Exploring the triarchic conceptualisation of psychopathic traits in relation to economic decision-making

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Abstract

The concept of a relationship between psychopathy and the hypothetical *Homo economicus* has long existed, but empirical support for either positive or negative relationship is mixed. The present study examines a cohort of 88 individuals, mostly university undergraduates and postgraduates of mixed gender and ethnicity, on performance in economic games against their scores in a psychopathy measure and an anxiety inventory. Modest relationships were found between psychopathy values and monetary offers made in dictatorial positions, but no significant relationships were found for first-round offers. No effect was found for anxiety. The results, while interesting, suggest further research into clinical and non-clinical populations as a comparison is necessary.

Lay Abstract

Behavioural economists use the term *Homo economicus* to describe a hypothetical human with perfectly rational decision-making, one who maximises personal gains and minimises personal losses. Research has suggested that high-psychopathy individuals represent this concept in the real world. In the study detailed here, university undergraduates were measured for psychopathy and anxiety scores, and tested in game-like situations where sums of money are split. It was found that increased psychopathic traits reduced monetary offers in certain types of game, but not in others. This is explained by the participants being considered a 'non-clinical' sample, meaning that the expression of psychopathic traits is not high enough to be considered a dominant personality framework.

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Dedicated to my grandfather, George Frederick Peter Lambert, whose passion for science was matched only by his fierce love and support for his family, and whose example I will always follow.

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Introduction

Theoretical Background of Psychopathy

Psychopathy as a construct has fascinated scientific and lay populations for centuries. First conceptions occurred in the 1800s, where it was labelled *manie sans delire* or "insanity without delirium" by Pinel (1806). A modern interpretation of psychopathy was first codified by Cleckley (1976) in his book "The Mask of Sanity", characterised as a pathological lack of impulse control, affective judgement, and fear. These traits were said to be 'masked' by a semblance of normal functioning, with psychopaths initially presenting as affable and confident. Cleckley notably did not consider violent behaviour to be a requisite trait for psychopathy; indeed, he explained it as little more than a consequence of their feckless attitude. Psychopathy was included in the DSM-II under the auspice of "Personality Disorder, Antisocial type" (APA, 1968), though this was not a set of diagnostic criteria for psychopathy but rather an attempt to better diagnose a cluster of similarly-presenting disorders.

Later conceptualisations took a decidedly more forensic stance, favouring observable traits like antisocial behaviour and criminality, which had been considered more useful diagnostically by contemporaries of Cleckley such as McCord & McCord (1964) and Robins (1978). Indeed, Robins' work largely influenced the conception of the related but distinct personality construct, antisocial personality disorder (ASPD) in the DSM-III (APA, 1980), and a focus on antisocial behaviours that persist from youth into adulthood as a basis for ASPD diagnosis was continued in the DSM-IV (APA, 2000). The general view of this approach is that it increased the reliability by which clinicians could diagnose 'psychopathy' at the cost of the diagnostic validity of the construct (Lykken, 1995) by equating criminality and violent behaviour with psychopathy; subsequent analysis has shown that most of those diagnosed under DSM III/IV conceptions of ASPD do not present with the personality features of psychopathy – namely Cleckleyan features such as low anxiety, glibness, guiltlessness, and poverty of affect (Skeem, Polaschek, Patrick, & Lilienfeld, 2011) indicating that ASPD and psychopathy are not synonymous.

A testament to this conceptualisation is the Psychopathy Checklist, Revised (PCL-R; Hare, 2003); the checklist is defined from Cleckley's original characteristics, and grades them on a 4-point Likert from 0-3, but dropped the positive adjustment features that Cleckley considered to be critical for identifying someone as a psychopath (Skeem, Polaschek, Patrick, & Lilienfeld, 2011). Hare's work was heavily influenced by criminal populations – the PCL-R was designed with and for criminal samples, and as a result the PCL-R takes on a more taxonomic view in which psychopathy can be diagnosed as either being present or not, based on widely held cut-off scores (30 out of 40,

sometimes lowered for research purposes). Supporting studies for this 'latent taxon' model are many, but according to a review of such studies by Walters *et al*. (2011) pervasive methodological issues produce weak correlations that are often mistaken for a taxon.

Pursuing a taxonomic view of psychopathy suggests there is a set of criteria by which an individual can be deemed 'psychopathic' or not. These taxonomic views interpret psychopathy as a unitary construct (Neumann, Hare, & Newman, 2007) and provide cut-off scores for the PCL-R that indicate where this line may be drawn, but this has faced criticisms in contemporary literature. For instance, it is clear that even under Hare's own work with the PCL-R, there are at least two correlated 'factors' that show divergent aspects (Hare, 2003; Patrick & Bernat, 2010), and these factors tend to be correlated in opposing directions with external variables (Hicks & Patrick, 2006). Additionally, there is remarkably little consensus on what symptom criteria for psychopathy may exist. It has been suggested that these results are more in line with a continuous and configural approach to psychopathy conceptualisation, wherein a constellation of traits are expressed to variable extent in the population, making 'psychopaths' an otherwise heterogeneous group. Researchers have shown evidence for dimensional traits, which manifest to varying degrees (Murrie, et al., 2007; Edens et al., 2006), indicating that 'psychopaths' are not fundamentally different from other individuals in kind, but rather by degree; the world is not divided into non-psychopaths and psychopaths, but that there is a continuum of behaviour at one end of which are 'more psychopathic' individuals (Edens, Marcus, Lilienfeld, & Poythress Jr., 2006). This focus also allows identification of potential 'subtypes' of psychopathy (Feilhauer, Cima, Korebrits, & Kunert, 2012).

The focus on criminality has been criticised as losing sight of the original definition of psychopathy, especially the maintainenance of a charming façade concurrent with poverty of affect and empathy. This has been found to also gloss over highly psychopathic individuals who are 'successful', which has led to many studies claiming a negative correlation between criminal 'success' and psychopathy (Aharoni & Kiehl, 2013) since the 'successful' ones tend not to be a recorded and readily available. To compound the issue, it is reckoned that the predominance of the PCL-R flouts the main principle of psychological measures which is that they are fallible and constantly evolving to keep pace with a growing understanding of the construct under investigation (Cronbach & Meehl, 1955); in this context, the PCL-R has become almost synonymous in research communities with the psychoapthic construct, limiting and stagnating further research (Skeem, Polaschek, Patrick, & Lilienfeld, 2011). The PCL-R has gained this reputation through its prevalent use in national and international criminal justice systems (Skeem et al., 2011; Skeem & Cooke, 2010). Skeem *et al*.'s judgement here is that a classification approach tends to not only ignore 'milder' psychopathy, but also only focus on incarcerated populations rather than the full breadth of 'highly psychopathic' individuals. Indeed,

the measure may not actually contribute much to treatment of incarcerated psychopaths, since recidivism has been found to be negatively correlated with moral development, irrespective of instance of psychopathic traits (Van Vugt, et al., 2011). In contrast, Ben-Yaacov and Glicksohn (2018) have found that the non-incarcerated population have as much to offer in terms of insights into the emotional and intellectual capabilities of high-psychopathy individuals, and that a continuous approach better frames this research.

A variety of other measures including the Psychopathic Personality Index (PPI, Lilienfeld & Andrews, 1996; PPI-R, Lilienfeld, Widows, & Staff, 2005) have sought to explore the construct as it manifests in non-criminal populations, deeming the construct not dependent on criminal behaviour. These newly developed models allowed experimentation on non-incarcerated populations, widening the scope of the construct (Sellbom, 2010). In fact, by the reckoning of some researchers the move to include non-psychopathic populations has produced similar increases in the general understanding of psychopathy, and improvements to child psychiatry, as the PCL-R has done for criminality in psychopathy (Kirkman, 2002).

Of these, the Triarchic Model of Psychopathy (Patrick, Fowles, & Krueger, 2009) has received a great deal of attention. Rather than adopting the more common 2-factor approach (Hare, 2003), the Triarchic Model is similar to contemporary models in that it is multidimensional, and it is indexed by three subscales in the Triarchic Psychopathy Measure (TriPM, Patrick C. , 2010); these subscales are Boldness (fearless dominance, high self-confidence, includes positive adjustment features), Meanness (poverty of empathy and of interpersonal relationships), and Disinhibition (poor foresight, affect, and impulse control). Even before publication of the triarchic conception by Patrick, Fowles, and Krueger, it had been suggested that a 3-factor model best encompassed the dimensions of psychopathy (Brinkley, Diamond, Magaletta, & Heigel, 2008), with even Hare's Psychopathy Checklist having been recommended towards a 3-factor interpretation (Cooke & Michie, 2001); and support for the validity of the model has been found in a variety of studies by forming strong associations with other established measures (Hall, et al., 2014; Drislane, Patrick, & Arsal, 2014; Sellbom & Phillips, 2013; Marion, et al., 2013; Stanley, Wygant, & Sellbom, 2012), and by extensive use on large population samples it has gained a reputation for good internal reliability as well (Drislane, et al., 2014; Skeem, Polaschek, Patrick, & Lilienfeld, 2011). The TriPM does have limitations given its nature as a self-report inventory, and it is relatively new in comparison, but seems to have great promise as a tool to explore the Triarchic conceptualisation (Evans & Tully, 2016). It also has the advantage of being capable of distinguishing psychopathy and ASPD through the Boldness subscale, which promotes its construct validity further (Venables, Hall, & Patrick, 2014).

