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Abstract

This is a discussant comment for the Special Issue on video games and youth culture. It is written from an unusual perspective: that of a video game designer, rather than that of an expert in Psychology. It presents in humorous fashion a straightforward, almost certainly biased position, which nevertheless reflects many views held by the designers, developers and players of video games. It is hoped that this will contribute to the debate in a manner that helps it move forward, or, failing that, at least sideways.

Special Note

This special issue on video games and youth culture is packed with Psychology goodness, but what does it look like to someone from the other side of the fence? Sadly, my days of being a part of youth culture are some four decades in the past; however, I do know rather a lot about video games. In this discussant comment, I take on the role of a "friendly critic", outlining what I believe game designers, developers and players might make of the papers. In a needy effort to be entertaining, at times I may phrase my observations rather less formally than is the norm for this fine journal. I therefore wish to stress at the outset that my words are not intended to be spiteful or malicious or insulting to anyone, and I apologise in advance should you take them that way. Please read them with the English accent I wrote them in; otherwise, you may find the humour comes across as aggression – which is the last thing we want to happen…

I am delighted to have been invited to write a discussant comment for this special issue on video games and youth of the American Psychological Association's journal, *Psychology of Popular Media Culture*. Despite the best efforts of many in the American Psychological Association, video games remain a popular culture; it'll look great on my CV.

From this instinctive and cynical breakdown of a high-prestige task into a risk/reward proposition, you may have gathered that I am not a psychologist, nor even a social scientist. I am, in fact, a game designer (albeit a scholarly one). If this journal's readership consisted primarily of climate change scientists, I would be the power station guy[1].

I was asked to remark on the contents of this special issue from the perspective of Game Studies. Actually, I'm going to do no such thing, because Game Studies is the academic discipline concerned with the critical study of games from a cultural and humanities point of view. I'm a game designer: I look at games as works of art, as glorious engines through which designers say things to players. Game

Studies is the games equivalent of Lit. Crit.; I come from a direction more akin to Creative Writing. I'm like a friendly critic, except without the critiques.

This could work out well, or it could work out unwell. The thing is, game designers of all hues read very widely; it's not that they are interdisciplinary, it's just that they have a need to know a little about a lot – and sometimes, a lot about a little. They will suddenly put all work to one side, seized by an urgent desire to spend a week learning all they can about pagodas or the inner workings of the Palestine Liberation Organisation in the mid-1970s[2]. This means that they tend to have an eclectic background of knowledge upon which to draw. When they read up in a specialist field and it contradicts what they've read elsewhere or know from experience, it sets off bells in their heads that may not be set off in the heads of those steeped in the lore of the subject. In other words, they tend to see the woods rather than the trees[3].

The downside of this is that they are perhaps more thorough than they need to be. This article is twice as long as I was asked to make it[4] because I decided to comment on each paper individually. Most regular readers of this journal aren't going to delve into every paper[5], but I did. In so doing, I made a number of observations about the field that I summarise at the end.

Oh, the order in which I comment on the papers is the traditional "the same as the one in which I read them". No-one told me it had to make sense.

TL;DR[6]: *O wad some Power the giftie gie us, To see oursels as ithers see us!* (Burns, 1786)

Deborah L. Linebarger: *Contextualising video game play: The moderating effects of cumulative risk and parenting styles on the relationship between video game exposure and problem behaviours*.

Titles are always tricky to get right. Short titles tend to promise more than they deliver, but long titles run the risk of turning into abstracts. Furthermore, the longer a title is, the greater the chance that it'll either bore the reader or frustrate them. In this particular case, it's the latter: the title seems to presuppose that there *is* a relationship between video game exposure and problem behaviours. The body of the paper, however, starts by noting that the jury is still out and ends by concluding that the jury can now come in to deliver a verdict of not particularly guilty.

This is just as well, because what we have here is an example of a disconnection between what some psychologists have found and the personal experience of the people who play games[7]. They would find it perplexing for a relationship between video game exposure and problem behaviours to be taken as axiomatic. However, I'm sure it's just as perplexing to many psychologists that gamers will readily question their fundamental concepts without having read the supporting literature. There's a definite fault line here that isn't going away.

Then again, perhaps it *is* going away. Time moves quickly in the games world. Linebarger states that "recent estimates" show that on a typical day, 17% of children under 8 played games for an average of 75 minutes a day – but the source is dated 2011. In games terms, 2011 is not "recent". That 17% could be higher or lower; those 8-year-olds are now aged 12. This isn't a criticism of the author (who presumably used the latest data available), or of the many moons it takes to get a paper accepted and published by *Psychology of Popular Media Culture*; it's simply a reminder that all the time you're doing your research on the effects of games, the world is moving

on regardless. It's like a Victorian gentleman reading the headline in his morning newspaper and spluttering "My God! Khartoum is close to falling! We must send aid!" – three weeks after Khartoum has already fallen.

The substance of Linebarger's paper concerns how parents' parenting styles affect their children's consumption of video games. This is useful to know if you're a parent, and also if you're a cynical video game marketer looking to find out which demographic is more likely to buy games for its kids. However, the question is actually only half what it could be. Whenever I see a research hypothesis along the lines of "Does X predict Y", I wonder why the words "or *vice versa*?" haven't been added to the end. Is it not reasonable to ask whether children's consumption of video games might affect parenting styles instead of the other way round? After all, upon the invention of the VCR it would not have been contrary to ask[8] if the sudden ability to dump children in front of *Thomas the Tank Engine* playing on a loop could tempt some parents to be more lax in their parenting than they might have been otherwise. Perhaps the same could be said of video games; indeed, perhaps Linebarger's existing data set already contains the answer.

Multiple sources are cited that suggest parents who exhibit inconsistent parenting tend to create environments in which discipline is inconsistently applied. Putting my game designer hat on, I can't help but notice that these parents have adopted a variable feedback schedule – exactly the kind of thing that crops up in certain types of game[9]. Could it be that the children were seeking out games that mimicked their parents' inconsistency, or ones that offered more stability instead? As in much research about video games, it would be very useful to know *what* games these children were playing, rather than merely that they *were* playing.

It's also rather odd that we don't know which parents played games and which didn't.

The survey methods, as is usual, look perfectly fine to social scientists and perfectly weird to non-social scientists[10]. If a parent is irresponsible in their parenting, there must be a fair chance that they're irresponsible at other things, too – such as conscientiously keeping a 24-hour time diary of their child's video game exposure. Similarly, if the survey takes 50 minutes to complete, then the survey's results only apply to parents who have 50 minutes to spare. I know that when my own children were growing up I had 50 minutes to spare only rarely (and when I did, I spent them playing games on my computer).

"Children whose parents identified them as Latino/a, African American, Hispanic, American Indian, or other were coded as minority status and coded at-risk." America, you have a problem.

One particular aspect of Linebarger's research that gamers would particularly applaud concerns its appreciation of other media. If children aren't playing games, what are they doing instead? A good many of them will be watching TV. Most research on the effects (deleterious or otherwise) of video games on individuals considers games in isolation, so the wider context is missing. If playing games is bad for your children, then whether or not you would prefer them to watch TV instead would surely depend on which activity is the less worse. Unusually – and very refreshingly – Linebarger has actually looked at similar relationships between parenting and children's consumption of TV. It will be fascinating to see what conclusions she can draw by comparing her results between the two media.

