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Essays on the political economy of development: Determinants of political and economic behavior

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PhD thesis

Gustav Agneman

Essays on the Political Economy of Development:

Determinants of Political and Economic Behavior

Advisor: David Dreyer Lassen

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Acknowledgements

I never really had a life plan. If I would have had one, it would most likely not have involved me doing a PhD in Economics. Somehow, many lucky encounters led me onto this path: one pushed me to study economics, another guided me towards Copenhagen, and a third suggested that I could do a PhD. It turned out I could. I am forever grateful to the teachers, professors, fellow students and friends that for some reason believed in me and helped me to punch above my weight.

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During my PhD I have led two large field research projects: one in Greenland and one in Tanzania. The former was made possible by a generous grant from Kraks Fond Byforskning, and through cooperation with a range of excellent researchers, namely Allan Olsen, Minik Rosing, Kelton Minor, Nadine Kleemann, Ulunnguaq Markussen, Navarana Davidsen, Hans Peter Mønsted and Betina Berthelsen, as well as faculty members of Ilisimatusarfik (the University of Greenland). The project in Tanzania was sponsored and supported by the Development Economics Research Group of the University of Copenhagen (DERG), and executed by a large number of outstanding research assistants from the Singida region in Tanzania: Antony Mwangolombe, Bruno Magazi, Emelita Kyenga, Fabian Changa, Gurisha Msemo, Leah Azza, Laurent Mkomwa, Lilian Sangawe, Malkia Mbilinyi, Mariam Denis, Mathew Saktay, Neema Stanford Kishebuka, Robert Chizinga, Sigisberto Mwacha, Selina Nombo, Sungu Muadh and Zakayo Ijojo. I owe my thanks to all of them as well as to the members of DERG who have been supportive of my work.

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Despite all the great encounters and experiences, writing a PhD has been the toughest challenge of my life. Periodically, I have struggled with stress and anxiety. The fact that I overcame these difficulties is mainly due to the great support I received from my family, friends and colleague-friends. For this, I am forever grateful.

Gustav Agneman
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Dansk introduktion

Denne ph.d.-afhandling består af fire selvstændige kapitler i politisk udviklingsøkonomi. De er alle empiriske projekter, der undersøger beslutningstagning under heterogene forhold. I det første kapitel undersøger jeg årsagssammenhængen mellem økonomiske forventninger og stemmeadfærd i en folkeafstemning om uafhængighed. Jeg dokumenterer en stærk forbindelse som modereres af vælgernes identitet. I det andet kapitel kortlægger vi statskapacitet på subnationalt niveau i Afrika og viser, at risikoen for olieinducerede konflikter afhænger af lokal statskapacitet. Det tredje kapitel fokuserer på sammenhængen mellem fødevareknaphed og pro-sociale investeringer. Ved at udnytte høsten som et eksogent chok for fødevareforsyningen i landdistrikterne i Tanzania dokumenterer vi en kausal sammenhæng fra fødevareknaphed til lavere samarbejde. Det fjerde og sidste kapitel studerer moralsk beslutningstagning i Grønland. Vi udvikler et mål for "parochial ærlighed - tilbøjeligheden til at opføre sig uærligt over for udenforstående, men ikke over for medlemmer af ens egen gruppe - og viser, at markedseksponering forudsiger graden af gruppedifferentiering.

Chapter 1 – How Economic Expectations Shape Preferences for National Independence: Evidence from Greenland

Dette første kapitel undersøger rollen som økonomiske forventninger spiller i udformningen af præferencer for og imod politisk uafhængighed. Selvom politiske kampagner i folkeafstemninger tenderer at fokusere på vælgernes økonomiske konsekvenser af de potentielle scenarier, er det uklart, i hvilken grad informationskampagner kan påvirke forventningerne, og om de økonomiske forventninger til gengæld former vælgernes adfærd. Jeg anvender et spørgeskemaeksperiment for at dokumentere den kausale effekt af økonomisk information på vælgeradfærd i en hypotetisk folkeafstemning i Grønland, og for at undersøge de mekanismer, der forklarer denne sammenhæng. Resultaterne afslører, at vælgerne er meget modtagelige for negativ økonomisk information, idet de både ændrer deres forventninger om økonomiske effekter af selvstændighed, og er mere tilbøjelige til at modsætte sig umiddelbar selvstændighed når de tilfældigt bliver eksponeret for økonomisk information. Den forandring i vælgeradfærd, der fremkaldes af informationen, er så stor, at den ville ændre resultatet af hele selvstændighedsafstemningen. Imidlertid er respondenter med en stærk grønlandsk identitet totalt upåvirket af informationen. Dette resultat understreger en mere generel indsigt fra undersøgelsen: informationskampagner kan kun svinge vælgere, der er villige til at opdatere deres forventninger, og hvis forventningerne faktisk påvirker, hvordan de afgiver deres stemme.

Chapter 2 – Predicting Local State Capacity in Africa: A Machine Learning Approach *with Kasper Brandt, Christoffer Pfeiffer Cappelen, and David Sjöberg*

Det andet kapitel præsenterer en ny metode for at måle statskapacitet på subnationalt niveau i områder hvor relevante data mangler. Vi konstruerer et indeks over statskapacitet baseret på spørgeskemadata om staters evne til at opretholde lov og orden, opkræve skat, samt levere tjenester på et lokalt niveau. Dernæst forudsiger vi dette indeks ved hjælp af faktorer der påvirker kapacitetsbygning, herunder rejsetid til hovedstaden, historisk befolkningsstørrelse og lysemissioner om natten, osv. Endelig ekstrapolerer vi forudsigelsen sådan at vi kan skabe et omfattende indeks af statskapacitet på tværs af det afrikanske kontinent. Vi viser i en række valideringstests at vores indeks korrelerer stærkt med alternative mål for statskapacitet, nemlig præ-kolonial centralisering, lokale etniske gruppers nutidige politiske magt og vaccinationsrater. Derefter anvender vi indekset af lokal statskapacitet som en modererende variabel i forholdet mellem olieformue og væbnet konflikt. Vi finder at ikke alle regioner er lige så tilbøjelige til at blive udsat for konflikt på grund af eksogene fluktuationer af deres olieformue. Mens regioner med lave niveauer af lokal statskapacitet ser kraftige stigninger i sandsynligheden for olierelaterede konflikter, når olieformuen stiger, er der ingen sådanne effekter i regioner med høj statskapacitet. Dette resultat fremhæver behovet for statslig kapacitetsopbygning for at begrænse vold i udviklingslande.

Chapter 3 – Does Scarcity Reduce Cooperation? Experimental Evidence from Rural Tanzania *with Paolo Falco, Exaud Joel and Onesmo Selejio*

Det tredje kapitel studerer indflydelsen af fødevareknaphed på samarbejde. Vi benytter eksogen variation i fødevareknaphed induceret af Msimu-høsten i Tanzania ved at gennemføre økonomiske eksperimenter med landmændene før og efter høsten. Landmænd er væsentligt mere tilbøjelige til at opleve knaphed på fødevarer inden høsten. De er også betydeligt mindre tilbøjelige til at investere i samfundsmæssigt optimale, men personligt risikable, investeringer, under den periode. Vi viser, at fødevareknaphed mindsker investeringer, en adfærdsmæssig ændring, som vi mener, forklares ved, at landmændene foretrækker sikre over risikable (men potentielt rentable) muligheder, når de står over for knaphed. Effekten af sæsonbestemt fattigdom på samarbejde, der er dokumenteret i denne undersøgelse, viser hvordan sæsonbestemt knaphed kan lede til yderligere knaphed, og dermed bidrage til vad vi kalder en adfærdsmæssig fattigdomsfælde.

Chapter 4 – Parochial Honesty and Market Exposure: Experimental Evidence from Greenland *with Esther Chevrot-Bianco*

I det fjerde og sidste kapitel undersøger vi prævalensen og determinanterne for "parochial ærlighed, tendensen til at være mere ærlig over for medlemmer af ens egen gruppe i forhold til medlemmer uden for ens egen gruppe. Vi gennemførte økonomiske eksperimenter om ærlighed i Grønland, hvor små og geografisk isolerede samfund skaber en naturlig afgrænsning mellem ind- og udgruppe. Resultaterne afslører en signifikant differentiering i moralsk beslutningstagning. Mens deltagerne snyder udgruppen, afstår de konsekvent fra at snyde deres egen gruppe. Parochial ærlighed er meget mere udbredt blandt deltagere i den traditionelle økonomi, der er mindre eksponeret for markedsinstitutioner og daglige transaktioner med udenforstående. Dette resultat bekræfter markedsintegrationshypotesen, som postulerer, at økonomisk og social integration styrker hinanden. Mere konkret udvider markedseksponering prosociale normer til at omfatte også udenforstående økonomiske partnere. Vores undersøgelse er den første til at styrke markedsintegrationshypotesen ved brug af mikro-data.

English introduction

This PhD dissertation consists of four self-contained chapters in the field of Political Development Economics. They are all empirical projects studying decision-making under heterogeneous conditions. In the first chapter, I investigate the causal link between economic expectations and voting in an independence referendum, and document that the effect is contingent upon voters' identity. In the second chapter we map state capacity at the subnational level in Africa and show that the risk of oil induced conflicts depends on levels of local state capacity. The third chapter focuses on the link between food scarcity and cooperative investments. Exploiting the harvest as an exogenous shock to food supply in rural Tanzania, we document a causal role of food scarcity in suppressing socially efficient cooperation. The fourth and final chapter studies moral decision making in Greenland. We develop a measure of parochial honesty – the propensity to behave honestly toward the ingroup but not toward the outgroup – and show that market exposure predicts the degree of group differentiation.

Chapter 1 – How Economic Expectations Shape Preferences for National Independence: Evidence from Greenland

This first chapter investigates the role of prospective economic evaluations in shaping preferences for and against political independence. Although campaigns in conjunction with referendums tend to focus on the economic consequences of the potential outcomes, it is unclear to what degree information campaigns can influence expectations and whether, in turn, changes in economic expectations influence voter behavior. I employ a survey experiment to outline the effect of information on voting in a hypothetical referendum in Greenland, and to document the mechanisms that explain this linkage.

The results reveal that voters are highly susceptible to pessimistic information, in that they both change their economic expectations of independence and are more likely to oppose immediate secession when exposed to the prime. The change induced by the information prime is so large it would alter the outcome of the entire independence referendum. However, respondents with a strong Greenlandic identity (as proxied by language proficiency) are completely unaffected by the prime. This finding conveys a more general insight from the study: information campaigns may only sway voters who are willing to update their economic expectations, and whose economic expectations actually influence how they cast their ballot.

Chapter 2 – Predicting Local State Capacity in Africa: A Machine Learning Approach *with Kasper Brandt, Christoffer Pfeiffer Cappelen, and David Sjöberg*

The second chapter presents a novel methodology to measure state capacity at sub-national levels where relevant data are lacking. We create an index of state capacity based on survey data on states' ability to uphold law and order, collect taxes and provide services at the local level. Next, we predict this index using satellite data on relevant constraints to capacity building, inter alia travel time to the capital, historic population size and night-time light emissions, to mention just a few factors. Lastly, we extrapolate the resulting prediction to construct a comprehensive index of state capacity across the African continent.

We show in several validation checks that our measure strongly correlates with alternative proxies of state capacity, namely pre-colonial centralization, contemporary political power of local ethnic groups, and vaccination coverage. Then we employ the index of local state capacity as a moderating variable in the relationship between oil wealth and armed conflict. We find, as hypothesized, that not all regions are equally likely to face conflict due to exogenous shocks in oil wealth. Whereas regions with low levels of local state capacity see sharp increases in the probability of oil related conflicts when oil wealth goes up, high state capacity-regions experience no such effects. This result highlights the need for state capacity building in order to constrain violence in developing contexts.

Chapter 3 – Does Scarcity Reduce Cooperation? Experimental Evidence from Rural Tanzania *with Paolo Falco, Exaud Joel, and Onesmo Selejio*

The third chapter studies the influence of food scarcity on cooperation. We leverage exogenous variation in food scarcity induced by the Msimu harvest in rural Tanzania, by conducting framed investment games with farmers before and after the harvest. Farmers are both more likely to experience food scarcity *and* to refrain from investing in socially efficient cooperation during the lean period prior to the harvest. We show that food scarcity suppresses investments, a behavioral change which we posit is explained by participants preferring safe over risky (but potentially profitable) options when facing scarcity. The detrimental effects of seasonal poverty on cooperation documented in this study highlights the need to consider seasonal scarcity as a force that might itself perpetuate poverty, and thus contribute to what is commonly referred to as a behavioral poverty trap.

Chapter 4 – Parochial Honesty and Market Exposure: Experimental Evidence from Greenland *with Esther Chevrot-Bianco*

In the fourth and final chapter we investigate the prevalence and determinants of parochial honesty, the tendency to behave more honestly toward members of the ingroup relative to members of the outgroup. To this end, we conducted experiments on honesty in Greenland, where small and geographically isolated communities provide for a natural demarcation between ingroup and outgroup. The results reveal significant differentiation in moral decision-making. While participants cheat the outgroup, they consistently refrain from cheating their own group.

The baseline differentiation is entirely driven by participants in the traditional economy, who are less exposed to market institutions and daily transactions with outsiders. This result aligns with the Market Integration Hypothesis, which posits that economic and social integration reinforce one another. More concretely, market exposure extends pro-social norms to encompass also economic partners from more distant groups. Our study provides a first account of within-community variation in support of this theory.

Chapter 1

How Economic Expectations Shape Preferences for National Independence: Evidence from Greenland

How Economic Expectations Shape Preferences for National Independence: Evidence from Greenland*

Gustav Agneman

Abstract

In this chapter, I investigate how economic expectations shape voting intentions in a hypothetical independence referendum in Greenland, a self-governing region of the Kingdom of Denmark. I identify the causal effect of economic expectations by randomly exposing respondents to a prime informing on Greenland's current fiscal deficit. The results show that respondents exposed to pessimistic economic information are 43 percent more likely to vote no to independence, an effect I demonstrate is due to information updating, and not to a mere shift in salience. I further document that respondents' identity moderates the impact of the prime. While being exposed to the information substantially increases opposition to independence among voters with mixed national identity, voting behavior is essentially unchanged among respondents with strong Greenlandic identity. I link this voter heterogeneity to a lesser degree of economic voting among respondents with stronger national identity. The present paper reveals a significant role of instrumental motives in shaping preferences for and against secession; the change induced by the information prime would alter the outcome of the independence referendum.

Keywords: secession, voting behavior, survey experiment
JEL Codes: H77, D74, D72

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1 Introduction

The economic costs and benefits of independence are typically valence issues of great contention in secession aspiring regions (Dardanelli and Mitchell 2014). Presumably, the heavy focus on the economic impact of independence is motivated by the strong association between voting and economic expectations; campaigners attempt to shift voters' economic expectations in order to win their votes. But while economic expectations may impact independence preferences, they might equally well reflect rationalizations of voters' pre-existing preferences for or against secession (Howe 1998). The circular nature of the relationship implies that mere correlations cannot inform on the effectiveness of economic information campaigns as political instruments. How malleable are voters' economic forecasts, and do changes in expectations translate into changes in voting behavior?

In order to outline the causal effect of economic expectations on voter behavior in an independence referendum, I collect novel data on independence preferences in Greenland, a constituent country with strong secessionist pressure and significant economic reliance on its current political union, the Kingdom of Denmark. Greenland is an ideal setting to investigate how economic concerns influence independence aspirations, since there is little doubt that independence, at least in the short run, would hurt the Greenlandic economy. Moreover, a former colony, Greenland provides a suitable context to study how economic and identity-based rationales interact to shape independence preferences.

I overcome the usually suspected endogeneity concerns (Wleizen, Franklin, and Twiggs 1997) by exposing a random subset of respondents to an information prime informing respondents about Greenland's fiscal deficit. The prime is a short text about the yearly direct and indirect transfers that Greenland receives from Denmark. It includes costs which are generally overlooked in the discussion about the economics of Greenlandic independence, such as public service provision funded directly by Denmark, and therefore primes respondents on a pessimistic economic scenario. I find that exposure to the economic information substantially increases opposition to independence. Respondents that are assigned to the information prime are 42.9 percent more likely to vote no to independence compared to the control group. For respondents reporting that they read the information (complied with the treatment), opposition increased by 58.1 percent. While respondents in the control group are predominantly in favor of independence, a majority of respondents exposed to the prime would vote against independence. In other words, the change induced by the prime is so large it would alter the outcome of the independence referendum.

Three channels can potentially account for the baseline results. First, the prime could negatively affect voters' economic expectations of independence, which, in turn, could nudge respondents to change their voting stance. I label this the *expectations effect*. Second, if information that align with prior beliefs increases certainty, and certainty promotes voting (see e.g. Lassen 2005), the prime could encourage voters with pre-existing pessimistic economic forecasts to participate in the referendum. I denote this the *rallying effect*. Third, the prime could impact also in the absence of information updating, by enhancing the salience of – and thereby the weight voters ascribe – the economic aspect of independence (henceforth labeled the *salience effect*).[1] I find evidence of the first two mechanisms and a marginal but insignificant salience effect, suggesting that information updating mainly accounts for the impact of the prime. Respondents exposed to economic information make more pessimistic prospective economic evaluations of independence. Moreover, conditional on making negative economic assessments, they are more likely to vote.

I proceed to show how identity moderates the effect of the information prime. In line with Muñoz and Tormos (2015), I expect respondents with a stronger national identity to be less concerned with the economic consequences of independence and, therefore, to exhibit less voting behavior malleability. The findings confirm this prior. Respondents with stronger Greenlandic identity (proxied by language proficiency in Greenlandic but not in Danish) are less likely to change their economic expectations when exposed to the prime and less likely to engage in economic voting. As a consequence, the information prime has no impact on their voting behavior.

The present paper contributes both to the literature on economic voting (Peltzman 1992; Aidt 2000; Lewis-Beck and Stegmaier 2000) and to the literature concerned with the

1. This channel is derived from Issue Ownership Theory (Bélanger and Meguid 2008). Issue Ownership Theory simply posits that a political cause (e.g. opposition to independence) can gain from focus being shifted to an issue (in this case the economic aspects of secession) in which this cause is considered to have a comparative advantage.

determinants of secession (Alesina and Spolaore 1997; Bolton and Roland 1997; Bordignon and Brusco 2001; Olofsgård 2003; Leduc 2002; Collier and Hoeffler 2011). Methodologically, I build on the burgeoning experimental political economy literature, and in particular I draw from studies outlining causal determinants of political behavior by means of randomized interventions (Tyran 2004; Gerber, Karlan, and Bergan 2009; Chiang and Knight 2011; Bassi, Morton, and Williams 2011; Alt, Lassen, and Marshall 2016; Pons 2018; Alesina, Stantcheva, and Teso 2018; Cantoni et al. 2019; Goodwin, Hix, and Pickup 2020).

My theoretical framework links closely with that of studies on the economics of secessionist conflicts (Le Billon 2001; Lujala 2009; Lujala 2010; Hunziker and Cederman 2017). But whereas the determinants of secessionist conflicts has attracted much academic attention, democratic secessionism has largely been exempted from quantitative inquiries. By investigating voting behavior in a hypothetical independence referendum in Greenland, the present study interlinks the small but growing empirical literature on the economics of democratic secessionism (Muñoz and Tormos 2015; Gehring and Schneider 2020) with the emergent literature on political behavior in the developing world (Vicente and Wantchekon 2009; Banerjee et al. 2010; Collier and Vicente 2014). Although most independence referendums take place in developing countries (Mendez and Germann 2018), quantitative micro-evidence on the underlying drivers of those processes is scarce. To the best of my knowledge, this study is the first to document a causal effect of economic expectations on democratic independence preferences in a developing context.

The paper is structured as follows. Section 2 reviews the literature on the economics of democratic secessionism and presents a simple model of voter motivations in independence referendums. Section 3 introduces the empirical setting. In Section 4, the data and experimental design are described, as are the hypotheses and empirical approach. Section 5 presents the main results and discusses their implications. Section 6 investigates the moderating role of identity, and Section 7 outlines the treatment effect on inter-temporal preferences for independence. Section 8 concludes.

2 Secessionist rationales

All democratic independence movements rely on some regional particularity(ies) that distinguish the separatist region from the current political union, for instance language, history or ethnicity (Lehning 2005). A territorially linked identity can unite diverse expressions of secessionism as well as legitimizing their end-goal (Buchanan 1997). Hence, a distinguished regional identity appears to be a *necessary* condition for secessionist movements to form. It is not, however, a *sufficient* condition for secessionism to grow strong (Sorens 2005). There is substantial variation in the intensity of secessionist pressure, both over time and across space, which cannot be explained by identity concerns. For instance, both Scotland and Wales are nations distinct from their current political union, but independence is mainly topical in Scotland. According to Gehring and Schneider (2020), economic expectations of independence is a key factor explaining the intensive margin of secessionist pressures in Scotland. They outline how regional wealth shocks impact secessionist preferences by showing that oil discoveries and oil price shocks, which bolster the regional wealth of Scotland but not of Wales, enhanced support for the Scottish National Party (SNP), while Plaid Cymru, the main secessionist party in Wales, remained unaffected. In conclusion, a territorially linked identity is a pre-requisite for secessionism to form in the first place; beyond that, economic prospects are a first order concern for voters.[2]

Evidence on the micro level determinants of independence aspirations is scarce, but the little evidence that exists align with the macro level findings. Muñoz and Tormos (2015) document an independent role for economic considerations in shaping voting intentions in a hypothetical referendum in Catalonia. Moreover, they find that economic expectations correlate *less* with voting behavior among respondents with *stronger* Catalan identities, relative to respondents with mixed identities. Although this evidence is purely correlational, it suggests that voters might trade off economic and identity-based concerns when voting on independence. In the following section, I present a model of voting behavior that incorporates

2. The macro level interlinkage between identity and economic rationales is documented also by Desmet et al. (2011), who study the disintegration of former Yugoslavia, as well as in the study by Morelli and Rohner (2015) on the interaction between ethnic homogeneity and resource wealth in promoting secessionist conflicts.

these dynamics, before proceeding to test the resulting predictions empirically.

2.1 Model of independence voting

In what follows, I construct a simple probabilistic voting model to formalize factors that influence voter choice in independence referendums. I consider a voter who can vote either leave or remain. The choice depends on a random ideology parameter I centered around 0, and on the utility of consumption in the leave and remain scenarios. Moreover, a weight parameter (α) indicates the importance a respondent assigns the economic consequences of independence relative to ideology. This characterization aligns with previous work on voter rationales (Shayo 2009; Klor and Shayo 2010), in which voters are shown to trade off identity and economic motives against each other. The voter will vote to leave if:

$$(1-\alpha)I - \alpha(u(c_r) - u(c_l)) > 0 \tag{1}$$

where c_l and c_r denote consumption of a representative voter if the region leaves or remains, respectively. To be more precise, c_l and c_r represent voters' beliefs about the average consumption in the two states of the world. While c_r is known, c_l is clouded by uncertainty. In order to add uncertainty, I substitute c_l for a lottery which gives ${c_l}^L$ (low consumption) with probability p and ${c_l}^H$ (high consumption) with probability $(1-p)$. So the voter now supports leave if:

$$(1-\alpha)I - \alpha(u(c_r) - u(p{c_l}^L + (1-p){c_l}^H)) > 0 \tag{2}$$

For simplicity, I assume that beliefs about consumption in the two states of the world are the same for all voters, and that they relate to each other as follows:

$${c_l}^H > c_r > {c_l}^L \tag{3}$$

The only variables that influence voter decisions are p_i and α_i. The economic expectations can be either positive or negative depending on p_i, but based on the economic realities of Greenland, I will assume that the median voter has a negative economic outlook of independence such that $(\tilde{p_i} \times {c_l}^L + (1 - \tilde{p_i}) \times {c_l}^H) < c_r$. The initial state of α is assumed to differ for different types of voters. Strong identity voters have relatively low α, whereas mixed identity voters exhibit relatively high α. In addition, α is malleable, but strong identity voters are assumed to have strictly lower α. The following predictions are then straightforward to derive:

1. If p increases, voters will be more likely to oppose independence.
2. If α increases, the probability that the median voter opposes independence increases.
3. For voters with relatively higher α, changes in p influence voter choices more.

3 Empirical setting

Secessionism in Greenland, the empirical setting of this study, is commonly discussed in the light both of identity and economic factors. Some 3,500 kilometers separate the regional capital, Nuuk, from Denmark's capital, Copenhagen, and the majority of the population in Greenland are Greenlandic Inuit, as opposed to Danes. The distinct nationhood has nurtured an identity-based appeal for independence (Breum 2015). But as a net-recipient of regional redistribution, independence would, at least in the short-run, entail substantial economic costs. The trade-off between identity and economic factors that many Greenlanders supposedly experience makes this an ideal case for studying the causal role of economic expectations in shaping independence preferences, and to investigate how identity moderates this relationship.

Formerly a Danish colony, Greenland has gradually gained political autonomy over time. In a national referendum 2008, a majority of the Greenlandic electorate favored a proposal to extend self-determination, which resulted in the passing of the Self-Government Act (Grydehøj 2016). The new act replaced the Home Rule Act from 1978 and meant that the Greenlandic government would overtake some core administrative duties that up until then had been the responsibility of Danish authorities. The arrangement also provided the Greenlandic government the legal means to unilaterally call for a referendum on its separation from the present political union (Gad 2014), stating that the "Decision regarding Greenland's independence shall be taken by the people of Greenland"[3].

Secessionism in Greenland has been closely linked to its natural resource wealth (Taagholt and Brooks 2016), and the potential of commercial hydrocarbon deposits has recurrently been highlighted as a way toward economic independence. The then Prime Minister Kuupik Kleist stated in 2011 that "... possible findings of oil have increased the debate on the issue of independence" (cross-referenced from Poppel 2018). But hopes of substantial petroleum rents have yet to materialize, and as a result the secessionist pressure has somewhat dampened.[4] As of now, Greenland relies on Denmark to finance its relatively large public sector. Besides an annual transfer of around 3,6 billion DKK[5], covering approximately half of Greenland's budget, Denmark still administers a number of costly public services.[6] Consequently, near-future political independence resulting in an immediate reduction or stop of Danish economic support would imply either dramatic cuts in welfare provision, sharp increases in tax rates, or both.

4 Data and experimental design

4.1 Sampling strategy

The empirical analysis builds on data from an original survey conducted on the ground in Greenland between July-September 2018. The selection of participants was determined by a two-stage random sampling procedure. First, we sampled 13 villages from all regions of Greenland using a stratified multi-stage cluster approach. Second, we randomly sampled respondents from the universe of adult residents in the selected villages using Greenlandic register data. As a result of the sampling procedure, the final sample approximates well the Greenlandic population, at least in terms of age (Figure A1 in the Appendix Section B) and party affiliation (Table A2 in the Appendix Section B).[7] Figure 1 maps the sampled localities

3. See Act on Greenland Self-Government from June 12th 2009 (Naalakkersuisut 2009).
4. See The Economist (January 21st 2015).
5. The block grant varies slightly from year to year due to inflation.
6. See CNBC (25th April 2018).
7. Notwithstanding the representativeness of the sample in the Greenlandic context, the results are not necessarily generalizable to other settings. In the Appendix C, I discuss how the results should be evaluated

from which respondents were recruited.

Figure 1: Map of Greenland

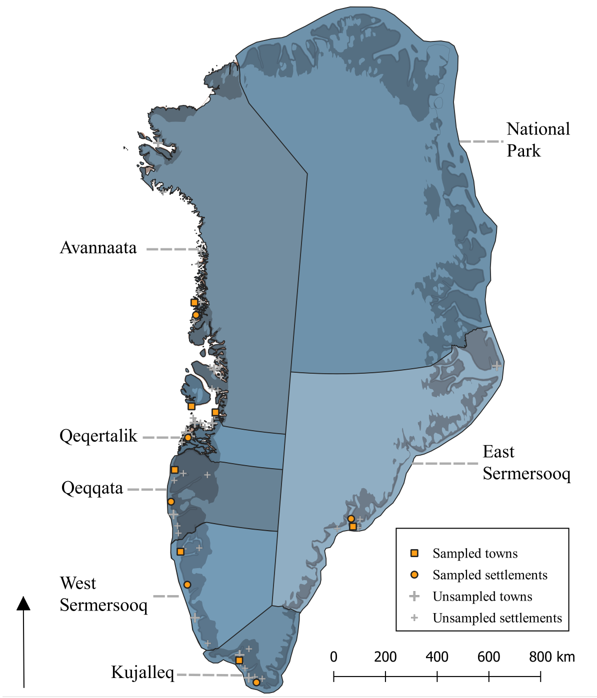

Figure 1: Map of Greenland displaying the 13 sampled as well as a the unsampled villages. The well-balanced spatial and demographic distribution of sampled localities was ensured by means of stratification of villages before randomization.

In total, 1400 Greenlandic residents were invited and 622 participated (approximately 1.5% of the adult population in Greenland). The survey was in pen-and-paper format, and participants completed it individually either at field sessions held at local schools and town halls, or in their homes. Trained Greenlandic enumerators interviewed respondents who were not able to complete the survey on their own. A detailed account of the sampling procedure and data collection is outlined in the Appendix Section A.

4.2 Data

The main variables of interest are presented along with descriptive statistics in Table A3 in the Appendix B. In order to complement the survey data, I linked 468 respondents[8] with

in the light of context specific factors.

8. Survey data from 154 respondents were not possible to merge with the register data due to missing identifying information.

objective individual data from the Greenlandic registers. The register based data is useful for ensuring that measurement error due to inaccurate reporting does not influence the findings.

4.2.1 Outcome variables

The outcome variable of main interest is a dummy indicator for opposition to independence. The variable is based on the survey item S1, from which the alternative “No” is defined as opposition to immediate independence. I further construct a variable that captures whether a participant voted (answered “Yes” or “No”) or abstained (answered “I don’t know” or “I would not vote”).

S1. Voting intentions
If there was a referendum on independence TODAY, asking if you wanted Greenland to leave the Kingdom of Denmark, how would you vote?
(Yes; No; I don’t know; I would not vote)

A mediating variable central to the analysis is respondents’ subjective economic expectations of independence, derived by the survey item S2. The question was asked in likert-scale format in order to ensure comprehensibility.

S2. Economic expectations
If Greenland becomes independent within the next 10 years, this will impact Greenland’s economy...
(Very negatively; Somewhat negatively; Not at all; Somewhat positively; Very positively)

4.3 Experimental design

The survey contained a randomized component which intended to experimentally shift economic expectations. This was implemented by assigning respondents into one of three treatment groups: the Control Group (CG), the Salience Treatment (ST) or the Information Treatment (IT). The experimental design is summarized in Figure 2. Vote stands for answering questions on independence preferences (S1), Expectations encompasses questions on the expected economic effects of independence (S2 as well as two additional questions detailed in Section 4.3.2), and Prime indicates the Information Treatment detailed in Section 4.3.1. Note that all respondents were subject to the same three survey sections; it was the timing of sections that differed between treatments.

Figure 2: Flowchart describing the timing of survey components

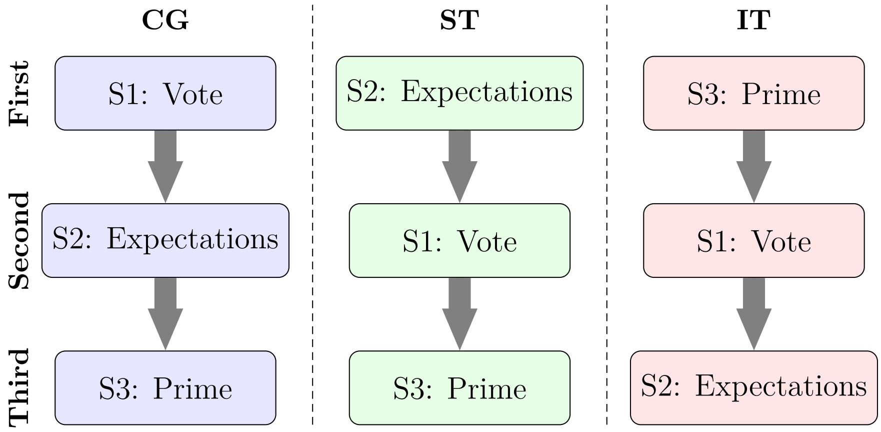

Figure 2: Flowchart depicting the timing of survey sections for each treatment group.

Respondents were assigned to treatments by means of a Randomized Block Design[9], with the village as the block unit, and an equal likelihood of being assigned into either one of the treatments. Consequently, each treatment group contains roughly a third of the respondents, both in the total sample and in each village. The random placement of individuals into treatments assures that, in expectation, treatment groups do not differ in terms of background characteristics. Table 1 confirms that this is true also in practice. While respondents in the Information Treatment differ slightly from those in the Control Group in terms of gender and age, all other co-variates are well balanced across treatments.

9. This approach tends to promote similarity of treatment groups in terms of pre-determined characteristics (Gerber and Green 2012).

Table 1: Balance table

Variable	Sample mean	IT vs. ST	IT vs. CG	Variable	Sample mean	IT vs. ST	IT vs. CG
Woman	0.518	0.077	0.082*	Internet	0.698	-0.035	0.004
	(0.500)	(0.049)	(0.049)		(0.460)	(0.045)	(0.046)
Age	45.811	0.247	-3.563**	TV	0.717	0.024	0.002
	(15.148)	(1.494)	(1.533)		(0.451)	(0.045)	(0.044)
Survey Taken at Home	0.638	0.087	0.032	Radio	0.633	-0.055	-0.057
	(0.481)	(0.047)	(0.046)		(0.482)	(0.048)	(0.048)
Lives in Town	0.783	-0.033	-0.031	Newspaper or Magazine	0.321	-0.070	-0.043
	(0.413)	(0.041)	(0.041)		(0.467)	(0.046)	(0.046)
Greenlandic	0.833	0.008	-0.040	Survey Language: GRL	0.707	0.042	-0.019
	(0.373)	(0.038)	(0.035)		(0.455)	(0.045)	(0.044)
Family in Denmark	0.820	-0.041	-0.009	Public Sector	0.384	-0.078	-0.054
	(0.384)	(0.050)	(0.052)		(0.487)	(0.054)	(0.053)
Lived in Denmark	0.415	0.032	0.028	Financial Difficulties	0.476	0.076	0.071
	(0.493)	(0.049)	(0.049)		(0.500)	(0.052)	(0.053)
Perceived Income Status	5.124	-0.198	0.000	HH Earnings < 200 K	0.413	0.035	-0.012
	(1.844)	(0.184)	(0.186)		(0.493)	(0.050)	(0.051)
Pol. Pref.: Left-Right	4.786	0.245	0.286	HH Earnings 200-500 K	0.370	0.007	-0.002
	(2.104)	(0.211)	(0.219)		(0.483)	(0.049)	(0.049)
Party pro-independence	0.606	0.049	-0.037	HH Earnings > 500 K	0.218	-0.042	0.013
	(0.489)	(0.048)	(0.048)		(0.413)	(0.042)	(0.041)
Party anti-independence	0.196	-0.043	-0.003	Primary School	0.451	0.039	-0.022
	(0.398)	(0.040)	(0.038)		(0.498)	(0.050)	(0.050)
Trust Den. Government	2.700	0.021	0.174	High School/Professional	0.380	-0.052	-0.023
	(1.035)	(0.108)	(0.117)		(0.486)	(0.049)	(0.049)
Trust Gre. Government	2.828	0.052	-0.036	University Degree	0.169	0.012	0.045
	(1.111)	(0.112)	(0.120)		(0.375)	(0.039)	(0.037)

Table 1 shows the sample means of relevant covariates, as well as balance tests comparing the Information Treatment with the other treatment groups. The tests are conducted by means of bivariate regressions. Woman is an indicator variable coded as 1 if the respondent is female and 0 otherwise; Age is the age of the respondent; Survey Taken at Home is an indicator variable coded as 1 if the respondent took the survey at home and 0 if it was taken at a field session; Lives in Town is a dummy for residing in a town (1) or settlement (0); Greenlandic is a dummy for having indicated Greenlandic as national identity; Family in Denmark indicates if the respondent stated having at least one close relative in Denmark; Lived in Denmark is a dummy indicating if the respondent ever lived in Denmark; Perceived Income Status denotes the income decile in which the respondent placed her household; Party pro-independence is a dummy on whether the respondent voted for a party in favor of near-future independence; Party anti-independence is a dummy on whether the respondent voted for a party opposing near-future independence (herein I include Democrats and Cooperation Party); Trust Den. Government indicates trust in the Danish government from “Not at all” (1) to greatly (5); Trust Gre. Government has the same wording but regarding trust in the Greenlandic government; Internet, TV, Radio and Newspaper or Magazine are dummies indicating whether the respondent consumes respective media; Survey Language: GRL is a dummy on whether or not the Survey was taken in Greenlandic; Public Sector is a dummy indicating whether the respondent works in Education, Health services or simply stated “Public Sector” as her employment; Financial Difficulties is a dummy indicating if the respondent would run into financial difficulties in less than a month if salary and/or transfers were discontinued; HH earnings stand for household earnings, and the ranges are in Danish Kroner; Primary School, High School/Professional and University Degree are dummies indicating the highest level of education obtained.
* (p<0.10), ** (p<0.05), *** (p<0.01)

4.3.1 Information Treatment

Respondents in the Information Treatment (IT) group read the information prime shown in S3 before answering questions on voting intentions in the hypothetical independence referendum. The goal of the Information Treatment was to shift the economic expectations of treated respondents by means of truthful and credible information. To this end, the prime was an extract from a scientific report evaluating the economic challenges associated with independence in light of the current fiscal reliance on Denmark (Rosing, Mosbech, and Mortensen 2014). The exact wording is shown in S3.

S3. Information prime

The cost of an independent Greenlandic economy has been estimated to be at least 5 billion DKK. An independent Greenlandic economy would require...

- 3.6 billion DKK a year to compensate for the block grant that Greenland currently receives each year
- 800 million DKK annually to fund public services not yet transferred to Greenlandic responsibility
- 190 million DKK annually to phase out subsidies from the EU
- 456 million DKK a year to carry out new tasks if Greenland decides to withdraw entirely from the Kingdom of Denmark
- 800 million DKK in increased annual costs by 2040

5 billion DKK split between all Greenlanders is around 90 000 DKK per citizen.

(Source: "To the benefit of Greenland, 2014")

While the public debate on independence in Greenland has been focused on how to compensate for the 3.6 billion DKK that the Danish state transfers each year, the report presented a more comprehensive assessment of the economic costs of independence. By including a number of additional costs that complete economic independence would entail, the assessment can be firmly positioned in the negative tail of impact assessments, and the information should thus be regarded as pessimistic economic information. The fact that the report was four years old at the time of surveying did not substantially impact the accuracy of the content, since the economic support that Greenland received from Denmark was largely unchanged during this period. To the extent that respondents still perceived the information to be outdated and therefore of lesser relevance, this would induce a downward bias in the estimated treatment effect.

4.3.2 Salience Treatment

In order to separate information updating from a potential salience effect induced by the information prime, a subset of the participants was assigned to the Salience Treatment (ST). The Salience Treatment entailed a positioning of questions on expected economic consequences of independence prior to the voting section (S1). Besides S2 – the survey item on expected consequences for Greenland's economy in case of independence – the Salience Treatment also included questions on the expected economic consequences for the village of residence, and for the personal income.[10] Answering questions on economic expectations should enhance the salience of the economic aspect of independence, without providing new information. This allows me to separate the treatment effect due to information updating from a mere salience effect.

4.3.3 Control Group

The remaining respondents were not subject to any prime before stating their voting intentions. I label this the Control Group (CG). Naturally, the Control Group serves as the reference category in most specifications.

4.4 Experimental concerns

A concern with using voter choice in a hypothetical independence referendum as the dependent variable of interest is the ambiguity of what political independence entails. Respondents could potentially attribute different meanings to the word independence. For instance, some respondents might assume that independence means that all economic support from Denmark would immediately cease, while others count on the economic support to continue. If the Information Treatment affects respondents' interpretation of independence, it could induce changes in voter behavior for semantic reasons. In order to ensure that all respondents interpret independence in the same way, the survey section on voting preferences was therefore introduced by I1, which served to align respondents' understanding of "independence".

I1. Section Introduction

In following section, we refer to independence as complete political independence from the Kingdom of Denmark. It is assumed that this means that fiscal transfers (including the block grant) from Denmark to Greenland would stop.

4.5 Experimental predictions

I now use the model presented in Section 2.1 to derive predictions from the survey experiment.

10. The exact wordings read: "If Greenland becomes independent within the next 10 years, this will impact... my personal income / my town or settlement's economy (very negatively; somewhat negatively; not at all; somewhat; positively; very positively)". These questions are not further analyzed in this chapter.

$$(1-\alpha)I - \alpha_i(u(c_r) - u(p_i {c_l}^L + (1-p_i){c_l}^H)) > 0 \tag{4}$$

The relevant variables are α_i, the relative weight a respondent ascribes to the economic aspect of independence, and p_i, the perceived probability of a low consumption state vis-à-vis a high consumption state following independence. Since the information prime exposes voters to pessimistic economic information, the expectation is that respondents in the Information Treatment on average will perceive a higher probability of the low consumption scenario (${c_l}^L$) compared with the respondents in the Control Group; formally $\overline{p_i}^{IT} > \overline{p_i}^{CG}$. All else equal, a shift toward more negative economic expectations should increase opposition to independence in the Information Treatment group.

Moreover, both the Information Treatment and the Salience Treatment are expected to increase the salience (α) of the economic aspect of independence, such that $\overline{\alpha_i}^{IT} = \overline{\alpha_i}^{ST}$ and $\overline{\alpha_i}^{IT} > \overline{\alpha_i}^{CG}$. Since respondents' economic expectations of independence are postulated to be negative for the median voter, a higher alpha (α) should increase opposition to independence on the margin. Taken together, the model implies that the share of voters opposing independence should be:

$$\mathbb{1}(IT = No) > \mathbb{1}(ST = No) > \mathbb{1}(CG = No) \tag{5}$$

Both information updating and a salience effect could in principle explain a larger share of voters opposing independence in the Information Treatment relative to the Control Group. However, if the share of voters is larger in the Information Treatment relative to the Salience Treatment, this can only be explained by information updating.

Finally, I consider how the treatment effect could differ for different types of voters. More concretely, I use the model to derive predictions for how respondents with a strong Greenlandic identity should react to the information prime relative to respondents with a mixed identity. To this end, I define two types of voters, strong identity respondents who give relatively more weight to the ideological aspect of independence, and mixed identity respondents who are relatively more concerned with the economic aspect of independence. Hence, the initial conditions are such that $\overline{\alpha}^{SI} < \overline{\alpha}^{MI}$, where SI refers to strong identity respondents, and MI refers to mixed identity respondents. If respondents with stronger national identity are more concerned with the ideological aspect of independence, and hence less focused on the economic aspect, they should be less susceptible to economic information and therefore react less to the prime. The heterogeneous treatment effect could further be amplified if identity and information updating interact, in that more ideological respondents are less willing to update their economic expectations, p_i.

4.6 Econometric specification

In order to test the predictions presented in the preceding section, I set up a number of empirical models to identify the intention-to-treat effect and the complier average treatment effect of the Information Treatment on opposing independence. Due to the random assignment of treatments, the identification of the intention-to-treat effect is straightforward. The reduced form equation (6) is estimated using Ordinary Least Squares (OLS):

$$(Vote = No)_i = \alpha + \beta_1 Z_i + X_i \gamma + \epsilon_1 i, \tag{6}$$

where Z is an indicator variable for assignment to the Information Treatment and X is a vector of control variables. In order to demonstrate that the results are invariant to potentially confounding factors, all models are presented in three steps: bivariate regressions, multivariate regressions with controls for Gender and Age (the imbalanced covariates), and multivariate regressions with an extensive set of controls (labeled "Additional Controls" in the tables). Additional Controls includes controls for (1) whether the participant filled in the survey at home or at a field session, (2) national identity fixed effects, (3) a dummy indicating whether the survey language was Greenlandic, (4) fixed effects on annual household income before taxes, (5) fixed effects on highest level of education achieved, (6) fixed effects on village of residence, and (7) fixed effects on party voted for in the last General Elections.

In the survey section that followed the Information Treatment, respondents were asked whether they read the information contained in the prime. I label respondents who indicated that they read the information "compliers". Under the assumption that respondents reported truthfully whether they complied with the information prime, I can estimate the complier average causal effect (CACE) by means of an instrumental variable approach. The first stage is shown in equation (7), where Treatment Complying is a dummy for being assigned to, and complying with, the Information Treatment.

$$(TreatmentComplying)_i = \alpha + \beta_1(Z_i) + X_i\gamma + \xi_i \tag{7}$$

The second stage regression estimates the average treatment effect among compliers. The specification is shown in equation (8).

$$(Vote = No)_i = \alpha + \beta_1(\widehat{TreatmentComplying_i}) + X_i\gamma + \mu_i \tag{8}$$

Having established a causal relationship between the Information Treatment and voting behavior, I turn to an investigation of potential mechanisms. First, I investigate the Salience Effect by using the Salience Treatment as the reference group. If a shift in salience toward the economic aspect of independence explains the Information Treatment effect, then the estimated effect should be substantially smaller in this specification compared to when the Control Group serves as the reference group. Second, I turn to the Expectations Effect. In order to investigate whether a shift toward more negative economic expectations may account for the impact of the prime on voting behavior, I estimate the effect of the Information Treatment on economic expectations using both standard linear regressions and Ordered Logit models[11]. Third, I investigate the Rallying Effect by comparing the likelihood of voting in the Information Treatment relative to the Control Group, separately for respondents with pessimistic and optimistic economic expectations.

Finally, I study how identity associates with economic voting, and how this in turn shapes the effect of the information prime. To investigate the moderating role of identity, I first define economic voting as voter behavior that (to a large extent) is influenced by economic concerns and therefore sensitive to new economic information (Holbrook and Garand 1996). In order to proxy for identity, I exploit information on respondents' language proficiency as signals of the strength of national identity. In Greenland, both Greenlandic and

11. I first consider Generalized Ordered Logit models, but since the parallel lines assumption is not rejected, allowing for a non-linear impact does not change the estimates (Williams 2016).

Danish are mandatory languages in school and widely used in the public sphere.[12] But for many the prevalence of Danish represents a lingering dependence on Denmark that should be discontinued (Gad 2009). According to this view, monolingual Danish speaking Greenlanders and bilingual Greenlanders possess mixed identities, while monolingual Greenlanders represent the more traditional Greenlandic identity. Consequently, proficiency of Danish is a marker of identity (Gad 2009), and not speaking Danish serves as a good proxy for having a strong Greenlandic identity. As a proof of concept, strong identity is shown to correlate negatively with "sense of belonging to Denmark" (correlation coefficient= -0.342).

5 Results

5.1 Independence support by treatment group

For a first outlook of the treatment effect, I present data on voting intentions in the hypothetical referendum on independence separately for each treatment group. Table 2 shows that the Control Group displays the strongest support for independence. In fact, for this group of respondents, the share of voters favoring immediate independence is larger than the share of respondents opposing it. Opposition to independence is somewhat stronger among respondents in the Salience Treatment, who were subject to questions on expected economic consequences of independence prior to indicating their voting decision. In the Information Treatment group, the majority of voting respondents are opposed to independence. These simple descriptive statistics provide an indication that the Information Treatment influenced voting behavior. Next, I formally test the treatment effect by means of regression analyses.

12. For the following analysis, I exclude 10 respondents with an identity other than "Danish" or "Greenlandic". In the remaining sample, about two-thirds of the respondents speak Danish.

Table 2: Independence preferences by treatment group

If there was a referendum on independence TODAY, asking if you wanted Greenland to leave the Kingdom of Denmark, how would you vote?			
	Control Group	**Salience Treatment**	**Information Treatment**
Yes	39.90 %	37.07 %	35.51 %
No	30.05 %	34.63 %	42.99 %
I don't know	19.70 %	20.00 %	15.42 %
I would not vote	10.34 %	8.29 %	6.07 %

Table 2 displays the percentage of voters for each outcome in a hypothetical referendum on full political independence, separately for each treatment status.

5.2 Economic information and voting intentions

The random assignment of treatments allows for straightforward identification of the Information Treatment effect on voting behavior. Table 3 displays the results from regressions using a dummy for voting no to independence as the dependent variable, and a dummy for assignment to the Information Treatment as the independent variable of interest. Columns 1-3 show OLS regression estimates (see equation 6) of the intention-to-treat effect. In columns 4-6, I present the average treatment effect of complying respondents, estimated using 2SLS regressions where Treatment Complying is instrumented by assignment to the Information Treatment (see equation 7 and 8). Panel A shows the regression estimates when both the Control Group and the Salience Treatment are used as reference groups; Panel B displays the effect of the Information Treatment relative to the Control Group; Panel C presents the regression estimates when the Salience Treatment is the reference group.

Table 3: Effect of the Information Treatment on Voting Behavior

Dep. Var.: Voted No	OLS	OLS	OLS	2SLS	2SLS	2SLS
Panel A: Full Sample	(1)	(2)	(3)	(4)	(5)	(6)
Information Treatment	0.106***	0.101**	0.106***			
	(0.0411)	(0.0423)	(0.0398)			
Treatment Complying				0.144***	0.137**	0.145***
				(0.0549)	(0.0567)	(0.0521)
Observations	622	588	536	622	588	536
R-squared	0.0111	0.0115	0.280	–	–	–
Dep. Var. Mean	0.360	0.366	0.377	0.360	0.366	0.377
First Stage F-statistic	–	–	–	546.3	559.7	601.8
Gender & Age	No	Yes	Yes	No	Yes	Yes
Additional Controls	No	No	Yes	No	No	Yes
Panel B: CG as reference	(1)	(2)	(3)	(4)	(5)	(6)
Information Treatment	0.129***	0.112**	0.0968**			
	(0.0468)	(0.0486)	(0.0481)			
Treatment Complying				0.175***	0.152**	0.136**
				(0.0626)	(0.0652)	(0.0634)
Observations	417	395	359	417	395	359
R-squared	0.0180	0.0212	0.304	–	–	–
Dep. Var. Mean	0.367	0.375	0.393	0.367	0.375	0.393
First Stage F-statistic	–	–	–	600.9	540.9	409.5
Gender & Age	No	Yes	Yes	No	Yes	Yes
Additional Controls	No	No	Yes	No	No	Yes
Panel C: ST as reference	(1)	(2)	(3)	(4)	(5)	(6)
Information Treatment	0.0836*	0.0838*	0.112**			
	(0.0475)	(0.0490)	(0.0462)			
Treatment Complying				0.113*	0.114*	0.151**
				(0.0638)	(0.0659)	(0.0587)
Observations	419	396	365	419	396	365
R-squared	0.007	0.010	0.313	–	–	–
Dep. Var. Mean	0.389	0.391	0.397	0.389	0.391	0.397
First Stage F-statistic	–	–	–	600.9	549.2	494.7
Gender & Age	No	Yes	Yes	No	Yes	Yes
Additional Controls	No	No	Yes	No	No	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3 displays both Ordinary Least Squares (OLS) and Two-Stage Least Squares (2SLS) regression estimates of the effect of information on the probability of voting no to independence. Columns 1-3 display OLS regressions with the full sample in Panel A (using both the Control Group and the Salience Treatment as reference), the Control Group as reference (Panel B) and the Salience Treatment as reference in Panel C. Columns 4-6 show 2SLS regressions in which complying respondents in the Information Treatment are instrumented by having been assigned the Information Treatment. Gender & Age indicates whether controls for age and gender were included. Additional Controls comprises (1) Survey taken at home or at field session, (2) National Identity FEs, (3) Greenlandic language survey dummy, (4) Income FEs, (5) Education FEs, (6) Village FEs and (7) Party FEs.

As can be seen in column 1 of Panel A, the information prime significantly increases opposition to independence by 10.6 percentage points; 32.7% higher than in the Control Group and Salience Treatment, where 32.4% of the respondents opposed independence. The effect remains largely unchanged when controlling for Gender and Age, and the more extensive set of controls labeled “Additional Controls” (see Section 4.6). The treatment effect for complying respondents – shown in columns 4-6 – is substantially larger. With no controls, the complier average causal effect is an increase in opposition of 14.4 percentage points (or 44% higher than in the baseline).

In Panel B, I show that the pattern is the same when using only the Control Group as reference group. Respondents in the Information Treatment group are 12.9 percentage points (43%) more likely to oppose independence, relative to respondents in the Control Group. The estimated effects are, intuitively, larger in the 2SLS regressions. Complying respondents are 17.5 percentage points (58%) more likely to oppose independence when subject to the Information Treatment. In conclusion, the prime induces more opposition toward independence when evaluated against the pooled sample and the Control Group.

5.3 Mechanisms

I now turn to the mechanisms that account for the baseline results presented in Panels A and B of Table 3. The effect of the information prime could either be accounted for by (1) information updating or (2) a salience effect, or a combination of the two. I start by investigating whether a salience shift toward the economic challenges associated with independence can account for the impact of the Information Treatment. Thereafter, I test whether the treatment effect can be attributed to altered economic expectations and whether increased turnout among pessimistic voters played a role.

5.3.1 The Salience Effect

First, I study whether a shift in salience explains the impact of the prime. The underlying idea is that, given that respondents already are aware of the economic challenges that independence would entail, the information prime may influence voter behavior merely by making salient the economic aspect of independence (Bélanger and Meguid 2008). In order to test whether salience played a role, I present regressions using the Salience Treatment as reference group in Panel C of Table 3. The estimated effects are qualitatively the same, albeit somewhat smaller in magnitude, as when the Control Group is used as the reference group, but the effects are now significant only at the 10% significance level. The relatively smaller treatment effect indicates that salience produced a minor increase in the opposition to independence. The opposition is, however, not significantly more pronounced in the Salience Treatment group relative to the Control Group[13], and I thus rule out a salience effect as a prominent mechanism.

5.3.2 The Expectations Effect

Next, I turn to the role of information updating in influencing voter behavior. Since the prime includes costs not generally discussed in association with independence, the Information Treatment should induce more negative economic forecasts, given that voters are receptive to the message. This prediction aligns well with the observed pattern. In Figure 3, I plot the distribution of respondents' economic expectations separately for the Control Group and the Information Treatment. The upper histogram shows the economic expectations of all respondents, whereas the bottom histogram includes complying respondents only.

