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ORIGINAL ARTICLE

The influence of becoming a parent on political participation in the United States

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Abstract

Objective: Focusing on the United States, this study assesses the near-term influence of introducing a child into a household on a parent's likelihood of participating in politics.

Methods: Logistic regression analysis and exact matching of data from the November 2010 and November 2014 Current Population Surveys. Conditional, fixed-effects logistic reygression analysis of data from the 2010–2014 Cooperative Congressional Election Study Panel Survey.

Results: Cross-sectional regressions and exact-matching results indicate a negative relationship between becoming a parent and numerous forms of political participation, including voter turnout. However, recognizing potential threats to causal inference, analyses that leverage panel data and sensitivity analysis applied to the exact-matching estimates provide weaker support for a causal claim.

Conclusion: The introduction of a child into a household appears to be associated with attenuated parental civic engagement in the near term, but the evidence provides weaker support for a causal claim.

KEYWORDS
causal inference, electoral behavior, parenthood, political participation, turnout

Having a child brings many potential joys into a new parent's life, but this transformative experience also introduces a variety of stressors and places additional demands on the parent's physical and emotional energy (Nomaguchi and Milkie 2003; Pearlin 1989). Providing and arranging childcare absorbs time and money, housework increases, and leisure and downtime evaporate—navigating both family life, including a spousal relationship if present (Belsky and Hsieh 1998; Lawrence et al. 2008), and work life, for those in the workforce, become more complicated. Relatedly, research examining the relationship between parental status and psychological well-being finds that parents, especially those of young children, report lower levels of mental health relative to nonparents—as reflected in elevated distress and anxiety and a greater likelihood of experiencing depression (McLanahan and Adams 1987; Umberson and Williams 1999).

These many new stressors may also affect new parents' well-being in less obvious ways that nonetheless have broad societal implications. In particular, the demands of parenthood may attenuate someone's civic engagement. By placing constraints on time, attention, and social interaction, parenthood threatens to impede the key determinants of political participation in the near term (Verba, Schlozman, and Brady 1995), and since public policy tends to reflect voters' interests over those of nonvoters (Griffin and Newman 2005), a relative absence of new parents from the active electorate may harm their representation. For example, biases in representation could account for why elected officials prioritize and privilege programs for the elderly relative to those that invest in early childhood development—posing a dilemma for the long-term welfare of American society (see Cunha and Heckman 2007; Cunha et al. 2006; Heckman 2006).

Existing work provides only limited evidence about whether becoming a parent influences civic engagement in the United States.[1] Most of the work relies on cross-sectional analysis, comparing the participation rate of parents with that of nonparents—after adjusting for age, education, and several other baseline differences between these two sets of individuals (e.g., Schlozman, Burns, and Verba 1994; Wolfinger and Wolfinger 2008). As we explain more fully below, this work requires two strong assumptions. First, we must assume that this adjustment has eliminated all confounding variables. Second, we must assume that the models have not adjusted for any covariates that themselves are a consequence of becoming a parent. Several recent studies with more plausible identification strategies (e.g., leveraging panel data) examine the voter turnout of citizens in Western European nations (e.g., Belletinni et al. 2018; Bhatti et al. 2019), but the only comparable research on the United States relies on now decades-old data. For instance, Plutzer (2002) examines a panel of nonrepresentative respondents, but the most recent wave in the data on which he relies is fielded in 1982. Given both the many societal changes in the United States across the past 40 years that have implications for parenthood (e.g., shifts in fertility rates and increases in the average age at which women have their first child) and the major differences between the United States and Western European nations in public policy regarding parental leave and early childhood care and education (Janta 2014), the results of existing studies may not generalize to the contemporary United States.

We reconsider whether the introduction of a child into a household affects political participation. Focusing on the contemporary United States, we examine empirically a range of participatory acts, including voter turnout, attending political meetings, and donating money to campaigns. To do so, we examine data from the Voting and Registration Supplement (VRS) of the November 2010 and the November 2014 Current Population Surveys (CPSs), the Civic Engagement Supplement of the November 2010 CPS,[2] and the 2010–2014 Cooperative Congressional Election Survey (CCES).[3] Our results suggest that introducing a child into a household likely reduces a new parent's political participation by a small amount. Further, we demonstrate how our estimates change after adjusting for plausible violations of our assumptions. Our study offers a foundation on which future work can build by analyzing additional data and addressing follow-up questions—among our takeaways: Existing survey data from the United States provide only limited power for estimating the baseline effect of becoming a parent on various forms of political participation and insufficient power to address convincingly whether the characteristics of a parent (e.g., a parent's gender) play a conditioning role.

DOES BECOMING A PARENT INFLUENCE POLITICAL PARTICIPATION IN THE UNITED STATES? EXISTING RESEARCH AND THEORY

Presenting a life cycle theory of participation more than 50 years ago, Wilensky (1961) suggests that the presence of children in a household should depress voter turnout specifically. Several studies that relied on data from the 1950s and 1960s indeed suggested that becoming a parent attenuated electoral participation, perhaps especially among women (Campbell et al. 1960, pp. 487–488; Jennings 1979, p. 757; Pomper 1975,

[1] Elder and Greene (2012) provide an expansive discussion and assessment of the consequences of parenthood for political attitudes and behavior in the contemporary United States. However, they do not assess the implications of becoming a parent for near-term political participation.

[2] These CPSs come from the U.S. Bureau of the Census 2010 and 2014.

[3] We focus specifically on the influence on political participation of introducing one child into a household that did not have a child previously. In our analyses that rely on CPS data, we are able to ensure that the new child is less than 5 years old.

p. 73; see also Burns, Lehman Schlozman, and Verba 2001, especially Chap. 12); however, examinations of voter turnout in the 1970s reported either no relationship (McGlen 1980) or, surprisingly, a negative effect for men only (Welch 1977).

