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TRADE EXPOSURE AND SOCIAL COHESION: EVIDENCE FROM UGANDA

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Trade Exposure and Social Cohesion: Evidence from Uganda[1]

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Abstract We examine and offer causal evidence on the link between trade exposure and social cohesion using rich micro tax data and a natural experiment of exchange rate liberalization in Uganda. Our results show that exposure to exogenous exchange rate shocks has significant albeit economically small effects on social cohesion: it reduces trust, enhances participation, and has ambiguous effects on identity. These effects operate largely through the expenditure channel (or household exposure) and to a lesser extent through the earnings channel (captured by worker and firm exposure).

I. Introduction

In his famous book, Putnam (2000, p. 283) reflected on the decline in social capital in the U.S. and argued that "global economic transformations are having an important impact on community life". More recently, following increasing globalization, there has been a backlash against globalization, including political polarization and a return to (trade) protectionism.[4] Despite its policy relevance, we know little about the way that trade and investment affect trust, participation, identity, and other dimensions of social cohesion.[5] One of the challenges has been to empirically test the hypothesis. While there are studies that find associations between globalization and social cohesion, accessing the causal relation is more difficult.

In this paper, we examine and provide causal evidence on the link between trade exposure and various dimensions of social cohesion in Uganda. We then explore specific mechanisms that may describe the relationship between exposure and cohesion, such as whether trade exposure affected cohesion through changes in local labour markets.[6] To address these questions, we combine a rich set of microdata that covers the universe of formal workers and firms in Uganda with detailed microdata on formal and informal households and individual-level public attitudes. We use this data to construct unique firm, worker, and household trade exposures that we match to individual-level data on social cohesion outcomes, which allows to offer causal identification between trade

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[4] On economic globalization and political backlash, see Rodrik (2018). On the return to (trade) protectionism in the U.S., see Fajgelbaum, Goldberg, Kennedy, and Khandelwal (2020) and Amiti, Redding, and Weinstein (2019). A range of studies identify unequal gains from trade on labour markets (e.g. Autor, Dorn, & Hanson, 2013; Hakobyan & McLaren, 2016) and the decline in labour shares from import competition (e.g. Elsby, Hobijn, & Şahin, 2013).
[5] For a literature review on globalization and social cohesion, see Schiefer and Van der Noll (2017).
[6] This is work-in-progress and will be in the next working paper version.

exposure and social cohesion.[7] It also enables us to explore in detail the mechanisms behind the relationship between trade exposure and cohesion, and whether the effects differ across the firm, worker, and household distribution.

For our identification strategy, we exploit an exchange rate liberalization episode in Uganda that started in 1990 and that formed an integral part of the government's trade liberalization policy. The policy coincided with increases in trade and perceptions that trade provides economic benefits. Between 2002-2014, exports in merchandise increased from US$ 0.5 to 2.3 billion, while the share of the population that perceived trade as beneficial increased from 64 to 70 percent, the majority believing trade to increase wages (79 percent) and create jobs (82 percent).[8] At the same time, the exchange rate liberalization escalated the exchange rate from 428 (LCU per US$) to 3,718 between 1990-2020. This resulted in a dramatic reduction in relative purchasing power for consumers and producers in respect to imports.[9] It also coincided with a perception among the Ugandan population that trade exposure *increases* domestic prices (79 percent). This suggests that while exchange under-valuation may have raised economic growth through relative price reductions in exports (Rodrik, 2008), its reductions in purchasing power affected some groups in society negatively, potentially affecting social cohesion.

Our starting point is that producers and consumers are differentially exposed to the exchange rate (henceforth trade) shocks when engaging in importing or exporting (for the firm), work at firms that engage in trade (for workers) or consume baskets of goods that include imported consumer goods or domestic goods with imported input (for consumers). The benefit of the exchange rate liberalization is that firms, workers, and households were unlikely able to anticipate such shocks or adjust to them in the short-term, thus providing plausible exogenous variation. Given that exchange rates changed over time, and because of differential input-output structures (for firms), sectors of employment (for workers) and consumption baskets (for consumers), it provides cross-time, cross-firm/worker/consumer variation. To the end of capturing this exposure, we construct three measures: (i) *firm exposure*, measured by how a firm's input-output structure and production network are influenced by exchange rate changes, (ii) *worker earnings exposure*, captured by worker exposure through employers, and (iv) *household expenditure exposure*, which captures changes in expenditures following changes in exchange rates.[10] With these measures at hand, we first offer reduced-form evidence of how trade exposure affects cohesion dimensions. Although various concepts of cohesion can be applied, in our analysis, we explore the effects of exposure on the dimensions of political and interpersonal *trust*, national and group *identity,* and

[7] In addition to its value for causal identification, individual level measurement is important; Alesina, Tabellini, and Trebbi (2017) show that cultural diversity (which includes identity) *within* EU countries is about ten times as large as between countries. Using a global dataset, Falk et al. (2018) document large within-country heterogeneity in trust, altruism and reciprocity between individuals.

[8] See PEW Research (2014).

[9] IMF International Financial Statistics

[10] A similar distinction between the earnings and expenditure channel is applied by Borusyak and Jaravel (2021) and Fajgelbaum and Khandelwal (2016).

civil and political *participation*.[11] In a next step, we explore the underlying mechanisms through which increased trade exposure affected social cohesion.

We find that exposure to exogenous exchange rate shocks has a significant but economically small effect on trust, identity, and participation - it reduces trust, enhances participation, and has ambiguous effects on identity. Results show that effects operate largely through the expenditure channel (household exposure) and to a lesser extent through the earnings channel (worker and firm exposure). Specifically, we find that a one percent increase in *household exposure* results in a 0.018 and 0.014 percent reduction in social and political trust, a 0.009 percent rise in political participation (with no significant effect on civic participation), and a 0.014 reduction in national identity and 0.034 increase in affiliation to group identity. The effects of *worker earning exposure* and *direct firm exposure* are less pronounced, both in magnitude and conventional statistical levels across different cohesion dimensions. These results are robust to several controls, including socio-demographic characteristics and factors which have shown to affect social cohesion, including institutional quality and access. The economically small effect is somewhat unsurprising given the multitude of co-founding factors that have been shown to affect social cohesion (see Green & Preston, 2001; Letki, 2008; Richey, 2010; Schiefer & Van der Noll, 2017; Charron & Rothstein, 2018; Walle, 2022). That the trade-cohesion relationship operates mainly through expenditures contrasts other studies that show that expenditure effects from trade are often diffuse and therefore such trade shocks may be largely undetected by consumers (Stantcheva, 2022). It is in line, however, with the attitude data described above, which indicates that, during the exchange liberalization episode, there was an increase in the perception among the Ugandan population that trade exposure *increases* domestic prices.

Our study contributes to different strands of the literature. A large literature has documented the distributional effects of trade. A range of papers study the effects of exposure to trade on labour markets, suggesting more exposed industries (and their workers) see larger reductions in wages and employment opportunities (e.g., Autor et al., 2013, Dauth et al. 2021). It also documents negative effects to be larger for lower-skilled workers, echoing earlier empirical findings that trade benefits higher-skilled as opposed to lower-skilled workers (Goldberg & Pavcnik, 2007). On the consumer side, Fajgelbaum and Khandelwal (2016) show that consumers are differently exposed to international price changes due to differential consumption baskets. Jaravel and Sager (2019) find that exposure to cheaper final goods from China results in lower consumer prices in the U.S. The main takeaway of these studies on consumers is that trade benefits the poor, although most evidence is on high-income countries and effects in lower-income countries are likely regressive (Atkin, Faber & Gonzalez-Navarro, 2018). We contribute to this literature by studying exposure

[11] We distinguish between horizontal and vertical dimensions of cohesion. The dimension of trust captures whether people trust their political leaders and its institutions, and the extent to which they trust other people in society. Identity refers to the extent to which groups within society are tolerated and whether people identify with the national identity. Cooperation is the degree to which groups cooperate within society and people cooperate with the state for common purposes. Alternative conceptualizations (which could be overlapping or orthogonal to each other) include cooperation, social interaction, pro-social behaviour, culture, cultural traits, social capital, and civic virtue (e.g., Lowes & Montero, 2021).

to trade across the three dimensions of firms, workers and households and retrieve estimates for a lower-income country.

Our paper also adds to a literature that studies how trade and its distributional effects propagate to broader social cohesion outcomes. Some studies examine the relationship between globalization and social cohesion. Fischer (2012) offers cross-country evidence that globalization lowers trust. Fang et al. (2021) use a sample of 149 countries and find that economic and political globalisation measures are negatively related to indicators of society and political polarisation. Several papers document factors that are plausibly relevant in explaining changes in social cohesion outcomes. Those regions more exposed to import competition from China are found to raise authoritarian values (Ballard-Rosa, Jensen, & Scheve, 2021) and political polarization in the U.S. (Autor et al., 2020) and see lower support for democracy and lower liberal values in the EU (Colantone & Stanig, 2018). A paper by Mendez and Van Patten (2023) is probably closest to our work. In the setting of Costa Rica, they document that firm exposure to a trade reform affects voting behaviour and attitudes towards trade policy among firm's employees. Our paper compliments this work by studying the effects of trade exposure to general cohesion outcomes, for a low-income country setting, and in the case of an exchange rate policy event.

Our paper also speaks to a growing literature that aims to understand the determinants of attitudes towards trade. Beaulieu (2002) finds that attitudes align with interests as predicted by economic theory. Sabet (2016) finds symbolic sources of trade preference (e.g., nationalism) to be important. Lack of information or "priming" also affects trade attitudes, see Rho and Tomz (2017) and Hainmueller and Hiscox (2006). It also has shown that trade elicits strong protectionism responses (Di Tella & Rodrik, 2020) and that socio-demographics explain heterogeneity in trade attitudes (Mayda & Rodrik, 2005). We add to these studies by documenting differences in public attitudes in relation to trade in a low-income country and studying factors and mechanisms driving these attitudes.

