Citation models and research evaluation

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Citations in science are being studied from several perspectives. On the one hand, there are approaches such as scientometrics and the science of science, which take a more quantitative perspective. In this chapter I briefly review some of the literature on citations, citation distributions and models of citations. These citations feature prominently in another part of the literature which is dealing with research evaluation and the role of metrics and indicators in that process. Here I briefly review part of the discussion in research evaluation. This also touches on the subject of how citations relate to peer review. Finally, I try to integrate the two literatures with the aim of clarifying what I believe the two can learn from each other. The fundamental problem in research evaluation is that research quality is unobservable. This has consequences for conclusions that we can draw from quantitative studies of citations and citation models. The term “indicators” is a relevant concept in this context, which I try to clarify. Causality is important for properly understanding indicators, especially when indicators are used in practice; when we act on indicators, we enter causal territory. Even when an indicator might have been valid, through its very use, the consequences of its use may invalidate it. By combining citation models with proper causal reasoning and acknowledging the fundamental problem about unobservable research quality, we may hope to make progress.

Keywords: science of science; research evaluation; peer review; metrics

The study of science itself has a venerable history, and is studied from several points of view. The field of scientometrics studies science from a quantitative perspective. Relatedly, the field of what has come to be called science of science is similarly taking a quantitative perspective, but often with a somewhat different approach. The two have much in common and share a more quantitative formal perspective on studying science, especially based on large-scale data sets of publications, its authors and their citations, i.e. bibliometric data sources. Scientometrics has been traditionally more focused on “measuring science”, while much of science of science is more focused on “modelling science”. This distinction is not absolute though: some publications in what most would consider scientometrics build models, while publications in science of science sometimes also address issues of measuring. I will review part of this literature, with a focus on citations.

There is one important aspect that is often left implicit, but often used as a motivation for studying large-scale bibliometric data sources. Studies are often motivated by the fact that citations are somehow thought to be relevant for how science operates: they may reflect advances in science and clarify intellectual contributions. Moreover, citations and related aspects seem to play a role in scientists’ own career, a development that seems to have become increasingly stronger over the years. The use of metrics in research evaluation is increasingly criticised. The role of metrics in research evaluation and the effects of metrics is less often discussed explicitly by the scientometric and science of science literature. In this chapter I aim to connect these two literatures, with a focus on citations.

First I discuss various observations of citation distributions, how they change over time, and how they can be modelled. This literature is largely based on a mix of scientometrics and science of science. Then, I review a small part of the literature on research evaluation. This includes some aspects relevant to national research evaluations. This also touches upon issues of comparing peer review and metrics, which I will also briefly discuss. After having reviewed this literature, I will ask what we can learn from both literatures, and how they can mutually strengthen each other. I deconstruct some aspects of citation dynamics and clarify that there are other factors that play a role in citation dynamics, the implications of which are, although sometimes acknowledged, not often appreciated in this literature. Additionally, I will clarify what I believe an indicator to represent, how indicators can be biased, and how they can be made less biased or more accurate. A causal understanding is key to understanding indicators, I believe.

The fundamental problem in research evaluation is that scientific quality is unobservable. Any study on the subject therefore must acknowledge this, and this has consequences for the type of conclusions that we can draw, especially from quantitative studies. Being more clear about our causal reasoning and being careful about what we can and cannot conclude helps to clarify that. Having a better understanding of the overall dynamics in citations, and relying on models that capture these
dynamics, we can improve what we can infer from observations.

There are a great number of books and reviews that cover quantitative science studies. An older overview of scientometrics is provided by Hood and Wilson (2001), providing also a history of the origins and various terms related to this field, such as bibliometrics and informetrics. A useful overview of informetrics is provided by Bar-Ilan (2008). De Bellis (2009) provides a comprehensive overview of the field, and also includes some of the more theoretical frameworks that underpin some of the research. Sugimoto and Larivière (2018) cover the essentials of measuring research, and makes for a great introductory read. The science of science approach was briefly reviewed by Fortunato et al. (2018), and more recently, was covered in a more accessible form by Wang and Barabási (2021). Some of the literature was also reviewed by Zeng et al. (2017) who took a complex network and complex systems approach. A related, but different perspective was offered by Evans and Foster (2011). An overview of some of the literature concerning research evaluation was written by de Rijcke et al. (2016).

I. CITATIONS

A. Citation distributions

One of the earliest and most commonly studied aspect in scientometrics and science of science is the distribution of the number of citations. Various authors have tried to find theoretical distributions that could fit the empirically observed distribution well. Part of the literature has tried to come up with theoretical models that could explain the observed type of distributions, and I will cover some such studies later. One important consideration here is that there does not exist such a thing as the distribution of citations. It is always a distribution of citations of a particular set of papers, and the results will vary according to what set of papers is studied. Some authors for example consider citation distributions of papers from various years. Other authors consider citation distributions of papers across various fields. Others study citation distributions limited to more specific sets of papers, from certain journals, departments or even individuals. Based on what set of papers is studied, different citation distributions are found. Some distributions may then be understood as a mixture of other distributions. For example, a citation distribution of papers of several years may be understood as a mixture of the citation distributions for each year. This raises the question what distribution is a “core” distribution and cannot be thought of as a mixture of other distributions. Ultimately, this might be a distribution of citations for a single paper, even though a single paper empirically never shows a citation distribution, only just a single realised number of citations. Nonetheless, the theoretical idea that a single paper can also show a citation distribution, instead of a single number has some relevance. It suggests that there could be a certain inherent uncertainty in citations themselves, in the sense that the same paper could have also been cited a different number of times.

One of the earliest studies of citations was covered by Price (1965). He studied the number of citations to all papers covered in one of the earliest edition of the Science Citation Index, the precursor of what is currently known as the Web of Science. Price (1965) finds that citations are distributed approximately as a power law:

$$\Pr(C \geq c) \propto c^{-\alpha+1},$$ (1)

with $\alpha$ estimated to be somewhere between 2.5–3.0. This points to a highly skewed citation distribution. Indeed, he finds that “only 1 percent of the cited papers are cited as many as six of more times each in a year”.

Physicists became increasingly interested in citations and citation networks in the late 1990s. Redner (1998) is an early study of citations in that literature and reports a power law distribution with exponent about 3, similar to Price (1965). He studies a few different citation distributions: a distribution from a single year (1981) coming from a precursor of the Web of Science, and a few different volumes of Physical Review D. These different datasets show quite a different number of average citations and older years generally have accumulated more citations, simply as the result of having had more time to accumulate citations. He finds that various datasets collapse onto a universal curve when dividing the number of citations by the average number of citations in that dataset.

Laherrère and Sornette (1998) study a slightly different citation distribution, namely the citation distribution of all citations to authors, instead of to individual papers. They find that a stretched exponential is the best fit for their distribution:

$$\Pr(C \geq c) = \exp -\left(\frac{c}{c_0}\right)^\alpha$$ (2)

However, they also find that a power law is a reasonable fit, with an exponent of about 3 again.

Radicchi, Fortunato, and Castellano (2008) study citation distributions of a few different fields and a few different years. They study these distributions separately and try to address the question whether the distributions are universal, in the sense that after some transformation, all the distributions look alike. They find that a simple scaling of citations with the average number of citations in the same field and the same year collapses all the distributions onto a single curve, hence finding evidence for universal scaling. That is, they define the normalised citations $\tilde{C}_i = \frac{C_i}{E(C_i)}$ where $C_i$ is the total number of citations received for publication $i$ and $E(C_i)$ is the average number of citations received over all publications from the same field and the same publication year. They find that the normalised citations $\tilde{C}_i$ are well fitted by a log-normal distribution $\text{LogNormal}(-\frac{\sigma^2}{2}, \sigma^2)$, with $\sigma^2 \approx 1.3.$
which by definition has an average of 1. In this study they used data from Web of Science and relied on the field definitions given by journal subject categories. They later repeat a similar study, but then use data from the American Physics Society (APS) and use PACS codes to define fields (Radicchi and Castellano, 2011), again finding a similar collapse of distributions onto a universal curve. Chatterjee, Ghosh, and Chakraborti (2016) perform a similar study of the universality of citation distributions, but then of institutions and journals. They also find evidence for universality and find that the normalised citations are well fitted by a lognormal distribution. For academic institutions they find that $\sigma^2 \approx 1.7$, somewhat more skewed than the paper level citation distribution identified by Radicchi, Fortunato, and Castellano (2008), while for journals the collapsed citation distributions is slightly less broad with $\sigma^2 \approx 1.4$. For both institutions and journals, the lognormal is less able to fit the tail of the distributions, which seems to be better approximated by a power law. Possibly, this could be a result of differences in sizes, which plays a role for institutions and journals, but not for individual paper distributions.

The universality claim by Radicchi, Fortunato, and Castellano (2008) was revisited by Waltman, van Eck, and van Raan (2011), who argued that citation distributions are not truly universal, and that differences can still be observed between some fields. They study this based on comparing the top 10% of all publications based on the normalised citations to the top 10% within each field. If citations would be perfectly universal, the overall top 10% would overlap with the top 10% of each field, but this is not the case. Ignoring uncited articles does make the case for universal distribution stronger. So, the probability for zero citations may be something that is slightly separate from the overall citation distribution.

In a follow-up analysis Radicchi and Castellano (2012) come up with a clever way of empirically deriving a slightly different normalisation such that citation distributions across different fields collapse. They base this on comparing the overall citation distribution to all distributions of citations per field, and find that the transformation $\left( \frac{C}{a} \right)^\alpha$ produces highly similar distributions across nearly all fields of science, where $a$ and $\alpha$ are estimated empirically. For this universal distribution, they find it is well fit by a lognormal distribution.