More recently published work again highlights the exhaustion and time demands that accompany raising young children, irrespective of the gender of the parent (Plutzer 1998). Plutzer (2002, p. 43) indicates that, even among those young adults who had already begun to establish a pattern of voting, a reversal may occur due to "a major life event that creates immediate demands for one's time and attention." Becoming a parent, especially of a young child, would appear to be an exemplar of this type of event. Perhaps surprisingly, Plutzer finds little support for a relationship between having children in a household and voting in a presidential election—although his results from a latent growth model provide some suggestive evidence that, for young adults, becoming a parent may inhibit their short-term turnout trajectory. His analyses rely on Jennings and Niemi's Student–Parent Socialization Study, with the 1982 panel wave (the third wave) as the most recent that he considers. Analyzing data from the 1990 cross-section of their Civic Participation Study, Schlozman, Burns, and Verba (1994) report that the presence of preschool children decreases the overall political activity of both men and women and by a similar amount.

Incorporating data from the Voting Supplement to the November 2000 CPS to assess explicitly Wilensky's argument, Wolfinger and Wolfinger (2008) focus on the turnout implications of the presence of children in a household in combination with various marital statuses. They conceptualize parental status as dichotomous—either a respondent has one or more children under 18 or does not have any children—and conclude that the presence of one or more children has a negative effect on voter turnout across the various marital statuses, controlling for a variety of other demographic characteristics. This investigation suffers from several limitations. First, the Wolfinger and Wolfinger specification strategy does not accommodate the possibility that variation in the age or number of children has implications for participation. For example, they assume that the influence on a parent's turnout of living with one infant child is equivalent to that of living with three teenagers. Second, the cross-sectional regression analyses on which their study relies impose several assumptions that cloud the causal interpretation of their estimates.[4]

THREATS TO CAUSAL INFERENCE

In a typical cross-sectional model that regresses an indicator of political participation on an indicator of parental status, the resultant parental status coefficient represents the difference between the participation propensity of parents and that of nonparents, conditional on the values of the other covariates included on the right-hand side of the model. To give this estimate a causal interpretation, we must assume that the model specification accounts for all of the differences between parents and nonparents that could confound the estimate. Moreover, we must assume that becoming a parent does not influence any of the other right-hand-side covariates in the model. Each assumption is tenuous at best.

The regression is unlikely to remove all of the differences between parents and nonparents. New parents differ in many ways from those yet to have children. Many of these differences exist even before the child arrives. Drawing from the causal inference literature, we can think of these differences as "pretreatment." If these pretreatment differences also affect participation, failing to adjust for them will lead to a biased estimate of the effect of becoming a parent on participation. Past work has sought to avoid this problem by attempting to control for other established causes of taking part in politics, measures of which are commonly included on political survey batteries. These variables, which include individuals' age, education, employment, income, and residential stability (Wolfinger and Wolfinger 2008), explain much of the individual variation in participation (Leighley and Nagler 2013; Rosenstone and Hansen 1993; Wolfinger and Rosenstone 1980). Adjusting for them should therefore significantly reduce the potential for bias.

[4] Any analysis of cross-sectional data, even if multivariate, that makes a causal claim rests on potentially tenuous assumptions. We are not indicting the Wolfinger and Wolfinger (2008) study as unique in this regard.

Unfortunately, research relying on cross-sectional survey data cannot feasibly capture all of the important pretreatment differences between new parents and those yet to have children. Many small, unobserved differences may add up to produce a large bias in the estimated effect. While cross-sectional comparisons may fail to account for many unobserved differences, panel data can account for much of this heterogeneity by eliminating the confounding influence of factors that are constant within individuals (see, e.g., Pattie, Whitworth, and Johnston 2015). In either case, scholars can assess the sensitivity of the estimated effect to different assumptions about the remaining bias induced by differences between the treatment group and the control group (Rosenbaum and Rubin 1983).

Controlling for education, employment, marital status, and other covariates reduces the bias arising from pretreatment differences but may also introduce posttreatment bias. Adjusting estimates for covariates that are a consequence of becoming a parent eliminates a causal mechanism on the path from becoming a parent to participating in politics, muting the apparent effect of becoming a parent (Rosenbaum 1984). Ideally, an analysis adjusts only for pretreatment covariates but not for posttreatment covariates. In practice, cross-sectional survey data are not well-suited for accomplishing this feat because a cross-sectional survey measures all variables at the same time. As a result, no clean solution exists for eliminating bias from variables that are both a cause and an effect of a treatment. Several such variables may bias the estimated effect of becoming a parent on participation. For instance, moving to a new neighborhood discourages taking part in politics by creating new burdens, such as the need to register to vote (Squire, Wolfinger, and Glass 1987). This relocation may also discourage parenthood because would-be parents lack a sufficient support network in their new neighborhood. Yet new parents may be more likely to move to a new residence—seeking more space, better schools, or a lower mortgage. In such cases, adjusting for the covariate may reduce pretreatment differences, making treatment and control more comparable, and also promote posttreatment bias. Panel data can overcome this problem by providing insight into the temporal order of the variation.[5] Sensitivity analysis can also help by allowing researchers to explore how estimates change under different assumptions about the biases arising from both pretreatment variables and posttreatment variables (Rosenbaum 1984).

Even if we accept the estimates of previous studies as unbiased, they may not generalize to the contemporary environment because, in most cases, their data are several decades old. Family structure has changed in many ways across the past decades, with fewer women having children, those women who do have children having their first child at an older age, fewer children per household, more parents raising children by themselves, and parents spending longer hours at work (O'Neill and Gidengil 2017).[6] These changes may either magnify or attenuate the effect of parenthood on electoral participation. With work taking a greater share of parents' time, new parents may have even lower levels of time and energy than in the past. Yet the decline in women having children may signal that, on average, parents today are better prepared for parenthood than were parents in prior generations. If so, parenthood may be less taxing and thus impose a lower barrier to political participation. The potential for countervailing influences suggests a need for research that relies on recent data to determine the magnitude of effects in the contemporary environment.

