/ПРИКАЗИ

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Kaplow, Louis. 2024. *Rethinking Merger Analysis*. Cambridge, Mass.: The MIT Press, 253.

Is contemporary merger control, as an indispensable segment of competition law, stringent enough, or should it be tougher, especially in the case of horizontal mergers? Should more mergers be challenged and prohibited than has been the case in the past few decades? Is competition policy, especially merger control, in decline, and should this decline be blamed for the rise of big American companies, modern corporate superstars, and the ostensible surge of market power in that country? These are questions that have dominated recent debates on competition law, especially in the US.

There are some available answers to these questions. For example, the New Brandeis movement has provided an unequivocal answer: Yes, merger control in the US has been too lenient and must be more stringent. Since this movement dominated the US antitrust authorities during the Biden administration, it produced the 2023 Merger Guidelines (US Department of Justice and Federal Trade Commission 2023), aiming to make mergers more difficult to materialise. From January 2025 there is a new sheriff in town, Biden's New Brandeis protégés (e.g. Lina Khan and Jonathan Kanter) are out

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of office (FTC and US DoJ, respectively), but since the sheriff is thoroughly obsessed with rising tariffs – which he considers a Swiss army knife tool for all the US economic trouble, real or ostensible – nobody knows what will be the policy of the 47th POTUS administration regarding merger control and whether the MMGA (Make Mergers Great Again) movement will emerge now in the US, with a more friendly attitude towards merger control – nobody, including the POTUS himself.

Nonetheless, in his latest book, Louis Kaplow claims all these questions are wrong, whatever the answer may be; accordingly, any answer to these questions would be defective. The reason why all these questions are wrong, according to the author, is that the incumbent horizontal merger control approach in competition law is fundamentally and fatally flawed. Everything in that approach is defective. The problem cannot be solved by fine-tuning or slight adjustment of the existing mechanisms and procedures. The author demonstrated that the reigning emperor (Merger Control the 1st) is naked, revealing grim distortions of his body, i.e. profound and fundamental flaws in the existing horizontal merger control paradigm. This provocative book is an appeal for a paradigm change, nothing less; it is a sobering cry to turn upside down the horizontal merger control procedures of competition authorities throughout the world (although the author primarily focuses on the US).

There are many attempts by some colourful people to demonstrate that certain government activities are fundamentally flawed, starting with the UFO policy. These attempts have been rightfully swept away. That cannot be done in this case! Louis Kaplow is one of the globally most prominent scholars in the field of economic analysis of law, a professor of law & economics at the Harvard University School of Law, a person with a flamboyant intellect, one of the most prolific authors in the field of economic analysis of competition law (e.g. Kaplow 2013; Kaplow 2015; Kaplow 2017), and a person who vividly demonstrated his ability for creative thinking outside of the box (constructed by both the academia and competition authorities) and who has the intellectual courage to jump into unknown territory regardless of the consequences. Louis Kaplow's previous contributions have demonstrated that even if one does not agree with his every insight, one must painstakingly ponder the rationale for rejecting his proposition. The point is that it is such an author that produced the book claiming not that 'something is rotten in the state of Denmark' but rather that everything is rotten in the state of horizontal merger control. Such a contribution cannot be ignored or sidelined.

In the Introduction, the author promises: 'This book seeks, often relentlessly, to ask all relevant questions without regard to where the answers may lead or whether they can directly be implemented in merger review' (p. xi). Furthermore, the forthcoming change of paradigm attitude is easily spotted early on, as Kaplow points out that '[t]his book also departs from much merger policy advocacy by analyzing how optimally to order mergers, from the most dangerous to the most beneficial, rather than advocating for more or less stringency' (p. xi).

Chapter 1 ('Introduction') is beneficial for the reader because the author not only spells out a detailed and precise synopsis of the book but also provides his main points and succinct arguments. This chapter is a tasteful teaser for the reader from the field of industrial organisation (IO) and competition law & economics. In addition to these points and arguments, Kaplow shares a few essential insights, starting with his experience: 'Some of the conclusions presented here surprised me and conflict with my own prior writing and teaching. But often, before I began digging into a subject, I was unaware of the significance of some of the key questions, much less of what answers would emerge' (p. 2). The two guiding principles follow. 'A guiding principle in this enterprise has been not to be deterred from asking hard questions by the possible lack of immediate, practical answers. I aim throughout to advance knowledge as far as I can, sometimes covering a good distance but other times coming up short. On some fronts, there are fairly direct implications for policy and practice. On others, research agendas are outlined. Another guiding principle has been to choose subjects in light of my own comparative advantage. Mostly, the analysis here is theoretical and conceptual, the realm of my prior work' (pp. 2–3). Accordingly, this is very shrewd management of expectations. Those who began reading the book for the rich conceptual analysis, with no stone left unturned, and imaginative new paths to the essence of what competition law is not, but should be, are buckling up in their seats and are set in the top gear, since 'The analysis in this book is complementary to an increasingly sophisticated body of applied research in industrial organization' (p. 3). Those seeking immediate practical and, if possible, painless solutions in the area of competition law enforcement are somewhat disappointed and wonder whether they should remove the reading of this book from their to-do list. They should not. The task of this review is to convince them why they should stick to reading the book.

Chapter 2 is about the framework in which competition authorities decide on merger control, allowing or prohibiting/challenging horizontal mergers.[1] The framework should have relevant elements, and they must be arranged appropriately, in order to guide merger analysis, reduce errors, and avoid omissions. The chapter deals with 'decision analysis and information collection' and from the start points out the anomaly that 'expected values should guide decision-making even though, for example, U.S. legal rules are sometimes interpreted to hinge on probabilities alone' (p. 16). Based on the expected value rationale, unanimously accepted in economics, Kaplow points out that optimally a merger associated with a modest probability of significant anticompetitive effects and a high probability of negligible effects should be prohibited, whereas a merger with a larger probability of modest anticompetitive effects and a somewhat smaller probability of significant benefits should be allowed. Nonetheless, a rule that prohibits mergers if and only if they are more likely than not to be non-trivially anticompetitive would err in both of these cases.

