Valuing the Future: Changing Time Horizons and Policy Preferences*

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Abstract

The short time horizons of citizens are a prominent explanation for why governments fail to tackle significant long-term public policy problems. Evidence for the influence of time horizons is mixed, complicated by the difficulty of determining how attitudes would differ if individuals were more concerned about the future. This paper approaches the challenge by leveraging a personal experience that leads people to place more value on the future: parenthood. The analysis compares new parents with otherwise similar individuals using a matched difference-in-differences design with a three-wave panel. The results show that parenthood increases support for stopping climate change. Falsification tests and two survey experiments suggest that longer time horizons explain part of this shift in support. Not only are scholars right to emphasize the role of individual time horizons, but changing valuations of the future offer a new way to understand how policy preferences evolve.

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Governments frequently fail to pass policies that would benefit citizens in the long run. Whether it involves international cooperation on climate change or investments in pandemic preparedness, leaders often respond to disasters after they strike rather than avert them. A common view is that citizens' short time horizons are responsible for policymakers' reluctance to make these long-term investments (e.g., Healy and Malhotra 2009; Nordhaus 1975).

How would people's policy preferences change if they valued the future more? Early studies found that longer time horizons have little correspondence with policy attitudes (Jacobs and Matthews 2012). This led scholars to focus instead on the temporal features of policies, such as the timing of costs and benefits, or institutions such as electoral rules (Finnegan 2022; Jacobs 2016). Recent research is only partly consistent with these initial findings (Bechtel et al. 2024; Bechtel and Mannino 2023). These mixed results are unsurprising given the "daunting empirical obstacles" to studying time horizons (Jacobs 2016, 438). Most studies rely on cross-sectional surveys, making it difficult to disentangle measures of patience from socio-demographic characteristics that influence how much people value the future. The ideal test would exploit exogenous variation in time horizons, but this is difficult to isolate with observational data or elicit in experiments.

This paper adopts a new approach to learn how time horizons affect policy preferences. The research design leverages parenthood, a consequential experience that affects how people view the future (Elder and Greene 2012a; Greenlee 2014; Urbatsch 2014). Parenthood lengthens time horizons, first, through a self-interested motivation to ensure the survival of children and leave a legacy (Dawkins 1976; Wade-Benzoni and Tost 2009). Second, parents have an altruistic desire to protect their child's future welfare (McAdams and Aubin 1992; Zaval, Markowitz, and Weber 2015). Data on household economic decisions corroborate these mechanisms (e.g., Browning 1992). While many changes accompany parenthood, these studies indicate that longer time horizons are an important shift accompanying children.

The policy context of this study is climate change. The short-term costs of stopping global warming overshadow the long-term benefits that citizens discount (e.g., Gazmararian

and Tingley 2023a; Hale 2024). Parenthood changes this calculation. Now, one's descendants will feel the long-run consequences of a hotter planet, so parents should have incentives to support climate policy.

A challenge for analysis is that parenthood is not random. Changes in household income or environmental concern could affect decisions to have kinds. A simple comparison of people with and without children would risk confounding.

This paper approaches the challenge with a difference-in-differences research design. A three-wave panel survey of 9,500 American adults from 2010 to 2014 allows for a comparison of how the same person's climate policy support changes after children. The assumption is that if an individual had not become a parent, they would have the same climate policy preferences, on average, as their childless peers.

I take several steps to strengthen and assess the plausibility of this parallel trends assumption. The models use covariate balancing propensity scores to match respondents based on time-varying factors predictive of parenthood (Imai, Kim, and Wang 2023). A placebo test further shows that people were not changing their attitudes prior to parenthood. Lastly, I examine the possibility of reverse causality and find no evidence that climate concern influences the decision to have children.

Parenthood increased climate policy support by 4.3 percentage points compared to otherwise similar childless individuals. The effect persists for at least 0-2 years after having a child. This shift takes place despite the partisan polarization of global warming that hardens beliefs and the time constraints of parenthood that undermine political engagement (Burns, Schlozman, and Verba 2001; Egan and Mullin 2017). Analysis of the sensitivity of the estimates to omitted variable bias indicates that an extreme confounder would be unlikely to overturn the results (Cinelli and Hazlett 2020).

Two survey experiments provide more direct tests of the time horizons mechanism. The first study examines the effect of priming a national sample of American adults to think about children and framing global warming as an issue affecting future generations. The

second study assesses a more subtle prime, asking a unique sample of American *parents* to journal about their children. Results show that the interventions lengthened time horizons and raised support for climate policy. A mediation analysis suggests that longer time horizons are responsible for part of the treatment's effect on increased policy support.

In addition to these experiments, the paper considers alternative explanations, such as changing risk perceptions and worldviews, determining that these are unlikely for three reasons. First, previous studies of parenthood suggest that these mechanisms, if present, would make parents more opposed to climate policy. I collected new survey data, which support this claim in the context of risk aversion. Second, the results hold when controlling for measures capturing risk perceptions and worldviews. Lastly, falsification tests show parenthood has no effect on attitudes correlated with these alternative explanations.

This paper makes progress understanding how myopia and personal experience shape attitudes about long-term public policy challenges. Previous mixed results led many to think that individual time horizons were not influential, but these studies have relied on a static approach that did not fully appreciate how time horizons could change. Taking a dynamic approach, the findings here suggest that personal experiences can lead individuals to place more value on the future. These longer time horizons, in turn, are one mechanism that increases support for policies addressing long-term challenges such as pandemics, deficits, and climate change.

Time Horizons and Policy Preferences

Voters' short time horizons are a prominent explanation for inaction on long-term problems (Hale 2024; Healy and Malhotra 2009; Nordhaus 1975). This paper defines time horizons as how much an individual values future outcomes. This valuation includes both the rate at which one discounts distant benefits and an altruistic sense of responsibility for future generations.

Evidence of time horizons affecting policy preferences is mixed. Early research using sur-

veys detected little correlation between patience and policy attitudes (Jacobs and Matthews 2012). These null results and the difficulty of studying time horizons led Jacobs (2016) to propose a new direction for research: instead of individual time horizons, scholars should examine how policy design and institutions influence information about long-term outcomes, credible commitment problems, and interest group opposition (Gazmararian and Tingley 2023b; Jacobs and Matthews 2017; Mullin and Hansen 2023).

Recent research only partially supports these initial null results. Bechtel, Scheve, and van Lieshout (2020) find that time horizons predict public preferences over the path of future carbon price increases. Bechtel and Mannino (2023) show that more patient individuals support larger disaster preparedness investments. Bechtel et al. (2024) uncover a relationship between time preferences and support for local, delayed investments but not for more complex, future-oriented policies.[1]

One reason for these mixed results is the challenge of studying time horizons. The ideal counterfactual would show how the same person's preferences change if she valued the future differently. However, researchers typically assume that it is infeasible to shift how someone values the future in an experiment or identify such variation in observational data.

Instead, most studies rely on one-off surveys. This approach limits analysis to variation in time horizons across individuals. This cross-sectional design can be informative, but it also makes it difficult to disentangle time horizons from other characteristics that render people more patient or shape the information environment. For example, Bechtel et al. (2024) find that time preferences do not have a consistent correlation with climate policy support. Their empirical model controls for variables predictive of patience, like age and income, which might mask the effect of time horizons. Yet, it is necessary to include these covariates since they are potential confounders. This dilemma illustrates the challenge of studying time horizons.

Within-individual changes in time horizons would offer powerful empirical leverage com-

1. An open question is how to measure time preferences, which vary when considering individual and collective outcomes (Prior, Alsharawy, and Andrews 2023).

pared to cross-sectional designs. Moreover, the idea that individuals might shift how they value the future would open new avenues for understanding preference change. While there is acknowledgment of differences in time preferences across individuals (Frederick, Loewenstein, and O'Donoghue 2002), scholars have not fully considered the political consequences of variation within the same person. Such individual-level change in time horizons would be enlightening to understand, if it exists, because it could help explain how preferences evolve.

Parenthood and Time Horizons

This paper uses parenthood to examine the effects of longer time horizons. Children cause parents to care more about the future for two reasons. First, children expand a parent's self-interest to include descendants who will be affected by future events (Greenlee 2014; Urbatsch 2014). This motivation to protect one's children may originate from the biological pressure to pass on genes (Dawkins 1976), and the desire to leave a legacy (Hoffman and Hoffman 1973; Wade-Benzoni and Tost 2009).

Second, parenthood creates a sense of obligation to safeguard the welfare of future generations. Some call this phenomenon "generativity," a stage of adult development where one derives fulfillment from helping the next generation (Erikson 1982; McAdams and Aubin 1992; Zaval, Markowitz, and Weber 2015). Both mechanisms involve self-interest and altruistic motives that lead parents to value the future.

Three types of evidence from different disciplines support the link between parenthood and longer time horizons. First, surveys and interviews compare the priorities of parents and non-parents. Parents consistently report greater concern about the future, consistent with the idea that they have longer time horizons (Gahtan, Wilcox, and Jones 2022; Greenlee 2014).

Second, experiments provide more direct causal evidence about the parenthood effect on time horizons. One marketing study found that priming men to think about their parental role led them to be more future-focused (Li, Haws, and Griskevicius 2019). Further work

needs to assess whether this shift translates into policy preferences.

Third, data on household economic decisions support the connection between parenthood and time horizons. These decisions demonstrate how much people value future benefits when money is on the line. Parents are more likely to sacrifice present consumption to build an inheritance (Browning 1992; Kopczuk and Lupton 2007; Love 2010). Parents also save for college expenses. Standard models of life-cycle savings behavior assume that parents have a "dynastic utility function," which means they derive utility from their children's welfare (Becker and Barro 1988).

These results indicate that parenthood lengthens time horizons. Parenthood is a "bundled" treatment that influences how much one discounts future benefits and the normative value one places on the future. These processes should influence policy attitudes on issues impacting future generations.[2] This offers an opportunity to learn how time horizons affect policy preferences.

Parenthood's effects likely vary with age, family structure, marital status, and socioeconomics. Men and women may react differently due to the social norms around father- and motherhood (Elder and Greene 2006, 2007, 2012a, 2012b, 2016; Greenlee 2014; Klar, Madonia, and Schneider 2014). The child's gender may also matter; some hypothesize that daughters make parents more liberal (Glynn and Sen 2015; Washington 2008).

A few studies examine parenthood and environmental concerns but yield mixed results (Shrum et al. 2023). Having children is positively correlated with a parent's support for environmental and climate policies (Dechezleprêtre et al. 2022; Dupont 2004). Other studies posit a conditional parenthood effect based on gender or framing (Blocker and Eckberg 1997; Ekholm and Olofsson 2017). But some find no effect of children (McCright 2010; Milfont, Poortinga, and Sibley 2020).

The correlational design of past studies limits what we can learn. Although some use panel data, there is little attention to how people become parents, which likely confounds

2. Follow-on studies should disaggregate the relative contribution discount rates and altruism.

inference. Changes in economic circumstances affect the decision to have kids and could also shape climate attitudes.

It is also not self-evident that policy attitudes will change after parenthood. The time commitments of having a child could prevent people from engaging in politics (Burns, Schlozman, and Verba 2001). This reduced participation might cause parents to be less exposed to messages necessary to translate experience into political attitudes (Mutz 1994).

Research Design

This study uses a difference-in-differences model with panel survey data to estimate parenthood's effect on support for stopping climate change. This research design compares the change in policy attitudes of people who become parents with otherwise similar individuals who did not have children.

Panel data allow us to track to the same individual over time to observe changes in their policy preferences. The dataset comes from the Cooperative Congressional Election Study's (CES) 2010-2014 Panel Study, a YouGov Internet sample of the American public using matched random sampling (Ansolabehere and Schaffner, Cooperative Congressional Election Study: 2010-2014 Panel Study). There are 9,500 respondents, each interviewed in 2010, 2012, and 2014. This sample includes respondents who completed all the three waves.

Attrition would be a concern if it correlated with parenthood and climate policy opposition. While the former is plausible because of the time constraints of being a parent, the latter is unlikely. Climate policy attitudes are unlikely to affect survey response propensity. Attrition is 53% in 2012 and 68% in 2014. The largest attrition occurs for Black respondents. There is little to no difference in attrition rates for variables that might correlate with children, such as age, employment, or marital status (Ansolabehere and Schaffner, Cooperative Congressional Election Study: 2010-2014 Panel Study). The low respondent drop-off on variables predictive of parenthood alleviates concerns about systematic attrition biasing the results.

Treatment: Parenthood

The treatment is whether a respondent became a parent through the birth or adoption of a child. The measure captures the event of becoming a parent, rather than the number of children. A parent's concern for the future becomes salient when the household structure changes with the first child. The binary treatment indicator is 1 for respondents who report having a child under the age of 18 and did not have a child before and 0 otherwise. The treatment is irreversible, meaning that people who became parents in 2012 remain parents in 2014.[3] A total of 187 survey-takers became parents during the study (Table A1). This modest number may bias against detecting an effect as the smaller sample weakens statistical power.

One potential concern is that people might know they want children long before they have them, so their time horizons could lengthen in anticipation of parenthood. If true, their policy preferences would also change, so the analysis would not detect a relationship between parenthood and climate policy support. Childbirth and adoption turn aspirations into reality, marking the point when people feel the greatest investment in the future because their stake is no longer hypothetical.

Outcome: Climate Policy Support

This paper examines climate policy support because time horizons are central to the politics of stopping global warming. Actions to reduce emissions impose short-term costs that often overshadow the long-term benefits that people discount. Support for climate policy implies an appreciation for the long-term benefits of a normal climate. Parents should be more likely to support climate policy to protect the welfare of their descendants who confront a warmer world.

The question measuring climate policy preferences asks respondents to characterize their

3. If someone had children more than 18 years apart, the measure would incorrectly mark them as new parents, which would bias against the hypothesis. This is unlikely because 95% of births in the United States occur within a 10-year interval (Thoma, Copen, and Kirmeyer 2016).

views on global warming. The options range from "Global climate change is not occurring, this is not a real issue" to "Global climate change has been established as a serious problem, and immediate action is necessary." This is a standard question for capturing climate policy support.[4] The question also mentions climate beliefs when justifying how much action should be taken. The wording does not describe an economic trade-off, which may increase hypothetical bias (Bechtel and Scheve 2013). The results are consistent in the subsequent survey experiments that include a budget constraint.

The analysis uses a dichotomous measure where 1 represents the view that immediate climate action is necessary, and 0 otherwise. This threshold corresponds with a break in the distribution of responses (Table A3).[5] This binary outcome captures the moment when people flip from climate policy opposition to support.

Around 54% of the sample support action to stop climate change. This share is similar to other polls of the era with different question wordings. A 2012 Gallup (n.d.) survey of the American public found that 55% thought that the seriousness of global warming was either underestimated or generally correct. This moderate support indicates that ceiling effects are not a concern.

Assumptions for Causality

The difference-in-differences research design assumes that if a parent did not have a child, her climate policy preferences would have followed the same average trajectory as non-parents. The primary concern is the endogeneity of parenthood to climate policy preferences. The main reason why climate concern might correlate with the decision to have a child would be if people became less likely to have children as they grew more worried about global warming.

The available data indicate that the majority of people do not consider climate change

4. This item is part of the Cumulative CES Policy Preferences data set. It does not name a specific mitigation policy, but respondents most likely interpreted the question as referring to government action because the item is part of a battery about approval of politicians and government policies.

5. Since the question bundles beliefs and preferences, it is not appropriate to use a linear scale.

when deciding to have children. A 2021 poll from Pew Research Center's American Trends Panel, a national probability sample of American adults (N = 9,676), found that among childless adults ages 18 to 49, only 5% said climate change kept them from raising children (Brown 2021). This marginal share of the population represents an upper bound. The salience of climate change in the 2020s, when the Pew survey took place, is much higher than in the 2010s, our study's period (Tyson, Funk, and Kennedy 2023). A related survey of American 12th graders from 2005-2019 finds that environmental attitudes correlate with a desire for a smaller family but not abstention from parenthood (Rackin, Gemmill, and Hartnett 2023).

Even if climate-concerned people chose not to have children, this would bias against the hypothesis. The people least worried about climate change and less supportive of action would be most likely to become parents.

