Who Captures the Windfall? Seat Allocation and Home Bias in Benefit-Sharing Boards

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Abstract

Are directors who govern natural resources biased towards their home districts? The literature predicts that they will be but we argue that mixed-territorial seat allocation can attenuate directors' incentives for home bias. When a regional director represents one community among several directors competing for a fixed budget, advocating for one's own community is constrained and directors are dissuaded from rent-seeking. We test our theory using the Columbia Basin Trust, a natural resource fund that distributes compensation for hydropower development across southeastern British Columbia. Using synthetic control and a difference-in-differences design that exploits a 2003 change to the composition of the Trust's board, we find positive developmental returns and no evidence of home district bias and this holds whether that director holds an official regional appointment or a provincial appointment without an assigned territory. Our findings suggest that board-level institutional design is consequential for successful development and redistribution.

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Thank you to Sergio Ascencio, Anna Getmansky, John Janmaat, Jamye Jones, Nikolay Marinov, Laura Montanaro, Johanna Rickne, Jake Ricks, Christopher Schulz, Jon Slapin, Martin Steinwand, Katy Stigers, Joanne Taylor and John Wagner for comments and questions. The authors used a large language model (Anthropic's Claude) as a coding and research-assistance tool, specifically to scrape the Columbia Basin Trust grants database and to build the board-level analysis panels.

1 Introduction

Large energy projects, including hydropower, wind, and nuclear, are now expected to not only power the grid but also to *deliver* and *develop* the communities that host them. In the United Kingdom, Sizewell C carries a £23 million community fund for East Suffolk, a county where deprivation in education, housing, and access to services is concentrated in rural areas (Suffolk Community Foundation, 2023). In Sweden, the government has set aside over SEK 1 billion for municipalities that approve onshore wind, compensating them for the burden of hosting the infrastructure (Pourmokhtari, 2023). Paying communities to accept a project is one thing but governing the allocation of such awards is another. Recently, governments and international organisations have promoted *benefit-sharing mechanisms* (BSM), with the aim of turning disruptive infrastructure into sources of regional development (World Commission on Dams, 2000; Schulz and Skinner, 2022). These mechanisms dedicate non-tax, windfall revenues to affected regions and often place them inside participatory local institutions (Lockwood, 2004; Mansuri and Rao, 2012). Because the revenue does not come from taxation, standard political economy theories predict that politicians will try to capture and misdirect the windfalls (Nikolova and Marinov, 2017; Ahmed, 2012).

Importantly, not all benefit-sharing institutions operate the same way and BSM redistribute benefits by different routes. One useful way to think about BSM is by comparing and contrasting how much they contribute to development and how much they contribute to redistribution. For example, Shetland shows what the distributional problem looks like when it goes wrong. The oil payments made to compensate the islands for the Sullom Voe terminal were placed in the Shetland Charitable Trust, a body whose board was for years dominated by the very councillors with local stakes in how the money was spent. Unfortunately, however, the Scottish charity regulator found systemic conflicts of interest and forced a restructure to dilute that control (Office of the Scottish Charity Regulator, 2009). Alaska's Permanent Fund Dividend (PFD), meanwhile, avoids political capture by paying oil revenues to residents as an equal cash sum. Because the formula is rule-based, this leaves no board to steward redistribution but also no discretionary spending for development goals. While payments are equal, local residents have no means to direct funds into their region's economic development and indeed, research shows that while the PFD has raised political engagement (Loeffler, 2023) economic returns are more limited (Goldsmith, 2001, 2012). Alternatively, Norway distributes oil and hydro resources using a completely different BSM. In this case, boards are bypassed and municipalities receive large hydropower revenues with mayors holding wide discretion over them. **?** find that the political rent-seeking from these windfalls are non-existent, which the authors attribute to electoral accountability mechanisms and an informed public that monitors spending. Unlike in Scotland and in Alaska, in Norway, both development and distribution occurs because the funds are treated as if they are earned tax-revenues, even though they are not.

Despite the differences in BSM, success implies that politicians are constrained from misusing or misplacing funds: while Shetland's board failed at this, Alaska removes discretion altogether by rule-based distribution whereas Norway relies on elections and an attentive electorate. In this paper,

we explore whether and under what conditions BSM boards can restrain their own decision-makers internally. We focus on one such institutional design feature – how board of director seats map onto geographical representation of the communities they serve. We argue that a system of mixed-territorial seat allocation (MTSA), or where one director holds a seat per competing community yet sits alongside appointees with no district representative function, delivers accountability previously only associated with external accountability. In particular, we argue that by allocating seats one per competing community, with upper-level appointees that hold no local stake in tandem, such a board structure is enough to hold directors in check and without the need for additional external oversight.

We test our argument using the case of the Columbia Basin Trust (CBT), a benefit-sharing mechanism created in British Columbia (B.C.) in 1995 to compensate B.C. communities for the reservoirs built under the Columbia River Treaty, signed between Canada and the United States. The CBT board holds five regional seats, each nominated by a local regional district and additionally a representative from the Ktunaxa Nation, a First Nation. These are place-based appointments. In addition, the CBT board has six other board members that are appointed by the provincial authority and who do not represent a specific geographical territory but must also still live in the greater region. We estimate the CBT's aggregate effect on regional economic development using the synthetic control method (Abadie and Gardeazabal, 2003; Abadie, 2021). Using both a difference-in-difference and a two-way fixed-effects model of grant allocation, we also test whether or not board directors exhibit home bias in grant allocation.

We find a small but positive cumulative effect of the CBT (hereafter Trust) on regional economic development, estimated as about 456 additional business registrations between 1995 and 2019, or roughly one extra year of regional growth over a decade. We read this as a directional (i.e. the results are positive) rather than a sharp estimate, given the few comparison regions available for comparison. But even so our findings are the opposite of what the literature on the sub-national resource curse predicts. Interestingly, we also find little evidence of home bias. A district whose nominating principal holds an active seat receives no larger share of community grants, and a district that happens to have a provincially appointed resident also receives no additional funding. Using a 2003 legislated change in the board size of the Trust, which stripped regional seats from different districts at different times, we also find that the districts that lost territorial representation lost no grant funding, which is again evidence inconsistent with home bias.

The CBT shows that discretion need not bring home bias, provided that seats are allocated one per competing community and geographical representatives are made to govern with non-territorial seat holding members. It also shows that benefit sharing mechanisms can encourage economic development, but only under certain conditions. For the growing set of appointed benefit-sharing bodies and boards now being built alongside renewable energy projects, our findings offer encouragement that mixed-territorial seat allocation is a low-cost and implementable board design associated with positive developmental and distributive consequences.

2 Benefit Sharing, Windfalls, and the Limits of Standard Remedies

2.1 The Resource Curse at the Sub-national Level

The resource curse literature has long shown that windfall revenues from natural resource extraction are associated with rent-seeking, misallocation, and weak developmental returns, both at the national level (Ross, 2001; Sachs and Warner, 1995) and at the sub-national level as well (Nikolova and Marinov, 2017; Havranek et al., 2016).

The reason for why runs through the fiscal contract between citizens and the government. When citizens pay taxes, they have a direct incentive to monitor how that money is spent and to punish officials who spend it badly. This monitoring incentive is the foundation of fiscal accountability (Ahmed, 2012; Bates, 1981). Windfall revenues, depending on how they are redistributed (if at all) can break this link. When governments receive money from oil, hydropower, or other resource rents rather than from taxation, they may not justify how or to whom they spend it on. Similarly, citizens have fewer reasons to monitor, less information on what is being spent, and weaker demands for accountability (Ross, 2001). The consequence is that windfall revenues are therefore systematically more likely to be captured, wasted, or misallocated than equivalent revenues raised and redistributed through taxation.

This problem is even more severe at the sub-national level for two reasons. First, sub-national governments typically face weaker means of accountability than national governments. This includes fewer independent auditors, less media scrutiny, fewer resources for organized citizen monitoring, and less participation in local politics (Flavin and Franko, 2017, 2020) Furthermore, whether shifting control downward to the sub national level improves accountability or simply relocates capture depends on local level conditions (Treisman, 2007). Second, communities that host large energy projects are often located in rural, remote, and economically dependent areas. These areas are precisely the types of territories least equipped in auditing, monitoring, and disciplining officials and controlling windfall spending. Nikolova and Marinov (2017) document this directly in Bulgaria. Bulgarian municipal politicians systematically misappropriate windfall disaster relief funds and the authors found that the effects are largest in locations with weaker civic capacity. Similarly, Havranek et al. (2016) show that the negative developmental effects of windfall revenues are significantly larger in settings with weaker institutional quality, which is the modal setting for energy infrastructure and benefit-sharing, especially in rural areas.

The second problem is distributional. Even when windfall revenues are invested rather than consumed, it is likely that funds move to politically connected places rather than to those communities most in need. When or if funds systematically find their way to the same district as their own local representative, this is called home bias. Fenno (1978) shows that representatives cultivate relationships with their home constituencies so as to claim credit for resources directed to them. Mayhew (1974) argues that the incentive structure of representative democracy makes this almost inevitable: legislators engage in credit-claiming by linking public expenditures to their own actions, building local visibility that sustains electoral careers. Ferejohn (1974) and Shepsle and Weingast

(1981) formalise the institutional conditions under which distributive coalitions form and sustain themselves in legislatures. More recently, scholars survey this pattern across many settings (Golden and Min, 2013) and find importance in who incumbents target (Cox and McCubbins, 1986; Dixit and Londregan, 1996; Stokes, 2005). Irrespective of how, a local representative is expected to make their own district better off.

Critically, home bias is not unique to elected officials and appointed board members often exhibit the same behaviours when their positions are linked to territorial interests. A board member nominated to represent a geographical area knows that their continuation depends on being seen to deliver for that district. This creates an informal accountability relationship that operates through reputation and ties to nominating principals rather than through elections. For example, in the case of the Federal Reserve System, the system of the US central bank, Bank Presidents are shown to represent their district even though they are not directly elected by citizens living in their districts (Meade and Sheets, 2005).

As mentioned in the introduction, Shetland shows that even in the context of the Global North, boards are interested in sending benefits home. To date, the literature highlights two remedies for home bias in the context of natural resources – electoral accountability, as per the case of Norway, and rules-based allocation, as per the case of Alaska. In Norway, the logic is that when voters can observe how windfall revenues are spent and punish officials who misallocate them, electoral accountability can discipline home bias. Andersen and Sørensen (2022) show "zero-rent seeking" behaviour. This is because citizens treat windfalls as if they are tax revenues, which are therefore "earned", even though they are not. The same community receiving the resource windfall is the same community that elects the mayor so the fiscal link between voters and politicians is preserved. While this is interesting and indeed a clever institutional design, most BSM lack a direct electoral link between voters and representatives.

In Alaska by contrast, rule-based distribution addresses home bias by removing discretion altogether and therefore making it impossible for representatives to funnel funds home. The PFD distributes oil revenues as an equal per-capita cash payment to every resident by statutory formula. Unfortunately, however, this same formula prevents economic development by way of targeting funds to go where it is most needed. As a cash-injection, the scheme works but it does not fund the business loans, community infrastructure, or local investment that converts a windfall into lasting economic growth; the developmental returns from the PFD are consequently limited (Goldsmith, 2001, 2012).