A more significant and perhaps critical issue that emerges throughout the merger review decision-making process is the sequential siloing of the following sort. First, anticompetitive price effects of the merger are examined: if they are estimated to be negligible, the merger is cleared; if not, if anticompetitive price effects are evaluated to be significant, and only in that case, the efficiencies generated by the merger are explored, and entry conditions are considered. The process of information collection is also sequential siloing in the same way. At first glance, it may even seem reasonable: why collect information about efficiencies, if this information will possibly not be used (in the case of negligible anticompetitive price effects)? Nonetheless, Kaplow demonstrates that much information in merger investigations pertains directly to multiple factors. 'For example, better understanding the merging firms' cost functions illuminates competitive interactions, helps to identify demand, informs efficiencies, and bears on entry. [...] Once substantial information has been collected to analyze anticompetitive effects, one has already learned much about these other considerations, so it would be irrational not to consider and revise those estimates as well' (p. 18).

Kaplow points out diminishing returns in particular avenues of inquiry – the optimal next steps will often alternate between different issues, e.g. anticompetitive effects and efficiencies. Moreover, the optimal next steps are

[1] The EU-style competition law encompasses competition authorities that can prohibit mergers. In US-style jurisdictions, competition authorities can only challenge the merger, and the (first instance) decision rests with the courts.

endogenous to what has been learned thus far. ‘No one-size-fits-all rubric makes sense, much less one that purports to front-load anticompetitive effects, consider them exhaustively, and only then (if they are sufficiently established) turn to efficiencies. Recalling that much information pertains to both (and to entry), and that many individual pieces can be understood only with multiple considerations in mind, we can see that optimal information collection and decision-making are inherently intertwined processes that depart substantially from a sequential, soloed approach’ (p. 19). Accordingly, the only way to optimise the information collection process is to equate marginal benefits and marginal costs, and that can be achieved only if all information pertinent to the merger is gathered simultaneously. According to Kaplow, this ‘broad front’ approach is essential for many reasons, not only because of the economy of information gathering, but also because it is this point where it all begins.

The most crucial topic of Chapter 2 deals with what Kapow refers to as neoclassical merger motives and the merging firms’ rationality constraint. ‘Firms are assumed to propose only jointly profitable mergers, and profits are understood to arise from some combination of anticompetitive effects and efficiencies’ (p. 22). This claim is undoubtedly true – it is almost trivial – but it is very often forgotten. It reinforces the need to collect simultaneously all the information about the merger – regardless of whether they are about anticompetitive (price) effects (including entry conditions) or about efficiencies – because even in the early stages of analysis, as Kaplow argues, if data points to limited increases in efficiencies, the motive of the merger seems to be anticompetitive effects, therefore more information about the price effects is needed. Furthermore, the rationality constraint prevents firms from mergers that, due to easy entry (i.e. low barriers to entry), will not generate additional profit from anticompetitive effects that are not higher than the merger’s transaction costs: ‘anticipation of post-merger entry may render unprofitable a merger proposal motivated primarily by the prospect of anti-competitive effects’ (p. 23). The reader ponders: so much for easy entry and a remedy against anticompetitive mergers – the merging firms already consumed it due to their rationality constraints.

Kaplow emphasises that not all mergers are undertaken due to neoclassical merger motives, and the merging firms’ rationality constraint should be applied cautiously in such cases. Merger proposals can arise as a consequence of agency problems (empire-building), behavioural infirmities (optimism bias and hubris), market pricing imperfections (an under-priced target or overvalued securities used to finance an acquisition), or tax savings. The rule of thumb suggested by the author is clear. ‘If less profitability is required, expected anticompetitive effects and efficiencies are both lower’

(p. 24). Unfortunately, there is no rule of thumb provided for distinguishing one type of merger from the other. Nonetheless, Kaplow provides a list of questions that competition authorities should ask if they have second thoughts about the merger evaluation because non-neoclassical merger motives compromised the merging firms' rationality constraint, at least to some degree.

The rest of the chapter deals with bias in collecting and structuring information about the merger, which downplays the information about entry, exit, and investments, especially from the long run perspective, focusing on the short run only, although all these phenomena (entry, exit, investments, in addition to technological progress and innovation) are long run issues. Furthermore, information collection is focused on the partial, single market equilibrium. However, it is reasonable to assume that the merger effects, as well as the effects of the decision not to clear the merger, are disseminated throughout the entire economy, therefore general equilibrium consideration is desirable, regardless of the practical issues related to this approach.

Chapter 3 discusses what most of the competition authorities are concerned with in the merger control exercise: price effects and market definition. Kaplow claims that their concern about the price effects of horizontal mergers is well placed, but their approach to this matter is entirely flawed. For those not involved in competition law, such as micro economists with a specialisation far from this area, a reasonable, common-sense question is: Why are price effects coupled with the market definition? Kaplow profoundly understands this anxiety, which is precisely the starting point of his devastating criticism of the price effects dogma incumbent among competition authorities.

In short, this dogma consists of two sequences. First, the relevant market should be defined. Second, inferences of price effects are made based on the pre– and post-merger market share of the merging firms, embodied in market concentrations, and structural presumptions.[2] Within such a dogma, the first step must be a relevant market definition; otherwise, market shares cannot be calculated, and market concentration cannot be figured out.

2 The notion of 'structural presumption' is a parlance typical of US-style competition law, which Kaplow mainly refers to. Nonetheless, the very content of the procedure is the same in the other jurisdiction, regardless of the parlance. In EU competition law stipulates that '[m]arket shares and concentration levels provide useful first indications of the market structure and of the competitive importance of both the merging parties and their competitors' (EC 2004). This is nothing but a structural presumption.

The problem with the concept of relevant market, Kaplow points out, is that it does not exist in IO. There is no economic theory of the relevant market.[3] There is hardly any economic meaning to the notion. It is merely a fiction created by the competition authorities that enables them to adhere to the dogma. Furthermore, the legislators have not developed that fiction – the administrators have.[4]

The next issue is how the relevant market is defined. The regular procedure is to apply the hypothetical monopolist test (HMT), starting with an arbitrarily selected market (in terms of product and geographic area) and answering the question whether a hypothetical monopolist could sustain at least a small but significant and non-transitory increase in price (SSNIP), of 5 to 10 per cent, i.e. whether such a price increase would increase or at least not decrease its profit.[5] If the answer is yes, it is inferred that all encompassed products and geographic areas are close substitutes, and this market is worth monopolising. Furthermore, with a positive answer to the question, the market is then enlarged to include new products and geographic areas, and the process continues until the answer to the question is no. Suppose the relevant market is defined without enlargement, in that case Kaplow refers to such a market as 'narrow'; if enlargement (or rather several steps of it) happens in the process of relevant market definition,

[3] Perhaps that was one of the reasons why the 2010 US Horizontal Merger Guidelines abandoned market definition as an indispensable step in horizontal merger analysis. Nonetheless, the 2023 US Merger Guidelines (which handle both horizontal and vertical mergers) brought the relevant market back into the game. Prominent IO academic economists (Shapiro 2010, Shapiro 2024) consider the 2023 US Merger Guidelines a huge step back from the 2010 US Horizontal Merger Guidelines (US DoJ, FTC 2010). A prominent US economist, a panellist at the 2024 CRESSE Conference (Chania, Crete, Greece), pointed out on the sidelines of his presentation that (quoting him from memory): 'The US 2023 Merger guidelines are like a blueprint for a Mars rocket without [relying on insights of] physics'. There was a consensus about that specific shortcoming of the 2023 US Merger Guidelines in a topical issue of the 2024 *Review of Industrial Organisation* (Vol. 65, Issue 1, Special issue on the 2023 Merger Guidelines).

