

Six Positive Words for Talking about Theory in Games Research (in Pursuit of Interdisciplinarity)

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ABSTRACT

We propose six words for talking about what a given theory or idea in games research is or does. Talking about the strengths and specialisations of each other's ideas is more likely to foster interdisciplinarity than enforcing ontological accord across our field's expanding landscape. First, we [1] review recent discussions about interdisciplinary games research and say why their recommendations don't seem optimally pluralistic. We zero in on what we take to be a promising compromise for most games scholars: Embracing middle-range theory. To bolster our rationale, we [2] discuss theoretical debates in two other young fields (film studies and organisational research) and show how they arrived at—and have been well served by—middle-range theory. Centrally, we then [3] argue for the value of six meta-synthetically derived dimensions of theory: Importance, interestingness, actionability, generality, simplicity, and accuracy. We review these six dimensions and demonstrate the framework in use. Weaknesses of our proposal are flagged prior to concluding.

Keywords

Theory, metatheory, ontology, disciplinarity, multidisciplinarity, interdisciplinarity

INTRODUCTION

Games research is massively multiplayer. It must thus aim to be profoundly pluralistic. Game *studies* is nowadays considered a subset of games *research* (Deterding 2017). Games *research*, in turn, continues to grow (Mäyrä 2008; Martin 2018; Malazita et al. 2024), abutting and establishing enclaves in media and communication research and its many subfields (e.g., platform and fan studies) as well as in education, healthcare, human–computer interaction, gambling studies, war studies, and more. Our shared object and domain of interest, “the Ludosphere” (Stenros and Kultima 2018), expands (Siitonen et al. 2021) in tandem with digital cultures and market segments, attracting academic “newcomers” all the while (deWinter and Smith 2025, 111).

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Games research (GR) and game studies (GS) are a field and a subfield respectively. Neither GR nor GS is a discipline in the traditional sense, since neither is monocultural. Neither embodies a unified paradigm or set of methods (Ouellette and Conway 2020). Both are *multidisciplinary* (typified by parallel but unintegrated efforts) and both show ambitions of *interdisciplinarity*. Pockets of interdisciplinary “proper” are evident in both (Eklund et al. 2024), but GR is not synergistic in the same way that, say, cognitive science is¹ (Deterding 2017; M. Smith 2017; Aarseth and Grabarczyk 2018).

Interdisciplinarity seems advantageous strategically, vocationally, and epistemically. Whether one thinks interdisciplinarity is preferable to multidisciplinary or merely an alternative to it, if GR is to weather the winds of change in the coming decades (from the erosion of funding landscapes or financial bubbles bursting, for instance), we’d be wise to at least *be prepared* to form a united front (Eklund et al. 2024; Siitonen et al. 2021; Malazita et al. 2024). Economic perils aside, knowledge generation profits from those who engage in it having a strong sense of what everyone else does, and being able to articulate and understand each other’s perspectives (Gregersen 2016; Stenros and Kultima 2018). How to promote erudite pluralism in a way that leverages GR’s differing intellectual commitments rather than framing them as an obstacle?

We present our two cents. Following Deterding’s (2017) call for GR to produce more “middle-range theory” (Merton 1968), we argue that adopting six meta-synthetically derived dimensions (Carton 2025) for discussing theory (or simply *ideas*) may expedite our collective efforts at breaking through silo walls and promoting interdisciplinarity at practically no cost to anyone’s intellectual preferences or commitments.

We all either use or produce theory. Or, more broadly, conceptual–theoretical tools. Theory is malleable and polymorphous. It means different things to different people. It can be hard to define yet seems readily identifiable. A note on terminology, then. Unless stated otherwise, “theory” is hence used to capture the breadth of concepts, lenses, typologies, taxonomies, frameworks, models, axioms, and idiographic as well as nomothetic explanations that are the currency of our craft. The phrase “*middle-range theory*” should be read differently. Middle-range theory, as we’ll elaborate, is neither too niche nor too sweeping, and usually entails explicating causal relationships between well-defined phenomena or constructs.

Our basic belief is that establishing a lingua franca to talk about theory (so construed) is a concrete step that can be taken to scaffold the integration of our diverse research paradigms’ individual strengths. We hold that this can be achieved without refocusing our primary object of study from games to play (cf. Gekker 2021); without lumping humanist–interpretivist and rationalist–post-positivist approaches together under the banner of “game science” (cf. Klabbers 2018); without stopping the press, so to speak, to sort out our ontologies (cf. Gregersen 2016; Aarseth and Grabarczyk 2018); without promoting *or* foreclosing the possibility of anti-, non-, or postdisciplinarity as remedies to the neoliberal side effects of interdisciplinarity (Malazita et al. 2024; Taylor 2024);

¹ Since its emergence in the 1950s, cognitive science has been an inherently interdisciplinary enterprise, enjoying input from philosophers, linguists, anthropologists, psychologists, AI engineers, neuroscientists, and cyberneticists. In part because it remains unclear what cognitive science’s object of study (the mind) quite *is*, exactly, such diverse disciplines can arguably make contrasting and complimentary contributions on equal footing that amount to something other than the sum of their parts.

and without prescribing that GR or GS espouse an exclusive design focus² (Deterding 2017; Martin 2018).

