Human Agency, Negative Cases, and Public Problem-Solving: Priorities for Public Policy Scholarship in China and Globally in the 21st Century

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Abstract

Public policy scholarship is increasingly flourishing in China, opening up possibilities for understanding governance dynamics in a cultural context and political system too long overlooked by western scholars. There are rich opportunities for exploring the extent to which policy theories crafted in developed democracies apply or should be changed to have relevance in developing countries and non-Western political systems. Science advances when the generalizability of theories and findings can be tested across diverse contexts; thus, scholars in China and around the world gain from the flourishing of Chinese policy scholarship. Here, I elaborate on three theoretical and methodological priorities that appear increasingly pressing as our field of study deepens and broadens: The importance of centering human agency in public policy scholarship, the value of studying and learning from negative policy outcomes, and the need to move beyond diagnosing public problems to investigating how people collectively solve such problems. Along the way, I highlight works from Chinese policy scholars that address these priorities and ways in which scholarship on China is uniquely posited to tackle them.

Keywords: human agency, negative cases, policy process, qualitative research

Agencia humana, casos negativos y resolución de problemas públicos: prioridades para la investigación en políticas públicas en China y el mundo en el siglo XXI

Resumen

La investigación sobre políticas públicas está en auge en China, lo que abre posibilidades para comprender la dinámica de la gobernanza en un contexto cultural y un sistema político que la academia occidental ha pasado por alto durante demasiado tiempo. Existen importantes oportunidades para explorar hasta qué punto las teorías políticas elaboradas en democracias desarrolladas se aplican o deberían modificarse para ser relevantes en países en desarrollo y sistemas políticos no occidentales. La ciencia avanza cuando la generalización de teorías y hallazgos puede comprobarse en diversos contextos; por lo tanto, la academia en China y en todo el mundo se beneficia del auge de la investigación sobre políticas chinas. En este artículo, desarrollo tres prioridades teóricas y metodológicas que parecen cada vez más urgentes a medida que nuestro campo de estudio se profundiza y amplía: la importancia de centrar la agencia humana en la investigación sobre políticas públicas, el valor de estudiar y aprender de los resultados negativos de las políticas, y la necesidad de ir más allá del diagnóstico de problemas públicos para investigar cómo las personas los resuelven colectivamente. A lo largo del camino, destaco trabajos de académicos de políticas chinos que abordan estas prioridades y las formas en que la investigación sobre China está excepcionalmente posicionada para abordarlas.

Palabras clave: agencia humana, casos negativos, proceso de políticas, investigación cualitativa

人类能动性、反面案例与公共问题解决：21世纪中国及全球公共政策研究的优先事项

摘要

公共政策研究在中国蓬勃发展，为"理解长期以来被西方学者忽视的文化情境和政治体系下的治理动态"提供了契机。大量机会能用于探究发达民主国家构建的政策理论在发展中国家和非西方政治体系中的适用性，或者是否需要进行调整

以使其具有现实意义。当理论和研究成果的普适性能够在不同情境下得到检验时，科学会取得进步；因此，中国乃至世界各地的学者都受益于中国政策研究的蓬勃发展。随着研究领域的深化和拓展，我阐述了三个日益紧迫的理论重点和方法论重点："在公共政策研究中以人类能动性为中心"的重要性；"研究和借鉴负面政策结果"的价值；以及需要超越对公共问题的诊断，转而探究人们如何集体解决这些问题。在此过程中，我将重点介绍中国政策学者针对这些重点所做的研究，以及中国研究如何以独特的视角来应对这些问题。

关键词：人类能动性，反面案例，政策过程，定性研究

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This is an exciting time for policy scholarship in China. Chinese scholars are increasingly producing theoretically valuable and methodologically rigorous work investigating policy processes and outcomes, both within China and internationally. Not only is this development valuable for China's policymakers, who benefit from increasing availability of evidence evaluating the design and impact of public programs, but it is also enormously valuable for the wider academic community. Many theories of the policy process were developed by western scholars who built into these theories assumptions based on the functioning of liberal developed democracies. The extent to which these theories explain policy processes in developing, less democratic contexts has been explored only minimally. Yet doing so is crucial. More than 80 percent of people around the world live in developing countries (Gerszon Mahler et al. 2024). The majority of the world's population resides in countries with non-democratic political systems (EIU 2024). China alone represents roughly 17 percent of the world's population. The dominant traditions of policy scholarship thus have a troubling blind spot. The point of policy scholarship is to produce data and analysis that help people better understand their world and improve public decision-making. We as policy scholars have to grapple with how differences in political systems and cultures affect residents' lived experiences and governance processes and outcomes.

