Cash transfers, political autonomy, and civic participation: Evidence from a natural experiment in Alaska

Paasha Mahdavi[*]

August 23, 2019

Abstract

An emerging solution by international NGOs to the so-called 'resource curse'—whereby wealth derived from natural resource extraction leads to bad economic and governance outcomes—is for governments to distribute resource revenues to citizens via direct cash transfers. What is the impact of such unconditional transfers on politics and political behavior? Rentier theories posit an acquiescence effect, such that direct distribution of rents derived from extractive resources will suppress civic engagement. In contrast, economic voting theories suggest that increased disposable income prompts greater political participation. This paper proposes a new framework based on political autonomy from the state: individuals receiving oil-to-cash transfers use this windfall to withdraw from public services, thereby severing a key state-citizen linkage that motivates participation for individuals living outside the political-economic core of the state. This paper leverages a natural experiment and a historical survey in 1976 Alaska to test the argument. Individuals receiving oil-to-cash transfers as-if randomly right before an election are roughly 8 to 20 percentage points less likely to vote than those receiving transfers right after the election. These findings bear theoretical implications not only for the study of natural resource politics but also the broader study of civic engagement and the political effects of unconditional cash transfers.

1 Introduction

Governments of oil-rich economies devise different methods of distributing hydrocarbon revenues to their citizens. The typical policies adopted involve the use of taxes on extractive sales to finance spending on public goods, such as infrastructure, state education programs, connected health networks, and other foundations of a functioning, modernized economy. Yet when these benefits do not materialize, either due to corruption or bureaucratic inefficacy, citizens seek to receive benefits directly via cash transfers of resource wealth (Moss, Lambert and Majerowicz, 2015). These "oil-to-cash" transfers are paid regularly and unconditionally, such that politicians are constrained from discretionary and distortionary spending of extractive-resource rents.

While much is known about how such unconditional transfers impact socio-economic outcomes such as poverty (Blattman, Fiala and Martinez, 2014), income inequality (Hsieh, 2003), health (Haushofer and Shapiro, 2016), and education (Baird, McIntosh and Özler, 2011), little is known about their political consequences (see Holbein et al., 2017). This paper analyzes one such effect: the impact of oil-to-cash transfers on individual participation in politics. I argue that these transfers have the effect of increasing individual autonomy from the state, such that people receiving this money become disincentivized from civic engagement. When transfers are paid to individuals prior to an opportunity to participate in politics, I argue that this windfall allows citizens the ability to withdraw from previously-used public services. These services serve both as an incentive to engage in the political process on ideological grounds and as a one of the few linkages between the state and citizens, especially for individuals living outside the state's political-economic core. Without involvement or interest in these services, an individual is left with fewer incentives to participate.

To test this argument, I examine a natural experiment in the context of voting in the 1976 general election by recipients of the Alaska Longevity Bonus Program (LBP), a now-defunct oil-to-cash transfer program that preceded the state's current Permanent Fund Dividend (PFD). In order to identify the effect of transfers on political participation, I exploit the

following as-if random assignment: residents who applied prior to the election and were approved prior to the election (*treated*) and residents who applied prior to the election and were approved after the election (*control*). The identification strategy hinges on the fact that applications were not approved entirely in the order they were received but rather depending on what numbered bin an application was placed in upon receipt at the state office. The results show that individuals receiving their first monthly oil-to-cash transfer of $125 (in nominal 1976 dollars; roughly $560 in real 2018 dollars) before the election are roughly 8 to 20 percentage points less likely to vote than those receiving their first checks after the election. Results from a survey conducted in 1976 on LBP recipients and non-recipients corroborate the argument that the Bonus provided individuals with the ability to opt out of state services such as Medicare, food stamps, and state-provided homecare even if they remained eligible for these social services.

This paper brings four innovations to the literature on extractive-resource politics and to the broader study of civic engagement. First, I empirically assess the widely circulated but largely untested mechanisms of citizen detachment from politics that drive the political maladies associated with the natural resource curse (see Ross, 2001). Using data on actual outcomes at the individual level, I construct a research design that allows for an estimation of the causal effect of resource wealth on civic engagement. This approach extends the work of prior scholars who have gone beyond the pale of cross-national time-series analysis of natural resource politics using either experimental methods or tools for causal inference with observational data (Brollo et al., 2013; Paler, 2013; de la Cuesta et al., 2017).

Second, I provide a new theoretical framework for explaining how windfall income at the individual level can hamper civic engagement. By providing a means for withdrawing from previously-used social services, windfalls can increase political autonomy from the state and thereby reduce incentives to participate in the political process. In contrast to classical studies of economic voting (Downs, 1957; Fiorina, 1978), I argue that unearned income— not tied to any party or individual political leader—does not have the same mobilizing and

resource effects on political participation as do increases in earned income.

Third, I provide a tough test of the resource curse theory in the context of an advanced democracy. Here I build on the pioneering work of Goldberg, Wibbels and Mvukiyehe (2008) and Simmons (2016) by evaluating the United States as a case in the resource curse context. But by exploiting exogenous shifts in oil-to-cash handouts and by employing methods to construct viable counterfactuals, I dive deeper into the sub-national context of the US with an identified evaluation of the political effects of oil wealth.

Fourth, I conduct a policy evaluation of transfers, using the example of the Longevity Bonus, on heretofore unexamined consequences relating to political behavior. Studies show that Alaskan dividends have noticeably reduced income inequality, increased household income stability, and improved the trade and service sectors of the local economy (Goldsmith, 2002; Hsieh, 2003). Yet despite these economic benefits, I find evidence that these handouts have severed the link between state and citizen, resulting in lower political participation by Alaskans living outside the political-economic core and receiving the Longevity Bonus before an election.

Alaska is an ideal testing ground for hypotheses about the political effects of resource wealth. The ethno-cultural heterogeneity of its indigenous and non-indigenous people, its lack of value-added and income taxes, and its extreme fiscal reliance on oil revenues make the state of Alaska representative of oil-rich countries in the Middle East, Africa, Southeast Asia, and Latin America. Of course, Alaska obviously differs from the typical petro-state in its strength of political institutions in the context of an advanced, developed democracy. Yet this suggests that findings from the Alaska case, if anything, could indicate a lower bound on the deleterious effects of direct resource transfers on political participation and civic engagement.

2 How oil-to-cash transfers affect participation

Resource curse theorists posit that extractive-resource revenues allow leaders to win citizen acquiescence through direct distribution rather than popular support through political representation (Anderson, 1987; Crystal, 1989; Herb, 1999). The argument rests on the theory of the *rentier* state: reliance on rents—typically natural resource revenues, but also sources such as foreign aid and remittance payments—weakens the government's accountability to citizens, since it can continue operating without extracting money from its citizens' personal incomes (Mahdavy, 1970; Beblawi and Luciani, 1987; Karl, 1997). This is the classical "*rentier* social contract" whereby "the state provides goods and services to society (such as subsidies on basic commodities) without imposing economic burdens, while society provides state officials with a degree of autonomy in decision-making and policy" (Wiktorowicz, 1999; Herb, 2005, 608, 298).

This distribution of resource revenue thus distorts how citizens view their government. In a world where governments pay their citizens instead of the other way around, the state-citizen linkage is broken—allowing leaders to stay in power indefinitely without much accountability, as long as the state delivers on its contract "to enhance quality of life rather than democratic principles" (Wiktorowicz, 1999, 608). Hence, the resource curse theorist avers that natural resource wealth hinders democracy and good governance (see Ross (2015) for a review), although several scholars have questioned the unconditionality of this effect (Smith, 2007; Brooks and Kurtz, 2016; Menaldo, 2016).[1]

The literature on conditional cash transfers and programmatic spending argues for the exact opposite effect. If leaders directly distribute revenues, citizens are theorized to respond

---

[1]One explanation for the acquiescence mechanism is that the direct distribution of resource revenues makes citizens less interested in political minutiae and more interested in goods and services that resource wealth now enables them to consume. This is effectively at the core of what many have referred to as "petro-mania" at the government level, whereby oil money is used to finance expenditures that quench short-term desires at the expense of long-term benefits (Karl, 1997). During boom times, these revenues are spent not on improving civil society but rather on lavish expenditures and "white elephant" projects of costly but useless infrastructure. The same logic of "petro-mania" could apply to the individual, albeit more as a substitution effect. If civic engagement is perceived as costly compared to other activities (Riker and Ordeshook, 1968), then resource transfers might increase the opportunity cost of participating in politics.

with political support for the provider of these goods (Stokes et al., 2013; Diaz-Cayeros, Estevéz and Magaloni, 2016). In this way, the transfer of cash or in-kind benefits is perceived as a clientelistic exchange for votes even though transfers are programmatic and not targeted to specific individuals. Empirical evidence supports this line of argument. Research on randomized timing of programmatic cash transfers in Mexico, for instance, shows a clear positive effect on turnout (and vote shares for the incumbent) of receiving the *Progresa* conditional cash transfer two years before the election (De La O, 2013).[2] Others find a similar effect using conditional cash transfer experiments in Brazil (Zucco, 2013), Colombia (Zárate et al., 2013), Honduras (Galiani et al., 2016), and the Philippines (Labonne, 2013).

