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

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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 high-income countries 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’ behavior and the state’s economy. We find the payment increases short-term fertility, particularly among socioeconomically disadvantaged women. In supplementary analyses, we find no change in the proportion of births that are unintended. We conclude that there is limited, suggestive evidence that the increased fertility is due to increased fecundity. Altogether, 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.
Recently, an international movement proposing cash transfers, sometimes proposed as a “Universal Basic Income” to individuals or families has become an increasingly prominent proposition to address poverty and inequality or to address current and future job losses due to technological change, automation or the economic downturn from the coronavirus pandemic (Aronowitz and DiFazio, 2010; Standing, 2017; Van Parijs and Vanderborght, 2017; Foran, 2020). Some versions of these proposals have been implemented: Governments in high, middle and low income countries around the world have instituted both conditional and unconditional cash transfer programs to benefit poor households (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). The first coronavirus stimulus package in the United States includes cash transfers to most Americans (Still and Uhrmacher, 2020). Some non-governmental organizations have mounted controlled experiments in low-income 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).

Social scientists and policymakers debate whether cash transfers would induce unintended consequences. 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. The experiments were conducted in four locations, and participants were randomly assigned to various combinations of base transfer amounts and tax rates. Simultaneously, a similar experiment was conducted in Manitoba, Canada. 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 in the United States.

One area of concern is whether the cash transfer would affect fertility. Most Americans hope to and do become parents (Hayford, 2009; Morgan and Rackin, 2010; Livingston et al., 2015), yet childbearing is a site of marked inequality. 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. Black and Hispanic people 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 (Morgan and Rackin, 2010). Finally, at a population level, declining birth rates have raised alarm in regions where they have fallen below replacement (United Nations, 2013; Lee et al., 2014; United Nations, 2015).

We know little about the effect of cash transfers on fertility because data on fertility from the NIT 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, and so researchers could draw few conclusions. Instead, scholars 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 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.

Our article proceeds as follows: first, we review the literature on income and fertility and propose hypotheses based on this literature. Next, we describe the case of the Alaska Permanent Fund Dividend. We then describe the methods and the data we employ to test the hypotheses and present the results. Following the main results, we present the results of supplementary analyses that explore possible mechanisms undergirding the effect of income on fertility. To do so, we marshal numerous additional data sources that we detail in the appendix. We lastly conclude the article with a discussion of our findings in light of the literature on income and fertility and the implications for Universal Basic Income proposals.

1 Income and Fertility

We 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 quantity of children they have but also their “quality”. 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 they were working rather than spending their time providing childcare.

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. The effect of wage income 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, which could result in a substitution effect. 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. The extent of the increase depends on individuals’ elasticity of demand for children. Variation in this sensitivity to the additional income shifts the composition of people giving birth.

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 in North America are mixed but, even when statistically 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 women of color and 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. A decrease in fertility was detected and no heterogeneous effects were found, but the sample was small and 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).

An exogenous shock to men’s income can also plausibly function as a shock to family income given that mothers are usually 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 Cost of Children Shocks

The cost of children can be manipulated in various ways through government policies often used to address population aging. 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 high-income countries, fertility is pro-cyclical; that is, fertility declines during recessions (Sobotka et al., 2011; Morgan et al., 2011). 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. 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.

These hypotheses regarding income and fertility 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 the social safety net, analyses are limited to the socioeconomically disadvantaged families who are eligible. Often the families that receive the benefit are not precisely identified in the data, so researchers rely on proxies. The dividend in Alaska, however, is given to every resident and so provides a much more diverse population to study and one that

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1We 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.
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 insufficient to identify a causal relationship between income and fertility as we test here.

Our primary contribution is a new test of whether there is a relationship between income and fertility, particularly for socioeconomically disadvantaged women, tested through a large cash transfer. As supplementary analyses, we examine the mechanisms that could underlie the relationship between income and fertility, which we briefly introduce next.

### 1.5 Mechanisms By Which Income Might Affect Fertility

Prior literature identifies four primary potential mechanisms by which income could affect fertility (Davis and Blake, 1956; Bongaarts, 1978): seeking intended childbearing, avoiding unintended childbearing (Birgisson et al., 2015; Lindo and Packham, 2017), sexual frequency (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) and a change fecundity due to an altered in utero environment. We detail the hypothesized causal pathways and research on each of these proposed mechanism in the Appendix but introduce them briefly here.

