

Melissa Dell: Winner of the 2020 Clark Medal

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The 2020 John Bates Clark Medal of the American Economic Association was awarded to Melissa Dell, professor of economics at Harvard University, for her path-breaking contributions in political economy, economic history, and economic development. Her work is distinguished and admired for its combination of painstaking data collection, careful empirical implementation, and audacious ambition.

Melissa is a native of Enid, Oklahoma, a relatively low-income rural community with a population of about 50,000. She was the first member of her family to go to college, and also the first person from her high school, the Oklahoma Bible Academy, to study at Harvard, where she began her undergraduate studies in 2001. She started in the Social Studies program at Harvard but soon realized that the central questions that interested her necessitated a more systematic empirical analysis and switched to the Department of Economics. Her performance as an undergraduate was beyond impressive: from the Harvard economics department, she was awarded the John Williams prize for the best student and the Seymour Harris prize for the best thesis. From outside Harvard, she was awarded a Truman Fellowship and a Rhodes Scholarship, which she used to complete a master's degree in economics at Oxford. She continued her studies with a PhD in economics at MIT (where I was fortunate to be her advisor).

In this essay, I will attempt to put Melissa's achievements and research in the broader context of what I am going to call *the fundamental questions of long-run economic*

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Melissa Dell

development: Why are some countries or regions so much richer than others? Why is there so much inequality? Why has growth since the middle of the 18th century been so unequal, and why did it start taking off in the second half of the 18th and early 19th centuries in some parts of the world but not others? Furthermore, what do these outcomes have to do with democracy, political participation, and constitutions or a lack thereof?

These fundamental questions have fascinated and energized not just economists but a wide array of social scientists, historians, and philosophers. Many aspiring economists (and I count my younger, 18 year-old self among their ranks) are attracted to economics by these questions. Yet many young economists are soon discouraged from delving into these issues; sometimes they are told that these questions are too broad or too big for careful empirical work, and sometimes they are even told that the big questions fall outside the boundaries of economics.

However, Melissa's work has broadened and illuminated the way we think about the fundamental questions. She has used state-of-the-art microeconomic techniques, like regression discontinuity approaches, to produce more credible evidence on the role of institutions and historical factors in the fundamental questions of long-run economic development. Instead of comparing countries that differ in many dimensions, she has shown how it is possible to focus on more granular variation, such as between villages or municipalities. It turns out that zeroing in on specific settings not only makes causal inference more straightforward, but it also allows a much deeper understanding of the economic impacts of institutions, how institutions persist or atrophy, and how various dimensions of institutions may work very differently depending on the historical and social context. For example, what were the effects of the labor coercion as practiced by Spanish colonialists in

the Andes region for mining? What were the long-lasting consequences of Mexican revolution, which seem to have arisen from the policies used to de-escalate that conflict? Why did the sugar cultivation system imposed by Dutch colonists in Java have unexpected sectoral effects? How did the way in which local states took shape in Vietnam influence the long-run development of different parts of the country?

I begin with a few words of background on the fundamental questions of long-run economic development and then take a closer look at some of Melissa's most prominent work concerning these topics and others in three broad areas: 1) colonial history, institutions, and modern development; 2) conflict, law enforcement, and politics; 3) other research including climate change and economic growth and new methods for data extraction. Throughout, I refer to her key papers by number as listed in Table 1.

Background: Fundamental Questions of Long-Run Economic Development

Most of the modern work in the fields of economic growth and development economics does not seek to address the fundamental questions of long-run economic development. Researchers in economic growth have typically worked in a framework focused on changes in physical capital, human capital, and technology, while looking at topics like “how does capital or human capital accumulate” or “how can we model endogenous and sustained technological change.” Development economists have focused on health conditions, schooling, imperfect credit markets, discrimination, and other issues critical for the lives of the poor in the developing world. But neither line of research looks at how historical and institutional factors shape political and economic trajectories and divergences.

Of course, there has always been a tradition in economics that was motivated by and tried to grapple with these fundamental questions. After all, the full title of Adam Smith's (1776) magnum opus is *An Inquiry into the Nature and Causes of the Wealth of Nations*, and Smith situated his analysis firmly in its institutional context. Marx (1867), too, had an “institutional” theory of divergence, first linking much of the potential of an economy as well as the nature of the inequalities that it created in particular to the ownership of the means of production. This “mode of production” then shaped other aspects of society, including its political and social system, and determined economic growth via its impact on the rate of capital accumulation and technological change.¹ Many 20th-century economists who focused on long-run economic development and institutions were influenced—or in some

¹Marx did at times sound like a technological determinist, linking the mode of production and all else to technology: for example, he asserted in *The Poverty of Philosophy* that “the hand-mill gives you society with the feudal lord, the steam-mill society with the industrial capitalist” (1847 [1920], p. 49). But Marx's thinking was often more nuanced, recognizing the autonomous role of institutions, perhaps most distinctively in his *18th Brumaire of Louis Napoleon* (Marx 1852).

