The New Empirics of Industrial Policy[1]

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ABSTRACT: Nations have and will continue to shape their economies through industrial policy. Nevertheless, the empirical literature on these interventions is thin, dwarfed by the attention industrial policies receive from policymakers across the world. In this paper, I discuss the difficulties of empirically studying industrial policy, and review how new econometric work is confronting these issues. Through careful research design and attention to institutional detail, I argue that emergent studies are rapidly expanding what we know—and updating what we thought we knew—about these policies. As well, I argue tools from policy evaluation allow us to study the impact of endogenous industrial interventions. This review is a proposal to take industrial policy, along with their complexities, more seriously as objects of inquiry. Doing so requires more serious evaluations of past policy, but also a reevaluation of past empirical work and consensus.

1. INTRODUCTION

Industrial policies are everywhere. From Germany's Industrie 4.0 to Strategic Priorities of Digital Bangladesh, national industrial strategies are omnipresent across the world. Though for decades industrial policies have been central to debates about the role of the state in economic development, the empirical literature on these policies has remained

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underdeveloped. Until recently, econometric evaluations of these interventions were exceedingly rare.[3]

This paper considers the new empirical research examining industrial policy. By using contemporary econometric methods and utilizing natural experiments, I show these studies have been swiftly expanding what we know—and updating what we thought we knew—about these policies.

To be clear: by *industrial policy*, I mean intentional political action meant to shift the industrial structure of an economy. Often policymakers hope this shift will be more favorable for growth, relative to what would have happened had the economy evolved according to its static comparative advantage (Chang 2003; Noland and Pack 2003; Lane 2019). Of course, there are many iterations of this definition, but most embed a causal argument: the idea that there is a better outcome that would not have occurred in the absence of the intervention. While it seems this would be ripe territory for causal inference, this has not been the case until recently.

The goal of this paper is to assess the emerging body of work on industrial interventions and to speak to the potential of evaluating these policies more broadly.

Doing so entails looking backward. I do so not only because economists have long debated the historical importance of these policies.[4] Instead, new studies of industrial policy have illustrated the value of using rich retrospective cases, new data, and institutional details to improve our understanding of these interventions. Moreover, by looking at the past body of empirical research, we can refine our understanding of these controversial policies. By critically examining this work, I hope to point out misconceptions that have persisted—and how contemporary econometric methods can and do address these deficiencies.

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[3] Ann Harrison and Andres Rodriguez-Clare (2010) best summarize the situation: "One challenge that we face in evaluating the empirical literature is the large gap between the theoretical justification for IP and the quantitative work that has been done to evaluate its 'success'" (p.4041-4042).

[4] Frank Taussig's (1914) important discussion of the role of tariffs and development of U.S. industry spawned a century of scholarship on the development of the American economy (Irwin 2017). Historians Paul Bairoch (1972) and Sidney Pollard (1981) argued that protections played an essential role in early European growth, inciting a long literature on the "tariff-growth paradox" (see O'Rourke 2000, Jacks 2006). Alice Amsden's (1992) and Robert Wade's (1990) comparative studies of East Asian industrial policy, and the sizeable critical literature exploring their arguments, were extensively synthesized by Noland and Pack (2003).

In light of early industrial policy studies, we often hear the refrain: "The evidence is mixed." An implication of this review is that we should hope for more mixed evidence. In evaluating new empirical work on industrial policy, and earlier high-quality case studies, my argument is that the evidence surrounding industrial policy will likely be nuanced and complicate the conventional wisdom. Precisely because industrial policy is complex and multidimensional, deployed across different economies and with different objectives, useful evidence will often be, in a sense, mixed.

Justifications for industrial policies introduce their own complications. Benefits and costs may be hard to fully capture in straightforward empirical exercises, much less distill into a single discrete punchline. Interventions may rely on hopes of long-run—or dynamic—benefits (promoting infant industries, accelerating technological change, encouraging agglomeration). These benefits often justify the short-run—or static—costs of interventions (Greenwald and Stiglitz 2006). More philosophically, the ambitious and abstract objectives of planners complicate our quantitative machinery. Generally speaking, either side of the cost-benefit ledger is difficult to fully capture with our current toolset.[5]

That said, past empirical evidence is not in fact mixed. It is often vacuous. Section 2 argues that aspects of earlier cross-country and industry-level regression studies have, indeed, created conceptual confusion in how we empirically think about industrial interventions. I use this section (2) to review the shortcomings of this literature, juxtaposing prior studies against new evidence from modern empirical analysis. First, I focus on conceptual confusion surrounding early evidence, namely, issues in interpreting regression studies, and issues related to the endogeneity of policy. Second, I show first-generation aggregate studies do not adequately articulate, contextualize, or even clearly measure many interventions. Third, early work has been mostly critical of the extent to which industrial interventions targeted spillovers—a principal motive for industrial interventions. In light of recent evidence, I argue that

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[5] Some goals of industrial interventions—such as strategic aims or national prestige—do not factor into our cost-benefit analysis. Static welfare costs may not have dissuaded General Park Chung Hee from building a national domestic military-industrial complex in the midst of a geopolitical crisis (Lane 2019). Similarly, strategic defense concerns may be a core motive behind China's shipbuilding industrial policy (Kalouptsidi 2018).

these tests may be incomplete, and may not adequately speak to the role of spillovers in industrial policymaking.

Nonetheless, empirical evaluation of industrial policy is notoriously challenging. Not only is randomization unlikely, by construction industrial policies are meant to promote special industries, products, and places. These endogenous interventions seem awkward in the world of randomized policy evaluations. I argue that these issues are not insurmountable. While many studies of industrial policy argue counterfactual analysis of industrial policy is impossible, Section 3 argues otherwise. Here, I discuss how the language of contemporary causal methods can accommodate the context of these interventions. I illustrate this by considering related work on place-based policy, which is actively confronting issues of endogenous policy and selection in policy evaluation (Neumark and Simpson 2015). Recent studies of place-based industrial policies in the U.K. (Criscuolo et al. 2019) and across the EU (Becker, Egger, and von Ehrlich 2010; Bernini and Pellegrini 2011; Cerqua and Pellegrini 2014) have used localized random variation to estimate the impact of targeted policy. A related wave of recent studies on industrial R&D incentives, similarly, show the promise of using rankings, thresholds, and similar discontinuities for causal estimation (Dechezleprêtre et al. 2016; Howell 2017).

Section 3 also argues that within-country studies of industrial interventions can answer valuable questions about national policies, even when they do not aggregate impacts. I argue that, much like microeconometric studies of trade policy and natural experiments in macroeconomics, more microeconometric work can deliver valuable insights into the proposed mechanisms and workings of policies. With little systematic empirical evidence as to the workings of industrial policy, this work is valuable.

The possibilities of microeconometric studies are detailed in Section 4. This final section shows how new case studies are confronting these issues through better research design, especially by combining natural experiments and new within-country data.

Merging newly digitized panel data with quasi-experimental design allows Harris, Keay, and Lewis (2015), Giorcelli (2019), Hanlon (2018), Juhasz (2018), Lane (2019), and Mitrunen (2019) to trace the medium- and long-run consequences of policy on industrial development. Relatedly, Aghion, Dewatripont, Du, Harrison, and Legros (2015), Martin, Nataraj, and Harrison (2017), Rotemberg (2017), and Criscuolo et al. (2019) estimate the impact of contemporary interventions using careful within-country policy variation with rich microdata. With rich network data, Blonigen (2016), Lane

(2019), Liu (2018), and Rotemberg (2017) consider the transmission of policy through linkages. Hanlon (2018) and Mitrunen (2019) show the potential of investigating other types of externalities: learning-by-doing and human capital spillovers from interventions. While it is hard to evaluate the aggregate implications of these interventions, new studies of industrial policies are already providing new insights.[6] Relatedly, quantitative work by Liu (2018) illustrates new avenues of evaluating the aggregate general equilibrium consequences of sectoral targeting.

