# Make Two Democracies and Call Me in the Morning: Endogenous Regime Type and the Democratic Peace *

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**Abstract**

The "democratic peace" — the "law like" observation that democracies are less likely to fight each other — has precipitated extensive inquiry and debate about its causes. Yet, liberal government is a relatively new or infrequent phenomenon in world affairs. Democracy must itself be generated by other variables, some of which appear likely to influence international conflict. There is thus a problem of endogeneity if peace and democracy share common predecessors. Estimating the impact of democracy on peace is especially difficult if some of the causes shared by both variables are unobservable. We seek to address possible endogeneity between regime type and conflict with an instrumental variables approach. Utilizing average national fertility rate as an instrument, we find that joint democracy is not directly pacifying. To the contrary, democratic countries appear more likely to attack other democracies. Our findings invite a reconsideration of theories linking dyadic democracy with interstate peace.

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The democratic peace — the observation that democracies are less likely to fight each other than are other pairings of states — is one of the most widely acknowledged empirical regularities in international relations. Prominent scholars have even characterized the relationship as an empirical law (Levy 1988; Gleditsch 1992). The discovery of a special peace in liberal dyads stimulated enormous scholarly debate and led to, or reinforced, a number of policy initiatives by various governments and international organizations. Although a broad consensus has emerged among researchers regarding the empirical correlation between joint democracy and peace, disagreement remains as to its logical foundations. Numerous theories have been proposed to account for how democracy produces peace, if only dyadically (e.g., Russett 1993; Rummel 1996; Doyle 1997; Schultz 2001).

At the same time, peace appears likely to foster or maintain democracy (Thompson 1996; James, Solberg, and Wolfson 1999). A vast swath of research in political science and economics proposes explanations for the origins of liberal government involving variables such as economic development (Lipset 1959; Burkhart and Lewis-Beck 1994; Przeworski et al. 2000; Acemoglu and Robinson 2006; Epstein et al. 2006) and inequality (Boix 2003), political interests (Downs 1957; Bueno de Mesquita et al. 2003), power hierarchies (Moore 1966; Lake 2009), third party inducements (Pevehouse 2005) or impositions (Peceny 1995; Meernik 1996), geography (Gleditsch 2002b), and natural resource endowments (Ross 2001), to list just a few examples. Each of these putative causes of democracy is also associated with various explanations for international conflict. Indeed, some as yet poorly defined set of canonical factors may contribute both to democracy and to peace, making it look as if the two variables are directly related, even if possibly they are not.

We seek to contribute to this literature, not by proposing yet another theory to explain how democracy vanquishes war, but by estimating the causal effect of joint democracy on the probability of militarized disputes using a quasi-experimental research design. We

begin by noting that some of the common causes of democracy and peace may be unobservable, generating an endogenous relationship between the two. Theories of democracy and explanations for peace are at a formative state; it is not possible to utilize detailed, validated and widely accepted models of each of these processes to assess their interaction. Indeed, to a remarkable degree democracy and peace each remain poorly understood and weakly accounted for empirically, despite their central roles in international politics.

We address the risk of spurious correlation by applying an instrumental variables approach. Having taken into account possible endogeneity between democracy and peace, we find that joint democracy does not have an independent pacifying effect on interstate conflict. Instead, our findings show that democratic countries are more likely to attack other democracies than are non-democracies. Our results call into question the large body of theory that has been proposed to account for the apparent pacifism of democratic dyads.

This article proceeds as follows. In the next section we briefly review previous attempts to estimate the effect of democracy on conflict and point out methodological issues that have plagued these previous efforts. We then introduce our research design and data. After presenting the main findings, we provide further support for our results with a series of falsification tests to assess the plausibility of the assumptions behind our empirical strategy. The final section concludes with recommendations for future research.

## The Democratic Peace Debate

Few scholars dispute the existence of a correlation between joint democracy and peace. However, vigorous debate persists over how to interpret this relationship. There are three broad clusters of arguments proposed to account for the empirical regularity. By far the largest cluster of explanations posit that democracy causes peace. Researchers have identified various aspects of democracy as causal mechanisms, including shared norms of peaceful dispute resolution (Maoz and Russett 1993; Dixon 1994; Flynn and Farrell 1999),

democratic institutions that constrain leaders' war-making abilities (Bueno de Mesquita et al. 1999), and more credible communication due to increased transparency (Schultz 1998; Choi and James 2007; Colaresi 2014) or audience costs (Fearon 1994; Schultz 1999).

The second cluster of explanations posit that the causal arrow actually runs in the other direction, that peace causes democracy. Researchers argue that democratization is more likely to occur in a peaceful (or stable) international environment. External threats require states to allocate their resources to defense and lead to a centralization of government power. Within this intellectual community, some scholars are ecumenical, suggesting that the causal arrow can run in both directions — peace causes democracy but democracy also causes peace (Gleditsch 2002b; Rasler and Thompson 2005; Reuveny and Li 2003; Thompson 1996). Others are more emphatic that peace causes democracy but that democracy does not cause peace (James, Solberg, and Wolfson 1999; Gibler and Tir 2014).

A third, much smaller cluster of explanations posit that a confounding variable may be responsible for both democracy and peace, thus generating a spurious correlation between democracy and peace that is the basis for the analysis here. Scholars have identified various factors such as Cold War alliances (Farber and Gowa 1997; Gowa 1999), common national interests (Gartzke 1998, 2000), market based "capitalist" economics (Gartzke 2007; Gartzke and Hewitt 2010),[1] so called "contractualist" norms associated with market based economic systems (Mousseau 2013, 2018),[2] stable territorial borders (Gibler 2007, 2012),[3] as well as other possible confounding factors that might explain away the democratic peace.

Each of these three clusters of explanations claims to find empirical support for their respective arguments. Given that these studies have typically relied on similar (or often identical) data, one might wonder why different researchers have drawn seemingly in-

[1] See also Choi (2011) and Dafoe (2011).

[2] See also Dafoe, Oneal, and Russett (2013).

[3] See also Park and Colaresi (2014) and Gibler (2014).


(a) Confounding $Z$ (b) Post-treatment $Z$

Figure 1: This figure illustrates two commonly presumed scenarios involving democracy, peace, and a third variable, $Z$. In scenario (a), $Z$ influences both democracy and peace. In scenario (b), democracy influences $Z$, which in turn influences peace.

compatible conclusions from a largely common set of observations. In our view, there are two methodological dilemmas at the heart of observational studies on the democratic peace that have allowed factions in the research community to draw contradictory inferences.

The first dilemma arises from the fact that researchers often disagree over the role of third variables in the democracy–peace nexus. Consider a given hypothetical determinant of peace, $Z$. If one believes that $Z$ also causes democracy, then $Z$ is a potential confounding variable, corresponding to the diagram (a) on the lefthand side of Figure 1. As such, we must control for $Z$ in assessing the relationship between democracy and peace because a failure to control for $Z$ would result in an endogeneity (omitted variable) bias.

If, on the other hand, one believes $Z$ is caused by democracy, then $Z$ is a post-treatment variable, corresponding to the diagram (b) on the righthand side of Figure 1. As such, we must *not* control for $Z$ in assessing the relationship between democracy and peace because mistakenly controlling for $Z$ would result in a post-treatment variable bias. Depending on the direction of the causal arrow between democracy and $Z$, the correct treatment of $Z$ will thus be different. This poses a dilemma – if we control for a particular $Z$, we reduce the risk of endogeneity bias, but this comes with the cost of increased risk of post-treatment variable bias. If, on the other hand, we omit $Z$, we reduce the risk of post-treatment variable bias, but this comes with the cost of an increased risk of endogeneity bias.

Suppose that we find statistical evidence for a bivariate association between democracy

and peace, but that this statistical association disappears when we control for the third variable, $Z$. If we believe that this $Z$ is a confounding variable, we must conclude that the democracy–peace relationship is spurious. If, however, we believe that $Z$ is a post-treatment variable, we can still conclude that democracy causes peace through $Z$.

Consider the debate over the role of states' preference similarity in assessing the democratic peace. One of the major disagreements between the two sides of the debate is about whether preference similarity is a confounding variable (preference similarity leads to regime similarity) (Gartzke 1998, 2000) or a post-treatment variable (regime similarity leads to preference similarity) (Oneal and Russett 1999). If one subscribes to the former view, then evidence that the statistical association between democracy and peace disappears when controlling for state preference similarity should lead one to conclude that the statistical association between democracy and peace is spurious. If instead one subscribes to the latter view — that democracy causes both peace and similar state preferences — then the very same evidence allows one to still conclude that democracy causes peace.

A second dilemma involves "unknown unknowns." No matter how many potential confounding variables researchers control for, there is always a risk that some unobserved or unobservable determinant of democracy and peace remains omitted from the analysis, generating endogeneity bias. Researchers have employed fixed-effects models to control for unit-specific, time-invariant unobserved confounders (Green, Kim, and Yoon 2001; Schultz 2001), but such methods will not eliminate endogeneity caused by unobserved confounders that vary over time. Fixed-effects models also tend to mask the impact of slow-changing variables such as regime type (Beck and Katz 2001).

## Looking for Causality

In face of these dilemmas, experimental approaches — randomized controlled trials, in particular — are the most powerful methods to estimate causality. Randomization of treatment, if possible, would allow us to eliminate any bias that may arise from unobservable

and observable confounding variables. That being said, a major limitation of experimental methods is that treatment randomization is not always possible due to feasibility and ethics considerations. An analyst typically cannot treat actual countries as experimental subjects, randomly assigning regime type or some other political or economic causal variable to them, and observe war and peace as the outcome variable.

Experimental studies of the democratic peace have thus focused on testing components of theories that are most amenable to experimental manipulation, disaggregating the causal chain between regime type and peace into their most tractable pieces. In particular, researchers have conducted survey experiments to explore the role of public opinion in the causal mechanisms (Mintz and Geva 1993; Johns and Davies 2012; Tomz and Weeks 2013). These studies seek to identify the causal effect of regime type on public support for military actions, which could, in turn, influence war and peace. By randomly assigning the regime type of the target country in a hypothetical military action in different experimental scenarios, they find that democratic publics tend to oppose military actions against democracies more than against non-democracies. [4]

Although findings of this type contribute in important ways to our understanding of the democratic peace, their focus on popular political attitudes also ensures a critical levels-of-analysis deficiency. Studies of public opinion offer only indirect tests of national action, lacking a description, or assessment, of agency. It is hard to imagine, for example, that public preferences in democracies and non-democracies translate into government decision making in an equivalent manner, even if attitudes do not differ across regimes.

[4] Bell and Quek (2018) replicate Tomz and Weeks's (2013) survey experiment in a non-democracy (China), finding that the public in at least one important autocracy also tends to oppose military actions against democracies more than against non-democracies. If both non-democratic and democratic publics oppose fighting with democracies, democracies should exhibit a lower probability of conflict with all regime types, something we do not observe. See also Suong, Desposato, and Gartzke (2020).

As such, survey experiments of public attitudes have a difficult time directly evaluating the causal relationships most closely associated with foreign policy behavior. Officials may follow public opinion or attempt to lead it, demonstrating the utility of actions that at first glance appear unpopular. Similarly, government decision making is often the result of processes that aggregate, redirect or even undermine popular preferences. Politics involves coalition building, log-rolls and horse-trading, tactics that seldom result in a transparent relationship between popular attitudes and foreign policy outcomes.

The link between public attitudes and the observed behavior of countries also tends to be complicated by strategic interaction. For example, if publics generally prefer to avoid fighting with democracies, this may embolden democracies in crisis bargaining, which may in turn lead to an increase in the probability of conflict for pairs of democracies. [5] Public attitudes that are understood abroad may fail to generate friction and war, precisely because they are either addressed or avoided by other powers (Leeds and Davis 1997).

One promising way to overcome shortcomings of both observational and experimental studies would be to combine the strengths of each approach. Specifically, we propose to use a quasi-experimental design — in particular, an instrumental variables (IV) framework — to identify the causal relationship between democracy and peace, relying on commonly used observational data. Such an approach allows us to study the overall relationship between regime type and war (rather than intermediate outcomes such as public support for war) and to identify the causal effect of each variable on the other in a credible manner.