The rest of the paper is organized as follows. Section II describes the datasets. The trade exposure measures and estimation strategy are presented in Section III. Section IV reports results, including descriptive statistics and regressions results. Section V concludes.

II. Data

Firm Balance Sheet, Employer-Employee, Firm-to-Firm Transaction and Customs Data To construct firm and worker measures of exposure, we combine four types of administrative datasets from Uganda. This data is collected by the Ugandan Revenue Authority when enforcing personal, corporate, value-added and customs taxation. It includes information on the universe of formal firms and workers between 2009-2021. The datasets are combined as follows. In a first step, we use corporate income tax returns to obtain information on a firm's balance sheet (e.g., sales, inputs, employees). We match this data with the employee data by means of pay-as-you-go (PAYE) tax forms that employers fill out when declaring individual taxation on behalf of their employees. This data contains details on worker earnings and any additional allowances (e.g., transport, medical, housing). In a third step, we merge this with information from the value-added tax return, which requires firms to report on each transaction with their domestic supplier and buyer, as well as the

transacted value. This data permits to construct the domestic production network of each firm. In a last step, we match the data to information from the customs tax office, which records for each firm any foreign transaction, including the specific product code, the source (destination) country of the imports (exports), the amount transacted, as well as the quantity (and therefore the price). As we show in the next section, each data source has its particular purpose: balance sheet data combined with customs data allow to construct a measure of *direct firm exposure*, the matched employer-employee data together with household surveys offers a *worker exposure measure*, and the firm-to-firm data permits to construct an *indirect (network) exposure* measure to the firm. From the corporate and individual tax returns we importantly obtain the specific location of firms and their workers, which allows us to use the locality (or parish) as the unit of analysis in our empirical strategy.

Representative Household Surveys To construct a *household* measure of exposure, we merge the customs with representative household survey data, as collected by the Ugandan Bureau of Statistics. From the customs data we obtain the price of each imported good from each source country and record the value of individual and total imports. The household surveys provide information on the consumption basket (both durable and non-durable goods) of each household. To combine data, we first map the consumption basket for each household and then link it, for each product and source country, to the price data from the customs returns. In total, the household surveys sampled around 3,000 households during each of the six survey rounds (in 2010, 2011, 2012, 2014, 2016 and 2019), resulting in a pooled sample of 18,000 households. It is sampled across 790 enumeration areas (EAs) at parish level. From the household surveys we further obtain information on formal and informal labour earnings by education-level, age and gender, which permits to study the effect of exposure on formal and informal employment, as well as to gauge any distributional effects of trade exposure.[12]

Public Attitudes Surveys To measure the different dimensions of social cohesion over time and across localities, we deploy data from Afrobarometer. These representative surveys measure public attitudes at the individual-level, asking respondents about attitudes towards (economic) conditions, (political) freedoms, (civil and political) participation, the political system, (political and personal) trust, public good provision, migration and economic opportunities. From the survey we capture specific questions that relate to three main dimensions of social cohesions: trust, participation, and identity (see Appendix A). For example, to record political trust, we employ the question on "how much do you trust the president, police, courts of law, etc." on a scale of 0 (not at all) to 3 (a lot). Each question is scaled differently, although across each dimension, higher (lower) values reflect higher (lower) cohesion. In total, we construct six dimensions of social cohesion: political and interpersonal trust, civil and political participation, and national and group identity. Individual responses are geocoded at the village- and town-level, which allows us to match it to the parish-level exposure measures at the firm, worker and household level. We obtain data for all available survey rounds (2002, 2005, 2008, 2012, 2015, 2017, 2019, 2020 and 2021), which results in a

[12] Between 2009-2011, 58% of Uganda's workforce was employed in the informal sector, of which were 13% paid employees, 23% unpaid helpers and 63% working proprietors (subsistence farmers) (ODI, 2015).

pooled sample of 21,600 observations. In Appendix B we discuss in detail how we ensure that the attitude data is representative at the parish-level.

Exchange Rates Changes In a final step, we compute the bilateral real exchange rates which we use in the construction of our instrument. As there are no readily available bilateral exchange rate data, we first compute bilateral nominal exchange rates for each of the trading partners to Uganda using the cross-exchange rates of country *c* and *d* against the US dollar. Here, exchange rates of country *c* is the bilateral exchange rate between Uganda and US, while exchange rates of country *d* is the exchange rate between US and respective Ugandan's trading partners. Original data used for this computation is obtained from IMF International Financial Statistics. For those countries not in the database, we manually gather information from corresponding central banks. This entails using the formula $\frac{EP^*}{P}$ to obtain the bilateral real exchange rate where E is the monthly average bilateral nominal exchange rate between country *c* and *d* (local currency per unit of foreign currency) and P^* and P are foreign and domestic consumer price indexes (CPI) with a base period of 2010. Thus, an increase in RER is a real depreciation of the home currency.

A detailed description of the construction, cleaning and matching procedures of the above datasets is found in Appendix B.

III. Measurement and Empirical Strategy

a. Measuring Trade Exposure

The ideal empirical setting to study trade exposure and social cohesion consequences would be to randomly assign trade exposure to individual firms and households and record cohesion outcomes. In absence of random treatment, we propose an alternative strategy, where we use exchange rates as plausible exogenous variation. The reason is that firms and households are unlikely to anticipate such shocks or adjust to them in the short-term. We exploit the fact that firms (and through them workers) and households are exposed differently to changes in exchange rate when importing and exporting goods. To approximate an experimental setting where treatment effects of trade exposure can be measured at detailed levels, we construct a granular measure of trade exposure at the firm-, worker- and household-level. The benefit of these granular exposure measures is that they exclude potential co-founders that may bias our estimates. A major concern is self-selection. For example, if firms self-select based on their input-output structure to trade exposure, it will lead to an upwards biased estimate of the treatment effects of trade exposure on social cohesion. Our assumption in the empirical strategy is that, at least in the short-run and based on the exposure to trade, there is no self-selection of (i) firms into type of input-output structure, (ii) workers based on the input-output structure of a firm, and (iii) households based on the import share of product groups. For example, a worker-specific measure records exposure to changes in exchange rates but is orthogonal to potential worker selection into firms as workers are unlikely to select into firms depending on its input-output structure.[13]

[13] A similar approach is used by Méndez-Chacón and Van Patten (2023).

Direct Firm Exposure We assume a framework where firms source inputs from domestic and foreign markets, while allocate output between domestic and export markets to raise revenue.[14] In that way, firm exposure can be defined as

$$FirmExp_{it} = \sum_{d=1}^{D} \sum_{j=1}^{J} \frac{X_{idt}}{L_{it}} \Delta\tau_t^{d,X} + \frac{M_{idt}}{L_{it}} \Delta\tau_t^{d,M} \quad (1)$$

where X_{idt} represents firm i's exports to destination country d in year t, $\Delta\tau_t^{d,X}$ is change in the exchange rate to destination country d in year t, M_{idt} is firm i's imports from source country d in year t, and $\Delta\tau_t^{d,M}$ is the change in the exchange rate to source country d in year t. Exports and imports are normalized by number of workers of firm i (L_{it}) to account for size and productivity differences between firms. The intuition behind the above equation is that firms that have exported (imported) more to (from) a given country see a larger positive (negative) shock to their marginal revenue productivity when the Ugandan Shilling depreciates relative to the foreign currency in country d, because each exported (imported) unit is relatively cheaper (costlier).

Indirect Firm Exposure To capture how shocks propagate (and amplify) from importing and exporting firms to domestic firms through domestic production networks, we construct an indirect measure of firm exposure:

$$IndFirmExp_{it} = \sum_{j=1}^{J} (v_{ji} - 1_{j=i}) \cdot FirmExp_{jt} + \sum_{j=1}^{J} (v_{fj} - 1_{j=i}) \cdot FirmExp_{jt} \quad (2)$$

(upstream effect) *(downstream effect)*

where the element v_{ij} capture input-output linkages and measures how reliant firm i's production is on inputs of firm j, considering all direct and indirect effects. 1 is an indicator function, which eliminates the direct effect of a shock to firm i. It takes the value 1 when $j = i$ and value 0 otherwise. We make a distinction between upstream effects (firms consuming inputs from firm i) and downstream effects (firms providing inputs to firm i) to distinguish between different types of network shocks.

To construct the network structure, we use the firm linkage data to create a $N \times N$ matrix of firm production dependencies, $W' = [w_{ij}]$, where w_{ij} measures the direct influence of input from firm j on the production of firm i. $(W')^2$ captures all second-order interdependencies between sectors, and so on for all $(W')^n$. Using the Leontief inverse, the infinite rounds of network effects can be captured using:

$$V = [v_{ij}] = I + \alpha W' + \alpha^2 (W')^2 + \cdots = (I - \alpha W')^{-1} \quad (3)$$

[14] For such a model of heterogeneous firms on the exporting-side, see Helpman, Itskhoki, Muendler, and Redding (2017).

Note that direct and indirect measures capture *formal* firm exposures we use tax data. While informal firms constitute an important part of the Ugandan economy, it is unlikely that they are exposed to trade shocks directly given that they mainly operate in local and informal markets. However, our expectation is that shocks to formal firms (either directly or through the domestic production network) may still affect informal firms through demand and supply linkages, which in turn could lead to change in social cohesion outcomes. The degree to which this occurs we can partly capture in our earning exposure measure below.

