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

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Abstract

Governments around the globe institute income policies in order to alleviate poverty. Whether these policies have unintended fertility effects is an open question, and the answer has implications for fertility theory, policy design and inequality. This question has been addressed in prior literature primarily by examining the introduction of means-tested relief to families with children. This limits analysis to families in poverty and provides insight only into the presence or absence of a policy. We overcome these weaknesses by examining 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 examine the relationship between income and fertility among a large and diverse population that has received varying amounts of money over time. We find that when the payment is more generous short-term fertility subsequently increases, particularly among socioeconomically disadvantaged women. The additional income removes economic constraints to reproductive health and autonomy. Further, universal cash transfers, proposals of which have gained popularity of late, could be a tool by which to mitigate population aging.
The United States and many other nations around the world institute income assistance policies in an attempt to alleviate poverty and inequality. These policies take different forms, from cash assistance to tax credits to near-cash transfers like food stamps or housing vouchers, but the goal is the same: to improve material well-being to reduce the harms of poverty. The underlying idea behind such transfer programs is that directly increasing individuals’ or families’ resources could improve their well-being immediately across a range of important outcomes, such as by increasing consumption of necessary goods like food or healthcare. In addition, it could enable behaviors that are beneficial in a longer time-horizon, such as investing in schooling.

While these policies are focused on immediate material well-being, scholars and lawmakers are aware that interventions aimed at one outcome may impact other outcomes in unanticipated ways. One topic that has received considerable attention in the social science literature on poverty, as well as in debates over welfare reform, is the impact of social policy on fertility. For instance, in the mid-1990s in the United States, some lawmakers expressed fears that welfare payments to impoverished families would enable them to have more children and therefore increase the population receiving government support. In response, the 1996 welfare reform included a controversial fertility disincentive called the “family cap” provision, which specified that families who had an additional child while receiving welfare would not have increased assistance in order to pay for that child’s expenses. The controversy hinged on two matters: whether it was effective and whether it impinged on individuals’ autonomy (Harris, 2001; Roberts, 1999). In contrast, others argue that income policies could increase reproductive autonomy and reduce inequalities in who gets to have the children they desire (Ross and Solinger, 2017).

Additionally, many high income countries are experiencing population aging and are either currently facing or will face a fiscal strain on public pensions and healthcare systems. Population aging further raises the spectre of slow economic growth (United Nations, 2013; Lee et al., 2014; United Nations, 2015). If an income assistance policy were to increase fertility, then it may help mitigate population aging.

On an individual level, questions about fertility are important for many reasons, including that most Americans hope to and do become parents and that childbearing is a site of marked inequality. Having children is among most young Americans’ aspirations (Hayford, 2009; Morgan and Rackin, 2010) and eighty-five percent of American women have children in their lifetimes (Livingston et al., 2015). Some people have more children than they want and even more have fewer children than they would like; this is patterned by education (Morgan and Rackin, 2010).

The question of the effect of income on fertility, particularly for a large and diverse population, is still unresolved. Scholars investigating the effect of income policies on fertility have primarily relied on changes in the tax code, such as those of the Earned Income Tax Credit, the tax exemption for dependents, or the
implementation of near-cash transfers like food stamps. These are policies that primarily benefit people who are already parents, and these policies are heavily means-tested. This limits the populations and processes in which they can provide insight; they cannot illuminate income’s effects on the transition into parenthood, nor how income influences people with more material resources.

To overcome these weaknesses, we examine a universal cash transfer made to every Alaskan resident since 1982. Every year, each resident of Alaska receives 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. Thus, PFD payments serve as a repeated quasi-natural experiment for hundreds of thousands of Americans over nearly 30 years. There are two sources of payment 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. These two sources of variation result in a payment range of $625 to $16,832. While most prior research considers the presence or absence of a policy, we can exploit this variation to consider the generosity of the transfer which we do using established sociological methods for studying fertility (Powers and Xie, 2008).

The Alaskan cash transfer is similar to a “Universal Basic Income” which 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; Murray, 2006; Stern, 2016; Bregman, 2017; Lowrey, 2018). The idea of a guaranteed income is hardly new. It was first implemented in Speenhamland, England in 1795, where, as an amendment to the Elizabethan poor laws, even able-bodied employed workers would get an income supplement tied to family size to account for the increase in grain prices (Speizman, 1966). Thomas Malthus was among the first to suggest that income supplements would increase fertility (and do so beyond a family’s material capacity to care for itself (Malthus, 1809; Wrigley and Smith, 2020). This was not systematically tested until the late 1960s and 1970s when 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. Fertility data, however, were only available for two sites - Gary, Indiana and Manitoba. In Gary, the cash transfer decreased fertility (Kehrer and Wolin, 1979; Wolin, 1978) and in Manitoba, there were no fertility effects Forget (2011). Both of these experiments were small, and the populations studied were homogeneous. By exploiting variation in the generosity of the cash transfer, we can draw conclusions about the relationship between income and fertility for a large and diverse population and can study some subgroups that were necessarily excluded.
from prior research.

To preview our main findings, we find an increase in short-term fertility, particularly among disadvantaged populations. We can draw a few important conclusions: First, income policies can effectively mitigate population aging which over one hundred governments globally have articulated is a major concern. Second, there was an economic barrier to having children in Alaska, a violation of the principles of reproductive justice. Reproductive justice is a relatively recent framework that emphasizes three principles: “(1) the right to not have a child; (2) the right to have a child; and (3) the right to parent children in safe and healthy environments” (Ross and Solinger, 2017). Third, a policy that, even inadvertently, increases fertility would best be followed by policies that enable communities and families to thrive in the face of the increased childbearing, policies such as those addressing parental leave and childcare access, among others.

