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The Great Reversal: How America Gave Up on Free Market

by **Thomas Philippon**

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For decades, if not centuries, America was a synonym for free market. Not anymore, according to Thomas Philippon. He begins his book with two examples of declining competition in America in the past two decades: the airline and telecommunications industries. Based on these two examples, the author, instead of specifying the aim of the book in a sentence or two, provides a batch of questions that will be addressed in the book: "(1) Do … higher prices (due to lack of competition in the US markets) affects all industries, or are airline and telecommunications industries special? (2) How did Europe, of all places, become more of a "free market" than the US? (3) Isn't it better if firms make profits rather than teeter on the verge of bankruptcy? (4) Is big beautiful? Can concentration be a good thing? What, if anything, sets Google, Apple, Facebook, and Amazon apart? (5) Should we worry more about privacy or about competition? Or are they perhaps two sides of the same coin? (6) What are the implications of market power for inequality and for growth, wages, and jobs? (7) Why are free markets so fragile? How did lobbyist end up wielding so much power?" (p. 8). This is a rather effective way of specifying the book's aim and the reader is eager to get the answers to those truly relevant questions, even though they are thoroughly restricted to the US and its domestic market(s). After all, that is the biggest market in the world.

In the start of answering those questions, the author offers three main hypotheses: "(1) competition has declined in most sectors of the US economy …; (2) the lack of competition is explained largely by policy choices, influenced by lobbying and campaign finance contributions …; (3) the consequences of a lack of competition are low wages, lower investment, lower productivity, lower growth, and more inequality" (pp. 9-10). Obviously, if these hypotheses are to be tested, a very comprehensive research plan must be in place.

Part One of the book (The Rise of Market Power in the United States) deals with the first hypothesis and with some elements of the third. At the very beginning the reader is provided some data about US economic growth and dynamics of economic inequality, some basics on competition and its effects, and a reminder of Mancur Olson (1965) seminal idea on the logic of collective action. Along these lines, the author claims that free markets are fragile because "concentrated special interests are likely to organize and fight to protect their rents, while diffuse majority interests are trumped" (p. 23). A trained industrial organisation economist would understand this as incumbent firms efficiently creating legal barriers to entry, due to their lower costs of collective action, with the aim of creating/protecting their market power. Considering that this insight has held true since the dawn of market economy, the reader wonders what exactly has changed in America in the last twenty years to transform it from the land of the free markets to land of market power. Perhaps the rest of the book will provide an explanation.

The author then turns to the issue of market concentration and provides a non-specialist crash course in market concentration and market power, and their effects on the consumer welfare, although focused only to allocative inefficiency (deadweight loss). Measurement of market concentration using the Herfindahl-Hirschman Index (HHI) is explained, as is the concept of relevant market. Although Philippon refers to the Carl Shapiro (2018) caveat that only concentrations in the relevant market can be related to market power, he insists that a recorded increase in market concentration in industries should be reason for concern, and that there are very few increasing market concentrations (e.g. Walmart, for example) that are welfare-enhancing.

The crucial methodological problem is that Philippon too often, though not always, confuses market concentration with competition. As modern industrial organization has demonstrated (Dennis W. Carlton and Jeffrey M. Perloff 2015) competition is about competitive pressure among firms in the market, and such a pressure can materialize irrespective of the number of firms, i.e. market concentration. In short, competition is about the incentives for a firm, which are created by other incumbent firms and potential entrants. Furthermore, as demonstrated by William J. Baumol (2002) in modern economy with substantial indivisibilities, i.e. fixed costs, and corresponding economies of scale, it is oligopolies in many industrial sectors that produce the strongest competitive pressure.

In short, increased market concentration - by whichever measure - does not necessarily mean decreased competition. After nor being direct about that, the author accepts that increased market concentration can be interpreted in different ways and that decreased competition is only one of the six hypotheses that are suggested, including the one that the market concentration increase is not verifiable because the data refers to the industrial sectors rather than the relevant markets.