Before I launch into this study, I want to make some things clear. First, when I discuss industrial policy, I am mainly talking about infant industry policies aimed at promoting the development of new economic activity. However, when necessary, I discuss policies aimed at aiding lagging industries. Although this discussion centers on industries and firms, many of these statements will likely apply to geographies as well—as in the case of place-based policy.

Second, throughout this study, I am mostly considering the case of industrial promotion in a developing country setting. However, especially in the case of sunset and terminal policies, this work also applies to mature, high-income countries.

Third, I largely discuss reduced-form studies: approaches where economists are trying to recover statistical relationships between variables, often using quasi-experimental variation to identify causal relationships between an outcome and an explanatory variable. I do not focus, however, on structural econometric studies—studies that estimate parameters directly using theoretical models. Nevertheless, I discuss valuable structural work from the field of industrial organization. Broadly, the contours of my argument map to such structural work, and both flavours of empirical work should be seen as complementary.

Finally, this review does not advocate industrial policy and targeted interventions—it is a review advocating better evidence. By interrogating earlier critical work on industrial policy, I am not arguing for a favorable interpretation of interventions. Instead, I am arguing for more precise studies to improve our understanding of these policies.

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[6]Rotemberg (2017), discussed below, presents interesting ways of considering the general equilibrium effects of industrial policy using within-country variation.

This study is organized as follows: Before considering new empirical tools and studies of industrial policy, Section 2 interrogates earlier studies of industrial policies. Section 3 clarifies some of the residual issues from past empirical work, specifying the possibility of microeconometric evaluation of industrial policy. I do so by focusing on contributions from place-based policy evaluation. Finally, Section 4 reviews new empirical research on industrial policy, describing the array of historic case studies tackling industrial policy issues. I argue this literature is rapidly updating our understanding of these policies. Section 5 closes this study, and describes both takeaways and shortcomings of the current literature. As well, I use this section to discuss future directions of research.

2. PAST AS PROLOGUE

In this section, I consider past evidence of industrial policy. I do so to discuss points of confusion and to highlight how contemporary empirical research can address these issues.

Starting with Krueger and Tuncer's 1982 test of Turkish infant industry policy, this early critical literature formed the foundation of how we understand industrial interventions in developing countries. While many conceptual arguments within this literature are still incisive, I argue that aspects of first-generation empirical work do not provide clear evidence of industrial policy's impact.[7] In some cases, it may muddle our understanding of these interventions. In reviewing these issues, I wish to highlight ways in which more precise empirical studies can provide more fruitful insights into the workings of these policies.

Misadventures in counterfactual land.

"The counterfactual" is a principal source of confusion in industrial policy studies. That is, how do we observe what would have happened to an economy (industry) in the absence of an industrial intervention? For empirical industrial policy studies

[7] For example, Bela Balassa (1978), after reviewing cross-country evidence on industry interventions, provides exemplary critiques against sector specific targeting, in favor of more "soft" infant industry measures.

quantitative and qualitative—the line is ubiquitous.[8] Many studies suggest counterfactual analysis of industrial policy is, in fact, impossible.[9]

The promise of contemporary econometric tools and the policy evaluation revolution is that we have frameworks that discipline the way we consider and construct counterfactuals.[10] Even while it may be hard to construct aggregate counterfactuals, the right tools, questions, and research designs do not preclude rich cross-country comparisons using coherent, data-driven counterfactuals (as in the case of reduced-form analysis by Abadie, Diamond, and Hainmueller [2015] and Acemoglu, Robinson, Naidu, and Restrepo [2018]).

Nevertheless, confusion permeates the discussion of industrial policy empirics—both in the potential of causal inference and the interpretation of first-generation evidence.

The malaise around industrial policy is not helped by the mass of industry- and country-level studies that shape our understanding of these interventions.[11] Many of these regression studies are surveyed in reviews by Noland and Pack (2003) and Pack and Saggi (2006), and more recently in a detailed analysis by Harrison and Rodriguez-Clare (2010).[12] Most of these early regression studies do not allow for a causal interpretation. Many are afflicted by the panoply of well-established issues cataloged by

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[8] Larry Westphal, World Bank economist during South Korea's industrialization, argues that "lack of agreement about the required counterfactual" has fueled the debate about East Asian industrial policy (Westphal 1990, p.48). In their critical review of industrial policy evidence, Pack and Saggi (2006) reiterate that "it is impossible to offer a single agreed counterfactual to evaluate the success of industrial policy targeted to individual industries" (p.283).

[9] With respect to Japanese industrial policy, Gilpin (2011) writes: "There is considerable evidence on both sides of this debate, but the outcome remains inconclusive because there is no counterfactual…" (p.162).

[10] This point applies to structural and reduced-form approaches alike.

[11] Early aggregate regression studies of interventions include Krueger and Tuncer (1982), Harrison (1994), Beason and Weinstein (1996), Lee (1996), El-Agraa (1997), Pack (2000), and Lawrence and Weinstein (2001).

[12] Westphal (1981) and Grossman (1990) provide excellent early reviews of case studies and simulation-based studies.

the growth econometrics literature (Durlauf, Johnson, and Temple 2005).[13] Of these, criticisms of trade policy regressions are applicable to early industrial policy studies; endogenous policies and reverse causality concerns are first-order (Rodriguez and Rodrik 2000), and fatally undermine the causal interpretation of results (Billmeier and Nannicini 2009).[14]

Criticising the endogeneity of growth empirics may be redundant, but a source of endogeneity is worth highlighting. Rodrik (2012) articulates why industrial policy regressions are particularly challenging to interpret. Consider two worlds. In one world, interventions are motivated by welfare-maximising politicians, where interventions respond to market imperfections. In another world, interventions are governed by economically harmful, rent-seeking politicians. In both cases, interventions are negatively correlated with growth; and these negative relationships tell us little about the nature of the intervention. This is because industrial interventions are often driven by unobservable forces, such as state capacity, the scope of market imperfections, and the welfare objectives of politicians. Using a stylized conceptual framework, Rodrik shows that these unobserved forces confound the relationship between economic performance and policy interventions.[15]

Estimates from endogenous growth regressions are, nonetheless, perpetually over-interpreted—by the authors of regression studies and, more importantly, in contemporary critical reviews of industrial policy empirics.

Lawrence and Weinstein's (2001) industry-level study of Japanese protectionism and industry development (1964–73) uses fixed effect regressions to relate industrial TFP growth to measures of trade performance and protection. Among their findings, they suggest that import protection might hurt productivity growth while imports enhanced productivity. In their critical review of industrial policy, Pack and Saggi

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[13] An exhaustive treatment of related issues, model uncertainty, mismeasurement, heterogeneity, and more is provided by Durlauf et al. (2005).

[14] Rodriguez and Rodrik (2000) consider the panoply of issues confounding studies of trade policy (specifically, openness); using the contemporary language of causal inference, Billmeier and Nannicini (2009) show how these issues undermine the causal estimation.

[15] Furthermore, under certain conditions, these negative relationships are more common in cross-country settings.

(2006) are explicit, stating that the Lawrence-Weinstein findings "conclude that Japan's growth would have been even faster if it had cut tariffs and exposed a greater share of its domestic producers to foreign competition" (p.284). Lawrence and Weinstein's results illuminate a compelling pattern, one that may be consistent with contemporary microstudies of trade policy—but they do not tell us about the aggregate performance of counterfactual postwar Japan.

Some influential evaluations of industrial policy attempt causal analysis through ad hoc counterfactual comparisons (for example Yoo [1990] and Pack [2000]). Relative to contemporary model-based or data-driven exercises, here the researchers have unique latitude in constructing their counterfactual scenarios.

