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Gaming and Performance Metrics in Higher Education: The Consequences of Journal Lists

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Gaming and Performance Metrics in Higher Education: The Consequences of Journal Lists

Abstract

This article analyses 'gaming' in higher education as a response to performance metrics. It shows how scholars react to performance measurement systems in the form of journal lists that define appropriate outlets for their research. Two types of gaming are identified: the adaptation of research methods and topics to align with the perceived ideals of top US journals, and strategic networking with editors and potential reviewers before submitting articles to prestigious journals. We employ scripting theory to analyse performance measurement and its consequences, differentiating between three levels of scripting: cultural scripts, represented by journal lists; interactional scripts, characterised by participants' redefinitions/enactments of these overarching scripts; and intrapsychic scripts, focusing on participants' self-scripting and personal attitudes towards gaming in academia. The study is based on qualitative interviews with 173 associate and full professors in the social sciences in Denmark.

Keywords: academia, gaming, higher education, journal lists, performance metrics, scripting theory, strategic networking.

Introduction

Academia has become increasingly competitive and performance-oriented over the past two decades (Espeland and Sauder, 2016; Fleming, 2021). Researchers have described a 'performatisation' of scholarly work, which involves demonstrating the capacity to publish in 'top' journals (Gendron, 2008; Humphrey and Gendron, 2015). At many universities, this culture has become institutionalised through the introduction of journal lists specifying the outlets that faculty

should choose for their articles. These lists assign varying levels of prestige to academic journals, primarily based on their impact factor. In Denmark, publishing in listed journals has become an important criterion when candidates are assessed for associate and full professorships during hiring and promotion. In this sense, journal metrics and faculty's successes in publishing with prestigious (primarily US) outlets have a notable impact on academic careers in Denmark and in many other European countries (Helgesson and Sjögren, 2019; Nygaard et al., 2022).

This development is part of a neoliberalisation of academia (Fleming, 2021). Neoliberal ideals prescribe that universities should be managed as market enterprises, with measurable goals being defined and performance being assessed and reported to stakeholders (e.g. Benner and Holmqvist, 2023; Gendron, 2008). A heightened 'control by numbers' has reinforced competition among individual scholars, departments and universities striving to live up to accountability demands (Lynch, 2015). Neoliberal universities are often associated with commodification of research where knowledge is produced not only for its own sake but also for the achievement of quantifiable goals (Gendron, 2008). In this sense, neoliberalism can be seen as a significant force behind performance measurement systems, such as journal lists, which increasingly regulate the conduct of researchers (Mingers and Willmott, 2013). Scholars argue that neoliberal management has generated a new academic ethos (Kallio et al., 2016) that displaces traditional values like Merton's (1968) imperatives of science. Studies also show that academics largely comply with the new managerial demands although sometimes resisting them at an ideological level (Kalfa et al., 2018). Rintamäki and Alvesson (2023: 258) coined the term 'resipliance' to describe the paradoxical behaviour of academics who express resistance towards their university's performance targets in principle, yet feel compelled to comply with them in their daily work.

Some studies focus on the behavioural consequences of performance measurement at universities (Adler and Harzing, 2009; Aguinis et al., 2020; Brooks and Schopohl, 2018), including activities such as ‘gaming’ (Biagioli et al., 2019; Gendron, 2008; Graf et al., 2019; Tourish and Craig, 2020). Gaming refers to strategic behaviours adopted by scholars to achieve performance targets, sometimes involving practices that may violate ethical norms and values. However, gaming is not a uniform concept, and various studies define it in different ways. For instance, Aboubichr and Conway (2023: 605) describe gaming as ‘deliberate, opportunistic and strategic behaviour’ aimed at achieving performance targets while breaching ethical values and codes of conduct. Graf et al. (2019: 754) view gaming as ‘deviant workplace behaviour’ that violates significant organisational norms. They argue that gaming can compromise the functioning of departments and universities as it may lead scholars to ignore other important aspects of their work, such as teaching or service tasks, which are not so easily measured (Järvinen and Mik-Meyer, 2024). Graf et al. (2019) identify three conditions that may invite gaming: performance goals are set (too) high; achievements are measured by outcome alone and not by the processes leading to the outcome; and performance measurement does not represent all work tasks, which, in turn, leads to a certain ‘tunnel vision’ (Graf et al., 2019: 756).

Some scholars distinguish gaming from cheating, defining gaming as ‘bending the rules’ and cheating as ‘breaking the rules’ (Pollitt, 2013). Others differentiate between fraud and gaming, with fraud involving plagiarism and fabrication or falsification of data and gaming referring to questionable research practices, such as salami slicing, data recycling, changing hypotheses after the fact and attributing false authorship (Butler et al., 2017). Furthermore, some scholars separate serious misconduct (equivalent to fraud) from gaming, describing the latter as ethically liminal practices that may raise concerns but are surrounded by disagreement regarding their acceptability

(Biagioli et al., 2019). While extensive research has been conducted on fraud and cheating in academia, few studies have focused on the grey areas where scholars 'bend the rules' or engage in ethically liminal publication behaviours to meet the performance targets set by their managers (Aboubichr and Conway, 2023; Butler et al., 2017; Graf et al., 2019).

Our study addresses this gap in the literature, asking the questions: What types of gaming can be identified among Danish academics responding to performance measurement in the form of journal lists? How are these responses related to participants' *beliefs* about the need for certain compliant behaviours, such as research adaptation and strategic networking in order to publish in designated journals? What is the relation between gaming and the academic ethos expressed by participants?

We focus on gaming for two reasons. First, we think it is crucial to highlight the unintended consequences of journal lists. These consequences have, to date, received little attention in sociological research – most existing studies have been conducted in the field of economics, especially in accounting research. Second, the ethically liminal status of gaming is intriguing in and of itself. On the one hand, gaming may have immediate positive outcomes for faculty members and management as it can help them achieve the goal of publishing in listed journals, which is a prerequisite for competitiveness at both individual and department/university level. Hence, it is not necessarily in the interests of management to prevent gaming (Aboubichr and Conway, 2023). On the other hand, gaming may be troublesome if it contradicts the classical academic values of research freedom and the pursuit of the highest possible quality goals. Publishing in a limited number of primarily US journals is not necessarily a guarantee for a sound development of the social sciences in European countries.

According to Espeland and Sauder (2007: 2), gaming is a form of 'playing to the test', meaning that academics focus on indicators rather than on the qualities that different types of performance measures are intended to assess. The type of gaming we analyse may create inequality among scholars, as some research areas, according to our interviewees, are more suitable for publication in the listed journals than other. Gaming feeds into hiring and promotion processes where academics are rewarded not only based on their research merits (and their teaching record, academic service work etc.) but also, and increasingly so, based on their capacity and willingness to network strategically. A central focus of this article is to analyse the degree to which participants find gaming questionable: Do they regard gaming as unethical behaviour or do they see it as a natural and unproblematic aspect of academia's 'new ethos' (Kallio et al., 2016)?

Previous Research on Journal Networks and Research Adaptation

In the following sections, we focus on two topics in extant research that are pertinent to this article. First, we review studies examining social communities formed around leading academic journals and how they may impact the chances of getting articles published. Second, we discuss studies investigating the implications of performance measurement in academia, including research on how scholars may adjust their research topics and methods to better match the preferences of top US journals.