Increased non-wage income may induce people to seek childbearing, which would result in an increase in intended births. Alternatively, a cash transfer could increase access to contraception and abortion; this would decrease fertility overall and increase the proportion of intended births. Sexual frequency and changes in in utero selection could either increase or decrease fertility.

Each of these mechanisms 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, described in the Appendix. Nonetheless, these data are imperfect. 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; we do not propose hypotheses.
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 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. 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; 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.

News reports estimate the dividend amount in the spring with marked accuracy (Kueng, 2018). The

\footnote{People sentenced or incarcerated for a felony during the year are excluded, as well as people with extensive criminal records.}
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.

1.5.1 The Case for Exogeneity

In order to establish causality, we first make a case for exogeneity. The fund was established in 1976; 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. 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.

In sum, the amount of the dividend is exogenous because it is not dependent on local economic conditions or individuals’ behaviors and does not induce migration.

1.5.2 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. Though the local economy is not related to the dividend amount in a way that would threaten our causal estimates, we nonetheless 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.

1.5.3 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. Contrary to popular belief, the sex ratio is not overly skewed in Alaska, but it does have more men than women. Our sensitivity analysis of Anchorage alone attends to the concerns about the rural population as well as the sex ratio since it is less skewed in Anchorage than the state as a whole.

1.5.4 Universal Basic Income

The PFD is the closest example of a Universal Basic Income policy in the United States. Some 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 from a fully realized UBI 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. Second, the transfer 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 proposed a $1000 a month
2 Methods

We first assess the effect of the dividend on the overall short-term fertility rate and then assess heterogeneous treatment effects across demographic groups.

Given that pregnancies are forty weeks, the literature’s typical fertility-response timeframe from transfer to birth of one year 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 miscarriage rates. We also empirically assess longer windows, discussed below. The fertility-response window may continue through early pregnancy, depending on access to abortion. We empirically assess this in our supplementary analyses, in Appendix section “Assessing acclimation to PFD payments.”

Fertility Response: Birth Rate and Composition of Who Gives Birth

To 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, married Alaska Native women who have a college degree and no children 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:

\[
\log \mu_{jt} = \log E_{jt} + \beta_1 DIV_{j(t-1)} + \beta_2 DIV_{j(t-2)} + \beta_3 X_{jt} + \beta_4 UNEMP_{t-2} + \beta_5 US_t + \beta_6 YEAR_t + \epsilon_{jt}
\]

where \( j \) indicates Demographic Groupings and \( t \) indicates year. \( \mu_{jt} \) 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 \( t \) (the rate’s denominator); \( DIV_{j(t-1)} \) and \( DIV_{j(t-2)} \) are the cash transfer to each household in that year; vector \( X_{jt} \) 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.

Lastly, we examine mechanisms underlying any effect between income and fertility. These are supplementary analyses that are standard regression analyses described in more detail in the appendix.

2.0.1 Sensitivity Analyses for the Fertility Effects

We perform four major 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 use South Dakota as a placebo test given that it is demographically similar to Alaska (Table 7). 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. 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.

Third, we perform the birth rate analyses for Anchorage alone. Obtaining substantively similar results for this subpopulation allays three concerns: regarding the exclusion restriction, generalizability with regard to rurality, and generalizability with regard to the sex ratio.

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 unanticipated jumps or dips in the dividend. These analyses, described further in the Appendix, do not suggest normalization. Following the life-cycle and permanent-income models, one might also anticipate that consumption is smoothed such that residents should respond very little if at all to anticipated income changes. If this were the case, then we would not see an effect from the transfer. In addition, recent empirical work indicates that the theory does not hold - that consumption does respond to anticipated income increases (for a summary see Jappelli and Pistaferri (2010) Fuchs-Schündeln and Hassan (2016).) Alaskans are not consumption smoothing in response to the PFD for either durables or non-durables (Kueng (2018) but see also Hsieh (2003)).
In addition to these main four, we conduct numerous other sensitivity analyses as a part of our main analyses. For example, we test our models using macro-economic indicators other than just the unemployment rate and we test other windows besides the 24 month window among others. We mention those when relevant below.

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 people 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. 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. Given that the dividend amount is accurately predicted in the spring, we also conducted a sensitivity analysis that aligned years from April to March. Results did not suggest possible anticipatory effects.

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 $t$.

We describe our extensive missing data procedures, as well as sensitivity analyses, in the Appendix section.
3.2 Population Counts

In order to assess birth rates, we require population count denominators by Demographic Grouping. We obtained these 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, making them 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).