Both of these two remedies therefore restrain political capture yet each requires conditions that most resource-dependent communities do not necessarily have such as electoral links, an attentive electorate and developed local media, and / or rule-based mandated mechanisms for distribution. Many BSM are governed neither by a binding formula nor an elected mayor but are instead run by trusts and appointed boards, like in Shetland, have discretion over how the money is spent. An important question, therefore, is whether there are conditions which can restrain capture in these types of board arrangements? The next section presents one institutional design that we think

presents an opportunity.

2.2 Mixed-Territorial Seat Allocation

Rather than look outside the board, we argue that the board's own internal structure can help to constrain political capture and limit home bias. We outline one type of board configuration that can do this, which we call *mixed-territorial seat allocation* (MTSA). In MTSA, board seats are allocated with two distinct appointment mechanisms. One set of board members are territory based and these members and their communities compete for the board's discretionary budget. Next, there are additional appointed board members with no territorial stake other than to the larger district as a whole. For example, if a BSM is split into six geographical districts, and if there are six geographically appointed board members each with a seat and a corresponding territory, this set-up is one seat per community. Alongside these geographically-anchored seats, there are another six terriroty wide appointees who have no community principal. The defining institutional feature of MTSA therefore is not simply that the board draws members from different territories but rather that no two community-facing directors share a principal. Each territorial seat represents exactly a single community, and that community's gain in grants is every other community's loss.

MTSA is important because it induces rivary among community-based directors. The logic of MTSA is clearest when set against the legislative bargaining literature of Baron and Ferejohn (1989). In the legislative literature, building a coalition is straightforward whereas under MTSA it is much more difficult. In legislative politics, a proposer assembles a minimum winning coalition by offering side payments to some members while excluding some others. A coalition is affordable to build because partners can be recruited at relatively low cost and the excluded have little to bargain with. Coalition building is even easier when the budget can grow as the above logic produces logrolling, as members trade support for one another's projects (Weingast et al., 1981). A benefit-sharing board inverts this logic. In a benefit-sharing board, the budget is fixed, so there is no larger pie to divide and no side payment to offer a partner. A director who wants to steer grants must try to recruit a majority of the other community directors, but each of them loses from the deal because the pot is fixed and each member wants to steer funds to their own district.

Having additional, no-regional stake appointees makes coalition formation even more difficult. Having no community to reward, non-territorial stake board members have no reason to join any redirection scheme, so even if a fully coordinated bloc of community directors form, which is unlikely, they cannot alone carry a majority. These two features work in sequence such that rivalry among the community seats makes a home-directed coalition more costly to build. Meanwhile, the non-territory holding appointees make it impossible to carry any coalition forward should one form.

Even if a board decides largely by consensus rather than by voting, a similar constraint operates on how hard each community director is willing to push for their own district at the expense of others. Pushing more is likely to yield little since the gain is bounded by a fixed pot and rival directorsare likely to resist. Pushing hard is also likely to carry a reputational cost before no-stake colleagues who are, by definition, likely to gain nothing from home bias favouritism. Thus it is

not marjority voting that governs MTSA but rivarly and an expanded decision-body that has no territorial interest.

The above considers the static case, but what about the introduction of over-time strategies? Does this still predict difficultly in coalition formation? Logrolling, or when members support another community's claim this year for the same or similar favour next year could revive a coalition that a single allocation rules out. But a tit-for-tat bargain also needs partners who can be held to the deal over time. For the same reasons as above, the structure of MTSA makes over-time deal making difficult. Because the community directors each lose from any one year's deal they continue to be rivals in allocation rather than repeat partners over time. Furthermore, as the no-stake appointees gain nothing in any year and so cannot serve as the standing members in such a coalition, it is unlikely to work over time. These internal incentives coupled with board turnover and no explicit commitment mechanism means that the dynamics that the logroll literature describes is unlikely to develop.

Importantly, MTSA keeps what the external remedies of Alaska and Norway offer, but do so internally rather than externally. Under electoral accountability, the board keeps full discretion to invest in local development. In order to maintain its accountability to citizens and not go the way of Shetland, the electorate must monitor the board. Unlike rule-based distribution in Alaska, MTSA also does not surrender the discretion that targeted development depends on and so it does not have to trade off development for redistribution. MTSA also does not require that directors be altruistic, or that their conduct be observable to voters or community members, though the effects of having these things on a board are fruitful areas for further study.

Importantly, the argument rests on three scope conditions, which we set out here and return to in the empirical strategy. Only the second admits a direct test; the first and third we secure through the research design and the construction of our measures rather than test on their own. First, the grant budget must be fixed within an allocation cycle. If board-members can expand total spending in a single sitting to satisfy every territory at once, the zero-sum constraint disappears and the rivary among members relaxes, which is the main constraint. Second, no community-facing seats may share a principal such that coalitions form. If members answer to one community they are not rivals but potential colluders, so they can coordinate and a coalition would likely form among the community seats. At it's simplest, MTSA requires that every community director be a rival to every other. Third, community-specific grants must be a meaningful share of spending. If all money flows through programmes distributed equally across regions by either equal share or another set formula then there is no targetable budget to compete over and the rivalry the mechanism depends on also never arises; in the extreme, this becomes a rule based rather than a discretionary based board and it would therefore be closer to the Alaska case, though the boundaries of this we leave for future research. What the mechanism does not require, however, is that grants be equal across communities, that directors be indifferent to their home places, or that any election, past or future, discipline them. The key point is that territory-based board members cannot assemble a winning coalition: either the other community directors refuse to join, because making deals cost

them their own share, or, if some do join, the non-territorial appointees tip the balance against them. The appointees and the appointment process matters here precisely because non-territorial appointees are indifferent to bringing home benefits, which makes them difficult to buy and outvote if they are large enough in number. In the empirical section below we consider whether someone's personal place residence matters in addition to territorial appointment, which is the softest version of territorial-representation we can think of. Future research may also want to consider birth place, familial, social, or employement connections.

2.3 Hypotheses

We test the theory in two stages, following the structure of the argument. The first stage asks whether MTSA delivers any developmental return at all, or whether, as the sub-national resource-curse literature predicts, development stalls or even declines. The second asks how MTSA distributes those benefits, and whether that distribution conforms to the logic of home bias or runs against it.

We recognise that evidence against a competing theory is not the same as evidence for our own account, and that on home bias a null result would rule out large bias rather than any bias. We therefore do not rest the argument on any single test. Because MTSA makes opposing predictions at each stage, results that fall across several independent tests count as suggestive support.

Hypothesis 1 (Development return). Let τ be the effect of a benefit sharing mechanism on regional economic development. The sub-national resource curse predicts that a windfall managed by an unelected, discretionary board delivers no positive returns, $\tau \leq 0$. Alternatively, if the seat allocation rule lets the board invest rather than consume it, $\tau > 0$.

Hypothesis 2 (No home bias). Distributive politics predicts that a district captures more of the budget when it has someone of its own sitting at the table. The seat allocation rule predicts it does not, because no single nominating principal can assemble a working majority for redirection. We separate two versions of "someone of its own", because they identify different mechanisms. Let β_{reg} be the effect on district i's community grants of district i having an active *regional* nominating director (reg). Alternatively, let β_{prov} be the effect of the district hosting a *higher-level* appointed director, with no district stake (prov). Distributive politics predicts $\beta_{\text{reg}} > 0$ and $\beta_{\text{prov}} > 0$; MTSA predicts $\beta_{\text{reg}} = 0$ and $\beta_{\text{prov}} = 0$. Comparing across the two β's tells us whether home bias runs through the appointment mechanism or through residence or through neither.

Hypothesis 3 (Socialisation). A further prediction concerns socialisation. Assume that a board has a anti-home-district bias. If directors are drawn towards such a norm then the longer they serve, the more likely it is that they would behave this way. If this is true then tenure-length should pull a director's home-district allocation towards an equal share rather than away from it. Alternatively, length of tenure may be positively associated with bringing home benefits. Let δ be the effect of a director's board tenure on home-district grant flow. The socialisation account predicts $\delta \leq 0$; a capture account, in which seniority brings the standing to deliver, predicts $\delta > 0$.

The next section outlines the case and the empirical strategy used to test these hypotheses.

3 The Columbia Basin Trust and Its Developmental Effect

The Columbia River Treaty, signed in 1961 between Canada and the United States, governs flood control and hydroelectricity on the Columbia River system, which is a system that acts as the giant watershed of the Canadian Rocky Mountains. The construction of three large storage reservoirs (Mica, Keenleyside, and Duncan) flooded agricultural land, displaced communities, and eliminated the natural flood regime that had supported rural economies and First Nations communities on the Canadian side of the Basin for thousands of years (Know History Inc., 2020). The benefits of flood control and cheap hydroelectricity flowed primarily to American ratepayers and the British Columbia treasury (Wilson, 1973). The Canadian Basin communities (hereafter Basin) that bore the costs received nothing in return as per the original agreement (Participant 4, 2024).

For three decades these communities lobbied the province of B.C. for compensation yet won little advancement (Participant 2, 2024; Participant 4, 2024). The compensation claim by local residents had no effect on provincial representatives until the 1991 provincial election, which returned New Democrats representatives in all four Basin seats, one of them a cabinet minister, and brought into government the very politicians who would design the Trust (Participant 4, 2024). In 1995, a deal was reached and the Trust received a $295 million endowment (Know History Inc., 2020). Of this, $250 million was committed to regional power projects, whose returns flow back to the region, and $45 million was earmarked for community grants controlled directly by the board (Know History Inc., 2020).

A clarification about what kind of redistribution is at stake. The CBT does not redistribute from rich individuals to poor ones living in the Basin in the classical sense (Rueda and Stegmueller, 2019; Dimick et al., 2016). Instead, the CBT redistributes resource rents within the rural communities that bore the costs of the original Columbia River Treaty through project spending. This is a place-based, externality-driven claim, and the electoral mechanisms that can deliver income redistribution where preferences align cannot straightforwardly deliver territorial redistribution when the political unit is dominated by communities that bore none of the costs (Beramendi, 2012; Rodden, 2006). Indeed, the Basin offers researchers an classic situation where placed-based identity and rural resentments are ripe (Cramer, 2016). It is therefore interesting though not deterministic that place-based solutions are tried (Ostrom, 1990; Abers and Keck, 2013)

For the empirical strategy, it is the $250 million injection in hydropower projects that we consider in order to test Hypotheses 1. Alternatively, it is the $45 million and growing community grants that constitutes the discretionary funds we examine for testing Hypotheses 2 and 3.

3.1 Synthetic Control Design

To test Hypothesis 1, we use the synthetic control method to construct a counterfactual trajectory for the Kootenay development region (Abadie and Gardeazabal, 2003; Abadie, 2021). The

method builds a weighted combination of untreated units that best matches the treated unit on pre-treatment covariates and outcome trajectories. Post-treatment divergence between the treated unit and its synthetic counterpart is interpreted as the causal effect of the treatment. The method is well suited to settings with a single treated unit and an infrequent treatment event, both of which apply here.