Social communities surrounding academic publishing have been investigated extensively (e.g. Baccini et al., 2020; Goyanes and De-Marcos, 2020; Santos and Mendonça, 2022; Tight, 2014). This body of research sheds light on the concentration of power within a limited circle of individuals who simultaneously hold positions as editors, editorial board members and authors in the same or related journals. This phenomenon is known as 'interlocking editorship' (Santos and

Mendonça, 2022), 'interlocking authorship' (Baccini et al., 2020) and 'editorial board interlocking' (Goyanes and De-Marcos, 2020). The key point here is that the researchers' roles as authors, editorial board members and editors overlap within small groups of journals. A study by Colussi (2018) examining publication patterns in top US journals in economics reveals that nearly half of the published articles were authored by scholars who had pre-existing affiliations with journal editors at the time of publication, for instance as (former) PhD students or colleagues. Scholars disagree as to whether this is problematic. Brogaard et al. (2014), for instance, show that articles written by editor-connected scholars receive more citations than other articles, concluding that these articles are generally published because of their high quality and not as a result of personal favours. Heckman and Moktan (2020) state that research on the mechanisms behind network effects is inconclusive. Network-based publishing may be tied to favouritism/nepotism, as well as to quality differences, for example, caused by inside information – editors being able to identify the best authors, and authors using their knowledge of journal standards and procedures to improve the quality of their work. Regardless of the background of editor-connected publishing, Heckman and Moktan (2020: 422) conclude that it is problematic because it penalises authors without connections to editors.

Importantly, studies show that publishing in renowned US journals substantially influences the stratification of higher education in many countries (Heckman and Moktan, 2020; Helgesson and Sjögren, 2019; Hussain, 2011; Kwiek, 2019; Nygaard et al., 2022; Orupabo and Mangset, 2022). For instance, it has been demonstrated that scholars from non-English-speaking countries in Europe publish increasingly in US- or UK-based outlets and less in their own language and that international publications are a prerequisite for their hiring/promotion (Orupabo and Mangset, 2022; Pelger and Grottge, 2015; Vanderstraeten, 2020). A major issue in the literature is that journal

rankings may affect research diversity and innovation (Gendron, 2008; Heckman and Moktan, 2020; Gendron and Rodrigue, 2021; Meyer et al., 2018). Related to this, scholars have investigated a possible 'identity shift' in academia, worrying that faculty substitute their traditional ethos of intellectual progress and scientific curiosity with a short-term focus on merely publishing in the 'right' outlets (Argento and Van Helden, 2022; Ramassa et al., 2023).

The degree of research adaptation to specific journals varies across disciplines and research fields. In a recent survey conducted among UK business academics from 22 research fields, Brooks et al. (2023) found that scholars in economics and finance were more inclined than others to customise their research for publication. By contrast, scholars in organisational studies were sceptical of this practice and expressed concerns about future resource allocation to subfields that do not align with the dominant understanding of what constitutes relevant research. In other studies as well, scholars call for more receptive attitudes towards research topics defined as 'peripheral' and a commitment to conducting research in a broader variety of areas (Argento and Van Helden, 2022; Gendron, 2008; Gendron and Rodrigue, 2021).

Taken together, the reviewed studies highlight the unintended consequences of journal lists, especially the prevailing emphasis on publishing in top US journals. This singular pursuit fosters an 'obsession' among scholars with a limited number of journals and a problematic relationship between publication success and belonging to journal communities (Heckman and Moktan, 2020: 419). Most studies in this field are quantitative, demonstrating ties between authors and editors and examining scholars' attitudes towards journal rankings and other metrics. A small number of studies investigate the mechanisms behind these connections and the behaviours of faculty responding to

the demands specified by journal lists (Argento and Van Helden, 2022; Brooks et al., 2023; Butler et al., 2017; Graf et al., 2019; Ramassa et al., 2023).

Our study contributes to this emerging research strand by focusing on two hitherto understudied forms of gaming explored through a qualitative lens: adapting scholarly practices to alleged journal preferences and building strategic relationships with editors and reviewers. These practices do not correspond to fraud or serious misconduct but should rather be regarded as ethically liminal behaviours (Biagioli et al., 2019) or, to use an expression of Macdonald and Kam (2007: 641) as enactments of 'gamesmanship', understood as instrumental behaviour aimed at winning games without actually cheating.

Analytical Frame

We use scripting theory to structure our analysis. Simon and Gagnon (1986) employ the concept 'script' as a metaphor for 'the production of behaviour in social life' or 'the operating syntax under which social life happens' (Simon and Gagnon, 1986: 98). They differentiate between three levels of scripting: cultural, interpersonal and intrapsychic. Cultural scripts exist at the institutional and societal levels; they are the overarching scenarios defining the frames for practices associated with specific roles. Interpersonal scripts refer to 'the application of cultural scenarios by participants acting in particular social contexts' (Simon and Gagnon, 1986: 97). Intrapsychic scripts concern participants' reflections about the other levels of scripting and their 'bargaining' with themselves about the role they enact (e.g. 'What kind of I am I? What kind of I do I want to be?') (Simon and Gagnon, 1986: 100).

We employ a slightly modified version of scripting theory to analyse academics' responses to journal lists. Although Simon and Gagnon (1986:99) mention that interpersonal and intrapsychic scripting should be seen as applications of cultural scripts as well as 'new scriptwriting', their focus is on interpersonal scripting as enactments of cultural scripts, and on intrapsychic scripting as personal inclinations/wishes. By contrast, we accentuate that scripting, or the creation of 'operating syntaxes' (Simon and Gagnon, 1986: 98) occurs at all three levels: the cultural, where the overarching goals for participants' behaviour are set; the interpersonal/interactional, where participants enact *and* redefine these scripts (sometimes in ways that conflict with the initial cultural scripts); and the intrapsychic, where participants engage in self-scripting.

In our study, cultural scripts are conceptualised as the instructional guidelines operating at departmental level, where journal lists inform faculty about how they should behave if they are to meet the demands of ideal scholarship. The central dimension of these scripts is that faculty should publish in prestigious journals but the scripts are also characterised by a general tribute to international networking. Interactional scripts are analysed as the enactments and redefinitions of cultural scripts by participants striving to improve their chances of getting published in listed journals. We identify two main forms of enactments/redefinitions: participants adapting their research topics and methods to the alleged preferences of top US journals, and participants building relationships with editors and potential reviewers before submitting their papers. In order to stress that this level of scripting concerns behaviours by agents interacting with other agents as well as the relationship between cultural scripts and agency, we use the term 'interactional scripts' rather than Simon and Gagnon's (1986, 2003) 'interpersonal scripts'. Finally, we deploy the concept of intrapsychic scripts when investigating participants' reflections on the two other levels of scripting, including their own self-scripting. In combination, our analysis of the three levels of scripting

among faculty shows that scripting at the interactional level generates *beliefs* about how scripts at the cultural level should be enacted most efficiently; that these beliefs (whatever their relationship is to the facts of publishing) take the form of paradigmatic truths for a considerable number of faculty; and that these 'truths' have severe *consequences* for participants' behaviour and for their self-scripting.

