

## How ICT will Influence Public Administration as a Scholarly Discipline

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### ABSTRACT

This paper asks how ICT will, i.e. might, change Public Administration as a scholarly discipline (PA) in the near future. Assuming mild techno-determinism, the Techno-Economic Paradigms approach, and that science is not primarily moved by science, but rather by economic dynamics, it first sketches out how PA looks today in the context of organized scholarship generally, and what tendencies (such as quantification, measurability, etc.) there are. Then, we look at what Information and Communication Technology can already do today and how it has changed the *Lebenswelt* by 2014. Two critical, interlinked phenomena are then analyzed: MOOCs (Massive Open Online Courses) and their effects, and the current ability of algorithms to write a certain type of texts. These may have the effect to strongly enforce, even lock in, the current tendencies of PA, but they may also give rise to an altogether different kind of development of scholarly inquiry in the discipline and beyond.

**Key words:** Public Administration, ICT, algorithms, MOOCs.

How Information and Communication Technology (ICT) influences, and especially will influence in the future, public administration (PA), is one of the most-researched and published – and talked-about – topics in our discipline, often under the label of e-Governance (see only *Information Polity* 2012 for a typical approach). This lecture, however, deals with ICT's influence on PA not in the sense of bureaucracy, but PA as a field of scholarly inquiry. In order to do that, I begin with three assumptions.

### 1. Assumptions

The first is what Carlota Perez has nicely called "mild techno-determinism" (Perez 2007). There is a conflict between people who think that history pushes technology and that we get new technologies to solve certain problems, and between others who

think that technology pushes history – technology unfolds by itself with its own logic and dynamic, and history and social arrangements only react to it. The second view is called “techno-determinism” because it holds that technology determines human affairs (Smith and Marx 1994; see also Dolata and Werle 2007), and “mild techno-determinism” means that you do not think that it fully determines it, that we are not just playing balls of an abstract technology, but that basically we can do very little about it. Technology, then, has a way to push human existence in a certain direction, which is not fully in our social control. This assumption is controversial, as any determinism is, but it is quite common and is gaining, once again, ground these days, perhaps ICT reality backs it up in a rather obvious way.

The second is a non-controversial assumption, and that is that ICT is the techno-economic paradigm of our times. That means that if we talk about relevant technology today, this is not waste-water management, it is not railroads, and it is certainly not cars or anything like that. It is also not anything ecological or pharmaceutical. But the technology that determines our lives, that pushes us around, in which we live – look at all of you having the screens open in front of yourself – is ICT (Perez 2002; see also Drechsler, Kattel and Reinert 2009).

So, technology is very important for our lives and the technology that is very important for our lives is ICT. And of course, ICT is also being pushed, that means it is not only automatically unfolding, but there are very, very strong powers that be that push ICT as the main technology on us and thus increase its importance.

The third assumption is that “science is not (primarily) moved by science”. Now this is something that academics do not like to admit very often; what it means is that science policy does not have a science logic. When we have reforms of our study plans, say the Bologna process or regarding study-fees laws from the government, or how we do research, about what we do research, careers of professors and so on – this is decided by politics for totally different reasons than out of science itself. We as scientists do not set our agenda at all. The Bologna process, for instance, was neither instituted nor pushed through because of, nor does it have any greater interest in, the academic sphere. It is a European political measure to create a certain cohesion within Europe. The entire idea of the Bologna process is Europeanization, not an increase of the quality of scholarship or studies, to which it surely has not led, either. You can, as a professor, not say, “This is the wrong reform, because it is detrimental to scholarship,” because it never was meant to be (Lotter 2008; see also Baltasiu and Bulumac 2013).

## **2. How does PA as a scholarly discipline operate today?**

Now for the first part: PA as a scholarly discipline today. PA as such is a field that in its natural state is without a methodology, because it is one of the fields that is created and brought together by its subject, not by its method. That subject is the working state. And traditionally PA has been done by lawyers, economists, public-finance people, governance scholars, bureaucracy scholars, management experts and so on and so on (see Drechsler 2001). But we live in a scholarly world in which method is more important than anything else, certainly more than subject. The application of methods has become the point, not a tool.