3.3 Supplementary Measures Regarding Possible Mechanisms

Data sources and measures used to assess possible mechanisms are described in Appendix section “Data Sources for Exploratory Mechanism Analyses.” Briefly, we draw on the Pregnancy Risk Assessment Monitoring System (PRAMS) to to measure pregnancy intentions, the Behavioral Risk Factor Surveillance System (BRFSS) and the Centers for Disease Control to measure contraception and abortion, and the natality data to measure the sex ratio, which provides insight into fecundity.

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. In an alternate test of whether the effects we identify are driven by only a few years, we re-estimated Model 1 with all possible three-consecutive year periods dropped. All of these models produced substantively similar results.

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. Model 4 presents the results of the birth rate model for South Dakota. The model indicates 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 statuses, 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 short-term 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

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. We discuss them in more detail in the Appendix section “Results of Exploratory Mechanism Analyses.” In brief, however, there is no relationship between the size of the cash transfer and pregnancy intentions, use of abortion, or use of contraception. There are two suggestive
indications that the fertility increase is due to an improved in utero environment. First, the fertility rate increased without a change in the pregnancy intentions of those pregnancies that ended in live births; this suggests an overall increase in fecundity. Second, when the dividend is higher, the sex ratio at birth changes, but this is not significant at traditional levels.

5 Discussion and Conclusion

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.

While time-series natural experiments are imperfect, they are one of the few tools available to legitimately analyze policy effects, and the case of PFD income transfers in Alaska is stronger than most for six reasons. First, the payments are exogenous to individual Alaskans. Second, the payment is universally given to every Alaskan resident. This is an unusually large and diverse treatment population for a natural experiment. Third, given its universality, there are few worries about confounders or selection on the basis of the personal and social characteristics demographers use when modeling fertility decisions. We nonetheless include maternal characteristics in our models as well as controls for macro-economic indicators. We also control for more general trends in fertility. Fourth, the payment amount varies from year to year, allowing for assessment of the effect of particular amounts of transfer rather than merely the absence or presence of a transfer. Fifth, in addition to annual variation, we exploit variation in the amount received by households of different sizes. Sixth, we can examine people who received the dividend while pregnant and compare them to those who do not.

A last strength of the case regards the data. Since nearly all Alaskans receive the dividend, the treated population is precisely identified, a rarity in most of the causal literature on income and fertility. Further, the data come from the vital registry, which provides data on the complete population of births and includes a wealth of information on people giving birth, including socio-demographic characteristics, with little measurement error. While for other examinations over time scholars need to rely on interpolation to create intercensal denominators, we have the advantage of Alaska’s intercensal population estimates that are markedly more accurate than mere interpolation.

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.

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.

Despite these limitations, the Alaskan case provides the best opportunity we have to study the potential effects of a universal basic income, a proposal that has garnered marked support as of late. Alaska 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 macro-economy. 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 people 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. These effects may largely reflect childbearing postponement or acceleration but may also leave an imprint on completed family size.

Our examination of mechanisms for this sensitivity suggest possible improvement in the in utero environment that increased fecundity. We cannot rule out the mechanism of sexual frequency given that we have no data by which to test it. Importantly, when the dividend increased, 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. This study contributes to the small literature on in utero selection, a process which could profoundly affect fertility, health at birth and later-life outcomes (Nobles and Hamoudi, 2019).

We interpret the increasing birth rate among socio-economically disadvantaged people as an indication that they 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. This is an enactment of the claims of the reproductive justice framework (Luna and Luker, 2013; Ross and Solinger, 2017) which calls our scholarly attention to the barriers to childbearing. This also confers with the description of “demand for children” prominent in economics articulated by Gary Becker who wrote “‘demand’ means the number of children desired when there are no obstacles to the production or prevention of children” (Becker, 2009).

