Direct cash transfers and political participation

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Abstract

The effects of increased personal income on voter turnout are well documented in political science research. There is little consensus, however, about the specific effects of increased unearned income—money that individuals receive regularly from non-employment sources, such as government-paid cash transfers. Economic voting theories suggest that any increase in disposable income prompts greater political participation. By contrast, rentier theories posit an acquiescence effect, such that unearned income transfers will suppress civic engagement. This paper proposes a new framework based on political autonomy from the state: individuals receiving unearned income use this windfall to withdraw from public services, thereby removing a key state-citizen linkage that motivates voter participation. This paper leverages a quasi-experimental assignment of cash transfers, individual-level administrative voter data, and a contemporaneous historical survey in 1976 Alaska to test the argument. Individuals receiving oil-to-cash transfers as-if randomly right before an election are roughly 20 to 27 percentage points less likely to vote than those receiving transfers right after the election. These findings bear theoretical implications for the broader study of civic engagement and the political effects of unearned income.

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1 Introduction

Recent policies to combat income inequality promote unconditional government cash transfers to individuals. These transfers—such as the universal basic income (UBI) and carbon dividends—are paid regularly and unconditionally, such that politicians are constrained from discretionary and distortionary spending of government revenues. 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 Akee et al., 2018).

This paper analyzes one such effect: the impact of cash transfers on individual participation in elections. I argue that these transfers have the effect of increasing individual autonomy from the state, such that people receiving this money have fewer incentives to engage civically. 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.

This paper brings a causal approach to the question of how repeated windfall income affects political behavior. 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 20 to 27 percentage points less likely to vote than those receiving their first checks after the election. Consistent with a theory of oil-financed quiescence where civil society organizations are lacking, treated individuals living in the civic periphery were far less likely to vote than individuals living in the civic centers of Anchorage and Juneau. Results from a survey conducted in 1976 on LBP recipients and non-recipients suggest that the Bonus provided a signal to individuals that they could use the personal windfall 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's findings question the conventional wisdom that increases in personal income positively affect civic engagement. Unearned income that is anticipated and delivered regularly over time—as opposed to one-time windfalls such as lotteries, or unpredicted transfers like tax refunds—is perceived differently than income earned from employment. 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. In this way, policies that distribute unearned income therefore have a negative unintended effect on political behavior, which can then adversely feedback into future policies (see, e.g., Pierson, 1993).

Alaska may seem at first to be an unorthodox testing ground given its socio-economic differences from the rest of the United States. Yet among the oil-producing countries of the world that publicly redistribute their petro-dollars using targeted and public transfers, Alaska is at once highly representative and broadly instructive. In fiscal and socio-economic terms, Alaska is undeniably a resource-reliant state. 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: multinationals 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.[1] Alaska also suffers from resource-curse maladies such as corruption and low levels of transparency in public reporting of state spending.[2] Despite GDP per capita routinely between $70,000 and $100,000—largely a result of its small population,[3] reaching 737,438 in 2018—between 10% and 12% of Alaskans live in poverty, with up to 32% under the poverty line in rural districts.[4] In governance terms, modern Alaska is an advanced, representative democracy with universal suffrage and multiple layers of political constraints and balances.[5] Yet the Alaskan government in 1976 lacked the state capacity it currently maintains given it had achieved statehood only 17 years prior. Outside of the core urban areas, the state was largely unable to reach its citizens in delivering public goods and services, even basic provisions such as electrification and reliable telecommunications networks. In this context, then, it is on par with most resource-rich producers in emerging markets.

[1] 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-petroleum corporate income taxes.

[2] 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 from `http://www.uspirg.org/sites/pirg/files/reports/FollowingtheMoney2015vUS.pdf`.

[3] 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.

[4] 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 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.

[5] In addition, 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. And despite not paying state taxes, Alaskans still file federal income taxes and are hence fiscally linked to the federal government.

2 How oil-to-cash transfers affect participation

Proponents of the resource "curse" 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.[6] 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 infras-

[6] See Ross (2015) for a review. Note that several scholars have questioned the unconditionality of this effect (Smith, 2007; Brooks and Kurtz, 2016; Menaldo, 2016).

tructure. 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.

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).[7] 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.[8]

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

[8]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

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., 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 propose that direct transfers of resource revenue lower incentives to participate 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, 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

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.

(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.[9] 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

[9] 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.

other mobilizing forces that would otherwise provide motivations to participate.

Citizens have one less 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 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 and leads instead to indifferent quiescence.

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. This implies that the negative effects of transfers on participation primarily exist for individuals living outside the civic core, where incentives for political participation are relatively low given fewer civil society institutions and mobilizing organizations.

3 Data & Methods: The 1976 Alaska Longevity Bonus

I test the argument using data from Alaska's Longevity Bonus Program (LBP) in 1976. The LBP was the first initiated in January 1973 (repealed in 1991 and officially closed in 2003) as a cash transfer based on age and length of residence.[10] Specifically, any person who was 65 or older and had maintained residency in the state for 25 or more years would qualify for

[10] Alaska Statutes §47.45.010.

a \$125 monthly 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.[11] This case represents an ideal testing ground for a repeated, predictable cash transfer that is not means-tested, is offered at a non-trivial amount, and is financed by non-tax revenues.

Here I operationalize participation specifically in terms 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. To assess core-periphery dynamics, I consider an individual living in Anchorage or Juneau as residing in the civic core, with all others as residing in the civic periphery.[12] I operationalize public service use as participation in state-provided social programs for nutritional, housing, and medical assistance.[13]

I collected data on LBP applicants from the Alaska State Archives. 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. Individual applications to the program are available on microfilm.[14] I then matched names and addresses from these applications to voter files from the Alaska Division of Elections.[15] This resulted in turnout data and LBP transfer data for 139 individuals receiving their first transfer check either in the month before the election (October 1976) or the month afterwards (November 1976). These 139 individuals represent the full universe of transfer applicants in the October–November 1976 period. Of

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

[12] Juneau is Alaska's state capital, and 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 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.

[13] 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.

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

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

these individuals, 59 reside in the core—Anchorage and Juneau—and 80 reside outside the core.

3.1 Process of treatment assignment

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.

Each individual mailed an application form to the LBP office at the Department of Administration (DOA) in Juneau, where it was stamped with the receipt date. These applications were then combined into "batches" with all other correspondence to the DOA office which, in addition to the LBP, also managed Alaska's Pioneer Homes and other senior benefits programs. This meant that on any given day, a specific batch could contain only LBP applications, a mix of LBP applications and other correspondence, or no LBP applications at all.[16] For example, on August 16, 1976, the DOA received five LBP applications, which were assigned to four different batches: two to batch 963, one to batch 1001, one to batch 1011, and one to batch 1045.

