Three Essays on Behavioral Game Theory

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Contents

Resumen

Esta Tesis presenta tres estudios sobre teoría de juegos del comportamiento. La teoría de juegos es un área que permite estudiar las interacciones estratégicas que son vitales en la economía ya que se dan constantemente entre todo tipo de agentes. Tradicionalmente, en la economía se ha supuesto que los individuos se comportan de forma racional. La literatura empírica en este campo ha demostrado que esto no es así. Sin embargo, estas desviaciones que presentan pueden ser lo suficientemente consistentes como para incorporarse como nuevos supuestos y predecir mejor el comportamiento. Esta tesis contribuye al estudio sobre qué factores son relevantes al guiar el comportamiento en situaciones estratégicas de interés.

Capítulo 1: Non-equilibrium Play in Centipede Games

El capítulo 1 investiga cuales son los modelos de comportamiento que mejor explican y predicen el comportamiento en el juego del ciempiés.

El juego del ciempiés, propuesto originalmente por Rosenthal (1981), representa una de las clásicas contradicciones de la teoría de juegos, ya que la solución del equilibrio de Nash perfecto en subjuegos contradice tanto la intuición de cómo jugar, como los datos reales de comportamiento en el mismo. Este juego tiene especial interés para los economistas dadas sus peculiares características. En este juego, dos agentes deciden de forma alternativa entre dos acciones, parar o continuar, durante varias rondas.

Si un agente decide parar, el juego termina de forma inmediata. Si elige continuar, pasa a ser el turno de decidir del otro agente. En la última ronda, ambas acciones terminan el juego. El pago que obtiene un agente al parar satisface dos características. Primero, este es siempre inferior al pago que obtendría al parar en rondas posteriores. Segundo, este es siempre superior al pago que ese agente obtendría si pasa y el otro agente para en la ronda directamente posterior. La primera característica da un incentivo a los agentes a parar lo más tarde posible, mientras que la segunda da un incentivo a parar siempre antes que el otro. Este contraste de incentivos es lo que hace a este juego tan relevante, de forma similar a otros como el dilema del prisionero jugado de forma repetida, y lo asemeja a situaciones de interés para la economía.

El juego del ciempiés presenta una única predicción del equilibrio de Nash perfecto en subjuegos: parar en la primera ocasión. Debido a la particular estructura descrita anteriormente, esta predicción es fácilmente calculable mediante la inducción hacia atrás. Sin embargo, los estudios experimentales de este juego muestran como muy pocos sujetos siguen esta predicción (véase McKelvey y Palfrey, 1992; Fey et al., 1996; Nagel y Tang, 1998; Rappaport et al., 2003; Bornstein et al., 2004). A pesar de esto, los economistas aún no tienen una compresión clara de cúales son los modelos de comportamiento que guían el comportamiento humano fuera del equilibrio. Resolver esta incógnita es el objetivo principal de este primer capítulo.

Han sido propuestas múltiples explicaciones para este comportamiento, las cuales clasificamos en tres categorías: explicaciones basadas en distintas preferencias, racionalidad limitada, y modelos que limitan el conocimiento común de la racionalidad. Las primeras son aquellas que argumentan que los individuos podrían no estar maximizando su pago como asume el equilibrio, sino que podrían tener un objetivo distinto. Por ejemplo, podrían tener preferencias de naturaleza altruista e incorporar los pagos del otro en su función de utilidad, buscar que la relación entre lo que ambos obtienen sea equitativa, o desear la eficiencia en términos de Pareto. La racionalidad limitada asume que los sujetos sí que maximizan su propio beneficio, pero presentan limitaciones sobre su propia racionalidad. Así, por ejemplo podrían equivocarse a la hora de calcular la respuesta óptima al comportamiento ajeno. Por último, los modelos basados en limitaciones al conocimiento común de la racionalidad, suponen que los individuos sí que con racionales y maximizan su propio beneficio, pero estos tienen la creencia de que los demás no van a comportarse tal y como dice el equilibrio.

Por lo tanto, el objetivo de este capítulo es averiguar cúales de estas explicaciones, el equilibrio y sus tres tipos de alternativas, son más relevantes explicando el comportamiento inicial en el juego del ciempiés. Para ello, utilizamos dos herramientas que nos permiten realizar este ejercicio y que diferencian a este estudio de la literatura previa: el diseño experimental y las técnicas econométricas.

Con respecto al diseño experimental, una de las mayores limitaciones de los estudios previos es que todos los juegos del ciempiés utilizados para estudiar el comportamiento son los presentes en McKelvey y Palfrey (1992) y Fey et al. (1996) o variantes con estructuras similares a estos. Estos juegos no son adecuados para discriminar entre distintos modelos de comportamientos, ya que frecuentemente predicen comportamiento similar. En consecuencia, partimos de la definición formal de juego del ciempiés y diseñamos un conjunto de ciempiés que difieren de los usados en la literatura. Estos presentan variaciones en la progresión de la suma de los pagos, donde en algunos esta es ascendente, constante, descendente, o varía a lo largo del juego. También son muy distintos en los incentivos que los individuos tienen a parar o continuar, con el objetivo de que los modelos de comportamiento predigan comportamiento distinto en estos, y por tanto un individuo que muestre sus preferencias en todos ellos deje una huella que pueda identificar qué está guiando su comportamiento.

Con respecto a las técnicas econométricas, aplicamos mixture-of-type models. Esta técnica permite unos resultados heterogéneos, indicando múltiples modelos candidatos al mismo tiempo si es que varios explican el comportamiento. Además, obliga a los distintos modelos de comportamiento a competir entre ellos, ya que la relevancia empírica de cada modelo es determinada endógenamente y siempre a costa de la de otros.

Los resultados muestran que el comportamiento de los sujetos es efectivamente demasiado diverso como para ser explicado por un solo modelo. Solo un 10% de los individuos sí que siguen el equilibrio parando en la primera oportunidad en la mayoría de los juegos del ciempiés. Sin embargo, la mayoría del comportamiento está mejor explicado por el fallo del conocimiento común de la racionalidad (concretamente level-k) y por racionalidad limitada (específicamente Quantal Response Equilibrium). Estos resultados no solo reflejan por primera vez la naturaleza diversa del comportamiento en juegos del ciempiés, si no que además ayudan a comprender la literatura, en donde diferentes estudios encontraban una u otra de estas explicaciones. Por último, el capítulo termina con múltiples tests de robustez que confirman la solidez de los resultados obtenidos.

CAPÍTULO 2: DO PEOPLE MINIMIZE REGRET IN STRATEGIC SITUATIONS? A LEVEL-k COMPARISON

El segundo capítulo analiza la sorprendente relación entre dos modelos que han conseguido explicar satisfactoriamente comportamiento fuera del equilibrio en la literatura de teoría de juegos: level-k y minimax regret. Sorprendentemente, a pesar de que ambos modelos suponen motivaciones muy diferentes, ambos predicen el mismo comportamiento en un gran número de juegos de la literatura. Es por esto que el comportamiento atribuido a uno de estos modelos en el pasado, podría haber sido motivado por el otro en realidad. En este segundo capítulo, analizo las relaciones entre ambos modelos de manera teórica y en la literatura, y después presento un diseño experimental específicamente diseñado para separarlos y dar respuesta a cuál de los dos modelos (o si ambos) es más relevante guiando el comportamiento.

El arrepentimiento es un sentimiento negativo de culpa por haber hecho una acción, el deseo de haber elegido otra opción mejor. Esta sensación solo se puede experimentar *ex post*, pero es posible anticiparla y tenerla en cuenta a la hora de tomar decisiones. En teoría de juegos, el arrepentimiento puede interpretarse como la diferencia entre el pago óptimo que un jugador podría haber obtenido dada la estrategia tomada por el resto de jugadores, y el pago que efectivamente obtuvo dado la estrategia que este tomó. Minimax regret es un criterio de decisión originado en Savage (1951) que toma en cuenta la anticipación de este arrepentimiento y trata de evitar la mera posibilidad de sufrir un arrepentimiento elevado eligiendo la opción que proporciona el mínimo máximo arrepentimiento. El rol del arrepentimiento ha sido explorado y testeado empíricamente multiples veces en diversas situaciones y campos del conocimiento.[1]

Level-*k* es un modelo de comportamiento que asume que los jugadores son racionales y reaccionan óptimamente a sus creencias, pero tienen una creencia particular y simplificada sobre cómo se comportan los demás. Distintos niveles representan distintos niveles de sofisticación en el pensamiento y los individuos creen siempre que son el más sofisticado (Costa-Gomes et al., 2001). De esta forma, el nivel menos sofisticado es *L0* que elige al azar cualquiera de las acciones posibles con la misma probabilidad. Un nivel k se define como un tipo que hace mejor respuesta a una población donde todos pertenecen al nivel anterior $k-1$. De la misma forma que minimax regret, level-*k* ha sido exitoso a la hora de explicar el comportamiento que se desvía del equilibrio.[2]

Este capítulo describe detalladamente ambos modelos de comportamiento y de-

[1] Inluyendo negociación (Linhart y Radner, 1989), establecimiento de precios (Renou y Schlag, 2010), toma de decisiones estratégicas (Halpern y Pass, 2012), problemas de elección de tratamientos (Manski, 2004, 2007; Stoye, 2009), proceso de decisiones (Baron y Ritov, 1994, 1995), subastas (Ozbay y Ozbay, 2007; Ratan y Wen, 2016), y diferencias culturales en decisiones (Giordani et al., 2010). Ver Wang y Boutilier (2003) para aplicaciones en ciencas de la computación, Zeelenberg (1999) para aplicaciones en psicología, Loulou y Kanudia (1999) para aplicaciones en problemas medioambientales, y Brehaut et al. (2003) para aplicaciones en medicina.

[2] Incluyendo juegos en forma normal (Stahl y Wilson, 1994, 1995; Costa-Gomes et al., 2001), concursos de belleza (Nagel, 1995; Costa-Gomes y Crawford, 2006), juegos de entrada (Camerer et al., 2004), subastas (Crawford y Iriberri, 2007), juegos de esconderse y buscar (Crawford y Iriberri, 2007), diseño de mecanismos (Crawford et al., 2009), juegos de información asimétrica (Brown et al., 2012) y juegos del ciempiés (Garcia-Pola et al., 2016). Ver Crawford et al. (2013) para un análsis completo.

spués analiza las relaciones entre ellos de forma teórica y práctica. Primero utilizamos los datos de 17 estudios experimentales diseñados para discriminar entre distintos modelos de comportamiento hasta juntar 277 decisiones estratégicas distintas. Mostramos que en un 83% de las mismas los dos modelos predicen exactamente el mismo comportamiento y solo en 35 decisiones (un 12% del total) predicen acciones totalmente diferentes. Después analizamos sus relaciones de forma teórica, mostrando que en cualquier juego 2×2 minimax regret y *L1* predicen lo mismo. Además, la presencia de relaciones de dominancia complica su separación. Por último, usamos varios juegos como ejemplo para mostrar que las coincidencias van más allá de lo descrito previamente y ambos modelos siguen coincidiendo en otras situaciones estratégicas de interés aun cuando estas presentan un gran número de acciones posibles para los jugadores.

Si unimos estos resultados al hecho de que ambos modelos han sido propuestos como explicaciones al comportamiento alternativas al equilibrio de Nash, se crea una duda razonable acerca de cuál es el que motiva realmente el comportamiento. Para resolverla, en el final de este capítulo realizamos un ejercicio doble. Primero, revisitamos el estudio de Costa-Gomes y Crawford (2006) donde analizan el comportamiento de múltiples guessing games. Este es el único de la literatura analizada previamente en el que minimax regret y *L1* predecían distinto comportamiento en la mayoría de sus juegos. Después, proponemos un experimento diseñado específicamente con el propósito de separar ambos modelos. Este consiste en una serie de juegos en forma normal con tres características particulares. La primera es que distintas variaciones del modelo minimax regret predicen la misma acción, de forma podemos poner a prueba este modelo de forma amplia. La segunda es que hacemos que los incentivos de seguir cada acción predicha por cada modelo sean lo más grandes posibles. Finalmente, y siguiendo el objetivo principal de este experimento, hacemos que los modelos relevantes estén separados de forma sistemática.

Las ventajas de este diseño es que nos permite saber cuál es el modelo más relevante empíricamente tanto en juegos con un número grande y continuo de opciones a elegir por cada jugador (guessing games de Costa-Gomes y Crawford (2006)), como en juegos en forma normal con pocas opciones discretas (nuestro experimento).

Los datos que arroja el análisis descriptivo es que *L1* es el modelo que explica la mayor parte del comportamiento en ambos experimentos, aunque hay un número no despreciable de decisiones que coinciden con las predicciones de minimax regret. Sin embargo, cuando realizamos un análisis en el que exigimos a los sujetos que sean consistentes a través de sus decisiones en los distintos juegos, encontramos poca evidencia para minimax regret (un 4%) en guessing games y ninguna en juegos en forma normal. Estos resultados sugieren que la relevancia de minimax regret como una explicación al comportamiento estratégico debe ser cuestionada.

CAPÍTULO 3: HOT VERSUS COLD BEHAVIOR IN CENTIPEDE GAMES

En este tercer y último capítulo añadimos evidencia al tradicional debate en economía experimental de hasta qué punto o en qué situaciones la obtención de comportamiento individual mediante el método directo (en *caliente*) es equivalente al strategy-method (en *frío*) en juegos extensivos. El primero implica que los jugadores observan el comportamiento que los demás han tenido en etapas anteriores del juego, y reaccionan directamente a estas acciones pasadas. El segundo consiste en que los jugadores describen su plan completo de acciones en todas las hipotéticas etapas sin saber lo elegido por los otros jugadores. Ambos métodos son equivalentes estratégicamente. Sin embargo, que esa equivalencia se traslade al comportamiento real ha sido debatido por décadas. Los motivos por los que podrían ser distintos, es porque en la obtención en frío los sujetos podrían pensar más profundamente sobre todos los posibles casos que potencialmente ofrece el juego y esto podría modificar su comportamiento. De la misma forma, la obtención en caliente podría modificarlo ya que hace reaccionar de forma directa a los jugadores a las elecciones reales de los demás y por tanto podría despertar en ellos reacciones más emocionales. Brandts y Charness (2011) ofrecen un análisis de 29 estudios diferentes donde comparan ambos métodos. Obtienen resultados mixtos, habiendo casos en donde coinciden y otros en los que no. Sugieren que esto se puede deber a las condiciones particulares de cada estudio, por ejemplo el comportamiento suele diferir cuando los jugadores pueden castigarse por lo que han hecho en etapas anteriores, dejando la cuestión abierta y requiriendo más evidencia para dar una respuesta más clara.

Este capítulo hace exactamente eso, proporcionando datos de comportamiento obtenidos mediante ambos métodos en cuatro variaciones distintas del juego del ciempiés. Los datos de la obtención del comportamiento en frío se obtienen del mismo experimento del primer capítulo, mientras que los datos de la obtención en caliente se obtienen de un nuevo experimento realizado específicamente para este capítulo. Dos juegos son los clásicos presentes en McKelvey y Palfrey (1992) y Fey et al. (1996), mientras que los otros dos son juegos del ciempiés que varían en la progresión de la suma de los pagos y en las asimetrías de los incentivos entre los jugadores.

Los datos obtenidos revelan que hay diferencias significativas en el comportamiento obtenido mediante uno y otro método de obtención en los dos juegos del ciempiés clásicos. En particular, el método en caliente desplaza el comportamiento hacia parar antes. Sin embargo, el estas diferencias no existen en los otros dos juegos. Dado que la diferencia principal entre ambos pares es que los dos jugadores tienen incentivos similares en los dos primeros pero no en los dos segundos, atribuimos estos efectos a la asimetría de los pagos. Las personas encuentran más facilidad para ponerse en la situación de los demás si están enfrentándose a una situación similar a la suya, y es por ello que esta diferencia puede jugar un papel importante. Por otro lado, participar en un juego con asimetría puede ser un elemento que aumente la complejidad de las decisiones. Si este efecto se produce de una forma más general es algo que debería ser sujeto de estudio en futuras investigaciones.

Chapter 1

Non-Equilibrium Play in Centipede Games

1.1 Introduction

The Centipede Game (CG, hereafter), proposed by Rosenthal (1981), represents one of the classic contradictions in game theory (Goeree and Holt, 2001) as the unique subgame perfect Nash equilibrium (*SPNE*, henceforth) is at odds with both intuition and human behavior. This has drawn considerable attention of economists. In this game, two agents decide alternately between two actions, take or pass, for several rounds and the game ends whenever a player takes. The payoff from taking in a particular round satisfies two conditions: (i) it is lower than the payoff from taking in any of the following rounds, which gives incentives to pass; but (ii) it exceeds the payoff received if the player passes and the opponent ends the game in the next round, providing incentives to stop the game right away. This payoff structure reflects a tension between payoff maximization and sequential reasoning, shared with prominent strategic environments such as the repeated Prisoner's dilemma (see Dal Bó and Fréchette, 2011, Friedman and Oprea, 2012, Bigoni et al., 2015, or Embrey et al., 2017, for recent advances). Such a tension characterizes other strategic repeated environments of high economic interest including Cournot competition, public goods provision, or the tragedy of the commons.

Due to its payoff structure, the CG has a unique *SPNE*, in which a utility-maximizing selfish individual stops in every decision node. Experimental tests of the unique prediction in CG confirm game theorists' intuition, as very few experimental subjects follow it (McKelvey and Palfrey, 1992; Fey et al., 1996; Nagel and Tang, 1998; Rappaport et al., 2003; Bornstein et al., 2004).[1] Despite the experimental work on CGs, economists still do not have a clear understanding of the underlying behavioral model that makes human play diverge from equilibrium play. This is the central question addressed in this chapter.

Many explanations have been proposed for the behavior of people not following the unique *SPNE* in the CG, which we broadly classify into three categories: preference-based explanations, bounded rationality, and models that relax the common knowledge of rationality. The preference-based approach argues that people do not maximize their own payoff, as typically assumed in *SPNE*. Rather, they may be altruistic, seeking Pareto efficiency, or inequity averse (e.g. McKelvey and Palfrey, 1992).[2]

An alternative explanation is that people are not fully but *boundedly* rational. For instance, people might make mistakes when calculating or playing the optimal response to others' expected behavior. To model this idea in CGs, Fey et al. (1996) apply the *quantal response equilibrium* (*QRE*, henceforth; McKelvey and Palfrey, 1995), in which players play mutually consistent strategies but may make mistakes in their choice of actions. These mistakes have the feature that costlier mistakes are less likely to occur.

Finally, observe that even a selfish, fully rational utility-maximizer should not stop in the first round if she expects her opponent not to stop in the following round. In

[1] Section 2 reviews the theoretical and empirical literature in more detail.

[2] Levitt et al. (2011) raise the possibility that their (relatively sophisticated) subjects view the game as a game of cooperation, suggesting that non-selfish preferences might be important.

fact, the best response to the typical observed behavior is to pass in the initial rounds. Hence, people may have non-equilibrium beliefs and/or expect others to have them. Two particular models relax the assumption of common knowledge of rationality. Initially, McKelvey and Palfrey (1992) proposed a Bayesian equilibrium approach, also known as "gang of four", in which people play against a mixture of fully rational players and a small fraction of "irrational" individuals who pass in every node. A rational decision-maker thus has incomplete information regarding the rationality of her opponent. Level-k thinking model also relaxes the assumption of equilibrium beliefs: decision-makers apply a simpler rule, forming their expectations about the behavior of others, and best respond to their beliefs (Kawagoe and Takizawa, 2012; Ho and Su, 2013).

We therefore consider four classes of model. We test the ability of *SPNE* to explain individuals' behavior as a default model.[3] Alternatively, we consider three other behavioral models. First, we allow for models based on preference-based explanations, such as altruistic types. Second, to model bounded rationality we consider *QRE* that relaxes the perfect rationality of individuals, allowing them to make mistakes but keeping equilibrium beliefs and common knowledge of (ir)rationality. Finally, we test the ability of both the "gang of four" model and level-k thinking to explain non-equilibrium behavior, two models that maintain the rationality assumption but relax the common knowledge of rationality.[4]

The purpose of this study is to discriminate between *SPNE* and the other three types of alternative explanations of *initial behavior* in CGs, combining experimental and econometric techniques. The experimental design and the econometric technique

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[3]We use the strategy method in our experiment. Therefore, we actually test the unique Nash equilibrium in the reduced normal-form game. Nevertheless, since both concepts are behaviorally equivalent in CGs, we abuse the terminology and call it $SPNE$ throughout to preserve the link with the CG literature. See Section 3.2.1 for a more detailed discussion.

[4]We also consider alternative specifications of these classes of models, as well as alternative models, as discussed in Section 1.2 and Section 3.2. We selected a particular set of models for their theoretical and empirical interest, focusing on those that have been proposed in the literature.

are precisely the two features that differentiate our study from existing work on CGs.

With respect to the experimental design, we show that the two commonly used CGs, the exponentially increasing-sum variant of McKelvey and Palfrey (1992) and the constant-sum version by Fey et al. (1996), are not well suited to discriminating between the four types of explanations. We, therefore, start from a formal definition and design multiple CGs, some of which depart substantially from the CGs used in the literature (see Figure 1.5 for our 16 CGs). We use three criteria to classify our CGs: they differ in the evolution of the sum of payoffs along the different nodes: increasing-sum, constant-sum, decreasing-sum, and variable-sum CGs; we have games that start with an egalitarian division of payoffs and games that start with a non-egalitarian division; we vary the incentives to pass and the incentives to stop the game right away. The main criterion in designing our CGs was the greatest possible separation of predictions of the candidate models, with the objective of identifying the behavioral motives underlying the non-equilibrium choices.

Observe that our focus on initial responses in CGs induces us to provide no feedback concerning others' behavior during the whole experiment, which determines the use of strategy method or "cold play", in contrast to the main papers studying behavior in CGs. There are two potential problems with eliciting behavior in "hot play" when identifying the behavioral model behind the initial behavior in CGs. First, hot play makes researchers observe the complete plan of action only of subjects who stop earlier in extensive-form games. In other words, hot play in CGs endogenously determines the behavioral types that the researcher observes.[5] However, one needs to observe the complete plan of action of each subject in several games to be able to identify the underlying behavioral model a particular individual follows. Second, hot play necessarily conveys feedback from game to game, inducing learning across different

[5]For example, people following *SPNE* stop immediately in each CG. Therefore, analyzing solely the actual play of matched subjects (rather than complete plan of behavior of subjects) might result in an overestimation of the proportion of *SPNE* in the population.

CGs as suggested by previous evidence (see Section 1.2). Therefore, we use the strategy method or cold play, whereby subjects simultaneously submit their strategies game by game without receiving any feedback until all decisions have been made. In CGs, hot and cold play have been shown to produce similar behavioral patterns (Nagel and Tang, 1998, and Kawagoe and Takizawa, 2012).[6] We also find no differences between the behavior of our subjects and the initial behavior reported in other studies (see Appendix A). Therefore, we have no reasons to believe that our results are affected by the cold play method. Moreover, note that since our subjects cannot observe any past behavior of any other individual in any game and their behavior is not different from behavior using hot play, reputation-based explanations of non-equilibrium behavior can be ruled out in our data.

With respect to the econometric techniques, we apply finite mixture-of-types models. Game theory has made considerable progress in incorporating the findings of experimental and behavioral economics but behavioral game theory currently offers a large number of behavioral approaches, often resting on very different assumptions and generating very different predictions. Even though most studies compare different behavioral models on a pairwise basis, the focus has recently shifted toward coexistence and competition between behavioral models (see Camerer and Harless, 1994, and Costa-Gomes et al., 2001, for early references). We take this latter approach, exploiting finite mixture models. These models offer two distinctive features. First, in contrast to the comparison of models on a pairwise basis, they are explicitly designed to account for heterogeneity, where multiple candidate models are simultaneously allowed. If, for instance, a small fraction of individuals behave according to *SPNE* while most people are, say, boundedly rational or if, alternatively, one explanation is enough to explain individual behavior, this would be detected endogenously at the estimation

[6] Brandts and Charness (2011) review the experimental literature on all two-person sequential games and conclude that the strategy method does not generally distort subjects' behavior compared to direct responses.

stage. Second and more importantly, this technique makes the alternative behavioral models "compete" for space, because whether a model is empirically relevant, and to what extent, is determined endogenously and at the cost of the alternative models.

We find that subjects' behavior is too heterogeneous for one model to explain why people do not adhere to *SPNE* in CGs. Consistently with previous findings, only about 10% of individuals take in the very first node in most of the 16 games. More importantly, the behavior of the majority is explained by level-k thinking model and by *QRE*. Preference-based models play a negligible role in explaining non-equilibrium choices in our data. In line with the conclusions of Fey et al. (1996) and McKelvey and Palfrey (1998), our analysis corroborates that the "gang of four" model contributes little to explaining non-equilibrium behavior in CGs. In addition to the fitting exercise, we also show that the estimated mixture-of-types model, composed of a small fraction of *SPNE* and a large proportion of level-k and *QRE* types, is also successful at predicting behavior across different CGs. As a result, researchers should account for behavioral heterogeneity in CGs not only for a better explanation of behavior as advocated by this study but also for a better prediction of choices in out-of-sample games.

These results have two important implications that go beyond the CG. First, several recent papers have stressed the ability of strategic uncertainty to organize the average behavior in games that reflect the tension between maximizing payoffs and sequential rationality (Dal Bó and Frechette, 2011; Calford and Oprea, 2017; Embrey et al., 2017; Healy, 2017). However, although these studies acknowledge important individual heterogeneity, they do not ask whether the heterogeneity can be described by a single behavioral model or whether it requires a mixture of them. We propose combining experimental techniques, individual-level data on initial responses, and mixture-of-types model to both qualify and quantify this heterogeneity. The advantage of CGs, as opposed to e.g. the repeated Prisoner's dilemma, is that the "stage" payoffs can be manipulated systematically such that different theories predict different behavior, the core of our design. Our results show that bounded rationality and the failure of common knowledge of rationality are particularly relevant, while preference-based explanations play a minor role.[7]

As a second contribution, many attribute non-equilibrium behavior in many extensive-form games to their dynamic nature and the failure of backward induction, whereas our study again shows that it constitutes a more general non-equilibrium phenomenon.[8] Our subjects follow $SPNE$-like behavior in CGs that lowers incentives to pass (constant- and decreasing-sum CGs), while they systematically violate $SPNE$'s prediction in games designed to facilitate passing. More importantly, virtually all non-equilibrium behavior is best explained by *QRE* and level-*k*, two behavioral models, which have been successful in explaining behavior in static environments. These findings suggest a unified perspective on non-equilibrium behavior in both simultaneous-move and extensive-form games and call for a reevaluation of the aspects that distinguish static from dynamic games from a behavioral point of view.

The chapter is organized as follows. Section 2 reviews the literature. Section 3 sets out the theoretical framework. Section 4 introduces our experimental design. Section 5 presents the main estimation results, as well as a battery of robustness tests including out-of-sample prediction test. Section 6 concludes. The Appendices A and B contain additional material and the experimental instructions.

[7] In the repeated Prisoner's dilemma, Cooper et al. (1996) also show that multiple models are necessary to explain the behavior and Embrey et al. (2017) conclude that the existence of cooperative types has only limited effect on the extent of cooperation, the equivalent of passing in CGs.

[8] Backward induction, a fundamental concept in game theory, is also frequently at odds with human behavior (e.g. Reny, 1988; Aumann, 1992; Binmore et al., 2002; Johnson et al., 2002). However, although CG is commonly associated with the paradox of backward induction in the literature, Nagel and Tang (1998) and Kawagoe and Takizawa (2012) show that human behavior also deviates from *SPNE* when presented in normal form and Levitt et al. (2011) show that following backward induction in other games does not make people follow it in CGs.

1.2 Literature Review

CG was first proposed by Rosenthal (1981) to point out that backward induction may be counterintuitive, predicting that human subjects would rarely adhere to the *SPNE* prediction in this particular game. The original game has 10 decision nodes and the payoff sums in each node increase linearly from the initial node to the final one.

Megiddo (1986) and Aumann (1988) introduce a shorter CG with an exponentially increasing-sum of payoffs in each node, called "Share or quit". The name *centipede* is attributed to Binmore (1987), who designed a 100-node version. Aumann (1992, 1995, 1998) was the first to discuss the implications of rationality and common knowledge of rationality in CGs. He shows that although rationality alone does not imply *SPNE*, common knowledge of rationality does. The epistemic approach to explaining the paradox using perfectly rational agents has been followed by others (e.g. Reny, 1992, 1993, Ben-Porath, 1997).

McKelvey and Palfrey (1992) pioneered the experimental analysis of the CG. They apply two modest variants of Aumann's game, with four and six decision nodes, where the payoffs increase exponentially. Figure 1.1 contains the six-node CG. They focus on exponentially increasing-sum versions to reinforce the conflict between *SPNE* and the intuition. Their results indeed confirm that *SPNE* is a bad prediction for behavior in the game: only 37 out of 662 games ended in the first terminal node as predicted by *SPNE*. The majority of matched subjects ended somewhere in the middle-late nodes of the game and 23 out of 662 matches reached the final decision node (see Figure 1.3 for their distribution of reached terminal nodes in the first round in the game from Figure 1.1). They also observe little learning over repetitions of the game. They explain their findings using the "gang of four" model (Kreps and Wilson, 1982; Kreps et al., 1982). In particular, by assuming the existence (and common knowledge of this existence) of 5% of subjects who pass in every node, and by combining them with the possibility of noise in both behavior and beliefs.[9]

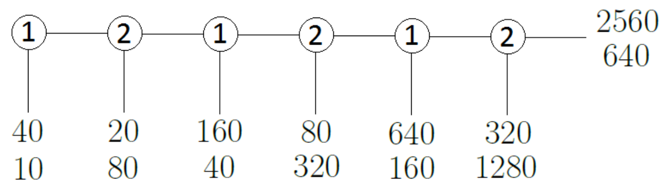


FIGURE 1.1: EXPONENTIALLY INCREASING-SUM CG IN MCKELVEY AND PALFREY (1992).

