

# A Online Appendix of: Who Turns out to Vote? A Fresh Look Into an Old Question, by Bellettni, Berti Ceroni, Iorio, Monfardini, Prarolo

## A.1 Data Appendix

A compact list of the variables used in the analysis, their brief description, sources, and tables with relevant descriptives follow. Some of these variables are described in greater detail in [Schafer et al. \(2022\)](#). Subscripts  $h$ ,  $b$ , and  $p$  indicate the level of aggregation (other than individual) at which variables are calculated: household, building, and precinct level, respectively.

- **Turnout:** Dummy representing if the individual has been recorded at the poll station. This information is available for municipal elections in 2004 and 2009 and for national elections in 2008 and 2013, and has been hand-collected from the Ministry of Interior’s Bologna archive. These data have been subsequently anonymously merged to administrative individual data by the municipality of Bologna’s statistical office.
- **HH income:** Household income, measured in Euro, is calculated in several steps. First, individual incomes (made available to the municipality’s statistical office by the Tax Authority) that are missing or equal to zero are set to the no-tax area threshold (ranging between 4186 euro in 2002 and 6518 in 2014). Then, the adjusted incomes of all members of a household are summed up and deflated by the consumer price index (base year 2002), and finally adjusted with the OECD modified scale. This scale assigns a weight of 1 to the household head, 0.5 to other household members aged 14 or older and 0.3 to those younger than 14. Within family, the summation of these coefficients is used as denominator when rescaling household income. In some cases we do not observe the HH income because nobody in the family filed taxes that year. It may occur for a number of reasons: a family who just moved to Bologna filed her taxes in her hometown (hence, we imputed income using information on her future income), or family income might be so low that they were exempted from filing. In the latter case, we imputed a minimum level required for survival.
- Age dummies for age bins 31-45, 46-60, and over 60.
- Female Dummy
- Dummies for Married, Divorced, and Widowed
- Number of Household Members (Household Members $_h$ )
- Share of individuals from Non-OECD countries living in the building (% Non-OECD $_b$ )
- Share of individuals from African and Asian countries living in the building (% Ethnic Others $_b$ )
- Population density expressed as thousands inhabitants per square kilometer (Pop. density $_p$ )
- Mean household income in the precinct (hh income $_p$ )
- Gini Index of household income calculated in the precinct (Gini Index $_p$ )

The following two tables show some statistics to describe income and diversity shocks. Table [A2](#) reports, by quintiles of income, the distribution of Italian families experiencing no shocks and large and medium positive and negative shocks. While large negative shocks are almost absent for the first quintile of the income distribution due to left-truncation, the other shocks are evenly distributed, with richer people hit more on average: families in the first (fifth) quintile not hit by an income shock are more than 75% (less than 60%). Table [A3](#) reports, by quintiles of income, the distribution of Italian families who have at least a family with ethnic diverse components living in their building, the share of ethnically diverse families within the building for those Italian families with at least a family with ethnic diverse components living in their building, and share of Italian families experiencing the arrival of a family with ethnic diverse components in their Italian-only building. Overall we see all these variables being quite balanced across quintiles, with just a slightly lower probability for rich people to live in the same building of ethnic diverse people. Note that the diversity shock only hits 3% or less of the families.

Table A1: Descriptive statistics

	mean	sd	min	max
Turnout	0.83	0.38	0.00	1.00
HH income	28076.01	33682.60	5839.48	5752246
Age: 31-45	0.24	0.43	0.00	1.00
Age: 46-60	0.24	0.43	0.00	1.00
Age: over 60	0.43	0.49	0.00	1.00
Female	0.54	0.50	0.00	1.00
Married	0.54	0.50	0.00	1.00
Divorced	0.04	0.20	0.00	1.00
Widowed	0.13	0.34	0.00	1.00
Household Members <sub>h</sub>	2.36	1.14	1.00	7.00
% Non-OECD <sub>b</sub>	6.38	10.63	0.00	100.00
% Ethnic Others <sub>b</sub>	3.29	7.94	0.00	100.00
Pop. density <sub>p</sub> (1,000/sqkm)	12.02	7.79	0.07	52.34
Mean hh income <sub>p</sub>	24.24	6.54	12.01	59.79
Gini Index <sub>p</sub>	0.39	0.07	0.25	0.66

Note: Sample used for the main estimations, totaling 1,081,141 individual-by-year observations, where years are 2004, 2008, 2009 and 2013. Subscripts *h*, *b* and *p* refer to household, building, and precinct level, respectively.

Table A2: Descriptives of income shocks (shares)

	(1)	(2)	(3)	(4)	(5)
Quintile	sh_HH <i>S</i> <sub>0</sub>	sh_HH <i>L</i> −	sh_HH <i>M</i> −	sh_HH <i>M</i> +	sh_HH <i>L</i> +
1	.758	.004	.025	.054	.154
2	.630	.105	.052	.056	.154
3	.645	.122	.054	.066	.111
4	.609	.143	.071	.078	.098
5	.587	.159	.091	.076	.085

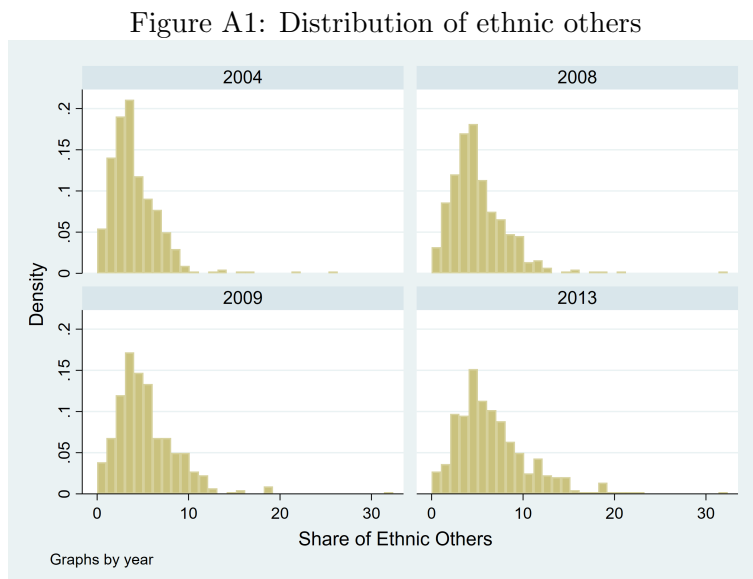
Note: Share of Italian families, by quintile of income, not experiencing income shocks (column 1), experiencing large (small) negative shock (column 2 (3, respectively)), and experiencing large (small) positive shock (column 4 (5, respectively)). See Section 2 for a detailed definition of income shocks. The shares do not sum horizontally to one because one of the variables we use as control (but is not interacted with our main explanatory variables) is a dummy for large income shocks greater than 10 standard deviations. These are few people (less than 0.004% of the population).

