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Income Shocks, Inequality, and Democracy*

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Abstract

In this paper, motivated by contradictory evidence on the effect of income on democracy, we investigate the hypothesis that it is income shocks – major income fluctuations relative to the trend – rather than marginal year-on-year variation in income levels that lead to non-trivial changes in the quality of political institutions. Empirical results provide support for this hypothesis, and show how income inequality plays a crucial role in the effects of economic shocks on democracy. In particular, negative income shocks reveal a positive effect on democracy in countries with high inequality, and vice versa.

Keywords: Downturns; democratization; stability of democracy
JEL classification: *D*72; *O*10; *O*17; *O*47

I. Introduction

Since Lipset's famous hypothesis that a sufficiently high level of income is a prerequisite for democracy (Lipset, 1959), the causal effect of income on democracy has been a central theme in social science research. While much of the previous literature found evidence consistent with a positive effect of income on the quality of democratic institutions (e.g., Barro, 1999), more recent work by Acemoglu *et al*. (2008, 2009) suggests that the positive association between income and democracy disappears if one accounts for systematic differences across countries that affect income and democracy. In particular, they find no significant effect of income on democracy in cross-country panel regressions with country-fixed and time-

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fixed effects. This result has initiated an ongoing debate about the role of income in democracy, with some studies finding evidence for a positive effect based on non-linear estimators or refined methods, others finding substantial heterogeneity in the effect of income, and still others providing evidence for significant improvements in democratic institutions in response to negative income dynamics or shocks. To date, there has been no coherent explanation offered for this apparently contradictory evidence on the effect of income on democracy.

In this paper, we test two hypotheses that imply a more subtle perception of the income-democracy nexus than considered previously. The first hypothesis is that it is income shocks – rather than minor fluctuations in income – that trigger major changes in institutional quality, as reflected by transitions from autocracy to democracy. The second hypothesis is that the effect of income shocks on democratization depends crucially on the social environment, as reflected by economic inequality. Both hypotheses are motivated by the theoretical literature on democratization, which considered – broadly speaking – two scenarios for democratization to occur. In the first scenario, the threat of revolution forces the elite to extend the franchise as a last means of staying in power for lack of other credible concessions (Acemoglu and Robinson, 2000, 2001, 2005). In the second scenario, the elite drives transitions to democracy in exchange for economic benefits (Lizzeri and Persico, 2004).[1] In both scenarios, negative economic shocks provide opportunities for democratization because they make revolutions more likely by shifting the opportunity costs for revolts and by weakening the elites, or by increasing the incentives to concede political power in exchange for economic benefits.

In light of this discussion, an appropriate empirical analysis of the income-democracy nexus should focus on negative economic shocks and non-marginal changes in democratic quality, instead of exploiting continuous and symmetric variation in income and institutional quality. In addition, the consequences of negative economic shocks crucially depend on inequality in both democratization scenarios. On the one hand, inequality determines the inherent distributional conflict between the elite, which wants to stay in power and avoid redistribution, and the disenfranchised population, which aims to gain political power over taxation and redistribution. If inequality is high, a negative income shock weakens the elite and increases the disenfranchised population's willingness to revolt because of fiercer distributive conflict.[2] On the other hand, inequality implicitly determines which section of society bears the main

[1]See Cervellati *et al*. (2014a) and Cervellati and Sunde (2014) for a unified theory of different transition scenarios and their implications for subsequent development.

[2]This argument was made by Lipset (1959, p. 83), who attributed it to De Tocqueville (1835).

consequences of negative economic shocks. With high inequality, the costs of severe downturns are disproportionately borne by the elite. Hence, the elite might be more willing in relative terms to concede political power and overcome autocratic institutions in response to economic shocks in an environment with high inequality. In sum, this discussion implies two testable implications that follow from both strands of the literature on democratization: (i) major fluctuations in income – in particular negative income shocks – affect the likelihood of democratization, and (ii) the prevailing inequality crucially affects the effect of income shocks on democratization.[3]

Our empirical results provide support for both hypotheses and document that negative economic shocks – instead of marginal income fluctuations or positive income shocks – play a significant role in major changes in institutions. Our findings further show that the effect of negative income shocks depends on the level of inequality. In particular, negative income shocks reveal a positive effect on democracy in countries with high economic inequality and a negative effect in countries with low inequality.

This paper contributes to the existing literature on the income-democracy nexus, which finds mixed and seemingly contradictory evidence. In response to the analysis by Acemoglu *et al*. (2008, 2009), several studies find a positive effect of income on democracy using non-linear estimators (Benhabib *et al*., 2013; Che *et al.*, 2013; Heid *et al*., 2012), data over longer time horizons (Boix, 2011), or historical event studies of ballot reforms (Aidt and Jensen, 2017). At the same time, papers analyzing the effect of exogenous income shocks find both positive and negative effects on democratic quality. For example, Papaioannou and Siourounis (2008) document pronounced negative income dynamics before democratization, and Aidt and Franck (2015) show that poverty-related riots led to democratic improvements in 19th-century England. Similarly, Brückner and Ciccone (2011) find that negative income shocks provided a window of opportunity for democratic improvements in Africa, whereas the absence of transitory negative income shocks was a factor that contributed to the consolidation of democracy in 19th-century France (Franck, 2016). Furthermore, evidence by Chaney (2013) documents the role of economic fluctuations in political revolts in ancient Egypt, Aidt and Jensen (2014) suggest that franchise extensions were the result of revolutionary threats in 19th- and 20th-century Europe, and Aidt and Leon (2016) show that drought-induced riots led to democratic concessions by incumbent elites in Africa. In contrast, Brückner

[3]Note that the theoretical prediction refers to a monotonic effect in the interaction between inequality and income shocks, but not to the effect of inequality per se, which has been conjectured to be non-monotonic (Acemoglu and Robinson, 2005) and which is more difficult to address from an empirical perspective, as discussed below.

et al. (2012) find that positive income shocks due to rising oil prices had a positive effect on democratic quality in countries that are net oil exporters.

Our finding that negative income shocks exert a positive effect on democratization complements this literature and reconciles earlier results for positive effects of income on democracy with evidence that negative income shocks have a positive effect on democratic improvements. We document the important role of substantial negative income fluctuations as the trigger for major changes in democratic institutions and a significant asymmetry in interaction with economic inequality. While this finding supports the theoretical implications noted above, it is not inconsistent with the positive effect of long-run economic development in facilitating the establishment of democracy as implied by Lipset (1959); however, the empirical identification of the effect of long-run improvements in economic development goes beyond the scope of analysis in this paper. Our findings complement recent evidence suggesting that income reveals vastly heterogeneous effects on democratic institutions across low- and high-income countries (Moral-Benito and Bartolucci, 2012) and with respect to colonial history (Cervellati *et al*., 2014b). By considering the role of modernization for different scenarios of democratization, our paper also complements evidence by Przeworski (2009) on the mechanisms behind franchise extensions to women.

This study most closely relates to work by Dorsch and Maarek (2014a,b), who explore whether episodes of democratization can be explained by variation in income inequality and investigate whether the effect of inequality is heterogeneous across the business cycle. Despite a similar focus on the interrelation of economic slumps and inequality, our paper differs from their work conceptually and empirically. Conceptually, our focus on economic shocks and their interaction with inequality is rooted in the theoretical literature, which has considered (negative) economic shocks as the main drivers of democratization, whereas inequality represents the underlying tension that magnifies or moderates the effects of these shocks. In contrast to the effect of income shocks, the effect of inequality is theoretically ambiguous (Acemoglu and Robinson, 2005). Empirically, our analysis requires exogeneity of income shocks for the identification of the effects of interest. This is a less demanding assumption than exogeneity of inequality, which moreover varies much less over time. Our findings indicate that regular business cycles do not have a systematic effect on democratization. Instead, large recessions do affect democratic institutions in interaction with inequality.

Our evidence also provides novel insights about the mechanism underlying the results, which point to social unrest as an indicator for the threat of revolution. Economic shocks and inequality exert a greater effect on the likelihood of peaceful democratic transitions than of violent

ones. At the same time, negative economic shocks are more likely to trigger riots if inequality is high. This evidence is in accordance with theoretical results derived from the first scenario of democratic transitions under the threat of revolution, where the elite concedes some political power to avoid a revolution if it is confronted with a credible threat, as reflected by revolts and riots. Finally, our empirical analysis also considers demographic pressure as another determinant of democratic transitions, thereby relating the results to demographic dynamics (e.g., Wilson and Dyson, 2017).