Before we review our approach in detail, it may be useful to explain why this type of analysis has not been pursued successfully in the past and what makes our effort different from other, broadly related projects. We are not the first to apply an IV framework (more

[5] Bueno de Mesquita et al. (2003) make an analogous claim, arguing that democracies are less likely to fight each other because they fight harder, which increases the cost of war in jointly democratic contests.

specifically) or multi-equation models (more broadly) to the democratic peace. However, previous attempts suffer from two major problems. First, previous studies have typically used a dyad (country pair) as the unit of observation in analyzing conflict, which requires some summary measure(s) of democracy for a pair of countries rather than the state-level (monadic) democracy measure. [6] Use of a dyadic aggregate to represent regime type creates a discrepancy between the first stage regression (predicting democracy at the country level) and the outcome stage regression (predicting conflict at the dyad level). [7] We avoid this problem by using the *directed* dyad as the unit of observation in predicting conflict, distinguishing between the potential challenger and target in a dispute. This allows us to connect the first stage equations (predicting the challenger's and target's regime types) and the outcome stage equation seamlessly. Doing so has several benefits: the outcome stage model could directly include country-level covariates (such as challenger's and tar-

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[6] The most commonly implemented way to summarize democracy values for the two countries in a dyad is to take the smaller of the two values that measure the degree of democracy, often coupled with the larger of the two values.

[7] Reuveny and Li (2003) and McDonald (2015) each use dyad (-year) as the unit of analysis in their multi-equation models of democratic peace, with dyadic conflict as the outcome variable and the lower democracy score as an endogenous independent variable (Reuveny and Li (2003) also use higher democracy). The first stage regression relies on a country-year unit of analysis to predict democracy scores. These authors then calculate the lower (and higher) predicted democracy scores for each dyad-year to utilize as predictors in the outcome stage probit regression predicting dyadic conflict. Such an approach has at least two problems. First, this two-step, plug-in method is an example of "forbidden regressions" (see footnote 8 below). Second, it is unclear whether or how the first-stage predictors (country-level democracy covariates) should be included in the dyadic-level outcome stage model, or how to interpret these results.

get's democracy) without having to convert them to a dyadic summary. This also allows us to estimate the system of equations jointly rather than relying on the "forbidden regression." [8]

Second, a more daunting challenge in applying an IV approach to democratic peace research is the difficulty of finding a plausible instrument for regime type — a variable that is strongly correlated with regime type but is unrelated to war. This is the challenge that has plagued empirical researchers in many fields. For example, a recent study of the effect of regime type on economic growth uses a diffusion-based measure of democracy (i.e., average value of democracies in a given region) as an instrument for democracy (Acemoglu et al. 2019). However, diffusion-based instruments such as this are unlikely to be a valid instrument, due to spatial spill-over, interdependence, and, most importantly, simultaneity (Betz, Cook, and Hollenbach 2018). Recognizing problems with spatial instruments, McDonald (2015) seeks to exploit the very discrepancy between country-level and dyad-level designs as the source of identification. His discussion, however, lacks a clear explanation as to why some determinants of regime type do not influence conflict. [9]

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[8] "Forbidden regression" refers to an approach to plug in the fitted values from the first-stage model in place of the endogenous covariate in a non-linear outcome-stage model. Such an approach works only when both stages are a linear model (Angrist and Pischke 2009).

[9] McDonald's (2015) IV model (presented in Table 6 of the online Supplementary Appendix) requires an instrument that strongly influences regime type but is unrelated to conflict. However, he claims that his instruments (e.g., great power alliance of one side of a dyad) are a strong determinant of conflict but are unrelated to the regime type of the other side in a dyad (pages 4–5 of Supplementary Appendix). In other words, he claims his instruments have no relevance (uncorrelated with the endogenous covariate) or exogeneity (correlated with the outcome), whereas he would need to claim the opposite.

We turn to a demographic variable — average female fertility rate in a given country — as a source of variation in regime type that is exogenous to international conflict. As we will argue below, a lower fertility rate is a strong driver of democratization. We will also present theoretical arguments and a series of falsification tests that support the claim that average national fertility rate does not directly influence international conflict.

## Research Design

In this study, we examine the relationship between democracy and conflict for the period between 1960 and 2010. [10] Our sample includes all contiguous directed dyad years identified by the Correlates of War (COW) data. A directed dyad is a country pair comprised of a potential challenger and a potential target. Distinguishing between the challenger and the target allows us to study how the regime type of each side influences the risks of militarized conflict initiation and targeting. We focus on contiguous country pairs (i.e., countries either share a land border or are separated by less than 400 miles of water [11]) to reduce the heterogeneity in the sample and make each observation unit more comparable. [12] The vast majority of non-contiguous country pairs do not interact with each other

[10] We chose this observation period due to data availability. The fertility rate variable is widely available only since 1960. Standard measures of conflict exist only up to 2010. This time period also represents a critical test for the democratic peace; few advocates emphasize the importance of earlier periods of democratization, during which only a relatively small number of nations became democratic.

[11] Based on version 3.2 of the Correlates of War (COW) Direct Contiguity data set (Stinnett et al. 2002).

[12] Reed and Chiba (2010) report considerable heterogeneity between contiguous dyads and non-contiguous dyads, not only in terms of covariate characteristics but also in terms of how these dyads respond to changes in covariate values. More specifically,

on a regular basis and thus have no opportunities to experience militarized conflict. Excluding such dyads from our analysis should thus enhance the internal validity of our study, although it may come at a cost of somewhat diminished external validity. [13]

We identify whether the potential challenger in a dyad initiates a militarized conflict against the target in a given year, based on COW's Militarized Interstate Dispute (MID) dataset. We use a new version of the MID data compiled by Gibler, Miller, and Little (2016) that introduces a number of corrections to version 4.01 of the MIDs. [14] To reduce heterogeneity in these data, we omit "protest-dependent" MIDs that are based on a substantially different coding logic (Gibler and Little 2017). [15] Our outcome variable, conflict initiation, is coded as 1 for a directed-dyad-year if the challenger initiates a new MID against the potential target, 0 otherwise, and missing if an MID is already ongoing (McGrath 2015).

We code regime type using the combined democracy-autocracy scale (polity2 variable) from the Polity5 Project (Marshall and Gurr 2020). [16] Following convention, we

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the study finds that the effect of regime type on conflict differs between contiguous and non-contiguous dyads.

[13] We will revisit the issue of external validity later in the paper.

[14] We use version 2.1.1 of the Gibler-Miller-Little MID data (Gibler, Miller, and Little 2020), available at http://svmiller.com/gml-mid-data/ (last accessed in March 2020). See also Palmer et al. (2020).

[15] An alternative approach to reduce heterogeneity of MIDs is to focus on those MIDs that involve fatalities (Hegre 2000). However, such escalation-based selection is problematic, as it prevents us from understanding the causes of disputes that were de-escalated before fatalities were incurred.

[16] These data are available at http://www.systemicpeace.org/polityproject.html (Last accessed in July, 2020).

code a country as a democracy if it receives a polity2 score of 6 or higher. [17] To assess the effects of regime type on conflict, we regress our dependent variable, MID initiation, on the challenger's regime type, target's regime type, and an interaction term (Joint Democracy). The interaction term allows us to capture the dyadic nature of the democratic peace — democracies are expected to be more peaceful largely only toward other democracies.

In analyzing the relationship between regime type and conflict, we acknowledge that some unobservable variables can influence these variables jointly, causing an endogeneity bias. To facilitate causal inference in the presence of this endogeneity, we employ a trivariate model that jointly estimates three equations for three endogenous binary variables (conflict, challenger's regime type, and target's regime type) as well as the correlations among the three error terms. The model is an application of a copula-based joint modeling framework for causal inference proposed by Braumoeller et al. (2018). [18] Our approach can thus be understood as an implementation of instrumental variables (IV) regression. [19]

In estimating the conflict equation of the model, we control for factors that could influence the probability that a challenger initiates conflict against a target. [20] We expect that a

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[17] We check the robustness of our results by using alternative thresholds (5 and 7) to code democracy, and find the results are unchanged. See the Appendix for the results using different thresholds.

[18] A detailed description of the model and Monte Carlo simulation results appear in the Online Appendix.

[19] As such, the causal effect we identify is a *local* average treatment effect (LATE), rather than an average treatment effect (ATE). Although LATE is not generally equal to ATE, this is the best we can hope for when we cannot perform treatment randomization. We will revisit this issue later in the analysis.

[20] To reduce the danger of post-treatment variable bias, we opted not to include some of the well-known correlates of MIDs that may be influenced by regime type. For example, foreign policy similarity and dyadic trade may both be influenced by regime

challenger is more likely to initiate a militarized conflict when it is more likely to prevail over its adversary in military battle. To capture this expectation, we include the challenger's military advantage operationalized as the challenger's military capability score (COW's CINC score) divided by the sum of CINC scores for challenger and target.

Economically developed countries have additional means to compel other states, or to deter aggression. Economic development should thus influence the likelihood that a country experiences a militarized conflict either as a challenger or a target. [21] We thus control for both challenger's and target's economic development measured by per capita GDP. [22] Finally, to control for the duration dependence of military conflict, we include the natural log of the number of years since a state was last involved in a militarized dispute. [23]

## Identification: fertility rate as an instrument

To identify the causal effect of regime type on conflict using the IV framework, we need an instrument (instrumental variable) that is both *relevant* (i.e., strongly correlated with democracy) and *exogenous* (i.e., unrelated to conflict except through its effect that goes through democracy). In other words, the two equations explaining democracy for challenger and target must include a significant determinant of democracy that does not belong in the equation explaining conflict. Finding an instrument that is both relevant and exogenous is extremely difficult in empirical research. Many well-known correlates of democracy, such as economic development, do not satisfy the exogeneity condition as we

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type and are thus excluded from the analysis here.

[21] As regime type may influence economic development, which would result in post-treatment bias, we also estimate a model that excludes economic development from all equations as a robustness check.

[22] Data on Gross Domestic Product (GDP) and population are from Gleditsch (2002a).

[23] We chose the natural log of time over other smooth functions (i.e., cubic polynomial, square root, and penalized thin plate regression splines) based on model fit statistics.

could think of plausible theoretical mechanisms linking them with conflict behavior.

We argue that fertility rate (the number of children, on average, that a woman is expected to give birth to over her lifetime) in a country serves as a valid instrumental variable in estimating the effect of democracy on conflict. We start by presenting a theoretical argument and empirical evidence as to the relevance of this instrument. After presenting our argument for instrument exogeneity, we detail the main empirical results. We then return to the issue of instrument exogeneity to provide additional discussion and testing.

**Instrument relevance**

There are at least two ways that a lower fertility rate contributes to the onset and stabilization of democratic government (Sommer 2018; Wilson and Dyson 2017). First, a decline in the fertility rate promotes the economic and political empowerment of women, which in turn leads to improved social gender equality (Inglehart and Norris 2003; Iversen and Rosenbluth 2010). As women are freed up from the heavy burdens of childbirth and childcare, they can invest more time and resources in education and salaried employment. Women with higher levels of education and stable employment have better access to information and resources necessary to participate in politics. With women becoming politically active, the descriptive and substantive political representation of women also improves. Gender equality in politics (i.e., better representation of female rights and better female political participation) is an essential component of democracy.

Second, a lower fertility rate leads to greater education spending per capita both at the macro and micro levels, which in turn supports democracy. At the macro level, having fewer children in a society for a fixed level of government spending on education amounts to improved education spending per capita. At the micro level, a declining fertility rate means that each family will have more time and resources to spend on educating their children. When it no longer becomes necessary for families to have many children to secure sufficient labor for their economic survival, families are able to allocate more resources to

education. The effect of improvements in education on political participation are well established in the literature (Inglehart and Norris 2003). Paglayan (forthcoming) presents credible evidence that improvements in primary education are a cause, rather than a consequence, of democratization.

There is a growing body of literature that examines the causal link between demographic transition and the emergence of democracy (e.g., Dyson 2013; Wilson and Dyson 2017). In conducting a systematic examination of this relationship, Sommer (2018) finds that the national fertility rate is strongly associated with the emergence of democracy even after controlling for other determinants of regime change and for the possibility of reverse causality. [24] Turning to our sample for the analysis here, there are many democracies that have a low fertility rate (e.g., most of Western advanced economies) and non-democracies that have a high fertility rate throughout the observation period, 1960-2010. [25]

As we will demonstrate later in the study, a nation's fertility rate is indeed a powerful predictor of democracy in our sample. More importantly for the purpose of illustrating the causal link between the two, the relationship between fertility and democracy is dynamic and ordered in the manner one would expect causally. There are many countries that (i) have a high fertility rate and are non-democratic during the first part of the observation period in our sample, (ii) experience a large decline in fertility rate, and (iii) the decline in fertility rate is followed by a democratic transition. Figure 2 details trends in national

[24] One cannot rule out the possibility of reverse causality in the fertility-democracy relationship. Perhaps the relationship may be bi-directional — lower fertility encourages democracy, and democracy lowers the fertility rate. However, this does not pose a problem for our purposes here. For an instrument to be relevant, there must be correlation, not necessarily causation, between fertility and democracy.