There was an economic barrier to childbearing for some people in Alaska, alleviated by the cash payment. 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 be able to test this supposition. The cash payments made to Americans as a part of the stimulus plan in light of coronavirus will be another opportunity to test the effects of a cash transfer. 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, though designed to address poverty and economic inequalities, can 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

<table>
<thead>
<tr>
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</tr>
</thead>
<tbody>
<tr>
<td>Educational attainment (%)(^a)</td>
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<td></td>
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<td></td>
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<td></td>
</tr>
<tr>
<td>High School or higher</td>
<td>66.5</td>
<td>82.5</td>
<td>75.2</td>
<td>86.6</td>
<td>80.4</td>
<td>88.3</td>
<td>85.0</td>
<td>88.4</td>
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<td>College Degree or higher</td>
<td>16.2</td>
<td>21.1</td>
<td>20.3</td>
<td>23.0</td>
<td>24.4</td>
<td>25.4</td>
<td>27.9</td>
<td>25.4</td>
</tr>
<tr>
<td>Median Age (years)(^a)</td>
<td>30.0</td>
<td>26.0</td>
<td>32.9</td>
<td>29.4</td>
<td>35.3</td>
<td>32.4</td>
<td>37.2</td>
<td>33.8</td>
</tr>
<tr>
<td>Hispanic (%)(^a)</td>
<td>6.4</td>
<td>2.4</td>
<td>9.0</td>
<td>3.2</td>
<td>12.5</td>
<td>4.1</td>
<td>16.3</td>
<td>5.5</td>
</tr>
<tr>
<td>Race (%)(^b,(^c)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
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<td>White</td>
<td>83.2</td>
<td>77.1</td>
<td>80.3</td>
<td>75.5</td>
<td>75.1</td>
<td>69.3</td>
<td>72.4</td>
<td>66.7</td>
</tr>
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<td>Black</td>
<td>11.7</td>
<td>3.4</td>
<td>12.1</td>
<td>4.1</td>
<td>12.3</td>
<td>3.5</td>
<td>12.6</td>
<td>3.3</td>
</tr>
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<td>American Indian or Alaska Native</td>
<td>0.7</td>
<td>15.9</td>
<td>0.8</td>
<td>15.6</td>
<td>0.9</td>
<td>15.6</td>
<td>0.9</td>
<td>14.8</td>
</tr>
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<td>Asian or Pacific Islander</td>
<td>1.6</td>
<td>1.9</td>
<td>2.9</td>
<td>3.6</td>
<td>3.7</td>
<td>4.5</td>
<td>5.0</td>
<td>6.4</td>
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<tr>
<td>Other Race or Multiracial</td>
<td>3.9</td>
<td>3.9</td>
<td>3.9</td>
<td>1.2</td>
<td>7.9</td>
<td>7.0</td>
<td>9.1</td>
<td>8.9</td>
</tr>
<tr>
<td>Median Household Income (dollars)(^c)</td>
<td>16,841</td>
<td>25,414</td>
<td>29,943</td>
<td>39,298</td>
<td>41,990</td>
<td>52,847</td>
<td>49,276</td>
<td>57,848</td>
</tr>
<tr>
<td>Poverty (%)(^d,(^e)</td>
<td>12.0</td>
<td>10.7</td>
<td>10.0</td>
<td>9.0</td>
<td>11.5</td>
<td>7.9</td>
<td>15.3</td>
<td>9.9</td>
</tr>
<tr>
<td>Urban (%)(^a,(^e)</td>
<td>73.7</td>
<td>64.4</td>
<td>80.0</td>
<td>67.5</td>
<td>79.0</td>
<td>65.6</td>
<td>80.7</td>
<td>66.0</td>
</tr>
<tr>
<td>Foreign-Born (%)(^a,(^d)</td>
<td>6.2</td>
<td>4.0</td>
<td>8.0</td>
<td>4.5</td>
<td>11.1</td>
<td>5.9</td>
<td>12.9</td>
<td>7.0</td>
</tr>
<tr>
<td>Children Living with a Single Parent (%)(^f)</td>
<td>19.7</td>
<td>19.3</td>
<td>24.7</td>
<td>20.0</td>
<td>26.7</td>
<td>19.7</td>
<td>26.6</td>
<td>21.7</td>
</tr>
<tr>
<td>Female (%(^g)(^h)</td>
<td>51.4</td>
<td>47.0</td>
<td>51.3</td>
<td>47.3</td>
<td>50.9</td>
<td>48.3</td>
<td>50.8</td>
<td>47.9</td>
</tr>
<tr>
<td>Fertility Rate(^i,(^a,(^g)</td>
<td>68.4</td>
<td>88.6</td>
<td>70.9</td>
<td>86.3</td>
<td>67.5</td>
<td>74.6</td>
<td>64.1</td>
<td>80.1</td>
</tr>
<tr>
<td>Pre-Term Births (%)(^i)</td>
<td>8.9</td>
<td>7.6</td>
<td>10.6</td>
<td>8.8</td>
<td>11.64</td>
<td>10.0</td>
<td>12.0</td>
<td>9.7</td>
</tr>
<tr>
<td>Low Birth Weight Births (%)(^i)</td>
<td>6.8</td>
<td>5.4</td>
<td>7.0</td>
<td>4.7</td>
<td>7.6</td>
<td>5.6</td>
<td>8.15</td>
<td>5.7</td>
</tr>
</tbody>
</table>

