“Clinically significant distress” in Internet Gaming Disorder: An individual participant meta-analysis

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Abstract

Although the validity of internet gaming disorder (IGD) in the DSM-5 hinges on a relationship between 5 or more IGD symptoms and “clinically significant impairment and/or distress”, to date most studies have focused on *statistical* significance. To address this, we conduct an individual participant meta-analysis comprised of primary data from 15 studies ($n = 38,851$). Study 1 finds that meeting the DSM-5’s proposed 5/9 diagnostic threshold is associated with $d = .65$ greater distress across 21 well-being constructs, which exceeds a conservative anchor for clinical significance. However, we also find that classifying participants above and below the 5/9 threshold has little power to reject that threshold: a 2/9 cut-off predicts similarly large differences. Study 2 shows that dimensional (continuous) modelling of IGD scores offers a more severe test of the proposed threshold. Finally, study 3 reveals that three criteria—preoccupation, tolerance, and loss of control—are roughly half as predictive of distress as withdrawal and escapism, highlighting limitations also present in dimensional modelling and suggesting the need to modify or remove these. In sum, we do not find evidence for invalidating IGD as proposed, but do identify issues with threshold-based categorization, inadequate differentiation between statistical and clinical significance, and the inclusion of potentially flawed criteria. We argue that these are possible negative consequences of a premature switch to confirmatory research on IGD.

Keywords: Internet Gaming Disorder, individual participant meta-analysis, DSM-5, distress, specification curve analysis

1. Introduction

For decades, both the public and scientists have been concerned about people developing problematic or pathological relationships with gaming (King & Delfabbro, 2019). In response to this, the American Psychiatric Association defined internet gaming disorder (IGD) prospectively in the DSM-5 as a condition for future study. Shortly thereafter, the World Health Organisation included “gaming disorder” in the ICD-11 (World Health Organization, 2018). The two conditions share significant conceptual overlap, but due to the additional specificity of its diagnostic criteria and threshold, IGD has seen greater proliferation of self-report measures used to categorize players as having probable IGD in research (King et al., 2020), and forms the basis of the current paper.

IGD is defined as “persistent and recurrent use of the Internet to engage in games, often with other players, **leading to clinically significant impairment or distress** as indicated by five (or more) of the following [nine criteria] in a 12-month period” (American Psychiatric Association, 2013, p. 795, emphasis added). The DSM-5 states that “Internet gaming disorder most often involves specific Internet games, but it could involve non-Internet computerized games as well” (p. 796). On this basis, throughout this paper we use this definition to refer to disordered gaming regardless of whether games are online or offline, omitting the word “internet” where necessary. This aligns with the vast majority of measurement tools, which tend to assess IGD without specifying internet games in the item wordings (King et al., 2020), as well as with the ICD-11, which differentiates predominantly offline and predominantly online as sub-types of a single gaming disorder condition.

The aforementioned nine DSM-5 criteria, originally adapted from the criteria for pathological gambling in the DSM-IV

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(in turn adapted from substance use disorders) are:

- **Preoccupation:** thinking about previous gaming activity, anticipating future play, gaming as dominant activity in one's life
- **Withdrawal:** irritability, anxiety, or sadness felt when gaming is taken away
- **Tolerance:** a need to spend increasing amounts of time engaged with gaming
- **Loss of control:** unsuccessful attempts to control the participation in games
- **Loss of interests:** a lack of desire to engage in previous hobbies and entertainment besides games as a result of games
- **Continuation despite problems:** ongoing excessive play despite awareness that it is leading to psychosocial problems
- **Lying/deception:** deceiving family members, therapists, or others regarding the amount of gaming
- **Escapism:** use of games to escape or relieve a negative mood (e.g., feelings of helplessness, guilt, anxiety)
- **Problems/jeopardization:** jeopardization or loss of a significant relationship, job, or educational or career opportunity because of participation in games

The emphasized portion of the APA's definition of IGD is the crux of this prospective disorder's validity as a psychopathological condition. "Persistent and recurrent use of the internet to engage in games" does not on its own warrant a clinical diagnosis, given a growing body of evidence suggesting that the amount of time players spend playing is not a meaningful predictor of well-being (e.g., Vuorre et al., 2021; Johannes et al., 2020; Skoric et al., 2009). Related work has shown that not all highly-engaged players will exhibit signs of impairment (Griffiths, 2010; Van Rooij et al., 2011; Deleuze et al., 2018), leading to repeated calls for more work on differentiating high-engagement vs disordered patterns of play (Billieux et al., 2019; Deleuze et al., 2017; Ferguson et al., 2011).

Although gaming is not inherently problematic (and may even confer well-being benefits; Granic et al., 2014), gaming associated with the DSM-5 criteria seems to be: numerous studies have found that IGD scores are associated with various forms of impairment/distress. For example, IGD has been linked in cross-sectional studies with greater likelihood of skipping school and worse grades (Rehbein et al., 2015), depression and loneliness (Montag et al., 2019; Marmet et al., 2018; Traş, 2019), social problems (Müller et al., 2015), self-esteem (Bányai et al., 2019; Laconi et al., 2017; Van Rooij et al., 2014), and obesity (Ko et al., 2020).

To date, however, this substantial body of research has largely concerned itself with *statistical* significance; less well-understood is whether these associations with impairment/distress are of *clinically* significant magnitude, and whether the current diagnostic threshold of 5 of out 9 endorsed criteria aligns with impairment of such magnitude.

In this paper, we seek to subject the DSM-5 5/9 threshold, and the criteria upon which it is based, to a test of falsification in relation to the notion of clinical significance. In this light, it is crucial to state that this study is incapable of validating IGD as a psychopathological condition, or of validating a particular IGD threshold. An association with impairment/distress, even if it is of clinically significant magnitude, does not imply that IGD is causally implicated in that impairment. To demonstrate this, longitudinal data are needed; such studies remain relatively rare, and paint a complicated picture about stability, causality, and the clinical magnitude of effects (Weinstein et al., 2017; Scharkow et al., 2014). In addition to these, some scholars argue that any validation of IGD would need to address concerns about a premature switch to a confirmatory research approach as opposed to exploratory (of which the current paper is itself an example) and the quality of the evidence base that led to its proposal (Aarseth et al., 2017; Colder Carras et al., 2020, but see Rumpf et al., 2018 for an opposing view).

Instead, our goal in study 1 is to test the DSM-5's implicit assertion that meeting the 5/9 threshold for IGD causes clinically significant impairment/distress by looking at this relationship contemporaneously. A lack of meaningfully large cross-sectional differences would likely preclude any further causal relationship and appear to invalidate IGD as proposed. In study 2, we investigate an alternative to threshold-based approaches to IGD assessment. Finally, in study 3, we attempt to understand these relationships at a more granular level by investigating the association between each DSM-5 criterion and distress.

1.1. Flexibility in the Assessment of IGD and Distress

While IGD research has not yet established a consensus "gold standard" method of clinical diagnosis, gold standards in other psychiatric disorders near-universally involve clinicians' evaluations, sometimes in conjunction with (semi-)structured interviews and/or self-report measures (Aboraya et al., 2005). Given the recency of IGD's proposal, however, results of clinical assessment in line with the DSM-5 comprise a relatively small portion of the literature (although for counter-examples see e.g., Starcevic et al.; Granero et al., 2021). Instead, both IGD and distress are often measured using self-report measures alone, and the measurement and modelling of these is subject to substantial analytical flexibility.