[25] Fertility rate data are from the Gender Statistics database from the World Bank, available at: https://datacatalog.worldbank.org/dataset/gender-statistics (last accessed in March, 2020).

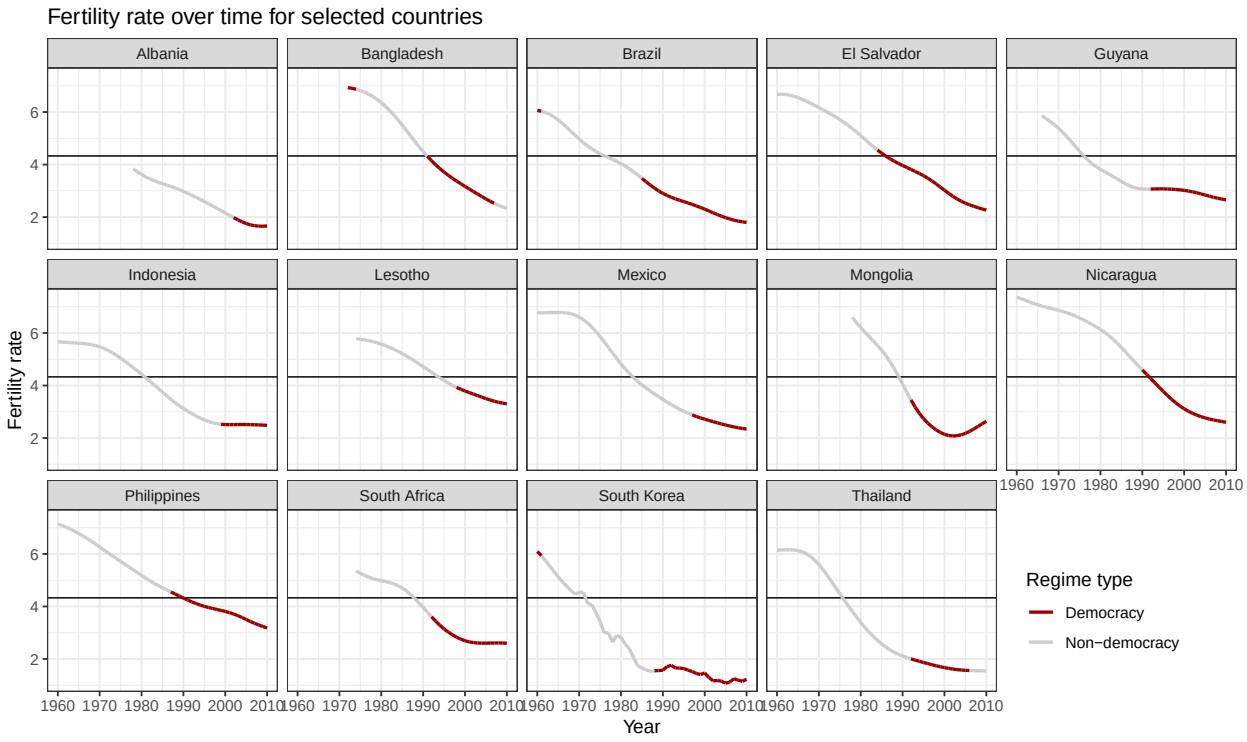


Figure 2: This figure shows fertility rate over time for selected countries during the observation period 1960-2010. We can see that these countries experienced democratization after their fertility rate plummeted below (or close to) the sample average (= 4.31, shown with the horizontal line in each panel).

fertility for some of the countries in our data sample that satisfy conditions (i)–(iii).

### Instrument exogeneity

For fertility rate to be a valid instrument, it must also satisfy the exogeneity condition [26] — fertility must not have any direct effect on conflict, that is, any effect of fertility on conflict must go through regime type. Although this is an untestable assumption, we seek to assess its plausibility by contemplating possible direct theoretical connections between fertility rate and conflict. One mechanism through which fertility could influence international conflict is through its effect on population size. For example, a high fertility rate might lead

[26] The exogeneity condition is often called the exclusion restriction. We use the two terms interchangeably.

to the eventual emergence of a "youth bulge," which could, in turn, encourage states to be conflictual internally and externally (Collier 2000). Alternatively, a low fertility rate could impose a shrinking domestic market, requiring peaceful external trade relationships with foreign economies to secure export markets. Both of these arguments expect the fertility rate and conflict to be positively correlated due to long-term demographic change.

Although empirical studies of international conflict often fail to consider the impact of demographic variables (such as population growth), this potential oversight is hardly sufficient to support our claim that fertility does not have a direct influence on interstate conflict. One of the few systematic studies that does include demographic factors as a determinant of conflict behavior reports that population growth is positively (albeit modestly) associated with conflict involvement (Tir and Diehl 1998). As far as a declining fertility rate implies slower population growth and a decline in the likelihood of youth bulges, [27] this finding may cast doubt on the exogeneity of fertility rate as an instrument.

However, the effect of demographic pressure on conflict is unlikely to be direct. Cranmer and Siverson (2008) argue that autocratic leaders are not as responsive to population pressure as democratic leaders. They show that population growth increases conflict initiation only among democracies. Building on their approach, we argue that the portion of population pressure and market pressure associated with the fertility rate first impacts regime type, which then affects leaders' conflict behavior. We will address the issue of instrument exogeneity more thoroughly by conducting a series of falsification tests that illustrate the plausibility of our assumptions. As with other questions of the robustness of our findings, we revisit this issue after we present the main empirical results.

[27] If a decline in infant mortality accompanies the decline in fertility, each effect may cancel the other out.

## Other covariates

Although we claim that fertility is exogenous to militarized conflict, we acknowledge that it is likely to be endogenous to certain economic, societal, and political forces. To reduce the possibility that such factors also influence conflict, which would open up a backdoor path to causation, we control for strong determinants of fertility in all three equations. [28] First, we control for infant mortality rates (log transformed) measured as the number of infants dying before reaching one year of age, per 1,000 live births in a given year. [29] The expectation is that lower infant mortality reduces the necessity for families to have many children as an insurance policy for economic security. Second, we control for the percentage of the population living in urban areas, as a proxy for economic modernization, which improves access to modern contraceptives. [30] Urbanization also increases the costs of raising children due to higher housing prices in cities (vis-a-vis rural areas) and higher opportunity costs. Third, we include per capita GDP to account for any residual effect of economic development levels that is not captured by infant mortality and urbanization. [31]

As the lefthand side panel of Figure 3 reveals, fertility has generally been declining throughout the period between 1960 and 2010. During this same time window, the pro-

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[28] The explanatory variables discussed below are intended to ensure that fertility is conditionally exogenous. We therefore include these variables not only in the democracy equations but also in the conflict equations, even though we may not have a clear expectation as to how these variables influence conflict.

[29] Data on mortality rate are obtained from the World Bank, available at: https://data.worldbank.org/indicator/ (last accessed in July, 2020).

[30] Data on urban population share are obtained from the United Nations, available at: https://population.un.org/wup/ (last accessed in July, 2020).

[31] Per capita GDP is highly correlated with infant mortality ($r = -0.80$) and urban population ($r = 0.81$).

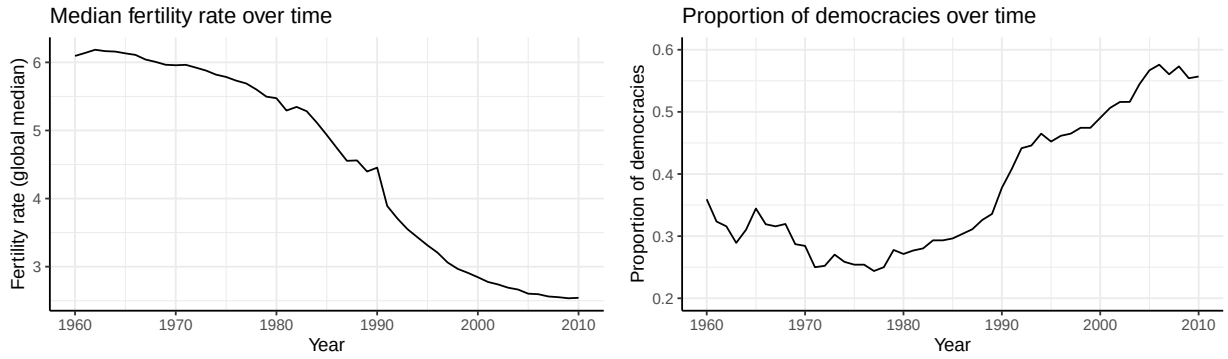


Figure 3: These figures show the secular trends of fertility rate (lefthand side) and the proportion of democracies in the world for the period between 1960 and 2010.

portion of democratic countries in the world has grown, albeit at a varying pace, as shown in the righthand side panel in the same figure. To guard against the possibility that unmeasured secular trends or regional idiosyncrasies influence at once fertility, democracy, and conflict, we control for decade and region fixed-effects in all three equations.

# Main Findings

Table 1 lists the estimated coefficients from models of democracy and militarized conflict initiation. As a reference, the first column (1) reports the results of a univariate model of conflict initiation that ignores regime type endogeneity. The second column (2) shows the results of our main trivariate model that jointly estimates conflict initiation and regime type to correct for the endogeneity. By comparing these models, we can see how our inferences change when we correct endogeneity while holding everything else constant.

Let us first look at the results from the "first stage" of our instrumental variables estimation, where challenger's and target's regime type variables are regressed. Consistent with our expectation, the coefficients for the fertility variables are negative and highly statistically significant in both challenger's and target's democracy equations, suggesting that a lower fertility rate is associated with democracy. These findings provide strong support for the relevance of our chosen instrument.

We now turn to the results from the outcome stage, where militarized conflict initiation is regressed on democracy measures and other covariates. The univariate clog-log model [32] that ignores the endogeneity, shown in column (1) in Table 1, successfully replicates the standard, dyadic democratic peace finding that democracies are peaceful, though only toward other democracies. Note that, while individual democracy measures have either a positive or insignificant coefficient, joint democracy has a negative coefficient that overwhelms the positive coefficients of individual democracy measures in the univariate model. As a result, the univariate model produces a result that, while democracy may increase conflict against a non-democracy, it decreases conflict against a democracy.

[32] We use clog-log link function for the outcome stage because militarized conflict initiation is a rare event (2.3% of all directed-dyad-year observations experience conflict initiation).

Table 1: Models of Democracy and Conflict Initiation, 1960–2010

| | (1) Univariate | (2) Trivariate |
|---|---|---|
| 1: Conflict equation (clog-log) | | |
| Challenger's Democracy | 0.133 | 1.200*** |
| | (0.137) | (0.371) |
| Target's Democracy | 0.357*** | 0.059 |
| | (0.107) | (0.376) |
| Joint Democracy | −0.802*** | −0.727*** |
| | (0.190) | (0.180) |
| Military Advantage | 0.623*** | 0.576*** |
| | (0.142) | (0.137) |
| Challenger's per capita GDP | −0.204*** | −0.259*** |
| | (0.069) | (0.067) |
| Target's per capita GDP | −0.297*** | −0.268*** |
| | (0.077) | (0.073) |
| Challenger's infant mortality | 0.157 | 0.351*** |
| | (0.117) | (0.120) |
| Target's infant mortality | −0.248* | −0.294** |
| | (0.127) | (0.125) |
| Challenger's urbanization | 0.011*** | 0.013*** |
| | (0.004) | (0.004) |
| Target's urbanization | 0.0004 | −0.0001 |
| | (0.004) | (0.004) |
| Peace Years | −0.925*** | −0.897*** |
| | (0.036) | (0.041) |
| Decades fixed-effects | ✓ | ✓ |
| Challenger's region fixed-effects | ✓ | ✓ |
| Target's region fixed-effects | ✓ | ✓ |
| 2: Challenger's democracy equation (probit) | | |
| Challenger's Fertility | | −0.133*** |
| | | (0.042) |
| Challenger's per capita GDP | | 0.299*** |
| | | (0.065) |
| Challenger's infant mortality | | −0.471*** |
| | | (0.105) |
| Challenger's urbanization | | −0.010*** |
| | | (0.003) |
| Decades fixed-effects | | ✓ |
| Challenger's region fixed-effects | | ✓ |
| 3: Target's democracy equation (probit) | | |
| Target's Fertility | | −0.131*** |
| | | (0.042) |
| Target's per capita GDP | | 0.299*** |
| | | (0.065) |
| Target's infant mortality | | −0.474*** |
| | | (0.105) |
| Target's urbanization | | −0.010*** |
| | | (0.003) |
| Decades fixed-effects | | ✓ |
| Target's region fixed-effects | | ✓ |
| Correlation coefficients | | |
| $\rho_{12}$ | | −0.306*** |
| | | (0.093) |
| $\rho_{13}$ | | 0.071 |
| | | (0.090) |
| $\rho_{23}$ | | 0.127*** |
| | | (0.041) |
| Number of observations | 34,312 | 34,312 |

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$
Standard errors (in parenthes) are clustered at directed-dyad level. Estimates for constant in each of the three equations are omitted for brevity.

