How Incumbent Politicians Respond to the Enactment of a Programmatic Policy: Evidence from Snow Subsidies[*]

Amy Catalinac[†]
Taishi Muraoka[‡]

December 16, 2021

Abstract

More than fifty studies have examined the programmatic incumbent support hypothesis, which posits that once enacted, programmatic policies increase electoral support for the incumbent. Despite the careful attention to causal inference in this work, empirical findings have been strikingly inconsistent. We make the case that these inconsistent results are likely explained by incumbents' strategic responses to the enactment of a programmatic policy. Specifically, incumbents have good reasons to distribute different amounts of non-programmatic goods to voters who do and do not receive a programmatic policy. To examine this conjecture, we turn to the case of Japan, where municipalities receive allocations of non-programmatic goods and vary in their eligibility for a programmatic policy (a snow subsidy) according to factors plausibly exogenous to voting behavior. Using a geographic regression discontinuity design, we find that municipalities receiving the programmatic policy receive systematically *more* non-programmatic goods than municipalities that do not.

Comparative politics scholars have long been interested in the question of whether targeted government programs increase electoral support for the incumbent. Since the 1990s, governments in more than fifty countries have enacted social assistance programs designed to lift low-income households out of poverty and kickstart economic development (Araújo, 2021; Kogan, 2021). The defining feature of these programs is that they are "programmatic", meaning subject to rules that are formalized, transparent (in the sense of being made public), and not manipulable by incumbents (Stokes et al., 2013). For scholars, the enactment of these policies offers an opportunity to examine what is now known as the "programmatic incumbent support hypothesis" (PISH) (Imai, King and Rivera, 2019; Layton and Smith, 2015; De La O, 2013; Zucco, 2013; Diaz-Cayeros, Estevez and Magaloni, 2009). The PISH posits that the enactment of a programmatic policy increases electoral support for the incumbent among policy beneficiaries. Why? Beneficiaries may be engaging in "ordinary pocketbook voting" (rewarding incumbents for improving their livelihoods), inferring pro-redistribution preferences or competence on the part of their incumbent, or wanting to help an incumbent who helped them. What is notable about this work, however, is its lack of consensus: in some cases, researchers found support for the PISH (Manacorda, Miguel and Vigorito, 2011; Pop-Eleches and Pop-Eleches, 2012), but in others, they found no support (and occasionally, evidence of the opposite) (Blattman, Emeriau and Fiala, 2018; De Kadt and Lieberman, 2020).

We make the case that studies of the PISH fail to consider a mediating variable that we think influences how programmatic policies impact electoral support for the incumbent. The typical study proceeds by choosing a programmatic policy and leveraging features of its implementation to examine whether policy beneficiaries return more votes for the incumbent than non-beneficiaries. But incumbent politicians are not passive bystanders in elections. They actively try to persuade voters to vote for them, and one tool they harness to this end is the strategic distribution of non-programmatic (or discretionary) goods. In developed countries, non-programmatic goods typically consist of pork-barrel spending, while in developing settings, pork is combined with clientelistic goods targeted at the individual. We offer reasons why incumbents facing re-election in a district in which some voters are eligible for a programmatic policy and others are not have reason to change the way they allocate the non-programmatic goods at their disposal. Depending on how they think the policy will influence voting behavior, they may distribute more (or less) non-programmatic goods to policy beneficiaries than to

2

non-beneficiaries. If so, then it is no longer appropriate to attribute any observed difference in voting behavior across the two groups to the effects of the policy alone; rather, these differences likely reflect the compound effect of the policy *and* the extra non-programmatic goods.

To examine this conjecture, we turn to the case of Japan. We select a single programmatic policy (a snow subsidy) and leverage features of its implementation that are plausibly exogenous to voting behavior. We examine the policy's impact on two outcomes: electoral support for the incumbent and the distribution of non-programmatic goods. Like other studies, we find that the voting behavior of beneficiaries and non-beneficiaries is statistically indistinguishable, suggesting that the policy does not increase support for the incumbent. However, we also find that the policy results in more non-programmatic goods being delivered to beneficiaries relative to otherwise-similar non-beneficiaries. We consider several mechanisms that could be driving this result and come down on the side of incumbents anticipating that the programmatic policy would *lower* their vote share among policy beneficiaries and acting to offset this anticipated *negative* impact by plying beneficiaries with more non-programmatic goods. For comparative politics scholars, our findings imply that prior studies may have inaccurately estimated the effects of their policies of interest on election outcomes by only looking at its direct effects. We specify the conditions under which empirical studies become more vulnerable to this omission and urge researchers working in those settings to make the collection of data to evaluate it front and center in their research designs.

1   The Electoral Impact of Programmatic Policies

One of the main tasks of government is crafting and implementing policies that redistribute benefits to different segments of the population. Stokes et al. (2013, 7) provides a typology that distinguishes government policies on the basis of their mode of distribution. One is a programmatic policy. This is a policy whose distribution is subject to formalized, transparent, and non-manipulable rules, which define who is eligible to receive the policy and why. In most industrialized democracies, the programmatic policy is the mainstay of parties' election campaigns. Parties fight elections by crafting programmatic policies aimed at broad swaths of voters, such as the unemployed, white-collar workers, parents, or the elderly, and promise that if elected, they will enact those policies. Critically, what underpins an electoral strategy based

on programmatic policies is the hope that these policies prove attractive enough to entice their would-be beneficiaries to turn out and vote for the party in the next election (Kitschelt and Wilkinson, 2007).

Current scholarly interest in the impact of programmatic policies on voting behavior stems from their adoption in developing countries. One of the earliest examples of such policies is Mexico's large-scale social assistance program, Progresa, which was introduced by President Ernesto Zedillo in 1997. The program established a cash transfer that low-income households could receive on the condition they complied with requirements such as sending their children to school and health clinics for regular checkups (Diaz-Cayeros, Estevez and Magaloni, 2009, 231). The use of objective criteria to determine eligibility represented a significant departure from previous social assistance programs provided in Mexico. Early studies documented huge effects of Progresa on child health and nutrition, caloric intake, and living standards (De La, Lorena et al., 2018).

Galvanized by these results, governments of other middle- and low-income countries adopted similar programs, often with financial assistance from organizations such as the World Bank, Inter-American Development Bank, and Asian Development Bank. Like Progresa, the majority of these programs were conditional cash transfers (CCTs): payments that low-income households could receive on the condition they met objective, non-manipulable, and publicly-available criteria (Araújo, 2021; Linos, 2013; Tobias, Sumarto and Moody, 2014; Layton and Smith, 2015; Labonne, 2013; Zucco, 2013; Manacorda, Miguel and Vigorito, 2011; Conover et al., 2018). By 2014, more than fifty countries had CCTs (World Bank, 2014). Other (non-CCT) programmatic policies adopted in this period include grants to start small businesses in Uganda (Blattman, Emeriau and Fiala, 2018), vouchers for the purchase of a computer in Romania (Pop-Eleches and Pop-Eleches, 2012), and a universal health care system in Mexico (Imai, King and Rivera, 2019).

The use of objective and non-manipulable criteria to determine eligibility for these policies, as well as features of their implementation, provided political scientists with unusually-good opportunities to study their effect on a host of election outcomes. Researchers studied whether policy beneficiaries rewarded the parties that had enacted the policy or the parties in government at the time of the election, whether any such electoral rewards trickled down to elections at lower levels of government, how fast any such rewards decayed over time, and how the policy's

4

enactment influenced the voting behavior of non-beneficiaries, among other questions (Zucco, 2013; Conover et al., 2018; Bechtel and Hainmueller, 2011; Correa and Cheibub, 2016; Labonne, 2013; Linos, 2013; Tobias, Sumarto and Moody, 2014; Kogan, 2021).

1.1 The Puzzle: Contradictory Findings

To date, there have been more than fifty studies of what has been dubbed the "programmatic incumbent support hypothesis" (PISH), which predicts that programmatic policies that improve voter livelihood lead to more positive evaluations of the government among beneficiaries, which translate into more votes in elections (Araújo, 2021; Kogan, 2021).[1] Almost all studies analyze a single programmatic policy and leverage features of its implementation to estimate its effects on voting behavior, especially electoral support for incumbent parties. Because every programmatic policy is different, and every country is different, expectations of similar conclusions are perhaps misguided. That said, the lack of consensus about the effects of these policies is striking.

On one side are studies that find support for the PISH. For example, Manacorda, Miguel and Vigorito (2011) studied the effects of a large-scale anti-poverty program in Uruguay, in which benefits were allocated to households whose scores fell below a given threshold. Surveying households in the vicinity of the threshold, the researchers found a discontinuous increase at the cutoff in reported feelings of satisfaction with the government. Similarly, Pop-Eleches and Pop-Eleches (2012) studied a program in Romania that awarded a computer voucher to families with school-age children whose household income lay below a certain cutoff. Leveraging the fact that the list of winning and losing households was made public, along with information about their incomes, the authors selected a random sample of near-winners and near-losers and surveyed their willingness to turn out and vote for the incumbent.[2] The authors found a discontinuous increase at the cutoff in these outcomes. Other studies providing empirical support for the PISH focused on Brazil's CCT, Bolsa Familia (Zucco, 2013), the Food Stamp Program in the United States (Kogan, 2021), and survey data on "monthly assistance" in Latin America (Layton and Smith, 2015).

On the other side are studies that find no support for the PISH. Imai, King and Rivera (2019)

---

[1]Pop-Eleches and Pop-Eleches (2012) describe several mechanisms through which this could occur, including the possibility that the policy increased trust in government, created the perception that governing parties held pro-poor policy preferences, or improved the standing of governing parties on valence dimensions such as honesty or competence.

[2]Incidentally, the incumbent had not been the party that enacted the policy.

studied Seguro Popular de Salud (SPS), a program established by the Mexican government that extended health care to approximately 50 million Mexican citizens. Researchers worked with the government to divide the country into thousands of geographically-defined "health clusters," and randomly assigned some clusters a program to construct new hospitals and health clinics. Analyzing the 2006 Mexican presidential election, the researchers found that assignment to SPS did not result in higher vote shares for the incumbent.[3]

Other studies whose conclusions did not support the PISH found effects in the *opposite* direction. Blattman, Emeriau and Fiala (2018) studied a World Bank-funded program in Uganda, the Youth Opportunities Program, which used a lottery to award cash grants for independent trades to groups of poor and unemployed young adults. Four years after the program's implementation, the researchers surveyed winners and losers and found that winners were more likely to support the opposition. De Kadt and Lieberman (2020) also found that greater improvements in basic services such as water, sewerage, and refuse collection had a negative effect on the vote share of the incumbent African National Congress (ANC) in South Africa. The authors reasoned that once they received these improvements, beneficiaries no longer needed the ANC to deliver, so did not feel as compelled to vote for them. Similarly, Saito (2010, chapter 6) studied communities that received large-scale infrastructure such as airports or bullet train stations in Japan. He too found that these difficult-to-rescind investments portended *declines* in vote share for ruling party incumbents, which he attributed to the same mechanism as De Kadt and Lieberman (2020).

1.2 The (Possible) Interference of Non-Programmatic Goods

In this section, we point to a variable whose effects have not been adequately theorized about, but which we have reason to believe influences the relationship between programmatic policies and votes for the incumbent. This is the *non-programmatic goods* incumbents have access to by virtue of being in control of government resources. We explain what these goods are and why we think their distribution may be confounding attempts to estimate the impact of programmatic policies. We make the case that careful theorizing as to the effects of this variable, and the collection of data to measure it, could go a long way toward making sense of the inconsistent

---

[3]The authors also re-analyzed the impact of Progresa on both turnout and votes for the incumbent and found similar null effects.

results discussed above.

