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Essays in Political Economics

by

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Abstract

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This dissertation consists of three chapters. The first two chapters study the local governance in China. Guided by theoretical insights originated from the political economics literature, I exploit natural experiments to provide empirical evidence on how local governance decisions are affected by bureaucratic incentives and incoordination between local governments. The third chapter is a descriptive study examining the broader social science research community on researchers' attitudes and behaviors towards open science practices.

In Chapter 2 (coauthored with Shaoda Wang), we investigate a question central to the long-standing debates on federalism and decentralization: how does decentralized decision-making distort the governments' incentives to internalize border spillovers, and what are the associated economic and welfare consequences? We attempt to answer these questions by exploiting the "township merger program" in China, where thousands of pairs of neighboring townships were required to merge over the last two decades. Collecting novel firm-level geocoded emission and production panel datasets, and exploiting more than 3000 cases of township mergers between 2002 and 2008, we find evidence that local governments are internalizing spillovers on the merging borders. Empirical results show that when a polluting firm suddenly "moves" from the border to the center of the town, it receives lower government subsidies, faces higher de facto tax rates, and at the same time reduces pollutant emissions and invests more in emission abatement equipment. Utilizing another transaction-level dataset containing the universe of land auctions in China, we observe that both land prices and new developments of residential buildings increase near the merging borders with polluting firms, indicating that household welfare increases with the

internalization of border pollution.

Chapter 3 puts focus on bureaucrats running the local governments and try to understand whether and how bureaucrats respond to non-pecuniary incentives besides career concern. Specifically, I investigate whether appointing bureaucrats in the place where they originate would improve or impair their performance. By exploiting exogenous variations in city leadership vacancy and the turnovers in the personnel decision-making body, I find that Chinese municipal leaders' biographical background indeed plays an important role in their governance decisions. Natives, who grew up in the city they serve, would implement policies that lead to a 7% reduction in total tax revenue. Estimates from firm-level data also show a significant drop in tax payment from firms during natives' tenure despite increases in outputs and profits. But further examination suggests that only firms in the home counties of native leaders benefit from the tax breaks. With respect to budgetary policies, native officials exhibit a pro-social tendency, allocating a higher share of municipal budget to education and health care, and a lower share to infrastructure. However, despite the changes in budget composition, real outcomes of public goods deteriorate under the native city leadership. Taken together, my results suggest that social proximity hampers bureaucrat performance and facilitates local favoritism.

Chapter 4, joint work with Garret Christensen, Elizabeth Levy Paluck, Nicholas Swanson, David Birke, Edward Miguel, and Rebecca Littman, offers a textured description of the current state of social science regarding research transparency and open science practices. Discussions about changes in practices such as posting data and pre-registering analyses have been marked by controversy—including controversies over the extent to which change has taken place. This study, based on the State of Social Science (3S) Survey, provides the first comprehensive assessment of awareness of, attitudes towards, perceived norms regarding, and adoption of open science practices within a broadly representative sample of scholars from four major social science disciplines: economics, political science, psychology, and sociology. We observe a steep increase in adoption: as of 2017, over 80% of scholars had used at least one such practice, rising from one quarter a decade earlier. Attitudes toward research transparency are on average similar between older and younger scholars, but the pace of change differs by field and methodology. According with theories of normal science and scientific change, the timing of increases in adoption coincides with technological innovations and institutional policies. Patterns are consistent with most scholars underestimating the trend toward open science in their discipline.

To my family

Contents

List of Figures

List of Tables

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Chapter 1

Introduction

Institutions are the rules of the game in a society or, more formally, are the humanly devised constraints that shape human interaction.

— Douglass North

Economic development varies greatly both across time and countries. How can we explain those variations? An increasing body of evidence suggests that institutional differences play an important role in explaining the disparity in economic development (Acemoglu et al., 2005; Evans and Rauch, 1999; Xu, 2018). More importantly, political institutions directly determine economic policies and indirectly shape the incentives of individuals, therefore they could have deep and persistent impacts on economic development (See Acemoglu et al., 2005, for more detailed discussions). To understand the interplay between political institutions and economic outcomes, we reach "the boundary between political science and economics", as Persson and Tabellini (2000) put it in the introduction to their renowned textbook *Political Economics*. Following the great tradition dating back to Adam Smith, Thomas Malthus, and David Ricardo, whose historic economic thoughts are often referred to as "political economy", political economics as a relatively new field aims to use main tools of analysis from economics to study interactions between economic outcomes and political institutions. The field attempts to explain why the government behaves as it does, how its behavior influences the behavior of individuals, and what the welfare effects of such changes in behavior are.

The following dissertation explores a diverse array of topics with a mutual theme of using empirical approaches to study institutions.

The first essay tries to contribute to the big debate on centralization versus decentralization and provides some empirical evidence on the impacts of the structure of the governments on regulations and economic policies. One of the key critiques against decentralization is that, under decentralized decision-making, the lack of coordination among local jurisdictions prevents local governments from properly internalizing regional spillovers and spatial externalities, thus failing to achieve a socially optimal equilibrium. Empirically testing this argument, however, is challenging. In this essay, we fill in this important gap in knowledge by focusing on the staggered roll-out of the township mergers program in China, where more than 30,000 pairs of neighboring townships were required to merge between 1995 and 2013. Our results show that the decentralized local governments indeed are unable to internalize border pollutions, which in turn has significant welfare implications.

Aside from the structure of government systems, the personnel running the bureaucracy also directly affect policies and the welfare of the citizens. The second essay examines whether appointing bureaucrats in the place where they originate would improve or impair their performance. Civil services typically have flexibility in assigning bureaucrats to postings in different locations. Without being directly held accountable by the people they serve, bureaucrats who are a key determinant of government performance do not necessarily implement the policies that citizens want. If the affinity to the place bureaucrats serve can help enhance their performance, it will be a very cost-effective way to upgrade overall bureaucracy performance, hence have significant impacts on development and growth. As there is no definite theoretic answer to this question, I set to use an empirical approach to look for the answer. By exploiting exogenous variations in city leadership vacancy and the turnovers in the personnel decision-making body, I find that Chinese municipal leaders' biographical background plays an important role in their governance decisions. Overall, my results suggest that social proximity hampers bureaucrat performance and facilitates local favoritism, albeit small positive impacts in some aspects.

The third essay takes a step back and examines the culture and norm in the research community in the political economics and broader social science research community. In the past two decades, the social sciences have grappled with scandals surrounding the unavailability of original data, examples of publication bias, replication challenges, and in some cases data fraud. One of the egregious cases happened recently in the political science research community. In 2015, once highly acclaimed research by LaCour and co-author were revealed to be an outright fraud (Bohannon, 2015). The purported experiment in their paper never took place and the research data were fabricated using a random number generator. Those incidences underscore the vulnerability of empirical research to the fraud and the importance of adopting better and more transparent research practices. So how many social scientists are adopting open science practices, and what are the average perceptions of these practices in the social sciences? This third essay attempts to provide a comprehensive assessment of awareness of, attitudes towards, perceived norms regarding, and adoption of open science practices within four major social science disciplines: economics, political science, psychology, and sociology.

Chapter 2

The Environmental and Economic Consequences of Internalizing Border Spillovers

2.1 Introduction

The long-standing debate on centralization versus decentralization bears tremendous importance for policy-making (Tiebout, 1956; Oates, 1972; Prud'homme, 1995; Fisman and Gatti, 2002; Foster and Rosenzweig, 2002). In this discussion, one of the key critiques against decentralization is that, under decentralized decision-making, the lack of coordination among local jurisdictions makes them unable to properly internalize regional spillovers and spatial externalities, creating distortions in the decentralized equilibrium (Oates, 1972; Wildasin, 1991; Saavedra, 2000; Fredriksson and Millimet, 2002; Wilson, 1999).

The classic example used to support this argument is the phenomenon known as "polluting your neighbor": the ambient pollution emitted by border firms affect both the host- and the neighboring-jurisdiction, but the economic benefits (e.g., tax revenue) are disproportionately enjoyed by the host-jurisdiction. As a result, the host-jurisdiction lacks incentives to fully internalize the negative spillovers affecting its neighbor, and will have incentives to impose relatively lenient environmental regulation standards on those border polluting firms (Burgess et al., 2012; Gray and Shadbegian, 2004; Helland and Whitford, 2003; Fredriksson and Millimet, 2002; Sigman, 2002, 2005; Konisky and Woods, 2010).

Despite its importance in the centralization-decentraliztion discussion, causally identifying the mechanisms and consequences of internalizing border environmental spillovers remains challenging. Since polluting firms would endogenously decide whether to locate in the center or the border of a jurisdiction, if we compare the "levels of environmental regulation" faced by central and border polluters respectively, such a comparison would reflect not only the local government's differential internalization of these firms' emissions, but also capture any other underlying differences between these firms. In addition, there also exists a data constraint, as firm-level differences in "regulatory burdens" and "responses to regulation" within a narrow region could rarely be credibly observed and measured by researchers.

In this paper, we fill in this important gap in knowledge by focusing on the staggered roll-out of the township mergers program in China, where more than 30,000 pairs of neighboring townships were required to merge between 1995 and 2013. We exploit the fact that when two townships merge, a firm originally located on the merging border will suddenly lie in the center of the newly merged township, and such an abrupt switch from being "border" to "central" would sharply increase the township government's incentives to internalize its externalities. By comparing the same firm under "border status" and "central status" before and after a merger, we could thus causally identify the effect of internalizing border spillovers on the firm's regulatory burdens as well as the associated emission and production outcomes.

Leveraging novel firm-level geocoded panel datasets with detailed information on production and emission activities, we find that when a polluting firm suddenly switches from being a "border firm" to being a "central firm," it on average faces a 5% reduction in government subsidies and a 1.2% increase in de facto tax rate, indicating that it now faces harsher punishment for its pollutant emissions. In response to the increased regulatory burdens, the firm now spends 6.8% higher in abatement fees and reduces more than 8% of its emissions, which altogether lowers its total factor productivity by 5.6%. In contrast, switching from border to center does not lead to any significant adjustments for non-polluting firms.

To understand the welfare impacts of internalizing border spillovers, we utilize a geocoded dataset on the universe of land auctions in China during this sample period. We find that the disappearance of a township border (due to township merger) leads to more residential projects being developed near this location, as well as higher residential land prices. Further analysis suggests that the effects are predominantly driven by merger cases with at least one polluting firm located near the merging border, suggesting that the internalization of border environmental spillovers caused an increase in household welfare.

This paper relates to four strands of literature. First and foremost, it adds to the literature on the competition and strategic interactions among local governments, which falls more generally into the broad literature on decentralization and fiscal federalism. Using a case of tax competition, Oates (1972) first shows that intergovernmental competition makes each decentralized jurisdiction fail to internalize regional spill-overs, which leads to distortions in local policies. This idea was later formalized (Zodrow and Mieszkowski, 1986), and extended to various other contexts, including income redistribution (Wildasin, 1991), government expenditure (Wilson, 1999), and welfare transfers (Saavedra, 2000), etc. In this literature, a classic context in which decentralization exacerbates regional spillovers and causes inefficiencies is the "polluting your neighbor" phenomenon, which has been documented intensively by researchers (Burgess et al., 2012; Gray and Shadbegian, 2004; Helland and Whitford, 2003; Fredriksson and Millimet, 2002; Sigman, 2002, 2005; Konisky and Woods, 2010). A particularly relevant paper is Lipscomb and Mobarak (2017), which shows that when the split of two counties reduces the distance between a water monitoring station and the county boundary, the water quality reading of this station would worsen significantly. Our paper confirms the findings of Lipscomb and Mobarak (2017) in the context of China's authoritarian governance system, and also complements it in two important ways: (1) our empirical setting allows us to pin down the political economic mechanisms through which local governments interfere with firms to reduce cross-border emissions; (2) our data enables us to quantify the economic loss and residential welfare gains associated with the internalization of border pollution spillovers.[1]

Second, our paper adds to the growing literature on the political economy of environmental regulation (List and Sturm, 2006; Burgess et al., 2012; Kahn et al., 2015; Lipscomb and Mobarak, 2017; Jia, 2017; He et al., 2019). Consistent with He et al. (2019), we find that under the same nominal environmental regulation standard, the actual level of regulation enforcement could vary tremendously across spatially adjacent firms even within narrowly-defined industries. Specifically, we find that a polluting firm will face much tighter environmental regulation enforcement when its negative externalities are fully internalized by the local government. Our findings suggest that variations in state-business relationships need to be taken into account

[1] Two previous studies that investigated China's water quality issue are also relevant to this study: Kahn et al. (2015) investigates water pollution abatement across provincial boundaries and finds that tighter environmental regulation by the central government incentivized local officials to reduce border pollution according to specific criteria; Cai et al. (2016) finds that provincial governments responded to the pollution reduction mandates by shifting their enforcement efforts away from the downstream counties. Our paper complements these works by exploiting only within-firm variation in "border" vs. "central," and quantifying the environmental, economic, and welfare consequences of internalizing border spillovers in the same context.

when designing optimal regulation programs.

Third, this study contributes to the discussion about the economic consequences of environmental regulation. While there exists a large empirical literature on environmental regulation in the United States (Becker and Henderson, 2000; List et al., 2003; Ryan, 2012; Greenstone, 2002; Reed Walker, 2011), much less is known about the environment-economy tradeoff in the developing world.[2] In this paper, we add to the existing literature by estimating the average pollution abatement cost for the entire Chinese manufacturing sector: exploiting within-firm variation in regulation stringency, we find that a 10% reduction in SO2 would cause approximately 7.6% drop in manufacturing TFP.

Fourth, our paper also adds to a growing literature on the socio-economic consequences of jurisdictional boundary changes, such as the impacts on market access and development (Redding and Sturm, 2008), regional population (Davis and Weinstein, 2002), local conflicts (Bazzi and Gudgeon, 2016), and water pollution (Lipscomb and Mobarak, 2017). To the best of our knowledge, this paper is the first to link jurisdictional boundary changes to precisely geocoded firm-level panel datasets, which creates a unique setting to investigate the dynamics state-business relations in the context of environmental regulation. Specifically, we document that the disappearance of a township border changes certain "border firms" into "central firms" for the local government, which leads to a significant increase in the regulatory burdens imposed on these firms.

The remainder of this paper is organized as follows. In section 3.2, we briefly discuss the role of township governments in China's governance system, and introduce the township consolidation program. In section 2.3, we layout the setup of our theoretical model, which rationalizes the impacts of township consolidation to guide the empirical analysis. In section 3.3, we introduce the datasets used in this project. We then present the empirical identification strategy and estimation results in section 3.4 and 3.5, respectively. In section 3.6, we discuss potential limitations in our baseline results and conduct robustness check. Section 2.8 interprets the results and discusses their implications. Section 3.7 concludes the paper.

[2] One exception is He et al. (2019), which estimates that China's water regulation costs a 2.7% reduction in manufacturing TFP for a 10% abatement of Chemical Oxygen Demand emissions.

2.2 Background

Township governments

China's local governance system has four official tiers: province, prefecture, county, and township.[3] Township, the lowest level of formal bureaucracy, is roughly comparable to a small city in the US in terms of average size. An average-sized township approximately contains about 20 villages, 20,000 residents and spans 80 km^2.

Despite being the lowest level of administrations, township governments undertake many fundamental tasks within their jurisdiction, such as tax collection, public good provision, policy enforcement, etc. It has been well-documented that Chinese township governments wield substantial discretionary powers in local governance and often selectively implement policies set by the central government (O'Brien and Li, 1999). More specifically, in the context of environmental regulation, studies have found that township governments have both the incentives and the leverage to favor certain polluting firms and impose less stringent regulation standards on them (Wang et al., 2008).

"Township consolidation" program

Due to China's mass rural-urban migration in the early 1990s, rural townships lost large fractions of their population to the urban areas, making the existence of some small townships economically and politically inefficient. In addition, many township enterprises, which were the driving force of the local economic growth in the 1980s, started to face large deficits and losses in the early 1990s. This severely worsened the fiscal burden of the township governments. Therefore, to mitigate the fiscal crisis and realize the economy of scale in rural governance, the central government started a large-scale "township consolidation" program in 1995.

Since the program started, provincial governments each year will receive a target of how many township consolidations to achieve from the central government. Then the target will be divided and assigned to the county governments, which would then follow a 4-step process to implement the consolidation: (1) the county government would first propose a suggestive plan of consolidation, and consult with the involved townships; (2) upon agreement, the involved townships would then submit a formal application for consolidation to the county government; (3) upon approval, the county government will report the decision to the prefectural government and ask for their approval; (4) the prefectural government will then report their decision to

[3]Villages are regarded as "self-governed entities" rather than a formal administrative units.

the provincial government, the final decision-maker. The provincial government will provide a formal notification to the county government regarding the schedule of the consolidation if the plan is approved.

The scale and the pace of the consolidation is shown in Figure 2.1. China had 47,136 townships in 1995, after 18 years of consolidation, this number decreased to 32,929 in 2013. According to the central government, the eventual goal of the program is to keep about 30,000 townships in China.

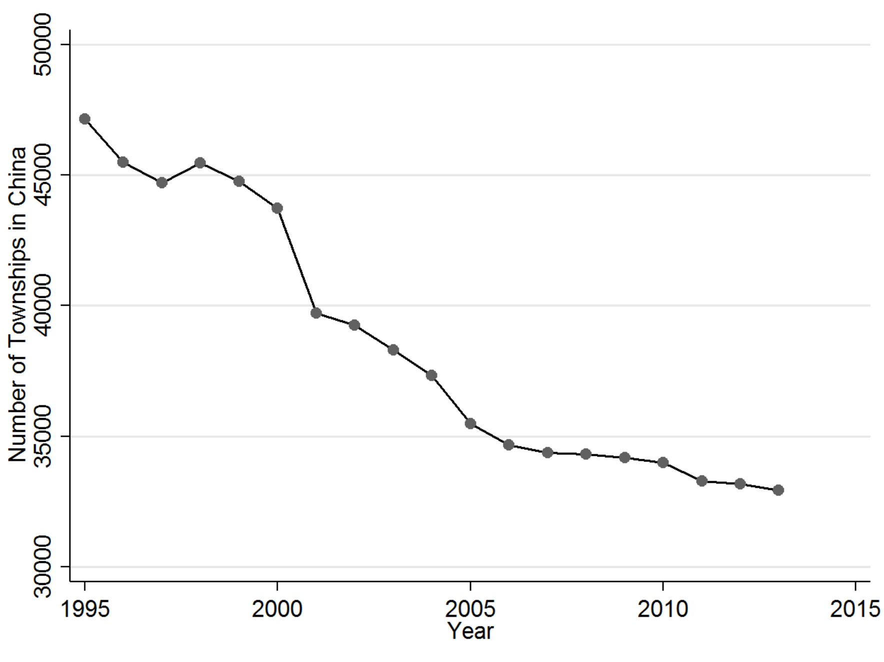


Figure 2.1: Number of Townships Over Time

2.3 Conceptual Framework

In this section, we present a highly stylized model to derive theoretical predictions for the impact of township merger on the government's regulation strategies, firm production, and housing prices.

Setup

The model has three types of agent, manufacturing firms, households, and township governments. For simplicity, we assume there are only two neighboring townships, A and B, and two periods, $t = 1$ and $t = 2$. In $t = 1$, the townships are two separate entities, making independent decisions to maximize their respective objectives. But in $t = 2$, A and B's governments are consolidated into one. The border between A and B is erased and the new government makes decisions to maximize the objective over the joint jurisdiction. A and B's borders with other townships are assumed to remain unchanged.

Firms

There are firms located in both A and B. Firms produce outputs but generate air/water emissions as by-products, thus facing environmental regulation (r) imposed by the local government. We assume that firms produce homogeneous goods, with a Hicks-neutral continuously differentiable production function $f(K, L)$[4], where K is capital, and L is labor. The amount of emission produced is $E(f(K, L), K_E)$, a differentiable function of total output $f(K, L)$, and emission abatement capital K_E. The emission grows as total output increases ($E_1 > 0$) but decreases as an investment in abatement capital increases ($E_2 < 0$). We believe there is a diminishing return to abatement equipment ($E_{22} > 0$), but an increasing marginal effect to total output on emissions ($E_{22} > 0$). In other words, pollution will get out of control if too much output is produced. Firms maximize their profits by choosing the optimal level of labor inputs, productive capitals, and abatement equipment while taking the local environmental regulation r as given:

$$
\begin{aligned}
&\max_{K,L,K_E} \pi(K, L, K_E) \\
&= \max_{K,L,K_E} (1 - t) \cdot f(K, L) - p_r(K + K_E) - w \cdot L - r \cdot E(f(K, L), K_E),
\end{aligned}
\tag{2.3.1}
$$

where p_r and w are market capital price and wage respectively, and t is the general production tax rate decided by the central government. The local environmental regulation r can be thought of as a fine for each unit of emission produced.

[4] We assume it satisfies standard assumptions for production function, $f_1, f_2 > 0; f_{11}, f_{22} < 0$.

Residents

Residents choose communities to live based on the housing price and amenities. We define a community i to be the area surrounding a firm i, and assume firms; emissions only affect the community where it is located. As jurisdiction boundary may cut through communities, we use $\alpha_{ic} \in [0, 1]$ represents the proportion of the land of the community i falls into township c.

We follow Greenstone and Gallagher (2008)'s practice to model the association between housing prices and environmental amenities. Since communities on the merging borders of merging townships only constitute a small part of the Chinese housing market, we focus on the case where general equilibrium price for amenities does not change in response to the increased supply of less polluted communities.

Consider a border community that saw an increase in local environmental quality after a township merger. The supply curve of the residential housing in the community is upward sloping in the relatively longer-term and demand is downward sloping. This is depicted in Figure 2.2 with S and D_1 and equilibrium outcome (P_1, Q_1). When there is an exogenous increase in environmental quality after the merger, the demand curve for residential housing near the improved community shifts out, because the improved amenity will attract more individuals with higher valuations of environmental quality to move in. This is depicted as D_2. This change causes prices to increase to P_2 and quantities to increase to Q_2. The shaded area is the welfare gain from the improved amenities. When the supply is inelastic, the changes in welfare are roughly proportional to changes in prices.

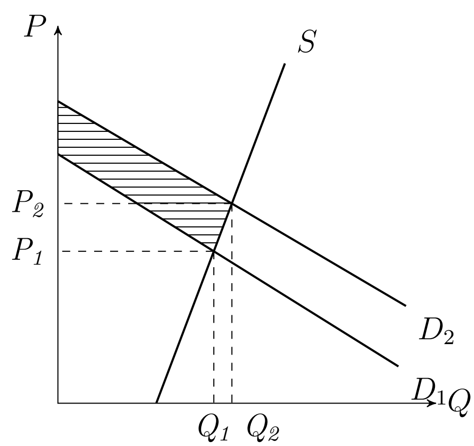


Figure 2.2: Illustration of Welfare Gains due to Amenity Improvements

Government

Although more production means more business tax revenue to the township government, it also means deteriorated amenities and decreased consumer and producer surplus for communities in the jurisdiction. The township government hence faces a trade-off between the land market surplus of its constituency and the tax revenue from firms when setting a regulation policy (r_i) for each firm i in its jurisdiction.

Let $\mathcal{I}_c$ be the set of firms in township c, and r_i be the regulation policy for the firms i. The government's objective is

$$\max_{\{r_i | i \in \mathcal{I}_c\}} \sum_i \left(\alpha_{ic} Surplus(r_i) + Tax(r_i) \right),$$

where $\alpha_{ic} Surplus(r_i)$ represents the portion of the land market surplus in community i that government is concerned and $Tax(r_i)$ represents tax revenue from all firms located in its boundary. If regulation policy r_i only affects firm i and its surrounding community i but not others, the government's problem can be simplified as maximizing the total welfare ($Surplus + Tax$) of each community separately, i.e:

$$\sum_{i \in \mathcal{I}_c} \max_{r_i} \alpha_{ic} Surplus(r_i) + Tax(r_i) \tag{2.3.2}$$

Pre-merger

Within our setup, the firms can only respond to the government's change in environmental regulation by changing their capital and labor inputs. Intuitively speaking, tightened regulation increases the marginal cost of emissions, which in turn raises the marginal cost of productive capital and labor. As a result, firms will decrease production by reducing labor and capital inputs. Besides reducing outputs, firms will also increase investment in emission abatement capital to stem the increasing costs of emissions, which leads to a decrease in estimated TFP because those capitals do not contribute to production. The decrease in outputs and increase in abatement capital investment means the total emission would decrease as regulation is increased. If the surplus in the land market is more sensitive than the firms' production to the environmental regulation, then we would expect a higher level of regulation for communities/firms that are further away from the border as literature has shown in cross-sectional studies.

We summarize these arguments in the following propositions and prove them under general conditions (without the restrictive functional form assumptions) in the Appendix.

Proposition 1. *Firms decrease capital (K) and labor(L) input, when facing a higher level of regulation (r). In particular, firms producing more emission per additional output (higher $E_1(\cdot)$) experience bigger impacts from the increased regulation.*

Proposition 2. *Firms increase investment in emission abatement capital (K_E) as regulation policy (r) increases, provided that firms do not shut down the production.*

Proposition 3. *Firms decrease total emissions as regulation policy (r) increases.*

Proposition 4. *If the marginal benefit of regulation to Surplus is more sensitive to regulation than the marginal cost of regulation to Tax, then the further away is a community from the border (higher α_{ic}), the tighter is the regulation (higher r_i^*).*

Township Merger

When townships A and B merges, within our model, it is equivalent to a shock to merging border communities' α_{ic}. Because the border between A and B dissolves, communities closer to the defunct border will see their α_{ic} increase from some number smaller than 1 to 1.

Based on the Proposition 4, this means that environmental regulations will be tightened for those firms closer to the merging border after the merger. In contrast, nothing will change for firms and communities on the non-merging border. Tightened regulation near the merging border could reduce production outputs and pressure firms investing more in the emission abatement capitals. As a result, pollution decreases and local amenity increases for the communities near the merging border. It then leads to increased housing prices and welfare gain for both consumers and landowners.

Under our model, the total welfare ($Surplus + Tax$) is trivially larger after the merger, because the merged government is maximizing the joint total welfare for township A and B. Therefore, the change in total welfare after the merger tells us the costs of incoordinations between local governments.

2.4 Data

In order to examine the economic and welfare consequences of township merger, we bring together detailed firm-level emission and production data, and transaction-level land price data. Then we obtain geocoordinates for all the firms and land parcels in the dataset and use these to measure their proximity to borders. In the following subsections, we describe each dataset in detail.

Production Data of Industrial Firms

To measure the economic impacts of decentralization, we use the Annual Survey of Industrial Firms (ASIF) from 1998 to 2013. The dataset, collected and maintained by the National Bureau of Statistics (NBS), includes all the industrial enterprises with annual sales exceeding 5 million Yuan. It contains a rich set of information obtained from the accounting books of these firms, such as input, output, sales, taxes, subsidies, etc. This dataset has been widely used in economic research, and more details about its construction and cleaning processes can be found in Hsieh and Klenow (2009),Song et al. (2011),Yu (2015),and Huang et al. (2017)

The detailed production information in this dataset allows us to construct TFP measures for each firm in each year between 1998 and 2007. There are several approaches to estimating firm-level TFP and each requires some particular assumptions (Van Biesebroeck, 2007). In this paper, we use the widely-used semi-parametric estimator suggested by Ackerberg et al. (2015). For robustness checks, we also construct alternative TFP measures following Olley and Pakes (1996) and Levinsohn and Petrin (2003).

Emission Data of Polluting Firms

For environmental outcomes, we collect firm-level emission data from China's Environmental Survey and Reporting (ESR) database. The ESR database is maintained by the Ministry of Environmental Protection (MEP) of China and is used to monitor the polluting activities of all major polluting sources, including heavily polluting industrial firms, hospitals, residential pollution discharging units, hazardous waste treatment plants, and urban sewage treatment plants.

It is the most comprehensive environmental dataset in China, documenting firm-level (polluting-source-level) emissions of various pollutants, such as SOX, NOX, etc. Another unique feature of this dataset is that it contains detailed information on each firm's investment in machinery and technology to reduce pollutant emissions.

The sampling criteria in the ESR is based on the cumulative distribution of emissions in each county. Polluting sources are ranked based on their emission levels by different "criteria" pollutants, and those jointly contributing to the top 85% of total emissions in a county are included in the database. Polluting sources are required to report their emission levels of various pollutants to county-level Environmental Protection Bureaus (EPBs). Local EPBs are responsible for checking the quality of the data and upper-level EPBs will then verify the data.[5]

Land Auctions Data

To examine the merger's impacts on the local land market, we collect land auction data from 2004 to 2015 published by the Land Bureau of China. Land in China is public, and its allocation is decided by the government. Since 2004, private developers can only obtain the "land development rights" through a public auction, with details of each transaction posted Land Bureau's website (Cai et al., 2013). For each parcel of land sold by the government to the developers, our data have detailed information such as the address, area, land use category, final sale value, date, etc. All prices are adjusted for inflation using GDP deflator with 2010 as the base year.

GIS data

The most important information we need in our identification is the geographic proximity of firms/land parcels to the border.

To get this measure, we first obtain township-level GIS maps of China in 2000 and 2010. By overlaying these two maps, we can visualize all the boundary changes happened between 2000 and 2010, as shown in figure 2.3. With the assistance of GIS software, we can systematically identify all the merging cases that occurred between 2000 and 2010 [6] . We also digitize the administrative records of township boundary changes published yearly by the Ministry of Civil Affairs (MCA) between 2002 and 2008[7]. With these official records, we are able to validate township merger cases inferred from the map and identify the exact year that each border changed.

Ideally, we would like to know each firm and land parcel's distance to the merging border, because the policy shock should be a function of this distance as described in the model. However, due to technical and resource constraints, we are unable to directly identify the border segments that become defunct. Instead, we compute each locations' minimal distance to a border in 2000 and 2010 respectively, and use the change in the minimal distance to border to proxy the policy shock to a location. If focusing on the border firms in 2000, a large increase in distance to the closest border means that the firm must be very close to the merging border prior to the

[5] More details of the database are described in Cai et al. (2016)

[6] Township splits or repeated merger in multiple years are rare. In our analysis, we keep only the townships that merged once, excluding township splits and repeated merger.

[7] The Ministry did not keep track of township-level changes until 2002 and was unable to provide us records for some years after 2008

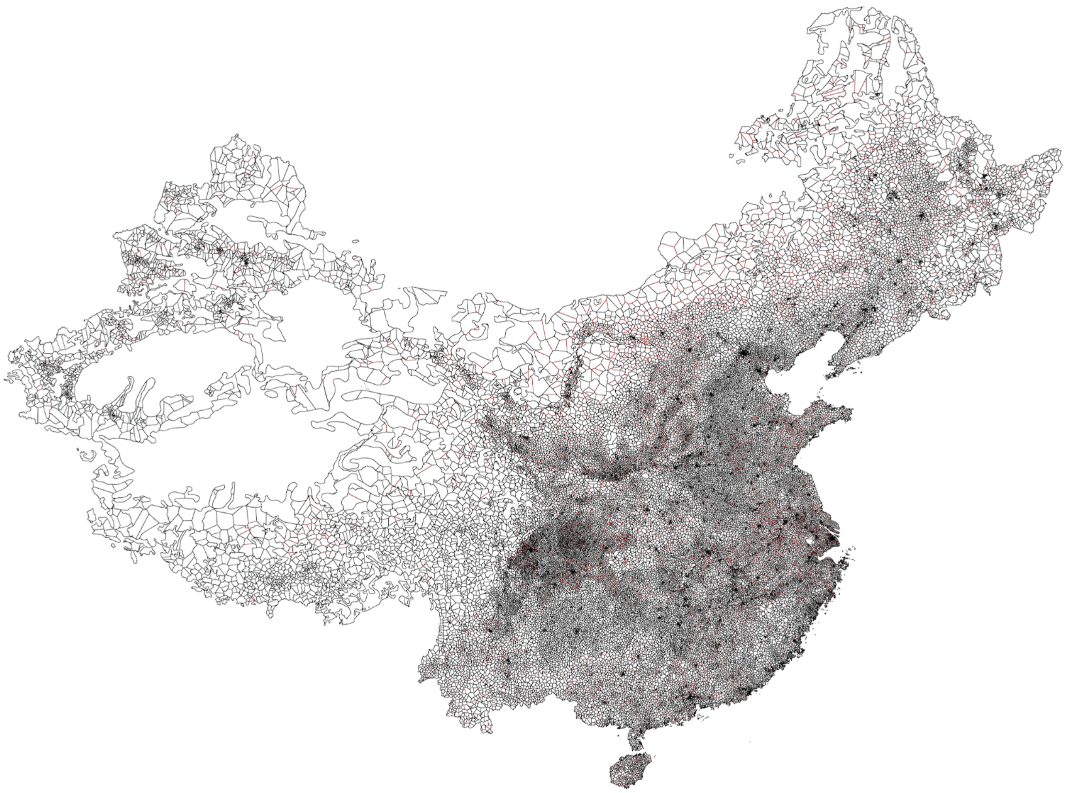


Figure 2.3: Township Boundary Changes[a]

[a]Disclaimer: All the maps presented in this dissertation are for illustrative purposes based on publicly available Geographic Information System (GIS) database. They do not imply any judgment or endorsement by the author to the legal status or frontier of any territory.

merger. Figure 2.5 shows the distribution of firms' distance to border in 2000 and 2010. It is apparent that a significant amount mass in the red distribution shift to the right, meaning the merger indeed have big impacts on firms' distance to the border. Figure 2.5 also includes the distribution of township centers' distance to border as a references. It shows that prior to the merger, firms are more likely to be located closer to the township border, which is consistent with literature findings.