Another threat to the analysis is time-varying confounders, such as educational attainment, that make people more likely to become parents and support climate action. To address this possibility, I employ panel matching methods to construct a control group of individuals most similar to people who become parents. The small number of new parents means there is a large pool of individuals from which to construct a matched control group. The panel matching estimator selects the control group based on other individuals in the same period with an identical treatment history. Covariate balancing propensity scores further refine the treatment and control groups so they have similar covariate histories (Imai, Kim, and Wang 2023).

The covariates for matching include predictors of both parenthood and climate policy support. One review identifies education, labor market participation, housing conditions, social values, and economic uncertainty as causes of parenthood (Mills et al. 2011). Therefore, the covariates include education, household income, home ownership, religiosity, age, employment, and marital status as covariates. Variables for partisanship, ideology, gender, and race further account for factors related to climate attitudes (Egan and Mullin 2017).

Appendix A.3 reports how this procedure improves covariate balance.

I attempt to falsify the parallel trends assumption with a placebo test. This test estimates the effect of children on climate policy support in the panel wave before a child's birth. If there were an effect of future parenthood on current policy preferences, it would signal a violation of parallel trends. Although there are only three survey waves, it is still possible to assess pretrends for individuals who had a child between 2012 and 2014. Figure 1 contains the results from a placebo test. Consistent with parallel trends, there is no effect of parenthood in the pre-treatment period.

To guard against omitted time-varying changes, such as a shift in political messaging that uniquely appeals to parents, one analysis compares *new* parents with people who are *already* parents. These two groups should be theoretically similar. The key difference is that the new parents recently welcomed a child into their household, which should lengthen time horizons. This comparison would detect a spurious factor that affected parents' political attitudes but was unrelated to becoming a parent, which is the experience that lengthens time horizons. Consistent with the time horizon interpretation, only new parents increase their support for climate policy (Table A4).

Effect of Parenthood on Climate Policy Support

Figure 1 shows the estimated effect of having a child on climate policy support. Parenthood increases climate policy support by 2.6 to 6.1 percentage points compared to the matched control group. Since the outcome is binary, this increase represents an individual moving from opposition or indifference to support for stopping climate change.

Support increases despite the polarization of climate change. Partisanship is a significant determinant of climate attitudes in the United States (Egan and Mullin 2017). The parenthood effect is 1.4 times larger than the effect of becoming a Democrat, though the null hypothesis of no difference between the coefficients cannot be rejected (Table A4). The magnitude of the parenthood effect is similar to other studies of experience and climate

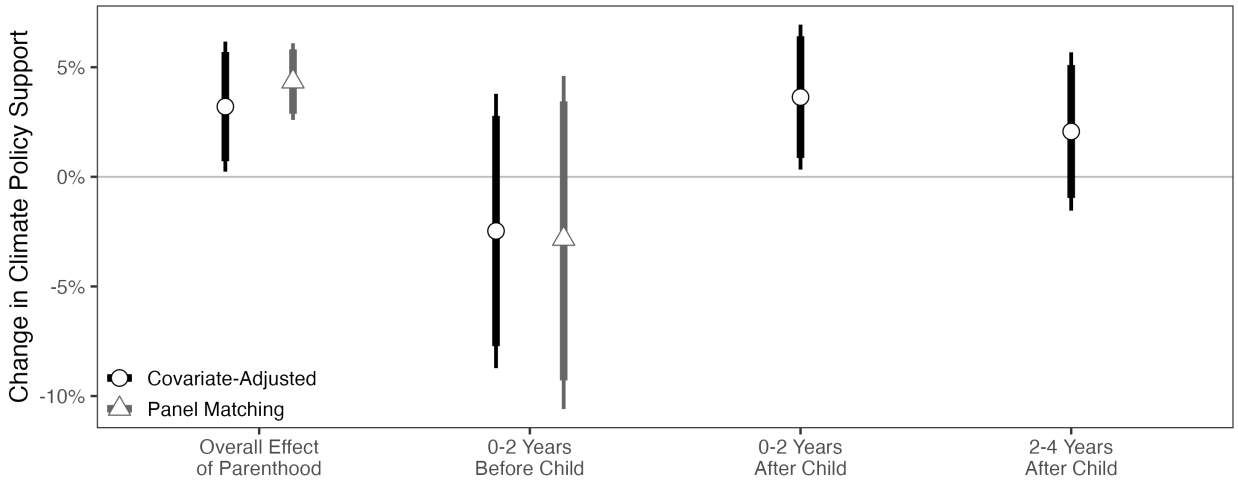

Figure 1: Estimated Effect of Parenthood on Climate Policy Support

Notes: The outcome is a binary variable for if a respondent supports action to stop climate change. Bars denote 95 and 90% confidence intervals, with heteroskedasticity and auto-correlation consistent standard errors clustered by respondent (covariate-adjusted) or conditional standard errors that account for dependence across time (panel matching). Missing data is handled with 30 imputed data sets (covariate-adjusted) or matching on treatment history missingness (panel matching). Table A4 contains covariate estimates. 9,500 respondents $\times$ 3 panel waves in 2010, 2012, and 2014.

change, such as Egan and Mullin (2012) who find that heat waves cause a 5 percentage point increase in belief in global warming and Gazmararian and Milner (2023) who find that fires increase support for climate action by 3-4 percentage points.

Unlike the relationship between temperature anomalies and climate beliefs, which Egan and Mullin (2012) find decays to zero in 12 days, the parenthood effect persists in the 2 years after having a child.[6] Figure 1 shows that it is not until 2-4 years later when the parenthood effect begins to attenuate.[7] This pattern of a stronger shift in climate attitudes in the years following parenthood is consistent with the mechanism where the life experience of having a child alters one's perspective about global warming's importance.

The effects of children might differ by gender because the norms of motherhood differ from fatherhood (Blocker and Eckberg 1997; Bush and Clayton 2023). However, there is no

6. There are no matched estimates for the contemporaneous and delayed effects because the panel matching procedure requires matching on the treatment history, which reduces the periods that could be analyzed.

7. This imprecision is unsurprising because the delayed effect is estimated with the smaller subset of people who had a child in 2012.

statistically distinguishable gender difference in parenthood's effect on policy support. Given the limited sample size, this analysis cannot rule out heterogeneity by gender. There are also no detectable differences in parenthood's effects by media consumption and partisanship (Table A5).

An additional analysis explores the relationship between prior climate attitudes and parenthood. The measure for prior beliefs takes the value 1 if an individual believed that climate change is not real and no action is warranted in the first panel wave, and 0 otherwise. The regression interacts this indicator with the parenthood treatment. The results show no difference in how parenthood affects the climate attitudes of skeptics and believers. This suggests that parenthood increases climate policy support by a similar amount across people with differing levels of climate beliefs (Table A5).

The results hold in an unmatched sample with time-varying covariates and individual and panel fixed effects (Table A4). Alternative weighting and matching techniques produce similar effect sizes (Figure A5).

I conducted a sensitivity analysis to see how large an unobserved confounder would have to be to change the conclusions (Cinelli and Hazlett 2020). The first benchmark covariate, marriage, is one of "the most salient predictor[s] for having children" (Heaton, Jacobson, and Holland 1999, 531). It would take a confounder more than three times as strong as the observed married covariate to change the findings. The next benchmark, partisanship, is one of the strongest correlates of climate policy preferences (Egan and Mullin 2017). It would take an unobserved confounder more three times times as strong as Democratic partisanship to overturn the results. These extreme confounders would also have to be orthogonal to the other controls, which is unlikely (Appendix A.6).

Testing the Time Horizons Mechanism

The results are consistent with the claim that parenthood lengthens time horizons because the benefits of climate policy will not be enjoyed for decades. However, the survey does

not include a direct measure of time preferences. Since parenthood brings personal, social, and economic changes, attributing these results solely to longer time horizons could be misleading. This section presents experimental evidence of the time horizon mechanism and evaluates alternative explanations.

Experimental Evidence

I conducted two pre-registered survey experiments to test the time horizon mechanism.[8] Surveys are well-suited for this question because they measure individual perceptions and attitudes. While it is not feasible to randomize parenthood, experiments can evaluate the consequences of priming respondents to think about their children.

The first study both primes respondents to think about their children and frames climate change as an issue affecting future generations. The second study only primes people to think about their children. Study 1 should have stronger effects than Study 2 because respondents can better connect the prime to their time horizons and, subsequently, policy attitudes. The estimates in Study 1 provide an upper bound on the parenthood effect, while those in Study 2 represent a lower bound.

The observational study captures the lived experience of becoming a parent. In contrast, the experiments remind people of that experience, making it top of mind when considering policy issues. This priming mimics situations where parents are reminded of their children's future, such as by political messages framing policy debates in terms of their impacts on future generations. The experimental effects should be smaller because parents' time horizons and policy attitudes have already internalized their children's influence.

Study 1: Priming and Framing

The first study uses a nonprobability online sample of American adults collected in May-June 2023. The survey, fielded with Qualtrics, used nationally representative quotas for age, sex, race, ethnicity, and education. After trimming respondents who failed data quality checks,

8. The pre-registrations are available here: https://osf.io/9fqr2. Appendices D.8 and E.7 discuss mild modifications.

the sample size is 2,006.

The experiment randomly assigned half of the participants to read this message: "If you are a parent, think about your children. If you are not a parent, imagine that you had children." This prompt aims to make parental identity salient for those with children and cause people without children to adopt the mindset of being a parent.

The survey next measures time horizons. Conceptually, a time horizon is the period someone considers when making decisions. This is related but not identical to time preferences, which refer to the extent individuals value goods more in the present than the future. The survey captures time horizons by asking, "How willing are you to give up something that is beneficial for you today so that the next generation of people will be better off in the future?" The answers run from "Not willing at all" to "Extremely willing," converted into a linear scale from one to five that is then scaled by the control mean and standard deviation.[9] The question adapts a validated measure of time preferences to mention future generations. This addition better captures inter-temporal trade-offs relevant to climate change. Stated time preferences, similar to how it is measured here, correlate with costly decisions in lab games (Falk et al. 2023).[10]

The climate policy support outcome asks, "How willing are you to pay higher taxes today to combat global climate change if it would make the next generation of people better off in the future?" The outcome scale is the same as before. The question makes costs explicit by saying the policy will involve taxes, which should minimize hypothetical bias (Bechtel and Scheve 2013).

The question frames climate policy as benefiting future generations. This is a common way politicians frame global warming.[11] People in the panel study likely have been exposed to this argument, which enhances the comparability of the experiment and observational

9. Results hold with a binary outcome (Table D2).

10. While stated preferences may be prone to social desirability bias, the treatment and control groups would face the same pressures, so any difference is attributable to the prime.

11. President Barack Obama (2014) said, "for the sake of future generations, our generation must move toward a global compact to confront a changing climate while we still can. This challenge demands our ambition. Our children deserve such ambition."

results. Since people in the treatment and control group both receive the same issue framing, the only difference is whether the respondent has been encouraged to consider her children.

One concern is that the respondent may conform his answers to satisfy the researcher or social norms. However, there is limited evidence of demand effects due to features of online surveys such as anonymity (Mummolo and Peterson 2019). The questions also include costly trade-offs; the prompt makes clear that the respondent would give up something of value today while the climate policy would require taxes.

Study 2: Priming Only

This section describes Study 2's research design before presenting the results. The population is American adults with children under 18 years old. This is because the intervention asks the respondent to reflect on the importance of her children, which lacks the same relevance for childless adults. Data come from a nonprobability national sample of American parents collected with Cint in February-March 2024. Since the intervention relies on priming, the recruitment message does not mention children. A question early in the survey asks if the respondent is a parent, and individuals who answered no were screened.[12] After trimming individuals flagged by the pre-registered data quality procedures, there are 1,269 complete responses.[13]

The experiment's intervention asks parents to reflect on their children. Specifically, the survey prompts respondents to journal about their aspirations for their children, while another question has survey-takers describe how having children affected their perspectives (Appendix E.6).[14] The treatment is block-randomized by respondent partisanship and climate concern, which predict climate policy support, so blocking should improve precision.

Unlike Study 1, distractor questions separate the prime and time horizons elicitation. This separation creates a harder test because the prime is more distant in the respondent's

12. I also screened individuals who said their children were older than 18.
13. This includes 38 responses from a soft launch.
14. A manipulation check shows that the treatment led parents to report having reflected more often on their children (Table E5).

memory. The time horizons measure is similar to Study 1, except that it does not mention future generations. Instead, the item is more generic: "How willing are you to give up something that is beneficial for you today in order to benefit more in the future?" The survey includes a related question about how a respondent would compare her willingness to make sacrifices for the future. To reduce measurement error, I use inverse covariance-weighting to combine these items into an index. The index is scaled by the control group's standard deviation and mean, so a one-unit change represents a standard deviation increase relative to the control group.

After another set of distractor questions, the survey elicits climate policy support. All respondents receive background information about climate change. Unlike Study 1, this information and the following questions do not frame global warming in terms of "future generations." The first outcome asks about willingness to pay higher taxes to reduce greenhouse gas emissions. The second outcome inquires about support or opposition to higher taxes to reduce emissions. The final question asks how much more a respondent would pay to receive renewable energy at their home. The analysis combines these questions into an index using inverse covariance weighting with the same scaling as before.

Experiment Results

Across experiments, the interventions priming people to think about their children lengthen time horizons. This is true whether the treatment both primes and frames or only subtly primes. As the left panel of Figure 2 summarizes, the treatment causes time horizons to lengthen by 0.17 standard deviations in the first study, and 0.11 in the second study. Consistent with the research cited above on parenthood and time horizons, these results provide further evidence that parenthood leads people to place more value on the future.[15]

Turning to policy preferences, the prime causes an increase in climate policy support. This effect appears stronger in Study 1 than 2, consistent with the idea that Study 2's

15. There is limited treatment effect heterogeneity (Tables D3, E4).

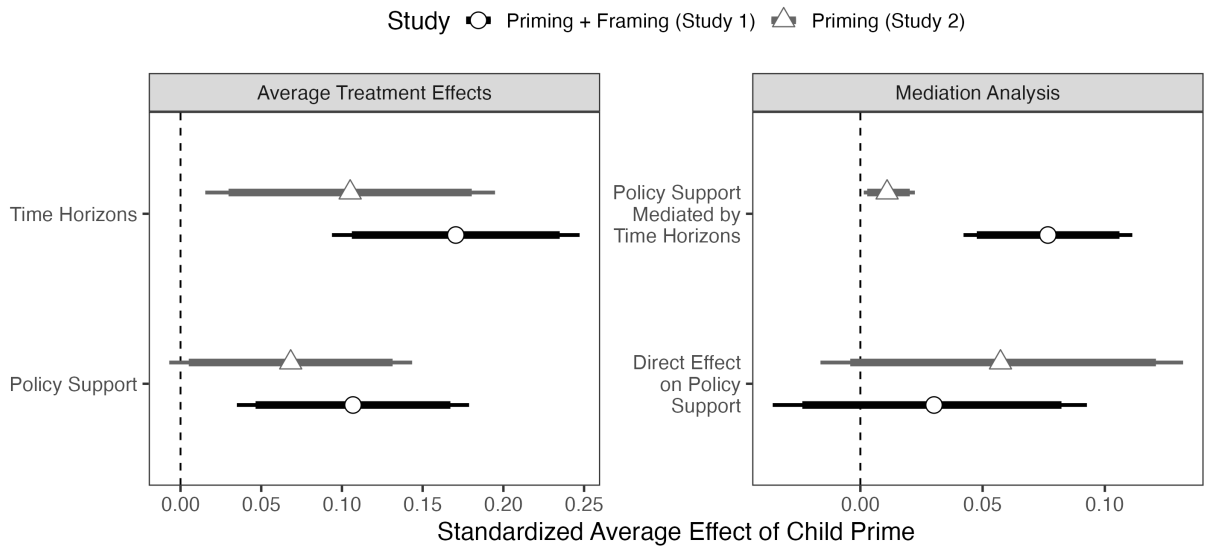

Figure 2: Experimental Evidence of Parenthood, Time Horizons, and Policy Support

Notes: Estimates from separate covariate-adjusted linear regressions of the outcome on the treatment (Tables D2, E2, E3, D4, E6). Outcomes are scaled so a one-unit change represents a standard deviation increase relative to the control group. The outcomes in Study 1 are the individual questions. The outcomes in Study 2 are indices combining all relevant questions. Bars denote 90 and 95% confidence intervals from heteroskedasticity robust standard errors for the left plot, and percentile credible intervals from a nonparametric bootstrap with 10,000 simulations for the right plot. National sample of American adults in Study 1 ($N = 2{,}006$). National sample of American parents in Study 2 ($N = 1{,}269$).

intervention is a lower bound.[16] These results indicate that considering one's children leads parents to be more willing to support policies to stop climate change.