Several dimensions of China's political system offer scholars opportunities to gain insights into questions of long-standing theoretical interest. China's 34 province-level administrative units, more than 300 prefectures, nearly 3,000 counties, more than 40,000 townships, and many thousands of villages can yield nuanced data on multi-level governance processes, helping scholars understand how to encourage communication and collaboration across scales and among jurisdictions of widely diverse capacities (Croese et al. 2021). They also offer fertile ground for studying policy diffusion, experimentation,

and learning through a comparative lens. Scholars might explore, for example, the understudied question of what factors facilitate a jurisdiction's uptake of complex policy innovations traditionally viewed as difficult to diffuse (Makse and Volden 2011; Kaine and Wright 2022) or could develop fine-grained indices for measuring success in policy experimentation across variegated contexts (Mattocks 2025). Studying dynamics of authority and accountability between officials at different levels of government can yield important insights about principal-agent relationships. Scholars often conceptualize these as bilevel, but the China case highlights the importance of studying how upper-level principals try to shape behavior across linked chains of lower-level principal-agent sets (Xu et al. 2025). The relative lack of bottom-up accountability mechanisms (i.e., voting) in the Chinese political system opens up avenues for studying how officials can be incentivized to improve governance in the absence of such pressures (Hou et al. 2022) or how, when, and why officials gauge and respond to public concerns (e.g., Fan et al. 2018).

As our field of study deepens and broadens, attracting talented scholars in China and the developing world, there are three theoretical and methodological priorities that I view as increasingly pressing: Centering human agency in public policy scholarship, studying and learning from negative policy outcomes, and moving beyond diagnosing public problems to investigating how people collectively solve such problems. These priorities confront all policy scholars, not just those working in or studying China. However, research on China can uniquely help address these priorities. The rest of this paper elaborates on these topics, aiming to improve the rigor and usefulness of our collective research enterprise.

First is the priority of studying human agency and its consequences. Conceptually, this is not novel; policy scholars have long recognized the role of street-level bureaucrats in steering policy implementation (Weatherly and Lipsky 1977), policy entrepreneurs in strategically shaping public agendas (Kingdon 1984), and advocates forming coalitions to seek policy change (Sabatier and Weible 2007), among others. What is novel is the magnitude of forces pushing scholars away from studying individuals. These forces influence both the methods and topics scholars choose. Methodologically, powerful computational technologies, alongside massive datasets describing all facets of human behavior and decision-making, are transforming what is possible to achieve with empirical analysis. We can build and run more sophisticated quantitative models than ever before, with greater predictive capacity and accuracy. Often, our models examine how outcomes are affected by attributes amenable to analysis using secondary data, like form of government, economic conditions, or public sentiment. Large language models (LLMs) allow scholars to analyze corpuses of millions of policy documents (e.g., Fang et al. 2025). Online survey platforms like Baidu or MTurk give researchers rapid, relatively low-cost tools for gathering data from large

samples and performing survey experiments offering respondents hypothetical choice scenarios. These approaches are appealing to empiricists hungry for data and analytical innovation and help explain why academic publishing is accelerating in rate and increasing in volume (Moorehouse 2024). Substantively, the ways in which information technologies and architecture are transforming governance is a hot academic topic. Literature is rapidly growing around the ways in which governments use algorithms, often informed by artificial intelligence (AI), to structure how citizens interact with the public sector, prioritize who receives public services and how, and "nudge" citizens towards preferred behaviors (e.g., Calboli and Engelen 2025). Growing too is scholarship around how algorithms shape, potentially in biased ways, the information government decision-makers access and policy solutions they pursue (e.g., Panch et al. 2019).

Scholars should certainly pursue exciting data and innovate in its analysis, and study phenomena, like AI, transforming political and socioeconomic landscapes. My concern is that these foci direct academic attention away from the creative and strategic powerful governance choices made by humans, in at least three ways. First is the "streetlight effect," wherein the approaches scholars choose affect what they find. If we mainly study policy processes in ways amenable to large-scale computational analysis, using secondary data or decontextualized survey responses, we will be challenged to learn about how the strategic actions of individuals, from policy entrepreneurs to politicians to ordinary citizens, influence governance processes and outcomes. To the extent that there are systematic patterns in the ways individuals strategically engage the policy process (and plenty of literature suggests there are), our analyses will be systematically biased.