Our aim is to convince you that simply nurturing a shared vocabulary for talking about what it is that a given theory *does* (without judgment, to the extent that it's possible) is roundly beneficial. It won't end disagreement or misunderstanding—far from it!—but it may help close some gaps.

The six dimensions along which theory varies in scope are derived via meta-synthesis. Carton (2025) qualitatively consolidates 284 accounts of what “strong” theory is, does, or should do according to decades-long debates in sociology and organisational studies. *Importance, interestingness, actionability, generality, simplicity, and accuracy* are the overarching themes. The advantage of Carton's sexpartite framework is this: A theory that's low in accuracy (for instance) is likely to be high in some other quality or virtue, for relatively few ideas are good for nothing at all. Far from being a yardstick with which to spank epistemic dissidents or progressives (Malazita et al. 2024), then, the six words let us talk about what a given theory or idea does *regardless* of whether it comes from rationalist or empiricist traditions; from critical cultural studies or the natural sciences; from queer, feminist, and decolonial schools of thought, or from ostensibly apolitical and objective methods and perspectives. Employing such positive language may foster amicability between and among intellectual camps historically divided along epistemic and ideological lines.

The paper proceeds thus. We first unpack some of the forecited proposals as to how collaboration can be approached in GR or GS. This sets the stage by sketching stances to which we're floating an alternative. The last example, Deterding's (2017) call for GR to prioritise middle-range theory, serves as a segue into the second section, in which we recount how two other young fields (film studies and organisational research) each independently arrived at—and seem to have been fairly well served by—middle-range theory as a means to interdisciplinarity. We're then in a position to detail Carton's six dimensions (or desiderata, or simply *features*) of theory. We demonstrate the terms' applicability and utility by way of worked examples, emphasising how such a “pidgin”³ is not an exercise in linguistic–epistemic hegemony but a likely optimal compromise between supposedly opposing epistemic traditions. We then consider several possible objections to our proposal, flagging its weaknesses and limitations before concluding.

SYNERGY HOW?

Let's consider how a few publications from the past decade or so have approached GR and GS as multi- or interdisciplinary fields and subfields, or something like them.

Gekker (2021) suggests that GS would do well to jettison its “dualistic” ontologies and debates, even if they're considered field-defining by certain histor(iograph)ies. This is difficult to deny and is consistent with the frustrations of several others. GS abounds

² We agree—and will note—how games research(ers) adopting “a design orientation” (Deterding 2017, *passim*) is generally a smart move. Since design is both a discipline and a property of games, it can act as “an important bridge between different communities” (Martin 2018, abstract). It's just not for everyone!

³ We use the term not to connote a “corrupted” language, but a simplified form of language developed by the members two or more groups that would otherwise lack a common tongue.

in references to its foundational “dull binaries” (Apperley 2019): Games versus players (cf. Juul 2008; Consalvo 2009), ludology versus narratology (Vossen 2025), and so on.

Gekker argues that a “post-humanities” approach would serve GS. This comes close to what we’ll suggest under a different label. He writes of a kind of “hybrid thinking” that stresses “intermingling between various forms of humanities and social sciences analyses” (Gekker 2021, 76). As we’ll elaborate, this can be painted as “middle-range theory” (Merton 1968; Deterding 2017), or “theory building” (M. Smith 2017, *passim*).

Gekker’s recommendation is to focus on play as much as (if not more than) games; an idea he credits to a DiGRA panel (Mäyrä 2015). This part is less likely to appeal to all possible GR scholars—particularly “visitors” interested in, say, the platform-economic aspects of game-like products and services. Is there not a risk that a notional play *and* games research agenda spawns a play *versus* games research schism, thus creating yet another binary that undercuts the possibility of a post-dualistic GS?

A second proposal comes from Klabbers (2018), who spells out his vision of a unified “game science”. Without getting into the minutiae, if this pitch is to be well received by the “disciplinary gravitational core” of GS (Eklund et al. 2024, vii) within the wider, multidisciplinary field of GR, then there may be a bit of a branding issue. GS, after all, is but “*one narrow cultural studies multidiscipline within the growing and diversifying field of game research*” (Deterding 2017, 1 – italics original).

Hence, one objection to Klabbers’ plan for a comprehensive “game science” is that it hinges on a moniker (and tacitly, an epistemic tendency) that some invitees may find unpalatable. “Science” is practically a dirty word in certain hardcore humanist circles. Rodowick (2007), for example, in his criticism of film studies’ turn towards cognitivism, so-called, laments that “the contestation of theory [by scientists and analytic thinkers] becomes a de facto epistemological dismissal of the humanities” (Rodowick 2007, 98; see also Nannicelli and Taberham 2014). This seems fatalistic. We hope to advance an elastic characterisation of “theory” and a way of talking about its virtues that sidesteps this charge, leaving plenty of space for everyone at the table.

A third trend in the conversation around how to mature mere multidisciplinary into interdisciplinarity proper entails ontological stocktaking (Gregersen 2016; Aarseth and Grabarczyk 2018). Here, the ontology in question is *disciplinary* ontology—not the ontology of games per se, though the two are obviously entangled. Gregersen (2016) suggests that representatives of GS’ (or GR’s) participating disciplines first provide answers to the twofold question, “what are games and why study them?” This would give a sense of “concepts and relationships ... acknowledged as real and relevant” by intellectual stakeholders (Gregersen 2016, 2; Merton 1945). Participating disciplines’ responses would be plotted or charted in a matrix to reveal points of both inter- and intradisciplinary misalignment.