As suggested by the analysis of Waltman, van Eck, and van Raan (2011), the case of zero citations may function slightly differently. These uncited articles are also considered by Wallace, Larivière, and Gingras (2009) when studying a century of citation distributions. In particular, they find that $e^{\beta N_r}$ is a good fit for predicting the number of uncited papers in a distribution, where $N_a$ is the total number of articles published in a year and $N_r$ the total number of references to those $N_a$ articles. This is based on a simple idea that the $N_r$ citations are randomly distributed across the $N_a$ articles, and the uncitedness is the probability of having drawn 0 references, at least within a short time window (2 years). They fit a stretched exponential to the citation distribution, where the probability to be cited $c$ times is

$$\Pr(C) \sim P(0) \exp \left( -\left( \frac{C}{\tau} \right)^\alpha \right)$$

where $\tau$ and $\alpha$ are estimated parameters, with $P(0)$ the separately modelled uncited publications. This is based on the idea that different papers accumulate citations at different rates, and that the overall distribution is a mixture of all those different rates. It is not clarified how the stretched exponential arises as a mixture of individual poisson processes with different rates. One possibility is to model the distribution as a mixture of Poisson distributions with the rate of each Poisson distribution following a Gamma distribution. That would result in a Negative Binomial distribution, which is studied by Mingers and Burrell (2006). Thelwall and Wilson (2014) finds that Negative Binomial regression is a bad fit, and advise against using it, and suggest to use simply an OLS logarithmic fit. To cover the entire range, Wallace, Larivière, and Gingras (2009) suggest that a distribution first suggested by Tsallis and de Albuquerque (2000) fits best:

$$\Pr(c) = \frac{P(0)}{[1 + (q - 1)\lambda c]^{\frac{1}{\tau}}}$$

with parameters $\lambda$ and $q$, but a clear behavioural motivation for this distribution is lacking.

The decline of concentration in citations is described by Larivière, Gingras, and Archambault (2009). They find that over time, the number of uncited papers continue to decrease (except for the humanities). Whereas in the 1920s about 70% of the articles remain uncited within 5 years, in the 2000s this has decreased to about 10-30%. The citation distribution also seem to become less skewed over time. Before the second World War, the percentage of papers that attract 80% of the citations increases from a few percent to 25–30%, and it continues to hover around that percentage, with most recent times seeing an even larger increase.

Redner (2005) also takes a longer term perspective, studying citation statistics from 110 years of Physical Review journals. He finds that the overall number of citations of all papers is well fit by a lognormal distribution. This is in a sense surprising, since he studies the distribution of papers from multiple years (1893–2003), in which case you might expect a mixture of yearly lognormal distribution, which could result in a stronger power law tail.

Moreira, Zeng, and Amaral (2015) find that citation distribution over sets of papers from authors and departments are well captured by a (discretised) lognormal distribution, and are relatively stable over time. Sinatra et al. (2016) find that the citation distribution over a set of papers from authors is well described by a lognormal distribution. Stringer, Sales-Pardo, and Amaral (2005) find that the citation distribution over journals is well captured by a (discretised) lognormal distribution,
and becomes stationary after about 10 years. They use this to rank journals by focusing on the probability that a paper from one journal is cited more highly than a paper from another journal. A similar journal ranking approach is taken by Milojević, Radicchi, and Bar-Ilan (2016) and also focuses on the probability that a paper from one journal is cited more highly than a paper from another journal. They find a clear relationship between this probability and the ratio of the average impact of the two journals that are compared. This seems to be a direct result of the fact that journal distributions are approximately lognormal. Other observed distributions most likely arise as mixtures of a lognormal, resulting in stronger power law tails.

B. Temporal decay

Citations generally decay over time. That is, most papers tend to cite recent work more frequently than older work. We can study this from two perspectives. We can study the age of references in papers, taking a retrospective, backward looking approach (Burrell, 2001), sometimes called a synchronous approach (Line and Sandison, 1974). Alternatively, we can look at how frequently a paper is cited in the years after it is published, taking a prospective, forward looking approach (Burrell, 2001), sometimes called a diachronous approach (Line and Sandison, 1974). This phenomenon is sometimes referred to as obsolescence, referring to the decline of use of certain publications over time. Publications need not become fully obsolete, but their usage may decline nonetheless. As Line and Sandison (1974) explain, there are various reasons why certain material may become obsolete. It is possible that the work is obsolete simply because its contribution has become incorporated as common knowledge in the field, sometimes referred to as obliterating by incorporation (Garfield, 1957). This is for example the case when a theory has become eponymised, where the author’s name has become synonymous with the theory, as is the case with the Nash equilibrium (McCain, 2011). Alternatively, the work may have become outdated, and has been replaced with more recent work. It may also really become obsolete, in the sense that it is no longer found to be of interest, for example because a paper belongs to an abandoned paradigm (Kuhn, 2012). Finally, it is possible that the work was simply incorrect, and that scholars tend to no longer cite it (Furman, Jensen, and Murray, 2012), although some citations continue after retractions, seemingly unaware of the retracted status (Bornemann-Cimenti, Szilagyi, and Sandner-Kiesling, 2016).

Many studies take a retrospective approach. In part, this is because this is easier to study because, as Egghe and Rousseau (2000) points out, prospective studies require to have linked references to the cited paper, while retrospective studies do not require this, and one can simply take the year of publication as mentioned in the reference.

Gross and Gross (1927) is one of the first who studied how publications reference literature in earlier years. Burton and Kebler (1960) introduced the half-life of attention/usage in this context, although half-life was already used earlier as a term according to Line (1970), while the concepts of growth, utility and obsolescence were introduced by Brookes (1970). Price (1965) introduced a measure of immediacy, later sometimes called the Price Index, defined as the percentage of references younger than t years.

Line (1970)\(^1\) discusses the real and apparent obsolescence, arguing that we should control for the number of papers being published, which increases exponentially each year. In his words “if every item had an equal probability of being used or cited, more use would be made of more recent literature simply because there is more of it.” He takes a retrospective approach and studies the (median) reference age. He introduces a very simple correction to the observed obsolescence factor. Suppose that the observed obsolescence is \(a(t) = \frac{c(t)}{c(t-1)}\), where \(c(t)\) is the total citations given to articles in the year t, from some reference year \(t' > t\). Now suppose that the number of citations \(c(t)\) has grown from year \(t - 1\) to t with a factor \(g(t)\) such that \(c(t) = g(t)c(t-1)\). In order to correct the obsolescence \(a(t)\) for this growth \(g(t)\), we should then divide \(c(t-1)\) by the expected number of citations \(\frac{c(t)}{g(t)}\) that were obtained had there been no growth. Hence, the growth corrected obsolescence should then be defined as \(a(t) = \frac{c(t-1)}{\frac{c(t)}{g(t)}}\). Assuming constant growth rates, \(a(t) = a\) and \(g(t) = g\), we then obtain constant corrected obsolescence \(d(t) = d\). The growth-corrected number of citations in year t then simply is \(c(t) = c(t-1)d\), such that \(c(t) = c(0)d^t\), and the infinite series \(\sum_t c(t)\) equals \(\frac{c(0)}{1-d}\).

The corrected half-life \(h\) is then \(\frac{\log \frac{1}{2}}{\log a + \log g}\), while the uncorrected (observed) half-life would be \(\frac{\log \frac{1}{2}}{\log a}\). Although a gross oversimplification, it captures nicely the way in which growth in the number of publications affects the apparent obsolescence of literature. With a yearly growth percentage of 5%, a median citation age of 7 years would suggest that items might be considered for removal from the library after 7 years, while in reality they would continue to be used for almost 14 years.

Brookes (1970) discussed some problems with estimating the obsolescence, and related this to geometric decay of utility such that \(c(t) \propto (1 - a)^{t-1}\) with an annual aging factor \(a\). Egghe and Ravichandra rao (1992) argue

\(^1\)Interestingly, this seems to be one of the earliest examples of a paper that append the report by one of the referees, at least that I am aware of. An early example of transparent peer review, even signed by the referee!
against the aging perspective from Brookes (1970) that assumes a constant aging factor. Instead, they find that aging has a certain minimum, suggesting there is a natural peak in reference age. They find that the most sensible distribution is then a lognormal distribution, based on finding a unique minimum in aging, and find it fits the data well.

Avramescu (1979) studies retrospective reference distributions. He suggested the following model to fit to the retrospective distribution:

\[ c(t) = C_0 \left[ \exp(-\alpha t) - \exp(-m\alpha t) \right] \]

(5)

where \( c(t) \) is the number of citations received \( t \) years after publication, and \( \alpha \) and \( m > 1 \) are some parameters.

Instead of working with obsolescence rates \( a(t) = \frac{f(t)}{f(t-1)} \), Egghe (1994) proposes a continuous counterpart for a continuous function \( c(t) \), namely \( a(t) = \exp(\log f(t))' \), where the prime ‘ indicates taking the derivative with respect to \( t \). This of course equals \( \exp \left( \frac{f'(t)}{f(t)} \right) \) so that this is the exponent of the relative growth of \( t \). This is a “true” rate as Egghe (1994) states, and makes intuitive sense and has some sensible properties. However, this formulation does not seem to have been used frequently.

Stinson and Lancaster (1987) studies citation aging from both a synchronous and diachronous perspective. They take into account a correction for the growth of the literature, but they do not report any particular distribution.

Redner (2005) finds an exponential decrease in the age distribution. As suggested by Nakamoto (1988), the growth in the number of publications is also relevant in this context. Combining such an exponential decrease with an exponential growth leads to a power law decrease in age (Redner, 2005; Egghe, 2005).

Vinkler (1996) formulates a relatively simple model for the possibility to be cited and finds that the possibility to be cited increases with the growth of the field. Faster growing fields are hence more likely to show higher chances of citations. This is also noticed by Hargens and Felmlee (1984) who argue that in growing fields, older work tends to gather more citations from recent work than in stable fields, in which the inequality between older and recent work is more balanced. This is also an argument for why scientists might be eager to jump on the bandwagon of a newly emerging field, exactly because it pays off to try to be one of the first movers in a new field.