Use of Economic Games

Other tools beyond personality inventories have been employed to explore the interpersonal aspects of the psychopathic construct. Economic games are generally widely used in both mathematics and within psychology, as well as in the overlapping discipline of behavioural economics; they were originally established within mathematical theory (especially Game Theory) to "analyse strategic decision-making among economic agents" (Zhao & Smillie, 2014). They are in essence decision-making tasks involving two or more hypothetical agents. Within psychological research, they have been extensively used due to their replicability, and the ease with which they can be controlled to model social interactions, making them ideal for connecting naturalistic data with the underlying theory or construct. What is noteworthy about the use of economic games specifically in the context of psychopathy and related pathologies is that because the observed behaviour is genuine (Exadaktylos, Espín, & Brañas-Garza, 2013), they capture and measure instances of antisocial traits in a way that self-report measures cannot and do not (Clark, et al., 2016). The most notable contribution economic games may have provided to behavioural economics is to discount the notion that the perfectly rational actor *homo economicus* (Pareto, 1906; though may be older) should ever be used as a foundation in economic theory, since these games prove that even in situations such as the Prisoner's Dilemma Game (Flood & Dresher; cited in Poudstone, 1992) where the rational option is always to 'screw over your neighbour', humans will not follow this behaviour consistently (if at all; Dohmen, Falk, Huffman, & Sunde, 2009). Of course, debate exists over whether these 'economically expected' playstyles are truly representative of 'rational' courses of action, since in real life individuals have future interactions to consider. Nonetheless, economic games in general have been shown to be useful for delineating clinical groups from other populations (Koenigs, Kruepke, & Newman, Economic decision-making in psychopathy: a comparison with ventromedial prefrontal lesion patients, 2010), as well as displaying how individuals in specific groups react in highly controllable situations (Rezlescu, Duchaine, Olivola, & Chater, 2012).

The Ultimatum Game (Güth, Schmittberger, & Schwarze, 1982), in particular, has a long history with psychological research. In most behavioural contexts, ultimatum games are considered the benchmark trade-off game between social concepts of 'fairness' and reciprocity, and economic rationality; players are either tasked with dividing a reward between themselves or another player, or are tasked with accepting/rejecting the split made by another player; in the event of a rejection, neither player receives any reward incentive. 'Rational actor' models predict acceptance of any offer, irrespective of size. In reality, relatively small offers (< 20–30% of total reward) are rejected around 50% the time (Bolton & Zwick, 1995; Guth et al., 1982). The game has been found to be especially useful in psychopathy research, both in observing how high-psychopathy individuals differ

in their approach to fairness/self-interest problems; and also in observing how even when making the same decisions as a 'healthy' sample, high-psychopathy individuals may still have differing neurological and somatic responses (Vieira, et al., 2013; Osumi & Ohira, 2010; Koenigs, Kruepke, & Newman, 2010; Sanfey, et al., 2003).

Similar in format to ultimatum games, the Dictator Game (Kahneman, Knetsch, & Thaler, 1986) differs from the former by cutting the game off after the first round, noting how a player will split money when there is no danger of retaliatory rejection (though in multiple iteration dictator games, there is the possibility of reciprocation). They can be used in reverse to gauge the extent to which participants believe these offers are 'fair', especially given the lack of need for co-operation. Dictator games are used less often within psychopathy research, but still provide a useful insight into how individuals may respond in comparison with other groups (Koenigs, Kruepke, & Newman, Economic decision-making in psychopathy: a comparison with ventromedial prefrontal lesion patients, 2010). In this context, 'rational actor' models would predict an extreme split resulting in the proposer taking the maximum possible amount, but in reality this tends not to happen (Guala & Mittone, 2010). As yet, the dictator paradigm has not been fully explored in relation to psychopathy, but can be used as a contrast to ultimatum games (Sharp, 2012); in the present study, they are used to ascertain the potential effect of social pressure and economic utility on decision-making.

The format of the Trust Game takes elements of both ultimatum games and dictator games (Berg, Dickhaut, & McCabe, 1995). In a trust game, the first player is endowed with a sum of money that they may choose to divide between themselves and another player however they wish; the share of the endowment given to P2 is then multiplied by a fixed amount, and P2 may now divide this sum between themselves and P1. The first round of the trust game operates similarly to an ultimatum game, in that P1 must weigh self-interest with the potential actions of the second player, but the second round of the game is almost exactly a dictator game interaction, with the sole difference being that P2 has had prior interaction with P1 and is therefore informed better and has greater likelihood of reciprocal behaviour. The rationale behind such games is that 'trust' is ignored by standard mainstream economics, when in fact the primacy of trust in real-life analogues of economics is what reveals the errors in economic predictions; Johnson and Mislin's (2011) meta-analysis found that people do tend to trust the second player more than a 'rational actor' model would suggest. It is reckoned by some that trust games could better explore how high-psychopathy individuals approach and react to mixed-motive negotiations (ten Brinke, Black, Porter, & Carney, 2015), in part because of the exploration of trust dynamics with a more variable reward potential.

These three economic games will be used in the present study for two reasons; firstly, they are all tried and tested in isolation, and all have at least some documented use comparing high-psychopathy samples with normal samples; and second, they are divided into ultimatum bargaining (ultimatum game and first round of trust game) and dictator-style (dictator game and second round of trust game) games, which allows for comparison between the paradigms.

Economic Games and Psychopathy

Economic games have become prominent among measures associated with psychopathy research, in part due to their ability to assess with high reliability the self-interest aspects of the constellation of traits that make up psychopathy (Sharp, 2012; Sharp, Monterosso, & Montague, 2012). They also allow comparison between high- and low-psychopathy groups, as well as comparison with the expected 'rational' behaviours theorised to be part of the adaptive significance of psychopathy within an evolutionary perspective, wherein exploiting reciprocation behaviours is a successful strategy for a small percentage of the population (Glenn, Kurzban, & Raine, 2011). Unfortunately, the literature is somewhat split on several key notions, with research evidence seemingly pointing in separate directions with regards to the behaviours psychopaths display in game situations.

On the one hand, the prominence of impulsive reactions reckoned to be integral in some conceptions of psychopathy (Sanfey, et al., 2003) suggests that higher instance of psychopathy should be related to poorer co-operation in economic games, with higher rates of defection and 'spiteful' rejection. For instance, in mixed motive negotiation games, high trait psychopathy has been found to lead to greater earnings in only those games that rely on competition, with the individuals seeming to be under an illusion of competition even where co-operation would make more sense (ten Brinke, et al., 2015). These conclusions have been drawn by a variety of studies looking at different economic games, though it is most prevalent with the Prisoner's Dilemma Game (Gervais, et al., 2013; Mokros, et al., 2008). Studies have also shown that psychopaths may be self-interested in dictatorial situations, but not in a way that suggests economic rationality; Koenigs, Kruepke, and Newman (2010) examined the relation between primary/secondary psychopathies with ventromedial prefrontal cortex (vmPFC) lesion patients, and found that individuals with psychopathy acted similarly to patients with vmPFC lesions by accepting unfair offers less often than a so-called 'healthy' population in ultimatum games (UG), while simultaneously proportioning less endowment in a dictator game (DG) to the second player. In recent overviews of this and other similar studies, it has been observed that non-cooperative behaviour seems the norm in economic game situations (King-Casas & Chiu, 2012). This is seemingly confirmed in a real-life context by

Boccio and Beaver's (2015) analysis of the effect of psychopathy on economic successes across life; it was found that greater presence of psychopathic traits was negatively correlated with household income, and such individuals were fired more often. The analysis seems to indicate that the behavioural impulses of the psychopath render them less successful in society where co-operation is necessary. However, this analysis looked largely at psychopaths who had been incarcerated at some point, and thus failed to take into account those psychopaths for whom 'success' had been attained by masking their psychopathy (whether intentionally or not) and who were therefore not included within the study's high-psychopathy sample (Aharoni & Kiehl, 2013). Indeed, a follow up experiment by Koenigs et al. (2011) showed that low-anxiety, high-psychopathy populations actually exhibit abnormally utilitarian judgements in moral decision tasks.

There has been a fair amount of discord on this issue. For one, Koenigs, Kruepke, and Newman's (2010) findings are based on much smaller samples than have been used in other studies; and the identification of 'primary' psychopaths using a combination of PCL-R and anxiety measures has been critiqued for the poor delineation of trait fear and trait anxiety (Sylvers, Lilienfeld, & LaPrairie, 2011), since it is fear dominance, not low anxiety, that high-psychopathy individuals present with. Consequent to this, various research has denounced the use of anxiety to denote psychopathy sub-categories, noting no predictive or moderating effect of anxiety when studied alongside standard psychopathy measures (Burns, Roberts, Egan, & Kane, 2014; Gao & Tang, 2013).
Additionally, the results run counter to other research which suggests that individuals with high trait-psychopathy operate more in line with economic predictions (Osumi and Ohira, 2010; Rilling, et al., 2007). In fact, even Koenigs *et al.* (2010) found that psychopaths act in more rational self-interst during Dictator Games, despite not doing so in ultimatum paradigms; and again in moral decision tasks (Koenigs et al., 2011). The critical difference seems to be one of research population – students scored as being high in psychopathic traits reject fewer unfair UG offers and tend towards less reciprocation, acting in the interest of the self (Ibáñez, et al., 2016; Osumi & Ohira, 2010), suggesting that psychopaths in non-criminal populations display utilitarian behaviours; but incarcerated psychopaths seem to display the opposite which has been interpreted as anger regulation deficit (Koenigs, et al., 2010). Koenigs *et al.*'s use of a dual Ultimatum/Dictator Game paradigm suggests differences may also exist in high-psychopathic populations between approaches to different economic games, rather than acting in a single manner across all situations. Research on a large (n = 446) nonstudent Japanese sample seems to confirm that psychopathic individuals align with the *Homo economicus* model (Yamagishi, et al., 2014); in this study, psychopathy also predicted economic success in opposition to Boccio and Beaver (2014), suggesting that individual differences play as significant a part in economic game performance as psychopathy as a whole does. For

instance, 'economically rational' behaviour has been associated with lateral PFC activation being higher than anterior insula activation (De Neys, et al., 2011), and it has been found that neither cognitive load nor fairness affected rejection rates in psychopaths (Vieira, et al., 2013) despite high VmPFC activity in relation to accepting unfair offers (compared with the low-psychopathy group); this is in addition to evidence that psychopaths suffer different deficiences of moral reasoning to non-psychopathic criminal populations, leading researchers to conclude that individual differences in attentional allocation may drive some failings (Brazil, et al., 2012). This falls in line with more general findings that behavioural economics has a tendency to ignore individual differences in favour of more aggregate grouping, where moral deliberation style (Clark, et al., 2017) or reward-responsiveness (Scheres & Sanfey, 2006) seem to have more useful predictions in economic games.