To test the hypothesis of altruism further, Fey et al. (1996) introduce a constant-sum version of CG, shown in Figure 1.2. Since the sum of the payoffs of both players in each node is the same, their and our altruistic type should be indifferent about where to stop. Less than half of the matched subjects play according to *SPNE* initially (see Figure 1.3 for the first-round behavior) even though people learn to play closer to *SPNE* with experience. Fey et al. (1996) find no evidence of altruistic types (individuals who “Always Pass") and reject the explanation based on “gang of four" provided in McKelvey and Palfrey (1992), and propose two models: an “Always Take” behavioral model, which can be rationalized by *SPNE*, *Maxmin* or *Egalitarian* (or inequity aversion, a model we consider among the social preferences), and *QRE*. They find evidence for *QRE*. Later, McKelvey and Palfrey (1998) extend *QRE* to extensive-form games, named agent-*QRE* ($AQRE$, henceforth) and apply it to the exponentially increasing-sum CG. They again reject the explanation based on “gang of four" and conclude that $AQRE$ fits individual behavior better than *QRE*. We corroborate the conclusions of both Fey et al. (1996) and McKelvey and Palfrey (1998) regarding “gang of four" (see Section 1.5.3) and consider both QRE and $AQRE$ (see Section 1.3.2).

Nagel and Tang (1998) test behavior using a 12-node CG. Unlike in previous re-

[9]This altruistic behavior, as noted by the authors, can be rationalized by assuming that altruistic subjects derive utility not only from their own payoffs but also from the payoffs of their opponents. In particular, in the exponentially increasing-sum CG, if the weight on their opponent’s payoff is 2/9 and the weight on own payoff is 7/9, altruistic subjects will always pass. The equilibrium type in McKelvey and Palfrey (1992) resembles our *SPNE* with noise (which differs from *QRE*) and differences in beliefs refer to beliefs concerning whether others are altruistic or not. The exception is their altruistic type, which is identical to our altruists. Zauner (1999) fits the proposed model to their data.

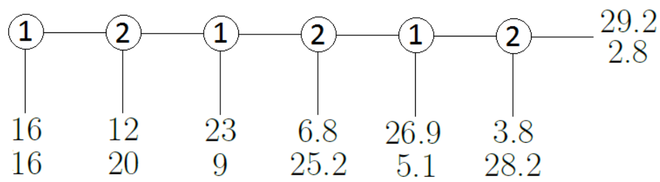


FIGURE 1.2: CONSTANT-SUM CG IN FEY ET AL. (1996).

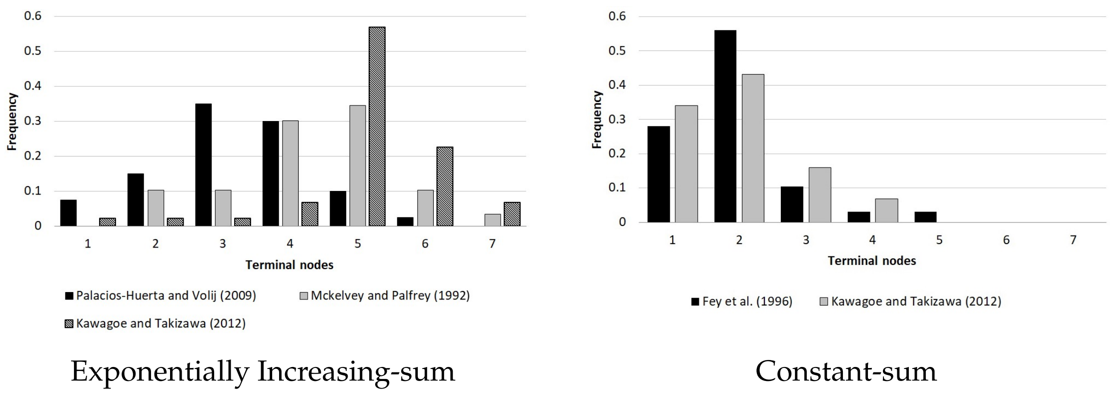


FIGURE 1.3: INITIAL BEHAVIOR IN DIFFERENT STUDIES

search, subjects in their experiment played a normal-form CG. In particular, they propose a *reduced* normal-form, which collapses all strategies that coincide in the first stopping node into one behavioral plan. In such a reduced normal-form each row/column represents the node, at which Player 1/2 stops the game if the node is reached. Subjects decide simultaneously in their experiment, but to make their approach as close as possible to a sequential play subjects only receive information about the final outcome of the game. That is, they never learn the strategy chosen by the opponent if they stop earlier. Interestingly, their results are very similar to those of McKelvey and Pafrey (1992), where the majority of subjects did not choose to take immediately and most ended the game in the middle-late nodes.[10] Their findings illustrate that non-equilibrium behavior in CGs cannot be attributed solely to the failure of backward induction but probably represents a more general non-equilibrium behavioral phenomenon.

[10]They also observe that people react differently depending on the outcome of the previous round. If they finish one game before the opponent, they tend to pass more in the next one; the opposite happens if the opponent stops first. Since we focus on the initial play here, this plays no role in our study.

In order to test for the relevance of common knowledge of rationality as opposed to other explanations, Palacios-Huerta and Volij (2009) manipulate the rationality of subjects and the beliefs about the rationality of opponents, combining students and chess players. Chess players are not only familiar with backward induction but are also *known* to be good at inductive reasoning. Using the exponentially increasing-sum CG in Figure 1.1, they find that chess players behave much closer to *SPNE* than students. More importantly, they find that chess players play closer to *SPNE* when matched with other chess players rather than students. Figure 1.3 shows the initial behavior of their students-against-students treatment, which is in line with the original findings by McKelvey and Palfrey (1992).

Later, Levitt et al. (2011) find that chess players who play *SPNE* in other games fail to do so in CGs, once again disconnecting the puzzling behavior in this game from backward-induction arguments. They comment on the possibility that their subjects may view the CG as a game of cooperation between the two players.

More recently, Kawagoe and Takizawa (2012) provide an analysis of the ability of level-k models vs. *AQRE* to explain behavior in CGs using new experimental data and the data from McKelvey and Palfrey (1992), Fey et al. (1996), Nagel and Tang (1998), and Rapoport et al. (2003). See Figure 1.3 for the behavior in the extensive-form CGs from Figures 1.1 and 1.2 in Kawagoe and Takizawa (2012). Their pairwise comparison concludes that level-k thinking model fits the data better than the *AQRE* model with altruistic players in the increasing-sum CG, while there is no difference between the models in the constant-sum CG. Related to this study, Ho and Su (2013) show that level-k thinking model explains the behavior in McKelvey and Palfrey (1992) well.

Our contribution over and above that of these two studies is that we allow multiple behavioral models simultaneously (not only *QRE* or only level-k thinking model) and that these alternative models compete with one another in explaining behavior across multiple CGs (not only the most common CGs as in Kawagoe and Takizawa, 2012, or only the exponentially increasing-sum CG as in Ho and Su, 2013, where we show that these two types of CGs are not enough to separate candidate theories). We show that different CGs are crucial in explaining non-equilibrium behavior in these games.

In a recent contribution, Healy (2017) carries out an epistemic experiment, eliciting utilities, first and second order beliefs, and actions in three variations of an increasing-sum CG. He finds important heterogeneity in both utilities and beliefs and rationalizes non-equilibrium behavior using an incomplete information setting similar in spirit to the original explanation proposed by McKelvey and Palfrey (1992).[11] In contrast to our study, Healy (2017) finds support for social preferences. Nevertheless, he only applies increasing-sum CGs that seem to exacerbate the role of altruism as pointed out by Fey et al. (1996).

Although *QRE* models bounded rationality via mistakes, there are other theories of bounded rationality that can explain behavior inconsistent with *SPNE* in CGs. Jehiel (2005) proposes an analogy-based equilibrium model in which agents have imperfect perception of the game. In particular, the decision nodes of other players are bundled into one as long as the set of actions in those nodes is the same (even if the payoff consequences differ across the decision nodes), forming a unique belief for all the bundled nodes. Depending on which nodes are bundled together, passing in CGs can be supported in equilibrium if the payoffs increase fast enough as the game develops. Another approach assumes that people have limited foresight. One example is Mantovani (2014), who proposes a model in which individuals only consider a limited number of subsequent decision nodes and truncate the CG afterwards. He shows that passing in CGs can be rationalized as long as the incentives for passing are high enough and the final node is not included in the limited horizon of individuals. We do not include these alternative bounded rationality models in our main analysis.[12]

[11] In line with Dal Bó and Frechette (2011) and Embrey et al. (2017), Healy (2017) employs the term strategic uncertainty, rather than the failure of common knowledge of rationality.

[12] Empirically testing Jehiel's (2005) model is not straightforward with our design based on a strategic method to identify initial responses. See e.g. Danz et al. (2016) for such a test. Regarding Mantovani

1.3 THEORETICAL FRAMEWORK

1.3.1 DEFINITION OF THE CENTIPEDE GAME

The CG is a two-player extensive-form game of perfect information, in which the players make decisions in alternating order. We denote by *Player 1* the player deciding in the odd decision nodes, while *Player 2* refers to the player who decides in the even decision nodes. The game can vary in length and we denote the number of decision nodes by R. In each decision node one player decides between two actions: *Take*, which ends the game immediately, and *Pass*, which leads to the next node, giving the turn to *Take* or *Pass* to the other player. Figure 1.4 shows an example of a CG with $R = 6$.

The game differs from similar extensive-form games in the conditions on the payoff structure. Let x_{ir} represent the payoff that the deciding player i receives if she takes in a decision node r and let x_{jr} be the payoff of the non-deciding player $j \neq i$ in r. Then, in any CG, for the decision node for player i:

$$x_{ir} < x_{ir+2} \text{ for } \forall r \text{ such that } 1 \leq r \leq R-1 \tag{1.1}$$

$$x_{jr} < x_{jr-1} \text{ for } \forall r \text{ such that } 2 \leq r \leq R+1 \tag{1.2}$$

Expressions (1.1) and (1.2) summarize the trade-off that people face in CGs. The first inequality represents the incentive to pass and move on in the game, since the payoff from choosing *Take* in the next decision node where i decides is higher than in the current one. By contrast, the second inequality illustrates the incentive to take

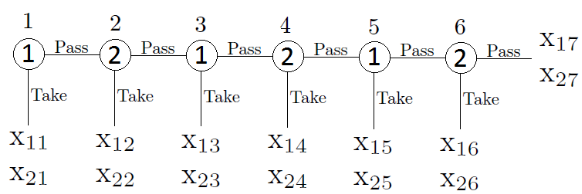
(2014), we re-estimate a variation of our main model which includes three additional behavioral types: players who consider two, three, and four subsequent decision nodes when deciding whether to take or pass. The predicted behavior of the types that consider two and three subsequent decision nodes is very similar to that of *L1* and *L2*, but when all models are jointly considered in one mixture-of-types model the shares of *L1* and *L2* remain virtually unaffected while we find no support for these two limited-foresight types. Hence, level-*k* explains individual behavior better in our data. If foresight is increased to four, such players behave as *SPNE* in almost all our games. Therefore, for their theoretical and empirical interest, we focus on *SPNE* and opt for *QRE* as a representation of bounded rationality.

before the opponent does.

We refer to the sum of player' payoffs in a particular decision node r by S_r:

$$S_r = x_{ir} + x_{jr} \tag{1.3}$$

Conditions (1.1) and (1.2) have some implications for the design of different variation of CGs. First, $x_{ir} > x_{ir-1}$; that is, the payoff in a decision node is higher than in the previous non-decision node. Second, $S_r < x_{ir+2} + x_{jr-1}$ in each r player i decides in. In words, the sum of payoffs in each decision node is lower than the sum of the payoff resulting from action *Take* by i in the player's next decision node and the payoff that the opponent "sacrifices" by passing in the previous decision node. Third, although the literature has only used CGs with increasing- or constant-sum evolution of payoffs over the different decision nodes, it is easy to show that (1.1) and (1.2) allow for any evolution of S_r as the game progresses. Hence, there are decreasing-sum versions and even CGs with variable-sum which show non-monotonic patterns, disregarded in the previous literature (see Figure 1.5 for examples; Figures 1.7 and 1.8 in the Appendix A provide an alternative visualization of the same CGs).



	Take the first time	Take the second time	Take the third time	Always pass
Take the first time	x_{11}, x_{21}	x_{11}, x_{21}	x_{11}, x_{21}	x_{11}, x_{21}
Take the second time	x_{12}, x_{22}	x_{13}, x_{23}	x_{13}, x_{23}	x_{13}, x_{23}
Take the third time	x_{12}, x_{22}	x_{14}, x_{24}	x_{15}, x_{25}	x_{15}, x_{25}
Always pass	x_{12}, x_{22}	x_{14}, x_{24}	x_{16}, x_{26}	x_{17}, x_{27}

FIGURE 1.4: EXTENSIVE-FORM (TOP) AND ASSOCIATED REDUCED NORMAL-FORM (BOTTOM) REPRESENTATION OF A GENERAL SIX-NODE CG.

In this study, we focus on CGs with six decision nodes. The upper part of Figure 1.4 displays a general version of the six-node CG in extensive form, and the lower part presents the corresponding *reduced* normal-form representation.[13] In this reduced normal-form, each player has the following four pure strategies: *Take the first time*, *Take the second time*, *Take the third time*, and *Always pass*. A player selecting the first option finishes the game the first time she plays. That is, Player 1 would finish the game in node 1 in the upper part of Figure 1.4. Analogously, Player 2 selecting this option would finish in node 2. *Take the second time* corresponds to pass once and ending the game the second time that the player has a chance to play. *Take the third time* consists of passing twice and choosing *Take* the third time. Finally, *Always pass* entails choosing always *Pass* and reaching the payoffs in the very last node.

1.3.2 Candidate Explanations of Behavior in Centipede Games

We introduce each behavioral type and describe its predictions in our reduced normal-form CGs. In some cases, one model is indifferent between different strategies, in which case we assume that people select uniformly among them. For the predictions of each behavioral model in the CGs used in this experimental study, see Tables 1.3 and 1.4 below. Figures 1.9 and 1.10 in the Appendix A show these same predictions using the game trees of the different games.

Nash Equilibrium in the Reduced Normal-form, *SPNE*

Given the payoff structure of the CG described in Section 1.3.1, the *SPNE* type should always choose *Take* in every decision node. In the reduced normal-form game, there only exist one Nash equilibrium in pure strategies, where both players choose *Take the first time*. Since this behavior is consistent with the *SPNE*, we abuse the terminology and refer to this Nash Equilibrium as *SPNE* throughout the chapter. This prediction is

[13]Note that the figure does not contain all the strategy combinations. Rather, each behavioral plan in Figure 1.4 represents all strategies that take for the first time in the same decision node. Nagel and Tang (1998) experimentally test the same reduced normal-form, instead of the full normal-form representation. The latter leads to an enormous strategy space where many strategy profiles have the same payoffs. For a more thorough discussion, see Footnote 1 in Nagel and Tang (1998).

unique and the same for all types of CGs.[14]

ALTRUISM, A AND $A(\gamma)$

In contrast to the standard selfish preferences, individuals might care about other players' payoffs in an altruistic way. We allow for two alternative models of altruism. In Section 1.5.3, we present two sets of results, one for each of the two definitions of altruism.

First, following Costa-Gomes et al. (2001), we assume that altruistic individuals (A, henceforth) weight their own payoffs as much as the payoffs of the opponent, such that they are maximizing the sum of payoffs, S_r, independently of how that sum will be split between the two players. Also, despite taking into account opponents' payoffs, A is non-strategic in that she chooses the strategy that leads to the maximum S_r out of all possible strategies and expects the same behavior from the opponent. Following this definition, the behavior of A is determined by the progression of the payoff sum in the CG. A chooses *Always pass* in increasing-sum CGs, *Takes the first time* in decreasing-sum CGs, and is indifferent between the four strategies in constant-sum CGs. The stopping node of A can be manipulated to lie anywhere in the variable-sum CGs.

Second, following how altruism has been modeled in economics and keeping such type fully strategic, we assume that altruistic individuals' utility is given by their own payoff and a weight (γ) on the payoff of the opponent, where $0 \leq \gamma \leq 1$. Such a type assumes that her opponent is of the same type and selects the Nash equilibrium in the reduced normal-form games expressed in terms of their utilities (rather than payoffs). We refer to this model by $A(\gamma)$. Note that for low values of γ this altruistic type is close to $SPNE$, with $A(0) = SPNE$.[15]

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[14] All pure strategies in the reduced normal-form CG are rationalizable. Using the extensive-form variation of rationalizability, only the *SPNE* is rationalizable.

[15] In fact, $A(\gamma) = SPNE$ for roughly $\gamma < 0.12$ in our experiment; for assessing the separation between $SPNE$ and $A(\gamma)$, see Table 1.10.

PARETO EFFICIENCY, *PE*

Pareto efficiency is another classic concept in economics. A payoff profile in a node is Pareto efficient if it is not possible to make a player better off without making the opponent worse off. For the sake of simplicity, we again assume that this type is non-strategic. In the reduced normal-form, *PE* type selects the strategy that yields a Pareto efficient payoff profile.

For instance, only the two payoff profiles in the last decision node are Pareto efficient in exponentially increasing-sum CGs. Hence, *PE*-Player 1 chooses *Always pass*, and *PE* Player 2 randomizes between *Take in the third* and *Always pass*. In fact it follows directly from the payoff structure of the game, described in Section 1.3.1, that the two payoff profiles in the last decision node are Pareto efficient in *any* CG. Moreover, the number of Pareto efficient outcomes and where they are located in the sequence of the game can vary substantially. By (1.1), every outcome can potentially be Pareto efficient. This is indeed the case in all the constant-sum and decreasing-sum CGs.

INEQUITY AVERSION, *IA* AND *IA*(ρ,σ)

Rather than caring about efficiency or others' payoffs directly, some people might care about payoff inequalities. Similar to altruism, we allow for two types of inequity aversion preferences and present two sets of results in Section 1.5.3, one for each of the two definitions of inequity aversion.

First, analogously to *A*, we assume that *IA* minimizes the difference in payoffs between the two players in a non-strategic way.[16] *IA* first calculates the absolute values of the differences between her payoffs and her opponent's payoffs for each strategy combination. Then, she takes the action (or actions if indifferent across more than one action) that leads to the minimum payoff difference.

[16] We follow the equivalent assumption as in the definition of *A* and assume that *IA* implicitly believes that other players are also *IA*.

For instance, consider *IA*-Player 1 in the in CG in Figure 1.1. The action *Take the first time* generates a difference of 30 independently of the choice made by Player 2, *Take the second time* yields the differences of 60, 120, 120, and 120 for the four respective strategies of Player 2, *Take the third time* leads to 60, 240, 240 and 240, and finally *Always pass* 60, 240, 960, and 1920. An *IA*-player computes the smallest differences (30 in the first case vs. 60 in the remaining cases) and selects the strategy corresponding to the minimum, i.e. *Take the first time*. The decision-making process of an inequity-averse Player 2 is characterized analogously.

Second, following more closely Fehr and Schmidt (1999) and Bolton and Ockenfels (2000) and keeping the type fully strategic, $IA(\rho, \sigma)$ individuals' utility is given by their own payoff minus the difference between the two individuals' payoffs. When the opponent is getting a lower payoff than oneself, the utility is given by own payoff minus the difference between own payoff and the opponent's payoff, weighted by $0 \leq \rho \leq 1$, and when the opponent is getting a higher payoff than oneself, the utility is given by own payoff minus the difference between the opponent's payoff and own payoff, weighted by $0 \leq \sigma \leq 1$. Other than that, $IA(\rho, \sigma)$ is modeled as $A(\gamma)$. If ρ and σ are small, $IA(\rho, \sigma)$ behaves very similarly to *SPNE*, with $IA(0, 0) = SPNE$.[17]

Optimistic, *O*

We also include a non-strategic type with naïvely optimistic beliefs (as in Costa-Gomes et al., 2001). These optimists (*O*) make *maximax* decisions, maximizing their maximum payoff over the other players' strategies. Such an *O*-type player assumes that each strategy yields the maximum payoff over all possible actions of the opponent and selects the action corresponding to the maximal payoff from among those.[18]

[17]In our experiment, $IA(\rho, \sigma) = SPNE$ for any σ if $\rho \leq 0.20$; for assessing the separation between $SPNE$ and $IA(\rho, \sigma)$, see Table 1.11.

[18]One can analogously define a pessimistic type, *P*, who makes *maximin* decisions. However, this behavioral type is almost indistinguishable from *SPNE* in CGs. By definition, type *P* never separates from *SPNE* for Player 1 and shows only minor separation for Player 2, so we do not include it in our analysis.

For instance, in the game shown in Figure 1.1 *O* calculates the maximum payoff from each of her strategies (40 from *Take the time*, 160 from *Take the second time*, 640 from *Take the third time*, and 2560 from *Always pass*) and selects the maximum of these maxima (leading to *Always pass*). In any CG, an *O*-Player 1 always passes, while an *O*-Player 2 always chooses *Take the third time*. Notice that this type is often closely related to *A* and *PE*, but their predictions differ enough in our games to be able to include them all in the analysis (see Table 1.2 in Section 1.4 and Tables 1.3 and 1.4 in Section 1.5.1 for particular predictions in the CGs used in our design).

LEVEL-*k* THINKING MODEL, *L1*, *L2*, *L3*

This section focuses on level-k as a representation of the failure of common knowledge of rationality. Fey et al. (1996) and McKelvey and Palfrey (1998) reject "gang of four" as a relevant explanation of behavior in CGs and Section 1.5.3 shows that the conclusions drawn from our benchmark models are robust to considering "gang of four."

Level-*k* thinking has proved successful in explaining non-equilibrium behavior in many experiments (see Crawford et al. (2013) for a review). Level-*k* types (*Lk*) represent a rational strategic type with non-equilibrium beliefs about others' behavior, in that they best respond to beliefs but they have a simplified non-equilibrium model of how other individuals behave. This rule is defined in a hierarchical way, such that an *Lk* type believes that others behave as *Lk-1* and best-responds to these beliefs. The hierarchy is specified on the basis of a seed type *L0*. We set the *L0* player as randomizing uniformly between the four available strategies in the reduced normal-form CG.[19] That is, *L0* selects each strategy with probability 0.25. We assume that this type only exists in the minds of higher types. *L1* responds optimally to the behavior of *L0*,[20]

[19] We also tested other $L0$ specifications. For example, following Kawagoe and Takizawa (2012), we also consider a dynamic version of level-*k*, in which *L0* uniformly randomizes in each decision node. The simultaneous version of level-*k* shows a better fit. We have also considered $L0$ an altruist, who maximizes the sum of payoffs, S_r, as well as an optimist. We find little evidence in favor of these alternative specifications in our data.

[20] *L1* is sometimes called *Naïve*. See e.g. Costa-Gomes et al. (2001).

L2 assumes that the opponents are *L1* and responds optimally to their optimal behavior, and finally *L3* believes that others behave as *L2* and best-responds to these beliefs. Since the empirical evidence reports that such lower-level types are the most relevant in explaining human behavior (Crawford et al., 2013), we do not include higher levels in our analysis.

Given the relative complexity of level-*k*, we illustrate the behavior of the different levels on the CG in Figure 1.1. As mentioned above, *L0* chooses each strategy in the normal-form with probability 0.25, independently of whether she is Player 1 or 2. Considering this behavior of *L0*, *L1* first computes the expected payoff from the four available strategies and selects the strategy that maximizes the expected payoff. For Player 1 in Figure 1.1, the four strategies yield expected payoffs of 40, 125, 345, and 745, respectively. Consequently, *L1*-Player 1 selects *Always pass*. *L1*-Player 2 and all the other *Lk* with $k > 1$ are defined analogously. In general, *Lk* types exhibit no particular pattern of behavior in CGs. Thus, they have to be specified on a game-by-game basis (see Tables 1.3 and 1.4 in Section 1.5.1 for different predicted behavior by level-*k*'s in the CGs used in our experiment).

QUANTAL RESPONSE EQUILIBRIUM, *QRE*

Lastly, we consider the logistic specification of McKelvey and Palfrey's (1995) *QRE*. In words, the *QRE* approach assumes that people, rather then being perfect profit maximizers, make mistakes and that more costly mistakes are less likely to occur. Moreover, in equilibrium, people also assume that others make mistakes that depend on the costs of each mistake. Each strategy is played with a positive probability, with *QRE* being a fixed point on these noisy best-response distributions. In the logistic specification, parameter λ reflects the degree of rationality such that if $\lambda = 0$ the behavior is purely random while as $\lambda \to \infty$ *QRE* converges to a Nash equilibrium. The evidence suggests that small λ's typically fit the data from individuals' initial behavior best (McKelvey and Palfrey, 1995). To compute the *QRE*'s for our games, we used Gambit software (McKelvey et al., 2014).[21]

It is worth stressing that *QRE* differs from ϵ-equilibrium, noisy *SPNE*, and noisy *Lk*. ϵ-equilibrium is defined as a profile of strategies that *approximately* satisfies the condition of Nash equilibrium (Radner, 1980). In CGs, the main difference between ϵ-equilibrium and QRE is that the former expands the set of Nash equilibria as ϵ increases while the latter moves equilibrium play away from *Take the first time*.[22] As for noisy *SPNE*, such players make mistakes while best-responding to error-free equilibrium behavior of others, whereas *QRE* individuals make mistakes and assume that others also make mistakes. Hence, both *QRE* and *SPNE* with noise embody the idea of bounded rationality (as opposed to level-k that reflects the idea of non-equilibrium beliefs and therefore the absence of common knowledge of rationality). Moreover, the mistakes in ϵ-equilibrium and noisy $SPNE$ do not necessarily possess any economic structure because the errors are specified at the estimation stage, rather than being part of the model as in *QRE*. More importantly, even though the three types predict similar behavior in many cases, they are far enough apart in our CGs. See Table 1.2 in Section 1.4 for an evaluation of the separation between these behavioral types', and Tables 1.3 and 1.4 in Section 1.5.1 for predicted behavior in the different CGs used in our experiment.

[21] Following McKelvey and Palfrey (1998) and Kawagoe and Takizawa (2012), we have also considered *AQRE*, the *QRE* applied to extensive form games, but, as it occurs for the dynamic version of level-*k*, simultaneous *QRE* shows a better fit.

[22] We have included ϵ-equilibrium as an additional behavioral type in our mixture-of-types model and we find little evidence for its relevance. We estimate very low frequency for this type and the estimated ϵ is so high that it includes almost any strategy, resembling a purely random type in almost all our CGs.

1.4 Experimental Design

1.4.1 Experimental Procedures

A total of 151 participants were recruited using the ORSEE recruiting system (Greiner, 2015) in four sessions in May 2015.[23] We ensured that no subject had participated in any similar experiment in the past. The sessions were conducted in the Laboratory of Experimental Analysis (Bilbao Labean; http://www.bilbaolabean.com) at the University of the Basque Country using z-Tree software (Fischbacher, 2007).

Subjects were given instructions explaining three examples of CGs (different from those used in the main experiment), how they could make their choices, the matching procedure, and the payment strategy. The instructions were read aloud. Subjects were allowed to ask any questions they might have during the whole instruction process. Afterwards, they had to answer several control questions on the computer screen to be able to proceed. An English translation of the instructions can be found in the Appendix B.

At the beginning of the experiment, the subjects were randomly assigned to a role, which they kept during the whole experiment. There were two possible roles: Player 1 and Player 2. To avoid any possible associations from being the first vs. second or number 1 vs. 2, subjects playing as Player 1 were labeled as *red* and those playing as Player 2 were called *blue*. Each subject played 16 different CGs one by one with no feedback between games. The games were played in a random order, which was the same for all subjects (see footnote 24). Subjects made their choices game by game. They were never allowed to leave a game without making a decision and get back to it later, and they never knew which games they would face in later stages. There was no time constraint and the participants were not obliged to wait for others while making their

[23]Given the matching mechanism described below, we did not need the number of participants to be even.

choices in the 16 games. Our design minimizes reputation concerns and learning as far as possible. Hence, the choice in each game reflects the initial play and each subject can be treated as an independent observation.

The CGs were displayed in extensive-form on the screens, as shown in the instructions in the Appendix B. The behavior was elicited using the strategy-method. More precisely, the branches in the game were generally displayed in black but the branches corresponding to each players' actual choices were displayed in red for Players 1 and in blue for Players 2. Depending on the player, they had to click on a square white box that stated either "Stop here" or "Never stop". To ensure that subjects thought enough about their choices, once they had made their decision of whether to stop at a node or never stop by clicking on the corresponding box, they did not leave the screen immediately. Rather, the chosen option changed color to red or blue depending on the player and they were allowed to change their choice as many times as they wished, simply by clicking on a different box. In such a case, the previously chosen option would turn back to white and the newly chosen action would change color to either red or blue. To proceed to another game in the sequence, the subjects had to confirm their decision by clicking on an "OK" button in the bottom right corner of the screen. They were only allowed to proceed once they had confirmed. In terms of strategies, for each game and each player type, participants faced four different options to click on: *Take the first time*, *Take the second time*, *Take the third time*, and *Always pass*, without knowing the strategy chosen by the other player. The appendix provides some examples of how the different stages were displayed to the subjects in the experiment.

When all subjects had submitted their choices in the 16 CGs, three games were randomly selected for payment for each subject. Hence, different participants were paid for different games. The procedure, which was carefully explained to the subjects in the instructions, was as follows. The computer randomly selected three games for each subject and three different random opponents from the whole session, one for each of these three games. This means that the same participant may have served as an opponent for more than one other participant. Nevertheless, being chosen as an opponent does not have any payoff consequence. To determine the payoff of a subject from each selected game, her behavior in each game was matched to the behavior of the randomly chosen opponent for this game. At the end of the experiment, the subjects were privately paid the sum of the payoffs from the three games selected, plus a 3 Euro show-up fee. The average payment was 17.50 Euro, with a standard deviation of 16.93.

At the end of the experiment, the participants were invited to fill in a questionnaire eliciting information in a non-incentivized way concerning their demographic variables, cognitive ability, social and risk preferences.

1.4.2 Experimental Games and Predictions of Behavioral Types

Figure 1.5 displays the 16 different games, CG 1 - CG 16, that each subjects faced in our experiment.[24] Figures 1.7 and 1.8 in the Appendix A provide an alternative graphical visualization of these games.

For predictions of behavioral types, see Tables 1.3 and 1.4, where each behavioral model's prescribed choice is shown for each game and player role.[25] For instance, in any of the 16 CGs, both players should stop immediately if they play according to *SPNE*. Hence, *SPNE* is written for both player roles below the choice of *Take the first time*. In a few instances, one model is shown to be indifferent between two or more strategies. In such case, one behavioral model appears in columns corresponding to different strategies. For example, in any of the 16 CGs, the last two strategies for Player 2, *Take the third time* and *Always pass*, include the *PE* label. That means that a PE-Player 2 is indifferent between the two choices *Take the third time* and *Always pass*.[26] To

[24]For the sake of illustration, we display them in a particular order. During the experiment, subjects played the 16 games in the following randomly generated order: CG 6, CG 13, CG 16, CG 1, CG 8, CG 12, CG 3, CG 14, CG 7, CG 10, CG 2, CG 4, CG 11, CG 9, CG 15, CG 5.

