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Does Having Children Affect Parents' Time Horizons? An Empirical Study Among UK Households

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Abstract: It has long been assumed in economic literature that parents possess longer time horizons than do individuals without children. These time horizon differences are used to explain economical outcomes in various areas such as policy development and saving behavior. Using longitudinal data from UK households, we test the hypothesis that an individual's time horizon for decision making is lengthened with the event of becoming a parent. As a proxy for time horizons, we use a measure of the individual's climate change concern. Our findings show that, when controlling for individual heterogeneity, the effect of parenthood on climate change attitudes, and arguably time horizons, decreases to become small and insignificant.

Keywords: Time Horizons, Children, Parents, Bequest, Climate Change, Gender

JEL: D90, D64, J12, J13, J16, Q54

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1 INTRODUCTION

Among economists it appears commonly understood that differences in time horizons—the length of time on which an individual bases his or her decisions[1]—between individuals have a significant effect on policy making, see for instance Kanbur (2001) and Congleton (1992). Furthermore, it is a recurring practice among economists as well as sociologists to assume or argue that time horizons are longer for parents than for individuals without children. In chapter 4 of his book, Sowell (1996) argued that time horizons have implications on the effectiveness of social policies. He also explained the personal differences between individuals as he stated that, among other factors, the time horizons for older people "often extend well beyond their own life-span, as in decisions made for their children's well-being", and in similar spirit argued that when becoming a parent, "it may well be that the time horizon lengthens."

Urbatsch (2014) declared that one of the principal ways in which children affect their parents' is through time horizon alteration and applied that reasoning to foreign policy decisions supported by parents.[2] Specifically, he investigated attitudes related to war and discussed potential future benefits that current wars may have in the distant future.[3] At the same time, Urbatsch acknowledged the possibility of a reversed causality problem: it is possible that the causation between parenthood and time horizons goes the opposite way, so that people with longer time horizons are more likely to beget children.

[1] Beyond the time horizon, benefits are so heavily discounted that they have a negligible effect on the decision making of the individual. The time horizon is subjective, varies across individuals and within individuals across time. It may also be different for different areas; the time horizon for financial decisions is not necessarily the same as the time horizon for environmental decisions.
[2] The other principal factor being self-interest.
[3] Specifically, Urbatsch (2014) argued that parents are more concerned about long term foreign policy decisions than child-less individuals and that parents to girls to larger extent favor military interventions, since potential losses of such are greater to boys (future men) than to girls (future women).

The topic of time horizons, and especially in cases where the time horizon extend beyond an individual's lifetime, is closely related to the topic of bequest motives (Wärneryd, 1999). The presumption that children have a positive effect on the length of parents' time horizons is logical and in line with theory on altruistic bequest motives (Barro 1974).[4] In this essay, we do not set out to unravel the intricate bonds between parents and their children. Instead, we acknowledge that an overwhelming majority of parents do love their children, or at least that parents are altruistically linked to their children, as is the case in the intergenerational models developed by Jouvet et al. (2000) and Barro (1974). For further reading on the basis of love, see Buller (2005) and Daly and Wilson (2001) on evolutionary psychology; Zeki (2007) on neurological reasons; Stets and Turner (2014) and Rohner et al. (2005) on sociological and cultural reasons.

Although having children sometimes have been assumed to be the sole purpose for having a bequest motive, Hurd (1987) found that consumption for those dissaving are greater for people with children, which contradicts the prior that people with children to a greater extent save for bequest.[5] Now, Hurd was focusing on bequeathable wealth, such as money on a savings account. It is possible that a parent's bequest motive would prove higher, if measuring the children's utility increase from the bequest, rather than their increase in monetary wealth. Indeed, such reasoning was strengthened by Kopczuk and Lupton (2007), as they found that parenthood, although it is not the only crucial factor, had a significant positive effect on having a bequest motive. That motive was also found to decrease when the wealth of the children was

[4] Reasons as to why individuals leave bequest have long been debated among economists. Three different answers frequently suggested are uncertainty of time of death (Davies 1981; Hurd 1989), as compensation for being taken care of in the final stage of life (Bernheim et al. 1985) or out of altruistic motives either out of care for the heir (Barro 1974) or because of the pleasant feeling you gain by acting kindly (Andreoni 1989).

[5] According to the life cycle model of consumption (Modigliani 1988), people aim to smooth out consumption over their life-time. This means they borrow early in life, save funds in the working years and dissave after retirement so that wealth is zero by their time of death.

equal to or larger than the wealth of the parent. In other words, if we consider time horizons to be the timespan on which we base decisions in order to maximize utility instead of monetary gain, according to von Neumann and Morgenstern's (1944) definition, then leaving a small amount of money to a child that is already financially well off offers a relatively low marginal utility. If we instead consider a non-monetary bequest, such as the environmental state of the world, then, since all children are worse off than previous generations (Stern 2006), environmental improvement bequests would have a high marginal utility. Thus, if the unit of measurement were something more strongly correlated with the utility of the children, it appears likely that we would see even stronger bequest motives for parents compared to individuals without children.

Is there any literature that has treated intergenerational transfers that do not primarily concern monetary transactions? Dupont (2004) found that parents had a higher willingness to pay (WTP) than individuals without children when it came to environmental goods improvements. She used a specific harbor restauration program in Canada, where respondents to a survey, following the contingent valuation model (CVM), indicated their WTP levels of contributing to the restauration of a polluted harbor site. The site would be transformed to a recreational area where fishing, swimming and other activities were to be made possible. Her estimates were found statistically significant and showed that parents were willing to contribute more to environmental improvements, than individuals who did not have children were. Now, these results may be interpreted as support for the argument that parents care about the long term utility of their children, and could, following the logic presented in previously reviewed literature, indicate longer time horizons for parents. However, the results may also be interpreted as if the restauration would merely benefit the utility of the parents themselves – since parents, rather than non-parents, may to a larger extent use such family-friendly recreational sites.

Four years before the study of Dupont (2004), Teal and Loomis (2000) performed a similar test, focusing on parents' WTP for the preservation of wetlands in the San Joaquin Valley. In contradiction to Dupont (2004), they were not able to find a significant effect of parenthood on WTP. The preservation program used in the study was not closely connected to daily activities of the respondents, which may suggest that children only have a positive effect on parents' environmental improvement WTP when the effort has a direct impact on every day life.

With this thesis, we aim to assess if the presumed relationship between having children and exhibiting longer time horizons have empirical support. To our knowledge, efforts to establish the direct effect of parenthood on the length of an individual's time horizon has never been made. Although previous research, showing how children increase parents' environmental concern, can be seen as signs of long term decision making, the interpretations are equivocal since the decisions also strongly impact the present (Dupont 2004; Teal and Loomis 2000). Furthermore, no previous study has made use of longitudinal data to control for individual time invariant characteristics, which may be main reasons of the perceived parental effect on time horizons.

