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Cash Transfers and Voter Turnout*

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Abstract

We estimate the effect of cash transfers on voter turnout, leveraging a large-scale natural experiment, the Alaska Permanent Fund Dividend (PFD) program, which provides residents with a check of varying size one month before election day. We find that larger transfers cause people to vote, especially in gubernatorial elections in which a 10% increase in cash ($182) causes a 1.4 percentage point increase in turnout. Effects are concentrated among racial minorities, the young, and poor. There is little evidence that transfers reduce logistical costs of voting, but rather operate by reducing voter apathy among the low-income electorate.

Keywords: Voter Turnout, Civic Engagement, Cash Transfers, Natural Field Experiment, Democratic Institutions

JEL Classification: D72; H31; H70; I38

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1 Introduction

Voter turnout in the United States is notoriously low and lags behind most other developed countries (OECD, 2019). The low-income electorate is especially unlikely to vote (Rosenstone, 1982; Leighley and Nagler, 1992; Schafer et al., 2021; Markovich and White, 2022).[1] This causes the poor to be under-represented in government (Griffin and Newman, 2005) and has important implications for the determination of public policy and elections that are often decided by slim margins (Fowler, 2013; Hartley, 2020). Election results in key battleground states including Florida, Pennsylvania, and Wisconsin are especially sensitive to voter turnout among the low-income electorate (Hartley, 2020).

Unconditional cash transfers (UCTs) have been championed by some as a vehicle to increase voter turnout by engaging low-income voters (Pateman, 2004; Morales, 2018). The theoretical argument is persuasive. People under financial distress become preoccupied with their personal and immediate well-being and so withdraw from less pertinent matters of politics and elections (Rosenstone, 1982). Economic adversity can keep even civically-minded people away from the polls if they lack adequate child care, health care, transportation, time off work, or other costly resources necessary to vote.

Could a UCT program be used to increase turnout in the United States? Existing empirical work is suggestive, but the reality is that UCTs are extremely rare. Existing studies almost exclusively estimate effects of conditional and means-tested cash-transfer programs on turnout in developing countries including Colombia (Conover et al., 2020), Mexico (De La O, 2013), Brazil (Araújo, 2021), and Honduras (Galiani et al., 2019). A related literature has leveraged natural field experiments in the U.S. to estimate the effect of more general income windfalls on voter turnout. For instance, increasing the minimum wage increased voter turnout in New York City (Markovich and White, 2022), as did Medicaid expansion in Oregon (Baicker and Finkelstein, 2018). Income is not a panacea for low voter turnout,

[1] Low-income Americans are about twenty percentage points less likely to vote than high-income Americans (Hartley, 2020).

however. Improved economic conditions may have reduced voter turnout in oil boom towns (Charles and Stephens, 2013).[2] Akee et al. (2018) estimate inter-generational effects of a targeted cash transfer program in North Carolina that was financed by the establishment of a casino on the Eastern Cherokee reservation and paid a portion of its profits to all adult tribal members (approx. $4,700 per household) regardless of income or employment status. The transfers had no effect on adult voter participation, but did increase future voting propensity among children from economically disadvantaged households (but not among those from economically advantaged ones).

We estimate the effect of a large-scale, universal cash transfer program on voter turnout by leveraging the Alaska Permanent Fund Dividend (PFD) program which supplies virtually every Alaskan resident with a dividend check of varying size roughly one month before election day. The features of this natural experiment are ideal for identifying the causal effect of universal cash transfers on voter turnout, and offer several advantages over those previously used in the existing literature. First, it is universal. With few exceptions (such as having a felony record) every Alaskan resident receives a PFD check each year regardless of age or income. This is in contrast to the cash transfer programs studied in the aforementioned literature, which are both means-tested and conditional on things like the presence of children, health status, or participation in health or educational programs. Further, the universality of the program, combined with the richness of our data, allows us to estimate effects along distributions of income, race, and education. Second, PFD checks are (with two exceptions) typically distributed the first week of October, roughly one month before general elections are held. This is ideal given that the transfers are large enough to create significant short-term liquidity, but may not generate long-run, more persistent effects (the average PFD size is roughly $\$1,800$ (2020 USD) per person). Third, there is substantial

[2] Charles and Stephens (2013) look at the effect of income and employment on voter turnout using energy booms as an instrument. However, such booms lead to heterogeneous inward and outward intrastate migration (Wilson, 2016; Richter et al., 2018) and cause environmental (Muehlenbachs et al., 2015), social (James and Smith, 2017; Gourley and Madonia, 2018; Cunningham et al., 2020) and institutional (Brollo et al., 2013; James and Rivera, 2022) disruptions that can also affect political engagement. This raises the question of whether the exclusion restriction is fully satisfied in this context.

annual variation in the size of the PFD that reflects the five-year rolling average rate of return on the Permanent Fund, Alaska's sovereign wealth fund (Goldsmith, 2001). The fund is an internationally diversified asset and its performance is not dependent on oil prices or Alaska-specific events. We rely on this feature of the fund to make causal inferences, leveraging variation in PFD size to identify intensive-margin effects, providing useful guidance to policymakers interested in implementing UCT programs.

We guide our empirical analysis with an analytical framework describing the relationship between cash transfers and voter turnout. According to this theory, unearned income necessarily increases voter turnout by reducing logistical costs (e.g., waiting in long lines at the polls) and uncertainty costs (e.g., costs associated with making "wrong" decisions at the polls). We test the core prediction of the model—that unearned income increases a person's propensity to vote—using historical U.S. state-level and individual-level voter-turnout data. Our core set of results is derived from administrative state-level voter turnout data in Presidential, Gubernatorial, and Congressional elections. We then estimate individual-level effects, using data collected by the Current Population Survey. Several important insights emerge. First, increasing the size of a cash transfer increases voter turnout, and effects are especially pronounced in gubernatorial elections in which a 10% increase in cash (about one hundred and eighty dollars, using the mean PFD amount as the baseline) causes a 1.4 percentage point increase in voter turnout. Second, effects are more pronounced among the poor (those with household income less than three times the poverty threshold). Third, effects are homogeneous across gender and education status, but are more pronounced among the young (under the age of fifty) and the non-White electorate. Fourth, consistent with reduced uncertainty costs, cash transfers decrease the percentage of people who decide not to vote due to a lack of interest in politics, or a lack of preference between political candidates.

To our knowledge, we are the first to estimate the causal effect of a universal cash transfer program on voting behavior. In doing so, we add to a few different, and individually sizeable, bodies of literature. Most directly, we contribute to a robust economics and political science literature on the determinants of democratic participation, and voter turnout in particular. Whereas economic conditions have received the lion's share of attention in this literature (e.g., Rosenstone (1982); Leighley and Nagler (1992); Charles and Stephens (2013); Schafer et al. (2021); Markovich and White (2022)), other factors also matter, including but not limited to access to television (Gentzkow, 2006), genetics (Aarøe et al., 2021), health (Lyon, 2021), social capital (Atkinson and Fowler, 2014), social approval (Gerber et al., 2008; DellaVigna et al., 2016), and various interventions including mailers, phone calls, and canvassing efforts (Green et al., 2013).

We also contribute to economists' understanding of the effects of unconditional cash transfer programs. Previous research has leveraged Alaska's PFD program to estimate the effect of UCTs on crime (Watson et al., 2020), labor market outcomes (Feinberg and Kuehn, 2018; Jones and Marinescu, 2018; Bibler et al., 2019), poverty (Berman, 2018), and charitable giving (List et al., 2021). We add to this set of works by showing that Alaska's PFD encourages voter participation, mostly among racial minorities, the young, and poor.

The remainder of the paper is organized as follows. Section 2 presents a theoretical framework based on Charles and Stephens (2013) that predicts cash transfers unambiguously increase voter turnout. Section 3 describes the history and structure of Alaska's PFD program. We describe the relevant data in Section 4, while Section 5 describes our identification strategy based on a set of continuous double-difference equations. We present our results in Section 6, test their sensitivity in Section 7, and empirically test for mechanisms in Section 8. Section 9 concludes.

2 Conceptual Framework

Our empirical analysis is guided by a conceptual framework presented by Charles and Stephens (2013) that models the decision to vote as a function of both "logistical" and "uncertainty" costs. Logistical costs (C_L) are hurdles that must be overcome in order to vote. Examples include things like transportation, bad weather, and long lines at the polls. Logistical challenges are assumed to be more costly among people working long hours (h) as they have less flexibility in their schedule and may be required to sacrifice wage income in order to vote.[3] We extend the model to include the effect of income. Our intuition is that the opportunity cost of overcoming logistical barriers to voting is lower among the high-income electorate:

$$C_L = (ah - f(y))g(t, k) + \phi, \tag{1}$$

where ϕ is an individual-specific idiosyncratic term, a measures the relative effect of working additional hours, and $g(t, k)$ are logistical challenges specific to location k on election day t (such as long lines or bad weather). The function $f(y)$ is an increasing function of income, y. With the exception of $f(y)$, this expression is identical to that presented by Charles and Stephens (2013).