[25]The same information is displayed differently in Figures 1.9 and 1.10 in the Appendix A.

[26]Since some types may lead to such indifferences more often than others, one may ask whether these types may not be artificially favored. Our below empirical approach controls carefully for such a possi-

make it easier to read the predictions of different behavioral types, we show the *QRE*'s predictions in a separate row. By definition, *QRE* predicts playing each strategy with a positive probability and the probabilities depend on the parameter λ. For the sake of illustration, we show the predicted frequencies of *QRE* for one particular value of the noise parameter $\lambda = 0.38$. Similarly, we show the predictions for $A(\gamma = 0.22)$ and $IA(\rho = 0.08, \sigma = 0.55)$. The values of the parameters were chosen once the estimations had been made (see below). Most information regarding these parametric types below will also be reported for their estimated values.

We now explain our games in more detail and comment on the prediction as regards behavioral models. First, since many studies apply the exponentially increasing-sum CG from McKelvey and Palfrey (1992) shown in Figure 1.1 and the constant-sum from Fey et al. (1996) shown in Figure 1.2, we also include them in our analysis. The former is labeled as CG 1 and the latter as CG 9 in Figure 1.5. Including these two games enables us to compare the behavior of our subjects with other studies that have analyzed these games using different experimental procedures. Appendix A shows that the behavior in these two games in our experiment replicates the patterns of behavior in other studies. When we look at the behavioral predictions of the different models in CG 1 and CG 9 in Tables 1.3 and 1.4, it is important to observe that using only these two games is not helpful for separating many candidate explanations. The predictions of most relevant models are highly concentrated in the middle or late nodes in CG 1, while the same models' predict stopping at in the initial nodes in CG 9. This makes it hard to discriminate between many models solely on the basis of behavior in these two games.

Second, as clearly shown by Figures 1.7 and 1.8 in the Appendix A, the payoff from ending the game at the very first decision node is characterized by an unequal split (40,10) in half of the games (CGs 1, 3, 5, 7, 11, 13, 15, 16), while the initial-node payoffs

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bility.

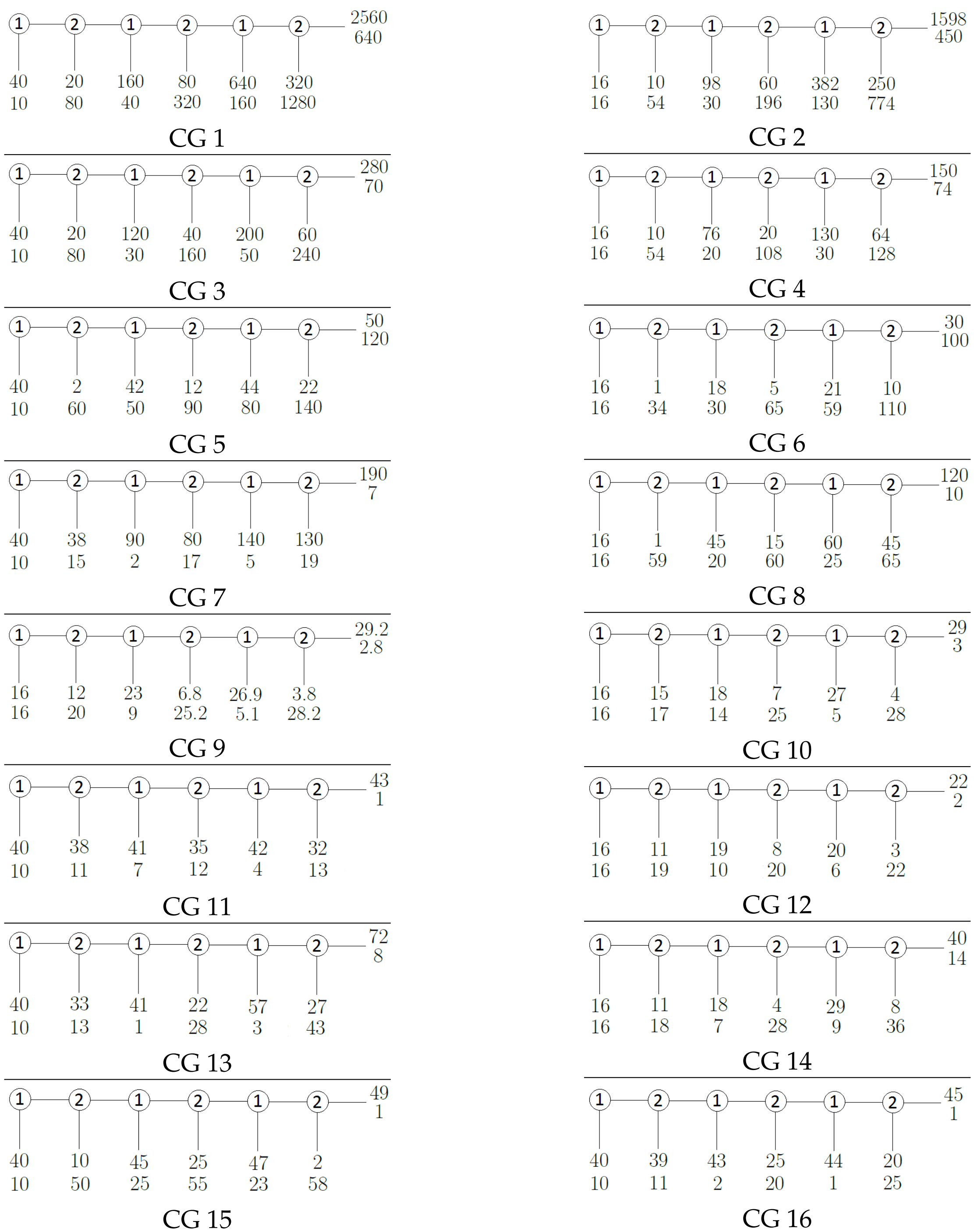


FIGURE 1.5: THE 16 CGS USED IN THE EXPERIMENT.

are the same for both players (16,16) in the other half (CGs 2, 4, 6, 8, 9, 10, 12, 14). The standard constant-sum CG of Fey et al. (1996) is the only CG in the literature that starts with an equal split of payoffs. As discussed above, this may make IA-players look like *SPNE* and one cannot distinguish between the two types on the basis of a single game. We therefore vary the payoff distribution across player roles in the initial node.

Third and more importantly, the games can be classified based on the evolution of the sum of payoffs, S_r, as the game progresses. This is again cleanly visible in Figures 1.7 and 1.8 in the Appendix A. There are four types: 8 increasing-sum games (CG $1-8$), 2 constant-sum (CG $9-10$), 2 decreasing-sum (CG $11-12$) and 4 variable-sum (CG $13-16$). The constant- and decreasing-sum CGs provide little room for discrimination, since most behavioral types predict stopping at early nodes in these games. Therefore, they only represent 25% of the games. The increasing- and variable-sum games provide the most room for separation of the alternatives, and therefore account for 75% of the games. For example, the (not necessarily exponentially) increasing-sum CGs are very successful at separating between *Lk* and *QRE*. In particular, CG 3 separates *L1* and *QRE* for both player roles (see also Figures 1.9 and 1.10 in the Appendix A). By contrast, exponentially increasing-sum CGs are not good at separating A from any *Lk*. CG $5-8$ offer important differences in the payoff path for each player, separating radically different levels of strategic reasoning. Interestingly, the variable-sum CGs allow for an arbitrary placement of the predicted stopping probabilities for many behavioral models and we design these games to exploit this feature. For instance, S_r decreases initially and increases afterwards in CG 13 and 14, with the very final payoff being greater than the initial one ($S_1 < S_7$). These two games are good at separating well the predictions of most of our alternatives (see Tables 1.3 and 1.4 and Figures 1.9 and 1.10 in the Appendix A). Additionally, CG 13 and 14 are the only games, in which only PE predicts stopping at the initial and final nodes. CG 16 is the only situation, where A takes earlier than our *Lk* types.

Finally, our games vary in the incentives to move forward and those to stop the game. To give an example, CG 10 has a constant-sum of payoffs in all nodes (as well as CG 9), but is designed such that *Lk* and *QRE* predict stopping as late as possible. In some games, the incentives to stop or not are different for the two player roles. For instance, CG 5 and 6 provide incentives for Player 1 to stop the game early and incentives for Player 2 to proceed. By contrast, CG 7 and 8 have the opposite incentive structure for the two roles. Figures 1.7 and 1.8 in the Appendix A also helps visualizing the differences in incentives by different player roles.

1.4.3 Prediction Precision of Different Behavioral Models and Their Separation

We start by assessing how precise the behavioral models are in their predictions. In a particular game and for a particular player role, if a behavioral model assigns probability one to a single strategy we say that the model is the most precise as it can only accommodate one out of four strategies, while if it assigns a positive probability to any strategy we say that the model is the least precise as it can accommodate any behavior.

Table 1.1 summarizes the average imprecision across our 16 CGs for each of the behavioral models, separated according to player roles. Each number is the average percentage of strategies predicted to be chosen with a positive probability by the corresponding model. For instance, 0.25 means that *SPNE* makes a single prediction (out of four) in all games for both players, whereas the 1's corresponding to *QRE* reflect the idea that all strategies are predicted to be played with a positive probability by this model.

The table reveals that *SPNE*, *O*, and *L1* make the most precise predictions on average. Naturally, *QRE* exhibits the lowest precision, followed by *PE* and *IA*. Although higher imprecision gives a higher probability of success *a priori*, overall compliance rates in Section 1.5.2 and our estimates in Section 1.5.3 show that this is not necessarily the case. Moreover, the proposed likelihood specification in our estimation method penalizes such imprecisions; see Section 1.5.3.

TABLE 1.1: AVERAGE IMPRECISION IN PREDICTION OF DIFFERENT MODELS ACROSS THE 16 CGS

Behavioral Type	Player 1	Player 2
SPNE	0.25	0.25
A(γ=.22)	0.30	0.31
IA(ρ=.08,σ=.55)	0.25	0.34
A	0.41	0.48
IA	0.31	0.80
PE	0.53	0.70
O	0.25	0.25
L1	0.25	0.25
L2	0.25	0.39
L3	0.25	0.55
QRE(λ=.38)	1.00	1.00

Notes: THE TABLE REPORTS THE AVERAGE IMPRECISION OF EACH BEHAVIORAL TYPE OVER THE 16 CGS FOR PLAYERS 1 AND 2, SEPARATELY. THE MAXIMUM PRECISION IS 0.25 WHEN A MODEL PREDICTS ONE UNIQUE STRATEGY IN EACH CG; THE MINIMUM PRECISION IS 1 WHEN A MODEL ASSIGNS POSITIVE PROBABILITY TO EACH STRATEGY IN ALL THE CGS.

Since the main criterion applied in the selection of the 16 games was to separate the predictions of the candidate explanations as far as possible, we now discuss and assess how suitable the selected CGs are for discriminating between the alternative theories.

To this aim, Table 1.2 shows the fractions of decisions (out of a total of 32 for both player roles) in which two different behavioral types predict different strategies.[27] The first row and column list the different behavioral types. A particular cell ij reports the separation value between the behavioral type in row i and the behavioral type in column j. The minimum value in a cell is 0 whenever two behavioral types make the same predictions in all the 32 decisions, while the maximum value would be 1 if two types differ in their predictions in all the 16 games for both player roles.[28]

[27]Table 1.12 in the Appendix A provides an alternative view of separability, which we refer to as separation in payoffs. It leads to the same conclusion as Table 1.2, so we relegate the table and its discussion into the Appendix A.

[28]The separation is computed as follows. When two types make a single prediction in a CG, it is either different or the same, and yields a separation value of 1 or 0, respectively. When at least one type predicts a distribution over more than one action in a CG, define $P = (P_1, P_2, P_3, P_4)$ for one type and

Note that *QRE* presents some difficulties.[29] To simplify matters, the separation values are thus computed assuming that a *QRE*-type player has a probability one of playing the action with the largest predicted probability given $\lambda = 0.38$. Obviously, this simplification is not made in the estimations below; the estimations consider the exact probabilities of each strategy (as reported in Tables 1.3 and 1.4).

TABLE 1.2: SEPARATION RATES IN THE DECISIONS BETWEEN DIFFERENT MODELS

	SPNE	$A(\gamma)$	$IA(\rho,\sigma)$	*A*	*IA*	*PE*	*O*	*L1*	*L2*	*L3*
SPNE	0.00									
A(γ=.22)	0.14	0.00								
IA(ρ=.08,σ=.55)	0.05	0.18	0.00							
A	0.88	0.84	0.86	0.00						
IA	0.58	0.62	0.53	0.72	0.00					
PE	0.87	0.81	0.84	0.28	0.63	0.00				
O	1.00	0.95	0.98	0.61	0.78	0.47	0.00			
L1	0.72	0.68	0.70	0.81	0.78	0.73	0.75	0.00		
L2	0.66	0.61	0.62	0.85	0.73	0.80	0.85	0.60	0.00	
L3	0.55	0.51	0.53	0.78	0.70	0.77	0.95	0.86	0.54	0.00
QRE(λ=.38)	0.33	0.29	0.33	0.88	0.72	0.85	0.97	0.66	0.54	0.52

Notes: THE TABLE REPORTS AVERAGE SEPARATION RATES OVER THE 16 CGS AND OVER THE TWO PLAYER ROLES BETWEEN THE BEHAVIORAL MODELS LISTED IN THE CORRESPONDING ROW AND COLUMN. THE MINIMUM SEPARATION IS 0 WHEN TWO MODELS PREDICT THE SAME BEHAVIOR FOR BOTH PLAYER ROLES IN EACH CG; THE MAXIMUM SEPARATION IS 1 WHEN TWO BEHAVIORAL MODELS ALWAYS PREDICT DIFFERENT BEHAVIOR.

It can be seen that the majority of the candidate behavioral types considered are separated in at least 50% of decisions in our design and the figures are even larger in most cases. For example, note that the separation between *Lk* and *QRE* is particularly interesting. The literature traditionally finds difficulties in separating these two models, as they prescribe very similar behavior in many games. This is not our case

P' analogously for the second. Let $n = |j : P_j > 0 \vee P'_j > 0|$ be the number of strategies predicted to be played with positive probability by at least one of the two types and $s = |j : P_j > 0 \wedge P'_j > 0|$ the number of strategies predicted with positive probability by both. Then, the separation value between both types in the CG is $(n - s)/n$. For example, if type *i* predicts choosing the actions *Take the first time*, *Take the second time*, *Take the third time*, and *Always pass* with probabilities (1/3,0,1/3,1/3) and type j with probabilities (0,1,0,0), the two types are fully separated, leading to the value of 1. If type j predicts (1/2,1/2,0,0) instead, the value is 3/4 because i's and j's predictions differ in only three out of four actions predicted by at least one of the model. Finally, if j predicts (0,0,1/2,1/2), the separation is 1/3.

[29] Since QRE assigns positive probability to all strategies, the usual calculation of separability for QRE would just reflect the relative imprecision of the predictions of the model that you are comparing QRE to.

though. Hence, our design enable us to discriminate between these two theories. However, there are few exceptions on which we comment in what follows. PE is separated from O in slightly less than 50% and from A in 28%. The table suggests that there might be separation problems between SPNE and QRE. Nevertheless, note that separation of QRE is computed differently from other models and $SPNE$ always predicts one unique strategy that is often the strategy predicted with the highest probability by QRE. As a result, the real separation between these two models is way higher than the 33% reported in Table 1.2.[30]

The real separability issues arise with $A(\gamma)$ and $IA(\rho, \sigma)$ in relation to $SPNE$ for the estimated values of their parameters. Observe that the behavioral predictions of $SPNE$ and these two social-preference models are the same in most games and player roles. As shown in Tables 1.3 and 1.4, $SPNE$ and $IA(\rho = 0.08, \sigma = 0.55)$ are only separated in two decisions (out of 32) whereas $SPNE$ and $A(\gamma = 0.22)$ only in six of them (out of 32). Moreover, both $A(\gamma = 0.22)$ and $IA(\rho = 0.08, \sigma = 0.55)$ predict multiple strategies in all these cases (but one), one of which often is the same as the one predicted by $SPNE$ (lowering further the separability). Tables 1.10 and 1.11 in the Appendix A evaluate the overall separability of these social-preference types with $SPNE$, for different values of their parameters γ, ρ, and σ. The tables reveal that both models can be very well separated from $SPNE$ if their parameters are high and relevant enough. In other words, if these preferences types cared *enough* about the payoff of others (positively for altruism, or positively and negatively depending on the relative position for inequity aversion), then social preferences types are very well separated from $SPNE$. That is, such a problem only arises for the estimated values. In other words, the estimated altruistic and inequity-averse types are so similar to selfish preferences that they are behaviorally almost indistinguishable from $SPNE$. This will be important for the interpretation of our estimation results.

[30] Table 1.9 in the Appendix A reports the separation values between QRE and all the other models for different values of λ.

1.5 RESULTS

We first present an overview of the results of our experiment and the extent of overall compliance with the different behavioral types. Second, we estimate the distribution of types from the experimental data.

1.5.1 OVERVIEW OF RESULTS

Tables 1.3 and 1.4 provide an overview of the behavior observed in our experiment, while Figures 1.12 and 1.13 in Appendix A present the experimental choices of subjects using histograms. In Tables 1.3 and 1.4, each row–corresponding to one of the 16 CGs–is divided into two parts, one for each player role. The top number in each cell reports the percentage of subjects in a particular role who chose the corresponding column strategy. In each cell, we additionally list all the behavioral models that predict choosing the corresponding strategy for the corresponding player. Again, *QRE* predicts each strategy to be played with a positive probability and we report the *QRE* probabilities for $\lambda = 0.38$. In the tables if, say, 0.01_{QRE} appears in a cell it means that the strategy of the particular player role should be chosen with probability 1% in the *QRE* if $\lambda = 0.38$. Similarly, in case of A(γ) and IA(ρ,σ), the table shows values for $\gamma = 0.22$, $\rho = 0.08$, and $\sigma = 0.55$.

In the increasing CGs (CG $1-8$) the modal choices are concentrated between *Take the second time* or *Take the third time* for both player roles. However, there are a few salient exceptions. In CG 5 and 6, the most frequent choices of Players 1 also include *Take the first time* and in CG 7 and 8, Players 2 also commonly play *Take the first time* for similar reasons. Observe that, in these particular games and player roles, the payoffs exhibit lower increasing rates than the rest. These variations prove to be crucial in separating different behavioral types.

In the constant-sum CGs, the modal behavior of Players 1 is *Take the second time*,

TABLE 1.3: OBSERVED AND PREDICTED BEHAVIOR FOR ALL MODELS ($\gamma = .22$, $\rho = .08$, $\sigma = .55$ AND $\lambda = 0.38$): CG $1-8$.

Games	Player	Take the first time	Take the second time	Take the third time	Always pass
CG 1 (increasing)	1	3.95%	32.89%	40.79%	22.37%
		SPNE, IA A(γ), IA(ρ,σ), 0.01_{QRE}	0.86_{QRE}	L2, L3 0.12_{QRE}	A, L1, PE, O 0.01_{QRE}
	2	18.67%	26.67%	50.67%	4.00%
		SPNE, IA A(γ), IA(ρ,σ), 0.76_{QRE}	L3, P, IA 0.21_{QRE}	L1, L2, PE, O, IA 0.02_{QRE}	A, PE, IA 0.00_{QRE}
CG 2 (increasing)	1	2.63%	34.21%	31.58%	32.58%
		SPNE, IA IA(ρ,σ), 0.00_{QRE}	A(γ), 0.69_{QRE}	L2, L3 0.29_{QRE}	A, PE, L1, O A(γ), 0.02_{QRE}
	2	8.00%	33.33%	52.00%	6.67%
		SPNE, IA A(γ), IA(ρ,σ), 0.00_{QRE}	L3, IA 0.89_{QRE}	L1, L2, PE, O, IA A(γ), 0.11_{QRE}	A, PE, IA 0.01_{QRE}
CG 3 (increasing)	1	15.79%	57.89%	18.42%	7.89%
		SPNE, IA A(γ), IA(ρ,σ), 0.78_{QRE}	L2, L3 0.18_{QRE}	L1 0.06_{QRE}	A, PE, O 0.01_{QRE}
	2	30.67%	49.33%	16.00%	4.00%
		SPNE, L3, IA A(γ), IA(ρ,σ), 0.84_{QRE}	L1, L2, IA 0.11_{QRE}	PE, O, IA 0.03_{QRE}	A, PE, IA 0.02_{QRE}
CG 4 (increasing)	1	9.21%	64.47%	21.05%	5.26%
		SPNE, IA IA(ρ,σ), 0.39_{QRE}	L2, L3 A(γ), 0.45_{QRE}	L1 0.09_{QRE}	A, PE, O 0.07_{QRE}
	2	37.33%	48.00%	13.33%	1.33%
		SPNE, L3, IA A(γ), IA(ρ,σ), 0.90_{QRE}	L1, L2, IA 0.09_{QRE}	PE, O, IA 0.01_{QRE}	A, PE, IA 0.00_{QRE}
CG 5 (increasing)	1	65.79%	14.47%	13.16%	6.58%
		SPNE, L1, L3 A(γ), IA(ρ,σ), 0.96_{QRE}	IA 0.03_{QRE}	L2 0.00_{QRE}	A, PE, O 0.00_{QRE}
	2	20.00%	20.00%	36.00%	24.00%
		SPNE, L2 A(γ), IA(ρ,σ), 0.27_{QRE}	L2, L3, IA IA(ρ,σ), 0.24_{QRE}	L1, L2, PE, O, IA IA(ρ,σ), 0.24_{QRE}	A, L2, PE, IA IA(ρ,σ), 0.24_{QRE}
CG 6 (increasing)	1	51.32%	15.79%	19.74%	13.16%
		SPNE, L1, L3, IA A(γ), IA(ρ,σ), 0.15_{QRE}	0.31_{QRE}	L2 0.41_{QRE}	A, PE, O 0.13_{QRE}
	2	10.67%	34.67%	40.00%	14.67%
		SPNE, L2, IA A(γ), IA(ρ,σ), 0.00_{QRE}	L2, L3, IA IA(ρ,σ), 0.14_{QRE}	L1, L2, PE, O, IA IA(ρ,σ), 0.53_{QRE}	A, L2, PE, IA IA(ρ,σ), 0.32_{QRE}
CG 7 (increasing)	1	15.79%	21.05%	25.00%	38.16%
		SPNE, L2 A(γ), IA(ρ,σ), 0.00_{QRE}	IA 0.25_{QRE}	L3, IA A(γ), 0.38_{QRE}	A, L1, PE, O, IA A(γ), 0.37_{QRE}
	2	57.33%	24.00%	17.33%	1.33%
		SPNE, L1, L3, IA A(γ), IA(ρ,σ), 0.60_{QRE}	L3 0.32_{QRE}	L2, L3, PE, O A(γ), 0.07_{QRE}	A, L3, PE A(γ), 0.01_{QRE}
CG 8 (increasing)	1	53.95%	21.05%	14.47%	10.53%
		SPNE, L2, IA A(γ), IA(ρ,σ), 0.77_{QRE}	0.12_{QRE}	L3 0.05_{QRE}	A, L1, PE, O 0.05_{QRE}
	2	52.00%	25.33%	22.67%	0.00%
		SPNE, L1, L3, IA A(γ), IA(ρ,σ), 0.77_{QRE}	L3, IA 0.13_{QRE}	L2, L3, PE, O, IA 0.08_{QRE}	A, L3, PE, IA 0.02_{QRE}

Notes: THE TABLE REPORTS, FOR EACH STRATEGY (COLUMNS 3-6) IN EACH CG (COLUMN 1) AND EACH PLAYER ROLE (COLUMN 2), (I) THE PROPORTION OF SUBJECTS CHOOSING THE STRATEGY, AND (II) THE BEHAVIORAL MODEL THAT PREDICTS THE STRATEGY TO BE CHOSEN WITH POSITIVE PROBABILITY. FOR QRE, WE LIST THE PROBABILITY WITH WHICH IT PREDICTS EACH STRATEGY.

TABLE 1.4: OBSERVED AND PREDICTED BEHAVIOR FOR ALL MODELS ($\gamma = .22$, $\rho = .08$, $\sigma = .55$ AND $\lambda = 0.38$): CG $9 - 16$.

Games	Player	Take the first time	Take the second time	Take the third time	Always pass
CG 9 (constant)	1	22.37%	59.21%	11.84%	6.58%
		SPNE, A, L2, L3, PE, IA	A, L1, PE	A, PE	A, PE, O
		A(γ), IA(ρ,σ), 0.43_{QRE}	0.38_{QRE}	0.13_{QRE}	0.06_{QRE}
	2	64.00%	26.67%	9.33%	0.00%
		SPNE, A, L1, L2, L3, PE, IA	A, L3, PE, IA	A, L3, PE, O, IA	A, L3, PE, IA
		A(γ), IA(ρ,σ), 0.67_{QRE}	0.20_{QRE}	0.08_{QRE}	0.04_{QRE}
CG 10 (constant)	1	11.84%	67.11%	15.79%	5.26%
		SPNE, A, PE, IA	A, L2, L3, PE	A, L1, PE	A, PE, O
		A(γ), 0.33_{QRE}	0.46_{QRE}	0.16_{QRE}	0.05_{QRE}
	2	29.33%	57.33%	12.00%	1.33%
		SPNE, A, L3, PE, IA	A, L1, L2, PE, IA	A, PE, O, IA	A, PE, IA
		A(γ), IA(ρ,σ), 0.37_{QRE}	0.42_{QRE}	0.13_{QRE}	0.08_{QRE}
CG 11 (decreasing)	1	64.47%	10.53%	15.79%	9.21%
		SPNE, A, L2, L3, PE	L1, PE	PE	PE, O, IA
		A(γ), IA(ρ,σ), 0.43_{QRE}	0.39_{QRE}	0.05_{QRE}	0.12_{QRE}
	2	70.67%	16.00%	10.67%	2.67%
		SPNE, A, L1, L2, L3, PE	L3, PE	L3, PE, O, IA	L3, PE
		A(γ), IA(ρ,σ), 0.42_{QRE}	0.24_{QRE}	0.22_{QRE}	0.13_{QRE}
CG 12 (decreasing)	1	55.26%	32.89%	7.89%	3.95%
		SPNE, A, L2, L3, PE, IA	L1, PE	PE	PE, O
		A(γ), IA(ρ,σ), 0.53_{QRE}	0.29_{QRE}	0.12_{QRE}	0.06_{QRE}
	2	66.67%	24.00%	9.33%	0.00%
		SPNE, A, L1, L2, L3, PE, IA	L3, PE, IA	L3, PE, O, IA	L3, PE, IA
		A(γ), IA(ρ,σ), 0.57_{QRE}	0.23_{QRE}	0.12_{QRE}	0.08_{QRE}
CG 13 (variable)	1	50.00%	17.11%	22.37%	10.53%
		SPNE, PE	L2, L3	L1, IA	A, PE, O, IA
		A(γ), IA(ρ,σ), 0.63_{QRE}	0.24_{QRE}	0.10_{QRE}	0.03_{QRE}
	2	33.33%	44.00%	22.67%	0.00%
		SPNE, L3, PE	L1, L2, IA	PE, O	A, PE
		A(γ), IA(ρ,σ), 0.44_{QRE}	A(γ), 0.32_{QRE}	0.15_{QRE}	0.09_{QRE}
CG 14 (variable)	1	31.58%	39.47%	15.79%	13.16%
		SPNE, PE, IA	L2, L3	L1	A, PE, IA
		A(γ), IA(ρ,σ), 0.50_{QRE}	0.30_{QRE}	0.14_{QRE}	0.07_{QRE}
	2	36.00%	42.67%	18.67%	2.67%
		SPNE, L3, PE, IA	L1, L2, IA	PE, O, IA	A, PE, IA
		A(γ), IA(ρ,σ), 0.48_{QRE}	0.31_{QRE}	0.14_{QRE}	0.08_{QRE}
CG 15 (variable)	1	72.37%	10.53%	14.47%	2.63%
		SPNE, L1, L2, L3	PE, IA	A, PE	PE, O
		A(γ), IA(ρ,σ), 0.94_{QRE}	0.05_{QRE}	0.01_{QRE}	0.00_{QRE}
	2	68.00%	28.00%	2.67%	1.33%
		SPNE, L1, L2, L3	A, L2, L3, PE, IA	L2, L3, PE, O, IA	L2, L3, PE, IA
		A(γ), IA(ρ,σ), 0.36_{QRE}	0.23_{QRE}	0.20_{QRE}	0.20_{QRE}
CG 16 (variable)	1	39.47%	40.79%	10.53%	9.21%
		SPNE, A, L3, PE	L1, L2, PE	IA	PE, O, IA
		A(γ), IA(ρ,σ), 0.38_{QRE}	0.46_{QRE}	0.11_{QRE}	0.05_{QRE}
	2	46.67%	29.33%	24.00%	0.00%
		SPNE, A, L2, L3, PE	L1,L3, PE, IA	PE, O, IA	PE
		A(γ), IA(ρ,σ), 0.67_{QRE}	0.22_{QRE}	0.10_{QRE}	0.07_{QRE}

Notes: THE TABLE REPORTS, FOR EACH STRATEGY (COLUMNS 3-6) IN EACH CG (COLUMN 1) AND EACH PLAYER ROLE (COLUMN 2), (I) THE PROPORTION OF SUBJECTS CHOOSING THE STRATEGY, AND (II) THE BEHAVIORAL MODEL THAT PREDICTS THE STRATEGY TO BE CHOSEN WITH POSITIVE PROBABILITY. FOR QRE, WE LIST THE PROBABILITY WITH WHICH IT PREDICTS EACH STRATEGY.

while the modal strategies of Players 2 consist of *Take the first time* in CG 9 and *Take the second time* in CG 10. In the decreasing-sum CG 11 and 12, both roles mostly select *Take the first time*, although both roles also choose *Take the second time* with non-negligible frequencies in CG 12.

We cannot describe the overall behavior in the variable-sum games well as they differ in important aspects, but the most common choices in these games are *Take the first time* and *Take the second time* for both roles.