Overall, I liked this paper. Perhaps because it doesn't charge at games head on, but comes from a more oblique direction (that of parenting styles), it has a more objective and less partisan attitude than much of what I read on the subject of the

psychology of gamers. It's not of tremendous direct use to game designers such as me, but probably a lot more use than game design papers are to people with children.

TL;DR: parents and carers have far more to worry about than video games when it comes to child behaviour problems.

Patrick M. Markey, Charlotte N. Markey and Juliana E. French: *Violent Video Games and Real-World violence Rhetoric versus Data.*

I love this paper, and not entirely because it ensures I'll still have a job in 10 years' time. I love it because it calls out an entrenched paradigm by calmly and rationally demonstrating that its predictions that the sky is falling in are not actually reflected by the behaviour of the sky.

The paper's principle finding is something that is absolutely blindingly obvious to anyone who plays "violent" video games or who has friends or family who play them: such games don't, in fact, make people act noticeably more violently; if anything, they do the opposite. I adore the deadpan way that Markey, Markey and French write: "Unexpectedly, many of the results were suggestive of a decrease in violent crime in response to violent video games" – yes, unexpectedly if you're anyone not so focused on your results that you don't consider what they *mean*. We've had violent video games for decades, for at least two of which they've been mainstream. If they were as violence-inducing as we've been led to believe, there should be rivers of blood in the streets by now. Where are they?

The way that video games have been singled out as potential causes for aggressive behaviour has always been suspect. Why aren't American Football or Ice Hockey regarded as dangers to society? Players of those don't merely use the context of a game to pretend to hurt pretend people, they *actually hurt actual people*. Boxing is often characterised as a way to *escape* from a violent stratum of society, not as a cause of its existence. Why are the psychological effects on aggression of the games that are contact sports not subjected to the same relentless study as for video games?

Perhaps it's the special relationship that video games appear to have with school shootings? Well no: what seems to have passed many journalists and political commentators by is that the reason we find that kids who pull the trigger in every school shooting are video gamers is because pretty well *all* kids are video gamers. As Markey, Markey and French deliciously point out, "finding out that a young man who committed a violent crime also played a popular video game … is as pointless as pointing out that the criminal also wore socks." I am *so* stealing that line.

The reason I wrote "pretty well all" kids are video gamers back there, by the way, is that there is a minority who aren't. I was waiting for a delayed flight at Indianapolis airport in 2007 when news of the Virginia Tech shooting broke. Everyone was watching the live news reports unfold on a large TV. A reporter was talking to a couple of students who knew the gunman personally; he asked if they'd noticed anything strange about their classmate. "Did he play video games?". The answer came back that yes, he was kinda strange because he *didn't* play video games.

The interview did not make it to the later news summaries.

This weirdly foundational assumption that violent video games lead inexorably to school shootings is very USA-centric. In countries with stricter gun laws, school shootings are almost unheard of. The last one we had in the UK was in 1996, undertaken by a 43-year-old man in Dunblane, Scotland. As a result of his actions, the private ownership of handguns was completely banned; even our Olympic shooting competitors have to practice abroad. The anecdotal, folk-wisdom

hypothesis that there is a strong correlation between people who own handguns and people who use handguns to shoot other people has subsequently been seen to be largely correct. Who would have guessed? Yet time and time again, when the topic of violent video games is brought up some reference is made to school shootings.

Hurtful though it is to say this, the academic study of violent video games and aggression has long been something of a joke for many people working in the games industry. They read some of your papers (you knew that, right?) and attend some of your conferences (you knew that, right?) and have conferences of their own to which they invite some of you as speakers (you knew that, right?) and they simply can't understand how the whole field has managed to persuade itself that a survey of 32 college kids can stand as a proxy for the whole of humanity. Some of these companies operate games with millions of players, collecting terabytes of data every day. They have much more information than you have to play with, and they mine it with gusto. They're the people for whom the phrase "big data" was invented. They *know* dubious results and methodologies when they see them.

Oh, what's that you say? You want to *look* at their data? Well when you asked in the past and they let you, you used what you found only to show that games are addictive[11]. This rather hurtful exhibition of ingratitude has led to an attitude of once bitten, twice shy. So no, you can't look at their data.

A basic lack of understanding of games from some scholars doesn't help matters. Now clearly, from the researcher's point of view, if you believe that playing games makes people violent then you're not going to want to play such games yourself because of the effect it could have on you. People studying heroin addiction are not going to make a regular habit of taking the stuff just to "understand" addicts better; likewise, people studying games aren't going to want to become aggressive by playing them themselves. Nevertheless, not all video games are the same as one another, and being violent or non-violent isn't the only differentiator. If you don't play them, you won't know this.

Take, for example, *Tetris*. *Tetris* is non-violent, but it's also a casual game. Casual games have many qualities, one of which is that they're (technical term ahead!) drop-in, drop out. They're a bit like TV soap operas in that regard: if you're interrupted while consuming one, you can abandon it without fuss and it's not going to bother you much. Now consider a violent game such as *Grand Theft Auto*. This is an example of what's called a AAA ("triple A") game: AAA games have longer-lasting and much deeper gameplay than casual games, a bit more like a movie than a soap opera.

So, ask yourself what would happen if you were watching a movie and just when you'd got interested a researcher switched it off to ask you to blast loud noises at strangers. You'd not be happy, right? Cover your ears, strangers, here it comes! If you were watching a soap opera, perhaps you'd be less perturbed. Is it any surprise, then, that if you compare the aggression you get from interrupting *Tetris* players after 15 minutes with that which you get from interrupting *GTA* players, the latter are more tetchy than the former?

Markey, Markey and French have blown the whistle on all this – and about time too. I see this paper as perhaps a turning point in the way that the psychology of video games is studied. The risk you take when you make a prediction is that sooner or later time catches up to it[12]. Eventually the past's future becomes the present; an informed gamble that something will happen tomorrow is lost or won when tomorrow becomes today. I'm sure that the researchers who have been repeatedly telling us how video games cause violent behaviour are sincere in their prophecies, but they really *have* had enough time now for their forecasts to manifest themselves

as actual societal changes. As French and the Markeys show, this clearly hasn't happened. *Why* hasn't it happened? Is research on violent video games having its phlogiston moment, or is there something else going on? We really do need a considered response here.

I do have a final point that may cheer up pro-aggression advocates, though. Consider this: as Markey, Markey and French reveal, *Grand Theft Auto IV* launched at the end of April, 2008, but there was no increase in violent crime in May, 2008. That's only to be expected, though: in May, the would-be thugs would all be at home playing *GTA*, rather than going out joyriding and beating up prostitutes. Idea! The evidence that games make people more violent *can be used as evidence that games are addictive* instead! People whom experiments have shown clearly *want* to be aggressive are *so addicted* to the games that *make* them aggressive that they'd rather play these than commit random acts of brutality. It's like a unified theory for gamer psychology research! It's win-win all round.

TL;DR: the belief that violent video games cause violent behaviour is the emperor, and he's wearing no clothes.