Non-programmatic goods are government policies whose distribution is not subject to formalized, transparent, and non-manipulable rules that determine eligibility. Stokes et al. (2013) distinguishes two categories of these.[4] One are grants, subsidies, and other transfers that governments channel toward groups of voters, geographically-defined or otherwise. Because incumbents retain discretion over the allocation of these goods, they are often distributed under an explicitly partisan logic, in some cases to a party's core supporters, and in other cases, to its swing voters. These goods, which can be delivered before or after elections, are called "partisan bias," and are perhaps more commonly known as "pork-barrel politics" (Golden and Min, 2013). The second category of non-programmatic good is the clientelistic good. Clientelistic goods are distributed to individuals on the condition that the individual vote for the incumbent and withdrawn as soon as it becomes obvious the person did not or will not (Stokes, 2007; Kitschelt and Wilkinson, 2007; Hicken, 2011; Weitz-Shapiro, 2014). Unlike pork-barrel projects, the distribution of a clientelistic good is *contingent* upon how someone votes.[5] Its purpose is to "buy" a vote.

How might non-programmatic goods interfere with our tests of the PISH? The large literature on pork-barreling, together with the equally-large literature on clientelism, provides ample evidence that in many democracies, both developed and developing, incumbents make use of non-programmatic goods to influence the number of votes they receive in the next election. Consider an incumbent member of the ruling party who is facing an upcoming election. Let us imagine that since the last election, a group of her voters has become eligible for a programmatic policy enacted by her party. Those voters are now receiving benefits under the policy. Because the policy is programmatic, beneficiaries know that they will continue to receive those benefits irrespective of who they vote for.[6] In this situation, the incumbent is likely to engage in a set of calculations as to how the programmatic policy is likely to influence voting behavior. On the basis of those calculations, we think it likely that the incumbent will *adjust her use of non-programmatic goods*.

Concretely, if the incumbent thinks the policy will increase the number of votes she receives

---

[4]In the typology in Diaz-Cayeros, Estévez and Magaloni (2016, 8), non-programmatic goods are termed "discretional" goods and the same two sub-categories are defined.

[5]This is the classic definition of clientelism (Kitschelt and Wilkinson, 2007; Stokes, 2007; Hicken, 2011). Recently, scholars have argued for jettisoning the requirement that distribution is contingent on how someone votes for it to qualify as clientelistic. We direct readers interested in this debate to Hicken and Nathan (2020) and Golden, Nazrullaeva and Wolton (2021).

[6]Studies of the PISH provide evidence that voters understand this (e.g. Imai, King and Rivera, 2019, 719).

from policy beneficiaries (Manacorda, Miguel and Vigorito, 2011; Pop-Eleches and Pop-Eleches, 2012), she may reduce the amount of non-programmatic goods directed at them (those goods are no longer needed), while keeping constant the amount directed at non-beneficiaries. Alternatively, she may reduce the amount distributed to beneficiaries and increase the amount distributed to non-beneficiaries (to make sure their electoral support for her does not decline, given they missed out on the programmatic policy).

Alternatively, the incumbent might calculate that the policy will decrease the number of votes she receives from beneficiaries (Blattman, Emeriau and Fiala, 2018; De Kadt and Lieberman, 2020; Saito, 2010). In a political system where non-programmatic goods feature prominently, voters may be accustomed to having to sing for their supper; meaning vote for the incumbent to receive material benefits. In these circumstances, a programmatic policy may have the effect of *untethering* policy beneficiaries from the incumbent, enabling them to vote according to policy, partisan, or valence preferences. An incumbent who thinks her support might decline among beneficiaries may decide to increase the volume of non-programmatic goods directed at them, to keep them interested in voting for her.

Although which scenario becomes more plausible may depend on context specific factors, what all of these scenarios have in common is that the presence of a programmatic policy results in changes to the distribution of non-programmatic goods. Whereas the amount of non-programmatic goods received by beneficiaries and non-beneficiaries may not have been systematically different prior to the advent of the policy, its enactment could kick off a redistribution of resources, such that one group ends up with systematically more than the other. If so, it would mean that policy beneficiaries and non-beneficiaries differ on a second dimension, the amount of non-programmatic goods they receive, which researchers interested in the PISH have not considered. If this is the case, then it is no longer safe to assume that any observed differences in these two groups' voting behavior are caused by the policy. Rather, researchers should operate under the assumption that they are caused by the compound effect of both the programmatic and non-programmatic goods.

To our knowledge, only one study of the PISH has entertained the possibility that incumbents respond to the advent of a programmatic policy by adjusting their distribution of non-programmatic goods (Labonne, 2013). This study focused on the effect of a government-enacted CCT on incumbent vote shares in the 2010 municipal elections in the Philippines. Leveraging

variation across villages in whether households were eligible to receive the CCT, the author found no statistically discernible difference in incumbent vote share between eligible and ineligible villages in the same municipality. He did find a statistically discernible difference in the expected direction (eligible villages returned higher vote shares than ineligible villages) only after limiting the analysis to municipalities that received small amounts of *other* government transfers. On this basis, he reasoned that when municipalities receive other government transfers, mayors can redistribute those transfers to ineligible villages. This, in turn, has the effect of bringing up their vote shares to a level indistinguishable to that of eligible villages. Because the government transfers he examined, the Internal Revenue Allocation, are distributed to municipalities (not to villages within municipalities), he was unable to subject this intriguing conjecture to further analysis. Thus, we do not have conclusive evidence that these extra government transfers were, in fact, being sent to ineligible villages (as he posits), or to eligible villages, or under an entirely different logic.

As far as we can tell, no subsequent study has subjected the core idea here — that incumbents use non-programmatic goods to offset an anticipated effect of the programmatic policy — to further theorizing or analysis. We do not think the lack of attention to this possibility is warranted. In industrialized democracies, the literature furnishes plenty of examples of incumbents adjusting year-to-year allocations of pork-barrel spending with a view to enhancing their re-election prospects (Stein and Bickers, 1994; Dixit and Londregan, 1996; Golden and Picci, 2008; Dahlberg and Johansson, 2002; Tavitz, 2009; Catalinac, Bueno de Mesquita and Smith, 2019; Spáč, 2021). To the extent that policy beneficiaries are concentrated in particular geographic regions, it is plausible that incumbents could increase or decrease the amount of pork-barrelling in those regions. In developing democracies, too, clientelism scholars document a vast array of material goods that flow from incumbent to voters at the time of elections, which can range from "cash to cookware to corrugated metal" (Hicken, 2011, 291). This is in addition to any pork-barrelling that occurs (Harris and Posner, 2018). The enormous network of brokers used to detect whose votes can be bought and for how much would presumably also be able to detect changes in the wake of a programmatic policy and relay that information upward (e.g. Brierley and Nathan, 2021).

The lack of attention to incumbent politicians' responses to programmatic policies is curious in light of the fact that some scholars have advanced the possibility that programmatic policies

could hasten the *demise* of clientelism (Frey, 2019; Blattman, Emeriau and Fiala, 2018; Larreguy, Marshall and Trucco, N.D.). The causal chain imagined by these scholars goes something like this: because policy beneficiaries know that benefits flow regardless of who they vote for, evidence that they vote for the incumbent in larger numbers is evidence they are considering *non*-clientelistic factors when casting their votes. This means that the programmatic policy has broken the clientelistic exchange by giving beneficiaries the room to evaluate the incumbent on an alternative, non-clientelistic dimension. If programmatic policies have the potential to transform voter calculus in this manner, then one policy implication would be to enact more programmatic policies in clientelistic settings. Our discussion above suggests that such a conclusion might be warranted, but only after scholars have verified that policy beneficiaries are not receiving systematically larger allocations of non-programmatic goods. If they are, then their increased tendency to vote for the incumbent likely reflects the compound effect of *both* the programmatic policy and the extra non-programmatic goods. This would leave the policy's impact on clientelism, and the corresponding policy fix, much less clear.

In what follows, we subject this conjecture, that programmatic policies bring about a change in the allocation of non-programmatic goods, to rigorous empirical analysis. Like other studies, we choose a single programmatic policy (a snow subsidy) enacted in a single country (Japan) and leverage features of its implementation to identify its effects.

2 Case of Japan

To examine whether voters who receive a programmatic policy receive systematically more or less non-programmatic goods than those who do not, we turn to Japan. Japan is an excellent case for us for at least two reasons. One, the Japanese government enacts policies that qualify as programmatic, meaning their distribution is subject to a set of formalized, publicly-available, and non-manipulable rules. Two, the Japanese government has been controlled by a single party for sixty two of the past sixty six years and a central component of this party's electoral strategy has been the judicious distribution of non-programmatic goods. The non-programmatic goods scholars know the most about are of the partisan bias/pork-barrel type, meaning they consist of discretionary spending on local communities. Our empirical strategy relies on the fact that both the programmatic policy and the non-programmatic goods are bestowed on the

same geographic unit (the municipality). This creates a setting in which we can compare the amounts of non-programmatic goods received by municipalities that receive the programmatic policy and otherwise-similar municipalities that do not.

First, the programmatic policy. In 1962, the Special Measures Act Concerning Counter-measures for Heavy Snowfall Areas (*Gosetsu Chitai Taisaku Tokubetsu Sochi Ho* in Japanese, henceforth called the "Snow Act") was enacted. Originating as a private member bill bearing the signatories of 101 Members of the House of Representatives (HoR), the Snow Act was one of a number of laws passed in the early 1960s that established government support for areas of Japan considered disadvantaged.[7] Historically, heavy snowfall had presented a major obstacle to industrial development and the improvement of living standards in Japan's snowiest regions. It hindered economic activity, paralyzed traffic, isolated communities, and facilitated depopulation. The Snow Act aimed to minimize this damage.

To this end, it established four main benefits for municipalities designated as "heavy snowfall municipalities."[8] First, these municipalities would qualify for extra central government money to cover the costs of maintaining roads, buildings and heating systems and providing education, medical infrastructure, and public livelihood assistance. This extra money would be paid through a formula-decided government transfer called the Local Allocation Tax (LAT). Second, when constructing roads or school buildings in revenue-sharing arrangements with an upper-tier government, a larger share of the cost would be shouldered by the latter. Third, these municipalities were permitted to issue special local bonds to finance measures to deal with snow, such as widening roads, investing in snow removal equipment such as snowplows or snow-melters, and implementing disaster-prevention measures. Fourth, their residents were granted special tax benefits, including reduced car, income, and property taxes, as well as home renovation assistance.[9]

The Snow Act and related ordinances stipulated that a municipality could be designated a "heavy-snowfall municipality" if more than two-thirds of its geographic area qualifies as a "heavy-snowfall area," in which the height of accumulated snow over the preceding thirty-year

---

[7]Others include the Mountain Villages Development Act, the Peninsular Areas Development Act, the Remote Islands Development Act, and the Special Measures Act for the Promotion and Development of the Amami Islands, as well as others listed in Naoi (2015, 54-55). and Saito (2010, 109).

[8]Municipalities consist of villages, towns, cities, and special wards.

[9]Examples of these benefits are available at: https://www.pref.niigata.lg.jp/sec/chiikiseisaku/1200330044375.html.