First, researchers have the choice to model IGD categorically or dimensionally (continuously). Previous work has pointed out validity problems resulting from categorical diagnoses of what are likely dimensional constructs,

leading to the incorporation of dimensionality into a select number of psychopathological conditions in the DSM-5 (Strickland et al., 2019; Adam, 2013). While categorical diagnosis for individuals is, at least for the time being, necessary for many health services (Chmura Kraemer et al., 2004), this is often not the case in research studies, and risks discarding information (Royston et al., 2006). Despite this, dichotomizing participants into probable/non-probable IGD groups based on the DSM-5’s 5/9 threshold remains a common practice in the literature, often in pursuit of estimating prevalence (Oka et al., 2021; Mihara & Higuchi, 2017). A somewhat more complex and exploratory variation of dichotomizing participants is the use of latent class analysis to identify subgroups empirically, rather than using IGD’s proposed cut-off (Colder Carras & Kardefelt-Winther, 2018; Faulkner et al., 2015).

Challenges associated with categorical assignment are compounded when dichotomization is undertaken with polytomous scales (e.g., 5-point Likert scales), which are common in IGD research, despite DSM-5’s dichotomous approach to criteria endorsement (King et al., 2020). While in some cases scale developers state an intended threshold for classifying an indicator as endorsed (e.g., ≥ 4 on a 5-point scale), the basis for this is not always clear, and many other scales do not have such information at all—leaving researchers to heuristically select their own thresholds. Variations in how polytomous scores are handled can cause substantial variation in the estimated prevalence of IGD and other types of problematic technology use (Connolly et al.).

While earlier iterations of disordered gaming were sometimes measured monothetically (i.e., all criteria must be endorsed for a probable diagnosis) (Lemmens et al., 2009)—an approach still adopted for other psychiatric conditions such as autism’s social communication symptoms (Lohr & Tanguay, 2013)—the DSM-5 and the majority of work in its wake adopt a polythetic model (i.e., a subset of any 5 of 9 criteria must be endorsed). This point lives on, however, when more than one indicator per criterion is present in a scale—for example, the IGDT-10 has items assessing whether players have jeopardized 1) a significant relationship and/or 2) school or work performance due to gaming, both mapping onto the DSM-5 “problems” criterion. Analysts have the choice to take the single highest-scoring item when assessing whether a criterion is present, or to take the average of all the items in that subscale.

Finally, there are a wide variety of variables that can be measured to establish links between IGD and impairment/distress, such as need satisfaction (Allen & Anderson, 2018), life satisfaction (Mills et al., 2019), externalising problems (Przybylski et al., 2019) and more. In the secondary data used in this paper, no fewer than 21 different aspects of well-being were measured across the various studies, which can make straightforward comparison challenging.

In sum, these aspects of analytical flexibility create difficulties interpreting and synthesizing IGD research. While

in most cases, relationships between IGD and distress have been statistically significant regardless of these analytical choices, their impact on the size of effects (i.e., their clinical significance) and the conclusions that may be drawn has not been directly explored, leaving an incomplete picture of the robustness of IGD’s evidence base.

1.2. Clinical Significance

The ability to conduct these tests relies on the ability to map a definition, or theoretical claim—“clinically significant”—onto a statistical one. Many scientists will be familiar with Cohen’s anchors for small, medium, and large effects, for which he suggested Cohen’s $d = .2$, $.5$, and $.8$, respectively (Cohen, 1988). However, these were explicitly intended for use only when researchers had no other information upon which to base their interpretations. Subsequent researchers have attempted to provide more meaningful guidelines for clinical contexts. These are closely related to the notions of minimally important differences (MIDs), practical significance, and the smallest effect size of interest.

A prominent review by Norman et al. (2003) found that previous estimates of clinical MIDs tended to converge around half a standard deviation, or Cohen’s $d = .5$. This estimate has subsequently been used as an anchor for research on “screen time” (Przybylski et al., 2019). Cuijpers et al. (2014) use a patient-centered technique connecting questionnaire scores to a utility function (U) ranging from 0 = death to 1 = perfect health. They find that patients report a U of approximately .04, equivalent to $d = .24$, as a meaningful change in their own health. Ferguson (2009), citing two previous suggestions (Franzblau, 1958; Lipsey & Hurley, 2009), tentatively suggests a minimum of Pearson’s $r = .2$ for practically significant effects in media effects research, equivalent to $d = .41$. However, these studies largely focus on interventions, and this is a key difference: what patients perceive as a meaningful change in their health is likely smaller than the divergence from normal functioning necessary to warrant its own mental health condition. A standardized mean difference of $.5$ might therefore be useful for interpreting for *practically* significant negative consequences associated with gaming, but not necessarily *clinically* significant consequences meriting their own disorder.

Indeed, evidence suggests that Health-Related Quality of Life (HRQoL) often differs between patients with and without mental health condition diagnoses by larger amounts. One meta-analytic study found that patients with a Generalized Anxiety Disorder differed from controls by $d = 1.35$ (Olatunji et al., 2007). Studies of children and adolescents diagnosed with ADHD have found differences in HRQoL of $d = .72$ and $d = 1.05$ compared to a normative sample (Hampel & Desman, 2006; Varni & Burwinkle, 2006). Studies of IGD’s most conceptually similar condition, pathological gambling, have found differences between pathological gamblers and normative samples ranging from $d = .47$ (Ekholm et al., 2018), to

$d = .99$ (Black et al., 2013) to $d = 1.48$ in a twin study (Scherrer et al., 2005). We expressly do not use previous IGD research to anchor minimal differences to avoid circular reasoning: if our minimally important difference value corresponds to what has been reported in previous IGD research, it is likely that subsequent research would also find similar effect sizes.

We argue that the definition of—and diagnostic threshold for—IGD should be informed by this previous work. The estimates above are all similarly plausible, and any choice of MID has a degree of arbitrariness; however, in the absence of research to justify larger minimal differences, we choose to err on the side of caution, and select the smallest justifiable estimate so as to avoid setting an unreachable or unfair standard for IGD. In the work reviewed above, the smallest reported differences between people diagnosed or meeting diagnostic criteria for a DSM-5 condition were approximately $d = .5$; given that this also aligns with a body of work on MIDs (Norman et al., 2003), we adopt this effect size as the threshold for clinically significant impairment in the current study. However, we note that this $d = .5$ threshold is a conservative one, and IGD researchers should continue to reflect upon how to align clinical language and statistical effects.

1.3. Present Research

As outlined above, the IGD literature suffers from an incomplete understanding of the impact of categorical diagnosis, a potential conflation of statistical and clinical significance, and a lack of information about the relative role of the 9 criteria. Drawing on these challenges, the present research attempts to address the following two research questions:

- RQ1: Does endorsement of 5 of the 9 IGD criteria by an individual correspond to a difference in impairment or distress of clinically significant magnitude (operationalized as $d = .5$) when compared to individuals not meeting this threshold?
- RQ2: Do all nine proposed individual IGD symptoms meaningfully contribute towards associations with impairment and/or distress?

To address these research questions in as robust a manner as possible, we conducted an exploratory individual participant meta-analysis (Riley et al., 2010) using a large set of secondary data from 15 prior studies which dealt with the relationship between disordered gaming and impairment or distress ($n = 38,851$). In brief, this approach consists of processing the raw data from a set of existing studies into a consistent format in order to address a particular research question that may or may not have been the subject of the original paper. We note that our datasets comprise a non-random sample of the total data in the literature, as it was not feasible to request data from every relevant study, and not every approached author agreed to share data.

2. Study 1

The DSM-5 specifies a threshold of 5 out of 9 endorsed criteria to receive an IGD diagnosis. This 5 out of 9 threshold has been supported for a number of self-report measures in psychometric studies (Király et al., 2017; Van Rooij et al., 2017), and has also shown support in clinical interviews (Ko et al., 2014).

However, diagnostic thresholds using self-report measures have at times been criticized (e.g., Dong et al.). One study of Korean adolescents found that 44% of participants who met the threshold for IGD on a DSM-5 self-report measure were not given the same diagnosis after a diagnostic interview with clinicians (based on the same 9 criteria); a further 10% of participants were diagnosed by clinicians but not the self-report measure (Jeong et al., 2018). By comparison, the majority of problem gambling measures were found to have false positive rates of $\leq 20\%$ and false negative rates of $\leq 15\%$ when validated against clinical interviews (Dowling et al., 2019).