[4] The relevant market is not mentioned in key pieces of legislation of the two most important competition laws: the US competition law (Sherman Act and Clayton Act) and the EU competition law (Treaty on the Functioning of the European Union). Hence, it is not a concept specified by the statutes but by sub-statutory texts, such as the EC Notice on the Definition of Relevant Market for the Purposes of Community Competition Law, *OJ* C372/5 [1998].

[5] For example, 2023 US Merger Guidelines stipulate that '[w]hen considering price, the Agencies will often use a SSNIP of five percent of the price charged by firms for the products or services to which the merging firms contribute value' (US DoJ, FTC 2023, 43). Details on the procedure of applying HMT in the EU competition law are provided by Bishop and Walker (2010).

Kaplow refers to such a market as ‘broad’. He associates narrow relevant markets with homogeneous goods and broad markets with heterogeneous goods.[6]

The main problem with applying HMT, at least if it is used properly is that it must be based on the crucial information for price effects – the price elasticity of demand.[7] Instead of directly using this information to evaluate the proposed merger's price effects – for example, the calculation diversion ratio – the crucial quantitative information on demand elasticity is utterly lost in defining the relevant market. Accordingly, instead of estimating the price effects directly using information on the elasticity of demand, especially cross-elasticity, the competition authorities are now turning to the calculation of the market shares of firms and inferring conclusions on the proposed merger's price effects based on the post-merger market concentration of firms and its change due to the merger, measured by the HHI and the ΔHHI.[8]

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[6] In both the book and this review, ‘goods’ is shorthand for both goods and services, i.e. products. Furthermore, Kaplow's assumption that ‘broad’ markets are associated with heterogeneous goods is reasonable, but it seems that ‘narrow’ markets should not necessarily be associated with homogeneous goods. Everything depends on the arbitrarily chosen initial market on which SSNIP is applied. Accordingly, even ‘narrow’ markets can be markets with heterogeneous goods.

[7] There is a critical level of price elasticity of demand that enables monopolisation. If the price elasticity is above that critical level, which depends on the shape of the demand curve/function, monopolisation is not feasible because an increase in price will produce a decrease in the firm's revenues and profit, i.e. increased revenues due to a higher price will not offset decreased revenues due to lost sales. Furthermore, the more inelastic the demand, the bigger the price effects of mergers due to a decrease in price competition will be. This is IO 101 (Carlton, Perloff 2015).

[8] HHI stands for Herfindahl–Hirschman index, which is calculated as the sum of the squares of the market shares of all the firms in the (relevant) market. If there is a monopoly, HHI is 1 or 10,000, because it is multiplied by 10,000 in applied competition law analysis. If there is an infinite number of firms, a feature of perfect competition, HHI tends towards 0. HHI is essentially a measure of how far a market is from being a perfectly competitive market with an atomised firm structure – the higher HHI, the further away the market structure is from the structure of the market in perfect competition. This makes the HHI completely counterproductive as a tool of competition law, since a perfectly competitive market – a concept based on very restrictive assumptions – exists only in microeconomic textbooks, and it is used as a theoretical reference point. Accordingly, it is senseless to set the aim of competition policy to move market structures closer to perfect competition.

The rationale is that the higher both the HHI and the ΔHHI, the lower the presumed competition, which is a structural presumption, based on the SCP doctrine.[9]

The crucial problem with this approach is that the information on elasticity of demand is entirely lost, even though it is essential for estimating the magnitude of the price effects. Kaplow rightfully points out that 'If the post-merger HHI is 3000 and the ΔHHI is 300, is the typically imagined price increase 18%, 1.8%, or 0.18%? We do not know even within an order of magnitude' (p. 45). The necessary, although not sufficient, condition for estimating the price increase due to the proposed merger is demand elasticity, however, the information on that is lost after the relevant market definition, or perhaps never obtained in such an approach.[10]

Kaplow analyses three cases of price effects of mergers. He starts with unilateral effects in the case of homogeneous goods, pointing out that the Cournot quantity competition model can be used to analyse those effects and is often used by competition authorities. The use of that model, under some restrictive assumptions, makes 'the output-weighted mark-up (share-weighted Lerner index) equals HHI/|ε|, where ε is the market elasticity of

[9] SCP stands for Structure – Conduit – Performance. According to this doctrine, completely rejected by modern IO, market structure is exogenous, and the causality goes from the market structure via the firms' conduit to their performance – the more concentrated the market structure, the more anticompetitive the behaviour, and the greater the market power, i.e. price effects. Nonetheless, Kaplow points out that modern IO considers market structure endogenous, since due to the market exit of less competitive firms (selection function of the market), efficient and competitive firms increase their share. The share of Walmart in the US retail sector is a typical case of a firm's endogenous market share. Accordingly, higher market concentration can be a testimony of strong competitive pressure on the market, as only a few (efficient) firms survive. In short, the SCP doctrine, embodied in the structural presumption that competition authorities have subscribed to (that low market concentration creates strong competitive pressure), is misleading.

[10] Perhaps it does not exist, i.e. the information was not obtained in the first place, depending on how the HMT was carried out. With some prior knowledge, it is only a segment of the demand function that is considered. Only a rough estimate of the demand price elasticity may be used, in only some segment of the demand function (local in terms of quantity). Therefore, competition authority staff may access the hard drive to retrieve the information, but it may not be available because the price elasticity of demand is not estimated for the entire function, especially taking into account that the demand elasticity is variable, as the coefficient of the price elasticity of demand increases (in absolute values, the coefficient is negative by definition) with the increase in price.

demand for the homogeneous good' (p. 50).[11] Hence, in this very specific case, there is at least some use of HHI, though supplemented with the coefficient of elasticity of demand.[12] Nonetheless, Kaplow points out that the Cournot quantity competition can easily deteriorate to the Bertrand price competition model, which drives prices to marginal costs; hence, none of the results based on the Cournot quantity competition model are valid.

