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Experimental insights on antisocial behavior: two meta-analyses

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Abstract

We report two meta-analyses on the determinants of antisocial behavior in experimental settings in which such behavior is not rationally motivated by pecuniary incentives. The first meta-analysis employs aggregate data from 95 published and unpublished studies (24,086 participants), using laboratory, field and online experiments carried out since 2000. We find that antisocial behavior depends significantly on the experimental setting, being highest in vendetta games and lowest in social dilemmas. As we find significant heterogeneity across the studies, including across game classes, in the second meta-analysis, we focus only on "Joy of Destruction" (JoD) and money-burning (MB) experiments, for which we have the most observations, 51 studies and around 16,784 participants. Overall, our findings suggest that procedural fairness and being observed by others reduce the frequency of antisocial behavior. Online and field experiments display more antisocial behavior than laboratory experiments. We also find that the strategy method biases antisocial behavior upward. However, we do not find evidence for a positive publication bias being correlated with higher destructive behavior, either in the general meta-analysis or in relation to JoD/MB experiments; if anything, there is evidence of a negative publication bias. The JoD/MB meta-analysis finds evidence of a price effect for destruction frequency, negative discrimination against outsiders, within-subject designs underestimating destructive behavior, and more antisocial behavior in one-shot interactions. Collectively, our results point to the value of more laboratory experiments that systematically build on paradigmatic experimental designs to enable comparability and the identification of key economic drivers of antisocial behavior.

Keywords: Antisocial behavior; Destructive behavior; Joy of Destruction games; Meta-analysis; Money-burning games

JEL Codes: C72; C90; D91

1. Introduction

Antisocial behavior is ubiquitous in the real world and often results in large efficiency costs (Gangadharan et al., 2020). Over the last two decades, while more heterogenous and smaller than the experimental research on prosocial behavior, there has been a proliferation of experimental studies attempting to help us better understand the incidence and causes of antisocial behavior.[1] In this article, we present two meta-analyses on antisocial behavior in economic experiments, where antisocial

[1]For example, Abbink and Herrmann (2011); Abbink and Sadrieh (2009); Abbink et al. (2011); Bolle et al. (2014); Cavatorta et al. (2023); Cinyabuguma et al. (2006); Denant-Boemont et al. (2007); (Harbring & Irlenbusch, 2008; 2011);

behavior is defined as the act of reducing or destroying other players' income without any prospects of pecuniary gains.[2]

Our general meta-analysis includes 95 studies (24,086 participants) and 359 observations (treatments) in total using laboratory, online, and field experiments across 38 countries. It considers experiments where antisocial behavior can be identified in the Joy-of-Destruction (JoD) and money-burning (MB) games, vendetta games, contests, allocation and coordination games, and social dilemmas with counter-punishment. As we find significant heterogeneity across the studies, including within game classes, we also conduct a second meta-analysis in which we focus only on JoD/MB experiments, for which we have the largest number of observations, 51 studies and around 16,784 participants and 222 observations in total.[3]

To understand what general lessons we can glean from experiments on antisocial behavior, we examine both the extensive and the intensive margins of antisocial behavior – that is, both the frequency of destruction and average destruction rates. In determining what drives individuals to act antisocially, we evaluate whether antisocial behavior is motivated by income inequality, group identity, social observability, pressure by authority and whether interactions are one-shot or repeated. We also assess the impact of researchers' design choices by including methodological variables that capture the impact of using the strategy method, whether the study was conducted in a laboratory, online or in the field, or whether subjects have worked for their earnings (i.e., house money effects).

Overall, we find that subjects engage in payoff destruction in about 30% of cases and destroy on average 20% of the maximum amount that can be destroyed. In both meta-analyses, procedural fairness and being observed by others reduce the frequency of antisocial behavior. Surprisingly, field experiments consistently display more antisocial behavior than in laboratory experiments, while this is at least as high or higher in online experiments than in laboratory experiments. We also find that the strategy method biases antisocial behavior upward. However, we robustly do not find evidence for a positive publication bias correlated with higher destructive behavior, either in the general meta-analysis or in relation to JoD/MB experiments. The level of antisocial behavior differs significantly across games, with the highest destruction rates observed in vendetta games (60% of all decisions) and the lowest in social dilemmas with counter-punishment (14% of all decisions). In the general meta-analysis specifically, we find evidence that destruction rates are lower among exogenously advantaged subjects. In the JoD/MB meta-analysis specifically, we find evidence of negative discrimination against outsiders, of within-subject designs underestimating destructive behavior, and of more antisocial behavior in one-shot interactions; we also find that, if destruction is costly, its frequency is lower.

The rest of this paper is structured as follows. Section 2 provides an overview of the experimental games included in the meta-analysis. Section 3 describes the methodology we followed in generating and analyzing our dataset. Section 4 reports our results, and section 5 concludes with a discussion of our findings.

Harbring et al. (2007); Nikiforakis (2008); Nikiforakis and Engelmann (2011); Sugden and Wang (2020); Zizzo and Oswald (2001); Zizzo (2003).

[2]A similar definition is used in the literature, especially on the money-burning and vendetta games (e.g., Abbink & Herrmann, 2009; 2011; Abbink & Sadrieh, 2009; Zizzo, 2003; Zizzo & Oswald, 2001). Destruction in our study can be due to psychological motives such as status-seeking (e.g., Charness, Masclet & Villeval, 2014), envy (e.g., Grossman & Komai, 2013; Schier & Waligora, 2016; Wobker & Kenning, 2013), spite (Fehr et al., 2013; Jauernig & Uhl, 2019), or benefits that are not exclusively monetary such as reinforcing cooperation (e.g., Fu, Kamei & Putterman, 2017; Gordon & Puurtinen, 2020; Nikiforakis, 2008).

[3]We are aware of one attempt of meta-analysis related to this second one, albeit with a narrower focus. Sanjaya (2023) analyzes only average destruction rates and only on a subset of JoD/MB games (44 studies, 88 observations). We explain the relationship of our second meta-analysis to Sanjaya's (2023) in section 5.

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2. Experimental games included in the meta-analysis

We focus on experimental studies in which subjects decide to reduce the earnings of others without the prospect of financial gains, as opposed to, for example, games in which destroying money is entirely or partially profit-maximizing. By this definition, the papers included in the meta-analysis can be classified into six classes of economic games: JoD/MB games, vendettas, allocation games, coordination games, contests, and social dilemmas with counter-punishment.[4]

Joy-of-Destruction/ money-burning games. In both JoD and MB games, subjects can choose to reduce the other players' income with or without a cost to themselves (Abbink & Herrmann, 2011; Abbink & Sadrieh, 2009; Zizzo, 2003; Zizzo & Oswald, 2001). The two sets of games are identical in structure, and other than JoD games, they have equal endowments. This is to remove inequity aversion as an explanation of destructive behavior. In either class of games, there are no financial gains to this act; a payoff-maximizing, rational agent would choose not to engage in "money burning." Thus, any income reduction corresponds to antisocial behavior, for example, due to spite or malice (or inequity aversion, for MB games).[5] Replications of the JoD/MB games and their variants have consistently supported the finding that individuals are willing to reduce other players' income, even at a cost to themselves (e.g., Abbink et al., 2018; Abbink et al., 2011; Dickinson & Masclet, 2019; Dickinson et al., 2018; Gangadharan et al., 2019; Gangadharan et al., 2021; Gangadharan et al., 2022; Grosch & Rau, 2020; Grossman & Komai, 2013; Sadrieh & Schröder, 2016, 2017; Zizzo & Fleming, 2011).

Vendettas. The vendetta game was developed by Bolle et al. (2014) and further examined by Umashev (2019), Sugden and Wang (2020), and Cavatorta et al. (2023). It studies how an initial act of aggression can lead to socially inefficient feuds. The game involves two players moving alternatingly across a finite number of periods. Each player receives an amount of money or an exogenous percentage of winning a lottery, and at each player's turn, they can choose to steal their co-player's money or win probability in blocks of percentage, but at a cost. The game ends if no player steals from each other in two consecutive periods or when both cannot steal any longer. Under self-interest, altruism, reciprocity, or inequality aversion, counter-stealing should not occur in the subgame perfect equilibrium under equal initial endowment (Bolle et al., 2014).

Allocation games. By allocation games, we refer to variants of the dictator game in which the player chosen as the dictator can decrease the opponent's earnings without any pecuniary consequences for themselves (Bracht & Zylbersztejn, 2018; García-Gallego et al., 2019; Sadrieh & Schröder, 2016). We also include choice experiments, in which subjects can choose between different income allocations among themselves and their opponents (Beckman et al., 2002; Fehr et al., 2013). In these games, the decision is considered costless burning if the subject chooses an option in which their partner receives a lower payoff, while they receive a payoff equal to the optimal amount, and costly burning if they receive a payoff less than the optimal amount.

Coordination games. Even though the coordination game class consists of a range of games, we only consider the first-strike game and the mobbing game. In the first-strike game, two or more players accumulate earnings over many rounds, and they can deactivate the opponent in any round. The deactivated player would lose all current earnings and face reduced future payoffs, without any monetary gains to the decision-makers, which makes deactivating a hostile act (Abbink & de Haan, 2014;

[4]We do not include ultimatum games in our meta-analysis. Ultimatum games have already been analyzed in the meta-analysis by Oosterbeek et al. (2004). With the exception of those studies that use the strategy method, the incidence of rejection in ultimatum games is not only endogenous to proposer behavior but also proposers normally keep offers above rejection thresholds, leading to data that would be problematic to interpret in terms of our meta-analysis.

[5]We discuss further the interpretation of JoD games in section 5.

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Abbink et al., 2021; Simunovic et al., 2013). In the mobbing game, subjects are divided into groups and can nominate a group member to be "mobbed" (Abbink & Doğan, 2019). If the same person is nominated by all other players, that person loses all their earnings. Following the inclusion criteria, we only include treatments in which mobbing players do not gain financially even if mobbing was successful. Because of the nature of the game, destruction decisions in the mobbing game are in terms of group decisions instead of individual decisions.

Contests. We consider two distinct types of contest games for our analysis. The first type involves a standard tournament in which subjects compete for a prize via a real-effort task under piece rate or tournament compensation schemes and then have a chance to underreport other players' scores (Carpenter et al., 2010; Charness, Masclet & Villeval 2014). The second type resembles a gift exchange game in which an employer (principal) offers a wage and a prize to multiple employees (agents) (Falk et al., 2008; Harbring & Irlenbusch, 2005; 2008; 2011). The agents then compete for the chosen prize by paying a cost to exert productive effort and sabotage the effort of others. Each unit of effort increases an agent's output by one unit, while each unit of sabotage reduces the output of other agents by one unit. In this case, sabotage can be partially explained by self-interest, as agents earn a higher payoff by winning the game (Harbring & Irlenbusch, 2005).[6]

Social dilemmas with counter-punishment. This set of games includes experiments where subjects play a social dilemma game in the first stage, followed by a subsequent stage of one round or multiple rounds of punishment and counter-punishment.[7] The social dilemma stage typically involves the public goods games, prisoner's dilemmas, trust games, and power-to-take games. In the punishment and counter-punishment stage, in each and every round, subjects can either a) punish whoever they want whether or not they were punished in the previous round (Engel et al., 2021; Fehr & Schurtenberger, 2018; Nikiforakis & Engelmann, 2011; Nikiforakis et al., 2012), or b) only are allowed to punish those who punished them in the previous round (Berná, 2013; Campos-Vazquez & Mejia, 2016; Cinyabuguma et al., 2006; Denant-Boemont et al., 2007; Engel et al., 2014; Kern, 2014). The cycles of retaliatory punishment and counter-punishment in these experiments bear a close resemblance to vendettas, especially in the later stages where punishment appears fueled by anger, revenge or nastiness, and often result in very negative outcomes for both subjects (Hopfensitz & Reuben, 2009; Nikiforakis & Engelmann, 2011).

