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The Epistemology of Media Effects, And Its Status as a Scientific Research Programme

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Abstract

This paper addresses epistemological issues in the area of media effects research. In the first section it is argued that the field hinges on conflicting notions of science and lacks a shared epistemic outlook. The second section tries to address this gap by conceptualizing media effects as a Lakatosian research programme. It is shown that Lakatos' framework is amenable to describe the history and practices of the field, and that it helps us evaluate the current state of the literature; in particular, it is concluded that effects research resembles a degenerative research programme. The third section of the paper takes this argument as a sign to reconsider how the field operates. Four epistemic commitments for effects scholars are put forward: (1) conduct reproducible and replicable research, (2) establish conceptual definitions, (3) make progressive use of the field's positive heuristic, and (4) rely on a severe testing account of statistical inference.

Keywords: media effects, epistemology, philosophy of science, Lakatos, research programme,

Popper, falsificationism

The Epistemology of Media Effects, And Its Status as a Scientific Research Programme

The scientific status of media effects research has long been a topic of controversy. Aside of the ever-recurring external critiques dismissing the purpose of effects studies (e.g., Gitlin, 1978; Gauntlett, 1998; Miller, 2009; Ruddock, 2001) the domain has also suffered from a fair share of internal disagreements. Some of the more divisive questions pertain to how should we define or conceptualize media effects (e.g., Potter, 2011), what characteristics allow us to differentiate between ‘good’ and ‘bad’ theories of media effects (e.g., Potter, 2014; Cacciatore, Scheufele, & Iyengar, 2015), what we are allowed to conclude about scientific progress in the domain (e.g., Lang, 2013), and what the evidence teaches us about the implications of media effects in everyday life (e.g., Ferguson, 2014). In essence, all these debates are rooted in a larger philosophical question about the field – namely, what is the *epistemological basis* of effects studies? That is, what kind of information may we count toward our knowledge about media effects, and what are the logical principles used to derive this information? The current paper will try to show that we have no consistent answer to these types of questions, which helps putting into perspective why there have been so many disputes about practice and purpose in the field.

The argument will proceed through three stages. First, we start off by briefly reviewing three epistemologies that have been referenced to characterize media effects studies: (logical) positivism, critical rationalism, and social constructivism. Discussing these philosophical traditions will, hopefully, serve to clarify that the effects literature references fundamentally conflicting notions of science and lacks a consistent epistemic outlook. The second section of the paper will propose to consistently conceptualize the field of media effects as a Lakatosian *research programme*. It will be shown that Lakatos’ perspective successfully combines ideas from the three epistemologies discussed before, that it provides a framework to meaningfully

describe the history and practices of media effects studies, and that it helps us determine how the field may want to proceed in the future. Finally, in the third section, the paper will translate the larger philosophical argument into four epistemic commitments for working scientists.

The Fractured Epistemology of Media Effects

The legacy of positivism

When scholars discuss the epistemology of media effects studies they often describe it quite briefly as a ‘positivist’ or ‘empiricist’ enterprise that is concerned with testing hypotheses based on observational methods and quantitative analyses (Miller, 2009; Perse & Lambe, 2017; Sanders, 2009; Walter, Cody, & Ball-Rokeach, 2018). While this characterization might seem to make intuitive sense, it actually reduces positivism to a label covering nearly all scientific studies and makes it “more of a term of abuse than a technical term in philosophy” (Giddens, 1978, p. 237).). The philosophical essence of positivism, and its applicability to media effects research, is better described if we trace back the origins of the term to two historical phases (Giddens, 1978). The earliest phase is strongly associated with the writings of August Comte, who proposed a sequential model for scientific progress. According to Comte, human knowledge goes through a law-like transition of three stages, starting at a theological stage where events in the world are explained in terms of supernatural entities, progressing through a metaphysical stage where those supernatural entities are replaced by theoretical entities within the world, and ending with a positive, or scientific, stage where all claims about events will be explained by referring to fundamental laws and empirical observations.

The second phase of positivism, commonly referred to as logical positivism, strongly held to Comte’s idea that observational proof should form the cornerstone of science. Logical positivism had its heydays between the 1920’s and 1950’s, and it is most strongly with the doctrine of scientists in the so-called Vienna Circle (or *Wiener Kreis*). A particular motive of these thinkers was to purify academic debate from the theoretical and esoteric language spread

by philosophers such as Heidegger, Hegel, and Husserl, who were, at the time, extremely influential in European academia (Friedman, 2000). For instance, the logical positivists took intellectual offense at Heidegger's famous metaphysical claim that "the nothing itself nothings" (Carnap, 1959, p. 69) because, analytically, it is a meaningless pseudo-sentence: it does not refer to anything in existence, so it possesses no truth-value (it is neither wrong nor right). For the logical positivists it was clear that science, which was supposed to look for truth, needed a criterion to prevent such meaningless uses of language. This is why they coined a so-called *verifiability criterion of meaning*: a sentence could be used in a scientific argument if its truth value was verifiable through observation or logical derivation (Carnap, 1959). This criterion had far-reaching implications, because it meant that science could only be concerned with theories that directly referenced observables, and scientific practice had to revolve around gathering observational evidence and calculating the degree of confirmation between the evidence and those theories (Carnap, 1962). This directly excluded most of philosophy from scientific debate, and it also forbade references to unobserved entities in scientific theories (unless they could be related to observation statements in a law-like manner).