The remainder of this paper is structured as follows. In Section II, we present the empirical approach, data sources, and variable construction. In Section III, we present the main results and provide a brief discussion of robustness and additional findings. We conclude in Section IV.

II. Empirical Framework and Data

Empirical Framework

To test our hypotheses, the empirical framework focuses on identifying the effect of major income fluctuations on the quality of political institutions in interaction with inequality. The estimation framework exploits within-country variation over time in a dynamic linear panel model.[4] However, when investigating the effect of income shocks on major changes in institutional quality reflecting democratization, the use of year-on-year variation does not seem entirely appropriate, as it might be some time until the shocks fully materialize and the resulting mechanisms lead to perceptible institutional change. The empirical framework therefore considers the possibility of a democratic transition between years t and $t + k$ as the result of an income shock during the previous m years, as shown in Figure 1. This timing structure rules out feedback from democratization on income shocks by avoiding overlap between income shocks and democratization.

Accordingly, the estimation equation is given by

$$d_{i,t+k} = \alpha s_{i,(t-m,t)} + \beta x_{i,t} + \gamma[s_{i,(t-m,t)} \cdot x_{i,t}] + w'_{i,t}\delta + \zeta_i + \eta_{t+k} + \varepsilon_{i,t+k}, \quad (1)$$

in which $d_{i,t+k}$ either denotes a continuous measure of democratic quality in country i and year $t+k$ or a binary measure of democratization reflecting major changes in democratic quality in country i between the years t and $t + k$; $s_{i,(t-m,t)}$ indicates whether an income shock occurred during the time period $t - m$ and t; $x_{i,t}$ denotes inequality; and $w_{i,t}$ is a vector of controls that includes the quality of democratic institutions, the level of

[4]Similar specifications were used by Acemoglu *et al*. (2008) and Murtin and Wacziarg (2014).

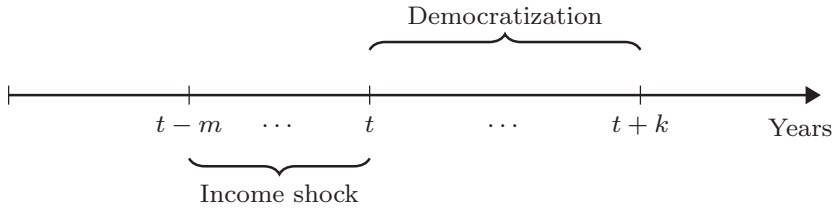

Fig. 1. Timing of shocks and democratization

income per capita, and education in period t. In the first step, our analysis considers marginal changes in income and democratic quality ($k = 1$ and $m = 0$) paralleling the set-up in earlier empirical literature using annual panel data. In the next step, our analysis considers major income shocks for the extended time window $(t-2, t)$ so as to allow these shocks to affect the political institutions within a fairly short period of time ($k = 1$ and $m = 2$). In the final step, our baseline model considers changes in democratic quality over the extended time window $(t, t+3)$ so as to capture major changes in democratic institutions ($k = 3$ and $m = 2$). The specification includes country-fixed and time-fixed effects, ζ_i and η_{t+k}. In light of our hypotheses, the coefficients of interest are α and γ.

We remove the country-fixed effects using the "within" transformation. As our analysis exploits a yearly panel from 1960 to 2014 with a maximum number of 54 time periods, the time dimension T is sufficiently large that the Nickell (1981) bias is of little concern for the identification of the coefficients of interest (Judson and Owen, 1999).

Identification of α and γ requires exogeneity of our measure of (negative) income shocks. We define them as cyclical fluctuations in income per capita. Unlike long-run growth trends, these fluctuations are arguably largely unforeseen by individual agents. For this purpose, we apply the Hodrick–Prescott (HP) filter (Hodrick and Prescott, 1997) to disentangle random cyclical fluctuations from long-run trends in economic development. In the final subsection of Section II, we discuss the construction of our shock indicator and alternative specifications in greater detail. Under the assumption that income shocks are plausibly exogenous, their direct effect α and their interaction with income inequality γ are estimated consistently, even if the exogeneity assumption does not hold for inequality (Bun and Harrison, 2019).[5]

[5]As one of the ways to postpone or even avoid democratization is to increase redistribution through taxation or price distortions, and as inequality only varies slowly, the exogeneity assumption for inequality, unlike for income fluctuations, is likely to be violated.

Data Sources

There is an ongoing debate about the appropriate measurement of institutional quality. Specifically, this debate refers to the information on which indices of institutional quality are based, as well as their measurement on a discrete or continuous scale. While continuous measurements conform more to the slowly changing nature of institutions described by North (1990), dichotomous measures provide a clearer distinction of the bi-modal distribution of political institutions as reflected in the democratization literature, but also as observed in practice (Cheibub *et al*., 2010). As there is no consensus regarding this question, we report results for both continuous and dichotomous measures from different sources. In particular, we use the composite PolityIV index by Marshall *et al*. (2013), a composite indicator based on the Freedom House (2014) Political Rights and Civil Liberties measures, and the binary Democracy–Dictatorship index by Cheibub *et al*. (2010). For comparability, we normalize all measures of democratic quality to a range from zero (full autocracy) to one (full democracy).[6] Following recent suggestions by Voigt (2013), we additionally construct an artificial indicator based on the principal components of the PolityIV, Freedom House, and Democracy–Dictatorship indicators. This composite index isolates and extracts the common variation among all three measures and combines them into a single indicator that can be interpreted as democratic institutions.[7]

Data for (log) income per capita are taken from the Penn World Tables by Feenstra *et al*. (2015). We proxy income inequality with market (pre-tax, pre-transfer) Gini coefficients from the Standardized World Income Inequality Database (SWIID) by Solt (2009, 2016). These data provide standardized Gini coefficients that are comparable across countries and time, and which appear to be the best available data for the purpose of this paper.[8] We normalize the Gini coefficients to vary between zero (maximum

[6]The PolityIV and composite Freedom House indices both constitute broad measures of institutional quality comprising features of not just the political but also the economic domain. Both dimensions are in practice highly correlated but not necessarily identical, as pointed out by Acemoglu and Johnson (2005) and Kotschy and Sunde (2017). We presume that democratic transitions aim to improve institutional quality compared to the previous regime. Hence, we choose rather broad measures of institutional quality to capture all facets of these transitions.

[7]Factor analysis synthesizes the variation contained in several variables into common, orthogonal factors, or principal components. In this way one can decompose the variation in institutional variables that corresponds more closely to democratic institutions from variation that corresponds more closely to other institutional dimensions, such as those affecting the economic domain. Hence, this artificial index corresponds to a narrower measure of democratic quality compared to its source indicators. For a detailed discussion, see Voigt (2013, pp. 20–21).

[8]In particular, the SWIID uses imputation procedures to construct a comprehensive set of inequality estimates over time, with the numerous Gini data points varying with respect to their

equality) and one (maximum inequality). Moreover, we control for human capital differences using data on average years of schooling from Barro and Lee (2013).

For additional analyses in the final subsection of Section III, we construct economic shocks based on alternative data sources. We use inflation rates obtained from the World Bank (2017) and data on sovereign defaults from Enderlein *et al.* (2012) and Trebesch and Zabel (2017). To examine the underlying mechanism of democratization, we furthermore use information on riots from Banks and Wilson (2013) and we distinguish between peaceful and violent democratization using the UCDP/PRIO Armed Conflict Dataset by Gleditsch *et al*. (2002) and Allansson et al. (2017). Finally, we measure demographic pressure based on data for age composition obtained from the United Nations (2017).

Economic Shocks and Binary Democracy Indicators

Our investigation focuses on the consequences of major income fluctuations – as opposed to marginal year-on-year variation – on major changes in democratic institutions. Likewise, we follow most theoretical models that consider dichotomous institutional regimes – democracy and autocracy – and use a dichotomous measure of political institutions rather than multi-valued indices or continuous measures.

