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It’s a weird time to be an industrial policy researcher. “[The Return of the Policy That Shall Not Be Named](#)” caught us off guard, and the demand for prescriptions looms over an embarrassingly scant body of knowledge—especially within the field of economics, which for the past few decades has offered little more than Gary Becker’s 1985 quip that “the best industrial policy is none at all.” At a time when governments are returning to industrial policy, we are largely clueless about how to make it work.

In this climate of ignorance, there is something a little jarring about Mazzucato, Kattel, and Ryan-Collins’s wish to lead us from today’s tepid practice to a bolder paradigm. In order to meet the vast challenges we face today, we are implored to reject our current “market failure approach” and think grander. Instead of surgical policies, we must marshal state, civil society, and markets all in service of a mission-driven cause.

While this call for moonshots is stirring, it ultimately says too little about how to turn this vision into reality. When it comes to the conceptual issues facing industrial policymakers, Mazzucato and colleagues are mostly right, albeit at a high level. Industrial policy certainly requires more institutional capacity (a vital point that is too easily lost in technocratic debates). We do need new tools for policy evaluation—since the criteria we wield today are often meant to decaw industrial policy rather than assess it—as well as new strategies for sharing risk and reward. But this abstract framework leads us astray in three ways, particularly for the United States.

First, this paradigm largely avoids any specificity about the hard decisions to be made—a blind spot created in part by their caricature of industrial policy’s past. Second, they say almost nothing about politics, which has prevented even the “market failure approach” they reject from being implemented robustly. And third, they dismiss some of the powerful tools progressives already have at this critical juncture. Without grappling with the details, vaulting ambition—however inspiring—risks leading us only further into the wilderness.

Start with Mazzucato and colleagues’ portrait of industrial policy’s past. They are right that the Washington Consensus viewed industrial policy as a failed vestige of postwar developmentalism. In the last two decades of the twentieth century, most academic economists who had anything to say about industrial policy quibbled over its theoretical flaws.

But even as the discipline deemed industrial policy implausible, the world has kept using it—and we are finally learning more about the complexity of these real world efforts in the wake of our field’s “empirical revolution.” Budding work over the last few years [has painted a rich, varied picture](#) of industrial policy in practice, not just in theory. Nearly every paper complicates received wisdom about how industrial policy worked, or didn’t. As an empiricist and economic historian who has learned a great deal from this work, I don’t recognize the tepidness that Mazzucato and colleagues think characterize postwar policies across the board: those Cold War interventions fixated on “narrowly defined technological goals and specific sectors” that they take as a foil.

The truth is that postwar industrial policy was hardly devoid of grand visions. Wading through forgotten Five-Year Plans across the (non-communist) developed and developing world, you will not see a timid focus on particular industries or lack of multi-sectoral missions. Ideas like the “Big Push,” the postwar concept that investment may need to be coordinated, were multi-front investment drives and pursued with a myriad of policy levers. And in other dimensions, Lyndon B. Johnson’s Great Society initiative embodied an ambitious social mission in the United States.

Industrial history also tells us that grand, mission-oriented policies do not absolve us from having to think about details. Postwar East Asia, the paragon of postwar policymaking, has hardly been characterized by timid policies or narrow focus; their successes don’t seem distant from the sweeping missions Mazzucato and colleagues champion. At the same time, their grandness required specific interventions, and they were crucial to their working. Moreover, past efforts didn’t fail for lack of ambition to bring together multiple sectors of the economy, much less society. They typically failed due to the [realities of their state capacity and political context](#).

That brings us to the second weakness of this framework: its failure to grapple with the realities of political economy. We might read Mazzucato, Kattel, and Ryan-Collins as urging a profound shift in our preferences, yet they fail to elucidate the constraints, much less the political game they are bound by. Policy and institution-building must contend with discordant interest groups and the realities of the political business cycle. In the United States, in particular, industrial policy must contend with staunch Republican opposition, whose whole raison d’être is to shrink the state to such an extent that it can be drowned in a bathtub, as Grover Norquist put it. Any moonshot will have to survive the punishing magnitude of U.S. political gravity.

Perhaps it is precisely because of this political climate that ambition is necessary: as a tactical matter, policies must be boldly formulated in order to survive the political bargain that will inevitably gut them. (Congressional winnowing of Biden’s grand visions for infrastructure may be a case in point.) But this kind of tactical ambition should not come at the cost of misdiagnosing the problem. Mazzucato and colleagues wish to save us from the tepidness of a “market failure approach”—“find the market failure; fix it with a support instrument”—as if it has actually been taken seriously in U.S. economic policy over the last forty years. If only. We should not conflate these political failures with the concept as such. In reality, market failure remains an indispensable tool.

Quantitative work in economics has shown that market imperfections can be a powerful guide for the allocation of policy. Ernest Liu, for example, [has recently demonstrated](#) how analysis of distortions that ripple through the industrial network can help pinpoint sectors most ripe for policy.

Closely related to network economies, concepts such as “granularity” reveal another interaction between market imperfection and industrial strategy. In a world rife with imperfections, a small number of firms can constitute the lion’s share of economic activity in a given market—domestically and internationally. As a result, a handful of firms [may shape](#) the comparative advantage of a national export industry. The implications of this form of market failure are important when it comes to industrial policy and regulation, as [Cecile Gaubert, Oleg Itskhoki, and Maximilian Vogler](#) have recently shown.

Or consider industrial policies in oligopolistic markets (another form of market imperfection). In a study of China’s shipbuilding push, [Myrto Kalouptsidji, Panle Jia-Barwick, and Nahim Bin Zahur](#) have shown how industrial policies interact with the complexities of market structure. Among other things, this work can speak to the multitude of policy levers mobilized by the Chinese Communist Party. These are undoubtedly useful tools for understanding the multitude of incentives that industrial policy can wield.

If Mazzucato, Kattel, and Ryan-Colins are wrong to dismiss the value of attending to market failures, they are correct about our need for new tools. The tools they promote, however—ones focused on dynamic efficiency—aren’t mutually exclusive of market failures.

Notions of dynamic efficiency are indeed important for evaluating industrial policy: as the authors note, when long-haul missions are meant to pay future dividends, short run attention to static efficiency can be misleading. But dynamic efficiency alone can’t save us. In fact, the move the authors make—rejecting the importance of market failure in favor of dynamic efficiency—has often been deployed in arguments *against* government intervention, from Harold Demsetz’s famous [critique](#) of Kenneth Arrow’s [argument](#) for government investment to Robert Bork’s limited conception of the scope of antitrust law and the [intellectual toolkit of Chicago School deregulation](#). Dynamic efficiency has even been deployed in the defense of [Jeff Bezos and the necessity of monopoly power](#). It’s not that dynamic efficiency is useless. But, unmoored from attention to market failures, it will not get us to where we need to be.

For all these reasons, the picture offered by Mazzucato and colleagues fails as a guide to the future of industrial policy. Beneath any moonshot must lie launchpad scaffolding, but we lack the messy, hard, complicated details of practical deployment. Grand ambitions should not blind us to the granular, technical decisions they will entail. Industrial policy does need to be ambitious, but even more urgently, it needs to be detailed, practical, and precise. ♦



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