[5] From a causal inference perspective, the most compelling, contemporary examinations of the influence of becoming a parent on voter turnout rely on data from Western Europe. Bhatti et al. (2019) incorporate individual-level register data sets from Finland and Denmark to assess the effect of having a baby on the electoral participation in local elections of expectant and new parents. They find a sizable, yet quite short-lived, attenuating effect on turnout likelihood immediately after childbirth. Another recent study incorporates panel data on the more than 380,000 residents of Bologna, Italy, and finds that introducing a young child into a household demonstrates a significant, negative influence on the voter turnout of women but not of men (Belletinni et al. 2018). However, in this Italian study, the attenuating effect on the participation of women, as well as the differential effect between men and women, is each quite small in terms of substantive magnitude—approximately two percentage points. Moreover, it is worth noting that these small effects manifest even in an Italian society in which traditional gender roles persist.

Both studies benefit from very large datasets that provide tremendous statistical power—and in the case of the Italian study, from a panel component as well. However, a major question is whether their findings generalize to the United States. Both Finland and Denmark represent very strong welfare states that provide generous state-subsidized social services regarding both parental leave and care for young children. Relative to the United States, nations in the European Union more generally devote a larger share of government resources to early childhood care and education (Janta 2014), which suggests that, on average, American parents, bear higher costs—in terms of personal finances, time, and psychological stress—when it comes to raising a young child.

[6] http://www.pewsocialtrends.org/2015/12/17/1-the-american-family-today/.

SOURCES OF HETEROGENEITY

Overcoming these threats to causal inference is a daunting task, but we can do so only if we limit the sources of heterogeneity that might moderate this effect. The effect we seek to identify is unlikely to be uniform across new parents since neither the benefits nor the burdens of parenthood are uniform for them. Becoming a parent may mobilize some individuals, render others slightly less politically active, and demobilize entirely yet others. For instance, other literature recognizes that the influence of parenthood on civic life likely depends on the age of a child (Belletinni et al. 2018) and the form of participation. School-age children appear to increase parents' community engagement and their involvement with the politics that surround schools specifically (Campbell 2006; Rosenstone and Hansen 1993; Voorpostel and Coffe 2012). In an early study that relies on the first two waves (the 1965 and 1973 waves) of his student–parent socialization study, Jennings (1979, p. 755) focuses on the participatory effect of having *school-age children*, concluding that "parenthood, as one stage in the life cycle, has a trivial or debilitating impact in the domain of national politics but a highly salutary one in school politics."

It is plausible that, on average, parenthood has mobilizing effects in the longer term. Parents' social networks expand as their children age—for example, via school-related and church-related activities—in ways that provide opportunities for parents both to be recruited into and, subsequently, to take part in political discussion and action. We also acknowledge that due to changes in parents' self-image and perspective on the future, parents' sense of civic duty and perceived stake in some policy areas—for example, education, healthcare, public safety, and the environment—may increase. Finally, some mobilization effects appear to trickle up directly from school-age children themselves—for example, via civics curriculum (McDevitt and Chaffee 2002). In other words, having a child does not have a single effect on participation, but likely many that vary over time. Since identifying and assessing any one of these effects requires careful attention, we cannot do justice to all of them in a single study. We, therefore, focus our attention on the effect of introducing a *young* child into a household on a parent's *near-term* civic engagement.

Gender provides another source of heterogeneity but is too important to relegate only to future work. Numerous studies indicate that the burdens of parenting a young child typically fall more heavily on a mother (Bianchi, Robinson, and Milkie 2006; Bianchi et al. 2000; Burns, Lehman Schlozman, and Verba 1997, 2001; Quaranta and Sani 2018). Anxo et al. (2011) investigation of time use surveys in the United States (as well as in Italy, Sweden, and France) reveals that child-care responsibilities continue to fall more heavily on women, regardless of their participation in the labor market. On initial review, a time constraints explanation would appear to suggest that the attenuating effect on political participation of having a young child should be greater for women. Tempering this expectation, Burns, Lehman Schlozman, and Verba (2001) find no evidence that an absence of free time negatively affects the political activity of women.[7] Furthermore, on average, the gap in free time between the male parents and the female parents of preschoolers is relatively small, in part because of the increase in work hours among this category of men, and the decrease in labor force participation among these women (Gauthier and Furstenberg 2002; Schlozman, Burns, and Verba 1994). Given these possibilities, we examine not only average treatment effects, but the estimates for women and men separately in each data set we analyze.

INITIAL DATA ANALYSIS

Considering electoral participation initially, whereas previous studies have focused on presidential elections, we assess midterm turnout with data from the 2010 and 2014 November CPS. First, doing so simply provides a novel contribution to this literature. More importantly, disruptive life course events quite likely have greater implications for participation in lower-stimulus election environments. For those around the

[7] Consistent with Burns, Schlozman, and Verba's (2001) conclusion regarding the absence of an effect for time constraints among American women, O'Neill and Gidengil's (2017, p. 277) study of Canadian women concludes that "what is striking is how little difference motherhood and its associated responsibilities make to women's political and civic activities."

threshold of electoral participation, with less campaign information flowing and fewer mobilization efforts taking place in an off-year, a major event in someone's personal life that taxes time and energy (e.g., becoming a parent) is more likely to keep that person at home. Also, practically speaking, given the waves available in the 2010–2014 CCES Panel Survey (i.e., two midterm elections and only one presidential election), we can only fully leverage the panel data in an examination of midterm participation. Moving beyond voter turnout, we also extend our investigation to consider a wide range of conventional acts of political participation, relying on data from the Civic Engagement Supplement of the November 2010 CPS and the CCES Panel Survey.