As mentioned, only limited research on the 5/9 threshold has used “ground truths” of clinically significant impairment/distress external to the criteria themselves. Without this, there is a risk that the evidence supporting the 5/9 threshold, and by extension IGD as proposed, is a product of circular reasoning. In study 1, we attempt to evaluate whether the 5/9 threshold—and, for comparison, other possible thresholds—correspond to clinically significant distress as measured by a range of well-being variables.

2.1. Method

2.1.1. Data

The data in this study is a composite dataset consisting of relevant data from 15 previous studies (Table 1). This data was drawn from 2 places: 1) studies regarding IGD and well-being that had been previously shared publicly on the Open Science Framework ($n = 4$ studies); 2) a search of the literature and subsequent requests for data sharing. Because we reanalyze existing data and no identifiable information was shared, this study was not required to undergo independent ethical review. Each of the studies in the combined dataset received ethical approval from their authors’ institutions.

Our search criteria specified that data should include an IGD measure as well as one or more well-being indicators (e.g. depression, anxiety, life satisfaction). To restrict the scope of this search and the ultimate burden of integrating each study’s data, we first searched all papers which cited the Internet Gaming Disorder Scale (Lemmens et al., 2015), a popular scale for measuring IGD symptoms. Of these papers, we identified 39 that incorporated measurement of one or more well-being constructs and IGD symptoms (either all 9 criteria or a subset of them). Studies that did not include all 9 DSM criteria were requested for use in study 3 below (study 3 was originally intended to be the main analysis), but are not used in studies 1 and 2.

Table 1: Summary of aggregated studies

Study	n	Includes all 9 DSM-5 Indicators	IGD Measure	Scale mat	For-	Well-being measures
Allen & Anderson (2018)	315	No	IGDS-27 (Lemmens et al., 2015)	6-pt		life satisfaction, loneliness, self-esteem, psychological well-being, physical well-being, relational well-being, need satisfaction, need frustration
Bányai et al. (2019)	5623	Yes	IGDT-10 (Király et al., 2017)	3-pt		self-esteem, depression, anxiety, psychiatric distress
Király et al. (2019)	12878	Yes	IGDT-10 (Király et al., 2017)	3-pt		depression, anxiety, psychiatric distress
Laconi et al. (2017)	418	Yes	IGDT-10 (Király et al., 2017)	3-pt		self-esteem, depression
Marmet et al. (2018)	5516	No	GAS-7 (Lemmens et al., 2009)	5-pt		stress, social anxiety, bipolar, hyperactivity, depression
Mills et al. (2018)	1689	No	IGDS-27 (Lemmens et al., 2015)	6-pt		need frustration
Mills et al. (2019)	1989	No	Combination of (Charlton & Danforth, 2007) and (Petry et al., 2014)	5-pt		need frustration, life satisfaction, positive/negative affect
Montag et al. (2019)	1429	Yes	IGDS9-SF (Pontes & Griffiths, 2015)	5-pt		depression, loneliness, attention problems
Przybylski & Weinstein (2019)	1004	Yes	9 items from (Przybylski et al., 2017)	binary		prosocial behavior, externalising problems, internalising problems, need satisfaction, need frustration
Stevanovic et al. (2020)	3382	Yes	ICMH-IGD (Stevanovic et al., 2020)	5-pt		depression, anxiety
Tras (2019)	430	Yes	IGDS9-SF (Pontes & Griffiths, 2015)	5-pt		loneliness
Van Rooij et al. (2014)	4519	No	VAT (Van Rooij et al., 2012)	5-pt		depression, social anxiety, life satisfaction, self-esteem, loneliness, internalising problems, externalising problems, prosocial behavior
Weinstein et al. (2017)	5777	Yes	9 items from (Przybylski et al., 2017)	binary		need satisfaction, physical well-being, relational well-being, psychological well-being
Yam et al. (2019)	642	Yes	IGDS9-SF (Pontes & Griffiths, 2015)	5-pt		depression, anxiety
(Zendle et al., 2020)	2664	No	IGDS-9 (Lemmens et al., 2015)	binary		depression, anxiety

To broaden our dataset with more studies that measured all 9 indicators, we then reviewed a recent systematic analysis of gaming disorder measures (King et al., 2020) and selected two of the most prominent, namely the IGDS9-SF (Pontes & Griffiths, 2015) and the IGDT-10 (Király et al., 2017). We looked at studies citing these measures with respect to our inclusion criteria. This analysis resulted in a total list of 82 corresponding authors and associated studies. These authors were all emailed with a request for data access.

From the 82 data requests, 53 did not respond to an initial email (10 were no longer contactable via their listed correspondence addresses), and 29 did. Of the 29, 7 requested further details of the study or the requested data and then ceased contact after follow-up, and 6 specified that further permissions would be necessary prior to sharing and then either forwarded the authors to unresponsive colleagues or ceased contact themselves. A further 4 cited ethical/consent concerns in stating that sharing was impermissible for their study, 1 specified a university-wide ban on data sharing, 1 requested coauthorship on any pub-

lication in return for data sharing, and 1 was temporarily unable to gain access to the data at the time the study was taking place. In total, 9 authors shared data. Two authors shared an additional relevant dataset separate from the one that was originally requested, resulting in our final total of 15 datasets.

The data for each study was processed into a consistent structure, with columns for each of the IGD criteria, all well-being variables, and control variables age and gender. Details of the particular IGD measure used in each study and the wording of the items in that measure can be found in the supplemental materials on the OSF (<https://osf.io/gvdf4>).

2.1.2. Measures

The IGD measures and well-being constructs used in each study are summarized in Table 1. In total, 8 measures of disordered gaming criteria were present in the data, not all of which map onto DSM-5 criteria equally well (e.g., the IGDS-27 includes “problems” and “conflict” subscales, which may be considered part of the DSM-5’s “problems/jeopardization” criterion, but lacks items for

Table 2: Details of methods used to dichotomize polytomous responses

Scale	Format	Lax	Strictness Strict	Strictest
2-pt		2 (“yes”)	2 (“yes”)	2 (“yes”)
3-pt		≥ 2 (“sometimes”)	3 (“often”)	3 (“often”)
5-pt		≥ 3 (“sometimes” or “moderately agree”)	≥ 4 (“often” or “strongly agree”)	5 (“very often” or “very strongly agree”)
6-pt		≥ 4 (“somewhat frequently” or “about once to three times a month”)	≥ 5 (“very frequently” or “once or more a week”)	6 (“almost always” or “every day or almost every day”)

“continuation despite problems”). To determine which criteria were present in each scale, we used the coding from King et al.’s 2020 systematic review of IGD measures. Further details about the precise well-being measures used in each study can be found in the supplemental materials.

2.1.3. Dichotomization

The DSM-5 adopts a binary approach to symptom measurement and diagnosis: symptoms are either endorsed or not endorsed; a sum of 5 or more endorsed symptoms classifies one as a disordered gamer or a non-disordered gamer. However, many existing IGD measures, including many of those in our composite datasets, do not use binary response formats. For example, in Allen & Anderson (2018), each symptom is rated on a Likert scale from 1 (“never”) to 6 (“every day or almost every day”).

To assess IGD in line with the wording in the DSM-5, we therefore chose to dichotomize all ordinal IGD questionnaires in our data to endorsement/non-endorsement. We employ a total 6 separate justifiable methods of dichotomizing ordinal responses to questionnaire items (Table 2). For clarity, we identify three possible degrees of conservativeness that we standardize across scales, referred to as lax, strict, and strictest.

Dichotomizing ordinal or continuous variables is widely considered poor statistical practice (Royston et al., 2006). In this case, however, we believe it is justified because 1) it constitutes the only way to evaluate the DSM-5’s diagnostic proposal, 2) there are many incidents of precedent, 3) it allows us analyze the varied studies in the dataset together using a single, easily interpretable effect size metric, and 4) we include the analytical flexibility in how precisely responses are dichotomized as a feature of our analysis.