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 argue that (1) the amount of the payment is exogenous to individual Alaskans, (2) there is no factor correlated with fertility that is also correlated with the dividend (the exclusion restriction) and (3) that Alaska is similar enough to other parts of America that we can draw inferences from it. We then describe the methods and the data we employ to test the hypotheses and present the results. There is no established causal approach to studying the Alaska case, and, while we make the argument for the approach we adopt in the main analyses, we present many sensitivity analyses to test other approaches. Our results are robust to all of them. We lastly conclude the article with a discussion of our findings in light of the literature on income and fertility and the implications for policy.

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 positive 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), and higher-order births to married white women and unmarried women of color who were eligible for the benefit (Duchovny, 2001). The EITC decreased time to the second child, particularly among unmarried mothers, but did not affect completed fertility overall (Meckel, 2015). 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
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, and this increased 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, job loss for men 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 have a smaller increase in fertility 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, women who were unmarried and had fewer socioeconomic resources 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 groups will be less responsive to the income changes than advantaged groups. However, we recognize that previous research on which populations are more sensitive to income shocks is conflicting. Certainly for disadvantaged groups, the income transfer is a larger portion of their income and so one could anticipate that they would be more sensitive. Nonetheless, except for recent work, disadvantaged people did not alter their fertility in the face of economic changes, therefore our hypothesis is:

*Hypothesis 3:* Additional income will have a smaller effect on socio-economically disadvantaged populations’ fertility than advantaged populations’ fertility.

If hypothesis 3 is correct then it will be evident in either no effect on disadvantaged population’s fertility or a smaller effect relative to advantaged populations.

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
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.

2 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 a parent or legal guardian. There are no low-income requirements as with welfare (Currie and Cole, 1993; Moffitt,

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1People sentenced or incarcerated for a felony during the year are excluded, as well as people with extensive criminal records.
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 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.

2.1 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 size of the dividend is determined by a formula specified in state law in which 10.5% of the APF’s past five fiscal years’ realized net income is withdrawn for the Permanent Fund Dividend (PFD). Operating costs and appropriations are then deducted. These are minimal. The remainder is then divided among and paid out to all permanent Alaska residents in the form of an individual PFD payment (Erickson and Groh, 2012; noa, 2013).

The remaining revenue is 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 have been 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 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.

2.2 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 we argue above that 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 price of oil in all models and separately include per capita income and the Alaskan unemployment rate 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.

2.3 Repeated Natural Experiment

We conceptualize the PFD payments as a repeated natural experiment. The repeated nature of the treatment requires careful identification of the appropriate counterfactual. We argue that once the dividend begins, no other state or group of states can serve as a proper counterfactual for continually treated Alaskans. Likewise,
we do not believe it is appropriate to include Alaska before the start of the program as a control for analyses that assess long periods of treatment, as ours does. Simply put, Alaskans in 1980 are not a suitable control for Alaskans in 1995, for instance, after 13 years of continuous, varying treatments. Because of this, we argue for an internal Alaska comparison over time to an external control group or pre-treatment control. We model Alaskans in years when the dividend is low to serve as a control group for years when the dividend is high. Our causal claims for this analysis therefore rest on two features: First, that the amount of the dividend is unrelated to individual Alaskan residents’ behavior (i.e., it is exogenous) and, second, that the amount varies year to year. Using the language of medical randomized experiments, since 1982, the Alaskan population has been “treated” every year to a “dose” of income, and the “dosage” varies year to year.

Though we contend that external controls and Alaskans prior to 1982 are not appropriate counterfactuals, we appreciate that others will disagree (e.g. see Chung et al. (2016); Jones and Marinescu (2018)). Further, though the PFD has garnered more scholarly attention of late (Evans and Moore, 2011; Watson et al., 2019a; Dorsett, 2019; Watson et al., 2019b), it has been exploited to understand such wide-ranging processes as mortality after the dividend receipt to childhood obesity. No consensus on how to identify the treated and control populations has developed. Given this, we conduct a sensitivity analysis in which we include pre-treatment years beginning in 1980; this yields substantively similar results. Further, other scholars have very recently assessed the effect of PFD payments on fertility in a working paper and used both pre-treatment years and external controls through a synthetic control approach. Though this approach cannot take into account variation in the dividend payments over time - a benefit of our approach - we note that they also find that payments increase fertility (Yonzan et al., 2020).

2.4 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.
2.5 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 three 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 scale 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 UBI. Third, in contemporary proposals the cash transfer is made periodically, often monthly. The PFD, however, is given yearly as a lump sum, a distinction that individuals are sensitive to (Benartzi and Thaler, 2013; Warner and Pleeter, 2001; Beshears et al., 2014).

3 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 shorter and longer windows, discussed below. The fertility-response window may continue through early pregnancy, depending on access to abortion. We examine this possibility in our supplementary analyses (see Appendix Table 11) and find that the abortion rate is not affected by the size of the dividend, likely because Medicaid covered abortion in Alaska during our entire study period.

3.1 Fertility Response: Birth Rate

To test hypotheses 1 and 2, we analyze groups defined by five 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.

Following standard sociological practices for studying fertility, the birth-rate model is a log-rate model that considers how the cash transfer affects birth rates one year and two years later (Powers and Xie, 2008). We estimate the log-rate model using negative binomial regression with an offset term equal to the logged exposure, or population of women in a given DG. We use DGs as the unit of analysis because the log-rate model assumes constant rates within each unit. Grouping women by detailed demographic subgroups makes this assumption more tenable than using larger groups, such as grouping by age alone. To be clear, this model estimates the average treatment effect and does not estimate effects by DG.