Table A3: Descriptives of diversity shocks (shares)

Quintile	(1) sh_HH Exposed	(2) sh_Exposure (if Exposure>0)	(3) sh_HH Div.Shocked
1	.279	.143	.031
2	.266	.140	.028
3	.255	.136	.028
4	.237	.132	.027
5	.208	.130	.025

Note: Share of Italian families, by quintile of income, who have at least a family with ethnic diverse components (column 1) living in their building, share of ethnically diverse families within the building for those Italian families with at least a family with ethnic diverse components living in their building (column 2), and share of Italian families experiencing the arrival of the first family with ethnic diverse components in their building.

Figure A1 reports the distributions of the shares of precinct-level ethnic others, by election years.



Note: Distributions of the shares of ethnic others across precincts, by election years.

More disaggregated descriptives on turnout are reported in Table A4, distinguishing by income quintile, age, gender and round, as well as by type of election: Administrative (2004 and 2009) and General (2008 and 2013) elections.

Table A4: Average turnout by groups

Quintiles (Adm-Gen)		Age (Adm-Gen)		Gender (Adm-Gen)		Round	
Q1	0.69 (0.68-0.70)	Below 30	0.78 (0.77-0.80)	Male	0.84 (0.83-0.84)	2004 (Adm.)	0.85
Q2	0.77 (0.77-0.78)	30-45	0.81 (0.81-0.82)	Female	0.81 (0.81-0.82)	2008 (Gen.)	0.86
Q3	0.82 (0.82-0.83)	45-60	0.88 (0.88-0.88)			2009 (Adm.)	0.80
Q4	0.86 (0.86-0.87)	Above 60	0.81 (0.81-0.82)			2013 (Gen.)	0.81
Q5	0.89 (0.88-0.90)						

Note: The table reports average turnout for the entire sample, and election types (administrative, in 2004 and 2009, versus general elections, in 2008 and 2013, in brackets), across income quintiles, age bins, and gender. The last column reports the average turnout in each electoral round. The averages are computed on the main estimation sample (1,081,141 individual-by-year observations).

## A.2 Estimation of Main Equations

Table A5 reports the full estimation results of Equations 1 (left panel) and 2 (right panel) in the main text, used to compute the marginal effects shown in Figure 2 in the Section 4. For example, based on the results collected in Table A5, left column, the marginal contribution to turnout for a  $M-$  (medium negative) income shock for an individual belonging to the second quintile of the income distribution is  $\hat{\beta}_{M-} + \hat{\gamma}_{2M-} = 0.0022 - 0.0461 = -0.0439$ . Alternatively, focusing on the results in the right column, the impact on turnout of the diversity shock for an individual belonging to the third quintile of the income distribution is  $\hat{\mu}_1 + \hat{\xi}_{31} = -0.0323 + 0.0166 = -0.0157$ .

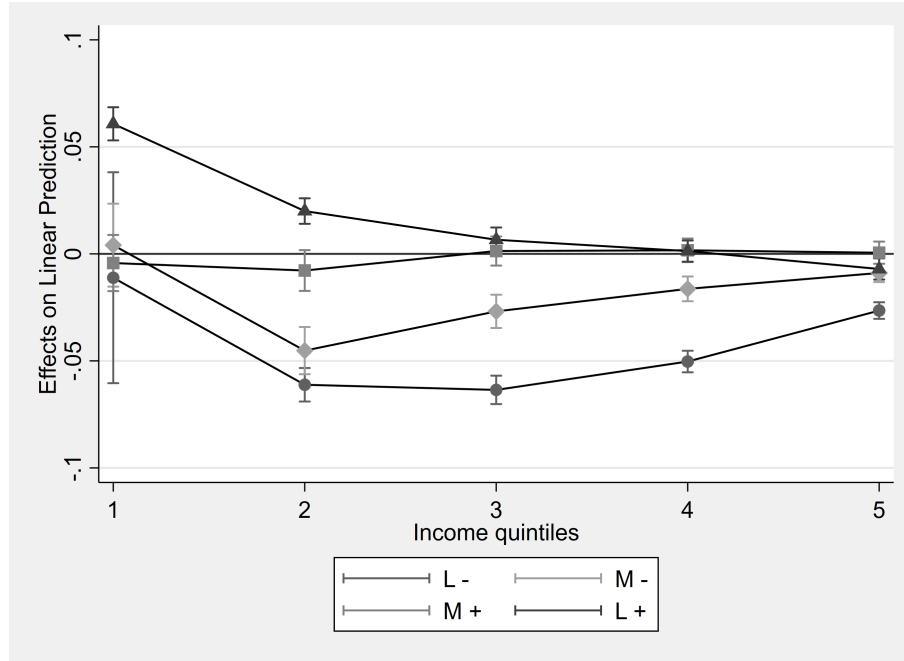
Table A5 reports standard errors clustered at the individual level (column 3 of each panel), used to show the statistical significance of the coefficients (column 2 of each panel) and to build the confidence intervals shown in Figure 2. Standard errors clustered at the precinct level, accounting for within-precinct correlations, are reported in column 4 of each panel. Clustering at the precinct level is particularly relevant for the diversity shock, as we leverage the variation in immigrant contact across buildings and within precincts. Comparing the confidence intervals in Figures A2 and A3 with their counterparts in Figure 2 shows that the significance of the marginal effects across quintiles is not affected when we adopt a more conservative version of the standard errors, i.e. we cluster at the higher precinct level. We also checked that our significance results are robust when clustering of the standard errors occurs at an intermediate aggregation level represented by building or higher level, such as the neighborhood level. These results are available upon request.

