

Crushing the Berlin Wall of Sociology: A Review Essay on *The Babel Tower of Ideas*

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The Babel Tower of Ideas: Myths of Contemporary Sociology. By Chi-chan Yeh. Taipei: Socio Publishing, 2005. Pp. 252. ISBN: 9572899066.

The Babel Tower of Ideas is a major piece of scholarship by Professor Chi-chan Yeh, one of the most influential and charismatic sociologists who has also been a prolific mentor for multiple generations of graduate students in Taiwan's premier department of sociology at National Taiwan University. The book represents a milestone synthesis of Professor Yeh's decades-long research in sociological theory. Substantively, it is an attempt to understand and critique the anthropological premises and the quintessential images of modern society as developed by Western sociologists. Professor Yeh's signature is evident throughout the book—a longing for originality and breadth driven by a provocative and creative mind.

The book has many charming features. The title of the book is a winner. Originally mentioned in the Bible, the "Tower of Babel" has come to signify the arrogance of those people who built the tower (*Genesis* chapter 11, v.4: "Come, let us build ourselves a city, with a tower that reaches to the heavens, so that we may make a name for ourselves"). It effectively conveys the critical tone of the book and promises to light off some academic fireworks. The objective of the book is admirable: it is

positioned not only to smash arrogant ideas and debunk common academic myths, but also to delineate the pitfalls of Western sociology in its statistical and theoretical genres. The scope of the book is impressive: it ranges widely from statistical sociology to psychological anthropology, from the sociology of the historical origins of key statistical ideas to the sociology of the contemporary sociological community in Taiwan, and also from a critique of myths in the quantitative practice of sociology to a critique of myths in the building of the sociological community. However, the book also has a disappointing tendency to rely on slippery deduction and undocumented empirical conjectures.

A wide-ranging and thoughtful book such as *The Babel Tower of Ideas* deserves a review symposium composed of scholars representing a wide range of academic expertise. It is virtually impossible for any individual to provide a comprehensive review and thorough discussion of the interesting ideas and arguments in the book. Instead of attempting this impossible mission, I will use the limited space available for a review essay to discuss a select subset of the most provocative claims raised in the book and highlight ones that are most relevant to the sociological community in Taiwan. The focus of this essay is on the sociology-of-knowledge analysis in the lead chapter and in the appendix chapter of the book.

The Berlin Wall of Sociology

For many years sociologists have witnessed the rise of an academic Berlin wall, an artificial barrier that has prevented “qualitative and theoretical sociologists” from understanding the meaningfulness of quantitative research and from appreciating the foundational importance of logical and evidential adequacy for all styles of non-tautological

sociological inquiry. Sociological knowledge production is segmented into divergent worlds of research style, epistemology, and standards of logical and evidential adequacy.

Manifest in the analysis of the two chapters I will focus on are deep underlying tensions. For instance, tension exists between those who practice qualitative style research and quantitative style research, between those who practice statistical analysis and those who do not, and between those who frequently participate in professional reviews and those who feel obstructed by the power game of academic sociology. The analysis is in many ways a well-intended attempt to deconstruct the social and political uses of statistics, demystify statistical sociology, and denounce the naïve worship of Western sociology and positivism. I find it easy to agree with many of the case materials and claims drawn from the STS (science, technology, and society) literature. Nonetheless, the book perpetuates the tensions by adding height to the Berlin wall of sociology.

The rest of this essay is devoted to the provision of counterpoints to the two focal chapters. I will highlight gross oversights of the chapters and their consequences. Contrary to the impression some readers may draw from the appendix, academic disagreements need not be about or driven by turf issues. My goal of identifying oversights is constructive rather than destructive or vindictive, and is, in fact, intended to enhance mutual understanding among Taiwanese sociologists. The ultimate concern is community building, not an academic ivory-tower battle. I sincerely hope that readers of all persuasions would read my comments with the intention

1 Two disclaimers are in order. This review essay is written in English and from the standpoint of an immigrant sociologist who is loosely connected to the power game of academic sociology in Taiwan. First, the choice of language has nothing to do with the internationalization movement of Taiwanese sociology. The choice is a direct consequence of my lack of practice in writing Chinese for over two decades. An unintended consequence of using English is that my claims will be subject to the scrutiny of a much wider audience than it would have been if I had used Chinese. Second, as an immigrant sociologist, I am

of understanding what constitutes this artificial Berlin wall and what resources and means can be used to break it down.¹

Iron Cage of the Past

From Quetelet to Contemporary Statistical Sociology

The lead chapter is central to the message of the book. Even the title of the book is borrowed from this chapter. For the sake of discussion, I find it useful to organize the message of the chapter into two main parts. The first part is historical and draws on the STS literature on Quetelet and the socio-political uses of statistics. The survey highlights the functional compatibility between the idea of the average man and the socio-political contexts of the nineteenth and the early twentieth century, followed by an attempt to connect Quetelet's ideas of the average man to Durkheim's notion of collective type and Weber's method of ideal type. This part is based on a fairly standard STS literature on statistics (Porter 1985, 1986; Gigerenzer et al. 1989; Hacking 1990, 1991; Desrosieres 1998). As there is no sweeping or consequential claim made, let us assume that this part is entirely valid.