The batch number determined the order applications were reviewed and approved (Figure 1). Batch 921 was the first in the sample to be reviewed, in August 1976, and batch 1066 was the last to be reviewed in December 1976. However, not every individual in each batch was immediately eligible to receive the LBP: some individuals sent in their applications slightly before they either turned 65 or had lived in the state for 25 years. Across

[16]Within a broader 1976–1977 sample of applications, only 48 out of 1,000 batches contained LBP applications, with a minimum of 1 LBP application and a maximum of 51 LBP applications in any given LBP-containing batch.

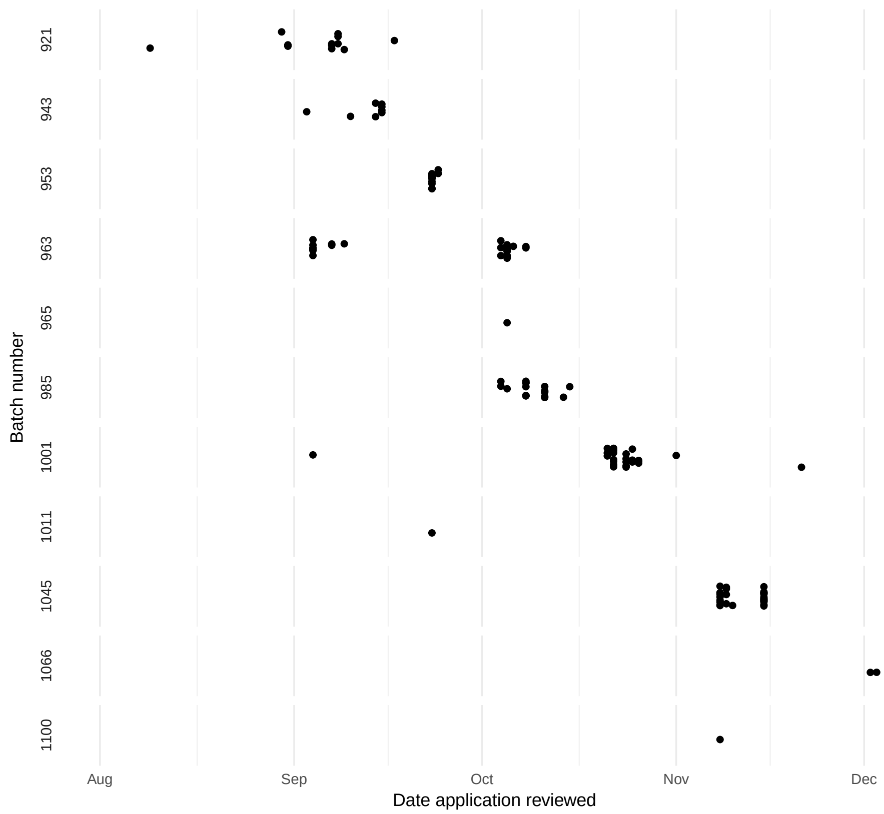

Figure 1: Application review date and batch number assigned at the Alaska Department of Administration (DOA). Each row of the plot represents the 11 different batches in the sample, with points within each row corresponding to the date each application in a given batch was reviewed at the DOA. Points are jittered within batches for illustrative purposes (but not jittered across the x-axis).

a broader time window, applications in batches 837–985 were reviewed and approved prior to the election, while batches 1000–1598 were reviewed and approved after the election.[17] Once applications were reviewed and approved, the DOA stamped each application with an "effective" date when an applicant would be sent their first LBP check by mail. Checks were processed on the first of each month, such that there are only 16 effective dates in the 17-month period (the first checks were sent out in June 1976). Treatment is defined as an

[17]The overlap in review and approval is minimal across all 1,092 applications reviewed in the 1976–1977 period: 32 applications in batches 1000–1598 were reviewed prior to the election and not approved until after the election, and 7 applications in batches 837–985 were reviewed prior to the election and not approved until after the election. Note that all applications in batches 837–985 were reviewed prior the election, and all other applications (except the aforementioned 32 applications) in batches 1000–1598 were reviewed after the election.

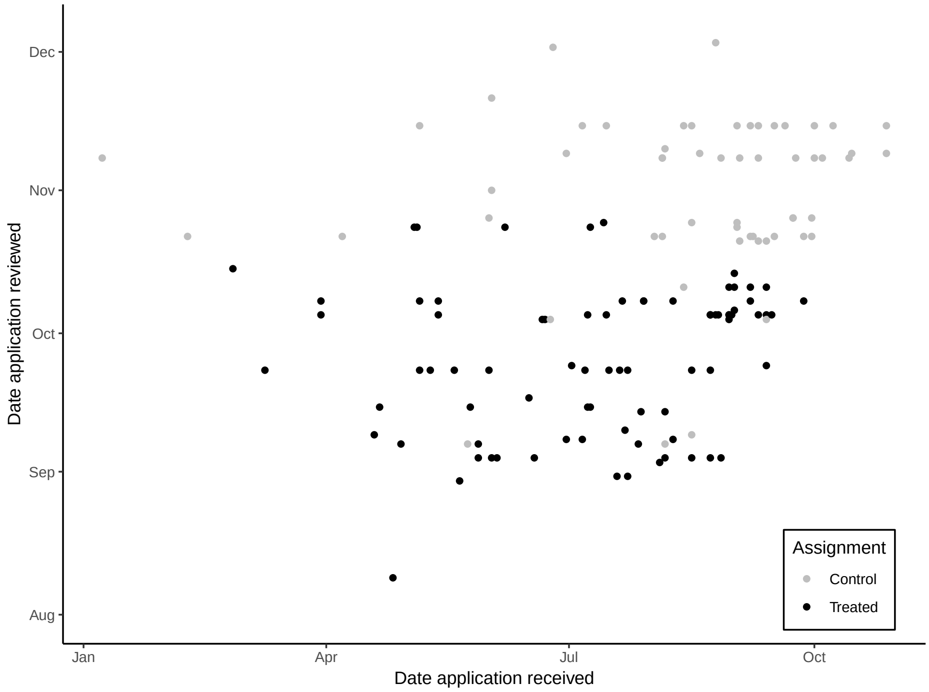

Figure 2: Application receipt date and date of application review at the Alaska Department of Administration (DOA). Black points represent individuals whose first transfers were sent out prior to the election. Gray points represent individuals whose first transfers were sent out following the election. Note that there are 6 instances in which an individual's application was reviewed prior to the election but whose transfers were not sent until after the election; these correspond to individuals who were not yet eligible at the time of application review, either due to age (not yet 65) or residency (not yet living in Alaska for 25 years).

effective date on or before October 1, 1976; control is defined as an effective date on or after November 1, 1976.