Table 1

Selected Papers by Melissa Dell

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1. “The Persistent Effects of Peru’s Mining Mita.” 2010. *Econometrica* 78 (6): 1863–1903.
 2. “Productivity Differences between and within Countries” (with Daron Acemoglu). 2010. *American Economic Journal: Macroeconomics* 2 (1): 169–88.
 3. “Path Dependence in Development: Evidence from the Mexican Revolution.” MIT Working Paper.
 4. “The Historical State, Local Collective Action, and Economic Development in Vietnam” (with Nathan Lane and Pablo Querubín). 2018. *Econometrica* 86 (6): 2083–2121.
 5. “The Development Effects of the Extractive Colonial Economy: The Dutch Cultivation System in Java” (with Benjamin Olken). 2020. *Review of Economic Studies* 87 (1): 164–203.
 6. “Trafficking Networks and the Mexican Drug War.” 2015. *American Economic Review* 105 (6): 1738–79.
 7. “The Violent Consequences of Trade-Induced Worker Displacement in Mexico” (with Benjamin Feigenberg and Kensuke Teshima). 2019. *American Economic Review: Insights* 1 (1): 43–58.
 8. “Nation Building Through Foreign Intervention: Evidence from Discontinuities in Military Strategies” (with Pablo Querubín). 2018. *Quarterly Journal of Economics* 133 (2): 701–64.
 9. “Temperature and Income: Reconciling New Cross-Sectional and Panel Estimates” (with Benjamin F. Jones and Benjamin A. Olken). 2009. *American Economic Review* 99 (2): 198–204.
 10. “Temperature Shocks and Economic Growth: Evidence from the Last Half Century” (with Benjamin F. Jones and Benjamin A. Olken). 2012. *American Economic Journal: Macroeconomics* 4 (3): 66–95.
 11. “What Do We Learn from the Weather? The New Climate-Economy Literature.” (with Benjamin F. Jones and Benjamin A. Olken). 2014. *Journal of Economic Literature* 52 (3): 740–98.
 12. “Information Extraction from Text Regions with Complex Tabular Structure” (with Kaixuan Zhang, Zejiang Shen, and Jie Zhou). 2019. In *33rd Conference on Neural Information Processing Systems*. Vancouver, Canada.
 13. “OLALA: Object-Level Active Learning Based Layout Annotation.” (with Zejiang Shen, Jian Zhao, Yaoliang Yu, and Weining Li). 2020. Working Paper.
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cases triggered—by Marx’s writings. This not only included Marxist economists,² but historians such as Christopher Hill, Perry Anderson, and Robert Brenner, and non-Marxist early institutionalists including Thorstein Veblen, Joseph Schumpeter, Karl Polanyi, and Barrington Moore, who revolutionized our understanding of the evolution of early modern institutions.³

This early work did not offer a systematic study of how differences in institutions determine the economic and political trajectories of nations. This was mostly because of an absence of theory of institutional dynamics—that is, how different historical, international, geographic and political characteristics of countries created

²For example, Baran and Sweezy (1968) and Dobb (1975).

³See Hill (1961), Anderson (1974), Brenner (1976), Veblen (1899), Schumpeter (1942), Polanyi (1944), and Moore (1966).

tendencies for different types of institutions to evolve.⁴ There was no conceptual framework for studying whether and how institutions are “efficient” —meaning whether they enable the organization of the productive capacity of the economy in a way that helps generate a high level of output, or whether they generate systematic “inefficiencies” and suboptimal organizations of the economy. These works also lacked a clear description of the set of channels through which history matters and systematic empirical work for evaluating various institutional channels.

