

Statement of Research Interests

Filipe Campante

My research is in the field of Political Economics, and concerned with understanding what underpins the variations we observe in the quality of governance. One central idea in that regard is that of “checks and balances”: political leaders must be constrained in their power, so as not to abuse it. I start from the belief that we can only understand good governance if we go beyond formal checks and balances, and study the informal constraints that bear on those leaders. Put simply, the degree to which they can get away with abusing their power depends on factors such as: How likely is it that people will take to the streets? If they do, how much of a threat do they pose? How much do they know about what the leaders are doing? How much do they care? All of these are especially important – though far from exclusively so – in the context of developing countries, where formal checks are relatively weak.

To provide insight into such questions, I employ a macro/applied microeconomic approach: I am interested in aggregate, “macro” questions – what happens to countries, or states, or cities? – but with a focus on the identification of causal effects that requires quasi-experimental, “applied micro” empirical methods. I strongly believe that this is the best way to maximize the potential for policy impact, by combining large-scale questions with credible evidence.

I have applied this approach to three separate but overlapping domains, which broadly match the three possible “fundamental causes” of economic and political development: A) **geography**, B) **culture**, and C) **institutions**. My perspective is to move these domains away from the broad context in which they are often pitted against one another, and instead bring them closer to a policy-actionable space.

A. GEOGRAPHY: SPATIAL DISTRIBUTION OF PEOPLE AND ECONOMIC ACTIVITY

It has become fashionable to speak of the “death of distance,” but a long-standing tradition in the social sciences – for instance, in urban economics and economic geography – has highlighted the importance of physical proximity in driving human interactions. I have strived to bring those insights into political economics. To bring geography closer to policy, my research emphasizes not its long-run, perennial aspects, but rather how one such element, namely physical distances, interacts with technology and policy decisions, in ways that affect development.

a) The Importance of Capital Cities

I start from the recognition that where people are located in space matters for how they engage with the political process. To that I add the insight that not all places are equal, as power is not distributed uniformly over space. This means that it matters where people are located relative to the center of political power, i.e. the capital city.

My research has established the first rigorous evidence on the topic. In “**Isolated Capital Cities, Accountability, and Corruption: Evidence from US States**” (*American Economic Review*, with Quoc-Anh Do), we find a causal impact of

isolated capital cities on corruption across US states, using the historical debate about the location of capitals in the US to come up with plausibly exogenous variation in the distribution of people around the capital. We also show that this works through different accountability mechanisms: media coverage and voter behavior. I have also studied how this matters for development. In “**Capital Cities, Conflict, and Misgovernance**” (*R&R American Economic Journal: Applied Economics*, joint with Quoc-Anh Do and Bernardo Guimarães), we argue that distance to the capital matters for how effective the threat of violent removal is. We find that our model’s predictions are borne out in the data outside of consolidated democracies: conflict over government is more likely closer to the capital city, and becomes more likely when the capital is moved closer. In addition, isolated capitals are associated with worse quality of governance, as incumbents have less to fear and are thus less accountable. The next, ongoing step is to look at the impact of distance from the capital at the local level. With Samuel Bazzi, Quoc-Anh Do, and Matt Gudgeon, we plan to exploit the experiment of redistricting in Indonesia, which entailed changes in capital cities for the newly created districts, to understand how that distance affects public good provision and conflict.

While capital city changes are a relatively extreme example, this illustrates the way in which my research brings the issue of geography and development closer to the realm of policy interventions. It helps flag contexts and situations with a higher risk of misgovernance, and highlights how geography, politics, and media can interact to influence that risk.

b) The Impact of Globalization

One key aspect that makes the current age of globalization unique is that it is now possible for people to travel back and forth across long distances, thus increasing by orders of magnitude the opportunities for face-to-face contact. This is the direct result of long-distance air travel. Based on the premise that such contact is key for human interactions, it is natural to think that this could have profound implications for economic and political development, as well as the evolution of culture.

The first step in the broad agenda motivated by this view is in “**Long-Range Growth: Economic Development in the Global Network of Air Links**” (*R&R Quarterly Journal of Economics*, joint with David Yanagizawa-Drott). In this paper, we tackle the endogeneity of air links by exploiting a discontinuous jump in the likelihood of two places being connected, at 6000 miles of distance, due to regulations on ultra-long-haul. We find a positive effect of long-distance air links on local economic activity, and show it is not merely due to spatial reallocation. We also estimate a causal impact on foreign direct investment (in particular, ownership links between businesses), with connected cities substantially more like to have such links. This provides evidence that the movement of people fosters the movement of capital, even though there is no technological reason why the latter would require flights.