Figure 1: The shaded areas depict areas that, as of 2016, had been designated "heavy-snowfall" areas under the rules of the 1962 Snow Act.
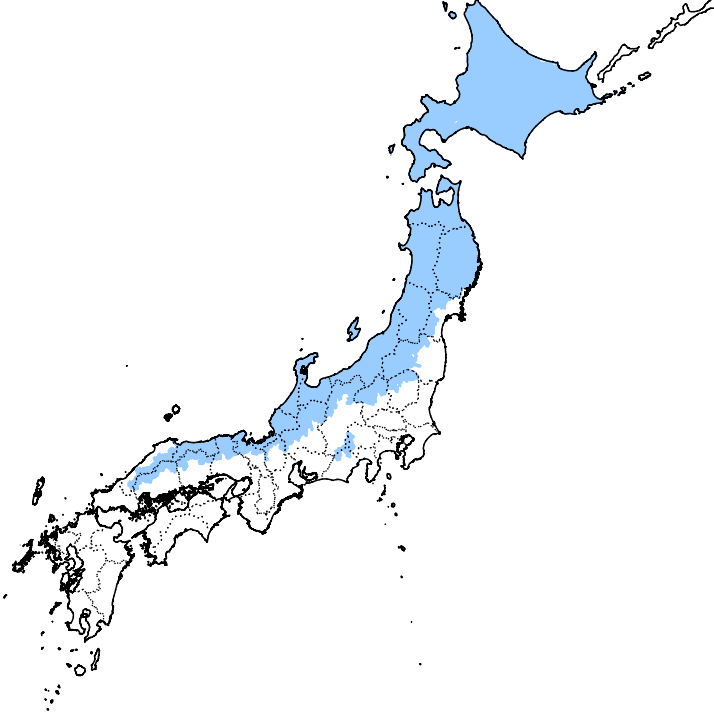
period exceeded $5,000$ cm (164 feet) per year.[10] For municipality m, the "height of accumulated snow" is given by calculating the average height of accumulated snow on a given day of the year, adding this to the average height of accumulated snow on the next day, and so on, for all the days in which the municipality had accumulated snowfall. Intuitively, if 50 cm of snow fell on the first day of winter and remained piled up for the next 100 days without any new snow falling, this municipality would have experienced $5,000$ cm of accumulated snow that year. Figure 1 presents a map of Japan. The shaded area consists of municipalities qualifying as "heavy-snowfall," which tend to be concentrated in the northwest. As of 1980, when our study begins, approximately 30% of Japanese municipalities had received this designation. Together, they make up approximately 50% of land in Japan.

The snow subsidy qualifies as programmatic. The rules governing eligibility are formalized and publicly-available on the government's website, along with a list of the municipalities that have qualified.[11] For reasons we explain below, we focus on the 1980-2005 period. In this period,

---

[10]Data from weather stations across Japan was used to define heavy snowfall areas. There are several additional ways municipalities can become eligible, which are described in Online Appendix A.

[11]The criteria is available at: http://www.mlit.go.jp/kokudoseisaku/chisei/crd_chisei_tk_000010.html.

the only time the list of designated municipalities changed was when a municipality ceased to exist due to a merger with a neighboring municipality. In these cases, the municipality disappeared off the list of designated municipalities (because it no longer existed), but its designation was transferred to the municipality with which it merged. Almost every merger happened between 2001 and 2005, when the number of municipalities was reduced from approximately 3,300 to approximately 1,800 (Horiuchi, Saito and Yamada, 2015). This means that in the period 1980-2000, there were no changes to the list of designated municipalities and after 2000, the only changes that occurred were the result of mergers. This provides indirect evidence that incumbent members of the ruling party were not manipulating eligibility.[12]

Second, the non-programmatic goods. The Liberal Democratic Party (LDP) has been in control of government since 1955, with the exception of ten months between 1993 and 1994 and three years between 2009 and 2012. A voluminous literature documents the single-minded focus of LDP politicians on securing pork-barrel projects for their districts (Catalinac, Bueno de Mesquita and Smith, 2019; Christensen and Selway, 2017; Naoi, 2015; McMichael, 2018; Krauss and Pekkanen, 2010; Scheiner, 2006; Hirano, 2006; Horiuchi and Saito, 2003; Ramseyer and Rosenbluth, 1993; Curtis, 1971). One driver of this was the electoral system used to select Members of the HoR. The system was single-non-transferable-vote in multi-member districts (SNTV-MMD). Under this system, majority-seeking parties had to run more than one candidate in most districts, which meant that individual LDP politicians were pitted against each other (Ramseyer and Rosenbluth, 1993). Numerous studies explain why intraparty competition tends to increase pork-barrelling (Martin, 2011; Golden and Picci, 2008; Carey and Shugart, 1995). In 1994, the system was changed to mixed-member majoritarian (MMM), which eliminated intraparty competition. LDP politicians ratcheted down their emphasis on pork-barreling in favor of programmatic goods (Catalinac, 2016). However, the party also began distributing pork under a different logic (funneling it toward supporters who complied with a vote-trading strategy designed to help the party win more seats) (Catalinac and Motolinia, 2021).

A second driver of pork-barrelling in Japan is the structure of fiscal relations between the central and local governments. According to Mochida (2001, 85), "the main features of the Japanese system are centralized tax administration, decentralized provision of public services,

---

[12]Because decisions to merge may have been influenced by a municipality's desire to receive the designation, including post-2001 municipalities in our analyses could introduce post-treatment bias. Results are unchanged statistically and substantively when we limit analyses to the 1980-2000 period.

and dependence of local government on intergovernmental transfers." Whereas approximately 60% of taxes are collected by the central government, most services are provided by local governments. Every year, the central government redistributes approximately 45% of its tax-generated revenue to local governments to help pay for services such as road construction, health care, sewerage, clean drinking water, and waste disposal. Because municipalities face restrictions on the taxes they are permitted to levy and on their ability to borrow, central government transfers constitute an average of 33% of their annual income (Saito, 2010; Scheiner, 2006; Fukui and Fukai, 1996). For the average municipality, about half of this comes from the formula-based transfer mentioned above (LAT), while the other half comes from a pool of discretionary funds called "national treasury disbursements" (NTD). Municipalities apply for NTD for the purpose of funding projects and bureaucrats are charged with deciding which projects to fund. Scheiner (2006) argues that dependence on the central government made local politicians susceptible to being pulled into clientelistic exchanges with their LDP HoR incumbents, in which they traded vote-mobilization efforts for help securing pork-barrel projects. Saito (2010) argued that LDP HoR incumbents used NTD to buy votes. He found that HoR electoral districts with more HoR incumbents per voter and more local politicians, respectively, received larger NTD allocations.

A third driver of pork-barrelling in Japan is the way votes are counted in elections. Catalinac, Bueno de Mesquita and Smith (2019) reasoned that municipalities' dependence on the central government for transfers, combined with the fact that votes are counted at the level of the municipality, virtually all municipalities are contained within a single electoral district,[13] and the LDP (almost) always wins, meant that LDP HoR incumbents would have been well-positioned to make the distribution of NTD contingent on how well the municipality had "performed" in the most recent HoR election. Their analyses, conducted on the seven HoR elections held between 1980 and 2000, show that a municipality's post-election NTD allocation was a function of the share of eligible voters in the municipality who had cast their votes for winning LDP candidates. Municipalities whose vote shares compared favorably to others in the same district received more NTD, while municipalities whose vote shares compared less favorably received less. Observationally, then, NTD flows to core supporters within districts, but those core supporters have to demonstrate their fealty in every election to continue receiving it.

---

[13]The percentage of municipalities that spanned more than one district was 0.09% in the 1980-1993 HoR elections, 0.45% in the 1996 and 2000 elections, 1% in the 2003 election, and 3.6% in the 2005 election (Mizusaki, 2014).

To summarize, NTD is not allocated according to formalized, publicly-available, and non-manipulable rules. Evidence suggests that it is distributed to municipalities with a view to enhancing the electoral prospects of LDP politicians. Thus, it is non-programmatic. Helpfully, our programmatic policy of interest (the snow subsidy) is also distributed to municipalities, but on the basis of a factor plausibly exogenous to voting behavior (snowfall). This provides a nice setting in which we can examine how incumbents adjust their distribution of non-programmatic goods in the presence of a programmatic policy.

3   Empirical Strategy

The Snow Act created a set of municipalities eligible for the conglomeration of benefits we call the "snow subsidy." Figure 1 shows that these "beneficiary municipalities" are clumped together in the northwest. This clumping generates a border separating beneficiary municipalities from non-beneficiary municipalities. Because accumulated snowfall determines beneficiary status, and the location of beneficiary and non-beneficiary municipalities determines the location of the border, we can use a geographic regression discontinuity (GRD) design (Keele and Titiunik, 2015, 2016). The identifying assumption of a GRD design is that when a border is (arguably) exogenously drawn, researchers can, providing certain conditions are met, treat units that are close to but on opposite sides of the border as identical on all dimensions apart from the fact that some are in the "treated" zone, while others are in the "control" zone. After verifying that those conditions are met, researchers can attribute any difference in outcome to the causal effect of being treated.[14]

In our case, our two outcomes of interest (levels of electoral support for the LDP and NTD allocations) differ systematically according to features of a municipality's electoral district such as number of local politicians (Saito, 2010), number of HoR representatives per voter (Horiuchi and Saito, 2003), and asymmetry in the size of constituent municipalities (Catalinac, Bueno de Mesquita and Smith, 2019). For this reason, it is critical to limit the comparison of beneficiary and non-beneficiary municipalities to those that are in close proximity to one another *and* in the same electoral district. In the nine HoR elections held between 1980 and 2005, between 11%

---

[14]Strictly-speaking, in our case the border itself does not determine treatment; cumulative snowfall over a thirty-year period does. We decided against a regression discontinuity design with snowfall due to the difficulty in assembling data on the height of accumulated snow over such a long period for the universe of Japanese municipalities.

and 19% of the total number of districts in each election were "mixed," meaning that beneficiary municipalities coexisted with non-beneficiary municipalities.[15] In the main analysis below, we limit our focus to municipalities in mixed districts and use district-year fixed effects to perform a strictly within-district comparison.

While the Snow Act was enacted in 1962, data on NTD allocations are not available until 1977 (Saito, 2010). We focus on the 1980-2005 period because data on all our variables is available. For this reason, our analyses capture the equilibrium effect of the programmatic policy in the long run. We build a comprehensive dataset comprising voting behavior in the nine HoR elections held during this time, annual NTD allocations, snow subsidy eligibility, and other features of the 3,300+ municipalities that existed. Because the snow subsidy encompasses different types of benefits, converting these to a monetary amount for each municipality-year be difficult, if not impossible. Thus, we do not exploit variation in the *amounts* of snow subsidy received by beneficiaries (the intensity of the treatment), but variation in *eligibility* for the treatment across municipalities. For data on voting behavior, NTD allocations, and other features of municipalities, we use the replication data for Catalinac, Bueno de Mesquita and Smith (2019), supplemented for the post-2000 period with the raw data from JED-M and Nikkei NEEDs (Mizusaki, 2014). For data on municipalities' eligibility for the snow subsidy, geographical location, and altitude, we use data from Japan's National Land Numerical Information Service and Geospatial Information Authority.[16]

Concretely, we take the universe of municipality-years in mixed districts and calculate the distance between their centroids and the nearest location on the border. Then we set a very narrow bandwidth of distance to the border and restrict our observations to the municipality-years that fall within this range. With this sample, we estimate a local linear regression:

$$y_{mdt} = \alpha_{dt} + \tau \text{Snow Subsidy}_{mdt} + f(D_{mdt} < 0) + f(D_{mdt} \geq 0) + \epsilon_{mdt} \tag{1}$$

where the unit of analysis is municipality m in district-year dt. We examine two outcomes, which are measured in municipality m in district dt. α_{dt} denotes fixed effects by district-year. D_{mdt} is the running variable, a one-dimensional distance between the centroid of municipality m

---

[15]Mixed districts exist in 14 of Japan's 47 prefectures: Miyagi, Fukushima, Tochigi, Gunma, Yamanashi, Nagano, Gifu, Shizuoka, Shiga, Kyoto, Hyogo, Shimane, Okayama, and Hiroshima.