The doctrine of logical positivism certainly left its mark on the history of the social sciences, as it was at the philosophical foundation of, for instance, radical behaviorism and the literature on operationalization (Moore, 1985). That said, it seems quite a stretch to argue that logical positivism played an immediate role in shaping the practices of media effects scholars (Bryant & Miron, 2004; Nabi & Oliver, 2010). Even though the early era of media effects studies temporally coincided with the logical positivist heydays, researchers never restricted themselves to only assessing observables - let alone that they were trying to develop formal systems of argumentation (as had been attempted in psychology by, for instance, by Hull, Hovland, Ross, Hall, Perkins, & Fitch, 1940). It can therefore be concluded that if media effects studies inherited anything from positivist epistemology, it will really only be the rather vague,

pseudo-positivist notion that studies need to look for observational evidence. There seems to be both a good and a bad side to this conclusion. The bad side is that such a watered down version of positivism provides no epistemic guidelines for doing media effects science: it gives us a sense of what type of game we should be playing (i.e., the game of empirical research), but it defines no rules for how this game should be played. The good side is that effects research has little to do with the actual doctrine of logical positivism, which was proven to be an inherently flawed enterprise after all. This was made especially clear by the critical rationalist and social constructivist schools, whose legacies also linger on in media effects scholarship.

The legacy of critical rationalism

Karl Popper's (1960, 1963) critical rationalism resides under the larger label of post-positivism and, as such, it has sometimes been mentioned in one breath with positivism to define effects studies (i.e., (post-)positivism: Walter et al., 2018). But the *post* in post-positivism is not just a redundant prefix. In fact, it signals a fundamental departure from the epistemological principles of (logical) positivist philosophy. Popper's (1960, 1963) attack on positivism was devastating, and it mainly revolved around the issues with the inductive logic on which positivists relied. The issue with induction was already acknowledged in the 18th century by David Hume: no number of observations n will ever suffice to rule out the existence of an observation $n+1$ showing the falsity of a universal conclusion at n . In other words, even if a scientist would manage to exhaust the population of current observations there is no guarantee that the future will give us the same pattern of observables and, therefore, lead to the same conclusion. Popper (1960, 1963) argued that Hume's critique had fundamental implications for scientific epistemology. Most importantly, it meant that observations could not, as the positivists had assumed, form the foundation of scientific knowledge. For Popper (1960, 1963), this status had to be attributed to theoretical speculation: while it is logically impossible to deduce a theoretical conclusion from a set of observations (which is what the

positivists tried to do), it is actually possible to deduce a very specific pattern of expected observations from the assumed truth of theoretical conjectures. The task of the scientist should therefore always be to start off with a theoretical idea and meticulously derive empirical hypotheses that may be compared to experimental observations (Hempel & Oppenheim, 1948).

Crucially, Popper (1960, 1963) realized that Hume's problem would also complicate the comparison of hypotheses with experimental evidence: regardless of how well empirical data seem to confirm theoretical predictions a scientist they can never be validly used to *prove* the underlying theory. So how could science then logically proceed? Popper (1960, 1963) argued that the rational task of a scientist is to search for data patterns that *disconfirm* or *falsify* their theories. This is because, in contrast to an empirical confirmation, an empirical falsification allows a certain conclusion about a theoretical conjecture through a logical syllogism known as *modus tollens*: formally, modus tollens refers to the fact that if we know the logical consequence of a conditional if-then-statement to be false, then the premise of that statement should have been false as well (if P, then Q. Not-Q, therefore not-P). For instance, if I have a theory about the weather predicting at least 12 raining days ("if the theory is true, it will rain at least 12 days"), then finding out that it remained dry over the whole month *necessarily implies* that there was something wrong with the theory in the first place. For Popper (1960, 1963), the fact that modus tollens provides logical certainty meant that it should be at the basis of the scientific search for truth: science should always consist of putting theoretical conjectures under critical scrutiny, look for counterevidence for those conjectures, and falsify them when needed.

But what should happen if scientists find evidence confirming their theory? Popper (1963) did not completely disregard these confirmatory instances, but he argued that they would only be meaningful if the scientists had initially started off with the right critical mindset and had tried to falsify their predictions. If this had been the case and they had failed to find falsifications – as Popper (1963) would say, if their predictions has been *corroborated* by

empirical findings - this allows for a reasonable hunch (though not a valid conclusion!) that the theory possesses at least some truth-value ('verisimilitude'). This hunch can be taken especially serious when the attempts to falsify the theory were very severe, and the empirical predictions were very risky to begin with. Risky predictions are those that seem highly unlikely *unless the theory is (at least partly) true*: if you live in Western Europe, predicting that it will rain at least once this year does not require any firm theoretical insight, but you surely need a strong theory to correctly predict that it will rain the day after tomorrow between 2.32 pm and 2.48 pm. A corroborator for a prediction in the former sense therefore possesses barely any evidential value for the validity of the underlying theory, but a corroborator for a risky theoretical prediction in the latter sense provides strong evidence for the theory's verisimilitude. To put it in other words, a strong corroborator can be defined as a finding that would be considered a *damn strange coincidence* if the underlying theory were *not* true (Salmon, 1984; see also Meehl, 1990).

Popper's (1960, 1963) notion of falsification has been called one of the most influential contributions to twentieth century philosophy of science (Parvin, 2010), so it hardly surprising to find media effects scholars citing it as a basic epistemological principle for their field (e.g., Arendt & Matthes, 2017; Berger, Roloff, & Roskos-Ewoldsen, 2010; Slater & Gleason, 2012; Krcmar, Ewoldsen, & Koerner, 2018; Sparks, 2015). However, it seems very difficult to actually find a media effects study – or any social-scientific study, for that matter (Holtz & Monnerjahn, 2017; Sayer, 1992) - where the purpose clearly was to prove the key hypothesis *wrong* rather than right. As such, it seems doubtful that the epistemology of media effects can really be traced back to the ideas of critical rationalism.