The author dismisses this hypothesis (Much Ado about Nothing) and then provides a list of five hypotheses that are "not mutually exclusive" (p. 49). Only one of them is the idea of decreasing competition in the US. The remaining four start with the Rise of Superstar Firms hypothesis, meaning that the industry leaders increased productivity leads to their competitive advantage being such that there is a reallocation of resources toward these firms - obviously, welfare enhancing. Hypothesis of Lower Search Cost explains higher market concentration by subscribing to the insight of David Autor et al. (2017) that consumers, being more price-elastic due to the Internet searching and ubiquitous online purchasing, lead to winner-takes-all outcomes and elimination of inefficient competitors. If this is the case, higher market concentration is caused by increased, not decreased competition, like in the case of Walmart, mentioned earlier by the author.

The hypothesis of Globalization is straightforward: foreign competition puts downward pressure on the profit margins of domestic firms, increasing competition pressure, which eliminates inefficient domestic incumbents, either through merger or exit. This hypothesis is relevant only for tradables, but as the author points out, based on the findings of Matias Covarrubias, Germán Gutiérrez, and Thomas Philippon (2019), in the case of tradables, such as US manufacturing, once the foreign competition is included, the market concentration remains stable.

Finally, the author mentions the hypothesis of Intangible Assets, pointing out that these assets, such as intellectual property - patents and copyrights - can create barriers to entry. As demonstrated by Jonathan Haskel and Stian Westlake (2017), there has been a steep rise of the share of intangible assets and today they represent a substantial segment of total assets, so from that point of view this could be a reasonable explanation. It is a shame that the author does not mention the characteristics of the costs generated by the intangible assets, such as the costs of R&D that produces innovative patents. With the rise of these costs, economy of scale becomes more important and then it is beneficial that market concentration increases as there are more large firms and economy of scale occurs.

The author dismisses all hypotheses, save for the decreasing domestic competition and the rise of superstar firms, simply by saying that “firms in concentrating industries experience rising profit margins; firms in stable industries do not” (p. 56). Even if such a correlation exists - and that is big *if*, as Susanto Basu (2019) demonstrated substantial methodological problems in measuring profit margins - it hardly proves much, especially without information on marginal costs. Furthermore, increasing the share of fixed costs due to intangible assets means that an increase in profit margin does not imply an increase in profit rates, at least not by the same magnitude, as demonstrated by Jan De Loecker, Jan Eeckhout, and Gabriel Unger (2020).

Turning to more bad news, the author concludes that increased market concentration and increase in profit margins leads to a decline in investment and productivity. Although there is no data in the book on investment as the share of the GDP, which would substantiate this claim, the ostensible smoking gun is the investment gap: the investments are lower than the optimal level that will make Tobin’s q (the ratio of replacement costs of fixed assets and market value of the firm) equal to 1. The gap somewhat increased after the year 2000 (there is no data table in the book, only a figure). Yes, the theory is that optimal investment rate from the standpoint of allocation of resources should be the one that makes Tobin’s q equal to 1, but that insight is based on a substantial number of restrictive assumptions, some of which are mentioned in the book, and notoriously unreliable data on the replacement costs of the firm’s fixed assets - a problem that is aggravated by the increase of intangible assets.

As to the productivity decline, the author offers empirical results from his own research (Covarrubias, Gutiérrez, and Philippon 2019) that prior to 2000 there was a statistically significant relationship between market concentration (measured as the share of four largest firms in total revenues) and the growth of total factor productivity (TFP), and that this significance disappeared after 2000. Notwithstanding the considerable difficulty of measuring TFP, this is hardly evidence of anything. There is no doubt that the TFP growth rate in the American economy has been in decline in last several decades, but perhaps the reasons for that should be found in the character of the technological progress, as Robert J. Gordon (2016) did, and not in the market structures. PayPal co-founder Peter Thiel points out: “We wanted flying cars, instead we got 140 characters”. It is hardly market concentration that is to blame for that.