Whitney DeCamp: *Impersonal Agencies of Communication: Comparing the Effects of Video Games and Other Risk Factors on Violence*.

This is one of those papers that one wonders why it hasn't been written before. As DeCamp points out, the idea that exposure to violent media might influence a person's behaviour has been around for quite a while, yet over time the counter-arguments have generally prevailed. As a layman, I would argue that "causes aggression" has merely replaced "undermines morality" as the concern *du jour* over new forms of entertainment. We no longer complain that the waltz is scandalous, which we did once (Knowles, 2009); instead, we complain that cinema, music, comic books and now games are scandalous. Whenever society detects a deterioration in *the status quo* that it finds worrying, new ideas are generally the first in line as candidate causes for this deterioration. It does make sense to start with them, because the new ideas really *could* be the cause (those who blamed rapid urban sprawl on increased motor car ownership were correct). What makes less sense is looking *only* at the new and over-emphasising its effects.

As I mentioned earlier in my comments on Linebarger's paper, this is one of the bugbears that game developers have with research in this area: there's little comparison with other media. Suppose we accept that playing a violent video game does make you aggressive: are the effects greater than or less than the effects of watching a violent movie or a violent demonstration on the TV news? Violent games are typically compared against non-violent games, but not against entertainment forms in general.

What if instead of violence you were looking at pleasure? If your studies identified that playing games gave people pleasure, you could further your research in one of two directions: try to figure out what it is about a game that makes it cause pleasure (so as to help producers make games that are more fun or less fun); try to place games' pleasure-causing features in the wider context (so as to help consumers choose activities that give more fun or less fun). With games and violence research, we get neither. Studies concentrate on showing that games cause aggression, but they only ever guess[13] at why games might do this and they rarely compare games' aggression-causing prowess with other aggression-causing phenomena.

DeCamp, however, grasps the nettle and *does* do such a comparison. Rather than looking only at games, he examines a range of potential causes for violent behaviour. His study uses very robust data from over 6,000 early-teenagers, which is in itself impressive. I was once at a conference at which a PhD student was drawing conclusions about addiction from a study of 16 individuals, four of whom didn't make it to the second stage and only two of whom were female. 6,000 is enough to dispel any notion that there may be noise or incompleteness in the data.

What DeCamp finds is that the playing of video games, violent or otherwise, has no bearing on actual expressions of violence. He makes the reasonable point that just because games may in the short term cause people to exhibit behaviour that can be considered aggressive, that doesn't automatically translate into long-term violent behaviour. As an analogy, it could be that if you make people play games featuring soft, fluffy kittens then afterwards you might find that your subjects demonstrate a greater propensity to eat marshmallows than they did before; this wouldn't mean that the playing of kitten games was an indicator of obesity to come, though.

Could it be that part of the problem with the public perception of games and aggression is to do with the vocabulary used? In Games Studies, when researchers refer to a concept such as "immersion", they often do so with a very specific interpretation in mind (sensory immersion); this is not the sense of the word typically understood by players (who use it more experientially). This means that when researchers pronounce on immersion, what they are saying can make perfect sense to other researchers but very little sense to players.

It seems that psychologists may have a similar relationship to the word "significant": it means something very particular in a statistical sense to say whether an effect is significant or not, but this isn't the meaning that the public at large understands. If the public is told that a study shows a "significant relationship" between playing violent games and aggressive behaviour, they don't think that this means there's less than a 5% chance that no such relationship exists: they think it means the relationship itself is sufficiently important that they needed to be told about it. If you wrote that the relationship between liking chocolate and eating chocolate was "significant" the public would mock you: in their view, it's not significant because it's not noteworthy – even though from your perspective the statement is entirely accurate and uncontroversial.

From such misunderstandings, entire media panics are born.

TL;DR: the more time you spend looking at things that don't cause violence, the less time you spend looking at things that do.

Johannes Breuer, Jens Vogelgesang, Thorsten Quandt and Ruth Festl: *Violent Video Games and Physical Aggression: Evidence for a Selection Effect among Adolescents*.

There are a lot of good things about this paper, but to a game designer's eye the best one is the beautiful way that its *raison d'être* reflects its own findings. The dominant model for describing games and aggression is socialisation-based and the main opposition is catalyst-based. However, constant exposure to the dominant model has failed to socialise Breuer *et al* into accepting it, and has catalysed them to put it to the test. It's great when such foreshadowing works in real life.

Breuer *et al* begin by noting that the debate about a link between video games and aggression is ongoing. They're right, it is, but it's more ongoing in some places than in others. It's big in the USA and Germany, for example, but not in Scandinavia

and the UK. I don't know why that is, but I'm sure there's a PhD in it somewhere for a social anthropologist.

Accepting that tests on people who consume violent media show them to be more aggressive than are people who don't consume it, Breuer *et al* identify two competing hypotheses from the literature that can explain why this is the case. The socialisation hypothesis says that the more you expose people to violent media, the more aggressive they will become over time. The selection hypothesis says that people who are naturally more aggressive will seek out media that contains violence.

Put another way, does more exposure to romance novels make people more romantic, or is it that romantic people are naturally drawn to romance novels?

Breuer *et al* further identify three models that describe the mechanical details of one or both of these two hypotheses. These are the General Aggression Model, the Catalyst model and the Downward Spiral model.

Professionals in the games industry are unimpressed by the General Aggression Model. Apart from the fact that its predictions have been the-opposite-of-substantiated by reality, it has a critical flaw in its premise. See, when you play a violent video game such as a first-person shooter, *half the time you lose*. You are on the receiving end of violence, not just on the meting-out end. Unless you play these things solo[14], then by the GAM's logic you are as likely to be socialised against violence as you are to be socialised for it.

The Catalyst model makes more sense, although its relationship to the two competing overarching hypotheses is unclear. It seems to suggest that people with latent aggression could live a life of complete peace and harmony if never subjected to a trigger, but if they encounter one then their aggressive tendencies will surface and they'll be attracted to outlets for it. Socialisation could be responsible for the trigger; selection alone would be responsible for the result. What this doesn't explain is why anyone without aggressive tendencies might nevertheless play violent games; I suspect that the model assumes they don't.

The Downward Spiral model is a recursive combination of both competing hypotheses. Aggressive people are attracted to violent games, which make them more aggressive, which attracts them to yet more violent games, and so on. This is described as being a "negative feedback loop" – a concept that game designers use a great deal, but I'm not entirely sure in the same way. It doesn't appear to have much currency among those studying violent games and aggression, though, so I won't labour the point[15].

Breuer *et al*'s analysis of the major previous studies in this area is thoughtful and forthright, and they make a number of very telling points concerning the age of the participants typically sampled. Minors of different ages have different propensity for aggression in general, and their very youth denies most of them access to the more violent games. In order to take account of this, Breuer *et al* ran their own longitudinal study.

Now I *can* make some criticisms of this study. While not self-selecting, it's just as self-rejecting as most of the surveys they criticise, and it uses self-reports of aggression rather than measuring what actually happens when respondents are goaded to be aggressive. Also, several opportunities are missed to run multiple analyses on the same data: I would have liked to discover whether the people in the first wave who both showed aggressive tendencies and played violent video games also showed aggressive tendencies in the second wave, for example. Nevertheless, these are relatively minor complaints; overall the survey looks reasonably sound[16].