As to the unilateral merger effect in the case of heterogeneous goods, according to the author, there is a consensus within the IO community that the Cournot quantity competition model is not applicable, meaning that any measure of market concentration, e.g. HHI, is irrelevant. Accordingly, the relevant market definition is redundant. With the Bertrand price competition model as the basis for the merger price effects consideration, the only relevant information is the diversion ratio, based on the price cross elasticities of demand for the good produced by merging firms. Nobody needs a market definition for calculating diversion ratios.

As to the coordinated merger effects, the effects that facilitate collusion in the market, Kaplow has no second thoughts. 'Perhaps the most systematic effect that facilitates coordination is that a merger of two firms reduces the number of firms by one, which lightens the coordination burden and reduces the number of possible sources of disagreement. Once one determines the premerger number of significant competitors and knows the post-merger reduction (here taken to be one), the HHI and ΔHHI provide no

[11] The Lerner index (Lerner 1934) is the relative price–cost margin, i.e. the (positive) gap between the price and the marginal costs divided by the price. It is often used as an indicator of market power. Nonetheless, careful reading of Lerner's contribution demonstrates that inelastic residual demand, i.e. a negatively sloped residual demand curve, is a necessary condition for market power. In short, the positive gap between price and marginal cost can be the consequence of many things that are not due to market power. For example, since firms in the same market are heterogeneous, with different cost functions, due to the absolute cost advantage (lower average costs), some firms obtain significant price–cost margins, while others only break even, with a zero price–cost margin, although there is no market power of any firm in the market at all, and the price is exogenous to all the firms.

[12] In short, under a set of assumptions, comparing pre– and post-merger HHI, i.e. ΔHHI, divided by the absolute value of ε, can provide some indication of the order of magnitude of the price effects of the merger. As Kaplow points out, HHI itself is irrelevant, and its change (ΔHHI) alone cannot provide that information on price effects without the price elasticity of demand. Accordingly, the critical values of HHI and ΔHHI in the merger guidelines are counterproductive, because mergers that produce both values above the critical values can result in smaller price effects than those below the critical values, as everything depends on elasticity. The critical values of HHI and ΔHHI without demand elasticity are the curse of the structural presumption approach, especially taking into account how these presumptions are important for the final decision on the merger (Hovenkamp, Shapiro 2018).

further illumination' (p. 71). The author points out numerous difficulties in analysing the possible coordinated effects of the proposed merger. Still, one thing is certain – there is no need for a relevant market definition for any of those analyses.

Taking all this evidence into account, is it reasonable to conclude that the relevant market paradigm is useless? Unfortunately, it is much worse than that – it is counterproductive for two reasons. The first is that an enormous amount of energy and resources is allocated to defining the relevant market in every merger review. The merging parties' experts do their best to demonstrate that the relevant market is 'broad' so the market shares of the merged firms would be lower, the HHI and ΔHHI would hopefully be below the thresholds, and the merger would be cleared. The broader the relevant market is, the better it is for the merging party. If the competition authority aims to challenge or prohibit the merger because its prior belief that the merger is anticompetitive (based on other evidence or no evidence at all), the authority's experts will do their best to demonstrated that the relevant market is 'narrow', meaning that the market shares of the merged firms would be higher, the HHI and ΔHHI would hopefully be above the thresholds, and according to structural presumptions – the merger would be *lege artis* challenged or prohibited. Hence, neither party is willing to ascertain the facts, i.e. to uncover the truth. The battle of the experts is a clash solely over an irrelevant issue, regardless of the facts, instead of focusing on allocating resources to gather information about more important issues, such as the price effects of the merger, and analyse them to determine their magnitude.[13]

The second point is that the entire process of decision-making is reversed and ridiculed. Hence, the price effects analysis ends in the debate on the relevant market definition. 'And if it is thought that a merger should be blocked, then a case can be brought that may embody, at least to some extent, a reverse-engineered approach to market definition. Specifically, a narrow market will be advanced, in which the market shares are high, satisfying the merger guidelines' thresholds and court precedents. And judges deciding cases may, at least implicitly, proceed likewise, finding a narrow market definition more convincing when they believe (based on

[13] In his review of the book, one of the most prominent IO academics honestly testifies to supporting this view. 'I confess to having done so myself on more than one occasion' (Shapiro 2025b, 1102).

other evidence) that the merger is indeed anticompetitive, but adopting a broad market definition when they are unconvinced of the merger's alleged anticompetitive effects' (p. 80).[14]

Chapter 4 of the book focuses on efficiencies. Kaplow's primary recommendation is an integrated assessment of efficiencies and anticompetitive effects in the merger control analysis. Something that is far from the incumbent procedures of competition authorities. For the author, due to the previously mentioned rationality constraint for the merging firms, merger decision-making inevitably involves the balancing of anticompetitive effects and efficiencies – and that must be done simultaneously. 'Whether at an early screening stage in an agency or various subsequent points, one obviously should consider both sides. Importantly, suppose efficiencies in a given merger seem unlikely to be significant. In that case, the anticompetitive effects required to justify blocking the deal should be lower, and conversely if efficiencies seem likely to be large' (p. 108).

For the author, there is a nexus between efficiencies and anticompetitive price effects. Still, the incumbent merger control policy insists that only the efficiencies must be merger-specific, because that policy takes for granted the merger specificity of the anticompetitive effects. Kaplow points out that symmetry must be established. 'Then we are back to the case of a direct trade-off because both the efficiencies and anticompetitive effects are not only merger specific but, more importantly, arise in the same part of the proposed combination' (p. 83). To clarify what is merger-specific, all available options should be specified, which is not common practice. 'Yet another important dimension that has received little attention in this regard concerns the alternatives to merger, whether they involve internal expansion or contractual arrangements between the merging firms short of merger. After all, inquiries into merger specificity entail comparison to a but-for world without the merger (which often differs from the status quo ante), so it is necessary to both characterize and analyse the hypothesized

[14] The same reversed process has been spotted in the EU competition law in the case of application of Article 102 of the TFEU. First, the EC decides (based on whatever information and evidence) that an abuse of dominant position took place, then it is necessary to establish that the abusing firm (an 'undertaking' in the EC parlance) has a dominant position, which is specified by its market share. Accordingly, the narrow relevant market is defined by the EC, as narrowly as feasible, because the threshold for dominance is then easier to reach (Cini, McGowan 1998). Naturally, legal representatives of the dominant firm insist on a broad relevant market definition, as broad as feasible. In short, there is no incentive for any party to ask what is the true definition of the relevant market.

non-merger scenario' (p. 83). Accordingly, Kaplow stands for a proper counterfactual analysis that would demonstrate the effects of all available options.