Table A5: Results of main specifications

Income Shocks				Diversity Shock			
VARIABLES	coeff.	s.e. (i)	s.e. (p)	VARIABLES	coeff.	s.e.(i)	s.e.(p)
IncQ=2	0.0723***	[0.0024]	[0.0030]	IncQ=2	0.0507***	[0.0022]	[0.0026]
IncQ=3	0.1167***	[0.0023]	[0.0031]	IncQ=3	0.0890***	[0.0022]	[0.0028]
IncQ=4	0.1464***	[0.0022]	[0.0030]	IncQ=4	0.1162***	[0.0021]	[0.0028]
IncQ=5	0.1616***	[0.0022]	[0.0032]	IncQ=5	0.1339***	[0.0021]	[0.0028]
L-	-0.0065	[0.0213]	[0.0251]	Past Exposure	-0.0686***	[0.0101]	[0.0181]
M-	0.0022	[0.0087]	[0.0099]	Current Exposure	-0.0323***	[0.0079]	[0.0089]
M+	-0.0043	[0.0059]	[0.0067]	Past Exposure X Current Exposure	0.0718***	[0.0130]	[0.0201]
L+	0.0605***	[0.0036]	[0.0039]	Past Exposure X IncQ=2	0.0385***	[0.0126]	[0.0205]
IncQ=2 X L-	-0.0548**	[0.0215]	[0.0253]	Past Exposure X IncQ=3	0.0542***	[0.0119]	[0.0214]
IncQ=2 X M-	-0.0461***	[0.0098]	[0.0113]	Past Exposure X IncQ=4	0.0633***	[0.0115]	[0.0216]
IncQ=2 X M+	-0.0031	[0.0072]	[0.0083]	Past Exposure X IncQ=5	0.0827***	[0.0111]	[0.0210]
IncQ=2 X L+	-0.0404***	[0.0044]	[0.0052]	Current Exposure X IncQ=2	0.0139	[0.0098]	[0.0112]
IncQ=3 X L-	-0.0570***	[0.0214]	[0.0255]	Current Exposure X IncQ=3	0.0166*	[0.0093]	[0.0102]
IncQ=3 X M-	-0.0290***	[0.0094]	[0.0107]	Current Exposure X IncQ=4	0.0248***	[0.0090]	[0.0100]
IncQ=3 X M+	0.006	[0.0067]	[0.0076]	Current Exposure X IncQ=5	0.0342***	[0.0088]	[0.0095]
IncQ=3 X L+	-0.0540***	[0.0044]	[0.0048]	Past Exposure X Current Exposure X IncQ=2	-0.0455***	[0.0162]	[0.0232]
IncQ=4 X L-	-0.0436**	[0.0214]	[0.0251]	Past Exposure X Current Exposure X IncQ=3	-0.0512***	[0.0154]	[0.0227]
IncQ=4 X M-	-0.0185**	[0.0091]	[0.0102]	Past Exposure X Current Exposure X IncQ=4	-0.0568***	[0.0148]	[0.0225]
IncQ=4 X M+	0.0059	[0.0064]	[0.0072]	Past Exposure X Current Exposure X IncQ=5	-0.0862***	[0.0143]	[0.0219]
IncQ=4 X L+	-0.0593***	[0.0042]	[0.0047]				
IncQ=5 X L-	-0.0199	[0.0214]	[0.0251]				
IncQ=5 X M-	-0.0111	[0.0089]	[0.0100]				
IncQ=5 X M+	0.0051	[0.0063]	[0.0073]				
IncQ=5 X L+	-0.0675***	[0.0042]	[0.0046]				
Individual Level Controls	YES			Individual Level Controls	YES		
Precinct FE	YES			Precinct FE	YES		
Year X Neighborhood FE	YES			Year X Neighborhood FE	YES		
Household Level Controls X Year FE	YES			Household Level Controls X Year FE	YES		
Precinct Level Controls X Year FE	YES			Precinct Level Controls X Year FE	YES		
Observations	1081141			Observations	1081141		
R-squared	0.0653			R-squared	0.0635		

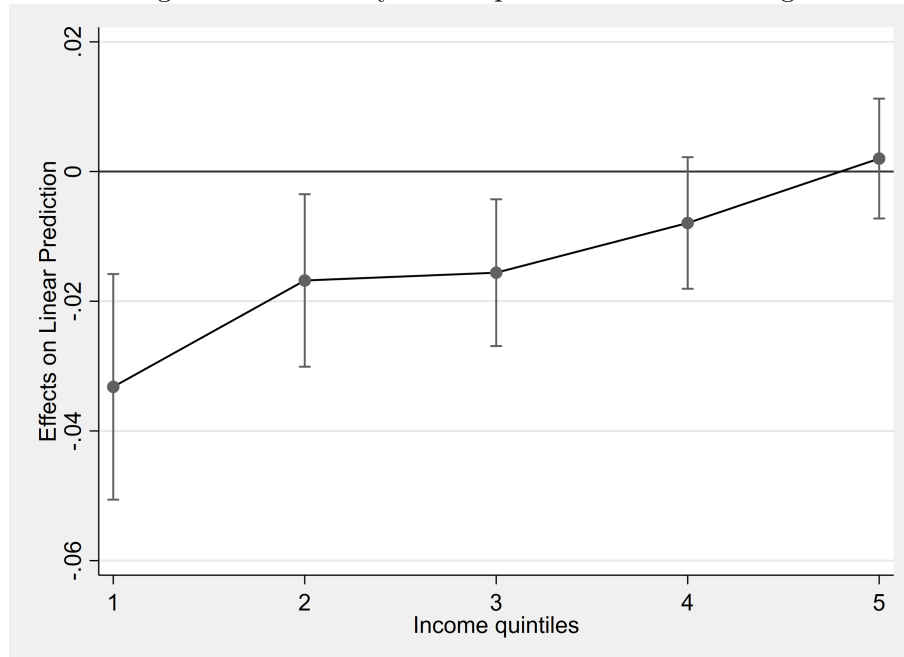
Note: Individual-level OLS regressions, with the sample based on all those eligible voters that did not change address over the 2003-2013 period. The dependent variable is a dummy indicating whether individuals voted in elections in years 2004, 2008, 2009, and 2013. Left (right) hand side results refer to the estimation of specification 1 (2) in the main text, with collinear coefficient in the interactions dropped by the estimation procedure. Controls are described in the main text and in Section A.1. Columns 3 and 7 contain standard errors clustered at the individual level, used to show the coefficients' significance (\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ ). Columns 4 and 8 display standard errors clustered at the precinct level.

Figure A2: Income shocks: precinct-level clustering



Note: Marginal effects of income shocks on turnout, by quintile. 95% confidence intervals are reported. Estimated coefficients are represented as follows: L+ as  $\triangle$ , M+ as  $\square$ , M- as  $\diamond$ , and L- as  $\circ$ . Standard errors clustered at precinct level.

Figure A3: Diversity shock: precinct-level clustering



Note: Marginal effects of diversity shock on turnout, by quintile. 95% confidence intervals are reported. Standard errors clustered at precinct level.

### A.3 Individual Fixed Effects results

This Section discusses the robustness of our main results to the inclusion of individual fixed effects in Equations 1 and 2. Here, we take advantage of the longitudinal nature of the data not only to construct the income shock and exposure measures, but also to control for individual unobserved heterogeneity, exploiting within person time variation in shocks. This is achieved at the cost of no longer estimating the coefficients of any time-invariant characteristics.

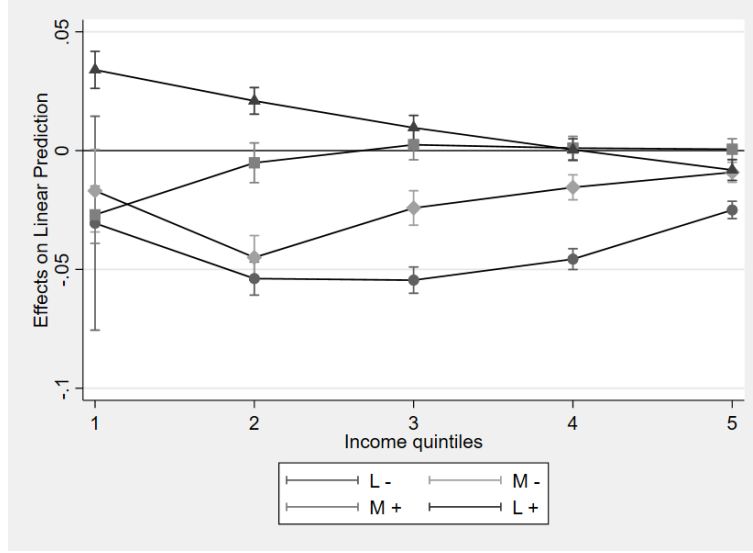
When considering individual fixed effects in this setting, two considerations are in order. First, note that if we include quintiles, all individuals who never switched across them would no longer contribute to the estimation. In order to keep them, we drop the main effects of the quintiles from the regression and therefore we don't use the within-person variation across quintiles, which is small. In this way, we preserve the comparability of our main results with those including individual fixed effects, and consider a similar estimation sample in terms of size and composition. Since we drop the quintiles from the specification (as if assuming that they are time-invariant), for consistency in the interaction term we fix the quintile to be the one the individual belonged to the first time we observe her. Second, perhaps more importantly, when we include individual fixed effects, only individuals experiencing variation over time in the different shock categories we consider contribute to the estimation of the coefficients. In particular, voters who never experienced an income shock no longer contribute to the estimation, so that we are in practice changing the composition of the baseline group.