That beliefs have consequences for behaviour and, in the process, create 'truths' about the social world is a well-established fact within sociology, famously summarised in the Thomas theorem (Thomas and Thomas, 1928). People respond not only to the objective attributes of a situation when choosing how to act but also to the subjective meaning that this situation carries for them. When acts are performed in accordance with this subjective meaning, they become an integral part of the situation, gradually influencing its actual features (Thomas, 1923; Thomas and Thomas, 1928). Goffman (1956) later showed how people collaborate in 'defining the situation' under which they act. This does not mean that they are in complete agreement but rather that they form a working consensus regarding the conditions they find themselves in and tend to adapt their behaviour accordingly. Scripting theory is anchored in symbolic interactionism (Blumer, 1969; Goffman, 1956) but offers an analytical structure that distinguishes between *levels* of meaning-making. Cultural scripts do not determine interactional and intrapsychic scripts; they are interpreted and acted upon within specific social contexts and under individual motivational structures. Simon and Gagnon (1986, 2003) make it clear that scripting theory is preoccupied with the interactional and individual levels as much as with cultural prescriptions. In our view, this is the strength of the theory: it facilitates an analysis of similarities and differences between levels of scripting and of the ambivalence that may characterise the relationship between cultural scripts, collective enactments/redefinitions of them and individual stances on these enactments/definitions.

Data and Methods

The study is based on 173 qualitative interviews with social science professors and associate professors at three universities in Denmark. The universities are publicly funded (as are all universities in Denmark) and have an international orientation with a notable number of faculty members from other countries. From associate professorship level onward, all positions are tenured.

The sample comprises 96 associate professors (55 men, 41 women) and 77 full professors (46 men, 31 women) working in the wider fields of economics (61), political science (67) and sociology (45). The average age of the associate professors was 42 years at the time of the interviews, whereas the full professors had an average age of 50.3. The response rate was exceptionally high: 82% among invited associate professors and 87% among full professors. Most interviews lasted between 1 and 1.5 h, with an average duration of 70 min. All interviews were fully recorded and transcribed.

All 173 interviews were conducted by the two authors of this article who are full professors in sociology. Hence, the study is based on 'peer interviews' or 'insider interviews' referring to research in which the interviewers and interviewees belong to the same group and share experiences related to the subject matter under discussion (Bryman and Cassell, 2006; Gunasekara, 2007). The interview questions concerned a wide range of topics related to academic careers (mentorship, networks, promotion criteria, conferences, balancing research, teaching, administration etc.) not analysed here. From our first reading of the interviews, we noticed the participants' preoccupation with their department's journal lists and strategies aimed at improving their chances of getting published in listed journals. Initially, NVivo coding was used to identify all sections of the interviews concerning journal lists and publication strategies. This dataset was then analysed, first

by applying initial codes and later developing these codes into analytic themes to examine the behaviours associated with journal lists.

The two themes in focus of this article – research adaptation and strategic networking – emerged from the dataset and were not chosen beforehand. They were the most frequently mentioned strategies when participants described what they regarded as common, or even necessary responses to the journal lists. When beginning the interviews, we were only partly aware of these behaviours and were surprised by the interviewees' consequent focus on them (see Table 1 in Appendix). When discussing publication practices in the interviews, we made an effort to stay neutral, neither endorsing nor criticising research adaptation and strategic networking. We did not use the word 'gaming' in the interviews, although some interviewees used it, especially those who were strategic networkers themselves. It was only after we had analysed the interviews that we decided to consult the literature on gaming, which led us to define research adaptation and strategic networking as 'ethically liminal practices' (Biagioli et al., 2019).

A notable pattern in the interviews was that participants talked about their 'beliefs' concerning publication successes, often stating that they 'thought', they 'had heard' or they 'had been told' that listed journals only accept some types of articles and that it is important to be acquainted with the editors and reviewers if one wishes to publish there. Approximately 80% of the participants subscribed to these beliefs, while two-thirds said that they (to varying degrees) acted in accordance with them.

The project follows the American Sociological Association's Code of Ethics (ASA, 2018) and the Danish Code of Conduct for Research Integrity (Ministry of Higher Education and Science, 2021)

for informed consent, participant anonymity and secure handling and storage of data. Participants were guaranteed full confidentiality and all personal information, anecdotes or events that could be traced back to specific individuals or departments have been withheld to maintain anonymity. All participant names mentioned in the analysis are pseudonyms.

Cultural Scripting

'Cultural scripts' are, in our adaptation of Simon and Gagnon's (1986) concept, organisational scripts stipulating that faculty should publish in prestigious journals if they are to fulfil their department's requirements. Most of the participating departments have explicit journal lists defining (predominantly) US-based 'top journals', whereas two departments have somewhat broader publication criteria, including some European generalist journals and field journals. However, also in these departments, publications in top US journals tend to be valued more highly than those in prestigious European journals.

In all the departments included, publishing in top journals is a central criterion when assessing faculty's qualifications for hiring and promotion, although some variations exist among disciplines. According to the interviewees, top publications are a more or less mandatory criterion for hiring/promotion in economics and in some political science departments. In sociology departments and some interdisciplinary departments in which the faculty comprises sociologists, political scientists and others, the link between top publications and career progress is less straightforward. In these departments too, publications in top journals are the central qualification criterion, which, however, can be combined with other criteria (e.g. monographs with prestigious international publishers). Furthermore, quantitative researchers in the included departments are more oriented

towards publishing in US journals than qualitative researchers, a difference that has also been found in other studies (Erola et al., 2015; Schwemmer and Wieczorek, 2020).

Interactional Scripting

Research Adaptation

The first interactional script identified in our data concerns the adaptation of research topics and methods to the supposed preferences of prestigious journals. The main contents of this script are the notions that listed US journals favour research based on quantitative over qualitative methods, empirical over theoretical contributions and monodisciplinary over interdisciplinary approaches. Research based on experiments (survey experiments and others), social stratification research and social science studies that include psychometrics are said to be particularly popular in these journals. Furthermore, several participants said that 'European and Scandinavian topics', such as EU research, comparative welfare studies and public administration research, are difficult to publish in top journals. These are *beliefs* shared by many of the participants and not necessarily facts about the journals' actual preferences. However, they are beliefs that are consequential to many interviewees' behaviours.

One example is 'Kurt', a professor, describing how he tailors all his research projects strategically to align with the interests of 'esteemed US professors'. He talks about his diligent efforts to comprehend 'how Americans think' in order to contribute with 'a key point' that they will find relevant. Despite living in Denmark and conducting publicly financed research based on Danish data, his main scholarly motivation revolves around a research agenda that captures the interest of US readers. The interview with Kurt contains no reflections on the potentially negative aspects of this practice, such as reduced diversity or the limited relevance of his research in local contexts. He

regards research adaptation as necessary for scholars coming from ‘an unknown place in Europe’ and worries about his ability to ‘keep the pace in the long run’ and continue to find ‘microscale points Americans find worthy of quoting’ (Kurt).

One of the female professors following the script of research adaptation is Julie, who, however, also says that she is moving away from this practice. She describes how she initially shifted from conducting qualitative research ‘with theoretical ambitions’ to quantitative research in order to get her work published, continuing: ‘However, moving forward, I hope to pursue research questions that are exciting and important, with the secondary consideration being how they can be published’. Julie regards research adaptation as both positive and negative. It is positive because ‘it keeps you on your toes; you get better at explaining what exactly your contribution is’. However, it is also negative because it may lead to ‘research paucity and homogeneity’. Scholars with a research focus that does not match that of the top journals ‘have to adapt, rewrite completely, choose another approach and use other references’ (Julie).

One of the interviewees describing their difficulties when it comes to research adaptation is David, a professor. He explains:

> My work is focused on European issues and of limited interest to US journals. In general, publishing context-sensitive research is difficult. Some research is more generic. For instance, psychological research on children’s behaviour is less dependent on context and is, therefore, more easily accepted. I am always met with the question, ‘Can this be generalised to a US context?’ (David).

