The Effects of Income on Birth Rates: The Case of a Universal Cash Transfer

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January 16, 2020

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Acknowledgments: The work was supported, in part, by a grant from the Economic Security Project. We received helpful feedback from Florencia Torche, Stephen Morgan, Andrew Gelman, Michael Hout, Paula England, Jeff Manza, Kenneth Wachter, Molly Martin, Tim Bruckner, Deborah Karasek, Hilary Hoynes, Eddie Hunsinger, Lawrence Wu, Siwei Cheng and Amanda Geller, participants of the Population Association of America 2019 annual Meeting, the Interdisciplinary Association of Population Health Science 2019 annual meeting and the Guttmacher Institute seminar series. We received research assistance from Anne Kou, Jessica Kalbfeld, Samantha Cunningham and Talya Nevins.

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Abstract

Though the idea of a universal basic income has attracted attention around the globe of late, evidence about the possible impact of such a massive income transfer in the developed world relies primarily on experiments conducted over fifty years ago in North America. We examine the effects of a universal cash transfer on childbearing, a site of inequality and a focus of commentators' and experts' concerns regarding the unintended consequences of social policy. We turn to the Alaska Permanent Fund Dividend, which has provided all Alaskan residents with a substantial annual cash payment since 1982. The amount of the payment varies year to year and is exogenous to individual Alaskans and the state's economy. We find the payment increases short-term fertility, particularly among socioeconomically disadvantaged women. Importantly, there is no change in the proportion of births that are unintended. In supplementary analyses, we find suggestive evidence that the increased fertility is due to increased fecundity. These results imply that additional income removes economic constraints to reproductive health and autonomy. We empirically advance the evidence base for a universal basic income policy and knowledge of the relationship between income and childbearing.

In recent years, an international movement proposing cash transfers to individuals or families has become an increasingly prominent proposal to address poverty and inequality or to address current and future job losses due to technological change and automation (Aronowitz and DiFazio, 2010; Standing, 2017; Van Parijs and Vanderborght, 2017). In wealthy countries, these proposals have generally taken the form of calls for a cash income grant that would be provided to all individuals, thereby establishing an income floor for everyone. Increased income, corporate or consumption taxes would be needed to finance those transfers (Murray, 2006; Stern, 2016; Bregman, 2017; Lowrey, 2018).

Some versions of these proposals have been implemented: Governments in both developed and developing countries around the world have instituted both conditional and unconditional cash transfer programs to benefit poor households. Conditional cash transfers require participants to comply with conditions related to specific arenas like health or education (Maluccio and Flores, 2005; Fernald et al., 2008; Lim et al., 2010; Fernald and Hidrobo, 2011; Rasella et al., 2013; Evans et al., 2014; Brownell et al., 2018); unconditional transfers do not require specific behavior from recipients. In the face of evidence that traditional foreign aid programs fail to improve the well-being of individuals in receiving countries (Easterly, 2006; Deaton, 2013), some non-governmental organizations have mounted controlled experiments in developing countries (Haushofer and Shapiro, 2016) to determine whether direct cash transfers would provide a more effective means of increasing well-being. In addition, several new small-scale cash transfer interventions have begun in the United States, one of which is a randomized control trial (Browne, 2017; Crane, 2018).

The underlying idea behind such cash transfers is that directly increasing individuals' or families' resources could improve their well-being immediately across a range of important outcomes, such as by increasing consumption of necessary items, thereby alleviating stress caused by income uncertainty. In addition, it could encourage behaviors that are beneficial in a longer time-horizon such as investing in schooling, thus indirectly helping families. But sociologists have also long known that changes such as large-scale cash transfers can have many unanticipated consequences (Merton, 1936). For example, the last time cash transfers received significant attention in the United States, social scientists and policymakers debated whether they would induce potentially unintended indirect consequences, such as reducing labor market participation or encouraging married couples with children to divorce or unmarried couples never to marry. To test for some of these consequences, in the late 1960s and 1970s the U.S. federal government funded a series of Negative Income Tax (NIT) experiments. These experiments were conducted in four locations and participants were randomly assigned to various combinations of base transfer amounts and tax rates. A similar experiment was conducted in Manitoba, Canada at the same time. Although the results regarding these unintended consequences from the NIT were mixed and the experiments were conducted over fifty years ago (Hannan et al., 1977; Burtless, 1986; Ashenfelter and Plant, 1990; Hannan and Tuma, 1990; Cain and Wissoker, 1990a,b), they have provided the primary evidence for consideration of the indirect effects of universal cash transfers on individual and household behavior in the United States.

One topic that has received considerable attention in the social science literature on poverty, as well as in debates over welfare reform, is the impact of social policy on fertility. There is a longstanding hypothesis that income impacts fertility in a variety of ways (which we discuss in the first section of the article). However, the NIT experiments generally did not collect data on fertility to consider this question, and major societal changes in family composition and fertility have occurred since the 1970s (Teachman et al., 2000; Cherlin, 2009, 2010; Smock and Schwartz, 2020).

More generally, questions about fertility are important for many reasons, including that most Americans hope to and do become parents and that childbearing is a site of marked inequality. Having children is among most young Americans' aspirations (Hayford, 2009; Morgan and Rackin, 2010) and eighty-five percent of American women have children in their lifetimes (Livingston et al., 2015). The patterns of family formation – at what age people become parents, within what kind of relationship, how many children they have with how many partners, how many pregnancies they have and how those pregnancies fare and end – reflect deep-seeded differences in American life. People of color have more children at younger ages and with more partners than white Americans (Martin et al., 2018, 2019; Stykes and Guzzo, 2019). Some people have more children than they want and even more have fewer children than they would like; this is patterned by education (Morgan and Rackin, 2010). Finally, at a population level, declining birth rates have raised alarm in regions where they have fallen below replacement level (United Nations, 2013; Lee et al., 2014; United Nations, 2015).

What do researchers know about the impact of income and cash transfers on fertility decisions and childbearing outcomes? Unfortunately, the data on the impact of the NIT transfers on fertility decisions are only available for one site - Gary, Indiana (Kehrer and Wolin, 1979; Wolin, 1978). The sample was small and largely homogeneous with respect to race, income and family structure. Instead, researchers in the United States have relied on changes in the tax code such as those of the Earned Income Tax Credit, in the tax exemption for dependents, or in the implementation of near-cash transfers such as with food stamps. Each of these has their weaknesses with regard to understanding the effects of a universal cash transfer on fertility; most importantly, they are policies that primarily benefit people who are already parents and are heavily means-tested. However, there is one other source of potentially rich data on the exogenous impact of income changes via cash transfers that could be exploited: annual exogenous cash transfers made to every Alaskan resident.

Every year since 1982, each resident of Alaska has received a cash transfer from the state through the Alaska Permanent Fund Dividend (PFD). The value of the payment is, as we and others argue (Hsieh, 2003; Kueng, 2018), as good as random with respect to individual Alaskans. There are three sources of variation within this program that we exploit to consider the effect of income on childbearing. The first is the substantial year-to-year variation in the amount of the dividend payment. The second is variation in a given year in the dividend paid to each household depending on the household size. The third is who receives a payment during pregnancy; given that gestation is ten months and a year is twelve months, most people who give birth in a given year receive a payment during their pregnancy, but some do not. [1]

Our article proceeds as follows: first, we review the economic and sociological literature on income and fertility. We then propose hypotheses based on this literature. Following the hypotheses, we describe the case of the Alaska Permanent Fund Dividend and argue that (1) amount of the payment is exogenous to individual Alaskans, (2) there is no factor correlated with fertility that is also correlated with the dividend (the exclusion restriction) and (3) that Alaska is similar enough to other parts of America that we can draw inferences from it. We then describe the methods and the data we employ to test the hypotheses and present the results. Following the results, we present a supplementary analysis that explores possible mechanisms undergirding the effect of income on fertility. To do so, we marshal numerous additional data sources which we detail briefly then and in more detail in the appendix. We lastly conclude the article with a discussion of our findings in light of the state of literature on income and fertility and the implications for Universal Basic Income proposals.

1 Income and Fertility

Modern scholarly debates about the relationship between income and fertility begin with Gary Becker's influential proposal, first advanced in a series of papers in 1957, that social scientists should consider fertility as a problem like any other kind of consumption, one that is subject to constraints. Families are faced with budget and time constraints; children cost both money and time. Parents need to allocate their income between consumption and childbearing and their time between working, leisure and caring for children. As the argument goes, complicating this is that parents care not just about the number of children they have (quantity) but also their "quality" and will spend more when they desire higher "quality" children. Therefore, the cost of children is a function of, among other things, the interaction between quantity of children and "quality." The cost of children, Becker argues, is in part a function of the opportunity cost of childcare, that is the wage the parent (usually the mother) could have earned if she was working rather than spending her time providing childcare.

[1] People of all gender identities give birth to children. We use gender neutral terms when we can. The data in studies that we cite, by and large, only identify women who have given birth; in those cases we refer to these people as women and use female pronouns.

According to Becker's theory, the effect of an income increase on fertility depends on the source of the income. If the income source is non-wage, as the PFD is, then the theory predicts that fertility will rise. This is because it would be an increase in the family's income without altering the opportunity cost of time spent caring for children. If the income source is through wages, then the effect on fertility is ambiguous. It increases family income, which could result in an increase in fertility, but it also increases the opportunity cost of time spent providing childcare. Now, each hour spent providing childcare would have earned even more money if spent working for pay. This increased opportunity cost could result in a substitution effect. In the face of increased wages, families may substitute other goods for fertility, and this would result in a negative effect on fertility. Whether the income effect or the substitution effect dominates is not evident unless information regarding preferences is available (Becker, 1960; Becker and Lewis, 1973; Becker, 2009).

The PFD is given to every household member, including children, and can therefore also be thought of as a "baby bonus." Using Becker's framework, this is a decrease in the fixed cost (that is, not dependent on the number of children) of each child. This would result in an increase in fertility, according to the theory. If some people are more sensitive to this influx in income, i.e. their demand for children is more elastic, then their childbearing would be more greatly affected by the income than their less sensitive peers. The result would be that the composition of people giving birth after the income influx would shift with a greater share of people most sensitive to the income.

The best approach to test the theory is to examine exogenous changes to any of the elements: non-wage income, the cost of children, or wage income (primarily maternal wage income). The PFD is both an increase in non-wage income and a reduction in the cost of childbearing, and we examine the empirical literature on those shocks next.

1.1 Non-Wage Income Shocks

Findings of prior work on the effects of exogenous shocks to non-wage income on fertility are mixed but even when significant primarily modest in magnitude. They consistently show heterogeneous effects, though the specific populations more sensitive to the transfer differ depending on the study. The welfare literature generally finds larger effects on fertility for white women than women of color (Moffitt, 1998). There are some exceptions. The introduction of Food Stamps in the 1960s and early 1970s resulted in small but statistically insignificant effects on fertility. The fertility effects were, however, greater for black women than white women (Almond et al., 2011). The 1970s expansions to the Earned Income Tax Credit (EITC) resulted in small reductions in higher order fertility for white women (Baughman and Dickert-Conlin, 2009). In the 1990s, the EITC increased first births among low-educated women, particularly among nonwhite, married women (Baughman and Dickert-Conlin, 2003). Higher base rates of welfare benefits had no effect on white unmarried mothers but increased the time to next birth for black unmarried mothers (Grogger and Bronars, 2001).