A second, related concern revolves around the difference between what occurs in the "world of action" (E. Ostrom 2005) and the "world of information." The factors that shape policy processes and outcomes—versus those that can be documented with "10,000-foot" data collected from policy documents, digital footprints, marketplace transactions, and observation of behavior trails in large samples—are not necessarily the same. Many important political decisions are made in backrooms and hallways where there is no official documentation. How individuals respond to hypothetical choice scenarios in an online survey experiment may diverge substantially from how they respond to in the real world. The way government officials actually make and implement policy often differ from than rules and procedures written into formal plans and constitutions that can be analyzed (for example) by LLMs; E. Ostrom (2005) called this the difference between "rules in form" and "rules in use."

Third, scholars studying how algorithms shape citizen behavior and policymaker responses are not necessarily studying algorithmic creators themselves, some of whom may be in the public sector, but many more of

whom operate in the private sector and market their products to governments. Yet such investigation is needed. Humans make decisions about where and how to collect data that form the massive databases scholars now analyze, and not merely on the basis of costs and benefits; their choices are shaped by personal and professional values, biases, and goals. Humans build assumptions about human nature and causal processes into computational models. Human policymakers choose when and where to deploy algorithms to automate some policy activities versus where to retain discretion. These choices have political motivations and political consequences and thus require serious scrutiny by policy scholars.

China offers a particularly compelling context for researchers to tackle these concerns, for at least two reasons. First, the country is a world leader in digitation, automation, and advanced technology. It is the world's largest e-commerce market (ITA 2025), has the most 5G stations and more than half of the world's 5G users (Moises 2023); and most basic government services can be accessed online (Li 2021). Digital penetration in China also allows the central government to collect a wide range of data about individuals, from purchases to movement and locations to the contents of digital communication (Davies 2021). The richness of this data infrastructure offers, in parallel, a target-rich environment for scholars to use case studies, interviews, ethnography, and process tracing to explore the agentic spaces that individuals carve out within these systems and strategies they use to

this end. An excellent example is a 2025 paper by Ye and Xue in *Governance*. The authors examine how experts shape public policy in China, using interviews with researchers at government-funded thinktanks and analyzing documents they produce to reveal how "hidden" policy entrepreneurs influence policy agenda-setting, formulation, implementation, and adjustment. Similarly, the diversity of digital algorithms being deployed for so many purposes in China offers, in theory, myriad opportunities to interrogate how algorithmic creators decide to build out their platforms and the goals and values that influence their choices. One challenge, discussed further below, is algorithmic creators may be unwilling to disclose these procedures.

Second, personal ties are an important mechanism that political actors in China leverage for securing professional promotion and policy influence. Personal networks form based on shared background or education, professional training, and iterated interaction (Yang et al. 2018). There are thus rich opportunities to examine how "rules in use" shape governance processes and outcomes in ways not necessarily accounted for by "rules in form." For example, in their 2024 paper in *Public Administration*, Yi and coauthors study how local government officials in China develop interlocal networks for policy learning and information-sharing. Past experiences of local officials, including where they worked and with whom, affect their choice of cities to look to when seeking to learn about a new policy issue. The authors also show

that when high-level officials like move postings between jurisdictions, they foster learning connections across jurisdictional boundaries. By studying relatively informal human relationships, Yi and coauthors improve knowledge about how formal policy instruments move across landscapes. We need more work like this.

A perspective on policy scholarship that centers human agency inquires after strategies used by elites, government officials, and citizens to influence policy decision-making. What approaches are more efficacious, when, and for whom? It asks how the stories humans tell about our lives and governing institutions affect policy viewpoints, building or diminishing civic efficacy or advantaging some social groups and disadvantaging others. This perspective asks why people take on leadership roles in social movements or advocacy campaigns despite real personal costs. Just as important is understanding why people stop engaging in advocacy or step back from leadership roles, because this helps us understand the determinants of civic engagement. Centering human agency involves studying how formal and informal networks shape who we communicate with, the information and resources we can access, and how we process that information and deploy those resources. It asks how people who implement policy shape and even remake public programs during implementation, informed by their own worldviews.