The main reason why Popper's (1960, 1963) philosophy is often cited but rarely implemented is that his principle of falsification is not actually amenable to scientific practice. Two problems, in particular, hamper its application. The first one is logical in nature and is known as the Quine-Duhem thesis (Quine, 1951): scientists never test a single hypothesis in

isolation but always need to rely on a host of background assumptions or hypotheses to link a theory to data (e.g., hypotheses about measurement instruments; assumptions about the research context, etc.). As a result, the rejection of an empirical hypothesis derived from a substantive theory can be caused by a problem in this chain of background assumptions and does not necessarily imply the falsehood of the substantive theory. This means that the use of *modus tollens* to refute a theory based on observational evidence is, strictly speaking, impossible.

A second counterargument against Popper's epistemology is historical: if scientists had ever actually held a falsificationist attitude they would have rejected many, if not all, theories that are usually considered successful. In fact, all great scientific "theories grow in a sea of anomalies" (Lakatos, 1999b, p. 99): already in the 1820's it was noted that Uranus did not follow the planetary orbit as predicted by classical mechanics, meaning that, in the Popperian sense, Newton's laws needed to be falsified and abandoned (Lakatos, 1999a). In other words, if critical rationalism had ever really defined scientific epistemology, scientists should have rejected theories such as Newton's which, in the history of the sciences, are usually considered to be most successful. The fact that the principles of critical rationalism failed to provide a reasonable reconstruction of scientific practice led many philosophers to turn away from trying to define the basic rational principle of science such as verification or falsification, and it lay the foundations of what came to be known as social constructivist epistemology. Social constructivism, in turn, has also been often referenced to characterize media effects research.

The legacy of social constructivism

Arguably one of the most influential publications in social constructivist epistemology is Thomas Kuhn's (1970) *The Structure of Scientific Revolutions*. In his book, Kuhn (1970) provided a historical analysis showing that science does not progress because it follows some universal standards of rationality such as verification or falsification. Rather, science should be understood as an inherently social and political activity: scientists are organized in specific

communities with their own internal sets of beliefs, language, customs, theories, methods, and assumptions. These so-called *paradigms* define the scientific problems worthy of investigation and also describe the guidelines for solving those problems. This means that the role of individual scientists is very limited indeed: a scientist simply solves puzzles as imposed by the paradigm, and if there are anomalies with commonly held theories or assumptions these are usually explained away because the paradigm has to continue its course. Only on rare occasions it will happen that someone discovers a factual or theoretical anomaly so convincing that it persuades a large part of the community to rethink their paradigmatic ideas. Such an event causes what Kuhn (1970) calls a *paradigmatic crisis*: the scientific community needs to engage in heavy debates about new theories, methods, and practices that could absorb the anomalies of the previous paradigm. If, after a while, the community manages to reach a consensus about a new paradigm the field will *shift* its paradigmatic basis, and the new paradigm will be maintained until a new crisis occurs. Thus, based on Kuhn's (1970) account of scientific history, what should be understood as change and progress in science is socially constructed rather than the result of individual rationality. In his words, "[a]s in political revolutions, so in paradigm choice – there is no standard higher than the assent of the relevant community", meaning that we do not just need "logic, but also the techniques of persuasive argumentation" (Kuhn, 1970, p. 94) to understand the state of a paradigm and to force its transition into a new one.

Kuhn's (1970) philosophy of science has often been referenced to characterize the field of media effects. The historical transitions in the field from 'powerful' to 'limited' and 'negotiated' effects have been characterized as paradigm shifts (Esser, 2008; McQuail, 2010), the notion of paradigmatic science has been applied to characterize media effects as the dominant paradigm within communication research (Jensen & Neuman, 2013; McQuail, 2010), and the paradigm concept has been referenced to evaluate the scientific status of media effects studies: Potter, Cooper, and Dupagne (1993) concluded that media effects research was

essentially pre-paradigmatic, Perloff (2013) called it a successful paradigmatic science, and Lang (2013) referred to it as a paradigm-in-crisis that needs to be replaced.

What seems striking about all these references to the paradigm concept – apart from them implying very different conclusions about the state of the field - is that they have ignored the more fundamental question that arises from describing media effects research in constructivist terms. That is, if effects researchers themselves describe progress in their field in terms of paradigmatic consensus rather than evidence per se, then what is left of the epistemological basis to justify their methodological decisions? Why would they even want to rely on their reductionist modelling schemes and relentless quantification if it is just the paradigm, and not logical verification or falsification, that happens to define progress? The fact that these questions have rarely been addressed might put into perspective why it has been so easy for postmodernist and constructivist scholars to criticize the philosophical value of the field (e.g., Gitlin, 1978; Gauntlett, 1998; Miller, 2009; Ruddock, 2001). Indeed, if we reconsider our philosophical overview, it seems clear that the field has never really had an epistemological basis: we have seen that (logical) positivism has been referenced to guide effects research, but this doctrine has been declared philosophically dead for over fifty years. Its assassin, critical rationalism, is also often referenced to justify the practices of effects scholars, but it was challenged by the Quine-Duhem problem as well as Kuhn's observation that science proceeds through social consensus. This might seem to relegate the enterprise of effects studies to an epistemic void, but the remainder of the paper will try to show that there could be a way out: Lakatos' (1978a, 1978b, 1978c) epistemology of scientific research programmes, and his principle of sophisticated methodological falsificationism.