Tables 1.3 and 1.4 also illustrate how misleading it can be to identify behavioral types on the basis of a single game. For instance, note that 3.95% of Player 1 subjects take at the first decision node in CG 1. This provides little support for *SPNE* or *IA*, the two theories that predict stopping at the first node. By contrast, the behavior in CG 11 seems to adhere to *SPNE* for both player roles. However, a closer look at the table reveals that this behavior in CG 11 is also consistent with a large number of other behavioral theories. Therefore, our experimental design uses multiple CGs designed to separate the predicted behavior of different behavioral types as much as possible.

1.5.2 Overall Compliance with Behavioral Types

To discriminate across the candidate explanations, we first ask to what extent behavior complies with each behavioral type in absolute terms. Note that we have 151 subjects making choices in 16 different CGs. This results in a total of 2416 decisions (151×16). Table 1.5 lists the compliance rates for each model, on aggregate and disaggregated across the types of games. For instance, 0.38 for $SPNE$ reflects that 38% of the choices (out of the 2416) correspond to actions predicted by $SPNE$ with positive probability and can thus be rationalized by this model. Since all strategies are played with positive probabilities in *QRE*, any behavior is in-line with this prediction. To allow comparison with other types, Table 1.5 only count the number of times that subjects selected strategies with the largest predicted probability by *QRE* conditional on λ.

TABLE 1.5: COMPLIANCE RATES OF ALL MODELS ACROSS DIFFERENT TYPES OF CENTIPEDE GAMES

Behavioral Type	All games	Increasing	Constant	Decreasing	Variable
SPNE	0.38	0.28	0.32	0.64	0.47
A(0.22)	0.47	0.44	0.32	0.64	0.53
IA(0.08,0.55)	0.43	0.39	0.32	0.64	0.47
A	0.37	0.12	1.00	0.80	0.34
IA	0.53	0.61	0.58	0.44	0.40
PE	0.53	0.27	1.00	1.00	0.58
O	0.17	0.24	0.08	0.08	0.13
L1	0.42	0.40	0.49	0.45	0.42
L2	0.50	0.46	0.53	0.64	0.50
L3	0.52	0.46	0.55	0.80	0.48
QRE(0.38)	0.45	0.38	0.53	0.64	0.47

Notes: THE TABLE REPORTS THE FRACTION OF CHOICES IN THE EXPERIMENT COMPLYING WITH EACH BEHAVIORAL MODEL FOR ALL THE CGS (COLUMN 2), AND SEPARATELY FOR THE INCREASING-SUM (COLUMN 3), CONSTANT-SUM (COLUMN 4), DECREASING-SUM (COLUMN 5), AND VARIABLE-SUM CGS (COLUMN 6).

What do we learn from the reported numbers? First, no rule explains the majority of decisions; this points to substantial behavioral heterogeneity. Second, the compliance rates illustrate that many rules could explain large proportion of choices in the experiment. However, a closer look at the compliance rates across different types of CGs in Table 1.5 reveals that some models exhibit considerable variation in compliance across the game types. For example, *SPNE* explains up to 64% of decisions in decreasing-sum games but only 28% in increasing-sum CGs. By contrast, *L1*'s compliance varies little across the different types of CGs. This shows that some behavioral types may "appear" highly relevant if one focuses only on one game or even on one type of game. Hence, careful selection of games is crucial if behavior in CGs is to be explained successfully.

The information in Table 1.5 should be interpreted with care. First, a decision may be compatible with several behavioral types (i.e. the proportions do not add up to one). That is, the candidate behavioral types do not compete against each other when compliance rates are calculated. Second and more importantly, these compliance measures impose no restriction on the consistency of each behavioral explanations within subjects. In this table, an individual could comply with one behavioral model in a certain number of CGs and with another in the rest. Lastly, rules that frequently predict more than one option (e.g. IA or PE; see Table 1.1) obtain higher compliance scores. These issues are absent in the mixture-of-types econometric approach introduced in the next section.

1.5.3 ESTIMATION FRAMEWORK

Our design enables us to use individual behavior across the 16 CGs to identify the behavioral type behind each subject's revealed choices. Table 1.13 (in the Appendix A) quantifies how many experimental subjects behave consistently with each behavioral type, for different minimum numbers of CGs in which they are required to comply. We observe that the choices of some subjects across the 16 CGs fully reveal their type. In particular, the decisions of 69 out of our 151 subjects (46%) comply with some behavioral type considered in at least 10 (out of 16) games. Disregarding $A(\gamma)$ and $IA(\rho, \sigma)$, 67 of these subjects could potentially be classified without relying on mixture-model techniques: 25 of them best comply with *SPNE*, 1 with *A*, 3 with *O*, 22 with *L1*, 11 with *L2*, and 5 with *L3*. However, two of these 69 subjects best comply with both *L2* and *L3* simultaneously. Moreover, for reasons explained in Section 1.4.3, almost all the subjects best complying with $SPNE$ are equally compatible with $A(\gamma)$ and $IA(\rho, \sigma)$.[31] Last, the remaining 82 out of our 151 subjects are not classifiable that easily and the actual estimation method is required.

Unlike other approaches, finite mixture-of-types models estimate the distribution of behavioral types in the population, requiring consistency of behavior within subjects and making the candidate models compete with each other.[32] Below, we first describe

[31] These examples illustrate why the numbers reported in Table 1.13 are generally higher than those mentioned here. Some subjects could equally comply with more than one model (not necessarily in the same games) and less separated behavioral models tend to include the same subjects, while here we only refer to the model that best explains the behavior of each individual.

[32] Our approach closely follows that of Stahl and Wilson (1994, 1995), Harless and Camerer (1994), El-Gamal and Grether (1995), Costa-Gomes et al. (2001), Camerer et al. (2004), Costa-Gomes and Crawford

in detail the maximum likelihood function and then present the estimation results. Readers familiar with mixture-of-types models may prefer to skip the next section and go directly to the estimation results.

MAXIMUM LIKELIHOOD UNDER UNIFORM AND SPIKE-LOGIT ERROR SPECIFICATIONS

Let i index the subjects in the experiment, $i = 1, ..., 151$, k the behavioral types considered, $k = 1, 2, ...K$, and g the CG from a set $G = \{1, 2, ..., 16\}$. In each g, each subject has four available strategies $j = 1, 2, 3, 4$. We assume that individuals comply with their types but make errors. We will present two sets of results, one in which we consider the extreme non-parameterized social-preference types A and IA and one in which we instead use the more flexible parameterized models $A(\gamma)$ and $IA(\rho, \sigma)$. In the latter case, the additional parameters are estimated jointly with the other parameters of our mixture models. For each of these alternatives, we estimate two model variations, differing in our assumptions regarding the underlying error structure.

Uniform errors. Under our benchmark specification, we assume that a subject employing rule k makes type-k's decision with probability $(1 - \varepsilon_k)$, but with probability $\varepsilon_k \in [0, 1]$ she makes a mistake. In such a case, she plays each strategy uniformly at random from the four available strategies. As in most mixture-of-types model applications, we assume that errors are identically and independently distributed across games and subjects and that they are type-specific.[33] The first assumption facilitates the statistical treatment of the data, while the second considers that some types may be more cognitively demanding and thus lead to larger error rates than others.

The likelihood of a particular individual of a particular type can be constructed as follows. Let $P_k^{g,j}$ be type-k's predicted choice probability for strategy j in game g.

(2006), and Crawford and Iriberri (2007a,b).

[33] See e.g. El-Gamal and Grether (1995), Crawford et al. (2001), Iriberri and Rey-Biel (2013), or Kovarik et al. (2018) among many others.

Some rules may predict more than one strategy in a CG. This is reflected in the vector $P_k^g = (P_k^{g,1}, P_k^{g,2}, P_k^{g,3}, P_k^{g,4})$ with $\sum_j P_k^{g,j} = 1$.[34] The probability that an individual i will choose a particular strategy j if she is of type $k \neq QRE$ is

$$(1-\varepsilon_k)P_k^{g,j} + \frac{\varepsilon_k}{4}.$$

Note that, since $P_k^{g,j} > 0$ for strategies predicted by k while $P_k^{g,j} = 0$ otherwise, the probability of choosing one particular strategy inconsistent with rule $k \neq QRE$ is $\frac{\varepsilon_k}{4}$.

For each individual in each game, we observe the choice and whether it is or not consistent with k. Let $x_i^{g,j} = 1$ if action j is chosen by i in game g in the experiment and $x_i^{g,j} = 0$ otherwise. The likelihood of observing a sample $x_i = (x_i^{g,j})_{g,j}$ given type *k* and subject *i* is then

$$L_i^k(\varepsilon_k|x_i) = \prod_g \prod_j \left[(1-\varepsilon_k)P_k^{g,j} + \frac{\varepsilon_k}{4}\right]^{x_i^{g,j}}. \tag{1.4}$$

In the variation of the model, in which–instead of A and IA–we apply $A(\gamma)$ and $IA(\rho,\sigma)$, the predicted choice probabilities depend on the parameters of each model and we write $P_A^g(\gamma)$ and $P_{IA}^g(\rho,\sigma)$, respectively. The expression (1.4) for $A(\gamma)$ and $IA(\rho,\sigma)$ then becomes:

$$L_i^k(\varepsilon_k,\theta|x_i) = \prod_g \prod_j \left[(1-\varepsilon_k)P_k^{g,j}(\theta) + \frac{\varepsilon_k}{4}\right]^{x_i^{g,j}}, \tag{1.5}$$

where $\theta = \gamma$ for $A(\gamma)$ and $\theta = (\rho,\sigma)$ for $IA(\rho,\sigma)$.

[34]The particular probabilities for each type considered here are listed in Tables 1.3 and 1.4 (and displayed visually in Figures 1.9 and 1.10 in the Appendix A). As an example, $P_{SPNE}^g = (1,0,0,0)$ for each g. That is, *SPNE* stops with probability one at the first decision node of each CG. Note that for the remaining models, the predictions are not symmetric across the player roles so we should also specify P for different player roles. Since the notation is already cumbersome in the current form, we omit the dependency of $P_k^{g,j}$ on player role in the presentation of the model.

Matters are different for QRE. In this case, $P_k^g = P_k^g(\lambda)$. Hence,

$$L_i^{QRE}(\lambda|x_i) = \prod_g \prod_j \left(P_{QRE}^{g,j}(\lambda)\right)^{x_i^{g,j}}, \tag{1.6}$$

where λ is a free parameter to estimate, inversely related to the level of error, and $P_{QRE}^g(\lambda) = \left[P_{QRE}^{g,1}(\lambda), P_{QRE}^{g,2}(\lambda), P_{QRE}^{g,3}(\lambda), P_{QRE}^{g,4}(\lambda)\right]$ are the *QRE* probabilities of each action in game g. Abusing slightly the notation, denote by $P_{QRE}^g(\lambda)$ a (mixed) strategy profile in game g and let $\pi^g(j, P_{QRE}^g(\lambda))$ be the expected payoff from choosing j in game g against the profile $P_{QRE}^g(\lambda)$. We follow the literature and work with the logistic *QRE* specification. Thus, the vector $P_{QRE}^g(\lambda)$ in each game is the solution to the following set of four equations per player role: for $j = 1, 2, 3, 4$,

$$P_{QRE}^{g,j}(\lambda) = \frac{\exp\left[\lambda \pi^g(j, P_{QRE}^g(\lambda))\right]}{\sum_l \exp\left[\lambda \pi^g(l, P_{QRE}^g(\lambda))\right]}. \tag{1.7}$$

As mentioned above, one might think that behavioral models that predict more than one strategy may be artificially favored by appearing more successful. However, this is not the case with our likelihood specifications, since models predicting more actions are punished in (1.4), (1.5), and (1.6) through lower $P_k^{g,j}$. Consequently, whenever someone takes a strategy predicted by these models, it is taken as evidence for them to a lower extent compared to models that generate more precise predictions.

Adding up for all k (including *QRE*) and i, and assigning probabilities $p = (p_1, p_2, ..., p_K)$ to each k yields one log-likelihood function of the whole sample:

$$\ln L(p, (\varepsilon_k)_{k \neq QRE}, \lambda|x) = \sum_i \ln\left[\sum_{k \neq QRE} p_k L_i^k(\varepsilon_k|x_i^{g,j}) + p_{QRE} L_i^{QRE}(\lambda|x_i^{g,j})\right]. \tag{1.8}$$

In case of $A(\gamma)$ and $IA(\rho,\sigma)$, the log-likelihood (1.8) changes to

$$\ln L(p,(\varepsilon_k)_{k\neq QRE},\gamma,\rho,\sigma,\lambda|x)=\sum\nolimits_i \ln\left[\sum\nolimits_k p_k L_i^k(.)\right]. \tag{1.9}$$

Spike-logit errors. Observe that, by construction, *QRE* is treated differently in (1.8) and (1.9) from other rules. Nevertheless, the logistic-error structure can also be specified for the error of any behavioral model, except those rules that do not involve any type of optimization. This only concerns *PE* in our case so we drop this type for this particular specification.[35] Hence, in our alternative specification we use a spike-logit error structure, in which we also assume that a subject employing rule k makes type-k's decision with probability $(1-\varepsilon_k)$ and err with probability $\varepsilon_k \in [0,1]$. If people make a mistake, we assume that they only play with positive probabilities strategies *not* predicted by the rule and these probabilities follow a logistic distribution. The probabilities of selecting such type-inconsistent strategies scale up with their payoffs or utilities for most behavioral types (as for QRE), they scale up with the sum of payoffs for A, or scale down with the absolute value of the difference between the payoffs of the two players for IA, given the corresponding type's beliefs about others' behavior. Moreover, this alternative error specification requires the estimation of one additional parameter λ_k for each $k \neq QRE$. Similarly to QRE, these parameters measure how sensitive the probability to choose a strategy inconsistent with a rule $k \neq QRE$ is to their goal (i.e. payoff, utility, sum of payoffs, or generated payoff difference).

Formally, denote by $\pi_i^{g,k}(j)$ the payoff of individual i who employs type $k \neq QRE$, A, IA, $A(\gamma)$, $IA(\rho,\sigma)$, who selects strategy j in game g, and who holds type k's beliefs about the behavior of opponents.[36] Let $j^{'}_{g,k}=\{j|P_k^{g,j}=0\}$ be the subset of strategies that are *not* predicted by a type k in game g. Define these concepts for A, IA, $A(\gamma)$, and $IA(\rho,\sigma)$ analogously.

[35] As shown below, we find no evidence for *PE* anyway, so our results are not affected by its elimination.

[36] *SPNE* believes her opponents are also *SPNE*, but *Lk*, for instance, believe her opponents are *Lk-1*. The beliefs of all behavioral types are described in Section 1.3.2.

Thus, i's likelihood of being of type $k \neq QRE$ is

$$L_i^k(\varepsilon_k, \lambda | x_i) = \prod_g \prod_j \left[(1-\varepsilon_k)P_k^{g,j} + \varepsilon_k V_k^{g,j}(\lambda_k)\right]^{x_i^{g,j}}, \tag{1.10}$$

where for any $j \in j'_{g,k}$

$$V_k^{g,j}(\lambda_k) = \frac{\exp[\lambda_k \pi_i^{g,k}(j)]}{\sum_{j'_{g,k}} \exp[\lambda_k \pi_i^{g,k}(j)]} \tag{1.11}$$

and $V_k^{g,j}(\lambda_k) = 0$ for $j \notin j'_{g,k}$. As mentioned above, λ_k's , $k \neq QRE$, are free parameters to estimate.

In the variation of the model with $A(\gamma)$ and $IA(\rho, \sigma)$ (instead of A and IA), the predicted choice probabilities depend on the parameters of each model and we write $P_k^g(\theta)$ being $\theta = \gamma$ and $\theta = (\rho, \sigma)$, respectively. The expression (1.10) then becomes:

$$L_i^k(\varepsilon_k, \theta, \lambda_k | x_i) = \prod_g \prod_j \left[(1-\varepsilon_k)P_k^{g,j}(\theta) + \varepsilon_k V_k^{g,j}(\lambda_k)\right]^{x_i^{g,j}}, \tag{1.12}$$

The *QRE* probabilities are defined as in (1.6) and (1.7) and, by analogy, the log-likelihood of the whole sample under A and IA is

$$\ln L(p, (\varepsilon_k)_{k \neq QRE}, \lambda | x) = \sum_i \ln \left[\sum_{k \neq QRE} p_k L_i^k(\varepsilon_k, \lambda_k | x_i^{g,j}) + p_{QRE} L_i^{QRE}(\lambda_{QRE} | x_i^{g,j})\right]. \tag{1.13}$$

In case of $A(\gamma)$ and $IA(\rho, \sigma)$, the log-likelihood (1.13) changes to

$$\ln L(p, (\varepsilon_k)_{k \neq QRE}, \gamma, \rho, \sigma, \lambda | x) = \sum_i \ln \left[\sum p_k L_i^k(.)\right]. \tag{1.14}$$

Estimation Results

We estimate two sets of parameters: frequency of behavioral types within the subject population $p = (p_1, p_2, ..., p_K)$ and the error-related parameters, one or two for each behavioral type depending on the error specification. Under uniform errors, the error-related parameters are ε_k for $k \neq QRE$ and the inverse error rate λ for QRE. In contrast, under the spike-logit specification, there are two error-related parameters per model, with the exception of QRE. More precisely, ε_k for $k \neq QRE$ and a vector $\lambda = (\lambda_1, ..., \lambda_K)$ that includes λ_{QRE}. Last, if $A(\gamma)$ and $IA(\rho, \sigma)$ are applied, we also estimate their parameters. Under mild conditions satisfied by the functions (1.8), (1.9), (1.13), and (1.14), the maximum likelihood estimation produces consistent estimates of the parameters (Leroux, 1992).

Tables 6A and 6B present the estimation results for both error specifications. Table 6A corresponds to estimations with A and IA; Table 6B to those with the parameterized $A(\gamma)$ and $IA(\rho, \sigma)$. Columns $(1-4)$ in Table 6A and columns $(1-6)$ in Table 6B contain the uniform-error specification estimates, while columns $(5-10)$ and $(7-14)$, respectively, show those for spike-logit errors. Standard errors shown in parentheses below each estimate and the corresponding significance levels (**$p > 0.01$, *$p > 0.05$) were computed using bootstrapping with 100 replications (Efron and Tibshirani, 1994). For the frequency parameters p_k, the inverse error rates λ_{QRE}, and for parameters γ, ρ, and σ, we simply report their significance levels. However, the error rates are well behaved if they are close to zero and far from one. Therefore, we test whether each ε_k differs significantly from one (rather than zero). The standard errors and significance levels reported jointly with the estimated ε_k's in Tables 6A and 6B correspond to these tests.[37]

There are several differences between the uniform and spike-logit error specifications. They mainly differ in how they treat choices inconsistent with a type k. The former treats all mistakes in the same way, while the latter punishes more costly mistakes more than less costly ones. The consequence of the payoff-dependent errors is

[37] We perform no statistical tests for the λ_k's corresponding to the models different from QRE in the spike-logit specification because these only tell how sensitive the mistakes are to each type's goal, but they are irrelevant for telling with which probability people make mistakes. These probabilities are determined by the estimated ε_k.

that the spike-logit specification uses more information, since it regards the payoffs.[38] Rather than favoring one error specification over the other, we let readers decide, in which assumptions they wish to place more confidence.

TABLE 6A: ESTIMATION RESULTS I: NON-PARAMETERIZED SPECIFICATION FOR SOCIAL PREFERENCES

	Uniform Error				Spike-Logit Error					
	Full		Selected		Full			Selected		
Type	p_k	ε_k, λ	p_k	ε_k, λ	p_k	ε_k	λ_k	p_k	ε_k	λ_k
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
SPNE	0.09**	0.32**	0.08**	0.31**	0.10**	0.27**	1.00	0.11**	0.29**	0.98
	(0.03)	(0.09)	(0.03)	(0.08)	(0.03)	(0.05)	(0.02)	(0.04)	(0.06)	(0.08)
A	0.02	0.38**			0.01	0.19**	0.02			
	(0.01)	(0.19)			(0.01)	(0.04)	(0.20)			
IA	0.03**	1.00			0.02	0.80**	0.00			
	(0.01)	(0.20)			(0.01)	(0.06)	(0.05)			
PE	0.00	0.75*								
	(0.02)	(0.15)								
O	0.03*	0.06**	0.03*	0.06**	0.03*	0.05**	0.08	0.03*	0.05**	0.08
	(0.01)	(0.11)	(0.01)	(0.17)	(0.01)	(0.08)	(0.14)	(0.01)	(0.07)	(0.14)
L1	0.29**	0.59**	0.31**	0.60**	0.41**	0.49**	0.01	0.43**	0.51**	0.01
	(0.05)	(0.04)	(0.05)	(0.05)	(0.07)	(0.03)	(0.05)	(0.07)	(0.03)	(0.06)
L2	0.18**	0.61**	0.21**	0.66**	0.08**	0.36**	0.19	0.08**	0.36**	0.19
	(0.06)	(0.06)	(0.06)	(0.04)	(0.02)	(0.04)	(0.08)	(0.03)	(0.04)	(0.08)
L3	0.10*	0.59**	0.11*	0.62**	0.06*	0.43**	0.01	0.06*	0.44**	0.00
	(0.04)	(0.08)	(0.04)	(0.07)	(0.04)	(0.12)	(0.02)	(0.04)	(0.12)	(0.01)
QRE	0.28**	0.38**	0.27**	0.42**	0.29**		0.39**	0.29**		0.37**
	(0.05)	(0.12)	(0.05)	(0.15)	(0.05)		(0.09)	(0.06)		(0.09)

Notes: THE TABLE REPORTS THE ESTIMATION RESULTS FOR THE UNIFORM ERROR SPECIFICATION IN COLUMNS (1-4) AND THE SPIKE-LOGIT ERROR SPECIFICATION IN COLUMNS (5-10). COLUMNS (1), (3), (5), AND (8) PRESENT THE ESTIMATED FREQUENCIES OF EACH BEHAVIORAL MODEL; COLUMNS (2), (4), (6), (7), (9), AND (10) PRESENT THE ESTIMATED ERROR-RELATED PARAMETERS. FOR EACH ERROR SPECIFICATION, WE REPORT BOTH THE FULL AND THE SELECTED MODEL. THE FULL MODEL INCLUDES ALL CONSIDERED BEHAVIORAL TYPES; THE SELECTED MODELS ONLY INCLUDE THE TYPES THAT SATISFY THE FOLLOWING: (I) THE ESTIMATED FREQUENCY IS SIGNIFICANTLY DIFFERENT FROM 0 AND (II) THE ESTIMATED ERROR RATE IS SIGNIFICANTLY DIFFERENT FROM 1 (FOR QRE, THE ESTIMATED λ IS DIFFERENT FROM 0).

We first discuss in detail the results from Table 6A. First, consider the models that include all the behavioral types introduced in Section 1.3.2, shown in columns $(1-2)$ and $(5-7)$ in Table 6A. Observe that both error specifications yield qualitatively similar results. First, non-strategic and preference-based behavioral models (*A*, *IA*, *PE*, *O*) all explain less than 5%. *IA* and *PE* additionally exhibit very high error rates. The *O* type is an exception in that, despite explaining only 3% of the population, its estimated

[38] Another potential difference might arise due to the joint estimation of λ with the other parameters if the estimated λ differs across the two models. The value of λ affects the degree of separability between *QRE* and the other candidates (see Table 1.9) and different separability may effect the estimated type frequencies. Since the estimated λ's are very similar in all our estimations, this concern does not apply here.

fractions are significant and the error rates very low. Moreover, its estimates are highly robust to the model specification. Second, *SPNE* explains the behavior of about 10% of subjects well, consistently across the two models, and *SPNE*'s estimated error rates are actually among the lowest. Third, the rest of the behavior–in fact, most of the behavior of subjects inconsistent with *SPNE*–is best explained by level-*k* thinking and *QRE*. Under both error specifications, level-*k* thinking is estimated to represent around 55% of the subject population, while *QRE* represents about 30%. Level-*k* thinking model also shows a familiar pattern compared to other estimation results with most subjects concentrated in *L1*, followed by *L2* and *L3* (see Crawford et al., 2013). However, the main difference between both error specifications comes from the proportions of each level. Under uniform errors, around half of the population best explained by level-*k* is classified as *L1*, while $L1$ absorbs half of the shares of *L2* and *L3* in the spike-logit specification.

Given that several models systematically fail to explain the behavior in our experiment, we estimate reduced models with some selected behavioral types. One widely debated and so far unresolved issue is the over-parameterization of mixture-of-types models and the related model selection (MacLachlan and Peel, 2000; Cameron and Trivendi, 2005, 2010).[39] Cameron and Trivendi (2005) propose using the natural interpretation of the parameters and MacLachlan and Peel (2000) argue that the best model minimizes the number of components selected if their proportions differ and all are different from zero. We take an approach that combines these recommendations. We require two conditions to hold for a type k to be included in our reduced model: $p_k >> 0$ *and* $\varepsilon_k << 1$ ($\lambda_k >> 0$ for QRE). In words, we require the share of each type k selected to be high enough and its error rate low enough (the inverse error rate high enough for QRE) to suggest that the decisions of people classified as k are made on

[39]Standard criteria for model selection (such as Akaike or Bayesian Information criteria or the likelihood ratio test) may perform unsatisfactorily in finite mixture models (Cameron and Trivendi, 2005, 2010).

purpose rather than by error.[40]

For both specifications, we eliminate those rules with negligible shares or high error rates. In particular, we estimate a reduced form of both (1.8) and (1.13), in which we only include the relevant behavioral types *O*, *QRE*, level-k, and *SPNE*. Columns $(3-4)$ and $(8-10)$ in Table 6A report the results. The estimates of the selected behavioral types are stable as we move from the full to the reduced model. Moreover, all the parameters estimated are well behaved. Under the uniform specification, the types excluded are entirely absorbed by $L1$ and $L2$. As a result, their error rates slightly increase. The estimates suggest that the composition of the population is 3% of *O*, 8% of *SPNE*, 27% of *QRE*, and 63% of level-k. Under the spike-logit specification, the types excluded are entirely absorbed by $L1$ and $SPNE$ and their error rates thus slightly increase. The estimates suggest that the composition of the population is 3% of *O*, 11% of *SPNE*, 29% of *QRE*, and 57% of level-k reasoning, figures very similar to the uniform-error specification.[41]

Let us now turn the attention to Table 6B, in which we consider the flexible models of social preferences $A(\gamma)$ and $IA(\rho, \sigma)$. In Table 6B, the uniform-error specification is positioned in the top of the table while the spike-logit specification in the bottom. Observe that the estimates of each estimation are displayed in three columns; columns $(1-3)$ and $(7-10)$ correspond to the full models and columns $(4-6)$ and $(11-14)$ to the selected ones. In line with Table 6A, the estimates are robust to the error specification and the elimination of rules from the full model. Most importantly, the majority of

[40] This approach follows Kovarik et al. (2018) who propose a related model-selection algorithm and, using Monte-Carlo simulations, show that such an algorithm successfully recovers the data-generating processes as long as the error rates are not too high.

[41] It might be thought that our comparison between *QRE* and level-*k* could favor the latter, as level-k allows for multiple types while we estimate a single *QRE*. Therefore, we re-estimate our uniform model with two *QRE* types (with different λ's). The estimation results are shown in Table 1.14 in the Appendix A. Compared to the original $p_{QRE} = 0.27$ and $\lambda_{QRE} = 0.42$, the introduction of another QRE type leads to $p_{QRE1} = 0.22$ with $\lambda_{QRE1} = 0.38$ and $p_{QRE2} = 0.06$ with $\lambda_{QRE2} = 0.32$, while the estimated frequencies of the rest of the types being virtually unaffected. Therefore, the proportion of people classified in each rule is unaffected by considering one or two *QRE* types and our benchmark model does not seem to favor level-*k* over *QRE*.

TABLE 6B: ESTIMATION RESULTS II: PARAMETERIZED SPECIFICATION FOR SOCIAL PREFERENCES

Uniform Error

Type	Full			Selected		
	p_k	ε_k, λ	γ, ρ, σ	p_k	ε_k, λ	γ, ρ, σ
	(1)	(2)	(3)	(4)	(5)	(6)
SPNE	0.01	0.00**				
	(0.01)	(0.15)				
A	0.08**	0.38**	0.21**	0.08**	0.38**	0.22**
	(0.03)	(0.07)	(0.04)	(0.03)	(0.07)	(0.04)
IA	0.06**	0.20**	0.07**,0.55**	0.06**	0.18**	0.08**,0.55**
	(0.02)	(0.03)	(0.02),(0.12)	(0.03)	(0.04)	(0.02),(0.13)
PE	0.01	0.45**				
	(0.02)	(0.23)				
O	0.03*	0.06**		0.03*	0.06**	
	(0.01)	(0.07)		(0.01)	(0.10)	
L1	0.28**	0.59**		0.29**	0.59**	
	(0.04)	(0.04)		(0.05)	(0.04)	
L2	0.20**	0.65**		0.21**	0.67**	
	(0.06)	(0.08)		(0.05)	(0.07)	
L3	0.08*	0.63**		0.08*	0.62**	
	(0.04)	(0.13)		(0.04)	(0.14)	
QRE	0.24**	0.37**		0.24**	0.38**	
	(0.04)	(0.09)		(0.05)	(0.09)	

Spike-Logit Error

Type	Full				Selected			
	p_k	ε_k	λ_k	γ, ρ, σ	p_k	ε_k	λ_k	γ, ρ, σ
	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
SPNE	0.06**	0.93**	0.49		0.06**	0.93**	0.34	
	(0.01)	(0.03)	(0.03)		(0.01)	(0.04)	(0.03)	
A	0.10**	0.39**	0.01	0.29**	0.10**	0.11**	0.00	0.19**
	(0.01)	(0.01)	(0.01)	(0.00)	(0.02)	(0.05)	(0.10)	(0.01)
IA	0.06**	0.12**	0.51	0.02,0.76**	0.06**	0.11**	0.53	0.02,0.80**
	(0.01)	(0.01)	(0.02)	(0.00),(0.00)	(0.00)	(0.01)	(0.04)	(0.01),(0.00)
O	0.03**	0.05**	0.08		0.03**	0.05**	0.08	
	(0.01)	(0.04)	(0.02)		(0.01)	(0.02)	0.04	
L1	0.37**	0.47**	0.01		0.37**	0.47**	0.01	
	(0.01)	(0.02)	(0.01)		(0.08)	(0.02)	(0.05)	
L2	0.09**	0.37**	0.14		0.09**	0.37**	0.14	
	(0.01)	(0.02)	(0.03)		(0.00)	(0.03)	(0.03)	
L3	0.00	0.63**	0.10**					
	(0.00)	(0.03)	(0.03)					
QRE	0.29**		0.34**		0.29**		0.34**	
	(0.02)		(0.06)		(0.06)		(0.03)	

Notes: THE TABLE REPORTS THE ESTIMATION RESULTS FOR THE UNIFORM ERROR SPECIFICATION IN COLUMNS (1-6) AND THE SPIKE-LOGIT ERROR SPECIFICATION IN COLUMNS (7-14). COLUMNS (1), (4), (7), AND (11) PRESENT THE ESTIMATED FREQUENCIES OF EACH BEHAVIORAL MODEL; COLUMNS (2), (5), (8-9), AND (12-13) PRESENT THE ESTIMATED ERROR-RELATED PARAMETERS; COLUMNS (3), (6), (10), AND (14) PRESENT THE PARAMETERS ESTIMATED FOR $A(\gamma)$ AND $IA(\rho, \sigma)$. FOR EACH ERROR SPECIFICATION, WE REPORT BOTH THE FULL AND THE SELECTED MODEL. THE FULL MODEL INCLUDES ALL CONSIDERED BEHAVIORAL TYPES; THE SELECTED MODELS ONLY INCLUDE THE TYPES THAT SATISFY THE FOLLOWING: (I) THE ESTIMATED FREQUENCY IS SIGNIFICANTLY DIFFERENT FROM 0 AND (II) THE ESTIMATED ERROR RATE IS SIGNIFICANTLY DIFFERENT FROM 1 (FOR QRE, THE ESTIMATED λ IS DIFFERENT FROM 0).

non-equilibrium behavior is still explained by level-k and QRE and their estimated parameters are virtually unaffected.

