Does democracy reduce corruption?

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ABSTRACT

While democracy is commonly believed to reduce corruption, there are obvious endogeneity problems in measuring the impact of democracy on corruption. This article attempts to address the endogeneity of democracy by exploiting the thesis that democracies seldom go to war against each other. We instrument for democracy using a dummy variable reflecting whether a country has been at war with a democracy in the period 1946–2008, while controlling for the extent to which countries have been at war in general. We find that democracy to a significant extent reduces corruption, and the effect is considerably larger than suggested by estimations not taking endogeneity into account. Democracy may hence be more important in combating corruption than previous studies would suggest.

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Introduction

Does democracy reduce corruption? The answer may not be obvious. Take electoral democracy for instance. On the one hand, competitive elections are likely to reduce corruption as corrupt incumbents may be voted out of office. On the other hand, the need to finance political campaigns may induce politicians to trade political decisions for funding. At a simple descriptive level, there are countries that do not fit into a pattern of more democracy, less corruption. Singapore is frequently mentioned as an example of a relatively undemocratic country where corruption is low. Conversely, democratic countries like Mongolia, Paraguay, or Nicaragua have high levels of corruption. There may of course be other variables that explain corruption levels in these countries. However, econometric studies also find very mixed results on the association between democracy and corruption. Some studies report a significantly negative relation between the two, some a non-linear relation, and others find no significant relation.1

Estimating the causal effect of democracy on corruption is complicated by the fact that democracy is endogenous. Both democracy and corruption are likely to be affected by variables that may be hard to observe or quantify. Moreover, there may be reverse causality; corruption may for instance undermine the confidence of voters in the
democratic system and hence trigger reversals. This means that ordinary least squares (OLS) estimates of the effect of democracy are likely biased, as there is a selection on unobservables problem. As emphasized in the recent study of Pellegata, it is important that “future research should seek appropriate empirical strategies to address [this] problem”.\(^2\) One way to address this challenge is to use an instrumental variable (IV) approach, but as pointed out by Treisman “researchers have not found any convincing instruments for democratic institutions”.\(^3\) This is perhaps not surprising, as democracy is not typically introduced through natural experiments, nor is it easy to find a variable correlated with democracy but not with corruption. Nevertheless, it means that previous studies may have produced biased results on the effect of democracy. Our results suggest that this is indeed the case.

This article attempts to identify the causal impact of democracy on corruption by using an instrument based on the conflict history of countries. Specifically, the instrument for democracy is a dummy variable indicating whether a country has been at war with a democracy in the period 1946–2008. The relevance of the instrument is based on the thesis that democracies seldom go to war against each other, hence we would expect a negative association between being a democracy and having been in conflict with a democracy. While there is a large literature debating this thesis, we show that there is enough of a correlation for the instrument to work. The validity of our instrument is based on the idea that having been involved in conflict with a democracy does not affect corruption, when controlling for whether countries have been involved in conflict in general. In other words, while it is plausible that countries that have a history of conflict with other countries may have higher levels of corruption, it is harder to come up with a reason why having a history of conflict with democracies in particular would be related to corruption levels. Controlling for conflict in general, conflict with democracies should therefore be a valid instrument. Based on this assumption, our estimates capture a causal effect of democracy on corruption.

The instrumental variable regression results show a significantly negative effect of democracy on corruption. In other words, our results suggest that democracy reduces corruption. The estimated effect is larger than comparable estimates not taking endogeneity into account, suggesting that democracy may be more important in combating corruption than previous studies would suggest. The downward bias (in absolute terms) of OLS estimates may be one reason why previous studies have failed to find a robust relation between democracy and corruption. We also look into the question of which countries our estimates are relevant for. If effects of democracy on corruption are heterogeneous, it is possible that our IV estimates capture a local average treatment effect relevant for some types of countries, rather than an average treatment effect across countries. The results indicate that our estimates identify the effect of democracy on corruption in developing countries at higher levels of democracy, and with more mature democracies. In other words, our results indicate a substantive impact on corruption of incremental changes in democracy in relatively poor and somewhat democratic countries.

The article is structured as follows: Section two briefly reviews the theoretical and empirical literature on democracy and corruption, and explains the estimation strategy. Section three presents the data used in the econometric analysis. Section four presents the main results, followed by a discussion of local average treatment effects. Section five concludes.
Background and methodology

A brief review of the literature on democracy and corruption

Corruption is standardly defined as the abuse of public office for private gain or the abuse of entrusted power for private gain. Various measures of corruption levels at the country level exist, from subjective perceptions indices to more objective experiential measures, and the pros and cons of these indices have been extensively explored elsewhere. For pragmatic reasons of country coverage, and in line with most previous empirical studies of corruption, we rely primarily on perceptions indices of corruption in our analysis. So, strictly speaking, we estimate the effect of democracy on corruption perceptions.