In Figure 2.4, we use a real example of township border changes in a county to illustrate our approach. The red dash lines represent the 2000 borders that become defunct after the merger, whereas the black lines represent the official borders in 2010. We categorize firms into two groups based on changes in distance to border

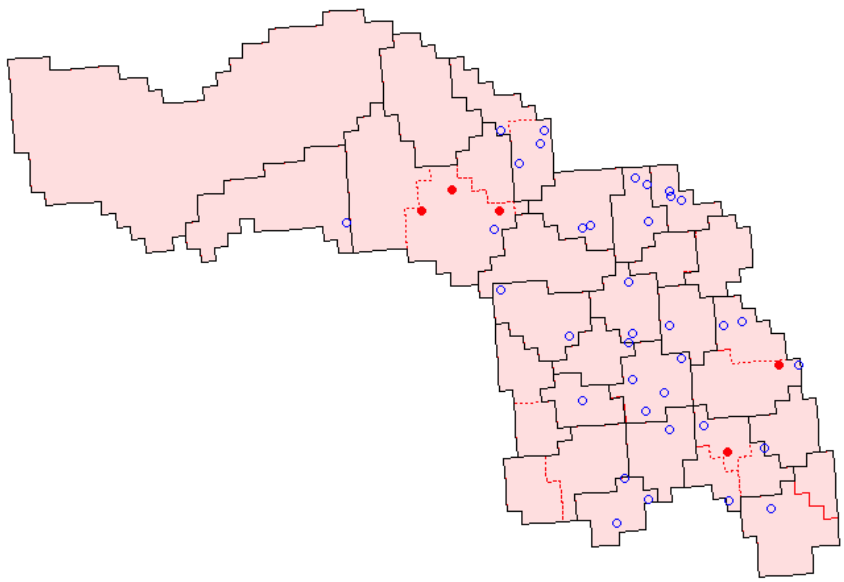


Figure 2.4: Illustration of Border Changes in one County

in 2000 and 2010. If the change in distance for a firm is larger than some small number ϵ [8], we label it as a red dot on the map, otherwise a blue circle. The red dots firms were all pretty close to the merging border, whereas the blue circle firms are not. After the merger, the red dot firms "move" closer to the center of the township and likely to be subject to stricter regulation according to our prediction. The discontinuous change in distance to border thus constitutes an intuitive measure of the "re-centralization shock" received by each firm.

Summary Statistics

Putting everything together, we compiled three datasets documenting firm production, firm emission and land auctions with firms and land parcels located in 3052 pre-2000 townships. Those townships were later consolidated into 1762 townships by 2010.

[8] We use an ϵ instead of 0 to tolerate measurement errors.

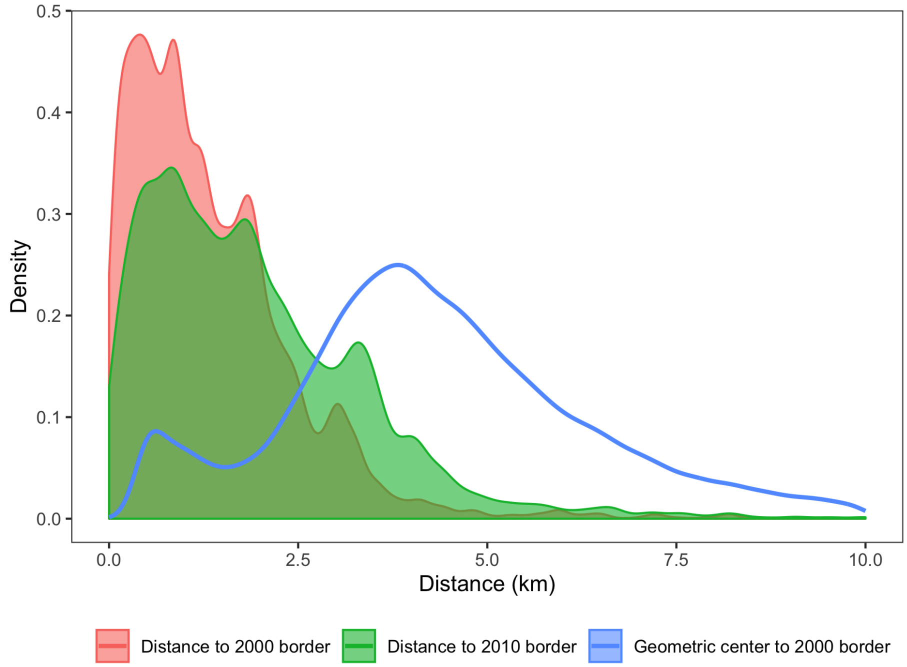


Figure 2.5: Distance to Border

Table 2.1 compares pre-merger descriptive statistics of the firms in the sample by the location. Panel A uses the firm production data and shows that there is no significant difference between the merging border firms and non-merging border firms before the mergers occur. In particular, there are similar shares of polluting firms near both types of borders. The average SO_2 emissions and abatement costs are also similar for firms on both locations, as shown in Panel B using Emission data. Those facts seem to suggest no disparity in environmental regulation between merging border and non-merging border. Firms are also not different in terms of productivity and size near both types of borders.

2.5 Empirical Strategy

As alluded to in previous sections, our empirical strategy focuses on sub-township level outcomes, exploiting the fact that border firms are similar before the merger but firms at the merging border will receive different treatments after the merger.

Table 2.1: Summary Statistics of Firms Prior to the Merger

	Non-merging Border (1)	Merging Border (2)	Difference (3)
Panel A: Production Data			
Dist10-Dist00 (km)	0.045	2.054	2.009***
	(0.123)	(1.993)	(0.021)
Polluting firms	0.408	0.420	0.012
	(0.492)	(0.494)	(0.009)
Firm Age	11.267	11.450	0.183
	(10.972)	(11.152)	(0.205)
Log Output	5.128	5.110	-0.018
	(1.095)	(1.083)	(0.020)
Log Value-added	3.781	3.776	-0.005
	(1.145)	(1.116)	(0.021)
Log Export	0.917	0.948	0.030
	(1.872)	(1.879)	(0.035)
Log Employment	0.871	0.861	-0.010
	(0.572)	(0.557)	(0.011)
TFP (ACF)	2.069	2.104	0.035
	(1.220)	(1.216)	(0.023)
Log Tax	0.246	0.236	-0.009
	(0.450)	(0.425)	(0.008)
Log Subsidy	0.196	0.215	0.019
	(0.621)	(0.636)	(0.012)
Observations	9,389	4,171	13,560
Panel B: Emission Data			
Log SO2	9.853	9.919	0.066
	(1.975)	(2.000)	(0.058)
Log Abatement Cost	2.318	2.275	-0.043
	(1.468)	(1.535)	(0.044)
Observations	3,511	1,742	5,462

Note: Standard errors are reported in the parenthesis.

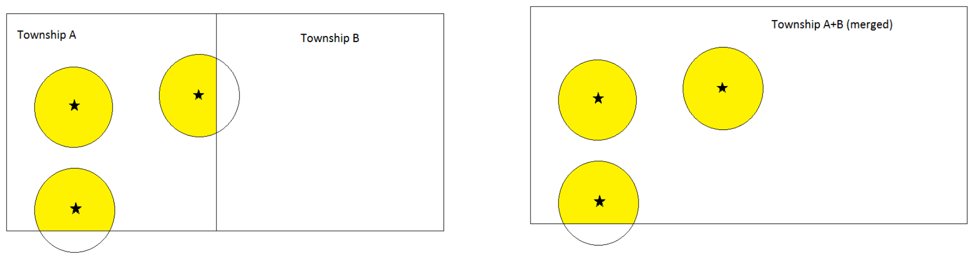


Figure 2.6: Identification

The intuition for our identification strategy is illustrated in Figure 2.6 with a simple case where township A and township B are merging. The firms located in township A could be classified into three categories according to their locations relative to township border: (1) those firms that are located close to the geographic center, so all their externalities are fully absorbed by township A itself; (2) those firms that are located close to a border that is NOT shared by townships A and B, then part of their externalities is jointly borne by another adjacent township; (3) those firms that are located close to the shared border of townships A and B, so part of their externalities is jointly borne by township B. The yellow shade in the circle around firms indicates the portion of externalities being internalized by township A. When the merger happens, the shared border between townships A and B disappears. As shown in the right panel in Figure 2.6, this merger has differential impacts on the three types of firms: for type (1) firms, their externalities remain fully absorbed by their own host township; for type (2) firms, their externalities remain partially borne by another adjacent township; but for type (3) firms, before the merger, all their externalities become fully absorbed by the new host township (A+B), in contrast to being only partially internalized by its original host township before the merger. Hence, if those type (1)/(2) firms can serve as a good counterfactual of type (3) firms experiencing no changes government's internalization decision, we can causally identify the causal effects of internalizing spillovers using a difference in difference approach.

To empirically test our predictions in section 2.3, we start by estimating the following difference-in-differences (DID) model using the firm production data:

$$Y_{ijst} = \alpha \cdot Distance_{ist} + \beta \cdot Distance_{ist} \cdot Polluting_j + \sigma_i + \lambda_{st} + \gamma_{jt} + \epsilon_{ijst} \quad (2.5.1)$$

where Y_{ijst} is the outcome of interest for firm i in industry j in township s in year t. $Distance_{ist}$ is the nearest geographic distance between firm i and the border of township s in year t. This variable increases for firms at the merging border after the integration but is constant for other firms. $Polluting_j$ is a dummy variable that equals to 1 if industry j is one of the 16 "heavily polluting" industries defined by the Ministry of Environmental Protection, and 0 otherwise. σ_i is the firm fixed effects. λ_{st} is the township-by-year fixed effects. And γ_{jt} is the industry-by-year fixed effects. ϵ_{ijst} is the error term. Standard errors are two-way clustered at the industry level and the province level.

The inclusion of firm fixed effects σ_i captures any time-invariant characteristics of the firm, such as any peculiarity of the location. Township-by-year fixed effects λ_{st} pickup township specific shocks, particularly the shocks from the merger. The industry-by-year fixed effects control for industry-specific shocks, for example, new industry-specific regulations, etc. The inclusion of firm, township-by-year, and industry-by-year fixed-effects in one specification makes the variation being used for identification highly restrictive: we are essentially comparing firms (in different locations relative to the merging border) within the same industry, the same township, and the same year, before and after the township merger happened. Therefore, if the timing of a township merger case is not driven by anticipation of a sudden change in outcome variable trajectory of the border and center firms within that township, α would identify the causal effect of a non-polluting firm being moved away from border by 1 kilometer, and β would identify the differential causal effect of a polluting firm being "moved" away from border by 1 kilometer compared to the non-polluting firms. The primary parameter of interest is β.

Besides impacts on firms' productivity, we can also investigate changes in regulation after the merger using the firm production data. The amount of taxes paid or the amount of subsidies received by the firms can provide insights on their relationship with local government. Tax and subsidy are the two major levers that a local township government can pull to influence business.

To study the impacts of merging on emissions, we need to use the emission data. Since there are only polluting firms in the emission data and theoretically non-polluting should always produce zero emissions, we estimate the following equation that does not include the interaction term $Distance_{ist} \cdot Polluting_j$.

$$Y_{ijst} = \beta \cdot Distance_{ist} + \sigma_i + \lambda_{st} + \gamma_{jt} + \epsilon_{ijst} \tag{2.5.2}$$

The interpretation of β in this equation is the same to the β in Equation .

Finally, we use the land transaction data to evaluate the impacts of the merger on the land market. Because each parcel in the land transaction data is only sold once, we can no longer control for parcel fixed effects as previous specifications require. We use two different ways to address this.

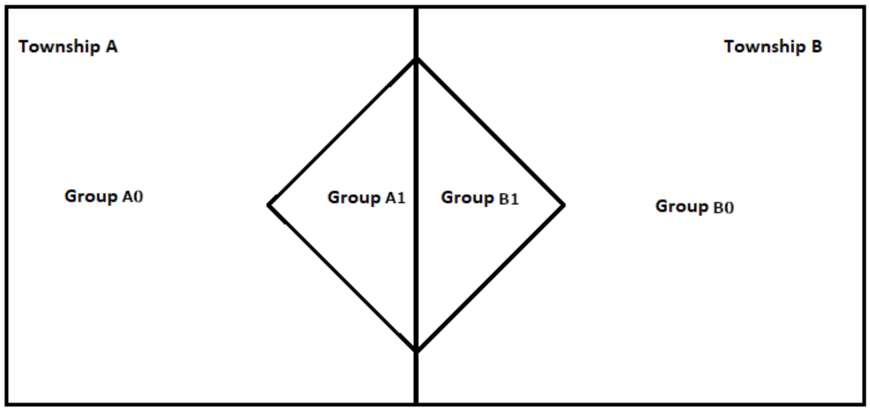


Figure 2.7: Residential Land: Extensive Margin

To study effects on quantities of residential land, we divide each township into two land areas, a *Merging Border* area and *Non-merging Border* area as illustrated in the Figure 2.7 to construct a panel data. [9]. Then with the constructed area-year panel, we can use a similar difference in difference method to estimate township merger's impacts on the likelihood of having any new residential development in the *Merging Border* area. Specifically, we estimate the following model:

$$\begin{aligned} Y_{ist} =& \alpha \cdot PostMerge_{st} + \beta \cdot MergingBorder_i \\ &+ \gamma \cdot PostMerge_{st} \cdot MergingBorder_i + \delta_s + \eta_t + \epsilon_{ist} \end{aligned} \tag{2.5.3}$$

where Y_{ist} is the outcome of interest for land area i in township s in year t. The $PostMerge_{st}$ is an indicator variable for whether township s in year t finished merger. $MergingBorder_i$ is also an indicator that equals to 1 if land area i is a *Merging Border* area, and 0 otherwise. δ_s and η_t, respectively, are township and year fixed effects. α identifies the overall impact of township merger, and β identifies the difference between *Merging Border* and *Non-merging Border* before the merger. The γ is the

[9]The *Merging Border* area is defined to be the areas whose distance to border increases more than 1.5km after the merger, and the rest of areas in the pre-merge township is defined as *Non-merging Border* area. Using larger than zero (1.5km) threshold help increase the power, because it excludes areas barely affected from the *Merging Border* area. The results are similar using different thresholds.

coefficient of interest. It estimates the average effects of changing from being a border area to a non-border area on the likelihood of new residential development.

To estimate the impacts on prices, we use a slightly different approach. Instead of creating land areas we define $log(\Delta Distance)_{it}$ to be the logarithm of the change in firm i's distance to the border after the township merger. We use this variable to proxy for an area's closeness to the merging border.

$$\begin{aligned} Y_{ist} =& \alpha \cdot PostMerge_{st} + \beta \cdot \log(\Delta Distance)_{it} \\ &+ \gamma \cdot PostMerge_{st} \cdot \log(\Delta Distance)_{it} + \delta_s + \eta_t + \epsilon_{ist} \end{aligned} \tag{2.5.4}$$

γ is still the coefficient of interest here, estimating the effects of changing distance to the border.

The identification of our central results on firm-level outcomes crucially depends on the assumption that there are no differential changes in the productivity trends for merging border firms and non-merging border firms in the same town and the same industry.

Nonetheless, there are many reasons why this assumption may not hold. For example, the merging borders and non-merging borders may be very different (one may be closer to transportation and have denser population than the other), therefore the firms located on different borders may also very different. We do not have sub-township social and demographic data, but the summary statistics of firms in Table 2.1 suggest that the firms on either type of border are similar in terms of the level of various characteristics before the merger. What's more, with the inclusion of township fix effects and industry fix effects, the comparison is between firms within the same township and the same industry.

One may also worry that firms have different political connections thus can directly influence the merging decision. But the merger is a lengthy political process and need to be approved by multiple levels of upper governments, it is reasonable to believe either group of firms does not hold much sway over the decision.

We will formally perform a falsification test to examine the parallel trend assumption. We keep only data before the merger and spuriously code the jump in $Distance_{ist}$ to be 1 year or 2 years before the actual merge year. If the difference in pre-trend exists, then the coefficients for the 1-year or 2-year lead variables of $Distance_{ist}$ should be statistically different from zero.

2.6 Results

Regulation changes and Economic Impacts

Our model in section 2.3 predicts that the merger will lead to an increase in the level of environmental regulation for the firms closer to the merging border. Because we do not have a direct measure of government regulation level, we use the amount of taxes paid and subsidies received by a firm as a proxy. It is commonly known that local Chinese governments often re-adjust the amounts of subsidies and tax breaks allocated to firms to interfere with firms' production decisions (Ma and Ortolano, 2000).

Column 1 and Column 2 in Table 2.2 presents the results of estimating Equation (3.4.2) for subsidies and taxes. Standard errors clustered at the province and industry level are shown in parenthesis. In the first column, the result suggests that for a non-polluting firm, "moving" further away from the border does not have any substantial impacts on the subsidies it receives. In contrast, a polluting firm receives approximately 1.2% fewer subsidies from the government when "moving" away from the border by 1 kilometer, as compared to non-polluting firms. This seems to suggest that local government increases pressures on merging border polluting firms after the merger, and in particular the pressure is higher for firms becoming further away from the new border. The result in the second column paints a similar picture. It shows that in comparison to non-polluting firms, polluting firms would pay 0.3% more tax if moving away from the border by 1 kilometer. Both coefficients for *Distance*Polluting* in Columns 1 and 2 are statistically significant at the 1% level.

These results are consistent with our model prediction, suggesting the local government is indeed internalizing border pollution and penalizing polluting firms that become farther away from the new border. To better understand the size of the estimated effect, we can do a simple thought experiment: if an average-sized township has the shape of a perfect circle, then its radius would be approximately 4km (see Figure 2.5). Therefore, our coefficients suggest that in a representative township, regulating a polluting firm near the township border at the same level as it was located in the geometric center would mean more than a 4.8% drop in government subsidy allocation and a 1.2% increase in tax collection for local government.

Table 2.2: Baseline Results: Firm Production Dataset

	(1) Log Gov Subsidy	(2) Log Tax	(3) Log Value-added	(4) Log TFP (ACF)
Distance	0.0047	0.0016	0.0162**	0.0045
	(0.0072)	(0.0058)	(0.0067)	(0.0051)
Distance*Polluting	-0.0116***	0.0030***	-0.0184**	-0.0142***
	(0.0033)	(0.0007)	(0.0084)	(0.0051)
Constant	0.2073***	0.3297***	3.9364***	2.5182***
	(0.0103)	(0.0099)	(0.0135)	(0.0095)
Mean of Dep Variable	0.207	0.335	3.950	2.516
Firm FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes
Township-Year FE	Yes	Yes	Yes	Yes
Number of Observations	159650	202924	157886	124260
R squared	0.677	0.612	0.845	0.780

Note: Standard errors are two-way clustered at the province and industry levels.

We further investigate the productivity impacts caused by such internalization in Columns 3 and 4. Interestingly, despite no significant impacts on TFP, a non-polluting firm is estimated to increase its value added by 1.6% if "moving" away from the border by 1 kilometer. But such benefits are not enjoyed by the polluting firms. Relative to non-polluting firms, a polluting firm would decrease its value added and have lowered TFP if it is being regulated as if it were farther away from the border. Specifically, it would be a 1.8% decrease in value added and a 1.4% decrease in TFP. These negative impacts specific to polluting firms indicate that the productivity losses may be resulted from the more stringent environmental regulation after the merger, as predicted in the model.

As discussed in the section 3.4, we estimate the same specification excluding the type (1) firms - the ones are far away from the border, to begin with. Appendix Table 2.12 reports the results after using only firms less than 4km away from the border before the merger. The estimates are very similar and exhibit the same pattern.

Table 2.3: Pre-trend: 1-year lead

	(1) Log Gov Subsidy	(2) Log Tax	(3) Log Value-added	(4) Log TFP (ACF)
Lead1.Distance	-0.0005	0.0014	-0.0062	-0.0032
	(0.0003)	(0.0091)	(0.0043)	(0.0092)
Lead1.Distance*Polluting	0.0003	0.0019	-0.0011	-0.0048
	(0.0006)	(0.0050)	(0.0127)	(0.0138)
Mean of Dep Variable	0.201	0.209	3.978	2.361
Firm FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes
Township-Year FE	Yes	Yes	Yes	Yes
Number of Observations	63924	66543	70977	68390
R squared	0.471	0.665	0.822	0.795

Note: Standard errors are two-way clustered at the province and industry levels.

Table 2.4: Pre-trend: 2-year lead

	(1) Log Gov Subsidy	(2) Log Tax	(3) Log Value-added	(4) Log TFP (ACF)
Lead2.Distance	-0.0003	-0.0053	-0.0088	-0.0022
	(0.0008)	(0.0065)	(0.0081)	(0.0118)
Lead2.Distance*Polluting	0.0007	0.0035	0.0002	-0.0035
	(0.0009)	(0.0047)	(0.0137)	(0.0096)
Mean of Dep Variable	0.201	0.209	3.978	2.361
Firm FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes
Township-Year FE	Yes	Yes	Yes	Yes
Number of Observations	63924	66543	70977	68390
R squared	0.471	0.665	0.822	0.795

Note: Standard errors are two-way clustered at the province and industry levels.

Table 2.3 and 2.4 presents the results of falsification test for parallel trend assumption. The results from using the 1-year lead variable and the 2-year lead variable both show that there are no significant pre-existing differential trends in the government subsidy, tax payment and industrial productivity for both non-polluting and polluting firms. The future changes in firms' distance to the border do not predict the outcomes of interest before the township merger happens.

Environmental Impacts

To investigate whether the differential treatments that polluting firms received from local government are indeed about environmental regulation, we study the merger impacts on pollutant emissions using the emission data. We estimate Equation 2.5.2 with *Log* SO_2, *Log* NO_2 and *Log Cleaning Cost* as outcome variables and report the results in Table 2.5. The SO_2 and NO_2, the two most common pollutants in China, are believed to be the culprits behind China's smog and are usually the focus of local environmental regulation. The cleaning cost is the amount of expenditure the firm spent to mitigate the pollution. The results suggest that "moving" away from the border by 1 kilometer would decrease polluting firms' SO_2 emissions by 1.9% and NO_2 emissions by 10%, but increase firms' cleaning cost by 1.6%. The number of observations is smaller for columns 2 because statistics for NO_2 emissions were not collected until 2006.

It's noteworthy that despite that the polluting firms are producing less (or equal) outputs as shown in Table 2.2 after the merger, they are spending more to actively mitigate the pollution. This presents a piece of strong evidence that the polluting firms are subject to increased environmental regulation after the merger.

Table 2.5: Emission Results

	(1) Log SO2	(2) Log NO2	(3) Log Abatement Cost
Distance	-0.0257***	-0.0764**	0.0263**
	(0.0062)	(0.0295)	(0.0097)
Mean of Dep Variable	9.751	8.958	1.102
Firm FE	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes
Township-Year FE	Yes	Yes	Yes
Number of Observations	65047	26662	87219
R squared	0.882	0.917	0.833

Note: Standard errors are two-way clustered at the province and industry levels.

Impacts on Land Market

Next, we examine the impacts on the local land market in terms of both quantity and price change. In Table 2.6, we present the results of estimating Equation 2.5.3. As described in section 3.4, we divide each township into two areas and define a dummy variable *Residential Construction* that equals 1 if any residential development occurs in the area. Column 1 shows that after the merger, the merging border areas are 12.5 percentage points more likely to see a residential construction whereas the merger has no significant effects on the non-merging border areas. What is also interesting about the result is that before the merger the merging border areas are 14 percentage points less likely to see a residential construction and the gap is almost closed after the merger. Column 2 controls for the area fixed effects and reports similar results.

Table 2.6: Likelihood of New Residential Development

	Residential Construction (Dummy)	
	(1)	(2)
Merging Border	-0.1417***	
	(0.0481)	
PostMerge	0.0125	-0.0010
	(0.0284)	(0.0324)
PostMerge * Merging Border	0.1252***	0.1190*
	(0.0477)	(0.0623)
Mean of Dep Variable	0.461	0.472
Township FE	Yes	No
Group FE	No	Yes
Year FE	Yes	Yes
Number of Observations	29119	28152
R squared	0.347	0.461

Table 2.7 presents the estimates of the impacts on the residential land price. We use the logarithm of per unit residential land price as the outcome variable. Column 1 reports the results of estimating Equation 2.5.4. Consistent with the model prediction, the residential land prices rise after the merger for areas with a big increase in distance to the border: a 10% increase in distance to border leads to

approximately a 4% increase in price. Column 2 controls more characteristics of the land and yields a similar estimate.

Table 2.7: Residential Land Price

	Residential Land Price(log)			
	(1)	(2)	(3)	(4)
PostMerge	0.1184	0.1254	0.1171	0.1314
	(0.1505)	(0.1478)	(0.1307)	(0.1297)
Log(Dis10-Dis00)	-0.3863**	-0.3760**	-0.1142	-0.1151
	(0.1572)	(0.1549)	(0.2483)	(0.2513)
PostMerge*Log(Dis10-Dis00)	0.4089***	0.3980***	0.0282	0.0288
	(0.1517)	(0.1492)	(0.2803)	(0.2832)
PostMerge*Polluting Border			0.0879	0.0609
			(0.3120)	(0.3147)
Log(Dis10-Dis00)*Polluting Border			-0.1719**	-0.1727**
			(0.0584)	(0.0543)
PostMerge*Log(Dis10-Dis00)*Polluting Border			0.3268***	0.3271***
			(0.0458)	(0.0410)
Log Land Area		-0.1007***		-0.0941**
		(0.0213)		(0.0385)
Land Quality		0.1043***		0.1050**
		(0.0349)		(0.0433)
Mean of Dep Variable	14.991	14.991	15.045	15.045
Township FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Transaction Type FE	Yes	Yes	Yes	Yes
Number of Observations	79115	79115	73155	73155
R squared	0.682	0.683	0.678	0.679

To further validate that the causal mechanism of price change is indeed through emission reduction, Column 3 and 4 employ a triple difference design. We define a dummy variable *Polluting Border* to be 1 if least a polluting firm is located on the merging border (whose change in distance to the border is larger than 1.5km).

By including the interaction terms with *Polluting Border*, we compare the pairs of merging townships with polluting firms on the sharing border to those merging-pairs without. Such comparison is illustrated in the Figure **??**. There seem not to be any significant impacts on merging border residential land prices if there are no polluting firms. But in contrast, when the merging border includes a polluting firm, the residential land price for the sharing border area will increase after the merger and the effect size is only slightly smaller than those estimated in columns 1 and 2. These results suggest that the majority of impacts of the merger on residential land prices come through the pollution reduction channel.

2.7 Robustness

In this section, we present some robustness checks to assess the validity of the empirical results.

Wind Flow Directions

Up to this point, we have assumed the impacts of pollution are evenly distributed in the surrounding area both in our model and empirical exercises. However, in the case of air/water pollution, the majority of the impacts will be borne by the areas at the downstream of the polluting source.

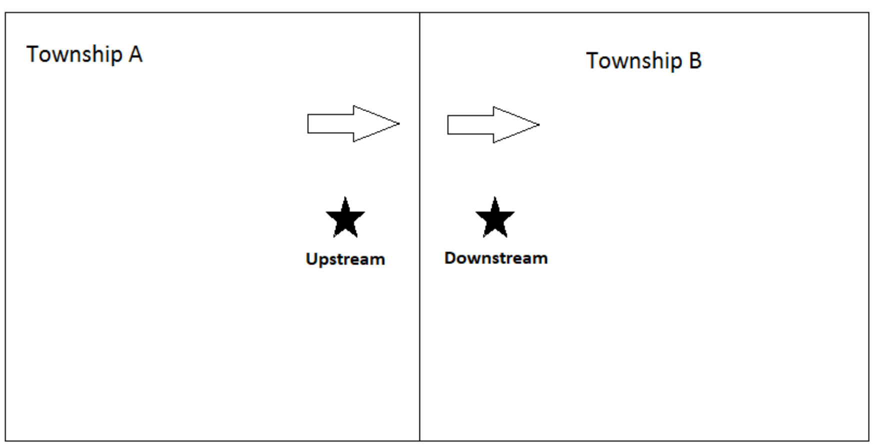


Figure 2.8: Wind Flow Directions

Consider a two-township scenario with a perennial wind blowing from the west to the east as shown in Figure 2.8. For polluting firms near the merging border in the downstream town (township B), any pollutants they release will be carried over by the wind to the center of the town. Therefore, the township merger will barely have any impacts on those merging border firms in the downstream town because regardless of the merger, township B has to bear the full costs of the pollution from those firms. In contrast, the merging border firms in the upstream town will be heavily affected by the township merger because only minuscule externalities from those firms were internalized before the merger.

We use National Surface Climate Data to find townships with consistent wind flow in one direction and determine those townships' relative location to their merging border, label it upstream or downstream. Then we repeat the baseline estimations separately for upstream townships and downstream townships. The estimation results for firm production outcomes are reported in the Table 2.8. The coefficients for *Distance*Polluting* in the upstream sample are all significant and on the similar (or larger) magnitude as our baseline estimates, whereas those coefficients are close to zero in the downstream sample. This result is consistent with the theoretical predictions, providing further evidence that the observed impacts of the merger came through the pollution channel.

Table 2.8: Wind Flow: Firm Production

	Log Gov Subsidy		Log Tax		Log Value-added		Log TFP (ACF)	
	(1) Upstream	(2) Downstream	(3) Upstream	(4) Downstream	(5) Upstream	(6) Downstream	(7) Upstream	(8) Downstream
Distance	0.0041	0.0115	0.0011	0.0017	0.0193	0.0089	0.0099	0.0021
	(0.0134)	(0.0094)	(0.0014)	(0.0022)	(0.0134)	(0.0096)	(0.0137)	(0.0113)
Distance*Polluting	-0.0141***	-0.0025	0.0021**	-0.0016	-0.0315**	-0.0011	-0.0336***	-0.0083
	(0.0022)	(0.0147)	(0.0010)	(0.0041)	(0.0127)	(0.0132)	(0.0117)	(0.0143)
Mean of Dep Variable	0.225	0.234	0.107	0.111	3.996	3.930	2.559	2.569
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Township-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	56540	55858	57083	58346	55458	56178	44566	44579
R squared	0.699	0.699	0.545	0.565	0.851	0.857	0.782	0.784

We also perform the same exercise to investigate firm emission outcomes and report the results in Table 2.9. The estimates are noisier for emission outcomes because of the limited sample size, but they exhibit a similar pattern to the production results.

Table 2.9: Wind Flow: Firm Emission

	Log SO2		Log NO2		Log Abatement Cost	
	(1) Upstream	(2) Downstream	(3) Upstream	(4) Downstream	(5) Upstream	(6) Downstream
Distance	-0.0144	-0.0099	-0.0523***	-0.0627	0.0303**	0.0016
	(0.0120)	(0.0303)	(0.0160)	(0.0780)	(0.0140)	(0.0268)
Mean of Dep Variable	9.784	9.820	9.025	9.116	1.142	1.173
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Township-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	18545	19900	7471	8146	24845	25627
R squared	0.903	0.900	0.932	0.928	0.853	0.870

Alternative emission results

Since our emission variables and production variables come from different datasets and can not be directly linked, one may be concerned that the observed results are due to some inconsistency between two datasets. To address this, we investigate the impacts of the merger on emission using the "emission fee" variable in the firm production dataset. The "emission fee", documenting the amount of money each firm paid to the government for their emission level, can serve as a proxy to the firms' emissions.