As these quotes show, Danish scholars face challenges and dilemmas when trying to balance their research approach with what they believe are the demands of top US journals. Some (like Kurt above) adjust their research completely to 'the ways Americans think', others (like Julie) alternate between research adaptation and studying topics they find 'interesting and important' while others again (like David) find it impossible to comply with the alleged preferences of the most prestigious journals. As a fourth interviewee with a more 'European profile' concludes: 'I would have to start all over; all the topics I am a specialist in are of no interest whatsoever for these US journals. I never even think about submitting my things to them' (Arthur, associate professor).

Becoming Part of Journal Communities

The second interactional script identified in our data concerns the interviewees' attempts to become part of the communities surrounding prestigious journals. Editors and associate editors are particularly important targets for networking because relationships with them are thought to 'smooth' the publication process. According to the participants, connections with editors lower the risk of desk rejection, improve the choice of reviewers and make comments accompanying the reviewer reports more helpful. One way to make oneself visible among editors and potential reviewers is to participate in 'as many international conferences and workshops as possible', as one of the female associate professor's states, continuing: 'You present your paper over and over again. For one thing, you want to be known among reviewers. For another, you want your future editor to be in the room' (Carla). Several participants indicate that papers *have* to be known in central networks before submission: 'I have heard that an experienced US editor once said, "If I receive a paper that I haven't seen presented at a conference, I know there is something wrong with it and I conclude it's not a good paper"' (Alfred, professor). Another way of improving one's publication chances is to actively seek out potential reviewers and ask them to comment on your paper before

submission: ‘You compliment them, tell them how inspired you are by their work, you quote them abundantly and hope they will be part of the review process. I cannot prove that it works but it’s worth trying’ (Glenn, associate professor).

A third way, and a more collective one, involves inviting journal editors or co-editors to local department seminars: ‘If you look at our list of guest speakers last semester, you can see that an astonishing part of them [laughs] hold positions in journals we are interested in’ (Esther, associate professor). A fourth method involves actually suggesting handling editors and reviewers for one’s submission: ‘At some journals, they ask you to suggest reviewers and nobody protests if you suggest a handling editor too, one who knows you and appreciates the type of work you do’ (Teresa, professor). A final and much-used strategy is to team up with US co-authors to become part of the ‘in-group’. One of the male associate professors says:

> It’s called piggybacking. You get one of the big names onboard and it’s much easier to get your paper published […] There may be many reasons for this, quality improvement and all of that but it’s also a question of having contacts. Don’t ask me for evidence of this [laughs]. It’s just something everybody knows (Oscar).

Like the last quote, many accounts describing the importance of being part of journal networks contain expressions such as ‘I cannot prove it’, ‘I have no evidence’ or ‘I cannot document it’, or as one of the male professors says: ‘These things are never straightforward. You never know, for sure. If you knew, the system would break down’ (Isaac). Hence, as in the case with research adaptation above, the second interactional script operates at the level of *beliefs*, beliefs, however, that are pivotal in shaping participants’ behaviour. Furthermore, formulations such as ‘this is how most

people think in my field' (Ivan, professor) and 'it's just something everybody knows […] We all think it's for our own good' (Oscar, professor) indicate that these scripts do not operate at the individual level alone but are shared and discussed in groups of people in the included departments. Other quotes confirm this: 'There was this seminar about publishing and everybody seemed to know this: you cannot submit a paper to a journal if the editor doesn't know you' (Ann, associate professor); 'You need to do the footwork before you submit, my PhD supervisor always said. Be sure you are known in the circles of potential reviewers' (Ruben, associate professor). These excerpts demonstrate that participants are socialised into strategic networking and are taught that this is for their 'own good'. The risk of not accommodating is that one becomes marginalised from 'the right circles' and potentially excluded from today's 'publication race' (Noah, professor).

The behaviours described in this section are adopted by scholars to achieve performance targets. Contrary to the cultural scripts about the importance of publishing in listed journals, interactional scripts about adapting your research and getting to know editors and potential reviewers are not authorised by university boards or departmental policy documents. They are, however, on the way to becoming institutionalised at the interactional level, proof of which is that they are discussed openly as 'tricks of the trade' among the participants (see Table 1 in the Appendix showing how widespread these conceptions are among the interviewees).

Intrapsychic Scripting

According to Goffman (1956), a definition of the situation also includes performers' attempts to project a certain image of themselves. Hence, in social interaction, such as in an interview context, performers are not only concerned with the topics discussed but also with impression management. In Goffman's (1956: 162) words, 'as performers, we are merchants of morality', striving to control

the impression others form of us. This morality concerns the performers' self but also their responsibility for the 'team' they are part of. When defining a situation, performers should not denounce their team because team members hold a certain loyalty towards each other and discrediting the team also weakens the image of the self that the performer is attempting to project. As Simon and Gagnon (1986: 100) state when describing the challenges of intrapsychic scripting: 'Questioning the sincerity of others ultimately becomes the condition for the questioning of the self by the self'. Intrapsychic scripting concerns the relationship between participants' self-conceptions/presentations and the other two levels of scripting, with reference to questions such as, 'what kind of I am I? What kind of I do I want to be?' (Simon and Gagnon, 1986: 100) and we may add: 'What kind of team am I a member of?'

Although most participants believe in the interactional scripts of research adaptation and strategic networking and about two-thirds say that they follow these scripts to varying degrees, there is considerable ambivalence regarding their personal identification with them. Samuel, an associate professor, is an active practitioner of research adaptation and strategic networking. Throughout the interview, he alternates between defending and criticising these behaviours. On the one hand, he says 'There are some, you could call them old gnomes, at the department who wail over the fact that our discipline is now defined by top US journals. The answer to them is that our focus on these journals guarantees research excellence'. On the other hand, he states, 'I am not completely fine with these journal lists […] because they bend our research questions away from things that may have more relevance for *our* society' (Samuel). Another example of ambivalent interviewees is Evelyn, a professor describing how she has benefitted from her international network and not least from her co-authorship with a well-connected US scholar. She relates a review process that was 'smoothened' because her co-author 'knew the editor extremely well', adding: 'I am not sure about

this of course but it seemed like some kind of friend service'. She goes on by saying that she discussed the situation with colleagues who 'agreed that this was unusual but not unethical' and that 'it was difficult for her to raise objections about something that was to her own advantage' (Evelyn). In the interview, however, Evelyn calls the process 'outrageous', also criticising the journal lists for leading to 'all kinds of strange behaviours'.

As shown in these examples, some participants perform a difficult balancing act: they legitimise participation in gaming while simultaneously acknowledging that it may be problematic. As Goffman (1956: 162) points out, impression management is a question of presenting a self that lives up to 'the standards by which the self is judged by others', including the standards the performer expects their audience to represent. In our interviews, participants did not know our opinions on research adaptation and strategic networking (as mentioned in the methods section, we tried to keep a neutral stance on this). Hence, they may have thought that we share their commitment to strategic networking and value their achievements in this regard, or they may have suspected the opposite and feared our judgement of their behaviour. Furthermore, many interviewees were well acquainted with extant debates on the intensified goals of performance measurement in academia, and sometimes with critical research on the matter. This awareness of ongoing debates and insecurity about our stance may have contributed to the ambivalent and sometimes contradictory statements in the interviews.