The definition of democracy has been extensively debated in political science. Minimalist definitions see democracy as an institutional arrangement where citizens express their preferences through elections. More extensive definitions also add conditions necessary for preferences to be effectively formulated, expressed, and fairly weighted in decisions, including civil liberties such as freedom of expression. This has evolved into a characterization of democracy as various forms of government accountability. Vertical accountability denotes the accountability of government to the people through elections, horizontal accountability refers to checks and balances within government, and societal accountability refers to the existence of a free press, civil society, and so on. While a number of democracy indices exist, we employ the ones most commonly used in previous studies of democracy and corruption. These capture primarily vertical and horizontal accountability, so strictly speaking we estimate the effect of these forms of accountability on perceived corruption.

From a theoretical perspective, there are several reasons why we might expect democracy to reduce corruption. Elections increase the probability that corrupt officials will be exposed and punished, as the opposition has an incentive to uncover corrupt activities by the incumbent, and voters have an interest in not re-electing politicians that favour their own private interests over those of the electorate. Moreover, competitive elections likely drive down the private rents that can be appropriated by officials, since offers of favourable treatment for special interests can be undercut by the opposition. Democracy can also entail a more open system of government, which means that private information on how the system works will become less prevalent, and information rents will go down. Effective checks and balances within government may similarly constrain the ability of officials to deviate from impartial practices. In other words, knowing someone in power becomes less valuable. Furthermore, democracy may affect the normative perceptions of corruption in a society, making corrupt activities less appealing as they carry a greater stigma and possibly also affecting the type of individuals attracted to public office. In sum, democracy may reduce corruption by reducing private benefits of corrupt actions and increasing expected costs.

There are, however, also theoretical arguments to the contrary. Election campaigns require funding, and more competitive elections may make political parties and candidates vulnerable to pressure from funders. As shown in recent work, even a rational and informed median voter may choose to vote for a corrupt government for strategic reasons. In some societies it has been argued that the introduction of democracy has served to reinforce existing patron–client relationships, leading to the democratization of corruption rather than its reduction. The effect of a more
open government is also ambiguous; transparency can make it easier to identify which official to bribe and this effect may dominate a corruption detection effect for small changes in transparency. Moreover, institutions of horizontal accountability are often appointed or funded by the government, which may reduce incentives and capacities to address government corruption. In the worst case, these institutions may be used to persecute political opponents of the government, rather than hold the government accountable. Finally, if normative perceptions or the risk of getting caught in corrupt acts depend on the number of corrupt officials in a society, this means that there may be multiple equilibria with different levels of corruption, and small changes in norms or behaviour brought about by democracy may be insufficient to dislodge a high corruption equilibrium. In total, these arguments imply that democracy may have no effect on corruption, or could in principle increase corruption.

Whether democracy reduces corruption is in the end an empirical question. It is, however, hard to draw any conclusions on the impact of democracy on corruption from existing empirical studies. Treisman generally finds a significantly negative relation between the two, but notes that the result is sensitive to the democracy index used in estimations. We show that our results are not sensitive to the indices employed. An earlier study by Treisman suggests that it is the duration of democracy that matters rather than democracy in itself. Rock finds support for the importance of democracy duration, and shows that the inclusion of this variable makes the democracy index insignificant. In similar terms, Brown, Touchton, and Whitford find an effect of political polarization on corruption, but not of democracy. In the analysis of Paldam, adding income level as a covariate makes democracy insignificant, and Uslaner suggests that it is the degree of inequality in a society rather than democracy that determines corruption. By contrast, we show that our results are robust to the addition of standard controls. Seligson, Tucker, Kostadinova, and Stockemer suggest the opposite direction of causality, from corruption to democracy. Our estimation strategy addresses the challenge of reverse causality. In sum, existing results on the relation between democracy and corruption are mixed.

A main problem that empirical studies should address is the possibility that democracy is endogenous. An analysis of the causal impact of democracy on corruption needs a strategy for addressing the possibility that there are unobservable variables correlated with democracy that affect corruption. Difficulties in finding a valid instrument may explain why previous empirical studies have struggled to come up with such a strategy. This may result in biased estimates, where the direction of the bias is not clear a priori. Unobserved variables that positively or negatively affect both democracy and corruption will entail OLS estimates that are biased upwards, indicating that democracy reduces corruption less than is actually the case. An example of such a variable could be cultural traits of deference to authority, which could make people accepting of authoritarianism but not of corruption. Conversely, unobserved variables that affect democracy and corruption in opposite ways would result in OLS estimates that are biased downwards, suggesting a negative or no effect of democracy where in fact democracy may possibly increase corruption. We address the problem of unobservables by using country conflict history as an instrument for democracy.

The focus of our analysis is on the causal impact of democracy per se on corruption. While there is a large literature discussing how different types or features of democracy affect corruption, we do not address these issues here. Since societies do not randomly
adopt different systems or features of democracy, the endogeneity problem arises again in these forms of analyses. Our instrument does not help us address these distinctions, and they are hence not our focus. For similar reasons, we do not address the question of whether the effect of democracy is conditional on other variables that are also likely to be endogenous. Dong and Torgler find that the impact of democracy is conditional on income distribution and property rights protection, Saha argues that it is conditional on economic liberalization, and Stockemer studies effects on governance in African and Latin-America and in countries at different levels of development. Finally, our approach is not suited for the analysis of non-linear effects of democracy on corruption, as we have only one instrument. Our findings on local average treatment effects do, however, suggest that the negative relation between democracy and corruption predominantly captures effects at the higher end of the democracy scale. Our results are hence consistent with previous studies suggesting that the relationship between democracy and corruption takes the form of an inverted U.