Numerous previous studies in the IGD literature have dichotomized responses. For example, the Game Addiction Scale (Lemmens et al., 2009) recommends dichotomizing at ≥ 3 (“sometimes”) on a 5-pt scale (Henchoz et al., 2016). Another study classified responses of ≥ 4 on a 5-pt scale as endorsement (Seok & DaCosta, 2012). The IGDS9-SF dichotomizes when the response is 5 (“very often”) on 5-pt scale (Pontes & Griffiths, 2016). For 3-pt scales, previous researchers have experimented with approaches in which only a 3 is considered endorsement (Király et al., 2017), as well as where ≥ 2 is endorsement (Choo et al., 2010).

Despite these instances of precedent, not all of the scale formats in our dataset have previously been dichotomized. Specifically, we could find no precedent for dichotomizing either a 6-pt scale, or a scale that has more than one indicator per DSM-5 criterion. For the 6-pt scales, we elected to use the closest analogues from the 5-pt scales, and thus consider viable options to be dichotomizing at ≥ 4 , ≥ 5 , or at 6. For multiple-indicator scales, we considered justifiable options to be either 1) taking the highest single indicator from each criterion, or 2) taking the mean score of indicators for that criterion.

Thus, we had 3 (lax, strict, or strictest) $\times$ 2 (highest item or average) = 6 justifiable methods of dichotomizing scores. In some cases (e.g., where all participants originate from a study that used a “yes”/“no” scale with 1 indicator per criterion), certain combinations of these were identical to others. Where this occurred, the dichotomization method is labeled “equivalent” in the analysis and only one of the identical models is retained.

2.1.4. Participants

Our dataset for study 1 consisted of 26,027 participants. The mean age was 28.8 (median = 23, SD = 15.1). This included 6,854 participants who identified as female, 17,986 as male and 1,187 participants who chose not to respond or preferred to specify another gender. Since each of the studies has a different set of well-being variables, the number of participants in each model of the specification curve ranges from 525 to 18,041. Further details about the participants in each study are available in the supplementary materials.

2.1.5. Analytical Approach

To investigate the strength of association between possible IGD diagnostic thresholds and distress, we conduct a specification curve analysis (Simonsohn et al., 2020). Specification curve is a method of integrating the findings from a series of competing models, and for comparing the impact of different analytic decisions on results. To do so, models are fit for every combination of justifiable analytical choices (e.g., which subsets of the data to use, which variable to use as the predictor, etc.) and examined in aggregate. It is therefore well-suited to an individual participant meta-analysis where significant analytical flexibility is present.

In the current study, we apply specification curve analysis to a set of variable-centred linear regressions of IGD thresholds and distress. For each pair of possible IGD threshold and distress variable, we fit a linear regression model with meeting/not meeting that IGD threshold as a binary predictor and distress as the criterion (response) variable. To avoid being locked into a fully confirmatory approach, we compare all 9 possible diagnostic thresholds: Endorsing any 1 of 9 symptoms; endorsing any 2 of the 9 symptoms; and so on and so forth.

This approach takes the choice of distress construct as arbitrary and interchangeable—as mentioned above, this is a consequence of considering any aspect of well-being as a valid indicator of (psychological) distress. Because well-being measures were assessed using a variety of scales and scale formats, raw scores cannot be straightforwardly compared. To address this, all well-being variables were standardized (i.e., z-scored) within studies. Effect sizes can therefore be interpreted as the difference between those meeting the IGD diagnostic threshold and those that did not, in terms of standardized mean difference relative to peers from the same study. This process allows us to integrate results from each of the included studies, despite potential differences in scale format or baseline well-being due to demographic or other factors.

In addition to how scores are calculated, previous work has also emphasized the importance of nationally representative samples (Stevens et al., 2021) and the clinical validation of IGD measures (Van Rooij et al., 2017). To investigate whether these impact upon results, we include possible analysis decisions where the data is limited to 1) only studies that used nationally representative samples, or 2) only studies that used an IGD measure that has undergone some degree of clinical validation.

Previous evidence conclusively shows that both age and gender causally relate to both our explanatory (gaming disorder criteria) (Scharkow et al., 2014; Teng et al., 2020) and criterion variables (distress) (Meade & Dowswell, 2016); these are therefore confounds that must be controlled for (Rohrer, 2018), and are included as covariates. Each model in the specification curve is therefore of the form

$$\mathbf{E}(\text{Well-being}) = \beta_0 + \beta_{age} + \beta_{gender} + \beta_{IGD}$$

for each valid combination of well-being variable and IGD threshold.

The assumptions of a specification curve analysis are identical to those of the underlying statistical model(s) it uses (in our case, multiple linear regression). Interpretation of the entire curve makes one further implicit assumption, namely that each model is equally justifiable (Del Giudice & Gangestad, 2021). Manual inspection of 25 random models within the SCA revealed no violations of homoscedasticity, but potential violations of normally-distributed residuals in 5 models. However, given that 1) linear models are easily interpretable, 2) more than 3/4 of inspected models did not have any evidence of violation, and 3) that linear models, particularly with large

samples, are generally robust to violations of residual normality (Gelman & Hill, 2007), we proceeded with its use.

Well-being variables (and IGD variables, where there was more than one item per criterion) were calculated using the mean score for available data; thus, participants with partial missing data were included. Participants who were missing data on all (sub)scale items for a given variable were deleted listwise for that particular regression model, but could still be included in other models where all relevant data was present.

2.2. Results

The overall results of the specification curve show that those who met an IGD threshold tend to report significantly greater distress than those who did not, across well-being measures and possible thresholds (93% of models significant at $p < .05$). In the bottom portion of Figure 1, we see that higher thresholds are generally associated with greater distress, as indicated by the overall shift of boxplots to the right as the value of threshold increases. The 9/9 threshold was an exception to this trend and exhibited a wider range, attributable at least in part to the low numbers of participants endorsing all nine criteria and correspondingly low precision.

The results of the specification curve are summarized numerically in Table 3. We first examined the models using 5 as the threshold, given the DSM-5’s proposal. We found that in 93% of models, meeting the 5/9 threshold is associated with significantly greater distress. In many, but not all of these models, this difference was also estimated as greater than or equal to our minimum threshold for clinical significance (77% of models with an association of magnitude ≥ 0.5 SD).

We found considerable heterogeneity related to the various distress constructs. Depression, anxiety, and loneliness were among the most strongly linked with IGD scores, whereas need satisfaction, life satisfaction, and positive affect were among the weakest. This is visible in the bottom portion of Figure 1, where boxplots for each well-being variable differ widely from each other; boxplots that are further to the right indicate that models using that well-being variable tended to find larger differences, while those further to the left indicate that models tended to find smaller differences.

We observed a moderate relationship between the method of dichotomization and model estimates, with the most conservative method (Average, Strictest) on average predicting $\Delta d = .18$ more distress than the least conservative one (Highest, Lax).

2.3. Study 1 discussion

Study 1 found that in the majority of models, the DSM-5’s 5/9 threshold correlates with differences in distress that meet our cut-off for clinical significance. Despite some variation in how strongly IGD threshold membership correlates with different aspects of well-being, those that met

Figure 1: Specification Curve Analysis of Distress and IGD Thresholds. Across 645 justifiable regression models, IGD thresholds are consistently associated with greater distress, even when the adopted threshold is as low as 2/9. In the top portion, estimated differences in distress associated with meeting a possible IGD threshold are ordered from least to greatest. Tightly-spaced dots represent results of a single multiple regression model; vertical lines depict the 95% confidence interval of that estimate. Dark red points (above the dashed line) represent models in which this difference exceeds our anchor for clinical significance, $d = .5$. In the bottom portion, each row depicts the distribution of estimates for models adopting that particular analytical choice, with each thin vertical line representing a single model and underlaid boxplots summarising all models in that row. Boxplots further to the right therefore indicate larger distress differences.