Our model is thus:

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

where \(j\) indicates DG 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 DG 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 DG; \(^2\) \(OIL_{t-2}\) indicates the crude price of oil at \(t-2\) in $2010; \(US_t\) is the U.S. birth rate in year \(t\); \(YEAR_t\) a linear time trend; and \(\epsilon_{jt}\) is the error term.

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.

While the main rate model estimates an average treatment effect for the entire population, we next test for heterogeneous treatment effects across major demographic subgroups (marital status, race, education, age and parity). 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.

### 3.2 Sensitivity Analyses for the Fertility Effects

We perform five 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.

\(^2\) Instead of including the characteristics that make up Demographic Groupings (DGs) as controls in the model, another approach is to include DG fixed effects in the model. Doing so yields substantively similar results. We chose the approach using covariates for our main analyses so we can estimate heterogeneous treatment effects across subgroups (e.g., married people, second births, people with some college) using interactions.
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 were generated 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 a second placebo test in which next year’s dividend payment is used to predict this year’s fertility. Future cash payments should not affect past fertility, so this analysis provides an additional test of our model and causal claims.

Fourth, 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 do not consumption smoothe in response to the PFD for either durables or non-durables (Kueng (2018) but see also Hsieh (2003)).

In addition to these main five, we conduct numerous other sensitivity analyses as a part of our main analyses. As we mentioned above, we tested the model with DG fixed effects and a pre-treatment period. We also test our models using macro-economic indicators other than just the crude price of oil like the unemployment rate, and we test other windows besides the 24 month window among others. We mention those when relevant below. Our substantive results are robust to all of these analyses.
4 Data

4.1 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 annual average crude price of oil as a control. Sensitivity analyses also include the unemployment rate and per capita income. 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 for these procedures, in the Appendix section “Missing Data.”

---

3 People of all gender identities give birth to children. We do not know the gender identity of people who give birth from the birth certificate data (our numerators). We construct denominators that are counts of people who report their sex as female on the Census or American Community Survey, which also does not ask about gender identity, to capture people at risk of giving birth. Recognizing this, we use gender-neutral language as much as possible. As Darwin and Greenfield (2019) note, “We have not yet developed shared language in research or practice to adequately describe reproductive histories outside of a cis birth mother’s.” For clarity, at times we use the term “women,” though inaccurate, to refer to people at risk of giving birth to clarify that the group to which we refer does not include males.
4.2 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. They have been used in other scholarly work, including one article on the PFD (Kozminski and Baek, 2017).

For our South Dakota 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 (see Appendix Table 10).

5 Results

5.1 Average Treatment Effect

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 ($DIV_{t-1} \text{ IRR} = 1.018; DIV_{t-2} \text{ IRR} = 1.019$; 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 $t$.

Figure 2 examines at what level of transfer significant fertility increases occur for various household
sizes. That is, it answers the question: what is the minimum payment required for a fertility increase? For each household size, fertility is predicted at the minimum and maximum payment amounts received by the household (e.g., $625 and $3,366 for households of one) as well as at each $1,000 increment in between. Payments at $t-1$ and $t-2$ are set to be equal, so the predicted rate at $2,000, for instance, represents predicted fertility if payments in both $t-1$ and $t-2$ were $2,000. To establish an effect threshold, we assess at what payment level predicted fertility is significantly higher than predicted fertility for the lowest payment amount for that household size. We use the lowest payment as the baseline because we do not include years with no dividend payments in our analysis. We graph a horizontal line to indicate the baseline predicted fertility. For households of size one and two, fertility significantly increases at transfer levels of $3,000 to the household and higher. For households of size three, the effect threshold is $4,000. Larger payments are required in larger households for an effect to occur.

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.

5.1.1 Sensitivity Analyses for the Average Treatment Effect

We now present results from a series of sensitivity analyses. The results from the main model are robust to all of them.

First, we consider different treatments of time. 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.

Next, we turn our attention to the response window. 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 Table 9 for full results). In addition, we test other macro-economic indicators other than the crude price of oil in Appendix Table 5, and our results are unchanged. Further, results for Anchorage alone are confirmatory and show similar positive effects on fertility (see Appendix Table 6).

Two placebo tests further validate our results. In Table 3, we extend our analyses from Table 2 and present results from our South Dakota 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.

Our second placebo test assesses the effect of dividend payments in $t+1$ on fertility in $t$. As shown in Appendix Table 4, future payments do not predict past fertility.

Finally, we also examined whether the effects of the payments on fertility were non-linear. Analyses revealed that quadratic terms for dividend payments at $t-1$ and $t-2$ are significant such that the positive effect of the payments on fertility increases at larger payment amounts, but the curve is slight (see Figure 4 in Appendix). Because of this, we opt for the simpler linear model as our main model.

### 5.2 Heterogeneous Treatment Effects

Since the dividend payments increase birth rates, we next explore whether this increase occurs heterogeneously across demographic groups. Figure 3 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 than unmarried women 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 more so than their advantaged peers. This confirms, over a longer timeframe, recent findings regarding the fertility response to the Great Recession.

6 Discussion and Conclusion

We contribute to the scholarly effort to identify the relationship between income and fertility and to examine the fertility consequences of income policy. In doing so, we also help illuminate the potential effects of a massive income transfer proposed by UBI advocates. We do so by examining a uniquely strong quasi-natural experiment, the Alaska PFD income transfers.