The first part of the book ends with the chapter “The Failure of Free Entry”. Surprisingly, a section of the chapter has the same title. The other surprise is that most of the chapter is about merger review, i.e. nothing to do with the free entry. A controversial contribution by John Kwoka (2015), containing the insight that the US merger control is not effective, is mentioned and a “sharp debate among antitrust experts” is acknowledged. The following sentence in the book is crucial: “Competition can take many forms, making IO (industrial organisation) a complicated filed in economics” (p. 91). Exactly! So, it would be helpful to stop confusing (increasing) market concentration with (decreasing) competition. The main insight in the failure of free entry section of the failure of free entry chapter is the seminal insight by George J. Stigler (1971) that economic regulation is designed primarily by incumbent firms to create barriers to entry, aimed at protecting their economic rents. Fine. But what is new about this? How to explain the fact that incumbent firms behaved the same when America was, according to the author, the land of free markets as they do now, when it is a land of declining competition. Philippon is silent about that.

He swiftly turns to Europe in the Part Two of the book (The European Experience). To keep a long story short, according to the author, the European Union is a success story. Prices are lower than in the US, profit margins and profit rates are lower, the share of labour in the national income is lower, etc. According to the author, this is because EU competition policy is more effective and barriers to entry have been decreasing. The reason for more effective competition policy is that the EC, i.e. Directorate-General for Competition (DG COMP), the supranational competition authority in the EU, is (more) independent than the two US enforcing authorities: the Federal Trade Commission (FTC) and the Department of Justice (DoJ). The author proposes a simple theory behind it: “politicians are more worried about the regulator being captured by the other country than they are attracted by the opportunity to capture the regulator themselves” (p. 142). Using the data on competition laws and their enforcement in Keith N. Hylton and Fei Deng (2007), the author concludes that the DG COMP, as a section of the European Commission, is more pro-competition than the DoJ and the FTC. The problem of this analysis is that it is the US courts that make decision in the competition law cases - not the DoJ or the FTC - and the independence of the US judiciary is beyond question.

Part Three of the book (Political Economy) starts at earnest with - lobbying. At the beginning of this part the author claims that: “American lobbying and politics might be different if the US did not have to maintain the largest military in the world” (p. 152). Be that as it may (the insight is not testable), the reader is puzzled: what does military spending have to do with competition? Nonetheless, this statement is an early warning about the content of this part of the book. There is no doubt that it is about political economy but the link between the insight in this part of the book and competition and its ostensible decline in the US in the last two decades is very difficult to establish.

The reader then goes through a profound discussion on lobbying, starting with the problems of measuring not only the level of lobbying, but also its impact. The author points out that lobbying is a zero-sum game and therefore it is very difficult to measure the impact. The point is that there are countervailing effects of lobbying by two sides with opposite interests, and the lobbying equilibrium - the outcome of relative lobbying pressure - can be achieved with substantially different levels of lobbying. Furthermore, the author considers every outcome of lobbying as economic rent, a dubious insight, and he emphasises that those who appropriate the rents do not have incentives to advertise it. The data on rents is virtually non-existent.

The author has no doubts that lobbying creates inefficiencies. Though he starts with a benign view of lobbying that: "it allows the sharing of relevant information between businesses, regulators and politicians" (p. 160), then the author subscribes to the view of Gene M. Grossman and Elhanan Helpman (2001) that lobbying is essentially rent seeking. The problem with this approach is that lobbying is rent seeking only if there is no regulation whatsoever, if there is an absolutely free market (Grossman and Helpman's argument is about free international trade), and no government intervention. Then and only then is it certain that every lobbying is aimed at creating some government invention that will produce rents, i.e. that it is rent seeking. Hence the conclusion that the policies advocated by lobbyist are necessarily inefficient is simply not correct.

The author mentions the case of taxi medallions as the outcome of lobbying and that more efficient competitors like Uber should pay a hypothetical lump-sum tax for free entry, as a kind of transfer to the less efficient incumbent. In the real-world, however, lump-sum tax is actually lobbying expenses paid by Uber to remove legal barriers to entry. Uber lobbies for less barriers to entry and more competition. In other words, Uber lobbying is not rent seeking but rent destroying. The same can apply to lobbying in many heavily regulated industries. After all, the banking industry lobbying is notorious for its quest for deregulation not for regulation.