Contrary to the historical focus of the first part, the second part is positioned to be a fundamental critique of contemporary quantitative sociology. This part consists of an assumption and a sweeping claim about an iron cage of contemporary statistical/quantitative sociology. The assumption is that the twin ideas of central tendency and dispersion have

peripheral

enough to offer what may be more detached responses than other colleagues might be to the many rhetorical allegations of the appendix.

- 2 To quote from the original Chinese text: 「常態分配曲線(curve of normal distribution)一向被視為最能普遍反映諸多(包含物理與社會)現象的基本樣態。繼而,所謂中央趨勢(central tendency)與離散度(dispersion)則是被視為用來描繪此一曲線之行為的兩個核心

been the cornerstones of statistical sociology since the early nineteenth century.² The central claim is that contemporary statistical sociology is inevitably trapped by the iron cage of its historical origin in general and Quetelet's interpretations of statistical concepts in particular.³ This iron-cage thesis is sweeping and points to the thought processes of researchers. How valid is the thesis? Without getting into the mind of quantitative sociologists, it is possible to evaluate the validity of the thesis by checking the validity of the cornerstone assumption. If the twin ideas are not central to quantitative sociology, the condition for the iron-cage thesis cannot be established and the rest of the thesis collapses.

The cornerstone assumption is flatly wrong as the chapter's characterization of contemporary statistics is highly incompatible with the large amount of contemporary literature that I have read.⁴ In contrast to the picture painted by the chapter, I will outline some core statistical principles and help reveal how contemporary quantitative sociology works in practice. Readers can judge for themselves which picture is better founded and consistent with their own observations.

量度概念。它們成為塑造整個理論架構之語言煉金術最為重要概念馬前卒。」(p. 3)

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- 3 To quote from the concluding section of the chapter: 「他們長期所依賴的概念——如中央趨勢和離散度這兩個客觀的神祕性概念，本身其實就是特殊文化與歷史背景底下的特殊概念產物。說穿了，它們只是啟蒙理性帶來的一種特殊魔咒，一沾用上了，就必然把特定的文化和歷史意涵一併地承擔下來。」(p. 63) 「統計概念並不是如實證社會學者們一向所以為的，可以超越特定時空場域、而有著普遍而客觀的有效性。它是特定文化與歷史背景的孳生品，也是特定意識形態的產物，一直就沉重地負載著特定哲學家人類學之存有預設的觀念包袱。」(p. 71)
- 4 More specifically, I speak from the vantage point of a long-time practitioner of quantitative sociology. Having read hundreds of technical papers in statistics and thousands of modern quantitative studies in a variety of subfields, I have direct observation over a large and diverse sample of modern quantitative research in action. The sample is not strictly random and may be biased toward the top journals, but the sample is still large enough to cover a reasonably representative range of quantitative studies. I am not clear the range of quantitative studies that might have provided the basis for the characterization of statistical sociology in the book. I do assume that if the book's sweeping generalizations of statistical

Prevalence Is Not Fundamental Significance

The presumed foundational significance of central tendency and dispersion for contemporary statistics is hugely overstated. Readers should not confuse prevalence with significance. Indeed, the two statistical ideas are the most common in what elementary textbooks classify as descriptive statistics. Let us consider an analogy with English: the alphabet “a” (as in “a”, “are”, and “and”) is likely the most common alphabet that appears in English writing, yet the alphabet “a” is no more foundational than any other letter of the alphabet. Most importantly, central tendency and dispersion per se, do not tell us anything about foundational statistical principles, just as letters of the alphabet tell us nothing about the grammatical and interpretive principles of English. In short, the two ideas might have been quite central to descriptive statistics and to statistical thoughts of the nineteenth century, yet they are far from representing the foundations of contemporary statistical sociology.

In fact, as far as statistical principles are concerned, more fundamental than central tendency and dispersion are factors such as (1) the distinction between a population of heterogeneous elements and a sample from the population, (2) variation across samples, (3) the estimator and the sampling distribution of an estimator, (4) various forms of the law of large numbers,⁵ and (5) models of statistical relationships. Indeed, without case heterogeneity, there is no sampling uncertainty and no room for statistics. Only in the presence of substantial heterogeneity (particularly of unobserved sources) is there much room for sampling uncertainty, hence the role for representative sampling, the idea of biased

sociology are true, telltale signs should have shown up many times in the literature I have read.

5 Contrary to the erroneous claim of the author (p. 22), the identically and independently distributed (i.i.d.) assumption is not a necessary condition for normal distribution (see, e.g.,

sample and biased estimator, the demand for quantifying uncertainty after determining the sampling distribution of a statistic (such as an estimator), the application of various forms of the laws of large numbers, and the problem of identifying causal relationships from experimental and natural observations. Even from this incredibly condensed overview, I hope the reader would recognize why the role for the twin focal ideas of the chapter is quite different than the impression suggested by the author.⁶

What Do Quantitative Sociologists Do?

