The Promise of Foreign Direct Investment: Experimental Evidence from the First Bank in a Native Nation*

Rachel L. Wellhausen† Donna Feir§ Calvin Thrall‡

September 2020

Abstract

Consider a consumer-facing retail bank, from one developing nation, with plans to open a branch abroad in a less developed nation. That host nation is currently a "banking desert" bereft of local bank branches, where citizens rely on predatory substitutes or services from abroad. Moreover, this is its first foreign direct investment (FDI) altogether. An international institution has the specific mission to promote the spread of formal financial services in the nation. The nation's government is not only an avid supporter, but also the bank's lynchpin local customer. Ultimately, stakeholders' interests rely on citizens as customers. We conduct a national survey of citizens under these circumstances: a firm from one American Indian nation is about to break ground on the first bank branch in the citizens' own American Indian nation. Without deception, we use survey experiments to test how support from the Federal Reserve and the tribal government moves individual views in this pre-FDI-treatment setting. We challenge scholars to consider how the body of theory in IPE applies to the full set sovereign actors in the global economy.

*Most of all, we thank our stakeholders and local partners in Native Nation [A], A's tribal college, and Nation [B] (redacted per Nation A's sovereign choice to maintain privacy). We thank students in Innovations for Peace and Development at the University of Texas at Austin for excellent research assistance. We also thank the Center for Indian Country Development at the Federal Reserve Bank of Minneapolis for their support as well as the Board of Governors. None of the views expressed here, unless otherwise stated, reflect those of the Federal Reserve Bank of Minneapolis, nor the Board of Governors.

†Associate Professor, University of Texas at Austin, rwellhausen@utexas.edu

§Assistant Professor, University of Victoria; Center for Indian Country Development, Federal Reserve of Minneapolis.

‡PhD Student, University of Texas at Austin, cthrall@utexas.edu

1 Introduction

Consider a foreign firm interested in investing in a developing nation, as the first entrant into a customer-facing industry otherwise absent: retail banking. Expecting a bank branch in its territory to bring material benefits to its residents, the nation's government not only endorses the venture but has become the branch's lynchpin local customer. An external, foreign organization has an explicit mission to support the provision of formal financial services in the nation. These stakeholders understand that success of the venture requires buy-in from the nation's residents – not just their abstract support, but as the bank branch's customer base. At the same time, the considerably impoverished, poorly endowed, and small nation is effectively a tabula rasa regarding foreign direct investment (FDI). Against this backdrop, stakeholders want to know: is the potential customer base indeed there? From a scholarly perspective: do theories of public attitudes and FDI, developed with reference to very different circumstances, aid our understanding of individual views and behaviors here?

In this article, we analyze such a setting. A groundbreaking ceremony is already on the calendar, to launch construction of the local bank branch building. In the time between the national government's and foreign organization's endorsements and the groundbreaking, we survey residents' attitudes toward the investment, their interests in becoming customers, and whether the promise of a local bank branch corresponds with increased attention to formal financial services.

We find that baseline views on the forthcoming local bank branch are overwhelmingly positive, consistent with the normative hope that FDI with the promise of substantial direct, material, individual-level benefits is a most-likely setting for ex ante buy-in. We find support, too, for previous findings in the literature that friendly ties between the home and host nations correlate with popular support for FDI. However, contrary to implications born of the FDI literature, survey experiments reveal mixed effects of the foreign organization's general statement of support for access to the safe banking services and as well as the national government's endorsement. We probe for underlying heterogeneous effects by respondent discount rates; financial resilience; trust in banks; and community connections. We find the most evidence for heterogeneous effects due to community connections: when those deeply tied to the community receive positive endorsements, they become more likely to support and plan to become customers of foreign-owned Bank [X]. Importantly, such individual-level heterogeneity is invisible to the bulk of FDI theory, which has been built around business-to-business FDI or FDI in the context of business-government contracts. Evidence of this individual-level heterogeneity is also important to stakeholders, given that the hoped-for, nation-

wide benefits of this FDI are built on the back of voluntary, individual-level choices.

Moreover, the nation we examine falls outside of the set of nation-states upon which international relations has been built. Specifically, the FDI-receiving nation [Nation A] governs one of the 326 unique political jurisdictions in Indian Country (as named by the US federal government).[1] First principles behind theories of FDI do not require "foreign" to refer to nation-states with voting seats at the United Nations. FDI results from a firm investing where its presence is, ultimately, at the behest of a host government (Wellhausen, 2017*b*). Governments in Indian Country are just some among the many in the world with the sovereign authority to make choices regarding economic openness. This sovereignty is at the behest of the applicable hierarchical power. The contemporary era of Native-US relations is one of "self-determination," consistent with Nation A's certainty that the US government will not intervene in Nation A's sovereignty over issuing business licenses to non-Nation A businesses seeking to operate in Nation A's territorial jurisdiction (i.e., reservation). Our underlying proposition is that Native nations' limited legal sovereignty is irrelevant to their place in theories of the politics of FDI, meaning that theories should be *internally* valid in these nations.[2] In putting forth this proposition, we are indebted to the large and growing body of scholarship that exposes the artificial limitations international relations theory resulting from excluding indigenous nations from the national unit of analysis (Lightfoot, 2016).

As this article has multiple intended contributions, its organization is not standard. First, we use the context of scholarship on public opinion and globalization to establish that Native Nations are among the national units relevant to IPE. We introduce Native Nation A, and we derive implications of scholarship for individual views on openness to FDI, from Nation A's vantage point. Second, we explain the unique conditions in which we are able to, without deception, study individuals' "pre-treatment" views in the context of Nation A's first FDI entry – of the first retail bank in what is otherwise a "banking desert" bereft of local formal financial services. In doing so, we recount the narrative around the origins of our nation-wide survey and embedded experiments, our stakeholders, and our approval by Nation A's Tribal Legislature. We highlight the ethical as well as legal constraints that shaped the set of hypotheses we were able to rigorously test. Therefore, we present our hypotheses, experimental treatments, and outcome measures third. We simultaneously situate them in the survey instrument for the ease of the reader. Fourth, we report results. Fifth, we consider post hoc explanations for heterogeneous treatment effects. At the same

[1] Nation A's government required anonymity as a condition of publication. There are 573 federally-recognized American Indian/Alaskan Native (AIAN) nations as of September 2020.

[2] We also suspect that it is irrelevant to much of the larger field of international political economy.

time, we reflect on the consequences of our choice to present some untested, possible explanations over others: in presenting these to stakeholders, we move into the field of agenda-setters and policymakers. Finally, we conclude with a call for IPE researchers to acknowledge the biases that result from drawing a sample of nations from only part of the full population.

2 Public Opinion and Globalization: Theory and its Limits

Today's active research agenda approaching individuals as a unit of analysis in the international political economy focuses on the the backlash against globalization in developed countries. Naoi (2020) summarizes the common "globalization-as-treatment" research design: scholars prime globalization as a "source of hardship" and probe which developed-country respondents increase their anti-integration sentiment in response. The overarching question is the extent to which "pocketbook" material interests – the classic IPE starting point – balance against or are swamped by non-material drivers of individual preferences. An important body of work identifies a multitude of non-material drivers of variation in developed-country views, including cultural, status, racial, nationalist, anti-immigrant, environmental, partisan, and other concerns (Ehrlich, 2018; Guisinger and Saunders, 2017; Mansfield and Mutz, 2013; Baker, 2015). In pursuing this developed country "globalization-as-treatment" research agenda, the aspect of the international political economy that transmits the hardship to individuals is typically trade (Owen and Johnston, 2017), although others have examined financial channels (Bearce and Tuxhorn, 2017; Ahlquist, Copelovitch and Walter, 2020) and foreign direct investment (Chilton, Milner and Tingley, 2020). Nonetheless, a common thread through much of the research agenda is the primacy of survey experiments that manipulate globalization-linked hardship and its effects on individual-level, attitudinal variables measuring respondent views (Tingley, 2014).

This body of work gives us a nuanced understanding of international political economy and public opinion in developed countries (Owen and Walter, 2017). But what if this individual-level variation means little for policy outcomes of interest? For example, scholars have found evidence that trade policy is such a low-salience issue in the US that individuals have little understanding or accounting of theoretical predictions regarding their material self-interest (Rho and Tomz, 2017), and that it has little effect on voting or tariff outcomes even among the most materially affected groups (Guisinger, 2009; Betz and Pond, 2019). It is not immediately clear that there are meaningful connections between non-material drivers of public opinion and policy outcomes in developed countries, either. In a review of the literature, Walter (2020) finds that mass public opinion

on economic globalization has in fact been stable and thus unable to account for anti-integration policy changes. Pepinsky (2014) raises the possibility that the considerable resources directed to researching individual-level "empirical microfoundations" in international political economy have been misallocated.

In our view, the shortcoming in the globalization and public opinion literature is not in the research agenda, but that it has overwhelmingly been centered on developed countries. Given the dearth of individual-level "empirical microfoundations" research in very poor developing contexts, it remains unknown whether the concerns in Pepinsky (2014) apply. In the developed context, scholarship focuses on wage and other price differentials as the material factors that generate "pocketbook" effects; scholars have considered these sources of pocketbook effects in developing contexts as well (Pandya, 2013; Ardanaz, Murillo and Pinto, 2013; Steinberg and Nelson, 2019). Yet material factors that could shape individual views on openness run deeper in developing countries. A growing body of research finds that economic globalization has material effects on potable water access (Rudra, 2011; Rudra, Alkon and Joshi, 2018), food security (Ballard-Rosa, 2016), fuel availability (Cheon, Lackner and Urpelainen, 2015), sanitation (Post, 2014), access to medicines (Sell and Williams, 2020), pollution (Spilker, 2013), revenue allocated to welfare policies (Bastiaens and Rudra, 2018), and more. Further, a number of non-material factors that vary little in the developed country context have been linked to public opinion in developing contexts: colonial histories (Arias and Girod, 2014), experiences with different bilateral or multilateral actors (Wellhausen, 2015; Findley et al., 2017), links to domestic governance quality (Mihalache-O'Keef, 2018; Bodea and LeBas, 2016), issues of fairness and exploitation (Weitz-Shapiro and Winters, 2017), and more.

In addition to pointing us to other explanatory variables, recentering the research agenda on developing countries reminds us that globalization promises economic growth, technological advancement, and other benefits that are less salient in developed contexts, or even middle-income emerging-market contexts (Rudra and Tobin, 2017). The grandest promises of IPE phenomena including FDI as drivers of economic development have fallen short (Pandya, 2016). Nonetheless, it is not obvious that the assumption of "globalization-as-hardship" is transferrable to a developing context in which material and non-material factors could be linked to pro-openness views.

Consider in particular a very small, less developed nation that has very few economic connections to foreign nations. Its exports are primarily low-value-added commodities. The government has little-to-no access to borrowing via capital markets; it relies on aid and state-to-state lending.

Domestic financial markets are shallow-to-nonexistent. And, the nation is effectively a tabula rasa regarding FDI, consistent with its poor endowment, small market, and low GDP per capita. This nation has not actively regulated against international economic ties; it has simply been so far bypassed by them.

What does scholarship on individual views and IPE imply for such a setting that is effectively pre-globalization? First, "globalization-as-backlash" findings rely on mechanisms born of long-standing and deep economic integration. Backlash emerges from widespread distributional effects that aggregate over time until they break through into politics; it is not quick. One implication is that a nation without accrued, politically problematic distributional effects, should skew toward high levels of individual support for economic globalization. Still, any given cross-border transaction has the potential to spark backlash. For example, unchecked dumping can put a domestic firm or entire domestic industry out of business. But in this less developed country setting, the general dearth of domestic producers mitigates import-competing firm- or industry-initiated protectionism. In the case of FDI in a customer-facing industry, benefits to individuals as consumers would likely far outweigh squeezing-out effects faced by the few (if any) domestic competitors. Many variables commonly raised by open-economy-politics-based theories in IPE are likely irrelevant to explaining variation in public opinion. Non-material factors found to be significant drivers of public opinion in post-globalization national settings may also be less relevant. In a small less developed nation in which even efficiency-seeking FDI is absent, explanations are unlikely to be rooted in xenophobia, environmental concerns, labor rights, and related concepts. Taken as a whole, the material and non-material factors linked in the literature to public opinion rely on lived experience with globalization's distributional effects.

Considering the determinants of public opinion on globalization in a pre-globalization setting is not an idle exercise. Such nations do exist in the world. Drawing on these nations' unique status as effectively outside the deeply integrated global economy provides an unexploited opportunity to push our theories. If the true population of nations in the world includes both pre- and post-globalized nations, and we sample only from the globalized set, we introduce bias.

We specifically advocate for American Indian/Alaskan Native Nations as among the national units relevant to IPE. Native Nations govern 326 unique political jurisdictions in Indian Country, and they have – and continually fight to preserve – considerable sovereign authority (Evans, 2011*a*; Spirling, 2012; Evans, 2014). In our setting, the most crucial piece of that sovereign authority is the right to control the entry of foreign businesses into their territorial jurisdictions. We ac-

knowledge that reducing native sovereignty to this one, very narrow slice sits uncomfortably with broader research agendas in indigenous-centered international relations that aim to reconceptualize sovereignty in ways that recognize tribes as sovereigns, full stop (Bruyneel, 2007). In the American context, Evans (2011*b*) treats sovereignty as dynamic: she explains tribes' strategic use of "salami tactics" in their interactions with the US federal government to expand, or at least preserve, facets of sovereignty. We intend with our focus on a well-established "salami slice" of sovereignty in Indian Country to provide a straightforward example of why we should examine the internal validity of theories of FDI in Native Nations.

We rely on a first-principles definition of FDI as investment by a firm originating from another political jurisdictions that crosses the border into a host nation. Native Nations' inward FDI commonly comes from multinational firms from the US as well as other nations – including multinationals that originate in one Native Nation and invest in another.[3] Of course, there is considerable variation across these Native Nations' political economies and integration. Therefore, while we introduce the sovereigns in Indian Country as an overlooked part of the population of IPE-relevant nations, we turn to one nation in particular to put a name on our hypothetical small, less developed, effectively pre-globalization nation.

3 Political Economy in a "Banking Desert": Native Nation A

Native Nation A is a federally-recognized tribe located in a Midwestern state, with less than 15,000 tribal members and descendants (i.e., citizens). Nation A has the sovereign right to designate citizenship; tribal membership is based on a blood quantum rule, and recognition as a descendant is based on a loosened standard. In terms of the set of individuals to which Nation A's government is accountable, and the potential customer base for a local bank, the member/descendant differentiation is not relevant in our setting. Nation A is a parliamentary democracy with two branches of government, the legislative and the judiciary, as set out in its constitution. Consistent with their sovereign rights, Nation A's government chose not to divulge information about turnout for tribal electons, although tribal leaders did characterize it as "very low" to the co-authors.[4] The Nation A Tribal Legislature is the democratically elected, governing entity that has full and final approval over applications to begin businesses on the reservation.