Unfortunately, the "emission fee" was only reported in 2004. To conduct a DiD analysis using just 2004 data, we need a stronger assumption than previously used: we assume that among all the townships that merged during the 2002-2008 time window, whether the merger happened before 2004 or after 2004 is as good as randomly assigned. Under this assumption, we could compare "the difference between merging border firms and non-merging border firms in a township that has already merged in 2004" to "the difference between merging border firms and non-merging border firms in a township that has not merged yet in 2004," which provides the DiD estimate for the impacts on border firms.

Formally, we estimate the following econometric specification:

$$Y_{is} = \alpha \cdot Merged_s + \beta \cdot \log\left(\Delta Distance_{is}\right) + \gamma \cdot Merged_s \cdot \log\left(\Delta Distance_{is}\right) + \epsilon_{is} \tag{2.7.1}$$

where Y_{is} is the log amount of emission fee paid by firm i in township s in 2004. $Merged_s$ is a dummy variable which equals 1 if township s has already merged in

2004, and 0 otherwise. $\log(\Delta Distance_{is})$ is the same as defined above in Equation 2.5.4.

We estimate Equation 2.7.1 for both polluting firms and non-polluting firms and report results in Table 2.10. The results in columns 1 and 2 show that the merger leads to significantly less emission fees for the polluting firms. This is highly consistent with our theoretical prediction and empirical results using the emission dataset. Moreover, the magnitude of such reduction is similar to the estimate from the emission dataset in Table 2.5.2. In contrast, the estimated effect of the merger is indistinguishable from zero for non-polluting firms as shown in columns 3 and 4.

Table 2.10: Pollution Fee in 2004

	Polluting Firms		Non-polluting Firms	
	(1) Log Emission Fee	(2) Log Emission Fee	(3) Log Emission Fee	(4) Log Emission Fee
Merged	-0.0035	0.0078	-0.0023	0.0058
	(0.0094)	(0.0109)	(0.0043)	(0.0047)
Log(Dis10-Dis00)	0.0299***	0.0221**	0.0045	0.0000
	(0.0087)	(0.0092)	(0.0044)	(0.0045)
Merged*Log(Dis10-Dis00)	-0.0351**	-0.0343**	0.0061	0.0039
	(0.0145)	(0.0151)	(0.0076)	(0.0077)
Mean of Dep Variable	0.125	0.126	0.045	0.045
Province-Industry Fixed Effects	No	Yes	No	Yes
Polluting Industry	Yes	Yes	No	No
Number of Observations	8150	8048	11322	11199

SUTVA

Since we are comparing firms within the same township, one potential concern is that the non-merging border firms are also affected by the increased emission regulation on merging border firms after the integration, which means the Stable Unit Treatment Value Assumption (SUTVA) is violated. For instance, there might exist a "monitoring constraint" for township governments. When a township government decides to spend more effort monitoring the merging border firms, they mechanically have to spend less effort monitoring other firms if bounded by the "monitoring constraint". If this is the case, our baseline specification would over-estimate the effects of internalizing border emissions.

To test this, we replace the township-year fixed effects in baseline specification with the county-year fixed effects. If the SUTVA violation is indeed driving the main result, then the baseline result should disappear after relaxing the control group from "firms within the same township" to "firms from the same county" because the treatment and control groups would no longer be subject to the same monitoring constraint.

The results are shown in the Table 2.11. We find that the estimated coefficients with county-year fixed effects are barely different from the baseline coefficients, indicating that the violation of SUTVA should not be a major concern.

Table 2.11: Testing SUTVA Violation

	(1) Log Gov Subsidy	(2) Log Tax	(3) Log Value-added	(4) Log TFP (ACF)	(5) Log SO2	(6) Log NO2	(7) Log Abatement Cost
Distance	0.0071	-0.0001	0.0169***	0.0071*	-0.0221**	-0.0789**	0.0209*
	(0.0055)	(0.0060)	(0.0049)	(0.0037)	(0.0081)	(0.0351)	(0.0107)
Distance*Polluting	-0.0116***	0.0027*	-0.0231**	-0.0122**			
	(0.0017)	(0.0013)	(0.0089)	(0.0057)			
Mean of Dep Variable	0.207	0.335	4.041	2.513	9.752	8.962	1.101
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	160304	203706	124721	124721	65982	27001	88152
R squared	0.673	0.605	0.823	0.779	0.878	0.914	0.828

Excluding Non-border Firms

In our baseline specification, we included non-border firms (type (1) firms described in Section 3.4) in the sample as part of the control group because there is no clear-cut method to differentiate border firms from non-border firms without relying upon an arbitrary threshold.

Given that firms strategically choose their locations, it is possible that firms located closer to the center of the township are different from those on the border in some unobserved dimensions. For example, they may have stronger ties with the local governments. If those unobserved characteristics also change after the merger, they might confound our baseline results.

To address this, we exclude firms that were more than 4km away from the border in 2000 and re-run the baseline specification. The results are presented in Table 2.12 and are very similar to those estimated in the baseline specification. We also tried using different distance cut-off to restrict sample, and the results are robust. These patterns suggest that our results are not confounded by some unobserved differences between non-border firms and border firms.

Table 2.12: Refinement

	(1) Log Gov Subsidy	(2) Log Tax	(3) Log Value-added	(4) Log TFP (ACF)	(4) Log SO2	(6) Log NO2	(7) Log Abatement Cost
Distance	0.0048	0.0003	0.0151*	0.0040	-0.0257***	-0.0764**	0.0263**
	(0.0067)	(0.0062)	(0.0086)	(0.0060)	(0.0062)	(0.0295)	(0.0097)
Distance*Polluting	-0.0114***	0.0047***	-0.0167*	-0.0083*			
	(0.0032)	(0.0013)	(0.0098)	(0.0044)			
Mean of Dep Variable	0.207	0.332	4.037	2.510	9.751	8.958	1.102
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Township-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	155867	198049	121693	121693	65047	26662	87219
R squared	0.679	0.609	0.825	0.780	0.882	0.917	0.833

2.8 Discussion

The findings in the previous sections suggest that, when the local governments make coordinated decisions following the merger of townships, they would internalize border emission spillovers and impose tighter environmental regulation on polluting firms located at the merging border. Such a change in regulation enforcement then leads to significant reductions in emissions and decreases in value-added for polluting firms at the merging border.

These results mean that the incoordination between local jurisdictions leads to a Pareto-inefficient equilibrium where both pollutions and economic outputs are over-produced compared to their optimal levels. And empirically, the efficiency loss from the incoordination seems to be significant. To get a sense of the magnitude, we conduct a back-of-envelope calculation to estimate the environmental and economic effects if all the township governments take the emission spillovers into consideration when they enforce regulations.

According to our baseline estimates, if all the polluting firms in our sample were regulated as if they are in the center of the township in 2000, those firms will reduce their total value-added each year by 5.1%, a 60 billion Chinese yuan (8.48 billion USD) decrease. In 2000, the industrial value-added in China approximately 4 trillion Chinese yuan (559 billion USD), 45% of which was contributed by the polluting industries. So the estimated value-added reduction from the polluting firms in our sample alone is equivalent to 1.5% of national industrial value-added from polluting firms in 2000.

The total SO_2 emissions from those firms in 2000 would decrease by 6.7%, and their total NO_2 emissions would decrease by 20%. This reduction in percentage is significant, considering it is achieved by only assuming local governments can make coordinated decisions without any fundamental changes in the central government's priority. In fact, these estimates seem to be on a similar magnitude as what a national top-down approach can achieve. In the 13th Five-Year Plan (2016 to 2020) published by the central government, the emission goal of 2020 is set to be a reduction of 15% for both SO_2 emissions and NO_2 emissions 15%, from their respective 2015 level.

Because location information is needed for the calculation above, we are unable to compute out of sample for national impacts on pollution and value-added. The impacts on smaller firms not included in our data could be even larger because smaller firms tend to have thin margins and are more likely to shut down under increased enforcement of environmental regulation.

2.9 Conclusion

This paper aims to contribute to the big debate about centralization versus decentralization by empirically estimate the economic and environmental impacts of internalizing border spillovers. The "township merger program" in China, where thousands of pairs of neighboring townships were merged over the last two decades, provides a good opportunity to study the border pollutions problem because the border changes introduce exogenous variations in firms' distance to the border. Exploiting the panel variations in firms' distance to the border and using firms at non-merging borders to proxy counterfactual enable us to adopt a stronger identification strategy than previously used in the literature.

To get a comprehensive understanding of the impacts, we compile a novel panel dataset that consists of multiple sources of information. We start with two township GIS maps in 2000 and 2010. Then we geo-code all the firms and parcels in the firm production data, firm emission data, and land transaction records so that those economic statistics can be linked to the changes in the border. Administrative records of townships boundary changes from the Ministry of Civil Affairs are also digitized and incorporated into township GIS maps to help identify the year of border change.

Our empirical results show that when local governments make coordinated decisions, they could indeed better internalize border environmental spillovers. Local governments would use government subsidies and tax collection as levers to pressure polluting firms to reduce emissions. We also found evidence that both land prices and new developments of residential buildings as a result of the internalization of border pollutions, indicating that household welfare increases. The reduced border pollutions comes at the cost of some productivity loss though, as firms need to reduce outputs and reallocate productive capital to abatement equipment.

Taken together, our results suggest that when local governments in China are acting independently, they are not able to perfectly coordinate or negotiate to fully internalize the border spillovers. The distortion from such failure in coordination between local governments is economically significant. More pollutions and economic outputs are produced than what governments would like if they can make joint decisions.

Our findings have important policy implications. First, as the Township Consolidation Program is still ongoing, our estimates can help policymakers to make projections when deciding which pairs of townships to merge with each other. Second, our results are relevant in other contexts for designing optimal environmental regulation policies. A key lesson from our estimates is that spillovers can be internalized through coordinated decision-making. And the cooperation of local governments can be achieved in many ways without forcing an annexation. For example, a regional emission regulation commission may be established to take control of the decision on environmental regulation policies similar to the transportation commissions in many metropolitan areas.

It is important to note that our findings do not yield a clear-cut prediction for overall welfare impacts from the consolidation of township governments. Besides making joint decisions over environmental regulation, there are many other important advantages and disadvantages of merging local governments. For example, the merged governments may utilize economies of scale to provide public goods more efficiently, which improves the welfare of citizens. But larger administrative areas means governmental services become less accessible to the citizens that live in the remote area, therefore decreasing their welfare. Our empirical approach is agnostic of the impacts from those other channels because any impacts affecting the entire township are captured by the year fixed effects for the control group. As a result, our estimated effects are only partial effects of the township consolidation through the environmental regulation channel and we are unable to answer overall welfare effects of the township consolidation. More research is needed to answer this important question.

Chapter 3

Home City Connection and Bureaucrat Performance

3.1 Introduction

Governments all over the world (though to varying degrees) consist of both elected representatives (politicians) and nonelected bureaucrats. The key difference between politicians and bureaucrats is that politicians are held accountable through elections, whereas bureaucrats are accountable to their superiors for how they have fulfilled the goals of their organization (Alesina and Tabellini, 2007). This big distinction means that the population has much less influence over the performance of bureaucrats than the performance of politicians, which is already not easy. While abundant literature has studied how politicians behave and respond to citizens' demand (e.g., Ferejohn, 1986; Adsera et al., 2003; Snyder Jr and Strömberg, 2010; Fujiwara, 2015), understanding bureaucrats' behaviors is still a nascent research area (Finan et al., 2017).

Without being directly held accountable by the people they serve, bureaucrats who are a key determinant of government performance do not necessarily implement the policies that citizens want. First, it is possible superiors of bureaucrats have their own agendas and set the goals not aligned with the best interest of the populace. With their careers on the line, the bureaucrats' incentive is more likely to be aligned with their superiors than with the general public. Second, even with perfect intentions from superiors, any performance scheme used to incentivize bureaucrats to achieve policy goals can create severe multitasking problems, where bureaucrats focus on the incentivized aspect of their job at the expense of the non-incentivized aspects (Finan et al., 2017; Holmstrom and Milgrom, 1987).

Therefore, it's important to understand whether and how bureaucrats respond to other incentives besides career concern and pecuniary incentive. This paper investigates whether appointing bureaucrats in the place where they originate would improve or impair their performance. Civil services typically have flexibility in assigning bureaucrats to postings in different locations. If the affinity to the place bureaucrats serve can help enhance their performance, it will be a very cost-effective way to upgrade overall bureaucracy performance, hence have significant impacts on development and growth.

Theoretically, the direction of the impacts is ambiguous because there are many competing forces at play. Native bureaucrats differ from outsiders mainly in three aspects. They likely have better local information, more affinity to the local community, are more concerned with local reputation. Incidentally, those three aspects correspond to the three components in Bénabou and Tirole (2006)'s model for prosocial behavior, i.e. greed, altruism and concerns for social reputation.

Informational advantage on local environment and connection in local social network could be abused for personal gain (Xu, 2018), and also lead to entrenched interest group which results in insubordination (Li, 2019). Exactly for those reasons, the rulers of ancient China have long held beliefs that allowing native officials serving in their hometown is detrimental to the stability of their bureaucracy system. Fearing nepotism and corruptions, Han dynasty initiated and enforced the "rule of avoidance" in the personnel appointment process, which forbade any officials to serve in his home county (Qian, 2000). The avoidance rule was inherited and perfected by later dynasties and still has impacts on modern day Chinese political system. In 2002, the Regulations on the Selection and Appointment of Party and Government Leaders were promulgated, and stated that head or ranking members of county governments must not be served by officials who grew up in the county. Appointing native officials as city heads is allowed but generally advised against.

However, with the affinity to their hometown, native bureaucrats' local information could be used for improving governmental services. Many literature on targeted transfer program have shown that local leaders and communities often have better information about who should be assisted than central governments and can achieve higher satisfaction (Alderman, 2002; Alatas et al., 2012; Fisman et al., 2017). Moreover, Alatas et al. (2019) found that the elite capture is economically insignificant in their context and much smaller than potential welfare improvement from correcting the targeting errors. In recent years, recognizing the importance of local knowledge, many governments in developing countries are shifting towards localization of public service delivery Bank (2004); Bandiera et al. (2018); Casey (2018). Literature on community-driven development finds that agents recruited from the local communities are performing better due to the informational advantages they have.

Native bureaucrats may be also obliged to behave in hometown because they are concerned with local reputation. Their embeddedness in the local social network means they are personally connected to people who experience the consequences of their policies. Local community might be able to use informal channels to hold native bureaucrats accountable but not outsiders, since people in the community are more likely to have repeated transactions with native bureaucrats (Tsai, 2007; Persson and Zhuravskaya, 2016).

In this paper, I empirically investigate the relationship between bureaucrats' social proximity to their workplace and their performance. I compiled a dataset of biographical and career information of Chinese officials, and match them with various economic statistics between 1996 and 2015 to empirically investigate the relationship between bureaucrats' social proximity to their workplace and their performance. As the highest level of local authority not under the direct control of central authorities, municipal governments have substantial autonomy and authority in policymaking and providing public goods and services. Hence, I follow the literature on the Chinese political economy to use economic statistics of the city where municipal officials serve to measure their performance (Jia et al., 2015; Yao and Zhang, 2015; Persson and Zhuravskaya, 2016). I also estimate the impacts of native municipal leaders on local firms, as literature has shown that there is extensive reciprocity between Chinese local officials and local firms (Chen and Kung, 2019; Lei, 2018; Bai et al., 2014; Fang et al., 2018).

To address the endogeneity issue in appointment decisions of native bureaucrats, I exploit two sources of variation to construct an instrumental variable. First, I use the exogenous variations in leadership vacancy introduced by established rules such as term limits, age limits and cadre rotation system. This creates variations in the time dimension. Second, I exploit the changes in the composition of the Provincial Party Standing Committee to introduce variations in the probability of a city receiving native officials. The Provincial Party Standing Committee, whose members are provincial leaders appointed by the central government, is in charge of personnel management decisions. Because Standing Committees members are normally native officials who had experience working in some city governments within the province, their connections to the cities often plays a role in appointing municipal leaders. As the composition of Standing Committees varies from year to year, the chance of receiving a native official for each city also changes. I discuss the construction and validity of the instrumental variable in detail in section 3.5, and further provide suggesting evidence for satisfying exclusion restriction.

My results show that the municipal leaders' biographical background indeed plays an important role in their governance decisions. Natives, who grew up in the city they serve, would implement policies that lead to a 7% reduction in total tax revenue. Tax incomes from three major sources declined significantly. This result is consistent with evidence from firm-level data: the firms are paying about 4.7% less in taxes under native city leaders despite seeing increases in outputs and profits. Further examination on firm-level data suggests only firms in the home counties of native leaders benefit from the tax breaks. With respect to budgetary policies, native officials exhibit a pro-social tendency. Relative to outsiders, they allocate a higher share of municipal expenditure to education and health care (1.3 percentage points or 6% increase), and a lower share to infrastructure spending (1 percentage points or a 15% decrease). However, objectively, the actual outcomes of public goods provision deteriorate under the native city leadership, in terms of almost all measuresI studied. The number of hospital beds, doctors, and public school teachers decline, but emissions from polluting firms increase. Interestingly, pollutions are even worse at the home counties of the native officials compared to other parts of the city, which suggests that the recipient of the favoritism displayed in the tax breaks distribution may not be the general population in the home counties.

Taken together, my results suggest that social proximity hampers bureaucrat performance and facilitates local favoritism from the perspective of the general public. Although I do not have direct evidence on kickbacks or corruption, my results are consistent with the depiction of "Chinese-style crony capitalism" as discussed in literature (Lei, 2018; Bai et al., 2014; Fang et al., 2018). According to Bai et al. (2014), local governments would utilize political and economic powers to support the businesses connected to them regardless of ownership, and then implicit arrangements will be made to personally benefit political leaders from the success of their cronies. Appointing native officials the city seems to only exacerbate this phenomenon, and without many positive impacts on public goods provision.

The remainder of this paper is organized as follows. In section 3.2, I briefly introduce the institutional background of the Chinese bureaucracy system. In section 3.3, I describe the datasets and variables used in this project. I then present the empirical identification strategy and estimation results in section 3.4 and 3.5. In section 3.6, I discuss potential limitations in my baseline results and conduct robustness checks. Section 3.7 concludes the paper.

3.2 Background

Institutional background

Local governments in China are divided into four tiers: province, city/municipality, county and township in descending order. The analysis of this paper focuses on the city level.

There are about three hundred of municipalities across 31 provinces in China. They are roughly equivalent to Metropolitan Statistical Areas (MSA) in the United States in terms of area and population. An average-sized city approximately spans 28,000 km^2 and has an average population of 4.2 million in 2015. As the highest level of local authority not under the direct control of central authorities, municipal governments have substantial autonomy and authority in policymaking and providing of public goods and services.

Each municipality has two leaders: mayor and municipal secretary. The former is the head of municipal government, directly involved with decision makings and policy implementations. The latter is the municipal branch of the Communist Party, officially responsible for party affairs at a municipal level. But since a secretary ranks higher than the mayor, it is not uncommon for a secretary to directly influence policies. Given the ambiguity in their roles in the government, I do not specifically differentiate between secretaries and mayors to increase statistical power.

Officially, a city official's term is five years, and there is a limit of two terms. By the end of each term, city officials will be appraised by the provincial leaders in Provincial Party Standing Committees and either be retained for a second term or promoted/transferred to other positions. In practice, however, turnovers within the term are not uncommon. According to Chinese Civil Service Law, municipal leaders must not serve in positions of the same rank for more than 15 years. And they also have to retire at age 60 if not being promoted to the provincial level. As a result, many city officials do not always complete the five-year terms. They either step down to create vacancies or are transferred to other positions to fill the vacancy.

Besides appraisal, the appointment and dismissal of city officials are also under the control of Provincial Party Standing Committees. The Standing Committees, made up of 11 to 13 most powerful leaders at the provincial level, are directly appointed by the central leadership and mandated to manage the day-to-day party affairs of a provincial party organization (Brødsgaard and Gang, 2010; Zuo, 2015). Unlike party chiefs and governors, who usually serve in a variety of locales during their careers, many Standing Committees members are native to the province in which they serve.

City leaders' authority

Since the political reform in 1980s and transition from planning system, a significant amount of decision making powers are delegated to lower levels of government (Edin, 2003). Politically, the personnel management is decentralized, which means that municipal leaders are able to evaluate and appoint lower(county) government officials without consulting with central or provincial party committees. Economically, more resources and discretion are given to local leaders. With the central government issuing only guidance targets, local leaders have autonomy choosing means to implement policies. Mayors and municipal secretaries also have *de facto* power in budgetary process government budget, despite the *de jure* authority of Municipal People's Congress over budgetary matters (Li, 2007).

Reciprocity between local officials and firms in China is also well documented in the literature. As Bai et al. (2014) famously put it, there are "local crony capitalists in each locality". Local leaders use political and economic powers to support the businesses connected to them so that they can be personally benefited through implicit arrangements, such as firm-sponsored entertainment, travel and other perks (Fang et al., 2018).

A common favor that municipals leaders can grant to firms is tax breaks. Local governments are known for manipulating the tax code for distributing additional tax deductions and subsidies (see Bai et al., 2014; Lei, 2018). Therefore, despite a standard nationwide corporate income tax rate, firms may receive preferential corporate income tax deductions and face a lower effective tax rate.

3.3 Data

In order to study effects on the performance of native municipal leaders, I merge biographical information and political tenure of municipal leaders with city-level and firm-level statistics. In this section, I discuss my main data sources and detail the construction of key variables.

Political Tenures and Biographical Information

The biographical information and political tenures of provincial and city-level leaders are drawn from the China Political Elite Database (CPED), a database containing detailed information for over 4,000 key municipal, provincial, and national leaders in China since the late 1990s. The database gathers from government websites, official yearbooks, and publicly available curriculum vitae, hence provides reliable demographic and career information for each leader. I match city officials' tenure information with other yearly panel data sets using the start and end year of their position. In years with turnovers, the person who served the longest in that year is chosen.

Table 3.1: City Leader Backgrounds

Variable	Served Home	Never Served Home	Difference
Female	0.05	0.05	-0.01 (-0.59)
Minority	0.34	0.1	0.24 (8.36)***
Bachelor or above	0.62	0.78	-0.15 (-5.07)***
Investigated	0.08	0.1	-0.02 (-0.86)
Tenure as City Leader (years)	7.37	6.85	0.52 (2.26)**
Both position	0.39	0.41	-0.03 (-0.83)
Served multiple cities	0.28	0.23	0.05 (1.65)
Served Prov/National Level	0.01	0.02	-0.01 (-1.12)
N	279	2,252	

Note: T-stats are reported in the parenthesis.

During my sample period, 2531 individuals served in 4424 municipal mayor or party secretaries positions (spells). I define a city leader to be *Native* if, he/she was born in the city and had worked in the city prior to assuming the current position. Table 3.1 presents some summary statistics of those officials. In general, officials who served home city are more likely to be an ethnic minority, which is reasonable as local knowledge and cultural proximity can be particularly valuable for serving in the city where minority group predominantly lives. In terms of career, there is no significant difference between officials served home and those had not.

In constructing the instrumental variable, I need a measure for the strength of the connection between a city and the Provincial Standing Committees. With my biographical information for provincial leaders, I track the career background of each provincial leader to record all the city governments they have worked in. Then for each city in each year, I am able to calculate the number of current provincial leaders that had a working history with the city. Although bigger cities are more likely to have connections in the Provincial Standing Committees. The year-to-year variations in the number of connections are likely to be exogenous to the city, as the appointment of high-level provincial officials is decided by the central government. More details on the instrument variable will be discussed in section 3.4.

Policy Outcomes

For impacts on policy outcomes, I am interested in changes in fiscal revenue, expenditure and direct measure of public goods provisions.

The fiscal data is taken from Municipal Public Finance Yearbook from year 1996 to 2007. The total revenue variable I use in this paper is referring to the revenue collected by the municipal government before receiving/remitting transfer from/to the central government, because transfers from upper level government is beyond the control of municipal leaders. For expenditure variables, I am in particular interested in the composition of municipal budget expenditure rather than the level of spending. I aim to capture the trade-off between spending on public goods provision, such as education and health care, versus investment spending on construction and infrastructure. The data are available only for large-expenditure categories.

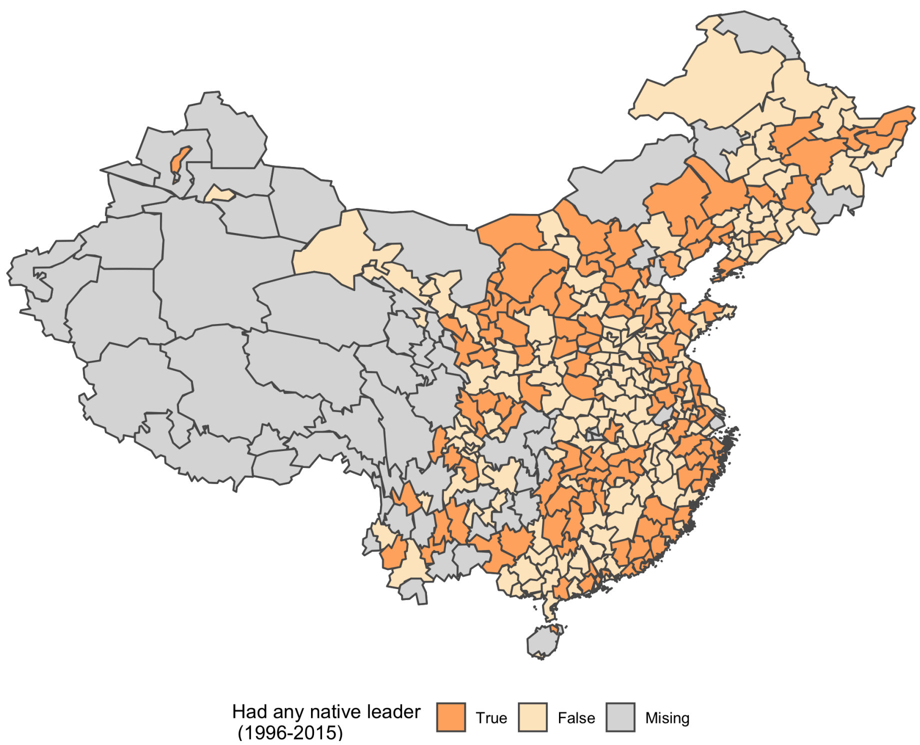
Other statistics such as population, GDP, some public goods measure are taken from China City Statistical Yearbook from 1996 to 2015. I use the number of hospital beds, doctors, teachers in primary and secondary school as real outcomes of the public goods provision.

The main sample includes 280 prefecture-level and sub-provincial units in mainland China. Besides districts under centrally administered municipalities (zhixia shi), the majority of missing cities are in the west region of China (including provinces such as Tibet, Xinjiang, Qinghai) as illustrated in Figure 3.1. Those areas are thinly populated and were not included in both yearbooks for the period of my study. Figure 3.1 also shows that the cities that had native leaders are geographically diverse and do not display any particular locational pattern. Summary statistics of cities in year 2000 are reported in Table 3.2. Except for population, and GDP as a result, cities that had native leaders are not significantly different from cities that never had native leaders.

Firm Outcomes

The firm's financial data are taken from the Annual Survey of Industrial Firms (ASIF). The ASIF dataset, collected and maintained by the National Bureau of Statistics (NBS), includes all the industrial firms that have annual sales above 5 million RMB (roughly 800,000 USD) from 1998 to 2013. Detailed information for

Figure 3.1: City Map



each firm is recorded, including their location, industry code, total outputs, tax, profits, assets, and etc. This dataset has been widely used in economic research, and more details about its construction and cleaning processes can be found in Hsieh and Klenow (2009); Song et al. (2011); Yu (2015); Huang et al. (2017).

As discussed in the previous section, a common favor that firms can get from a local leader is the corporate income tax deduction. The focus of my firm analysis therefore is tax-related variables. The total tax variable reflects not only the corporate income tax paid by the firm but also includes all kinds of government fees and subtract subsidies. Besides the tax paid, the ASIF also reports firms' pre-tax profits. With the pre-tax profits, I am able to calculate the effective tax rate firms are facing.

Besides the investment in health care and education, local leaders can provide another important public good by reducing pollution. I use firm-level emission data as a measure of (failure in) supplying this public goods. The emission data is taken from

Table 3.2: City Statistics in 2000

Variable	Had Native Leader	Had Not	Difference
Area (1,000 km^2)	16.03	13.56	2.47 (1.1)
In Autonomous Regions	0.11	0.09	0.02 (0.53)
Population (million persons)	4.15	3.61	0.54 (2.03)**
GDP (billion Yuan)	35.69	26.28	9.42 (2.47)**
GDP per capita (Yuan)	8,927.37	8,151.97	775.39 (0.67)
Fiscal Revenue (billion Yuan)	1.63	1.41	0.22 (0.89)
Fiscal Expenditure (billion Yuan)	2.35	2.05	0.31 (1.11)
Share of Expenditure (%):			
Heath and Education	1,953.79	1,940.18	13.61 (0.23)
Justice	700.48	699.12	1.37 (0.07)
Administrative Costs	1,271.34	1,302.55	-31.21 (-0.77)
Infrastructure	546.66	549.34	-2.69 (-0.04)
N	122	158	

Note: T-stats are reported in the parenthesis.

China's Environmental Survey and Reporting (ESR) database. The ESR database is maintained by the Ministry of Environmental Protection (MEP) of China and is used to monitor the polluting activities of all major polluting sources, including heavily polluting industrial firms, hospitals, residential pollution discharging units, hazardous waste treatment plants, and urban sewage treatment plants.[1]

3.4 Empirical Strategy

To estimate the causal effect of having native vs. outsider city leaders on governance. I estimate the following panel fixed effects equation for city-level outcomes:

$$Y_{ct} = \alpha Native_{ct} + \beta' X_{ct} + \tau_t + \sigma_c + \varepsilon_{ct} \tag{3.4.1}$$

where Y_{ct} is the outcome of interest for city c in year t. $Native_{ct}$ is my main explanatory variable, a dummy variable for having a Native mayor and/or Native municipal party secretary in the city c in year t. X_{ct} includes a vector of attributes of the city c at time t that directly affect the outcome of interest. City and year fixed effects, τ_t

[1]More details of the database are described in Cai et al. (2016)

and σ_c, control for all time-invariant differences between cities and region-invariant changes over time, respectively. ε_{ct} is the error term. Standard errors are clustered at the city level. The primary parameter of interest is α. If having native city leaders affects the policy outcomes, then $\alpha \neq 0$.

For all of my city-level outcomes, I control for the population of the city in the current year because the changes in per capita unit are more meaningful. I also include the tenure of the leaders in the current position to control for career concerns of the officials. Officials tend to put less and less effort when getting closer to the end of their tenure.

I use a similar specification to examine the impacts on firms:

$$Y_{ijct} = \alpha Native_{ct} + \beta' X_{ct} + \tau_t + \lambda_i + \gamma_{jt} + \varepsilon_{ijct} \tag{3.4.2}$$

The firm-level regression replaces the city fixed effects in the previous specification with more restrictive firm fixed effects,λ_i, and add industry-by-year fixed effects, γ_{jt} to control for industry-specific shocks.

Instrumental Variable

An important challenge in estimating α is the endogeneity of $Native_{ct}$ variable, which arises if there are unobservable city characteristics that both affect the outcomes and are correlated with whether a native or outsider is appointed as city leaders. For instance, a native may be appointed to his/her hometown whenever the upper government wants to tackle certain challenging problems by leveraging the native's local network and knowledge. To address the endogeneity concern, I construct an instrument for $Native_{ct}$: a variable that affects the probability that a native city leader is appointed in a given city at a given point in time, but that does not have a direct effect on the outcome variables.

For this purpose, I exploit two sources of variation. First, I use the exogenous variations in leadership vacancy introduced by established rules such as term limits, age limits and cadre rotation system. As discussed in section 3.2, although official term length for city leader is five years, there are frequent turnovers mid-term due to age limits and other limits. Those rules, therefore, create exogenous variations in the time dimension. I use a dummy variable *Term Expires* to indicate whether the city leadership is expected to change in a given city at a given time.