Figure 3: The effect of the Information Treatment on economic expectations

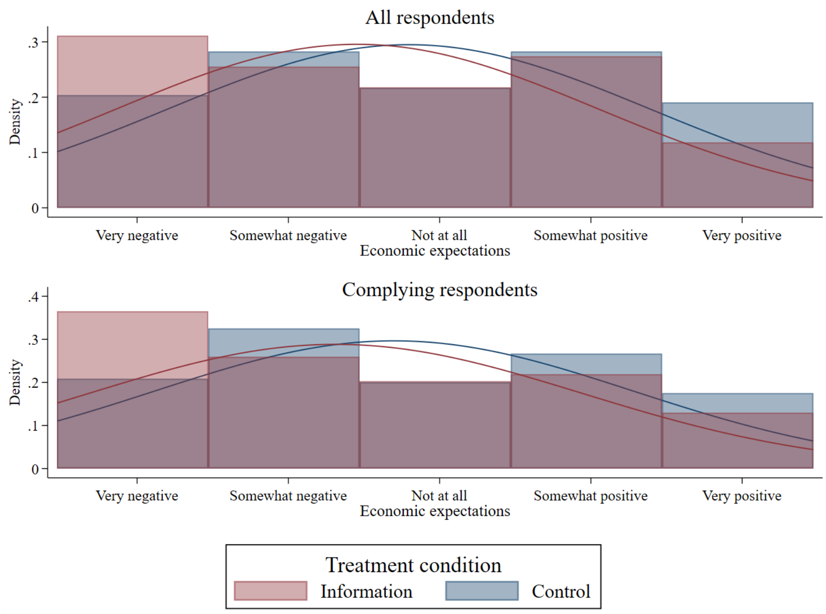

Figure 3: Distribution of economic expectations by treatment group. The upper plot displays histograms with corresponding kernel densities for all respondents in the Information Treatment and the Control Group, and the lower plot shows histograms with corresponding kernel densities for complying respondents. The survey item from which economic expectations are derived read: "If Greenland becomes independent within the next 10 years, this will impact Greenland's economy... (Very negatively; Somewhat negatively; Not at all; Somewhat positively; Very positively)".

13. Regression coefficient= 0.046; p-value= 0.323, in a bivariate regression with robust standard errors.

The Information Treatment increased the prevalence of respondents believing that independence would affect the Greenlandic economy very negatively by approximately 50%. The effect is entirely driven by complying respondents. In this subgroup, the share of respondents indicating that the economy would be very negatively affected by independence increases by 69% when exposed to the Information Treatment. In Table A4 in the Appendix F, I formally test the impact of the Information Treatment on economic expectations, both by means of Ordered Logit and OLS regressions. The regression analyses confirm that the prime significantly shifted the distribution of economic forecasts in the negative direction. In columns 7 and 8 of Table A4, I show that the impact is conditional upon compliance with the treatment.

5.3.3 The Rallying Effect

Another channel through which the Information Treatment could impact voting behavior is the potential of information to inspire voting participation of certain types of voters. Since the prime presents negative economic information, it should make already pessimistic voters more certain regarding their beliefs and thereby encourage them to vote. Conversely, the Information Treatment should make voters with optimistic expectations more insecure, and thereby dissuade them from voting. In Figure 4, I plot the likelihood of voting by treatment group, separately for respondents who make negative (left) and positive (right) forecasts of the economic impact of independence. Voting is a dummy defined as 1 if the respondent indicated either “Yes” or “No” to S2, and 0 if the respondent indicated “I don’t know” or “I would not vote”.

Figure 4: The effect of the Information Treatment on the probability of voting

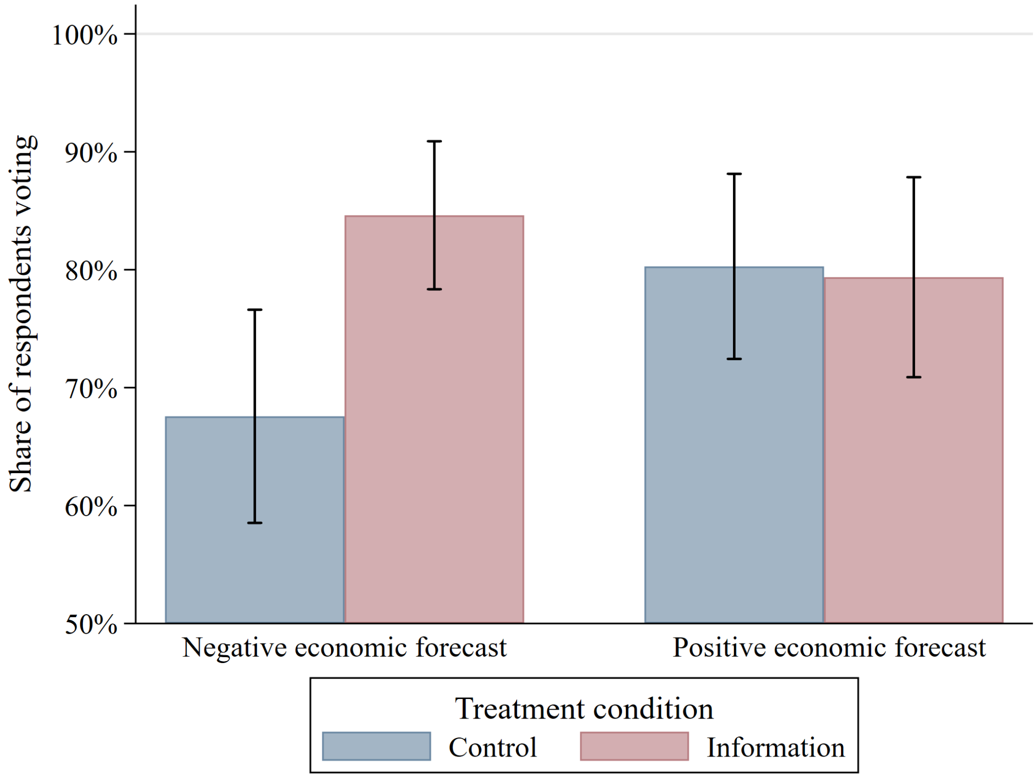

Figure 4: Bar graphs showing the likelihood of voting in the Information Treatment and the Control Group split by expected economic impact of independence. The vertical lines represent corresponding 90% confidence intervals based on robust standard errors.

As shown in Figure 4, respondents who make negative economic assessments are more likely to vote when exposed to the Information Treatment. In other words, the Information Treatment appears to rally negative respondents. Meanwhile, there is no rallying effect, but neither a dissuading effect, for respondents who exhibit positive expectations about the economic impact of independence: the participation rate among these voters is completely unaffected by the prime. Table A5 in the Appendix G shows that the Information Treatment increased voter participation by 17.1 percentage points (statistically significant at the 5% level) among respondents making negative prospective economic evaluations, whereas respondents with positive economic expectations were unaffected in terms of voting participation. As a consequence of the Rallying Effect, the Information Treatment changed the voter composition in favor of relatively more voters with negative economic expectations.

5.3.4 Documenting the underlying mechanisms

To show that the proposed mechanisms account for the treatment effect, I stepwise include economic expectations and voting participation as controls in the reduced form regression. In Table A6 in the Appendix H, the sample is restricted to respondents with data on economic expectations (S3). For this sub-sample, the Information Treatment increases the probability of voting no by 15.4 percentage points (significant at the 1% level). The estimated effect drops to 10.3 percentage points (significant at the 5% level) when I control for the Expectations Effect by including a set of dummy variables indicating economic expectations. When I account for the Rallying Effect by adding a dummy for voting (thereby restricting the analysis to respondents moving from "Yes" to "No"), the treatment effect drops further to 5.2 percentage points (insignificant). This exercise shows that the Information Treatment impacted voting behavior by shifting respondents' economic evaluations and by encouraging voters with pessimistic economic expectations to participate.

5.4 Robustness checks

In this section, I present a number of alternative specifications as well as placebo tests to corroborate the baseline findings. First, I investigate whether the results are robust to an alternative test of statistical significance, namely Randomization Inference. In relation to inference based on OLS regressions – which relies on assumptions of asymptotic properties – Randomization Inference provides exact p-values (Imbens and Wooldridge 2009). The approach entails randomly assigning respondents fictional treatments and estimating the treatment effect using the placebo treatment distribution. This procedure is then repeated 100,000 times. Figure 5 plots the kernel densities of Information Treatment-betas obtained from Randomization Inference, respectively when using the full sample (column 1), the Control Group (column 2) and the Salience Treatment (column 3), as reference groups. In the first row, all respondents are included, and in the second row only complying respondents are considered. The vertical lines indicate the estimated effects of the Information Treatment from the actual treatment assignment. The corresponding p-values are estimated as the proportion of times that (absolute value of) the fictional treatment effect was larger than the (absolute value of the) actual treatment effect.

Figure 5: The effect of IT on opposition to independence using Randomization Inference

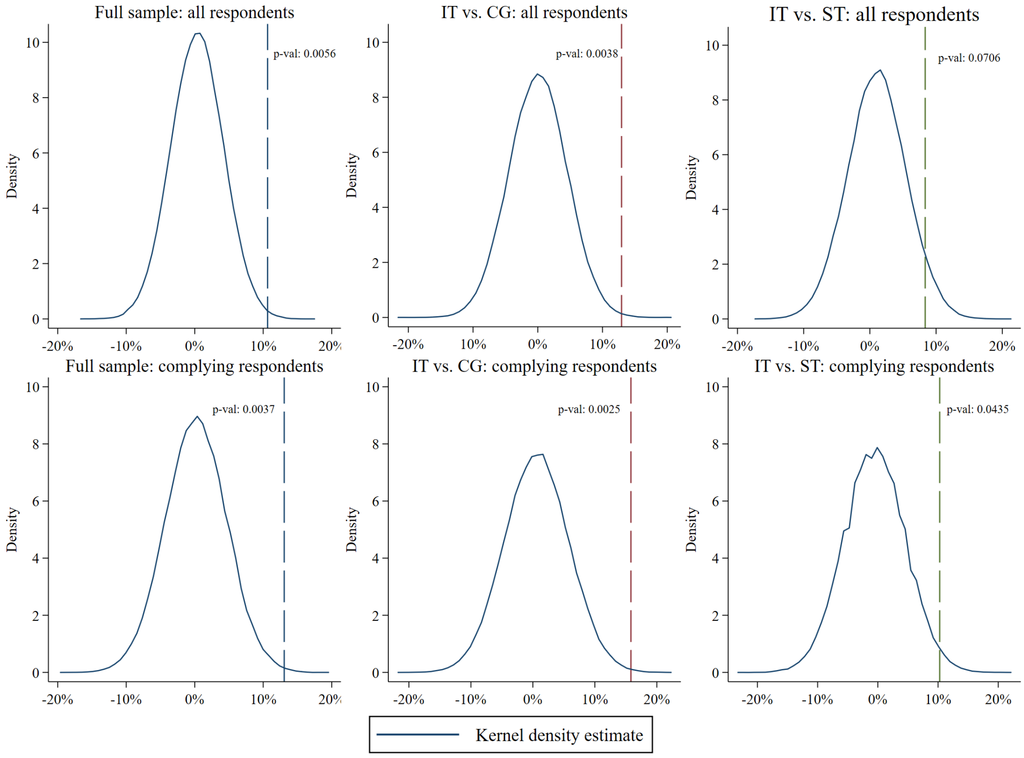

Figure 5: Kernel density plots from Randomization Inference estimations. Each Kernel displays a distribution of Information Treatment-betas obtained from 100,000 permutations of fictional treatment status. The vertical lines show the estimated effects of the Information Treatment in the actual treatment assignments, and the corresponding p-values indicate the probability that such extreme values would be estimated by chance. The first row shows simulated estimations when all respondents are considered, and the second row when only complying respondents are included. The reference groups are the full sample (column 1), the Control Group (column 2) and the Salience Treatment (column 3).

The impact of the Information Treatment on the probability of voting no appears even more robust when using Randomization Inference instead of OLS regressions. For the full sample, as well as when using the control group as the reference group, the treatment effects are estimated at significance levels below 1%. As the Salience Treatment slightly increases opposition to independence, the estimated effects of the Information Treatment are significant only at the 10% and 5% level when using the Salience Treatment as reference group.

In Table A7 in the Appendix I, I replicate the baseline regressions using several alternative specifications: in columns 1 and 2, I only include respondents who indicated a

Greenlandic nationality in the regressions, and show that the results remain significant for this sub-sample; column 3 reports treatment effects estimated at the 5% significance level when using standard errors clustered at the village level[14]; in column 4, I weigh[15] observations in order to better approximate the intention-to-treat effect for the general Greenlandic population, and show that, due to higher treatment responsiveness among respondents in underrepresented areas, the treatment effects are larger in this exercise; in column 5, I show that the results are robust to using logistic regression specifications; and in column 6, I show that the estimates remain statistically significant also in PDS LASSO models[16], which account for potential issues of multicollinearity in the multivariate models (Chernozhukov, Hansen, and Spindler 2015).

Next, I turn to placebo tests of the treatment effect. For information to have an impact, voters ought both to pay attention to the information and to perceive it as credible (Chiang and Knight 2011). By restricting the sample to respondents who did not read (comply with) the prime, and who did not trust the prime, I can conduct placebo tests on respondents that should not have been affected. In the full sample, 21.2% of respondents did not read the information prime (CG: 19.8%; IT: 22.6%; ST: 21.2%), and 37.5% of respondents did not trust it (CG: 36.6%; IT: 39.5%; ST: 37.11%). In Table A9, I show that the Information Treatment effect in these subgroups was close to zero and statistically insignificant.

Lastly, in the Appendix D, I investigate whether a double treatment in a number of surveys may have impacted the results. 26 (out of 622) respondents received both the Information Treatment and the Salience Treatment due to a misprint in these surveys. The results confirm that double treatment did not alter voter behavior relative to single treatment.

14. The standard errors are bootstrapped to account for the issue of few clusters (Cameron, Gelbach, and Miller 2008).

15. The population share of each stratum is divided by respective stratum's sample share. As a consequence, respondents from strata that are overrepresented in the data are given weights lower than 1, whereas respondents from strata that are underrepresented are given weights higher than 1.

16. The Post-Double Selection Least Absolute Shrinkage and Selection Operator (PDS LASSO) models are implemented using Stata's pdslasso package (Ahrens, Hansen, and Schaffer 2019). The LASSO models introduce a shrinkage parameter which penalizes coefficients of the independent variables and includes in the model only regressors with non-zero coefficients post regularization. As an effect of regularization, the LASSO introduces a downward bias in the estimates but potentially also reduces the variance.

6 Identity and economic voting

In this section, I investigate the role of identity in moderating the Information Treatment-effect. I hypothesize that voters with strong national identity attach a larger weight to the identity aspect of independence and, correspondingly, a smaller weight to the economic aspect, relative to voters with more ambiguous national identity. Moreover, I posit that strong identity respondents exhibit less malleable economic expectations. As a consequence, strong identity voters should be less likely to change their voting stance when exposed to the information prime. This exploratory analysis is motivated by the emergent literature on the influence of identity on economic and political behavior (Constant and Zimmermann 2008; Benjamin, Choi, and Fisher 2016; Kaufmann 2019), and the predictions align with the theoretical model (in Section 2) in which voters trade off identity-based and economic concerns when voting on independence.

To investigate the proposed voter heterogeneity, I proxy for strong national identity using information on language proficiency. I code monolingual Greenlandic speakers as respondents with strong identity, and label the identity of bilingual and Danish speaking respondents as mixed identity (see Section 4.6 for a discussion on this operationalization).[17]

I begin the analysis by investigating the correlation between identity and preferences for independence. In Figure 6, I plot the share of voters for and against independence separately for respondents speaking only Greenlandic, bilingual respondents and monolingual Danish speaking respondents. The figure reveals a clear pattern. While monolingual Greenlandic speakers predominantly support independence, the race is almost tied for bilingual respondents, while nearly all of the monolingual Danish speaking respondents oppose independence.

17. In the subsequent analysis, I exclude 10 respondents that indicated a national identity "other" than the Danish Kingdom.

Figure 6: Identity and independence

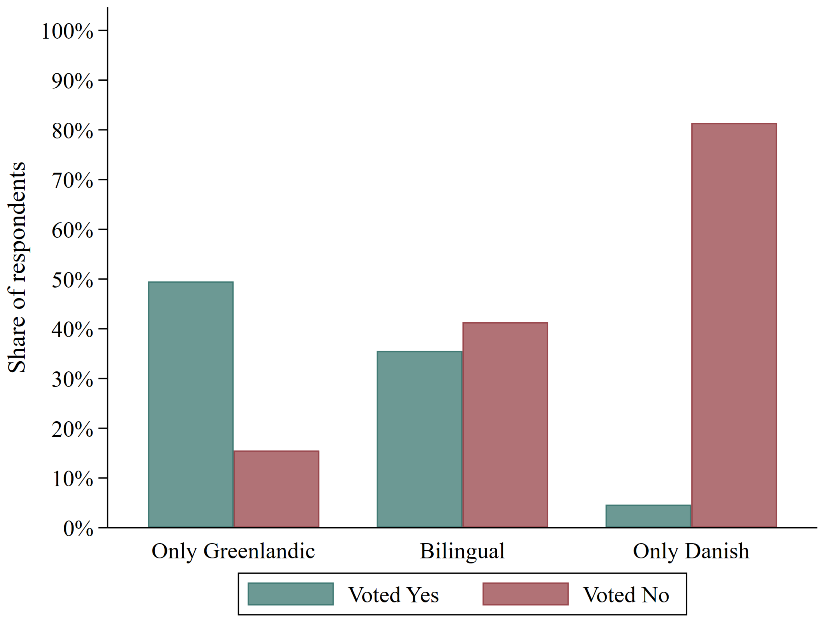

Figure 6: Voting on independence by groups of language speakers. Only respondents that indicated a "Greenlandic" or "Greenlandic and Danish" identity are included. Participants indicating either "I don't know" or "I would not vote" are excluded. Hence, the bars do not add to 100%.

Next, I study how identity moderates the degree of economic voting. I define economic voting as a (strong) correlation between economic expectations and opposition to independence. More economic voting should make voters more susceptible to the information in the prime. In Table 4, I formally test the differences in economic voting, as well as for a heterogeneous treatment effect based on identity. SI refers to Strong identity respondents, whereas MI refers to Mixed identity respondents.

In columns 1 to 3 of Table 4, I explore the relationship between economic expectations and opposing independence separately for mixed and strong identity respondents. All specifications confirm that respondents with strong identity are relatively less likely to base their vote on prospective evaluations of the economic consequences of independence, relative to respondents with mixed identity. Next, in columns 4 and 5, I document how identity correspondingly moderates the effect of the information prime on voting behavior. The estimates show that whereas mixed identity respondents react to the Information Treatment by increasing opposition to independence, the treatment effect is negative (albeit statistically insignificant) for strong identity respondents. In other words, strong identity respondents are if anything less likely to oppose independence after having been exposed to pessimistic economic information. Column 6 confirms that this heterogeneity in voter response is statistically significant.

Table 4: Identity and Economic Voting

Dep. Var.:	Voted No					
	SI (1)	MI (2)	Pooled (3)	SI (4)	MI (5)	Pooled (6)
Economic Expectations	-0.116***	-0.212***	-0.212***			
	(0.0302)	(0.0177)	(0.0177)			
Strong identity			-0.512***			-0.200***
			(0.137)			(0.0617)
Economic Expectations × Strong identity			0.0959***			
			(0.0349)			
Information Treatment				-0.0781	0.191***	0.191***
				(0.0577)	(0.0594)	(0.0595)
Information Treatment × Strong identity						-0.269***
						(0.0828)
Observations	108	254	362	138	275	413
R-squared	0.163	0.323	0.375	0.0127	0.0362	0.139
Dep. Var. Mean	0.139	0.496	0.390	0.138	0.476	0.363

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4 displays Ordinary Least Squares (OLS) regressions on the interaction effect between Strong identity and Economic Expectations (columns 1-3), and the interaction effect between Strong identity and the Information Treatment (columns 4-6). A dummy indicator for opposing independence is the outcome variable. Columns 1 and 4 display OLS regressions with the sample restricted to respondents with a strong identity (SI), whereas columns 2 and 5 show results from regressions including respondents with a mixed identity (MI). In columns 3 and 6 the sample is pooled. In all specifications, the reference group is the Control Group.

An important disclaimer at this point is that when I split the sample, I lose precision and the initial balance of treatments might be threatened. In order to ensure that new imbalances do not explain the heterogeneous treatment effect presented above, in Tables A10 and A11 in the Appendix J, I show that the vast majority of background characteristics remain balanced across treatments even after splitting the sample. To further corroborate the findings, I link the data with register-based information on income, education, and a range of other potentially confounding factors. In Table A12, I show that the results remain significant and the coefficients of similar magnitude when controlling for such socio-demographic characteristics. Lastly, in Figure A2, I plot the distribution of bootstrapped correlation coefficients between (1) economic expectations and opposition to independence and (2) exposure to the Information Treatment and opposing independence, separately for respondents with strong and mixed identity. The findings confirm that mixed identity respondents display a stronger correlation between economic expectations and voting behavior and, are more likely to change their vote stance when exposed to the prime, compared to strong identity respondents.

While a relatively smaller treatment effect for strong identity respondents in principle could be explained solely by differences in economic voting, the insignificant treatment effect mandates further investigation. An obvious candidate explanation is the degree of information updating, given that respondents who weigh higher the identity aspect also might exhibit less malleable economic expectations. In Figure 7, I show that while respondents with a mixed identity report more pessimistic economic expectations when exposed to the prime, strong Greenlandic identity respondents are completely unaffected.

Figure 7: Identity and information updating

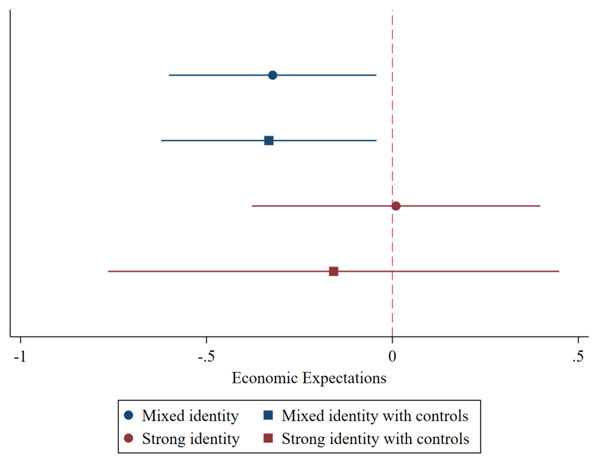

Figure 7: Coefficient plot based on OLS regressions with Economic Expectations as the dependent variable and the Information Treatment as the independent variable, separately for Strong identity and Mixed identity respondents. Controls encompasses controls on (1) Gender (2) Age, (3) Survey taken at home or at field session, (4) National Identity FEs, (5) Greenlandic language survey dummy, (6) Income FEs, (7) Education FEs, (8) Village FEs and (9) Party FEs. 90% confidence intervals based on robust standard errors.

In conclusion, the findings presented in this section indicate a substantial and important heterogeneous treatment effect based on identity. I link this voter heterogeneity to different degrees of economic voting and information updating. However, mixed and strong identity respondents differ in a range of ways that could confound the interpretation of this result. In what follows, I turn to these alternative explanations and progressively rule out that they explain voter heterogeneity based on identity.

6.1 Alternative explanations

Strong Greenlandic identity correlates with a number of factors (see Figure A3 in the Appendix J), e.g. political knowledge, economic status and institutional trust, that potentially could influence the findings presented above. I consider several variables to proxy for each factor. Given that these variables interact with the Information Treatment, the moderating role of identity could be explained by such underlying differences, rather than economic voting. In the proceeding analyses, I investigate whether respondents with strong identity are less susceptible to economic information because of differences in political knowledge, economic status and institutional trust.

6.1.1 Differences in political knowledge

I operationalize political knowledge in three ways: educational attainment, political interest and complying with the prime.[18] Political knowledge could impact both the ability to update expectations when receiving new information and the degree to which such updates influence voter choices (Alt, Lassen, and Marshall 2016). In columns 1-3 of Table A13, I account for one proxy of political knowledge at the time by interacting each of the variables with the Information Treatment. The coefficient of interest is that of the interaction term between Information Treatment and strong identity. As the results show, the estimates remain essentially unchanged and are significant at the 1% level in all three specifications.

6.1.2 Differences in economic motives

Next, I turn to the possibility that voters with a strong Greenlandic identity react less to the Information Treatment because of differences in economic interests vested in independence.

18. Educational attainment is a dummy equal to 1 if the respondent had started or finalized a level of education beyond primary school; Political interest is a continuous variable ranging from 1 ("Not at all interested") to 7 ("very interested"), based on the survey item: "How interested or uninterested are you in politics?"; Complying with the prime is a dummy indicating whether respondents stated that they read the Information Treatment.

I proxy for economic motives using three variables, namely household income, perceived wealth status and working in the public sector.[19] Current economic status could influence voters' susceptibility to economic information by altering the perceived risks and gains from political independence. If the information prime enhances perceived risks relatively more for well-off participants, material motives could explain why mixed identity voters (who are richer on average) are more impacted by the prime. In columns 4-6 of Table A13, I investigate the sensitivity of the results to the inclusion of proxies for economic motives. Throughout, the results remain unchanged. While economic motives indeed correlate positively with opposition to independence, accounting for this does not alter the insight that identity moderates information susceptibility.

6.1.3 Differences in trust

Finally, I consider potentially confounding variation due to differences in institutional trust. I measure institutional trust in three ways: trust in the Greenlandic Government, trust in the Danish Government and a dummy for whether the respondent trusted the information presented in the Information Treatment.[20] Respondents with a stronger Greenlandic identity are more trusting of the Greenlandic government, less trusting of the Danish government, and less likely to trust the information in the prime. In columns 7-9 of Table A13, I show that differences in trust cannot account for the heterogeneous treatment effects. However, the estimated coefficient of the interaction term drops somewhat in magnitude when I control for trust in the Danish government, indicating that at least some of the heterogeneity can be accounted for by lower levels of trust in the Danish Government among strong identity respondents.

19. Household income is employed as a continuous variable, ranging from 1 (earning less than 100,000 DKK per year) to 7 (more than 1,000,000 DKK per year); Perceived wealth status is a continuous measure based on the survey item: "Please imagine a ten-step ladder where on the first step, stand the poorest people in Greenland, and on the highest step, the tenth, stand the richest people in Greenland. On which step of the ten is your household today?" (1 (poorest decile) to 10 (richest decile)); Public sector is a dummy indicating whether respondents stated "health services", "education" or "public sector" as current occupation.

20. Trust in the Greenlandic Government is a continuous measure ranging from 1 (Not at all) to 5 (Greatly); similarly, Trust in the Danish Government also ranges from 1 (Not at all) to 5 (Greatly).

7 The timing of independence

The Information Treatment makes respondents more likely to vote against immediate independence, but how does the economic information impact inter-temporal preferences for independence? In the survey, respondents who indicated that they want independence "at some point" (66.9% of the respondents) were asked to specify their preferred year of independence. In Figure 8, I plot the cumulative distribution functions of 'favored year of independence' separately for all treatments.[21] In line with the results on preferences for immediate independence, respondents in the Information Treatment prefer independence at a later point in time (average response=2043) compared with both the Control Group (average response=2036) and with the Salience Treatment (average response=2037). Table A14 in the Appendix K shows that the delaying effect of the Information Treatment on favored year of independence is estimated at conventional significance levels.

Figure 8: The effect of the IT on inter-temporal independence preferences

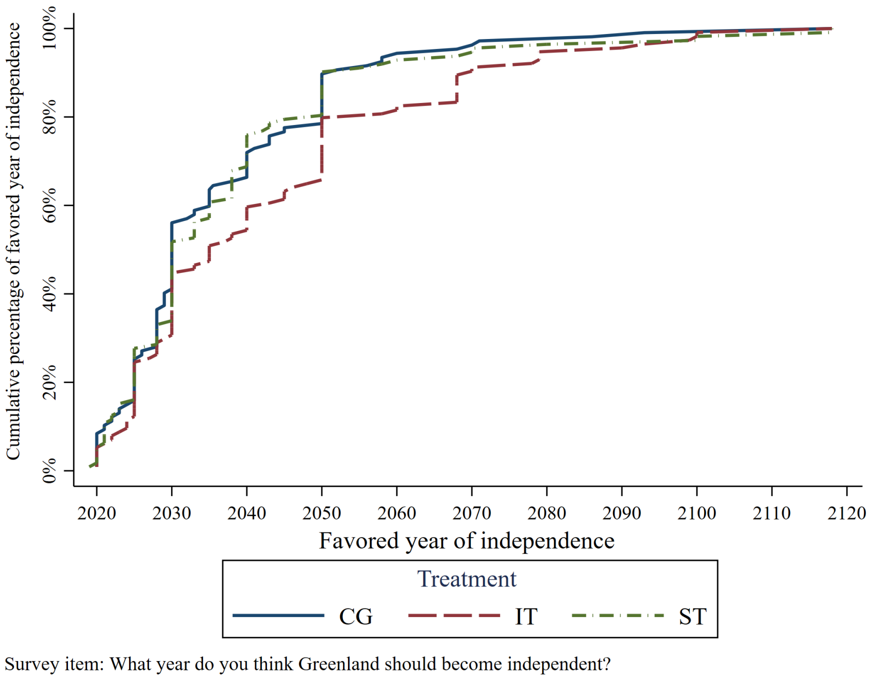

Figure 8: Cumulative distribution function displaying the preferred timing of independence for respondents who favor independence at some point, plotted separately for each treatment.

21. 5 outlier respondents that indicated a preferred year of independence later than 2118 were dropped in this exercise.

8 Conclusions

In this study, I have documented significant voter susceptibility to pessimistic economic information in a hypothetical independence referendum. For theorists of secession, this finding highlights the need to consider how financial disincentives suppress independence aspirations. Materialistic motives, however, did not impact equally the stance of all voters. Respondents with a strong Greenlandic identity were completely unaffected by the information. Having ruled out alternative interpretations, I claim that the moderating role of identity can be explained by a lesser degree of economic voting, manifested both by less information updating and a weaker correlation between economic expectations and voting behavior. This insight resonates with evidence both from the European (Alt, Lassen, and Marshall 2016) and the American (Chiang and Knight 2011) political contexts: information campaigns may only sway voters who are willing and able to update their expectations, and whose expectations actually influence how they cast their ballots.

For respondents that are receptive to the information, the treatment effect presented in the present paper is substantially larger compared to similar studies conducted in Europe (e.g. Muñoz and Tormos 2015 and Goodwin, Hix, and Pickup 2020). The high degree of voter preference malleability uncovered in this chapter calls for a diversification of the contexts studied by political economists. Perhaps not surprising, the western voter appears to be a poor approximation of voters elsewhere.

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Appendix

A Sampling strategy

The Greenlandic Perspectives Survey was a nationwide survey project conducted between July-September 2018. In order to obtain a representative sample of the Greenlandic population, we used the stratified multi-stage cluster sampling procedure detailed below. Compared to sampling by means of pure randomization, stratified sampling generally decreases sampling bias by ensuring that each stratum is represented in the final sample (Deaton 1997). Employing official administrative divisions and data both reduces concerns of convenience sampling and ensures that the whole population has about the same likelihood of being sampled.

First, Greenland was divided into geographic strata. As of 2018, there are 5 municipalities in Greenland: Sermersooq, Avannaata, Kujalleq, Qeqertalik and Qeqqata. These municipalities differ from each other economically, culturally and politically speaking, but are relatively homogeneous within the municipality borders. The exception is Sermersooq, the most populous municipality, which spans both the East and the West coast, and thereby contains areas which differ for instance in terms of language. West Greenlandic – or Kalaallisut – is spoken on the West coast, whereas East Greenlandic – or Tunumiit Oraasiat – is spoken on the East coast. To account for the heterogeneous nature of Sermersooq, we decided to split the municipality into East and West, and used the resulting 6 geographic divisions as the first level of stratification.

Second, the differences between urban towns and rural settlements were accounted for by classifying all localities as either settlements or towns using the categorization operated by Statistics Greenland (the cut-off is approximately at 500 inhabitants). This village division constituted the second level of stratification. In combination, our two levels of stratification yielded 12 strata: settlements and towns of each geographic region. Each region contained at least two towns and a number of settlements.

Third, we sampled one locality from each stratum. In all but two strata, we randomly selected villages to be covered by the survey. We deemed it necessary to make two exceptions from the within-stratum randomization to ensure a comprehensive final sample. Due to the uniqueness of Nuuk – the capital and by far the most populous town – we decided to fix its inclusion in the final sample. We also fixed the inclusion of Upernavik, a town from the northern-most part of Greenland, to account for the vast geographic reach of Avannaata municipality. The other 11 localities were randomly drawn from the subset of villages in respective stratum. The sampled localities are displayed in Table A1.

Finally, we randomly drew a number of residents (age 18 and above) from each sampled village. The sample size of each locality was determined by the relative size of the adult population in the stratum that the locality represented. Settlements were slightly oversampled, in order to ensure statistical power for estimations on this relatively smaller sub-population. The randomization of respondents was carried out by Statistics Greenland, ensuring a truly random selection of participants.

In total, 1400 respondents were drawn from the Greenlandic adult population (which numbered 42,145 in 2018 ()). All residents, including foreigners (who constitute a relatively small proportion of the population), were considered. Out of a gross-sample of 1400, we reached and collected data from 589[22] residents during field visits to the 13 sampled localities in July-September 2018. The respondents were visited 3 times in order to increase the chance of finding sampled respondents. In case the sampled respondent was not available, another (randomly chosen) adult member of the household would be asked to participate. In case there was no other adult member in the household, or the house was empty, the most proximate neighbor would be asked to participate.

In order to reach more of the non-respondents, we invited by mail participants who were absent during the field visits to take the survey online (following Dillman et al. 2009). 33 participants filled out the survey this way. The final count was 622 respondents and the response rate was therefore 44.4%. The sample comprises approximately 1.5% of the total adult population of Greenland in 2018.

Surveys were administered as physical paper copies, and were printed in Greenlandic, Danish and English. The enumerators were fluent in all three languages. If needed, they would assist respondents who were not physically or mentally capable of completing

22. This number excludes 22 respondents who did not respond to the independence section.

Table A1: Sampled villages

Village	Geographic stratum	Demographic stratum	Sample size	Percent
Qaqortoq	Kujalleq	Town	48	7.72%
Narsarmiit	Kujalleq	Settlement	20	3.22%
Nuuk	West Sermersooq	Town	167	26.85%
Qeqertarsuatsiaat	West Sermersooq	Settlement	16	2.57%
Tasiilaq	East Sermersooq	Town	26	4.18%
Tiilerilaaq	East Sermersooq	Settlement	21	3.38%
Sisimiut	Qeqqata	Town	85	13.67%
Kangaamiut	Qeqqata	Settlement	19	3.05%
Qeqertarsuaq	Qeqertalik	Town	63	10.13%
Iginniarfik	Qeqertalik	Settlement	24	3.86%
Ilulissat	Avannaata	Town	83	13.34%
Upernavik	Avannaata	Town	15	2.41%
Kangersuatsiaq	Avannaata	Settlement	35	5.63%

Table A1 displays the sampled villages, the strata each was drawn from, as well as the sample size and corresponding sample share.

the survey on their own. The survey took between half an hour to one hour to complete. Participation was incentivized by voluntary enrollment into a lottery in which the prize was 10 000 DKK ($ 1500), and by small monetary rewards in economic games (see Agneman and Chevrot-Bianco (2020)).

B Representativeness of the sample

This section compares the sampled respondents in the Greenlandic Perspectives Survey to the total Greenlandic populations by two relevant characteristics: age (Figure A1) and voting in the general elections 2018 (Table A2). Younger respondents were slightly underrepresented, and respondents from the major parties slightly overrepresented. In general, however, both Figure A1 and Table A2 show that the the randomly selected survey respondents well approximates the adult population of Greenland.

Figure A1: Age distribution

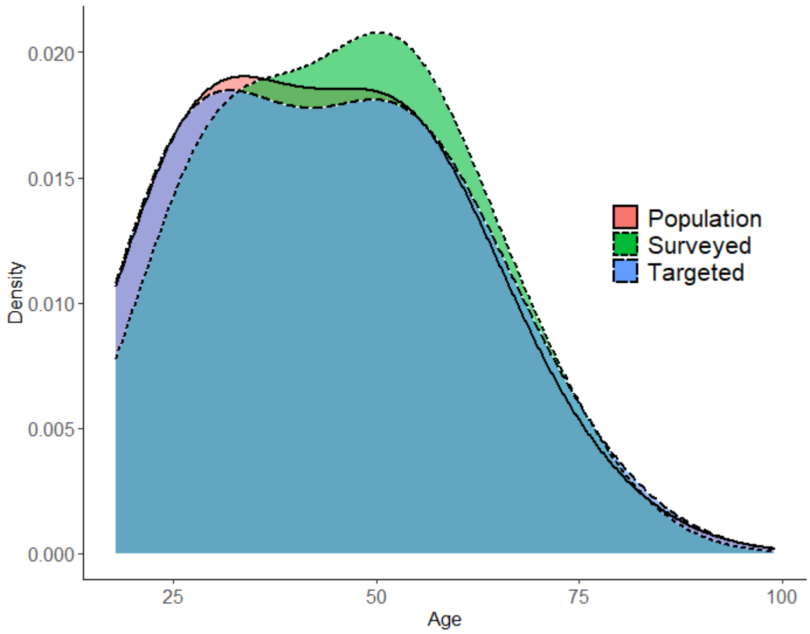

Figure 13: Density plot of the age in (1) the Greenlandic population (2) the targeted survey participants (3) the final sample of respondents participating in the survey.

Table A2: Party shares in the Greenland General Elections 2018 and the survey data

Party	**Election**	**Survey data**
Forward / Siumut (S)	27.2%	28.1%
Community of the People / Inuit Ataqatigiit (IA)	25.5%	27.5%
Democrats / Demokraatit (D)	19.5%	20.5%
Point of Orientation Party / Partii Naleraq (PN)	11.5%	11.0%
Solidarity / Atassut (A)	5.9%	6.1 %
Cooperation Party / Suleqatigiissitsisut (SA)	4.1%	4.1%
Descendants of Our Country / Nunatta Qitornai (NQ)	3.4%	2.2%

Table A2 compares the actual party shares in the Greenlandic elections 2018 (column 1) with the shares of each party in the present study (column 2). Both data are from 2018.

C External validity

The present study is based on a representative sample of the adult population in Greenland (see Appendix B), and the results are therefore generalizable to the target population

(Rodrik 2008) at the particular time of surveying. However, the external validity could be compromised by timing specific factors (Rosenzweig and Udry 2018), e.g. if the weakness of the Greenlandic economy was particularly topical or if independence was not a salient issue during the survey period. Neither of these potential problems appear to be relevant for the present case. In 2018, the GDP per capita growth in Greenland was 3.85% (World Bank 2019).[23] Moreover, independence was highly salient during this year, as it was one of the key issues during the General Elections that took place earlier in 2018.[24] This reduces the concern that the target group featured particularly malleable preferences during the survey period.

Still, the unique characteristics of Greenland should limit the generalizability to other secessionist movements. But while Greenland indeed is a special case (as argued e.g. by Grydehøj 2016), it also shares many features with other regions that aspire for independence. The trade-off between identity-concerns and economic security that many Greenlanders experience played a central role also in the Scottish referendum on independence in 2014, as well as in independence referendums in many former colonies, most recently in New Caledonia in 2018[25]. Consequently, the dynamics uncovered in the present case feature also in other secessionist regions, and this in turn implies a potential of replicating the experiment presented in this study elsewhere.

D Double treatment

26 participants in the Information Treatment group read both the information prime and the economic expectation items before answering questions about independence (due to a misprint of a few of the Danish surveys). I include them as respondents in the Information Treatment group based on both theoretical and empirical arguments. Since the information prime enhances focus on the economic problems of independence in itself, the salience prime should have no impact over and above the information prime, i.e. the effects are

23. The average GDP per capita growth in Greenland over the past 20 years was 2.61% (World Bank 2019).
24. See *SBS News* from April 25th: https://www.sbs.com.au/news/independence-dilemma-for-greenland-voters
25. See *The Diplomat* from November 9th 2018: www.thediplomat.com/2018/11/new-caledonia-moving-beyond-the-independence-debate/

assumed not be additive. I demonstrate the invariance of the double treatment empirically by comparing the probability of voting no among respondents subject only to information with those exposed to both information and the Salience Treatment by means of two-sided t-tests. Opposition to independence is indistinguishable between the“single treated” and the “double treated” (full sample: coefficient = -0.063, p-value= 0.590); complying respondents: coefficient = 0.017, p-value= 0.890).

E Variable descriptions

Table A3: Descriptive Statistics

Panel A: Categorical variables					
Label	Definition	Category	Freq.	Percent	Cum.
S1: Voting	If there was a referendum on independence TODAY, asking if you wanted Greenland to leave the Kingdom of Denmark, how would you vote?	Yes	233	37.46	37.46
		No	224	36.01	73.47
		I don't know	114	18.33	91.8
		I wouldn't vote	51	8.2	100
Read prime	Have you have read and understood the above [S3] info.?	No	127	21.20	21.20
		Yes	472	78.80	100
Trust prime	Do you trust the above [S3] info.?	No	218	37.46	37.46
		Yes	364	62.54	100
S2: Econ. Exp.	If Greenland becomes independent within the next 10 years, this will impact Greenland's economy...	Very negatively	123	22.40	22.40
		Somewhat negatively	147	26.78	49.18
		Not at all	99	18.03	67.21
		Somewhat positively	113	20.58	87.80
		Very positively	67	12.20	100
Gender	What is your gender?	Male	300	48.23	48.23
		Female	322	51.77	100
Survey Taken at Home	Enumerator indicates if survey was taken at a field session or both home and at field session, or only at participant's home	Survey session / both	225	36.17	36.17
		Only home	397	63.83	100
Nationality	What do you identify yourself as?	Other	10	1.62	1.62
		Both GRL and DAN	60	9.72	11.35
		Danish	29	4.70	16.05
		Greenlandic	518	83.95	100
Survey Language: GRL	Coded as 1 if the survey language was Greenlandic and 0 if language was Danish or English	Other	182	29.26	29.26
		Greenlandic	440	70.74	100
Income	What is your total annual household income, from all sources, before taxes?	0 - 100 000 DKK	132	22.80	22.80
		100 000 - 200 000 DKK	107	18.48	41.28
		200 000 - 300 000 DKK	86	14.85	56.13
		300 000 - 400 000 DKK	73	12.61	68.74
		400 000 - 500 000 DKK	55	9.50	78.24
		500 000 - 1 000 000 DKK	97	16.75	94.99
		1 000 000 DKK or more	29	5.01	100
Education	What is the highest level of education that you have achieved?	No education	90	15.20	15.20
		Some years of primary	20	3.38	18.58
		Primary school	157	26.52	45.10
		Currently at high school	6	1.01	46.11
		High School	22	3.72	49.83
		Vocational Training	197	33.28	83.11
		Currently at the University	8	1.35	84.46
		Bachelor degree	61	10.30	94.76
		Masters degree	29	4.90	99.66
		Ph.D.	2	0.34	100
Village	Enumerator indicates the participant's village of residence	Ilulissat	83	13.34	13.34
		Upernavik	15	2.41	15.76
		Kangersuatsiaq	35	5.63	21.38
		Tasiilaq	26	4.18	25.56
		Tiilerililaaq	21	3.38	28.94
		Nuuk	167	26.85	55.79
		Qeqertarsuatsiaat	16	2.57	58.36
		Qaqortoq	48	7.72	66.08
		Narsarmiit	20	3.22	69.29
		Sisimiut	85	13.67	82.96
		Kangaamiut	19	3.05	86.01
		Qeqertarsuaq	63	10.13	96.14
		Iginniarfik	24	3.86	100
Party	Which party did you vote for in the 2018 election?	Atassut	30	4.87	4.87
		Demokraatit	101	16.40	21.27
		Inuit Ataqatigiit	136	22.08	43.34
		Nunatta Qitornai	11	1.79	45.13
		Partii Naleraq	57	9.25	54.38
		Samarbejdspartiet	20	3.25	57.63
		Siumut	139	22.56	80.19
		Not disclose	122	19.81	100
Wants independence	Do you think that Greenland should become an independent country at some point in the future?	No	204	33.06	33.06
		Yes	413	66.94	100
Language	Which language(s) do you speak?	Greenlandic	571	91.80	91.80
		Danish	414	66.56	66.56
Panel B: Discrete variables		Freq.	Mean	Min	Max
Independence year	If yes [you want Greenland to be independent in the future], what year do you think Greenland should become independent?	333	2038.9	2019	2118
Age	What is your age?	588	45.81	18	85
Political interest	How interested or uninterested are you in politics?	614	4.74	1	7

F Economic expectations

Table A4: Effect of the Information Treatment on Economic Expectations

Dep. Var.:	Economic Expectations							
	Ologit (1)	Ologit (2)	Ologit (3)	OLS (4)	OLS (5)	OLS (6)	Not Comply (7)	Comply (8)
Information Treatment	-0.380**	-0.436**	-0.368*	-0.290**	-0.324**	-0.240*	-0.199	-0.328**
	(0.186)	(0.193)	(0.223)	(0.141)	(0.146)	(0.142)	(0.280)	(0.161)
Observations	368	349	325	368	349	325	82	286
R-squared	–	–	–	0.0114	0.0203	0.351	0.00637	0.0143
Dep. Var. Mean	2.829	2.811	2.797	2.829	2.811	2.797	3.183	2.727
Gender & Age	No	Yes	Yes	No	Yes	Yes	No	No
Additional Controls	No	No	Yes	No	No	Yes	No	No

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A4 displays regression estimates of the effect of the Information Treatment on different measures of economic expectations of independence. Columns 1-3 report Ordered Logistic Regressions estimations with Economic expectations (see Table A3) as the dependent variable. Columns 4-6 present the equivalent OLS regressions. Finally, in column 7 the sample is restricted to include only participants who did not comply with the treatment, whereas column 8 presents the treatment effect for these who reported complying with the treatment. Gender & Age indicates whether controls for age and gender were included. Additional Controls comprises (1) Survey taken at home or at field session, (2) National Identity FEs, (3) Greenlandic language survey dummy, (4) Income FEs, (5) Education FEs, (6) Village FEs and (7) Party FEs. The reference group is the Control Group.

G Turnout

Table A5: Effect of the Information Treatment on Turnout

Dep. Var.:	Voted					
	Neg. (1)	Pos. (2)	Neg. (3)	Pos. (4)	Pooled (5)	Pooled (6)
Information Treatment	0.170**	-0.00917	0.230***	-0.00760	-0.00917	-0.0177
	(0.0667)	(0.0700)	(0.0739)	(0.0865)	(0.0700)	(0.0767)
Negative Expectations					-0.127*	-0.202**
					(0.0725)	(0.0800)
Information Treatment × Negative Expectations					0.180*	0.237**
					(0.0967)	(0.107)
Observations	165	134	151	118	299	269
R-squared	0.0406	0.0001	0.370	0.375	0.0246	0.274
Dep. Var. Mean	0.770	0.799	0.775	0.805	0.783	0.788
Gender & Age	No	No	Yes	Yes	No	Yes
Additional Controls	No	No	Yes	Yes	No	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A5 displays OLS regression estimates of the effect of information on the probability of voting, separately for respondents with negative economic expectations of independence and respondents with positive economic expectations. Voted is defined as answering either "Yes" or "No" to S2. Columns 1-4 present regressions with split samples, while columns 5-6 present the results from regressions with an interaction between the Information Treatment and a dummy for having negative economic expectations. Gender & Age indicates whether controls for age and gender were included. Additional Controls comprises (1) Survey taken at home or at field session, (2) National Identity FEs, (3) Greenlandic language survey dummy, (4) Income FEs, (5) Education FEs, (6) Village FEs and (7) Party FEs. The reference group is the Control Group.

H Mechanisms

Table A6: The mediating effects of economic expectations and increased turnout

Dep. Var.:	Voted No					
	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	OLS (6)
Information Treatment	0.153*** (0.0505)	0.102** (0.0425)	0.0523 (0.0352)	0.125** (0.0516)	0.0973** (0.0471)	0.0279 (0.0403)
Observations	366	366	366	324	324	324
R-squared	0.0247	0.364	0.560	0.316	0.429	0.612
Dep. Var. Mean	0.393	0.393	0.393	0.414	0.414	0.414
Gender & Age	No	No	No	Yes	Yes	Yes
Additional Controls	No	No	No	Yes	Yes	Yes
Econ. Exp. Dummies	No	Yes	Yes	No	Yes	Yes
Voting Dummy	No	No	Yes	No	No	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A6 displays OLS regression estimates of the effect of information on the probability of voting no to independence. Columns 1 and 4 are baseline regressions where only respondents with non-missing data for economic expectations are included. In columns 2 and 5, economic expectations are controlled for. In columns 3 and 6, a dummy for voting is included. Gender & Age indicates whether controls for age and gender were included. Additional Controls comprises (1) Survey taken at home or at field session, (2) National Identity FEs, (3) Greenlandic language survey dummy, (4) Income FEs, (5) Education FEs, (6) Village FEs and (7) Party FEs. The reference group is the Control Group.

I Robustness

Table A7: Effect of the Information Treatment on Voting Behavior: Alternative Specifications

Dep. Var. = Voted No	Only GRL	Only GRL	Clustered	Weighted	Logit	LASSO
Panel A: Full Sample	(1)	(2)	(3)	(4)	(5)	(6)
Information Treatment	0.0977**	0.0905**	0.106**	0.127***	0.606***	0.121***
	(0.0433)	(0.0443)	(0.0428)	(0.0421)	(0.231)	(0.0399)
Observations	518	451	622	536	513	536
R-squared	0.0103	0.216	0.0111	0.270	–	–
Dep. Var. Mean	0.293	0.310	0.360	0.410	0.390	0.377
Gender & Age	No	Yes	No	Yes	Yes	Yes
Additional Controls	No	Yes	No	Yes	Yes	Yes
Panel B: CG as reference	(1)	(2)	(3)	(4)	(5)	(6)
Information Treatment	0.118**	0.0902*	0.129**	0.118**	0.588**	0.113**
	(0.0486)	(0.0515)	(0.0573)	(0.0489)	(0.281)	(0.0463)
Observations	351	304	417	359	333	359
R-squared	0.0166	0.254	0.0180	0.285	–	–
Dep. Var. Mean	0.299	0.322	0.367	0.430	0.420	0.393
Gender & Age	No	Yes	No	Yes	Yes	Yes
Additional Controls	No	Yes	No	Yes	Yes	Yes
Panel C: ST as reference	(1)	(2)	(3)	(4)	(5)	(6)
Information Treatment	0.0765	0.0895*	0.0836**	0.128***	0.668**	0.115**
	(0.0503)	(0.0526)	(0.0336)	(0.0486)	(0.295)	(0.0459)
Observations	343	304	419	365	347	365
R-squared	0.00671	0.265	0.00734	0.291	–	–
Dep. Var. Mean	0.321	0.329	0.389	0.433	0.412	0.397
Gender & Age	No	Yes	No	Yes	Yes	Yes
Additional Controls	No	Yes	No	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A7 displays estimates of the effect of information on the probability of voting no to independence using a range of alternative statistical specifications. In columns 1 and 2, only respondents who consider their primary identity to be Greenlandic are included. In column 3, I bootstrap standard errors clustered at the village level, to account for the potential observations within localities are inter-dependent. Column 4 provides weighted estimates in order to adjust for under/over sampling of the different strata. In column 5, I relax the linearity assumption by estimating the treatment effect using Logistic Regressions. Column 6 show estimates from Post-Double-Selection (PDS) Least Absolute Shrinkage and Selection Operator (LASSO) models (see the discussion in Section 5.4). Additional Controls comprises (1) Survey taken at home or at field session, (2) National Identity FEs, (3) Greenlandic language survey dummy, (4) Income FEs, (5) Education FEs, (6) Village FEs and (7) Party FEs.

Table A8: Effect of the Information Treatment on Voting Behavior: Register-Based Controls

Dep. Var. = Voted No	OLS	OLS	OLS	2SLS	2SLS	2SLS
Panel A: Full Sample	(1)	(2)	(3)	(4)	(5)	(6)
Information Treatment	0.124***	0.126***	0.111**			
	(0.0480)	(0.0483)	(0.0459)			
Treatment Complying				0.162***	0.164***	0.145**
				(0.0618)	(0.0621)	(0.0571)
Observations	468	468	461	468	468	461
R-squared	0.0150	0.0151	0.244	0.0238	0.0241	0.245
Dep. var. Mean	0.346	0.346	0.345	0.346	0.346	0.345
First Stage F-statistic	–	–	–	498.3	489.4	464.1
Gender & Age	No	Yes	Yes	No	Yes	Yes
Additional Controls	No	No	Yes	No	No	Yes
Panel B: CG as reference	(1)	(2)	(3)	(4)	(5)	(6)
Information Treatment	0.137**	0.136**	0.145***			
	(0.0544)	(0.0553)	(0.0532)			
Treatment Complying				0.179**	0.177**	0.189***
				(0.0701)	(0.0712)	(0.0652)
Observations	308	308	301	308	308	301
R-squared	0.0205	0.0228	0.293	0.0339	0.0355	0.291
Dep. Var. Mean	0.360	0.360	0.359	0.360	0.360	0.359
First Stage F-statistic	–	–	–	497.2	469.4	403.3
Gender & Age	No	Yes	Yes	No	Yes	Yes
Additional Controls	No	No	Yes	No	No	Yes
Panel C: ST as reference	(1)	(2)	(3)	(4)	(5)	(6)
Information Treatment	0.112**	0.113**	0.102*			
	(0.0548)	(0.0549)	(0.0532)			
Treatment Complying				0.145**	0.147**	0.134**
				(0.0706)	(0.0707)	(0.0647)
Observations	311	311	305	311	311	305
R-squared	0.0133	0.0138	0.302	0.0258	0.0264	0.305
Dep. Var. Mean	0.373	0.373	0.370	0.373	0.373	0.370
First Stage F-statistic	–	–	–	497.2	484.9	399.1
Gender & Age	No	Yes	Yes	No	Yes	Yes
Additional Controls	No	No	Yes	No	No	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A8 displays both Ordinary Least Squares (OLS) and Two-Stage Least Squares (2SLS) regression estimates of the effect of information on the probability of voting no to independence. Columns 1-3 display OLS regressions with the full sample in Panel A (using both the Control Group and the Salience Treatment as reference group), the Control Group as reference (Panel B) and the Salience Treatment as reference in Panel C. Columns 4-6 show 2SLS regressions with complying (having read the information prime) × having been assigned the Information Treatment instrumented by having been assigned the Information Treatment. Gender & Age indicates whether controls for gender and age (register based) were included. Additional Controls comprises (1) Survey taken at home or at field session, (2) Nationality FEs (register based), (3) Greenlandic language survey dummy, (4) Income FEs (register based), (5) Education FEs (register based), (6) Village FEs and (7) Party FEs.