3. Methodology

This section outlines our methodology, including study selection criteria, dataset description, analytical procedures, and hypotheses. We start by discussing the inclusion and exclusion criteria used for our literature search (3.1). We then proceed with describing our datasets (3.2) and the analytical

[6] For the second type, we deducted the equilibrium level of sabotage from the total reported amount and divided it by the maximum sabotage amount that could be exerted in order to calculate the proportion of destruction that is not fueled by material incentives.

[7] Our study examines both second-party and third-party punishment behaviors. We do not include social dilemmas followed by one stage of punishment due to the difficulty in identifying an antisocial component of behavior within this large literature, particularly in a way that is coherent with our definition of antisocial behavior, for which data is available and which does not require mental models of what explains the punishment. As an example of this last point, assume we observe that a subject punishes another subject who has been making a mean contribution; perhaps this is due to being antisocial, but perhaps this is because the subject hopes to induce a higher contribution on the part of the subjects that they see as "redeemable" (as opposed to free riders who may simply not care). The key point is that there is an unclear relationship between the punishment and whether the subject believes this will make them better or worse off. Many papers do not provide a clear definition of antisocial punishment or report it separately from other punishment and, in addition, a good inclusive meta-analysis on social dilemmas with punishment already exists (Balliet & Van Lange, 2013). Because of negative reciprocity (and its spillovers) generally driving second and later-stage punishment behavior, and because of the generally established negative welfare outcomes this has, we instead see it as appropriate to include social dilemmas with counter-punishment in our general meta-analysis.

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methods employed (3.3). Next, we provide a descriptive overview of antisocial behavior for our complete and unweighted dataset (3.4). Finally, we define our variables and present our hypotheses (3.5).

3.1. Inclusion and exclusion criteria

We use economics papers that (a) present novel experimental data and follow the standard experimental economics methodology (i.e., financial incentives, no deception), (b) demonstrate antisocial behavior that involves subjects destroying other players' income and which is not a profit-maximizing or dominant strategy from the perspective of a purely self-interested agent who cares only about their own payoff from the game, (c) are published journal articles or working papers that were last updated after January 2000, (d) do not (only) have ultimatum game data, which is excluded (see section 2), and (e) are written in the English language.

Based on the first criterion, we excluded experiments studying hypothetical and unincentivized decisions. Papers using the strategy method, such as Dickinson and Masclet (2019), are included if all decisions are incentivized or at least one randomly chosen decision among them is payoff relevant. We also excluded papers on social dilemmas with only punishment and no counter-punishment because it is difficult to disentangle antisocial preferences with prosocial motives associated with punishment decisions in these contexts.

We also did not include second-price auctions (SPAs), in which an agent who knows he cannot win might feel tempted to raise his bid above his own evaluation to minimize the winner's profit because we could not establish a clear-cut rule to disentangle spiteful bidding from overbidding. The literature on overbidding in SPAs presents various explanations for overbidding behavior but lacks consensus on their dominance. While some studies investigate nonstandard preferences such as the "joy of winning" or "spite" (Georganas et al., 2017; Sheremeta, 2010; Tan, 2020) in SPAs, bounded rationality – specifically, the illusion that overbidding enhances winning probabilities with minimal cost – emerges as a compelling explanation of overbidding behavior (Cooper & Fang, 2008; Kagel, 1995). Cognitive abilities, computation, or strategic mistakes have been found to play a key role in shaping overbidding behavior (Georganas et al., 2017; Kagel, 1995; Kagel et al., 1987; Kagel & Levin, 1993; Lee et al., 2020; Li, 2017; Schneider & Porter, 2020). As have experience and learning (Gwin et al., 2005; Garratt, Walker & Wooders, 2012), level-k and cursed equilibrium (Brocas et al., 2017; Crawford & Iriberri, 2007), risk aversion (Andreoni, Che, Kim, 2007), feedback mechanisms (Harstad, 2000), and information provision (Zhang et al., 2019). Given the diverse explanations for overbidding and the challenges in isolating antisocial preferences from other plausibly more dominant motives, we opted to exclude SPAs from our analysis.

3.2. Literature search and dataset

Figure 1 summarizes our selection process and the number of studies excluded at each step. Details of each study are presented in Online Appendix B. Four main sources were utilized to identify relevant studies on antisocial behavior in noncooperative games: EconLit, Scopus, Web of Science, and Google Scholar. All databases were searched using combinations of keywords on types of game (JoD, MB, vendetta, contests/tournaments/competition, lotteries, riots), types of antisocial behavior or behavioral reason behind these actions (sabotage, spite, malice, envy), as well as other descriptive aspects of the studies (economic, experiment, game, game theory). The search phrase used was *experiment* AND economic AND antisocial* AND game AND (destroy* OR burn* OR nast* OR sabotage* OR contest OR competition OR tournament OR lottery OR riots OR spite* OR malice*)*. The search was limited to economic journal articles and working papers written in English published from 2000 to 2020, which yielded 2,000–5,000 hits on EconLit, Scopus, and Web of Science. An independent search on Google Scholar using the same combination of keywords resulted in 7,470 hits, of which

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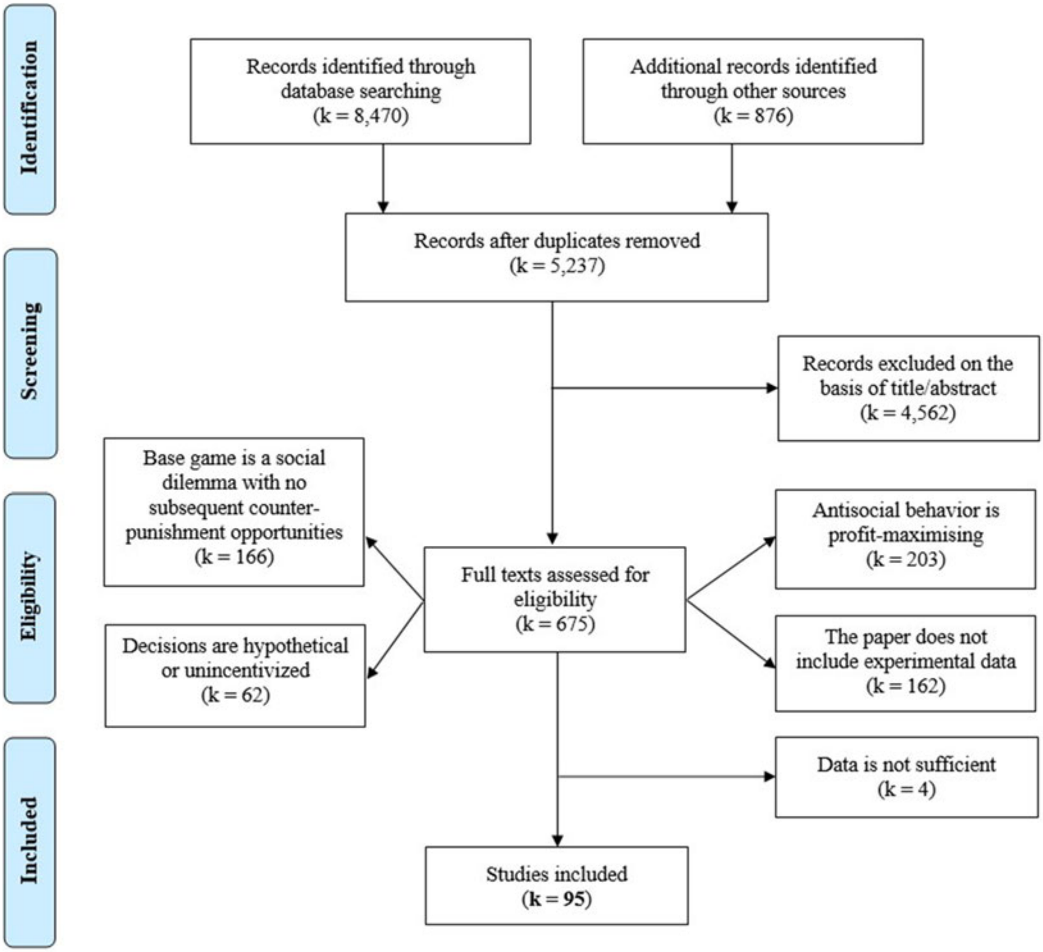

Fig. 1 Flow diagram of the literature search

Google Scholar displayed the first thousand. To identify papers on social dilemma games with counter-punishment produced from the year 2000 onward, we also conducted a search using the search phrase *counter punishment AND antisocial AND experiment* on the same databases, which returned 488–692 results.

To supplement the electronic search, we conducted a manual search through the reference sections of all papers included in the first stage. Relevant papers based on title and abstract were then extracted for full-text screening. We searched studies using Google Scholar's "cited by" function to find further research that did not contain the above keywords or did not explicitly refer to spiteful preferences as antisocial behavior and thus was not included in the initial search, identifying a further 23 studies. We also posted a topic in the ESA Experimental Methods Discussion group calling for more published and unpublished papers, and we received suggestions of 27 studies, among which 5 were added to our list as they met the inclusion criteria. All searches and screening were conducted between August and November 2020, with one exception. Specifically, on July 11, 2023, we checked whether any unpublished paper had been published by then: This was done to minimize any bias arising from extended publication lags in Economics. For instance, working papers posted online between 2018 and 2020 might not have had sufficient time for acceptance for publication and published online by the end of

Table 1 Summary of included articles by classes of games

	Number of (% of total)			*Number of observations (n)* by Geographic location			
Classes of games	Articles	Observations	Subjects	Africa	America	Asia-Pacific	Europe
Allocation games	8 (8%)	19 (5%)	1,491 (6%)	0	1	8	10
Contests	9 (9%)	43 (12%)	1,426 (6%)	0	5	0	38
Coordination games	4 (4%)	13 (4%)	592 (2%)	0	0	8	5
Money-burning games	28 (30%)	132 (37%)	9,968 (41%)	3	33	32	64
Joy of Destruction	23 (24%)	90 (25%)	6,816 (29%)	19	14	8	49
Social dilemmas	20 (21%)	46 (13%)	2,923 (12%)	0	7	7	32
Vendetta games	3 (3%)	16 (4%)	870 (4%)	0	0	5	11
Total	**95 (100%)**	**359 (100%)**	**24,086 (100%)**	**22 (6%)**	**60 (17%)**	**68 (19%)**	**209 (58%)**

Notes: The coordination game class consists of the first-strike and mobbing games. The allocation game class includes dictator, double-dictator, heaven-dictator, allocation, and voting games. N refers to the number of observations.