As discussed above, previous work has typically relied on cross-sectional regression analysis to determine whether becoming a parent influences participation. For example, turnout investigations typically estimate a regression model that specifies parental status along with a long list of covariates that predict turnout. Following this conventional approach and relying on the CPS data, Tables 1 and 2 report regressions that specify the demographic controls commonly used in these models. Focusing on the transition from zero to one child, we remove from the data, parents of two or more children. We also remove respondents younger than 18 or otherwise ineligible to vote. Since we are interested in the effect of becoming a parent, we examine new parents, defined as those with a child under age 5.[8]

In both the 2010 and 2014 turnout models (see Table 1), the coefficient associated with parental status is negative and statistically significant, suggesting that parents of a young child are less likely to vote than are nonparents. In 2010, the coefficient suggests that the odds of these parents turning out to vote are only $\exp(-0.32) = 0.73$ (95 percent CI [0.66, 0.79]) those of nonparents, conditional on the other covariates. In 2014, the negative relationship is a bit weaker, with the odds of parents voting $\exp(-0.17) = 0.85$ [0.78, 0.92] those of nonparents. We find a similar result for the other participatory acts: a significant, negative relationship between parenthood and participation (see Table 2). Becoming a parent appears to be associated with a lower likelihood of discussing politics, contacting a public official, participating in a community group, participating in a civic group, and serving as a group officer, controlling for other factors.[9]

Based on these preliminary CPS results, we may be tempted to conclude that becoming a parent discourages political participation—that is, that our general expectation receives strong support. As discussed previously, however, we should not interpret these initial coefficients associated with parental status as causal estimates. Thankfully, each data set provides some useful features to help us assess the robustness of this finding.

APPROACH TO CAUSAL INFERENCE

We seek to estimate the average treatment effect on the treated, which represents the effect of parenthood for those who have recently become parents. The CPS studies are useful for this purpose because each of the VRSs and the Civic Engagement Supplement provides a sample exceeding 60,000 useable

[8] A common choice in this literature, this cut-off at age 5 reflects the age at which many children start attending kindergarten (e.g., Burns, Schlozman and Verba 2001; McGlen 1979; Plutzer 2002).

[9] The questions from the November 2010 CPS Civic Engagement Supplement on which we rely asked respondents how frequently they had discussed politics in a typical month in the prior year and whether, in the prior year, they had contacted a public official to express their opinion, participated in a school group, neighborhood, or community association (such as a parent-teacher association (PTA)), participated in a service or civic organization (such as American Legion or Lions Club), or been an officer or served on a committee or group of any organization or club. In addition to providing information on a respondent's validated midterm turnout, the 2010–2014 CCES Panel Study asked whether a respondent worked for a candidate or campaign, put up a political sign, donated money to a candidate, campaign, or political organization, and/or attended a political meeting in the prior year.

Our analyses are limited to the participation items commonly included on national surveys. The limited nature of the participation items may mask a changing meaning of "political participation" that expands beyond the commonly included items. For example, the traditional participation battery from the American National Election Study (ANES) is biased for comparisons by age, education, wealth, and gender (Pietryka and MacIntosh 2022). This finding suggests that acts of participation qualitatively differ for people of different ages, socioeconomic statuses, and genders and mirrors work suggesting that traditional political knowledge items underestimate women's knowledge because they omit items focusing on the political matters most relevant to women (Dolan 2011).

TABLE 1 Regressions of turnout on parenthood status and controls using Current Population Surveys (CPSs) data

	2010	2014
Parent of young child (0 = *no*; 1 = *yes*)	−0.32 (0.04)*	−0.16 (0.04)*
Age (in years)	0.03 (0.00)*	0.03 (0.00)*
Female (0 = *no*; 1 = *yes*)	0.04 (0.02)*	0.07 (0.02)*
Student (0 = *no*; 1 = *yes*)	0.23 (0.06)*	0.26 (0.06)*
Employment status (reference = unemployed/not in labor force)		
Employed (0 = *no*; 1 = *yes*)	0.35 (0.03)*	0.37 (0.03)*
Retired (0 = *no*; 1 = yes)	0.36 (0.03)*	0.40 (0.03)*
Education (reference = less than HS graduate)		
HS graduate (0 = *no*; 1 = *yes*)	0.79 (0.03)*	0.74 (0.03)*
Some college (0 = *no*; 1 = *yes*)	1.39 (0.04)*	1.29 (0.04)*
College graduate (0 = *no*; 1 = *yes*)	1.92 (0.04)*	1.81 (0.04)*
Advanced degree (0 = *no*; 1 = *yes*)	2.14 (0.05)*	2.05 (0.05)*
Relationship (reference = never married)		
Married (0 = *no*; 1 = *yes*)	0.38 (0.03)*	0.29 (0.03)*
Separated/divorced (0 = *no*; 1 = *yes*)	−0.10 (0.03)*	−0.16 (0.03)*
Widowed (0 = *no*; 1 = *yes*)	−0.29 (0.05)*	−0.32 (0.04)*
Income (reference = in first quartile)		
In second quartile (0 = *no*; 1 = *yes*)	0.17 (0.03)*	0.21 (0.03)*
In third quartile (0 = *no*; 1 = *yes*)	0.31 (0.03)*	0.39 (0.03)*
In fourth quartile (0 = *no*; 1 = *yes*)	0.39 (0.03)*	0.45 (0.03)*
Time in residence (reference = less than 1 year)		
1–2 years (0 = *no*; 1 = *yes*)	0.36 (0.04)*	0.43 (0.04)*
3–4 years (0 = *no*; 1 = *yes*)	0.65 (0.04)*	0.66 (0.04)*
5+ years (0 = *no*; 1 = *yes*)	0.99 (0.03)*	0.98 (0.03)*
Race (reference = white)		
Black (0 = *no*; 1 = *yes*)	0.50 (0.03)*	0.47 (0.03)*
Asian (0 = *no*; 1 = *yes*)	−0.85 (0.05)*	−0.87 (0.05)*
Other (0 = *no*; 1 = *yes*)	0.02 (0.06)	−0.09 (0.05)
Intercept	−3.96 (0.06)*	−4.28 (0.06)*
AIC	67,895.45	72,019.68
BIC	68,102.69	72,227.92
Log likelihood	−33,924.73	−35,986.84
Deviance	67,849.45	71,973.68
Num. obs.	60,483	63,200

*$p < 0.05$.
Source: CPS 2010 and 2014 Voting and Registration Supplements.