Yet these types of transfers are by definition not universally distributed: politicians can punish non-compliers by withholding transfers from entire districts or by discontinuing the programs in entirety. Further, programmatic transfers are often attributed directly to specific leaders or parties, such as the PRI in Mexico, rather than the government in general even if this is not actually the case. These reasons explain the pro-incumbency mobilization effect of cash transfers *via* a reciprocity mechanism, whereby voters reward those who claim credit for programs they find desirable (Mayhew, 1974; De La O, 2013). So while we might expect a conditional cash transfer to increase participation in order to support one's patron, an oil-to-cash transfer that is distributed unconditionally and without credit-claiming may not foster the same reaction.[3]

Previous empirical research on how resource revenues affect individual behavior finds

---

[2]See Imai, King and Rivera (2016) which disputes the programmatic spending-incumbency linkage.

[3]In terms of the timing of the transfer, classical economic theory would suggest a null effect on political participation. If an individual expects a cash transfer at some point in the future, then receiving this transfer at time $t - 1$ rather than later at time $t + 1$ should have no effect on behavior. In the long run, this will undoubtedly be true, especially if the individual receiving her transfer later is compensated for interest and inflation. But in the short run, this might not be the case. Consider an individual i who receives the transfer at $t - 1$, and an individual j who receives the transfer at $t + 1$. Let's say that these individuals face an exclusive choice between a, participating in politics, and b, consuming a non-political good, where the latter is preferred given a budget constraint. At time t, individual i has the means to consume b while individual j cannot, and thus i chooses b and j chooses a. At time $t + 2$, individual i will have depleted his budget and thus now chooses a, while individual j now has the means to consume b. By this point, both individuals have participated in politics. But if, for instance, an election is held at time t, then we expect that only individual j will turn out to vote, while individual i will be using his cash transfer to consume his preferred non-political good.

conflicting results. In measuring the impact of oil wealth on individual political perceptions and behavior, some find that these rents hinder individual demands for democratic accountability, as measured in national surveys (McGuirk, 2013), natural experiments (Bhavnani and Lupu, 2016), or field experiments (Paler, 2013; Grossman, Paler and Pierskalla, 2016). Others see no such effect, instead finding that oil wealth is no different than other types of revenue in affecting how citizens view government and the extent to which citizens engage in politics (de la Cuesta et al., 2017).

This study widens the current theoretical understanding of resource wealth and political behavior by examining how resource wealth is distributed, to whom it is distributed, and how it is used by recipients. I argue that direct transfers of resource revenue disincentivize participation in politics when delivered to individuals whose relationship to the state is based on the transaction of public services. For citizens whose primary connection with government is the provision and usage of social services, resource wealth transfers provide the financial opportunity to withdraw from this exchange and replace the consumption of state services with private services. Absent involvement or interest in state services, individuals have fewer incentives to check government performance in providing these services. Hence, at the margin, individuals receiving resource transfers will be less likely to engage in the political process than individuals not receiving these transfers.

The provision of societal goods serves as not only an important allocative function of government but also a key linkage between state and citizen. Welfare-state citizens, for instance, find that their personal well-being is directly connected to government performance (Marshall, 1964; Tilly, 1975; Skocpol, 1992). The provision of social goods also serves as a cognitive signal—in the context of a crowded, complex informational environment—that fosters individual interest in the policy process (Pierson, 1993). Service provisions can further enable a psychological bond with the state by intertwining the fate of citizen outcomes with government action. A classic example of this linkage is Andrea Campbell's study of Social Security and senior voting:

> [Social security] ties seniors as citizens to state functions in an immediate way.
> Their engagement with public affairs is enhanced because their self-interest is so
> significantly and obviously implicated. Self-interest is seldom a guide to behavior,
> since people do not often recognize their interests or may choose to act in
> an altruistic manner. But seniors' interest in Social Security is so immediate,
> quantifiable, and tangible that it influences their activities (Campbell, 2003, 7).

Campbell describes two mechanisms for this effect: resources and mobilization. Social Security provided time and money for seniors to engage in politics. Prior to the program, many seniors were on restricted incomes and struggled to engage in basic societal activities— let alone get involved in politics. But Social Security also importantly provided a mobilizing force to hold government accountable to maintaining and expanding the program. Special interest groups arose to mobilize seniors to actively participate in politics to counter any potential threats to senior programs. Indeed, mobilization effects are just as strong, if not stronger, than the classic income and educational pillars of explanations for why individuals participate in politics (Verba, Schlozman and Brady, 1995). And there is little debate that mobilizing groups—such as unions, community organizations, professional associations, and religious groups—are central to understanding who votes and why (Verba and Nie, 1972; Verba, Schlozman and Brady, 1995; Han, 2014).

This would suggest that the state-citizen linkage effect of social services will be strongest in communities outside the political and economic core of the state. In these peripheral communities, access to civil society organizations is inherently limited (Blair, 2000; Skocpol and Fiorina, 2004), and the delivery and usage of social services can often be the only mobilizing force for political engagement. By contrast, individuals living in civically core communities have greater exposure to forces that mobilize participation: unions that highlight salient labor issues on which to press politicians; activist groups that galvanize issue engagement to mobilize political involvement; and senior citizen organizations like AARP that rally their members to participate in political events.

For some individuals, a large and recurring windfall provides the means to stop using social services if so desired. Consider a woman living on a fixed income in the United States, one who uses Social Security benefits, receives housing assistance, is on Medicare and Medicaid, and uses supplmental nutrition assistance programs (SNAP). If she were to suddenly receive a monthly stipend that doubles her monthly income, she may decide to end her enrollment in, for example, Medicaid, housing assistance, and SNAP for personal reasons.[4] This could be the case even if the stipend is exempt from her gross income, such that she would still be eligible for these programs. With the extra windfall income, this individual now has the autonomy to withdraw from social services. And even though she is still enrolled in Social Security and Medicare, she may not use these benefits as often or be as immediately interested in the political details of these programs.

In effect, windfalls such as these provide extra income that can be used to cover expenses for services that previously had been performed by the state. With less reliance on the state, an individual has one less reason to hold government to account if and when these services are not properly delivered or are threatened. The withdrawal from social services—or even the loss of interest in service outcomes—thus weakens a key state-citizen linkage that otherwise incentivizes engagement in the political process (Verba, Schlozman and Brady (1995), but see also Mettler (2018) for a contrasting view). The withering of incentives to engage politically will be most prominent in the civic periphery, which lack civil society organizations and other mobilizing forces that would otherwise provide motivations to participate.

Citizens have little incentive to engage civically as long as this windfall continues, unless there are ideological or issue-specific grounds to contribute to the political debate. And if this windfall is not tied to any particular political leader or party, unlike the case of most conditional cash transfers, there are few partisan-based motivations for political engagement. Note that while this line of argument leads to the same expected outcome as *rentier* state

---

[4]Recipients of these welfare programs often feel "demeaned" by the recurring and probing interactions with program officers (Soss, 1999). Given the opportunity to stop using these programs, some individuals may do so even if withdrawing from eligible programs is not economically rational.

theory—that windfall wealth leads to less civic engagement—it does not identify acquiescence as the mechanism that underpins the effect. By contrast, this argument develops the notion of autonomy from the state's typical functions as a consequence of resource wealth. It is this autonomy, rather than being "acquiesced" in a submissive or subjugated manner, that removes what used to be a strong motivator for political participation.