The thesis is structured as follows. Section 2 formulates our research question and hypothesis. Section 3 introduces our data and describes how we set up our dataset. Section 4 specifies our empirical model. In Section 5, we present our results and discuss the robustness of our results. Section 6 is devoted to discussing the implications of our results. We conclude the thesis in Section 7 and suggest relevant areas for further research.

2 SETTING UP THE HYPOTHESIS

2.1 RESEARCH QUESTION

Although reviewed literature indicate that parents arguably exhibit longer time horizons than people without children, no such research has been made using large-scale data. Furthermore, no previous studies have used non-monetary variables that directly can be linked to the utility of the children. Since differences in time horizons are often used to explain different policy standpoints, and since having children is argued to affect such horizons, we find it appropriate to test that presumption. Ergo, the research question we ask is: Does having children directly affect the length of an individual's time horizon?

2.2 FORMULATING A TESTABLE HYPOTHESIS

As we want to investigate the effect that children (becoming a parent) have on the individual's time horizon, we start by considering the following model:

$$time\ horizon_{it} = \beta_0 + \beta_{parent} * parent_{it} + \beta_X * \boldsymbol{X}_{it} + a_i + u_{it} \qquad (1)$$

where:

$time\ horizon_{it}$ is a measure of the length of person *i's* time horizon at time *t*; $parent_{it}$ is a dummy variable taking on the value 1 if person *i* has at least one child at time *t*; $\boldsymbol{X}_{it}$ is a vector accounting for a set of control variables, these will be addressed further in Section 4.1.2; a_i is the individual fixed effect or unknown individual heterogeneity. In other words, a_i represents factors that differ across individuals but not within individuals over time; u_{it} is the idiosyncratic error term. This term represents unobserved factors affecting time horizons, that vary across individuals and over time within individuals.

Reviewed literature lead us to expect an affirmative answer to our research question, that having children is expected to positively affect the individuals concern for climate change. With this a priori, we set up our hypothesis:

Table 1: Hypothesis Tested

Hypothesis #1	
H_0:	$\beta_{parent} = 0$
H_1:	$\beta_{parent} > 0$

As formulated in Table 1, the null hypothesis is that the effect of parenthood on time horizons is nonexistent and our alternative hypothesis is that parenthood has a positive effect on the length of time horizons.

3 DATA

The data we use to test our hypothesis comes from "The UK Household Longitudinal Study" (UKHLS), commonly referred to as "Understanding Society". UKHLS is the world's largest longitudinal survey on values and beliefs, as it is compiling data from more than 40,000 households or approximately 100,000 individuals in the United Kingdom. The survey is of annual nature and was carried out, with funding from the Economic and Social Research Council, by the Institute for Social and Economic Research at the University of Essex, for the first time in 2009 as a development of the former British Household Panel Survey (BHPS).

The questionnaire of the survey includes a wide array of topics and issues that respondents give their opinion on. Except for a detailed component with personal background questions such as educational history, ethnicity and fertility, there are also questionnaires asked annually regarding financial situation, employment, health and happiness, and family and friends as well as a number of topics that are only used in selected waves. One module, which has not been included in every wave, is one regarding environmental attitudes. Since the survey

was first introduced, the environmental module has been used in wave 1 and wave 4, and is scheduled to be used again in wave 7.[6] We have access to wave 1 and wave 4, corresponding to a two-period panel dataset that we can use in order to empirically estimate our models.

The sample has been randomly drawn using a stratified sampling method with proportionate allocation from the general population of the United Kingdom. The draw was stratified for Great Britain as England was divided into ten strata, Scotland one and Wales one. On Northern Ireland, the draw was conducted in a single stage. In total, 49,915 households were drawn.

To ensure that missing values remain few, and that all questions are understood and answered correctly, the main method of interviewing for the UKHLS has been what is known as computer-assisted personal interviewing (CAPI).[7] However, the environment module, which in our thesis is used to provide our dependent variable, is carried out as a self completion questionnaire on paper. The interviewer follows up to make sure that the self completion form is filled out and returned, most often within the day. In this way, we can be confident that all respondents have partaken in all modules of the survey. In the self completed modules, there may be missing values on specific questions, since some respondents fail to answer.

3.1 Setting Up the Dataset

As will be thoroughly described in Section 4, we will use the first difference of our variables in order to obtain our results. When first differencing our model, the independent variable on which we focus (parent), will take on the value 1 if a respondent has become a parent for the first time in the time period between wave 1 and wave 4. Likewise, the variable

[6] Even if the survey is carried out annually, the interview period reaches over two years. For wave 1, interviews were conducted in 2009 and 2010. For wave 4, interviews were conducted in 2012 and 2013.
[7] CAPI is an interview form where the respondents answer the interview questions on a computer screen with the aid of a present interviewer. The interviewer makes sure that all questions are understood correctly.

will take on a value of 0 if a respondent either was a parent by the time wave 1 interviews were conducted, or was not a parent by the time of wave 4.

Although we use a randomly selected sample of the UK citizens, we cannot say that our "treatment group" has been randomly assigned. As people are more likely to become parents around the age of 30 than they are in any other time in life, the age distribution of our "change in parental status" variable is radically different than the age distribution of the rest of the sample, our control group. If first time parents are directly compared to the entire population of UK citizens, our estimates will suffer from a selection bias, which threatens the model's internal validity due to the fact that we do not have equivalent treatment and control groups. Therefore, we want the treatment and control groups to match before running our regressions.

We solve this problem by weighting each observation in the control group depending on the age of the respondent. In the group of people that become parents for the first time between the two waves (the treatment group) the largest age group are the individuals who were 28 at wave 1. Therefore, all 28 year olds in the control group are assigned a weight of 1. Likewise, there is a very small group of 40 year olds in the treatment group, but a very large group of 40 year olds in the control group. Consequently, we assign a weight to each observation of that age, corresponding to the probability that the observation would be chosen if we drew a subsample according to the distribution of the treatment group. See Appendix, Table A1, for the exact weights on each age category and Appendix, Figure A1-A3 for age distributions of the treatment and control groups. After weighting the control group, the control group in effect exhibits the same age distribution as the "treatment group".