Figure 2 plots receipt date and review date for each applicant along with treatment assignment. While there is a modest positive relationship between the two, there is a considerable range in review dates for each given receipt date. For example, the review date for the 34 applications received in August 1976 ranged from September 3 to December 3, such that 19 individuals received their first check prior to the Nov 2 election (treated) while 15 individuals received their first checks after the election (control).

The plausibility of quasi-randomness in treatment assignment depends on the following assumption: 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 month just before and after the threshold (Dunning, 2012). Indeed, applicants who are approved several months prior to the election are likely different from those approved several months after the election. As such, comparisons of turnout for individuals in the direct neighborhood of the threshold should allow an estimate of the causal effect of receiving direct oil cash handouts on political participation.

In total, 81 treated individuals were sent their first checks on Friday October 1st, 1976, and received them no later than Thursday October 7th, while 58 control individuals were sent their first checks on Monday November 1st, 1976, receiving them anytime between November 3rd and November 5th.[18] Of the 81 treated, 34 lived in the core (Juneau and Anchorage) and 47 lived outside the core; of the 58 control, 25 lived in the core and 33 lived outside the core. All 139 applications were received prior to 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 on all applicants in the 1976-1977 period—not just those within the one-month election threshold—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

[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.

	Control mean	Treated mean	Difference	p-value	Non-missing observations
Prior voting (1972)	0.67	0.59	-0.08	0.34	139
Registered to vote (1 = yes)	0.95	0.89	-0.06	0.20	139
Gender (1 = male)	0.53	0.65	0.12	0.16	139
Birth year	1909.72	1909.47	-0.26	0.72	139
First year of residency	1935.21	1934.68	-0.53	0.85	139
Born in Alaska (1 = yes)	0.30	0.22	-0.07	0.36	130
Social Security (1 = yes)	0.62	0.66	0.04	0.62	135
Core resident (1 = yes)	0.43	0.42	-0.01	0.90	139
Alaska Native (1 = yes)	0.21	0.17	-0.04	0.56	131

Table 1: 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 well-balanced across these two variables, and also across prior voting history, gender, place of birth, social security status, geography (core versus periphery), and ethnicity. See Appendix Figure A1 for corresponding equivalence tests using these pre-treatment covariates.

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 A1).[19]

Applications also include information on individual characteristics as reported by the applicant. This includes gender, date of birth, address, place of birth, duration of residency in Alaska, voter registration, self-identification as Alaska Native, and whether the individual received Social Security at the time of application. Balance checks indicate no statistical

[19] 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. Note that this batch is outside the one-month election window sample.

differences in characteristics between treated and control units (see Table 1). In addition, more conservative tests of equivalence (Hartman and Hidalgo, 2018) similarly show that we can reject the null hypothesis of a difference in means between the treated and control groups (Appendix Figure A1).

While this is a surprising amount of individual-level information for historical observational data, we might still be concerned that there are additional, unobserved factors that could play a role in an individual's decision to vote for which the data are not statistically balanced.[20] To account for relatively static unobserved factors—such as education, income, ideology, parental partisanship, and to a lesser extent, personal partisanship—I collected additional data from the voter file on individual turnout in the 1972 election. As with the other characteristics, there is no statistical difference between the treated and control groups in prior voter turnout (Table 1 and Appendix Figure A1).

3.2 Illustrative vignette of LBP recipients

To get a clearer picture of the individual-level data used here, consider the following tale of two men from Wrangell, a small island village in the Alexander Archipelago of Southeastern Alaska (names altered to preserve anonymity). Al was born in July 1911, and sent in his LBP application on August 18, 1976. It was received by the state office in Juneau the next day on August 19, 1976, and was assigned to batch 1045. This batch was not reviewed until November 9, 1976, and so the first monthly check for Al's Longevity Bonus was sent out after the election.

Then we have Wes, born in Wrangell in September 1911. Wes sent in his application on September 10, 1976, and it was received in Juneau on September 13, 1976. His application was placed in batch 985, which was reviewed on October 11, 1976—a month before Al's

[20]For instance, it could be that partisanship determined turnout in the 1976 election: it was the first presidential election since Richard Nixon's resignation (and Ford's controversial pardon of Nixon), and Republican voters may have felt less mobilized to turn out in support of their candidate (Miller, 1978). If by chance the treatment and control groups vary by partisanship—which is not observed or measured in the sample—this could explain any differences in turnout between the groups.

application was reviewed despite being sent in one month afterwards. Wes's first Longevity Bonus check was sent out following review, arriving at his home in Wrangell three weeks before the election.

Neither was an Alaska Native, both received Social Security, and both were registered to vote at the time they sent in their applications. But one key difference is that Al received his transfer after the election, while Wes received his first transfer before the election. Consistent with the argument above, Al voted in the 1976 general election, but Wes did not. Of course, these are only two data points and the difference in political behavior could simply be due to chance. To increase confidence in the overall pattern of effects, I now turn to an analysis of the set of individuals receiving cash transfers before and after the election.

3.3 Modeling approaches

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 of cash transfers for individuals in the core and in the periphery.[21] To increase precision and account for any remaining imbalance across treatment and control groups, I also analyze the data by including individual-level covariates and county-level fixed effects. I also condition on month of application receipt to account for any potential differences in political behavior between individuals sending in applications "early" and "late" (relative to the election).

I next relax the one-month bandwidth assumption and estimate the local average treatment effect using a sharp regression discontinuity design (Lee and Lemieux, 2010). This allows for a data-based estimation of the optimal bandwidth for comparing individuals who received their transfers before the election to those receiving it afterwards. Since the running

[21] I cannot credibly estimate the ATE for the other side of this interaction—the effect on voter turnout of living in the core versus the periphery—since assignment is not plausibly random.