To better understand the change of the marginal effects that would result from a shrink and a compositional change of the estimation sample (due to the loss of individuals without income shocks), and isolate this change from the one due to the inclusion of individual fixed effects, we proceed in two steps. We first compare the estimates obtained using our benchmark specification with and without this sample restriction. Next, we include individual fixed effects.

In the first step, we would expect the marginal effect to be attenuated in Figure A4, because we are increasing the financial vulnerability in the baseline group, who does not experience a shock at  $t-1$  but does at some other period. Hence, we are comparing individuals who all faced a shock at some point. We find that while the marginal effects of the negative income shocks are only somewhat smaller, the marginal effect of the large positive income shock shrinks considerably.



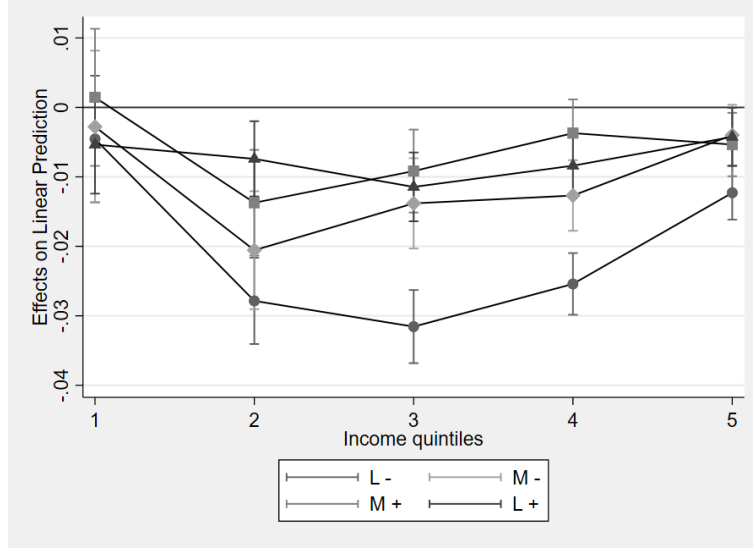
Figure A4: Income shocks: sample of shocked individuals



Note: Marginal effects of income shocks on turnout along the income distribution, obtained from equation 1, where the quintiles dummies are time-invariant and the sample is restricted to those individuals experiencing at least an income shock. Estimated coefficients are represented as follows: L+ as  $\triangle$ , M+ as  $\square$ , M- as  $\diamond$ , and L- as  $\circ$ . 95% confidence intervals are reported.

With this caveat in mind, we next include individual fixed effects in the benchmark specification as a robustness check, and compare this final set of findings with those in the main text, see Figure A5. Reassuringly, we observe that all marginal effects of negative income shocks become only slightly smaller in magnitude and, while less precisely estimated, we still retain sufficient variation and statistical power to estimate the within-person effects. In sum, the qualitative patterns across quintiles of the estimated coefficients of negative income shocks are similar, with and without fixed effects. Only the positive effect induced by a large positive shock for low-income individuals vanishes when we include fixed effects. However, as we pointed out, part of this shrink is due to the compositional change at baseline.

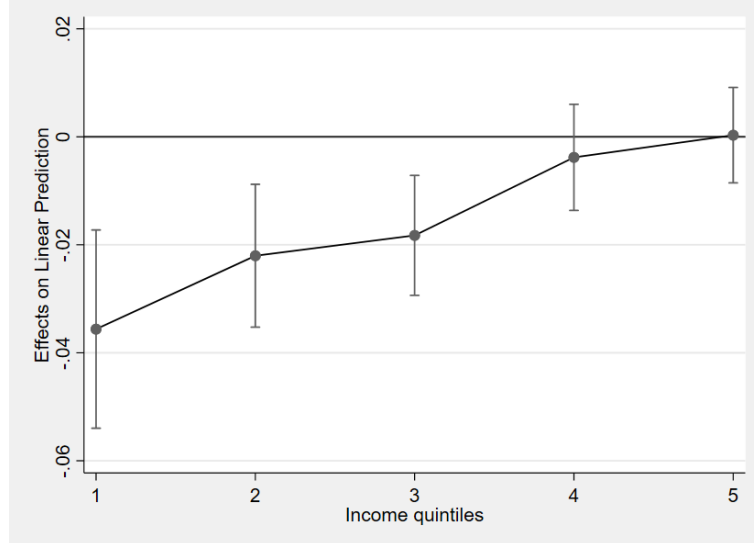
Figure A5: Income shocks: with individual fixed effects



Note: Marginal effects of income shocks on turnout along the income distribution, obtained from regression 1, where individual fixed effects are added and the quintiles dummies are time-invariant. Estimated 95% confidence intervals are reported. coefficients are represented as follows: L+ as  $\triangle$ , M+ as  $\square$ , M- as  $\diamond$ , and L- as  $\circ$ .

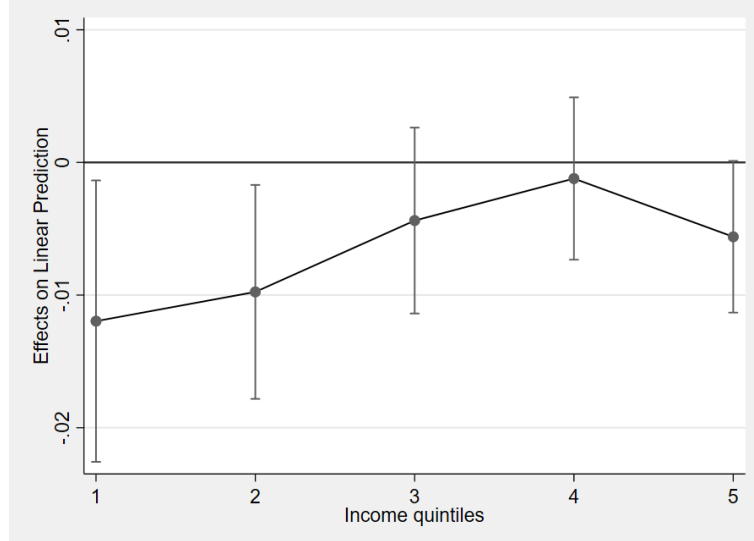
Similar conclusions are reached when we replicate the same procedure to the diversity shock, i.e. running the analysis on the subsample of individuals experiencing at least one diversity shock and then adding individual fixed effects to the main specification, again using with time-invariant quintiles in the interaction term with the diversity shock. Results of the former (latter) specifications are reported in Figure A6 (A7). Note that the figure with the main findings and Figure A6 are quite similar. When we include fixed effects in Figure (A7), the pattern across quintiles is confirmed, although it shrinks in size and is somewhat less precisely estimated. Once more, we show that mainly those with low incomes are negatively impacted.

