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Forms of Knowledge for the Practice of Public Administration

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Forms of Knowledge for the Practice of Public Administration

Edoardo Ongaro

In this chapter of the *EPPA I* book I suggest that *knowledge for the practice of public administration comes in three main forms*:

- “Enlightening” knowledge: the social-scientific, “theoretical” knowledge generated according to the (highest) standards of the scientific community;
- *Problem-orientated knowledge*: the kind of knowledge that is mostly produced by practitioners engaged in professional social inquiry and driven by societal problems rather than by the generation of additional knowledge *per se*, as in discipline-orientated academic knowledge;
- *Practice-extrapolation knowledge*: a kind of knowledge that is embodied into “practices that work,” i.e. knowledge derived by learning from actual experiences of tackling a problem and improving a public service or process, practices typically contrived by practitioners engaged in public services, that are spotted, showcased and analysed for extrapolation and transfer elsewhere.

All three of these forms are significant for the development of the practice of public administration – hereafter PA (intended broadly to encompass the areas of “public policy and administration,” “public governance” and “public management”) – and *my main argument is that these forms of knowledge have to be seen and to be used in a joint way for progress in the practice of PA to occur*, in Europe and elsewhere.

I do not discuss the actors of these forms of knowledge: how academics or practitioners navigate more or less successfully the terrain of “academic research for practice-orientated purposes” – the territory that Christopher Pollitt has called “shadowland” (Pollitt, 2019). The nature of this “middle land,” the (non-)confidence with which either academics or practitioners get out of their comfort zone and traverse this land, the motivations and inclinations that drive them to do so and how they interact with each other along the way,

the “logistical tensions that arise when [academics are] trying to make an impact – the different timelines of the academic and ‘real world’; clashing incentive structures of different professionals; unequal resource endowment; and, the lack of formal power on the academic side” (Dunlop, 2019; see also Pollitt in this book). At the end of the chapter the role of the EGPA – the European Group for Public Administration – as a learned society in fostering the generation and spread of knowledge about the practice of PA is discussed.

1 Enlightening knowledge

The starting point of the argument in support of the practical relevance of this first form of knowledge is (deceptively) summed up in the most famous statement “*Nothing is more practical than a good theory.*” According to McCain’s review of the usage of this expression (McCain, 2016), “Lewin’s Maxim can be found in three forms – ‘there is nothing as practical as a good theory’ (Lewin, 1943, 1944, 1945), ‘there is nothing so practical as a good theory’ (Lewin, 1951; Marrow, 1969), and ‘there is nothing more practical than a good theory’ (generally attributed to Lewin but no canonical source known),” to which prominent management theorist van de Ven (1989) has added his own, slightly more cautious, tweak: “Nothing is quite so practical as a good theory.”

The basic idea underpinning the argument is that shedding light on phenomena – understanding the causes of things: *rerum cognoscere causas* (the expression is drawn from the great Roman Poet Virgil: “*Felix, qui potuit rerum cognoscere causas*,” verse 490 of Book 2 of the Georgics, which may be translated as: “a fortunate life is that of the person who was able to know the causes of things,” a reference which has been adopted as its motto by the London School of Economics and Political Science) – is a practically valuable form of knowledge in and by itself, enabling (though not coinciding with) practical reasoning (one of the two main forms of reasoning, according to the ancient Greek Philosopher Aristotle) and the pursuit of value-laden social action (a concept most effectively condensed in the ancient Greek term: *praxis*). In this line of argument, it is theoretical knowledge that, by definition, sheds light on phenomena.

What, then, is theoretical knowledge? Consistently with the overall thrust of this edited volume which has a different format from “standard” academic pieces and rather aims at indicating broad lines of development of and for PA from European perspectives, we do not dwell here on definitions of theory. Rather, we start from a conventional definition of PA as the interdisciplinary study of government in action. Bauer speaks of “a multidisciplinary endeavour

with a prime focus on studying government in order to produce insights to improve government practice” (Bauer, 2018). Theoretical knowledge is the knowledge about the causes of things generated in accordance with the scientific standards adopted by the respective disciplines employed to study PA, a list which includes but is not limited to: political science, management, organisation science, law, sociology, economics, and social psychology. It is further integrated by a wider range of disciplines from across the social-natural sciences divide, and notably in the applied fields, thereby including on a more sectoral basis inputs from disciplines ranging from public economics to criminology, from engineering to architecture and urban development, from informatics to medicine, and so on. Hence – the argument runs – the same level of rigour that applies to each of the constituent disciplines in advancing theory applies when such disciplines, often in conjunction, are employed to generate theoretical knowledge of PA (the root word for science in Latin – the verb “*scire*” – refers to “knowing,” and the Greek root word for epistemology denotes “rigour,” so scientific-theoretical knowledge of PA consists of knowledge obtained through rigour in investigating the administrative phenomena).