A group of pioneering scholars more familiar with economic mechanisms and economic data made the next major contributions in this area. Mancur Olson, Douglass North, Oliver Williamson, and Barry Weingast, for example, begin clarifying questions related to the efficiency of institutions, emphasizing the inseparability of the distribution of resources in society from the efficiency question—put simply, we cannot presume that institutions maximize the size of a pie, which will then be divided in whatever way we feel appropriate; the size of the pie is linked to its slicing. Elinor Ostrom and Williamson initiated the study of how important market and non-market institutions work. North, Joel Mokyr, Stan Engerman, and Ken Sokoloff started building comprehensive accounts of how certain aspects of institutions persist, for example in Engerman and Sokoloff’s famous work on New World colonies, because of how inequality breeds inequality and via this channel, shapes the economic and political development trajectory of a country.⁵

The next important milestone was the realization by a number of economists, led by Allan Meltzer and Scott Richard (1981), that political economy ideas did not just belong to the normative or theoretical realm but could also be useful for understanding major social and economic changes—in Meltzer and Richard’s case, the increase in the size of government that arose from the expanding voting franchise. Many modern economists took off from this insight and applied political-economic reasoning to shed light on questions related to investment, growth, and inequality, for example, by studying how and why inequality may retard economic growth and how property rights are determined that impact investment and growth.⁶

My own work with Simon Johnson and James Robinson built on this literature, attempting to develop a more systematic framework for the study of the fundamental questions of long-run economic development. James and I attempted to develop a theory of institutional change, with special emphasis on understanding the comparative statics of institutions: why does democracy emerge in some places

⁴I certainly do not mean to claim that early scholars in this tradition did not understand the importance of comparative statics. For instance, Barrington Moore’s (1966) ambition in his seminal *Social Origins of Dictatorship and Democracy* was a type of comparative static—to uncover the historical and social origins of communism, fascism, and market capitalism. But these attempts did not crystallize into a coherent framework. In particular, Moore’s explanation could not easily be generalized to apply to Africa, the Americas, or Asia, or to different eras.

⁵For relevant examples by these authors, see North (1981), Olson (1982), Williamson (1985), North and Weingast (1989), Mokyr (1990), Ostrom (1990), and Engerman and Sokoloff (2011).

⁶See Alesina and Rodrik (1994), Grossman (1994), Perotti (1994), Persson and Tabellini (1994), Besley and Coate (1997), and Benabou (2000).

and not others, and why are certain institutions more growth-enhancing in countries with certain histories (for example, see Acemoglu and Robinson 2000, 2006, 2012)? On the empirical front, Simon, James, and I used some of these ideas in order to probe the causes of economic disparities across countries. We tried to build our empirical work on core ideas from theory—for example, to describe the conditions in which dominant groups, such as European colonizers in Asia or the New World, accept or allow property rights for a broad cross-section of society versus when they will try to monopolize resources and take control of labor. In this way, we attempted to move towards establishing a causal role of institutions in cross-country disparities and inequalities. We also strove to document the role of the colonial period in the economic and political development of many societies around the world (Acemoglu, Johnson, and Robinson 2001, 2002).

Melissa was attracted to economics by these fundamental questions of long-run economic development and was aware of this intellectual background from early on. James Robinson recounts how he met Melissa while teaching at Harvard. At the time, Melissa was taking his undergraduate course on the political economy of Africa and Jeffrey Williamson's graduate economic history class. She had come across our then-recent papers, "Colonial Origins" (2001) and "Reversal of Fortune" (2002). She walked into James's office asking whether he was the same James Robinson who had co-authored these papers and proceeded to discuss not just the economic issues, but the comparative historical questions that these papers were raising.

I, too, remember my first conversation with Melissa, when she was a prospective graduate student trying to decide between several institutions vying to attract her. It was clear to me that I was in the presence of somebody with great drive, ability, and insight, not least because we started talking about how parts of various parts of Bolivia, despite their initial similarities, developed very differently. She was already aware that this might have something to do with the long-run effects of coercive labor market institutions—a perspective that would become the basis of one of her most important works.

Colonial History, Institutions, and Modern Development

When Melissa came on the research scene in the mid-2000s, she already understood that the economic literature on the fundamental questions was too "macro," both theoretically and empirically. In particular, it focused on country-level variation. Not only was much of the relevant inequality and variation within countries, but the ways in which different institutions worked—the specific pathways of persistence and their political and social foundations—varied greatly at the subnational and micro level. No area illustrates the nature of Melissa's contributions better than her work on the impact of colonial institutions and history on modern economic and political outcomes.

As I mentioned above, work by several scholars in the 1990s and early 2000s had started providing evidence and new ways of thinking about how European

colonial policies had shaped the divergent fortunes of former colonies. But there are obvious problems in comparing the development trajectories of the United States and Mexico from the 17th century onwards, because the two countries differ in so many ways.

Melissa, who is a fluent Spanish speaker and erudite concerning Latin American history, had started thinking about divergences within Latin America from the moment she walked into James Robinson's office as an undergraduate. Her deep dive into Latin American colonial history ultimately led to what is arguably her most well-known and important paper, "The Persistent Effects of Peru's Mining *Mita*" [1].