It is therefore clear that the matter is far from settled. More evidence is needed to explore how individuals may differ across multiple economic games, especially when such individuals are assessed via the triarchic conception of psychopathy. Following on from the work of Scheres & Sanfey (2006), Osumi and Ohira (2010), and Vieira *et al.* (2013), the present study seeks to further explore the usefulness of economic games in relation to psychopathy; notably, in the ability of economic games to denote aspects of selfishness or capitulation to social pressure/norms of reciprocity. In light of the mixed results from studies involving incarcerated and student populations, and informed by the current understanding of psychopathy as a continuum of traits rather than a taxonomic disorder, the present study will examine the correlational strength and regressional power of a relationship between psychopathy and performance in economic games, and also the predictive power of anxiety scales as a measurement device, since Koenigs *et al*.'s (2011) conclusions seem based on a difference of anxiety levels in the incarcerated population. Additionally, the present study will seek to fill a gap in contemporary psychopathy literature by utilising greater ecological validity to explore how behaviour in economic games can serve as an analogue for real-life responses, something which is somewhat lacking in research on behavioural economics in psychopathy; this will be achieved through face-to-face interaction, and a reduced number of trials per game to counteract trial fatigue (Zizzo, 2010; Yu & Zhou, 2007), a factor that reduces ecological validity. Finally, the present study will seek to offer a view on how expression of psychopathic traits in a non-incarcerated population moderates self-interested decision-making.

The present study assesses the relationship in a sub-clinical non-incarcerated population between psychopathic traits, as measured by the Triarchic Psychopathy Measure, and economic decision-making in three economic games. The main hypothesis for the present study is that a negatively-correlated relationship exists between instance of psychopathic traits and offers made across all economic games; secondarily, it is hypothesised that greater instance of psychopathic traits will

predict more unfair offers in Dictator-style rounds than in Ultimatum-style rounds, as noted in Scheres and Sanfey (2006). A minor aim of the study will be to explore the usefulness of anxiety as a mediating factor in expression of psychopathic behaviour.

Methods

Design

The experiment consisted of two independent continuous variables, the Triarchic Psychopathy Measure (TriPM) and the State/Trait Anxiety Inventory (STAI); four dependent variables were measured, which were the outputs of the three economic games – (a) the average offer made by participants across 3 rounds of the Ultimatum Game, (b) the average offer made across 3 rounds of the Dictator Game, (c) the average initial offer made across 3 rounds of the Trust Game, and (d) the average return offer in the Trust Game, measured as a percentage. In total, 3 groups of 3 trial trials were run; the smaller number of trials was used to reduce experimental fatigue and thus increase the natural validity of the decision making.

Participants

Participants were recruited via opportunity sampling; adverts were posted on the Durham Psychology department's 'participant pool', and on social media. In total, 88 participants were recruited, mean age 19.85, SD=2.05, with a range of ages from 18 to 27.

Of these, 59 identified as female (mean – 19.24, SD – 1.76), 27 identified as male (mean – 21.11, SD – 2.14), and 2 identified as non-binary (mean – 21.00, SD – 0.00). In the sample, 70.45% were of white-British ethnicity, 12.5% identified as white-European, 3.4% were Chinese and the remaining 13.65% consisted of other ethnicities.

Participants were rewarded for their time with either £5 cash, or 1 hour 'participant credit' if they were Psychology undergraduates. A further reward of £1 was given for accumulating the most money in any given game; psychology students could exchange this £1 for 15 minutes extra participant pool credit. The participants were not made aware of what the 'further rewards' specifically entailed until after the experiment, but were told during the experiment that they would win a portion of their total allocation at each game's end.

During the initial briefing period, participants were given opportunity to ask questions about the use of their data and the structure of the experiment. Though many participants did not ask questions, those that were asked included whether or not their data would be seen by others, to which they

were told that the data would be anonymised in a password-secured document and only seen by others as part of the present thesis; and whether or not they would be allowed to withdraw from the experiment or know their results, to which they were informed that they may ask to be withdrawn from the study at any time, and that they would be able to learn the outcome of the experiments but not the nature of their individual data.

Materials

Instructions for each game were printed to be read by the experimenter before the running of each game (Appendix D). Offer/response slips (Appendix D) were made which reiterated the game's central premise "You have a sum of £10, how do you split this?". Each slip for a given game was identical, save for a letter in the upper-right corner that identified to the experimenter which participant the offer had originated from.

Triarchic Psychopathy Inventory *(Appendix E)*

The Triarchic Psychopathy Measure was designed by Patrick (2010) to operationalise the triarchic model of psychopathy, emphasising a more continuous measurement of 'psychopathic traits', which fall under three subunits of 'boldness', 'meanness', and 'disinhibition'. These subscales aim to capture the 'fearless dominance', 'impulsiveness-irresponsible', and 'callous-aggressive' features of the psychopathy construct. The TriPM consists of 58 questions, answered on a 4-point likert scale ranging from 'very true' to 'very false'; 19 questions fall under the boldness subscale, 19 questions fall under the meanness subscale, and 20 questions fall under the disinhibition subscale.
The subscales themselves are inter-related to only a modest degree, around a .2-.3 correlation, but together predict an overall PCL-R score to a higher degree than this would suggest (r~.5; Patrick, 2010); the measure is reported to successfully tap into key aspects of the psychopathy construct and of construct validity therein (Sellbom & Phillips, 2013; Stanley, Wygant, & Sellbom, 2012).
Cronbach's alpha for the present study of mixed gender undergraduates is .93, slightly higher than the internal consistency of earlier studies which has been around .86 (Drislane, et al., 2014).

State-Trait Anxiety Inventory, Form Y *(Appendix F)*

The State-Trait Anxiety Inventory (STAI) is commonly used in clinical settings to diagnose anxiety and to distinguish it from depressive syndromes (Spielberger, Gorsuch, Lushene, Vagg, & Jacobs, 1983),

though it does align with depression and other internalising psychopathologies (Gao & Tang, 2013). Form Y has 20 items for assessing trait anxiety (anxiety as inherent characteristic) and 20 for state anxiety (anxiety at the time of measurement). The 40 items are rated on a 4-point Likert scale (from "Almost Never" to "Almost Always"). Higher scores indicate higher anxiety. It has been included in the present study to explore potential predictive power of anxiety tests for psychopathy, since some studies (Gao & Tang, 2013; Sylvers, Lilienfeld, & LaPrairie, 2011; *et al*.) have claimed the use of anxiety tests may be redundant in the delineation of psychopathic sub-groups. Internal consistency coefficients for the scale have ranged from .86 to .95; test-retest reliability coefficients have ranged from .65 to .75 over a 2-month interval (Antony, Orsillo, & Roemer, 2001; Spielberger et al., 1983). Considerable evidence attests to the construct and concurrent validity of the scale (Spielberger, 1989).

Procedure

Participants were recruited through opportunity sampling throughout the Durham university student population. Participants were timetabled into groups of 4 based on availability, and although some participants knew each other, this did not detract from the design since all aspects of the in-game interactions were made anonymous (with a crucial exception in the Dictator Game, noted later). All participants having arrived for a given session, they were led into the experiment room and sat facing opposing corners. An information sheet (*Appendix A*) containing basic details of the experiment was read by each, and a consent form (*Appendix B*) was signed. Unknown to the participants (at least initially), they were assigned a letter A, B, C, or D; this was used during the games to identify a participant's offers with their identity for later analysis, and more importantly to ensure that when offers were swapped anonymously the experimenter could keep track of whom had sent it originally. Participants were seated in opposite-facing corners of the same room, so that they were aware of the other participants but not able to see or influence each other's actions.

The Ultimatum Game was run first. In the Ultimatum Game, player one divides a sum (in this case, £10) between themselves and a second participant. The second participant receives this offer and may choose to accept or reject it. Acceptance results in both players receiving money as dictated by the offer; rejection means neither player receives anything. Participants were given a slip of paper with instructions (*Appendix D-1*), and given opportunity to ask questions. Once any questions had been answered to satisfaction, the participants were given a slip of paper (*Appendix D-2*) on which to make their offers. All offers were collected simultaneously and were then redistributed anonymously; once a decision had been made to accept or reject the offer, the slips were collected

in and a fresh slip was given to each participant. This process was repeated for 3 iterations of the game. Participants were told that they would receive a portion of their overall winnings, incentivising non-equal offers.