What happened during the last ca. 30 years is that international globalized PA borrowed its method. Very simplified, PA borrowed its methodology from political science. And political science borrowed its methodology also, just a little earlier, from economics. And economics did not have this kind of methodology before World War II, either, but subsequently, it borrowed its methodology from the hard sciences (see Drechsler 2011; cf. Raadschelders 2013). (That is even true for a current sub-stream of PA especially in the United States, i.e. to move towards experimental research, randomized controlled trials, “new behavioral PA” and so on, which also started first in economics and originally came from the hard(er) sciences; cf. Duflo 2007.) That is why today, we in PA, if totally watered down and still more in tendency than in practice, have more and more a methodology for top articles in top journals that pretend that we all are physics professors. (Groeneveld et al. 2014)

So we have a hard-science approach, we ideally want things expressed by formulae. Everything should be rigorous and empirical with hard data and reproducible experiments. That is the ideal, that is what we actually want (although in PA, of course, we hardly ever achieve it). Why do we want that? Because that is what physics people want, or wanted. In a world which trusts hard scientists, the economists borrowed their methodology from physics, so they looked more serious. The political scientists borrowed it from the economists, so that they looked more serious. And we in PA borrowed it from the political scientists. And why is this so? 100 years ago, people in the university did not want to look as if they were engineers or scientists. That was embarrassing. At Oxford or Cambridge, if someone said, “you look like an engineer”, this was an insult. (Snow 2001) Today, our society, our world privileges the natural sciences; the natural sciences are cool. We think what is true is what has been proven by a natural scientist. So we borrow their certainty for our field via this big cascade of methodological legitimacy.

We tend today to think of claims that are mantled in scientific attitude as more convincing than of those that are just based on anecdotes, although there often is no reason for this. If somebody tells you, for instance, that people in Belgium prefer chocolate over vanilla ice cream, you would say, “how do you know?” And anecdotes and experience would not be good enough. But if he says, “careful studies show that 87.73% of all Belgians prefer chocolate ice cream”, all of a sudden you would think, “oh, yeah, that’s a scientific fact”, although very likely it is less scientific than the first remark.

The leading journals in the social sciences promote this attitude. The top journal in political science is the *APSR*, published by the American Political Science Association. It actually makes room for one or two essays about thinking and theory, but all the other essays are very empirical, and the more math you have in it, the better. The scholarship in the top journals, also in PA – say, *JPART*, *PAR*, and so on – tends towards quantification, counting, not discussing something, and modeling; in PA admittedly less so than in most other social sciences, but the tendency is arguably clear. That means that ideally, I create a theoretical model by mathematical means, preferably of how things work.

This bias towards modeling and quantification strongly pushes articles that tell their readers what they already know. Anything that is based on models, anything that is based on quantification is a push towards something that is not new. You think

intuitively that an essay with a research line is correct if it confirms something you already know; so, you replicate what people want to hear. The recent Jan Hendrik Schön plagiarism case illustrates this very well – he wrote all kinds of articles, two top journal articles every month (we only have one person in PA who does this). Schön really had a mass production of articles, all of which he made up himself. The way he did this is that he did experiments with results that were exactly as all his colleagues expected them to be. So he did not have anything new, but all his essays corroborated the findings of senior colleagues, and for career purposes, that was fine (see Ross, n.d.).

The funny thing with all this quantifying, modeling methodology is that nobody in the hard sciences agrees with that anymore. Physicists believed something like that 70 years ago, and mathematicians believed it maybe 90 years ago. For physicists, all that precise modeling is laughable (see Drechsler 2011). But for us in the social sciences, it is *au courant*. Albert Einstein, who in general has a reputation of being a quite successful hard scientist – probably he is the iconic image of hard science in our modern times – in his famous Prussian Academy lecture of 1921 already said that the more mathematical modeling you do, the less the model will say about reality, and the more you talk about reality, the less you are precise (Einstein 1970).