Notes:
[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.
[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

<table>
<thead>
<tr>
<th></th>
<th>Household Payment</th>
<th>Individual Payment</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Model 1: AK Rate</td>
<td>Model 2: AK Rate w/ Decade F.E.</td>
</tr>
<tr>
<td>IRR</td>
<td>C.I.</td>
<td>IRR</td>
</tr>
<tr>
<td>$DIV_{t-1}$</td>
<td>1.018** (1.008 , 1.029)</td>
<td>1.016** (1.005 , 1.026)</td>
</tr>
<tr>
<td>$DIV_{t-2}$</td>
<td>1.019*** (1.009 , 1.030)</td>
<td>1.025*** (1.014 , 1.035)</td>
</tr>
</tbody>
</table>

**Notes:**

4. IRR = Incidence Rate Ratios.
5. *p < .05; ** p < .01; *** p < .001
6. Dividend is in 2010 constant dollars. It is measured in $1,000 units.
7. Unit of analysis is Demographic Groupings - demographic groups of women determined by age, race, marital status, educational attainment, and parity.
8. Controls are by age, race, marital status, educational attainment, parity, year, the U.S. birth rate, and the AK unemployment rate lagged two years.
<table>
<thead>
<tr>
<th></th>
<th>Household Payment</th>
<th>Individual Payment</th>
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<tbody>
<tr>
<td></td>
<td>IRR</td>
<td>C.I.</td>
</tr>
<tr>
<td>$DIV_{t-1}$</td>
<td>1.005 (.994 , 1.016)</td>
<td>1.003 (.968 , 1.039)</td>
</tr>
<tr>
<td>$DIV_{t-2}$</td>
<td>1.010 (.998 , 1.021)</td>
<td>1.014 (.976 , 1.053)</td>
</tr>
</tbody>
</table>

Notes:
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

<table>
<thead>
<tr>
<th>Mechanism</th>
<th>Theorized Effect of Cash</th>
<th>Data</th>
<th>Measures</th>
<th>Results</th>
<th>Appendix</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Mechanisms that could increase fertility</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pregnancy Intention</td>
<td>Increase in pregnancy-seeking/intended pregnancies</td>
<td>PRAMS</td>
<td>Traditional intention measure, Trying to get pregnant, Preventing pregnancy</td>
<td>Three measures of pregnancy intention: Null</td>
<td>Appendix Table10</td>
</tr>
<tr>
<td><strong>Mechanisms that could decrease fertility</strong></td>
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<tr>
<td>Contraception</td>
<td>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</td>
<td>(1) &amp; (2) PRAMS</td>
<td>(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</td>
<td>(1) Pregnancy intention: Null (2) Not contracepting reason: Null (2) No indication of financial barrier to contraception</td>
<td>Appendix Table11</td>
</tr>
<tr>
<td>Abortion</td>
<td>Increase in abortion access that would: (1) Increase abortion rate (2) Increase proportion of births to intended pregnancies</td>
<td>(1) Abortion rate from CDC (2) PRAMS</td>
<td>(1) Abortion rate (2) Three measures of pregnancy intentions</td>
<td>(1) Abortion rate: Null (2) Pregnancy intention: Null</td>
<td>Appendix Table12</td>
</tr>
<tr>
<td><strong>Mechanisms that could either increase or decrease fertility</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sexual Frequency</td>
<td>(1) Increased frequency: increased fertility (2) Decreased frequency: decreased fertility</td>
<td>No data</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>In Utero Selection</td>
<td>(1) Improved in utero environment: - Increase in fertility - Sex ratio changes - No change in pregnancy intentions (2) Worse in utero environment: - Decrease in fertility</td>
<td>(1) Natality (1) PRAMS</td>
<td>(1) Sex ratio at birth (1) Three measures of pregnancy intentions</td>
<td>(1) Sex ratio at birth: Suggestive evidence of change (1) Pregnancy intentions: Null</td>
<td>Appendix Figure3</td>
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</table>
7 Appendix
7.1 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. Multiple imputation was not feasible because the natality data do not contain enough parental characteristics to accurately predict missing values. 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.
Table 5: Birth Rate Analysis with Macro-Economic Controls Added: Log-Rate Model Results