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 individual 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.

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.

The Alaskan case provides the best opportunity we have to study the effects of an income policy that is universally given, not targeted to specific populations. 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, controls for the macro-economy, varying model specifications, and two placebo tests. 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 reflect childbearing postponement or acceleration but may also leave an imprint on completed family size.

6.1 Limitations

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.

Since 1982, when the PFD was implemented, Medicaid, Title X and other maternal and child health services have changed dramatically both in terms of who is eligible and how care is managed. This would be a threat to our causal estimates if those changes were correlated with the size of the dividend due to some factor underlying them both. These changes were primarily on the federal level and therefore we suspect their implementation would not be correlated with the size of the dividend. At the state level, Medicaid in Alaska has always covered abortion, so this has not changed over the course of the study period.

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.

6.2 Policy Implications

We find that additional income enables an increase in fertility, particularly among disadvantaged people. For policymakers who are concerned about increasing fertility among disadvantaged people, potentially increasing the population of those dependent on government assistance, this would be troublesome. For those who are concerned about reproductive autonomy, inequality, and population aging, this would be beneficial.

To those who might find it troublesome, we note that most families do not have more children with the increase in the dividend but likely all the families are better-off due to the additional income. For those disadvantaged families who do have additional children, we interpret this as an indication that they faced an economic barrier to having children prior to the payments. 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 instantiation 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 bearing children and raising them in healthy environments. This also confers with the description of “demand for children” prominent in economics articulated by Gary Becker who wrote that “‘demand’ means the number of children desired when there are no obstacles to the production or prevention of children” (Becker, 2009).

The contention that higher fertility among impoverished individuals is a chief cause of poverty was once quite prominent and unfortunately undergirded policies that violated personal autonomy, such as forced sterilizations and mandated contraception, among others (Ross and Solinger, 2017; Roberts, 1999; Gordon, 2002). The solution to poverty, including intergenerational poverty (Cheng and Song, 2019; Torche, 2015), should not be found in limiting fertility. Families face burdens, including poor working conditions (Hepburn, 2019), segregated schools (Johnson, 2011), incarcerated fathers (Geller et al., 2011), high childcare costs (Han and Waldfogel, 2001; Ruppanner et al., 2019), among many others. Policies that alleviate these burdens could disrupt processes of poverty and inequality (Waldfogel, 2008) and do so without impinging on the very personal choice and reproductive right to decide when and whether to have children.

The PFD is an example of a universal cash transfer and is the closest the United States comes to a universal basic income. Recently, mayors of 11 major American cities announced their support for basic income, and some have instituted ones of various generousies and targeted populations (Tubbs et al., 2020). Nevada is debating a similar program as the PFD for their mining income (Sadler, 2020). In the coming years,
as these and other ongoing American basic income programs and experiments (Martin-West et al., 2018; Rhodes, 2020) are evaluated, we will be able to continue to test the relationship between these income policies and fertility. 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 and increase fertility overall.
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7 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>
<th>Measure</th>
<th>1980</th>
<th>1990</th>
<th>2000</th>
<th>2010</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>U.S.</td>
<td>Alaska</td>
<td>U.S.</td>
<td>Alaska</td>
</tr>
<tr>
<td>Educational attainment (%)</td>
<td></td>
<td></td>
<td></td>
<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>
</tr>
<tr>
<td>College Degree or higher</td>
<td>16.2</td>
<td>21.1</td>
<td>20.3</td>
<td>23.0</td>
</tr>
<tr>
<td>Median Age (years)</td>
<td>30.0</td>
<td>26.0</td>
<td>32.9</td>
<td>29.4</td>
</tr>
<tr>
<td>Hispanic (%)</td>
<td>6.4</td>
<td>2.4</td>
<td>9.0</td>
<td>3.2</td>
</tr>
<tr>
<td>Race (%)</td>
<td>83.2</td>
<td>77.1</td>
<td>80.3</td>
<td>75.5</td>
</tr>
<tr>
<td>White</td>
<td>6.7</td>
<td>2.4</td>
<td>9.0</td>
<td>3.2</td>
</tr>
<tr>
<td>Black</td>
<td>0.7</td>
<td>15.9</td>
<td>0.8</td>
<td>15.6</td>
</tr>
<tr>
<td>American Indian or Alaska Native</td>
<td>0.7</td>
<td>15.9</td>
<td>0.8</td>
<td>15.6</td>
</tr>
<tr>
<td>Asian or Pacific Islander</td>
<td>1.6</td>
<td>1.9</td>
<td>2.9</td>
<td>3.6</td>
</tr>
<tr>
<td>Other Race or Multiracial</td>
<td>3.9</td>
<td>1.6</td>
<td>3.9</td>
<td>1.2</td>
</tr>
<tr>
<td>Median Household Income (dollars)</td>
<td>60,841</td>
<td>25,414</td>
<td>29,943</td>
<td>39,298</td>
</tr>
<tr>
<td>Poverty (%)</td>
<td>12.0</td>
<td>10.7</td>
<td>10.0</td>
<td>9.0</td>
</tr>
<tr>
<td>Urban (%)</td>
<td>73.7</td>
<td>64.4</td>
<td>80.0</td>
<td>67.5</td>
</tr>
<tr>
<td>Foreign-Born (%)</td>
<td>6.2</td>
<td>4.0</td>
<td>8.0</td>
<td>4.5</td>
</tr>
<tr>
<td>Children Living with a Single Parent (%)</td>
<td>19.7</td>
<td>19.3</td>
<td>24.7</td>
<td>20.0</td>
</tr>
<tr>
<td>Female (%)</td>
<td>51.4</td>
<td>47.0</td>
<td>51.3</td>
<td>47.3</td>
</tr>
<tr>
<td>Fertility Rate (f, g, h)</td>
<td>68.4</td>
<td>88.6</td>
<td>70.9</td>
<td>86.3</td>
</tr>
<tr>
<td>Pre-Term Births (%)</td>
<td>8.9</td>
<td>7.6</td>
<td>10.6</td>
<td>8.8</td>
</tr>
<tr>
<td>Low Birth Weight Births (%)</td>
<td>6.8</td>
<td>5.4</td>
<td>7.0</td>
<td>4.7</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></td>
<td>IRR</td>
<td>C.I.</td>
</tr>
<tr>
<td>$DIV_{t-1}$</td>
<td>1.016** (1.006 , 1.026)</td>
<td>1.017** (1.007 , 1.027)</td>
</tr>
<tr>
<td>$DIV_{t-2}$</td>
<td>1.019*** (1.009 , 1.029)</td>
<td>1.024*** (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 average crude price of oil lagged two years.
Figure 2: Examination of Threshold for Fertility Effects by Household Size. Each line plots predicted fertility rate from minimum to maximum dividend payment received for each household size as well as each $1,000 increment in between. Horizontal line indicates upper bound of 95% confidence interval for estimated fertility at lowest payment amount. Threshold for effect is payment amount at which predicted fertility exceeds the horizontal line. All other covariates set to mean.
<table>
<thead>
<tr>
<th></th>
<th>Household Payment</th>
<th>Individual Payment</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Model 4: South Dakota Rate</td>
<td>Model 5: South Dakota Rate</td>
</tr>
<tr>
<td></td>
<td>IRR</td>
<td>C.I.</td>
</tr>
<tr>
<td></td>
<td>IRR</td>
<td>C.I.</td>
</tr>
<tr>
<td>$DIV_{t-1}$</td>
<td>1.004 (.992, 1.015)</td>
<td>.997 (.962, 1.034)</td>
</tr>
<tr>
<td>$DIV_{t-2}$</td>
<td>1.012 (.9997, 1.023)</td>
<td>1.021 (.983, 1.062)</td>
</tr>
</tbody>
</table>