Uncertainty costs (C_U) manifest from the disutility a person receives from feeling regret, uncertainty, or ambiguity (Matsusaka, 1995; Merlo, 2005; Ghirardato and Katz, 2006; Ashworth, 2007) about their voting decisions. Assuming people are more likely to learn about electoral issues during leisure time, uncertainty cost is modeled as an increasing function of hours worked. We extend this framework by allowing for income to reduce uncertainty costs for a given level of labor input. Our intuition is two fold. First, the high-income electorate has greater access to news and information via internet, smart phones, and newspaper subscriptions. Second, to the extent that voters (correctly or not) give credit to incumbent politicians for favorable economic conditions, this may provide the uncertain electorate with some clarification. Existing evidence suggests voters do indeed (irrationally) give credit to incumbent governors for economic conditions outside of their control (Wolfers et al., 2002).

[3] Logistical costs are likely reduced for people voting by mail. Currently eight states conduct all elections by mail. Oregon has allowed for ballots to be returned by mail since 2000. Washington adopted this policy in 2012 and Colorado did so in 2014. California, Hawaii, Nevada, Utah and Vermont followed suit in 2020. Alaska currently allows for elections to be conducted by mail provided they are not held on the same day as a general, party primary, or municipal election.

Uncertainty costs are given by:

$$C_U = -(1 + bh + m(y))I(e,t,k) + \psi, \tag{2}$$

where $-1 < b < 0$ measures the relative effect of hours worked on uncertainty cost, m maps income into uncertainty cost ($m' > 0$), and ψ is an individual-specific idiosyncratic term. The total amount of published information regarding election e, in year t and location k is given by I.

Income, y, is the sum of earned and unearned income:

$$y = wh + \Omega, \tag{3}$$

where w is the competitive wage rate, and Ω is the size of the unearned cash transfer. A person votes if the net benefit of doing so is positive. Benefits of voting may arise to people who think they have a reasonable chance at affecting an electoral outcome (Filer and Kenny, 1980), feel a sense of duty to vote (Blais et al., 2000), or benefit from pro-social reputation and self respect (Bénabou and Tirole, 2006). For example, DellaVigna et al. (2016) find that being able to "tell others" is one reason people vote, and estimate that eliminating asking about voting would decrease turnout by roughly two percent.

Following Charles and Stephens (2013), let $\omega = \phi + \psi$ be distributed Uniform (0,1). Voter turnout in election e, time t, and place k is then given by

$$V^e_{t,k} = F_\omega(B - (ah - f(wh + \Omega))g(t,k) + (1 + bh + m(wh + \Omega))I(e,t,k)), \tag{4}$$

where F_ω is the cumulative density function of the standard uniform distribution and B is the benefit of voting. Taking the derivative of (4) with respect to Ω shows that cash transfers increase voter turnout by reducing both logistical and uncertainty costs of voting:[4]

$$f'g(t,k) + m'I(e,t,k) \geq 0. \tag{5}$$

Note that, by incorporating income into this framework, the effect of hours worked on voter turnout is ambiguous. Even an exogenous improvement in the wage rate has an ambiguous effect on turnout provided the elasticity of labor supply is positive (as reported by Costa (2000); Fehr and Goette (2007)). This observation highlights a key distinction between the effect of cash transfers and earned income; the former necessarily increases voter turnout, and the latter does not. Therefore, our empirical results should not be interpreted as the effect of income on voter turnout. Rather, they are specific to unearned cash transfers.

3 Alaska's Permanent Fund Dividend Program

Alaska's PFD program pays all eligible Alaskan residents a dividend check, typically the first week of October.[5] The program is unique for a variety of reasons, one being that it is universal and not means tested. Virtually all residents, regardless of age or income, are eligible to receive a dividend check provided they register by the deadline (currently the deadline is the last day of March).

The size of the PFD varies from year to year, reflecting the five-year rolling average rate of return on the Permanent Fund, Alaska's sovereign wealth fund (Goldsmith, 2001).[6]

[4]Under reasonable assumptions, the optimal number of hours worked, h, is decreasing in the size of the cash transfer. However, this would further lower the cost of voting and so would not change the sign of the comparative static. Also, existing research documents rather small short run labor market effects of Alaska's PFD program (Bibler et al., 2019).

[5]To be eligible to receive a PFD, at the time of application a person must: i) have been an Alaskan resident for the entire preceding calendar year, ii) plan to remain an Alaskan resident indefinitely, iii) not claim residency in any other state, and iv) not have been convicted of a felony crime in the preceding year. Eligibility requirements have evolved over time and other requirements apply. For additional details, see: `https://pfd.alaska.gov/eligibility/eligibility-requirements`.

[6]In 2016, low oil prices placed fiscal stress on the State of Alaska. In response, the 2016 PFD was vetoed. Since 2016, the PFD's amount has been determined by ad hoc appropriation by the legislature rather than the formula described. We drop these years from some specifications in our empirical analysis for robustness.

The Fund's portfolio is diversified across a range of international assets and tracks broader market movements rather than oil markets or the Alaskan economy. The establishment and capitalization of the fund was not random and reflects the timing of the Prudhoe Bay oil discovery in 1968. Following the construction of the Trans-Alaska oil pipeline in 1977, oil started to flow and the state found itself awash in a gigantic oil windfall. The oil revenue funded a variety of public goods, the Permanent Fund, the PFD program, as well as the repeal of the state's income tax.[7] This history is important to consider when estimating effects of the PFD program. One cannot easily disentangle the effects of implementing the PFD program from the repeal of the income tax, or the broader influence of resource-based specialization. However, annual variation in the PFD's size is exogenous to the particulars of the Alaska economy.

The first PFD was paid in 1982 and was fixed at $1,000 ($2,681 in 2020 U.S. dollars). Since then, the (real) PFD has ranged from $825 in 1984 to $3,931 in 2008 when an additional $1,200 rebate was offered to help Alaskans manage rising energy costs.[8] Because every Alaskan resident, regardless of age, is eligible for the PFD, larger families receive larger cash transfers. From Table 1 the average PFD size is $1,817 (using 2020 dollars, averaged across even years from 1982 to 2020). This means that on average, five-person Alaskan households have received roughly $9,080 one month before election day since 1982 (throughout our sample, general elections are held between November 2 and November 8). Averaged from 1982 to 2020, the PFD has paid out nearly $1 billion annually to an average population of 574,000 people.

Most residents currently receive their PFDs by direct deposit into residential bank accounts on a single day around the first week of October.[9] PFDs are typically distributed

[7] Alaska remains the only state in the United States without a broad-based state tax, lacking both a sales and personal income tax.

[8] The $1,200 rebate in 2008 was not exogenous, and was in response to relatively high energy prices in the state of Alaska. To the extent that rising energy prices influence turnout (either positively or negatively) including 2008 in our sample may yield biased estimates of the effect of the PFD.

[9] The majority of Alaskan residents (82% as of 2014) receive their checks through direct deposit and the remainder of eligible residents receive their checks through the mail over the following few weeks (Watson et al., 2020).

in early fall, ranging from September 12 (in 2008) to October 12 (in 2004). There are two exceptions to this general pattern: in 1982 the PFD was disbursed on June 14, and in 2020 it was disbursed on July 1 (in response to the COVID-19 pandemic). Descriptive statistics for the PFD are in Table 1. Figure 1 illustrates annual variation in PFD size which we exploit for identification. Note that variation in PFD size is uncorrelated with that of the price of oil using the full sample ($p = 0.548$) and after restricting it to even-numbered election years (p=0.488).

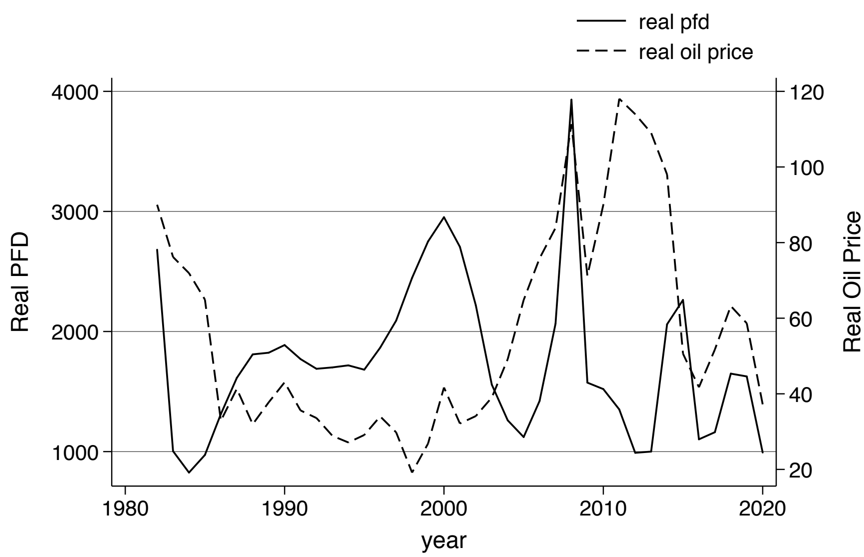


Figure 1: Real PFD Size

Notes: This figure depicts real PFD size and price of imported crude oil (measured using 2020 U.S. dollars) from 1982 until 2020. PFD data collected from the Alaska Department of Revenue:`https://tinyurl.com/mww5jjjw`, oil-price data collected from the Energy Information Administration: `https://www.eia.gov/outlooks/steo/realprices/`. The correlation between PFD size and the oil price is insignificant using the full sample ($p = 0.548$) and after restricting it to even-numbered years ($p = 0.488$).