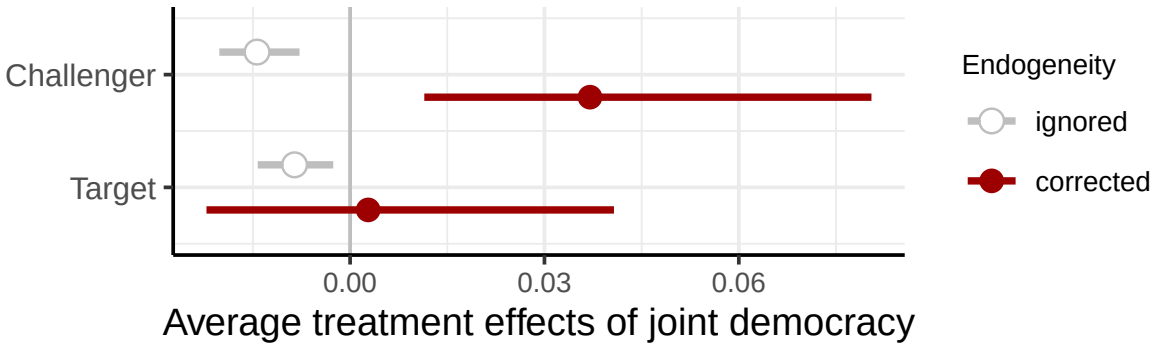


Figure 4: These figures show the estimated average treatment effects of challenger's and target's democracy on militarized conflict. Solid circles shown in red are the point estimates from the trivariate models that correct for endogeneity, whereas hollow circles shown in gray are those from the univariate models that ignore endogeneity. Horizontal line segments cover 95% confidence intervals from 1,000 bootstrap replicates.

To illustrate this, we calculate the average treatment effect of joint democracy for the challenger and for the target based on the univariate model. These effects are calculated by comparing the predicted probabilities of conflict initiation when changing the regime type of self (challenger or target) from non-democracy to democracy, holding constant the regime type of the other (target or challenger) as democracy. [33] Gray, hollow circles in Figure 4 show the treatment effects of challenger's and target's democracy. We can see that both effects are negative and statistically significant at the 95% confidence level.

Once we correct the endogeneity, however, the data no longer support such conclusions. In column (2) in Table 1, the negative coefficient for joint democracy no longer overwhelms the positive coefficient of challenger's democracy. Challenger's democracy now appears to increase conflict even against a democratic target. Red, solid circles in Figure 4 show the average treatment effects of challenger's and target's democracy, calculated from the trivariate model. The effect is positive and statistically significant for challenger's democracy, although the effect is indistinguishable from zero for target's democracy.

[33] Effects are averaged over all observations in the estimation sample. We obtain uncertainty estimates using non-parametric bootstrapping.

Whether we correct for endogeneity thus makes a significant difference in our estimates of the effect of joint democracy on conflict. The key to understanding why these changes occur lies in the estimated correlations between the error terms for different equations. The estimated error correlation between equations for conflict and challenger's democracy, $\rho_{12}$, is negative and statistically significant. This suggests that unobservable or unmeasured determinants of a country's democracy make it less likely for that country to attack another country. A failure to control for such factors would generate a negative omitted variable bias, making it look as if challenger's democracy has a pacifying effect on conflict behavior. On the other hand, the estimated error correlation between conflict and target's democracy equations, $\rho_{13}$, is indistinguishable from zero, suggesting that the endogeneity problem does not seem to operate for target's regime type.

We evaluated the robustness of our results with respect to some of our research design choices. First, we estimated models by altering the threshold to code regime type as democracy. We obtained consistent findings when we use polity2 $\geq 5$ or polity2 $\geq 7$ as a threshold. Second, we estimated the model by dropping per capita GDP and military balance to reduce the danger of post-treatment variable bias. Third, we estimated the model with a smaller sample that excluded country-pairs involving China. The Chinese government sought to suppress the fertility rate during this period by implementing a "one child" policy. This may have created heterogeneity in the data that could distort the findings. Finally, to assess the generalizability of our findings beyond contiguous dyads, we estimated the model with a larger sample that added non-contiguous country-pairs that belong to the same geographic region. This increases the sample size more than five times, from 34,312 to 199,168 observations. We find consistent results from all of these additional estimations. [34]

[34] All the results are presented in the Online Appendix.

## Falsification Tests

Our approach to correct the endogeneity via an IV estimation critically depends on our identification assumptions, the most notable of which is the exclusion restriction — the fertility rate must influence militarized conflict exclusively through regime type, conditional on covariates. We have sought to reduce the danger of violating this assumption by controlling for several strong covariates of fertility, but a violation could still occur if there is a causal link from fertility to conflict that does not go through regime type. Although we cannot verify this assumption empirically when it is correct, we can still falsify the assumption when it is wrong (Labrecque and Swanson 2018). We proceed next to assess the plausibility of the exclusion restriction by conducting a series of falsification tests.

### Test 1: Fertility rate and leader gender

We argue that a decline in fertility leads to female social empowerment, which encourages democratization and stabilizes existing democracies. Female empowerment, however, may also have a direct impact on interstate conflict, and if so this violates the exclusion restriction. An emerging body of research has focused on the effect of gender on international relations (Reiter 2015). Most notably, important research by Caprioli (2000, 2003) suggests that gender equality measures, including fertility, influence international conflict. [35] We assess the extent to which this poses a threat to our identification strategy.

We begin by examining the theoretical mechanism linking fertility and conflict that Caprioli (2000, 2003) and other scholars have proposed. Their argument proceeds in several steps. First, it is argued that (i) women tend to have more peaceful foreign policy

[35] See also Regan and Paskeviciute (2003) and Sobek, Abouharb, and Ingram (2006). Regan and Paskeviciute's (2003) findings contradict the hypothesized relationship between fertility and conflict. Sobek, Abouharb, and Ingram (2006) report mixed evidence linking respect for women's rights to conflict.

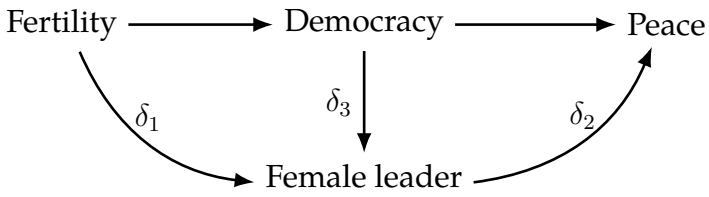


Figure 5: This diagram illustrates a scenario where the exclusion restriction may be violated. The exclusion restriction would be violated when both $\delta_1$ and $\delta_2$ are non-zero at the same time, because that opens up a path that connects fertility and peace that does not go through democracy. The exclusion restriction is still valid if the effect of fertility on leader gender goes through democracy rather than directly (i.e., $\delta_3 \neq 0$ and $\delta_1 = 0$).

preferences than men. Second, it assumes that (ii) declining fertility leads to female empowerment, which, in turn, contributes to gender equality. Third, (iii) as social gender equality improves, a state's foreign policy is more likely to reflect women's preferences.

We agree with the first and second premises, [36] but we doubt that the third premise could operate independent of regime type. As we argue above, when gender equality improves thanks to female empowerment, the regime is very likely to become more democratic. In other words, the process anticipated by premise (ii) should lead to democratization. Moreover, premise (iii) assumes that a country's foreign policy *automatically* reflects the preferences of those constituents that are more powerful than other constituents in the society. This, however, would not happen in the absence of some mechanism that incentivizes the government to implement policies that reflect powerful constituents' preferences. Let us now consider what factors, other than democracy, could motivate the government to adopt foreign policies that more clearly conform with women's preferences.

[36] Experimental studies report mixed evidence for the first premise. For example, Press, Sagan, and Valentino (2013) find no significant difference between men and women in terms of their preference for the use of nuclear weapons. There are reasons, however, to expect males to be more aggressive than females. For example, men tend to have higher levels of testosterone (McDermott et al. 2007; Klinesmith, Kasser, and McAndrew 2006), a hormone that is positively correlated with aggression.

One way that women might be empowered and have influence over foreign policy *in the absence of democracy* would be through a female leader. When lower fertility rates empower women, the probability that a woman becomes a national leader may increase, even in a non-democracy. A female national leader may then implement foreign policies that reflect the policy preferences of women. [37] Figure 5 presents a causal diagram that shows how this could violate the exclusion restriction. Suppose that female empowerment due to a declining fertility rate has a direct impact on a leader's gender (i.e., $\delta_1 \neq 0$). Suppose also that a leader's gender has a direct influence on war and peace (i.e., $\delta_2 \neq 0$). [38] A non-zero $\delta_1$ and a non-zero $\delta_2$ together open up a back-door causal pathway from fertility to interstate conflict that does not go through regime type, violating the exclusion restriction.

If, on the other hand, any effect of fertility on leader gender *goes through* regime type (i.e., $\delta_3 \neq 0$) and not directly (i.e., $\delta_1 = 0$), the exclusion restriction is still valid. Our first falsification test thus examines whether the fertility rate has a direct impact on the likelihood that a country has a female leader, independent of effects that go through regime type. We estimate a series of binary time-series cross-sectional clog-log models using country-year as the unit of observation. [39] The dependent variable is a dummy variable coded as 1 for a country that has at least one female national leader in a given year, and 0

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[37] Another causal pathway may be through legislative representation. Caprioli (2000) and Regan and Paskeviciute (2003) use the percentage of women in legislative bodies to measure gender equality. As Bjarnegård and Melander (2013) point out, however, such measures are heavily influenced by gender quotas and may not accurately reflect how political power is actually shared between men and women.

[38] There is no consensus in the literature about whether leader gender affects conflict. For example, Horowitz, Stam, and Ellis (2015) find no evidence that leader gender influences conflict behavior.

[39] We use a clog-log link function as female leaders are rare (2.87% of country-years have a female leader).

otherwise. [40]

Table 2 reports the estimation results. Models (3) and (4) include fertility rate and regime type separately, whereas model (5) includes the two variables at the same time. We find no relationship between fertility and leader gender (i.e., estimates of $\delta_1$ are indistinguishable from zero), whether or not we control for regime type. On the other hand, democracies are much more likely to have a female leader (i.e., estimates of $\delta_3 > 0$). The most plausible explanation for these findings is that any effect of the fertility rate on leader gender goes exclusively through regime type, lending support to the exclusion restriction.

Table 2: Clog-log Models of Female Leaders

| | (3) | (4) | (5) |
|---|---|---|---|
| Fertility rate ($\delta_1$) | −0.042 | | −0.095 |
| | (0.153) | | (0.175) |
| Democracy ($\delta_3$) | | 1.166*** | 1.186*** |
| | | (0.325) | (0.318) |
| Per capita GDP | −0.443* | −0.427* | −0.392 |
| | (0.238) | (0.237) | (0.252) |
| Infant mortality | −0.615** | −0.638** | −0.542* |
| | (0.271) | (0.280) | (0.325) |
| Urbanization | 0.010 | 0.006 | 0.005 |
| | (0.010) | (0.011) | (0.011) |
| Decade fixed-effects | ✓ | ✓ | ✓ |
| Region fixed-effects | ✓ | ✓ | ✓ |
| Years since last female leader | ✓ | ✓ | ✓ |
| N | 6,595 | 6,595 | 6,595 |

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$
Estimated coefficients for fixed effects and the intercept are suppressed for brevity.
Standard errors (in parentheses) are clustered at the country level.

