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Big Strategy/Small Strategy

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My title plays on Rumelt's (2011) instant classic *Good Strategy/Bad Strategy*. That book focuses on what works well or otherwise for organizations. Big Strategy/Small Strategy switches the focus to different issues. 'Small Strategy' is about financial performance, typically of firms in competitive industries. 'Big Strategy' is about significance – impacts and purposes that stretch far beyond firm performance. Here Big Strategy research is motivated by three crucial facts. Today, many of the world's most powerful firms are not simple profit-maximizers; these firms are weakly disciplined by competition; and their effects reverberate throughout society. Most strategy research ducks these issues. The Big Strategy concept seeks to rally efforts to give powerful firms and their wider effects a more central place in the discipline.

This essay, therefore, is about thinking bigger in strategy research. In particular, the discipline should take more seriously the macro-impacts of the world's most powerful firms. Big Strategy implies a broadening of dependent variables; increased emphasis on critical case and industry studies; and a greater readiness to engage with public policy issues. The way the world is changing, we are going to need more Big Strategy research.

Big effects

Our world is one of powerful firms, dominant in their markets. Think of Google and Baidu in search; Facebook and Twitter in social media; BHP and Gazprom in natural resources; Amazon and Walmart in retail; the 'Big Four' in accounting; or the 'Bulge Bracket' in investment banking. The strategies of these powerful firms have all kinds of impacts on the world, for better and for worse. Take one example, the so-called 'Walmart Effect'. Economists, sociologists and geographers have flocked to study the various impacts of the world's largest retailer (see Basker, 2007). It seems that Walmart's strategies have helped poor consumers, extended trade and even reduced inflation. They have also changed communities within America and promoted a structural shift in manufacturing across the Pacific. Equivalent big effects could be identified for other powerful firms, whether Baidu or BHP, Gazprom or Goldman.

But traditional strategy research misses a lot of this big stuff out, for two sets of reasons. The first set centres on the choice – and it is a choice – of what the discipline seeks to explain, its characteristic dependent variable. The second set of reasons is methodological.

To start with, strategy researchers remain peculiarly fixed on explaining strategy outcomes at the level of the firm. Analysing research articles over more than two decades, Nag et al. (2007: 944) find strong empirical convergence on a limited domain: effectively, the strategy field 'deals with the major intended and emergent initiatives taken by general managers on behalf of owners, involving utilization of resources to enhance the performance of firms in their external environments'. Strategy is specifically for and about firms. As in the Walmart case, this exclusive concern for the firm immediately cedes a lot of ground to other disciplines. Strategy researchers have uniquely valuable insights to offer on Walmart, but we recuse ourselves from commenting on the larger part of the 'Walmart effect'. It is for economists, geographers and sociologists to worry about the wider repercussions of strategy. For strategists, these are off-limits: firms are strictly interesting only for themselves.

Worse, the strategy discipline has increasingly focused on just a subset of firm-level outcomes. Originally strategy concerned itself with the selection and delivery of firm 'goals', an eclectic and discretionary notion. Since the 1960s, however, Ronda-Pupo and Guerras-Martin (2012) find a shift in the journal literature away from the general notion of 'goals' to the much more specific criterion of 'performance', typically expressed in financial terms such as 'profits', 'value', 'rents' and 'efficiency'. This focus on financial outcomes is not obviously a close fit with what a good deal of contemporary business is actually about. We should recall that state-controlled companies account for 80% of the national stock-market index in China, 62% in Russia and 38% in Brazil (*The Economist*, 2012); that about half of listed companies in the 10 largest Asian markets are family-controlled (Credit Suisse, 2011); and that the founding entrepreneurs at Google, Facebook and LinkedIn (among others) have imposed two-tier shareholder structures that deliberately keep investors at arm's length. States, families and wealthy founders are likely to have wider goals than just financial performance. For many of these firms, strategy researchers may be missing the point.

Methodological concerns compound the discipline's neglect of the big stuff. The focus on the standard dependent variable of financial performance is, of course, reinforced by its convenience for econometric modelling. But strategy is also biased by its preference for large-sample statistical studies. From the early 1980s to the early years of this century, the average sample size of articles in the *Strategic Management Journal* has multiplied more than six times, from just over 200 to nearly 1300 (Ketchen et al., 2008). From an econometric perspective, there is much good in these increasing sample sizes, including the prospect of picking up more statistically significant results. But there are at least two kinds of loss.

First, large samples are typically achieved by orienting to industries with many competitors. But the methodological desideratum of many data points is liable to side-line exactly the oligopolistic industries that are becoming more important nowadays. Network effects, economies of scale and globalization are all pressing an increasing number of industries towards high concentration (Nolan et al., 2008). Dominant oligopolies are not good fits for econometric models; they are outliers to be deleted. Second, the need for good statistical data marginalizes firms that do not meet western accounting standards. Chinese, Russian and Indian accounts can be tricky, to say the least. It is tempting to stick with standard American databases, familiar to reviewers and legitimized by prior use. But methodological purity on these two counts is radically restrictive. In a world where industries are concentrating and economic power is moving from the West, the strategy discipline's empirical reach is liable to get smaller and smaller. Here the discipline makes a version of the 'standard error' common to many quantitative disciplines (Ziliak and McCloskey, 2008). In chasing after statistical significance, strategy neglects substantive significance. Too often, we study fruit flies in a world of elephants.