Questions around human agency can be explored with varied research methods, but I want to highlight the particular importance of qualitative research. The inner worlds of humans are difficult, if not impossible, to understand using only behavioral observations or trends in secondary data. Motivation drives agentic choices, but motivation is a complex phenomenon best interrogated by directly engaging people in conversations that can reveal and elaborate this complexity. The problem is that qualitative research takes time and energy and, frankly, is not prioritized by many current models of academic publishing. It can be difficult and time-consuming to identify people who can offer insights into social dilemmas, develop trusting relationships with them, and learn from their experiences. But we can and should do hard things. There is much wisdom to the widely accepted argument that researchers should use both quantitative and qualitative methods to triangulate and increase confidence in their findings. I am increasingly of the viewpoint, though, that qualitative approaches have an important function that is more encompassing—and perhaps more important—than collecting data for any particular research project. Qualitative approaches, done well, treat humans as humans, inherently worthy of dignity and respect. These approaches recognize that people are more than data points. All research modalities have the potential to be extractive, gobbling up data without giving research subjects anything meaningful in return. But qualitative research at least points a researcher toward a possibly less extractive path; by investing their

own time in forging relationships, the researcher gives something valuable to the research subject. This, in turn, signals that the research subject is themselves valuable. Qualitative research, as a practice, pushes back against the increasing depersonalization and atomization of our world.

There is already a strong tradition of qualitative research in China, particularly fieldwork aiming to understand the circumstances of the country's large rural population and their experiences with policy interventions to reduce poverty and increase development (Hsiung 2015). My aim is to underscore the ongoing importance of this work in the face of incentives encouraging researchers to instead pursue computationally intensive analyses of secondary data. It is also important for scholars from outside of China, who are increasingly drawn to the country's opportunities for research, to recognize realities that affect qualitative research in a Chinese context. In an environment where certain topics are perceived as sensitive, individuals often align their expressed opinion with established positions (Lowell et al. 2024). Tensions between China and other countries may make research subjects reluctant to engage with researchers from outside China. Personal connections can allay some of these difficulties, but researchers from outside of China often lack extensive or strong personal networks in-country (Ying Yang and Le 2008). Language barriers and potential for intercultural misunderstandings pose additional barriers. For these reasons, international researchers pursuing qualitative research should seek meaningful collaborations with Chinese scholars who have on-the-ground knowledge, cultural fluency, facilitative social networks, and political situational awareness. Chinese collaborators can assess which kinds of research questions can (not) be pursued, and how; they also are best positioned to shape research so that it provides real benefits to subjects.

My second point is that we need to pay more attention to negative cases—situations when policy efforts go awry—if we are going to make policy theories rigorous and distinguish more impactful interventions from less. This is not a theoretical point as much as a methodological one. That is, there is a literature wherein scholars seek to define policy failure, develop typologies capturing its diverse dimensions, and theorize about how policy failures manifest (e.g., McConnell 2010; Bovens and 't Hart 2016). This literature is relatively small and deserves more attention. My focus here, though, is on scholars whose main interest is not elaborating the concept of policy failure, but who nonetheless encounter it in their empirical work. Our scholarship is full of studies examining when and why governments adopt laws; how legislators got an issue on the public agenda or secured policy adoption; and how advocacy groups cultivate networks of supporters, among others. We less commonly see studies examining when and why jurisdictions try but fail to adopt a law, what tactics tend to be less successful for legislators trying to get a policy adopted, the conditions under which policy advocates experience defeat, or

why efforts to build coalitions falter. There are several practical reasons for this. First, these negative cases can be difficult to study because people involved with them may be embarrassed, frustrated, or tired, and thus disinclined to share insights into what went wrong. Second, it also may be difficult to identify relevant informants (e.g., the puzzle of how to search for people who might have attended a protest but decided not to) or data sources (e.g., websites for a failed electoral campaign may not be maintained after the campaign's loss). Yet is critical that researchers devote effort to overcoming these obstacles.

Policymakers generally want to know how to create societally beneficial outcomes and avoid negative ones, so from an end-use perspective, it is a problem that we as scholars focus more on answering the first question than the second. Understanding negative cases also matters for efforts at building strong policy theory. The point of theory is to develop generalizable expectations, so that we aren't tackling every problem anew, but rather can apply insights to classes of cases or processes. Empirical testing helps us understand which cases or processes a theory can explain and relevant scope conditions. But if we do not develop theories about why policy efforts falter, and do not test that theory across a range of cases, we are left with a situation in which every policy misstep appears particularistic and unique, and we cannot learn from it. This is specifically a call for scholars of leading theories of the policy process, like Advocacy Coalitions Framework (ACF) or Multiple Streams Framework (MSF) or Social Construction Theory (SCT), among many others, to pay more attention to negative cases in order to develop and test theory-rooted explanations for phenomena like a policy coalition failing to achieve subsystem dominance (ACF), a window for policy change failing to open (MSF), or an advantaged group failing to secure policy benefits (SCT) (see Weible 2023).