Table 3: Associations between distress and IGD classifications at each possible threshold, summarized across models in the specification curve

Threshold	Median distress difference [95% CI]	SD of Associations	Stat. Sig. Models (%)	Clin. Sig. Models (%)	Median Fulfillment (%)
1	0.47 [0.40,0.59]	0.15	97	47	46
2	0.57 [0.44,0.68]	0.20	95	66	29
3	0.60 [0.46,0.74]	0.25	97	77	19
4	0.63 [0.47,0.77]	0.31	92	68	9.9
5	0.67 [0.50,0.85]	0.36	93	77	7.2
6	0.69 [0.53,0.85]	0.40	87	76	6.4
7	0.67 [0.52,0.90]	0.47	93	78	5.6
8	0.93 [0.87,1.00]	0.54	95	88	4.1
9	0.88 [0.61,1.01]	0.45	81	81	2.6
Total	0.62 [0.46,0.78]	0.28	93.3	44.0	19.3

Fulfillment refers to the proportion of participants meeting the respective threshold; we report this as a median because fulfillment varies across models of the SCA, due primarily to differences in the 6 dichotomization methods

the 5/9 threshold generally differed from those who did not by a standardized mean difference of at least .5 on most well-being measures.

Surprisingly, however, we also found lower IGD thresholds continue to predict large differences in distress. For example, we find that that an IGD threshold of just 2/9 criteria is sufficient to predict an average difference in distress of clinically significant magnitude in well over half of models (66%). Furthermore, these relationships were generally robust to analytical flexibility in how to dichotomize responses.

We replicate previous findings that the strength of association differs across distress measures (Ballou & Van Rooij, 2020). It is noticeable that many of more weakly-correlated distress variables are reverse-scored positive constructs; this may reflect differences in reporting patterns for such variables, but this finding also raises the possibility that gaming disorder symptoms are more strongly associated with greater negative feelings than with fewer positive ones. We see the investigation of why some aspects of well-being hang more closely together with IGD than others as a potentially valuable area for future work that may benefit our understanding of both risk factors and possible intervention targets. One lens through which to understand this may be the comparison of hedonic (e.g., affect) and eudaimonic (e.g., life satisfaction) aspects of well-being, whose relationships with digital technology use are known to differ (Dienlin & Johannes, 2020); changes in these distinct areas may be indicative of different etiological pathways through which a problematic relationship with games may develop.

Taken together, the results of study 1 do not provide evidence for rejecting the DSM-5 threshold—we find considerable evidence of large enough relationships to be of potential clinical significance. However, they do indicate that alternative diagnostic thresholds may be approximately equally predictive of distress, and that the particular one proposed in the DSM-5 may lack sufficient justifi-

cation. At first glance, this finding is surprising—how can it be the case that endorsing just 2 criteria is sufficient for us to predict clinically significant differences in depressive symptoms or other aspects of well-being? We attempt to answer this question in study 2.

3. Study 2

In study 1, we found relatively large differences in distress for low possible IGD thresholds. Indeed, endorsement of just 2 or more IGD criteria was associated with clinically significant distress. Does this mean that we should be diagnosing players who endorse just 2 of 9 criteria as disordered gamers? To make sense of this finding, we compare those results with those from an alternative analysis strategy. For clarity and to reflect that this approach was an evolution in our thinking as we proceeded with this research, we present this as study 2.

One potential critique of using the endorsement of a specific number of symptoms as a threshold for the presence of a disorder concerns the contribution of high-scoring individuals to variability between groups. Consider a situation in which one classifies individuals as disordered or non-disordered gamers on the basis of endorsing 2 or more symptoms of IGD. The group of ‘disordered gamers’ will not just include individuals who endorsed 2 symptoms of IGD; it will also include those who endorsed 3, 4, 5, 6, 7, 8, or 9. It is possible that the variance in distress associated with these high-scoring individuals is sufficient to push the group average up above a standardized mean difference of 0.5 (our threshold for clinical significance) even if individuals who endorsed exactly 2 out of 9 symptoms did not tend to exhibit clinically meaningful differences in distress from their ‘non-disordered’ peers.

One way to examine whether this might be the case is to not classify participants into probable/non-probable IGD based on a particular threshold, but instead to model IGD as a continuous latent quantity: In simple terms, to examine the amount of distress associated with endorsing

1 symptom rather than 0, or 2 symptoms rather than 1, and so on and so forth.

Two variants of this have been used in previous research. The first involves summing (or averaging) the total score of polytomous scales, such that different symptoms may contribute non-equally to an individual’s total score. The second involves summing the total score of binary criteria endorsement (which may in turn be a dichotomized polytomous scale). Some of the primary studies in our aggregated dataset adopt both the former and latter approaches (Allen & Anderson, 2018; Marmet et al., 2018; Przybylski & Weinstein, 2019). Given that

In such analyses, the research question is not whether scoring above or below a specific threshold correlates with clinically significant distress; instead, the number of endorsed symptoms is treated as a (non-)linear predictor, with each additional endorsed symptom expected to be associated with greater overall distress on average.

To our knowledge, however, these two analytical strategies (dimensional and threshold-based) have not previously been directly compared with each other to see whether they lead to substantively different conclusions. This is our goal in study 2.

3.1. Method

The data (including dichotomization methods and 26,027 participants) for study 2 are identical to study 1. However, in study 2, we adopt an alternative analytical approach. Specifically, instead of comparing differences between those meeting and not meeting a particular IGD threshold, we model IGD scores as a continuous variable ranging from 0–9, referred to for clarity as *IGD severity*.

For the purposes of this analysis, we are not interested in differences between well-being measures, nor are we comparing different predictor variables (as we did with possible thresholds in study 1), and therefore specification curve analysis is not needed. Instead, we average all well-being variables that each participant completed into a single “composite distress” score, which allows us to retain the majority of the information provided by each participant while ensuring that each observation is independent. We then run 6 separate linear mixed effects models, one for each dichotomization method. To investigate between-study heterogeneity, each model contains a random intercept for composite well-being and a random slope for IGD severity varying within study.

3.2. Results

Results (Figure 2) indicate that endorsement of each additional IGD indicator was associated with greater distress. Table 4 shows the coefficients of these estimates. The two models estimated using lax dichotomization methods converged upon estimates of approximately .13 (as a standardized mean difference) greater distress for each additional criterion endorsed, while the four models using strict and stricter methods yield estimates of between 0.18 and .22.

We observe a moderate amount of heterogeneity between studies (SD of IGD coefficient = .035 for the two lax models, rising to SD = .10 for the strictest models). The slope for a single study differed from the global estimate by a maximum $\pm .06$ for lax models, and a maximum of $\pm .19$ for the strictest models. IGD severity uniquely accounted for between 8.6% and 12.0% of the variance in distress across the 6 models.

3.3. Study 2 Discussion

In the dimensional models, we found that endorsement of each additional IGD symptom correlates with greater distress. When using a lax dichotomization method, endorsement of at least 4/9 criteria is associated with clinically significant distress ($4 \times 0.13 = 0.52$, $0.52 \geq 0.5$), thereby supporting the falsification of possible thresholds of 3 or below. When using strict(est) dichotomization methods, results indicate that endorsing 3/9 criteria is associated with clinically significant distress ($3 \times 0.18 = 0.54$, $0.54 \geq 0.5$), thereby supporting falsification of thresholds of 2/9 or 1/9.

This reflects an important divergence from the threshold-based approach in study 1: The dimensional approach leads to different inferences about the appropriateness of thresholds, and is better able to falsify low ones. A likely contributor to this divergence is that the distress levels for those who just barely met the threshold may be weighted, or upwardly biased, by those that greatly exceeded it (as well as the converse, downward weighting of the below-threshold group’s distress scores by people endorsing 0 criteria.)

