Population and Conflict (Acemoglu, Fergusson & Johnson, 2020)

Research Question

Does rapid population growth increase the risk of social conflict, especially via resource scarcity? The paper investigates this classic Malthusian hypothesis in a modern empirical context. The authors develop a simple theoretical model to formalize when population increases translate into conflict. In the model, output is produced with labor and a fixed factor (land/natural resources), implying diminishing returns to labor. As population N rises while land Z and technology A remain fixed, land becomes more scarce: wages fall and land rents rise. This makes controlling land more valuable. The model considers two groups competing for resources; a group's incentive to initiate conflict (e.g. seize land from the other group) increases with population because the payoff to capturing additional land is higher when population pressure is greater. In other words, high population *intensifies competition over scarce resources*, raising the likelihood of violent conflict – a mechanism rooted in Malthus (1798).

Importantly, the model also highlights conditions under which population growth would not fuel conflict. If population growth is accompanied by improvements in technology or institutions (i.e. A rises along with N), then output can keep pace and resource scarcity is alleviated. In such cases, a higher population need not lead to conflict. Likewise, in economies where fixed natural resources are less important (for instance, more industrialized settings), adding population may not provoke violence. Thus, the theoretical framework predicts that population surges will incite conflict only in a Malthusian scenario: when resource endowments are fixed and not offset by productivity gains. The research question, then, is tested empirically: *Does an exogenous increase in population lead to more social conflict in settings where resource scarcity becomes acute?* The authors specifically examine the mid-20th-century "Epidemiological Transition" – global health innovations that sharply raised life expectancy – as a natural experiment to answer this question. By linking health-driven population growth to subsequent conflict, they aim to identify a causal effect of population on conflict risk.

Estimation & Equations

Empirical Strategy: The paper employs an instrumental-variable approach (two-stage least squares, 2SLS) to estimate the causal impact of population growth on conflict. The key challenge is that population and conflict could be endogenous – for example, conflict itself can reduce population, and both could be influenced by other factors (governance, economic conditions, etc.). To isolate exogenous variation in population, the authors leverage the international epidemiological transition of the 1940s, when medical advances (e.g. antibiotics, vaccines, DDT for malaria) diffused globally. Countries with higher pre-intervention disease mortality experienced larger drops in mortality and thus larger rises in life expectancy and population, essentially as a *quasi-random shock* to population size. This idea builds on Acemoglu and Johnson (2007), who constructed a *predicted mortality* instrument using a country's 1940 prevalence of various infectious diseases interacted with global innovations timing:

$$MI_{it} = \sum_{d \in D} ((1 - I_{dt})M_{di40} + I_{dt}M_{dFt})$$

where M_{di40} denotes mortality in 1940 (measured as number of deaths per 100 individuals per annum) for country i from disease $d \in D$; I_{dt} is a dummy for intervention for disease d that takes the value of 1 for all dates after the intervention; M_{dFt} is mortality from disease d at the health frontier of the world at time t; and D is the set of 13 diseases used.

The instrument (MI_{it}) forecasts how much mortality *should* fall in country *i* after 1940 due to global medical progress, independent of country *i*'s own actions. Crucially, this predicted mortality decline is plausibly unrelated to country-specific political or social trends, satisfying the exogeneity requirement.

In the first-stage regression, log population is regressed on predicted mortality (plus fixed effects and controls).

Formally, for country *i* at time *t*, the authors estimate:

$$x_{it} = \phi MI_{it} + \varsigma_i + \mu_t + Z_{it}\beta + u_{it}$$

where x_{it} is log population, MI_{it} is the predicted mortality instrument, ς_i are country fixed effects, μ_t are year (decade) fixed effects, and Z_{it} is a vector of other controls. The fixed effects absorb any time-invariant country traits and global time trends, so identification comes from differential mortality shocks across countries. The first-stage results confirm that the instrument is strong: countries with higher 1940 predicted mortality saw significantly larger population increases after 1940. Falsification tests show no correlation between predicted mortality and pre-1940 population trends or conflict, supporting the instrument's validity. The exclusion restriction is that mortality shocks affect conflict only via population changes, not through other channels. The authors address potential violations by checking, for instance, that the instrument's effect is not driven by altered age structure or education – they include controls for youth population share and find the results robust. They also test whether the findings are distorted by World War II by excluding countries most affected by the war, reassigning postwar conflict events to the 1940s, and dropping the core war years entirely. In all three cases, the results remain consistent, reinforcing the validity of the identification strategy.