For example, Pack (2000) evaluates the impact of Japanese and South Korean industrial policy on aggregate growth (1960–79 and 1966–85, respectively) with a counterfactual growth accounting exercise. The process entails calculating the effects of industrial policy on TFP growth for the manufacturing sector in the East Asian economies, then comparing them to what TFP growth would have been in the absence of industrial policy. The steps for constructing this counterfactual world are incredibly arbitrary, however. Among other things, they entail 1) constructing counterfactual sectoral weightings (in this case, constructed from a set of countries with similar end-period GDP per capita); 2) choosing counterfactual TFP growth rates; and 3) deciding how these ingredients are combined into a coherent counterfactual. In the end, it is unclear whether this counterfactual reflects Japan or South Korea in the absence of treatment.[16]

Some early single-industry case studies offer more precise analysis of infant industry interventions. The studies are exemplified by Baldwin and Krugman (1988a, 1988b), Head (1994), Irwin (2000a, 2000b) and others.[17] Though these structural or calibration-based studies rely on stringent modeling assumptions for their counterfactual analysis, they are much more precise about the context and terms of their study. One particular aspect of these studies will be unsatisfying to economists: They often do not

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[16] As I discuss below, it is unclear what the specific treatment is.

[17] Many earlier studies of this flavor are reviewed by Grossman (1990). Other empirical examples include Baldwin and Krugman's (1988a) analysis of industrial policies surrounding Airbus and Baldwin and Flam's (1989) study of the Canadian airplane protection and its effects on Swedish and Brazilian markets. Irwin (2000a) studies the potential impact of 19th century steel industry protection.

provide a single message on industrial policy. Their evidence is mixed (Harrison and Rodriguez-Clare 2010). For example, Baldwin and Krugman (1988b), using a calibrated model, suggest both that Japanese interventions in the 16K memory market were crucial for developing the industry, and that they may have been detrimental to consumers. Irwin (2000b) studies McKinley-era protection of the U.S. tinplate industry. Using a calibration-simulation exercise, he also concludes that tariffs were helpful in advancing the development of the sector, but with high welfare costs to consumers.

Of these, Head (1994) uses a more structural approach to study the impact of infant industry protection (tariffs) of the U.S. steel rail industry. In particular, his framework specifically accounts for learning-by-doing effects. Head argues that interventions in the 19th century U.S. steel rail sector make a particularly compelling case study. First, the industry was initially disadvantaged, but emerged as a competitive industry by the early 20th century. Second, the protection of the industry was truly temporary. Head's analysis shows that protection was a critical factor in the long-run growth and price reductions in the industry, but that the welfare impacts were likely marginal.

Nonetheless, studies like Head (1994) and others indicate the value of carefully studying granular cases. Many of these studies rely on stringent assumptions for counterfactual exercises, especially earlier calibration studies. Furthermore, in these stylistic settings, empirical analysis often does not capture the potential long-run benefits of the policy. These studies are nonetheless transparent about policy context and institutional environment—traits I suggest are essential ingredients in future studies. Interestingly, Grossman's (1990) review of these early model-simulation studies of industrial policy could apply to the cases discussed in Section 4: "[From] the limited empirical evidence available to date, it appears that the strongest case for government intervention may arise in the early stages of development of a new, technologically-innovative product" when firms may not internalize the potential benefits to consumers and other firms in the industry (p.199).

Bringing policy back into industrial policy.

In many early regression studies of industrial policy, it is hard to find clearly articulated policies.

To explore the effectiveness of interventions, earlier studies usually explore a correlation between interventions and industrial development—across industries and countries (including Krueger and Tuncer 1982; Harrison 1994; Lee 1996; Lawrence and Weinstein 2001).[18]

The consensus has been that this body of work casts doubt on industrial interventions (Noland and Pack 2003; Harrison and Rodriguez-Clare 2010). Yet in the absence of research design or context, it is hard to map empirics to specific policy interventions or policy lessons. Do many studies of industrial policy, in fact, study purposeful industrial promotion interventions? Many of these studies cannot precisely reveal the in/effectiveness of industrial policy.

The coarseness of older empirical studies seldom speaks to a precise policy episode. This measure may be a blob of interventions (indices of protection, aggregations of various tariff or non-tariff measures, and more) tracked over a span of time. Such aggregation blinkers us to the potential of evaluating policy and does not show *which* industrial policies work. While this research investigates significant periods of interventionism, they seldom correspond to specific policy episodes. They may lump together both periods of liberalization and *dirigisme*, or may come completely after the most intensive periods of intervention.[19]

Consider Lee's (1996) regression study of government interventions and industrial development—a study cited as evidence against South Korean industrial policy (Noland and Pack 2003, Pack and Saggi 2006). Lee examines the relationship between productivity and measures of protection, following 38 sectors for five periods between 1963 and 1983. With disaggregated policy data, he estimates the relationship between (endogenous) policies and TFP growth and capital accumulation. He estimates the average relationship for all five periods. From 1963 through 1983, however, South Korea pursued markedly different interventions. From 1963 to 1973, Korea pursued a general (sector-agnostic) export-led drive through a jungle of incentive schemes. In 1973–79,

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[18]In a well-known comment on Krueger and Tuncer (1982), Harrison (1994) highlights some conceptual and statistical issues with their approach. In fact, her reanalysis overturns the results of the original 1982 analysis.

[19] Studies of both South Korean and Japanese protection may treat "protectionist periods" as one homogenous period. However, liberalization of Japanese trade policy began in the 1960s, and in South Korea in the 1980s (though there had been a steady decline in restrictions prior).

they pursued an intense sector-specific Heavy Chemical and Industry (HCI) big push, notably through the expansion of subsidized credit (Lane 2019). Starting in the late 1970s, the country pursued an economy-wide restructuring, marked by the rationalization of earlier interventions. Holding constant issues of endogeneity, regression studies like Lee (1996) do not differentiate drastically distinct policy efforts (distinct pushes, goals, and five-year plans).

Different forms of industrial policy have different goals. Many cross-industry studies cannot—and do not—distinguish between different types of industrial policies; for instance, policies aimed at promoting new (sunrise) industries versus aiding ailing (sunset) industries. It is far more common to observe policies allocated to declining industries (Baldwin and Nicoud 2007; Harrison and Rodriguez-Clare 2010). It may not be surprising to find negative correlations between interventions and industry development (as seen in Beason and Weinstein [1996], Lee [1996], and many studies).

With this in mind, endogenously targeted sunset and sunrise policies introduce their own distinct econometric issues.

Studies are also not clear as to the political context of the policies they explore. Studies that rely on imprecise measures of protectionism fail to consider whether they are studying industrial interventions at all. Precisely because industrial policy is state action, its scope, form, and efficacy are shaped by political forces. Political forces may undermine the extent that measures of industrial policy, especially protectionism, reflect industrial promotion versus political economy forces behind policy (rent seeking, lobbying, and international retaliations).

Very early evaluations of infant industry policies recognized that industrial policy analysis is confounded by politics (Taussig 1914, Grubel 1966).[20] A regression relationship between growth and endogenous policy cannot distinguish between the impact of industrial policy and unobserved political forces behind the policy—as described above by Rodrik (2012). Without context, it is difficult to ascertain whether studies are testing the technical aspects of industrial policy *per se* versus the interaction of these policies with broader political forces. While the political implications and

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[20] In an excellent early review of infant industry policy, Herbert Grubel (1966) describes the complications faced by Frank Taussig's early studies of U.S. interventions; Taussig found "tariffs once imposed never seem to get removed, thus making it impossible to observe whether protection has been successful or not" (p. 339).

feasibility of industrial policy is first order, these may be distinct questions from whether an industrial policy, when implemented properly, works according to theory. For this reason, Juhasz's (2018) natural experiment is helpful; she uses an exogenous event that mimics the effect of a temporary infant industry policy (the great Napoleonic Blockade) on French textile industry. By doing so, she is able to unpack the workings of an idealized intervention without the political and institutional issues that usually confound analysis.

Furthermore, technocratic policies may not be as salient. Broad industry- and country-level measures of observed protection miss many non-tariff measures. Theoretically, non-tariff measures, such as subsidies and quantitative restrictions, may be the preferred tools of social planners (Melitz 2005, Ederington and McCalman 2013). Observability of such barriers and the lack of complete cross-country data is the bane of contemporary trade research (Anderson and Wincoop 2004, Goldberg and Pavcnik 2016).[21]

Accordingly, relying only on observable measures of protection may give a distorted view of industrial policy. Measures based merely on the usual suspects (tariffs) would miss the many more obscure bureaucratic measures used across postwar East Asia. For example, Lane (2019) shows during the heavy interventionist period of 1970s South Korea, tariffs and quantitative restrictions were actually falling, as the state relied heavily on other levers such as subsidized loans.