Development regions are regional entities used for statistical analysis in Canada, grouping whole census divisions. British Columbia has eight development regions. The Kootenay development region almost entirely overlaps with the Basin area, encompassing the Regional Districts of Kootenay Boundary, Central Kootenay, and East Kootenay. Some Basin communities, including Revelstoke, Golden, and parts of the Columbia Shuswap fall outside the Kootenay development region and are omitted from the outcome measure. This means that by using development regions, we likely underestimate rather than overestimate the CBT's developmental effect. We treat the $250 million endowment as a one-time windfall shock, ignoring the $2 million in annual BC Hydro grants paid between 1990 and 1995, interest earnings, and returns on CBT investments. This further understates the total resource injection into the Basin over the study period.

Historical data for B.C. development regions are patchy prior to 1990. We address this historical missingness using multiple imputation with Amelia (Honaker et al., 2011), creating 1,000 imputed datasets and running the synthetic control on each imputed dataset. We present the full distribution of gaps between treated and control rather than offering a single point estimate. Doing so means that we attempt to show both imputation as well as estimation uncertainty. Our time period is $T = 1986–2019$ and the treatment event occurs at $T_0 = 1995$:

$$\hat{Y}^N_{1t,k} = \sum_{j=2}^{J} \omega_j Y_{jt,k}$$

$$\tau_{1t,k} = Y_{1t,k} - \hat{Y}^N_{1t,k}$$

where $\hat{Y}^N_{1t,k}$ is the counterfactual from J weighted donor units on imputed dataset k and $\tau_{1t,k}$ is the estimated causal effect. Because the outcome region excludes several communities the Trust also served, and because we treat the endowment conservatively as a single 1995 shock rather than crediting its later returns, the estimate is more likely to understate than to overstate the Trust's developmental effect.

3.2 Data and Measures

Our main outcome is local economic development. Statistics Canada does not release sub-provincial GDP data. As a proxy, we measure economic development using the number of business incorporation registrations per development region over time. British Columbia Statistics publishes annual incorporation counts by development region from 1990 onwards. As a robustness check, Figure 1 shows the correlation between incorporations and annual building permit values across the same development regions in B.C.. There is a strong positive association between the two

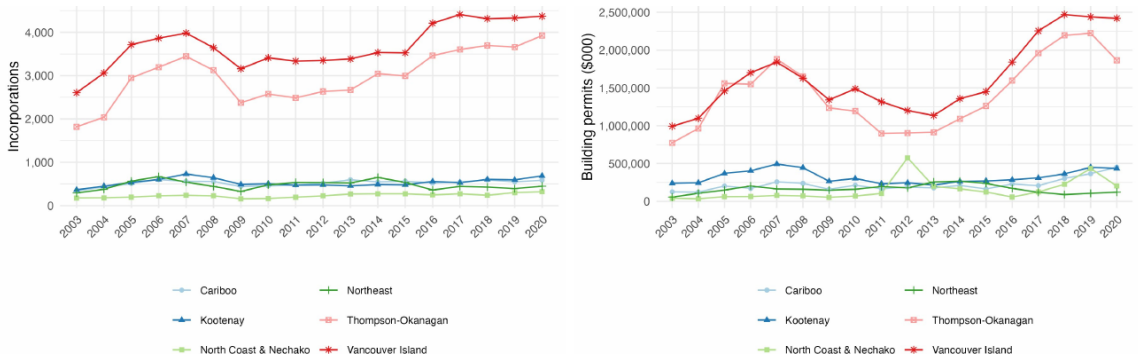

Figure 1: (a) Business incorporations and (b) total building permits ($000) by B.C. development region, 2003–2020. Lower Mainland suppressed for visual clarity but included in correlation analysis. Pearson $r = 0.99$ ($p < 0.001$).

series ($r = 0.99$, $p < 0.001$). While neither is a perfect metric of local area GDP, we use business incorporations as a proxy for broader local economic activity.[1]

Table 1 shows the input variables that we use for the matching of development regions. These include unemployment and employment in different sectors of the economy as well as the size of the male adult population to proxy the active labour market. Also apparent from this table is that that the match on employment and population characteristics is close in the Treated and Synthetic units after processing. They are most similar on unemployment and employment across sectors that reflect the rural, resource-dependent character of the Kootenay region. Also as shown in (Table 2), the Lower Mainland, the most urban part of the province, contributes zero weight to the synthetic unit. This is not imposed by the researchers but rather the model assigns zero-weight because the Lower Mainland does not resemble the Kootenay development region on the pre-treatment trajectory or on covariates. As is also shown, the donor pool is dominated by the Cariboo (0.30) and North Coast (0.47) development regions of the province. Both regions share the Kootenay region's rural, resource-sector profile. We also check that the donor weights are consistent across all 1,000 imputed datasets and find this to be the case.

Table 1: Covariate balance: treated and synthetic units

Variable	Treated	Synthetic	Sample mean
Unemployment Tertiary	43.59	44.34	51.57
Unemployment Goods	61.32	107.42	98.54
Unemployment Services	50.93	43.67	50.67
Employment Tertiary	388.58	389.73	1,844.21
Employment Goods	289.60	158.43	653.31
Male population	70,611	70,823	266,419

[1]The Lower Mainland is suppressed from the figure for visual clarity but included in the correlation.

Table 2: Donor unit weights

Region	Weight
Vancouver Island	0.06
Lower Mainland	0.00
Thompson-Okanagan	0.05
Cariboo	0.30
North Coast	0.47
Nechako and North East	0.13

3.3 Results

Hypothesis 1 predicted a null or negative effect consistent with the resource curse literature, and the estimates point the other way across the full distribution of imputed datasets. With six donor regions, the permutation distribution has a floor of $2/7 \approx 0.29$, so we lean on the pattern, its sign and its timing, not on a single significance test. And the synthetic control speaks only to a macro question, whether the windfall produced any developmental return at all; it cannot speak to whether the board allocates that return without bias, which is the question that analysis on grant disbursement takes up in the next section.

Figure 2 shows the results for Hypothesis 1. Inspecting the pre-treatment period, the gap distributions sit tightly around zero. After 1995, the distributions shift upward. They remain positive through approximately 2013 before narrowing back toward zero. This inverted-U pattern is consistent across the full distribution of imputed datasets.

The timing is informative. A pure wealth effect from the CBT endowment would predict a flat positive shift that persists indefinitely. The data show a rise-and-fall pattern instead. Over the full period, the estimate is that the CBT added approximately 456 incorporations in total. This is equivalent to roughly 1.5% of the region's baseline business stock and approximately one additional year of regional growth stretched over a decade. Remembering that while the Basin is geographically large, the communities in the Basin are small and rural. The estimated effects are modest and time-limited and fading thereafter. While on the one hand we find that the CBT is no panacea for regional economic development, on the other hand, the results run opposite to what the resource curse literature expects.

We conduct two robustness checks: leave-one-out and backdating. We first re-run the synthetic control dropping each donor region in turn (Figure 6), and find that the inverted-U pattern is consistent across all specifications. The Lower Mainland contributes zero weight in every model. Removing it has no effect on the estimates. The models closest to the main result are those dropping the Lower Mainland and Thompson-Okanagan. This is unsurprising as these regions contribute very little to the donor pool and are also the most urban parts of the province.

Second, we backdate the treatment to 1990, when the provincial government and BC Hydro increased grant money to regional districts following a consultation with Basin communities (Participant 4, 2024). If this earlier grant expansion drove the main result rather than the CBT endow-

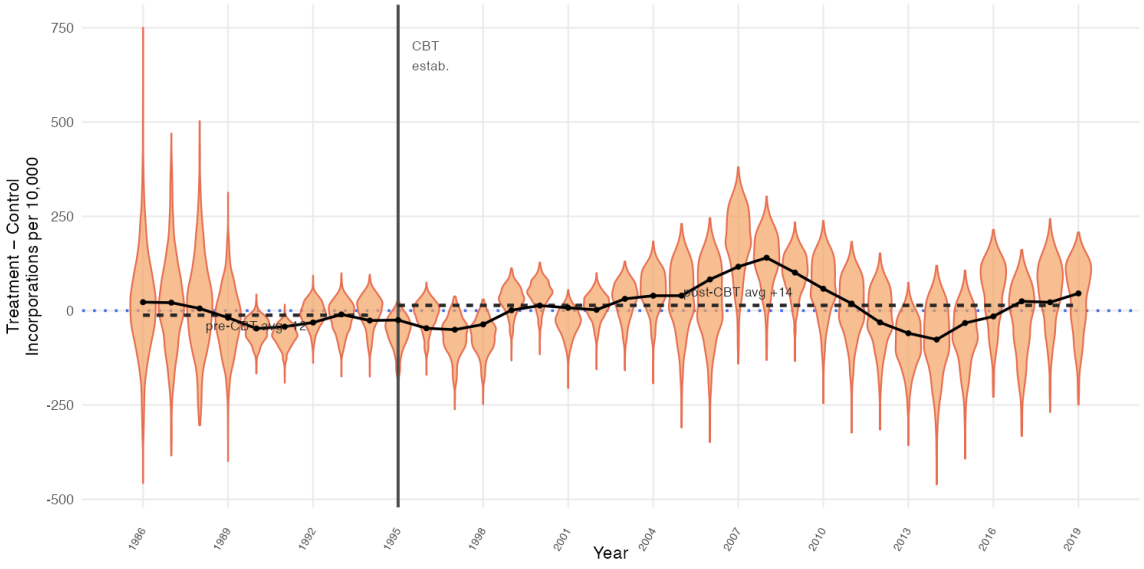

Figure 2: Distribution of synthetic control estimates across 1,000 imputed datasets. Each violin represents the distribution of $\hat{\tau}_{1t}$ for one year. Vertical line marks CBT establishment (1995). Pre-treatment distributions confirm parallel trends. The post-treatment inverted-U pattern is most pronounced 2001–2013. The solid black line is the three-year rolling mean of the per-year average gap; the dashed segments mark the pre- and post-1995 average gaps, which sit near zero before the Trust is established (−12) and above zero afterwards (+14).

ment, we would expect the positive gap to emerge around 1990. We find much weaker evidence of an inverted-U and little to no effect in the 1990–1995 window (Figure 7). This is inconsistent with the idea that it would be the earlier windfall driving the result.

We considered modern extensions of the synthetic control estimator, in particular the generalised synthetic control of Xu (2017) and the Bayesian alternative of Pang et al. (2022), both of which can deliver sharper inference and additional flexibility in matching. Both methods, however, require either a substantially larger donor pool than the six B.C. development regions available here, or a panel structure with multiple treated units. Neither condition holds in our setting, and we therefore present the standard synthetic control with full distributional uncertainty across the 1,000 imputed datasets as the most credible estimator the data support. Permutation-based placebo tests, the natural inferential companion to synthetic control, are similarly limited by the small donor pool. We rely on the leave-one-out and backdating checks above.

4 Grant Allocation, Board Composition, and the Mechanism

The preceding results establish that early CBT investment in hydropower had a small positive aggregate effect on regional economic development. In this section, we test more directly whether board members channel disproportionate grant funding toward their home districts.