An alternative way to solve the issue would be to draw a subsample from the control group according to the age distribution of the treatment group. The solution with weights, however, is preferable since it allows us to base our estimations on the entire sample.[8]

4 SETTING UP THE EMPIRICAL MODEL

4.1 ECONOMETRIC SPECIFICATION

4.1.1 THE DEPENDENT VARIABLE

Consider again equation 1 from Section 2.2.

$$time\ horizon_{it} = \beta_0 + \beta_{parent} * parent_{it} + \beta_X * \boldsymbol{X}_{it} + a_i + u_{it} \qquad (1)$$

We need a measurable variable that we can use as an indicator of the respondent's time horizon. Since Kopzuk and Lupton (2007) found that the measurable bequest motive is reduced when the income of children increases, we conclude that monetary transfers may be a poor indicator of time horizons as defined in Section 1. Instead of material transactions, is there any immaterial bequest that parents leave to their children? As mentioned in the introduction, the state of the world and its environment is one such thing. Now, not many people have the ability to singlehandedly change environmental issues such as global warming on a notable scale. Even so, each respondent likely differs in the amount of which she or he cares and worries about such change. Based on the insight that climate change will have a greater negative impact in the future than it has today (Stern 2006), one can argue that people who exhibit long time horizons should be more worried about climate change threats than people who primarily focus on the

[8] Within the age span of the treatment group.

present. Following that reasoning, it seems suitable to use a measure of climate concern as a proxy for non-monetary time horizons.

In the UKHLS survey, there is a question, particularly suitable to use as a dependent variable for our regressions, since it includes both a measurement of worry about climate change and a time component. The question reeds: "The effects of climate change are too far in the future to really worry me", to which the respondent can answer: 1) Strongly agree, 2) Tend to agree, 3) Neither agree or disagree, 4) Tend to disagree or 5) Strongly disagree.[9] With a measure of climate change concern in the dependent variable, we are in effect investigating the weights that people ascribe to events taking place in the future and thereby their time horizons. In our base case presented in Section 5.1, we assume continuity in the scale on which our the dependent variable is measured, in other words that the gap between each step in the ordered scale is the same. Since we are using qualitative data based on subjective answers, however, it is not sure that this is always the case. For instance, one could argue that going from "tend to agree" to "strongly agree" is a smaller or larger step than going from "tend to disagree" to "neither agree or disagree." To make sure our results are robust, we apply an alternative approach in Section 5.3.

[9] In wave 1, the question asked is formulated in the exact same way, but the response options differ. Instead of a five step scale, binary alternatives are given according to 1) Yes, I believe this or 2) No, I don't believe this. This discrepancy is not desirable or optimal and has to be handled. We choose to transform the answers from the first wave so that "Yes, I believe this" translates into "Agree" or 1.5 and "No, I don't believe this" translates into "Don't Agree" or 4.5. Using this method, there is a risk that we create some false variation. Therefore, we test our results for robustness in Section 5.3. We do this by running two linear probability model regressions: one where the dependent variable takes on the value of 1 if climate concern goes from negative to positive, and 0 otherwise, and one regression where the dependent variable takes on the value of 1 if climate concern goes from positive to negative, and zero otherwise. An alternative way of dealing with the problem could have been to leave the differing scales as they are, and then run OLS regressions on wave 4 but controlling for answers in wave 1. Yet another option could be to convert the wave 4 scale into the less precise wave 1 scale. The problem with this approach is that it generates missing values for all people answering neither agree or disagree in the second interview, yielding less precise estimates.

4.1.2 THE INDEPENDENT VARIABLES

Our main independent variable of interest is the parent variable. Parental status is represented in equation 1 as a dummy that takes on a value of one if respondent *i* has at least one child at the time of the interview, and takes on the value of zero if respondent *i* does not have any children. Our remaining independent variables are represented by $\boldsymbol{X}_{it}$. The control variables we will take into consideration are education level and log of household net income.

Education has been found to have effects on climate concern (Hamilton 2011) (although equivocal depending on political orientation), and it is also reasonable to assume that education to some extent affects the timing of getting children, for instance if students wait with getting children until after the degree.[10] Therefore, we want to control for education in our regressions. Since we are first differencing the data, much of the variation in the degree term is lost (see Appendix, Table A2). Even so, there is enough variation to yield valuable insights. Our education variable is a dummy which takes on the value of 1 if the respondent has a degree at university or college level, the dummy variable is set to 0 if the respondent has not graduated with a university or college degree.

Sowell (1996) discusses that time horizons differ greatly by socioeconomic class. For instance, maximizing benefits for a family with low income may well mean that the family maximizes payoff in the short run. In extreme cases, if much effort is put on solving the day-to-day living puzzle, not much focus will be put on worrying about the future; tomorrow will worry about itself. It is also likely that some people wait with having children until they have an economically stable situation. Thus, it may be the case that income affects both the dependent variable and the parent variable. For this reason, we also control for household net income in our regressions. We use the natural log of household income for two reasons: logging

[10] Or in some cases wait with the degree until the children are older.

the income variable changes the distribution so that it more resembles the normal distribution. Furthermore, the natural log of income is favorable since it allows us to view changes in income in percent rather than absolute numbers. A certain percentage change in income is more likely to effect the respondents evenly than a certain change in absolute income. As an example, a £5,000 increase for a family earning £20,000 annually is likely to have a large marginal utility whereas the same absolute increase for a family earning £200,000 would offer a relatively low increased utility. A 5 *percent* increase or decrease is likely to have a more equivalent effect on the families' utilities.

There are a number of other factors that are likely to affect the individual's time horizon or climate concern. As will be further discussed in Section 4.2, many of these are in effect controlled for since first differencing the variables effectively removes all time-invariant factors. Example of such factors are gender, religion, ethnicity, political sympathy, and personal characteristics such as sense of responsibility, optimism or pessimism, and the way the alternative answers are interpreted. For other factors, that are in the idiosyncratic error, u_{it}, we assume strict exogeneity, in other words that they are uncorrelated with our independent variables across both time periods.[11] These factors are not the focus of our study and are often hard to measure on an individual level.

4.2 Choosing a Relevant Estimator

In order to unbiasedly estimate OLS coefficients from time series (or panel) data, strict exogeneity must hold. In other words, the error term, which in a regular OLS setting would include both the unobserved heterogeneity (a_i) and the idiosyncratic error (u_{it}), cannot be correlated with any of the independent variables in any of the time periods.[12] Since we have

[11] In our case, the most important thing is that the assumption of exogeneity holds between the u_i and the parent variable on which we are focusing our research.
[12] Individual unobserved heterogeneity is also commonly referred to as the individual fixed effect. We use the two terms interchangeably in this thesis.

access to panel data, however, we are able to control for unobserved factors that are constant over time for each respondent but that differ between the respondents. In doing so, we eliminate the individual fixed effect (a_i) and consequently reduce the scope of our error term ($a_i + u_{it}$), We are left only with the idiosyncratic error, to which the assumption of strict exogeneity must apply.