Nation A is among the most impoverished Native nations. As documented in Table 1, Nation

[3]For Australian FDI, see: Kimbrell, Nick. 31 July 2015. "The Apache v. Rio Tinto." The Nation.

[4]Based on US voting data, we infer that tribal member turnout in federal and state elections is lower than the state average, and a large majority consistently prefer Democratic candidates.

A's development level is considerably lower than average levels across all AIAN individuals. Nation A's poverty rate is 11.7% higher than the AIAN-wide average; a greater proportion of its (age 25+) population is educated only at the high school level or lower; and median household income is lower (at 85% of the AIAN average). Disparities between Nation A and the United States as a whole are considerably starker. Most troublingly, life expectancy of 60.3 years in Nation A is 12.7 years less than the AIAN average, which itself is 5.8 years less than the US average. For comparison, the lowest life expectancy in world regions is 61.2 years in Sub-Saharan Africa (WHO 2017).

Table 1: Comparison of Nation A, AIAN, and United States Development Indicators

	Nation A	**AIAN Avg.***	**United States**
Poverty rate	37.9%	26.2%	14.6%
% ≤ High school education**	56.3%	45.9%	37.9%
Median household income	$33,836	$39,719	$57,652
Life expectancy***	60.3	73.0	78.8

Notes: *AIAN Avg. = Average values for individuals self-identifying as AIAN. **Of adults age 25+. ***2013-2016 average. Sources: Tribal records, Indian Health Service, US Center for Disease Control and Prevention, American Community Survey conducted from 2013-2017.

A common way to get at financial precariousness in US surveys is to ask whether a respondent could come up with $400 in case of an emergency. A "yes" answer implies that the respondent either has at least $400 in savings or is confident that they could borrow the money, whether formally or informally. In 2019, 12% of Americans answered "no," that they could not do so.[5] We asked the same question of our Nation A respondents in January-March 2020 – when the US economy exhibited strong financial indicators, and before pandemic-related unemployment and economic crisis came about. Disturbingly, 31% of our respondents answered "no," and a further 16% were unsure. This finding suggests that even predatory non-bank lending is not meeting the needs of Nation A's population.

Nation A is characteristic of a long-standing problem of limited access to capital in Indian Country, in which historical limitations on capital access trace through to current limitations (Brown, Cookson and Heimer, 2019). There is compelling evidence, for example, that tribal members pay higher interest rates on mortgage debt (Cattaneo and Feir, 2019; Wellhausen, 2017*a*); that they have lower usage of credit and credit scores (Dimitrova-Grajzl et al., 2015); and that they face discrimination from US financial institutions (Guedel and Colbert, 2016). Much of this may be attributable to the uncertainties inherent in international economic transactions, whether due to

[5]Federal Reserve Board's 2019 Survey of Household Economics and Decision-making (SHED).

political risk or foreign providers' poor understanding of domestic commercial laws, regulations, and enforcement (Akee et al., 2010; Wellhausen, 2017*a*).

Worldwide, 31% of adults are "unbanked," meaning that they do not have an account with a financial institution or through a mobile money service.[6] In high-income countries, there are on average 1081 financial accounts per 1000 adults, compared with 88 in low-income countries (Dimitrova-Grajzl et al., March 2017). In the US, 6% of households in the United States do not have any financial account, and 16% are "underbanked," in that they have at a account but still rely on alternative credit sources.[7] The most important sources of non-bank credit in the US are credit cards: three-quarters of American adults have at least one credit card.[8] While expensive, credit card debt typically carries lower interest rates than other non-bank alternatives.

Of our Nation A respondents, 33% report being "unbanked" without bank accounts, and 50% of those with bank accounts report having debt from alternative credit sources or use check cashing services consistent with being "underbanked."[9] Of particular note is that only 44% of respondents have a credit card. Thus, the majority of respondents do not even have the option to accrue relatively expensive credit card debt, rather than access more predatory services.

Some 84% of Americans visited a physical bank at least once in the last year, and almost all did more than access the ATM, even though half use online or mobile banking.[10] Worldwide, 22% of unbanked adults report that physical distance from financial institutions is a barrier (Demirguc-Kunt et al., 2018). The closest physical bank to Nation A is a regional bank branch about 10 miles away, on roads that are difficult to drive in the winter. Only a handful of ATMs are located on the reservation, although a common complaint is that zero to two ATMs are operational at any moment. Moreover, only two ATMs can be accessed without fees by members of two different foreign (US regional) banks. Nation A's infrastructure limits the possibility of internet-enabled substitutes; for example, the second biggest town is not covered by cell service.[11]

In his 2006 Nobel Peace Prize acceptance speech, Muhammad Yunus points to the role globalization can play in the process of expanding financial services in underserved communities: "Powerful

[6] Demirguc-Kunt et al. (2018). Only 28% of unbanked adults report having saved in any way in the last year. Sub-Saharan African countries are global leaders in mobile money accounts, with 21% of adults reporting having one, half of whom do not have traditional commercial bank accounts.

[7] Sweet, Ken. 23 October 2018. "Americans who don't have a bank account at lowest level ever." Associated Press.

[8] As of 2018. See: Foster, Greene and Stavins, "The 2018 Survey of Consumer Payment Choice: Summary Results." *Federal Reserve Bank of Atlanta Research Data Reports*, No.19-02, 2019, Figure 2.

[9] Including auto title, payday, and non-bank loan debt.

[10] Merry (2018). Lower-income and older users were among the most likely to visit a branch.

[11] 91% of US urban areas have at least 10 Mbps/3 Mbps mobile LTE broadband, compared to 64% of tribal areas (Commission et al., 2018).

multi-national social businesses can be created to retain the benefit of globalization for the poor people and poor countries" (Yunus, 2007). Yunus further advocates that "credit is a fundamental human right." If economic advocates can improve the provision of what Yunus labels a human right, the stakes around the success of a foreign-owned retail bank branch in a pre-FDI "banking desert" are very high. It is thus normatively important, in addition to an opportunity to push IPE theory, to understand how tribal members-as-customers view such an unknown quantity, and how stakeholders with interests consistent with the foreign-owned bank's success might effectively reinforce it.

4 Background, Stakeholders, and Approval

We take a moment to explain the unique conditions in which we are able to, without deception, study individuals' "pre-FDI-treatment" views in the context of Nation A's first FDI entry – of the first retail bank in what is otherwise a "banking desert" bereft of local formal financial services. In doing so, we recount the narrative around the origins of our nation-wide survey and embedded experiments, our stakeholders, and our approval by Nation A's Tribal Legislature. We highlight the ethical as well as legal constraints that shaped the set of hypotheses we were able to rigorously test.

4.1 Background

Over the last 30 years, Nation A has had many conversations about attracting a branch from a foreign (i.e., non-Nation A) retail bank, or subsidizing a private venture owned by a tribal member, but with little progress.[12] In a similar time frame, nearby Nation B has grown considerably richer and more developed than Nation A.[13] Nation B's urban location helps to make its casino and hotel much more profitable than Nation A's rural competitor. Like many wealthier tribes, Nation B has increased its focus on diversifying out of reliance on gaming, given gaming's uncertain future. It charted a state-owned Bank [X], which serves its own tribe and has expanded services to American (non-Native) communities in the region. Bank [X] is formally incorporated and licensed in the US and is subject to US banking regulations; however, it originates in Nation B given its 100% ownership by Nation B's government. In the context of its interest in expansion, its board's

[12] An elder who has long been involved with tribal government recounted the story of a foreign bank that "tested the waters" in the 1970s, but was robbed and exited (Author interview, June 2018).

[13] Nation A's government has borrowed money from Nation B in recent years, although our local partners believe that this is not widely known.

recognition that its identity likely provides its competitive advantages in serving Indian Country customers, as well as social motivations, Bank [X] formally inquired about opening a branch in Nation A.

In testimony before the Tribal Legislature, Bank [X]'s CEO has described that Nation B's commitment to development in Indian Country is a key motivation behind its willingness to take on this investment risk. At the same time, he has made it clear that Bank [X]'s investment needs to be commercially viable.[14] It took well over a year from Bank [X]'s initial inquiry to a positive (unanimous) vote from the Legislature to allow its entry. Much of this time lag was due to extensive negotiations between the tribal government and Bank [X] over the terms of the investment. Points of consideration included documenting commitments from Bank [X] consistent with Nation A's development priorities. Additionally, the parties negotiated legal protections for Bank [X]'s property rights in this foreign jurisdiction. It is public knowledge that Bank [X] also required the tribal government to move its finances to Bank [X], serving as Bank [X]'s lynchpin Nation A customer.[15] The official, final motion from the Tribal Legislature confirming Bank [X]'s entry came in early 2019.

Bank [X] had been considering conducting an informal survey in Nation A to collect citizens' feedback on what banking services are of most interest. Inspired by Bank [X], our research team came together to consider how a more formal survey with embedded experiment could be of local and scholarly use. This would be the first formal, non-Census survey on the reservation that would elicit information from tribal members on their financial situations, their views, and their preferences over financial development choices made by their government. To be clear, Bank [X] is not a partner in the survey nor part of the research team. This is consistent with Federal Reserve requirements, federal bank regulations, and the ethical standards and legal obligations of both Bank [X] and the co-authors. When the research team sought approval from the Tribal Legislature, legislators asked for Bank [X]'s endorsement of our skills and the usefulness of the research; Bank [X] representatives appeared beside and on behalf of the research team for those purposes at government hearings. Bank [X] specified in its testimony that its investment in a retail branch on the reservation is independent of the survey and was not contingent on its approval. Bank [X] completed formal commitments to the Tribal Legislature while the survey approval process was ongoing.

[14] Legislative session, June 2019.

[15] Consistent with Nation A's sovereign authority, it chose not to disclose to the research team the final terms.

4.2 Nation A Approval

The lynchpin local formal requirement to roll-out the survey was a resolution and positive vote from Nation A's Tribal Legislature, which itself required approval from Nation A's Language and Culture Commission. Earning these approvals included presenting the draft survey to the Legislature; in-person testimony from members of the research team at several legislative sessions; and iterated revisions to survey questions consistent with legislators' requirements.[16] The most relevant required revisions included cutting standard political-science-research voting and political ideology questions, which several legislators saw as violations of privacy and sovereignty. We also cut replications of questions from national surveys intended to gauge financial knowledge that were viewed by some legislators as simplistic and in that way disrespectful. Subsequent to approval by Nation A's government, the Institutional Review Board (IRB) at Nation A's tribal college also approved the survey.[17]

Nation A has the sovereign authority to maintain the privacy of its data.[18] Consistent with their requirements, this article is anonymized and refers to the tribe's location in a "Midwestern state," (host) "Nation A", (home) "Nation B", and Nation B's state-owned enterprise Bank [X].

4.3 Implementation Decisions

The co-authors assembled a research team in collaboration with Nation A's tribal college, which took responsibility for the survey roll-out and enumeration. The co-authors raised funds via their affiliate institutions; we compensated our partners at the tribal college; and Nation A contributed no funds.[19] Our implementation decisions were driven by scientific best practices in conjunction with meeting our stakeholders' needs, and our own deeply considered ethical concerns. Overall, our survey instrument itself was designed to accomplish two goals. First, it was important that we collect comprehensive data on respondents' knowledge of, use of, and opinions about personal finance. To this end, the survey contains items from other surveys conducted on these topics in

[16] As such, tribal leaders reviewed the full set of questions, including each of our randomized treatments. To account for potential confounding if/when they completed the survey, we check robustness to a set of controls accounting for those involved in the process, while maintaining anonymity (i.e., employment by the tribal government and prior knowledge of Bank [X]'s opening).

[17] We also gained IRB and equivalent approval from the co-authors' university and the Center for Indian Country Development at the Federal Reserve Bank of Minneapolis.

[18] Data sovereignty is a salient issue throughout Indian Country; see the US Indigenous Data Sovereignty Network (https://usindigenousdata.org/). We provided a non-anonymized report to Nation A's government, the tribal college, and to Bank [X]. Nation A's government has control over the dissemination of that report. To repeat, Bank [X] has no privileged access to data or analysis.

[19] The tribal college benefitted from a charitable donation from Bank [X], consistent with US and Nation A charitable laws.

the US.[20] Second, we embedded experiments in the survey. As laid out in Section 5, given the lack of retail banking or FDI in Nation A, we focus on how endorsements from institutional actors with applicable expertise might move individuals more in favor of Bank [X], and more likely to report that they plan to become a customer of its branch. We committed to doing so without deception, by incorporating true statements from the Federal Reserve (the international institution in this context) and the Nation A Tribal Legislature into the survey instrument.

To implement the survey, we deferred to our tribal college partner to hire ten enumerators, prioritizing our partner's judgement of candidates who would commit to this novel endeavor.[21] The competitive process resulted in ten enumerators who are all former or current tribal college students and all women, none of whom had previous enumeration experience. Their formal training centered on a series of in-person sessions led by the co-authors, in which content included best practices, role-playing, and group brainstorming as to how to mitigate potential biases.[22] Each enumerator was equipped with and trained on a tablet computer to conduct surveys via the offline Qualtrics app (Bush and Prather, 2019).[23] Enumerators set up stations in high-foot traffic areas on the Nation A reservation, including the casino lobby, which is a typical space used for community events; the (only) on-reservation grocery store; the health clinic; senior centers, the state job center located at the tribal college; government offices; and the main tribally-owned enterprise during shift breaks.[24] Enumerators took the initiative to leverage their links to many places and people in the community, including: the main on-reservation bar and restaurant; schools and day cares; NGOs and tribal committees; small businesses; tribal elders; disability care services; drug and alcohol rehabilitation services; current and formerly incarcerated tribal members; and tribal social media "influencers."

We instructed enumerators to use convenience sampling, rather than selecting potential respondents randomly or randomly within demographic strata. We provide three justifications for our convenience sampling strategy. First, the Tribal Legislature required as as a condition of approving the project that as many people from their community as possible in the survey.[25] Second, our

[20] Specifically, from the Consumer Financial Protection Bureau and the Financial Industry Regulatory Authority.

[21] Enumerators were paid a wage of $15/hour; the average on-reservation wage is around $9/hour.

[22] Enumerators also completed an online ethics course provided by our affiliate university.

[23] We provided the incentive that, upon collecting 100 responses, the enumerator could keep the tablet. Given that the pandemic caused us to abruptly stop the survey, we allowed all enumerators to keep their tablets.