2020. If they did get published, the analysis presented in the main text of this paper includes the data as presented in the published version.[8]

For all articles, we first attempted to infer the necessary information from the test statistics and/or figures provided. If this was not possible, we contacted the authors to obtain the primary data between July and October 2021. After receiving data from 60 of the 76 sets of contacted authors, we ended up with sufficient measures for 95 studies. Table 1 summarizes the distribution of the included studies categorized by types of games and geographic region. Overall, our dataset includes 95 studies (24,086 participants) and 359 observations (treatments) in total using laboratory, online, and field experiments across 38 countries. Specifically, 54 studies (17,654 subjects) examined destruction in JoD/MB and vendetta games, 20 (2,923 subjects) examined social dilemma with counter-punishment opportunities, and the rest (21 studies, 3,509 subjects) looked at contest, allocation, and coordination games. The majority of experiments are conducted in Europe (57 studies, 15,575 subjects), followed by Asia-Pacific (18 studies, 4,544 subjects), America (16 studies, 2,867 subjects), and Africa (4 studies, 1,100 subjects). Lastly, of the 95 studies, 18 were unpublished at the time of the July 2023 check.

Our data is aggregated to the player type within a treatment, and an "observation" is defined as each of these treatments by type. For example, if, within a treatment, subjects are randomly assigned into Low- and High-income categories and the study reports sufficient data, we coded the treatment into four observations – High versus High, High versus Low, Low versus High, and Low versus Low. If, in the same treatment, subjects make decisions against other players from the same group and from a different group, we separated the treatment into two observations – Ingroup and Outgroup. If there is no type, we have one observation per treatment. We use n to refer to the number of these observations and k to refer to the number of studies.

[8] In the case of two working papers, additional data was collected before they were published. The online appendix provides a robustness analysis by which we treat these papers as published but only with the data as available in the working paper version; for comparison, it further provides an analysis treating only papers published by November 2020 as published (with the corresponding data available by then).

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3.3. Analytical methods

Following Gerlach et al. (2019), we use two standardized measures to compare the frequency and magnitude of antisocial behavior across diverse game settings.

Mean destruction decision rate (M_{dd}) measures the average percentage of times subjects engaged in destruction. Specifically, we calculate the mean destruction decision rate by dividing the number of decisions in which subjects decide to destroy other players' income by the total number of decision opportunities. Thus, it captures the *extensive margin* of antisocial behavior.

$$M_{dd} = \frac{Number\ of\ destruction\ decisions}{Total\ number\ of\ opportunities\ to\ destroy}$$

Mean destruction rate (M_{dr}) measures the average percentage of income subjects destroyed relative to the maximum amount that they could destroy. Specifically, it is calculated by subtracting the mean amount of income subjects destroyed by the equilibrium level of destruction determined by the game and dividing the result by the maximum amount of income subjects were allowed to destroy. Thus, it measures how much subjects are willing to destroy or the *intensive margin* of destruction.

$$M_{dr} = \frac{Amount\ destroyed - Equilibrium\ destruction}{Maximum\ amount\ allowed\ to\ destroy}$$

To analyze the two continuous dependent variables, we treat them as individual "effect sizes." We first use random-effects meta-analysis procedures to determine the average level of antisocial behavior for our full sample and for the subsamples based on game classes, separately for $\mathrm{M_{dd}}$ and $\mathrm{M_{dr}}$. This type of meta-analysis allows the true effects to vary between studies by assuming that they have a normal distribution around a mean effect (DerSimonian & Laird, 1986; Hedges, 1983; Hedges & Vevea, 1998). These are simply aggregated estimates for the overall level of antisocial behavior in the relevant samples; they do not control for independent variables. The procedure weighs each observation by the inverse of its standard error, thus assigning more importance to results from studies with larger samples and smaller deviation from the mean (Whitehead & Whitehead, 1991).

Random-effects meta-regressions allow us to include independent variables in the analysis and thus explain the between-study heterogeneity as a function of moderators (Berlin & Antman, 1994; Berkey et al. 1995; Thompson & Higgins, 2002). These models account for potential additional variability unexplained by the included moderators by assuming that the true effects follow a normal distribution around the linear predictor (Harbord & Higgins, 2008; Thompson & Sharp, 1999). They can be considered either an extension to fixed-effects meta-regression that allows for residual heterogeneity or an extension to random-effects meta-analysis that includes study-level covariates and other controls.

3.4. Overview of the complete dataset

Our dataset contains a total of 359 observations (95 studies), including 346 observations (90 studies) for the destruction decisions and 326 observations (82 studies) for the destruction rate. Table 2 reports the descriptive statistics for our complete set of variables based on the number of observations for the destruction decision rate (346 observations). On average, subjects reduced their co-players' income in 30% of all destruction opportunities and destroyed approximately 20% of the maximum amount that could be destroyed. In total, 61% of our observations examine antisocial behavior in JoD/MB games, and 85% are from published studies. Figure 2 depicts the distributions of our outcome variables, showing a considerable variation across studies.

Table 3 highlights how antisocial behavior differs across classes of games. The highest destruction is observed in the vendetta game, where subjects engaged in payoff destruction in 60% of all decisions and destroyed 51% of the maximum amount. In contrast, antisocial behavior is lowest in

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Table 2 Descriptive statistics

Variable	N	Mean	Std. dev.	Std. er.	Min	Max
Destruction decisions rate (M_{dd})	346	.3	.22	.01	0	.88
Destruction rate (M_{dr})	326	.2	.19	.01	.01	.84
Allocation games	346	.05	.21	.01	0	1
Contest	346	.12	.33	.02	0	1
Coordination games	346	.03	.18	.01	0	1
Money-burning games	346	.37	.48	.03	0	1
Joy-of-Destruction games	346	.24	.43	.02	0	1
Social dilemma	346	.12	.33	.02	0	1
Vendetta	346	.05	.21	.01	0	1
Endogenous inequality	346	.46	.5	.03	0	1
Exogenous inequality	346	.21	.41	.02	0	1
Exogenous advantageous inequality	346	.1	.3	.02	0	1
Exogenous disadvantageous inequality	346	.12	.32	.02	0	1
Group identity	346	.28	.45	.02	0	1
Outgroup	346	.1	.29	.02	0	1
Ingroup	346	.07	.26	.01	0	1
Observed by others	346	.12	.32	.02	0	1
Observed by victims	346	.54	.5	.03	0	1
Procedural fairness	346	.74	.44	.02	0	1
Pressure	346	.02	.14	.01	0	1
Costly	346	.75	.43	.02	0	1
One-shot	346	.66	.47	.03	0	1
Published	346	.85	.36	.02	0	1
Between-subject	346	.9	.3	.02	0	1
Strategy method	346	.15	.36	.02	0	1
House money effect	346	.57	.5	.03	0	1
Number of players in group	346	3.8	2.65	.14	2	14
Lab/Online	346	.84	.36	.02	0	1
Africa	346	.06	.24	.01	0	1
America	346	.16	.37	.02	0	1
Asia-Pacific	346	.2	.4	.02	0	1
Europe	346	.58	.49	.03	0	1

Notes: Data is unweighted. N refers to the number of observations. Destruction rates are available for fewer observations than destruction decision rates.

social dilemmas, with only 14% of destruction decisions and 6% of the upper cap destroyed via punishment and counter-punishment. This indicates that the difference in game contexts might have induced subjects toward feuding in vendettas and cooperating in social dilemmas, thus lowering destruction in the latter case (e.g., Abbink et al., 2011; Bolle et al., 2014; Ferraro & Vossler, 2010). It is also worthwhile noting that reported destruction rates are unconditional, implying considerably higher destruction rates by those who choose to destroy. The dependence of antisocial behavior on its context, and the interpretation of the summary statistics, are further discussed in section 5.

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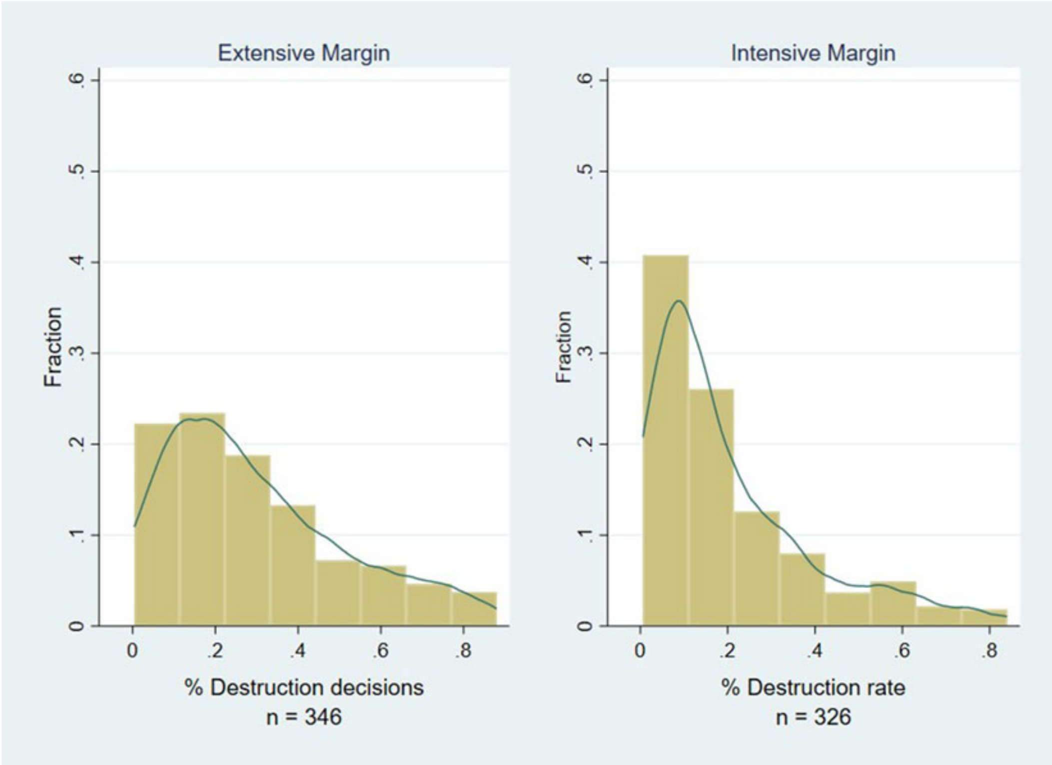

Fig. 2 Distribution of antisocial behavior

Table 3 Antisocial behavior by classes of games

	Destruction decisions *(extensive margin)*			Destruction rate *(intensive margin)*		
Classes of games	N	Mean	SD	N	Mean	SD
Allocation games	18	.25	.12	16	.21	.14
Contests	43	.48	.27	35	.15	.15
Coordination games	12	.39	.23	12	.39	.23
Money-burning games	129	.23	.16	127	.16	.12
Joy-of-Destruction games	82	.34	.19	79	.29	.2
Social dilemmas with counter-punishment	46	.14	.12	41	.06	.06
Vendettas	16	.6	.17	16	.51	.17
Total	346	.3	.22	326	.2	.19

Notes: Data is unweighted.