Today, many industrial policies take the form of non-tariff barriers and are not readily observed. Measuring the extent of these policies—past or present—will entail more granular evidence and deeper institutional context than the older generation of empirical work. Even in the context of contemporary international trade law, industrial policies are still ubiquitous. The scope and extent of unobserved industrial subsidies are a central object of international disputes. Recent examples from industrial organization illustrate how combining theoretical machinery with institutional detail can help us

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[21] Anderson and Wincoop (2004): "The grossly incomplete and incomplete and inaccurate information on policy barriers available to researchers is a scandal and a puzzle" (p.693).

understand the impact of even latent industrial policy. With this in mind, Kalouptsidi (2018) tackles the challenging case of targeted subsidies to shipbuilding in China.[22]

China's 11th five-year plan (2006–10) promoted shipbuilding as a strategic industry. By developing a detailed structural model of the shipbuilding industry, Kalouptsidi (2018) investigates the effects of government subsidies on the ascendant sector. Her empirical framework allows her to forensically detect the scope of subsidies during the five-year plan and quantify their impact. The analysis suggests that industrial policy subsidies significantly reallocated global production towards China: in the absence of the targeted subsidies, China's global market share in ships would be halved, with Japan reaping an even larger world market share. Though policies expanded the Chinese shipbuilding sector, this expansion only marginally decreased domestic freight costs.

Spillovers and externalities.

Marshallian externalities have long been a key motivation for industrial policy interventions. Early empirical work questioned the extent to which industrial policies generated benefits to external firms or industries, and questioned the extent to which externalities guided canonical industrial interventions. For example, Pack (2000) and Noland (2004) conclude industries treated by Korean industrial policy had few (input-output) linkages with non-favored sectors, and Pack (2000) argues that policy did not promote inter-industry spillovers. An important early work by Beason and Weinstein (1996) on postwar Japan confronts the degree to which policy targeted another source of spillovers: increasing returns to scale.

Studies like Beason and Weinstein (1996), Pack (2000), and Noland (2004) assume that observable statistics reflect the extent to which spillovers shape the sectoral allocation of industrial policies. While they may test whether scale-economies play a role, they may not reflect other targeting criteria.[23] As Rodrik (2012) argues: it is hard

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[22] Also see Irwin and Pavcnik (2004), who used a structural framework to explore the impact of subsidy reforms on the international airplane market when subsidies themselves are hard to observe.

[23] Also see Spencer (1986), who considers the question "What Should Trade Policy Target?" through the lens of new trade models.

to infer the intention of policy by economic performance on endogenous policy when unobserved forces shape the scope of policy.

Recall Beason and Weinstein (1996), who show evidence that Japanese industrial policy may not have targeted sectors with increasing returns to scale—which they measure from aggregate sectoral data.[24] Do these measures adequately capture the intentions of policymakers? Pons-Benaiges (2017) revisits this study, collecting Japanese firm-level data and previously unobserved measures of industrial policy data (1964–83). The former allows him, crucially, to estimate *both* returns to scale (static externalities) and learning-by-doing (dynamic externalities). Indeed he shows that sectors with increasing returns to scale and those with learning-by-doing externalities are often different. In other words, static externalities may not capture the full picture. Interestingly, he shows that Japanese policy indeed targeted industries with increasing returns to scale and industries with weak (as opposed to strong) learning-by-doing.

Consider a (perhaps rare) world where policymakers target sectors to maximize welfare. Would simple input-output connections, such as those considered in the work of Pack (2000) and Noland (2004), reveal the extent to which optimal policies targeted network externalities? Deeper notions of bottlenecks and network structure could inform interventions.

Liu (2018) posits a sufficient statistic for guiding industry-specific interventions. Harnessing a general equilibrium growth model, characterized by a rich input-output network and incomplete markets, he develops a "distortion centrality" measure for guiding optimal policy. Using contemporary Chinese input-output data, along with 1970s data from South Korea's HCI big push episode (from Lane 2019), he shows that distortion centrality predicts industry-specific interventions in both settings. His study shows that in an economy populated by market imperfections that propagate through the input-output network, interventions may be growth-enhancing if targeted to relevant sectors. Seemingly controversial choices might reflect this. Thus, a naive correlational analysis between industry interventions and simple input-output connection may not reveal the externalities guiding the planning process.

While Pack (2000) and Noland (2004) critically examine the role of externalities in South Korea's HCI drive, they do so by analyzing a cross-section of input-output

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[24] And similarly, sectors with higher output growth or total factor productivity.

linkages between treated and non-treated sectors in the years after South Korea's big push—which targeted a discrete set of heavy industrial sectors. Ex post snapshots of input-output linkages cannot answer two crucial questions, however: first, whether the relationships between treated and non-treated significantly changed; and second, whether industries connected to treated sectors benefited from the policy. To address these limitations, Lane (2019) compares the development of external industries with differential degrees of connections to HCI, before and after the HCI intervention. By doing so, he shows evidence that downstream sectors experienced more growth, invested in more capital equipment, and had lower output prices.

By neglecting how industrial policy influences the development of external sectors, studies may miss the multitude of potential negative spillovers from interventions. For example, in many industrial economies the steel ingots industry has important downstream links (Leontief 1986). Across the world, steel industries are a perennial target of industrial policy. If these policies are deleterious, they may have critical consequences for the development of downstream sectors.

This point is highlighted by careful contemporary cross-country work by Blonigen (2016), who explores downstream spillovers related to steel sector interventions. Collecting steel policies from around the world and matching them to international trade and input-output data, his research indicates a negative relationship between interventions and the performance of downstream exporters. However, there is a striking degree of heterogeneity across countries. On average, steel industrial policies correlate with higher steel prices and weak export performance in downstream industries—an effect that is particularly extreme in LDCs. However, in East Asian countries, and others such as Sweden, steel industrial policy is consistent with positive downstream effects.

This section considered issues surrounding first-generation studies of industrial policies. I argue that this literature has critical deficiencies. First, many of these studies are marred by well-known issues of endogeneity that plague cross-country and industry-level studies of policy. I discuss how these issues undermine the interpretation of their results. Second, due to problems with measurement and lack of context, it is hard to derive precise policy conclusions from many early regression studies. Third, I consider how previous treatment of spillovers have perhaps been incomplete. With these issues in mind, I now consider the potential of contemporary econometric techniques. I focus on

microeconometric analysis of industrial interventions.

3. NEW EMPIRICS OF INDUSTRIAL POLICY

In this section I consider the microeconometric approaches to unpacking industrial policy interventions. Even with modern econometric tools, there is still confusion as to how to evaluate these policies and the extent to which it is possible.

That confusion is not surprising. After all, it is difficult to randomize industrial policies—which are often deployed "at scale." Not only is randomization unlikely, but industrial development strategies should be aimed at supporting specific industries or geographies. This empirical landscape, pockmarked by endogeneity, can seem increasingly alien against the canonical goal of many development practitioners: evaluating the average treatment effect of randomized interventions.

Nevertheless, there has been a proliferation of methods for estimating endogenous policy, and endogenous participation in these policies (Heckman, Ichimura, and Smith 1996; Heckman, Lalonde, and Smith 1999).[25] In development economics, researchers have long been considering how to evaluate targeted programs through *natural* "natural experiments" (Pitt et al. 1993, Rosenzweig and Wolpin 2000), and the literature on endogenous development interventions is vast (Todd 2007). Importantly, however, insights from the empirical urban economics literature are particularly useful for considering industrial policy (Bartik 2004, Neumark and Simpson 2015). In this domain, a large body of applied empirical work has emerged to evaluate place-based policies—interventions endogenously aimed at subnational regions.

Evaluation in a messy world: evidence from place-based studies.