The Spanish Viceroyalty of Peru established in 1573 relied on a system of coerced labor, the mining *mita*. The *mita* was a compulsory labor draft on villages located near mines, in particular the Potosí silver mines in what is today Bolivia, from which one-seventh of the adult male population would be forced to go and work in the mines for extended periods. Although the mining *mita* was not as extensive and brutal as the chattel slavery that came to dominate parts of Brazil, Colombia, the Caribbean, and southern United States, it was nonetheless an onerous burden, bolstered by a suite of repressive institutions. The labor draft was abolished at the beginning of the 19th century. The key question is whether this particular colonial institution has had persistent effects during the 200 years since then.

Poring over archival and historical material on the colonial mining system, Melissa learned that there was a specific catchment area for the mines, and villages outside of the catchment area were not subject to the labor draft. She was able to locate the exact boundaries of the catchment area. This allowed a geographic regression discontinuity design, whereby one could compare adjacent villages on either side of the boundaries of the catchment area.

There are at least four challenges confronting this particular strategy for identifying causality, and how Melissa dealt with each one of them shows the creativity and maturity of her thinking. First, there is a danger that one could throw the baby out with the bathwater in undertaking such a micro approach. The mining *mita* was coupled with other institutions in the Viceroyalty of Peru that enabled the control of labor by force and repression, and these institutions persist in some form throughout Bolivia today. If so, by comparing two neighboring places within this area, one might filter out the true effects of coercive labor institutions. In response to this challenge, Melissa lays out a plausible case for why many of the effects of labor coercion will be at a more local level. As she documents, many of the affected municipalities were isolated then and now, and the Bolivian state is weak and has limited reach. All the same, as Melissa recognizes, one might wish to interpret estimates of the effects of the *mita* system from such a strategy as lower bounds, because some of the effects of the institutions undergirding labor coercion will also affect the control municipalities.

Second, the area where mines are located may not be random, and thus the catchment area for mining labor may not be random, either. Indeed, part of the catchment area boundary follows the edge of the Altiplano, a high plateau area located in central South America where altitude and several other geographic characteristics change sharply. From an econometric point of view, this raises a concern

that villages inside and outside the catchment area may vary in other systematic ways. Melissa recognized this problem and determined that the northern part of the Altiplano area did not suffer from this geographic discontinuity. The northern boundary was also attractive for her purposes because its location was fixed in the 16th century according to estimates of how long it would take laborers to walk from their village to Potosí (a capital city for the area during the colonial period). Her empirical work then focused on this northern boundary.

Third, one would need to understand the organization of the local economy to determine how significant differences could persist for over 200 years between neighboring villages. What mechanisms might separate the trajectories of these nearby villages? This, of course, is both a challenge and an opportunity, because it may be easier to make progress on understanding the mechanisms of persistence within a specific country, rather than in cross-country work.

Finally, in contrast to the most common applications of regression discontinuity at the time, where there is a single running variable, spatial regression discontinuity design has to grapple with the fact that distances from the relevant boundaries are two-dimensional. Melissa carefully and transparently dealt with this issue in a way that has enabled many other researchers since then to build on her work.

Melissa's regression discontinuity estimates using the northern border of the mining *mita* reveal sharp differences between neighboring municipalities on either side of the catchment boundary. For example, places inside of the catchment area have household-equivalent consumption levels that are somewhere between 20 and 30 percent lower today than neighboring municipalities outside of the catchment area. They also have significantly higher rates of child stunting (low height-for-age among children). This is an eye-popping finding. Imagine that a block of streets in your town has 30 percent lower income per capita than a neighboring block because they had a different history 200 years ago! Labor coercion must have been hugely scarring to generate such durable consequences.

But the issue is even more complex and more interesting than the headline finding might suggest because these municipalities are not like two city blocks in a modern American metropolis. Understanding the difference in their social, political, and economic terrain is the second major contribution of Melissa's paper.

Melissa documents that the economies of neighboring villages remain very distinct, in part because these places are isolated and poorly connected. In fact, one of the proximate causes of poverty in the catchment area is likely to be the low prevalence and quality of basic public services, such as roads, water, sewage, and education. But why might labor coercion 200 years ago cause lower public good provision today? To answer this question, Melissa delves into the history of Spanish colonial politics and economics.