To our knowledge, no study has been published in which the authors investigate the correlation between parenthood and environmental attitudes that also takes the individual fixed effect, a_i, into consideration. Not considering the individual heterogeneity when researching parental effects on the environment may prove problematic. Such studies cannot with confidence establish that the observed parent status and environmental attitudes correlation is caused directly by having children. It may just as well be the case that unmeasurable, personal characteristics affect attitudes toward the environment, but also affect the likelihood of that individual becoming a parent. One of many possible scenarios is the following: Consider a sample of two persons where one possesses a very strong sense of responsibility, and the other does not. It is not unlikely that a sense of responsibility influences both a person's attitudes toward environmental issues, such as pollution and climate change, and *also* affects that individual's propensity to becoming a parent. By eliminating the fixed effect, we would reduce the need to include a very large set of control variables. Many factors, which arguably are not *fully* time invariant, are to a large extent so (especially, as in our case, when considering data from two time periods relatively close to each other). A few examples of factors that are effectively controlled for, although not included in the model, would be right wing/left wing affiliation, gender of respondent, propensity to internalize social norms and other personal characteristics, the circumstances under which the respondent grew up and religious beliefs.

There are a number of methods that we can use in order to relax the assumption of zero correlation between the fixed effect, a_i, and our regressors. Common approaches include

instrumental variables (IV), the fixed effects estimator (FE) and the first differences estimator (FD). Using IV, we would need to find an instrument that is correlated (preferably highly so) with our regressor of interest, and at the same time argue that the same instrumental variable is uncorrelated with the error terms, both *ai* and u_{it}. Now, such variable is hard to find in general, and even more so within the data we have at hand. Fortunately, as we have access to panel data, we can turn to the FE and FD methods instead. With a FE estimator, we time demean our variables, whereas with an FD estimator, we subtract the observations at time *t-1* from the observations at time *t*. With any of these approaches, we get rid of the fixed effect, a_i, and as our data is collected over two time periods, the two methods will yield the exact same coefficients and standard errors.[13] In this thesis, we choose to apply an FD method to our data because of its nice feature of emphasizing that we in fact regress the change in variables rather than the variables themselves.

No method comes without weaknesses; there are negative aspects of using estimators that only take within variation into consideration.[14] First, the within variation in our sample is much smaller than the variation between individuals (see Appendix, Table A2 for a specification of the within and between variation of our variables). For instance, it is rather likely that a person changes their response from "agree" to "strongly agree" from wave 1 to wave 4. However, if we look at the variation across different individuals, the sample exhibits richer variation that spans all the way from "strongly disagree" to "strongly agree". Since we do have a large sample, we do not consider this to be a severe problem in our case. Second, we are not able to include variables in our regression that differ within observations but not

[13] As time-demeaning the variables mean that we are subtracting the average value in each observation from the value of each observation, it follows that for two period data, FE and FD are the same model with different notation.

[14] Since we have differenced away all variation *between* individuals, the FE and FD models base their estimation solely on variation *within* individuals over time.

between. An example of this is that we cannot control for gender differences.[15] This is primarily a problem if we are interested in the coefficient of the specific variable (gender in our example), or 2) if we believe that change in the specific variable affect the dependent variable differently in different time periods. In a regular OLS regression, we would simply include a gender dummy as an independent variable to control for gender effects in the first scenario, and an interaction term with gender and time to control for the second. In FD regressions, such fixed effects are already being controlled for, even though we cannot see its particular impact or coefficient. Including a gender dummy in our FD regression would solve the second case, as it effectively serves as an interaction term between gender and time. Third, although no longer having to make the assumption of no correlation between unobserved fixed effects and our variables of interest, we still have to assume strict exogeneity between the remaining error term, u_{it}, and our independent variables. In other words, if there is a factor that we cannot observe, and if that factor has an effect on both our dependent variable and at least one of our independent variables, in any time period, our model will yield biased estimates.

5 RESULTS

5.1 THE BASE CASE

After inserting a climate concern variable as a proxy for time horizons, and first differencing equation 1 to get rid of the unknown heterogeneity term, we get:

$$\Delta\, climate\ concern_{it} = \beta_{parent} * \Delta parent_{it} + \beta_X * \Delta \boldsymbol{X}_{it} + \Delta\, u_{it} \qquad (2)$$

Where:

[15] We have already discussed how we solve this particular problem, as gender falls within the fixed effect, a_i. Although in rare cases, gender may change from one wave to another, this is not the case for any of the observations in our sample.

$\Delta\, climate\ concern_{it}$ is the change in climate change concern for person i between wave 1 and wave 4; $\Delta parent_{it}$ is the change in parental status between the two interviews; $\Delta \boldsymbol{X}_{it}$ represents the change in education and log of household net income between the two waves.

We run the first differenced regressions with weights according to Section 3.1 on each observation in the control group. We also use robust standard errors to control for heteroskedasticity in the error variance. The results can be seen in Table 2 (column 1 and 2).

Table 2: FD Estimations of Climate Change Concern on Parental Status and Control Variables, Compared to OLS Estimation on Between Variation

	(1)	(2)	(3)
VARIABLES	FD	FD With Controls	OLS
Parent	0.0131	0.0149	0.0488***
	(0.0496)	(0.0499)	(0.0147)
University Degree		0.0567	0.316***
		(0.0737)	(0.0146)
Log of Household Net Income		-0.0718***	0.118***
		0.0149	(0.0109)
Observations	11,475	11,375	37,153
First Differenced Variables	YES	YES	NO

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The third column (3) in Table 2 features the results from an OLS regression using only between variation. For our case, using between estimators is not a good approach since in line with previous discussions, it does not control for all unobserved, individual heterogeneity. We include the OLS estimates in the table as a comparison to our FD coefficients. The method used in the third column is very similar to earlier research on the area, where they find significant correlation between being a parent and environmental concern, see for instance Dupont (2004). When estimating our coefficients using OLS, we find that being a parent, as well as having a

university degree or higher household income, is positively correlated with climate change concern. This is in line with our prior expectation. The coefficients from the OLS estimation are all statistically significant at the one percent level, meaning that we can be 99% confident that our sample coefficients have the same sign as the true population coefficient. The parent coefficient suggests that if you are a parent, on average, your response on the climate change concern question would be 0.049 steps higher than the average of none-parents, on the scale that ranges from 1 to 5.

In the first column of Table 2, we display the results of a restricted FD model regression, where we regress change in climate change concern on change in parenthood status alone. In this regression we have 11,475 observations out of which 1047 had become parents between the interviews. We find that when individual time invariant factors are differenced away, there is a very small correlation between becoming a parent and becoming more concerned about the climate change. Moreover, the small effect that is displayed, is not statistically significant on any reasonable level, meaning that we cannot infer our sample coefficient to the true population of all UK citizens.[16]

In Section 4.1.2, we argued that level of education and log of household net income are factors that may have an effect on both parenthood status and climate change concern, and should therefore be included in the model. These are not our primary variables of interest, but due to their possible correlation with our parent variable, it is important that we include them. In the second column of Table 1, we show our estimates. The inclusion does not seem to affect our primary independent variable much; becoming a parent still have a small and insignificant

[16] The significance level reported in all our regression output tables are based on two-sided t-tests of each variable. Since we do have a strong prior, namely that parenthood will have a *positive* effect on time horizons, we can also choose to perform one-sided t-tests. None of our variables, that are reported statistically insignificant, reaches a 10 percent level of significance when one sided t-tests are conducted. We report all p-values for one- and two-sided tests in Appendix, Table A3.