One way or the other, what is it that has changed in the last twenty years? The amount of lobbying, according to the author. A figure (*alas*, no data table) shows that the lobbying expenditures in the US increased from around 1.5 billion USD in 2000 to around 3.5 billion USD in 2010 and then steadily declined to around 3.0 billion USD. Meanwhile, in Europe, according to the figure it has been teetering around 1.5 billion USD in the last few years. This is hardly a smoking gun for causality between lobbying and competition. The main reason is endogeneity, mentioned by the author himself a few pages earlier. The point is that the level of lobbying not only affects the level of regulation, but the level of regulation (actual or expected) also affects the level of lobbying. The more regulations, the more lobbying can be expected, as those firms who are adversely affected by the regulations have incentives to change or remove them through lobbying. As for the comparatively low level of lobbying in Europe, the data (www.lobbyfacts.eu) refers only to the lobbying in Brussels, i.e. targeting EU legislation and institutions, overlooking the lobbying at the member states level.

The author himself provides circumstantial evidence that it is the level regulation that affects lobbying, by providing the distribution of lobbying contributions of the US industries. Once again, a figure (no data table) demonstrates that real estate is the industry with the lowest contribution to the aggregate lobbying expenditures (less than 1 *per cent*) and finance is the industry with the highest contribution (nearly 15 *per cent*). The point is that the finance industry is the most regulated industry in the US; some would say not regulated enough, but that is beyond the point. There is an obvious positive correlation between the level of regulation and lobbying expenditures. Had the purpose of lobbying predominantly been aimed at increasing regulation, creation of barriers to entry and creating/protecting the rents, then this correlation would have been negative.

Knowing very well that aggregate data on lobbying hardly proves anything, the author focuses on the specific relation between lobbying and nonmerger competition law cases in the US. The finding is that "a doubling of lobbying expenditures to the DoJ and the FTC reduces the number of cases in a given industry by 9 *per cent*" (p. 173). Notwithstanding obvious methodological issues, such as how data on lobbying expenditures specifically aimed at the DoJ and the FTC was obtained, this finding raises several important questions. The first one is about the aim of lobbying: is it changing the regulatory rules, i.e. the legislation, or the way that the legislation is enforced. Up to this moment in the book, the author considers legislative changes. The point is that the US competition law statutory legislation has not been changed since 1976 (Hart-Scott-Rodino Act), and the last change was about merger control and compulsory pre-merger notification. Sub-statutory legislation, like horizontal merger guidelines, has changed regularly, but there is hardly any evidence that there was anti-competition drift in these changes.

As to the law enforcement, private enforcement of the competition law in the US is the most developed in the world and with an abundance of case seeking lawyers, so leniency of the DoJ and the FTC in bringing cases before courts does not mean much. Because, at the end of the day, the cases are decided by the courts, not by the federal administration, and that is what counts, not the number of cases brought by the DoJ and the FTC. And lobbying the courts - or rather their corruption - is not something that is common in the US. Even Richard M. Nixon did not try that.

The chapter on money and politics is highly informative. The reader learns a lot about US politics, but there are two open questions: (1) What is new (in last two decades)? (2) How does that affect competition law and its enforcement? As to the first question, *Citizen United v. FEC* happened. The decision of the US Supreme Court enables corporations to donate money to super PACs and they "make no contributions to candidates or parties, but they make independent expenditures in federal races to advocate the election or defeat of a specific candidate" (pp. 187-188). These contributions need not be effective all the time. The author offers some hard data in the table - finally, to the joy of the reader - demonstrating that in the 2016 elections Hillary Clinton raised 84 *per cent* more money than Donald Trump.