Ambivalence also characterises the accounts of interviewees who do not practise research adaptation or strategic networking. Some participants express criticism of the journal lists, typically along the lines already mentioned: journal lists are said to disfavour qualitative, interdisciplinary and theoretical research; they are criticised for leading to standardisation and homogenisation of

disciplines and for neglecting research topics relevant to Danish (but not US) society. As one of the male professors puts it: 'We work on Danish taxpayers' money but are in the grotesque situation that the less we mention Denmark in our publications, the more successful is our research' (Tobias). Although these participants problematise the journal lists, they do not criticise their colleagues for adapting their research and/or engaging in strategic networking. One of the female associate professors says:

> I have no problems at all with colleagues betting the whole store on top publications. They are fortunate to pursue research topics that match these top journals' profiles, or else they have made them match. That's fine with me, although I haven't been able to change my research profile (Lillian).

A male associate professor says:

> I feel like this is something I have missed out on a bit, networking with US scholars, for instance. I am impressed and slightly envious [laughs] watching how my colleagues handle our performance demands. I have never been good at strategic behaviour (James).

In these interviews and several others, gaming is not condemned but rather presented as a natural consequence of performance measurement. As two other interviewees say about strategic networking, 'It's a necessity to be instrumental, I don't say it is good but it is wise' (Charlie, associate professor); 'It's natural, it's naïve to think that you can do this in other ways' (Frank, professor).

Accounts like these may be signs of collegial loyalty and an unwillingness to 'denounce one's team' (Goffman, 1956: 135). When individuals appear before others, they intentionally or unintentionally project a 'moral truth' about the situation, a truth of which an acceptable conception of themselves and their team is an important part (Goffman, 1956: 155). The overall 'truth' emanating from our interviews is that scholars cannot be blamed for gaming because management's decision to reward publications in listed journals compels them to adopt strategic behaviours. The fact that faculty are positioned unequally when it comes to research adaptation and networking is acknowledged but regarded as an individual rather than a collective problem – 'I haven't been able to change my research profile', 'I have never been good at strategic behaviour', as Lillian and James said above, tying their publication challenges to personal shortcomings.

Discussion

This article used scripting theory (Simon and Gagnon, 1986) to structure a three-level analysis of gaming in academia. While the first level defines performance goals (publishing in listed journals), the second level creates beliefs about how these goals should be reached (research adaptation and strategic networking) – beliefs that are related to participants' self-scripting in varying ways. Some participants do practise research adaptation and strategic networking and see no problems with that or the journal lists. Others do not engage in these behaviours themselves (for various reasons) but find their colleagues' gaming and the journal lists to be legitimate. Others again criticise the journal lists but not the practices with which these lists are associated.

In the analysis, we focused on beliefs and their consequences. Our findings do not prove that successful publishing *is* dependent on research adaptation or relationships between authors,

reviewers and editors. We have not analysed the actual behaviour of editors receiving manuscripts from known versus unknown authors, nor have we investigated the numerical article profiles (qualitative or quantitative, theoretical or empirical, disciplinary or interdisciplinary) of the listed journals. What is clear, however, is that most of the interviewees believe in the necessity of research adaptation and strategic networking and that many act in accordance with these beliefs.

As described in the introduction, previous research defines gaming as 'workplace deviance' violating organisational norms (Graf et al., 2019: 754) or as 'opportunistic' behaviour breaching rules and codes of conduct (Aboubichr et al., 2023: 605). The behaviours we focused on in this article are typically not conceptualised as deviant, opportunistic or rule violations. Furthermore, there are no 'codes of conduct' condemning networking with reviewers and editors or adapting one's research topics and methods to supposed journal preferences. Hence, although these behaviours are not authorised by the involved departments, none of the participants practising them describe sanctions or critiques from management or colleagues.

Many interviews were permeated with ambivalence. While the scripting processes at interactional level define research adaptation and strategic networking as 'natural' and relatively innocent 'tricks of the trade', the relationship between gaming and participants' self-scripting is more complicated. At this level, several interviewees hesitate when it comes to identifying themselves with (the behaviours we call) gaming, wavering between describing their own research adaptation and strategic networking as problematic and unproblematic. A similar ambivalence is seen among non-practitioners of these behaviours who sometimes criticise the journal lists and their consequences *in principle,* yet avoid condemning their colleagues who engage in research adaptation and strategic networking. Following previous research (Kalfa et al., 2018; Rintamäki and Alvesson, 2023), this

shows that the new ‘academic ethos’ of performance measurement may be ideologically resisted yet accepted as inevitable in practice.

Besides ambivalence, there are clear signs of resignation in the interview accounts. The participants work at universities governed by the market logic of neoliberalism where performance metrics facilitate the manageability of their research and where alignment with their department’s definitions of ‘publication successes’ is a requirement for promotion. Seen from this perspective, it may be wiser for individual researchers to comply with the demands represented by journal lists than resist them. Furthermore, managerial reasoning at all the departments involved reflects the idea that US scholarship should also be the precedent for social scientists in Denmark. Extant studies describe the expansion of US research and publication standards as ‘epistemic domination’ or ‘epistemic imperialism’ (Boulianne, 2019; Gonzales et al., 2024). Epistemic domination/imperialism refers to situations where some knowledge claims are given superior status because they are seen as representing universal truths while others are dismissed and regarded as reflecting specific interests and contexts (here: national). Epistemic domination is built on rules, seemingly neutral but in fact contingent, for how research should be framed, conducted and presented in different fields (Gonzales et al., 2024). This mechanism is at play when the interviewees talk about finding the ‘microscale points Americans find worthy of quoting’ (Kurt) or state ‘the less we mention Denmark, the more successful is our research’ (Tobias). Such accounts are presented matter-of-factly, and often with resignation: opposing US definitions of how social research should be conducted and what constitutes relevant research is something few interviewees engage in.

Graf et al.'s (2019) three conditions for the development of gaming were mentioned in the introduction: performance goals are set (too) high, attainment is measured by outcome alone and performance criteria concern some work tasks and not others. The journal lists in the involved departments meet all these criteria. First, publishing in top US journals is an ambitious goal, as all listed journals have immensely high rejection rates. Many participants, especially those without US co-authors, describe the long and strenuous processes of paper presentations and repeated rewriting before their papers are even considered for publication in these journals. Second, the cultural scripts of high-status publishing centre on results rather than processes. Hence, when our participants are evaluated, for instance, in hiring and promotion processes, the dominant focus is on their publication 'hits' and not on the behaviours – be they legitimate or illegitimate – lying behind them. Finally, the 'tunnel vision' described by Graf et al. (2019) is evident in our data: journal lists create a narrow concern with publishing in accredited outlets and disinterest in other journals and other publication forms (such as monographs and anthologies or publications in Danish).

Successful research requires continuous performance at a high level in an extremely competitive environment. Journal lists insert this competition into a formula based on US publication standards. Many of the participants in our study describe research as a means of achieving publication and not primarily as a means to advance knowledge (cf. Becker and Lukka, 2023). They design their studies in a way that they believe enhances their chances of publication success and not necessarily following what they regard as the most intriguing research questions. Hence, being a well-performing scholar today is not necessarily equal to being a scholar in search of 'the truth' or doing research for the 'common good', for example by studying topics relevant to Denmark. In this sense, journal lists have severe consequences for the development of the social sciences. They incite

scholars to conform to the specific interests of a small group of journals instead of seeking the best possible understanding of issues of importance for science and/or society at large.