Beyond the mechanisms discussed in the previous section, there may be "fixed" effects of the PFD program on voter turnout. Eligibility is based on residency and voter registration is one way in which individuals can establish their residency in the state. Because of the PFD program's popularity and reach, it has been a vehicle for voter registration efforts. In 1993, voter registration forms were included with PFD application forms. Since 2017, PFD filing automatically registers voters and updates voter information. Further, one often-cited

Table 1: The Permanent Fund Dividend - Descriptive Statistics

Variable	Mean	Std. Dev.	Min	Max
Nominal PFD	$1,235	$629	$331	$3,269
Real PFD	$1,817	$756	$825	$3,930

Notes: This table displays main descriptive statistics on the Permanent Fund Dividend. Nominal values are converted to real ones using the Consumer Price Index with 2020 as the base year. Only even years between 1982 and 2020 are included.

purpose for the PFD is that it serves to create a political constituency to protect the wealth of the Permanent Fund and its integrity as an institution. As direct beneficiaries of a well-managed financial asset, voters have an incentive to check on both the fund's management board and politicians who might seek to raid its principal. This relationship has become more explicit since 2016 as the state relies on the Fund as a source of revenue for basic agency operations. Much of the political conversation in the State is now dominated by discussions of the PFD's structure and amount.

While we cannot measure these fixed effects of the PFD program, they are likely positive and it is important to acknowledge they may exist. Nonetheless, it is interesting to note that, from 1970 to the implementation of the PFD program in 1982, average turnout in Alaska was nearly identical to the average for all other states. Since 1982, turnout in Alaska is relatively high in Presidential elections (62% vs 56%), Gubernatorial elections (52% vs 44%), and House elections (55% vs 46%). While suggestive of a "fixed" effect on turnout from the existence of the PFD, we caution against making causal inference given simultaneously occurring structural changes to the Alaskan economy.

4 Voter Turnout Data

4.1 State-Level Voter Turnout

We gathered state-level voting data from the CQ Press Voting and Elections Collection on the presidential, senatorial, house, and gubernatorial elections from 1970 to 2018.[10] Along with voting records for the general elections, we also gather information on primary gubernatorial and senatorial elections from the same source. We combine these data with voting-age population estimates from the Population Division of the U.S. Census Bureau by age and year to obtain state-level voter turnout, that is, the total number of votes cast in each of these elections relative to individuals aged 18 years or older.[11] Panel A of Table 2 displays average state-level turnout rates for each of these elections. General-election turnout is similar across gubernatorial, senatorial, and house elections, ranging from 44.8% to 47.5%. Average turnout is relatively high in Presidential elections (56.6%) and low in primary elections (20%).

4.2 Individual-Level Voter Turnout

Data on individual voter turnout come from the Current Population Survey (CPS) Voting and Registration Supplement from 1996 to 2016. The CPS Voting and Registration Supplement is conducted by the U.S. Census Bureau every two years during November to monitor trends in voting behavior of U.S. citizens in terms of their different demographic and economic characteristics. From the raw data, we select individuals aged 18 years old or older and with interviews flagged as fully complete.[12]

Mean reported turnout data for the years 1996 to 2016 are displayed in panel B of Table

[10] CQ Press Voting and Elections data is available here: http://library.cqpress.com/elections/.

[11] Population estimates are reported by age group for the period between 1990 and 1999. To obtain a state-level eligible population for those years, we took two-fifths of the 15-19 years old cohort and added it to the subsequent age groups.

[12] Fully complete computer-assisted telephone interviews (CATI) and computer-assisted personal interviews (CAPI) correspond to an 88.41% of all 1996-2016 interviews.

Table 2: Voter Turnout - Descriptive Statistics

Variable	Mean	Std. Dev.	Obs.
Panel A. State-Level Voter Turnout (CQPress)			
Panel A.1. General Elections			
Presidential	0.566	0.077	500
Gubernatorial	0.448	0.101	524
House	0.461	0.114	1,000
Panel A.2. Primary Elections			
Gubernatorial	0.209	0.083	406
Panel B. Individual-Level Voter Turnout (CPS)			
Overall	0.627	0.483	884,374
Seniors	0.736	0.441	377,820
Females	0.637	0.481	468,105
Married	0.695	0.460	575,051
Non White	0.579	0.481	125,795
Poor	0.505	0.499	233,287

Notes: This table displays main descriptive statistics on state-level voter turnout (panel A) and individual-level voter turnout (panel B). Panel A: Voter turnout is from 1982 to 2018. Panel B: Voter turnout is from 1996 to 2016. Average voter turnout including missing observations equals 0.556. Seniors are people older than fifty years. Poor are people earning less than 1.5 times the poverty threshold.

2 and is reported by basic demographic characteristics. Overall, 62.7% of survey respondents report voting. This is markedly higher than turnout reported in panel A, a difference which is at least partially explained by selection;[13] the roughly ten percent of respondents who did not indicate whether they voted were disproportionately poor, less educated, male, and young—a group of people who are also relatively unlikely to vote.[14,15] We also find that seniors (people older than fifty) and low-income respondents (with household income less than 1.5 times the poverty rate) are significantly less likely to vote than other people.

[13] We tested whether the difference is driven by the difference in sample periods by restricting the state-level administrative data to the same CPS sample period of 1996-2020 or by larger states receiving greater weight in the CPS data (each state receives equal weight using state-level administrative data).

[14] We regressed an indicator equal to unity for respondents who did not indicate whether they voted on observed respondent characteristics. These results are provided in the first column of Table A1.

[15] One may be concerned this type of selection may cause bias in the average treatment effect of the PFD. However, we do not think this is a significant problem given that i) ninety percent of respondents did indicate whether they voted and ii) baseline results drawn from the CPS data are qualitatively similar to those from the state-level administrative data for which selection is not an issue.

5 Empirical Strategy

5.1 Cash Transfers & State-Level Voter Turnout

Alaska PFDs have been paid every fall (typically the first week of October) since 1982. We exploit annual variation in the size of the PFD for identification under the assumption that PFD size is exogenous to differential trends in voter turnout between Alaska and the rest of the country.

We start by estimating the effect of PFD size on state-level voter administrative turnout data. We separately consider three different elections: Presidential, House, and Gubernatorial elections.[16] Alaska is uniquely dependent on oil. This is problematic if oil-price fluctuations are spuriously correlated with PFD size. We therefore condition our estimates on both the annual price of oil, and the annual price of oil interacted with an Alaska indicator variable. In this way, we directly control for the possibility that oil shocks have unique influences on turnout in Alaska. We define voter turnout $V^e_{s,t}$ during election e in state s in year t to be affected by the PFD as follows:

$$V^e_{s,t} = \beta^e_0 + \beta^e_1 \ln(PFD)_{s,t} + \delta^e \text{Oil Price}_t \times 1[AK]_s + \gamma^e_t + \lambda^e_s + \epsilon^e_{s,t}. \tag{6}$$

The variable $\ln(PFD)_{s,t}$ is the natural log of PFD size, measured in 2000 U.S. dollars. Notice that this variable is zero in all years for all states other than Alaska. The variable Oil Price_t is the real first-purchase price of U.S. crude oil during year t and $1[AK]_s$ is an indicator taking 1 if state s is Alaska (=0 otherwise). Year and state fixed effects are given by γ^e_t and λ^e_s, respectively, and $\epsilon^e_{s,t}$ is an idiosyncratic effect.

We estimate Equation (6) using an OLS estimator allowing for heteroskedasticity-robust

[16]Estimating effects within senatorial elections is complicated by the staggered nature of election timing: each state has two senators that face elections every six years. This means that the reference group of states changes from year to year. Additionally, senatorial and house elections are similar in that voters are electing state representatives to a national legislative chamber, but house races are held every other year and thus offer a richer and more balanced data set. For these two reasons, we do not include senate races in our core analysis but do report senate results in Table A2 and Figure A1 in the Appendix.

standard errors. Notwithstanding, standard errors may be correlated within states. Yet, clustering standard errors at the level of treatment (or applying a Wild Cluster Bootstrap) is not feasible given that only one cluster is ever treated (i.e., Alaska), which leads to negatively-biased standard errors and over-rejection of the null hypothesis (Conley and Taber, 2011; MacKinnon and Webb, 2018; Ferman and Pinto, 2019). We therefore also carry out hypothesis testing using Permutation Inference (PI), which does not require the estimation of standard errors.[17]

We carry out permutation inference as follows: after standard estimation of Equation (6), we re-estimate it using every "control" state as the "treatment" state. That is, we substitute the $1[AK]_s$ indicator with an indicator for a given control state, having removed Alaska observations from the sample. Each control state estimation gives us a "placebo" treatment effect, which differs from zero only by random sampling error under the assumption that the PFD size has no impact on a control state's voter turnout. The p-value is then the fraction of absolute value placebo effects greater than or equal to the absolute value effect originally estimated for Alaska, or the estimated probability under the null hypothesis that the true estimate is at least as far from zero as it is due to sampling error alone. Permutation inference does not allow us to evaluate the precision of the estimate but, crucially, does not require any assumptions about the distribution of the error terms. This method has been used in other recent studies involving only a single treated unit, including Baron et al. (2020) and Reimer and Haynie (2018).