Since specificity has been clarified, Kaplow moves to the relationship between merger specificity and the theory of the firm. Quite a reasonable move, because it was Coase (1937) who launched the modern economic study of the theory of the firm by posing provocative questions about comparative transaction costs of exchanges that take place within or between firms.[15] In that sense, the merger-specific efficiencies question can be reformatted as whether the merger would substantially decrease transaction costs. 'Legal form – here, the distinction between contracts and firms – cannot guide our analysis; we need to examine the underlying substance' (p. 88). Kaplow provides the nuts and bolts of that substance, and the reader realises that none of those exist in merger decisions, whether to clear or to block the merger, understanding Kaplow's puzzlement about the divorce of competition law and theory of the firm. At the end of the day, contracts (market) and the firms, in Coasian tradition, are nothing but two alternative ways to arrange business activities and exchanges. Kaplow refers to Williamson (1975) and his distinction between the two. 'The marketplace is taken to entail one amalgam of attributes: high-powered incentives, specified obligations, and formal adjudication of disagreements through the legal system. By contrast, firms are taken to employ a different package of properties: low-powered incentives, discretionary authority, and their own informal systems of dispute resolution. These differences are regarded not as independent choices but rather as constituting complementary bundles' (p. 90).

It is precisely the theory of the firm that enables Kaplow to point out that most of the efficiencies in the horizontal mergers involve synergies that involve the combination of complementary assets. In short, most of

[15] According to Coase (1937) and his model, in a world with zero transaction costs, there would be no firms whatsoever and all exchanges and all cooperation would be done on the market, governed by contracts. Coase's contribution was neglected for decades, but then in the 1970s, a surge of contributions emerged, basically following up on Coase's approach (Alchian, Demsetz 1972; Williamson 1975; Jensen, Meckling 1976; Grossman, Hart 1986; Hart, Moore, 1990). The consideration of the firm as a substitute for contracts cleared the way for the economic theory of incomplete contracts (Hart 2009; Hart 2017). Three of these authors won the Nobel Prize for economic sciences (officially the *Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel*): Ronald Coase (1991), Oliver Williamson (2009), and Oliver Hart (2016). Kaplow points out several times in the book, that the competition authorities completely neglect the contribution of those Nobel prize laureates, which is crucial for understanding the merger-specificity of efficiencies.

the merger-specific efficiencies should be expected from the economies of scope, not scale, since 'the synergy from combining firms arises from the complementary, vertically related features rather than concerning the core asset subject to scale economies. Arguably, most types of efficiencies in horizontal mergers are vertical in character. Complex contracts designed to coordinate economic activities that might better be conducted inside a single firm typically involve complementary activities and assets rather than identical ones. When horizontal mergers involve two firms that do not each consist solely of the same type of stand-alone asset, there will be vertical elements. When those features are entangled with the purely horizontal ones, as they often are (which is why they are already conducted within at least one of the merging firms), there may be vertical efficiencies from horizontal integration' (p. 95).

This fresh view on the merger-specific efficiencies is based on a fact that is frequently neglected in competition law enforcement – that the firms successfully operating in the market are heterogeneous, i.e. that it is not one-size-fits-all when it comes to firms. Accordingly, the merger of two heterogeneous, distinctive firms creates something new that can be more efficient than the two separate old firms. This is not to say that economy of scale is irrelevant, especially when technological progress changes the cost function in a way that increases the share of fixed costs. In such conditions, a merger is a reasonable reaction of the industry to technological change and business challenges, or rather opportunities, because of the efficiencies that will be achieved. This is the case of what Kaplow calls 'pure economy of scale'.

As to the usual complaint of the competition authorities that it is difficult to obtain relevant evidence, which can be understood as the reason for them to avoid analysing efficiencies, Kaplow provides a straightforward answer: 'if it is harder to obtain and analyse evidence on efficiencies than on anticompetitive effects (to the extent that the two are separable), optimal information collection, guided by consideration of the diagnosticity-to-cost ratio, will reflect that in any event' (p. 111). This is practical advice to the competition authorities, part of Kaplow's advocacy of the approach aimed at a nexus between efficiencies and anticompetitive effects.

Chapter 5 of the book is about entry. At the very start of the consideration, the author points out that entry may, but not necessarily, enhance variety and innovation, however, it also (necessarily) consumes additional resources. This is a welcome departure from the conventional wisdom in the competition authorities community that entry is always good.[16]

There are two general cases of entry that Kaplow considers. The first case is ex post entry, something that competition authorities are so familiar with. Their conventional wisdom is that if barriers to entry are low enough, a merger with substantial anticompetitive price effects should be allowed, because post-merger economic profit will attract new entry, the number of competitors (structural presumption again) will be at least restored, consequently prices will move downwards, and there will be no more economic, only normal profit, as was the case before the merger. In short, if there are low barriers to entry, even an anticompetitive merger should be allowed, because its price effects will be zero or negligible. Low barriers to entry and new entries are considered a panacea by competition authorities.

Kaplow convincingly undermines this logic. The consideration, he believes, should start with the rationality constraint of the merging firms. If the barriers to entry are low, and if the new entry will siphon off all economic profit generated by the merger, i.e. profit surplus due to the anticompetitive price effects, then they do not have any incentive to start the merger in the first place, because such a move involves transactional costs and no future excess profit will cover these costs. If competition authorities are aware of low barriers to entry, the so are the firms considering a merger, with perhaps even better information, as they have a stronger incentive to acquire it. In short, such a merger will never be proposed, let alone completed.