<table>
<thead>
<tr>
<th>Covariate</th>
<th>AK Unemp. Rate Included</th>
<th>AK Income Included</th>
<th>Oil Price Included</th>
</tr>
</thead>
<tbody>
<tr>
<td>$DIV_{t-1}$</td>
<td>1.018*** (1.008 , 1.029)</td>
<td>1.015** (1.006 , 1.025)</td>
<td>1.016** (1.006 , 1.026)</td>
</tr>
<tr>
<td>$DIV_{t-2}$</td>
<td>1.019*** (1.009 , 1.030)</td>
<td>1.019*** (1.009 , 1.030)</td>
<td>1.019*** (1.009 , 1.029)</td>
</tr>
</tbody>
</table>

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

<table>
<thead>
<tr>
<th></th>
<th>Household Payment</th>
<th>Individual Payment</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Model 1: Rate</td>
<td>Model 2: Rate w/ Decade F.E.</td>
</tr>
<tr>
<td>IRR</td>
<td>C.I.</td>
<td>IRR</td>
</tr>
<tr>
<td>$DIV_{t-1}$</td>
<td>1.015* (1.001, 1.030)</td>
<td>1.013+ (0.999, 1.027)</td>
</tr>
<tr>
<td>$DIV_{t-2}$</td>
<td>1.023** (1.009, 1.037)</td>
<td>1.026*** (1.012, 1.040)</td>
</tr>
</tbody>
</table>

Notes:
(3) AK N = 9,168 Demographic Groupings; 93,231 births.
(4) IRR = Incidence Rate Ratios.
(5) $p < .10; * p < .05; ** p < .01; *** p < .001$
(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

<table>
<thead>
<tr>
<th>Measure</th>
<th>1980</th>
<th>2010</th>
<th>1980</th>
<th>2010</th>
</tr>
</thead>
<tbody>
<tr>
<td>Educational attainment (%)(^a)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>High School or higher</td>
<td>82.5</td>
<td>67.9</td>
<td>88.4</td>
<td>89.3</td>
</tr>
<tr>
<td>College Degree or higher</td>
<td>21.1</td>
<td>14.0</td>
<td>25.4</td>
<td>25.3</td>
</tr>
<tr>
<td>Median Age (years)(^a)</td>
<td>26.0</td>
<td>28.9</td>
<td>33.8</td>
<td>36.9</td>
</tr>
<tr>
<td>Hispanic (%)(^a)</td>
<td>2.4</td>
<td>0.1</td>
<td>5.5</td>
<td>2.7</td>
</tr>
<tr>
<td>Race (%)(^a,b)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>White</td>
<td>77.1</td>
<td>92.6</td>
<td>66.7</td>
<td>85.9</td>
</tr>
<tr>
<td>Black</td>
<td>3.4</td>
<td>0.3</td>
<td>3.3</td>
<td>1.3</td>
</tr>
<tr>
<td>American Indian or Alaska Native</td>
<td>15.9</td>
<td>6.6</td>
<td>14.8</td>
<td>8.8</td>
</tr>
<tr>
<td>Asian or Pacific Islander</td>
<td>1.9</td>
<td>0.3</td>
<td>6.4</td>
<td>0.9</td>
</tr>
<tr>
<td>Other Race or Multiracial</td>
<td>1.6</td>
<td>0.2</td>
<td>8.9</td>
<td>0.9</td>
</tr>
<tr>
<td>Median Household Income (dollars)(^c)</td>
<td>25,414</td>
<td>13,156</td>
<td>57,848</td>
<td>45,352</td>
</tr>
<tr>
<td>Poverty (%)(^c,d)</td>
<td>10.7</td>
<td>16.9</td>
<td>9.9</td>
<td>14.4</td>
</tr>
<tr>
<td>Urban (%)(^a,e)</td>
<td>64.4</td>
<td>46.4</td>
<td>66.0</td>
<td>55.3</td>
</tr>
<tr>
<td>Foreign-Born (%)(^a,d)</td>
<td>4.0</td>
<td>1.4</td>
<td>7.0</td>
<td>2.7</td>
</tr>
<tr>
<td>Children Living with a Single Parent (%)(^c)</td>
<td>19.3</td>
<td>13.6</td>
<td>21.7</td>
<td>27.6</td>
</tr>
<tr>
<td>Female (%)(^a)</td>
<td>47.0</td>
<td>50.7</td>
<td>47.9</td>
<td>49.5</td>
</tr>
<tr>
<td>Fertility Rate(^f,g,h)</td>
<td>90.5</td>
<td>88.3</td>
<td>80.1</td>
<td>77.3</td>
</tr>
<tr>
<td>Pre-Term Births (%)(^f,g)</td>
<td>7.6</td>
<td>6.0</td>
<td>9.7</td>
<td>11.4</td>
</tr>
<tr>
<td>Low Birth Weight Births (%)(^f,g)</td>
<td>5.4</td>
<td>5.1</td>
<td>5.7</td>
<td>6.8</td>
</tr>
</tbody>
</table>