In terms of practice, it is useful to paraphrase what I once said in a recent critique of quantitative sociology (Tam 2001, p. 253). Not only is it logically possible for social scientists to infer population properties from limited sample data, it is possible to accomplish two fundamental analytic objectives: (1) to do what case studies and ethnographic methods are, by design, unable to do—conduct statistically controlled comparisons of

White 2001). Nor is it essential to the notion of representative sample, the application of most contemporary statistical models, or most laws of large numbers.

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- 6 To wit, descriptive statistics only constitute a tiny fraction of introductory statistics, even less of advanced econometrics and mathematical statistics, and it appears in quantitative studies merely as a routine description of one's raw data and is usually irrelevant to the evidential strength of an argument. Moreover, the most frequent use of central tendency and dispersion by quantitative studies in professional journals involves model estimation rather than descriptive statistics: the biasedness of a point estimator (whether its central tendency is different from the true parameter) for an effect parameter and its standard error (the dispersion or statistical uncertainty with which a point estimate is obtained from a sample). The context and application does not mirror the notion of the average man and dispersion from it, as an effect parameter is not about properties of individuals but about a conceptual model, hence far removed from what Quetelet had in mind.
- 7 Before the invention of multivariate modeling for analyzing large data sets, this kind of specification proved to be an impossible challenge even for a couple of hundred cases and half a dozen variables. In spite of their virtues and contributions, qualitative methodologies cannot handle the multivariate specification problem. Caution is always due for any method, both quantitative and qualitative. But avoidance is not a sensible response. Critics of statistical methods have mistakenly assumed that avoiding quantitative methods would give them greater leverage to understand reality. In fact, avoidance only ties one's hands, not adding strength to what one already has. Just consider this: the relationships among multiple variables often involve such questions as how each variable may be dependent on some of the other variables directly and indirectly. The complex controlled comparisons required for

outcomes across a complex range of conditions and scenarios, and (2) to specify the structure of causal relationships among multiple concepts⁷—a task that many statisticians once thought logically impossible but now can be rigorously articulated and has been shown to be sound.

Skepticism about the possibility of empirically identifying causal effects turns out to be the result of muddled thinking about correlation and causality. This is the conclusion from the perspectives of a Stanford philosopher of science (Cartwright 1989), a Nobel Laureate econometrician and empirical economist (Heckman 2000), and a computer scientist of artificial intelligence (Pearl 2000). In a path-breaking treatise on the subject, Pearl (2000) presents an ingenious synthesis of the cumulative wisdom of artificial intelligence, philosophy of science, statistics, and econometrics, and a seminal graph-theoretic approach to bridge practical intuitions and rigorous mathematical proofs. Although confusions and doubts have sustained for decades, by now it is clear that causal ideas, models, and conjectures can have precise representations, easy-to-understand practical rules of identification, and so are often testable even without strong parametric assumptions of functional form or distributional forms of observed and unobserved concepts.

The major problem with the central claim of the chapter boils down to this: it is mainly devoted to two basic ideas of statistics that have little relationship to what quantitative sociology has actually done under either analytic objective to date.⁸ The contrast is stark: the world of contemporary quantitative sociology revealed in the chapter has nearly no

addressing these questions are simply infeasible and unreliable without the aid of multivariate statistical methods and a computer.

8 The fleeting reference to correlation and regression (p. 37) does not come close to demonstrating a recognition of the core practice of quantitative research. The laws of large numbers are indeed central to the derivation of sampling distributions in asymptotic theory, but the laws are mentioned by the book only in the context of justifying the presumed

overlap with the world I know as a long-time practitioner and consumer. The twin ideas fall within the domain of descriptive statistics but are unrelated to the crown jewels of contemporary statistical sociology. Ignorant of how peripheral these ideas are, the chapter has mistakenly overstated the fundamental role of the average man for contemporary quantitative sociology.

Who's the Prisoner of the Past?

The book does not present any evidence for the continuing influence of Quetelet's conception of the average man on contemporary sociologists, such as quotes from contemporary statistical texts/articles or quantitative sociological research. Directing attention to the core principles of contemporary statistics or the practice of quantitative sociology, I have argued that the role of central tendency and dispersion is mainly in descriptive statistics and so their role in quantitative sociology is minimal. In other words, both the cornerstone assumption and the iron-cage thesis are incompatible with the diverse literature I have read. It is hard to imagine why the book attributes such importance to these peripheral concepts and tools in contemporary practice.

As a matter of fact, the thesis attempts to draw a generalization from a past case and apply it to thousands of contemporary practitioners of the present, and does so without checking the generalization against contemporary evidence. The following fable about air travel helps underscore my point. Once upon a time, a social scientist wanted to determine the social meaning of traveling on a jet plane. Firmly believing that all social meanings are products of the past, the social scientist undertook an extensive archaeological search into the past records of human civilization. Amazingly, the social scientist discovered from Stone Age records plenty of information on human fantasies of traveling like a

bird and the fantasies consistently attribute a religious meaning to avian flight. Overjoyed with the discovery, the social scientist concludes that modern airplane travelers, whether they know it or not, share with Stone Age human beings the same religious interpretation of traveling like a bird. The conclusion suggests that modern travelers are prisoners of past fantasies and the original religious interpretation of avian flight.