**Estimation strategy**

Our estimation strategy is to use a dummy variable for whether a country has been in conflict with a democracy in the period 1946–2008 as an instrument for its level of democracy in 2008. In other words, we estimate the following equations:

\[
\text{Democracy}_i = \alpha_1 + \beta_{11}\text{Democracy conflict}_i + \beta_{12}\text{Conflict}_i + \gamma_1X_i + \varepsilon_{1i}
\]

\[
\text{Corruption}_i = \alpha_2 + \beta_{21}\text{Democracy(predicted)}_i + \beta_{22}\text{Conflict}_i + \gamma_2X_i + \varepsilon_{2i}
\]

Democracy is first regressed on the instrument Democracy conflict in equation (1), and predicted democracy values are then used to estimate a causal effect $\beta_{21}$ of democracy on Corruption in equation (2). Crucially, we control for whether a country has been in conflict in general in the period 1946–2008, captured by the Conflict variable in the two equations. We also control for a vector of other covariates $X_i$, including income levels of countries. Our identifying assumption is that conditional on the general conflict proclivity of countries (and the other covariates), whether a country has been in conflict with a democracy does not affect its level of corruption. In simple terms, we are assuming that while countries that have been in conflict may have more corruption, one would not expect countries that have been in conflict with democracies in particular to have higher or lower corruption. Controlling for Conflict, we can then use Democracy conflict as an instrument to identify a causal effect of democracy on corruption.

Our argument for using this instrument has two parts; (i) a past history of democracy makes it less likely that a country has been at war with a democracy, and (ii) a history of democracy positively affects democracy today. This generates a negative association between a history of war with democracies and levels of democracy today, which we exploit in our analysis. There is an implicit causal model underlying our estimation strategy. While part (ii) of our argument reflects the idea that institutions are sticky or path-dependent, the basis for part (i) comes from the political science literature discussing the extent to which democracies go to war against each other. The so-called democracy peace thesis goes back at least to Immanuel Kant, who argued that since voters bear the cost of conflict, democracies would be less likely to go to war. This thesis re-emerged in political science in the 1960s, and its theoretical rationale has since been elaborated on. One central argument is that
in institutional constraints make it more difficult for democratic states to go to war, due to checks and balances and the need to mobilize broad political support. This provides a signal of commitment to non-aggression, which makes a pair of democratic countries less likely to attack each other, but does not similarly constrain aggression between other pairs of states. Another argument is that democracies develop norms and a culture of resolving conflict through negotiation and compromise, which similarly also affect their international relations. Democracies may also exhibit stronger beliefs in human rights, which prevents conflict with other democracies, but not with non-democracies. These arguments suggest that while democracies are not necessarily less likely to go to war with other countries, they will less likely be involved in conflict with other democracies.

The proposition that democracies are generally at peace with each other has broad empirical support. This does not mean that the thesis is uncontroversial, and critics often use counter-examples like the conflict between India and Pakistan to demonstrate flaws in the theory. The nuances brought to light by Raknerud and Hegre, that autocratic countries are also less likely to be in conflict with each other, and that democracies have a tendency to join other democracies in fighting autocracies, only strengthen the case for our instrument. Henderson argues that the democracy peace thesis only holds for Western countries. Mansfield and Snyder claim that the democracy peace thesis is accurate only for mature democracies, because well-developed institutional structures in mature democracies keep elites accountable to cost-conscious voters. More immature democracies with weak institutions do not secure this type of accountability. While our central aim is not to test the democracy peace thesis, our results are consistent with it in the sense that there is a negative correlation for the countries in our sample between having been in conflict with a democracy during the years 1946–2008 and the level of democracy today. In addition, our analysis of local average treatment effects elaborates on the question of whether the thesis is more accurate for some countries than for others.

As reflected in equations (1) and (2), the econometric analysis is based on data from a cross-section of countries. For variables other than those related to conflict, data are taken from the year 2008, the latest year for which data on all variables used were available at the time of analysis. Since data on corruption are available from the mid-1990s for both corruption indices used here, and there are also data for most independent variables from this time, it would in principle be possible to use panel data analysis. One way to address the endogeneity of democracy would then be to use country fixed effects to capture all time-invariant differences between countries. However, fixed effects estimation is vulnerable to attenuation bias, in particular where we have persistent regressors, such as democracy. If democracy over time is fairly stable, a lot of what we see in variation over time may be noise due to measurement error, with the result that the estimated association between democracy and corruption becomes small. An alternative would be to use instrumental variable panel data estimators; however, our instrument is not amenable to this form of analysis, as it needs to be measured across a longer time period for its association with democracy to emerge. For these reasons, the main results reported in subsequent sections are based on cross-sectional data. We do, however, also report results from fixed effects estimation. In addition, to test the robustness of using data from a single year such as 2008, we report results using the between estimator, which estimates the relation between democracy and corruption using averages across years.
Data

Table 1 presents the main variables used in our estimations. As measures of our dependent variable, we use two different indices: the control of corruption index calculated by the World Bank, and the corruption perceptions index published by Transparency International. To facilitate comparison across estimations using each of the two indices, and to make results intuitive, we have rescaled the original indices so that they run from 0 to 10, with higher values representing more corruption. An alternative to these perceptions-based indices is the Global Corruption Barometer, which includes questions about corruption experiences. A problem with this index is that the country coverage is much smaller, in our case the sample was cut to 59 countries. The countries covered are typically richer and likely differ in a number of ways from countries not covered, making results hard to generalize without a way of controlling for selection.