TABLE 2 Regressions of nonvoting participation on parenthood status and controls using CPS data

	1. Discuss	2. Contact	3. Community	4. Civic	5. Officer
Parent of young child (0 = *no*; 1 = *yes*)	−0.18 (0.03)*	−0.40 (0.07)*	−0.25 (0.06)*	−0.41 (0.09)*	−0.52 (0.08)*
Age (in years)	0.01 (0.00)*	0.02 (0.00)*	0.00 (0.00)*	0.03 (0.00)*	0.02 (0.00)*
Female (0 = *no*; 1 = *yes*)	−0.17 (0.02)*	−0.16 (0.03)*	0.41 (0.03)*	−0.37 (0.03)*	0.20 (0.03)*
Student (0 = *no*; 1 = *yes*)	0.21 (0.05)*	−0.23 (0.11)*	0.82 (0.08)*	0.64 (0.11)*	0.87 (0.10)*
Employment status (reference = unemployed/not in labor force)					
Employed (0 = *no*; 1 = *yes*)	0.12 (0.02)*	−0.01 (0.04)	0.11 (0.04)*	0.14 (0.05)*	0.38 (0.05)*
Retired (0 = *no*; 1 = *yes*)	0.02 (0.03)	−0.34 (0.05)*	−0.03 (0.05)	0.13 (0.06)*	0.09 (0.05)
Education (reference = less than HS graduate)					
HS graduate (0 = *no*; 1 = *yes*)	0.46 (0.03)*	0.88 (0.07)*	0.32 (0.06)*	0.66 (0.07)*	0.89 (0.09)*
Some college (0 = *no*; 1 = *yes*)	0.89 (0.03)*	1.53 (0.07)*	0.91 (0.06)*	1.08 (0.07)*	1.53 (0.09)*
College graduate (0 = *no*; 1 = *yes*)	1.17 (0.03)*	1.89 (0.07)*	1.36 (0.06)*	1.17 (0.07)*	2.00 (0.09)*
Advanced degree (0 = *no*; 1 = *yes*)	1.39 (0.03)*	2.33 (0.08)*	1.78 (0.07)*	1.43 (0.08)*	2.45 (0.09)*
Relationship (reference = never married)					
Married (0 = *no*; 1 = *yes*)	0.17 (0.02)*	0.30 (0.04)*	0.44 (0.04)*	0.24 (0.05)*	0.38 (0.04)*
Separated/divorced (0 = *no*; 1 = *yes*)	0.02 (0.03)	0.21 (0.05)*	0.26 (0.05)*	0.11 (0.06)	0.02 (0.05)
Widowed (0 = *no*; 1 = *yes*)	−0.09 (0.04)*	−0.00 (0.07)	0.19 (0.07)*	0.11 (0.07)	0.03 (0.07)
Income (reference = in first quartile)					
In second quartile (0 = *no*; 1 = *yes*)	0.14 (0.02)*	0.06 (0.04)	0.05 (0.04)	0.15 (0.05)*	0.23 (0.05)*
In third quartile (0 = *no*; 1 = *yes*)	0.28 (0.02)*	0.15 (0.04)*	0.25 (0.04)*	0.34 (0.05)*	0.40 (0.05)*
In fourth quartile (0 = *no*; 1 = *yes*)	0.40 (0.02)*	0.25 (0.04)*	0.45 (0.04)*	0.40 (0.05)*	0.50 (0.05)*
Race (reference = white)					
Black (0 = *no*; 1 = *yes*)	0.08 (0.03)*	−0.48 (0.06)*	0.31 (0.04)*	−0.50 (0.07)*	−0.06 (0.05)
Asian (0 = *no*; 1 = *yes*)	−0.84 (0.04)*	−1.34 (0.10)*	−0.69 (0.07)*	−1.10 (0.11)*	−0.98 (0.09)*
Other (0 = *no*; 1 = *yes*)	−0.01 (0.05)	0.05 (0.08)	0.07 (0.08)	−0.13 (0.10)	−0.08 (0.10)
Thresholds					
0|1 (intercept)	0.73 (0.04)*	−4.71 (0.10)*	−3.89 (0.08)*	−4.94 (0.10)*	−5.69 (0.11)*
1|2	1.52 (0.04)*				
2|3	2.45 (0.04)*				
3|4	3.77 (0.04)*				
AIC	179,236.83	41,932.35	42,815.00	34,299.20	37,517.59
BIC	179,444.04	42,113.10	42,995.77	34,479.96	37,698.32
Log likelihood	−89,595.41	−20,946.17	−21,387.50	−17,129.60	−18,738.80
Deviance	179,190.83	41,892.35	42,775.00	34,259.20	37,477.59
Num. obs.	60,446	62,182	62,240	62,198	62,101

Note: Model 1 (discuss politics) is an ordered logistic regression, and the remaining models are binary logistic regressions.
*$p < 0.05$.
Source: CPS 2010 Civic Engagement Supplement.

respondents. The respondents to the VRSs were asked about their status as a parent and whether they had voted in the recent election. The respondents to the Civic Engagement Supplement were also asked about their status as a parent and whether they had taken part in the other forms of participation indicated above. These large samples enable an exact-matching approach in which we match parents of one child under age 5 to all nonparents who have the same values on matched covariates. In this analysis, we match parents to nonparents on family income, age, gender, education level, student status, marital status, race, and employment status.[10] After matching, we analyze the data using weights to equalize the number of parents and nonparents with any given set of covariate values. With this approach, we can be confident that any matched pretreatment covariate will not bias the estimated effect. Unfortunately, the CPS contains relatively few potential covariates, and unobserved variables may still bias the estimate. Likewise, we must recognize that the estimate will suffer from posttreatment bias to the extent that becoming a parent causes any of the matched covariates. To illustrate, we also apply sensitivity analysis to our exact-matching estimates to determine whether conclusions change after adjusting for varying levels of assumed bias.

The 2010–2014 CCES Panel Survey provides greater leverage for eliminating confounds. We can compare respondents in 2014 to themselves in 2010, effectively treating each respondent as his or her own control. This approach eliminates confounds arising from *all* stable respondent characteristics—measured or not. Such stable characteristics include time-invariant demographics, as well as traits that are difficult to measure—such as family upbringing and political socialization. In addition to the leverage that these CCES panel data provide regarding causal inference, they also contain a validated measure of voter turnout for both of the years that we investigate (see Ansolabehere and Hersh 2012).