We construct a binary indicator for income shocks that takes a value of one if there is a negative cyclical fluctuation that is larger than or equal to 5 percent relative to the income trend. Negative cyclical shocks of more than 5 percent are sizable. Even during the Great Recession and its aftermath, most Western countries did not experience shocks of more than 2 or 3 percent of income per capita. For example, in Greece, a country that was hit especially hard by the recession and the subsequent Euro Crisis, the largest shock amounted to a value of −5.33 percent in 2011. Consequently, such events are rare and occur in less than 5 percent of the country–year observations in our data. A large number of these shocks occurred in low- and middle-income countries. Because they impose a sizable strain on incomes and the political discourse within countries for some considerable time, we allow our income shocks to take a value of one up to $m = 2$ years after the shock occurred. Thus, when considering a democratic transition that begins in period t, the income shock $s_{i,(t-m,t)}$ is coded to take a value of one if a cyclical shock occurred in either one of the years $t-2$, $t-1$, or t, and zero otherwise. Overall, the income shocks measured in this way take

(un)certainty. According to Solt, "those pursuing research on income inequality across many countries and over time [...] will often find that the SWIID is their best choice of data source" (Solt, 2015, p. 690).

a value of one in 12 percent of the country–year observations. Choosing a longer window ($m > 2$) for income shocks to affect democratic institutions results in a larger number of shock observations and, correspondingly, a noisier measure. In contrast, choosing a shorter window ($m < 2$) results in fewer but more concentrated effects of income shocks on democratic institutions. At the same time, a shorter window is more likely to miss negative shocks that reveal their full effects only after several years. In the robustness analysis, we present results for alternative window lengths m.

We apply the HP filter to decompose the income per capita time series into a trend and a cyclical component. This requires setting a smoothing parameter that determines the smoothness of the trend component of the filtered series and thus the variability of the cyclical fluctuations. A lower smoothing parameter produces smaller cyclical fluctuations and thus stronger changes in the long-run trend, and vice versa. Following the rule suggested by Ravn and Uhlig (2002), we set the filter's smoothing parameter to $\lambda = 6.25$ in the baseline analysis.

In a recent contribution, Hamilton (2018) criticized the use of the HP filter. He showed that, under the usual assumptions made when calculating the HP filter, the decomposition produces a cyclical component that is unpredictable from past income dynamics. This randomness of the cyclical component is undesirable in the standard context, in which researchers want to discern particular dynamics of the cyclical component for economic analysis. In contrast, this randomness of the cyclical component is useful in our context, as it provides us with the exogenous variation for income shocks that is needed to identify the effect of interest. In addition, Hamilton mentions several drawbacks of the HP filter. Various robustness checks address his criticism and confirm the main findings throughout.

We construct a binary measure of changes in democratic quality to capture major changes in democratic institutions. This "democratization" indicator takes a value of one if the change in the normalized democratic quality between the years t and $t + k$ (where $k = 3$) exceeds a certain threshold. We account for disparities in variation among the different indicators of democratic quality by setting these thresholds to 0.5 for the PolityIV index, 0.3 for the Freedom House indicator, and 0.4 for the artificial principal components indicator. Our coding generates a similar number of roughly 80 democratic transitions across the three continuous democracy indices. By construction, democratization takes a value of one if the dichotomous Democracy–Dictatorship index changes from zero to one. Because the dependent variable is binary, the empirical framework corresponds to a linear probability model that estimates the likelihood of democratization conditional on economic shocks and inequality. In the robustness section, we explore the sensitivity of our findings with respect to

alternative choices of k and alternative thresholds for the democratization indicators.

In the baseline specification, income shocks occur before the democratization process starts. Conceptually, the income shocks might overlap with the switch from autocracy to democracy. In such a case, however, the income shock might be endogenous because the democratization process might feedback to economic performance. The baseline coding therefore represents a cleaner but also more conservative view on the effects of income shocks and inequality on democratization. Nevertheless, we also report results for income shocks that overlap with democratization in the robustness analysis.

In summary, the empirical analysis uses yearly unbalanced panel data of 130 countries for the period 1960–2014 with more than 3,000 country–year observations.[9]

III. Empirical Results

Income, Income Shocks, Inequality, and Democracy

Income and Income Shocks. As a first step, we replicate the standard specification in the related literature.[10] We use annual observations of a democracy index as the dependent variable and those of income per capita as the main regressor. We additionally include income inequality and its interaction with income. Panel A of Table 1 presents the results, which reproduce the analysis of Acemoglu *et al*. (2008) extended to the consideration of economic inequality and its interaction with income. The results confirm their main finding: that income does not affect democracy once country-fixed effects are accounted for. Moreover, the estimates provide no evidence for an interaction between income and inequality in shaping democratic institutions. The estimated coefficients of income and the interaction term do not significantly differ from zero.[11]

In the next step, we go beyond estimating the effect of marginal changes in income levels, and instead investigate the effects of substantial negative economic shocks. The dependent variable remains an index of democratic quality. Panel B of Table 1 presents the results. A negative cyclical income shock reveals a significant, negative direct effect on democratic quality.

[9]Table A1 in the Online Appendix presents descriptive statistics of the data.

[10]The replication data and code are available on the Harvard Dataverse at https://doi.org/10.7910/DVN/XF1QC7.

[11]Figure A1 in the Online Appendix illustrates this finding by comparing the (collapsed) unconditional variation between log gross domestic product (GDP) per capita and the PolityIV index with the residuals of both variables after partialling out country-fixed and time-fixed effects.

Table 1. *Income, inequality, and democracy*

	Democratic institutions in $t+1$ are measured by:			
	PolityIV index (1)	Political Rights & Civil Liberties (2)	Democracy–Dictatorship (3)	Principal components (4)
Panel A: Acemoglu et al. (2008) with inequality interaction				
Democratic Quality$_t$	0.86***	0.84***	0.80***	0.84***
	(0.02)	(0.02)	(0.02)	(0.02)
Income$_t$	0.02	0.01	0.01	0.02
	(0.03)	(0.02)	(0.05)	(0.03)
Inequality$_t$	0.42	0.19	−0.45	0.25
	(0.50)	(0.35)	(1.01)	(0.51)
(Income$_t$ ·Inequality$_t$)	−0.06	−0.02	0.01	−0.04
	(0.06)	(0.04)	(0.11)	(0.06)
Controls	Yes	Yes	Yes	Yes
Countries	128	133	131	125
Observations	3,898	3,794	3,307	3,026
R^2	0.82	0.78	0.71	0.79
Panel B: Negative cyclical income shocks and inequality				
Democratic Quality$_t$	0.86***	0.84***	0.80***	0.84***
	(0.02)	(0.02)	(0.02)	(0.02)
Shock$_{(t-2,t)}$	−0.09***	−0.05*	−0.16***	−0.09***
	(0.03)	(0.02)	(0.04)	(0.03)
Inequality$_t$	−0.11	−0.04	−0.38**	−0.16
	(0.08)	(0.06)	(0.19)	(0.11)
(Shock$_{(t-2,t)}$·Inequality$_t$)	0.19***	0.12**	0.35***	0.21***
	(0.06)	(0.05)	(0.09)	(0.06)
Controls	Yes	Yes	Yes	Yes
Shocks	492	490	400	382
Countries	128	133	131	125
Observations	3,881	3,782	3,290	3,015
R^2	0.82	0.79	0.71	0.79

Notes: All regressions include country-fixed effects, time-fixed effects, and a control for average years of schooling. The regressions in Panel B additionally control for log income per capita. The shock indicator in Panel B takes a value of one if there is at least once a negative cyclical shock of at least −5 percent within the time interval $(t-2, t)$, and zero otherwise. Standard errors are clustered on the country level. Asterisks indicate significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

According to the estimates in Column 1, the direct effect of a negative income shock implies a reduction in democratic quality of 0.09 on the normalized PolityIV index, which ranges from zero to one. However, the interaction term suggests that the marginal effect of a negative income shock increases with the level of income inequality. For sufficiently unequal countries, the income shock thus has a positive effect on democratic

quality. To exemplify this, the empirical results in Column 1 imply that the marginal effect of a negative economic shock on democratic quality turns positive above a threshold value of 0.46 for the Gini coefficient. This value corresponds to the 60th percentile of the Gini coefficient in the estimation sample. Hence, the estimated marginal effect of a negative cyclical shock on democratic quality is positive for approximately 40 percent of the observations and negative for the remainder. Similar estimates are obtained for other indices of democratic quality. This result provides a first piece of evidence that income shocks – rather than minor fluctuations in income – trigger changes in democratic quality. Lastly, we find no coherent pattern regarding the main effect of inequality.