[40] We rely on data from version 4.1 of the Archigos data set (Goemans, Gleditsch, and Chiozza 2009).

### Test 2: Reduced-form analysis of conflict

Another way to assess the plausibility of the exclusion restriction is to explore reduced-form relationships between fertility and militarized conflict. [41] A reduced-form model omits the endogenous variable (i.e., regime type) from the outcome regression and looks at the direct relationship between the instruments (i.e., fertility rate) and the outcome (i.e., militarized conflict). That is, we simply replace the regime type variables with fertility rate in the univariate clog-log model predicting conflict initiation. Given the finding from our IV model that reduced fertility promotes democracy and that challenger's democracy increases conflict, we expect to find a negative relationship between challenger's fertility and conflict when we conduct a reduced-form analysis on the entire sample.

However, the exclusion restriction implies that a reduced-form analysis should reveal no relationship between the fertility variables and conflict, if we are to analyze those cases where the causal path from fertility to democracy is blocked. In other words, the exclusion restriction would be violated if fertility rate is found to influence conflict in situations where the fertility-democracy path is plausibly shut out. Our second falsification test thus examines a reduced-form relationship for some subsets of our sample, for which it is plausible to assume that the path from fertility to regime type is blocked.

How can we create or find a situation where the path from fertility to conflict is plausibly blocked? We seek to emulate such an instance by considering hypothetical principal stratification based on treatment compliance (Angrist, Imbens, and Rubin 1996), imagining our study were an ideal randomized control trial (RCT) on the democratic peace. With regime type being our "treatment" variable (democracy and non-democracy are the possible values) and conflict being the outcome variable in such an imagined RCT, fertility rate would correspond to the "treatment assignment" variable. For example, receiving a low

[41] This analysis is motivated by Nunn and Wantchekon (2011) and Acharya, Blackwell, and Sen (2016).

fertility rate would correspond to being assigned to the "treated" group (i.e., assigned to be democratic). Then, we identify four latent types based on the correspondence between treatment assignment and treatment reception: Compliers are those who would be democratic if and only if fertility rate is low; Defiers are those who would be non-democratic if and only if fertility rate is low; and Always-takers (never-takers) are those who would always (never) be democratic regardless of the assigned fertility level.

Although we cannot directly observe these four ideal types, we can narrow down the possibilities of latent types based on observed treatment and treatment assignment, as shown in Table 3. For example, if we observe a country is democratic and has a low fertility rate, then that country is either a complier or an always-taker. [42] Likewise, non-democracies with high fertility rates are either a complier or a never-taker. Given our theoretical argument that lower fertility leads to democracy, it would be reasonable to assume that we do not have a defier in our sample. [43] Any observations that fall in the top-right cell or the bottom-left cell in Table 3 would then be either an always-taker or a never-taker. Specifically, we identify "always-takers" as democratic country-years with a fertility rate higher than its median value, and "never-takers" as non-democratic country-years with a fertility rate lower than the fertility rate's median value in our sample of country years. [44]

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[42] It is not possible to narrow this down further purely by observation. To distinguish between compliers and always-takers among those subjects in the top-left cell of the table, we would need to know what the value of the regime type be for the same subject under a counterfactual scenario where the fertility were hypothetically switched to high. The same applies to the other three cells in the table.

[43] This is often called the monotonicity assumption. Given that lower fertility is statistically associated with democracy, there are not many observations that fall into the top-right cell or the bottom-left cell.

[44] For example, our classification rule would code China as never-takers for all years after 1975. China has experienced a rapid decline in fertility rate due to the "one-child"

Table 3: Observed and principal strata

| | Fertility=Low | Fertility=High |
|---|---|---|
| Democracy | Complier / Always-taker | Defier / Always-taker |
| Non-democracy | Defier / Never-taker | Complier / Never-taker |

Because the path from fertility to regime type is blocked for always-takers and never-takers, reduced-form analyses of such cases should reveal no relationship between the fertility rate and conflict — otherwise, there must be a direct path from fertility to conflict, violating the exclusion restriction. Figure 6 reports the estimated reduced-form relationship between the challenger's fertility and conflict for all observations in the sample (top) and for dyads comprised of either always-takers or never-takers (bottom). The x-axis shows the estimated effect of a challenger's fertility rate on the probability of militarized conflict initiation in reduced-form models. Effects are calculated by comparing the predicted probabilities of conflict when increasing the value of the challenger's fertility rate from its mean value for democracies (2.82) to its mean value for non-democracies (5.17), controlling for the target's fertility rate at its mean value for democracies.

As the top estimate reveals, the reduced-form model finds a negative relationship between a challenger's fertility rate and interstate conflict when estimated for all observations. This is consistent with our findings from the IV model that shows that challenger's democracy has a positive effect on conflict. [45] However, this relationship disappears when we analyze the sample that is restricted to either always-takers or never-takers as defined above. As with Test 1, the findings for Test 2 again lend support to the exclusion restriction.

policy implemented from 1979 to 2015, while it has been non-democratic during the entire period analyzed in our study.

[45] Regan and Paskeviciute (2003) also find a negative relationship between fertility and interstate conflict.

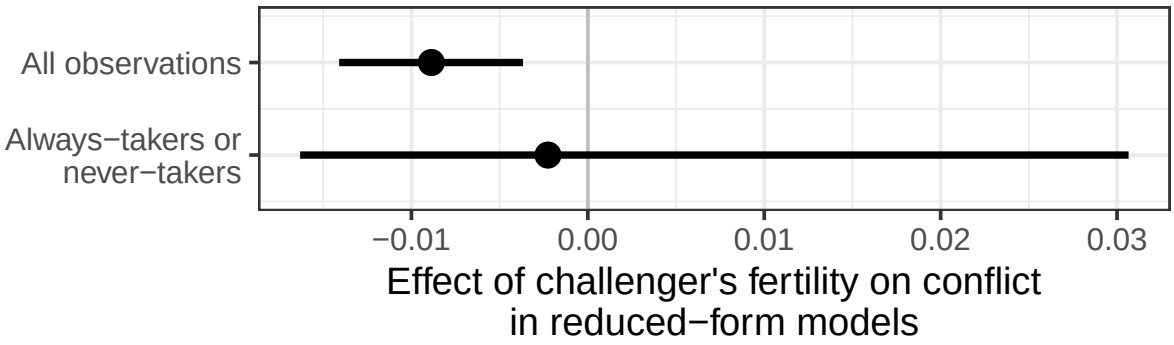


Figure 6: This figure shows the effects of the challenger's fertility rate on the probability of militarized conflict estimated with reduced-form models. The top estimate shows the result for all observations, whereas the bottom estimate reports the effect for a sample that includes only "always-takers" or "never-takers."

## Test 3: Conflict and fertility

Another possible violation of the exclusion restriction could occur when fertility is affected by conflict. Past militarized disputes may influence the present-day fertility rate, creating a correlation between fertility and conflict, independent of regime type. To guard against this possibility, our final falsification test examines the relationship between fertility as the dependent variable and past conflict involvement as an independent variable.

Table 4: OLS Models of Fertility

| | (6) | (7) | (8) |
|---|---|---|---|
| # of conflicts in the previous year | −0.012 | | |
| | (0.025) | | |
| # of conflicts in the previous 3 years | | −0.004 | |
| | | (0.011) | |
| # of conflicts in the previous 5 years | | | −0.003 |
| | | | (0.007) |
| Per capita GDP | 0.064 | 0.055 | 0.043 |
| | (0.092) | (0.095) | (0.095) |
| Infant mortality | 0.794*** | 0.786*** | 0.780*** |
| | (0.111) | (0.113) | (0.115) |
| Urbanization | −0.019*** | −0.019*** | −0.018*** |
| | (0.004) | (0.004) | (0.004) |
| Decade fixed-effects | ✓ | ✓ | ✓ |
| Region fixed-effects | ✓ | ✓ | ✓ |
| N | 6,507 | 6,266 | 6,010 |

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$
Estimated coefficients for fixed effects and the intercept are suppressed for brevity.
Standard errors (in parentheses) are clustered at country level.

Table 4 shows the results from linear regression models predicting the fertility rate for country-year data. In all three models, the dependent variable is fertility rate for a country in a given year. We test three different measures of past conflict involvement; they count the number of militarized conflicts in the past 1 year, 3 years, or 5 years. In all three models, the coefficients for past conflict involvement cannot be distinguished statistically from zero, again lending support for the exclusion restriction. [46]

[46] These results are consistent with the findings from studies that look at the effect of *intrastate* conflict. For example, Urdal and Chi (2013) find intrastate conflict both within and in the neighboring countries do not affect fertility rate.

# Conclusions

We proposed an approach to estimate the causal effect of joint democracy on violent conflict between countries. Our empirical results suggest that democracy does not have a pacifying effect — on the contrary, democratic countries tend to attack other countries more than non-democratic ones do. These findings not only have important policy implications for foreign policy decision making but also contribute to ongoing scholarly efforts to understand, explain, and predict state behavior in world politics. The modern study of the democratic peace started with an empirical observation that democracy and peace are correlated, which was then followed by theoretical efforts to make sense of the observed pattern. Our findings contradict the initial empirical observation and thus bring into question many of the theories that have been proposed to explain the democratic peace.

If democracy is not the driving factor, we naturally wonder, what explains the observed peace between democracies? While our findings suggest that it is not the first-cluster (democracy-as-cause) argument, they do not reveal which variants of the second-cluster (reverse causality) or third-cluster (spurious correlation) argument are valid. Nevertheless, we offer an additional criterion to choose among multiple possible explanations. Any theory of liberal peace must be able to explain not only the observed peace between democracies but also why democratic challengers are more conflict-prone than non-democratic challengers, even against a democratic target. A promising avenue for future research would be to theorize about these (apparently contradictory) empirical patterns.

Another important avenue for future research is to apply the analytical framework of our study to exploring different variables to see if our conclusions generalize beyond our data. As pointed out above, what our IV approach identifies is a *local* average treatment effect (LATE) — "local" in the sense that this is the fertility-induced causal effect of democracy, which may not be equal to the overall causal effect of democracy. It is still possible that a LATE of democracy induced by a different instrument has a negative or an even

greater positive effect on conflict — we just cannot identify this effect without additional instruments and assumptions. We therefore agree with Imbens's (2010) aphorism: "better LATE than nothing." As we cannot do randomized controlled trials on democracy and peace, LATE is the best we can hope to achieve in empirical research on the democratic peace. We hope that other scholars follow our lead and conduct a study that identifies LATEs of joint democracy on conflict using alternative instruments.

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# Make Two Democracies and Call Me in the Morning: Endogenous Regime Type and the Democratic Peace

## Online Appendices

## Not for Print Publication

## Contents

# A The Statistical Model

We begin by presenting a statistical model that allows us to estimate three discrete-choice equations jointly. In doing so, we illustrate how a separate estimation could generate endogeneity bias due to omitted variables. Let $y_i$ denote conflict initiation by a potential challenger country $c_i$ against a potential target country $t_i$ in a directed dyad (challenger-target pair) $i$, where the value of 1 is observed when conflict is initiated and 0 otherwise. [1] Suppose we have binary measures of regime type for the challenger and target, $D_i^c$ and $D_i^t$, coded 1 for democracies and 0 for non-democracies. To evaluate the effects of democracy on conflict initiation, we could estimate the following regression:

$$y_i = 1[\beta_1 D_i^c + \beta_2 D_i^t + \beta_3 D_i^c D_i^t + \boldsymbol{x_i}\boldsymbol{\beta_x} + \eta_i > 0], \tag{1}$$

where $\beta_1$, $\beta_2$, and $\beta_3$ are coefficients for democracy measures, $\boldsymbol{x_i}$ is a vector of exogenous covariates for $i$, $\boldsymbol{\beta_x}$ is a vector of coefficients for $\boldsymbol{x_i}$, and $\eta_i$ is an error term.

The most widely accepted interpretation of the dyadic democratic peace thesis posits that democracies are peaceful largely only toward other democracies. [2] Testing this claim requires that we compare the predicted probabilities of conflict initiation when we change the value of regime type in self (challenger or target) from non-democracy to democracy, while holding constant the regime type of other (target or challenger) as democracy. More precisely, let $\tau^c$ denote the sample average treatment effect of joint democracy (SATE) for

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[1] We omit the time component on observations throughout presentation in this Appendix for simplicity.