By the same token, the iron-cage thesis claims that contemporary quantitative sociologists are prisoners of Quetelet's conception of the statistical mean and dispersion. The thesis is committed to the belief that the past exerts a magical grip on quantitative sociologists, all this despite the fact that the causal mechanism is never spelled out and the predictions are completely at odds with the contemporary literature. In fact, the thesis itself is likely the real prisoner of the past.

How could a serious and scholarly study end up colliding so badly with reality? It is not my intent to determine the cause, but instead I will present the options and allow the reader to decide. Two possibilities come to mind and they are not mutually exclusive. First, the author might have no direct knowledge of what contemporary quantitative sociologists actually do and how they think, relying instead on like-minded peers for unreliable and stereotypical characterizations of contemporary quantitative sociology. Thus, the poverty of the iron-cage thesis is not as obvious to the author as it is to contemporary quantitative sociologists. This scenario adds urgency to the need for breaking down the Berlin wall of sociology, and thus my comments above focus on supplying a description from the insider's perspective. Second, he might subscribe to a different standard of logical coherence and evidential adequacy, which brings me to a subject that is central to the appendix. In the next two sections, I will identify a spirit of disciplined inquiry that should be equally applicable to qualitative and quantitative styles of sociology. When followed consistently,

sociological studies of all styles can provide reliable understandings of social phenomena. But to put the spirit to work is costly because it demands, for instance, serious attention to methodological issues, precaution against spurious association, avoidance of overgeneralization, and appreciation of the institutions of gatekeeping that are inconvenient for all parties but crucial to collective progress.

Power Game and the Spirit of Disciplined Inquiry

The appendix chapter is admittedly not an integral part to the book and it originates from a different purpose than the other chapters. However, it does reinforce the critical message of the book against contemporary sociology and it shows a similar reliance on rhetorical persuasion rather than evidential strength. The appendix stands out in particular for the remarkable amount of emotional language and unsubstantiated accusations. Behind the rhetoric and emotional language are two analyses devoted to identifying the nature of the power game in Taiwanese sociology. The first is an explanatory analysis of the power structure of Taiwanese sociology from inception to 2003 (the original publication year of the appendix). The second is a cultural critique of the ideology of extreme positivism and the blind faith in Western sociology, both of which allegedly have started to tilt the balance of the power game in favor of positivists and worshippers of American sociology. While some readers may be distracted by the rhetoric and emotional language, let me

prevalence of the normal distribution, which is a distribution that can be fully defined by its mean and variance parameters.

9 The power of party A over party B with respect to an action or choice is the capacity of party A to get party B to act or choose in a way contrary to party B's preference. For example, when I submit a manuscript to *Taiwanese Journal of Sociology*, my preference is to have it

try to focus on what we can learn from the analyses.

Old Game: Absence of Constraint

The first analysis offers a structural and historical perspective on the power game of sociological knowledge production in Taiwan.⁹ The structural analysis (p. 182) resulted in the sharp prediction that Taiwanese sociology was at risk of hegemony by a small clique of sociologists. But the analysis later concludes that Taiwanese sociology has never witnessed any hegemony, not when there were just a handful of sociologists (including the author), not when the field started to expand rapidly after the 1980s, and not even now.¹⁰ This conclusion is comforting. However, it also means that the prediction of the structural analysis does not stand and so the analysis appears to have missed something crucial. To explain the failure, the appendix suggests several reasons but has not seriously examined any of them (p. 191).

In light of the failure, the appendix focuses on claiming that new power groups are emerging (p. 192). The reader may wonder: as long as the functioning of a modern academic community involves resource allocation, status allocation, and scholarly gatekeeping, isn't power relation a necessarily part of the process? Indeed the presence of power relation is no surprise. It has nothing to do with the size or history of Taiwanese sociology. The real concern of the appendix seems to be that the power game of Taiwanese sociology is no longer what it used to be from the standpoint of a senior scholar.

published in the journal. But if my manuscript is rejected by reviewers and the editorial board, my preferred outcome is overridden. Whether I like it or not, the reviewers and the board have

power over me with regard to the publication of my work in the journal. Similarly, peer reviewers of my funding proposals also have power over time. I have heard personal testimonies of world-class quantitative sociologists and economists indicating that each had

Perhaps inadvertently, the analysis is even more revealing about the power game of the past than the present. When power is exercised, the action or outcome of at least one party is overridden or constrained by the will of another party. Judging from the appendix's earlier analysis, the effects of power game used to be absent (at least to the author and his close peers). The author was one of the beneficiaries of the old game and, being one of the most esteemed sociologists in Taiwan, he is unlikely to be a typical victim of the new game—whatever the new game is. This tells us that, for senior sociologists to date, the good old days were really friendly days because they hardly felt the pinch of being overpowered. Compared to those enviable days, any new game would be detestable.

From the perspective of senior sociologists, the game was not a tough play until the influx of many new sociologists in the 1990s. From what I heard over the past decade, the gatekeeping of funding and publication has been tough on everybody: senior and junior members, qualitative and quantitative researchers. By some account, the game is toughest for the youngest cohorts, not the most senior cohorts. The million-dollar question is why the game has gotten tougher, and whether it is for better or worse.