effect on change in climate concern. Graduating from university has in our sample a positive effect on climate concern, so that if you get your degree in between the two waves, on average, your concern for climate change increases 0.05 units more between the two waves than it did for those who did not graduate.[17] Just like with the parent variable, however, we cannot infer these results to the population as they too are insignificant. For the change in log of household net income variable, we see a negative effect indicating that our prior may be wrong. This change is significant at the 1 percent level and says that with a one percent increase in the ratio (*household net income*$_{i2}$)/(*household net income*$_{i1}$),[18] change in climate concern is reduced by 0.000718 units. We see that although the change is statistically significant, it is also so small that it is hard to consider it economically significant. This, somewhat surprising, negative effect may be present due to the high correlation between education and income level. The people who are getting a degree between the waves, are likely to be the ones whose income increase the most (percentagewise). Another possible explanation is that people may think that future climate changes will have a larger impact on those in lower socioeconomic classes. Then, such an effect could be what takes out the arguments of our prior, even if we expected an increased concern with increased income.

5.2 Differing Gender Effects

Several researchers have suggested that women, and especially mothers, foster larger concern for the environment than do men, see for instance Stern et al. (1993) and Blocker and Eckberg (1989). Furthermore, researchers also suggest that mothers foster a larger concern for the well-being of their children, either because of biological reasons (Zeki 2007) or because they in approximately 90 percent of the cases are the person closest to the child (Brehaut et al. 2004; Burton et al. 2002). Therefore, we have reason to believe that time horizons of women

[17] Remember that units are measured on the assumedly continuous scale from 1 to 5
[18] Where time t=2 represents wave 4 and time t=1 represents wave 1.

would on average lengthen more than time horizons of men when becoming a parent. Gender is, in our sample, a time invariant factor and is already controlled for in our earlier regressions as part of the fixed effect. It could however, in line with aforementioned literature, add to our insights if we present the results separately for each gender.

Table 3: Hypotheses Tested

Hypothesis #2		Hypothesis #3	
H_0:	$\beta^F_{parent} = 0$	H_0:	$\beta^M_{parent} = 0$
H_1:	$\beta^F_{parent} > 0$	H_1:	$\beta^M_{parent} > 0$

Where F refers to female respondents and M refers to male respondents. The β is defined as in equation 2.

As formulated in Table 3, we test the hypotheses that becoming a mother has a positive effect on the length of the woman's concern for climate change, and that becoming a father has a positive effect of the man's concern for climate change. Coefficients for equation 2, estimated for women and men separately, are presented in Table 4.

Table 4: Gender Separated FD Estimations of Climate Change Concern on Parental Status and Control Variables, Compared to Estimations from the Total Sample Regression

VARIABLES	(1) FD	(2) FD Women	(3) FD Men
Parent	0.0149	0.0454	-0.0349
	(0.0499)	(0.0648)	(0.0782)
University Degree	0.0567	0.0400	0.0739
	(0.0737)	(0.0933)	(0.119)
Log of Household Net Income	-0.0718***	-0.0458	-0.0992**
	(0.0270)	(0.0356)	(0.0426)
Observations	11,375	6,789	4,582
Women Included	YES	YES	NO
Men Included	YES	NO	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The first column of Table 4 shows the FD estimates from our base case previously discussed in Section 5.1 and Table 2. The second and third column display the output from the same regression as in the first column, yet only applied to female respondents' data in column 2 and male respondents' data in column 3. As expected, the output indicates that women do exhibit a stronger increase in climate concern when becoming parents than the gender averaged value. Women in the sample who have become parents between wave 1 and wave 4, have on average increased their concern for climate change by 0.0455 units more than women who have not received their first child during the same time period.[19] Even if we see an increase in the coefficient displaying the amount of impact the change in parent variable has for women compared to the total sample, we do not find the estimates significant at any relevant level and cannot infer the results to the overall population of female UK citizens. Looking at column 3, we see that the coefficient for men is instead lower than the estimate of the total sample. In fact, becoming a parent could have a negative effect on climate change concern. As before, the estimate is not statistically significant.

Even if we have insignificant estimates, the table also shows that in our sample, education seems to increase men's climate concern more than it increases women's. This finding is in line with previous research by McCright (2010), as he discovered that women on a general level have a higher level of climate change knowledge than men. Accordingly, education could arguably yield a larger marginal return of climate change awareness for men.

The estimated coefficients for household income only feature minor changes. As opposed to when measured for the total population, we no longer find any significance in the income estimates for women, and the significance level is reduced for men.

[19] Remember that units are measured on the assumedly continuous scale from 1 to 5.

5.3 Robustness Test

In Section 4.1.1, we discussed two assumptions which had to apply to our dependent variable in order for our coefficients to be unbiased. First, we had to assume that the scale of response options is continuous. In other words, that a one step move in any direction along the scale is an equally large move no matter where on the scale it takes place. Second, we also assumed that the conversion of the scales did not create any false variation. In this section, we apply an alternative method that relaxes both of these assumptions.[20]

Converting climate change concern responses from wave 1 to fit wave 4 alternatives is not problem free. Under the assumption that respondents interpret "Yes, I believe this" as "Agree" and "No, I don't believe this" as "Don't agree", each respondent chooses to go from "agree" to "strongly agree" or "tend to agree", from "don't agree" to "strongly disagree" or "tend to disagree" etcetera. It can be argued that the direction is indeed chosen by the respondent, and that the sample is likely to be a good approximation of the reality. Even so, one can imagine scenarios where the assumption does not hold. For instance, a respondent at wave 1 may have wanted to answer "strongly agree" if that option had been available in the wave 1 questionnaire, but instead chose to answer "agree" since it was the best alterative present. In such cases, we create a false variation within that respondent and need to make sure that this do not affect our results.

In order to test for robustness of our previous models, we estimate two linear probability models (LPM) where the first aims to establish if new first time parents have a higher or lower probability than the overall population to go from a negative response on climate change concern to an affirmative response.[21] In other words, we are comparing the cases were

[20] Relaxing the assumptions comes at a cost. The observations in our control group is drastically reduced from more than 1000 to only 58 in equation 4 and 37 in equation 3.
[21] Note that a negative response to the original question means an affirmative response when interpreted as climate change concern.

respondents have gone from not worrying about climate change to worrying about climate change, and compare these cases to all other cases. We neglect all change within the affirmative or negative response categories. In the second regression, we use a similar model, but instead test the hypothesis for the cases where the change has gone in the opposite direction. Consider the following two models:

$$Start\ Worrying = \beta_{\Delta parent} * \Delta parent + \beta_X * \Delta \boldsymbol{X} + \Delta\, u_{it} \qquad (3)$$

$$Stop\ Worrying = \beta_{\Delta parent} * \Delta parent + \beta_X * \Delta \boldsymbol{X} + \Delta\, u_{it} \qquad (4)$$

Where the right side is the same as in equation 2, and the left hand side is represented by dummy variables. In equation 3, the dummy variable, "*Start Worrying*", takes on the value 1 if the respondent answered that she or he worries in wave 4 but not in wave 1. In all other cases, the "Start Worrying" dummy is zero. The dummy variable of equation 4, "Stop Worrying", takes on a value of 1 if the respondent *did* worry in wave 1 but no longer does when asked in wave 4. Estimating the first LPM model (equation 3) yields the results presented in Table 5.