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Appendix: Examples of quotes from all participants (thematically ordered)

First interactional script: research adaptation
P1: I think these journals influence which research topics you choose and possibly also the way you conduct your research.
P9: It's all about adapting your research. If, for example, you conduct survey experiments, you stand a good chance with those journals.
P10: It's unfair in many ways because it depends on your research area. There seem to be fields that US journals are more interested in than others.
P11: In my opinion, my research has exactly the same quality as my colleagues' who easily get their things published in top journals. They just study the 'right' topics.
P13: This also reflects epistemological differences and the fact that certain quantitative methods seem to be valued highly at the expense of other methods, both quantitative and qualitative.
P14: There are essentially two career tracks which are also linked to the journals. There's a fast track for the quantitative researchers and a slower track for everyone else.
P18: I've started doing survey experiments which they obviously like in the US, so it might be easier to get my work accepted.
P28: Experimental research is fashionable. It's the key to getting published in the major journals. You simply replicate the research model that is popular in the US right now.
P31: My research interests concern topics that don't receive much attention. I plan to continue with this although I haven't been able to convince editors and reviewers that it is important.
P32: I'm considering doing something more empirical. Apparently, you can't publish theory in the top journals. If you do quantitative research, I believe it's easier to get published.
P36: There has been a move towards quantification in the social sciences. If you look at the top journals, I guess the bulk of what's in there is quantitative work.
P38: It seems difficult for theorists to publish in top journals. I mean, I have the highest regard for people doing empirical work, but I think it's easier for them to get their work published.
P40: Management assumes there is something like a pyramid of journals in every research field, and that US journals are at the top. In reality, they confuse quality with location, which in the long run is a dangerous thing to do.
P42: Personally, I don't see the focus of the journals as a problem but I think those who want to study specific Danish issues face some challenges.
P43: You know, quite a few of these US journals have a specific focus which means that some researchers are favoured while others are left out in the cold.
P44: The American journals favour particular types of researchers and it's not the qualitative ones.
P45: I think there's too much focus on 'outstanding publications' in a few selected journals. That means ignoring the differences in publishing opportunities depending on the research area you work in.
P46: It's obvious that one's self-esteem can take a hit. I'm a theorist, right, and I don't master all these quantitative methods that have become the golden standard for getting a paper accepted in US journals.
P47: My feeling is that when you publish in top journals, you have to compromise and phrase things in a certain way. If you want to follow or not, that's a dance between the idealist and the strategist part of you.
P48: My research is interdisciplinary and I don't think that's an advantage if you want to get into the listed journals.
P49: I do more and more quantitative research with a group of colleagues. You often hear people say that qualitative research doesn't have a chance in US top journals.
P50: It's the experimental set-ups that are popular in top journals now. Another thing that is popular is the use of psychometrics in various contexts.
P51: It's important for me to do what I find exciting, rather than adapting my research to fit the journals and yet... I've worked quite a bit with qualitative and theoretical issues but right now, I'm leaning more towards the quantitative side.

P52: I cannot fight this development. Maybe I should be more pragmatic and just try to publish my research in the outlets defined by others, even if I don't feel these journals are the right place for my work.
P53: I write about social theory, so I haven't really aimed for these journals. I mean, if you work in the interpretive and social-theoretical tradition, they are not really an option.
P55: You can have all sorts of thoughts about journal lists but I take them as a given, something we can't change but it is a problem that the Anglo-American tradition is so dominant in defining what constitutes relevant research.
P59: These journals prefer behavioural research. International studies seem to be difficult to publish. If you do comparative research, you have to compare the US to a European country if you want access.
P65: Survey experiments are popular in these journals, so that's what we do [laughs].
P66: My research is within public administration and I don't think there is interest in that in the 'top journals' defined by our department.
P71: My profile is very interdisciplinary, so I haven't published in these high-ranking journals. I don't think they are interested in my research.
P73: One could say that publicly funded research in Denmark should benefit Denmark but publishing in US journals doesn't necessarily contribute to that.
P74: My type of research doesn't stand a chance in those US journals. I'm in competition with my colleagues for the same professorships and some of them can get their work published there, which leaves me at the mercy [of the journal lists].
P75: It's really difficult because my research field is public administration, and those top journals, well, I don't think they publish anything in that area.
P80: The journal lists promote specific methods, specific paradigms, specific research questions and not others. In that sense, academic freedom suffers.
P81: It seems that research in social stratification has good prospects in US journals. Theoretical work and interdisciplinary research have challenges.
P84: The journal lists create very specific research profiles and if your research doesn't match the profiles, you'll never become a professor.
P88: I do a lot of research on Europe, so it's not obvious for me to send my research to American journals.
P90: It's different for us who do qualitative research. I don't think we have a chance in these journals.
P95: Pure theory is becoming more and more difficult to publish. There is still a market but it's getting tighter and tighter. I think most top journals today want some empirical contributions.
P96: Things are moving in the direction of more standardised research. The problem with the listed journals is that it's only certain kinds of research that can be published there.
P97: For one thing, these journals are not interested in interdisciplinary work and on the methods side, they are typically not open for qualitative research. If I was dealing with experimental methods or that kind of stuff, I think they would be interested.
P98: My research doesn't align well with the US journals. It's because I do welfare research. It's not that it's never published in the US but it's not an obvious fit. You probably need to do research on individual behaviour to get published there.
P99: I've become more quantitative, more experimental in recent years in order to try something new and of course, it's also because it's easier to publish quantitative papers than qualitative ones.
P100: You can adapt to the journals and it's often said that it's easier to get your papers accepted if you, for example, do research on behaviour or other things at individual level, and that's probably okay.
P116: My research fits well in top journals or I've adjusted it to fit. You have to. It's the standard everyone follows.
P117: I do a lot of interdisciplinary work, so it has been a bit difficult for me to break through.
P124: I hear all the stories about people changing their research topics because of the journal lists. It's completely understandable but I think the lists create a lot of pathologies.
P126: I'm in a field that is relatively European-dominated, where context matters a lot. It's not obvious for me to publish in US journals.
P131: There are some fields, gender studies, for instance. There are no top journals focused on gender.

P132: These journals are not interested in the EU. If you are to write about the EU, you need to make it a case of something, as if it was a special kind of federal system. Then US editors and reviewers might find it an acceptable topic.
P148: I never start a project if I don't think it has a chance in a top-five journal, but there's also a significant overlap between what I find interesting and what can be published in the best journals.
P149: In the end, it's about quality and it's crucial to publish in the best international journals but they have no interest in Danish topics.
P151: I've thought, how can I develop as a young researcher and I've tried to think a lot about finding an angle that Americans find interesting.
P157: There's this Americanisation of research going on now, where there are certain ways of doing things that I'm becoming more and more sceptical about.
P162: There are several top journals where I don't think it's possible to get in with theoretical contributions.
P164: Then there's a hierarchical order in how easy it is to get high metrics. Behavioural studies and various experiments are the areas where it's easiest, and then there's theory which I suppose is most difficult to publish.
P166: Some of the work I've done has been interdisciplinary. It can be challenging to get that kind of work into top journals.
P167: My research career started with me doing qualitative work, very theoretical. Later, my research has become quantitative and causally oriented which probably made it easier to publish in US journals.
P171: They say that you can't publish theory there but I would dare to say that if you have a solid theory and empirical evidence that is convincing, you can publish in the best journals.
P172: It's always easiest with individual-level data in US top journals. Some of the things I do require a good understanding of welfare systems in different countries. I have colleagues whose research is much better suited to the journals.
Second interactional script: Building networks with editors and reviewers
Editors
P3: Perhaps it was easier back in the days to get in as someone who wasn't part of the circle. Nowadays, there's a significant investment in networking if you want to publish in the best journals.
P4: You often hear that you need to have the right contacts if you want to publish in the American top journals
P7: I don't think my co-author has a personal network with the editors but they do know who he is and they are therefore a bit less inclined to desk reject, I suppose.
P12: We invite international guests to our seminars and it's often people from good journals, editors and associate editors. I guess it helps us build useful networks.
P17: Quality matters a lot, for sure but I also believe it's easier to publish if your co-author is someone whom the editors know.
P19: If you come into one network, you easily come in contact with other networks and you know, knowing people who are editors is extremely valuable.
P20: We make sure to invite the right people to our department seminars, some are editors [laughs]. We hear a bit about their research but more importantly, they hear about ours.
P21: My network is not sitting on influential positions at journals or elsewhere so I guess the impact isn't that great. I think it would have been more of an advantage if I had established US editor contacts.
P26: My impression is that the top journals get so many submissions that the editors just flip through them and if they see someone they know, the paper probably goes into review.
P27: I hope and expect that the scientific quality of my work is in order but it's obvious that if you know the editor, there's a bit more goodwill in getting an article through.
P30: I knew her a bit and thought, I wonder if you can suggest her as a handling editor and actually, you could. Suddenly, I felt like I was getting better treatment, to be honest.
P34: I have sent most of my papers to journals where I didn't know the editors but there are a couple of exceptions where I think it helped that the editor knew me but I don't know for sure.