As measures of democracy, our main independent variable, we use the Polity IV democracy index and the Freedom House political rights index. Polity IV has the advantage of covering more years, while Freedom House covers more countries. In our case it is an advantage to have an index which covers more years, as it allows us to pick up more cases of relevant conflict, so the main specification uses the Polity IV index. Results using the Freedom House index are, however, useful in discussing robustness, both in terms of differences in what the indices measure and in country coverage. The Polity IV democracy index runs from 0 to 10, with higher values reflecting greater democracy. For our analysis, we have rescaled the Freedom House political

| Variable                  | Explanation                                                                 | Source                                      |
|---------------------------|-----------------------------------------------------------------------------|---------------------------------------------|
| **Dependent variable**    |                                                                             |                                             |
| Corruption WB             | Control of corruption index (World Bank), rescaled (0 – low corruption, 10 – high corruption) | Quality of Government Institute             |
| Corruption TI             | Corruption Perceptions Index, rescaled (0 – low corruption, 10 – high corruption) | Quality of Government Institute             |
| **Independent variables** |                                                                             |                                             |
| Democracy Polity          | Polity IV Democracy Index                                                   | Quality of Government Institute             |
| Democracy FH              | Freedom House political rights index, rescaled (0 – low democracy, 10 – high democracy) | Quality of Government Institute             |
| In GDP/capita             | Log of GDP per capita, PPP adjusted, constant 2005 USD                      | World Development Indicators                |
| Conflict                  | Dummy = 1 if country has been in conflict with another country 1946–2008    | Adapted from UCDP/PRIO Armed Conflict Dataset v 4–2010 |
| Duration democracy        | Polity IV index of regime durability                                         | Quality of Government Institute             |
| Legal origin              | Set of dummies capturing origin of legal system                             | Quality of Government Institute             |
| Colonial origin           | Set of dummies capturing colonizer                                           | Quality of Government Institute             |
| Labour participation rate | Labour participation rate (% of population aged 15+)                         | World Development Indicators                |
| Proportion Catholics      | Proportion Catholics in population (%)                                      | Quality of Government Institute             |
| Instrument Democracy      | Dummy = 1 if country has been in conflict with a democracy 1946–2008        | Adapted from UCDP/PRIO Armed Conflict Dataset v 4–2010, and the Quality of Governance Institute |
rights index to also run from 0 to 10, with higher values representing more democracy to facilitate comparison of results.

Our instrument for democracy is a dummy variable taking the value 1 if a country has been in conflict with an opposing country that at the time of conflict was a democracy and 0 otherwise. This variable was constructed by combining data from the Uppsala Conflict Data Program/International Peace Research Institute Oslo (UCDP/PRIO) armed conflict data set with democracy data from Polity and Freedom House. In specifications where the Polity IV democracy index is included as an independent variable, the democracy conflict instrument indicates countries that have been at war with a country scoring 6 or higher on the Polity IV democracy index. This cut-off is consistent with that used in Polity IV documents, where countries with an index score of 6 or more are counted as democracies. Where the Freedom House political rights index is used as a dependent variable, the democracy conflict instrument captures countries at war with an opponent scoring 6 or higher on the rescaled Freedom House index. Our results are not very sensitive to these cut-off values. Due to different time coverage of the two democracy indices, the instrument captures conflict history for the period 1946–2008 in the case where Polity data are used, and 1972–2008 in the case where Freedom House data are used. The instrument is described in greater detail in Appendix A.

We add as covariates a range of variables that have been previously used in the empirical literature on corruption. The variable most robustly related to corruption across empirical studies is the log of gross domestic product (GDP) per capita. Our main estimations include this variable, consistent with the main specifications of previous studies, adding the dummy variable for whether a country has been in conflict in the period 1946–2008. In further estimations, we add the measure of the duration of democracy, used by among others Rock, to see whether our results are driven by duration rather than level of democracy. This turns out not to be the case, and the relationship between duration and corruption is not robust to the addition of further covariates, but we return to this issue also in the section on local average treatment effects. In additional robustness tests, we add a number of other covariates previously found to matter for corruption. The legal origin of countries is captured by a set of dummies indicating whether the company law or commercial code in a country originates in English common law, French commercial code, socialist/communist law, German commercial code, or Scandinavian commercial code, and the classification is due to La Porta et al. Colonial origin is similarly captured by dummy variables created from the ten-category classification of Teorell and Hadenius. We also include labour participation rates taken from the World Development Indicators and the proportion of Catholics in a country originally from La Porta et al. In initial estimations, we added a number of other covariates that proved insignificant, and were hence dropped in the specifications reported here. These include schooling (average years of education and enrolment rates at different levels), GDP growth, trade (both as total volume to GDP and in specific industries including natural resources), bureaucratic barriers (cost and time to export, documents required to export, cost of business start-up), infrastructure (telephone lines per 1000 people), labour market characteristics (participation rates, unemployment), population size and structure, land area, regime durability, duration of democracy, fractionalization (ethnic, religious, and linguistic), and proportions of other religions (Protestant, Muslim).
Table 2 presents summary statistics for our variables. There are 151 observations in our main sample, which is the sample of countries for which we have observations using the main specification, with the World Bank control of corruption index as dependent variable and the Polity IV democracy variable as the main independent variable. The countries in the main sample are listed in Table A1 in Appendix A.34 As noted above, all corruption and democracy variables have been rescaled into the range 0 to 10, with higher values representing more corruption or greater democracy, respectively. Eighty-seven percent of the countries in our main sample have been in conflict during the period 1946–2008, and 19% have been in conflict with a democracy (when using Polity data to assess the level of democracy in the opposing country).