Sophisticated Falsificationism and The Methodology of Scientific Research Programmes

Lakatos' philosophy of science does not appear to have received a lot of attention in the media effects literature. This is regrettable, because it was developed as a compromise between the very epistemologies that have been cited as the disciplinary basis of effects research (see Motterlini, 2002). As a starting point Lakatos (1978a, 1978b, 1978c) rejected the notion of science as an enterprise progressing on the basis of individual rationality. In this he sided with constructivists: science is an inherently social activity, with communities of researchers working according to the principles and theories established within their scientific paradigm or, as Lakatos (1978a) called it, their research programme. To fully grasp his argument we need to imagine this research programme as consisting of two distinct layers: a *hard core*, and a *protective belt*. The hard core consists of the theoretical chain making up the very essence of a scientific discipline – its laws, commonly shared facts, and key theoretical postulates – whereas the protective belt contains the constellation of auxiliary theories and assumptions relating the core to specific empirical predictions in a particular research context. Thus, the protective belt is Lakatos' (1978a, 1978c) metaphorical elaboration of the Quine-Duhem problem: an empirical prediction can only ever be derived from the *conjunction* of a hard core postulate and a set of auxiliaries in the protective belt, so a falsifier can only ever show that there is problems with either the core theoretical premise or the protective auxiliaries (see also Meehl, 1990).

Lakatos (1978a) argued that the Quine-Duhem problem could simply be dealt with by assuming two decision rules for working scientists. The first decision rule is called the *negative heuristic*, and it entails that researchers should consider their field's hard core as unfalsifiable 'by fiat': the core is what keeps the business of a research programme going, and abandoning a core principle based on empirical anomalies may well be an irrational decision if there are so many alternative reasons for falsification in a given research context. The negative heuristic immediately leads to Lakatos' (1978a) second decision rule, called the *positive heuristic*: if we cannot point the arrow of the modus tollens to the core we will always need to point it toward

our auxiliary theories in the protective belt. Thus, the positive heuristic hands scientists “a problem-solving, anomaly-digesting technique” (Motterlini, 1999, p. 106) that allows them to defend their field’s core principles, and this is what makes Lakatos’ (1978a, 1978b, 1978c) account such a tentative description of scientific practice: if we fail to find evidence for a hypothesis we never destroy our pet theories and go back to the drawing board, but we argue that the research situation was not adequately controlled, that statistical power was too low to detect the phenomenon, that we probably forgot to model an important moderator, or that we need to understand more of the underlying mechanisms to make better predictions. A falsificationist in the Popperian sense would have considered this kind of theory-saving as a signal of preposterous pseudo-science, but Lakatos (1978b) noted that a certain degree of stubbornness is necessary to stimulate scientific progress. As we noted before, if Newton’s gravitational laws had been abandoned because the orbit of Uranus was known to be inconsistent with Newton’s predictions the world would have been without classical mechanics for almost two hundred years. Luckily, this is not how a healthy science works: the anomalous orbit of Uranus was simply predicted to be caused by an unknown stellar body not taken into account in the initial calculations. Indeed, several decades later, Neptune was discovered, which turned the initial falsifying observation of Newton’s laws into a strong corroborator.

Importantly, the fact that scientific stubbornness can be fruitful does not hand researchers with a wildcard to explain away anomalies at will. For Lakatos (1978a, 1978c), it was paramount to maintain some rational standard to appraise scientific practice, and this is why he developed a more *sophisticated* version of Popper’s (1960, 1963) falsificationism. Lakatos’ (1978a, 1978c) sophisticated falsificationism mainly differs from Popper’s account in its unit of analysis: for Lakatos, a research programme should not be evaluated on the basis of falsifications of individual predictions per se. Rather, the criterion of evaluation should be the manner in which a research programme uses its positive heuristic to deal with predictive failures

and falsifications. Lakatos specifically defined two criteria to appraise the use of the positive heuristic. The first criterion is *theoretical progressiveness*: the protective belt should only be altered if the proposed changes are based on a preconceived theoretical idea (Lakatos, 1978c, p. 149) and if they contain *excess empirical content* – that is, if they entail new, risky, and falsifiable, empirical predictions (p. 167). The second criterion is *empirical progressiveness*, and it refers to the fact that these predictions have to be tested and convincingly corroborated by new studies. When both of these criteria are fulfilled, the changes to the protective belt can be considered valuable: there is an increase in theoretical knowledge about how the scientific phenomenon operates, so it is reasonable to conclude that the research programme is making progress.¹ If, however, the positive heuristic only manages to explain away empirical anomalies without making any new predictions, or if those predictions can only be rarely or feebly corroborated, the added auxiliary theory has no real value. If this happens time and time again without ever forcing a real breakthrough the research programme is said to be *degenerating*.

The question how to deal with a degenerating programme is not an easy one to answer: in principle, researchers will be able to rely on the positive heuristic indefinitely, but the whole purpose of Lakatos (1978a, 1978b, 1978c) was to maintain a rational basis to appraise a field's evolution. To him, it seemed obvious that long-lasting failures to make significant progress would require a debate about the viability of the research programme and, in extreme cases, a literal intervention of scientific law and order:

One may rationally stick to a degenerating programme until it is overtaken by a rival and even after. What one must not do is to deny its poor public record. It is perfectly rational to play a risky game: what is irrational is to deceive oneself about the risk. [...] Editors of scientific journals should refuse to publish [these] papers which will, in general, contain either solemn reassertions of their position or absorption of

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¹ Interestingly, this conclusion required Lakatos' philosophy to allow a "whiff of inductivism" (Lakatos, 1978c, p. 159: i.e., a reference to positivist verificationism) to work.

counterevidence (or even of rival programmes) by ad hoc, linguistic adjustments.

Research foundations, too, should refuse money. (Lakatos, 1978b, p. 117)

Media Effects as a Research Programme – What it Was. What it Is.