If the firms decide to merge and propose (i.e., notify) the merger to the competition authority despite low barriers to entry, it means they expect to generate some surplus profit even after new entry. Kaplow believes that there can be two rationales for such behaviour. The first is that the new entry will not completely undo the anticompetitive price effects, i.e. the prices will be higher after the post-merger new entry compared to the pre-merger equilibrium. The evidence supporting this view is that the firm that

16 This difference in view is apparently the consequence of the difference between Kaplow's general equilibrium approach and the competition authorities' partial equilibrium obsession. A trained economist has no second thoughts on the superiority of the general equilibrium approach, which takes into account all welfare effects. To what extent this is feasible and how it can be achieved – are different questions altogether.

would enter the market after the merger did not do so before the merger – the market price was too low for that firm to break even at the time. This is testimony that new entry is less efficient than the merging firms, even before the merger, which can enhance the efficiency of the merging firms. Again, firms are heterogeneous, with distinctive cost functions, meaning different levels of production efficiency. Without merger, the market provides selection efficiency, keeping inefficient firms out of the market. In short, new entry in these conditions is not socially optimal, i.e. not desirable regarding the welfare from the general equilibrium point of view – in these circumstances, a new entry is the wrong remedy. Yet, it is very likely that the competition authorities would clear such a merger, because of the prospect of swift entry.

The second rationale for firms to go ahead with a merger, despite low barriers to entry, is that they expect a merger-specific efficiency increase, for example, due to the economy of scope. In such cases, the profit of the merged firm would increase both due to the price effects and due to the decreased average costs – a result of increased efficiency.[17] Both the merged firm and the entry would appropriate profit, but the profit rate of the merged firm would be significantly higher. The overall welfare effect of a cleared merger in this situation would depend on the relative magnitude of the loss due to inefficient entry and the gains due to increased efficiency of the merged firm. The reader is not convinced that the competition authorities recognise this trade off (regardless of the outcome), sticking to the new entry as a panacea in the merger evaluation cases.

The second case is ex ante entries – something Kaplow claims competition authorities have been neglectful of. The point is that many startups, many new companies are created with the strategic aim to be acquired one day – nothing else. In many instances, the entrepreneurs starting up the venture are motivated solely by the prospect of a subsequent buyout premium. Merger control regime substantially influences the prospects of such a buyout and, in that way, creates incentives relevant for investment decisions and for starting up businesses. Kaplow analyses the welfare effects of different merger control regimes and the incentives they inevitably create ex ante for investors and entrepreneurs. 'In simple settings – with homogeneous goods, a dominant firm, and some other assumptions – the prospect of entry for buyout tends to be inefficient. In such cases, a tough merger policy may raise

[17] Caradonna, Miler and Sheu (2025) provide a model of such a merger with differentiated, i.e. heterogeneous goods, with some reference to the T-Mobile–Sprint merger.

social welfare by discouraging inefficient entry' (p. 118).[18] Nonetheless, Kaplow points out that many of these start-ups are very innovative, and they contribute to product variety, innovation, and efficiencies – some of them due to merger synergies.[19] Without ex ante incentives, embodied in prospects for premium buyout in due course, there would hardly be such innovative start-ups. Consequently, dynamic efficiency would be compromised.

At the same time, Kaplow emphasises that acquisitions by dominant incumbents may extinguish disruptive threats posed by nascent entrants. According to the author, in reviewing subsequent acquisitions, competition authorities will naturally be inclined to take the target's existence and capabilities as a given – it is exogenous. In short, these authorities do not even consider that these start-ups are endogenous to the merger-control policy, i.e. that the merger-control policy creates incentives for these investments. Furthermore, by labelling these buyouts as 'killer acquisitions', the competition authorities emphasise the motives of the dominant incumbent firm for removing the prospective competitive threat posed by the nascent entrants, rather than the efficiencies that can materialise in such a buyout and the efficiencies that were generated by the sheer start-up beginning operation.[20] If competition authorities do not take into account ex ante mergers and do not accept that their harsh merger control policies can undermine innovative start-ups, the consequences of the innovation process and economic growth based on increased total factor productivity could be devastating, especially in countries that are close to the technological frontier (primarily the USA) and whose economic growth is predominantly based on productivity increase.

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18 This insight by Kaplow is a perfect example of the application of general rather than partial equilibrium thinking. The opportunity cost of the investment in one industry is the forgone investment of exactly these resources in the other.

19 In a very well researched review article, Shapiro (2025a) provides ample evidence to support the thesis that most of these start-ups are in the area of high-tech industries, that they are innovative and that the entrepreneurs who start these projects do not believe that they will have sufficient resources to finish the job, i.e. to make the innovation marketable. That should be a job for well-established, big, dominant firms with ample resources and experience in this segment of operations.

20 This was precisely the motivation of the FTC to open the case against Facebook (Meta) for the acquisition of WhatsApp and challenge the merger two years after the very same institutions cleared the acquisitions (Begović, Ilić 2024). In the FTC complaint, there is no word about efficiencies due to synergies that materialised after the merger. To be fair to the competition authorities, the notion of 'killer acquisitions' holds ground in the academic community (Cunningham, Ederer, Ma 2021).

After the crescendo in the three chapters dedicated to the trinity of competition law – price effects, efficiencies, and entry – the tension in the book inevitably plunges. That is not to say that the following chapters do not deal with relevant issues of horizontal merger control, but the pressure of the steam in the engine slowly goes down. Chapter 6 ('Priors, Predictions, and Presumptions') addresses the difficulty of analysing anticompetitive effects, efficiencies, and entry in the review of proposed mergers, because of information limitations and the inherent challenges of prediction. The chapter demonstrates that Kaplow understands very well the position of competition authorities tasked with merger control and the obstacles that they face. The bottom line is that information-hungry competition authorities should seek different types of evidence that can illuminate merger review: industry studies, merger retrospectives, merger simulations, stock market event studies, and industry expertise – with all their strengths and limitations (well explored in the book). No rules of the thumb, no shortcuts, only 'toil, tears and sweat' – the good news is that blood is missing.

Chapter 7 is about institutions, their internal organisation and operations regarding merger control. Most of the chapter is, quite appropriately, about competition authorities, as Kaplow considers the enhancement of their skill sets through greater expertise in business operations and organisation, as well as knowledge of particular industries. The author also examines whether greater use of ex post merger review may be valuable, given the difficulty of predicting the effects of mergers, particularly in the case of acquisitions of nascent entrants in rapidly changing industries. Since the book predominantly focuses on the US competition law (antitrust in said jurisdiction's parlance), in which the courts decide to block or clear mergers, this chapter briefly discusses how the US courts could better adjudicate merger challenges by drawing more directly on the expertise of all types to help structure litigation in a manner that would better engage with battling experts, business witnesses, and other evidence. Since there is substantial wisdom in these recommendations, they can be applied in other jurisdictions, especially for the courts as a second instance in the legal process, where a competition authority decides about the merger in the first instance. Finally, in this chapter, Kaplow provides some comments on the division of regulatory labour between competition authorities and other government entities – a topic especially relevant for regulated industries.