Table A9: Effect of the Information Treatment on Opposing Independence:
Treatment effects of respondents who did not Read or Trust the information prime

Dep. Var.:	Voted No					
	Not read (1)	Not read (2)	Not read (3)	Not trust (4)	Not trust (5)	Not trust (6)
Information Treatment	0.0557 (0.0926)	0.0359 (0.0989)	0.0621 (0.146)	-0.00964 (0.0773)	-0.0331 (0.0797)	0.000432 (0.0830)
Observations	85	80	71	146	141	130
R-squared	0.00429	0.00523	0.458	0.000108	0.0105	0.475
Dep. Var. Mean	0.235	0.250	0.254	0.308	0.319	0.315
Gender & Age	No	Yes	Yes	No	Yes	Yes
Additional Controls	No	No	Yes	No	No	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A9 displays OLS regression estimates of the effect of information on the probability of voting no to independence. Columns 1-3 present regressions where only respondents who did not read the treatment are included, whereas columns 4-6 show the estimates when only participants who did not trust the information prime are considered. Gender & Age indicates whether controls for age and gender were included. Additional Controls comprises (1) Survey taken at home or at field session, (2) National Identity FEs, (3) Greenlandic language survey dummy, (4) Income FEs, (5) Education FEs, (6) Village FEs and (7) Party FEs. The reference group is the Control Group.

Table A10: Covariates balance between the Control Group and Information Treatment for **Strong identity respondents**

	Control Group			Information Treatment			
	N	Mean	S.d.	N	Mean	S.d.	Difference
Woman	75	0.51	0.50	63	0.48	0.50	-0.030
Age	71	47.37	17.39	58	45.50	17.43	-1.866
Lives in Town	75	0.63	0.49	63	0.48	0.50	-0.150*
Survey Taken at Home	75	0.68	0.47	63	0.76	0.43	0.082
Greenlandic	75	0.96	0.20	63	0.98	0.13	0.024
Survey Language: GRL	75	0.95	0.23	63	0.97	0.18	0.022
Family in Denmark	37	0.65	0.48	32	0.56	0.50	-0.086
Lived in Denmark	73	0.10	0.30	58	0.05	0.22	-0.044
Pol. Pref.: Left-Right	70	4.50	2.67	57	4.77	2.88	0.272
Party anti independence	74	0.07	0.25	60	0.07	0.25	-0.001
Party pro independence	74	0.77	0.42	60	0.80	0.40	0.030
Public Sector	57	0.26	0.44	47	0.17	0.38	-0.093
Perceived Income Status	68	4.43	1.90	57	4.39	2.00	-0.041
Financial Difficulties	60	0.60	0.49	50	0.62	0.49	0.020
HH Earnings < 200 K	65	0.69	0.47	57	0.65	0.48	-0.043
HH Earnings 200-500 K	65	0.25	0.43	57	0.26	0.44	0.017
HH Earnings > 500 K	65	0.06	0.24	57	0.09	0.29	0.026
Trust Den. Government	49	2.53	1.04	35	2.46	1.27	-0.073
Trust Gre. Government	58	3.02	1.21	48	3.17	1.19	0.149
Primary School	70	0.74	0.44	58	0.84	0.37	0.102
High School/Professional	70	0.23	0.42	58	0.16	0.37	-0.073
University Degree	70	0.03	0.17	58	0.00	0.00	-0.029
Internet	73	0.53	0.50	59	0.46	0.50	-0.077
TV	73	0.66	0.48	59	0.64	0.48	-0.013
Radio	73	0.64	0.48	59	0.56	0.50	-0.085
Newspaper	73	0.22	0.42	59	0.14	0.35	-0.084

Table A10 shows descriptive statistics on a range of relevant covariates for the strong identity respondents in CG and IT. The Difference column displays coefficients and corresponding significance levels from OLS regressions with the Information Treatment as the sole regressor and robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: Covariates balance between the Control Group and Information Treatment: **Mixed identity respondents**

	Control Group			Information Treatment			
	N	Mean	S.d.	N	Mean	S.d.	Difference
Woman	126	0.48	0.50	149	0.61	0.49	0.127**
Age	119	48.98	14.40	143	44.47	13.95	-4.515**
Lives in Town	126	0.89	0.32	149	0.88	0.33	-0.010
Survey Taken at Home	126	0.63	0.49	149	0.65	0.48	0.024
Greenlandic	126	0.82	0.39	149	0.77	0.43	-0.052
Survey Language: GRL	126	0.62	0.49	149	0.62	0.49	-0.002
Family in Denmark	79	0.89	0.32	83	0.92	0.28	0.030
Lived in Denmark	124	0.58	0.50	147	0.58	0.50	-0.002
Pol. Pref.: Left-Right	118	4.80	1.85	139	5.06	1.68	0.261
Party anti independence	125	0.24	0.43	148	0.22	0.42	-0.017
Party pro independence	125	0.58	0.49	148	0.54	0.50	-0.043
Public Sector	108	0.46	0.50	122	0.40	0.49	-0.061
Perceived Income Status	118	5.39	1.74	145	5.32	1.63	-0.073
Financial Difficulties	110	0.38	0.49	135	0.50	0.50	0.114*
HH Earnings $<$ 200 K	118	0.30	0.46	143	0.33	0.47	0.032
HH Earnings 200-500 K	118	0.43	0.50	143	0.42	0.50	-0.013
HH Earnings $>$ 500 K	118	0.27	0.45	143	0.25	0.44	-0.019
Trust Den. Government	109	2.63	1.12	131	2.83	0.94	0.199
Trust Gre. Government	117	2.82	1.13	137	2.72	1.10	-0.098
Primary School	118	0.33	0.47	148	0.31	0.46	-0.020
High School/Professional	118	0.47	0.50	148	0.44	0.50	-0.027
University Degree	118	0.20	0.40	148	0.25	0.43	0.047
Internet	124	0.77	0.43	147	0.78	0.41	0.016
TV	124	0.77	0.42	147	0.76	0.43	-0.012
Radio	124	0.66	0.48	147	0.62	0.49	-0.042
Newspaper	124	0.38	0.49	147	0.35	0.48	-0.032

Table A11 shows descriptive statistics on a range of relevant covariates for the mixed identity respondents in CG and IT. The Difference column displays coefficients and corresponding significance levels from OLS regressions with Information Treatment as the sole regressor and robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

J Heterogeneous treatment effects

Figure A2: Identity and economic voting

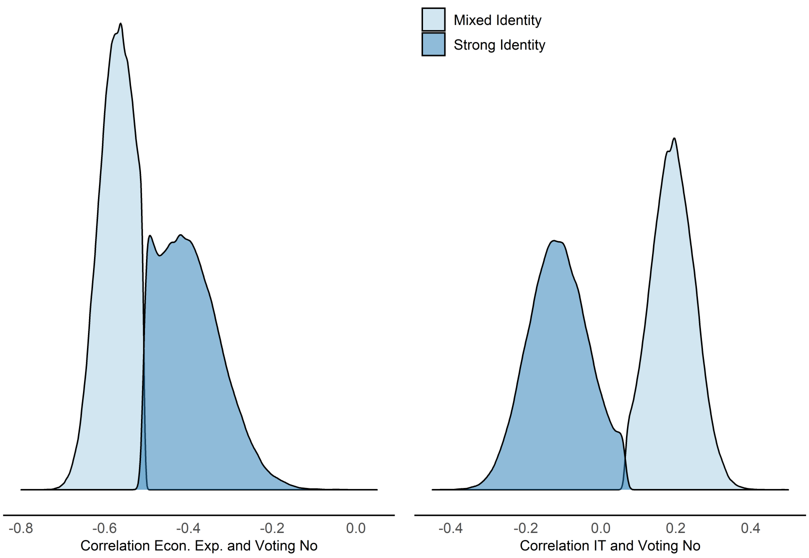

Figure A2: Distributions of correlation coefficients from bootstrapping (10,000 random sampling with replacement). The samples were separated by strong and mixed national identities.

The correlation between economic expectations and opposing independence is significantly stronger for respondents that speak Danish (correlation coefficient= -0.566) relative to those who do not (correlation coefficient= -0.410, the difference is significant at the 5% level). The change in voting behavior induced by the prime is, correspondingly, significantly stronger for the mixed identity respondents (the difference between the groups is significant at the 1% level.). Whereas exposure to the information prime increases opposition to independence in the mixed identity group (correlation coefficient= 0.193), it is associated with less opposition in the strong identity group (correlation coefficient=-0.116).

Table A12: Identity and Economic Voting: Register based controls

Dep. Var.:	Voted No					
Sample:	SI	MI	Pool	SI	MI	Pool
Panel A.	(1)	(2)	(3)	(4)	(5)	(6)
Economic Expectations	-0.0965***	-0.199***	-0.199***	-0.0619	-0.185***	-0.175***
	(0.0341)	(0.0216)	(0.0217)	(0.0373)	(0.0284)	(0.0275)
Strong identity			-0.532***			-0.441**
			(0.158)			(0.181)
Economic Expectations			0.103**			0.0842*
× Strong identity			(0.0402)			(0.0456)
Observations	78	187	265	78	184	262
R-squared	0.112	0.282	0.336	0.608	0.447	0.457
Dep. Var. Mean	0.141	0.497	0.392	0.141	0.495	0.389
Gender & Age	No	No	No	Yes	Yes	Yes
Additional Controls	No	No	No	Yes	Yes	Yes
Sample:	SI	MI	Pool	SI	MI	Pool
Panel B.	(1)	(2)	(3)	(4)	(5)	(6)
Information Treatment	-0.0663	0.194***	0.194***	-0.0855	0.219***	0.205***
	(0.0643)	(0.0691)	(0.0692)	(0.0677)	(0.0713)	(0.0708)
Strong identity			-0.222***			-0.0515
			(0.0687)			(0.0830)
Information Treatment			-0.260***			-0.229**
× Strong identity			(0.0944)			(0.101)
Observations	103	203	306	100	200	300
R-squared	0.00980	0.0376	0.149	0.519	0.312	0.325
Dep. Var. Mean	0.126	0.478	0.359	0.130	0.475	0.360
Gender & Age	No	No	No	Yes	Yes	Yes
Additional Controls	No	No	No	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A12 displays OLS regression analyses of the link between identity and economic voting. SI stands for Strong identity and MI stands for Mixed identity. In Panel A, I show correlations between identity and opposition to independence and interact it with the measure of identity. Panel B reports how the information prime influenced voting behavior separately for respondents with strong and mixed identity. Gender & Age indicates whether controls for gender and age (register based) were included. Additional Controls comprises (1) Survey taken at home or at field session, (2) Nationality FEs (register based), (3) Greenlandic language survey dummy, (4) Income FEs (register based), (5) Education FEs (register based), (6) Village FEs and (7) Party FEs.

Figure A3: Identity and economic voting

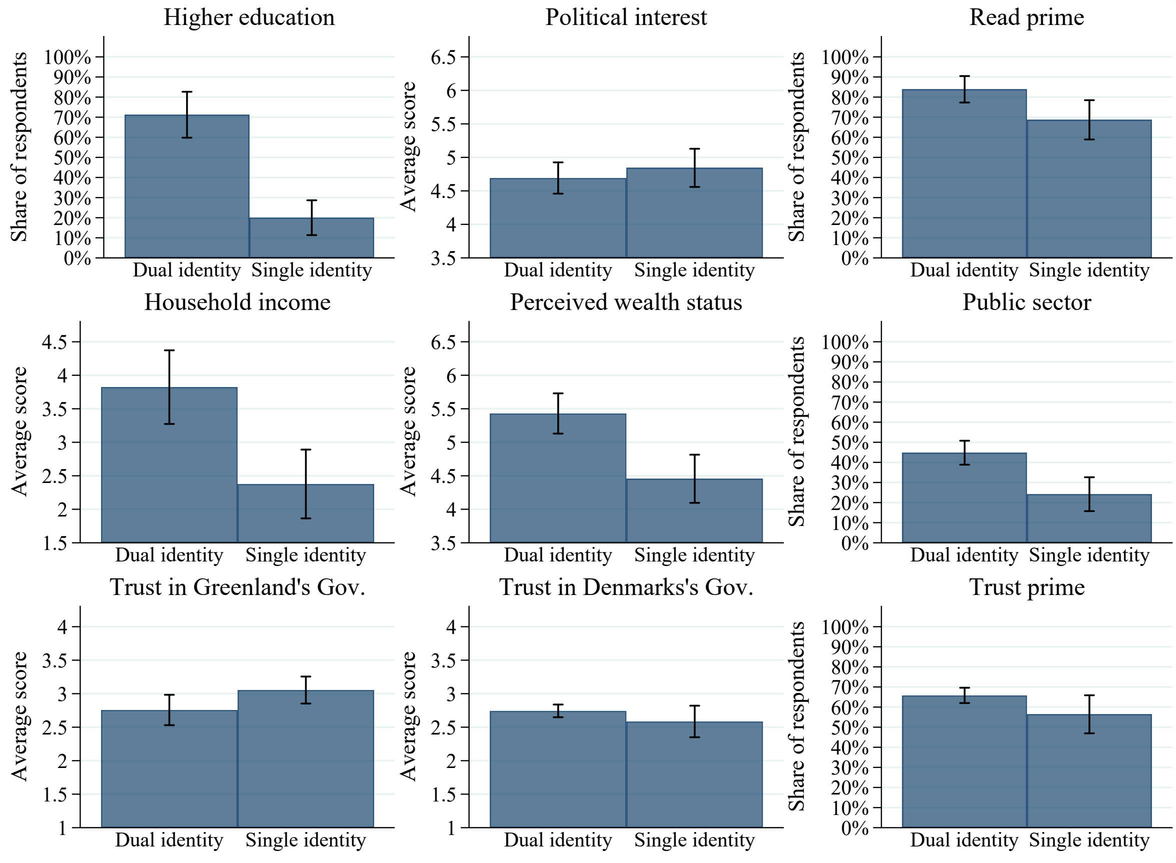

Figure A3: Underlying differences between respondents with with strong and mixed identity by relevant background characteristics. The vertical bars represent 95% confidence intervals.

Table A13: Alternative explanations

Dep. Var.:	Voted No (1)	Voted No (2)	Voted No (3)	Voted No (4)	Voted No (5)	Voted No (6)	Voted No (7)	Voted No (8)	Voted No (9)
Information Treatment	0.205**	0.328***	0.0987	0.199*	0.0775	0.115	0.138	0.0374	0.0449
	(0.0860)	(0.126)	(0.103)	(0.114)	(0.152)	(0.0812)	(0.129)	(0.150)	(0.0879)
strong identity	-0.113*	-0.217***	-0.203***	-0.116	-0.211***	-0.231***	-0.220***	-0.216***	-0.228***
	(0.0666)	(0.0616)	(0.0619)	(0.0720)	(0.0661)	(0.0700)	(0.0658)	(0.0730)	(0.0636)
Information Treatment	-0.238**	-0.241***	-0.250***	-0.276***	-0.209**	-0.207**	-0.235**	-0.189*	-0.230***
× strong identity	(0.0985)	(0.0848)	(0.0845)	(0.0965)	(0.0908)	(0.0956)	(0.0915)	(0.105)	(0.0863)
Higher Education	0.256***								
	(0.0669)								
Information Treatment	-0.0561								
× Higher education	(0.101)								
Political interest		0.0202							
		(0.0173)							
Information Treatment		-0.0305							
× Political interest		(0.0240)							
Read prime			0.0723						
			(0.0764)						
Information Treatment			0.115						
× Read prime			(0.104)						
Household income				0.0651***					
				(0.0193)					
Information Treatment				-0.00541					
× Household income				(0.0258)					
Perceived wealth status					0.0267				
					(0.0188)				
Information Treatment					0.0172				
× Perceived wealth status					(0.0259)				
Public sector						0.0455			
						(0.0758)			
Information Treatment						0.138			
× Public sector						(0.109)			
Trust Greenlands Government							-0.0900***		
							(0.0271)		
Information Treatment							0.0189		
× Trust Greenland's Government							(0.0391)		
Trust Denmarks Government								0.0393	
								(0.0342)	
Information Treatment								0.0464	
× Trust Denmark's Government								(0.0495)	
Trust prime									-0.0280
									(0.0698)
Information Treatment									0.212**
× Trust prime									(0.0949)
Observations	394	406	397	383	388	334	360	324	384
R-squared	0.179	0.143	0.153	0.189	0.156	0.159	0.183	0.131	0.162
Dep. Var. Mean	0.373	0.367	0.368	0.379	0.376	0.374	0.392	0.414	0.375

Table A13 displays OLS regressions with a dummy for voting no to independence as the dependent variable, and the interaction between the Information Treatment and strong identity as the independent variable of interest. Each column introduces one potentially confounding variable interacted with the Information Treatment. In columns 1-3, the confounding variables I account for are (1) a dummy for higher education (coded as 1 for educational attainment higher than primary schooling), (2) political interest and (3) a dummy indicating whether the respondent stated that they read the information prime. In columns 4-6, I control respectively for (4) Household income, (5) Perceived wealth status and (6) Public Sector, which is a dummy indicating working in the public sector. Lastly, columns 7-9 account for trust in the Greenlandic Government, trust in the Danish Government, and whether the respondent trusted the information prime or not. All regressions include robust standard errors. The reference group is the Control Group. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

K Timing of independence

Table A14: The timing of independence

Dep. Var.:	Preferred Year of Independence					
	Ref: CG (1)	Ref: ST (2)	Ref: CG (3)	Ref: ST (4)	Ref: CG (5)	Ref: ST (6)
Information Treatment	6.278**	5.363**	6.299**	5.668**	6.634**	7.061**
	(2.499)	(2.613)	(2.628)	(2.702)	(2.954)	(2.762)
Observations	221	226	213	216	203	207
R-squared	0.0276	0.0184	0.0287	0.0201	0.334	0.378
Dep. Var. Mean	2039.6	2040.0	2039.7	2040.1	2039.8	2040.4
Gender & Age	No	No	Yes	Yes	Yes	Yes
Additional Controls	No	No	No	No	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A14 displays OLS regression estimates of the effect of information on the preference for the year of independence. Columns 1, 3 and 5 use the Control Group as the reference group, whereas columns 2, 4 and 6 use the Salience Treatment as the reference group.5 outlier observations, who stated a preferred year of independence later than 2119, are excluded from all regressions. Gender & Age indicates whether controls for age and gender were included. Additional Controls comprises (1) Survey taken at home or at field session, (2) National Identity FEs, (3) Greenlandic language survey dummy, (4) Income FEs, (5) Education FEs, (6) Village FEs and (7) Party FEs. All regressions include robust standard errors.

Chapter 2

Predicting Local State Capacity in Africa: A Machine Learning Approach

Predicting Local State Capacity in Sub-Saharan Africa:

A Machine Learning Approach*

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Abstract

Despite the need to measure state capacity at a sub-national level, most studies still use country-level indicators as rough approximations of the local counterpart. We estimate a measure of state capacity at the 2.5×2.5 arc-minutes grid cell level (≈ 5 kilometers) for Sub-Saharan Africa. The measure builds on geocoded survey-based data on local state capacity which we predict and extrapolate using an ensemble of regression trees. We demonstrate the usefulness of measuring state capacity at a disaggregated level by including our local state capacity index as a moderating factor in the relationship between oil wealth and armed conflict. The findings suggest that cells with higher local state capacity face lower risks of conflicts caused by oil price hikes.

Keywords: state capacity, machine learning, conflict
JEL Codes: H11, H70, Q34

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1 Introduction

Strong states are thought to facilitate and promote a range of desirable political and economic outcomes. High state capacity has been associated with economic growth (Acemoglu and Robinson 2012; North 1982; Gennaioli and Rainer 2007; Michalopoulos and Papaioannou 2013) as well as democracy and democratic consolidation (Fukuyama 2013; Linz and Stepan 1996; Huntington 1996). The importance of well-functioning state institutions is perhaps most notable in its absence. Many developing countries do not pertain equal control within their borders (Herbst 2000; O'Donnell 1993; Boone 2012), and commonly feature pockets of weak state presence. In areas where states lack the capacity to uphold the most basic services, such as policing, their authority may even be challenged by non-state actors. The resulting friction between state and non-state actors is a breeding ground for intra-state conflict (Fearon and Laitin 2003; Buhaug and Rød 2006; Fjelde and De Soysa 2009; Braithwaite 2010; Hendrix 2011), the most extreme outcome of state failure.

Despite the fact that state capacity is inherently local, the literature has generally treated the concept as a property of the central state, ignoring the spatial unevenness of state authority and territorial control. For instance, in their seminal paper, Fearon and Laitin (2003) argue that rebel groups often form and operate in remote areas where states are weaker. The argument implies a local dynamic of sub-national territorial reach; yet, the authors rely on cross-country measures. While advancements in disaggregated conflict data has allowed conflict research to go increasingly local (Buhaug and Rød 2006; Rustad et al. 2011), the research on local state capacity is lagging behind. This is due to a lack of quantitative measures of statehood at the sub-national level. Consequently, mapping geographic variation in state capacity is imperative to improve our understanding of the state, its causes, and effects.

In this chapter, we demonstrate a new approach to measuring local state capacity in Sub-Saharan Africa. First we construct an index of perceived local state capacity using geocoded survey data from the Afrobarometer (Afrobarometer Data 2004, 2005) capturing the three constitutive dimensions of state capacity: coercive, extractive, and administrative capacity. Second, we predict the state capacity index by means of non-parametric machine learning techniques, using inputs which regulate costs and benefits of state presence, e.g. infrastructure, population, topography, and economic activity, as predictors. Third, we extrapolate a predicted measure of state capacity to all 2.5×2.5 arc-minutes grid cells ($\approx 5 \times 5$ kilometers) in Sub-Saharan Africa.[1] To document the validity of our state capacity measure, we correlate it with ethnic political power, pre-colonial centralization, and vaccination coverage: factors that, for different reasons, should co-vary with the sub-national distribution of state capacity.

We further showcase the usefulness of our measure of local state capacity by considering it as a moderating factor in the relationship between oil wealth and conflict. Using a panel dataset on oil wealth and conflict in Sub-Saharan Africa, we estimate two-way fixed effects models with oil-related conflict as the dependent variable and the triple interaction between oil price, oil deposits, and local state capacity as the explanatory variable of main interest. The results show that while low state capacity oil regions face a higher risk of oil-related conflict when the price of oil increases, this exogenous shock to oil wealth has no such effects in areas with high state capacity.

Our work contributes to the literature on state capacity, and in particular to the applied research concerned with measuring the concept. Increasingly, scholars have recognized that state power tends to be spread unevenly within countries (Herbst 2000; O'Donnell 1993; Boone 2012), yet the empirical research on state capacity has continued to rely on national measures (Soifer 2008). In order to adequately capture dynamics that are local by nature, state capacity ought to be measured at the appropriate level, i.e. where the action takes place.

We are not the first to attempt overcoming the data scarcity endemic to research on state capacity. A number of proxies have been proposed to capture different dimensions of sub-national state power, e.g. historical state institutions (Michalopoulos and Papaioannou 2013; Acemoglu, García-Jimeno, and Robinson 2015; Dell, Lane, and Querubin 2018), census data (Lee and Zhang 2017), sub-national tax collection (Harbers 2015), satellite data (Koren and Sarbahi 2018), and survey data (Luna and Soifer 2017; Wig and Tollefsen 2016; Fergus-

1. This approach follows Mosser et al. (2019) predicting vaccination coverage across Africa based on survey data and grid cell covariates, and Bergquist et al. (2019) predicting agricultural output for households in Uganda.

son, Molina, and Robinson 2020). While these are innovative approaches, they inevitably entail limitations. Historical data can only inform on the effect of institutions that persisted over time, and is often confined to a specific dimension of the state, such as road provision (Acemoglu, García-Jimeno, and Robinson 2015). Similarly, more minimalist measures of state capacity, such as the quality of census data (Lee and Zhang 2017)[2] and sub-national tax collection (Harbers 2015), are narrow in scope, and the data availability restricted to few countries. Night-time light emissions and other satellite data, on the other hand, provide for global samples, but are noisy and at best indirect proxies of local state capacity. Survey data can more accurately capture citizens' perceptions of state institutions (Fergusson, Molina, and Robinson 2020), but does not comprehensively cover cross-country territories, and may thus only provide patchy views of local state capacity.

Our novel approach to predict and extrapolate a spatially disaggregated measure of state capacity suggests a way forward for studies concerned with measuring local state capacity in data scarce territories. The approach combines several of the strategies outlined above, and, in doing so, overcomes some of the limitations inherent to each methodology. We achieve this by (1) combining information from multiple factors related to state capacity, (2) inductively deriving how these factors relate to state capacity in a data driven manner, and (3) extrapolating the predictions of the model to inform on local state capacity across comprehensive territories. While we make use of the detailed information of survey data based approaches, we link this information to structural factors, and thereby reduce idiosyncratic errors typical of subjective data. Moreover, since our model learns the actual weights of a range of factors, instead of presupposing their relevance, the signal-to-noise ratio of our measure is substantially higher relative to satellite data-based proxies of state capacity. Our methodology can easily be replicated and applied to other settings, and scholars interested in more specific dimensions of state capacity (or related concepts) can with relatively little effort modify the methodology described in this chapter to construct similar local measures with extensive coverage.

We also contribute to the research studying the effects of natural resource wealth,

2. Lee and Zhang (2017) argue that deviations from a smooth age distribution within a sub-national area reveal the state is incapable of conducting a reliable census in that area, suggestive of low state capacity.

in particular oil wealth, on conflict. The point of departure of this literature is the abundant anecdotal evidence of oil-induced conflict in oil-producing developing countries (Ross 2012). Empirical studies have confirmed the link between oil and conflict in specific contexts (Dube and Vargas 2013; Nwokolo 2018), but the external validity of these findings have been contested (Cotet and Tsui 2013; Bazzi and Blattman 2014). As potential reconciliations of these conflicting findings, a number of studies have suggested that the risk of conflict depends on contextual factors (Morelli and Rohner 2015; Lessmann and Steinkraus 2019; Cordella and Onder 2020; Berman et al. 2017; Buhaug 2010; Nordvik 2019). We propose a new moderating factor of first-order importance, namely local state capacity. We advance the literature on oil conflicts by documenting that our measure of local state capacity moderates the risk of conflict induced by oil price hikes, a result which suggests that the natural resource curse is conditional on local state strength in oil regions.

The paper is structured as follows. In Section 2, we discuss how previous studies have conceptualized and attempted to measure state capacity. In Section 3, we describe the data and strategy used for predicting local state capacity. In Section 4, we validate our predicted measure by linking it to other data sources related to state capacity. In Section 5, we describe and present the empirical application of local state capacity by examining its potentially moderating effect on the relationship between oil wealth and risk of conflict. In Section 6, we discuss the limitations of our approach. Section 7 concludes.

2 State capacity: concept and measurement

2.1 State capacity concept

Over the last decades, there has been a renewed focus on the importance of state institutions. Well-functioning states have been shown to promote economic development (Acemoglu and Robinson 2012; North 1982), democracy (Fukuyama 2005; Linz and Stepan 1996), and a range of other development outcomes (van Ham and Seim 2018; Seeberg 2019; Sullivan 2020). Conversely, where states lack the ability to monitor and deter rebellion, the risk of intra-state violence and conflict is substantially higher (Fearon and Laitin 2003; Buhaug and

Rød 2006; Fjelde and De Soysa 2009; Braithwaite 2010; Hendrix 2011).

At the center of this literature is the concept of state capacity. State capacity is typically defined broadly as "a government's ability to make and enforce rules, and to deliver services" (Fukuyama 2013, p. 350), or "the ability of the state to effectively implement official goals" (Hanson and Sigman 2013, p. 2). In other words, it is the state's ability to "get things done". The concept of state capacity is perhaps best captured by what Michael Mann calls the *infrastructural power* of the state: "the capacity of the state to actually penetrate civil society, and to implement logistically political decisions throughout the realm" (Mann 1984, p. 189; see also Soifer 2008; Soifer and vom Hau 2008).[3] Mann's concept of infrastructural power can be traced back to Weber's canonical definition of the state (Weber 1991), but it also brings attention to a dimension of state power especially pertinent to the focus in this chapter: the unevenness of the state's ability to penetrate society and exercise control of territory within its borders.

Mann defines the state with reference to the "political relations [which] radiates outwards from a center to cover a territorially demarcated area" (Mann 1984, p. 187). This way of conceptualizing state capacity implies a spatial dimension, but the role of geography is conditional on the power of the central state. While strong states are able to penetrate and govern remote areas, weak states lack this ability (see also O'Donnell 1993; Boone 2012; Soifer and vom Hau 2008; Fukuyama 2013). This idea can also be found in Boulding's concept of a loss-of-strength gradient (Boulding 1962). While initially applied to international conflict, the idea also captures the projection of power in domestic settings. Basically, a state's strength (or capacity) is largest at its home base and diminishes as one moves away from the center. The extent of this decline depends on the loss-of-strength gradient capturing the cost of exerting authority across space (see also Buhaug 2010; Boone 2012). The relevance of structural limitations to state building is highlighted also by e.g. Herbst (2000), who

3. Mann distinguishes between the *infrastructural* power and the *despotic* power of the state, with the latter capturing the state's autonomy from societal actors. This also highlights an important distinction that is nonetheless neglected by some scholars, i.e. that between the state's ability to make autonomous decisions and the ability to actually implement and carry out those decisions. For instance, Gleditsch and Ruggeri (2010) distinguish between repressive and accommodative forms of state capacity, but this distinction is arguably less related to the capacity of the state and more to how the state is governed, i.e. the regime type (or its autonomy from societal actors); these are still important concepts, but different from the usual conception of state capacity.

stated that "the fundamental problem facing state-builders in Africa . . . has been to project authority over inhospitable territories that contain relatively low densities of people" (p. 11). In the absence of data on state capacity *within* countries, however, such high-level assertions remain untested.

While most scholars would agree about the broader meaning of state capacity, extensive academic focus has not led to a consensus on the more specific meanings and operationalizations. Defining the concept is further complicated by the multitude of related and synonymous terms, e.g. state strength, state power, political capacity, or state fragility. Recently scholars have begun to disentangle the concept, categorizing state capacity into several distinct dimensions such as extractive, coercive, and administrative capacity (e.g. Hendrix 2010; Hanson and Sigman 2013; Berwick and Christia 2018). Partitioning the concept of state capacity into multiple dimensions has added to conceptual development by making explicit which dimensions that are of concern, and how these relate to one another. However, it also stands to reason that these dimensions are highly co-dependent. Extractive capacity requires coercive and administrative capacity, which in turn can be improved by the ability to collect revenue and finance the coercive apparatus. At minimum, a prerequisite for state capacity is the ability of the state to project power (Lindvall and Teorell 2016; Berwick and Christia 2018). A high capacity state must, first of all, be able to enforce its rules and collect revenues across the territory it claims to rule (Johnson and Koyama 2017, p. 2).

2.2 Measuring state capacity

The spatial heterogeneity of states' projection of power presents researchers with challenges in measurement. The "national capabilities"-approach (Soifer 2008) has dominated in the quantitative literature, and this is in part due to a lack of suitable measures at the sub-national level. While there have been a number of attempts at measuring local state presence, they often entail limitations in coverage, scope, and measurement validity.

Traditionally, the most prominent measure of state capacity is based on indicators of tax collection—typically some ratio of tax revenues to total GDP.[4] As has long been

4. Following this approach, Harbers (2015) collects data on municipal tax revenues to capture local state capacity in Ecuador.

recognized in the fiscal sociology literature (Schumpeter [1918] 1954; Tilly 1975; Levi 1989; see also Lieberman 2002; Cheibub 1998), taxation is a central characteristic of the state. The more capable the state is, the more it can extract revenue from its citizens. However, local tax revenue indicators are typically not readily available across a large sample of countries, thus limiting the empirical (and geographical) scope of studies using tax revenues to capture state reach. Moreover, tax revenues are not only a function of the state's ability to tax, but also of political preferences (Fukuyama 2013). Lastly, some economic activities are easier to tax than others, e.g. revenues from trade and natural resources (see for example Sánchez de la Sierra 2020), and total tax revenues are therefore not always reflecting the state's extractive capacity, but rather structural factors moderating the ease with which taxes can be collected.

Instead of relying on tax revenues or tax ratios to proxy state capacity, scholars have increasingly turned to more specific dimensions of the state to approximate its presence. One innovative approach introduced by Lee and Zhang (2017) aims to capture a minimalist concept of state presence, building on the idea that states are intrinsically in need of information (e.g., in order to tax its citizens, it needs to know its citizens). They use census data at both the national and sub-national level and consider the deviation from a smooth age distribution to develop a measure of state presence. While their approach is conceptually clean and intuitive, it suffers from a lack of cross-country availability, especially at the sub-national level.

Another way of proxying for state capacity has been output oriented, focusing in particular on public goods provision.[5] This operationalization allows researchers to track the impact of state capacity on relevant development outcomes; examples include the spread of postal services and innovation in 19th century United States (Acemoglu, Moscona, and Robinson 2016), as well as infrastructural investments (Acemoglu, García-Jimeno, and Robinson 2015) and the expansion of schooling (Dittmar and Meisenzahl 2016) on subsequent economic growth. These innovative measures have provided for causal identification of state investments in particular contexts, but the contextual nature also means that they

5. Rotberg (2003) writes that "Nation-states exist to provide a decentralized method of delivering political (public) goods to persons living within designated parameters (borders)".

are not readily available on a larger scale.[6]

An emergent literature has instead propounded a survey-based approach to capture state capacity. Luna and Soifer (2017) use survey data to capture the variation of state capacity across space. They focus on specific dimensions, such as territorial reach and taxation, and formulate a series of questions to gauge peoples' experience with the state. For instance, they ask how long it would take for the police to arrive at their home and how often they receive a receipt without requesting one (the latter intended to capture enforcement of VAT taxes). Similarly, Fergusson, Molina, and Robinson (2020) use survey data on tax evasion to measure local state capacity and find positive correlations with measures of clientelism. The survey-based approach has the advantage of enabling specifically tailored questions to the concept of interest, while also capturing sub-national variation. However, conducting surveys across a large number of countries (while still covering the entire territory of each country) is costly and survey-based measures of state capacity are, thus, in themselves inherently limited in their spatial reach.

Another approach to measuring local state capacity takes advantage of the increasing availability of geographical information systems (GIS), such as satellite data on both human impacts and geographical factors (e.g. terrain). Koren and Sarbahi (2018), for instance, attempt to overcome the limitations of national-level measures by using night-time light emissions as proxy for sub-national state capacity. However, while available at the global level, light density is a poor proxy for state capacity when used on its own. It is typically said to capture local economic development (e.g., Henderson, Storeygard, and Weil 2012; Michalopoulos and Papaioannou 2013), and therefore falls prey to the same issues as Fearon and Laitin (2003), who use per capita income to capture state capacity at the national level. Income captures not only state capacity, but also other factors potentially linked to the outcome of interest (e.g. in the case of conflict: opportunity costs of rebellion (Collier and Hoeffler 2004) or political preferences (Fukuyama 2013, p. 353)). Still others

6. An emergent literature in a similar spirit exploits border discontinuities of historical states to study long-run effects of e.g. state centralization (Dell, Lane, and Querubin 2018; Becker et al. 2016). Other historical measures of statehood, such as Murdock's political centralization of pre-colonial ethnic groups (Murdock 1959), allow researchers to study "stateness" at a global scale. However, although such measures have been found to positively predict contemporary development through institutional persistence (e.g. Gennaioli and Rainer 2007; Michalopoulos and Papaioannou 2013), they lack both precision and generalizability exactly because of the historical dimension.

have used measures of mountainous terrain (Hendrix 2011), road density (Buhaug and Rød 2006), or simply distance to capital (Buhaug 2010). While each measure might correlate with state capacity, they are also at risk of capturing other aspects about local areas; they are in other words noisy and, at best, indirect proxies of state capacity.[7] In addition, it is not clear whether one measure is superior to another, or whether these different proxies complement each other in predicting state capacity. As outlined in the next section, we overcome the limitations of both the survey- and GIS-based approaches by using a survey-based measure of state capacity which we predict and extrapolate using machine-learning models.

3 A new approach to measuring state capacity

In this section, we outline the three steps we take to construct a measure of state capacity at the local level. We combine several features of the approaches discussed in the previous section to overcome the limitations of each individual approach. First, in a similar fashion as Luna and Soifer (2017) and Fergusson, Molina, and Robinson (2020) we estimate an index of perceived local state capacity using data from the Afrobarometer (Afrobarometer Data 2004, 2005). Second, we predict the survey-based measure of state capacity using both local and national-level variables that are likely to affect (or reflect) the ability of states to penetrate civil society and project power across territories. Third, we extrapolate the state capacity index to all 2.5×2.5 arc-minutes grid cells ($\approx 5 \times 5$ kilometers) in Sub-Saharan Africa.

3.1 Estimating an index of state capacity based on survey data

The first step involves the use of survey data to measure local state presence. The use of survey data relies on the assumption that we can use citizens' experiences and perceptions to gauge the local level of state penetration. While respondents are unlikely to be able to directly assess the strength of the state, they are nonetheless the ones who ultimately interact with local institutions and experience the state's presence (or lack thereof). Thus, by asking respondents about their experiences with, e.g., authorities and local institutions,

7. Distance, for instance, has been argued to be related to conflict for other reasons than state capacity (Campante, Do, and Guimaraes 2019), and local road density does not take into account the geographical and infrastructural constraints at other points on the route from the capital to a particular area.

we can utilize these experiences and perceptions to estimate to what extent the state is present at the local level.

We use data from the second and third rounds of the Afrobarometer, comprising 25,103 households.[8] We select these two rounds because they contain survey items that capture dimensions of state capacity we aim to proxy, namely extractive, coercive, and administrative capacity. Extractive capacity refers to the ability to collect revenue, coercive capacity to the enforcement of order, and administrative capacity refers more broadly to the ability to effectively regulate society and deliver public goods and services. In Table 1, we detail the questions and categorize them under the dimension of state capacity which we argue they tap into.

Table 1: Selected survey items

Question	Item	Label	Dimension
How likely do you think it would be that the authorities could enforce the law if a person like yourself: (Very likely; Likely; Not very likely; Not at all likely)	Committed a serious crime?	**Law enforcement**	Coercive
	Did not pay a tax on some of the income they earned?	**Tax enforcement**	Extractive
Based on your experience, how easy or difficult is it to obtain the following services: (Very Easy; Easy; Difficult; Very Difficult)	Help from the police	**Police ability**	Coercive
	An identity document (such as a birth certificate, driver's license, or passport)	**Information capacity**	Administrative
	Household services (like piped water, electricity, or phone)	**Service provision**	Administrative
	A place in primary school for a child	**School provision**	Administrative

The questions labeled 'Law enforcement' and 'Tax enforcement' ask respondents to indicate on an ordinal scale their belief in the ability of the authorities to enforce the law if a person like themselves committed a serious crime or avoided tax payments. These two clearly relate to coercive and extractive capacity, respectively. We include four additional survey items asking respondents to indicate the ease with which a range of services can be obtained. 'Police ability' again taps into the coercive dimension, whereas the remaining three serve to capture the administrative capacity of the state. We include the ease of obtaining identity documents as an indicator of 'Information capacity'. As several scholars have recognized (e.g. Lee and Zhang 2017; Brambor et al. 2020), a core function of the state is the ability

8. In total, there are 46,778 respondents, but 21,675 respondents did not provide complete answers on the survey items and are therefore dropped.

to collect information on its citizens (e.g., in order to tax, enforce the law, etc.). Finally, we include two items, 'Service provision' and 'School provision', which tap into the presence of key state institutions and services, and thereby slightly expand our operationalization of local state capacity.[9]

We opt for a relatively broad understanding of state capacity and therefore consider items tapping into each of the three constitutive dimensions of state capacity. At the conceptual level, the selected survey items link to Mann's (1984) concept of infrastructural power, the ability to penetrate civil society and implement decisions throughout the realm. Without being present in the form of either law enforcement, revenue extraction, information gathering, or provision of public services, the state's ability to penetrate civil society is questionable. Furthermore, in the absence of trust in law and tax enforcement, the state lacks the ability to implement its rules and decisions.[10]

Next, we conduct a factor analysis to estimate an index of state capacity based on the six survey items shown in Table 1. Compared to choosing just one proxy for state capacity, deriving a weighted average of six survey items mitigates noise. Moreover, instead of arbitrarily deciding how the survey items relate to the index, factor analysis optimally assigns weights (factor loadings) such that an item's contribution to the index is based on its correlation with the latent factor.[11] The resulting factor loadings are displayed in Table 2.

9. While there are other survey items that could also proxy for local state presence, these are also likely to reflect more political dynamics. For instance, Wig and Tollefsen (2016) measure local institutional quality (not state capacity) using questions about trust in local politicians, trust in courts, perceived levels of corruption, performance of local politicians, among others. We avoid such questions because these are likely also affected by, e.g., partisan (and potentially also ethnic) identity and regime characteristics. We therefore focus on those questions that more directly ask about respondents' experiences with local institutions rather than their trust or performance evaluations.

10. The inclusion of institutions for public goods provision arguably makes this a relatively broad conceptualization. However, what we are ultimately interested in, and what we are capturing below, is not the quality of such institutions *per se* (cf.), but rather the presence (or absence) of such institutions at the local level, which—all else equal—indicates a generalized territorial reach of the state. As Harbers (2015) also noted, we thus take a more inclusive view of the state, focusing not on whether the central state dominates local territories, "but to what extent effective state structures exist in these areas" (p. 374).

11. We use a principal factor analysis after having residualized the input variables on potentially confounding variation due to spatial imbalances in the data on age, household head, gender, and the round of the Afrobarometer. We employ the polychoric package in Stata, which allows for the use of ordinal-scaled variables in the factor analysis.

Table 2: Factor loadings from Principal Factor Analysis

Variable	Factor loading	Variable	Factor loading
Law enforcement	0.4082	Information capacity	0.4743
Tax enforcement	0.4065	Service provision	0.4694
Police ability	0.4454	School provision	0.4282

We construct an index to proxy state capacity at the enumeration area (EA) level by taking the average of the individual-level perceived state capacity, which we derived from the factor analysis, in each EA. We do this for two reasons, one methodological and one conceptual. First, we aggregate to the EA level to reduce noise in the individual-level data, which is likely to exhibit measurement error. Second, while state presence is local, it is arguably not an individual-level phenomenon. The extent of the state's local infrastructural power should conceptually (and empirically) be the same for all individuals in a given area (neighborhood, village, etc.). While each respondent might have different perceptions and experiences with the local state institutions, they are ultimately referring to the same institutions (hence, the need to also reduce noise), and by aggregating their responses we can gauge the overall level of state presence in that area. In other words, by aggregating to the EA level, we are measuring state presence at the level at which the state is actually projecting power. In order to further mitigate noise, we restrict the sample to include only EAs with five or more respondents with complete answers.[12] The final sample comprises 2,151 EA–year combinations and 1,938 unique EAs in 17 countries.[13] Figure 1 maps the presence of survey EAs and their estimated state capacity index.

We conduct a series of sanity checks of our survey state capacity measure in Appendix Figure A1. The checks are undertaken by splitting the EAs into high and low state capacity using the median value as cutoff, and studying how state capacity relates to variables that convey information about the levels of clientelism, crime, infrastructure, and public trust in institutions. The figure reveals, reassuringly, that EA level state capacity

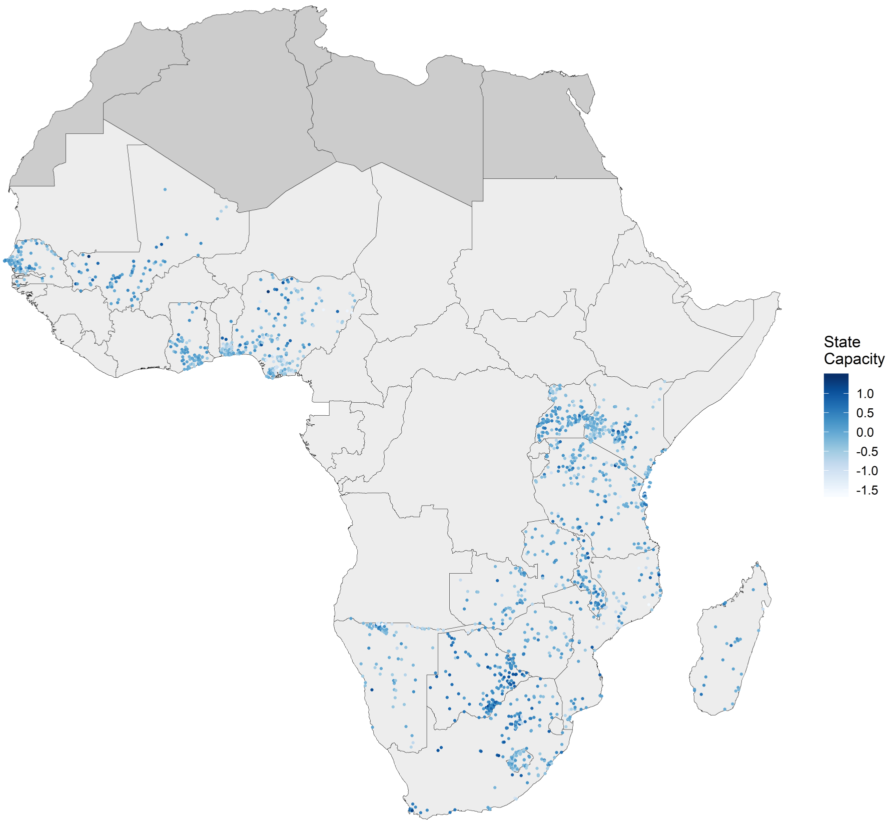
12. While this also limits the number of observations in our training sample, we believe this balances the trade-off between coverage and measurement error.

13. We exclude Cape Verde as a few key predictors are unavailable for island nations.

is associated with less violence, crime, and clientelism. Moreover, it is positively correlated with the presence of publicly provided infrastructure, namely post offices and piped water, but not with the presence of religious buildings, which serves as a form of placebo test. Lastly we show that while survey state capacity links positively with trust in courts and in the ruling party, it exhibits no relationship with trust in the political opposition. This last check serves as another placebo test, in that it shows that trust is not just generally higher in high state capacity areas. Importantly, except for the presence of post offices, all relationships remain unchanged when controlling for whether the EA is urban or rural, fixed effects at the country level, and clustering at the country level.[14]

14. The regression coefficients and p-values are as follows. Vote buying: regression coefficient = 0.120, $N = 1,276$, $p < 0.01$; Feared crime in own home in the past year: regression coefficient = -0.199, $N = 2,099$, $p < 0.01$; Family member physically attacked in the past year: regression coefficient = -0.389, $N = 2,099$, $p < 0.01$; EA has post office: regression coefficient = 0.045, $N = 2,121$, $p = 0.195$; EA has piped water: regression coefficient = 0.045, $N = 2,128$, $p < 0.01$; EA has religious building: regression coefficient = 0.051, $N = 2,124$, $p = 0.184$; Trust in courts: regression coefficient = 0.130, $N = 2,151$, $p < 0.01$; Trust in ruling party: regression coefficient = 0.149, $N = 2,151$, $p < 0.01$; Trust in ruling party: regression coefficient = -0.012, $N = 2,151$, $p = 0.795$.

Figure 1: EA level state capacity



Notes: The map displays EA-level state capacity.

3.2 Predicting the index with machine learning models

Notwithstanding the fact that the Afrobarometer has an exceptional coverage across Africa, for our purposes we need a measure of local state capacity for *all* of Sub-Saharan Africa. The next step is thus to develop a procedure for extrapolating the survey-based measure to areas not covered by the Afrobarometer. While the survey data is limited in coverage, there are numerous structural factors which (1) associate with the state's ability to project power, and (2) are available at fine-grained spatial levels across large territories. We therefore consider an approach in which we, (1) link the survey-based measure of state capacity estimated in the previous section with multiple factors related to state capacity (outlined below), (2) predict the survey-based measure of state capacity using these factors as predictors in an ensemble of machine learning models, and (3) extrapolate the predictions of the model to all of Sub-Saharan Africa.

As a starting point, we rely on the stylized fact that state power radiates outwards from a political center (the capital city) and diminishes as one moves away (Mann 1984; Herbst 2000). This idea can be found in Boulding's concept of a *loss-of-strength gradient* (Boulding 1962), and researchers have attempted to operationalize it using e.g. distance from the capital (Buhaug 2010) and measures of road density (Buhaug and Rød 2006). While these are intuitive proxies, there are arguably more factors that determine local state strength. The projection of state power through space is costly, and rational state-builders will accordingly weigh the costs and benefits of extending power into remote areas (Boone 2012).

To capture sub-national territorial reach of the state, we predict the survey-based measure of state capacity with a set of structural factors. Similar to Boone (2012), we distinguish between two constitutive drivers of state building: the demand for – and cost of – state presence. On the demand side, we posit that the state is more willing to invest in territories of higher revenue potential, e.g. populous and wealthy areas. We therefore include several measures capturing population density and economic activity (as proxied by night light density). On the cost side, we consider several geographical, topological, and infrastructural constraints which may influence the feasibility of state building. The farther away an area is from the capital city and urban centers, the higher the cost for reaching that area. The costs are accentuated by inaccessible terrain such as mountains and dense forests. Finally, local state capacity is contingent upon the capacity of the political center from which power radiates, and we thus include government effectiveness and regulatory quality from the World Governance Indicators (WGI) to capture national level state capacity. Table A1 in the Appendix presents the full list of sub-national level predictors used in the model.

We use a two-step prediction procedure, where we separately predict between- and within-country variation in our measure of state capacity. First, we use a simple linear regression model to predict between-country variation in survey state capacity, using government effectiveness and regulatory quality from WGI as sole predictors (Kaufmann and Kray 2019). Both national-level government effectiveness and regulatory quality are positively and statistically significantly correlated with our measure of state capacity,[15]. Thus,

15. The t-values from simple linear regressions are 4.5 and 2.9, respectively.

ceteris paribus, areas in countries with better government effectiveness and regulatory quality will be assigned higher values of predicted state capacity.

Next, we predict state capacity at the sub-national (EA) level, using all predictors outlined in Table A1. The outcome variable as well as the predictors are country-demeaned in order to capture within-country variation.[16] Using the 'SuperLearner' package in R, we employ an ensemble of machine learning models consisting of a bagging algorithm, a number of random forest algorithms, and several gradient boosting algorithms.[17] The underlying algorithms are run on ten folds and the performance of each algorithm is evaluated based on the accuracy of the predicted outcome in the holdout sample. The higher the performance, the higher the weight the ensemble model assigns the algorithm.[18] The main advantage of this methodology is that the composition of algorithms is determined by their predictive power, a feature which reduces the risk of researcher bias.

The ensemble model gives weight to one random forest algorithm with two predictors in each tree (74 percent) and one gradient boosting model with interaction depth of two and a shrinkage parameter of 0.005 (26 percent).[19] Appendix Figure A2 illustrates the 'importance' of each predictor in one of the algorithms embedded in the ensemble model.[20] Population density and night-time light emissions stand out as the most useful predictors, but travel time to urban center and the capital are also found to be important.

In an out-of-sample testing exercise , Appendix Figure A3 illustrates that the predicted measure strongly correlates with the survey measure the model is trained on, even within countries (*p-value* < 0.0001). As expected, however, the prediction model contains substantial noise, reflecting the difficulties in predicting perceived local state capacity using only satellite data as inputs. Appendix Figure A4 examines the bivariate within-country

16. We use both absolute and standardized deviations from the country means for the predictors, as it is not obvious a priori whether the functional forms are better approximated by absolute or relative measures of the predictors.

17. Appendix 7 briefly introduces each of the non-parametric algorithms. For a more elaborate discussion on regression trees, see James et al. (2013).

18. The model further penalizes the weights of algorithms producing (close to) identical predictions.

19. For each algorithm embedded in the ensemble model, we restrict end nodes to have at least 25 observations.

20. In random forest one can evaluate the importance of each explanatory variable. This is done by separately permuting each variable and deriving the mean squared error (MSE). This MSE is compared to the MSE from the baseline non-permuted model. The higher the increase in MSE from permuting a variable, the more important the predictor is.

relationships between the predicted measure of state capacity and selected explanatory variables. In line with expectations, the predicted state capacity is negatively correlated with travel time to the capital, travel speed to the capital, and forest cover. Conversely, predicted state capacity is positively correlated with night-time light emissions and population density.