As mentioned before, among the Negative Income Tax sites, fertility effects were only analyzed for Gary, Indiana. There, the experiment gave 1,000 black, urban and largely female-headed families differing and large amounts of cash (about $260 monthly, $100 more than provided by welfare, Aid to Families with Dependent Children). About 400 babies were born during the study. This represented a decrease in fertility in the treated group compared to the control group. There were no heterogeneous effects found, but the sample was largely homogeneous so as to leave many possible axes of difference impossible to examine (Wolin, 1978; Kehrer and Wolin, 1979). The Manitoba, Canada study, conducted around the same time, showed no fertility effects (Forget, 2011).

In sum, the literature to date on transfers and fertility is mixed with regard to average effect. Some studies show no effect, some show negative and others show positive effects. The most consistent finding is that of heterogeneous effects, but even that is not universal. Across the existing literature, the transfers examined are made to low-income populations, limiting their capacity to illuminate how families from across the income range respond to additional income.

An exogenous shock to men's income can also plausibly function as a shock to family income given that mothers are the primary caregivers. The coal boom from 1950 to 1990 resulted in an increase in income for men, which resulted in an increase in fertility; unfortunately, researchers did not assess whether certain groups were more or less affected by this income increase (Black et al., 2013). In a study with a more diverse sample, male job loss accelerated childbearing but decreased total fertility by reducing later births (Lindo, 2010). In this study as well, researchers did not assess heterogeneous effects.

1.2 Tax Code Changes and Baby Bonuses

The cost of children can be manipulated in various ways through government policies. Pro-natalist changes in the tax code resulted in positive and statistically significant effects on fertility (Whittington et al., 1990; Whittington, 1992, 1993) (but see also Crump et al. 2011). Baby bonuses such as those in Quebec decrease the cost of a child and raise fertility and do so differentially by parity, marital status, age and nonlabor income. Most relevant for our discussion, lower income women were less responsive than higher income women (Milligan, 2005). In contrast, family cap or child exclusion policies increase the cost of children for welfare recipients by not providing additional income for children born while the mother was receiving welfare. Thus, the children born during this time cost more than those born prior to welfare receipt. These policies have mixed effects. Some studies find no meaningful effect (Grogger and Bronars, 2001; Kearney, 2004), whereas others find the family cap reduced fertility, particularly for black women, though this research is controversial (Loury, 2000; Jagannathan and Camasso, 2003). Reviews of welfare benefits more broadly indicate no or a modest relationship between welfare and fertility (Hoynes, 1996; Moffitt, 1998). In sum, literature on changes to the cost of children is not conclusive regarding whether there is an average effect on fertility overall; those studies that do show an effect using individual level data (as opposed to aggregate data) tend to indicate heterogeneous effects across many personal characteristics.

1.3 Economic Cycles

A related economic literature discusses the relationship between the business cycle more broadly and fertility. Across the developed world, fertility is pro-cyclical; that is, fertility declines during recessions (Sobotka et al., 2011; Morgan et al., 2011). This is typically a fertility postponement rather than an effect on completed fertility (Sobotka et al., 2011). The decline is an average effect, but there are substantial heterogeneous effects by parity, income, education and marital status. Entry into parenthood is most sensitive to economic conditions. Less educated or less skilled men tend to have the greatest declines in fertility during a recession. Typically, highly educated women postpone childbearing during recessions, while lower educated women are either less sensitive to the macro-economy or accelerate their childbearing (Sobotka et al., 2011).

In sum, people delay fertility when the economy is weak; historically, socioeconomically advantaged women have been particularly sensitive, whereas disadvantaged women were not. Drawing upon both qualitative and quantitative work, sociologists of the family have argued that socioeconomically disadvantaged women responded to economic conditions with regard to marital decisions but not childbearing (Edin et al., 2004; Edin and Reed, 2005; Gibson-Davis, 2009). These patterns shifted during the Great Recession: Unexpectedly, unmarried and low-SES women were sensitive to the recession and reduced their fertility (Schneider and Hastings, 2015; Schneider, 2017). Americans in general reacted by reducing their fertility (Cherlin et al., 2013; Schneider, 2017) which is consistent with prior economic downturns.

1.4 Hypotheses

On the basis of the literature on income and childbearing, we propose the following hypotheses:

Hypothesis 1: Additional income will result in increased short-term fertility.

Hypothesis 2: Additional income will induce heterogeneous fertility effects.

Consistent with the majority of the work on income and fertility, we hypothesize that socioeconomically disadvantaged women will be less responsive to the income changes than advantaged women.[2] That is:

Hypothesis 3: Additional income will have a smaller effect on socio-economically disadvantaged women's fertility than advantaged women's fertility.

If hypothesis 3 is true then it will be evident in either no effect on disadvantaged women's fertility or a smaller effect relative to advantaged women. We operationalize socioeconomic disadvantage in three ways: as women with less than a high school education, who are unmarried, or who are women of color.

These hypotheses have been tested before, but the literature on transfers and fertility to date suffers from methodological weaknesses that are overcome by examining the Alaska Permanent Fund Dividend. First, for the literature that exploits changes in welfare benefits such as food stamps or the Earned Income Tax Credit, the population of interest is limited to those eligible for those benefits, socioeconomically disadvantaged families. Often those families that receive the benefit are not precisely identified in the data, but researchers use proxies to identify families that are likely eligible such as low educated single mothers. The dividend in Alaska, however, is given to every resident regardless of income, working status or family structure and so provides a much more diverse population to study and one that can be precisely identified. Second, the literature on macro-economic changes such as the Great Recession measure an individual's exposure to the recession using aggregate measures like the state unemployment rate. These studies can capture an overall sense of economic uncertainty but cannot capture any individual's experience of the recession or sense of their economic future. With the PFD, we know precisely the size of the transfer to every person and family. Third, the prior literature captures a few years at a time - when the policy was implemented or changed or the years of the Great Recession, for instance. We can capture changes for nearly thirty years. Lastly, some prior work cannot identify an exogenous income shock, relying on endogenous ones such as earnings changes. This is sufficient for some claims like identifying a differential sensitivity to economic conditions for marriage and childbearing (Gibson-Davis, 2009) but is unable to identify a causal relationship between income and fertility as we test here. We now describe the case in more detail.

Empirical Case: Alaska Permanent Fund Dividend

We examine the payments the Alaskan state government has made to every Alaskan resident since 1982 through the Alaska Permanent Fund Dividend. Every October, this cash transfer is given to a large and

[2] We do not know the gender identity of people who give birth from the birth certificate data (our numerators). We construct denominators that are counts of people who report their sex as female on the Census or American Community Survey, which also does not ask about gender identity, to capture people at risk of giving birth. Recognizing this, we use gender-neutral language as much as possible. As Darwin and Greenfield (2019) note, "We have not yet developed shared language in research or practice to adequately describe reproductive histories outside of a cis birth mother's." For clarity, at times we use the term "women," though inaccurate, to refer to people at risk of giving birth to clarify that the group to which we refer does not include males.

diverse population, including children (via their guardians), irrespective of its characteristics or behavior.

We argue, as others did before us, that the amount given every year increases and decreases in a way that mimics random assignment in an experiment (Hsieh, 2003; Kueng, 2018). That is, for over three decades a repeated quasi-natural experiment has occurred involving the hundreds of thousands of people living in Alaska.

The amount of the transfer varies markedly: It ranges from a low of $331 (1984; $626 in 2010 dollars) to a high of $3,269 (2008, including a $1,200 bonus; $3,366 in 2010 dollars) per resident with a mean of $1,547 (SD =$586), as can be seen in Figure 1. To put this into context, the value of the cash transfer for a family of four ranges from the equivalent of 70 percent of the value of food stamps to three times the value of food stamps. For each household, it typically exceeds the value of the federal EITC (Crandall-Hollick, 2018). This variation in "treatment dosages," which we argue is random with respect to individual Alaskans, is analytically useful for identifying the treatment effect.

Unlike other American cash transfers – or near-cash transfers like food stamps – the dividend is given to every resident. Any individual who has been resident in Alaska for the prior 12 months or who is born in Alaska in the prior 12 months is eligible, with rare exceptions.[3] Minors' dividends are paid to one parent or legal guardian. There are no low-income requirements as with welfare (Currie and Cole, 1993; Moffitt, 1998), food stamps (Almond et al., 2011) or the EITC (Baughman and Dickert-Conlin, 2009; Strully et al., 2010; Hoynes et al., 2015). It is not only available to working people like the EITC is, or only to pregnant people like Women, Infants and Children (WIC). It does not phase out, even at high levels of income like the tax provision of personal exemption does (Whittington et al., 1990; Whittington, 1992). Given this, and Alaska's similarity to the nation as a whole (detailed below), our case provides the best opportunity available to study the effects of an unconditional cash transfer for the entire country.

There is an extensive application for first-time applicants that requires proof of residency, among other verifications. Subsequent annual applications are trivial. Participation rates are high, above 92% in many years and often above 97% (Division, 2000). The universality of the dividend means there are few worries about confounders or selection on the basis of personal characteristics demographers use when modeling fertility decisions. We nonetheless control for numerous characteristics, as we discuss below.

Applications are due in March. News reports estimate the dividend amount in the spring with marked accuracy (Kueng, 2018). The official amount is announced in September, and the payment is made as a lump sum in October. In the early years of the dividend, the payment was made via check; in 1993, direct deposit became available.

[3] People sentenced or incarcerated for a felony during the year are excluded as well as people with extensive criminal records.

1.4.1 The Case for Exogeneity

Our causal claims rest on three features. First, the amount of the dividend is unrelated to individual Alaskan residents' behavior (i.e., it is truly exogenous). Second, the amount varies year to year. Third, there is no factor correlated with fertility that is also correlated with the dividend, which would otherwise violate the exclusion restriction.

The fund was established in 1976 via an amendment to the state's constitution. Court battles delayed implementation of the dividend disbursement until 1982. The initial endowment was from mineral royalties and leases of Alaskan public lands. The Alaska Permanent Fund Corporation, a quasi-independent state agency, invested the endowment in broadly diversified financial and real assets. The revenue for the transfer comes from smoothed five-year returns from the investment fund.

Importantly though, not all the fund's returns are distributed; most of the returns are re-invested. Therefore, while the principal continues to be fortified by mineral royalties and leases, proceeds from the minerals have substantially declined as a portion of the fund's total market value. The fund's revenue from mineral royalties and leases represents less than .06% of the total market value today. State mineral revenue represents only 2% of annual fund additions. Since 1985, investment returns are the primary growth mechanism. Therefore, this assuages concerns that the dividend amount is reflective of local Alaskan economic conditions. Further, oil price shocks that might affect the non-PFD portion of Alaskans' income and the local economy generally do not affect the dividend amount.