Unless researchers explicitly study negative cases, they cannot know whether factors they identify as driving positive outcomes actually are determinative. For example, much of my own research concerns policy entrepreneurs (e.g., Arnold 2021). There is an extensive literature on these strategic political actors, the vast majority qualitative case studies describing positive cases wherein policy entrepreneurs achieved their goals. Studies often show that these individuals build networks of committed supporters; frame societal problems to favor their proposed solutions; are sociable and empathetic, grasping the needs and motivations of people they seek to persuade; and invest in pilot or demonstration projects that can yield evidence supporting their policy proposal (Petridou and Mintrom 2021). Consequently, these are the strategies that scholars often recommend people pursue to influence policy. But because it is rare for scholars to examine cases of unsuccessful policy entrepreneurship, there may be plenty of situations wherein policy entrepreneurs do all of these things but nonetheless do not achieve their aims (Arnold 2023). And if that is possible, it implies that the strategies we typically recommend pol-

icy entrepreneurs pursue might actually make no difference—that we observe them in successful and unsuccessful (or moderately successful) cases because it is really some other set of factors, like elite preferences or the dominant policy image, that have the real causal leverage over outcomes. Similarly, if we only study cases where policy entrepreneurs achieve adoption of the policy they champion—and fail to study cases where such a policy is adopted, even without a strong and committed advocate—then we cannot infer that policy entrepreneurship is a key driver.

This argument is not unique to my particular corner of the policy literature. It emerges whether we are studying cities, provinces, countries, organizations, businesses, or advocacy campaigns, across all sectors. For example, if our research shows that advocacy groups with extensive financial resources often achieve their aims, we have to ask whether there are also well-resourced advocacy groups that fail, suggesting that perhaps something about the issue or opponents actually matters as much or more than resources. If some children in foster care receive few visits from government social workers but are still as healthy and well-adjusted as children who receive many, then devoting public resources to boosting supervision might not be the best way to ensure quality livelihoods for vulnerable children.

When scholars examine negative cases, particularly using quantitative approaches, they need to think through and then evaluate the causal processes they expect to produce positive versus negative outcomes. Too often, we tend to assume that the relationships we find statistically significant in predicting positive outcomes of interest, like policy adoption or lobbying efficacy, also predict negative outcomes, just with a flipped sign: If having more financial resources appears to help an advocacy group capture attention, we assume that having fewer financial resources must make such a group less efficacious. If more home visits from social workers help improve health outcomes for foster children, we assume fewer home visits will be correlated with worse health outcomes, and so on. But we rarely actually test whether these phenomena are bidirectional, leaving open the possibility that the factors driving negative outcomes might be different than those driving positive ones. Lieberson (1985) discussed this problem some decades ago, but it is still recognized—and addressed—too infrequently. This is also an argument for pairing quantitative research that can identify patterns with qualitative research that can help explain patterns; in-depth case studies relying on expert informants may reveal differences in causal pathways between negative and positive cases.

China again offers a compelling context for evaluating and learning from negative cases. The country has a long tradition of encouraging local policy experiments so as to derive lessons from more and less successful efforts and, sometimes, incorporate these into national-level policy rollouts (Wang and Yang 2025). There are thus many opportunities, across many sectors,

types of policy, and type of administrative unit, to examine divergent outcomes and how these outcomes manifested. For example, Wei and coauthors (2025) examined how three county governments in China grappled with large COVID-19 outbreaks, exploring how variation in insight, integration, learning, and innovation capacity helps explain why counties had different levels of success in controlling the virus's spread. While there may be selection biases in terms of which local governments pursue policy experiments (Cai and Chen 2024), careful case selection by researchers can help mitigate this issue. Indeed, studying policy experimentation as a mode of governance, with China as a canonical case, can help scholars understand how heavily institutionalized political systems learn and adaptively manage (e.g., Han and Fu 2024). The Chinese context also offers an interesting opportunity to examine pathways to negative results in a less-democratic developing state, given that the already limited scholarship in this domain tends to focus on western developed countries. For example, research suggests that advocacy coalitions in China engage in less inter-coalitional policy learning than coalitions in western contexts (Li and Weible 2021). Accordingly, a researcher asking why an advocacy coalition in China fails to learn might posit that lack of learning results from lack of diversity (and thus limited informational resources) within a coalition rather than lack of engagement with an opposing coalition.