[2] While democracies can increase conflict against non-democracies (i.e., $\beta_1$ and $\beta_2$ may be positive), joint democracy decreases conflict (i.e., $\beta_3$ is negative and greater in absolute value than $\beta_1$ and $\beta_2$).

the challenger and $\tau^t$ for the target. These SATE values are obtained as follows:

$$\tau^c = \frac{1}{n}\sum\{F_\eta(\hat{\beta}_1 + \hat{\beta}_2 + \hat{\beta}_3 + \boldsymbol{x_i}\hat{\boldsymbol{\beta}}_{\boldsymbol{x}}) - F_\eta(\hat{\beta}_2 + \boldsymbol{x_i}\hat{\boldsymbol{\beta}}_{\boldsymbol{x}})\} \tag{2}$$

$$\tau^t = \frac{1}{n}\sum\{F_\eta(\hat{\beta}_1 + \hat{\beta}_2 + \hat{\beta}_3 + \boldsymbol{x_i}\hat{\boldsymbol{\beta}}_{\boldsymbol{x}}) - F_\eta(\hat{\beta}_1 + \boldsymbol{x_i}\hat{\boldsymbol{\beta}}_{\boldsymbol{x}})\}, \tag{3}$$

where $n$ is the number of directed dyads in the sample and $F_\eta(\cdot)$ is the cumulative distribution function for the error term.[3] If democratic challengers are less likely to fight democratic targets than are non-democratic challengers, $\tau^c$ will be negative, which is equivalent to $\hat{\beta}_1 + \hat{\beta}_3 < 0$. Similarly, $\tau^t$ will also be negative (i.e., $\hat{\beta}_2 + \hat{\beta}_3 < 0$) if democratic targets pacify democratic challengers.

However, these causal effects estimated with the univariate regression (1) may suffer from endogeneity bias if there are unmeasured factors influencing both conflict ($y_i$) and regime type ($D_i^c$ and $D_i^t$). For example, when some unmeasured factors encourage democracy and discourage conflict, $D_i^c$ and $D_i^t$ tend to take on higher values in cases where $\eta_i$ takes a lower value. Lower values of the error term will then be absorbed by the democracy coefficients, generating a downward bias on the estimates of $\beta_1$, $\beta_2$, and $\beta_3$ and making it more likely to estimate a pacifying effect of democracy, even when this does not exist.

To correct for this endogeneity problem, we model $y_i$, $D_i^c$, and $D_i^t$ as a joint process using a trivariate model. The model has three equations connected via the trivariate Gaussian copula, thus allowing us to capture the correlation between the error terms for the equations. We have the following system of equations:

$$y_i = 1[\beta_1 D_i^c + \beta_2 D_i^t + \beta_3 D_i^c D_i^t + \boldsymbol{x_i}\boldsymbol{\beta}_{\boldsymbol{x}} + \eta_i > 0] \tag{4}$$

$$D_i^c = 1[\boldsymbol{z_i^c}\boldsymbol{\gamma_c} + \epsilon_i^c > 0] \tag{5}$$

$$D_i^t = 1[\boldsymbol{z_i^t}\boldsymbol{\gamma_t} + \epsilon_i^t > 0], \tag{6}$$

[3] As the outcome variable, conflict initiation, is a rare event, we use the complementary log-log (clog-log) specification for the conflict equation throughout this study.

where $\boldsymbol{z_i^c}$ and $\boldsymbol{z_{ci}^t}$ are vectors of covariates for the challenger's and the target's regime type in dyad $i$, respectively, and $\boldsymbol{\gamma_c}$ and $\boldsymbol{\gamma_t}$ are vectors of coefficients for variables in $\boldsymbol{z_i^c}$ and $\boldsymbol{z_i^t}$.

Let $\epsilon_i^y = \Phi^{-1}\{F_\eta(\eta_i)\}$, where $\Phi^{-1}(\cdot)$ denotes the quantile function for the standard Normal distribution. Then, $\epsilon_i^y$ and the error terms for the second and third equations follow the trivariate Normal distribution $(\epsilon_i^y, \epsilon_i^c, \epsilon_i^t)' \sim^{iid} \mathcal{N}_3(0, \Sigma)$, where [4]

$$\Sigma = \begin{bmatrix} 1 & \rho_{12} & \rho_{13} \\ \rho_{21} & 1 & \rho_{23} \\ \rho_{31} & \rho_{32} & 1 \end{bmatrix}.$$

On the diagonal are the error variance terms, normalized to 1. The off-diagonal elements represent the correlation between the error terms, such that $\rho_{jk} = \rho_{kj}$ for $j \neq k$.

We thus jointly model democracies and conflict with an instrumental variables (IV) regression. Equations (5) and (6) above constitute the "first stage" of an IV estimation, where endogenous predictors, $D_i^c$ and $D_i^t$, are regressed on covariates. Equation (4) is the "second stage" or the outcome stage, where the outcome of interest, $y_i$, is regressed on endogenous predictors and other covariates. We can plug in estimated $\beta$s from the trivariate model to equations (2) and (3) to calculate SATEs that correct for the endogeneity.

[4] The first equation in our model has the clog-log link and the other two equations use the probit link.

# B Monte Carlo Simulations

We conduct Monte Carlo simulations to illustrate how a univariate estimation of equation (1) produces bias in the estimate of the causal effect of democracy on conflict and that a trivariate estimation of equations (4)–(6) recovers the correct causal effects. We do this because a trivariate model is not commonly used as an IV model in political science. [5]

We assume the following data generation process. First, we generate a set of four covariates, $x_{1i}$, $x_{2i}$, $x_{3i}$, and $x_{4i}$, for $i = 1, 2, 3, \ldots, 1000$, each based on independent uniform distribution $\mathcal{U}(0, 1)$. We fix the values of these covariates constant throughout the entire simulation. For each round of simulation, we draw a set of three error terms, $\epsilon_{yi}$, $\epsilon_{ci}$, and $\epsilon_{ti}$, from a trivariate Normal distribution. We fix the value of $\rho_{23}$ (error correlated between challenger's and target's democracy) at 0.3 throughout the simulation, but change the value of $\rho_{12}$ (error correlation between conflict and challenger's democracy) at $\{0, -0.2\}$ and the value of $\rho_{13}$ (error correlation between conflict and target's democracy) at $\{0, -0.2, -0.4, -0.6\}$. Permutations of two different values of $\rho_{12}$ and four different values of $\rho_{13}$ produce eight combinations of the error correlations. For example, a combination $(\rho_{12} = 0, \rho_{13} = -0.4, \rho_{23} = 0.3)$ means that unobservable factors that encourage democracy are uncorrelated with unobservable determinants of challenger's conflict initiation but negatively correlated with unobservable determinants of target's propensity to attract conflict. Note that a combination $(\rho_{12} = 0, \rho_{13} = 0, \rho_{23} = 0.3)$ corresponds to a case where unobservable determinants of democracy are uncorrelated with conflict, suggesting that there is no endogeneity in a univariate estimation of equation (1).

[5] We are aware of one application in the field of economics Filippou et al. (2019) where the trivariate probit model is used to endogenize two binary predictors. They also conduct Monte Carlo simulations, but one of the key differences between their simulation study and ours is that our model has an interaction term between the two endogenous regressors.

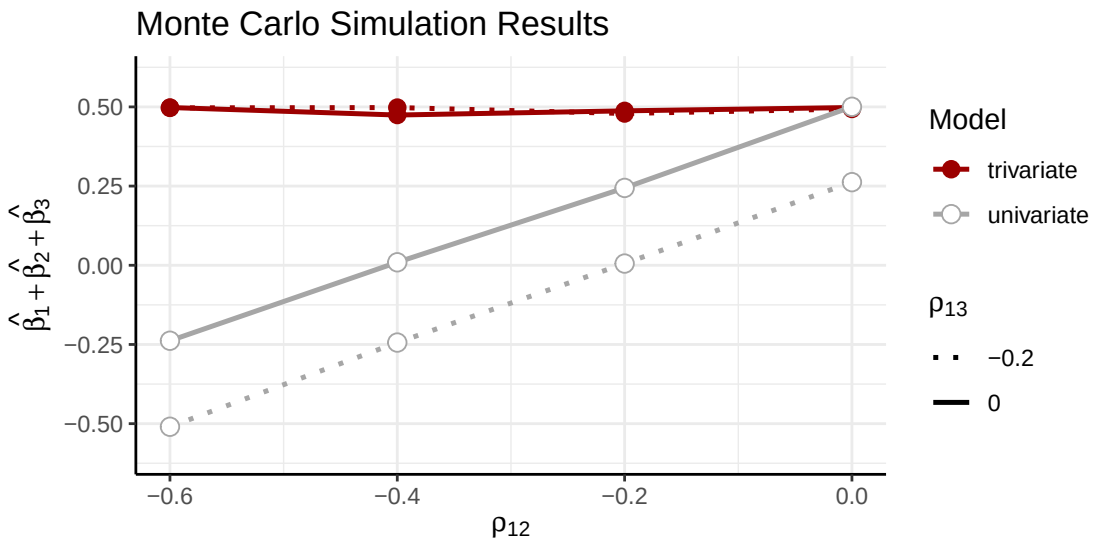


Figure A.1: This figure shows the results of Monte Carlo simulations. Solid circles show the mean values of the estimated $\beta_1 + \beta_2 + \beta_3$ from the trivariate probit models for different values of $\rho_{12}$ and $\rho_{13}$, whereas hollow circles (in gray) are from the univariate probit models. Solid lines indicate $\rho_{12}$ is set to 0, whereas dotted lines indicate $\rho_{12}$ is set to $-0.2$. The true value of $\beta_1 + \beta_2 + \beta_3$ is held constant at 0.5 and $\rho_{23}$ is held at 0.3 throughout the simulations.

For each of the eight combinations of $\rho$'s, we perform 200 iterations of simulations. For each iteration, we generate endogenous variables $D_i^c$, $D_i^t$, and $y_i$ as follows:

$$D_i^c = 1[2x_1 - x_3 + \epsilon_{ci} > 0] \tag{7}$$

$$D_i^t = 1[2x_2 - x_3 + \epsilon_{ti} > 0] \tag{8}$$

$$y_i = 1[-1 + \beta_1 D_i^c + \beta_2 D_i^t + \beta_3 D_i^c D_i^t + 0.5x_3 - 0.5x_4 + \epsilon_i^y > 0]. \tag{9}$$

We set $\beta_1 = 0.5$, $\beta_2 = 0.5$, and $\beta_3 = -0.5$, so that $\beta_1 + \beta_2 + \beta_3 = 0.5$. This means that joint democracy does *not* have a pacifying effect in truth. For each iteration, we estimate a univariate and a trivariate probit model and store the estimated values of $\hat{\beta}_1 + \hat{\beta}_2 + \hat{\beta}_3$.

Figure A.1 summarizes the results of our Monte Carlo simulations. As expected, when $\rho_{12} = 0$ and $\rho_{13} = 0$, both univariate and trivariate models can recover the true sum of democracy coefficients ($\beta_1+\beta_2+\beta_3 = 0.5$). When either $\rho_{12}$ or $\rho_{13}$ deviates from 0, however, univariate probit models produce bias while trivariate probit models consistently recover

the correct value of $\beta_1 + \beta_2 + \beta_3$. As $\rho_{13}$ gets smaller, univariate estimation yields greater negative bias, making it more likely to mistakenly associate joint democracy with peace.

# C Descriptive Statistics

Table A.1 provides descriptive statistics for data in the main analysis and robustness checks.

Table A.1: Descriptive statistics for the main analyses

| Statistic | Mean | St. Dev. | Min | Pctl(25) | Pctl(75) | Max |
|---|---|---|---|---|---|---|
| Dispute initiation | 0.023 | 0.149 | 0 | 0 | 0 | 1 |
| Democracy (Polity $\geq 6$) | 0.405 | 0.491 | 0 | 0 | 1 | 1 |
| Democracy (Polity $\geq 5$) | 0.435 | 0.496 | 0 | 0 | 1 | 1 |
| Democracy (Polity $\geq 7$) | 0.358 | 0.479 | 0 | 0 | 1 | 1 |
| Joint democracy (Polity $\geq 6$) | 0.268 | 0.443 | 0 | 0 | 1 | 1 |
| Joint democracy (Polity $\geq 5$) | 0.296 | 0.456 | 0 | 0 | 1 | 1 |
| Joint democracy (Polity $\geq 7$) | 0.226 | 0.418 | 0 | 0 | 1 | 1 |
| Fertility rate | 4.075 | 2.097 | 1.076 | 2.067 | 6.073 | 8.864 |
| Capability balance | 0.500 | 0.326 | 0.0002 | 0.187 | 0.813 | 1.000 |
| Per capita GDP | 8.411 | 1.267 | 4.889 | 7.379 | 9.443 | 13.357 |
| Infant mortality | 3.541 | 1.092 | 0.788 | 2.734 | 4.494 | 5.633 |
| Urbanization | 51.423 | 23.361 | 2.233 | 31.300 | 71.736 | 100.000 |
| Peace years | 22.193 | 16.691 | 1 | 8 | 34 | 65 |

The number of observation is $34,312$.