Democratic Quality versus Democratization. As a third and final step, the analysis considers binary democratization indicators instead of index measures of democratic quality. The following analysis thus considers the effect of income shocks on major changes in democratic institutions (i.e., democratization), as opposed to marginal changes as reflected by an index measure. Table 2 presents our baseline results for the effects of negative income shocks and their interaction with inequality on the likelihood of democratization. Countries with high initial democratic quality are less likely to undergo a transition from a non-democratic to a democratic regime. Negative income shocks have a negative direct effect $\widehat{\alpha}$, which varies between −0.13 and −0.33. A negative income shock thus lowers the likelihood of democratization by between 13 and 33 percentage points. The direct effect is moderated by the interaction between income shocks and inequality. The estimated interaction term $\widehat{\gamma}$ has a positive sign for all democratization indicators and varies between 0.31 and 0.68. The estimated direct effect and the estimated interaction term are both significant at the 5 percent level across all specifications. Furthermore, they are quantitatively larger than those of the model that uses index measures of democratic quality.

Hence, the marginal effect of an income shock on the likelihood of democratization is non-monotonic and can be either positive or negative, conditional on the level of inequality. The marginal effect can be obtained by computing

$$\mathrm{ME}_{i,t+3}^{\mathrm{shock}} = \widehat{\alpha} + \widehat{\gamma} \cdot x_{i,t}. \tag{2}$$

The estimates in Column 1 of Table 2 therefore imply a positive marginal effect of a negative income shock on the likelihood of democratization for Gini values above 0.46 in year t, and a negative marginal effect otherwise.

Figure 2 displays the marginal effect of a negative income shock on the likelihood of major changes in democratic institutions for all specifications

Table 2. *Negative cyclical income shocks, inequality, and democratization*

	Indicator for democratization between t and $t+3$ based on:			
	PolityIV index (1)	Political Rights & Civil Liberties (2)	Democracy–Dictatorship (3)	Principal components (4)
Democratic Quality$_t$	−0.29***	−0.35***	−0.32***	−0.40***
	(0.04)	(0.05)	(0.03)	(0.05)
Shock$_{(t-2,t)}$	−0.17***	−0.13**	−0.33***	−0.26***
	(0.05)	(0.06)	(0.08)	(0.07)
Inequality$_t$	−0.32**	−0.02	−0.82**	−0.54**
	(0.16)	(0.17)	(0.33)	(0.26)
(Shock$_{(t-2,t)}$·Inequality$_t$)	0.37***	0.31**	0.68***	0.55***
	(0.12)	(0.14)	(0.16)	(0.15)
Controls	Yes	Yes	Yes	Yes
Transitions	76	81	114	85
Shocks	448	450	349	332
Countries	128	133	129	124
Observations	3,678	3,575	3,036	2,773
R^2	0.14	0.12	0.21	0.16

Notes: All regressions include country-fixed effects, time-fixed effects, and controls for log income per capita and average years of schooling. The dependent variable takes a value of one if there is a major change in democratic institutions between t and $t+3$ that equals or exceeds the respective threshold described in the final subsection of Section II, and zero otherwise. The shock indicator takes a value of one if there is at least once a negative cyclical shock of at least −5 percent within the time interval $(t-2, t)$, and zero otherwise. Standard errors are clustered on the country level. Asterisks indicate significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

in Table 2. The solid lines represent the marginal effects, whereas the dashed lines depict the 95 percent confidence intervals.[12] The histogram shows the distribution of inequality in the different estimation samples. The marginal effects are significantly positive (negative) at the 5 percent level for a sufficiently unequal (equal) distribution of incomes. For intermediate levels of inequality, by contrast, they are quantitatively small and statistically insignificant.

Our model predicts that negative income shocks will reveal a positive marginal effect on the likelihood of democratization in roughly 53 percent of the country–year observations and a negative marginal effect in the remaining 47 percent. Given the domain of Gini coefficients in year t of $(0.23, 0.68)$, the estimated marginal effects lie in the interval $(-0.17, 0.13)$ with a zero effect for intermediate levels of inequality. In the most unequal

[12]For a large sample, the confidence interval of the marginal effect is $(ME \pm z_{1-\tau/2} \cdot \widehat{SE})$ with $\widehat{SE} = \sqrt{\text{var}(\widehat{\alpha}) + x_t^2 \cdot \text{var}(\widehat{\gamma}) + 2 \cdot x_t \cdot \text{cov}(\widehat{\alpha}, \widehat{\gamma})}$ and the critical value $z_{1-\tau/2}$ of a two-sided t-test of size τ.

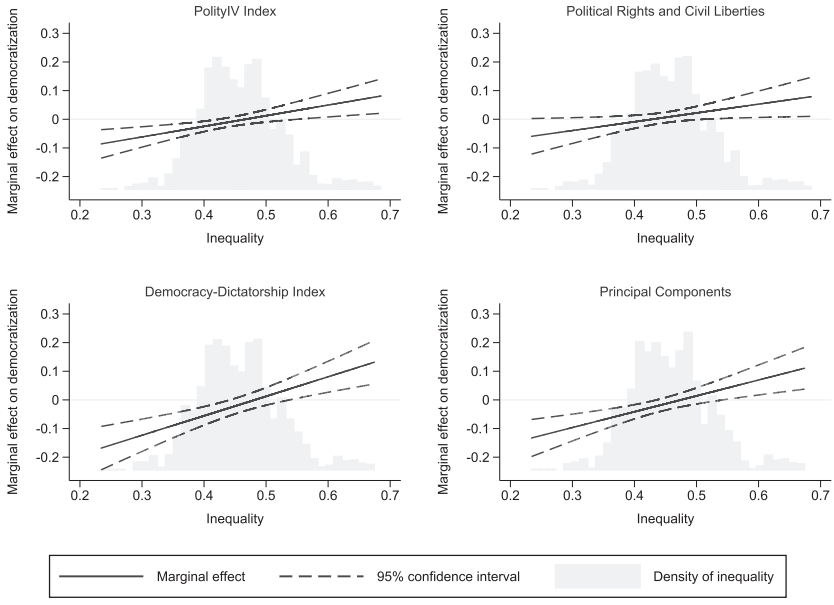

Fig. 2. Effect of a negative income shock on major changes in democratic quality
Notes: The marginal effects of negative income shocks are based on the estimates in Table 2.

society, a negative income shock increases the likelihood of democratization by 13 percentage points. In contrast, a negative income shock reduces the likelihood of a substantial change in democratic quality by 17 percentage points in the most equal society. Finally, in the absence of shocks, inequality is associated with lower democratic quality.

These findings are in accordance with historical examples. For example, negative economic shocks preceded democratic reforms such as elections and the implementation of a multi-party system in several African countries with high levels of inequality.[13] On the opposite spectrum, several countries with low inequality did not experience major democratic improvements after negative economic shocks. In some cases, there were even tendencies toward greater autocracy.[14] When interpreting particular historical examples

[13]Examples are the Central African Republic, Malawi, and Zambia. They were governed autocratically until around 1990 and had high inequality with Gini values of 0.55 or higher. Economic shocks during this period led to riots and ultimately the end of their autocratic systems.
[14]Examples are Kazakhstan, Tajikistan, Tanzania, and Uganda, which all exhibited a Gini value of around 0.35 during the 1990s. Despite repeated economic shocks, such as recessions or phases of high inflation, these countries' democratic institutions did not improve and in some cases even deteriorated.

in light of the empirical findings, one should, however, keep in mind that the estimation results correspond to the average marginal effect of negative economic shocks on the likelihood of democratization. Hence, they need not provide an accurate description of each historical event. In Iran, for example, the realized political institutions do not correspond to the empirical predictions.[15]

Positive and Negative Shocks. Up to this point, the analysis has restricted attention to negative income shocks and confirmed the finding of a non-monotonic effect of such shocks on the likelihood of democratization conditional on the extent of inequality. Similarly to the rationale of a window of opportunity for democratization during economic downturns, however, positive shocks might dampen support for a switch from autocracy to democracy during economic upturns and stabilize autocracies. Analogous to the negative income shocks, we also construct a binary indicator for positive income shocks. We code a value of one if there is at least once a positive cyclical shock of at least 5 percent within the time interval $(t-2, t)$, and zero otherwise. About 15 percent of the country–year observations qualify as positive income shocks, of which most occur in low- and middle-income countries.

Table 3 presents the results for a specification with positive and negative income shocks. Both variables enter the estimation equation separately in order to account for the possibility of heterogeneous effects of positive and negative income shocks. Country–year observations that do not exhibit either type of shock therefore constitute the reference category. Across all specifications, negative income shocks again exhibit a negative direct effect on the likelihood of democratization. Moreover, the interaction term between negative income shocks and inequality is again positive and significant at the 5 percent level. Notably, the point estimates for the negative income shock and the interaction term are quantitatively almost identical to the baseline estimates. Positive income shocks, in contrast, do not affect the likelihood of democratization: the results provide evidence for neither a direct effect nor an indirect effect through the interaction with inequality. In light of this result, we concentrate only on negative income shocks for the remainder of this paper.