| Variable           | Obs | Mean | Std. dev. | Min | Max |
|--------------------|-----|------|-----------|-----|-----|
| Corruption WB      | 151 | 5.19 | 1.96      | 0.31| 8.24|
| Corruption TI      | 150 | 6.04 | 2.08      | 0.70| 8.60|
| Democracy Polity   | 151 | 5.81 | 3.80      | 0.00| 10.00|
| Democracy FH       | 174 | 6.04 | 3.45      | 0.00| 10.00|
| ln GDP/capita      | 151 | 8.68 | 1.32      | 5.67| 11.34|
| Conflict           | 151 | 0.87 | 0.33      | 0.00| 1.00|
| Duration democracy | 151 | 27.11| 31.43     | 0.00| 199.00|
| Labour participation rate | 148 | 64.86 | 9.97 | 36.40 | 89.40 |
| Proportion Catholics | 148 | 29.84 | 35.20 | 0.00 | 96.90 |
| Democracy conflict | 151 | 0.19 | 0.39      | 0.00| 1.00|

Notes: Corruption WB is the World Bank control of corruption index rescaled from 0 to 10, with higher values representing more corruption. Corruption TI is the Transparency International corruption perceptions index similarly rescaled. Democracy Polity is the Polity IV democracy index. Democracy FH is the Freedom House political rights index rescaled from 0 to 10, with higher values representing greater democracy. ln GDP/capita is the natural log of gross domestic product per capita, in PPP adjusted 2005 USD. Conflict is a dummy variable indicating whether a country has been in conflict with another country 1946–2008, and democracy conflict a dummy variable indicating whether a country has been in conflict with a democracy in this period. Duration democracy is the Polity index of regime durability. Labour participation rate is the percentage of the population aged 15 or older in the labour force. Proportion Catholics is the percentage of Catholics in the population.

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**Results**

**Main results**

Table 3 reports the results for our main specification using the Polity IV democracy index as the main explanatory variable. Columns one to three report results using the World Bank control of corruption index as dependent variable, while columns four through six report results when the Transparency International corruption perceptions index is used. All specifications reported control for GDP per capita and conflict in general.

The first two columns of Table 3 present results from the two stages of the instrumental variable regression using conflict with a democracy as the instrument. As revealed by the first stage results, the instrument has the expected negative relation to democracy. In other words, the coefficient of the democracy conflict variable in the first column indicates that there is a negative correlation between being a democracy and having been at war with a democracy in the post-World War II period. The variable is significant at the 5% level, and an F-test of whether the variable has a zero coefficient in the democracy equation yields a test statistic of 6.65. This is somewhat below the conventional level of 10 suggested by Stock, Wright, and Yogo in assessing instrument
Table 3. Main regression results using the Polity Democracy index as independent variable.

| Dependent variable | First stage | Second stage | First stage | Second stage | First stage | Second stage | First stage | Second stage |
|-------------------|-------------|--------------|-------------|--------------|-------------|--------------|-------------|--------------|
|                   | IV-regression 1 | OLS 1        | IV-regression 2 | OLS 2        |             |              |             |              |
|                   | Democracy Polity | Corruption WB | Democracy Polity | Corruption WB | Democracy Polity | Corruption TI | Democracy Polity | Corruption TI |
| Democracy Polity  | −0.459**     | −0.156***   | −0.452**     | −0.156***    | −0.459**     | −0.156***    | −0.452**     | −0.156***    |
| In GDP/capita     | 0.973***     | −0.605***   | 0.970***     | −0.715***    | 0.970***     | −0.715***    | 0.970***     | −0.715***    |
|                   | (0.23)       | (0.20)      | (0.23)       | (0.21)       | (0.23)       | (0.21)       | (0.23)       | (0.21)       |
| Conflict          | 1.304        | 0.321       | 1.043        | 0.094        | 1.043        | 0.094        | 1.043        | 0.094        |
|                   | (1.01)       | (0.50)      | (1.02)       | (0.50)       | (1.02)       | (0.50)       | (1.02)       | (0.50)       |
| Democracy conflict| −1.937**     | (0.75)      | −1.937**     | (0.75)       |              |              |              |              |
| Constant          | −3.426       | 12.827***   | 13.877***    | −3.134       | 14.809***    | 15.747***    |              |              |
|                   | (2.09)       | (1.07)      | (2.08)       | (1.04)       | (2.08)       | (1.04)       |              |              |
| R-sq.             | 0.163        | 0.286       | 0.160        | 0.364        | 0.160        | 0.364        | 0.160        | 0.364        |
| N                 | 151          | 151         | 150          | 150          | 150          | 150          | 150          | 150          |