The Dictator Game was run second. The Dictator Game runs with the same premise of dividing a sum of money, but there is no second round – the offer is final. In the present experiment, instructions (*Appendix D-3*) were given and also read aloud by the experimenter, and another offer/reply slip was given (*Appendix D-4*). The players on the receiving end of the offer were given the slip, and all participants were offered the chance to learn the identity of the 'dictator' for that round. Since each participant would only receive one offer from each of the others, this information could not be used to reciprocate offers; instead, the threat of losing anonymity (stated at the beginning of the game) was incentive for players to offer more equal amounts, as a form of social pressure. As before, all slips for a given round were collected and redistributed, then taken away at the onset of the next round. This was run for 3 iterations, and once again participants were informed that their winnings would inform the allocation of further reward.

Lastly, the Trust Game was run. This was titled the Multiplication Game in the present experiment to avoid cuing behaviour (towards 'trusting' other participants with more money). Once again, instructions (*Appendix D-5*) and response slips (*Appendix D-6*) were disseminated, with the instructions being explained by the experimenter. Participants divide the initial sum, and the sum offered to the second participant was multiplied by 5. The second participant (now acting as a dictator) decides how much of this increased endowment to allocate back to the original participant. Participants were also re-coded so that anonymity could be retained after learning identities in the Dictator Game. Once again, slips were taken in, redistributed anonymously, and then taken in once again having been completed; a total of 3 iterations of the game were run.

In the Ultimatum and Trust Games, participants were told to always give at least £1 in the first round of the game. In the case of the former, this is to ensure the second player isn't presented with £10/0 and £0/0 options, as there is no incentive even for a hypothetical *Homo economicus* to ever allow another player advantage. In the latter game, a sum of 0 prevents the second player having a choice in how much is returned, and would therefore invalidate response data for that game. However, in both the Dictator Game and the second round of the Trust Game participants have the option of giving £0 offers, since that does not impact further rounds of the game. All games were run in the same experimental session, and game order was switched between experiments to prevent the occurrence of order effects. In between games, short distraction tasks involving counting backwards from 100 in 3s, or simple drawing tasks, were performed to decrease possibility of remembering

handwriting from the previous game and using it to reciprocate. These tasks were random and arbitrary, designed to prolong time between games rather than occupy specific cognitive resources.

Having finished the three games, participants were given the Triarchic Psychopathy Inventory (*Appendix E*) and State/Trait Anxiety Inventory (*Appendix F*) to complete. Participants were given opportunity to ask questions before and during completion of the inventories (for instance, clarifying the meaning of the phrases "I feel at ease" or "I feel secure" in the STAI) though they were encouraged to answer each question without deliberation. Once all participants had finished, a debrief form (*Appendix C*) was given for them to read, and further opportunity for questions about the experiment was allowed. At this time, several participants asked whether they were 'psychopaths', to which they were informed that the TriPM is not a diagnostic tool and that high instance on the measure does not denote someone clinically; they were also reminded that their individual results would not be divulged. Participants were also informed that, should they wish, they could remove their data from the experiment at any time before the end of the experiment. None chose to do so.

To analyse experimental results, IBM SPSS (for Windows) v.22 was used for correlational and regressional analysis, and MatLab for linear mixed-effect model analysis.

Results

Table 1: descriptive statistics for independent variables, and dependent variables

	Mean	Std. Deviation	Range	Min./Max. value
TriPM Score	116.32	22.32	104	81 / 185
STAI Score	81.48	19.88	90	42 / 132
Mean UG Offer	4.20	.900	4	£1 / £5
Mean DG Offer	3.27	1.49	5	£0 / £5
Mean TG Offer	4.96	2.52	9	£1 / £10
Mean TG Response (%)	29.88	15.96	64.57	1.33% / 65.90%

Initial exploration of the data revealed no outliers for any of the dependent variables, except in the case of the "mean UG offers" dependent variable (DV). Since analysis has shown that the standard SPSS outlier algorithm (which adopts a 1.5x multiplier) is inaccurate 50% of the time, and that a larger multiplier should be used (Hoaglin & Iglewicz, 1987), and that SPSS marks outliers calculated

with a 3x multiplier differently, the larger multiplier was used; no outliers were observed under this criterion. Therefore, analysis proceeded as planned. Unless otherwise noted, all testing is two-tailed.

A Shapiro-Wilk test of normality was run on all the dependent variables. "Mean UG Offers" output $W_{(88)} = .814$, $p < .001$ (sig); "Mean DG Offers" output $W_{(88)} = .900$, $p < .001$ (sig); "Mean TG Offers" output $W_{(88)} = .947$, $p = .001$ (sig); and "Mean TG Response (percent)" output $W_{(88)} = .959$, $p = .007$ (sig). The output for "TriPM Score" was $W_{(88)} = .956$, $p = .005$ (sig). Therefore, all variables are non-normally distributed, so Spearman's correlations were used. In addition, for each correlational pairing a multiple linear regression was conducted to assess the predictive and moderative powers of the variables "TriPM score" and "STAI score" over the respective dependent variable.

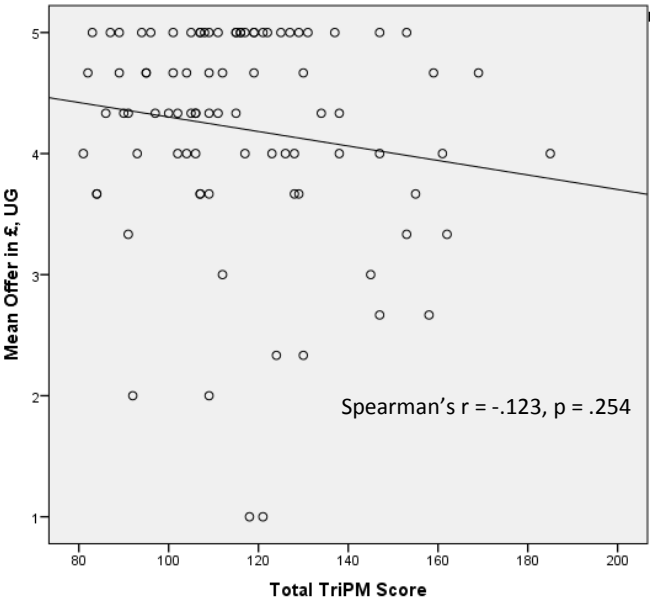

Figure 1: scatterplot of TriPM score against mean Ultimatum Game offer in £.

A Spearman's rank-order correlation was run to analyse the relationship between TriPM score and each participant's average offer in the Ultimatum Game. Contrary to our hypothesis, a weak non-significant negative correlation was found, $r_s = -.123$, $p = .254$. *Figure 1* illustrates the correlation, with an additional line of fit using a linear regression predictive equation with TriPM score as the input variable. This result was unexpected given the body of research between psychopathy and Ultimatum Games.

When mean UG offer was predicted, it was found that TriPM ($\beta = -.13$, $p = .229$) and STAI ($\beta = .146$, $p = .125$) were not significant predictors. Though the lack of moderating effect of anxiety was expected, the lack of predictive power for TriPM score was not. The overall model fit was $R^2 = .043$, meaning 4.3% of the variance is accounted for.

The relationship between TriPM score and average offer in the Dictator Game was assessed with another Spearman correlation. In line with predictions, it was found that a moderate, statistically significant negative correlation exists between the variables, $r_s = -.343$, $p = .002$, showing a clear relationship between psychopathy and greater self-interested behaviour in a dictator paradigm. In *figure 2*, a scatterplot also displaying the regression line of fit can be observed.

When mean DG offer was predicted, it was found that TriPM score ($\beta = -.426$, $p < .001$) was a

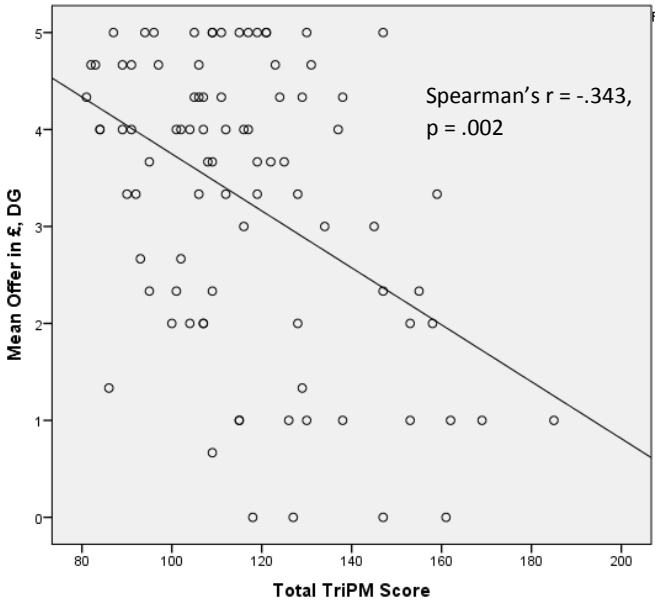

Figure 2: scatterplot of TriPM score against mean Dictator Game offer in £.

significant predictor. STAI score (β = .10, p = .307) was not significant. These results support the hypothesis that psychopathy may be a predictor of economically self-interested actions. Overall, model fit was R^2 = .203, meaning 20.3% of variance was accounted for.

The Trust Game provided two output variables – average offer value, and average response value. Spearman's rank-order correlations were carried out on both against TriPM Score.