[15] By the late 1970s, Iran was characterized by high inequality, as reflected by a Gini value of 0.56 in 1979, and economic shocks arguably contributed to the Shah's downfall, which, it was widely believed, would eventually give rise to democracy. Nevertheless, there was no democratization. Instead, the quality of democratic institutions fluctuated considerably during the 1990s and 2000s, before the political system stabilized as an autocracy.

Table 3. *Cyclical income shocks, inequality, and democratization*

	Indicator for democratization between t and $t+3$ based on:			
	PolityIV index (1)	Political Rights & Civil Liberties (2)	Democracy–Dictatorship (3)	Principal components (4)
Democratic Quality$_t$	−0.29***	−0.35***	−0.33***	−0.40***
	(0.04)	(0.05)	(0.03)	(0.05)
Negative Shock$_{[t-2,t]}$	−0.17***	−0.13**	−0.33***	−0.27***
	(0.05)	(0.06)	(0.08)	(0.07)
Positive Shock$_{[t-2,t]}$	−0.04	−0.02	−0.10	−0.03
	(0.07)	(0.08)	(0.10)	(0.09)
Inequality$_t$	−0.33**	−0.01	−0.84**	−0.55**
	(0.16)	(0.17)	(0.33)	(0.26)
(Negative Shock$_{(t-2,t)}$·Inequality$_t$)	0.36***	0.31**	0.68***	0.56***
	(0.12)	(0.14)	(0.17)	(0.15)
(Positive Shock$_{(t-2,t)}$·Inequality$_t$)	0.12	0.08	0.27	0.11
	(0.16)	(0.18)	(0.21)	(0.20)
Controls	Yes	Yes	Yes	Yes
Transitions	76	81	114	85
Negative shocks	448	450	349	332
Positive shocks	534	523	430	394
Countries	128	133	129	124
Observations	3,678	3,575	3,036	2,773
R^2	0.14	0.12	0.21	0.16

Notes: All regressions include country-fixed effects, time-fixed effects, and controls for log income per capita and average years of schooling. The dependent variable takes a value of one if there is a major change in democratic institutions between t and $t+3$ that equals or exceeds the respective threshold described in the final subsection of Section II, and zero otherwise. The shock indicator takes a value of one if there is at least once a negative/positive cyclical shock of at least ±5 percent within the time interval $(t-2, t)$, and zero otherwise. Standard errors are clustered on the country level. Asterisks indicate significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Robustness

This section discusses robustness checks regarding alternative specifications or modifications in the coding of variables. The corresponding tables are presented in the Online Appendix.

Accounting for Multiple Imputation. The SWIID uses imputation procedures to construct a comprehensive set of inequality estimates over time. As this imputation procedure might understate the uncertainty in the data, the dataset provides 100 potential realizations of the Gini coefficient for a given country and year, which allow for a standard error adjustment as suggested by Solt (2016). Accounting for uncertainty related to multiple imputation requires a time-consuming procedure in which the analysis is conducted 100 times for the different potential Gini values. Table A2 in the

Online Appendix shows that this correction is inessential for the quantitative and qualitative results of the baseline specification.

Time Window for Income Shocks and Positive Income Shocks. The construction of the income shock variable requires an assumption about the time window m, during which the cyclical component is sufficiently large relative to the trend component. We investigate the robustness of the main results with respect to the length of this time window for income shocks and with respect to the construction of binary shock indicators during this window. Regarding the length, a narrower time window ($m < 2$) might allow for more precise timing of the effect, but it might miss the effects of prolonged economic downturns. Instead, choosing a longer window ($m > 2$) results in a larger number of income shocks and thus a noisier measure. Table A3 in the Online Appendix shows that the results do not change considerably for different codings of the window length of income shocks (for example, $m = 0$ or $m = 4$). Overall, the estimates qualitatively confirm our main findings. Income shocks reveal a smaller marginal effect on the likelihood of democratization if $m = 0$, whereas the marginal effect is almost identical to the baseline result if $m = 4$.

Regarding the construction of the binary indicator, the baseline analysis uses yearly data but our income shocks combine information over a sequence of three years to estimate the effect on the likelihood of democratization during the subsequent three-year time window. A shock in one year thus triggers a coding of the binary indicator for three consecutive years. We verified the robustness of our results when ensuring that the same country–year shock does not appear in more than one observation. To that end, we collapsed the data into non-overlapping three-year observation periods (a panel of three-year frequency). The results are quantitatively very similar, as shown by Table A4 in the Online Appendix.

Finally, sufficiently strong cyclical fluctuations over the time window $(t - 2, t)$ might generate observations that qualify as both positive and negative income shocks; in fact, few observations fall into this category. In order to be fully symmetric and transparent regarding the possibility that the presence of negative shocks could take up the effect of positive shocks in the same period, Table A5 in the Online Appendix shows the results for a specification that restricts attention exclusively to positive shocks (analogous to the specifications that only consider negative shocks in the baseline specification). The results confirm that positive income shocks have no effect on democratization.

Length of Time Window for Changes in Democracy. Ideally, democratization processes that are triggered by economic shocks should reveal the first

detectable results within a short time-frame. However, the democratization process might span over a longer period and require more than one year to be completed. This is the main reason for considering a three-year horizon in the baseline specification. Table A6 in the Online Appendix shows that the empirical results are insensitive to different codings of the democratization period (for example, $k = 1$ or $k = 5$). In particular, the qualitative patterns in terms of sign and significance of the coefficients are identical, while the effect sizes appear to be slightly larger when considering longer time windows.

Sensitivity of the Trend-Cycle Decomposition. The construction of the variable of income shocks is based on a standard HP decomposition. The use of the HP filter has recently been criticized by Hamilton (2018) on theoretical and practical grounds. Hamilton mentions several drawbacks of the HP filter that relate to the appropriate choice of the smoothing parameter, to instability close to the start or end of the observation window, and to autocorrelation in the cyclical component (which is induced by the HP filter itself as a consequence of the use of future realizations in the computation of the decomposition). To account for these drawbacks of the HP filter and to explore the robustness of our results with respect to the use of the HP filter, we conduct a series of robustness checks.

As the HP decomposition requires an assumption about the smoothing parameter λ, we explore how the effect of negative income shocks on democratic institutions varies for different specifications of λ. The results in Table A7 in the Online Appendix document that income shocks reveal a stronger effect on the likelihood of democratic transitions for a more smoothed long-run trend ($\lambda = 1$) with fewer shocks, and a weaker effect for a less smoothed long-run trend ($\lambda = 100$) with more shocks. We also account for potential instability of the cyclical component at the margins of our sample by omitting these observations from the analysis. The corresponding results in Table A8 in the Online Appendix confirm the main findings. Finally, Hamilton (2018) proposes an alternative approach to constructing income shocks based on the residuals of regressions of income levels on a constant and lagged values, which does not suffer from the problem of autocorrelation in the cyclical component induced by the use of future realizations in the computation of the HP decomposition. Using this method, we obtain similar results as in the main analysis; see Table A9 in the Online Appendix.

Thresholds for Changes in the Binary Democracy Variable. Constructing a binary democratization variable requires an assumption about a threshold for the required change in the normalized democracy index. Table A10 in

the Online Appendix shows that our findings do not hinge on the difference in thresholds for the construction of our dichotomous democratization variable.

Alternative Binary Democracy Measures. As an additional robustness check, we apply our coding of negative income shocks and democratic transitions to alternative measures of democratization. First, we replicate the analysis for the binary democracy variables constructed by Papaioannou and Siourounis (2008) and the alternative variable suggested by Acemoglu *et al*. (2019). Both measures code democratizations based on sizable changes in the democracy indicators by PolityIV and Freedom House in conjunction with stability criteria for successful democratic transitions. Table A11 in the Online Appendix shows that applying our coding choices to the dataset of Acemoglu *et al*. (2019) yields the same qualitative results, albeit with quantitatively slightly larger coefficients. The cut-off values for the Gini coefficient in period t are quantitatively almost identical to our baseline specification. Thus, the marginal effects are slightly larger in absolute terms for a given level of inequality. Hence, the main results do not hinge on coding conventions regarding democratic quality. Moreover, our baseline results provide a conservative view on the role of income shocks and their interaction with inequality on the likelihood of democratic transitions.