Second, I combine the variations in leadership vacancy with the changes in a city's connection to its Provincial Party Standing Committees. Provincial Party Standing

Committees, appointed by the central leadership, are in charge of personnel management of provincial party organizations, which includes city officials. Standing Committees members are normally native to the province in which they serve, which means that they most likely have had experience working in some city governments within the province. I argue that their experience will influence their decisions in appointing city leaders. Intuitively, since city leaders are important positions within a province that can undoubtedly influence policies and change power dynamics in the provincial government, each Standing Committees member would prefer to appoint officials whom he/she personally trusts to these positions. Each Standing Committees member is more likely to have allies in cities where had worked, and as a result, cities that have more connections in the Committees should have a higher probability of seeing a native official being promoted. As the composition of the Standing Committees changes, a city's connection to the Standing Committees also varies from year-to-year. Therefore, I use variations in the city's connection to the Standing Committees to capture exogenous changes in the probability of landing a native city leader. Specifically, I create a dummy variable *More Connected to Prov Committee* that equals to 1 if the number of connections in Committees for a city is above the city's median connections over its history, and 0 otherwise.

Although the Standing Committees may always want to have people they prefer at the helm of the municipal governments, replacing a city official before the end of the term without a legitimate reason is unusual and attracts unwanted attention. Therefore, *More Connected to Prov Committee* only increases the chance of having native city leaders when there is a vacancy in the city. As a result, I use the interaction between *More Connected to Prov Committee* and *Term Expires* as the instrumental variables for the $Native_{ct}$ variable.

Table 3.3 presents the first stage results of my instrumental variable. In column (1), there are no covariates except for city and year fixed effects. Column (2) adds the logarithm of the city population and the city leaders' tenure in positions. The result shows that my instrument is a strong and significant predictor for a city to have a native leader in both specifications. When the instrument becomes 1, the chance that the city is having a native leader increases by 32 percentage points. The increase is not only statistically significant but also economically significant because cross-sectionally only about 13 percent of cities are served by a native leader each year. In both columns, F-statistic with clustered standard error is above the conventional level of 10 ruling out the weak-instrument concerns.

Having established the first stage, I present the evidence in support of the exclusion restriction. The main concern is that cities may receive different treatment

Table 3.3: IV First Stage Regression

	(1) Native Leader(s)	(2) Native Leader(s)
More Connected to Prov Committee × Term Expires	0.3227*** (0.0221)	0.3263*** (0.0219)
Log Population		-0.1055 (0.1032)
Tenure in Position		0.0118*** (0.0035)
Mean of Dep Variable	0.134	0.135
Year FE	Yes	Yes
City FE	Yes	Yes
Number of Observations	5095	5071
R squared	0.457	0.461
F-stat	21.4	22.3

Note: Standard errors are clustered at the city level and reported in the parenthesis. * Significant at 10%; ** significant at 5%; *** significant at 1%.

when they have more representations in the Provincial Party Standing Committees. This should not be the case *de jure*, because Provincial Party Standing Committees, though powerful, is only part of the Party Organization and not supposed to intervene in governance decisions. Provincial policies are only decided by governors, or sometimes provincial party secretaries.

Nevertheless, it is possible for Provincial Party Standing Committees members to use their political clout to benefit certain cities, so I proceed to check whether the instrumental variable is correlated with benefits from provincial governments. One important measurable benefit a provincial government can grant to cities is higher intergovernmental fiscal transfers, which have a significant discretionary component. If being more connected to the Standing Committees can directly benefit a city, then the city should receive more fiscal transfers and in particular more discretionary transfers. However, the results in Table 3.4 show that the coefficient for the instrument variable is close to zero and not significant. This provides suggestive evidence that my instrumental variable may not directly affect policy outcomes.

Table 3.4: IV Exclusion

	(1) Log Total Transfer	(2) Log Discretionary Transfer
More Connected to Prov Committee × Term Expires	-0.0085	-0.0049
	(0.0145)	(0.0248)
Log Population	-0.1438	-0.3616
	(0.2087)	(0.3172)
Tenure in Position	0.0049	-0.0019
	(0.0034)	(0.0059)
Mean of Dep Variable	12.259	10.224
Year FE	Yes	Yes
City FE	Yes	Yes
Number of Observations	2429	2756
R squared	0.945	0.934

Note: Standard errors are clustered at the city level and reported in the parenthesis. * Significant at 10%; ** significant at 5%; *** significant at 1%.

3.5 Results

Tax Revenue

First, I examine the impacts of having native city leaders on the municipal government's revenue. The estimation results are presented in Table 3.5. Panel A presents the OLS results and panel B presents the 2SLS results.

For all outcomes, the IV estimates yield the same negative effects as the OLS estimates, although sometimes larger in magnitude than the OLS estimates. This may be explained either by an attenuation bias due to measurement error in the main explanatory variable or the endogeneity bias in the OLS regression. For example, if native city leaders are only appointed to the cities where tax revenue was expected to grow, then OLS estimates could underestimate the true negative effects as results presented here. Overall, the disparity between the OLS estimates and IV estimates are quite small, so I will mainly focus on the IV results for discussions.

Column (1) presents the results for the total revenue. IV results point to a

Table 3.5: Tax Revenue

	(1)	(2)	(3)	(4)	(5)
Panel A: OLS					
	Log Revenue	Log Revenue	Log VAT	Log Biz Operating Tax	Log City Construction Fee
Native Leader(s)	-0.0631**	-0.0513**	-0.0785***	-0.0623**	-0.0586**
	(0.0245)	(0.0235)	(0.0278)	(0.0302)	(0.0226)
Log Population	0.0264	-0.1184***	0.0171	0.0511	0.0454
	(0.0392)	(0.0337)	(0.0464)	(0.0369)	(0.0523)
Tenure in Position	0.0022	0.0006	-0.0040	0.0043	0.0038
	(0.0039)	(0.0038)	(0.0039)	(0.0043)	(0.0034)
Log GDP		0.2338***			
		(0.0538)			
Year FE	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes
R squared	0.962	0.965	0.956	0.959	0.972
Panel B: IV					
	Log Revenue	Log Revenue	Log VAT	Log Biz Operating Tax	Log City Construction Fee
Native Leader(s)	-0.0721**	-0.0502	-0.1482***	-0.0864**	-0.1636***
	(0.0353)	(0.0342)	(0.0435)	(0.0413)	(0.0346)
Log Population	0.5123***	0.3706***	0.1204	0.3721***	0.2323**
	(0.0908)	(0.0877)	(0.1117)	(0.1055)	(0.0911)
Tenure in Position	0.0014	0.0001	-0.0037	0.0027	0.0038
	(0.0027)	(0.0026)	(0.0034)	(0.0032)	(0.0027)
Log GDP		0.2204***			
		(0.0166)			
Mean of Dep Variable	11.816	11.817	9.999	10.193	9.104
Year FE	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes
Number of Observations	2821	2816	2821	2821	2607

Note: Standard errors are clustered at the city level and reported in the parenthesis. * Significant at 10%; ** significant at 5%; *** significant at 1%.

significantly smaller fiscal revenue when a city is governed by native leaders. Having a native city leader is estimated to reduce the city's revenue by 7.2% compared to having outsider leadership. The reduction in revenue could be either a result of reduced from the city's total GDP or a result of more tax breaks and lowered tax rates. To investigate the channel, I additionally control for the logarithm of the city's GDP in column (2). The point estimates decrease slightly in both OLS and IV results. Although the coefficient for *Native Leader* is not statistically significant in the IV regression, examining the OLS and IV estimates as a whole suggests that the change in city's GDP does not explain all the reduction in tax revenue.

In column (3) - (5), I further investigate the change in revenue from three specific taxes, the top three tax sources for local governments. The IV estimates for the impacts of *Native Leader* on all three types of tax revenues are negative. The magnitude is as follows: if a native city leader replaces an outsider, the value-added tax revenue decreases by 14.8%, business operating tax decreases by 8.6%, and city construction fee [2] decreases by 16.3%.

Overall, the results for tax revenue seem to suggest that native city leaders are implementing policies to reduce local tax rates across the board.

Tax Expenditure

Having established native leaders' impacts on fiscal revenue, I study their influences on fiscal expenditure. The results are reported in Table 3.6, again with OLS results in Panel A and IV results in Panel B.

As expected, results from column (1) show that the total expenditure also decreased under the native leadership, and the magnitude is similar, a 6.8% reduction. But there is no significant change in the probability of recording a budget deficit as estimates in column (2) suggest, meaning that the decreased spending is mainly driven by the decrease in the revenue. I also estimate a regression similar to column (1) with additional control of the logarithm of total revenue. Although not reported in the table here, the results also show no significant impact of the native leader on total expenditure after controlling for total revenue.

Next, I address the question of how the composition of the municipal budget is affected by having a native leader in columns (3) to (6). The results show that there are significant changes in the expenditure share of major categories. Notably, spending on health care and education increases by 1.3 percentage points, a 6% increase from being 21.8 percent of total expenditure on average; whereas, the spending in construction decreases by 9.8 percentage points, a 15% decrease from the average level. The trade-off between investment in social services and infrastructure is interesting. During the period of my study, local governments in China were competing in spending on infrastructure in order to score political points, while paying little

[2] A local tax collected by the city from both businesses and residents.

Table 3.6: Tax Expenditure

Panel A: OLS						
			Expenditure Share (%)			
	(1)	(2)	(3)	(4)	(5)	(6)
	Log Expenditure	Deficit	Health and Education	Justice	Admin	Construction
Native Leader(s)	-0.0367**	0.0122	0.6126**	0.0810	0.2436*	-0.5903**
	(0.0169)	(0.0211)	(0.2544)	(0.0984)	(0.1430)	(0.2744)
Log Population	0.0297	0.0231	0.3490	-0.2161**	-0.6221***	0.4677
	(0.0192)	(0.0544)	(0.3455)	(0.0953)	(0.1983)	(0.4550)
Tenure in Position	0.0034	0.0045	-0.0785**	-0.0145	-0.0268	0.0673
	(0.0022)	(0.0042)	(0.0375)	(0.0169)	(0.0263)	(0.0604)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
R squared	0.981	0.667	0.871	0.752	0.849	0.709
Panel B: IV						
			Expenditure Share (%)			
	Log Expenditure	Deficit	Health and Education	Justice	Admin	Construction
Native Leader(s)	-0.0680***	0.0093	1.3620***	0.0279	0.5692**	-0.9861*
	(0.0217)	(0.0482)	(0.3688)	(0.1522)	(0.2370)	(0.5880)
Log Population	0.1327**	0.0047	4.5531***	-1.0504***	0.3888	-4.0210***
	(0.0559)	(0.1231)	(0.9419)	(0.3912)	(0.5989)	(1.5292)
Tenure in Position	0.0030*	0.0064*	-0.0589**	-0.0183	-0.0274	0.0515
	(0.0017)	(0.0037)	(0.0283)	(0.0117)	(0.0189)	(0.0499)
Mean of Dep Variable	12.508	0.291	21.848	6.874	12.647	6.358
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	2821	2781	2821	2821	2540	2282

Note: Standard errors are clustered at the city level and reported in the parenthesis. * Significant at 10%; ** significant at 5%; *** significant at 1%.

attention to public goods provisions because they were not rewarded by the central government Persson and Zhuravskaya (2016). The fact that native leaders' behaviors in allocating spending deviate from the political promoting scheme sends a nice signal that their affinity to the city plays a role in their decisions.

Public Goods Provision

Although a higher percentage of government spending may go to health care and education under a native city leadership, the actual outcomes of the provision of the public goods do not necessarily improve because of the large reduction in total spending. I examine the impacts of native city leaders on measures of public goods provision and report the results in Table 3.7.

In columns (1) and (2) I use the number of hospital beds and doctors to proxy for the actual outcomes of health care. The estimated coefficients for native leaders are both negative and statistically significant. On average, having a native leader in the city decreases the number of hospital beds by 3.4% percent and the number of doctors by 6.6%. Columns (3) and (4) study the impacts on education outcomes using the number of teachers and the number of primary and secondary school enrollment. Native city leaders' impacts on the number of teachers are also negative, resulting in a 5% decrease. Their effects on school enrollment are inconclusive, but with a negative point estimate.

Table 3.7: IV Results for Public Goods

	(1) Log Hospital Beds	(2) Log Doctors	(3) Log Teachers	(4) Log School Enrollment
Native Leader(s)	-0.0348**	-0.0669***	-0.0501*	-0.0286
	(0.0153)	(0.0253)	(0.0259)	(0.0264)
Log Population	0.7309***	0.9063***	1.3132***	1.5003***
	(0.0342)	(0.0563)	(0.0586)	(0.0598)
Tenure in Position	-0.0009	-0.0048**	0.0014	-0.0003
	(0.0012)	(0.0020)	(0.0020)	(0.0021)
Mean of Dep Variable	9.197	8.583	10.187	13.039
Year FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Number of Observations	4966	4967	5001	5014

Note: Standard errors are clustered at the city level and reported in the parenthesis. * Significant at 10%; ** significant at 5%; *** significant at 1%.

The results in Table 3.7 suggest that despite the increased proportion of the budget spent on health care and education, the actual outcomes of health care and education seem to deteriorate unconditionally under native leaders. From the local population's perspective, although native city leaders prioritize social spending over construction spending in the municipal budget, this change does not directly translate into actual improvements in public goods provision.

Firm Outcomes

To further understand the reduction in tax revenue, I examine the impacts of native leaders on firm-level outcomes.

Table 3.8 reports the estimates for equation 3.4.2. The results show that private manufacturing firms produce 10% more total output and earn 7% more pre-tax profits under the native city leadership. However, growth in outputs and profits is not accompanied by an increase in tax payment. On the contrary, the total tax paid by the firms decreases by 4.7%, on a similar magnitude as the estimated city-wide revenue loss. Controlling for the pre-tax profits, the decrease in total tax paid by firms is even greater, at 5.4% percent, meaning a considerate drop in effective tax rates.

Table 3.8: Firm Outcomes

	(1) Log Ouput Value	(2) Log Pre-tax Profit	(3) Log Tax	(4) Log Tax
Native Leader(s)	0.1032***	0.0721***	-0.0471***	-0.0544***
	(0.0056)	(0.0086)	(0.0059)	(0.0040)
Log Pre-tax Profit				0.6437***
				(0.0005)
Mean of Dep Variable	5.446	2.829	2.114	2.160
Industry Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Number of Observations	1615482	1597665	1688988	1595689

Note: Standard errors are clustered at the city level and reported in the parenthesis. * Significant at 10%; ** significant at 5%; *** significant at 1%.

There are two ways to interpret the results of the reduction in tax rates. On the one hand, the native leaders based on their knowledge and understanding of the local community may be using tax breaks to alleviate the burden of local business and therefore boost the local economy. On the other hand, through the lens of "crony capitalism", tax breaks may be used by native officials as a vehicle to support connected firms and further develop new relationships with profitable local firms. As discussed in section 3.2, reciprocity between firms and local officials is not uncommon in China.

Although I do not have the necessary data to directly test either hypothesis, progress can be made by further investigating the recipients of the tax breaks. If a native city leader were to grant firms tax breaks in the hope of private returns, he is more likely to trust or develop crony firms in his home counties where he has more entrenched networks. To test this hypothesis, I interact the main explanatory variable *Native Leader* with a *Home County* dummy, which indicates whether a firm is in the home counties of the native city leaders [3]. Because not all the city officials report their home county, I code the *Home County* variable to be zero for all the firms in the city whose leader's home county is missing. If anything, this should attenuate the estimated coefficient of the interaction term.

The estimation results are presented in Table 3.9. Those estimates show that the firms in the city leaders' home counties are indeed receiving different treatments. It seems that the effects observed in Table 3.8 is mostly driven by the effects on home county firms. The total output value only increases for firms in leaders' home counties while the decrease for other firms. There is no significant difference in profits between home county firms and others. But the home county firms pay significantly less tax and conditioned on the profits, they pay about 2% less total tax under the native city leaders in contrast to a 2.6% increase for other firms.

Next, I examine the impacts of native city leaders on firms' emissions. While the results above suggest that native city leaders are selectively giving tax breaks to firms in their home counties, it still can rule out the possibility that it's for the benefit of people in home counties, albeit being outright local favoritism. As China was struggling with smog across countries, improved air quality could be a valued public goods the city leaders want to provide to their home town population.

[3] By definition, the *Home County* variable is always zero when the city leaders are not native.

Table 3.9: Firm Outcomes: Home County Effects

	(1) Log Ouput Value	(2) Log Pre-tax Profit	(3) Log Tax	(4) Log Tax
Native Leader(s)	-0.0423***	0.0745***	0.0593***	0.0262***
	(0.0078)	(0.0098)	(0.0084)	(0.0057)
Native Leader(s)× Home County	0.1561***	-0.0072	-0.0406***	-0.0449***
	(0.0094)	(0.0118)	(0.0102)	(0.0069)
Log Pre-tax Profit				0.6436***
				(0.0005)
Mean of Dep Variable	5.446	2.829	2.114	2.160
Industry Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Number of Observations	1615482	1597665	1688988	1595689

Note: Standard errors are clustered at the city level and reported in the parenthesis. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3.10: Firm Emissions

	(1) Log Total Waste Air	(2) Log Total Waste Air	(3) Log SO2	(4) Log SO2	(5) Log NO2	(6) Log NO2
Native Leader(s)	0.1217***	0.0683*	0.0628*	0.0308	0.0959	0.1266
	(0.0309)	(0.0353)	(0.0355)	(0.0406)	(0.0700)	(0.0903)
Native Leader(s) × Home County		0.1465***		0.0873*		-0.0691
		(0.0429)		(0.0490)		(0.1045)
Mean of Dep Variable	7.624	7.624	9.754	9.754	8.960	8.960
Industry Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	102398	102398	97970	97970	40020	40020

Note: Standard errors are clustered at the city level and reported in the parenthesis. * Significant at 10%; ** significant at 5%; *** significant at 1%.

However, results in Table 3.10 show that the opposite is true. The firms' emissions increase dramatically under the native leaders. Under the native city leaders, the total waste air emissions rise more than 12% and the SO_2 emissions increase by 6.2%. The estimate for the impacts on NO_2 is also positive but not significant. This may be partly due to the sample size, as statistics for NO_2 emissions were not collected until 2006. When I include the interaction term between *Native Leader* with a *Home County* dummy, results show that those increased emissions mainly come from firms in the city leaders' home county. The increase in total waste air is more moderate and marginally significant for firms not in home counties, while total waste air emission from home county firms is estimated to increase by more than 20%. A similar pattern is observed for SO_2 emissions.

3.6 Robustness

In this section, I present two robustness checks to assess the validity of my baseline estimates.

Since cities in my sample are geographically diverse as seen in Figure 3.1, it could be a concern if appointments of native city leaders are correlated with regional shocks, and the estimated causal impacts of native leaders are merely reflections of real impacts from the regional shocks.

There are indeed divergent development trends between different regions in China. In response to that, three successively large-scale regional development initiatives were launched by the national government in order to address the lagged development in specific parts of the county. The Great Western Development Strategy started in 1999; the Rise of Central China Plan began in 2004; then Northeast Area Revitalization Plan was set in motion in 2006.

To account for the disparity in growth trend and regional shocks, I add region-year fixed effects to control for the six economic regions: East Coast, South Coast, North Coast, Central Core, Hinterland, and Far West. The six economic regions are defined following Persson and Zhuravskaya (2016) and depicted in Figure B.1 in the Appendix. The regression results are presented in Table 3.11 and 3.12 and show that my baseline estimates are robust and barely change after the added region-year control.

Table 3.11: Robustness: Control for Region-year Fixed Effects, Panel A and B

Panel A: Fiscal Revenue

	(1) Log Revenue	(2) Log VAT	(3) Log Biz Operating Tax	(4) Log City Construction Fee
Native Leader(s)	-0.0464	-0.1342***	-0.0476	-0.1410***
	(0.0330)	(0.0418)	(0.0378)	(0.0341)
Log Population	0.6157***	0.1174	0.4910***	0.2813***
	(0.0866)	(0.1110)	(0.0992)	(0.0922)
Tenure in Position	0.0028	-0.0047	0.0027	0.0027
	(0.0025)	(0.0033)	(0.0029)	(0.0027)
Mean of Dep Variable	11.816	9.999	10.193	9.104
Year FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Region-Year FE	Yes	Yes	Yes	Yes
Number of Observations	2821	2821	2821	2607

Panel B: Fiscal Expenditure

			Expenditure Share			
	(1) Log Expenditure	(2) Deficit	(3) Health and Education	(4) Justice	(5) Admin	(6) Construction
Native Leader(s)	-0.0577***	0.0000	1.2342***	0.1127	0.5861**	-1.0096*
	(0.0206)	(0.0486)	(0.3621)	(0.1420)	(0.2303)	(0.5693)
Log Population	0.2394***	-0.0862	3.4493***	-1.5073***	0.9169	-4.0971***
	(0.0547)	(0.1283)	(0.9506)	(0.3766)	(0.5982)	(1.5165)
Tenure in Position	0.0025	0.0062	-0.0521*	-0.0083	-0.0089	0.0324
	(0.0016)	(0.0038)	(0.0279)	(0.0110)	(0.0185)	(0.0483)
Mean of Dep Variable	12.508	0.291	21.848	6.874	12.647	6.300
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	2821	2781	2821	2821	2540	2303

Note: Standard errors are clustered at the city level and reported in the parenthesis. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3.12: Robustness: Control for Region-year Fixed Effects, Panel C

Panel C: Actual Outcomes	(1) Log Hospital Beds	(2) Log Doctors	(3) Log Teachers	(4) Log School Enrollment
Native Leader(s)	-0.0260*	-0.0499**	-0.0452*	-0.0247
	(0.0153)	(0.0249)	(0.0261)	(0.0265)
Log Population	0.7324***	0.8767***	1.2842***	1.4554***
	(0.0355)	(0.0576)	(0.0612)	(0.0622)
Tenure in Position	-0.0003	-0.0028	0.0015	0.0002
	(0.0012)	(0.0019)	(0.0020)	(0.0021)
Mean of Dep Variable	9.197	8.583	10.187	13.039
Year FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Region-Year FE	Yes	Yes	Yes	Yes
Number of Observations	4966	4967	5001	5014

Note: Standard errors are clustered at the city level and reported in the parenthesis. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Another robustness check I perform is to exclude the cities that never had any native leaders during the period of my study. In my baseline estimation, I included all the cities in the sample even if about half of cities never receive any native leaders. The inclusion of those never treated cities increases statistical power and help better estimate year fixed effects, but should not introduce selection bias because of the existence of the city-fixed effects. Nevertheless, to check the robustness of my estimation, I exclude those never treated cities and re-estimate the baseline regressions. The results are similar to the baseline estimates and reported in Appendix Table B.1.

3.7 Conclusion

It is an open question about whether appointing bureaucrats to serve their home city would enhance or depress their performance. At the heart of the question is whether intrinsic pro-social incentives plus informal institutions are able to overcome the drive for self-enrichment.

This paper tries to answer this question empirically in the context of Chinese bureaucracy. I make progress by combining detailed biographical information of municipal leaders with various economic statistics to estimate the impacts of appointing native bureaucrats to lead their cities. To address the endogeneity in appointment decisions of native bureaucrats, I construct an instrument exploiting the turnovers in the decision-making body, Provincial Party Standing Committees and the exogenous variations in city leadership vacancy. The richness of my data allows me to study the impacts on multiple aspects.

My results show that the municipal leaders' biographical background indeed plays an important role in their governance decisions. First, my estimate reveals that home-appointed leaders seem to implement policies that lead to a significant reduction in tax revenues. Fiscal incomes from all major sources of tax collected by the city government decrease substantially. Results from firm-level data further corroborate this finding. Firms produce more output and earn more profits with native officials as city leaders, but pay significantly less amount of tax. Although it's possible that the native leaders reduced tax rates out of kindness aimed to boost the local economy, further examination suggests only firms in home counties of native leaders benefit from the tax breaks.

As a result of the lowered budget, public goods provision deteriorate under the native city leaders in terms of almost all measures of public goods this paper examines. The number of hospital beds and doctors decline as well as the number of public school teachers. Meanwhile, the pollutions and waste air emissions from firms increase. Moreover, pollutions are even worse at the home counties of the native officials compared to other parts of the city, which suggests that the favoritism exhibited in tax breaks distribution may be only concerned with profits of the firms in home counties but does not extend to the amenity in local communities.

The silver lining, however, is that native officials allocate a higher share of municipal budget to education and health care compared to outsiders' budget, and a lower share to infrastructure. During the period of my study, local governments in China were overzealous with big infrastructure projects, which not only help score political points but also are prone to corruptions misappropriations, but unsympathetic to public goods provision. This shift in budgetary policies under native city leaders moves in the right direction towards the local populace's needs.

Taken together, my results suggest that social proximity hampers bureaucrat performance and facilitates local favoritism from the perspective of the general public. At the expense of deteriorating public goods, extensive tax breaks are given to firms in the home counties of native leaders, which may be reciprocated back as legal or illegal benefits (see discussions in Lei, 2018; Bai et al., 2014; Fang et al., 2018). The findings in this paper resonate with the literature on favoritism and patronage (Bandiera et al., 2018; Xu, 2018; Fisman et al., 2017) and may be relevant to other week institutionalized environments in which bureaucrats have large discretionary power.

Chapter 4

Open Science Practices are on the Rise

4.1 Introduction

Across many scientific disciplines there has been a movement to promote open science practices: posting data, code, and study materials online, and pre-registering studies, hypotheses, and analyses prior to a research study (Miguel et al., 2014; Nosek et al., 2015). In the social sciences for the past two decades, disciplinary organizations and journals have increasingly endorsed open science practices. More recently, cross-disciplinary social science organizations have been founded to accelerate awareness of open science and to provide training and supportive open science technologies, such as pre-registration platforms and open archives (Christensen et al., 2019). During this period, the social sciences have also grappled with debates and scandals surrounding the unavailability of original data, examples of publication bias, replication challenges, and in some cases data fraud (Bhattacharjee, 2013; Borsboom and Wagenmakers, 2012; Broockman et al., 2015; Carey, 2011; Enders and Hoover, 2004; Feilden, 2017; Neuroskeptic, 2012).

Beyond reducing the incidence of fraud (Simonsohn, 2013), open science practices have been linked to the improved quality and credibility of research findings across fields. For example, study registration could increase the visibility of results, improving meta-analysis and reducing the selective reporting of null, unexpected or otherwise unfavorable results (Kaplan and Irvin, 2015; de Vries et al., 2018), and data sharing could increase later data re-use and article citations (Piwowar and Vision, 2013)

Yet controversy and opposition have followed many research transparency proposals in the social sciences, particularly the use of pre-registration (Open Science Collaboration, 2015; Gilbert et al., 2016; Coffman and Niederle, 2015). For instance, some worry that pre-registration might hamper creative research (Goldin-Meadow, 2016; Kupferschmidt, 2018). Others suggest that it maybe be used instrumentally or selectively, therefore doing little to remedy the underlying problems it was proposed to address (Claesen et al., 2019). Altogether, some debates over the merits of open science may be natural extensions of the disagreement and scandals that prompted open science proposals in the first place, while others may arise from uncertainty over the effectiveness of proposed solutions, or simply because open science practices represent a break from the status quo.

Addressing these controversies, and in particular the debates about the effect of open science practices on the social scientific literature, is beyond the scope of the present paper. Rather, we pose a question that logically precedes answers to those questions, specifically: how many social scientists are adopting open science practices, and what are the average perceptions of these practices in the social sciences? While some researchers are publicly starting to adopt open science practices (Christensen and Miguel, 2018), there may be a lag between private adoption and public representation. For example, there are lags between pre-registration of a study or preparation of shareable code and article publication. Additionally, there are a small number of highly vocal scholars (including some authors of this article) who have expressed strong opinions either in support of or against the adoption of open science practices. However, these prominent voices may not be representative of the opinions of most scholars. Thus, there remains a considerable degree of uncertainty about researchers' current adoption of and attitudes toward open science practices (Anderson et al., 2007).

Previous attempts to quantify adoption of open science practices tend to have small and largely unrepresentative convenience samples of survey respondents, and focus on just a single research discipline (e.g. van Assen et al., 2015; Baker, 2016; Buttliere, 2014; Feilden, 2017). Researchers largely send solicitations to complete non-remunerated surveys to academic listserves, or to their personal networks via email or social media. In these surveys, scholars often claim to be more supportive of open science practices than their peers.

The present research, based on the State of Social Science (3S) Survey, generates a more robust estimate of the adoption of open science practices over time, and of general support and perceived norms of research transparency across four major social science disciplines: economics, political science, psychology and sociology. In addition, we connect the patterns in the data to theories regarding how institutions and technological innovations may affect the pace of scientific change (Romer, 1990; Griliches, 1957) and the development of new norms (Kuhn, 1962; Hacking, 1981).

4.2 Sample and Data

We solicited information using a monetarily incentivized survey from a representative sample of active, elite social science researchers in the fields of economics, political science, psychology, and sociology who work with empirical quantitative or qualitative data. The 3S survey queried respondents on awareness of, attitudes towards, perceived norms regarding, and adoption of open science practices. We randomly drew the sample from the complete set of authors who had published within a range of 3 years (2014-2016) in 10 of the most cited journals for each discipline. We also drew from the complete set of PhD Students enrolled in the top 20 North American departments in each discipline during the first half of 2018; see supplementary materials for details. We pre-registered analyses for our survey and posted our pre-analysis plan and study materials on the Open Science Framework. The present survey and descriptive analysis are the first part of a broader project described in the pre-analysis plan.

In total, we invited 6,221 individuals to complete a survey between April and August 2018 of whom 6,058 were contacted (emails did not bounce). Published Authors were compensated either \$75 or \$100 (randomly), and graduate students either \$25 or \$40 (response rates did not significantly vary by level of compensation). Arguably, our response rate represents an upper bound on the rate that is possible to achieve with a reasonable incentive strategy: at a median length of 15 minutes per survey, faculty were compensated at minimum \$300 per hour.

Our incentive scheme achieved a completed survey response rate of 46.2%, implying that the study sample is broadly representative of active Published Authors and PhD Students in these four fields. Figure 4.1 presents the overall response rate of 46.2%, which ranged from 40% in Psychology to 55% in Political Science. We consistently obtained a majority of PhD Students, who responded at or above 50% in every field, while Published Authors (who had predominantly completed their doctoral training) responded at somewhat lower rates. Among respondents with North American email addresses, the response rates are slightly higher at 49% overall, 44% for Published Authors, and 53% for PhD Students.

As shown in Figure 4.1, the response rate for Published Authors from psychology journals is somewhat lower than that for the other disciplines' journals. This may be due to the fact that a subset of psychologists often publish with scholars or clinicians from other fields who are less active empirical researchers, and therefore may be less likely to respond to an invitation to complete a survey focused on research methods. Consistent with this explanation, the response rate from authors who published in clinical and neuroscience-focused journals is considerably lower than the rate for social and developmental psychology journals (see Appendix Figure C.4 for survey response rates by journal). Similarly, the response rate for authors who had published in macroeconomics journals is somewhat lower than the rate from other economics journals, possibly due to the greater share of articles based on theoretical or simulation approaches, rather than quantitative empirical data analysis, in those journals.

Two concerns about the validity of our study design might remain. First, our survey results are entirely self-reported and one might be concerned that individuals could misstate their open science behavior, for example, due to surveyor demand effects. Second, even though to our knowledge the current sample is by far the largest and most representative attempt to assess open science attitudes and practices to date, one might still be concerned about the nature of selection into the sample. It remains possible that scholars who responded to the survey are non-randomly selected from the population along important dimensions. Indeed, we find that the response rate among Published Authors was significantly higher for those with more publications in leading disciplinary journals during the last three years, and for those at institutions in North America (see Appendix Table C.9).

To better understand the degree to which non-random survey response may be a concern, we conducted an audit of open science behavior for a random sample of Published Author respondents and non-respondents from economics; economics was chosen because the vast majority of scholars use the same study registry and data posting platform, increasing the accuracy of the audit. We checked publicly available repositories and each author's website to determine whether they had previously pre-registered a study or posted data online; the details of the audit activity can be found in the SOM.