Mediation analyses assess the extent to which longer time horizons explain the increase in climate policy support (Imai, Keele, and Tingley 2010). The right panel of Figure 2 shows that there is no direct effect of the child prime on climate policy support. However, in both studies, there is a precisely estimated positive effect of the treatment on climate policy support as mediated by time horizons. The size of this mediating effect is much larger in Study 1, which is unsurprising as it both primes and frames.

The coefficient on the average causal mediation effect is modest. This suggests that while time horizons are relevant in explaining the change in climate policy preferences, they may not be the exclusive mechanism. This is consistent with this paper's argument that time

16. When examining the individual questions in Study 2, instead of the index that reduces measurement error, the standard errors grow too large to distinguish the estimates from zero.

horizons are an important but not the only channel through which parenthood increases climate policy support.

Mediation analysis assumes that the observed time preferences mediator is independent of the potential outcomes after conditioning on the treatment and pre-treatment covariates. This is a strong, un-testable assumption. I approach it by including a set of covariates identified in the literature to be related to time horizons, such as age, education, income, and gender (Jacobs and Matthews 2012). For good measure, the models also control for race, partisanship, social preferences, risk preferences, and trust.[17] Sensitivity analyses show that an extreme violation of the sequential ignorability assumption would be unlikely to change the results for Study 1, while Study 2's results are more sensitive (Figures D3, E1).

Evaluating Alternative Mechanisms

Risk Aversion

The experiments provide direct evidence that parenthood lengthens time horizons and this weight on the future is one mechanism mediating increased climate policy support. I consider two alternative explanations for the earlier observational results. First, instead of parents valuing the future more, children could lead their guardians to become more risk-avoidant (Görlitz and Tamm 2020). Risk orientations correlate with policy preferences, especially when politicians frame an issue in terms of losses (Arceneaux 2012; Druckman and McDermott 2008; Kam and Simas 2010), which is the case with "existential" climate change.

However, risk aversion should create more pressure to focus on short-term issues, such as immediate risks to a child's safety and the household's economic situation. Global warming is a long-term risk. Global warming's effects have begun to manifest, but were of lower salience during the study period (2010–2014). Risk-averse individuals should be *less* willing to support a climate policy that would impose costs that people should interpret as a more immediate loss.

17. See Appendices D.5 and E.5 for a full list of controls.

Indeed, in an original survey for this study, risk aversion negatively correlates with climate policy support. More risk-averse individuals are less supportive of climate policy (Table D5). Even if parenthood increased risk aversion, it would make stronger climate policy support less likely.

Changing Worldviews

Another explanation is that parenthood alters worldviews. If people became more liberal, for example, and they adopted policy views that liberals hold for reasons unrelated to time horizons, this would confound the results. To account for shifting worldviews, the regressions control for ideology, partisanship, and religiosity.

The evidence is also mixed on whether parenthood makes people more liberal. If anything, they become more conservative, and by that logic would be less climate concerned. Mothers are more liberal on social welfare policy but are more conservative on social issues like abortion, whereas fathers generally are more conservative (Elder and Greene 2012a, 2012b). These diverging patterns suggest that any changes in worldviews would not produce a strong treatment effect on average across men and women.

Falsification tests using the panel survey data provide further evidence against changing the worldviews explanation. These tests use survey questions related to worldviews but unrelated to time horizons. Parenthood does not shift attitudes on these validated proxies, suggesting that worldviews are not behind increased climate policy support.[18]

Other Future-Oriented Issues

A final test of the time horizons mechanism evaluates whether children affect opinions about other policies that impact their future welfare. If time horizons lengthen because parents become exposed to policies that impact their children in the future, there should be shifts in policy attitudes concerning these issues. Appendix B.5 provides details about the issue selection and results, which are consistent with the time horizons mechanism.

18. See Appendix B for results and an original survey validating the proxies for worldviews.

Time Horizons Can Change and Affect Policy Attitudes

Governments routinely under-invest in solving long-term problems like global warming, pandemic preparedness, and unregulated emerging technologies. Despite the prevalent view that voters' short time horizons are to blame, the evidence for this contention is mixed. The challenge of studying individual time horizons led scholars to shift their focus to policy design and institutions (Jacobs 2016).

This article makes progress in understanding the influence of individual time horizons on policy attitudes. It brings together theories of personal experience, temporality, and policy support while applying a more credible research design. Past studies have focused on differences in time horizons across individuals. This paper leverages changes in how the same person values the future. The results show that as life experiences lengthen an individual's time horizons, support for addressing long-term problems grows. Specifically, parenthood leads people to become more supportive of stopping climate change, which involves short-term costs and long-term benefits for future generations.

Incorporating personal experience into theories of long-term reforms provides a new way to understand how policy attitudes change. While scholars acknowledge differences in time preferences across individuals (Frederick, Loewenstein, and O'Donoghue 2002), this study suggests that the same person may alter how much they weigh the future in their decisions. While research has begun to investigate the instability of time preferences (Chuang and Schechter 2015), the findings here suggest that these changes may emerge from personal experiences and have political consequences. Studies of political behavior have long emphasized the role of experience in shaping beliefs (Druckman and Lupia 2000). This paper shows how experience can also alter the weight people place on distant outcomes, affecting the policies they support.

Beyond parenthood, researchers could explore other events that affect time horizons and, consequently, policy attitudes. For example, a recession could lead people to value the future

more because of a newfound appreciation for financial security. The experiences most likely to change time horizons are those that alter beliefs about exposure to the future. Whether these experiences influence preferences is a separate question. Media framing influences how personal experiences translate into political attitudes (Mutz 1994). In the climate context, the American media has begun to cover climate change, which may facilitate the link between parenthood and preferences.

The findings also have implications for understanding elite behavior. Prior studies show how the time preferences of such elites as foreign policy officials and elected representatives influence their preferred policies (Hafner-Burton et al. 2014; Sheffer et al. 2018). This article suggests that the very same officials might change their valuations of the future in response to personal experiences. Time horizons add a new mechanism to explain how life events like parenthood affect elite decision-making (e.g., Glynn and Sen 2015; Washington 2008). Scholars should explore the consequences of changing elite time horizons on decisions ranging from international cooperation to domestic public investments.

While longer time horizons can shift policy attitudes, this may not automatically improve governance. Institutions stand in the way. The inability of the government to make credible commitments to carry out long-term policies could undermine public support despite the value citizens place on the future (Gazmararian and Tingley 2023b; Jacobs and Matthews 2017). Even if voters had longer time horizons, information asymmetries could lead them to re-elect incumbents who fail to invest in long-term policies (Gailmard and Patty 2019). The findings here demonstrate the relevance of individual time horizons for policy preferences, but the results also reaffirm the importance of continued research on the conditions that enable policy responsiveness.

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Online Appendix: Valuing the Future

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A Difference-in-Difference Appendix

A.1 Parenthood Status of Panelists

Table A1: Parenthood Status of Panelists

Description	N	Group
Becomes a parent in 2012	105	Treatment
Becomes a parent in 2014	82	Treatment
Already a parent	1730	Control
Never a parent	7583	Control

Notes: The treatment groups correspond with people who had a child in 2012 or 2014. Treatment onset is staggered to correspond with the timing of the onset of parenthood.

A.2 Summary Statistics

Table A2: Summary Statistics

	Mean	SD	Min	Max	N	NA
New Parent	0.01	0.10	0.00	1.00	28500	0
Climate Policy Support	0.54	0.50	0.00	1.00	28427	73
Immigration Index	0.54	0.40	0.00	1.00	28494	6
Gun Control	0.46	0.50	0.00	1.00	28452	48
Donate Blood	0.13	0.34	0.00	1.00	28500	0
Age	55.74	11.62	18.00	91.00	28500	0
Female	0.45	0.50	0.00	1.00	28500	0
Black	0.06	0.24	0.00	1.00	28500	0
Latino	0.04	0.21	0.00	1.00	28500	0
Education: Some College	0.34	0.47	0.00	1.00	28500	0
Education: Bachelor's or Post-Grad	0.45	0.50	0.00	1.00	28500	0
Employed	0.40	0.49	0.00	1.00	28500	0
Income Q1	0.07	0.25	0.00	1.00	28466	34
Income Q2	0.34	0.48	0.00	1.00	28466	34
Income Q3	0.26	0.44	0.00	1.00	28466	34
Income Q4	0.14	0.35	0.00	1.00	28466	34
Income Q5	0.07	0.26	0.00	1.00	28466	34
Income Not Say	0.00	0.00	0.00	0.00	28466	34
Homeowner	0.80	0.40	0.00	1.00	28378	122
Married	0.65	0.48	0.00	1.00	28499	1
Democrat	0.36	0.48	0.00	1.00	28498	2
Republican	0.30	0.46	0.00	1.00	28498	2
Liberal	0.28	0.45	0.00	1.00	28498	2
Conservative	0.41	0.49	0.00	1.00	28498	2
Religion: Not at all important	0.21	0.41	0.00	1.00	28499	1
Religion: Not too important	0.15	0.35	0.00	1.00	28499	1
Religion: Somewhat important	0.24	0.43	0.00	1.00	28499	1
Religion: Very important	0.40	0.49	0.00	1.00	28499	1
Watch TV	0.78	0.41	0.00	1.00	28487	13

Notes: To handle missingness, the panel matching estimator matches on patterns of missingness (Imai, Kim, and Wang 2023). Multiple imputation accounts for missing values in the unmatched sample (Blackwell, Honaker, and King 2017).

Table A3: Survey Marginals for Climate Policy Outcome

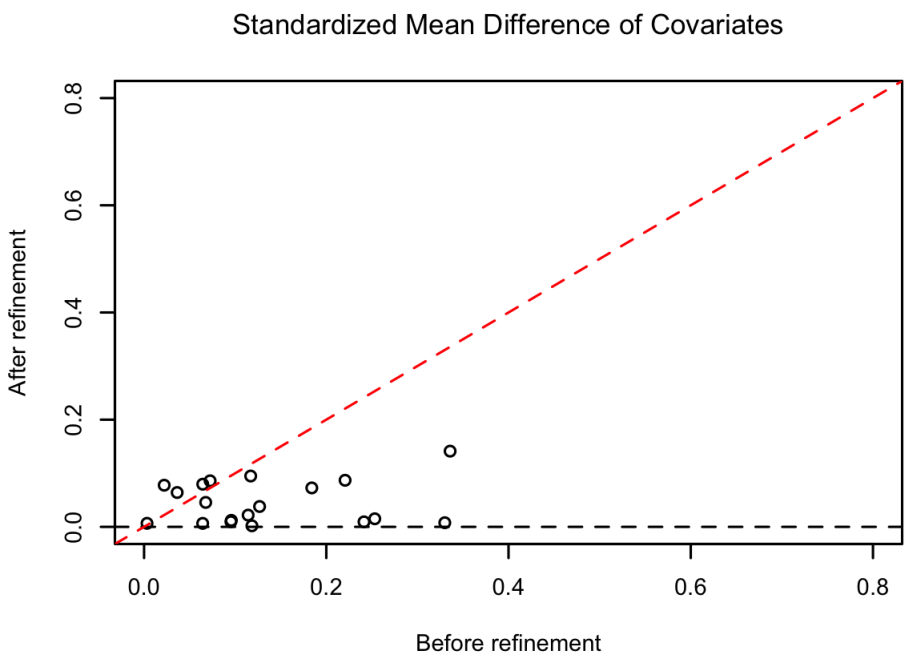
Answer	N	%
Global climate change is not occurring; this is not a real issue.	2213	8
Concern about global climate change is exaggerated. No action is necessary.	5897	21
We don't know enough about global climate change, and more research is necessary before we take any actions.	4980	18
There is enough evidence that climate change is taking place and some action should be taken.	6307	22
Global climate change has been established as a serious problem, and immediate action is necessary.	9030	32

Notes: The outcome dichotomizes the scale by coding the first two answer options as 1, indicating a preference for action on climate change, and the remaining three answer options as 0.

A.3 Covariate Balance

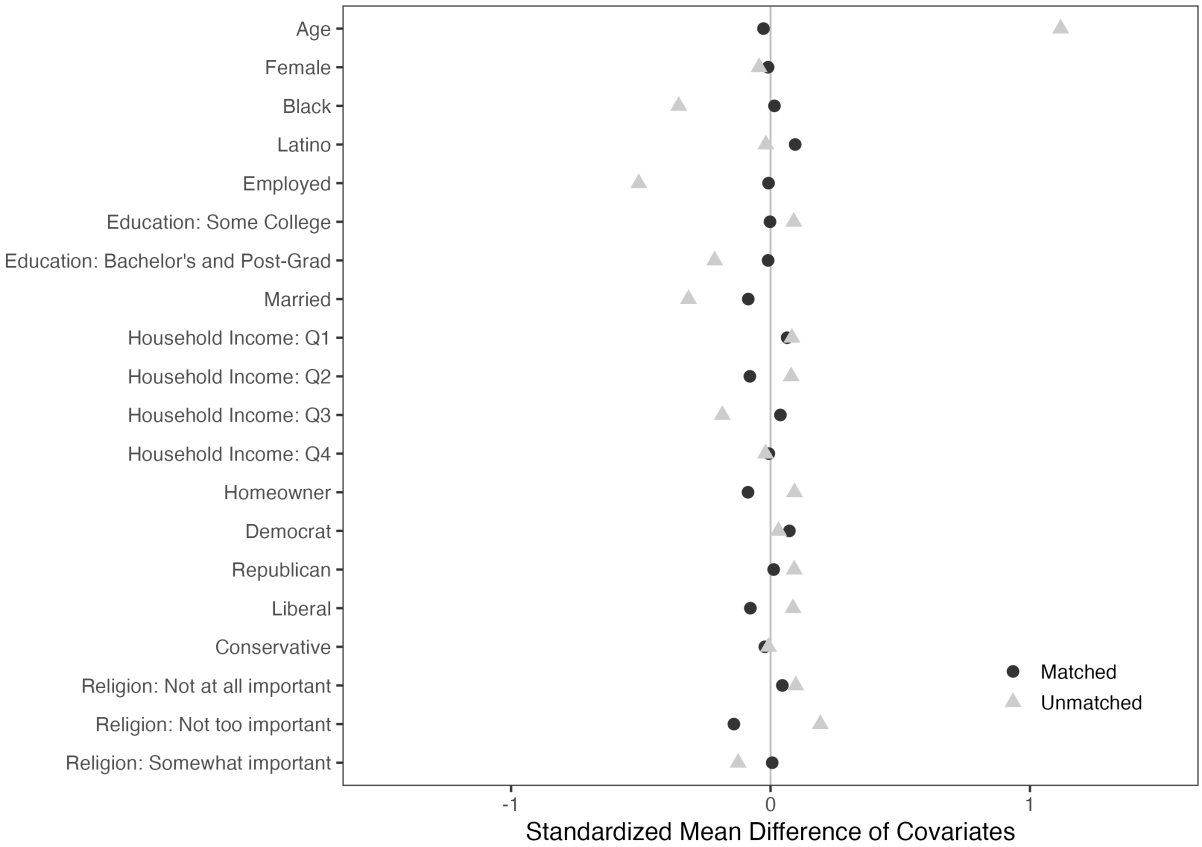
Figures A1 and A2 show that the matching procedure improves covariate balance. Age, education, employment, and marriage are the greatest pre-matching imbalances. While this procedure makes meaningful improvements in balance, it is essential to remember that the primary causal identification assumption is parallel trends. Matching helps to increase the probability of satisfying this assumption by constructing treatment and control groups that are most similar and, thus, more likely to exhibit parallel trends. The primary assumption is that if individuals with new kids had not had children, their climate policy preferences would have been the same on average as individuals who never had children, which the placebo test in Figure 1 indicates is plausible after matching.