# D Robustness Checks

We perform the following additional analyses as robustness checks:

1. Models that code alternative thresholds of democracy
2. Models that drop potential post-treatment variables
3. A sample that drops China as a potential outlier
4. A sample that includes dyads within the same geographic regions

## 1. Models that code alternative thresholds of democracy

Following the convention in the literature, we code countries with the combined polity2 score of 6 or higher as a democracy. To assess the robustness of the results with respect to this, we estimated our models using a lower (5) or a higher (7) threshold to code democracy. Figure A.2 replicates Figure 4 in the main text but with two additional democracy thresholds. Estimated coefficients for models with alternative thresholds are presented in Table A.2. We can see that the main finding is robust to alternative thresholds — challenger's democracy has a positive effect on conflict when we correct the endogeneity.

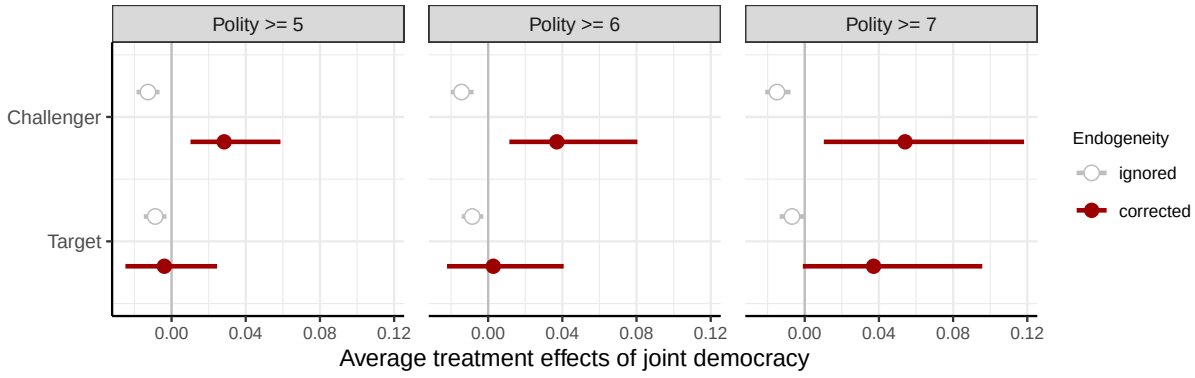


Figure A.2: These figures show the estimated average treatment effects of democracy on militarized conflict with different thresholds to democracy.

## Table A.2: Models with Alternative Democracy Thresholds

| | Polity $\geq 5$ | | Polity $\geq 7$ | |
|---|---|---|---|---|
| | Univariate | Trivariate | Univariate | Trivariate |
| 1: Conflict equation (clog-log) | | | | |
| Challenger's Democracy | 0.261** | 1.078*** | 0.089 | 1.160*** |
| | (0.131) | (0.334) | (0.138) | (0.424) |
| Target's Democracy | 0.406*** | −0.093 | 0.405*** | 0.675* |
| | (0.130) | (0.363) | (0.113) | (0.402) |
| Joint Democracy | −0.828*** | −0.774*** | −0.766*** | −0.732*** |
| | (0.187) | (0.182) | (0.239) | (0.210) |
| Military Advantage | 0.620*** | 0.584*** | 0.642*** | 0.610*** |
| | (0.141) | (0.137) | (0.141) | (0.139) |
| Challenger's per capita GDP | −0.207*** | −0.235*** | −0.204* | −0.275*** |
| | (0.069) | (0.066) | (0.068) | (0.066) |
| Target's per capita GDP | −0.287*** | −0.264* | −0.311*** | −0.322*** |
| | (0.076) | (0.072) | (0.078) | (0.077) |
| Challenger's infant mortality | 0.170 | 0.306*** | −0.147 | 0.306** |
| | (0.114) | (0.116) | (0.119) | (0.128) |
| Target's infant mortality | −0.251** | −0.322** | −0.257** | −0.212* |
| | (0.126) | (0.125) | (0.126) | (0.120) |
| Challenger's urbanization | 0.011*** | 0.012*** | 0.011*** | 0.013*** |
| | (0.004) | (0.004) | (0.004) | (0.004) |
| Target's urbanization | −0.0003 | −0.0004 | 0.001 | 0.001 |
| | (0.004) | (0.004) | (0.004) | (0.004) |
| Peace Years | −0.924*** | −0.901*** | −0.924*** | −0.901*** |
| | (0.035) | (0.038) | (0.036) | (0.044) |
| Decades fixed-effects | ✓ | ✓ | ✓ | ✓ |
| Challenger's region fixed-effects | ✓ | ✓ | ✓ | ✓ |
| Target's region fixed-effects | ✓ | ✓ | ✓ | ✓ |
| 2: Challenger's democracy equation (probit) | | | | |
| Challenger's Fertility | | −0.094** | | −0.181*** |
| | | (0.041) | | (0.045) |
| Challenger's per capita GDP | | 0.189*** | | 0.467*** |
| | | (0.063) | | (0.063) |
| Challenger's infant mortality | | −0.408*** | | −0.331*** |
| | | (0.108) | | (0.101) |
| Challenger's urbanization | | −0.006** | | −0.012*** |
| | | (0.003) | | (0.003) |
| Decade fixed-effects | | ✓ | | ✓ |
| Challenger's region fixed-effects | | ✓ | | ✓ |
| 3: Target's democracy equation (probit) | | | | |
| Target's Fertility | | −0.092** | | −0.180*** |
| | | (0.041) | | (0.045) |
| Target's per capita GDP | | 0.188*** | | 0.469*** |
| | | (0.063) | | (0.063) |
| Target's infant mortality | | −0.411*** | | −0.333*** |
| | | (0.108) | | (0.101) |
| Target's urbanization | | −0.006* | | −0.012*** |
| | | (0.003) | | (0.003) |
| Decade fixed-effects | | ✓ | | ✓ |
| Target's region fixed-effects | | ✓ | | ✓ |
| Correlation coefficients | | | | |
| $\rho_{12}$ | | −0.233*** | | −0.303*** |
| | | (0.088) | | (0.113) |
| $\rho_{13}$ | | 0.123 | | −0.082 |
| | | (0.082) | | (0.102) |
| $\rho_{23}$ | | 0.158*** | | 0.124*** |
| | | (0.038) | | (0.042) |
| Number of observations | 34,312 | 34,312 | 34,312 | 34,312 |

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$
Standard errors (in parenthes) are clustered at directed-dyad level. Estimates for constant in each of the three equations are omitted for brevity.

## 2. Models that drop potential post-treatment variables

Some of the explanatory variables we include in the conflict equation could be considered post-treatment variables. For example, one could argue that democracy influences economic development. If so, then including per capita GDP in the conflict equation may create a post-treatment variable bias. Similarly, one could argue that the dyadic military balance is a consequence of dyadic regime type (dis)similarity. To guard against these possibilities, we estimate models that exclude per capita GDP, military advantage, or both. [6]

Note that per capita GDP is also dropped from each of the first-stage equations (challenger's and target's democracy equations). This is necessary because retaining them only in the first-stage equations would treat them as instrumental variables. Dropping these variables from the first-stage equations also comes at the cost of increasing the danger of violating the conditional ignorability assumption (fertility is assigned as if random, conditional on covariates). We are less concerned about this in light of the findings reported in Table 4 of the main text that per capita GDP is *not* a significant determinant of fertility. [7]

Figure A.3 replicates Figure 4 in the main text but with models that drop per capita GDP and/or Military Advanrage. Estimated coefficients for these models are presented in Table A.3. The main finding is robust to dropping potentially post-treatment variables — challenger's democracy has a positive effect on conflict when we correct the endogeneity.

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[6] Dropping these variables from the conflict equation increases the danger of omitted variable bias.

[7] This finding is consistent with (Urdal and Chi 2013), who find that per capita GDP becomes insignificant when they include infant mortality and urbanization in the model of fertility. As noted in the main text, per capita GDP is highly correlated with both infant mortality and urbanization.

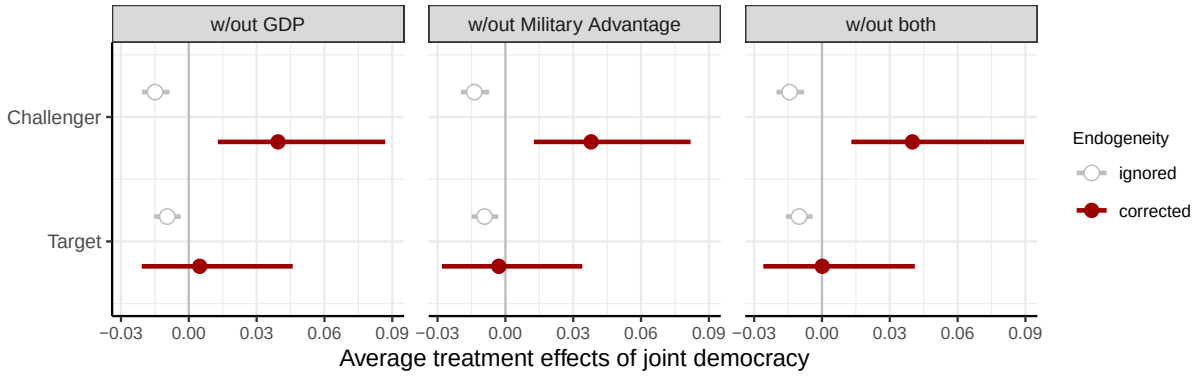


Figure A.3: These figures show the estimated average treatment effects of democracy on militarized conflict, dropping per capita GDP, Military Advantage, or both variables.