This research has direct policy implications for local development policy: establishing air links stimulates businesses and economic activity. However, our

findings add considerable nuance to that basic prescription, particularly in how they relate to the links between globalization and inequality. First, policy makers would have to weigh the effects on inequality at the local level. Second, but no less important, we find that the benefits of integration come only to places that were sufficiently wealthy to begin with: if a place is too poor, getting a good shock in terms of potential connections does not translate into more flights. This underscores that globalization, in its aerial dimension, can be a force of convergence for middle-income countries, but that low-income countries got left behind.

We intend to push this agenda further by looking at the political and cultural effects of globalization, an area where we can bring credible empirical evidence where it is sorely lacking. We are interested in how it may affect the prevalence of political turmoil: our preliminary results indicate that different forms (civil war, protest, terrorism) are indeed reduced near cities that get connected, with no effect in more distant areas. We are also interested in how exposure to different people across long distances may affect views and attitudes: for instance, does that make people more likely to hold, say, cosmopolitan or liberal values? Does that depend on whom you get exposed to? We already have preliminary results indicating an impact on attitudes towards gender, and on the prevalence of gender bias in education.

B. CULTURE: THE EVOLUTION AND IMPACT OF CULTURAL TRAITS

The second broad strand of my research addresses culture, again with the idea of bringing this domain closer to policy action. As such, rather than focusing on the long-run evolution and determinants of cultural views and attitudes, I am instead interested in how they may change in a policy-relevant horizon, or how they can interact with a changing environment in affecting the policy options available.

a) Religion

One ubiquitous aspect of culture is religion, and all religions involve prescribed practices that can be economically costly. If substantial costs exist, we must understand what explains the existence and persistence of the practices. A long-standing literature in economics and beyond has addressed the topic, but it is hard to provide convincing empirical evidence on whether there is an aggregate economic impact, and on the potential mechanisms for persistence.

In “**Does Religion Affect Economic Growth and Happiness? Evidence from Ramadan**” (*Quarterly Journal of Economics*, with David Yanagizawa-Drott), we take advantage of a natural experiment induced by a specific religious rule: fasting during the Islamic month of Ramadan. We find that the strictness of Ramadan fasting has a significant negative impact on economic growth in Muslim countries, but a positive impact on subjective well-being. In the process of unpacking these findings, we then provide direct evidence for the “club good” model of costly religious practices, which posits that the latter can screen away less committed practitioners, thereby reducing free-riding and increasing welfare for the group.

These findings also have practical implications. First, they establish an instance where cultural practices affect economic outcomes, but also individual perceptions of well-being. This illustrates how culture can condition the environment in which policy makers operate, in ways that fluctuate and operate in the short run, and not just as a slow-moving backdrop. Just as important, we find that standard measures of economic welfare (e.g. GDP) and subjective perceptions of well-being can move in opposite directions. This substantiates the recent calls for including more expansive measures that can complement the traditional ones in evaluating policy.

b) Cultural Attitudes, the Economy, and Political Protest

If culture affects the economic environment and perceptions of well-being that could underpin the prevalence of discontent, we need to ask how these elements get translated into political action to constrain policy makers. In **“Schooling, Political Participation, and the Economy”** (*Review of Economics and Statistics*, lead article, with Davin Chor), we showed that the operation of one crucial determinant of political participation – namely, education – varies with the economic environment: when the relative abundance of different factors of production is such that human capital is particularly valuable in market activities, the link between education and political activity is weaker. In **“Why was the Arab World Poised for Revolution? Schooling, Economic Opportunities, and the Arab Spring”** (*Journal of Economic Perspectives*, with Davin Chor) and **“The People Want the Fall of the Regime: Schooling, Political Protest, and the Economy”** (*Journal of Comparative Economics*, with Davin Chor) we show that this can be used to understand the Arab Spring, as many of the affected countries had combined remarkable growth in schooling and a dearth of opportunities in labor markets. The next step in this agenda is to bring cultural attitudes fully into the picture, studying how views that could affect the propensity for protest can affect and be affected by the economic environment. Specifically, we have turned our attention to attitudes towards obedience, and how they affect and are affected by patterns of specialization, in ways that could lead to a development “obedience trap.”

c) Intergenerational Transmission of Cultural Attitudes

The idea that cultural attitudes constrain policy becomes that much more relevant if we take them not as shaped only by very-long-run forces, but rather as potentially evolving over shorter time horizons as well. In particular, it seems plausible that policy itself could affect views and beliefs, in ways that then get passed on via mechanisms of cultural transmission, and that might generate policy persistence.