The main difference between Tables 6A and 6B concerns $SPNE$ and the social-preference types. We find no evidence for A and IA in Table 6A, whereas 14% of subjects are classified as either $A(\gamma)$ or $IA(\rho, \sigma)$ and their error rates are well behaved in Table 6B. We can observe that these shares come at the cost of $SPNE$, which receives no support in the full model and is, therefore, eliminated in the selected one. However, a closer look at the estimated γ, ρ, and σ of subjects classified into these social-preference types reveals the reason: they exhibit almost no social concerns and their behavior matches closely that of $SPNE$. As shown in Tables 1.2, 1.3, and 1.4, $SPNE$ and $IA(\rho = 0.08, \sigma = 0.55)$ predict exactly the same strategy in 30 decisions (out of 32) whereas $SPNE$ and $A(\gamma = 0.22)$ in 26 of them. Moreover, in the remaining cases (with one exception) both $A(\gamma = 0.22)$ and $IA(\rho = 0.08, \sigma = 0.55)$ predict multiple actions, one of which is typically the same as the one prescribed by $SPNE$. That is, even if $A(\gamma)$ and $IA(\rho, \sigma)$ could theoretically be well separated from $SPNE$ (simply by being truly non-selfish; see Tables 1.10 and 1.11 in the Appendix A), the actually estimated altruistic and inequity-averse types become behaviorally almost indistinguishable in order to explain about 14% of the population. They thus account for a very small part of non-equilibrium choices in our data. Most importantly though, since these social-preference types only compete for space with $SPNE$ and never with the other non-$SPNE$ theories, the conclusions that most non-equilibrium choices can be explained by the failure of common knowledge of rationality (level-*k*) and bounded rationality (QRE) and preference-based arguments play at most a negligible role in explaining non-equilibrium play in CGs still hold even if we allow for more flexible social-preference types.

An issue linked naturally to the objectives of finite-mixture modeling is whether the estimations generate a well separated, non-overlapping classification of subjects at the individual level. In particular, a desirable property of type mixtures is that individuals are ex-post classified to one and only one of the candidate types, rather than, say, 50% *QRE* and 50% level-*k*. Therefore, we compute posterior probabilities of each individual belonging to a certain type (see MacLachlan and Peel, 2000). Given that our two specifications in Tables 6A and Tables 6B deliver qualitatively and quantitatively similar results, this exercise is only performed for the selected model under uniform errors in Table 6A. If people are accurately classified at the individual level then those posterior probabilities are close to one for a single behavioral model and close to zero for the remaining ones for each individual. This is indeed the case here (see Figure 1.14) so we conclude that our classification is also successful at the individual level. As a result, two departures from *SPNE* seem to be crucial for non-equilibrium behavior in CGs and, as shown by this posterior-probabilities exercise, each model is relevant for different individuals.

ROBUSTNESS 1: GANG OF FOUR

The previous subsection shows that non-equilibrium behavior in CGs is explained by both QRE and level-k as representations of bounded rationality and the failure of common knowledge of rationality, respectively. Due to its prominence in the early literature on CGs, this section analyzes whether these conclusions are robust to considering the “gang of four" model (GoF, hereafter). Similarly to level-k, GoF relaxes the assumption of common knowledge of rationality but, as opposed to level-k, it is an equilibrium approach. In this model, there are two types of players, strategic types and non-strategic types, and the type distribution is common knowledge. However, players have incomplete information about the type of their opponent. McKelvey and Palfrey (1992) propose such model to rationalize individual behavior in their exponentially-increasing CG. In particular, they allow for the existence of a type who always passes (rationalized as an altruist in their paper), such that there is a fraction

(*1-q*) of such non-strategic types and a fraction q of the strategic individuals in the population. This constitutes an incomplete-information game and one can compute the Bayes-Nash equilibria of any game in function of q. If $q = 1$, $GoF = SPNE$ in our framework; if $q = 0$, the strategic type best responses to the non-strategic one. We make the original model more flexible by assuming that the non-strategic type is an altruist, A, who maximizes the sum of payoffs between the two players. Assuming A rather than an "Always Pass" type as in McKelvey and Palfrey (1992) enables such a type to react to differing incentives to take or pass across our CGs and thus gives a better chance to GoF to explain the behavior of at least some subjects in our data.

Observe that including GoF among our candidate models requires to estimate q jointly with other parameters of the model. In our fitting exercise, we interpret q as a–not necessarily correct–belief an individual following GoF holds regarding the population frequency of the strategic types. Remember that McKelvey and Palfrey (1992) find that experimental subjects' behavior can be rationalized with a $q = 0.95$.

We first analyze the separation rate between GoF and the other candidate models. This is shown in Table 1.15. Among the relevant explanations, there might exist separation problems between GoF on one side and $L1$ and $L2$ on the other for certain values of q. No issues arise with $L3$. Naturally, if $q = 1$, $GoF = SPNE$ but both models are well separated for any $q < 1$ in our CGs. Last, it may potentially resemble the estimated QRE if $q = 1$ but the separation for the estimated λ is good otherwise. In sum, if GoF is relevant in our data it will most likely compete with *L1* and *L2* and might thus impact their estimated shares but should not alter the frequencies of other models.[42]

The estimation results shown in Table 1.7 confirm this conjecture. Compared to the uniform-error estimations in Table 6A, the estimated shares of all models but level-k

[42] If GoF competes with $SPNE$, it would mean that the estimated q is so close to 1 that people classified as GoF are rather $SPNE$. In Table 1.7, q is always significantly different from 1.

TABLE 1.7: ESTIMATION RESULTS INCLUDING GANG OF FOUR

	Uniform Error								
	Full			Selected 1			Selected 2		
Type	p_k	ε_k, λ	q	p_k	ε_k, λ	q	p_k	ε_k, λ	q
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
SPNE	0.09**	0.32**		0.08**	0.32**		0.08**	0.32**	
	(0.04)	(0.10)		(0.03)	(0.08)		(0.03)	(0.08)	
A	0.01	0.36**							
	(0.01)	(0.16)							
IA	0.00	0.99							
	(0.01)	(0.01)							
PE	0.03*	1.00							
	(0.02)	(0.02)							
O	0.02*	0.00**		0.02	0.00**				
	(0.01)	(0.06)		(0.01)	(0.09)				
L1	0.23**	0.56**		0.23**	0.56**		0.24**	0.57**	
	(0.04)	(0.04)		(0.05)	(0.06)		(0.05)	(0.05)	
L2	0.15**	0.61**		0.19**	0.67**		0.18**	0.66**	
	(0.05)	(0.06)		(0.06)	(0.09)		(0.06)	(0.09)	
L3	0.10**	0.59**		0.10*	0.61**		0.11*	0.61**	
	(0.04)	(0.05)		(0.05)	(0.09)		(0.05)	(0.08)	
GoF	0.08*	0.51**	0.37**	0.10*	0.54**	0.37**	0.11*	0.57**	0.32**
	(0.04)	(0.08)	(0.05)	(0.05)	(0.08)	(0.09)	(0.05)	(0.08)	(0.08)
QRE	0.28**	0.38**		0.28**	0.38**		0.28**	0.38**	
	(0.05)	(0.10)		(0.05)	(0.12)		(0.05)	(0.11)	

Notes: THE TABLE REPORTS ESTIMATION RESULTS FOR UNIFORM ERROR SPECIFICATIONS. COLUMNS (1), (4), AND (7) PRESENT THE ESTIMATED FREQUENCIES OF EACH BEHAVIORAL MODEL; COLUMNS (2), (5), AND (8) PRESENT THE ESTIMATED ERROR-RELATED PARAMETERS; COLUMNS (3), (6), AND (9) PRESENT THE ESTIMATED PARAMETER q FOR GoF. THE FULL MODEL INCLUDES ALL CONSIDERED BEHAVIORAL TYPES; THE SELECTED MODELS ONLY INCLUDE THE TYPES THAT SATISFY THE FOLLOWING: (I) THE ESTIMATED FREQUENCY IS SIGNIFICANTLY DIFFERENT FROM 0 AND (II) THE ESTIMATED ERROR RATE IS SIGNIFICANTLY DIFFERENT FROM 1 (FOR QRE, THE ESTIMATED λ IS DIFFERENT FROM 0). SINCE THE ESTIMATED p_O IS NOT STATISTICALLY DIFFERENT FROM 0 IN THE FIRST SELECTED MODEL, WE APPLY OUR MODEL SELECTION PROCEDURE TWICE.

are virtually unaffected in both the full and selected models.[43] The estimated q in the selected model takes the value of 0.32, a value for which the predictions of GoF are relatively close to both *L1* and *L2*. However, note that both $L1$ and $L2$ still exhibit higher estimated shares than the estimated 8% for GoF even though the latter has one additional degree of freedom due to the parameter q. Moreover, the estimated value of $1-q$ is 68% in our estimation. This means that individuals classified as GoF believe that there are 68% of altruists in the population. This figure is considerably larger that the 5% proposed in McKelvey and Palfrey (1992) to explain the behavior in their experiment and contrasts starkly with the estimated proportions of A in any of our model specifications. We believe that these observations cast certain doubt on GoF as a relevant explanation of behavior in our data.

This conclusion notwithstanding, if one accepts GoF as a relevant explanation, observe that the estimated share of level-k is 63% in the uniform-error model in Table 6A, while the fraction of level-k plus GoF–the two models that relax the assumption of common knowledge of rationality–is 64% in Table 1.7. That is, our conclusions regarding the explanation of non-equilibrium choices in the data are *both* qualitatively and quantitatively robust to whether we include GoF among our candidate models: above 60% of behavior in our experiment can be attributed to the failure of common knowledge of rationality, and common knowledge of rationality and bounded rationality explain virtually all non-equilibrium behavior while preference-based approaches play a negligible role.

ROBUSTNESS 2: ESTIMATION BY PLAYER ROLE AND OMITTED TYPES

In this section, we provide two additional exercises. First, we estimate the selected models separately for the two player roles. Second, we report an exercise testing for omitted behavioral types. Since all our model specifications deliver similar messages,

[43] Since the estimated p_O is not statistically different from 0 in the first selected model (Selected 1), this is the only case in which we apply our model selection procedure twice.

the robustness checks in this section are only performed for the selected models from Table 6A.

Estimation by player role. There is some evidence that people adapt their sophistication to their strategic situations (see e.g. Kovarik et al., 2018, or Mengel and Grimm, 2012).[44] Since this might potentially also apply to different roles in the same game, we would like to make sure that our conclusions still hold if we re-estimate our selected models separately for each player role. Table 1.8 reports the estimates for both the uniform and spike-logit error specifications. Observe that our main conclusions are qualitatively unaffected by only considering one player type: a relatively small fraction of subjects is classified as *SPNE*, while the majority is best described by either *QRE* or level-k. Again, level-*k* is the most relevant model, classifying most people as *L1*. However, we observe two systematic quantitative differences across the player roles. In particular, Players 2 exhibit higher estimated shares of $L1$ and lower shares of $L3$. In fact, the estimates suggest that no subject in role 2 can be classified as $L3$.[45]

To see whether the type composition changes across the two player roles, we formally test whether the parameters estimated differ across the two models, using the uniform error specification. Brame et al. (1998) propose a statistical test for the equality of maximum-likelihood regression coefficients between two independent equations. Despite the differences mentioned above, these tests detect no difference between the corresponding pairs of estimated coefficients across the two models at the conventional 5% level, with the exception of the error rate of *L1*, ε_{L1}. These tests thus support the idea that the above classification differ neither qualitatively nor quantitatively across the two player roles.

Omitted types. One important question inherent in finite mixture-of-types models is the selection of the candidate types. What if there is a type that explains a relevant

[44] Gill and Prowse (2016) document that some people change their degree of sophistication depending on the sophistication of their opponents in Beauty Context Games.

[45] The available data do not allow us to explore the reason behind the differences.

TABLE 1.8: ESTIMATION RESULTS BY SUBJECTS' ROLE: PLAYER 1 AND PLAYER 2

	Uniform Error				Spike-Logit Error					
	Player 1		Player 2		Player 1			Player 2		
Type	p_k	ε_k, λ	p_k	ε_k, λ	p_k	ε_k	λ	p_k	ε_k	λ
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
SPNE	0.05*	0.33**	0.05	0.16**	0.05	0.23**	1.00	0.14**	0.26**	0.93
	(0.03)	(0.09)	(0.06)	(0.15)	(0.03)	(0.17)	(0.03)	(0.05)	(0.09)	(0.10)
O	0.05*	0.33**	0.05	0.16**	0.03*	0.00**	0.26	0.03	0.09**	0.08
	(0.03)	(0.09)	(0.06)	(0.15)	(0.01)	(0.20)	(0.17)	(0.02)	(0.12)	(0.11)
L1	0.31**	0.81**	0.44**	0.54**	0.31**	0.61**	0.01	0.44**	0.44**	0.05
	(0.07)	(0.04)	(0.09)	(0.05)	(0.08)	(0.04)	(0.04)	(0.06)	(0.02)	(0.06)
L2	0.10*	0.60**	0.23**	0.66**	0.10*	0.41**	0.16	0.06*	0.26**	0.42
	(0.06)	(0.14)	(0.08)	(0.09)	(0.05)	(0.05)	(0.09)	(0.02)	(0.11)	(0.15)
L3	0.22**	0.58**	0.05	0.69*	0.21*	0.45**	0.01	0.00	0.51**	0.05
	(0.09)	(0.03)	(0.03)	(0.15)	(0.10)	(0.08)	(0.02)	(0.00)	(0.15)	(0.07)
QRE	0.33**	0.38**	0.23**	0.67**	0.31**		0.38**	0.33**		0.27
	(0.07)	(0.10)	(0.05)	(0.15)	(0.06)		(0.10)	(0.06)		(0.17)

Notes: THE TABLE REPORTS THE ESTIMATION RESULTS FOR THE UNIFORM ERROR SPECIFICATION IN COLUMNS (1-4) AND THE SPIKE-LOGIT ERROR SPECIFICATION IN COLUMNS (5-10). COLUMNS (1), (3), (5), AND (8) PRESENT THE ESTIMATED FREQUENCIES OF EACH BEHAVIORAL MODEL; COLUMNS (2), (4), (6), (7), (9), AND (10) PRESENT THE ESTIMATED ERROR-RELATED PARAMETERS. FOR EACH ERROR SPECIFICATION, WE REPORT BOTH THE FULL AND THE SELECTED MODEL. WE ONLY REPORT BEHAVIORAL TYPES FROM THE SELECTED MODELS IN TABLE 6A.

part of our subjects' behavior but is not included in the set of candidate explanations?

To test for this possibility, we perform the following "omitted type" exercise. We re-estimate our models separately for each player role 76 and 75 times for Player 1 and Player 2, respectively. In each of these 151 estimations, we add to the set of considered models an additional type, whose predicted behavior is identical to the experimental behavior of one particular subject's choices. That is, each subject represents one type in one these 151 estimations. If someone's behavior approximates the behavior of many others well and is sufficiently different from any of the theories considered, this would provide evidence for a relevant type being missing from our set. Note that this exercise is only possible under the uniform error specification because we take the behavioral profile as a type without actually observing the underlying optimization problem of such subjects.

For such an omitted type to be relevant, two criteria are applied. First, we require the type to attract a non-negligible frequency. In particular, we look for subjects who attract a share of at least 10% the population. Second, we require the type to be sufficiently separated from any candidate explanation already considered. In particular, types must be separated in at least half of the 16 CGs. It turns out that there are five subjects who play as Player 1 (subjects 10, 17, 39, 67 and 72) and three who play as Player 2 (subjects 100, 140, 151), who satisfy both conditions. A closer look at their behavior reveals that they all behave as hybrids between different consecutive types of level-*k*. When we add those two combinations as possible behavioral types, *L1-L2* and *L2-L3*, which consist of either one type or the other, none of the predictions of the omitted types survives the application of the separation criteria mentioned above.

Hence there are some subjects whose behavior taken as a behavioral type could potentially explain that of a non-negligible part of other subjects. However, these omitted types hybridize level-*k* and mostly affect the share of different level-*k* types, such that they still maintain the assumption of perfect rationality and relax the common knowledge of rationality.[46] Therefore, their existence does not affect our main conclusions in that equilibrium behavior is represented by a minority and that the majority of individuals can be explained by either level-*k* or *QRE*.

ROBUSTNESS 3: OUT-OF-SAMPLE PREDICTIONS

The ability to predict out-of-sample is a desirable feature of any model. In this section, we assess the extent to which the selected uniform-error mixture model (columns (3 - 4) in Table 6A) that best fits the behavior of subjects in some games, referred to as *in-sample* games, is able to predict both individual and population-level behavior in other games, referred to as *out-of-sample* games. For this model to generate successful out-of-sample predictions, we require two things. First, the estimated composition of the population, as well as the individual posterior probabilities of belonging to a particular behavioral type must be stable across different in-sample games. This would actually show how robust the estimates of our mixture-of-types model in Table 6A is

[46] See Gill and Prowse (2016) for further evidence on these hybrid types. A finer analysis of these hybrid types is out of the scope of the present study.

to the removal of one particular game. Second, this model must predict behavior successfully in and consistently across different out-of-sample games, at both individual and population level.[47]

The individual- and population-level out-of-sample prediction exercises that we carry out exploit the behavioral variation across the 16 different CGs used in our experiment and have the following common basis. First, we estimate 16 variations of the model with 15 games only (rather than 16 as in Table 6A). Since we remove a different game each time from each estimation, this yields 16 different population-level estimations, reported in Tables 1.16 and 1.17 in the Appendix A.

We first check the robustness of the estimates to such removals. At the population level, we formally test whether any of the parameters estimated in these 16 models is significantly different from its counterpart in the benchmark model in columns (3 - 4) in Table 6A. It is remarkable that none of the $16 \times 12 = 192$ parameters is significantly different at conventional 5% from its original estimation with 16 games.[48] At the individual level, we employ the parameters estimated in the 16 models based on 15 games and compute the posterior probabilities of our 151 subjects belonging to each behavioral type.[49] For each of the 16 models, we assign each individual to the type that best explains his/her behavior in the in-sample games. This enables us to assess the individual-level stability and consistency of our classification from the previous section. We observe that 42% of the subjects (63 out of 151) are fully consistent, such that they are classified in the same behavioral type across all 16 estimations; 77% (117 out of 151) are consistently classified in at least 12 out of 16 (75%) games. Most of our subjects are thus consistently classified into the same behavioral model at the individual level

[47]There only exists scarce evidence of whether subjects' behavior is stable across different games. Georganas et al. (2015) examine whether level-k model generates stable cross-game predictions at the individual level in two types of games (undercutting games and two-person guessing games). They report stable classification within types but large instability across game types. Given these results, our out-of-sample exercise solely focuses on different CGs.

[48]We do not report the details of these 192 tests here. They are available upon request from the authors.

[49]This would lead to 16 four-panel graphs similar to Figure 1.14 in the Appendix A.

even in subsets of our games. Therefore, our main estimation results are highly robust to the removal of any single game at both the population and the individual level.

Individual-level out-of-sample predictions. We use the above individual-level classification of subjects based on the 15 in-sample games and predict the strategy that each subject should take in the out-of-sample CG excluded while making the classification. Our individual-level test compares this predicted behavior with the action actually taken by the corresponding subject in the corresponding game. This exercise generates a 151×16 matrix of "hits", or "probability of hits" for QRE, for the 151 subjects in the 16 CGs, which enables us to assess the ability of the mixture-of-types model to predict the behavior of each subject in the out-of-sample CGs.

To provide a relative prediction performance of our mixture model, we repeat this procedure for each relevant behavioral type (*SPNE*, *QRE*, *O*, and level-*k*) in isolation. For *SPNE* and *O*, we simply take the average compliance between their predictions and actual individual behavior across the 16 CGs. For *QRE* we estimate 16 models that assume that all subjects are classified as *QRE* using their observed behavior in the in-sample games, yielding one λ per estimation. With the estimated λ's, we assess the average ability of *QRE* to predict the observed behavior in the out-of-sample CGs. For level-*k*, we estimate 16 mixture models with three types, *L1*, *L2*, and *L3*, and compute accordingly the average individual-level ability of this level-*k* mixture to predict out of sample.

The left panel of Figure 1.6 reports the average improvement in the ability to predict individual behavior out of sample (in percentage terms) in each model, be it our mixture model or one of the four one-type models described above, compared to a pure random hit of 0.25. The latter corresponds to a purely random type that selects each strategy with probability one fourth. The figures reported should thus be interpreted as how much better in percentage points the out-of-sample prediction of a particular model is than a pure random selection of action. The vertical bars reflect standard errors of the 16 improvements and reflect how sensitive the ability of each model to predict individual out-of-sample behavior is to different out-of-sample CGs. A good model should on average exhibit significantly greater improvements with respect to random behavior and the average improvement should not be too sensitive to which game is being predicted. Our mixture and the mixture of different level-k's exhibit the largest improvements compared to random prediction, but a comparison between them shows that allowing non-level-*k* types in our mixture model improves the ability to predict individual behavior significantly. Our mixture model improves the prediction ability of a random type by almost 90%, compared to less than 80% in case of level-k. Additionally, our mixture-of-types model and *QRE* reveal the lowest sensitivity to which CG is being predicted. However, our mixture-of-types model largely outperforms *QRE*. We thus conclude that jointly considering alternative explanations of behavior significantly enhances the ability of a model to predict individual behavior in out-of-sample games.

Population-level out-of-sample predictions. This test is again based on the 16 estimations with the 15 in-sample games described above.[50] With the estimates in hand, we compute the log-likelihood (1.8) for the *observed* behavior of our subjects in the out-of-sample game. This generates 16 log-likelihood values. Again, to be able to assess the relative ability of the mixture-of-types model in columns (3 - 4) in Table 6A to predict out of sample *vis-à-vis* the individual explanations of behavior in the in-sample games, we apply the estimated parameters and compute the loglikelihood of the observed behavior in the out-of-sample CG. As before, we preform the same exercise for a random type, which selects each strategy with probability one fourth, and use it for normalization. More precisely, we compute the difference between the log-likelihood of each model (once again either our mixture model or all the one-type

[50]This exercise is motivated by Wright and Leyton-Brown (2010) except that we predict the behavior of the same subjects in different games, rather than using one part of the subject pool for predicting the behavior of other part of the pool.

models) and the log-likelihood of the uniform model divided by the log-likelihood of the uniform model, which gives the percentage improvement in log-likelihoods of each model with respect to random behavior. The right-hand panel of Figure 1.6 reports the average percentage improvement of the log-likelihood values with respect to random behavior (see Table 1.18 for more details). The vertical bars reflect the standard errors of these 16 improvements and again reflect how sensitive the ability of each model to predict is to the CG considered. A good model should on average exhibit significantly greater improvement with respect to random behavior and the average improvement should not be too sensitive to which game is being predicted. In the latter case, if the standard errors are large the particular model predicts well for some games but fails to predict successfully for others. Observe that the first condition is only satisfied for the mixture-of-types model and a mixture of level-k. That is, *QRE*, *SPNE*, and *O* alone are not significantly better at predicting the behavior of our subjects out-of-sample than a random selection of an action. Furthermore, the mixture-of-types model on average outperforms the level-k alone.

As for standard errors, the mixture-of-types model is also the most stable at predicting behavior, while all the others show greater sensitivity to the out-of-sample games. In fact, our mixture-of-types model is the only one that always outperforms the random type (see Table 1.18). The remaining models always predict the behavior worse than a pure random type for at least two games.[51] This includes the mixture of level-k.

We thus conclude that, at both the individual and population levels, our mixture-of-types model is the most successful in predicting behavior and the least dependent on which out-of-sample game is chosen to predict. As a result, researchers should account for behavioral heterogeneity in CGs not only for a better explanation of behavior as advocated by this chapter, but also for a better prediction of choices in out-of-sample games.

[51] *O* is an exception but it is because the estimated error is $\varepsilon_o = 1$. Therefore, it always performs as the random behavioral type.

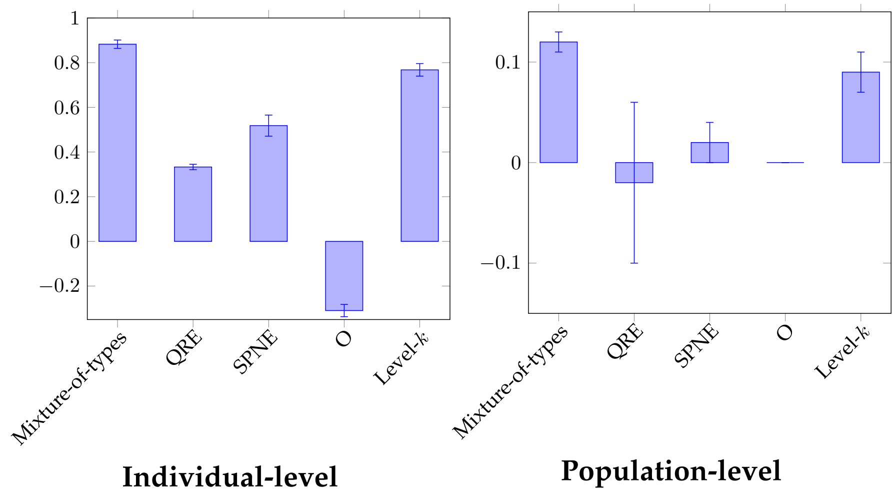


FIGURE 1.6: AVERAGE PERCENTAGE IMPROVEMENT IN THE ABILITY TO PREDICT (LEFT) AND LOG-LIKELIHOOD (RIGHT) OVER THE RANDOM BEHAVIOR, WHEN COMPARING THE OBSERVED BEHAVIOR IN OUT-OF-SAMPLE GAMES USING IN-SAMPLE GAMES FOR ESTIMATION.

1.6 CONCLUSIONS

We report a study designed to explain initial behavior in CGs, combining experimental and econometric techniques. Our approach enables us to classify people into different behavioral explanations and discriminate between them. Crucially, this approach determines endogenously whether one or multiple explanations are empirically relevant. We show that people are largely heterogeneous and more than one explanation is required to both explain and predict individual behavior in CGs. Independently of our model specification, roughly 10% of people behave close to *SPNE* and most non-equilibrium behavior seems to be due to two reasons: either the failure of common knowledge of rationality, as advocated by Aumann (1992, 1995) and modeled via level-k thinking model in our setting, or bounded reasoning abilities of subjects, simulated by *QRE*.

The reported results may stimulate future research in two directions. Our study contributes to the "competition" between level-k models and *QRE* as two behavioral alternatives to standard equilibrium approaches. Some authors argue for the former while others prefer the latter, but empirical literature has found difficulties in discriminating between the two approaches because both theories often predict very similar behavior. As a result, most studies compare their abilities to explain behavior using the representative-agent approach. Our design allows us to separate the predictions of the two theories and we show that–at least in our setting–both level-k models and *QRE* are empirically relevant for the explanation of non-equilibrium choices but each model explains the behavior of different subjects. Future research should determine how these conclusions extend to other strategic contexts.

Second, the behavior in CGs and other extensive-form games have been attributed to their dynamic nature and the failure of backward induction, whereas our study again shows that it may be a more general non-equilibrium phenomenon. Since most non-equilibrium choices in our experiment are best explained by *QRE* and level-k that have been successful in explaining behavior in static environments, our findings call for a reevaluation of the aspects that distinguish static from dynamic games in the analysis of non-equilibrium behavior.