The audit activity yielded three main results. First, there is a high rate of agreement between self reports and actual behavior as presented in Table 4.1: despite only checking a limited number of online sources we were able to validate almost 80% of individuals' responses regarding adoption of open science practices. Second, while there is some selection into the sample, this appears to be primarily driven by scholars with a more empirical orientation being more likely to respond: response rates for theory-focused economists and macroeconomists are far lower than for other fields, at

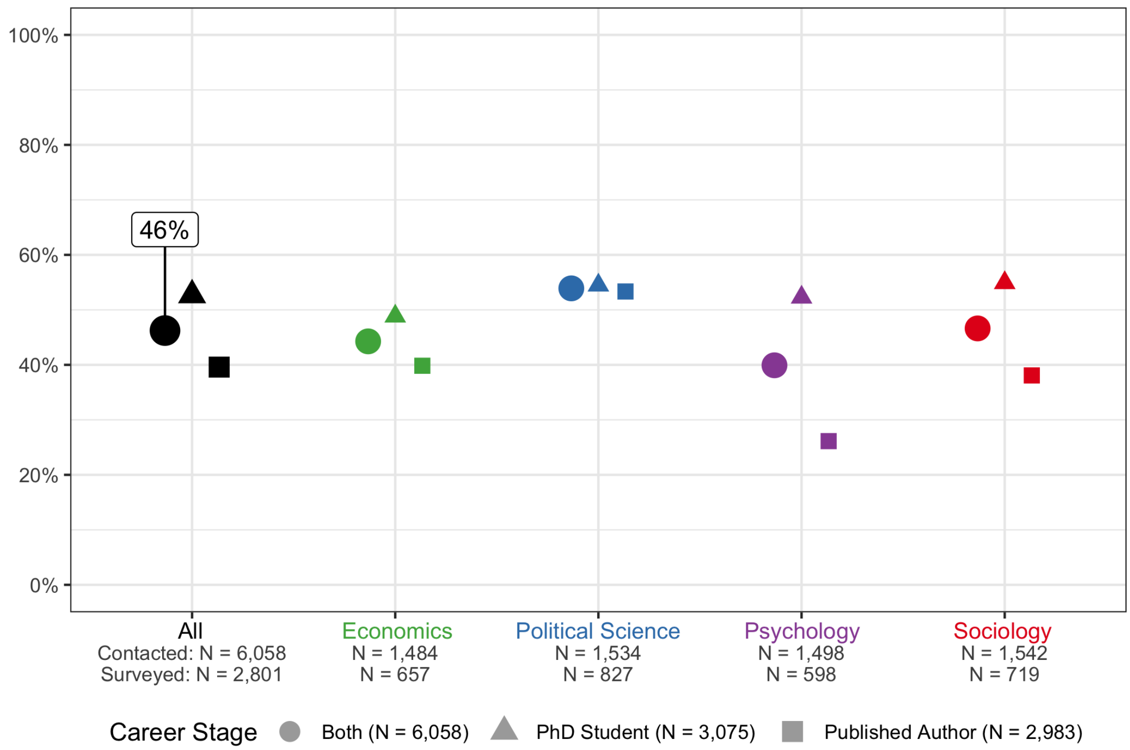


Figure 4.1: Response Rates are High Across Disciplines

Response rates by discipline and by career stage (PhD Student or Published Author). We contacted 6,058 researchers (6,221 researchers were invited via email but 163 emails bounced). Above figure consists of 2,787 respondents and 3,434 non-respondents, including 65 explicit opt-outs and 244 partially complete surveys, but excluding the 163 bounced emails.

27.2% for theory/macroeconomics/finance focused Published Authors versus 50.4% for the others, as shown in Table 4.2. Third, scholars with a more empirical orientation do not appear to be selecting into our survey in a manner related to previous open science behavior (see Table 4.2). Taken together, these patterns suggest that the survey results are broadly representative of the behaviors and views of Published Authors with a more empirical orientation.

Table 4.1: Differences in Behaviors for Published Authors Respondents and Non-respondents on Economics Subfield Validation Data

Parameter	Any (1)	Posting data or code online (2)	Posting study instruments (3)	Pre-registering hypotheses or analyses (4)
Respondent				
Share of Respondents Doing Practice (self-report) (S_R)	84.0%	73.0%	44.3%	20.3%
Share of Respondents Doing Practice (validation) (V_R)	65.3%	63.3%	34.4%	19.0%
Difference between S_R and V_R (D_R)	18.7%	9.7%	9.8%	1.3%
Non-respondent				
Predicted share of non-respondents doing practice ($\widehat{S_N}$)	70.7%	63.4%	37.3%	7.5%
Share of non-respondents verified doing practice (V_N)	55.0%	55.0%	29.0%	7.0%
Difference between $\widehat{S_N}$ and V_N ($\widehat{D_N}$)	15.7%	8.4%	8.3%	0.5%
$\frac{V_N}{V_R}$	84.2%	86.8%	84.2%	36.8%
Share of Practices Verified ($\frac{V_R}{S_R}$)	77.7%	86.7%	77.7%	93.6%

This table presents stated and observed open science behavior for Published Authors in Economics who are respondents and non-respondents in our sample. Observed behavior comes from our audit of all the economists who completed the survey and a random sample of 100 economists who did not complete survey. This audit was completed between March 15, 2019 and April 15, 2019. For pre-registration and posting data and code online, S_R is the percentage of respondents who report engaging in the specified open science practice in our survey. V_R is the percentage of Published Author respondents who we find in our audit to engage in the open science practice. D_R reports the difference between the two. V_N is the percentage of non-respondents in our audited sample that we verify have done an open science practice. $\widehat{S_N}$ is an imputed value for the stated percentage of non-respondents that would have reported doing an open-science practice had they been surveyed. To estimate this, we multiply the audit value V_N by the ratio between stated and observed of respondents (i.e the ratio $\frac{S_R}{V_R}$). $\widehat{D_N}$ is the difference between $\widehat{S_N}$ and V_N. Since we did not conduct an audit for "Posting study instruments online", the "Any" category refers either "Posting data or code online" or "Pre-registering hypotheses or analyses". And "Posting study instruments online" therefore V_R is imputed using the ratio of S_R to V_R in the "Any" category. The remainder of the methodology for this open science practice is the same as listed above.

Table 4.2: Differences in Observables for Published Authors Respondents and Non-respondents in Economics Subfield Validation Data

	Overall (1)	Respondent (2)	Nonrespondent (3)	Difference (2) - (3)
Share of sample:				
— Theory Focused	0.19	0.15	0.22	-0.07 (-1.58)
— Macro/Finance Focused	0.26	0.16	0.33	-0.17 (-3.28)***
— not Theory/Macro/Finance Focused	0.55	0.69	0.45	0.24 (4.29)***
Verified Open Science Behavior				
— all Economics Published Author	0.59	0.65	0.55	0.10 (1.81)*
— among Theory Focused	0.35	0.39	0.32	0.07 (0.54)
— among Macro/Finance Focused	0.56	0.58	0.55	0.04 (0.33)
— not Theory/Macro/Finance Focused	0.69	0.73	0.67	0.06 (0.76)
N	753	300	100	

This table shows the percentage of economics Published Authors who work in different subfields among those who responded and did not respond to the survey. The first panel reports response rates and share of each sample for each subfield. Column 1 shows the response rate for each subfield. Columns 2 and 3 show the share of respondents and non-respondents identifying with each subfield respectively. Panel B shows the fraction of individuals in each subfield for whom we verified open science behavior during our audit activity. For respondents, the subfield is determined by the subfield that the respondent listed in our survey. For non-respondents, we constructed the individual's subfield in an audit activity that was completed between March 15 2019 and April 15 2019. In this activity, we used publicly available data sources to collect data on the primary subfield of these non-respondents. We manually collected all of the subfields that an individual listed working in on their website or CV. After these subfields were collected we manually categorised these subfields into one of three categories. The first of these was "Theory focused", which is categorised as any individual who listed Microeconomic Theory or Econometrics as a primary subfield. The second was "Macroeconomics/Finance", which was any author who listed Macroeconomics or Finance as a primary field. Finally, all other authors were categorised in the residual category. The final column in the table provides t-statistics for tests for differences in the mean between those respondents and non-respondents. * indicates significance at the 10% level, ** indicates significance at the 5% level and *** indicates significance at the 1% level.

4.3 Retrospective Open Science Behavior

We first assess how the adoption of open science practices has changed over time, using survey respondents' self-reports and bounding them with a verification exercise (described below). We find that the last decade has been a time of rapid change across disciplines, with adoption of open science practices increasing dramatically.

Figure 4.2 presents the cumulative proportion of Published Authors who have adopted open science practices over time. We focus on scholars who received their

PhD by 2009, as they had the opportunity to engage in these practices over much of the last decade (see Appendix Figure C.5 for robustness to different PhD cutoff dates). 84% of Published Authors reported adopting an open science practice by 2017 (the last complete year for which we collected data), nearly doubling from 49% in 2010. The sharing of data, code and survey instruments show rapid increases starting after 2005, while the use of pre-registration has increased dramatically since 2013. Posting data or code online is the most common practice, followed by posting study instruments online, and then pre-registration. We also find in our survey data that those who reported adopting an open science practice at some point in the past are overwhelmingly likely to also have employed it in their most recent research project (see Appendix Table C.11), indicating that scholars' adoption of these practices tends to be persistent.

The shaded areas underneath these lines adjust the adoption graph to incorporate the adoption rates of non-respondents, using the verified open science behavior for non-respondents found in our audit activity. Details on how these estimates are constructed are presented in Table 4.1. Even incorporating the likely behavior of non-respondents, we estimate that 76% of Published Authors have adopted an open science practice by 2017.

While there is an upward trend in all four disciplines, Figure 4.3 shows that adoption patterns differ across disciplines. The evolution of adoption in economics and political science appear relatively similar, with a rapid increase in the rates of posting data or code online. In economics, there has been a steady rise in posting study instruments online and pre-registration since around 2011. Political science has seen an increase in posting study instruments since 2005, and a steeper rise in pre-registration since 2014.

Psychology researchers were lagging behind economics and political science scholars until recently for all practices, but over the last few years psychology has had the most rapid increase in adoption. Psychologists also currently report the highest adoption rate for study pre-registration. Sociology has the lowest levels of adoption for all open science practices, but as with the other fields, there has been a steady increase in recent years.

Adoption rates of all three highlighted open science practices have been highest for researchers using experimental methods across social science disciplines, while adoption rates for posting study materials and pre-registration have been lower among researchers using non-experimental quantitative methods. Rates for all practices are the lowest among researchers using exclusively qualitative methods (Moravcsik, 2012), which likely helps to explain the lower adoption rates in sociology, where such methods are more common (see Figure 4.4).

As Figure 4.3 shows, the timing of increases in the reported adoption of transparent practices across disciplines coincides with notable developments in technology and institutional policy within and across disciplines. With respect to technology, online study registries and pre-registration plan registries seem to be accompanied by upward shifts in adoption. For example, the American Economic Association (AEA) registry was launched in April 2013, and in 2013, the Center for Open Science (COS) online archives allowed for pre-registration posting in economics, psychology and other social science fields. Institutionally, psychology journals began requiring data sharing and code or data posting quite recently, which could explain some of the more rapid trends in that field, whereas the AEA required data posting in 2005, which could partly explain why economics is the social science discipline with the earliest rise in adoption of data and code posting. The interdisciplinary organizations COS and Berkeley Initiative for Transparency in the Social Sciences (BITSS) (Miguel et al., 2014) were founded in 2012, and have been homes for researchers working in all four social science disciplines. These developments in technology and institutions, along with the others labeled in Figure 4.3 as well as many others not mentioned in the figure, accord with theories of normal science and how occasional revolutions in scientific theory and practice take hold (Kuhn, 1962; Hacking, 1981).

Of course, there is also a role for bottom-up adoption rates in which students, faculty, and other researchers take up open science practices through processes of communication with peer networks. In 2012, some of the earliest economics articles using pre-analysis plans were published (Finkelstein et al., 2012; Casey et al., 2012), setting an example that many colleagues followed. It was in 2015, additionally, when a critical mass of blogs and Facebook groups addressed open science practices in psychology, and discussions about open science on Twitter gained momentum around 2016 (Singal, 2016; Huston, 2019). These bottom-up processes of change in attitudes and practices among scholars also likely played a role in driving the technological and institutional changes across disciplines noted above and in Figure 4.3.

While we are confident in our verification of a subset of respondents' reported adoption, and the resultant bounds we can place around our estimates of disciplinary and overall adoption trends, we acknowledge that reports were based on memory and thus may be imperfect. However, the fact that the slope of the adoption rates correspond to technological and institutional events provides some amount of confidence that they correspond to actual dates of adoption. Moreover, memories of first experiences (e.g., the first time posting data) are often better recalled than later instances

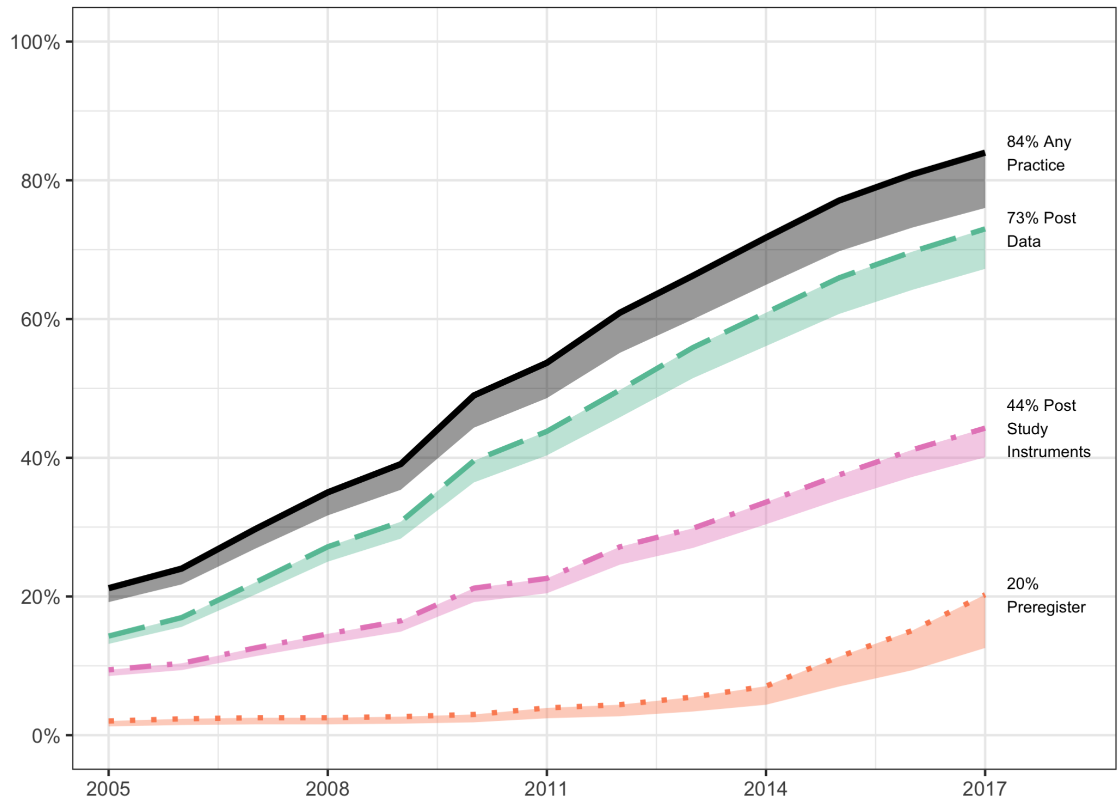


Figure 4.2: Year of Adoption of Open Science Practices

The chart shows for a given year the proportion of Published Authors who had first completed an open science practice in that year or previously. The solid black line shows the proportion of authors who had completed any open science practice by that year. The dashed green line shows the proportion of Published Authors who had posted data or code online by that year. The dash-dotted purple line shows the proportion of Published Authors who had posted study instruments online by that year. The dotted orange line shows the proportion of authors who had pre-registered an analysis or hypothesis by that year. Posting study instruments online is the response to the question "Approximately when was the first time you publicly posted study instruments online?". Posting data or code online is the response to the question "Approximately when was the first time you publicly posted data or code online?". Pre-registering hypotheses or analyses is the response to the question "Approximately when was the first time you pre-registered hypotheses or analyses in advance of a study?" The sample is restricted to Published Authors who completed their PhDs by 2009 (N = 637). The bottom of the shaded region is an estimated adoption rate for the entire sample contacted, including non-respondents; the methodology for calculating the adoption rate of non-respondents is outlined in Table 4.1.

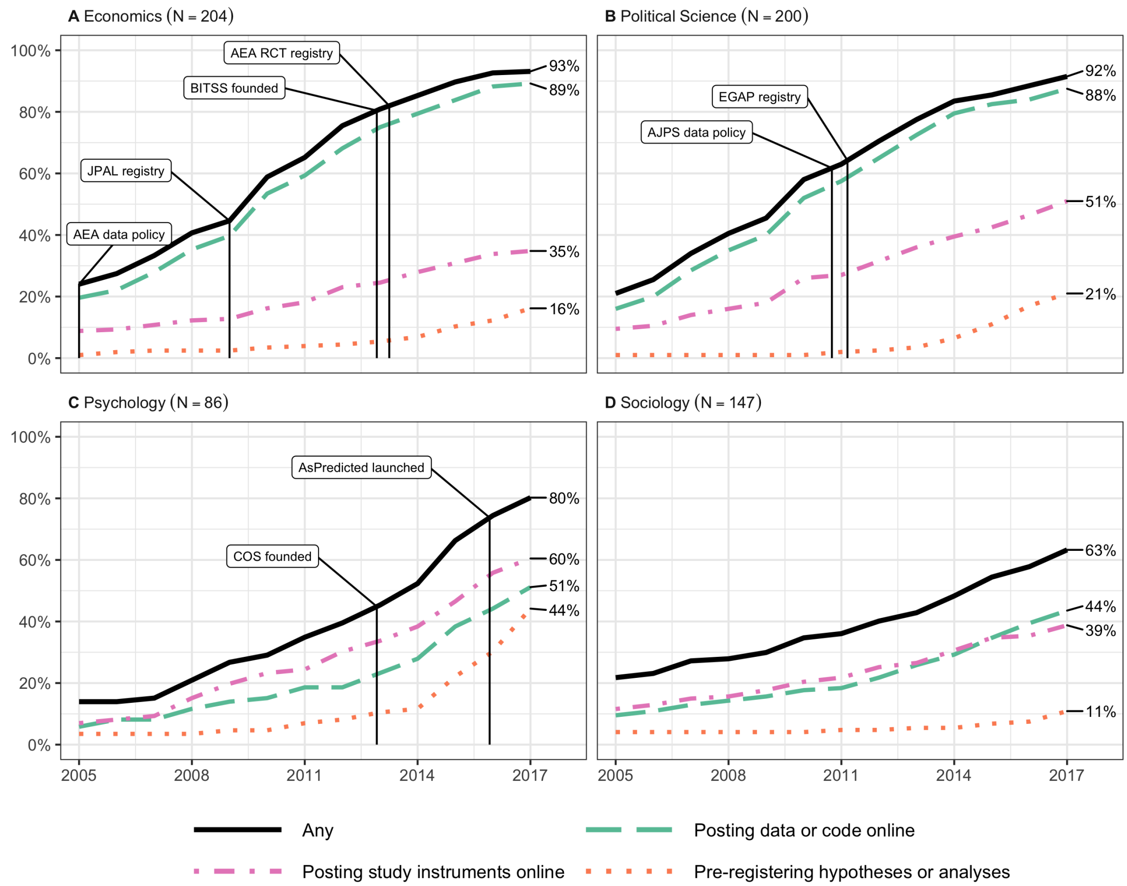


Figure 4.3: Year of Adoption of Open Science Practices - by Discipline

The chart shows for a given year the proportion of Published Authors who had first completed an open science practice in that year or previously, by discipline. The abbreviated names of the organizations used in the labels represent the American Economic Association (AEA), the Abdul Latif Jameel Poverty Action Lab (JPAL), the Berkeley Initiative for Transparency in the Social Sciences (BITSS), the American Economic Association's registry for randomized controlled trials (AEA RCT), the American Journal of Political Science (AJPS), Evidence in Governance and Politics (EGAP), and the Center for Open Science (COS). The organizations mentioned in the figure are included in the panel of the discipline that they work in. BITSS and COS are interdisciplinary organizations, but are included with the discipline they are most associated with.

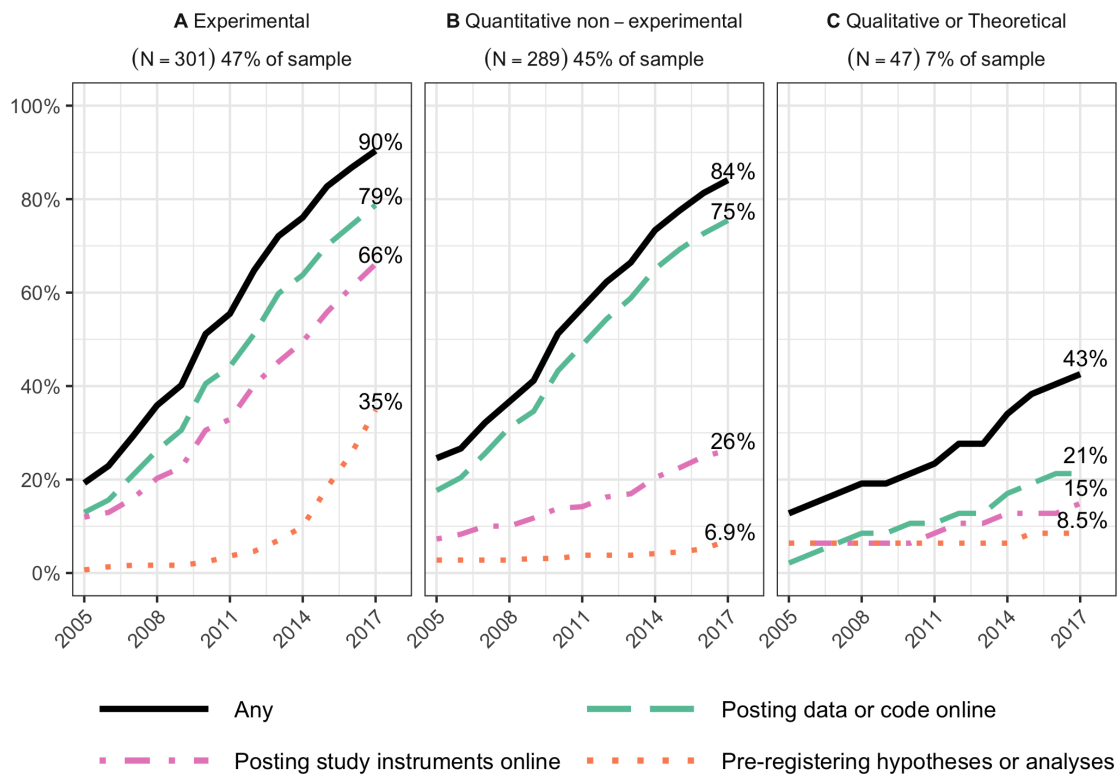


Figure 4.4: Year of Adoption of Open Science Practices - by Research Focus

The chart shows for a given year the proportion of Published Authors who had first completed an open science practice in that year or previously, categorized by the focus of their research. The classification is based on answers to the question "What methods do you use in your research? Please check all that apply." If a scholar only selected "Qualitative" or "Theoretical", they are classified as "Qualitative or Theoretical"; if they selected "Quantitative - Observational" or "Quantitative - Other" but not "Quantitative - Experimental", they are classified as "Quantitative non-experimental"; if they selected "Quantitative - Experimental", they are classified as "Experimental".

(Rubin et al., 1998).

4.4 Current Open Science Beliefs & Practices

The data indicate that open science practices are on the rise across four major social science fields, but how supportive of research transparency are scholars today? How much are they currently planning to engage in open science practices? Figure 4.5 suggests that awareness levels of and support for open science practices are high across all four disciplines. Scholars are generally aware of open science practices (for instance, respondents were asked "Have you ever heard of the practice of publicly posting data and code online for a completed study?"), and they are favorably inclined toward them (e.g., "To what extent do you believe that publicly posting data or code online is important for progress in [Discipline]?"). There is not much of a difference between disciplines, apart from sociology researchers having a somewhat lower level of awareness, support, and adoption. Patterns are similar across specific open science practices (see Appendix Tables C.12 - C.20).

Although comparison across opinion scales and adoption rates is challenging, it appears that actual rates of adoption of open science practices may currently lag behind stated support. It is notable that there are fairly high levels of stated support for open science even among scholars in a discipline like sociology where these tools are not (yet) widely used or taught and where there is a relative lack of institutionalization of these practices.

Perhaps surprisingly, Published Authors and PhD students show similar levels of awareness of and support for open science practices as shown in Appendix Figures C.7 and C.8 respectively. This is in contrast to the authors' prior expectation that PhD Students would exhibit a more supportive attitude toward open science, and suggests that PhD Students may not be the vanguard of changing practices. Open science practices are actually higher among Published Authors, though this is likely because many PhD Students—especially those in their first few years, when they are taking coursework—have not yet had the opportunity to apply the practices to their own work. Researchers across disciplines who use experimental methods show the highest levels of awareness, support, and practice, followed by researchers who use non-experimental quantitative methods. Although qualitative researchers show the least awareness, support, and practice, their awareness and stated support are still at relatively high levels as shown in Appendix Figure C.9.

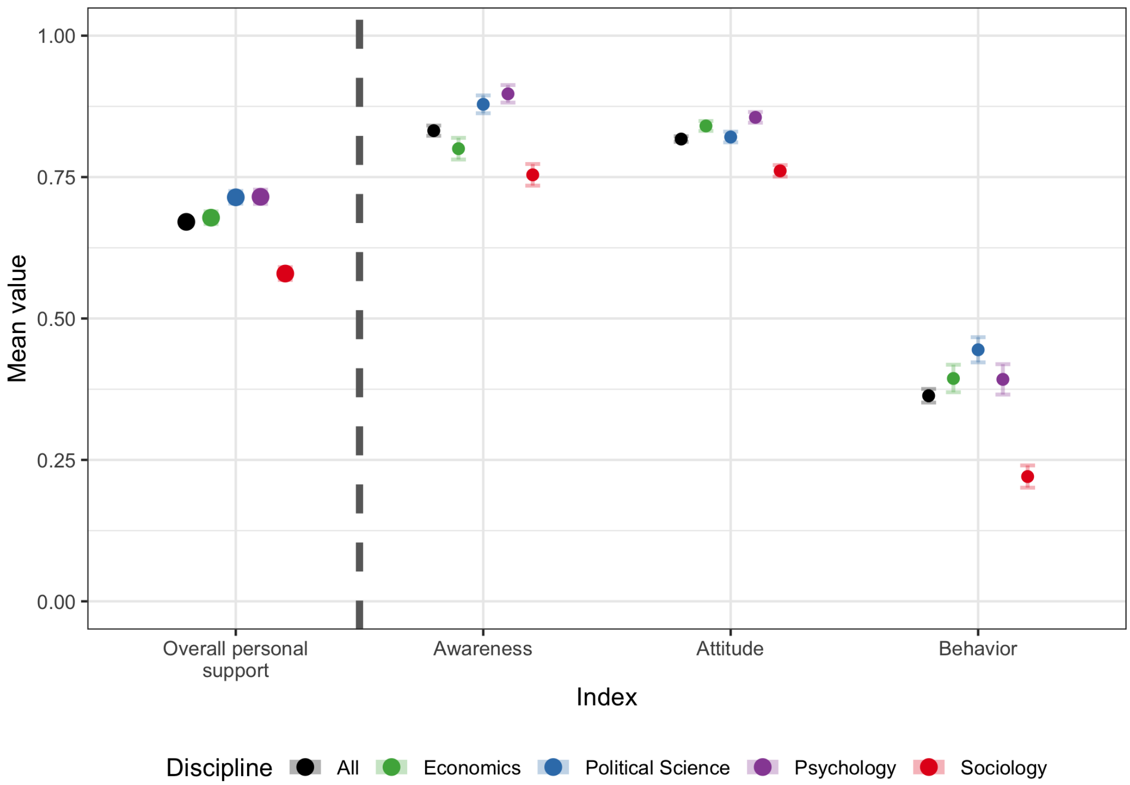


Figure 4.5: Open Science Awareness, Attitudes and Behavior - by Discipline

Lines around the dots are 95% confidence intervals for the estimates. Awareness is an index comprised of questions related to the respondent's i) Awareness of posting data and code online, ii) Awareness of posting study instruments and iii) Awareness of pre-registration. Behavior is an index comprised of questions related to the respondent's i) Behavior of posting data and code online, ii) Behavior of posting study instruments and iii) Behavior of pre-registration. Attitudes is an index comprised of questions related to the respondent's i) Attitudes of posting data and code online, ii) Attitudes of posting study instruments and iii) Attitudes of pre-registration. Overall Personal Support is an average of the three indices. The questions and methodology that are used to construct the indices can be found in Appendix Table C.7.

4.5 Perceived Norms

How do social scientists perceive their fields today, in terms of support for and adoption of open science practices? We measured respondents' perceptions of norms in their disciplines, and compared these perceptions of field-wide opinion and behavior to the average opinion and behavior reported directly in the survey. To measure norms of opinion, we asked respondents to estimate how supportive others in their field are of (1) posting code and data online, and (2) pre-registering hypotheses or analyses in advance of a study. Respondents estimated the percentage of people in their field who fall into each of five opinion categories, ranging from "Not at all in favor" to "Very much in favor," using a dynamic histogram (see Figure 4.6). To measure norms of behavior, we asked respondents to estimate what percentage of researchers in their field actually engage in each of these practices.

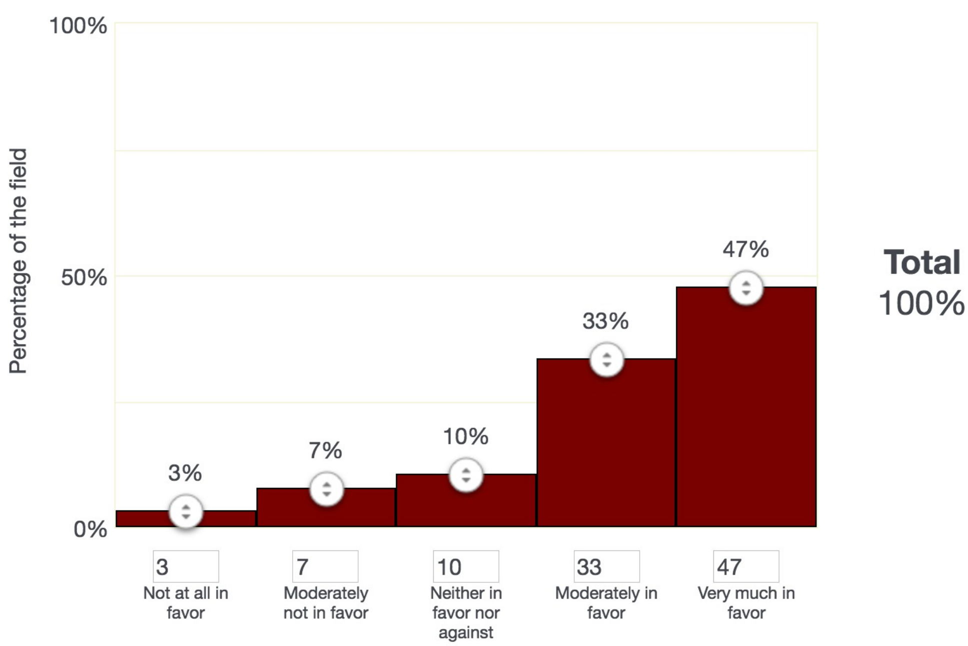


Figure 4.6: Dynamic Histogram Used in the Survey

This chart shows the dynamic histogram that survey respondents used to indicate perceived support for open science in their field. Bars need to add up to 100% for respondents to proceed in the survey.

Figure 4.7 depicts scholars' perceptions of their field, in terms of the distribution of opinion about and adoption rates of the two open science practices, against the actual distribution of opinion and adoption rates as reported by survey respondents in their field. Two findings are apparent. First, perception of support, in green, is consistently smaller than actual support—by a substantial amount when considering attitudes toward posting data or code online. Second, perception of opposition toward open science practices is much greater than actual (survey-estimated) opposition, particularly for the case of attitudes toward pre-registration. (Respondents substantially overestimated the proportion of scholars who are indifferent toward posting data or code online, as well).

A second finding depicted in Figure 4.7 is that survey-estimated rates of support for both open-science practices is substantially larger than the rates of actual behavior–particularly when taking into account respondents who said they were either "Very much" and "moderately" in favor of the practice. This pattern is consistent with substantial latent support for adoption of these practices in the four social sciences that may contribute to further rises in adoption rates in future.