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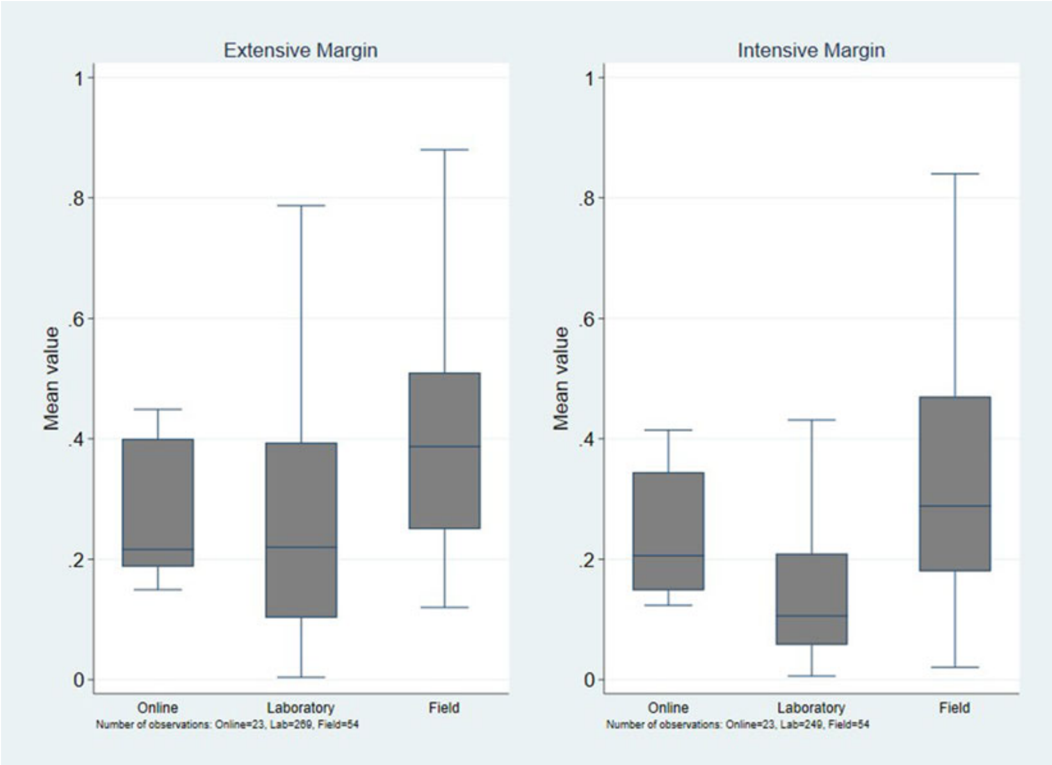

Fig. 3 Antisocial behavior by experiment type
Notes: Data is unweighted and excludes outlier values.

Figures 3 and 4 provide a breakdown of the data by region and type of experiments conducted. Figure 3 shows that antisocial behavior seems to be highest among field experiments, while Fig. 4 seems to suggest more antisocial behavior in Africa.

3.5. Independent variables and hypotheses

This subsection presents our hypotheses for the effects of potential moderators and experimental settings, namely inequality, group identity, observability, cost, and house money effect, on antisocial behavior, based on economics theories and experimental evidence. We then present the methodological and other variables to be included in the main regression analysis. Table 4 presents the complete set of independent variables used in our analysis.

Inequality and procedural fairness. In line with the theoretical and experimental literature on inequality aversion (e.g., Abbink et al., 2018, 2011; Charness & Rabin, 2002; Fehr & Schmidt, 1999; Gangadharan et al., 2021; Grosch & Rau, 2020; Zizzo, 2003; Zizzo & Oswald, 2001) we predict that subjects would behave more antisocially when they are exogenously disadvantaged in terms of income relative to when they are not.[9] This literature also shows that agents care about the procedure by which

[9] The predictions in other cases are more ambiguous. On the one hand, we may expect inequality-averse exogenously advantaged subjects to destroy less, but, on the other hand, this depends on game details, including specifically the expectation of being attacked by others. For settings in which inequality is induced endogenously (i.e., as a result of choices by the subjects themselves), other factors inducing selection in specific income brackets complicate the interpretation of any findings.

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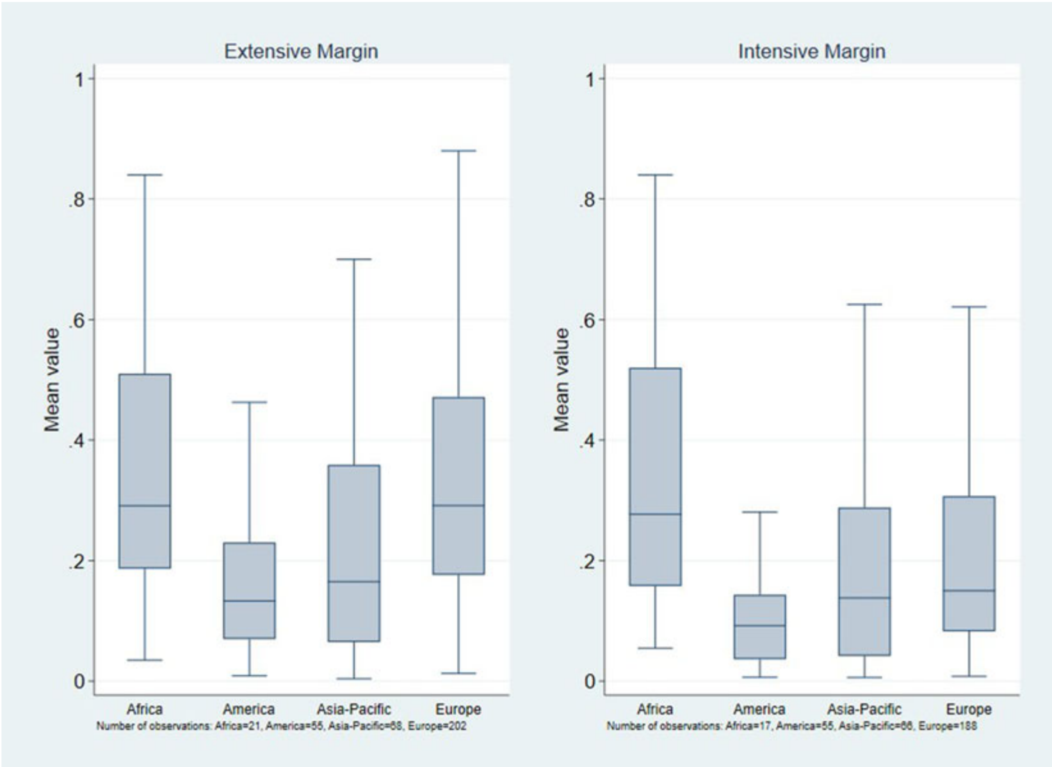

Fig. 4 Antisocial behavior by geographic region
Notes: Data is unweighted and excludes outlier values.

Table 4 List of independent variables

Variable	Type	Description/Specification
Game variables		
Game	Factorial	Classes of game: 0. Allocation games 1. Contests 2. Coordination games 3. Money-burning games 4. Social dilemmas with counter-punishment 5. Vendettas 6. Joy-of-Destruction games
Economic variables		
Inequality	Factorial	Whether inequality in earnings is due to real effort (endogenous inequality) or a certain arbitrary attribute determined by the experimenter/luck (exogenous inequality). Exogenous inequality can be classified as advantageous (when the conductor of antisocial behavior receives an unfair advantage over his or her opponent) or disadvantageous (otherwise). Four categories pertaining to the source of inequality: 0. No inequality 1. Endogenous inequality 2. Exogenous advantageous inequality 3. Exogenous disadvantageous inequality

(*Continued*)

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Table 4 (*Continued.*)

Variable	Type	Description/Specification
Group identity	Factorial	Whether group identity is induced in the experiment and whether it pertains to ingroup aggression or outgroup aggression: Type of intergroup discrimination: 0. No group identity 1. Outgroup 2. Ingroup
Observability	Factorial	Whether antisocial behavior can be observed by: 0. No one (Hidden) 1. The victims 2. Other subjects but not the victims
Procedural fairness	Dichotomous	Whether the income-generating process is fair (payoff is exogenously equally endowed or effort-based with equal returns on effort) or not (payoff is exogenously unequally endowed or effort-based with unequal returns on effort): 0. The process is fair 1. The process is unfair
Pressure	Dichotomous	0. The study does not use pressure from an authority 1. The study uses pressure from an authority
Costly	Dichotomous	0. Destruction comes at a monetary cost 1. Destruction is free
One-shot	Dichotomous	0. The game is repeated 1. The game is one-shot
Methodological and other variables		
Experiment type	Factorial	Experiment type: 0. Online 1. Laboratory 2. Field
Published	Dichotomous	0. Unpublished or working paper 1. Published journal article (at least online by July 11, 2023).
Between-subject	Dichotomous	0. Within-subject design 1. Between-subject design
Strategy method	Dichotomous	0. The study uses direct response 1. The study uses strategy method
House money effect	Dichotomous	0. Endowment is earned by subjects 1. Endowment is exogenously given by the experimenter
Number of players	Discreet	Number of players interacting in a group
Region	Factorial	0. Africa 1. America 2. Asia-Pacific 3. Europe

earnings are obtained – that is, they care about procedural fairness, and thus would behave more antisocially if the income-generating process is unfair.[10]

Hypothesis 1 (H1): *Subjects make antisocial decisions less often and destroy less income when (a) they play against a more disadvantaged player rather than an advantaged player or when there is no inequality, and (b) when there is procedural fairness.*

Group identity. The theory of social identity predicts that when placed in groups, regardless of whether they are natural or artificially induced, people tend to favor the ingroup at the expense

[10]For example, Zizzo and Oswald (2001) create an unfair wealth distribution by offering an arbitrary gift to some subjects and find that disadvantaged subjects target undeservedly earned money substantially more than they do. Fehr (2018) allows subjects to "cheat" by manipulating their own performance and finds that subjects engage in substantially more MB only if inequality results from an unfair and nontransparent income-generating process.

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of the outgroup (e.g., Akerlof & Kranton, 2000; Bauer et al., 2020; Chen & Li, 2009; Hargreaves Heap & Zizzo, 2009; Tajfel, 1978; Tajfel et al., 1979). Following this literature, we hypothesize that antisocial behavior would be lower against outgroup members relative to groups with no group identity manipulation. As the existing literature on ingroup versus outgroup effects primarily focuses on ingroup favoritism (alias prosocial behaviors) and antisocial hostilities directed at outgroup members, we do not make predictions on antisocial behavior directed toward ingroup members.

Hypothesis 2: *The frequency and extent of antisocial behavior is higher against outgroup subjects than under no-group identity manipulation (negative discrimination).*

Social observability and pressure. Both theory and experimental evidence on social observability and the effect of eye cues suggest that people tend to behave less antisocially when their action can be observed by others (e.g., Bohnet & Frey, 1999; Dea et al., 2019; Ekström, 2012; Ernest-Jones et al., 2011; Haley & Fessler, 2005; Kraft-Todd et al., 2015). Consistently with the literature, we would expect to observe less antisocial behavior when such behavior is observable to victims and to other subjects compared to when it is hidden from them. An exception to this observation occurs when individuals are peer pressured to take an antisocial action along the lines of the literature on obedience to authority (Karakostas & Zizzo, 2016).

Hypothesis 3 (H3): *The frequency and extent of antisocial behavior is lower (a) when their actions can be observed by other subjects and (b) when there is social pressure from an authority.*

Economic cost. Standard economic theory posits that the consumption of goods increases when the cost of obtaining them increases. Under the assumption that the act and outcome of destruction is an ordinary good and not a Giffen good, we expect less antisocial behavior when such behavior is costly compared to when it is costless.[11]

Hypothesis 4 (H4): *The frequency and extent of antisocial behavior is lower if acting antisocially comes at a monetary cost.*

Repetition vs. one-shot. Repeated play with the same players yields the possibility of retaliation. Therefore, we expect one-shot interaction to lead to greater antisocial behavior than in the case of repeated interaction with other players.