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.[22] 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 Statistical analysis of cash transfers and voting

4.1 Inference using direct regression

Simply regressing turnout on whether or not an individual received the transfer before the election shows a negative effect (Table 2). 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 26.5 percentage points compared to individuals getting their checks just after the election. The difference in treatment effects between core and periphery voters is 36.9 percentage points, such that treated individuals in the core are 10.4 percentage points more likely to vote than non-treated individuals in the core (though this positive effect is not statistically significantly different from zero). Adding individual-level controls and county-fixed effects slightly decreases magnitude of the effects: treated individuals in the periphery are 21.5 percentage points less likely to vote than non-treated individuals in the periphery.

Individuals outside the core receiving their transfers in October show a 55.3 percent

[22] There is no statistical difference in the density of the forcing variable (month application was reviewed) before and after the election. A McCrary test produces an insignificant p-value of 0.353, suggesting there is no clear manipulation in sorting around the election cutoff. See Appendix Figure A3.

	Dependent variable:				
	Voted in 1976 election				
	(1)	(2)	(3)	(4)	(5)
Treated	−0.265***	−0.206***	−0.221***	−0.245***	−0.200**
	(0.076)	(0.076)	(0.075)	(0.089)	(0.097)
Core resident	−0.098	−0.049	−0.100	−0.207***	−0.073
	(0.108)	(0.090)	(0.089)	(0.055)	(0.119)
Treated × Core	0.369***	0.303**	0.339***	0.384***	0.324**
	(0.125)	(0.119)	(0.119)	(0.130)	(0.136)
Controls?	N	Y	Y	Y	N
Fixed effects?	N	N	N	Y	Y
Observations	139	139	135	135	139
R^2	0.070	0.236	0.249	0.393	0.103
Note:					*p<0.1; **p<0.05; ***p<0.01

Table 2: Regression results from OLS of turnout for 139 Longevity Bonus recipients across October-November 1976. 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 (1 =voted; 0 =did not vote). Columns 1 show results from a model without any controls. Column 2 adds a control for whether the individual voted in the 1972 election. Column 3 adds individual-level controls: prior voting (1972), voter registration, age, duration of residency in Alaska, gender, and whether the individual is a recipient of Social Security. Column 4 includes all controls in column 3, plus county-fixed effects. Column 5 replicates the baseline model in column 1, conditioning on the date application was received using receipt–date fixed effects. All standard errors are clustered by county (borough).

turnout rate compared to those receiving their transfers in November (after the election), 81.8 percent of whom turned out to vote (Figure 3). Keep in mind these figures are for individuals who are above 65 years of age—typically the likeliest group to vote in US elections. In Anchorage and Juneau, turnout rates are on par with the non-core control group: 82.4 percent of those receiving their transfers in October voted, compared to turnout of 72.0 percent for those receiving their transfers in November.

Next, I use receipt–date fixed effects to address concerns regarding variation in the timing of application submission. This allows for an estimate of the cash transfer effect conditional on when individuals sent in their applications. It could be the case, for example, that "early senders" who submitted an application in the Spring several months before the election are

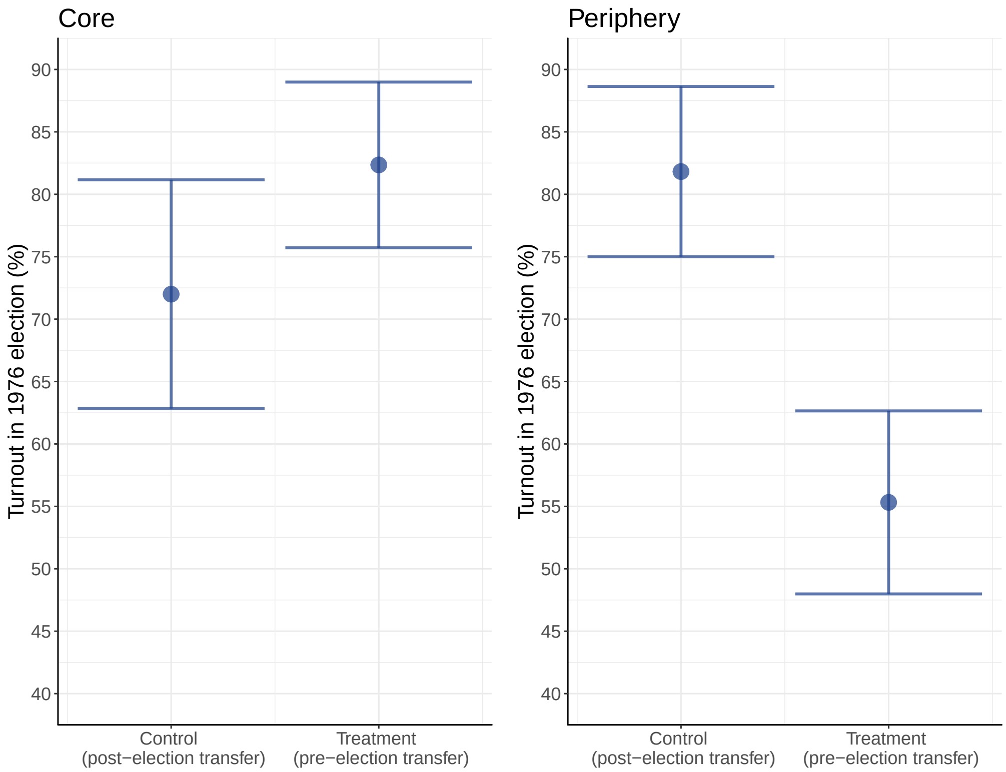

Figure 3: Difference in turnout between individuals receiving oil-to-cash transfers before the 1976 election (treatment) and those receiving transfers after the election (control), for individuals living in the civic core (left panel) versus those outside the civic core (right panel). Means by group are plotted along with 95% error bars.

systematically different—in terms of turnout—from “late senders” whose applications were not sent in until September. Indeed, 81 percent of applications received prior to June 1976 were reviewed before the election, while only 33 percent of those received in September were reviewed in time for individuals to receive their transfers before Nov 2 (see Figure 2). Yet the data do not support this alternative explanation: the estimated treatment effect is robust to conditioning on month of receipt (Table 2, column 5).[23]

[23]This finding is also robust to using receipt–date random effects instead of fixed effects (Appendix Table A4). In addition, a simple ANOVA test allows us to reject the null hypothesis that the mean turnout rate across treatment and control groups are equal when controlling for receipt date ‘blocks’ (Appendix Table A5).