As before, we remove parents of two or more children and ineligible voters from the data.[11] Since the conditional, fixed-effects logistic regressions examine the influence of change in the variables of interest, we remove individuals who were already parents in 2010 from the CCES data. Before analysis, we use multiple imputations to correct for problems created by missing data. The results we present are estimated on 20 imputed data sets and then combined using Rubin's rules (Rubin 2009). The imputed data sets were generated in R using the Amelia II package (Honaker, King, and Blackwell 2011).[12]

CPS 2010 AND 2014 MATCHING ANALYSIS

Approximately 4000 respondents in each year of the CPS data were parents of a single child under age 5. For example, focusing on voter turnout, we can see from Figure 1a that in both elections, these parents voted at a lower rate than did nonparents. We should be skeptical that this difference is causal because the comparison ignores the systematic differences between parents and nonparents. Yet the *descriptive* difference is nonetheless important for understanding contemporary U.S. electoral politics. New parents' absence from the electorate gives legislators relatively lower incentive to provide policies that might address these parents' many challenges. But understanding *why* new parents participate at lower rates require us

[10] For voter turnout, we also match on residential stability—the residential stability variable is not part of the Civic Engagement Supplement. Before matching, we coarsened several of these covariates from a broader set of categories. For age, we coarsened the data into a bin of 18 and 19 years old, followed by bins spanning 5-year increments (e.g., 20 to 24 years old, 25 to 29 years old, etc.). For residential stability, we grouped people into one of three categories: lived at current residence for less than 1 year, for 1 to 4 years, or for 5 years or longer. For education, we matched on the following values: less than a high school diploma, only a high school diploma, some college, bachelor's degree, and advanced degree. For marital status, we combined "married–spouse absent," "divorced," and "separated" into one category. For race, we created three groups: white, black, and other. For employment, we created an indicator of whether the individual reports being "employed-at work." To illustrate, in the 2010 Voting Supplement data, we find 13,547 nonparents who provide exact match for 3019 parents. No exact matches exist for 1216 parents, whom we thus omit from the analysis. In the 2014 Voting Supplement data, we find 16,959 nonparents matched to 3345 parents, omitting 785 parents without exact matches. In additional analyses, we have (1) increased the coarsening to find exact matches for more parents and (2) used genetic matching for all parents. These approaches yield results similar to those reported here.

[11] The results are broadly similar when we include these parents.

[12] The CCES panel features relatively little missing data among the covariates we use, except for the family income measure. To address the extent of missingness on this variable, the model uses respondents' 2010 reported income to impute their 2014 income and vice-versa. To provide numeric stability in the face of this covariance structure, the imputation model uses a ridge prior equivalent to 1 percent of the data.

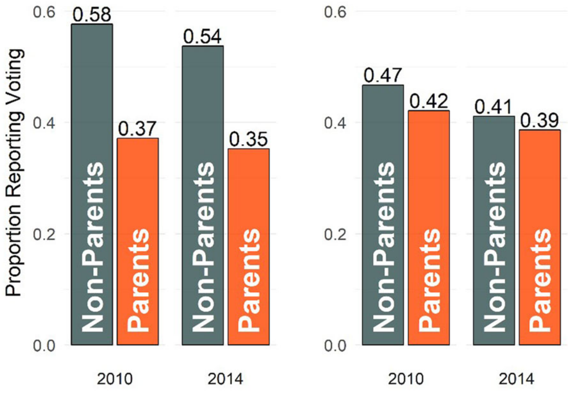

FIGURE 1 Turnout levels before and after matching. The bars represent the proportion of nonparents and parents voting in (a) the unmatched data and (b) the weighted, matched sample. *Source*: Current Population Survey (CPS) 2010 and 2014 Voting and Registration Supplements (VRSs)

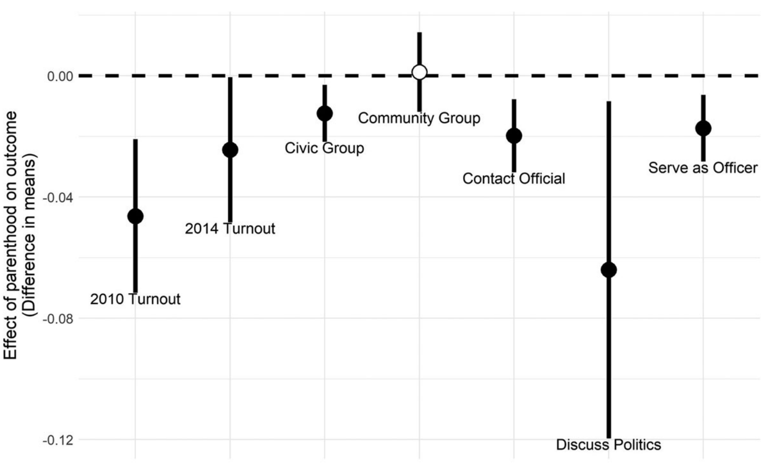

FIGURE 2 The estimated treatment effects after matching. The dots represent the difference between parents and nonparents for the outcome variable in the weighted, matched samples. The lines represent 95 percent confidence intervals. *Source*: 2010 and 2014 CPS Voting and Registration and Civic Engagement Supplements

to examine the causes of this descriptive difference. Figure 1b, therefore, compares the turnout among parents to that of their matched comparison group. Here, we see that much, but not all, of the apparent effect of parenthood disappears after conditioning on observable covariates.[13] In other words, most of the descriptive difference shown in Figure 1a seems to arise for reasons other than the effect of parenthood on turnout.

For a direct estimate of the treatment effect, we use the *matched* data to estimate the difference between parents and nonparents in mean levels of turnout, discussing politics, contacting a public official, participating in a community group, participating in a civic group, and serving as an officer in a group. As shown in Figure 2, these estimates roughly mirror those from the multiple regression models above—statistically significant, negative effects for parenthood, except for the estimate of the effect on participating in a community group, which becomes negligibly positive and indistinguishable from zero.