5.2 Cash Transfers & Individual-Level Voter Turnout

The state-level analyses described above provides a test of the effect of universal cash transfers on aggregate voter turnout. We also analyze individual-level data (see Section 4) that allow us to estimate heterogeneous treatment effects, and provide insights into the

[17]Ferman and Pinto (2019) and Hagemann (2020) offer alternative inference methods for difference-in-differences estimation with few treated clusters, but these methods apply to binary treatment definitions. Aside from PI, we are unaware of valid inference methods when only a single unit is "treated" but the treatment can vary continuously, as in the present study.

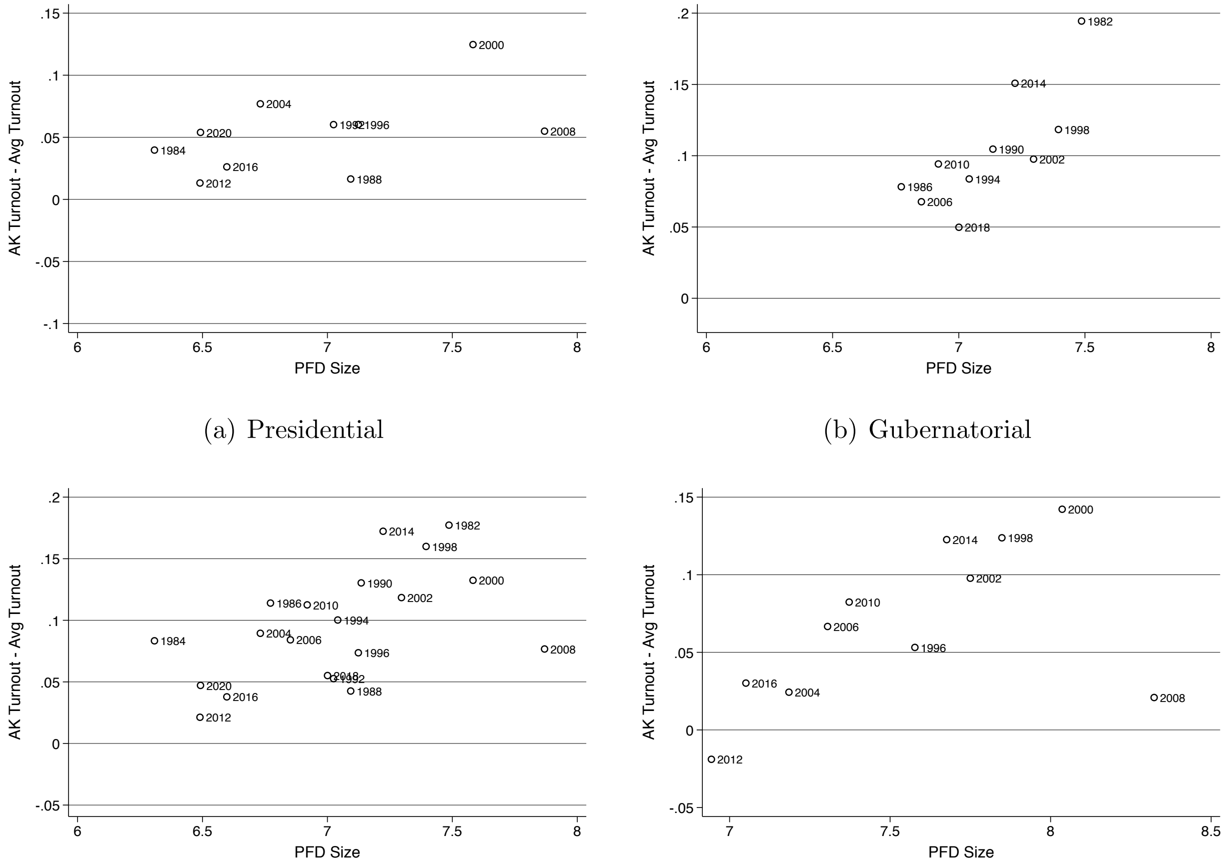
underlying mechanisms. The estimation equation used to analyze the individual-level data is similar to Equation (6), but is distinct in a couple ways. First, rather than being continuous, the outcome variable is binary and equal to unity among people who voted in election e in year t. Second, we condition the effect of the PFD on observed respondent heterogeneity including age, gender, and race. This is important for interpreting heterogeneous effects that may be correlated with these observed characteristics (that might also influence turnout).

6 Results

6.1 State-Level Analysis

We start by estimating the effect of PFD size on aggregate, state-level voter turnout across the three election types. The unconditional relationship between relative voter turnout (i.e. turnout in Alaska minus average turnout for all other states) and PFD size by election type is provided in panels (a), (b) and (c) of Figure 2 (panel (d) provides the scatter plot using CPS data and is discussed below). As can be seen, the unconditional relationship between relative turnout in Alaska and PFD size is positive, though only weakly so in presidential elections. Note that 2008 is a clear outlier, which happens to be the year that the PFD was matched with a $1200 energy rebate to help Alaskans deal with rising energy costs.

These unconditional results are borne out by estimating Equation (6), the results of which are provided in column (1) of Table 3. Across elections, coefficients on $\ln(PFD)$ range from 0.035 (in Presidential elections) to 0.142 (in Gubernatorial elections). Estimates for all three election types are statistically significant at the 5% level using either heteroskedasticity-robust standard errors or permutation inference. Figure 3, panels (a)-(c) provide graphical representations of the permutation tests applied to the state-level administrative data. They show histograms of the placebo (non-Alaska) estimates along with the true estimate. For gubernatorial and house elections, the true estimate is larger in absolute value than all placebo estimates, while for presidential elections it is larger than all but two.



(a) Presidential (b) Gubernatorial

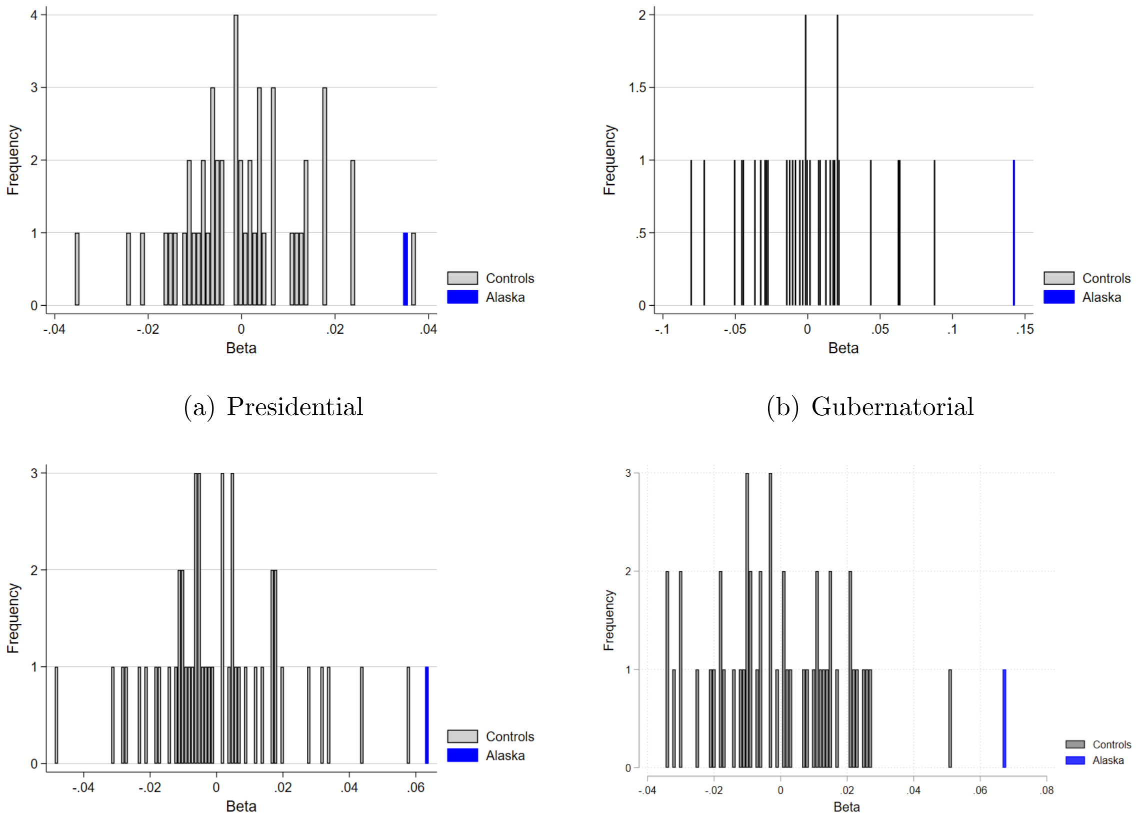
(c) House (d) CPS

Figure 2: Relative Voter Turnout by PFD Size

Notes: Panels depict the real value of PFD by year and the difference between turnout in Alaska and that in the average U.S. state by year. In panels (a) - (c) turnout is measured using actually state-level turnout by election type. In panel (d) turnout is measured using CPS survey data.