Gatekeeping Loathed but Would be Much Appreciated

According to the appendix, the academic culture of the new generation has two components: (1) an extreme form of positivism¹¹ and (2) a naïve and blind faith in Western sociology,¹² particularly American many rejections by top journals, including papers.

10 「台灣社會學界一開始發展，就呈現『群雄並立』的格局，沒有誰有條件占有所有（乃至只是大部份）的權力資源，也沒有誰可以長期擁有左右一切的條件。」(p. 191)

11 To quote from the original text: 「我所不能同意的，是那極端實證主義的信仰者所以為『唯有透過經驗實徵（甚至是其中更特定）的方式而經營起來的知識，才是科學的，也因此才有價值』這樣的主張。」(p. 197). Note that this statement does not specify what constitutes empirical evidence, what science is, and what valuable means. As far as I can tell, the range of positions encompassed by this vague statement is huge. Nonetheless the author labels the statement as the position of extreme positivism.

sociology, as a source of ideas, traditions, and practice. The critique consists of sweeping statements about the naïve academic beliefs of the new generation as well as the institutions of gatekeeping. Unfortunately, it provides no case material (personal or impersonal) or any other evidence on the faults of the new institutions that define the power game.

Reading the critique actually strengthens my appreciation for the emerging institutions of gatekeeping in Taiwanese sociology. The institutions can do a lot of good for the health of the discipline. For instance, straw man should not be allowed, rhetorical persuasion should not substitute for evidential strength, and alternative interpretations should not be suppressed or ignored. So effective gatekeeping would reduce or prevent the use of the straw-man tactic in scholarly publications;¹³ detect and remove rhetorical tactics that help authors evade the demand for identifying one's own position while criticizing others;¹⁴ shoot down a straw man argument before it finds its way into print.

Effective gatekeeping would also check for improper research design that may seriously undermine a scholarly work, and encourage researchers to take proper account of alternative explanations. To be concrete, let us consider two major examples from the book. The first one is from the

12 Again to quote from the original text: 「無論實際進行自己的研究、傳遞知識、撰寫論文、或對別人的研究和論述推行評論時，一切都以美國（或謂西方）社會學所設下的認知典範做為唯一的歸依。」(p. 199)

13 When referring to the target of critique, a rhetorical tactic is to set up a straw man that identifies an extreme position, which scholars are smart enough to avoid, as in an earlier quote on extreme positivism: 「唯有透過經驗實徵（甚至是其中更特定）的方式而經營起來的知識，才是科學的，也因此才有價值」(p. 197). This form of positivism excludes even mathematics and statistics, which are universally recognized as scientific and valuable but not based on empirical observations. I am not aware of anyone who holds such a view. Nor does the book lists anyone who subscribes to the position and how extensive this position is in Taiwan or anywhere else.

14 A rhetorical tactic to evade identifying one's own position is the following quote from the original text: 「我沒有意思完全否定以實證主義之科學觀所經營出來的社會學知識，以為它是完全無效、也是無用的」(p. 197), which says little to distinguish the author's position from any practitioners of empirical research and renders one's own position ambiguous and practically not falsifiable.

appendix and the second from the lead chapter.

Example 1. A cultural critique of the new power game.—The author presents justifications for the absence of any empirical evidence in support of the many claims made in the appendix (p. 178). The justifications are twofold: (1) self confidence in validity of all the claims¹⁵ and (2) sensitivity of personal case materials.¹⁶ Both justifications are invalid for not providing any evidence for the claims in the appendix. Consider the first reason. Self confidence in a claim is not a proof of validity for any claim for scholarly purpose, nor is it valid for the purpose of making a case in court. Social sciences are distinguished from, and much more valuable than, amateur works in part because scholars pay the extra price to communicate and document their claims. Even if an author is absolutely confident of the validity of a claim, it remains the author's duty to document the claim and convince the reader with relevant evidence. Consider the second reason. The social tension that may result from revealing the author's personal case materials is given as an excuse for not presenting all other evidence. While personal case materials may be interesting, they are neither necessary nor sufficient evidence for the claims in the appendix. Why are other kinds of evidence not presented? The second reason simply misses the point.

What could have been done? The reader should note that the claims in the cultural critique of the contemporary power game in Taiwan are not framed as specific instances of the author's experience, but are generalized statements about the power game in Taiwanese sociology. Historians of science, including Theodore Porter on whom the lead chapter relies very much, have routinely used documentary analysis to identify academic

15 The author is confident of his claims because he has nearly three decades of experience to know that the claims are factually accurate. So he thought there is no need for any additional empirical evidence to confirm his personal knowledge.

16 The author thinks that some of the claims can be supported by his personal experiences. But

cultural ideas and practices. Sociological studies, whether American or Taiwanese, are easily accessible in the public domain. The claims are general about Taiwanese sociology. Such general claims have many empirical implications other than the personal experience of the author. Many published articles can be analyzed without causing personal tensions. Would it not be meaningful to conduct a textual analysis of 20 randomly sampled quantitative articles from the top two journals of sociology of Taiwan over the past 10 years? Alternatively or in tandem, why not try an in-depth analysis of five of the most cited quantitative articles?