[24] Our tribal college partner organized a large initial roll-out in the casino lobby, with free breakfast and lunch. Unexpectedly, the casino donated $5 match play coupons to respondents on the day. Results are robust to a casino fixed effect.

[25] High-quality, community-wide survey data is often lacking in Native communities. For example, American Indians/Alaskan Natives are historically the most undercounted racial group in the US Census. See Ben Kessler, "Native Americans, the census' most undercounted racial group, fight for an accurate 2020 tally," *NBC News*, 29

enumerators helped us settle on a $10 gift card to the (only) on-reservation grocery store as an effective form of compensation for survey respondents, which had the added benefit of keeping funds in the local economy. Our enumerators assured us that, in this small and highly impoverished nation, news of this incentive would travel fast. We therefore saw it as a high risk that randomly denying some tribal members the opportunity to receive a gift card would generate unpredictable confounders via resentment or other mechanisms. Third, methodology aside, the co-authors believed that implementing randomization – thereby forcing enumerators to prevent fellow tribal members from having their voices heard – was simply inappropriate. We show in Section **??** that our sample is still plausibly representative. In our empirical analyses, we confirm robustness to enumerator fixed effects.[26]

The survey was intended to run from January through May 2020, which would be approximately one month before Bank [X]'s scheduled groundbreaking. After that time, Nation A would no longer be in a credibly pre-FDI context. We of course stopped the survey abruptly in March, consistent with public health priorities around the arrival of the COVID-19 pandemic. Nonetheless, we collected 982 high-quality responses from the target population, adult (18+) Nation A enrolled members and recognized descendants, which gives us sufficient power to test our hypotheses. For its part, Bank [X] delayed its scheduled groundbreaking until finally able to hold a socially-distanced event in early summer 2020.

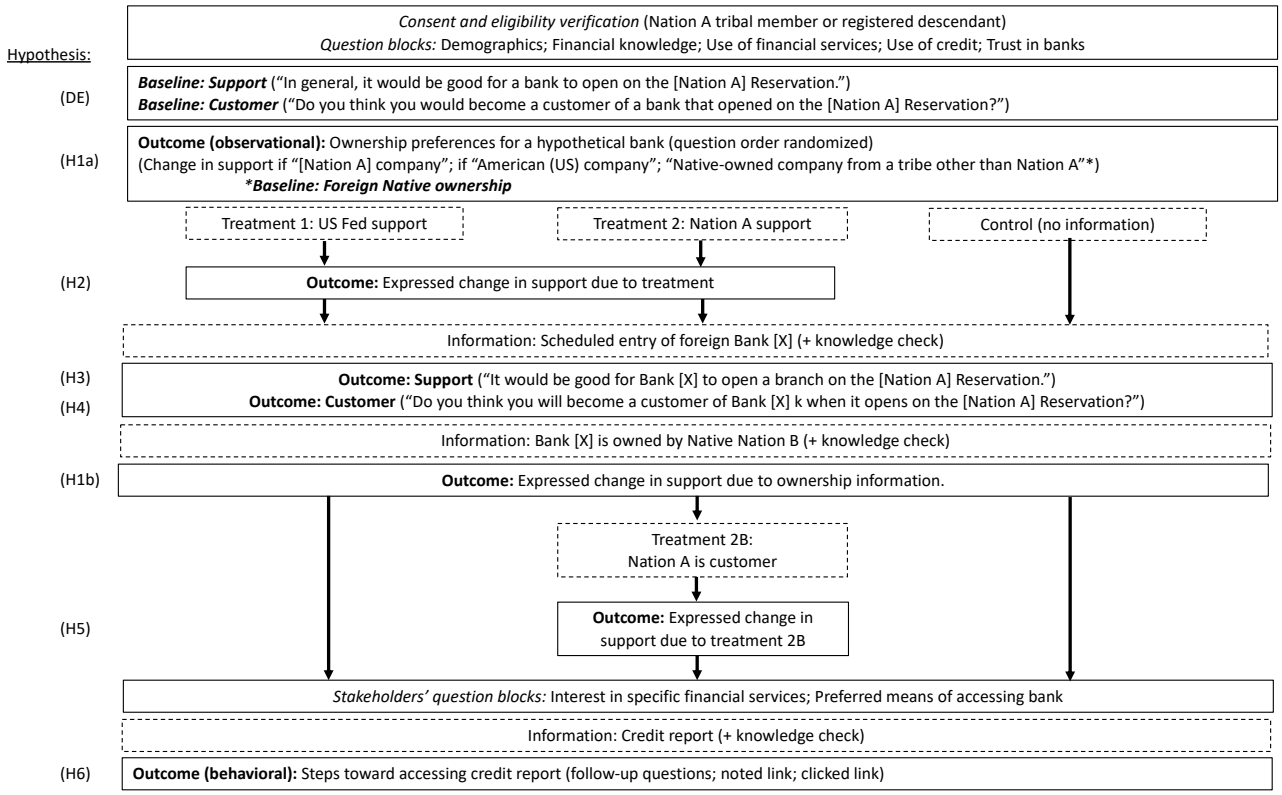
5 Hypotheses and Survey Flow

In this section, we present our hypotheses, experimental treatments, and outcome measures. We simultaneously situate them in the survey instrument for the ease of the reader. Figure 1 illustrates the flow of the survey. The questions that form the basis of our outcomes of interest are labeled with the relevant hypothesis on the left-hand side of the figure. Dotted boxes indicate the points at which the survey introduced different pieces of information. After randomization occurs, the two treatment groups and the control group follow the associated vertical arrows. Note that the group receiving Treatment 2 receives a follow-up treatment 2B toward the end of the survey. As explained in Section 5.3.1, we were not able to do a parallel second treatment for the group receiving Treatment 1 without introducing deception.

December 2019.

[26] The fixed effect for one particular enumerator is consistently significant. We expected as much, as we intervened with retraining on several occasions to mitigate likely social desirability bias given her personal enthusiasm about Bank [X].

Figure 1: Survey Flow



At the start of the survey, enumerators would verify a respondent's eligibility for the survey with a tribal ID and/or other identification. The enumerator would then assist the respondent in completing the survey on the tablet computer to the extent required.[27] If the respondent desired, enumerators would share a link to allow them to complete the survey on their own device (12% of surveys). Respondents could also complete the survey at the state job center with the lab attendant filling the enumerator role attendant (2% of surveys). The survey instrument contained 50-57 items, and the average time to completion was approximately 15 minutes.[28] Enumerators distributed $10 gift cards to the on-reservation grocery store to those who completed the survey.

The survey begins with blocks of questions dealing with demographics; financial knowledge; use of financial services; use of credit; and trust in banks. These blocks are followed by our main questions of interest. The last block of questions in the survey consists of questions specific to

[27] Enumerators recorded their extent of intervention in holding the tablet and entering values.

[28] The exact number of items depended on the experimental condition to which the respondent is assigned and the respondent's answers to certain demographic questions that could trigger follow-up questions. The 15 minute average excludes outliers resulting from enumerator errors.

the needs of our stakeholders, including questions about respondents' priorities regarding financial services and their preferred means of accessing a local bank branch. We walk through our questions of interest and their motivating hypotheses in the next sections.

5.1 Descriptive Expectations

Questions capturing pre-treatment views provide information relevant to our expectations about descriptive patterns in the data. The BASELINE: SUPPORT question asks the respondent's level of agreement with the following statement: "In general, it would be good for a bank to open on the [Nation A] Reservation" (1/disagree to 10/agree scale). This is immediately followed by the BASELINE: CUSTOMER question measuring the respondent's self-reported likelihood that they would become a customer (1/definitely no to 5/definitely yes scale).[29] Note that we are unable to ask about respondents' behavior in actually becoming customers, which we see as a reasonable tradeoff against the opportunity to survey them pre-FDI-entry.

In a highly impoverished less developed nation without a retail bank located in its borders, our expectation is that answers to these baseline questions will skew toward the top of the available scales. We also expect such positive views to be widely shared across the nation, given the absence of longstanding experience with FDI and its possible downsides. This implies that variance on these questions should be low. We set these descriptive expectations aside with what we label DE, given that these expectations are not hypotheses, as there is no comparison national setting against which to test them.

Descriptive Expectations (DE): *Respondents' (pre-treatment) baseline support for a local bank will skew toward top of the scale, and variance should be low. The same should be true of respondents' (post-treatment) support for Bank [X].*

These descriptive expectations suggest that we are more likely to face empirical challenges raised by ceiling effects rather than floor effects. They also suggest that treatment effects may be small in magnitude in terms of movement on the relevant scale. In part because of these prior expectations, several questions ask respondents directly whether and in which direction a prompt changes their support. This allows even the most (least) enthusiastic respondents to express even more (less) enthusiasm without censoring.

[29] We exclude 29 respondents who report that they are already customers of Bank [X], given that resulting biases are uncertain.

5.2 National Origin

Because of power considerations, our questions on national origin are observational and not experimental. Before introducing the specifics of Bank [X], we ask all respondents their opinion on the extent to which different national origins of the owner of a hypothetical on-reservation bank would cause their support to increase, decrease, or stay the same (1-5 scale). All respondents consider the same three kinds of owners: a "Nation A-owned company," "a Native-owned company from a tribe other than Nation A," and "an American (US) company." Our corresponding hypothesis follows from broadly supported expectations in the FDI literature that domestic firms are preferred over foreign, all else equal. Additionally, we posit that firms from countries with closer ascriptive or cultural ties to the host nation may be preferred, although this is conditional on the absence of belligerent bilateral relations (Wellhausen, 2015). Given that Native-US federal government relations have been problematic over history, to say the least, we expect that American (US) firms are the least preferred, all else equal.

Hypothesis 1a. *Respondents are more likely to support a Nation A-owned company over a Native-owned company from a tribe other than Nation A, and both types of Native-owned companies over an American (US) company.*

We chose the label "American (US) company" in consultation with our local partners, so as to establish that this is a non-Native company from the United States, without implying that Native companies are not themselves American. The order in which respondents received the three items is randomized in order to avoid issues with order effects.

As shown in Figure 1, we return to the issue of national origin later in the survey (H1b). This is after we have moved from discussion of a hypothetical bank to the specific Bank [X]. We inform all respondents that Bank [X] is 100% owned by Nation B.[30] We then ask whether that information changes their support.

Moving from a hypothetical to the specific Nation B and its specific Bank [X] introduces a variety of potential mechanisms other than those tied to nationality that could change respondent views. As confirmed by our tribal partners, much about Nation B is familiar to those in Nation A. There is not a history of conflict between Nations A and B; in the mid-1900s, Nation A took actions to support Nation B during a difficult period in B's relations with the US federal government. It is certainly well known that Nation B's economic success, particularly through its casino, translates

[30] Wherever relevant, we include knowledge check questions. We examine these in the Appendix.

into a very high per capita disbursement to its (considerably larger) membership, compared to the low-to-no per capita disbursement in Nation A.[31] In general, Bank [X]'s well-respected, jovial CEO is Nation B's face on Nation A's reservation. He seems to know the name of everyone walking by – and to have an inside joke with most of them. These kinds of characteristics could reasonably affect Nation A respondent views separately from a Native-ownership effect.

Nonetheless, an increase in support for Bank [X] when Nation B is identified as its owner would be consistent with the general expectation that individuals on average prefer FDI from nations with closer cultural ties.[32]

Hypothesis 1b. *Respondents increase their support for Bank [X] following information that it is owned by Nation B, relative to their support prior to receiving that information.*

Note in Figure 1 that the question relevant to H1b is asked post-treatment. We do not expect there to be heterogeneous effects across the treatment and control groups.

5.3 Survey Experiments: Endorsement Treatments

In brief, our survey experiments are built on the theoretical expectation that, particularly in an information-poor environment, credible endorsements can have a causal effect on individual views. In this sense, our setting without a retail bank or FDI is a most likely one in which credible endorsements would increase support. By "endorsements," we mean statements of support from institutional actors with relevant expertise. In this case, that expertise would be relevant to the expansion of a foreign-owned retail bank into Nation A. Our underlying presumption is that the views proffered by such institutional actors can carry sufficient credibility to move public opinion. Our unique setting allows us to specify two institutional actors that, in theory, are likely to be credible sources of information for people in Nation A. Indeed, the fact that these actors support the principle of expanding formal financial services into Nation A, and have made public statements to that effect, would suggest that they think (hope) such statements would have positive effects.

5.3.1 Treatment 1: US Federal Reserve Support

First, the Federal Reserve "supports the expansion of safe and accessible retail financial services for underserved populations and minority communities." This is a true statement from the

[31] Per capita disbursements to tribal members are common in Native Nations, especially since the rise of the gaming industry. Consistent with Nation B's sovereignty over its data, the value of their per capita payment was not disclosed to the co-authors.

[32] At the same time, we acknowledge extreme variation in Indian Country as to perceptions of those cultural ties and the extent to which tribes share a common identity.

Federal Reserve Bank Board of Governors, which they agreed to have in the survey. This statement does not mention, nor is it specific to, Nation A and Bank [X]. Consistent with the Federal Reserve's ethical standards and legal requirements, the Board of Governors endorses the principle of expanding formal financial services in underserved areas, but it does not endorse any specific retail bank. Nonetheless, this statement clearly encompasses Nation A, which is an underserved, minority community. As Indian Country is part of the United States, promoting the stability and economic health of Indian Country is part of the Federal Reserve's core mission.

The Federal Reserve is an institutional actor that objectively has expertise relevant to this setting, which fits with our definition of an endorser with views that could carry sufficient credibility to move public opinion. But what does the Federal Reserve mean to a given individual in Nation A? In short, we certainly do not expect any respondent to know, much less understand, the Federal Reserve's complicated status as a unique non-profit entity that is not strictly private nor is it a part of the US federal government.[33]

Without deception, we provide context around the Federal Reserve's statement with the intention of helping the respondent interpret the endorsement. The treatment begins: "We would like you to know that the Central Bank of the United States, the Federal Reserve, supports..." We expect that the mention of the United States in the treatment emphasizes that the Federal Reserve is an external international institution, consistent with the fact that it is not under the control of Nation A. The treatment specifically categorizes its international status as bilateral, a US-government-affiliated institution – therefore subject to the baggage that the US government carries in Indian Country.[34] Our theoretical expectation is that bias due to a US government identity would move treatment effects in a negative direction. Therefore, we expect that our framing of the Federal Reserve as will make it more difficult to find support for our hypotheses. The net empirical effect – whether Federal Reserve "branding" is in itself counterproductive – is of practical interest to the Federal Reserve. We label this treatment US Fed support.