What Breuer *et al* found was that physical aggression predicts the use of violent video games, but the use of violent video games does not predict physical

aggression. Given how many people actually play violent video games, this could be akin to saying that physical aggression predicts the use of a knife to eat, but the use of a knife to eat does not predict physical aggression. Nevertheless, it's not what the results show we *can* predict that's important so much as what the results show we *can't* predict. We can't say that playing violent video games makes individuals more aggressive than those who don't play them, because a good many players aren't more aggressive at all.

This is important, because it's compatible with the Catalyst model but incompatible with the GAM. It's a longitudinal study, so socialisation-hypothesis effects ought to show up easily: they don't. As a game designer, this tells me what I knew anyway; I can hope that it will tell those who advocate the GAM that they're barking up the wrong tree, but realistically I expect that the barking will continue for a while yet.

Breuer *et al*'s findings support the Catalytic model for aggression, but I'm left wondering whether this could have wider implications. The thing is, many game designers are a little hypocritical when it comes to accepting what people pick up from play. Do games make people more aggressive? No! Do games make people smarter? Yes! It would appear that games are great tools for learning, but only when learning good things, not bad things. Designers can't have it both ways: either games can carry a pedagogical payload or they can't. The entire Serious Games industry says that they can; this being so, why is it unreasonable to suppose that aggressiveness isn't among the "skills" players can learn? A catalytic model provides a possible answer: different triggers cause different learning experiences, and more people have triggers for learning problem-solving skills than have triggers for learning aggression.

The other possibility, of course, is that Serious Games have themselves been researched for 20 years and still haven't really got anywhere, so maybe games aren't really all that good at teaching *anything*.

TL;DR: people know what they like, rather than like what they know.

Ray Surette and Allison Maze: *Video Game Play and Copycat Crime: An Exploratory Analysis of an Inmate Population.*

Surette and Maze's study features an unusual and hard-to-reach demographic: 249 adults enduring incarceration. As these individuals have been proven to have each committed at least one serious crime[17], they are in a unique position to explain what led to their committing that crime. The aim of the study is to assess the influence of video games in copycat crimes. Spoiler alert: there is none, or if there is then gamers are better at evading capture than are non-gamers so are not in the sample set.

The study itself didn't shine a light specifically on video games – it included other media, which I guess will have their own papers written about them if they haven't already. Thus, although the title of the paper may give the impression that video games are being picked on unfairly, for once they're not. We'll have to wait for all the analyses to be presented before we'll discover if any of the media tested have an impact on the propensity of individuals to commit copycat crimes. If they don't, it would rather undermine the precepts of media effects research.

It's not clear to me as a game designer why media effects research necessarily applies in its pure form to games anyway. Games are a medium, but each medium differs from others in subtle ways. One of the assumptions implicit in looking at

"exposure to violent media content" is that the content comes as a stream (or a *text* if you want to write a critique about it). Games aren't quite the same as other media in this regard: they aren't themselves streams; rather, they are artefacts for *creating* streams, with the player (through play) in control of the streams so created (Hunicke *et al*, 2004). The fact that players can continually influence the streams means they have some say in what violent media content they're being exposed to; the general assumption seems to be that this is a bad thing, but it could be positive or (as Surette and Maze have found here) completely anodyne.

It's not obvious why "media" should be a major factor anyway. As I mentioned when discussing Markey, Markey and French's paper, people enact violent behaviour in boxing, American football, ice hockey… Does that make these people more aggressive in general? Why would hitting representations of people in a video game make you be more aggressive than if you were hitting real people in real life? At least the game doesn't give you the skills you need to act on your new-found desires with greater success, unlike, say, martial arts training.

This brings me to one of the arguments that Surette and Maze report as being used to support the notion that violent video games make people more aggressive. The supposition is that if you play a violent character then you are more likely to identify with that character than in other media, because you don't just *watch* violence happen vicariously, you *enact* it yourself. OK, well let's assume this is true. Let's also note that roughly 40% of male MMO[18] players at any moment will be playing as a female character[19]. You don't just *watch* someone being female, you *enact* it yourself. Is now a good time to be investing in gender reassignment clinics?

I'm not an expert on copycat crimes, and I suspect that the same can be said of most other readers of this journal. However, there seems to be a distinction between getting an idea to do something (which would affect passive media such as movies more) and building the skills to do it (which would affect active media such as games more). Both are in some sense "copycat", but not in the same way. It would be interesting to be able to identify the nature of the copying that a copycat criminal undertook, and relate that to their exposure to their preferred media. The hopeful phrase, "further research is necessary" springs to mind…

The paper concludes that people who commit copycat crimes are no more likely to have played video games than those who didn't play games. Of course, if they spent all their days playing *Candy Crush Saga*, it's hard to imagine what copycat crime they might commit that would lead to a stay in a correctional facility; the type of game that people play is often very important, but it's rarely given much attention. Not all games are the same.

Something else that isn't really recognised by people looking into the effects of games on people is that the term "game play" (or *gameplay*, as game designers prefer to call it) is to do with the mechanics and dynamics of a game. It's *not* to do with its fiction or the dressing. Consider the 1980s game *Tetris*, which is often used as an example of a non-violent video game. Suppose that instead of dropping blocks onto a pile, you were dropping people into a hole (Koster, 2013). This "Black Hole of Calcutta" version of *Tetris* would feel completely different to the player[20], because its dressing has changed. However, its gameplay would be *exactly the same*. Many of the concerns people have about violent video games are to do with the dressing – the association of symbols to game tokens – rather than the gameplay. Most of the concerns to do with addiction, however, are indeed gameplay-related. A knowledge of this simple distinction would benefit research in the area no end.

Finally, I was astonished to learn that 52% of the population of the facility where Suzette and Maze undertook their study were African Americans. 52%?! You really *do* have a problem there, America, but it's nothing to do with video games.

TL;DR: game designers will need to come up with more original ideas if they hope to inspire players to commit copycat crimes.

Corinna S. Martarelli, Lilla M. Gurtner and Fred W. Mast: *School-Age Children Show a Bias Toward Fantasy Classifications After Playing a Platform Game*.

This paper finds that playing a video game exerted a short-term influence on children's ability to distinguish between fantasy[21] and reality. The, er, term "short term" is important: it could, for example, be argued that by being exposed as children to fantasy worlds, individuals might be able to internalise the experience sooner and so in the long term become *more* able to tell truth from fiction as adults. This would be great news for me as I'm an atheist, but I can appreciate that it might take a rather longer experiment to discover whether or not such a hypothesis was true.

My first reaction when I read the title of this paper was to wonder if the same effect happened with picture books and films and stage magicians. By the end, I was none the wiser. Martarelli, Gurtner and Mast are aware of the question[22], but I'm puzzled as to why they didn't consider answering it. If it's true that *no* previous work has investigated this topic, why would you start by comparing a video game with a memory game? Why wouldn't you compare it with a comic book or an episode of *SpongeBob SquarePants*?