Table 5: LPM Estimations of "Start Worrying" on Parental Status and Control Variables for the Total Sample, Women and Men Separately

VARIABLES	(1) All	(2) Women	(3) Men
Parent	0.00559	0.00195	0.0100
	(0.00693)	(0.00867)	(0.0112)
University degree	0.0148	0.0187	0.00995
	(0.00936)	(0.0128)	(0.0137)
Log of Household Net Income	-0.00432	-0.00401	-0.00473
	(0.00307)	(0.00426)	(0.00435)
Observations	15,927	9,020	6,907
FD	YES	YES	YES
Women	YES	YES	NO
Men	YES	NO	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Column 1 in Table 5 displays the estimated coefficients from equation 3. When compared to the results from our base case in Table 2, we see that there are no remarkable differences. The parent variable, which represents change in parental status, is small and has a positive coefficient of 0.00559.[22] According to the model, the probability of going from not exhibiting a concern for climate change in wave 1 to doing so in wave 4 is 0.56 percentage points higher if the respondent have become a parent for the first time within that same period of time. As in our base case, the estimate is not statistically significant on any acceptable level.

In contrast to the results we saw when regressing our base model separately for each gender, we see that in the LPM model, first time fathers exhibit a 1 percentage point higher probability to shift opinion from not worrying to worrying compared to other men whereas first time mothers only have 0.02 percentage point higher probability of changing opinion in favor of climate concern. In our gender differentiated model in Table 4, women exhibited a higher return to parenthood in terms of climate concern than did men. One explanation of these differing results will become clear when analyzing our second LPM regression output. Again, none of the coefficients are significantly different from zero.

As for the education and income variables, they show similar results to our base case regressions. For recent graduates, we see slightly higher probabilities to change opinion than among people who did not graduate between the two waves. A respondent's income increase has a small and insignificant effect on the probability of starting to worry about climate change.

Now we turn to estimating our second LPM model (equation 4). The regression results are presented in Table 6.

[22] Remember that all variables are first differenced and consequently the parent variable represents change in parental status.

Table 6: LPM Estimations of "Stop Worrying" on Parental Status and Control Variables for the Total Sample, Women and Men Separately

VARIABLES	(1) All	(2) Women	(3) Men
Parent	0.00480	0.000612	0.0106
	(0.00858)	(0.0112)	(0.0133)
University degree	-0.00454	-0.0166	0.0110
	(0.00969)	(0.0117)	(0.0162)
Log of Household Net Income	0.00787**	0.00790	0.00755
	(0.00383)	(0.00551)	(0.00520)
Observations	15,927	9,020	6,907
FD	YES	YES	YES
Women	YES	YES	NO
Men	YES	NO	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The estimates from Table 6 are interesting, as they suggest that when becoming a parent for the first time, it is not only more likely that you change in *favor* of climate change concern, as Table 5 estimates indicated. It is also more likely that you change opinion in the other direction. If an individual becomes a parent for the first time between the interview moments, there is a 0.48 percentage points higher probability that he or she stops worrying about climate change than the corresponding probabilities are for people who did not become parents for the first time in the same period. It is very important to remember that these estimates are *not* significant, so we should be very careful about interpreting anything from them. However, in our sample, it seems like the event of becoming parent may have an effect on respondents' opinions, but that change may be either positive or negative. This contradicts our prior expectations of a positive effect. In the discussion in Section 6.1 below, we discuss reasons as to why having children may cause a negative effect on time horizons. Although insignificant, the parental effect is higher for men than for women, which may partly explain why we saw a higher positive effect for women in our base case.

The education estimates are small and insignificant. As for the income variable, we find a statistically significant effect when estimating the coefficient for the entire sample. The estimate suggests that as a person's income increase ratio rises by one percent, the probability that he or she stops worrying about the threats of climate change is 0.008 percentage points higher than corresponding probability for individuals whose household income remained unchanged. These findings are consistent with the findings of our base case. Although the income effect is statistically significant, it is barely economically so, given its limited level of impact.

6 DISCUSSION

6.1 DISCREPANCY BETWEEN RESULTS AND A PRIORI

When testing our hypothesis – that having children has a positive effect on parents' time horizons – we find no significant effects and can therefore not reject our null hypothesis – that such effect is nonexistent. Previous literature has put forward arguments that lead us to having a prior expectation of affirmative estimates. What can cause this discrepancy? Not being able to reject our null hypothesis does not mean that we accept it to be true. We see three major reasons why this discrepancy between prior expectations and results have emerged, and discuss these one by one below.

First, our a priori may in fact be wrong. If parents do feature longer time horizons than others, it may be that the effect is not caused by having children, but instead by other factors, which also have an effect on the likelihood of becoming a parent. A plausible explanation, discussed briefly in Section 4.2, is that people who exhibit a stronger sense of responsibility, are more likely to want to become parents and also more likely to worry about climate change. Such personal characteristics have not been controlled for in previous research due to lack of

longitudinal data. With the fresh UKHLS data, we are able to difference away all individual heterogeneity, and in doing so, we see how parenthood goes from having a strong and highly significant effect on climate concern (and by extension time horizons), to having a small and statistically insignificant effect. These results challenge the common presumption of children's effect on their parents' time horizons and suggest caution in interpreting parental effects as explanatory.

Second, there may exist an offsetting effect on time horizons for parents of very young children. In the UKHLS survey, the climate concern questionnaire has so far only been included twice, with three years in between. The consequence of this is that all parents in our treatment group, at the time of wave 4, have very small children, all aged three or younger. It can be argued that for parents with young children, much focus is on the present, as new parents may worry about managing work, arranging daycare pickups and making sure the child eats at the same time as they get less sleep every night. Such commitments may crowd out worries of the future. If this is the case, children may have a positive impact on parents' time horizons even if those effects do not show in the first years of being parent. To elaborate further on the difficulty of deciding at what particular time the effect is realized, it is not only possible that the effect is seen with a time delay, it may also be the case that the time horizon of an individual is extended before the birth of the individual's child. For instance, a person may be taking future generations into consideration if he or she believes that he or she will one day have children. The effect may come when planning to become a parent, when the future parent is expecting the child, or at other times that do not correspond well with the particular moment of birth. In a hypothetical scenario, then, it could for some individuals, be that the time horizon is first lengthened when planning for children, then temporarily shortened during stressful periods in life, such as when the children are very small or during the teenager years.