[33]The twelve Federal Reserve Banks are separately incorporated and their employees are not government employees. Its Board of Governors is appointed by the president and confirmed by the Senate, and its mission and structure are determined by the Federal Reserve Act. The Federal Reserve is a non-profit, because it is funded by its operations and returns all funds in excess of its operations to the US Treasury.

[34]To be frank, respondents likely gloss that the Federal Reserve is part of the US federal government; regardless, its legal status in the US system is immaterial for our purposes.

5.3.2 Treatment 2: Nation A Government as Supporter and Customer

Our second treatment recounts a true statement provided by the government of Nation A: "We would like you to know that the [Nation A] Tribal Legislature supports the opening of a bank on the [Nation A] Reservation." Nation A's government is an endorser per our definition, as an institutional actor with expertise relevant to the expansion of a retail bank into Nation A. The reality of politics is that public support for national government preferences – underpinned by elected political leaders – can be fickle. Dissatisfaction with the endorser would make it more difficult for us to find positive treatment effects. Again, the net empirical effect is of practical interest to Nation A's government. We label this treatment GOVT A SUPPORT.

We take advantage of the opportunity to augment the Government A treatment, as a means of (non-experimentally) probing whether a more robust endorsement affects the presence or magnitude of treatment effects. Toward the end of the this treatment group's survey instrument, we inform them of the following true statement: "We would like you to know that the [Nation A] Tribal Legislature voted unanimously to move all of the Tribe's banking services (excluding investments and 401k) to Bank [X]."[35] Upon receiving this, the Government A treatment includes both abstract support and specific information that the Government is a customer of Bank [X]. We label this augmented treatment GOVT A SUPPORT + CUSTOMER.

5.4 Endorsement Effects on Support and Customer Likelihood

To be commercially successful, the foreign firm in our setting requires buy-in from the people of Nation A both in principle and as customers, such that public views on both are of theoretical and practical importance. We therefore examine treatment effects with regard to both outcomes. One set of questions in the survey asks respondents to express their level of support by considering whether "it would be good for..." (1/strongly disagree to 10/strongly agree). Each support question is immediately followed by a complementary question: "Do you think you [would/will] become a customer of..." (1/definitely not to 5/definitely yes). The verb in the customer question changes based on whether the question refers to a hypothetical or the actual upcoming entry of Bank [X].

Our primary expectation is that the FED SUPPORT and the GOVT A SUPPORT treatments increase respondent support for a local bank in principle, and a respondent's expressed likelihood of becoming a customer of a hypothetical local bank.

[35] As introduced above, this was required by Bank [X], but we do not include that requirement in the statement.

Hypothesis 2. *Respondent support for a local bank will increase following statements endorsing the principle of a local bank from the (1) US Federal Reserve or (2) Nation A's government, relative to the control group.*

Next, we expect both treatment to have positive effects when the local bank in question is identified as Bank [X] specifically. We introduce Bank [X] as such: "Now we would like to ask you about a specific bank called Bank [X]. Bank [X] is a commercial bank based in [nearby US, off-reservation city]. Bank [X] is scheduled to open a branch on the [Nation A] Reservation in 2020." The information that Bank [X] is based outside of the Nation A reservation (i.e. abroad) is intended to communicate to the respondent that it is not a domestic bank, without specifically establishing who its owners are.

Methodologically, changing from a hypothetical to a true, concrete situation could reduce noise in question answers if, for example, respondents' attention increases. At the same time, specifying Bank [X] raises the possibility of new confounders that, if not balanced across the treatment groups, would mean that subsequent changes in views could be misattributed to treatment effects. This is one motivation for our empirical strategy that compares both levels and within-respondent changes relative to appropriate pre-treatment baselines. Our stakeholders hope that (and we continue to expect that) respondents increase their support for and their interest in becoming a customer of Bank [X] following either treatment.

Hypothesis 3. *Respondent support for Bank [X] will increase following statements endorsing the principle of a local bank from the (1) US Federal Reserve or (2) Nation A's government, relative to the control group.*

Hypothesis 4. *H3 holds with regard to respondent self-reported likelihood of becoming a customer.*

Late in the survey, we introduce an augmented Govt A support + customer, Treatment 2B (see Section 5.3.2). We expect that the second treatment may move individual views further into a positive direction. It would be reasonable for Bank [X] to expect that having Government A as its customer would be a useful foundation on which to build its local customer base. We examine this through observational data comparing outcomes within Treatment group 2.

Hypothesis 5. *Respondents who have received the (2) Nation A's government endorsement will increase their support after receiving the additional information that Nation A's government has become a customer of Bank [X].*

5.5 Endorsement Effects on Behavior: Accessing credit reports

Finally, behavioral outcomes provide an important way to avoid problems associated with testing attitudes on attitudes. Potential behavioral outcomes in our setting are limited in part because of its particular theoretical usefulness: the foreign retail bank has not yet entered, so people cannot yet patronize the branch. We use a behavioral outcome that is linked to respondent interest in increasing their use of formal financial services. At the end of the survey, enumerators informed respondents that, under US federal law, they are entitled to receive free credit reports annually from each of the three major credit reporting bureaus, and that accessing these reports has no effect on their credit. The enumerator recorded whether the respondent subsequently expressed unprompted interest; the enumerator also showed every respondent the link and recorded whether the respondent took a note of it.[36] For those taking the survey on a private internet-connected device, we record whether they click the link. We combine unprompted interest, taking a note of the link, and/or clicking the link into a single outcome. We expect that both treatments have a positive effect on this outcome.[37]

Hypothesis 6. *Respondents who have received information about support from the (1) US Federal Reserve or (2) Nation A government's support that it is a Bank [X] customer are more likely to take steps toward accessing their credit reports, relative to the control group.*

6 Results

We report and discuss four sets of results. First, we report a set of descriptive results in which we assess the representativeness of our sample compared to Nation A's own records, as well as data from the American Community Survey (ACS). We check to make sure that our treatment groups are balanced on observable covariates. We also examine how well the data fits our descriptive expectations (DE). Second, we report our main experimental results: how did the randomized endorsement treatments affect respondents' attitudes and behavior? Fourth, we test for heterogeneous effects along several theoretically-informed dimensions: respondents' discount rates; financial resilience; knowledge about finance; community connections; and negative views of banks. Finally, we report observational results from survey items in which we asked all respondents how much their support for the bank would change in response to different types of ownership.

[36] https://www.annualcreditreport.com.

[37] At this point the Government A treatment group has received the augmented GOVT A SUPPORT + CUSTOMER treatment, so the treatments are no longer parallel.

6.1 Representativeness and Balance

As noted previously, we did not use random or stratified random sampling techniques to identify potential survey respondents. Instead, we encouraged our enumerators to set up temporary survey stations in high-traffic areas and draw on their own personal networks in order to survey as much of the Nation A population as possible. In this section, we investigate the demographic characteristics of our convenience sample to determine whether or not it is reasonably representative of the population. In order to preserve the anonymity of Nation A, we report only differences between our sample and other data sources. The preserves anonymity by not allowing the summary statistic means to be compared to other publicly available information.

First, we compare our sample demographics to the population averages according to Nation A's official records. Nation A maintains a database of all enrolled members and their recognized descendants; the database, to which we have access, contains information on each individual's enrollment status (member vs. descendant) as well as their gender, birthdate, and home address. The first column of Table A.1 compares our sample to the tribal population averages as maintained by Nation A. We sample over 10% of all enrolled members of Nation A, and over 5% of known descendants. Relative to the population, we oversample enrolled members and undersample descendants; this may be due in part to the fact that our data collection took place either on our very close by to the Nation A Reservation, and enrolled members are more likely to live on or nearby the reservation than descendants.[38] Our sample is quite representative of the population with regard to age. Finally, we oversample women relative to their proportion of Nation A's population. This may be a function of the gender composition of our enumerators' social networks, though a recent and similarly-administered survey of Native American populations also oversampled women (Schroedel et al., 2020).

Second, we compare our sample demographics to a relevant comparison group from the US Census Bureau's American Community Survey (2014-2018 wave). Specifically, we compare our sample to the ACS sample of adult respondents who live in the same state as the Nation A Reservation and identify as American Indian and Alaskan Natives ($N = 2{,}171$). Unsurprisingly, we again find that we have oversampled women. Likewise, respondents in our sample are more likely to be unmarried, and substantially more likely to be employed, than those in the ACS sample. Our sample contains fewer individuals who have not completed high school, and more individuals who

[38] A bivariate regression model using Nation A's official data suggests that enrolled members are 3% more likely to live in the same state as the Nation A reservation than descendants.

have received an associate's degree, than the ACS sample. Finally, we slightly undersample the youngest (18-24) and oldest (65+) adults. Thus, while there is some evidence that our respondents may be more highly educated and more frequently employed than the statewide AIAN population average, our convenience sample is generally representative of that population.

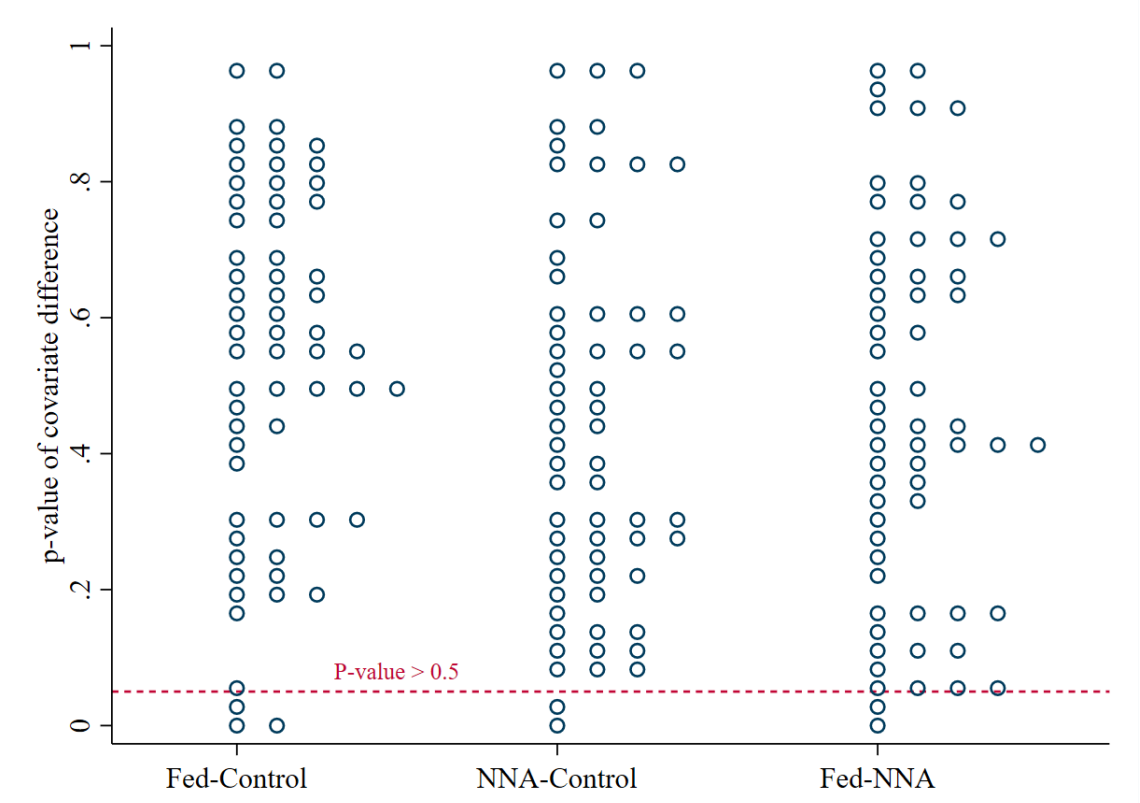
Finally, in order for our experimental results to identify average treatment effects, it is necessary that our procedure for assigning respondents to their treatment group was successfully randomized. While we cannot determine whether or not our groups are balanced on unobservable covariates, Figure 2 demonstrates that the groups are well-balanced on all observable covariates: of the large number of balance tests that we conducted, in only seven cases did two treatment groups differ significantly. However, since one of these mean differences is in one of our important baseline covariates (specifically in whether respondents believe it is good for a bank to open on the reservation) and we find differences across the distribution of responses in some of our other baseline variables, we report all outcomes in terms of differences from their respective baselines and also conduct robustness checks to ensure that covariate imbalance is not confounding our estimates. Generally speaking however, we believe the results from our balance tests are strongly indicative of successful randomization.

Finally, Figure 3 shows patterns in the data consistent with our descriptive expectations that, in this "banking desert" setting, support for the entry of a bank will be on average quite high, and variance would tend to be low. This holds for all treatment groups. There is some indication of less support for Bank [X], but the general pattern holds for both it and an a hypothetical bank.

6.2 Main Results

Our main results, presented in Figure 4, consist of difference-in-means tests conducted between each pair of treatment groups (US Federal Reserve treatment vs. control, Nation A treatment vs. control, and Nation A treatment vs. US Federal Reserve treatment) for each of our outcome variables. For each panel "A vs. B," the estimates presented are equal to the outcome variable mean among Group A minus the mean among Group B. All presented estimates have been standardized by their mean and standard deviation, and thus can be interpreted as the ATE measured in standard deviations of Y. As noted previously, all respondents were asked to answer comparable versions of each of the outcome items (except for the behavioral measure and the self-assessed impact of the statements on support) prior to receiving treatment. This allows us to examine the effects of treatment on each outcome variable in two different ways:

Figure 2: Treatment groups are well-balanced on observable covariates



Notes: This figure presents the p-values for the difference in means of our pre-treatment covariate measures including our baseline opinion measures, age, sex, education, income, employment, opinions about banks, knowledge of finance, access to financial services and location.

1. For each outcome variable, we examine the difference between treatment groups in the average value of the corresponding survey item that appeared *post-treatment*. Formally, we calculate the quantity $\bar{Y}^{Post}_{D=1} - \bar{Y}^{Post}_{D=0}$. These estimates are presented in Figure 4a.