Since “ontological inventory is the real backbone of a discipline and its practitioners”, Gregersen argues that “the sub-disciplines of game[s research] ... need to prioritise enquiry into and succinct communication of disciplinary ontologies in order to foster critical and constructive debate in the field as a whole”, so that we may all “explicate and justify these commitments to each other” (Gregersen 2016, 2). Signalling our assumptions and ideals is vital, he holds, “for looking further into our agreements and disagreements, parallels, [and] continuities” (Gregersen 2016, 2).

Aarseth and Grabarczyk (2018) also see value in ontological reflection and declaration. Similarly recognising GR's shifting boundaries and ever-evolving objects, they suggest that "a meta-ontological perspective is needed ... to identify, connect and coordinate ... perspectives": They, too, hold that "[i]t is ... useful for every discipline to have ... a bird's eye view of all possible perspectives, and ... the ways they relate to each other" (Aarseth and Grabarczyk 2018, 4).

We agree in large part. Reflexive awareness is important, and being well equipped to spar with others helps till the soil of which our expanding field is in in constant need. Yet who speaks for a discipline? Who stipulates which disciplines constitute a field?

It's probable that having one's ontological ducks in a row (and being willing and able to declare them) is ultimately productive. But we do not believe that putting ontology above or before all else is optimal or even possible. Gregersen writes that "ontological declaration is ... the necessary cost of entry into a healthy multi- and inter-disciplinary academic community" (2016, 2). Taking this cost of entry/club metaphor further, we ask; is it not better to *give* someone something at the door (a glossary, for instance) than to demand something from them? If GR is a club, then who checks the tickets? And does there need to be a bouncer?

Malazita, Rouse, and Smith (2024) address interdisciplinarity's propensity to embody neoliberal and even "bourgeois" atmospheres of institutional oppression (Malazita et al. 2024, ¶121). They consider anti- and non-disciplinarity from different angles, noting both the emancipatory and insidious potentials of varying construals of both terms. On their reading, rhetorics of anti-disciplinarity are often in service of technocratic "disruption"; the ruinous logic of "move fast and break things" championed by a toxic masculine oligarchy. Taylor similarly warns of interdisciplinarity's commensurability with "neoliberal logics" (2024, 228) of new public management.

It would take a dedicated discussion paper to do justice to the sophisticated views on power and agency elaborated in these articles. At risk of giving their authors short shrift, we suggest that while anti-, non-, and postdisciplinarity stay part of the "perennial topic of debate" (Taylor 2024, 231) that is the merits, mutual obligations, and dangers of interdisciplinarity, such alternative concepts remain too fuzzy to form the basis of an optimally inclusive, pluralist mode of collaboration. By Taylor's own admission, certain "postures" on postdisciplinarity are "necessarily idiosyncratic and personal" (2024, 227), and like non- and anti-disciplinarity, appeal mainly to scholars who are "nomadic" (2024, 225), leaving disciplinary scholars less well accommodated.

Lastly, Deterding's (2017) diagnosis is done through the lens of historiography. In his "Pyrrhic Victory" article, the Cadmean conquest in question is GS' push to legitimise games research in the first two decades of the 21st century. GS scholars, he believes, "have sown the seeds of their own contraction into a narrow multidisciplinary field by establishing the relevance of game research across disciplines" (Deterding 2017, 2). GS' quest for self-definition, he holds, has beneficially borne GR yet turned many disciplinary scholars back to their home turfs.

His advice? Not the privileging of play over games, not the establishment of a unified "game science", not ontological introspection or adherence to a meta-model, and not the substitution of the "inter-" in interdisciplinarity with some other prefix, but rather increased attention to game design and more efforts at building *middle-range theory*.

MIDDLE-RANGE THEORY

Middle-range theory (or *theories*—we’ll use the singular collective noun) is that which aims for the “Goldilocks spot” between too-narrow problem spaces on the one hand, and nebulous, insoluble phenomena on the other. There are other criteria that typify middle-range theory such as empirical substantiation and strong causal reasoning but let’s first briefly consider what kinds of objects of study lend well to the middle range.

Deterding advises that “a design orientation” (2017, *passim*) dovetails nicely with middle-range efforts. Martin (2018) concurs, as do we. “Design is an important bridge between different communities” (Martin 2018, abstract)—perhaps because it is both a discipline and a feature of games. Middle-range theory can be developed in relation to design patterns in isolation, or in relation to player habits and/or media effects. An example of the former could be a historical analysis of how the decline in popularity of coin-op arcade machines drove decreased difficulty in home console games and a corresponding increase in “the assistance paradigm” (Therrien 2011). An example of the latter could be *extending* such research to probe player attitudes towards quality-of-life features (e.g. quest markers on minimaps) and whether player mollycoddling correlates with different demographics’ sense of entitlement to demand things from developers on social media.

The problem is, not everybody *wants* to focus on the “middle-range” of game design, and we need to be as intellectually inclusive as possible. Scholarship that takes as its object not discrete and quantifiable phenomena but macro-level trends is sometimes called “grand theory” (Mills 1959). Grand theory can proceed from unsubstantiated⁴ observations accepted by many as premised on general truths. Consider the opening line of the book in which the term was coined: “Nowadays[,] men often feel that their private lives are a series of traps” (Mills 1959, 3).