P41: You have to be able to play a bigger game than just doing quality work and I probably haven't been good at pushing myself onto key editors and co-authors.
P54: Networks open many doors and in terms of publishing, it's obviously good to know people. It might even be *necessary* to know people, especially editors and reviewers.
P56: I suppose there is a bias in the system in the sense that acceptance of papers is dependent on networks.
P62: Editors are not your friends. I guess it's more like 'Oh, I had this conversation with that person and what she was doing sounded interesting, let's take a closer look at it'.
P63: We also make sure to invite editors and associate editors to our department seminars. It's a good way to get to know each other.
P67: It's not that the editors are your buddies and that they will accept your paper if they know you but I believe they will take the paper under more serious consideration.
P68: If editors just get a paper from someone they might not even know, I think they will either reject it or send it to referees who are a bad match.
P82: The editor was X, who I had actually visited in the US and I just thought, wow. Suddenly I could see the quality of the reviews we received.
P83: You have to make sure the journals know you. If you're just working silently and not going out shouting how good you are, you might never get recognised.
P85: We submitted the article to an editor whom my co-author knew very well. Then two reviews came in, one major and one minor and we only used the latter, and the article was accepted. I cannot be sure it was a network effect but it seemed that way.
P87: We have invited key editors to various events. It's something I've only started to understand now, that knowing the right people at the journals matters.
P89: The editors I know personally, they really read my things closely. That's different from other editors who just pass on the reviewers' comments without remarks and don't make their own decisions.
P94: My co-author had a meeting with the editor. They knew each other and I've also met him at conferences, and my impression is that it means quite a bit.
P101: I know this editor at a top journal where I've published. I think he probably looks more positively at my papers than if I were an unknown author.
P102: It means something if the editor knows you, I guess. It might sound a bit shady but it's not personal favours in that way, it's rather that they view the article through a more positive lens.
P103: I know all the editors in my field but I don't think it makes a difference when I submit. I get just as many rejections as I did before I knew them [laughs].
P104: I have often known and still know the editors of several journals and they know of me but I have never thought that made a difference. I never had a sense of that.
P106: I believe that if you know the editors, you're more likely to get into review, so it's really about desk reject versus review. If the editor knows who you are, that may be a tipping point.
P107: I suppose editors look at the referee reports differently depending on what they think of the author. It's just regular network effects.
P108: They say, 'our peer review system is so objective' and all that but that's just bullshit. It's clear that if the editors know me and my research, they also know who will review my paper on its own terms.
P109: It may be that editors still reject it but at least they don't do it automatically. I think the chances are very, very small if they don't see that the paper has been presented at a number of conferences.
P110: I think there is sort of an urban myth around this that just because editors do not know an author's name, they will desk reject them.
P111: I certainly believe it helps. My colleague said to me, 'submit it to that journal, and I'll say that you're a good colleague'. He was on the journal's editorial board and everything went well, the article was accepted after a couple of revisions.
P112: I often hear my colleagues talk about how important it is to know the editors but I haven't personally experienced it. Maybe it's just me.
P113: I can proactively approach the editor and say, 'I have this idea, do you think it would fit?' I wouldn't dare to do it if I didn't know the editors.

P114: Knowing editors does matter because rejection rates in our field are around 90 percent. I think it's common knowledge that you need to know the editors.
P115: It's about the power of the editorial offices. Ideally, you should know someone from the good American journals. Otherwise, it's all about writing with big names.
P119: I know the editor of that journal, so I had a paper where I was like, 'okay, I need to submit it while he's still the editor there', because I knew he was positive.
P120: It's probably not a disadvantage to know the editors. I think it matters in terms of whether your paper gets looked at in the first place but it's just a hypothesis.
P121: It matters if you know the editor but it's hard to say how much. Maybe it has an impact on whether they send it for review or not. At least, that's what I hear from others.
P122: It's difficult to tell but I think it means something if the editor has seen the paper being presented. I guess you can call that a network effect.
P123: It's not something I've necessarily experienced often but yes, I believe it helps if you've seen the editors somewhere and maybe chatted with them at a conference.
P125: Of course, you'd like to know editors. There's a lot of power in decisions about desk rejections and the selection of reviewers and in general, the power over where the field is heading.
P127: I think it eases the process if the editor knows you, and it's not necessarily because people want it that way but I think it's hard not to let yourself be influenced a bit by who you know.
P128: It probably means something if the editor knows you but it's difficult to say if it's because you have made a good scholarly contribution or if they think you are good to share a bottle of red wine with.
P130: I imagine people get a slightly different treatment if the editor knows them, not necessarily personally but in terms of knowing that they are capable researchers. They get the benefit of the doubt.
P133: Well, I don't know how much it matters and I don't know the editors personally. I know them professionally, in a scholarly sense, so it's hard to say but I guess it doesn't hurt.
P140: I don't know, I would say I've made efforts to know the editors' preferences rather than knowing them as individuals. I try hard to figure out what the editors like.
P141: If you have supporting editors, they can push things through unless the reviewers are absolutely negative, as opposed to 'please take into account all the reviewers' comments'. In the latter case, you get no editorial help whatsoever.
P142: I think that was probably easier for me five to ten years ago because now the associate editors are younger than I am. They don't necessarily know me and they pick reviewers from their own networks.
P143: A lot of these editors are associate editors at other key journals. To be an associate editor or on the board of three key journals is not unusual at all. It's like a lobby.
P144: I don't think it matters if you know the editor or not. Or does it? It doesn't hurt, but I don't think these are friend services. Maybe I am naive.
P150: I assume that it means something if editors know you. That they've heard of a paper, know the author's name and maybe the PhD supervisor. I think the process is smoother then, but I don't have any hard evidence.
P152: I remember once when I met the editor and he grinned and said, 'your article has gone through two revisions; if you buy me a beer, we'll accept it now', but that was just a joke.
P153: You cannot know for sure if it's a question of networks with reviewers and editors but either way, it's a bit of a scam in the sense that it undermines the idea of blind peer review.
P155: What people typically do if they know the editor is to write a private email to him and ask, 'would you be interested in something of this type?' Then he'd reply, 'yes, just send it and we'll take a look'.
P159: Well, we all expect that knowing editors is a significant issue in certain contexts.
P160: I knew the old group of editors and I submitted several times and was never desk rejected. Now it's a different group of editors that I don't know and the last time I submitted, I was desk rejected but I can't prove that was the reason.
P161: I don't think it makes a big difference if they know you or not. Maybe it can have a small impact on whether you get desk rejected or not but it's hard to say.
P163: I imagine there is a positive effect of knowing editors. You know, the difference between a minor or another R & R and a new reviewer coming in out of nowhere.