Notes:
(3) N = 11,338 Demographic Groupings; 277,406 births.
(4) IRR = Incidence Rate Ratios.
(5) *p<.05; ** p<.01; *** p<.001
(6) Dividend is in 2010 constant dollars. It is measured in $1,000 units.
(7) Unit of analysis is Demographic Groupings - demographic groups of women determined by age, race, marital status, educational attainment, and parity.
(8) Controls are by age, race, marital status, educational attainment, parity, year, the U.S. birth rate, and the average price of crude oil lagged two years.
Table 4: Birth Rate Analysis Results: Placebo Test for Dividend at t+1

<table>
<thead>
<tr>
<th>Covariate</th>
<th>IRR</th>
<th>95% C.I.</th>
</tr>
</thead>
<tbody>
<tr>
<td>DIV_{t+1}</td>
<td>1.009</td>
<td>(0.999 , 1.019)</td>
</tr>
<tr>
<td>DIV_{t-1}</td>
<td>1.013**</td>
<td>(1.003 , 1.024)</td>
</tr>
<tr>
<td>DIV_{t-2}</td>
<td>1.024***</td>
<td>(1.012 , 1.035)</td>
</tr>
</tbody>
</table>

Notes:
4. + p < .10; *p < .05; ** p < .01; *** p < .001
5. Controls are: Year (aligned to APF dividend disbursement), race, marital status, age, maternal education, parity, average price of crude oil 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.
Figure 3: 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 = 240,285).
8 Appendix
8.1 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. Multiple imputation was not feasible because the natality data do not contain enough parental characteristics to accurately predict missing values. To address missing data, we employed a threshold deletion strategy. Specifically, we chose a threshold of 7.5 percent and excluded from our analysis any year in which more than 7.5 percent of births had missing values on one or more covariates. Based on this threshold, we excluded 2001, 2003, and 2008 from our analyses. For all other years, we dropped all cases with missing values on covariates and then randomly dropped more observations until the percentage dropped reached 7.5 percent. That is, for all included years, exactly 7.5 percent of cases were dropped. We use this approach to ensure that the total number of births per year is not impacted by different rates of missing data across years. Because the rate model assesses changes in the number of births per year relative to the total number of women at risk of giving birth, we must be attentive to any data manipulations that alter the birth counts in some years and not others, as such changes could induce an artificial effect on birth rates. The threshold-deletion strategy ensures that birth counts for each year are artificially reduced by the same proportional amount. As a sensitivity analysis, we also tested alternative thresholds – 6 percent and 9 percent – and our results were not substantively altered.
<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.
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>Household Payment</th>
<th>Model 1: Rate</th>
<th>Model 2: Rate w/ Decade F.E.</th>
<th>Individual Payment</th>
<th>Model 4: Rate</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>IRR</td>
<td>C.I.</td>
<td>IRR</td>
<td>C.I.</td>
</tr>
<tr>
<td>DIV$_{t-1}$</td>
<td>1.014*</td>
<td>(1.001, 1.027)</td>
<td>1.009 (0.995, 1.023)</td>
<td></td>
</tr>
<tr>
<td>DIV$_{t-2}$</td>
<td>1.023**</td>
<td>(1.009, 1.037)</td>
<td>1.027*** (1.013, 1.041)</td>
<td>1.019 (0.978, 1.061)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>1.056* (1.012, 1.102)</td>
</tr>
</tbody>
</table>