Figure A6: Diversity shocks: sample of shocked individuals



Note: Marginal effects of diversity shocks on turnout along the income distribution, obtained from regression 2, where the quintiles dummies are time-invariant and sample is restricted to those individuals experiencing a diversity shock. 95% confidence intervals are reported.

Figure A7: Diversity shocks: with individual fixed effects



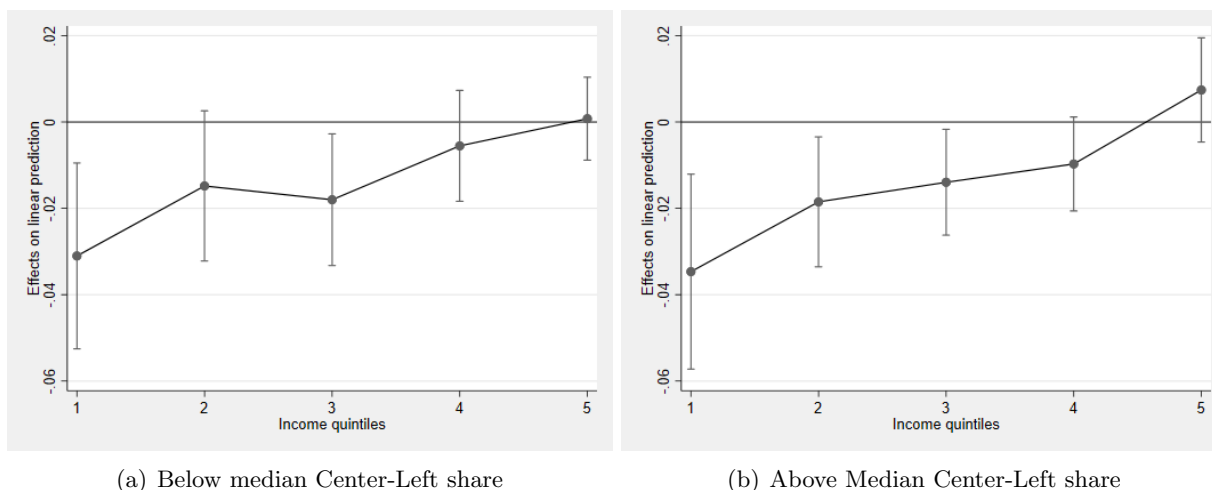
Note: Marginal effects of diversity shocks on turnout along the income distribution, obtained from regression 2, where individual fixed effects are added and the quintiles dummies are time-invariant. 95% confidence intervals are reported.

#### A.4 Diversity shock: sample split

In this section, we provide additional evidence to corroborate the interpretation of our results by examining the effect of diversity shocks on turnout across precincts with different political leanings. Figure A8 contrasts precincts with above- versus below-median support for the Center-Left, while Figure A9 focuses on the least left-leaning precincts (bottom quartile, with a Center-Left vote share

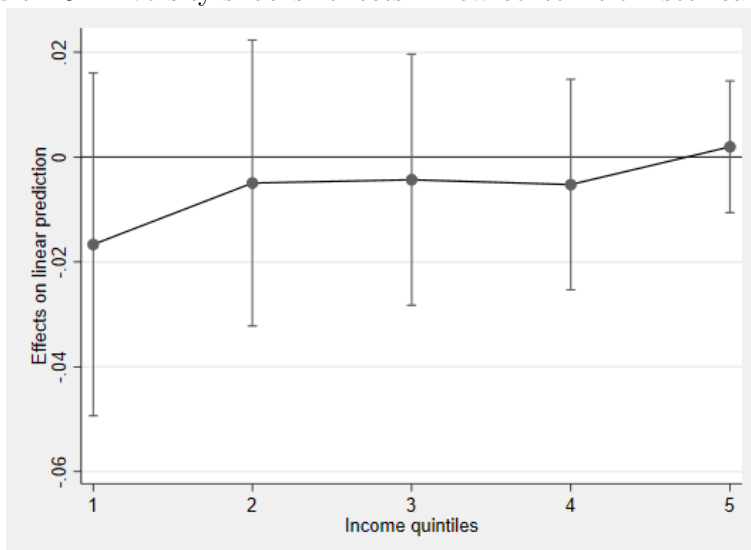
below 35%). The results in Figure A8 show that the abstention effect of diversity shocks is more pronounced in stronger left-leaning precincts (Panel b) than in weaker ones (Panel a). By contrast, Figure A9 indicates no significant abstention effect in the least left-leaning precincts. Taken together, these findings suggest that turnout reductions after diversity shocks occur specifically in strong left-leaning precincts, consistent with the idea that dissatisfied left-wing voters are more likely to abstain than to shift their support to right-leaning parties.

Figure A8: Diversity shocks: heterogeneous effects by political leaning



Note: Marginal effects of diversity shock on turnout along the income distribution in precincts with below (above) median vote share for Center-Left, panel (a(b)), obtained from the main regression. In both panels 95% confidence intervals are reported.

Figure A9: Diversity shocks: effects in low center-left historical vote

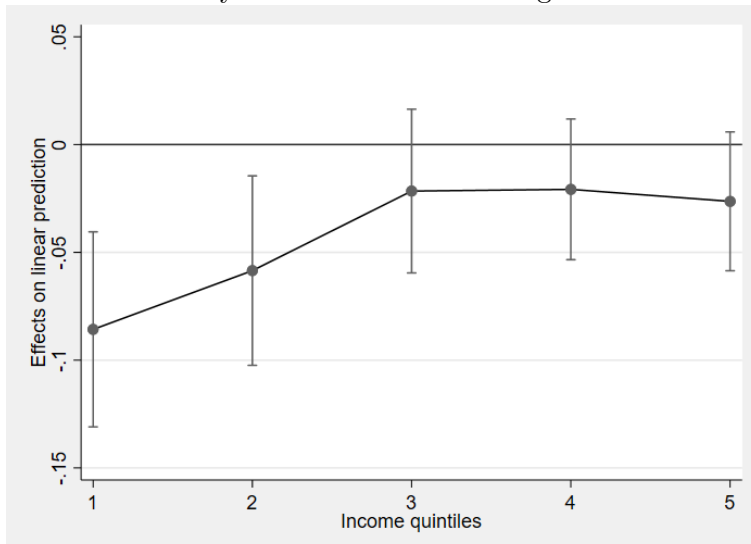


Note: Marginal effects of diversity shock on turnout along the income distribution in precincts with vote share for Center-Left below 35% (bottom 25% of the observations). 95% confidence intervals are reported.