Importantly, in our definition PA is both a science and – also and constitutively – an *art*, a *profession*, and, crucially in our view, a form of *humanism*. The conception of PA as an art and a profession is well consolidated; more distinctive is the notion of PA also as a humanism. PA as “government in action” or the administration of the public institutions and the delivery of public goods, is about *wisdom* and practical reasoning, it is about the making of *value judgments* and not just factual judgments, it is about the *understanding* of things as much as it is about the knowing of things – along all of these dimensions PA, and notably the practice of PA, is a form of humanism. And we could and should add that the very term “explanation” requires qualifications when used in the field PA: in fact “explanation” in PA means identifying the causes of something – the standard notion of explanation in the natural and social sciences alike – but it may also mean “attributing meaning” to something. Possibly this interpretation of PA as both a science and a humanism may better be understood in the sense conveyed by the German language term of *Wissenschaft*, a field of intellectual inquiry, with an applied thrust and practical implications.

If we agree that PA is (also) a humanism, then we need to adopt definitions of theoretical knowledge as encompassing what theory means also in such disciplines like cultural anthropology (as so aptly illustrated by Christopher Hood’s 1998 work which deploys Mary Douglas’ *grid-group* cultural theory to PA, and which is so significantly entitled and subtitled “The Art of the State:

Culture, Rhetoric and Public Management”), or what theory may mean more broadly in humanities like history (historiography, where theory may mean the drawing of “limited historical generalisations” from in-depth investigations into individual which produce accounts of unique historical episodes), or in human geography (where “contextualised” theories sensitive to spatial and temporal circumstances may be the main currency). Indeed in this conception of PA we should revisit the very foundations of theoretical knowledge into philosophy and philosophical thought (in its articulate branches, from ontology to political philosophy, from epistemology to ethics, which tackle the big questions about reality underpinning and giving sense to any effort to generate knowledge). Indeed, we may qualify PA as an “academic discipline” provided the whole gamut of PA as the interdisciplinary study of government, the art, the profession, and the humanism are considered in an integrated way.

I make the argument for philosophy, as an academic field, to be foundational for PA (Ongaro, 2017). This consideration leads us to another implication of the nature of PA as also a humanism: that the languages utilised to give meaning to PA do matter. In fact, concepts and notions in the humanities and the social sciences may travel much less well when translated from and into the English language than what happens for the natural sciences.

To illustrate, such English-language terms as “governance,” “accountability,” “management,” “leadership,” “performance,” “agency,” “policy” and the like may have nuanced meanings which become problematic to transmit and convey both when translated from English into another language and when used in the original English in the scholarly or especially the public debate of a country where English is not a national language. Analogously, words in other languages, like the Italian *azienda* or the Croatian *uhljeb* or the German *Regierungspräsidien* or the Hungarian *jogalkalmazás*, may equally not travel that well into English – the relationship of signifiers and signified may entail meanings which are subtly different across languages. This consideration does not necessarily entail any form of radical inter-subjectivism or relativism: linguists like Noam Chomsky remind us of the possibility of distinguishing between superficial structure and deeper structure of a sentence; but it remains a constitutive feature of PA, in its composite nature of a social science, an art, a profession and a humanism, that language plurality matters: this trait demands to be paid substantive attention, and Europe – a region of the world with plenty of national languages each enjoying a rich intellectual history, and all scattered one next to the other in that small hem of the Eurasian landmass which forms the European continent – is a natural laboratory for vetting the implications of language differences for PA. The favourable conditions available in Europe for studying the influence of languages on PA are also

reinforced by the very compact and highly developed PA scholarly community present in Europe. This PA community which works together is facilitated also by infrastructures like the European Union institutions for the funding of research, which supports research provided it is pan-European in nature, and by the fact of having a natural home like the learned society, the *European Group for Public Administration* – EGPA – where PA scholars can socialise and arrange ambitious pan-European research. Adopting the language prism for PA entails addressing in an explicit way the issue of the implications of language diversity for the study of PA, and focusing the problem of what is lost in translation: this topic is tackled in a dedicated chapter in Ongaro & van Thile (2018b), which discusses the issues around how English words like the ones mentioned above travel into the PA scholarly and public debate of 18 European countries.