The second-stage regression estimates the impact of population on conflict using the instrument. The main specification is:

$$c_{it} = \pi x_{it} + \varsigma_i + \mu_t + Z_{it}\beta + \varepsilon_{it}$$

where c_{it} is a measure of conflict in country *i* at reference year *t*, x_{it} is log population, and ς_i, μ_t are country and time fixed effects. In practice, the authors use decade-level data. They consider "fraction of the decade in conflict" as a primary outcome – i.e. the proportion of years in a given decade that the country experienced internal conflict or civil war (from international conflict datasets). For example, an observation for t=1980 corresponds to conflict incidence in the 1980s, and t=1940 corresponds to the 1940s. Thus, one approach is a long-difference from 1940 to 1980 (comparing conflict in the 1980s vs 1940s, and population in 1980 vs 1940). In that case, the formula above simplifies to a difference-in-difference regression of change in conflict on change in log population. They also estimate a panel with intermediate decades (1950s, 1960s, etc.), including all decade observations and fixed effects. Additional controls Z_{it} include factors like baseline GDP, colonial status, and regional trends to ensure results are not driven by omitted heterogeneity. Overall, the 2SLS setup uses the predicted mortality instrument (first equation) to instrument population in the conflict equation (second equation). This empirical design aims to *causally identify* the effect of population growth on conflict risk.

Within-Country Analysis (Mexico): In addition to the cross-country analysis, the paper presents a complementary study within a single country (Mexico) to bolster the mechanism. Mexico in the mid-20th century undertook anti-malaria campaigns that differentially affected regions due to varying malaria ecology. Some municipalities, owing to climate, were highly suitable for malaria transmission, so they benefited greatly from eradication efforts (a big mortality decline and subsequent population boom), whereas others saw little change. The authors exploit this as another source of quasi-experimental variation. They construct a malaria-suitability-based instrument for local population growth: essentially, an interaction of baseline malaria suitability with a post-campaign period indicator (a difference-in-differences IV). The specification mirrors the country-level model: they use a panel of Mexican municipalities (with municipality fixed effects and year fixed effects) and instrument the change in population with the predicted mortality decline from malaria eradication. The outcome is social conflict in the 1960s measured by counts of protests, riots, and other disturbances from newspaper archives. By focusing on local protests (often over land, wages, etc.), this case captures lower-level conflict that might not register as a civil war nationally. The rationale is to see if areas with larger population increases (due to improved health) experienced more social unrest, consistent with heightened resource competition. The inclusion of the Mexico study also helps address data limitations of cross-country conflict measures (which often miss smaller scale conflicts). Across both the international and Mexico analyses, the empirical design is structured to test the same core relationship: whether exogenous population growth leads to more conflict, with equations analogous to the two previous equations applied at different scales.

Results and Conclusions

The paper finds strong evidence that rapid population growth *caused* a rise in social conflict in the mid-20th century, consistent with the Malthusian mechanism. This result holds in both the cross-country analysis and the within-Mexico case study:

- **Cross-Country Evidence:** Countries that experienced larger medically-driven increases in population saw significantly more conflict in the subsequent decades. The 2SLS estimates show a positive and sizable effect of population on conflict. For example, in the baseline long-difference specification (1940s to 1980s), the coefficient on log population is around 0.6-1.0 (depending on controls), implying that a 10% larger population leads to roughly 0.62 more years of conflicts in the 1980s relative to the 1940s. The authors verify this result across multiple conflict metrics: whether conflict is measured as an indicator of civil war years, the proportion of years in conflict, or log battle deaths per capita, the population surge is associated with more violence. This robustness across alternative definitions of "conflict" confirms it's not an artifact of one particular dataset. Moreover, the finding is not driven by pre-existing trends or baseline differences: countries with high disease mortality (the instrument) were not already on diverging conflict trajectories before 1940, and controlling for baseline income, colonial status, etc. does not eliminate the effect.

- **Nature of conflict – Resource competition:** The evidence points to resource scarcity as the channel by which population growth provoked conflict. The rise in violence largely took the form of conflicts over land and resources. In the international data, the authors distinguish conflicts related to natural resources (e.g. territorial conflicts, uprisings over land distribution or resource access) from other conflicts (ideological, ethnic, etc.). They found that the population increase led to a disproportionate jump in resource-related conflicts, whereas conflicts unrelated to resource pressure did not increase as much. Similarly, in Mexico, the municipalities with greater population growth saw more frequent protests over resources, such as land disputes and peasant uprisings, rather than just political demonstrations. The authors explicitly coded newspaper reports to identify protests linked to resource issues (land, water, etc.), and those incidents drive the results. This consistency reinforces that it was scarcity of fixed resources, brought on by a sudden population boom, that fueled the unrest.