For TriPM score vs. initial offer average, no significant correlation was found, r_s = .095, p = .376 (*Figure 3*). For the TriPM score vs. average response value, however, a statistically significant negative correlation was found, r_s = -.282, p = .008. *Figure 4* displays this latter relationship, once again with the regression equation for TriPM as a predictor variable represented as a line of fit.

Predicting mean first offer in the Trust Game, TriPM score (β = .073, p = .504) and STAI (β = -.129, p = .235) were not found to be significant predictors. The model fit was R^2 = .024, meaning 2.4% of variance is explained by the model.

When average response in the Trust Game was predicted, TriPM score (β = -.350, p < .001) was found to be a significant predictor. STAI score (β = -.052, p = .611) was not. Model fit for this regression was R^2 = .120, accounting for 12% of variance.

These results corroborate the idea that a difference exists between Ultimatum and Dictator style paradigms, and that this difference results in observed differences in psychopathic behaviour.

To explore how TriPM value may have a linear effect on participant's game-history (i.e. how they respond in each trial, and whether this changes), as well as elucidate any interaction effects, several linear mixed-

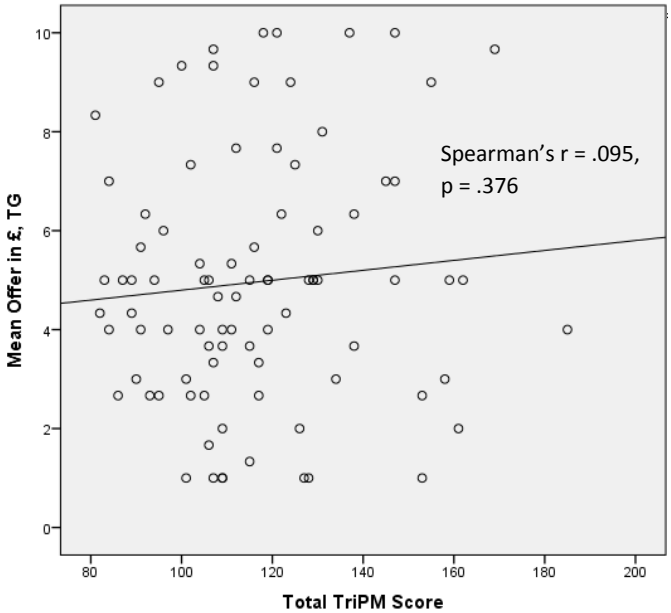

Figure 3: scatterplot of TriPM score against mean Trust Game offer in £.

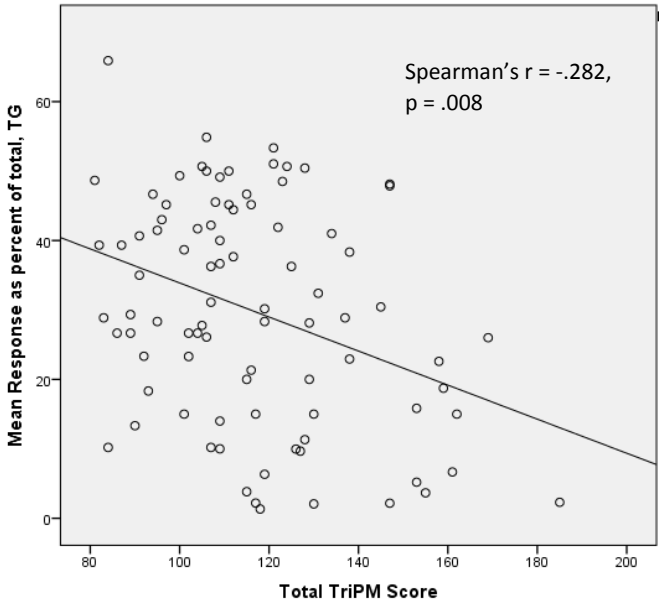

Figure 4: scatterplot showing TriPM score against mean Trust Game responses (as percentage of total money).

effects analyses were run on each of the games using MatLab (Mathworks, 2016). A full covariance matrix was used, using Matlab's default Cholesky parametrisation (code generously provided by Dr. Ulrik Beierholm).

A linear mixed-effects analysis was performed to examine the relationship between Ultimatum Game offers and TriPM score. The fixed effects entered were the intercept, trial, TriPM, and the TriPM*trials interaction; and between-subject variation in TriPM value and was entered as a random effect. In brief, the model for UG offers was "UG ~ 1 + TriPM + trial + (TriPM*trial) + (1|subject)". It was found that in a LME model, TriPM (β = -.013, SE = .007, t = -1.943, p = .053), trial (β = -.367, SE = .309, t = -1.183, p = .235), and the TriPM*trial interaction (β = -.003, SE = .002, t = 1.359, p = .175) were all non-significant, meaning that none of the above factors had a significant effect on offers in the Ultimatum Game, though TriPM score presented a borderline-significant effect, and TriPM value had no effect on offers made across the course of the game. Though these are in line with results from the previous tests, they are nonetheless unexpected given the literature.

A second linear mixed-effects model was created for the relationship between the offers (to use the term loosely) made in the Dictator Game, and the TriPM values of the subjects making the offers, and subjects' history throughout the game. Once again, fixed effects for the model were the intercept, the trials, TriPM value, and TrPM*trial interaction; and the random effect entered was once again the subjects. The model, in summary, was "DGR ~ 1 + TriPM + trial + (TriPM*trial) + (1|subject)". As in the correlational and regressional analyses, TriPM was found to be highly significant (β = -.023, SE = .008, t = -2.696, p = .007), showing that psychopathy moderates offer amounts. However, no linear effect was found for trials over the course of the game (β = -.041, SE = .338, t = -.123, p = .901), nor the interaction between those trials and TriPM value (β = -.003, SE = .003, t = -1.088, p = .277). Therefore, TriPM caused no linear effect across the course of the game, but did have a large impact on offers made overall.

A linear mixed-effects analysis was run on Trust Game offers to examine the relationship between TriPM score and trial over the course of the game. Fixed effects entered were the intercept, trial, TriPM value, and TriPM*trial interaction. Random effect entered were once again the subjects. The model summary is as follows: "TG ~ 1 + TriPM + trial + TriPM*trial + (1|subject)". The results of the analysis showed that neither TriPM (β = .01, SE = .016, t = .636, p = .525), nor trial (β = -.009, SE = .665, t = -.014, p = .988), nor TriPM*trial interaction (β = -.0002, SE = .005, t = -.037, p = .97) were even close to having a significant linear effect on Trust Game offers. It is therefore concluded that none of the factors of TriPM value, trial, or the interaction between them had any linear impact on offers made in the Trust Game.

A linear mixed-effects model was created for responses to offers in the Trust Game, and how TriPM and trial may have had a linear effect over the course of the game on the responses. The fixed effects entered were intercept, trial, TriPM value, and TriPM*trial interaction, while the random effect was once again the between-subject variation. The model for the analysis was "TGR ~ 1 + TriPM + trial + TriPM*trial + (1|subject)"; and it was found that contrary to the previous analyses, none of TriPM (β = -.122, SE = .11, t = -1.107, p = .269), trial (β = 6.039, SE = 4.975, t = 1.214 , p = .225), or the interaction (β = -.061, SE = .042, t = -1.462, p = .144) were significant. This runs counter to the Spearman's correlation and linear regression run on the TGR values against TriPM values, which indicated significance. From the LME model, however, we would conclude no significant linear effects of any of these factors upon responses in the Trust Game. In this light, a second model was run with the same parameters, but with the TriPM*trial interaction excluded from the fixed effects. The model therefore became "TGR ~ 1 + TriPM + trial + (1|subject)"; it was found that although trial remained non-significant (β = -1.109, SE = .937, t = -1.182, p = .238), TriPM was now found to become significant (β = -.245, SE = .071, t = -3.424 p < .001). This therefore means TriPM value has a highly significant effect on responses in the Trust Game. Though this discrepancy is worthy of note, it is beyond the scope of the present study to discuss further.

Discussion

It was hypothesised that a negative correlation would be found between psychopathic traits as indexed by the TriPM and offer amount across all 3 economic games. Although this was not wholly the case, evidence was found that this was true in dictatorial situations but not in ultimatum situations; this was indicated by the presence of a modest correlation in the Dictator Game and in the second round of the Trust Game (which functions as a DG), and an absence thereof in the Ultimatum Game and the first round of the Trust Game (which although distinct from the UG, has

similarities related to trust and expected outcomes). Support for these correlations (and lack of correlations) was found from both regressions and linear mixed-effects models. Therefore, as predicted based on Scheres and Sanfey (2006), the second experimental hypothesis was confirmed, though with mixed evidence from linear mixed-effects analysis with regards to responses in the Trust Game.

The present study differs heavily from previous literature, in that despite mixed results on whether high trait-psychopathy influences more economically self-interested or reciprocal behaviour, a relationship is usually demonstrated. Here, this relationship was only present in half of the rounds played, and specifically only in one type of paradigm rather than across all games. Burns et al. (2014) have noted that in similar situations, the heterogeneity of a given population sample has caused problems with exploration of the psychopathy construct. Notably, the present study not only had a large bias towards university students, but specifically the students of Durham University who are traditionally from more affluent backgrounds; it is thought that this bias may have impacted the framing in which the experiment was placed, as greater affluence is related to decreased prosociality in ultimatum games (Ensminger & Cook, 2014). Additionally, the high proportion of £5/£5 splits suggest that some participants may not have understood the competitive nature of the games, resulting in non-standard behaviour. This may provide a further reason why the test results favour the 2nd round in 2-round games – in the first round, a player who does not understand the experiment may divide the amount evenly, but when receiving another player's offer may then notice that the offer made to them is more competitive. This represents a prospective limitation of the present study, and future research should ensure participants have several trial rounds to ensure they understand the game concepts.