Big Strategy

Big Strategy starts with impact: the firm strategies that matter most are those with the greatest repercussions, of all kinds. Big Strategy values big effects over large sample sizes. In most respects, one Walmart (US$450bn revenues and 2.2 million employees) counts for more than 10,000 small retailers. And Big Strategy takes seriously all the outcomes of firm strategy, beyond financial performance. News Corporation is partly about political influence and the Murdoch family. Gazprom is an instrument of the Russian state. It is not clear that the most interesting thing to say about such firms' strategies is that they are profitable.

Thus for Big Strategy it is broad effects that matter, not simply size of firm. Goldman Sachs ranks only 181 in *Fortune*'s Global 500 list of large corporations, but, as *Rolling Stone*'s 'vampire squid', its tentacles reach wide in contemporary society. Twitter has just 900 employees and revenues of US$140m, but this minnow has transformed the news business and shaken governments. Where small firms have big effects, they belong in Big Strategy.

The recent economic crisis amply demonstrates the dangers of neglecting Big Strategy. In focusing on Small Strategy, strategy researchers made the equivalent mistake to that of macro-economists before the crisis. As former Federal Reserve chairman Alan Greenspan confessed to Congress, his 'flaw' had been to rely on macro-economic models which simply assumed that firms, particularly banks, were rational profit-maximizers, disciplined by markets (*New York Times*, 23 October 2008). Greenspan believed he could safely ignore firm conduct. Strategy researchers do the reverse: we focus on firms and assume the invisible hand will look after the macro-stuff. This too is careless. The world's biggest banks were not rational, and the result was macro-economic chaos.

The big banks' potential for catastrophe should have been no surprise. After all, the strategy discipline has plenty of disquieting knowledge about firms. For example, we know that firms can blunt competitive disciplines by building market power (Porter, 1980): the bulge bracket investment banks most certainly did dominate their markets. We know too that top management teams suffer cognitive bias and dysfunctional decision-making (Powell, 2011): Lehman Brothers' egregious CEO, Dick Fuld, is part of a pattern. We increasingly recognize that firms' strategies are prone to fashion and bandwagon effects (Xia et al., 2008): in CEO Chuck Prince's notorious phrase, it was the mimetic desire to keep dancing while the music still played that drove Citigroup to pour more money into private equity even as the crisis swelled (*New York Times*, 10 July 2007). Strategists understand that ownership structures affect strategic purpose: in the decade leading to the crisis, canny Goldman Sachs may have averted some of the excessive risk-taking of its peers because of the cultural legacy of its recently dissolved partnership structure (Birkinshaw, 2010). On the other hand, strategy researchers are sensitive to how shareholder interests may be subverted by managerial agents (Wright et al., 2007): Goldman Sachs' senior executives have seen rich returns in the last decade, while investors enjoyed negligible stock appreciation. In short, the strategy discipline had a lot to tell Alan Greenspan about the banks, if only we had been thinking bigger.

Our reticence with regard to the banks was, however, entirely characteristic. As Agarwal et al. (2009) have noticed in this journal, strategy researchers have long been reluctant to enter policy debates. The fault lies in part with our preference for Small Strategy. Small Strategy directs us to the kinds of competitive industries that do fit Alan Greenspan's macro-economic model, just about. Today, these industries count for less and less. Small Strategy too teaches us to care most about narrow performance outcomes. We worry less about wider effects, or even about whether these outcomes are all that matters to the firms themselves. Small Strategy makes us small-minded. Big Strategy, on the other hand, promotes a larger research agenda.

Enlarging the agenda

Small Strategy underplays uncompetitive industries and non-profit outcomes. State-owned oligopolies, eccentric family behemoths and control-hungry entrepreneurs are awkward for econometric modelling and theoretically marginal besides. For Big Strategy, such firms are not anomalies, but central. This implies a widening of the research agenda. There are many directions to take, but I highlight three prime opportunities for Big Strategy researchers, each interrelated.