In other words, operationalization, that means putting real-life questions into a testable formula, creates scenarios that are trivial. Anything that can be put into a formula is not interesting because it is so simple. If you can say, “this is the question, these are the databases, that is what we want to know, and now we are going to rigorously test it by so and so many interviews, n=this, and so on, and so on – you know this kind of article – Einstein, and not only Einstein, would say, that this is necessarily a trivial article. “Trivial” is a technical mathematical term, but in the social sciences, it is a normative judgment – if you say to social scientists that their essays are trivial, they are usually not happy. But this is a good reminder nonetheless. What is scientifically interesting is what you cannot operationalize. (Keep this thought in mind, it is very important later on.)

A consequence of “method first” is, of course, that it privileges form over content. We want a rigorous empirical kind of essay, never mind what questions it answers, rather than a new question, an interesting thought or anything like that. That would, as the saying goes, “not make it through peer review”. So we do not so much look for problems which we can investigate, or investigate phenomena that present themselves to us, that astonish us, which is actually the traditional basis of inquiry since Aristotle (*θαυμάζειν*). Rather, what we do – or at least what tendency we have – is that we look for obvious things that we can state in a “rigorous” manner in order to get a publication out of it, so that we have another publication, so that we have a good h-factor (Hirsch 2005), so that we are being promoted or gain prestige among our peers.

A very important yet ironic aspect of this is that rigorous papers like these are actually not rigorously proven to have any beneficial effect on society or science. There is no substantive reason that we do science like this. We might as well have a science culture that says, “Dress well, lean back, drink a lot of good wine and talk with your friends about problems of Greek philosophy.” That is also a way of doing science that has been done in the Western history, and there is no real reason why we

do not do it this way anymore, but do science the way we do now. Actually, there are a couple of economic and political reasons, including an explicit project culture due to an excessive lack of trust (cf. Aidarov and Drechsler 2013), but there is no scientific reason for it. We just happen to do science this way, and you cannot fight this system. This is the system you buy into if you enter contemporary mainstream academia, and you have to play that game by the rules.

The current trend, however, of how we measure whether an article is good or not is a move from input to output. And that is enabled by ICT. As I mentioned before: The usual way today of how you describe that a professor is good is that she has published in the right journals (yesterday the criterion was *what* she had published, with an emphasis on books). Why do you publish in the right journals? Because on average, more people read a good journal than a bad one. So if you get yourself into a good journal, more of your colleagues read what you are writing and will, perhaps, also cite it. So a journal is good because on average, articles in this journal are good. But that, of course, is a 1980s way of thinking, because today we have very large databases, the most famous one being Google Scholar ([scholar.google.de](http://scholar.google.de)) (but there are also payment-oriented ones like Thomson Reuters' Web of Science [thomsonreuters.com/thomson-reuters-web-of-science/](http://thomson-reuters-web-of-science/)), which track exactly how often individual essays are actually cited by one's colleagues (see Bakkalbasi et al. 2006). So the reason why we say that an essay is good because it appears in a specific journal is obsolete.

Today, and *a fortiori* tomorrow, even a working paper that you just put online yourself, if it is cited a lot, might be much "better" than an essay in a top journal. There are articles in the top journals in PA which do not ever get quoted by anyone, not even by the author's sister. They were written, essentially, for nothing. But 20 years ago one could not know very easily how many people quoted an article. Now "the Web" reads all the articles in the universe, and a few more. That is what Google is all about, to read everything. And so Google can tell you how many people cited your articles in a semi-objective way.