In **“The Intergenerational Transmission of War”** (*R&R Journal of the European Economic Association*, with David Yanagizawa-Drott), we ask whether war service by one generation causally affects the likelihood of the next generation serving in war, using variation induced by the fact that the likelihood of service peaks at age 21. We estimate a positive impact on wartime service, but a negative one on peacetime service. We also find evidence consistent with a simple model of cultural transmission. However, outside of war, the positive impact of father’s service on the sons’ educational opportunities implies that service is less likely – it takes war, in this case, to translate culture into action.

C. INSTITUTIONS: TRANSPARENCY AND ACCOUNTABILITY

In addition to the formal institutions that are what constitutes political systems there are other institutional features that operate alongside them, and condition the performance of those institutions. In particular, they affect the degree of transparency and accountability under which policy makers have to work.

a) The Political Impact of Changes in Media Technology:

One prominent such institution is the media. I study how the introduction of new media technologies affects the behavior of politicians and citizens. In **“Media and Polarization: Evidence from the Introduction of Broadcast TV in the US”** (*Journal of Public Economics*, with Daniel Hojman), we exploit idiosyncratic variation in the diffusion of broadcast radio and TV in the US to show that it decreased legislative polarization. In **“Politics 2.0: The Multifaceted Effect of Broadband Internet on Political Participation”** (*Journal of the European Economic Association*, with Ruben Durante and Francesco Sobbrío), we look at the diffusion of broadband Internet in Italy, using variation induced by technological features. We find that it initially decreased turnout, but fostered online participation that eventually fed back into elections, and reversed that initial decline. I am now working on research focused on the recent explosion in social media and its political impact in the low-accountability context of Brazil. We have obtained data on the diffusion of 3G cell phone networks – the main tool of social media access in Brazil – and matched that to self-gathered data to analyze the online and offline behavior of politicians.

b) Corruption

I have also looked at how we can flag situations where corruption is more likely to emerge, and at the distortions that it entails when it comes to policy. The first topic is one of the central concerns of my aforementioned research on the role of geography and capital cities. Another contribution is **“Instability and the Incentives for Corruption”** (*Economics & Politics*, with Davin Chor and Quoc-Anh Do), where we find an empirical pattern whereby perceived corruption tends to be relatively high both in very stable and very unstable settings, and explain it based on the prevalence of different kinds of corruption. In **“Why Is Fiscal Policy Often Procyclical?”** (*Journal of the European Economic Association*, with Alberto Alesina and Guido Tabellini), we argue that inefficient procyclical fiscal policy can be explained by voters who think corrupt politicians will squander rainy day funds, and show that the data are consistent with that explanation.

This work has clear policy implications, for instance, with respect to the desirability of both the possibility of reelection and the imposition of term limits. On the other hand, it shows how institutional mechanisms of accountability can have unintended consequences in settings where other constraints are weak.

c) Money in Politics

Last but certainly not least, my research has looked into another key institutional feature potentially affecting accountability: the role of money. In **“Redistribution in a Model of Voting and Campaign Contributions”** (*Journal of Public Economics*), I show that policy is biased in favor of the relatively wealthy even when contributions are small, and use that to explain the key puzzle of why inequality does not lead to more redistribution. In **“Inefficient Lobbying, Populism, and Oligarchy”** (*Journal of Public Economics*, with Francisco Ferreira), we study lobbying, and show that, when it competes for real resources, it will tend to bias policy outcomes in favor of the less efficient groups. This helps us understand inefficient “oligarchic” policies (if the rich happen to be relatively less efficient) but also inefficient “populist” ones. In sum, my research on the topic shows that the inefficiencies that may arise from having money play a political role can take different forms, many of them surprising, depending on the specifics of that role, and of the economic environment.

In sum, my research has contributed to enhancing our understanding of different dimensions and underpinnings of governance. Its impact can be illustrated by around 1400 citations (measured by Google Scholar as of November 2016) and, beyond academia, multiple mentions in many of the most prestigious media outlets in the world, such as the *New York Times* (including authoring three op-ed pieces), *Washington Post*, NPR, *The Economist*, *The Guardian*, *Foreign Affairs*, *Foreign Policy*, *Science* among many others.