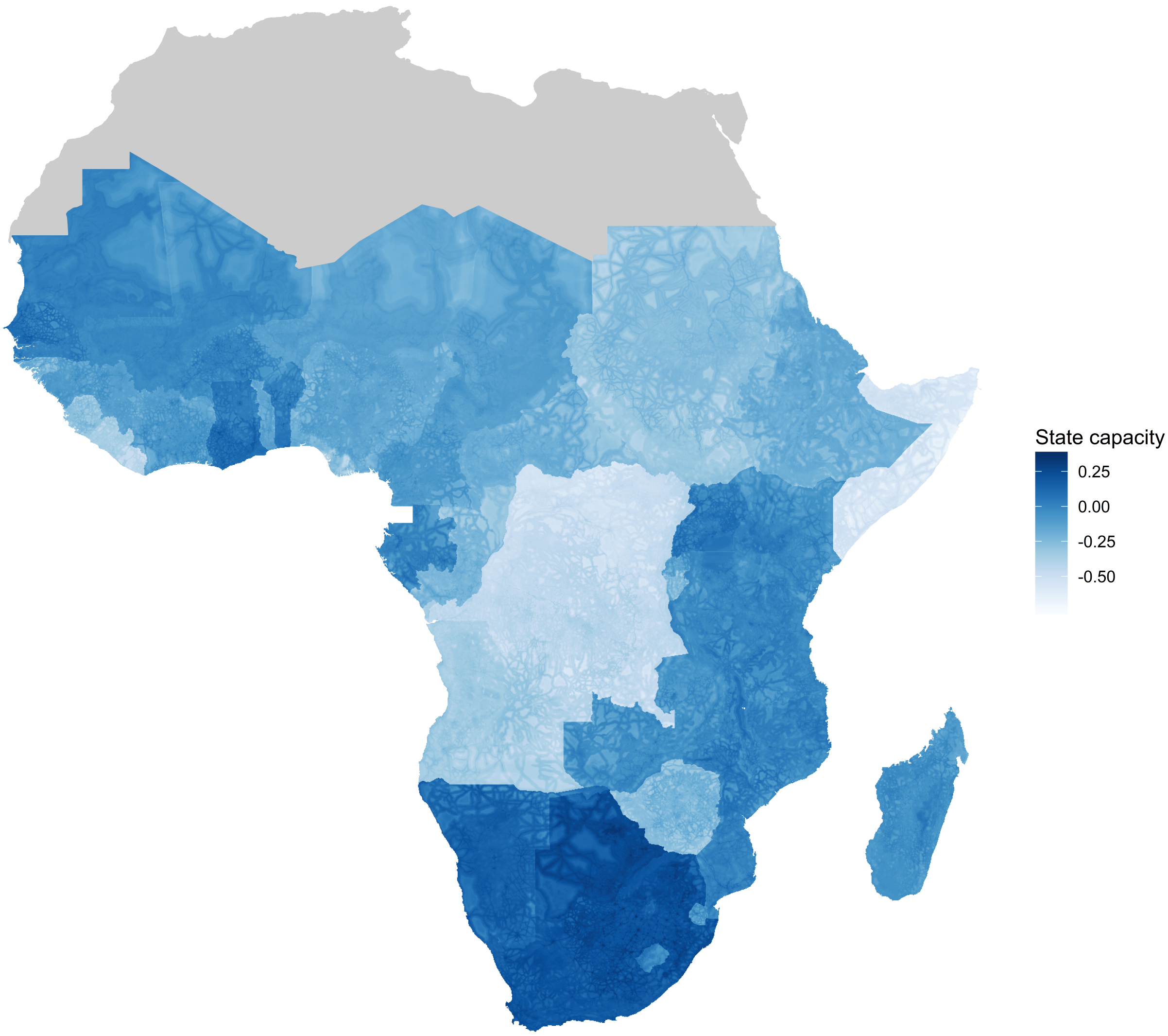
Finally, the between- and within-country predictions are added. Separately predicting between- and within-country state capacity allows us to overcome the problem of overfitting endemic to machine learning models. Furthermore, by linearly extrapolating our predicted state capacity index, we can meaningfully assign values to areas in countries outside common support of the survey sample. The disadvantage of imposing additive separability is that we do not account for potential interactions between local predictors and the baseline capacity of the state.

3.3 Extrapolation to non-surveyed cells

Having calibrated the prediction model, we proceed to extrapolate the prediction model to 2.5×2.5 arc-minutes grid cell level ($\approx 5 \times 5$ kilometers). The extrapolation is based on grid-cell data on the predictors we use in the ensemble model. Since the data are from the year 2000 or earlier (see Table A1), we label the resulting proxy *state capacity in year 2000*. We thereby obtain a spatially fine-grained measure of state capacity for all of Sub-Saharan Africa (1,166,249 grid cells). Figure 2 maps the predicted measure of local state capacity, where dark blue represents higher predicted state capacity.

Figure 2 reveals a few patterns worth highlighting. First, local state capacity is highly clustered at the national level. Among the countries with low state capacity, we find Liberia, Somalia, Zimbabwe, as well as The Democratic Republic of the Congo (DRC). On the other end, Botswana and Ghana, countries that – relative to the rest of the region – have enjoyed political stability (Naudé 2013; Acemoglu, Johnson, and Robinson 2002) display higher average state capacity. Second, while predicted state capacity is clustered at the national level, it also features substantial within-country variation: the map depicts networks and clusters representing e.g. infrastructure, border regions and economic and political centers. Next, we link our measure of local state capacity to factors which should co-vary with state capacity, namely ethnic power relations, pre-colonial centralization and

Figure 2: Predicted measure of local state capacity in year 2000



Notes: The map illustrates the predicted measure of state capacity in year 2000 at the 2.5×2.5 arc-minutes grid cell level ($\approx 5 \times 5$ kilometers). Grey cells represent excluded countries from North Africa. Equatorial Guinea is excluded due to missing *travel time to the capital*, as the capital is located on an island in the Gulf of Guinea, meaning we cannot predict within-country state capacity.

vaccination coverage. We demonstrate the validity of the machine learning predictions by correlating our state capacity measure with these factors.

4 Validating the measure of local state capacity

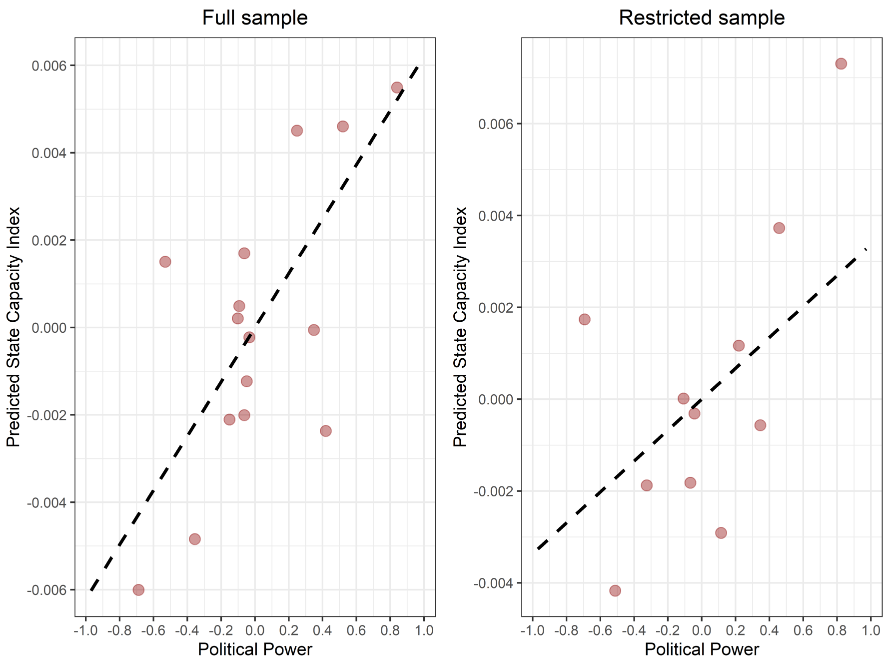
4.1 Ethnic Power Relations

Ethnic favoritism characterizes the politics of many African countries (Dickens 2018). For instance in Kenya, the ruling ethnic group has been shown to disproportionately allocate road investments to their home regions (Burgess et al. 2015). If similar patterns exist for the continent at large, we should observe higher levels of state capacity in territories inhabited by politically powerful groups. We test this prediction by linking our measure of state capacity with the GeoEPR dataset (Vogt et al. 2015).[21] The GeoEPR contains geocoded data on the political power of all African ethnic groups linked with a specific territory.[22] Out of the 662 ethnic groups contained in the data, we code 151 groups – labeled "Dominating", "Monopoly" or "Senior Partner" (in the year 2000) – as politically powerful (1) and the rest as powerless (0). Finally, in Figure 3 we correlate the dummy for political power with the predicted state capacity index, separately for the "full sample" using all grid cells, and the "restricted sample" which includes only grid cells from countries *not* covered by the Afrobarometer.

21. We spatially merge the EPR-polygons with our state capacity measure at the 2.5×2.5 arc-minutes grid cell level.

22. The GeoEPR measure of political power is a categorical variable taking one of the following labels: "Dominating", "Monopoly", "Senior Partner", "Junior Partner", "Irrelevant", "Discriminated", "Powerless", "Self-excluded", and "State Collapse".

Figure 3: Correlation between local state capacity and ethnic political power



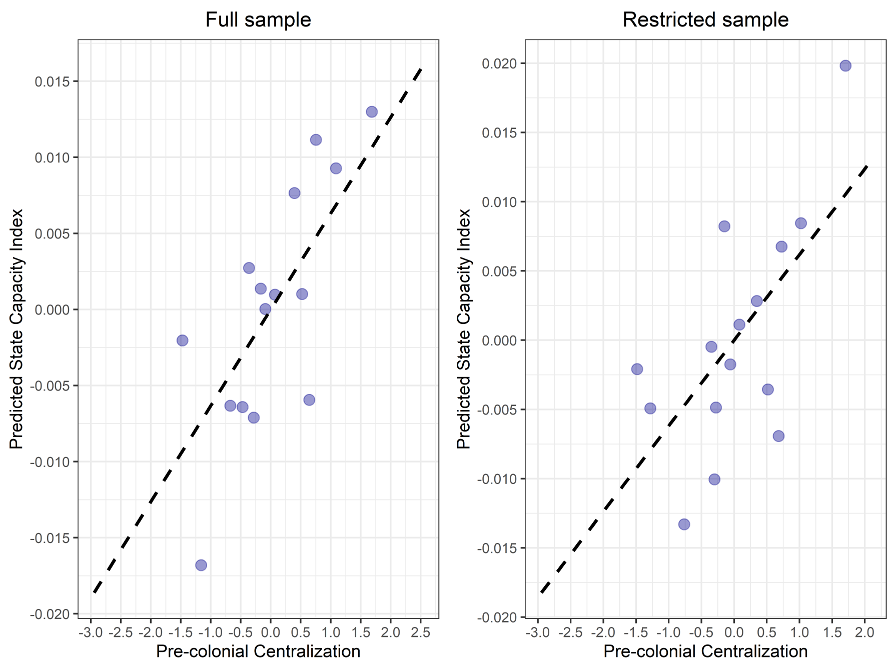
Notes: The figure displays bin-scattered relationships between ethnic political power and, respectively, state capacity in the full sample (left) and the restricted sample with only non-surveyed countries (right). The variables are demeaned at the country level in order to depict within-country variation.

Figure 3 depicts within-country correlations between political power and our measure of local state capacity in binscatter plots. Political power is positively associated with our measure of local state capacity (Pearson correlation coefficient $= 0.055$, $N = 1,150,206$, $p < 0.01$). Importantly, the relationship between political power and predicted state capacity is positively correlated also when considering only countries that *were not* surveyed by the Afrobarometer (Pearson correlation coefficient $= 0.03$, $N = 644,440$, $p < 0.01$). This shows that the predicted measure of local state capacity carries relevance also in countries outside of common support. Country fixed effects regression estimates with standard errors clustered at the ethnic group level, however, indicate that the correlations are relatively weak, with p-values of 0.12 and 0.34 for the full sample and restricted sample, respectively.

4.2 Pre-colonial centralization

According to numerous scholars, state institutions tend to persist over time (see e.g. Michalopoulos and Papaioannou 2013; Acemoglu, García-Jimeno, and Robinson 2015). We should thus observe a positive correlation between our measure of state capacity and the degree of political centralization in pre-colonial times. Pre-colonial centralization[23] at the society level was coded in the Ethnographic Atlas by Murdock (1959) according to the following categories: "No levels (no political authority beyond community)", "One level (e.g., petty chiefdoms)", "Two levels (e.g., larger chiefdoms)", "Three levels (e.g., states)", and "Four levels (e.g., large states)". We create an index of pre-colonial centralization[24] at the 2.5×2.5 arc-minutes grid cell level, by overlaying our measure of local state capacity with geocoded societies from the Ethnographic Atlas (Michalopoulos and Papaioannou 2013).

Figure 4: Correlation between local state capacity and pre-colonial centralization



Notes: The figure displays bin-scattered relationships between pre-colonial centralization and, respectively, the full sample of state capacity (left) and the restricted sample with only non-surveyed countries (right). The variables are demeaned at the country level in order to depict within-country variation.

23. The variable is more precisely defined as "Jurisdictional Hierarchy Beyond Local Community", and contains information on centralization for 843 pre-colonial societies in Africa.

24. The index is used as a continuous variable that ranges from 1 (no political authority beyond community) to 5 (large states).

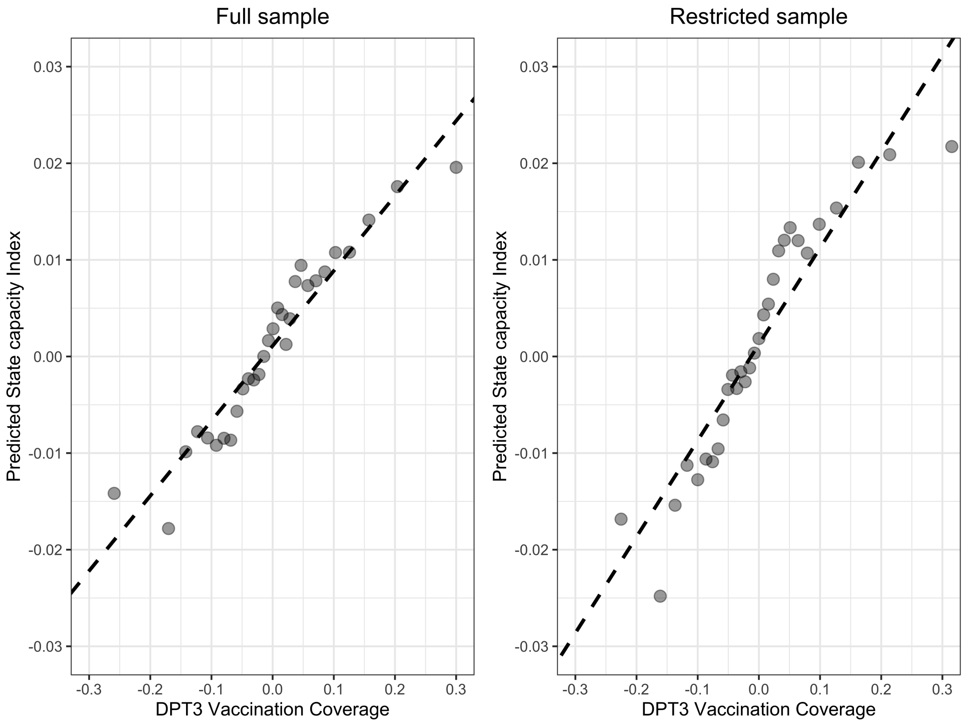
Figure 4 shows how our measure of state capacity correlates with pre-colonial centralization within countries. The correlation is positive and significant for the full sample (Pearson correlation coefficient = 0.120, $N = 722,945$, $p < 0.01$) as well as when focusing solely on countries not covered by the Afrobarometer (Pearson correlation coefficient = 0.123, $N = 370,784$, $p < 0.01$). The strong association between predicted state capacity and pre-colonial centralization is confirmed in country fixed effects regressions with standard errors clustered at the pre-colonial society level. The regression estimates are statistically significant both for the full sample including all grid cells intersecting with a pre-colonial society ($p < 0.01$) and for the restricted sample ($p < 0.05$).

4.3 Local vaccination coverage

For the final validation exercise, we compare our predicted values of state capacity to data on local vaccination coverage. Vaccinations are generally considered one of the most cost-effective methods of improving public health. Yet, vaccine-preventable diseases remain a major cause of mortality (Mosser et al. 2019), partly because of the lack of capable state institutions to administer and record vaccination programs. Arguably, the presence of the state is a minimal condition for effective vaccination programs. As such, vaccination coverage is a useful proxy for the state's administrative capacity. This has been argued e.g. by Soifer (2012) who uses vaccination rates to capture the administrative capacity of Latin American states.

Data on vaccination rates are typically only available at the national (sometimes also the administrative unit) level, which limits the usefulness as proxy for local variations in state capacity. However, Mosser et al. (2019) recently published data on local diphteria-pertussis-tetanus (DPT) vaccine coverage of children aged 12–23 months in Africa. The DPT vaccine is included in the WHO's Expanded Programme on Immunisation (EPI), a standardized vaccination programme designed to increase childhood vaccinations throughout the world, and DPT coverage is widely used to measure the performance of routine vaccine delivery systems. Similar to our approach, they use a Bayesian geostatistical model based on survey data and a suite of spatial covariates to estimate annual (2000–2016) local DPT vaccine coverage at a high spatial resolution (5×5 km). We use estimates for completion of three doses of the DPT vaccine (DPT3) in the year 2000 to proxy for local state reach.[25] The spatial resolution matches our predicted measure of state capacity and are therefore readily comparable.[26] In line with the above approach, we further demean the values using the country mean to isolate the within-country variation.

Figure 5: Correlation between local DPT3 vaccination coverage and predicted state capacity



Notes: The figure displays bin-scattered relationships between local DPT3 vaccination coverage and predicted state capacity. The left panel shows the scatterplot for the full sample of Sub-Saharan Africa. The right panel includes only Sub-Saharan African countries not included in the Afrobarometer survey. The variables are demeaned at the country level in order to depict within-country variation.

Figure 5 shows the binned scatterplot of DPT3 vaccination coverage and our predicted measure of local state capacity. The left panel includes the full sample of Sub-Saharan Africa, whereas the right panel includes only countries not covered by the Afrobarometer survey data. As both figures illustrate, predicted state capacity correlates positively with

25. Completion of the initial DPT vaccination routine requires three doses (DPT3), but not all children complete the vaccine. DPT3 coverage is therefore more demanding in terms of state capacity than the initial dose (DPT1).

26. The raster grids do not share the same origin, i.e. they do not align perfectly. The DPT3 raster is therefore resampled using the `Raster` package in R, which uses bilinear interpolation to match values to the shifted grid cells.

local DPT3 coverage, further corroborating our measure of local state capacity. The within-country Pearson correlation for the full sample is 0.20 ($N = 923,848$, $p < 0.01$) and for the non-survey sample it is 0.24 ($N = 526,297$, $p < 0.01$). Country fixed effects regression estimates with standard errors clustered at the country level yield significant coefficient estimates both for the full sample of cells ($p < 0.01$) and for the restricted sample including only cells in non-surveyed countries ($p < 0.01$). Thus, the results lend further credibility to our predicted measure of local state capacity.

5 State capacity and oil conflicts across space and time

5.1 Background

In this section, we turn to an application of our local state capacity measure by investigating its potentially moderating role in the oil wealth–conflict relationship. Oil, just like other extractive resources, has been found to increase the risk of conflict in many contexts (Dube and Vargas 2013; Berman et al. 2017; Nwokolo 2018; Nordvik 2019). One mechanism that has been proposed to explain this link is the so-called greed channel, according to which the concentrated and seizable value of oil encourages armed groups to engage in both extortion and resource grab of oil assets. A second mechanism, the grievances channel, emphasizes the perceived injustices of the unequal distribution of oil rents as a driver of unrest in the form of protests, riots and sabotage, often directed toward states or directly at oil companies. But oil rents can also reduce the risk of conflict, through what Bazzi and Blattman (2014) label the state capacity effect: the use of revenues to invest in repressive capacity or to buy off potential insurgents (see also (Ross 2012)). However, due to corruption and extortion, oil rents may also weaken state building efforts by deteriorating capacity and eroding public confidence in state institutions.

While most studies find that increased oil wealth leads to higher risks of conflict, a few studies suggest that oil wealth has no impact and that it can even decrease the risk of conflict. Bazzi and Blattman (2014) use cross-country panel data to estimate the association between changes in commodity prices and the onset, intensity, and ending of armed conflicts.

The authors find no effect of current oil prices and a negative effect of lagged oil prices on conflict incidence. In a panel fixed effects model with countries as the unit of observation, Cotet and Tsui (2013) find no significant effect of oil discovery on conflict onset, conditional on oil exploration.

The somewhat inconsistent findings in the oil–conflict literature is partly due to a failure to measure relevant conflict events at appropriate geographical units. By focusing on all types of conflicts, and at coarse levels of spatial aggregation (often at the country level), many previous empirical studies present estimates that suffer from attenuation bias. Another reason why the estimated effect of oil wealth varies between studies may simply reflect the contextual nature of the effect of oil wealth on conflict. Contextual factors such as country institutions (Buhaug 2010; Berman et al. 2017), companies' Corporate Social Responsibility (CSR) commitments (Berman et al. 2017), the spatial distribution of natural resources (Morelli and Rohner 2015; Lessmann and Steinkraus 2019) and redistributive government transfers (Justino 2011; Justino and Martorano 2018; Cordella and Onder 2020) have been found to moderate the risk of conflict.

In what follows, we investigate the role of local state capacity as a moderating factor in the oil–conflict relationship. We hypothesize that local state capacity decreases the risk of oil-induced conflict by reducing the incentives of confrontation. Higher levels of state capacity implies higher extractive[27], coercive[28], and administrative capacity[29], factors which jointly determine states' ability to avoid and, if need be, repress conflict.

5.2 Empirical approach

In order to investigate how state capacity conditions the risk of conflict, we create a panel dataset on state capacity, oil wealth, and oil-induced conflicts for Sub-Saharan Africa in 2001-2019. First, we aggregate our predicted state capacity index in year 2000 to 0.5×0.5 degree grid cells ($\approx 55 \times 55$ kilometers). We then link the index with data on oil deposits in year 2000, and yearly data on oil prices and oil-induced conflicts in 2001-2019. The

27. Our survey state capacity index contains this dimension in the survey item Tax enforcement.
28. Captured by the survey items Law enforcement and Police ability in the survey state capacity measure.
29. This dimension is represented in our survey state capacity measure by the survey items Information capacity, Service provision and School provision.

interaction between oil deposits in year 2000 and the oil price constitutes our exogenous spatio-temporal measure of oil wealth. Higher oil prices means that oil deposits become relatively more valuable, which could activate the greed or grievances channel and trigger conflict. The panel structure allows us to estimate two-way fixed effects models to explore how local state capacity moderates the risk of oil induced conflict while holding constant potentially confounding variation at both the cell and the year level.

5.3 Data

Our measure of local state capacity in the empirical analysis is the measure we construct and present in Section 3. To ease the interpretation of the results in the empirical analyses, we standardize the state capacity measure and add the minimum value such that the lowest possible value is 0 and a unit equals one standard deviation.

The data on geocoded petroleum deposits is obtained from PRIO-GRID data 2.0 (Lujala, Rod, and Thieme 2007). Since conflict, and even the risk of conflict, could affect hydrocarbon exploration and production decisions, endogeneity is a first-order concern that we need to circumvent. In order to do so, we define cells as oil cells if they contained onshore oil resources in year 2000.[30] Next, we create a time- and space-varying measure of oil wealth by interacting the spatial indicator of oil deposits with the log of the average yearly international oil price.[31]

To measure oil-related conflict events at the grid cell level, we obtain geocoded conflict data from the Armed Conflict Location and Event dataset (ACLED) (Raleigh et al. 2010). ACLED provides information on the location and exact date as well as the nature of conflict events. The data builds on information compiled by a range of stakeholders, including news agencies, researchers, and humanitarian organizations. Although the ACLED data may contain measurement error due to inaccurate reporting, and the frequencies of misreporting potentially differ between high and low state capacity regions, such inconsistencies would be

30. We combine the variables *petroleum_s* and *petroleum_y* in year 2000 from PRIO-GRID 2.0 into one indicator, since the combination of both constructs a measure on “known petroleum deposits in year 2000”, which is the most suitable definition of an oil cell for our purposes.

31. The oil price is measured as an equally weighed average spot price of Brent, Dubai and West Texas Intermediate crude oil prices. The data is obtained from www.indexmundi.com.

accounted for by our year and cell fixed effects. We exploit the detailed information provided by ACLED to create measures of conflict that are closely linked with our hypothesis. To begin with, we classify oil-related conflict events based on qualitative information on the recorded events. Specifically, we label an event 'oil-related' if it contains the words "oil" or "petroleum".[32] By applying a narrow definition of oil conflict, we are able to estimate the role of state capacity in mitigating the risk of oil-induced conflict with better precision. Moreover, we define two subsets of oil conflicts to allow for suggestive tests of the mechanisms linking oil wealth to conflict, and to study the moderating role of state capacity for respective channel. First, we focus on violent oil-related conflict events,[33] which potentially relate to contests over oil rents, in order to test for the greed channel. Second, by using oil events classified by ACLED as either riots or protests as our dependent variable, we are able to study civilian actions related to oil grievances.

The ACLED dataset for Sub-Saharan Africa from January 1st 2001 to December 31st 2019 includes 767 geocoded oil-related conflict events. Out of these, 404 are coded as "violent events" and 313 are labeled "demonstrations".[34] In Figure 6, we map oil deposits, state capacity, and conflict events at the cell level. The vast majority of oil-related conflict events are in, or in close proximity to, oil cells, which provides a simple sanity check of our coding of oil conflicts.

5.4 Empirical strategy

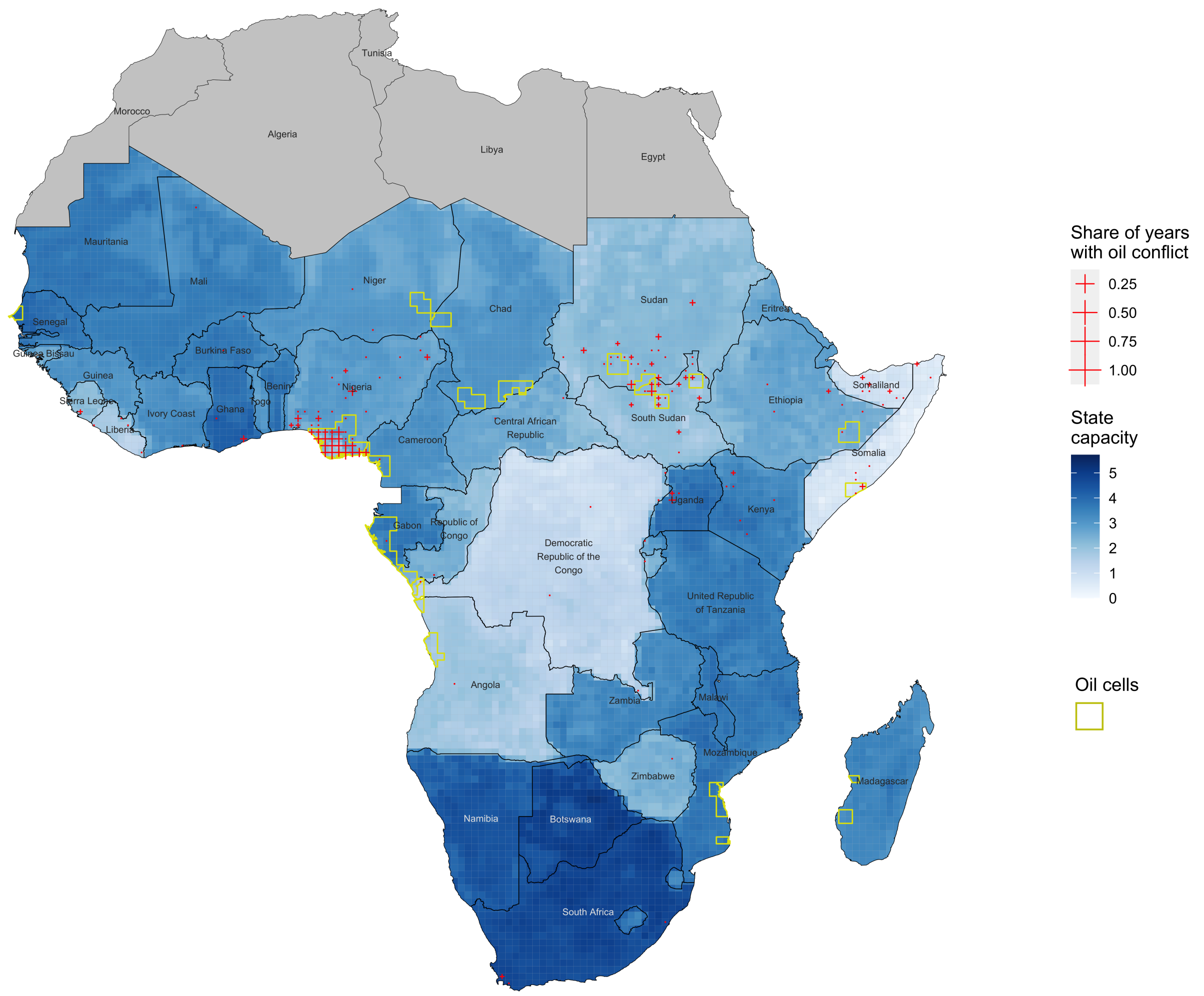
In order to abstract from time- and space-confounding variation, we include both year and cell fixed effects in all regressions, such that we essentially estimate difference-in-difference models. The key identifying assumption is that international oil prices do not respond to conflict events contained in our sample. Since oil production in Sub-Saharan Africa constituted only 7.3 percent of world output in 2008 and 5.6 percent in 2018 (Dudley 2019), this appears to be a reasonable assumption. Our empirical estimations all build on variants

32. We further label two special cases that include the strings "right-to-oil" and "gas-and-oil" as oil-related, and we label conflict events with the word combinations "palm oil", "cooking oil" and "oil mill" as non oil-related.

33. This subset of conflict events encompasses battles, violence against civilians, and explosions/remote violence.

34. The remaining 60 oil-related events encompass non-violent actions included in the ACLED data.

Figure 6: State capacity, oil cells, and oil conflict events in Sub-Saharan Africa 2001-2019



Notes: The map plots oil deposits and oil conflict data onto our grid cell state capacity measure. The state capacity index is standardized and re-scaled to have a minimum of zero for the empirical analysis.

of the following model:

$$\begin{aligned} Oil\_Conflict_{it} = &\beta_0 + \beta_1 Oilprice_t \times Oilcell_i + \beta_2 Oilprice_t \times SC_i + \\ &\beta_3 Oilprice_t \times Oilcell_i \times SC_i + \gamma_i + \delta_t + \epsilon_{it}, \end{aligned} \tag{1}$$

where the outcome variable indicates whether there was an oil-related conflict event in cell i in year t; *Oilprice* is the international oil price; *Oilcell* indicates whether the cell had known oil deposits in year 2000; SC is the predicted measure of state capacity; γ_i and δ_t are cell and year fixed effects, respectively; and ϵ is the error term. The triple interaction term captures the role of state capacity in moderating the effect of oil-induced conflicts.

In an extension of the baseline model, we consider the contagious nature of conflict across space and time. In oil cells, the likelihood of oil conflict is 5.2 percent if there was no oil conflict in the past year, but 17.1 percent if there was. Similarly, while the risk of oil conflict in oil cells is 5.3 percent if no neighboring cells experience oil conflict that year, the risk of conflict is 8.4 percent if at least one neighbor recorded an oil conflict. We account for the auto-correlation and spatial spillovers by including temporal and spatial lags of the outcome variable. Recognizing that standard errors may correlate across space and time, we accordingly cluster them at the country level. This specification allows standard errors to correlate both across large territories and over the entire sample period, an approach that generally increases the estimated standard errors relative to clustering of standard errors at more limited spatial or temporal levels.[35]

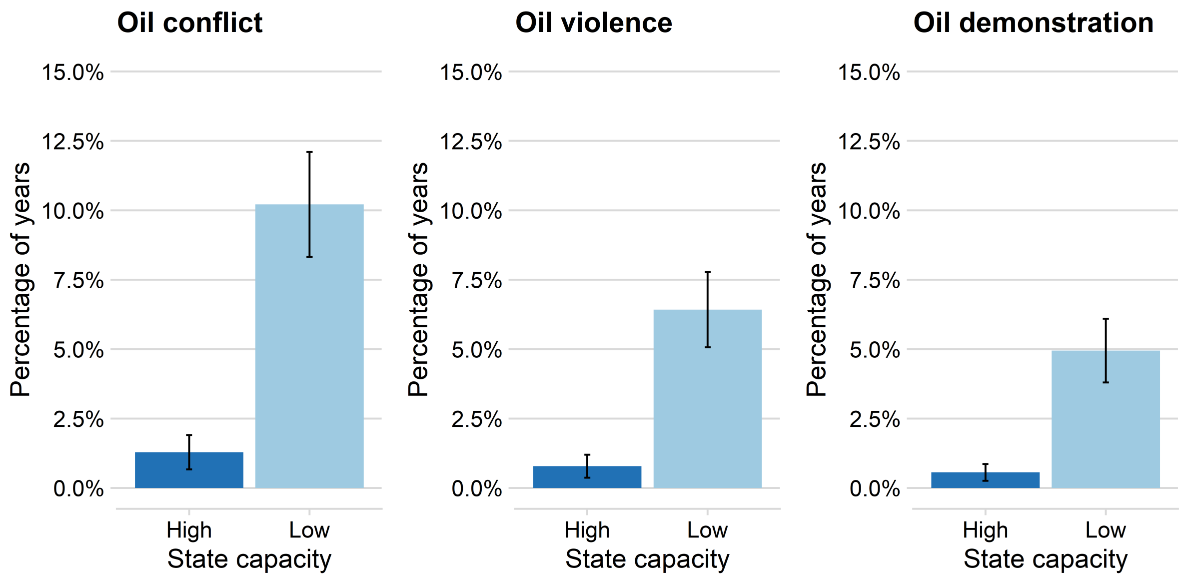
We further consider a continuous measure of "oil deposits" by instead of using a dummy indicator for oil cell, we use the inverse (square root) distance to an oil cell (labelled *oil proximity*). Lastly, we present a placebo test by substituting current- with lead oil prices. While auto-correlation in prices may cause the lead prices to pick up an effect associated with current prices, the precision ought to be smaller, and hence we expect the significance to drop.

5.5 Results

We now turn to the empirical link between oil wealth and conflict, and the role of local state capacity as a moderating factor in this relationship. First, we show in Figure 7 how the risk of oil-related conflict events differ for high and low state capacity cells. The risks of oil-induced conflict, violent oil events, and oil demonstrations, are substantially higher in oil cells with low state capacity compared to cells with high state capacity. The difference is significant and of a magnitude of 10. The effect remains significant when including country fixed effects, showing that our measure of local state capacity matters also for the spatial distribution of oil conflicts *within* countries (p-*value* < 0.05). While these results indicate a moderating role of state capacity, the effect is not necessarily causal. In order to test if oil wealth induces

35. Indeed, the estimated standard errors are significantly smaller if we cluster instead at the cell or at the country–year level (results not reported).

Figure 7: The frequency of oil-related conflicts and battles in oil cells by level of state capacity



Notes: The figure plots the share of years that oil cells experience oil-related conflicts (left), oil-related violent events (middle), and oil-related demonstrations (right). In all three plots, oil cells are divided into high and low state capacity based on the median predicted state capacity in year 2000.

oil-related conflict, and if this effect is conditional on the level of state capacity, we estimate panel regressions with our indicator variables for oil conflict as the outcome variables, and oil price, oil deposits, and state capacity as the explanatory variables. The results are displayed in Table 3.

Table 3: The conditional effect of oil price fluctuations on the likelihood of oil conflict 2001–2019

	Oil event (1)–(3)			Oil violence (4)–(6)			Oil demonstration (7)–(9)		
	Low SC (1)	High SC (2)	Interaction (3)	Low SC (4)	High SC (5)	Interaction (6)	Low SC (7)	High SC (8)	Interaction (9)
Panel A:									
Oil price × *Oil cell*	0.039**	-0.0014*	0.053**	0.025**	0.00003	0.038*	0.014*	-0.0014*	0.016*
	(0.017)	(0.00076)	(0.024)	(0.011)	(0.00015)	(0.020)	(0.0068)	(0.00068)	(0.0080)
Oil price × *State capacity*			-0.00045			-0.00039*			-0.00005
			(0.00031)			(0.00020)			(0.00013)
Oil price × *State capacity* × *Oil cell*			-0.011*			-0.0085*			-0.0028
			(0.0058)			(0.0050)			(0.0022)
R-squared	.317	.128	.292	.251	.065	.244	.234	.130	.211
Observations	80,142	79,591	159,733	80,142	79,591	159,733	80,142	79,591	159,733
Panel B:									
Oil price × *Oil cell*	0.035**	-0.0013*	0.048**	0.022**	0.00005	0.035*	0.011**	-0.0014*	0.012**
	(0.015)	(0.00075)	(0.022)	(0.0095)	(0.00021)	(0.018)	(0.0051)	(0.00067)	(0.0059)
Oil price × *State capacity*			-0.00036			-0.00030*			-0.00004
			(0.00024)			(0.00015)			(0.00010)
Oil price × *State capacity* × *Oil cell*			-0.0099*			-0.0079*			-0.0019
			(0.0053)			(0.0046)			(0.0019)
Dependent variable temporal lag	0.078***	-0.033	0.062***	0.052**	-0.070***	0.047**	0.034	-0.036	0.019
	(0.014)	(0.028)	(0.015)	(0.022)	(0.0086)	(0.020)	(0.029)	(0.037)	(0.035)
Dependent variable spatial lag	0.028***	0.0022	0.022***	0.027***	0.0058	0.025***	0.054***	-0.0019	0.036***
	(0.0065)	(0.0062)	(0.0058)	(0.0078)	(0.0091)	(0.0074)	(0.011)	(0.0087)	(0.012)
R-squared	.322	.129	.295	.253	.069	.246	.235	.131	.211
Observations	80,142	79,591	159,733	80,142	79,591	159,733	80,142	79,591	159,733
Cell & year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent variable mean	.0044	.00067	.0025	.0027	.00011	.0014	.0019	.00053	.0012
Dependent variable std. dev.	.066	.026	.050	.052	.011	.038	.043	.023	.035

Notes: The dependent variables in columns (1)-(3), (4)-(6), and (7)-(9) are cell-year indicator variables for 'oil-related conflict event', 'violent oil-related conflict event' and 'oil-related demonstration', respectively. Columns (1), (4), and (7) restrict the sample to cells below the median state capacity, whereas columns (2), (5), and (8) restrict the sample to cells above median state capacity. Columns (3), (6), and (9) display results from models including the continuous measure of local state capacity in a triple interaction with oil cell and the oil price. *Oil price* is the log of the average oil price for each year, *Oil cell* is an indicator variable on known oil deposits in year 2000, and *State capacity* refers to our measure of local state capacity in year 2000. Standard errors clustered at the country level are in parentheses. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

In Panel A columns 1 and 2 of Table 3, we investigate how increased oil prices differentially impact the risk of conflict in oil cells with low and high levels of state capacity. The results depict an interesting pattern. While the risk of oil-related conflict increases with oil prices in low state capacity cells, the impact in high state capacity oil cells is negative. In column 3 we show the effect for the full sample, where state capacity is added as a continuous variable in a triple interaction term with oil cell and oil prices. The results corroborate the insights from columns 1 and 2. If oil prices increase by 25 percent year to year, the likelihood of oil-related conflict increases by 0.5 percentage points ($0.053 \times log(1.25)$) for oil cells with the lowest level of state capacity (where state capacity $= 0$). This is a relatively large increase, since the share of years with oil-related conflict in oil cells is 5.9 percent. The effect is muted in cells with higher levels of state capacity. For a cell with state capacity at the 75th percentile (3.6 standard deviations higher than the minimum state capacity), a 25 percent year to year oil price hike is estimated to increase the risk of oil-related conflict in oil cells by 0.1 percentage points.[36]

Next, we investigate whether the effect of oil price shocks differ for violent oil events and oil-related demonstrations. In columns 4-6, we display the regression models with a dummy indicator for oil-related violent events, e.g. battles and remote violence, as the dependent variable. The effects resemble the ones obtained when using the broad measure of oil conflicts: oil price hikes increase the risk of violent oil events in low state capacity cells, but the risk is moderated by state capacity. In fact, for oil cells with higher than median state capacity, there is virtually no effect of oil price shocks on the likelihood of experiencing a violent oil event. A similar pattern is apparent also for oil demonstrations. Increased oil prices positively affect the risk of oil-related demonstrations in low state capacity oil cells, whereas in high state capacity cells the effect is the reverse. Although the triple interaction is insignificant for this subset of oil-related conflict events, the main takeaway from the panel models is that oil price shocks increase the risk of oil-related conflicts, but only in oil cells with low local state capacity. The results support both greed and grievances as underlying mechanisms linking oil price shocks to conflict, and suggest a role of local state capacity in counteracting both channels.

36. $0.053 \times log(1.25) + (-0.00045 \times log(1.25)) \times 3.6 + (-0.011 \times log(1.25)) \times 3.6$.

In Panel B of Table 3, we explore the robustness of the baseline findings to accounting for both time and spatial spillovers. As we saw in SubSection 5.4, oil conflicts tend to persist over time, and the risk of oil conflict is associated with oil conflicts in the adjacent area.[37] As expected, the year and space lags are positively correlated with the dependent variable in columns 1, 4, and 7, where we consider only low state capacity cells. Strikingly, this is not the case for high state capacity cells (columns 2, 5 and 8), which indicates that local state capacity may mitigate the risk of conflict persisting over time as well as spreading across territories. Importantly, when we include the year- and spatial lags, the baseline results remain qualitatively unchanged. Even though this exercise arguably entails "over-controlling", the stability of the coefficient estimates is reassuring.

Next, we consider a few alterations to our preferred specification. First, in Table A2 in the Appendix we present results using lead and lagged – instead of current – oil prices in the regression models. The only way that lead prices could affect conflict is through its correlation with present oil prices. Lagged oil prices, on the other hand, convey information that may matter for present conflict, for instance if greed and grievances build up over time. Accordingly, in Appendix Table A2 we show that while lead prices induce no significant effects, lagged oil prices display a similar impact on the risk of oil conflict as current oil prices.

Finally, we use the inverse (square root) distance to oil cells as indicator of oil deposits. By operationalizing oil deposits as a value that dissipates with distance to oil, we capture potential spillovers in oil wealth, e.g. due to transport networks. This exercise, shown in Appendix Table A3, does not impact the results substantially, but improves the precision of the estimates somewhat. We also consider a more restrictive model by including country–year fixed effects. Even this alteration to our baseline specification does not impact the estimates appreciably, further lending support to the notion that state capacity moderates the risk of oil induced conflict.

37. Since conflict in neighboring cells may be jointly determined, this positive correlation does not necessarily imply a causal link.

6 Discussion

A number of limitations, both with regards to the construction of our local state capacity measure, and with regards to the empirical application, are worth highlighting. First, the prediction model is somewhat limited by lack of common support, since the Afrobarmoter does not cover all countries in Sub-Saharan Africa. Nor does the survey data cover comprehensively all types of within-country territories. For instance, extreme altitude areas, which feature in the data that we extrapolate to but not in the survey, are assigned the same values as the highest-altitude areas contained in the prediction model. Despite the limitations imposed by lack of common support, we show in Section 4 that our measure of local state capacity is meaningful also in countries not covered by the Afrobarometer.

Second, the prediction strategy relies on the assumption of additive separability between national and sub-national predictors. That is, the strategy does not allow for potential interactions between predictors at the local and national levels. This strong assumption is necessary for our purposes, as including national-level predictors in the machine learning model essentially corresponds to including country fixed effects, and thereby increases the risk of overfitting. Separating our prediction model in two steps allows us to overcome this issue, while still considering between-country variation.

Third, our prediction model incorporates determinants that may moderate the risk of oil-induced conflict for reasons other than local state capacity. For instance, we are not able to abstract from the potentially confounding covariance between local state capacity and local income levels, and as a consequence we cannot rule out that the results presented in Section 5 are affected by confounding elements. Fourth, as a time-invariant measure of state capacity based on data from the year 2000, our measure does not account for the changes in state capacity that occurred during the study period. While our prediction methodology enables us to construct a panel dataset of local state capacity, the usefulness of such a measure, at least for the present purposes, is not obvious. The improved precision of local state capacity would come at the price of endogenous spatio-temporal variation, an issue we mitigate by using a pre-determined variable of state capacity. Fifth, measuring only oil-induced conflicts entails both methodological advantages and disadvantages. The increased precision enables us to reduce noise in the estimates, but it also reduces the number of data points that convey information.[38]

7 Conclusion

The conceptualization of state capacity is ever contested and to date there exists no universal definition of the term. While scholars disagree about what exactly state capacity encompasses, there is no controversy regarding the centrality of state capacity in shaping economic, social, and political life. Variability in states' ability to project power across territories, especially for developing countries, highlight the need to measure state capacity at a local level. But due to the data scarce nature of these contexts, doing so has proven challenging.

In this chapter, we have showcased a novel methodology to measure state capacity at a spatially disaggregated level, and corroborated the relevance of the measure in several exercises. In order to operationalize state capacity, we considered three important dimensions: the state's extractive, coercive, and administrative capacity. We trained a tree-based prediction model on geocoded survey data on state performance, using publicly available satellite data to measure within-country variation in state capacity. We extrapolated the resulting measure of state capacity to all 2.5×2.5 arc-minutes grid cells in Sub-Saharan Africa.

In order to validate the spatial distribution of our novel measure of local state capacity, we correlated it with variables that have been argued to convey information about state capacity in previous work, namely political power of ethnic groups, pre-colonial centralization, and vaccination coverage. Using only *within* country variation, we showed that our measure of local state capacity linked positively with these factors. Finally, we employed the measure of state capacity in fixed effects panel models on the relationship between oil wealth and conflict, and documented a suggestive moderating effect of local state capacity.

While our approach entails certain disadvantages discussed in Section 6, this chapter provides a first account of a data-driven solution to measure local state capacity. The

38. Each year, 12.5 percent of cells in Sub-Saharan Africa experienced some type of conflict event, but only 0.25 percent experienced oil-related conflict. However, in oil cells, oil-related conflicts constitute more than 4 percent of all conflict events.

methodology enables us to overcome the data scarcity inherent to the empirical state capacity-literature. The flexible nature of the framework presented in this chapter allows for various conceptualizations of state capacity. Researchers interested in specific dimensions of state capacity – e.g., extractive, coercive, or administrative capacity – can easily adapt the approach to their specific needs by simply augmenting the index used for training the prediction model. Both the data[39] and methodology thus add to a burgeoning literature on the role of the state, and provide a new approach to measure state capacity at the relevant level, i.e. where the action takes place.

39. The disaggregated data on state capacity will be made publicly available in order to promote empirical research on the determinants and consequences of state capacity.

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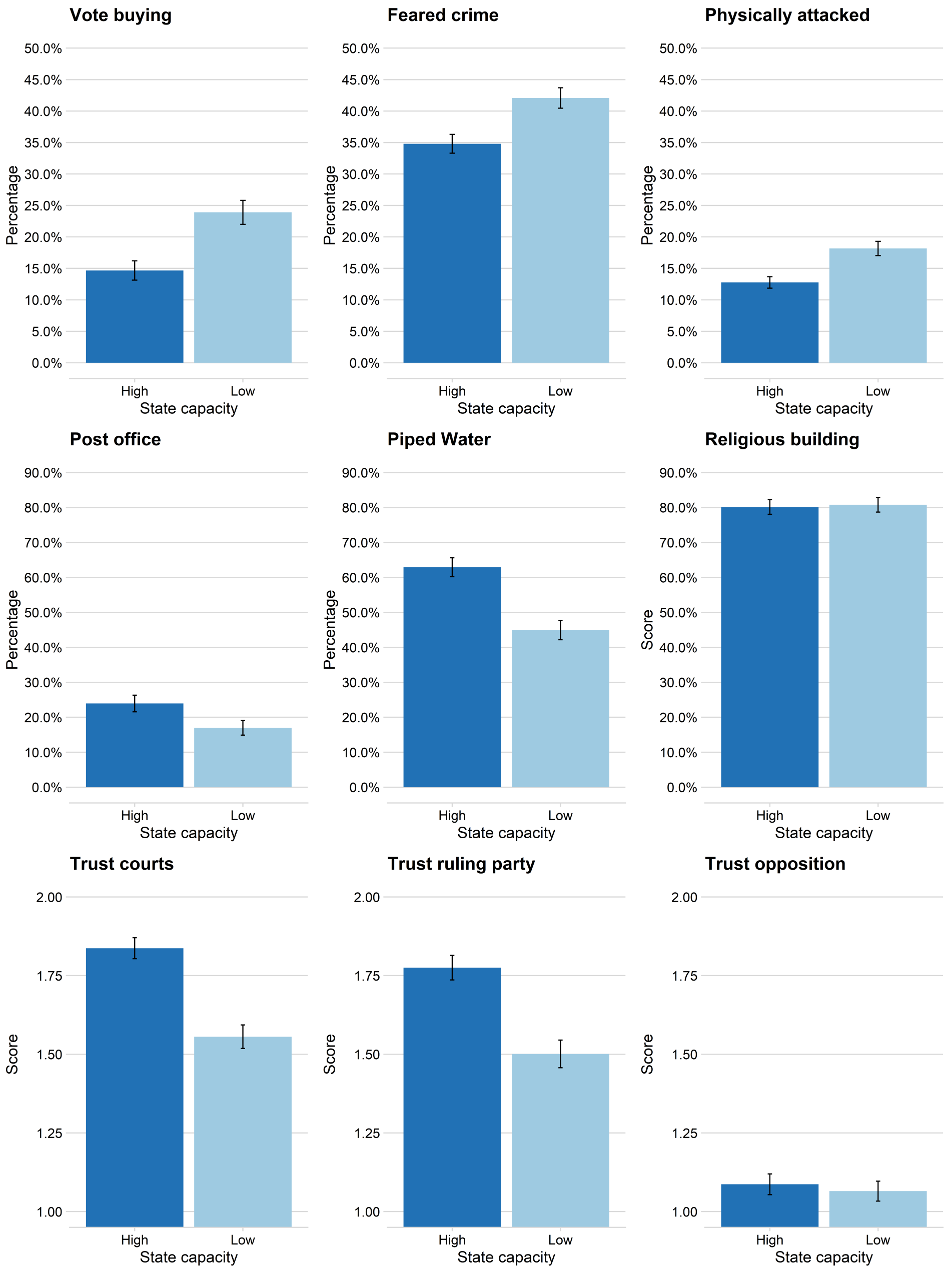
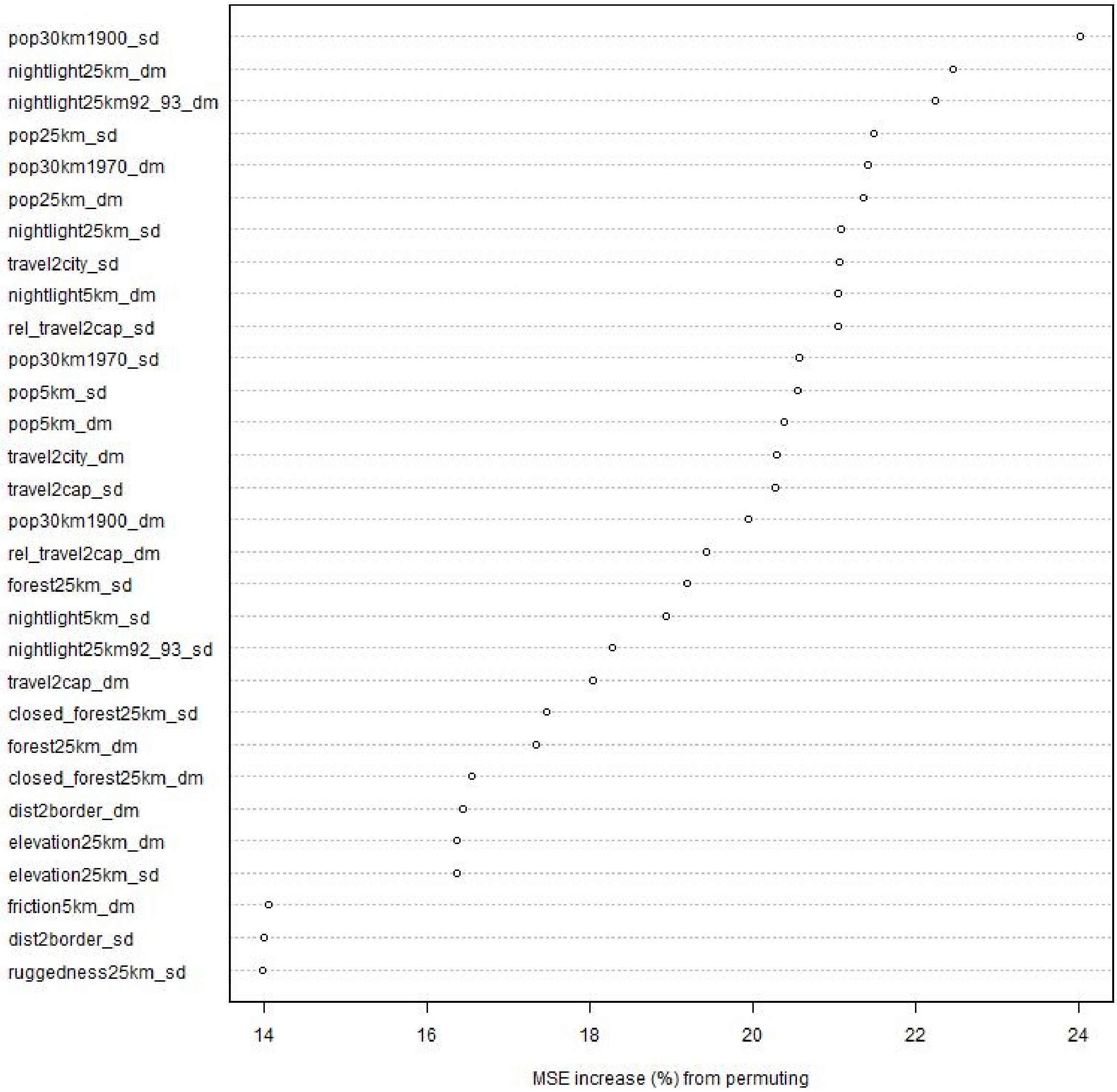
Appendix A: Additional figures and tables

Figure A1: Survey data sanity check



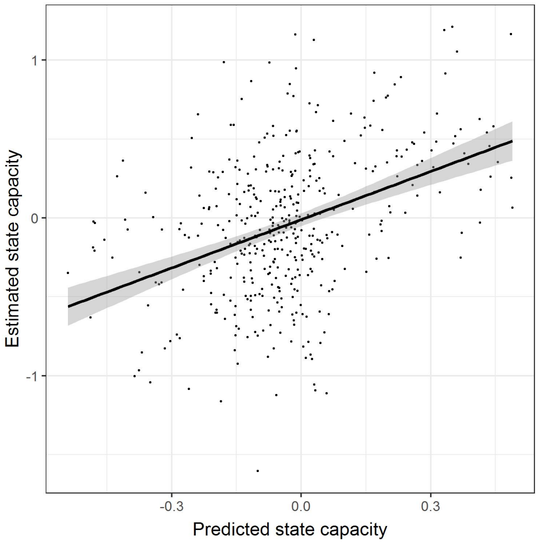
Notes: This figure provides sanity checks of the survey data. The first row shows the share of EA-level vote buying, fear of crime, and actual physical attacks for high (above median) and low (below median) state capacity EAs, respectively. The second row features bar graphs on physical infrastructure reported by the enumerators in the visited areas. The last row depicts levels of trust in the courts, ruling party, and political opposition. Source: Afrobarometer.