Chapter 8 discusses the objectives of competition law. Perhaps it should have come earlier, but this is not a bad way to conclude the book. The first dilemma regarding the objectives is the choice between consumer and total welfare standards. This is not the first time that this dilemma has been considered. Still, Kaplow provides the background of the debate and suggests that solving this dilemma may depend on the outcome of some other dilemmas regarding the objectives of the competition law. The conventional wisdom among academic economists – based on the welfare economics insight – is that the total welfare standard is superior, but the issue of (re) distribution has produced a consumer surplus standard.[21] That becomes clear with the insight that 'consumer welfare might better be viewed as total welfare minus the producer surplus of the parties under scrutiny' (p. 189). Kaplow has no second thoughts about income distribution as an objective of competition law. 'Both institutional considerations and the principle that instruments should be appropriately matched to targets favor a total welfare approach. Competition agencies, like environmental protection agencies, occupational health and safety agencies, and many others, do not have expertise in income distribution, do not in fact typically analyze distributive effects, do not state social welfare functions or offer preferred estimates of labor supply elasticities in their published or internal rules, and do not seek to coordinate their regulatory efforts with the activities of the income tax and transfer system, including social insurance' (p. 189). Nonetheless, most jurisdictions, as well as some economists,[22] stick to consumer welfare, rather than the total welfare standard.[23]

[21] Kaplow reminds that the rise of a consumer welfare standard in US case law, notably that of the Supreme Court, reflects an interesting path dependence and some confusion. Bork (1978) advanced consumer welfare but actually meant total welfare; the view being attacked was one that sacrificed both consumer and total welfare to the protection of small producers. But his misnamed term became adopted, Kaplow points out, no doubt because of the appeal of the consumer welfare standard to less sophisticated audiences and subsequent increasing concerns about economic inequality. Accordingly, the consumer welfare standard in the literal sense has become increasingly common.

[22] Pittman (2007) provides arguments for using consumer welfare standards for merger control, especially having in mind US mergers. Depending on the assumptions, this standard can prevent some of the redistribution in the aftermath of the merger, provided that total welfare standard is applied.

[23] It is taken for granted that EU competition law applies the consumer welfare standard. Nonetheless, the term 'consumer welfare' was first spelt out by a senior EU official (Joaquin Alamunia, European Commissioner for Competition) as late as 12 May 2010. The Treaty of Functioning European Union only specifies in Article 101 (essentially formulated back in 1957), and the exemptions from Article 101, according to Article 101(3), will be given to the agreements that 'contribute to [...] promoting technical or economic progress, while allowing consumers a fair share of the resulting benefits'. This is hardly a consumer welfare standard. Furthermore, the EC 2004 Merger Guidelines (EC regulation 139/2004) stipulated (Article 79) that 'The relevant benchmark in assessing efficiency claims is that consumers will not be worse off as a result of the merger. For that purpose, efficiencies should be substantial and timely, and should, in principle, benefit consumers in those relevant

In addition to the consumer or total welfare standard dilemma, Kaplow examines an essential and intertwined choice that has received less attention: should the focus be on short– or long-run outcomes of merger decisions and merger policy as a whole. It is evident that in the long run, fixed costs and quasi-rent (producer's surplus) disappear as motives for entry, meaning that the consumer and total welfare tend to converge.

The author points out that pragmatic considerations may favour a short-run framing when competition authorities examine particular merger proposals because time and other resources are scarce, and the ability to predict the future beyond a few years is quite limited. He believes that these points are well taken, although with some qualifications. 'First, agencies should employ general rules and other protocols that are conducive to long-run welfare maximization, even if the most useful proxies and shortcuts do not involve undertaking anywhere near a complete long-run analysis. Second and relatedly, many of the most important effects of merger decision-making do not materialize immediately, so a short time frame can be incomplete and even misleading. [...] an important rationale for permitting a firm with market power to charge a supracompetitive price is that the prospect of such profits induces ex ante investment that tends to raise consumer welfare in the long run' (p. 196). For any reader who thinks that it is too narrow, too technical, Kaplow spells out the essence of the long-run approach to merger analysis. 'More broadly, as already noted, effects of competition policy on innovation [...] manifest themselves in the long run. Long-run welfare, including consumer welfare, depends largely on an economy's dynamism. An important purpose of competition policy is to preserve and enhance dynamism, not to reduce it. However difficult these effects may be to determine, simply ignoring them in formulating rules may produce choices that reduce welfare, perhaps greatly' (pp. 198–199). This approach effectively shifts the focus from the competition authorities' emphasis on the price effects of mergers and price effects-led competition law to innovation – a direction previously suggested by Gilbert (2020). Again, this is especially important for economies that are close to the technological frontier. Effectively, moving competition law from the short to the long run framework means changing the paradigm from minimising price effects and market power to maximising innovation. Quite a change!

markets where it is otherwise likely that competition concerns would occur'. The bottom line is 'that consumers will not be worse off as a result of the merger'. Again, this is hardly a consumer welfare standard.

The author does not stop there but moves forward and considers 'almost entirely neglected' matter of whether to adhere to the single-sector partial equilibrium approach that is currently done in scholarship on competition law and in competition authorities practice, or to consider 'as well' the multisector general equilibrium effects of competition policy that may substantially amend or upset conventional enforcement protocols and priorities. For understanding this dilemma, 'as well' is a crucial notion – it is not either/or. Kaplow does not advocate that standard partial equilibrium analysis should be abandoned, but only that it should be enhanced by general equilibrium consideration. His consideration of excessive, socially undesirable entries in the book (Chapter 5) is precisely the example of such an approach.

Kaplow somewhat reluctantly reminds the reader that he has a powerful intellectual ally – Lerner (1934) and his seminal paper on mark-ups and market power. Lerner's point, made in the paper published more than 90 years ago (but the Lerner index is still used as a measure for market power), was that the economy-wide level of mark-ups is irrelevant to total welfare – their 'deviations' are all that matter. 'The simple intuition is that deadweight loss from price in excess of marginal cost arises from too few resources being spent in the distorted sector; they flow instead to other sectors, where marginal utility is lower relative to production costs. However, if every sector is marked up by the same proportion, resources have nowhere else to go' (p. 202). There would be no effect on total welfare, and consumer welfare would be lower precisely by the amount of the producer's surplus increase. It is only income distribution that is affected. Nonetheless, as already demonstrated, in the long run, the two welfare standards (consumer and total) converge.