Notes:
(3) Anchorage N = 9,168 Demographic Groupings; 93,231 births.
(4) IRR = Incidence Rate Ratios.
(5) * p < .05; ** p < .01; *** p < .001
(6) Dividend is in 2010 constant dollars. It is measured in $1,000 units.
(7) Unit of analysis is Demographic Groupings - demographic groups of women determined by age, race, marital status, educational attainment, and parity.
(8) Controls are by age, race, marital status, educational attainment, parity, year, the U.S. birth rate, and the average crude price of oil 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 (%)&lt;sup&gt;a&lt;/sup&gt;</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)&lt;sup&gt;a&lt;/sup&gt;</td>
<td>26.0</td>
<td>28.9</td>
<td>33.8</td>
<td>36.9</td>
</tr>
<tr>
<td>Hispanic (%)&lt;sup&gt;a&lt;/sup&gt;</td>
<td>2.4</td>
<td>0.1</td>
<td>5.5</td>
<td>2.7</td>
</tr>
<tr>
<td>Race (%)&lt;sup&gt;a,b&lt;/sup&gt;</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)&lt;sup&gt;c&lt;/sup&gt;</td>
<td>25,414</td>
<td>13,156</td>
<td>57,848</td>
<td>45,352</td>
</tr>
<tr>
<td>Poverty (%)&lt;sup&gt;c,d&lt;/sup&gt;</td>
<td>10.7</td>
<td>16.9</td>
<td>9.9</td>
<td>14.4</td>
</tr>
<tr>
<td>Urban (%)&lt;sup&gt;a,e&lt;/sup&gt;</td>
<td>64.4</td>
<td>46.4</td>
<td>66.0</td>
<td>55.3</td>
</tr>
<tr>
<td>Foreign-Born (%)&lt;sup&gt;a,d&lt;/sup&gt;</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 (%)&lt;sup&gt;c&lt;/sup&gt;</td>
<td>19.3</td>
<td>13.6</td>
<td>21.7</td>
<td>27.6</td>
</tr>
<tr>
<td>Female (%)&lt;sup&gt;a&lt;/sup&gt;</td>
<td>47.0</td>
<td>50.7</td>
<td>47.9</td>
<td>49.5</td>
</tr>
<tr>
<td>Fertility Rate&lt;sup&gt;f,g,h&lt;/sup&gt;</td>
<td>90.5</td>
<td>88.3</td>
<td>80.1</td>
<td>77.3</td>
</tr>
<tr>
<td>Pre-Term Births (%)&lt;sup&gt;f,g&lt;/sup&gt;</td>
<td>7.6</td>
<td>6.0</td>
<td>9.7</td>
<td>11.4</td>
</tr>
<tr>
<td>Low Birth Weight Births (%)&lt;sup&gt;f,g&lt;/sup&gt;</td>
<td>5.4</td>
<td>5.1</td>
<td>5.7</td>
<td>6.8</td>
</tr>
</tbody>
</table>

Notes:

[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.
[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.
8.2 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>Coefficient</th>
<th>S.E.</th>
<th>IRR</th>
<th>95% C.I.</th>
</tr>
</thead>
<tbody>
<tr>
<td>DIV$_{t-1}$ (Thous.)</td>
<td>.016**</td>
<td>.005</td>
<td>1.016**</td>
<td>(1.006 , 1.026)</td>
</tr>
<tr>
<td>DIV$_{t-2}$ (Thous.)</td>
<td>.019***</td>
<td>.005</td>
<td>1.019***</td>
<td>(1.009 , 1.029)</td>
</tr>
<tr>
<td>Year</td>
<td>.010***</td>
<td>.001</td>
<td>1.011***</td>
<td>(1.008 , 1.013)</td>
</tr>
<tr>
<td>Married</td>
<td>.533***</td>
<td>.018</td>
<td>1.704***</td>
<td>(1.644 , 1.765)</td>
</tr>
<tr>
<td>Parity</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Parity 2</td>
<td>.382***</td>
<td>.022</td>
<td>1.466***</td>
<td>(1.404 , 1.531)</td>
</tr>
<tr>
<td>Parity 3</td>
<td>.040</td>
<td>.028</td>
<td>1.041</td>
<td>(.985 , 1.099)</td>
</tr>
<tr>
<td>Parity 4+</td>
<td>.212***</td>
<td>.035</td>
<td>1.237***</td>
<td>(1.155 , 1.324)</td>
</tr>
<tr>
<td>Race</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Alaska Native</td>
<td>.484***</td>
<td>.018</td>
<td>1.622***</td>
<td>(1.565 , 1.682)</td>
</tr>
<tr>
<td>Other Race</td>
<td>-.011</td>
<td>.019</td>
<td>.989</td>
<td>(.953 , 1.026)</td>
</tr>
<tr>
<td>Education</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>High School</td>
<td>-.114***</td>
<td>.021</td>
<td>.892***</td>
<td>(.857 , .929)</td>
</tr>
<tr>
<td>Some College</td>
<td>-.324***</td>
<td>.022</td>
<td>.724***</td>
<td>(.693 , .755)</td>
</tr>
<tr>
<td>Bachelor’s or more</td>
<td>-.085**</td>
<td>.026</td>
<td>.919**</td>
<td>(.873 , .966)</td>
</tr>
<tr>
<td>Age</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>20-24</td>
<td>.145***</td>
<td>.031</td>
<td>1.156***</td>
<td>(1.089 , 1.228)</td>
</tr>
<tr>
<td>25-29</td>
<td>-.106**</td>
<td>.031</td>
<td>.900**</td>
<td>(.847 , .956)</td>
</tr>
<tr>
<td>30-34</td>
<td>-.594***</td>
<td>.242</td>
<td>.552***</td>
<td>(.519 , .587)</td>
</tr>
<tr>
<td>35-39</td>
<td>-1.419***</td>
<td>.033</td>
<td>.242***</td>
<td>(.227 , .258)</td>
</tr>
<tr>
<td>40-44</td>
<td>-2.946***</td>
<td>.037</td>
<td>.053***</td>
<td>(.049 , .056)</td>
</tr>
<tr>
<td>US. Birth Rate</td>
<td>.037***</td>
<td>.004</td>
<td>1.038***</td>
<td>(1.029 , 1.046)</td>
</tr>
<tr>
<td>Crude Oil Price$_{t-2}$</td>
<td>-.000</td>
<td>.000</td>
<td>1.000</td>
<td>(.999 , 1.000)</td>
</tr>
<tr>
<td>Constant</td>
<td>-25.840***</td>
<td>2.891</td>
<td>.000***</td>
<td>(.000 , .000)</td>
</tr>
</tbody>
</table>