In the wake of the DSM-5 proposal, numerous studies—both cross-sectional and longitudinal—have compared probable IGD and non-IGD groups on a number of psychosocial outcomes using the DSM-5 threshold (e.g., Wang et al., 2018; Bargerion & Hormes, 2017; Ostinelli et al., 2021). These studies’ estimations may not appropriately characterize the distress patterns of participants scoring at the boundary; these estimated differences, in turn, may be misinterpreted as support for the 5/9 threshold, despite the participants who endorsed exactly 5/9 not exhibiting clinically significant distress. This can be viewed as another problematic consequence of the premature switch to confirmatory research on IGD.

As above, while the dimensional results provide evidence to falsify potential thresholds—falsifying 2/9 or below for strict methods, and 3/9 or below for lax methods—they do not therefore support thresholds of 3 and 4, respectively. In the case of mental disorders, which are slow to develop and likely involve reciprocal effects of the disorder severity and well-being, the absence of correlation is evidence for the absence of causation, but the presence of correlations does not imply causation. This is especially salient when we look at the implications for prevalence, which is regularly assessed with cross-sectional surveys (Mihara & Higuchi, 2017). Were our results taken at face value, and a 3/9 threshold adopted in place of the 5/9

Figure 2: **Linear relationship between IGD severity and distress** For readability, only a random subsample of 50,000 points are plotted and horizontal jitter is added.

Table 4: Associations between each additional IGD symptom endorsed and distress

Dich. Method	IGD Reg. Coef.	SE	t	p-value
Highest, lax	0.125	0.012	10.621	<.001
Highest, strict	0.177	0.025	7.096	<.001
Highest, strictest	0.194	0.032	6.045	<.001
Average, lax	0.129	0.012	10.458	<.001
Average, strict	0.189	0.026	7.160	<.001
Average, strictest	0.216	0.034	6.341	<.001

one, the estimated prevalence of IGD would be as high as 29%, implausibly high for a psychiatric disorder (for comparison, a recent study estimated the 12-month prevalence alcohol use disorder to be 10% and problem gambling to be 2% among American adults; Sussman et al., 2011).

Given the exploratory, prospective nature of IGD, we recommend that future work avoid grouping participants above and below the 5/9 threshold and instead model criteria endorsement as a continuous variable.

4. Study 3

So far, we have seen that a dimensional approach may yield better inferences than a threshold-based one. A necessary feature of both approaches, however, is that all criteria are treated equally—i.e., which criteria an individual endorses is arbitrary with regard to its predicted association with distress.

This procedure may not be justifiable; a portion of the nine indicators directly assess negative consequences of gaming on a person’s well-being or functioning, while others ask about a person’s cognitive and behavioral engagement, but do not necessarily imply negative consequences (King et al., 2020). This distinction is sometimes referred to as “core” vs “peripheral” criteria (Brunborg et al., 2015; Charlton & Danforth, 2007). Under this categorization, peripheral criteria are preoccupation, tolerance, and escapism; core criteria include withdrawal, problems/jeopardizing, loss of control, loss of interests, lying/deception, and continuation despite problems.

Previous research has in some cases supported the inclusion of peripheral criteria in the DSM-5, but at other times revealed problems see (see Ko et al., 2020, for a review). Some of these critiques have been on psychometric grounds. One early study found that preoccupation, tolerance, and escapism load onto a separate factor than core criteria (Charlton & Danforth, 2007). Another found

that preoccupation and escapism items were poor at discriminating players with probable IGD from those without (Király et al., 2017).

Other critiques are conceptual in nature. King et al. (2018) find that tolerance may not fully apply to gaming as currently described in the DSM-5, and might instead be itself multidimensional. Another study found that escapism was the strongest single-criterion predictor of well-being, but speculates that escapism items often may not assess the use of games to escape from difficulties or unpleasant feelings, but rather the presence of difficulties in the first place, thereby confounding cause and effect (Ballou & Van Rooij, 2020).

A recent Delphi consensus study found that experts were in agreement about the diagnostic validity, clinical utility, and prognostic validity of only three criteria, namely loss of control, continuation despite problems, and problems/jeopardizing (Castro-Calvo et al., 2021). Tolerance and lying/deception were noted for having extremely low ratings (i 20% of experts rating them as “very” or “extremely” important for each category). This aligns with previous work calling into question the discriminatory ability of lying/deception (Ko et al., 2014).

To ensure that players who experience minimal negative consequences alongside their gaming are not classified as having probable or diagnosable IGD, we argue that indicators of gaming disorder should themselves also be associated with—and ultimately causally implicated in—the impairment and distress around which the condition is predicated.

4.1. Method

The method for study 3 closely mirrored that of study 1. The primary difference was that study 2 was able to take advantage of shared datasets that used measures of gaming disorder that did not map directly onto all 9 of the DSM-5 criteria (thus precluding assessing the 5/9 threshold for those participants). We again conducted a specification curve analysis, this time with endorsement of each individual DSM-5 criterion as the predictor of distress change, controlling for age and gender. The same 6 dichotomization methods were applicable.

Our dataset for study 1 consisted of 38,851 participants (i.e., the 26,027 from study 1 plus 12,824 participants from studies that did not measure all 9 DSM-5 criteria). The mean age was 27.9 (median = 24, SD = 14.6). This included 10,275 participants who identified as female, 27,373 as male and 1,203 participants who chose not to respond or preferred to specify another gender. The number of participants in each model ranged from 525 to 28,644.

4.2. Results

Results of this analysis (Table 5) show that in 81.8% of models, endorsement of a single IGD criteria is associated with significantly greater distress. The median difference associated with endorsing an arbitrary IGD criterion was 0.4 standard deviations greater distress (95% CI

[0.26,0.52]). In 34.1% of models, this difference was estimated to be of clinically significant magnitude (using the same $d = .5$ threshold).

Results show that endorsing escapism or withdrawal were the strongest predictors, with the median model predicting a distress difference of $d = .60$ and $d = .54$, respectively. Loss of interests, continuation despite problems, problems/jeopardizing, and to a slightly lesser extent lying/deception were the next most predictive, with median models estimating a distress difference of around $d = .45$. Preoccupation, tolerance, and loss of control were the least predictive, with median differences of between $d = .25$ and $d = .33$.

The method of dichotomization had a negligible influence, accounting for less than 1% of the variance in effect sizes across models in the specification curve.

4.3. Study 3 Discussion

These results show that endorsement of withdrawal or escapism each single-handedly predicted a clinically significant distress difference for over half of models in which they were present. In contrast, preoccupation, tolerance, and loss of interests were much more weakly associated with distress: endorsement of any of these three was associated with roughly half as much distress as the most predictive criteria.

We do not expect that every criterion will be equally predictive of distress (a true additive model). However, the results here indicate that these inequalities are large in nature, and that as a result it may be problematic to treat all IGD criteria equally for diagnostic purposes. These results align with previous work finding evidence for modifying preoccupation and tolerance, or possibly even excluding them from the list of diagnostic criteria (e.g., King et al., 2018; Charlton & Danforth, 2007).

In addition to the statistical concerns we raise about less distress-informative criteria, our findings also highlight conceptual challenges with the two most distress-informative ones. Withdrawal and escapism, despite being closely associated with distress in our data, are among the criteria that were rated as having the least utility/validity by experts (Castro-Calvo et al., 2021). In particular, the authors write that escapism “should not be considered in the definition and diagnosis of [gaming disorder]” (Castro-Calvo et al., 2021, p. 9), noting that high involvement need not be problematic when it is a “temporary coping strategy as an expected response to common stressors or losses” (Billieux et al., 2017, p. 1723). Should researchers’ and clinicians’ intuitions be accepted, this may indicate that a portion of the distress associations uncovered in this study are reflective of processes other than a causal effect of problematic game use.