We have made the argument that theoretical, “academic” knowledge may contribute to the practice of PA by virtue of its enlightening potency. *What is, then, the impact of academic knowledge, and how can such impact be gauged?* It is a European country, the UK, that has been leading the way worldwide in endeavouring to assess the impact of academic research, that is, academic-generated knowledge. In fact, a major exercise in attempting to measure the impact of knowledge has been carried out in the UK since the inclusion, from the 2014 round of evaluation of the research produced by the UK higher education institutions, in the research assessment of a specific category “impact,” which was weighted as 20% of the entire assessment and influenced the allocation of around £1.6 billion worth of public funds. This exercise has been considered a major attempt to assess the impact of scientific knowledge (Pollitt, 2016).

Before delving more into what it means to assess the practical impact of research, we should notice that this thrust towards assessing the impact of research is not confined to one, however major, initiative, but is rather part of a growing eco-system: since 2009 researchers in the UK submitting a grant application have had to delineate the expected “pathways to impact” of their research (Australia and Hong Kong introduced a similar requirement in respectively 2014 and 2017).

Within this exercise of evaluation of research, impact is defined in terms of how basic, fundamental research engendered “an effect on, change or benefit to the economy, society, culture, public policy or services, health, the environment or quality of life, beyond academia” (HEFCE, 2011) – a very broad definition, made even more encompassing in the UK’s Economic and Social Research Council (ESRC) threefold definition where non-academic impact can be: *instrumental*: influencing the development of policy, practice or service

provision, shaping legislation, altering behaviour; *conceptual*: contributing to the understanding of policy issues, reframing debates; or *capacity building*: through technical and personal skill development (<https://esrc.ukri.org/research/impact-toolkit/what-is-impact/>, accessed 12 April 2019). Importantly, the inclusion of impact in the assessment of research is not implying that all research has impact: the category is that of *possibility*: research *may* have an impact, and if and when such is the case such impact can be evaluated.

Claire Dunlop has investigated the evidence about the impact of research in the field of PA, as collectable from the 2014 exercise of evaluation of research in the UK (called REF 2014: the Research Excellence Framework for the period from 2008 to 2013, followed by REF 2021, the Research Excellence Framework covering the period from 2014 to 2021). Within this assessment exercise, PA has had a lion's share within the social scientific field of "Politics and International Relations," with 48 out of 163, or 23%, of impact case studies in the disciplinary field being classified under the label of "public policy and administration impact case studies": more than any other sub-field within "Politics and International Relations." PA has also had a non-irrelevant presence within business and management studies, with 39 out of 410, or 9.5% (see Dunlop, 2018 and 2019, where limitations to this analysis are also discussed: e.g. that some cases have undergone a non-disclosure procedure for issues of confidentiality, and hence are not included in these data, or more importantly that these impact case studies have been self-selected by the universities and units of assessment undergoing an evaluation process, and presented within the context of, at a more technical level, a strict format and auditing procedure, and, at a more sociology-of-knowledge level, within the context of the dominating purpose of receiving recognition and funding for one's own institution, hence with a high stake in the evaluation game).

It is important to emphasise that what has been measured in this assessment exercise is not the academic impact (like the number of citations or other indicators of how successful academically a publication has been), nor scientific dissemination. What is being detected in the REF evaluation of the impact of research is whether and how the outcomes of academic research have had impact on a social or a policy issue.

A number of considerations arise from Dunlop's study. First, impact takes various forms: from contributing to public debate, to engendering a novel practice that has been made possible to contrive as a result of academic research, to setting an issue on the policy agenda... to "stopping a decision"(!), when an impact case pointed to academic knowledge having shed light on possible (likely) negative effects of a policy decision which eventually led to it being turned down. A second and not unexpected consideration is

that impact takes time to manifest itself (hence reinforcing the argument originally in Weiss, 1977, and developed by Alkin, 2013), and it is unlikely that enlightening knowledge may manifest any meaningful influence on the practice of PA (or any other domain for that matter) over short time spans. A third observation is that in most cases the impact of academic research is about “influencing the elite”: the connections between academic and elite (decision-makers’) circuits continue to represent the main conduit linking the results of research to the practice of PA: the impact of research in the field of PA on “the general public” remains a rarity.