Although the average effects appear small, one might expect larger effects for some types of parents. As discussed above, women may bear a greater share of the demobilizing burdens of parenthood. Fitting

[13] Note that the difference between matched parents and nonparents is smaller than the unmatched difference both because matched nonparents turn out at lower rates than all nonparents, *and* because matched parents turn out at higher rates than all parents. Alternative matching approaches that include more parents (e.g., greater coarsening, genetic matching) nonetheless yield similar estimates of the difference between parents and nonparents.

an interactive model to our matched data, we find that, if anything, the apparent demobilizing effect of parenthood on turnout is *weaker* for women than it is for men in both 2010 ($\hat{\beta}_{Parent \times Female} = 0.02$; 95 *percent* CI [−0.03, 0.07]) and 2014 ($\hat{\beta}_{Parent \times Female} = 0.01$; 95 *percent* CI [−0.03, 0.05]). However, considering their confidence intervals, the estimates from the interactive models are consistent with no gender difference in the effect, a larger effect for men, or a larger effect for women. Thus, the estimates lack the precision necessary to provide meaningful information about heterogeneous effects by gender (see Gross 2015; Rainey 2014). Given the importance of gender, we continue to examine its role but refrain from further analysis of heterogeneous treatment effects because underpowered estimates are often misleading in both magnitude and direction (Gelman and Carlin 2014). Rather than pursuing analyses that require more statistical power than these data can provide, we focus instead on providing credible estimates of average effects.

Although statistically significant, the negative effects in Figure 2 are relatively small substantively and potentially vulnerable to remaining pretreatment differences between our matched parents and nonparents.[14] To illustrate, in online Appendix A, we perform a sensitivity analysis for the estimates in Figure 2, examining how they would change after adjusting for potential bias. In general, the sensitivity analysis provides evidence of a modest demobilizing effect of parenthood, but only under the assumption that the matching removed most of the pretreatment differences that make parents less likely to vote than nonparents. This is a relatively strong assumption: Because of the limitations of the CPS data, the matching adjusted for several demographic variables, while leaving unaccounted other well-established influences on turnout, such as political interest and exposure to mobilization efforts. We, therefore, turn next to the CCES data, which can account for many more pretreatment differences.

2010–14 CCES PANEL ANALYSIS

Turning to the 2010–2014 CCES Panel Survey, we rely on the 2010 and 2014 waves. Each of these waves provides information on a respondent's validated midterm turnout, as well as whether the respondent reported working for a candidate or campaign, putting up a political sign, donating money to a candidate, campaign, or political organization, and/or attending a political meeting in the prior year. We identify respondents who became a parent as those who indicated that they were not a parent in the 2010 wave but had become the parent or guardian of a child under 18 by the 2014 wave.[15] Under this definition, 154 of the 7633 respondents without children in 2010 became parents by the 2014 wave. We use these individuals to estimate a series of conditional, fixed-effects logistic regressions for each outcome variable.[16] As discussed above, this approach eliminates all confounds arising from features of individuals that do not vary over time. Nonetheless, any changes that an individual experienced between the 2010 and 2014 waves may still bias the estimate. To avoid this bias, we must identify and control for the time-varying causes, but not the effects, of parenthood.

Since no clean partitioning between cause and effect is possible for many of these variables, we estimate three models: a baseline model that includes no individual-level controls, an intermediate model that includes additional controls only for the variables that most plausibly precede parenthood, and a full model that controls for a large set of variables that may vary pretreatment or posttreatment.[17] As we add

[14] Since we coarsened several matched covariates, small differences exist between matched parents and nonparents. Including these imbalanced variables as controls in matched regressions leads to substantive conclusions similar to those we report here.

[15] To reduce heterogeneity, the analyses that we present omit individuals who had more than one child at the time of the 2014 wave. Including these individuals leads to substantive conclusions similar to those presented here.

[16] For detailed discussions of the conditional, fixed-effects logistic regression model, see Allison (2009), Cameron and Trivedi (2010), and Wooldridge (2010).

[17] The intermediate model controls for whether the individual graduated college, was contacted by a political party in the months before the election, and experienced a change in family income using income quartile. The full model includes the intermediate controls, as well as measures of change in church attendance frequency, political interest, county of residence, and the following statuses: marital, homeowning, student, and employment. Of course, even the intermediate controls may feature posttreatment variation. Nonetheless, all three models yield similar results, suggesting that the particular controls are unlikely to change the substantive conclusions.

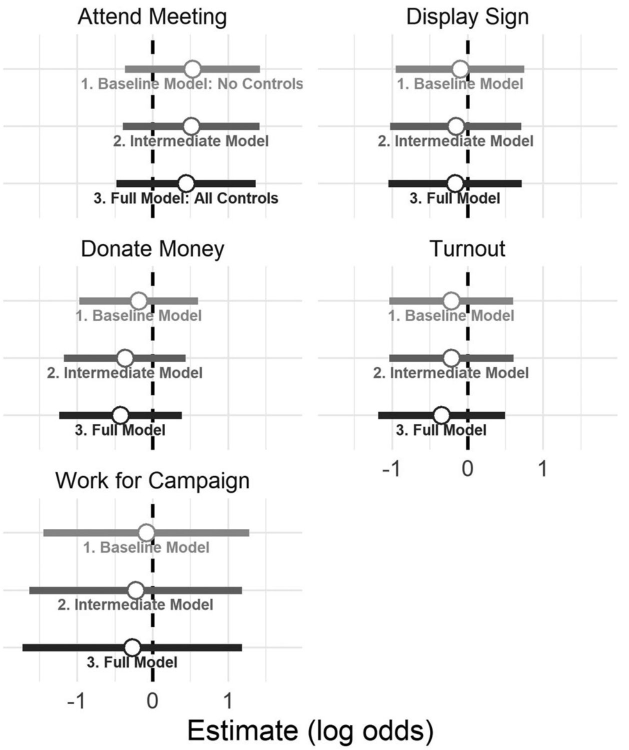

FIGURE 3 Conditional, fixed effects models to estimate the effect of becoming a parent on turnout and nonvoting participation. The estimates represent coefficients and 95 percent confidence intervals from conditional, fixed effects logistic regressions. Estimates from three regressions are reported for each outcome variable. The baseline models include a year fixed effect with no additional controls. The intermediate models include the year fixed effect and controls for whether the individual graduated college, was contacted by a political party in the months before the election, and experienced a change in family income using income quartile. The full models include the year fixed effect and the intermediate controls, as well as measures of change in church attendance frequency, political interest, county of residence, marital status, homeowner status, student status, and employment status. *Source*: 2010–2014 Cooperative Congressional Election Study (CCES) Panel Survey