Figure A2: Variable importance in Random Forest model with two predictors in each tree

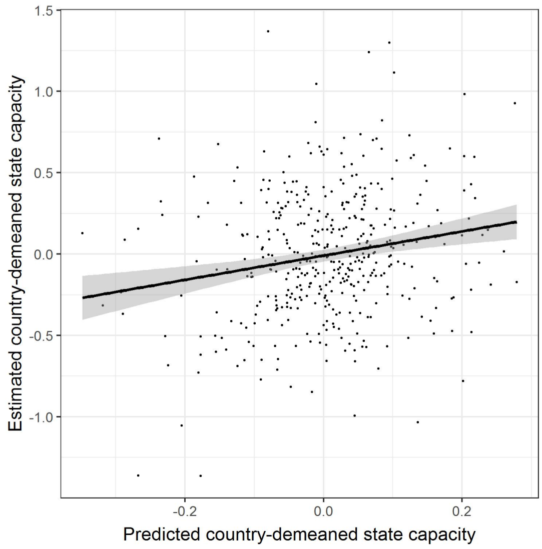


Notes: The x-axis refers to the mean increase in MSE from permuting a specific variable. The higher the value, the more important is the variable in predicting the outcome variable in the random forest algorithm. Suffix "_sd" refers to variables that have been within-country standardized, whereas suffix "_dm" refers to variables that have been country-demeaned.

Figure A3: Relationship between predicted and survey state capacity for a hold-out test sample



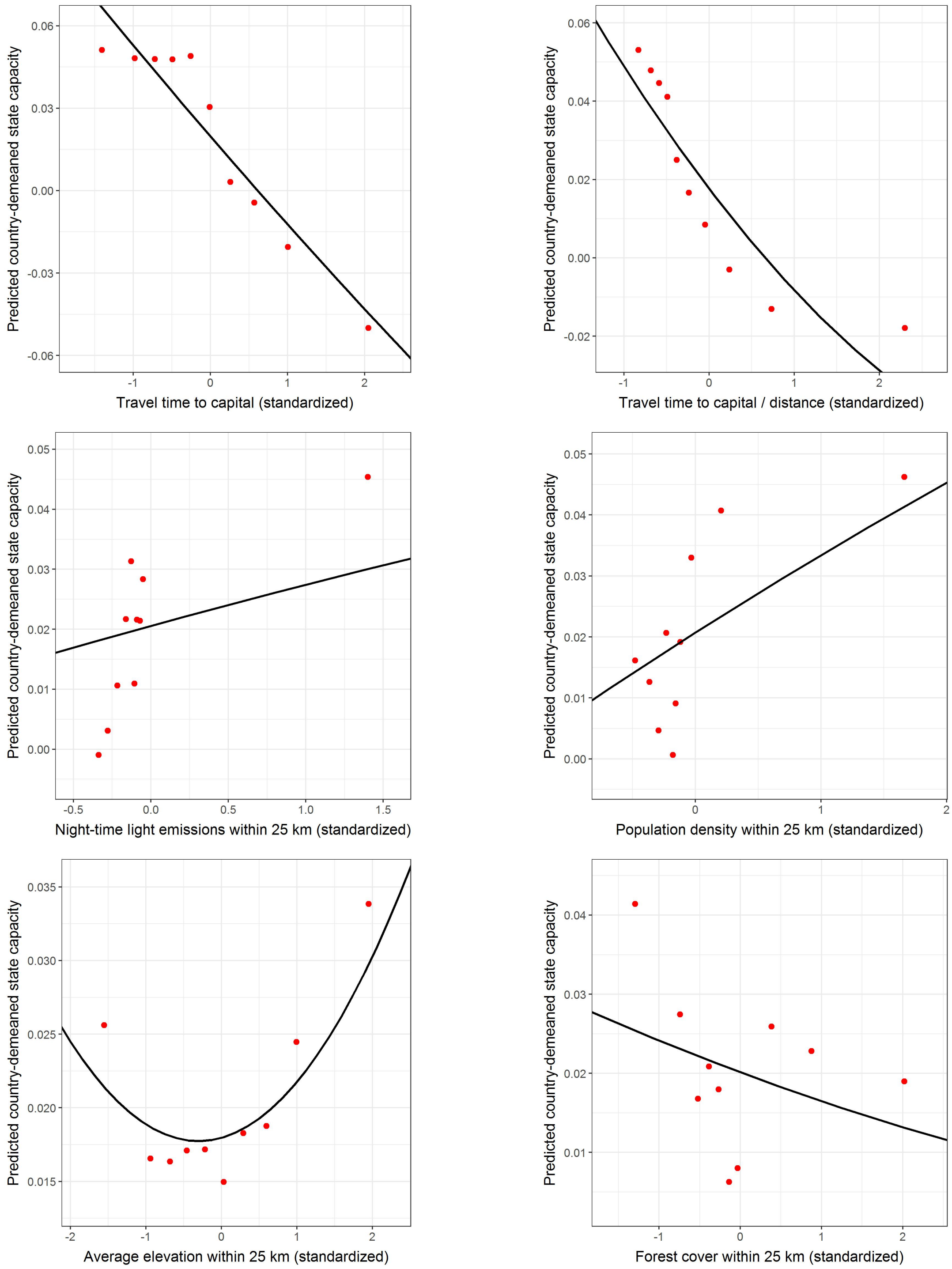
(a) Country baseline + country-demeaned predictions



(b) Country-demeaned predictions only

Notes: The left graph shows the relationship between the estimated state capacity index (based on factor analysis) and the predicted state capacity index (based on between- and within-country prediction models). The right graph shows the relationship between the country-demeaned state capacity index and the predicted country-demeaned state capacity index. The best linear fits in the left and right graphs have highly statistically significant (*p-values* < 0.0001) coefficient estimates of 1.02 and 0.74, respectively, and the shaded areas represent 95 percent confidence intervals. The R^2 in the left and right graphs are 0.158 and 0.037, respectively. For the illustrative purpose of this graph, the test sample is not used to fit the ensemble model. In the ensemble model used for extrapolation, all EAs are used.

Figure A4: Bivariate relationships between predicted state capacity and predictors



The plots show the average predicted country-demeaned state capacity and average of the predictor for the different deciles of the predictor. The illustrated predictors are standardized within each country. The black lines represent best quadratic fits to underlying non-binned data.

Table A1: Country-demeaned and standardized variables used for predicting local state capacity

Predictor name	Description
travel2cap	Travel time to country capital in year 2000.
rel_travel2cap	Travel time to country capital divided by geodesic distance to . capital (travel time per kilometer)
travel2city	Travel time to nearest urban center in year 2000.
dist2border	Geodesic distance to the country border.
friction5km	Infrastructural accessibility within a 5 kilometers radius in year 2000 (easiness of crossing a 1 × 1 kilometer cell).
nightlight25km92_93	Average night-time light emissions within a 25 kilometers radius in . 1992 and 1993
nightlight5km	Average night-time light emissions within a 5 kilometers radius in 2000.
nightlight25km	Average night-time light emissions within a 25 kilometers radius in 2000.
pop30km1900	Population density within a 30 kilometers radius in 1900.
pop30km1970	Population density within a 30 kilometers radius in 1970.
pop5km	Population density within a 5 kilometers radius in 2000.
pop25km	Population density within a 25 kilometers radius in 2000.
elevation25km	Average elevation above sea leavel in meters within a 25 kilometers radius.
ruggedness25km	Average topographic ruggedness index (TRI) within a 25 kilometers radius. For each 2.5 × 2.5 arc-minutes grid cell (≈ 5 × 5 kilometers), we take the mean of the absolute differences between a cell's elevation and its eight surrounding neighbors. Next, we take the average TRI for cells within a 25 kilometers radius.
forest25km	Share covered by forest within a 25 kilometers radius.
closed_forest25km	Share covered by closed forest within a 25 kilometers radius.

Notes: All predictors are included both as country-demeaned variables and as standardized within-country variables. Travel time and friction variables are from Nelson (2008), night-time light emission variables are from Earth Observation Group, NOAA (2019), population data from year 2000 is from Center for International Earth Science Information Network et al. (2011) and Balk et al. (2006), historic population data is from Klein Goldewijk et al. (2017), elevation and ruggedness are based on Danielson and Gesch (2011), and forest cover is from Shaver, Carter, and Shawa (2019).

Table A2: The conditional effect of oil price fluctuations on the likelihood of oil conflict; Lag and lead prices

	Oil event (1)–(3)			Oil violence (4)–(6)			Oil demonstration (7)–(9)		
	Low SC (1)	High SC (2)	Interaction (3)	Low SC (4)	High SC (5)	Interaction (6)	Low SC (7)	High SC (8)	Interaction (9)
Panel A:									
Lagged oil price × *Oil cell*	0.049**	0.00031	0.070**	0.026**	0.0016	0.039**	0.023*	-0.0012**	0.031*
	(0.022)	(0.0019)	(0.031)	(0.0097)	(0.0017)	(0.018)	(0.014)	(0.00055)	(0.017)
Lagged oil price × *State capacity*			-0.00058			-0.00048*			-0.00011
			(0.00039)			(0.00024)			(0.00016)
Lagged oil price × *State capacity* × *Oil cell*			-0.015*			-0.0085*			-0.0063*
			(0.0074)			(0.0046)			(0.0035)
R-squared	.319	.128	.294	.252	.065	.244	.235	.130	.212
Observations	80,142	79,591	159,733	80,142	79,591	159,733	80,142	79,591	159,733
Dependent variable mean	.0044	.00067	.0025	.0027	.00011	.0014	.0019	.00053	.0012
Dependent variable std. dev.	.066	.026	.050	.052	.011	.038	.043	.023	.035
Panel B:									
Lead oil price × *Oil cell*	0.025	0.0013	0.029	0.026	0.0010	0.036	0.0029	0.00029	-0.00087
	(0.017)	(0.0011)	(0.025)	(0.020)	(0.0012)	(0.033)	(0.0028)	(0.00043)	(0.0077)
Lead oil price × *State capacity*			-0.00037*			-0.00025**			-0.00009
			(0.00021)			(0.00011)			(0.00011)
Lead oil price × *State capacity* × *Oil cell*			-0.0047			-0.0072			0.0011
			(0.0059)			(0.0076)			(0.0024)
R-squared	.318	.131	.294	.258	.068	.251	.226	.136	.206
Observations	75,924	75,402	151,326	75,924	75,402	151,326	75,924	75,402	151,326
Dependent variable mean	.0044	.00066	.0039	.0028	.00012	.0015	.0018	.00053	.0012
Dependent variable std. dev.	.066	.026	.062	.053	.011	.038	.042	.023	.034

Notes: The dependent variables in columns (1)-(3), (4)-(6), and (7)-(9) are cell-year indicator variables for 'oil-related conflict event', 'violent oil-related conflict event' and 'oil-related demonstration', respectively. Columns (1), (4), and (7) restrict the sample to cells below the median state capacity, whereas columns (2), (5), and (8) restrict the sample to cells above median state capacity. Columns (3), (6), and (9) display results from models including the continuous measure of local state capacity in a triple interaction with oil cell and the oil price. *Lagged oil price* refers to the *Oil price* in $t-1$, whereas *lead oil price* refers to the *Oil price* in $t+1$. *Oil cell* is an indicator variable on known oil deposits in year 2000, and *State capacity* refers to our measure of local state capacity in year 2000. Standard errors clustered at the country level are in parentheses. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

Table A3: The conditional effect of oil price fluctuations on the likelihood of oil conflict: Oil proximity

	Oil event (1)–(3)			Oil violence (4)–(6)			Oil demonstration (7)–(9)		
	Low SC (1)	High SC (2)	Interaction (3)	Low SC (4)	High SC (5)	Interaction (6)	Low SC (7)	High SC (8)	Interaction (9)
Panel A:									
Oil price × Oil proximity	0.062**	-0.0015	0.096**	0.040**	0.00018	0.068**	0.021**	-0.0016	0.028**
	(0.025)	(0.0013)	(0.039)	(0.016)	(0.00027)	(0.033)	(0.010)	(0.0011)	(0.013)
Oil price × State capacity			0.00056			0.00036			0.00023
			(0.00040)			(0.00036)			(0.00015)
Oil price × State capacity × Oil proximity			-0.022**			-0.016*			-0.0057
			(0.0097)			(0.0086)			(0.0034)
R-squared	.318	.128	.293	.251	.0646	.244	.234	.130	.211
Observations	80,142	79,591	159,733	80,142	79,591	159,733	80,142	79,591	159,733
Panel B:									
Oil price × Oil proximity	0.055**	-0.0014	0.087**	0.035**	0.00019	0.062**	0.017**	-0.0016	0.022**
	(0.022)	(0.0013)	(0.036)	(0.014)	(0.00027)	(0.029)	(0.0076)	(0.0011)	(0.0093)
Oil price × State capacity			0.00056			0.00038			0.00017
			(0.00036)			(0.00033)			(0.00012)
Oil price × State capacity × Oil proximity			-0.019**			-0.015*			-0.0041
			(0.0089)			(0.0078)			(0.0030)
Dependent variable temporal lag	0.078***	-0.033	0.061***	0.052**	-0.070***	0.047**	0.034	-0.036	0.019
	(0.014)	(0.028)	(0.015)	(0.022)	(0.0086)	(0.020)	(0.029)	(0.037)	(0.035)
Dependent variable spatial lag	0.028***	0.0022	0.022***	0.027***	0.0058	0.025***	0.054***	-0.0019	0.036***
	(0.0066)	(0.0062)	(0.0058)	(0.0078)	(0.0091)	(0.0073)	(0.011)	(0.0087)	(0.012)
R-squared	.324	.129	.297	.256	.0693	.248	.245	.132	.217
Observations	80,142	79,591	159,733	80,142	79,591	159,733	80,142	79,591	159,733
Cell & year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent variable mean	.0044	.00067	.0025	.0027	.00011	.0014	.0019	.00053	.0012
Dependent variable std. dev.	.066	.026	.050	.052	.011	.038	.043	.023	.035

Notes: The dependent variables in columns (1)-(3), (4)-(6), and (7)-(9) are cell-year indicator variables for ‘oil-related conflict event’, ‘violent oil-related conflict event’ and ‘oil-related demonstration’, respectively. Columns (1), (4), and (7) restrict the sample to cells below the median state capacity, whereas columns (2), (5), and (8) restrict the sample to cells above median state capacity. Columns (3), (6), and (9) display results from models including the continuous measure of local state capacity in a triple interaction with oil proximity and the oil price. *Oil price* is the log of the average oil price for each year, *Oil proximity* is a continuous variable capturing the inverse of the square root of distance to known oil deposits in year 2000, and *State capacity* refers to our measure of local state capacity in year 2000. Standard errors clustered at the country level are in parentheses. Significance levels: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A4: The conditional effect of oil price fluctuations on the likelihood of oil conflict: Country-year fixed effects

	Oil event (1)–(3)			Oil violence (4)–(6)			Oil demonstration (7)–(9)		
	Low SC (1)	High SC (2)	Interaction (3)	Low SC (4)	High SC (5)	Interaction (6)	Low SC (7)	High SC (8)	Interaction (9)
Panel A:									
Oil price × Oil cell	0.038**	-0.00071	0.056**	0.024**	0.00013	0.042*	0.013**	-0.00083	0.015*
	(0.016)	(0.00064)	(0.023)	(0.010)	(0.00015)	(0.021)	(0.0061)	(0.00057)	(0.0075)
Oil price × State capacity			-0.0013			-0.0010			-0.00022
			(0.0014)			(0.00096)			(0.00075)
Oil price × State capacity × Oil cell			-0.012**			-0.0100*			-0.0027
			(0.0057)			(0.0055)			(0.0023)
R-squared	.326	.139	.301	.260	.078	.253	.247	.143	.223
Observations	80,028	79,534	159,676	80,028	79,534	159,676	80,028	79,534	159,676
Panel B:									
Oil price × Oil cell	0.034**	-0.00059	0.052**	0.022**	0.00021	0.039**	0.011**	-0.00081	0.012*
	(0.015)	(0.00073)	(0.022)	(0.0094)	(0.00025)	(0.019)	(0.0050)	(0.00060)	(0.0062)
Oil price × State capacity			-0.0011			-0.00089			-0.00022
			(0.0013)			(0.00090)			(0.00067)
Oil price × State capacity × Oil cell			-0.011**			-0.0094*			-0.0021
			(0.0054)			(0.0051)			(0.0022)
Dependent variable temporal lag	0.075***	-0.032	0.059***	0.052**	-0.072***	0.046**	0.027	-0.037	0.013
	(0.015)	(0.028)	(0.015)	(0.022)	(0.0079)	(0.021)	(0.029)	(0.036)	(0.033)
Dependent variable spatial lag	0.019**	-0.0074	0.014**	0.020***	-0.00026	0.018***	0.043***	-0.013	0.026**
	(0.0072)	(0.0074)	(0.0065)	(0.0068)	(0.0098)	(0.0065)	(0.011)	(0.011)	(0.012)
R-squared	.331	.140	.304	.264	.083	.256	.254	.145	.226
Observations	80,028	79,534	159,676	80,028	79,534	159,676	80,028	79,534	159,676
Cell & country × year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent variable mean	.0044	.00067	.0025	.0027	.00011	.0014	.0019	.00053	.0012
Dependent variable std. dev.	.066	.026	.050	.052	.011	.038	.043	.023	.035

Notes: The dependent variables in columns (1)-(3), (4)-(6), and (7)-(9) are cell-year indicator variables for 'oil-related conflict event', 'violent oil-related conflict event' and 'oil-related demonstration', respectively. Columns (1), (4), and (7) restrict the sample to cells below the median state capacity, whereas columns (2), (5), and (8) restrict the sample to cells above median state capacity. Columns (3), (6), and (9) display results from models including the continuous measure of local state capacity in a triple interaction with oil cell and the oil price. *Oil price* is the log of the average oil price for each year, *Oil cell* is an indicator variable on known oil deposits in year 2000, and *State capacity* refers to our measure of local state capacity in year 2000. Standard errors clustered at the country level are in parentheses. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

Appendix B: Tree-based prediction methods Bagging uses bootstrapping to generate B random data sets from the training sample. For each random data set, we use the predictor that minimizes the residual sum of squares (RSS) by cutting the sample into two subgroups and taking the mean of the outcome variable. This procedure continues until the algorithm wants to divide a subgroup such that at least one of the proposed subgroups has less than 25 observations. Each observation is given the mean outcome value of the subgroup it belongs to. The predicted outcome variable of observation i is the mean of the B different estimates. The disadvantage of bagging is that a few strong predictors may dominate less influential predictors. Hence, the less influential predictors will never divide the sample, and we would falsely predict they are not associated with the outcome variable. The issue is resolved in random forest by considering a subset of the predictors in each regression tree rather than all predictors. Random forest and bagging are otherwise identical.

Boosting starts by predicting all observations to have an outcome variable of zero, and hence residuals are equal to the actual outcome variable. Next, a regression tree is fitted on the residuals with d splits (called the interaction depth), and observations are given the mean value of the outcome variable of the subgroup they end up in. The predicted outcome variable is multiplied by a factor called the shrinkage parameter, which in turn is subtracted from the initial residuals. Next, the procedure of fitting a regression tree starts over, this time fitting the updated residuals. By letting this procedure continue in eternity, boosting would overfit the training sample and it would not be good at predicting the test sample. We use 5-fold cross-validation to select the number of trees (iterations) in order to avoid overfitting. In addition to the interaction depth and shrinkage parameter, we tune parameters related to penalizing small improvements to overall performance (the gamma parameter), share of the training sample used for each tree, and share of predictors used for each tree. These parameters are tuned by evaluating the prediction performance of the model when changing the parameters.

Chapter 3

Does Scarcity Reduce Cooperation? Experimental Evidence from Rural Tanzania

Does Scarcity Reduce Cooperation?
Experimental Evidence from Rural Tanzania*

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Abstract

Cooperation is essential to reap efficiency gains from specialization, not least in poor communities where economic transactions often are informal. Yet, cooperation might be more difficult to sustain under scarcity, since defecting from a cooperative equilibrium can yield safe, short-run benefits. In this study, we investigate how scarcity affects cooperation by leveraging exogenous variation in economic conditions induced by the Msimu harvest in rural Tanzania. We document significant changes in food consumption between the pre- and post-harvest period, and show that lean season scarcity reduces socially efficient but personally risky investments in a framed Investment Game. This can contribute to what is commonly referred to as a behavioral poverty trap.

Keywords: scarcity, cooperation, field experiment
JEL Codes: C71, C93, D91

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1 Introduction

Many economic transactions rest on mutual trust and cooperation, and self-enforced compliance is thus essential for the functioning of markets. Countries, organisations, and communities with higher levels of trust have recurrently been shown to attain better economic results (e.g., Algan and Cahuc 2010; Bohnet, Herrmann, and Zeckhauser 2010; Knack and Keefer 1997; La Porta et al. 1997). However, cooperative equilibria are inherently unstable, since defection can yield safe and short-run private benefits (Dal Bó and Fréchette 2011). This is particularly a problem in developing countries where contracts are hard to enforce and informality is widespread. In order to better understand obstacles to economic development, we therefore ought to map factors that underpin or undermine economic cooperation.

In this chapter, we ask whether adverse economic conditions, and in particular the experience of food scarcity, can lead to a breakdown in cooperation. We hypothesize that people invest less in cooperative solutions when resources are scarce, since scarcity increases the relative cost of defection by others. As a consequence, agents may forego investment opportunities that are both individually profitable and socially efficient.

Our study takes place in the poverty-stricken region Singida, Tanzania, where we document significant variation in food scarcity between the pre-harvest (early May) and the post-harvest (mid-July) period. We exploit this exogenous variation in food supply to study how scarcity impacts farmers' willingness to engage in cooperative behaviour, by measuring cooperation both before and after the harvest through a lab-in-the-field experiment. Investing is socially efficient and potentially profitable from the investor's perspective, but the outcome is uncertain as it rests on reciprocation from another agent.

We find that scarcity depresses cooperation. Before the harvest, when farmers face greater food shortages, they invest significantly less compared to after the harvest. The reduced form impact is significant both in a between-subject design (different participants before and after the harvest) and a within-subject design (the same subjects participating twice). Intuitively, the effects are substantially larger for relatively poorer farmers, who experience greater scarcity in the pre-harvest period. We further corroborate the interpretation of the effect as one driven by food scarcity by means of an instrumental variable approach.

Our study extends a growing literature on the relationship between poverty and economic behavior, which encompasses studies on e.g. self-control (Banerjee and Mullainathan 2010), risk-aversion (Yesuf and Bluffstone 2009 and Blalock, Just, and Simon 2007), and borrowing choices (Shah, Mullainathan, and Shafir 2012 and Agarwal, Skiba, and Tobacman 2009). In particular, we contribute to the emergent literature concerned with the *causal* effect of scarcity on economic behaviors (e.g., Miguel 2005; Shah, Mullainathan, and Shafir 2012; Haushofer, Schunk, and Fehr 2013; Prediger, Vollan, and Herrmann 2014; Shah, Shafir, and Mullainathan 2015; Carvalho, Meier, and Wang 2016; Lichand et al. 2020). Our identification strategy builds on the seminal study by Mani et al. (2013), who exploit the timing of sugar cane harvests in India to investigate how scarcity influences cognitive abilities.

A few studies have previously investigated links between seasonal scarcity and adverse behavior in the context of Tanzania (most notably the study by Miguel (2005) on witch-hunts)). Closely related with the present study, Hadley, Mulder, and Fitzherbert (2007) find that "instrumental social support" – meaning economic support in case of need – associates negatively and strongly with incidences of food scarcity in South-western Tanzania. They draw the conclusion that social support determines food scarcity. While a lack of cooperation could aggravate food scarcity, our findings suggest that the impact also runs in the opposite direction: scarcity depresses cooperation.

We differ from previous studies on scarcity and economic behavior in a number of important respects. First, by investigating the influence of scarcity on potentially *self-serving* cooperation, our conceptualization of cooperation contrasts with studies measuring behavior in, e.g., one-shot prisoner's dilemmas (Boonmanunt and Meier 2020) and joy-of-destruction games (Prediger, Vollan, and Herrmann 2014). Cooperating in a one-shot prisoner's dilemmas can be interpreted as an act of altruism, since defecting is invariably personally profitable. In the real world, decisions on whether to cooperate or not depend crucially on potential personal gains from successful cooperation. The sequential way in which senders and receivers interact in the Investment Game allows us to capture cooperation motivated also by personal interest; cooperation in our study is a risky but potentially profitable option.[1]

1. Indeed, altruistic motives are not a dominant predictor of behavior in the Investment Game (Brülhart

Second, we distinguish our work from previous research by focusing on food scarcity as opposed to scarcity of financial resources (Mani et al. 2013; Aksoy and Palma 2019; Boonmanunt and Meier 2020). In e.g. Mani et al. (2013), the sampled farmers are able to smooth food consumption and are not eating less prior to the harvest. In our context, a substantial proportion of the farmers do not accumulate any savings, and are forced to reduce food consumption in the lean period. While food and financial scarcity are correlated, food scarcity is a more severe form of deprivation and can be expected to trigger larger behavioral changes (Schofield 2014). Our results confirm this hypothesis. To the best of our knowledge, we are the first to document how food scarcity depresses socially efficient investment.

Third, by instrumenting scarcity using the post-harvest shock in food supply as an instrument, we go further than studies focusing exclusively on the reduced form impact of the harvest (Mani et al. 2013; Bartos 2016; Aksoy and Palma 2019; Boonmanunt and Meier 2020). Using a two-stage least squares approach, we are able to document that scarcity is indeed the mediating channel depressing cooperation in the lean period.

The paper is structured as follows. In Section 2, we discuss relevant features of the Singida region, the empirical context of the present study. Section 3 outlines our experimental design and in Section 4 we present the main findings. We discuss the implications of the findings in Section 5.

2 Empirical setting: the region of Singida

The study was conducted in Singida, a poverty-stricken region in central Tanzania with a diverse population. In our sample, the Nyaturu (36%), Sukuma (25%), and Gogo (23%) constitute the main ethnic groups, 63% adhere to Christianity, whereas 26% are Muslims. The region is semi-arid and the economy heavily centered around agricultural production. Food crops are the dominant products: 9 out of 10 participants in our sample grow maize, 1 in 5 grows sorghum, and 1 in 5 grows millet. Sweet potatoes and sunflower seeds, which are sometimes used for food consumption and sometimes as cash crops, are cultivated by 15% and 40% of the farmers, respectively. While the timing of the harvest varies somewhat

and Usunier 2012).

between crops, the main harvesting period is between May and June.

The harvest constitutes the main source of income and food for the farmers of Singida; in our sample, 88.1% of the respondents report that (at least) some of their income comes from farming. Hence, consumption can vary significantly between the lean pre-harvest period and the abundant post-harvest period (as we later show in Section 4.1). The variation is accentuated by major obstacles to consumption smoothing, such as credit constraints and limited access to saving mechanisms. Poor farmers – like the participants in our study – tend to lack reliable storage opportunities, both in terms of food (Parfitt, Barthel, and Macnaughton 2010) and in terms of cash (Aryeetey 1997). Saving is risky due to a weak justice system (Bates 1987). Lastly, present bias may further enhance seasonal fluctuations in food availability (Laajaj 2017), as a preference for immediate consumption may contribute to depleting surpluses, especially among people who can barely satisfy their basic needs.

3 Experimental design

Our analysis is centered around an Investment Game (Berg, Dickhaut, and McCabe 1995), which we conduct before and after the yearly harvest, with participants from two distinct sets of randomly selected villages in the region of Singida. In this section, we outline the Investment Game, introduce two experimental manipulations, and discuss our sampling strategy.

3.1 The Investment Game

We conduct an Investment Game à la Berg, Dickhaut, and McCabe (1995). The game has two players (A and B) who are anonymous to each other (they never meet and their decisions are only reported to the other player after the game is concluded). Player A begins the game with a certain endowment and chooses how much to invest in a common project with Player B. The amount s/he invests is then tripled and Player B gets to split the income from the investment between the two players. For simplicity, Player A could choose to invest all, half, or none of the initial endowment.

To facilitate understanding, the game was framed as a situation familiar to farmers: Player A had to decide how much to invest in seeds that would result in a harvest worth three times the investment. The initial endowment Player A was given amounted to 4,000 Tanzanian Shillings, roughly corresponding to a day's worth of the minimum wage in the agricultural sector in Tanzania (De Blasis 2020). S/he was informed that Player B would decide how the income from the harvest would be split between the two players. The payoffs were paid in cash at the end of the day, after the game and an accompanying survey were completed. The rules of the game were explained carefully by means of examples and visual aids. The full script and the visual aids are shown in Appendix B. In Figure 1 we depict the sequencing of the game.

Figure 1: Decision tree

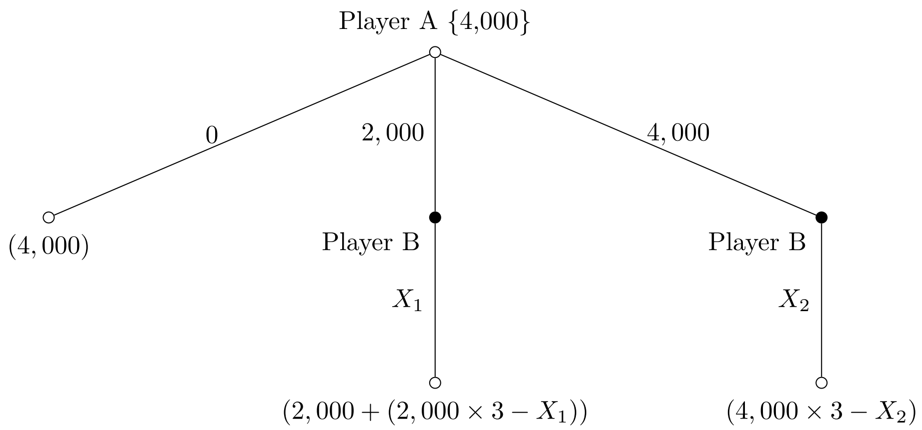


Figure 1 displays the decision tree and thereby the information set of Player A when making the initial investment (or not). The amount in curly brackets refer to the initial endowment of Player A. The amounts in parentheses indicate the potential payoffs of Player A. X_1 and X_2 denote the sum that Player B decides to keep for herself, respectively in the scenarios when 2,000 and 4,000 was invested by Player A. X_1 is bounded between 0 and 6,000, whereas X_2 is bounded between 0 and 12,000.

Based on these rules, Player A was asked to indicate how much s/he wanted to invest. Player B, on the other hand, was asked to indicate how much s/he would give back to Player A for each level of investment Player A could have made (the actual choice made by Player A was not revealed until after the game and the ensuing interview was concluded). In order to minimize experimenter demand effects that might be caused by the presence of interviewers, participants were asked to make their choice in a private space by indicating their decision on a sheet. They were then asked to fold the answer sheet and hand it back to the enumerator (who did not look at their answer until later).

3.2 Manipulations

The primary focus of this study is how cooperation depends on participants' current food situation. We hypothesize that food scarcity hampers respondents' ability to choose the socially efficient option of investing, by leading them to prefer a safe option (not investing). We rely on the seasonal variation in food scarcity induced by the harvest to identify the causal effect of food scarcity on investment. In addition, we embed two experimental treatments in the Investment Game: (a) a prime that makes scarcity particularly salient prior to the game; (b) an ingroup/outgroup manipulation.

3.2.1 Scarcity prime

A growing literature has documented the psychological impacts of poverty on decision-making (Shah, Shafir, and Mullainathan 2015; Lichand et al. 2020). At least to some extent, it is the awareness of trade-offs (e.g. between risk and reward) – what Mullainathan and Shafir (2013) label a scarcity mindset – which influences behavior. In our context, this means that in addition to scarcity influencing behavior directly, when a current state of scarcity is made salient it should depress investment *further*. To trigger this mechanism, we asked respondents a series of questions about their current food consumption (we detail the questions in Figure A2 in the Appendix). Half of our respondents (primed) are asked those questions before they play the Investment Game. The other half (control) answers those questions after they play the game.

3.2.2 Ingroup vs Outgroup

Our second experimental manipulation is employed to test whether scarcity is more damaging for cooperation with people who are more socially distant. This may be the case, for instance, if social proximity makes reciprocity easier to sustain. We test this proposition by randomly varying the counterpart that respondents face in the Investment Game (i.e., Player B) between an ingroup and an outgroup member. Specifically, while half of the participants are told that Player B is another (anonymous) person from their own village, the rest are told that Player B is from another part of Tanzania. The ingroup/outgroup manipulation

Table 1: Distribution of subjects across treatments

	Pre-harvest		Post-harvest	
	Ingroup	Outgroup	Ingroup	Outgroup
Scarcity Prime	45	39	51	40
No prime	38	45	45	60

Table 1 displays the number of participants per treatment arm.

is embedded in the game instructions outlined in Appendix B.2. The pre- and post-harvest sampling in conjunction with the two experimental manipulations gives rise to a 2x2x2 design that we summarise in Table 1.

3.3 Timing of the survey

The timing of the survey was chosen with respect to the Msimu harvest, from which farmers in Singida derive most of their food and income. We conducted the first round of surveying just prior to the harvest, in early May, while the second round was conducted at a time when most of the gains from the harvest had been realized, in mid-July.

3.4 Sampling

Our study was conducted in two districts within the region of Singida, namely Ikungi and Manyoni.[2] We randomly selected 12 "wards" (administrative units consisting of several villages) – 6 from each district – for the first wave of surveying.[3] From each ward, we randomly selected one village, such that the sample of the first wave consisted of 12 villages. In the second wave, we randomly selected 8 of the wards from the first round and drew new villages from each of these wards. By sampling villages from the same wards in both survey rounds, we ensure that villages in the pre- and post-harvest wave are as similar as possible. In addition, we re-visited the remaining 4 villages from the first wave and interviewed the same subjects a second time. This allows for an additional within-subject analysis. Finally,

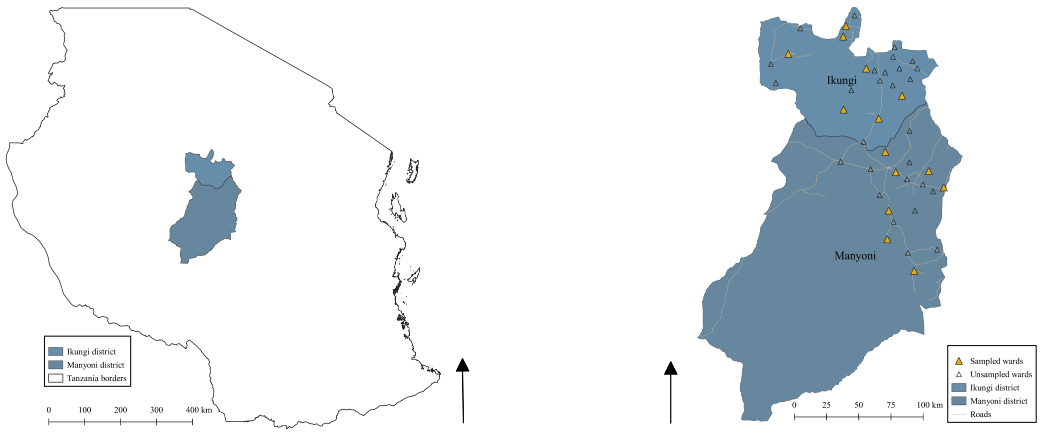
2. For logistical reasons and budget limitations, we could not cover the entire region and chose two large districts that were suitable for the present study, due to the high degree of agricultural reliance in conjunction with relatively high levels of poverty.

3. We excluded a number of wards from the randomization due to infrastructural constraints (some wards were temporarily inaccessible by car).

to increase statistical power, in the second wave we also included two new villages from randomly selected wards that were not part of the first round.

In total, we visited 22 unique villages, 4 of which were sampled twice. The sampled villages and wards are detailed in Table A1 in Appendix A. Panel (b) of Figure 2 maps the sampled and unsampled wards.

Figure 2: The empirical setting



(a) Sampled districts **(b)** Sampled wards

In each village, we randomly selected households by means of a random walk sampling methodology, and invited one (randomly chosen) adult per household to take part in the survey. The number of people interviewed in each village ranges between 28 and 32. In this chapter, we restrict our analysis to participants who obtained at least some income from the harvest, and the final sample thus consists of 363 subjects in the role of Player A (whose investment decision is our primary focus). In Table 2, we document that the random selection of villages and households was successful in attaining balance between the pre- and post-harvest round across a large range of covariates.

Table 2: Covariates balance between the pre- and the post-harvest sample

	Pre-harvest			Post-harvest			
	N	Mean	S.d.	N	Mean	S.d.	Difference
Woman	167	0.44	0.50	196	0.46	0.50	0.021
Age	167	42.92	14.72	196	41.65	13.76	-1.274
Years in village	167	23.13	15.97	196	26.06	18.02	2.924
Non-farm earnings	167	0.10	0.30	196	0.13	0.33	0.026
Post-primary education	167	0.11	0.31	196	0.14	0.35	0.030
Literacy	167	0.84	0.36	196	0.87	0.34	0.023
Muslim	167	0.30	0.46	196	0.22	0.42	-0.075
Christian	167	0.58	0.49	196	0.66	0.47	0.082
Nyaturu	167	0.37	0.48	196	0.36	0.48	-0.008
Sukuma	167	0.25	0.44	196	0.25	0.43	-0.001
Gogo	167	0.21	0.41	196	0.24	0.43	0.035
Head of household	167	0.71	0.45	196	0.64	0.48	-0.075
Married	167	0.83	0.37	196	0.82	0.38	-0.011
Owns cattle	167	0.53	0.50	196	0.51	0.50	-0.028
Owns chickens	167	0.77	0.42	196	0.74	0.44	-0.033
Owns goats	167	0.46	0.50	196	0.42	0.50	-0.032
Maize cropping	167	0.90	0.30	196	0.86	0.35	-0.047
Sunflower seed cropping	167	0.38	0.49	196	0.44	0.50	0.067
Sorghum cropping	167	0.20	0.40	196	0.22	0.42	0.021
Millet cropping	167	0.18	0.39	196	0.19	0.39	0.009
Owns tractor	167	0.01	0.11	196	0.00	0.00	-0.012
Owns plough	167	0.47	0.50	196	0.44	0.50	-0.028
Using fertilizer	167	0.10	0.30	196	0.12	0.33	0.021
Rain irrigation	167	0.99	0.08	196	0.99	0.07	0.001
Recent family death	167	0.02	0.13	196	0.02	0.14	0.002
Recent property theft	167	0.05	0.23	196	0.07	0.26	0.018
Receives financial/food support	167	0.13	0.33	196	0.08	0.27	-0.044
Has outstanding loan	167	0.24	0.43	196	0.21	0.41	-0.025

Table 2 shows descriptive statistics on a range of relevant covariates for the pre- and post-harvest sample. The Difference column displays coefficients and corresponding significance levels from simple a regression with post-harvest treatment as the sole regressor and robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4 Results

In this section, we present the results of our analysis. First, we document large variation in food scarcity between the pre- and post-harvest period. Second, we present evidence of a significant change in cooperative behavior between the two periods. Third, we estimate a causal impact of food scarcity on cooperative behavior by instrumenting the level of scarcity with an dummy variable indicating whether the Investment Game was played in the lean- or abundant period. Fourth, we show that the results are robust to accounting for a broad set of potential confounders. Fifth, we document the impact of the two experimental manipulations embedded in the Investment Game. Finally, we show that cooperation on average paid off.

4.1 The harvest changes scarcity levels

The first step in our analysis is to investigate whether reliance on a yearly harvest leads to fluctuations in food scarcity among the farmers in our sample. We find a substantial effect of the harvest on levels of food scarcity. Figure 3 plots histograms of how frequently people did not have sufficient food in the month prior to the survey. It shows a clear shift in the distribution with the share of people declaring food shortages falling significantly after the harvest. While 7 out of 10 households reported some degree of food scarcity before the harvest, only 3 out of 10 did so after the harvest.

Figure 3: Effect of harvest on scarcity

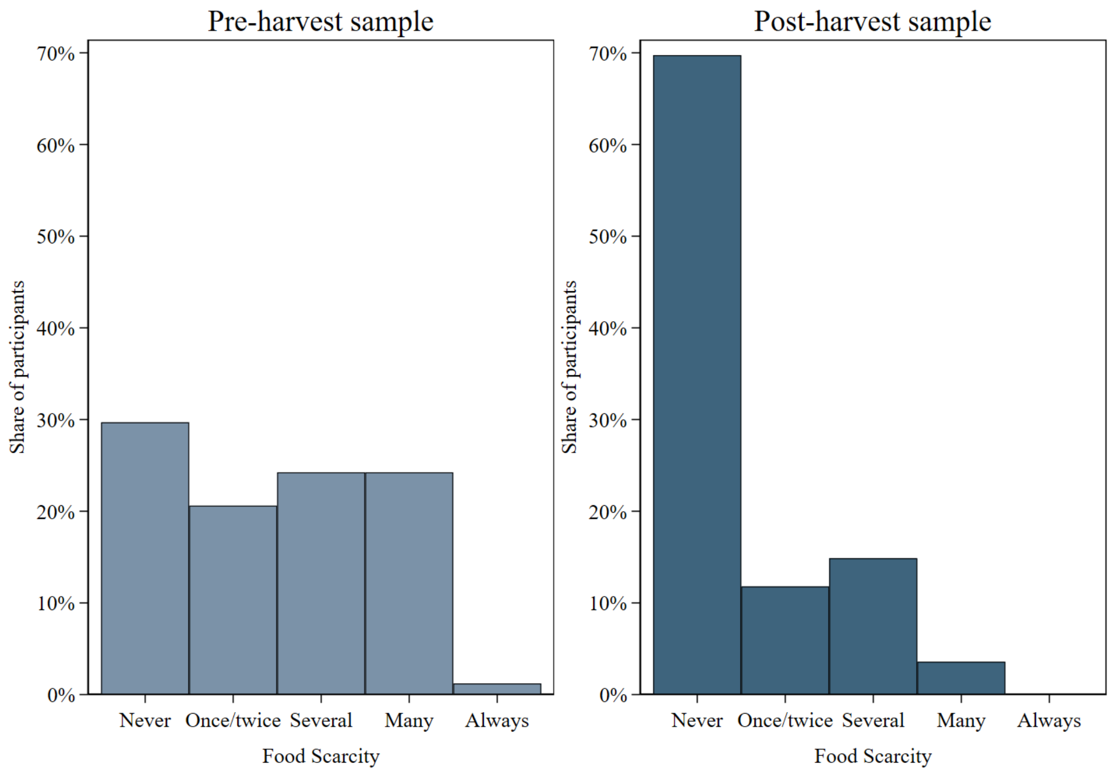


Figure 3 displays the change in food scarcity from round 1 to round 2. Food scarcity is measured as the response to the following question: "Over the past month, how often, if ever, have you or anyone in your family gone without enough food to eat?".

In Table A3 in Appendix C.3, we show that the shift is both statistically significant and economically meaningful. Scarcity decreased in the post-harvest period by more than four fifths of a standard deviation. The results are robust to using alternative measures of food scarcity, namely the number of days with fewer meals than normal over the past month (see Figure A4 in the Appendix C.1).

4.2 Cooperation is lower before the harvest

Having established a link between the harvest and food scarcity, we can now investigate how this exogenous source of variation affects investment behavior. We estimate the impact for four different samples. The Full sample, which includes all farmers in our sample; the Limited sample, from which we exclude the post-harvest observations on participants that also took part in the first round[4]; the Within sample, where we focus on individuals who participated twice (and can therefore include individual-level fixed effects); and finally the Village sample, which reports the effect of the harvest on average village-level investment. In Table 3 we report the results.

Table 3: Effect of the harvest on investment

Dep. Var.:	Investment in the Investment Game			
Sample:	Full	Limited	Within	Village
	(1)	(2)	(3)	(4)
Post-harvest Treatment	258.8**	218.6*	583.3**	259.7**
	(120.8)	(122.2)	(265.2)	(122.1)
Constant	2802.4***	2802.4***	3708.3***	2799.7***
	(105.1)	(105.3)	(336.9)	(104.0)
Observations	363	310	106	26
R-squared	0.0112	0.00762	0.550	0.167
Dep. Var. Mean	2942.1	2903.2	2849.1	2939.6
Individual F.E.	NO	NO	YES	NO

Cluster-robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3 displays OLS regression estimates of the effect of the harvest on investment in the Investment Game. Individual F.E. indicates Individual Fixed Effects. All specifications report cluster-robust standard errors at the village-round level.

Regardless of the specification, the results show that cooperation is significantly

4. The rationale for this specification is to ensure that learning effects – which may affect behavior of participants that participated twice – do not influence the findings.

lower in the lean period that precedes the harvest. For the full sample presented in column 1, we document an increase in investment amounting to almost 10% of the baseline investment level after the harvest. The effect is somewhat smaller and less precisely estimated when we restrict the analysis to the limited sample, but the effect remains significant at the 10% level. In column 3, we zoom in on farmers who participated twice and estimate a diff-in-diff model with individual fixed effects. Once again, the results show a large and positive impact of the harvest on investment decisions. Lastly, in column 4 we report the impact of the harvest on average investment at the village level. By studying the effect at this level of aggregation, we ensure that the results are not sensitive to intra-group correlations in investment behavior (Angrist and Pischke 2008).[5] The positive impact of the harvest is statistically significant also in this sample.

Finally, we show that the harvest induced a more significant reduction in food scarcity – and a larger increase in investment – among relatively poorer participants (as measured by a self-reported evaluation[6]). In Figure 4, we show that while relatively poor participants are much more likely to experience scarcity before the harvest, these differences are largely levelled out by the harvest. Correspondingly, relatively poor farmers increase their investment levels substantially more compared to the relatively well-off in the post-harvest period.

5. The main regressions report clustered standard errors at the village-round level for this reason, but group averages are somewhat more reliable when the number of clusters is relatively small (Angrist and Pischke 2008).

6. The survey item read “How rich or poor is your household in comparison with other households in the village? ” (Much poorer; A little poorer; Same; A little richer; Much richer).

Figure 4: Heterogeneous effect of the harvest on food scarcity and investment

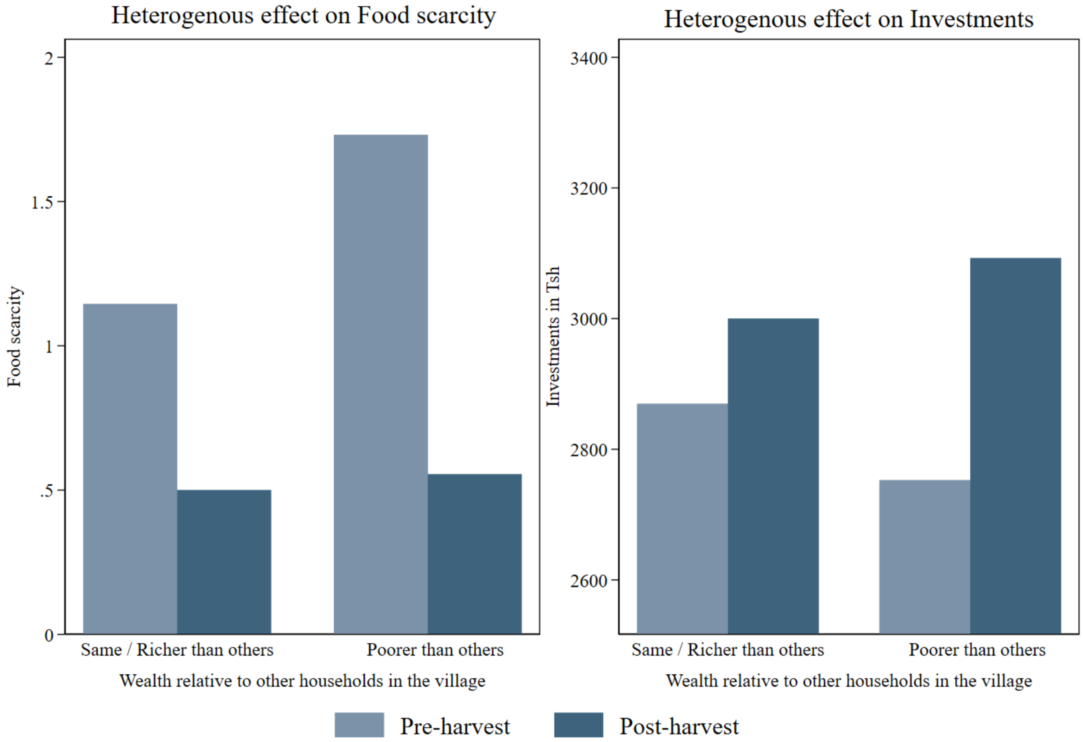


Figure 4 shows how the harvest changed differently both the level of scarcity and investment as a function of relative poverty. Food scarcity ranges from 0 (no food scarcity in the past month) to 4 (constant food scarcity in the past month).

4.3 Food scarcity is associated with lower levels of cooperation

The underlying assumption so far has been that the harvest shifted food scarcity and therefore also the level of cooperation. This causal chain requires that food scarcity link negatively with investment levels. Instead of assuming this linkage, we can document it. In Figure 5, we plot average investment in each village against the average level of food scarcity in the pre- (light blue) and post-harvest (dark blue) sample. At the village level, the negative correlation between experienced scarcity and cooperation is strong and statistically significant (N=26, coefficient=-361, p-value=0.001, linear regression with robust standard errors). The figure also confirms that the harvest significantly decreases food scarcity, as documented in Section 4.1 (N=26, coefficient=-0.816, p-value<0.001, linear regression with robust standard errors), and increases average investment levels (as shown in column 4 of Table 3). In the

Appendix Section C.2 (Table A2), we also document the negative association between food scarcity and investment levels at the individual level.

Figure 5: Village-level food scarcity and investment (pre- vs post-harvest sample)

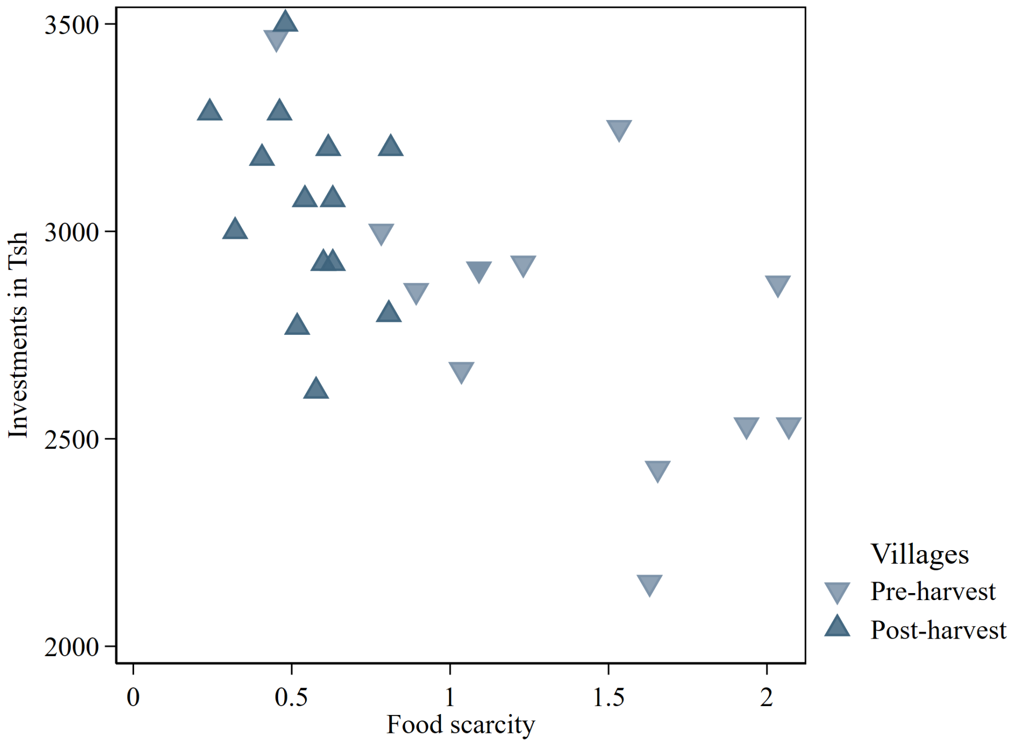


Figure 5 shows the correlation between village level food scarcity and investment. Moreover, the figure displays how both the level of scarcity and investment levels changed with the harvest. Food scarcity ranges from 0 (no food scarcity in the past month) to 4 (constant food scarcity in the past month).

A mere correlation between food scarcity and investment does not, however, prove that a lack of food *causes* lower investment levels. Next, we complete the analysis by investigating the causal impact of food scarcity on cooperation.

4.4 The causal impact of scarcity on cooperation

We estimate the direct impact of food scarcity on cooperation by means of a standard two-stage least squares approach, exploiting the harvest as an instrument for food scarcity. For this strategy to be valid, we need the harvest to have had substantial influence on the levels of food scarcity. This was demonstrated in Section 4.1. Moreover, we need our pre- and post-harvest samples to be similar in all respects that matter for cooperation *except* for food scarcity. Though we cannot be certain that such a restriction is fulfilled, we can use the information contained in the survey to alleviate concerns that either sampling error or unaccounted seasonal shocks might threaten the causal interpretation of our results. First, we showed in Table 2 that the two samples are strongly balanced across a large set of covariates. In addition, we show in Section 4.5 that while we observe seasonality in other domains besides food scarcity (e.g., festive events and weather shocks), these factors do not confound our analysis.

The results from the two-stage least squares regressions are reported in Table 4. We display estimates for the Full-, Limited-, Within-, and Village-sample. In Panel A, we show that the harvest is a first-order predictor of food scarcity. The associated F-values range between 26 and 35, which is evidence of a strong first stage. In Panel B, we outline the second stage estimates. The results show that scarcity significantly reduces cooperative behavior. The economic significance is substantial. Since scarcity is measured on a scale from 0 to 4, the linear estimates imply that going from no to constant scarcity would decrease investment levels from 3209 Tsh to 2105 Tsh, on average. Just like in the reduced form results in Table 3, the effect is even larger for the difference-in-difference estimation on the within-subject sample presented in column 3.