At the end of the chapter and the book, Kaplow takes up the possibility that competition rules should aim to protect competition as a process rather than to directly seek to obtain good outcomes, whether for consumers (consumer welfare) or society (total welfare). This is essentially a response to the New Brandeis movement attitude (Kahn 2018) that competition law should protect competition considered as the competitive process, which allocates resources to their best uses and provides incentives through price signals. The problem with this approach is that it aims to restore perfect competition, although such competition has never existed. Kaplow has no second thought about such an aim. '... perfect competition is infeasible, and policies aimed to approximate it as closely as possible would be highly destructive. In most sectors, truly atomistic firms could not accomplish much of what is done in a modern economy. And if prices never exceeded marginal costs, most investments could never be recovered'

(p. 206).[24] It is the prospect of quasi-rents that incentivise investment and innovation, Kaplow concludes. In this way, his rebuttal of the protection of competition as a process, the reader concludes, is the lethal blow to the concept of competition as static, as opposed to dynamic, i.e. an essential feature of protection of competition as process. In short, the New Brandeis movement considers competition through comparative statics of different market structures. Kaplow, along with the vast majority of mainstream IO economists, considers it Schumpeterian creative destruction.

At the end of the day, Kaplow concludes that economists deal with things that can be measured. More competition? More competitive pressure, competitive constraints, in the EU parlance? 'But what would be the measure of such strength? And what counts as a competitive force rather than a competitively neutral or anticompetitive one? And how would these features be converted to a common denominator with the direct reduction of rivalry? And how would that rivalry reduction itself be measured? It is no surprise that economists have not developed such an abstract index of rivalry or competition but instead have focused on the analysis and measurement of effects on welfare, or effects on prices, quality, and other determinants of welfare' (pp. 208–209). End of (New Brandeis movement) story!

In the final chapter, Kaplow spells-out that 'This book attempts to identify overlooked questions, sharpen our appreciation of existing deficiencies, advance analysis by building on the strengths of what is already known in industrial organization economics and other areas of inquiry, and offer suggestions for further research, policy formulation, and the practice of merger review' (p. 213). The reader expected this sentence in the Introduction, but actually, it is much better to have it here, at the end of the book, having digested the rich insights.

This book is suitable only for a very restricted, specialised audience, consisting only of IO specialists, academics specialising in the economic analysis of competition law, and competition authority specialists (including judges in the US and similar jurisdictions). Even for them, this book will be difficult to read for three main reasons. The first one is that the book is

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[24] This point by Kaplow is very important for putting into context the fascinating debate about the existence and the increase of price–cost margin in the US economy in recent decades, and the conclusion that this development is evidence for the competition decline in the US markets (Hall 2018; De Loecker, Eeckhout, Unger 2020). Basu (2019) and Syverson (2019) provide devastating methodological criticism on the way how the price–cost margin is estimated, and question whether the price–cost margin is a proper indicator for inferring the decline of competition. Begović (2022), insisting on the heterogeneity of firms and pointing out the existence of quasi-rents without any market power whatsoever, surveys the debate.

densely packed with insights and evidence supporting them. One page of the book contains more wisdom about competition law and its enforcement than multiple academic articles put together. For that reason, the reader will keep going back again and again to the pages already read, to see whether they have got it right. The second reason is that Kaplow's writing style is very demanding: he expects a fully engaged reader, so there is no room to relax and for easy reading. The third reason is that some of the claims are counterintuitive – it takes time and effort to follow Kaplow's arguments. Based on these insights, a caveat is in order. The potential reader who is a novice in IO and economic analysis of competition law should skip this book and first proceed to readable textbooks in those areas, and then both landmark and review academic articles. Substantial accumulated specialised knowledge is a non-negotiable prerequisite for reading Kaplow's book.

Although it produces ample new insight about merger control, both positive and normative, this book is not a castle in the air. It is well-founded on the insights of modern IO, and that is one of the reasons why it is so convincing. This book does not start from square one. The author provides ample references, so the reader interested in some of the claims can do their own further research on the topic.

As to the impact of Kaplow's provocative book, it challenges the very foundation of conventional merger analysis, and Kaplow argues that mergers should be evaluated using entirely different methodology (Shapiro 2025b). What Kaplow is advocating is nothing less than the change of the paradigm, because the incumbent one is faulty. He does not offer practical solutions (he was clear in the Introduction that this was not his intention), nor guidelines for reaching these solutions. Instead, he provides guidelines for further thinking and consideration of what can be practically done and how to achieve it. For example, he claims that although long-run general equilibrium analysis is infeasible in the review of particular mergers, the broad contours of policy as well as practical protocols should be developed with these effects in mind.

Taking all this into account, the specialised academic community will definitely enjoy reading this book (again, not an easy task, but academics are used to such things). Whether they will like it or not, whether they will agree with the claims – is another question altogether. What will be the reaction of the competition law practitioners, especially competition authorities, to this book that is an appeal for a paradigm change? Paradigm change is difficult and costly, so competition authorities have strong incentives to ignore the book, whatever the excuse may be. Perhaps something along the lines of 'Professor Kaplow is a very clever and knowledgeable person, but he misses the points that only we, in the trenches of competition law enforcement, with bullets fired from big corporations whistling over our heads, can see, as we feel the nuts and bolts of competition law enforcement'. It would be a pity if that happened because Kaplow's book provides ample food for thought for substantial improvements in competition law and the radical advancement of protecting and enhancing competition. Ultimately, improvements are made on the front line, but the ideas that improvements are needed and the strategy for these improvements are formulated in general staff cabinets, far removed from the trenches. Yes, one must see the whole forest to realise that something is wrong with the trees – deeply flawed in the case of the merger control forest. Precisely for that reason, the change, if it comes, will take time. Nonetheless, even 'a journey of a thousand miles begins with a single step'. Acknowledging the insights from Kaplow's path breaking book should be the first step for competition authorities.

Louis Kaplow did his part of the job – now it is time for the others. A careful and painstaking reading of the whole book should be a reasonable first step. For those who may be daunted by the task, Kaplow (2025) provided an article, a short version of the book. Warning: this is only a short, not a light version of the book, *á la Gitanes* light. The article is *Gitanes* short. The reader must still fully inhale the intense fumes of the original cigarette recipe. There is no other way. *Bonne chance*!

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