The Trust embodies the mixed-territorial seat allocation (MTSA) structure set out in Section 2.

The current Act specifies a twelve-member board. Five seats are each nominated by a different regional district and one by the Ktunaxa Nation Council, the single First Nations body named in the Act; the remaining six seats are appointed directly by the province with no district nomination. The Lieutenant Governor in Council makes all final appointments and may decline a nominee, so the province retains formal authority over the whole board,[2] and all twelve directors must reside in the Basin. The founding language frames the nominated seats as a representational principle, that every community that bore the costs of the Treaty deserves a voice in how the revenues are spent. Whether or not anti-capture design was the intent, the structural consequence is the one our theory turns on.[3]

The board was larger in its early years. Before 2003 it had roughly eighteen members, with a larger regional component and two Ktunaxa seats rather than one, and under the original Act the regional districts appointed their own directors directly. In May 2001, the incoming B.C. Liberal government placed all provincial bodies under a core-services review, and in 2003, Bill 79, the Columbia Basin Trust Amendment Act, cut the board from eighteen to twelve people, reduced the regional component from eleven to five people, and replaced direct community appointment with nomination subject to provincial approval. The change matters for our purposes because the changes shifted the ratio of community-facing to no-stake seats, and we use it below as within-case variation in board composition. The restructuring also dropped any population weighting: Fraser-Fort George, with roughly 8,000 Basin residents, holds the same single seat as East Kootenay, with over 55,000.

The structural consequence of changes to the board is one that MTSA can speak to. The five regional districts and the Ktunaxa Nation each nominate one director, and the same members compete directly for the community grant budget. Any director who steers disproportionate funding home takes it directly from the nominating principals of the others, who have every reason to redirect it to their own communities instead, while the six provincial appointees have no community to reward. Two episodes bear on these scope conditions, and we treat them differently. The 2003 amendment, which shifted the seat ratio against the community-facing bloc, gives a direct test of the rivalry condition, and we exploit it below. The rapid growth of total CBT spending after 2014 loosened the fixed-budget condition; rather than test it, we accommodate it in the design, since the year fixed effects absorb the common annual budget and identify home bias within the year, and we read the developmental effect mainly in the earlier, more fixed-budget years.

4.1 Data and Methods

We assemble the cross-sectional time-series data from two sources. Our main dependent variable is district-level grant flows and this data comes from a complete scrape of the CBT's public project

[2]We know of no instance in which a nominee put forward by a regional district or the Ktunaxa Nation has been declined.

[3]Participant 4 (2024) in an interview talks about the founding board as having representation wider than territory to include characteristics such as union membership. However, with the change in the board size and composition outlined below, we have little means to test the effects of these changes directly.

database for fiscal years 1999–2000 through 2024–25. Once we have gathered all of the grants, using the location name tagged to each grant, we assign each grant to a district with a location-to-district crosswalk: grants that name a single community or electoral area are credited to that district; grants spanning several areas are split equally across the named districts; grants that are allocated to a Ktunaxa or First Nations body are credited to the Ktunaxa; and Basin-Wide and other region-wide grants are placed in the equally-distributed pot that our community-specific measure excludes. These coding rules place 99.7 per cent of grant dollars that we gather.

The appointment mechanism is our main independent variable and the variable that our theory turns on. We validate it against the British Columbia Orders-in-Council (OIC) that names each appointment. OICs specify the appointment mechanism directly in their announcement of the appointment, however, we also cross-check against the annual reports and founding-period meeting minutes, looking for cases where it is stated whether the director is appointed by the province or regionally. Wherever a primary source states the mechanism, the roster agrees with the legal record. The cross-check also shows that the mechanism is time-varying for a few directors, at least one of whom moved from a community nomination to a provincial appointment, so we code it at the director-year level.[4]

A final point about the dependent variable. The $45 million originally earmarked for community grants (Section 3) is an initial allocation, not an annual budget. The grants actually disbursed vary sharply from year to year. Over the full series, grants grow several-fold, from roughly one to two million CAD a year in the early 2000s to fifteen million and more after 2014, with one-off peaks in 2011, 2021 and 2023 (Figure 3); cumulative community-grant spending over 2000–2025 is about $246 million, well beyond that initial earmark. This growth matters for the *scope* of the mechanism: the fixed-budget condition bites hardest in the earlier years and maps those years where the synthetic-control developmental effect is concentrated. Changes in grant dollars does not threaten the grant-allocation estimates that follow because the year fixed effects absorb the common annual budget and its spikes. The home-bias coefficients as per Hypothesis 2 are identified only from differences *across districts within a year*, however, it is important to note these changes as one of the scope conditions is a fixed grant allocation within a year.

4.2 Measures and Empirical Strategy

Our dependent variable is *community-specific* grants, projects targeted to an identifiable district, as distinct from Basin-Wide grants that are distributed equally across all six districts by construction and so are uninformative about board behaviour. The resulting panel has 156 district-years: six districts observed over twenty-six years. In total dollars no district sits systematically at the top of the community-specific distribution (Figure 4). Per capita, the Ktunaxa Nation is highest in several years, but this reflects its small population rather than a level of funding that crowds out

[4]Orders-in-Council before roughly 2003 often record only that a person "is appointed to the Board of Directors" without naming the mechanism in the prose, encoding it in the cited statutory subsection instead; for those directors we take the mechanism from the annual reports and the minutes. Clean nomination-by-district information is complete for the founding board and through the mid-2000s, and again around the 2003 reform discussed below.

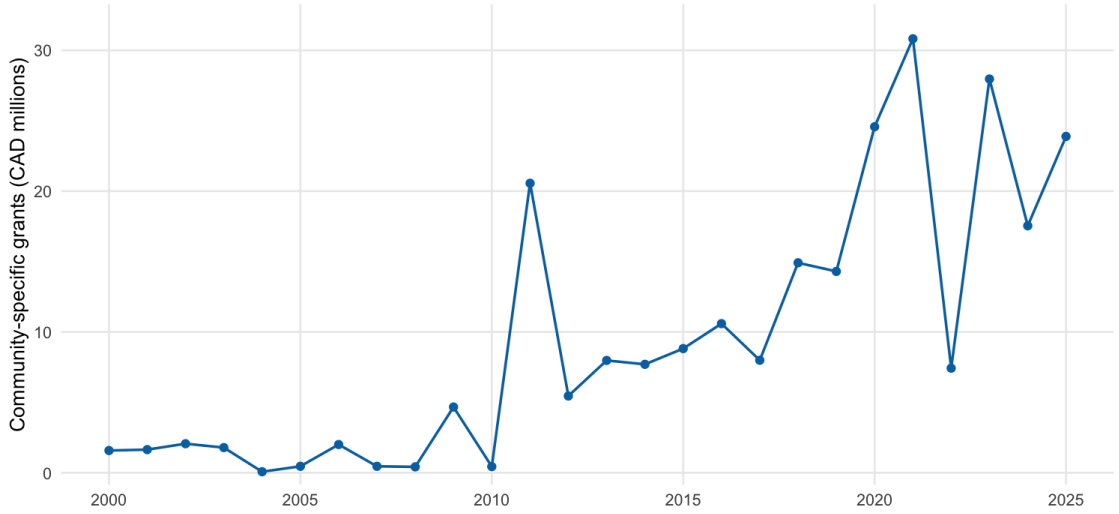

Figure 3: Total community-specific grants disbursed by year, 2000–2025 (CAD millions; Basin-Wide allocations excluded). The $45 million earmark is an initial commitment, not an annual budget: disbursements grow several-fold over the period, with peaks in 2011, 2021 and 2023.

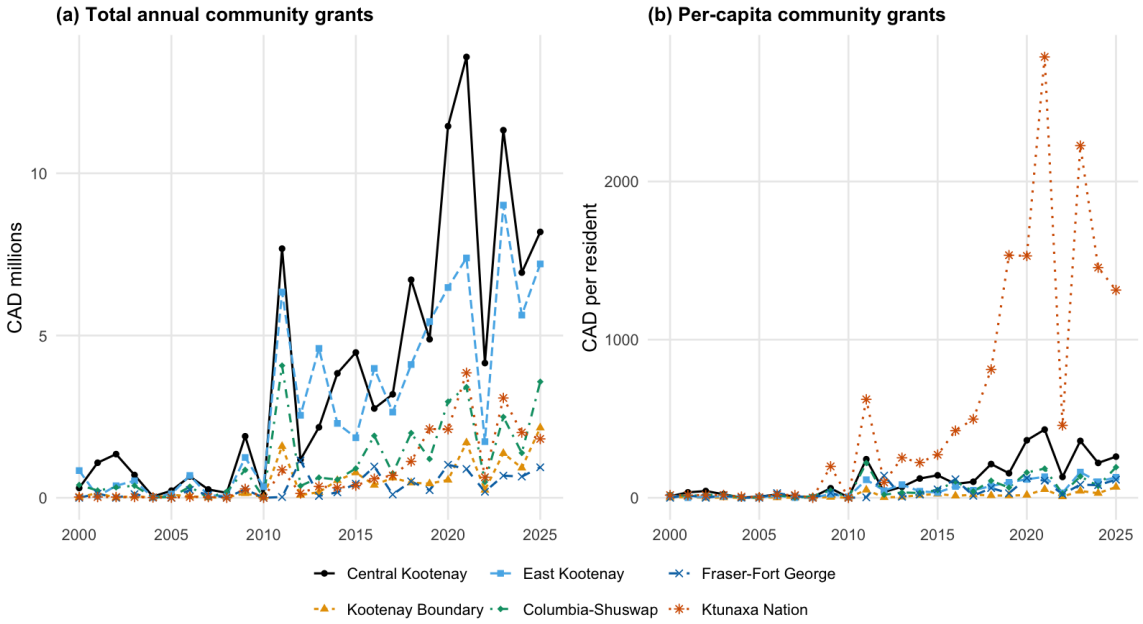

Figure 4: Community grant funding by district, 2000–2025. (a) Total annual community grants in CAD millions. (b) Per-capita community grants. Population denominators: Central Kootenay 31,500; Kootenay Boundary 31,800; East Kootenay 55,600; Columbia-Shuswap 18,500; Fraser-Fort George 8,200; Ktunaxa Nation 1,381.

other districts. The dependent variable is therefore specific grant funding to district i in year t, in thousands of CAD.

Home bias might run through either of two channels. A Regional or Ktunaxa director is the formal agent of district i's nominating principal, so the question is whether a district whose prin-

cipal has an active director on the board brings home more community-grant funding. This is the principal-level prediction the mixed-territorial seat allocation setup is meant to close, and it is the channel our two main tests probe directly. We use two strategies. First, the 2003 reform, which removes a district's shared principal, and second, the director-level analysis, which asks whether an individual director steers money to their own home district (Hypothesis 2). A Provincial appointee has no district principal but might still favour the town where they reside, and so as a weaker, informal residence channel, we also test this in the aggregate district-year model reported in Appendix A.

Finally, for each director, we also include their length of tenure on the board, which we then use to test the socialisation mechanism specified in Hypothesis 3.