Table 4: Effect of food scarcity on investment

Panel A: First Stage

Dep. Var.:	Food scarcity			
Sample:	Full	Limited	Within	Village
	(1)	(2)	(3)	(4)
Post-harvest Treatment	-0.937***	-0.907***	-1.125***	-0.920***
	(0.160)	(0.163)	(0.211)	(0.157)
Constant	1.473***	1.473***	1.063***	1.459***
	(0.156)	(0.156)	(0.125)	(0.152)
Observations	363	310	106	26
R-squared	0.169	0.152	0.687	0.621
Dep. Var. Mean	0.967	1.055	1.038	0.964
F-value	34.86	33.27	28.34	26.85
Individual F.E.	NO	NO	YES	NO

Panel B: Second Stage

Dep. Var.:	Investment in the Investment Game			
Sample:	Full	Limited	Within	Village
	(1)	(2)	(3)	(4)
Food scarcity	-276.1**	-241.1**	-518.5***	-318.3***
	(124.0)	(114.9)	(161.0)	(122.8)
Constant	3209.2***	3157.5***	4259.3***	3233.2***
	(114.3)	(109.1)	(200.2)	(111.7)
Observations	363	310	106	26
Dep. Var. Mean	2942.1	2903.2	2849.1	2939.6
Individual F.E.	NO	NO	YES	NO

Cluster-robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4 displays instrumental variable regression estimates of the effect of food scarcity on investment in the Investment Game. Food scarcity is operated as a continuous variable ranging from 0 (no food scarcity in the past month) to 4 (constant food scarcity in the past month) and is instrumented by a dummy for participating in the Investment Game after the harvest. Individual F.E. indicates Individual Fixed Effects. All specifications report standard errors clustered at the level of wards.

4.5 Addressing potential confounders

In this subsection we address concerns that time-varying factors other than food scarcity may have influenced our findings. Based on the existing literature and on the specific context of our study, we identified four key factors which varied between the pre- and post-harvest season (see Figure A3 in Appendix C.1) and may have influenced changes in cooperative behavior. First, the harvest relaxes financial constraints as well as constraints on the availability of food (Aksoy and Palma 2019). Many farmers grow cash crops and one may hypothesize that it is the greater availability of money, rather than increased abundance of food, that changes people's decisions in the game. Second, more resources – and a lower workload –

could improve people's cognitive abilities, as shown by Mani et al. (2013), and this may in turn affect behavior in the game by simply improving understanding. Third, the pre- and post-harvest periods coincide with social events such as weddings and festivities, as well as Ramadan. Since festivities may change behavior for reasons unrelated to food scarcity, in what follows we control for these events and study whether this changes our estimates. Finally, seasonal variation in other kinds of adverse shocks may also influence the results. Our study took place at the onset of the Covid-19 pandemic, and we therefore test whether worries about the virus may have influenced investment behavior differentially across the two survey rounds. We also test whether other shocks such as extreme weather events played a role.

Table 5: Addressing potential confounders

Dep. Var.:	Investment in the Investment Game							
Alternative explanation:	Financial scarcity (1)	Financial security (2)	Work load (3)	Cognitive ability (4)	Ramadan effect (5)	Covid-19 worry (6)	Festive events (7)	Adverse events (8)
Post-harvest Treatment	252.9**	259.0**	264.8**	266.8**	300.5*	283.4**	211.6*	269.0**
	(119.7)	(125.7)	(120.2)	(126.9)	(151.3)	(124.9)	(117.2)	(116.2)
Financial scarcity	-14.86							
	(55.84)							
Savings		0.0726						
		(0.0596)						
Current Loans		280.7*						
		(157.3)						
Work load			13.92					
			(24.08)					
Correct RM				17.81				
				(29.33)				
Muslim					167.9			
					(146.5)			
Muslim × Post-harvest Treatment					-129.6			
					(250.6)			
Covid-19 worry						82.47**		
						(36.63)		
Other celebrations							-291.1	
							(209.3)	
Religious celebrations							-150.0	
							(199.0)	
Wedding							84.31	
							(179.9)	
Lost livestock								-27.09
								(173.9)
Property theft								-90.35
								(225.9)
Family death								1075.9***
								(136.6)
Family illness								68.75
								(191.7)
Extreme weather								17.75
								(131.0)
Constant	2832.8***	2730.6***	2711.2***	2737.1***	2752.1***	2530.5***	3278.2***	2709.2***
	(153.5)	(120.5)	(180.0)	(169.8)	(122.6)	(169.1)	(494.5)	(309.5)
Observations	363	363	363	363	363	360	314	363
R-squared	0.0114	0.0215	0.0121	0.0120	0.0132	0.0218	0.0223	0.0268
Dep. Var. Mean	2942.1	2942.1		2942.1	2942.1	2938.9	2955.4	2942.1

Cluster-robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5 displays OLS regression estimates of the effect of the Post-harvest treatment on investment in the Investment Game. All specifications report cluster-robust standard errors at the village-round level.

Table 5 shows that the range of hypothesized confounders had little or no impact on the baseline results. In Appendix C.5, we further corroborate these insights by adding a large battery of controls (Table A5) and documenting the stability of the effect of the harvest, as well as of food scarcity, on investment. Moreover, since assignment into the pre- and post-harvest sample was random by nature, we can study the effect by means of randomization inference. In Figure A6, we show that the effect of the post-harvest treatment does not rely on the distributional assumptions invoked in OLS regressions; the effect is estimated at the same level of statistical significance also when using randomization inference. While it is impossible to ensure that all the potential confounders are accounted for, the stability of the post-harvest effect across specifications is reassuring. In the following section, we further strengthen the interpretation of the scarcity effect by presenting results from our experimental manipulations.

4.6 Experimental manipulations

Next, we use our experimental manipulations to explore two channels that may play an important role in aggravating the effect of food scarcity on cooperation. First, we study whether the effect becomes stronger when scarcity is more salient in respondents' minds. Second, we investigate whether scarcity is more harmful for cooperation with people who are not from the same village and hence typically fall outside the participant's network of support.

4.6.1 Perceived scarcity

According to the work by Shah, Mullainathan, and Shafir (2012) and Shah, Shafir, and Mullainathan (2015), the psychology of scarcity is not only driven by the actual state of scarcity; rather, a scarcity mind-set can be activated or deactivated dependent on the current *saliency* of scarcity. As a consequence, we should expect that shifting attention towards present scarcity should lower cooperation further. To test this proposition, we experimentally exposed a subset of participants' to a scarcity prime which intended to make the state of scarcity more salient. The prime was a survey section asking respondents questions about consumption, relative wealth, and food shortages they may have recently experienced. Half of the respondents played the Investment Game *after* answering these questions, whereas the other half played *before* being exposed to them.

In Table 6, we study the effect of food scarcity under the different experimental manipulations. In the full sample (column 1), we document a negative and significant relationship between scarcity and investments. When we restrict the sample to participants that were subject to the scarcity prime (column 2), the effect is substantially larger in magnitude, indicating that food scarcity matters especially when participants are primed on their current levels of consumption. In columns 3 and 4 we show that the interaction effect between food scarcity and the scarcity prime is negative but statistically insignificant. In conclusion, our findings indicate that priming participants on their current levels of consumption can aggravate the effect of food scarcity on investments, but the effect of the experimental manipulation is not estimated with sufficient precision.

Table 6: The moderating effects of the Scarcity- and Ingroup primes

Dep. Var.:	Investment in the Investment Game						
Manipulation:		Scarcity prime			Outgroup prime		
Sample::	Full (1)	Prime (2)	Full (3)	Full (4)	Outgroup (5)	Full (6)	Full (7)
Food scarcity	-138.4**	-214.0**	-63.98	-45.35	-179.8*	-89.55	-88.10
	(61.07)	(85.19)	(76.36)	(79.25)	(96.26)	(69.67)	(73.65)
Scarcity prime			90.05	102.6			
			(131.0)	(131.0)			
Scarcity prime × Food scarcity			-150.0	-172.1			
			(103.4)	(102.6)			
Outgroup prime						51.99	17.12
						(140.1)	(132.8)
Outgroup prime × Food scarcity						-90.22	-77.59
						(121.1)	(121.4)
Constant	3075.9***	3129.0***	3039.0***	3553.3***	3100.8***	3048.8***	3579.4***
	(69.57)	(84.77)	(103.6)	(386.8)	(90.22)	(105.7)	(386.5)
Observations	363	174	363	363	183	363	363
R-squared	0.0166	0.0390	0.0220	0.0498	0.0282	0.0186	0.0447
Dep. Var. Mean	2942.1	2896.6	2942.1	2942.1	2918.0	2942.1	2942.1
Controls	No	No	No	Yes	No	No	Yes

Cluster-robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6 displays OLS regression estimates of the effect of the Scaricty and Ingroup primes on investment in the Investment Game. Food scarcity ranges from 0 (no food scarcity in the past month) to 4 (constant food scarcity in the past month). Controls includes the following variables: (1) Age, (2) Years in Village, (3) Gender, (4) Tribe fixed effects, and (5) Religion fixed effects. All specifications report cluster-robust standard errors at the village-round level.

4.6.2 Ingroup differentiation

Finally, we study how the effect of scarcity on cooperation varies depending on the identity of the second player. Previous research has suggested that resource scarcity can enhance group differentiation and animosity (e.g. Krosch and Amodio 2014), and recent evidence points to the conclusion that scarcity could exacerbate the negative effects of diversity on cooperation (Schaub, Gereke, and Baldassarri 2020). In our context, such a mechanism would lead to food scarcity having a stronger negative effect on cooperation when the second player is from the outgroup compared to when the second player is part of the ingroup. To investigate this, we experimentally varied the identity of the second player between someone from *the local village* and from *another part of Tanzania*.[7]

We find only suggestive evidence that scarcity is more damaging for cooperation towards outgroup members. In column 5 of Table 6, we show that food scarcity is associated with *lower* investment levels when the second player is from the outgroup relative to the baseline sample (column 1). Put simply, participants that experience scarcity send less money on average, but the reduction is *larger* when they are matched with an outgroup member. To investigate whether the difference is statistically significant, we re-run the analysis over the entire sample and add an interaction term between the outgroup prime and being exposed to scarcity (column 6 and 7 of Table 6). While the interaction term is negative (indicating that people are less cooperative with outgroup members), it is imprecisely estimated and we cannot conclude that scarcity is more damaging for cooperation towards outgroup members.

In Appendix C.4, we also investigate how trust in the ingroup and outgroup, respectively, influences investment levels in the two experimental treatments. As expected, higher self-reported trust in the ingroup is associated with higher investment when the subjects are paired with an ingroup member. Similarly, higher outgroup trust increases investment when subjects are paired with an outgroup member. We next consider a measure of parochial trust, which we define as trust in the ingroup minus trust in the outgroup (see Figure A5), and introduce it in an interaction term with the ingroup treatment. We find that respondents who declare trusting ingroup members more than outgroup members are more cooperative

7. Since networks of support are often built within a village, this strategy captures a salient ingroup vs outgroup distinction.

with ingroup members in the game, and vice versa.

4.7 Does cooperation pay off?

Throughout the analysis, we have considered higher investment in the game as a positive result since it is the socially efficient option (the total payoff triples when invested). To conclude the results section, we investigate whether cooperation is also privately profitable for investors. Upon deciding how much to invest, Player A faces the risk that Player B may send back less than the invested amount. The degree to which the investment pays off, therefore, is conditional on reciprocation.

In Figure 6, we show the distribution of amounts returned by Player Bs when Player A invests 2,000 and 4,000 Tsh, respectively. The choices of Player Bs were elicited by means of the strategy method, meaning that participants did not know the actual sum invested but made conditional choices for the different possible scenarios.

Figure 6: Return on investment

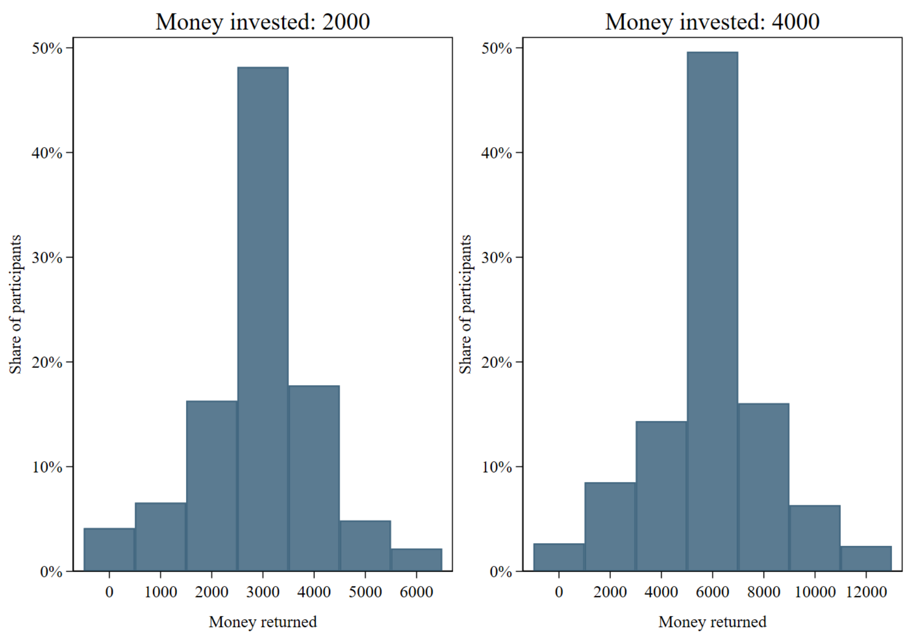


Figure 6 displays the distribution of money sent back by Player B for investments by Player A of 2,000 Tsh and 4,000 Tsh, respectively.

Figure 6 confirms that investing did pay off in expectation. For Player A, the decision to keep the money, e.g. not to invest, would result in a secure payoff of 4,000

Tsh. If half instead was invested, the expected payoff would be 4,922 Tsh (the 2,000 the participant did not invest, plus an average expected return of 2,922 Tsh). If all 4,000 Tsh were invested, the participant would in expectation receive a payoff of 5,931 Tsh. However, investing entailed risks, since Player B in some cases decided to send back less than the initial investment.

5 Discussion

The present study has shown that scarcity of food can depress socially efficient cooperation. Prior to the harvest – in a state of scarcity – farmers were less likely to make an investment that could benefit both themselves and another participant in the economic experiment. In line with Mullainathan and Shafir (2013), we hypothesize that this pattern is due to safer but low yielding options (not investing) becoming relatively more attractive in times of scarcity. The effect was lower realized payoffs of both senders and receivers in the lean season relative to the abundant season. The harvest served as a great (albeit temporary) leveler between rich and poor, both in terms of food consumption and in terms of investment behavior. In fact, after the harvest, the relatively poorer farmers were no less likely to invest.

Our findings bear important insights for both researchers and policy makers. Communal cooperation is essential for the efficient functioning of local economies, not least in rural developing contexts which lack strong formal institutions. Cooperation is conducive to economic growth, but may also be a by-product of improved economic conditions, in that economic slack enables people to afford the risks that cooperative behavior entails. This study supports the latter channel and is the first one, to the best of our knowledge, that documents a causal link between food scarcity and cooperation. If a shortage of food brakes down local networks of cooperation, this means that scarcity, even if temporary, can induce more scarcity in the future. By uncovering the detrimental impacts that deprivation can have on agents' willingness to invest, we make an important contribution to our understanding of behavioral poverty traps.

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Appendix

A Sampling strategy

In order to reduce idiosyncratic variation due to sampling error, we visited 12 villages from 12 different wards (a ward is a political unit consisting of several villages) in the first wave, and 14 villages in 14 different wards (12 of which were the same as in the preceding surveying wave) in the second wave. The 12 wards in the pre-harvest wave (6 from the Manyoni district and 6 from the Ikungi district, respectively) were selected by means of simple randomization from the universe of relevant and accessible wards in Ikungi (23 wards) and Manyoni (20 wards). 1 village was sampled from each of the selected wards and the resulting 12 villages constituted the sample of the first wave. In the second wave, we randomly selected 8 of the wards from the first wave and drew new villages to survey. This way, we ensured that the villages in the pre- and post-harvest samples were as similar as possible. Moreover, in order to allow for a within-subject analysis, we revisited the same villages in the remaining 4 wards and re-interviewed the same participants from the first round. Lastly, we complemented the second round with two randomly selected villages from wards that were not included in the first sample. The sampled wards and villages are shown in Table A1.

Table A1: Sample of villages and wards in the first and the second round

District	Ward	Village round 1	Village round 2
Ikungi	Sepuka	Musimi	Musimi
Ikungi	Iglansoni	Mnyange	Mnyange
Ikungi	Ighombwe	Ighombwe	Msosa
Ikungi	Ihanja	Ihanja	Chungu
Ikungi	Issuna	Tumaini	Ng'ongosoro
Ikungi	Mang'onyi	Mang'onyi	Sambaru
Ikungi	Mtunduru	–	Mtunduru
Manyoni	Kintinku	Kintinku	Kintinku
Manyoni	Sasilo	Chisingisa	Chisingisa
Manyoni	Chikola	Itetema	Winamila
Manyoni	Isseke	Igwamadete	Isseke
Manyoni	Mkwese	Kinyika	Mitoo
Manyoni	Muhalala	Kapiti	Muhalala
Manyoni	Makutopora	–	Mbwasa

Table A1 displays the sampled villages, as well as the wards and districts from which they are drawn.

B Experimental procedure

B.1 Location of the experiment and introduction

Enumerators visited participants in their homes, and found suitable locations for the interviews in the vicinity (a quiet place where the respondent could answer the questions without being disturbed or influenced by other family members). The interviews were conducted on tablets using KoBo, a survey software. Participants were informed that the survey was part of an international research project, but not about the research focus. Moreover, they were told that the survey included three games which would determine the payoffs they received. In total, participants could earn a minimum of 1,000 Tsh ($\approx$ 0.44 USD) and a maximum of 31,000 Tsh ($\approx$ 13.5 USD). The analysis in this game is focused entirely on the Investment Game.

B.2 Game instructions

The Investment Game was explained to respondents using the following script. The game was played at the beginning of a questionnaire (after some basic questions on demographic characteristics), except for the group that received our random prime. In that case, the game instructions followed a module with questions aimed at capturing scarcity of food and income.

Player A instructions: In this game, you are paired with another respondent from *YOUR VILLAGE / ANOTHER PART OF TANZANIA*. You will not know who this player is, and he/she will not know who you are, except that you are from the *SAME VILLAGE / ANOTHER PART OF TANZANIA*. We will simply call him or her Player B. You begin the game with 4000 Tsh, which are yours. You own a farm together with player B, who begins the game with 0 Tsh.
You have to decide how much money to spend on seeds. The seeds you will buy will be planted and produce a harvest. The harvest will be sold by Player B, who will decide how to divide the money between the two of you. You have the following three options:
(1) You buy 4 000 Tsh worth of seeds. This investment yields 12 000 Tsh when the harvest is sold. Player B then decides how these 12 000 are divided between the two of you. Player B can take as much from this sum as he/she wants, and what is left will be yours.
(2) You keep 2 000 Tsh and buy 2 000 Tsh worth of seeds. This investment yields 6 000 Tsh when the harvest is sold. Player B then decides how these 6 000 are divided between the two of you. Player B can take as much from this as he/she wants, and what is left will be yours. You will at a minimum receive the 2 000 you kept.
(3) You keep all 4 000 Tsh and do not buy any seeds. With no investment, Player B doesn't receive any money. You will receive the 4 000 that you kept.
Let's try to think of some examples:

- If you decide to buy 4,000 worth of seeds, how much does the investment yield?
- If you decide to buy 4,000 worth of seeds and Player B keeps 6 000 Tsh, how much do you get?
- If you decide to buy 4,000 worth of seeds and Player B keeps all the money obtained from selling the harvest, how much do you get?
- If you decide to buy 2,000 worth of seeds, how much does the investment yield?
- If you decide to buy 2,000 worth of seeds and Player B keeps 2 000 Tsh, how much do you get?
- If you decide to buy 2,000 worth of seeds and Player B keeps all the money obtained from selling the harvest, how much do you get?
- If you keep 4 000 Tsh and do not buy any seeds, how much do you get?
- If you keep 4 000 Tsh and do not buy any seeds, how much does Player B get?

Now - please make your decision. Would you like to invest 4 000 (Option 1), invest 2 000 and keep 2000 (Option 2), or keep your 4 000 Tsh and not invest (Option 3)?

Player B instructions: In this game, you are paired with another respondent from *YOUR VILLAGE / ANOTHER PART OF TANZANIA*. You will not know who this player is, and he/she will not know who you are, except that you are from the *SAME VILLAGE / ANOTHER PART OF TANZANIA*. We will simply call him or her Player A. You begin the game with 0 Tsh. You own a farm together with player A, who begins the game with 4 000 Tsh. Player A had to make a decision on how much money to spend on seeds by picking one of the following options:
(1) Buy 4 000 Tsh worth of seeds. If Player A chose this option, the investment would yield 12 000 Tsh when the harvest was sold. You decide how these 12 000 are divided between the two of you. Player A knew that you can decide to take as much from this sum as you want, and what is left will belong to him/her.
(2) Keep 2 000 Tsh and buy 2 000 Tsh worth of seeds. If Player A chose this option, the investment would yield 6 000 Tsh when the harvest was sold. You decide how these 6 000 are divided between the two of you. Player A knew that you can decide to take as much from this sum as you want, and what is left will belong to him/her (on top of the 2 000 he/she decided to keep).
(3) Keep all 4,000 Tsh for himself/herself, and not buy any seeds. If Player A chose this option, he/she knew that would receive 4 000, and that you would not receive any money.
Let's try to think of some examples:

- If Player A decided to buy 4 000 Tsh worth of seeds, how much did the investment yield?
- If Player A decided to buy 4 000 Tsh worth of seeds, and the harvest yielded 12 000, how much does Player A get if you keep 4 000 Tsh?
- If Player A decided to buy 2 000 Tsh worth of seeds, and the harvest yielded 6 000, how much does Player A get if you keep 2 000 Tsh?
- If Player A decided to buy 2 000 Tsh worth of seeds, how much did the investment yield?
- If Player A decided to buy 4 000 Tsh worth of seeds, and the harvest yielded 12 000, how much does Player A get if you keep 8 000 Tsh?
- If Player A decided to buy 2 000 Tsh worth of seeds, and the harvest yielded 6 000, how much does Player A get if you keep 4 000 Tsh?
- If Player A decided not to buy any seeds and keep all the 4,000 for himself/herself, how much do you get?

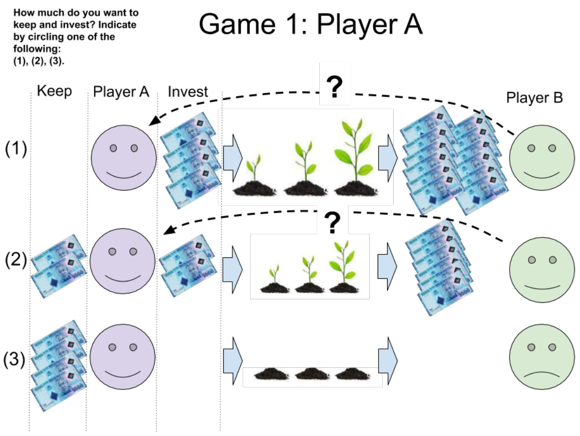
How would you divide the money from selling the harvest between the two of you, if Player A chose to buy 4 000 Tsh worth of seeds, which yielded 12 000?
How would you divide the money from selling the harvest between the two of you, if Player A chose to buy 2 000 Tsh worth of seeds, which yielded 6 000?

B.3 Visual aids

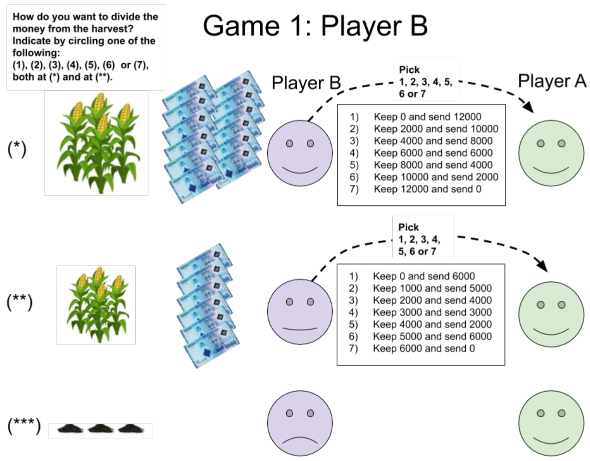
The game was explained by means of the visual aid shown in Figure A1. A copy of the visual aid was handed to the respondent and it also served the purpose of an answer sheet. Respondents were instructed to go to a private space and to make their decision by circling their preferred option. When this was done, they were told to fold the paper before returning it. The enumerator would then save the sheet, but not look at it in the presence of the participant.

Figure A1: Visual Assistance Investment Game

(a) Game Sheet Player A



(b) Game Sheet Player B



The survey also contained a Dictator Game and a Dice Game (to measure honesty). This paper focuses only on the Investment Game.

Figure A2: Questions contained in the Scarcity prime

- Over the past month, how often, if ever, have you or anyone in your family gone without any money left?
- Over the past month, how often, if ever, have you or anyone in your family gone without enough food to eat?
- Over the past month, how often, if ever, have you or anyone in your family gone without enough clean water for home use?
- Which of these periods is normally the worst for you in terms of food?
- Which of these periods is normally the worst for you in terms of net income (the food/cash you have after covering all your expenses)?
- How many meals does your household usually have per day?
- In the past 30 days has your household ever had fewer meals than this usual number?
- If Yes, how many days?
- In the past week how many days did the household consume meat or fish?
- How rich or poor is your household in comparison with other households in the village?

Figure A2 displays the survey items which constitute the scarcity prime.

B.4 Pre-registration and pilot study

The project was pre-registered in the American Economic Association registry for randomized controlled trials (ID: *AEARCTR-0005794*). The underlying power analysis was based on a pilot study conducted in 2 villages that were not part of the study sample.

C Results

C.1 Seasonal variation

Figure A3: Seasonal variation

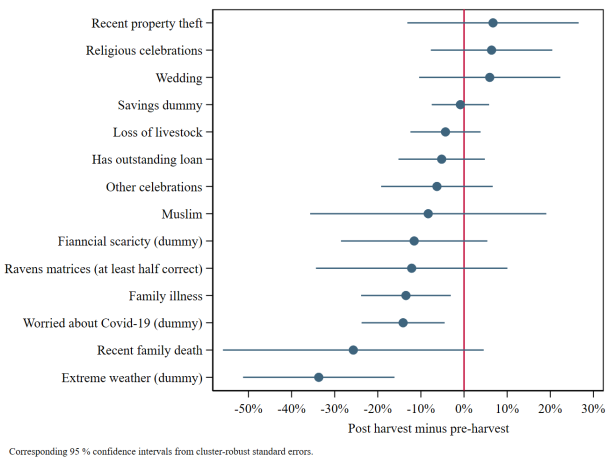


Figure A3 shows a number of potentially confounding factors that vary across the pre- and post-harvest samples. The confidence intervals are computed based on standard errors clustered at the village-round level.

Figure A4: Farmers have fewer meals than normally before the harvest

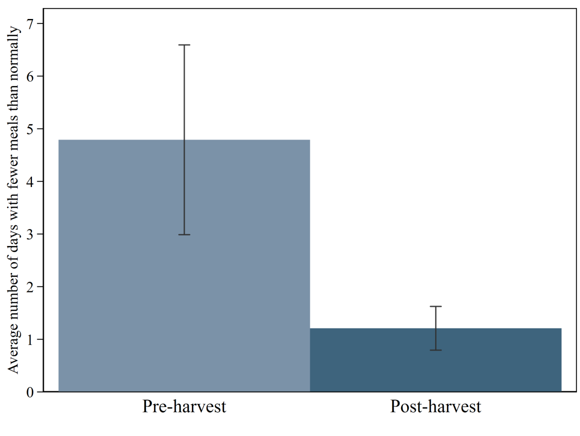


Figure A4 shows the average number of days over the previous month when the respondent's household had *fewer* meals than normal, before and after the harvest. The exact question was: "In the past 30 days has your household ever had fewer meals than this usual number? If Yes, how many days?". The confidence intervals are computed based on standard errors clustered at the village-round level.

C.2 Does food or financial scarcity matter?

In Table A2, we report regression estimates where investment is the dependent variable and food scarcity, as well as financial scarcity, the regressors. The results document a significant negative correlation between food scarcity and investment. In other words, participants that experience *more* food scarcity invest *less*. Conversely, there is no link between financial scarcity and investment. While the sign is negative, the estimated effects are both small in magnitude and statistically insignificant. This difference attests to the notion that food scarcity is a more severe form of scarcity (as per the discussion in Section 1 and in Schofield 2014).

Table A2: Correlations between scarcity and cooperation

Dep. Var.:	Investment			
	OLS (1)	OLS (2)	OLS (3)	OLS (4)
Food scarcity	-138.4**	-131.2**		
	(60.30)	(62.03)		
Financial scarcity			-33.14	-5.972
			(54.30)	(55.07)
Constant	3075.9***	3606.9***	3002.9***	3510.1***
	(75.28)	(373.4)	(110.1)	(387.9)
Observations	363	363	363	363
R-squared	0.0166	0.0429	0.00102	0.0287
Dep. Var. Mean	2942.1	2942.1	2942.1	2942.1
Controls	No	Yes	No	Yes

Cluster-robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A2 displays OLS regression estimates of Food scarcity and Financial scarcity, respectively, on investment in the Investment Game. Food and Financial scarcity range from 0 (no scarcity in the past month) to 4 (constant scarcity in the past month). The wording of the Financial scarcity survey item was: "Over the past month, how often, if ever, have you or anyone in your family gone without any money left?". Controls include the following variables: (1) Age, (2) Years in Village, (3) Gender, (4) Tribe fixed effects, and (5) Religion fixed effects. All specifications report cluster-robust standard errors at the village-round level.

C.3 First stage results

Table A3: Effect of the Harvest on Scarcity

Dep. Var.:	Food scarcity			Financial scarcity		
	Ologit (1)	OLS (2)	OLS (3)	Ologit (4)	OLS (5)	OLS (6)
Post-harvest Treatment	-1.631***	-0.920***	-0.932***	-0.606***	-0.393***	-0.394***
	(0.209)	(0.111)	(0.110)	(0.189)	(0.121)	(0.118)
Constant		1.456***	1.260***		2.041***	1.502***
		(0.0915)	(0.333)		(0.0849)	(0.350)
Observations	365	365	365	365	365	365
R-squared	–	0.164	0.217	–	0.0281	0.103
Dep. Var. Mean	0.962	0.962	0.962	1.830	1.830	1.830
Controls	No	No	Yes	No	No	Yes

Cluster-robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A3 displays Ologit and OLS regression estimates of the effect of the harvest on a measure of food and financial scarcity. Food and Financial scarcity range from 0 (no scarcity in the past month) to 4 (constant scarcity in the past month). The wording of the Financial scarcity survey item was: "Over the past month, how often, if ever, have you or anyone in your family gone without any money left?". Controls include the following variables: (1) Age, (2) Years in Village, (3) Gender, (4) Tribe fixed effects, and (5) Religion fixed effects. All specifications report cluster-robust standard errors at the village-round level.

C.4 Additional results

Figure A5: Parochial trust

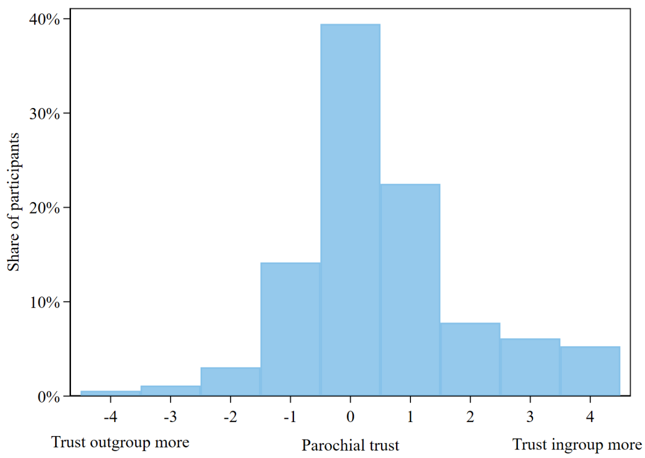


Figure A5 shows the distribution of parochial trust. Parochial trust is defined as trust in people from the village (a scale from 1 to 5) minus trust in people from other parts of Tanzania (also a scale from 1 to 5). In other words, positive parochial trust means that participants trust the ingroup more than the outgroup, and a negative number indicates the reverse.

Table A4: The association between trust and investment

Dep. Var.:	Investment in the Investment Game			
Sample:	Ingroup prime	Outgroup prime	Full	Full
	(1)	(2)	(3)	(4)
Ingroup trust	174.5***			
	(39.45)			
Outgroup trust		137.7**		
		(64.62)		
Ingroup prime			-75.50	-48.42
			(139.5)	(131.2)
Parochial trust			-147.7**	-137.7**
			(59.14)	(63.54)
Ingroup prime × Parochial trust			246.0***	245.0***
			(71.50)	(75.84)
Constant	2375.2***	2535.7***	2979.4***	3533.4***
	(135.5)	(231.2)	(100.8)	(375.3)
Observations	180	183	363	363
R-squared	0.0390	0.0191	0.0221	0.0499
Dep. Var. Mean	2966.7	2918.0	2942.1	2942.1
Controls	No	No	No	Yes

Cluster-robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A4 displays OLS regression estimates of the association between trust and investment. Ingroup trust is defined as trust in people from the village (a scale from 1 to 5), whereas outgroup trust as trust in people from other parts of Tanzania (also a scale from 1 to 5). Controls include the following variables: (1) Age, (2) Years in Village, (3) Gender, (4) Tribe fixed effects, and (5) Religion fixed effects. All specifications report cluster-robust standard errors at the village-round level.

C.5 Robustness checks

Table A5: Effect of scarcity on cooperation: additional controls table

Dep. Var.:	Investment in the Investment Game					
	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	OLS (6)
Post-harvest Treatment	234.6** (102.3)	244.4** (90.36)	240.5** (99.31)			
Food scarcity				-121.3* (64.56)	-118.6* (61.75)	-133.0** (57.90)
Constant	3112.3*** (115.4)	3634.5*** (332.2)	4932.9*** (789.5)	3355.0*** (88.99)	3865.2*** (324.2)	5237.2*** (796.2)
Observations	363	363	363	363	363	363
R-squared	0.0317	0.0637	0.117	0.0351	0.0658	0.121
Dep. Var. Mean	2942.1	2942.1	2942.1	2942.1	2942.1	2942.1
Ward F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Religion F.E.	No	Yes	Yes	No	Yes	Yes
Ethnic group F.E.	No	Yes	Yes	No	Yes	Yes
Age, Woman, Years in village	No	Yes	Yes	No	Yes	Yes
Education F.E.	No	No	Yes	No	No	Yes
Employment F.E.	No	No	Yes	No	No	Yes
HH head, HH adults, HH children	No	No	Yes	No	No	Yes

Cluster-robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Columns 1-3 of Table A5 display OLS regression estimates of the effect of the harvest on investment when subsequently adding a large battery of control variables. Similarly, columns 4-6 show the robustness of the relationship between food scarcity and investment levels. Food scarcity ranges from 0 (no scarcity in the past month) to 4 (constant scarcity in the past month). All specifications report cluster-robust standard errors at the village-round level.

Figure A6: Randomization inference

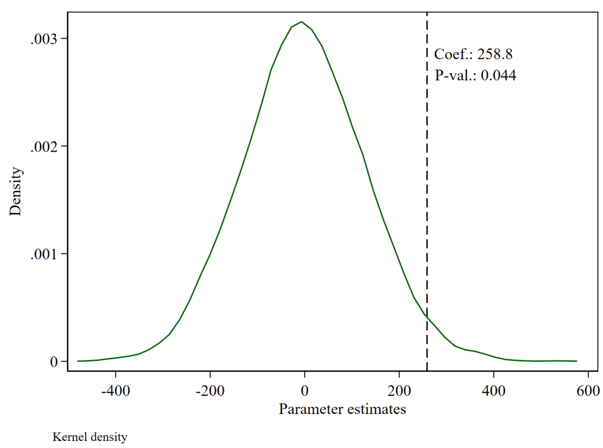


Figure A6 displays a kernel density from Randomization Inference estimations. The kernel is a distribution of post-harvest-betas obtained from 10,000 permutations of fictional treatment status. The vertical line shows the estimated effect of the actual treatment assignment, and the corresponding p-value indicates the probability that such an extreme value would be estimated by chance.

Chapter 4

Parochial Honesty and Market Exposure: Experimental Evidence from Greenland

Parochial Honesty and Market Exposure: Experimental Evidence from Greenland*

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Abstract

This article investigates the prevalence and determinants of parochial honesty, the tendency to behave more honestly toward members of the ingroup than toward outgroups. To this end, we conducted a large-scale experiment (N=543) on honesty in 13 villages across Greenland, where small and geographically isolated communities provide for a natural demarcation between ingroup and outgroup. In order to study group differentiation, we introduced an externality based on a moral decision, and randomly varied information about the identity of the aggrieved party. The results reveal significant parochial honesty: about 25% of the participants misreport either fully or to some extent when the aggrieved party is an outsider, but they completely refrain from misreporting when it negatively affects members of their local community. Exploiting variation in market integration within communities, we find that only participants who operate in the traditional economy differentiate between the ingroup and outgroup. This result supports the Market Integration Hypothesis, which posits that markets foster generalized prosocial norms through regular interaction with strangers.

Keywords: honesty, market exposure, field experiment
JEL Codes: O17, C93, D91

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1 Introduction

In many economic activities, exposure and sanctioning of rule breaking is unlikely[1], providing actors the opportunity to cheat to increase their monetary gain. Yet, most people are honest in most situations[2], suggesting that rule breaking is costly not only because of the threat of externally imposed sanctions, but also due to an internal code of conduct. In this study we investigate a factor that influences the code of conduct, namely the identity of the aggrieved party. To this end, we conducted a large-scale experiment designed to capture "parochial honesty", the tendency to behave more honestly toward the ingroup than toward the outgroup. Building on the literature on the relationship between economic institutions and norms (Henrich et al. 2001; Henrich et al. 2004; Henrich et al. 2010; Meier 2007; Baldassarri 2020), we further study whether variations in parochial honesty can be accounted for by individual variation in market exposure.

The notion that social identity matters for human behavior is not novel. Social Identity Theory was formulated by Tajfel et al. (1979) and posits that individuals perceive themselves and others along social categories such as age cohort, gender, professional categories, religious affiliation or community membership. By placing others as well as themselves into categories, people construct an "ingroup" composed of individuals sharing their own identity, and an "outgroup" consisting of all others. Categorization and identification is thought to promote parochialism, a mindset in which people favor members of their ingroup over the outgroup (Akerlof and Kranton 2000). It contrasts with a mentality in which individuals do not differentiate between in- and outgroup, commonly labeled universalism (Waytz et al. 2019). Over the past decades, scholars have documented how parochialism influences behaviors such as altruism, reciprocity and trust (Fershtman and Gneezy 2001; Buchan, Croson, and Dawes 2002; Buchan, Johnson, and Croson 2006; Chen and Li 2009; Leider et al. 2009), the willingness to cooperate (Eckel and Grossman 2005; Ruffle and Sosis 2006; Charness, Rigotti, and Rustichini 2007; Chen and Chen 2011), and engaging in

1. Consider for instance the second-hand market.

2. In a meta-analysis of 72 experimental studies on honesty, (Abeler, Nosenzo, and Raymond 2019) estimate that subjects forgo on average about three-quarters of the potential gains from cheating, even when they run no risk of being detected.

third-party punishment (Bernhard, Fischbacher, and Fehr 2006; Goette, Huffman, and Meier 2006; Mussweiler and Ockenfels 2013). Moreover, it has been shown that people positively discriminate their own group in public goods provision (Solow and Kirkwood 2002), charity giving (Croson and Shang 2008), and that they support redistributive schemes favoring their ingroup (Klor and Shayo 2010). Recently, social scientists have also turned to the role of parochialism as a determinant of honest conduct (Hruschka et al. 2014; Cadsby, Du, and Song 2016; Purzycki et al. 2018b; Benistant and Villeval 2019). Yet, the prevalence of parochial prosociality, as well as its determinants, remain contested (Baldassarri 2020).

There is a burgeoning literature showing how norms and social preferences respond to structural factors such as political (Becker et al. 2016; Hruschka et al. 2014; Lowes et al. 2017), religious (Shariff et al. 2016; Lang et al. 2019), and economic (Henrich et al. 2001; Henrich et al. 2004; Henrich et al. 2010; Gneezy, Leibbrandt, and List 2016) institutions. The idea that economic institutions shape norms and social preferences dates back at least to the 18th century (see Hirschman (1982) for a review of this literature). One strand of research has highlighted the destructive elements of markets, claiming that the ephemeral and impersonal nature of market relations erode the cornerstones of "nice behavior" (Bowles 1998; Falk and Szech 2013), and that market incentives crowd out prosocial motivations (Bénabou and Tirole 2006).[3] In contrast, another body of literature has argued that economic incentives need not crowd out prosocial motivations (Lacetera, Macis, and Slonim 2012, 2013), and that market interactions rather have a positive influence on prosocial behavior. This theory is henceforth referred to as the "Market Integration Hypothesis" (Henrich et al. 2010).

We refer to markets as the rule-based, monetized and impersonalized transactions that are prevalent in advanced economies. In impersonal markets, complete strangers interact and gain from mutually beneficial economic exchanges. Hence, market transactions demand faith in the other party's intentions and – according to the Market Integration Hypothesis – thereby promote prosocial norms to sustain mutual trust and cooperation. A number of empirical studies have supported this hypothesis (e.g. Henrich et al. (2001); Hen-

3. Bowles (1998) suggested that market integration should have pronounced effects on norms: "economic institutions influence the structure of social interactions and thus affect the evolution of norms by altering the returns to relationship-specific investments such as reputation-building, affecting the kinds of sanctions that may be applied in interactions, and changing the likelihood of interaction for different types of people".

rich et al. (2010); Baldassarri (2020)), showing that market integration positively correlates with average prosociality. Notably, however, none have documented how market exposure *differentially* influences prosocial behavior toward in- and outgroups.

In this chapter, we present results from field experiments on parochial honesty conducted in 13 villages across Greenland, a constituent country within the Kingdom of Denmark. We studied parochial honesty using the "Dice Game" introduced by Fischbacher and Föllmi-Heusi (2013), in which participants privately roll a die, report an outcome (truthfully or not), and receive a monetary payoff determined by the reported number. We introduced an externality based on the reported outcome, by passing the residual payoff (the maximum possible payoff minus the actual payoff) on to another participant in the experiment. Inflating one's own payoff thus entailed a negative externality on someone else. In order to test for the presence of parochial honesty, we randomly varied information about the externality and the identity of the externality recipient. In the first treatment (the No Externality treatment), the externality was not mentioned. In the second treatment (the Externality/No Identity treatment), the externality was mentioned but not the identity of the recipient. In the third treatment (the Externality/Ingroup Identity treatment), we informed participants both about the externality and their community affiliation (same village of residence) with the externality recipient.

Based on Social Identity Theory, we expected participants to exaggerate their outcomes less in the Externality/Ingroup Identity treatment compared to the treatments that did not refer to the shared community identity. Furthermore, the Market Integration Hypothesis posits that participants exposed to market institutions would differentiate less between the ingroup and outgroup. In line with these predictions, our results reveal significant parochial honesty, especially among participants that are less exposed to market institutions. One fourth of the participants inflated their payoffs when exposed to the No Externality- and the Externality/No Identity treatments[4], and the average reported outcome in these treatments was approximately 11% higher than the expected outcome in the absence of misreporting. In contrast, when participants were informed that the negative externality of

4. We estimate the share of dishonest participants by means of the statistical technique proposed by Garbarino, Slonim, and Villeval (2018).

exaggerating their outcome affected a resident from their own community, they completely refrained from misreporting.

We investigated the Market Integration Hypothesis by proxying for market participation in two ways: food source (wild foods- or market-based) and employment (employment in the "traditional" or "modern" sector). The results show that participants who are less exposed to market institutions displayed parochial honesty, in that they misreported against outsiders but acted honestly toward their ingroup, while participants in the market economy displayed generalized honesty, since they did not inflate their own payoffs either when playing against the in- or outgroups. The strong negative association between market participation and parochial honesty is significant also when comparing participants from the same villages, controlling for a wide range of potentially confounding socio-demographic characteristics, and when addressing issues of self-selection.

The present paper provides insights on the determinants of (dis)honest conduct and on the co-evolution of market integration and prosocial conduct. Similar to Henrich et al. (2001), Henrich et al. (2010), and Baldassarri (2020), we find a strong positive association between market exposure and generalized prosocial conduct. In addition, our findings highlight that, by impacting ingroup and outgroup prosociality differently, market exposure also influences group differentiation in prosocial behavior. While previous studies have relied on between-society variation, we exploit the stark contrasts in market exposure *within* Greenlandic villages, and are thus able to abstract from potentially confounding factors such as political and religious institutions.

Empirical setting

Greenland provides an ideal setting for the study of parochialism. The Greenlandic population resides in relatively small towns and settlements[5], which are isolated from each other in the sense that there are no interconnecting roads between localities. Instead, marine and air traffic constitute the means of inter-community transportation, making traveling both

5. Greenland Statistics classifies villages as either towns or settlements. The threshold distinguishing towns from settlements is approximately 500 inhabitants. The localities in our sample had a median population size of 856 in 2018, with a maximum of 17796 and a minimum of 71.

time-consuming and expensive. Consequently, life is organized at the village level and most Greenlanders identify strongly with their village of residence (Nuttall 2001; Dahl 1989)[6]. The salience and everyday relevance of the local community provides a natural demarcation between in- and outgroup, namely people from the village versus people from outside the village. We use this clear distinction to study how group identity and honest conduct interact.

The Greenlandic context is equally well-suited for testing the Market Integration Hypothesis, due to substantial variation in individual market exposure. The Inuit peoples that populated Greenland historically relied on hunting and fishing, as well as the associated food sharing practice, for subsistence (Dahl 1989; Nuttall 1991). These subsistence activities were organized in local networks that demanded constant interactions with community members, and little (if any) contact with non-community members (Dahl 1989; Nuttall 1991). In the mid-twentieth century, Danish authorities initiated a modernization and assimilation process, whereby rapid population growth and urbanization transformed much of Greenland (Rasmussen 2000). Today many Greenlanders operate in the market economy, in which actors frequently interact with, and rely on, outsiders. Yet, the traditional way of life remains a vital part of society, and subsistence activities provide the livelihood for a substantial share of the population.[7]

Participants in the traditional economy rely on their own as well their communities' catch[8] for subsistence. While hunters and fishermen sell their produce, these transactions are often personalized, e.g. sold through local markets called *kalaalimineerniarfik* (translated as "the place where Greenlandic foods are sold") or directly to households in the village of residence (Marquardt and Caulfield 1996). These transactions are regulated by sharing principles grounded in local traditions (Nuttall 2000; Poppel 2006). For instance, subsistence

6. Local identification is even reflected in the language: Greenlandic contains a suffix to indicate "a sense of identity from" a given town or settlement. A sense of local identity is expressed by the suffix -susseq (identity), so Qeqertarsuarmiut (person from Qeqertarsuaq) can have Qeqertarsuarmiussuseq, a sense of identity as Qeqertarsuarmiut (Nuttall 2001).

7. 4 out of 5 households in the settlements at least complement their food consumption by means of fishing or hunting (Poppel 2006).

8. As emphasized by e.g. Marquardt and Caulfield (Marquardt and Caulfield 1996), "non-monetized patterns of sharing and exchange within and between families and communities [that] continue to be important in Greenlandic society".

whaling requires strong cooperation (Henrich et al. 2001), and the sharing of large prey in "networks of close social association" is regulated by community-level norms (Nuttall 2000). Such cultural practices not only serve as communal welfare systems, but also strengthen the bonds between participants and their respective local communities (Appadurai and Fardon 1995).

Participants in the market economy operate in a completely different environment[9] and their subsistence, rather than obtained from nature or personalized transactions, requires market transactions (Poppel 2006). The market economy entails daily customer-vendor interactions governed by general principles and rules (Henrich et al. 2010). This feature contrasts with the traditional economy, in which transactions are based on social relations and sharing norms. The marked differences in organization of the traditional- and market-based economies allow us to test whether exposure to impersonal markets promotes generalized honesty.

Experimental design

To investigate parochial honesty and its determinants, we collected data from 13 localities across Greenland during July-September 2018. In order to ensure a geographically dispersed and demographically varied set of locations, we first stratified Greenland into 6 strata based on municipality borders[10], and sampled at least one settlement and one town from these strata. From this set of villages, the Statistical Agency of Greenland randomly selected participants such that the sample size of each locality approximately corresponds to the national population weight of the strata it represents. Section A in the Appendix details the sampling strategy. The sampled localities are mapped in Figure 1.

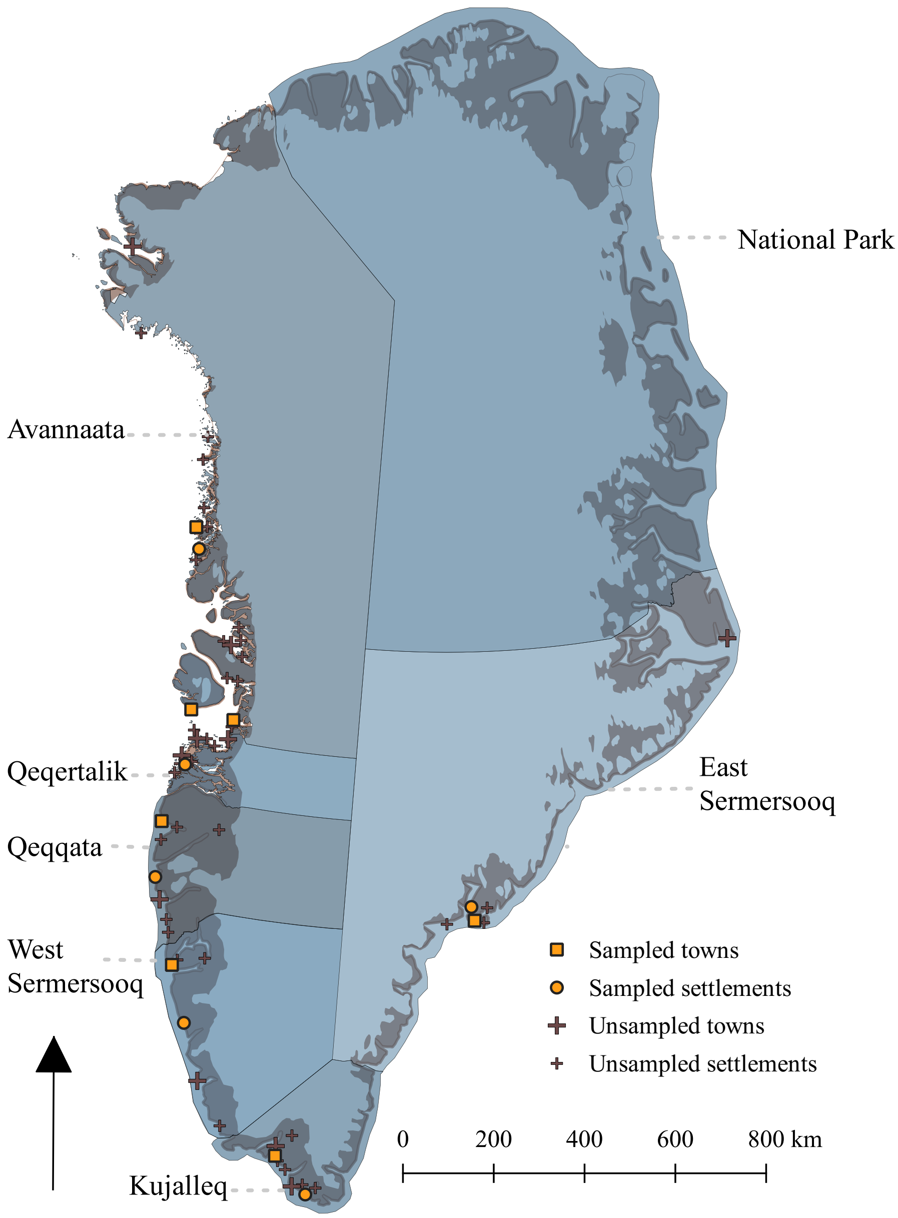
Selected individuals were invited to complete a survey and participate in economic experiments in field laboratories set up in schools and town halls in the sampled localities. We incentivized participation both by lottery enrollment and a monetary payoff in the economic

9. All towns and settlements are serviced by at least one supermarket and feature work opportunities in the market sector.

10. We made one exception to this rule by splitting the most populous and heterogeneous municipality, Sermersooq, into West- and East Sermersooq.

games. In order to increase the response rate, participants who did not show up at the field sessions were visited by research assistants, and completed both the survey and the Dice Game in their homes. Our final sample comprises 543 Greenlandic residents[11], out of which 210 participated in the field sessions. We control for experimental environment in the main specifications to ensure that it does not influence the results.

Figure 1: Map showing the sampled towns and settlements covered by the survey.



11. The gross sample was 1400, entailing a response rate of 39%.

Our experimental measure of honesty is the standard Dice Game (Fischbacher and Föllmi-Heusi 2013), in which participants privately roll a die to determine their payoffs (1=10 DKK/$1.5; 2=20 DKK/$3; 3=30 DKK/$4.5; 4=40 DKK/$6; 5=50 DKK/$7.5; 6=60 DKK/$9). The Dice Game was completed in a shielded space, such that nobody except participants themselves observed the outcome of the die roll. Since all outcomes of the six-sided die were equally likely, misreporting can only be inferred when aggregating outcomes to the group level. Thus, dishonesty cannot be detected at the individual level. This feature provided participants the opportunity to misreport anonymously, ensuring that reputational concerns and fear of punishment did not impact behavior. By abstracting from these alternative motives, we are able to study decisions regulated solely by internal motivations.

To study how honest conduct depends on group identity, we introduced an externality based on the reported outcome in the Dice Game. The residual money, the maximum possible payoff (60 DKK) minus the actual payoff, was passed on to another participant in the experiment. The first treatment, denoted the No Externality treatment, did not provide participants any information about the residual money. Although this treatment did not mention any externality, participants had been informed that the survey project was an independent research project supported by the University of Copenhagen and the University of Greenland, and misreporting might thus be perceived as taking from the project. The second treatment informed participants that higher reported outcomes were detrimental to another participant in the experiment but left out any reference to the local community (labeled Externality/No Identity). Since participants had been informed that they took part in a nation-wide survey project, the participant should perceive misreporting to be at the expense of another survey participant. In the third treatment, participants were informed that the residual money would be passed on to a resident from their own town or settlement. We label it the Externality/Ingroup Identity treatment, since it informed participants that inflating their payoff would harm the material interests of an ingroup member. Our distinction of treatments thus taps into the centrality of relationship information emphasized by Henrich et al. Henrich et al. 2010: "... measures of fairness in situations lacking relationship information (for example, anonymous others) should positively covary with market integration". Our distinction of treatments taps into the centrality of relationship information emphasized by Henrich et al. (2010): "... measures of fairness in situations lacking relationship information (for example, anonymous others) should positively covary with market integration". The full game instructions are provided in Section B of the Appendix.