Due to the nature of industrial policy, there is also much confusion about the quantity of interest researchers are pursuing. Many policy evaluation discussions revolve around the ubiquitous average treatment effect (ATE) of policy. However, in many circumstances, we are not interested in how an intervention might impact the general population, as in the case of public health researchers investigating the impact of antiretrovirals on pregnant women or labor force evaluations of an unemployment

[25]Notably in evaluating labor market programs in observational settings (Heckman et al. 1996, Heckman et al. 1999)

training policy. In such cases, we might be much more interested in recovering the average treatment effect on the treated population (ATET).

Consider planners keen to know the impact of infant industry policy. They likely want to understand the average impact of intervening versus not intervening with firms in a specific manufacturing industry. They are unlikely to want to understand the average impact of providing (versus not) that treatment to just any industry (including say, non-manufacturing sectors). In an idealized setting, particularly the right experimental one, estimating the ATE may be the same as estimating the ATET. In the world we are considering, however, the ATET will likely require different objectives and considerations.

Regardless of the quantity of interest, endogenous policy complicates practical policy evaluation. Criscuolo et al. (2019) provides an excellent example of estimating the effect of narrowly targeted interventions. This study explores the impact of a place-based industrial policy (the Regional Selective Assistance program)—principally manufacturing subsidies—supporting underperforming U.K. regions. Throughout the developed world, similar policies are aimed at lagging industries or locales. Thus, the empirical issue is similar to policies aimed at promoting potential sunrise (industries) regions. A significant difference in the case of lagging economies, however, is the issue of negative bias: in the absence of these policies, treated areas are likely worse off relative to the average non-treated area.

Estimating the impact of the U.K. policy, Criscuolo et al. compare the outcomes of areas that receive subsidies to comparable areas. They do so by isolating exogenous changes in whether an area is eligible to receive subsidies; this amounts to comparing areas that have been randomized in or out of policy for idiosyncratic reasons. In their setting, eligible areas are chosen based on a set of observable characteristics (such as GDP per capita and unemployment), where eligibility is updated every several years according to external criteria set by the E.U. However, whether an area (or firm) is eligible is determined by changes in their economic fortunes *and* also by external changes in grant criteria. Naturally, an area in decline might become eligible for subsidies and create a negative bias of the grant. To address this issue, the authors instrument for participation. They do so by using only the exogenous part of the rule: the changes in E.U. criteria.

The authors are thus able to use an instrumental variable analysis to estimate the average treatment effect on treated regions and firms.[26] They find a 10 percentage point increase in the maximum investment subsidy stimulates a 10 percent increase in industrial employment, and also promotes investment in incumbent firms. However, they do not find an impact on total factor productivity. They find that these effects are mainly driven by small firms, where larger firms appear to be unresponsive to the grants.

Broadly, the empirical literature on place-based industrial policy has fruitfully used "near-misses" for research designs to estimate the impact of endogenously targeted policies.[27] For example, Criscuolo et al. (2019) utilize exogenous variation on who becomes eligible or ineligible to receive industrial incentives. Others, such as Becker, Egger, and von Ehrlich (2010, 2012, 2013, 2018); Bernini and Pellegrini (2011); Cerqua and Pellegrini (2014, 2017); Bernini, Cerqua, and Pellegrini (2017); and Pellegrini and Muccigrosso (2017) use policy rules as random experiments to evaluate policy.

A number of regional industrial policy papers have centered on Italy's Law 488/92 (L488). This place-based policy sought to promote regional employment through investment subsidies, with the goal of closing regional disparities in Italy. The program allotted grants in regional competitions: investment projects are (annually) ranked on observable, predetermined criteria. Then grants were awarded by rank until regional funds were depleted (Cerqua and Pellegrini 2014, p.114). Bernini and Pellegrini (2011) studied the industrial policy through a difference-in-difference estimator, paired with a matching procedure. They use the fact that rankings are done each year and are region-specific: annually, regions have different levels of funding and numbers of applicants. By pooling applicants across space and time, this means observationally similar firms may or may not be funded, depending on the year and region.

Results from Bernini and Pellegrini (2011) echo Criscuolo et al. (2019); they show significant effects of subsidies on output growth, employment, and capital accumulation,

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[26]Or, more specifically, something similar. For expositional purposes, I will treat this as an ATET.

[27]Greenstone, Hornbeck, and Moretti (2010) notably used this strategy to identify the agglomeration effects of large plant openings. The generalization of “near-miss” identifications is from Neumark and Simpson’s (2015) discussion.

but do not find that subsidies increased short-run TFP.[28] Cerqua and Pellegrini (2017) confirm their previous findings on the effect of investment subsidies and growth, but note that earlier results may not have accounted for the fact that subsidies negatively affect firms not participating in the program. Moving beyond differences-in-differences estimates: The L488 policy introduced a ranking system with sharp discontinuities between recipients and non-recipients (within region-years). This allows Cerqua and Pellegrini (2014) to use a type of regression discontinuity design (RDD) to estimate the impact of subsidies. In this context, the authors find impacts on capital accumulation and turnover, but do not find significant effects on labor productivity. Using an RDD approach, Pellegrini and Muccigrosso (2017) show that L488 subsidies did not merely delay firm exit and lowered the risk of default in recipients.

A related set of papers by Becker et al. (2010, 2012, 2013, 2018) explicitly use an RDD to evaluate the impact of an EU-wide funding scheme aimed at revitalizing regional economies. The authors estimate the impact of the program using eligibility criteria: regions were able to receive structural funds if their GDP per capita was below 75 percent of the EU average. Intuitively, only areas below the cutoff received funds, but (before treatment) these areas will have similar characteristics to those above the cutoff.[29] Becker et al. (2010) estimate that the program significantly impacted GDP per capita growth in regions (1.6 percentage points for a treatment period) and back of the envelope calculations hint at potential multiplier effects. They did not find significant employment effects, however. Becker et al. (2013) shows regions with greater human capital and higher quality institutions are better at transforming funds into regional growth and investment. Becker et al. (2018) show that while the funds had a positive impact on growth for each period between 1989 and 2013, the effects were weaker during the crisis, as well for areas more exposed to the crisis.

Similarly, a number of recent studies surrounding government R&D policy—itself a popular industrial policy instrument—have confronted endemic endogeneity with similar research designs (Bronzini and Piselli 2016, Dechezleprêtre et al. 2016, Howell

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[28]Longer-run analysis (Bernini et al. 2017) argues TFP eventually increased, and that these effects may take time.

[29] In the case of the RDD design, the authors estimate the local average treatment effect (LATE) of the program.

2017).[30] Howell (2017) utilizes rankings rules to estimate the impact of the U.S. Department of Energy's SBIR grant program. She finds important positive effects on financing, patenting activity, and revenue. Dechezleprêtre et al. (2016) estimate the causal effect of U.K. R&D incentives on innovation, using shifts in asset-based threshold in eligibility for tax subsidies; they find significant impacts on R&D and patenting activity, along with evidence of spillover effects. Bronzini and Piselli (2016) study a regional R&D policy in Emilia-Romagna, a region in northern Italy. Utilizing policy selection criteria, they estimate the impact of the policy with an RDD design, and find a positive effect of subsidies on patenting activity—but results were limited to smaller firms.

Accordingly, moving beyond *average* treatments will undoubtedly expand our understanding of industrial policy interventions. Consider the case of firm size and the impact of export activity. Our theoretical understandings of industrial dynamics are firmly grounded in issues of heterogeneity—most notably, firm size. Unsurprisingly, much of the empirical evidence from the developed world has revealed that policies have substantial impacts along firm-size dimensions. Unpacking the average impact of policies allows our research to explore the applicability of policy across contexts.

Evaluating Danish export promotion policy, Munch and Schaur (2018) find positive effects across several firm performance measures. While the policies had a positive impact on exporters of all sizes, the policy was particularly useful in helping small firms enter export markets. Similarly, Martincus and Carballo (2010) demonstrate that average treatment effects can mask heterogeneity in the developing world. Using Chilean data, they show that export promotion activity was particularly beneficial for small firms relative to large firms. Given the proliferation of techniques for examining heterogeneous effects, the skillful application of these tools may tell us a lot about the specificity of policies across domains.

Two cheers for microeconometric approaches.