In the context of direct resource wealth distribution *via* cash transfers, the argument implies that transfer recipients will be less likely to participate in politics than non-recipients. The autonomy mechanism implies that the difference in participation rates exists because transfer recipients are less likely to use public services even if they are entitled or eligible to receive these services. These implications provide the basis for two testable hypotheses in the context of resource (i.e., oil-to-cash) transfers:

Hypothesis 1: *Individuals receiving cash transfers are less likely to participate in politics than individuals not receiving transfers. This effect is most pronounced for individuals in the civic periphery, and less pronounced for individuals in the civic core.*

Hypothesis 2: *Public service use among individuals receiving cash transfers is lower than individuals not receiving transfers.*

Here I operationalize participation in terms of the costly action of turning out to vote, as opposed to survey-based measures of intentions to vote, perceptions of government behavior, and self-reported attendance at political events. I operationalize public service use as participation in state-provided social programs for nutritional, housing, and medical assistance. Note that the argument does not imply differences in public service use between core and periphery; individuals receiving cash transfers should be more likely to withdraw from social services irrespective of location. Importantly, core-residing individuals who withdraw from services are exposed to other mobilizing forces which incentivize participation compared to periphery-residing individuals who withdraw from services.

9

3   The Alaska Case

Alaska presents a highly relevant yet unorthodox context for testing hypotheses regarding natural resource politics. The penultimate state to join the US, its nascent history is deeply intertwined with petroleum, from the first oil boom in 1969, to the Exxon Valdez spill of 1989, to the ongoing fight over drilling in its pristine Arctic National Wildlife Refuge, and to the current fiscal crisis that threatens the survival of public health centers, senior benefits, and public universities. In strictly *rentier* terms, Alaska is undeniably a resource-reliant state, as oil and gas provide between 65% and 90% of state revenues (McBeath et al., 2008). The central players in its local economy are not small- and medium-size enterprises, but rather large transnational corporations: MNCs such as Alaska's "Big Three"—BP, ExxonMobil and ConocoPhillips—make up 95% of total petroleum corporate income taxes paid to the state, or roughly 72% of total statewide corporate taxes.[5]

Not surprisingly, petroleum dominates state politics. The industry spends highly on public relations campaigns, and as a result citizens are highly politically aware of the role oil and gas plays in the state's political economy. Furthermore, it is estimated that one in three Alaska jobs depends on the oil industry either directly through industry employment or indirectly through labor sponsored by oil revenues (ISER, 2006). "In short," according to McBeath et al. (2008, 77), "Alaskans know who butters their bread, and Alaskans overwhelmingly favor oil and gas development." This high awareness helps allay concerns about the salience of oil wealth in public perceptions of government finances, which is generally lacking in studies of the oil-participation link in new oil producers.

Many Alaskans still lament the government squandering its fortune from the state's first oil boom in 1969.[6] But one positive outcome was the creation of the Longevity Bonus

---

[5]The "95%" figure is drawn from McBeath et al. (2008, 4). Total statewide corporate taxes amounted to $407.5 million in fiscal year 2014, of which $307.6 million corresponds to total petroleum corporate income taxes and $99.9 million corresponds to non-pteroleum corporate income taxes.

[6]Goldsmith (2002, 2) notes that the $900 million payment for exploration leases in 1968–1969 "seemed to disappear overnight, leaving behind not a legacy of new assets, but rather one of bigger government without an enhanced ability to pay for it."

Program (LBP), initiated in January 1973 (repealed in 1991 and officially closed in 2003) as a cash transfer based on age and length of residence.[7] Specifically, any person who was 65 or older and had maintained residency in the state for 25 or more years would qualify for a $125 per month cash payment ($562.70 per month in real 2019 dollars; $6,752.40 per year in real 2019 dollars). Applications were reviewed on a rolling basis each month and residents could apply as soon as they qualified.[8] In a way, the LBP operationalized the *rentier* state's relationship with its citizens: a monthly check in the mail based directly on government oil revenues.

So can Alaska be studied in comparison with oil-rich countries in the developing world often used in the study of the resource curse? Besides its high reliance on oil revenues and its minimal taxation, Alaska also suffers from resource-curse maladies such as corruption and low levels of transparency in public reporting of state spending.[9] Despite GDP per capita routinely between $70,000 and $100,000—largely a result of its small population,[10] reaching 738,432 in 2015—between 10% and 12% of Alaskans live in poverty, with up to 32% under the poverty line in rural districts.[11] And much like oil-producers such as Ecuador and Malaysia, Alaska is home to a high concentration of indigenous peoples (16% of the population), many of whom live in proximity to areas of petroleum extraction and distribution.

Yet in perhaps the most obvious ways Alaska is nothing like other oil-producing parts of the world. As part of the United States, Alaska's government is an advanced, representative democracy with universal suffrage and multiple layers of political constraints and balances.

---

[7]Alaska Statutes §47.45.010.

[8]Alaska Statutes §47.45.020. The Bonus was replaced with a means-tested cash transfer to senior citizens, irrespective of duration of residency in the state.

[9]Alaska ranked 49th out of 50 states in providing online access to government spending data, and received a failing grade on overall budget transparency. See "Following the Money 2015" U.S. Public Information Research Group report. Accessed 25 June 2016 from http://www.uspirg.org/sites/pirg/files/reports/FollowingtheMoney2015vUS.pdf.

[10]The state also has an extremely low population density of less than 1 person per square kilometer. This puts Alaska on par with resource-rich producers like Libya, Botswana, Mongolia, Namibia, and Kazakhstan—governments which despite great resource wealth have difficulties in providing public services to their populations living in remote corners of the state.

[11]United States Census Bureau (N.d.). Compare this figure, for instance, to rural poverty rates of 31% and 52% in oil-rich Iraq and Nigeria, respectively (World Bank WDI, *population below national poverty line: rural %*).

And despite not paying state taxes, Alaskans still file federal income taxes and are hence fiscally linked to the federal government. Unlike nearly all major oil-exporting countries (with the notable exception of Norway), Alaska maintains a vibrant, free press, and strong legal protections for its citizens against human and labor rights violations. But it is for this very reason that makes Alaska such an interesting case to test the argument: if oil wealth hinders participation in the context of a long-established democracy, how can we expect civic engagement to thrive under oil-to-cash policies in developing democracies, transitioning regimes, and dictatorships?

4   Evidence from the 1976 Alaska Longevity Bonus

I test the first hypothesis using data from Alaska's Longevity Bonus Program in the months before and after the 1976 election. To avoid confounding with Alaska's income tax repeal in September 1980 and the rollout of the Permanent Fund Dividend in 1982, I analyze the pre-1980 period of the Longevity Bonus and focus on political participation in the general election on November 2nd, 1976. This is the only general election between January 1973 and September 1980.

I collected data on LBP applicants from the Alaska State Archives. Individual applications to the program were available on microfilm for the June 1976 to May 1977 period.[12] I then matched names and addresses from these applications to voter files from the Alaska Division of Elections.[13] This resulted in turnout data and LBP transfer data for 614 registered voters.[14] Of these individuals, 166 reside in the core, which I define as the Anchorage area, and 448 reside outside the core.[15]

---

[12]I thank Wayne Norlund for digitizing and sending these applications electronically.

[13]I thank Brian Jackson at the Juneau office for his assistance in providing these data.

[14]The original sample from the LBP applications contained 1,092 individuals. Each application has a question asking applicants, "Are you registered to vote in Alaska?". Of these, 614 indicated yes (and provided a voter registration number), 73 indicated no, and the remaining 405 did not answer. If I impute zeroes for the latter group and control for registration status, the results below remain unchanged.

[15]While Juneau is Alaska's state capital, Anchorage is the center of Alaska's economy and civil society: the city accounts for roughly 50% of the state's total GDP (Bureau of Economic Analysis, 2018) and, especially

With this period in mind, I leverage the following as-if random assignment: residents who applied prior to the election and were approved prior to the election (*treated*) and residents who applied prior to the election and were approved after the election (*control*). These residents cannot readily sort themselves on either side of the approval threshold: applications were not approved necessarily in the order they were received but rather in a somewhat ad hoc fashion depending on what bin an application was placed in upon receipt at the state office. Residents who sent in applications prior to the election and who were approved prior to the election do not differ from residents who sent in applications prior to the election but were approved just after the election in any plausible way that is related to their potential outcomes of voting in the 1976 election. Thus, assignment to receive the Longevity Bonus just prior to the election can be considered as-if random in the months just before and after the threshold (Dunning, 2012).[16] As such, comparisons of turnout for individuals in the neighborhood of the threshold should allow an estimate of the causal effect of receiving direct oil cash handouts on political participation.