Figure A1: Improved Covariate Balance Due to Matching over the Pre-Treatment Period



Notes: The scatter plot compares the absolute value of standardized mean difference for each covariate before (horizontal axis) and after (vertical axis) the refinement of matched sets.

Figure A2: Breakdown of Improved Covariate Balance



Notes: The plot shows the improvement in covariate balance after the matching procedure. The standardized mean difference of covariates for the unmatched group takes the difference between the treatment and control covariate means in the second panel wave and divides it by the control group's standard deviation. The mean for the matched group applies the CBPS weights that are subsequently used to construct matches (Imai, Kim, and Wang 2023).

A.4 Covariate-Adjusted Regression Results

Table A4: Effect of Parenthood on Climate Policy Support

	Climate Policy Support					Falsification		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Parenthood	0.03**	0.04**	0.03**			0.00	0.04*	0.00
	(0.02)	(0.02)	(0.02)			(0.02)	(0.02)	(0.02)
0-2 Years After Child				0.04**				
				(0.02)				
2-4 Years After Child				0.02				
				(0.02)				
Parenthood$_{t-1}$ (Placebo)					−0.02			
					(0.03)			
Employed	0.01	0.00	0.01	0.01	0.00	0.01	0.00	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Education: Some College	0.04***	0.05**	0.04***	0.04***	0.03**	−0.01	0.02	0.03**
	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Education: Bachelor's or Post-Grad	0.02	−0.02	0.02	0.02	−0.03*	−0.01	0.03	0.02
	(0.01)	(0.04)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)
Married	0.01	0.01	0.01	0.01	0.00	−0.01	−0.01	0.00
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Household Income: Q2	0.00	−0.02	0.00	0.00	−0.01	−0.01	−0.01	−0.02**
	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Household Income: Q3	0.00	−0.03	0.00	0.00	0.00	−0.01	0.00	−0.02
	(0.01)	(0.03)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Household Income: Q4	0.00	−0.03	0.00	0.00	0.00	−0.01	0.00	−0.01
	(0.01)	(0.03)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)
Household Income: Not Say	0.01	−0.03	0.01	0.01	0.01	−0.01	−0.02	−0.01
	(0.01)	(0.03)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Homeowner	−0.01	0.00	−0.01	−0.01	0.00	0.00	−0.01	0.00
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Democrat	0.02***	0.02	0.02***	0.02***	0.01	−0.03***	0.05***	0.01
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Republican	0.01	0.01	0.01	0.01	−0.01	0.01	0.01	0.01
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Ideology: Liberal	0.02**	−0.01	0.02**	0.02**	0.00	−0.02**	0.01	−0.02*
	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Ideology: Moderate	0.02**	0.01	0.02**	0.02**	0.01	−0.01	0.01	−0.01**
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Ideology: Not Sure	0.01	0.01	0.01	0.01	−0.02	−0.01	−0.03	−0.02
	(0.02)	(0.04)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)
Religion: Not too important	−0.01	0.00	−0.01	−0.01	−0.01	0.00	−0.02*	0.00
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Religion: Somewhat important	−0.01	−0.01	−0.01	−0.01	−0.01	0.03***	−0.02*	0.01
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Religion: Very important	−0.01	0.00	−0.01	−0.01	−0.01	0.03***	−0.05***	0.00
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Watch TV			0.00					
			(0.00)					
N	28500	5478	28500	28500	19000	28500	28500	28500
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Coefficients come from a linear probability model with heteroskedasticity and auto-correlation consistent covariance estimator clustered by respondent. Multiple imputation performed with 30 imputed datasets to account for missing values (Blackwell, Honaker, and King 2017). No goodness-of-fit statistic is reported because of the imputation procedure. The reference category is Q1 for household income, neither for partisan identification, and conservative for ideology. Model (2) examines the contrast between new parents and people who were already parents. The outcome in Model (5) is support for immigration restrictions. The Model (6) outcome is support for gun control. The Model (7) outcome is whether one donated blood. Models do not include a covariate for age because it increases at the same rate for all individuals and is thus differenced out by the individual fixed effect. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

A.5 Heterogeneous Treatment Effects

Table A5: Heterogeneous Effects of Parenthood on Climate Policy Support

	(1)	(2)	(3)	(4)	(5)
Parenthood	0.02	0.04*	0.03*	0.03*	0.03**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Parenthood x Female	0.02				
	(0.03)				
Parenthood x Watch TV		−0.01			
		(0.03)			
Parenthood x Republican			−0.01		
			(0.03)		
Parenthood x Climate change not real				0.02	
				(0.04)	
Parenthood x Climate change not real/concern exaggerated					0.00
					(0.02)
Climate change not real				0.00	
				(0.00)	
Climate change not real/concern exaggerated					0.00
					(0.00)
Employed	0.01	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Education: Some College	0.04***	0.04***	0.01	0.04***	0.04***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Education: Bachelor's or Post-Grad	0.02	0.02	0.04***	0.02	0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Married	0.01	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Household Income: Q2	0.00	0.00	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Household Income: Q3	0.00	0.00	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Household Income: Q4	0.00	0.00	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Household Income: Not Say	0.01	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Homeowner	−0.01	−0.01	−0.01	−0.01	−0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Democrat	0.02***	0.02***	0.02	0.02***	0.02***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Republican	0.01	0.01	0.02***	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Ideology: Liberal	0.02**	0.02**	0.02**	0.02**	0.02**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Ideology: Moderate	0.02**	0.02**	0.02**	0.02**	0.02**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Ideology: Not Sure	0.01	0.01	0.01	0.01	0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Religion: Not too important	−0.01	−0.01	−0.01	−0.01	−0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Religion: Somewhat important	−0.01	−0.01	−0.01	−0.01	−0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Religion: Very important	−0.01	−0.01	−0.01	−0.01	−0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Watch TV		0.00			
		(0.00)			
N	28500	28500	28500	28500	28500
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes
Panel Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes: Coefficients come from a linear probability model with heteroskedasticity and auto-correlation consistent standard errors clustered by respondent in parentheses. Multiple imputation performed with 30 imputed datasets to account for missing values (Blackwell, Honaker, and King 2017). No goodness-of-fit statistic is reported because of the imputation procedure. The reference category is Q1 for household income, neither for partisan identification, and conservative for ideology. Models do not include a covariate for age because it increases at the same rate for all individuals and is thus differenced out by the individual fixed effect. For the conditional effects based on prior climate policy beliefs, the prior beliefs are fixed at the 2010 levels. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

A.6 Sensitivity Analysis

I conduct a sensitivity analysis to estimate how strong an unobserved confounder—one that is orthogonal to the covariates in the model—would have to be to (a) bring the lower bound of the ATT estimate to touch 0 at the 5% significance level, in other words, the sensitivity of the t-value, and (b) to bring the ATT estimate to 0, a bias of 100% of the original estimate. I use the `sensemakr` package, which implements the methodology proposed by Cinelli and Hazlett (2020). The covariates selected for benchmarking are either strong predictors of parenthood or strong predictors of climate policy preferences.

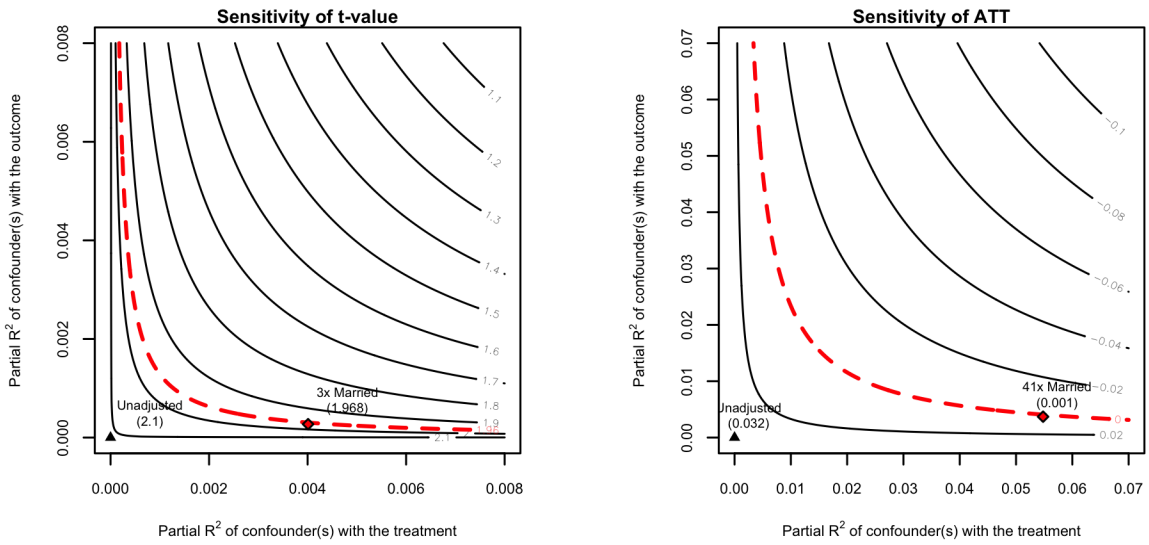


Figure A3: Sensitivity of the Parenthood Effect to Unobserved Confounding, Married as Benchmark

Notes: Bias contour plots of the t-value (left) and ATT estimate (right). Red diamonds indicate that a confounder up to 3× as strong as the observed married covariate would not bring the lower bound of the confidence interval below 0 at the 5% significance level, while it would take a confounder 41× as strong as the observed married covariate to bring the estimate to 0. Estimates from model 1 in Table A4.

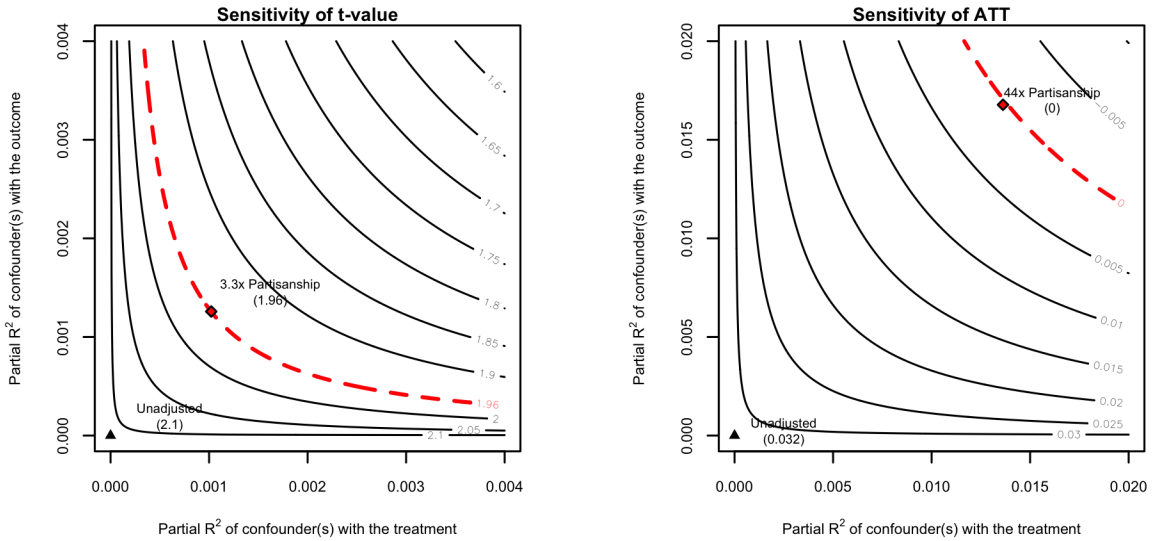


Figure A4: Sensitivity of the Parenthood Effect to Unobserved Confounding, Partisanship as Benchmark

Notes: Bias contour plots of the t-value (left) and ATT estimate (right). Red diamonds indicate that a confounder up to 3.3× as strong as the observed partisan identification covariate would not bring the lower bound of the confidence interval below 0 at the 5% significance level, while it would take a confounder 44× as strong as the observed partisan identification covariate to bring the estimate to 0. Estimates from model 1 in Table A4.

A.7 Alternative Covariate Balancing and Matching Approaches

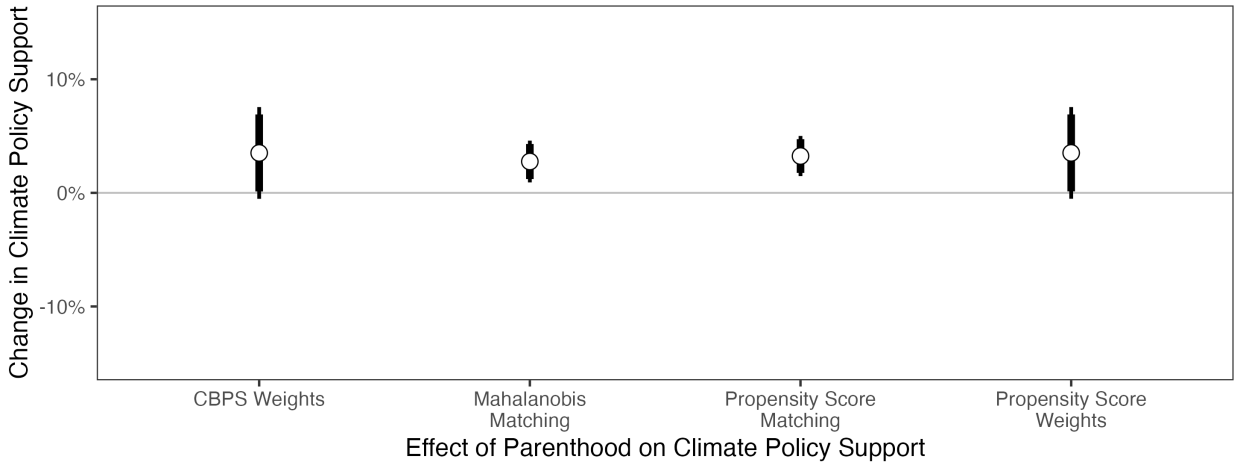


Figure A5: Robustness to Alternative Covariate Balancing and Matching Techniques

Notes: The outcome is a binary variable for if a respondent supports climate policy. All models use the panel matching estimator with conditional standard errors that account for dependence across time. Bars denote 90 and 95% confidence intervals. Weighting methods tend to inflate the standard errors, so the confidence intervals are wider than the more precise estimates from the matching estimators. 9,500 respondents $\times$ 3 panel waves in 2010, 2012, and 2014.

B Mechanism Tests

B.1 Shifting Worldviews

One alternative interpretation of the results is that parenthood transforms one's worldview. If people became more liberal, community-oriented, or open to others, and these shifts in worldviews subsequently led to increased climate policy support, that may confound inference. Of course, one could easily imagine counter-examples where parents become more conservative, household-focused, and less open to out-groups. Indeed, studies turn up mixed results about the ideological effects of parenthood (Elder and Greene 2012a, 2012b).

This paper's primary strategy to deal with shifting worldviews is to control for ideology, partisanship, and religiosity. Measures such as ideology directly capture worldviews that are most likely to be correlated with policy attitudes.

Still, I also conduct falsification tests to further subject this alternative explanation to scrutiny. The logic is that if worldviews are shifting, then policy attitudes should also shift regarding issues where openness to out-groups strongly predicts attitudes. Of the questions on the survey, immigration comes closest to this ideal test. In their review, Hainmueller and Hopkins (2014, 225) conclude that immigration preferences are most related to symbolic issues stemming from attitudes toward ethnic and racial out-groups. If support for immigration increased following parenthood, that could signal that shifting worldviews, as opposed to lengthening time horizons, explain the increase in climate policy support.

I use immigration policy questions asked across all panel waves to construct an index for support for immigration restrictions (Appendix C.4). The scale sums the questions, which takes the values either 1 or 0. I normalize the index to run from 0 to 1. There is high inter-item reliability ($\alpha = 0.73$). Using an index helps to minimize measurement error, which provides for a better-powered falsification test.

In Appendix B.4, I collect new survey data to test whether this battery of immigration questions is a valid proxy for worldviews. I find strong correlations between immigration policy attitudes, as measured by the proxy questions, and social dominance orientation, social value orientation, and community-oriented preferences. This suggests that the immigration policy question, in addition to capturing changing worldviews, is also useful for assessing altruism, an alternative explanation mentioned later.