Paradoxically, it turns out that the Spanish colonial state wanted to "protect" the people in the catchment area so that they could be a reliable source of labor for the mines. This meant, in particular, shielding them from the exploitation of local Spanish settlers, who would set up their own haciendas and recruit and sometimes coerce the indigenous population to work for them for low wages. The most

straightforward way for the Spanish colonial state to do this was to prevent settlers from building haciendas inside the catchment area.

Melissa documents that haciendas are common outside of the *mita* and much less common inside. The hacienda owners became increasingly powerful in the 19th century. They became the ones who controlled state investments in roads, local amenities, and schools. They directed such investments to the areas where their haciendas were, because they would need to transport their produce and also would benefit from these other services directly or indirectly via their workers. Consequently, municipalities inside the catchment area were often ignored and deprived of critical public services. Because they could not reach the market, subsistence farming and economic self-sufficiency became more prevalent in these areas.⁷

Since its publication in 2010, this paper [1] has become very well-known and features in many graduate and some undergraduate reading lists. It combines painstaking data collection and high-quality econometric analysis to create a nuanced picture of colonial institutions and their persistent effects. This combination emerges again and again in Melissa's work.

Many of the qualities that make [1] so important for the literature are seen in a second major paper Melissa wrote on Latin American colonial history, "Path Dependence in Development: Evidence from the Mexican Revolution" [3]. This paper investigates the long-term consequences of the armed conflict in the context of the Mexican Revolution, which raged across Mexico in the 1910s following the overthrow of the longtime Mexican dictator Porfirio Diaz. The paper is interesting for its discussion of the unique and rich context of early 20th century Mexico. But it also raises the big question of institutional persistence. All countries in South America became independent in the first half of the 19th century and many of them have experienced various social revolutions. Why didn't these upheavals erase the effects of the colonial institutions of the past?⁸

The Mexican Revolution is an ideal case for studying these questions because it destroyed many parts of the existing system, ushered in a new era of politics in Mexico and a new dominant party, and fundamentally changed the contours of Mexican politics. While the causes of rebellion in many parts of the country were variegated, much of it was caused by the desire of local groups to challenge the authority of the state. Eventually, the Mexican Revolution led to the victory of the "constitutionalists," whose plan was to centralize political power in Mexico.

Melissa's idea is that the centralizing powers would treat local areas that rose up against central power differently, and this would have long-run influences through politics in these areas. Melissa shows that severe droughts between 1906 and 1910 were a major predictor of greater revolutionary activity and armed conflict, and goes

⁷I should also mention a more minor paper that Melissa and I co-authored, "Productivity Differences Between and within Countries" [2], where we document the extent of within-country between-municipality inequality, and link it, as in [1], to public good provision and especially road infrastructure.

⁸In Acemoglu and Robinson (2008, 2012), we suggested that initial institutions influenced subsequent political and economic development through a process of "path-dependent change," but we could not nail the exact mechanisms and did not have systematic evidence on how this process worked.

through her usual careful work to document that this is a causal effect. Interestingly, these places experiencing greater revolutionary activity are significantly poorer today and also have access to fewer public goods and lower education. Why this persistence?

Melissa's investigation of Mexican history provides a convincing explanation. The constitutionalists' Institutional Revolutionary Party (the PRI) that dominated Mexican politics until the 2000s controlled these areas by setting up patron-client relations—for example, in the form of agricultural communes, the *ejidos*. Political reform and public goods were provided but always embedded within this system of distorted property rights and highly politicized resource allocation. This political channel then created a new type of persistence: long-run underdevelopment resulting from inefficient ways of appeasing the local community in order to ensure order within the context of a non-representative, largely extractive regime.

Put differently, Melissa proposed a new channel for persistence. The political disruption created by temporary shocks—droughts between 1906 and 1910—during this critical juncture generated persistent political and economic effects. Given the Mexican context and the nature of PRI's tenuous control over this vast country, this response took the form of concessions and resources for these areas but in a particularly inefficient form which then generated persistent economic costs.

Given Melissa's deep knowledge of Latin American history, one might have expected her to continue to focus on this area. Instead, she turned to colonial history in Asia. One fundamental difference here is that several Asian nations managed to embark upon a process of rapid economic growth over the last 70 years, unlike most of Latin America. The causes of this "second divergence" are still poorly understood. Motivated by these issues, Melissa started voraciously reading and learning about this history. Here, I will discuss two important papers in this area and omit her very promising work-in-progress on Japan and Taiwan.

In "State Capacity, Local Governance, and Economic Development in Vietnam" [4], jointly written with Nathan Lane and Pablo Querubín, Melissa not only shifts focus from Latin America to Asia but also turns to a new set of political economy mechanisms: the role of bureaucratic state capacity at the local level.