A critical first step in steering toward positive outcomes is understanding how we create them. This is my third and last point. My own country is enormously polarized right now, across a range of dimensions—political, ideological, economic, and cultural. Trying to bridge these divides is the challenge of our time. How do we talk to each other? How do we move beyond echo chambers and engage in genuine conversation about how we want our society to function and what we are willing to contribute to make that happen? As policy scientists, we're already pretty good at studying and diagnosing problems. Yet studying how people try to solve problems is even more important. How do people with different views nonetheless collaborate? How do people learn during the governance process, finding ways to improve public policies rather than doubling down on preexisting beliefs and screening out dissonant information? How do decision-makers engage the public meaningfully? How do we foster trust in government so that the public is willing to engage in decision-making?

Scholars of collaborative governance, deliberative democracy, and co-production are intimately familiar with these questions. From their work, we know, for example, that trust in government can encourage citizens to engage in co-production, and the converse (Liu et al. 2024). Collaborative governance mechanisms can increase representation of marginalized stakeholders in public decision-making (Dobbin and Lubell 2021). Deliberative institutions like citizen and assemblies and mini-publics can reduce polarization among participants (Fishkin et al.

2021). When citizens know that such institutions have been employed in public decision-making, they view the decisions as more legitimate, even when they themselves were not involved in the deliberation (Boulianne 2017). The particular value we as policy scholars can add to these literatures is examining the processes by which interventions to increase deliberation, collaboration, and stakeholder engagement and buy-in—phenomena that are often studied as experiments or trials, isolated from larger sociopolitical contexts (e.g., Gronland et al. 2023), or as tools or methods rather than governance modes (Lin 2025)—can be integrated and institutionalized into governance systems. Our expertise in studying the policy process, from agenda-setting to adoption to implementation, allows us to analyze how these innovations fare across diverse political venues and over time. We can inquire, for example, into the necessity of public mobilization or policy entrepreneurship for convincing government officials to integrate public participation more systematically into decision-making (Ravazzi 2017), how a public organization's governance culture affects the extent to which it institutionalizes participatory planning (Gollatta et al. 2021), or how traditional modes of bureaucratic policy implementation respond to bottom-up demands for collaborative change (Hofstad et al. 2024).

In this effort, the Chinese case again offers fruitful empirical grist as well as opens new avenues for scholarship. The aforementioned tradition in China of piloting policy innovations locally and adapting and adopting successful innovations at the national level provides opportunities for studying how policy innovations are scaled up and out and the challenges that must be met along the way. The complexity of multi-level governance in China allows scholars to examine the scales at which interventions can be most effective and how multiplex principal-agent relationships affect policy institutionalization. China also offers an empirical setting for studying important questions around how public participation, deliberation, and consensus-building can be incorporated into political systems not built on the western liberal model. In particular, an emerging literature is developing around how Confucian conceptualizations of democracy and political meritocracy can support the integration of public participation in Chinese public decision-making (Bitton 2022; Tong 2025; Li 2024). Interesting emerging empirical work in this area includes Cao's (2022) analysis of how grassroots engagement in urban governance in Nanjing buffers and even repairs disjunctures in policy goals and practices across hierarchical levels of government, and how appeals to social norms and encouragement by influential individuals and entities encourages citizen engagement in neighborhood renewal initiatives (Tang et al. 2022).

This brings us back to a human agency-centered perspective. The public choice theorist Vincent Ostrom—who made the first of multiple visits to China 1997 along with his wife and future Nobel Prize winner in Economics, Elinor Ostrom—often reminded us that

cities, countries, or organizations do not, on their own, do anything at all (V. Ostrom 1997). People do. People pursue goals, learn fallibly, seek to effectuate beliefs and values, build relationships, and lead and inspire others—and these individual choices are what aggregate to the policy outputs we attribute to governments or organizations (E. Ostrom 2005). As policy scholars interested in understanding and improving governance, we need to study the agentic choices that undergird it. We cannot shy away from studying negative cases where policy phenomena do not manifest as expect or interventions do not work as intended; this analysis is vital to gaining explanatory leverage over causal processes. And we must not just diagnose societal problems but investigate how humans work actually work together to solve them, and how people can institutionalize problem-solving into governance regimes. People are artisans and governance is their craft (V. Ostrom 1997; E. Ostrom 2005). Our challenges are vast. We are the only ones who can save us. And our job as policy scientists is to help support our communities as they collectively pursue the artisanship of governance.

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