P165: Yes. I know most editors in my field but I'm not strategically choosing editors. I don't do that. I write the paper I want to write and send it where I want to send it.
P170: It's my feeling that if it's an editor who has a positive attitude towards my research, then I get better reviewers but I think it's more about them knowing that what I'm doing is solid, rather than knowing me personally.
Reviewers
P2: For example, when my most recent paper was published, I had talked to two out of three reviewers within a year. One of them even told me he was a reviewer while he was reviewing it. It's maybe not entirely ethical [laughs].
P5: It's also something to do with certification of papers through conference presentations, getting reviewers to see your paper before it is submitted. It's also a selling argument in itself that the paper has been presented, say, 30 times.
P6: Many of my colleagues send their papers to key persons to get comments. They probably do it hoping that the reviewers will be recruited from that group.
P8: One thing I haven't previously put much effort into is networking with reviewers at conferences, so I'm an exception in the department in that regard.
P15: It's almost a requirement that you travel around and present your paper to important people before you submit. I don't think you can expect good reviews of an unknown paper.
P16: I know about colleagues sending their papers to prestigious professors in order to get their comments before they submit.
P22: It's about being perceived as a member of the community. It's about making people, including reviewers, aware of my work before it is submitted.
P23: It helps a lot that my co-authors are Americans and have the opportunity to present our paper in the right places and get the right people to see it before it's sent for review.
P24: It may be a good idea to contact people by email and ask for comments on your paper. You can always write that you find their research exciting and that we have parallel research interests.
P25: In some fields, it's easy to guess who will be invited to review your paper and then it's a good idea to have their comments before you submit. If they are critical, they should have told you so in the first place.
P29: There is a huge difference if you have US connections. My US co-author was able to present our paper at good universities before submission. Some of the reviewers must have met him. I think that paved the way for the paper.
P33: If you're well known in an international network, you probably have an easier time getting published, I think, because referees can probably figure out who submitted the paper and if they know you, it helps.
P35: You have to present your paper to the right people. Otherwise, it is extremely difficult to convince reviewers that the paper is good.
P37: I think I presented the paper 20-30 times. You have to send the paper to people and have them look at it. The may become reviewers of the paper.
P39: The paper was presented, I don't know, at dozens of conferences. A lot of potential referees saw it and I think this probably gave them a more positive view.
P57: Reaching out to reviewers is important but do you want to put in the effort? Do you want to go with 'those who drink together, think together'? Do you want to do all that to get into publishing networks?
P58: I wouldn't reach out to people [reviewers] just to buttress my publication chances. Some people do that and I don't blame them. We are all immensely pressed.
P60: I have never been shy to network up. If that's what it takes to get my things published, I will certainly do it.
P61: We presented it maybe 20-30 times. Initially, it's about getting feedback but then you also try to sell it to potential referees.
P64: So my US co-author travelled around and presented the paper in the right places. He invited himself to tour Ivy Leagues... Everyone had seen the paper because it was submitted.
P69: Pandering to certain scholarly groups so that your work gets out as citable, quotable and relevant. I think many people do that strategically well.

P70: We send our papers to people who are bigger names because I think it looks good to thank them, even though they might have said nothing, and some of them may become reviewers.
P72: I remember at the beginning of my career, I didn't know anyone and it was really a terrible experience to attend conferences. That was before I learned that conferences are about making connections.
P76: Everyone says that networking is super important for those top journals and it probably plays a role, but I think, or at least I hope that reviewers also look at the quality of the article and the study.
P77: They say that you need to enter the right networks, including reviewer networks. I could have been more strategic but it's not my nature. I'm an incredibly shy person.
P78: I just finish my articles and then I submit them to a journal I think fits. I know I should do a lot of groundwork with potential reviewers but I've never done that.
P79: I know that you need contacts with high-profile American professors to get into the journals and I have no problem with my colleagues cultivating their network. I've never done it myself.
P86: In the beginning of my career, I was not aware of why you go to conferences. I just knew that people sent their abstracts there and presented papers. I was completely oblivious to the fact that you need to build relationships with people who may get involved in the review process.
P91: You present your paper over and over and then you hope the editor says, 'she's actually known in that field' and then you hope they pick the reviewers from those who have already commented on the paper.
P92: I present my papers many times in order to get feedback. My field is very big so you won't benefit from knowing the reviewers. It's impossible to know who is going to review.
P93: We've presented our paper 30 times or something like that. You present it to a lot of potential referees. That means you get many of the comments you'd otherwise get in the review process.
P105: Using conferences to network is not just about submitting an abstract and trying to get on a panel. You have to build a name for yourself and establish collaborations that may help you in the review process.
P118: You need to enter the right networks. [This is] all the hard work I keep telling my PhD students they have to do if they are to become successful.
P129: Together, we've probably presented that paper 30 times and you thank all those who provided comments. I think it increases the likelihood of getting it accepted.
P134: I'm not very good at networking with potential reviewers. When it comes to conference stuff and networking and small talk, that's definitely one of the areas I can improve, absolutely.
P135: At some point, you no longer present to get feedback. If you want to get into the top journals, you have to market your paper among reviewers.
P136: It's hard to say how much these conferences matter for your publications. Maybe there are some reviewers who see you present and find it exciting. I don't know. Ideally, it shouldn't matter.
P137: You meet some of the potential referees before they get the paper, so you've kind of paved the way for the paper in the right circles. I think it helps.
P138: The presentation of your paper often doesn't yield much. It's more like a promotional showcase of your research. It's important to travel around and market it.
P139: It's probably effective to use networking for publishing purposes. Some play that game a lot and I don't condemn them in any way, but I'm not good at it.
P145: You need to be known in the pool of reviewers, my supervisor always told me. You have to contact them at conferences, have a cup of coffee with them, send your papers to them. It all helps.
P146: We gladly send our paper around to reviewers, or potential reviewers and ask if they have comments. If they don't have any and then become reviewers, it's a bit harder for them to tear the paper apart.
P147: Much of what we know about human psychology would suggest that the process might be smoother if you have a positive relationship with the reviewers. If I have the opportunity, I always opt for a non-double-blinded top journal.
P154: I sent my last paper out to potential reviewers in advance to get their comments. I understand from colleagues who do this a lot that this is the way to do it, using it strategically in that way.

P156: One tries to contact likely reviewers, quite clearly. I think many people do that. Then you can shield your paper from their criticism in advance. It's a kind of pre-review.
P158: I suppose I may have benefited at times from some reviewers knowing who I was. It's not something I know for sure, but I could imagine, 'that might be him who wrote this and that, and he's sympathetic'.
P168: Then there's a less noble aspect, and that is that it's not necessarily just the quality of an article that is decisive but having a large network that may have heard about it and therefore review it with somewhat more positive eyes.
P169: It's a matter of networking which might close in on itself. Those who network have an advantage over those who don't. No doubt about that.
P173: You need to have a critical mass of reviewers you know. We're all like that, I suppose – if we know someone, we're more inclined to look at what they write with kind eyes.