The plausibility of quasi-randomness in treatment assignment depends on the procedure used to sort applications. Each individual mailed an application form to the LBP office in Juneau that was then stamped with the receipt date. However, the office did not process applications in the order they were received; instead, the office assigned each application to a numbered batch and placed them into bins in the LBP office. The batch number determined the order applications were processed. For instance, batch 859 was reviewed in July 1976, batch 900 was reviewed in August 1976, batch 66 (numbers restarted above 1000) was reviewed in January 1977, and so on. These batches contained other forms and documents besides LBP applications, such that the office sometimes processed batches that did not contain any LBP applications. (This explains why batch numbers in the sample are not consecutive.)

---

in the period under study, served as the headquarters for nearly all civil society groups—namely NGOs, labor unions, activist groups, and senior organizations such as the Older Persons Action Group.

[16]Indeed, applicants who are approved several months or years prior to the election are likely different from those approved several months or years after the election.

The key assumption here is one of ignorability of the treatment assignment. Cash transfers for individuals with the same characteristics and applying at the same time should be just as likely to have been processed before the election as they were after the election. One way to assess the validity of this assumption is to model batch assignment. If applications were as-if randomly assigned to different batches, then batch assignment should not correlate with any individual attributes. A multinomial logit model confirms this is indeed the case. I use batch id number as a categorical dependent variable and date of application receipt, applicant gender, applicant age, number of years living in Alaska, whether or not an applicant was born in Alaska, and applicant zip code as independent variables. For 40 out of 41 batches with at least eight applicants, none of the covariates correlate with batch assignment at $p < 0.05$ (see the full table of p-values in Table A3).[17]

I examine the data using two different techniques. I start with a direct regression to estimate the average treatment effect of receiving the first cash transfer before the election on an individual's decision to vote. If the above assumptions hold, then a simple difference-of-means test using an interaction between treatment and a core-periphery dummy variable should provide an unbiased estimate of the ATE for individuals in the core and in the periphery. To increase precision and account for any imbalance across treatment and control groups, I also analyze the data by including individual-level covariates. Balance checks indicate statistical differences in characteristics between treated and control units (see Table A2) on age, residency, birth place, and ethnicity, so these variables are added as controls.

I next estimate the local average treatment effect using a sharp regression discontinuity design (Lee and Lemieux, 2010). This allows for a close-up comparison of individuals who received their transfers right before the election to those receiving it immediately afterwards. In practice, the narrowest window to assess this comparison is October-November 1976:

---

[17]For one batch, # 901, only zip code is correlated with batch assignment, with a p-value of 0.0299. This batch contains only 9 LBP applicants, but four happened to be from Fairbanks zip codes. Eight batches were excluded from the multinomial regression because they contained fewer than eight LBP applications; five of these eight only contain 1 application each. With seven independent variables in the model, coefficients could not be estimated for these batches.

treated individuals were sent their first checks on Friday October 1st, 1976, and received them no later than Thursday October 7th, while control individuals were sent their first checks on Monday November 1st, 1976, receiving them anytime between November 3rd and November 5th.[18] Since the running variable here is time, however, the approach is closer to an interrupted time-series than to a classic RD design.

Nevertheless, this design allows for an estimate of the LATE without making strong specification assumptions (Keele and Minozzi, 2013). There is no reason to suspect that turnout will differ for individuals just before and after the cutoff for any reason other than treatment assignment, and there is no reason to suspect that treatment assignment is driven by anything other than when applications were reviewed. Further, the design is credible in this context given that individuals cannot sort themselves on either side of the threshold: individuals could not choose which batch their applications were placed in at the state office.

4.1 Results

4.2 Inference using direct regression

Simply regressing turnout on whether or not an individual received the transfer before the election shows a negative effect (Table 1). Without including any controls (i.e, a difference of means estimator), I find that for individuals living outside the civic core receiving the Longevity Bonus before the election decreases the probability of voting by 10.1 percentage points compared to individuals getting their checks just after the election. The difference in treatment effects between core and periphery voters is 17.1 percentage points, such that treated individuals in the core are 7.0 percentage points more likely to vote than non-treated individuals in the core (though this positive effect is not statistically significantly different

---

[18]The time to mail a check from Juneau to the farthest point in the sample, Kaktovik, was at most four business days in 1976 (it is now down to three business days). In the case of someone living this far from Juneau, his/her check would have been sent on Monday the 1st and arrived no later than Friday, November 5th. Note that there is no way of ensuring compliance, i.e. that each individual cashed his/her check. But we can assume that given the amount of money involved, these checks were cashed at a high rate. We can further assume that individuals cashed their checks at roughly the same rates before and after the election.

	Dependent variable:		
	Voted in 1976 election		
	(1)	(2)	(3)
Treated	-0.101^{**}	-0.085^{**}	-0.076^{*}
	(0.047)	(0.043)	(0.042)
Core resident	-0.004	-0.047^{**}	-0.042^{*}
	(0.034)	(0.023)	(0.024)
Treated × Core	0.171^{***}	0.156^{***}	0.143^{***}
	(0.043)	(0.042)	(0.042)
Observations	614	554	545
R^2	0.013	0.046	0.048
F Statistic	2.776^{**} (df = 3; 610)	3.762^{***} (df = 7; 546)	2.981^{***} (df = 9; 535)
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 1: Regression results from OLS of turnout for 614 Longevity Bonus recipients who are registered voters. Treatment is whether an individual received the bonus prior to the 1976 election. Coefficient estimates refer to the effect of receiving a transfer on the likelihood of voting (0-1). Columns 1 show results from a model without any controls. Column 2 adds all controls for which the treatment is not balanced: age, duration of residency, Alaska Native, and Alaska-born individuals. Column 3 adds all controls in column 2 plus gender and a dummy for whether individuals are receiving Social Security. Standard errors are clustered by borough (which is the Alaskan equivalent of county).

from zero). Adding controls for which the treatment is not balanced—age, residency, ethnicity, and place of birth—slightly decreases magnitude of the effects, though at the expense of dropping 10% of the sample due to missing values. Treated individuals in the periphery are 8.5 percentage points less likely to vote than non-treated individuals in the periphery. Adding additional controls for gender and whether an individual receives Social Security also slightly decreases the effect size, now down to a 7.6 percentage-point difference between treated and non-treated individuals in the periphery.

	Optimal bandwidth	Half-bandwidth	Double-bandwidth
LATE estimate	28.63**	19.77***	18.33**
	(12.99)	(7.50)	(9.14)
Bandwidth size (months)	2.338	1.169	4.675
Observations	160	83	325
F	1.915	4.244*	2.403
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 2: Difference in turnout rates for individuals living outside the economic core, using a sharp regression discontinuity design adapted to an interrupted time series. Estimates refer to the effect of *not* receiving a transfer on the likelihood of voting, in percentage points (0-100). Estimates obtained using the `rdd` package in `R`, with a triangular kernel to determine the optimal bandwidth.

4.2.1 Inference using an interrupted time-series design

Zooming in on the months immediately before and after the election for periphery individuals, I find a larger negative effect.[19] Looking at just one month before and after the election, I find a 19.8 percentage point effect (Table 2). Individuals receiving their transfers in October show a roughly 60 percent turnout rate compared to those receiving their transfers in November (after the election), 80 percent of whom turned out to vote. Keep in mind these figures are for registered voters who are above 65 years of age—typically the likeliest individuals to vote in US elections. Increasing the bandwidth to two months on either side of the election shows a larger effect size of 28.6, but with slightly greater uncertainty. Over the course of the full timeframe, the difference in means of turnout by treatment group—estimated at 10.1 percentage points in Table 1—is plotted in Figure 1.