While the rates of adoption demonstrated by our previous measures may or may not have seemed surprising to readers, these data show that the high adoption rate of open science practices would be surprising to our survey respondents, who appear to significantly underestimate open science adoption and support.

There are various possible explanations for why respondents appear to be more in favor of data posting and pre-registration than they believe others in their field to be. One immediate possibility is that our survey sample is selected and unrepresentative in important ways. For instance, we selected respondents based on their publication history in leading research journals and among the most highly-ranked PhD programs, and these populations are not representative of the entire discipline about which respondents are making estimates. Of course, this subgroup of "elite" scholars may be particularly influential in driving the change of social norms in the discipline. Moreover, those who chose to respond to our survey invitation may be more supportive of open science than non-respondents, further shifting sample means, although the evidence we presented above from the audit activity suggests this is less likely.

Another explanation is that respondents are over-reporting their support for open science for reasons of self or social image. However, admitting some social desirability toward responding favorably about open science in an anonymous survey seems to support the idea that a relatively strong social norm in favor of open science has already developed, as suggested in the rates of "actual reports from the field" in Figure 4.7. The figure shows that the median respondent is in favor of these practices.

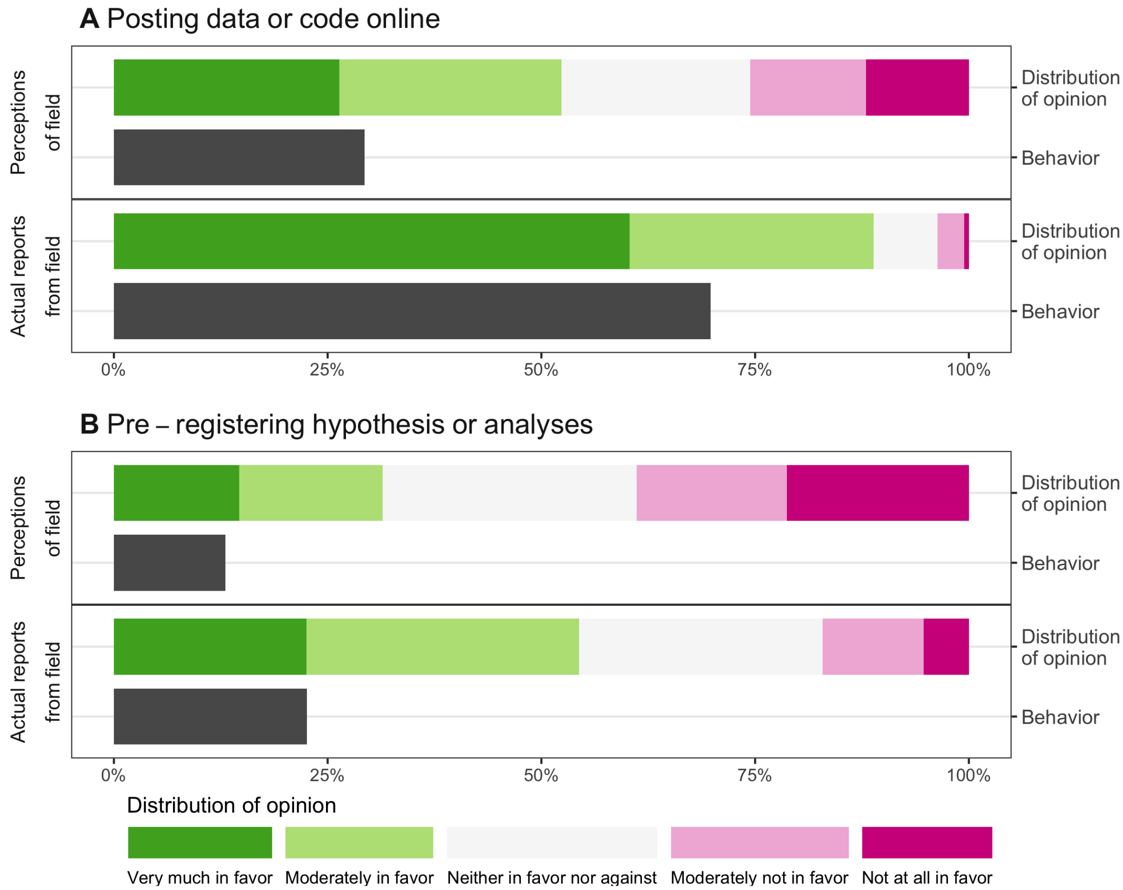


Figure 4.7: Perceived and Actual Support for Open Science among Published Authors

The chart shows differences between perceived and actual support for two practices: posting data or code online and pre-registering hypotheses or analyses. The sample is restricted to Published Authors; the analogous data for Ph.D. students are presented in Appendix Figure C.10. Within each panel, the first bar shows the perceived distribution of support for the practice among Published Authors. This is constructed by asking individuals what percentage of researchers in their field they believe fall into each opinion category, and then averaging over their responses. The solid black bar below shows the fraction of researchers in their field they believe have done the practice. The third bar in the panel shows the distribution of support for the practice constructed using the responses elicited from the Published Authors that we sampled. The final solid black bar shows the proportion of researchers who have actually done the stated practices, using the responses elicited from our survey. Colors indicate the level of support, with green indicating more and red indicating less support. Adjusting the behavior figures to account for non-respondents (using the same methodology as in Figure 4.2) we find that the adjusted share of Published Authors posting data is 64.3% and the adjusted share of Published Author's posting pre-analysis plans is 14%.

This interpretation suggests a social norm in favor of open science at work, even if practices lag behind the ideal. Similarly, the social science research community could be in a period of rapid methodological change, in which case we might expect that beliefs about practices could be temporarily out of sync with actual behaviors. For instance, scholars' views about the state of open science in their discipline could be shaped by their own experiences during their graduate training, or based in part on current journal publications, but both would only capture actual attitudes and practices in the field with a lag.

This set of analyses is consistent with the idea of a current cultural shift in social science research communities, in which behaviors and attitudes are already changing and community members are partially attuned to the change.

4.6 Discussion

Data from a recent representative survey of scholars in four large social science disciplines – economics, political science, psychology, and sociology – indicates that the adoption of open science practices has been increasing rapidly over the past decade. Behaviors such as posting data and materials that were nearly unknown in some fields as recently as 2005 are now practiced by the majority of scholars. Other newer practices, such as study pre-registration, have experienced a sharp rise in adoption just in recent years, especially among scholars who engage in experimental research. While trends are similar to other fields, overall levels of adoption are lowest in sociology. Contrary to our expectations, there is no clear evidence of a generational shift, or of an old guard standing in the way of change: attitudes towards open science practices are remarkably similar among both PhD Students and more established Published Authors. The high levels of support for open science practices expressed among our respondents indicates that the classic scientific ethos famously described by Merton (1979 [1942]) is alive among today's social scientists. A data validation activity confirms that self-reported behaviors are strongly related to actual behavior, and that the selection of survey respondents into the sample has not produced misleading results.

The second main finding of the analysis is that stated support for open science practices is outpacing both their actual adoption and respondents' beliefs about others' support. Taken together, this pattern suggests that social science research communities are in a period of rapid transformation in terms of their research practices, a shift that is not yet entirely appreciated by the community. To follow this co-evolution of behavioral adoption, awareness, and support for open science practices, we plan to collect additional rounds of the 3S survey in the future. These representative snapshots of open science adoption and perception, we argue, can describe the state of the social sciences from the perspective of whether they are currently in the type of transition state described by historians of science as a shift out of "normal" science into one of crisis and eventual transformation (Kuhn, 1962; Hacking, 1981).

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Chapter 5

Conclusion

This dissertation uses empirical approaches to investigate factors affecting local governance and their impacts on the economic policies and welfare of the residents. The finding presented here suggest that both the decentralized structure of governments and the social proximity of bureaucrats can significantly influence the government decisions and economic outcomes.

My results from analyzing the Chinese Township Consolidation program suggest that when local governments in China are acting independently, they are not able to perfectly coordinate or negotiate to fully internalize the border spillovers. The distortion from such failure in coordination between local governments is economically significant. More pollutions and economic outputs are produced than what governments would like if they can make joint decisions.

Analyzing turnovers and appointment of native city leaders in China, I find that the municipal leaders' biographical background plays an important role in their governance decisions. The results suggest that social proximity hampers bureaucrat performance and facilitates local favoritism from the perspective of the general public. At the expense of deteriorating public goods, extensive tax breaks are given to firms in the home counties of native leaders, which may be reciprocated back as legal or illegal benefits (see discussions in Lei, 2018; Bai et al., 2014; Fang et al., 2018). The findings in this paper resonate with the literature on favoritism and patronage (Bandiera et al., 2018; Xu, 2018; Fisman et al., 2017), and may be relevant to other week institutionalized environments in which bureaucrats have large discretionary power.

Besides the research into local governance, I also document the culture and norm shift regarding research transparency in the research community. Data from our representative survey of scholars in four large social science disciplines – economics, political science, psychology, and sociology – indicates that the adoption of open science practices has been increasing rapidly over the past decade and stated support for open science practices is outpacing both their actual adoption and respondents' beliefs about others' support. Taken together, this pattern suggests that social science research communities are in a period of rapid transformation in terms of their research practices, a shift that is not yet entirely appreciated by the community.

The research in this dissertation has some important implications. First, for the specific contexts of my studies, the numerical results can be directly used for policy improvements. For example, as the Township Consolidation program is still ongoing, my estimates can help policymakers to make projections when deciding which pairs of townships to merge with each other. Second, the lessons from my results contribute to the theoretical framework for local governance and may provide guidance on designing a better structure of governments and regulations. In the light of inefficiency costs from incoordination between local governments, more measures can be taken to encourage joint decision-making between localities, even if changes in administrative boundaries are improbable. To reduce corruption and local favoritism, governments may need to redesign the protocols while assigning officials to their posts.

There are also many important questions left unanswered but worth investigating to help improve local governance. What are the other costs of having more centralized governments? How do those costs compare to the benefits of internalizing negative spillovers? Are we able to minimizes the rent-seeking behaviors of native bureaucrats by selecting more prosocial candidates? More quality empirical research following open science practices are definitely needed.

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Appendix A

Appendices for Chapter 2

Tables

Table A.1: Alternative TFP Measure

	(1) Log TFP (ACF)	(2) Log TFP (OP)	(3) TFP (LP)
Distance	0.0045	0.0045	0.0105
	(0.0051)	(0.0069)	(0.0065)
Distance*Polluting	-0.0142***	-0.0118**	-0.0129*
	(0.0051)	(0.0054)	(0.0063)
Constant	2.5182***	2.7230***	5.8614***
	(0.0095)	(0.0110)	(0.0116)
Mean of Dep Variable	2.516	2.722	5.869
Firm FE	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes
Township-Year FE	Yes	Yes	Yes
Number of Observations	124260	122899	122722
R squared	0.780	0.755	0.765

Proofs

Proof of Proposition 1

Proof. Suppose the regulation policy (r) is still reasonable after the increase such that the optimization problem always has an interior solution, then the first order condition for K is:

$$\left.\frac{\partial \pi(K,L,K_E)}{\partial K}\right|_{K^*,L^*,K_E^*} = (1-t)f_K(K^*,L^*) - p_r - rE_1(f^*,K_E^*)f_K(K^*,L^*) = 0$$

$$\Rightarrow f_K(K^*,L^*) = \frac{p_r}{1-t-rE_1(f^*,K_E^*)} > 0$$

From Implicit Function Theorem, we get:

$$\frac{\partial K^*}{\partial r} = -\frac{\left.\frac{\partial^2 \pi(K,L,K_E)}{\partial K \partial r}\right|_{K^*,L^*,K_E^*}}{\left.\frac{\partial^2 \pi(K,L),K_E}{\partial K^2}\right|_{K^*,L^*,K_E^*}} \tag{A.0.1}$$

$$= \frac{E_1(f^*,K_E^*)f_K(K^*,L^*)}{(1-t-rE_1(f^*,K_E^*))f_{KK} - rE_{11}(f^*,K_E^*)f_K(f^*,K_E^*)^2} < 0 \tag{A.0.2}$$

This result means that K^* decreases in r. Following the same logic, it's easy to show L^* decreases in r. As a result, total output decreases. From equation A.0.2, it is easy to see that the absolute value of $\frac{\partial K^*}{\partial r}$ is larger when $E_1(\cdot)$ is larger. The same is true for $\frac{\partial L^*}{\partial r}$.

If regulation policy (r) is so large that marginal revenue of capital becomes negative, there will not be an interior solution and the firm will shutdown the production. The proposition still holds in this scenario. ■

Proof of Proposition 2

Proof.

$$\left.\frac{\partial \pi(K,L,K_E)}{\partial K_E}\right|_{K^*,L^*,K_E^*} = -p_r - rE_2(f^*,K_E^*) = 0$$

From Implicit Function Theorem, we get:

$$\begin{aligned}\frac{\partial K_E^*}{\partial r} &= -\frac{\left.\frac{\partial^2 \pi(K,L,K_E)}{\partial K_E \partial r}\right|_{K^*,L^*,K_E^*}}{\left.\frac{\partial^2 \pi(K,L,K_E)}{\partial K_E^2}\right|_{K^*,L^*,K_E^*}} \\ &= -\frac{E_2(f^*,K_E^*)}{rE_{22}(f^*,K_E^*)} \\ &= \frac{p_r}{r^2 E_{22}(f^*,K_E^*)} > 0\end{aligned}$$

■

Proof of Proposition 3

Proof.

$$\frac{\partial E^*}{\partial r} = E_1(f^*,K_E^*)\frac{\partial f^*}{\partial r} + E_2(f^*,K_E^*)\frac{\partial K_E^*}{\partial r} < 0,$$

because $\frac{\partial f^*}{\partial r} < 0$ and $\frac{\partial K_E^*}{\partial r} > 0$ as shown in Proposition 1 and 2, and $E_1(\cdot) > 0$ and $E_2(\cdot) < 0$ by assumption. ■

Proof of Proposition 4

Proof. For Implicit Function Theorem, we have:

$$\frac{\partial r^*}{\partial \alpha} = \frac{\frac{\partial Surplus(r^*)}{\partial r}}{-\alpha\frac{\partial^2 Surplus(r^*)}{\partial r^2} - \frac{\partial^2 Tax(r^*)}{\partial r^2}}$$

Because marginal benefit of regulation to Surplus is more sensitive to regulation than the marginal cost of regulation to Tax, $|\frac{\partial^2 Surplus(r)}{\partial r^2}| - |\frac{\partial^2 Tax(r)}{\partial r^2}| > 0$.

As a result, $\frac{\partial r^*}{\partial \alpha} > 0$, which shows that the optimal level of regulation r^* is increasing in α if the condition is met. ■

Appendix B

Appendices for Chapter 3

Figure B.1: China's Six Economic Regions

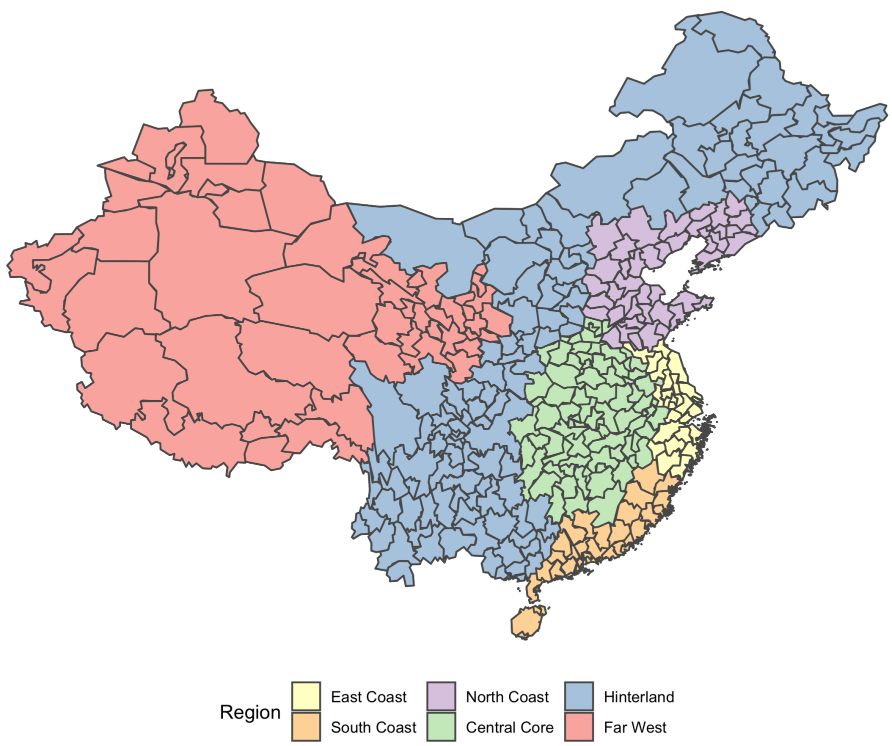

Table B.1: Robustness

Panel A: Fiscal Revenue	(1) Log Revenue	(2) Log VAT	(3) Log Biz Operating Tax	(4) Log City Construction Fee
Native Leader(s)	-0.0763**	-0.1310***	-0.0680**	-0.1105***
	(0.0303)	(0.0377)	(0.0337)	(0.0295)
Log Population	0.0922	-0.3601**	-0.0836	-0.1323
	(0.1273)	(0.1585)	(0.1414)	(0.1240)
Tenure in Position	0.0027	-0.0026	0.0049	0.0021
	(0.0042)	(0.0053)	(0.0047)	(0.0042)
Mean of Dep Variable	11.980	10.210	10.370	9.293
Year FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Number of Observations	1228	1228	1228	1138

Panel B: Fiscal Expenditure			Expenditure Share			
	(1) Log Expenditure	(2) Deficit	(3) Health and Education	(4) Justice	(5) Admin	(6) Construction
Native Leader(s)	-0.0620***	0.0288	1.1352***	0.0503	0.4695**	-0.7432
	(0.0195)	(0.0391)	(0.3239)	(0.1239)	(0.1827)	(0.4917)
Log Population	-0.0748	0.1478	5.2257***	-0.7151	1.3501*	-4.5593**
	(0.0818)	(0.1632)	(1.3063)	(0.5202)	(0.7469)	(1.9291)
Tenure in Position	0.0036	0.0006	-0.1443***	-0.0289*	-0.0298	0.1627**
	(0.0027)	(0.0054)	(0.0433)	(0.0172)	(0.0268)	(0.0697)
Mean of Dep Variable	12.660	0.266	22.047	6.886	12.512	6.156
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	1228	1207	1228	1228	1105	1063

Panel C: Actual Outcomes	(1) Log Hospital Beds	(2) Log Doctors	(3) Log Teachers	(4) Log School Enrollment
Native Leader(s)	-0.0331**	-0.0638***	-0.0221	-0.0152
	(0.0129)	(0.0214)	(0.0202)	(0.0222)
Log Population	0.5839***	0.5815***	1.1654***	1.3275***
	(0.0528)	(0.0873)	(0.0835)	(0.0919)
Tenure in Position	-0.0011	-0.0086***	0.0027	-0.0007
	(0.0018)	(0.0030)	(0.0028)	(0.0031)
Mean of Dep Variable	9.300	8.708	10.300	13.134
Year FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Number of Observations	2158	2158	2175	2179

Note: Standard errors are clustered at the city level and reported in the parenthesis. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix C

Appendices for Chapter 4

C.1 Materials and Methods

Sample

Our population consists of scholars at two career stages.

Published Authors:

These are active social science researchers who have published in a top-10 leading journal within their discipline. We use the following definitions:

- *Active:* At least one publication in 2014-2016.
- *Top-10 leading journals:* The selection of journals was based on citation impact factor. We also added the respective version of the Annual Review for each discipline. In total we have 45 journals, shown in Appendix Tables C.2 through C.5.
- *Discipline:* Before a participant entered the survey, we took an initial guess of their discipline. For PhD Students it was their department, for Published Authors the discipline they have published in most frequently during 2010-2016, with ties split by the most recent publication. We used the initial guess to draw our sample, and for the analysis. The exception was the following, which occurred in a small number of cases: at the beginning of the survey we ask each participant for their primary discipline. If their answer did not match with the initial guess, and they indicated that they do not feel familiar enough to comment on the initially guessed discipline, we asked them to choose which

of the four disciplines they feel sufficiently familiar with. We assigned this discipline to them for our analysis. If they did not feel familiar enough with any of our four disciplines, the survey ended, and they did not become part of our analysis sample.

PhD Students:

These are current PhD Students from top-20 North American doctoral programs within each discipline. We use the following definitions:

- *Current:* Listed on departmental websites in Fall 2017.
- *Top-20 North American Universities:* The 20 US and Canadian universities with the highest rank according to the Times Higher Education World University Rankings 2017. The complete list of schools used can be seen in Appendix Table C.6.

PhD Students who are also Published Authors were sampled only as PhD Students.

Participation Incentives:

Achieving a high response rate and sample size was a critical issue for the validity of our study. Several previous surveys on related transparency and reproducibility topics featured minimal or no monetary compensation for participants and had fairly low response rates, most in the range of 10 to 24% (see Baker, 2016; John et al., 2012). We seek to generate longitudinal data on a far more representative population of leading social science researchers by offering much higher levels of compensation.

Participants were randomly offered either a standard or high incentive. The levels differ between Published Authors and PhD Students, and are based on the response rates from our pilot.

Initial contact was made via email. There were three reminders at intervals following the initial email contact. The survey was administered using a customized online tool (a custom-built interface on top of Qualtrics). Appendix Table C.1 shows the monetary value of the incentives used in the survey. PhD students offered the High incentive had an 8.2 percentage point higher response rate and Published Authors offered the High incentive had a 0.8 percentage point higher response rate.

Table C.1: Participation Incentives

Career Stage	Standard (80% of sample)	High (20% of sample)
Published Authors	$75	$100
PhD Students	$25	$40

Descriptive Analysis:

We aggregate individual survey questions into five measures (awareness, behavior, attitudes, descriptive norms, and prescriptive norms) for each of the three practices (posting data and code online, posting study instruments, and pre-registration). Details of the aggregation method are described in Appendix Table C.8.

We also measure trustworthiness of the literature, behavioral intentions, and projected norms through a set of questions.

We then aggregate the large number of measures to a smaller number of sub-indices and broad indices. Each sub-index is a simple average of measures, and each broad-index is a simple average of sub-indices. See Appendix Table C.7 and Appendix Table C.8 for details.

Altogether, our outcome variables for the descriptive analysis are:

- Sub-Indices: Awareness, Behavior, Attitudes, Descriptive Norms, Prescriptive Norms, Posting data and code online, Posting study instruments, Pre-registration
- Broad Indices: Personal support for open science, Norms, Overall open science, Trustworthiness of literature

The mappings from questions to sub-indices, and from sub-indices to broad-indices can be found in Appendix Tables C.7 through C.8.

Power Calculations:

We based power calculations on conservative estimates of response rates from prior transparency surveys and our own pilot. We conducted power calculations expecting roughly equal numbers in each discipline. These assumptions yield an expected final sample size between 3,000 to 4,000, with N=3200 as our best guess. As shown in the

Appendix Figure C.1, with a power threshold of 80%, we are able to detect small differences in means across groups.

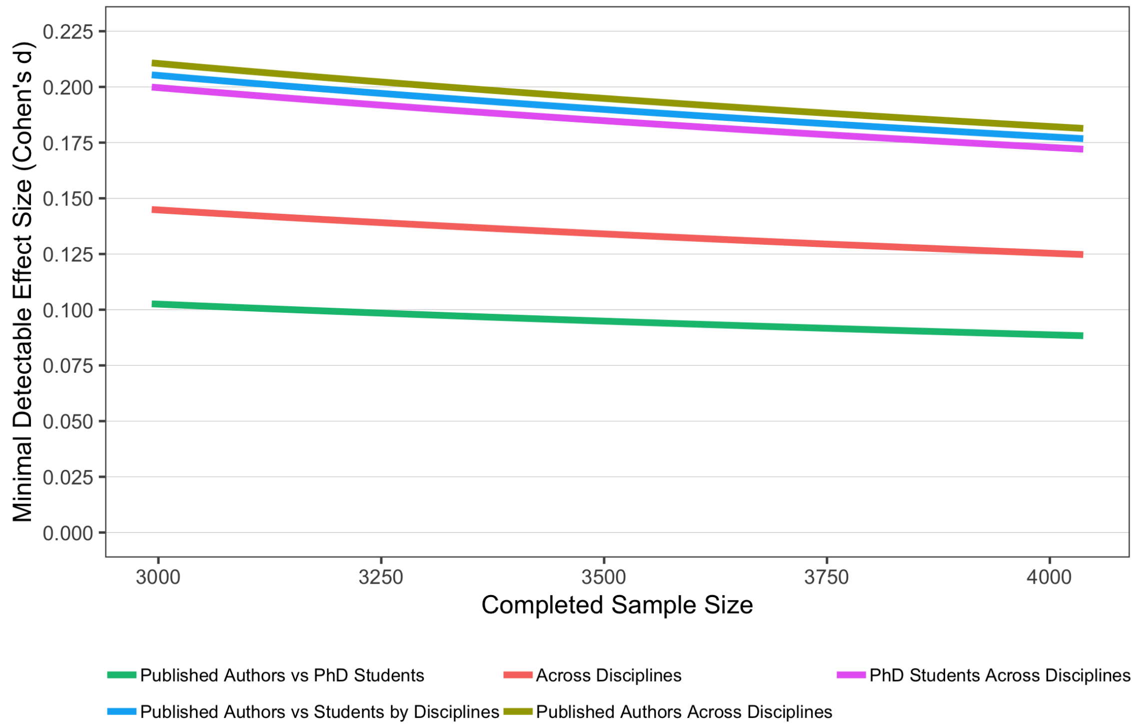

Figure C.1: Power Calculations

The chart shows the minimum detectable effect size at different sample sizes for comparing different subgroups. Power calculations were preregistered. The figure shows the power calculations that we pre-registered. Our realized sample size was 2801. At this sample size, the minimum detectable effect by author type is 0.106, the minimum detectable effect by discipline is 0.1497 and the minimum detectable effect for the interaction is 0.212.

Regression Specifications:

For each outcome variable described in the previous sub-section, we run the following linear regressions.

First, an analysis of differences across disciplines (dropping subscripts denoting individual participants).

$$y = \alpha_1 + \beta_{1a} * \mathbb{I}\{Econ\} + \beta_{1b} * \mathbb{I}\{PoliSci\} + \beta_{1c} * \mathbb{I}\{Psych\} + u_1$$

Second, an analysis of differences between Published Authors and PhD Students.

$$y = \alpha_1 + \beta_2 * \mathbb{I}\{PublishedAuthor\} + u_2$$

Third, an analysis that examines both of these dimensions of heterogeneity:

$$\begin{aligned} y = \alpha_1 + \beta_{3a} * \mathbb{I}\{Econ\} + \beta_{3b} * \mathbb{I}\{PoliSci\} + \beta_{3c} * \mathbb{I}\{Psych\} + \beta_{3d} * \mathbb{I}\{PublishedAuthor\} \\ + \beta_{3e} * \mathbb{I}\{Econ\} * \mathbb{I}\{PublishedAuthor\} + \beta_{3f} * \mathbb{I}\{PoliSci\} * \mathbb{I}\{PublishedAuthor\} \\ + \beta_{3g} * \mathbb{I}\{Psych\} * \mathbb{I}\{PublishedAuthor\} + u_3 \end{aligned}$$

We employ a multiple testing adjustment to address risk of false positives. In particular, we use the False Discovery Rate (FDR) adjustment in Benjamini et al. (2006) and discussed in Anderson (2008). We carry out FDR adjustment across the primary outcome variables.

We also present the averages of our outcome variables by discipline and career stage graphically and estimate regression specifications adjusted for covariates (age, gender, tenured status, US department, leadership position).

Validation Exercise:

In order to validate our survey responses and check for balance across respondents and non-respondents, we conducted an audit of our economics Published Authors. Specifically, we randomly sampled i) 300 economics Published Authors who completed our survey and ii) 100 economics Published Authors who were contacted but did not complete our survey.

We then conducted two audit activities. For *all* sampled individuals we conducted an audit of these authors' pre-registration and data posting behaviors using publicly available information. The protocol for this activity is the first subsection below. This audit activity was completed between March 15, 2019 and March 29, 2019.

The second audit activity was conducted only for the non-respondent sample, and was completed between April 4, 2019 and April 15, 2019. In this activity, we used publicly available data sources to collect data on the primary subfield of these non-respondents. The protocol for this activity is below.

After these subfields were collected we manually categorised these subfields into one of three categories. The first of these was "Theory focused", which is categorised as any individual who listed Microeconomic Theory or Econometrics as a primary subfield. The second was "Macroeconomics/Finance", which was any author who listed Macroeconomics or Finance as a primary field. Finally, all other authors were categorised in the residual category.

Audit Protocol - Open Science behaviors

The goal of the audit is to identify whether a Published Author in the selected sample has (i) pre-registered an analysis or (ii) posted data or code for their projects. We use an author's last name as a keyword to search a set of popular open science websites used by economics scholars.

General Procedure Since the collection of last names was fully automated, auditors first verify whether an author's last name corresponds to a Published Author by looking for a university affiliation using a Google search.

The auditors then go to the websites listed below, and search by last name only. They look through the search results and try to identify the Published Author using their first name or affiliation. Then, following the link associated with an identified author, auditors look for a (i) pre-analysis plan or (ii) posted data or code on the websites. As soon as a match is found, auditors stop searching and record the match and a link to the matched page. If no match can be found, the auditors record that no match was found.

Websites for posting data or code online

- Dataverse.org
- Authors' personal websites

Websites for pre-registering analysis (PAP)

- SocialScienceRegistry.org (AEA RCT registry). Details of some pre-analysis plans may not be visible to the public, but we still count those as having pre-registered.
- OSF.io
- Authors' personal websites

Audit Protocol - Author Subfield

The goal of this activity is to collect data on the primary subfields of Economics Published Authors that did not complete the survey. The following steps are followed to complete this activity:

- Go to the author's webpage. Record subfields information if subfields of interest are listed on the homepage or another part of the webpage.
- Open the author's CV. Record any subfields that are listed on the author's CV.

Sampling frame and Outcome Index Construction:

Table C.2: Economics Journals

Index	Journal	Publisher
NR	Annual Review of Economics	Annual Reviews
1	The Quarterly Journal of Economics	Oxford University Press
2	Journal of Political Economy	University of Chicago Press, JSTOR
3	American Economic Review	American Economic Association, JSTOR
4	Econometrica	Wiley, JSTOR
5	Journal of Economic Growth	Springer, JSTOR
6	Review of Economic Studies	Oxford University Press
7	Journal of Monetary Economics	Elsevier
8	Journal of Econometrics	Elsevier
9	Journal of Labor Economics	University of Chicago Press
10	The Review of Economics and Statistics	MIT Press

Sampling Frame Economics Published Authors Journals used to sample economics Published Authors. While the Annual Review of Economics is not ranked, it is included as it is an influential journal for the field. The selection of journals is based on citation impact factor.

Table C.3: Political Science Journals

Index	**Journal**	**Publisher**
NR	Annual Review of Political Science	Annual Reviews
1	American Journal of Political Science	Wiley
2	American Political Science Review	Cambridge University Press
3	The Journal of Politics	University of Chicago Press
4	British Journal of Political Science	Cambridge University Press
5	Political Analysis	Oxford University Press
6	Comparative Political Studies	SAGE Publishing
7	World Politics	Cambridge University Press
8	Political Behavior	Springer
9	International Organization	Cambridge University Press
10	International Studies Quarterly	Wiley

Sampling Frame Political Science Published Authors Journals used to sample political science Published Authors. While the Annual Review of Political Science is not ranked, it is included as it is an influential journal for the field. The selection of journals is based on citation impact factor.

Table C.4: Psychology Journals

Index	**Journal**	**Publisher**
NR	Annual Review of Psychology	Annual Reviews
1	Psychological Science	SAGE Publishing
2	Psychological Bulletin	American Psychological Association
3	American Psychologist	American Psychological Association
4	Journal of Experimental Psychology - General	American Psychological Association
5	Trends in Cognitive Sciences	Elsevier
6	Social Cognitive and Affective Neuroscience	Oxford University Press
7	Journal of Personality and Social Psychology	American Psychological Association
8	Journal of Consulting and Clinical Psychology	American Psychological Association
9	Child Development	Wiley
10	Developmental Psychology	American Psychological Association

Sampling Frame Psychology Published Authors Journals used to sample psychology Published Authors. While the Annual Review of Psychology is not ranked, it is included as it is an influential journal for the field. The selection of journals is based on citation impact factor.