Problematic gendering aside, here we see several features of “strong” theory. The phrasing hints that the theory could be *interesting* (Davis 1971), *defamiliarizing* (DiMaggio 1995, 391), and *generative* (Healy 2017; Carton 2025) insofar as it could spawn new questions. This is despite that grand theory embraces its *abstract remit*: It is an approach to theorising that often relies on metaphor or analogical reasoning. Its currency is concepts—their “associating and dissociating” (Mills 1959, 26)—not so much the clockwork of causal logic or the evidential heft of replicable interventions. Hence, as we’ll soon see, grand theory is in tension (though not hopelessly so) with hypothetico-deductive approaches.

Before we detail the six terms that we propose can be used to discuss theory or ideas regardless of their origins—importance, interestingness, actionability, generality, simplicity, and accuracy—let’s explore how two other young fields have navigated the middle-range/grand theory incompatibility issue.

⁴ We mean this literally—not in a backhanded way. Marx’s theory of alienation or Gramsci’s theory of hegemony are examples of grand theory that proceed not from hard data or by way of falsification but via sweeping observations about states of affairs. Detractors can deny their premises, but adherents of grand theory/ies are often won over by their face validity and generative potential for informing praxis.

THEORY IN OTHER YOUNG FIELDS

GR is not the first young field to exhibit schisms in the study of contemporary objects. Film studies, which mainly (though not solely) examines artefacts, and organisational research, which mainly (but again, not exclusively) studies collective human activity are instructive cases. Their theoretical teething troubles and paths forward may offer prognoses for GR's own conundrums. We describe how key figures in both fields have construed theory, which further evidences why middle-range theory-building may be an acceptable halfway house.

Film Studies

For decades after cinema's emergence, film scholarship was defined by the import and application of lenses from preexisting disciplines. Saussurean semiotic, Lacanian psychoanalytic, Althusserian neo-Marxist, and Barthesian textual analyses abounded. It wasn't until the mid-1990s that Bordwell, Carroll, and their co-authors challenged the status quo in the controversial compendium *Post-Theory* (1996).

The influential volume's rhetoric was tendentious if not inflammatory. Bordwell, for instance, used the acronym "SLAB" to deride the forecited thinkers. This was received by some as a blanket dismissal of their cherished intellectual lineages. But the critique was coming from a place of genuine epistemic concern: Bordwell argued that in film studies as in literary studies, both "theory" and "method" had been synonymous with "interpretative school", and that the frameworks championed by such schools were dogmatic. The book's blurb spells out its modest provocation: "Why not employ many theories tailored to specific goals, rather than searching for a unified theory that will explain all sorts of films, their production, and ... reception?"—a call for the shrinking of grand theory and the proliferation of middle-range theory in all but name.

Hence the pushback was not simply against interpretivism, poststructuralism (a label that most adherents disavowed anyway), or continental philosophy. Bordwell et al. felt that certain "semantic fields" of sanctioned concepts had been unduly treated as *a priori* apt in and to the analysis of film despite their origins and lack of evaluability. "Desire", "sadism", "voyeurism" (etc.) held outsized influence, as if natural categories. *Post-Theory's* charge was that off-the-shelf interpretative frames, while potentially valuable, shouldn't be confused with grounded concepts that arise from the empirical object with as few baked-in or tacked-on assumptions as possible⁵. Claims about how films influence individuals, culture, and society should ideally be interrogable with recourse to more than intuition and abstract, esoteric systems like "the Symbolic"⁶.

The issue, then, was not with the epistemic commitments of grand theory, but with its oversights. It's not that unfalsifiable claims have no place in knowledge production. (Evolutionary biologists tell "just-so" stories about as often as cultural theorists do.)

⁵ It'd be remiss not to note that this epistemic conflict is echoed in Aarseth's criticism of literary theory's purported colonisation of game studies in the 1990s and 2000s (cf. Aarseth 2004; Vossen 2025).

⁶ Would Lacan have agreed? On the one hand, he might find reason to employ his ideas in film analysis. On the other hand, he states in a 1957 interview that "[p]sychoanalysis is a terribly efficient instrument, and because it is more and more a prestigious instrument, *we run the risk of using it with a purpose for which it was not made for, and in this way[,] we may degrade it*" (L'Express 1957 – italics added).

This issue was (and still is) that if interdisciplinary progress is to be made, we'd be wise to align on what good theory *is* and *does*, even if we can't agree on how it's best made.

Good or strong theory, according to Bordwell, should possess “internal coherence, empirical breadth, discriminating power[,] ... recognition of historical change” (Bordwell 1985, xiii), and, speaking about theories that concern artworks or artefacts, “categories and propositions pertaining to the artwork's structure, the perceiver's relation to the work, and the broader functions of the work” (Bordwell 1985, xii). This may not be exhaustive but is surely uncontroversial. One need only imagine the opposite of each criterion to quickly characterise something that's roundly unhelpful.

In pursuing these things, Bordwell advocates disciplinary openness if not promiscuity: “I do not accept any hard-and-fast distinctions among theory, criticism, and history ... [For some questions] have no regard for disciplinary boundaries” (Bordwell 1985, xi). The sentiment is galvanised years later: *Post-Theory* describes the act of theorising as “a commitment to using the best canons of inference and evidence available ... The standards ought to be those of the most stringent philosophical reasoning, historical argument, and sociological, economic, and critical analysis we can find, in film studies or elsewhere (even in science)” (Bordwell and Carroll 1996, xiv).