1.7 Appendix A: Additional Tables and Figures

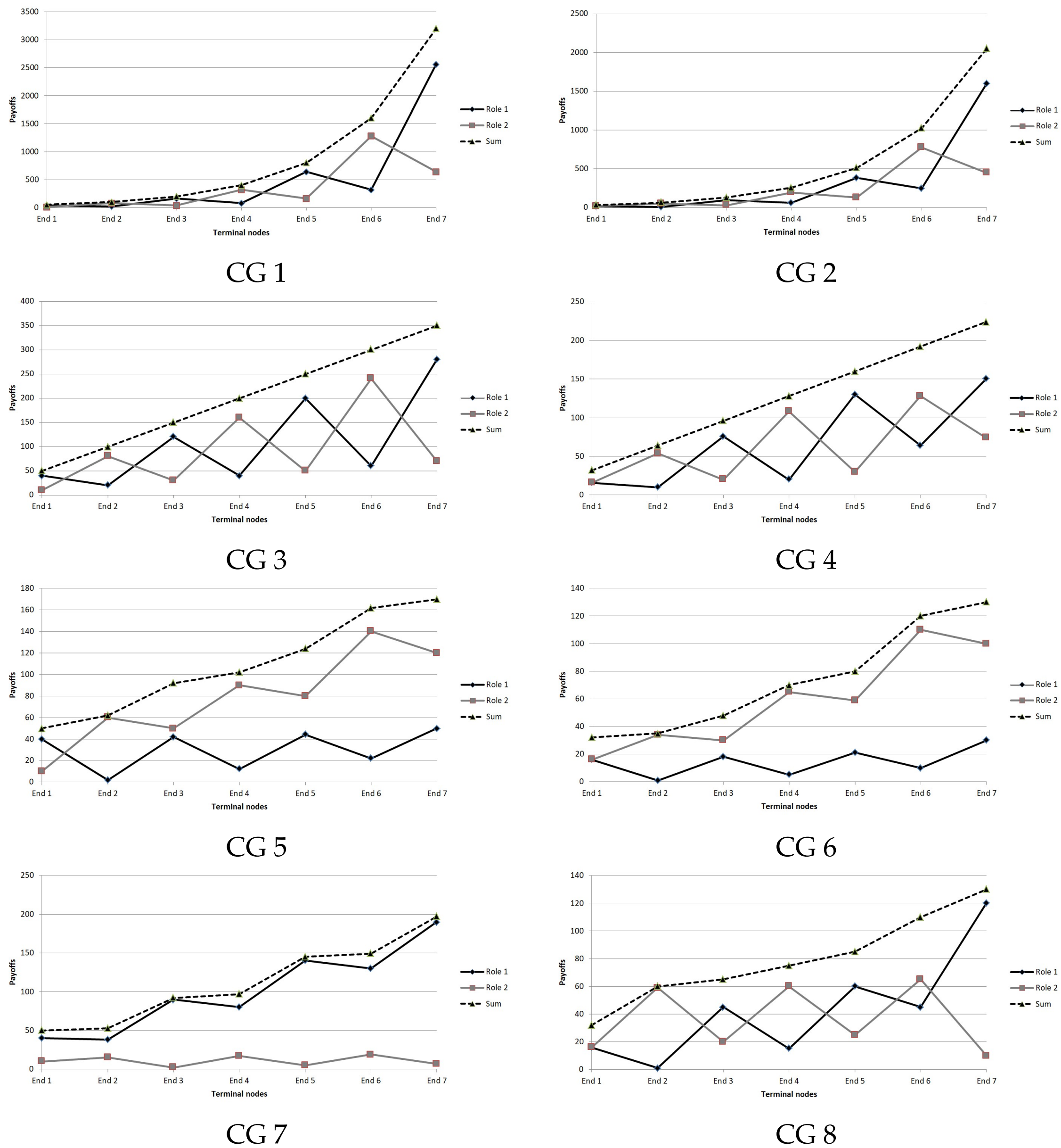
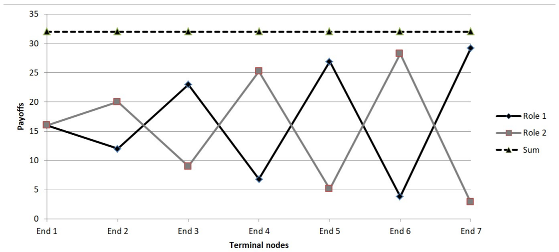
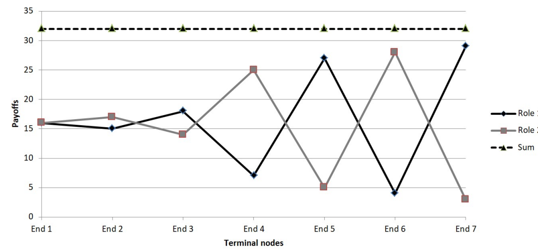


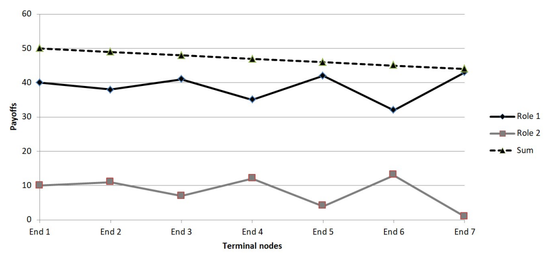
Figure 1.7: Alternative Representation of the Centipede Games used in the Experiment (1-8).



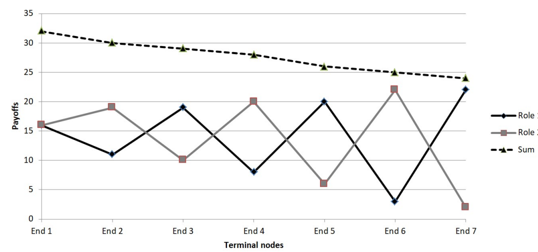
CG 9



CG 10



CG 11



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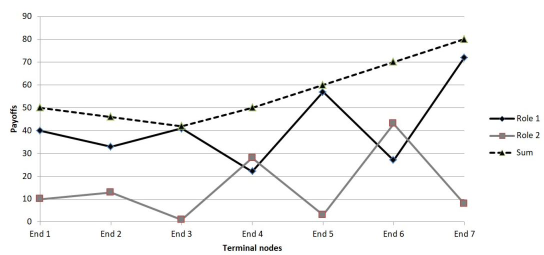
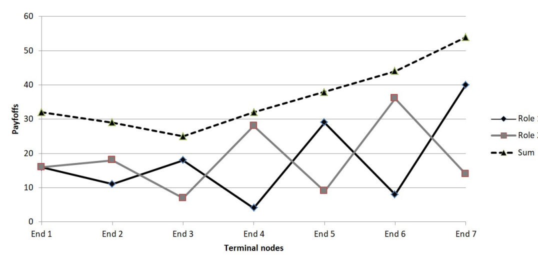
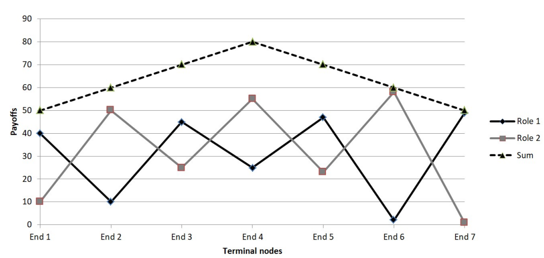


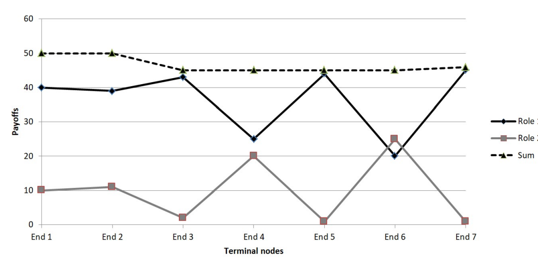
CG 13



CG 14



CG 15



CG 16

FIGURE 1.8: ALTERNATIVE REPRESENTATION OF THE CENTIPEDE GAMES USED IN THE EXPERIMENT (9-16).

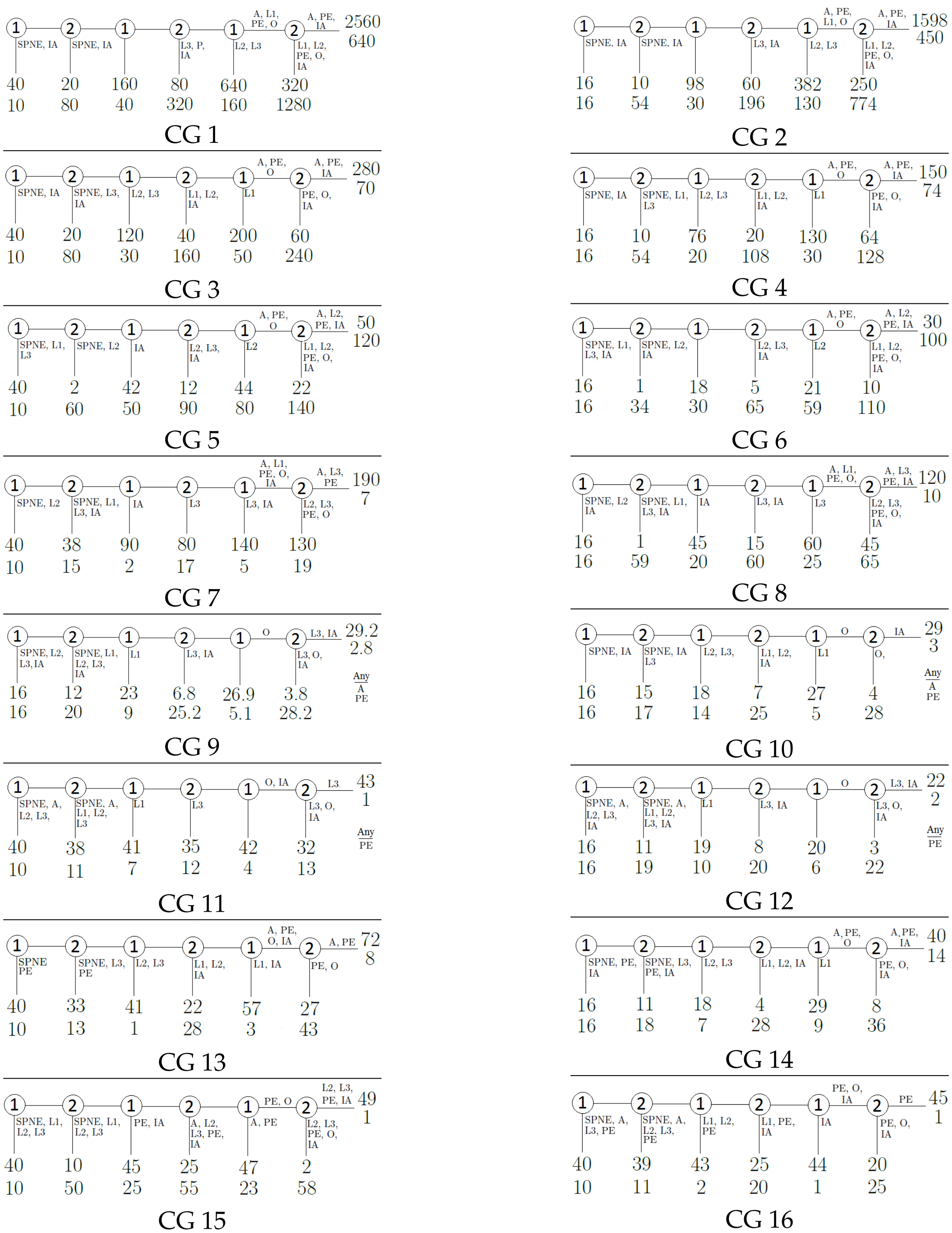


FIGURE 1.9: THE 16 CGS USED IN THE EXPERIMENT WITH THE PREDICTIONS OF EACH OF THE BEHAVIORAL MODELS EXCEPT QRE, $A(\gamma)$ AND $IA(\rho, \sigma)$ (SEE FIGURE 1.10).

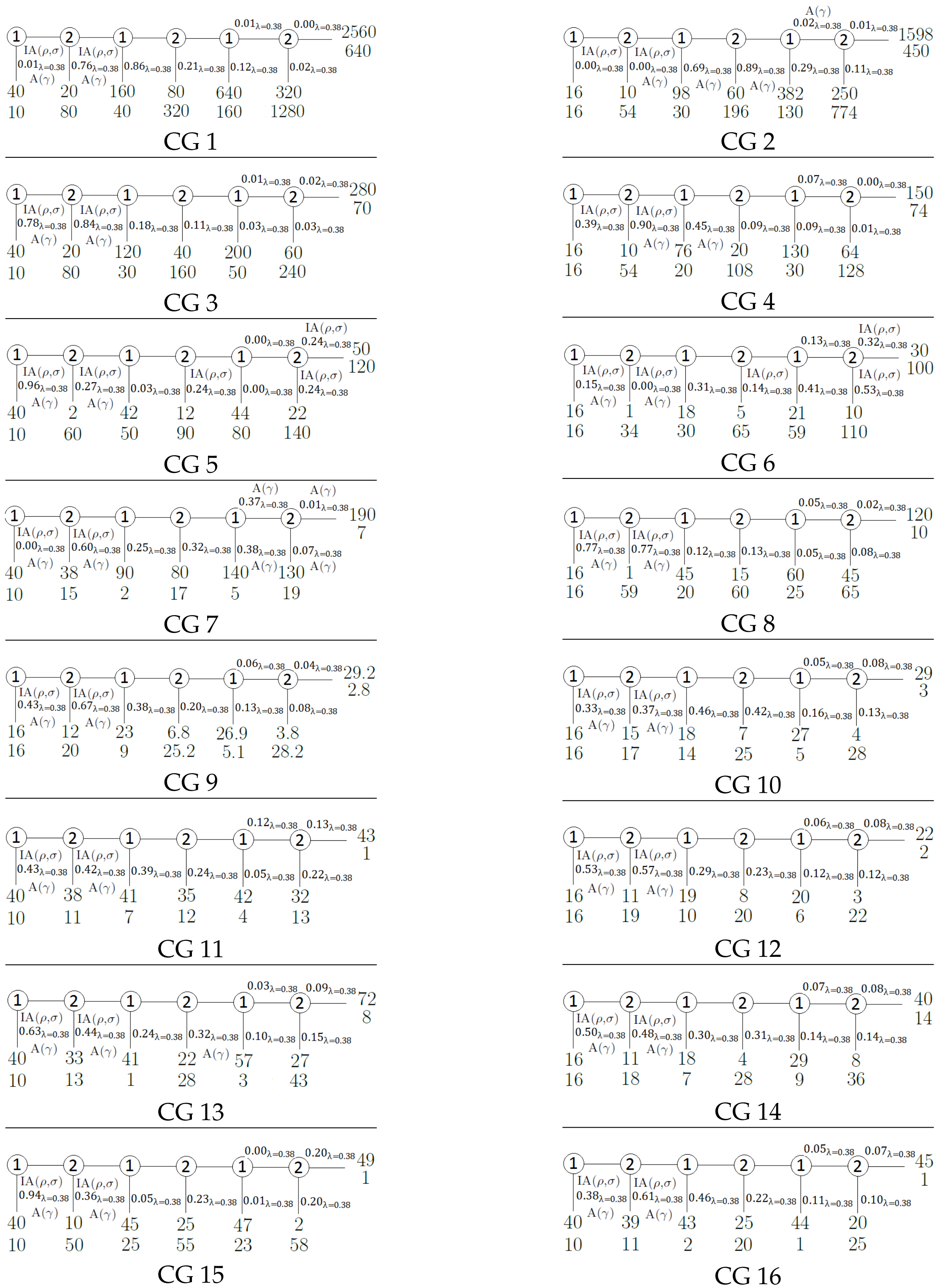


FIGURE 1.10: THE 16 CGS USED IN THE EXPERIMENT WITH THE PREDICTIONS OF *QRE*, $A(\gamma)$ AND $IA(\rho, \sigma)$ FOR THE VALUES $\lambda = 0.38$, $\rho = 0.22$ AND $\sigma = 0.55$.

TABLE 1.9: THE SEPARATION RATES BETWEEN QRE AND ALL OTHER BEHAVIORAL MODELS FOR $\lambda = \{0, 0.1, ..., 1\}$

	SPNE	*A(γ=.22)*	*IA(ρ=.08,σ=.55)*	*A*	*IA*	*PE*	*O*	*L1*	*L2*	*L3*
$\lambda = 0.0$	0.75	0.70	0.70	0.55	0.45	0.38	0.75	0.75	0.68	0.61
$\lambda = 0.1$	0.66	0.60	0.64	0.91	0.87	0.84	0.91	0.47	0.54	0.58
$\lambda = 0.2$	0.50	0.44	0.48	0.84	0.74	0.83	0.91	0.53	0.48	0.58
$\lambda = 0.3$	0.34	0.32	0.35	0.84	0.76	0.82	0.94	0.59	0.57	0.58
$\lambda = 0.4$	0.31	0.29	0.32	0.88	0.72	0.85	0.97	0.66	0.54	0.52
$\lambda = 0.5$	0.25	0.29	0.26	0.88	0.69	0.85	0.97	0.69	0.60	0.52
$\lambda = 0.6$	0.22	0.27	0.23	0.88	0.69	0.85	0.97	0.72	0.66	0.52
$\lambda = 0.7$	0.22	0.27	0.23	0.88	0.69	0.85	0.97	0.72	0.66	0.52
$\lambda = 0.8$	0.19	0.24	0.20	0.88	0.66	0.85	0.97	0.72	0.66	0.52
$\lambda = 0.9$	0.16	0.22	0.17	0.88	0.66	0.85	0.97	0.72	0.66	0.55
$\lambda = 1.0$	0.16	0.22	0.17	0.88	0.66	0.85	0.97	0.72	0.66	0.55

Notes: THE TABLE REPORTS THE SEPARATION RATES BETWEEN THE *QRE* AND EACH OF THE BEHAVIORAL MODELS LISTED IN THE FIRST ROW FOR DIFFERENT VALUES OF λ. THE MINIMUM SEPARATION IS 0, WHEN TWO BEHAVIORAL MODELS PREDICT EXACTLY THE SAME STRATEGY FOR EACH OF THE PLAYER ROLES AND EACH OF THE CENTIPEDE GAMES. THE MAXIMUM SEPARATION RATE IS 1 WHEN TWO BEHAVIORAL MODELS PREDICT A DIFFERENT STRATEGY FOR EACH OF THE PLAYER ROLES AND EACH OF THE CENTIPEDE GAMES.

TABLE 1.10: THE SEPARATION RATES BETWEEN $A(\gamma)$ AND $SPNE$ FOR $\gamma = \{0.01, 0.1, 0.2, .., 1\}$

γ	Separation Rates
0.01	0.00
0.10	0.00
0.20	3.83
0.30	10.83
0.40	13.59
0.50	15.67
0.60	17.08
0.70	18.92
0.80	21.08
0.90	22.58
1.00	26.33

Notes: THE TABLE REPORTS THE SEPARATION RATES BETWEEN THE $A(\gamma)$ AND $SPNE$ FOR DIFFERENT VALUES OF γ. THE MINIMUM SEPARATION IS 0, WHEN TWO BEHAVIORAL MODELS PREDICT EXACTLY THE SAME STRATEGY FOR EACH OF THE PLAYER ROLES AND EACH OF THE CENTIPEDE GAMES. THE MAXIMUM SEPARATION RATE IS 1 WHEN TWO BEHAVIORAL MODELS PREDICT A DIFFERENT STRATEGY FOR EACH OF THE PLAYER ROLES AND EACH OF THE CENTIPEDE GAMES.

TABLE 1.11: THE SEPARATION RATES BETWEEN $IA(\rho, \sigma)$ AND $SPNE$ FOR $\rho = \{0.01, 0.1, 0.2, .., 1\}$ AND $\sigma = \{0.01, 0.1, 0.2, .., 1\}$

ρ/σ	0.01	0.10	0.20	0.3	0.40	0.50	0.60	0.70	0.80	0.90	1.00
0.01	0.00	0.00	0.75	1.50	1.50	1.50	1.50	1.50	1.50	1.50	1.50
0.10	0.50	0.50	1.25	1.25	1.25	1.25	1.25	2.00	2.00	2.00	2.00
0.20	3.00	3.17	3.00	3.00	3.00	3.00	3.00	3.00	3.00	3.00	3.00
0.30	5.92	5.75	5.25	5.25	5.25	5.25	5.25	5.25	5.25	5.25	5.25
0.40	7.58	6.92	6.75	6.25	6.25	6.25	6.25	6.25	6.25	6.25	6.25
0.50	10.92	10.17	8.75	8.25	8.25	8.25	8.25	8.25	8.25	8.25	8.25
0.60	12.75	11.00	9.92	9.42	8.42	8.42	8.42	8.42	8.42	8.42	8.42
0.70	19.33	14.25	11.58	11.00	11.00	9.75	9.58	9.58	9.58	9.58	9.58
0.80	20.92	16.08	14.67	12.83	11.58	11.58	11.58	10.25	10.25	10.17	10.17
0.90	21.92	19.58	16.00	14.17	13.17	13.17	11.58	11.58	11.58	10.25	10.25
1.00	21.08	20.75	16.00	14.33	14.33	14.33	13.33	13.33	11.75	11.75	11.75

Notes: THE TABLE REPORTS THE SEPARATION RATES BETWEEN THE $IA(\rho, \sigma)$ AND $SPNE$ FOR DIFFERENT VALUES OF ρ AND γ. THE MINIMUM SEPARATION IS 0, WHEN TWO BEHAVIORAL MODELS PREDICT EXACTLY THE SAME STRATEGY FOR EACH OF THE PLAYER ROLES AND EACH OF THE CENTIPEDE GAMES. THE MAXIMUM SEPARATION RATE IS 1 WHEN TWO BEHAVIORAL MODELS PREDICT A DIFFERENT STRATEGY FOR EACH OF THE PLAYER ROLES AND EACH OF THE CENTIPEDE GAMES.

Separation in Payoffs. This part provides an alternative look at how well the predictions of the candidate explanations are separated in our CGs, taking into account the incentives of each behavioral type to behave as a different type. More precisely, Table 1.12 compares the incentives of each behavioral type to follow its predictions in our 16 CGs vs. the predictions of any alternative theory, measured according to the *goal* of each type (e.g. the sum of payoffs of both players for A, payoff difference for IA, and simply payoffs or utilities for the other types). The first row and column list the different behavioral types; the upper (lower) part of the table corresponds to Player 1(2). A particular cell ij reports the aggregate payoff that the behavioral type in row i earns in the 16 CGs if she behaves true to type in column j and her opponents behave in line with the beliefs of type i. In particular, if $i = j$ the cell contains the total payoff over the 16 CGs of a subject who is of type i always behaves as predicted by rule i and the opponents always behave according to the beliefs of type i. For example, a Player 1 who adheres to *SPNE* always ends the game at her first decision node, leading to $8 \times 40 + 8 \times 16 = 448$ experimental points. For $i \neq j$, the cells contain the total payoff in the 16 CGs that type i obtains if she keeps the beliefs of type i but behaves as type j instead. As example, consider a *SPNE* player with *SPNE* beliefs who behaves as A. Since such a player expects the opponent to behave according to *SPNE*, the cell $(SPNE, A)$ contains $20+10+20+10+2+1+38+1+13+15.25+40+16+33+11+10+39.25 = 279.5$. Note that no such analysis can be carried out for *PE* as they are not following an optimization problem, although it is possible to calculate the payoff that other behavioral types would earn if they followed the *PE* prescription rather than their own optimal strategy.

TABLE 1.12: SEPARATION IN PAYOFFS BETWEEN DIFFERENT MODELS

Player 1	*SPNE*	*A*(γ)	*IA*(ρ,σ)	*A*	*IA*	*PE*	*O*	*L1*	*L2*	*L3*	*QRE*($\lambda = 0.38$)
SPNE	**448.00**	434.67	448.00	279.50	368.00	280.33	271.00	354.00	329.00	366.00	380.41
A(γ=.22)	492.82	655.28	492.82	681.26	490.62	684.13	673.95	**733.67**	633.42	732.46	618.81
IA(ρ=.08,σ=.55)	**429.34**	368.06	**429.34**	45.11	352.50	44.35	31.12	181.97	181.91	202.36	264.52
A	656.00	1856.67	656.00	**6859.00**	831.67	6827.42	6856.00	6408.00	2165.00	2191.00	1403.06
IA	240.00	323.33	240.00	436.50	**141.00**	447.42	427.00	397.00	424.00	415.00	366.31
O	448.00	1423.33	448.00	5283.28	587.00	5258.69	**5307.20**	5133.00	1571.00	1703.00	1065.40
L1	448.00	796.25	448.00	1781.78	484.08	1783.61	1755.95	**1861.75**	1028.00	1122.00	776.19
L2	448.00	664.67	448.00	851.25	383.00	863.75	830.00	902.00	**1571.00**	1542.00 1	816.95
L3	448.00	729.33	448.00	1011.13	495.25	1024.08	992.50	1034.00	1539.50	**1703.00**	910.26
QRE($\lambda = 0.38$)	448.00	539.28	448.00	466.69	437.23	472.25	443.48	512.85	517.39	541.97	**553.37**

Player 2	*SPNE*	*A*(γ)	*IA*(ρ,σ)	*A*	*IA*	*PE*	*O*	*L1*	*L2*	*L3*	*QRE*($\lambda = 0.38$)
SPNE	**208.00**	**208.00**	**208.00**	**208.00**	**208.00**	**208.00**	**208.00**	**208.00**	**208.00**	**208.00**	**208.00**
A(γ)	381.00	580.72	381.00	740.04	572.11	740.04	740.04	744.56	**756.49**	462.75	490.55
IA(ρ,σ)	**76.25**	**76.25**	**76.25**	**76.25**	**76.25**	**76.25**	**76.25**	**76.25**	**76.25**	**76.25**	**76.25**
A	876.00	1436.00	991.75	**6859.00**	3302.50	5432.33	4037.00	3685.00	3771.75	1557.75	1480.50
IA	218.00	215.67	201.50	189.75	**141.00**	181.75	158.00	159.00	187.00	191.50	184.71
O	595.00	962.17	680.75	1661.10	1635.27	2304.93	**3009.20**	2847.00	2774.75	1031.60	1029.40
L1	498.25	592.83	526.50	700.04	740.45	860.08	1027.33	**1106.25**	1054.00	662.54	632.86
L2	487.00	853.17	487	1284.25	1278.50	1806.08	2299.00	2565.00	**2584.00**	840.00	773.69
L3	499.00	531.00	536.75	584.00	621.58	591.67	581.00	581.00	582.75	**942.00**	700.51
QRE($\lambda = 0.38$)	396.19	401.73	409.12	346.52	377.21	347.05	330.05	335.18	412.13	384.42	**427.23**

Notes: THE TABLE REPORTS THE SEPARATION IN PAYOFFS BETWEEN THE BEHAVIORAL MODELS LISTED IN THE FIRST COLUMNS AND FIRST ROWS, FOR PLAYERS 1 AND 2 IN THE TOP AND BOTTOM PANELS. A PARTICULAR NUMBER IN ROW i AND COLUMN J REPORTS THE PAYOFF A BEHAVIORAL MODEL LISTED IN ROW i OBTAINS IF IT FOLLOWS THE STRATEGIES PREDICTED BY THE BEHAVIORAL MODEL LISTED IN COLUMN j.

By construction, the comparison of the $i = j$ values with $i \neq j$ illustrate the incentives of a subject of a particular type to comply or not with her type. In the table, the highest values (lowest payoff difference for *IA*) are in bold. As expected, almost all types maximize their goal if they follow the prescriptions of their type, while alternative decision rules typically yield a lower payoff (higher payoff difference in case of *IA*).[52] The behavioral types that show the widest separation in payoffs is those of A

[52] If Player 1 behaves according to *SPNE* Player 2's behavior is irrelevant. Hence, the 208 experimental points in all columns for *SPNE*-Player 2.

and *O*, while *SPNE* and *QRE* show the smallest separation.

Replication of Behavior. Compared to earlier experimental studies on CGs, we have changed several features in the procedures in carrying out our experiment. First and most importantly, we apply the strategy method rather than the direct hot-hand method. Second, our subjects played several CGs and we only elicit the initial responses in each game. Third, we only pay for three randomly chosen games. As a result, we first ask whether these features do not distort subjects' behavior *vis-à-vis* other studies.

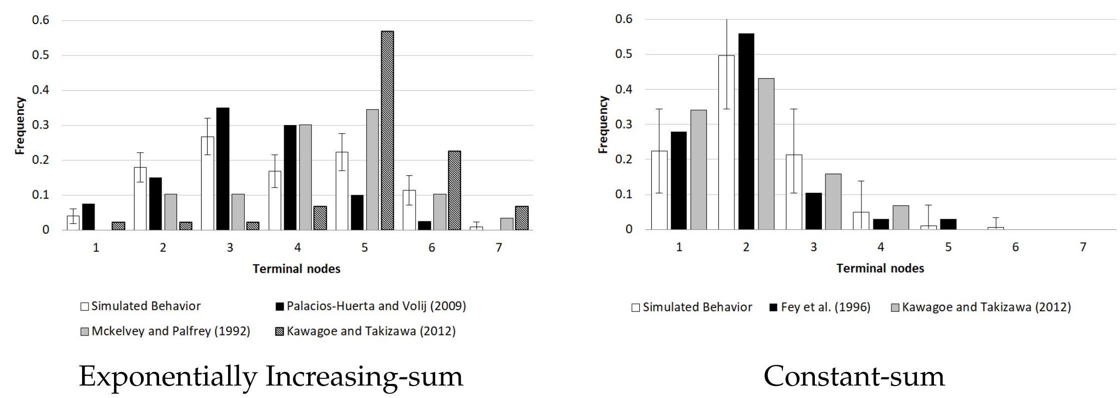


FIGURE 1.11: COMPARISON OF BEHAVIOR ACROSS DIFFERENT STUDIES

Our exponentially increasing-sum CG 1 belongs to the most commonly tested variations of CGs. We compare the behavior of our 151 subjects who played the no-feedback, cold version of the game with three other studies. First, we contrast their behavior with the initial lab behavior of students in Palacios-Huerta and Volij (2009). Their subjects (like ours) came from the University of the Basque Country. There were 80 students (40 in each role, as opposed to our 151, 76 as Player 1 and 75 as Player 2) and none of their students came from Economics or Business (whereas ours mostly come from these two fields). Second, we also compare our subjects' behavior with those of the 58 subjects (29 in each player role) in McKelvey and Palfrey (1992). Third, we contrast our data with the obtained in Kawagoe and Takizawa (2012). They do not report the exact number of subjects; we approximate it from the information on the number of sessions and participants in each session. Given the different elicitation methods, we cannot directly compare the behavior. To be able to compare them, we create 100,000 random sub-samples from our data to match the number of subjects in Palacios-Huerta and Volij (2009) and McKelvey and Palfrey (1992), and the approximate number of subjects in Kagawoe and Takizawa (2012), respectively. For each sub-sample, we randomly pair Players 1 and 2 and record the behavior that we would observe if these two individuals interacted. Each of the 100,000 sub-samples thus generates one possible distribution of behavior if our subjects participated in an experiment under the conditions of the studies in the comparison. The white bars in Figure 1.11, on the left, report the average *simulated* stopping frequencies at a particular decision node in the 100,000 simulated sub-samples corresponding to the conditions of Palacios-Huerta and Volij (2009). The black and grey bars show the *observed* stopping frequencies in Palacios-Huerta and Volij (2009), McKelvey and Palfrey (1992), and Kawagoe and Takizawa (2012), in this order. The horizontal bars present the 95% percentiles of the simulated distributions of behavior under Palacios-Huerta and Volij (2009)'s conditions.[53] It can be seen that the behavior in our experiment is relatively similar to that in the study by to Palacios-Huerta and Volij (2009). When it differs, it typically deviates towards the frequencies observed in McKelvey and Palfrey (1992) or Kawagoe and Takizawa (2012), where there is a general tendency to stop somewhat later.