---

[19]There is insufficient data in the core-individual sample to construct optimal monthly bandwidths. However, using the same optimal bandwidths for the periphery-individual sample (2.338, 1.169, and 4.675 months), the LATE is between 17 and 20 percentage-point increase in voting for treated versus non-treated individuals, though none of the estimates are statistically significantly different from zero. This echoes the OLS results albeit at higher magnitudes with greater uncertainty.
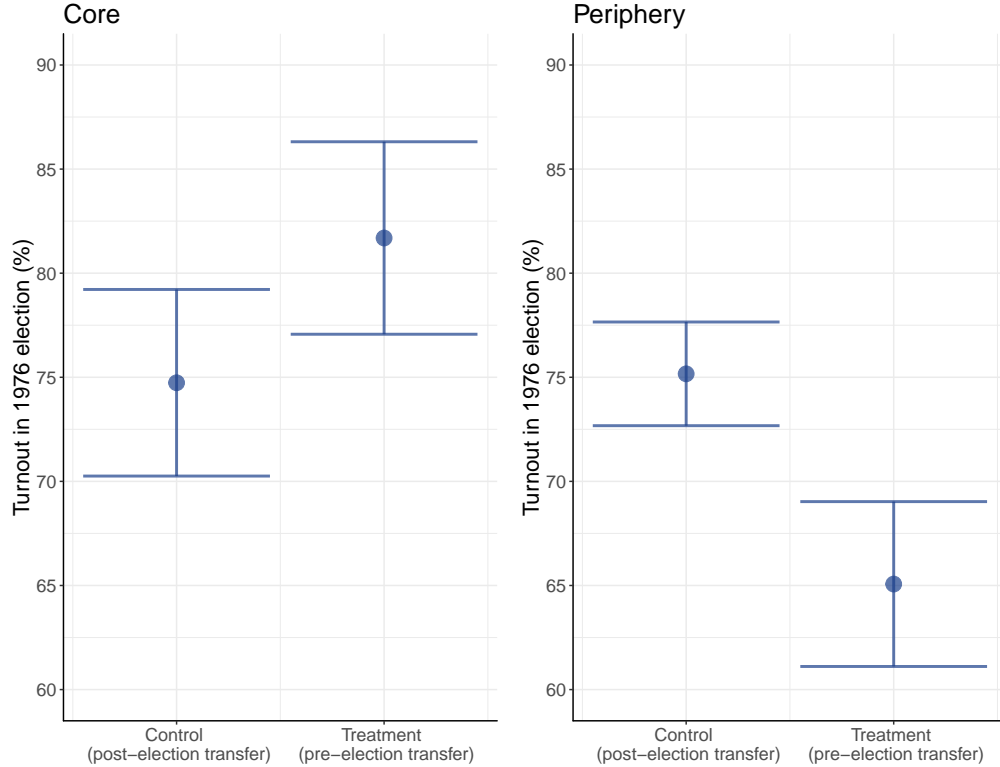
Figure 1: Difference in turnout between individuals receiving oil-to-cash transfers before the 1976 election (treatment) and those receiving transfers after the election (control), for individuals living in the civic core (left panel) versus those outside the civic core (right panel). Means by group are plotted along with 95% error bars.

4.3   Discussion of results

These different methods effectively show the same results as we would find by looking at individual cases in the data. Consider the following tale of two Fairbanksans (last names omitted to preserve anonymity). Don was born in June 1911 and moved to Alaska in June 1951. From November 1951 onwards, he had been living in a small house on the south bank of the Chena river in Fairbanks. He sent in his application for the Longevity Bonus on September 2nd, 1976; it was received by the state office in Juneau on September 9th, 1976 and placed in bin 66. This bin was not reviewed until December 8th, 1976, and so the check for Don's Longevity Bonus was sent out with an effective date of December 1st, 1976, meaning that he didn't get his first check until after the election.

Then we have Bob, born September 1911 and an Alaskan resident since April 1941. Bob lived just a mile upriver from Don in Fairbanks. Bob sent in his application on August 27th, 1976, and it was received in Juneau on September 1st, 1976. His application was placed in bin 963, which was reviewed on October 6th, 1976. His first Longevity Bonus check was sent out with an effective date of October 1st, 1976, arriving at his home in Fairbanks a few weeks before the election. Both men were born outside the state, neither lived in a state-assisted living facility ("Pioneer Homes"), neither received Social Security, and both were registered to vote at the time they sent in their applications.

Consistent with the argument above, Don voted in the 1976 general election, but Bob did not. In total, fifty-five people in Fairbanks sent in Longevity Bonus applications in the four months before and after the election; twenty received their first check prior to the election and the other thirty-five received theirs after the election. While thirty out of the thirty-five non-recipients voted in the general election (turnout rate of 86%), only twelve of the twenty who received their check prior to the election voted (turnout rate of 60%). This is roughly in line with the effect estimated with the interrupted time-series design.

Note that we don't have information as to what Bob did with his Longevity Check—and thus cannot assess the validity of the *autonomy* mechanism. Indeed, while we have been able to identify a negative effect of receiving oil-to-cash transfers on voting, there is not yet sufficient evidence to determine whether or not the culprit is that transfer money was used to opt out of social services. It could be the case that Bob did not vote in the election because he perceived the timely receipt of his oil-to-cash transfer as a signal of an effective government that did not need his approval or disapproval.

Furthermore, it is important to keep in mind that these results are still based on observational data. We do not know the exact process of assignment to treatment, nor do we know with precision the level of compliance with treatment. Specifically, while we know that individuals in the treatment group were sent their first checks prior to the election, it is not possible to ascertain whether these individuals received and cashed these checks prior to the

election. For this reason, the effects above should be interpreted as intent-to-treat effects.[20] That said, given the amounts of money involved it is slightly unreasonable to expect an individual receiving what is the equivalent of a $560 check in today's dollars not to deposit this money in a timely manner.

5 Oil-to-cash transfer recipients report greater individual autonomy

Why would receiving the cash transfer prior to the election disincentivize turnout? While I do not have detailed data on the sample of Longevity Bonus applicants used above, I address this question by drawing on a survey conducted on a comparable group of Longevity Bonus recipients. I specifically investigate whether the Bonus correlates with an increase in individual autonomy from the state, which could remove existing incentives to participate in politics.

The survey was implemented in 1975 by the Alaska Department of Health and Social Services and the Alaska Division of Public Assistance with the stated aim of assessing whether the Bonus "increase[s] the life-satisfaction of those who receive it," and whether it has "a beneficial economic impact upon the state by encouraging decreased dependency upon institutions...and social services" (Pagenkopf and Quinn, 1976). A sample of 472 senior citizens was drawn at random from the universe of 5,699 individuals of ages 65+ living in the state at the time the survey was administered. The sample was deliberately divided into three separate strata: recipients of the Longevity Bonus ("LBP", $n = 137$), recipients of Old-Age Assistance ("OAA", $n = 104$), and recipients of both ("OAA-LBP", $n = 231$). The intention of doing so was to construct a comparable "control" group of non-Bonus recipients that most closely resemble Bonus recipients. Those receiving OAA only are individuals who are for

---

[20]On the other hand, it is unlikely that there is noncompliance with being assigned to the control group since individuals could not cash in Longevity Bonuses that were not sent to them yet.

the most part not eligible to apply to the Bonus given the duration of their residency in Alaska, but would be eligible in terms of age. However, since OAA-only recipients might demographically differ from LBP-only recipients—eligibility for OAA depended on having less than $420 in monthly income at the time—a third group was analyzed of individuals who were eligible for both programs. Unfortunately, individual-level data is not available to estimate whether or not the results from the survey are statistically significantly different across groups or to discern the core-periphery divide across individuals.[21] Furthermore, neither comparison group serves as an ideal control group since the issue of selection bias remains unresolved. Despite these pitfalls, these data offer as close a window as possible into the behavior of Bonus recipients during the historical period under study.

The survey reveals that Bonus recipients are more financially and physically independent and less reliant on social services than non-recipients. Across a range of different outcomes, individuals in the LBP-only and OAA-LBP groups exhibit a greater degree of independence in everyday activities, such as transportation, shopping for food and clothing, paying for medical expenses, and participating in social and outdoor engagements. Figure 2 shows the average survey responses by group to questions relating to financial and physical independence. For instance, only 78.1% of respondents in the OAA-only group indicated that they are able to pay for non-Medicaid/non-Medicare medical expenses by themselves, compared to 90.7% and 96.7% of individuals in the OAA-LBP and LBP-only groups, respectively. Similarly, only 74.2% of OAA-only respondents stated that they are able to buy the food they want to eat, compared to 85.2% and 92.2% of OAA-LBP and LBP-only respondents. In answering whether they are able to participate in outdoor activities such as hiking, hunting, or fishing, there is a similar divide across the groups: 41.0% and 33.2% of LBP-only and OAA-LBP respondents said yes, while only 25.5% of OAA-only respondents answered affirmatively.[22] On the basis of these responses, the Bonus appears to provide individuals

---

[21]All reported figures are drawn from the survey summary presented in Pagenkopf and Quinn (1976).