The estimate for gubernatorial elections implies that a 10% increase in the PFD increases turnout by approximately 1.4 percentage points. These are large qualitative effects. Consider that the PFD has roughly ranged from $1,000 to $4,000 (in 2020 dollars). A $1,000 increase from the average PFD size causes a 6.2 percentage point increase in voter turnout in gubernatorial elections (or 13.8% of the sample mean of 44.8%).[18] Columns (2)-(4) present alternative model specifications designed to gauge the robustness of our baseline findings and are discussed later in Section 7.

[18]The average real PFD size in our sample is $1,817, which has a logged value of 7.50. Adding $1000 and logging yields a value of 7.94. Multiplying the difference of 0.44 by the coefficient estimate of 0.142 yields an estimated increase in turnout of 6.2 percentage points.



(a) Presidential (b) Gubernatorial

(c) House (d) CPS

Figure 3: Permutation Inference Histograms

Notes: Each panel provides histograms for PFD effect estimates for each "placebo" state and Alaska.

In column (5) of Table 3, we report the estimated effects of PFD size on the share of democratic votes. Among national elections, increasing the PFD is associated with reduced Democratic vote share, but is only statistically significant within Presidential elections. This is consistent with the idea that personal economic gain decreases support for Democratic candidates, presumably because it decreases support for social programs that Democratic candidates are more likely to support (Fedaseyeu et al., 2015; Margalit, 2019). However, we do not observe similar patterns for other election types—perhaps reflecting the variety of partisan politics that exist at the U.S. state level—and so we are hesitant to draw strong conclusions.

Table 3: The Impact of PFD Size on State-Level Voter Turnout

	(1)	(2)	(3)	(4)	(5)	(6)
	Full Sample	Drop '82 & '20	Drop '16-'20	Drop '08	Dem Share	Primary
Panel A. Gubernatorial (non-presidential years)						
AK × ln(PFD)	0.142***	0.109***	0.131***	0.142***	0.121	-0.036
	(0.035)	(0.039)	(0.032)	(0.035)	(0.169)	(0.083)
Obs.	524	477	460	513	524	406
Mean dep. var.	.448	.445	.439	.445	.492	.193
# of elections*	10	9	9	10	10	10
Perm. test (p-value)	0.029	0.082	0.020	0.020	0.408	-
Panel B. Presidential (presidential years)						
AK × ln(PFD)	0.035**	0.037**	0.033**	0.053*	-0.057***	-
	(0.017)	(0.018)	(0.016)	(0.029)	(0.015)	-
Obs.	500	450	400	450	500	-
Mean dep. var.	.566	.556	.554	.563	.478	
# of elections*	10	9	8	9	10	
Perm. test (p-value)	0.020	0.000	0.041	0.000	0.020	-
Panel C. House (all years)						
AK× ln(PFD)	0.063**	0.048*	0.052*	0.098***	-0.060	-
	(0.027)	(0.025)	(0.029)	(0.022)	(0.049)	-
Obs.	1,000	900	850	950	1,000	-
Mean dep. var.	.461	.455	.446	.456	.495	
# of elections*	20	18	17	19	20	
Perm. test (p-value)	0.000	0.000	0.000	0.020	0.163	-

Notes: This table provides the estimated coefficient on PFD in Equation (6) along with robust standard errors in parentheses. Reported p-values are from Permutation Inference. Column (1) gives the estimated effect of PFD size on voter turnout out using the full sample. Column (2) gives the results dropping years 1982 and 2020. Similarly, column (3) drops the years 2016 - 2020, and column (4) the year 2008. Column (5) reports the estimated effect of PFD size on the share of votes that were cast for the democratic candidate. Column (6) gives the estimated effect of PFD size on voter turnout in primary elections. We report the number of elections held in Alaska. *, **, *** indicate statistical significance at the 10%, 5%, and 1% confidence level, respectively.

6.2 Individual-Level Analysis

The baseline individual-level results are provided in Table 4 and support the state-level aggregated results.[19] Using the full sample of all even-numbered years from 1996-2016 (column (1)), a 10% increase in the PFD causes a 0.67 percentage point increase in the probability of voting. Note that, averaged across election types, this effect size is nearly identical to that reported in Table 2 which relies on aggregated state-level data. The estimate is statistically significant using either robust standard errors or permutation inference. Panel (d) in Figure 3 shows the histogram of placebo estimates for the full sample individual-level specification. The true estimate is significantly larger in absolute value than all placebo estimates.

We condition these estimates on observed voter heterogeneity including age bins, gender, and race.[20] The omitted age bin includes people aged 18-29. Voter turnout monotonically increases across the four age groups. The effects are qualitatively large; people over 64 years old are 32.2 percentage points more likely to vote than the youngest age group. We also find that women are nearly two percentage points more likely to vote than men, and that non-White people are roughly two percentage points less likely to vote than White people. As with the state-level results, here we restrict the data in two different ways in columns (2) and (3), the results of which are discussed in Section 7.

6.2.1 Heterogeneous Effects

We explore heterogeneity along a set of dimensions, starting with income. Existing literature suggests that efforts to increase political participation are more successful among young people (Yu, 2019). We integrate this finding into our analysis by reporting heterogeneous income effects among people older and younger than fifty years. We measure household in-

[19]See panel (d) of Figure 2 for a scatter plot illustrating the relationship between relative CPS voter turnout and PFD size.

[20]We also observe other socio-economic data such as income and education but do not control for these factors because they are arguably endogenous to voting. By restricting controls to exogenous factors, their estimates can be interpreted causally. These baseline results are however robust to controlling for a broader set of factors including education, income, and the presence of children in the household (see Table A3 in the Appendix.)

Table 4: The Impact of PFD Size on Individual-Level Voter Turnout: Baseline CPS Results

	(1)	(2)	(3)
	Full Sample	Drop '16- '20	Drop '08
AK × ln(PFD)	0.067***	0.053***	0.161***
	(0.011)	(0.011)	(0.016)
Age 30-44	0.167***	0.172***	0.173***
	(0.002)	(0.002)	(0.002)
Age 45-64	0.287***	0.295***	0.297***
	(0.002)	(0.002)	(0.002)
Age>64	0.322***	0.331***	0.335***
	(0.002)	(0.002)	(0.002)
Female	0.019***	0.017***	0.017***
	(0.001)	(0.001)	(0.001)
Non-White	-0.021***	-0.019***	-0.024***
	(0.002)	(0.002)	(0.002)
Constant	0.434***	0.432***	0.430***
	(0.005)	(0.005)	(0.005)
R^2	0.105	0.106	0.106
Obs.	884,374	805,047	804,791
Perm. test (p-value)	0.000	0.020	0.000

Notes: This table provides the estimated coefficient on $AK\times$ PFD in Equation 6 along with robust standard errors in parentheses. Reported p-values are from Permutation Inference. The omitted age category is eighteen to twenty nine. Regressions include the real oil price interacted with an Alaska indicator. *, **, *** indicate statistical significance at the 10%, 5%, and 1% confidence level, respectively.

come as a fraction of the poverty threshold which is individually defined and based on family size.[21] For example, $1.5 < r < 2$ indicates a person's household income is greater than 1.5 times, but less than two times the poverty threshold.[22] People with household income less than 1.5 times the poverty threshold serve as the reference group.[23] By splitting the sample

[21] Income is not precisely reported, rather it is defined in bins. We define household income as the median value for each respective income bin. For example, a person with a household income between fifty and sixty thousand dollars is assigned a household income of sixty five thousand dollars.

[22] Poverty thresholds for Alaska in 2022 are distinct from the rest of the country and provided here: https://aspe.hhs.gov/topics/poverty-economic-mobility/poverty-guidelines. We converted nominal household income to real income using the 2022 CPI so that all years are comparable to the 2022 poverty threshold.

[23] One hundred and fifty percent of the poverty threshold is a commonly used measure of "poor" as many federal aid programs are available to households that meet this criteria. See for example thresholds used by

according to income and age, as done in Table 5, small-sample size is a reasonable concern. For reference, restricting the sample to people under the age of fifty (column 2), across all income bins the smallest Alaskan sample size is 892 (for $2 \leq r < 3$). Among people older than fifty, the smallest Alaskan sample size is 311 (again for $2 \leq r < 3$).