A number of very recent studies have proposed new ways of exploring the impact of

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[30] Also see Azoulay, Graff Zivin, Li, and Sampat's (2019) RDD study of the impact of United States NIH funding. The findings of recent quasi-experimental studies dovetails with contemporaneous (control-based regression) research by Le and Jaffe (2017) on the efficacy of New Zealand's R&D grant policies.

industrial policy. In doing so, these approaches confront the many issues raised in this study. In particular, these studies focus on specific case studies and natural experiments to estimate the impact of industrial policy. This section argues that while there are limitations to this approach, these studies can still deliver valuable insights into industrial policy.

To do so, new approaches to industrial policy evaluation can follow the lead of literatures such as trade policy: turning to micro-level evidence, using within-country variation to inform research designs, and more. However, does getting granular—or more specifically, drawing inferences from within-country data—limit what we can say about industrial policy?

With industrial policy, there is an uncomfortable tension in the objects we study. Industrial policies are often national-level policies with national-level objectives. However, these policies are targeted at subnational units: products, industries, places, etc. While the deployment of policies within a country permits reduced-form estimation, aggregating these estimates is challenging. Martin Rotemberg (2017) considers the general equilibrium implications of industrial policy (in his case, Indian credit subsidies) when the interventions are estimated from within-country data. Among other things, his work proposes a framework where firm-level responses can be used to calculate a sufficient statistic for the elasticity of aggregate growth to private growth. More generally, Sraer and Thesmar (2018) and Baqaee and Farhi (2018) suggest methods that allow economists to explore the aggregate impacts of policy using firm data with minimal assumptions. Sraer and Thesmar (2018), in particular, propose a methodology that maps firm-level estimates into aggregate outcomes (such as TFP or output) from quasi-experimental variation.

More broadly, a critical downside of reduced-form empirical studies is that evaluating the aggregate welfare implications of policy is still an unresolved problem.

Since industrial policy is a catch-all for many terms and concepts, it is difficult to propose a single means of examining their impact. To this end, microeconometric methods are invaluable. Microdata allows us to examine common performance measures, such as output prices or employment. Though microeconometric techniques, especially firm-level empirics, are invaluable for capturing the canonical performance measure: TFP.

A rapidly expanding set of structural econometric techniques have allowed researchers to estimate firm-level production functions and TFP in more robust ways.[31] Building on well-established procedures (Olley and Pakes 1996, Levinsohn and Petrin 2003), Ackerberg, Caves, and Frazer (2015) propose new restrictions for robust estimation of production functions;[32] Gandhi, Navarro, and Rivers (2018) propose nonparametric identification of production functions where gross output and intermediate inputs are observed.[33] In a series of recent papers, De Loecker (2011) and De Loecker, Goldberg, Khandelwal, and Pavcnik (2016) consider the estimation of production functions in the presence of various forms of output price and input price heterogeneity.[34] Relatedly, De Loecker (2013) considers a method of estimating learning-by-exporting effects using firm-level data.

Nonetheless, quasi-experimental studies using within-country variation can be valuable in and of themselves. Many emerging studies are already addressing a multitude of unexplored theories, mechanisms, and implications of industrial policy. Given how little we know and how often these policies are used, such estimates are critical. The studies I present at the end of this paper, for example, shed light on previously rejected mechanisms with the help of careful research designs.

The potential value added of exploring industrial policy using within-country data can be seen in the blossoming literature on trade policy. Like studies of industrial policy, it is not obvious how microeconometrics estimates of trade liberalization translate into aggregate objects (e.g., growth). While the clear connection between liberalization and growth can be elusive, within-country studies have nonetheless been invaluable for understanding the impacts of contemporary trade policy across industrial

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[31] See Van Beveren (2012) for an extensive review of work on structural estimation of TFP.

[32] In particular, Ackerberg et al. (2015) propose means of dealing with deficiencies of the early control-function approaches to production function estimation.

[33] Also see Doraszelski and Jaumandreu (2013), who consider similar issues in light of R&D expenditures. Eslava, Franco, Roux, and Verhoogan (2017) propose promising new methods that utilize input prices for production function estimation.

[34] De Loecker and Goldberg (2014) provide a comprehensive review of these issues in the estimation of production functions, with an emphasis on evaluating the performance of firms in response to trade policy; see Garcia-Marin and Voigtländer (2019) for an application of these methods, evaluating the impact of exporting on firm productivity.

development outcomes (Goldberg and Pavcnik 2016).[35]

For example, recent microeconometric work on trade policy has highlighted how policy liberalization impacts productivity growth, especially through intermediate input markets, as seen in recent work by Amiti and Konings (2007) and Topalova and Khandelwal (2010). Studying trade liberalization in Indonesia and India, respectively, both emphasize the role intermediate inputs play in promoting productivity relative to the competitive effects of liberalization. Importantly, firm-level studies have illustrated how the impact of trade policy has been mediated in surprising ways through market imperfections. For example, De Loecker et al. (2016) show that while Indian trade liberalization reduced firm costs, these benefits were not passed directly onto consumers. The authors indicate that market imperfections might play a role.

In summary, though industrial policies are often endogenously targeted, contemporary microeconometric methods nevertheless allow us to better understand these policies. The preceding section described how in the world of place-based policy, a related literature is carefully applying tools from causal inference to understand endogenous policies. I also describe the benefits and shortcomings of microeconometric approaches to industrial policy evaluation. Though, like the literature in empirical trade policy, these approaches do not allow us to capture the aggregate effects of policy, they have advantages: They 1) permit the application of contemporary research designs that test the basic hypothesis of interventions; and 2) they provide clearer consideration of precise policies, outcomes, and performance measures—such as TFP—that are directly related to these interventions.

4. NEW EVIDENCE FROM HISTORIC CASES: A REVIEW

A relatively new wave of empirical studies is already presenting a far more nuanced picture of industrial policy. These new studies share something in common. While these studies tend to be reduced-form, they nonetheless have much in common with earlier, more structural work on industrial policy (e.g., Head 1994) in that they draw empirical lessons from specific policy episodes. Similarly, they use granular institutional details to inform their studies. While these institutional features improve the contextual

[35] See Goldberg and Pavcnik's (2016) extensive review of current trade policy studies.

interpretation of their findings, they also play a key role in the empirical research design of these studies. Historical knowledge thus plays a key role in many emerging studies. Moreover, because the role of industrial policy and protection has been a central question in economic history, many of these papers consider seminal policy cases.

All these studies leverage aspects of natural experiments—often external changes in policies, costs, or events that mimic those of an industrial policy. Unlike many first-generation studies, this has meant research has had to take the institutional setting seriously and speak directly to specific policies. Like micro-empirical studies of trade policy, many of these studies tend to combine nationally imposed (or even internationally imposed) policy changes with disaggregated data to estimate the impacts of industrial policy forces.

Reviewing new evidence.

Reka Juhasz (2018) provides one of the first attempts to study infant industry policy using microeconomic methods. She takes the case of the French mechanized cotton-spinning industry, the bedrock of 19th century industrialization, and uses the Napoleonic Blockade as a natural experiment in temporary infant industry protection. In the case of early French manufacturing industry, the blockade of British goods (1806–13) during the Napoleonic Wars had the effect of rerouting trade between Britain and Continental Europe, exposing Northern French industry to more protection relative to Southern French industry.

The temporary nature of the blockade mimics important components of infant industry policy. The design potentially isolates the policy forces from the political economy surrounding implementation—not least because many infant industry policies are never withdrawn. Moreover, the policy allows her to trace the evolution of the cotton industry, comparing those who were more/less exposed to the blockade-imposed trade costs, before and after the Continental Blockade. Thus, Juhasz effectively investigates a counterfactual.

Using difference-in-differences estimation, Juhasz shows the short- and long-run effects of the blockade. In the short run, temporary protection had a large, significant impact on the adoption of mechanized cotton spinning technology (measured by spinning capacity per capita). Using a slightly similar instrumental variable design, she

shows that forces driving early 19th century technological adoption are related to higher industrial development through 2000.