[16]For more information about the data, as well as descriptive statistics of the variables used in our analyses, see Online Appendix B.

and its nearest point on the border (beneficiary municipalities receive positive values and non-beneficiary municipalities receive negative ones).[17] $f(\cdot)$ represents a polynomial function of distance to the border estimated separately for the municipalities on both sides. Snow Subsidy$_{mdt}$ is a dummy variable that takes a value of 1 if the municipality receives the subsidy and 0 otherwise. τ captures the local average treatment effect (LATE) of the snow subsidy at the threshold (border). Following standard practice, observations are weighted by their distance to the border using triangular kernel weighting and standard errors are clustered on municipality. We use a range of bandwidths between $\pm 4,000$ and $\pm 15,000$ (in meters) to select observations and report the local average treatment effect (LATE) estimated with all of these bandwidths.[18]

A GRD design yields valid causal estimates of a treatment when the border is not associated with other discontinuities in unit-level characteristics, actors have not manipulated the treatment assignment since the law's enactment, and there is no "compound treatment" (which occurs when the border is synonymous with other boundaries). First, Online Appendix C checks for discontinuities in eight municipality-level attributes.[19] We find a discontinuity only in area size: beneficiary municipalities immediately proximate to the border are slightly larger than their same-district non-beneficiary counterparts. This means we must exercise caution in interpreting our estimates of the treatment as causal, but we run all the analyses below with and without a control for area size and find similar results. Second, the criteria governing subsidy eligibility makes it unlikely sorting occurred. Online Appendix D reports the results of a McCrary (2008) sorting test showing no evidence of self-sorting. Third, because the treatment is assigned to municipalities, our border is drawn *around* municipalities. It is not synonymous with a single municipality, nor any other administrative or political entity. We are not aware of anything that could occur along this border that might signify a compound treatment.

---

[17]A one-dimensional distance can be problematic because the units being compared could be close to the border yet far from each other (Keele and Titiunik, 2015). Our use of district-year fixed effects avoids this concern (municipalities are only ever compared to others in the same district).

[18]An alternative approach is to use the mean squared error optimal bandwidth selector, which yields the bandwidth of ± 6375. This is too narrow, however, because it leaves us with a single observation in most district-years. Given that we want to compare beneficiary and non-beneficiary municipalities in the same district-year, we must use slightly wider bandwidths. Note that the full range of distances to the border among municipalities in mixed districts is $[-98,580, 54,663]$, so using a bandwidth of $\pm 15,000$ still represents a considerable narrowing of the sample.

[19]These are population, per capita income, population density, the proportion of the population who is dependent, the proportion of the population employed in agriculture, fiscal power, altitude, and area size.

4 Results

To preview our empirical findings, we first show that the snow subsidy has no statistically discernible impact on the LDP's vote share at the geographical threshold. This is inconsistent with the PISH. Next, we examine how the snow subsidy impacts the allocation of non-programmatic goods. Here, we find that the snow subsidy has a statistically significant, positive impact on the amount of NTD at the threshold. This means that policy beneficiaries receive *more* non-programmatic goods than otherwise-similar non-beneficiaries in the same district. To make sense of these two results, we turn to anecdotal and survey evidence. We provide suggestive evidence that access to a programmatic policy lowers beneficiaries' incentives to continue voting for their incumbent, providing the incumbent with an incentive to offset this negative effect by promising more non-programmatic goods. Finally, we explore two alternative explanations for why beneficiary municipalities receive more NTD. This enables us to rule out explanations based on the material need or lobbying capacity of beneficiary municipalities.

4.1 The Impact of the Snow Subsidy on LDP Vote Share

We begin with the examination of the PISH. In this analysis, the outcome is LDP Vote Share, the proportion of total votes cast for LDP candidates in the municipality. Because districts were multi-member (electing between two and six winners) prior to 1994 and single-member after 1994, they typically saw between two and four LDP candidates prior to 1994 and one after 1994. This is the operationalization of electoral support for the incumbent used in most studies on the PISH.

Figure 2 summarizes the LATE of Snow Subsidy on LDP Vote Share estimated on the nine HoR elections, 1980-2005. On the x-axis, we vary the bandwidths of the local linear regression from $\pm 4,000$ to $\pm 15,000$. The y-axis displays the coefficient on Snow Subsidy and corresponding 90% and 95% confidence intervals. The number of observations changes from 1,221 at the most narrow bandwidth, which equates to an average of 5.5 municipalities per district-year, to 3,802 at the widest bandwidth shown, which equates to an average of 17 municipalities per district-year. Even at the widest bandwidth, then, we are only including 53% of the municipalities in mixed districts.

In Figure 2, the LATE of Snow Subsidy consistently shows a negative and statistically

Figure 2: Receiving the snow subsidy results in no statistically-significant difference in LDP Vote Share for municipalities in mixed districts, 1980-2005.
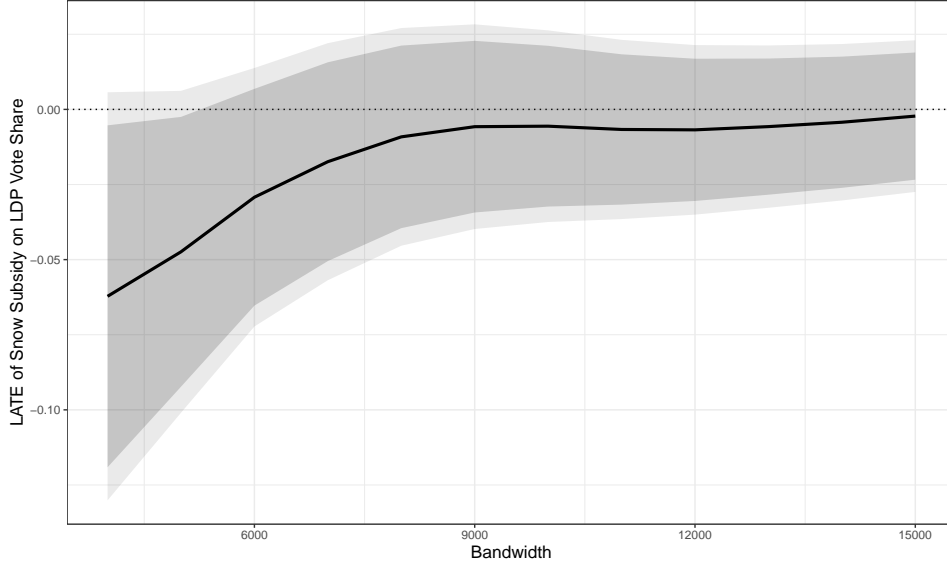
Note: This figure depicts the coefficient estimates on Snow Subsidy obtained from local linear regressions of LDP Vote Share on beneficiary status when the bandwidth is changed from $\pm 4,000$ to $\pm 15,000$. Shaded areas indicate 90%/95% confidence intervals.

insignificant sign across the entire range of bandwidths. This means that there is no difference in LDP vote share at the geographical threshold. Therefore, we do not have strong evidence that the snow subsidy boosts electoral support for the incumbent party. This result is at odds with the PISH.[20]

4.2 The Impact of the Snow Subsidy on NTD

Next, we analyze how the presence of a programmatic policy influences the allocation of non-programmatic goods. In this analysis, our outcome of interest is Post-Election Per Capita Transfers, or the logarithm of per capita NTD received by municipalities in the fiscal years following the same nine HoR elections, 1980-2005. We use the amount received after elections because NTD is withheld until after municipalities' "performance" in the election is discerned (Catalinac, Bueno de Mesquita and Smith, 2019).

Figure 3 depicts the LATE of Snow Subsidy on Post-Election Per Capita Transfers. The bandwidths used are the same as those in Figure 2. We see that at narrower bandwidths, the effect of Snow Subsidy is positive but imprecisely estimated because at these ranges, we do not have a sufficient number of observations on both sides of the border in many district-years.

---

[20]Online Appendix E shows that we obtain the same null effect of Snow Subsidy when we use the alternative operationalization of LDP support used in Catalinac, Bueno de Mesquita and Smith (2019).

Figure 3: Receiving the snow subsidy results in larger per capita NTD allocations after elections for municipalities in mixed districts, 1980-2005.
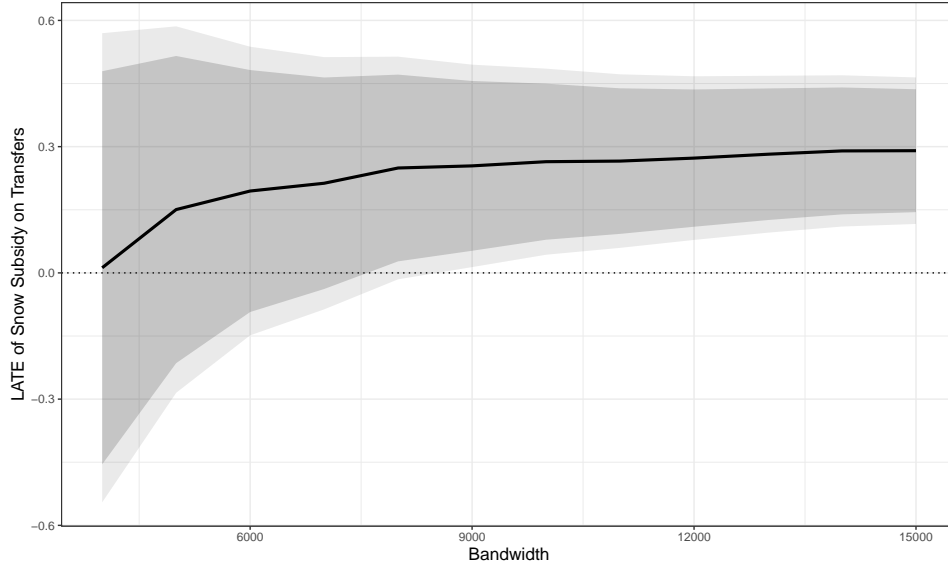
Note: This figure depicts the coefficient estimates on Snow Subsidy obtained from local linear regressions of Post-Election Per Capita Transfers on beneficiary status when the bandwidth is changed from $\pm 4,000$ to $\pm 15,000$. Shaded areas indicate 90%/95% confidence intervals.

However, once we widen the bandwidths to include more observations (bandwidth $\geq 9,000$), the positive effect of Snow Subsidy becomes statistically significant.[21] The estimated effect of the snow subsidy is roughly 0.23, which means that beneficiary municipalities near the geographical border receive a per capita NTD allocation that is 25.9% ($exp(0.23) = 1.259$) larger than their otherwise-similar, same-district non-beneficiary counterparts.