Worker and Household Earnings Exposure We construct two earning exposure measures. The aim is to capture exposure in both formal and informal markets. First, we use information on basic salary from the PAYE schedule data to construct formal *workers earning exposure*, which we specify as

$$WorkerExp_{kit} = \sum_{i=1} (FirmExp_{it} + IndFirmExp_{it}) * Wage_{kit} \quad (4)$$

where all subscripts are as previously defined, $Wage_{kit}$ is the basic salary of individual k in firm i situated at period t.[15] The intuition of the above equation is that exposure to the firm (directly or indirectly) and its shock to its marginal revenue productivity is fully passed-on to its workers and their wage.[16]

The above earning exposure measure captures individuals in formal employment. We complement this with household data to observe earnings in the informal sector. As the household data does not hold information on a firm's input-output structure, we use an alternative specification where we aggregate to locality-level and integrate formal firm exposure such that

$$WorkerExp_{hlt} = \sum_{s=1} (FirmExp_{lt} + IndFirmExp_{lt}) * \ Earning_{hlt} \quad (5)$$

where $FirmExp_{lt}$ and $IndFirmExp_{lt}$ are the respective *average* direct and indirect exposure at parish l (as obtained from administrative data), while $Earning_{hlt}$ is the disclosed labour income (both formal and informal) of household h situated in parish l. As described, the underlying assumption is that shocks to formal firms propagate to informal sectors by means of demand linkages between formal firms and informal workers.[17] Given that we deploy the entire household sample, which includes both formal and informal workers, comparing estimates of equation (4) and (5) can suggest the degree to which trade exposure operates through the informal versus the formal labour market. The household data also offers the advantage to check for heterogeneous effects across the worker distribution, e.g. sociodemographic characteristics (e.g., gender).

[15] This is work-in-progress: we construct the worker earning exposure solely on the basis of direct firm exposure and omit indirect firm exposure.

[16] A larger literature explores how variation in firm productivity affects worker earnings (Kline, Petkova, Williams, & Zidar, 2019)

[17] In Uganda, 51.5 percent of the informal market consists of agriculture subsistence workers (ILO, 2021). While there are many frictions (e.g. search) that inhibit linkages between formal and informal markets, there exist private sector market brokerage firms and traders that match formal buyer contracts to informal suppliers (Bergquist, McIntosh, & Startz, 2021).

Household Expenditure Exposure Finally, we define a household-level measure of exposure through *expenditure*. To do so, we use representative household surveys that hold information on the basket of consumption, which in combination with customs data, allows to define expenditure exposure as

$$HouseholdExp_{ht} = \sum_{j=1}^{J} (-\Delta p_{\mathrm{q}t})(s_{\mathrm{q}ht} - S_{\mathrm{q}t}) \quad (6)$$

where p_{q} is the price of good q, $s_{\mathrm{q}ht}$ is the share of good q in the consumption basket of household h, and $S_{\mathrm{q}t}$ is share of good q in aggregate expenditures in year t. The household consumption data includes both purchased and home-grown products; therefore, for the latter, we set p to zero as home-grown products are not directly exposed to trade shocks. $-\Delta p_{\mathrm{q}t} s_{\mathrm{q}ht}$ is the variable of interest and captures changes to costs of living for households, i.e. consumer expenditure effect. As with worker exposure, we use household characteristics recorded in the consumption data to examine how effects through household exposure differ along several sociodemographic characteristics.

To capture price changes p, we identify exchange rate changes and import shares from each country d as follows:

$$p_{\mathrm{q}t} = \sum_{d=1}^{D} \Delta\tau_t^{dM} M_{\mathrm{q}dt} \Big/ M_{\mathrm{q}t} \quad (7)$$

where $\Delta\tau_t^{dM}$ is change in exchange rate from source country d in year t, while $M_{\mathrm{q}dt} \Big/ M_{\mathrm{q}t}$ captures the share of imports of good q from source country d to total imports, such that p_{q} is an import-share weighted measure. As with firm exposure, the intuition of the measure is that households using imported goods from a given country d see a negative shock to their expenditures when the Ugandan Shilling depreciates relative to foreign currency in country d, because imported goods become relatively more costly.[18]

b. Empirical Strategy

To examine the link between trade exposure and social cohesion, we follow a two-staged approach. In the first stage we are interested in capturing the causal relation between exposure and cohesion. As described in the data section, we exploit the richness of the micro data and use parish as the unit of observation throughout the analysis. We estimate

$$lnY_{lt} = \gamma + blnExp_{lt} + Z_{lt}d + \varepsilon_{lt} \quad (8)$$

[18] It is important to account for the fact that exchange shocks affect consumers both directly and indirectly (Jaravel, 2021); *directly* through the consumption of imported goods and *indirectly* through imported intermediate input used in production of domestic goods. We address this in a next version of the paper.

where lnY_{lt} is the log of the outcome variable, where depending on the equation it is one of the six dimensions of social cohesion in locality l for year t. γ is the intercept, while $lnExp_{lt}$ is the log of the *average* trade exposure in a locality l at year t. b is therefore the leading coefficient that captures the effect of trade exposure on social cohesion. Depending on the equation, $lnExp_{lt}$ is either the direct or indirect firm exposure, household exposure, or worker earning exposure in logs. Z_{lt} is a vector of time-varying covariates at parish-level, and d the vector of coefficients. Inspired by existing literature (see Green & Preston, 2001; Letki, 2008; Richey, 2010; Schiefer & Van der Noll, 2017), we add as control socio-demographic characteristics (including average age, share of female, share of people employed and ethnic diversity), institutional quality and access to public goods and services.[19] It is important to include these controls to capture the true effect of exposure on cohesion. It is plausible, for instance, that variation in political trust between localities is due to differences in institutional quality as opposed to actual differential trust in local institutions between localities. Standard errors are clustered at parish-level.

IV. Trade Exposure Reduces Trust, Enhances Participation and has Ambiguous Effects on Identity

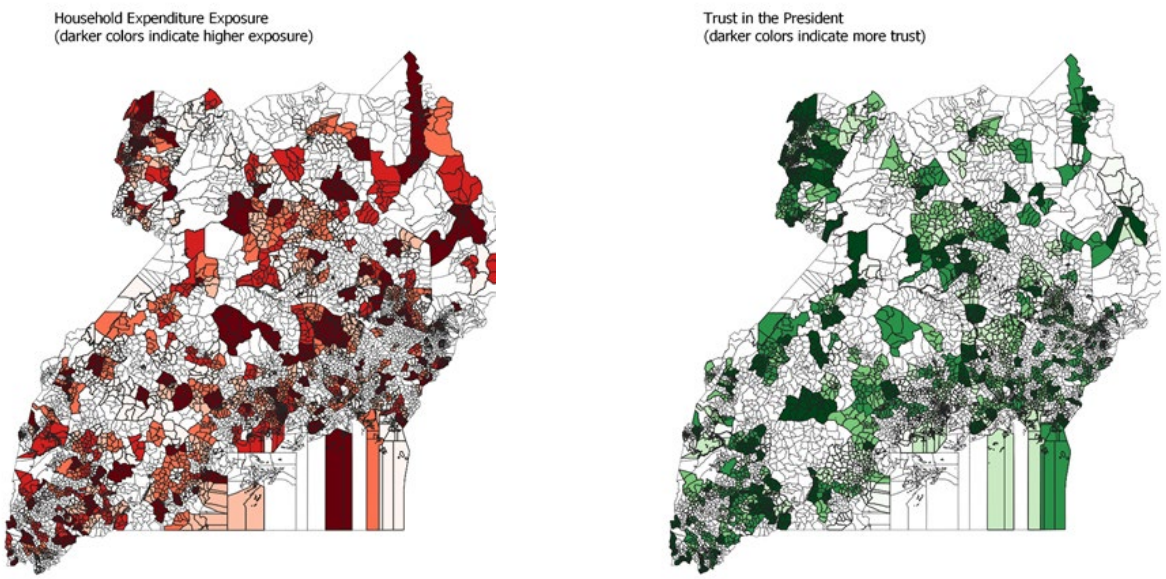
a. Descriptive Statistics

In what follows we explore the association between trade exposure and social cohesion, using the full sample of levels and changes in trade exposure and social cohesion at parish level between 2012-2018. Figure 1 shows the causal relation between exposure and cohesion that we aim to explore. The graphs show spatial variation in exposure and cohesion. Importantly, it suggests that on average there is a negative relation between exposure and trust; parishes with high household exposure are associated with lower trust levels in political institutions.

Figure 1 – Relation between Exposure and Cohesion

Trade Exposure ➔ **Social Cohesion**

[19] We measure institutional quality using the averages of two variables, local government corruption and police corruption. For access to public goods and service, we use the first principal components of four variables, access to electricity, water, and sewage and the inverse distance to school and hospital.



Notes: Graph shows household expenditure exposure (which is normalized to range between zero and one) and trust in the president (which ranges from zero to three) at locality level, using respectively representative household survey and public attitude data. Household exposure is constructed using equation (6).

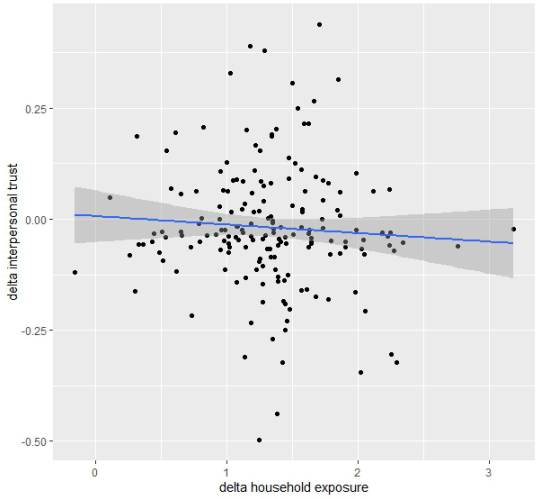
We explore this relation further and run naïve correlations between each type of exposure (i.e., firms, workers, and households) and across each social cohesion dimension (trust, participation, identity). A priori, the expectation is that exposure to trade reduces trust (both social and political), because people become less trustful to both others in society as well as those in political institutions when they see negative effects from trade. In contrast, it is expected that trade exposure raises participation (both with political institutions and in society) and identity (group identity, with ambiguous effects expected on national identity), because negative exposure to trade likely raises the willingness of people to voice concerns and raises togetherness within the nation or towards own (ethnic or religious) groups in response to trade shocks.