Lariviére, Archambault, and Gingras (2008) take a long term perspective, and find an increase in the average reference age over the last decades. Similarly, in physics the average reference age was found to have increased over the last decades (Sinatra et al., 2015). Verstak et al. (2014) also finds that the average reference age increased over the last decades in various fields. There are some interesting peaks observed during both world wars by Lariviére, Archambault, and Gingras (2008). Relatively few publications were published during those two periods, showing a dip in the number of publications. The reference age increased during those periods, because mostly papers from before the war were referenced.

Egghe (2010) proposes a simple model for some observations of increasing reference age, as observed by Lariviére, Archambault, and Gingras (2008), while it still has a decreasing Price Index (i.e. proportion of references in the last \( d \) years). The model is quite straightforward, and just assumes that the literature grows exponentially, and that publications are cited completely at random. Even in that simplest case, one already sees an increasing reference age, but a decreasing Price Index, and generally, an increasing reference age is associated with a lower Price Index. Hence, qualitatively, these things do not require an explanation beyond simple exponential growth of the literature. Of course, the interesting question is now to what extent reference decay is different from such a pattern driven purely by publication growth.

Parolo et al. (2015) study the prospective citation distribution and state that the nature of the decay is not well established, varying between an exponential decay and a slower power law decay. They find that attention decays faster more recently than in earlier years. If they renormalise time in terms of number of papers however, they find that the decay rate is stable. Hence, the faster attention decay is simply a result of the increasing number of publications. They only study the decay after the initial peak of citations. Over time, the peak in citations has come increasingly faster, consistent with the increasing reference age found by Lariviére, Archambault, and Gingras (2008), according to Parolo et al. (2015). The decay after the initial peak is best fit by an exponential function. The half-life decreases over time, so that citations taper off increasingly faster in more recent years. Again, when rescaling time in terms of number of publications, this decrease is no longer visible.

Subelj and Fiala (2017) finds that the peak year of reference distributions (i.e. retrospective) has stayed stable in computer science and physics. The peak year of the the citation distribution (i.e. prospective) has shifted however, and is more volatile. The citation distribution seems to have changed over the years, especially for computer science. However, when normalising the citations based on the number of publications, all distributions seem to collapse onto a universal curve again. This corroborates the findings of Parolo et al. (2015), and the growth of the literature is an important factor that should be considered. As Egghe and Rousseau (2000) explain, growth influences aging, but it does not cause aging per se. They find that increasing growth rates leads to higher obsolescence, i.e. papers tend to become obsolete more quickly.

Pan et al. (2018) also studies the aging of reference distributions, and finds evidence of “citation inflation”: papers need increasingly more citations in order to be part of the top 5%. They find that in particular, citations to recent literature, and citations to very old liter-
nature decreases, while citations to the “middle” part are increasing.

Gingras et al. (2008) find that the average age of the references depends on the age of researchers. Younger researchers initially tend to cite younger references, but when researchers become older, their references age with them, with a turning point around 40 years of age. Most likely, researchers have established a certain library of references until 40, and only add new literature to this library of references gradually, meaning that the cited literature becomes increasingly older.

Herman (2004a,b) studies scholars’ literature search behaviour qualitatively. She finds that most people only go back a few year to look for references, otherwise, you are no longer considered to be up-to-date about the most recent evidence in your particular field. Most scholars mentioned that they will not search the literature further back than just a couple of years, but will perhaps follow up by chasing down references from that literature.

Porcena-Casasnovas et al. (2019) find that papers that reference a highly cited paper and are relatively highly cited themselves as well are published relatively shortly after each other. This suggest something like the start of a field, where an initial publication is cited by another paper shortly afterwards, both of which play a role in the ensuing citation dynamics and the influx of authors to such a field. Higher impact papers tend to cite younger papers and very young papers (< 1 year). They find that method references are typically older. This is also found by Bertin et al. (2016), who also find that references in the introduction of a paper are typically older. This most likely sets the stage and background of a field for a paper.

In principle, there is a certain connection between a retrospective and a prospective approach. The exact connection depends on the dynamics of the number of publications and the number of references throughout time. But, in general, if the retrospective distribution remains stable throughout time, it can be used to infer the prospective distribution, while making use of the empirically observed publication and referencing dynamics. Yin and Wang (2017) provide an exact relationship between the two approaches and find that

\[
\Pr(t_2 | t_1)M(t_1) = \Pr(t_1 | t_2)L(t_2)
\]

with \(\Pr\) the prospective distribution and \(\Pr\) the retrospective distribution, where \(M(t) = m(t)N(t)\) is the total number of references given at time \(t\), with \(m(t)\) the average number of references in year \(t\) and \(N(t)\) the total number of publications in year \(t\), which approximately grows exponentially over time \(N(t) \sim e^{\beta t}\), while \(L(t) = \int_0^\infty \Pr(t | \tau)M(\tau)d\tau\) is the total number of citations received by papers at time \(t\). Hence, one can derive the one distribution from the other. Arguably, the retrospective distribution is primary and the prospective distribution is derivative. After all, the retrospective distribution describes how researchers behave and choose to cite previous literature, while the prospective distribution is the result of that process.

Yin and Wang (2017) find that after normalising the citations for the number of publications it is well fit by a lognormal distribution (both prospective and retrospective). This entails that the unnormalised, crude, age distributions are a mixture of the publication and referencing dynamics and the actual lognormal decay. Why the decay is exactly lognormal is not clear though, but I will introduce some models that try to explain this in the next section.

C. Citation models

The various models that I cover in this section attempt to capture various observations. Some models try to explain the overall citation distribution, others target the aging distribution of references, while others aim to model individual paper citation dynamics, sometimes with an eye on predicting future citations.

One of the first models that was introduced in this context was developed by Price (1976). It introduced the notion of cumulative advantage, based on the ideas of the Matthew effect introduced earlier by Merton (1968), sometimes called a rich-get-richer effect. The model of Price (1976) aims to explain the broad distribution of citations that was observed earlier, and that I studied in section IA. The model is relatively straightforward and works as follows. For each time step, an additional paper is added to the population, citing \(m\) earlier papers. These references are not added randomly, but are assumed to be distributed proportional to the current number of citations of each publication. That is, the probability that paper \(i\) would be cited is proportional to \(C_i(t)\), the total number of citations at time step \(t\). Often some constant is added, in order to make sure that papers for which \(C_i(t) = 0\) also have some non-zero probability to be cited. The overall citation distribution is then affected by the influx of new papers at each time step, which initially have no citations, and the earlier publications which accumulate increasingly more citations. These forces give rise to a distribution, which Price (1976) calls the Cumulative Advantage Distribution \(c \sim (m + 1)B(c, m + 2)\) where \(B(a, b)\) is the Beta function. In the limit of large citations \(c\) this approaches a power law with exponent \(m + 2\), which for \(m = 1\) is close to the earlier observations of citation distributions reviewed in section IA. This idea of cumulative advantage was again suggested in the late 1990s in the context of complex networks by Barabási and Albert (1999), who termed this preferential attachment.

Redner (2005) finds some evidence for a linear preferential attachment, and suggests that a redirection mechanism could be reasonable. That is, instead of directly connecting to a paper with probability proportional to \(C_i(t)\), the idea is to pick a reference from a randomly selected paper (with probability \(1 - r\)) or simply reference
the randomly selected paper itself (with probability \( r \)), leading to a linear preferential attachment.

Demonstrating that there is a cumulative advantage effect in empirical observations is not easy. Often scholars study the relationship between the cumulative number of citations \( C(t) \) after time \( t \) and the additional citations in some time period \( \Delta t \) after \( t \). If the cumulative number of citations \( C(t) \) is correlated with this increase \( \Delta C(t + \Delta t) = C(t + \Delta t) - C(t) \), this is often taken as evidence for the existence of a cumulative advantage. However, this does not need to be the case. The inherent problem with this approach is that some latent variable \( i \) that the rate of attracting citations is a time dependent variable \( \lambda_i(t) \), with some constant latent citation rate, modulated by some time factor, i.e., \( \lambda_i(t) = \lambda_i f(t) \), with \( f(t) \) the obsolescence function. They estimate their model for journals, estimating the overall citation distribution and the decay, which allows them to predict citations for articles in that journal, but do not predict citations for individual articles. Burrel (2001) considers a similar starting model, where each paper \( i \) accumulates citations at a latent citation rate \( \lambda_i \) modulated by some time dependence \( f(t) \), so that the effective citation rate is \( \lambda_i(t) = \lambda_i f(t) \). In this model, the shape of the distribution of the time to the first-citation is independent of the mixing distribution of the latent citation rates, and only depends on the shape of the obsolescence function, suggesting that the obsolescence function follows some S-shaped pattern. Moreover, after sufficiently long time, the number of citations depend only on \( \lambda_i \) and not on the obsolescence function \( f(t) \). Burrel (2002) follows up on this work and investigates the \( n \)-th citation distribution of this model. He finds that a Gamma distribution of latent citation rates, which leads to a Negative Binomial distribution of citations, fits well the data, while relying on a obsolescence function that also follows a specific Gamma distribution \( \Gamma(2,1) \), corresponding to \( f(t) = 1 - e^{-t(1+t)} \).