Next, we restrict the sample to buildings in the top quintile in terms of new household arrivals.

The marginal effects of the diversity shock are presented in Figure A10. They mirror those from the full sample and show somewhat stronger effects at the lower end of the income distribution.

Figure A10: Diversity shocks: restricted to high-turnover buildings



Note: Marginal effects of diversity shocks on turnout along the income distribution, obtained from the estimation of 2 when the sample includes only the top 20% of buildings with the highest family turnover. 95% confidence intervals are reported.

## A.5 Movers

To address the determinants of moving out from a building, which could introduce biases in the main analysis, we exploit the longitudinal individual data (not limited to election years) and construct a dummy variable equal to one if a household moved in year  $t$  ('moving out'), with zeros replaced by missing values from  $t + 1$  onward, as is common, for example, in the conflict literature (Ciccone, 2011). We then estimate the relationship between moving out in  $t$  after experiencing a diversity shock in  $t - 1$ .

Table A6: Diversity shock and moving

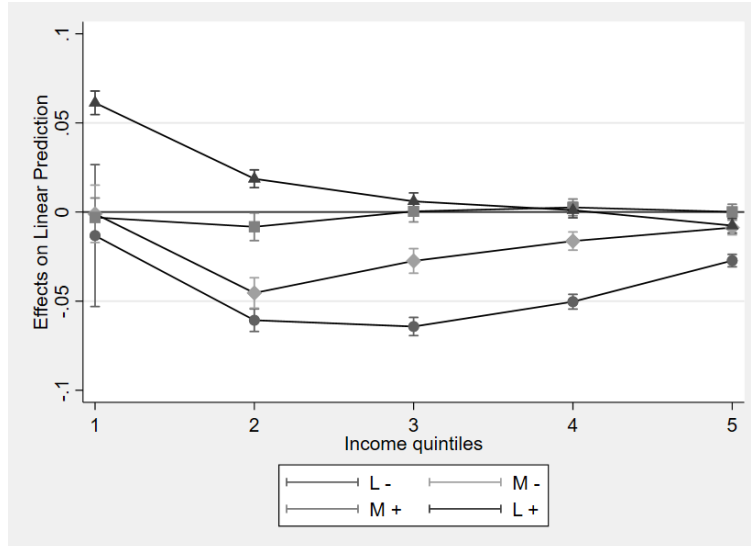
VARIABLES	(1) Moving Out	(2) Moving Out	(3) Moving Out	(4) Moving Out
Diversity Shock (t-1)	-0.0009 [0.0008]	-0.0004 [0.0006]	-0.0006 [0.0006]	-0.0006 [0.0006]
Household Members				-0.0310*** [0.0005]
HH income				-0.0001*** [0.0000]
perc. aged over 64				-0.0005*** [0.0000]
Fixed Effects	NO	ID;time	ID;time;precinct	ID;time;precinct
Observations	2,671,296	2,671,296	2,671,296	2,671,296
R-squared	0.0000	0.3492	0.3816	0.3899

Note: Individual-level OLS regressions, where the dependent variable is a dummy equal to one if the individual changes address. Time period: 2003-2013. Standard errors are clustered at the individual level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

As it is clear from the estimated coefficients in Table A6, experiencing a diversity shock in  $t-1$  is not significantly associated with the likelihood of changing address the year after.

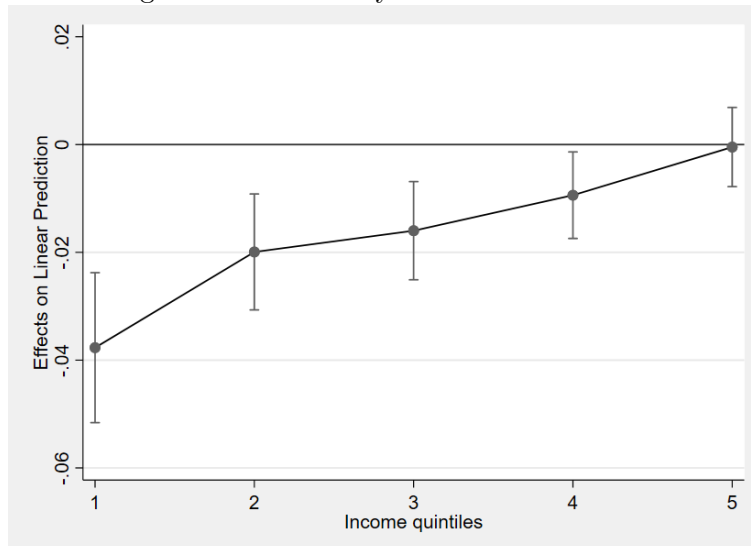
Next we show the results for the two main specifications when we include people that eventually change address, in Figures A11 and A12, and results visually match with the main ones.

Figure A11: Income shocks: with movers



Note: Marginal effects of income shocks on turnout along the income distribution, obtained from regression 1, where individuals changing address are included in the sample. Estimated coefficients are represented as follows: L+ as  $\triangle$ , M+ as  $\square$ , M- as  $\diamond$ , and L- as  $\circ$ . 95% confidence intervals are reported.

Figure A12: Diversity shocks: with movers



Note: Marginal effects of income shocks on turnout along the income distribution, obtained from regression 2, where individuals changing address are included in the sample. 95% confidence intervals are reported.

## A.6 Robustness on diversity definition

Regarding the ethnic shocks, we consider a robustness check for our main results (panel b of Figure 2) by providing a different measure of ethnic diversity. Specifically, rather than relying on the definition used in Caselli and Coleman II (2013), which stressed the visual identifiability of foreigners, (according to which ethnic diverse people are those from Africa and Asia), we flag foreigners as those of non-OECD origin. Results when using the latter measure are presented in Table A7. When compared to the main marginal effects (reported in column 2 for comparability), the ones estimated using non-OECD individuals are still negative from the first to the fourth quintiles, although the pattern is somewhat flatter along the income distribution. This could reflect the fact that ethnically diverse immigrants in Italy are, on average, less skilled than the general population.

Table A7: Marginal effects of diversity shock (Non-OECD and Ethnic Others)

	Non-OECD	Ethnic Others
1st quintile	-0.0239*** (0.0062)	-0.0323*** (0.0079)
2nd quintile	-0.0207*** (0.0043)	-0.0184*** (0.0058)
3rd quintile	-0.0163*** (0.0037)	-0.0157*** (0.0049)
4th quintile	-0.0097*** (0.0032)	-0.0076* (0.0042)
5th quintile	-0.0059** (0.0028)	0.0019 (0.0039)
Observations	1081141	1081141

Note: Marginal effects of diversity shock on turnout as from the main analysis reported in Figure 2 (column 1) and by using the diversity definition based on non-OECD individuals (column 2). Standard errors in brackets, \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$



## A.7 Further Evidence on the diversity shock

In this section, we provide further evidence supporting the conjecture that the diversity shock is plausibly exogenous within a precinct. To this end, we document that the building characteristics are not significantly associated with the arrival of a migrant family, and we implement an event study exercise to examine whether pre-trends are present.