Example 2. The Iron-Cage Thesis for Statistical Sociology.—To understand why the lead chapter embraced the ill-fated iron-cage thesis, I have pointed out two possibilities. The first one, already dealt with above, concerns the information source. The second one concerns the standard of logical coherence and evidential adequacy and it shows in the chapter's treatment of the research design. Whether quantitative or qualitative, all empirical studies are vulnerable to oversights, wrong assumptions, incoherence and the like. The research design of the chapter is to focus on Quetelet of the nineteenth century and completely omit any reference to contemporary quantitative sociologists and their practices. In fact, the original text establishing the research design is a brief clause following a terse statement of the three research questions of the chapter. In lieu of a full-scale investigation, perhaps a more appropriate approach is to return to the history of statistical sociology.¹⁷

then personal case materials would have been inappropriate because anonymity of the actors in each case is virtually impossible to protect and exposing the cases will cause results in unnecessarily tensions among certain parties.

17 To be precisely, here is the original text in context: 「至少有三個問題是不能不追問的。其一、一向，統計學家（尤其是社會學家們）如何實際運用這兩個概念？其二、這些概念何以是如此這般地被操作著？其三、它們具有著怎樣的社會學意涵？很明顯的，要恰適地回答這些問題（特別是前二者），並不是很容易，一個比較恰當的做法或許

The reader might have wondered why the research questions call for a historical approach, even to the exclusion of contemporary data. Having been a long-time insider of quantitative sociology, I am well aware of the fact that the iron-cage thesis is at odds with contemporary reality. No where in the chapter does the argument specify any theoretical mechanism through which Quetelet's conceptions of statistical ideas could have a major influence on the state of mind of contemporary quantitative sociologists,¹⁸ nor does it provide any empirical evidence on the link from Quetelet to contemporary sociologists—not even a single illustrative case, not to mention a careful assessment of the prevalence of Quetelet's influence among contemporary quantitative sociologists.

The chapter's one-sentence treatment of the research design reflects a cavalier attitude toward issues of design and theoretical coherence. Not giving research design and theoretical mechanism the due attention can easily have disastrous consequences, as it does for the lead chapter. In general, even when an author fails to take heed of the design problems and alternative views of contemporary quantitative sociology, the referees could have alerted the author to the design and evidence problems if they are more sensitive to the pivotal role of research design and alternative interpretations.¹⁹

是回到西方統計社會學的發展歷史（尤其是結合著現代社會思想史）中去考察。」(p. 4)

- 18 The iron-cage thesis is articulated as if it were the confident conclusion of a thorough assessment of evidence. To quote again from the text in the context of the iron-cage thesis:

「不幸的是，大多數從事實證量化研究的社會學者，不是以不承認的方式把這個特殊的文化歷史特質完全架空，就是根本不知道統計學史上有著一段難以化解的思想糾結。他們一直存在於一種潛意識夢遊當中，把追求普遍法則的知識理想與方法論之理路規則的實情劃上等號。於是，他們以夢囈的方式毫無細緻思索地供奉著平均值——一個自 Quetelet 以來即竭力塑造的理性神祇。而且，在夢遊當中，他們頑強地相信自己乃奉「科學」的客觀之名，手上揮舞的是「真理」的利劍，可以隨時砍殺「異端」。這表現的其實只是一種有「知識」、但實則卻是無知的傲慢態度。」(p. 63) This paragraph may be likened to a psychoanalytic report on the subconscious but primordial emotion and cognition of quantitative sociologists without any personal interview.

- 19 At issue is not quantitative versus qualitative style of research, but a matter of logical coherence and evidential adequacy. So if the author or referees were more cautious about

In short, “to make logic matter and evidence count” is the fundamental mission of institutional rules of gatekeeping. But institutions of gatekeeping are not God. Institutions are imperfect tools and always require the intervention of imperfect human operation. The functioning of institutions is intertwined with the people who operate them. Gatekeeping is costly and inconvenient for all parties; it may even be vehemently detested by those who are uncomfortable with the constraints implied or do not appreciate the collective wisdom of gatekeeping. Let us applaud those colleagues who have been bearing a disproportionate share of the price of effective gatekeeping; the professional community and the society at large owe it to their dedication to the idea of knowledge production.

Logic, Evidence, and the Social Dynamic of Objectivity

The book’s critique of Taiwanese sociology pigeonholes the academic culture of the new generation of sociologists in the form of simple beliefs, such as an extreme form of positivism. This mode of dialogue is unwarranted as it adopts a mechanical and naïve approach to defining a culture. The outcome of the dialogue is also detrimental to the discipline as it would only reinforce the Berlin wall of sociology. A more constructive dialogue is to identify common grounds.

To be specific, let me zoom in again on the case of quantitative sociology for which I have accumulated a wide range of observations.²⁰ The first thing I would say is that the epistemological and methodological beliefs of the community are too loosely coupled and diverse to be summarized by a few sentences, let alone a few words. But if one looks at coherence, or more sensitive to alternative facts and interpretations, the flaws of the iron-cage thesis would not have been overlooked for so long, surviving the evidence evaluation phase, the referee process, and several years after publication in a prominent journal.