Time Overlap between the Occurrence of Shocks and Changes in Democracy. In our baseline specifications, economic shocks occur before the democratization process starts. Conceptually, the switch from autocracy to democracy and economic shocks might also overlap. The baseline specification thus imposes a timing restriction, which is potentially too conservative to capture the full effects of economic shocks on the likelihood of democratization. However, this timing restriction prevents feedback from democratization to economic shocks that would render the explanatory variables endogenous. We test the robustness of our results with respect to this timing restriction by estimating models that allow for the democratization process and the income shocks to overlap. Specifically, we continue to measure changes in democratic quality between t and $t + 3$. However, the income shocks enter the empirical analysis in either year $t + 1$ or $t + 2$ such that the income shocks and democratic transitions overlap. As Table A12 in the Online Appendix documents, the estimated interaction term for income shocks and inequality is larger than in the baseline specification. Our baseline coding thus presents a rather conservative view on the marginal effect of income shocks on the likelihood of democratization.

Logit Estimates. Even though the linear probability model is preferable in the context of specifications with interactions, the binary dependent variable suggests the use of a logit estimator. As Table A13 in the Online Appendix shows, conditional logit regressions produce similar qualitative results as the linear probability model. Due to the small number of major political transitions and income shocks in the sample, however, the logistic regressions drop a large number of observations for which the estimated likelihood would diverge to infinity. Therefore, the logit results should be considered suggestive at best.

Additional Results

This section reports further results with respect to different measures of economic shocks and sheds light on the mechanisms underlying the main results.

Inflation Shocks. In our baseline specification, we define economic shocks as cyclical fluctuations around the long-run growth trend. Another way of modeling economic shocks is to consider hikes in inflation rates. Many low-income countries, for example, rely heavily on the export of certain agricultural products and natural resources. Fluctuations in international commodity prices might put substantial strain on per capita incomes and create a window of opportunity for changes in political institutions.[16] We construct inflation shocks based on price changes derived from the GDP deflator. Because countries that catch up economically typically experience high inflation during the convergence process, we limit our attention to inflation rates that equal or exceed 20 percent. We follow our coding of negative income shocks and code an inflation shock if there is at least once inflation of at least 20 percent within the interval $(t-2, t)$, and zero otherwise. Approximately 24 percent of the country–year observations of the dataset can be classified as inflation shocks. Most of these cases are observed in low- and middle-income countries with strong persistence regarding the (in-)stability of prices. Due to data availability, the estimation sample shrinks by between 300 and 400 country–year observations.

Table 4 reports results for the effect of inflation shocks and their interaction with inequality on the likelihood of democratization. The point estimates of the inflation shock and the interaction term have similar magnitudes as those obtained for negative cyclical shocks in the baseline

[16]See, for example, Brückner *et al*. (2012), who exploit oil price shocks to identify changes in democratic quality.

Table 4. *Inflation shocks, inequality, and democratization*

	Indicator for democratization between t and $t+3$ based on:			
	PolityIV index (1)	Political Rights & Civil Liberties (2)	Democracy–Dictatorship (3)	Principal components (4)
Democratic Quality$_t$	−0.31***	−0.33***	−0.33***	−0.39***
	(0.05)	(0.05)	(0.03)	(0.06)
Shock$_{[t-2,t]}$	−0.18***	−0.13*	−0.26**	−0.17
	(0.06)	(0.08)	(0.11)	(0.11)
Inequality$_t$	−0.35*	−0.06	−1.00**	−0.57*
	(0.19)	(0.15)	(0.40)	(0.33)
(Shock$_{[t-2,t]}$·Inequality$_t$)	0.40***	0.29	0.53**	0.34
	(0.13)	(0.18)	(0.23)	(0.23)
Controls	Yes	Yes	Yes	Yes
Transitions	64	61	94	70
Shocks	715	682	639	606
Countries	123	123	118	118
Observations	3,239	3,132	2,547	2,440
R^2	0.15	0.10	0.20	0.14

Notes: All regressions include country-fixed effects, time-fixed effects, and controls for log income per capita and average years of schooling. The dependent variable takes a value of one if there is a major change in democratic institutions between t and $t+3$ that equals or exceeds the respective threshold described in the final subsection of Section II, and zero otherwise. The shock indicator takes a value of one if there is at least once inflation of at least 20 percent within the time interval $(t-2, t)$, and zero otherwise. Standard errors are clustered on the country level. Asterisks indicate significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

specification. However, due to the considerably larger number of shocks, the coefficients are less precisely estimated than those obtained for negative cyclical shocks. The empirical model thus lacks the ability to estimate an interaction term that is statistically different from zero at conventional significance levels for the specifications in Columns 2 and 4. Nonetheless, the results again confirm the non-monotonic effect of economic shocks on the likelihood of democratization conditional on the degree of inequality. Given the distribution of Gini coefficients in year t, the marginal effects of an inflation shock on the likelihood of democratization lie in the interval $(-0.14, 0.10)$. This range closely conforms to the range of marginal effects obtained for the baseline model. We interpret this result as another piece of evidence that shocks to (disposable) income – rather than minor fluctuations in income levels – trigger major changes in democratic institutions.

Sovereign Defaults. Yet another way of modeling shocks is to consider financial crises that result in sovereign default. To this end, we use the data of Enderlein *et al.* (2012) and Trebesch and Zabel (2017), which provide information on 25 sovereign defaults in 61 developing and emerging

market economies between 1980 and 2009. Following the definition of Standard & Poor's (2011), these papers classify a country as being in default if the government misses payments on bonds or loans on due time or, alternatively, if it announces debt restructuring.[17] We adopt their classification and code a sovereign default indicator, which takes a value of one if a country fails to repay a part of its debt or announces a restructuring of its debt at least once within the time interval $(t-2, t)$, and zero otherwise.

The data by Enderlein *et al.* (2012) and Trebesch and Zabel (2017) exclude advanced economies, small countries with fewer than one million inhabitants, countries with insufficient data quality, states that restructured their debt in the context of state dissolution, and highly indebted poor countries that have only very limited access to financial markets. We code our default indicator to take a value of zero for these countries for a variety of reasons. The advanced economies experienced essentially no sovereign default during the observation period. For the small and highly indebted poor countries, we lack data on inequality and income to the point where they do not enter the estimation sample. Countries that democratize during the dissolution of larger states enter the sample only after the completed dissolution, as we lack data from before and during dissolution. For countries with insufficient data quality, the picture is less clear; however, our coding represents a lower bound for the number of defaults, which in case of doubt leads to a less informative measure. Therefore, the default indicator provides a conservative view on the effect of sovereign defaults on the likelihood of democratization. In total, roughly 6 percent of the country–year observations in the data can be classified as sovereign defaults.

Table 5 reports the estimated effects of sovereign defaults and their interaction with inequality on the likelihood of democratization. The point estimates are quantitatively similar to the baseline estimates although to some degree less precise. The findings again confirm the non-monotonic pattern of economic shocks on the likelihood of democratization conditional on inequality. Given the distribution of Gini coefficients in year t, the marginal effects of a default shock on the likelihood of democratization lie within the interval $(-0.15, 0.15)$. This range again conforms closely to the range of marginal effects obtained for the baseline model. Interestingly, sovereign defaults seem to affect democratization independently of negative cyclical shocks: a model that controls for negative cyclical shocks, sovereign defaults, and their interactions with inequality delivers quantitatively almost identical point estimates for cyclical shocks and sovereign defaults.[18] We view this as evidence for different types of economic shocks, which –

[17] See Trebesch and Zabel (2017, p. 422) and Standard & Poor's (2011, Online Appendix 1).
[18] See Table A14 in the Online Appendix.