At least, this is what most places are tending towards – we are not there yet, but the road on which we are leads to assessing the quality of a professor with one number. All of us professors really hate this, but this is the logic of the ICT techno-economic paradigm. We are getting one number, and this, by and large, is the previously mentioned h-factor (Hirsch 2005; see also Bornmann and Daniel 2007). The h-factor is a mathematical model that indicates how many essays one has published that have been cited by at least as many people as articles you have published. So if you have an h-factor of 13, it means that you have at least 13 essays that have minimally been cited 13 times each. So you may have 100 essays, but many are not cited that often, but you have 13 essays of which the least-cited essay has been cited 13 times. So your h-factor of 13 is the number at which the number of your essays and the number of your citations meet. That is partially very nice because it privileges continuous publication of pretty decent stuff, and it is, of course, horrible for the one-book people who write one classic and nothing more, because that does not show at all. There are many problems with that approach, but it gives you, also – and not least – students, a very quick reference of how good somebody is.

This approach is strongly pushed by the need for competitive research funding. How important this is today depends on where you are. In Wallonia, for instance, the

situation is actually quite nice; there are professors who are tenured and who may think and talk about pretty much what they want, their own salary is at least fine. However, for more junior researchers, competitive research funding by the FNRS (<http://www.fnrs.be/>) is key for survival, and the FNRS asks applicants about citations and impact. In Estonia, if you do not get a substantial part of your own salary in research money, you are in trouble, no matter how senior you are. Recently, there was a big thing in the press about two University of Columbia anthropology professors who were fired because they brought less than 80% of their salary every year. And so their contracts were terminated. (Goldberg 2014) Yes, there are attempts and practices today to establish “social relevance” or “social impact” as additional criteria for measuring the quality of scholars and scholarship, but I would say that so far, all boils down to research, measured in top-level publications and/or their impact. There is also a serious, hitherto unsolved problem of how to measure social impact beyond appearance in media or board membership in NGO’s and the like – and the fact that, once again, what will be measured will not be the actual relevance or impact, but their indicators, i.e. at best the shadows at the cave’s wall.

Most of science and research money these days, and for well over a decade, comes from central agencies, either national or international (Connell 2004; Conraths and Smidt 2005). “International” in Europe is code for the EU – and national science-funding agencies often follow the EU trend. You really need this money to survive, to go to a conference, to buy a keyboard, to pay your PhD students, whatever. And the decision on whether you get competitive research money or not is assessed based on your quality and your track record and a couple of other things, including the project itself, of course, but how good you are in terms of publications, measured by input (top-journal essays) and/or output (citations), is crucial. So this is not just a matter of vanity, but it is about how you live and even whether you survive.

Now, what about scientists’ quest for the truth, social scientists wanting to change the oppressive world, social scientists trying to find out how the world really is held together? As my Tallinn colleague Rainer Kattel likes to say, today, science is really just a way for a segment of the middle class to live in peace. The argument is that most people who go into science these days do not want to know anything and that universities essentially promote the life that the monasteries did. If you are not fit enough for business, you go into science, because as a professor you can still live. But it only seems this way – in reality, of course, you are pushed into a business environment. All of us in science have to behave like entrepreneurs these days. If we do not, there is little money left and where there still is money left, these are niches that are rapidly dwindling away.

So in sum, this means for PA scholarship in the year 2015 that the incentive, that means what we should do, how the system pushes us to behave, is to write replicative essays, essays that say the same thing that we know, for mainstream journals, essays that will be cited often, that say exactly what our colleagues want to hear. By and large, this is what makes a career. And these are essays that put method over content and that tend towards the countable, the quantifiable, the rigorous, the scientific – towards what can be put, in essence, into a table, an equation, a graph.

### 3. What can ICT do?

What is, then, the state of information and communication technology, especially as it can influence PA scholarship, as it already exists? In this context, I like to say that in ICT, the future is here already. What that means is that most people, and the older they are, the less, are not fully aware of what is actually going on in ICT and what we already have achieved. The Snowden affair (see, e.g., Greenwald 2013) brought some of us a little closer to realizing how the world ticks and how many people try to know what we all think. The idea of all big social-media sites, as they will very openly say – both Eric Schmidt from Google and the chief finance officer of Facebook, Sheryl Sandberg, have said this (see Thompson 2010; Vis 2014) – is that they want to know what you want before you know it yourself. A key idea of social media, maybe the main one that drives it beyond the rhetoric of uniting people, is that information is there that knows better about yourself, who you are, what you feel, what you want and whom you love than you do.