Hypothesis 5 (H5): *The frequency and extent of antisocial behavior is higher with one-shot than with repeated interaction.*

Methodological variables. One methodological concern is that laboratory MB exaggerates the relevance of antisocial behavior. The Experiment type variable checks the effect of having an online or a field experiment relative to a laboratory experiment. Another methodological concern is that research on antisocial behavior may suffer from publication bias. Published work may differ from unpublished work for its average scientific quality rather than just due to publication bias. Nonetheless, our Published variable provides one test for the extent of publication bias. A third methodological concern is that in at least some experiments money is given and not earned; as a result, potentially, there may be a house money effect that increases the incidence of antisocial behavior. The house money effect variable allows us to test for this. Within-subject designs may be more subject to experimenter demand effects (Zizzo, 2010) and, while generally benign (Brandts & Charness, 2011) and used infrequently, the use of the strategy method might also lead to bias: This leads to additional methodological

[11]Ideally, we would be able to test how the frequency and extent of antisocial behavior decreases with the size of the cost. However, there is too much heterogeneity across studies (not just in terms of game structures but also in destruction technologies) for us to be able to test this.

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variables. Taken together, these methodological variables speak to different dimensions that may reduce the external validity of experimental evidence on antisocial behavior.

Additional controls. We expect there to be considerable heterogeneity across games and therefore our main meta-analysis below includes controls for games. Additional controls are for the geographic region in which the study was conducted, the number of players, and whether the experiment was one-shot or repeated.

4. Results

Our results are grouped into three subsections. Section 4.1 presents a meta-analysis summary of studies by game class and economic variables. Section 4.2 examines the impact of our economic, methodological and other variables on antisocial behavior using meta-regressions on the complete dataset. Finally, section 4.3 restricts the meta-regression analysis to the largest game class subset – namely, to the JoD/MB games, to identify the robustness of our results within a more homogenous set of studies.

4.1. Meta-analysis: distributional analysis

4.1.1. Variation across game classes

First, we conduct a univariate random-effects meta-analysis on the whole dataset. The overall levels of antisocial behavior are 29% for the destruction decisions[12] (CI = [27%, 32%]) and 20% for the destruction rate (CI = [18%, 22%]). Figure 5 summarizes our findings, split by game class and publication status. Details on each observation can be found in Online Appendix B.

As shown in Fig. 5, destruction decisions and destruction rates varied significantly across games and within games. Heterogeneity (in terms of I^2 and τ^2) was above 70% in all game classes, which is expected due to large variations across game and experimental designs. Other meta-analyses studying behavior across different experimental paradigms (e.g., Gerlach et al., 2019; Li et al., 2021) also reported substantial heterogeneity of above 80% among their studies. Allowing for this variability, destruction decisions frequency and destruction rates are highest for vendetta games and lowest for social dilemmas with counter-punishment, as per Table 4 above.

To test for publication bias, we ran specialized meta-analysis regressions with game dummies, a Published dummy and game x dummies interaction terms (see Table A5 in the online appendix). The Published dummy is not significant, and no game-specific coefficients are significant at the $p = .05$ level.[13] Further tests of publication bias are presented in subsections 4.2 and 4.3.

4.1.2. Variation across subgroups by economic variables

Tables 5 and 6 provide descriptive statistics by economic variables for which we have hypotheses (descriptive statistics for other variables can be found in Tables A1 and A2 in the online appendix).

Because the number of studies looking at different factors varies across games, the results of statistical tests should be taken with caution and be verified via the meta-analysis regressions in the next subsections. The fact that there is the most destruction under inequality is a case in point: This is very likely to reflect the specific features of the large number of studies that have no inequality. More plausibly, cost appears to reduce destruction rates. Group identity does not appear to affect destruction decisions, but, when subjects are split by groups, destruction rates are higher. Being observed by

[12]There is a .3% difference in the mean estimate from the unweighted value in the previous section, leading to a rounding difference (29.6% rounding up to 30% in the previous section, 29.3% rounding down to 29% in this meta-analysis).

[13]Out of all the game-specific coefficients, only one coefficient is positive and significant at the $p = .1$ level (destruction rates for allocation games), while two are significant at the $p = .1$ level but *negatively* signed (destruction frequency and destruction rates for JoD games).

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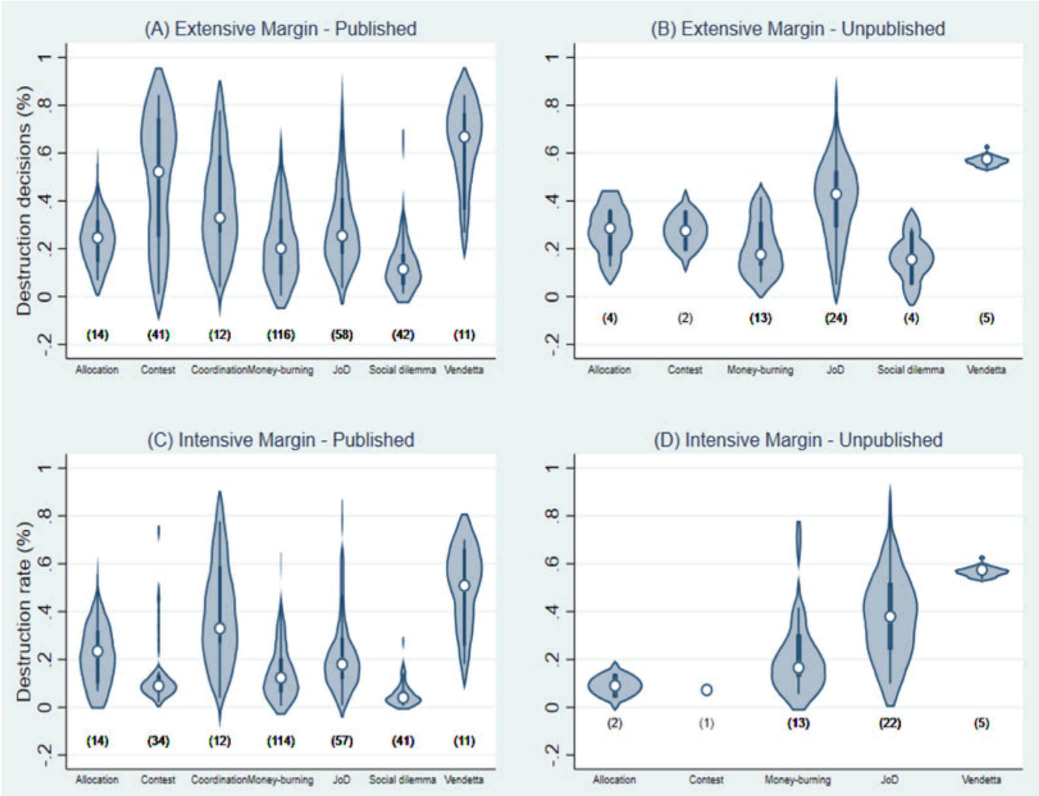

	Allocation games	**Contest**	**Coordination games**	**Money-burning**	**JoD**	**Social dilemma**	**Vendetta**
(A): Published, Destr. Decisions							
Random effect	M_{dd} = 25%	M_{dd} = 49%	M_{dd} = 40%	M_{dd} = 22%	M_{dd} = 30%	M_{dd} = 13%	M_{dd} = 61%
[95% CI]	[19%, 32%]	[40%, 57%]	[26%, 53%]	[19%, 25%]	[25%, 34%]	[10%, 17%]	[49%, 73%]
	z = 7.37	z = 11.52	z = 4.66	z = 14.68	z = 12.58	z = 7.55	z = 9.71
	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$
Observations	n = 14	n = 41	n = 12	n = 116	n = 58	n = 42	n = 11
Heterogeneity	I^2 = 97%	I^2 = 100%	I^2 = 99%	I^2 = 99 %	I^2 = 100%	I^2 = 100%	I^2 = 99%
	τ^2 = 0.02	τ^2 = 0.07	τ^2 = 0.06	τ^2 = 0.03	τ^2 = 0.03	τ^2 = 0.01	τ^2 = 0.04

Fig. 5 Plots by game class and publication status

Notes: Violin plots showing the distribution of destruction decisions/rates by game class and published/unpublished status of underpinning observations. Game classes from left to right: Allocation – Contest – Coordination – Money-burning – JoD – Social dilemma – Vendetta. The number below each violin plot represents the number of observations underpinning the corresponding violin plot. The white dot represents the median of the relevant sample. The thick bar in the center represents the interquartile range. The thin line represents the rest of the distribution, except for "outliers" that lie outside the range of the lower/upper adjacent values (first quartile – 1.5 IQR, third quartile + 1.5 IQR). On each side of the line is a kernel density estimation to show the distribution shape of the data. Wider sections of the violin plot represent a higher probability that members of the population will take on the given value, and the skinnier sections represent a lower probability. As the method involves smoothing, distributions can go below zero. The table below the figure presents the summary statistics of the random-effects models: M_{dd} is the extensive margin (with a 95% confidence interval), M_{dr} is the intensive margin (with a 95% confidence interval), k is the number of studies, and n is the number of observations. I^2 describes the percentage of variation across studies that is due to heterogeneity rather than chance (Higgins & Thompson 2002; Higgins et al., 2003), and τ^2 is the between-study variance. There are no observations for coordination games in panel (B) and for both coordination games and social dilemma games in panel (D).