Table 5: The Impact of PFD Size on Individual-Level Voter Turnout: Heterogeneous Income Effects

	(1)	(2)	(3)
	Full	<50 Yrs. Old	>50 Yrs. Old
AK × ln(PFD)	0.094***	0.131***	0.025
	(0.027)	(0.034)	(0.043)
$1.5 \leq r < 2$	0.081***	0.066***	0.100***
	(0.002)	(0.003)	(0.004)
$2 \leq r < 3$	0.142***	0.131***	0.156***
	(0.002)	(0.003)	(0.003)
$3 \leq r < 4$	0.202***	0.194***	0.212***
	(0.002)	(0.003)	(0.003)
$4 \leq r$	0.279***	0.269***	0.288***
	(0.002)	(0.002)	(0.003)
$1.5 \leq r < 2$ × AK × ln(PFD)	0.011	0.034	-0.028
	(0.046)	(0.060)	(0.073)
$2 \leq r < 3$ × AK × ln(PFD)	0.020	-0.025	0.071
	(0.038)	(0.048)	(0.063)
$3 \leq r < 4$ × AK × ln(PFD)	-0.075**	-0.103**	-0.004
	(0.037)	(0.048)	(0.056)
$4 \leq r$ × AK × ln(PFD)	-0.049	-0.101**	0.027
	(0.030)	(0.040)	(0.047)
R^2	0.151	0.142	0.086
Obs.	811,281	454,937	340,485

Notes: This table provides the estimated coefficient on $AK\times$ PFD in equation 6 along with robust standard errors in parentheses. r is the ratio of household income to the household-specific poverty threshold. Effects are conditioned on age bins, indicators for gender and non-white status, state and year fixed effects, and the real oil price interacted with an Alaska indicator. In each regression, The income group is also separately interacted with the AK indicator. *, **, *** indicate statistical significance at the 10%, 5%, and 1% confidence level, respectively.

Results are provided in Table 5. Utilizing the full sample (column (1)), the coefficient on

the U.S. Department of Health and Human Services: https://tinyurl.com/nhjkk77z.

$\ln(PFD)$ is 0.094 implying that a 10% increase in PFD size increases the likelihood of voting by 0.94 percentage points among people earning less than 1.5 times the poverty threshold. Similar effects are documented for people earning between 2 and 3 times the threshold. Significantly smaller effects are documented for people earning more than three times the threshold. In fact, the PFD has no significant effect on people earning between three and four times the poverty threshold (p-value = 0.453). While the effect on people earning more than four times the threshold is relatively small, it remains statistically significant (p-value = 0.002). Roughly 47% of the sample are from households at three times the poverty threshold or less, so these results imply that the overall turnout effect is heavily concentrated in the lower half of the income distribution.

Splitting the sample by age reveals that relatively young adults (less than 50 years old) are most responsive to the cash transfers (column (2)) and that the PFD has no effect on young adults earning between three and four times the poverty threshold ($p = 0.425$) or for people earning more than four times the threshold (p-value = 0.190). The effect of the PFD on turnout is statistically insignificant across all income bins among older people (column (3)).

We also estimate heterogeneous effects by race, education, gender, and age in Table 6.[24] We document minimal heterogeneities by education and gender. Consistent with previous results reported by age, here we document smaller PFD effects among people who are at least fifty years old ("Senior"). We also find that the PFD effect size is roughly twice as large for people who report to be "Non-White", a group of people who are otherwise much less likely to vote than the White majority of the electorate.

[24]Note that each of these estimates are conditioned on age, gender, and a non-white indicator. However, when estimating effects by race, for example, we do not condition on the indicator for Non-White status. The same holds for the other sources of heterogeneity considered in Table 6.

Table 6: The Impact of PFD Size on Individual-Level Voter Turnout: Heterogeneous Demographic Effects

	(1)	(2)	(3)	(4)	(5)
AK × ln(PFD)	0.068***	0.060***	0.063***	0.078***	0.082***
	(0.011)	(0.011)	(0.012)	(0.016)	(0.014)
Native	-0.139***				
	(0.006)				
Native × AK × ln(PFD)	0.056				
	(0.039)				
Non-White		-0.019***			
		(0.002)			
Non-White × AK × ln(PFD)		0.052*			
		(0.031)			
HS			-0.238***		
			(0.001)		
HS × AK × ln(PFD)			-0.002		
			(0.022)		
Female				0.019***	
				(0.001)	
Female× AK × ln(PFD)				-0.022	
				(0.021)	
Senior					0.188***
					(0.001)
Senior × AK × ln(PFD)					-0.035*
					(0.021)
Constant	0.107***	0.109***	0.393***	0.111***	0.263***
	(0.014)	(0.014)	(0.013)	(0.014)	(0.014)
R^2	0.106	0.105	0.162	0.105	0.0832
Obs.	875,143	875,143	884.374	884,374	884,374

Notes: This table provides the estimated coefficient on $AK\times$ PFD in Equation (6) along with corresponding interactions. Robust standard errors are provided in parentheses. Effects are conditioned on age bins (except when exploring heterogeneous effects by "Senior" status), indicators for gender and non-white status, state and year fixed effects, and the real oil price interacted with an Alaska indicator. "Native" indicates a person reports to either be American Indian or Alaskan Native. "Non-White" indicates a person did not report to be "White Only" in the CPS report. "HS" indicates a person has received a high school diploma (or less education). "Senior" indicates a person is older than fifty years of age. In each regression, the demographic characteristic is also separately interacted with the AK indicator. *, **, *** indicate statistical significance at the 10%, 5%, and 1% confidence level, respectively.

7 Robustness Checks

We assess the sensitivity of our results by carrying out a series of robustness checks. First, we limit the sample period in various ways and re-estimate the baseline set of results. We drop 1982 and 2020 because these years featured PFDs paid in July and June, respectively. One may therefore expect to find less significant treatment effects these years. We also estimate effects dropping both 2008 and 2016 onward. Our rationale is twofold. First, 2008 featured a supersized PFD (that was a political response to high energy prices in the state of Alaska), and also corresponds to a national macroeconomic recession, the effects of which were somewhat muted in Alaska due to relatively high oil prices at the time. Second, from 2016 onward, the size of the PFD was capped, reflecting economic challenges and corresponding state-budget deficits in the state of Alaska. To the extent that high energy prices or macroeconomic recessions influence turnout, including these years in our analysis can introduce endogeneity bias into our estimates. These results for aggregated state-level turnout are provided in columns (2)-(4) in Table 3 and are largely consistent with our full-sample set of results. Columns (2) and (3) of Table 4 provide the results after dropping the years 2016-2020 and 2008, respectively. Results are robust, and actually increase in magnitude when omitting 2008 data, consistent with the state-level analysis.

We also estimate the effect of PFD size on primary voter turnout. We anticipate finding relatively small, non-zero positive effects given that Alaskans partially smooth their consumption in anticipation of the PFD (Hsieh, 2003; Kueng, 2018) and are well aware of the size of the PFD in the months leading up to disbursement. In the absence of smoothing or anticipatory effects, we would not expect to find any effects of the PFD on turnout given that primaries are held roughly six weeks before disbursement (primaries in Alaska have been held the third Tuesday of August since the establishment of the PFD program). To ensure comparability among observations, we limit the sample to observations where both the Democratic and Republican parties held primaries that were contested by more than one candidate. For gubernatorial elections, this restriction leaves 406 observations while dropping

72. The estimated effect of PFD size on gubernatorial primary-election turnout is provided in column (6) of Table 3. The effect is small, negative, and statistically insignificant.[25,26]

As an alternative identification strategy, we exploit the fact that virtually every Alaskan resident is eligible to receive a PFD (regardless of age), so larger families receive larger cash transfers. Identifying larger treatment effects among larger families supports the causal interpretation of our baseline estimates. Estimating marginal effects by family size is not feasible due to small sample sizes. For example, in 2012, only thirty five Alaskan respondents had three children. We therefore create indicators for whether or not a respondent has any: i) children, ii) mature children (between the ages of six and eighteen), and iii) young children (under the age of six). It is possible that older children decide how to spend their PFDs. As such, we anticipate finding larger effects among people with children, especially those with young children. Results are given in Table 7, which reports coefficients on PFD size interacted with indicators for children. The effect of the PFD is more than twice as large for respondents with children compared to those without (column (1)). While the effect size increases among households with young children present, this result is statistically insignificant.

While we condition all estimates on the interaction of the real oil price and an Alaska indicator, one may still be concerned that results are biased by a spurious correlation between oil prices and PFD size. To further address this concern, we restrict the sample to oil-rich states and re-estimate effects using both the state-level administrative data, and the CPS data.[27] Results are provided in Table A4. While the sample size is significantly reduced from our baseline set of results (the control set of states decreases from forty nine to ten), effect sizes are not altered in meaningful ways.

[25] We do not carry out Permutation Inference for primary elections because our restriction to only include elections where both parties hold a primary means that there are very few "placebo" states with observations in the same set of years as Alaska, which we consider a requirement to be a valid placebo comparison.

[26] We also observe primary turnout in senatorial elections, these results are provided in Appendix Table A2. While these results show a large, statistically significant effect of PFD on senatorial turnout, one should be cautious interpreting these results given the uneven, staggered nature of senate elections (states have two senators and hold senatorial elections every six years, such that the reference group of states changes over time in our analysis).

[27] We define oil-rich states as Alaska, Colorado, Kansas, Louisiana, Montana, New Mexico, North Dakota, Oklahoma, Texas, and Wyoming.