One of the big horrors of science fiction was that we have machines like robots that imitate being human persons; that is already a pre-ICT fear; recall Olimpia in E.T.A. Hoffmann's *Sandmann*. (1958) But in order not to recognize that Olimpia is an automaton, your vision must be manipulated. When you chat with somebody, and that is not a real person, it is a computer program, an algorithm. The big horror was always that you have machines that are not recognizable anymore; more so, that lie to you and tell you that they are actually not machines but real persons (Dvorsky 2013a). A recent story had it that this had actually happened – lying algorithms that try to sell health insurance – it looked that way, but later, some people said that this had not actually been a robot but nice ladies in India with a heavy accent, who when they interact with you, have a selection of prerecorded statements, and they click on it to reply, so there is still a human in it, and that may well be (Dvorsky 2013b). But on the other hand, this showed how close the step to a computer telling you “I am a real person” really is (Nicks 2013). Again, this is not an AI movie. This is what happens right now. On 7 June 2014, a program called “Eugene Goostman” passed the Turing test (Furness 2014). And while there are well-known issues with the Turing test (Halpern 2006), the fact remains that this does make a difference, as we will see.

Academics in the past few years have been very much impressed by the rise of what is called “massive open online courses” or MOOCs. These days, everybody has to be into them. The main driver in science is what is cool and in fashion, and if everybody does it, you have to do it, cuz if not, you look like a loser. And you don't want to look like a loser – if this sounds like 14-year-olds, that is how it is (the usual fanboys: Economist 2012; Friedman 2013; Wenger 2013). MOOCs are mass-enrolment online courses, centrally offered, to which everybody can subscribe; the legatees of putting lectures online and long-distance learning. There is no business model for them yet, the creation of MOOCs is very expensive, but since everybody does it, you do it, too. You study something online, the teacher is online, maybe the exams are local, but often there are no exams. Some of the MOOCs are really successful, many of them have enrolments in the 100,000s, and there are a couple of important platforms, like Coursera, that offer them and bring them to about any connected household (Bös 2012). And then you get the story that

this is so nice for poor little children in poor countries, that they can get a top Harvard education, as well, and for free.

But the MOOCs have an interesting consequence, and this now slowly ties together what I have been talking about so far. If you have an online course for 200,000 people, what kind of exam can you give? How can this be graded? You can only ask questions that are basically multiple-choice checkable or checkable by computer or other infrastructure, because otherwise it is not possible. So that is one of the influences of the MOOCs, and if MOOCs get more popular, that, in return, has an influence on science. MOOCs privilege knowledge that is replicable and general and usual. You can basically not have courses for a lot of people in which you ask essay questions for students to react in a nuanced way to complicated problems. Of course, the more literary computers become, the more they can ask complex-looking questions, because they can understand, and judge, the answers to them as well. On the other hand, the tendency to ask simple, easily evaluated questions is not only technology-driven, but it also conforms to the logic both of large Bologna-type classes and of teaching being a quite low priority in an academic system where – often existentially necessary – funding is allotted based on anything but good teaching, however measured.

So the MOOCs are pushing the very technical approach which we talked about before and which, remember, is our approach in PA, and in university teaching generally, anyway. This has two consequences: First, an already visible split between elite and virtual education, i.e. actually it is not so that now the poor people from the provinces get a Harvard education. Harvard people still get a Harvard education, for which they pay a lot of money, tens of thousands every year or semester, and whether the education is worth it or not may be debatable (I myself think it actually is), but the networking surely is (Rothman 2014). So you get a mass education for the masses, and the top people still get to talk in an exclusive environment with the top professors (I do not mean top people in an intelligence sense, but in a money one) (see Allen 2013; Harvard Magazine 2013).