The study looked at 34 children, split into two groups. The experimental group of 20 played the video game *Super Mario 3D Land*; the control group of 12 played the card game *Memory* (also known as *Pairs* and *Concentration*[23]). I have to confess, when I saw that one of the sub-groups only had 12 in it, my heart sank. I appreciate that it's expensive to conduct large surveys, and that n=34 is regarded as being respectable in Psychology circles, but when your control group has only 12 in it then you're in danger of seeing individual effects over-influence aggregate effects.

The meat of the study lies in the test undertaken after the players had spent 15 minutes having fun (for *Super Mario 3D Land*) or being bored (for *Memory*). The participants were shown images that were either of fantasy objects (such as creatures that don't exist) or of real objects (such as motor vehicles); the images were taken directly from either from *Super Mario 3D Land* or from other video games. The participants had to identify each one as being either real or fantasy.

As the full test was going to ask them to indicate whether, say, an image of a tree taken from a game showed a real or an imaginary object (answer: real), the children were given a pre-test to make sure that they understood how they ought to respond. This brief training period was a good idea, but even so only 83.2% of the participants' responses were correct upon its completion. This meant that going into the full test we could already expect one answer in six to be wrong. This introduces rather a lot of noise into the system, which for a larger sample perhaps wouldn't matter but for groups of 20 and 12 is a little worrying.

The big finding is that children who played *Super Mario 3D Land* were more likely to classify reality images from that game (and that game only) as being fantasy images. In all other respects, they identified images as being real or fantasy with an accuracy similar to that of the control group.

I don't wish to quibble with these results, which seem reasonably valid to me. However, I do feel that the way it is reported unnecessarily plays to the gallery.

Although this finding does indeed demonstrate a shift in the participants' "ability to distinguish fantasy from reality", this is an ambiguous way of putting it. If you were to read a newspaper headline, "VIDEO GAMERS CAN'T TELL REAL FROM IMAGINARY", you could expect many readers to go away with the impression that those who play computer games believe the imaginary to be real, not that they believe the real to be imaginary (which is what the study actually found), nor that the same may or may not be true of people who watch TV or read manga but we don't know because we haven't looked.

As for why the results came out how they did, well that's always the fun part in Psychology papers as you get to speculate! A couple of possible explanations spring to mind in addition to those proposed by Martarelli, Gurtner and Mast.

It could be that in playing the experimental game, the children developed a deeper understanding of it. For example, in *Super Mario 3D Land* there are mushrooms that are rendered as if they had heads for the stem wearing hats for the cap. Clearly, real-life mushrooms don't have eyes, but nevertheless mushrooms are a real-life thing so for the purposes of this test they ought to be classified as being real. Now if you only had the image to go on, that's how you would indeed classify them: as being real. However, if Mario eats a Super Mushroom then it turns him into Super Mario. Real mushrooms don't have that effect on people. Is this functionality enough to transform a categorisation of a Super Mushroom from "real" to "fantasy"? You can appreciate why an 8-year-old might think it is, training or not. I don't know if Super Mushrooms featured in the study itself, but I can see how their mere existence (alongside other wackily-functioned items such as Fire Flowers) might lead a person to form the opinion that the whole game was completely made up. Martarelli, Gurtner and Mast are aware of this possibility, but because they didn't tell us whether the misclassification was dominated by particular images or was distributed more or less evenly across the real-objects-from-Mario category, it's hard to draw conclusions.

Another explanation arises from noting that when asked to identify an image, the experimental group responded faster than the control group. I'd expect this, as time is a big factor in a platformer but not so much in a memory game. If you've just finished doing something for which you were required to make quick decisions, well it's no surprise that you might continue in that vein when you stop. If you spend less time assessing an image, could that make you more likely to make mistakes? It's a possibility.

Of course, none of this guesswork would have been necessary if the experimenters had simply *asked* the experimentees afterwards why they gave the answers they did. For most studies, this kind of anecdotal ontervention is impractical because you have to crunch the numbers before you find out what the differences are between the control group and the experimental group, by which time you have no access to the people involved to ask them why they responded in the way they did. However, with only 20 people in the experimental group, it wouldn't have hurt to ask *some* of those who did misclassify what their thinking was, just in case it did turn out they were more prone to giving the wrong answer. Sure, this would have been informal at best, and the children would have been unlikely to propose that they had created an autobiographical memory trace that had disrupted the fantasy/reality distinction. All the same, it could perhaps have helped to ground the speculation – or to hint at new possibilities that the authors hadn't considered.

Finally, I was interested in the results that came out of the recognition task. In this the children were shown images, some of which they had seen before and some of which they hadn't. They were asked to say which was which. This didn't form a big

part of the paper because the experimental group was no different to the control group: both were equally good (or bad) at knowing whether they'd seen an image before. OK, that's fair enough – but hold on a moment there: members of the control group were playing a *memory* game. Does this mean that memory games *don't* actually improve memory? That's a bit of a blow for the makers of brain training games.

I did quite like this paper, because although it was frustratingly narrow in scope it did discover something non-obvious that has interesting implications. I don't know what those implications *are* quite yet, but I'm looking forward to finding out[24].

TL;DR: children who have just finished playing a video game are more likely than those who haven't played it to identify the imaginary depictions in the game of real objects as being imaginary depictions of imaginary objects.

Aaron Drummond and James D. Sauer: *Daily Videogame Use and Metacognitive Knowledge of Effective Learning Strategies.*

This is a great topic for a paper! Many game designers harbour the belief that playing games makes people smarter[25]. Metacognitive knowledge of effective learning strategies is at the heart of smartness, so this is something that ought to grab their attention.

Well, perhaps it would have done so if Drummond and Sauer had found a positive link. In fact, they found a slightly negative link. To be fair, where game designers think games come into their own is in the area of problem-solving, an executive function which is just one part of what this study studies. Nevertheless, the overall result is still not really what they might have hoped to expect.

The natural (as opposed to the scientific) thing to do when a study comes up with the "wrong" answer is to try figure out *why* it's wrong – especially if you've built your career on the foundations that the research undermines. This is made altogether more difficult if the research is actually right. So, is that the case here?

Well the first thing to point out is that the survey size is measured in the hundreds of thousands and the participants were not self-selecting, so the findings can't be challenged on that basis. They have to be accepted as true. That said, asking the same questions to 470,000 15-year-olds in 65 countries is an expensive affair, so the exercise can't be undertaken lightly. There are only so many things you can ask a teenager before they get authority fatigue, so you have to make each assault on their attention span count. Many researchers bid for questions to be asked, but not all can be accepted. It's a bit like sending a probe to an interstellar body: there are lots of scientific experiments you could in *theory* do, but the available payload is limited.

Because of this, unfortunately we don't know what particular games the participants played: all we know is whether they were single-player or online multi-player. Also, we don't know how much time they spent playing them beyond "daily", "once or twice a week", "once or twice a month" or "never or hardly ever". Games differ greatly in both what they ask of the player and what the player gets from them. *Candy Crush Saga* may be something you can do to pass the time while sitting in a government committee meeting[26], but it's not the same thing as playing *Dragon Age: Inquisition* for 100 hours. Not having the level of fidelity to be able to disambiguate between the two, it could therefore be that some games are mind-numbing and others make savants of all of us, but if there are more people playing the former than the latter then the end result is a net loss in humanity's brainpower[27].