One potential concern with focusing on immigration is that it could also be a long-term issue, which would mean it might be affected by lengthening time horizons. However, compared to climate change, the effects of the immigration policies the questions ask about are more immediate. One question asks about granting legal status to people currently here. Another asks about increasing border patrols. The last question is about whether the police should have the authority to question anyone they think may be in the country illegally. These focus the survey-taker on the shorter-term issues surrounding immigration.

Figure B1 contains the results of this falsification test. Parenthood does not affect immigration policy preferences. These results are inconsistent with the alternative explanations that changing worldviews explain the shift in climate policy preferences, whereas they are consistent with the time horizon mechanism.

Figure B1: Mechanism Test Results

Notes: Estimates from the panel matching estimator with conditional standard errors that account for dependence across time. Bars denote 95 and 90% confidence intervals. The blood donation outcome is an indicator of whether an individual reports donating blood. The gun control outcome is an indicator of whether the respondent supports more restrictive gun ownership laws. The outcome for immigration policy attitudes is an index that runs from 0 to 1, with higher values indicating greater support for immigration restrictions. 9,500 respondents $\times$ 3 panel waves in 2010, 2012, and 2014.

B.2 Altruism

Another alternative explanation is that parenthood makes people more altruistic, not just to one's descendants but to people outside of the family. Other-regarding preferences, in turn, could enhance support for action to address climate change, which will increase global inequality (Kennard 2021).

However, there is limited evidence that parenthood increases altruism. Instead, the available evidence suggests that parents become more risk averse and selfishly focused on the family unit (Görlitz and Tamm 2020). Therefore, it is unlikely that parents are becoming more altruistic which spuriously changes their climate policy preferences.

The empirical model also partially accounts for changing altruism using measures of partisanship and ideology. Research on political psychology finds that there is a "strong positive association between altruism and left-wing attitudes" (Zettler and Hilbig 2010), so including these controls should help to reduce the risk of spuriousness.

Lastly, I leverage self-reported blood donation as a falsification test for altruism. Studies of why people donate blood indicate that altruism is an important predictor (Ferguson et al. 2012; Yuan et al. 2011). If people became more other-regarding as a consequence of parenthood, they might be more likely to donate blood. Despite these previous studies, the validation survey finds mixed evidence that blood donation is a strong proxy for altruism. However, the validation test shows that immigration preferences are a strong predictor of altruism, and those results show little effect of parenthood on immigration attitudes. There is also no change in blood donation behavior after parenthood.

B.3 Trust in Science

Another alternative explanation is that having a child might increase parents' trust in science, which could enhance belief in climate change and support for action.

I consider increased trust in science to be implausible for four reasons. First, there is no evidence that parenthood increases trust in science. In fact, there are examples of parents who are skeptical of vaccinating their children. Researchers who have interviewed vaccine-hesitant parents find that long-standing distrust explains this behavior, finding no indication that parenthood shifted scientific beliefs (Rozbroj, Lyons, and Lucke 2020). It is not apparent that becoming a parent enhances trust in the scientific community.

Second, skepticism of science derives from a general tendency of people to defend their prior beliefs rather than a trait that might vary over time in response to events like parenthood (Kraft, Lodge, and Taber 2015). Indeed, one theory of trust in science is that it stems from a "cultural cognition of risk," where individuals' views about the existence of scientific consensus conform to their values (Kahan, Jenkins-Smith, and Braman 2011).

Third, the research design controls for ideology, partisanship, and religiosity, which are strong predictors of trust in science. For example, studies show how partisanship can undermine the effect of messages emphasizing the scientific consensus behind climate change (Bolsen and Druckman 2018).

Lastly, the falsification test using blood donations helps further rule out this alternative explanation. The proxy validation survey shows that blood donation behavior is a strong predictor of trust in science. The lack of a change in reported blood donation behavior suggests that trust in science is not changing as a consequence of parenthood.

B.4 Proxy Question Validation

I collected new data to validate the proxies employed in the falsification tests. Using a national sample of the American public, I asked the original questions from the falsification tests. The new survey also included batteries directly capturing the proxied concepts. With the two on the same survey, I can estimate the correlation between the proxy and the more direct measure. A strong correlation would signal greater confidence in the falsification tests.

One possible limitation of this approach is that the survey is at a later point in time than the original panel study. If there was a change in the relationship between the proxy variables and their concepts, that could confound the relationship between the two observed today. However, temporal confounding is unlikely because the proxies and their concepts are capturing long-standing political issues like immigration, behavioral patterns like blood donation, and underlying psychological orientations like altruism. Their correlations are likely to change slowly, if at all.

B.4.1 Data Collection

I used Lucid Theorem to collect a national sample of the American public in December 2023-January 2024. Lucid Theorem is a non-probability opt-in Internet panel. People participate in exchange for cash, gift cards, and other incentives, with compensation handled by the survey vendor. Lucid Theorem fills the survey using a version of quota sampling, which yields a sample that matches the distribution of age, gender, ethnicity, and region of the national population.

After trimming respondents who did not consent to participate, were ineligible, or failed quality control checks, 972 completed the survey. I employed the following procedures to ensure data quality. These tools address threats to data quality, such as people who speed through the survey, bots that take the survey instead of humans, and duplicate responses.

- Completion time: Responses less than one-half of the median total survey completion time are screened.
- Instructional manipulation check (IMC): There are two IMC questions that ask respondents to select a particular answer if they are paying attention. Individuals who do not select this answer will be screened to improve the reliability of the dataset (Ternovski and Orr 2022).
- reCAPTCHA: Respondents must pass a reCAPTCHA check designed by Google, which humans can easily pass, whereas bots cannot.
- Invisible reCAPTCHA: The survey employs Google's invisible reCAPTCHA throughout the survey, which calculates the probability that a respondent is a bot based on how they interact with the survey. Responses with values less than 0.5, indicating that there is less than a 50% probability of being human, will be screened.
- Honeypot: The survey includes a question that is invisible to humans but would be visible to bots. If the question is answered, the survey-taker is screened as a bot.
- Unique IDs: Each respondent is assigned a unique identification (ID) number, so the same ID cannot take the survey more than once.

- RelevantID: The survey employs RelevantID based on the respondent's browser, operating system, and location to flag whether the same individual attempts to take multiple surveys.
- Recorded-reported mismatch: If the demographic information on file at Lucid does not match the reported demographic information in 3 or more categories, the respondent will be excluded for inattentiveness. The categories include ZIP code, sex, age, race/ethnicity, and college education. The threshold is set at 3 because it is possible that a respondent may have moved or graduated, so changes in 2 or fewer of these categories are more likely (Ternovski and Orr 2022).

For transparency, the numbers in parentheses indicate the percent screened out by each procedure.

- Attention check 1 ($< 1\%$)
- Attention check 2 (8.4%)
- Did not consent (3.2%)
- Under 18 years old ($< 1\%$)
- Incomplete survey (1.5%)
- Significant mismatch between recorded and reported demographics ($< 1\%$)
- Duplicate or bot (8.2%)
- Less than one-half the median (5.7 minutes) survey completion time (9.7%).

B.4.2 Sample Description

Table B1: Sample Description for Proxy Validation

	Mean	Min	Max	SD	Missing
Age	48.21	18.00	94.00	16.25	0
Female	0.53	0.00	1.00	0.50	0
White	0.77	0.00	1.00	0.42	0
Black	0.12	0.00	1.00	0.33	0
Hispanic	0.11	0.00	1.00	0.31	0
Married	0.42	0.00	1.00	0.49	0
College	0.34	0.00	1.00	0.47	0
Income Q1	0.18	0.00	1.00	0.38	0
Income Q2	0.21	0.00	1.00	0.40	0
Income Q3	0.19	0.00	1.00	0.39	0
Income Q4	0.12	0.00	1.00	0.33	0
Income Q5	0.30	0.00	1.00	0.46	0
Employed	0.54	0.00	1.00	0.50	0
Democrat	0.43	0.00	1.00	0.49	0
Republican	0.39	0.00	1.00	0.49	0
Conservative	0.31	0.00	1.00	0.46	0
Liberal	0.26	0.00	1.00	0.44	0
Religiosity	3.55	1.00	5.00	1.46	0
Blood Donor	0.14	0.00	1.00	0.35	0
Immigration Index	0.53	0.00	1.00	0.33	0
SDO	0.00	-3.23	1.32	1.00	0
SVO	0.74	0.00	1.00	0.44	0
Science Trust Index	0.00	-2.23	1.85	1.00	1

Notes: National sample collected with Lucid Theorem between December 30, 2023 and January 14, 2024. SDO is the social dominance orientation scale. SVO is the social value orientation scale. "Prefer not to say" responses for income (2%) imputed with the median value. $N = 972$

B.4.3 Measurement

Worldviews The survey included two measures of worldviews. The first is social dominance orientation (SDO). SDO is a concept developed by Pratto et al. (1994) to measure an individual's preference for hierarchy in a social system, specifically with respect to relationships between one's in-group and an out-group. People who have high levels of SDO have more anti-egalitarian worldviews. For example, Pratto et al. (1994, 741) show that SDO predicts "a large number of social and political ideologies that support group-based hierarchy (e.g., meritocracy and racism) and to support for policies that have implications for inter-group relations (e.g., war, civil rights, and social programs), including new policies." SDO has also been used in political science to predict attitudes on issues such as trade (Mutz and Kim 2017).

SDO is measured using the 4-item scale (Pratto et al. 2013). There is good inter-item reliability ($\alpha = 0.77$). I convert these questions to a numeric scale and construct an index using inverse covariance-weighting, which is then standardized so a one-unit shift corresponds with a standard deviation change. The scale is decreasing in SDO, so higher values indicate that an individual has less of a social dominance orientation.

- Show how much you favor or oppose each idea below by selecting an answer on the scale. You can work quickly; your first feeling is generally best.
  - Group equality should NOT be our primary goal.
  - It is unjust to try to make groups equal.
  - We should do what we can to equalize conditions for different groups.
  - We should work to give all groups an equal chance to succeed.

  Strongly Oppose; Somewhat Oppose; Neutral; Somewhat Favor; Strongly Favor

A second measure of worldviews seeks to capture community orientation. Community orientation refers to a sense of responsibility that people feel for their self-described community around them, which may correspond to a willingness to incur personal costs on behalf of the collective. I use the following two questions to measure community orientation. There is good inter-item reliability ($\alpha = 0.8$), so I combine the questions into an index using inverse covariance-weighting. A one-unit increase in this index corresponds with a standard deviation greater community orientation.

- How willing would you be to make sacrifices for your community?

  Extremely willing; Very willing; Moderately willing; Not too willing; Not at all willing

- How important is your community to you?

  Extremely important; Very important; Moderately important; Not too important; Not at all important

Altruism The first measure of altruism uses the Social Value Orientation scale developed and validated by Murphy and Ackermann (2014) and Murphy, Ackermann, and Handgraaf (2011). SVO refers to the extent to which people exhibit concern for others. Using the established methodology, from the following questions I can calculate whether people are altruistic or pro-social. For analysis, I create an indicator that takes the value 1 if a respondent is altruistic or pro-social, and 0 if not.

- In this task, imagine that you have been randomly paired with another person, whom we will refer to as **the other**. This other person is someone you do not know. All of your choices are confidential.

  You will be making decisions about how to allocate money between you and this other person. We are not providing any actual payout, but we still ask you to carefully think about each decision that you make – respond how you would if money was at stake.

  There are no right or wrong answers; this is all about personal preferences.

  As you can see below, your choices influence both the amount of money you receive as well as the amount of money the other receives.

  Select your preferred distribution of money between you and other.

  - You receive: $85 Other receives: $85
  - You receive: $85 Other receives: $76
  - You receive: $85 Other receives: $68
  - You receive: $85 Other receives: $59
  - You receive: $85 Other receives: $50
  - You receive: $85 Other receives: $41
  - You receive: $85 Other receives: $33
  - You receive: $85 Other receives: $24
  - You receive: $85 Other receives: $15

  Select your preferred distribution of money between you and other.

  - You receive: $85 Other receives: $15
  - You receive: $87 Other receives: $19
  - You receive: $89 Other receives: $24
  - You receive: $91 Other receives: $28
  - You receive: $93 Other receives: $33
  - You receive: $94 Other receives: $37
  - You receive: $96 Other receives: $41
  - You receive: $98 Other receives: $46
  - You receive: $100 Other receives: $50

Select your preferred distribution of money between you and other.

- You receive: $50 Other receives: $100
- You receive: $54 Other receives: $98
- You receive: $59 Other receives: $96
- You receive: $63 Other receives: $94
- You receive: $68 Other receives: $93
- You receive: $72 Other receives: $91
- You receive: $76 Other receives: $89
- You receive: $81 Other receives: $87
- You receive: $85 Other receives: $85

Select your preferred distribution of money between you and other.

- You receive: $50 Other receives: $100
- You receive: $54 Other receives: $89
- You receive: $59 Other receives: $79
- You receive: $63 Other receives: $68
- You receive: $68 Other receives: $58
- You receive: $72 Other receives: $47
- You receive: $76 Other receives: $36
- You receive: $81 Other receives: $26
- You receive: $85 Other receives: $15

Select your preferred distribution of money between you and other.

- You receive: $100 Other receives: $50
- You receive: $94 Other receives: $56
- You receive: $88 Other receives $63
- You receive: $81 Other receives: $69
- You receive: $75 Other receives: $75
- You receive: $69 Other receives: $81
- You receive: $63 Other receives: $88
- You receive: $56 Other receives: $94
- You receive: $50 Other receives: $100

Select your preferred distribution of money between you and other.

- You receive: $100 Other receives: $50

- You receive: \$98 Other receives: \$54
- You receive: \$96 Other receives: \$59
- You receive: \$94 Other receives: \$63
- You receive: \$93 Other receives: \$68
- You receive: \$91 Other receives: \$72
- You receive: \$89 Other receives: \$76
- You receive: \$87 Other receives: \$81
- You receive: \$85 Other receives: \$85

I also measure altruism using a version of a question validated against incentivized laboratory games (Falk et al. 2023). I create an indicator for if an individual is above the median in terms of willingness to give.

- How do you assess your willingness to share with others without expecting anything in return when it comes to charity?

  Extremely willing; Very willing; Moderately willing; Not too willing; Not at all willing

Trust in Science I measure trust in science with questions I discovered reading Krause et al. (2019), which they borrow from the General Social Survey. These questions have good inter-item reliability ($\alpha = 0.9$). I combine them into an inverse covariance weighted index, where higher values denote greater trust in science. The index is scaled so a one-unit shift represents a standard deviation increase.

- How much do you trust the things that scientists say about the environment?

  Not at all; A little; A moderate amount; A lot; Completely

- How much do you generally trust **scientists** to tell you the truth?

  Not at all; A little; A moderate amount; A lot; Completely

- How much confidence do you have in the people running the **scientific community**?

  None at all; Not too much; A moderate amount; A lot; A great deal

B.4.4 Proxy Validation Results

I estimate a linear regression of the validated measure on the proxy. This model includes a set of political and socio-demographic controls, so any observed correlation represents variation that is orthogonal to what the difference-in-difference model is already able to account for through its covariate adjustments.

Table B2 presents the results when examining immigration policy preferences. Models 1-4 reveal a strong association between immigration policy preferences and the validated measures for worldviews. People who have more restrictive immigration policy preferences are much less community-oriented. The magnitude of the correlation is strong: a 0.29 standard deviation shift. When it comes to SDO, a one-unit shift in immigration preferences corresponds with 0.80 standard deviation increase in SDO. This estimate is notable because the model controls for powerful predictors of SDO like partisanship and ideology. To the extent that worldviews predict climate policy preferences, the immigration proxy questions would identify spuriousness from such changes in attitudes.

Models 5-8 in Table B2 also show that immigration attitudes strongly predict altruism when measured with the SVO and stated preference approaches. I had not initially anticipated this, but in hindsight, it makes sense that people who are more willing to help others might be more supportive of immigration. To the degree that altruism predicts climate policy support, the immigration proxy questions may identify spuriousness from such dispositional shifts.

Turning to the blood donation proxy, Models 1-2 in Table B3 show no evidence of a correlation between the two when using the SVO measure. However, Models 3-4, using the stated measure of altruism, show a strong correlation. This may suggest that blood donation better captures a dimension of altruism related to social desirability bias. This suggests that blood donation as a falsification test may detect if parenthood affects altruism.