- **Within-Mexico Evidence:** The subnational analysis in Mexico confirms the cross-country findings. Regions with higher malaria prevalence experienced larger population growth after eradication efforts from 1940 to 1960, leading to significantly more social conflict in the 1960s, including violent protests and other forms of instability like riots and demonstrations. Quantitatively, areas more prone to malaria grew faster: from 1940 to 1960, municipalities above the median for malaria suitability recorded an average log-population increase of 0.46, compared with 0.36 in less-suitable areas. The 2SLS results show a positive, significant effect of population on local conflict, matching national findings. This approach strengthens the result by comparing communities within the same institutional context and captures "softer" forms of social instability. Although Mexico had no civil war during this period, local data highlight significant conflict in rapidly growing areas, illustrating limitations of standard conflict measures and the importance of multi-scale analysis.

- **Economic Conditions:** An important nuance in the results is that the conflict-inducing effect of population growth was more pronounced during economic hardship. Cross-country data shows that countries with poor economic growth in the decades after 1940 experienced more conflict following a population boom, whereas countries with strong GDP per capita growth handled demographic pressures better (consistent with higher A (technology) mitigating the Malthusian trap). Similarly, in Mexico, the population-driven conflicts spiked during periods of drought, as reduced crop yields worsened resource scarcity in rapidly growing communities. Violent protests surged significantly more in drought-affected municipalities with higher population increases compared to normal rainfall periods. This interaction suggests that population pressure plus an adverse shock (economic downturn or drought) is a combustible combination: a booming population raises the baseline risk of conflict, but whether that tension ignites may depend on short-term conditions (e.g. bad harvests, economic recessions). The results support the model's claim that conflict arises when multiple factors jointly exacerbate resource scarcity, especially when demographic growth surpasses economic capacity.

- **Dynamics Over Time:** The paper explores how the population–conflict relationship evolved over the late 20th century, finding that the effect emerged gradually and diminished somewhat in recent decades. Including data from the 1990s and 2000s, the estimated impact of population on conflict is still positive but smaller. One reason is that the early effect (1940s–1980s) had a delayed onset – conflicts often lagged the initial population shock (e.g., children born in the 1940s contributing to instability in the 1960s and 1970s). By the 1990s and 2000s, many developing countries had undergone demographic transitions (slowing population growth) and some improvements in institutions, potentially weakening the Malthusian link. Additionally, post-Cold War changes altered conflict dynamics: many late-century conflicts were driven by superpower interventions or ideological struggles, masking population pressures. Indeed, they find the population effect remained stronger in countries that had heavy U.S./Soviet involvement (proxying ongoing Cold War-fueled conflicts), suggesting that the *population-conflict nexus is context-dependent*.

In sum, the empirical results robustly support the theoretical prediction: rapid population growth, in the absence of commensurate gains in resources or productivity, led to a higher incidence of social conflict. This holds true across different measures of conflict and at both international and local levels, with evidence pointing to resource scarcity as the primary driver of the violence. The paper does not claim that population growth inexorably causes conflict in all circumstances. Rather, it reinforces a conditional narrative: population growth is problematic when unaccompanied by adequate gains in productivity or strong institutions. In fact, rising life expectancy or birth rates can put significant pressure on governments to provide economic opportunities and access to resources. However, when population growth is paired with technological progress this pressure can be offset, preventing instability and conflicts.

Limits and Further Analysis

While the paper makes a strong case, there are several limitations and considerations for further analysis:

- **Data Limitations and Sample Bias:** The cross-country analysis relies on historical disease mortality data that were only available for a subset of countries (mostly outside sub-Saharan Africa) . This omission is significant, given that many post-1960 conflicts occurred in sub-Saharan Africa, raising concerns about sample bias. The authors acknowledge this as an “important constraint”. But this leads to questioning whether the results generalize globally, or if they primarily reflect the experience of Latin American, Asian, and North African countries with available data. Finally, while the authors treat the global diffusion of medical technology as exogenous, colonial ties, Cold-War alignments, or proximity to trade routes could have influenced adoption speed, posing a remaining identification concern.

  Scope of the Time Period: The study focuses on the 1940–1980 period, a time of unprecedented global mortality decline and population boom, questioning whether the findings are specific to mid-20th-century conditions. For example, the Green Revolution and economic globalization in later decades may have relieved some Malthusian pressures, meaning population increases after 1980 might not generate the same conflict outcomes. The authors themselves note that population surges tend to cause conflict and resource competition chiefly when they are “unaccompanied by productivity growth and unmediated by strong institutions.” This implies that policy responses, technological progress in agriculture, industrial development, or improved governance, can decouple population growth from conflict. Critics of a purely Malthusian interpretation stress that the study’s historical result may not hold in eras or regions where institutions and technology adjusted to rising population. Therefore, the relevance of the paper’s findings may be limited in today’s world, where settings resembling those in the study are less common.