Figure 3: **Specification Curve Analysis of Distress and Each of the 9 IGD Criteria.** Across 1092 justifiable regression models, endorsing IGD criteria is consistently associated with greater distress. In the top portion, estimated differences in distress associated with IGD criteria endorsement are ordered from least to greatest. Tightly-spaced dots represent results of a single linear regression model; vertical lines depict the 95% confidence interval of that estimate. Dark red points (above the dashed line) represent models in which this difference exceeds our anchor for clinical significance, $d = .5$. In the bottom portion, each row depicts the distribution of estimates for models adopting that particular analytical choice, with each thin vertical line representing a single model and underlaid boxplots summarising all models in that row. Boxplots further to the right therefore indicate larger distress differences.

Table 5: Associations between endorsement of each IGD criterion and distress, summarized across models in the specification curve

IGD Criterion	Median distress difference [95% CI]	SD of Associations	Stat. Sig. Models (%)	Clin. Sig. Models (%)	Median Endorsement (%)
Preoccupation	0.25 [0.12,0.37]	0.170	75.2	6.0	22.9
Withdrawal	0.54 [0.38,0.68]	0.242	95.1	59.0	6.8
Tolerance	0.33 [0.14,0.48]	0.253	70.6	21.4	10.0
Loss of control	0.29 [0.15,0.41]	0.232	83.6	20.0	11.0
Loss of interests	0.49 [0.36,0.61]	0.220	96.7	41.8	9.3
Continuation despite problems	0.46 [0.31,0.59]	0.259	89.8	40.7	18.9
Lying/deception	0.43 [0.24,0.63]	0.276	74.8	43.5	7.0
Escapism	0.60 [0.48,0.68]	0.190	98.4	65.9	16.1
Problems	0.42 [0.28,0.56]	0.316	87.7	41.1	8.8
Total	0.42 [0.29,0.56]	0.265	85.5	37.1	11.8

Medians and SD are calculated with reference to all models of the specification curve that used that criterion as a predictor

5. General Discussion

5.1. IGD and clinically significant distress

Overall, we find that IGD scores and criteria are consistently associated with various forms of psychological distress. Participants meeting the 5/9 threshold for IGD tended to report significantly greater distress across 21 well-being constructs, by amounts that may warrant the qualifier “clinically significant”. While this finding again does not in any way validate the notion that IGD *led to* clinically significant distress, we do not find evidence that supports falsifying the 5/9 threshold.

Instead, we had the opposite problem: much as our study 1 results failed to falsify the 5/9 threshold, so too did they fail to falsify thresholds as low as 2/9 or even 1/9. This appears to be because at each possible threshold, differences between groups are skewed upwards by participants endorsing many more than the minimum number of criteria for that threshold. Our results therefore indicate that the common practice of dichotomizing participants above and below the proposed IGD threshold is unlikely to provide evidence against the suitability of that threshold; in other words, it does not constitute a severe test of that threshold or of IGD (Mayo, 2018).

Instead, study 2 showed that a dimensional approach offers a more severe test than the threshold-based one. Contingent upon our choice of $d = .5$ as the cut-off for potentially clinically significant distress, the results of study 2 falsify thresholds of 3 or below when criteria are marked as endorsed using a lax dichotomization method (i.e. ≥ 2 on a 3-pt scale, ≥ 3 on a 5-pt scale, and ≥ 4 on a 6-pt scale), and falsify thresholds of 2 or below when using a strict dichotomization method (i.e. 3 on a 3-pt scale, ≥ 4 on a 5-pt scale, and ≥ 5 on a 6-pt scale).

While the limited power to falsify IGD may not be inherently problematic, issues arise when such studies are then interpreted as evidence in support of the disorder

as proposed (in other words, that the 5/9 IGD diagnostic threshold is meaningful). This interpretation is often implicit when IGD is treated as *de facto* legitimate as a condition, citing differences in below- and above-threshold groups as evidence (e.g., Paulus et al., 2018; Kuss et al., 2018; Naskar et al., 2016).

We reiterate that our analysis strategy is not capable of supporting an alternative threshold. This becomes particularly salient when examining the implications for prevalence: IGD is proposed on the basis of 5 or more criteria *leading to* impairment or distress, but is often treated as fulfilled on the basis of those 5 criteria alone. Prevalence estimates from such (cross-sectional) studies are then reported and widely cited (Mihara & Higuchi, 2017). In the current study, this would lead to an estimated prevalence of 7.2% with the 5/9 threshold. However, such data (including ours) may be better viewed as a soft upper limit on the true prevalence of IGD, as they represent the aggregate of a variety of causal pathways—for example, some of our sample may be individuals whose impairment or distress led to gaming, rather than the other way around. Assessing IGD prevalence through self-reports alone likely overestimates the true prevalence of the proposed condition (Eaton et al., 2019).

It is also paramount to re-emphasize that the current study cannot give a definitive answer to what quantitative distributions warrant the qualifier “clinically significant.” The threshold adopted here was intentionally conservative, based on a limited prior literature that focused largely on interventions and is not specific to gaming or behavioral addictions. Rather than ending the discussion, we believe this study highlights an improved approach to the study of IGD with three important considerations for the field.

First, given the DSM-5 definition, discussions of the validity of IGD are not meaningful in the absence of information about distress/impairment—in other words, construct validity is not sufficient. More thorough consideration of (Health-Related) Quality of Life measures is needed in be-

havioral addiction research (see e.g., Laudet, 2011) for a discussion of the importance of HRQoL in the context of substance use disorders).

Second, we draw attention to the fact that the DSM-5’s definition of IGD (and other disorders) can and should be operationalized using concrete effect size metrics where possible. We are not nearly the first to note the importance of mapping theoretical claims onto statistical ones (e.g., Fritz et al., 2012), but to date this remains largely absent from research on IGD. Although it is a difficult task, we are encouraged by the fact that it is one that can be addressed with known empirical methods (e.g., anchor-based methods, Anvari & Lakens, 2020). We strongly encourage further work both with (disordered) players and clinicians (e.g., Colder Carras et al., 2018) to hone in on domain-specific statistical anchors for what constitutes clinically significant impairment or distress.

Third, we find that the potentially premature switch to confirmatory research on IGD (in which participants are grouped above and below the 5/9 threshold) may lead to erroneous conclusions about the legitimacy of this threshold. With an ever-growing number of studies finding evidence for differences between above-threshold and below-threshold groups, we run the risk of “fossilizing” an incompletely justified diagnostic threshold without having fully understood the experience of players who report scores close to the boundary. A dimensional approach to modeling combined with anchors of clinical significance can alleviate this somewhat, but are no substitutes for fully exploratory research on alternatives to the DSM-5’s model of problematic gaming.

5.2. IGD criteria

In study 3, we contextualized the results from studies 1 and 2 by comparing the 9 IGD criteria. With highly unequal “loadings” from individual criteria onto correlations with distress, certain combinations of 5/9 are likely to be less associated with distress than others. While a large proportion of the relationships between IGD criteria and distress were statistically significant, closer investigations reveal that three of the criteria—preoccupation, tolerance, and loss of interests—are roughly half as predictive of differences in distress as the two most predictive criteria (withdrawal and escapism). This finding bolsters a large body of existing evidence that these peripheral criteria are problematic (King et al., 2018; Király et al., 2017; Deleuze et al., 2017).

Given these differences between criteria, a strictly additive approach using the current list of 9 criteria may not be the most appropriate method of scale-based assessment either—although it may remain more appropriate than threshold-based methods in many situations. We recommend the exploration of more sophisticated classification models (e.g., Conditional Inference Trees, Rehbein et al., 2015; Pontes et al., 2019), but using gold standard self-report or clinician ratings of clinically significant im-

pairment/distress as the “ground truth” outcome, rather than whether participants met the 5/9 threshold.