Potentially, the dividend could attract people to migrate to Alaska or to compel people to stay who might otherwise have left. Then we would be concerned that selection into receiving the dividend was not random but due to the dividend itself. This would affect our results if those who were living in Alaska due to the dividend were more or less sensitive to the dividend with regard to their fertility than those who were living in Alaska for other reasons. Net migration to Alaska, however, is small. Across our study period, on average, net migration represents one tenth of one percent of the state's population. At its peak, net migration added five percent of the total population (1982-1983), and at its trough it reduced the population by four percent (1986). In addition, the rate of in and out-migration has slowed over time (Alaska Department of Labor, 2020) despite increased awareness of the dividend across the nation. This assuages concerns about migration's threat to our causal inference.

1.4.2 The Exclusion Restriction

If there is a factor that affects fertility that is correlated with the dividend amount, then we might misidentify the dividend as the source of that effect. Two possible confounders are Alaskan economic conditions and public health investments. As discussed above, the revenue for the dividend is not directly derived from state mineral extraction revenue, therefore the local economy is not related to the dividend amount in a way that would threaten our causal estimates. Nonetheless, we include the annual Alaskan unemployment rate in all models and separately include per capita income and the price of oil in sensitivity analyses to account for local economic conditions, and our findings are robust (see Appendix Table 5).

If when the dividend payments were higher there were also public health investments in Alaska with a focus on maternal and child health then we would be unable to parse the effects of the cash transfer from the investment. To assess this possibility, we examined historic public health expenditure reports and public health histories and interviewed five Alaskan public health officials. The public health system changed over our study time period by expanding access to maternal and newborn healthcare for rural Alaskans, though this was a gradual change and did not fluctuate like the dividend payments (Borland et al., 2015; Nord, 1995). To attend to this, we conduct a sensitivity analysis of Anchorage residents alone where there was no meaningful new investment. The results for Anchorage are substantively similar for the state as a whole (Appendix Table 6), which allays concerns that public health funding rather than the PFD income supplement is driving our results.

1.4.3 Generalizability

Despite popular assumptions, the Alaskan population resembles that of the United States as a whole. This is in large part due to the city of Anchorage, where over half of Alaskans live. Table 1 compares the Alaskan population and U.S. population over our study period on key demographic factors. There are some notable differences: In both Alaska and the United States, similar proportions of the population are non-Hispanic white. In Alaska, however, the non-white population is composed of markedly more Alaska Natives and fewer black Americans and Hispanic Americans than the country as a whole. It also has a larger proportion of rural residents. Parts of rural Alaska are markedly more remote than rural areas in the other 49 states. Our sensitivity analysis of Anchorage alone attends to this.

1.4.4 Universal Basic Income

While specific proposals vary, the primary features of a fully realized UBI are: (1) that a large portion of a population or an entire population receives regular cash payments; (2) the payments are not targeted to a particular subset of the population (e.g. pregnant people or the elderly), nor is it conditional on income; (3) that the benefit is large enough to provide for basic needs.

The PFD is the closest example of a UBI policy in the United States. Many argue that it is the closest example to a UBI policy worldwide as it is universally given to Alaskan residents (Hoynes and Rothstein, 2019). But it also differs in in two important ways. First, the size of the transfer varies year-to-year. While this is convenient for analytic purposes, the canonical UBI would not vary except perhaps to increase according to the cost of living. Secondly, though the transfer is often large compared to other benefits such as SNAP or the EITC, it falls far short of the benefits many UBI proponents advocate (but see also Van Parijs and Vanderborght 2017; Banerjee et al. 2019). For example, 2020 Democratic Party presidential candidate Andrew Yang has proposed a $1000 a month UBI.

1.5 Strengths of the Case

In summary, the PFD provides a stronger case than most to analyze the effects of income on fertility and the impact of a UBI. First, the payments are exogenous. Second, the payments are universally given to Alaskan residents, which provides an unusually large and diverse population for a natural experiment and one which we can precisely identify in the data. Given that it is universally given, there are few worries about confounders or selection into receiving the dividend on the basis of the personal and social characteristics used when modeling fertility decisions. Third, there are three sources of variation we can exploit in our analyses: the marked annual variation in the cash transfer, the variation within a year given to households depending on their size and the variation in whether people receive the dividend while pregnant. Fourth, the data are high quality, and we can draw upon numerous data sources for our main and sensitivity analyses.

2 Methods

We model the cash transfer's effect on short-term birth rates. We do this by first assessing the effect of the dividend on the overall fertility rate and then by assessing heterogeneous treatment effects across demographic groups of people giving birth.

Given that pregnancies are forty weeks, a fertility-response timeframe from transfer to birth of one year, which is typical in the literature, is insufficient. This window will only capture people who respond immediately to the income transfer, conceive quickly and do not miscarry. While this is an appropriate window for exceptional people, on average it takes longer than two months to conceive (Gnoth et al., 2003; Wesselink et al., 2017). Further, as a conservative estimate, 15 percent of recognized pregnancies end in miscarriage (Rai and Regan, 2006), and at least a third and likely more of conceptions do not end in a live birth (Wilcox et al., 1988; Boklage, 1990), thus lengthening the time to a live birth. We extend the fertility-response timeframe to 24 months prior to birth to account for variation in the speed of decision-making and conception and rates of miscarriage. Though we believe 24 months to be the appropriate window given research on time-to-live-birth, we also empirically assess longer windows, as we discuss below. The fertility-response window may continue through early pregnancy, depending on access to abortion. We empirically assess this in our supplementary analyses.

Fertility Response: Birth Rate and Composition of Who Gives Birth

We first test the effect of the dividend payments on fertility by assessing their impact on Alaskan short-term birth rates. This will test hypotheses 1 and 2.

We analyze groups defined by specific demographic characteristics: age, marital status, educational attainment, racial identity, and parity. We refer to these demographic-specific groups as Demographic Groupings (DG). As an example, Alaska Native women who are married, have a high school degree or less, have one previous child and are between the ages of 25 and 29 are grouped together.

The birth-rate model is a log-rate model that considers how the cash transfer affects birth rates one year and two years later:

$$\log\, \mu_{jt} = \log\, E_{jt} + \beta_1 DIV_{j(t-1)} + \beta_2 DIV_{j(t-2)} + \beta_3 \mathbf{X}_{jt} + \beta_4 UNEMP_{t-2} + \beta_5 US_t + \beta_6 YEAR_t + \epsilon_{jt}$$

where j indicates Demographic Groupings and t indicates year. μ_{jt} is the count of births; log E_{jt} is an exposure term, or offset, and is the population of women in each Demographic Grouping at t; $DIV_{j(t-1)}$ and $DIV_{j(t-2)}$ are the cash transfer to each household in that year; vector $\mathbf{X}_{jt}$ indicates a set of controls for individual characteristics by Demographic Grouping; $UNEMP_{t-2}$ indicates a control for the Alaskan unemployment rate at *t-2*; US_t is the U.S. birth rate in year t; and $YEAR_t$ is the year. We include dividend payments from both *t-1* and *t-2* to provide a 24-month window in which cash transfers can affect birth rates in year t. Below we discuss analyses testing longer windows. Though dividend payments began in 1982, the birth rate model includes years 1984-2010 to account for the 24-month window.

Following estimation of the main rate model, we test for heterogeneous treatment effects across demographic subgroups. We accomplish this by estimating the main rate model with an interaction between the demographic characteristic and $DIV_{j(t-1)}$ and $DIV_{j(t-2)}$ (i.e., two two-way interactions) added.

2.0.1 Sensitivity Analyses for the Fertility Effects

We perform four sets of sensitivity analyses to test the robustness of our fertility findings.

First, to test the robustness of our results, we also estimate the fertility models with the per-person transfer amount rather than the total household transfer.

Second, we also perform a placebo test for our birth rate analysis by testing the effect of the dividend payments on birth rates in South Dakota. South Dakota is demographically similar to Alaska, especially with regard to the proportion of Native American residents. See Table 7 in the Appendix for a demographic comparison. South Dakota birth rate data was created using a similar procedure as Alaska, described in the next section. The South Dakota birth rate model is identical to the Alaska birth rate model. Given that no resident of South Dakota received the dividend, we expect that there will be no relationship between the size of the dividend and fertility in South Dakota. If there is a relationship, it would suggest that the dividend is a proxy for something else such as unaccounted-for macro-economic conditions that affect fertility.

Third, we perform the birth rate analyses for Anchorage alone. Obtaining substantively similar results for this subpopulation would allay two concerns: first, regarding the exclusion restriction discussed above and second, regarding generalizability because much of the rural population in Alaska is especially remote.

Finally, because the cash transfer occurs every year, it is possible that after an initial period of adjustment, individuals come to expect the dividend and anticipate it each year, removing its effect as an income "shock." The Alaskan dividend's variation over time allows us to assess whether such a normalization occurs by measuring jumps or dips in the dividend that can be thought of as unanticipated. These analyses, described further in the Appendix, do not suggest normalization.

3 Data

3.1 Birth Counts: Natality Data

All analyses use restricted natality data provided by the National Vital Statistics System for 1984 to 2010. These data contain the complete population of U.S. births and include a wealth of information on people who give birth, including their demographic characteristics. For most of our analyses, we use only births to people residing in Alaska. Characteristics of individuals who give birth included in our analyses are age, racial identity, marital status, education and parity. Following convention, we group age into five year age-groups and restrict to ages 15 to 44. Given the racial composition of Alaska, we group racial identity into white, Alaska Native and other. Marital status is dichotomized as married and unmarried. We group educational attainment into less than high school, high school, some college and college degree or greater. Parity is coded as first birth, second, birth, third birth, and fourth or above birth.

In all analyses, we use the dividend amount given to each household. We identify household size through the marital status and parity variables in the natality data. An alternative specification would be to include only the annual per-person dividend. We argue that the total household payment is a better measure for a number of reasons. First, it represents the total amount of money that each family receives. To model, for instance, a household of four as receiving the same amount of money as a household of one would be to dramatically misrepresent the cash transfer. Second, we can account for the fact that with each additional family member comes additional expenses by including parity and marital status in our analyses. This adjusts for family size but further acknowledges that a spouse's net expenses are quite distinct from a child's. Though our main analyses all use the total household dividend amount, we also present sensitivity analyses using the per-person dividend amount. All dividend measures are converted to 2010 dollars.

We align all data sources to years based on PFD payment distribution. Because payment occurs in October of each year, PFD-aligned years begin in October and end in the following September. For instance, a birth occurring in March 2000 was coded as PFD year 1999 because it falls in the twelve months following the distribution of the 1999 dividend payment. All references to years below refer to PFD-aligned years.

All analyses also account for macro-level Alaskan economic trends through inclusion of the Alaskan unemployment rate as a control. Sensitivity analyses also include per capita income and the crude price of oil. These measures are also aligned to PFD distribution. Because income per capita is only reported annually, we aligned by assigning one-fourth of the annual value of the measure in year t to PFD year t and three-fourths of the value of the measure in year $t+1$ to PFD year t.