4.3 The 2003 Reform: A Difference-in-Differences

The strongest within-case leverage comes from the 2003 reform, and it is sharper than a before-and-after comparison because the shrinking of the board did not happen all at once. Sitting regional directors served out their terms and were not reappointed, so the number of regional directors attached to each district fell at different times and by different amounts between 2003 and 2005 (Table 3).[5] We leverage this staggered draw-down because a smaller number of potential coalition partners matters directly for our theory. If directors steer grants home, a district that sheds its own directors should see its grant share fall more, relative both to districts that keep theirs and to its own in the past.

We test changes in the effects of board composition directly using a binary treatment because our theory says that it is (potential) coalition partners that matter. Scope condition 2 fails when a district has two or more co-nominating directors, who can coordinate on their shared district, and holds when it has one, who cannot. Let $\text{SharesPrincipal}_{i,t} = 1$ when district i has two or more regional or Ktunaxa nominees in year t, the configuration the seat rule is meant to rule out, and zero otherwise. The 2003 contraction switches this indicator off at staggered times, as districts fall to a single seat. We estimate

$$\text{CommunityGrants}_{i,t} = \delta\,\text{SharesPrincipal}_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t},$$

with district fixed effects α_i and year fixed effects γ_t, so δ is identified from which districts cross the two-to-one boundary, and when, with the common timing of the reform absorbed by the year effects. We find limited evidence that losing seats is associated with lower levels of district level funding. The estimate is $\delta = +36$ thousand CAD (SE 278, $p = 0.90$) over the full series[6] and

[5]The appointment type of every director entering Table 3 is confirmed against the British Columbia Orders-in-Council that effect each appointment: of the regional and Ktunaxa director-years over 2001–2006, the large majority match the Orders-in-Council at high confidence with no appointment-type disagreements, and the handful the public record leaves as unnamed placeholders carry no district location and so enter neither the per-district counts nor the regression. East Kootenay went from five regional directors in 2001 to two by 2004; Central Kootenay fell from three in 2002 to one by 2004; Kootenay Boundary from three to one by 2005; Fraser-Fort George lost one of its two seats in 2004; and Columbia-Shuswap lost its single seat in 2005.

[6]The full-series estimate pools every such transition over 2000–2025, not only the reform: a district crosses from

Table 3: Regional (district-nominated) directors seated by district, 2001–2006. The 18-to-12 contraction proceeded by attrition: directors served out their terms and were not reappointed, so each district's representation fell at a different time and by a different amount. Bold marks years in which a district holds two or more co-nominating directors, the "shared principal" configuration the 2003 reform removes.

District	2001	2002	2003	2004	2005	2006
East Kootenay (RDEK)	**5**	**4**	**3**	**2**	**2**	**2**
Central Kootenay (RDCK)	**2**	**3**	**2**	1	1	**2**
Kootenay Boundary (RDKB)	**2**	**3**	**2**	**2**	1	1
Fraser-Fort George (FFGRD)	**2**	**2**	**2**	1	1	1
Columbia-Shuswap (CSRD)	1	1	1	1	0*	0*
Ktunaxa Nation	1	1	0†	1	0†	0†

Notes: Cell entries are the number of regional, district-nominated directors seated in each year; the Ktunaxa Nation's reserved seat appears in the final row. Bold cells are the treatment the difference-in-differences switches off (two or more co-nominators).
*Columbia-Shuswap's regional nominee was not renewed after 2004; the Revelstoke seat continued as a provincial appointment, so the district kept a resident director but no regional nominee.
†The Ktunaxa Nation holds a separate designated seat across the whole period; the recorded gaps reflect turnover between appointees, not loss of the seat.

+71 (SE 147, $p = 0.65$) in the 2000–2008 window where the seat contraction is occurring. When a district *could* field co-nominators who might collude, it captured slightly more grant money than when it could not, but only by a very small and statistically insignificant amount. Under home bias the coefficient should be clearly positive, which it is, but it is small relative to its standard error.

We also find that this result does not depend on how we code the variable. As a check, we replace the indicator variable with the number of regional seats a district holds; the per-seat estimate is also null though still positive (+52 thousand CAD, $p = 0.84$ on the full series; +58, $p = 0.40$ in the window). While we think that the binary indicator is a better metric because a linear per-seat speaks directly to the mechanism in the theory we also note that both models find similar results.

There are of course limits to this model. There are only six districts (provincial nominees cannot allocated directly), so the inference carries some caveats. Further robustness checks are included in the appendix A; we also observe three pre-contraction years (2000–2002) which means that there is little data; and finally, the timing of non-reappointment, though plausibly driven by term expiry rather than by grant dynamics, is exogenous but not randomised. As a result, we caution that the results should be read as if we have identification leverage and not as a causal claim. With those caveats in mind, however, these first results offer within-case evidence. The director-level analysis we present next. The aggregate district-year model we also leave for the the appendix.

two or more co-nominating directors to one or none fourteen times across the panel, and eleven of these fall outside the 2003–2005 reform window, as directors' terms end and appointments lapse. The 2003 contraction is the sharpest and most legible episode, but the null reflects every district that sheds representation, wherever in the period it occurs.

4.4 Home Bias at the Director Level

Our next test of Hypotheses 2 and 3 is at the level of the individual director. The board allocates grants collectively and we cannot observe how any one director votes as direct voting is not in the meeting minutes. Instead what we do is link each director to the grant flow of their home district and compare across the two groups. Directors holding a regional or Ktunaxa seat are the formal agents of a district's nominating principal. Directors appointed by the province represent no district and serve as the comparison group. If regional-seat directors steer money home, their home districts would receive more than the home districts of provincial appointees do.

We build a director-year panel from the board roster, 65 directors and 369 director-years with a mapped home district over 2000–2025 and we estimate the effect of holding a nominating seat on home-district grant flow with year and home-district fixed effects, clustering on the director ($G = 65$). The year and home-district effects make this a like-with-like comparison, within the same district and the same year, so the difference is not simply that some districts receive more than others. Working at the director level also gives sixty-five clusters, which gives us some variation to exploit.

Table 4: Director-level fixed-effects models of home-district grant funding (CAD thousands). The outcome is the community-grant flow of each director's home district. Column (1) enters the nominating-seat indicator and board tenure; column (2) adds their interaction. Year and home-district fixed effects throughout; standard errors clustered on the director ($G = 65$).

	(1)	(2)
Holding a nominating seat	−280.7	110.3
	(304.6)	(447.2)
Board tenure (years)	58.6	111.5
	(39.6)	(47.0)
Tenure × nominating seat		−77.8
		(66.2)
Year and home-district FE	✓	✓
Clusters (directors)	65	65
Director-years	369	369

Notes: Cluster-robust standard errors (CR1) on the director in parentheses. "Holding a nominating seat" equals one for Regional and Ktunaxa directors. The nominating-seat indicator and the tenure interaction are indistinguishable from zero; board tenure carries a positive coefficient, the opposite of the negative sign the socialisation account predicts.

Table 4 reports the result. Holding a nominating seat has no effect on home-district grants: the point estimate is negative, about −281 thousand CAD ($p = 0.36$). A director who is the formal agent of a district's nominating principal directs no more money home than a provincial appointee who represents no district at all. This is Hypothesis 2 at its most reliable: where distributive politics predicts a positive effect, we find little evidence of this.

The same panel delivers the socialisation test of Hypothesis 3. If longer service drew directors

towards the equal-share benchmark, board tenure would lower home-district allocation. It does not: the tenure coefficient is small, positive, and indistinguishable from zero, about +59 thousand CAD per additional year ($p = 0.14$). Socialisation, if it operates at all, should act most on the directors who have a home district to advocate for, so the sharper test is the interaction of tenure with holding a nominating seat. This interaction asks whether seat-holders steer less money home the longer they serve. The interaction is negative, the direction socialisation predicts, but small and far from significant, −77.8 thousand CAD per year of tenure, ($p = 0.24$), so it is also null. Further, the no-home-bias result holds whatever a director's tenure.[7]

Two further analyses corroborate these findings, which we also present in the appendix A. The first is an aggregate district-year fixed-effects model, which returns the same null for both the regional and the provincial-residence channel. Because it and the 2003 difference-in-differences both rest on only six districts, we re-check their nulls there with a wild cluster bootstrap, randomisation inference, and cluster robust variance estimator, none of which moves any coefficient near significance. We also report a design-based power analysis showing that the six-district designs can rule out only large home bias, with a minimum detectable effect near the mean annual community grant disbursement of just over one-million Canadian dollars (1.27 Million CAN). This is why the director-level test, with sixty-five clusters, is in our mind more reliable although no model shows any evidence of home bias.

4.5 The Deliberative Record

The above analyses observe the *equilibrium output* of the board's decisions, not the bargaining that produces it, and that gap is structural rather than incidental. The CBT, like most appointed boards, develops grant and budget decisions in staff procedures and committees and ratifies them at the full board, so its minutes record decisions already reached rather than the proposals weighed and dropped along the way. Coalition behaviour on such a board is close to unobservable to an outside analyst, and that is true of most appointed boards, not only this one.

We none the less read the full set of 238 board-minutes documents (1995–2024), released under the aforementioned Freedom of Information request, and the corpus bears out that description. It contains roughly 3,800 recorded motions. Almost none are contested: 27 are recorded as defeated and a further 29 draw a recorded opposition from a named director, so fewer than one motion in sixty meets visible resistance, and not a single decision is taken by a recorded vote tally. Substantive items reach the floor already worked up as committee recommendations, of which there are some ninety-six. This is a board that decides by consensus on committee-prepared business, which is precisely the setting in which an extraction *attempt* would leave no trace even if it were made.

What the contested motions are about is nonetheless telling. Of the roughly fifty-six motions that were defeated or drew a recorded opposition, only five mention a grant or a named district,

[7] In levels the tenure main effect in the interaction specification (column 2) is positive and significant (+112 thousand CAD per year, $p = 0.02$), but this is because the dependent variable is skewed. It vanishes under a logarithmic transformation (-0.007, $p = 0.77$) and under population weighting ($p = 0.84$), so it reflects the absolute grant totals flowing to the populous home districts of long-serving provincial appointees, not a socialisation effect.

and those concern basin-wide programmes or board appointments, not a director steering funds to their own district. The recorded contestation is over governance, policy, and the size of basin-wide commitments, not over home-district capture, and there is no record of a home-directed extraction proposal, carried or even tabled. We read this as corroboration. Because the decisive business is pre-settled in committee and ratified by consensus, the absence of a recorded extraction attempt is consistent with our equilibrium but cannot on its own establish that coalitions were suppressed rather than never attempted. Coding grant motions by district target, fate, and mover–seconder identity in the pre-2008 minutes, where names appear, would sharpen this analysis, however, we leave further textual analysis to follow-up work.

The minutes also record conflict-of-interest declarations, in which a director with a stake in a matter declares it and withdraws. These appear in 47 of the 238 meetings, 79 times in all, across the whole period (Figure 5). We are also cautious about what this shows. Where a declaration can be matched to a named director, more than half come from a handful of long-serving members, and they are split across regional and provincial appointees. The record therefore establishes that *some* directors recuse when they have conflicts, however, it does not show that recusal is a a board-wide norm. Figure 5 also marks the limit on what the record can show because the number of meetings held partly in camera and released only in redacted form rises sharply after the mid-2000s, from almost none to most years. Confidential business is, by construction, where any bargaining would be least visible, which is one more reason we rest the argument on the equilibrium evidence and treat the minutes as corroboration. In summary, while we do not uncover a large number of on the record attempts to direct funds home, we also face significant challenges in finding such evidence.