In terms of which PA themes have found their way in the first systematic exercise to assess the impact of academic research, the analysis conducted by Dunlop (2019) detects that “[w]hen we look at the universe of 90 case studies over half of them (56%) are primarily focussed on effectiveness (...) Equity or equality and efficiency themes are each covered in 20 per cent of the cases. The major finding is that only 4 per cent of the public administration case studies address the big legitimacy themes – with three addressing corruption and transparency and one on privacy. Incredibly, we found no studies primarily addressing trust in government or accountability.” Whether this finding is of any generalisability, to Europe or beyond, we cannot claim in a warranted way, but it does send a strong message: that the key issue of trust (in PA, and of PA towards citizens), legitimacy (of PA in citizens’ eyes), and accountability are domains of inquiry ripe for more research in general and surely for more *impactful* research in the field of PA, as they seem to be mostly off the radar of academics when they reckon whether their research has had an impact on society.

To sum up on this form of knowledge: we argue that theoretical knowledge can have an impact on PA, and evidence from the UK REF 2014 evaluation of research exercise does add confidence to the claim. We could even argue that the impact agenda is on the rise: with all the caveats that we have recalled here (and more are discussed in Dunlop, 2018 and 2019), the emphasis placed on impact by the 2014 REF has been reinforced in the 2021 REF, in which impact is weighted as 25% of central government academic funding for research: this may well be a sign that the pendulum – in PA as well as, more generally, in academic research – may be swinging back from an (over-)emphasis on the criterion of “rigour” towards (re-)giving prominence to the criterion of “relevance.” The message might be gaining traction that research must also be relevant, that it is of limited value to “know more and more about less and less,” that we must be able to generate knowledge that is relevant –possibly also sacrificing a bit of the rigour along the way to gain the prize of being able to make a difference, for the good, to society. We might further qualify the claim by arguing that, logically, relevance in research is not necessarily

in trade-off with rigour: that relevance and rigour are not by necessity in opposition. But at the very minimum we should not be afraid of arguing that research that is relevant be not displaced by the obsessive search for “rigour only” to which so many scholars are driven by a number of academic rankings and fashionable trends in designing academic career paths that seem to be so dominant in many universities. Ultimately, we should put more emphasis on the adjective “good” when we (rightly) state that “there is nothing more practical than a good theory”: “good” does not refer only to the rigorous procedures through which theory has been built and tested, but also to the relevance of the domain of application of the theory itself.

2 Problem-orientated knowledge

It has long been debated whether, and the conditions under which, social scientific knowledge is (or should be) structured around “problems” rather than around disciplines and their distinctive research questions (as is the dominant format of organising knowledge in academia).

One horn of the debate concerns the reasons why academia is organised around disciplines, and such reasons range from more functional ones, inherent in the nature of the discipline, to more historical-institutional ones, whereby past choices continue to have a huge influence and shape the way in which the production of knowledge is being organised in academia. The other horn of the debate is the conditions under which knowledge can be supplied, and the extent to which public policy-makers are in demand of it: the giving and taking of advice – in other words, whether there is a supply and demand, a market for problem-orientated knowledge.

Colin and Carole Talbot (2018) make the case for problem-orientated social science (drawing from the works of authors like Lindblom and Cohen). The authors consider problem-orientated knowledge as a development with the potential to provide a more productive framework through which academics and policy-makers can interact than the extant organisation of knowledge in academia and a framework within which the taking of advice by practitioners and policy-makers may be more productive than the way in which it is currently framed, in most instances.

There are two key points to the argument. The first point is that this professional social science is very significant, as a minimum for its sheer size, as I argue using the case of the UK. The second point is that problem-orientated knowledge is based on different assumptions from the discipline-orientated knowledge that is the dominant format in academia.

Starting from the consideration of size, Talbot and Talbot observe that professional social inquiry is broader than academe:

“[T]here are many more professional practitioners of ‘professional social inquiry’ (PSI) than merely academics. These non-university practitioners include government officials (at all levels), public service agencies, businesses, voluntary and campaigning organisations, trade unions, political parties, think-tanks, consultancies, market researchers, opinion-pollsters, media organisations and professional groups. The people active in these sorts of organisations and functions are not mere ‘translators and mediators’ but active producers of social scientific knowledge. Given the UK produces more than 600,000 social science graduates per year it is fairly likely that most of these non-academic PSI producers are themselves academically trained in the social sciences, at least to degree level. We estimate there are about 10,500 academic social scientists in the base disciplines (Economics 3,000; Politics 1,750; Sociology 3,400; Social Psychology 1,500; Anthropology 800). There are about another 25,000 in other social science disciplines that are ‘intermediary’ or ‘applied’ (Business Studies; Management Studies; Social Work; Social Policy; Human and Social Geography; Media Studies; Marketing; Accounting; HRM; Tourism; Information Services; Planning; Publicity Studies; Publishing)(Categories and numbers from Bastow et al 2014). This brings the total of academic practitioners of social science to somewhere around 35,000 in the UK. We have estimated that there are perhaps between 350,000 and 500,000 non-academic practitioners of Professional Social Inquiry generating social knowledge. This knowledge may be of variable quality but as a great deal of it is generated for serious purposes and consumes substantial amounts of resources (e.g. in government and business) we can assume that a reasonable proportion is at least as good quality as academic outputs.” (Talbot & Talbot, 2018:1216).