With infant industry policy in mind, a series of papers have revisited the implications of Canada's protectionist National Policy (1870–1913). Harris, Keay, and Lewis (2015) consider the National Policy experience as a natural experiment in infant industry policy—one that radically altered the extent of protection across sectors. Even with aggregate industry data, they are able to explore the comparative development of industries, finding positive effects of tariffs on growth, price reductions, and productivity. Similarly Alexander and Kaey (2018) apply new quantitative techniques (à la Arkolakis, Costinot, and Rodriguez-Clare 2012) to evaluate the welfare effects of 19th century protectionism.[36] Using a multisector general equilibrium model they show that 1879 tariff changes may have had positive impacts and (static) welfare effects in the small open economy. These short-run effects challenge the conventional wisdom of Canada's National Policy, which was largely seen as having "fostered long run-dynamic welfare improvements by, for example, sheltering infant industries, promoting agglomeration and encouraging technological change" but with large static costs to consumers (p.2).

The factor accumulation promoted by the Marshall Plan across Western Europe looms large as an explanation behind "Les Trente Glorieuses"—the decades of sustained economic growth following World War II. Michela Giorcelli (2019) explores the long-run effects of one such industrial development intervention. She focuses on the long-run implications of a particularly successful Marshall Plan project in Italy, the United States Technical Assistance and Productivity Program (1952–58). The Productivity Program not only extended credit for purchasing technologically advanced American capital but also heavily promoted modern management practices in Italian industry (factory operations, production planning, marketing, etc.). In unpacking this policy, this study provides a useful example of evaluating industrial interventions in historic, non-experimental settings.

Specifically, institutional details of the interventions allow her to construct an intuitive research design. The program was originally planned to be rolled out across five regions; however, unexpected budget cuts reduced the size of the program—and did so

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[36] Also see Costinot and Rodriguez-Clare (2014) review of this literature.

after nearly 4,000 firms had applied for the program. Instead of being administered to five regions, the program was scaled back and targeted to a single province in each region. This accident gives her two natural experimental groups. Within the same region, she is able to compare treated firms in "experimental provinces" to applicant firms in "nonexperimental provinces." Accordingly, she shows that the two sets of firms are observationally equivalent.

Giorcelli finds the policy had significant effects—including effects on long-run development. First, she finds important effects on firm survival. Moreover, firms that survived fifteen years after the program had significantly more sales, employment, and productivity—as measured by (revenue) total factor productivity. Importantly, she argues that the persistence effect of the intervention was not due to physical capital investment per se, but due to the complementarity between new managerial expertise *and* equipment investment.

Using the experience of 19th and 20th century metal shipbuilding, Walker Hanlon (2018) studies a core concept at the heart of industrial policy: dynamic comparative advantage. Through the 19th century, U.S. and Canadian shipbuilders were leaders in wood shipbuilding. However, this dominance was disrupted by the transition to steel and steam shipping, and the ascent of the British ship manufacturers. After 1850, British steel shipbuilding became competitive due to early access to cheap, crucial intermediates (via developed domestic iron and steel industries). Though these initial cost advantages evaporated with the discovery of iron ore in North America, Britain—surprisingly—maintained its international dominance in the industry. Why?

Using careful counterfactual empirics, Hanlon shows how the British industry dominated—and the extent to which protection allowed the U.S. to transition from wood to steel. He finds that midwestern U.S. shipbuilders, naturally protected from British competition, were able to adopt steel and weather changes in the new competitive industry. On the other hand, shipyards most directly confronted by British shipbuilding faltered. In particular, Canadian shipbuilders, unable to engage in industrial policy against their British brethren, were wiped out. U.S. Atlantic shipbuilders fared mildly better, supported by U.S. government procurement.

A key aspect of his analysis demonstrates the potential role of learning-by-doing spillovers. Within the United States, he finds that shipbuilders that were proximate to U.S. naval yards were more likely to make the transition from wood to steel. In the U.S.,

Hanlon argues that a skilled workforce helped the transition to steel. On the other side of the Atlantic, he provides qualitative evidence to show that Britain's dominance in the industry was likely due to a pool of highly skilled labor.

Hanlon's work echoes earlier insights from endogenous growth models with trade and learning-by-doing spillovers (Young 1991, Stokey 1988); in the case of British shipbuilding, early entry into markets may translate into persistent leads. Bardhan (1995), shows how such frameworks can be used to argue for infant industry export promotion. On the other side of the Atlantic, he shows that temporary protection helped U.S. industry transition. However, this was not sufficient to surpass the British lead.

Jaworski and Smyth (2018) examine government protection and learning in the rising U.S. airframe market. Like shipbuilding, the airframe industry is highly exposed to government procurement and is the location of protected national champions. Brazil's Embraer and France's Airbus are two textbook examples. Using newly digitized data on market share for all American airframe manufacturers between 1926 and 1965, Jaworski and Smyth find that government contracts helped early producers avoid periods of "shakeout"—the turbulent exit and consolidation seen in many emerging industries. Their estimates show that government contracts are positively related to market share in postwar periods, and suggest that military experience granted young firms advantages vis-à-vis learning capacity.

Using a natural experiment from postwar Finland, Mitrunen (2019) studies the emergence of Finland's high skilled industries—and highlights a critical dynamic spillover from industrial development.

In 1944 the Moscow Armistice ended an internecine conflict between Finland and the Soviet Union. Beyond territory ceded to Stalin, the treaty required Finland to pay significant war reparations in the form of heavy industrial goods (ships, locomotives, cables, and engines)—products that the Soviet Union struggled to procure on the world market. Between 1944 and 1952, the small Nordic nation was compelled to finance and expand an advanced export sector under direct Soviet oversight, including quality control guidelines and the real threat of invasion.

Finland had little experience or expertise in producing the heavy industrial commodities demanded by Stalin. With a sizeable agrarian sector and comparative expertise in wood-based products, the order could be said to have defied Finland's

comparative advantage (Lin and Chang 2009). Nonetheless, Mitrunun shows how the policy had significant, long-run impacts on Finland's industrialization—not only through direct industrial channels but through its effects on the Finnish labor market. Using differences-in-differences estimates, Mitrunen compares the differential evolution of targeted and non-targeted industries and shows the treated industries grew more (in terms of output and employment).

Echoing aspects of endogenous growth theory, Mitrunen also shows how the policy impacted human capital accumulation and earnings across different generations of workers. Using rich Finnish registry data, Mitrunen compares workers near versus far from the labor demand shock of the policy. For the first generation of workers, he finds the policy significantly impacted the decision of adult agricultural workers to switch to manufacturing—a choice that was both permanent (workers still tended to be employed 20 years later), and significantly raised their wages.

For younger cohorts, Mitrunen finds that the policy significantly impacted educational and labor market outcomes. The expansion of high-skilled industries incentivized individuals to invest more in higher education. For the younger generation exposed to the intervention, the probability of getting a higher education had the same magnitude as the opening of a new university within 200 kilometers of one's home. Accordingly, these same individuals were also significantly more likelyto enter white-collar occupations.

Mitrunen's research touches on an influential thesis by Robert Lucas (1993), who considered the theoretical forces that could explain the fundamental patterns of the East Asian growth miracle: among other things, the conspicuous patterns of human capital and capital accumulation, alongside rapid trade expansion. This theoretical work circles around the potential spillovers from on-the-job learning that accrued as these economies shipped increasingly sophisticated goods to the world market.

Lane (2019) examines South Korea's Heavy Chemical and Industry (HCI) big push. Like the studies above, he uses a natural experiment to examine whether this temporary industrial push had long-term consequences—in sectors that were treated directly by the intervention and in industries exposed to the policy through supply chain linkages.

Importantly, the policy was both started and ended due to external political forces. In 1973, following a geopolitical crisis sparked by the Nixon administration

withdrawal from the Asia Pacific region (the so-called Guam Doctrine, or Nixon Doctrine), South Korea pursued a radical new heavy industrial big push. The principal aim of the policy was to promote an intermediate input industry capable of fueling a military-industrial complex. The push, which targeted a discrete set of industries, was withdrawn, however, in part due to the assassination of President Park Chung Hee, the policy's chief architect, in 1979.