Participants were assigned treatments by means of a randomized block design, which ensured that treatments were proportionally distributed both within and between villages. Appendix, Table A2 reports balance tests showing that the vast majority of relevant covariates are balanced as a consequence of randomization. Only with respect to age and a dummy for conducting the Dice Game at home do we observe imbalances between treatment groups. To ensure that these differences in background characteristics do not bias the results, both age and conducting the game at home are controlled for in all main specifications.

In order to investigate how market exposure influences rule breaking and differentiation, we construct two proxies for market participation. The first proxy, the "Diet proxy", follows Henrich et al. (2010) and indicates whether participants' food consumption is "wild foods-based" or "market-based". It is defined as wild foods-based if 50% or more of participants' food consumption is obtained by traditional subsistence methods such as hunting, fishing, gathering or sharing, and market-based if more than 50% of the food consumption comes from the market. In the coding of this variable, 9 participants were excluded due to missing data. Among included participants, 240 participants (44.94%) were coded as having a wild foods-based diet and 294 were coded as having a market-based diet (55.06%).

The second proxy, the "Employment proxy", closely aligns with the distinction of workplace organizations in Gneezy, Leibbrandt, and List (2016) , and indicates whether a participant works in the "traditional" or "modern" sector. Participants are coded as working in the modern sector if they indicated banking and finance, education, farming, fish production (industry), handicraft and design or retailing, health services, information technology, mining, public sector, transportation, tourism or "other employment" as current occupation. These occupations are characterized by frequent interactions with, and dependency on, external actors. Participants are labeled as working in the traditional sector if their occupation is fishing, hunting or boating, occupations which entail less exposure to impersonalized markets as well as a higher degree of self-sufficiency.[12] 198 participants were

12. As formulated by Dahl (1989): "Each individual hunter controls the primary process of production

excluded due to ambiguous occupation (students, unemployed and retirees) or because of missing data. Out of the remaining 345 participants, 104 (30.14%) were coded as having a "traditional occupation" and 241 (69.86%) were coded as having a "modern occupation".

Section E in the Appendix outlines the survey items used to construct the Diet and the Employment proxies, and in Figure A4 we display correlations between the two proxies. The only occupations that positively and significantly link with the Diet proxy are those coded as traditional occupations. In order to further validate our variables on market exposure, we link the survey data with Greenlandic register data on individual hunting licenses. Among the participants with a professional hunting license, 95.45% are coded as working in the traditional sector (compared with 23.1% of those who do not hold a professional hunting license; difference p-value< 0.001 using a two-sided t-test), and 91.7% are coded as having a wild foods-based diet (as reference, 39.5% of the participants that do not have a hunting license are labeled as having a wild foods-based diet; difference p-value< 0.001 using a two-sided t-test).

Results

In this section we present the experimental results on parochial honesty. We start by outlining how behavior in the Dice Game is contingent upon treatment status, and then turn to heterogeneous treatment effects based on participants' exposure to market institutions.

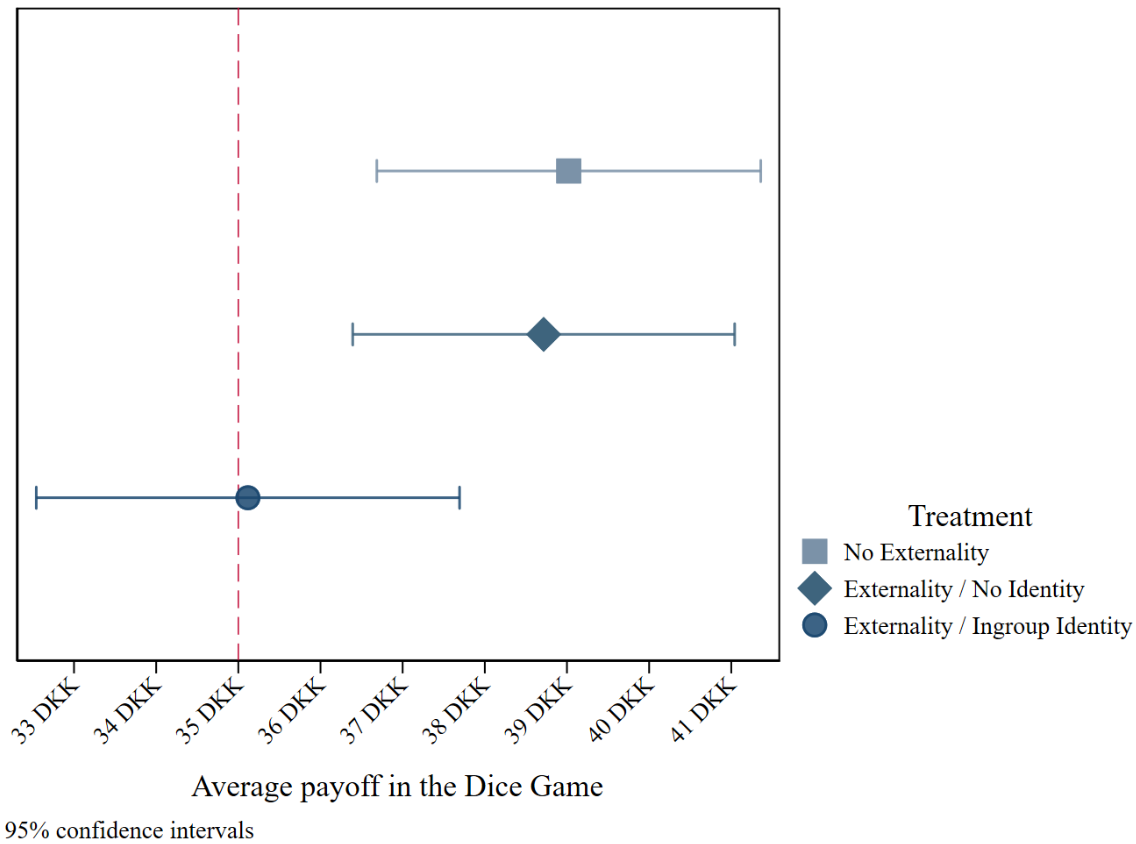
Parochial honesty

We first document parochial honesty in the full sample (N=543). In Figure 2, we plot the average reported die rolls with corresponding confidence intervals separately for each treatment. As is evident in Figure 2, participants' over-reported their outcomes in the No Externality and the Externality/No Identity treatments. The average payoffs were 39 DKK in the No Externality treatment and 38.7 DKK in the Externality/No Identity treatment, respectively 11.4% and 10.6% higher than the expected average under no exaggerated re-

(hunting, fishing) and also the means of production, among which the most important are boats..." (see also Rasmussen (Rasmussen 2000)).

porting (35 DKK). The group average of each treatment is significantly higher than 35 DKK (p-values$<$ 0.001 using one-sided t-tests), and we can thereby confidently conclude that participants over-reported in the treatments where exaggeration did not entail negative consequences for ingroup members. Meanwhile, in the Externality/Ingroup Identity treatment the average reported outcome was 35.1 DKK (statistically indistinguishable from 35 DKK, p-value$=$ 0.465 using one-sided t-test), and we can thus establish that participants in this treatment reported outcomes truthfully. In the Appendix, Figure A5 we plot the distributions of reported die rolls separately for each treatment. In the No Externality and the Externality/No Identity treatments participants are shown to be twice as likely to report the high (4, 5, 6) relative to the low (1, 2, 3) outcomes, whereas the distribution of reported outcomes is uniform for the Externality/Ingroup Identity treatment.

Figure 2: Coefficient plot depicting the average reported die rolls in the different treatments. The horizontal bars represent 95% confidence intervals based on robust standard errors.



We formally test the differences between the Externality/Ingroup Identity treatment and the other treatment groups by means of Ordinary Least Square (OLS) regressions presented in the Appendix, Table A4. All specifications confirm that participants report lower die rolls when misreporting affects an ingroup member. The difference is statistically significant at conventional levels in bivariate regressions, as well as when controlling for the variables that were imbalanced between treatments (Age and Game done at home). Since participants do not report lower outcomes in the Externality/No Identity treatment compared to the No Externality treatment (coefficient = -0.305, p-value = 0.873 in a bivariate regression), it appears to be the ingroup aspect, and not the externality awareness, that causes participants to refrain from over-reporting in the Externality/Ingroup Identity treatment. The differentiation is not due to group-directed altruism, as the die roll outcome is orthogonal to self-reported altruism,[13] which suggests that group-specific honesty norms explain the findings.

Next, we undertake two robustness checks to corroborate the baseline findings (Appendix, Section I). In Figure A6, we show that the effect of the Externality/Ingroup Identity treatment remains statistically significantly when using randomization inference (Gerber and Green 2012a). Finally, we link our experimental data to Greenlandic register data, and show In Table A5 that the estimated treatment effects are stable when controlling for objective data on income and education.

Market exposure and parochial honesty

We proceed to test how the degree of parochial honesty is contingent upon the economic institutions in which participants operate. According to the Market Integration Hypothesis (Henrich et al. 2010), market institutions promote prosocial behavior toward more socially distant people. We should thus expect less market-integrated participants to report higher outcomes in the outgroup treatments and to differentiate more between the in- and outgroup. To test this proposition, we leverage individual level variation in market exposure,

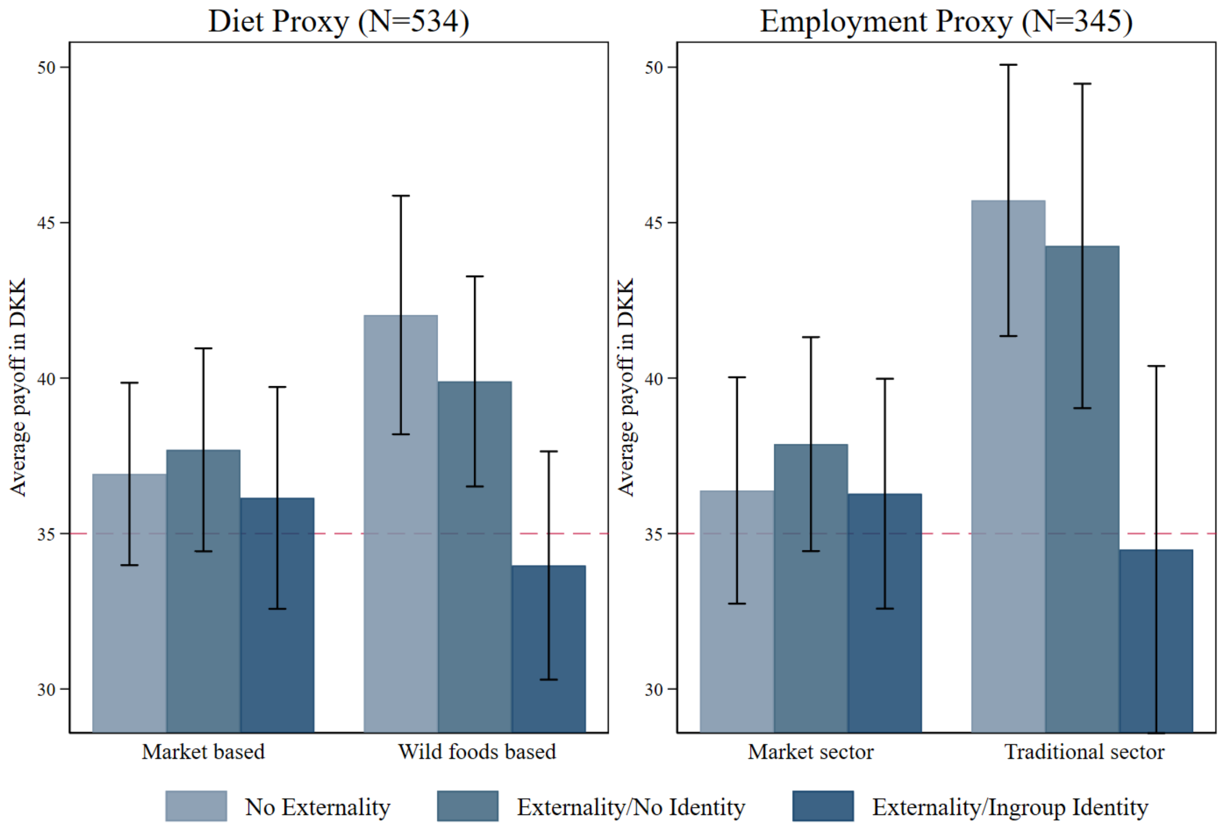
13. Coefficient= -0.047; p-value= 0.88; N=514, in a bivariate linear regression with reported die roll as the dependent variable and self-reported altruism (ranging from 1 to 10) as the independent variable (see variable definition in Table A1 in the Appendix).

and are thereby able to conduct analyses keeping village-level factors constant.[14] We later complement the baseline results with robustness checks which serve to alleviate concerns of omitted variable bias and endogeneity due to self-selection.

In Figure 3 we plot average reported die rolls by treatment status, separately for each category of market exposure. Note that the number of observations is smaller for the Employment proxy, as this sample excludes participants with ambiguous job categories (students, unemployed and retirees). The figure displays substantial behavioral differences between traditional and market-integrated participants. Participants classified as having a wild foods-based diet and working in the traditional sector report substantially higher outcomes in the outgroup treatments relative to the ingroup treatment. Market based participants, on the other hand, do not inflate their payoffs regardless of whether they interact with ingroup or outgroup members. In other words, the parochial honesty documented for the full sample is entirely driven by participants in the traditional economy; market-integrated participants display generalized honesty.

14. Market exposure as we define it – both in terms of consumption and production – varies mainly *within* villages: only around 15% of the total variation is due to between-village differences. A linear regression with the Employment Proxy as the dependent variable and village dummies as explanatory variables yields an R-squared of 0.153; a linear regression with the Diet Proxy as the dependent variable and village dummies as explanatory variables results in an R-squared of 0.144.

Figure 3: Average payoff in the Dice Game by treatments displayed separately for each category in the two proxies. The vertical bars represent 95% confidence intervals.



In the Appendix Table A6, we formally test the behavioral differences using OLS regressions[15]. Since market exposure is not independent of village characteristics, we report village cluster–robust standard errors (using the wild-bootstrap approach (Cameron, Gelbach, and Miller 2008; Roodman et al. 2019) to correct for the small number of clusters). First, we show that participants classified as traditional according to the Diet proxy reduce their reported outcomes when exposed to the Externality/Ingroup Identity treatment by 6.9 DKK in the Diet proxy (16.9% lower than in the outgroup treatments). Similarly, participants employed in the traditional sector reduce their reported die rolls by 10.5 DKK (23.4% lower compared to in the outgroup treatments) when exposed to the Externality/Ingroup Identity treatment. Conversely, the Externality/Ingroup Identity treatment does not in-

15. In all specifications, the reference group is a pooled sample of the No Externality treatment and the Externality/No Identity treatment.

cur any effect on reported die rolls among market-integrated participants. We proceed to study whether these differences are statistically significant by introducing an interaction term between the Externality/Ingroup Identity treatment and our proxies for market exposure. The regression results show that participants in the traditional economy report significantly higher outcomes in the outgroup treatments, and that reporting is substantially reduced when facing members of the ingroup. The magnitudes of the estimated coefficients are largely unchanged by the inclusion of village fixed effects, and when controlling for gender, age and the experimental environment.

Next, we investigate the sensitivity of the results to variable coding. We conduct a series of alterations to our preferred definitions of market- and traditional participants to ensure that arbitrary cutoffs do not generate false insights. In the Appendix, Table A7, we show that the magnitude of the Diet proxy coefficient increases when we narrow the definition of market participation. Consistent with this, Table A8 shows that when using a categorical definition of the Diet proxy, ranging from (1) *no food consumption based on wild foods* to (4) *most/all food consumption based on wild foods*, the general insight remains: participants less dependent on markets for subsistence tend to inflate their payoffs when facing the outgroup, but not when facing the ingroup. In the Appendix, Table A9 we consider alternative definitions of the Employment proxy. As expected, when we drop the most ambiguous employment categories (handicraft and design, fish production on land, transportation, farming, the "other" category, as well as boating and shipping), the magnitudes of the estimated coefficients are generally larger.

In what follows, we corroborate the baseline findings in a number of robustness checks. The results are summarized in Table 1 and detailed in the Appendix Section L. Traditional participants differ from market-integrated participants in a range of ways other than market exposure (see Appendix, Table A3), e.g. in terms of educational attainment and income. Given that these factors may, in turn, influence behavior in the Dice Game, the preceding interpretation of the findings might be premature. In order to ensure that differences in economic organization – and not differences in other factors – explain why participants in the traditional economy display parochial honesty whereas market-integrated participants exhibit generalized honest conduct, we proceed to rule out that any of the identified factors confound the analysis.

Table 1: Market exposure and parochial honesty: robustness checks

	Panel A: Diet proxy				
	Baseline estimates (1)	Additional controls (2)	Alternative explanations (3)	Excluding migration (4)	IV estimates (5)
Externality/Ingroup Identity	-1.127	-0.638	-0.433	0.366	1.736
	.717	.756	.92	.86	0.747
Wild foods-based diet	3.566	3.575	3.816	3.167	11.440
	.029	.053	.038	.14	0.086
Externality/Ingroup Identity	-5.739	-5.170	-6.984	-5.462	-13.361
× Wild foods-based diet	.035	.049	.005	.132	0.258
Village F.E	No	Yes	Yes	Yes	Yes
Surveyor F.E	No	Yes	Yes	Yes	Yes
Observations	534	406	455	188	394
R^2	.02	.134	.153	.131	
Mean of D.V	37.734	37.537	37.692	37.553	38.071
	Panel B: Employment proxy				
	Baseline estimates (1)	Additional controls (2)	Alternative explanations (3)	Excluding migration (4)	IV estimates (5)
Externality/Ingroup Identity	-0.835	0.611	3.106	4.147	1.303
	.792	.917	.603	.17	0.77
Traditional Employment	7.817	6.032	10.068	10.704	11.808
	.003	.154	.008	.041	0.054
Externality/Ingroup Identity	-9.616	-10.657	-13.825	-14.328	-18.67
× Traditional Employment	.021	.065	.003	.029	0.12
Village F.E	No	Yes	Yes	Yes	Yes
Surveyor F.E	No	Yes	Yes	Yes	Yes
Observations	345	279	303	121	239
R^2	.046	.173	.223	.232	
Mean of D.V	38.406	38.244	38.317	38.017	38.876

First, we include a more extensive set of control variables in the regression analyses. In Table A10 and A11, we add fixed effects for education, income, perceived income status, financial resilience, language, national identity, as well as controls for self-reported social preferences (trust, altruism, patience and risk-preferences). Second, we rerun the baseline regressions using controls on income and education obtained from official Greenlandic registers (Appendix, Table A12). Third, we undertake a more demanding test of omitted variable bias, by dropping from the analysis traditional participants with relatively low income and education, as well as market-integrated participants with relatively high income and education (Appendix, Figure A7). The insight that market exposure is associated with generalized honesty remains intact across all three robustness checks.

Next, we account for a set of institutional factors which have been linked with moral decision-making, namely religion, kinship structure, past exposure to political institutions, and media consumption. Religion has been claimed to promote the extension of moral behavior toward socially distant co-religionists (Purzycki et al. 2016; Purzycki et al. 2018a; Lang et al. 2019); kinship tightness to influencing moral behavior toward ingroup and outgroup differentially (Enke 2019); political institutions to incentivizing behavioral patterns e.g. by legal or regulatory means (Lowes et al. 2017; Becker et al. 2016); and media consumption to reducing moral barriers between ingroup and outgroup by connecting physically distant people (Hruschka and Henrich 2013). As shown in the Appendix Table A3, participants in the traditional economy are more religious, have tighter kinship networks, have been less exposed to Danish institutions, and are less likely to spend time on the Internet. In the Appendix Table A14 and A15, we progressively rule out confounding influence of each of these factors, and show that market exposure remains a key predictor of generalized honest conduct.

Finally, we consider the possibility that participants with more (less) parochial morality may have self-selected into the traditional (market based) economy. To alleviate concerns that selection influences the findings, we first exploit the fact that Greenlanders are, to some extent, tied to their home communities. 40% of the participants[16] resided in their birth village at the time of surveying. Due to village specific economic environments, these people were arguably more constrained in their occupational and lifestyle choices. Therefore, restricting the analysis to non-migrating participants should reduce bias due to selection. As shown in Table A16, the estimated coefficients remain largely intact when such restrictions are introduced.

A second concern, however, is that selection into the market or traditional economy is present within villages, and we therefore undertake an instrumental variable approach as a final check. Specifically, we use two pre-determined factors which influence whether participants chose a traditional lifestyle/occupation as instruments: (1) birth village population[17] and (2) a count variable for (number of) parents born in Greenlandic settlements (in which the traditional economy is more prevalent). While we cannot assure that these factors *did*

16. With valid data on village of birth.
17. Measured in 1977.

not shape parochial preferences through channels other than market exposure, exploiting pre-determined variation in market exposure enables us to circumvent endogeneity due to self-selection. Notwithstanding the respective weaknesses of the migration- and instrumental variable approaches as well as the noisy estimates, the fact that the qualitative insights align with the OLS regressions is reassuring. Taken together, the results suggest a causal interpretation of the findings.

Conclusion

The unique blend of the modern and traditional economy characterizing Greenland today enabled us to study how within-community variation in market exposure influences moral decision-making. Our findings render support to – as well as expand on – the Market Integration Hypothesis. We document a positive relationship between market integration and honest conduct, and thereby corroborate previous studies showing that market exposure increases average prosociality (Henrich et al. 2001; Henrich et al. 2004; Henrich et al. 2010; Baldassarri 2020). In addition, we identify a key role of markets in *generalizing* prosocial behavior, in that participants in the traditional economy exhibit parochial honesty, whereas market-integrated participants behaved equally honest toward all groups. Overall, our findings suggest that economic integration is conducive to social integration and cross-community cooperation. We provide tentative evidence of changing patterns of emotional attachment as a potential mechanism linking market exposure to generalized morality (Appendix, Section M). Future studies should focus more explicitly as to why markets promote generalized prosocial conduct.

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Appendix

A Sampling strategy

The population of Greenland is small, widely dispersed, and displays strong regional clustering. In order to obtain a sample that reflects well these geographic differences, we stratified the universe of localities in Greenland prior to sampling. Stratified sampling, as opposed to sampling by means of pure randomization, generally decreases sampling bias (Deaton 1997). Stratification accounts for regional heterogeneity by ensuring that each stratum is "represented" in the final sample.

- First, Greenland was divided into geographic strata. In 2018, there were 5 municipalities in Greenland: Sermersooq, Avannaata, Kujalleq, Qeqertalik and Qeqqata. These municipalities reflect the geographic clustering of institutional and economical differences rather well. The exception is Sermersooq, which covers both the East and West coasts. Due to the substantial institutional and linguistic divide between the two coasts[18], we decided to split this municipality into East and West, and used the resulting 6 geographic regions as the first level of stratification. Using the official administrative units reduces researcher discretion and thereby concerns of "convenience sampling".

- Second, the urban/rural-divide was accounted for by stratifying localities based on the categorization of villages as "settlements" or "towns" operated by Greenland Statistics (the cutoff between settlement and town is at approximately 500 inhabitants). Each of the geographic strata consist of at least two towns and a number of smaller settlements. We only considered localities with at least 50 inhabitants.

- Third, the selection of localities was implemented by randomly drawing one settlement and one town from each geographic strata. In order to ensure a comprehensive final

18. Exemplifying this division is the fact that different dialects of Greenlandic are spoken in West and East: Kalaallisut - or West Greenlandic - is spoken on the West coast, whereas Tunumiit Oraasiat - or East Greenlandic - is spoken on the East coast. West Greenlandic is however taught in schools also on the East Coast, and most of the inhabitants master it well.

sample , we made two exceptions to the within-stratum randomization. Due to the political, economic and demographic weight of the capital Nuuk, we decided to fix its inclusion in the final sample, and therefore did not randomly select a town from Sermersooq West. Furthermore, to account for the vast geographic reach of Avannaata, we also fixed the inclusion of Upernavik (a northern town of the municipality) in the final sample, in addition to the randomly selected locality, Ilulissat. The other 11 locations were randomly drawn from their respective stratum.

- Fourth, Statistics Greenland, the statistical organization of Greenland, randomly selected participants (aged 18 and above) from the universe of residents in each locality. The sample size of respective locality was determined by the relative adult population size of the stratum which the locality represented. Settlements were slightly oversampled, in order to ensure statistical power in these relatively smaller subpopulations.

The gross sample consisted of 1,400 adult residents. 543 of of those completed both the Dice Game and the survey, yielding a response rate of 38.8%. We exploited Greenlandic register data to validate the representativeness of our sample based on two relevant variables: age and professional hunting licenses. In the adult population of Greenland, the median age is 44 (mean 44.17) and 4.96% have a professional hunting license. In our sample, the median age is 48 (mean 46.23) and 5.32% are professional hunters. The fact that the share of hunters in our sample equals that of the total adult population suggests a successful random sampling and indicates that people in the traditional economy were no more difficult to recruit than market integrated participants.

B Experimental instructions

For participating in this questionnaire you will receive a small additional payoff. However, this payoff is not the same for every participant. You determine your own payoff by throwing your die once. The throw decides how much you receive. You can see the exact payoff from the following table:

Number thrown	1	2	3	4	5	6
Resulting payoff	10	20	30	40	50	60

The next sentence(s) varied by treatment.

No Externality treatment:

The maximum amount you can receive is 60 DKK.

Externality/No Identity treatment:

The maximum amount you can receive is 60 DKK. If your payoff is lower than 60 DKK, the remaining amount will be given to another person taking the survey. You will not know who this person is, and he or she will not know who you are.

Externality/Ingroup Identity treatment:

The maximum amount you can receive is 60 DKK. If your payoff is lower, the remaining amount will be given to another person from your town or settlement taking the survey. You will not know who this person is, and he or she will not know who you are.

If you have any questions, please contact the surveyor. If you are ready, please roll the die.

Please indicate the outcome of your die-roll below:

C Variables definitions

Table A1: Variable definitions

Label	Survey item	N
Woman	What is your gender? (**Female**; Male; Other)	543
Age	What is your age?	512
Game Done At Home	Enumerator indicates if survey was taken at a field session (0) or at participant's **home** (1)	543
Level of education	What is the highest level of education that you have achieved? (No education; Some years of primary school; Primary school; Currently at high school; High School; Vocational Training; Currently at the University; Bachelor; Master; Ph.D.)	522
Primary School	What is the highest level of education that you have achieved? (**No education**; **Some years of primary school**; **Primary school**; Currently at high school; High School; Vocational Training; Currently at the University; Bachelor; Master; Ph.D.)	522
High School/Professional	What is the highest level of education that you have achieved? (No education; Some years of primary school; Primary school; **Currently at high school**; **High School**; **Vocational Training**; Currently at the University; Bachelor; Master; Ph.D.)	522
University Degree	What is the highest level of education that you have achieved? (No education; Some years of primary school; Primary school; Currently at high school; High School; Vocational Training; **Currently at the University**; **Bachelor; Master; Ph.D.**)	522
Household income	What is your total annual household income, from all sources, before taxes? (0 - 100 000 DKK; 100 000 - 200 000 DKK; 200 000 -000 DKK; 300 000 - 400 000 DKK; 400 000 - 500 000 DKK; 500 000 - 1 000 000 DKK; 1 000 000 DKK or more)	511
HH Earnings < 200 K	What is your total annual household income, from all sources, before taxes? (**0 - 100 000 DKK**; **100 000 - 200 000 DKK**; 200 000 -000 DKK; 300 000 - 400 000 DKK; 400 000 - 500 000 DKK; 500 000 - 1 000 000 DKK; 1 000 000 DKK or more)	511
HH Earnings 200-500 K	What is your total annual household income, from all sources, before taxes? (0 - 100 000 DKK; 100 000 - 200 000 DKK; **200 000 -000 DKK**; **300 000 - 400 000 DKK**; **400 000 - 500 000 DKK**; 500 000 - 1 000 000 DKK; 1 000 000 DKK or more)	511
HH Earnings > 500 K	What is your total annual household income, from all sources, before taxes? (0 - 100 000 DKK; 100 000 - 200 000 DKK; 200 000 -000 DKK; 300 000 - 400 000 DKK; 400 000 - 500 000 DKK; **500 000 - 1 000 000 DKK**; **1 000 000 DKK or more**)	511
Perceived Inc. Status	Please imagine a ten-step ladder where on the first step, stand the poorest people in Greenland and on the highest step, the tenth, stand the richest people in Greenland. On which step of the ten is your household today? (1-10)	517
Financial Resilience	If, for one reason or another, you suddenly no longer receive earnings and/or transfers, how long would your household be able to get by before you run into financial problems? (Less than one week; Less than two weeks; Less than four weeks; Less than two months; Less than six months; Six months or more)	476
Language	Which language(s) do you speak? (Greenlandic; Danish; English; Other)	542
Identity	What do you identify yourself as? (Greenlandic; Both Greenlandic and Danish; Danish; Other)	534
Generalized Trust	Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people? (1-10)	529
Patience	How willing are you to give up something that is beneficial for you today in order to benefit more from that in the future? (1-10)	507
Risk Preference	How willing are you to take risks, in general? (1-10)	505
Altruism	How do you assess your willingness to share with others without expecting anything in return? (1-10)	514
Attend Church	How often do you go to church? (Never; Once a month or less; **2-3 times a month**; **Once per week**; **More than once a week**)	527
Traditional Beliefs	Are Inuit or Inughuit spiritual beliefs an important part of your life? (**Extremely important**; Very important; Somewhat important; Not too important; Not at all important)	523
Kinship tightness	Ratio of the number of relatives living in the local village to the total village population. Relatives encompass all first- and second degree relatives contained in the Greenlandic register data.	
Lived in Denmark	Have you ever lived in Denmark? (No (0); **Yes** (1))	531
Close relatives in Denmark	Do any of your family members live in Denmark now? (**Yes, parents**; **Yes, siblings**; **Yes, children**; Yes, other; No)	533
Media: internet	Which one of these news sources do you use the most to get your information? (Family and friends; **The internet on a computer**; **The internet on a mobile phone**, Local newspaper, Magazine, National newspaper, People in your community, Radio, TV)	537
Media: TV and radio	Which one of these news sources do you use the most to get your information? (Family and friends; The internet on a computer; The internet on a mobile phone, Local newspaper, Magazine, National newspaper, People in your community, **Radio**, **TV**)	537
Parochial Attachment	On a scale from 1 to 5, to what extent do you feel a weak or a strong sense of belonging to each of the following areas? (1-5) (Your town or settlement; Greenland; Denmark; The Earth/the whole world). Parochial Attachment is the attachment to the local village divided by the mean attachment to Greenland, Denmark and the world.	308

D Balance table

Table A2: Balance table

Variables	Sample Mean (1)	Externality/ Ingroup Mean (2)	(No Externality) - (Externality/Ingroup) (3)	(Externality/No Identity) - (Externality/Ingroup) (4)
Woman	0.527	0.494	0.011	0.083
	(0.500)	(0.501)	(0.053)	(0.053)
Age	46.23	45	3.753**	-0.130
	(15.05)	(14.91)	(1.643)	(1.621)
Game Done At Home	0.613	0.552	0.078	0.100*
	(0.487)	(0.499)	(0.052)	(0.052)
Primary School	0.477	0.449	0.051	0.032
	(0.500)	(0.499)	(0.054)	(0.054)
High School/ Professional	0.379	0.401	-0.029	-0.035
	(0.486)	(0.492)	(0.053)	(0.052)
University Degree	0.144	0.150	-0.022	0.003
	(0.351)	(0.358)	(0.038)	(0.038)
HH Earnings < 200 K	0.429	0.399	0.042	0.046
	(0.495)	(0.491)	(0.054)	(0.054)
HH Earnings 200 - 500 K	0.368	0.356	0.025	0.011
	(0.483)	(0.480)	(0.053)	(0.052)
HH Earnings > 500 K	0.204	0.245	-0.067	-0.057
	(0.403)	(0.432)	(0.045)	(0.045)
Wild foods-based diet	0.449	0.432	-0.023	0.073
	(0.498)	(0.497)	(0.053)	(0.053)
Wild foods-based diet (4-point scale)	4.65	4.592	-0.050	0.227
	(1.74)	(1.744)	(0.183)	(0.189)
Traditional Employment	0.301	0.271	0.026	0.062
	(0.460)	(0.447)	(0.060)	(0.061)
Perceived Inc. Status	5.064	5.192	-0.203	-0.175
	(1.823)	(1.869)	(0.201)	(0.196)
Generalized Trust	4.938	4.696	0.477	0.232
	(2.907)	(2.952)	(0.315)	(0.310)
Patience	6.229	6.281	0.168	-0.309
	(2.287)	(2.255)	(0.249)	(0.250)
Risk Preference	5.950	6.081	0.007	-0.385
	(2.373)	(2.253)	(0.255)	(0.259)
Altruism	6.578	6.576	-0.079	0.082
	(2.277)	(2.150)	(0.250)	(0.238)
Attend Church	0.186	0.174	0.033	0.003
	(0.389)	(0.380)	(0.042)	(0.041)
Traditional Beliefs	0.098	0.113	-0.040	-0.006
	(0.297)	(0.318)	(0.031)	(0.034)
Lived in Denmark	0.392	0.377	0.004	0.038
	(0.489)	(0.486)	(0.052)	(0.052)
Close relatives in Denmark	0.411	0.444	-0.035	-0.061
	(0.492)	(0.498)	(0.053)	(0.053)
Media: internet	0.678	0.700	-0.039	-0.026
	(0.468)	(0.460)	(0.050)	(0.049)
Media: TV and radio	0.777	0.776	-0.011	0.012
	(0.417)	(0.418)	(0.045)	(0.044)

Notes: Table A2 displays sample means as well as balance tests on relevant characteristics (all variables are described in Table A1) across treatments. In column 1 we report the mean value of each variable in the full sample and column 2 shows the mean in the Externality/Ingroup Identity treatment group. Standard deviations are reported in parentheses. In columns 3 and 4, we test for mean differences between the Externality/Ingroup Identity treatment and, respectively, the No Externality treatment and the Externality/No Identity treatment in bivariate regressions. Standard errors are reported in parentheses. * (p<0.10), ** (p<0.05), *** (p<0.01).

E Market exposure proxies: definitions

Figure A1: Diet proxy: dichotomous

If a participant replied 3 or 4 to at least one of the following two questions, she is coded as having a wild foods-based diet.

How much of your family's diet comes from wild foods you hunt, fish, or gather for yourselves?

1. None of it.
2. Some of it.
3. **Half of it.**
4. **Most or all of it.**

How much of your family's diet comes from wild foods that other people in your town or village share with you?

1. None of it.
2. Some of it.
3. **Half of it.**
4. **Most or all of it.**

Figure A2: Diet proxy: 4-point scale

The categories are defined as follows:

- **Wild foods-based diet: none**: participant answered 1 to both questions in Figure A1.
- **Wild foods-based diet: at least some**: answered 2 (but not higher) to one of the questions in Figure A1.
- **Wild foods-based diet: at least half**: answered 3 (but not higher) to one of the questions in Figure A1.
- **Wild foods-based diet: most or all**: answered 4 to at least one of the questions in Figure A1.

Figure A3: Employment proxy

Occupations in bold text are coded as traditional, in normal text as modern and the occupations in italics were excluded due to their ambiguous categorization.

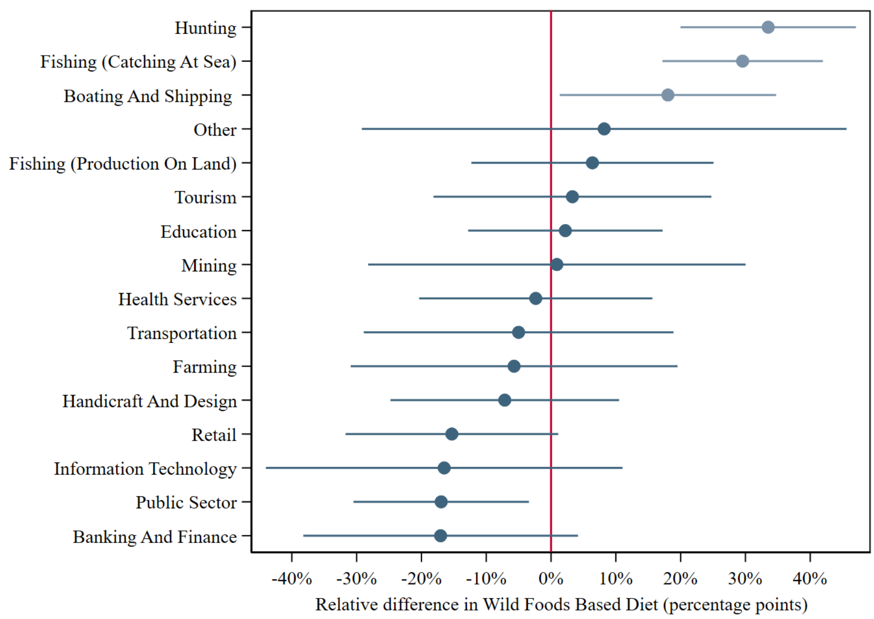
What best describes your current occupation?

- **Boating and shipping**
- Banking and finance
- Education
- Farming
- **Fishing (catching at sea)**
- Fishing (production on land)
- Handicraft & design
- Health services
- **Hunting**
- Information technology (IT)
- Mining
- Public Sector
- *Retired or pensionist*
- Retail
- *Student*
- Tourism
- Transportation
- *Unemployed*
- Other

F Proxy correlations

We proceed to show correlations between the proxies on market exposure. Although the proxies capture different aspects of market exposure, consumption and production related exposure respectively, we should expect a positive correlation between the two variables. To investigate this, we plot differences in the likelihood of having a wild foods-based diet for all occupations. The coefficients are estimated by means of bivariate OLS regressions including all respondents contained in the survey with non-missing data for both the Diet and the Employment proxies (N=408). The estimates are plotted in Figure A4.

Figure A4: Correlation between Employment and Diet proxies



Corresponding 95 % confidence intervals from robust standard errors.

G Market-integrated and traditional participants

Table A3 displays variable means and average differences in relevant characteristics between market integrated and traditional participants. All variables are described in Table A1. The tests are conducted by means of bivariate regressions. In columns 1, 2, 4 and 5, standard deviations are reported in parentheses. In columns 3 and 6, standard errors are reported in parentheses. * (p<0.10), ** (p<0.05), *** (p<0.01).

Table A3: Average differences between market integrated and traditional participants

Variables	Modern Employment (1)	Traditional Employment (2)	Difference (3)	Market based diet (4)	Wild foods based diet (5)	Difference (6)
Woman	0.560	0.269	0.291***	0.503	0.554	-0.051
	(0.497)	(0.446)	(0.057)	(0.501)	(0.498)	(0.043)
Age	44.96	46.53	-1.569	45.45	47.15	-1.698
	(12.37)	(15.21)	(1.598)	(14.31)	(15.47)	(1.328)
Game Done At Home	0.585	0.654	-0.069	0.551	0.688	-0.136***
	(0.494)	(0.478)	(0.057)	(0.498)	(0.464)	(0.042)
Primary School	0.294	0.709	-0.415***	0.415	0.552	-0.136***
	(0.457)	(0.457)	(0.054)	(0.494)	(0.498)	(0.044)
High School/ Professional	0.479	0.252	0.227***	0.422	0.328	0.095**
	(0.501)	(0.437)	(0.057)	(0.495)	(0.470)	(0.043)
University Degree	0.227	0.0388	0.188***	0.163	0.121	0.042
	(0.420)	(0.194)	(0.043)	(0.370)	(0.326)	(0.031)
HH Earnings < 200 K	0.237	0.485	-0.248***	0.377	0.487	-0.110**
	(0.426)	(0.502)	(0.054)	(0.485)	(0.501)	(0.044)
HH Earnings 200 - 500 K	0.461	0.347	0.115*	0.395	0.338	0.057
	(0.500)	(0.478)	(0.06)	(0.490)	(0.474)	(0.043)
HH Earnings > 500 K	0.302	0.168	0.133**	0.228	0.175	0.053
	(0.460)	(0.376)	(0.052)	(0.420)	(0.381)	(0.036)
Wild foods-based diet	0.382	0.673	-0.291***			
	(0.487)	(0.471)	(0.057)			
Wild foods-based diet (4-point scale measure)	4.504	5.452	-0.948***	3.349	6.237	-2.888***
	(1.652)	(1.811)	(0.200)	(0.800)	(1.174)	(0.086)
Traditional employment				0.186	0.432	-0.246***
				(0.390)	(0.497)	(0.048)
Perceived Inc. Stat	5.595	4.515	1.080***	5.129	4.975	0.154
	(1.686)	(1.770)	(0.202)	(1.730)	(1.922)	(0.161)
Financial resilience: < 6 months	0.425	0.520	-0.096	0.498	0.495	0.003
	(0.495)	(0.502)	(0.060)	(0.501)	(0.501)	(0.046)
Financial resilience: 1 - 6 months	0.327	0.255	0.072	0.281	0.259	0.022
	(0.470)	(0.438)	(0.056)	(0.451)	(0.439)	(0.041)
Financial resilience: > 6 months	0.248	0.224	0.023	0.221	0.245	0.022
	(0.433)	(0.419)	(0.052)	(0.415)	(0.431)	-0.025
Language: only Greenlandic	0.212	0.510	-0.298***	0.221	0.487	-0.266***
	(0.409)	(0.502)	(0.052)	(0.416)	(0.501)	(0.040)
Language: Greenlandic and Danish/ English	0.685	0.471	0.213***	0.680	0.496	0.184***
	(0.466)	(0.502)	(0.056)	(0.467)	(0.501)	(0.042)
Language: only Danish/ English	0.104	0.0192	0.085***	0.0986	0.0167	0.082***
	(0.306)	(0.138)	(0.031)	(0.299)	(0.128)	(0.021)
Identity: Greenlandic	0.802	0.961	-0.159***	0.820	0.941	-0.121***
	(0.400)	(0.194)	(0.041)	(0.385)	(0.236)	(0.029)
Identity: Greenlandic and Danish	0.131	0.0291	0.102***	0.114	0.0506	0.064***
	(0.338)	(0.169)	(0.035)	(0.319)	(0.220)	(0.024)
Identity: only Danish	0.0675	0.00971	0.058**	0.0657	0.00844	0.057***
	(0.251)	(0.0985)	(0.026)	(0.248)	(0.0917)	(0.017)
Generalized Trust	5.332	4.610	0.722**	5.210	4.595	0.615**
	(2.813)	(2.895)	(0.338)	(2.731)	(3.084)	(0.253)
Patience	6.538	5.980	0.558**	6.234	6.219	0.015
	(2.022)	(2.615)	(0.265)	(2.207)	(2.389)	(0.205)
Risk Preference	6.140	6.109	0.031	5.939	5.960	-0.022
	(2.182)	(2.514)	(0.272)	(2.294)	(2.475)	(0.213)
Altruism	6.803	6.392	0.410	6.539	6.611	-0.073
	(2.119)	(2.389)	(0.261)	(2.240)	(2.319)	(0.202)
Attend Church	0.112	0.284	-0.172***	0.135	0.248	-0.113***
	(0.316)	(0.453)	(0.043)	(0.342)	(0.433)	(0.034)
Traditional Beliefs	0.0708	0.121	-0.050	0.0623	0.141	-0.079***
	(0.257)	(0.328)	(0.033)	(0.242)	(0.349)	(0.026)
Kinship	0.007	0.025	-0.018***	0.008	0.023	-0.015***
	0.019	0.051	0.004	0.029	0.047	0.004
Lived in Denmark	0.535	0.194	0.341***	0.478	0.289	0.189***
	(0.500)	(0.397)	(0.056)	(0.500)	(0.454)	(0.042)
Close relatives in Denmark	0.441	0.385	0.057	0.440	0.377	0.063
	(0.498)	(0.489)	(0.058)	(0.497)	(0.486)	(0.043)
Media: internet	0.780	0.635	0.145***	0.720	0.637	0.083**
	(0.415)	(0.484)	(0.051)	(0.450)	(0.482)	(0.040)
Media: TV and radio	0.768	0.827	-0.059	0.785	0.775	(0.040)
	(0.423)	(0.380)	(0.048)	(0.412)	(0.418)	(0.036)

H Parochial honesty: baseline results

The distributions of reported die rolls in the three treatments are depicted in Figure A5. In the absence of misreporting, we should see uniform distributions with each outcome reported approximately 16.7% of the time. In the No Externality treatment and the Externality/No Identity treatment, participants were almost twice as likely to report the high outcomes as they were to report the low outcomes (two-sided binomial tests confirm that the likelihood of 4, 5 or 6 being reported is significantly higher than 50% in the No Externality treatment (62.50%, p-value<=0.001) and in the Externality/No Identity treatment (62.03%, p-value<=0.01). Conversely, the flat distribution of outcomes reported in the Externality/Ingroup Identity treatment strongly suggests that participants in this treatment reported their outcomes truthfully.

Figure A5: Histograms showing the distribution of reported outcomes by treatment. The dashed horizontal lines indicate the expected percentage of each outcome in the absence of misreporting.

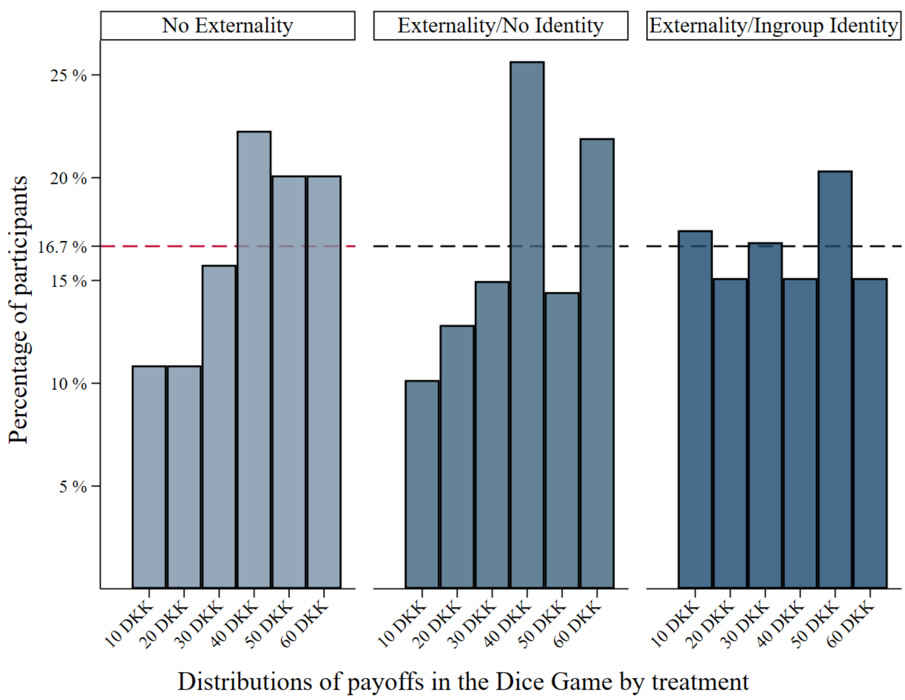


Table A4 displays OLS regressions on the effect of the Externality/Ingroup Identity treatment on reported payoffs. In columns 1 and 2, the comparison group is the No Externality treatment. In columns 3 and 4, the comparison group is the Externality/No Identity treatment. In columns 5 and 6, both the No Externality treatment and the Externality/No Identity treatment constitute the reference. The payoffs are reported in Danish Kroner (DKK). "Age" is the participants' age, "Game done at home" is a dummy variable equal to 1 if the Dice Game was conducted at participants' homes, and 0 if it was played at one of the field sessions. P-values based on robust standard errors are reported in parentheses.

Table A4: Treatment effect

	Reference No Externality treatment		Reference Externality/No Identity treatment		Reference Pooled sample	
	(1)	(2)	(3)	(4)	(5)	(6)
Externality/Ingroup Identity	-3.905**	-3.883**	-3.600**	-3.028*	-3.752**	-3.573**
	(0.027)	(0.034)	(0.041)	(0.099)	(0.016)	(0.027)
Age		0.053		0.021		0.010
		(0.363)		(0.731)		(0.831)
Game done at home		1.640		2.714		1.769
		(0.385)		(0.156)		(0.248)
Observations	356	335	359	338	543	512
R^2	0.014	0.020	0.012	0.016	0.011	0.014
Mean of D.V.	37.135	37.075	36.992	36.627	37.680	37.461

I Parochial honesty: robustness checks

I.1 Randomization Inference

We next demonstrate the robustness of the estimates to Randomization Inference[19], a non-parametric technique which relaxes the assumption of normally distributed errors invoked in standard regressions (see Gerber and Green 2012b for a detailed description). Randomization Inference randomly assigns "placebo treatments" to participants and estimate the placebo treatment effect. This exercise is repeated 10,000 times (permutations) so that we obtain distributions of the placebo treatment effects. The number of placebo treated participants in each permutation corresponds to the number of participants in the Externality/Ingroup Identity treatment. In the same manner as the actual treatment assignment, the placebo treatments are block randomized on the village level using the "strata option".

The probability of obtaining the *actual* treatment effect by chance is calculated by comparing the estimated treatment effects with the placebo treatment effects. The p-value derivation is expressed mathematically as:

$$\frac{k}{10,000} \tag{1}$$

where

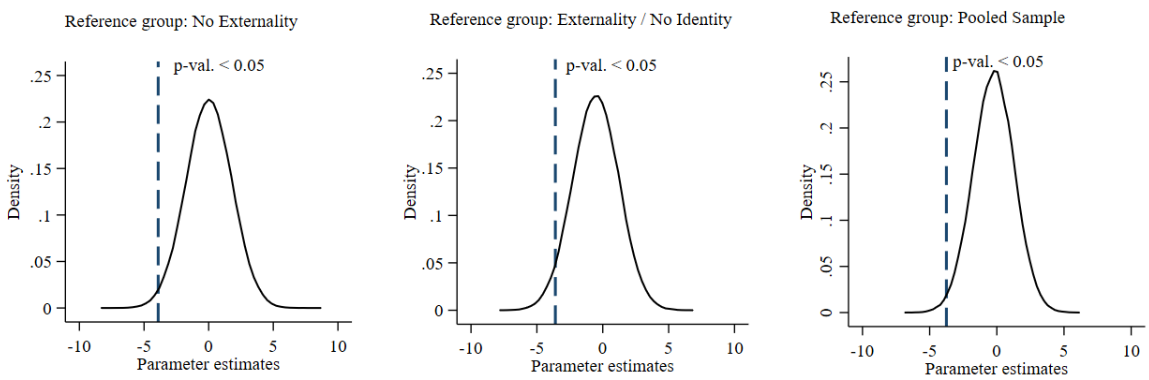
$$k = \sum_{m=1}^{10,000} 1(TE_i^{placebo}| \geq |TE_i^{actual}|\ ) \tag{2}$$

or in words as *the proportion of times that the absolute value of the placebo treatment effect is larger than the absolute value of the actual treatment effect.*

Figure A6 shows distributions of parameter estimates from 10,000 permutations of placebo treatments. The vertical lines represent the actual treatment effects. The p-values from two-sided Randomization Inference simulations are almost identical to the p-values based on standard regressions (Reference No Externality treatment: p-value=0.026; Reference Externality/No Identity treatment: p-value=0.0475; Reference Pooled Sample: p-value=0.015).

19. We execute the Randomization Inference test using the Stata package ritest.

Figure A6: Randomization Inference



I.2 Register-based controls

In order to ensure that the results are invariant also to objective data on income and education, we linked 418 participants to Greenlandic register data (125 participants could not be linked due to missing identifying information). Table A5 displays OLS regression results on the effect of the Externality/Ingroup Identity treatment on reported payoffs using register-based data on education and income. In columns 1 and 2, the comparison group is the No Externality treatment. In columns 3 and 4, the comparison group is the Externality/No Identity treatment. In columns 5 and 6, both the No Externality treatment and the Externality/No Identity treatment constitute the reference group. The payoffs are reported in Danish Kroner (DKK). "Age" is the participants' age, "Game done at home" is a dummy variable equal to 1 if the Dice Game was conducted at participants' homes, and 0 if it was played at one of the field sessions. P-values based on robust standard errors are reported in parentheses.

Table A5: Treatment effect when using register based controls

	Reference No Externality treatment		Reference Externality/No Identity treatment		Reference Pooled sample	
	(1)	(2)	(3)	(4)	(5)	(6)
Externality/Ingroup Identity	-3.432*	-3.142	-3.691*	-3.547*	-3.555**	-3.514**
	(0.081)	(0.120)	(0.069)	(0.092)	(0.040)	(0.045)
Age		0.096		-0.021		0.021
		(0.154)		(0.763)		(0.706)
Game done at home		2.095		1.877		1.590
		(0.293)		(0.377)		(0.341)
Secondary School		-2.057		-4.094		-2.316
		(0.411)		(0.145)		(0.266)
Higher Education		4.641		3.735		3.130
		(0.144)		(0.242)		(0.220)
HH Earnings 100-200 K		-2.149		1.391		0.100
		(0.486)		(0.705)		(0.972)
HH Earnings 200 - 300 K		-4.120		3.368		-0.561
		(0.226)		(0.460)		(0.860)
HH Earnings 300 - 400 K		-4.586		4.906		0.867
		(0.244)		(0.262)		(0.795)
HH Earnings 400 - 500 K		-4.572		2.959		-0.358
		(0.237)		(0.525)		(0.919)
HH Earnings 500 - 1000 K		-2.457		4.223		1.590
		(0.484)		(0.301)		(0.608)
HH Earnings > 1000 K		-8.315		-1.844		-3.347
		(0.122)		(0.748)		(0.452)
Observations	288	288	274	272	420	418
R^2	0.011	0.040	0.012	0.050	0.010	0.026
Mean of D.V.	36.458	36.458	36.496	36.581	37.071	37.129

J Market exposure and parochial honesty

Table A6 shows the interaction between the Externality/Ingroup Identity treatment and exposure to market institutions. In Panel A, exposure to market institutions is proxied by a dummy indicating if at least 50% of the participant's diet is based on wild foods. In Panel B, exposure to market institutions is proxied by a dummy indicating whether the participant works in the traditional sector (hunting, fishing, or boating and shipping). In columns 1 and 2, the effect of the Externality/Ingroup Identity treatment is showed separately for market-integrated and non market-integrated participants, and in columns 3 to 7, exposure to market institutions is interacted with the Externality/Ingroup Identity treatment. In all specifications, both the No Externality treatment and the Externality/No Identity treatment constitute the reference group. The unit is Danish Kroner (DKK). The p-values are based on village level cluster-robust standard errors using the wild-bootstrap procedure (Cameron, Gelbach, and Miller 2008; Roodman et al. 2019).