One potential explanation for the disparity of these results is that behaviour in economic games is in part dictated by social cues and pressures. In the Ultimatum Game, reciprocation represents a social pressure that incentivises cooperation – P1's actions can affect P2's decision, irrespective of anonymity, and this is made clear in the rules for the game; however, in the Dictator Game there is no opportunity for reciprocal action, since it is a single round game against an anonymous opponent, and so the social pressure is lessened. In the context of psychopathy, then, how do we interpret this difference in behaviour? Since high-psychopathy individuals do not differ much from 'healthy' individuals on explicit moral judgements (Radke, Brazil, Scheper, Bulten, & de Bruijn, 2013; Young et al., 2012), it would seem to follow that when social cues are not as explicit (as in the DG, where there is no obvious price for self-interested behaviour) a greater expression of psychopathic traits would result in more socially aberrant behaviour, in this case proportioning a greater amount of the total funds to the self. Essentially, in the absence of explicit social cues, self-interest guides

behaviour, a conclusion more in line with Osumi and Ohira (2010). The present study follows a similar line of reasoning, as in the 2-round games the players were made aware on the response slips that a second round would follow, and that behavioural reciprocation (or retaliation) would be possible. This mirrors the findings of Ibáñez *et al*. (2016), though the present study goes further and explores how they may react to such offers.

However, Radke *et al.* (2012) make the point that the work of Osumi and Ohira (2010), Koenigs *et al.* (2010), Rillings *et al*. (2007) etc. can only be interpreted in terms of outcome-based fairness considerations rather than social dynamics, since they use the classic UG paradigm – a criticism that also applies to the present study. This aside, the lack of social cues in the DG (or presence of social context cues in the UG) may go some ways to explaining why psychopathic individuals perform 'normally' in an ultimatum situation but not in a dictatorial situation.

It has also been evidenced by some that even in anonymous games there is a latent social pressure incentive based on cultural fairness norms – an incentive that, by extension, high-psychopathy individuals are less sensitive to (Ibáñez, et al., 2016), as they have been shown to be more deficient on social exchange and implicit cues than on descriptive rules (Ermer & Kiehl, 2010); this goes further to supporting the present findings that in dictatorial situations, where no explicit cues to norms of fairness exist, individuals with higher trait psychopathy act in a socially aberrant manner by ignoring said norms in favour of self-interest (Rilling, et al., 2007). This would seem to neatly explain the discrepancy, but others have noted defection or uncooperative behaviour (in the present study denoted by lower offers) by such individuals can be conditional on interactions with their potential partners – including a 'null interaction', or the absence of proper information (Gervais, Kline, Ludmer, George, & Manson, 2013). In essence then, the lack of relationship between psychopathic traits and offer amount in the present experiment may not be the result of the lack of general social cues, but potentially interpersonal cues by which psychopathic individuals gauge expected relational value. However, given the relative abundance of research demonstrating the social and moral deficiencies towards cues and pressures (Ibáñez, et al., 2016; Young, et al., 2012; Aharoni, et al., 2011; Ermer & Kiehl, 2010), it seems more prudent to assume the behaviours of highly psychopathic individuals are guided by self-interest except in the presence of descriptive rules, which they are capable of interpreting as a healthy individual would. Another criticism of this type of research, however, is that laboratory experiments contextualise these moral/social dilemmas in a way that is disconnected from the situation e.g. "would you accept or reject this offer", thus setting up a hypothetical scenario rather than something an individual would consider 'real' (Radke, et al., 2013); although the present experiment contextualised the games for participants in terms of their real earnings each round, there is still some question as to how much the outcomes of the games map

onto real-life decision-making. Counter to this, several studies have shown that although there may be a response bias due in economic games due to demand characteristics, social desirability bias etc., there is no significant effect on the association between psychopathy and psychopathic behaviours as a result (Warren & Clarbour, 2009), meaning it is highly likely the present study's inherent response bias is made more relevant by the relatively low levels of trait-psychopathy exhibited in the sample.

There is a possible effect for the participants in that their backgrounds and socialisation were largely similar. As students from a university background (including some of the non-students in the present sample), they may have values and social attitudes that impacted the present study in ways that other studies (e.g. Koenigs, Kruepke, and Newman (2010)) were not beholden to. This may be doubly true in the present study, since participant selection largely rendered students from the University's Psychology Department, many of whom knew each other; this risk was introduced by a desire for participants who were naïve of the economic games used, which excluded 3rd year and many 2nd year Psychology students. Although efforts were made to maintain anonymity on a round-by-round basis, it was impossible to ensure total anonymity once participants knew who their fellow participants were. As it has been found that social proximity increases sensitivity to fairness in ultimatum games (Kim, Schnall, Yi, & White, 2013), it seems fair to assume that individuals in 'friendship-group' iterations of the experiment, where all or most participants knew each other, would represent a confounding variable that was not actively corrected for in the experimental design since participants may act more pro-socially overall to avoid accidentally cheating a friend. This said, to the best knowledge of the experimenters the trials were conducted on a balanced mix of familiar and unfamiliar groups, which should have levelled out any effects present, and so the extent to which this variable impacted the results remains uncertain.

Although it mostly provided supporting evidence for conclusions already drawn above, the linear mixed-effects analysis did provide an interesting caveat in that the Trust Game response data showed no significant effect for psychopathy until the linear effect of trial was removed from the model. Though beyond the scope of this study, it provides an interesting avenue for future research, as this was both unexpected (especially given the prior analyses) and seemingly without explanation.

Due the nature of the study, there were several critical limitations which may go some way to bridging the gap between the present study and the wider literature. Of particular note are the ways in which Ultimatum Game data was collected differently to prior studies. Within both economics and psychological research, the interest in ultimatum games has been predicated on responses, whereas the present study used the ultimatum responses mostly as a pretext for the game as a whole.

Response data for the Ultimatum Game *was* collected in the present study, but the limited spread of offer amounts made by participants (predominantly falling into the 'fair' offers of 5/5 or 6/4) made analysis impossible; very few, if any, participants made extreme offers, and so without aggregating acceptance rates for groups of participants there was no way to match response data of individuals to their TriPM scores. It may have been possible to split the dataset into 'high-psychopathy' and 'low-psychopathy' groups and take aggregate percentages, but the decision was made to not do this for two reasons: first, that this would ignore the continuous nature of psychopathy as the literature currently understands it, and would also be impossible since the TriPM does not have cutoff scores for this reason; and second, that the splitting of the dataset would cause a loss of statistical power (Cohen, 1983). As a result, there was no way of comparing percentage acceptance rates with TriPM scores in the present study. Since participants were kept anonymous from each other during the course of the experiment, it would be possible in future iterations of this study to provide manipulated first round offers without the participants' knowledge for each iteration of the UG, but this was not possible in the present study due to resource constraints.

Another notable disparity with comparable research is the lack of 'clinical' population, and although this was in line with the research aims of looking at how varying degrees of psychopathic traits affect behaviour, a distinct lack of high-end scorers on the TriPM may have biased results, especially if the relationship between psychopathic traits and offer amount is curvilinear rather than linear. The results therefore cannot be extended to individuals with so-called 'clinical' levels of these traits, since they are essentially unrepresented in the present sample. Additionally, a somewhat limited sample size in the present study may explain why other studies (e.g. Hall, et al., 2014) have found clearly-defined relationships where the present study only found limited evidence.

Issues have been raised over the validity of economic games as a stand-in for real-life behaviour (Berg, et al., 2013; Sanfey, et al., 2003). A significant way in which the present experiment departed from previous research was in allowing participants to decide the offers proposed in each of the rounds of all 3 games, where in previous experiments these splits have been presented as having been forced choice, and the offers themselves are usually controlled. This introduced a greater level of ecological validity into the experiment, giving participants more freedom to act how they might do in a real-world setting; it also meant that all participants were convinced of the veracity of the experiment, since a common criticism of the ultimatum game when manipulated by experimenters is that many participants are semi-aware of the offers being from fictitious counterparts (Sanfey, et al., 2003). This stands as a strength in favour of the present design, allowing more valid conclusions to be drawn from the experiment without losing the basic design of the economic games used.

However, a limited number of rounds per game (3 rounds, so that each player plays against each of their 3 opponents once per game) reduces the reliability of these results. An improved design would run the participants through 4-5 rounds with each of the opponents (16-20 rounds total) to better ascertain the extent to which practice with the game, habituation/retaliation to certain opponents' behaviours, and potential contextual norms affect offer amounts and/or rejections. This would also investigate the reliability of the the present study's results.

It is also possible that the games themselves cue behaviours through their very names. Though care was taken in the present study to rename the Trust Game to 'Multiplication Game', a similar approach was not adopted for the Ultimatum or Dictator Games. Both of these games have evocative names that may prime or even actively cue corresponding behaviours; for instance, knowing that a game is called a 'dictator' game may normalise behaviours thought of as dictatorial (e.g. extreme selfishness). This is a matter not often brought into question and may be a future direction for research.

A key limitation faced by researchers was the need to use the offers created by the participants to inform the second player's decision in the second rounds of the Ultimatum and Trust Games. Control over this variable would allow more in-depth analysis of the interactions both in first-round decisions, and in the response rates to these decisions. As mentioned, manipulating the first round data so that participants fill in their initial split, then receive what they *think* is a genuine offer from another player in the room whilst actually receiving a controlled offer, would posit more useful results that could be compared more readily than the present design.