Table 5. *Sovereign defaults, inequality, and democratization*

	Indicator for democratization between t and $t+3$ based on:			
	PolityIV index	Political Rights & Civil Liberties	Democracy–Dictatorship	Principal components
	(1)	(2)	(3)	(4)
Democratic Quality$_t$	−0.29***	−0.35***	−0.32***	−0.40***
	(0.04)	(0.05)	(0.03)	(0.05)
Defaults$_{(t-2,t)}$	−0.22**	−0.13*	−0.30**	−0.17
	(0.11)	(0.07)	(0.15)	(0.13)
Inequality$_t$	−0.30*	−0.03	−0.79**	−0.52**
	(0.15)	(0.16)	(0.32)	(0.26)
(Defaults$_{(t-2,t)}$·Inequality$_t$)	0.50**	0.35**	0.66*	0.38
	(0.25)	(0.15)	(0.34)	(0.28)
Controls	Yes	Yes	Yes	Yes
Transitions	76	81	114	85
Sovereign defaults	226	227	225	223
Countries	128	133	129	124
Observations	3,695	3,587	3,053	2,784
R^2	0.14	0.12	0.20	0.16

Notes: All regressions include country-fixed effects, time-fixed effects, and controls for log income per capita and average years of schooling. The dependent variable takes a value of one if there is a major change in democratic institutions between t and $t+3$ that equals or exceeds the respective threshold described in the final subsection of Section II, and zero otherwise. The sovereign default indicator takes a value of one if a country fails to repay a part of its debt or announces a restructuring of its debt at least once within the time interval $(t-2, t)$, and zero otherwise. Standard errors are clustered on the country level. Asterisks indicate significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

independently from each other – affect the quality and stability of political institutions.

Income Trends. The specifications up to this point considered short-term income shocks together with inequality as the key determinant for democratization. Long-run growth trends constitute an alternative angle to approach the definition of economic shocks. A period of prolonged stagnation or shrinkage presents a potential environment in which the people could voice their discontent with the existing political institutions. According to this rationale, we code a negative economic trend indicator, which takes a value of one if the HP-filtered income per capita series shrinks by at least 5 percent between years $t-3$ and t, and zero otherwise.[19]

[19]Note that the coding of growth trends refers to changes between years and not shocks within years. Hence, this coding requires us to include the change between years $t-3$ and $t-2$ to be fully symmetric to the shock indicators that refer to the time interval $(t-2, t)$.

Table 6. *Negative growth trends, inequality, and democratization*

	Indicator for democratization between t and $t+3$ based on:			
	PolityIV index (1)	Political Rights & Civil Liberties (2)	Democracy–Dictatorship (3)	Principal components (4)
Democratic Quality$_t$	−0.29***	−0.35***	−0.32***	−0.40***
	(0.04)	(0.05)	(0.03)	(0.05)
Trend$_{[t-3,t]}$	−0.22***	−0.19*	−0.24*	−0.25**
	(0.07)	(0.11)	(0.13)	(0.11)
Inequality$_t$	−0.32**	−0.02	−0.84**	−0.57**
	(0.15)	(0.16)	(0.33)	(0.26)
(Trend$_{[t-3,t]}$·Inequality$_t$)	0.49***	0.49*	0.50*	0.57**
	(0.18)	(0.25)	(0.29)	(0.26)
Controls	Yes	Yes	Yes	Yes
Transitions	76	81	114	85
Trends	256	263	242	237
Countries	128	133	129	124
Observations	3,663	3,561	3,022	2,759
R^2	0.14	0.12	0.20	0.16

Notes: All regressions include country-fixed effects, time-fixed effects, and controls for log income per capita and average years of schooling. The dependent variable takes a value of one if there is a major change in democratic institutions between t and $t+3$ that equals or exceeds the respective threshold described in the final subsection of Section II, and zero otherwise. The trend indicator takes a value of one if the HP-filtered income per capita series shrinks by at least 5 percent between $t-3$ and t, and zero otherwise. Standard errors are clustered on the country level. Asterisks indicate significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Prolonged trends of income shrinkage occur in 7 percent of the country–year observations, with a strong concentration in low- and middle-income countries.

Table 6 reports results for the effects of negative growth trends and their interaction with inequality on the likelihood of democratization. The estimated coefficients again confirm the finding of a non-monotonic effect of economic downturns on the likelihood of democratization conditional on the level of inequality. The point estimates show a similar qualitative pattern as those obtained for the baseline model. However, the estimated coefficients for negative growth trends vary less across the different specifications. The estimated coefficients for negative growth trends and their interaction with inequality are significant at the 10 percent level across all specifications, with the most precise estimates being for the PolityIV index. The marginal effects of a negative growth trend on the likelihood of democratization are quantitatively similar to the baseline estimates and lie in the interval $(-0.13, 0.14)$. We acknowledge that it is much harder (if not impossible) to argue that prolonged stagnation is unforeseen by the people as compared to the different economic shocks

described above. Nevertheless, we view these results as another piece of evidence that highlights the importance of major income fluctuations – rather than marginal changes in income levels – for major changes in democratic institutions.

Mechanisms: Democratization Scenario, Riots, and Demography. We close the empirical analysis by exploring the underlying mechanisms stipulated by the theoretical literature. Recall that the empirical analysis builds on the hypotheses that negative income shocks affect the likelihood of democratization and that this effect is crucially affected by the prevailing inequality, which follow from the two strands of democratization models. So, in order to explore the empirical relevance of the different democratization models, we first investigate the heterogeneity in the income-democracy nexus depending on the type of democratization. Specifically, we distinguish between peaceful transitions, during which there is no or only little armed conflict, and violent transitions, which are accompanied by significant armed conflict. This distinction is motivated by theoretical predictions and empirical findings that civil conflict during the democratization process reveals persistent negative effects on subsequent institutional quality and economic growth (Cervellati and Sunde, 2014; Cervellati *et al*., 2014a).

For the classification of peaceful and violent democratization, we use (binary) information from the UCDP/PRIO Armed Conflict Dataset on incidences of armed conflict in a given country and year. In this dataset, an armed conflict is defined as "a contested incompatibility that concerns government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at least 25 battle-related deaths in a calendar year" (UCDP/PRIO, 2017, p. 1). We ignore armed conflicts that involve another state and confine our sample to internal armed conflicts only. Specifically, we classify a democratization as violent if during the period of democratization there was violent conflict in at least one year. If there was no violence in any year, the democratization is considered non-violent and therefore peaceful.

Table 7 reports results distinguishing between peaceful and violent transitions to democracy. The specification controls for conflicts over the past three years and, in order to keep the results comparable across transitions, we restrict our attention to the subsample of countries that experienced at least one democratic transition.[20] The baseline specification is extended to account for civil conflicts during past years. Panel A shows

[20]The results are qualitatively similar for a specification without a control for past conflict or when considering the full sample: see Tables A15 and A16 in the Online Appendix.

Table 7. *Peaceful and violent democratization*

	Indicator for democratization between t and $t+3$ based on:			
	PolityIV index (1)	Political Rights & Civil Liberties (2)	Democracy–Dictatorship (3)	Principal components (4)
Panel A: Peaceful democratization				
Democratic Quality$_t$	−0.30*** (0.05)	−0.45*** (0.07)	−0.24*** (0.04)	−0.37*** (0.06)
Past Conflict$_{[t-2,t]}$	−0.03** (0.01)	−0.07** (0.03)	−0.02 (0.02)	−0.05** (0.02)
Shock$_{[t-2,t]}$	−0.25*** (0.09)	−0.34*** (0.10)	−0.37*** (0.11)	−0.37*** (0.12)
Inequality$_t$	−0.10 (0.31)	−0.01 (0.35)	−0.93* (0.48)	−0.68 (0.47)
(Shock$_{[t-2,t]}$·Inequality$_t$)	0.54*** (0.20)	0.77*** (0.22)	0.79*** (0.26)	0.77*** (0.26)
Controls	Yes	Yes	Yes	Yes
Transitions	52	59	68	56
Shocks	222	197	174	167
Countries	47	56	54	48
Observations	1,473	1,453	1,371	1,096
R^2	0.17	0.17	0.17	0.18
Panel B: Violent democratization				
Democratic Quality$_t$	−0.12*** (0.04)	−0.11*** (0.04)	−0.14*** (0.03)	−0.15*** (0.04)
Past Conflict$_{[t-2,t]}$	0.01 (0.02)	−0.01 (0.02)	0.03 (0.02)	0.01 (0.02)
Shock$_{[t-2,t]}$	−0.00 (0.04)	0.06 (0.11)	−0.22** (0.09)	−0.12 (0.07)
Inequality$_t$	−0.02 (0.26)	0.02 (0.20)	−0.52 (0.37)	−0.11 (0.36)
(Shock$_{[t-2,t]}$·Inequality$_t$)	0.00 (0.09)	−0.13 (0.22)	0.44** (0.19)	0.23 (0.15)
Controls	Yes	Yes	Yes	Yes
Transitions	24	22	46	29
Shocks	222	197	174	167
Countries	47	56	54	48
Observations	1,473	1,453	1,371	1,096
R^2	0.08	0.09	0.13	0.10