Hence a first consideration is that problem-orientated social science in general (and for public policy and public management in particular) is simply “out there,” overlooked if not outright disregarded by academia, but potentially available for tapping.

Most important is the second point: that problem-orientated knowledge is based on different assumptions from discipline-orientated knowledge, for the very fact of being driven by problems rather than the internal organisation of knowledge within a discipline. Governments, businesses and charities start out with social problems or issues they want to tackle, not the consideration of where, in which academic discipline, the problem they are interested in lies,

which is a central concern for many academic social scientists. The “knowledge production model” for non-academic professional social inquiry is therefore radically different from the academic equivalent in most instances (Talbot & Talbot, 2018). This framework is in sharp contrast to discipline-orientated knowledge, whereby research is designed to meet the needs of the discipline for generating more knowledge, rather than addressing social issues. Discipline-orientated knowledge is concerned with accumulating knowledge within the discipline, it is focused on *more* knowledge; problem-orientated knowledge is instead focused on a different kind of knowledge: indeed, whatever knowledge provided it addresses the problem.

One factor driving further the divergence between the way in which academia is organised and problem-orientated knowledge is that performance management systems within higher education institutions are driving towards an extreme emphasis on disciplinary-orientated, academic knowledge, at least in the West and in those non-western countries which are more directly importing their academic models from the US or the UK. Surely in most of Europe they put a premium on discipline-orientated knowledge and generate a disadvantage for those academics engaging in problem-orientated research (although the growth in significance of the evaluation of the impact of research, that we have discussed in the previous section, might be a tensor rather pulling in the direction of emphasising problem-orientated research).

Most fundamentally, the point is that problem-orientated research is based on a different framework – we might call it a “paradigm” – from discipline-orientated research. Problem-orientated research is inherently interdisciplinary: it resorts to whatever discipline may provide knowledge that is usable to address the extant societal issue. And, a second key distinguishing feature, problem-orientated research has a problem focus: it is driven by problems rather than by the generation of additional knowledge *per se*. To illustrate with a concrete example, for those acquainted with the structure of an academic paper or book, it will be all too familiar that the second section (or thereabouts) of the paper is devoted to the literature review, whose aim is spotting a gap in the literature and arguing that the rest of the paper will successfully fill such gap. This passage of having the literature review section of the paper pursuing the function of demonstrating that the paper adds something new to the very literature that is being reviewed has been rehearsed so many times (especially for those who have reviewed thousands of academic papers, and, as a journal editor and an active external reviewer for some forty journals, the author of this chapter happens to be in such condition) that one may be induced to overlook its significance, but it does reflect an implicit paradigmatic choice of overarching importance: it reinforces the

claim that research is worth pursuing if it fills a gap in the accumulation of knowledge within the reference discipline. The specific piece of research in the paper might or might not argue explicitly about the societal relevance of the research that is being presented (some will argue thus and others will not), but ultimately the literature review section of the paper conveys a second and deeper message than the specific informative contents it contains: that the function pursued by the research presented in the paper is to contribute to the discipline, to accumulate knowledge within the framework of the discipline. Problem-orientated knowledge in a sense adopts the opposite approach, in the most fundamental sense: since it takes the move from a societal problem, research within the paradigm of problem-orientated knowledge craves for finding already in the literature the answer to its problem (or at least to part of it). For a researcher engaged in discipline-orientated research, finding out through literature review that the research question has already been answered may induce dejection: the thrust is being able to demonstrate that something has not been said, in order to be able to argue that our own research adds something (and hence the publication counts for academic promotion!). The overall thrust of reviewing the literature in problem-orientated research is – or at least should be – the other way round: the best (albeit rare) case is when it is sufficient to review the literature in order to be able to solve the extant societal problem. Only after it has been tested that the answer is not already available in the literature, does the problem-orientated researcher engage in social-scientific inquiry in manners that methods-wise are not that dissimilar to discipline-orientated knowledge. Even if the methods are similar, the direction of travel and overall thrust are radically different.