The second key consequence of MOOCs, more important for us, is mainstreaming and non-specificity. If a school says, “we borrow the accounting class from Ohio State, and everybody in the world takes the Ohio State accounting class”, what that means is that everybody learns Ohio State accounting. Now with accounting this may be okay, and with introduction to mechanics, and with a survey of astronomy. But in our field, in PA, if absolutely everybody takes the intro class from Ohio State, there is no specificity anymore, there is no different methodology, there is no way for a young scholar or a very senior one to challenge the mainstream, because the mainstream has become the law (see Bustillos 2013; Heller 2013) – and that in a field that does not even have standard textbooks.

This might not sound very horrible, but actually this is a serious horror story if one believes in context, legacy and specificity. Because what does it mean that everybody does the same in the world? That means that everybody does what is done in the United States, because that is, of course, with English but not only because of that, the nation that is dominating the science world and particularly, together with Britain, the PA world. (Drechsler 2013) And that is the type of PA we have talked about, the one inherited from political science, economics, and, ultimately, 19<sup>th</sup>-century physics.



This brings me to my connecting point, and that is that algorithms, computer programs can already write essays today (Lohr 2011). We already live in a world today in which some normal human texts, or what sound like normal human texts, have not been created by a human, have never even been revised or checked or edited by one. Texts can be written by machines. But what kind of texts can be written by machines? Writing is done by machines already especially if just data change, but how they are put together remains, over the years and decades perhaps, the same. What the computer programs do is that they look at a field, then they see how humans have written about it before, they see that just the data change, sometimes this, sometimes that, and then they take the sentences, of tens, hundreds, thousands, maybe soon millions of texts on the same subject, tie the particles and connections around new information and present it to you as a report, or statement, or even an essay. Once again, we are not talking about the future, we are talking about now, and this is not realized that often (Lobe 2014).

It started, apparently, with sports reports. For a lot of people in the world, in the newspaper, this is the central part, or it was when we still had paper papers (see Alterman 2008), but even today, sports reporting is key. You read about how people played soccer and who won. But if you think about it, even live soccer reporting is always the same. There is always an inflated piece of leather, and some young men, sometimes women, run behind it, trying to catch it and putting it into the goal. It is always the same story, always the same people. There is nothing new in sports, really. It is an internal game that goes on and on and on. Anything that has happened in soccer has basically happened before. So all you actually need is the hard information: ball goes from A to B, you can know that by tagging, and then you say, even live, “Yes, yes, yes, he should be ... there is a struggle here ... he is taking over ... he is not taking over ... he is going in ... yeah, goal!” and things like that, and in the end, it sounds exciting – but any machine can do that.

The first reports that were actually done by computers concerned, as it seems, American football, not the major league but regional or college football. This was apparently done by a firm that is called Narrative Science, and their motto is, “retransform data into stories and insight”. That sounds cool, and it also sounds really scary. And this has steadily increased. So there is sports reporting in the media already that has not been written by humans, because it is, essentially, always the same, and nobody notices (Kurz and Rieger 2013, 250-251, 260-261; Lobe 2014).

Another example is weather forecasting. If you go to any of these weather pages, like weather.com or cnn.weather or anything like that, which are very popular and thus lucrative, as they attract a lot of viewers, what you will see is that there is usually not just graphics and tables but also a text, “tomorrow it’s getting more cloudy but still warm in Mannheim, but we expect some more sunshine on the weekend.” Nobody has actually written this; this has been generated. It is like the text from your GPS. The weather data are there, and a computer program ties this into language.

Yet another by now typical way for computers to write essays are the quarterly reports of firms (Kurz and Rieger 2013, 251). That means if a text is nothing but a story based on data, a computer program is probably even better than you at writing the text around it. And so these texts – sports texts, weather texts, report texts – can

be written by computers. Whenever prose narrates a table, algorithms can write it (Kurz and Rieger 2013, 251). By now, this has even reached the level of normal news (Dorrier 2014; Lobe 2014).