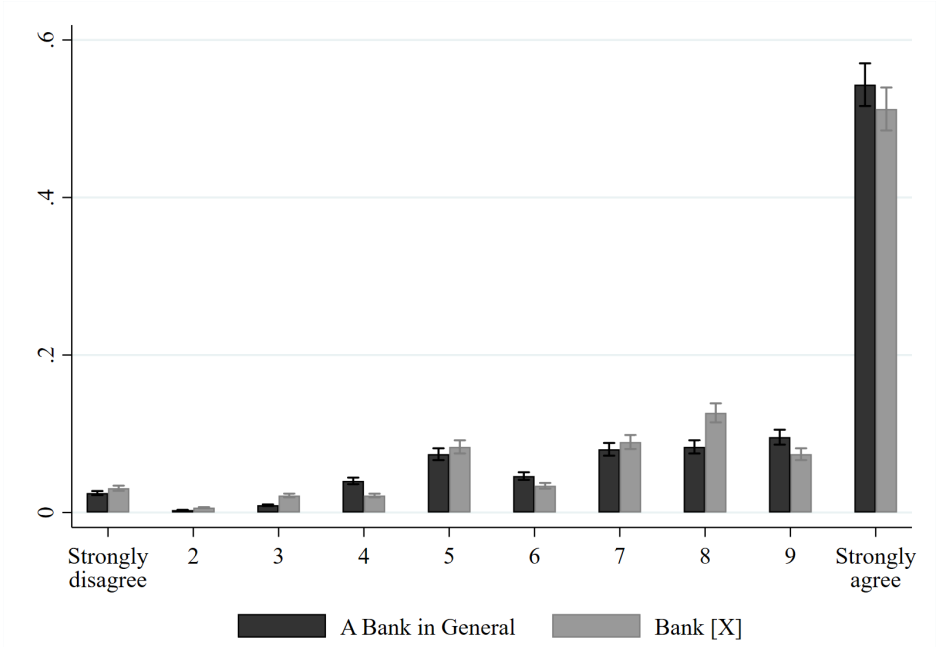
2. For each outcome variable, we also examine the difference between treatment groups in the average *change* between respondents' baseline (pre-treatment) and their post-treatment responses to each corresponding survey item. Formally, we calculate the quantity:

$$[\frac{\sum_{i=1}^{n} Y^{Post}_{i,D=1} - Y^{Pre}_{i,D=1}}{n}] - [\frac{\sum_{i=1}^{m} Y^{Post}_{i,D=0} - Y^{Pre}_{i,D=0}}{m}] = \overline{\Delta Y}_{D=1} - \overline{\Delta Y}_{D=0} \quad (1)$$

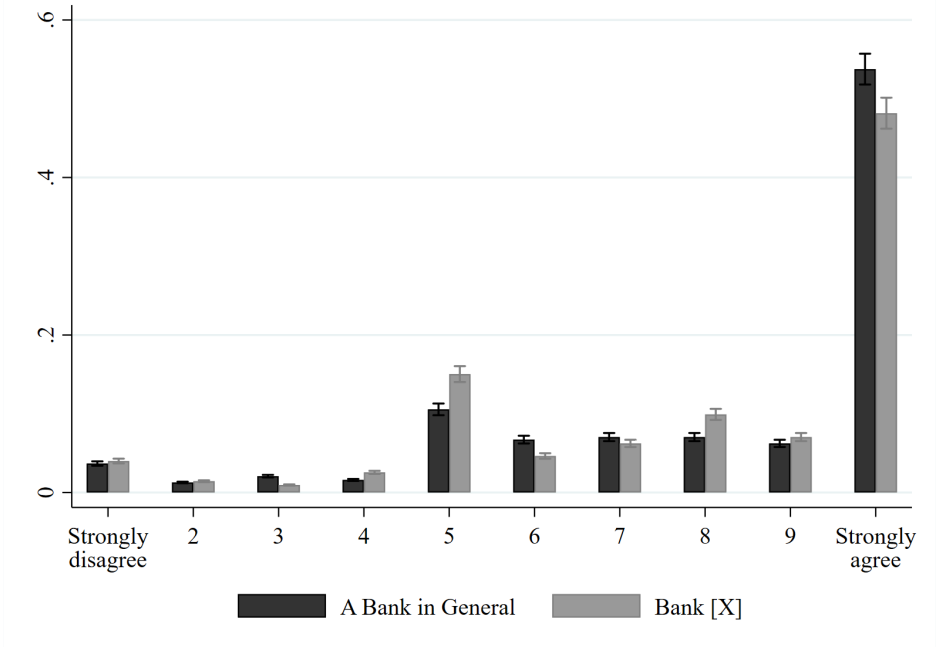
   Estimates of the above kind are labeled "(change from baseline)" and presented in Figure 4b.

The first outcome variable, titled "Expressed effect of treatment," captures respondents' own assessments of how the Federal Reserve/Nation A statement affected their support for a bank

Figure 3: Patterns are consistent with our descriptive expectation (DE): Distributions for both treated and untreated groups are highly right-skewed.

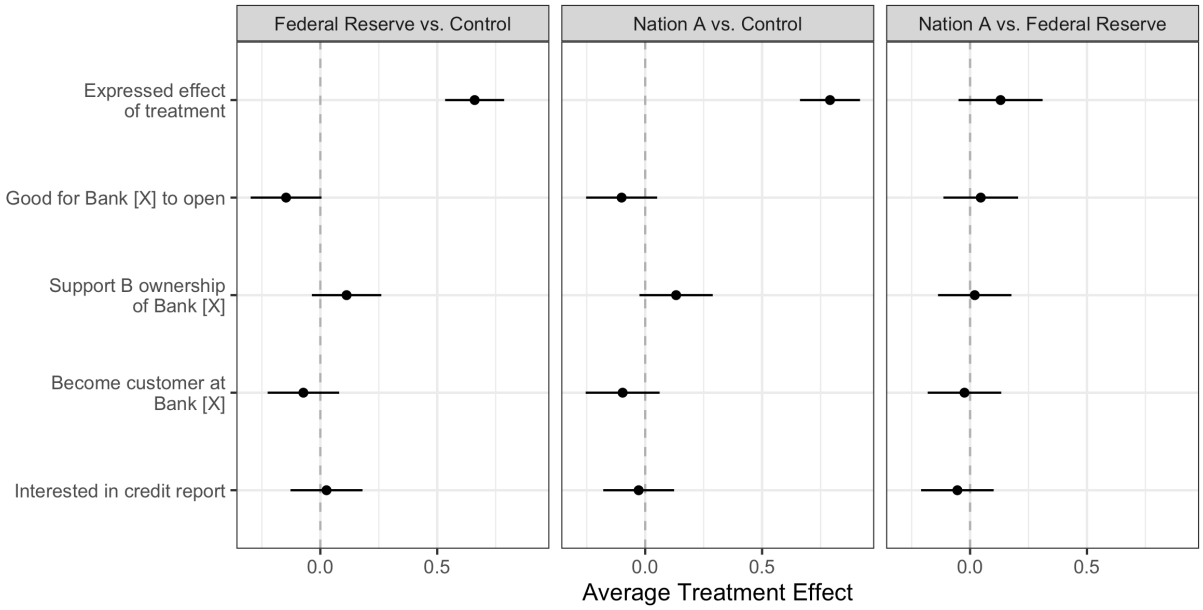


(a) Control Group Only.

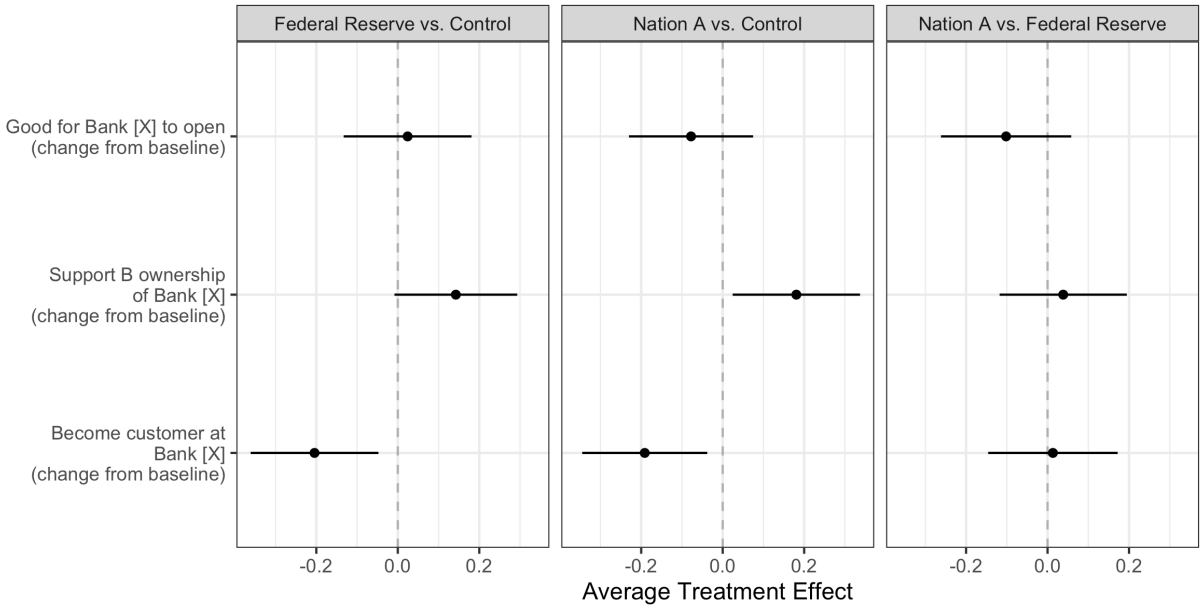


(b) Treatment Groups Pooled.

Figure 4: Causal effects of knowledge of Federal Reserve support for the expansion of safe and accessible financial services for under-served populations and Nation A support for the opening of a bank in their Nation with 95% CIs.



(a) Differences presented are of the form $\bar{Y}_{D=1}^{Post} - \bar{Y}_{D=0}^{Post}$.



(b) Differences presented are of the form $\overline{\Delta Y}_{D=1} - \overline{\Delta Y}_{D=0}$ (equation (1)).

Notes: All treatment effects presented as proportions of the outcome variables standard deviation.

opening on the Nation A Reservation. The control group baseline is merely a vector of zeroes, reflecting our assumption that control group individuals' post-treatment support for a bank is no different from their pre-treatment support, as they did not receive treatment. Figure 4a shows that both treatments had substantial positive effects on individuals' self-reported support for a bank to open on the reservation; on average, treated respondents report that the treatments increased their support for a bank. These effects are also large in magnitude, as the Nation A and US Fed treatments increased support for a bank by .66 and .79 standard deviations (respectively). The Nation A endorsement effect is larger than that of the Federal Reserve statement, although the difference is statistically significant only at the 10 percent level at the mean of the index.[39]

The second outcome variable, titled "Good for Bank [X] to open," measures respondents' support for the specific Bank [X] opening a branch on the Nation A Reservation on a scale from 0 to 10.[40] We find no evidence that respondents' support for Bank [X] was affected by either of our endorsement treatments, as none of the estimated average treatment effects achieve statistical significance. While Figure 4a shows that the effect of the US Fed treatment had a borderline significant ($p = .058$) *negative* effect on support for Bank [X], Figure 4b shows that this effect disappears once respondents' baseline beliefs are accounted for. The effect size is also small in magnitude, constituting a change of only .14 standard deviations in the "Good for Bank [X] to open" variable.

Our next outcome variable, "Support B ownership of Bank [X]", reflects respondents' answers to the following question: "Does knowing that Bank [X] is 100% owned by Nation B make your support of Bank [X] increase, decrease, or stay the same?" Figure 4 shows that respondents in both treatment groups were more supportive of Bank [X] after learning that it was owned by Nation B than those in the control group. The only statistically significant difference is that between the group that received the Nation A endorsement and the control group (accounting for baseline responses); however, the baseline-adjusted effect of the US Fed endorsement is also positive and borderline significant ($p = .064$), and Figure 4b shows that the difference between the two treatment groups is small and statistically indistinguishable from zero. The effect sizes are again fairly small, as both treatments result in an average change of less than .2 standard deviations in the "Support B ownership of Bank [X]" variable.

Next, we turn from measures of support for Bank [X] to a measure of whether or not the individ-

[39] Tests for differences across the distribution of the ranking between the Nation A and the Federal Reserve treatment suggest that the Nation A treatment does induce greater positive self-assessed response.

[40] Specifically, respondents were asked to rank their agreement or disagreement with the following statement: "It would be good for Bank [X] to open a branch on the Nation A Reservation."

uals in our survey intend to directly benefit from Bank [X]'s entry. The outcome variable "Become customer of Bank [X]" reflects respondents' self-reported likelihood of becoming a customer of Bank [X] once it opens a branch on the Nation A Reservation. When looking only at differences in post-treatment averages (in Figure 4a), both Federal Reserve and Nation A statements have slightly negative but insignificant effects on the likelihood that respondents will become customers of Bank [X]. However, once baseline responses are adjusted for, both treatments have significant negative effects that are similar in magnitude. Only the Nation A treatment, however retains it size and statistical significance upon adjusting for pre-treatment differences in observables (results presented in Table A.3). Again, the effect sizes are relatively modest; both treatments result in a decrease of approximately .2 standard deviations in the "Become customer of Bank [X]" variable.

Finally, we examine the effects of our treatments on the behavioral outcome measure of steps toward accessing their free annual credit reports. We find that neither treatment had any discernible effect on respondents' propensity to seek out additional information about their own credit. Further, the ATEs are very close to zero and estimated fairly precisely.

Appendix Tables A.2 and A.3 show that quantitatively and qualitatively, most of these findings are robust for adjusting for both random differences in pre-treatment observables that may affect the outcomes we observe,[41] but have been asymmetrically distributed across treatments due to the finite sample size; enumerator fixed effects; and controls for where the survey was taken and by what method it was taken (with an enumerator on a tablet, in the job center, or on a personal device). We show whether we use regression adjustment, inverse probability re-weighting, a doubly robust combination of them both, or ordered or binary probit to estimate the average treatment effects above, the results are largely unchanged. The one significant exception is the estimated effect of the Federal Reserve treatment on the likelihood of becoming a customer or nation B ownership. This effect is statistically insignificant and smaller in magnitude once differences in pre-treatment observables are accounted for. For completeness, these tables show the treatment effects for the unscaled variables (without the mean and standard deviation normalization).

In sum, while the self-reported measure suggests that both the Federal Reserve's support of access to finance and the Nation A government's endorsement of a bank opening in the nation have a large effect on respondents' expressed support for FDI in retail banking in general, we find limited support for the hypothesis that statements from the Federal Reserve or the Nation A government

[41] These include measures of age, income, education, gender, access to technology, knowledge about finance, opinions about banks and opinions of value of native, American and Nation A member ownership of the bank, and indicators for being an enrolled member.

increase respondents' support for a particular new foreign entrant (Bank [X]). Specifically, we find no evidence that the treatments directly increase support for Bank [X]; however, they may increase support *in*directly through magnifying the positive effect of Bank [X]'s native (Nation B) ownership. Contrary to our prediction, we find that both statements make respondents *less* likely to report that they intend to become customers of Bank [X] once it opens on the reservation. All significant effects are relatively small in magnitude, particularly when compared to the expressed effects of treatment. We also find that our treatments did not increase respondents' interest in learning more about personal finance, as captured by our behavioral measure. Further, we find that the differences between the effects of the Federal Reserve statement and the Nation A endorsement are small and insignificant; respondents did not react differently to endorsements from the international institution or their own national government.