Other main features of problem-orientated knowledge unearthed by Talbot and Talbot include that problem-orientated research tends to envisage a broad participation of stakeholders, who are not only respondents or the objects of the inquiry, but will tend to have a steering role in the process of generation of research. Problem-orientated research may also tend more towards experimentalism – by this we do not mean experimentation as the favoured research method, rather a general attitude towards trying out “whatever works” if it is deemed to be helpful in addressing the pressing concern that drives the research effort.

In sum, the problem-orientated form of knowledge tends to unfold and develop around a different paradigmatic base from the discipline-, theory-building-orientated “enlightening” knowledge considered in the previous section.

In concluding this section, it is worth mentioning *action research*, on which there is a wide literature reflecting on its very nature and location within the processes of knowledge generation in the social sciences. What

is the relationship between the problem-orientated research and what goes under the label of “action research”? Action research is part and parcel of – if not outright synonymous with – the kind of research which produces problem-orientated knowledge, with one important qualification. It may be a way explicitly and deliberately to interconnect and “bridge” processes of generation of enlightening knowledge with processes of production of problem-orientated knowledge. This may constitute an especially valuable and laudable contribution, as the two types of knowledge need each other, but the dynamics of their production (including the career pressures and paths of those involved) may actively diverge and make the pursuit of this bridging function both high in demand and short in supply.

3 Practice-embodied knowledge: Learning from experience and replicating

At the opposite pole of the enlightening knowledge discussed above, there is what we can label “embodied” knowledge: the kind of knowledge that is incorporated into the “practices that work,” the practices that are being adopted by public administrators and managers to tackle extant problems. These practices are very often referred to as “best practices,” and there is a consultancy industry that thrives on the sale of these so-called best practices (that generally do not deserve to be qualified as “best,” apart from highly specified, very rare instances, as we shall see). The alleged trait of being “best” aside, the key issue here is that practices that work do represent a form of knowledge: *a kind of knowledge that is incorporated and lies in a gizmo, a “system that works”*, rather than manifesting itself in verbal-conceptual forms – as a set of propositions or hypotheses or of an argument or a narrative – as typically occurs for both theoretical (enlightening) knowledge and problem-orientated knowledge.

How is it possible to learn from vicarious experience for replication elsewhere? How is it possible to learn from the often tacit knowledge that has been incorporated into *this* practice that works “here and now” and use this knowledge to contrive elsewhere a practice that, appropriately adapted to the diverse circumstances, is also able to work and produce similarly positive effects? How can this embodied knowledge – knowledge incorporated into a practice – be leveraged for purposes of bettering the functioning of PA? With a slightly more technical formulation, the question is whether and how analysts in public management can address the problem of improving the performance of public sector organisations in one situation (target site) by

employing experience acquired elsewhere (source site). We attempt to take stock of what we know about the use of practice-orientated knowledge (Ferlie & Ongaro, 2015, chapter 8; key scholars who have worked on the question of how to learn from practices include: Eugene Bardach, Michael Barzelay, Stuart Bretschneider and Colin Campbell).

We argue that *the research of and into practices that work, which is known under the label of “best practices research,” can be deconstructed into two problems:*

- *The search for excellence problem*, which tackles the issue of what is “best” and how to detect it – and if, as is the case, very rarely we can talk about a truly “best” practice, to drop the notion of best and talk about “good” or “adequate” practices – or, put simply, “practices that work under certain circumstances”; and
- *The extrapolation problem*: how to “extract” what makes the practice produce positive effects under certain circumstances to replicate those effects elsewhere.

3.1 The search of excellence problem

The first problem concerns how to find “where excellence lies”: how to identify cases that contain practices of excellence, i.e. cases in which the highest possible level of performance is manifested. Bretschneider et al. (2005) have developed a thorough assessment of this problem, and they come out with very stringent conditions: the two joint necessary and sufficient conditions for finding a “best” practice (or better: for finding a case that contains a best practice to be extrapolated) are the completeness of cases considered and the comparability of cases. When pondered appropriately, it arises that meeting these joint conditions is very rare at most (indeed nearly impossible) and, moreover and crucially, these conditions can be satisfied almost exclusively when the performance takes the form of a production function. In fact, Bretschneider et al. “work out a general framework for selecting the proper technical option available to researchers for estimating relationships of inputs to outputs capable of focusing on extreme behaviours, in order to find the best performing case, i.e. for finding the unit where the transformation of inputs into outputs – the production function – is performed at the best level) (...) The focus on the production function is also, in our view, the main limitation in the approach suggested by Bretschneider and colleagues [this entails that] other categories of situations, like the management of complex change processes in the public sector, where performance cannot generally be expressed in the terms of a production function [are not encompassed]” (Ferlie & Ongaro 2015, pp. 183–184).