Higham et al. (2017) propose that the rate of attracting additional citations is a separable function of preferential attachment and some obsolescence function, while taking a forward looking prospective view. The rate of attracting citations in year \( t \) is then

\[
\lambda(C(t),t) = a(C(t)) f(t),
\]

where \( f(t) \) is some obsolescence function that depends on time \( t \) only and \( a(c) \) is some cumulative advantage function that depends on citations \( C(t) \) only. In particular, they use functional forms \( a(c) = c^\alpha + c_0 \), and \( f(t) = d_0 \exp(-\frac{t}{\theta}) \). They test if these can indeed be separated by checking various years and bins of citations against each other, and find support for the idea of separability. They find that \( \alpha \) is about 1.0–1.2 while the exponential fit performs well only for \( t \geq 3 \). Based on this prospective model, they also derive an expression for the retrospective distribution of references. The evidence in favour of separability of citation dynamics into two processes is an important observation that simplifies ensuing modelling. Some earlier authors also proposed separable models. Dorogovtsev and Mendes (2000) seems to have introduced the earliest aging with preferential attachment model, including a separable formulation, and used \( f(t) = t^{-\alpha} \) and \( a(c) = c \), in terms of Eq. 7. Wang, Yu, and Yu (2009) also proposed a separable model with \( f(t) = \exp(-\lambda t) \) and \( a(c) = c \), and find it fits well some empirical data. Neither study explicitly address the separability though. Hajra and Sen (2006) propose to change earlier models to publish multiple papers at the same time, instead of sequentially introducing single papers, as was usually done, and find that this improves the fit.

Although the previously discussed models consider obsolescence, they have no theoretical explanation of why the obsolescence should follow a particular functional form. Simkin and Roychowdhury (2007) provide a mathematical theory of citation which does provide such an explicit mechanism. It is one of the few models that provides a mechanism for explaining why a certain aging function may appear. They propose that every year \( t \)
there are $N$ papers published that contain $N_r$ references on average. A fraction $\alpha$ of these references goes to randomly selected papers in the preceding year $t - 1$ (with $\alpha \approx 0.1$–$0.15$). This leads naturally to the first-year citations for papers published in $t - 1$ being distributed Poisson, in line with earlier discussed result from Wallace, Larivi`ere, and Gingras (2009), with $\alpha N_r$ expected citations. With probability $1 - \alpha$ then, a random reference from a random publication in year $t - 1$ is followed and is cited. Such a publication from year $t - 1$ might have cited a random publication from year $t - 2$ (with probability $\alpha$) or might have cited a random reference from that publication (with probability $1 - \alpha$), and so on. This leads to a branching process which Simkin and Roychowdhury (2007) solve analytically. The prospective distribution of citations is found to be a power law with an exponential cut-off. The retrospective distribution can also be solved analytically, and again results in a power law with exponential cut-off. In order to account for large exponential cut-offs and obtain a power law scaling, Simkin and Roychowdhury (2007) propose to add a latent citation rate parameter for each paper, similar to what I discussed above. Instead of choosing a random paper and a random reference from a random paper, scientists then choose a paper proportional to the latent citation rate. They consider a uniform distribution of latent citation rates, and marginalise the decay over this to obtain a citation distribution across all articles. The fitness distribution is not observed directly, and several possibilities yield similar observed citation distributions.

Peterson, Press´e, and Dill (2010) created a similar model as Simkin and Roychowdhury (2007), but propose for the initial step to find random papers from all years instead of the preceding year only. Their model is focused on the citation distribution, not on the aging distribution. Goldberg, Anthony, and Evans (2015) also consider a similar copying model, and find it to be the best fitting model.

Pan et al. (2018) propose a model that combines some elements from earlier models. It takes redirection from the models by Simkin and Roychowdhury (2007) and Peterson, Press´e, and Dill (2010), but also use an initial preferential attachment. That is, in each time step, $n(t)$ new publications are added (which itself grows exponentially). Each new publication cites directly an existing publication $j$ with probability $(a + C_j(t)) f(t_j)^\alpha$ where $C_j(t)$ is the number of citations to publication $j$, which is published at time $t_j \leq t$, and with an additional $k$ random references from publication $j$, with $k$ binomially distributed. They find their model to reproduce several stylistic features of citation networks.

Eom and Fortunato (2011) finds that a shifted power-law is a best fit for citation distributions. They propose to model the citation network as follows. Each step a new paper, i.e. node, is added to the citation network. The new paper $i$ cites a previous paper $j$ proportional to their current cumulative number of citations $C_j(t)$ and a certain decay as

$$c_{ij} \propto C_j(t) + \lambda_j f(t)$$

where $\lambda_i$ is a sort of latent citation rate of article $j$ and $f(t)$ is some decay factor, assumed to be exponential by Eom and Fortunato (2011). They find this model to reproduce various distributions reasonably well.

Wang, Song, and Barabási (2013) introduced a model that similarly combines a temporal decay with a rich-get-richer effect while also allowing for an individual article level parameter to account for variability across papers. In a sense, this approach is similar to what was proposed by Eom and Fortunato (2011), but they only considered aggregate properties, such as citation distributions, whereas Wang, Song, and Barabási (2013) try to predict citation dynamics of individual papers. This hence combines most previous elements, and is relatively similar in spirit to the model by Pan et al. (2018). More specifically, Wang, Song, and Barabási (2013) model the rate of attracting additional citations $c_i(t)$ at time $t$ as

$$c_i(t) \propto \lambda_i C_i(t) f(t_i)$$

with $C_i(t)$ the total number of citations up until time $t$ and $\lambda_i$ the latent citation rate of article $i$. It might be interesting to empirically compare this model, using a multiplicative formulation, to the earlier model by Eom and Fortunato (2011), which uses an additive formulation. Solving the model by Wang, Song, and Barabási (2013) leads to the result that

$$C_i(t) \propto e^{\lambda_i f(t_i)} - 1$$

where $F(t_i)$ is the cumulative distribution of $f(t_i)$. For $t_i \to \infty$ then, we have that $F(t_i) = 1$ so that after long enough we arrive at

$$c_i(t) \propto e^{\lambda_i} - 1.$$

This means that ultimately, after waiting long enough, the total number of citations is expected to be a result only of the latent citation rate $\lambda_i$, similar to what was observed by Burrell (2002). Wang, Song, and Barabási (2013) found their model to fit well the citation dynamics of many papers.

In a response, Wang, Mei, and Hicks (2014) wrote that they found that the predictive capability of Wang, Song, and Barabási (2013) was not so good and found that a naive prediction was actually more accurate. In a rebuttal Wang et al. (2014) argued that overfitting of their model should be prevented by using informative priors (in a Bayesian analysis), or alternatively, by regularizing the fitting procedure. One element of dispute seems to be the purpose of models. From the perspective of Wang, Mei, and Hicks (2014) the complexity of the model by Wang, Song, and Barabási (2013) is simply not necessary, since a simple prediction performs equally well, while Wang et al. (2014) argue that they model the dynamics that are seen in citations. One difference between
the two seems to be that, once the model of Wang, Song, and Barabási (2013) is in place, one could in principle predict forward citations across multiple years over time. The naive prediction that Wang, Mei, and Hicks (2014) considered was to actually assume citations after 5 and after 30 years simply have not changed, which is of course not informative. In addition, Penner et al. (2013) point to a problem when predicting citations, namely that many studies focus on cumulative number of citations, which is also relevant in this particular disagreement. Penner et al. (2013) argue that comparing cumulative citations is misleading, because one can easily predict cumulative citations from earlier cumulative citations, even if the process is completely random. That is, suppose that \( c_i(t) \) is a completely random variable, with the cumulative number of citations up until time \( t \) being \( C_i(t) = \sum_{\tau=0}^{t} c_i(\tau) \). The correlation of \( C_i(t) \) between time \( t \) and \( t + \Delta t \) can then be quite high and equals
\[
\sqrt{\frac{t}{t + \Delta t}}.
\] (12)

Hence, if \( \Delta t \) is small compared to \( t \), the correlation will be high (for \( \Delta t = 0 \) the correlation will naturally always be 1). Additionally, this states that for small \( t \), it is relatively more difficult to predict the future. It should be realised that these results are purely mechanical because the correlations that are studied are cumulative in nature. If, instead of the cumulative citations \( C_i(t) \), we would try to predict the instantaneous citations \( c_i(t) \), we would quickly learn that the expected correlation between any \( C_i(t) \) and \( c_i(t) \) is zero, exactly because the process is completely random. Hence, when comparing predictive capabilities of different models, the focus should be on predicting \( \Delta C(t + \Delta t) \), not on predicting \( C(t + \Delta t) \), which, as Wang, Mei, and Hicks (2014) also observe, can nearly trivially be predicted based on \( C(t) \).

II. EVALUATION OF RESEARCH

Research is regularly being evaluated, for various reasons. Sometimes this is done in order to choose what research to fund. Sometimes research is evaluated for hiring or promotion decisions. Research can also be evaluated with an eye on some form of quality assurance or accountability. The use and misuse of citation-based metrics is a regular feature in the literature on this topic. Here I briefly review some of that literature.

The role of journals in research evaluation is a contested subject since quite some time already. The Journal Impact Factor (JIF), which essentially aims to capture the average number of citations to a journal in the preceding two years, was originally developed for decisions about journal collection management in libraries (Larivière and Sugimoto, 2019). From the 1990s onwards, JIFs were increasingly used in research evaluation (Hicks et al., 2015). It was remarked that journals show a high heterogeneity of what they publish, and that you should not evaluate an individual article based on where it is published (Seglen, 1997), similar to the common adage that you should not judge a book by its cover. The JIF became increasingly contested, resulting in a call to abandon them for research evaluation in the Declaration on Research Assessment (DORA, 2013). The subject was also discussed in a workshop on Rethinking JIFs (Wouters et al., 2019). Some even talked about “Impact Factor mania” (Casadavall and Fang, 2014). Following a call to publish the full citation distribution instead of the JIF (Larivière et al., 2016), this is now available from the Journal Citation Reports. Still, also in recent times, the JIF continues to be used in promotion and tenure (McKiernan et al., 2019).

The JIF was reported to feature not only when evaluating research that has already been produced, but to also shape decisions of what research to work on (Rusforth and de Rijcke, 2015). The JIF structures discussions around what is novel and sufficiently high-quality to target high-impact journals. The JIF was not used per se to say something about the potential novelty and quality of the science itself, but was also seen as a “ticket” to advance one’s career. Importantly, this shows that the JIF is not just about targeting specific journals once the research itself is already done; the research is done and shaped with impact factors in mind. Indeed, this phenomenon has been called “thinking with indicators”, shaping not only post-research where a manuscript should be submitted, or how something is evaluated, but also actively shaping what research is done (Müller and de Rijcke, 2017). These effects of indicator usage have been reviewed more broadly by de Rijcke et al. (2016).