Table A6: Market exposure and parochial honesty

Panel A: Diet proxy	Market-based diet	Wild foods-based diet	Full sample				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Externality/Ingroup Identity	-0.127	-6.866	-1.127	-1.243	-1.032	-1.057	-0.939
	.717	.015	.717	.698	.725	.826	.843
Wild foods-based diet			3.566	3.340	3.508	3.345	3.290
			.029	.031	.032	.025	.027
Externality/Ingroup Identity × Wild foods-based diet			-5.739	-5.556	-6.284	-5.804	-5.911
			.035	.03	.019	.013	.013
Woman					-0.745	-0.345	-0.381
					.64	.833	.807
Age						0.008	0.006
						.86	.874
Game done at home							1.168
							.348
Village F.E	No	No	No	Yes	Yes	Yes	Yes
Surveyor F.E	No	No	No	No	Yes	Yes	Yes
Observations	294	240	534	534	534	504	504
R^2	.006	.036	.02	.04	.06	.058	.059
Mean of D.V.	36.905	38.75	37.734	37.734	37.734	37.5	37.5

Panel B: Employment proxy	Modern occupation	Traditional occupation	Full sample				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Externality/Ingroup Identity	-0.835	-10.451	-0.835	-0.363	-0.241	0.061	0.125
	.792	.013	.792	.906	.948	.987	.973
Traditional employment			7.817	8.008	7.890	7.760	7.805
			.003	.003	.007	.01	.012
Externality/Ingroup Identity × Traditional employment			-9.616	-11.220	-11.531	-11.898	-11.961
			.021	.006	.007	.004	.004
Woman					-0.329	0.306	0.284
					.884	.888	.894
Age						0.039	0.038
						.535	.537
Game done at home							0.609
							.773
Village F.E	No	No	No	Yes	Yes	Yes	Yes
Surveyor F.E	No	No	No	No	Yes	Yes	Yes
Observations	241	104	345	345	345	328	328
R^2	.001	.086	.046	.084	.099	.107	.107
Mean of D.V.	36.846	42.019	38.406	38.406	38.406	38.201	38.201

K Proxy alterations

K.1 Diet proxy: alternative specifications

Table A7 displays OLS regression results on the effect of the Externality/Ingroup Identity treatment for participants with a wild-foods based diet (i.e if at least 50% of food is obtained from traditional methods) compared to participants obtaining less than 50% of their food from traditional methods (column 1), to participants obtaining some food from traditional methods (column 2) and to participants never obtaining food from traditional methods (column 3). In all specifications, both the No Externality treatment and the Externality/No Identity treatment constitute the reference group. The unit is Danish Kroner (DKK). The p-values are based on village level cluster-robust standard errors using the wild-bootstrap procedure (Cameron, Gelbach, and Miller 2008; Roodman et al. 2019).

Table A7: Diet proxy: alternative specifications

Participants with Wild foods-based diet are compared to:	Full sample	Participants sometimes using traditional methods	Participants never using traditional methods
	(1)	(2)	(3)
Externality/Ingroup Identity	-0.939 .843	-2.310 .607	3.824 .16
Wild foods-based diet	3.290 .027	2.675 .056	5.731 .031
Externality/Ingroup Identity × Wild foods-based diet	-5.911 .013	-4.679 .036	-10.346 .05
Woman	0.381 .807	0.114 .945	4.085 .065
Age	0.006 .874	-0.000 .994	0.000 .999
Game done at home	1.168 .348	0.590 .736	1.713 .401
Village F.E	Yes	Yes	Yes
Surveyor F.E	Yes	Yes	Yes
Observations	504	448	285
R^2	.059	.064	.117
Mean of D.V.	37.5	37.746	37.789

K.2 Diet proxy: intensive margin

Table A8 displays OLS regressions on the effect of the Externality/Ingroup Identity treatment interacted with an alternative operationalization of the Diet proxy (see Figure A2). Based on the original survey items on food consumption obtained from traditional methods, we divide participants into four categories: participants who do not obtain any food from traditional methods; participants who obtain at least some food from traditional methods; participants who obtain at least half of their food from traditional methods; participants who obtain most or all their food through traditional methods. Since the resulting variable is categorical, we employ it as such in columns 1 and 2. The reference category is participants who do not obtain any food from traditional methods. In columns 3 and 4, the 4-point scale measure is included as a continuous variable. Both the No Externality treatment and the Externality/No Identity treatment constitute the reference group in all specifications. The p-values are based on village level cluster-robust standard errors using the wild-bootstrap procedure (Cameron, Gelbach, and Miller 2008; Roodman et al. 2019).

Table A8: Diet proxy: intensive margin

	Categorical specification		Continuous specification	
	(1)	(2)	(3)	(4)
Externality/Ingroup Identity	3.107	4.195	3.267	3.539
	.191	.052	.183	.217
Wild foods-based diet: at least some	2.343	3.250		
	.22	.074		
Externality/Ingroup Identity	-5.020	-6.316		
× Wild foods-based diet: at least some	.253	.174		
Wild foods-based diet: at least half	6.048	6.601		
	.106	.087		
Externality/Ingroup Identity	-13.865	-13.788		
× Wild foods-based diet: at least half	.063	.042		
Wild foods-based diet: most or all	5.202	5.738		
	.137	.107		
Externality/Ingroup Identity	-7.654	-8.773		
× Wild foods-based diet: most or all	.08	.031		
Wild foods-based diet (continuous)			1.729	1.644
			.086	.091
Externality/Ingroup Identity			-2.689	-2.770
× Wild foods-based diet (continuous)			.034	.022
Woman		-0.234		-0.462
		.878		.771
Age		0.007		0.009
		.86		.841
Game done at home		1.426		0.841
		.249		.491
Village F.E	Yes	Yes	Yes	Yes
Surveyor F.E	Yes	Yes	Yes	Yes
Observations	532	502	532	502
R^2	.07	.07	.05	.05
Mean of D.V.	37.707	37.47	37.707	37.47

K.3 Employment proxy: alternative specifications

Table A9 shows the interaction between the Externality/Ingroup Identity treatment and exposure to market institutions through employment, for several alternative definitions of traditional and market sector occupations. In each column, we drop or add one job category at the time. In columns 1 to 5, we exclude the arguably most ambiguous job categories coded as market sector occupations in the baseline regressions. In column 6, students – which are coded as missing in the baseline regressions – are defined as participants in the market sector. In column 7, participants employed in boating and shipping are coded as missing (they are included in the traditional sector in the baseline regressions). In all specifications, both the No Externality treatment and the Externality/No Identity treatment constitute the reference group. The unit is Danish Kroner (DKK). The p-values are based on village level cluster-robust standard errors using the wild-bootstrap procedure (Cameron, Gelbach, and Miller 2008; Roodman et al. 2019).

Table A9: Employment proxy: alternative specification

	Market sector occupations: alternative def.						Traditional occupations: alternative def.
	− Handicraft & Design	− Fishing (production on land)	− Transportation	− Farming	− Other	+ Students	− Boating & shipping
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Externality/Ingroup Identity	0.563	-0.697	-0.269	-0.178	-0.051	-0.819	-0.138
	.899	.902	.968	.974	.991	.814	.973
Traditional employment	8.243	7.721	7.953	7.741	7.869	7.474	7.241
	.011	.008	.012	.01	.009	.012	.094
Externality/Ingroup Identity × Traditional employment	-12.505	-10.847	-11.935	-12.011	-11.535	-10.759	-11.848
	.004	.008	.006	.007	.003	.01	.033
Woman	1.891	1.027	0.209	-0.062	-0.156	0.064	0.306
	.251	.623	.941	.973	.943	.982	.887
Age	0.044	0.015	0.036	0.039	0.034	0.036	0.062
	.4	.832	.563	.531	.581	.448	.45
Game done at home	0.779	0.789	0.598	0.695	0.949	1.365	0.059
	.804	.665	.764	.767	.664	.54	.959
Village F.E	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Surveyor F.E	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	300	315	321	324	323	344	314
R^2	.12	.11	.11	.11	.11	.1	.1
Mean of D.V.	38.4	38.254	38.01	38.241	38.05	38.140	37.994

L Robustness analysis

L.1 Underlying differences

L.1.1 Additional controls

In this analysis, we include a more extensive set of controls. Tables A10 and A11 show the interaction between the Externality/Ingroup Identity treatment and exposure to market institutions. Columns 2 to 6 include education fixed effects, income fixed effects, perceived income status and financial resilience. Columns 7 to 9 include controls for language and national identity. Columns 10 to 14 include self-reported generalized trust, patience, risk preferences and altruism. In column 15 all control variables are included. In all specifications, both the No Externality treatment and the Externality/No Identity treatment constitute the reference group. The unit is Danish Kroner (DKK). The p-values are based on village level cluster-robust standard errors using the wild-bootstrap procedure (Cameron, Gelbach, and Miller 2008; Roodman et al. 2019).

Table A10: Diet proxy: additional controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Externality/Ingroup Identity	-0.939	-1.031	-0.755	-0.396	-0.385	-0.305	-0.957	-0.652	-0.844	-0.730	-0.853	-0.186	-0.685	-0.473	-0.638
	.843	.744	.76	.872	.873	.896	.693	.684	.625	.753	.781	.939	.764	.879	.756
Wild foods-based diet	3.290	3.730	4.271	4.531	4.452	4.412	3.988	4.394	4.023	4.344	3.588	4.111	3.917	3.554	3.575
	.027	.026	.016	.011	.008	.017	.018	.013	.014	.017	.024	.017	.016	.031	.053
Externality/Ingroup Identity × Wild foods-based diet	-5.911	-6.427	-6.548	-6.520	-6.351	-6.153	-6.365	-6.679	-6.396	-6.700	-5.547	-6.769	-5.838	-6.132	-5.170
	.013	.009	.001	.005	.008	.022	.004	.001	.001	.002	.002	.003	0	.002	.049
Woman	-0.381	-0.719	-0.171	0.191	1.411	1.502	-0.123	-0.068	-0.008	-0.276	0.509	0.175	0.391	0.570	2.129
	.807	.686	.922	.919	.482	.461	.957	.975	.998	.881	.776	.925	.817	.771	.351
Age	0.006	0.006	0.020	0.016	0.045	0.038	0.017	0.026	0.018	0.024	0.028	0.013	0.017	0.014	0.025
	.874	.906	.727	.827	.284	.382	.777	.653	.708	.7	.651	.771	.791	.823	.558
Game done at home	1.168	0.700	0.958	1.050	1.900	1.821	1.033	0.638	0.589	0.889	1.147	1.607	0.935	1.523	1.451
	.348	.534	.413	.439	.273	.298	.287	.598	.569	.446	.396	.295	.474	.355	.467
Some Primary School		1.600													2.495
		.769													.757
Primary School		-4.251													-1.920
		.326													.687
Some High School		-7.112													-8.832
		.187													.043
High School		2.955													1.416
		.565													.813
Professional Education		-2.252													-2.215
		.378													.588
Some University		-4.994													-4.490
		.776													.8
Bachelor Degree		-1.668													-0.660
		.668													.856
Master Degree		3.877													0.964
		.292													.82
PhD Degree		-16.416													-17.208
		.434													.34
HH Earnings 100 - 200 K			-2.368												-1.907
			.475												.575
HH Earnings 200 - 300 K			-6.841												-6.144
			.079												.129
HH Earnings 300 - 400 K			1.417												2.622
			.683												.523
HH Earnings 400 - 500 K			-3.501												-2.731
			.337												.408
HH Earnings 500 - 1000 K			-1.123												3.664
			.768												.451
HH Earnings > 1000 K			2.221												6.997
			.716												.382
Perceived income				-0.589		-0.499									-0.694
				.076		.154									.12
Resilience: 1 - 6 months					-2.274	-2.123									-1.784
					.382	.411									.508
Resilience > 6 months					-2.940	-2.438									-1.310
					.278	.403									.503
Language: Greenlandic and Danish/ English							-2.388		-2.253						0.477
							.326		.345						.86
Language: only Danish/ English							-0.554		3.750						7.276
							.925		.596						.282
Identity: Greenlandic and Danish								-2.580	-3.462						-3.082
								.161	.146						.208
Identity: only Danish								0.043	-5.230						-5.324
								.99	.471						.416
Generalized trust										-0.047				-0.015	0.053
										.826				.952	.876
Patience											0.096			-0.116	0.080
											.86			.831	.922
Risk-taking												0.746		0.828	0.740
												.155		.097	.101
Altruism													-0.050	-0.323	-0.365
													.914	.503	.422
Village F.E	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Surveyor F.E	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Education F.E	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income F.E	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	504	494	474	466	432	430	474	469	469	470	454	451	460	443	406
R^2	.059	.081	.106	.109	.125	.126	.109	.106	.11	.108	.102	.115	.104	.115	.134
Mean of D.V.	37.5	37.51	37.595	37.554	37.477	37.419	37.595	37.719	37.719	37.596	37.599	37.539	37.5	37.517	37.537

Table A11: Employment proxy: additional controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Externality/Ingroup Identity	0.125 .973	-0.655 .91	-0.759 .88	-0.337 .957	0.510 .927	0.520 .931	-0.719 .885	-0.287 .94	-0.224 .954	-0.771 .876	-0.127 .982	-0.797 .884	-0.387 .937	-0.693 .907	0.611 .917
Traditional employment	7.805 .012	7.313 .018	7.646 .021	7.599 .021	7.681 .049	7.539 .045	7.602 .022	7.786 .022	7.713 .031	7.840 .024	7.453 .025	7.977 .023	7.255 .034	6.767 .073	6.032 .154
Externality/Ingroup Identity × Traditional employment	-11.961 .004	-10.719 .031	-10.494 .019	-10.804 .019	-10.833 .052	-10.668 .053	-10.603 .017	-11.176 .014	-11.229 .013	-10.839 .01	-11.257 .031	-10.934 .02	-10.408 .03	-10.632 .038	-10.657 .065
Woman	0.284 .894	0.662 .764	0.942 .664	1.149 .61	1.855 .438	1.873 .439	0.928 .674	1.159 .591	1.158 .601	1.017 .642	1.281 .582	0.823 .711	1.341 .547	1.330 .576	2.360 .327
Age	0.038 .537	0.050 .645	0.049 .715	0.047 .752	0.057 .531	0.051 .591	0.046 .727	0.055 .601	0.049 .597	0.048 .742	0.035 .813	0.048 .694	0.059 .678	0.028 .83	0.030 .736
Game done at home	0.609 .773	-0.123 .954	1.294 .563	1.442 .551	1.526 .541	1.551 .547	1.336 .551	1.087 .631	1.055 .646	1.076 .624	1.193 .601	0.954 .647	0.677 .76	0.773 .727	0.420 .865
Some Primary School		9.275 .088													7.811 .196
Primary School		0.812 .882													0.234 .972
Some High School		-7.786 .158													-12.962 .163
High School		10.301 .091													8.828 .166
Professional Education		0.605 .876													-0.556 .886
Some University		0.293 .884													-0.100 .978
Bachelor Degree		0.643 .921													0.411 .955
Master Degree		7.730 .263													5.250 .387
PhD Degree		-10.077 .343													-9.514 .608
HH Earnings 100 - 200 K			-3.060 .37												-3.967 .276
HH Earnings 200 - 300 K			-6.185 .082												-6.559 .071
HH Earnings 300 - 400 K			-0.058 .991												-0.828 .822
HH Earnings 400 - 500 K			-4.818 .196												-4.636 .321
HH Earnings 500 - 1000 K			-1.805 .614												0.577 .913
HH Earnings > 1000 K			-3.216 .81												0.253 .967
Perceived income				-0.006 .985		-0.008 .968									-0.616 .16
Resilience: 1 - 6 months					-2.624 .28	-2.737 .254									-1.970 .334
Resilience > 6 months					-2.679 .276	-2.681 .287									-1.218 .576
Language: Greenlandic and Danish/ English							-1.051 .733		-1.071 .694						1.088 .752
Language: only Danish/ English							-1.276 .722		0.778 .933						2.443 .824
Identity: Greenlandic and Danish								-0.132 .962	-0.427 .883						0.988 .774
Identity: only Danish								-1.932 .661	-3.484 .86						-2.659 .926
Generalized trust										0.298 .333				0.479 .096	0.518 .072
Patience											-0.096 .863			-0.140 .86	-0.347 .732
Risk-taking												0.187 .746		0.382 .366	0.377 .337
Altruism													-0.560 .278	-0.601 .285	-0.428 .303
Village F.E	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Surveyor F.E	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Education F.E	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income F.E	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	328	325	314	310	295	293	314	310	310	311	305	307	309	301	279
R^2	.107	.137	.156	.154	.174	.17	.156	.152	.152	.16	.151	.157	.162	.163	.173
Mean of D.V.	38.201	38.246	38.312	38.29	38.203	38.123	38.312	38.484	38.484	38.36	38.262	38.404	38.317	38.339	38.244

L.1.2 Register-based controls

In this analysis, we redo the baseline regressions using controls on income and education obtained from official Greenlandic register. Table A12 displays the interaction between the Externality/Ingroup Identity treatment and exposure to market institutions when including register-based controls for education and income. In Panel A, exposure to market institutions is proxied by a dummy indicating if at least 50% of the participant's diet is based on wild foods. In Panel B, exposure to market institutions is proxied by a dummy indicating whether the participant works in the traditional sector (hunting, fishing, or boating and shipping). In columns 1 and 2, the effect of the Externality/Ingroup Identity treatment is showed separately for market-integrated and non market-integrated participants, and in columns 3 to 7, exposure to market institutions is interacted with the Externality/Ingroup Identity treatment. In all specifications, both the No Externality treatment and the Externality/No Identity treatment constitute the reference group. The unit is Danish Kroner (DKK). The p-values are based on village level cluster-robust standard errors using the wild-bootstrap procedure (Cameron, Gelbach, and Miller 2008; Roodman et al. 2019).

Table A12: Register-based controls

Panel A: Diet proxy	Market-based diet	Wild foods-based diet	Full sample				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Externality/Ingroup Identity	0.044 .982	-8.622 .034	-0.043 .974	-0.457 .815	-0.536 .822	-0.459 .848	-0.399 .876
Wild foods-based diet			3.253 .228	3.409 .174	3.858 .031	3.826 .049	3.807 .054
Externality/Ingroup Identity × Wild foods-based diet			-7.628 .039	-7.329 .042	-7.954 .02	-8.001 .021	-8.042 .018
Woman	-4.054 .076	7.492 0			0.715 .53	0.638 .554	0.668 .525
Age	0.086 .392	0.025 .772				0.029 .64	0.029 .64
Home	1.387 .55	-1.948 .278					0.570 .584
Secondary School	-1.942 .49	0.771 .813			-2.385 .348	-2.165 .385	-2.143 .387
Higher Education	4.096 .354	0.727 .835			3.428 .404	3.517 .407	3.542 .405
HH Earnings 100 - 200K	1.628 .63	-0.640 .898			0.896 .75	0.655 .818	0.627 .825
HH Earnings 200 - 300K	0.230 .979	-0.124 .98			-0.009 .996	-0.244 .961	-0.263 .957
HH Earnings 300 - 400K	6.180 .213	-2.205 .723			1.770 .634	1.615 .67	1.580 .682
HH Earnings 400 - 500K	2.647 .592	2.549 .73			1.433 .716	1.215 .78	1.151 .788
HH Earnings 500 - 1000K	7.605 .02	-5.311 .273			1.440 .687	1.457 .68	1.486 .676
HH Earnings > 1000K	1.356 .845	-4.563 .438			-1.876 .742	-2.310 .682	-2.431 .676
Village F.E	Yes	Yes	No	Yes	Yes	Yes	Yes
Surveyor F.E	Yes	Yes	No	No	Yes	Yes	Yes
Observations	237	177	416	416	414	414	414
R^2	.14	.173	.021	.047	.085	.086	.086
Mean of D.V	37.004	37.571	37.188	37.188	37.246	37.246	37.246

Panel B: Employment proxy	Modern occupation	Traditional occupation	Full sample				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Externality/Ingroup Identity	0.695 .757	-7.690 .057	0.020 .99	0.794 .726	0.843 .728	0.780 .741	0.699 .771
Traditional employment			7.107 .033	7.461 .042	8.218 .051	7.706 .054	7.665 .056
Externality/Ingroup Identity × Traditional employment			-8.982 .103	-11.980 .039	-13.098 .053	-13.011 .044	-12.919 .046
Woman	-3.194 .181	2.845 .672			0.585 .788	0.349 .853	0.284 .871
Age	0.169 .046	0.130 .154				0.115 .07	0.116 .062
Home	-4.247 .008	7.531 .328					-0.928 .737
Secondary School	-1.476 .53	-2.979 .52			-3.193 .246	-2.278 .305	-2.249 .304
Higher Education	3.017 .388	-9.293 .471			2.292 .557	2.648 .507	2.599 .518
HH Earnings 100 - 200K	-0.310 .925	7.156 .275			0.687 .887	0.432 .931	0.417 .937
HH Earnings 200 - 300K	-2.423 .771	2.027 .802			-1.225 .85	-1.989 .736	-2.012 .737
HH Earnings 300 - 400K	3.263 .847	4.548 .641			1.615 .722	1.055 .824	1.122 .827
HH Earnings 400 - 500K	-1.732 .576	31.602 .174			-0.800 .858	-1.716 .717	-1.654 .714
HH Earnings 500 - 1000K	-5.499 .56	4.918 .477			-3.072 .605	-3.337 .568	-3.404 .563
HH Earnings > 1000K	-2.073 .767	11.888 .379			-1.977 .818	-3.624 .703	-3.445 .722
Village F.E	Yes	Yes	No	Yes	Yes	Yes	Yes
Surveyor F.E	Yes	Yes	No	No	Yes	Yes	Yes
Observations	187	77	265	265	264	264	264
R^2	.195	.493	.033	.087	.13	.137	.137
Mean of D.V.	36.952	41.169	38.113	38.113	38.182	38.182	38.182

L.1.3 Restricted samples

In this analysis, we restrict the samples such that we only consider traditional participants with higher levels of income and education than the market integrated participants (and vice versa). Figure A7 shows the samples used in the restricted analysis. Simply put, we now compare the highly educated/rich traditional participants to low-educated/poor market-integrated participants. To create the restrictive samples, we have coded as missing participants with a modern occupation/market-based diet and a self-reported yearly household gross income *above* 300,000 DKK (approximately 45,000 dollars), and those with a traditional occupation/with a wild foods-based diet with a self-reported yearly household gross income *below* 300,000 DKK. Similarly, we drop participants with a modern occupation/market-based diet and an education level that is *higher* than primary school, and those with a traditional occupation/with a wild foods-based diet who report as highest education level at or *below* primary schooling. Figure A7 shows income and educational attainment distributions by proxy. The empty bars show income and education categories that are dropped, and the new restrictive samples are the colored bars.

Figure A7: Restricted samples: income and education

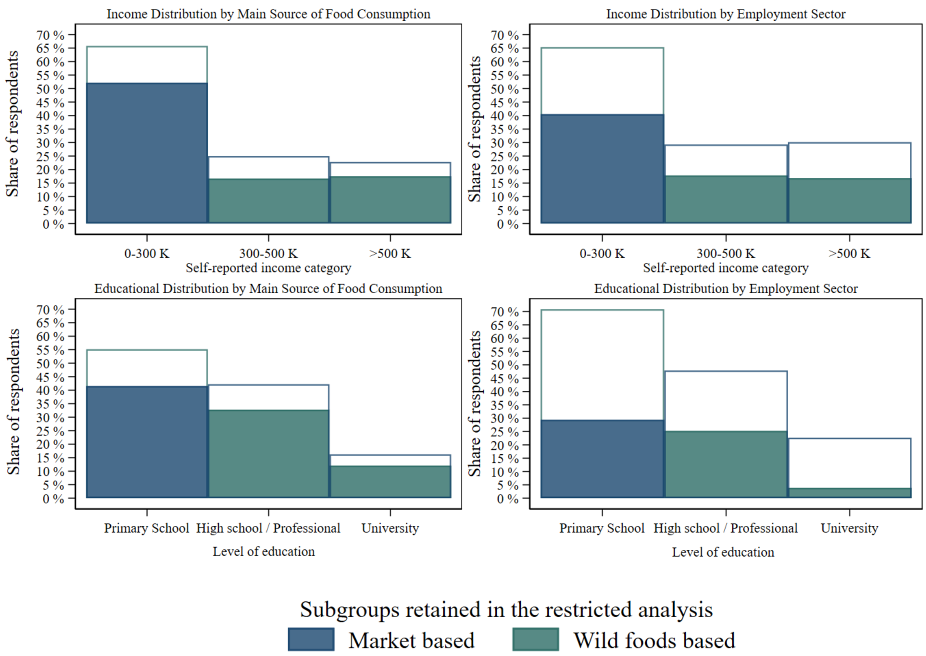


Table A13 shows the interaction between the Externality/Ingroup Identity treatment and exposure to market institutions using the restricted samples. In columns 1 and 3, we employ the samples outlined in the first row of Figure A7, and in columns 2 and 4, we use the samples shown in the second row of Figure A7. In all specifications, both the No Externality treatment and the Externality/No Identity treatment constitute the reference group. The unit is Danish Kroner (DKK). The p-values are based on village level cluster-robust standard errors using the wild-bootstrap procedure (Cameron, Gelbach, and Miller 2008; Roodman et al. 2019).

Table A13: Restricted samples

	Diet proxy		Employment proxy	
	Top vs Bottom Income	Top vs Bottom Education	Top vs Bottom Income	Top vs Bottom Education
	(1)	(2)	(3)	(4)
Externality/Ingroup Identity	-0.393	2.090	2.434	-0.637
	.833	.494	.631	.88
Wild foods-based diet	7.517	4.664		
	.081	.092		
Externality/Ingroup Identity × Wild foods-based diet	-8.123	-9.497		
	.099	.022		
Traditional employment			13.626	11.760
			.113	.021
Externality/Ingroup Identity × Traditional employment			-16.498	-14.831
			.092	.095
Woman	-2.741	0.646	-2.895	0.395
	.147	.772	.453	.913
Age	0.004	0.010	-0.136	-0.041
	.967	.887	.126	.652
Game done at home	2.965	3.996	-1.962	9.149
	.273	.012	.765	.001
Village F.E	Yes	Yes	Yes	Yes
Surveyor F.E	Yes	Yes	Yes	Yes
Observations	214	214	122	95
R^2	.11	.1	.26	.37
Mean of D.V.	37.477	37.383	37.787	38.105

L.2 Alternative explanations

In this sub-section, we consider a set of institutional factors which co-vary with market exposure and therefore could influence the findings. As shown in Table A3, participants in the traditional economy are more religious, have tighter kinship networks, have been less exposed to Denmark, and are less likely to spend time on the internet.

According to a growing literature, religion – and in particular moralistic high gods – have promoted an extension of moral behavior toward distant others (Purzycki et al. 2016; Purzycki et al. 2018a; Lang et al. 2019). In our data, religion is unlikely to be a confounding element, since participants in the traditional economy in fact are relatively more religious; they are both more Christian and more likely to adhere to traditional Inuit Beliefs.

Differences in kinship structure (Enke 2019) could also influence parochial behavior in the Dice Game. Participants in the traditional economy co-reside with more family members (Table A3), and might exhibit more parochial honesty since misreporting in the Externality/Ingroup Identity treatment is more likely to negatively affect a family member. Exploiting administrative register data on relatives and their respective residence, we compute the share of family members in the local population, and denote this measure kinship tightness (see Table A1 for a detailed description).

Exposure to other political institutions could also affect behavior and preferences (Lowes et al. 2017; Becker et al. 2016). Since participants in the market economy are more likely to having lived in Denmark (see Table A3), and somewhat more likely to have family there, differential exposure to Danish institutions could potentially confound the findings. Accordingly, we account for these factors as well.

Lastly, market and traditional participants differ in their media consumption. Market sector respondents are more likely to spend time on the internet (Table A3), and thus potentially more exposed to outsiders through web interactions. In order to ensure that these behavioral differences do not confound the findings, we control for media consumption below.

In Tables A14 and A15, we subsequently rule out each of the potentially confounding factors. Columns 2 to 4 include as controls interactions between the Externality/Ingroup Identity treatment and indicator variables for whether the participant regularly visits church and adhere to traditional Inuit beliefs. Column 5 includes an interaction between the Externality/Ingroup Identity treatment and a measure of kinship tightness. Columns 6 to 8 include interactions between the Externality/Ingroup Identity treatment and indicator variables for whether the participant lived in Denmark and whether he/she has close relatives living in Denmark. Columns 9 to 11 include interactions between the Externality/Ingroup Identity treatment and indicator variables for whether the participant uses the internet and TV or radio as news sources. Column 12 includes controls for all potentially confounding factors except kinship tightness[20]. In all specifications, both the No Externality treatment and the Externality/No Identity treatment constitute the reference group. The unit is Danish Kroner (DKK). The p-values are based on village level cluster-robust standard errors using the wild-bootstrap procedure (Cameron, Gelbach, and Miller 2008; Roodman et al. 2019).

20. Kinship tightness is calculated based on Greenlandic register data. Because of missing identifying data, we lose a significant amount of observations in this exercise, and therefore do not account for kinship tightness in this specification.

Table A14: Diet proxy: alternative explanations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Externality/Ingroup Identity	-0.755 .76	-1.591 .557	-1.522 .612	-2.352 .399	-1.228 .462	-0.600 .776	-0.802 .665	-0.725 .795	2.868 .364	-0.442 .832	3.015 .285	-0.433 .92
Wild foods-based diet	4.271 .016	4.288 .012	4.169 .013	4.262 .013	4.542 .033	3.866 .02	4.462 .014	4.082 .015	4.313 .017	4.082 .016	4.237 .018	3.816 .038
Externality/Ingroup Identity × Wild foods-based diet	-6.548 .001	-6.930 .009	-6.624 .004	-7.024 .012	-9.038 .008	-6.568 .002	-6.755 0	-6.630 0	-6.961 0	-6.469 .002	-6.947 .001	-6.984 .005
Woman	-0.171 .922	0.095 .953	-0.459 .816	-0.295 .882	-0.102 .947	0.170 .919	0.043 .977	0.312 .862	-0.059 .961	0.038 .977	0.039 .982	0.512 .836
Age	0.020 .727	0.063 .376	0.019 .787	0.059 .438	0.065 .437	0.035 .651	-0.007 .909	0.003 .963	0.033 .549	0.011 .859	0.018 .759	0.034 .727
Game done at home	0.958 .413	0.788 .555	1.013 .443	0.723 .625	0.093 .913	0.537 .655	0.653 .585	0.355 .781	0.999 .418	0.937 .492	0.933 .505	0.037 .993
Attend church		-5.439 .086		-6.437 .058								-6.442 .051
Externality/Ingroup Identity × Attend church		4.622 .45		5.773 .37								5.220 .367
Traditional beliefs			-2.308 .6	-1.203 .761								0.288 .945
Externality/Ingroup Identity × Traditional beliefs			8.278 .134	7.040 .176								4.666 .242
Kinship tightness					-0.087 .849							
Externality/Ingroup Identity × Kinship tightness					0.230 .758							
Lived in Denmark						-3.591 .155		-3.891 .11				-4.948 .064
Externality/Ingroup Identity × Lived in Denmark						0.050 .996		-0.228 .985				1.102 .884
Close relatives in Denmark							2.371 .268	2.771 .169				3.287 .208
Externality/Ingroup Identity × Close relatives in Denmark							0.072 .993	0.083 .984				-1.911 .69
Media: internet									2.348 .373		2.180 .405	0.315 .918
Externality/Ingroup Identity × Media: internet									-4.955 .261		-4.917 .262	-3.021 .518
Media: TV or Radio										3.157 .032	3.022 .034	2.994 .143
Externality/Ingroup Identity × Media: TV or radio										-0.503 .792	-0.236 .887	1.068 .65
Village FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Surveyor FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Education FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	474	469	464	461	332	471	470	468	473	473	473	455
R^2	.106	.118	.11	.125	.126	.114	.114	.125	.111	.112	.116	.153
Mean of D.V.	37.595	37.633	37.586	37.592	37.169	37.707	37.617	37.692	37.632	37.632	37.632	37.692

Table A15: Employment proxy: alternative explanations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Externality/Ingroup Identity	-0.759	-1.128	-0.863	-1.357	2.322	2.572	0.756	3.154	3.728	-0.356	3.760	3.106
	.88	.844	.863	.836	.309	.411	.759	.409	.164	.957	.358	.603
Traditional employment	7.646	8.436	8.655	9.545	5.697	8.538	7.873	8.612	7.944	7.315	7.635	10.068
	.021	.013	.005	.008	.185	.011	.013	.01	.014	.021	.014	.008
Externality/Ingroup Identity × Traditional employment	-10.494	-11.047	-11.681	-12.292	-11.736	-11.905	-10.932	-11.986	-11.188	-10.848	-11.710	-13.825
	.019	.017	.003	.002	.061	.008	.005	.008	.008	.013	.006	.003
Woman	0.942	1.097	0.482	0.695	1.629	1.252	1.150	1.467	0.952	0.893	0.929	1.536
	.664	.612	.832	.747	.507	.584	.601	.536	.649	.672	.639	.486
Age	0.049	0.071	0.051	0.074	0.143	0.044	0.037	0.026	0.041	0.035	0.024	0.013
	.715	.566	.707	.551	.251	.749	.87	.891	.779	.832	.89	.95
Game done at home	1.294	1.162	1.441	1.259	-0.421	1.285	1.134	1.205	1.453	1.168	1.332	1.156
	.563	.611	.548	.629	.892	.564	.63	.599	.531	.628	.588	.711
Attend church		-5.328		-6.804								-7.919
		.291		.192								.167
Externality/Ingroup Identity × Attend church		2.345		3.754								1.638
		.832		.73								.871
Traditional beliefs			-2.437	-3.061								-0.190
			.623	.561								.957
Externality/Ingroup Identity × Traditional beliefs			3.560	3.641								-1.027
			.646	.658								.893
Kinship tightness					-0.024							
					.935							
Externality/Ingroup Identity × Kinship tightness					-0.704							
					.129							
Lived in Denmark						-0.001		-0.531				-0.932
						.999		.812				.744
Externality/Ingroup Identity × Lived in Denmark						-6.770		-6.353				-6.547
						.736		.63				.584
Close relatives in Denmark							2.110	2.094				3.376
							.254	.213				.029
Externality/Ingroup Identity × Close relatives in Denmark							-3.044	-1.577				-2.444
							.894	.769				.695
Media: internet									-0.710		-0.799	-3.423
									.783		.752	.13
Externality/Ingroup Identity × Media: internet									-5.338		-5.785	-1.279
									.327		.266	.732
Media: TV or Radio										3.892	3.805	3.002
										.181	.217	.496
Externality/Ingroup Identity × Media: TV or radio										-0.382	0.627	2.199
										.899	.848	.676
Village FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Surveyor FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Education FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	314	312	308	307	219	313	311	310	314	314	314	303
R^2	.156	.164	.167	.179	.171	.167	.165	.176	.161	.163	.17	.223
Mean of D.V.	38.312	38.301	38.247	38.241	38.265	38.403	38.296	38.387	38.312	38.312	38.312	38.317

L.3 Selection

In this section, we undertake two exercises to alleviate concerns of endogeneity due to selection influencing the findings. First we exploit the fact that participants in our survey to some extent are tied to their home village; 40% of the participants were living in their village of birth at the time of surveying. To complement the migration checks, we consider an instrumental variable approach using information on both the participant's and his/her parents' villages of birth as instruments.

L.3.1 Migration

Table A16 shows the interaction between the Externality/Ingroup Identity treatment and exposure to market institutions using samples that exclude participants who we denote as "movers". The rationale for this exercise is that between-village migration is likely to convey information on selection in and out of the market economy. Participants who remained in their home town were arguably more constrained in their choice to enter the market or the traditional sector. We therefore restrict the analysis to "remaining" participants, defining migrating participants in two different ways.

The first restricted sample ("No selective migration") excludes participants who were born in a settlement and living in a town at the time of the data collection (N=58), as well as participants who were born abroad or in a town and were living in a settlement at the time of surveying (N=62). In addition, 32 participants were excluded due to insufficient information on the village of birth (whether they were born in a settlement or town could not be determined). The second restricted sample ("No migration") excludes all participants who were born in a different village from the one they were currently residing in at the time of the survey (N=298). 46 participants were excluded due to missing birth village data.

In columns 1 and 3, we use the first sample ("No selective migration"), and in columns 2 and 4, we use the second sample ("No migration"). In all specifications, both the No Externality treatment and the Externality/No Identity treatment constitute the reference group. The unit is Danish Kroner (DKK). The p-values are based on village level cluster-robust standard errors using the wild-bootstrap procedure (Cameron, Gelbach, and Miller 2008; Roodman et al. 2019).

Table A16: Regression results: excluding movers

	Diet based proxy		Employment proxy	
	No selective migration	No migration	No selective migration	No migration
	(1)	(2)	(3)	(4)
Externality/Ingroup Identity	-0.732 .933	0.366 .86	-0.292 .955	4.147 .17
Wild foods-based diet	2.403 .252	3.167 .14		
Externality/Ingroup Identity × Wild foods-based diet	-5.644 .067	-5.462 .132		
Traditional employment			6.121 .065	10.704 .041
Externality/Ingroup Identity × Traditional employment			-9.652 .008	-14.328 .029
Woman	-1.501 .415	0.282 .908	-1.775 .492	-3.150 .441
Age	-0.011 .861	-0.032 .676	0.081 .328	0.147 .324
Game done at home	1.130 .571	2.239 .56	0.426 .849	4.998 .397
Village F.E.	Yes	Yes	Yes	Yes
Surveyor F.E.	Yes	Yes	Yes	Yes
Observations	362	188	247	121
R^2	.08	.13	.12	.23
Mean of D.V.	37.403	37.553	37.126	38.017

L.3.2 Instrumental variable approach

In this sub-section, we investigate the causal direction of the relationship between market exposure and parochial honesty by means of instrumental variable regressions. We consider two pre-determined and plausibly exogenous factors as instruments: the population of the village of birth (in year 1977) and the birthplace of parents. For this reason, we are only able to use data on participants that indicated both their own and their parents villages of birth. Moreover, we confine our analysis to respondents who were born in Greenland[21]. This limits our analyses to 249 participants in the prediction of the Employment proxy, and 394 participants in the prediction of the Diet proxy.

The rationale for including the population of participants' birth villages in 1977 (which is as far back as the Greenlandic registers contain data on village populations) is that village population is a strong proxy for the supply of traditional vis-à-vis market-related occupations as well as the prevalence of subsistence hunting and sharing. If childhood environment at least partly determines subsistence method and occupation, this proxy allows us to tap into variation in market exposure that cannot be due to selection. For the same reason we also consider the birthplace of parents, an indicator variable for the number of parents born in a settlement. Since settlements are smaller localities in which the traditional economy is particularly prevalent, the variable therefore indicates the degree of market exposure of parents. Given that parents pass on their preferences and skills to their children, parents born in settlements should increase the likelihood of participants ending up in the traditional sector.

We estimate instrumental variable regressions using two different first stage approaches. First, we conduct standard OLS first stage regressions. Second, we improve the fit of the first stage (see e.g. Newey 1990) by predicting the probability of working in the traditional sector/ obtaining a wild foods-based diet by means of Machine Learning techniques. The Machine Learning model we employ is called Calibrated Lattice Model (from the Tensorflow Lattice library, see You et al. 2017). The Calibrated Lattice Model applies piece-wise linear

21. 21 participants who indicated invalid addresses of the village of birth were further dropped.

and categorical calibration to improve the fit of the prediction. It looks for interactions between the input variables and, essential for the present purpose, it allows for monotonicity constraints. This feature means that we restrict each piece-wise regression such that the predicted outcome never decreases (or increases) with the predictors, even if there might be local irregularities in the data that would support deviations from the global trend. Monotonicity constraints are appropriate for the present application because it hedges toward problems of overfitting endemic to Machine Learning models.

Next, we use the predicted value from the first stage as instrument in standard two-stage least squares regressions[22]. Our approach is thus a three-stage procedure (following the example of Adams, Almeida, and Ferreira 2009). This approach conveys several advantages, both from a theoretical and a practical viewpoint. First, when using the data on participants in all treatment groups in the first stage, we ensure that for any given value of the instrumental variable, the predicted Diet and Employment proxies are the same regardless of treatment status. Second, it allows us to maximize power in the first stage. This is important given that the sample size is relatively small. Most importantly, it allows us to obtain asymptotically valid standard errors (Wooldridge 2010). In short, we run the first stage regression using information from all three treatment groups, store the predicted value and then use the predicted value as instrument in standard instrumental variable estimations.

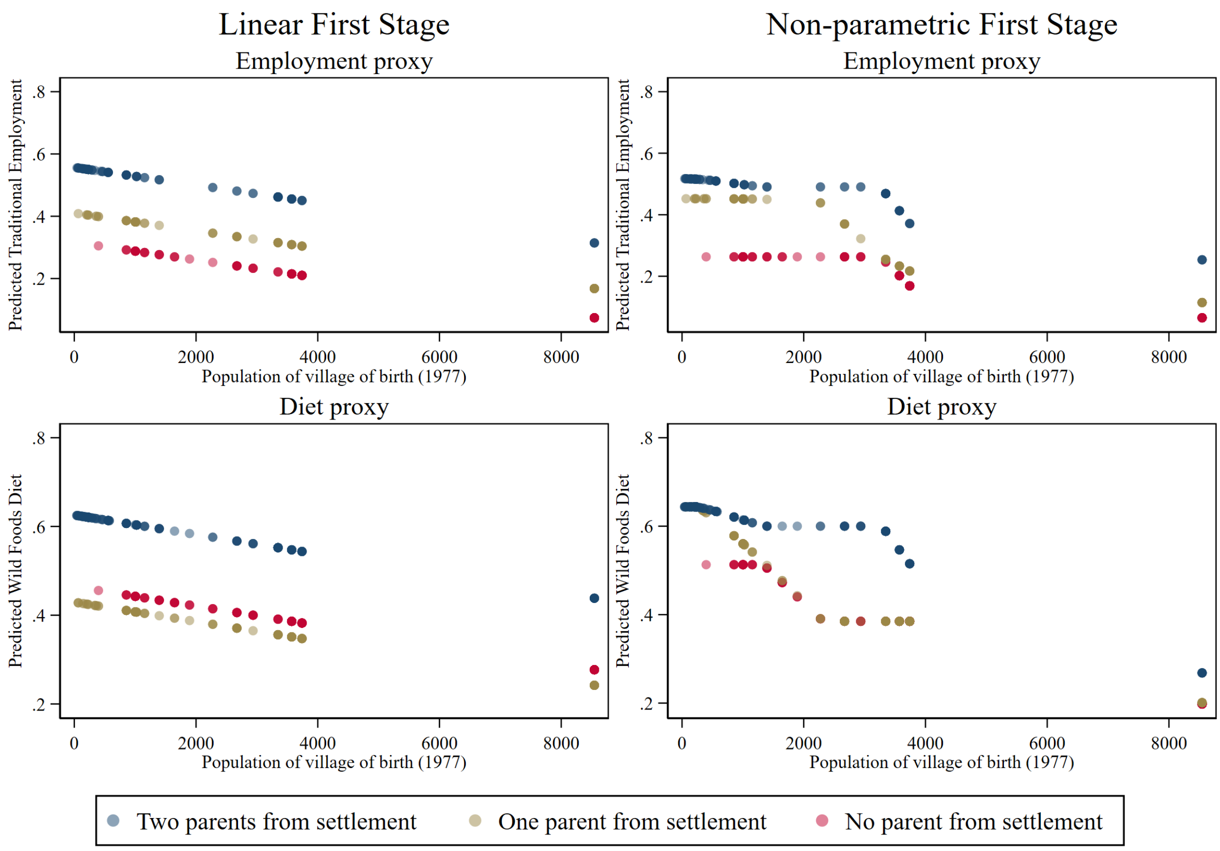
First stage estimation The linear first stage is estimated using simple linear regressions. Figure A8 shows that lower population of the birthplace, as well as having both parents born in settlements, increases the likelihood of the participant operating in the traditional economy. The linear model indicates that the estimated likelihood of having a traditional occupation is less than 10% for a participant who was born in Nuuk (population > 8000) and whose parents were not born in settlements, whereas it is higher than 50% for a participant whose both parents were born in settlements and were born in the least populated settlements. Similarly, the difference in the likelihood of obtaining a wild foods-based diet ranges from less than 30% to more than 60% depending on the pre-determined factors. The

22. Using the Stata package ivreg2.

F-statistics for the linear first stages are reported in columns 1 and 2 of Table A17 and above the rule of thumb for weak instruments (Stock, Yogo, and Wright 2002).

In the Calibrated Lattice Models, we constrain the estimations such that a smaller population as well as having more parents from settlements could not decrease the likelihood of operating in the traditional sector. We then randomly divide our samples into training and test samples, where the test samples constitute 30% of the observations. We set the hyper parameters of the lattice model such that: learning rate is equal to 0.01; batch size is equal to 128; number of epochs is equal to 500; and prefetting number of epochs is equal to 10. Since we work with dichotomous outcome variables, we use binary cross entropy as our loss function. Using these specifications, we fit our models to predict the likelihood of working in the traditional sector / obtaining a wild foods-based diet by means of simple piece-wise regressions. We evaluate the models using Area Under the Curve (AUC) Bradley 1997, which is a measure ranging from 0 (0% accurately classified outcomes) to 1 (100% accurately classified outcomes). The prediction model for the Employment proxy is estimated with an AUC at 0.7639, whereas the prediction of the Diet proxy renders an AUC of 0.6533. While much variation remains unexplained, the precision of the models exceeds expectation given that we only use two coarse inputs as predictors. This shows that both personal and parental background to a large degree influence the life choices of Greenlanders in our sample.

Figure A8: First stage by proxy and methodology.



Instrumental variables estimation Having predicted measures of market exposure using two pre-determined factors, we next turn to the instrumental variables estimations. If a parochial mindset influenced selection into traditional occupations or subsistence hunting and fishing, the association between die-roll reporting when playing against outgroups, and our predicted measures of market exposure should be substantially smaller than in the OLS regressions. In Table A16, we document that this is not the case. In columns 1 and 4, the sample is restricted to participants exposed to the No Externality and Externality/No Identity treatments. In columns 2 and 5, the sample is restricted to participants exposed to the Externality/Ingroup Identity treatment. In columns 3 and 6, the interaction term between each proxy and the Externality/Ingroup Identity treatment is instrumented with the interaction between the predicted values obtained from the first stage estimation and the Externality/Ingroup Identity treatment. Both the No Externality treatment and the

Externality/No Identity treatment constitute the reference group. The unit is Danish Kroner (DKK). The p-values are based on village level cluster-robust standard errors using the wild-bootstrap procedure Cameron, Gelbach, and Miller 2008; Roodman et al. 2019. In columns 1, 2, 4 and 5, the F-statistics from the relevant first stage estimation are shown. In columns 3 and 6, we show the Cragg-Donald statistics.

Going from 0% to 100% likelihood of working in the traditional sector is expected to increase reporting when playing against the outgroup by 12-15 DKK (significantly estimated at the 5 and 10% level). Similarly, a 100 percentage point increase in predicted wild foods-based diet is estimated to increase reporting against the outgroup by around 11 DKK. Conversely, participants with higher predicted likelihood of engaging in the traditional economy do not enhance payoffs when playing against the ingroup. Because of more noise relative to the OLS regressions, the interaction terms are estimated below conventional significance levels. Finally, it should be stressed that the exclusion restriction is untestable, and we cannot rule out that the population of the village of birth and the birthplace of parents do not shape parochial preferences through channels other than market exposure. As a consequence, the results should be interpreted with caution.

Table A17: Instrumental variable estimates

Panel A: Employment Proxy	Linear First Stage			Non-parametric First Stage		
	No Externality & Outgroup	Ingroup	Full sample	No Externality & Outgroup	Ingroup	Full sample
	(1)	(2)	(3)	(4)	(5)	(6)
Externality/Ingroup Identity			1.654 .743			1.303 .77
Traditional Employment	15.698 .035	-4.120 .734	15.698 .041	11.808 .054	-6.862 .534	11.808 .054
Externality/Ingroup Identity × Traditional Employment			-19.818 .119			-18.670 .12
Observations	178	71	249	178	71	249
Mean of D.V.	40.393	35.07	38.876	40.393	35.07	38.876
F-/ Cragg-Donald statistic	19.78	19.78	12.33			14.12

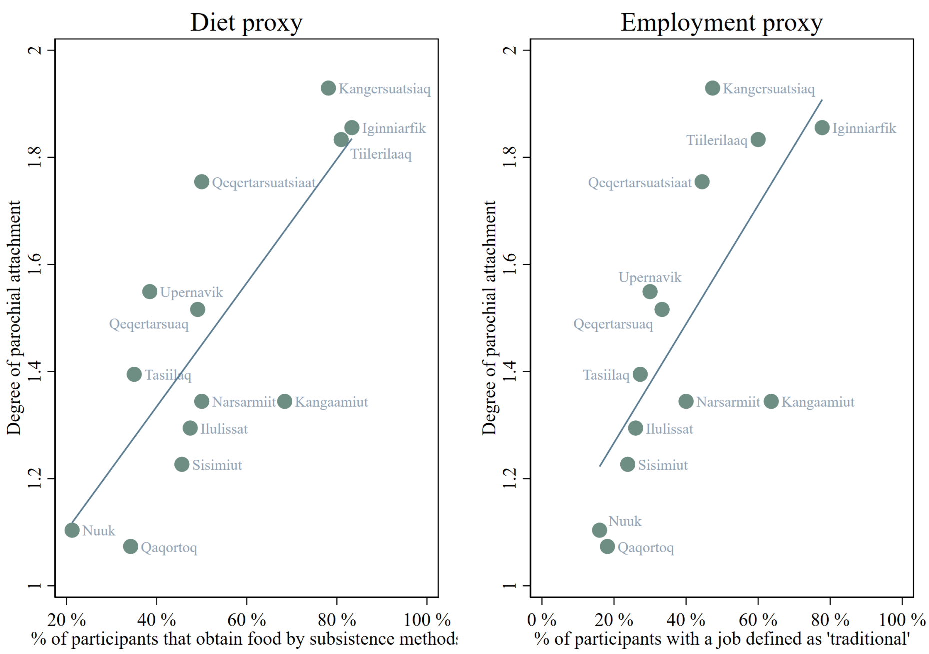
Panel B: Diet Proxy	Linear First Stage			Non-parametric First Stage		
	No Externality & Outgroup	Ingroup	Full sample	No Externality & Outgroup	Ingroup	Full sample
	(1)	(2)	(3)	(4)	(5)	(6)
Externality/Ingroup Identity			3.068 .494			1.736 .747
Wild foods-based diet	11.122 .147	-5.136 .696	11.122 .145	11.440 .092	-1.921 .88	11.440 .086
Externality/Ingroup Identity × Wild foods-based diet			-16.258 .18			-13.361 .258
Observations	279	115	394	279	115	394
Mean of D.V.	39.391	34.87	38.071	39.391	34.87	38.071
F-/ Cragg-Donald statistic	12.76	12.76	9.14			8.52

M Parochial attachment

In this section, we consider a mechanism that may link market exposure to generalized honesty, namely parochial attachment. Through regular interactions with people from the outgroup, market integrated participants may develop a stronger emotional attachment to these outsiders (Allport 1954). Our data allows us to explore this tendency, and to investigate whether it can account for (some of) the behavioral differences between traditional economy- relative to market economy-participants.

Figure A9 shows a positive village level-correlation between the prevalence of the traditional economy and parochial attachment, where parochial attachment is defined as sense of belonging to the local village divided by sense of belonging to outgroups (Greenland, Denmark and the World). See table A1 for a more detailed variable definition. The relationship is statistically significant at the village level (Employment proxy: coefficient= 1.109, p-value= 0.005, N= 13; Diet proxy: coefficient= 1.154 p-value= 0.002, N= 13), as well as at the individual level (Employment proxy: coefficient= 0.322, p-value= 0.001, N= 217. Diet proxy: coefficient= 0.338, p-value< 0.001, N= 305).

Figure A9: Parochial attachment and market exposure.



Next, we show that the higher level of parochial attachment among participants in the traditional economy accounts for some of the variation in parochial honesty. In Table A18, we document that the magnitude and precision of the proxy coefficients, as well as the interaction terms, generally decreases when we control for parochial attachment. In column 1, we show the baseline specifications with the samples restricted to include only participants with non-missing parochial attachment data. In column 2, we add an interaction between the Externality/Ingroup Identity treatment and parochial attachment. In all specifications, both the No Externality treatment and the Externality/No Identity treatment constitute the reference group. The unit is Danish Kroner (DKK). The p-values are based on village level cluster-robust standard errors using the wild-bootstrap procedure (Cameron, Gelbach, and Miller 2008; Roodman et al. 2019).

Table A18: Parochial attachment and parochial honesty

	Reference: Pooled sample	
Panel A: Diet proxy	(1)	(2)
Externality/Ingroup Identity	-1.618	0.936
	.692	.936
Wild foods-based	3.882	3.226
	.023	.095
Externality/Ingroup Identity × Wild foods-based	-6.019	-5.381
	.008	.148
Parochial Attachment		2.593
		.345
Externality/Ingroup Identity × Parochial Attachment		-2.037
		.779
Woman	0.922	1.103
	.484	.392
Age	-0.020	-0.029
	.684	.579
Game done at home	1.665	2.076
	.277	.149
Observations	295	295
R^2	.135	.141
Mean of D.V	37.695	37.695
Panel B: Employment proxy	(1)	(2)
Externality/Ingroup Identity	0.539	3.281
	.931	.816
Traditional Employment	9.645	8.948
	.015	.007
Externality/Ingroup Identity × Traditional Employment	-17.321	-16.444
	.008	.024
Parochial Attachment		1.950
		.569
Externality/Ingroup Identity × Parochial Attachment		-2.159
		.749
Woman	0.701	0.784
	.652	.596
Age	0.027	0.025
	.649	.696
Game done at home	2.603	2.842
	.197	.134
Observations	209	209
R^2	.166	.168
Mean of D.V	37.895	37.895
Village F.E	Yes	Yes
Surveyor F.E	Yes	Yes