In terms of trust in science, Models 5-6 of Table B3 indicate there is a strong association with blood donation. People who report donating blood have 0.35 standard deviation greater trust in science. This coefficient appears even when controlling for predictors of trust in science like partisanship, ideology, religiosity, and education. These results demonstrate that the blood donation proxy would detect if parenthood caused increased trust in science.

Table B2: Validation of Immigration as Proxy for Worldviews and Altruism

	World Views				Altruism			
	Community		SDO		SVO		Stated	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Immigration Preferences	−0.23**	−0.29***	−1.08***	−0.79***	−0.14***	−0.16***	−0.07*	−0.09**
	(0.10)	(0.11)	(0.09)	(0.10)	(0.04)	(0.05)	(0.04)	(0.04)
Age		−0.03		0.02		0.02		0.02
		(0.04)		(0.03)		(0.02)		(0.01)
Female		−0.22***		0.09		−0.01		−0.02
		(0.06)		(0.06)		(0.03)		(0.03)
Black		0.12		0.05		−0.09*		−0.03
		(0.10)		(0.09)		(0.05)		(0.04)
Hispanic		−0.05		−0.15		−0.12**		0.05
		(0.11)		(0.10)		(0.05)		(0.04)
Republican		−0.07		−0.39***		−0.04		−0.05
		(0.09)		(0.08)		(0.04)		(0.04)
Independent		−0.12		−0.21**		0.03		−0.05
		(0.10)		(0.09)		(0.05)		(0.04)
Conservative		0.07		−0.22***		0.02		−0.02
		(0.09)		(0.08)		(0.04)		(0.03)
Liberal		0.23**		0.16**		0.02		0.03
		(0.09)		(0.08)		(0.04)		(0.04)
Unsure of ideology		−0.18		−0.09		0.05		0.01
		(0.12)		(0.12)		(0.06)		(0.05)
Religiosity		0.19***		0.03		0.01		0.05***
		(0.02)		(0.02)		(0.01)		(0.01)
Income Q2		0.06		−0.03		0.07		0.00
		(0.10)		(0.09)		(0.05)		(0.04)
Income Q3		−0.01		−0.13		0.03		0.02
		(0.10)		(0.09)		(0.05)		(0.04)
Income Q4		−0.02		−0.10		0.00		−0.10**
		(0.10)		(0.10)		(0.05)		(0.04)
Income Q5		0.12		−0.01		0.07*		0.02
		(0.09)		(0.09)		(0.04)		(0.04)
College		0.04		−0.14**		−0.03		−0.02
		(0.07)		(0.06)		(0.03)		(0.03)
Employed		0.02		−0.03		0.02		0.01
		(0.07)		(0.06)		(0.03)		(0.03)
Parent		0.28***		−0.04		−0.03		0.07**
		(0.07)		(0.07)		(0.03)		(0.03)
(Intercept)	0.12**	−0.60***	0.58***	0.62***	0.81***	0.77***	0.23***	0.08
	(0.06)	(0.15)	(0.05)	(0.14)	(0.02)	(0.07)	(0.02)	(0.05)
N	972	972	972	972	972	972	972	972
Adjusted R^2	0.00	0.11	0.13	0.20	0.01	0.02	0.00	0.04

Notes: The table reports the estimates from a linear regression of the proxied concepts on the proxy measure. Heteroskedasticity-robust standard errors are in parentheses. The immigration policy preferences index is constructed using the same questions as in the panel study. Age is standardized for interpretation. The omitted category is neither party for partisanship, moderate for ideology, and the first income quintile for income. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table B3: Validation of Blood Donation as a Proxy for Altruism and Trust in Science

	Altruism				Science	
	SVO		Stated		Trust	
	(1)	(2)	(3)	(4)	(5)	(6)
Blood Donation	0.02	0.03	0.14***	0.12***	0.41***	0.35***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.09)	(0.08)
Age		0.01		0.02		0.00
		(0.02)		(0.01)		(0.03)
Female		−0.01		−0.02		−0.10*
		(0.03)		(0.03)		(0.06)
Black		−0.08*		−0.02		−0.18*
		(0.05)		(0.04)		(0.09)
Hispanic		−0.11**		0.06		0.09
		(0.05)		(0.04)		(0.09)
Republican		−0.06		−0.05		−0.59***
		(0.04)		(0.04)		(0.08)
Independent		0.03		−0.04		−0.50***
		(0.05)		(0.04)		(0.09)
Conservative		0.01		−0.03		−0.19**
		(0.04)		(0.03)		(0.08)
Liberal		0.05		0.05		0.31***
		(0.04)		(0.04)		(0.07)
Unsure of ideology		0.06		0.03		−0.36***
		(0.06)		(0.05)		(0.11)
Religiosity		0.01		0.05***		−0.06***
		(0.01)		(0.01)		(0.02)
Income Q2		0.07		0.00		−0.03
		(0.05)		(0.04)		(0.09)
Income Q3		0.04		0.03		−0.05
		(0.05)		(0.04)		(0.09)
Income Q4		0.00		−0.10**		−0.15
		(0.05)		(0.04)		(0.10)
Income Q5		0.08*		0.01		−0.05
		(0.04)		(0.04)		(0.08)
College		−0.03		−0.02		0.13**
		(0.03)		(0.03)		(0.06)
Employed		0.02		0.01		0.01
		(0.03)		(0.03)		(0.06)
Parent		−0.04		0.06**		0.06
		(0.03)		(0.03)		(0.07)
(Intercept)	0.73***	0.69***	0.18***	0.02	−0.06*	0.55***
	(0.02)	(0.06)	(0.01)	(0.05)	(0.03)	(0.12)
N	972	972	972	972	971	971
Adjusted R^2	0.00	0.01	0.01	0.05	0.02	0.24

Notes: The table reports the coefficient estimates from a linear regression of the proxied concepts on the proxy measure. Heteroskedasticity-robust standard errors are in parentheses. The blood donation covariate is an indicator of whether someone donated blood, matching the question in the panel study. Age is standardized for interpretation. The omitted category is neither party for partisanship, moderate for ideology, and the first income quintile for income. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

B.5 Other Future-Oriented Issues

I evaluate whether having children changes opinions about other policies that affect their future welfare. If time horizons lengthen because parents become exposed to policies that impact their children in the future, policy attitudes concerning these issues should change.

I selected gun control for this test for two reasons. First, one of the primary political arguments for gun control is to address school shootings that impact children. The topic receives widespread media coverage, which should make the issue salient for a new parent whose children will be enrolled in school in the future (Elsass, Schildkraut, and Stafford 2016).

Second, the timing of the survey is crucial for interpreting this test as being about the near future as opposed to the immediate present. The timing of the panel and treatment means that the survey occurs prior to the respondent's child entering school. The two-year panel intervals imply that the respondent's child is at most two years old, while children typically begin school at five in the US. From the perspective of parents, they are answering the question about gun control, thinking about how one's child in the coming years will be affected when they enter school. While this anticipation of exposure to gun violence is certainly not on the same time scale as global warming, it is still future-oriented

Figure B1 presents the effect of parenthood on support for gun control. As hypothesized, having a child increases support for gun control.[19] While gun control does not have as long of a time horizon as climate change, this result is consistent with the claim that parenthood can extend time horizons by exposing parents to policies that affect their children's well-being in the future.

19. Out-group bias is another explanation for pro-gun attitudes (Filindra and Kaplan 2017), but the immigration falsification test suggests that this is not behind the change in parents' gun control preferences.

B.6 Additional Alternative Explanations

Another interpretation of the results is that becoming a parent changes media consumption habits, which could explain their shifting climate policy preferences for reasons unrelated to time horizons. For example, people might have less time to watch partisan television, which could shape their policy attitudes. However, it is unclear why disengagement from watching political content would produce a systematic shift in favor of climate policy. Additionally, when including a measure for whether one watches television news, the results persist (Table D2).

C Panel Survey Instrument

C.1 Questions for Covariates

1. In what year were you born?

2. Are you male or female?

   Male; Female

3. What is the highest level of education you have completed?

   No HS; High school graduate; Some college; 2-year; 4-year; Post-grad)

4. What racial or ethnic group best describes you?

   White; Black; Hispanic; Asian; Native American; Mixed; Other; Middle Eastern

5. Which of the following best describes your current employment status?

   Full-time; Part-time; Temporarily laid off; Unemployed; Retired; Permanently disabled; Homemaker; Student; Other

6. What is your marital status?

   Married; Separated; Divorced; Widowed; Single; Domestic partnership

7. Generally speaking, do you think of yourself as a ...?

   Democrat; Republican; Independent; Other

8. Would you call yourself a strong [Democrat/Republican] or a not very strong [Democrat/Republican]?

   Strong [Democrat/Republican]; Not very strong [Democrat/Republican]

9. Do you think of yourself as closer to the Democratic or the Republican Party?

   Democratic Party; Republican Party; Neither; Not sure

10. Thinking about politics these days, how would you describe your own political viewpoint?

    Very liberal; Liberal; Moderate; Conservative; Very Conservative; Not sure

11. How important is religion in your life?

    Very important; Somewhat important; Not too important; Not at all important

12. Thinking back over the last year, what was your family's annual income?

    Less than $10,000; $10,000 - $19,999; $20,000 - $29,999; $30,000 - $39,999; $40,000 - $49,999; $50,000 - $59,999; $60,000 - $69,999; $70,000 - $79,999; $80,000 - $99,999; $100,000 - $119,999; $120,000 - $149,999; $150,000 - $199,999; $200,000 - $249,999; $250,000 - $349,999; $350,000 - $499,999; $500,000 or more; $150,000 or more; $250,000 or more

13. In the past 24 hours have you ... Watched TV news.

    Yes; No

C.2 Questions for Treatment

1. Are you the parent or guardian of any children under the age of 18?

   Yes; No

2. How many children under 18 do you have?[20]

   1; 2; 3; 4; 5; 6; 7; 8; 9; 10; 11; 12; 13; 14; 15; 16; 17; 18; 19; 20

C.3 Question for Outcome

1. From what you know about global climate change or global warming, which one of the following statements comes closest to your opinion?[21]

   Global climate change has been established as a serious problem, and immediate action is necessary;

   There is enough evidence that climate change is taking place and some action should be taken;

   We don't know enough about global climate change, and more research is necessary before we take any actions;

   Concern about global climate change is exaggerated. No action is necessary;

   Global climate change is not occurring, this is not a real issue.

C.4 Immigration Index

1. What do you think the U.S. government should do about immigration? Grant legal status to all illegal immigrants who have held jobs and paid taxes for at least 3 years, and not been convicted of any felony crimes.[22]

   Yes; No

2. What do you think the U.S. government should do about immigration? Increase the number of border patrols on the US-Mexican border.

   Yes; No

3. What do you think the U.S. government should do about immigration? Allow police to question anyone they think may be in the country illegally.

   Yes; No

20. The exact wording of this question is not provided. The question label in the data appendix is, "Number of children under 18."
21. The first two answer options are coded as 1, and the rest are coded as 0.
22. For consistent valence, I reverse the direction of this question, so 1 is no and 0 is yes.

C.5 Gun Control

4. In general, do you feel that the laws covering the sale of firearms should be made more strict, less strict, or kept as they are?

   More Strict; Less Strict; Kept As They Are

C.6 Blood Donation

5. During the past year did you ... (Check all that apply) ... Donate blood

   Yes; No

D Study 1 Appendix

D.1 Sample Description

The sample employed nationally representative quotas using Census values.

- Gender: 48% Male; 52% Female
- Age: 30% 18-34; 32% 35-54; and 38% 55+
- Race: 75% white; 13% Black; 6% Asian or Pacific Islander; 6% American Ind./Alaskan Native or Other.
- Hispanic: 18% Hispanic: 82% Non-Hispanic
- Education: 65% No college degree; 35% 4-year degree or higher

Table D1: Study 1 National Sample Description

	Mean	SD	Min	Max	N	NA
Age	49.89	17.66	19.00	94.00	2006	0
Female	0.52	0.50	0.00	1.00	2006	0
Black	0.14	0.35	0.00	1.00	2006	0
Latino	0.18	0.38	0.00	1.00	2006	0
College Education	0.35	0.48	0.00	1.00	2006	0
Employed	0.40	0.49	0.00	1.00	2006	0
Income Q1	0.26	0.44	0.00	1.00	2006	0
Income Q2	0.30	0.46	0.00	1.00	2006	0
Income Q3	0.21	0.41	0.00	1.00	2006	0
Income Q4	0.21	0.41	0.00	1.00	2006	0
Income Not Say	0.02	0.14	0.00	1.00	2006	0
Democrat	0.46	0.50	0.00	1.00	2006	0
Republican	0.35	0.48	0.00	1.00	2006	0
Liberal	0.29	0.45	0.00	1.00	2006	0
Conservative	0.28	0.45	0.00	1.00	2006	0
Social Preferences	7.60	2.40	0.00	10.00	2006	0
Risk Preferences	5.51	2.69	0.00	10.00	2006	0
Trust Index	0.00	1.00	−2.37	2.39	2006	0

Notes: Sample collected with Qualtrics, May-June 2023.

D.2 Average Treatment Effect Estimates

Table D2: Study 1 Effect of Child Prime on Time Horizons and Climate Policy Support

	Time Horizons				Climate Policy Support			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Child Prime Treatment	0.16***	0.17***	0.09***	0.09***	0.11**	0.11***	0.04**	0.04**
	(0.04)	(0.04)	(0.02)	(0.02)	(0.04)	(0.04)	(0.02)	(0.02)
N	2006	2006	2006	2006	2006	2006	2006	2006
Adjusted R^2	0.01	0.20	0.01	0.15	0.00	0.32	0.00	0.22
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates from linear regression models with controls for age, sex, Black, Hispanic, income, education, employment, partisanship, social preferences, risk preferences, and trust. Heteroskedasticity-robust standard errors in parentheses. Models (1), (2), (5), and (6) use a numeric outcome, scaled by the control group standard deviation and mean so a one-unit change corresponds with a standard deviation increase relative to the control group. Models (3), (4), (7), and (8) use a binary outcome where 1 represents "extremely" or "very willing," and 0 not. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

D.3 Heterogeneous Treatment Effects

In an exploratory analysis, I examine whether the effect of the treatment differs by gender, partisan identification, and risk preferences. Unlike the panel study, where there is limited statistical power, the sample size for the survey experiment allows for a more credible investigation of treatment effect heterogeneity. Appendix D.4 contains a post hoc power analysis supporting this claim. Table D3 shows no heterogeneous effects by sex, partisan identification, and risk preferences.