One important recurrent theme in this context is that of goal displacement. This phenomenon is sometimes known as Goodhart’s law, or Campbell’s law: scoring high on assessment indicators becomes more important than doing well on whatever those indicators were meant to measure. This is closely related to the so-called constitutive effects of performance indicators (Dahler-Larsen, 2014). When indicators are used in practice, they may have an effect on how people respond. This should not be thought of as an “unintended consequences”, but rather, the usage of the indicator itself defines that which is thought to be evaluated. Hence, if citations are used to evaluate research quality, they might not necessarily be misused, but rather, an indicator, such as citations, comes to represent the very object that they purport to measure. For instance, by publishing university rankings, by their very usage alone, such rankings may come to constitute measures of university “performance”. More highly ranked universities may attract more students and more high-qualified personnel. These effects may pertain not necessarily because the university ranking itself is “correct”, but because the ranking itself produces these effects. Something similar may happen with journal impact. Journal impact rankings and publicly visible indicators, such as the JIF, may reify through constitutive effects any initial ranking of “journal impact”. That
is, if scholars start to judge journals by such a ranking, they might start to submit their best work to the highest ranked journal, which thereby may solidify, or even improve their ranking, while lower ranked journals may start to receive less well manuscripts, thereby potentially lowering their ranking. In this sense, constitutive effects may function similar to self-fulfilling prophecies. Whether constitutive effects ameliorate or deteriorate outcomes is not clear a priori.

Molas-Gallart and Rafols (2018) provide a broad critique of indicators. Citation based indicators may not align well with research objectives, leading to an “evaluation gap”. They argue that scientists respond to evaluation by aiming to improve their performance as measured by indicators, similar to constitutive effects. If such an evaluation has the desired properties, this effect might be positive, but this need not be the case. Even without responding strategically to such incentives, evaluations may act as a selective pressure (Smaldino and McElreath, 2016), that is it does not require constitutive effects in order to exert an influence.

Bhattacharya and Packalen (2020) also critique metrics based on the argument that attention (i.e. citations) to novel ideas have decreased, and that evaluating people based on citations effectively selects against novelty. They argue that more scientists have started to work on only incremental advances that will be more likely to be cited, instead of working on foundational groundwork.

A particular context in which metrics are sometimes used for research evaluation is in performance-based university research funding systems (PBRFS), which were reviewed by Hicks (2012). PBRFS are (1) about research; (2) evaluated ex post; (3) distributing funding according to the evaluation; and (4) national. Although the distribution of funding is an important component, it seems that many PBRFS also feed into a prestige competition. The first and perhaps most well-known PBRFS is the UK’s Research Assessment Exercise (RAE), nowadays known as the Research Excellence Framework (REF). In general, PBRFS aim to stimulate excellence, or fund more selectively, in order to allocate scarce resources more effectively. The resource concentration also has been linked to the “new public management” that became more dominant in research policy circles. The most common unit of evaluation is the department of universities or research organisations, although some countries also evaluate individual scientists, for example for appointing professors.

There is a larger literature discussing the potential effects of PBRFS. Butler (2003) performed a seminal study on the increase of publications in lower impact journals following the introduction of a PBRFS in Australia. Another effect of introducing PBRFS is that researchers may cite other work more heavily (Baccini, De Nicolao, and Petrovich, 2019). Some authors found evidence that self-citations increased after the introduction of an evaluation system for promotion in Italy (Seeber et al., 2017). Moed (2008) showed that the UK RAE exercises seemed to affect publishing practices by UK scholars. The classical work by Butler (2003) was revisited by van den Besselaar, Heyman, and Sandström (2017), reaching different conclusions: productivity and impact both increased in the Australian case. However, generally, causes and effects in PBRFS are rather challenging to disentangle (Aagaard and Schneider, 2017), as argued earlier by Osuna, Cruz-Castro, and Sanz-Menéndez (2011). Gläser and Landel (2016) describe the overall problem of inferring how macro level science policies affect macro level outcomes. Their central question is: How does research governance change knowledge production? In order to convincingly study this, one not only needs to have some study at the macro level, which is bound to be affected by problematic confounding effects (e.g. other changes happening simultaneously), one also needs to make a convincing case for a macro-micro-macro link. That is, it should be made reasonable that the macro policy affects researchers’ behaviour, which in turn becomes visible at the system level again. One problem which has not been noted often in this context, is that an increase in national productivity may also increase national citations. Such higher within-country citations are regularly observed (Schubert and Glänzel, 2006; Bakare and Lewison, 2017), similar to citations in the same language (Bookstein and Yitzhaki, 1999). This raises the question of how to disentangle an increase in citations due to a higher productivity from an increase in citations due to actual differences in research quality. Whether such observations are really driven by national citation biases, or whether they are a result of more general geographical patterns, as observed by Pan, Kaski, and Fortunato (2012) is not clear.

Sandström and Van den Besselaar (2018) study the performance of several national science systems. They conclude that having ex post evaluation, combined with high institutional funding may be most efficient. Ex ante evaluation, either through grant funding, or through lower professional autonomy and more university management, may result in lower efficiency, and may possibly reinforce the existing academic elite.

Schneider, Aagaard, and Bloch (2016) compared effects of PBRFS in Australia and Norway. They find that, unlike in Australia (Butler, 2003), the introduction of a PBRFS in Norway that awarded publications did not show a decreasing impact or an increasing output in lower impact journals. The important difference here is whether the evaluation differentiates the awards based on some impact indicator. In the Norwegian case they differentiated between lower and higher impact tier outlets. Bloch and Schneider (2016) study the effects of the Norwegian model further, and conclude that due to the fractionalization, the system may not properly reward collaboration.

In principle, evaluation at the institutional level is to be stimulated (Tiokhin et al., 2021). Institutional evaluation may alleviate some problems that might appear at the individual level, where contributions other than
scholarly publications might be disregarded. An institution can take a broader perspective, and can in for example hire someone who does not directly produce scholarly output, but who has a large indirect effect on scholarly output, for instance by maintaining critical infrastructure. Unfortunately, one recurrent problem of evaluation at the institutional level seems to be that institutions pass down the institutional requirements directly to lower levels (Gläser, 2007). For example, in the UK REF system, which is an institutional evaluation, the institutions organise so-called mock-REFs to identify areas where individual scientists could improve their performance, with sometimes dire consequences for their future career (Owens, 2013).

One important point of the causal inference of the effect of the introduction of a PBRFS is that we should differentiate between system level effects and individual level effects. For example, consider that we fund institutions differentially, based on some performance indicator. After a few years, it might be possible that the overall performance has increased. At the same time, it might be that the differences between institutions have become smaller: all institutions have increased their performance. Differentiating between institutions then may become more difficult, and those who receive more funding may not necessarily do much better than other institutions that received less funding in one year. More formally: suppose that there are \( n \) institutions (or individuals), each with a certain quality \( q_i \). Suppose that we select a certain number of individuals \( k_1 \) out of the \( n \) existing ones, where evaluation is based on some evaluation score \( q_i + \epsilon_i \), with \( \epsilon_i \) some noise, with only the top \( k_1 \) individuals being selected. After selection the average \( q_i \) will be higher than before selection. Repeating the selection, we select \( k_2 \) individuals out of the \( k_1 \) earlier selected individuals. We then see that the evaluation score \( q_i + \epsilon_i \) will show a lower correlation with the underlying quality \( q_i \), and will hence show greater variability. It may then appear that doing such a selection may not be predictive of future performance, while at the same time, the selection did increase the overall quality.

A. Peer review

Most scientists would argue that the scientific “quality” of a paper is a multidimensional concept (Aksnes, Langfeldt, and Wouters, 2019). For example, in most journals peer review is based on multiple criteria, such as novelty, potential impact and methodological rigour or correctness. In recent years, peer review is heavily discussed, with multiple possible interventions on several fronts, such as open peer review, post-publication peer review or collaborative peer review (Woods et al., 2022). In almost any evaluative settings, the focus is on trying to evaluate research “quality”. The question is how either peer review or citations can reflect such “quality”. Let me briefly review some of the literature on peer review.

Bornmann (2011) provides a general overview of peer review and identifies a number of problems of peer review. One particular problem is poor reliability: the inter-rater reliability between peer reviewers is generally low. This was already observed early by Cole, Cole, and Simon (1981), but was also confirmed in later research again by Ernst, Saradeth, and Resch (1993), Rothwell and Martyn (2000) and Pier et al. (2018). Perhaps the uncertainty in peer review is also one of its strengths: it is difficult in advance to tell how something will be evaluated by peers, so using peer review for evaluation decreases the chances of people targeting a specific indicator. The poor reliability of peer review also opens up the possibility of another problem, namely bias. When a decision needs to be made, and it is difficult to come to a clear conclusion because the evaluation suffers from a high uncertainty, then possibility for bias perhaps becomes larger to “tip the scale”. At the same time, poor agreement on evaluation may simply also reflect different positions and considerations that reviewers may have on a paper. Peer review can indeed improve the reporting of findings (Goodman et al., 1994), although the textual changes are often relatively minor (Klein et al., 2016). In a sense, poor agreement demonstrates that multiple reviewers can provide more comprehensive feedback than a single reviewer. If reviewers would simply reiterate the same point, there is little added value of the additional reviewer, and would be redundant in a sense. Initiatives, such as the consultative peer review from eLife (King, 2017), in a way try to benefit from this diversity and suggest an innovative approach to consolidate the various points raised by multiple reviewers.