Notes:
(2) Population count sources: 1980-2000 Decennial Censuses
   and 2008-2012 American Community Survey.
(3) Total N = 11,696 category IDs; 240,285 births.
(4) *p<.05; ** p<.01; *** p<.001
(5) Reference groups are: Parity 1, Non-Hispanic White,
    Less than High School, and Age 15-19.
(6) Dividend is in 2010 constant dollars and adjusted
    for household size. It is measured in $1,000 units.
Table 9: Birth Rate Analysis with Varying Dividend Time Lags: Log-Rate Model Results

<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>1.005</td>
<td>(.988 , 1.0238)</td>
</tr>
<tr>
<td>$DIV_{t-1}$</td>
<td>1.013+</td>
<td>(.999 , 1.027)</td>
</tr>
<tr>
<td>$DIV_{t-2}$</td>
<td>1.019***</td>
<td>(1.009 , 1.030)</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>1.016**</td>
<td>(1.006 , 1.026)</td>
</tr>
<tr>
<td>$DIV_{t-2}$</td>
<td>1.019***</td>
<td>(1.009 , 1.029)</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>1.012**</td>
<td>(1.001 , 1.022)</td>
</tr>
<tr>
<td>$DIV_{t-2}$</td>
<td>1.025***</td>
<td>(1.012 , 1.038)</td>
</tr>
<tr>
<td>$DIV_{t-3}$</td>
<td>.995</td>
<td>(.981 , 1.009)</td>
</tr>
</tbody>
</table>