	Optimal bandwidth	Half-bandwidth	Double-bandwidth
LATE estimate	0.28***	0.40***	0.21**
	(0.08)	(0.12)	(0.10)
Bandwidth size (months)	3.33	1.66	6.65
Observations	245	169	381
F	2.698*	3.933**	2.331
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 3: Difference in turnout rates for individuals living outside the civic 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 (0–1). Estimates obtained using the `rdd` package in `R`, with a triangular kernel to determine the optimal bandwidth.

4.2 Inference using an interrupted time-series design

Relaxing the narrowness of the bandwidth from one month to data-determined monthly bandwidth produces similar results. The optimal bandwidth estimated using the Imbens-Kalyanaram algorithm is 3.3 months, which expands the periphery sample from 81 applicants to 245 applicants.[24] For applicants outside the core, I find a 28.4 percentage point effect at the optimal-month bandwidth and a surprisingly large 39.8 percentage point effect at the half-optimal bandwidth of 1.7 months (Table 3).

An alternative approach is to estimate the difference-in-means at each bandwidth, increasing from a one-month window to a five-month window.[25] This shows a more conservative estimate for higher bandwidths, such that as the number of bandwidths increases, the difference in means decreases (Figure A2). This is in line with the notion that the comparability between groups weakens as the bandwidth increases. For example, applicants who are approved five months prior to the election all applied far earlier than those approved five months after the election; there might be systematic differences in general behavior among "extremely early" appliers and "extremely late" appliers on factors that drive engagement.

[24]The optimal bandwidth for the core sample is estimated separately at 2.4 months, increasing the sample from 58 core applicants to 96 core applicants.

[25]Note that this is the maximum symmetric window given the first transfers in the sample were sent in June 1976, five months before the November 2nd election.

4.3 Placebo test: 1980 election

Since all applicants in the sample received the transfer at some point during the period of study, there should be no evidence of a treatment effect for future elections. Looking at the November 1980 general election, we should not expect differences in turnout rates for individuals who received their first transfer four years and one month prior (October 1976) compared to those who received their first transfer four years prior (November 1976). Using 1980 turnout as the dependent variable instead of 1976 turnout, I find no evidence of a treatment effect (Table A8).

4.4 Discussion of results

It is important to keep in mind that these results are still based on quasi-experimental 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.[26] 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 Exploring the autonomy mechanism

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 explore this question using a survey conducted on a comparable group of Longevity Bonus recipients. I

[26] 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.

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 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.[27] 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

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

in everyday activities, such as transportation, shopping for food and clothing, paying for medical expenses, and participating in social and outdoor engagements. The top panel in Figure 4 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.[28] On the basis of these responses, the Bonus appears to provide individuals 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. The bottom panel in Figure 4 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.[29] OAA-only respondents were

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

[29] 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 are not as relevant for these comparisons, as the majority of these respondents indicated that they either do

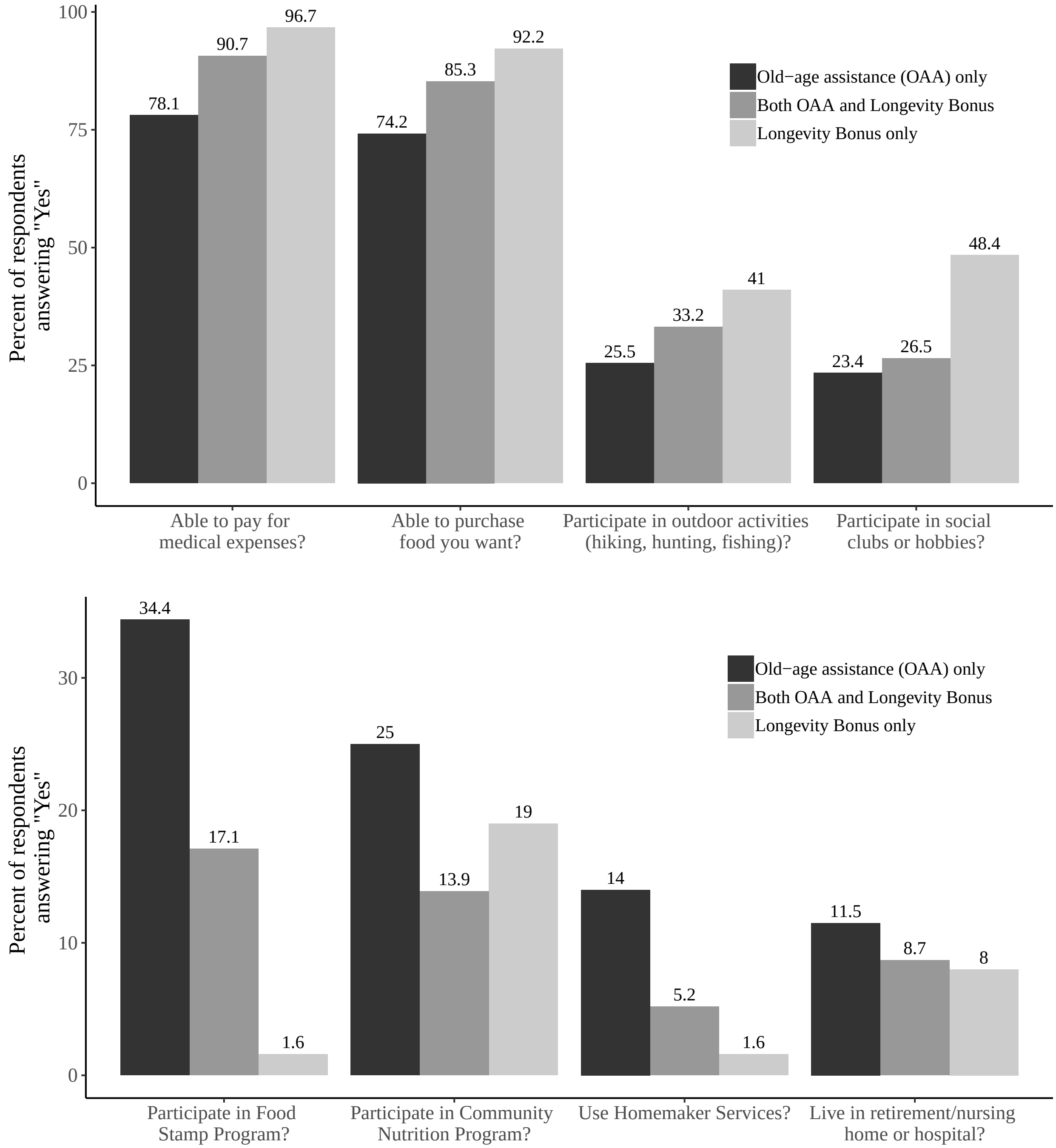

Figure 4: Indicators of financial independence (top panel) and social service usage (bottom panel) among Alaskan senior citizens in 1976, based on self-reported survey data collected by Pagenkopf and Quinn (1976).

twice as likely 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.