The right-hand side of Figure 1.11 also compares the corresponding simulated behavior in CG 9 with the initial behavior of the 58 subjects in Fey et al. (1996) and the behavior of the 40-48 (approximated by 44) subjects presented in Kawagoe and Takizawa (2012). The behavior is very similar in all cases.

[53] For the sake of readability, we omit the corresponding simulated behavior for McKelvey and Palfrey's (1992) and Kawagoe and Takizawa (2012) conditions in the figure and only use them for the statistical tests below. Since there are more observations in Palacios-Huerta and Volij (2009), their conditions generate less variability in the simulated behavior and thus present a more conservative comparison.

We thus conclude that the behavior in our CG 1 and 9 is comparable to that of other studies.[54] In CG 1, our observed behavior is particularly close to the one observed in the experiment by Palacios-Huerta and Volij (2009), conducted few years earlier at the same University; in CG 9, our observed behavior is very close to both Fey et al. (1996) and Kawagoe and Takizawa (2012).

Finally, in the third chapter, we explore whether the hot and cold methods generate the same behavior using four of our CGs: the games CG1, CG9, CG7 and CG16. We observe that the use of direct method tends to make individuals stop somehow earlier than the strategy method in CG1 and CG9. However, the behavior elicited using the cold method in this study is still in between the behavior using direct method by our experiment and the behavior using the direct method by the original studies. We found no differences across the two elicitation methods in CG7 and CG16. Hence, our design features do not seem to distort subjects' behavior.

[54]We performed Pearson chi-square tests of independence with two alternative null hypotheses. First, the test of the null hypothesis that the simulated play of different ending nodes in our experiment is not different from the behavior in other studies yields p-values of 0.38, 0.34, 0.64, 0.00 and 0.56 for the comparison with Palacios-Huerta and Volij (2009), McKelvey and Palfrey (1992), Fey et al. (1996), and the increasing- and constant-sum treatments of Kawagoe and Takizawa (2012), respectively. Second, the test of the null hypothesis that the behavior from other studies come from our simulated play yields p-values of 0.09, 0.07, 0.49, 0.00 and 0.35 respectively. No test is rejected at conventional 5% significance, with the exception of the increasing-sum treatment of Kawagoe and Takizawa (2012) where subjects stop significantly later than in our and the other studies. Since this difference also arises in the comparison of Kawagoe and Takizawa (2012) with McKelvey and Palfrey (1992) and Palacios-Huerta and Volij (2009), we conclude that the behavior of our subjects does not differ from that in other studies.

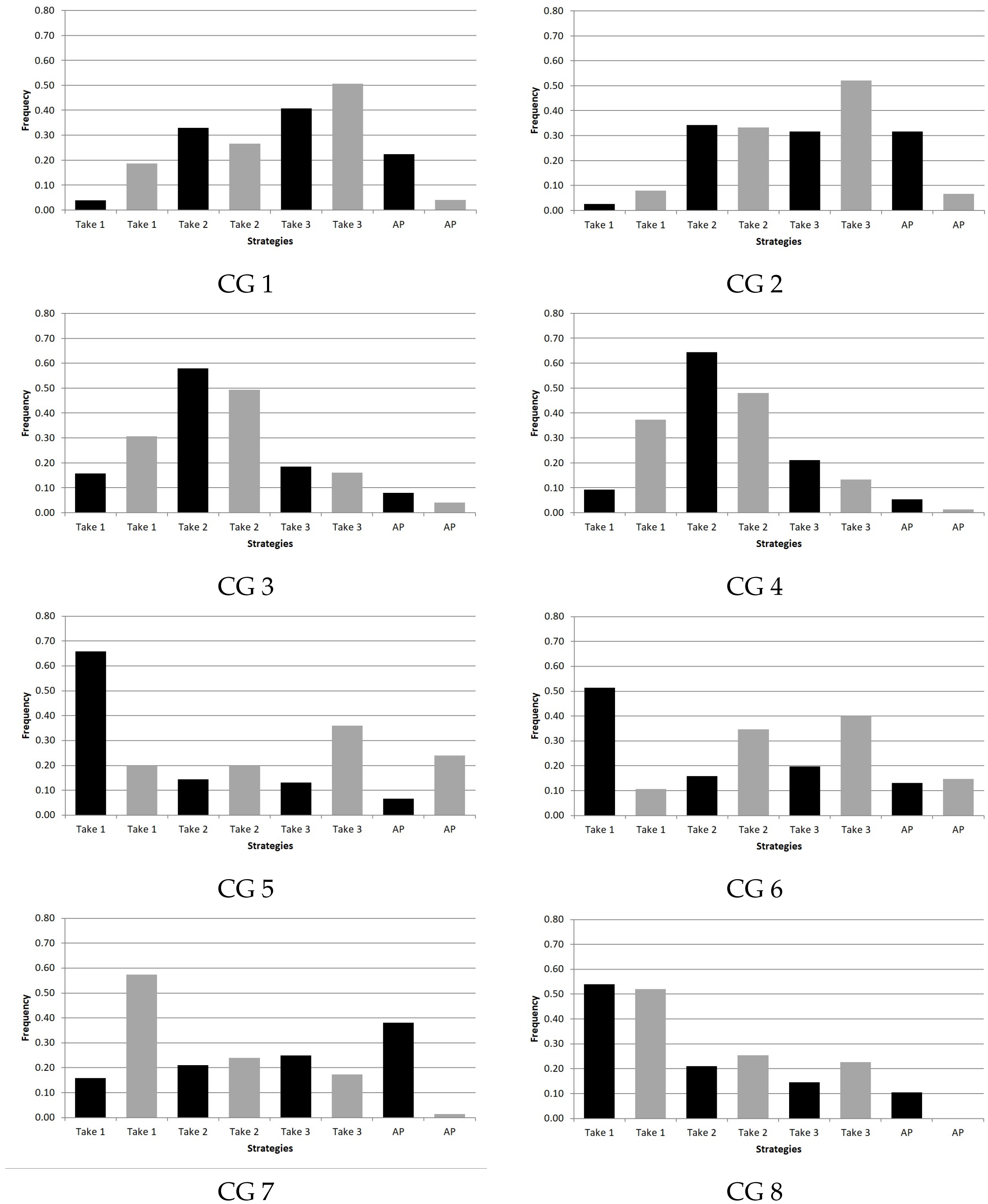
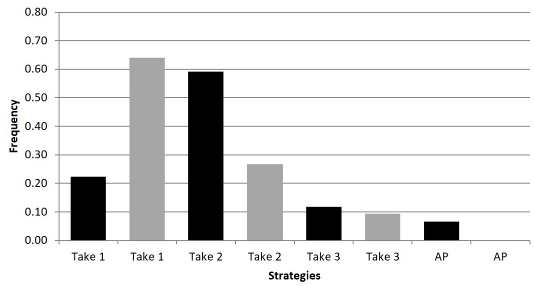
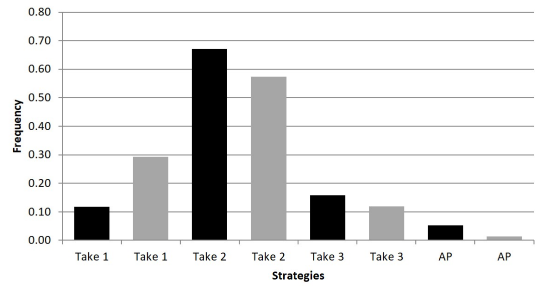


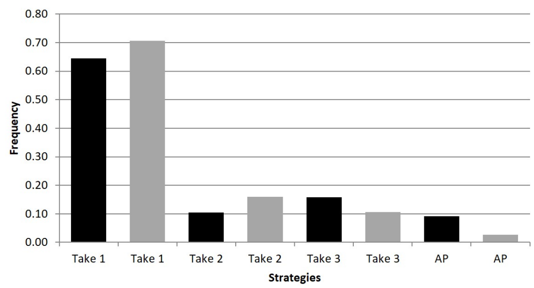
FIGURE 1.12: OBSERVED AGGREGATE BEHAVIOR IN CGS 1-8.



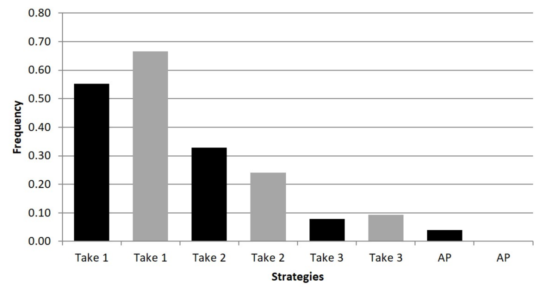
CG 9



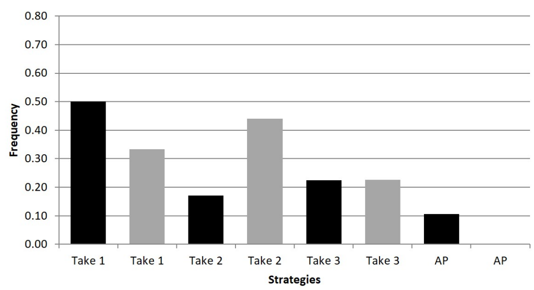
CG 10



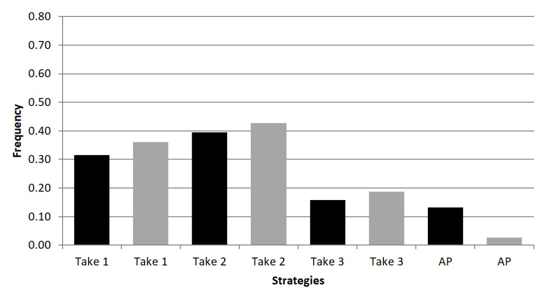
CG 11



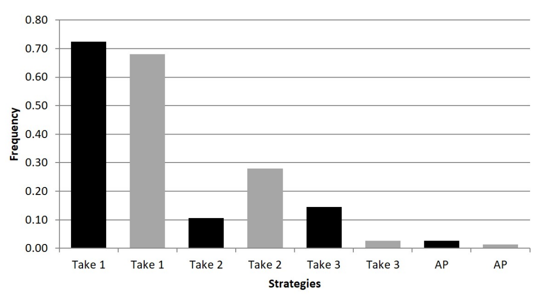
CG 12



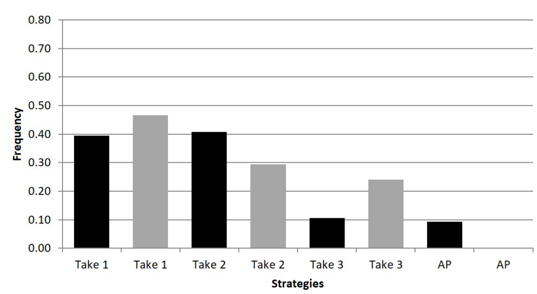
CG 13



CG 14



CG 15



CG 16

FIGURE 1.13: OBSERVED AGGREGATE BEHAVIOR IN CGS 9-16.

TABLE 1.13: CONSISTENCY WITH A PARTICULAR BEHAVIORAL MODEL FOR DIFFERENT CRITERIA

	SPNE	*A*(γ=.22)	*IA*(ρ=.08,σ=.55)	*A*	*IA*	*PE*	*O*	*L1*	*L2*	*L3*	*QRE*(0.38)
0	151	151	151	151	151	151	151	151	151	151	151
1	142	144	142	150	147	151	124	151	149	149	151
2	139	141	139	95	133	124	92	150	148	138	149
3	128	127	127	54	117	78	57	148	145	128	142
4	108	113	108	25	63	35	39	134	136	117	129
5	96	95	94	13	32	10	33	116	123	101	96
6	79	80	77	8	12	7	19	100	110	81	77
7	65	64	62	6	5	3	14	79	85	63	48
8	49	50	47	5	2	3	8	61	53	46	19
9	39	38	36	4	0	2	6	34	24	30	4
10	29	26	26	3	0	2	4	26	9	16	0
11	19	16	17	2	0	0	4	15	4	5	0
12	12	7	12	0	0	0	4	5	2	1	0
13	6	2	5	0	0	0	4	3	0	0	0
14	4	1	2	0	0	0	3	1	0	0	0
15	1	0	0	0	0	0	3	0	0	0	0
16	1	0	0	0	0	0	3	0	0	0	0

Notes: THE TABLE REPORTS THE NUMBER OF SUBJECTS (OUT OF 151) THAT COMPLY WITH EACH OF THE BEHAVIORAL MODELS, LISTED IN THE FIRST ROW, FOR DIFFERENT NUMBER OF GAMES, AS LISTED IN THE FIRST COLUMN. WHEN THE COMPLIANCE CRITERION IS LOW, I.E. ONE GAME, THEN THE NUMBER OF SUBJECTS THAT COMPLY WITH EACH OF THE BEHAVIORAL NUMBERS IS CLOSE TO 151. WHEN THE COMPLIANCE CRITERION IS HIGH, I.E. 15 GAMES, THEN THE NUMBER OF SUBJECTS THAT COMPLY WITH EACH OF THE BEHAVIORAL MODELS IS LOWER.

TABLE 1.14: ESTIMATION RESULTS WITH TWO QRE TYPES

Type	Original Results		Two QRE	
	p_k	ε_k, λ	p_k	ε_k, λ
	(1)	(2)	(3)	(4)
SPNE	0.08	0.31	0.09	0.06
O	0.03	0.06	0.03	0.60
L1	0.31	0.60	0.31	0.66
L2	0.21	0.66	0.21	0.62
L3	0.11	0.62	0.10	0.76
QRE 1	0.27	0.42	0.22	0.38
QRE 2			0.06	0.32

Notes: THE TABLE REPORTS THE ESTIMATION RESULTS FOR THE UNIFORM ERROR SPECIFICATION AND THE RESTRICTED MODEL WHEN ONE UNIQUE *QRE* MODEL IS CONSIDERED, COLUMNS 1-2, AND WHEN TWO *QRE* BEHAVIORAL MODELS ARE ALLOWED, COLUMNS 3 AND 4. COLUMNS 1 AND 3 PRESENT THE ESTIMATION RESULTS ON THE FREQUENCIES FOR EACH OF THE BEHAVIORAL MODEL. COLUMNS 2 AND 4 PRESENT THE ESTIMATION RESULTS FOR THE ERROR PARAMETERS.

TABLE 1.15: SEPARATION OF GANG OF FOUR MODEL WITH OTHER BEHAVIORAL MODELS CONSIDERED, FOR DIFFERENT VALUES OF q.

	SPNE	*A*	*IA*	*PE*	*O*	*L1*	*L2*	*L3*	*QRE*(λ=.38)
$q = 0.00$	0.91	0.59	0.83	0.47	0.38	0.44	0.70	0.95	0.81
$q = 0.10$	0.91	0.59	0.83	0.47	0.38	0.44	0.70	0.95	0.81
$q = 0.20$	0.80	0.54	0.82	0.47	0.38	0.52	0.56	0.82	0.67
$q = 0.30$	0.78	0.59	0.82	0.53	0.44	0.50	0.50	0.80	0.66
$q = 0.37$	0.78	0.67	0.82	0.58	0.53	0.41	0.48	0.80	0.66
$q = 0.40$	0.78	0.66	0.78	0.58	0.53	0.41	0.47	0.80	0.66
$q = 0.50$	0.78	0.67	0.78	0.60	0.56	0.41	0.42	0.80	0.63
$q = 0.60$	0.73	0.66	0.74	0.58	0.56	0.44	0.42	0.77	0.63
$q = 0.70$	0.70	0.68	0.72	0.59	0.61	0.47	0.41	0.70	0.63
$q = 0.80$	0.66	0.70	0.71	0.58	0.64	0.51	0.43	0.67	0.58
$q = 0.90$	0.65	0.73	0.64	0.65	0.76	0.58	0.48	0.58	0.59
$q = 1.00$	0.00	0.88	0.58	0.87	1.00	0.72	0.66	0.55	0.31

Notes: THE TABLE REPORTS THE SEPARATION RATES BETWEEN THE GANG OF FOUR TYPE AND EACH OF THE BEHAVIORAL MODELS LISTED IN THE FIRST ROW FOR DIFFERENT VALUES OF q. THE MINIMUM SEPARATION IS 0, WHEN TWO BEHAVIORAL MODELS PREDICT EXACTLY THE SAME STRATEGY FOR EACH OF THE PLAYER ROLES AND EACH OF THE CENTIPEDE GAMES. THE MAXIMUM SEPARATION RATE IS 1 WHEN TWO BEHAVIORAL MODELS PREDICT A DIFFERENT STRATEGY FOR EACH OF THE PLAYER ROLES AND EACH OF THE CENTIPEDE GAMES.

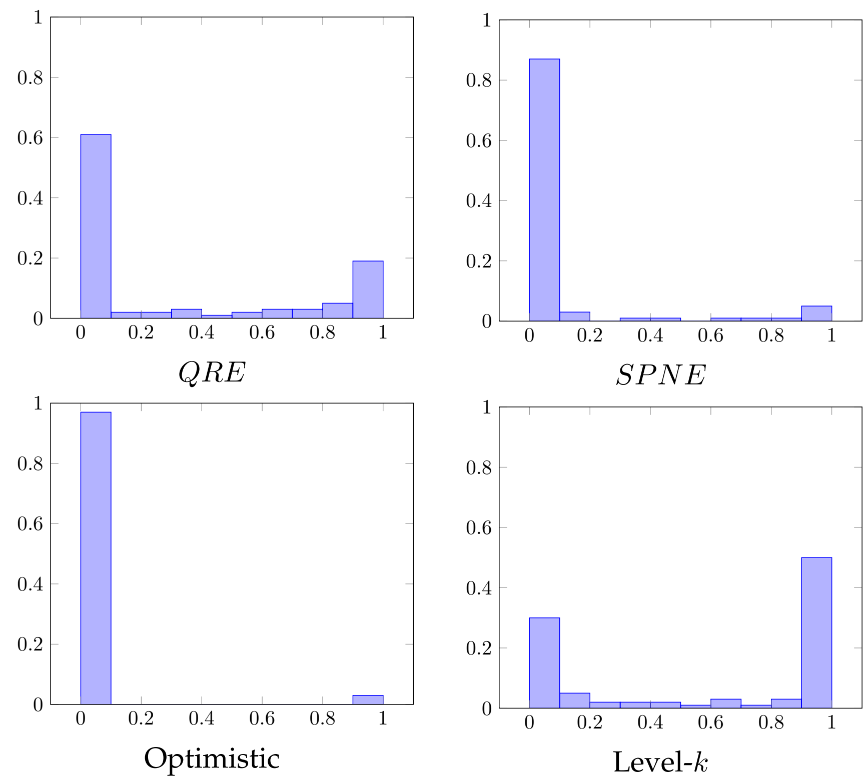


FIGURE 1.14: DISTRIBUTION OF PER-SUBJECT POSTERIOR PROBABILITIES OF BELONGING TO EACH MODEL, COMPUTED FOR THE REDUCED MODEL WITH THE UNIFORM-ERROR SPECIFICATION (3-4) IN TABLE 6A. THE TABLE SHOWS THAT A VAST MAJORITY OF SUBJECTS IS CLASSIFIED WITH PROBABILITY CLOSE TO 1 TO ONE UNIQUE RULE, SUGGESTING A CLEAN SEGREGATION OF BEHAVIORAL TYPES IN OUR DATA.

TABLE 1.16: ESTIMATION RESULTS WITH 15 IN-SAMPLE GAMES. OUT-SAMPLE CGS 1-8.

	Out-sample CG							
Type	1	2	3	4	5	6	7	8
SPNE (p_k)	0.10	0.07	0.09	0.09	0.07	0.08	0.09	0.09
	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
O (p_k)	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
L1 (p_k)	0.30	0.31	0.31	0.34	0.30	0.29	0.25	0.31
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.05)	(0.05)	(0.06)
L2 (p_k)	0.13	0.20	0.17	0.18	0.26	0.25	0.22	0.19
	(0.06)	(0.06)	(0.06)	(0.05)	(0.07)	(0.06)	(0.05)	(0.05)
L3 (p_k)	0.08	0.12	0.09	0.09	0.09	0.05	0.03	0.15
	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)	(0.06)
QRE (p_k)	0.36	0.27	0.30	0.28	0.25	0.31	0.39	0.24
	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)	(0.06)	(0.05)
SPNE (ε_k)	0.31	0.24	0.35	0.33	0.26	0.27	0.33	0.33
	(0.09)	(0.14)	(0.08)	(0.08)	(0.07)	(0.06)	(0.07)	(0.09)
O (ε_k)	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.07
	(0.13)	(0.09)	(0.10)	(0.14)	(0.11)	(0.08)	(0.14)	(0.14)
L1 (ε_k)	0.60	0.60	0.61	0.65	0.60	0.58	0.57	0.59
	(0.04)	(0.10)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.04)
L2 (ε_k)	0.60	0.65	0.68	0.63	0.59	0.60	0.64	0.66
	(0.11)	(0.17)	(0.12)	(0.05)	(0.05)	(0.05)	(0.08)	(0.12)
L3 (ε_k)	0.89	0.62	0.62	0.65	0.79	0.92	0.99	0.58
	(0.17)	(0.05)	(0.15)	(0.13)	(0.17)	(0.18)	(0.19)	(0.10)
QRE (λ)	0.32	0.43	0.41	0.43	0.44	0.38	0.25	0.42
	(0.09)	(0.08)	(0.07)	(0.11)	(0.11)	(0.09)	(0.08)	(0.14)

Notes: THE TABLE REPORTS THE ESTIMATION RESULTS FOR THE UNIFORM ERROR SPECIFICATION AND THE RESTRICTED MODEL WHEN ONE OF THE CENTIPEDE GAMES HAS BEEN TAKEN OUT (THE ONE LISTED IN THE FIRST ROW). A PARTICULAR NUMBER IN ROW i AND COLUMN j, THE TABLE SHOWS THE ESTIMATED COEFFICIENT ON THE PARAMETER SPECIFIED IN ROW i WHEN THE GAME IN COLUMN j WAS TAKEN OUT. THE NUMBER IN PARENTHESIS PRESENT

TABLE 1.17: ESTIMATION RESULTS WITH 15 IN-SAMPLE GAMES. OUT-SAMPLE CGS 9-16

	Out-sample game							
Type	9	10	11	12	13	14	15	16
SPNE (p_k)	0.08	.011	0.09	0.09	0.07	0.07	0.04	0.08
	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
O (p_k)	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
L1 (p_k)	0.28	0.33	0.30	0.30	0.30	0.34	0.29	0.30
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)	(0.05)
L2 (p_k)	0.23	0.18	0.21	0.18	0.21	0.20	0.25	0.23
	(0.07)	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)	(0.06)	(0.06)
L3 (p_k)	0.14	0.05	0.03	0.10	0.15	0.10	0.15	0.10
	(0.06)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)
QRE (p_k)	0.25	0.30	0.35	0.30	0.24	0.27	0.23	0.26
	(0.05)	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)
SPNE (ε_k)	0.32	0.32	0.34	0.34	0.29	0.30	0.19	0.31
	(0.10)	(0.06)	(0.08)	(0.09)	(0.08)	(0.07)	(0.11)	(0.11)
O (ε_k)	0.07	0.07	0.05	0.04	0.07	0.07	0.05	0.07
	(0.10)	(0.10)	(0.15)	(0.15)	(0.20)	(0.16)	(0.11)	(0.13)
L1 (ε_k)	0.59	0.62	0.60	0.60	0.59	0.62	0.61	0.56
	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
L2 (ε_k)	0.65	0.66	0.65	0.66	0.62	0.63	0.66	0.69
	(0.09)	(0.09)	(0.06)	(0.06)	(0.08)	(0.08)	(0.06)	(0.08)
L3 (ε_k)	0.58	0.61	0.99	0.69	0.60	0.59	0.65	0.55
	(0.12)	(0.16)	(0.15)	(0.12)	(0.12)	(0.15)	(0.08)	(0.11)
QRE (λ)	0.43	0.26	0.26	0.27	0.43	0.42	0.58	0.38
	(0.15)	(0.10)	(0.10)	(0.13)	(0.14)	(0.14)	(0.14)	(0.12)

Notes: THE TABLE REPORTS THE ESTIMATION RESULTS FOR THE UNIFORM ERROR SPECIFICATION AND THE RESTRICTED MODEL WHEN ONE OF THE CENTIPEDE GAMES HAS BEEN TAKEN OUT (THE ONE LISTED IN THE FIRST ROW). A PARTICULAR NUMBER IN ROW *i* AND COLUMN *j*, THE TABLE SHOWS THE ESTIMATED COEFFICIENT ON THE PARAMETER SPECIFIED IN ROW *i* WHEN THE GAME IN COLUMN *j* WAS TAKEN OUT. THE NUMBER IN PARENTHESIS PRESENT THE STANDARD ERRORS.

TABLE 1.18: THE PERCENTUAL IMPROVEMENT OF THE LOG-LIKELIHOODS EXPLAINING BEHAVIOR OUT-OF-SAMPLE, WITH RESPECT TO RANDOM BEHAVIOR. EACH COLUMN CORRESPONDS TO ONE OUT-OF-SAMPLE GAME; THE ROWS LIST THE DIFFERENT MODELS. LEVEL-k CORRESPONDS TO A MIXTURE OF $L1$, $L2$, AND $L3$.

	Game predicted									
	1	2	3	4	5	6	7	8	9	10
Mixture	0.05	0.12	0.05	0.13	0.13	0.04	0.10	0.11	0.17	0.15
QRE	-0.60	-1.00	-0.06	0.17	0.13	-0.08	-0.22	0.13	0.19	0.15
SPNE	-0.10	-0.14	-0.04	-0.04	0.05	0.00	0.02	0.09	0.05	-0.05
O	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Lk	0.04	0.05	0.07	0.10	0.08	0.06	0.03	0.04	0.21	0.14

	11	12	13	14	15	16	Mean	St.dev.	St.Er.
Mixture	0.18	0.22	0.09	0.08	0.24	0.13	0.12	0.06	0.01
QRE	0.09	0.18	0.11	0.08	0.32	0.12	-0.02	0.34	0.08
SPNE	0.14	0.12	0.05	0.01	0.15	0.05	0.02	0.08	0.02
O	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Lk	0.18	0.22	-0.10	-0.04	0.28	0.03	0.09	0.10	0.02

Notes: THE TABLE REPORTS THE PERCENTUAL IMPROVEMENT OF THE LOG-LIKELIHOODS EXPLAINING BEHAVIOR OUT-OF SAMPLE. THE BENCHMARK MODEL IS RANDOM UNIFORM BEHAVIOR. EACH COLUMN CORRESPONDS TO THE CASE IN WHICH ONE GAME IS TAKEN OUT. THE ROWS LIST THE DIFFERENT BEHAVIORAL MODELS. LEVEL-K CORRESPONDS TO A MIXTURE OF L1, L2, AND L3.

1.8 APPENDIX B: INSTRUCTIONS IN ENGLISH (ORIGINAL IN SPANISH)

THANK YOU FOR PARTICIPATING IN OUR EXPERIMENT!

This is an experiment, so there is to be no talking, looking at what other participants are doing or walking around the room. Please, turn off your phone. If you have any questions or you need help, please raise your hand and one of the researchers will assist you. Please, do not write on these instructions. If you fail to follow these rules, YOU WILL BE ASKED TO LEAVE THE EXPERIMENT AND YOU WILL NOT BE PAID. Thank you.

The University of the Basque Country has provided the funds for this experiment. You will receive 3 Euros for arriving on time. Additionally, if you follow the instructions correctly you have the chance of earning more money. This is a group experiment. Different participants may earn different amounts. How much you can win depends on your own choices, on other participants choices, and on chance.

No participant can identify any other participant by his/her decisions or earnings in the experiment. The researchers can observe each participant earnings, but they will not associate your decisions with the name of participant name.

During the experiment you can win experimental points. At the end, these experimental points will be converted into cash at a rate of 1 experimental point = 0.10 euros. Everything you earn will be paid in cash, in a strictly private way at the end of the experimental session.

Your final earnings will be the sum of the 3 Euros that you get just for participating and the amount that you earn during the experiment.

Each experimental point earns you 10 Euro cents, so 10 experimental points make 1 euro (10 x 0,10 = 1 Euro).

For example, if you obtain a total of 80 experimental points you will earn a total of 11 Euros (3 for participating plus 8 from converting the 80 experimental points into cash).

For example, if you obtain a total of 45 experimental points you will earn a total of 7.5 Euros (45 x 0.10 = 4.5 + 3 = 7.5)

For example, if you obtain a total of 190 experimental points you will earn a total of 22 euros (190 x 0.10 = 19 + 3 = 22)

Groups:

All participants in these sessions will be randomly divided in two different groups, the RED group and the BLUE group. Before you start making decisions, you will be informed if you are RED or BLUE, and you will maintain that status throughout the experiment. Each participant in the RED group will be randomly matched with a BLUE participant.

Game and options:

The experiment will consist of 16 games. In each game you will be matched randomly with a participant form other group. Nobody will know the identity of the participant with whom you are matched, nor will it be possible to identify him/her by his/her decisions during or after the experiment.

A description of the games follows. Every game has the same format, as represented in graphic form below.

If you are a RED participant, you will see this version of the game, where you can choose between the red circles only.

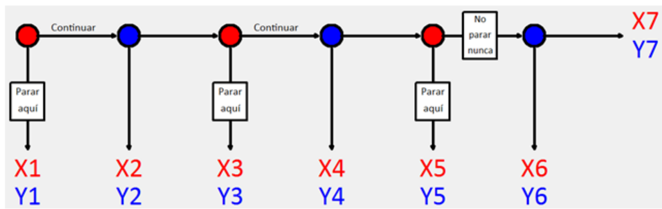


FIGURE 1.15: ROJO

If you are a BLUE participant, you will see this other version of the game, where you can choose between the blue circles only.