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.
\(^d\) 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.
7.2 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

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

Notes:
(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>
<thead>
<tr>
<th>Covariate</th>
<th>IRR</th>
<th>95% C.I.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lags 0, 1, 2; N = 11,696 Demographic Groupings</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(DIV_t)</td>
<td>1.010</td>
<td>(.998 , 1.023)</td>
</tr>
<tr>
<td>(DIV_{t-1})</td>
<td>1.015***</td>
<td>(1.007 , 1.022)</td>
</tr>
<tr>
<td>(DIV_{t-2})</td>
<td>1.020***</td>
<td>(1.013 , 1.027)</td>
</tr>
<tr>
<td>Lags 1, 2; N = 11,696 Demographic Groupings</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(DIV_{t-1})</td>
<td>1.018***</td>
<td>(1.011 , 1.025)</td>
</tr>
<tr>
<td>(DIV_{t-2})</td>
<td>1.019***</td>
<td>(1.012 , 1.027)</td>
</tr>
<tr>
<td>Lags 1, 2, 3; N = 11,208 Demographic Groupings</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(DIV_{t-1})</td>
<td>1.014***</td>
<td>(1.006 , 1.022)</td>
</tr>
<tr>
<td>(DIV_{t-2})</td>
<td>1.025***</td>
<td>(1.015 , 1.035)</td>
</tr>
<tr>
<td>(DIV_{t-3})</td>
<td>.997</td>
<td>(.985 , 1.009)</td>
</tr>
</tbody>
</table>

Notes:
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.
7.3 Potential Mechanisms by Which Income May Affect Fertility: Details

Prior literature identifies four potential mechanisms: seeking intended childbearing, avoiding unintended childbearing, sexual frequency, and a change in fecundity due to an altered in utero environment. The first possibility is 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.

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 would also anticipate heterogeneous effects.
Given that different populations of women have different sex ratios at birth (Almond and Edlund, 2007),
it is unclear in which direction the sex ratio will change. 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.

7.4 Data Sources for Exploratory Mechanism Analyses

7.4.1 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.

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.

7.4.2 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”.

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

### 7.4.3 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.

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

### 7.4.4 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.

**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).

### 7.5 Results of Exploratory Mechanism Analyses

**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. This can be seen in Table 10.

**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 BRFFS
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. This can be seen in Table 10.

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 as can be seen in Figure 3.
## Table 10: Pregnancy Intention, Trying to Get Pregnant And Pregnancy Prevention