Relatedly, the findings from study 3 highlight the potential for more nuanced IGD sub-types. Certain combinations of criteria are likely to have unique distress profiles, and may differ in how they can best be identified and treated. This varying association with distress by possible symptom profile is not unique to IGD (Fried, 2017; Hoo-ley et al., 1987); while it poses a challenge for diagnosis, it also represents an opportunity for more individualized treatment (Wright & Woods, 2020). More work is needed to understand typical emotional and behavioral patterns of disordered players both qualitatively and quantitatively (ideally including telemetry data), as well as to identify potentially varied “pathways,” or underlying etiologies, of IGD (e.g., Perales et al., 2020). This line of research could also compare the criteria used in the ICD-11’s definition of gaming disorder, in light of evidence that the ICD-11 and DSM-5 diagnostic criteria capture at least partially non-overlapping groups (Higuchi et al., 2021).

While it may initially be surprising that escapism is so strongly linked with distress—especially when juxtaposed with experts’ general agreement that escapism is of limited utility in IGD diagnosis (Castro-Calvo et al., 2021)—this finding is in line with some previous results (Ballou & Van Rooij, 2020; Montag et al., 2019). One possible explanation is that escapist play is a behavior associated with elevated distress, regardless of whether that distress is IGD-related; in other words, players who experience unpleasant feelings in their daily life may seek out games to escape those feelings, but this does not imply that their gaming caused those feelings (Iacovides & Mekler, 2019). A clear causal link between diagnostic criteria and distress grows even more important in light of a host of behavioral addictions that have been proposed and measured using similar items, including addiction to Facebook/Social Media (Andreassen et al., 2012), work (Andreassen et al., 2012), and food (Schulte & Gearhardt, 2017). Some researchers have pointed out that the validation procedure for such self-report measures is generally problematic, and can result in classifying people as addicted to activities considered healthy, such as spending time with one’s friends offline (Satchell et al., 2020). This may be in part the result of inappropriate inclusion of criteria (including, but not limited to escapism) that were derived from substance use disorders but whose causal relationship to the behavioral addiction itself are unclear.

5.3. Dichotomization and Analytical Flexibility

Our findings paint a mixed picture with regard to the influence of analytical flexibility in dichotomization in research on IGD. For any individual criterion, the dichotomization method does not appear to be particularly important (Study 3); scores for all criteria are right-skewed, and thus many people who meet the lax threshold will also meet stricter ones.

However, in the threshold-based approach, the influence of dichotomization grew to moderate in size, with models using the laxest dichotomization differing from those using the strictest by $\Delta d = .18$ on average. When IGD was modeled dimensionally, stricter dichotomization methods were associated with a near-doubling of the association between endorsement of a single symptom and distress.

The influence of dichotomizing responses might therefore best be understood as cumulative, with an increasing impact on the model with each additional dichotomized item. As seen in study 1, the practice of “double dichotomization”—dichotomizing participants into probable/non-probable IGD based on dichotomized responses to individual items—represents a particularly large loss of information, yet remains relatively common in the literature (Bargeron & Holmes, 2017; Laconi et al., 2017; Beranuy et al., 2020). Under the current DSM-5 definition of IGD, however, dichotomization of at least the 9 criteria may remain a necessary evil.

To ease these issues, we recommend that future developers of polytomous scales explicitly discuss whether their scale is intended for assessment in line with the DSM-5 monothetic approach, and if so, how responses should be dichotomized. If possible, cut-offs for endorsement should be derived by linking self-reports by clinical evaluations of IGD symptomatology. Where this is not possible and no other information is available, we recommend the use of lax thresholds, as these most closely converged with the estimates from binary (“yes”/“no”) scales in our data.

5.4. Meta-Analysis and Data Quality

In addition to its contributions to our understanding of IGD, we hope that this study also demonstrates the value of data aggregation and reanalysis. While rates of data sharing in the psychological sciences remain as low as 2% (Hardwicke et al., 2020), the rates of willingness to share data privately for secondary analysis may be greater. This study was made possible by the willingness of the aforementioned authors to share their data, ultimately contributing to a far more highly-powered and informative study than would have been possible by any individual group of researchers or with meta-analysis via summary statistics.

Even in meta-analysis, however, the protocol and data quality are crucial (Colder Carras et al., 2020) (and it is worth noting that this can be better assessed in individual participant meta-analysis than with summary statistics). The results in Studies 1 and 3 indicate that when limiting inclusion to nationally representative samples or studies that used clinically validated instruments, effect sizes attenuate somewhat: nationally representative studies find that meeting the 5/9 threshold is associated with .41 z-scores greater distress, in comparison to .69 for both studies with clinically validated measures and when all data is included. While we are hesitant to read too heavily into this finding, given that it is not a core aspect of the paper, this does suggest that IGD symptoms may be more

heavily linked with distress in some populations than in others, and that greater methodological rigor can help unpack this.

5.5. Limitations

There are several key limitations to the current study. As mentioned previously, we use one of many possible anchors for clinical significance, and are limited to correlational data—this study therefore cannot and does not validate IGD. Instead, IGD passed a possible test of invalidation, with the aforementioned caveats for future work.

Next, despite the diversity of distress variables present in the current study, nearly all of these pertain psychological well-being. Clinically significant impairment may manifest in a variety of ways, and future work can improve the generalizability of these results by extending them to other psychological distress constructs and HRQoL indicators like work/academic performance or objective measures of physical health.

We note that individual participant meta-analysis, and specification curve as a method of analyzing such data, suffer from the limitations and biases of the literature on which they are built. Notable limitations of the IGD literature—and by extension the data in this study—include low uptake of clinically-validated IGD scales, lack of samples with clinical interviews to use as a ground truth distress criterion, and a high reliance on convenience sampling. In addition, we were only aware of the existence of datasets that had either been referenced in a published paper or posted publicly, and it may be the case that other datasets with weaker associations between IGD and well-being remain in the file drawer.

6. Conclusion

In this paper, we found that IGD is consistently associated with various forms of psychological distress, and that these associations may be strong enough to be considered clinically significant. Participants meeting the 5/9 IGD threshold differed from those who did not meet the threshold by an average of $d = .67$. However, using the same criteria and method, we found that a threshold of just 2/9 predicts similarly large differences. Rather than supporting the legitimacy of such a threshold, this instead shows a limitation of confirmatory, threshold-based modeling of IGD. In study 2, we show that a dimensional, more exploratory approach has greater ability to falsify thresholds, and may be more appropriate for many studies on IGD. Finally, in study 3, we showed that the various criteria do not contribute uniformly to information about distress, and that preoccupation and tolerance may be candidates for removal from the DSM-5’s definition of IGD. Two other criteria, withdrawal and escapism, were highly associated with distress, but we speculate that they may be reflective of alternative causal processes.

We believe this study has important implications for ongoing research on IGD. We hope that future work will

1) establish more meaningful and context-specific statistical anchors for clinically significant impairment/distress, 2) subject these thresholds to similar tests in a causal framework, at the within-person level using longitudinal data and hierarchical models, 3) model IGD severity as a continuous variable when seeking to evaluate possible thresholds, and 4) strongly consider revisions to the list of 9 criteria.

Data Availability

Due to restrictions with our reuse of secondary data, not all data from this manuscript is publicly available. However, our supplementary materials on the Open Science Framework contain all analysis code used in the paper alongside a (partially) synthetic dataset generated using *Synthpop*. This synthetic dataset is a combination of the true primary data where we hold permission to share it, alongside synthetic data that has been generated to very closely approximate the properties of the original data. Link to OSF materials: <https://osf.io/gvdf4>.

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Author Contributions

NB and DZ conceived of the study. NB conducted statistical analyses and wrote the first draft of the manuscript. DZ oversaw data requests, supervised the study, and participated in revisions. All authors have read and approve of the final manuscript.

Conflicts of Interest

The authors have no conflicts of interest to declare.

Supporting information

We used R (Version 4.0.3; R Core Team, 2020) and the R-packages *effsize* (Version 0.8.1; Torchiano, 2020), *specr* (Version 0.2.1; Masur & Scharkow, 2019), and *tidyverse* (Version 1.3.0; Wickham et al., 2019) for our analyses.

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