Future research should build upon the present study and similar literature by delineating what causes high-psychopathy individuals to act 'normally' in certain situations but not others. In particular, it may be interesting to adapt the research of some such as Ibáñez, *et al.* (2016), Vieira, *et al.* (2013), and Osumi & Ohira (2010) and examine how individuals differ psychosomatically across the conditions detailed in the present experiment. It would also be of valuable insight to better analyse the relationships between individual subscales of the Triarchic Psychopathy Measure with offer amounts and rejection rates, since it is reckoned that self-reported psychopathy is essentially heterogeneous (Berg, Lilienfeld, & Waldman, 2013), and that higher scores on some subscales may result in radically different behavioural presentations. Taking this into account may explain other disparities within the literature such as Koenigs, Kruepke, & Newman's work: individuals with notably low cold-heartedness/meanness scores could explain the lack of coldly rational action, but would not necessarily preclude such individuals from being considered psychopathic if their boldness/fearless dominance and disinhibition scores were quite high. This is a callback to the issues

surrounding the use of incarcerated individuals as stand-ins for a psychopathic sub-sample, since these individuals may be unusually high in disinhibition when compared with other sub-samples, psychopathic or otherwise (Sellbom & Phillips, 2013). The present study stands out as an example of using both non-incarcerated populations, and a more ecologically valid approach, to examine the behaviours linked to expression of psychopathic traits.

To conclude, it was hypothesised that a negative relationship would exist between Triarchic psychopathy traits and amounts of money offered across a variety of economic games. Through correlational and regressional analysis it was found that this is indeed the case for dictator-style situations, but in ultimatum rounds this was found not to be true. It was also hypothesised that there would be some form of disparity between ultimatum and dictator style offers, which was found to be true. No moderating effect was found for anxiety in any test. Further clarification of the affective and behavioural deficits prevalent in psychopathy is necessary.

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Appendices

Appendix A

INFORMATION SHEET

Project title: Personality and its impact on fairness

Researcher(s): Cameron Hector
Supervisor: Dr. Ulrik Beierholm
Contact details: cameron.c.hector@durham.ac.uk

I would like to invite you to take part in a study that I am conducting as part of my Postgraduate Project at Durham University under the supervision of Dr. Ulrik Beierholm. The study has been approved by the Department of Psychology Ethics Committee.

Before you decide whether to agree to take part it is important for you to understand why the research is being done and what your participation will involve. Please read the following information carefully. Please contact me or my supervisor on the contact details shown above if there is anything that is not clear or if you would like more information.

The purpose of the study is to determine how factors in one's personality impact attitudes of fairness. To this end, you will play a series of games with the other participants. Each game will be explained to you before you play it, and you will have opportunity to ask questions.

Afterwards, you will be asked to complete two personality inventories. These are not diagnostic – that is, they cannot be used to diagnose any personality disorder, mental illness, or any clinical facet of you as a person. It is not possible to 'fail' these inventories, but please try to answer as truthfully as possible. You may omit to answer any question, and this will result in your data being excluded from the study.

Your participation is voluntary and you do not have to agree to take part. You can withdraw, without giving a reason, at any time if you decide you would no longer like to take part. You can also withdraw your data by contacting either me or my supervisor.

For your time, you will be rewarded with either participant pool credit (if you are a psychology student) or £5 cash (if you are not). An additional reward based on game performance will also be available.

All information obtained during the study will be kept confidential and if the data are published it will not be identifiable as yours. You will be allocated an anonymous number for data collection which will not be connected to your name or identity. Thank you for reading this information and considering taking part in this study.

If you have any questions at any time, please feel free to ask!

TITLE OF PROJECT: *Appendix B*

Personality and Fairness

2 words

Age: Gender: Ethnicity:

Please cross out as necessary

Have you read the Participant Information Sheet? YES / NO

Have you had an opportunity to ask questions and to discuss the study? YES / NO

Have you received satisfactory answers to all of your questions? YES / NO

Have you received enough information about the study and the Intended uses of, and access arrangements to, any data which you supply? YES / NO

Were you given enough time to consider whether you want to participate? YES/NO

Who have you spoken to? Dr/Mr/Mrs/Ms/Prof.....

Do you consent to participate in the study? YES/NO

Do you understand that you are free to withdraw from the study:

* at any time and
* without having to give a reason for withdrawing and
* without any adverse result of any kind? YES / NO

Signed ..... **Date** .....

(NAME IN BLOCK LETTERS) .....

Signature of witness ..... **Date** .....

Appendix C

DEBRIEFING SHEET

Project title: How does psychopathy relate to making more self-interested decisions?

Thank you for taking part in my study. The true aim of the experiment was to assess the extent to which incidence of traits linked to psychopathy can affect economically rational behaviour.

The inventories you completed were to assess psychopathy and anxiety; however, these inventories are non-diagnostic, meaning they cannot highlight any 'troubling' or 'problematic' aspects in a person. If you have any ongoing concerns about mental health, I advise you to seek help from a GP or the NHS Talking Changes service.

I would ask that you keep the details of this experiment secret until the experiment has run its course. This is to ensure that potential participants are not made aware of the true aims of the study, in case their behaviour is modified as a result.

If you would like further information about the study or would like to know about what my findings are when all the data have been collected and analysed then please contact me on cameron.c.hector@durham.ac.uk. I cannot, however, provide you with your individual results.

Thank you for your time!

Appendix D

Ultimatum Game You are player [x]

In this game, you are allocated £10. You may divide this sum into two parts – one part for you, one part for another player (please use whole pound values, no pennies!). You must offer at least £1. This offer is given to one of the other players, who will either choose to accept or reject the offer. If they accept, you both earn as much money as was offered; if the second player rejects the offer, you both will earn nothing. You will not be told which player is which, to preserve anonymity. This will be repeated for a number of rounds, until you have sent and received an offer from all other players.

At the end of the game, you will receive a portion of the money you have accumulated. The experimenter is keeping track, so do not feel the need to note down any information – take each offer as a separate entity.

UGA

You have a sum of £10. How do you divide this?

I take: _____ The other person takes: _____

Player 2 (circle/cross out): Accept/Reject

Dictator Game

This game is similar to the last game, in that you have a sum of £10 that you may divide into two parts (whole numbers again please!). This time the offer you make will be the final allocation – the player to whom you offer the money must take whatever share you offer. They will be told the identity of the person who gave them their share, however.

Once again, players will receive a portion of what they have accumulated

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10,382 words

DGA

You have a sum of £10. How do you divide this?

I take: _____ The other person takes: _____

Multiplication Game You are player [x]

This game works similarly to the first game. You have a total, and it is divided as you see fit. The second player receives their share of the offer, and it is multiplied by 5. It is then the turn of the second player to decide how to divide the new sum of money. Once again, the offer made here is final and cannot be rejected. At the end of the game, you will earn part of your accumulated money.

MGA

You have a sum of £10. How do you divide this?

I take: _____ The other person takes: _____

Player 2, the sum on the right is now multiplied by 5. How do you divide this?

I take: _____ The first person takes: _____

Appendix E

Triarchic Psychopathy Measure

1. I'm optimistic more often than not.
2. How other people feel is important to me.
3. I often act on immediate needs.
4. I have no strong desire to parachute out of an airplane.
5. I've often missed things I promised to attend.
6. I would enjoy being in a high-speed chase.
7. I am well-equipped to deal with stress.
8. I don't mind if someone I dislike gets hurt.
9. My impulsive decisions have caused problems with loved ones.
10. I get scared easily.
11. I sympathize with others' problems.
12. I have missed work without bothering to call in.
13. I'm a born leader.
14. I enjoy a good physical fight.
15. I jump into things without thinking.
16. I have a hard time making things turn out the way I want.
17. I return insults.
18. I've gotten in trouble because I missed too much school.
19. I have a knack for influencing people.

20.	It doesn’t bother me to see someone else in pain.	♦	❄	☞	↗
21.	I have good control over myself.	♦	❄	☞	↗
22.	I function well in new situations, even when unprepared.	♦	❄	☞	↗
23.	I enjoy pushing people around sometimes.	♦	❄	☞	↗
24.	I have taken money from someone's purse or wallet without asking.	♦	❄	☞	↗
25.	I don't think of myself as talented.	♦	❄	☞	↗
26.	I taunt people just to stir things up.	♦	❄	☞	↗
27.	People often abuse my trust.	♦	❄	☞	↗
28.	I'm afraid of far fewer things than most people.	♦	❄	☞	↗
29.	I don't see any point in worrying if what I do hurts someone else.	♦	❄	☞	↗
30.	I keep appointments I make.	♦	❄	☞	↗
31.	I often get bored quickly and lose interest.	♦	❄	☞	↗
32.	I can get over things that would traumatize others.	♦	❄	☞	↗
33.	I am sensitive to the feelings of others.	♦	❄	☞	↗
34.	I have conned people to get money from them.	♦	❄	☞	↗
35.	It worries me to go into an unfamiliar situation without knowing all the details.	♦	❄	☞	↗
36.	I don't have much sympathy for people.	♦	❄	☞	↗
37.	I get in trouble for not considering the consequences of my actions.	♦	❄	☞	↗
38.	I can convince people to do what I want.	♦	❄	☞	↗
39.	For me, honesty really is the best policy.	♦	❄	☞	↗