There is a growing literature on state capacity, and much of this work focuses on within-country variation. But various problems afflict this literature. It is often difficult to pinpoint either the mechanisms that led to variation in state capacity or even how state capacity works. Moreover, it is difficult to dispel concerns that state capacity co-varies with other institutional or cultural characteristics. The paper seeks to overcome these concerns by exploiting the different types of institutions that became deep-rooted within Vietnam because of the influence of two competing empires: the Dai Viet in the north and the Khmer Empire in the south. While the Dai Viet in the north had a centralized political structure, empowering village institutions as a continuation of the centralized state, the Khmer Empire in the south collected tribute from the areas it controlled via patron-client type relationships (a type of "indirect rule") that did not invest in or develop bureaucratic, institutionalized states at the local level.

Melissa, Nathan, and Pablo use this historical and geographic context to shed light on the role of state capacity and its historical development in Vietnam. They exploit the fact that there was a clear boundary between the two empires and obtain regression discontinuity estimates of the effects of these different types of political institutions on the long-term development of state capacity and contemporary outcomes.

As is typical of Melissa's careful historical work, the paper uses very rich data, mostly hand-collected over several years, to document persistent differences in living standards, favoring the north of the boundary with its history of better-developed state capacity. Melissa, Nathan, and Pablo document that the effects largely work through better provision of public goods. Once again, the ability of Melissa (and her co-authors) to develop a nuanced understanding of how institutions work on the ground is deeply impressive.

One important finding is that the strong centralized institutions of Dai Viet appear to have "crowded in" (rather than crowded out) stronger civil society and cooperative relations within villages, which are ultimately responsible for the provision and distribution of these public goods. This thesis is of particular interest because several scholars have observed or conjectured that what distinguishes Asian political economy (especially in places such as China, Vietnam, and Korea) from Africa and Latin America is the greater capacity of bureaucracies and state institutions that have evolved over millennia. But how does this capacity emerge in reality? Is it a hallmark of top-down imperial control as in the Chinese case, where efforts to build a capable bureaucracy under Imperial command go back more than two millennia? This paper is thought-provoking in this context because it suggests that there are multiple paths to high state capacity, and in fact state capacity can be strengthened by local political organization and cooperation (which is rather different than the most common interpretation of the Chinese path).

The other paper on persistent effects of Asian institutions is Melissa's recent work with Ben Olken, "The Developmental Effects of Extractive Colonial Economy: The Dutch Cultivation System in Java" [5]. This paper is also based on meticulous data collection from Dutch archives that Melissa located. It uses a regression discontinuity design as well as a creative placebo strategy, each based on historical differences in the reach of the Dutch colonial apparatus.

There are some parallels between this paper and Melissa's work on the mining *mita* [1]. There is once more a catchment area, subject to colonial coercive activities, this time related to sugar cultivation. Again, production is organized under coercion with the chief purpose of enriching European colonists who kept control via their military superiority. But institutions work quite differently depending on historical context. In this instance, Melissa and Ben find that villages in the catchment area of the colonial sugar production system in Dutch-colonized Java are no poorer than the control groups, and in fact, have higher levels of education and more manufacturing activity. Why would this be?

The mining *mita* and sugar cultivation in Java have some important differences. For one, sugar workers in Java were not moved to the mine, but cultivation instead

took place in their local community—which meant investments in roads and amenities. Even more importantly, sugar cultivation, though taking place under harshly extractive conditions, also came with sugar mills to process the sugar. Melissa and Ben show that these mills, as well as the infrastructure, stayed after the forced cultivation regime disappeared. They further document via a placebo strategy—by comparing locations where the mills were placed to plausible counterfactual locations along the same rivers—that the mills led to more economic activity, and this effect persisted for more than 100 years after the closure of the mills. It therefore appears that the economic structure implemented by the Dutch, centered on sugarcane production and processing, created a platform for continued economic activity after the colonial period, and was supplemented by schools and other public good investments. The economic opportunities generated by these investments had persistent effects.

This is another very well-crafted, careful paper, based on a massive amount of historical data collection. Yet again, what is most impressive is the detailed institutional context and the deep dive into how these institutions work on the ground, depending on historical conditions.

Conflict, Law Enforcement, and Politics

Melissa has also made defining contributions concerning the interplay of conflict, law enforcement, and politics. Of course, there is a large literature on crime and law enforcement both in developed and developing economies. But previous to Melissa's work, their relationship to politics had not been central. For example, might political factors determine when law enforcement is effective and when it backfires?