As said, another problem of peer review is that of potential bias: factors unrelated to “quality” may affect peer review (Lee et al., 2013). The difficulty here is that it can be challenging to establish whether something is a bias. For example, simply showing that authors from a particular institution have higher peer review scores is insufficient: it is possible that such authors simply more often produce higher quality work. Comparisons of double-blind to single-blind peer review reveal some interesting effects, where author and affiliation reputation seems to affect the acceptance of manuscripts (Tomkins, Zhang, and Heavlin, 2017; Okike et al., 2016).

Another problem that Bornmann (2011) identifies is that of validity: peer review might be unable to predict scientific impact or relevance. However, the problem is that scientific impact and peer review itself may be noisy: how will we measure scientific impact? For example, if you compare the best unfunded scholars to funded scholars, as done by van den Besselaar and Sandström (2015), it might very well be that the best unfunded outperform the funded, not because the best unfunded are “better” than the funded, but simply because citations are such a noisy proxy (Lai, Traag, and Waltman, 2020). Bornmann and Daniel (2008b) analyse the citation outcomes of both accepted and rejected publications at the prestigious Angewandte Chemie International Edition, and
find that peer review outcomes are predictive of the subsequent citations. However, this conclusion is problematic if there is a causal effect of where a paper is published on how frequently it is cited. Being published in a high-ranked journal will affect the subsequent citations (Traag, 2021), and the citations do not necessarily reflect whether peer review is predictive, the citations just reflect the causal effect of being published in a certain venue. A similar problem plays in a recent analysis of the predictive validity of peer review when highlighting publications in a journal (Antonoyiannakis, 2021).

### B. Metrics

Much research in scientometrics is not necessarily interested in the citation models that I briefly covered in section I. Instead, much research is interested in factors that somehow seem to affect citations, ranging from effects of authors to institutions. Some also study aspects such as title length, number of pages and other characteristics, but I will ignore those studies here. Most of the more quantitative studies do not explicitly use any citation model, but simply compare different articles with each other in one way or another, and try to draw conclusions from that comparison. Other studies focus on the meaning of citations, and study the different types of “influence” that citations capture.

MacRoberts and MacRoberts (1989) provides a comprehensive overview of some of the problems in citation analysis. Although their overview is over thirty years old by now, many of the identified problems are still playing a role, and continue to be discussed and studied. I already covered some common problems in section I, namely varying citation patterns in different fields, years and document types. Another category of problems concerns whether citations really capture the idea of “influence” or impact: not all influences are cited and some works are cited that have no influence (so-called perfunctory citations). There are different types of citations (Bornmann and Daniel, 2008a), which do not show an equal influence, with some citations for example being negative (Lamers et al., 2021). This does not necessarily mean that highly-cited publications are not influential. For example, Teplitsky et al. (2020) finds that highly-cited publications are actually more likely to have an intellectual influence on the work they are cited. Another category of problems mentioned by MacRoberts and MacRoberts (1989) is more technical and relates to coverage issues (Visser, van Eck, and Waltman, 2021), problems of reference matching (Olensky, Schmidt, and van Eck, 2016) and problems of author disambiguation (Caron and van Eck, 2014).

Co-authored papers are cited more frequently, and this holds for multiple authors, multiple institutions and multiple countries (Larivière et al., 2015). This seems not a result of self-citation, but really represents greater “epistemic value”, as stated by Larivière et al. (2015). Of course, whether citations accurately reflect this “epistemic value” is up for discussion. One question is what causes this larger number of citations to this type of work. Possibly the most epistemically interesting research question are addressed by larger teams. If so, the collaboration is a result of the epistemic value, and not a cause, and increasing collaboration itself will not lead to more “epistemically valuable” work. Wu, Wang, and Evans (2019) have looked at this from a slightly different angle and finds that larger teams typically produce less disruptive papers, but they are more likely to be more highly cited.

Cole and Cole (1968) find that visibility of authors is determined by the prestige of a department. Cole (1970) finds that departmental prestige also affect (early) citation counts, especially for work that is of lower quality. Similarly, Medoff (2006) finds that institutional prestige drives citations in economics, but only for elite universities (Harvard and Chicago in this case). Still, it could be that authors at more prestigious departments are more likely to perform higher quality research. Way et al. (2019) find evidence that research quality is driven by scholars’ current work environment, and that it is not driven by selection of more highly cited scholar into more prestigious departments.

The role of journals in citations has been debated for a long time. As I already discussed earlier, citation distribution of journals are roughly lognormal. Correlations between the JIF of a journal, and the individual citations for each article is generally low (Seglen, 1997). A more recent revisit of the work by Seglen (1997) again found that correlations between impact factors and citations are relatively low (Zhang, Rousseau, and Sivertsen, 2017). It was also shown that the correlation between the JIF and citations is weakening over the years (Lozano, Larivière, and Gingras, 2012), which was speculated to have been caused by digitalization. Indeed electronic publication was observed to narrow the referencing, also to more recent literature (Evans, 2008). Yet, at the same time, where an article is published is one of the strongest single predictive factor of citations in several studies (Stegelhuis, Litzvak, and Waltman, 2015; Callaham, 2002; Abramo, D’Angelo, and Di Costa, 2010; Mingers and Xu, 2010).

As already stated earlier, the fundamental problem is that research quality is unobservable. Clearly, citation distributions are highly skewed for each journal, and also overlap to a large extent, as I discussed earlier. However, citations are only a proxy of quality, and are not equal to research quality. Similarly, being published in a certain journal may be a proxy of quality. The question is then: which is a better proxy? Although many people may argue that citations are a more accurate proxy, this need not be the case, as Waltman and Traag (2020) demonstrate. It is possible that all articles within the same journal have the same quality and that the broad distribution of citations is simply due to citations being a noisy proxy of this identical quality. The average of these noisy citations can then be a more accurate representation of...
the underlying identical quality than the actual citations. The extent to which journals publish similar quality articles is up for debate. This for example will depend on reviewer uncertainty when scholars submit publications. If there is substantial uncertainty, and reviewers try to assess the actual quality of the papers, then the resulting distributions of quality in journals may largely overlap (Starbuck, 2005).

High-impact journals are more widely circulated, and hence have a higher readership (Peritz, 1995). There is a certain circularity here, and path dependency: higher impact journals have a higher readership, which attracts more interesting submissions, which in turn attracts more readers, which in turn attracts more citations. Ellis and Durden (1991) found that current journal prestige seems to be mostly determined by previous journal prestige and current impact, lending some support to this idea of path dependency and conservatism of journal prestige.

More generally, there are clear effects of publicity on citations. Phillips et al. (1991) analysed what papers were being discussed in the New York Times, and how that influenced citations ten years later. Using a three month period during which the NYT did not appear, but the editorial process and selection remained, they studied the causal effect of publicity in the NYT. The found a quite strong effect: featured papers received 73% more citations. At the same time, the newsworthiness itself also seems to predict the impact of the journal in which an article will appear (Callaham, 2002).

Citations to identical papers showed that versions that were published in more highly cited journals were cited more often (Knothe, 2006; Perneger, 2010), which was also coined as the Impact Factor’s Matthew effect (Larivière and Gingras, 2010). Seglen (1994) questioned whether there was any causal relation between JIF and citations. I will get back to this in section III.

C. Comparing peer review and metrics

Metrics have been regularly compared to peer review outcomes. Both are thought to somehow reflect scientific “quality” or “impact”, and both have been used in research evaluation. One central difference is that metrics can only be used post-publication, while peer review is also used frequently pre-publication, for example when reviewing journal submissions. Peer review for grant applications is a mixed exercise in a sense. On the one hand it evaluates past performance of applicants and is backward looking. On the other hand, it evaluates proposed research and its expected impact, quality or novelty, and is forward looking. Many national PBRFS I discussed earlier, such as the UK REF, the Italian VQR or the Norwegian system are post-publication evaluation systems, and some are based explicitly on metrics (such as the Norwegian model), while others are based on peer review (such as the UK REF) or a mixture of the two (such as the Italian VQR). In the influential Metric Tide report (Wilsdon et al., 2015), the use of metrics in the national research evaluation in the UK was extensively discussed. They concluded that metrics could support but not supplant peer review, as also summarised by Wilsdon (2015).

Aksnes and Tønsberg (2004) compare peer review and metrics in Norway. They find that normalised citations correlate best with peer review evaluations at the research group level, and report higher correlations for higher aggregate levels. The average journal impact shows a similar level of correlation with peer review. Interestingly, when considering citations relative to the journal (i.e. controlling for the journal impact) they find the lowest correlation, which is non-significant.

Bornmann and Leydesdorff (2013) finds that peer review, in the form of recommendations from F1000, is correlated with a number of citation based indicators. Noticeably, this again finds that when normalising based on the journal, there is barely any correlation between peer review and this journal-normalised citation-based indicator.

Radicchi, Weissman, and Bollen (2017) asked respondents to compare pairs of papers, and asked them which paper had a higher influence on their own work. Generally, they find a rather low correlation between citations and those pairwise preferences, but for respondents’ own papers, more highly cited papers were more often said to have a higher influence on their own work.

Adams, Gurney, and Jackson (2008) compared evaluation outcomes of papers in the RAE with journal-normalised citation scores, and found that they essentially did not correlate. Eyre-Walker and Stoletzki (2013) also showed that correlations between evaluation and citations are minimal when controlling for the journal, although some of their conclusions have been questioned by Eisen et al. (2013).

