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d'une méthodologie inspirée des travaux de Max Weber sur les conditions de travail agricole en Europe centrale. Il termine le chapitre en ouvrant une réflexion sur les orientations de recherche susceptibles d'éclairer les composantes et les conditions préalables à une nouvelle constitution du travail, qui s'inscrirait dans le mouvement de *Constitutionnalisation du droit du travail* au Canada à l'œuvre depuis 2007.

Cet ouvrage, fort bien écrit, est riche en informations et facile à saisir. Il s'adresse aux jeunes chercheurs, aux étudiants et aux professeurs en RI désireux de comprendre les nouvelles relations d'emploi et leurs enjeux actuels. Chaque chapitre aborde une question d'actualité qu'il propose d'analyser à partir d'un ancrage conceptuel et dans une perspective historique. Aussi, le regroupement des chapitres par champ de spécialisation rend la lecture agréable et facilite la compréhension. Cette organisation de l'ouvrage et de ses chapitres permet au lecteur de suivre l'évolution des RI, à partir de ses origines comme domaine de recherche et d'enseignement. Autre attrait de ce livre, plusieurs pistes de recherche sont évoquées à la fin de chacun des chapitres. D'ailleurs, dans le dernier chapitre, P. Jalette formule plusieurs questions de recherche liées à la pandémie de la Covid-19 qu'il invite les RI à se saisir.

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Management Studies in Crisis: Fraud, Deception and Meaningful Research

By Dennis Tourish

(2019), New York: Cambridge University Press, 312 pages. ISBN: 978-1108480475.

Dennis Tourish's book, *Management Studies in Crisis*, is a work of erudition and, frankly, courage. It took guts to write and publish this book. And only a scholar with Tourish's accomplishments could hope to see their work published. This book has caused a major splash in the field. Last year there was a forum in the *Academy of Management Learning and Education Journal* (AMLE) about Tourish's work. The overall opinion was something less than positive. This should not surprise—scholars who publish in AMLE have a commitment to maintaining the status quo. I do not believe much will come of his book except for the occasional scholar mentioning agreement among friends. That is a sad comment. However, please do not take the lack of improvement to be a referendum on his book. It should be read by every management scholar; scholars in other business enterprises would profit from reading it as well.

Most of what you need to know about management research is revealed in Tourish's discussion of an article where the response to reviewers was much longer than the article itself. The time from submission to acceptance was a period of two years! Partly, such a review time suggests that there is little agreement on even what basic ideas mean. Take Tourish's discussion of theory. Various scholars have argued what theory means, to the point where some of them have thrown up their hands and told us what theory is not. Funny how we cannot define what science has defined for years. In addition, Tourish is also correct in saying that interesting data, which could lead to future research, may never see the light of

day. Likewise, null results, which could indicate that a theory does not work, may not be published either. That speaks of profound confirmation bias.

Tourish's discussion about the replication and reproduction of data is another excellent chapter. How could we be considered a science when we do not replicate or leave enough evidence to reproduce? Replication is the hallmark of science. Likewise, there is little understanding of basic concepts such as p -values. We accept 0.05 as the level of significance without bothering to quantify the actual difference. Even more damning is Tourish's chapter on frauds. Scholars are pressured to HARK, p -hack and torture the data. This is a common academic problem: significant and interesting results matter more than actual rigour. However, management scholars can take solace in the fact that no one cares about our research.

Another strong chapter is the one on the ranking of journals and predatory journals. I somewhat disagree with Tourish about the ranking of journals; there needs to be some metric to justify decisions on hiring, promotion and tenure. There are simply too many journals out there, and only a Faustian could master all the material. However, the ranking of journals is, in many ways, a political process. I wish Tourish had spent more time asking why some journals are ranked high and others are not. I believe there is a real bias in numerous rankings. I also wish Tourish had spent a bit more time on publishing cartels and how the game is rigged for some. To me that is a scandal that warrants reporting. But those are not real limitations. Like other good authors, Tourish forces the reader to think of additional lines of inquiry.

