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Everything is Obvious Once You Know the Answer About Duncan J. Watts, *Everything is Obvious Once You Know the Answer* (New York, Crown Business, 2011).

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EVERYTHING IS OBVIOUS ONCE YOU
KNOW THE ANSWER*

FOR MANY AMERICAN sociologists, C. Wright Mills' *The Sociological Imagination* (1959)¹ is accorded something close to canonical status. If asked to name their favorite American sociologist of the 20th Century, Mills would almost certainly eclipse Parsons, Merton, Goffman, or anyone else. And the most popular Mills book would unquestionably be *The Sociological Imagination* (the only other contender would perhaps be his 1956 work *The Power Elite*). The brilliant first chapter of *The Sociological Imagination*, containing its memorable definition of sociology as the intersection of history and biography and the distinction between personal troubles and public problems, remains a powerful and vivid statement of the centrality of social structures.

Yet Mills' suggestion that sociology would, in the future, be the cutting-edge of social science knowledge has taken a rather severe battering in recent decades. One devastatingly nasty but oft-heard view is that sociology is nothing more than common sense, or that the problems that sociologists study are trivial or easy. Another set of challenges has emerged from the evolution of social science fields in the more than half-Century since Mills wrote. The adoption and diffusion of formal models of theory construction, the rise of computational social science, and the application of genetic and neuroscience models to the understanding of human behavior are all now being deployed to study questions that were once clearly the domain of sociology. It is clear that, in a variety of ways, sociology has been on the defensive for some time, and that condition is likely to continue, if not deepen, in the future.

If ever there was a moment when a bold, Millsian-like statement of the fundamental logic and importance of the sociological enterprise is needed, that moment is now. But in the face of relentless physics envy across the social sciences, who could actually write such a book, and make it convincing to sociology's critics? Duncan Watts is perhaps uniquely suited for the task. His unusual biography includes a background in Physics, a PhD in Engineering, followed by a migration into

* About Duncan J. WATTS, *Everything is Obvious Once You Know the Answer* (New York, Crown Business, 2011).

¹ Mills, C. WRIGHT, 1959, *The Sociological Imagination* (New York, Oxford University Press).

sociology (and a professorship at Columbia), where he gained notoriety with the publication of some very widely cited articles and books in network analysis, including a powerful new demonstration of the famous “six degrees of separation” hypothesis about the small world phenomenon.

Watts’ *Everything is Obvious Once You Know the Answer: How Common Sense Fails Us* offers an engaging, occasionally brilliant retort to some of the most important contemporary threats and challenges to sociology. Framed as a commentary on “common sense”, it is in fact a great deal more than that. Watts opens his preface, entitled “A Sociologist’s Apology”, with a telling quote from a physicist who suggested in the mid-1990s that if budget cuts made the job prospects for young physics scholars too difficult, they could always “consider a career in social science, where it ought to be possible to solve the problems the social scientists are all worked up about in a trice”. Watts goes on to note that sociology and the social sciences are indeed, as the joke goes, *not* rocket science, but not for the reasons most people think. While any semi-competent team of rocket scientists can figure out how to get a missile from point A to point B, many of the issues of contemporary social science are of such complexity that unearthing patterns and being able to make viable predictions or find policy solutions is likely to continue to prove elusive. Problems like enduring poverty, understanding why public schools are not better, explaining why organizations fail, why racism persists, or whether economies will grow or not in the next quarter all remain difficult in spite of generations of research.

That this conclusion remains as valid as it would have been fifteen years ago, even after the ubiquitous rise of all manner of new kinds of computational social science initiatives such as network science, agent-based models, complex social systems analysis, to name just a few, is indeed intriguing. It is also a surprising but refreshing conclusion coming from an author who has very much been at the forefront of developing and applying the new techniques.

So what has failed, and why? One clever and instructive example Watts offers early in the book in relation to the limits of everyday common sense points to the problem. Watts notes that a long list of widely used aphorisms encourage exactly the opposite courses of action (for example, “haste makes waste” versus “he who hesitates is lost”, or “birds of a feather flock together” versus “opposites attract”). Blind applications are just as likely to lead to the wrong course of action as the right course. The error of these aphorisms, of course, is that they are issued context-free. They only provide wisdom and valuable guidance in

certain contexts, but not others. A skilled user of common sense must be able to apply an analysis of the structural contexts of action.

At the heart of the problem, Watts argues, is the fact that all of the new analytical approaches embrace versions of methodological individualism that simply cannot account for the many ways in which social structures have properties that are not reducible to individual units. This is hardly an original insight – indeed, it is perhaps the foundational insight of classical sociological theory – but in rediscovering and restating the power of social structures in his own terms, Watts manages to find new wine in old bottles and provide something of a *mea culpa* for the failed ambitions of the new generation of data scientists and their popularizers. With repeated examples and clever summaries of a wide range of literatures, Watts re-confirms that even the most sophisticated models of regularities of individuals and individual behavior cannot be solved through computational sophistication alone.

