Do cash transfers decrease political participation? Evidence from a natural experiment in Alaska

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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. Yet little is known about the impact of such unconditional transfers on politics and political attitudes. This paper leverages a natural experiment in 1976 Alaska to find that oil-to-cash transfers diminish participation in politics. The data show that individuals receiving their transfers right before the election are roughly 16 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 the political effects of unconditional cash transfers.

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1 Introduction

In resource-rich countries around the world, governments devise different methods of distributing natural resource wealth to their citizens. The typical policy adopted is 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.

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. Following the logic of the *rentier* state theory, I argue that these transfers have an acquiescence-inducing effect, such that individuals receiving this money are 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 to substitute political consumption for non-political goods.

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, a now-defunct oil-to-cash transfer program that preceded the state's current Permanent Fund Dividend. Specifically, 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 $100 (in nominal 1976 dollars) before the election are roughly 16 to 20 percentage points less likely to vote than those receiving their first checks after the election.

This paper brings four innovations to the literature on natural resource politics and to the broader study of energy policy. First, I empirically assess the widely circulated but largely untested mechanism of citizen acquiescence driving the political maladies associated with the resource curse (Ross, 2015). 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., 2016).

Second, I provide a tough test of the *rentier* state 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.

Third, I contribute to the ongoing debate within the study of oil politics on how to measure oil wealth. The variable I employ offers a precise, per capita amount of direct resource wealth that citizens actually receive every month—not an estimate derived from noisy data on production and prices (Ross and Mahdavi, 2014) or reserves and percentages of GDP (Cotet and Tsui, 2013), nor a measure based on one-time amounts given to survey or experiment participants.

Fourth, I conduct a policy evaluation of oil-to-cash programs, using the example of the Longevity Bonus, on heretofore unexamined consequences relating to political behavior. Studies show that Alaskan oil-to-cash transfers 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 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 oil-to-cash transfers on political participation and civic engagement.

2 How oil-to-cash transfers depress participation

According to resource curse theory, oil revenues allow leaders to win citizen acquiescence through direct distribution rather than popular support through political representation (Anderson, 1987; Crystal, 1989; Herb, 1999). This argument rests on the theory of the *rentier* state, wherein 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 citizen-state 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 posits that natural resource wealth hinders democracy and good governance (for a review, see Ross, 2015).

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. A large oil-to-cash transfer in Alaska, for instance, may provide a would-be voter the means to take a vacation or go hunting in lieu of going to the polls on election day.

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 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).[1] 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.

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

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

turn out to vote, while individual i will be using his cash transfer to consume his preferred non-political good.

Previous empirical research on how resource revenues affect individual behavior finds 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., 2016). But none of these studies addresses the impact of directly receiving resource revenues on individual political behavior.

Here, I leverage a natural experiment with the distribution of an oil-to-cash transfer in Alaska that precisely identifies this effect. Due to an as-good-as random decision to give oil-to-cash transfers to some voters before the election and to the rest after the election, I can empirically test the following hypothesis:

Hypothesis 1: *Citizens receiving resource handouts are less likely to participate in politics than citizens not receiving handouts.*

Rentier theory would predict that diverting oil money to citizens should disincentivize individuals from civic engagement. In contrast, the literature on conditional cash transfers would predict that such spending increases support for the incumbent, and hence increased involvement in politics. 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.

3 The Alaska Case

Alaska presents a highly relevant yet unorthodox context for testing hypotheses regarding the resource curse. 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. 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.[2]

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.[3] But one positive outcome was the creation of the Longevity Bonus Program (LBP), initiated in January 1973 (and repealed in 1991) as a cash transfer based

[2]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.

[3]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."

on age and length of residence.[4] Specifically, any person who was 65 or older and had maintained residency in the state for 25 or more years would qualify for a $100 per month ($6,648 per year in real 2015 dollars) cash payment. Applications were reviewed on a rolling basis each month and residents could apply as soon as they qualified.[5] 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.[6] Despite GDP per capita routinely between $70,000 and $100,000—largely a result of its small population,[7] 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.[8] 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. 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

[4] Alaska Statutes §47.45.010.

[5] Alaska Statutes §47.45.020.

[6] 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.

[7] 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.

[8] 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 %*).