Bordwell et al.'s embrace of fallibilism (the mundane idea that theories can be *wrong*), critical rationalism, or post-positivism fuelled accusations of scientism. That is, over-deferentiality to the scientific method. The editors of a volume titled *Cognitive Media Theory*⁷ (2014) are at pains to point out that prizing middle-range theory does not require subscribing to the scientific method alone. They characterise a dedication to *dialectical* theorising. One of the most productive ways to argue for an idea, they hold, is to demonstrate that it does a better job of addressing problems—or that it animates more interesting questions—than its competitors. “Thus,” they continue, “theorizing usually includes some element of criticism of existing views, both in order to motivate a different perspective and highlight its advantages over ... existing views” (Nannicelli and Taberham 2014, 7–8).

Crucially, the modern school of media analysis descended from Bordwell and Carroll's seismic 1996 interlocution reserves the right for experience to override science in the case of subjective phenomena. “We would be wrong to reject the idea that the [E]arth is round because it does not feel that way to us, but we would be justified in dismissing an account of character engagement that does not line up with our ... experience” (Nannicelli and Taberham 2014, 17). This both opens the door to and levels the playing field for “inter-theoretical criticism and debate” (Nannicelli and Taberham 2014, 4), and is a good example of how middle-range theory, as can be applied to artefacts like films or video games, is neither inherently scientific nor inordinately gatekept.

Murray Smith (2017) contrasts conceptual analysis and engineering with middle-range theory-building in terms of their differing degrees of closeness to empirical enquiry. For him, the former has embodied a Western analytic tradition: Philosophers define, refine, and explicate the concepts that empirical researchers (prototypically scientists) operationalise as constructs, measure, and test. What he calls “theory-building”, then,

⁷ Cognitive media theory is not a singular theory but rather the semi-empirical tradition of moving image media analysis descended from the ideas of Bordwell, Carroll, and their contributors in *Post-Theory*, via Bordwell's historical poetics (1985, 1989) and Thompson's neoformalism (1988).

we call middle-range theory. “[C]onceived as the naturalistic philosopher’s alternative to conceptual analysis ... [theory-building] emphasizes th[e] interplay between conceptual and empirical work ... the movement of traffic in both directions. Conceptual clarification does not simply precede and guide empirical research; there is feedback from the empirical domain to the conceptual sphere” (M. Smith 2017, 28).

Hence perhaps what’s needed in interdisciplinary GR and GS, if we may be so bold, is more enthusiasm from empirical experts to operationalise and quantify, in service of middle-range theory-building, concepts more readily identified with “defamiliarizing” grand theory—concepts like affect, agency, or hegemony (as in, e.g., the noteworthy analytic contributions of Bódi 2024).

Organisational Research

Metatheoretical meditations in organisational research date back further than in film studies, as the interdiscipline stems from the more mature field of sociology. We dive into our recent and partial history in the 1990s. Note, as we unfurl our rapid narrative review, that the field’s primary objects of study—i.e., people rather than artefacts—means its criteria for what theory *is* or *should do* differs slightly from film studies’.

Sutton and Staw (1995) offer an influential if incomplete definition of theory by way of negation. Their article, “What Theory Is Not”, first reminds us that data, variables, diagrams, hypotheses, and references are not theory, and cannot substitute causal logic. Second, their paper (and the replies it garnered) begins to chart trade-offs to be navigated in spinning middle-range theory out of conceptual and empirical threads. Generality versus specificity and simplicity versus complexity are the properties that they flag. DiMaggio, in his reply, calls these “vexing choices” (1995, 392): Such features can be mutually determining. Inflating a theory’s scope or applicability (i.e., enhancing its generality) means detracting from its exactitude (i.e., sacrificing its specificity).

Another binary property according to DiMaggio is focus versus multidimensionality, where the latter captures “the extent to which a theory includes reference to agency, culture, ... and[/or] ... other abstract categories” (1995, 393). DiMaggio follows Sutton and Staw in their preference for “strategic reduction”, or “abstracting away enough of the world ... to develop pointed explanations” (1995, 393). Healy (2017) concurs: Over-focusing a theory while simultaneously attempting to inflate its complexity—and indirectly, its presumptive “sophistication” (Healy 2017, *passim*)—is a fool’s errand. “Fuck Nuance” is his wryly titled contribution. Better to aim for “a fruitful combination of simplicity and strength” (Healy 2017, 119) and modestly speak to only part of a problem (Sutton and Staw 1995, 371) than fall to into the “nuance trap” (Healy 2017, *passim*) of attempting to futilely map the infinite messiness of social reality.

Segueing into theory’s “aesthetic” qualities (Healy 2017, 123 *et passim*), DiMaggio and Healy separately muse that clarity or clarifying power versus defamiliarising potential is another important dyad. Noting the value of “enabling a society ... or an academic discipline ... to see ... [things] with new eyes”, DiMaggio suggests that theoretical foci and claims can act as a “surprise machine”; “a device of sudden enlightenment” (1995, 391). He advises that “[t]heorists [may] enlighten not through ... clarity ... but ... by startling the reader” (DiMaggio 1995, 391). This flavour of theory is highly *creative*, aimed as it is at “clearing away conventional notions to make room for *artful* and *exciting* insights” (DiMaggio 1995, 391 – italics ours), and enjoys contributions that are varied in remit, method, and style (Healy 2017).

