or aggravated assault and intimidation not involving partners (column 5). Mail-voting period is an indicator variable equal to 1 if mail ballots can have legally been sent out on that day. All estimations control for maximum daily temperature and its square, total daily precipitation and its square, national holidays and 4 indicator variables equal to 1 in post-election days [0-13],[14-27],[28-41] or [42,55] in mail states. All estimations include county $\times$ year, days-to-Election Day (Tuesday after the first Monday) and day-of-the-month fixed effects. Standard errors are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

F.3 Robustness checks

Figure F2: Effect of mail voting periods on intimate partner violence - Leave-one-out: states



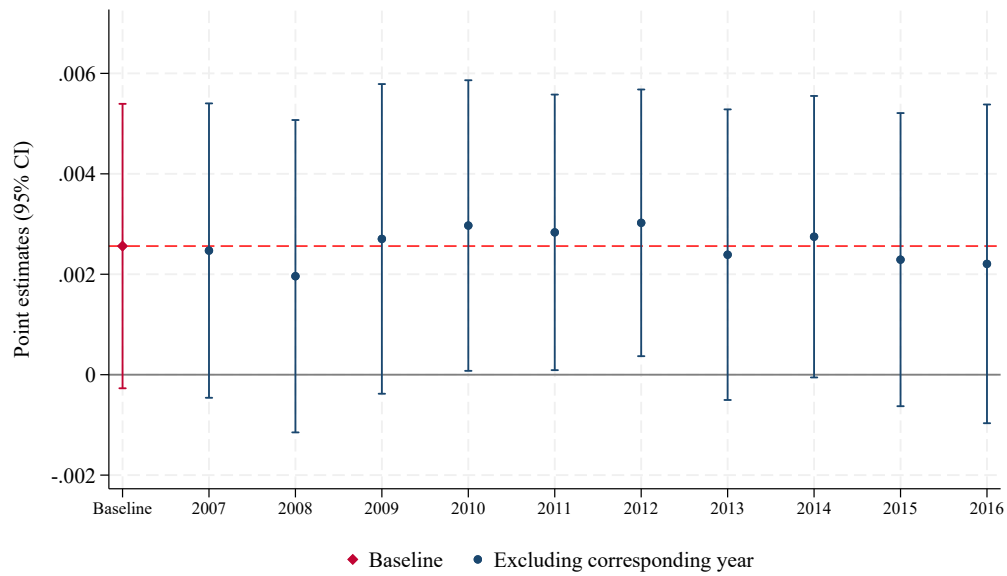
(a) At least one incident



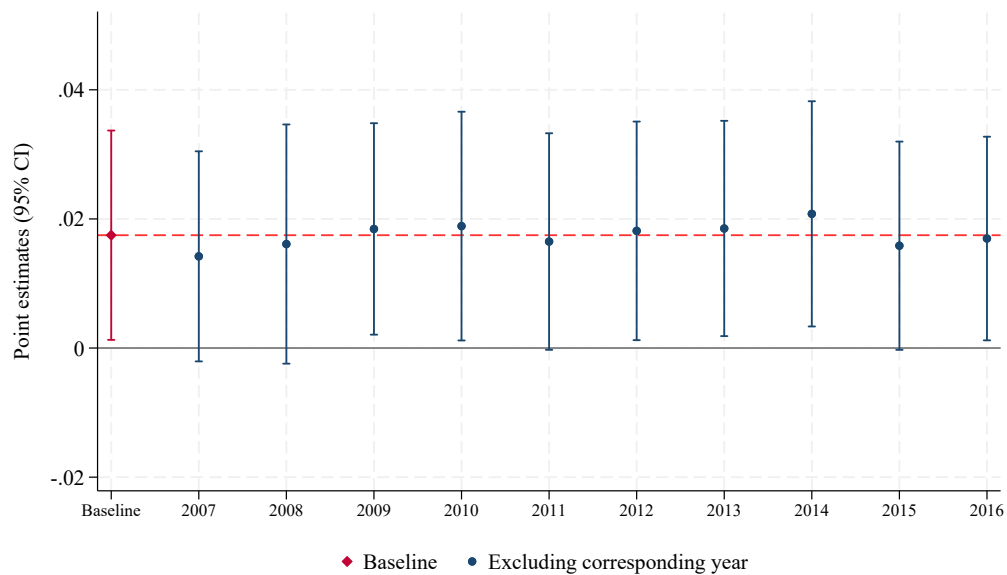
(b) Total number of incidents

Notes: This figure reports OLS (Panel A) and PPML (Panel B) estimates for the effect of mail voting periods on intimate partner violence, successively excluding one state. The unit of observation is a county $\times$ day. The dependent variable is either an indicator variable equal to 1 if at least one incident was reported (Panel A) or the total number of incidents (Panel B). Red markers and horizontal lines correspond to the baseline estimate of the coefficient associated with *Mail-voting period*. Blue markers correspond to the analogous estimates obtained when excluding the corresponding state from the same estimation. All estimations control for maximum daily temperature and its square, total daily precipitation and its square, national holidays and 4 indicator variables equal to 1 in post-election days [0-13],[14-27],[28-41] or [42,55] in mail states. All estimations include county $\times$ year, days-to-Election Day (Tuesday after the first Monday) and day-of-the-month fixed effects. Bars represent 95% confidence intervals.

Figure F3: Effect of mail voting periods on intimate partner violence - Leave-one-out: years



(a) At least one incident



(b) Total number of incidents

Notes: This figure reports OLS (Panel A) and PPML (Panel B) estimates for the effect of mail voting periods on intimate partner violence, successively excluding one year. The unit of observation is a county $\times$ day. The dependent variable is either an indicator variable equal 1 if at least one incident was reported (Panel A) or the total number of incidents (Panel B). Red markers and horizontal lines correspond to the baseline estimate of the coefficient associated with *Mail-voting period*. Blue markers correspond to the analogous estimates obtained when excluding the corresponding year from the same estimation. All estimations control for maximum daily temperature and its square, total daily precipitation and its square, national holidays and 4 indicator variables equal to 1 in post-election days [0-13],[14-27],[28-41] or [42,55] in mail states. All estimations include county $\times$ year, days-to-Election Day (Tuesday after the first Monday) and day-of-the-month fixed effects. Standard errors are clustered at the state level. Bars represent 95% confidence intervals.