### A.7.1 Building Characteristics and the Diversity Shock

In Table A8 we present regressions at the building level, focusing on the subsample of buildings hosting only Italian families in  $t - 1$ . The dependent variable is the arrival of the first foreign family in the building. When we include only neighborhood fixed effects, it seems that immigrants are less likely to move into buildings where the average family income is higher. However, when we restrict the analysis to within-precinct variation in Column 2, the income effect diminishes, and it completely disappears in Column 3 when we condition on the vacation of a flat due to a family moving out. Interestingly, all the estimated coefficients are zero and insignificant, supporting the identifying assumption of the conditional random arrival of the first foreign family, which is central to the main analysis.

Table A8: Diversity shocks and building characteristics			
Dep. Var.	(1) Div. Shock	(2) Div. Shock	(3) Div. Shock
Average HH income	-0.0326 [0.0225]	-0.0207* [0.0111]	-0.0089 [0.0103]
Average HH size	-0.0022*** [0.0006]	-0.0012** [0.0006]	-0.0005 [0.0005]
Share of females	0.0044** [0.0018]	0.0030 [0.0020]	0.0025 [0.0020]
Share of 65+	-0.0023 [0.0019]	-0.0027* [0.0016]	-0.0002 [0.0015]
Household turnover			0.0287*** [0.0016]
Observations	93,174	93,174	93,174
R-squared	0.0017	0.0131	0.0193

Note: Building-level OLS regressions. The sample includes all building-year observations where all residents were Italians prior the arrival of an Asian or African family in  $t-1$ . The dependent variable is a dummy equal to one if at least one ethnically diverse family arrives. Explanatory variables are: average family income at the building level (million euros), average household size, share of females, share of residents over 65, and, in column 3, an indicator for family turnover at  $t-1$ . Standard errors clustered at the neighborhood level in column 1, and precinct level in columns 2 and 3, \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

### A.7.2 Event Study Analysis

To support our identification strategy we perform an event study exercise to investigate whether treated and control units do not exhibit diverging turnout dynamics prior to treatment, i.e. the

arrival of the (first) Asian or African family in their building in the year before an election.<sup>28</sup> Moreover, findings from the event study provide insight into whether the effect of the diversity shock is long-lasting or vanishes over time.

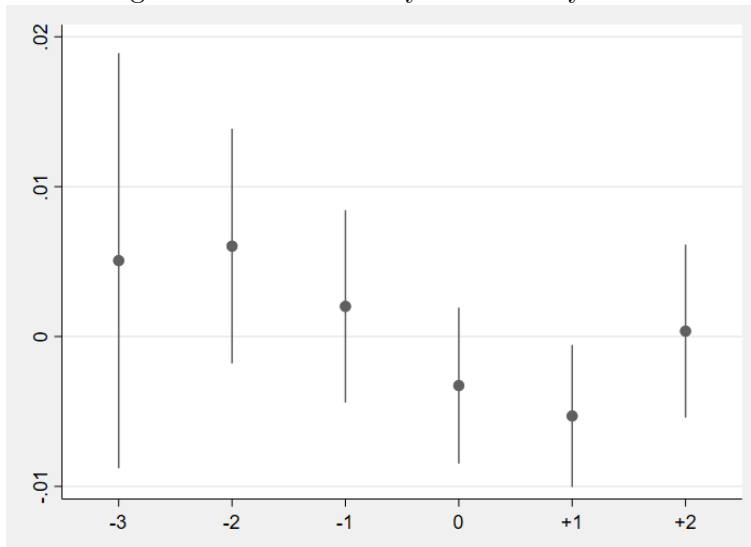
To this end, we use the estimator developed by [Sun and Abraham \(2021\)](#) that addresses bias arising in traditional two-way event studies when treatment effects are heterogeneous across cohorts, and it is suitable to test for pre-trends in contests as ours where the effect of the diversity shock differs across groups of eligible voters treated at different electoral rounds. In our setup, treated units are individuals exposed to a diversity shock for the first time in the year before the election. The control group includes the never-treated units.

In [Figure A13](#) the estimated coefficients are the cohort-specific average treatment effects on the treated (CATT), conditional on individual and year fixed effects, and baseline individual controls.

The pre-treatment coefficients are generally small and statistically indistinguishable from zero, suggesting broadly similar turnout dynamics between treated and control units prior to exposure. Nonetheless, mild differences cannot be ruled out, and we interpret this evidence with caution, viewing it only as suggestive of the absence of pre-trends.

In line with the main results, a negative effect of the diversity shock on turnout materializes in the electoral round immediately after the shock, i.e. at  $t = 0$ . Moreover, the negative effect persists in the subsequent electoral round, i.e. at  $t = 1$ , and it vanishes thereafter. The magnitude of the effect is hardly comparable with the results of our main specification, given the many differences in the estimation setting. Nevertheless, the size of the coefficient at  $t = 0$  is unsurprising given that the CATT estimates do not exhibit heterogeneity across the income distribution. If we were to infer the average effect from the main [Figure 2](#), panel (b), we would find a value closer to zero. This is because the estimated coefficients for the diversity shock are negative across income quintiles, but are much larger in magnitude at the lower end of the income distribution and diminishing to near zero at the top two quintiles.

Figure A13: Event study for diversity shock



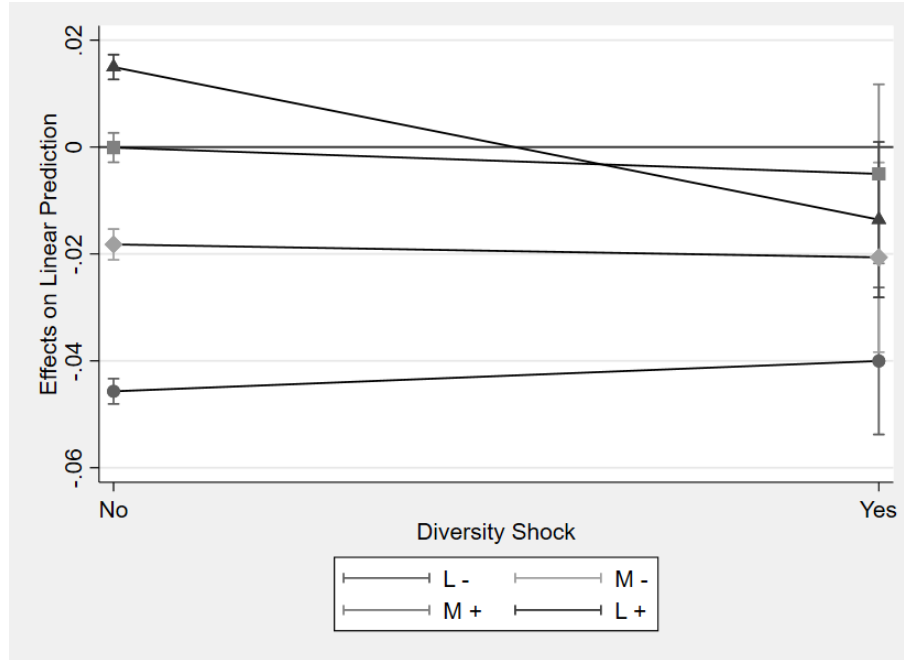
Note: The figure plots event-time coefficients relative to the year of first exposure to a diversity shock, obtained by using the Sun and Abraham estimator ([Sun and Abraham, 2021](#)), and including individual and year fixed effects, as well as the baseline individual controls. The horizontal axis reports electoral rounds around the diversity shock. 95% confidence intervals are reported.