Figure 2 shows the results for *household exposure*, which confirm these expectations. When household exposure increases, we observe corresponding reductions in average trust levels and increases in average participation levels. These results hold both for the horizontal level (e.g., interpersonal trust) and vertical level (e.g., political trust) of cohesion. An increase in trade exposure to households is further associated with an increase in identification with groups within a society and a declining identification with the nation as a whole. Figure 5 in Appendix C shows how *direct firm exposure* correlates with each social cohesion dimension. The association between direct firm exposure and social cohesion is more muted but conveys a similar message: increased firm exposure at parish level is linked to reductions in trust (both to others in society and to political institutions), with insignificant but negative effects on participation and identity, i.e., higher exposure is linked to lower participation and identity (both across the horizontal and vertical dimension). This finding is plausible given that we expect direct exposure to household to elicit greater changes in social cohesion outcomes compared to the somewhat indirect exposure to households through shocks to firms where household may work or consume products from. Figure 6 in Appendix C shows correlations between *worker earning exposure* and social cohesion –

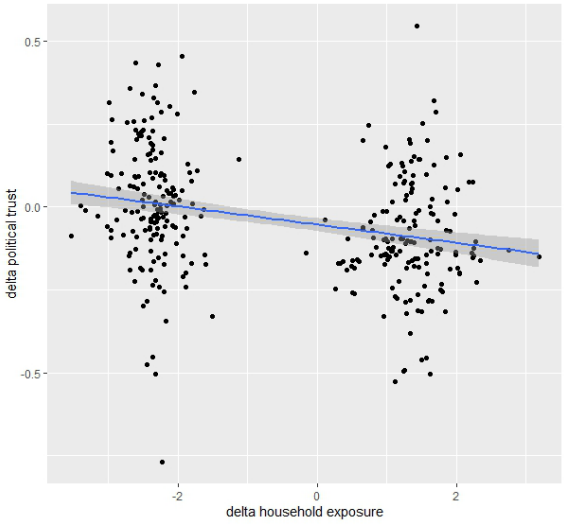
unsurprisingly, given that worker earning exposure is based on firm exposure, it exhibits similar results to direct firm exposure.

Figure 2 – Correlations between household exposure and social cohesion dimensions

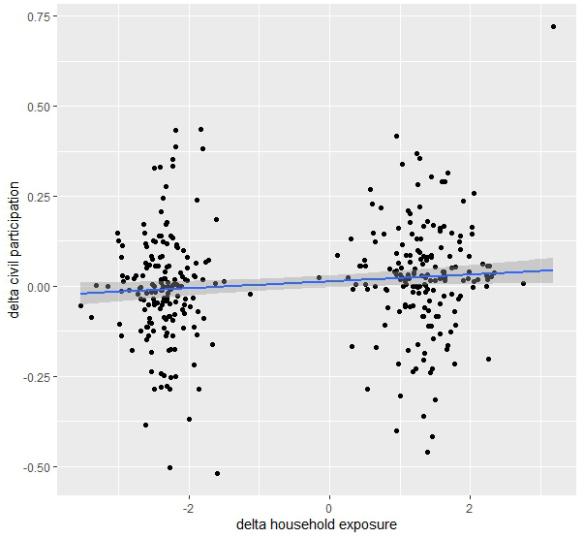
(A) Interpersonal Trust



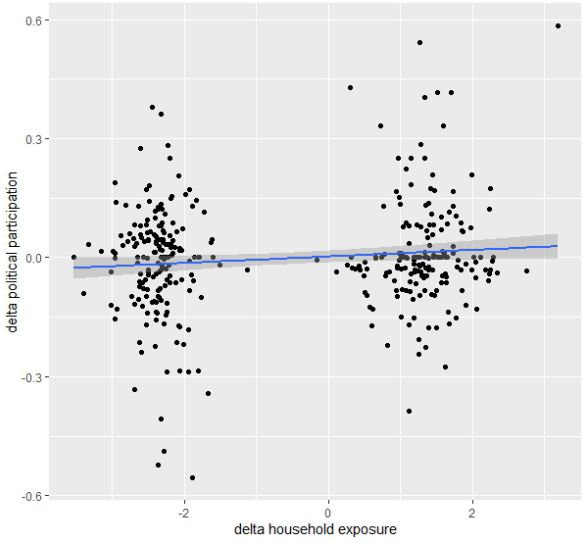
(B) Political Trust



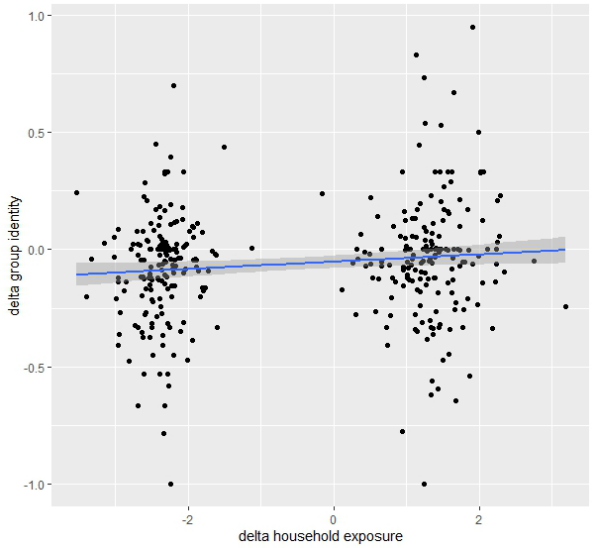
(C) Civil Participation



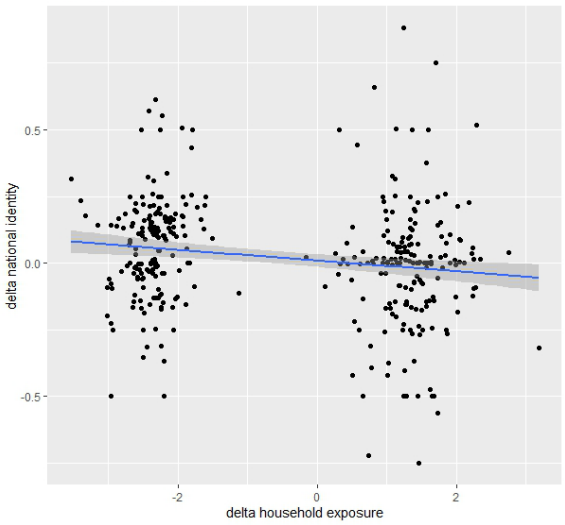
(D) Political Participation



(E) Group Identity



(F) National Identity



Notes: graph uses full sample of change in household trade exposure and social cohesion at parish level between 2012-2015 and equation (6). Each data point represents the average change in values for a particular parish in either 2012-2015 or 2015-2018 (interpersonal trust only has data for 2015-2018). It excludes data points on parishes that have zero exposure (and therefore zero change) across any two years. Social cohesion values are normalized to range from zero (0) to one (1). Household expenditure exposure is log-transformed. Appendix B describes the number of parishes for which trade exposure and social cohesion information is available.

b. Regression Estimation

In a next step we explore the causal relation between trade exposure and social cohesion using the full sample of data points at parish level between 2012-2018 and using equation (8). Given that the exchange rate variation at the core of our exposure measure is plausibly exogenous, a simple regression of social cohesion on trade exposure is sufficient to offers causal estimates. Table 1 presents the regression results for (i) direct firm exposure, (ii) worker exposure, and (iii) worker earning exposure.[20] Each reported regression result contains unreported time-varying covariates as discussed in the preceding section.

Panel A shows the results of *direct firm exposure* on dimensions of social cohesion. Direct firm exposure has a negative and significant but economically small effect on political trust and civic participation. A one percent increase in direct firm exposure reduces political trust by 0.002 percent and civic participation by 0.001 percent. This suggests that a trade shock through the exchange rate leads to reductions in trust of political institutions and leads people to reduce their participation in public actions, such as attending a community meeting or participate in demonstrations. Otherwise, the estimated coefficient of direct firm exposure for all other social cohesion outcomes is statistically insignificant. Although these effects are economically small, it fits with a priori expectations that firm exposure affects social cohesion only indirectly if shocks propagate to its workers and to consumers of its products. This is confirmed by economically larger

[20] A next version of the paper includes indirect firm exposure and household earning exposure.

effects found for worker and household exposure (see Panel B and C). It also fits to other studies that cohesion is likely affected by multiple factors and trade may not be the main driver of social cohesion outcomes (e.g., Schiefer & Van der Noll, 2017).

Panel B reports results on how *workers earning exposure* to trade affects cohesion. Similar to direct firm exposure, worker earning exposure is a significant (albeit economically unimportant) predictor of political trust and civic participation changes. A one percent rise in worker exposure lowers political trust by 0.002 percent and civic participation by 0.001 percent. We also find that workers voice to lesser extent their views to government when their exposure to trade through their employer increases – a one percent rise in worker earning exposure also reduces political participation by 0.001 percent. This is somewhat surprising given that shocks to employment and wages likely result in workers voicing concerns towards government. Perhaps workers mitigate negative shock through temporary self-employment or the informal sector (Amodio et al., 2023), employers observe part of the shock to wages, or households instead of voicing concerns directly to politicians participate in civic participation. Generally, given that workers are exposed through their employers it is not surprising that we obtain similar results to direct firm exposure.