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(B): Unpublished, Destr. Decisions							
Random effect	$M_{dd} = 26\%$	$M_{dd} = 26\%$	.	$M_{dd} = 21\%$	$M_{dd} = 42\%$	$M_{dd} = 16\%$	$M_{dd} = 57\%$
[95% CI]	[14%, 37%]	[10%, 42%]	.	[15%, 28%]	[33%, 50%]	[3%, 29%]	[54%, 60%]
	z = 2.57	z = 3.12	.	z = 6.45	z = 10.09	z = 2.49	z = 37.81
	p < 0. 001	p < 0.05	.	p < 0.001	p < 0.001	p < 0.001	p < 0.001
Observations	N = 4	N = 2	N = 0	N = 13	N = 24	N = 4	N = 5
Heterogeneity	$I^2 = 73\%$	$I^2 = 77\%$	.	$I^2 = 100\%$	$I^2 = 98\%$	$I^2 = 100\%$	$I^2 = 0\%$
	$\tau^2 = 0.01$	$\tau^2 = 0.01$	.	$\tau^2 = 0.01$	$\tau^2 = 0.04$	$\tau^2 = 0.02$	$\tau^2 = 0.00$
(C): Published, Destr. Rate							
Random effect	$M_{dr} = 23\%$	$M_{dr} = 15\%$	$M_{dr} = 39\%$	$M_{dr} = 14\%$	$M_{dr} = 24\%$	$M_{dr} = 5\%$	$M_{dr} = 48\%$
[95% CI]	[16%, 30%]	[10%, 20%]	[26%, 53%]	[12%, 16%]	[19%, 29%]	[4%, 7%]	[37%, 60%]
	z = 6.17	z = 5.69	z = 5.83	z = 12.85	z = 9.84	z = 7.53	z = 8.17
	p < 0.001	p < 0.001	p < 0.001	p < 0.001	p < 0.001	p < 0.001	p < 0.001
Observations	N = 14	N = 34	N = 12	N = 114	N = 57	N = 41	N = 11
Heterogeneity	$I^2 = 99\%$	$I^2 = 100\%$	$I^2 = 100\%$	$I^2 = 99\%$	$I^2 = 100\%$	$I^2 = 100\%$	$I^2 = 99\%$
	$\tau^2 = 0.02$	$\tau^2 = 0.02$	$\tau^2 = 0.06$	$\tau^2 = 0.01$	$\tau^2 = 0.03$	$\tau^2 = 0.002$	$\tau^2 = 0.04$
(D): Unpublished, Destr. Rate							
Random effect	$M_{dr} = 8\%$	$M_{dr} = 7\%$	.	$M_{dr} = 21\%$	$M_{dr} = 40\%$	.	$M_{dr} = 57\%$
[95% CI]	[0%, 18%]	[0%, 14%]	.	[16%, 29%]	[30%, 48%]	.	[54%, 60%]
	z = 1.67	z = 2.10	.	z = 5.46	z = 9.06	.	z = 37.81
	p < 0.01	p < 0.05	.	p < 0. 001	p < 0. 001	.	p < 0.001
Observations	N = 2	N = 1	N = 0	N = 13	N = 22	N = 0	N = 5
Heterogeneity	$I^2 = 79\%$	$I^2 = .$	.	$I^2 = 100\%$	$I^2 = 99\%$	.	$I^2 = 0\%$
	$\tau^2 = 0.004$	$\tau^2 = .$	.	$\tau^2 = 0.02$	$\tau^2 = 0.04$	.	$\tau^2 = 0.00$

Fig. 5 (Continued.)

others seems to lead to less destruction, while pressured by an authority seems to increase it. Other results are in a more plausible direction.

4.2. Meta-regressions on the complete dataset

We begin this section by examining the impact of our economic variables on the levels of destruction. We then explore how the games, methodological variables, and geographic location influence antisocial behavior. Table 7 presents our meta-regressions on the complete dataset.[14]

Games variables. Different game classes generally display robust differences. Relative to the largest class of games (JoD/MB games), vendetta games exhibit a considerably larger frequency of destruction decisions and larger destruction rates by about 1/3 when including all covariates. Coordination games also exhibit more destruction decisions and higher decision rates. Contest games also exhibit more destruction decisions, while the negative coefficient on the destruction rate is insignificant once we control for all covariates. Social dilemmas with counter-punishment have less destruction, though the negative coefficient on the destruction rates is again insignificant once we control for

[14]Meta-regressions with separate categories for JoD and MB games are included in Table A3 in the online appendix. Overall, results are robust to this change in-game categorization, with only minor changes in significance and magnitude of few coefficients. However, given that by definition, the only difference between JoD and MB games is whether there is inequality in endowments, the interpretation of the inequality variables – as well as of the JoD and MB games dummy variables – becomes problematic due to collinearity.

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Table 5 Summary of extensive margin by economic variables

Group	N	M_{dd}	Lower 95%	Upper 95%	Q	P > Q	τ^2	% I^2	H^2
Cost									
Costless	87	.34	.29	.38	21,134.38	0	.04	99.4	166.59
Costly	259	.28	.25	.31	94,697.3	0	.05	99.96	2,417.45
Inequality									
No inequality	138	.41	.37	.45	23,959.69	0	.06	99.79	482.2
Endogenous	136	.21	.18	.24	40,830	0	.03	99.93	1,469.5
Exogenous advantageous	32	.18	.14	.23	2,883	0	.01	99.45	181.2
Exogenous disadvantageous	40	.26	.21	.3	2,978.01	0	.02	99.26	134.65
Group identity									
No group	288	.29	.27	.32	110,000	0	.05	99.95	2,077.51
Outgroup	33	.30	.23	.38	3,097.99	0	.05	99.84	640.77
Ingroup	25	.28	.21	.35	1,347.86	0	.03	99.6	248.05
Observability									
Not observed	156	.28	.24	.31	24,930.6	0	.05	99.86	732.83
Observed by others	4	.14	.09	.19	7.22	.07	.00	56.56	2.3
Observed by victim	186	.31	.28	.34	89,082.01	.00	.05	99.96	2,312.62
Procedural fairness									
Unfair	91	.26	.22	.30	15,302.5	0	.03	99.71	343.04
Fair	255	.31	.28	.33	100,000	0	.05	99.96	2,463.58
Pressure									
No pressure	339	.29	.27	.31	120,000	.00	.05	99.95	1,872.52
Pressure	7	.41	.23	.58	111.21	.00	.05	93.61	15.65
One-shot									
Repeated	117	.26	.21	.31	37,131.12	0	.07	99.97	3,942.75
One-shot	229	.31	.29	.34	48,845.78	0	.03	99.82	559.52
Overall	**346**	**.29**	**.27**	**.32**	**120,000**	**0**	**.05**	**99.95**	**1,844.17**

Notes: Cost refers to whether the destruction was free or costly. *Inequality* is *endogenous* when it is due to effort, *exogenous* when it is due to luck, *exogenous advantageous* when a subject has an advantage over their opponent, and *exogenous disadvantageous* when their opponent has an advantage. *Procedural fairness* = 1 if income is exogenously and equally endowed or endogenously earned with equal returns on effort for all subjects, and = 0 otherwise. To induce *group identity*, a subject is matched against either members of their own group (*ingroup*) or players from other groups (*outgroup*). Antisocial behavior can be *observed by other players, observed by the victim*, or no one (*not observed*). *Pressure* = 1 when subjects are under pressure to comply by an authority and = 0 otherwise. Finally, the game can be played only once (*one-shot*) or repeatedly (*repeated*).

all covariates. Overall, the picture is broadly consistent with the one emerging from Table 4 and Fig. 5 above.

Economic variables. The coefficients on exogenous advantageous inequality are negative and significant at the 1% level in Models 2 and 3 and at the 10% level in Model 5 but become insignificant when controls are added in Model 6. This indicates that, when subjects play against disadvantaged opponents, they engage in destruction significantly less often but destroy just as much income as

Table 6 Summary of intensive margin by economic variables

Group	N	M_{dd}	Lower 95%	Upper 95%	Q	P > Q	τ^2	% I^2	H^2
Cost	85	.26	.22	.30	11,696.41	0	.03	99.66	290.87
Costless	241	.18	.16	.21	230,000	0	.03	99.99	8,075.39
Costly									
Inequality	127	.28	.24	.31	22,335.06	0	.04	99.87	775.33
No inequality	127	.12	.10	.15	27,878.4	0	.02	99.99	7,046.21
Endogenous	32	.17	.11	.22	3,071.78	0	.02	99.77	435.09
Exogenous advantageous	40	.23	.18	.28	3,138.41	0	.02	99.41	169.15
Exogenous disadvantageous									
Group identity	268	.19	.17	.21	230,000	0	.03	99.99	7,132.55
No group	33	.27	.20	.34	3,026.99	0	.04	99.84	639.92
Outgroup	25	.26	.19	.33	1,452.42	0	.03	99.62	261.03
Ingroup									
Observability	154	.16	.13	.18	15,344.72	0	.03	99.86	716.33
Not observed	4	.13	.04	.21	36.08	0	.01	87.11	7.76
Observed by others	168	.24	.21	.27	210,000	0	.04	99.99	9,578.62
Observed by victim									
Procedural fairness	235	.19	.16	.23	11,975.32	0	.03	99.79	472.23
Unfair		.21	.18	.23	230,000	0	.04	99.99	9,024.53
Fair	319								
Pressure	7	.20	.18	.22	240,000	0	.03	99.98	6,190.84
No pressure		.41	.23	.58	111.21	0	.05	93.61	15.65
Pressure	111								
One-shot	215	.11	.09	.14	28,128.5	0	.02	99.99	8,169.11
Repeated		.25	.22	.28	62,142.09	0	.03	99.9	1,012.15
One-shot									
Overall	**326**	**.2**	**.18**	**.22**	**240,000**	**0**	**.03**	**99.98**	**6,252.85**

Notes: Cost refers to whether the destruction was free or costly. *Inequality* is *endogenous* when it is due to effort, *exogenous* when it is due to luck, *exogenous advantageous* when a subject has an advantage over their opponent, and *exogenous disadvantageous* when their opponent has an advantage. *Procedural fairness* = 1 if income is exogenously and equally endowed or endogenously earned with equal returns on effort for all subjects, and = 0 otherwise. To induce *group identity*, a subject is matched against either members of their own group (*ingroup*) or players from other groups (*outgroup*). Antisocial behavior can be *observed by other players, observed by the victim*, or no one (*not observed*). *Pressure* = 1 when subjects are under pressure to comply by an authority and = 0 otherwise. Finally, the game can be played only once (*one-shot*) or repeatedly (*repeated*).

when there is no inequality. The coefficient on exogenous disadvantageous inequality is negative and significant at the 1% level in Model 2, which is counterintuitive; however, this effect attenuates and is only marginally significant when accounting for other factors in Model 3.[15] It may be an effect

[15]In Table A3 in the online appendix, when we include separate categories for the JoD and MB games, the coefficients on exogenous disadvantageous inequality decrease in magnitude but become statistically significant at the 5% level. This is counterintuitive; however, we should exercise caution in interpreting these results due to the limited number of observations

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Table 7 Meta-regressions on complete dataset

	Destruction frequency *(extensive margin)*			Destruction rate *(intensive margin)*		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Game – Base: JoD/MB games						
Allocation games	−.00957 (.0457)	−.0564 (.0459)	−.0286 (.0439)	.0116 (.0404)	.0162 (.0405)	.0589 (.0367)
Contest	.21*** (.0313)	.202*** (.0347)	.197*** (.0364)	−.0568** (.0285)	−.0248 (.0298)	.0473 (.0289)
Coordination games	.13** (.055)	.131** (.056)	.214*** (.0569)	.192*** (.0462)	.227*** (.0465)	.33*** (.0456)
Social dilemma	−.128*** (.0298)	−.0824** (.0397)	−.0494 (.04)	−.145*** (.0265)	−.0751** (.0353)	−.0251 (.0342)
Vendetta	.333*** (.0476)	.227*** (.0528)	.314*** (.0525)	.308*** (.0405)	.272*** (.0444)	.328*** (.0435)
Economic Variables						
Inequality – Base: No inequality						
Endogenous		−.107*** (.0273)	.0269 (.0372)		−.0786*** (.0236)	−.0138 (.0308)
Exogenous advantageous		−.207*** (.0429)	−.14*** (.0467)		−.0697* (.0366)	−.00799 (.0383)
Exogenous disadvantageous		−.126*** (.0396)	−.086* (.0476)		−.0244 (.0335)	.0259 (.0387)
Procedural fairness		−.0416 (.0299)	−.0919*** (.0311)		−.000824 (.025)	−.0247 (.025)
Group identity – Base: No group identity						
Outgroup		.0276 (.0346)	−.004 (.0331)		.048* (.0291)	.026 (.0267)
Ingroup		−.0114 (.0395)	−.041 (.0377)		.0333 (.0331)	.0222 (.0302)
Observability – Base: Hidden						
Observed by others		−.104 (.091)	−.152* (.0881)		−.047 (.0766)	−.075 (.0709)
Observed by victim		.0344 (.0253)	.00938 (.0245)		.0239 (.023)	.00573 (.0214)
Pressure		.0494 (.0732)	.128* (.0716)		.137** (.0624)	.282*** (.059)
Costly		−.025 (.0264)	−.0227 (.0267)		.0237 (.0231)	−.0291 (.0223)
One-shot		.045* (.0241)	−.0414 (.0269)		.0816*** (.0206)	.0385* (.0216)
Methodological and Other Variables						
Published			−.0323 (.0278)			−.0753*** (.0246)