As to the link between money in politics and competition, the author skips the topic and goes back to lobbying, with the insight that "the ... (DC Comp) is entirely independent from actions taken by the European Parliament" (p. 194). As are the US courts from Capitol Hill, the reader might add. Furthermore, the author claims that "European (unlike American) *competition* authorities do not seem to subject to the same revolving-door effect" (p. 201, italics in the original). The revolving-door effect is well documented in the case of sectoral regulators, as regulator officials, after their tenure, find well-paid jobs in the industry that they regulated. This is intuitive, because banks, for example, all have incentive to hire people who actually regulated them, to help them in the process of regulatory compliance. Nonetheless, that mechanism works only in the case of specialised sectoral regulation, with an *ex ante* approach, not in the case of general-purpose, non-specialised regulators, with an *ex post* approach, such as competition authorities. No competition authority in the world, neither in Europe nor in America, is susceptible to the revolving-door effect.

Part Four of the book (An In-Depth Look at some Industries) focuses on three controversial US industries that are, according to the author, show-cases of "the same economic forces at play: lack of competition, barriers to entry, and lobbying" (p. 205). The chapter on finance industries starts with 101 explanation of the purpose of the financial industry, but then the author turns to the issue of entry barriers. "Entry in finance is … limited by heavy - and sometimes biased - regulations" (p. 216). A comparison with retail and Walmart case proves to be instructive, according to the author. "That's an important lesson: where retail store owners largely failed to prevent Walmart's expansion, the bankers prevailed" (p. 216). That is hardly an important lesson, because the banking sector and retail have nothing in common. The banking sector is prone to crisis and instability owing to its intrinsic features, like information asymmetry and substantial spill-over effects, because the assets of one bank are liabilities of another. That is the reason why insolvency spreads through the financial system like wildfire, threatening its stability. This is the rationale for capital adequacy control and for "too big to fail" actions, as demonstrated by Ben S. Bernanke, Timothy F. Geithner, and Henry M. Paulson (2019). Accordingly, that is also the rationale for banking regulation quite independently of banking lobbying. After all, it is the US banking/financial sector that is constantly lobbying for deregulation rather than regulation. The author's appeal "we need financial regulators who can stand up to the lobbies" (p. 222) disguises how notoriously controversial financial regulation is and how difficult it is to achieve a balance between free market and regulation in financial industry.

It is hard not to subscribe to the author's thesis that American health care is a self-made disaster, but is also hard to see how the decline of competition contributed to this. So, the reader turns to the stars: the GAFAMs (Google, Amazon, Facebook, Apple and Microsoft) - the leading global companies by market value. From the very beginning of the chapter, the author puts things in a historical perspective: "In the 1960s, AT&T alone represented more than 6 *per cent* of the market. Apple today is less than 3 *per cent*" (p. 245). So, the reader wonders what the problem is. It seems that it is in the dynamics, as the author points out that "the contribution of superstar firms to US productivity growth has decreased by over 40 *per cent* over the past twenty years" (p. 257). This is hardy fair to the new superstars, as their technology is completely different from that of the old ones like AT&T, ExxonMobile, General Electric and the like. Taking into account that productivity growth in countries on the technological frontier, like the US, depends only on technological progress, it is methodologically incorrect to compare productivity growth based on different technologies in different historical times. It is simply comparing apples and oranges. Strange enough, the author refers to Nicolas Bloom et al. (2020) and their insight that in modern time ideas are becoming harder to find. So, it is the general crisis of innovations that matters, not GAFAMs.

After asking the question "Is concentration required in the digital economy?" (p. 265) and answering "no", based on dismissing, without any in-depth analysis, economy of scale and network economies arguments, the author focuses on the question of dealing with GAFAMs, basically within the competition law framework. The first option is to limit their acquisitions by making merger control more stringent. In this way, GAFAMs would be prevented from acquiring start-ups that can be their potential competitors, like Facebook purchased Instagram and WhatsApp, or Google purchased Waze and DoubleClick. The author is aware that options for more stringent merger control - like lower turnover threshold for merger notification, threshold based on market value, and *ex post* merger control that creates substantial legal uncertainty - create more problems than potential benefits.