(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 above hypothesis: 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 Data and methods

I test the above 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.[9] I then matched names and addresses from these applications to voter files from the Alaska Division of Elections.[10] This resulted in turnout data and LBP transfer data for 448 registered voters.[11]

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:

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

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

[11] 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.

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).[12] 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.[13]

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.

[12] 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.

[13] These batches contained other forms and documents besides LBP applications, such that the office processed batches that did not contain any LBP applications. This explains why batch numbers in the sample are not consecutive.

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, and applicant zip code as independent variables. For 40 out of 41 batches with at least seven applicants, none of the covariates correlate with batch assignment at $p < 0.05$ (see the full table of p-values in Table A2).[14]

I examine the data using three 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 should provide an unbiased estimate of the ATE. To increase precision, I also analyze the data by including individual-level covariates and municipal-level fixed effects. As balance checks indicate no statistical differences in characteristics between treated and control units (see Table A1), I do not expect adding covariates to the model to influence the results in any substantive way.

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: 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.[15] Since the running variable here is time, however, the approach is closer to

[14] 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 seven LBP applications; five of these eight only contain 1 application each. With six independent variables in the model, coefficients could not be estimated for these batches.

[15] 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.

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.

Since it is impossible to rule out bias from unobserved confounders, I try to reduce unit heterogeneity through a focused comparison of individuals using matching techniques. In this case, I construct a suitable control individual for each treated individual based on their characteristics. Because data are limited to what is reported on LBP applications, I cannot control for prior voting habits or partisanship.[16] But I attempt to capture these characteristics using what information is available: applicant age, gender, duration of residency in Alaska, and physical location. I use genetic matching (Diamond and Sekhon, 2013) to assemble two statistically equivalent groups based on these characteristics. Note that the subsequent regression on these groups provides the average treatment effect on the treated as opposed to the ATE and LATE in the above methods.

5 Results

5.1 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 or geographical fixed effects (i.e, a difference of means estimator), I find that receiving the Longevity Bonus

[16] Unfortunately, I was unable to obtain voting data for the 1972 elections (or prior) from the Division of Elections office in Juneau. These voter rolls are also on microfilm but could not be located in their archives.

	Dependent variable:					
	Voted in 1976 election					
	OLS			*Logit*		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	−0.101**	−0.112**	−0.111**	−0.485**	−0.695**	−0.697**
	(0.045)	(0.050)	(0.052)	(0.219)	(0.299)	(0.317)
Controls?	N	N	Y	N	N	Y
Muni. fixed effects?	N	Y	Y	N	Y	Y
Observations	448	448	448	448	448	448
R^2	0.011	0.358	0.372			
Adjusted R^2	0.009	0.120	0.129			
Log Likelihood				−263.739	−175.248	−171.457
Akaike Inf. Crit.				531.478	594.496	594.915
Note:						*p<0.1; **p<0.05; ***p<0.01

Table 1: Regression results from OLS and logit models of turnout for 448 Longevity Bonus recipients and registered voters. Estimates refer to the effect of receiving a transfer on the likelihood of voting (0-1). Columns 1 and 4 show results from models without any controls, columns 2 and 5 show results from models with municipal fixed effects, and columns 3 and 6 show results from models with municipal fixed effects and controls for age, duration of residency, and gender.

before the election decreases the probability of voting by 10.2 percentage points compared to individuals getting their checks just after the election. Adding controls and municipal fixed effects only slightly changes the results, with effect sizes ranging from an 11.1 to 11.2 percentage point decrease in the likelihood of voting. Logit models (Table 1, columns 4-6) show the same effect.

5.2 Inference using an interrupted time-series design

Zooming in on the months immediately before and after the election, I find a larger negative effect. 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.

	Optimal bandwidth	Half-bandwidth	Double-bandwidth
LATE estimate	28.63**	19.77**	18.33*
	(14.31)	(8.93)	(10.12)
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 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.