6.2.1 Augmented Nation A Treatment

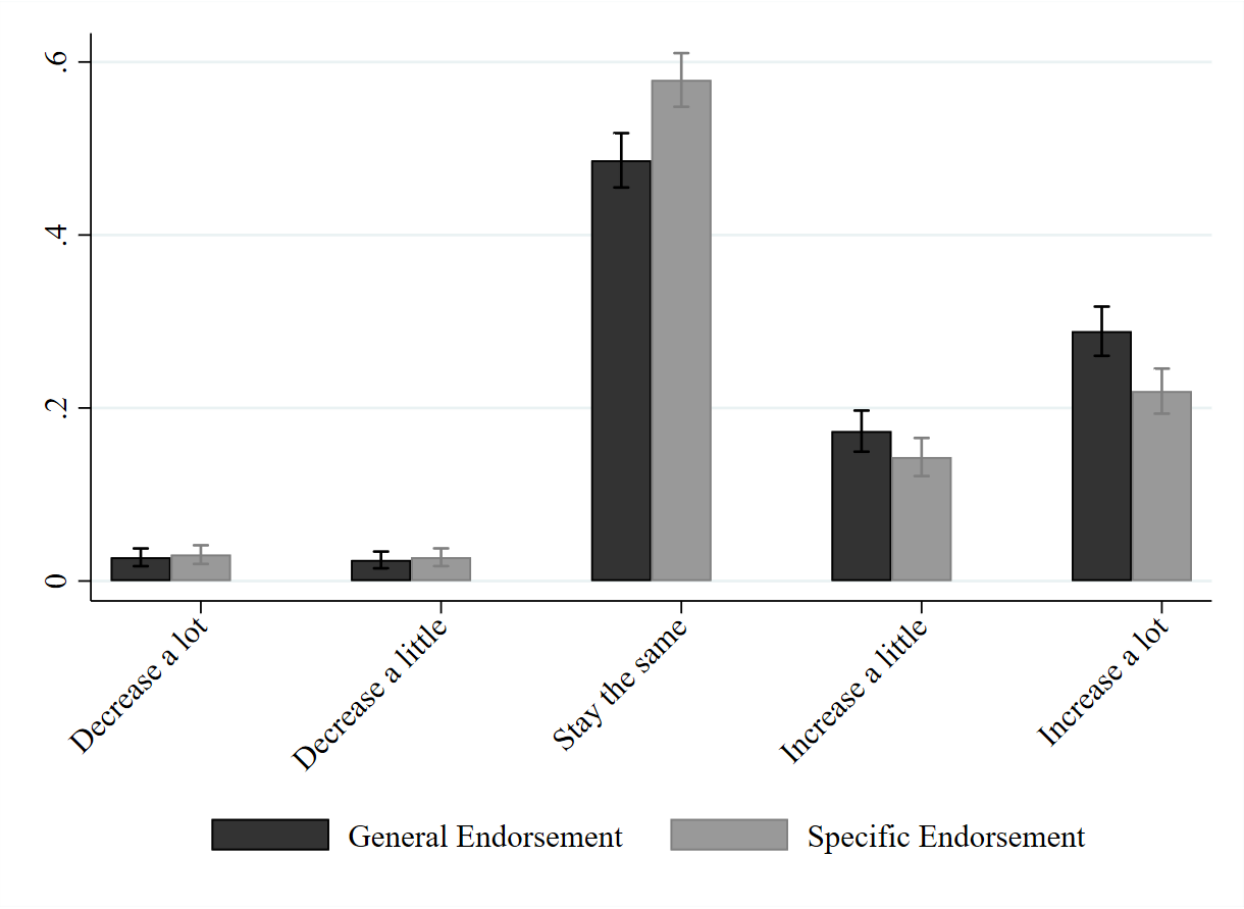
Is there evidence consistent with H5, that respondents in the Nation A treatment group are further influenced by the follow-up information treatment that Nation A has become a customer of Bank [X]? Figure 5 shows descriptive data on respondents' self-reported effect of the first "support" treatment (black) and the second "customer" treatment (gray) on their views. The distributions are very similar; the modal answer for both treatments is that it did not directly influence their views. However, that answer occurred significantly more often in reference to the specific "customer" treatment. The proportion of respondents reporting that the "customer" treatment moved their views in a positive direction is also lower. While only descriptive, this plot suggests possible declining marginal returns relevant to institutional choices over resource allocation.

6.3 Heterogeneous Effects

There is often significant heterogeneity in how people respond to information. We did not specify ex ante theoretical predictions on how respondents might answer, but we ex post explore a few dimensions that strike us as possibilities. We acknowledge that this exercise privileges our theoretically motivated intuitions that have consequences in our choice to probe some untested, possible explanations over others. In presenting these to stakeholders, we move into the field of agenda-setters and policymakers.

First, it is plausible that those with more pressing needs for access to affordable credit and banking services (i.e., high discount rates) would be less responsive to treatments than those with

Figure 5: Change in Stated Support Among Respondents Who have Received Statements of Support for a Local Bank in Principle from Nation A after receiving additional information that Nation A's government has become a customer of Bank [X]



lower needs for access to banking; and that they would not be a subgroup contributing to unexpected negative effects of the treatment. We also examine whether those who are already financially resilient might respond differently, since their material interests in a specific local bank opening are less important given their current financial position. Those that are financially knowledgeable might have a greater sense about what the statements from the Federal Reserve and their government imply and thus also respond differently than those with less financial knowledge. Those who are more connected to the Nation A community may also have a different response, especially to statements from their government. Finally, those with a negative history or beliefs about banks might also respond to the treatments differently because of their prior beliefs.

To measure each of these dimensions of potentially meaningful heterogeneity, we construct a series of indices using relevant questions available from our survey. We then construct binary indicators for each category that equal to one if a respondent's value of that the index passes a threshold such that 75% or more of respondents have at least this value of the index. To more clearly illustrate, consider our index of having a higher discount rate. In this index, we create indicators for each time one of the following things is true about a given respondent and then sum across them: the respondent does not have a bank account; they could not get $400 in an emergency; they are the primary earner in a household with at least two more people beyond their spouse; and they have very poor self-assessed credit. We divide this sum by the total number of these questions they answered. Once we have a value for this index, we compute the threshold for which as least 75% of respondents are covered and give a respondent a value equal to one if they pass this threshold and zero otherwise. Table A.4 describes the questions and thresholds used in generating each index. Table A.5 presents the correlation between all of these indexes and related binary variables to show they are all relatively uncorrelated (the maximum correlation coefficient is roughly 0.3) and thus capturing conceptually and empirical distinct dimensions of heterogeneity among respondents.

Our ex post intuition was that heterogeneity around discount rates would be meaningful; we therefore present those first. In Figure 6 we show the interaction between the treatment effect estimated in a given panel and the indicator equal to one marking a respondent with a high discount rate for financial services. Somewhat counter to our prediction, those that have a higher discount rate express similar support to the average respondent and tend to be likely to become a customer upon hearing the Federal Reserve treatment.

In Table 2 and 3 we present the sign and statistical significance of the results of the same exercise

for our other indices.[42] Per Table 2, respondents represented in each of these indices respond more positively to US Fed treatment than averages. This is especially true of those who are connected to their community. Per Table 3, we see again that there are heterogeneous treatment effects for those that are connected to their community, as well as some evidence for those who are knowledgable about finance. Results on the other indices are more sensitive to different specifications.

Table 2: **US Federal Reserve Treatment Heterogeneity:** This table reports the sign and significance (when present) for the interaction between the column/index and the row/outcome.

Outcome	Financial resilience	Knows about Finance	Connected to Community	Negative Views of Banks
Expressed effect of treatment	(+)***	(+)***	(+)***	.
Interested in credit report	.	.	.	.
Levels				
Good for Bank [X] to open	.	.	.	.
Support B ownership of Bank [X]	.	.	(+)**	.
Become customer of Bank [X]	.	.	.	.
Differences				
Good for Bank [X] to open	.	.	.	(+)*
Support B ownership of Bank [X]	(+)**	.	(+)**	.
Become customer of Bank [X]	.	.	.	.

See Table A.4 for variable definitions and Tables A.6 and A.7 for coefficients. Observations vary due to missing responses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6.4 Observational Results

Figure 7 provides observational evidence consistent with H1a that domestic ownership by Nation A would be significantly preferred to American (US) ownership. Nation A ownership is also preferred to foreign ownership by another Native Nation, but that effect is not as stark. It is about on par with the positive effect that, between foreign ownership choices, Native (non-A) is significantly preferred to American (US). These observational results are consistent with qualitative

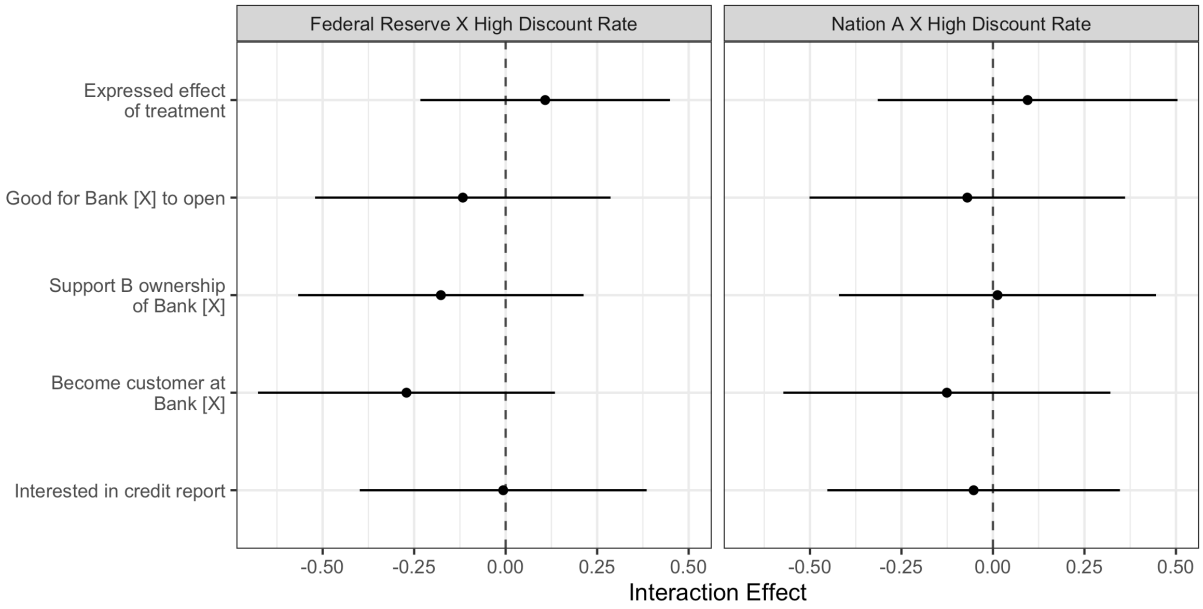
[42] Estimated coefficients and standard errors can be found in Appendix Tables A.6 and A.7.

Table 3: **Nation A Treatment Heterogeneity**: This table reports the sign and significance (when present) for the interaction between the column/index and the row/outcome.

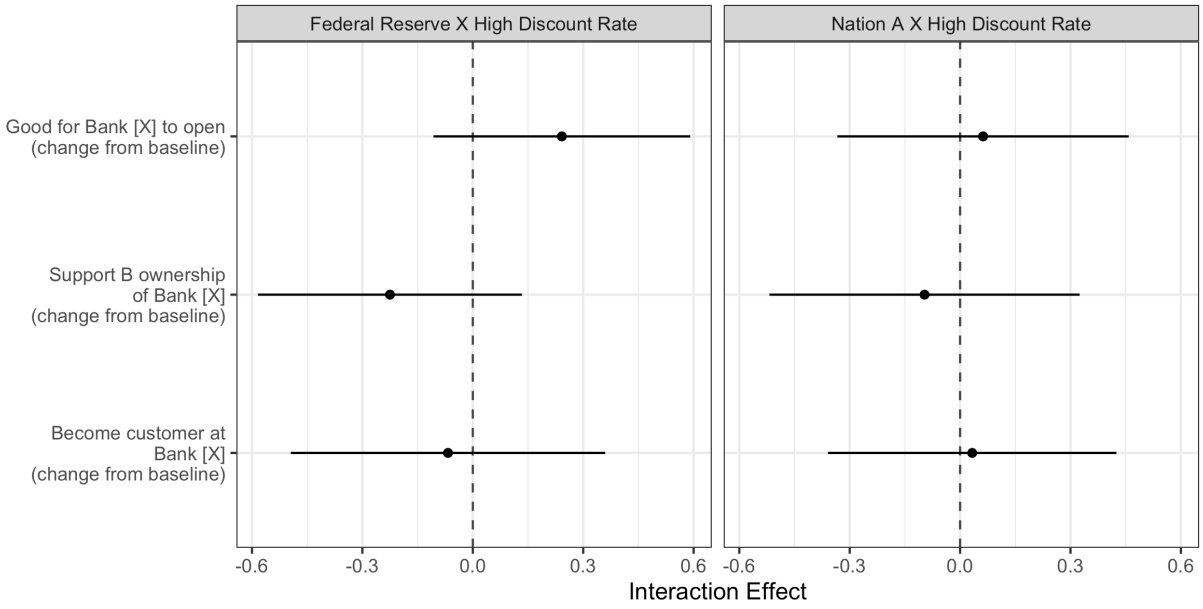
Outcome	Financial resilience	Knows about Finance	Connected to Community	Negative Views of Banks
Expressed effect of treatment	(-)***	(+)***	(+)***	.
Interested in credit report	.	.		.
Levels				
Good for Bank [X] to open	.	.		.
Support B ownership of Bank [X]	.	.		.
Become customer of Bank [X]	.	.	(+)***	.
Differences				
Good for Bank [X] to open	.	.	.	(+)*
Support B ownership of Bank [X]	(+)**	.		(-)*
Become customer of Bank [X]	.	.	(+)***	.

See Table A.4 for variable definitions and Tables A.6 and for coefficients. Observations vary due to missing responses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 6: Causal effects of knowledge of Federal Reserve support for the expansion of safe and accessible financial services for under-served populations and Nation A support for the opening of a bank in their Nation with 95% CIs. for respondents with **higher discount rates.**



(a) Differences presented are of the form $\bar{Y}_{D=1}^{Post} - \bar{Y}_{D=0}^{Post}$.