There are a few problems when comparing peer review and metrics. Studies have reported wildly varying correlations, ranging from as low as 0.3 to as high as 0.97. There are two major factors that explain the differences in these results (Traag and Waltman, 2019). The first factor is what level of aggregation is being studied. At the individual paper level, one typically sees a lower correlation, and papers with the same number of citations are evaluated quite differently in peer review, or vice-versa, papers with the same peer review outcome can receive quite a number of different citations. However, when aggregating to higher levels, correlations typically go up. For instance, at the author level, one might expect slightly higher correlations. At the level of research groups one might expect again higher correlations. At the level of faculties, or even entire universities, one might again expect correlations to be typically higher. The second factor is whether an analysis studies size-dependent or size-independent correlations. In the size-dependent perspective the total quantities are being studied, for example the total number of publications that are in the top 10% of the citation distribution, and compare that to the total number of publications that were eval-
uated as “world class” by peer review. To a large extent, correlations in the size-dependent perspective are driven by the common denominator for both quantities, namely the total number of publications $n_i$. The distinction between size-dependent and size-independent is of course only relevant at higher aggregate levels, since this distinction is void in the case of the individual paper level. Correlations at the aggregate university level using a size-dependent perspective indeed achieve the highest correlations (as high as 0.97, e.g. Harzing (2017)), while correlations at the individual paper level are typically the lowest (as low as 0.3, e.g. Wilsdon et al. (2015)).

How to reconcile these different correlation outcomes then? What level of correlation is most relevant? Perhaps more importantly, what level of correlation should be considered “sufficiently high”? As we argued elsewhere, the answers depend on the intention of the evaluation (Traag and Waltman, 2019). If the outcomes are being used at a paper level, then the correlation at the paper level is most relevant. If the outcomes are being used at a higher aggregate level, then the correlation at that higher aggregate level is most relevant. What correlation should be deemed “sufficiently high” is not directly clear. Although a correlation will generally fall between −1 and 1, being near 1 does not necessarily make it a high correlation. For example, if we have two independent (hence uncorrelated) $x_i$ and $y_i$, but study the correlation between $n_i x_i$ and $n_i y_i$, then still the correlation might be high$^2$. This is exactly what is happening in the size-dependent perspective. The question then is: what is the upper-limit of correlations that we can expect from a certain process? For the correlation between $n_i x_i$ and $n_i y_i$, the upper limit can be quite high, and hence, even if we would observe a correlation empirically of 0.8, this should not necessarily be thought of as high. The correlation between $x_i$ and $y_i$ however, should be close to zero, so if we see a correlation of 0.3, then that is already quite a clear difference. Of course, we do not know the exact generative process underlying metrics and peer review. However, when comparing metrics to peer review, it might make sense to put the observed correlations in perspective by comparing peer review to other peer review outcomes. In an evaluation exercise this can be thought of: what if we were to repeat the evaluation exercise, how large can we expect the differences to be then?

Analysis of data from the Italian VQR exercise shows that peer review is not very reliable (Bertocchi et al., 2015), as I already discussed earlier. Compared to correlations between two peer reviewers, correlations between peer review and metrics are found to be comparable. This holds not only at the individual paper level (Bertocchi et al., 2015) but also at the aggregate institutional level (Traag, Malgarini, and Sarlo, 2020). The correlations at the institutional level are typically higher, and this holds both for correlations between two peer reviewers and between peer review and metrics. Of course, an average evaluation outcome can be estimated more accurately when using more peer reviewers. Hence, in the limit of many peer reviewers, repeating an evaluation exercise should give identical answers, but may still leave a difference between peer review and metrics. However, in practice, the resources to do such a large scale peer review exercise are generally limited, and so this is infeasible in practice. A recent study by Forscher et al. (2019) reported that in the context of NIH funding, one would need as many as 12 reviewers to obtain a modest reliability in funding decisions.

The more interesting question now is: if differences between peer reviewers are similar in terms of magnitude to differences between peer review and metrics, then are these differences somehow systematic or are they random? If the difference is random, the distinction between the two does not matter much. However, if there are systematic differences, it might be important to investigate this further. After all, if differences are systematic, then perhaps using peer review instead of metrics means effectively selects on other things. For instance, if there is a systematic difference and some highly reputed institutions score lower on citations than on peer review, then changing a funding system to use citations instead of peer review will fund prestigious institutions less well than when using peer review. Again, the fundamental problem appears: research quality itself is unobservable, and hence whether citations or peer review is affected by such institutional reputation is not immediately clear. Perhaps both are affected by institutional prestige, but in different magnitudes, in which case neither citations, nor peer review is “unbiased”. As always, it is difficult to argue on the basis of these differences whether peer review or metrics provide a “better” basis for evaluation.

### III. LEARNING

I briefly reviewed both citation models and some relevant aspects of research evaluation, including peer review, metrics and comparing the two. Now the central question is: what can we learn from citation models when studying aspects of research evaluation? Vice-versa, what can we learn from research evaluation when studying citation models? I will try to answer both these questions below.

#### A. Learning from citation models

There are two things I believe we can learn from citation models. First of all, more explicit citation models may help us to draw correct inferences about certain effects, as I will argue below. In addition to such a basis for

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$^2$ There is a certain similarity here with the earlier argument from Penner et al. (2013) that cumulative citations are expected to correlate highly.
inference, it also provides some clarity about uncertainty, which I believe can be useful in various settings.

As I reviewed in section II B, there are many questions of how various factors may or may not affect citations, such as author reputation, institutional reputation and journal reputation. However, the inference of these effects is tricky. Such comparisons are often complicated by the fact that these need to compare temporal aspects. Without properly considering those, it may make inferences about such effects more difficult.

Models such as the one by Wang, Song, and Barabási (2013) may help to disentangle these various factors. Of course, other models could be chosen as well. But let us take that formulation as a starting point. Essentially, the rate at which an article attracts citations can be formulated as \( \lambda(t) \), where \( \lambda(t) \) can be composed of multiple factors, such as authorship, affiliation status, nationality, language, or, in this case, the journal. Following Wang, Song, and Barabási (2013), the number of citations at time \( t \) can then in general be modelled as

\[
c(t) \sim \lambda(t) f(t) C(t)
\]

with \( C(t) = \sum_{\tau=0}^{t-1} c(\tau) \) the cumulative number of citations. As said, \( \lambda(t) \) can be composed of various factors, and let us assume that \( \lambda(t) = \prod_k \phi_k(t) \) is a product of these factors \( \phi_k(t) \), which may include factors like author reputation, affiliation reputation, journal reputation, but also novelty, interdisciplinarity, methodological rigour, data quality, et cetera. In general, this formulation would be highly degenerate: the overall rate \( \lambda \) may be caused by a higher \( \phi_1 \) or a higher \( \phi_2 \) and it is not clear how we can properly identify and estimate effects of these various factors separately. With additional assumptions, some of these effects may sometimes be estimated, but this can still be quite tricky. For example, we could posit that each individual scholar has a particular effect that is stable throughout time, which we could then try to infer. Still, in that case, the problem is that the actual quality as a factor \( \phi_\text{a} \) could be correlated with the author effect \( \phi_\text{a} \), so that we still cannot identify the two. Alternatively, some \( \phi_k \) may change at some point \( t' \), making the \( \lambda(t) \) time dependent. This would allow us to infer the effect of this factor \( \phi_k \). For example, if we see that the overall inferred \( \lambda(t) \) before \( t' \) differs from the inferred \( \lambda(t) \) after \( t' \) (if all other factors remain identical), we could then use those to estimate the change in this factor \( \phi_k \). For example, one can consider differences in citation rates when authors become affiliated with other institutions, as was done by Way et al. (2019).

Let me briefly illustrate the approach for trying to infer the effect of the journal in which articles are being published (Traag, 2021). Of course, articles are (usually) only published in a single journal, except in rare cases. As I discussed earlier, some of these rare duplicates revealed that citations mostly flowed to the version that was published in the highest impact journal. However, it is not clear whether this effect is specific to these type of publications, or whether this generalises to other papers. Instead of using duplicate publications, I try to infer the effect of journals on citations by comparing citation rates to the preprint version with the citation rates to the published version. This is exactly the type of setting where \( \lambda(t) \) is changing at time \( t' \), namely when it is published, in a single factor \( \phi_j \), the journal where it is published. All other factors arguably remain unchanged upon publications.

In Traag (2021) I showed that, using this approach, we indeed see a causal effect of where a publication is published on how often it is cited. Hence, it matter where you publish a paper, potentially quite a lot: a paper in Condensed Matter from 2016 published in Nature, might have gotten as much as 350 citations in Science, although estimates of these effects come with quite some uncertainty.

This also points to another advantage of such citation models. It clarifies that even for a single paper, the number of citations are not uniquely pre-determined. That is, for each observed outcome, a different outcome might have been observed, if the entire citation dynamics were replayed. In a sense, this raises a deeper question about noise. Is noise just modelling some unknown factors that are influencing citations? Or does noise represent some inherent uncertainty in the process? Note that, even for a simple Poisson process, the variation equals the mean, so even in such a simple process, the latent citation rate can be considered to be quite uncertain given a certain number of observed citations. If citations are following some form of cumulative advantage process, the variation would be even greater, meaning there is even more uncertainty regarding any possible latent citation rate that this underlies. This uncertainty can be well-captured in a Bayesian approach, I believe. Nonetheless, we should keep in mind that regardless of what model we use to infer parameters and their uncertainty, there is an additional level of uncertainty that concerns the uncertainty of these models themselves. Any inference with regards to such parameters and uncertainty are contingent on the model itself.

Nonetheless, such an explicit consideration of uncertainty might be good to consider in many cases. For example, for an early career researcher, we might observe only a few papers and a few citations. When using empirical means, or other aggregate statistics, we might easily reach overly extreme conclusion. For example, if none of the few early papers have been cited yet, a direct empirical citation indicator would simply be 0. In reality of course, such papers may still be cited, and may not reflect the citation potential of future papers. Similarly, if one of those early papers happens to have already become very highly cited, a direct empirical citation indicator might be extremely high. Relying on Bayesian inference while using informed priors, based on earlier observed distributions of citations, might then provide much more reasonable estimates of performance, shrinking the observed outcomes (either upwards or downwards) towards a more reasonable estimate. Hence, in such a setting, having a
clearer understanding of the distributions involved, or the models involved, helps provide more reasonable estimates. One such setting is for example the impact of journals, for which Antonoianakis (2018) observed that smaller journals tend to have more extreme citation averages. As Antonoianakis (2018) argues, to some extent, this is simply a result of the law of large numbers: larger samples will show less variation. The most principled way of addressing this may be based on reasonable informed priors of citation distributions for journals. This way, the more extreme results for smaller journals would be more strongly shrunk to the prior than for larger journals, thereby providing a more reasonable estimate of their citation impact. Similar arguments could be provided for estimates of citation impact of for example research groups or departments, and perhaps even at higher institutional levels.