(*Continued*)

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Table 7 (*Continued.*)

	Destruction frequency (*extensive margin*)			Destruction rate (*intensive margin*)		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Between-subject			−.0106			.0495
			(.0452)			(.0389)
Strategy method			.0553*			.0898***
			(.0311)			(.0275)
House money effect			.113***			−.0129
			(.0356)			(.0306)
Number of players			.00209			.0054
			(.00529)			(.00458)
Experiment type – Base: Laboratory						
Online			.0841			.128***
			(.0598)			(.0496)
Field			.153***			.154***
			(.0375)			(.0327)
Region – Base: Europe						
Africa			−.0604			.0262
			(.0434)			(.0383)
America			−.101***			−.0792***
			(.027)			(.0215)
Asia-Pacific			−.13***			−.0617***
			(.0277)			(.0228)
Constant	.264***	.343***	.349***	.202***	.138***	.158**
	(.0129)	(.0456)	(.0791)	(.0112)	(.0397)	(.068)
Observations	346	346	346	326	326	326
p-value	7.24e − 26	1.04e − 32	1.81e − 50	1.31e − 24	4.86e − 36	7.06e − 65

Notes: All models were specified using random-effects meta-regressions, which weigh each observation by the inverse of its standard error, assigning more weight to results from studies with larger samples and smaller deviation from the mean (Harbord & Higgins, 2008; Thompson & Sharp, 1999).
Standard errors in parentheses, * $p < .10$, ** $p < .05$, *** $p < .01$.

of (expected) reciprocity given that lower destruction frequency is observed from the exogenously advantaged subjects they are playing with. Additionally, we found a negative effect of endogenous inequality on the destruction rate, indicating that subjects destroy less from their opponent when the endowment is earned rather than exogenously given. Nonetheless, this effect vanishes with the addition of controls. The coefficient on procedural fairness is significant at the 1% level in Model 3, indicating that subjects make fewer destruction decisions when the income-generating process is fair.

Result 1 (H1): *Subjects engage in destruction less often when (a) there is exogenous advantageous inequality and (b) when there is procedural fairness.*

The coefficient on "Outgroup" is marginally significant at the 10% level in Model 5, but the effect disappears when controlling for covariates. No other coefficients on "Ingroup" and "Outgroup" are statistically significant.

(79 observations) and the almost perfect correlation (Pearson r = −.89) between the exogenous disadvantageous inequality variable and the JoD category.

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Result 2 (H2): *We did not find evidence for negative discrimination toward outgroup subjects.*

The coefficient on "Observed by others" is negative and marginally significant (once again controlling for covariates) at the 10% level in Model 3. Subjects appear to engage in less antisocial behavior when their actions can be observed relative to when they are hidden. The effect size is relatively large: On average, subjects make 15% fewer destruction decisions when their action is observable to others, but the 10% level significance points to large variability across observations. "Pressure" increases the destruction rate and possibly the frequency of destruction, though this effect is only marginally significant at the 10% level for the latter.[16]

Result 3 (H3): (a) *In the complete sample, subjects appear to make antisocial decisions less often when their actions can be observed by other participants and the experimenters but not when they are observed by the victims.* (b) *Pressure by an authority to destroy leads them to destroy more.*

The coefficients on "Costly" are not significant in any of the models.

Result 4 (H4): *We did not find an effect of cost on either destruction decisions or destruction rates.*

One-shot interaction significantly increases destruction rates by around 8% in Model 5, though the effect is halved and at the 10% significance level once we control for covariates.[17]

Result 5 (H5): *Destruction rates tend to be higher in one-shot interactions.*

Methodological variables. The coefficient on "Published" is negative and significant at the 1% level in Model 6, indicating a *negative publication bias* – on average, subjects destroyed about 7% less in published studies. This complements previous results in section 4.1 where, when not controlling for covariates, we broadly found no evidence of a positive publication bias. We will discuss these findings in section 5.

The use of the strategy method appears problematic in that it appears to increase both the frequency and the rate of destruction. There is a house money effect on the frequency but, fortunately, not on the rate of destruction. The results of other methodological variables are reassuring for laboratory experiments. Specifically, it does not matter whether the experiment is between-subject or within-subject. Online studies have a higher destruction rate (by around 13%, $p < .01$) and field studies have a higher frequency of destruction decisions and a higher destruction rate (in both cases by around 15%) relative to laboratory experiments.

Result 6: *The use of the strategy method biases destruction frequency and rates upward, whereas house money effects only affect destruction frequency.*

Result 7: *Controlling for covariates, there is evidence for a negative publication bias for destruction rates.*

Result 8: *Online studies have higher destruction rates than laboratory experiments. Field studies have more antisocial behavior than laboratory experiments, for both destruction decisions and destruction rates.*

[16]However, this is driven by a single study (Karakostas & Zizzo, 2016). As a robustness check, we excluded Karakostas and Zizzo (2016) along with the variable Pressure and ran the meta-regressions again. The results are similar to Table 7, with no significant differences in either size or significance of coefficients.

[17]We ran specialized regressions for 16 papers in which round-level data is available and did not find evidence for a last round effect for both the extensive and intensive margins of antisocial behavior. Results are provided in Table A4 in the online appendix.

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Geographical variables. Both levels of destruction are significantly lower in experiments conducted in America and Asia-Pacific compared to those conducted in Europe (see Models 3 and 6), which is consistent with the descriptive statistics. There is no difference in the levels of destruction detected among studies conducted in Africa and Europe.

4.3. Meta-regressions on JoD and MB experiments

We conjecture that a key explanation of why we may be getting such weak results in relation to our key economic variables, and possibly in relation to some methodological variables, is the sheer heterogeneity that we observe between and within game classes. We can control at least for the former by running a meta-analysis that focuses on what are often considered as antisocial games, the JoD and MB games. In the meta-regression that follows, we consider both the JoD and MB games as a single category due to the limited number of observations, and thus power, for each of these games separately. The class of JoD/MB games is the one game class that has around 62% of our observations and that therefore has enough statistical power to enable a meaningful meta-analysis to be conducted. We can then verify the extent to which within-game class heterogeneity can be explained by our economic (and other) variables.

The JoD/MB dataset includes 51 studies (222 observations and 16,784 subjects) in total, with 47 studies (211 observations and 16,466 subjects) for the destruction decisions and 44 studies (206 observations and 15,573 subjects) for the destruction rates. Table 8 presents our meta-regressions on the JoD/MB dataset.

A negative effect of exogenous inequality on destruction frequency and rate disappears conditioning on the covariates. Controlling for covariates, endogenous inequality increases the frequency of destruction. but not the destruction rate. Procedural fairness consistently reduces the frequency of destruction: The effect is large (around 9%).

Result 9 (H1): *In the JoD/MB games, (a) controlling for covariates, there is no evidence to support greater antisocial behavior by exogenously advantaged subjects; (b) procedural fairness decreases the destruction frequency.*

There is now some evidence of negative discrimination against outsiders: Destruction rates are higher with respect to subjects who are not part of one's own group (at $p < .01$ and $p < .10$ in Models 3 and 4, respectively).

Result 10 (H2): *In the JoD/MB games, there is some evidence of negative discrimination in the form of higher destruction rates against outgroup subjects.*

In relation to the social information variables, the picture is the same for the JoD/MB sample as for the complete sample.

Result 11 (H3): *In the JoD/MB games,* (a) *subjects appear to make antisocial decisions less often when their actions can be observed by other participants and the experimenters but not when they are observed by the victims;* (b) *pressure by an authority to destroy leads them to destroy more.*

We now find an effect of cost on the extensive margin of antisocial behavior: Costly destruction leads to lower destruction decisions relative to when it is costless ($p < .05$). The coefficient on one-shot is now significant in all models ($p < .01$).

Result 12 (H4): *In the JoD/MB games, costly destruction appears to decrease the frequency of destruction decisions but not affect destruction rates.*

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Table 8 Meta-regressions on JoD/MB dataset

	Destruction frequency *(Extensive margin)*		Destruction rate *(Intensive margin)*	
Economic variables	Model 1	Model 2	Model 3	Model 4
Inequality – Base: No inequality				
Endogenous inequality	−.0338 (.0319)	.0917** (.0407)	−.112*** (.0317)	−.0108 (.0376)
Exogenous advantageous	−.164*** (.0433)	−.0706 (.0479)	−.106** (.042)	−.0308 (.0428)
Exogenous disadvanta-geous	−.0807** (.0384)	.00886 (.0481)	−.0257 (.0369)	.0385 (.0424)
Procedural fairness	−.0717** (.0299)	−.0894*** (.0324)	.0297 (.0289)	.0226 (.0293)
Group identity – Base: No group identity				
Outgroup	.0683** (.0324)	.0468 (.0316)	.0640** (.031)	.0519* (.0277)
Ingroup	.0360 (.0365)	.00738 (.0347)	.0418 (.0352)	.0303 (.0306)
Observability – Base: Hidden				
Observed by others	−.15* (.0902)	−.199** (.0892)	−.127 (.0874)	−.108 (.0804)
Observed by victim	−.0213 (.0287)	−.0347 (.0287)	−.0208 (.0296)	−.00186 (.0266)
Pressure	.101 (.0632)	.234*** (.0615)	.123** (.0613)	.254*** (.0555)
Costly	−.0311 (.0275)	−.0628** (.0282)	.0329 (.0277)	−.0108 (.0265)
One-shot	.224*** (.0309)	.132*** (.0411)	.130*** (.0287)	.0813** (.0339)
Methodological Variables				
Published		−.0577** (.027)		−.106*** (.0243)
Between-subject		.116** (.051)		.0845* (.045)
Strategy method		.0151 (.0324)		.11*** (.0312)
House money effect		.0558 (.0381)		−.0208 (.0366)
Number of players		.000422 (.00576)		.000603 (.00522)
Experiment type – Base: Laboratory				
Online		.165*** (.0567)		.134*** (.0508)
Field		.201*** (.0378)		.0857** (.0363)

(*Continued*)

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Table 8 (*Continued.*)

Economic variables	Destruction frequency *(Extensive margin)* Model 1	Model 2	Destruction rate *(Intensive margin)* Model 3	Model 4
Region – Base: Europe				
Africa		−.021 (.0387)		.049 (.0369)
America		−.043 (.0322)		−.0636** (.0279)
Asia-Pacific		−.0534 (.0398)		.000753 (.0337)
Constant	.203*** (.0573)	.108 (.0917)	.109* (.0561)	.0957 (.0802)
Observations	211	211	206	206
p-value	1.37e − 18	1.66e − 32	1.74e − 15	4.83e − 39

Notes: All models were specified using random-effects meta-regressions which weight each observation by the inverse of its standard error, assigning more weights to results from studies with larger samples and smaller deviation from the mean (Harbord & Higgins, 2008; Thompson & Sharp, 1999).