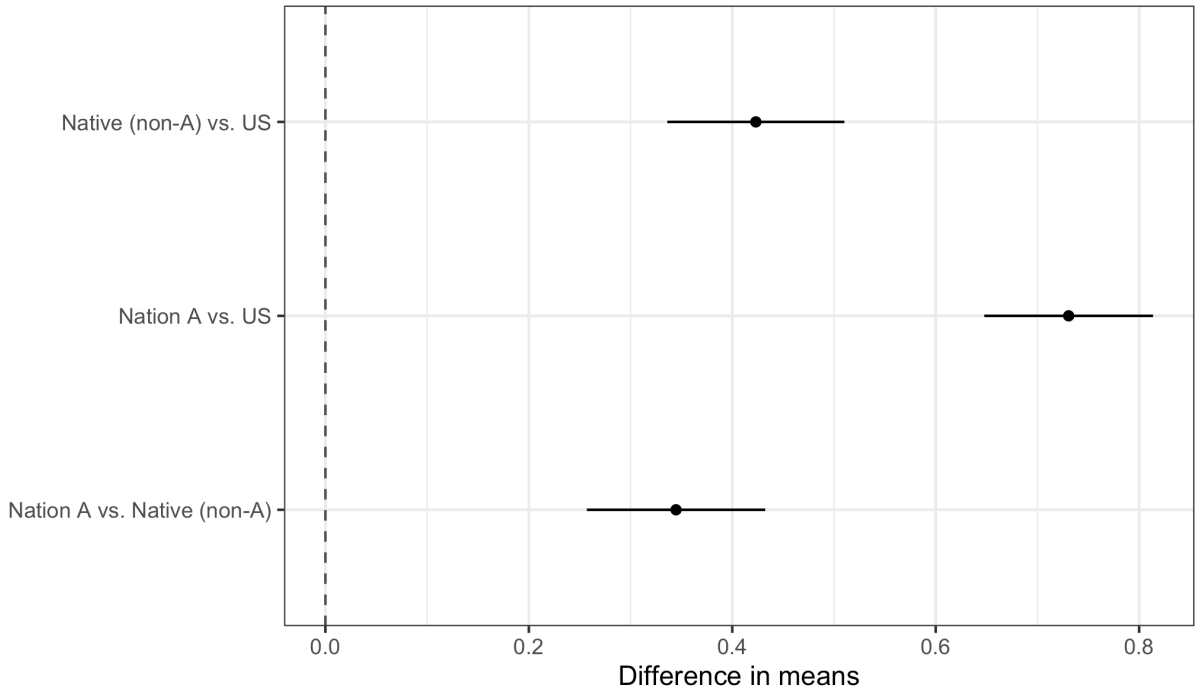
(b) Differences presented are of the form $\overline{\Delta Y}_{D=1} - \overline{\Delta Y}_{D=0}$. (equation 1)

Notes: All treatment effects presented as proportions of the outcome variables standard deviation.

evidence gleaned from the overall attitudes of actors at both Bank [X] and the Nation A Tribal Legislature – that this FDI is something special, and something important for Indian Country as

a whole.

Figure 7: Differences in self-reported change in support for a bank in response to different hypothetical owners

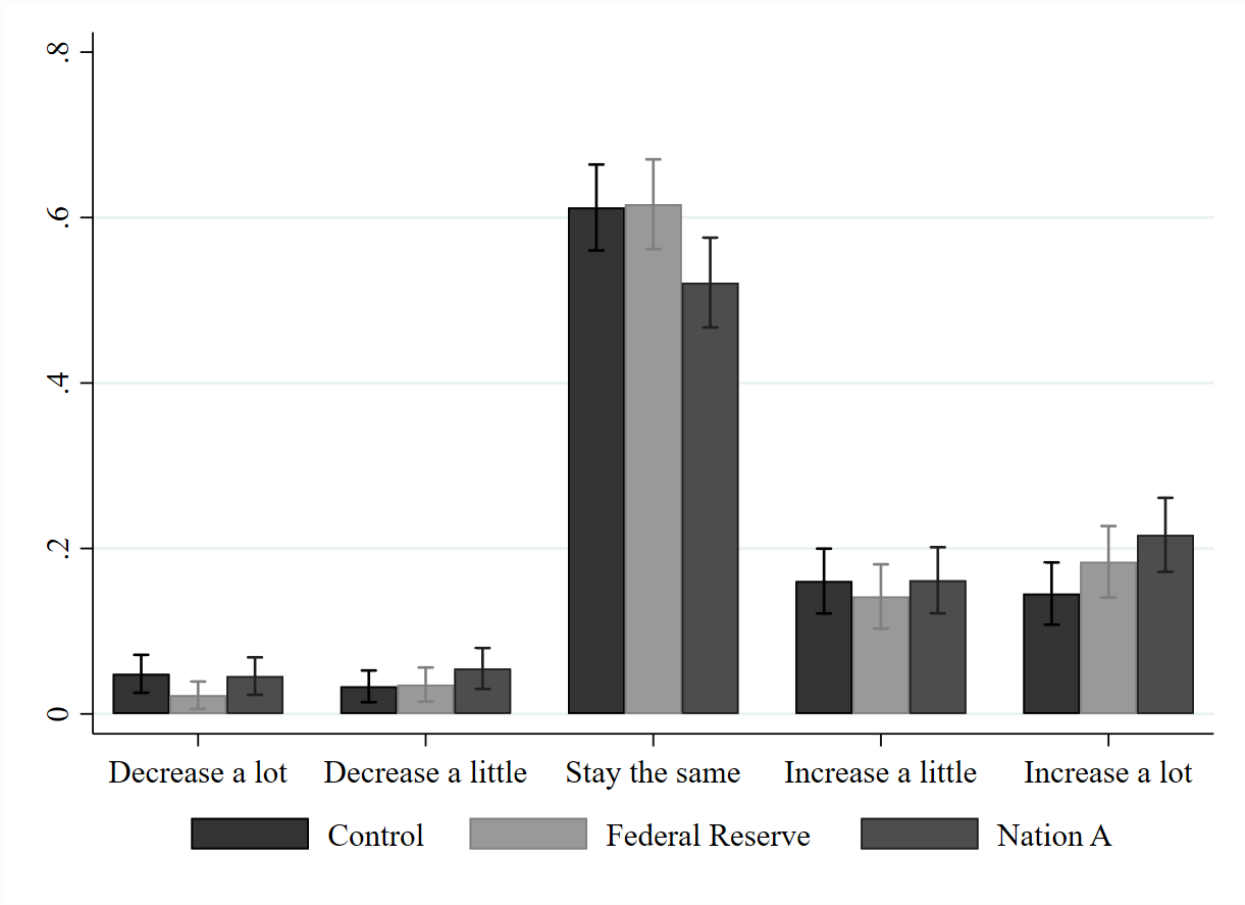


Post-treatment, we inform all respondents that Bank [X] is 100% owned by Nation B and ask them to self-report how this information might change their views (Figure 8). As expected, there is not meaningful heterogeneity across the treatment groups. While the modal response is "stay the same," the proportion of respondents reporting that it increases support is significantly greater than those who report that it decreases support. We takeaway that there is not obvious opposition to the fact that the Native owner is from Nation B, which suggests that it is unlikely that the observational results in Figure 7 would be so different as to flip signs if the unidentified Native owner were replaced with Nation B – which is good news, practically, for Bank [X]'s public relations.

7 Conclusion

In this article, we report results from a unique survey and set of survey experiments in pre-FDI-treatment Native Nation A, in which a retail bank is overwhelmingly desired by Nation A's citizens, and that overwhelming support does not fade away when the FDI breaking ground in a few months is identified as Bank [X] owned by Native Nation B. Without deception, we probe

Figure 8: Change in Stated Support of Bank [X] After 100% Nation B Ownership Information Shared



Notes: Expressed change in support due to ownership information.

statements of support from both the US Federal Reserve and Nation A's Tribal Legislature, to evaluate their treatment effects and possible heterogeneity within them. Endorsements an important concept deserving of theoretical attention when it comes to foreign firm-government relations and public opinion on globalization-related issues, especially in very low information and experience environments.

That our results are in many ways complex and conditional is worthy of acknowledging for several reasons. First, even given high baseline support for the entry of a foreign bank to a formal "banking desert," and institutional actors with specific expertise in exactly this issue, it is not a foregone conclusion that institutional endorsements are useful in moving public opinion toward their preferred outcome. Second, the deep research agenda into individual-level heterogeneity around globalization is of practical use even in an effectively pre-globalization setting. Third, there are many actors in the world interested in bringing the benefits of economic integration to underserved areas – including the US Federal Reserve, Nation A's government, and a firm like Bank [X] that is taking a risk in investing to provide some of those needed services. Especially when national, international, and private interests overlap, one might hope that scholarly work could inform normative goals.

Fourth, there exist nations that are not Westphalian nation-states but nonetheless have a rightful place in IPE. When a nation has full sovereign authority over whether a business, a cash flow, a good or service, or an economic migrant can come across its border, then that nation is fertile territory for understanding the internal validity of theories such as those on public opinion and government choices over openness. We suggest that researchers consider the full set of applicable (semi-)sovereigns in international economic relations, and acknowledge whether datasets cover the population, a random sample, or a biased sample excluding nations like Nation A where steps toward economic globalization are incredibly salient.

References

Ahlquist, John, Mark Copelovitch and Stefanie Walter. 2020. "The Political Consequences of External Economic Shocks: Evidence from Poland." *American Journal of Political Science* .

Akee, Randall KQ, William E Copeland, Gordon Keeler, Adrian Angold and E Jane Costello. 2010. "Parents' incomes and children's outcomes: a quasi-experiment using transfer payments from casino profits." *American Economic Journal: Applied Economics* 2(1):86–115.

Ardanaz, Martin, M Victoria Murillo and Pablo M Pinto. 2013. "Sensitivity to issue framing on trade policy preferences: evidence from a survey experiment." *International Organization* 67(2):411–437.

Arias, Luz Marina and Desha M Girod. 2014. "Indigenous Origins of Colonial Institutions." *Quarterly Journal of Political Science* 9(November 2013):371–406.

Baker, Andy. 2015. "Race, Paternalism, and Foreign Aid: Evidence from U.S. Public Opinion." *American Political Science Review* 109(1):93–109.

Ballard-Rosa, Cameron. 2016. "Hungry for Change: Urban Bias and Autocratic Sovereign Default." *International Organization* 70(02):313–346.

Bastiaens, Ida and Nita Rudra. 2018. *Democracies in Peril*. Cambridge University Press.

Bearce, David H and Kim-Lee Tuxhorn. 2017. "When are monetary policy preferences egocentric? Evidence from American surveys and an experiment." *American Journal of Political Science* 61(1):178–193.

Betz, Timm and Amy Pond. 2019. "The Absence of Consumer Interests in Trade Policy." *The Journal of Politics* 81(2):585–600.

Bodea, Cristina and Adrienne LeBas. 2016. "The origins of voluntary compliance: attitudes toward taxation in urban Nigeria." *British Journal of Political Science* 46(1):215–238.

Brown, James R, J Anthony Cookson and Rawley Z Heimer. 2019. "Growing up without finance." *Journal of Financial Economics* 134(3):591–616.

Bruyneel, Kevin. 2007. *The third space of sovereignty: The postcolonial politics of US-indigenous relations*. U of Minnesota Press.

Bush, Sarah Sunn and Lauren Prather. 2019. "Do electronic devices in face-to-face interviews change survey behavior? Evidence from a developing country." *Research & Politics* 6(2).

Cattaneo, Laura and Donna Feir. 2019. The Higher Price of Mortgage Financing for Native Americans. Center for Indian Country Development series 4-2019 Federal Reserve Bank of Minneapolis.

Cheon, Andrew, Maureen Lackner and Johannes Urpelainen. 2015. "Instruments of Political Control." *Comparative Political Studies* 48(3):370–402.

Chilton, Adam S., Helen V. Milner and Dustin Tingley. 2020. "Reciprocity and Public Opposition to Foreign Direct Investment." *British Journal of Political Science* 50(1):129–153.

Commission, Federal Communications et al. 2018. "Broadband deployment report." *Washington DC: Federal Communications Commission* .

Demirguc-Kunt, Asli, Leora Klapper, Dorothe Singer, Saniya Ansar and Jake Hess. 2018. *The Global Findex Database 2017: Measuring financial inclusion and the fintech revolution*. The World Bank.

Dimitrova-Grajzl, Valentina, Peter Grajzl, A Joseph Guse and Richard M Todd. 2015. "Consumer credit on American Indian reservations." *Economic Systems* 39(3):518–540.

Dimitrova-Grajzl, Valentina, Peter Grajzl, A Joseph Guse and Richard M Todd. March 2017. "A Broader Reach." *Finance and Development* 54(1).

Ehrlich, Sean D. 2018. *The politics of fair trade: Moving beyond free trade and protection*. Oxford University Press.

Evans, Laura E. 2011*a*. "Expertise and Scale of Conflict: Governments as Advocates in American Indian Politics." *American Political Science Review* 105(4):663–682.

Evans, Laura E. 2011*b*. *Power from powerlessness: Tribal governments, institutional niches, and American federalism*. Oxford University Press.

Evans, Laura E. 2014. "Tribal-state relations in the Anglosphere." *Annual Review of Political Science* 17:273–289.

Findley, Michael G, Adam S Harris, Helen V Milner and Daniel L Nielson. 2017. "Who Controls Foreign Aid? Elite Versus Public Perceptions of Donor Influence in Aid-Dependent Uganda." *International Organization* 71(4):633–663.

Guedel, W Gregory and JD Colbert. 2016. "Capital, Inequality, and Self-Determination: Creating a Sovereign Financial System for Native American Nations." *American Indian Law Review* 41(1):1–49.

Guisinger, Alexandra. 2009. "Determining Trade Policy: Do Voters Hold Politicians Accountable?" *International Organization* 63(3):533–557.

Guisinger, Alexandra and Elizabeth N. Saunders. 2017. "Mapping the boundaries of elite cues: How elites shape mass opinion across international issues." *International Studies Quarterly* 61(2):425–441.

Lightfoot, Sheryl. 2016. *Global indigenous politics: A subtle revolution*. Routledge.

Mansfield, Edward D. and Diana C. Mutz. 2013. "US versus them: Mass attitudes toward offshore outsourcing." *World Politics* 65(4):571–608.

Merry, Ellen A. 2018. "Mobile banking: a closer look at survey measures." *FED Notes* (2018-03):27.

Mihalache-O'Keef, Andreea S. 2018. "Whose greed, whose grievance, and whose opportunity? Effects of foreign direct investments (FDI) on internal conflict." *World Development* 106:187–206.

Naoi, Megumi. 2020. "Survey Experiments in International Political Economy: What We (Don't) Know about the Backlash against Globalization." *Annual Review of Political Science* 23:333–356.