[22]A "yes" in this case refers to whether the respondent participates once a month or less, while a "no" means not participating at all.
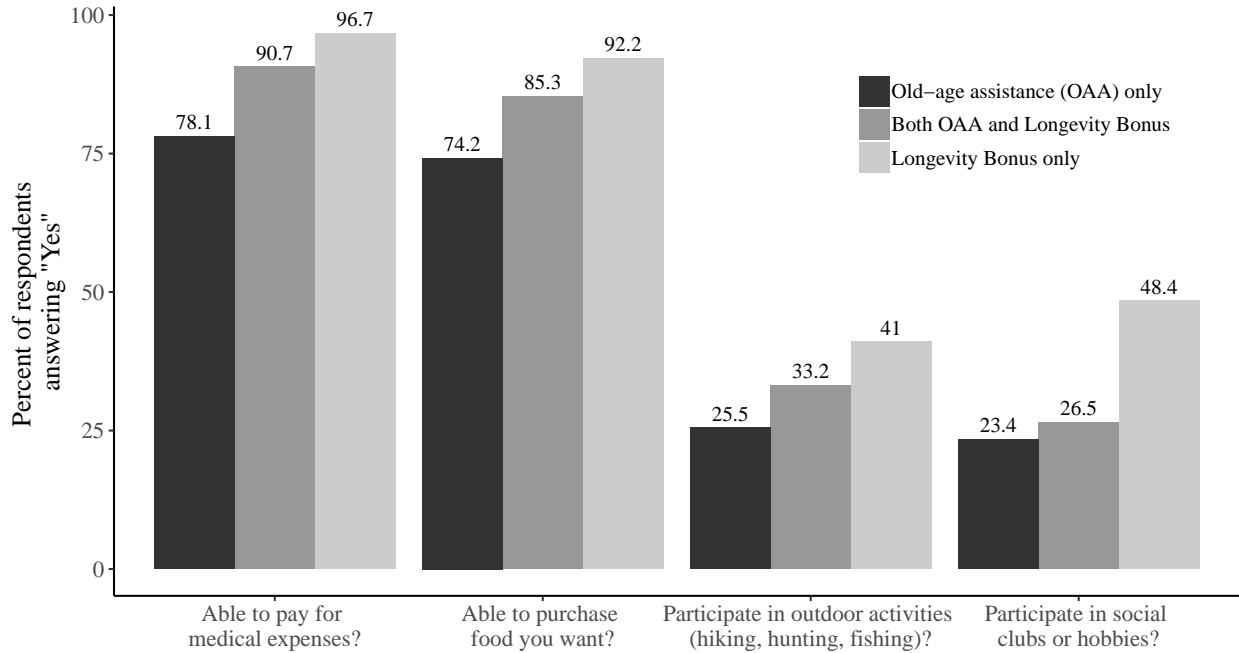
Figure 2

with a greater sense of personal independence when compared to non-recipients in terms of financial independence and in terms of the ability to participate in social and physical activities of their choosing.

The results paint a similar picture when looking at the usage of state-funded social services. Individuals in the OAA-only group are more likely to use public medical services, state-provided homecare services, and public food programs when compared to individuals in the OAA-LBP and LBP-only groups. Figure 3 shows the average survey responses to questions relating to social service use. The two starkest comparisons across groups are in the usage of food stamps and homemaker services, which refer to state-provided housekeeping and/or nursing care in the individual's own home. For these particular services, it is best to compare just the OAA-only and OAA-LBP groups, given that these services are fully available to all individuals in both groups.[23] OAA-only respondents were twice as likely

---

[23]Anyone receiving OAA is automatically eligible for homemaker services, and the income qualifications for OAA are the same for food stamps. The analysis here excludes individuals living in state institutions such as nursing homes, since these respondents are not eligible for either program. The LBP-only respondents
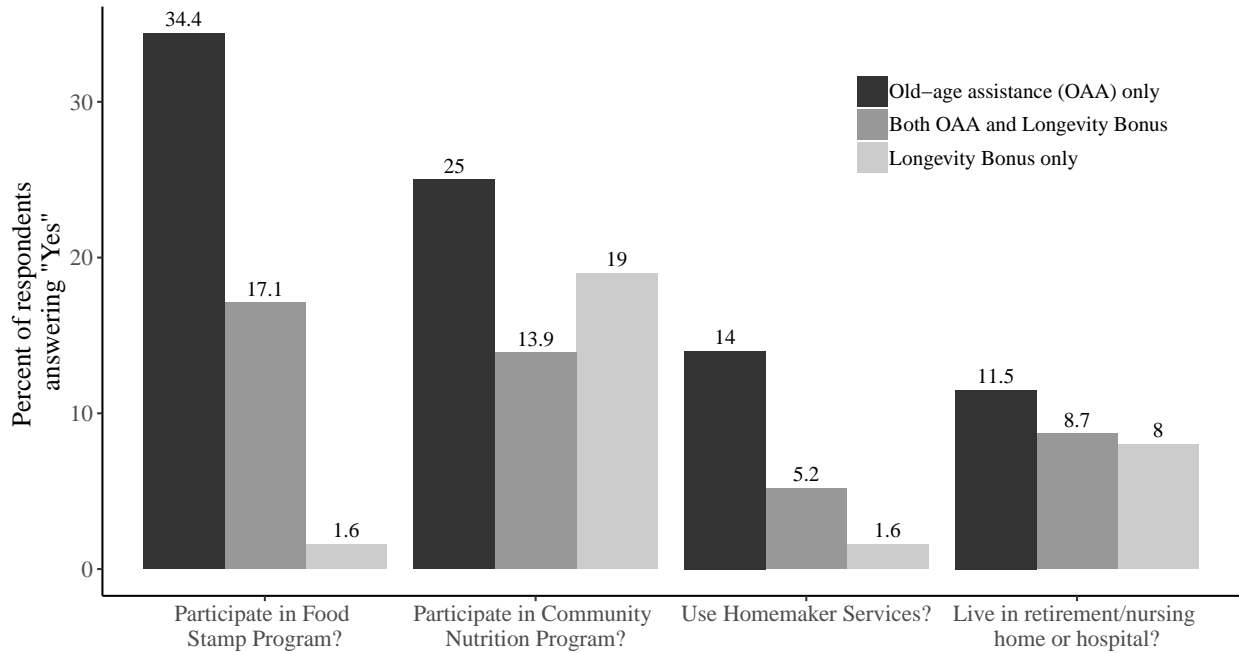
Figure 3

to use food stamps (34.4% versus 17.1%), and were just under three times as likely to use homemaker services (14.0% versus 5.2%).

The composition of the groups themselves also indicates how the Bonus enables individuals to remain off of social services: in terms of income eligibility, nearly 30% of LBP-only respondents qualify for OAA but are deliberately not OAA recipients. And when asked whether they used Medicare, Medicaid, or the Public Health Service to pay for medical expenses, 91.3% of OAA-only respondents said yes while only 78.8% of LBP-only respondents said yes. This is especially surprising given that all individuals in the sample are qualified for Medicare in particular. In general, the evidence suggests that the Bonus provides a means for decoupling from state-provided social services if so desired—despite being entitled to and eligible for these programs.

_____

are not as relevant for these comparisons, as the majority of these respondents indicated that they either do not need (35.5%) or are ineligible for (16.3%) food stamps.

6 Conclusion

Answering whether resource wealth erodes democratic principles remains a critical puzzle in the study of natural resource politics in particular and comparative political economy in general. In the context of Alaskan state politics, I show that the direct distribution of resource wealth in the form of individual cash transfers reduces political participation. Alaskans living outside the civic core who received a dividend of \$125/month (in nominal 1976 dollars) before the 1976 general election were roughly 8 to 20 percentage points less likely to vote than individuals with similar characteristics who received their first transfers after the election.

These findings offer evidence supporting microfoundations of the political resource curse even in the context of an advanced, long-established democracy. Specifically, these results shed light on the initial steps leading up to the failure of democracy—by breaking down the desire for civic engagement—in the context of resource-reliant countries. While we do not expect the state of Alaska to succumb to dictatorship, a reduction in political participation as a consequence of natural resource wealth is troubling, especially considering these effects in the highly politically active context of registered voters over the age of 65. It is in some ways refreshing, then, that current threats to these payments in Alaska have spurred increased engagement in politics in 2019—especially among senior citizens whose benefits are on the proverbial fiscal chopping block.[24]

Finally, this study provides policy implications regarding the timing of distribution for unconditional cash transfers such as the Longevity Bonus. Recent proponents of a carbon tax, for instance, have argued that public support for climate change policy could be increased if part of these tax revenues were distributed as carbon dividends in the form of direct cash transfers (Baker III et al., 2017; Kotchen, Turk and Leiserowitz, 2017). Advocates of the

---

[24]Alaskan seniors have grown more vocal in demanding that these programs be exempt from upcoming budget cuts, in part leading to senior involvement in the effort to recall Alaskan Governor Mike Dunleavy. The "Recall Dunleavy" campaign, for example, is co-chaired by octogenarian Joe Usibelli, Sr., and nonagenarians Arliss Sturgulewski and Vic Fischer. See Zachariah Hughes, "Dunleavy reverses potential cuts to senior benefits," *Alaska Public Media* (Aug 12, 2019).

universal basic income see the payment of unconditional cash transfers as a means to decrease poverty, reduce inequality, and improve economic development in general (see Lowrey, 2018). If policymakers want to avoid the participation-diminishing effects of cash transfers, then the distribution of transfers should be timed ideally after major political effects such as elections or referendums. This is all the more important when considering annual lump-sum payments of cash transfers, where the windfall shock could exacerbate these effects.