There is no such thing as a perfect book. Tourish's book has some limitations. The first one is that he bounces back and forth in some chapters (especially the ones on academic life and fraud) between various

academic disciplines, thus somewhat confusing his analysis. Likewise, the complaints about academic life and its stressfulness does not ring true to me. Academic life may not be as promised, but it is difficult to complain when we make rather high salaries for publishing nonsense. In addition, I wish Tourish had gone further. For example, he mentions one scholar who was "busted" for publishing papers with misleading data. What Tourish does not mention is that the same scholar is still publishing in high-level journals. That is a severe indictment of academia.

Another issue that goes unmentioned is the creep of cancel culture, which, granted, was not as bad when Tourish was writing his book. There are topics, such as diversity, where positions that oppose the orthodoxy would lead to the destruction of the career of any academic who takes a stand. Simply put, we cannot be a science if we cannot question and test our ideological positions. Another issue is raised in the first chapter when Tourish takes aim at Taylor and Mayo. Firstly, there is considerable debate on whether Taylor made a "pig-tale" and fudged the data. In fact, Taylor may have used the story to sell his idea. He was not publishing an academic paper. Likewise, Tourish is overly critical of the Hawthorne studies, as Mayo did not argue that their findings settled the issue on cooperation—a point he was attacked on. Nor were scholars at the time unfamiliar with the limitations of the studies. I wish Tourish had spent more time reading the literature than trying to make a quick point.

However, this book is an important work that makes some very strong points. The chapter on leadership is excellent and reveals the limitations of management research, Tourish is also correct in saying that history, anecdotes and qualitative evidence should play a greater role in research than they do currently. Right now, publishing in journals is based on connections and using Stata for

data analysis. Few practical or insightful ideas are emerging from modern management research. As one distinguished scholar told me, any manager who used work published in our high-level journals to manage would be fired within six months. I could not disagree. At least our forebears, however disreputable they may have been, dealt with issues that mattered. We better consider what Tourish says very carefully.

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Nothing Succeeds Like Failure: The Sad History of American Business Schools

By Steven Conn

New York: Cornell University Press.

Attacking business schools is like shooting fish in a barrel, as business schools make an easy target. Business schools feature more highly-paid professors, do not have a connection to traditional research, borrow social science methods (and then proceed to misuse them) and have better facilities, more funding and better furniture than do other academic institutions. For that reason, it should not surprise us, in any way, that we are hated on campus. Steven Conn has written a book deeply criticizing the modern business school. The book is worth a read but has some serious limitations. It offers occasional profound insights surrounded by some “snark” and what I would consider to be some “falsehoods.”

Let’s say a few words on Conn and the limitations of his work. First, he is an excellent historian and a fine writer. The sections on basic history are well-researched, insightful and well-written. That is especially true about his work on how business sought to become a profession. He is a first-class historian, and his academic career clearly

bears that out. Second, however, I am not sure that Conn has taken a class in business or even has any understanding of the field. Such a background greatly limits the scope of his criticism. Even worse, Conn seems neither willing nor interested in grappling with some of the larger issues of the field. Khurana’s book on the business school, while very critical, is more reasoned and understanding. Conn does not seem willing to approach the field with any sense of empathy or understanding, and that unwillingness limits his historical vision. Third, since he has not really interviewed or taken time to understand business professors, that failing limits his understanding of how a business school works.

Take an obvious issue: Conn’s writings on AACSB, which is the major accreditor of business education in the world. I would first like to make my credentials known: I am director of assessment at my business school. Therefore, I have worked directly with AACSB and understand the power they wield. Simply put, AACSB calls the tune, and schools are forced to dance to it. Strategy and business ethics are part of the curriculum because of AACSB. Business academics are forced to publish—even at teaching schools—because of AACSB. Losing AACSB certification would be damaging to any business school. Even institutions such as Harvard must show deference to AACSB. Yet Conn is very skeptical about the influence of AACSB.

Another issue: Conn is correct in saying that business schools do not spend a tremendous amount of time writing about business events, such as the Great Recession or the collapse of Enron. He is wrong in saying that those issues are not discussed in our classroom teaching and textbooks; both play a key role in ethics classes. We are silent in our research, but that silence is a function of the peer review process: it takes several years to publish a journal article. We are also devoted to theory building. Therefore,