3.1.1 Missing Data

In total, 4 percent of births had missing values on one or more covariate, though this ranged from 0.93 percent of births in 1996 to 15.3 percent of births in 2003. To address missing data, we employed a threshold deletion strategy. Specifically, we chose a threshold of 7.5 percent and excluded from our analysis any year in which more than 7.5 percent of births had missing values on one or more covariates. Based on this threshold, we excluded 2001, 2003, and 2008 from our analyses. For all other years, we dropped all cases with missing values on covariates and then randomly dropped more observations until the percentage dropped reached 7.5 percent. That is, for all included years, exactly 7.5 percent of cases were dropped. We use this approach to ensure that the total number of births per year is not impacted by different rates of missing data across years. Because the rate model assesses changes in the number of births per year relative to the total number of women at risk of giving birth, we must be attentive to any data manipulations that alter the birth counts in some years and not others, as such changes could induce an artificial effect on birth rates. The threshold-deletion strategy ensures that birth counts for each year are artificially reduced by the same proportional amount. As a sensitivity analysis, we also tested alternative thresholds – 6 percent and 9 percent – and our results were not substantively altered.

3.2 Population Counts

In order to assess birth rates, we require denominators that are population counts. The denominators for the birth rates are population estimates for women aged 15-44 by Demographic Grouping that were obtained in two steps. First, proportions of the population by Demographic Grouping were linearly interpolated from 1980, 1990, and 2000 Census five-percent samples and the 2008-2012 American Community Survey (ACS) sample. Second, these proportions were multiplied by intercensal population counts for women ages 15-44 by five-year age groups provided by the Alaska Department of Labor and Workforce Development (Alaska Department of Labor, 2014a,b). These population counts incorporate information from the applications to the annual Permanent Fund Dividend. Given these unique, annual data from Alaska, the denominators are more accurate than those typically used in rate analyses that rely merely on intercensal interpolation.

For our placebo test, we also calculate birth rate denominators by Demographic Grouping for South Dakota. These counts are obtained from linear interpolation of Demographic-Grouping-specific counts of women from the 1980-2000 Censuses and 2008-2012 ACS.

Finally, we calculate the annual U.S.-level birth rate as a control. This is obtained by using annual counts of all births in the United States from the natality data as the numerator and linearly interpolated counts of women age 15-44 derived from the 1980-2000 Censuses and 2008-2012 ACS as the denominator. In the place of a U.S.-level control we also test controls for demographically-similar states such as Utah and South Dakota, and the results were substantively similar but produced worse model fit (analyses available upon request).

We provide information on the data and methods used to explore potential mechanisms underlying the relationship between income and fertility below and in the appendix.

4 Results

We first examine the impact of the dividend on the overall birth rate using a log-rate model. Table 2 presents the coefficients for the dividend payments at *t-1* and *t-2*. Increased income results in more births one and two years after disbursement (DIV_{t-1} IRR = 1.018; DIV_{t-2} IRR = 1.019; dividend units in thousand dollars). These results support Hypothesis 1. This model predicts that for women with a household size of 1 (i.e., unmarried women with no previous children), two consecutive average dividend payments for this household size of $1,519 at years *t-1* and *t-2* relative to two minimum payments of $626 would result in 1.93 more births per 1,000 women with a household size of 1 in year *t*. For women with a household size of three, the model predicts that two consecutive average payments of $4,522 relative to two minimum payments of

$1,878 would result in 12.97 more births per 1,000 women with a household size of 3 in year t. Appendix Table 8 presents the complete regression results for the rate model, but we note here that the coefficients for all covariates have signs consistent with established literature. For instance, being married has a positive effect on birth rates, while having a bachelor's degree is associated with a lower birth rate relative to having no high school diploma.

The main model, Model 1, includes year as a covariate but still allows the model to compare years across the entire study period. In order to ensure that our results are not solely driven by comparing temporally distant years to one other, we add decade fixed effects in Model 2 and obtain very similar results. [4]

In Model 3, we re-estimate Model 1 using the per-person dividend amount rather than the household-adjusted amount. With this measure we see once again that increased income leads to larger birth rates; for the per-person dividend measure, the effect size is larger in magnitude: DIV_{t-1} IRR = 1.037; DIV_{t-2} IRR = 1.044. This larger effect size is expected given that the scale for the individual-level dividend is smaller than the household-adjusted scale.

Though we theorize that a 24 month pre-birth window is the appropriate amount of time for assessing the effect of income on fertility, we also empirically assess this by testing additional time frames. Only payments one and two years prior to birth significantly affect fertility (see Appendix 9 for full results). In addition, we test other macro-economic indicators other than unemployment in Appendix 5, and our results are unchanged. Further, results for Anchorage alone are confirmatory and show similar positive effects on fertility (see Table 6 in the Appendix).

In Table 3, we extend our analyses from Table 2 and also present results from our placebo tests. Models 4 and 5 present the results of the birth rate model for South Dakota using the household-adjusted and per-person dividend amounts, respectively. Both models indicate the there is no effect of the dividend on birth rates in South Dakota. These placebo results reduce concern that the dividend payments significantly affect the Alaskan birth rate because they are a proxy for broader economic conditions, including for the global stock market.

Since the dividend payments increase birth rates, we next explore whether this increase occurs heterogeneously across demographic groups. Figure 2 presents the results of the models that interact each maternal characteristic - marital status, racial identity, education, age, and parity - with the dividend payments by displaying the predicted change in the birth rate if the dividend payments at *t-1* and *t-2* were at the average household value ($4,297) compared to the minimum value ($626), a difference in income of $3,671. The model results indicate heterogeneous effects across multiple demographic characteristics. Across marital sta-

[4]In an alternate test of whether the effects we identify are driven by a few years, we re-estimated Model 1 with all possible three-consecutive year periods dropped. All of these models produced substantively similar results. We thank Stephen Morgan for the suggestion.

tuses, the payments have opposite effects on birth rates. While additional cash increases the birth rate for unmarried women, the effect for married women is negative. Despite these opposite effects, married women overall still have higher birth rates throughout the study period. For women of all racial identities, the dividend has a positive effect on birth rates, but the effect is largest for Alaska Native women. The difference of the effect between white and other race women is not significantly different from zero. Assessing education, we find that cash transfers increase the birth rate among all educational groups, but the largest effect is for women who did not complete high school. Across age groups, we find significant positive effects for women 25 and older. The effects for women between 15 and 24 are not statistically significant and may be zero. For the age groups for which there is a significant effect, we see larger effects for women that are 25-29 and 30-34 than for women who are 35-39 and 40-44. Finally, when assessing effects across parity levels, we find that there is a large effect on the birth rate for first births and a smaller but still positive effect for second births. There is no effect for third births and a very small positive effect for fourth plus births.

We hypothesized that the income would increase fertility (H1), and it did. We hypothesized that there would be heterogeneous effects; that is, some women would be more responsive to the size of the payment than others with regard to childbearing (H2). We found this also to be true. We expected that socioeconomically disadvantaged women would be less responsive than advantaged women (H3). This hypothesis was unsupported. Contrary to our hypothesis and literature showing that childbearing of socioeconomically disadvantaged women is largely unconnected to economic conditions, we find that disadvantaged women increased their fertility after a larger dividend payment moreso than their advantaged peers. This confirms, over a longer timeframe, recent findings regarding the fertility response to the Great Recession.

4.1 Exploratory Examination of Factors Shaping Income's Effects on Fertility

4.1.1 How Might Income Affect Fertility?

Our primary contribution has been a new test of whether there is a relationship between income and fertility, particularly for socioeconomically disadvantaged women. Our results raise additional questions about possible mechanisms that could underlie the relationship between income and fertility. Prior literature identifies four potential mechanisms: seeking intended childbearing, avoiding unintended childbearing, sexual frequency and a change fecundity due to an altered in utero environment (Davis and Blake, 1956; Bongaarts, 1978). We discuss each briefly here.

The first possibility is the suggestion that additional income may lead some people to intentionally seek pregnancy and childbearing. For instance, the additional income could lead to a greater feeling of financial security, which could induce some people to decide to try to become pregnant. If additional income leads to an increase in the number of people intentionally seeking pregnancy, then, all other things equal, we would see an increase in fertility following larger dividends and an increase in the proportion of births that are the result of intended pregnancies. There is a lengthy literature that critiques the measurement of pregnancy intentions and even the notion that people's feelings toward their pregnancies can be captured on a continuum from intended to not intended (Ryder, 1973; Bachrach and Newcomer, 1999; Aiken et al., 2016). Some people, however, do seek pregnancy, and income may enable more people to do so.

Second, if there are economic barriers to accessing abortion and contraception, then additional cash may diminish or even remove those barriers, enabling effective avoidance of childbearing (Birgisson et al., 2015; Lindo and Packham, 2017). All other things equal, this would result in an increased abortion and/or contraceptive use rate, decreased fertility and an increase in the proportion of births that are due to intended pregnancies. Though we find an overall positive effect of additional income on fertility, we assess this contraceptive mechanism because it is possible that multiple competing mechanisms are occurring at once.

Third, additional money may change how often people have sex. For example, when people leave relationships, sexual frequency may decrease, which could lower fertility. When additional income facilitates people staying in relationships, then this may result in increased sexual frequency, which, all other things equal, could result in increased fertility. Prior literature on this question offers mixed results (Hannan et al., 1977; Hannan and Tuma, 1990; Cain and Wissoker, 1990b,a; Harknett and Gennetian, 2003; Gassman-Pines and Yoshikawa, 2006; Riccio et al., 2013; Cancian and Meyer, 2014).

Finally, if additional income alters the in utero environment, then this can either promote or discourage conception and fetal survival. For instance, additional income could reduce stress, which would increase the likelihood of conception and fetal survival regardless of pregnancy intention. All other things equal, this would result in an increase in fertility and no change in the proportion of births that are due to intended pregnancies. If the additional income is used to purchase and consume products like tobacco or alcohol then, all other things equal, this would reduce the likelihood of conception and fetal survival. We would then see a decrease in the fertility rate and the proportion of births to intended pregnancies would remain unchanged.

A change in the in utero environment could also be evident in the sex ratio at birth (the secondary sex ratio). A negative shock to the population as a whole, such as an earthquake, or a negative shock to a woman such as marital stress, results in a decrease in the proportion of male babies due to selective miscarriage of male fetuses (Catalano and Bruckner, 2005; Torche and Kleinhaus, 2011; Hamoudi and Nobles, 2014; Casey et al., 2019). If a positive shock is symmetrical, then we would anticipate an increase in the proportion of male babies due to increased survival of male fetuses. We, however, hypothesize that the positive shock will affect different populations of women differently. Given that those populations of women have different sex ratios at birth (Almond and Edlund, 2007), it is unclear in which direction the sex ratio will change.

Socioeconomically disadvantaged women increase their fertility to a greater extent as a result of the cash transfer so perhaps we will see a decrease in the proportion of males because the disadvantaged women tend to have fewer males. To acknowledge this ambiguity, we merely expect that the sex ratio will change depending on the size of the cash transfer, particularly for the populations whose fertility rate is more responsive to the size of the transfer.

Each of these mechanisms - pregnancy intention, contraception and abortion use, sexual frequency, and the in utero environment - are difficult to measure, and, as a result, there are limited data sources on them. In fact, to our knowledge there is no data on sexual frequency for our study population that would be useful for assessing it as a mechanism. For the other three mechanisms, we have marshalled several data sources to use the best data available to assess whether these are driving the effect of income on fertility and to assess potential countervailing mechanisms. These analyses should be regarded as exploratory and as a best attempt to capture hard-to-assess mechanisms to further provide insight into our primary findings.