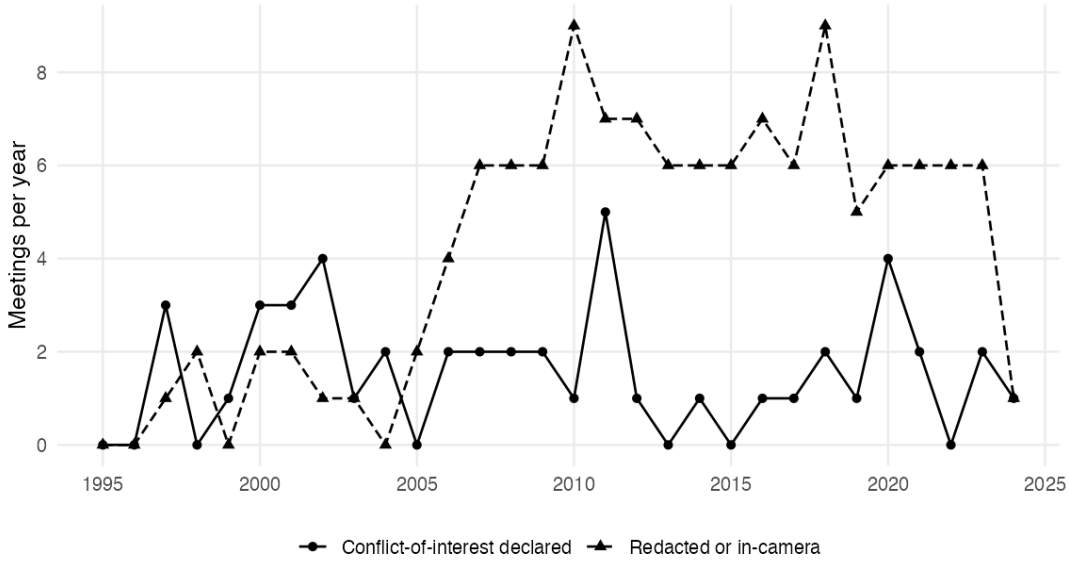

Figure 5: Board minutes, 1995–2024. Conflict-of-interest declarations recorded per year (solid) and meetings held partly in camera or released only in redacted form (dashed). A recusal practice operates throughout; the confidentiality of board business rises after the mid-2000s. Source: 238 board-minutes documents

4.6 Discussion

Our theory suggests that under conditions of mixed-territorial seat allocation, no home-directed coalition can form and consequently that no district is advantaged by a board member holding a territorial seat. The evidence shows this in a number of places including the staggered 2003 contraction and the director-level panel. We also find no obvious evidence of this in the deliberative record. What a single case cannot reveal of course is a comparison against a counterfactual board where coalitions do form. Because the board develops its decisions in committee, coalition behaviour is not directly observable, so we treat the result as confirmation of the predicted equilibrium outcome rather than as direct test of our theory.

Interviews with former Trust directors and with participants in the Trust's founding moments give the mechanism some context but they also do not settle the score. Drawing on a set of semi-structured interviews conducted in the Columbia Basin between April and May 2024 with former Trust directors, participants in the Trust's founding, residents, and a regional author and historian, members talk both about institutional design and also about socialisation.[8] One thing that is substantiated by the interviews is that members relied on internal rather than external measures for accountability. On the evidence that speaks directly to coalition formation, directors describe a board built to suppress territorial claims. They suggest that board seats are purposely appointed rather than elected to ward of home bias that is entwined with electoral politics. One former director noted that the board position is not elected "because we knew that that would just . . . make . . . people running for the job saying I'll get money for our town" (Participant 4, 2024). On the socialisation account, directors also describe enforcing norms to new board members when they join. The same participant and former member recalled a retiring colleague telling his replacement at his first meeting that he was "not here to represent" his own town but was "here because of 160,000 people over all this space" (Participant 4, 2024). The two accounts are of course hard to separate. A director who never pushes for grants to be allocated to his/her/their own home-district may be simultaneously deterred by an institutional design that makes such a push futile, and counseled upon arrival to the board on norms about not pushing for one's district at all. However, if the norm is taught at induction rather than acquired over years, it would likely leave no trace in the model where we estimate the effects of tenure. We therefore cannot say definitively which channel carries most of the explanatory weight.

One possible alternative explanation might be that it is not the internal mechanisms of the board at play but rather the external mechanisms, and in particular, the role that provincial level oversight and appointment processes which are driving our results. The Lieutenant Governor in Council makes every appointment and may decline any nominee, so the province might screen out rent-seeking nominees even before they join, thus leading to a selection effect on membership. This explanation does align with the evidence presented, however. The community-facing seats, where

[8] Interviews were recorded and transcribed, and participants are identified only by an anonymised label and a generic role. The study holds ethics approval from UZH, (ETH2324-0852). Appendix B sets out the interview guide, recruitment, the anonymised participant list, and the coding scheme.

home bias would operate, are nominated by the regional districts and the Ktunaxa Nation, and we know of no instance in which a nominee was declined. Whether the board has nominating power *de jure* does not mean that the province uses its power *de facto*. Furthermore, the seats the province does fill directly, its own appointees, are where a selection story should bite hardest, yet those directors direct no additional funding to the district in which they happen to live and are not statistically different from their regional counterparts. Even more interesting is that one provincial appointee recalled being recruited not by the Province but by the Trust's chief executive, who said he "thought that I could be a valuable asset to the board" and asked "would you be interested in sitting on this board?" (Participant 1, 2024). While it may be the case that appointment selection reinforces the outcome of no home bias, considered independently, a story of board-seat selection is inconsistent with our findings.

Finally, a second consideration is that the board initially depended on public engagement for informal mechanisms of accountability but that this has not endured. A founding director recalled that the intensive early consultation with civil society "probably wouldn't work" today because "people get tired." The participant also mentioned that communities came together in order to delegate the task to the board rather than monitor it (Participant 2, 2024). The purpose of delegation was, at least in their mind, to rid the local communities of having to actively participate in redistribution and how they did this was entrusting and empowering the Trust to take this task over.

A final alternative that ought to be mentioned is the role of the staff administration behind the board. Several directors described a board that leans heavily on its professional staff, who prepare the grant packages and whose recommendations are rarely overturned; one former member could not recall a staff recommendation that the board had rejected (Participant 1, 2024). How far an appointed board depends on its administration deserves study in its own right yet having a well functioning staff does not change where authority sits. The board remains the governing body, and the staff who advise it remain internal to the institution under its governance structure. Any observable dependence between staff and a board is a feature of how an appointed board does its work. It is importantly not a return of the external checks, electoral or otherwise, that are absent in this case.

5 Conclusion

Benefit-sharing mechanisms are increasingly being set up alongside mechanisms that redistribute the windfalls from energy production. Previous literature has focused on accountability mechanism found outside the governance structure of the institution itself, such as a rule-based formula that removes discretion or electoral accountability and an attentive public. In this paper, we present one way that institutional design can be used to also discipline its decision-makers from within. The particular design that we focus on is the way that board of director seats are allocated. We show that when each place-based seat represents a different community competing over a fixed budget, and who also govern alongside appointees with no territorial seat to serve, directors have

difficulty assembling a coalition who can help them steer money to their home districts. Such a structure makes rent-seeking costly and that is because every territory-based potential ally is also a self-interested rival.

We use the case of the Columbia Basin Trust to illustrate and to probe the plausibility of our theory. We find that despite having the features associated with the sub-national resource-curse, discretionary spending generated a small and time-limited developmental gain in the region. Our estimate is that it produced about 456 additional business registrations over 1995–2019, equivalent to roughly one extra year of growth spread across a decade. With only six donor regions, we read this as a directional estimate rather than a sharp causal one, yet even so it runs opposite to that expected by the standard theory. On distribution, we find no evidence of home bias. Districts that shed regional seats in the 2003 restructuring of the CBT found themselves with slightly less money as potential coalition partners reduced. Perhaps even more telling is that we also find evidence that directors who hold a regional seat are also associated with no more money than those without a regional seat.

Our results place mixed-territorial seat allocation alongside elections and statutory formulas as a possible route to equitable distribution, and, importantly, we think such a design is the least demanding and most affordable of the three. We find that discretion, if internally constrained, need not be associated with home bias so long as the board governing its windfalls are built such that rent-seeking behaviours are disincentivised.

The policy implications our our findings are also specific. As governments attach benefit-sharing bodies to hydropower, wind, and solar development, how seats are distributed across affected communities is a design choice with measurable consequences. A board on which every community-facing seat answers to a different community, and competes with the others for the same budget, raises the cost of coalition-building for extraction without depending on elections. In the context of the Columbia Basin Trust, while such a structure has not lead to miraculous growth, the Trust demonstrates a model of governance, at least in the early years, that can be both locally responsive and politically desirable.

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A The District-Year Model and Robustness

This appendix reports some additional tests including an aggregate district-year fixed-effects model presented in Section 4, together with few-cluster inference, design-based power analysis, and measurement checks that accompany it. The district-year model is the least powerful of our tests, as there are only six clusters. As a result, we treat it as corroboration for the director-level results presented in the main effects rather than as primary evidence.

A.1 Synthetic Control Robustness

We report two robustness checks for the synthetic-control estimate of the Trust's developmental effect (Section 3), each averaged over the 1,000 imputed datasets as in the main estimate. The first is a leave-one-out check that re-estimates the per-year treatment-minus-control gap dropping each donor region in turn (Figure 6); the estimated trajectory is not driven by any single donor. The second is a backdating, or placebo-in-time, check that assigns the treatment a placebo date of 1990, fitting the synthetic control on 1986–1989 (Figure 7); no gap opens at the placebo date, and the positive developmental gap emerges only after the true 1995 treatment.