Using newly digitized data on South Korean industrialization, Lane compares the differential industrial evolution of industries that were targeted versus industries not targeted by the HCI big push, comparing the two sets of industries before and after the introduction of the policy. He finds that the policy promoted rapid and radical capital deepening in targeted industries, and likewise rapid industrial expansion—effects that persisted long after the withdrawal of the policies in 1979. Importantly, like the results shown by Blonigen (2016), Lane shows that the policies inspired a rapid reduction in the price of output in treated sectors. Surprisingly, data also indicates that throughout the period, nominal protection was not significantly different for the treated industry. Instead, he highlights the potential role of subsidized intermediate imports during the period.

With measures digitized from pre-1973 input-output tables, Lane explores how the policy impacted non-targeted industries upstream and downstream from treated sectors. He shows that the price decreases in treated sectors were transmitted to downstream industries. Industries that purchased larger shares of inputs from targeted sectors also invested more in capital, saw higher market entry, and also had lower output prices. Conversely, he finds that the effects of the industrial policy have more mixed effects on domestic *suppliers* to treated industries. In particular, industries supplying high shares of output directly to treated industries contracted. He shows this was driven by import competition, as treated sectors imported inputs from abroad.

This section reviewed the new body of work evaluating infant industry policy and issues surrounding industrial development interventions. This work illustrates the potential of using natural experiments to identify the impacts of industrial interventions. Given the paucity of evidence surrounding basic aspects of industrial policy—such the persistence of protection or cost advantages or spillovers—within-country case studies provide potentially valuable evidence. The next section speaks to the limitations of these

studies and future directions for empirical work.

5. CONCLUSION

Past, present—and future.

This study reviews empirical studies of industrial policy, past and present. I focus on how new observational studies of industrial policy are updating our understanding of these controversial interventions. However, to better understand these new studies, I consider the body of first-generation studies of industrial policy.

I do so critically. The early empirical studies of industrial policy formed the foundation of how we understand industrial interventions in developing countries. While many conceptual arguments within this literature are incisive, aspects of this first-generation empirical work do not provide us with evidence as to the efficacy or workings of industrial policy. In some cases, research has muddled the way we understand these interventions. By reviewing empirical and conceptual issues in these papers, I indicate how contemporary studies may provide more precise insights. When necessary, I juxtapose first-generation studies of industrial policy against insights from contemporary work.

The third part of this study proposes how current tools and empirical frameworks can confront the shortcomings of first-generation studies. I explain a common issue about the possibility of evaluation of endogenously targeted policies. I do so by considering how some economic literatures, such as the growing body of work on place-based policy, are addressing similar issues in policy evaluation. Moreover, I highlight how microeconometric approaches to industrial policy can enable researchers to unpack policy in more sophisticated ways.

In the last section of this study, I review the growing body of case studies that take the mechanisms and underlying arguments of industrial policy interventions seriously. These studies share something in common: These (mostly) reduced-form studies draw empirical lessons from precise cases, industries, and policy episodes. This specificity makes institutional details essential; these details are used to build more critical research designs, but also to better understand the mechanisms of each policy.

A great deal of these studies—though not all—come from economic history. These cases demonstrate that extensive knowledge of historical cases and detail has been

—and likely will continue to be—an asset in understanding interventions, as well as informing the application of causal inference.

Generally, the takeaways for this review can be distilled into these points:

- First, a bulk of the first-generation empirical work has been uncritically accepted. Rather than constituting a body of evidence, many correlational studies complicate our understanding of these interventions. These studies do not provide economists with precise guidance as to the workings of industrial policy.

- Second, industrial policy is a multidimensional concept. Empirical studies of industrial policy interventions are most valuable when the interventions are clear, and researchers are transparent about the context in which they are implemented.

- Third, by focusing on specific policy cases, institutional details, and cleaner research designs, new empirical research on industrial policy incrementally contributes to our understanding of these interventions. Currently, the paucity of evidence means that fundamental questions about the operation and mechanisms of industrial policy are open.

This study argues that applied microeconometric analysis of industrial policy is fruitful and is already paying dividends. I highlight studies that have relied on reduced-form econometric methods and within-country variation. Often, these studies cannot directly speak to the aggregate impacts of industrial interventions. Unlike the domains of public finance, we do not yet have a toolbox for coherent welfare analysis in these settings. Though infant industry policies have a long history in economic policy, contemporary frameworks to analyze policy are still emerging. Moreover, by focusing mostly on reduced-form studies, I have necessarily left out many emerging structural studies in the field of structural industrial organization. The theoretical specificity of these analyses can assuage the limitations of reduced-form methods.

A limitation of this study is that it focuses too much on causal inference. Beyond questions of causality, we understand far too little about the scope, patterns, and

correlates of industrial policy in general. Correlational and descriptive studies of interventions will undoubtedly expand our understanding of industrial policies (if interpreted properly). Consider recent work by Nunn and Trefler (2010), who explore the cross-country relationship between the skill-bias of protection (tariffs) and industry growth.[37] Using rich data on international tariffs and growth, the authors find a positive relationship between the skill-bias of tariffs structure and growth. They also show that countries with poor institutions have tariffs structures biased toward less skill-intensive sectors. Similarly, Amiti and Khandelwal (2013) explore the relationship between protectionism and product quality for 10,000 goods across 56 countries. They find that lower tariffs are associated with quality upgrading for producers close to the international frontier, but lower tariffs seem to discourage quality upgrading for products far from the international frontier. Given the paucity of contemporary studies, future descriptive research of this sort can uncover first-order empirical patterns.

Beyond regression studies, future empirical work should also attempt to provide deeper, more precise measures of industrial policy. In the contemporary world, industrial policies are less likely to appear as easily observed tariffs. The scarcity of comparable retrospective data on non-tariff barriers is a perennial hurdle for cross-country studies of trade policy. The future of industrial policy, just like trade policy, will depend on our ability to provide cleaner measures of interventions. This is important now more than ever, when industrial policies are often obfuscated and embodied in a constellation of levers. Penelopi Goldberg and Nina Pavcnik (2016) call for a renewed emphasis on fundamental measurement. The same holds for research on industrial policy interventions.

Without a doubt, future research must do more to understand the interaction between political economy and industrial policy. Because industrial policy is state policy, its success, scope, and efficacy is sensitive to institutional context. Qualitative work by comparative social scientists, such as Peter Evans (1995) and Robert Wade (1990), has long emphasized the importance of strong state institutions in successful (versus deleterious) industrial policy. Establishing how the efficacy of industrial policy is

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[37] Lehmann and O'Rourke (2011) perform a similar correlational exercise, exploring the relationship between the structure of sectoral protection and growth across several 19th-century European economies. Distinguishing between agricultural, manufacturing, and revenue-oriented tariffs, they find that only manufacturing tariffs are significantly correlated with aggregate growth during the period.

shaped by political forces will undoubtedly allow us to learn from the past. Similarly, surprisingly few empirical papers explore the political endogeneity of industrial policy. Many qualitative papers also draw an association between successful industrial policy and autocracy, given the successful deployment of industrial interventions across Asia. Nonetheless, little to no empirical work has confirmed this. Considering the infant industry policies pursued by liberal democracies, such as Canada (the National Policy) and Australia (Tariff Board), the relationship between the success of industrial policy and regime type is still an open question. The related role of state, administrative, and bureaucratic capacity in deploying development policy will undoubtedly expand our understanding of industrial policy (Lane 2019). Dell, Lane, and Querubin (2018) discuss how East Asian and Southeast Asian statecraft enact divergent developmental policies—a thesis that could well speak to the heterogeneous experiences of industrial policy across the world.

Nations have and will continue to shape their industrial destinies. Nevertheless, even with the recent papers reviewed in this study, the literature on industrial policy is still thin, dwarfed by the attention these policies receive from policymakers. This review is a proposal to take these interventions—along with all their complexities—more seriously as objects of inquiry. Doing so requires serious evaluations of past policy, but also reevaluation of past consensus.

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