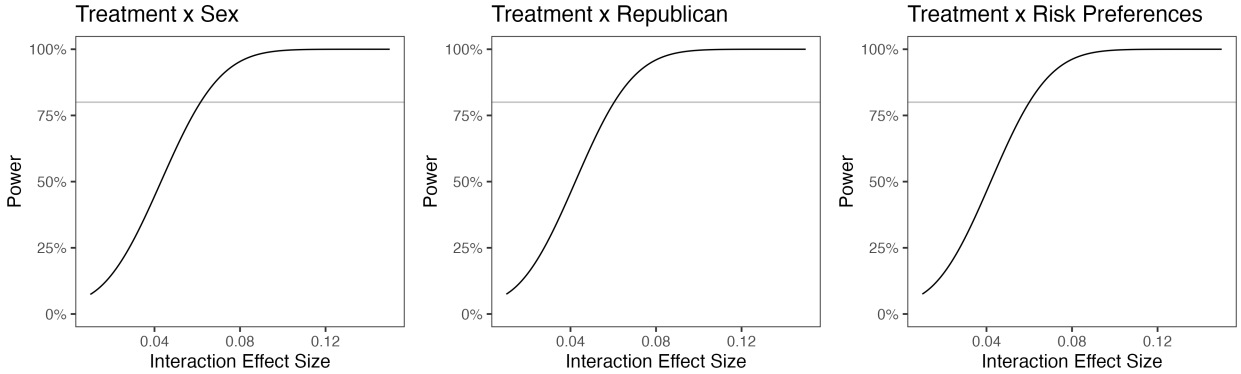
Table D3: Study 1 Heterogeneous Treatment Effects of the Child Prime

	Time Horizons					Climate Policy Support				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment	0.17***	0.18***	0.18***	0.28***	0.17***	0.11***	0.14***	0.10*	0.15*	0.11***
	(0.04)	(0.06)	(0.06)	(0.10)	(0.04)	(0.04)	(0.05)	(0.05)	(0.08)	(0.04)
Treatment x Male		−0.02					−0.07			
		(0.08)					(0.07)			
Treatment x Neither Party			−0.05					0.06		
			(0.11)					(0.10)		
Treatment x Republican			0.00					0.00		
			(0.09)					(0.08)		
Treatment x Risk Preferences				−0.02					−0.01	
				(0.02)					(0.01)	
Treatment x Age					0.06*					0.03
					(0.04)					(0.04)
N	2006	2006	2006	2006	2006	2006	2006	2006	2006	2006
Adjusted R^2	0.20	0.20	0.20	0.20	0.20	0.32	0.32	0.32	0.32	0.32

Notes: Estimates from linear regression models with controls for age, sex, Black, Hispanic, income, education, employment, partisanship, social preferences, risk preferences, and trust. Heteroskedasticity-robust standard errors in parentheses. All models use a numeric outcome, scaled by the control group's standard deviation and mean, so a one-unit change corresponds with a standard deviation increase relative to the control group. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

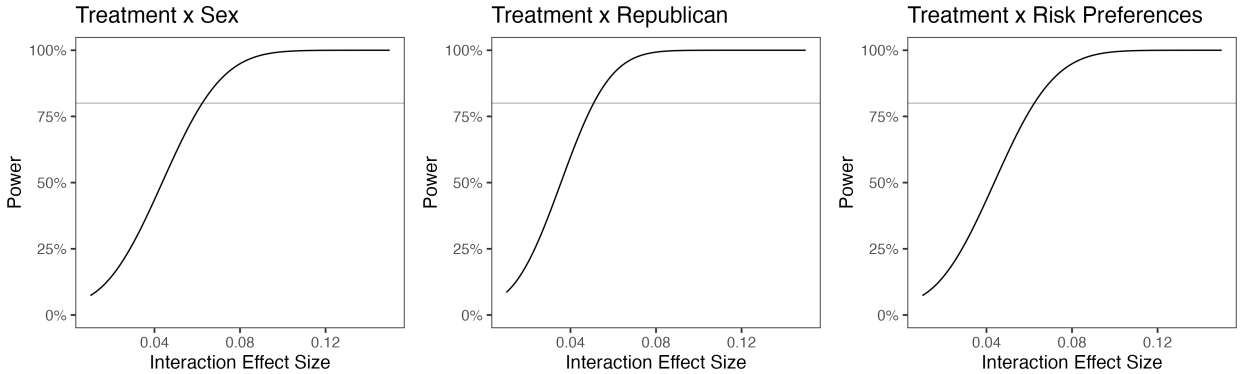
D.4 Post Hoc Power Analysis for Treatment Effect Heterogeneity

Figure D1: Power Analysis for Treatment Effect Heterogeneity, Time Horizons



Notes: Power analysis assumes a sample size of 2,000 and a target significance level of 5%. The x-axis shows the correlation between the interaction term and the outcome. The horizontal gray bar corresponds with the conventional 80% power level. Analysis conducted using the methodology and software from Baranger et al. (2022).

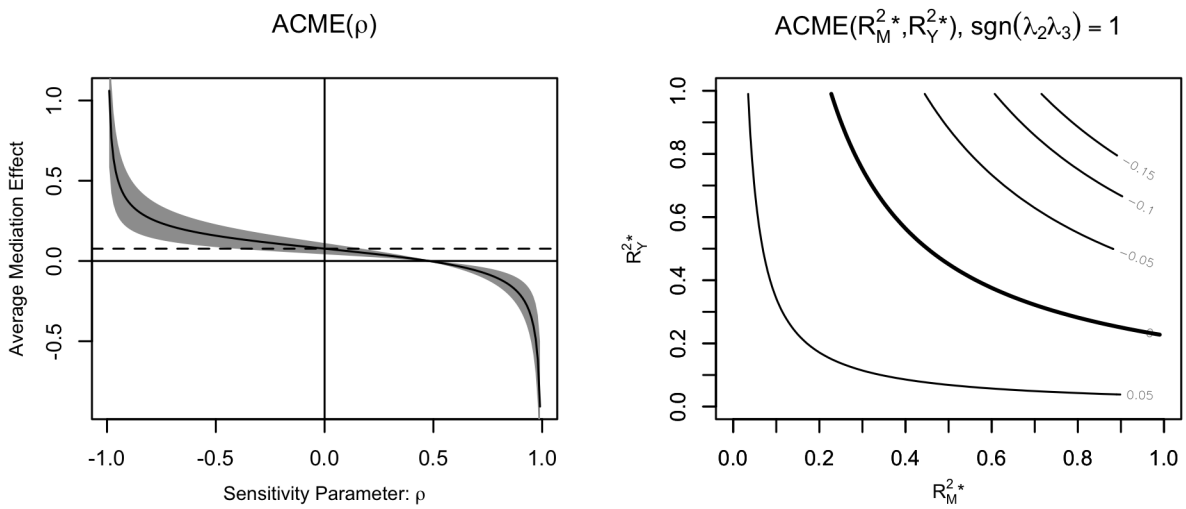
Figure D2: Power Analysis for Treatment Effect Heterogeneity, Climate Policy Support



Notes: Power analysis assumes a sample size of 2,000 and a target significance level of 5%. The x-axis shows the correlation between the interaction term and the outcome. The horizontal gray bar corresponds with the conventional 80% power level. Analysis conducted using the methodology and software from Baranger et al. (2022).

D.5 Mediation Analysis

Figure D3: Study 1 Sensitivity Analysis of the Sequential Ignorability Assumption



Notes: Sensitivity analysis conducted using 10,000 simulations. The left plot uses ρ as the sensitivity parameter, which is the correlation between the residuals of the mediator and the outcome regressions. The ρ for which the ACME is 0 is 0.48. The right plot shows what the R^2 for the residual variance in the mediator and the outcome explained by a hypothesized unobserved confounder would have to be for the ACME to be reduced to 0, which would be 0.23. The models for the mediation analyses include covariates for age, sex, race, income, college education, employment, partisan identification, social preferences, risk preferences, and trust, so any confounder would have to be orthogonal to these covariates.

Table D4: Study 1 Mediation Analysis Regressions

	(1)	(2)
Child Prime Treatment	0.17***	0.03
	(0.04)	(0.03)
Time Horizons Mediator		0.45***
		(0.02)
N	2006	2006
Adjusted R^2	0.20	0.47
F	36.525	156.296
Covariates	Yes	Yes

Notes: Estimates from linear regression models with controls for age, sex, Black, Hispanic, income, education, employment, partisanship, social preferences, risk preferences, and trust. Heteroskedasticity-robust standard errors in parentheses. Model 1 regresses the time horizons mediator on the treatment. Model 2 regresses climate policy support on the treatment and mediator. All models use a numeric outcome, scaled by the control group's standard deviation and mean, so a one-unit change corresponds with a standard deviation increase relative to the control group. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

D.6 Survey Instrument

Below are the relevant questions from the survey instrument.

Background Characteristics

1. What sex were you assigned at birth?

   Male; Female

2. Are you Spanish, Hispanic, or Latino or none of these?

   Yes; None of these

3. Choose one or more races that you consider yourself to be:

   White; Black or African American; American Indian or Alaska Native; Asian; Native Hawaiian or Pacific Islander; Other (please specify)

4. In what year were you born?

5. What is the highest level of education you have completed?

   No High School; Some High School; High School Diploma or GED; Some college course work but non-degree or certificate; Technical Certificate; Associate Degree; Bachelor's Degree; Advanced degree (post college, such as JD or MBA)

Trust Index

6. How often can you trust the federal government in Washington to do what is right?[23]

   Always; Most of the time; About half the time; Some of the time; Never

7. How often can you trust your electric power company to do what is right?

   Always; Most of the time; About half the time; Some of the time; Never

8. How often can you trust local officials, such as the County Commissioners or City Council members, to do what is right?

   Always; Most of the time; About half the time; Some of the time; Never

9. Generally speaking, how often can you trust other people?

   Always; Most of the time; About half the time; Some of the time; Never

23. I construct an index for trust using this battery of questions. There is a theoretical relationship between trust and support for actions to address long-term risks, which often rely on faith in experts and governments (Siegrist 2021). To construct the index, I standardize the response scale by subtracting the mean and dividing it by the standard deviation. Then, I construct an equally weighted index, which is once more standardized so the mean is 0 and the standard deviation is 1. The index has high inter-item reliability ($\alpha = 0.79$).

Risk and Social Preferences

10. How do you assess your willingness to share with others without expecting anything in return when it comes to charity? Please use a scale from 0 to 10, where 0 means you are "completely unwilling to share" and a 10 means you are "very willing to share."[24]

    0 - Completely unwilling to share; 1; 2; 3; 4; 5; 6; 7; 8; 9; 10 - Very willing to share

11. How do you see yourself: are you a person who is generally willing to take risks, or do you try to avoid taking risks? Please use a scale from 0 to 10, where a 0 means you are "completely unwilling to take risks" and a 10 means you are "very willing to take risks."

    0 - Completely unwilling to take risks; 1; 2; 3; 4; 5; 6; 7; 8; 9; 10 - Very willing to take risks

Treatment and Outcomes

12. (Randomize whether the text in [ ] is presented.)

    [If you are a parent, think about your children. If you are not a parent, imagine that you had children.]

    How willing are you to give up something that is beneficial for you today so that the next generation of people will be better off in the future?

    Not willing at all; Slightly willing; Moderately willing; Very willing; Extremely willing

13. How willing are you to pay higher taxes today to combat global climate change if it would make the next generation of people better off in the future?

    Not willing at all; Slightly willing; Moderately willing; Very willing; Extremely willing

24. These measures of risk and social preferences have been validated to demonstrate that they predict costly behavior (Falk et al. 2023).

D.7 Regression of Climate Policy Preferences on Risk Aversion

Table D5: Correlates of Climate Policy Support in National Sample, Control Group Subset

	Scale		Binary		Ordered	
	(1)	(2)	(3)	(4)	(5)	(6)
Age		−0.01***		0.00***		−0.02***
		(0.00)		(0.00)		(0.00)
Male		0.02		0.01		−0.01
		(0.08)		(0.03)		(0.13)
Latino		0.24**		0.04		0.38**
		(0.10)		(0.04)		(0.16)
Black		−0.13		−0.07*		−0.13
		(0.10)		(0.04)		(0.18)
College Education		0.20**		0.05*		0.35**
		(0.08)		(0.03)		(0.14)
Party Identification: Neither		−0.33***		−0.09**		−0.52***
		(0.10)		(0.04)		(0.18)
Party Identification: Republican		−0.53***		−0.13***		−0.85***
		(0.10)		(0.03)		(0.16)
Ideology: Conservative		−0.21**		0.03		−0.53***
		(0.10)		(0.03)		(0.17)
Ideology: Liberal		0.34***		0.14***		0.49***
		(0.09)		(0.04)		(0.15)
Ideology: Not Sure		−0.12		−0.04		−0.19
		(0.16)		(0.05)		(0.27)
Social Preferences		0.07***		0.02***		0.13***
		(0.02)		(0.01)		(0.03)
Risk Preferences	0.15***	0.06***	0.05***	0.02***	0.21***	0.10***
	(0.02)	(0.02)	(0.00)	(0.01)	(0.02)	(0.03)
Trust Index		0.32***		0.07***		0.60***
		(0.04)		(0.01)		(0.07)
Intercept	1.82***	2.39***	0.01	0.13		
	(0.09)	(0.30)	(0.03)	(0.11)		
Not willing at all—Slightly willing					0.13	−0.85
					(0.13)	(0.53)
Slightly willing—Moderately willing					1.02***	0.28
					(0.14)	(0.53)
Moderately willing—Very willing					2.21***	1.83***
					(0.15)	(0.53)
Very willing—Extremely willing					3.20***	3.08***
					(0.17)	(0.54)
N	1008	1008	1008	1008	1008	1008
Adjusted R^2	0.08	0.36	0.07	0.27		
BIC	3398	3130	1146	1009	3089	2823

Notes: Linear regression estimates. Heteroskedasticity-robust standard errors in parentheses. Data are subsetted to the control group so that the child prime treatment does not affect these correlates. Models (3) and (4) use a binary outcome, where 1 represents "extremely" or "very willing," and 0 not. Models (5) and (6) use an ordered logistic regression. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

D.8 Pre-Analysis Plan

The pre-analysis plan and first version of this manuscript reported Bell and McCaffrey (2002) standard errors (Imbens and Kolesár 2016). For consistency with the inference criteria in Study 2, the revised manuscript reports HC2 standard errors. Code to estimate Bell and McCaffrey (2002) standard errors is available in the replication package.

E Study 2 Appendix

E.1 Sample Description

Since the population is adult parents, standard nationally representative quotas of the adult population would be inaccurate. To estimate the socio-demographics of parents, I use data from the Current Population Survey's Annual Social and Economic Supplement.[25] These micro-data come from probability samples of the American public and are the primary source of detailed information on household economics. I use the person-level data, which includes 146,133 observations. I subset these observations to individuals 18 and over and those in households with one or more children. This procedure yields the following quotas for American parents with children under 18.

- Hispanic: 23% yes; 77% no
- Race: 78% white; 10% Black; 7% Asian; 5% Other
- Age: 19% 18-29; 31% 30-39; 30% 40-49; 13% 50-59; and, 7% 60 plus.
- Sex: 46% male; 54% female
- Education: 36% bachelor's degree or higher; 64% less than bachelor's degree.
- Region: 20% Midwest, 37% South, 29% West, 15% New England

As an ex post check of representativeness, I examined the share of the sample with health insurance, for which there was no quota. The value matches the estimate from the CPS probability sample (90%). I also examined the share of married respondents, which is lower in the sample (58%) compared to CPS (66%).

25. These are the same data used by Pew to characterize the demographics of American parents (https://www.pewresearch.org/internet/2013/05/01/part-1-a-profile-of-parents).

Table E1: Study 2 American Parents Sample Description

	Mean	SD	Min	Max	N	NA
Age	42.05	12.04	18.00	88.00	1269	0
Female	0.53	0.50	0.00	1.00	1269	0
White	0.76	0.43	0.00	1.00	1269	0
Black	0.12	0.33	0.00	1.00	1269	0
Hispanic	0.24	0.43	0.00	1.00	1269	0
Asian	0.04	0.21	0.00	1.00	1269	0
Other Race	0.08	0.27	0.00	1.00	1269	0
Employed	0.67	0.47	0.00	1.00	1269	0
High School or Less	0.26	0.44	0.00	1.00	1269	0
Some College	0.39	0.49	0.00	1.00	1269	0
BA or Higher	0.35	0.48	0.00	1.00	1269	0
Income Q1	0.18	0.39	0.00	1.00	1269	0
Income Q2	0.18	0.39	0.00	1.00	1269	0
Income Q3	0.21	0.41	0.00	1.00	1269	0
Income Q4	0.12	0.32	0.00	1.00	1269	0
Health Insurance	0.90	0.29	0.00	1.00	1269	0
Married	0.58	0.49	0.00	1.00	1269	0
Children	1.71	0.89	1.00	5.00	1264	5
Democrat	0.32	0.47	0.00	1.00	1269	0
Republican	0.38	0.48	0.00	1.00	1269	0
Conservative	0.31	0.46	0.00	1.00	1269	0
Liberal	0.21	0.41	0.00	1.00	1269	0
Trust Govt Sometimes	0.40	0.49	0.00	1.00	1269	0
Climate: Not at all worried	0.12	0.33	0.00	1.00	1269	0
Climate: Not very worried	0.21	0.41	0.00	1.00	1269	0
Climate: Somewhat worried	0.39	0.49	0.00	1.00	1269	0
Climate: Very worried	0.28	0.45	0.00	1.00	1269	0

Notes: Sample collected with Cint, February-March 2024.