Panel C shows the result for *household expenditure exposure*. Except for civic participation, the estimated coefficient of household exposure is statistically significant across all social cohesion outcomes. Household expenditure exposure has statistically significant and negative effects on political and personal trust, and national identity. It positively and significantly influences civic and political participation, and group identity. This suggests that when households see negative trade exposure through price increase in their consumption basket, households reduce their trust (both to others in society and towards political institutions), increase participation (both in public actions and to political actors), with ambiguous effects on identity (reducing their affiliation to national identity and increasing affiliation towards own ethnic or religious groups). Compared to firm and worker exposure, these effects are economically much larger albeit remain small – for example, a 0.014 percent reduction in political trust with each one percent rise in household expenditure exposure. These results suggest that trade shocks from exchange rate fluctuations operate largely by means of the expenditure channel, with lesser effects occurring through the earnings expenditure. Note that compared to earnings exposure, political participation is statistically significant, positive and economically larger, but civic participation is no longer significant. One interpretation of this is that negative trade shocks stifle household expenditure, and without any absorption mechanism other than savings (and as opposed to earnings where firms could capture part of the shock), it is more likely that household directly engage in politics to voice their concerns.

Table 1 – Regression results on Trade exposure and Social Cohesion

	Trust		Participation		Identity	
	Political trust	**Interpersonal trust**	**Civic participation**	**Political participation**	**National Identity**	**Group Identity**
Panel A						
Firm Direct Exposure	-0.002***	-0.000	-0.001***	-0.001	0.000	-0.000
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
Controls	Yes	Yes	Yes	Yes	Yes	Yes

No. of Observation	694	567	811	721	812	806
R-Squared	0.061	0.036	0.063	0.024	0.013	0.033
Panel B						
Workers Earning Exposure	-0.002***	-0.000	-0.001**	-0.001***	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
No. of Observation	712	582	850	739	850	845
R-Squared	0.083	0.048	0.051	0.034	0.018	0.037
Panel C						
Household exposure	-0.014***	-0.018***	0.001	0.009***	-0.014***	0.034***
	(0.004)	(0.006)	(0.003)	(0.003)	(0.004)	(0.006)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
No. of Observation	952	725	981	987	987	983
R-Squared	0.088	0.070	0.055	0.028	0.049	0.067

Note. Standard errors clustered at the parish level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Each reported regression result contains unreported time-varying covariates. The outcome variable and the trade exposure measures are both expressed in logs. Worker earning exposure is work-in-progress and only includes direct firm exposure and excludes indirect firm exposure.

V. Concluding Remarks

We use rich micro administrative data and a natural experiment of exchange rate liberalization in Uganda to provide causal evidence that trade exposure reduces trust, enhances participation and has ambiguous effects on identity. Our results show that exposure to exogenous exchange rate shocks has significant but economically small effects on social cohesion and operates mainly through the expenditure channel and a lesser extent through the earnings channel. We find that a one percent increase in *household exposure* results in 0.018 and 0.014 percent reductions in social and political trust, a 0.009 percent increase in political participation, and a 0.014 reduction in national identity but 0.034 increase in affiliation to group identity. The effects of *worker earning exposure* and *direct firm exposure* are less pronounced, both in magnitude and conventional statistical levels across different cohesion dimensions. These results hold after including a range of controls, including socio-demographics, institutional quality and access, and other factors, which may plausibly affect social cohesion levels.

Our results suggest that trade exposure is a relevant (but small) predictor of social cohesion and therefore trade shocks may cumulate into social disintegration; it (i) lowers trust of people towards others and towards political institutions, it (ii) enhances participation (i.e. participation in public meetings when labour earnings are exposed, and outreach to political constituents when expenditures are exposed), and (iii) affects identity by lowering affiliation with ethnic and religious groups whilst reducing affiliation with national identity. These results somewhat contrast other research that trade expenditure effects are often diffuse (Stantcheva, 2022) and therefore unlikely to lead to social disintegration. Instead, we find that effects are economically larger (almost by a factor of seven for political trust) compared to firm and earning exposure. One interpretation is that workers (through savings or temporary employment) or firms (through reduced profits) might

absorb some of the trade shock in case of worker earnings, while shocks to expenditure are directly absorbed by households if government has no relevant policy mechanism in place (e.g., price caps). For policy, our results therefore point to the importance of introducing policy mechanisms that can mitigate expenditure shocks, and by this lower the probability of social disintegration.

We see three avenues for future research (of which some we address in the next version of the paper). First, our results point to significant social cohesion effects from exposure to exchange rate fluctuation. Whilst an important component of trade policy, exchange rate policy is one component among many and other policies should be evaluated (e.g., tariffs, quotas). Second, it is plausible that the general effects we observe differ across the firm, worker and household distribution. We see it as an important step forward to study effects of exposure across industries (for firms), earning scales (for workers) and household characteristics, e.g., age, gender (for households). Finally, as described, an open question is how trade exposure manifests itself into social cohesion outcomes and why it seems to propagate more strongly through household expenditures as opposed through worker earnings. Studying these mechanisms is an important avenue for future research.

V. Appendices

A. Selected Questions on Trust, Participation and Identity from the Afrobarometer Survey

Dimension	*Survey Question*	*Answer Scale*	*Variable Construct*
Political Trust	Q41: How much do you trust each of the following: The president The national assembly Electoral commission Local government council Police Armed forces Courts of law	Not at all (0) – A lot (3)	Mean across all political actors
Interpersonal Trust	Q86: Would you want people from the following group as neighbours: A) People of a different religion B) People from other ethnic groups C) Homosexuals D) Immigrants or foreign workers E) People who support a different political party	Strongly dislike (1) – Strongly like (5)	Mean across question dimension
Civil participation	Q11: Which of the following actions that people sometimes take as citizens have you done: A) Attended a community meeting B) Get together with others to raise an issue C) Participate in a demonstration or protest march	If Yes: often (4) several times (3), once or twice (2). If No: if had the chance (1), never (0)	Mean across all activities

Political participation	Q12: During the past year, how often have you contacted any of the following persons about some important problem or to give them your views? A) Local government councillor B) Member of parliament C) Political party official D) Traditional leader	Never (0) – Often (3)	Mean across all activities
National identity	Q82B: Do you more identify with being a Ugandan or with your ethnic group?	I feel only Ugandan (5) – I feel only [ethnic group] (1)	Mean across both questions
	Q85: Is there more that unites all Ugandans as one people, or more that divides them?	Much more that unites them (4) – Much more that divides us (1)	
Group identity	Q82A: How often, if ever, is your ethnic group treated unfairly by the government?	Never (0) – Always (3)	Mean across all questions
	Q82CD: Do you feel comfortable: A) Speaking your mother tongue in public B) Wearing your traditional or cultural dress in public	No (0) or Yes (1)	
	Q84: Have you been treated unfairly by other Ugandans based on: A) Your economic status B) Your religion C) Your ethnicity	Never (0) – Many times (3)	

B. Data Structuring, Cleaning and Matching

This section discusses the construction, cleaning and the characteristics of the final datasets.

a. *Data Conversion and Matching Process*

In terms of matching all datasets, we started from the public attitude data (our outcome variables of interest) as the baseline structure and matched all administrative and survey data to this data, in the following order: (i) household survey data, (ii) customs data, (iii) firm data, and (iv) worker data. We discuss this procedure, including cleaning procedures and robustness checks below.

Each dataset is constructed and matched at the parish level (the unit of analysis). As a spatial baseline we use 2006 parish boundaries.[21] In order to obtain as many unique matches as possible, we performed a fuzzy approach during each matching step. After the match, we went through all non-perfectly matched locations to check compatibility. To be compatible, locations have to have at least two perfectly matching geographical levels (i.e., parish, sub-country, country, region). For example, if an observation has the same parish and sub-county name, but not county name of the 2006 boundaries file, it is considered matched. If for an observation only the parish name matches the 2006 boundaries file, the two are not considered matched.

b. *Matching public attitude and household data*

First, we match public attitude to household data to obtain a sample of localities capturing changes over time in (i) public attitudes and (ii) household (informal) employment and expenditure. (ii) is in a later step used to construct the worker earnings and household exposure measures. The attitude and household data are each independently constructed using a random sample, which means that we do not have comparable localities in each year. To address this, we adopt several procedures, as discussed below.

We obtain attitude data from Afrobarometer (AB), which measures attitudes on trust, identity and participation at the individual-level in 2010, 2011, 2012, 2015, and 2017. The household (HH) data are from the Ugandan Bureau of Statistics and available for 2010, 2011, 2013, 2015 and 2018. To construct the AB data, 2,400 individuals were sampled approximately in each round. For the HH data, around 25,000 individuals across 3,000 households were sampled.[22] The lowest spatial level in the AB data is the town-village (GPS coordinates are also provided). In the HH data the parish is the lowest geolocation level. The AB and HH data both hold data on socio-demographics (e.g., age), which we use to match AB and HH data and ensure representativeness of the data at the locality level (see discussion below).