<table>
<thead>
<tr>
<th>Model</th>
<th>Full Sample</th>
<th>Model 2</th>
<th>Model 3</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Intention</td>
<td>Trying</td>
<td>Prevention</td>
</tr>
<tr>
<td><strong>DIV_t−1</strong></td>
<td>1.00001</td>
<td>1.00007</td>
<td>.9999769</td>
</tr>
<tr>
<td></td>
<td>(.0000115)</td>
<td>(.0000112)</td>
<td>(.0000163)</td>
</tr>
<tr>
<td><strong>DIV_t−2</strong></td>
<td>.9999714*</td>
<td>.9999739</td>
<td>.9999716</td>
</tr>
<tr>
<td></td>
<td>(.0000134)</td>
<td>(.00000143)</td>
<td>(.0000208)</td>
</tr>
<tr>
<td><strong>DIV_t−1</strong></td>
<td>.9999921</td>
<td>1.000015</td>
<td>.9999823</td>
</tr>
<tr>
<td></td>
<td>(.0000237)</td>
<td>(.0000221)</td>
<td>(.0000289)</td>
</tr>
<tr>
<td><strong>DIV_t−2</strong></td>
<td>.9999726</td>
<td>.9999765</td>
<td>.9999765</td>
</tr>
<tr>
<td></td>
<td>(.0000307)</td>
<td>(.0000297)</td>
<td>(.000038)</td>
</tr>
<tr>
<td><strong>DIV_t−1</strong></td>
<td>1.000006</td>
<td>1.000017</td>
<td>1.000018</td>
</tr>
<tr>
<td></td>
<td>(.0000138)</td>
<td>(.0000138)</td>
<td>(.0000193)</td>
</tr>
<tr>
<td><strong>DIV_t−2</strong></td>
<td>.9999844</td>
<td>1.000013</td>
<td>1.000013</td>
</tr>
<tr>
<td></td>
<td>(.0000157)</td>
<td>(.0000175)</td>
<td>(.0000239)</td>
</tr>
<tr>
<td><strong>DIV_t−1</strong></td>
<td>1.000009</td>
<td>.9999943</td>
<td>1.000059</td>
</tr>
<tr>
<td></td>
<td>(.0000376)</td>
<td>(.0000336)</td>
<td>(.0000464)</td>
</tr>
<tr>
<td><strong>DIV_t−2</strong></td>
<td>.999997</td>
<td>.999976</td>
<td>.9998834*</td>
</tr>
<tr>
<td></td>
<td>(.0000457)</td>
<td>(.0000425)</td>
<td>(.000058)</td>
</tr>
<tr>
<td><strong>DIV_t−1</strong></td>
<td>.9998863**</td>
<td>.9998811</td>
<td>1.000008</td>
</tr>
<tr>
<td></td>
<td>(.0000423)</td>
<td>(.0000408)</td>
<td>(.0000624)</td>
</tr>
<tr>
<td><strong>DIV_t−2</strong></td>
<td>1.000083</td>
<td>1.000077</td>
<td>.9998438</td>
</tr>
<tr>
<td></td>
<td>(.0000505)</td>
<td>(.0000569)</td>
<td>(.0000923)</td>
</tr>
</tbody>
</table>

Notes:
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, maternal age 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.

<table>
<thead>
<tr>
<th></th>
<th>Full Sample</th>
<th>Not Married</th>
<th>Alaska Native</th>
<th>Low-Ed</th>
<th>First Birth</th>
</tr>
</thead>
<tbody>
<tr>
<td>$DIV_{t-1}$</td>
<td>1.000015</td>
<td>1.000086</td>
<td>1.000015</td>
<td>1.000041</td>
<td>1.00008</td>
</tr>
<tr>
<td></td>
<td>(.0000498)</td>
<td>(.0000737)</td>
<td>(.0000594)</td>
<td>(.0000897)</td>
<td>(.0001883)</td>
</tr>
<tr>
<td>$DIV_{t-2}$</td>
<td>1.000008</td>
<td>.9999385</td>
<td>1.000054</td>
<td>.9998705</td>
<td>.9996489</td>
</tr>
<tr>
<td></td>
<td>(.0000682)</td>
<td>(.0000972)</td>
<td>(.0000857)</td>
<td>(.0001465)</td>
<td>(.0003606)</td>
</tr>
</tbody>
</table>

Notes:
(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 age, maternal education, maternal race and year.

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

<table>
<thead>
<tr>
<th>Covariate</th>
<th>IRR</th>
<th>95% C.I.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lag 0; N = 28 years</td>
<td>(DIV_t)</td>
<td>.001 (-.007 , .009)</td>
</tr>
<tr>
<td>Lags 1, 2; N = 28 years</td>
<td>$DIV_{t-1}$</td>
<td>-.005 (-.013 , .003)</td>
</tr>
<tr>
<td></td>
<td>$DIV_{t-2}$</td>
<td>.007 (-.002 , .016)</td>
</tr>
<tr>
<td>Lags 0, 1, 2; N = 28 years</td>
<td>$DIV_{t-1}$</td>
<td>.002 (-.006 , .010)</td>
</tr>
<tr>
<td></td>
<td>$DIV_{t-2}$</td>
<td>-.005 (-.014 , .003)</td>
</tr>
<tr>
<td></td>
<td>$DIV_{t-3}$</td>
<td>.007 (-.002 , .016)</td>
</tr>
</tbody>
</table>

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 $3,315$. 