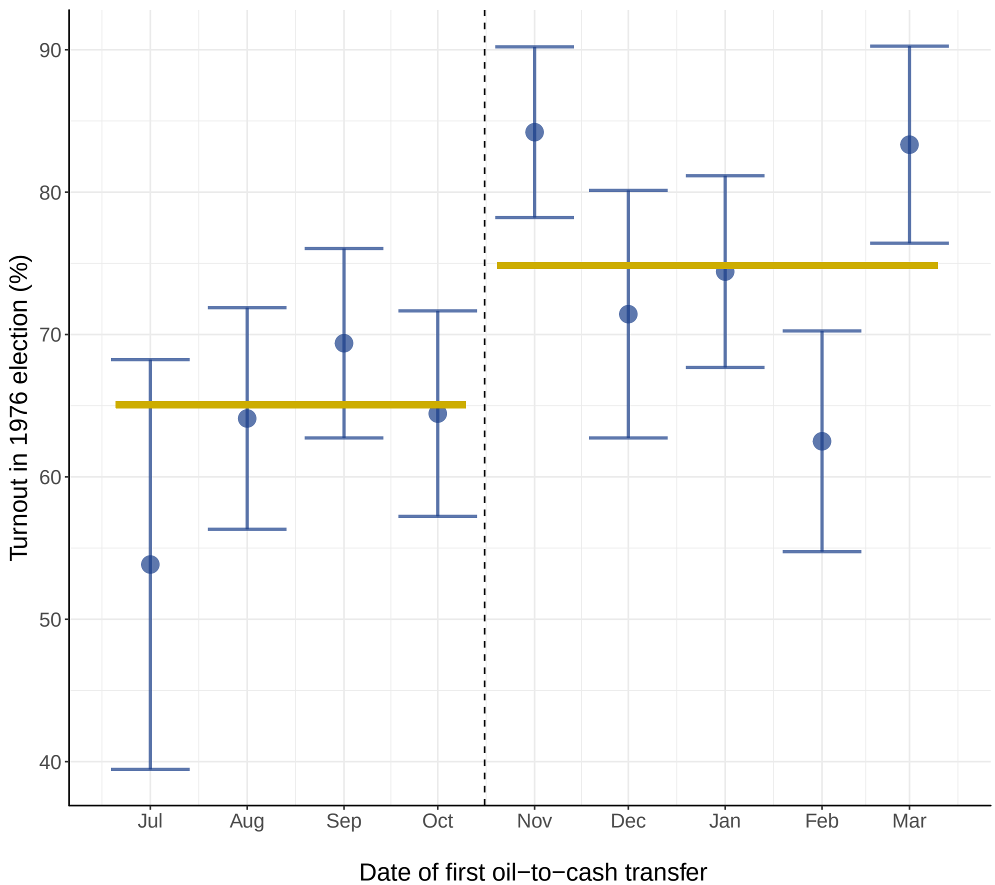

Figure 1: Plot of the interrupted time-series. Difference in turnout between individuals receiving oil-to-cash transfers before the 1976 election and those receiving transfers after the election. Monthly means are plotted along with 95% error bars in blue; treatment vs. control means plotted in gold.

Matched estimate	-15.868
Std. error	6.049
p-value	0.0087057
Observations	448
Treated observations	146
Matched number of observations	146
Balance	*KS p-value*
Gender	1.00
Age	0.97
Residency	0.41
Latitude	0.40
Longitude	0.38

Table 3: Difference in turnout rates using genetic matching. Individuals are matched on age, duration of residency, gender, and geographical location (latitude and longitude). Bottom panel shows the KS bootstrap p-value for each covariate after matching. Estimates obtained using the *GenMatch* function from the `Matching` package in `R`.

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. Monthly averages of turnout are plotted in Figure 1, with a non-parametric smoother with confidence bands plotted for individuals on either side of the threshold.

5.3 Inference using matching

Matching methods provide a different look at the data but give a substantively similar result. With genetic matching on age, duration of residency, gender, and geographic coordinates, I find an estimated average treatment effect on the treated of -15.9 percentage points with a standard error of 6.0 percentage points ($p = 0.0087$).[17] Balance checks shown in Table 3 indicate a good fit, with the minimum p-value at 0.38. The matching estimate suggests that a pair of individuals with similar characteristics differ in their likelihood of turning out to vote by roughly 16 percentage points if one person received their Longevity Bonus before the election while the other received it afterwards.

[17] Results from propensity score matching (coarsened, without replacement) show a similar, albeit slightly smaller, effect: -13.7 percentage points with a standard error of 4.9 percentage points ($p = 0.0052$).