6 Conclusion

Answering whether windfall income affects political participation remains a critical puzzle in the study of political behavior in general and of natural resource politics in particular. In the context of Alaskan state politics, I show that the direct distribution of resource wealth in the form of individual cash transfers reduces turnout. Alaskans living outside the civic core who received a dividend of $125/month (in nominal 1976 dollars) before the 1976 general election were roughly 20 to 27 percentage points less likely to vote than individuals with similar characteristics who received their first transfers after the election. Exploratory evidence from a contemporaneous survey suggests that these negative effects could be driven by an increased sense of personal autonomy from the state. Individuals receiving cash transfers were less likely to consume state services, thereby removing a typically strong incentive to engage in the political process.

These findings offer evidence supporting microfoundations of the political resource curse even in the context of an advanced, long-established democracy. Specifically, these results

not need (35.5%) or are ineligible for (16.3%) food stamps.

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.[30]

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 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.

[30] 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).

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Appendix

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 A1: 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.

	Dependent variable:				
	Voted in 1976 election				
	(1)	(2)	(3)	(4)	(5)
Treated	−0.265***	−0.206***	−0.221***	−0.245***	−0.200**
	(0.076)	(0.076)	(0.075)	(0.089)	(0.097)
Core resident	−0.098	−0.049	−0.100	−0.207***	−0.073
	(0.108)	(0.090)	(0.089)	(0.055)	(0.119)
Prior voting (1972)		0.385***	0.339***	0.213***	
		(0.075)	(0.087)	(0.068)	
Registered voter			−0.001	0.003	
			(0.008)	(0.009)	
Birth year			0.005**	0.005	
			(0.002)	(0.003)	
First year of residency			0.070	0.133	
			(0.081)	(0.083)	
Gender (1 = male)			−0.023	0.013	
			(0.061)	(0.080)	
Social Security recipient (1 = yes)	0.369***	0.303**	0.339***	0.384***	0.324**
	(0.125)	(0.119)	(0.119)	(0.130)	(0.136)
Treated × Core	0.818***	0.538***	−7.012	−13.708	1.000***
	(0.066)	(0.087)	(14.004)	(15.556)	(0.00000)
Observations	139	139	135	135	139
R^2	0.070	0.236	0.249	0.393	0.103
Note:	*p<0.1; **p<0.05; ***p<0.01				

Table A2: Full model results with coefficient estimates for all covariates (excluding fixed effects) for results reported in Table 2. Model 4 includes county fixed effects. Model 5 includes receipt–date fixed effects. All standard errors are clustered at the country level.

	Dependent variable:				
	Voted in 1976 election				
	(1)	(2)	(3)	(4)	(5)
Treated	−0.262***	−0.199**	−0.217***	−0.233**	−0.215**
	(0.080)	(0.082)	(0.077)	(0.093)	(0.099)
Core resident	−0.119	−0.053	−0.117	−0.259***	−0.103
	(0.112)	(0.098)	(0.087)	(0.071)	(0.128)
Prior voting (1972)		0.377***	0.323***	0.199***	
		(0.077)	(0.084)	(0.066)	
Registered voter			−0.001	0.002	
			(0.008)	(0.009)	
Birth year			0.006**	0.005	
			(0.002)	(0.003)	
First year of residency			0.070	0.132	
			(0.089)	(0.095)	
Gender (1 = male)			−0.023	0.005	
			(0.063)	(0.085)	
Social Security recipient (1 = yes)	0.390***	0.307**	0.346***	0.384***	0.354**
	(0.127)	(0.127)	(0.117)	(0.127)	(0.145)
Treated × Core	0.815***	0.536***	−8.589	−13.478	1.000***
	(0.070)	(0.092)	(14.096)	(16.497)	(0.00000)
Observations	131	131	127	127	131
R^2	0.069	0.226	0.252	0.378	0.101
Note:					*p<0.1; **p<0.05; ***p<0.01

Table A3: Model results for specifications in Table 2, excluding 8 individuals with applications received on or after October 1, 1976. Model 4 includes county fixed effects. Model 5 includes receipt–date fixed effects. All standard errors are clustered at the country level.

	Dependent variable:
	Voted in 1976 election
Treated	-0.206^{**}
	(0.092)
Core resident	-0.049
	(0.107)
Prior voting (1972)	0.385^{***}
	(0.071)
Treated × Core	0.303^{**}
	(0.141)
Constant	0.538^{***}
	(0.087)
Observations	139
Groups (receipt–date)	10
Log Likelihood	-77.137
Akaike Inf. Crit.	168.275
Bayesian Inf. Crit.	188.816
Note:	*p<0.1; **p<0.05; ***p<0.01

Table A4: Random effects model of voting and cash transfers including receipt–date random intercepts. Model fit using restricted maximum likelihood with the `lme4` package in `R`.

	Df	Sum Sq	Mean Sq	F value	Pr(>F)
Treated	1	1.36	1.36	6.01	0.0167
Factor(receipt–date)	9	0.90	0.10	0.44	0.9077
Residuals	69	15.63	0.23		

	Df	Sum Sq	Mean Sq	F value	Pr(>F)
Treated	1	0.15	0.15	0.81	0.3723
Factor(receipt–date)	8	0.65	0.08	0.43	0.8997
Residuals	49	9.33	0.19		

Table A5: ANOVA results for individuals in the periphery (top table) and core (bottom table). Results in the periphery indicate we can reject the null hypothesis that the mean turnout rate across treatment and control groups are equal, controlling for receipt date blocks. Results in the core indicate we cannot reject the same null hypothesis, suggesting there is no difference in turnout between transfer recipients and non-recipients in the core (consistent with Table 2).