Still, the overall negative effects on civic engagement of the Alaska Longevity Bonus should not be under-appreciated. That this can happen even in an advanced, developed democracy such as the United States suggests great caution with how hydrocarbon transfers will be implemented in developing democracies such as Brazil, Nigeria, and Indonesia, or in transitional systems such as Iraq, Myanmar, and South Sudan.

References

Anderson, Lisa. 1987. "The State in the Middle East and North Africa." *Comparative Politics* 20(1):1–18.

Baird, Sarah J., Craig McIntosh and Berk Özler. 2011. "Cash or Condition? Evidence from a Cash Transfer Experiment." *Quarterly Journal of Economics* 126:1709–1753.

Baker III, James A, Martin Feldstein, Ted Halstead, N Gregory Mankiw, Henry M Paulson Jr, George P Shultz, Thomas Stephenson and Rob Walton. 2017. "The conservative case for carbon dividends." *Climate Leadership Council* .

Beblawi, Hazem and Giacomo Luciani. 1987. *The Rentier State.* London: Croom Helm.

Bhavnani, Rikhil R. and Noam Lupu. 2016. "Oil Windfalls and the Political Resource Curse: Evidence from a Natural Experiment in Brazil." Unpublished Manuscript, University of Wisconsin.

Blair, Harry. 2000. "Participation and accountability at the periphery: democratic local governance in six countries." *World development* 28(1):21–39.

Blattman, Christopher, Nathan Fiala and Sebastian Martinez. 2014. "Generating Skilled Employment in Developing Countries: Experimental Evidence from Uganda." *Quarterly Journal of Economics* 129:697–752.

Brollo, Fernanda, Tommaso Nannicini, Roberto Perotti and Guido Tabellini. 2013. "The Political Resource Curse." *The American Economic Review* 103(5):1759–1796.

Brooks, Sarah M. and Marcus J. Kurtz. 2016. "Oil and Democracy: Endogenous Natural Resources and the Political 'Resource Curse'." *International Organization* 70(2):279–311.

Campbell, Andrea Louise. 2003. *How Policies Make Citizens: Senior Political Activism and the American Welfare State.* Princeton University Press.

Crystal, Jill. 1989. "Coalitions in Oil Monarchies: Kuwait and Qatar." *Comparative Politics* 21(4):427–443.

de la Cuesta, Brandon, Helen V. Milner, Daniel Nielson and Steve Knack. 2017. "Taxation without representation?" *World Bank Policy Research* 8137.

De La O, Ana L. 2013. "Do conditional cash transfers affect electoral behavior? Evidence from a randomized experiment in Mexico." *American Journal of Political Science* 57(1):1–14.

Diaz-Cayeros, Alberto, Federico Estevéz and Beatriz Magaloni. 2016. *The Political Logic of Poverty Relief: Electoral Strategies and Social Policy in Mexico.* Cambridge University Press.

Downs, Anthony. 1957. *An Economic Theory of Democracy.* New York: Harper Collins.

Dunning, Thad. 2012. *Natural Experiments in the Social Sciences: A Design-Based Approach.* New York, N.Y.: Cambridge University Press.

Fiorina, Morris. 1978. "Economic retrospective voting in American national elections: A micro-analysis." *American Journal of Political Science* 22(2):426–443.

Galiani, Sebastian, Nadya Hajj, Pablo Ibarraran, Nandita Krishnaswamy and Patrick J McEwan. 2016. Electoral reciprocity in programmatic redistribution: Experimental Evidence. Technical report National Bureau of Economic Research. Working Paper 22588.

Goldberg, Ellis, Erik Wibbels and Eric Mvukiyehe. 2008. "Lessons from Strange Cases: Democracy, Development, and the Resource Curse in the U.S. States." *Comparative Political Studies* 41(4):477–514.

Goldsmith, Scott. 2002. "The Alaska Permanent Fund Dividend: An Experiment in Wealth Distribution.".

Grossman, Guy, Laura Paler and Jan Pierskalla. 2016. "Public perceptions of oil governance in Uganda." Unpublished Manuscript, University of Pennsylvania.

Han, Hahrie. 2014. *How Organizations Develop Activists: Civic Associations and Leadership in the 21st Century.* Oxford, UK: Oxford University Press.

Haushofer, Johannes and Jeremy Shapiro. 2016. "The Short-Term Impact of Unconditional Cash Transfers to the Poor: Experimental Evidence from Kenya." *Quarterly Journal of Economics* 131(4):1973–2042.

Herb, Michael. 1999. *All in the Family: Absolutism, Revolution and Democracy in the Middle Eastern Monarchies.* State University of New York Press.

Herb, Michael. 2005. "No Representation without Taxation? Rents, Development, and Democracy." *Comparative Politics* 37(3):297–316.

Holbein, John B., Randall Akee, Emilia Simeonova William Copeland and Jane Costello. 2017. "The Effect of Unconditional Cash Transfers on Intergenerational Patterns of Voter Participation." Brigham Young University manuscript.

Hsieh, Chang-Tai. 2003. "Do Consumers React to Anticipated Income Changes? Evidence from the Alaska Permanent Fund." *American Economic Review* 93(1):397–405.

Imai, Kosuke, Gary King and Carlos Velasco Rivera. 2016. "Do Nonpartisan Programmatic Policies Have Partisan Electoral Effects? Evidence from Two Large Scale Randomized Experiments.".
URL: *http://gking.harvard.edu/files/gking/files/progpol.pdf?m=1456583131*

ISER. 2006. *Understanding Alaska: People, Economy, and Resources.* Anchorage: University of Alaska Press.

Karl, Terry Lynn. 1997. *The Paradox of Plenty: Oil Booms and Petro-States.* Berkeley, CA: University of California Press.

Keele, Luke and William Minozzi. 2013. "How much is Minnesota like Wisconsin? Assumptions and counterfactuals in causal inference with observational data." *Political Analysis* 21(2):1–24.

Kotchen, Matthew J, Zachary M Turk and Anthony A Leiserowitz. 2017. "Public willingness to pay for a US carbon tax and preferences for spending the revenue." *Environmental Research Letters* 12(9):094012.

Labonne, Julien. 2013. "The local electoral impacts of conditional cash transfers: Evidence from a field experiment." *Journal of Development Economics* 104:73–88.

Lee, David S and Thomas Lemieux. 2010. "Regression discontinuity designs in economics." *Journal of Economic Literature* 48(2):281–355.

Lowrey, Annie. 2018. *Give People Money.* Penguin.

Mahdavy, Hussein. 1970. The Patterns and Problems of Economic Development in Rentier States: the Case of Iran. In *Studies in Economic History of the Middle East*, ed. M A Cook. London, UK: Oxford University Press.

Marshall, T. H. 1964. *Class, Citizenship, and Social Development.* Chicago: University of Chicago Press.

Mayhew, David. 1974. *Congress: The Electoral Connection.* New Haven: Yale University Press.

McBeath, Jerry, Matthew Berman, Jonathan Rosenberg and Mary F. Ehrlander. 2008. *The Political Economy of Oil in Alaska.* Boulder, CO: Lynne Reiner.

McGuirk, Eoin F. 2013. "The illusory leader: Natural resources, taxation, and accountability." *Public Choice* 154(3–4):285–313.

Menaldo, Victor. 2016. *From Institutions Curse to Resource Blessing.* New York: Cambridge University Press.

Mettler, Suzanne. 2018. *The Government-Citizen Disconnect.* Russell Sage Foundation.

Moss, Todd, Caroline Lambert and Stephanie Majerowicz. 2015. *Oil-to-Cash: Fighting the Resource Curse through Cash Transfers.* Washington, DC: Center for Global Development.

Pagenkopf, Ray and Christine Quinn. 1976. Alaska Longevity Bonus Impact Survey: 1975. Technical report Department of Health and Social Services and Division of Public Assistance.