Table C.5: Sociology Journals

Index	**Journal**	**Publisher**
NR	Annual Review of Sociology	Annual Reviews
1	American Sociological Review	SAGE Publishing
2	American Journal of Sociology	University of Chicago Press
3	European Sociological Review	Oxford University Press
4	Social Forces	Oxford University Press
5	Social Problems	Oxford University Press
6	Demography	Springer
7	Criminology	Wiley
8	Gender & Society	SAGE Publishing
9	Administrative Science Quarterly	SAGE Publishing
10	Sociology of Education	SAGE Publishing
11	Social Networks	Elsevier

Sampling Frame Sociology Published Authors Journals used to sample sociology Published Authors. While the Annual Review of Sociology is not ranked, it is included as it is an influential journal for the field. The selection of journals is based on citation impact factor and disciplinary expert recommendation.

Table C.6: Top 20 North American Doctoral Programs

Rank	**University**	**Country**
1	Stanford University	US
2	Yale University	US
3	University of Chicago	US
4	Harvard University	US
5	Massachusetts Institute of Technology	US
6	University of Michigan-Ann Arbor	US
7	Princeton University	US
8	University of California, Los Angeles	US
9	University of California, Berkeley	US
10	Columbia University	US
11	University of Pennsylvania	US
12	Cornell University	US
13	Duke University	US
14	University of Wisconsin-Madison	US
15	University of Toronto	Canada
16	University of British Columbia	Canada
17	New York University	US
18	Northwestern University	US
19	University of Washington-Seattle	US
20	University of California, San Diego	US

Sampling Frame PhD Students PhD Students in the paper were sampled from universities listed in the table. The ranking is the Times Higher Education 2017 Social Science ranking.

Table C.7: Mapping Measures to Indices

Measure	**Sub-Index**	**Broad Index**
1.1.1 Awareness of posting data and code online	1.1 Awareness	1. Personal support for open science
1.1.2 Awareness of posting study instruments		
1.1.3 Awareness of pre-registration		
1.2.1 Behavior of posting data and code online	1.2 Behavior	
1.2.2 Behavior of posting study instruments		
1.2.3 Behavior of pre-registration		
1.3.1 Attitudes of posting data and code online	1.3 Attitudes	
1.3.2 Attitudes of posting study instruments		
1.3.3 Attitudes of pre-registration		
2.1.1 Descriptive norms of posting data and code online	2.1 Descriptive norms	2. Norms
2.1.2 Descriptive norms of pre-registration		
2.2.1 Prescriptive norms of posting data and code online	2.2 Prescriptive norms	
2.2.2 Prescriptive norms of pre-registration		
3.1.1 Awareness of posting data and code online	3.1 Posting data and code online	3. Overall Open Science
3.1.2 Behavior of posting data and code online		
3.1.3 Attitudes of posting data and code online		
3.1.4 Descriptive norms of posting data and code online		
3.1.5 Prescriptive norms of posting data and code online		
3.2.1 Awareness of posting study instruments	3.2 Posting study instruments	
3.2.2 Behavior of posting study instruments		
3.2.3 Attitudes of posting study instruments		
3.3.1 Awareness of pre-registration	3.3 Pre-registration	
3.3.2 Behavior of pre-registration		
3.3.3 Attitudes of pre-registration		
3.3.4 Descriptive norms of pre-registration		
3.3.5 Prescriptive norms of pre-registration		
4. Trustworthiness of literature		4. Trustworthiness of literature

Measures incorporated in Indices The table shows the mapping from measures (see Appendix Table C.8) to indices. Each sub-index is a simple average of measures, and each broad-index is a simple average of sub-indices.

Table C.8: Mapping Questions to Measures

Question	**Measure**	**Rescaling and Aggregation**
Have you ever heard of the practice of publicly posting data and code online for a completed study?	1.1.1 Awareness of posting data and code online	"No" → 0, "Yes" → 1
Approximately how many times have you publicly posted data or code online?	1.2.1 Behavior of posting data and code online	Question "Approximately…" coded as 0 → 0, anything ≥ 1 → 1; Question "Think about the last…" coded as "No" → 0, "Yes" → 1, "I have not published an empirical paper" → NA; Question "Do you encourage …" coded as ("No, and I don't plan to", "No, but I plan to in the future") → "0", ("Yes, I do") → "1"; Average over questions
Think about the last empirical paper you published. Have you publicly posted the data or code online for that paper?		
Do you encourage students to publicly post data or code online?		
To what extent do you believe that publicly posting data or code online is important for progress in [Discipline]?	1.3.1 Attitude of postin data and code online	Rescale from 1-5 to 0-1; Average over questions
What is your opinion of publicly posting data or code online?		

continued …

Question	**Measure**	**Rescaling and Aggregation**
In your estimation, what percentage of researchers across the discipline of [Discipline] publicly post data or code online?	2.1.1 Descriptive norm of posting data or code online	Average over questions
In your estimation, what percentage of researchers in your sub-field of [Sub-discipline] publicly post data or code online?		
In your estimation, what is the distribution of opinion across the discipline of [Discipline] about publicly posting data or code online?	2.2.1 Prescriptive norm of posting data or code online	Calculate mean of distribution; Rescale from 1-5 to 0-1
In your estimation, what is the distribution of opinion in your sub-field of [Sub-discipline] about publicly posting data or code online?		
Have you ever heard of the practice of publicly posting study instruments online for a completed study?	1.1.2 Awareness of posting study instruments	“No” → 0, “Yes” → 1

continued . . .

Question	Measure	Rescaling and Aggregation
Approximately how many times have you publicly posted study instruments online?	1.2.2 Behavior of posting study instruments	Question “Approximately…” coded as 0 → 0, anything ≥ 1 → 1; Question “Think about the last…” coded as “No” → 0, “Yes” → 1, “I have not published an empirical paper” → NA; Question ”Do you encourage …” coded as (“No, and I don’t plan to”, “No, but I plan to in the future”) → “0”, (“Yes, I do”) → “1”; Average over questions
Think about the last empirical paper you published. Have you publicly posted the study instruments online for that paper?		
Do you encourage students to publicly post study instruments online?		
To what extent do you believe that publicly posting study instruments online is important for progress in [Discipline]?	1.3.2 Attitude of posting study instruments	Rescale from 1-5 to 0-1; Average over questions
What is your opinion of publicly posting study instruments online?		
Have you ever heard of the practice of pre-registering hypotheses or analyses in advance of a study?	1.1.3 Awareness of pre-registration	Rescale from 1-5 to 0-1; Average over questions

continued …

Question	Measure	Rescaling and Aggregation
Approximately how many times have you pre-registered hypotheses or analyses in advance of a study?	1.2.3 Behavior of pre-registration	Question "Approximately…" coded as 0 $\rightarrow$ 0, anything $\geq$ 1 $\rightarrow$ 1; Question "Think about the last…" coded as "No" $\rightarrow$ 0, "Yes" $\rightarrow$ 1, "I have not published an empirical paper" $\rightarrow$ NA; Question "Do you encourage …" coded as ("No, and I don't plan to", "No, but I plan to in the future") $\rightarrow$ "0", ("Yes, I do") $\rightarrow$ "1"; Average over questions
Think about the last empirical research you completed. Did you pre-register the hypotheses or analyses for that research?		
Do you encourage students to pre-register hypotheses or analyses in advance of a study?		
To what extent do you believe that pre-registering hypotheses or analyses is important for progress in [Discipline]?	1.3.3 Attitude of pre-registration	Rescale from 1-5 to 0-1; Average over questions
What is your opinion of pre-registering hypotheses or analyses?		
In your estimation, what percentage of researchers across the discipline of [Discipline] pre-register hypotheses or analyses in advance of a study?	2.1.2 Descriptive norm of pre-registration	Rescale from 0-100 to 0-1; Average over questions
In your estimation, what percentage of researchers in your sub-field of [Sub-discipline] pre-register hypotheses or analyses in advance of a study?		

continued …

Question	**Measure**	**Rescaling and Aggregation**
In your estimation, what is the distribution of opinion across the discipline of [Discipline] about pre-registering hypotheses or analyses in advance of a study?	2.2.2 Prescriptive norm of pre-registration	Calculate mean of distribution; Rescale from 1-5 to 0-1
In your estimation, what is the distribution of opinion in your subfield of [Sub-discipline] about pre-registering hypotheses or analyses in advance of a study?		
How confident are you that the influential research findings in [Discipline] would replicate?	4. Trustworthiness ofliterature	Rescale from 1-5 to 0-1; Average over questions
When researchers run studies testing the canonical research findings in [Discipline], how confident are you that the studies will be able to replicate the canonical results?		
When researchers run studies testing recent research findings in [Discipline], how confident are you that the studies will be able to replicate the recent results?		
Think about the table of contents in the latest issue of [Discipline]'s top journal. How confident are you that the results of the studies will replicate?		

Questions incorporated in Measures The table shows the survey questions that are included in each measure. Each measure is then combined with other measures to produce indices (see Appendix Table C.7). In the cases where multiple questions are used in a single measure, how these questions are aggregated is also described.

Results

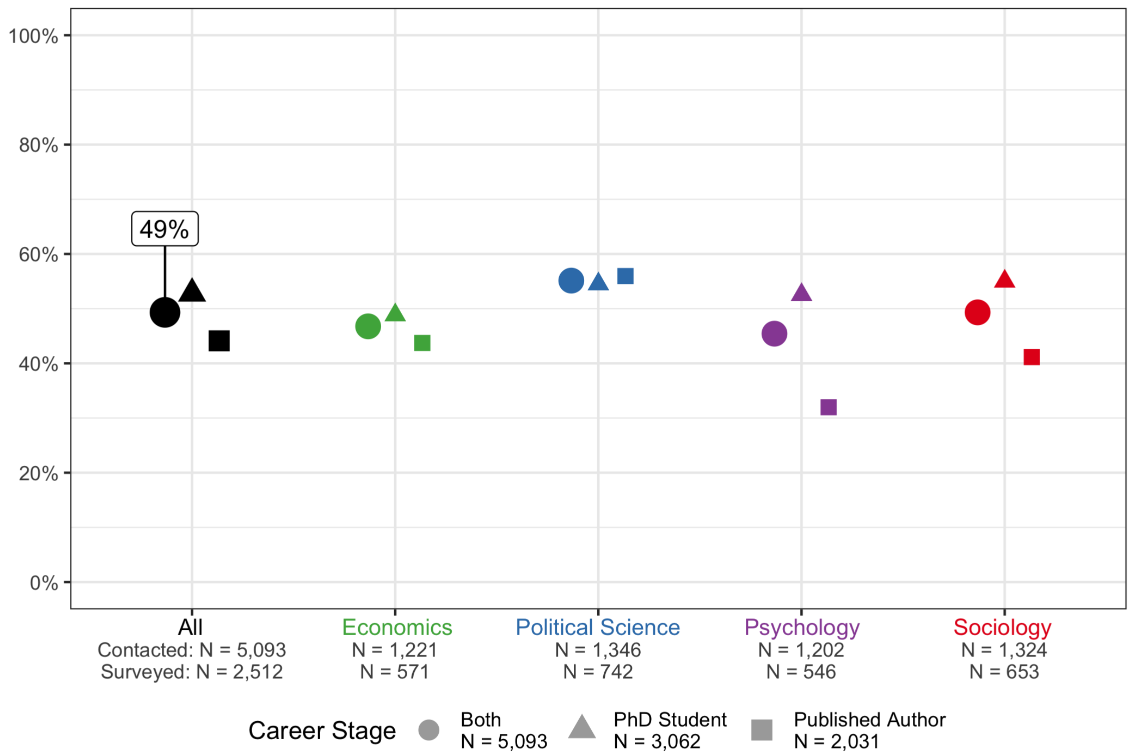

Figure C.2: Response Rates are Higher in the United States and Canada Sample

Response rates by discipline and by career stage (PhD Student or Published Author). This figure shows the response rate by discipline and author status for all PhD Students and Published Authors whose institution was based in the United States or Canada.

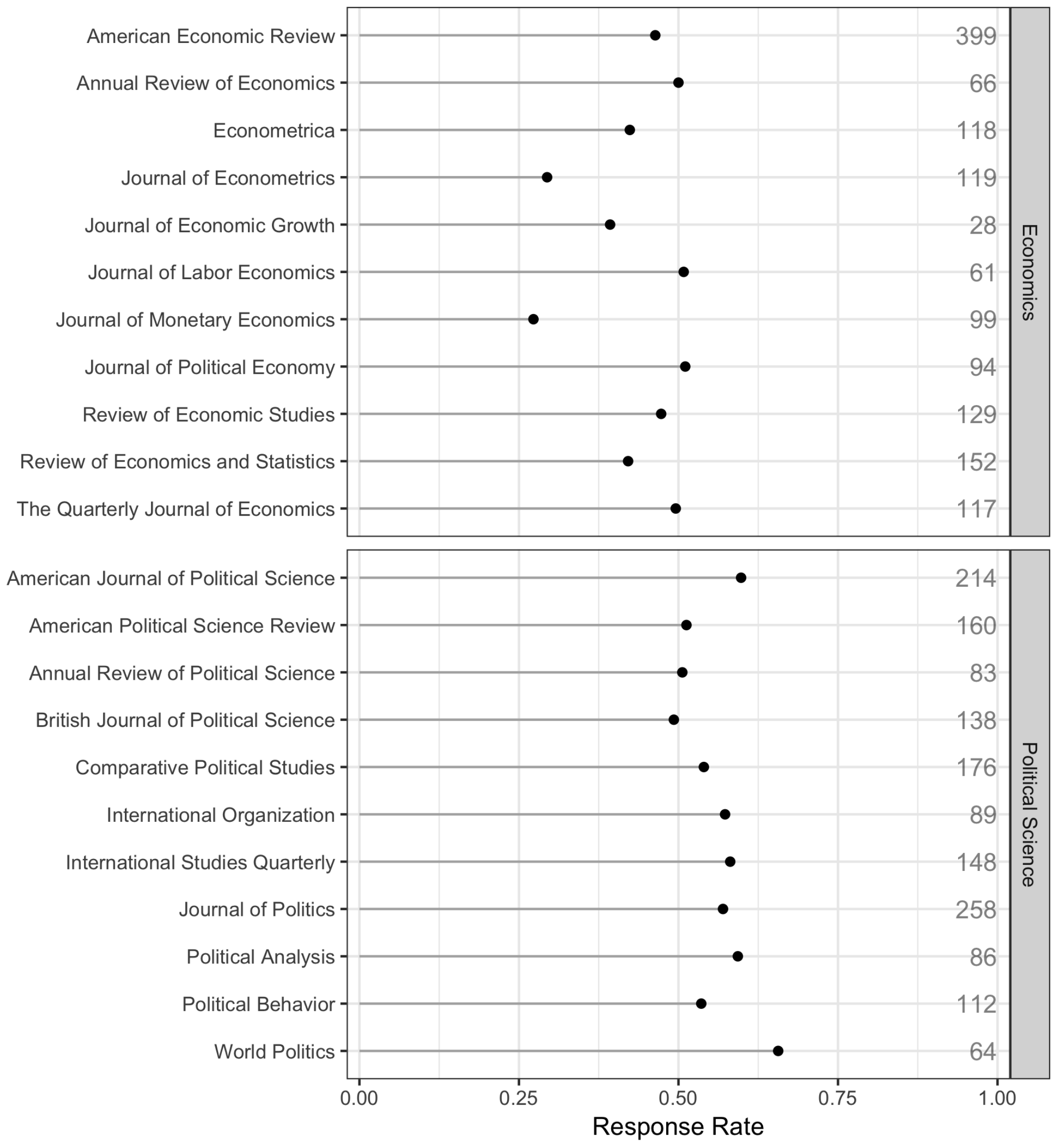

Figure C.3: Response Rate by Journal, Part 1

This figure shows the response rate by journal for the universe of journals that were used as the sampling frame for Published Authors in this project. Each panel denotes the journals for a different discipline. Numbers in grey on the right hand side of the figures show the raw number of respondents from each journal. The published author sample is drawn from the universe of authors that published in one of the above journals during the timeframe 2014-2016. However, the Published Authors are matched to any journal in the above table by any journal that they published in during the period 2010-2016. Therefore the number of Published Authors in the table above is larger than the number of Published Authors in our sample.

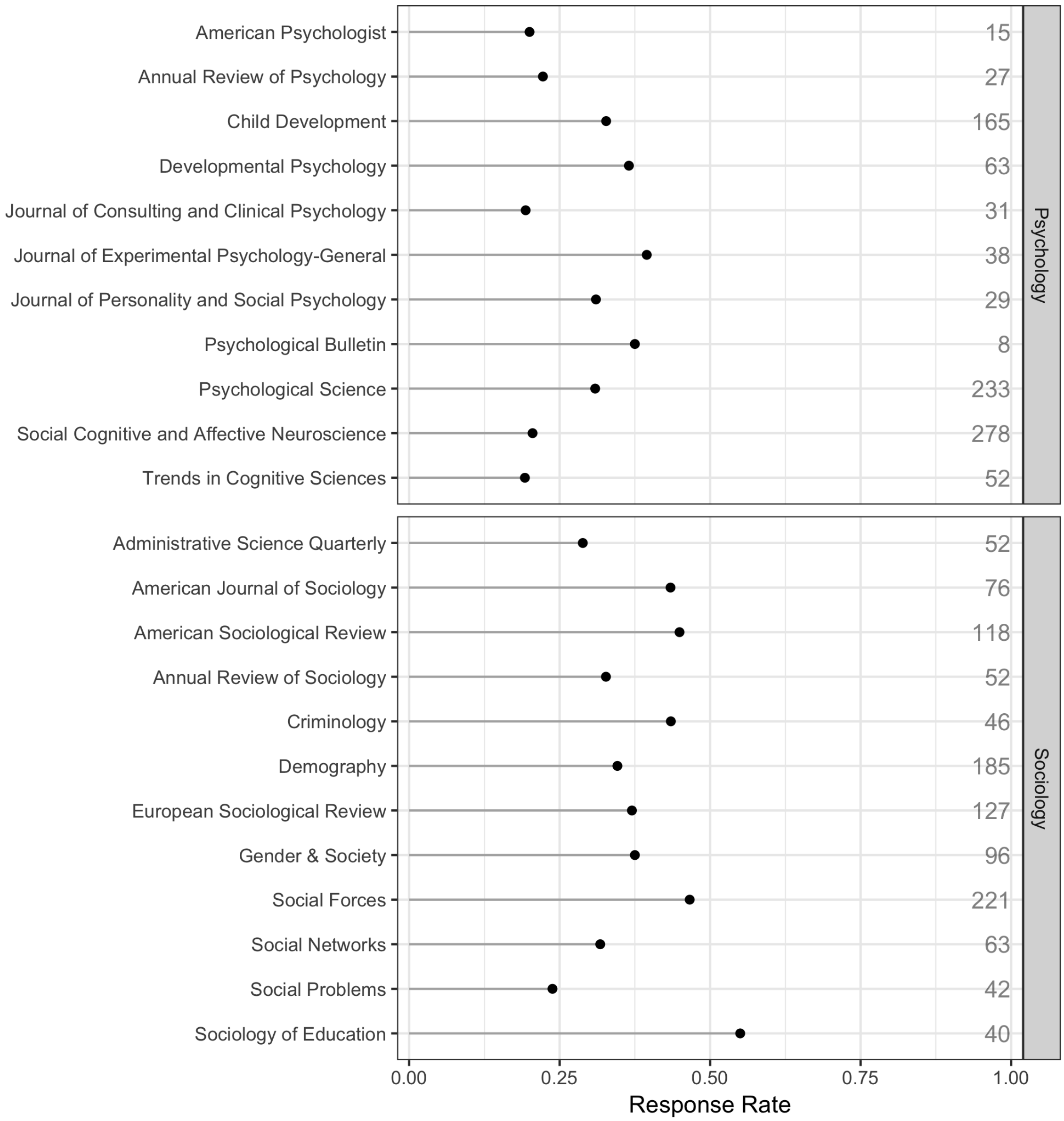

Figure C.4: Response Rate by Journal, Part 2

This figure shows the response rate by journal for the universe of journals that were used as the sampling frame for Published Authors in this project. Each panel denotes the journals for a different discipline. Numbers in grey on the right hand side of the figures show the raw number of respondents from each journal. The published author sample is drawn from the universe of authors that published in one of the above journals during the timeframe 2014-2016. However, the Published Authors are matched to any journal in the above table by any journal that they published in during the period 2010-2016. Therefore the number of Published Authors in the table above is larger than the number of Published Authors in our sample.

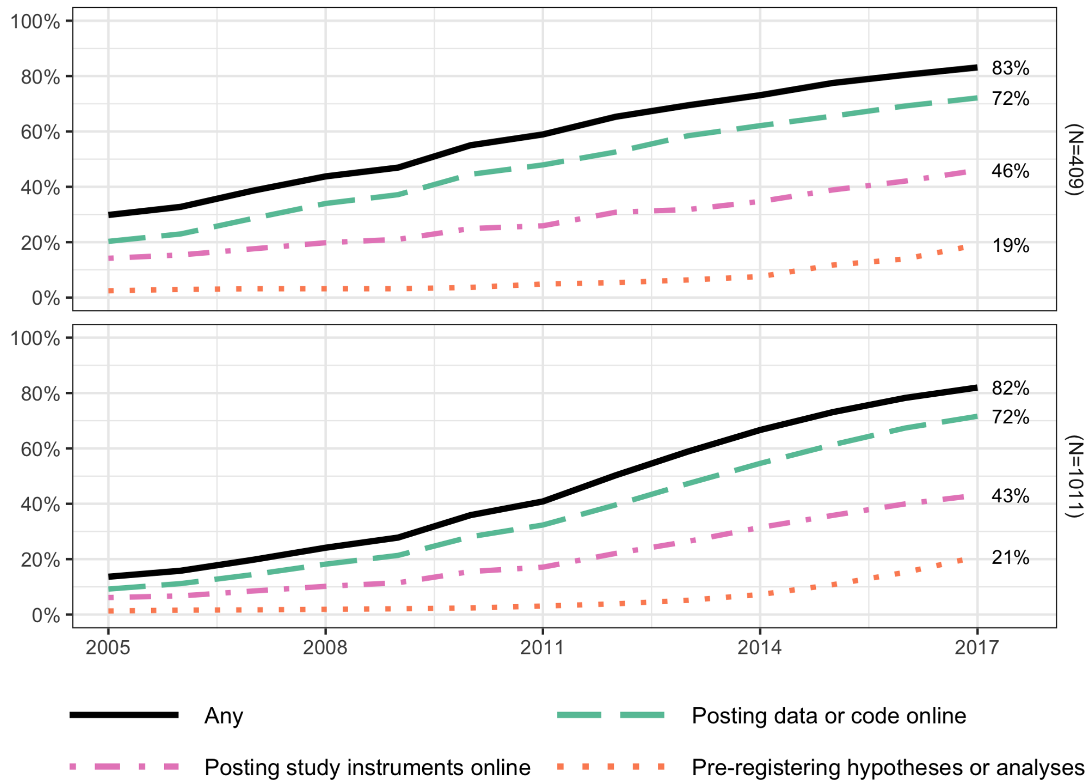

Figure C.5: Year of Adoption of Open Science Practices - Alternate Cutoff Dates

The chart shows for a given year the proportion of Published Authors who had first completed an open science practice in that year or previously. Posting study instruments online is the response to the question "Approximately when was the first time you publicly posted study instruments online?". Posting data or code online is the response to the question "Approximately when was the first time you publicly posted data or code online?". Pre-registering hypotheses or analyses is the response to the question "Approximately when was the first time you pre-registered hypotheses or analyses in advance of a study?". The sample is restricted to Published Authors who completed their PhD by 2005 in the first panel, and Published Authors who completed their PhD prior to 2016 in the second panel.

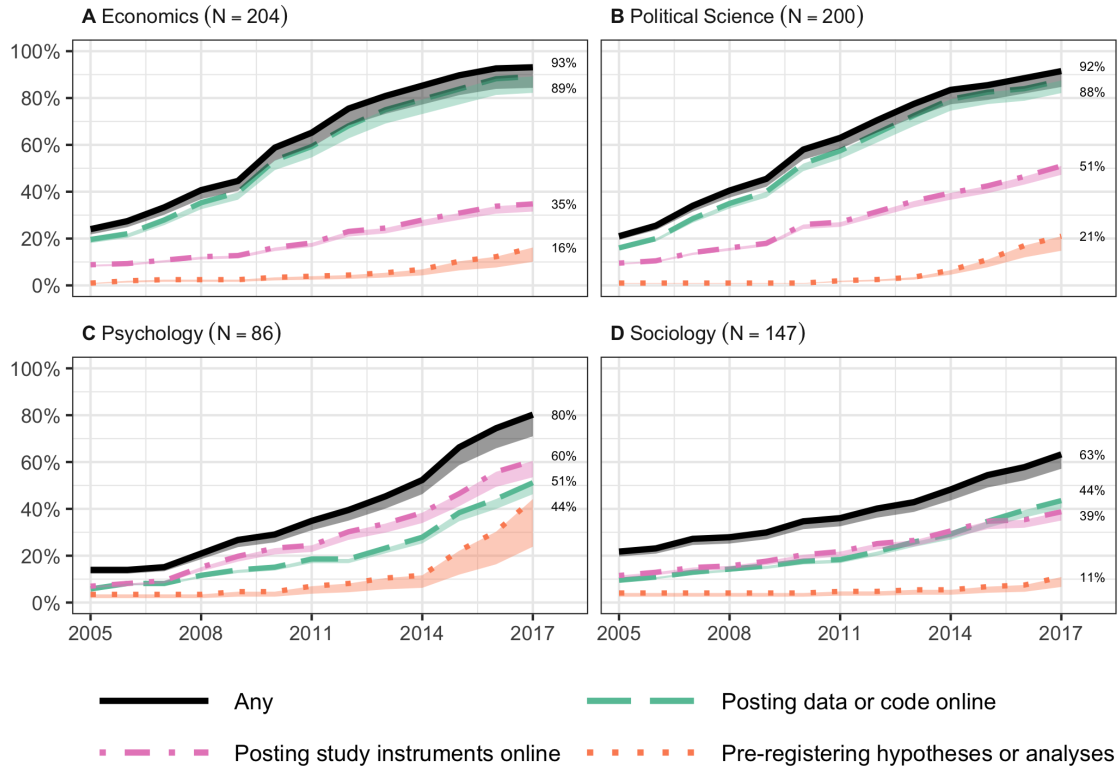

Figure C.6: Adoption by Discipline

The chart shows for a given year the proportion of Published Authors who had first completed an open science practice in that year or previously. Posting study instruments online is the response to the question "Approximately when was the first time you publicly posted study instruments online?". Posting data or code online is the response to the question "Approximately when was the first time you publicly posted data or code online?". Pre-registering hypotheses or analyses is the response to the question "Approximately when was the first time you pre-registeredhypotheses or analyses in advance of a study?". The sample is restricted to Published Authors who completed their PhD by 2009. The bottom of the shaded region is an estimated adoption rate for the entire sample contacted, including non-respondents; the methodology for calculating the adoption rate of non-respondents is outlined in Table 4.1.

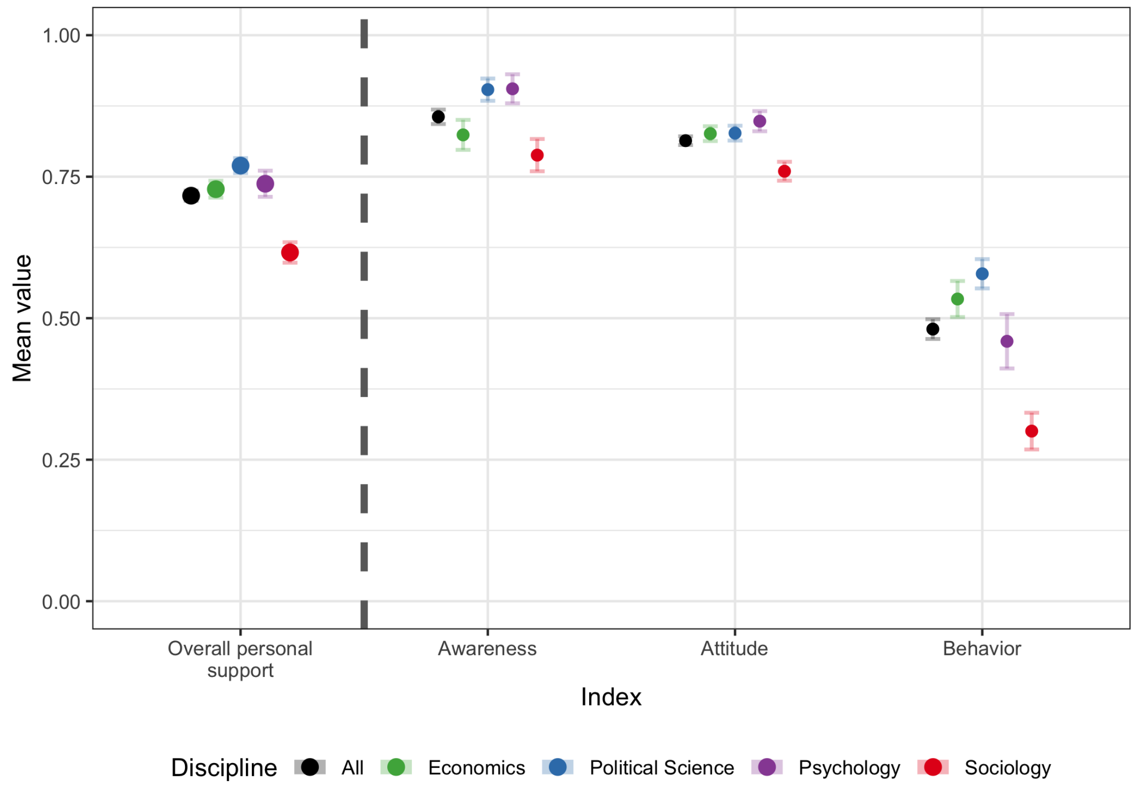

Figure C.7: Published Author Open Science Awareness, Attitudes and Behavior - by Discipline

Lines around the dots are 95% confidence intervals for the estimates. Awareness is an index comprised of questions related to the respondent's i) Awareness of posting data and code online, ii) Awareness of posting study instruments and iii) Awareness of pre-registration. Behavior is an index comprised of questions related to the respondent's i) Behavior of posting data and code online, ii) Behavior of posting study instruments and iii) Behavior of pre-registration. Attitudes is an index comprised of questions related to the respondent's i) Attitudes of posting data and code online, ii) Attitudes of posting study instruments and iii) Attitudes of pre-registration. Overall Personal Support is an average of the three indices. The questions and methodology that are used to construct the indices can be found in Appendix Table C.7.