In "Trafficking Networks and the Mexican Drug War" [6], Melissa asks how violent conflict affects economic opportunities and outcomes. She recognizes that this question cannot be answered without understanding the institutional context in which violent conflict is taking place and the policies that central authorities are using to contain it.

The context on which Melissa focuses is Mexico's drug wars and the policies that the National Action Party (PAN) took against drug cartels in the second half of the 2000s. Felipe Calderon from PAN ran for president in 2006 on an agenda to fight the drug cartels and emphasized how these cartels had started controlling local politics. Calderon's strategy involved using federal police against drug cartels in municipalities where they had taken root. Implementing these policies needed at least some support of local politicians, however, so whether local mayors were aligned with PAN was critical in the war against drug cartels.

Melissa first shows that local PAN mayors have a causal effect on the fight against drug cartels. To do this, she has to resolve the usual omitted variable bias and selection problems associated with certain areas having different political preferences and ideologies (or desperately turning to a party promising a cleanup in their worst

hour). Melissa addresses this problem using a different type of regression discontinuity design; in this case, comparing municipalities that narrowly elected mayors from PAN to those that narrowly elected mayors from a different party (typically PRI). She finds that places that narrowly elected PAN mayors, relative to those that narrowly did not, experienced greater anti-drug cartel action, which resulted in a significant increase in violence. While one might expect this to have been due to police versus drug cartel confrontations, it turns out that the rise in violence is mostly driven by fighting between drug gangs.

The federal action appears to have been successful in weakening the dominant cartels and sometimes removing their kingpins. This and other evidence suggest that the harder line against drug cartels destabilized the market and triggered a turf war between rival gangs for the control of the municipality and the local routes. Thus, in the presence of weak political institutions, fighting drug cartels can create unintended consequences—especially so long as the drug trade remains lucrative.

Melissa also wanted to understand how these anti-drug actions have reshaped the drug network in Mexico and the implications of these changes for the lives of ordinary Mexicans. Originally, certain municipalities were well-located to serve the large US market for drugs, but once the turf wars erupted, these municipalities ceased to be as attractive for the established drug cartels. Based on this observation, Melissa maps the road network taking drugs from their origins to US entry points. She finds a shift of drug cartel activity from the municipalities affected by turf wars towards those that form alternative feasible paths to destinations. This analysis is, to the best of my knowledge, the first carefully established instance of spillover of criminal activity through spatial networks.

The paper then uses this variation to tackle the initial economic question: the effects of drug cartel activity on economic opportunities and outcomes for residents. Melissa uses the source of variation generated by Calderon's drug war to answer this question. Rather than improving the lot of ordinary Mexicans, Melissa's evidence shows that the war against drug cartels had significant negative economic consequences for a municipality's residents, most likely because of the collateral damage and the instability that they suffered during the turf wars.

In many ways, this is a very different research agenda than investigating the historical persistence of colonial institutions. But it shares the keen-eyed focus on how the details of an institutional setting matter for the effects of policies. In a different historical context and institutional environment, the federal action against drug cartels could have been very effective and it may have even improved the economic opportunities for residents. Yet the situation in Mexico, with weak local institutions, limited federal control, and continuing lucrative opportunities from the US demand for drugs, made the outcome very different.

A more recent paper, "The Violent Consequences of Trade-Induced Worker Displacement in Mexico" [7], cowritten with Benjamin Feinberg and Kensuke Teshima, is also related. This paper turns to the converse problem: what are the impacts of economic shocks (in this instance trade shocks coming from manufacturing job losses due to Chinese imports) on drug trafficking and violence. When

negative trade shocks reduce local labor market opportunities, more low-skill residents (those that are especially hard-hit by manufacturing job loss) appear to turn to illicit jobs in the drug trade or gangs. This increases criminal activity and also leads to greater violence, once again likely related to turf wars—this time because economic (trade) shocks have disrupted the balance between different cartels.

An even more ambitious and original paper within this broad agenda is “Nation Building through Foreign Intervention: Evidence from Discontinuity in Military Strategies” [8], jointly written with Pablo Querubín. The question is whether foreign intervention, in this instance US reactions during the Vietnam War, is effective in holding insurgencies at bay. The narrower question is whether bombing and military tactics focused on the use of robust force against insurgents “work.” This issue is often complicated by the fact that such strategies affect civilians among whom insurgents hide and live, and the damage that the civilians suffer in the process might motivate them to support the insurgents. This question is relevant not just to the broad area of law enforcement and politics but also for military strategy and the study of anti-insurgent activities in areas with weak state presence.