20 I have not done any formal survey, but I have close encounters in person and in writing the

how quantitative sociologists conduct their research activities, the convergence is striking in terms of how quantitative sociologists write and build arguments, how they debate and criticize the work of peers in professional conferences, anonymous journal reviews, or articles. There is ample evidence for a coherent culture that has not been acknowledged in *The Babel Tower of Ideas* and to my best knowledge not recognized by any other work critical of quantitative social sciences.

The Culture of Quantitative Sociology in Action

Unifying the community of quantitative sociology is the effort to (1) stay vigil against all forms of mischief while (2) striving toward inter-subjective standards of logical coherence (including what we commonly regard as explicit specification of theoretical mechanism) and evidential strength. Staying vigil signifies a spirit of realism about the many ways in which human investigators can corrupt the process of building evidence and making inference. Quantitative sociology makes progress by continuously recognizing that to lie with number and statistics is just as easy as it is to lie with qualitative data and methods.²¹ Striving toward inter-subjective standards does provide impetus for quantification, but it is widely understood that quantification does not guarantee reliability and qualitative evidence is often necessary and complementary to quantitative

working minds of over a hundred quantitative sociologists of a wide range of status and backgrounds. They mostly work on stratification, labor market, educational sociology, social networks, and organizational studies. I must declare that none of them fits the iron-cage thesis.

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- 21 There are already many books devoted to exposing the misuses of statistics in real life. Interesting, the examples almost invariably come statistics that appear in the mass media rather than reputable academic journals. Also interesting is the absence of any books devoted to exposing the misuses of qualitative data and methods.
- 22 It is reassuring to know when a book such as *The Bell Curve* misuses statistics to perpetuate ill-founded ideas, it will draw immediate and massive rebuttals. In the case of *The Bell Curve*, competent quantitative researchers were willing to pay the price to conduct rigorous reanalysis of the same data and other supplementary data to demonstrate that, if logical and

analysis.²² Quantitative sociologists, just like qualitative sociologists, aspire to big ideas and discoveries. But they also take heed of the advice of the book: don't learn to run before you learn to walk (p. 204). They are committed to avoiding shortcuts that do not usually go up, but often go astray to the left, to the right, or down to the drain.

The spirit of vigilance and adequacy leads to a deep appreciation of the pivotal role of peer monitoring. To be sound, knowledge production has to be a collective and social venture. The production process should involve dynamic cross-checking, which is what contemporary institutions of gatekeeping are designed to promote. Hence the book's characterization of quantitative sociology, as a dogmatic faith in the power of numbers and the like, should have been turned on its head. In fact, quantitative sociologists are among the most willing supporters of tough standards.²³ This support reflects their widespread sensitivity to and understanding of the many traps on the road to discovering and explaining social phenomena. The cautious spirit is not directed specifically to quantitative data and method, but to all styles of research. I am therefore not surprised that the author of the book feels the pinch from quantitative sociology even when his work is hardly ever reviewed by quantitative sociologists. But it would be a gross mistake to confuse the cautious spirit with a dogmatic faith in quantification.

While my representation of the culture of quantitative sociology is unlike the one in the book or similar philosophical critiques of quantitative social science, my view is not extraordinary. Even though I prefer to call

evidential rules are followed closely, crucial conclusions of the book fail to be supported. The enforcement of standards is not conducted with arm-chair debates, but with costly but adherents to logical and evidential rigor. This is modern quantitative sociology in action. Many similar instances occur at a smaller scale all the time.

23 I have been a participant observer in the review of many quantitative studies. This opportunity has provided me with a large informal data source of interactions between gatekeepers and quantitative sociologists as authors. One of the clearest norms about the duty

the scholarly standards as rules of logical and evidential adequacy, “objectivity” is much often the word used in the literature. By way of discussing the opportunity and danger of the new movement for public sociology, I have recently articulated a cautionary note about objectivity:

I have found many sociologists ... perpetuate the claim that scientific objectivity is an impossibility and a myth (even charge that the myth was constructed by professional sociology to buttress its prestige and dominance). It is time to bury this misleading and misguided caricature of scientific objectivity. The kind of objectivity that is ever relevant for the natural and social sciences I know is not an abstract state of mind. Scientifically relevant objectivity is known and practiced by scientists engaging in down-to-earth empirical research; it cannot be couched in such metaphysical terms as value free or the separation of the subjective and objective.