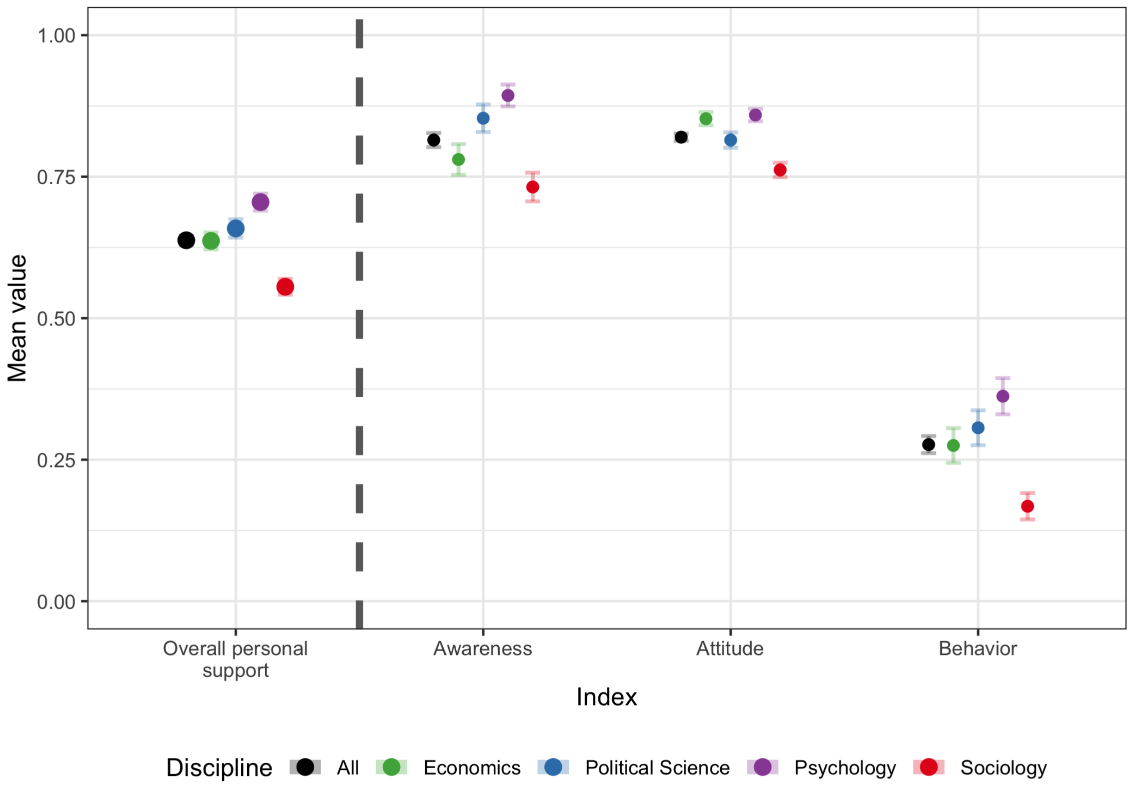

Figure C.8: Student Open Science Awareness, Attitudes and Behavior - by Discipline

Grey lines around the dots are 95% confidence intervals for the estimates. Awareness is an index comprised of questions related to the respondent's i) Awareness of posting data and code online, ii) Awareness of posting study instruments and iii) Awareness of pre-registration. Behavior is an index comprised of questions related to the respondent's i) Behavior of posting data and code online, ii) Behavior of posting study instruments and iii) Behavior of pre-registration. Attitudes is an index comprised of questions related to the respondent's i) Attitudes of posting data and code online, ii) Attitudes of posting study instruments and iii) Attitudes of pre-registration. Overall Personal Support is an average of the three indices. The questions and methodology that are used to construct the indices can be found in Appendix Table C.7.

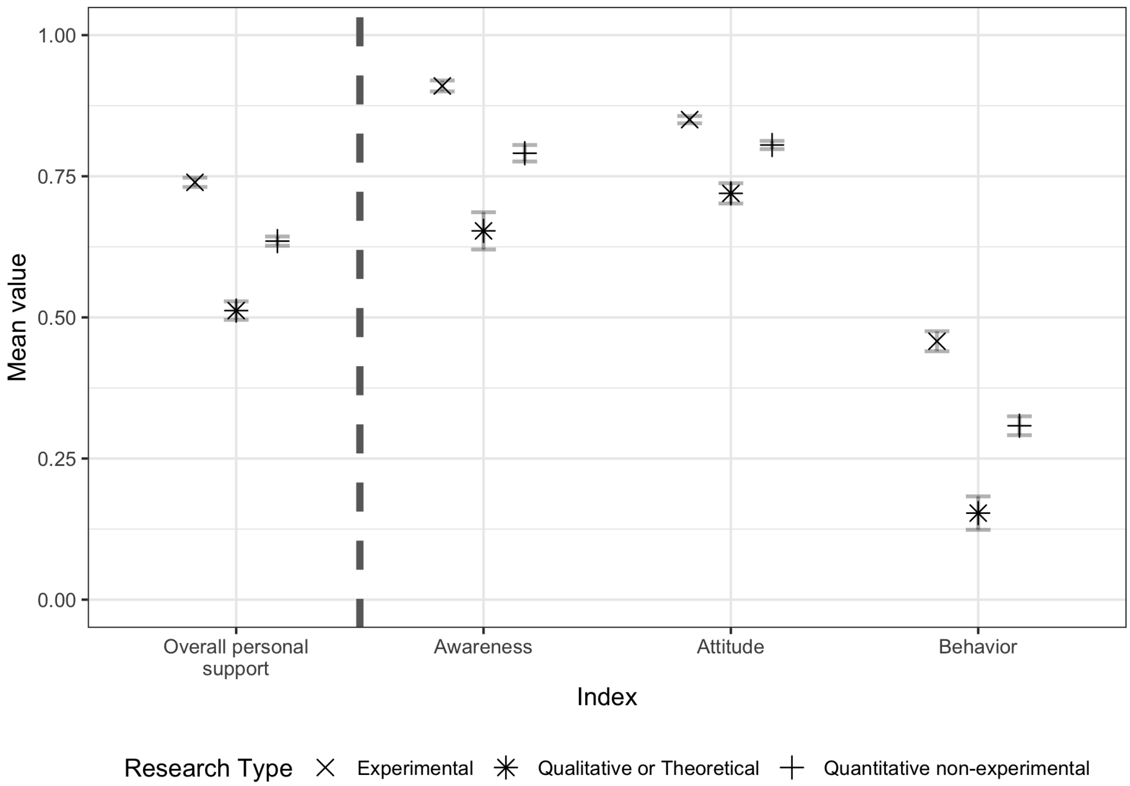

Figure C.9: Open Science Awareness, Attitudes and Behavior - by Research Type

Grey lines around the dots are 95% confidence intervals for the estimates. Awareness is an index comprised of questions related to the respondent's i) Awareness of posting data and code online, ii) Awareness of posting study instruments and iii) Awareness of pre-registration. Behavior is an index comprised of questions related to the respondent's i) Behavior of posting data and code online, ii) Behavior of posting study instruments and iii) Behavior of pre-registration. Attitudes is an index comprised of questions related to the respondent's i) Attitudes of posting data and code online, ii) Attitudes of posting study instruments and iii) Attitudes of pre-registration. Overall Personal Support is an average of the three indices. The questions and methodology that are used to construct the indices can be found in Appendix Table C.7.

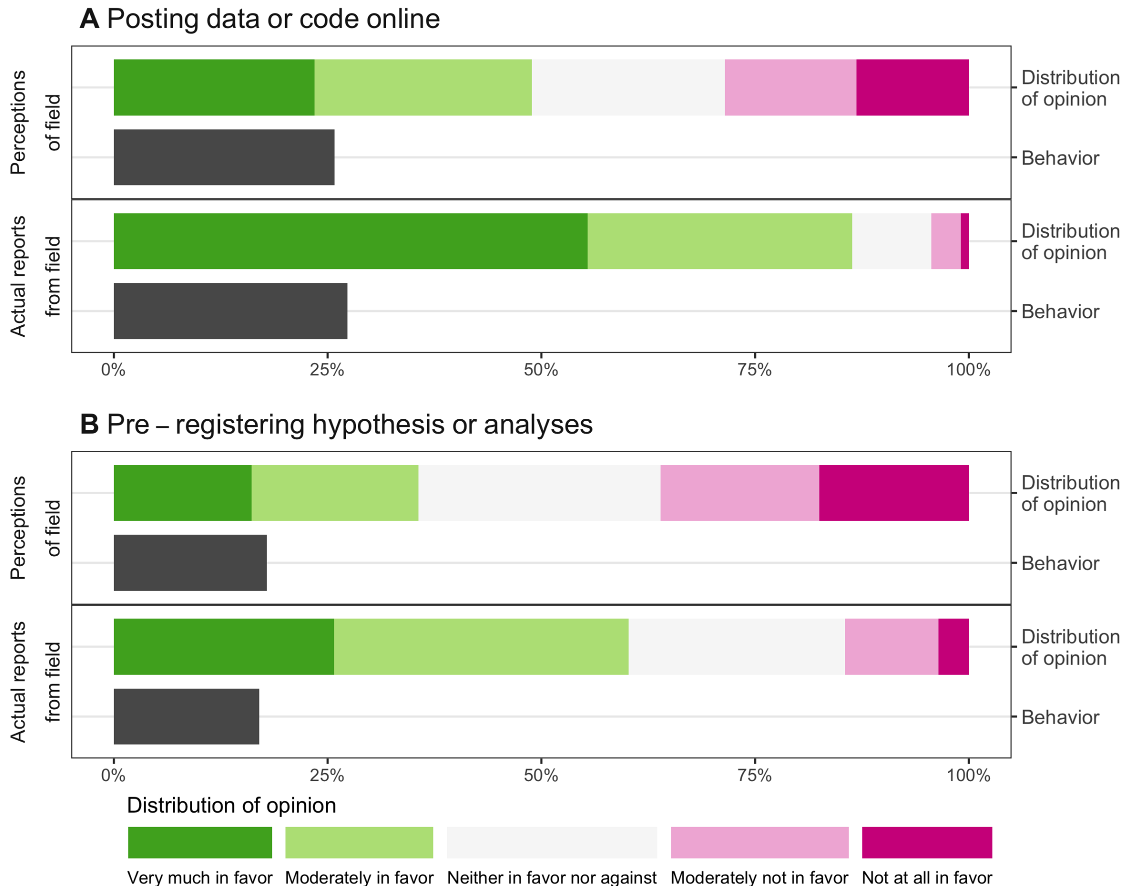

Figure C.10: Perceived and Actual Support for Open Science - Students

The chart shows differences between perceived and actual support for two practices: posting data or code online and pre-registering hypotheses or analyses. The sample is restricted to PhD Students. Within each panel, the first bar shows the perceived distribution of support for the practice among Students. This is constructed by asking individuals what percentage of researchers in their field they believe fall into each opinion category, and then averaging over their responses. The solid black bar below shows the fraction of researchers in their field they believe have done the practice. The third bar in the panel shows the distribution of support for the practice constructed using the responses elicited from students. The final solid black bar shows the proportion of students who have actually done the stated practices, using the responses elicited from our survey. Colors indicate the level of support, with green indicating more and red indicating less support.

Table C.9: Differences in Observables for those Completing and Not Completing Survey

Variable	Overall (1)	Respondent (2)	Nonrespondent (3)	Difference (2) - (3)
All				
Publication Count[1]	2.08	2.21	1.99	0.22 (4.24)***
USA and Canada	0.68	0.76	0.63	0.13 (7.64)***
N	2983	1181	1802	
Economics				
Publication Count	2.29	2.37	2.23	0.14 (1.28)
USA and Canada	0.65	0.72	0.61	0.11 (3.07)***
N	753	300	453	
Political Science				
Publication Count	2.38	2.45	2.31	0.14 (1.27)
USA and Canada	0.76	0.80	0.72	0.08 (2.56)**
N	763	407	356	
Psychology				
Publication Count	1.74	1.81	1.71	0.1 (0.96)
USA and Canada	0.59	0.72	0.54	0.18 (4.48)***
N	708	185	523	
Sociology				
Publication Count	1.89	1.98	1.84	0.14 (1.47)
USA and Canada	0.71	0.77	0.68	0.09 (2.83)***
N	759	289	470	

This table presents differences in means for the number of publications and geographic location of the university for published scholars who did and did not complete the survey. The third column shows differences in means and t-statistics in parentheses. * indicates significance at the 10% level, ** indicates significance at the 5% level and *** indicates significance at the 1% level.

[1] Publication Count is right winsorized.

Table C.10: Characteristics of those Completing Survey

	Completed Survey				
	All	Psychology	Economics	Political Science	Sociology
	(1)	(2)	(3)	(4)	(5)
USA and Canada	0.13***	0.14***	0.11***	0.10**	0.11***
	(0.02)	(0.03)	(0.04)	(0.04)	(0.04)
Publication Count (right winsorized)	0.02***	0.01	0.01	0.01	0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Constant	0.26***	0.17***	0.31***	0.43***	0.27***
	(0.02)	(0.03)	(0.04)	(0.04)	(0.04)
Observations	2,983	708	753	763	759

This table presents regression coefficients and standard errors from ordinary least squares regressions. The outcome variable in each regression is an indicator variable for whether the individual contacted completed the survey. The covariates are observable characteristics of the individual contacted. The sample is limited to Published Authors. * indicates significance at the 10% level, ** indicates significance at the 5% level and *** indicates significance at the 1% level.

Table C.11: Relationship between Past and Current Open Science Behavior

	Used in Last Paper:			
	Any practice	Posting data or code online	Posting study instruments	Pre-registering hypotheses or analyses
	(1)	(2)	(3)	(4)
Has done any practice ever	0.73*** (0.03)			
Has done posting data or code online		0.69*** (0.02)		
Has done posting study instruments			0.59*** (0.02)	
Has done pre-registering hypotheses or analyses				0.55*** (0.02)
Constant	0.01 (0.03)	0.01 (0.02)	0.003 (0.01)	0.002 (0.01)
Observations	1,182	1,182	1,182	1,182

This table presents regression coefficients and standard errors from ordinary least squares regressions. The outcome variable in each regression is an indicator variable for whether the individual conducted an open science behavior in their last paper. The covariates are indicator variables for whether the individual had ever undertaken such an open scienncee practice. The sample is limited to Published Authors. * indicates significance at the 10% level, ** indicates significance at the 5% level and *** indicates significance at the 1% level.

Table C.12: Differences in Broad Indices across Disciplines

	Personal support (no norms)		Norms		Overall (includes norms)		Trustworthiness of literature	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Economics	0.10^{***}	0.08^{***}	0.12^{***}	0.12^{***}	0.08^{***}	0.07^{***}	0.01	0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Political Science	0.13^{***}	0.12^{***}	0.11^{***}	0.10^{***}	0.10^{***}	0.09^{***}	-0.04^{***}	-0.04^{***}
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Psychology	0.14^{***}	0.15^{***}	0.09^{***}	0.09^{***}	0.10^{***}	0.10^{***}	-0.05^{***}	-0.04^{***}
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Years since started PhD		0.001^{*}		-0.001^{***}		0.0001		-0.0004
		(0.0004)		(0.0003)		(0.0003)		(0.0005)
Male		0.05^{***}		0.01^{**}		0.03^{***}		-0.02^{***}
		(0.01)		(0.005)		(0.005)		(0.01)
Tenured		0.03^{**}		0.01		0.02^{**}		0.06^{***}
		(0.01)		(0.01)		(0.01)		(0.01)
Leadership Position		-0.002		-0.002		-0.0002		0.01
		(0.01)		(0.005)		(0.005)		(0.01)
USA and Canada		-0.03^{***}		-0.01		-0.02^{**}		-0.03^{***}
		(0.01)		(0.01)		(0.01)		(0.01)
Constant	0.58^{***}	0.57^{***}	0.31^{***}	0.32^{***}	0.48^{***}	0.47^{***}	0.64^{***}	0.66^{***}
	(0.01)	(0.01)	(0.004)	(0.01)	(0.004)	(0.01)	(0.01)	(0.01)
Observations	2,707	2,663	2,703	2,660	2,707	2,663	2,703	2,660

This table presents regression coefficients and standard errors from ordinary least squares regressions. The outcome variable in each regression is one of the broad indices described in appendix table C.7. The covariates are indicator variables for the discipline of the respondent. In odd numbered specifications no other control variables are included. In even numbered specifications individual-level covariates are included. The omitted discipline in the regressions are Sociology Published Authors and PhD Students. Significance stars are indicated for standard errors computed using a multiple testing adjustment to address risk of false positives. In particular, we use the False Discovery Rate (FDR) adjustment in Benjamini et al. (2006) and discussed in Anderson (2008). * indicates significance at the 10% level, ** indicates significance at the 5% level and *** indicates significance at the 1% level.

Table C.13: Differences in Broad Indices by Author Type

	Personal support (no norms)		Norms		Overall (includes norms)		Trustworthiness of literature	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Published Author	0.08***	0.09***	0.002	0.02**	0.04***	0.05***	0.04***	0.02*
	(0.01)	(0.01)	(0.005)	(0.01)	(0.005)	(0.01)	(0.01)	(0.01)
Years since started PhD		−0.002***		−0.002***		−0.002***		−0.001
		(0.0005)		(0.0004)		(0.0004)		(0.001)
Male		0.05***		0.03***		0.03***		−0.02**
		(0.01)		(0.01)		(0.005)		(0.01)
Tenured		0.002		0.02		0.005		0.05***
		(0.01)		(0.01)		(0.01)		(0.01)
Leadership Position		0.002		−0.01		0.002		0.01
		(0.01)		(0.01)		(0.005)		(0.01)
USA and Canada		−0.005		−0.003		0.0004		−0.03
		(0.01)		(0.01)		(0.01)		(0.01)
Constant	0.64***	0.63***	0.39***	0.38***	0.53***	0.52***	0.60***	0.63***
	(0.004)	(0.01)	(0.003)	(0.01)	(0.003)	(0.01)	(0.004)	(0.01)
Observations	2,707	2,663	2,703	2,660	2,707	2,663	2,703	2,660

This table presents regression coefficients and standard errors from ordinary least squares regressions. The outcome variable in each regression is one of the broad indices described in appendix table C.7. The covariates are indicator variables for the whether the respondent has published in one of the journals in appendix tables C.2 through C.5. In odd numbered specifications no other control variables are included. In even numbered specifications individual-level covariates are included. The omitted discipline in the regressions are PhD Students. Significance stars are indicated for standard errors computed using a multiple testing adjustment to address risk of false positives. In particular, we use the False Discovery Rate (FDR) adjustment in Benjamini et al. (2006) and discussed in Anderson (2008). * indicates significance at the 10% level, ** indicates significance at the 5% level and *** indicates significance at the 1% level.

Table C.14: Differences in Broad Indices across Disciplines and Author type

	Personal support (no norms)		Norms		Overall (includes norms)		Trustworthiness of literature	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Published Author	0.06***	0.07***	−0.01	0.01	0.03***	0.04***	0.03**	0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Economics	0.08***	0.07***	0.12***	0.11***	0.08***	0.07***	−0.003	0.005
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Political Science	0.10***	0.10***	0.10***	0.10***	0.08***	0.08***	−0.05***	−0.05***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Psychology	0.15***	0.15***	0.09***	0.09***	0.11***	0.11***	−0.05***	−0.05***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Published Author:Economics	0.03	0.03	0.01	0.01	0.01	0.01	0.03	0.02
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Published Author:Political Science	0.05	0.04**	0.01	0.01	0.03	0.02*	0.01	0.01
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Published Author:Psychology	−0.03	−0.04	−0.01	−0.01	−0.02	−0.02	−0.001	0.004
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Years since started PhD		−0.001**		−0.001***		−0.001***		−0.001**
		(0.0005)		(0.0004)		(0.0003)		(0.001)
Male		0.05***		0.01**		0.02***		−0.02***
		(0.01)		(0.005)		(0.005)		(0.01)
Tenured		−0.004		0.004		−0.0001		0.05***
		(0.01)		(0.01)		(0.01)		(0.01)
Leadership Position		−0.0003		−0.002		0.002		0.01
		(0.01)		(0.01)		(0.005)		(0.01)
USA and Canada		−0.01		−0.002		−0.0002		−0.02
		(0.01)		(0.01)		(0.01)		(0.01)
Constant	0.56***	0.55***	0.31***	0.32***	0.46***	0.46***	0.62***	0.66***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	2,707	2,663	2,703	2,660	2,707	2,663	2,703	2,660

This table presents regression coefficients and standard errors from ordinary least squares regressions. The outcome variable in each regression is one of the broad indices described in appendix table C.7. The covariates are indicator variables for the discipline and author type of the respondent. In odd numbered specifications no other control variables are included. In even numbered specifications individual-level covariates are included. The omitted category in the regressions are Sociology PhD Students. Coefficients on disciplines not interacted with Published Authors are effects for PhD Students in these disciplines. Significance stars are indicated for standard errors computed using a multiple testing adjustment to address risk of false positives. In particular, we use the False Discovery Rate (FDR) adjustment in Benjamini et al. (2006) and discussed in Anderson (2008). * indicates significance at the 10% level, ** indicates significance at the 5% level and *** indicates significance at the 1% level.

Table C.15: Differences in Sub Indices across Disciplines

	Awareness		Attitude		Behavior		Descriptive Norms		Prescriptive Norms	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Economics	0.05***	0.03**	0.08***	0.07***	0.17***	0.13***	0.13***	0.13***	0.11***	0.11***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Political Science	0.12***	0.12***	0.06***	0.06***	0.22***	0.20***	0.14***	0.14***	0.08***	0.07***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Psychology	0.14***	0.15***	0.09***	0.09***	0.17***	0.20***	0.09***	0.09***	0.08***	0.08***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Years since started PhD		−0.001		−0.001**		0.004***		−0.001***		−0.001
		(0.001)		(0.0004)		(0.001)		(0.0004)		(0.0004)
Male		0.05***		0.02***		0.09***		0.001		0.02***
		(0.01)		(0.01)		(0.01)		(0.01)		(0.01)
Tenured		0.04**		−0.02**		0.06**		0.02**		−0.01
		(0.02)		(0.01)		(0.02)		(0.01)		(0.01)
Leadership Position		−0.01		−0.01		0.01		0.0001		−0.004
		(0.01)		(0.01)		(0.01)		(0.01)		(0.01)
USA and Canada		0.004		−0.03***		−0.07***		0.01		−0.02
		(0.02)		(0.01)		(0.02)		(0.01)		(0.01)
Constant	0.75***	0.73***	0.76***	0.80***	0.22***	0.19***	0.14***	0.14***	0.48***	0.50***
	(0.01)	(0.02)	(0.005)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	2,707	2,663	2,706	2,662	2,667	2,623	2,700	2,657	2,674	2,634

This table presents regression coefficients and standard errors from ordinary least squares regressions. The outcome variable in each regression is one of the sub indices described in appendix table C.7. The covariates are indicator variables for the discipline of the respondent. In odd numbered specifications no other control variables are included. In even numbered specifications individual-level covariates are included. The omitted discipline in the regressions are Sociology Published Authors and PhD Students. Significance stars are indicated for standard errors computed using a multiple testing adjustment to address risk of false positives. In particular, we use the False Discovery Rate (FDR) adjustment in Benjamini et al. (2006) and discussed in Anderson (2008). * indicates significance at the 10% level, ** indicates significance at the 5% level and *** indicates significance at the 1% level.

Table C.16: Differences in Sub Indices by Author Type

	Awareness		Attitude		Behavior		Descriptive Norms		Prescriptive Norms	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Published Author	0.04***	0.07***	−0.01	0.02**	0.20***	0.19***	0.001	0.01	0.004	0.02**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Years since started PhD		−0.003***		−0.002***		−0.001		−0.002***		−0.001***
		(0.001)		(0.0004)		(0.001)		(0.0005)		(0.0005)
Male		0.04***		0.02***		0.09***		0.02***		0.03***
		(0.01)		(0.01)		(0.01)		(0.01)		(0.01)
Tenured		0.02		−0.02**		0.01		0.03***		−0.01
		(0.02)		(0.01)		(0.02)		(0.01)		(0.01)
Leadership Position		−0.0001		−0.01		0.01		−0.004		−0.01
		(0.01)		(0.01)		(0.01)		(0.01)		(0.01)
USA and Canada		0.02		−0.03***		−0.01		0.01		−0.01
		(0.02)		(0.01)		(0.02)		(0.01)		(0.01)
Constant	0.81***	0.78***	0.82***	0.85***	0.28***	0.24***	0.23***	0.22***	0.54***	0.55***
	(0.01)	(0.02)	(0.003)	(0.01)	(0.01)	(0.02)	(0.004)	(0.01)	(0.004)	(0.01)
Observations	2,707	2,663	2,706	2,662	2,667	2,623	2,700	2,657	2,674	2,634

This table presents regression coefficients and standard errors from ordinary least squares regressions. The outcome variable in each regression is one of the sub indices described in appendix table C.7. The covariates are indicator variables for the whether the respondent has published in one of the journals in appendix tables C.2 through C.5. In odd numbered specifications no other control variables are included. In even numbered specifications individual-level covariates are included. The omitted discipline in the regressions are PhD Students. Significance stars are indicated for standard errors computed using a multiple testing adjustment to address risk of false positives. In particular, we use the False Discovery Rate (FDR) adjustment in Benjamini et al. (2006) and discussed in Anderson (2008). * indicates significance at the 10% level, ** indicates significance at the 5% level and *** indicates significance at the 1% level.

Table C.17: Differences in Sub Indices across Disciplines and Author type

	Awareness		Attitude		Behavior		Descriptive Norms		Prescriptive Norms	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Published Author	0.06***	0.08***	−0.002	0.02*	0.13***	0.13***	−0.01	0.003	−0.005	0.02
	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)
Economics	0.05***	0.04**	0.09***	0.08***	0.11***	0.09***	0.14***	0.14***	0.10***	0.10***
	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Political Science	0.12***	0.12***	0.05***	0.05***	0.14***	0.13***	0.13***	0.13***	0.08***	0.07***
	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Psychology	0.16***	0.17***	0.10***	0.10***	0.19***	0.20***	0.10***	0.10***	0.08***	0.08***
	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Published Author:Economics	−0.01	−0.01	−0.02	−0.02	0.13	0.11***	−0.01	−0.01	0.02	0.02
	(0.03)	(0.03)	(0.01)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
Published Author:Political Science	−0.01	−0.02	0.01	0.01	0.14	0.13***	0.02	0.02	0.001	−0.002
	(0.02)	(0.02)	(0.01)	(0.01)	(0.03)	(0.03)	(0.01)	(0.02)	(0.01)	(0.01)
Published Author:Psychology	−0.04	−0.05	−0.01	−0.02	−0.04	−0.05	−0.03	−0.03	0.004	−0.003
	(0.03)	(0.03)	(0.01)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
Years since started PhD		−0.002**		−0.001***		0.0001		−0.001**		−0.001**
		(0.001)		(0.0004)		(0.001)		(0.0005)		(0.0005)
Male		0.05***		0.02***		0.08***		0.0004		0.02***
		(0.01)		(0.01)		(0.01)		(0.01)		(0.01)
Tenured		0.02		−0.03**		−0.01		0.02		−0.02
		(0.02)		(0.01)		(0.02)		(0.01)		(0.01)
Leadership Position		−0.004		−0.01		0.01		0.001		−0.01
		(0.01)		(0.01)		(0.01)		(0.01)		(0.01)
USA and Canada		0.02		−0.03***		−0.01		0.004		−0.01
		(0.02)		(0.01)		(0.02)		(0.01)		(0.01)
Constant	0.73***	0.70***	0.76***	0.79***	0.17***	0.14***	0.14***	0.14***	0.48***	0.49***
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	2,707	2,663	2,706	2,662	2,667	2,623	2,700	2,657	2,674	2,634

This table presents regression coefficients and standard errors from ordinary least squares regressions. The outcome variable in each regression is one of the sub indices described in appendix table C.7. The covariates are indicator variables for the discipline and author type of the respondent. In odd numbered specifications no other control variables are included. In even numbered specifications individual-level covariates are included. The omitted category in the regressions are Sociology PhD Students. Coefficients on disciplines not interacted with Published Authors are effects for PhD Students in these disciplines. Significance stars are indicated for standard errors computed using a multiple testing adjustment to address risk of false positives. In particular, we use the False Discovery Rate (FDR) adjustment in Benjamini et al. (2006) and discussed in Anderson (2008). * indicates significance at the 10% level, ** indicates significance at the 5% level and *** indicates significance at the 1% level.

Table C.18: Differences in Practice Indices across Disciplines

	Posting data or code online		Posting study instruments		Pre-registering hypotheses or analyses	
	(1)	(2)	(3)	(4)	(5)	(6)
Economics	0.17***	0.16***	−0.01	−0.02*	0.07***	0.07***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Political Science	0.16***	0.15***	0.06***	0.05***	0.08***	0.08***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Psychology	0.06***	0.07***	0.08***	0.09***	0.16***	0.15***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Years since started PhD		0.0001		0.002***		−0.002***
		(0.0003)		(0.001)		(0.0004)
Male		0.04***		0.03***		0.01**
		(0.01)		(0.01)		(0.01)
Tenured		0.03***		0.02		−0.01
		(0.01)		(0.01)		(0.01)
Leadership Position		−0.005		0.003		0.001
		(0.01)		(0.01)		(0.01)
USA and Canada		−0.01		−0.02		−0.01
		(0.01)		(0.01)		(0.01)
Constant	0.45***	0.44***	0.65***	0.63***	0.33***	0.36***
	(0.005)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Observations	2,707	2,663	2,707	2,663	2,707	2,663

This table presents regression coefficients and standard errors from ordinary least squares regressions. The outcome variable in each regression is one of the sub indices described in appendix table C.7. The covariates are indicator variables for the discipline of the respondent. In odd numbered specifications no other control variables are included. In even numbered specifications individual-level covariates are included. The omitted discipline in the regressions are Sociology Published Authors and PhD Students. Significance stars are indicated for standard errors computed using a multiple testing adjustment to address risk of false positives. In particular, we use the False Discovery Rate (FDR) adjustment in Benjamini et al. (2006) and discussed in Anderson (2008). * indicates significance at the 10% level, ** indicates significance at the 5% level and *** indicates significance at the 1% level.

Table C.19: Differences in Practice Indices by Author Type

	Posting data or code online		Posting study instruments		Pre-registering hypotheses or analyses	
	(1)	(2)	(3)	(4)	(5)	(6)
Published Author	0.06***	0.06***	0.08***	0.08***	−0.02***	0.02**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Years since started PhD		−0.002***		−0.0004		−0.003***
		(0.0004)		(0.001)		(0.0005)
Male		0.07***		0.01		0.01
		(0.01)		(0.01)		(0.01)
Tenured		0.04***		−0.01		−0.02
		(0.01)		(0.01)		(0.01)
Leadership Position		−0.01		0.01		0.004
		(0.01)		(0.01)		(0.01)
USA and Canada		0.01		0.004		−0.01
		(0.01)		(0.01)		(0.01)
Constant	0.52***	0.49***	0.65***	0.64***	0.42***	0.43***
	(0.004)	(0.01)	(0.01)	(0.02)	(0.004)	(0.01)
Observations	2,707	2,663	2,707	2,663	2,707	2,663

This table presents regression coefficients and standard errors from ordinary least squares regressions. The outcome variable in each regression is one of the sub indices described in appendix table C.7. The covariates are indicator variables for the whether the respondent has published in one of the journals in appendix tables C.2 through C.5. In odd numbered specifications no other control variables are included. In even numbered specifications individual-level covariates are included. The omitted discipline in the regressions are PhD Students. Significance stars are indicated for standard errors computed using a multiple testing adjustment to address risk of false positives. In particular, we use the False Discovery Rate (FDR) adjustment in Benjamini et al. (2006) and discussed in Anderson (2008). * indicates significance at the 10% level, ** indicates significance at the 5% level and *** indicates significance at the 1% level.

Table C.20: Differences in Practice Indices across Disciplines and Author type

	Posting data or code online		Posting study instruments		Pre-registering hypotheses or analyses	
	(1)	(2)	(3)	(4)	(5)	(6)
Published Author	0.02**	0.03**	0.08***	0.08***	−0.01	0.02*
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
Economics	0.15***	0.14***	−0.01	−0.01	0.08***	0.08***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Political Science	0.13***	0.12***	0.04***	0.04***	0.09***	0.09***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Psychology	0.08***	0.08***	0.09***	0.09***	0.15***	0.15***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Published Author:Economics	0.04	0.04***	−0.01	−0.02	−0.01	−0.01
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Published Author:Political Science	0.07	0.06***	0.02	0.02	−0.01	−0.01
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
Published Author:Psychology	−0.03	−0.03*	−0.03	−0.03	0.01	−0.0001
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Years since started PhD		−0.001**		0.0001		−0.002***
		(0.0004)		(0.001)		(0.0004)
Male		0.04***		0.02***		0.01**
		(0.005)		(0.01)		(0.01)
Tenured		0.01		−0.002		−0.01
		(0.01)		(0.01)		(0.01)
Leadership Position		−0.005		0.01		0.002
		(0.01)		(0.01)		(0.01)
USA and Canada		0.002		0.001		−0.004
		(0.01)		(0.01)		(0.01)
Constant	0.44***	0.43***	0.62***	0.60***	0.34***	0.35***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Observations	2,707	2,663	2,707	2,663	2,707	2,663

The outcome variable in each regression is one of the sub indices described in appendix table C.7. The covariates are indicator variables for the discipline and author type of the respondent. The omitted category in the regressions are Sociology PhD Students. Coefficients on disciplines not interacted with Published Authors are effects for PhD Students in these disciplines. Significance stars are indicated for standard errors computed using a multiple testing adjustment to address risk of false positives. * indicates significance at the 10% level, ** indicates significance at the 5% level and *** indicates significance at the 1% level.

C.2 Online Materials

This project's OSF page
The survey conducted, uploaded to OSF
The link to the Pre-Analysis Plan, uploaded to OSF