This even pertains to posing questions, as well – research, if you will. Wolfram Alpha for instance, you ask a certain question, and then it goes through the archives, finds this thing out and gives you the answer in somewhat nice prose (Wolfram Alpha some of you might know because if you use Siri, Wolfram Alpha is behind it to a good extent; see Sterling 2012). And so you could even go and find your research question and ask one of these computers, and they will answer the question – and they have done so for quite some time already (see Spivack 2009).

So, anything where I have the same kind of essay, just a variant of questions where the core of knowledge is a table or a quantification the outcome of which I then report, is something that can be done by machines. They can also do the basic research, they can probably do this better than you, the more so the more databases we have, and then they can put it together as an article. And precisely this structure, this shape, this content is typical, as I explained *supra*, of what counts for the best or most successful PA articles today.

We are therefore at a point in time right now when normal mainstream PA articles can almost be written by machines. When I mentioned that to the managing editor of *Public Money and Management*, she said, “Oh my God, that means you and I are going to be unemployed.” I, of course, said, no, that *Public Money and Management* is actually a really good journal, with many essays where this would be impossible, but it depends on what essay you write. Mainstream PA articles of the variety I described are in principle machine-makeable. (And in fact, but this is still close to science fiction, when machines will be able to grade standard student papers, it would hardly be a problem for them to review this kind of PA articles as well, potentially cutting out the human middle-man altogether.)

#### 4. Prospects

So in sum we can say that MOOCs (etc.) push PA scholarship further towards research that can basically be done, and that can be done soon, by machines, and this kind of research is our default anyway. So in a sense, PA scholarship is on the road to becoming obsolete, and the business we PA scholars are in is like owning a bookshop. Remember bookshops, shops you went in to buy books? Bookshops are quaint and cute and nice and dead. We buy books from Amazon (Giersberg 2011; Siracusa 2009).

But of course, this is a highly pessimistic scenario, and there is no reason to be pessimistic unless one has to be. One of the fathers of the ICT world, Nikolai Tesla, when talking about computers taking over – and he was one of the pioneers of that as well –, famously said,

Today the robot is an accepted fact, but the principle has not been pushed far enough. In the twenty-first century the robot will take the place which slave labor occupied in ancient civilization. There is no reason at all why most of this should not come to pass in less than a century, freeing mankind to pursue its higher aspirations (Tesla 1935, 7).

Two German authors and internet activists, Kurz and Rieger, who wrote a book called *Free of Labor*, which is a general account of machines replacing people, especially in intellectual jobs (2013; see also Frey and Osborne 2013), have pointed out, talking about science, that if robots wrote all these boring essays, scholars could focus on interesting ones again (Kurz and Rieger 2013, 272-273; see also Paul 2014). So we could, if we wanted, leave “Mid-Level Civil-Service Motivation in Ministries in Sweden and Finland Compared” and the like to the algorithms. Those papers often do not need humans to write them, sometimes not even machine-like humans. (It would be interesting, if not very collegial, to go through a list of PA scholars and categorize their work by machine-writable or not.) The “traditional” scholars could get back to the aforementioned seminar room with some good students to seriously discuss what matters as far as the institutions of the state are concerned (which is of serious policy relevance, as well; see Drechsler 2015, 2001).

We thus might actually see the return of classical scholarship, exactly when we need it, because of the challenges to mankind that we are facing because of ICT, including the shift in human self-identity. So the positive story would be for us to say that because of this insight, we will quit writing machine articles and switch back to writing scholarly articles. The dynamics of Western-global science is not like that today, because funding is not allotted thus, but tomorrow that may change. And it certainly will be more likely to change if those of us who share the perspective keep pushing, in the various roles they have within the scientific world and outside of it, into the right direction.

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