In sum, our results show that beneficiary municipalities do not exhibit more electoral support for their LDP incumbent than otherwise-similar non-beneficiary municipalities in the same electoral district. Hence, our case reveals no support for the PISH. By contrast, our intuition that the two sets of municipalities would receive systematically different amounts of non-programmatic goods is borne out in the analysis. Beneficiary municipalities receive systematically *larger* per capita NTD allocations than their otherwise-similar, same-district non-beneficiary counterparts. The fact that beneficiary municipalities receive *both* the programmatic policy *and* the extra NTD, yet do not deliver more electoral support for the incumbent, is difficult to reconcile with the PISH.[22]

---

[21]Online Appendix C shows that even when bandwidth $\geq 9,000$, we are preserving the covariate balance (absence of discontinuities) across the other municipality characteristics.

[22]Online Appendix F shows that our results hold when we use an alternative research design and analyze all municipalities in mixed districts.

4.3 Mechanism

One way to interpret the above findings is that programmatic policies *decrease* the willingness of beneficiaries to vote for incumbents (De Kadt and Lieberman, 2020; Blattman, Emeriau and Fiala, 2018; Saito, 2010), prompting incumbents to try to offset this anticipated negative effect by *increasing* the volume of non-programmatic goods delivered to beneficiaries. This may be why we observe a situation in which beneficiaries of the snow subsidy receive more NTD than non-beneficiaries. By extension, this means that if incumbent politicians had not distributed more non-programmatic goods to beneficiaries, they would have received fewer votes from them. In this section, we use anecdotes and survey data to subject this interpretation to greater scrutiny.

First, newspaper articles offer anecdotal support for the possibility that the snow subsidy, by improving living conditions in heavy-snowfall regions, has made voters in these regions *less* likely to support LDP incumbents. In one article, the president of a rice-growing company in a beneficiary municipality described feeling less compelled to vote for his LDP incumbent because his community now had "a bullet train, a highway, and underground pipes with nozzles that can melt snow" (*shosetsu paipu*) (Asahi Shinbun, 2000). In another, the head of a construction company in a beneficiary municipality explained that construction companies depended on LDP politicians getting elected and funneling public works contracts their way, but it was becoming harder and harder to convince the area's residents to vote for LDP politicians. He said that residents used to understand the value of politicians who could build the roads needed to ensure that the region was not cut off from the rest of Japan due to heavy snowfall, but snow melters had solved this problem, reducing residents' enthusiasm for the LDP (Asahi Shinbun, 2001).

Second, our analysis of the Nationwide Survey of Neighborhood Associations (Pekkanen, Tsujinaka and Yamamoto, 2014) provides further evidence that access to the snow subsidy can lower beneficiaries' willingness to support the LDP. Conducted between 2006 and 2007, this survey aimed to understand the function of Japan's neighborhood associations (henceforth "NHAs"). NHAs are informal, voluntary groupings organized at the level of the neighborhood. They provide social services, mediate interactions between residents, bureaucrats and politicians, and mobilize voters during election campaigns (Pekkanen, 2009).[23] Of the 18,404 NHA heads who responded to the survey, approximately 3,000 were located in our 32 mixed districts, spanning 53 beneficiary municipalities and 106 non-beneficiary municipalities therein.

---

[23]According to one study, nearly all Japanese adults reported being part of an NHA (Pekkanen, 2009, 30).

Of particular interest is the question "What type of activities does your NHA conduct?" In the question, one of the items was "Assisting [and recommending] a particular candidate in election campaigns," and NHA heads were presented with a binary Yes or No choice. Although this question is not specifically about supporting an LDP candidate, it at least allows us to analyze whether systematic differences exist between beneficiary and non-beneficiary municipalities in residents' willingness to get involved in electoral campaigns. If we find a negative relationship between eligibility for the snow subsidy and this outcome, it could mean that the snow subsidy has alleviated beneficiaries' need to help a candidate from the LDP or another party in order to maintain access to non-programmatic benefits.

In this exercise, we estimate a multilevel linear probability model of Yes answers on Snow Subsidy for the universe of observations in mixed districts.[24] The model includes NHA- and municipality-level controls, random effects by municipality (as NHAs are nested within municipalities), and fixed effects for electoral district (to compare attitudes in beneficiary and non-beneficiary municipalities in the same district). As controls, we use the number of member households at the NHA level and the same time-varying controls used above at the municipality level, with the exception of population, which we replace with the finer-grained measure of household size at the NHA level.

Table 1 presents the results. The coefficient on Snow Subsidy is negative and marginally significant ($p = 0.079$). This means that NHA heads in beneficiary municipalities are less likely to report getting involved in election campaigns on behalf of a particular candidate relative to their counterparts in same-district non-beneficiary municipalities. Substantively, receiving the snow subsidy can decrease the probability of campaign involvement by 6 percentage points, which corresponds to a 22.7% decrease over the sample mean.

Together, the anecdotal and survey evidence points to the possibility that access to a programmatic policy can, at least to some extent, lower beneficiaries' need to support candidates from the LDP or another party. This, in turn, implies that LDP politicians in mixed districts may expect lower vote shares among program beneficiaries. To offset this, they may want to promise a greater amount of non-programmatic goods to beneficiaries.

---

[24]We cannot use a GRD design here because of the small sample size at the municipality level.

Table 1: The negative, statistically significant coefficient on Snow Subsidy shows that NHA heads in beneficiary municipalities are less likely to report supporting a particular candidate during election campaigns.

	(1) Supporting a Candidate During Electoral Campaigns
Snow Subsidy	$-0.06^{\dagger}$
	(0.03)
NHA Household (log)	-0.01
	(0.01)
Population Density (log)	0.03
	(0.02)
Income per capita (log)	0.04
	(0.20)
Primary Industry Proportion (log)	0.06^{*}
	(0.03)
Dependency Proportion	0.60
	(0.65)
Fiscal Power	0.10
	(0.09)
Municipality Random Effects	Yes
District Fixed Effects	Yes
N	3,090
N of Districts	35

Note: $^{\dagger}p<0.10$; $^{*}p<0.05$. NHA = neighborhood association. Observations are NHA heads in mixed districts who responded to the survey. The model is estimated with a linear probability model with random effects by municipality and fixed effects by district.

4.4   Alternative Explanations

In this section, we consider two plausible alternative explanations for why beneficiary municipalities receive more non-programmatic goods than non-beneficiary municipalities. One is a need-based explanation, and the other is based on the lobbying capacity of beneficiary municipalities.

On the one hand, it might be the case that beneficiary municipalities receive more non-programmatic goods simply because they have greater needs. In other words, despite its stated aim, the snow subsidy may not be sufficient to meet the needs of heavy-snowfall municipalities, and NTD may be used to make up the shortfall. A perfect way to test this possibility would be to quantify how much extra need the snow subsidy fails to meet in beneficiary municipalities and assess whether the amount of NTD the municipality received is enough to cover it. But given this is implausible, what we can do is an indirect test of this hypothesis: if the snow subsidy is failing to meet the needs of the beneficiary municipalities in our sample, then it is reasonable to expect that it would also be failing to meet the needs of beneficiary municipalities

outside our sample (in other parts of Japan). To the extent that the dummy for Snow Subsidy is simply capturing differences in remaining needs between the two types of municipalities (and not the effects of programmatic benefits), we should expect that beneficiary municipalities outside of mixed districts also receive larger per capita NTD allocations than their non-beneficiary counterparts.

To examine this, we regress Post-Election Per Capita Transfers on Snow Subsidy for all municipalities outside of mixed districts. In this specification, we cannot use district-year fixed effects because there is no within-district variation in beneficiary status (districts are comprised only of beneficiary or non-beneficiary municipalities). However, it is still important to control for district-level features that influence Post-Election Per Capita Transfers. Our specification therefore includes year fixed effects, district-year random effects, and time-varying municipality- and district-level controls.[25] Standard errors are clustered on the municipality.

Table 2 presents the results. The coefficient on Snow Subsidy is negative and statistically significant, the opposite of what we observe in the above analysis. This means that outside of mixed districts, beneficiary municipalities tend to receive *smaller* per capita NTD allocations than non-beneficiary municipalities. This casts doubt on the possibility that beneficiary municipalities in mixed districts receive larger NTD allocations because of differences in needs. It is only in districts with a specific configuration of municipalities (i.e. the coexistence of beneficiaries and non-beneficiaries) that program beneficiaries receive more transfers than non-beneficiaries.

On the other hand, it might be the case that beneficiary municipalities receive more non-programmatic goods because of organizational advantages in lobbying. One of the Snow Act's goals is to "promote cooperation among residents and volunteer activities" in beneficiary municipalities. It is possible that the snow subsidy endows beneficiary municipalities with advantages in the process through which NTD allocations are applied for and received. The central government shrouds this in mystery, but we know that municipalities put together proposals for projects and solicit the help of LDP Diet members in lobbying government bureaucrats. If beneficiary municipalities have greater access to government figures (both at the local and national levels), enhanced lobbying skills, or greater social capital, this could explain why they are more

---

[25]In addition to the municipality-level controls included in the above regressions, we include district-level versions of the first six municipality-level controls, plus four other attributes flagged by scholars as influencing transfers: number of municipalities, asymmetry in municipality size, people per seat (an indicator of malapportionment), and share of seats won by the LDP).

Table 2: The negative, statistically significant coefficient on Snow Subsidy shows that outside of mixed districts, beneficiary municipalities receive *smaller* per capita NTD allocations than their non-beneficiary counterparts.

	(1)
	Post-Election Per Capita Transfers (log)
Municipality-Level Variable	
Snow Subsidy	−0.079*
	(0.033)
Fiscal Power	0.324*
	(0.028)
Proportion Dependent	2.858*
	(0.178)
Proportion in Agriculture	−0.133
	(0.096)
Population (log)	0.211*
	(0.058)
Income per Capita (log)	−0.172*
	(0.031)
Population Density (log)	−0.282*
	(0.058)
Area Size (log)	−0.131*
	(0.058)
Altitude (log)	0.005
	(0.004)
District-Level Variable	
Fiscal Power	−0.976*
	(0.088)
Proportion Dependent	0.823*
	(0.275)
Proportion in Agriculture	−2.071*
	(0.558)
Population (log)	−0.251*
	(0.057)
Income per Capita (log)	−0.059
	(0.062)
Population Density (log)	0.050*
	(0.022)
Population Per Seat	0.071*
	(0.024)
Asymmetry in Municipality Size	0.480*
	(0.081)
Number of Municipalities (log)	0.035
	(0.035)
Number of LDP Seats	−0.135*
	(0.029)
District-Year Random Effects	Yes
Year Fixed Effects	Yes
N	18,918
N of Districts	1,193

Note: *$p<0.05$. The model is estimated with a multilevel linear model with random effects by district-year and fixed effects by election.

successful in getting their projects funded (Saito, 2010).

To examine this possibility, we return to the NHA data. We analyze seven questions that probe the NHA's access to, influence on, and relationship with local government.[26] Specifically, five asked about the means used to ensure resident interests are reflected in policymaking, another asked about the degree to which the NHA feels it can monitor local government, and the seventh asked about the extent to which the NHA can influence local government policies. While NHA heads are not ordinary voters and these questions do not perfectly capture our quantity of interest, it is reasonable to expect that if beneficiary municipalities are endowed with superior connections to government officials or lobbying ability, this would be reflected in answers to these questions. We run a multilevel model with NHA- and municipality-level controls, random effects by municipality, and fixed effects for electoral district, as described in Table 1.