Note: Standard errors in parentheses. *** indicates significance at the 1% level, ** at 5%, * at 10%.
strength, so we will use weak instrument robust inference tests to assess the impact of democracy. Since we only have the one instrument, we cannot test the validity of the instrument using an over-identification test.

Our main result is at the top of column two in Table 3. The effect of democracy on corruption is negative and highly significant (p < 0.018). In other words, the result suggests that democracy reduces corruption. A comparison with corresponding OLS estimates in column three indicates the potential importance of addressing the endogeneity of democracy. The point estimate in the IV regression is almost three times that of the OLS regression. The Anderson-Rubin test used for weak instrument robust inference shows that the instrumental variable estimate is significantly different from zero (p < 0.002) and significantly greater (in absolute terms) than the OLS estimate (p < 0.0274). This is an indication that the approach used in almost all previous empirical work tends to underestimate the effect of democracy. A Durbin-Wu-Hausman test of endogeneity reaffirms this finding, showing that the null hypothesis that democracy is exogenous can be rejected (p < 0.024). The downward (in absolute terms) bias in OLS results may be the reason some previous studies find an insignificant effect of democracy on corruption. In sum, our results suggest that democracy may be more important than previous studies indicate.

Our main result is robust to changes in the corruption index used. Columns four through six report results from corresponding estimations using the Transparency International corruption perceptions index as the dependent variable. Though we lose one observation, results are remarkably similar. Not surprisingly, the coefficient of the instrument in the democracy equation is the same, and the F-test of whether the coefficient is zero yields a statistic of 6.64. The estimated effect of democracy on corruption at the top of the fifth column is only marginally different from the effect estimated using the World Bank corruption index. The OLS estimate is also the same, so the level at which the two are significantly different is very similar (the Anderson-Rubin test in this case reveals that the IV estimate is different from zero with p < 0.003 and different from the OLS estimate with p < 0.036).

For the covariates included in the main specification, results conform roughly to expectations. Income levels measured by the log of GDP per capita has a positive association with levels of democracy and a negative correlation with corruption. The estimates are highly significant and fairly stable across specifications using different corruption and democracy indices. The dummy variable indicating whether a country has been in conflict in the period 1946–2008 has the expected sign in the corruption equation, but is insignificant in both the democracy and corruption equations, across all estimations. Conflict in general hence does not seem to be systematically related to levels of corruption, but our approach does not permit testing for any causal relationship between conflict and corruption.

In the literature on the democracy peace thesis, Mansfield and Snyder argue that the thesis is accurate only for mature democracies. If this is the case, our results may reflect an effect of the instrument on the durability of democracy rather than its level. To rule out this possibility, we added the duration of democracy index as a covariate, the results of which are presented in Table 4. The first two columns correspond to the first and second stage of an instrumental variable regression using our basic specification with the duration variable added. As the results from the first stage show, including duration does not diminish the strength of the instrument in predicting the level of democracy, and the F-test statistic falls only very slightly from 6.65 to
Moreover, results for the democracy variable in the second stage are virtually unchanged when the duration variable is added. While the coefficient of the duration variable suggests that corruption falls with the duration of democracy, the significance of this result is not robust to the addition of further covariates, as shown in column three in Table 4.

We perform a number of further robustness tests; results are reported in Appendix B. Substituting the Freedom House political rights index for the Polity IV index yields a significant and highly similar estimate of the effect of democracy (point estimates $-0.433$ and $-0.406$ using the WB and TI corruption indices, respectively). The instrument becomes somewhat weaker in this case, which likely reflects fewer conflicts with democracies being picked up in the shorter time period covered by the Freedom House index. Our results are robust to the exclusion of any country classified as having been in conflict with a democracy, and so are not driven by misclassification. Adding further covariates does not substantially change our main result. Some previous studies have weighted observations using the inverse of the standard error of the corruption values, the case for doing so is not straightforward, but using these forms of weights does not qualitatively change results in our case. In panel estimations, between estimation yielded results very similar to the cross-sectional ones, so our results are not artefacts of the 2008 data. While fixed effects estimation produced a significantly negative estimate of the effect of democracy, the estimate ($-0.042$) is quantitatively much smaller in absolute terms, which probably reflects attenuation bias.