Lakatos' (1978a, 1978b, 1978c) epistemology may hand us a useful framework to describe the history and practices of the field of media effects. As a historical account, it embeds Neuman and Guggenheim's (2011) analysis of the literature on media effects as having proceeded through an accumulation of different theory sequences: in the very beginning of media effects studies there were only relatively crude, stimulus-response theories conjecturing direct effects of mediated messages on attitudes and behaviors. The theory cluster that developed later on still maintained this as their core hypothesis, but they also started taking into account "an increasingly sophisticated set of social, cultural, and structural conditions and cognitive mechanisms that help explain when mass-mediated messages do and do not affect the beliefs and opinions of audience members" (Neuman & Guggenheim, 2011, p. 171). Gradually, models of media effects started referring more and more to pre-existing personal motivations and opinions of media users, the interpersonal context, the cumulative nature of effects, the wide variety of processes and mental contents that are involved in a media effect, and the complicated continuous interactions between exposure and effect in modern-day media environments. As such, the history of effects research clearly resembles the natural trajectory of a Lakatosian research programme: effects studies always maintained the postulate of media effect at their core (McQuail, 2010; Perse & Lambe, 2017), and the separate theory clusters gradually accumulated theoretical auxiliaries to strengthen the protective belt.

Adopting a Lakatosian viewpoint on the history of media effects has the advantage that it prevents us from describing the field as the outcome of a Kuhnian sequence of radical shifts between 'powerful', 'limited', and 'negotiated' effects paradigms (Esser, 2008; McQuail, 2010). This reconstruction has been noted to be inherently problematic, as it sets illusory

endpoints to the uses of theoretical frameworks and does not account for the fact that many early perspectives on media effects research have maintained their influence throughout the years (Lang & Lang, 1981; Neuman & Guggenheim, 2011). Indeed, the assumption of discrete paradigms is an inherent problem with Kuhn's account of scientific history, and it is one of the reasons why Lakatos' notion of cumulative science is much more useful to describe scientific evolutions in general (Blaug, 1975).

The value of Lakatos' account is also apparent when we consider how the gradual historical evolutions of the protective belt have shaped current practices in the literature. To illustrate, consider the four key propositions of the oft-cited *Differential Susceptibility Model of Media Effects* (DSMM; Valkenburg & Peter, 2013):

Proposition 1: Media effects are conditional; they depend on three types of differential-susceptibility variables: dispositional, developmental, and social. (p. 226)

Proposition 2: Media effects are indirect; three media response states mediate the relationship between media use and media effects. (p. 227)

Proposition 3: The differential-susceptibility variables have two roles; they act as predictors of media use and as moderators of the effect of media use on media response states. (p. 231)

Proposition 4: Media effects are transactional; they not only influence media use, but also the media response states, and differential-susceptibility variables. (p. 235)

Clearly, these propositions reflect the theoretical structure of the media effects research programme: effects are assumed to exist (the core postulate), but if they are to be detected the right social- and individual-level circumstances and processes need to be modelled (the auxiliaries in the protective belt). Indeed, the modelling of moderators, mediators, and reciprocal effects has gradually become standard practice in the literature (Arendt & Matthes,

2017; Slater & Gleason, 2012), and when effects are not found researchers often refer back to auxiliaries that were not properly modelled (the positive heuristic) (e.g., Valkenburg, 2015).

The fact that protective auxiliaries have come to define research in the field has been cited as a good thing – as a sign of theoretical sophistication (Perloff, 2013). To a certain extent, this might also seem to make sense in light of Lakatos' notion of scientific progress: a protective belt exists to assure the growth of a scientific research programme, and by shaping a positive heuristic it brings about a "considerable continuity" (Lang & Lang, 1981, p. 662) that is to be expected of a mature science. However, it needs to be emphasized that the existence of a positive heuristic alone is not enough to identify a field as progressive. Recall that, for Lakatos, the criterion for evaluating a scientific research programme is not whether it has a positive heuristic, but how it uses its positive heuristic to generate theoretical and empirical progress. Unfortunately, when viewed against these standards, the effects literature does not appear to fare very well. To illustrate, reconsider the four DSMM propositions and try to answer the following question: given the generality of auxiliary hypotheses that have been put into place to protect the core postulate of media effects, what excess theoretical or empirical content can the research programme still generate? That is, what *novel* and *risky* empirical predictions can researchers still derive from the combination of the core effects postulate and its protective belt? There appear to be very few, if any at all.

The first problem is that the protective belt has come to entail *every* possible prediction that could ever be made. This is not to say that there are no new variables left to be considered (the size of the pool of potential variables that could be relevant for media effects depends on an arbitrary conceptual definition of those variables and is, therefore, potentially infinite), but the point is that the predictions that could be derived from these variables will always be exemplars of the existing protective belt and cannot really lead to anything new or surprising. As such, there is no excess content to be generated in the field of media effects. A second, and

related, problem with the protective belt is that it is capable of explaining *every* study outcome and, thus, hampers a critical test of theory: any pattern remotely suggesting evidence for an effect can be taken as a corroborator for a media effects hypothesis, and even the absence of evidence is to be expected based on the constellation of protective auxiliaries. This means that it is impossible to define or identify an empirical anomaly that, if it were to occur, would require a disciplinary reconsideration of the protective belt or a collective effort toward finding novel theories or hypotheses.

Together, these problems render the media effects research programme into a tautology: effects scholars can only ever investigate and conclude what they, as members of their research programme, already assume. This clearly violates Lakatos (1978a, 1978b, 1978c) criteria for a healthy, progressive science: if there is barely any room to come up with new, risky predictions, there can be no theoretical progress; and in the absence of such theoretical predictions there can be no strong empirical corroborators – that is, no empirical progress – either. As such, the field of media effects is characteristic of what Lakatos (1978a) calls a *degenerative* research programme: it revolves around a core hypothesis and relies upon a positive heuristic to ensure its own continuity, but this whole process does not deliver any cumulative insights. This conclusion actually resonates with many prior observations that the field of media effects has not made a lot of progress and is, for the most part, a-theoretical (Kamhawi & Weaver, 2003; Potter, 2018; Trumbo, 2004). Indeed, if we are allowed to simply sample from the protective belt at will there does not even appear to be a need to develop an integrated body of theory that could serve as background for the programme as a whole (Holbert & Tchernev, 2013).