4.1.2 Supplementary Measures Regarding Possible Mechanisms

Here we briefly outline the data sources and measures used to assess possible mechanisms. Full data descriptions including data limitations can be found in Appendix section "Data Sources for Exploratory Mechanism Analyses."

Pregnancy intentions: We use a survey of mothers who recently gave birth to live infants, Pregnancy Risk Assessment Monitoring System (PRAMS), that includes three questions on pregnancy intendedness.

Contraceptive and abortion access: To assess contraceptive access, we use a PRAMS question regarding reasons for not using contraception at the time of conception. We also confirm these results with data from the Behavioral Risk Factor Surveillance System. We examine abortion rate data from the Centers for Disease Control.

Sex ratio: We use the natality data to measure the annual secondary sex ratio (the ratio of male infants to female infants) at birth. We construct the sex ratio for the total population of newborns as well as by subgroups identified in the fertility analysis to be most affected by the cash transfer: Women who have less than a high school diploma, are Alaska Native, are unmarried, or are having their first child.

4.1.3 Supplementary Results Regarding Possible Mechanisms

Table 4 provides a summary of the factors we consider, their potential effects on fertility, the data and measures available to assess them, and the results.

Pregnancy Intentions: There is no relationship between the size of the cash transfer and three measures of pregnancy intention. This lack of relationship remains even when we consider the sub-groups of women whose fertility was most sensitive to the size of the dividend. This suggests that the mechanism through which additional income increased fertility was not through more people seeking to get pregnant and give birth.

Abortion and Contraception: Further, there is no relationship between the size of the cash transfer and access to contraception or abortion. This is not a surprise given that the fertility rate is positively affected by the income and pregnancy intentions are not. It is further unsurprising because in Alaska abortion has been covered by Medicaid during the entirety of the timeframe of this study. In the three years BRFSS asked about preventing pregnancy (1998, 2002 and 2004), no Alaskan respondents indicated an economic barrier to accessing contraception, though it was endorsed by people in other states. It appears that there is no economic constraint to preventing childbearing in Alaska.

Sexual Frequency: We have no data by which to assess sexual frequency and therefore cannot empirically assess whether it is a mechanism by which the cash payments increase fertility. Thus, we cannot rule out the possibility that fertility increased due to increased sexual frequency alone or in combination with other mechanisms.

In Utero Environment: To complete our assessment of mechanisms, we turn our attention to in-utero selection. If the increased income reduced stress or increased nutrition, then we would see an increase in fecundity orthogonal to pregnancy intention. That is, people who were seeking pregnancy would find it easier to conceive and carry to term as would people who were not seeking pregnancy. We have no reliable data on time of trying to conceive to live birth, conception rates or on miscarriage rates. Thus, we need to triangulate between two sets of results: pregnancy intentions and the sex ratio at birth. Assessing both sets of results together, the intentions results and the sex ratio provides some suggestive evidence of an improved in utero environment. The fertility rate increased without a change in the pregnancy intentions of those pregnancies that ended in live births. The results on the sex ratio at birth also provide insignificant but possible suggestive evidence of an improved in utero environment (See Figure 3 in Appendix).

5 Discussion and Conclusion

While the idea of a Universal Basic Income has gained significant attention of late around the globe, evidence regarding its potential effects on a number of important outcomes is surprisingly sparse. This is particularly the case in the developed world. We contribute to the scholarly effort to understand the potential effects of a massive income transfer by examining a uniquely strong quasi-natural experiment, the Alaska PFD income transfers. We investigated how this annual exogenous income shock affects childbearing, an undertaking desired by most Americans and a critical site of inequality in America.

We analyzed Alaska because it comes closer to proposed UBIs than any other effort in the U.S.. Given its large population of over 700,000 people and the duration of the program, it provides an empirical case far larger and more extensive than any randomized experiment realistically could. Larger payments cause an increase in the birth rate shortly thereafter, confirming Hypothesis 1. This effect is robust to numerous controls for maternal characteristics, time trends, secular trends in the birth rate and controls for the macroeconomy. Some demographic groups have greater sensitivity to the additional income, confirming Hypothesis 2. Contrary to prior scholarship in family demography (Edin et al., 2004; Edin and Reed, 2005; Gibson-Davis, 2009), socioeconomically disadvantaged women in Alaska are the most sensitive to the size of the transfer, increasing their birth rates more than more advantaged groups. This did not support Hypothesis 3. Importantly, the proportion of births due to unintended pregnancies remains the same, suggesting, along with other evidence from the mechanisms analyses, that the dividend increases fecundity across the population. We interpret these results as indicators that some socio-economically disadvantaged people faced an economic barrier to having children prior to the payments. Through whatever mechanism, a larger income transfer shrinks that barrier and therefore reduces a structural impediment to enacting a deeply personal and socially consequential endeavor: childbearing.

According to the reproductive justice framework, an economic barrier to having children constitutes a violation of the right to have a child (Luna and Luker, 2013; Ross and Solinger, 2017). The pioneers of the reproductive justice framework, mostly American women of color, draw upon their communities' histories of forced sterilization, environmental degradation of their reproductive health and fecundity, child removal and policies aimed at preventing poor people from bearing children to assert a right to childbearing that is often eroded (Luna, 2009; Ross and Solinger, 2017). Similar to the canonical theory of fundamental causes of health inequality (Link and Phelan, 1995), reproductive justice scholars ask us to consider not the proximate causes of any health event - e.g., lack of medication or poor nutrition - but the more distal and upstream causes. However, while fundamental cause theory focuses on that which we want to avoid - morbidity and mortality - reproductive justice scholars expand our understanding of health to include that which we may want to seek: healthy, wanted pregnancies and the right to parent wanted children in healthy environments. The PFD cash payments help enable Alaskans to exercise that right.

Though we contend that the case of Alaska PFD payments provide an excellent case for causal analysis, we note some limitations of our work. While Alaska is surprisingly similar to the U.S. population demographically, it is different in some important ways that raise concerns about generalizability. The population of people of color is composed more of indigenous peoples and less by other racialized groups than many other states. Some of Alaska's rural population is far more remote than other rural populations in America. Anchorage, however, is more similar to other American cities than the rural areas in Alaska are to other

American rural areas, and our sensitivity analyses including only Anchorage produce substantively similar results.

In addition, Alaska is somewhat unusual in that Medicaid pays for abortions for the poor. A relatively large proportion of abortions are paid for by Medicaid (42% in 1990; 46% in 2010) (for a summary, see New 2015). We contend that this is likely why there is no relationship between the dividend and abortion rates in Alaska. Most states (34), however, do not fund abortions through state Medicaid. [5] In these places, we anticipate that abortion will be a more important pathway by which income affects newborn health than we found in Alaska.

The natality data provide incredible detail regarding people, their pregnancies and their newborns and are the best data available to assess our research question. Nonetheless, they are imperfect. They cannot link people across births. The data report parity but not birth intervals or other personal histories that may matter for fertility. The natality data also do not report on cohabitation, which has become an increasingly common family structure during this time period (Manning, 2013). Absent information on parental income, we need to rely on education to have a sense of household material well-being.

The natality data also, obviously, lack data on pregnancies that do not end in a live birth and on people who were never pregnant. This affects our main analyses in that we need to look elsewhere for population counts. This problem is not unique to our research, but Alaska provides a unique solution. Rather that merely relying exclusively upon intercensal interpolation, we can utilize Alaska's intercensal population counts that are significantly more accurate than those derived from intercensal interpolation. It also affects our analyses of potential mechanisms in that we do not have data on miscarriages and only state-level data on abortion.

Despite these limitations, the Alaskan case provides the best opportunity we have to study the effects of universal income transfers in the American population. Having found that an increased dividend was followed by an increase in fertility, we conducted supplementary analyses to explore potential mechanisms undergirding that relationship. We marshalled all available data, but this examination suffers from numerous data limitations mentioned above and in the Appendix. After examining all these potential mechanisms and taking into account the data limitations, we concluded that there is suggestive evidence that improved fecundity explains the positive relationship between income and fertility.

Alaskan households receive a substantial amount of money from the PFD annually; its value often exceeds major components of the American welfare system such as food stamps or the EITC. It is, nonetheless, smaller than the transfer for which many UBI proponents advocate. We can reasonably anticipate that with a larger income transfer, the fertility effects would be even greater than what we found here. In the coming years, as ongoing American experiments which involve larger transfers to smaller populations are analyzed, we will

[5] Federal Medicaid covers abortion only in rare exceptions.

be able to test this supposition. Based on this analysis of thirty years of transfers in Alaska, we conclude that additional income increases reproductive autonomy, particularly for disadvantaged groups, by reducing economic barriers to childbearing. Cash transfers can thus successfully reduce fertility inequalities in the United States.