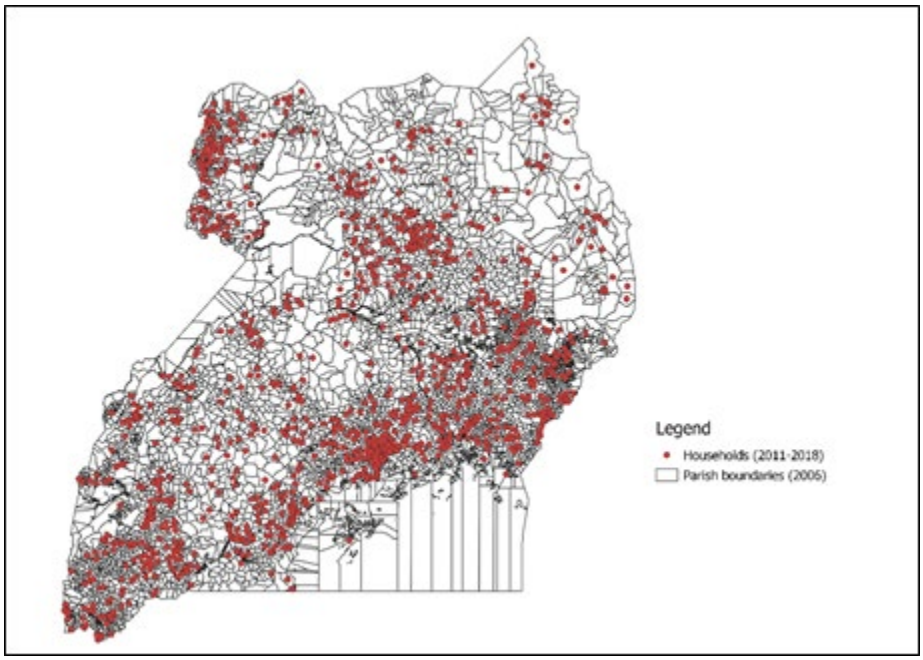
With this data at hand, we proceed as follows. First, we match all AB and HH data for years that correspond (i.e., 2010, 2011 and 2015). To ensure sufficient overlapping years, we match 2012 AB data to 2013 HH data and 2017 AB data to 2018 HH data, generating a panel of five years: 2010, 2011, 2012/13, 2015, and 2017/18. We drop observations in AB with missing values on location or parish name (this drops 10 percent of the sample). Importantly, this does not affect mean values – households with no location information also have missing values for most variables.[23]

In a second step, we construct AB and HH data at locality level, using the 2006 parish level map as the boundary structure. We treat the town-village in the AB data and the parish in the HH data

[21] The underlying assumption is that parish boundaries do not change over time (there are no maps to verify this, but anecdotal evidence suggests this is unlikely).
[22] The sampling of individuals (households) for the HH data differed across years: 25,579 (2,716) in 2010, 27,327 (2,850) in 2011, 21,194 (3,119) in 2013, 22,893 (6,617) in 2015, and 44,468 (3,242) in 2018. The sampling procedure for the AB data is described here: https://www.afrobarometer.org/surveys-and-methods/sampling/. The sampling procedure for HH data here: https://microdata.worldbank.org/index.php/catalog/3902
[23] The exception is the variable age. However, mean values across both samples are similar; average age before dropping the missing values is 35.2, after dropping it is 35.1.

as comparable localities (we do name-matching to check this and find that they are).[24] For the HH data, Figure 3 shows locations of households across parishes between 2011-2018; 82.5 percent of parishes are uniquely associated with a parish in the HH data. As AB data from 2010 and 2011 do not report GPS coordinates, we match parish names reported in Afrobarometer data with those in the 2006 parish map. For the AB data for years 2012-2017, GPS coordinates were used to localise town-villages into parishes. Figure 4 shows household locations for the AB data. Approximately 96 percent of parishes have AB data available.

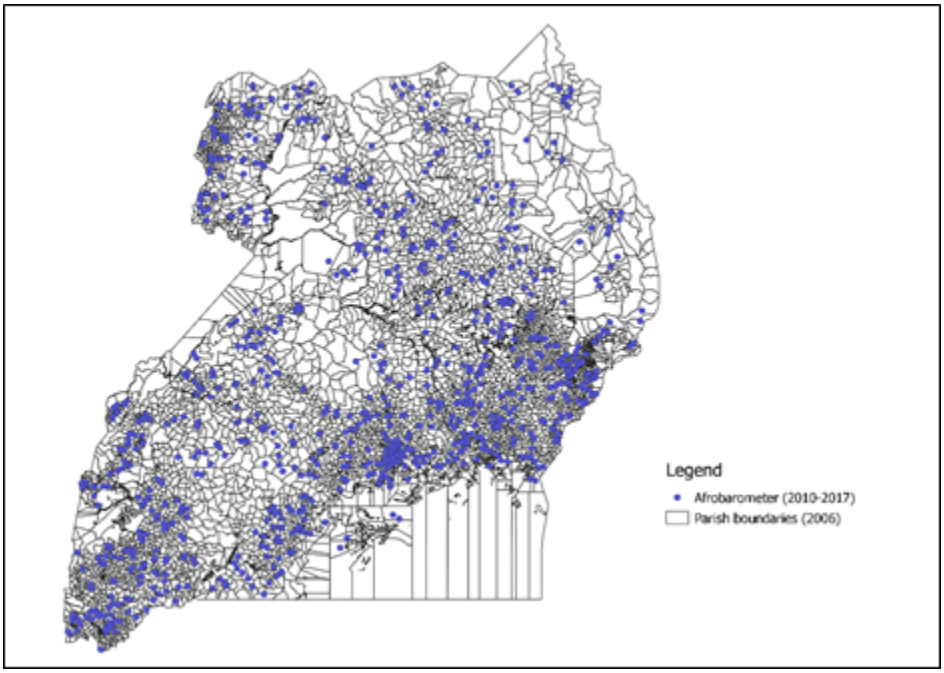
Figure 3 – Location households across parishes in Uganda (2011-2018)



Source: Household surveys. Note: each dot represents a household observation in the household surveys conducted between 2011-2018.

Figure 4 – Location Afrobarometer households across parishes in Uganda (2010-2017)

[24] AB data reports both town and parish names, and we find that they correspond.



Source: Afrobarometer surveys. Note: each dot represents a household observation in the AB surveys conducted between 2010-2017.

After constructing the locality level data, the number of parishes for which data is available from both AB or HH in each year are: 37 parishes in 2010, 38 parishes in 2011, 44 parishes in 2012/13, 54 parishes in 2015, and 25 parishes in 2017/18. A total of 198 observations are matched between AB and HH data, which corresponds to 59 unique parishes. Table 2 below reports how many years of information are present for these parishes. (In our analysis, we use panel variation and thus drop the parishes for which we have only one year of data.)

Table 2 – Coverage AB-HH panel data across localities

	1 year	**2 years**	**3 years**	**4 years**	**5 years**
Number of Parishes	6	21	14	6	12

As Table X shows, only few parishes are matched between the AB and HH datasets each year. The main cause is the limited number of parishes with AB data. To enhance this, we adopt a procedure where we create a buffer of 15 km around each parish, considering all parishes within this radius to be comparable to that of the AB parish (Martorano, Metzger, and Sanfilippo (2020) apply a similar approach using DHS data). All parishes within the radius are assigned data from the AB parish. This increases the number of parishes matched between AB and HH data from 198 to 1,086. The underlying assumption is that neighbouring parishes are homogenous. To test this, we perform one-way ANOVA analysis of several parish-level indicators to estimate intra-cluster correlation of HH parishes matched with AB parishes. Table 3 reports intra-cluster correlation and estimated

reliability (a group-averaged measure similar to intra-cluster correlation) and show to be relatively strong among matched HH parishes, therefore indicating homogeneity. Note that expenditure and consumption are least correlated indicators, meaning that there are differences in these variables across the parishes to which we add AB data using the buffer procedure.

Table 3 – Intra-cluster correlation of HH parishes matched with AB data

	Age	**Expenditure**	**Consumption**	**Gender of HH head**	**Employment**	**Average earning**	**Primary education**
Intra-cluster correlation	0.66	0.18	0.20	0.61	0.74	0.51	0.54
Estimated reliability of group-average	0.78	0.32	0.34	0.77	0.85	0.68	0.71

To improve locality matching across AB and HH data, we evaluate a propensity score matching (PSM) procedure where *within* each locality we allocate a propensity score between AB and HH observations based on socio-economic characteristics using nearest neighbour matching. With this procedure, individual-level AB and HH data are appended to generate a propensity score (which is done on employment levels, female headed-households and age; i.e. the socio-economic characteristics available in the data), which is used as a sample weight when collapsing the data to parish-level. The number of matched parishes remains comparable to that of the initial matching process (113 parishes), which means the PSM procedure does not drop parishes from the sample. In our final sample, we therefore introduce the buffer matching with PSM strategy, and obtain a total of 1,086 matched parishes.

To check the robustness of the final sample, we perform several exercises. First, we study whether matching AB and HH data at locality level affected the sample. To this end, we compare average values of social cohesion dimensions and socio-economic characteristics between the original and final sample. We find that there are no significant differences (e.g., share female, age, trust, identity, cooperation). We also check if the matching procedure affected the HH sample. Average values between individuals in the original HH sample and the HH data at locality level in the final sample we find are not significantly different from the original sample (e.g., on age, number of household members, share with primary education). Table 4 below reports average values between the two samples.