Table 7: Heterogeneous Family Size Effects

	(1)	(2)	(3)
AK × ln(PFD)	0.047***	0.058***	0.063***
	(0.013)	(0.013)	(0.011)
Kids	0.006***		
	(0.001)		
Kids × AK × ln(PFD)	0.051**		
	(0.022)		
Mature Kids		0.011***	
		(0.001)	
Mature Kids × AK × ln(PFD)		0.028	
		(0.023)	
Young Kids			0.000
			(0.003)
Young Kids × AK × ln(PFD)			0.066
			(0.050)
R^2	0.105	0.105	0.105
Obs.	884,374	884,374	884,374

Notes: This table provides the estimated coefficient on $AK\times$ PFD in Equation (6) along with respective interacted coefficients. Robust standard errors are in parentheses. Effects are conditioned on age, indicators for gender and non-white status, state and year fixed effects, and the real oil price interacted with an Alaska indicator. The indicator "Mature Kids" ("Young Kids") is unity for people with any children living in their home aged six years or more (five years or less). In each regression, The demographic characteristic is also separately interacted with the AK indicator. *, **, *** indicate statistical significance at the 10%, 5%, and 1% confidence level, respectively.

One may be concerned about serial correlation if effects of the PFD are persistent. This could potentially create upward bias in our estimates by partially attributing the effect of the previous year's PFD to that the following year. One way to address this is to estimate a distributed lag model, in which we regress turnout in election e and year t on PFD size and the one year lag of PFD size. These results are shown in Appendix Table A5 and largely complement our baseline results. One key difference is that the effect of (non lagged) PFD size on turnout in Presidential elections decreases from 0.0346 to 0.0251 and loses statistical significance (though the estimates are not statistically different from each other). Adding the lagged term does not have any meaningful effect among the other election types.

8 Mechanisms

Why do cash transfers increase voter turnout? In Section 2, we propose two possible mechanisms: reductions in logistical costs (e.g., the cost of getting to the polls) and uncertainty costs (e.g., the cost of casting a vote with incomplete information). In this section, we leverage additional data collected from the CPS to explore which of these mechanisms best fit the data.

The CPS asks non-voters to explain why they failed to vote. Respondents can give one of eleven answers, which are described in Appendix Table A6. We group stated reasons into two broad categories: logistic and uncertainty. Transportation poses clear logistical challenges to voting. Physical disability also creates financial strain that makes voting more difficult and costly (Schur and Kruse, 2000). Poor health more generally significantly reduces voter turnout, but only among the poor (Lyon, 2021). Based off of these observations, we define logistical reasons for not voting as: i) lacking transportation to the polls, and ii) being too sick or disabled to vote.

Unfortunately, the CPS does not list "uncertainty" about candidates or electoral issues as a potential reason for not voting. Respondents could, however, cite being uninterested in the election or disliking all of the candidates as reasons for not voting. These characteristics are reasonably associated with the amount of electoral information a person possesses and so we use them to proxy for voter "uncertainty".[28]

We restrict the sample to people who either voted, or did not and indicated why they failed to vote (roughly 60% of people who indicated they did not vote did not explain why).[29] Small-samples create a challenge in this context. For example, just twelve Alaskan respondents reported to have not voted in 2012 due to a lack of transportation or health

[28]We also use more encompassing definitions of both logistical and uncertainty costs, the results of which are provided in Table A7 and reinforce our baseline results.

[29]We regressed an indicator equal to unity for respondents who did not indicate why they did not vote on observed respondent characteristics. These results are provided in the second column of Appendix Table A1. These results show that being female, older, White, wealthy and educated all increase the probability of indicating the reason for not voting.

issue. Such a small sample makes it difficult to estimate marginal effects. We address this by estimating the effect of the PFD non-parametrically. We first create a binary temporal variable equal to unity when the PFD is above $2,000$ (which occurs in the years 1982, 2000, 2008, and 2014, see Figure 1) and then interact this variable with an indicator for Alaska. The outcome variable is unity for people who did not vote, and indicated the reason was either "logistical" or "uncertainty", respectively. Treatment effects are conditioned on year and state fixed effects which capture the direct effect of both the year being a high PFD year, and Alaska. As before, effects are also conditioned on respondent age bins, gender, and non-White status. The coefficients on the interaction terms are interpreted as the relative effect of a large PFD on the probability of not voting for a particular reason.

The results are provided in Table 8. Large (relative to small) PFDs reduce the number of people failing to vote due to uncertainty by 2.2 percentage points. This is a large effect and amounts to a 56.6% reduction in the number of people claiming to have not voted for this reason (note that on average 3.8% of respondents claim to have not voted due to uncertainty). We document some evidence that large PFDs reduce logistical costs of voting, albeit this effect is only significant at the ten percent level (and is qualitatively small).

Table 8: PFD & Reasons For Not Voting

	Full		$r < 3$		$r > 3$	
	Logistic	Uncertainty	Logistic	Uncertainty	Logistic	Uncertainty
AK × High PFD	-0.006*	-0.022***	-0.006	-0.045***	-0.005	0.003
	(0.004)	(0.005)	(0.006)	(0.008)	(0.004)	(0.006)
μ	0.043	0.038	0.055	0.055	0.023	0.034
$\%\Delta$	0.150	0.566	-0.119	-0.829	-0.232	0.080
R^2	0.031	0.014	0.024	0.016	0.044	0.014
Obs.	686,500	686,500	323,282	323,282	363,218	363,218

Notes: This table provides the estimated coefficient on $AK\times$ High PFD along with robust standard errors in parentheses. Effects are conditioned on age bins, indicators for gender and non-white status, state and year fixed effects, and the real oil price interacted with an Alaska indicator. Logistical reasons for not voting include transportation problems and illness or disability. Uncertainty reasons include not being interested in voting, and disliking all of the candidates running. *, **, *** indicate statistical significance at the 10%, 5%, and 1% confidence level, respectively.

Recall from Table 5 that the effect of the PFD on turnout is more pronounced among people earning less than three times the poverty threshold. We therefore split the sample according to this income threshold and re-estimate effects. Large PFDs cause a 4.5 percentage point reduction in the number of low-income people claiming not to vote due to uncertainty (disliking all of the candidates, or being uninterested in the election). This amounts to an 83% reduction (among this low-income group, 5.5% of respondents on average do not vote because of uncertainty). The PFD created a much smaller (and statistically insignificant) reduction in the percent of low-income respondents claiming not to have voted due to logistical challenges. We find no evidence that cash transfers reduce either logistical or uncertainty costs of voting among the high-income electorate (the last two columns of Table 8). These results are consistent with Conover et al. (2020) that finds cash transfers engage the non-voting, low-income electorate by stimulating interest in electoral issues and reducing uncertainty. Surprisingly, we find little evidence that cash transfers help the low-income electorate overcome logistical challenges to voting.

9 Conclusion

Voter turnout in the United States is low, especially among the low-income electorate. This creates unequal representation in government and public policies that disproportionately reflect the needs and preferences of high-income voters. Understanding how to engage the low-income electorate is therefore of critical economic importance.

While some have championed unconditional and universal cash transfers as a possible remedy to address low rates of civic engagement among the poor, tests of this theory are lacking in the literature. We fill this gap by leveraging a large-scale natural field experiment in which virtually every Alaskan resident, regardless of age or income, receives a check of varying size roughly one month before election day. The structure of this program is ideal for our purposes, and allows us to identify marginal effects of cash transfers across distributions of income, age, and race.

Measuring turnout using administrative state-level voter turnout data, and individual-level data collected by the Current Population Survey, we find that increasing the size of a cash transfer increases the likelihood of voting by economically significant margins. For example, a 10% increase in cash (roughly $182, relative to the mean PFD amount), increases turnout in gubernatorial elections by 1.4 percentage points. Smaller effects are documented for federal elections, but remain statistically significant. Exploring treatment heterogeneities, we find that effects are more pronounced among people who are young, non-White, and low-income. Somewhat surprisingly, cash transfers do not reduce the number of people citing logistical challenges (e.g., transportation to the polls, bad weather, or being too busy with work) as the reason for not voting. Rather, the mechanism appears to be increased interest in politics and electoral issues. Consistent with recent research (Yoder et al., 2021), this suggests that reducing logistical costs of voting (e.g., by implementing vote-by-mail) may be relatively ineffective at raising voter turnout.

Whether our results are specific to government transfers or apply more generally to income windfalls remains an important question for future research to consider. Taken together, we conclude that cash transfers are a useful tool for increasing democratic engagement in the United States, especially among groups of people who are disproportionately unlikely to vote.

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Appendix

Table A1: Determinants of Missing CPS Data

	Missing Voted	Missing No Vote Reason
Age 30-44	-0.022***	-0.078***
	(0.001)	(0.002)
Age 45-64	-0.031***	-0.010***
	(0.001)	(0.002)
Age>64	-0.054***	-0.176***
	(0.001)	(0.003)
Female	-0.016***	-0.038***
	(0.001)	(0.002)
Non-White	0.028***	0.026***
	(0.001)	(0.002)
High-Income	-0.053***	-0.069***
	(0.001)	(0.002)
HS	0.004***	0.135***
	(0.001)	(0.002)
Constant	0.168***	0.636***
	(0.001)	(0.002)
R^2	0.023	0.074
Obs.	997,514	329,452

Notes: The outcome variable in the first column is unity for CPS respondents who did not indicate whether they voted. The outcome in the second column is unity for respondents who did not indicate why they did not vote, and this regression only includes respondents who indicated they did not vote. Robust standard errors are provided in parentheses. The age bin 18-29 is omitted. Non-White indicates a person did not report to be "White Only". HS indicates a person has received a high school diploma (or less education). High-income is unity for people with a household income at least three times the poverty threshold. Regressions include state and year fixed effects. *, **, *** indicate statistical significance at the 10%, 5%, and 1% confidence level, respectively.