Paler, Laura. 2013. "Keeping the public purse: An experiment in windfalls, taxes, and the incentives to restrain government." *American Political Science Review* 107(04):706–725.

Pierson, Paul. 1993. "Review: When Effect Becomes Cause: Policy Feedback and Political Change." *World Politics* 45(4):595–628.

Riker, William H. and Peter C. Ordeshook. 1968. "A Theory of the Calculus of Voting." *American Journal of Political Science* 62(1):25–42.

Ross, Michael L. 2001. "Does Oil Hinder Democracy?" *World Politics* 53(3):325–361.

Ross, Michael L. 2015. "What Have We Learned about the Resource Curse?" *Annual Review of Political Science* 18(1):239–259.

Simmons, Joel. 2016. "Resource Wealth and Women's Economic and Political Power in the U.S. States." *Comparative Political Studies* 49(1):115–152.

Skocpol, Theda. 1992. *Protecting soldiers and mothers: The political origins of social policy in the United States.* Cambridge, MA: Harvard University Press.

Skocpol, Theda and Morris P Fiorina. 2004. *Civic engagement in American democracy.* Brookings Institution Press.

Smith, Benjamin. 2007. *Hard Times in the Land of Plenty: Oil Politics in Iran and Indonesia.* Ithaca, NY: Cornell University Press.

Soss, Joe. 1999. "Lessons of welfare: Policy design, political learning, and political action." *American Political Science Review* 93(2):363–380.

Stokes, Susan C, Thad Dunning, Marcelo Nazareno and Valeria Brusco. 2013. *Brokers, Voters, and Clientelism: the puzzle of distributive politics.* Cambridge University Press.

Tilly, Charles. 1975. *The Formation of National States in Europe.* Princeton University Press.

United States Census Bureau. N.d. "State Population Estimates: Annual Time Series.". **URL:** *https://www.census.gov/popest/data/historical/*

Verba, Sydney, Kay Lehman Schlozman and Henry E. Brady. 1995. *Voice and Equality: Civic Voluntarism in American Politics.* Cambridge, MA: Harvard University Press.

Verba, Sydney and Norman H. Nie. 1972. *Participation in America: Political Democracy and Social Equality.* New York, NY: Harper & Row.

Wiktorowicz, Quintan. 1999. "The Limits of Democracy in theMiddle East: The Case of Jordan." *Middle East Journal* 53(4):606–20.

Zárate, R. A., E. Conover, A. Camacho and J. E. Baez. 2013. "Conditional cash transfers, political participation, and voting behavior." Unpublished manuscript.

Zucco, Cesar. 2013. "When payouts pay off: Conditional cash transfers and voting behavior in Brazil 2002–10." *American Journal of Political Science* 57(4):810–822.

Appendix

	Dependent variable:		
	Voted in 1976 election		
	(1)	(2)	(3)
Treated	−0.143***	−0.069**	−0.061*
	(0.041)	(0.034)	(0.033)
Core resident	0.059**	−0.037*	−0.032
	(0.025)	(0.020)	(0.023)
Treated × Core	0.085**	0.129***	0.120***
	(0.037)	(0.032)	(0.033)
Observations	1,092	626	613
R^2	0.051	0.187	0.190
F Statistic	14.498*** (df = 4; 1087)	17.770*** (df = 8; 617)	14.087*** (df = 10; 602)

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table A1: Regression results from OLS of turnout for 1,092 Longevity Bonus recipients, imputing zeroes in missing values for registration status and controlling for voter registration. Treatment is whether an individual received the bonus prior to the 1976 election. Coefficient estimates refer to the effect of receiving a transfer on the likelihood of voting (0-1). Columns 1 show results from a model with only registration status as a control variable. Column 2 adds all controls for which the treatment is not balanced: age, duration of residency, Alaska Native, and Alaska-born individuals. Column 3 adds all controls in column 2 plus gender and a dummy for whether individuals are receiving Social Security. Standard errors are clustered by borough (which is the Alaskan equivalent of county). Compare to results in Table 1.

	Control mean	Treated mean	Difference	p-value
Gender (1 = male)	0.59	0.61	0.02	0.69
Birth year	1909.83	1909.24	-0.59	0.04
First year of residency	1933.02	1936.51	3.49	0.01
Born in Alaska (1 = yes)	0.36	0.24	-0.12	0.00
Social Security recipient (1 = yes)	0.67	0.70	0.03	0.42
Core resident (1 = yes)	0.24	0.33	0.09	0.02
Alaska Native (1 = yes)	0.32	0.19	-0.13	0.00

Table A2: Balance table: Difference of means for applicant attributes receiving transfers prior to the election (treatment) versus those receiving transfers after the election (control). Age and residency are the two requirements for the Longevity Bonus, as applicants must be 65 or older and must be living in Alaska for 25 or more years. The table indicates the sample is not well-balanced across geography (core versus periphery), age, residency, place of birth, or ethnicity. These variables are therefore included as controls when estimating treatment effects.

Batch number	(Intercept)	Gender	Age	Residency	Date received	Mailing zip code	Alaska-born	Number of observations
45	1.00	1.00	1.00	1.00	1.00	1.00	1.00	32
66	1.00	1.00	1.00	1.00	1.00	0.99	1.00	32
100	1.00	1.00	1.00	1.00	1.00	0.99	1.00	35
112	1.00	1.00	1.00	1.00	0.99	0.91	1.00	22
146	1.00	1.00	0.98	1.00	0.99	0.58	1.00	16
156	0.99	1.00	0.94	1.00	1.00	0.15	1.00	13
167	1.00	1.00	1.00	1.00	1.00	0.99	1.00	35
200	1.00	1.00	1.00	1.00	1.00	0.97	1.00	24
210	1.00	1.00	1.00	1.00	1.00	0.97	1.00	21
220	1.00	1.00	1.00	1.00	1.00	1.00	1.00	31
253	1.00	1.00	1.00	1.00	1.00	0.97	1.00	22
263	1.00	1.00	1.00	1.00	1.00	1.00	1.00	33
273	1.00	1.00	1.00	1.00	1.00	0.99	1.00	25
306	1.00	1.00	1.00	1.00	1.00	0.99	1.00	35
316	1.00	1.00	1.00	1.00	1.00	0.99	1.00	31
326	1.00	1.00	1.00	1.00	1.00	0.97	1.00	51
361	1.00	1.00	1.00	1.00	1.00	1.00	1.00	28
371	1.00	1.00	1.00	1.00	1.00	0.95	1.00	20
381	1.00	1.00	1.00	1.00	1.00	0.99	1.00	38
428	1.00	1.00	1.00	1.00	1.00	0.98	1.00	37
437	1.00	1.00	1.00	1.00	1.00	1.00	1.00	28
473	1.00	1.00	1.00	1.00	1.00	0.98	1.00	41
483	1.00	1.00	0.99	1.00	1.00	0.75	1.00	16
493	1.00	1.00	1.00	1.00	1.00	0.97	1.00	50
531	1.00	1.00	1.00	1.00	1.00	0.95	1.00	21
540	1.00	1.00	0.99	1.00	1.00	0.75	1.00	16
550	1.00	1.00	1.00	1.00	1.00	1.00	1.00	28
589	0.99	1.00	0.94	1.00	1.00	0.15	1.00	10
598	1.00	0.99	0.97	1.00	1.00	0.47	1.00	13
837	1.00	1.00	1.00	1.00	1.00	1.00	1.00	29
859	0.97	1.00	0.79	1.00	1.00	**0.01**	1.00	10
869	1.00	1.00	1.00	1.00	1.00	0.99	1.00	35
879	1.00	1.00	1.00	1.00	1.00	1.00	1.00	28
901	0.99	0.99	0.92	1.00	1.00	**0.08**	1.00	9
911	0.99	1.00	0.95	1.00	1.00	0.25	1.00	11
921	1.00	1.00	1.00	1.00	1.00	0.97	1.00	50
943	0.99	0.99	0.96	1.00	1.00	0.36	1.00	12
953	0.99	0.99	0.93	1.00	1.00	0.15	1.00	12
963	1.00	1.00	1.00	1.00	1.00	0.99	1.00	32
985	1.00	1.00	0.99	1.00	1.00	0.82	1.00	17

Table A3: Multinomial logit of assignment to batch number at the state office in Juneau. Each column corresponds to a covariate, each row corresponds to each batch number. Numbers in cells refer to the estimated p-value of the correlation between each covariate and the likelihood of being assigned to a given batch. The last column indicates how many Longevity Bonus applications were contained in each batch. Cells in bold font indicate a p-value less than 0.10.