Melissa and Pablo tackle this question with a clever regression discontinuity design. The US Department of Defense collected detailed information in every Vietnamese hamlet using about 170 monthly and quarterly questions on security, politics, and economics. These questions were aggregated into an overall security score, which then became the basis of the Air Force’s decisions on which parts of Vietnam to target with airstrikes. Because of limited computing resources at the time, these security scores were rounded to the nearest integer (in particular, printing continuous security scores from memory was deemed too costly and time-consuming). The source data were not discarded, and Melissa and Pablo were able to locate and recover these continuous security scores.

The paper then uses the rounding thresholds as a discontinuity cutpoint in the likelihood of being bombed. Villages that ended up just below the rounding threshold were deemed to have higher insurgency risk and experienced a higher likelihood of bombing than otherwise comparable villages just above the rounding threshold. The striking result is that US Air Force bombing campaigns, rather than discouraging them, helped the Viet Cong insurgents. The residents of bombed villages, compared to similar villages with essentially the same risk and sympathies for insurgents, became more likely to cooperate with the Viet Cong after being bombed.

The paper also compares the different strategies of the US Army and Marines, where the latter pursued an early form of the “winning hearts and minds” campaign. The identification here is based on a very different approach, this time rooted in the effects of different military traditions. The Marines were much less heavy-handed than the Army given their traditions and prior experiences, so they approached the problem of fighting the Viet Cong very differently. Using this different strategy, Melissa and Pablo estimate similar effects, confirming that the heavy-handed bombing tactics backfired in containing the insurgency.

The potential lessons from this paper are obvious beyond the context of the Vietnam War. It is another thought-provoking paper, which has already affected the literature and will continue to trigger more innovative work in this new area that Melissa is now carving out.

Other Work

Melissa has other important lines of work, which I briefly discuss now. Two papers Melissa co-authored with Ben Jones and Ben Olken, [9, 10], study the effects of climate on long-run economic development. The question is whether and how climate (and by implication, climate change) matters for economic outcomes.

In “Temperature Shocks and Economic Growth: Evidence from the Last Half Century” [9], the authors exploit within-country climate and temperature fluctuations to look at the effects of temperature on the level of income and economic growth. The paper finds statistically significant and substantial negative effects. Given the nature of the aggregate cross-country data, the authors are limited in their ability to understand the root causes of this surprising headline effect. Nevertheless, they show that temperature affects a variety of industries and may even impact political instability. The second paper, [10], is an offshoot that further explores the same questions.

Melissa has also contributed to the practice of high-quality empirical work. In “What Do We Learn from the Weather? The New Economy-Climate Literature” [11], Melissa, Ben, and Ben provide a lucid exposition of best practices in studies using microdata on weather variation across space. The paper shows how such variation can be useful for answering a variety of questions but also points out various pitfalls. For example, temperature and rainfall data are often interpolated or extrapolated from satellite data or a few weather stations, which can create spatial correlation and the possibility of spurious inference.

In [12, 13], Melissa and her co-authors develop new computational image processing methods for extracting information from non-standard (and foreign-language) text data, which have become increasingly common with expanding digital access to archival records.

Conclusion

Melissa has set a new standard in applied research because she has shown by example that one does not need to shy away from the fundamental questions of long-run economic development and political economy—which are also some of the fundamental questions of the social sciences as a whole—to do credible and sometimes literature-changing empirical research. The demands of the academic research and publication environment for credible identification strategies need

not limit researchers to narrow contexts and questions. Melissa's path-breaking approach has shown how to use painstaking and meticulous data collection and sophisticated, well-executed empirical approaches so that economists and other social scientists can understand the way institutions work. Her work is rooted in the history, context, and details of specific institutions. In the course of her broad research agenda, she has forged several distinctive lines of research.

As this essay has expounded, Melissa's insight, energy, and creativity are unrivaled. But equally impressive for all of us who have known her is her stupendous work ethic and commitment to academic excellence. I remember vividly how whenever we were on the same flight on the way to a conference, I would see her engrossed in the pages of a history book even as she was lining up on the jet bridge. This is the work ethic that has made her not only an amazing scholar but also a top long-distance runner (appropriate training for somebody who would doggedly work to change her field, a task which is not unlike a marathon).

We will all continue to admire Melissa's work, but it is the next generation of young scholars in economic development and political economy who will be the real beneficiaries of her track record. There is much for them to learn from Melissa's work ethic, sophisticated approach to history and empirical work, and ambition and courage to tackle big questions. Indeed, Melissa has helped make the fundamental questions of long-run economic growth cool for ambitious applied economists.

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