Table 4 - Comparison between individual and collapsed data – AB data

Variables	**Mean individual**	**Mean collapsed**
Age	21.7	21.5
Tot. food consumption	68715.5	67934.7

Tot. non-food consumption	39340.4	38411.3
Tot. non consumption	10280.1	12255.5
Tot. dur. consumption	30196.4	27771.4
Tot. expenditure	148532.4	146372.9
Tot. consumption	138252.3	134117.4
N. female	4.0	4.1
N. male	9.9	10.2
N. hh members	8.1	8.4
N. adults	4.4	4.8
% some primary edu.	0.5	0.5
% primary edu.	0.4	0.4
% higher edu.	0.1	0.1
% female headed	0.3	0.3
% employed	0.3	0.3
% informal emp.	0.8	0.8
Average earning	118940.8	140442.1
Avg. earning (informal)	96746.5	104811.0
Avg. earning (formal)	454851.2	553539.6

A potential threat to identification is that variation in our sample (either in cohesion, or household consumption and employment) is migration of individuals between regions. For example, changes in trust may result from the inflow of people that exhibit different trust levels from those people already in a locality (as opposed to changes in levels of individuals residing in a parish). To test this, we merge all possible households from HH data between years (i.e., 2010 with 2011, 2011 with 2013, and so on). Results are available for year pairs between 2010 and 2018. Between 2010 and 2011, less than 1 percent of the respondents reported having moved to another parish. Between 2011-2013, approximately 17 percent of households reported moving to a different parish, 15 percent between 2013-2015, and 12 percent in 2015-2018. As a robustness check, we drop households that move between regions from our sample (it drops 100 of 1,432 matched parishes). This does not affect mean values of the household characteristics, which suggests that between-region migration is unlikely to biases results.

c. *Matching administrative data to the baseline AB-HH dataset*

In a second step, we match administrative data to the AB-HH matched dataset. Following the same methodology, two merging strategies are used. First, we employ a perfect matching approach, i.e., every parish in the administrative data is matched with the same parish in the AB-HH data. Alternatively, we apply a buffer matching method. Administrative data is matched to parishes in the AB-HH data with the exact same name and parishes in the 15km radius. This includes several repetitions in the AB-HH data, i.e., AB-HH parishes are matched with more than one administrative data parish. If unique matches are considered, a total of 952 parishes are matched. As we discuss below, we merge three types of administrative data onto the baseline AB-HH data: firm, customs and worker data.

Firm Data The first merge constitutes adding firm (location) data from the individual firm dataset. Together with the customs data this data allows to construct (a) the *direct firm exposure* (from the individual firm data) and (b) the *indirect firm exposure* (from the firm-to-firm data). The firm administrative dataset records location at respectively the region, country, sub-country and parish level. Firms without parish level information are dropped from the sample. We also drop duplicates across localities (e.g., firms located in more than one location that report exactly the same values for total balance), which corresponds to a 3.55 percent reduction of the sample. To capture direct firm exposure accurately, we assign zero exposure to firms that operate but that do not import and export. After merging individual firm data on the AB-HH dataset we end up with 86 parishes for which we have information. When we apply the buffer approach, it increases the number of parishes to 1,086. Information on firm-to-firm data is obtained from the VAT data (this is work in progress).

Customs Data In a second step, we merge in the customs data. Together with the household consumption data (matched previously), this allows us to construct the *household expenditure exposure* measure. In particular, we match items consumed by households with goods imported. The household survey reports 140 durable and non-durable goods, and household location; firm imports are reported in HS codes. In the raw customs data, we observed 10-digit product codes, but in transforming the data to the appropriate structure, we selected the first 6 digits of HS codes (the customs data were too long to be matched with HS codes associated with items consumed by households). Subsequently, associated HS codes (i.e., those in the 10-digit level) were matched into the customs data. Over half of household and customs product categories was matched, while 279 associated codes remained unmatched. Further checks (through a fuzzy matching process) showed that no corresponding HS code could be found. A total of 940 correspondences were made between consumption items and HS importing codes (several HS codes matched to multiple consumption items in the household survey, some household items did not have a corresponding HS code). The customs data is available from 2012 (data on previous years is available in a different data structure and the authors are in the process of obtaining this data). This means that the household exposure considers 2013 (recorded as 2012), 2015, and 2018. Once collapsed at parish level, 1086 parishes hold relevant information (i.e., on firm and household expenditure exposure) across all years.

Earnings Data The third step includes merging in the worker earnings exposure. As described in the empirical strategy section, we construct two measures using (a) formal wage data from PAYE, and (b) formal and informal wage data from the household surveys (the latter is work in progress).

Because PAYE is filled-out by employers we observe the employer ID and therefore can obtain employee location (our assumption is thus that employees reside in parishes in which they work). After merging in the PAYE data, we have information on the relevant exposure measures for 206 parishes (and 1,086 parishes after applying the buffer approach).

d. *Final dataset characteristics*

Table 5 below shows descriptive statistics for each trade exposure and social cohesion dimension following the matching and cleaning procedures.

Table 5 – Descriptive Statistics of Final Dataset

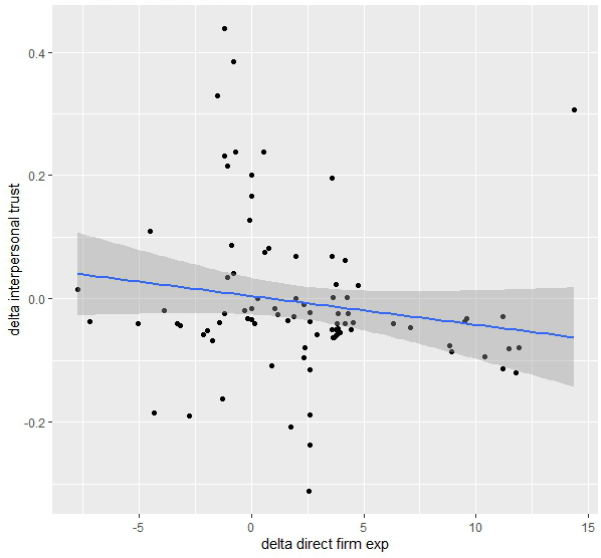
Measure	**Number of observations**	**Mean**	**Min**	**Max**
Trade Exposure				
Direct Firm Exposure	1135	1.81e+09	0	5.21e+11
Household Exposure	1470	-2.43	-20.88	0.61
Worker Earning Exposure	1173	6.2e+16	0	3.91e+19
Social Cohesion				
Interpersonal Trust	875	0.48	0	1
Political Trust	1272	0.58	0	1
Civil Participation	2069	0.39	0	1
Political Participation	1313	0.1	0	1
Group Identity	2072	0.31	0	1
National Identity	2078	0.56	0	1

Notes: the table shows the full sample of values at parish-level between 2012-2018. Direct firm exposure is zero for those firms with no imports or exports and hyperbolically (log) transformed. Working earning exposure shows exposure using formal wage information from the PAYE. Social cohesion dimensions are normalized to range between zero (o) and one (1). Note that we describe number of observations across available parishes across years.

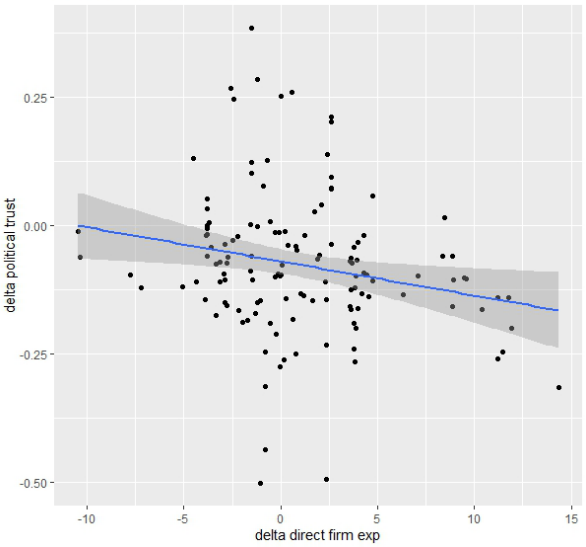
C. Additional Results

Figure 5 – Correlations between direct firm exposure and social cohesion dimensions

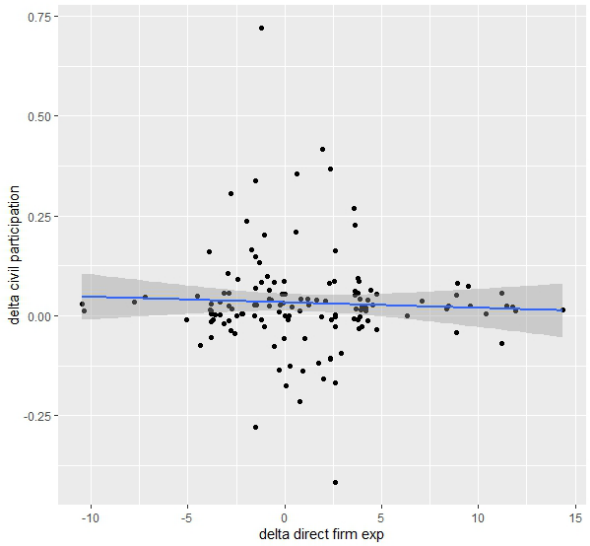
(A) Interpersonal Trust (B) Political Trust