Third, climate change concern may not be a sufficiently good proxy for time horizons. Brulle et al. (2012) analyze factors that affect climate change concern in the United States. They find that exogenous factors such as media coverage and elite partisan cues have a large influence on climate change concern.[23] In line with Brulle et al. it may be the case that people exhibit low knowledge about climate change, and that opinion on climate change is not yet internalized. Then, a majority of the people would likely find climate change worrisome only if the issue got much air time on the news. Nevertheless, Gallup Polls (Pugliese and Ray 2009) made at the same time of wave 1 in UKHLS, show that 97 percent of UK citizens demonstrate awareness about the threats of global warming, which in contrast to Brulle et al. would support using climate change as a proxy for time horizons.

Which of these three potential explanations can we expect to best explain the discrepancy between prior notion and regression results? We deem it highly likely that personal characteristics (first explanation) do have an affect on both climate change, time horizons and parenthood, and that these personal attributes are to an extent correlated. Is this individual heterogeneity enough to completely erase the significance of having children, as our regression results project? Probably not. The research by Kopczuk and Lupton (2007) is rather comprehensive, and indicates a connection between parents' and children's utility functions. As for the third explanation, we have argued that climate change concern in fact is a good proxy for time horizons in a UK setting. This is also in line with the thinking of Urbatsch (2014) as he specifically mentions environmental concern as an indicator of time horizons.

Much of the explanation of the discrepancy between our a priori and results may lie in the second point – that the effect do not appear immediately at the time of birth. Such reasoning would also be consistent with Sowell's (1996) arguments of crowding out effects. Although he

[23] They test their hypotheses on a national level as they analyze the public opinion rather than individuals.

applies the reasoning to low income, a link to other concerns can easily be seen: there is only so much one can worry about at any one time, no matter the reason. Measuring long term apprehension during a period in life when many people are particularly stressed because of short term concerns, may yield weak results.

6.2 Reversed Causality

One could imagine scenarios where the causality runs the opposite way, so that people with longer time horizons are more prone to getting children. For instance, in regions of the world where political instability, threat of war or severe natural disasters are imminent, people may await a more tolerable future before trying to get children. In such cases, our hypothesis is deceitfully formulated, and even with significant results, we could not be sure of the estimates' internal validity. In a UK setting, however, where this survey is conducted, we see very small risks of such reversed causality and consider our model valid.

7 Conclusions

The aim of this thesis is to establish whether or not the often assumed positive relationship between parental status and length of time horizons has empirical support. We use UKHLS panel data collected in the United Kingdom between year 2009 and 2013 to estimate parental effects with a first differences estimator. First, we test our hypothesis on the entire sample, then we conduct gender specific tests to ascertain whether or not the effects differ between men and women. As an intermediary measurement for the length of time horizons, we use individual concern for future climate change.

Following our prior expectations, the hypothesis we test is that when an individual becomes a parent for the first time, his or her time horizon lengthens. When performing OLS regressions using variation *between* individuals in our sample, we find a statistically significant

higher concern for future climate change among parents than we do among non-parents. When taking individual fixed effects into account by using a FD estimator, however, estimated effects on the total sample are small and statistically insignificant. These findings suggest that much of the explanatory value in determining individuals' levels of concern for future climate change are caused by individual heterogeneity. Characteristics within a person, such as level of responsibility, may be a factor influencing people to become parents and also to worry more about future climate change. In any event, our results serve to advocate caution in assuming a parental effect on time horizons.

Based on previous research, we are lead to believe that individuals of differing gender may see different effects of parenthood. When gender differentiating the sample, we find that the parental effect estimates increase for women and decrease for men, but the estimates are still statistically insignificant. In summary, we do not find adequate evidence in the UKHLS data on climate change concern, to support a view where the actuality of having children directly affects a person's time horizon.

We have based our research on the most resent data of its kind. Although the new data has opened possibilities to control for individual heterogeneity, it is not without limitations. Conducting similar tests on data covering more time periods would be advantageous for several reasons. Valuable properties of data with more time periods include that it would increase the size of the treatment group, increase within variation and consequently yield more precise estimates. With lower standard errors, it is possible that we will be able to establish a statistically significant positive *or* negative parental effect on time horizons. Additionally, with the study stretching over a longer time period, it will be possible to test for different age classes of children and pin down when and if a parental impact takes place: is it when planning for children, when becoming pregnant, or when the child turns 4?

In this thesis, we have only considered UK data. For future research, it would also be interesting to include data from other countries. Do cultural differences and internalized social values influence the way in which children affect parents' time horizons? Are individuals from highly individualistic countries, such as Sweden, less affected by having children than individuals from countries with more traditional values?

8 REFERENCES

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8.1 DATA SOURCE

University of Essex. Institute for Social and Economic Research and NatCen Social Research, *Understanding Society: Waves 1-5, 2009-2014* [computer file]. *7th Edition.* Colchester, Essex: UK Data Archive [distributor], November 2015. SN: 6614, http://dx.doi.org/10.5255/UKDA-SN-6614-7

9 APPENDIX

Table A1: Weights Ascribed to Different Age Categories

Age Wave 1	Number of New Parents	Total Number of Respondents	Weight
16	9	774	0.1040
17	23	683	0.3013
18	34	582	0.5226
19	31	571	0.4857
20	28	526	0.4762
21	33	462	0.6390
22	36	494	0.6519
23	38	537	0.6330
24	42	555	0.6770
25	56	542	0.9243
26	55	584	0.8425
27	63	612	0.9209
28	74	662	1.0000
29	77	714	0.9648
30	73	688	0.9492
31	58	697	0.7444
32	49	708	0.6191
33	51	706	0.6462
34	41	692	0.5300
35	47	750	0.5606
36	35	784	0.3994
37	27	778	0.3105
38	22	786	0.2504
39	9	855	0.0942
40	9	840	0.0958
41	6	866	0.0620
42	7	859	0.0729
43	7	769	0.0814
44	2	795	0.0225
45	2	804	0.0223
49	1	754	0.0119
50	2	734	0.0244

Table A2: Summary of Dependent and Independent Variables: Within and Between Variation Reported Separately

Variable		Mean	Std. Dev.	Min	Max	Observations
Climate Change Concern	overall	3.646944	1.17193	1	5	N = 37351
	between		1.069917	1	5	n = 23362
	within		.592583	1.896944	5.396944	T-bar = 1.59879
Parent	overall	.635992	.4811554	0	1	N = 53084
	between		.4708002	0	1	n = 26542
	within		.0993072	.135992	1.135992	T = 2
University Degree	overall	.381981	.4858765	0	1	N = 53055
	between		.4781601	0	1	n = 26537
	within		.0862312	-.118019	.881981	T-bar = 1.99928
Log of Household Net Inc.	overall	7.862494	.6458809	.0413815	9.903487	N = 45811
	between		.596783	.0413815	9.903487	n = 26459
	within		.2853348	4.26687	11.45812	T-bar = 1.7314