Table 3 presents the results. In all seven models, the estimate of Snow Subsidy is not statistically distinguishable from 0. Hence, NHA heads in beneficiary municipalities seem to be no different from their counterparts in same-district non-beneficiary municipalities in terms of their perceptions of their lobbying capacity. On this basis, we think it unlikely that an explanation based on beneficiaries' organizational ability could account for the positive effect of the snow subsidy on the amount of NTD.

5   Conclusion

Literature on the electoral impacts of programmatic policies tends to treat incumbents as passive bystanders, who sit back and watch as the effects of these policies unfold in their electorates. Doing so enables researchers to attribute any observed differences in electoral support for the incumbent between policy beneficiaries and non-beneficiaries to the impact of the policy. While it might make sense to do this in some settings, we made the case that it does not make sense in settings where incumbents have access to non-programmatic goods and use those goods to influence election results. In these settings, which characterize many developed and developing democracies, we argued that incumbents have incentives to anticipate how a given programmatic policy is likely to change the voting calculus of beneficiaries and respond to this by adapting their

---

[26]These appear in Online Appendix G.

Table 3: There are no statistically-discernible differences in answers to questions probing NHA heads' relationship with local government actors between beneficiary and non-beneficiary municipalities in the same district.

	(1) Monitor Local Government	(2) Consult Local Government	(3) Consult Senior Officials	(4) Consult Local Politicians	(5) Attend Government Meetings	(6) Lobby Local Assembly	(7) Influence on Local Government
Snow Subsidy	−0.002	0.02	−0.07	−0.05	0.09	−0.13	−0.01
	(0.04)	(0.08)	(0.10)	(0.08)	(0.08)	(0.09)	(0.08)
NHA Household (log)	0.03*	0.16*	0.13*	0.06*	0.03	−0.01	0.12*
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Population Density (log)	−0.06	−0.07	−0.07	−0.05	0.05	−0.13*	−0.10
	(0.03)	(0.06)	(0.07)	(0.06)	(0.06)	(0.06)	(0.06)
Income per capita (log)	0.16	−0.33	−1.07*	−0.51	−0.76	−0.18	−0.02
	(0.26)	(0.46)	(0.54)	(0.49)	(0.46)	(0.51)	(0.49)
Primary Industry Proportion (log)	0.01	0.16*	0.08	0.005	0.07	0.03	0.06
	(0.04)	(0.07)	(0.08)	(0.07)	(0.07)	(0.08)	(0.08)
Dependency Proportion	−0.69	−1.55	−2.12	−0.44	−1.98	0.28	−2.82
	(0.90)	(1.48)	(1.77)	(1.60)	(1.52)	(1.67)	(1.64)
Fiscal Power	−0.18	0.08	0.17	0.09	−0.24	0.22	−0.36
	(0.13)	(0.21)	(0.26)	(0.24)	(0.22)	(0.25)	(0.23)
Municipality Random Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	2,921	2,952	2,746	2,810	2,780	2,723	2,992
N of Districts	35	35	35	35	35	35	35

Note: $^*p<0.05$. NHA = neighborhood association. Observations include neighborhood associations in mixed districts. The model is estimated with a multilevel linear model with random effects by municipality and fixed effects by district.

allocation of non-programmatic goods. It follows that policy beneficiaries could end up with systematically more or less non-programmatic goods than their non-beneficiary counterparts, which would confound attempts to estimate the causal impact of these policies.

To evaluate this conjecture, we turned to Japan, where municipalities receive annual allotments of non-programmatic goods and differ in eligibility for a programmatic policy on the basis of historical levels of snowfall, a plausibly-exogenous factor. Our results show that the amount of non-programmatic goods incumbents deliver to municipalities receiving the programmatic policy differs systematically from the amount delivered to municipalities not receiving the policy. More specifically, beneficiary municipalities receive more non-programmatic goods than otherwise-similar non-beneficiary municipalities. It is worth reiterating that our research design enables us to have a high degree of confidence in these results. We consider evidence for several different mechanisms. On balance, our evidence is consistent with incumbents calculating that the programmatic policy would *decrease* support for them among beneficiaries and seeking to retain their support by sending more non-programmatic goods their way.

For comparative politics scholars, the main takeaway is that any study of the PISH that does not consider the possibility that incumbents are behaving in this manner may be inaccurately estimating the effects of their programmatic policy. We suggest that incumbents will have more leeway to adjust their electoral strategies when they have greater access to non-programmatic goods and can target those goods at policy beneficiaries with relatively high degrees of accuracy. In settings in which the programmatic policy is bestowed on individuals and the non-programmatic goods incumbents have access to are targetable at groups, the incumbent may not be able to adjust her allocation of non-programmatic goods. In contrast, in settings in which both the programmatic policy and the non-programmatic goods are bestowed on individuals (a setting that characterizes many developing democracies) or on groups (like Japan), incumbents will be freer to engage in this type of strategic behavior. Access to non-programmatic goods and/or the ability to target those goods effectively likely varies among incumbents from the same party at different levels of government. This could help explain why the same programmatic policy is found to have different "effects" on votes for incumbents at one level of government relative to another (Tobias, Sumarto and Moody, 2014; Zucco, 2013).

Going forward, we urge scholars interested in the effects of programmatic policies to consider the possibility that incumbents are engaging in this type of strategic behavior. This will involve

the selection of cases where it is possible to collect data to evaluate it. We also urge future research to delve much deeper into the conditions under which incumbents will decide to respond the way they did in Japan (by increasing the distribution of non-programmatic goods to policy beneficiaries), or in another way.

References

Araújo, Victor. 2021. "Do anti-poverty policies sway voters? Evidence from a meta-analysis of Conditional Cash Transfers." *Research & Politics* 8(1).

Asahi Shinbun. 2000. "Yureru Yuukensha: 2 (2000 Nen So Senkyo Niigata)." *June 17, 2000* .

Asahi Shinbun. 2001. "Kawaru Kachikan (Jiban Hyoryu 2001 Sanin Sen: 1)." *July 5, 2001* .

Bechtel, Michael M. and Jens Hainmueller. 2011. "How Lasting Is Voter Gratitude? An Analysis of the Short- and Long-Term Electoral Returns to Beneficial Policy." *American Journal of Political Science* 55(4):852–868.

Blattman, Christopher, Mathilde Emeriau and Nathan Fiala. 2018. "Do Anti-Poverty Programs Sway Voters? Experimental Evidence from Uganda." *The Review of Economics and Statistics* 100(5):891–905.

Brierley, Sarah and Noah L Nathan. 2021. "The Connections of Party Brokers: Which Brokers Do Parties Select?" *The Journal of Politics* 83(3):000–000.

Carey, John and Matthew Shugart. 1995. "Incentives to Cultivate a Personal Vote: A Rank Ordering of Electoral Formulas." *Electoral Studies* 14:417–39.

Catalinac, Amy. 2016. "From Pork to Policy: The Rise of Programmatic Campaigning in Japanese Elections." *Journal of Politics* 78(1).

Catalinac, Amy, Bruce Bueno de Mesquita and Alastair Smith. 2019. "A Tournament Theory of Pork Barrel Politics: The Case of Japan." *Comparative Political Studies* 0(0).

Catalinac, Amy and Lucia Motolinia. 2021. "Geographically-Targeted Spending in Mixed-Member Majoritarian Electoral Systems." *World Politics* 73(4).

Christensen, Ray and Joel Sawat Selway. 2017. "Pork-Barrel Politics and Electoral Reform: Explaining the Curious Differences in the Experiences of Thailand and Japan." *The Journal of Asian Studies* 76(2):283–310.

Conover, Emily, Roman A. Zarate, Adriana Camacho and Javier E. Baez. 2018. "Cash and Ballots: Conditional Transfers, Political Participation and Voting Behavior." *working paper* .

Correa, Diego Sanches and Jose Antonio Cheibub. 2016. "The Anti-Incumbent Effects of Conditional Cash Transfer Programs." *Latin American Politics and Society* 58(1):49–71.

Curtis, Gerald. 1971. *Election campaigning, Japanese style.* New York, NY: Columbia University Press.

Dahlberg, Matz and Eva Johansson. 2002. "On the vote-purchasing behavior of incumbent governments." *American Political Science Review* 96(1):27–40.

De Kadt, Daniel and Evan S Lieberman. 2020. "Nuanced accountability: Voter responses to service delivery in southern Africa." *British Journal of Political Science* 50(1):185–215.

De La O, Ana L. 2013. "Do Conditional Cash Transfers Affect Electoral Behavior? Evidence from a Randomized Experiment in Mexico." *American Journal of Political Science* 57(1):1–14.

De La, O, Ana Lorena et al. 2018. "Crafting policies to end poverty in Latin America." *Cambridge Books* .

Diaz-Cayeros, Alberto, Federico Estevez and Beatriz Magaloni. 2009. Welfare Benefits, Canvassing, and Campaign Handouts. In *Consolidating Mexico's Democracy*, ed. Jorge I. Dominguez, Chappel Lawson and Alejandro Moreno. Baltimore: Johns Hopkins Press.

Diaz-Cayeros, Alberto, Federico Estévez and Beatriz Magaloni. 2016. *The political logic of poverty relief: Electoral strategies and social policy in Mexico.* Cambridge University Press.

Dixit, Avinash and John Londregan. 1996. "The Determinants of Success of Special Interests in Redistributive Politics." *Journal of Politics* 58:1132–55.

Frey, Anderson. 2019. "Cash transfers, clientelism, and political enfranchisement: Evidence from Brazil." *Journal of Public Economics* 176:1–17.

Fukui, Haruhiro and Shigeko N. Fukai. 1996. "Pork Barrel Politics, Networks, and Local Economic Development in Contemporary Japan." *Asian Survey* 36(3):268–286.

Golden, Miriam and Brian Min. 2013. "Distributive Politics Around the World." *Annual Review of Political Science* 16(1):73–99.

Golden, Miriam, Eugenia Nazrullaeva and Stephane Wolton. 2021. "Politics in Poor Places? Clientelism and Elections in Democracies." *Working Paper* .

Golden, Miriam and Lucio Picci. 2008. "Pork-Barrel Politics in Postwar Italy, 1953-94." *American Journal of Political Science* 52(2):268–289.

Harris, J. Andrew and Daniel N. Posner. 2018. "(Under What Conditions) Do Politicians Reward Their Supporters? Evidence from Kenya's Constituencies Development Fund." *American Political Science Review* p. 1–17.

Hicken, Allen. 2011. "Clientelism." *Annual Review of Political Science* 14(1):289–310.

Hicken, Allen and Noah L. Nathan. 2020. "Clientelism's Red Herrings: Dead Ends and New Directions in the Study of Nonprogrammatic Politics." *Annual Review of Political Science* 23(1):277–294.

Hirano, Shigeo. 2006. "Electoral Institutions, Hometowns, and Favored Minorities: Evidence from Japanese Electoral Reforms." *World Politics* 59(1):51–82.

Horiuchi, Yusaku and Jun Saito. 2003. "Reapportionment and Redistribution: Consequences of Electoral Reform in Japan." *American Journal of Political Science* 47(4):669–682.

Horiuchi, Yusaku, Jun Saito and Kyohei Yamada. 2015. "Removing Boundaries, Losing Connections: Electoral Consequences of Local Government Reform in Japan." *Journal of East Asian Studies* 15(1):99–125.

Imai, Kosuke, Gary King and Carlos Velasco Rivera. 2019. "Do Nonpartisan Programmatic Policies Have Partisan Electoral Effects? Evidence from Two Large Scale Experiments." *Journal of Politics* 81(2).