In practice, an objective research process is distinguished by two ingredients: (1) a rigorous demand for logical tightness in any reasoning of a scholarly work—in the interpretation of prior studies, establishing adequacy of an empirical claim, substantive inference (descriptive or causal) irrespective of the method of inquiry (ethnographic or large sample survey), and (2) a consistent demand for all empirical claims to be subject to verifiable testing, peer review, cross-examination, and cross-validation. The two ingredients, logical and empirical rigor, are the most intersubjective elements of knowledge building. By demanding any claims be subject to logical and empirical constraints, scientific discourse effectively ensures the externalization of the basis of legitimate claims and inferences. I would argue that this kind of

external and inter-subjective check is the essence of objectivity. There is nothing metaphysical and mysterious about it. (Tam 2004, p. 56)

It is reassuring to know that my view is pretty compatible with the findings of Porter's (1995) *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life*. Even though Porter's approach is social constructionist, the findings do not deny the possibility of objective knowledge. The findings demonstrate instead that cultures of objectivity across many domains and over the course of history are almost never a matter of metaphysical arguments and beliefs. Objectivity is a social, collective, and dynamic construction. But it is often driven by the need to respond to external pressure on weakly positioned elites for credibility to the public. This insight fits in well with my participant observation of quantitative sociology.²⁴

Don't Throw the Baby Out with the Bathwater

A key part of the Berlin wall of sociology is built upon the view that qualitative and quantitative traditions of research represent two divergent worlds of inquiries. I regard this view one of the most destructive and misguided myths of contemporary sociology.²⁵ Fortunately, the

of referees for quantitative sociology is well understood to be a rigorous cross-examination of logical and evidential adequacy, irrespective of the research design, sample size, and statistical techniques. Quantitative sociologists monitor peers to guard against sloppy reasoning and unwarranted interpretations.

24 After this essay was in the final stage of galley proofreading, I came across Chen's (陳瑞麟 2005) critical survey of the science wars literature and found his view of how science works highly consistent with my own position.

25 As far as I am concerned, different styles of research are complementary rather than mutually exclusive ways to get at reality. I can cite many examples of quantitative research drawing on qualitative research in major ways (e.g. quantitative social network studies building on Granovetter's articles on the strength of weak ties (1973) and the problem of social embeddedness (1985), two of the most frequently cited sociology articles).

quantitative sociologists I know the best do not fit the stereotypes in the book. None is a naïve worshipper of quantitative methods. To the contrary, all are realists who demand any style of research to measure up to a high standard of logical and evidential adequacy. They share a healthy spirit of skepticism (hence a demand for transparency, replicability, and so forth) and openness for cross-examination (to be challenged by alternative explanations and on methodological and empirical grounds). This spirit of disciplined inquiry is down-to-earth, social, and dynamic. They are not alone. The spirit is highly consistent with the one codified by three Harvard political scientists. Combining the insights of different research styles, King, Keohane, and Verba (1994) have made a powerful and influential case for the logical isomorphism between qualitative and quantitative inquiries. They argue that qualitative empirical research should and can confront the same set of methodological and inferential problems. There is no good reason to restrict the spirit of disciplined inquiry to quantitative research alone.

Among the top items on my wish list for qualitative research are two that will help lower the Berlin wall of sociology:

1. Skeptics and critics of quantitative sociology become appreciative of the hard lessons that quantitative researchers have learnt about traps for knowledge discovery throughout human history and in the everyday practice of empirical social science. For the conscientious and serious empirical researchers, adopting the quantitative approach is anything but cheap. These practitioners of quantitative sociology have individually and collectively paid a high price in order to uphold

Quantitative researchers do not routinely ignore qualitative studies. But the skeptical spirit of quantitative researchers does result in an extra caution toward those qualitative studies that seem to think they are virtually immune from potential problems of research design, data issues, and inferential problems.

26 The price is high because of the time, effort, printed pages devoted to document each

the lessons.²⁶

2. Qualitative researchers directly confront the challenge of King et al. (1994). They can demonstrate to the discipline that they are not short on logical and evidential rigor, that they do have rigorous ways to uphold the spirit of disciplined inquiry in practice,²⁷ that they have the cultural taste and the institutional support to make ensure the imprint of the spirit in the production of sociological knowledge. Or they can reject and replace the arguments of King et al.

If the two wish items come soon, there is a good chance that the Berlin wall of sociology will be crushed.

The reader should recognize that the current institutions of gatekeeping in Taiwan does not have to be motivated and sustained by a naïve ideological worship of Western sociology. The institutions that fundamentally changed the old power game actually serve the indispensable role of a guardian angel for Taiwanese sociology. The institutions help prevent the discipline from dominance by a handful of reviewers, minimize the pitfalls of deference to specific personalities, facilitate governance by multiple standards of excellence, and raise the deference to logical and empirical adequacy. The challenge for those who feel shortchanged by the institutional game of power is to propose a wise alternative arrangement that can accomplish as much with less negative side effects. In any event, sociologists should take heed of a folk wisdom for anyone who tries to do good: don't throw the baby out with the bathwater. The institutions of gatekeeping certainly need continuing improvements, but they also have irreplaceable functions. Alternative arrangements should be designed to guard the spirit of disciplined inquiry against the countless forces and pathways that can subvert it.

By now it should be clear that much of the Berlin wall of sociology is built and sustained by the reification of straw men, continuing ignorance

about how quantitative sociology actually works, untenable adherence to a metaphysical approach to defining the spirit of objectivity, an underappreciation of the institutions of gatekeeping for the production of sociological knowledge, and the lack of a serious response to appeals for unification (King et al. 1994). Hopefully the discipline is closer to demolishing the wall after the publication of this essay.

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