- **Mexico Case study**: The Mexico case study provides micro-level evidence using malaria eradication campaigns as a source of localized variation. Using one country’s subnational data helps bolster the causal case, it shows that even within a single country, areas with bigger population jumps (due to public health improvements) saw more social conflict, consistent with the global result. However, Mexico in the 1960s might be a unique setting: a semi-authoritarian regime with growing social movements and an unequal land distribution. There is a concern that results from one country (even if consistent with the theory) may not generalize to others with different socio-political contexts. At the subnational level, conflict is proxied by protest and disturbance counts from newspaper archives. Scholars have raised concerns about potential measurement error or bias here: media-based conflict counts might under-report events in more remote or censored areas and overrepresent urban protests. In addition to this, in 1960s Mexico saw a gradual opening of political space after decades of one-party rule, the recorded surge in protests may partly reflect greater press freedom and mobilization, rather than a pure population-driven effect. Critics suggest caution in attributing causality to population growth alone when the data-generating process for conflict events could be influenced by contemporaneous political changes or reporting biases. In short, the social conflict measures used (newspaper-coded protests and riots) might capture broader political trends in Mexico, not just resource-driven unrest. Moreover, the malaria suitability is intertwined with climatic factors that also determine agricultural yields and, by extension, protest behavior; even with municipality fixed effects, time-varying droughts mechanically interact with suitability, making it difficult to isolate the impact of population growth.

- **Exclusion Restriction Concerns for the IV**: Countries that experienced large post-1940 mortality declines typically began with worse health, weaker institutions, and lower levels of development. These baseline differences may themselves be predictors of future conflict. The supposed "natural experiment" relies on variation that may be endogenous to long-standing developmental paths. If, for instance, poor health in 1940 reflected weak colonial governance or geographic disadvantage, and if those features continued to shape conflict risk later, then the instrument risks capturing these persistent structural traits, not just exogenous population growth. Acemoglu et al. try to address this by controlling for initial income, political independence, and institutional quality, and by including time interactions with those variables. They also show that conflict patterns before 1940 are unrelated to predicted mortality, reinforcing the exogeneity claim. Nonetheless, external critics emphasize that no set of controls can perfectly account for unobservable preconditions. Because initial disease environments correlate with deep, path-dependent differences in development, the instrument may still reflect more than the causal effect of population size.

- **Validity of the Mortality-Based Instrument**: Beyond the exclusion restriction, scholars have discussed the broader validity and generalizability of using global disease-control shocks as an instrument. One point is that this strategy leverages a unique historical episode, the mid-20th century epidemiological transition, which may not be repeatable or applicable outside that context. The instrument effectively compares countries on the basis of their disease environments and how much benefit they could reap from 1940s medicine. This might bundle together other post-WWII changes: for example, decolonization and nation-building occurred in many high-mortality countries after 1940, possibly affecting conflict risk. If those processes coincided with the health interventions, it complicates the IV interpretation. The authors try to mitigate such concerns by controlling for whether a country was colonized or independent in 1940 and by including region fixed effects, but subtle post-war dynamics (like the intensity of Cold War alignments or varying aid flows) could still bias results if correlated with initial disease burdens.

- **Policy Implications and Further Research:** The paper's findings carry important policy implications that merit further analysis. One implication is the significance of managing resource scarcity in tandem with demographic changes. Governments facing rapid population increases (for instance, due to public health improvements or declining infant mortality) should anticipate stresses on land, food, water, and employment. Policy measures could include investing in agricultural productivity, infrastructure, or family planning to ease the Malthusian pressure. Future research could evaluate case studies where such measures were taken (or not taken) to see if conflict outcomes differed. Another avenue is exploring how international aid or climate change factor into this nexus. Climate change is essentially another stress on resources (droughts, etc., similar to those mentioned in the Mexico analysis) and is happening alongside population growth in parts of the world. Investigating whether current climate variability exacerbates the population–conflict relationship would be valuable. Lastly, the research could be extended to consider migration as a safety valve or spark: high population pressure might lead to migration (internal or external) which might reduce conflict by relieving pressure, or simply relocate tensions.

In summary, while *"Population and Conflict"* answers a crucial question with rigorous evidence, it also opens several new questions about how societies can navigate demographic changes peacefully. The work stands as a compelling confirmation that Malthusian dynamics are real, but it also invites scholars and policymakers to explore how humanity can avoid the worst outcomes as our numbers grow.

Bibliography

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