	Dependent variable:			
	Voted in 1976 election			
	(1)	(2)	(3)	(4)
Treated	-0.148^{***}	-0.124^{***}	-0.111^{***}	-0.102^{***}
	(0.044)	(0.036)	(0.036)	(0.038)
Core resident	0.086^{***}	0.077^{***}	0.054	0.056^{*}
	(0.033)	(0.025)	(0.033)	(0.029)
Treated × Core	0.070	0.104^{***}	0.101^{**}	0.091^{*}
	(0.051)	(0.038)	(0.046)	(0.050)
Observations	1,092	1,092	984	984
R^2	0.055	0.227	0.246	0.271
Note:				*p<0.1; **p<0.05; ***p<0.01

Table A6: Regression results from OLS of turnout for 1,092 Longevity Bonus recipients across all months in the sample. 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 prior voting in 1972 as a control. Column 3 adds all individual controls. Column 4 adds county fixed effects. Standard errors are clustered by borough (which is the Alaskan equivalent of county). Compare to results in Table 2.

	Dependent variable:		
	Voted in 1976 election		
	(1)	(2)	(3)
Treated	-1.291^{**}	-1.212^{**}	-1.219^{*}
	(0.538)	(0.594)	(0.626)
Core resident	-0.560	-0.352	-0.631
	(0.634)	(0.697)	(0.780)
Prior voting (1972)		2.048^{***}	1.740^{***}
		(0.440)	(0.485)
Registered voter			1.518
			(0.927)
Birth year			-0.037
			(0.058)
First year of residency			0.022
			(0.015)
Gender (1 = male)			0.524
			(0.504)
Social Security recipient (1 = yes)			-0.186
			(0.505)
Treated × Core	1.886^{**}	1.881^{**}	2.049^{**}
	(0.831)	(0.914)	(1.002)
Constant	1.504^{***}	0.282	26.467
	(0.451)	(0.533)	(111.761)
Observations	139	139	135
Note:			*p<0.1; **p<0.05; ***p<0.01

Table A7: Model results from Logistic regression

	Dependent variable:			
	Voted in 1980 election			
	(1)	(2)	(3)	(4)
Treated	−0.004	0.012	0.025	0.018
	(0.073)	(0.065)	(0.075)	(0.074)
Core resident	-0.168^{**}	-0.124^{*}	-0.139^{**}	-0.299^{***}
	(0.070)	(0.069)	(0.065)	(0.034)
Treated × Core	0.060	0.060	0.049	0.058
	(0.074)	(0.066)	(0.079)	(0.073)
Observations	139	139	135	135
R^2	0.151	0.385	0.413	0.505
Note:				*p<0.1; **p<0.05; ***p<0.01

Table A8: Placebo test. Model results using 1980 turnout as the outcome variable. For a list of controls used across specifications, see Table 2.

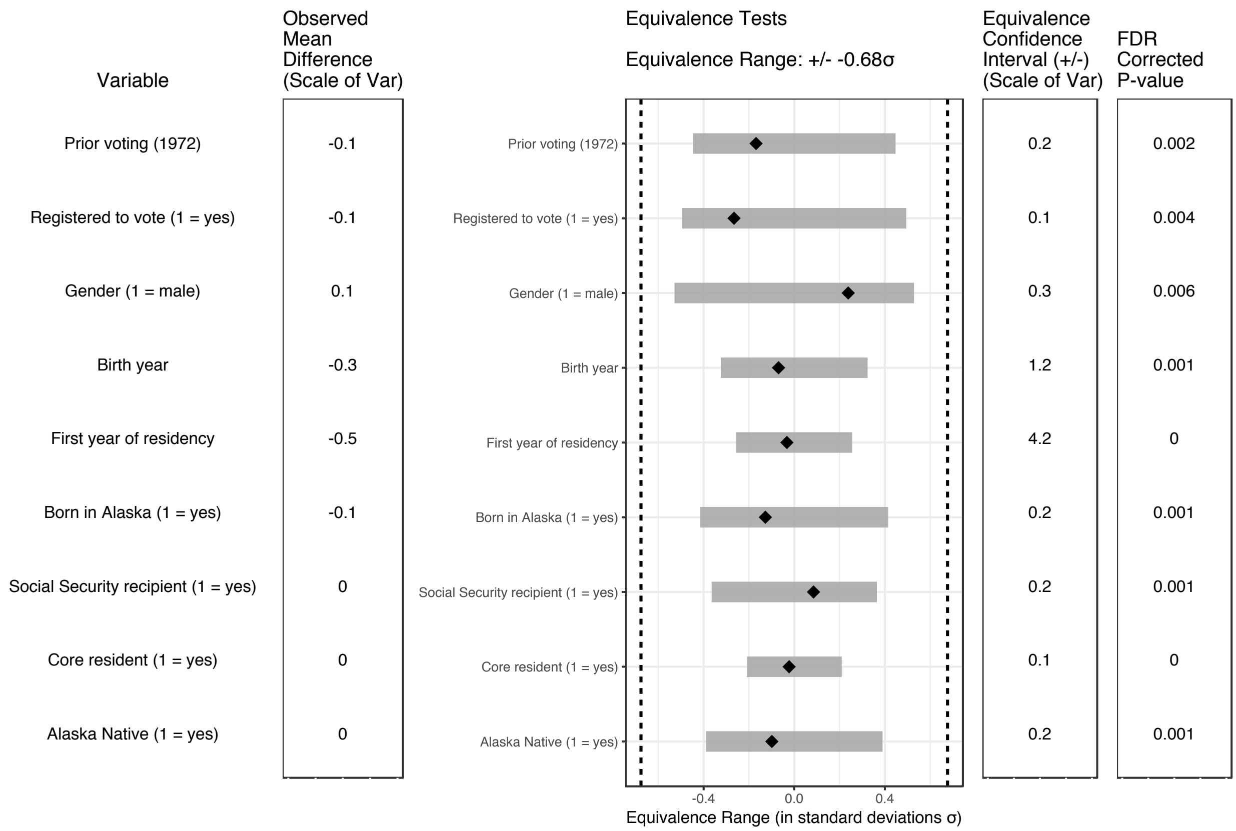

Figure A1: Equivalence tests for observed pre-treatment covariates, following Hartman and Hidalgo (2018). The equivalence range is estimated using Glass's Δ, which is the average difference in outcomes between the treated and control group, divided by the standard deviation of the control group. This yields an absolute value of 0.68. The hypothesized equivalence range is represented by vertical dashed lines; the inverted equivalence range supported by the data are shown using gray bars; and the black diamonds represent the observed standardized mean difference for each covariate. The last column gives the false-discovery-rate corrected p-value of the test of the null equivalence range. Lower p-values here indicate that we can reject the null hypothesis of a difference.