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6 Tables and Figures

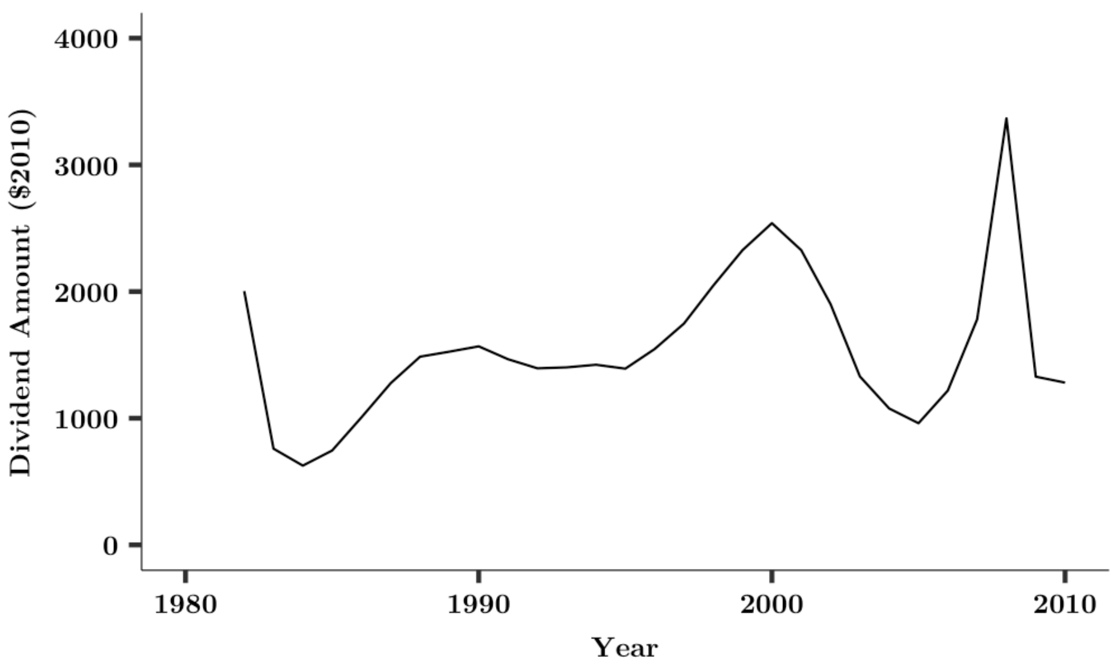

Figure 1: PFD dividend payments in $2010, 1982-2010

Table 1: Comparison of United States and Alaska Demographics: 1980-2010

	1980		1990		2000		2010	
Measure	U.S	Alaska	U.S.	Alaska	U.S.	Alaska	U.S.	Alaska
Educational attainment (%)[a]								
High School or higher	66.5	82.5	75.2	86.6	80.4	88.3	85.0	88.4
College Degree or higher	16.2	21.1	20.3	23.0	24.4	25.4	27.9	25.4
Median Age (years)[a]	30.0	26.0	32.9	29.4	35.3	32.4	37.2	33.8
Hispanic (%)[a]	6.4	2.4	9.0	3.2	12.5	4.1	16.3	5.5
Race (%)[a,b]								
White	83.2	77.1	80.3	75.5	75.1	69.3	72.4	66.7
Black	11.7	3.4	12.1	4.1	12.3	3.5	12.6	3.3
American Indian or Alaska Native	0.7	15.9	0.8	15.6	0.9	15.6	0.9	14.8
Asian or Pacific Islander	1.6	1.9	2.9	3.6	3.7	4.5	5.0	6.4
Other Race or Multiracial	3.0	1.6	3.9	1.2	7.9	7.0	9.1	8.9
Median Household Income (dollars)[c]	16,841	25,414	29,943	39,298	41,990	52,847	49,276	57,848
Poverty (%)[c,d]	12.0	10.7	10.0	9.0	11.5	7.9	15.3	9.9
Urban (%)[a,e]	73.7	64.4	80.0	67.5	79.0	65.6	80.7	66.0
Foreign-Born (%)[a,d]	6.2	4.0	8.0	4.5	11.1	5.9	12.9	7.0
Children Living with a Single Parent (%)[c]	19.7	19.3	24.7	20.0	26.7	19.7	26.6	21.7
Fertility Rate[f,g,h]	68.4	88.6	70.9	86.3	67.5	74.6	64.1	80.1
Pre-Term Births (%)[f,g]	8.9	7.6	10.6	8.8	11.64	10.0	12.0	9.7
Low Birth Weight Births (%)[f,g]	6.8	5.4	7.0	4.7	7.6	5.6	8.15	5.7

Notes:

[a] Source: 1980-2010 U.S. Census.

[b] In the 1980 and 1990 Censuses, individuals could report only one race. This changed in the 2000 Census, where individuals could report more than one race.

[c] Source: 1980-2010 Current Population Survey.

[d] Source: 2010 American Community Survey.

[e] The census definition of "urban" changed in 2000, from places of 2,500 or more to a density measure.

[f] Source: National Center for Health Statistics.

[g] Source: Alaska Health Analytics and Vital Records.

[h] Fertility rate is calculated as the number of births per 1,000 women aged 15-44.

Table 2: Birth Rate Results

	Household Payment				Individual Payment	
	Model 1: AK Rate		Model 2: AK Rate w/ Decade F.E.		Model 3: AK Rate	
	IRR	C.I.	IRR	C.I.	IRR	C.I.
DIV_{t-1}	1.018**	(1.008 , 1.029)	1.016**	(1.005 , 1.026)	1.037*	(1.002 , 1.072)
DIV_{t-2}	1.019***	(1.009 , 1.030)	1.025***	(1.014 , 1.035)	1.044**	(1.011 , 1.078)

Notes:

(1) Birth count source: U.S. Natality Detail File, 1984-2010.

(2) Population count sources: 1980-2000 Decennial Censuses and 2008-2012 American Community Survey.

(3) N = 11,696 Demographic Groupings; 240,285 births.

(4) IRR = Incidence Rate Ratios.

(5) *p<.05; ** p<.01; *** p<.001

(6) Dividend is in 2010 constant dollars. It is measured in $1,000 units.

(7) Unit of analysis is Demographic Groupings - demographic groups of women determined by age, race, marital status, educational attainment, and parity.

(8) Controls are by age, race, marital status, educational attainment, parity, year, the U.S. birth rate, and the AK unemployment rate lagged two years.

Table 3: Birth Rate Results: Placebo Tests

	Household Payment Model 4: South Dakota Rate		Individual Payment Model 5: South Dakota Rate	
	IRR	C.I.	IRR	C.I.
DIV_{t-1}	1.005	(.994 , 1.016)	1.003	(.968 , 1.039)
DIV_{t-2}	1.010	(.998 , 1.021)	1.014	(.976 , 1.053)

Notes:

(1) Birth count source: U.S. Natality Detail File, 1984-2010.

(2) Population count sources: 1980-2000 Decennial Censuses and 2008-2012 American Community Survey.

(3) N = 11,338 Demographic Groupings; 277,406 births.

(4) IRR = Incidence Rate Ratios.

(5) *p<.05; ** p<.01; *** p<.001

(6) Dividend is in 2010 constant dollars. It is measured in $1,000 units.

(7) Unit of analysis is Demographic Groupings - demographic groups of women determined by age, race, marital status, educational attainment, and parity.

(8) Controls are by age, race, marital status, educational attainment, parity, year, the U.S. birth rate, and the AK unemployment rate lagged two years.

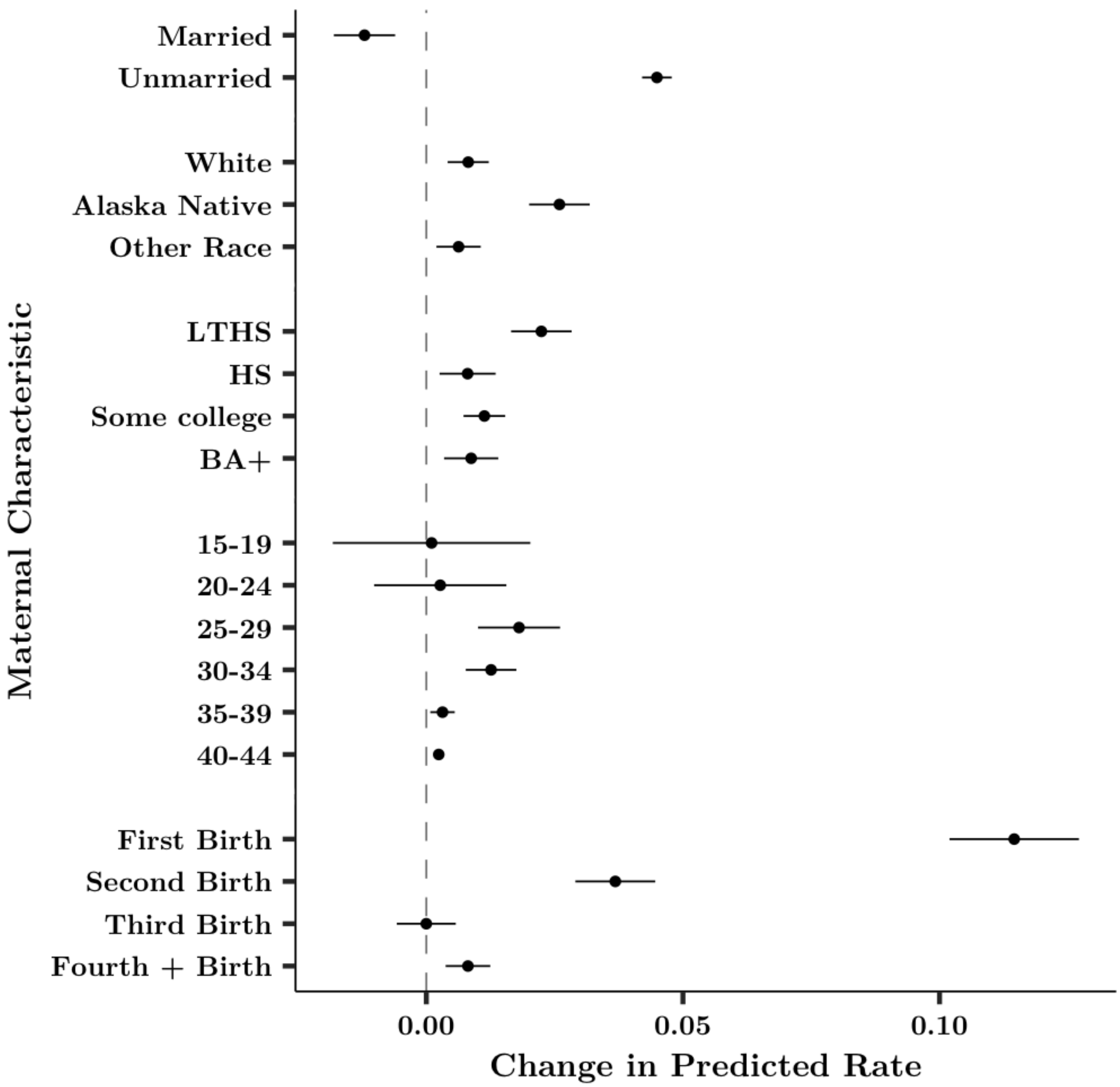

Figure 2: Change in Predicted Birth Rate among Women with Given Characteristic after Dividend Payment: Dividends at *t-1* and *t-2* at $626 versus $4,522 (N = 237,793).

Table 4: Mechanisms Linking Income and Fertility

Mechanism	Theorized Effect of Cash	Data	Measures	Results	Appendix
Mechanisms that could increase fertility					
Pregnancy Intention	Increase in pregnancy-seeking/intended pregnancies	PRAMS	Traditional intention measure, Trying to get pregnant, Preventing pregnancy	Three measures of pregnancy intention: Null	Appendix Table10
Mechanisms that could decrease fertility					
Contraception	Increase in contraception access that would: (1) Increase proportion of births due to intended pregnancies (2) Decrease proportion of births due to not contracepting because of restricted access	(1) & (2) PRAMS (2) BRFSS	(1) Three measures of pregnancy intention (2) Reason was not contracepting when got pregnant was because of access difficulties (2) Not contracepting because could not afford birth control	(1) Pregnancy intention: Null (2) Not contracepting reason: Null (2) No indication of financial barrier to contraception	Appendix Table 11
Abortion	Increase in abortion access that would: (1) Increase abortion rate (2) Increase proportion of births to intended pregnancies	(1) Abortion rate from CDC (2) PRAMS	(1) Abortion rate (2) Three measures of pregnancy intentions	(1) Abortion rate: Null (2) Pregnancy intention: Null	Appendix Table12
Mechanisms that could either increase or decrease fertility					
Sexual Frequency	(1) Increased frequency: increased fertility (2) Decreased frequency: decreased fertility	No data			
In Utero Selection	(1) Improved in utero environment: - Increase in fertility - Sex ratio changes - No change in pregnancy intentions (2) Worse in utero environment: - Decrease in fertility	(1) Natality (1) PRAMS	(1) Sex ratio at birth (1) Three measures of pregnancy intentions	(1) Sex ratio at birth: Suggestive evidence of change (1) Pregnancy intentions: Null	Appendix Figure3

7 Appendix

Table 5: Birth Rate Analysis with Macro-Economic Controls Added: Log-Rate Model Results

Dependent Variable	AK Unemp. Rate Included (Included in Main Text)	AK Income Included	Oil Price Included
DIV_{t-1}	1.018**	1.015**	1.016**
	(1.008 , 1.029)	(1.006 , 1.025)	(1.006 , 1.026)
DIV_{t-2}	1.019***	1.019***	1.019***
	(1.009 , 1.030)	(1.009 , 1.030)	(1.009 , 1.029)

Notes:

(1) Coefficients are incidence rate ratios. 95% C.I. in parentheses.