Theory's dimensions can interact, bearing on one another, but they don't necessarily. Theory can be high in defamiliarising potential without making strong commitments to being either general or specific, focused or multidimensional. Consider Trammell's examination of play through the lens of torture (Trammell 2020, 2023), which urges especially non-Black American readers to expand their view of play to accommodate its unilateral applications as a tool for subjugation. Or Fizek's *Playing at a Distance* (2022), which explores non-human agency and play; the delegation and indeed the *relegation* of the actions if not the pleasures of play to remote or nonhuman forces, as spectacle. Such theory tends to be rich in figurative, metaphorical, or analogical reasoning—"a common and powerful tool" (Healy 2017, 121)—and is often peppered with thought-provoking paradox, which can help to render *communicable* that which may otherwise seem insoluble, while also making it *memorable*.

The last bipartite property of theory noted in these particular sources is memorability versus comprehensiveness (DiMaggio 1995). Memorability is mentioned by Healy as another *aesthetic* quality: It's tied to clarity versus defamiliarisation as well as other twofold properties. For example, memorable theory typically has broad applicability or appeal (generality), few moving parts (focus), and is likely intellectually surprising. Memorable theory is *resonant*, challenging or clarifying the dominant conceptions of a cultural or intellectual epoch; it is *pithy*, lending well to aphorism or even rephrasing as a "slogan" (DiMaggio 1995, 395 – if this sounds silly, think of McLuhan or Voltaire); and it must be open to mutation and adaptation. Here, DiMaggio is emphasising the collaborative nature of knowledge production, transmission, and refinement between and among communities of theory and practice: "Theories are not just constructed, they are *socially [co-]constructed* after they are written. ... [!]ideas take on a life of their own" (DiMaggio 1995, 395 – italics ours). It's noted that to an extent, the perceived quality of a theory is a function of the quality of the scholarship that employs it.

With that said, we hope not to sully the following typology of theory's six dimensions, which we now finally review.

SIX DIMENSIONS OF THEORY

You may have noticed that the abovementioned qualities of theory are not only non-exhaustive, but they also overlap. Complexity and multidimensionality share semantic associations, as do specificity and focus. Memorability is likewise hard to disentangle from interestingness, which has also long been considered a property of good theory⁸.

How many qualities of theory have been proposed over the decades? How would one go about collating them all? And is there some kind of linguistic reduction exercise—a systematic search for near-synonyms and substitutable descriptors—that collapses unproductive distinctions, abolishes ambiguity, and posits an indivisible core for each theoretical virtue? Happily, yes—someone has done the hard work for us.

Carton (2025) metasyntesises 284 commentaries on what theory is and should do. He arrives at six dimensions of "strong" theory and proposes that they covary (that is, either compliment or detract from one another) in more or less stable ways. They are

⁸ Albeit not without critique. (Cf. Davis 1971; Tsang 2022.)

[1] importance, [2] interestingness, [3] actionability, [4] generality, [5] simplicity, and [6] accuracy. Let’s review his methodology before we elaborate each dimension, note how they influence one another, and discuss the typology’s strengths and limitations.

Carton trawled the organisational research literature, notably influenced by sociology, for treatments of theory using a search strategy detailed in his 2025 paper’s appendix. The 284 resultant texts date from 1946 to 2024 and span “editorials, commentaries, ... prescriptive articles”; “memoirs[,] and retrospectives” (Carton 2025: 1265). Vitally, his definition of theory is “arguments that describe how distinct concepts are causally, conceptually, or temporally related” (Carton 2025, 1243)—a view that encompasses five *types* of theory. (Cursorily, “explaining theories” concern causal relationships, “ordering theories” categorise and structure concepts, “comprehending theories” unpack meanings, “enacting theories” chart or predict how phenomena unfold over time, and “provoking theories” challenge conventional intuitions.)

Carton’s analysis of the dataset yielded 14 themes which were distilled into six higher-order dimensions. Importance, for example, is loaded by themes like “significant”, and “substantial”; interestingness arrives via the words “engrossing” and “revelatory”; “actionability” is informed by “applicability”, and so on. This is how Carton tackles the conceptual overlap flagged earlier with reference to Sutton and Staw, DiMaggio, etc. He rules out alternative syntactic choices by using Wordnet, “a linguistic dictionary that maps the similarity between ... 117,000 distinct concepts” (Carton 2025, 1244), to identify analogous and disparate adjectives. He then calculated statistical similarity scores and ran regression analyses to show that his chosen labels possess “sufficient variation between versus within dimensions”, which is to “provide some confirmatory evidence” that the final chosen descriptors weren’t arrived at arbitrarily or by chance (Carton 2025, 1244).

So—what defines each dimension? Below we present a simplified version of Carton’s (2025) write-up.

Dimension	Definition
<i>Importance</i>	Addresses a significant problem, trend, or widely studied topic
<i>Interestingness</i>	Stirs curiosity and a desire to share and discuss ideas with others
<i>Actionability</i>	Informs urgent, real-world problems; can easily be acted upon
<i>Generality</i>	Pertains to a broad range of topics, contexts, and phenomena
<i>Simplicity</i>	Clear, digestible, and easy to understand; few moving parts
<i>Accuracy</i>	Precisely captures behaviours and events; may be nuanced

Table 1: Dimensions of theory and their definitions, paraphrased from Table 3 in Carton (2025, 1258). His non-exhaustive “Examples” column is omitted for brevity.