Table A2: The Impact of the PFD Size on State-Level Senatorial Voter Turnout

	(1)	(2)	(3)	(4)	(5)	(6)
	Full Sample	Drop 82 & 20	Drop 16-20	Drop 08	Dem Share	Primary
AK × $\ln(PFD)$	0.041	0.031	0.019	0.098***	-0.079	0.093***
	(0.028)	(0.028)	(0.027)	(0.018)	(0.086)	(0.029)
Obs.	667	601	567	634	664	468
Perm. test (p-value)	0.1176	0.2245	0.5918	0.0204	0.3673	-

Notes: This table provides the estimated coefficient on PFD in equation 6 along with robust standard errors in parentheses. Reported p-values are from Permutation Inference. Column (1) gives the estimated effect of PFD size on voter turnout out using the full sample. Column (2) gives the results dropping years 1982, 2008, and 2016 - 2020. Column (3) reports the estimated effect of PFD size on the share of democratic and republic votes that were cast for the democratic candidate. Column (4) gives the estimated effect of PFD size on voter turnout in primary elections. *, **, *** indicate statistical significance at the 10%, 5%, and 1% confidence level, respectively.

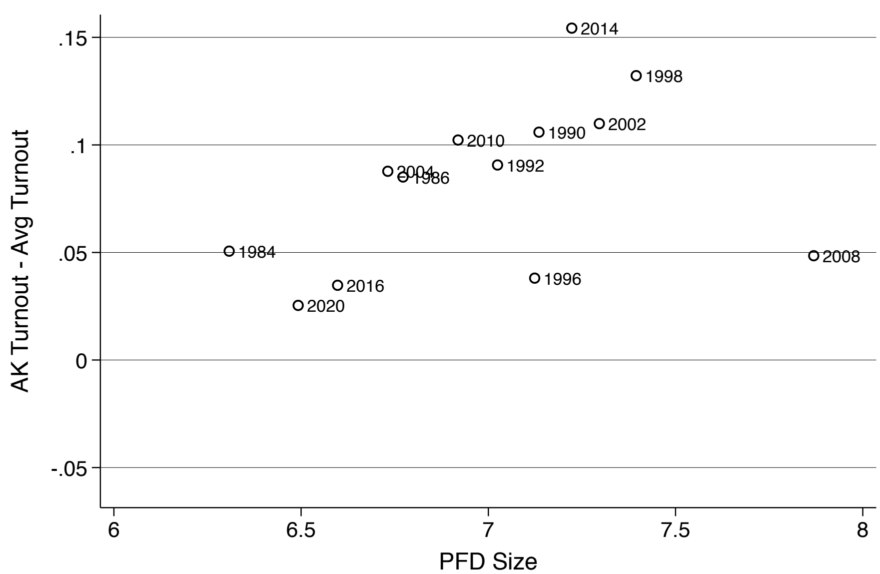

Figure A1: U.S. Senate Turnout & PFD Size

Notes: PFD Size and the the difference between turnout in Alaska and that in the average U.S. state by year. Turnout is measured as the share of adults who voted in the U.S. senatorial election.

Table A3: Baseline CPS Results with Additional Controls

	(1)	(2)	(3)
	Full Sample	Drop '16- '20	Drop '08
AK × ln(PFD)	0.0573***	0.0495***	0.149***
	(0.010)	(0.011)	(0.015)
Age 30-44	0.140***	0.145***	0.146***
	(0.002)	(0.002)	(0.002)
Age 45-64	0.270***	0.279***	0.280***
	(0.002)	(0.002)	(0.002)
Age > 64	0.367***	0.380***	0.380***
	(0.002)	(0.002)	(0.002)
Female	0.020***	0.019***	0.018***
	(0.001)	(0.001)	(0.001)
Non-White	0.013***	0.015***	0.010***
	(0.002)	(0.002)	(0.002)
High-income	0.113***	0.113***	0.114***
	(0.001)	(0.001)	(0.001)
HS	-0.206***	-0.206***	-0.205***
	(0.001)	(0.001)	(0.001)
Kids	0.014***	0.016***	0.014***
	(0.001)	(0.001)	(0.001)
Constant	0.504***	0.500***	0.499***
	(0.005)	(0.005)	(0.005)
R^2	0.174	0.174	0.173
Obs.	884,374	805,047	804,791

Notes: This table provides the estimated coefficient on $AK\times$ PFD in equation (6) along with robust standard errors in parentheses. The omitted age category is eighteen to twenty nine. High-income is unity for people with a household income at least three times the poverty threshold. HS is unity for people with at most a high school education, and Kids is unity for respondents with any children living in their house. *, **, *** indicate statistical significance at the 10%, 5%, and 1% confidence level, respectively.

Table A4: The Impact of PFD Size on State-Level Voter Turnout: Oil-Rich Economies Only

	Gubernatorial	Presidential	House	CPS
AK$\times$ ln(PFD)	0.149***	0.025	0.053**	0.056***
	(0.033)	(0.018)	(0.023)	(0.011)
R^2	0.948	0.880	0.865	0.121
Obs.	100	100	200	155,589

Notes: This table provides the estimated coefficient on AK$\times$ ln(PFD) in Equation (6) along with robust standard errors in parentheses. The first three columns use U.S. state-level administrative data. The last column uses CPS survey data. All estimates are conditioned on state and year fixed effects, and the real oil price interacted with an Alaska indicator. The CPS estimates are additionally conditioned on age bins, and indicators for race and gender. The sample is restricted to oil-rich states including: Alaska, Colorado, Kansas, Louisiana, Montana, New Mexico, North Dakota, Oklahoma, Texas, and Wyoming. *, **, *** indicate statistical significance at the 10%, 5%, and 1% confidence level, respectively.

Table A5: The Impact of the PFD Size on State-Level Voter Turnout: Adding Lags

	Governor	President	House
AK $\times$ ln$(PFD)_t$	0.1332***	0.0251	0.0745***
	(0.036)	(0.025)	(0.025)
Perm. test (p-value)	0.082	0.510	0.020
AK $\times$ ln$(PFD)_{t-1}$	-0.026	0.025	-0.052*
	(0.028)	(0.057)	(0.031)
Obs.	488	500	950

Notes: This table provides the estimated coefficient on PFD in Equation 6 as well as the coefficient on the one year lag of PFD. Robust standard errors in parentheses. Reported p-values in brackets are from Permutation Inference. *, **, *** indicate statistical significance at the 10%, 5%, and 1% confidence level, respectively.

Table A6: Possible Reasons for Not Voting (CPS Data)

	Frequency	Percent
(1) Illness or disability (own or family's)	20,925	6.35
(2) Out of town or away from home	15,393	4.67
(3) Forgot to vote (or send in absentee ballot)	9,232	2.80
(4) Not interested, felt my vote wouldn't make a difference	18,073	5.49
(5) Too busy, conflicting work or school schedule	29,471	8.95
(6) Transportation problems	5,119	1.55
(7) Didn't like candidates or campaign issues	11,702	3.55
(8) Registration problems (i.e., didn't receive absentee ballot, not registered in current location)	5,579	1.69
(9) Bad weather conditions	1,242	0.38
(10) Inconvenient hours, polling place or hours or lines too long	2,482	0.75
(11) Other	12,360	3.75
Missing	197,874	60.06

Notes: This table provides the frequency and percent of respondents who did not vote for each respective reason. Data collected from the Current Population Survey.

Table A7: PFD & Expanded Reasons For Not Voting

	Full		$r < 3$		$r > 3$	
	Logistic	Uncertainty	Logistic	Uncertainty	Logistic	Uncertainty
AK × High PFD	-0.015**	-0.025***	-0.017	-0.046***	-0.012	-0.003
	(0.006)	(0.006)	(0.010)	(0.009)	(0.008)	(0.007)
μ	0.086	0.057	0.107	0.073	0.068	0.043
%Δ	0.173	0.440	-0.157	-0.634	-0.175	0.080
R^2	0.045	0.019	0.041	0.022	0.050	0.018
Obs.	686,500	686,500	323,282	323,282	363,218	363,218

Notes: This table provides the estimated coefficient on $AK\times$ High PFD along with robust standard errors in parentheses. Effects are conditioned on age bins, indicators for gender and non-white status, state and year fixed effects, and the real oil price interacted with an Alaska indicator. Logistical reasons for not voting include transportation problems, illness or disability, bad weather, being too busy with work or school, or polling hours and locations being too inconvenient. Uncertainty reasons include not being interested in voting, disliking all of the candidates running, and forgetting to vote. *, **, *** indicate statistical significance at the 10%, 5%, and 1% confidence level, respectively.