Notes:
(3) Total N = 11,696 Demographic Groupings; 240,285 births.
(4) + p<.10; *p<.05; ** p<.01; *** p<.001
(5) Controls are: Year (aligned to APF dividend disbursement), race, marital status, age, maternal education, parity, average price of crude oil 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.
<table>
<thead>
<tr>
<th>Control Rate:</th>
<th>U.S.</th>
<th>South Dakota</th>
<th>New Mexico</th>
<th>Oklahoma</th>
<th>Utah</th>
</tr>
</thead>
<tbody>
<tr>
<td>(DIV_{t-1})</td>
<td>1.016** (2.24)</td>
<td>1.047*** (6.00)</td>
<td>1.048*** (7.36)</td>
<td>1.017** (2.11)</td>
<td>1.012** (2.28)</td>
</tr>
<tr>
<td>(DIV_{t-2})</td>
<td>1.019*** (3.24)</td>
<td>0.999 (0.65)</td>
<td>1.016** (3.23)</td>
<td>1.009 (1.80)</td>
<td>1.009 (1.71)</td>
</tr>
<tr>
<td>Year</td>
<td>1.011*** (7.31)</td>
<td>1.018*** (8.17)</td>
<td>1.034*** (9.83)</td>
<td>1.008*** (5.18)</td>
<td>1.008*** (4.56)</td>
</tr>
<tr>
<td>Married</td>
<td>1.704*** (29.31)</td>
<td>1.675*** (24.98)</td>
<td>1.631*** (29.95)</td>
<td>1.734*** (29.95)</td>
<td>1.737*** (29.99)</td>
</tr>
<tr>
<td>Parity 2</td>
<td>1.466*** (17.33)</td>
<td>1.443*** (16.06)</td>
<td>1.405*** (14.63)</td>
<td>1.492*** (18.03)</td>
<td>1.495*** (18.08)</td>
</tr>
<tr>
<td>Parity 3</td>
<td>1.041 (1.42)</td>
<td>1.010 (0.34)</td>
<td>0.957 (1.41)</td>
<td>1.080** (2.73)</td>
<td>1.083** (2.82)</td>
</tr>
<tr>
<td>Parity 4+</td>
<td>1.237*** (6.08)</td>
<td>1.183*** (4.29)</td>
<td>1.088* (2.05)</td>
<td>1.310*** (7.61)</td>
<td>1.316*** (7.70)</td>
</tr>
<tr>
<td>Other Race</td>
<td>0.989 (0.60)</td>
<td>0.991 (0.50)</td>
<td>0.990 (0.50)</td>
<td>0.991 (0.50)</td>
<td>0.991 (0.50)</td>
</tr>
<tr>
<td>Education</td>
<td>0.892*** (-5.52)</td>
<td>0.892*** (-5.50)</td>
<td>0.893*** (-5.51)</td>
<td>0.893*** (-5.45)</td>
<td>0.893*** (-5.45)</td>
</tr>
<tr>
<td>High School</td>
<td>0.724*** (-14.73)</td>
<td>0.721*** (-14.87)</td>
<td>0.722*** (-14.81)</td>
<td>0.722*** (-14.81)</td>
<td>0.722*** (-14.80)</td>
</tr>
<tr>
<td>Bachelor's or More</td>
<td>0.919*** (-3.31)</td>
<td>0.917*** (-3.37)</td>
<td>0.918*** (-3.31)</td>
<td>0.916*** (-3.42)</td>
<td>0.916*** (-3.42)</td>
</tr>
<tr>
<td>Age 20-24</td>
<td>1.156*** (4.72)</td>
<td>1.156*** (4.71)</td>
<td>1.156*** (4.71)</td>
<td>1.156*** (4.70)</td>
<td>1.156*** (4.71)</td>
</tr>
<tr>
<td>Age 25-29</td>
<td>0.900*** (-3.43)</td>
<td>0.900*** (-3.41)</td>
<td>0.900*** (-3.43)</td>
<td>0.900*** (-3.37)</td>
<td>0.900*** (-3.37)</td>
</tr>
<tr>
<td>Age 30-34</td>
<td>0.552*** (-18.83)</td>
<td>0.552*** (-18.78)</td>
<td>0.552*** (-18.81)</td>
<td>0.553*** (-18.75)</td>
<td>0.553*** (-18.75)</td>
</tr>
<tr>
<td>Age 35-39</td>
<td>0.242*** (-43.45)</td>
<td>0.242*** (-43.35)</td>
<td>0.242*** (-43.41)</td>
<td>0.242*** (-43.29)</td>
<td>0.242*** (-43.29)</td>
</tr>
<tr>
<td>Age 40-44</td>
<td>0.0525*** (-79.29)</td>
<td>0.0526*** (-79.14)</td>
<td>0.0526*** (-79.24)</td>
<td>0.0525*** (-79.09)</td>
<td>0.0525*** (-79.09)</td>
</tr>
<tr>
<td>Crude Oil Price</td>
<td>1.000 (-0.46)</td>
<td>0.997*** (-4.25)</td>
<td>0.997*** (-5.55)</td>
<td>1.001 (1.32)</td>
<td>1.001 (1.66)</td>
</tr>
<tr>
<td>U.S. Birth Rate</td>
<td>1.038*** (8.84)</td>
<td>1.021*** (5.31)</td>
<td>1.021*** (5.31)</td>
<td>1.021*** (5.31)</td>
<td>1.021*** (5.31)</td>
</tr>
<tr>
<td>SD Birth Rate</td>
<td>1.044*** (7.89)</td>
<td>1.044*** (7.89)</td>
<td>1.044*** (7.89)</td>
<td>1.044*** (7.89)</td>
<td>1.044*** (7.89)</td>
</tr>
<tr>
<td>NM Birth Rate</td>
<td>0.997 (0.78)</td>
<td>0.997 (0.78)</td>
<td>0.997 (0.78)</td>
<td>0.997 (0.78)</td>
<td>0.997 (0.78)</td>
</tr>
<tr>
<td>OK Birth Rate</td>
<td>0.997 (0.78)</td>
<td>0.997 (0.78)</td>
<td>0.997 (0.78)</td>
<td>0.997 (0.78)</td>
<td>0.997 (0.78)</td>
</tr>
<tr>
<td>UT Birth Rate</td>
<td>1.000 (-1.24)</td>
<td>1.000 (-1.24)</td>
<td>1.000 (-1.24)</td>
<td>1.000 (-1.24)</td>
<td>1.000 (-1.24)</td>
</tr>
<tr>
<td>N</td>
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<td>11696</td>
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</tr>
<tr>
<td>AIC</td>
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<td>70491.2</td>
<td>70457.4</td>
<td>70518.6</td>
<td>70517.7</td>
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<tr>
<td>BIC</td>
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<td>70645.9</td>
<td>70612.1</td>
<td>70673.3</td>
<td>70672.4</td>
</tr>
</tbody>
</table>

Notes:
(3) Total N = 11,696 Demographic Groupings; 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.
Figure 4: Predicted Fertility Rate for Quadratic Model. Predictions generated from a model that includes dividend payments at $t-1$ and $t-2$ as well as squared terms for each payment. All other covariates set to mean.
Table 11: 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></td>
<td></td>
</tr>
<tr>
<td>(DIV_t)</td>
<td>.001</td>
<td>(-.007 , .009)</td>
</tr>
<tr>
<td>Lags 1, 2; (N = 28) years</td>
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<td></td>
</tr>
<tr>
<td>(DIV_{t-1})</td>
<td>-.005</td>
<td>(-.013 , .003)</td>
</tr>
<tr>
<td>(DIV_{t-2})</td>
<td>.007</td>
<td>(-.002 , .016)</td>
</tr>
<tr>
<td>Lags 1, 2, 3; (N = 28) years</td>
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<td></td>
</tr>
<tr>
<td>(DIV_{t-1})</td>
<td>.002</td>
<td>(-.006 , .010)</td>
</tr>
<tr>
<td>(DIV_{t-2})</td>
<td>-.005</td>
<td>(-.014 , .003)</td>
</tr>
<tr>
<td>(DIV_{t-3})</td>
<td>.007</td>
<td>(-.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.