Keele, Luke J and Rocío Titiunik. 2015. "Geographic Boundaries as Regression Discontinuities." *Political Analysis* 23(1):127–155.

Keele, Luke and Rocío Titiunik. 2016. "Natural Experiments Based on Geography." *Political Science Research and Methods* 4(1):65–95.

Kitschelt, Herbert and Steven Wilkinson, eds. 2007. *Patrons, Clients, and Policies.* New York: Cambridge University Press.

Kogan, Vladimir. 2021. "Do Welfare Benefits Pay Electoral Dividends? Evidence from the National Food Stamp Program Rollout." *The Journal of Politics* 83(1):58–70.

Krauss, Ellis S. and Robert J. Pekkanen. 2010. *The Rise and Fall of Japan's LDP. Political Party Organizations as Historical Institutions.* Ithaca, New York: Cornell University Press.

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

Larreguy, Horacio, John Marshall and Laura Trucco. N.D. "Breaking clientelism or rewarding incumbents? Evidence from an urban titling program in Mexico." Available at: https://scholar.harvard.edu/files/jmarshall/files/corett_v18.pdf.

Layton, Matthew L. and Amy Erica Smith. 2015. "Incorporating Marginal Citizens and Voters: The Conditional Electoral Effects of Targeted Social Assistance in Latin America." *Comparative Political Studies* 48(7):854–881.

Linos, Elizabeth. 2013. "Do Conditional Cash Transfers Shift Votes? Evidence from the Honduran PRAF." *Electoral Studies* 32(4):864–874.

Manacorda, Marco, Edward Miguel and Andrea Vigorito. 2011. "Government Transfers and Political Support." *American Economic Journal: Applied Economics* 3(3):1–28.

Martin, Shane. 2011. "Electoral institutions, the personal vote, and legislative organization." *Legislative Studies Quarterly* 36(3):339–361.

McCrary, Justin. 2008. "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test." *Journal of Econometrics* 142(2):698–714.

McMichael, Taylor C. 2018. "Electoral Strategy and Intergovernmental Transfers in Postwar Japan." *Asian Survey* 58(5):847–873.

Mizusaki, Tokifumi. 2014. *Sosenkyo Deta Besu. JED-M Deta.* Tokyo, Japan: LDB.

Mochida, Nobuki. 2001. Taxes and Transfers in Japan's Local Public Finance. In *Local Government Development in Post-War Japan*, ed. Michio Muramatsu, Ikuo Kume and Farrukh Iqbal. Oxford, United Kingdom: Oxford University Press p. 85–111.

Naoi, Megumi. 2015. *Building Legislative Coalitions for Free Trade in Asia: Globalization as Legislation.* Cambridge, UK: Cambridge University Press.

Pekkanen, Robert J. 2009. Japan's neighborhood associations. Membership without advocacy. In *Local Organizations and Urban Governance in East and Southeast Asia*, ed. Benjamin L. Read and Robert Pekkanen. London: Routledge chapter 2, pp. 27–57.

Pekkanen, Robert J, Yutaka Tsujinaka and Hidehiro Yamamoto. 2014. *Neighborhood Associations and Local Governance in Japan.* New York: Routledge.

Pop-Eleches, Cristian and Grigore Pop-Eleches. 2012. "Targeted Government Spending and Political Preferences." *Quarterly Journal of Political Science* 7(3):285–320.

Ramseyer, Mark and Frances Rosenbluth. 1993. *Japan's Political Marketplace.* Cambridge: Harvard University Press.

Saito, Jun. 2010. *Jiminto Choki Seiken no Seiji Keizaigaku.* Tokyo, Japan: Keiso Shobo.

Scheiner, Ethan. 2006. *Democracy Without Competition in Japan. Opposition Failure in a One-party Dominant State.* New York, NY: Cambridge University Press.

Spáč, Peter. 2021. "Pork barrel politics and electoral returns at the local level." *Public Choice* 188(3):479–501.

Stein, Robert M. and Kenneth N. Bickers. 1994. "Congressional Elections and the Pork Barrel." *The Journal of Politics* 56(2):377–399.

Stokes, Susan C. 2007. Is Vote Buying Undemocratic? In *Elections for Sale: The Causes and Consequences of Vote Buying*, ed. Frederic Charles Schaffer. Boulder, CO: Lynne Rienner chapter 6, pp. 117–45.

Stokes, Susan C., Thad Dunning, Marcelo Nazareno and Valeria Brusco. 2013. *Brokers, Voters, and Clientelism: The Puzzle of Distributive Politics.* New York NY: Cambridge University Press.

Tavitz, Margit. 2009. "Geographically targeted spending: exploring the electoral strategies of incumbent governments." *European Political Science Review* 1(1):103–123.

Tobias, Julia E., Sudarno Sumarto and Habib Moody. 2014. "Assessing the Political Impacts of a Conditional Cash Transfer: Evidence from a Randomized Policy Experiment in Indonesia." *The SMERU Research Institute* .

Weitz-Shapiro, Rebecca. 2014. *Curbing Clientelism in Argentina: Politics, Poverty, and Social Policy.* New York, NY: Cambridge University Press.

World Bank. 2014. "A Model from Mexico for the World.". Available at: `https://www.worldbank.org/en/news/feature/2014/11/19/un-modelo-de-mexico-para-el-mundo`.

Zucco, Cesar. 2013. "When Payouts Pay Off: Conditional Cash Transfers and Voting Behavior in Brazil 2002–10." *American Journal of Political Science* 57(4):810–822.

Online Appendix:
How Incumbent Politicians Respond to the Enactment of a Programmatic Policy: Evidence from Snow Subsidies

A   Additional Information About the Snow Act

- The Special Measures Act Concerning Countermeasures for Heavy Snowfall Areas (*Gosetsu Chitai Taisaku Tokubetsu Sochi Ho*) can be found at: https://elaws.e-gov.go.jp/search/elawsSearch/elaws_search/lsg0500/detail?lawId=337AC1000000073.

- The criteria for eligibility for the snow subsidy is stipulated in two ordinances: Cabinet Ordinance on the Criteria for the Designation of Heavy Snowfall Area (https://elaws.e-gov.go.jp/search/elawsSearch/elaws_search/lsg0500/detail?lawId=338CO0000000344) and Ministerial Ordinance on the Stipulation of Period and Facilities in Cabinet Ordinance on the Criteria for the Designation of Heavy Snowfall Area (https://elaws.e-gov.go.jp/search/elawsSearch/elaws_search/lsg0500/detail?lawId=338M50000002047).

- According to these ordinances, a municipality is eligible for the snow subsidy if it is located in "heavy-snowfall area" in which the height of accumulated snow over the post thirty years exceeded 5,000 cm per year, and if it satisfies either of the following conditions:

  1. More than two-thirds of the municipality's area is in a "heavy-snowfall" area.

  2. More than one-half of the municipality's area is in a "heavy-snowfall" area, and it is in a prefecture whose capital is in a "heavy-snowfall" area.

  3. Either the municipal government office (e.g., city hall), Class 1 and 2 national highways, or prefectural road/city road/national railway station in Article 56 of the Road Law is in a "heavy snowfall" area.

  4. More than one-half of the municipality's area is in a "heavy-snowfall" area, and more than two-thirds of the municipal border is in contact with municipalities that satisfy conditions 1, 2, or 3.

- The following table illustrates how one would calculate the height of accumulated snow over the first thirteen days of December for a hypothetical municipality:

December	Snowfall	Snow Height on the Ground	Height of Accumulated Snow
1st	0	0	0
2nd	1	1	1
3rd	2	2	3
4th	1	1	4
5th	24	24	28
6th	0	22	50
7th	0	17	67
8th	22	36	103
9th	0	34	137
10th	2	27	164
11th	0	27	191
12th	2	19	210
13th	0	19	229

- Prior to 2001, the Snow Act stated that the Prime Minister is responsible for the designation of a "heavy snowfall" area. After 2001, it stipulates that the Ministries of Land, Infrastructure, and Transport, Internal Affairs and Communications, Agriculture, Forestry, and Fisheries, and the National Land Development Council are responsible.

- The Snow Act was introduced by a non-partisan coalition of politicians. The affiliations of the signatories include the LDP (75), Japan Socialist Party (25), and Democratic Socialist Party (1). Not all signatories hailed from districts containing municipalities that would eventually fit the criteria for designation.

B Descriptive Statistics

Table B.1: Descriptive Statistics of Municipalities in Mixed Districts

Statistic	N	Mean	St. Dev.	Min	Max
Post-Election Per Capita Transfers (log)	6,578	−3.737	0.698	−5.814	0.021
Snow Subsidy	6,882	0.286	0.452	0	1
Winning LDP Vote Share	6,882	0.395	0.184	0.000	0.909
Fiscal Power	6,807	0.389	0.229	0.000	1.990
Proportion Dependent	6,543	0.367	0.040	0.182	0.585
Proportion in Agriculture	6,543	0.098	0.066	0.001	0.450
Population (log)	6,882	9.240	1.128	6.047	13.638
Income Per Capita (log)	6,807	−0.170	0.355	−1.538	0.602
Population Density (log)	6,882	4.923	1.330	0.265	9.069
Area Size (log)	6,882	4.311	0.899	0.962	7.688
Altitude (log)	6,864	5.333	1.445	−0.916	7.569

As the main paper explains, the data used in this study are based on Catalinac, Bueno de Mesquita and Smith (2019), JED-M (Mizusaki, 2014), Nikkei NEEDs (http://www.nikkei.co.jp/needs/contents/regional.html), the National Land Numerical Information Service (http://nlftp.mlit.go.jp/ksj-e/index.html) and the Geospatial Information Authority of Japan (http://www.gsi.go.jp/ENGLISH/index.html).