Table A.3: Models without Potential Post-treatment Variables

| | w/out GDP | | w/out Mil. Adv. | | w/out both | |
|---|---|---|---|---|---|---|
| | Univariate | Trivariate | Univariate | Trivariate | Univariate | Trivariate |
| 1: Conflict equation (clog-log) | | | | | | |
| Challenger's Democracy | 0.153 | 1.231*** | 0.162 | 1.278*** | 0.178 | 1.290*** |
| | (0.144) | (0.379) | (0.143) | (0.390) | (0.149) | (0.408) |
| Target's Democracy | 0.358*** | −0.098 | 0.336*** | −0.059 | 0.336*** | 0.003 |
| | (0.107) | (0.359) | (0.109) | (0.375) | (0.108) | (0.362) |
| Joint Democracy | −0.854*** | −0.774*** | −0.809*** | −0.717*** | −0.857*** | −0.764*** |
| | (0.192) | (0.178) | (0.193) | (0.185) | (0.195) | (0.184) |
| Military Advantage | 0.599*** | 0.565*** | | | | |
| | (0.142) | (0.137) | | | | |
| Challenger's per capita GDP | | | −0.203*** | −0.260*** | | |
| | | | (0.070) | (0.068) | | |
| Target's per capita GDP | | | −0.289*** | −0.254*** | | |
| | | | (0.077) | (0.072) | | |
| Challenger's infant mortality | 0.238** | 0.468*** | −0.171 | 0.384** | 0.260** | 0.506*** |
| | (0.111) | (0.120) | (0.116) | (0.124) | (0.108) | (0.125) |
| Target's infant mortality | −0.108 | −0.167 | −0.266** | −0.324** | −0.138 | −0.207* |
| | (0.117) | (0.117) | (0.130) | (0.127) | (0.120) | (0.120) |
| Challenger's urbanization | 0.005 | 0.006* | 0.012*** | 0.014*** | 0.006* | 0.007** |
| | (0.004) | (0.004) | (0.004) | (0.004) | (0.003) | (0.003) |
| Target's urbanization | −0.007** | −0.007** | −0.001 | −0.001 | −0.009** | −0.008** |
| | (0.004) | (0.003) | (0.004) | (0.004) | (0.004) | (0.004) |
| Peace Years | −0.934*** | −0.906*** | −0.893*** | −0.901*** | −0.934*** | −0.903*** |
| | (0.036) | (0.041) | (0.036) | (0.041) | (0.036) | (0.041) |
| Decades fixed-effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Challenger's region fixed-effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Target's region fixed-effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| 2: Challenger's democracy equation (probit) | | | | | | |
| Challenger's Fertility | | −0.128*** | | −0.134*** | | −0.129*** |
| | | (0.042) | | (0.042) | | (0.043) |
| Challenger's per capita GDP | | | | 0.299*** | | |
| | | | | (0.065) | | |
| Challenger's infant mortality | | −0.627*** | | −0.469*** | | −0.626*** |
| | | (0.091) | | (0.105) | | (0.091) |
| Challenger's urbanization | | −0.003 | | −0.010*** | | −0.003 |
| | | (0.003) | | (0.003) | | (0.003) |
| Decade fixed-effects | | ✓ | | ✓ | | ✓ |
| Challenger's region fixed-effects | | ✓ | | ✓ | | ✓ |
| 3: Target's democracy equation (probit) | | | | | | |
| Target's Fertility | | −0.126*** | | −0.131*** | | −0.126*** |
| | | (0.043) | | (0.042) | | (0.043) |
| Target's per capita GDP | | | | 0.299*** | | |
| | | | | (0.065) | | |
| Target's infant mortality | | −0.631*** | | −0.473*** | | −0.630*** |
| | | (0.091) | | (0.105) | | (0.091) |
| Target's urbanization | | −0.003 | | −0.010*** | | −0.003 |
| | | (0.003) | | (0.003) | | (0.003) |
| Decade fixed-effects | | ✓ | | ✓ | | ✓ |
| Target's region fixed-effects | | ✓ | | ✓ | | ✓ |
| Correlation coefficients | | | | | | |
| $\rho_{12}$ | | −0.31*** | | −0.319*** | | −0.318*** |
| | | (0.098) | | (0.093) | | (0.095) |
| $\rho_{13}$ | | 0.063 | | 0.097 | | 0.083 |
| | | (0.089) | | (0.092) | | (0.084) |
| $\rho_{23}$ | | 0.101*** | | 0.127*** | | 0.101** |
| | | (0.039) | | (0.039) | | (0.043) |
| Number of observations | 34,312 | 34,312 | 34,312 | 34,312 | 34,312 | 34,312 |

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Standard errors (in parenthes) are clustered at directed-dyad level. Estimates for constant in each of the three equations are omitted for brevity.

## 3. Dropping China as a potential outlier

We estimate the models for a subset of the data that drops potential outliers. Specifically, we drop any country pairs that involve China either as a challenger or as a target. China's fertility rate may have been artificially low due to the "one-child" policy it implemented from 1979 through 2015. Figure A.4 replicates Figure 4 in the main text but with data that drop dyads containing China. Estimated coefficients for these models are presented in Table A.4. We continue to find the same results as previously in these analyses.

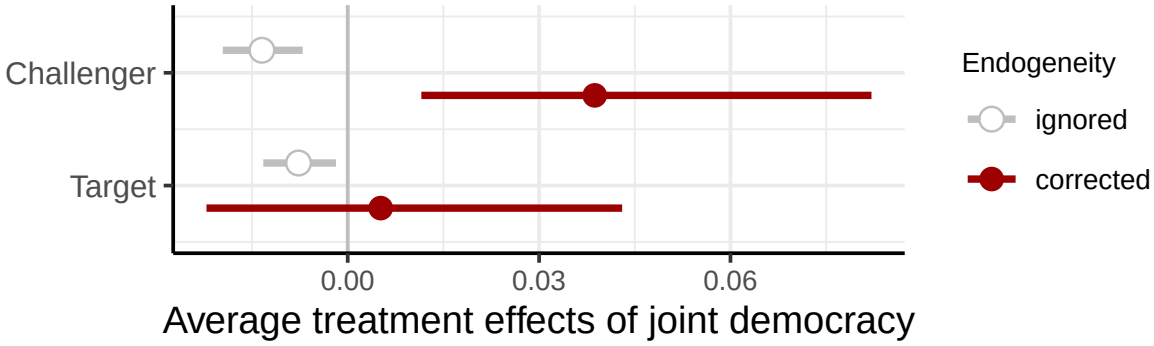


Figure A.4: This figure shows the estimated average treatment effects of democracy on militarized conflict, dropping observations involving China either as challenger or target.

Table A.4: Dropping China

| | Univariate | Trivariate |
|---|---|---|
| 1: Conflict equation (clog-log) | | |
| Challenger's Democracy | 0.146 | 1.251*** |
| | (0.146) | (0.359) |
| Target's Democracy | 0.375*** | 0.109 |
| | (0.113) | (0.395) |
| Joint Democracy | −0.784*** | −0.709*** |
| | (0.192) | (0.181) |
| Military Advantage | 0.632*** | 0.565*** |
| | (0.156) | (0.152) |
| Challenger's per capita GDP | −0.204*** | −0.264*** |
| | (0.070) | (0.068) |
| Target's per capita GDP | −0.288*** | −0.260*** |
| | (0.078) | (0.074) |
| Challenger's infant mortality | 0.166 | 0.382*** |
| | (0.124) | (0.126) |
| Target's infant mortality | −0.174 | −0.224* |
| | (0.136) | (0.133) |
| Challenger's urbanization | 0.011*** | 0.014*** |
| | (0.004) | (0.004) |
| Target's urbanization | 0.0004 | −0.0003 |
| | (0.004) | (0.004) |
| Peace Years | −0.935*** | −0.903*** |
| | (0.037) | (0.043) |
| Decades fixed-effects | ✓ | ✓ |
| Challenger's region fixed-effects | ✓ | ✓ |
| Target's region fixed-effects | ✓ | ✓ |
| 2: Challenger's democracy equation (probit) | | |
| Challenger's Fertility | | −0.193*** |
| | | (0.042) |
| Challenger's per capita GDP | | 0.318*** |
| | | (0.068) |
| Challenger's infant mortality | | −0.466*** |
| | | (0.108) |
| Challenger's urbanization | | −0.014*** |
| | | (0.003) |
| Decades fixed-effects | | ✓ |
| Challenger's region fixed-effects | | ✓ |
| 3: Target's democracy equation (probit) | | |
| Target's Fertility | | −0.191*** |
| | | (0.042) |
| Target's per capita GDP | | 0.318*** |
| | | (0.065) |
| Target's infant mortality | | −0.469*** |
| | | (0.109) |
| Target's urbanization | | −0.014*** |
| | | (0.003) |
| Decades fixed-effects | | ✓ |
| Target's region fixed-effects | | ✓ |
| Correlation coefficients | | |
| $\rho_{12}$ | | −0.317*** |
| | | (0.084) |
| $\rho_{13}$ | | 0.058 |
| | | (0.093) |
| $\rho_{23}$ | | 0.141*** |
| | | (0.040) |
| Number of observations | 33,156 | 33,156 |

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$
Standard errors (in parenthes) are clustered at directed-dyad level. Estimates for constant in each of the three equations are omitted for brevity.

## 4. Including dyads within the same geographic region

Our main analyses limit our focus to geographically contiguous dyads to reduce heterogeneity in observations. To assess the generalizability of our findings beyond the contiguous sample, we expand the sample to include those dyads where both sides belong to the same geographic region. We identify the following five geographic regions based on COW's country coding system: Western Hemisphere (COW code between 1 and 199), Europe (between 200 and 399), Africa (between 400 and 599), Middle East (between 600 and 699), and Asia (between 700 and 999). By including not only contiguous dyads but also intra-region dyads, the sample size expands from 34,312 to 199,168. Results from the estimation are summarized in Figure A.5 and Table A.5. Note that these effects are smaller in absolute size because conflict is even rarer in the expanded data than in the contiguous data. Aside from size, we continue to find consistent results from these expanded data.

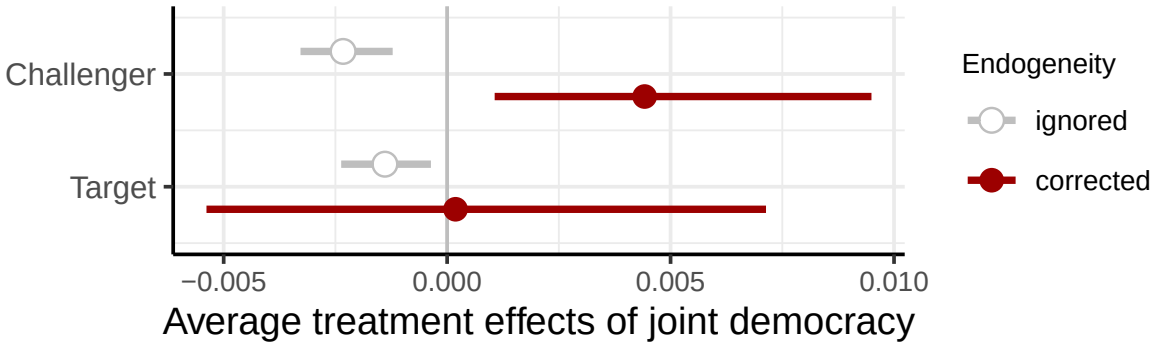


Figure A.5: This figure shows the estimated average treatment effects of democracy on militarized conflict calculated with the expanded data that include intra-region dyads.

Table A.5: Including intra-region dyads

| | Univariate | Trivariate |
|---|---|---|
| 1: Conflict equation (clog-log) | | |
| Challenger's Democracy | 0.069 | 0.821*** |
| | (0.129) | (0.307) |
| Target's Democracy | 0.253** | 0.025 |
| | (0.100) | (0.471) |
| Joint Democracy | −0.601*** | −0.570*** |
| | (0.158) | (0.157) |
| Contiguity | 2.622*** | 2.588*** |
| | (0.114) | (0.115) |
| Military Advantage | 0.592*** | 0.569*** |
| | (0.128) | (0.126) |
| Challenger's per capita GDP | −0.186*** | −0.209*** |
| | (0.064) | (0.063) |
| Target's per capita GDP | −0.298*** | −0.286*** |
| | (0.068) | (0.066) |
| Challenger's infant mortality | 0.047 | 0.230* |
| | (0.100) | (0.118) |
| Target's infant mortality | −0.247** | −0.297** |
| | (0.107) | (0.134) |
| Challenger's urbanization | 0.008** | 0.009*** |
| | (0.003) | (0.003) |
| Target's urbanization | 0.0003 | −0.0001 |
| | (0.003) | (0.003) |
| Peace Years | −0.936*** | −0.926*** |
| | (0.033) | (0.033) |
| Decades fixed-effects | ✓ | ✓ |
| Challenger's region fixed-effects | ✓ | ✓ |
| Target's region fixed-effects | ✓ | ✓ |
| 2: Challenger's democracy equation (probit) | | |
| Challenger's Fertility | | −0.158*** |
| | | (0.016) |
| Challenger's per capita GDP | | 0.179*** |
| | | (0.027) |
| Challenger's infant mortality | | −0.719*** |
| | | (0.045) |
| Challenger's urbanization | | −0.009*** |
| | | (0.001) |
| Decades fixed-effects | | ✓ |
| Challenger's region fixed-effects | | ✓ |
| 3: Target's democracy equation (probit) | | |
| Target's Fertility | | −0.158*** |
| | | (0.016) |
| Target's per capita GDP | | 0.179*** |
| | | (0.027) |
| Target's infant mortality | | −0.720*** |
| | | (0.045) |
| Target's urbanization | | −0.009*** |
| | | (0.001) |
| Decades fixed-effects | | ✓ |
| Target's region fixed-effects | | ✓ |
| Correlation coefficients | | |
| $\rho_{12}$ | | −0.198*** |
| | | (0.068) |
| $\rho_{13}$ | | 0.058 |
| | | (0.087) |
| $\rho_{23}$ | | 0.0006 |
| | | (0.015) |
| Number of observations | 199,168 | 199,168 |

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$
Standard errors (in parenthes) are clustered at directed-dyad level. Estimates for constant in each of the three equations are omitted for brevity.

# References

Filippou, Panagiota, et al. 2019. "A Trivariate Model for Estimating the Eects of Two Binary Endogenous Explanatory Variables: Application to the Impact of Medical Care Usage on Work Absenteeism."

Urdal, Henrik, and Primus Che Chi. 2013. "War and Gender Inequalities in Health: The Impact of Armed Conflict on Fertility and Maternal Mortality." *International Interactions* 39 (4): 489–510.