E.2 Average Treatment Effect Estimates

Table E2: Study 2 Effect of Child Prime on Time Horizons

	Outcome: Time Horizon								
	Own			Comparison			Index		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Child Prime	0.11**	0.11**	0.17**	0.11*	0.10**	0.16*	0.11**	0.11**	0.16**
	(0.06)	(0.05)	(0.08)	(0.06)	(0.05)	(0.08)	(0.05)	(0.05)	(0.08)
Covariates	No	Yes	No	No	Yes	No	No	Yes	No
Estimand	ATE	ATE	CATE	ATE	ATE	CATE	ATE	ATE	CATE

Notes: Estimates from a linear regression of the outcomes on the treatment indicator and covariates where specified. Heteroskedasticity-robust standard errors in parentheses. All outcomes are normalized using the control group's standard deviation and mean, so a one-unit change represents a standard deviation increase in time horizons. Covariates include age, quadratic age, trust, employment, Hispanic, race, female, education, climate change worry, belief about future climate damage, partisan identification, ideology, donation intentions, and religiosity. The complier average treatment effect (CATE) was estimated using a two-stage least squares regression with the child prime as an instrument for compliance. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table E3: Study 2 Effect of Child Prime on Climate Policy Support

	Outcome: Climate Policy Support											
	Tax			Support			WTP			Index		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Child Prime	0.06	0.08*	0.09	0.04	0.07	0.07	0.04	0.06	0.05	0.04	0.07*	0.07
	(0.06)	(0.04)	(0.09)	(0.06)	(0.05)	(0.09)	(0.06)	(0.05)	(0.09)	(0.05)	(0.04)	(0.07)
Covariates	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes	No
Estimand	ATE	ATE	CATE	ATE	ATE	CATE	ATE	ATE	CATE	ATE	ATE	CATE

Notes: Estimates from a linear regression of the outcomes on the treatment indicator and covariates where specified. Heteroskedasticity-robust standard errors in parentheses. All outcomes are normalized using the control group's standard deviation and mean, so a one-unit change represents a standard deviation increase in climate policy support. Covariates include age, quadratic age, trust, employment, Hispanic, race, female, education, climate change worry, belief about future climate damage, partisan identification, ideology, donation intentions, and religiosity. The complier average treatment effect (CATE) was estimated using a two-stage least squares regression with the child prime as an instrument for compliance. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

E.3 Treatment Effect Heterogeneity

I pre-registered the following expectations about heterogeneous treatment effects:

- Sex: Mothers may respond more strongly to the prime than fathers because of the social norms tied to motherhood (Banducci et al. 2016; Burlacu and Lühiste 2021; Elder and Greene 2006, 2007, 2012a, 2012b, 2016).
- Partisanship: The prime may have a larger effect for Republicans and Independents because their baseline support for climate policy is lower than Democrats (Egan and Mullin 2017). Alternatively, it could be the case that Republicans and Independents have stronger pre-existing beliefs, so they are less likely to respond to the treatment.
- Income: The effect of the prime on time horizons and climate policy support might be larger among individuals with higher incomes because they face fewer fiscal constraints, so they can consider longer-term outcomes.
- Number of children: The effect of the prime may be stronger on time horizons and climate policy support among individuals with only one child. Having only one child may heighten a parent's perception of vulnerability and responsibility towards their offspring's future, and the prime may lead them to focus more intensely on their child compared to an individual with multiple children.
- Future climate change concern: The effect of the prime should be larger among individuals who believe that global warming is more likely to harm their children in the future.

Table E4 reveals the absence of heterogeneous treatment effects.

Table E4: Study 2 Treatment Effect Heterogeneity

	Time Horizons					Climate Policy Support				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Child Prime	0.16**	0.10	0.07	0.12*	0.11*	0.01	0.13*	0.08	0.10*	0.11*
	(0.07)	(0.08)	(0.07)	(0.07)	(0.06)	(0.06)	(0.07)	(0.06)	(0.05)	(0.06)
Child Prime × Female	−0.10					0.10				
	(0.09)					(0.08)				
Child Prime × Republican		0.06					−0.15			
		(0.11)					(0.09)			
Child Prime × Independent		−0.07					−0.03			
		(0.11)					(0.10)			
Child Prime × High Income			0.07					−0.02		
			(0.09)					(0.08)		
Child Prime × Future Climate Harm					−0.03					−0.03
					(0.10)					(0.10)
Child Prime × More than 1 Child				−0.03					−0.07	
				(0.09)					(0.08)	
N	1269	1269	1269	1264	1269	1269	1269	1269	1264	1269
Adjusted R^2	0.24	0.24	0.24	0.24	0.24	0.37	0.37	0.37	0.37	0.24
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates from a linear regression of the outcomes on the treatment indicator and covariates where specified. Heteroskedasticity-robust standard errors in parentheses. All outcomes are normalized using the control group's standard deviation and mean, so a one-unit change represents a standard deviation increase in time horizons or climate policy support. Covariates include age, quadratic age, trust, employment, Hispanic, race, female, education, climate change worry, belief about future climate damage, partisan identification, ideology, donation intentions, and religiosity. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

E.4 Manipulation Check

As a manipulation check, the survey measures parental identity. As the pre-registration plan acknowledges, it is possible that parental identity is a latent characteristic that is strong across individuals in the control and treatment groups, so there might be no effect. Indeed, in two of the three items measuring parental identity (Crocetti, Rubini, and Meeus 2008; Piotrowski 2018), Table E5 shows there is no change. Specifically, the prime does not affect people having said they regret having children or that being a parent makes them feel sure of themselves.

However, the prime does have a very strong, positive effect on the belief that one often reflects on their children. This strong effect for reflection about one's children indicates that the prime worked.

Table E5: Study 2 Manipulation Check

	Regret	Sure	Reflect	Index
	(1)	(2)	(3)	(4)
Child Prime	0.00	0.03	0.16***	0.07*
	(0.06)	(0.05)	(0.06)	(0.04)
N	1269	1269	1269	1269
Adjusted R^2	0.05	0.08	0.05	0.09
Covariates	Yes	Yes	Yes	Yes
Estimand	ATE	ATE	ATE	ATE

Notes: Estimates from a linear regression of the outcomes on the treatment indicator and covariates where specified. Heteroskedasticity-robust standard errors in parentheses. All outcomes are normalized using the control group's standard deviation and mean, so a one-unit change represents a standard deviation increase. Covariates include age, quadratic age, trust, employment, Hispanic, race, female, education, climate change worry, belief about future climate damage, partisan identification, ideology, donation intentions, and religiosity. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

E.5 Mediation Analysis

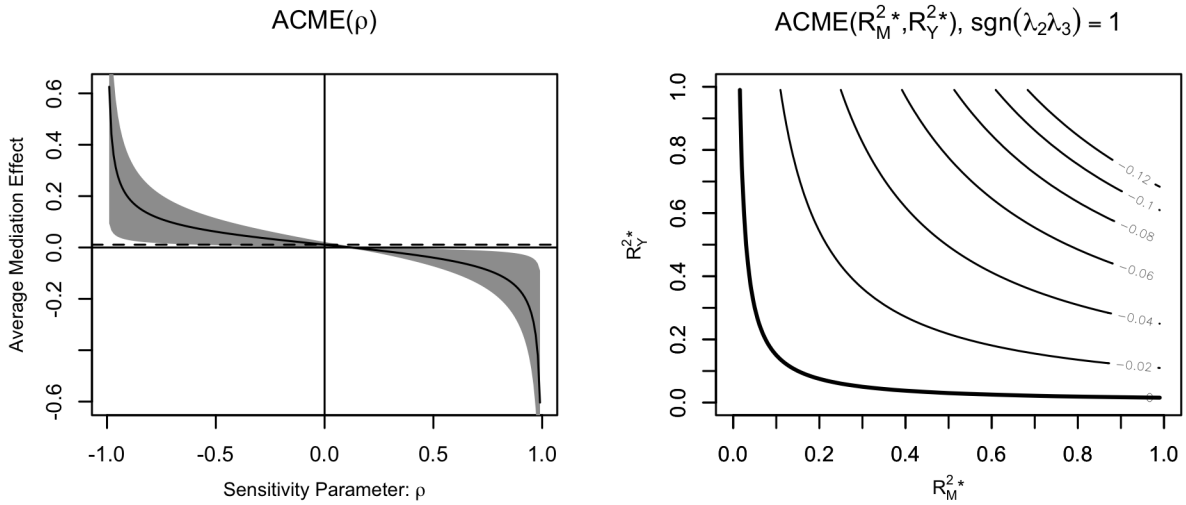


Figure E1: Study 2 Sensitivity Analysis of the Sequential Ignorability Assumption

Notes: Sensitivity analysis conducted using 10,000 simulations. The left plot uses ρ as the sensitivity parameter, which is the correlation between the residuals of the mediator and the outcome regressions. The ρ for which the ACME is 0 is 0.12. The right plot shows what the R^2 for the residual variance in the mediator and the outcome explained by a hypothesized unobserved confounder would have to be for the ACME to be reduced to 0, which would be 0.01. The models for the mediation analyses include covariates for age, quadratic age, trust, employment, Hispanic, race, female, education, climate change worry, belief about future climate damage, partisan identification, ideology, donation intentions, and religiosity. Any confounder would have to be orthogonal to these covariates.

Table E6: Study 2 Mediation Regression Estimates

	(1)	(2)
Child Prime Treatment	0.11**	0.06
	(0.05)	(0.04)
Time Horizons Mediator		0.10***
		(0.03)
N	1269	1269
Adjusted R^2	0.24	0.38
Covariates	Yes	Yes

Notes: Estimates from a linear regression with outcomes are normalized using the control group standard deviation and mean, so a one-unit change represents a standard deviation increase. Heteroskedasticity-robust standard errors in parentheses. Model 1 regresses time horizons on the treatment, while Model 2 regresses climate policy support on the treatment and the time horizons mediator. Covariates include age, quadratic age, trust, employment, Hispanic, race, female, education, climate change worry, belief about future climate damage, partisan identification, ideology, donation intentions, and religiosity. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

E.6 Survey Instrument

Below are the relevant questions from the survey instrument.

Background Characteristics

1. What is your sex?

   Male; Female

2. Are you Spanish, Hispanic, or Latino or none of these?

   Yes; None of these

3. Choose one or more races that you consider yourself to be:

   White; Black or African American; American Indian or Alaska Native; Asian; Native Hawaiian or Pacific Islander; Other (please specify)

4. In what year were you born?

5. What is the highest level of education you have completed?

   No High School; Some High School; High School Diploma or GED; Some college course work but non-degree or certificate; Technical Certificate; Associate Degree; Bachelor's Degree; Advanced degree (post college, such as JD or MBA)

6. Are you the parent of any children under the age of 18?

   Yes; No

7. In general, how would you describe your own political viewpoint?

   Very conservative; Conservative; Moderate; Liberal; Very liberal; Not sure

8. Generally speaking, do you usually think of yourself as a [Democrat/Republican], a [Republican/Democrat, an independent, or what?

   [Democrat/Republican], [Republican/Democrat], Independent, Other party (Please specify).

9. Would you call yourself a strong [Democrat/Republican] or not so strong [Democrat/Republican]?

   Strong [Democrat/Republican]; Not so strong [Democrat/Republican]

10. Do you think of yourself as closer to the [Democratic/Republican] Party or [Republican/Democratic] Party?

    Democratic Party; Republican Party; Neither; Not sure

11. How often can you trust the federal government in Washington to do what is right?

    Always; Most of the time; About half the time; Some of the time; Never

12. How important is God in your life?

    Extremely important; Very important; Moderately important; Slightly important; Not at all important

13. When it comes to charity, how willing are you to share with others without expecting anything in return?

    Extremely willing; Very willing; Moderately willing; Not too willing; Not at all willing

14. Do you have health insurance?

    Yes; No

15. How worried are you about global warming?

    Very worried; Somewhat worried; Not very worried; Not at all worried

16. How much do you think global warming will harm future generations of people?

    A great deal; A moderate amount; Only a little; Not at all; Don't know

Treatment

17. Take a moment to think about your child. Think about what makes your child special.

    I have taken a moment to think about my child

18. Think about the role your children play in your life. Please describe in detail how they have affected your perspectives or your values.

19. Think about your aspirations for your children. Please describe in detail the key goals you have for them.

Distractor Questions

20. The next questions are about how you use the Internet: What device do you prefer to use when browsing the Internet?

    Smartphone; Tablet; Computer; Something else

21. What device are you using to take this survey?

    Smartphone; Tablet; Computer; Something else

Time Horizons

22. The next questions are about how you think about the future.

    How willing are you to give up something that is <u>beneficial for you today</u> so you can <u>benefit more in the future</u>?

    Extremely willing; Very willing; Moderately willing; Not too willing; Not at all willing

23. Now, think about yourself in comparison to others. Compared to others, how willing are you to give up something that is <u>beneficial for you today</u> so you can <u>benefit more in the future</u>?

    Extremely willing; Very willing; Moderately willing; Not too willing; Not at all willing

Distractor Questions

24. Now, we'd like some feedback on this survey. How interested are you in this survey?

    Extremely interested; Very interested; Moderately interested; Not too interested; Not at all interested

25. How easy has it been to understand these survey questions so far?

    Extremely easy; Very easy; Moderately easy; Not too easy; Not at all easy

Climate Policy Preferences

26. The next questions are about climate change.

    When we burn coal, oil, and gas for energy, it releases emissions that warm the planet. We need to reduce these emissions to solve the problem.

    Left unchecked, climate change may cause harm in the future, such as more extreme heat, hurricanes, wildfires, and sea level rise.

    I have read this page

27. How willing are you to pay higher taxes to reduce emissions that cause climate change?

    Extremely willing; Very willing; Moderately willing; Not too willing; Not at all willing

28. How much do you support or oppose paying higher taxes to reduce emissions that cause climate change?

    Strongly support; Somewhat support; Neither support nor oppose; Somewhat oppose; Strongly oppose

29. Using renewable energy to power our homes would help the climate.

    On top of your current power bill, <u>how much more</u> would you be willing to pay <u>each month</u> for power from renewable energy?

    $0, $7, $14, $21, $27, $34, $41, More than $48

Manipulation Check

30. Below are questions about you. Please select the answer that most closely matches your opinion.

    - Being a parent makes me feel sure of myself
    - I often reflect on my children
    - I often think it would have better not to have had any children

    Completely untrue; Untrue; Sometimes true/sometimes not; True; Completely true

Endline Background Characteristics

31. What best describes your current employment status?

    Working full time now; Working part time now; Temporarily laid off; Unemployed; Retired; Permanently disabled; Taking care of home or family; Student; Other

32. Are you now married, widowed, divorced, separated or never married?

    Married; Widowed; Divorced; Separated; Never married

33. How many children under the age of 18 do you have?

    Note; 1; 2; 3; 4; 5; 6; More than 5 (please specify)

E.7 Pre-Analysis Plan

The following modifications were made regarding the covariates included in the empirical models.

- Race: Added indicators for Asian and other races, in addition to Black as pre-registered, because of slight imbalances across the treatment and control group.
- Income: Income was not predictive of climate policy support and exhibited possible signs of being affected by the treatment, so it was trimmed from the model.
- Future climate change impacts: The pre-analysis plan specified that models would account for climate change concern because it was predictive of potential outcomes. Similarly, belief about the future impacts of climate change is predictive of potential outcomes, so it is included in the reported models to improve precision.
- Social preferences: Willingness to donate to charity is predictive of potential outcomes, so it is included in the reported models to improve precision. Study 1 also controlled for a similar measure of social preferences.

F Research Ethics

The study conforms to the APSA Principles and Guidance for Human Subjects Research.

Power I did not engage with vulnerable populations (e.g., children, prisoners). The questions were not sensitive.

Consent I obtained voluntary informed consent from all subjects prior to taking the survey. I transparently communicated my name and affiliation, the general purpose of the research, an explanation of what participation entailed, the potential risks and benefits to participants, how identities and data would be protected, and any other information relevant to the study.

Deception No deception was used.

Harm and trauma No harm or trauma was anticipated or identified.

Confidentiality I clearly communicated assurances of confidentiality during the consent process.

Impact No impact on political processes was anticipated or identified.

Laws, regulations, and prospective review The study complied with all relevant laws and regulations. The researchers obtained prospective review by IRB at [[redacted institution]].

As is standard practice in survey research, the respondents were compensated for their time by the vendors Qualtrics and Cint. The nature of this compensation depends upon the agreement between the survey-taker and the survey panel company. The compensation is fair because a potential survey-taker sees how much one would be compensated if they completed the survey and how long it would take, so they can choose not to take any given survey if they do not think it would be fair compensation for their time.

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