Figure A1: Age Distribution of Respondents, Total Sample (Frequency)

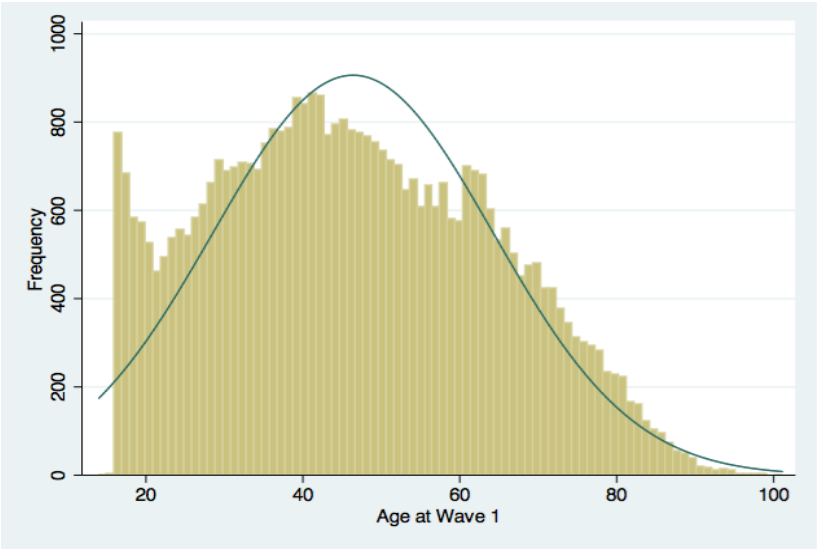


Figure A2: Age Distribution of Respondents, First Time Parents (Frequency)

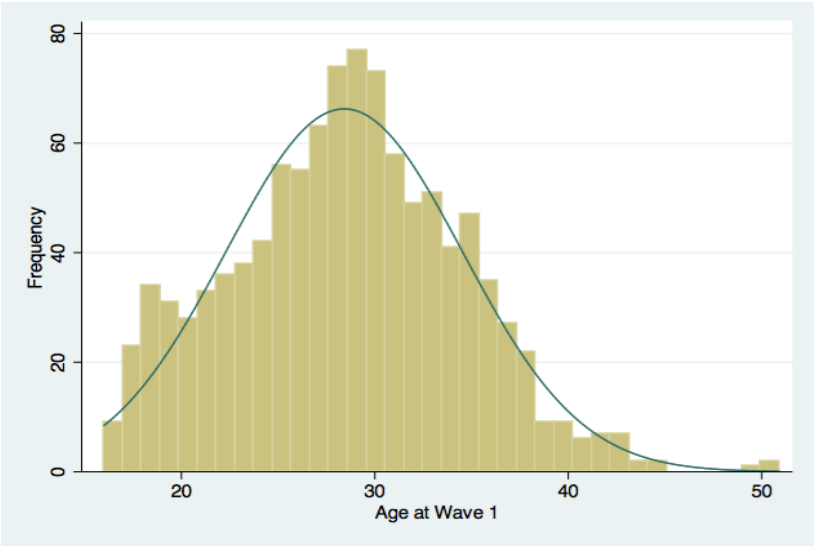
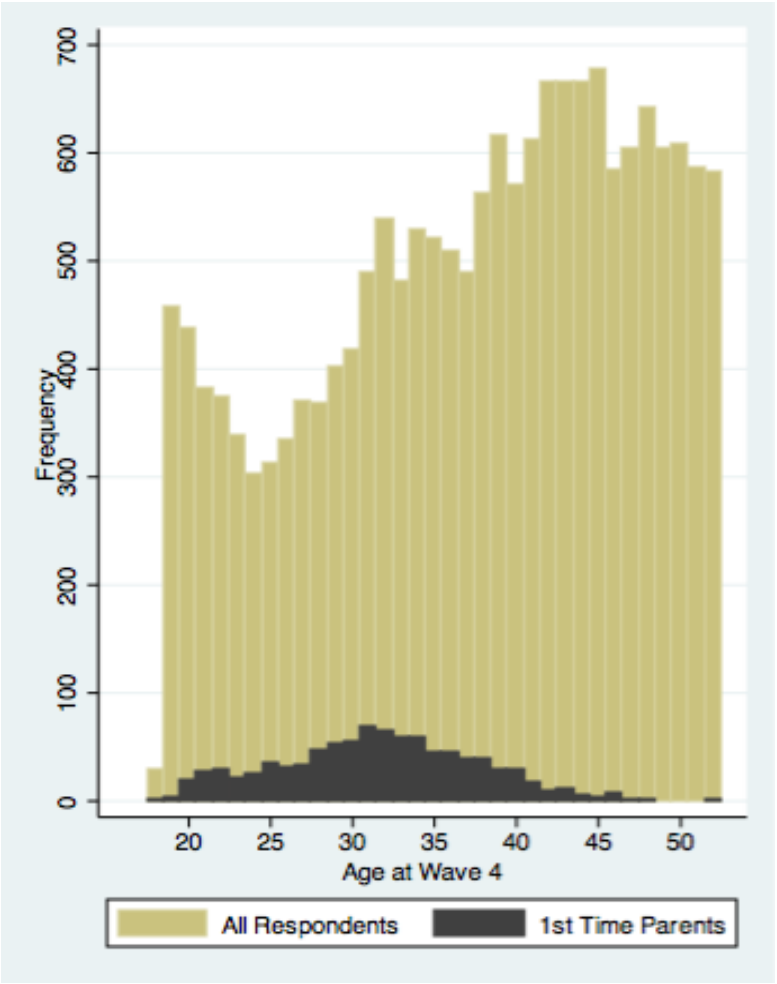


Figure A3: Age Distribution of Respondents Under 54, 1st Time Parents in Dark



Notes: In Figure A1 and A2, ages are reported from wave 1 data. In Figure A3, ages are reported from wave 4 data. Furthermore, the mean age of all respondents (Figure A1) is 46.40 years whereas the mean age of first time parents (Figure A2) is 28.42 years.

Table A3: P-Values from One-Sided and Two-Sided T-Tests

Table:Column	Variable	P-value	
		Two-sided	One-sided
2:1	Parent	0.792	0.396
2:2	Parent	0.765	0.383
	University Degree	0.442	0.221
	Log of Household Net Income	0.008***	0.004***
4:2	Parent	0.483	0.242
	University Degree	0.668	0.334
	Log of Household Net Income	0.198	0.099*
4:3	Parent	0.656	0.328
	University Degree	0.535	0.268
	Log of Household Net Income	0.020**	0.010***

***Significance at the 1 percent level
***Significance at the 5 percent level
***Significance at the 10 percent level