The characterization of media effects as a degenerative research programme will inevitably sound controversial to readers. However, it is crucial not to interpret this conclusion as a plea to abandon the enterprise of effects studies. In fact, there appear to be three plausible strategies to deal with the problems facing the field. A first reaction would indeed prompt us to

abandon the practices, theories, and methods of effects studies and establish a new research programme that is theoretically and methodologically superior (e.g., Bennett & Iyengar, 2010; Bolls, Weber, Lang, and Potter, 2020). For instance, Bolls and colleagues (2020) propose to shift the focus from traditional effects studies to media research drawing on psychophysiology and neuroscience. In their view, research should not be concerned with static forms of media effects but investigate the constant neurological and physiological changes occurring whilst people interact with media. Definitely, such an exercise in developing a competitor programme could have a lot of potential in the long, but it should be noted that this competitor, too, will need to become theoretically and empirically progressive to serve as an alternative for traditional effects research. And as it currently stands, this does not appear to be the case.

A second plausible reaction would be to deny that media effects research is actually degenerating. If this would be the argument, however, then one would need to provide an epistemological justification showing that the field is actually doing well. And in developing such a justification, it will not be enough to state that theories and methods of media effects research have become ever-more advanced and complicated. After all, Occam's Razor is there to remind us that the complexity of a science cannot, by itself, reflect theoretical progress. Quite the contrary, the way in which media effects research has stacked up the auxiliaries in its protective belt (and its positive heuristic) seems to have been a main cause for its degeneration.

A third way to react is to think about a future of media effects research based upon the premises of sophisticated falsificationism, and this is the path the remainder of this paper will follow. The aim here will not at all be to settle the argument, but to invite a proper discussion about the foundations and procedures of media effects science. Or, to put it in Lakatos' words: "[w]hen a scientific school degenerates into pseudoscience it may be worthwhile to force a methodological debate [...] (just as when ordinary language degenerates into, say, journalese, it may be worthwhile to invoke the rules of grammar)". (Lakatos, 1978, p. 137b).

Media Effects as a Research Programme – What We Might Want it To Be.

A key implication of adopting the research programme conceptualization is that it requires us to think about how the discipline of media effects, as a whole, may progress. This presupposes a very different perspective on theory development and testing than the idiosyncratic approach that is so common in the field. That is, it does not suffice to simply choose a given topic of interest - say, sexualized media, violence, or politics – and then subjectively sample auxiliaries from the protective belt to generate any set of hypotheses from the infinite number of hypotheses that could have been generated. This approach, as we noted, is unable to falsify or corroborate any theoretical claim, so it cannot generate cumulative knowledge about media effects either. What the research programme may actually need is a collective effort to shake off its degenerative protective belt and a shared commitment to develop theory in a manner that allows for more risky tests. This epistemic commitment, as one could call it, may sketch a shared purpose for all scholars working on media effects and, thus, could also serve to address the oft-criticized lack of organizational power in the field (Craig, 1999; Holbert & Tchernev, 2013). However, accepting such a commitment will also require the field to establish and uphold some methodological and theoretical standards. In particular, it will become necessary to (1) establish a culture of reproducible and replicable research, (2) develop proper conceptual definitions, (3) make use of the positive heuristic *only* in a progressive manner, and (4) embed the practices of hypothesis testing within a more formalized, and falsificationist, account of statistical inference.

Commitment 1. Establish a culture of reproducible and replicable research

It has become increasingly common in the social sciences to stress the importance of establishing norms about openness and transparency in research. The 2020 ICA Conference, for example, described it as its main theme to “facilitate and deepen the conversation about Open Science in the field of communication” (ICAhdq.org, 2019). This concern with open

scientific practice is a reaction to accumulated findings that the scientific literature is riddled with false or, at least, unreplicable results (e.g., Ioannidis, 2005). One of the main reasons for this lack of replicability lies in researchers being led into Questionable Research Practices (QRP's) to make their findings more attractive for publication (Simmons, Nelson, & Simonsohn, 2011). Examples of such QRP's are HARKing (Hypothesizing After Results are Known), P-hacking, and the selective reporting of results (John, Loewenstein, & Prelec, 2012).

In the literature on communication and media effects, there is evidence of these QRP's in the form of unjustified sample sizes, unexplained removable of cases and items, and excess and misreported P-values just below the threshold of significance (Matthes, Marquart, Naderer, Arendt, Schmuck, & Adam, 2015; Vermeulen, Beukeboom, Batenburg, Avramiea, Stoyanov, van de Velde, & Oegema, 2015). It seems obvious that if we want the media effects research programme to make progress we need to weed out the phony corroborations caused by QRP's. If not, it is impossible to distinguish corroborated hypotheses from falsified ones, or to distinguish knowledge from nonsense. For this reason, the research programme will have to organize itself in a way that prevents QRP's as much as possible. This can be done by installing some rigorous standards of *methodological transparency* for the field, such as a tradition to publish direct replications of previous studies, share data, and pre-register hypotheses (Lewis, 2019). All of these standards ensure more risky tests of media effects: sharing data allows an independent reproduction of results, pre-registering hypotheses makes it more easy to assess the value of a corroboration, and finding corroborators in several independent samples would make a finding much more of a damn strange coincidence if the underlying theory had no verisimilitude (Salmon, 1984).