First, we should pay more attention to important firms: a guiding principle should be substantive significance, not just statistical significance. Here there is plainly a role for more case study research, especially of powerful firms, oligopolistic industries or impactful new strategies. There have been well-received gestures in that direction (e.g. Daneels, 2011; Tripsas and Gavetti , 2000), but still the overwhelming majority of strategy research is quantitative, typically dealing with anonymous firms en masse (Molina-Azorin, 2012). This leaves some remarkable gaps. Facebook – with its enormous wealth creation, its 900 million users and its extraordinary social effects – surely cries out for rigorous case analysis, not only for its intrinsic importance but for what it may tell us generally about strategy in today's strange world. Yet a Google Scholar search finds that by early 2012 the *Strategic Management Journal* had not published a single article so much as mentioning Facebook, let alone a detailed analysis of the most successful start-up of the last 10 years. Of course, we need statistical research too, only less biased towards industries capable of supplying large samples. Recall that Rumelt (1974), in his foundational quantitative study of diversification, sampled from the 500 largest US corporations *across* industries: he recognized that the increasing diversification of American big business in general raised not only performance issues at the level of the firm, but massive resource allocation issues at the level of the nation. We need more such quantitative studies of the world's most powerful corporations, tracking variables that count – diversification and delocalization, innovation and internationalization.

The second broad opportunity for researchers follows from the first, and that is to take more seriously other outcomes than financial performance. Performance is not the only strategic outcome, and, for many powerful firms, it may not matter very much anyway. In a global context where state and family interests often prevail, strategy researchers need to adjust to the complex variety of contemporary capitalism. When the American economy comes second to China's, the world will care less about shareholder returns in competitive industries. Indeed, there is goal ambiguity even among America's most successful firms. Mark Zuckerberg told investors in his famous 2012 IPO letter: 'more and more people want to use services from companies that believe in something beyond simply maximizing profits. . . . We [Facebook)] don't wake up in the morning with the primary goal of making money' (www.sec.gov/Archives/edgar/data/). The Walton family owns 48% of Walmart, not obviously a financially rational way of maximizing its wealth. Respecting the plurality of goals has methodological implications too. Rather than simply presuming financial drivers from the outside, researchers would do well to get inside firms, to investigate the goals they actually have. As evidenced by the Murdoch family's influence-peddling and clannishness at News Corporation, a company's controllers may have ambitions that differ substantially from stated goals. Researchers should penetrate beneath the economistic stereotypes of 'principals' and 'agents'. They should consider goals such as, for example, the intergenerational preservation of family control, the furthering of national interests and the achievement of political influence.

The third opportunity follows from the first two: Big Strategy researchers should step up to the public policy agenda. We cannot be indifferent to strategies whose repercussions reach wide, far beyond control by the invisible hand. Google's innovation strategies have huge significance for productivity worldwide, not to mention privacy. Kremlin-controlled Gazprom accounts for 10% of Russian GDP and 35% of Europe's gas. The way such firms allocate resources really matters. Strategy scholars have not been entirely silent with regard to public policy. Michael Porter himself has been consistently active in policy debates (e.g. Porter and Rivkin, 2012), while Ghemawat (2002) scolded the discipline for its role in promoting the faddish 'new economy' ideas behind

Enron and the dotcom bubble. But such contributions are rare and typically made outside the strategy discipline's principal journals. Our journal editors are bystanders, when they should be actively bringing strategy's unique analytical skills to the stress points of the contemporary economy. The discipline has a role in the critical investigation of powerful firms, influential industries and new and consequential business models: if not the dotcoms and investment banks earlier, then perhaps the social media and resource companies now. Here the journals should not wait on the slow processes of theory accumulation and peer review, but accelerate analysis through the prompt commissioning of expert views. As with the banks, neither markets nor regulators can be entirely trusted with strategy. That is our job too.

Finally

My essay title takes liberties with Rumelt's (2011) *Good Strategy/Bad Strategy*. His book is a wise and insightful one, full of excellent advice about what is involved in firm success. My intention is to add to that agenda, not subtract from it. Small Strategy insights can provide critical benchmarks for evaluating Big Strategy deviations. Rumelt himself shows that many firms pursue 'bad strategies' in his terms, and recognizes too that these strategies can snowball with catastrophic effects, as in the recent financial crisis. Given the power of many contemporary firms, however, the capacity of strategies to reverberate across nations and through societies is not exceptional.

The Small Strategy approach has neither the tools nor the attention span to grasp such larger effects. Small Strategy's criterion of financial performance captures only a part of firm outcomes, and indeed is often not even true to firm purposes. Small Strategy's preference for large samples and good data orientates research towards industries that are less and less typical, while diverting attention from the parts of the world that matter increasingly more. The Big Strategy concept seeks to enlarge the research agenda, therefore. Big Strategy adds the consequential firms that do not easily fit econometric models and it widens the range of dependent variables. As it does so, Big Strategy can inform and sharpen public debates about the behaviour of the world's most important firms.

Strategy researchers should not be shy of this public agenda. We not only have responsibility, but also unusual skills. As the investment banking example shows, we have plenty of theoretical insights to deploy, should we choose. Besides, we are well-used to analysing individual industries and firms in the classroom. In our theories and in our teaching practice, we have the apparatus that macro-economists, financial analysts, government regulators and business journalists appear to lack. Strategy researchers have much to offer. With Big Strategy, the discipline can step onto a larger stage.

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