Result 13 (H5): *In the JoD/MB games, one shot interaction increases the frequency of destruction decisions as well as destruction rates.*

There is evidence of a negative publication bias: The coefficient on "Published" is significant at the 5% level for destruction decisions and at the 1% level for destruction rates. The strategy method still increases the destruction rate, and both field studies and online studies have more antisocial behavior than laboratory experiments for both destruction decisions and destruction rates. A between-subjects design now increases destruction frequency and possibly also the destruction rate.

The picture with geographical variables is less pronounced than in the complete sample. In part this may be a matter of statistical power, but point coefficients also tend to be smaller. Destruction rates, however, remain statistically significantly lower in America than in Europe.

5. Discussion and conclusion

Our meta-analyses investigated the potential determinants of antisocial behavior and how such behavior varies across different aspects of the experimental design. A key choice was in how to define the boundaries of antisocial behavior. We only considered behavior that entails subjects reducing another player's income without any pecuniary gains and is not a dominant strategy of a payoff-maximizing, rational agent. This excludes behavior that may be partially explained by self-interest or is difficult to disentangle from other motives. Examples of the latter, the exclusion of which we respectively justified in sections 2 and 3.1, include punishment in the public goods game when not followed by counter-punishment[18] or overbidding in the SPAs.[19]

[18]Where defined within this literature, antisocial punishment refers to the sanctioning of people who behave prosocially (Herrmann et al., 2008; Irwin & Horne, 2013; Rand & Nowak, 2011; Thöni, 2014). However, as discussed in a section 2 footnote, particularly with first-stage punishment, there is an unclear relationship between the punishment and whether the subject believes this will make them better or worse off, and many papers do not help in this regard in terms of the data provided.

[19]A spiteful agent is interested in increasing their own payoff while simultaneously decreasing the payoff of their opponent (Brandt & Weiß, 2001; Brandt et al., 2005; Kimbrough & Reiss, 2012). Overbidding could be classified as spiteful when an agent, knowing they would lose for sure, places a bid higher than her own value and lower than the winning bidder's value in order to raise the price they have to pay and thus reduce their profit (e.g., Cooper & Fang, 2008; Kirchkamp & Mill, 2021; Kogan & Morgan, 2010; Nishimura et al., 2011). "Joy of winning" (Tan, 2020) could also be an antisocial preference. However, as discussed in section 3.1, there are compelling and plausible bounded rationality and other reasons for overbidding.

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Overall, based on the general meta-analysis, we find that subjects engaged in socially inefficient payoff destruction in 29% of the times and destroyed about 20% of the maximum destruction allowed. Given the across-games and across-settings heterogeneity, the game level values are more worth focusing on. The highest level of destruction is observed in the vendetta game, while destruction is lowest in social dilemmas with counter-punishment. One explanation is that the moral cost of destruction is lower in the JoD/MB and vendetta games, which are contexts where antisocial behavior is salient by nature (Abbink & Herrmann, 2011; Zizzo, 2003; Zizzo & Oswald, 2001). This result also suggests that the economic setting matters for nastiness: The social dilemma context where cooperation is socially desirable may have prompted subjects toward cooperating in these games (e.g., Ferraro & Vossler, 2010; Zizzo, 2010), as opposed to rivalry and nasty feuding in vendettas (e.g., Abbink et al., 2011; Bolle et al., 2014).

There is significant heterogeneity not just across but also within different classes of games. This heterogeneity makes inference regarding economic variables difficult, particularly given the limited number of studies looking at antisocial behavior – let alone all the key economic variables – in most classes of games. The limited evidence on the effect of costly destruction may be explained by the bluntness of the variable, which simply states whether MB is costly or free – not the price; however, given the heterogeneity of pricing schemes, it was not possible to have a more precise price measure.

Nevertheless, some results emerge. There is evidence that the frequency of destruction is reduced with social information, specifically being observed by others (but not the victim) and pressure by an authority. Procedural fairness leads to significantly fewer destruction decisions in both meta-analyses. Within JoD/MB games, destruction rates are higher toward outgroup members. While much of the literature focuses on the prosocial benefit of group identity (such as Akerlof & Kranton, 2000; Tajfel et al., 1971; Tajfel, 1978; Tajfel et al., 1979), this result shows that group identity may be damaging toward outsiders (as emphasized in Eswaran & Neary, 2022; Hargreaves Heap & Zizzo, 2009; Zizzo, 2011). We also find that one-shot interaction plausibly increases antisocial behavior, at least in the JoD/MB subsample, but the effect is robust to the general meta-analysis for destruction rates.

An unexpected finding was the broad lack of support for H1 on the effect of payoff inequality on destruction. If anything, *both* exogenous advantage *and* disadvantage lead to a *lower* destruction frequency according to the general meta-analysis (Table 7). The picture is less clear in the JoD/MB meta-analysis (Table 8): The coefficients on exogenous advantage or disadvantage are significant in Model 1 – with the point estimate for exogenous advantage larger – but their significance disappears in Model 2 while endogenous inequality becomes significant. In both meta-analyses, in terms of destruction rates, no inequality variables are significant controlling for covariates. Not controlling for them, the coefficient on exogenous advantage has the predicted negative sign, and that on endogenous inequality is also significantly negative.

It is difficult to interpret these findings. We cannot exclude that they possibly simply reflect unobserved and uncaptured across-studies heterogeneities. One conjecture is that, in sufficient studies with endogenous inequality, the inequality is seen as justified and therefore attenuating antisocial behavior, though it is not clear why this is not fully captured by the procedural fairness variable. The limited or no evidence of asymmetry between exogenous advantage and disadvantage may be easier to explain: The structure of many studies entails that, if I believe or observe that the exogenously advantaged will be less likely to destroy, then from a reciprocity viewpoint, I may be in turn less likely to destroy even if I am exogenously disadvantaged. It is of course also possible that inequality aversion does not operate in the antisocial behavior domain as it does in the prosocial behavior domain. Clearly, further research is needed.

The use of the strategy method biases destruction rates upward in both the complete dataset and in the JoD/MB subsample; this is consistent with Oosterbeek et al. (2004), who found a higher rejection rate in their meta-analysis of ultimatum games. The results on house money effects and between-subject manipulations are less clear. House money effects increase only the frequency of destruction decisions in the complete sample but not in the JoD/MB sample. Somewhat counterintuitively, between-subject manipulations raise destruction decisions and possibly destruction rates in the JoD/MB sample. A possible explanation could be that in within-subject designs, subjects draw inferences about the nature of the decision problem that leads them to be more compliant to social norms not to destroy. This would be an instance of a class of experimenter demand effects discussed by Zizzo (2010) and would suggest that within-subject designs tend to underestimate destructive behavior.

Another issue that applies particularly to the simplest and acontextual (Abbink & Herrmann, 2011) type of JoD design is whether, when there is only one choice to make in the experiment, a choice to destroy may be motivated by a desire of "doing something." While this cannot be ruled out in baseline cases, a lot of the interest in these studies is in the between-treatment variation, which cannot be explained by this conjecture. Furthermore, in a very simple setting Sadrieh and Schröder (2016) try to control for this bias and still find antisocial behavior. What it does point to, however, is that, depending on the specifics of the experimental designs, the fact that behavior in JoD games may be driven by more than "joy of destruction," often deliberately so (as for example in Karakostas & Zizzo, 2016).[20]

Two methodological findings speak to the value of experimental findings on antisocial behavior and particularly conventional laboratory experiments. First, and relevantly for any type of experiment, we do not find evidence that there is a publication bias toward finding antisocial behavior. This is true in aggregate and in the sample of JoD/MB experiments. As discussed in section 4.1.1, Table A5 in the online appendix shows the lack of a positive publication bias. If anything, in Table 7 (for destruction rates) and 8 (for both destruction frequency and destruction rates), and controlling for covariates, the evidence is one of a negative publication bias. Table A7 in the online appendix shows that, if we do not consider the July 2023 check of which working papers have been published up to then, the negative publication bias remains significant only at the 10% level, and is smaller, though there is still no positive publication bias. However, if we consider as Published the working papers that have been published up to our July 2023 check – allowing time for the most recent working papers to be published but making no other change to the dataset – Table A7 shows that this is enough to get the same negative publication bias results as in Tables 7 and 8. We conclude that a possible negative publication bias may have become more prominent in recent years. Based on Table A5, JoD games might be particularly affected. Second, we find a higher level of antisocial behavior in field and online experiments compared to laboratory experiments, both in the complete dataset and the JoD/MB dataset. Our results on laboratory versus field lends support to the external validity of laboratory experiments (Alm et al., 2015; Anderson & Bushman, 1997; Armantier & Boly, 2012; Stoop, 2014) and specifically to the importance of antisocial behavior as elicited in the laboratory literature.

Sanjaya (2023) does a meta-analysis on destruction rates in JoD/MB games; however, due to a smaller sample size (44 studies, 88 observations), he can only consider a small set of independent variables. He uses a combination of covarying proxies to determine whether the data is from a field experiment, such as a students dummy. His results are consistent with ours regarding field experiments having more destruction. In his sample he finds a "Working paper" effect, but we do not replicate this in our larger sample. He has a "one-shot" variable that is positive and significant in one out of two of his more ambitious models. His "costly" variable is not significant once covariates are included, and ours is also not significant for destruction rates. His one regional variable, "Europe," displays a significantly positive coefficient; what we find is that in the JoD/MB games sample, this

[20]It might be argued that the mobbing game could also be affected by activity bias. However, while an activity bias might explain why a subject may choose to target someone rather than remain inactive, it does not explain why subjects would coordinate in mobbing someone, which is what the mobbing game (as a coordination game) is about. As noted in section 2, our mobbing game data is at the group-decision level.

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effect is driven by less destruction than in America-based experiments. While Sanjaya (2023) does not look at within-sample heterogeneity, we found this as a major concern.

Apart from the scale and scope, an advantage of our study over Sanjaya (2023) lies in our meta-analysis technique. While he uses standard linear regressions with observations weighted by the number of observations, our meta-analysis procedure weighs each study by the inverse of the variance of the effect estimate (i.e., one over the square of its standard error). Thus, larger studies with smaller standard errors, contribute more to the weighted average estimate than smaller studies with larger standard errors, which improves the precision of the estimate (Whitehead & Whitehead, 1991). Our meta-regressions also account for the between-study variance unexplained by the included moderators by assuming that the true effects follow a normal distribution around the linear predictor (Harbord & Higgins, 2008; Thompson & Sharp, 1999).

If we consider together our methodological results on the absence of any upward bias of antisocial behavior in laboratory experiments, on the broad absence of publication bias, and on the excessive heterogeneity obstructing firm conclusions on key economic variables, we conclude that there is value in more laboratory experiments that systematically build on paradigmatic experimental designs to enable comparability and the identification of key economic drivers of antisocial behavior.

Supplementary material. The supplementary material for this article can be found at https://doi.org/10.1017/eec.2024.10.

Data availability statement. The usual disclaimer applies. The list of papers included in the meta-analyses, and the corresponding dataset, can be found in the online appendix. The replication material for the study is available at https://osf.io/rbupm/.

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