## A.8 Joint Shocks

In this section we analyze the effects of simultaneous income and diversity shocks. While these dimensions are conceptually distinct, they might reinforce or counteract each other's in their association with turnout.

Figure A14 plots the marginal effects of different types of income shocks (not interacted with quintiles) alongside the presence or absence of a diversity shock. The estimated effects for income shocks are generally similar, except for large positive shocks, regardless of whether the individual also experienced a diversity shock. This suggests that there are no clear complementarities in their marginal effects (at least on average), and the two types of shocks do not meaningfully interact, particularly when both are negative. Note that confidence intervals are somewhat large when a surprise diversity shock occurs, emphasizing the low incidence of simultaneous shocks.

Figure A14: Diversity and income shocks: joint estimation



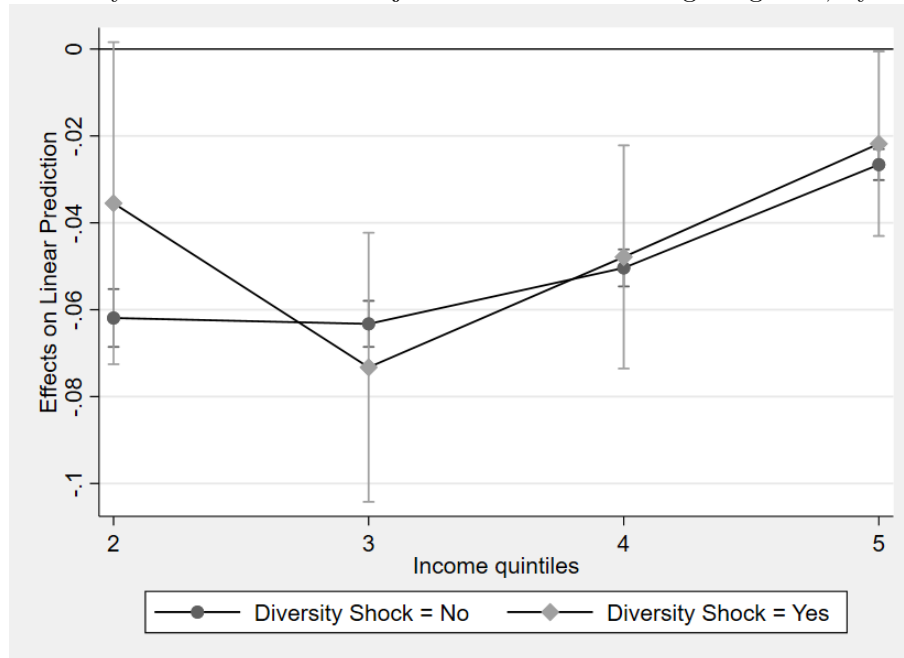
Note: Marginal effects of income shocks by presence of diversity shock on turnout. Estimated coefficients are represented as follows: L+ as  $\triangle$ , M+  $\square$ , M- as  $\diamond$ , and L- as  $\circ$ . 95% confidence intervals are reported.

Next, we incorporate triple interactions to examine whether differences across quintiles emerge when considering large income shocks, either negative or positive.

For large negative shocks, there is no systematic gap across quintiles, which might explain why the estimated "overall" effects in Figure A14 were similar. Note that confidence intervals are large in the presence of a diversity shock. This is due to the very low probability of experiencing both shocks simultaneously across quintiles.

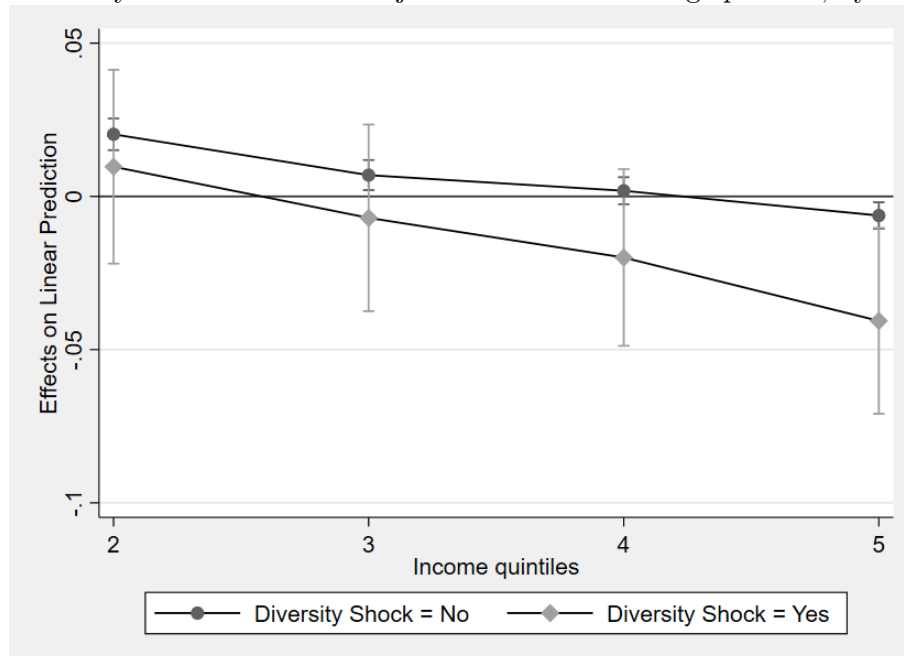
For large positive shocks, at the lower end of the income distribution, we observe little reaction to simultaneous shocks (i.e., experiencing both a large positive income shock and a diversity shock), and this is consistent with our main results, where these two effects counteract each other. At the upper end, however, while the marginal effects of each shock individually were small, their combined impact reinforces each other, leading to a dampening of turnout. As before, all these results shall be taken with a grain of salt as the confidence intervals are wide when a surprise diversity shock also occurs, further emphasizing the low incidence of simultaneous shocks.

Figure A15: Diversity and income shocks: joint estimation for large negative, by income quintile



Note: Marginal effects of large negative income shocks by presence of diversity shock on turnout, by income quintile. 95% confidence intervals are reported.

Figure A16: Diversity and income shocks: joint estimation for large positive, by income quintile



Note: Marginal effects of large positive income shocks by presence of diversity shock on turnout, by income quintile. 95% confidence intervals are reported.