Identifying excellence – a truly “best” practice, a practice that deserves to be qualified as “best” – is an absolute rarity, up to the point that talking of “best” practices should be deemed utterly misleading in most cases. Indeed when consultants tout the solutions they propose as “best” practices they are possibly bordering, if not outright trespassing, on the notion of cheating – and this misleading emphasis on best practices, in the public as in the private sector, has engendered a wide range of nefarious effects: the “best practices industry” should definitely adopt much more restraint if it is to provide a contribution for good – and as a minimum should change its name to “the good practices/adequate practices” industry.

The key question remains to be addressed: after having found a practice that produces positive (“good,” “adequate”) results, how does one replicate it elsewhere? To answer this question we need to shift our focus from “universalistic” practices to “contextualised” practices, and we do so by specifying the conditions under which the practice works – and more generally recognising that practices are good *under certain contextual conditions*, and nearly no practice works irrespective of conditioning – enabling or hindering – circumstances (on the meanings of “context” and “contextualised” in public management and policy, and a range of theories and perspectives to study context in public management, see the edited work by Christopher Pollitt, 2013). Doing this means addressing the extrapolation problem.

3.2 The extrapolation problem

How can “a practice that works” be extracted from one case (source site) and applied elsewhere (target site) to generate similarly successful outcomes? This is the core question of the extrapolation problem, which can also be formulated as follows: how to extract the identified good practice and adapt it to other circumstances to replicate its effects.

It should be noted that “knowledge” here becomes generated in a certain sense only when the practice is analysed, causality is understood, and some form of verbal illustration of how the practice works is developed. This occurs because implicit knowledge becomes explicit/codified/verbal knowledge – but more than that, the point is that in a certain sense knowledge is generated when the “embodied practice” is extrapolated, so extrapolation continues and completes the process of knowledge generation that was initiated when the practice was initially contrived. This may reassure the reader who, albeit having patiently followed me all the way here, is still (somewhere in the back of her/his mind) concerned that this “practice-embodied knowledge”

represented something magic or exoteric: quite the contrary, this knowledge can be likened to engineering knowledge, a problem-solving knowledge which is incorporated into a machine (in our case not exactly a “machine,” but a socio-technical practice, because PA problems can never be solved entirely at a technical level and always have a human relations, sociological component). However, unlike engineering, this knowledge is first found in a gimmick, the practice that works, and only later is it codified in more expressible terms, into a project or design that enables the practice to be replicated elsewhere. If it can be likened to engineering, this form of knowledge should more appropriately be likened to *reverse engineering*. (As said, the other main difference from engineering lies in the fact that practices are socio-technical in nature rather than only technical. To be even more nuanced, also unlike engineering there is in PA practices also an artistic component, which might not be present in an engineering machine – although the author of this chapter, himself an engineer by training before turning to the social sciences and PA in later university degrees, is mindful of the preface to a handbook of construction science where a prominent scholar and engineer, himself the designer of countless civil engineering infrastructures scattered across the globe, warned about the artistic component to any engineering artefact: it is only intuition that can bridge the hiatus between the design of a machine and the real machine; there is always an artistic component to engineering.)

How can the process of extrapolation of a practice for replication elsewhere unfold? The extrapolation protocol worked out in Ferlie and Ongaro can be summarised in a very schematic way in the following steps (for more detail see Ferlie & Ongaro, 2015:186–197):

- *Identify the function to be performed*: the first step lies in defining what is the nature of the performance (in both the meanings of the English word performance: doing something, and achieving a level or standard in doing it) that is being sought: for example in defining whether the practice is one that facilitates the management of change (the function to be performed by the practice is then a change function), or whether it is about the delivery of a certain output (the function to be performed by the practice is then a production function), and so forth: what “performance” is required for the effects to be produced;
- *Define and analyse the practice*: usually this step should be split into two: first, the understanding of how the system operates (what is the nature of the socio-technical system on which the practice intervenes), and then, second, how the practice takes advantage of the way the system operates to produce the given positive effects that are ultimately being sought. Too

often, the practice is thought “directly” to produce the positive effects: instead, more generally the practice is a way of interacting on a system (which possesses its inner dynamics) in such a way as to beget certain outcomes that eventually steer the system towards producing the desired results;

- Consider all the effects of the practice, including side effects or negative effects that may come with the practice. A bit like in medicine, it is very rare that a practice produces only positive effects without side or outright negative effects, to be stemmed or contained when replicating the practice in the target site;
- Define the *context factors*: the conditions under which the practice produces its effects – to forecast the impact of the transferred practice under the varied circumstances where it is being replicated.