C Covariate Balance Between Heavy Snowfall and Non-Heavy Snowfall Municipalities

Figure C.1: Investigating Discontinuities in Covariate Characteristics Between Heavy Snowfall and Non-Heavy Snowfall Municipalities Proximate to the Border in the Same District
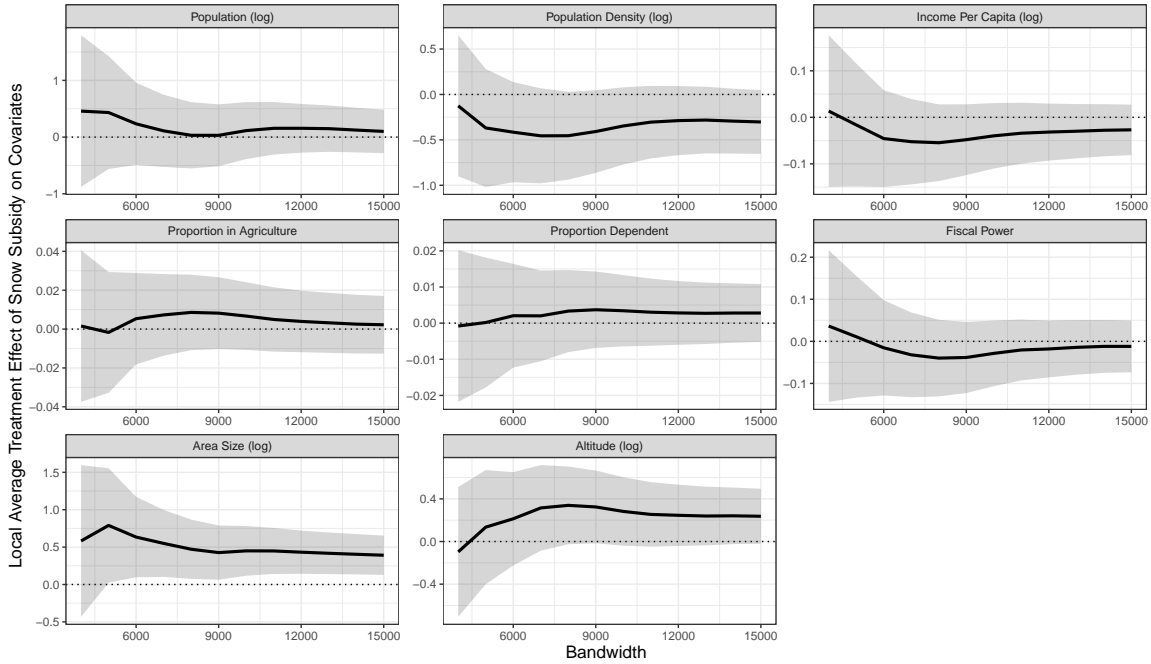
To examine whether there are any discontinuities (sudden changes) in covariate characteristics at the border, we estimate the same local linear regression with triangular kernel weights described in Equation 1 in the paper on eight municipality-level attributes that may influence LDP Vote Share or NTD. Population, population density, income per capita, proportion of the population employed in agriculture, proportion of the population who are dependent, and fiscal power are standard controls in work on transfers. Area size and the altitude of the municipality's centroid are geographical features of a municipality that may also influence transfers. We use the same range of bandwidths used in the paper's Figures 2 and 3. In all specifications, we include district-year fixed effects. Figure C.1 summarizes the results for each covariate. The y-axis displays the coefficients on Snow Subsidy and their corresponding 95% confidence intervals. If the confidence intervals include 0, there is no evidence for a discontinuity in characteristics of the municipalities just left and right of the border. Figure C.1 shows that the snow subsidy does not have a significant effect on any covariate except for area size (left panel in the third row). Importantly, there is no discontinuity in any of the variables thought to influence transfers, such as income per capita, fiscal strength, or proportion of the population who are dependent.

4

D   Investigating Self-Sorting

Figure D.1: Results of a McCrary Sorting Test
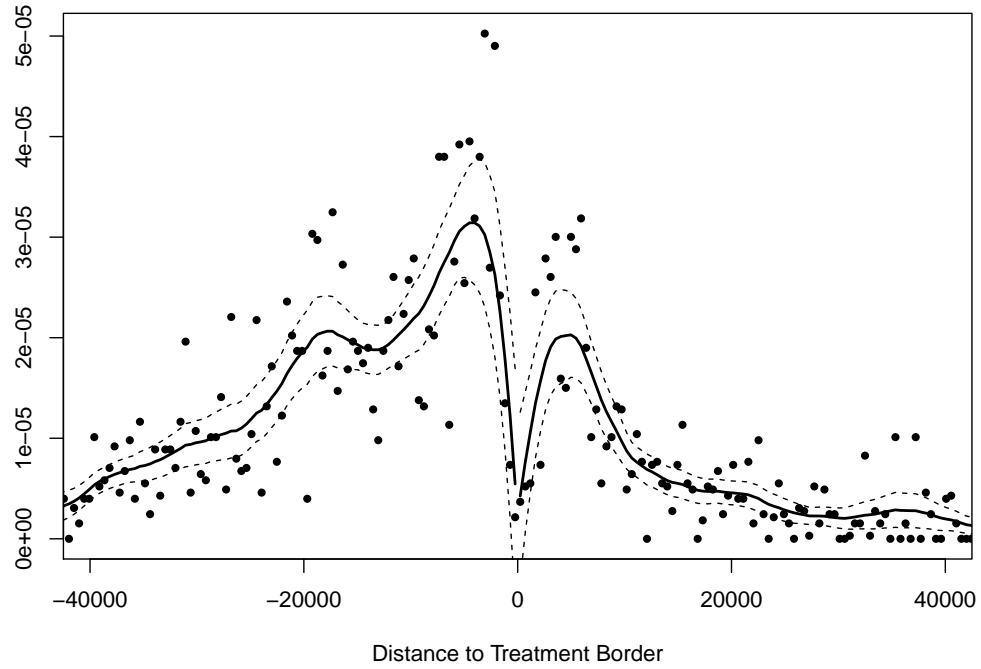
The results of the McCrary (2008) sorting test in Figure D.1 indicate that there is little evidence of self-sorting. This is unsurprising given than a municipality's eligibility for the snow subsidy is determined by the objective criteria laid out in the main paper.

E   Alternative Measure of Electoral Support for the LDP

Figure E.1: Receiving the snow subsidy results in no statistically-significant difference in Winning LDP Vote Share for municipalities in mixed districts, 1980-2005.
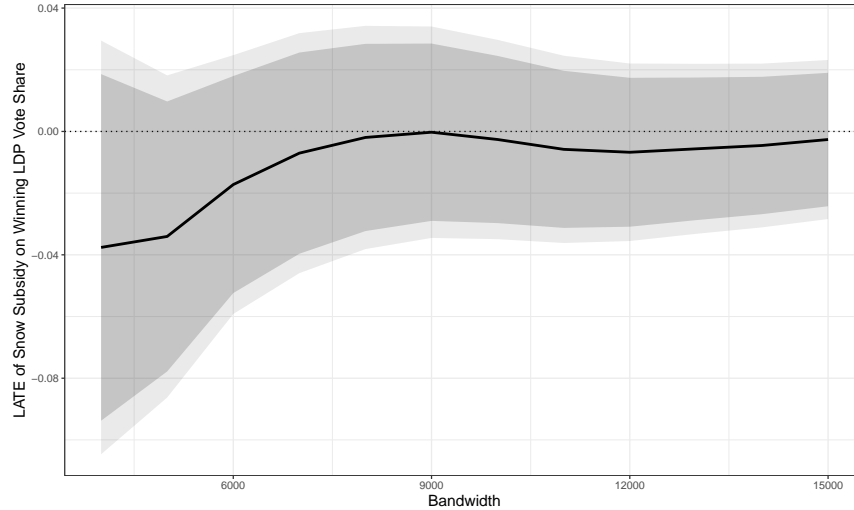
Note: This figure depicts the coefficient estimates on Snow Subsidy obtained from local linear regressions of Winning LDP Vote Share on beneficiary status when the bandwidth is changed from $\pm 4,000$ to $\pm 15,000$. Shaded areas indicate 90%/95% confidence intervals.

To reinforce our finding that the snow subsidy has a null effect on incumbent support, in this section we use an alternative measure of LDP support, Winning LDP Vote Share. This operationalization is proposed by Catalinac, Bueno de Mesquita and Smith (2019) and measured as the proportion of a municipality's voting population who voted for the district's LDP winner(s) in each election. Hence, this measure is different from LDP Vote Share, used in the main text, in two ways. First, the denominator is not the total number of votes cast but the total number of voters in the municipality. This means that Winning LDP Vote Share incorporates differences in turnout across municipalities. Second, while in LDP Vote Share, the numerator is the overall support for the LDP, in Winning LDP Vote Share, it is restricted to the total number of votes for LDP candidates who actually won the election. Districts could have more than one winner prior to 1994, but only one winner after 1994.

Figure E.1 reports the LATE of Snow Subsidy on Winning LDP Vote Share. The results are similar to those in Figure 2. There is no statistically significant difference in Winning LDP Vote Share between otherwise-similar, same-district beneficiary and non-beneficiary municipalities. This is further evidence that the snow subsidy does not boost electoral support for the LDP.

F Regressions with All Municipalities in Mixed Districts

Table F.1: Controlling for other differences between municipalities, beneficiary municipalities do not deliver higher levels of electoral support for their LDP incumbents than non-beneficiary municipalities in the same district (Model 1 and 2). Beneficiary municipalities do, however, receive larger per capita NTD allocations after elections than their same-district non-beneficiary counterparts.

	(1)	(2)	(3)
	LDP Vote Share	Winning LDP Vote Share	Post-Election Per Capita Transfers (log)
Snow Subsidy	−0.001	0.010	0.087*
	(0.006)	(0.006)	(0.035)
Fiscal Power	−0.004	−0.019	0.352*
	(0.020)	(0.016)	(0.100)
Proportion Dependent	0.004	0.093	1.316*
	(0.063)	(0.062)	(0.468)
Proportion in Agriculture	0.058	0.021	−0.229
	(0.055)	(0.049)	(0.347)
Population (log)	0.015	−0.002	−0.074
	(0.010)	(0.008)	(0.169)
Income Per Capita (log)	−0.081*	−0.059*	−0.187
	(0.020)	(0.018)	(0.161)
Population Density (log)	−0.045*	−0.036*	−0.098
	(0.010)	(0.008)	(0.171)
Area Size (log)	−0.025*	−0.012	0.113
	(0.010)	(0.008)	(0.169)
Altitude (log)	−0.006*	−0.008*	−0.009
	(0.003)	(0.002)	(0.017)
District-Year Fixed Effects	Yes	Yes	Yes
N	6,452	6,452	6,446
R^2	0.721	0.800	0.344

Note: *p<0.05. Robust standard errors clustered on the municipality.

In Table F.1, we present the results of fixed effect regressions that use all municipalities in mixed districts, 1980-2005. These models include, in addition to district-year fixed effects, time-varying municipality-level controls. Standard errors are clustered on the municipality.

Overall, widening the sample to include all observations in mixed districts (not just the ones immediately proximate to the border) and controlling for other differences between municipalities produce results similar to the GRD. Specifically, Snow Subsidy is not a significant predictor of LDP Vote Share nor Winning LDP Vote Share (models 1 and 2). By contrast, its effect on Post-Election Per Capita Transfers is positive and significant (model 3). While we prefer the GRD for cleaner causal identification, our main findings are robust to alternative research

designs.

G Do Beneficiary Municipalities Have Different Relationships with Local Government?

The questions we use in the analysis in Table 3 of the main text are as follows (response items are reordered so that higher values indicate greater influence on local government):

1. Does your NHA monitor the local government's policy implementation? Monitoring refers to regular examination and observation of policies (Q26).

   - Always, regardless of relevance
   - Only when the policy is relevant to our NHA
   - Never

2. How often does your NHA use the following methods to reflect the NHA's interests and opinions in policies? Consult the corresponding section in local government (Q27A).

   - Often
   - Regularly
   - Sometimes
   - Seldom
   - Never

3. How often does your NHA use the following methods to reflect the NHA's interests and opinions in policies? Consult senior officials in local government (Q27B).

   - Often
   - Regularly
   - Sometimes
   - Seldom
   - Never

4. How often does your NHA use the following methods to reflect the NHA's interests and opinions in policies? Consult local politicians (Q27C).

   - Often
   - Regularly
   - Sometimes

- Seldom

- Never

5. How often does your NHA use the following methods to reflect the NHA's interests and opinions in policies? Attend local government-sponsored round-table conferences (Q27F).

    - Often

    - Regularly

    - Sometimes

    - Seldom

    - Never

6. How often does your NHA use the following methods to reflect the NHA's interests and opinions in policies? Lobby the local assembly (Q27G).

    - Often

    - Regularly

    - Sometimes

    - Seldom

    - Never

7. How much do you think NHAs influence local government's decision making? (Q30).

    - Influential

    - Some influence

    - Moderate

    - Not very much

    - Not at all