40. I've injured people to see them in pain. ♦ ❄ ☞ ↗

41. I don’t like to take the lead in groups. ♦ ❄ ☞ ↗

42. I sometimes insult people on purpose to get a reaction from them. ♦ ❄ ☞ ↗

43. I have taken items from a store without paying for them. ♦ ❄ ☞ ↗

44. It's easy to embarrass me. ♦ ❄ ☞ ↗

45. Things are more fun if a little danger is involved. ♦ ❄ ☞ ↗

46. I have a hard time waiting patiently for things I want. ♦ ❄ ☞ ↗

47. I stay away from physical danger as much as I can. ♦ ❄ ☞ ↗

48. I don't care much if what I do hurts others. ♦ ❄ ☞ ↗

49. I have lost a friend because of irresponsible things I've done. ♦ ❄ ☞ ↗

50. I don't stack up well against most others. ♦ ❄ ☞ ↗

51. Others have told me they are concerned about my lack of self-control. ♦ ❄ ☞ ↗

52. It’s easy for me to relate to other people’s emotions. ♦ ❄ ☞ ↗

53. I have robbed someone. ♦ ❄ ☞ ↗

54. I never worry about making a fool of myself with others. ♦ ❄ ☞ ↗

55. It doesn’t bother me when people around me are hurting. ♦ ❄ ☞ ↗

56. I have had problems at work because I was irresponsible. ♦ ❄ ☞ ↗

57. I’m not very good at influencing people. ♦ ❄ ☞ ↗

58. I have stolen something out of a vehicle. ♦ ❄ ☞ ↗

Appendix F

State-Trait Anxiety Inventory (with reverse marked questions indicated)

State ("Right now, that is, at this moment")

1. I feel calm (R)
2. I feel secure (R)
3. I am tense
4. I feel strained
5. I feel at ease (R)
6. I feel upset
7. I am presently worrying over possible misfortunes
8. I feel satisfied (R)
9. I feel frightened
10. I feel comfortable (R)
11. I feel self-confident (R)
12. I feel nervous
13. I am jittery
14. I feel indecisive
15. I am relaxed (R)
16. I feel content (R)
17. I am worried
18. I feel confused
19. I feel steady (R)
20. I feel pleasant (R)

Trait ("Indicate how you generally feel)

1. I feel pleasant (R)
2. I feel nervous and restless
3. I feel satisfied with myself (R)
4. I wish I could be as happy as others seem to be
5. I feel like a failure
6. I feel rested (R)
7. I am "calm, cool, and collected" (R)
8. I feel that difficulties are piling up so that I cannot overcome them
9. I worry too much over something that really doesn't matter
10. I am happy (R)
11. I have disturbed thoughts
12. I lack self-confidence
13. I feel secure (R)
14. I make decisions easily (R)
15. I feel inadequate
16. I am content (R)
17. Some unimportant thought runs through my mind and bothers me
18. I take disappointments so keenly that I can't put them out of my mind.
19. I am a steady person (R)
20. I get in a state of tension or turmoil as I think over my recent concerns and interests

Appendix G

Raw linear mixed-effects model outputs from MatLab

UG

Subject as random effect with interaction term

lme = fitlme(data,'UG~TriPM +trial+TriPM*trial+(1|subject)')

Linear mixed-effects model fit by ML

Model information:

Number of observations	264
Fixed effects coefficients	4
Random effects coefficients	88
Covariance parameters	2

Formula:

UG ~ 1 + TriPM*trial + (1 | subject)

Model fit statistics:

AIC	BIC	LogLikelihood	Deviance
743.66	765.12	-365.83	731.66

Fixed effects coefficients (95% CIs):

Name	Estimate	SE	tStat	DF	pValue	Lower	Upper
'(Intercept)'	5.6354	0.79732	7.0679	260	1.4452e-11	4.0653	7.2054
'TriPM'	-0.013082	0.0067332	-1.943	260	**0.053101**	-0.026341	0.00017623
'trial'	-0.36723	0.30904	-1.1883	260	**0.2358**	-0.97577	0.24131
'TriPM:trial'	0.0035479	0.0026098	1.3595	260	**0.17518**	-0.0015911	0.0086869

Random effects covariance parameters (95% CIs):

Group: subject (88 Levels)

Name1	Name2	Type	Estimate	Lower	Upper
'(Intercept)'	'(Intercept)'	'std'	0.7663	0.62719	0.93625

Group: Error

Name	Estimate	Lower	Upper
'Res Std'	0.76826	0.69205	0.85286

DG

Subject as random effect with interaction term

lme = fitlme(data,'DGR~TriPM +trial+TriPM*trial+(1|subject)')

Linear mixed-effects model fit by ML

Model information:

Number of observations	264
Fixed effects coefficients	4
Random effects coefficients	88
Covariance parameters	2

Formula:
DGR ~ 1 + TriPM*trial + (1 | subject)

Model fit statistics:

AIC	BIC	LogLikelihood	Deviance
847.7	869.15	-417.85	835.7

Fixed effects coefficients (95% CIs):

Name	Estimate	SE	tStat	DF	pValue	Lower	Upper
'(Intercept)'	6.7687	1.0168	6.6571	260	1.6498e-10	4.7665	8.7708
'TriPM'	-0.023151	0.0085864	-2.6963	260	**0.0074701**	-0.040059	-0.0062435
'trial'	-0.041791	0.33835	-0.12351	260	**0.9018**	-0.70805	0.62447
'TriPM:trial'	-0.0031089	0.0028573	-1.088	260	**0.27759**	-0.0087353	0.0025176

Random effects covariance parameters (95% CIs):
Group: subject (88 Levels)

Name1	Name2	Type	Estimate	Lower	Upper
'(Intercept)'	'(Intercept)'	'std'	1.2424	1.0471	1.4742

Group: Error

Name	Estimate	Lower	Upper
'Res Std'	0.84113	0.7577	0.93376

TG

Subject as random effect with interaction term
lme = fitlme(data,'TG~TriPM +trial+TriPM*trial+(1|subject)')

Linear mixed-effects model fit by ML

Model information:

Number of observations	264
Fixed effects coefficients	4
Random effects coefficients	88
Covariance parameters	2

Formula:
TG ~ 1 + TriPM*trial + (1 | subject)

Model fit statistics:

AIC	BIC	LogLikelihood	Deviance
1195.8	1217.2	-591.88	1183.8

Fixed effects coefficients (95% CIs):

Name	Estimate	SE	tStat	DF	pValue	Lower	Upper
'(Intercept)'	3.8147	1.9443	1.962	260	0.050833	-0.013928	7.6433
'TriPM'	0.010451	0.016419	0.63648	260	0.**52502**	-0.021881	0.042783
'trial'	-0.0098081	0.66555	-0.014737	260	**0.98825**	-1.3204	1.3007
'TriPM:trial'	-0.00020876	0.0056204	-0.037143	260	0.**9704**	-0.011276	0.010859

Random effects covariance parameters (95% CIs):

Group: subject (88 Levels)

Name1	Name2	Type	Estimate	Lower	Upper
'(Intercept)'	'(Intercept)'	'std'	2.3009	1.9332	2.7386

Group: Error

Name	Estimate	Lower	Upper
'Res Std'	1.6545	1.4904	1.8367

TGR

Subject as random effect with interaction term

lme = fitlme(data,'TGR~TriPM +trial+TriPM*trial+(1|subject)')

Linear mixed-effects model fit by ML

Model information:

Number of observations	264
Fixed effects coefficients	4
Random effects coefficients	88
Covariance parameters	2

Formula:

TGR ~ 1 + TriPM*trial + (1 | subject)

Model fit statistics:

AIC	BIC	LogLikelihood	Deviance
2218.7	2240.2	-1103.4	2206.7

Fixed effects coefficients (95% CIs):

Name	Estimate	SE	tStat	DF	pValue	Lower	Upper
'(Intercept)'	46.328	13.073	3.5438	260	0.0004675	20.586	72.071
'TriPM'	-0.12227	0.1104	-1.1075	260	0.**26911**	-0.33966	0.095126
'trial'	6.0398	4.9752	1.214	260	**0.22585**	-3.757	15.837
'TriPM:trial'	-0.06146	0.042014	-1.4628	260	0.**14472**	-0.14419	0.021272

Random effects covariance parameters (95% CIs):

Group: subject (88 Levels)

Name1	Name2	Type	Estimate	Lower	Upper
'(Intercept)'	'(Intercept)'	'std'	13.083	10.773	15.888

Group: Error

Name	Estimate	Lower	Upper
'Res Std'	12.368	11.141	13.73

Subject as random effect without interaction term

lme = fitlme(data,'TGR~TriPM +trial+(1|subject)')

Linear mixed-effects model fit by ML

Model information:

Number of observations 264
Fixed effects coefficients 3
Random effects coefficients 88
Covariance parameters 2

Formula:
TGR ~ 1 + TriPM + trial + (1 | subject)

Model fit statistics:

AIC	BIC	LogLikelihood	Deviance
2218.8	2236.7	-1104.4	2208.8

Fixed effects coefficients (95% CIs):

Name	Estimate	SE	tStat	DF	pValue	Lower	Upper
'(Intercept)'	60.626	8.6842	6.9812	261	2.4177e-11	43.526	77.726
'TriPM'	-0.24519	0.071605	-3.4241	261	0.**00071608**	-0.38618	-0.10419
'trial'	-1.1091	0.93793	-1.1825	261	**0.23809**	-2.956	0.73779

Random effects covariance parameters (95% CIs):
Group: subject (88 Levels)

Name1	Name2	Type	Estimate	Lower	Upper
'(Intercept)'	'(Intercept)'	'std'	13.06	10.746	15.872

Group: Error

Name	Estimate	Lower	Upper
'Res Std'	12.443	11.209	13.813