So, in short, I believe that citation models are not necessarily most relevant to predict the future. It is not necessarily of interest per se to predict whether a particular paper will become highly-cited or not. Rather, I believe one of its main uses is to serve as a basis for the inference of other effects. This both holds in terms of full citation dynamics, but also for more elementary distributional observations. Using such reasonably informed prior distributions may prove helpful in drawing more reasonable conclusions.

B. Learning from research evaluation

What we can learn from research evaluation is that citations are not relevant per se. Rather, citations might be used as an indicator, a proxy, for research quality. When building citation models, we should therefore acknowledge the fundamental problem: research quality is unobservable. This means we cannot simply rely on citations models to draw inferences of research quality, and cannot simply draw conclusions about “academic success” based on citation information.

However, what then do we learn from citations in the context of research evaluation? As said, citations are assumed to be some type of indicator. But what is an indicator exactly? Let us try to develop a preliminary notion of what an indicator is. In my understanding, any variable that is causally affected by the variable of interest would be an indicator. So, if somehow $X \rightarrow \ldots \rightarrow Y$, then $Y$ would be an indicator for $X$, with the arrows representing a causal effect. Typically, we do not know $X$ and we therefore use $Y$ to say something about $X$, and it is in this sense that $Y$ is an indicator for $X$. However, what is typically the case is that some other factors $U$ also affect $Y$ such that $U \rightarrow Y$. In this case, $Y$ might still be an indicator for $X$, but if $U$ is not also exclusively affected by $X$, we could say that $Y$ is a biased indicator for $X$. After all, we use $Y$ to say something about $X$ in this context, but $Y$ is also affected by $U$ which is not relevant for $X$. More concretely, if we have $Q$ the “quality” of an article (which can be multidimensional) and $C$ citations, where it is assumed that $Q \rightarrow \ldots \rightarrow C$, then citations are an indicator for quality. However, if citations $C$ are also influenced by other factors $U$ that are deemed irrelevant, such as author affiliation, then using citations $C$ as an indicator for quality $Q$ would be biased by the affiliation $U$.

There are a number of circumstances in which we assume that the quality $Q$ is unrelated to some other factors that are typically related to citations. The two most well-known such factors are the field of study $F$ and the year $Y$, where we assume that $F \rightarrow C$ and $T \rightarrow C$, but that $F$ and $T$ are independent of $Q$ otherwise, see Fig. 1 for an illustration. Indeed, under the assumption that $F$, $T$ and $Q$ are independent, we can try to make $C$ a more accurate indicator for $Q$ by normalising citations $C$ based on $F$ and $T$, which amounts to conditioning on $F$ and $T$. In most circumstances, most researchers in bibliometrics (implicitly) assume that the field and year of publishing are unrelated to quality, and so doing this might make sense.

Indeed, consistent with the universality of citation distribution by Radicchi, Fortunato, and Castellano (2008), a common way to normalise citations is by dividing by the average number of citations in the same field in the same year (Waltman, 2016). This is an indicator that is sometimes called the normalised citation score (ncs)

$$\tilde{C} = \frac{C}{E(C)},$$

where $E(C)$ refers to the average number of citations in the same year and in the same field. What constitutes a field exactly is open for discussion (Waltman and van Eck, 2019). As a robust alternative, some analysts prefer to focus on whether a paper belongs to the top 10% of its field or not, and then simply consider what proportion of the publications belong to the top 10% of its field.

Sometimes, the normalisation also considers the document type $D$, which implicitly assumes that $D \rightarrow C$ but that $D \perp Q$, that is, the quality is independent of the document type. Of course, it could well be that $D \perp Q$ and that higher quality work is typically made available as a research article, instead of for example as an editorial or a letter to the editor. In that case, if we normalise citations by taking into account the document type, this pathway of quality $Q \rightarrow D \rightarrow C$ is blocked, and hence, this might actually deteriorate the accuracy of using normalised citations as an indicator for $Q$.

In research evaluation, the field-normalisation of indicators already alleviates some concerns that are sometimes raised about field differences and temporal dependencies. In other words, normalising citations makes $C$ arguably a more accurate indicator of $Q$, and, if $Q$ would be possible to observe, we could actually study to what extent the normalisation of citations makes $C$ a better predictor of $Q$. Directly comparing $C$ to $Q$ is not possible, because of the fundamental problem: research quality $Q$ is unobservable. Instead of comparing $C$ to $Q$,
scholars regularly compare $C$ to another indicator of $Q$, namely peer review $E$, as I discussed in section II C. Let us assume that $Q \rightarrow \ldots \rightarrow E$, which seems a reasonable starting point. By then comparing $C$ and $E$, we hope to learn something about whether $C$ is a accurate indicator of $Q$ or not. Again, the fundamental problem in research evaluation is that we cannot observe $Q$, and so any correlation between $C$ and $E$ does not necessarily establish that they are accurate indicators of $Q$. It merely establishes that $C$ and $E$ are correlated, but this can potentially also be caused by other causal factors. For example, consider that the journal $J$ influences both citations $C$ (for which we have some clear evidence) and possibly also influences $E$. We would then observe a correlation between $C$ and $E$, but this could well be due to the journal $J$ and have nothing to do with $Q$ directly (except that we might also assume that $Q \rightarrow J$).

Now this raises an interesting question: what role does the journal $J$ actually play in the citations $C$? If journal $J$ is independent of quality $Q$, then we should control for $J$, and also use it for normalisation, similar to our argument for why we should normalise citations for fields $F$ and year of publication $Y$. As I discussed in section II B and II C, controlling for the journal was often seen to deteriorate any correlation between metrics and peer review, or leave no correlation at all. We already established that $J \rightarrow C$, and the assumption that $Q \rightarrow E$ is quite reasonable. In this case then, there are two possibilities: (1) $Q \rightarrow J \rightarrow C$ so that conditioning on $J$ closes the backdoor path $E \leftarrow Q \rightarrow J \rightarrow C$, or (2) $J \rightarrow E$ so that $J$ closes the backdoor path $E \leftarrow J \rightarrow C$. There are some other more convoluted possibilities, but all have in common that conditioning on $J$ closes all backdoor paths. This may not come as a surprise to many: perhaps peer review is just influenced too much by where something is published. However, this would also imply that $Q \not\rightarrow C$, since otherwise the backdoor path $E \leftarrow Q \rightarrow C$ would not be closed by $J$. Perhaps surprisingly then, this implies that citations $C$ are not affected by quality $Q$ at all directly, it is just mediated through the journal $J$. In this case, we should not normalise citations for the journal $J$, because doing so would most likely make the normalised citations a less accurate indicator for quality $Q$, not a more accurate indicator for quality $Q$. In fact, based on this observation, the journal $J$ might be a more accurate indicator of $Q$ than the citations $C$, as suggested by Waltman and Traag (2020).

Now suppose that author prestige $A$ and departmental prestige $P$ affects acceptance, so that $A \rightarrow J$ and $P \rightarrow J$, for which there is some evidence, as we saw in section II A. If we assume that $A$ and $P$ are independent of $Q$ we might want to normalise for those effects, so that the normalised citations are not biased by $A$ or $P$, but a more accurate reflection of $Q$. However, perhaps author and departmental prestige are associated with quality $Q$ (and perhaps mutually reinforce each other). Possibly $A$ and $P$ could also affect evaluation $E$. However, they cannot directly affect citations $C$ as well, since then controlling for the journal $J$ would no longer block all backdoor paths.

All in all, we then have a couple of prestige feedback loops: author prestige, departmental prestige and journal prestige. Journals seem to be mediating most influence of quality to citations. These prestige cycles are not completely independent of actual underlying quality of the science. That is, author prestige, departmental prestige, and journal prestige presumably are related to quality, at least to some extent. But they do seem to obfuscate and confound much of the measurement of research quality by citations, such that citation-based indicators may perhaps better be seen as indicators of academic prestige than as scientific impact.

In a sense, these prestige feedback loops may be similar to what O’Neil (2016) referred to as pernicious cycles. By not considering effects of predictions when people act upon predictions, the predictions themselves become ill-informed, and may potentially have serious consequences. For example, if we use citations to predict departmental scientific performance, scientists may possibly leave certain departments or institutions because of this. On the face of it, citations may then seem to have some predictive value, but it is exactly because citations were used

Figure 1. Possible causal model of how citations $C$ can act as an indicator for research quality $Q$. Here the field $F$ and the year $Y$ are assumed to be independent of quality $Q$, so that normalising citations $C$ by field $F$ and year $Y$ improves the accuracy of the normalised indicator for quality $Q$. 

\[ \text{Quality} \quad \downarrow \quad \text{Review} \quad R \quad \text{Journal} \quad J \quad \downarrow \quad \text{Citations} \quad C \quad \downarrow \quad \text{Author prestige} \quad A \quad \downarrow \quad \text{Institutional prestige} \quad I \quad \downarrow \quad \text{Field} \quad F \quad \downarrow \quad \text{Year} \quad Y \]
to predict scientific performance that resulted in this behaviour. If we would have correctly considered the potential effect of citations on this behaviour, we would perhaps have concluded that on the contrary, citations do not have any predictive value. This again calls attention to clearer considerations of causality. Whenever an indicator, or a prediction, is used in practice, that is, we act on it, we enter causal territory. Even when an indicator initially might have been valid, through its very use, the consequences of its use may invalidate it. This is perhaps what could be called a causal understanding of constitutive effects. By combining citation models with proper causal reasoning and acknowledging the fundamental problem about unobservable research quality, we may hope to make some progress.

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