**Local average treatment effects**

Our instrument does not permit us to test for non-linear effects of democracy on corruption. Nor can we test for heterogeneous effects more generally, since we have only one instrument. However, if there are heterogeneous effects of democracy on

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**Table 4.** Regression results including duration of democracy as a covariate.

| Dependent variable | IV-regression 1 | | | IV-regression 2 | | |
|-------------------|----------------|---------|---------|----------------|---------|
| Democracy Polity  | Democracy Polity | −0.444** | (0.18) | −0.518** | (0.24) |
| ln GDP/capita     | 0.816*** | (0.28) | −0.414** | (0.18) | −0.886*** | (0.16) |
| Conflict          | 1.207 | (1.00) | 0.427 | (0.50) | 0.321 | (0.56) |
| Duration democracy| 0.012 | (0.01) | −0.016*** | (0.01) | −0.010 | (0.01) |
| Labour participation rate | | | | −0.048** | (0.02) |
| Proportion catholics | | | | 0.016 | (0.01) |
| Democracy conflict | −1.915** | (0.76) |
| Constant          | −2.310 | (2.38) | 11.421*** | (1.07) | 17.878*** | (1.86) |
| Colonial dummies  | No | | No | Yes |
| R-sq.             | 0.170 | 0.362 | 0.470 |
| N                 | 151 | 151 | 148 |

Note: Standard errors in parentheses. *** indicates significance at the 1% level, ** at 5%, * at 10%.
corruption across countries, our estimate may capture a local average treatment effect for the countries for which there is a strong association between levels of democracy and having been in conflict with a democracy, rather than an average treatment effect across all countries. Characterizing the countries for which our instrument has a stronger effect allows us to shed light on three issues: (i) whether the effect identified is relevant mainly for developed countries, as implicitly suggested by Henderson; (ii) whether the effect identified is relevant mainly for mature democracies, consistent with the arguments of Mansfield and Snyder that the democracy peace hypothesis holds mainly for mature democracies; and (iii) whether our results are consistent with an inverted U relation between democracy and corruption as suggested by Mohtadi and Roe.

To address the first question, we split the sample down the middle according to income levels, and separately ran the first stage of the instrumental variable regression for below median income countries and above median income countries. The results (not reported) show that the coefficient of the instrument is markedly greater (−2.4) for the below median income countries than for the above median income countries (−1.52). Our instrument therefore seems more closely related to levels of democracy in low income countries than in high income countries. Or, put differently, poor countries are overrepresented among the countries where there is an association between conflict with a democracy and being a democracy. In other words, if there are heterogeneous effects of democracy on corruption, our results capture the effect of democracy in poorer countries.

For the second question, we used a similar approach and split the sample in two according to the duration of democracy. Results from the first stage of instrumental variable regressions performed separately on countries above and below the median in duration, which is 17 years of duration. The results (not reported) show that the coefficient of the instrument for the at and above median sample (−2.51) is greater in absolute terms than the coefficient for the below median sample (−1.31). In other words, the estimated effect of democracy on corruption to a larger extent captures effects for countries with more mature regimes than more recent ones, consistent with the suggestion by Mansfield and Snyder that the democracy peace thesis holds for mature democracies.

The third question is about possible variable treatment intensity across the multiple values of the democracy index. In other words, the unit causal response of going from one to two on the democracy index may be different from the unit causal response of going from two to three. Our instrumental variable estimates in this case capture a weighted average of these unit causal responses, where the weights reflect the extent to which the instrument is closely related to democracy for countries at different levels of democracy. To see where on the democracy scale our instrument creates the most action and hence which returns to democracy our results are picking up, we apply an approach that compares the cumulative density functions (CDF) of the endogenous variable with the instrument switched on and off. The solid line in Figure 1 represents this difference for different values of the Polity IV democracy index and shows where the instrument has the greatest effect on predicted democracy levels. As the figure shows, the instrument does the most work at higher levels of democracy, specifically in the range of seven to eight on the democracy index. Our estimates, therefore, predominantly capture the returns from democracy in terms of reduced corruption at high levels of democracy. If returns are heterogeneous, our results hence do not rule out an inverted U shaped relationship between democracy and corruption.
In sum, this means that if there are heterogeneous effects of democracy on corruption, our estimates capture effects for poor countries at higher levels of the democracy scale and for more mature democracies. In other words, our estimates suggest a substantive impact on corruption of incremental changes in democracy in countries such as Honduras or Sri Lanka (whose scores on the Polity IV democracy index were seven in 2008). In the presence of heterogeneous effects, our estimates tell us little about the effect of incremental changes in democracy in highly undemocratic countries, such as Sudan (which scores zero on the democracy index in recent years), Guinea (score one), or Angola (score two). This does not imply that the impact of democracy on corruption is necessarily smaller in these countries, it just means that the impact is not identified through our particular instrument. Similarly, our estimate may not capture the effect of democracy on corruption in developed countries or in countries with less mature democracies.

Concluding remarks

This article attempts to estimate the causal impact of democracy on corruption, using an instrument reflecting whether countries have been in conflict with a democracy in recent history. The results suggest that democracy may be more effective in reducing corruption than indicated by estimates not taking the endogeneity of democracy into account. Whether there is heterogeneity in impacts of democracy on corruption is a question our analysis does not resolve. Nevertheless, our results suggest that there may be a substantial effect of improving democracy in developing countries, where the problem of corruption is the most prevalent. While the indices of corruption employed capture perceived rather than actual corruption levels, this reflects limitations in data availability, not in analytical approach. The empirical approach used would be applicable to analyses using other corruption indices, should these attain wider country coverage.