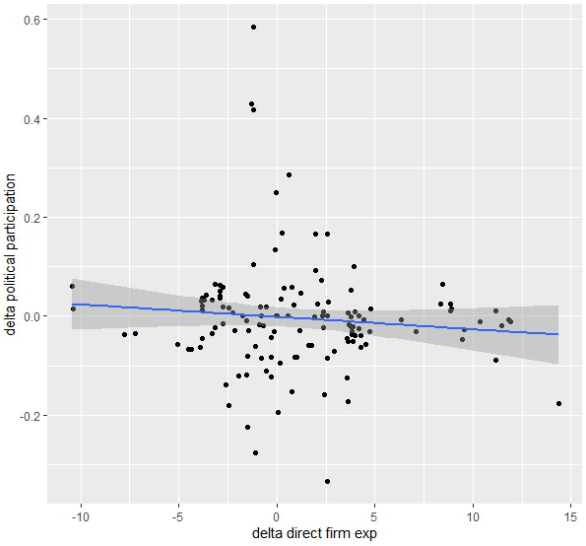
(C) Civil Participation



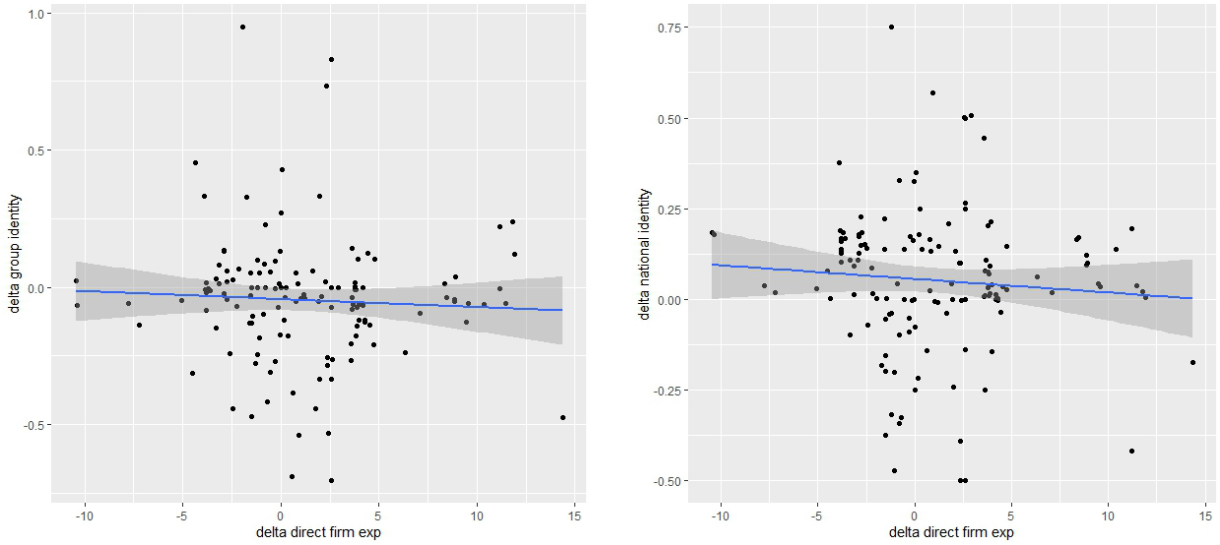
(D) Political Participation



(E) Group Identity



(F) National Identity

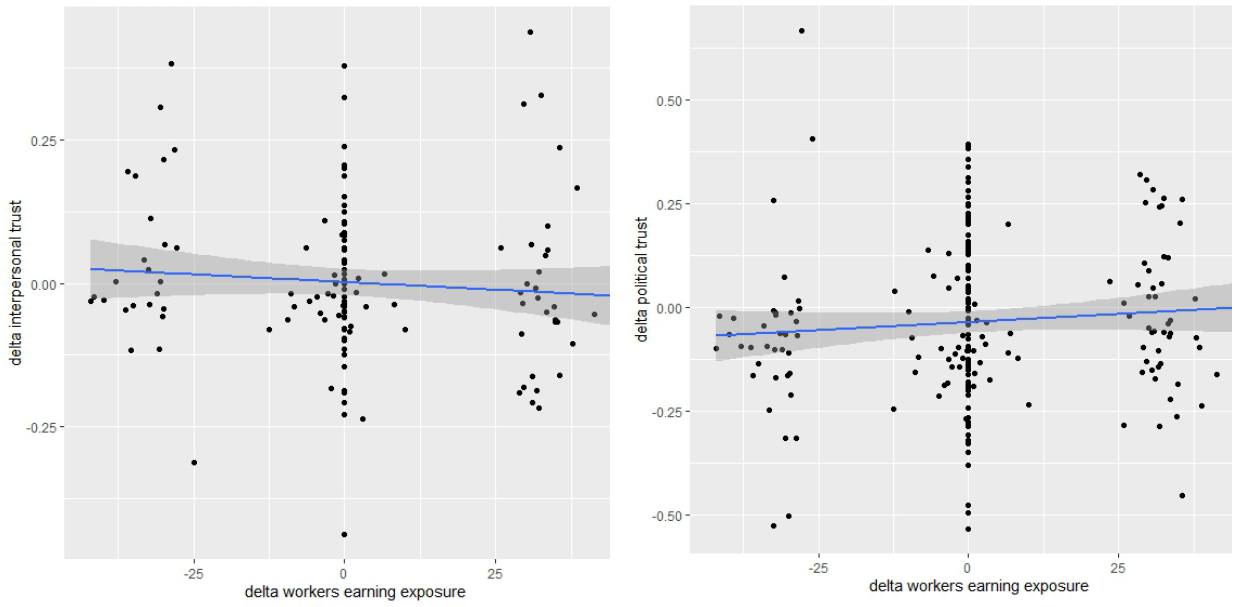


Notes: graph uses full sample of change in firm trade exposure and social cohesion at parish level between 2012-2015 and equation (1). Each data point represents the average change in values for a particular parish in either 2012-2015 or 2015-2018 (interpersonal trust only has data for 2015-2018). It excludes data points on parishes that have zero exposure (and therefore zero change) across any two years. Social cohesion values are normalized such to range from zero (0) to one (1). Direct firm exposure is hyperbolically (log) transformed. Appendix B describes the number of parishes for which trade exposure and social cohesion information is available.

Figure 6 – Correlations between worker earning exposure and social cohesion dimensions

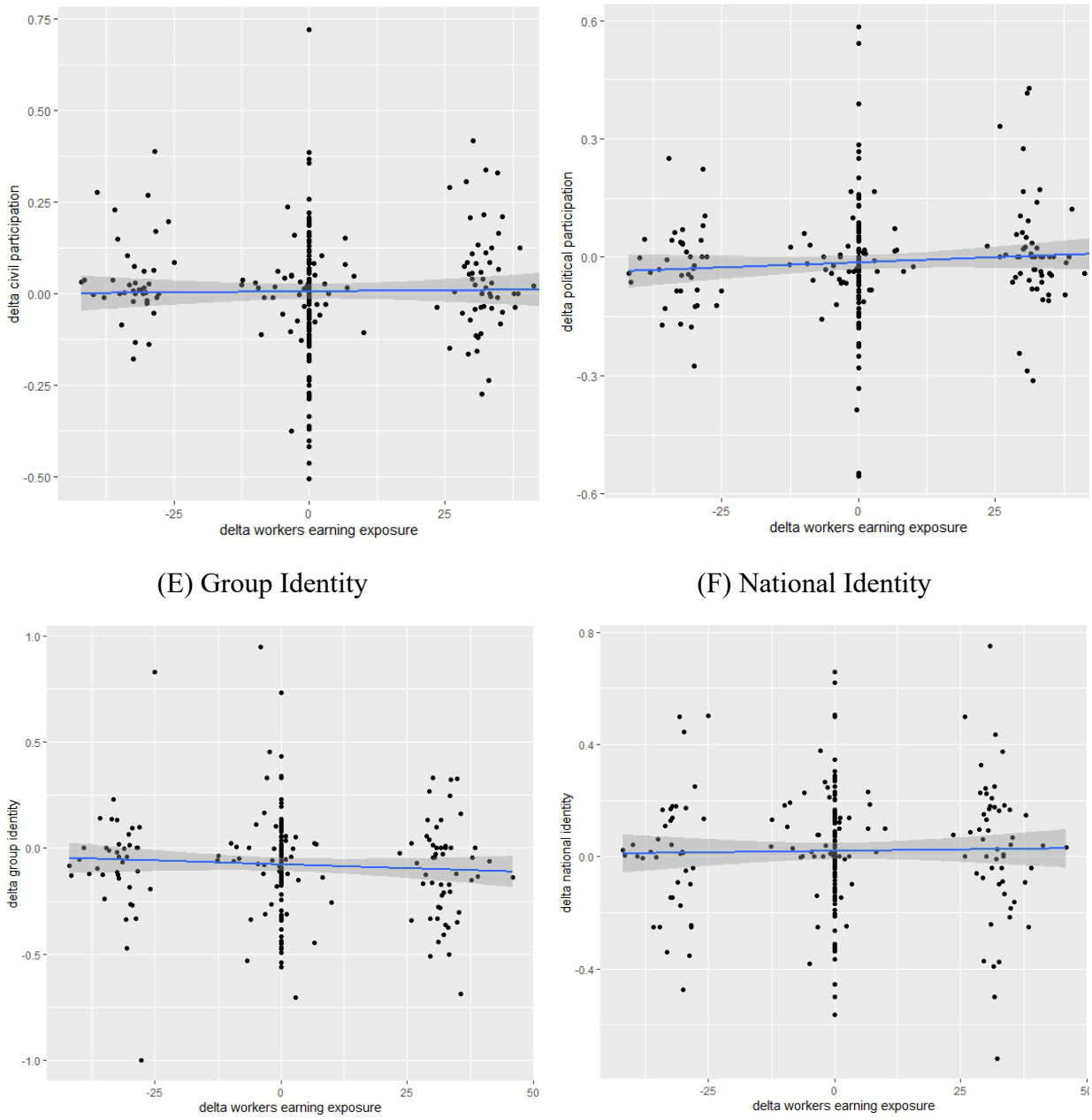
(A) Interpersonal Trust

(B) Political Trust



(C) Civil Participation

(D) Political Participation



(E) Group Identity (F) National Identity

Notes: graph uses full sample of change in worker earning exposure and social cohesion at parish level between 2012-2015 and equation (4). Working earning exposure shows exposure using formal wage information from the PAYE. Each data point represents the average change in values for a particular parish in either 2012-2015 or 2015-2018 (interpersonal trust only has data for 2015-2018). It excludes data points on parishes that have zero exposure (and therefore zero change) across any two years. Social cohesion values are normalized such to range from zero (0) to one (1). Worker earning exposure is log-transformed. Appendix B describes the number of parishes for which trade exposure and social cohesion information is available.

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