5.4 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 Fairbanksians (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; 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 substitution effect that drives the *acquiescence* 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 substitution effect is the culprit. 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.[18] That said, given the amounts of money involved it is slightly unreasonable to expect an individual receiving what is the equivalent of a $500 check in today's dollars not to deposit this money in a timely manner.

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 direct evidence that the direct distribution of oil wealth reduces political participation. Registered Alaskan voters who received an oil-to-cash transfer of $100/month (in nominal 1976 dollars) before the 1976 general election were roughly 16 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, my results

[18] 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.

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.

Finally, this study provides policy implications regarding the timing of distribution for unconditional cash transfers such as the Longevity Bonus. 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 oil transfers will be implemented in developing democracies such as Brazil, Nigeria, and Indonesia, or in transitional systems such as Iraq, Myanmar, and South Sudan.

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Appendix 1: Supplementary Tables

	Dependent variable:
	Treated
Gender (1 = male)	0.065
	(0.045)
Age (years)	−0.009
	(0.008)
Residency (years)	0.002
	(0.001)
Latitude	−0.005
	(0.007)
Longitude	−0.001
	(0.003)
Constant	14.395
	(15.652)
Observations	448
R^2	0.017
Adjusted R^2	0.006
Residual Std. Error	0.468 (df = 442)
F Statistic	1.547 (df = 5; 442)
Note:	*p<0.1; **p<0.05; ***p<0.01

Table A1: Balance table: OLS regression on the likelihood of receiving transfers prior to the election (treatment). 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. Latitude and Longitude correspond to the mailing address of each applicant.

Batch number	(Intercept)	Gender	Age	Residency	Date received	Mailing zip code	Number of observations
45	1.00	1.00	1.00	1.00	1.00	0.99	32
66	1.00	1.00	1.00	1.00	1.00	0.99	32
100	1.00	1.00	1.00	1.00	1.00	0.99	35
112	1.00	1.00	1.00	1.00	1.00	0.95	22
146	1.00	1.00	0.98	1.00	0.99	0.60	16
156	0.99	1.00	0.96	1.00	0.99	0.29	13
167	1.00	1.00	1.00	1.00	1.00	0.99	35
200	1.00	1.00	1.00	1.00	1.00	1.00	24
210	1.00	1.00	1.00	1.00	1.00	0.92	21
220	1.00	1.00	1.00	1.00	1.00	0.99	31
253	1.00	1.00	1.00	1.00	1.00	0.95	22
263	1.00	1.00	1.00	1.00	1.00	0.99	33
273	1.00	1.00	1.00	1.00	1.00	1.00	25
306	1.00	1.00	1.00	1.00	1.00	0.99	35
316	1.00	1.00	1.00	1.00	1.00	0.99	31
326	1.00	1.00	1.00	1.00	1.00	0.97	51
361	1.00	1.00	1.00	1.00	1.00	1.00	28
371	1.00	1.00	1.00	1.00	1.00	0.88	20
381	1.00	1.00	1.00	1.00	1.00	0.98	38
428	1.00	1.00	1.00	1.00	1.00	0.99	37
437	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	41
483	1.00	0.99	0.98	1.00	1.00	0.60	16
493	1.00	1.00	1.00	1.00	1.00	0.97	50
531	1.00	1.00	1.00	1.00	1.00	0.92	21
540	1.00	1.00	0.99	1.00	1.00	0.60	16
550	1.00	1.00	1.00	1.00	1.00	1.00	28
589	0.99	1.00	0.93	1.00	0.99	**0.06**	10
598	0.99	0.99	0.96	1.00	1.00	0.29	13
837	1.00	1.00	1.00	1.00	1.00	1.00	29
859	0.99	1.00	0.91	1.00	1.00	**0.06**	10
869	1.00	1.00	1.00	1.00	1.00	0.99	35
879	1.00	1.00	1.00	1.00	1.00	1.00	28
901	0.98	0.97	0.89	1.00	1.00	**0.03**	9
911	0.99	0.99	0.93	1.00	1.00	0.12	11
921	1.00	1.00	1.00	1.00	1.00	0.97	50
943	0.99	0.99	0.94	1.00	1.00	0.20	12
953	0.99	0.99	0.95	1.00	1.00	0.20	12
963	1.00	1.00	1.00	1.00	1.00	0.99	32
985	1.00	1.00	0.99	1.00	1.00	0.69	17

Table A2: 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.