The gist of this approach is that it relies on a form of knowledge that is different from both the enlightening knowledge and the problem-orientated knowledge (though closer to the latter). It is an embodied knowledge, incorporated into the practice whose process of knowledge generation is more akin to that of reverse engineering.

There is a final and very important qualification: we have so far considered “practices that work,” meaning that we have assumed that the effects produced by the practice were ultimately (overall and with the important qualifications outlined above) “positive,” i.e. capable of creating, rather than depleting, “public value.” It is worth noting that there is a need also to study “practices that do not work,” meaning that their effects may be conducive to worsened, rather than improved, situations. These practices too could and should be considered, with a view to “learning from failure” in ways that are not distant from the proposed protocol, although obviously engineered in the opposite way, to try to avoid negative effects to be replicated elsewhere and spread out. This final reflection leads us to reckon a topic which is not discussed directly in this chapter, and this is the question of “who learns,” next to the questions of “what is learnt, and how”...

We can now turn to wrapping up on the conjoint usage of the three forms of knowledge we have been discussing. We do so by interrogating what role the leading learned society in Europe can play in each of these three forms of knowledge, for the advancement of the practice of PA.

4 Forms of knowledge and the contribution of EGPA

We may conclude these reflections on the forms of knowledge for the practice of PA for this *EPPA I Book* by considering what could and ought to be the role of EGPA, as the leading learned society in knowledge generation about PA in Europe, in the production of knowledge in the three forms identified and defined in this chapter.

Looking at the EGPA portfolio of activities (Bouckaert and van de Donk, 2010; Ongaro, 2019), it stands out how the distinctive feature of the organisational model of EGPA lies in the centrality of the Permanent Study Groups, the platforms for the development of research across the sub-fields of PA into which EGPA is organised. They represent major “engines” of the production of research in the field of PA in Europe. Apart from that, already back in the 1980s and 1990s and then with reinforced impetus since the early 2000s, EGPA has engaged in the development of strategic partnerships with institutions and organisations for the practice of PA in Europe, like EUPAN, the European Public Administration Network which gathers the Departments for Public Administration of most EU Member States and Associate Countries across Europe. In 2014, EGPA has also launched a series of “Policy Papers on European Governance” explicitly aimed at drawing the implications of research generated within the EGPA study groups for the practice of PA. In the mid and second half of the 2010s, the International Institute of Administrative Sciences has further developed its orientation to produce counsel and advice for policy-makers on key global policy issues, and EGPA as the European regional group of IIAS – with its strong research base – has also developed its thrust to engaging into policy advice for the institutions of the EU and the countries of Europe.

Taking all these developments together and interpreting them in the terms of the forms of knowledge discussed in this chapter, we can read (but also critically query and reckon the appropriateness of) the EGPA portfolio of activities as follows:

- The EGPA Permanent Study Groups as aimed at producing *enlightening knowledge*;
- The EGPA Policy Papers on European Governance and the EGPA partnerships as aimed at producing *problem-orientated* and *practice-embodied knowledge*;
- EGPA and IIAS, when engaged in advising policy-makers, as aimed at delivering *problem-orientated knowledge*.

The same questions that have been discussed throughout this chapter can be applied to EGPA and its contribution to the three forms of knowledge: what is the “impact” of the theoretical knowledge which EGPA contributes to generating? How can demand-driven/problem-driven knowledge be organised at and for the European level, given the features of European governance? How can EGPA contribute to a critical and constructive development of “good/adequate practices” for the development of PA?

Wrapping up, in this chapter I argue that knowledge for the practice of PA comes in three main forms: enlightening knowledge (theory-centred); problem-orientated knowledge; and practice-embodied knowledge. All three forms of knowledge are required for the development of the practice of PA. This interrogates the extent to which the European PA scholarly community –organised in the national academic systems, in the EU research frameworks, and in European learned societies like EGPA – is apt and adept in its governance forms, in its career paths for scholars, in its professional-epistemic culture to bolster all three forms of knowledge and to integrate them in such a way as to make a difference to the practice of PA.

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