(2) Birth count source: U.S. Natality Detail File, 1984-2010.

(3) Population count sources: 1980-2000 Decennial Censuses and 2008-2012 American Community Survey.

(4) Total N = 11,696 Demographic Groupings; 240,285 births.

(5) *p<.05; ** p<.01; *** p<.001

(6) Controls are: Year (aligned to APF dividend disbursement), race, marital status, age, maternal education, parity, and U.S. birth rate.

(7) Dividend is in 2010 constant dollars and adjusted for household size. It is measured in n $1,000 units.

(8) Macro-economic measures are Alaska unemployment rate, Alaska income per capita, and the crude price of oil.

Table 6: Anchorage Birth Rate Results

	Household Payment				Individual Payment	
	Model 1: Rate		Model 2: Rate w/ Decade F.E.		Model 4: Rate	
	IRR	C.I.	IRR	C.I.	IRR	C.I.
DIV_{t-1}	1.015*	(1.001 , 1.030)	1.013+	(.999 , 1.027)	1.018	(.974 , 1.065)
DIV_{t-2}	1.023**	(1.009 , 1.037)	1.026***	(1.012 , 1.040)	1.056*	(1.012 , 1.102)

Notes:

(1) Birth count source: U.S. Natality Detail File, 1984-2010.

(2) Population count sources: 1980-2000 Decennial Censuses and 2008-2012 American Community Survey.

(3) AK N = 9,168 Demographic Groupings; 93,231 births.

(4) IRR = Incidence Rate Ratios.

(5) *p<.05; ** p<.01; *** p<.001

(6) Dividend is in 2010 constant dollars. It is measured in $1,000 units.

(7) Unit of analysis is Demographic Groupings - demographic groups of women determined by age, race, marital status, educational attainment, and parity.

(8) Controls are by age, race, marital status, educational attainment, parity, year, the U.S. birth rate, and the AK unemployment rate lagged two years.

Table 7: Comparison of Alaska and South Dakota Demographics: 1980 and 2010

	1980		2010	
Measure	Alaska	South Dakota	Alaska	South Dakota
Educational attainment (%)[a]				
High School or higher	82.5	67.9	88.4	89.3
College Degree or higher	21.1	14.0	25.4	25.3
Median Age (years)[a]	26.0	28.9	33.8	36.9
Hispanic (%)[a]	2.4	0.1	5.5	2.7
Race (%)[a,b]				
White	77.1	92.6	66.7	85.9
Black	3.4	0.3	3.3	1.3
American Indian or Alaska Native	15.9	6.6	14.8	8.8
Asian or Pacific Islander	1.9	0.3	6.4	0.9
Other Race or Multiracial	1.6	0.2	8.9	0.9
Median Household Income (dollars)[c]	25,414	13,156	57,848	45,352
Poverty (%)[c,d]	10.7	16.9	9.9	14.4
Urban (%)[a,e]	64.4	46.4	66.0	55.3
Foreign-Born (%)[a,d]	4.0	1.4	7.0	2.7
Children Living with a Single Parent (%)[c]	19.3	13.6	21.7	27.6
Fertility Rate[f,g,h]	90.5	88.3	80.1	77.3
Pre-Term Births (%)[f,g]	7.6	6.0	9.7	11.4
Low Birth Weight Births (%)[f,g]	5.4	5.1	5.7	6.8

Notes:

[a] Source: 1980 and 2010 U.S. Census.

[b] In the 1980 Census, individuals could report only one race. This changed from the 2000 Census onward, where individuals could report more than one race.

[c] Source: 1980 and 2010 Current Population Survey.

[d] Source: 2010 American Community Survey.

[e] The census definition of "urban" changed in 2000, from places of 2,500 or more to a density measure.

[f] Source: National Center for Health Statistics.

[g] Source: South Dakota Department of Health.

[h] Fertility rate is calculated as the number of births per 1,000 women aged 15-44.

Assessing acclimation to PFD payments

When a cash transfer occurs every year, it is possible that after an initial period of adjustment, individuals come to expect the dividend and anticipate it each year, removing its effect as an income "shock." The Alaskan dividend's variation over time allows us to assess whether such a normalization occurs by measuring jumps or dips in the dividend that can be thought of as unanticipated.

We performed a series of analyses assessing whether birth rates are more responsive to changes in the dividend amount than the absolute magnitude of the dividend itself. These analyses used two types of measures. First, we measured a given year's dividend amount as a deviation from prior years' average payment amounts. We created measures using multiple lags: one, three, and five years. Second, we regressed dividend payments on year for the previous three and five years and used the model results to predict the dividend amount in a given year. We then calculated the residual by subtracting the observed payment from the predicted payment.

We replicated our birth-rate analysis using the measures described above – deviations from averages and residuals from predictions – with one- and two-year lags to predict birth rates instead of the lagged dividend amounts used in the main analyses. Overall, these measures did not significantly predict the birth rate, suggesting that the actual magnitude of the dividend payment matters more than the portion of the payment that might be unanticipated. This provides evidence against the normalization hypothesis. Exceptions were models using a measure of deviation from the average of the prior three years and the residual of a model predicting payments for the prior five years: These measures showed positive and significant effects on birth rates, but their coefficients were smaller in magnitude than the coefficient of the actual dividend amount. The lack of evidence for adjustment or smoothing comports with the contemporary assessment of consumption responses to income changes (Jappelli and Pistaferri, 2010).

Table 8: Birth Rate Analysis: Log-Rate Model Results

Covariate	IRR	95% C.I.
DIV_{t-1} (Thous.)	1.018**	(1.008 , 1.029)
DIV_{t-2} (Thous.)	1.019***	(1.009 , 1.030)
Year	1.011***	(1.008 , 1.014)
Married	1.697***	(1.636 , 1.760)
Parity		
Parity 2	1.460***	(1.397 , 1.525)
Parity 3	1.032	(.976 , 1.092)
Parity 4+	1.222***	(1.137 , 1.313)
Race		
Alaska Native	1.622***	(1.564 , 1.682)
Other Race	.989	(.953 , 1.026)
Education		
High School	.892***	(.856 , .929)
Some College	.724***	(.693 , .756)
Bachelor's or more	.918**	(.873 , .965)
Age		
20-24	1.157***	(1.089 , 1.229)
25-29	.900***	(.847 , .956)
30-34	.552***	(.519 , .588)
35-39	.242***	(.227 , .258)
40-44	.053***	(.049 , .0566)
US. Birth Rate	1.038***	(1.030 , 1.046)
AK Unemployment	1.011	(.992 , 1.031)
Constant	.000***	(.000 , .000)

Notes:
(1) Birth count source: U.S. Natality Detail File, 1984-2010.
(2) Population count sources: 1980-2000 Decennial Censuses and 2008-2012 American Community Survey.
(3) Total N = 11,696 category IDs; 240,285 births.
(4) *p<.05; ** p<.01; *** p<.001
(5) Reference groups are: Parity 1, Non-Hispanic White, Less than High School, and Age 15-19.
(6) Dividend is in 2010 constant dollars and adjusted for household size. It is measured in $1,000 units.

Table 9: Birth Rate Analysis with Varying Dividend Time Lags: Log-Rate Model Results

Covariate	IRR	95% C.I.
Lags 0, 1, 2; N = 11,696 Demographic Groupings		
DIV_t	1.010	(.995 , 1.026)
DIV_{t-1}	1.014*	(1.003 , 1.026)
DIV_{t-2}	1.020***	(1.010 , 1.030)
Lags 1, 2; N = 11,696 Demographic Groupings		
DIV_{t-1}	1.018**	(1.008 , 1.029)
DIV_{t-2}	1.019***	(1.009 , 1.030)
Lags 1, 2, 3; N = 11,208 Demographic Groupings		
DIV_{t-1}	1.014*	(1.003 , 1.025)
DIV_{t-2}	1.025***	(1.012 , 1.038)
DIV_{t-3}	.997	(.983 , 1.011)

Notes:

(1) Birth count source: U.S. Natality Detail File, 1984-2010.

(2) Population count sources: 1980-2000 Decennial Censuses and 2008-2012 American Community Survey.

(3) Total N = 11,696 Demographic Groupings; 240,285 births.

(4) *p<.05; ** p<.01; *** p<.001

(5) Controls are: Year (aligned to APF dividend disbursement), race, marital status, age, maternal education, parity, AK unemployment rate lagged two years, and U.S. birth rate.

(6) Dividend is in 2010 constant dollars and adjusted for household size. It is measured in $1,000 units.

(7) If a dividend payment was not given during pregnancy, DIV_t is set to 0.

8 Data Sources for Exploratory Mechanism Analyses

8.1 PRAMS

To consider intended pregnancies as a potential mechanism linking income and fertility, we examine data from the 1990-2010 Pregnancy Risk Assessment Monitoring System (PRAMS) surveys. PRAMS surveys are conducted annually by the Centers for Disease Control (CDC) in collaboration with individual state health departments. In each participating state, a stratified sample of postpartum people is drawn from the state's birth certificate file, and questionnaires are filled out in paper; telephone follow-up is used to reduce non-response.

Alaska's PRAMS samples the birthing parent of approximately one in every six newborns each year. They oversample Alaska Natives and weighting adjusts to reflect the total population of Alaskans who gave birth in a given year.

To assess pregnancy intention or planning, we use three measures. The first is a standard pregnancy intention measure. "Thinking back to *just before* you got pregnant, how did you feel about becoming pregnant? Check the best answer." Response options were "I wanted to be pregnant sooner," "I wanted to be pregnant later," "I wanted to be pregnant then," "I didn't want to be pregnant then or at any time in the future." We characterize the "sooner" response and the "then" as intended pregnancies and the "later" or "never" as not intended.

Second, we examined whether the mother had been trying to get pregnant. The question was "When you got pregnant with your new baby, were you trying to become pregnant?" Yes or no were the response options. Lastly, we examined a question regarding pregnancy prevention: "When you got pregnant with your new baby, were you or your husband or partner doing anything to keep from getting pregnant? (Some things people do to keep from getting pregnant include not having sex at certain times [rhythm], and using birth control methods such as the pill, Norplant, shots [Depo-Provera], condoms, diaphragm, foam, IUD, having their tubes tied, or their partner having a vasectomy)." The response options were yes or no. Due to changes in the question wording, we use the trying and prevention questions from 2000-2010.

We also use the PRAMS data to examine access to contraception. Respondents who indicated that they were not doing anything to prevent pregnancy were asked a follow-up question: "What were your or your husband's or partner's reasons for not doing anything to keep from getting pregnant?" Respondents had numerous options from which to choose all that applied. We excluded from our analyses anyone who endorsed "I didn't mind if I got pregnant". With this sample, we examined the relationship between the dividend in *t-1* and the dividend in *t-2*'s relationship with the likelihood of endorsing as a reason for not preventing pregnancy that "I had problems getting birth control when I needed it." This response option is available from 2000-2010. Here we use a logistic regression predicting the likelihood of endorsing this reason with the dividend one and two years prior and controls for maternal characteristics.