Commitment 2. Establish conceptual definitions of media effects

Perhaps an even more daunting issue with the media effects literature is the absence of proper conceptual definitions. As Potter (2011) noted, pinpointing the meaning of the concept

of media effect is an extremely difficult exercise: many different typologies of media effects exist, all of which use their own specific categories to delineate what types of media effects there are, and what they have in common. Of course, it can be argued that the difficulty of establishing a definition for the general concept of media effect is not much of an issue, because it merely serves as an organizing label for more specific types of media effects. Then again, the theoretical frameworks that have been developed to address specific types of effects do not appear to do a very good job at defining their concepts either. This becomes apparent when we consider the discussions surrounding the fuzzy meaning of, for instance, framing (Cacciatore et al., 2015), priming (Roskos-Ewoldsen, Roskos-Ewoldsen, & Carpentier, 2009), cultivation (Potter, 2014), and persuasion effects (O’Keefe, 2016). All of these concepts have had their definitions stretched and amended over the years and, as a result, have also come to overlap very heavily with one another (see also Coenen & Van den Bulck, 2018).

The fact that definitions of different types of media effects have come to converge may not be really surprising: effects scholars are all concerned with the same core hypothesis, and they can easily borrow from different frameworks to generate hypotheses. This conceptual convergence is actually promising, because it provides an opportunity for integrating theories from different subdomains into a coherent whole. However, it does not appear that the research programme has ever made a real effort to do so (Holbert & Tchernev, 2013). Rather than taking up the challenge of developing and testing more basic theories about media effects, researchers often just seem to be living quite happily on their own intellectual islands, using whatever idiosyncratic chain of variables (‘model’) seems reasonable in a given research situation (Potter, 2018). From our prior characterization of media effects as a degenerative research programme this is not actually surprising: the protective belt allows effects scholars to rely on an indefinite number of auxiliaries to guide their hypotheses, which means that basic theory is never really

at stake. And if basic theory is not much of an issue, establishing conceptual definitions will be, for the most part, out of the question.

The field has not been completely ignorant of its conceptual problems, but it appears that the solution is rarely traced back to the workings of the research programme. Usually, it is the concepts from specific theoretical frameworks which are put under scrutiny; if these are considered to be unclear or ambiguous, then the conclusion is that the field needs to adopt a purified definition (e.g. Cacciatore et al., 2015) or abandon the concepts altogether (Ewoldsen, 2017). However, given that the definitional problems are spread across the research programme abandoning or purifying specific concepts might not be enough. In fact, there might even be only one structural solution: to expect all researchers in the field to commit not only to *methodological transparency*, but also to *conceptual transparency*.

From the perspective of sophisticated falsificationism it is obvious why conceptual transparency is important: definitions clarify what a media effects study investigates and predicts and, thus, also what it does *not* investigate or predict. Making explicit what is *not* predicted may have advantages in two ways. First, it may specify that certain observations are ruled out by definition and, thus, serve as falsifiers of the investigated theory. Second, clarifying what is *not* meant with specific concepts provides a barrier preventing researchers from stretching the meaning of a phenomenon over the course of a study or a sequence of studies (which has, arguably, been a problem with many of the concepts in media effects research; e.g., Cacciatore et al., 2015; Potter, 2014). If we cannot find any evidence corroborating the phenomenon as defined, we need to accept the fact that there are only two options: (1) use our positive heuristic to try and develop auxiliaries to make better predictions in the future, or (2) focus on a different phenomenon, with a different definition, altogether.

Of course, the exact manner in which the conceptual transparency should be established or evaluated is not straightforward, and what constitute a good or bad definition will always be

open to subjective preference. This is why it may not be reasonable to expect every study to provide a perfectly closed definition of the investigated media effect and its associated concepts, nor should it be expected that every reader will agree on the labelling of those concepts or phenomena (Coenen & Van den Bulck, 2018). What is to be expected, however, is that a study makes explicit a definition in the first place, and that this definition is specific enough to help others in the research programme assess the viability of the proposed theory, predictions, and measurements. This means that studies will need to be evaluated on the basis of whether they provide an explicit and coherent answer to the following types of questions (see also Meehl, 1990; Potter, 2011): (1) What is the level of analysis of the investigated media effect (Coenen & Van den Bulck, 2018; Potter, 2011)? (2) What are the physical/psychological/social entities involved, how are they defined, and how do they relate to one another? (3) Is the media stimulus defined in terms of generalized contents, specific types of messages, or specific aspects of those messages? Is it about repeated or one-shot exposure? (4) What does all of this say about the functional form of the effect? Is it a linear function of exposure or does the effect follow a different curve? (5) And how do all these questions translate into a measurement theory? That is, how do we derive our measurements from our substantive definitions? This last question requires making explicit methodological auxiliaries (i.e., substantive theories of measurement validity: Borsboom, Mellenbergh, & van Heerden, 2003) linking the investigated phenomenon to measurements. While crucial, making explicit methodological auxiliaries is rarely done in the social sciences, and this severely hampers measurement validity (Flake & Fried, 2019).

Commitment 3. Make use of the positive heuristic in a progressive manner

As we have argued throughout the paper, a prime reason for the degeneration of media effects research is the manner in which research programme has built on vague, and overly general, auxiliaries in its protective belt. If we want media effects research to progress it may therefore be necessary to cut loose the protective belt and try building it up anew, based on

more specific theoretical predictions and empirical corroborations. This may not sound very appealing at first, because it means that we need to strip away the apparent ‘sophistication’ of the field and are left with a ‘simplistic’ effects postulate that does not inherently embrace the complexity of everyday life. However, it should be stressed that the argument for stripping the protective belt in the *scientific investigation* of media effects does not imply a denial of the complexities of media effects in *social reality*. Few communication scientists would argue against the intrinsic interplay between message, source, recipient, channel and context in communication (McGuire, 1989), but the question at stake is how those factors can be embedded within theories that allow for a proper scientific test.