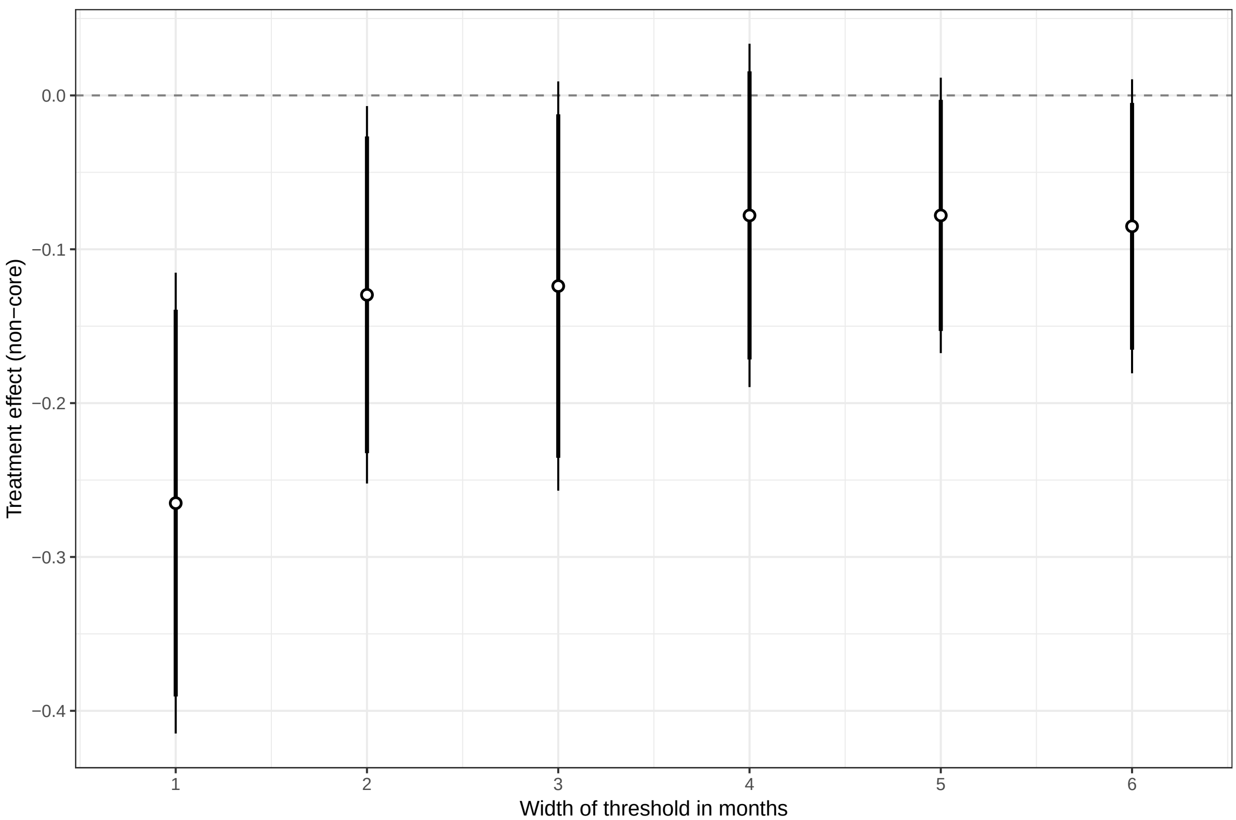

Figure A2: Treatment effects for periphery applicants, varying treatment bandwidth by months.

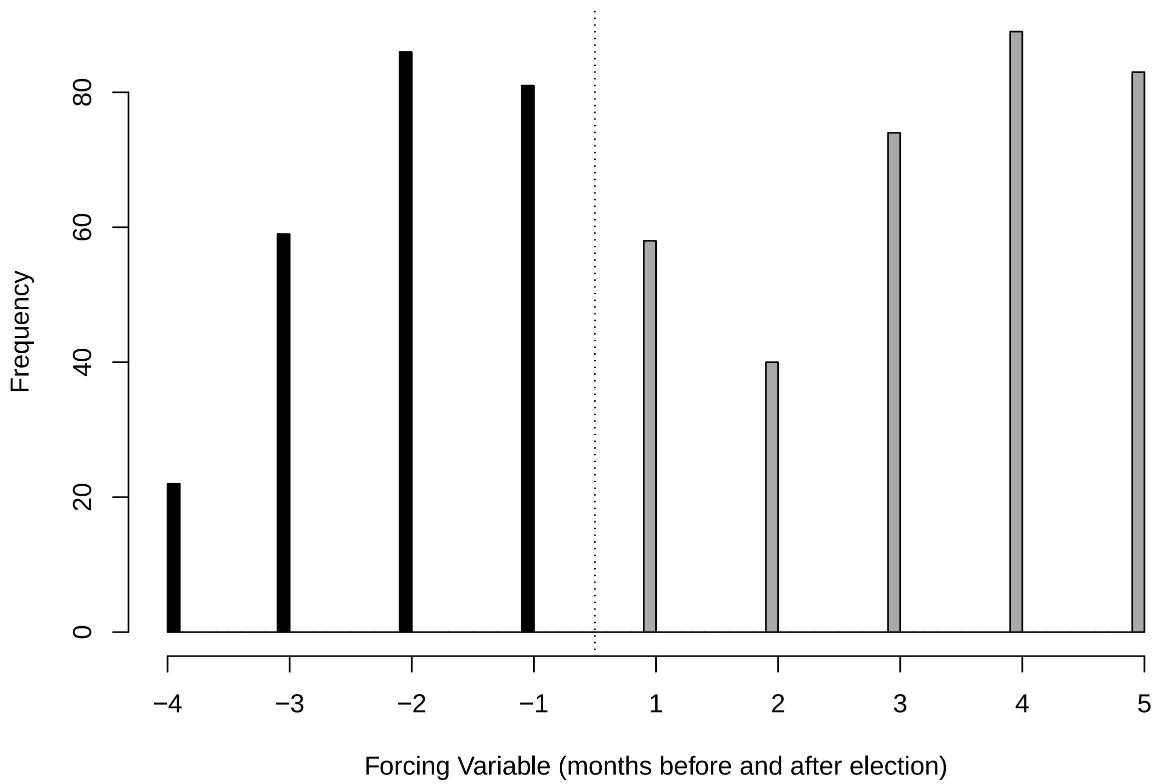

Figure A3: Density of forcing variable (month application reviewed) before and after the November 1976 election. A McCrary test of the difference in density of the forcing variable around the cutoff produces a p-value of 0.353, suggestive that there is likely no manipulation or sorting before and after the election.