From a policy point of view, this means that developing democratic institutions should be part of strategies to reduce corruption. While previous results have been ambiguous on this issue, our analysis suggests that this may be due to selection bias. In terms of policy, there are of course a number of more detailed issues that need to
be addressed. Importantly, the effect of democracy on corruption likely varies across forms of democracy. Any analysis of the effectiveness of different forms of democracy runs into the same type of methodological problem described here; the form of democracy adopted by countries is likely endogenous. Estimating causal effects hence requires the use of an empirical strategy which addresses this challenge. However, as our instrument does not help us distinguish between different forms of democracy, this is a matter for further research.

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Notes

1. For reviews of earlier studies see Pellegata, “Constraining Political Corruption”; Rock, “Corruption and Democracy”; and Lambsdorff, “Consequences and Causes of Corruption.”
2. Pellegata, “Constraining Political Corruption,” 1211.
3. Treisman, “What Have We Learned,” 236.
4. The former definition is advanced by the World Bank, the latter by Transparency International, see for example Shaxson, “Oil, Corruption and the Resource Curse.”
5. Svensson, “Eight Questions About Corruption”; Treisman, “What Have We Learned.”
6. Schumpeter, Capitalism, Socialism, and Democracy.
7. Dahl, Polyarchy.
8. Myerson, “Effectiveness of Electoral Systems”; Ades and Tella, “Rents, Competition and Corruption.”
9. Hollyer, Rosendorff, and Vreeland, “Democracy and Transparency.”
10. Rose-Ackerman, Corruption and Government.
11. Pani, “Hold Your Nose and Vote.”
12. Bac, “Corruption, Connections and Transparency.”
13. Andvig and Moene, “How Corruption May Corrupt.”
14. Treisman, “What Have We Learned.”
15. Treisman, “The Causes of Corruption.”
16. Rock, “Corruption and Democracy.”
17. Brown, Touchton, and Whitford, “Political Polarization as a Constraint.”
18. Paldam, “The Cross-country Pattern of Corruption”; Uslaner, Corruption, Inequality, and the Rule of Law.
19. Seligson, “The Measurement and Impact”; Tucker, “Enough!”; Kostadinova, “Abstain or Rebel”; Stockemer, LaMontagne, and Scruggs, “Bribes and Ballots.”
20. Brunetti and Weder, “A Free Press”; Persson and Tabellini, “Economic Effects of Constitutions”; Persson, Tabellini, and Trebbi, “Electoral Rules and Corruption”; Kunicova, “Democratic Institutions and Corruption”; Treisman, “What Have We Learned”; Lessmann and Markwardt, “One Size Fits All?”; Kolstad and Wiig, “Is Transparency the Key”; Saha, Gounder, and Campbell, “Democracy and Corruption.”
21. Dong and Torgle, *Democracy, Property Rights*; Saha, Gounder, and Su, “The Interaction Effect”; Stockemer, “Does Democracy Lead to Good Governance?”; Stockemer, “Regime Type and Good Governance.”

22. Pellegrata, “Constraining Political Corruption”; Mohtadi and Roe, “Democracy, Rent Seeking.”

23. Kant, *Perpetual Peace*.

24. Doyle, “Kant, Liberal Legacies”; Russett, *Grasping the Democratic Peace*; Russett and Antholis, “Do Democracies Fight Each Other?”

25. Tomz and Weeks, “Public Opinion.”

26. Raknerud and Hegre, “The Hazard of War.”

27. Henderson, “Disturbing the Peace.”

28. Mansfield and Snyder, *Why Emerging Democracies Go to War*.

29. Marshall and Cole, *Global Report 2009*.

30. Svensson, “Eight Questions About Corruption”; Treisman, “What Have We Learned.”

31. Rock, “Corruption and Democracy.”

32. La Porta et al., “The Quality of Government.”

33. Teorell and Hadenius, *Determinants of Democratization*.

34. The sample is cut to 150 observations if we instead use the Transparency International corruption perceptions index as the dependent variable. If we use the Freedom House political rights index as the main explanatory variable, samples increase to 174 observations (with the World Bank corruption index as dependent variable) and 169 observations (with the TI corruption index as dependent variable). Adding labour participation rates and proportion of Catholics in the population reduces the main sample from 151 to 148 countries.

35. Stock, Wright, and Yogo, “A Survey of Weak Instruments.”

36. In further estimations, we used the approach of Lewbel, “Using Heteroscedasticity to Identify,” to generate further internal instruments, and the exogeneity of our instrument was not rejected through a Sargan overidentification test ($p > 0.838$).

37. Mansfield and Snyder, *Why Emerging Democracies Go to War*.

38. Henderson, “Disturbing the Peace.”

39. Mansfield and Snyder, *Why Emerging Democracies Go to War*.

40. Mohtadi and Roe, “Democracy, Rent Seeking.”

41. Mansfield and Snyder, *Why Emerging Democracies Go to War*.

42. Acemoglu and Angrist, “How Large Are the Social Returns to Education?”

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