Owen, Erica and Noel P. Johnston. 2017. Occupation and the political economy of trade: Job routineness, offshorability, and protectionist sentiment. In *International Organization*.

Owen, Erica and Stefanie Walter. 2017. "Open Economy Politics and Brexit: Insights, Puzzles, and Ways Forward." *Review of International Political Economy* 24(2):179–202.

Pandya, Sonal S. 2013. *Trading Spaces*. Cambridge: Cambridge University Press.

Pandya, Sonal S. 2016. "Political Economy of Foreign Direct Investment: Globalized Production in the Twenty-First Century." *Annual Review of Political Science* 19(1):455–475.

Pepinsky, Thomas B. 2014. "Surveys, Experiments, and the Landscape of International Political Economy." *International Interactions* 40(3):431–442.

Post, Alison E. 2014. *Foreign and Domestic Investment in Argentina*. New York: Cambridge University Press.

Rho, Sungmin and Michael Tomz. 2017. "Why Don't Trade Preferences Reflect Economic Self-Interest?" *International Organization* 71(S1):S85—-S108.

Rudra, Nita. 2011. "Openness and the politics of potable water." *Comparative Political Studies* .

Rudra, Nita and Jennifer L Tobin. 2017. "When Does Globalization Help the Poor?" *Annual Review of Political Science* 20:287–307.

Rudra, Nita, Meir Alkon and Siddharth Joshi. 2018. "FDI, Poverty, and the Politics of Potable Water Access." *Economics and Politics* 30(3):366–393.

Schroedel, Jean, Aaron Berg, Joseph Dietrich and Javier M. Rodriguez. 2020. "Political Trust and Native American Electoral Participation: An Analysis of Survey Data from Nevada and South Dakota." *Social Science Quarterly* .

Sell, Susan K and Owain D Williams. 2020. "Health under capitalism: a global political economy of structural pathogenesis." *Review of International Political Economy* 27(1):1–25.

Spilker, Gabriele. 2013. *Globalization, political institutions and the environment in developing countries*. Vol. 3 Routledge.

Spirling, Arthur. 2012. "US treaty making with American Indians: Institutional change and relative power, 1784–1911." *American Journal of Political Science* 56(1):84–97.

Steinberg, David A and Stephen C Nelson. 2019. "The mass political economy of capital controls." *Comparative Political Studies* 52(11):1575–1609.

Tingley, Dustin. 2014. "Survey Research in International Political Economy: Motivations, Designs, Methods." *International Interactions* 40(3):443–451.

Walter, Stefanie. 2020. "The Backlash Against Globalization." *Annual Review of Political Science* 817582(817582):1–33.

Weitz-Shapiro, Rebecca and Matthew S Winters. 2017. "Can citizens discern? Information credibility, political sophistication, and the punishment of corruption in Brazil." *The Journal of Politics* 79(1):60–74.

Wellhausen, Rachel L. 2015. *The Shield of Nationality: When governments break contracts with foreign firms.* Cambridge University Press.

Wellhausen, Rachel L. 2017*a*. "Sovereignty, Law, and Finance: Evidence from American Indian Reservations." *Quarterly Journal of Political Science* 12(4):405–436.

Wellhausen, Rachel L. 2017*b*. "The Political Economy of US Territories and Indian Country." *PS - Political Science and Politics* 50(2):510–514.

Yunus, Muhammad. 2007. "Nobel lecture, Oslo, December 10, 2006." *Explore (New York, NY)* 3(5):445–448.

Appendix

A Tables

Table A.1: Evaluating Representativeness of Our Respondents: Comparison to Nation A Administrative Data and 2013-2018 American Community Survey Data for American Indians Living in the Same State as Nation A

	Nation A Records	ACS
Proportion enrolled members	0.13***	
Average age	0.64	
Proportion female	0.11***	0.13***
Single		0.09***
No children in household		-0.03^{+}
Employed		0.16***
Less than HS		-0.09***
High school or GED		0.00
Some college		0.03
2-year degree		0.05***
4-year degree		0.01
Advanced degree		-0.01
18 to 24		-0.04**
25 to 34		0.06***
35 to 44		-0.01
45 to 54		-0.03^{+}
55 to 64		0.00
65 and over		-0.04**

Differences in proportions or means reported. Observations vary due to missing responses. Significance stars: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$ *** $p < 0.001$.

Table A.2: Average Treatment Effect in Levels: Adjusting for Random Differences Respondent Characteristics Across Treatments

	Unadjusted	Regression	IPW	Doubly Robust	Orded Probit or Probit
			Expressed effect of treatment		
Federal Reserve	0.561***	0.567***	.	.	0.934***
	(0.054)	(0.053)	.	.	(0.099)
Nation A	0.672***	0.711***	.	.	1.073***
	(0.055)	(0.056)	.	.	(0.102)
			Interested in credit report		
Federal Reserve	0.0131	0.0152	0.00952	0.00684	-0.099
	(0.039)	(0.038)	(0.039)	(0.036)	(0.087)
Nation A	-0.0139	-0.0211	-0.0226	-0.0278	-0.111
	(0.038)	(0.037)	(0.037)	(0.035)	(0.089)
			Good for Bank [X] to open		
Federal Reserve	-0.370*	-0.278	-0.258	-0.25	-0.156*
	(0.195)	(0.174)	(0.184)	(0.165)	(0.086)
Nation A	-0.255	-0.18	-0.225	-0.205	-0.080
	(0.195)	(0.183)	(0.192)	(0.173)	(0.088)
			Become a customer of Bank [X]		
Federal Reserve	-0.0844	-0.0579	-0.0232	-0.0485	0.0331
	(0.091)	(0.099)	(0.099)	(0.095)	(0.098)
Nation A	-0.112	-0.101	-0.0763	-0.0956	-0.0352
	(0.094)	(0.097)	(0.098)	(0.092)	(0.097)
			Support of B ownership of Bank [X]		
Federal Reserve	0.108	0.0825	0.0935	0.0759	0.117
	(0.073)	(0.068)	(0.069)	(0.065)	(0.084)
Nation A	0.127*	0.225***	0.196**	0.200***	0.135
	(0.077	(0.077	(0.078)	(0.071)	(0.089)

Marginal effects; Standard errors in parentheses. Linear outcome model used. Multinominal logit used for propensity score reweighting. Heteroskedasticity robust standard errors reported. Observations vary due to missing responses. Significance stars: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. "IPW" is inverse probability weighted estimates. "Doubly-robust" is a doubly-robust inverse propensity score weighted and regression adjusted estimator.

Table A.3: Average Treatment Effect Conditional on Difference in Baseline Opinion Measures: Adjusting for Random Differences Respondent Characteristics Across Treatments

	Unadjusted	Regression	IPW	Doublely Robust	Orded Probit
			Good for Bank [X] to open		
Federal Reserve	0.0497	0.0676	0.00674	0.0243	0.026
	(0.165)	(0.172)	(0.163)	(0.162)	(0.086)
Nation A	-0.16	-0.165	-0.220	-0.188	-0.099
	(0.160)	(0.167)	(0.170)	(0.158)	(0.083)
			Become a customer of Bank [X]		
Federal Reserve	-0.158**	-0.087	-0.0904	-0.0891	-0.238**
	(0.062)	(0.065)	(0.063)	(0.062)	(0.093)
Nation A	-0.148**	-0.154**	-0.183**	-0.158**	-0.257***
	(0.060)	(0.071)	(0.073)	(0.067)	(0.092)
			Support of B ownership of Bank [X]		
Federal Reserve	0.163*	0.0825	0.0798	0.0759	0.141*
	(0.088)	(0.068)	(0.069)	(0.065)	(0.082)
Nation A	0.207**	0.225***	0.241***	0.200***	0.185**
	(0.091)	(0.077)	(0.074)	(0.071)	(0.084)

Marginal effects; Standard errors in parentheses. Linear outcome model used. Multinominal logit used for propensity score reweighting. Heteroskedasticity robust standard errors reported. Observations vary due to missing responses. Significance stars: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. "IPW" is inverse probability weighted estimates. "Doubly-robust" is a doubly-robust inverse propensity score weighted and regression adjusted estimator.

Table A.4: Components of Indexes for Heterogeneous Treatment Effects

Index	Component Description	Median of Index	Cut-off Point for Indicator =1
Discount Rate	No bank account	0.143	0.28
	Can't get 400 dollars in an emergency		
	Household less than $10,000 per year		
	Has more than three different source of debt		
	Most of the time or always uses cash checking		
	Primary earner for a larger household		
	(responsible for at least 3 children)		
	(and one other non-spouse adult)		
	Very poor self-assessed credit		
Financial Resilience	Household income over 80,000	0	0.5
	Very good self-assessed cred		
Negative Views of Banks	Believes banks don't have their best interests at heart	0	0.1
	Has opinion bank accounts are too complicated		
	They have felt disrespected by banks		
	Has opinion that bank fees are too high		
	Has opinion you lose control of your money in a bank		
	Has opinion that banking is unnecessary		
	Has opinion money is not safe in a bank		
	Has opinion you lose your privacy to banks		
	Has opinion going to banks is unpleasant		
	Low trust in banks		
	(ranks trust in banks less than 5 out of 10)		
Community Connection	Lives on reservation	0.5	0.667
	Knew a bank was opening		
	Pays attention to news most of the time		
	Employed bin Tribal Government		
	Has tribal loan debt		
	Learned about finance from community program		
Financial Knowledge	High self accessed financial knowledge	0.667	1
	Knew they could get credit score for free		
	Handles household finances		

The index is constructed by $\frac{\sum components}{no.non-missingresponses}$. The binary indicator equals one when the value of the index surpasses the 75th percentile of the distribution. When the median and the 75th percentile have the same value, we chose the 90th percentile of the index as the cut-off.

Table A.5: Correlations Between Measures for Heterogeneous Treatment Effects

	Indexes				
	Higher discount rate	Financial resilience	Knows about Finance	Connected to Community	Negative Views of Banks
Higher discount rate	1				
Financial Resilience	-0.2897	1			
Knows about finance	0.0076	0.1725	1		
Connected to Community	-0.0095	0.0667	0.324	1	
Negative Views of Banks	0.1574	-0.09	-0.1173	-0.0666	1
	Binary Measures				
	Higher discount rate	Financial resilience	Knows about Finance	Connected to Community	Negative Views of Banks
Higher discount rate	1				
Financial Resilience	-0.0758	1			
Knows about finance	0.0778	0.0313	1		
Connected to Community	0.0024	0.0362	0.2787	1	
Negative Views of Banks	0.1123	-0.0408	-0.0947	-0.0359	1

See description in Table A.4 for the construction of the indexes. The cells present the correlation coefficients between the indexes in the first panel and the binary measures in the second.

Table A.6: Heterogeneity in treatment by indicators of financial stability, knowledge, connection to the local community, and beliefs about banks: Outcomes of interest in levels

	Expressed effect treatment	Good for Bank [X] to open	Support for B Ownership of Bank [X]	Become a Customer of Bank [X]	Expressed Interest in credit report
			Higher Discount Rate		
Federal Reserve	0.108	-0.117	-0.177	-0.271	-0.00666
	(0.174)	(0.206)	(0.199)	(0.207)	(0.200)
Nation A	0.0947	-0.0701	0.0124	-0.126	-0.0528
	(0.209)	(0.220)	(0.221)	(0.228)	(0.204)
			Financially Better Off		
Federal Reserve	-0.374	-0.286	0.0218	-0.846	-0.193
	(0.459)	(0.447)	(0.422)	(0.538)	(0.502)
Nation A	-0.597***	-0.137	0.111	-0.458	0.288
	(0.153)	(0.364)	(0.432)	(0.486)	(0.498)
			Knowledgeable about Finance		
Federal Reserve	0.471***	0.113	-0.0218	-0.0456	0.0803
	(0.175)	(0.172)	(0.180)	(0.199)	(0.181)
Nation A	0.624***	0.139	0.209	0.165	0.0379
	(0.160)	(0.170)	(0.192)	(0.196)	(0.179)
			Connected to Community		
Federal Reserve	0.417**	-0.119	0.306*	0.0695	-0.0341
	(0.174)	(0.172)	(0.180)	(0.186)	(0.181)
Nation A	0.396**	0.0757	0.207	0.372**	-0.239
	(0.162)	(0.158)	(0.196)	(0.180)	(0.181)
			Negative Believes about Banks		
Federal Reserve	0.0952	-0.0263	-0.0255	-0.0712	-0.0962
	(0.129)	(0.154)	(0.151)	(0.156)	(0.157)
Nation A	0.0471	0.0982	0.0869	0.0845	0.0277
	(0.132)	(0.155)	(0.160)	(0.161)	(0.155)

Notes: The cells show the sign and statistical significance of the interaction term between the binary measures of respondents having a higher discount rate, being financially resilient, knowledgeable about finance, and connected to the community and whether they have negative attitudes about banks. The construction of these variables are discussed in A.4. The underlying coefficients can be found in Tables A.6 and . Observations vary due to missing responses. Significance stars: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Heterogeneity in treatment by indicators of financial stability, knowledge, connection to the local community, and beliefs about banks: Outcomes of interest in differences

	Good for Bank [X] Open	Support for B Ownership] of Bank [X]	Become a Customer of Bank [X]
		Higher Discount Rate	
Federal Reserve	0.0996	-0.244	0.0304
	(0.184)	(0.202)	(0.224)
Nation A	-0.251	-0.194	0.00231
	(0.195)	(0.219)	(0.219)
		Financially Better Off	
Federal Reserve	0.349	0.936*	0.429
	(0.324)	(0.525)	(0.630)
Nation A	-0.146	1.051**	0.564
	(0.232)	(0.451)	(0.623)
		Knowledgeable about Finance	
Federal Reserve	-0.0742	0.219	0.0494
	(0.172)	(0.187)	(0.219)
Nation A	0.0205	0.168	0.268
	(0.170)	(0.196)	(0.187)
		Connected to Community	
Federal Reserve	-0.102	0.411**	0.117
	(0.171)	(0.191)	(0.199)
Nation A	0.0119	0.299	0.548***
	(0.139)	(0.192)	(0.164)
		Negative Believes about Banks	
Federal Reserve	0.288*	-0.113	-0.0504
	(0.160)	(0.154)	(0.160)
Nation A	0.286*	-0.270*	-0.208
	(0.157)	(0.160)	(0.157)

Notes: Normalized outcomes variables calculated as 1: The cells show the sign and statistical significance of the interaction term between the binary measures of respondents having a higher discount rate, being financially resilient, knowledgeable about finance, and connected to the community and whether they have negative attitudes about banks. The construction of these variables are discussed in A.4. The underlying coefficients can be found in Tables A.6 and . Observations vary due to missing responses. Significance stars: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.