additional controls, the chance of bias from pretreatment variables decreases, but the chance of post-treatment bias increases. The CCES panel waves also featured a large decline in average turnout among respondents from 2010 to 2014—in 2010, 76 percent of our subsample cast votes as compared to 55 percent in 2014—as well as smaller declines across these years in the other participatory acts. To account for these declines, we include a 2014 indicator in all models. Under this specification, the resulting estimate associated with the parental treatment variable is analogous to a difference-in-differences estimator.

Figure 3 presents the estimated treatment effect from each of these models.[18] The estimates in the figures reflect the logistic regression coefficients. The results from all three specifications suggest that becoming a parent has a small, negative effect on turnout, working for a candidate or campaign, putting up a political sign, and donating money, but a small positive effect on attending political meetings. However, none of the effects is statistically distinguishable from zero. Although analyses based on the panel data likely provide estimates that are more plausible, they lack sufficient power to provide statistically compelling evidence in favor of a demobilizing effect. The imprecise estimates reflect a common tradeoff for observational studies (Matthay et al. 2020). By focusing only on respondents who eventually become parents, the models provide greater causal leverage at the cost of reduced statistical power.

Since gender is inexorably linked to parental roles, we perform a final analysis to estimate the effects of parenthood on turnout separately for women and men. In Figure 4, we repeat the turnout analysis from Figure 3, but this time, we estimate the models separately for women and men. As with the previous analysis, the estimates for both women and men remain imprecise and statistically insignificant. But the figure suggests that women become less likely to vote following parenthood, while men do not see a corresponding decrease. Future research should examine whether this pattern, consistent with many theoretical expectations, is systematic or a statistical aberration.

[18] Tables B1, B2, and B3 of online Appendix B present the complete model results.

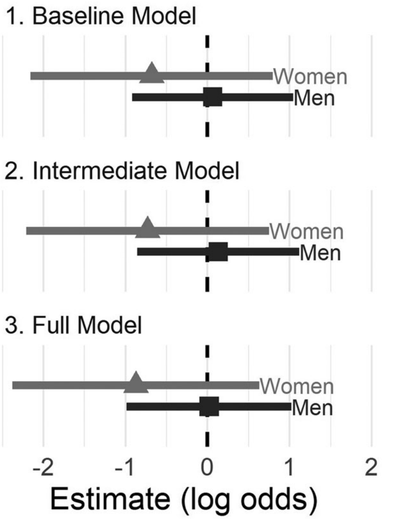

FIGURE 4 Conditional, fixed effects models to estimate the effect of becoming a parent on turnout (women and men assessed separately). The estimates represent coefficients and 95 percent confidence intervals from conditional, fixed effects logistic regressions. Estimates from three regressions are reported for turnout. The baseline models include a year fixed effect with no additional controls. The intermediate models include the year fixed effect and controls for whether the individual graduated college, was contacted by a political party in the months before the election, and experienced a change in family income using income quartile. The full models include the year fixed effect and the intermediate controls, as well as measures of change in church attendance frequency, political interest, county of residence, marital status, homeowner status, student status, and employment status. *Source*: 2010–14 CCES Panel Survey

CONCLUSION

Most people experience a series of major personal events across the course of their lives that uproot previously established daily life patterns and time allocations, disrupt social networks, absorb time, energy, and financial resources, and cause stress—for example, residential moves, getting married, exiting a marriage, downturns in their own health or that of family members, losing a job, among a host of possibilities. Becoming a parent is one of these events. Even if someone has not had the first-hand experience of becoming a parent, spending any time around a new parent, especially of a young child, makes obvious the immersive, consuming nature of that person's new adventure. What are the near-term implications for the civic life of a new parent? Opportunity costs likely shift, transforming the calculus of political participation. In normal language, a new parent simply may now have "more important things to do."

Analyzing CPS data, cross-sectional regressions that specify conventional sets of controls, as well as exact matching, indeed reveal a statistically significant, negative relationship between becoming a parent and multiple forms of civic engagement. With these CPS results supporting a plausible hypothesis that emerges from a judicious read of various literature, the temptation would be to close the case. However, threats to causal inference loom. Sensitivity analysis applied to the exact-matching estimates reveals that the attenuating effect of becoming a parent on turnout (as well as on many other forms of participation) can disappear after adjusting for plausible violations of underlying assumptions.[19] Additionally, the results from analyses that leverage CCES panel data, although suggestive of a negative influence of becoming a parent on most forms of participation, are not statistically significant. The introduction of a child into a household appears to be associated with attenuated parental civic engagement in the near term, but our evidence provides weaker support for a causal claim.

[19] Again, these assumptions are that no pretreatment differences of consequence for participation remain between new parents and nonparents after the matching and that the matching has not conditioned on any posttreatment covariates.

Overall, our substantive results are consistent with those emerging from the recent studies that rely on Western European data, which also find, for example, that becoming a parent has a small, negative effect on the likelihood of voter turnout—reducing it by a few percentage points. Although public policy in the United States regarding parental leave and investment in early childhood care and education is less generous, new parents' patterns of electoral participation appear to follow a similar contour on each side of the Atlantic.[20] Our results reinforce that civic engagement is rather "sticky," likely reflective of a habitual component (Coppock and Green 2016; Green and Shachar 2000; Gerber, Green, and Shachar 2003). Systematic biases do manifest in the types of people who are more likely to comprise the active electorate (Leighley and Nagler 2013; Verba, Schlozman, and Brady 1995). However, entry into parenthood does not attenuate political participation to a degree that should raise major concerns about subsequent representational bias along this specific demographic dimension.

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