An advantage of these dimensions is that their “technical” meanings are close to their everyday usages, lending them intuitability. Let’s see how the terms might describe two example theories or ideas from differing traditions within GS and GR respectively.

Ford’s “mytholudics” (2025, 2024) lets us make sense of games as and through myth. Regarding *interestingness*, the mytholudical view aspires to “[help] games enthusiasts and creatives in the games industry to think critically about the games they play and make and, crucially, how things could be different” (Ford 2025, 307). *Accuracy* seems less relevant for mytholudics, since the idea doesn’t aim to precisely map relationships or dependencies between human behaviours and outcomes. Yet Ford’s discussion of “How You [C]an Use Mytholudics” (2025, 317–18) directs readers’ attention to the theory’s *actionability* for scholars and practitioners: Mytholudics has applications in game analysis as well as in writing or design practices like worldbuilding.

Self-Determination Theory (SDT), a psychological macro-theory of human motivation (Ryan et al. 2006), has gained considerable traction in GR (Tyack and Mekler 2024) and GS (Farmer et al. 2025; Wen 2024). Tyack and Mekler argue that SDT’s popularity stems in part from its expedient applicability to many areas of GR (i.e., its *generality*)—particularly topics and practices that animate much GR and game industry discourse (its *importance*) like wellbeing and player retention. This is further facilitated by the looseness and apparent intuitability of SDT’s key concepts (its *simplicity*): Autonomy, competence, and relatedness. It’s fair to suppose that SDT’s uptake in GR is owed in part to its components’ perceived *interestingness*, though in this case the quality of interestingness lacks the “defamiliarising” quality often associated with critical lenses.

At the same time, Tyack and Mekler (2024) argue that SDT remains underspecified to fruitfully illuminate precise empirical work in GR. In other words, SDT currently lacks *accuracy* vis-à-vis predictive power when it comes to capturing games- or play-related phenomena like perceived difficulty (e.g. Deterding and Cutting 2023). Another point of contestation is SDT’s *actionability*: Tyack and Mekler note difficulties in translating SDT to game design. By contrast, Azadvar and Canossa argue that SDT’s constituent constructs “benefit game developers in offering them feedback that is not only game-oriented and *actionable*” (Azadvar and Canossa 2018, 3 – italics added).

These examples show that the six dimensions of theory—*importance*, *interestingness*, *actionability*, *generality*, *simplicity*, and *accuracy*—are elastic in that they can be used to talk about otherwise incomparable kinds of theory. They cut across domains of applicability: Scholarship and industry are both covered. Further, they are “scalable”: The kind and degree of appraisal that can be undertaken given the terms’ applicability is dependent upon the critic’s insight or expertise. As our SDT example illustrates, more knowledge of a topic means more mileage from the six terms, informing higher-level discussion.

We believe that Carton’s six empirically-derived terms for talking about what a given theory or idea *is* or *does* are valuable talking points—not gospel. Were we to evaluate Carton’s proposal using his own terms, we might frame it as *important*, *general* (that is, cross-disciplinarily applicable), and *interesting*. We find it striking, if nothing else, that nobody’s hitherto surveyed an influential body of literature to qualitatively meta-synthesise what “strong” theory is. That Carton’s metatheory is *important* (dovetailing with perennial questions of interdisciplinarity) perhaps detracts from its *actionability* only to the extent that recommendations for synergising disciplinary efforts within a given field tend to hinge on some vital but nebulous possibility of epistemic concord.

CONCERNS, LIMITATIONS

Before we conclude, let's attempt reflexivity by considering problems stemming from our suggestion that GR and/or GS expedite interdisciplinarity by embracing middle-range theory and a positive lexicon for discussing what theories *do*.

First, fans of grand or "capital T" theory may decry the middle-range ideal that research involves empirical data. Some theory proceeds from subjective experience. It's a context in which "judgment is needed but measurement is hard" (Healy 2017, 123). Again, think of Trammell's (2020, 2023) framing of play as torture-like. It can be immeasurably beneficial for the voices of scholars with certain positionalities to be heard above those of others. How best to test the soundness of such contributions?

The point is precisely that it may be unnecessary, undesirable, and impossible to foist epistemic value judgments onto such theoretical contributions. Falsifiable knowledge must sometimes play second fiddle to rhetoric and *writerliness* (Kember 2016). Almost the entire history of the philosophy of science centres on a tension between testable and untestable *yet edifying* ideas. We're unlikely to solve this antinomy any time soon. Thus, we endorse cooperation. We're not suggesting that everyone in GR do empirical work, or even do work that's open to either empirical support or falsification. We do, however, hold that it's strategic to collaborate across the so-called epistemic divide. Decisionmakers are often readily swayed by data produced by interdisciplinary teams, and we won't be doing much without funding. Let's strive to set aside our differences.