Although this explanation would help those game designers who believe that *their* games are the intellectual ones (which is to say all of them), it still doesn't alter the fact that a good many games would have to impact negatively on metacognitive knowledge in order for the survey to uncover the effect it did.

Another explanation makes an appeal to neuroscience. The way that metacognitive effects are considered by Drummond and Sauer (and, from what I can gather, by most other people working in this area) is at the experiential level. What matters is the conscious awareness that an individual has of their metacognition. Games are more of a process-oriented activity than a data-analytic one, though. Through play, gamers practice the skills they use so often that these become second nature. As a result, an individual may play exceptionally well but be unable to articulate in words what they are doing – a bit like the way that people who have internalised how to drive a car can use their driving skills expertly without necessarily being conscious of what exactly they're doing. The same is seen in mathematical prodigies who can calculate 83 to the power of 20 in under half a second[28] but who do not have much of a sense of how they do it. It could be that problem-solving skills picked up in games work the same way: gamers may possess them magnificently without being able to explicate them. It could also be that they don't have them immediately to hand, but they do have the means to create them at a moment's notice[29].

These explanations are all "could be", though. I suspect that the most apposite answer is that yes, daily video game use does indeed have a minor adverse effect on metacognitive awareness for many people, but as yet we don't know why.

TL;DR: gamers don't know *how* they think quite as well as non-gamers know how *they* think.

Paul J. C. Adachi and Teena Willoughby: *From the Couch to the Sports Field: The Longitudinal Associations Between Sports Video Game Play, Self-Esteem , and Involvement in Sports.*

The premise of this paper goes something like this: it's long been known that playing video games about killing people turns players into murderers, so does playing video games about sports turn players into athletes?

In order to find out, Adachi and Willoughby conducted a study of 1,492[30] adolescents over a 4-year period. This is a decent number of participants over a decent period, so their findings do carry authority.

Remarkably, Adachi and Willoughby report that no prior research has looked at the association between sports video games and sport involvement. They put this down to the field's obsession[31] with video games and aggression. I'm somewhat confused here, though. As I've already mentioned, plenty of sports are themselves downright violent; the video games about them tend to reflect this. The *Madden NFL* series, for example, routinely features armoured men body-checking one another to the ground and causing injuries. Given the level of activity in psychology-of-aggression research, it would be extraordinary indeed for there to have been no studies looking at the association of violent sports video games and aggression. It would also be surprising if none of these studies were extended to consider other possible effects that a connection would imply (such as actually taking up the featured sports). Nevertheless, I'll take the word of Adachi and Willoughby that this is the case. Tunnel vision is rifer than I thought.

Earlier research does show that playing sports video games predicts a higher sense of self-esteem, and (separately) that higher self-esteem predicts greater involvement in sports. This is one reason why Adachi and Willoughby specifically looked at self-esteem in their study[32]. However, the genre of sports games can't be the *only* one that predicts higher self-esteem, can it? Do first-person shooters? Strategy games? Casual games? Adventure games? Surely they must: why would people ever play such games if they consistently felt worthless afterwards?

Largely through no fault of the researchers themselves, studies such as this one rarely have enough controls. For reasons I suspect are to do with funding, other variables that may be important can't be tested. OK, so thanks to Adachi and Willoughby we now know that adolescents who play sports video games are more likely to participate in actual sports. What we don't know is whether this is because the video games are about sports, because they're video games, or because they're games in general. Would board games based on sports have the same effect as video games? Remember that sports are *themselves* games[33]: could it be that people who like games in general will try sports because sports are just another game? Right now, we can't say.

Adachi and Willoughby conclude by suggesting that "playing video sports games may help combat sports attrition during the adolescent period of development". Much as I like this rare glimpse of a positive finding about games, unfortunately I don't think they can justify going that far (even with the "may"). They're conflating prediction with causality. For all we know, it may be exposure to live sports, either on TV or in a stadium, that causes people to want to get involved in sport. They may start reading sports web sites, they may start playing sports video games, they may start following the Twitter feed of a sporting celebrity – all because they've *already* become interested. In that case, playing sport video games would be a symptom, not a cause. Getting people to play a sports game in order to encourage them to play sports would therefore be like getting people to wear a wedding dress in order to encourage them to get married. A predictor, no matter how good, is not a cause.

TL;DR: video games could well cause people to act out their fantasies in real life, but some of these fantasies are good for them.

Christopher J. Ferguson and Elly A. Konijn: *She Said/He Said: A Peaceful Debate on Video Game Violence*.

This is the final paper I read. From an external observer, it's wonderful stuff as it shows how psychologists think[34]. If I'd never read any papers in this field before, I might have been angry over coming to it last as it lays out the groundwork quite well. However, as I mentioned in my introduction, game designers do read Psychology papers; I am therefore merely despondent, rather than aggressive.

Posterity, on the other hand, may wonder why Ferguson and Konijn didn't state at the beginning on which side of the debate they were standing. I realise that they're both famous right now, but 50 years hence when this paper is read by the Committee for Truth and Reconciliation in Psychology, it won't become apparent to the reader who was on the winning or losing side until the fourth page.

The context of the debate as outlined in the introduction made some agreed-upon points that may surprise the external observer[35]. "The media violence debate often seems obfuscated by *à priori* fixed positions". This may be how it looks from within the field, but from without it looks as if there's only one position – the anti-

games[36] one – with anyone suggesting a different view dismissed as just another everyday, flat-Earth, climate-change denier.

Also, the debate is characterised as USA *versus* Europe, but as I mentioned when discussing Breuer *et al* I wouldn't say Europe was necessarily speaking with one voice here. The UK and Scandinavia are scared of guns (like the rest of Europe but not the USA) but are blasé about game violence (unlike both the rest of Europe and the USA). I was in Germany at the time of the 2011 Norwegian shootings, watching a bank of TVs in a hotel lobby as the story was being reported on BBC World, CNN and numerous German stations. The German stations pretty well led on the perpetrator's claim to be a *World of Warcraft* player; CNN mentioned it as a factor in his actions; the BBC only spoke about his Facebook page, with his game-playing habits meriting no attention whatsoever. So yes, the USA may be less stuffy about violent computer games than is, say, the Netherlands, but it's still far more concerned about the topic than is, say, Norway.

In order to conduct the debate convivially, a number of ground rules were set out at the start. It wasn't expected that either side would concede defeat, but it was nevertheless conceivable. So let's see: you had a contest, with rules, and an unlikely but possible victory condition. That's a game. You used email to conduct it? And read those emails on computer screens? That's a video game, then. It's lucky things didn't turn violent or the readership of the *Psychology of Popular Media Culture* would either have been imperilled or in no danger whatsoever.

The great irony here is that this discussion of whether or not particular media effects exist is itself founded on a media effect. These things always are. Concerned parents become worried about their children's taking part in some new medium of entertainment which they, the parents, don't understand; newspapers and other documentary media pick up on this and feature stories on the subject from the more extreme end of the spectrum; this causes more people who don't understand what's going on in ~~vaudeville~~ ~~radio~~ ~~movies~~ ~~TV~~ ~~comic books~~ ~~rock music~~ ~~videos~~ ~~the Internet~~ video games to worry, which leads to calls for the phenomenon to be investigated. Thus, the funding supporting the research of most readers of this journal itself derives from a media effect. Don't you love recursion?