The other option considered is "Limiting the exercise of the GAFAMs' market power directly targets their dominance in some markets" (p. 275). This would, according to the author, in case of Facebook require interoperability (the ability to interconnect with other networks) and data portability (the ability to move one's data from one network to the other). Actually, data protection is the key issue for the author, as he considers that Google and Facebook exercise their market power by obtaining information on users, as they track them "on millions of websites, whether the users like it or not" (p. 275). A countermeasure for this would be an effective opt-out option, which would allow users to decide whether they want to let companies like Google and Facebook track them on other websites. But all these measures, as the author is aware, require regulatory control. In short, this is not the job for competition authorities, but for a sector specific regulator - one that does not exist yet.

Competition authorities, however, can trigger the break-up of the GAFAMs, like the DoJ did it in the case of AT&T. The author is sceptical about that option. It is not clear how that would proceed, especially taking into account that substantial scale and scope economies exist in the case of GAFAMs, not least due to network externalities. Hence, the author believes that this is the most controversial option. The author has no second thoughts: "The priority should be to define privacy regulation and property rights over digital data and give customers effective opt-out clauses" (p. 276). Be that as it may, this is not the role of competition law, but of some new statutory regulation. This is not to say that such a regulation is counterproductive, only to clarify that competition authorities should not be burdened with such a task.

In the conclusion of the book, following an unconvincing calculation of the US labour income annual loss due to the hypothetical increase of the profit margin from 5 to 10 *per cent*, then turns to the recommendations embodied in three principles. The first one is "Free Entry, Always, and Everywhere" (p. 294). A nice slogan, no doubt, and many academic economists would subscribe to it in most cases, though not for all industries. In the banking sector, for example, the balance between competition and regulated entry is key for both efficiency and stability. Furthermore, the reader is not quite sure whether the author is considering only legal barriers to entry, which stems from his idea that regulations that hinder the entry or growth of small firms should be removed, and also economic barriers to entry, which stems from his request that the regulators should be required to ensure that incumbents do not impede free markets. Finally, there is no hint of how the principle can be achieved, or at least what is the way forward towards the advocated free entry.

The second principle "Governments Should Make Mistakes Too" is very interesting, especially as it is not found in any part of the book. In short, governments should be creative in finding new approaches to regulation and new solutions involve mistakes, trial and error, and corrections. The author has no second thought that "In matters of economic policy in particular, perfect can be the enemy of the good" (p. 295). Even if the reader is convinced of this finding, even if governments are benevolent, the questions remain - such as how do governments identify mistakes, what are the incentives for them to do so, how fast can the government correct its mistake and, above all, how to isolate the process from the influence of private interest. Shedding more light on those issues would have been highly beneficial.

The third principle "Protect Transparency, Privacy and Data Ownership" is puzzling. Even if those values are relevant to competition, they can be relevant only in a few cases, such as Google and Facebook - the usual suspects for every bad outcome - not to the US economy with many sectors where this principle is hardly applicable. Again, there is no hint of how the principle can be achieved.

The author laments "Only two decades ago, the United States was effectively the land of free markets and a leader in deregulation and antitrust policy" (p. 288). His view is that the US is no longer what it used to be and that the decline of competition has been substantial in the last two decades. Even if this is true, and that is a big *if* because there is no convincing evidence in the book - the author fails to provide a smoking gun, a convincing explanation of causality that produced this change. Even if the reader subscribes to the insight of declining competition, the reason for this decline remains an academic mystery, as no hard evidence is provided on what led to America's transformation into the land of deficient competition in last twenty years.

This is not to say that the changes in the US economy in the last twenty years have been insubstantial. All the methodological difficulties notwithstanding, it seems that the profit margins and profit rates have increased. The share of capital in national income has increased and the share of labour has decreased. There is some empirical evidence, however unreliable, that market concentrations have increased. All these developments deserve meticulous exploration, scrupulous consideration and, above all, detailed and balanced analysis of their origins. Jumping to conclusions is counterproductive.

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