The book contains numerous examples of flawed thinking starting from individualist principles. One example, drawing from the research of Watts and his team, can be seen in his debunking of the widespread idea, popularized by the best-selling writer Malcolm Gladwell, that individual influential people can create fads. Gladwell's original idea, which Watts characterizes as "catnip" for marketers, tries to apply the insights about contagion processes of network analysts to derive a general theory of fads and marketing breakthroughs. If a new product can attract the right mix of influential people – hipsters in Manhattan, London, or Berlin, for example – those people will spread ideas about the product far and wide as others seek to emulate them. Watts tests this idea by looking at the pattern of "retweets" by over 1.6 million Twitter users. Only a handful of the 39 million total tweets were retweeted a second time, and far fewer after that. Examining what distinguished those tweets from everything else, Watts and his colleagues find that they simply could not be traced to a small number of influential people playing key roles. Instead, the pattern revealed in this data is that the most retweeted items are those that catch the fancy of a significant number of different people, many of whom often engage in retweeting. While each one of these individuals may have only a handful of others following them, the outreach can trigger a small chain reaction. Watts concludes that it is not the well-placed influential individual, but multiple people capable of being influenced by the same idea and signaling it to others.

Another application takes on generations of cultural criticism and commentary which seek to explain the success of cultural products in

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terms of their intrinsic qualities. Watts asserts that this is an impossibility; we cannot know what features of, say, the fame of da Vinci's *Mona Lisa* are the result of aspects of the painting we can observe independently of its later reception. We all too often develop and derive explanations in such post-hoc ways. It could well be the case that the surprising ambiguity of the subject's expression or the depth and vividness of the painting are keys to its success, but we only seek those explanations (and find them convincing) after the fact of the painting's success. Further, it is difficult if not impossible to assess our theories about the *Mona Lisa* without falling into a pattern of circular reasoning in which the *Mona Lisa* is special because it has features that are like, well, the *Mona Lisa*.

Watts spends considerable time reviewing the limits of generalizable knowledge in the historical social sciences. History, he says, is a "fickle" teacher. As with cultural products, there are few enduring lessons we can learn from history that are not derived from their outcomes. This problem of selecting the outcome is an almost inevitable problem of choosing and studying historical cases, and more importantly in determining which factors had causal significance. Watts reaches these conclusions from the standpoint of an experimentalist: because history cannot be re-run, and experiments are not possible, any attempt to derive general causal lessons from history is inherently flawed. This is deeply contested territory, a topic that has been heavily debated in recent years among comparative-historical scholars. The cutting edge of those debates is not referenced here, but it is certainly fair to say that Watts' position has largely been digested by most historical sociologists. The grand ambitions of an earlier generation to use history to build and test theories have been largely abandoned in favor of more nuanced claims and less ambitious agendas.

More generally, the problem that Watts is driving at in his discussion of history is captured in the observation that social scientists are bad at prediction. He notes that knowledge, historical or otherwise, provides a very uneven way to predict the future. Invoking the work of social psychologist Philip Tetlock (2006)², Watts notes that even in their areas of expertise social scientists are very poor at predicting future events (a trait shared by all manner of other types of "experts" as well). If we could know in advance which types of stocks are likely to rise, which types of paintings are going to catch the public's fancy and become

² Philip TETLOCK, 2006, *Expert Political Judgment: How Good Is It? How Can We Know?* (Princeton, Princeton University Press).

extraordinarily valuable, or which dictatorial regime is most vulnerable to revolutionary upheaval, we could be confident about the utility of social knowledge. But successful prediction runs headlong into the powers of social structure; there are simply too many moving parts in the social world to consistently isolate what matters, and thereby derive consistently accurate predictions.

Sociology has long needed a convincing account of the limits of newer methodological fixes to substantive problems, and Watts here has provided such a statement. That is, in its own right, an important achievement. But what is the alternative? What sorts of inquiry and sociological insight can avoid the problems he identifies? Watts does not take readers too far beyond a combination of patiently sticking with “good” science (and recognizing and avoiding the errors described above), combined with a pluralism of knowledge and a fairly conventional view that multiple applications should eventually culminate in useable (and verifiable) knowledge. Experimental methods, where possible, are preferred as they provide at least one solution to dealing with the inferential problems that so commonly impact on other kinds of work. But field experiments of the sort that would be required to take social structures into account (as opposed to laboratory settings where researchers can arbitrarily isolate factors of interest) are often difficult to mount and execute. More generally, in view of all the errors and vulnerabilities of conventional social science and common sense identified in *Everything is Obvious*, one does wonder whether Watts really thinks we will ever arrive at the moment where we will “know” how to reduce poverty or predict the next recession. But that is not the only way to think about social science knowledge. Providing rich interpretations of the past, or quasi-explanations of how something occurred after the fact, is also a valuable type of knowledge in its own right.