The fundamental starting point here will be to assume that all scientific models of media effects will be literally wrong. However, some of them (those making strongly corroborated predictions) may possess a reasonable degree of verisimilitude, and it is those models we want to formulate, nurture, and evolve. In order to do this, we need to start off with relatively crude versions of a theory, observe that its predictions fail, and rely on our positive heuristic to build auxiliary theories that render better predictions. The auxiliaries may take on numerous forms (Meehl, 1990), but often they will be either methodological or theoretical in nature. Methodological auxiliaries provide the explicit link between entities in a media effects theory and their measurements (e.g. theories of validity, as alluded to above). Theoretical auxiliaries address and change peripheral aspects of a theory to add to its prediction, such as the moderators (conditions) or mediators (processes) typically investigated in media effects studies. In any case, it will be important not to accept auxiliaries as given (‘social reality is complicated’), but to consider them as hypotheses in their own right that require critical evaluation.

Assessing the value of an auxiliary will require us to keep in mind the goal of quantitative social sciences in general – that is, to establish so-called *hedged regularities* (Roberts, 2004). Hedged regularities are regular patterns in social reality, qualified by our

knowledge that such regularities will have their exceptions. This also defines the role of auxiliaries in media effects research: if we add a statement about a given auxiliary to the protective belt of media effects, we are actually trying to pinpoint the exceptions to the proposed effect. But if we keep in mind that our goal is to establish hedged regularities, exceptions to the investigated effect should, themselves, be hedged regularities. In other words, if we propose that a media effect (a hedged regularity) will be qualified by particular circumstances or processes, then the theory about this qualification should also be relatively regular. If not, it cannot be used to make a generalizable prediction that is amenable to a risky test.

The goal of establishing hedged regularities is important in light of the widespread practice of investigating moderation and mediation hypotheses in media effects research. In particular, it means that whenever a study proposes a mediation or moderation effect, then the hypotheses derived from it should imply a regular hedged regularity. If the regularity is very idiosyncratic (that is, if it is only relevant for a very specific study or research question) the moderator or mediator may be useful to help out the argument of a given study, but it will not help the research programme to progress. It is much better if a study determines generalizable regularities – that is, mechanisms and conditions adding to the prediction of media effects across different research contexts (topics, samples, variables,...). If these are found they provide a progressive change to the auxiliaries and can be added to the protective belt of the research programme. Note, however, that, in any case, auxiliaries will not be accepted into the protective belt unless they have withstood a risky test. Presently, the absence of risk is one of the main problems with moderation hypotheses in communication studies: researchers rarely clarify how moderators should *specifically* influence the size and direction of an effect, which means that various kinds of patterns can all be considered as corroborators of a moderation hypothesis (Holbert & Park, 2018). As such, there is typically little risk involved in moderation hypotheses, and corroborators will rarely entail a damn strange coincidence (Salmon, 1984).

Commitment 4: Use statistical inference as risky testing

If we accept that the goal of effects scholars will be to establish a risky test of their hypotheses it is obvious that statistical testing procedures will also play a crucial role. However, the null hypothesis significance testing tradition that is so common in the social sciences is not well equipped to actually provide risky tests. Even if we needed not be worried about multiple testing or Type-I-errors, the simple refutation of a null hypothesis does not provide a lot of evidence for a substantive effect – especially in the case of highly powered studies. For this reason it might be necessary to also rethink our uses of statistical methodology. In recent years, several approaches to statistical inference have been developed that might be of value in this regard. Within the frequentist school, Mayo (2018) put forward a so-called severe testing account of statistical inference. The underlying idea is relatively straightforward: posit an expected effect size, derive the sampling distribution of the test statistic assuming this effect size, and assess where the observed test statistic in a study falls within this sampling distribution. This logic is nearly identical to the practice of null hypothesis significance testing, with the exception that the sampling distribution used for the inference is not the distribution under the null, but under a pre-defined effects size. The value of this approach is that it assesses how surprising a given finding would be assuming that particular effect sizes is true, and, as such, it actually allows for a falsification or corroboration of a statistical hypothesis (non-null) derived from theory. The Bayesian school also offers a falsificationist account of statistical inference (Gelman & Shalizi, 2012). Here, the goal of a statistical analysis is to put very stringent assumptions on a theoretical model (e.g., a prior distribution) and assess to what extent the model *fails* to meet these assumptions. If the model posits stringent assumptions but does not fail, the data can be said to serve as a strong corroborator for the underlying hypothesis.

Concluding note: embracing slow progress does not preclude social relevance

Even if readers would be willing to agree with the basic problem statements put forward in this paper, the actual recommendations developed in the last section might still seem relatively unattractive. After all, if we are to be concerned with first establishing a proper conceptual definition of media effects phenomena, link this to the definition of the variables involved, come up with a measurement theory, do proper tests, and wait for replications, this will heavily slow down the process to make knowledge claims. However, the fact that this approach will take a long time is not much of a counterargument against its necessity. Proper science, after all, is slow and cumulative. We cannot expect things to occur overnight.

Granted, the longevity of scientific progress could be especially problematic in an area such as media effects: questions in the field have direct social relevance, and the very existence of the field can be traced back to recurring political and social concerns about technological innovations (Orben, 2019). As such, taking up epistemic commitments may seem to run counter to the field's *raison d'être*. However, the attempt to establish a proper epistemological and theoretical basis for the field does not make one impotent to address pressing social questions. It will simply be important to identify what a study tries to achieve – that is, whether it tries to develop and test substantive theory, or whether it tries to establish what empirical observations happen to say about a particular social problem. To make this distinction more formal, it seems valuable to identify a study as belonging to what philosophers have called the context of discovery, or the context of justification (Reichenbach, 1938). Responding to social questions can easily be done from within the context of discovery, as long as the conclusions remain limited to 'what the data suggest'. However, if we want the research programme to make theoretical progress, and, eventually, arrive at strong knowledge claims, we will need to make work of designing the context of justification. This, indeed, is where the research programme conceptualization, and the epistemic commitments, really come into play.

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