8.1.1 Limitations

The pregnancy intentions questions are widely criticized. Despite the validity of the criticisms, our concerns about measurement error are assuaged by two factors: first, we are interested in changes over time and how those changes relate to the dividend. Though there may be measurement error in any year's data, there is no reason to believe that the error is correlated with the size of the cash transfer. Second, we have multiple measures of pregnancy intentions, which all show similar results.

There are two important weaknesses to using these data to assess access to contraception. First, the sample is only of people who gave birth. Ideally, we would have repeated measures of a population of women who might use birth control, not just those who recently gave birth. That being said, people who have unintended births are the population most likely to report contraceptive barriers. Second, this is an imperfect measure of economic constraints to contraceptive access. Perhaps the problems could have been alleviated by money but perhaps they could not have. If, for instance, the problem arose from the logistical challenge of getting a prescription then more income may not solve that problem.

8.2 BRFSS

We confirm our analyses of contraception by examining the Behavioral Risk Factor Surveillance System (BRFSS) in order to assess the economic constraints to birth control access. Annually, BRFSS provides cross-sectional data on various health-related topics for all 50 U.S. states, the District of Columbia, Guam, Puerto Rico, and the U.S. Virgin Islands. Alaska began participating in 1991. Surveys are administered by trained interviewers over the phone throughout the year and are representative of the non-institutionalized adult population within each state and territory. Questions regarding birth control use were asked in an optional module in 1998 and in the core module in 2002 and 2004. These questions were asked to females aged 18-44 years and males aged 18-59 years. People who indicated that they were sexually active but were not protecting themselves or their partners from pregnancy were asked why they weren't. Respondents were asked: "What is your main reason for not doing anything to keep you from getting pregnant?" or "What is your main reason for not doing anything to keep your partner from getting pregnant?", as appropriate. Among the answers was "You can't pay for birth control".

8.2.1 Limitations

Unfortunately the BRFFS contraceptive access question is only for a very limited number of years.

8.3 CDC Abortion Rates

Reliable abortion data are only available at the state-year level. We use abortion rates for Alaska by year from the Centers for Disease Control for 1982-2010.

8.3.1 Limitations

Given the data are at the state level, we cannot assess group-specific effects.

8.4 Natality Data: Sex Ratio

We compute the secondary sex ratio from the natality data. The sex ratio is calculated as the proportion of male infants in a PFD-aligned year. We also calculate the sex ratio by the following subgroups: less than a high school diploma, Alaska Native, unmarried, and first child.

8.4.1 Limitations

We do not have any direct data on the in utero environment nor do we have any data on miscarriages. There are data on fetal deaths but that regards pregnant people who spontaneously abort after twenty weeks of pregnancy and report this miscarriage. The data are not sufficient to infer all pregnancies that end in miscarriage even if the reporting were complete, which it is not. These are problems which afflict most researchers interested in the in utero environment (Bruckner and Catalano, 2018).

9 Results of Exploratory Mechanism Analyses

Table 10: Pregnancy Intention, Trying to Get Pregnant And Pregnancy Prevention

	Model 1 Intention	Model 2 Trying	Model 3 Prevention
Full Sample			
DIV_{t-1}	1.00001	1.000005	.99998
	(.0000116)	(.0000113)	(.0000164)
DIV_{t-2}	.99997*	.99997**	.99997
	(.0000134)	(.00000144)	(.0000209)
Not Married			
DIV_{t-1}	.9999958	1.000018	.9999848
	(.0000237)	(.0000225)	(.0000294)
DIV_{t-2}	.9999648	.9999848	.9999765
	(.0000298)	(.0000298)	(.000039)
Alaska Native			
DIV_{t-1}	1.000003	1.000016	1.00002
	(.0000138)	(.0000138)	(.0000193)
DIV_{t-2}	.9999856	1.000017	1.000013
	(.0000157)	(.0000176)	(.000024)
Low-Ed			
DIV_{t-1}	1.000004	.9999912	1.000054
	(.0000353)	(.0000337)	(.0000469)
DIV_{t-2}	.9999943	.9999738	.9998868*
	(.0000426)	(.0000414)	(.0000569)
First Birth			
DIV_{t-1}	.9998819**	.9999763	1.000015
	(.000041)	(.0000405)	(.0000623)
DIV_{t-2}	1.00008	1.000069	.9998555
	(.0000496)	(.0000567)	(.0000907)

Notes:
(1) Data: Pregnancy Risk Assessment Monitoring System Model 1: 1990-2010.
Models 2 and 3: 2000-2010.
(2) Model 1 N = 28,149; Model 2 N = 14,537; Model 3 N = 7,616
(3) *p<.05; ** p<.01; *** p<.001
(4) Dividend is in 2010 constant dollars. It is measured in n $1,000 units.
(5) Models control for marital status, parity, maternal education, maternal race and year.
(6) Models restricted to mothers with high school education or less were nearly identical or showed neither dividend was significant.
(7) Models restricted to unmarried mothers showed neither dividend was significant.

Table 11: Not Using Contraception When Got Pregnant Because Could Not Get It
Note: Sample excludes mothers who did not mind getting pregnant.

	Full Sample	Not Married	Alaska Native	Low-Ed	First Birth
DIV_{t-1}	1.000015	1.000086	1.000015	1.000041	1.00008
	(.0000498)	(.0000737)	(.0000594)	(.0000897)	(.0001883)
DIV_{t-2}	1.000008	.9999385	1.000054	.9998705	.9996489
	(.0000682)	(.0000972)	(.0000857)	(.0001465)	(.0003606)

Notes:
(1) Data: Pregnancy Risk Assessment Monitoring System, 2000-2010.
(2) Full Sample N= 2,395; Not Married N= 1,505 Alaska Native N= 1,299
Low-Ed N= First Birth N=1,014
(3) *p<.05; ** p<.01; *** p<.001
(4) Dividend is in 2010 constant dollars. It is measured in $1,000 units.
(5) Models control for marital status, parity, maternal education, maternal race and year.

Table 12: PFD Dividend Effects on the Abortion Rate 1982-2010: OLS Regression Results

Covariate	IRR	95% C.I.
Lag 0; N = 28 years		
DIV_t	.001	(-.007 , .009)
Lags 1, 2; N = 28 years		
DIV_{t-1}	-.005	(-.013 , .003)
DIV_{t-2}	.007	(-.002 , .016)
Lags 0, 1, 2; N = 28 years		
DIV_{t-1}	.002	(-.006 , .010)
DIV_{t-2}	-.005	(-.014 , .003)
DIV_{t-3}	.007	(-.002 , .016)

Notes:
(1) OLS coefficients shown. S.E. in parentheses.
(2) Abortion data are state-level abortion rates obtained
from the Centers for Disease Control for 1982-2010.
(3) Abortion rates are aligned to PFD-disbursement years.
(4) Dividend is in 2010 constant dollars. It is measured in $1,000 units.
(5) Significance: Models predicting the abortion rate
with various lag structures and controlling for
a time-trend indicate there is no relationship
between PFD payments and the abortion rate.

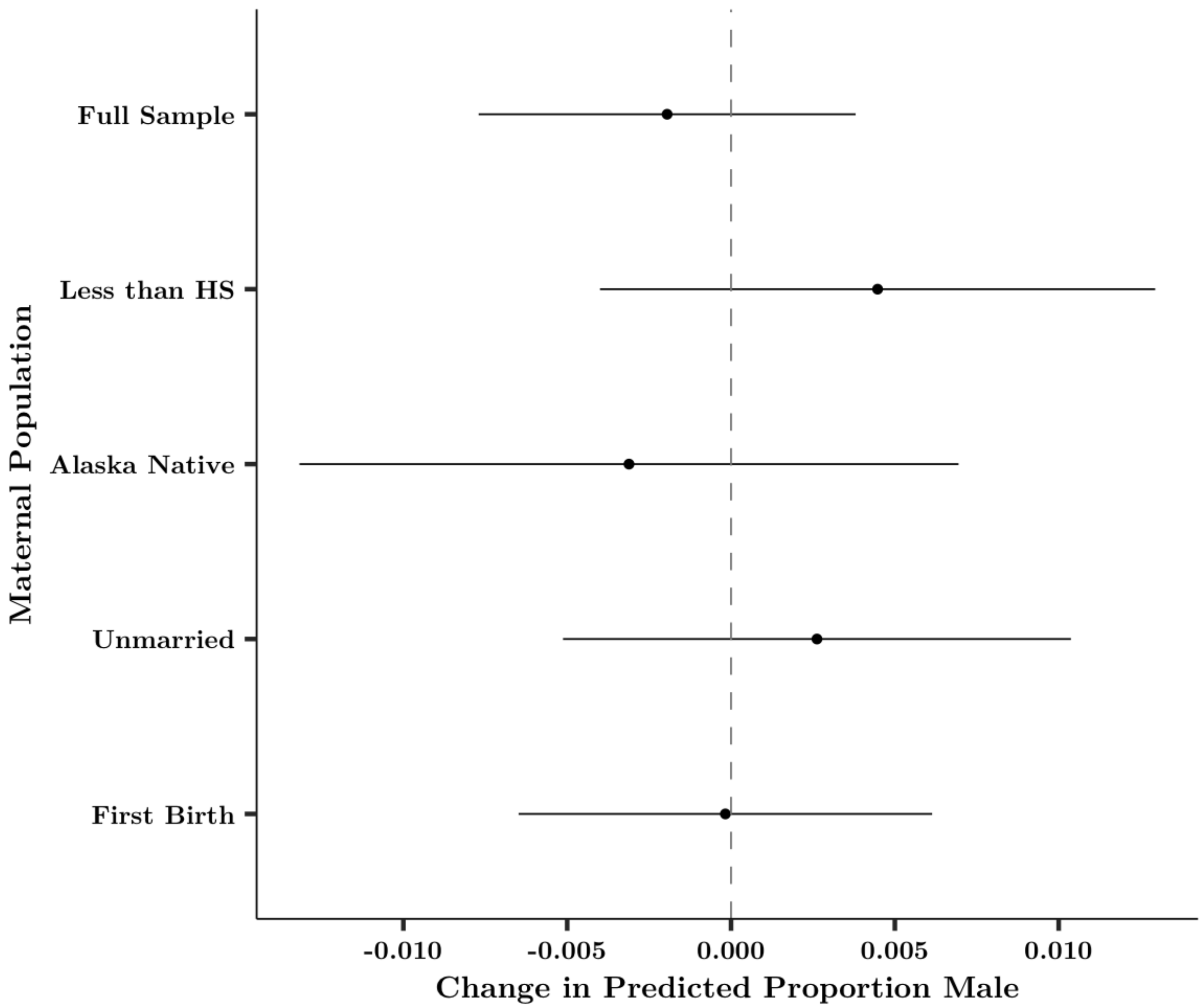

Figure 3: Change in Predicted Proportion Male Newborns among Mothers with Given Characteristic after Dividend Payment with 95 Percent Confidence Intervals: Dividends at *t-1* and *t-2* at \$626 versus \$3,315.