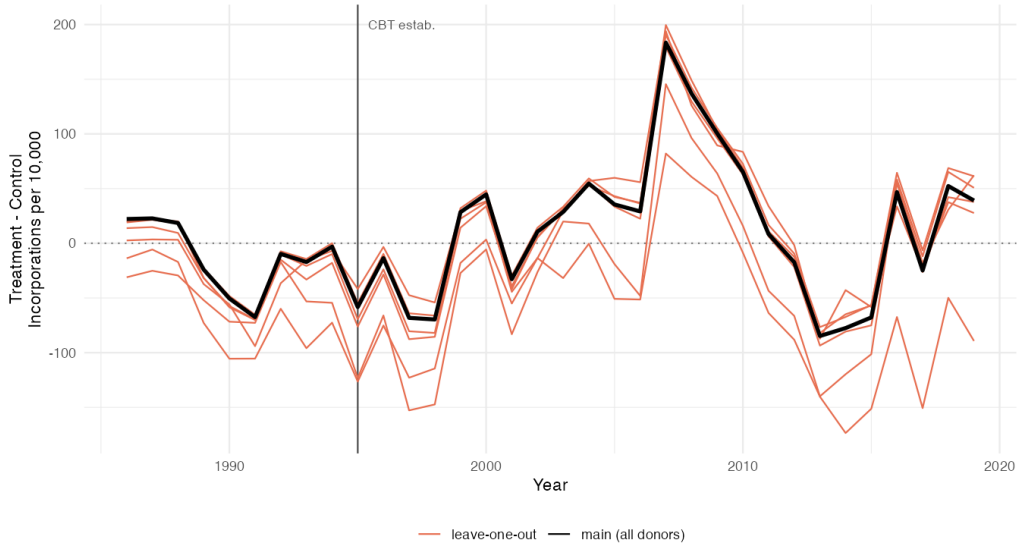

Figure 6: Leave-one-out robustness for the synthetic-control estimate. Each thin line re-estimates the per-year treatment-minus-control gap (incorporations per 10,000) dropping one of the six donor regions; the heavy black line is the main estimate using all donors. The vertical line marks CBT establishment in 1995. Averaged over the 1,000 imputed datasets; no single donor drives the result.

A.2 The District-Year Model

We measure board composition in two ways. The first is *presence*: `has_regional_nom` equals one when the Regional or Ktunaxa principal that nominates district i has an active director in year t, and `has_provincial_resident` equals one when a Provincial appointee resides in district i in

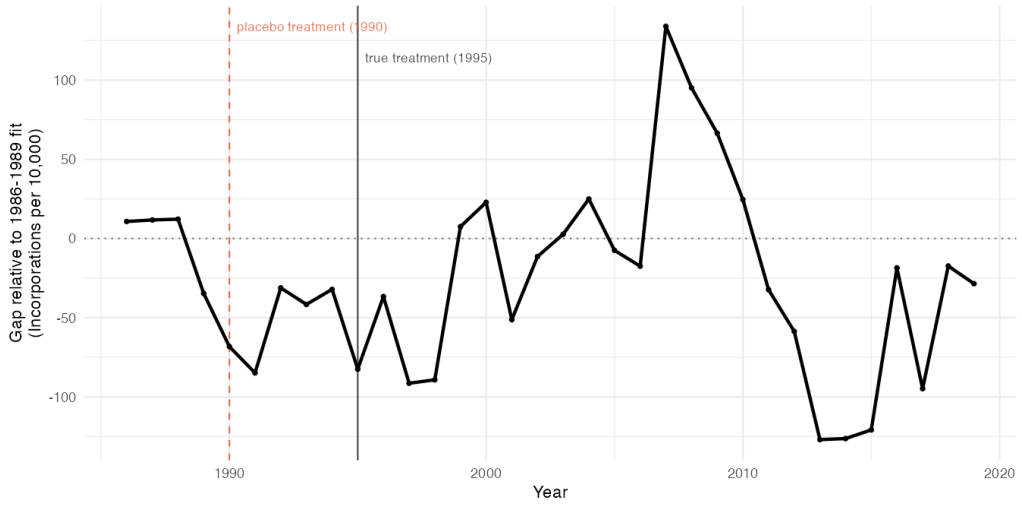

Figure 7: Backdating (placebo-in-time) check. The treatment is assigned a placebo date of 1990 (dashed line) and the synthetic control is fit on 1986–1989, with the gap shown relative to that fitting window. No gap opens at the placebo date; the positive developmental gap appears only after the true 1995 treatment (solid line). Averaged over the 1,000 imputed datasets.

year t. The second is *strength*: `n_regional_nom` and `n_provincial_resident` count how many such directors district i has in year t. Presence asks whether having a representative from a local area at the table matters at all; strength asks whether having more of them, regardless of nomination pathway, matters.

We estimate the district-year panel model

$$\text{CommunityGrants}_{i,t} = \beta_1 X^{\text{reg}}_{i,t} + \beta_2 X^{\text{prov}}_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t},$$

where X^{reg} and X^{prov} are the regional and provincial measures (presence or count), α_i are district fixed effects, γ_t are year fixed effects, and standard errors cluster at the district level. The fixed effects mean β_1 and β_2 are identified from changes within a district over time, not from differences between districts in size or wealth.

We report four specifications (columns 1–4 of Table 5): regional presence alone, its count, both presence indicators jointly, and both counts jointly. The joint presence model is the one we focus on. The panel has $G = 6$ clusters, below the textbook minimum for asymptotic CR1 inference. CR1 is the standard, default cluster-robust estimator. Additionally, the inference subsection below re-estimates it with a wild cluster bootstrap, randomisation inference, and the CR2 estimator.

At the aggregate district-year level the same picture holds. Table 5 and Figure 8 report the four specifications. In the joint presence model (column 2) the coefficient on `has_regional_nom` is 101 (SE 401, $p = 0.81$). The coefficient on `has_provincial_resident` is 1,172 (SE 720, $p = 0.16$). Both estimates are statistically indistinguishable from zero. The 95% confidence interval on `has_regional_nom` runs from approximately -931 to $+1{,}134$ thousand CAD; the interval on `has_provincial_resident` runs from approximately -678 to $+3{,}021$ thousand CAD. The count

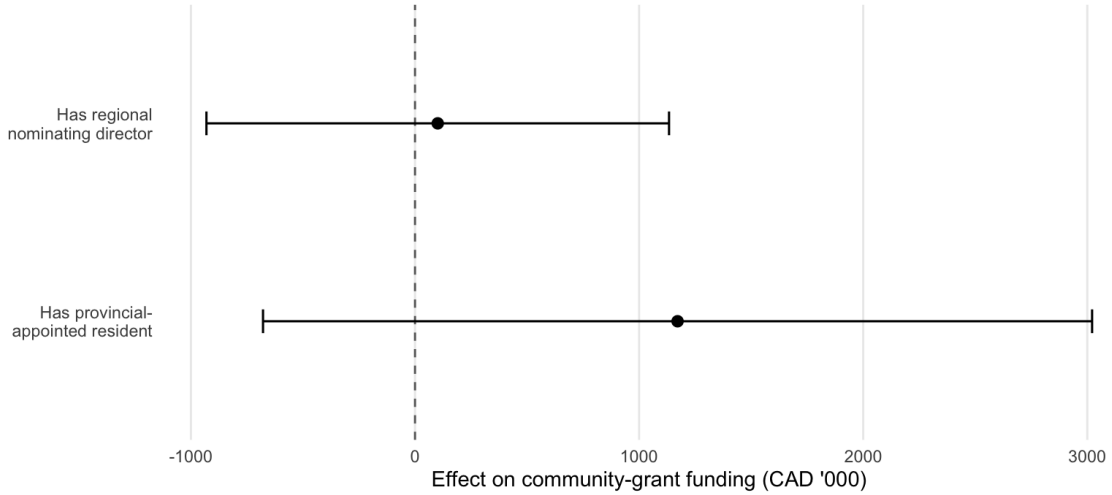

Figure 8: Coefficient plot for the joint presence model (column 2 of Table 5): effect of board-composition indicators on community-grant funding (CAD '000). Both 95% confidence intervals span zero. Cluster-robust standard errors at the district level ($G = 6$).

Table 5: District-year fixed-effects models of community grant funding (CAD thousands). The two left columns measure board composition as *presence* (0/1), the two right columns as a *count*; within each, the first enters the regional indicator alone and the second enters the regional and provincial indicators jointly. Column (2), the joint presence model, is the headline. No coefficient is distinguishable from zero.

	Presence (0/1)		Count	
	Regional (1)	**Both** (2)	Regional (3)	Both (4)
Regional nominating director	7.1	**101.4**		
	(350.5)	**(401.5)**		
Provincial-appointed resident		**1,171.6**		
		(719.6)		
No. of regional directors			121.3	−34.1
			(558.2)	(385.6)
No. of provincial residents				613.1
				(585.2)
District and year FE	✓	✓	✓	✓
Clusters (districts)	6	6	6	6
Observations	156	156	156	156

Notes: Cluster-robust standard errors (CR1) at the district level in parentheses, with critical values from t_{G-1}, $G = 6$. No coefficient is significant at the 10% level.

specification (column 4) gives the same picture: neither the number of regional nominating directors nor the number of provincial-appointed residents predicts community-grant flow to the district. Both indicators are positive but small relative to their standard errors in the joint presence model, and

neither becomes significant when entered alone (columns 1 and 3).

The pattern is what MTSA predicts. Districts whose nominating principal has an active director on the board do not receive disproportionate community-grant flows, because the seat allocation rule prevents a within-board majority for extraction from forming. Districts that happen to host a Provincial-appointed director do not benefit through any informal residence-based channel either: Provincial directors have no district stake regardless of where they live, and the data confirm they do not act as if they did.

Both nulls are robust to using `grant_funding_all` (which adds equally-distributed Basin Wide allocations to community-specific grants) as the dependent variable, and to dropping Ktunaxa from the sample.[9] The 2003 board contraction, reported in the main text, offers sharper, staggered within-case leverage on the same question through the district-by-district loss of regional seats.

The community-grant data are heavily skewed. The median grant is about CAD 5,000, the mean about CAD 40,000, and the largest single grant is CAD 16 million; the top one per cent of grants hold 36 per cent of all dollars. This skew shapes how the results should be read, so we report three checks. First, functional form. On the full 1999–2025 series the null holds under a logarithmic and an inverse-hyperbolic-sine transformation as well as in levels: the coefficient on `has_provincial_resident` is statistically indistinguishable from zero in every form, and is small in proportional terms (the log coefficient is $+0.77$, $p = 0.31$); its large dollar value in levels is a scale artefact that we return to below.

Second, we also examine the impact of outliers. Because a handful of very large grants could drive a level regression, we re-estimate the joint model after removing them, keeping the dependent variable in dollars so the coefficients stay interpretable. Dropping the largest one per cent of individual grants, dropping the largest half a per cent, and winsorising the district-year outcome at its 99th and 95th percentiles all leave the same picture. The coefficient on `has_regional_nom` falls towards zero as the largest grants are removed (from about 101 to roughly zero, and slightly negative (-8 to -27) under the strongest trims), so its positive point estimate in the headline model is carried by a few exceptional grants rather than by any systematic flow to nominating districts. The coefficient on `has_provincial_resident` is more persistent, staying positive at roughly 900 to 1,170 thousand CAD across every treatment, but it never approaches significance (p between 0.15 and 0.18) and it is the dollar-scale, not the proportional, result. Two further checks confirm that the dollar-scale provincial estimate reflects district size rather than per-resident favouritism. First, it is concentrated in the two largest districts: a leave-one-district-out refit roughly halves it when either Central Kootenay ($+613$) or East Kootenay ($+920$) is dropped, while dropping any of the other four leaves it unchanged or larger. Second, weighting the outcome by population removes most of it: the per-capita coefficient is about CAD 198 per resident ($p = 0.29$) on the full sample and about CAD 29 per resident ($p = 0.35$) once the small-population Ktunaxa Nation is excluded,

[9] On the full series, using `grant_funding_all` as the dependent variable leaves the joint model numerically unchanged, because the equally-distributed Basin-Wide top-up is constant across districts within a year and is absorbed by the year fixed effect; dropping the Ktunaxa Nation leaves both coefficients positive and insignificant (regional $+423$, SE 541; provincial $+1{,}467$, SE 890).

against a mean of CAD 144 per resident. The provincial association is a feature of the absolute dollar totals flowing to the populous districts, not of disproportionate per-resident funding.