Second, talking about theory in English is obviously regrettably Anglocentric. This not only risks being exclusory but can also be conceptually limiting. Western scientific and philosophical ideals boast de facto primacy, while complimentary or contrasting ways of knowing are sidelined. This may be best redressed at the institutional level. Entities like DiGRA already invite multilingual pluralism through local chapters. Special journal issues dedicated to (for instance) translations of disciplinary and metatheoretical debates across different research cultures could be one way to cross-pollinate and lend equity to a more unified, pan-continental GR programme.

Third, Cunliffe (2022) sardonically asks, "Must I Grow a Pair of Balls to Theorize about Theory ... ?" She is concerned that the twin enterprises of metatheory and ontology smack of machismo. She notes that most metatheoretical papers "are written by men, and espouse forms of theorizing that are based on a masculinized rationality that privileges [reductive] abstraction, a logic of objectivity[,] and proceduralization" (Cunliffe 2022, 1). This, she warns, limits the possibility of theorising in more "human" and "imaginative" ways "due to ontological blindness, epistemological defensiveness, hegemonic masculinity[,] and myopic self-referentiality" (Cunliffe 2022, 1).

Kember (2016) would agree. Evoking the Derridean portmanteau of phallogocentrism (which combines logocentrism—the equation of language, meaning, and reality—and phallogocentrism, or a focus on the masculine perspective), she argues that some of us risk perpetuating the pernicious ideology of technological or institutional "innovation" of the kind warned against by Malazita et al. (2024). Empirical or "disembodied" knowledge practices, Kember writes, "tend toward both scientism ... and masculinism. It transpires that there is no 'we' in t(he)ory" (Kember 2016, 19). Again, we're not sure how to address this other than to encourage open-mindedness across the board—especially from advocates of epistemic traditions and methods that vie to categorise and quantify the world.

We've admittedly based our proposal on the work of a white man whose qualitative meta-synthesis aggregates the work of mostly other white men. And we've knowingly limited the examples that partly motivate our argument to sociological organisational research and film studies. This is despite having noted that GR and GS consist in disciplines as diverse as media studies, design, human-computer interaction, psychology, pedagogy, anthropology, philosophy, critical theory, cultural studies, and more. Where are all the marginalised voices? This paragraph is our *mea culpa*. Our excuse that it would take *multiple* papers to give fair and equal treatment to both entrenched ideas (such as falsification being a means to truth) *and* radical ones, like Barad's agential realism and its edifying yet complex compound concept of "ethico-onto-epistemology" (2007; Geerts and Carstens 2019). We think that problem spaces can be defined in normative terms before abstruse but productive ones are applied. We've used masculinist figures of speech like "currency" and "camps"—metaphors rooted in warfare or market logics of economic exchange. But again, this is to frame what's at stake using familiar concepts before appealing to more progressive ones.

A fourth issue (related to the foregoing point as well as our first and second concerns) is that theory is increasingly developed and distributed through non-textual artefacts. Playable essays and other interactive "publication" formats (for lack of a better term) are increasingly common. (See, e.g., Juul 2021; Gualeni and Van de Mosselaer 2023; Khaled et al. 2024.) Arguably, by relegating our acknowledgment of such novel ways of knowing to the penultimate page of our paper, we've done little to redress the epistemic injustice flagged earlier, or promote the kind of reformist pluralism that we have ourselves touted. Still, we hold that the terms introduced in this paper can be used to talk about theory explored through such esoteric and "embodied" (cf. Kember 2016, above) practices as, say, LARP, or Live Action Role-Play (e.g., Cazeneuve 2021).

A fifth concern is that like Gregersen's proposal to map disciplinary disagreement (see pp. 4–5), our suggestion requires that action be taken "*en bloc*" (2016, 2)—something we deemed impractical. This is partially true. If the terms tabled in this paper are to prove productive, then as many GR/GS scholars as possible must "get the memo", so to speak. Is it not unrealistic to expect uniform movement from such a sprawling, mutable, and multidisciplinary field? One difference between our modest proposal and others' recommendation to frontload ontology (Gregersen 2016; Aarseth and Grabarczyk 2018) is that the latter (i.e., prescribing adherence to sanctioned concepts and disciplinary truths) must seemingly be enforced *before* joint progress towards interdisciplinarity can be made. By contrast, lobbying editors, boards, and committees to adopt six positive terms to talk about theory fosters inclusivity and pluralism *while* instituting changes that assist diverse disciplinary positions in converging on common truths. Basically, we believe that this proposed way of doing things is less reliant on signaling to scholars, "you're doing it wrong" (Vossen 2025).

A sixth concern is that the approach we're advocating simply postpones disagreement until peer review. We say that this may be a luxury problem. If ontology can be put on the back burner then perhaps, like in organisational studies (Carton 2025), reviewers' sometimes-conflicting demands will more often be about how to "weight" a given contribution (should it prioritise accuracy or generality? interestingness or simplicity? actionability or importance?) than about, say, what polysemic concepts like agency or narrative mean in relation to games and play.

CONCLUSION

We've presented a case that interdisciplinarity (or something like it) is desirable if not inevitable. Either way, to mature the ideally pluralist and cooperative project of games research, we need to be able to engage with one another charitably and in good faith. The positive terms introduced should help achieve this. We hope to have shown that *importance, interestingness, actionability, generality, simplicity, and accuracy* are to theory what personality traits are to people: No one's totally deficient on *all* fronts. Let's endeavour to see the value in each other's work even if epistemically speaking, we don't "get" where it's coming from it at all.

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