Regarding the discussion itself, well multivariate causality seems to be a big issue but it rather misses the point. Media violence isn't the *only* cause of aggression, sure, and no-one says it *is* the only cause; however, you really need to consider your presentation. If someone publishes a paper that *only* states computer games cause aggression, well it looks as if they're implying that *only* computer games cause aggression – at least in comparison to other leisure activities. Imagine if a newspaper ran the headline "BLACK MEN STINK!". That's a majorly racist claim – and a sexist one, too, come to that. It can't possibly be true! Yet when you look at the research, you find that it *is* true: every adult human being gives of waves of powerful pheromones and can therefore be said to stink. So yes, black men do stink, but so does everyone else on the planet. That's not how the headline came across, though. So when you pat yourself on the back for discovering yet again that violent computer games cause aggression, OK so you aren't saying that other factors *don't* cause aggression – but it's going to be interpreted that way[37].

Casual observation: the larger the study, the less dramatic the results.

I was intrigued to see the discussion turn to points regarding the validity of early results on violence and video games. There is some suggestion that these results may no longer be valid because they were "carried out within the perceptual context of that time". We don't think *Pac Man* and *Centipede* are violent video games now, but we did back in the day; therefore, the fact that the aggression effects from playing

Pac Man in 1986 are on a par with those from playing *Grand Theft Auto V* today can be explained by the mere passage of time. Well that does sound eminently plausible, yes, but actually it's more than that: it's testable. You can repeat those earlier studies today and see if they *do* have the same results. If so, then *Pac Man* remains every bit as dangerous as *GTA* (and *vice versa*); if not, well the implied-meanings-may-change hypothesis has more weight to it.

From what Ferguson and Konijn say about the term "violent video game", it's as if it's treated as a binary concept. A game can't be a little bit violent or a lot violent: it either is violent or it isn't. This makes little sense. Take a game and add a drop of violence. Add more drops. Keep adding more drops. Can it really be true that the game remains resolutely r=0 until you add one drop too many whereupon it becomes r=.15? Is it the same drop that does it for everyone, or are some people more tolerant than others? This should be a smooth curve, not a step function, but everyone seems to treat it as a step function.

I have to say, I'm alarmed to discover that noise blast experiments are not only allowed but are in not infrequent use. If I'm reading Ferguson and Konijn right, it seems that the experimenter tells someone that what they can do will permanently damage someone else's hearing, then they give them a chance to do it. The fact that it won't *actually* hurt anyone much makes it OK.

Well no, it's not OK, and for three reasons.

Firstly, if you told someone their mother had died and she hadn't, that person would be upset. The detail their mother hadn't died is irrelevant: you shouldn't have betrayed their trust like that. Telling someone they can damage someone else's hearing is going to have an emotional effect on people. Revealing to them afterwards that they were the victim of a practical joke is not going to undo that effect. Experienced game designers avoid this kind of prank, as they don't consider it fair on the players. Who would have thought they were more ethical than psychologists?

Secondly, if you get someone to participate in this kind of study then you have a set of individuals who are either a) extremely gullible; b) sadistic; or c) playing you for a fool, because they know that *no way* is what you claim will happen actually going to happen. Whatever, your subjects aren't exactly going to be representative of the general population, who would be shocked to be asked to do something that could burst an eardrum in the name of science.

Thirdly, could it be that believing it's OK to blast people to the brink of deafness is itself something that might make an individual more aggressive? Before, I was a mild-mannered accountant; now, I am empowered! Hurting people is fun! I'm going to go out there with my ghetto-blaster to blast some ghettos!

Overall, though, this was an informative discussion. I don't know whether the target journal for the resulting paper was decided at the outset of the exchange, but either way one of the interlocutors was going to end up in the lion's den; as it turned out it was Konijn, but both participants acquitted themselves well. I'd suggest a similar idea for a Games Studies paper, except that as the authors would be gamers it would require a referee.

TL;DR: you say tomato, I say tomato.

So, what general points can I raise from my game designer's reading of these papers? Are there common themes that are worth addressing? What are the implications for the future direction of research in this area?

Let's begin with a summary of the themes.

1) What psychologists say about games and aggression doesn't measure up with the experience of those who play them. It measures up against what those who don't play games believe about them, but that's not the same thing.
2) What game designers and developers believe about the positive effects of games isn't necessarily true. Some of their anecdotal evidence is mere wishful thinking.
3) The world is moving faster than is research. A 5-year longitudinal study of the players of video games is like a 5-year longitudinal study of the characters in *Game of Thrones*: the conditions at the start are not the same as they are at the end.
4) Games are too often studied in isolation. Are games better or worse than TV for causing aggression? Is someone who has just spent 15 minutes playing rugby less aggressive than someone who spent the same period playing *GTA*? If you don't know the answer and games-and-aggression is your research field, why *don't* you know it?
5) Games are not all the same, but all too often they are treated as if they were. In some studies, we're told *that* people play games but we're not told *what* games. It's as if they're all regarded as being the same. Replace the words "video game", "play" and "player" by "book", "read" and "reader" and you'll soon see what I mean. Children who read books become aggressive? *Which books?!*
6) Different parts of the world are more uptight about games than others. Germany and the USA are more worried about the possibility that games can cause violence than are Scandinavia and the UK[38].
7) The cavalier way that the term "game play" is used demonstrates a lack of understanding of what it is. I did have a 700-word explanation for you, but it wouldn't really work as a summary point so I deleted it. Basically, though, gameplay emerges from tokens, rules and features, not from the game's fiction. It's process-based, not label-based. This is important, because an understanding of what it actually *is* could open up new areas to research.
8) Game psychologists have an image problem among games developers, mainly because of the image problem the games developers among psychologists. They may accept some of your games-are-addictive findings (especially if these indict a company they despise), but they regard your games-cause-aggression findings as lacking any credibility whatsoever.

As for the implications of all this, well I see this special issue of *Psychology of Popular Media Culture* as something of a watershed. Markey, Markey and French have said in a journal what non-academics have been saying for years. Really, folks: if you still hold to the belief that violent video games cause enough aggression that researching the topic remains valuable, you really *do* have to explain why this hasn't been reflected by an increase in aggression in society.

Furthermore, you have to do it in the teeth of Breuer *et al*'s finding that playing violent video games does not predict physical aggression. When it comes to video games, the GAM is wrong. All those studies that show a short-term aggression increase after playing a video game remain valid, but the model they are using to predict long-term effects is invalid. This is true even though under point 3) above the effects of games should have been magnified (as they increased in general popularity during the period of the longitudinal study).

You also have to explain why, if you're so concerned with media violence, you're looking specifically at video games. Linebarger, DeCamp and Surette and Maze all look not just at video games but at other media, too. Rather tellingly, none of them found anything especially remarkable about video games in their (rather

different) contexts. Are you bullying video games because they're the new kid on the block, or do they really have it coming?

Research is a little like mining. You stake a claim, you dig a hole, you find a precious metal, you dig some more. Other people do the same. You might find a large seam, there may be smaller side seams. You keep following a seam until it's mined out, then you look for another seam. The research on video games and aggression has been following one main seam that has little worthwhile ore remaining. You're not going to get rich mining it. Seriously: do you believe that 10 years from now people will really care about what you dig up – even your fellow miners? They're busy looking for better nuggets elsewhere.