Third, we also consider measurement of the provincial-resident indicator. This variable is built from the hand-coded board roster. A district-year is coded one when a provincially appointed director resides in the district. The roster is incomplete for some appointees, whose residence the public record does not state, and these are coded zero by default. The gap is not random; it concentrates in the early 2000s and the early 2010s. Where the annual reports list the provincial appointees with their residence town in a form we can read cleanly, we fill the gap directly: the 2000/01 and 2003/04 reports resolve the early-2000s placeholders, and we apply those corrections (the 2011–2016 reports do not parse cleanly and are left as coded). Doing so slightly raises the provincial coefficient, from about 1,094 to 1,172 thousand CAD, consistent with attenuation. We also re-estimate the model dropping the years that contain an unplaced provincial appointee, and again restricting to 2005 onward. The provincial coefficient does not weaken under either restriction; if anything it grows, which is what attenuation from mismeasurement predicts. The coding gap biases the estimate towards zero rather than manufacturing it, so the positive sign is not an artefact of incomplete coding. It remains statistically indistinguishable from zero throughout. We read the provincial-resident association as suggestive in direction but again, not statistically significant.

A.3 Robustness, Inference, and What the Design Can Detect

Three features of this analysis warrant scrutiny, and we address each in turn: inference with few clusters, the statistical power of the design, and the way an "active" director is measured.

Few-cluster inference. The standard errors in Table 5 cluster at the district level, but with only $G = 6$ districts the usual cluster-robust variance estimator (CR1) relies on a large-G approximation that is known to under-state uncertainty when the number of clusters is this small. We therefore re-estimate the joint model three further ways, each designed for the few-cluster setting, and compare them with CR1. First, a wild cluster bootstrap with Rademacher weights and the null imposed, resampling at the district level over 9,999 replications. Second, randomisation inference: we re-assign the six districts' treatment histories to districts at random 10,000 times and compare the observed coefficient against this permutation distribution, which makes no asymptotic assumption at all. Third, the bias-reduced CR2 variance estimator with Satterthwaite degrees of freedom, the recommended small-sample correction. On the full 1999–2025 series the four methods give two-sided p-values for the regional-nominee coefficient of 0.81 (CR1), 0.82 (CR2), 0.83 (wild bootstrap) and 0.86 (randomisation), and for the provincial-resident coefficient 0.16, 0.22, 0.18 and 0.24. No method places either coefficient near conventional significance; the CR2 procedure assigns the provincial coefficient only 2.1 Satterthwaite degrees of freedom, a direct measure of how little independent information six clusters has. The no-home-bias result is not an artifact of the standard-error convention, but the wide agreement at high p-values reflects a design with limited power, which we quantify next.

What the design can detect. The headline indicators vary little within districts once district

fixed effects are included. The regional-nominee indicator equals one in 87 per cent of district-years and never changes for one district (East Kootenay), so the coefficient is identified off roughly fifteen within-district transitions; the provincial-resident indicator is identified off only four of the six districts, the others never hosting a provincial appointee. With this little movement, the relevant question is not only whether we detect home bias but whether we *could*. To answer it we run a design-based power analysis: holding the real fixed-effect structure and residual variance fixed, we inject a known home-bias effect of a given size onto the regional indicator, regenerate the outcome, re-estimate the joint model, and record how often the test rejects, repeating across effect sizes. The design has essentially no power against small effects (a CAD 100k per district-year effect is detected about seven per cent of the time), reaches roughly 28 per cent power at CAD 500k, about 66 per cent at CAD 1M, and is near-certain only around CAD 2M. The implied minimum detectable effect at 80 per cent power is approximately CAD 1.27 million per district-year, which is below the mean annual community grant of CAD 1.58 million on the full series. The null should therefore be read as ruling out *large* home-bias effects, not as establishing the precise absence of any effect.

Attendance. The treatment codes a district as having an active director from board membership alone, but a director who rarely attends cannot advocate for their district or block a rival, so membership may overstate the effective treatment. To check this we reconstruct an attendance record from the "Directors Absent" lists in the board meeting minutes, taking meeting counts and absences from the full set of 238 board minutes (1995–2024), and keep only the absences of regional and Ktunaxa directors, since the absences of provincial appointees are irrelevant to the regional indicator. We then discount the regional indicator in each district-year by the share of that year's meetings its nominees missed, and re-estimate the joint model. The null survives: the attendance-weighted regional coefficient is about +246 thousand CAD (SE 261, $p = 0.39$), close to the unweighted estimate and statistically indistinguishable from zero. Discounting for attendance does not move the result, which is what we expect if the absence of home bias is genuine rather than an artefact of inactive directors diluting the treatment.

A.4 Inference and Robustness for the 2003 Difference-in-Differences

The staggered binary difference-in-differences reported in Section 4 rests on the same six districts as the district-year model, so the few-cluster caveats apply to it equally. The estimate is a tight null: $\delta = +36$ thousand CAD over the full series ($p = 0.90$ under CR1) and +71 in the 2000–2008 reform window ($p = 0.65$). We subject δ to the same few-cluster checks. The bias-reduced CR2 estimator with Satterthwaite degrees of freedom gives $p = 0.90$ (full) and 0.65 (window); a wild cluster bootstrap with Rademacher weights and the null imposed (9,999 replications) gives $p = 0.88$ and 0.72; and exact randomisation inference, permuting the six districts' treatment trajectories across districts over all 720 permutations, gives $p = 0.91$ and 0.68. No method places δ near significance. Dropping the Ktunaxa Nation, whose seat is coded least reliably, leaves the null intact: $\delta = +136$ thousand CAD ($p = 0.71$) on the full series and +37 ($p = 0.80$) in the reform window.

A.5 Resident Perceptions

Table 6: Survey sample sizes per wave

Year	Total N	% aware of CBT	Familiar N
2010	811	56%	654
2012	815	59%	702
2014	808	–	660
2016	808	63%	644
2018	808	80%	535
2020	808	87%	590
2022	800	81%	573
Total	5,658	–	4,358

Notes: Familiar subsample = respondents reporting at least some familiarity with the CBT's role or history. Awareness column reports those recognising the CBT when prompted; the two are not nested.

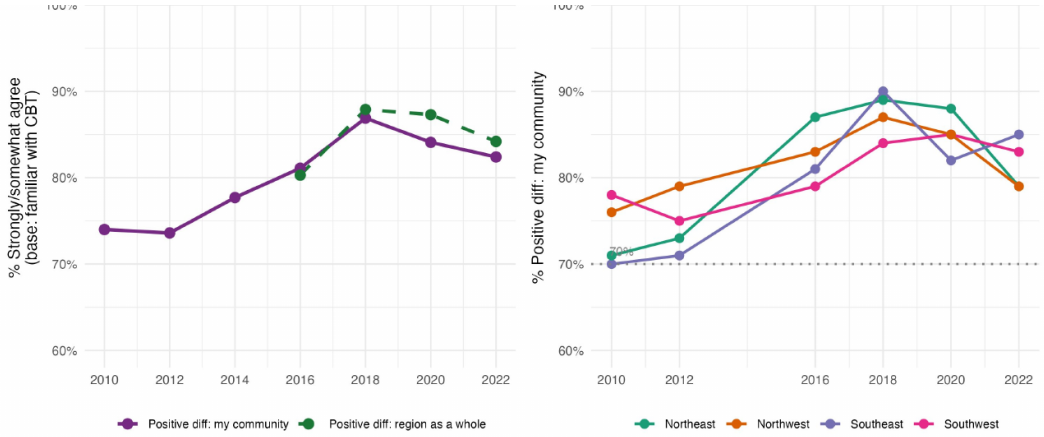

Figure 9: CBT Resident Survey evidence, 2010-2022. Left: percentage of familiar residents who agree the CBT makes a positive difference in their community (solid) and in the region as a whole (dashed). Right: regional breakdown by quadrant. No quadrant is systematically below the others. Cross-quadrant range within any single wave is at most 10 percentage points.

As a demand-side check on distribution we draw on seven biennial resident surveys (2010–2022, $N \approx 800$ per wave; Table 6) covering the four quadrants of the Basin.[10] These data cannot speak to absolute satisfaction, but they bear on distribution through cross-quadrant variation: were some communities systematically short-changed, their residents should rate the Trust less favourably. They do not. Across all seven waves the share rating the CBT a positive difference in their community runs from 70 to 90 per cent with no quadrant consistently below the others, the within-wave spread never exceeds ten percentage points (Figure 9), and an individual-level logistic

[10]The 2010 and 2012 waves were conducted by Synovate/Ipsos; the 2014–2022 waves under the CBT's standard resident survey programme, with a consistent questionnaire. All are CBT-commissioned and so say little about *absolute* satisfaction; we use only cross-quadrant variation.

regression on the 2022 microdata finds no quadrant significantly less positive than the Southeast baseline once demographics are controlled. The demand side echoes the supply side. We read it as corroboration rather than a test, given the commissioned source and the coarse four-quadrant geography, which does not map neatly onto the six grant districts.

B Interview Evidence

This section documents the qualitative interview evidence we draw on in the discussion. The interviews are a secondary, illustrative strand alongside the quantitative design; we use them to qualify claims about the board's internal workings, not as a freestanding test. The full coding scheme is also held in the replication materials.

We conducted a set of semi-structured interviews in the Columbia Basin between April and May 2024 with former Trust directors, participants in the Trust's founding, long-time residents, and a regional author and historian. Interviews ran between roughly forty-five and ninety minutes, were recorded with consent, and were transcribed. Participants were recruited through a combination of board and regional-district records and referral, and the set is purposive rather than representative: it is weighted towards people with first-hand knowledge of the board and the Trust's creation, the questions this paper turns on.

The study holds ethics approval from UZH ETH2324-0852. Participants gave informed consent under an approved information sheet and consent form. We identify participants only by an anonymised label and a generic role; the key linking labels to identities is held securely by the authors and is not part of the replication package, and the transcripts are not redistributed. Where a verbatim quotation would identify a participant or a community, we generalise the identifying term in square brackets.

The interviews drawn on in the text are:

Label	Role	Interview
Participant 1	Former Trust board member	May 2024
Participant 2	Former regional-district director and founding advocate	May 2024
Participant 3	Regional author and historian	April 2024
Participant 4	Former Trust board member and founding-era insider	April–May 2024

Transcripts were coded against eight themes: (1) developmental returns; (2) home bias and capture; (3) the role of non-territorial appointees; (4) consensus and committee deliberation; (5) external versus internal accountability; (6) socialisation, tenure, and norms over time; (7) board composition and the 2003 reform; and (8) disconfirming evidence, coded deliberately so that material running against our argument was recorded rather than set aside. A first coding pass was assisted by a large language model and then verified by the authors against the transcripts; every quotation used in the paper was checked verbatim against its source, with transcription artifacts and identifying terms handled as described above.