Notes: All regressions include country-fixed effects, time-fixed effects, and controls for log income per capita and average years of schooling. Observations of democratization are split into two categories: non-violent and violent. In Panel A, the dependent variable takes a value of one if two conditions are met: (i) there is a major change in democratic institutions between t and $t+3$ that equals or exceeds the respective threshold described in the final subsection of Section II, and (ii) during the period of democratization there is no violent conflict in any year; otherwise the dependent variable takes a value of zero. In Panel B, the dependent variable takes a value of one if two conditions are met: (i) there is a major change in democratic institutions between t and $t+3$ that equals or exceeds the respective threshold described in the final subsection of Section II, and (ii) during the period of democratization there is violent conflict in at least one year; otherwise the dependent variable takes a value of zero. Past conflict takes a value of one if there is violent political conflict at least once within the time interval $(t-2, t)$, and zero otherwise. The shock indicator takes a value of one if there is at least once a negative cyclical shock of at least −5 percent within the time interval $(t-2, t)$, and zero otherwise. Standard errors are clustered on the country level. Asterisks indicate significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

the estimates for peaceful democratic transitions, which account for roughly two thirds of all democratic transitions in the sample, as the dependent variable. The estimated coefficient for past conflict has a negative sign and is statistically significant in most specifications, indicating that past conflict reduces the likelihood of a peaceful democratization. The point estimates for economic shocks are significant and negative across all specifications, whereas the coefficient for the interaction of shocks with inequality is positive and significant throughout. The results thus confirm once again a non-monotonic effect of economic shocks on the likelihood of democratization conditional on inequality, although the point estimates are in fact slightly larger than for the baseline analysis in Table 2.

Panel B of Table 7 reports the estimates for violent democratic transitions. With this specification, the estimated coefficients for economic shocks and their interaction with inequality become insignificant in the specifications with democratization measured using the simple indices. When using the Democracy–Dictatorship index or the principal components for measuring democratization in conjunction with violent events, the results deliver the same pattern as for the baseline. In particular, the occurrence of a negative shock reduces the likelihood of democratization, whereas the shock interacts with income inequality, delivering a positive overall effect on democratization of shocks when inequality is high. At the same time, the coefficients are considerably smaller than for the baseline, and the explanatory power, as measured by the R^2, is halved compared to the model for peaceful democratic transitions. This evidence suggests that peaceful and violent democratization differ considerably in their determinants. While economic shocks and inequality are important determinants for peaceful democratization, their influence is weaker for violent democratic transitions. This could be due in part to the lower statistical power and in part to structural differences in the democratization process if violent transitions to democracy are related to other mechanisms and contingencies.

However, this does not necessarily mean that the results are incompatible with the strand of models that view democratization as the consequence of an explicit or implicit conflict, as reflected by a revolution constraint. According to models in which the elite extends the franchise for lack of other credible concessions when the revolution constraint binds (as, for example, in the spirit of Acemoglu and Robinson, 2000, 2001, 2005), the transition will, in fact, be peaceful along the equilibrium path if the elite succeeds in avoiding open conflict in exchange for democratization.

Another way to test the relevance of the mechanism underlying this strand of models is to investigate the role of negative income shocks, and their interaction with inequality, on the likelihood of riots and revolts. Table 8 presents the estimation results. Negative income shocks reveal a negative direct effect on the likelihood of riots, which is moderated by

Table 8. *Negative cyclical income shocks, inequality, and riots*

	Dependent variable is the binary indicator for riots between t and $t+3$.			
Democratic quality proxied by	PolityIV index (1)	Political Rights & Civil Liberties (2)	Democracy–Dictatorship (3)	Principal components (4)
Democratic Quality$_t$	−0.14	−0.26*	−0.08	−0.21*
	(0.09)	(0.14)	(0.07)	(0.11)
Shock$_{(t-2,t)}$	−0.76***	−0.75***	−0.79***	−0.84***
	(0.26)	(0.27)	(0.27)	(0.28)
Inequality$_t$	−0.30	−0.02	−0.26	−0.15
	(0.76)	(0.71)	(0.79)	(0.81)
(Shock$_{(t-2,t)}$·Inequality$_t$)	1.68***	1.63***	1.77***	1.84***
	(0.58)	(0.59)	(0.61)	(0.62)
Controls	Yes	Yes	Yes	Yes
Shocks	425	427	421	401
Countries	126	132	132	126
Observations	3,442	3,334	3,407	3,127
R^2	0.06	0.05	0.05	0.04

Notes: All regressions include country-fixed effects, time-fixed effects, and controls for log income per capita and average years of schooling. The dependent variables takes a value of one if there are riots between at least t and $t+3$, and zero otherwise. The shock indicator takes a value of one if there is at least once a negative cyclical shock of at least −5 percent within the time interval $(t-2, t)$, and zero otherwise. Standard errors are clustered on the country level. Asterisks indicate significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

their positive interaction with inequality. In particular, the higher the level of inequality, the more likely it is that a negative economic shock will induce riots. Therefore, the results mirror those for democratization as an outcome; moreover, they are consistent with the predictions of the literature on democratization under the shadow of conflict.

We also replicated the analysis using the dataset provided by Geddes *et al*. (2014) for alternative codings of democratic transitions from autocracy and information about riots as dependent variables. The results in Table A17 in the Online Appendix confirm the earlier results across the different specifications.

The theoretical literature also suggests that demographics are a possible driving force behind political transitions. For instance, according to Fuller (1995, p. 152), economically unequal countries with a relatively large population share of young individuals "who are in demand of land, jobs, higher education, opportunity, and other kinds of resources in society" exhibit a greater likelihood of conflict and struggle for power, and ultimately of a democratic transition. To explore the relevance of this "youth bulge" hypothesis in our context, we replicate the analysis with an extended

specification that also contains the share of individuals aged 15–24 years in the population as a proxy for demographic pressure and its interaction with economic inequality to allow for a non-monotonic effect conditional on the cohesiveness of the society.[21] The results, which are shown in Table A18 in the Online Appendix, deliver estimates for the non-monotonic effect of negative income shocks on the likelihood of democratic transitions that are quantitatively almost identical to those for the baseline specification. The likelihood of democratization decreases with the increasing share of young people. At the same time, the interaction with inequality implies that in more unequal societies with a Gini coefficient of above 0.45, a youth bulge increases the likelihood of democratization. However, the coefficient estimates are not always statistically different from zero. Specifications in which economic shocks and demographic pressure can also interact with each other deliver small and insignificant coefficient estimates, which indicate that economic shocks and demographic pressure seem to affect the likelihood of democratization separately from each other.[22]

IV. Conclusion

In this paper, we have documented novel cross-country panel evidence for a non-monotonic effect of income on democracy. In particular, the evidence suggests that focusing on major income shocks – rather than continuous year-on-year variation in income levels – reveals a significant effect on democratic quality. Moreover, the results reveal an important asymmetry of this effect: negative income shocks exhibit a significantly negative effect on democratic institutions, whereas no comparable countervailing effect is found for positive income shocks. Additionally, negative income shocks reveal an important interaction effect with economic inequality: negative income shocks lead to a deterioration of democratic quality in equal societies, whereas they entail an improvement in democratic quality in unequal societies. No such interaction is found for positive income shocks. This suggests that negative economic shocks might initiate democratic

[21]This categorization follows Fuller (1995) and represents "the conventional cut-off for youth in the literature" (Nordås and Davenport, 2013, p. 932).

[22]Table A19 in the Online Appendix reports results for an empirical model without negative income shocks. These parsimonious specifications produce parameter estimates for demographic pressure and its interaction with inequality that are qualitatively and quantitatively similar to those reported in Table A18 in the Online Appendix. Extended specifications that account for a triple interaction term between negative income shocks, demographic pressure, and inequality also do not deliver significant coefficient estimates, suggesting that both channels operate in isolation; see Table A20 in the Online Appendix. However, inequality and the youth bulge might themselves be related phenomena that are hard to disentangle with the present approach using cross-country panel data. Shedding light on these issues constitutes an interesting avenue for future research.

movements in an environment with high inequality, which is consistent with the theoretical mechanisms for democratization discussed in the literature. By highlighting the role of asymmetric shocks and their interaction with inequality, the results shed new light on the intricate relation between income and democracy and on the seemingly contradictory findings in the literature.

Supporting Information

Additional supporting information may be found online in the Supporting Information section at the end of the article. The replication data and code are available on the Harvard Dataverse at https://doi.org/10.7910/DVN/XF1QC7.

Online Appendix

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