I have one final point that I'd like to mention, something that hasn't cropped up already.

Suppose that you are Dr Jeckyll and you have concocted a new potion that you suspect may turn people into monsters. It doesn't work on animals, only on people. In order to test it out, you either take it yourself (which would be a bad move if it did indeed turn you into a monster, but would probably make for a good story) or you get a bunch of scruffs off the street, have them sign a consent form, then furnish half of them with the potion and half of them with beer. If more of the ones who drink the potion turn into monsters than the ones who just drink the beer, your hypothesis has some evidential basis of being correct.

Except, of course, you will have *created several monsters*.

If you truly believe, in your heart of hearts, that playing video games is harmful, *why on earth* are you experimenting on people – especially children?! You risk turning them into monsters! Oh, but the potion wears off and they turn from Mr Hyde back into Dr Jeckyll again afterwards, it's only *repeated* exposure that's harmful. Fine, except your colleagues in the League of Extraordinary Gentlemen are telling you that your potion is also addictive. You are knowingly putting people in harm's way possibly for a lifetime: don't you feel in any way uneasy about that? Isn't it irresponsible to do what you're doing time and time again? Or is it that maybe, deep down, you sense that actually the potion is really pretty harmless to most people, so you don't have to worry that little Billy from experimental set A might as a result of your showing him the joys of violent video games go on to shoot up his school ten years later?

I've greatly enjoyed this opportunity to write a discussant comment. I got to read a selection of very interesting papers and was provided with a valid excuse to put off writing four examination papers. I hope I haven't offended too many people[39] in doing so: I've simply tried to give my own, honest opinion, in the hope that it may prove useful. If anything does make you angry, count to 10 before acting on it; it's just a short-term effect.

TL;DR: all the world's a game, and all the men and women merely players.

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[1] I figured that this would probably make me appear marginally less evil than: "If this journal's readership consisted primarily of social workers, I'm the local drug pusher" – even if for some researchers that's the more accurate analogy.
[2] I'm not kidding: those are actual examples.
[3] Except when it comes to game design itself, obviously. Then, they're analysing the bark for insects like every other specialist.
[4] And four times as long as I was planning to make it.
[5] Admit it, you know it's true.
[6] This means "too long; didn't read". Given that one of the topics of this special issue is youth culture, you ought to know this already without my having to explain it. If it's not something you've come across before, I apologise for the feeling of unease you're now experiencing.
[7]Which is to say everyone else except politicians, media pundits and those who believe them
[8] Indeed, it may well have *been* asked; as this isn't my field, I'm afraid my ignorance is showing here…
[9] Not typically games that professional game designers approve of, I should add.
[10] Which is to say antisocial scientists.
[11] Ideally, at this point I would direct you to a paper that did just this. However, unfortunately I don't know any. The allegation may, therefore, be made up – although I've heard it reported multiple times, so am inclined to believe it isn't. The more modern way that requests for data are rejected is more neutral, citing commercial sensitivity and trade secrets. Thus, if you do manage to prise anything from a developer's hands, it may well be at the expense of their having a veto on what you submit for publication regarding it.
[12] This is why people like the Mayans and physicists make predictions that the world will end so far in the future that they'll be long dead before their assertions can be tested against reality.
[13] The code-word for this is "hypothesise".
[14] In which case the word "socialised" isn't a great one to use.
[15] OK, so maybe I will. In a (game-style) negative feedback loop, the further you are ahead, the harder it becomes to remain ahead; this makes it easier for those behind to catch up. For example, the more you expand your territory in *Crusader Kings II*, the more difficult it is to hold. In a positive feedback loop, the further you are ahead, the easier it becomes to remain ahead; this makes it harder for those behind to catch up. The dominant presence of a positive feedback loop in *Monopoly* is the primary reason that so many people dislike the game. Applying this view of negative feedback to the Downward Spiral model, then, the implication is that the more violence you seek out then the harder it gets for you to become more aggressive. Eventually, you will reach a limit: you can't become more aggressive because you'd have to play the whole time just to maintain your current level of aggression. It seems unlikely that this is what the Downward Spiral model is trying to say, though. I therefore invoke cross-disciplinary confusion as a defence.
[16] For standard social science definitions of "reasonable" and "sound".
[17] The survey's respondents, this is, not Surette and Maze.
[18] MMO is an abbreviation for MMORPG, which itself is an acronym for Massively Multi-Player Online Role-Playing Game. The study of video games is such a young field that its proponents have yet

to learn that if you want to create an acronym, you shouldn't make it so long that it has to be abbreviated before people will use it.

[19] Most informal surveys come up with this kind of number and have done since the pre-graphics era. Academic ones tend to vary more, but the numbers are always fairly substantial. Roberts and Parks have it at 40% (Roberts *et al*, 1999); Nick Yee put it at 23% (Yee, 2005); Hussain and Griffiths have it at 57% (Zaheer *et al*, 2008), but you might want to get in touch with me before you cite that last one…

[20] Try it yourself: http://www.loodo.com.br/2008/09/calabouco-tetrico/ .

[21] I should perhaps mention that to a gamer, the adjective "fantasy" refers to a particular, Tolkienesque genre, rather than being a nuanced version of "imaginary". Fortunately (or unfortunately, depending on how you look at it), this paper doesn't produce any comical misunderstandings from this difference in usage.

[22] They left it late to say so, but at least they *are* aware of it – unlike a good many authors in the field for whom the possibility that other media may have similar effects never enters their heads.

[23] I did mention that I was a game scholar, didn't I?

[24] I realise that I'm ending a sentence with a preposition here, but using "out of which to finding" is a step too far.

[25] This is not entirely because they regard themselves as smart and they play games.

[26] "Tory MP admits playing *Candy Crush* during committee". http://www.bbc.co.uk/news/uk-politics-30375609

[27] It could also be that something a few of the non-gamers are doing has a disproportionate effect, but this seems unlikely given the number of people studied.

[28] In German. https://www.youtube.com/watch?v=KS-qcISU2ec

[29] Science Fiction author Isaac Asimov never bothered to remember Newton's laws, because whenever he needed to use them he worked them out from first principles.

[30] This is written as "1492" in my review copy, presumably because Christopher Columbus paid for a product placement.

[31] My word, not theirs (they talk about "focus").

[32] Fortunately, self-esteem is one of the few things people can self-report that you can actually trust to be accurate.

[33] I say "remember", but am regularly impressed by how many people have never considered that something like soccer is a game – even when they've just said they "watched the game last night".

[34] The psychology of psychologists appears to be much the same as the psychology of real people. Programmers are always warned against writing self-modifying code as it's a devil to debug, so this is perhaps as well. Otherwise, all psychologists would need psychiatrists.

[35] That would be me, then.

[36] Or I guess you could call it "pro-peace" if you wanted to make your negativity appear positive.

[37] Especially if you did the equivalent of only looking at black men.

[38] Here in the UK, we attribute violence to binge drinking. It seems that if you take away the capacity of the brain to use its higher-order functions, there is much opportunity for people to develop misunderstandings.

[39] Especially ones with lawyers.