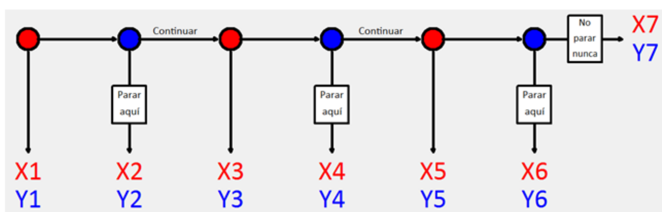


FIGURE 1.16: AZUL

In each game, each participant, RED or BLUE, has three chances to determine the earnings of both participants, in which he/she can choose one of two actions: stop or continue. In the graphic representation, the circles colored, RED and BLUE, identify which participant chooses. As the direction of the arrows shows, the game should be read from left to right. The earnings of the two participants are represented by X and Y, which in each circle of each game will be different numbers, representing experimental points.

The RED participant has the first chance to choose: he/she can "Stop here" or continue. In the graphic representation the downward arrow in the first RED circle represents “Stop” and the rightward arrow represents continue. If the RED participant chooses “Stop here”, the RED participant receives X1 and the BLUE participant Y1, and the game ends. If the RED participant does not choose “Stop here”, then the game continues and it is the BLUE participant who chooses in the first blue circle.

The BLUE participant can choose “Stop” or continue. In the graphic representation, the downward arrow in the first BLUE circle represents “Stop here” and the rightward arrow represents continue. If the BLUE participant chooses “Stop here” the RED participant receives X2 and the BLUE participant Y2, and the game ends. If the BLUE participant does not choose “Stop here”, then the game continues and it is the RED participant who chooses again in the second red circle

This description is repeated in the second red and blue circles, until the last chance is reached by the RED and BLUE participants.

In the last chance for the RED participant, represented by the third and last red circle, the RED participant can choose “Stop here” or “Never stop”. If the RED participant chooses “Stop here” the RED participant receives X5 and the BLUE participant Y5, and the game ends. If the RED participant chooses “Never stop”, then it is the BLUE participant who chooses for the last time.

In the last chance for the BLUE participant, represented by the third and last blue circle, the game ends. If the BLUE participant chooses "Stop here" each participant receives, X6 for the RED and Y6 for the BLUE, and the game ends. If the BLUE participant chooses “Never stop” the game ends and the quantities that the participants receive are X7 for the RED and Y7 for the BLUE.

In summary, in each game you have to choose where to stop or whether not to stop. That means that in each game you can choose between four different options: stop in the first circle of your color, stop in the second circle of your color, stop in the third circle of your color, or “Never stop”. The quantities change on each occasion and the participant who chooses "Stop here" before the other participant is the one who ends the game and determines the experimental points earned by both participants.

In order to make the game easier to understand, three examples are shown below. In the examples we show a choice by the RED participant (shaded in red) and one by the BLUE (shaded in blue) for a hypothetical game, and we identify the earnings for each participant.

Example 1:

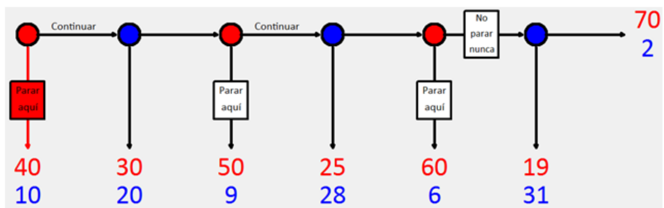


FIGURE 1.17: EJEMPLO 1(ROJO)

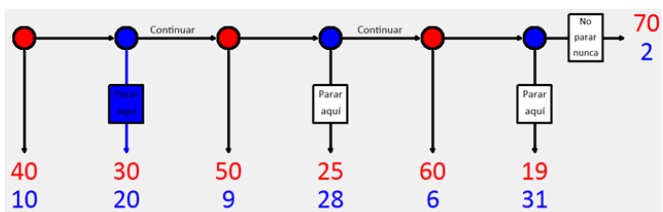


FIGURE 1.18: EJEMPLO 1(AZUL)

The RED participant has chosen "Stop"in the first red circle and the BLUE participant has chosen "Stop"in the first blue circle. Because the RED participant has stopped before the BLUE participant:

The RED participant earns: 40

The BLUE participant earns: 10

Example 2:

The RED participant has chosen "Stop"in the second red circle and the BLUE participant has chosen "Never stop". Because the RED participant has stopped before the BLUE participant:

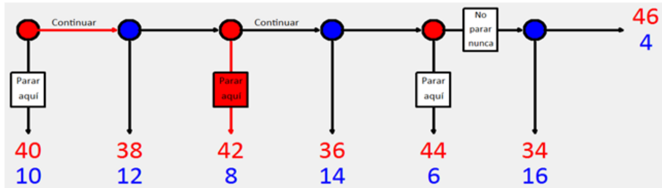


FIGURE 1.19: EJEMPLO 2(ROJO)

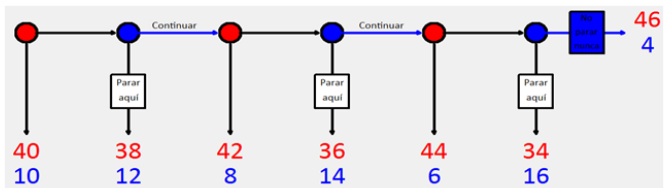


FIGURE 1.20: EJEMPLO 2(AZUL)

The RED participant earns: 42

The BLUE participant earns: 8

Example 3:

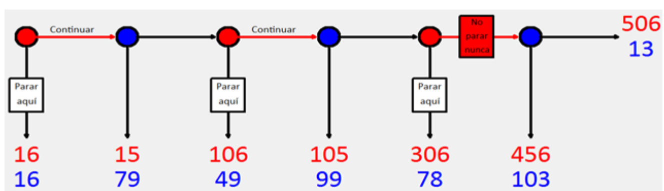


FIGURE 1.21: EJEMPLO 3(ROJO)

The RED participant has chosen “Never stop” and the BLUE participant has chosen stop in the third blue circle. Because the BLUE participant has stopped before the RED participant:

The RED participant earns: 456

The BLUE participant earns: 103

Note: These examples are just an illustration. The experimental points that appear are examples, i.e. they are not necessarily the ones that will appear in the 16 games. In addition, the examples ARE NOT intended to suggest how anyone should choose between the different options.

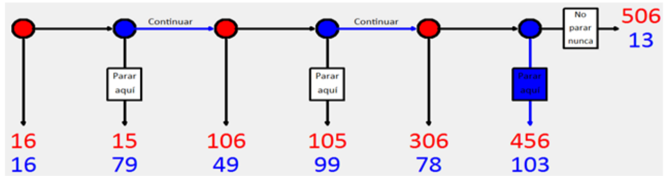


FIGURE 1.22: EJEMPLO 3(AZUL)

How the computer works: In each game, you will see 4 white boxes, one for each of your possible options. To choose an option, click on the corresponding box. When you have selected an option, the box will change color, as shown in the examples. This choice is not final: you can change it whenever you want by clicking on other box as long as you have not yet clicked the "OK" button that will appear in the bottom-left corner of each screen. Once you click "OK" your choice will be final and you will move on to the next game. You cannot pass on to the next game until you have chosen an option and have clicked "OK".

Earnings:

Once you have submitted your choices in the 16 games, the computer chooses three games at random for each participant for payment. You will be paid depending on the actions that you chose and the ones that the participant you were matched with chose in each of those three games.

Summary:

- The computer will choose randomly whether you are a RED or BLUE participant for the whole experiment.

- You will participate in 16 different games and in each of them you will be matched randomly with a participant of the other color.

- In each game, each participant can choose between four different options: stop in the first circle of his/her color, stop in the second circle of his/her color, stop in the third circle of his/her color or "Never stop". The quantities change on each occasion and the participant that chooses "Stop here" before the other participant is the one that ends the game and determines the experimental points for both participants.

- At the end, the computer will randomly choose 3 of the 16 games for each player, and you will be paid depending on the actions chosen by you and by the participant you were matched to in each of those three games.

The experiment will start shortly. If you have any questions or you need help, please, raise your hand and one of the researchers will help you.

Chapter 2

Do people minimize regret in strategic situations? A level-k comparison

2.1 Introduction

The Nash equilibrium is the benchmark solution concept in game theory. However, it fails to predict the observed behavior in many strategic situations of interest, especially if subjects face a situation for the first time (Goeree and Holt, 2001; Bosch-Domenech et al., 2002; Cabrera et al., 2007; Camerer, 2003). Many alternative explanations have been proposed and tested to explain the deviations from Nash equilibria. The purpose of this study is to provide an analysis of the relationships between two of those explanations, regret minimization and level-k thinking. We show that these two behavioral models prescribe the same behavior in many canonical experimental games and in a large number of games explicitly designed to discriminate between different theories of behavior. This comes as a surprise given these two behavioral models differ in their origins and, more importantly, in their underlying motivations. Hence, behavior rationalized by regret minimization may often be due to level-k reasoning and vice versa. We test whether people follow regret minimization and level-k thinking, and if so to what extent.

Regret is a negative feeling of sorrow or remorse for an action. Even though regret is only felt *ex post*, it can be anticipated and taken into account when evaluating different options. In terms of payoffs, regret can be defined as the difference between the payoff that a player could have earned if she chose the best response to the final state of the world or behavior of others, and the payoff that she actually obtained. Minimax regret (MR, henceforth) takes into account the anticipation of regret and applies the traditional minimax rule to the measure of regret (instead of payoffs), choosing the decision that minimizes the maximum regret that one could possibly obtain.[1] MR as a criterion for decision making under uncertainty exists at least since Savage (1951).[2] The subsequent literature has treated the notion of regret in different ways, for instance Loomes and Sugden (1982) and Bell (1982) incorporate it into the utility function. The role of regret has so far been explored or empirically tested in multiple situations across different fields.[3] Different models of MR proposed for strategic situations are addressed in Section 2.2.1.[4]

Level-k is a behavioral model that assumes that players best respond to their beliefs but have a simplified, non-equilibrium model of how other individuals behave (Stahl

[1]Note that this is different from the minimax decision rule (applied to payoffs), even in 2×2 games.

[2]There have been several axiomatizations of MR. See Milnor (1954) and Stoye (2011) for preference ordering, and Hayashi (2008) and Stoye (2011) for choice correspondence.

[3]Examples include bilateral bargaining (Linhartand Radner, 1989), price-setting (Renou and Schlag, 2010), strategic decision making (Halpern and Pass, 2012), treatment choice problems (Manski, 2004, 2007; Stoye, 2009), decision processes (Baron and Ritov, 1994, 1995), auctions (Ozbay and Ozbay, 2007; Ratan and Wen, 2016), and cultural differences in decisions (Giordani et al., 2010). See Wang and Boutilier (2003) for applications in computer science, Zeelenberg (1999) for applications in psychology, Loulou and Kanudia (1999) for applications in environmental issues, and Brehaut et al. (2003) for applications in medicine.

[4]Two strategic models based on MR are not considered in this study. Hyafil and Boutilier (2004) create an equilibrium for incomplete information games where agents minimize regret with respect to different agents' utility types and not choices of others as in our study. Renou and Schlag (2010) introduce an equilibrium that, unlike our approach, allows conjectures about the play of others. The former differs considerably from standard models of regret, while our aproach and the latter are identical under specific assumptions regarding the conjectures of players about the behavior of others.

and Wilson, 1994, 1995; Nagel, 1995). In this model, level-k types (*Lk*) represent different levels of sophistication and assume that their strategy is the most sophisticated (Costa-Gomes et al., 2001). The least sophisticated level, *L0*, takes each available action with the same probability. An *Lk* type is defined as an individual who best responds to a population of *Lk-1* individuals. As well as MR, level-k thinking has also proved successful in explaining non-equilibrium behavior.[5]

This chapter analyzes the relationships between the behavior predicted by MR and *L1*. We first show that both behavioral models prescribe the same behavior in a large number of games that have been explicitly designed to discriminate between different theories of behavior in strategic situations. We use experimental results from 17 experimental studies, providing a total of 277 different strategic decisions. MR and *L1* predict the same behavior in 83% of these 293 different decisions, and the two models predict entirely different actions in only 35 decisions (12%). These figures raise the question of whether MR and *L1* cannot be easily separated or whether no efforts have been made to separate them. In the work on initial responses that we have analyzed, Costa-Gomes and Crawford (2006) represent a notable exception as their guessing games separate these two theories of behavior very well. Therefore, our empirical analysis below re-examine the data from Costa-Gomes and Crawford (2006) to see if individual behavior can be explained by *L1* or MR.

Since the predictions of the two models coincide in so many cases, we analyze the link between them theoretically. We show that MR and *L1* always predict the same behavior in any 2×2 game. Moreover, the presence of dominant or dominated actions complicates the separation of these two behavioral models. Last, we use several examples to illustrate that the two models predict the same behavior in other strate-

[5] Examples include normal-form games (Stahl and Wilson, 1994, 1995; Costa-Gomes et al., 2001), beauty contests (Nagel, 1995; Costa-Gomes and Crawford, 2006), entry games (Camerer et al., 2004), auctions (Crawford and Iriberri, 2007), hide and seek games (Crawford and Iriberri, 2007), mechanism design (Crawford et al., 2009), asymmetric information games (Brown et al., 2012) and centipede games (Garcia-Pola et al., 2016). See Crawford et al. (2013) for a review.

gic situations of interest, well beyond simple 2×2 games and games with dominant strategies.

Due to the overlapping predictions of the two models in both theory and the experimental literature and given that both MR and *L1* have been proposed as explanations of behavior in games of varying structures (see Halpern and Pass, 2012, for the former and Crawford et al., 2013, for the latter), we proceed as follows. First, we revisit the data from Costa-Gomes and Crawford (2006). As mentioned above, this is the only study in the literature analyzed that enables the predictions of MR to be separated successfully from those of *L1*. Second, we propose an experiment explicitly designed to separate the predictions of these two theories of behavior, consisting of a series of simple normal-form games with three particular features. First, the games are designed such that different models of MR predict the same action. This enables a clean test to be conducted of the idea of MR *vis-à-vis* other theories. Second, we make the incentives to follow the action prescribed by each behavioral model as large as possible. This provides incentives for subjects to behave in line with their behavioral type and decreases the probability of making a mistake. Finally, and most importantly, the games are designed such that the two behavioral models are systematically separated, which is the main objective of the experiment.

The advantage of using the data from both experiments is that we can test the ability of MR and *L1* to explain behavior in games with continuous, large strategy spaces from Costa-Gomes and Crawford (2006) and games with finite and small numbers of strategies presented in normal-form form as in our own experiment.

L1 explains a larger proportion of behavior in both experiments, but a non-negligible number of decisions are in line with MR. However, when subjects are required to be consistent in their behavior across different games we find little evidence for MR in the guessing games (4%) and no evidence for it in the data from our own experiment. In fact, the estimates suggest that no subject follows MR systematically in our normal-form games. These findings show that the relevance of MR as an explanation of behavior in strategic situations should be questioned.

The chapter is organized as follows. Section 2.2 formally introduces level-k thinking and MR models. Section 2.3 analyzes the relationships between the predictions of the two models, first theoretically and then using many games employed in the experimental literature. Section 2.4 describes the two data sets used to separate *L1* and MR and presents the results. Section 2.5 concludes.

2.2 Theoretical Framework

In this section, we first specify the notation and then introduce MR and level-k models.

Consider a game $G = (N, A, \vec{u})$ where $N = \{1, ..., n\}$ is the set of players and $A = A_1 \times \cdots \times A_n$ is the set of action profiles (pure strategy profiles). A_i is the action space of player i and the vector $\vec{a}$ is an action profile consisting of an action a_i for each player i, that is $\vec{a} = \{a_1, ..., a_n\}$. Let $\vec{a}_{-i}$ denote the action profile of all players except for player i and $u_i(\vec{a})$ denote the payoff obtained by player i if the action profile $\vec{a}$ is played.

Let $S = S_1 \times \cdots \times S_n$ denote the set of mixed strategy profiles where $S_i = \Delta(A_i)$ is the set of mixed strategies of player i. A mixed strategy s_i is a probability distribution $s_i \in \Delta(A_i)$, where $s_i(a_i)$ is the probability that an individual i playing s_i assigns to choosing a_i. $\vec{s}$ and $\vec{s}_{-i}$ are defined analogously and $u_i(\vec{s})$ is the standard expected utility of player i from a mixed strategy profile $\vec{s}$.

2.2.1 Minimax Regret

MR is a non-strategic decision rule in which people choose the strategy that minimizes the maximum regret that they could possibly experience. In contrast to the standard equilibrium approach, an individual following this rule does not need to know the behavior or payoffs of the other players. Formally, let $u^*(\vec{a}_{-i})$ be the maximal payoff an individual i can earn if her opponents play $\vec{a}_{-i} \in A_{-i}$:

$$u^*(\vec{a}_{-i}) = \max_{a_i \in A_i} u_i(a_i, \vec{a}_{-i}). \tag{2.1}$$

Define the regret of each action a_i given the actions played by the opponents ($\vec{a}_{-i}$) as the difference between the maximal payoff that a_i could yield to i and the actual payoff that i receives. That is,

$$regret_i(a_i|\vec{a}_{-i}) = u^*(\vec{a}_{-i}) - u_i(a_i, \vec{a}_{-i}) \geq 0. \tag{2.2}$$

Let $regret_i(a_i)$ be the maximum regret that player i could possibly obtain by choosing a_i for any possible behavior of the opponents:

$$regret_i(a_i) = \max_{\vec{a}_{-i} \in A_{-i}} regret_i(a_i|\vec{a}_{-i}). \tag{2.3}$$

Finally, we define $MR_i(A)$ as the set of actions that minimize the maximum regret:

$$MR_i(A) = \arg\min_{a_i \in A_i} regret_i(a_i). \tag{2.4}$$

Last, $MR(A) = MR_1(A) \times \cdots \times MR_n(A)$. This definition generalizes in a straightforward way for mixed strategies, where $MR_i(S)$ is the set of mixed strategies that a MR player i can choose from.[6]

Notice that MR can predict multiple strategies for each player. Iterated regret minimization (IRM hereafter) is a solution concept proposed by Halpern and Pass (2012) that involves the iterated deletion of strategies that do not minimize regret. IRM consists of using MR as a deletion operator, which, applied to a game once, eliminates

[6] In the literature, mixed strategies for MR are treated in two different ways. Renou and Schlag (2010) compute the regret of a mixed strategy as the corresponding convex combination of regrets of the pure strategies while Halpern and Pass (2012) treat them as introduced here. Both definitions predict the same behavior in the framework of this study.

all actions that do not minimize regret for all players, $MR^1(A) \subseteq A$. When MR is applied to the remaining strategies $MR^1(A)$, the resulting actions are $MR^2(A) = MR(MR^1(A))$ and so forth. The IRM solution $MR^{\infty}(A)$ is obtained by continuing with this process an infinite number of times. The set of actions that IRM prescribes for player i is

$$IRM_i(A) = MR_i^{\infty}(A) = \cap_k MR_i^k(A), \tag{2.5}$$

where $MR_i^1(A) = MR_i(A)$ and $MR_i^{k+1}(A) = MR_i(MR^k(A))$. Notice that, if the first iteration leaves only one action for each player, $IRM_i = MR_i$. For an example of how to apply MR and IRM to particular games, see Appendix A.

2.2.2 LEVEL-*k* THINKING

Level-k is a strategic behavioral model which assumes that individuals best respond to their beliefs but have a simplified non-equilibrium model of how other players behave. This rule is defined in a hierarchical way with different levels of sophistication and each agent assumes that their strategy is the most sophisticated. We follow Costa-Gomes, et al. (2001) and assume that a *Lk* type best responds to *Lk-1* players.[7] The hierarchy is specified on the basis of the *L0* type, which is the least sophisticated. We set the *L0* type as an individual who takes each available action with the same probability (Stahl and Wilson, 1994, 1995; Nagel, 1995; Costa-Gomes et al., 2001; Camerer et al., 2004).

Formally, let $r_i(A)$ be the mixed strategy for player i, who takes each action in A_i with the same probability. An *L1* player believes that the other players are *L0* types, and best responds to that belief. If the best response is not unique, an *L1* individual has the same probability of taking each best responding action. Let *L1*(A) denote the set of action profiles that can be played by *L1* players in the game G. That is, *L1*$_i(A) =$

[7] Alternatively, Stahl and Wilson (1994, 1995) and Camerer et al. (2004) define an *Lk* type as a player who believes that all other players are a distribution of all the lower levels. Our main focus is on *L1*, which is identical under both approaches.

$\arg\max_{a_i \in A_i}\{u_i(a_i, \vec{r}_{-i}(A_{-i}))\}$. In general:

$$Lk_i(A) = \arg\max_{a_i \in A_i}\{u_i(a_i, \vec{r}_{-i}(Lk\text{-}1_{-i}(A)))\} \tag{2.6}$$

$Lk(A)$ is the set of pure strategy profiles in A that can be played by *Lk* players in the game G. For an example of how to apply *L1* to particular games, see Appendix A.

2.3 Relationship between Minimax Regret and Level-*k* Thinking

In this section we analyze the similarities in the predictions of MR and $L1$. First, we show formally in which games the predictions of the two behavioral models coincide. Second, we analyze games from 17 experimental studies to show that both behavioral models predict the same behavior in many games applied in the literature, which is quite surprising given they differ significantly in their underlying motivations.

2.3.1 Theoretical Approach

Let $WD(S)$ denote the set of weakly dominant strategies in the game G with $WD_i(S) = \{s_i \in S_i : u_i(s_i, \vec{s}_{-i}) \geq u_i(s_i', \vec{s}_{-i}) \; \forall s_i' \neq s_i$ and $\forall \vec{s}_{-i} \in \vec{S}_{-i}\}$. Let $d(S)$ denote the set of strictly dominated strategies in the game G with $d_i(S) = \{s_i \in S_i : u_i(s_i, \vec{s}_{-i}) < u_i(s_i', \vec{s}_{-i}) \; \forall \vec{s}_{-i} \in \vec{S}_{-i}$ for at least one $s_i' \neq s_i\}$. Proofs of the propositions can be found in Appendix B.

Proposition 1.

(i) If $WD(S) \neq \emptyset$, then $WD(S) = MR(S) = L1(S)$.

(ii) $\forall s \in d(S), s \notin MR(S)$ and $s \notin Lk(S) \; \forall k$.

Proposition 2.

$L1(A) = MR(A)$ in any 2×2 game.

These two propositions restrict the types of game suitable for separating MR from $L1$. Part (i) of the Proposition 1 shows that if there are weakly dominant strategies in a game G, those strategies and only those strategies are the ones predicted by both MR and $L1$. Part (ii) of Proposition 1 shows that a dominated strategy cannot be predicted by MR or level-k.[8] Proposition 2 shows that both models predict the same behavior in any 2×2 games.

Below, we show that overlapping predictions of MR and level-k arise in other strategic settings. As an illustration, we analyze four specific games: the Traveler's Dilemma, Bertrand competition, the Nash bargaining game and the 11-20 money request game. These games are chosen for three reasons. First, the propositions do not provide any result regarding these games. Second, each of them has a large action space, which *a priori* might make it even more surprising that $L1$ and MR make the same predictions. Last, the behavior observed in these three games has been used as evidence supporting either level-k, MR or both (Brañas-Garza et al., 2011; Arad and Rubinstein, 2012; Halpern and Pass, 2012; Baghestanian, 2014).

Example 1: Traveler's Dilemma. The Traveler's Dilemma models a situation in which an airline company loses two identical suitcases belonging to two different travelers. The airline does not know the value of the suitcases, so it offers the travelers the following compensation system. The airline separates the two travelers and asks each the value of their suitcase. Each traveler can claim any positive integer $m \in [\underline{a}, \overline{a}]$. If both ask for the same amount, $m = m'$, each receives the amount requested. However,

[8] A weakly dominated strategy cannot be prescribed by any level-k, but it can be predicted by MR. This may occur when both the weakly dominated strategy, s_i^d, and the strategy that dominates strategy s_i^d, s_i^D, minimize the maximum regret, which can happen if the following conditions are met. First, s_i^d must have the same payoff as s_i^D for at least one strategy profile of the opponents ($\vec{s}_{-i}$). Therefore, $regret_i(s_i^d|\vec{s}_{-i}) = regret_i(s_i^D|\vec{s}_{-i})$. Second, $regret_i(s_i^d) = regret_i(s_i^D) = regret_i(s_i^d|\vec{s}_{-i}) = regret_i(s_i^D|\vec{s}_{-i})$. Last, s_i^d and s_i^D must be two of the strategies that minimize the maximum regret, that is s_i^d and $s_i^D \in \arg\min_{s_i \in S_i} regret_i(s_i) = MR_i(S)$.

if they ask for different amounts, $m < m'$, the smaller amount m is treated as the true value of the suitcase, and both travelers receive m plus/minus an integer $r > 1$ as follows: whoever asks for lower amount gets $(m + r)$ while the other player gets $(m - r)$. Iterative elimination of dominated strategies leaves us with the sole Nash equilibrium of this game, which is to ask for $\underline{a}$ for any value of $\underline{a}$, $\overline{a}$ and p. Nevertheless, the predictions of MR and *L1* differ depending on the parameter values:

(a) If $\overline{a} - \underline{a} \leq r$, then $L1(A) = MR(A) = IRM(A) = \{\underline{a}\}$.

(b) If $\overline{a} - \underline{a} = r + 1$, then $L1(A) = IRM(A) = \{\underline{a}\} \subset MR(A) = \{\underline{a}, \underline{a} + 1\}$.

(c) If $r + 2 \leq \overline{a} - \underline{a} < 2r$, then $L1(A) = \{\underline{a}\} \neq MR(A) = IRM(A) = \{\underline{a} + 1\}$.

(d) If $2r \leq \overline{a} - \underline{a}$, then $IRM(A) = \{\underline{a} - 2r + 1\} \subset L1(A) = \{\underline{a} - 2r, \underline{a} - 2r + 1\} \subset MR(A) = [\overline{a} - 2r, \overline{a}]$.

Observe that the predictions of MR and *L1* cannot be separated in Traveler's dilemmas under conditions (a), (b) or (d). Nevertheless, most studies apply parameters that meet these conditions (see Basu, 1994; Capra et al., 1999;[9] Goeree and Holt, 2001; Becker et al., 2005; Rubinstein, 2006, 2007; Chakravarty et al., 2010). Hence, most studies cannot tell whether individuals are choosing according to *L1* or MR.

Example 2: Bertrand competition. Bertrand competition is a game in which two players representing firms that produce the same good choose a price. For the sake of simplicity, we assume that prices must be non-negative integers and that producing the good carries no cost. Each player chooses a price $p_i \in [0, \overline{a}]$ at which to sell the good. Each player gets a share of a fixed demand $d > 0$ depending on the prices chosen by the two firms. If $p_i = p_j = p$, each player obtains $(d/2) * p$. If a player chooses a higher price than their opponent, she obtains 0. If she chooses a price lower than the opponent, she obtains $d * p_i$. The Nash equilibria of this game are setting the prices (0, 0), (1, 1) or (2, 2), independently of the parameters. In contrast, the predictions of MR and *L1* depend on the particular value of $\overline{a}$. For any d:

[9] The conditions are satisfied in four out of the five cases tested in Capra et al. (1999).

(a) If $\overline{a}$ is odd, then $L1(A) = MR(A) = IRM(A) = \{\overline{a}/2 + 0.5\}$.

(b) If $\overline{a}$ is even, then $L1(A) = IRM(A) = \{\overline{a}/2\} \subset MR(A) = \{\overline{a}/2, \overline{a}/2 + 1\}$.

MR and *L1* predict the same behavior if $\overline{a}$ is odd, and *L1* prediction is a subset of MR prediction if $\overline{a}$ is even. Hence, *L1* and MR predictions cannot be fully separated.

Example 3: Nash bargaining game. In the Nash bargaining game, two agents share a surplus $\overline{a}$. Each player can claim a share of the surplus by choosing an integer $m \in [0, \overline{a}]$. If one player chooses m, the other m' and $m + m' \leq \overline{a}$, each receives the amount demanded. However, if $m + m' > \overline{a}$ both receive 0. Each pair of amounts that satisfy $m = \overline{a} - m'$ is a Nash equilibrium, while the predictions of MR and *L1* differ depending on the particular value of $\overline{a}$ as follows:

(a) If $\overline{a}$ is odd, then $L1(A) = MR(A) = IRM(A) = \{(\overline{a}/2) + 0.5\}$.

(b) If $\overline{a}$ is even, then $IRM(A) = \{\overline{a}/2\} \subset L1(A) = MR(A) = \{\overline{a}/2, (\overline{a}/2) + 1\}$.

This time, MR and *L1* predict the same behavior in any case and it is not possible to tell which model individuals are following.

Example 4: 11-20 money request game. In this game, two players claim an integer $m \in [11, 20]$. Each player receives the amount requested. However, a player receives 20 additional monetary units if she asks for exactly one unit less than the amount claimed by the other player, that is if $m = m' - 1$. This game has the particularity that the *L1* predicted choice is to claim 19 no matter whether the definition of $L0$ behavior is to randomize between all available actions or to choose any distribution in which 20 is the most probable strategy. The sole Nash equilibrium of this game is to request each amount with a probability of (0, 0, 0, 0, 0.25, 0.25, 0.20, 0.15, 0.10, 0.05). The predictions of the three models considered in this study are $L1(A) = IRM(A) = \{19\} \subset MR(A) = \{19, 20\}$. In this game, the predictions of *L1* and MR are not fully separated.

As shown by these four examples, *L1*, MR and IRM prescribe very similar behavior despite the relatively large strategy space of the considered games.

2.3.2 Experimental Literature

To illustrate further the overlapping predictions of MR and *L1*, we show how often the two behavioral models prescribe the same behavior in different games from 17 experimental studies. The common features of these studies are that they all aim to explain non-equilibrium behavior and that the data are available. In particular, we analyze whether, and if so to what extent, the predictions of MR and *L1* coincide in the different strategic situations provided in these studies.

Table 2.1 summarizes the results. The table reveals that the two models coincide in a large number of different games. Out of the total of 293 different strategic situations that we consider, MR and *L1* prescribe the same behavior in 243 (82.94%) decisions, and make predictions that are entirely separated in only 35 (11.95%). In the remaining 15 decisions the predictions of the two models overlap, coinciding in some actions and differing in others. In the 35 decisions in which the two predictions are entirely separate, 32% of subjects played the *L1* prediction while 20% played according to MR. Are subjects playing the actions prescribed by one of the models because these actions also coincide with the prediction of the Nash equilibrium? Out of the 35 decisions, the Nash equilibrium, *L1* and MR predict different behavior in only 21 cases. In those 21 decisions, 16% of subjects are observed to play the Nash equilibrium prediction, 22% the *L1* prediction, and 12% that of MR.

Is it possible to tell which model describes individual behavior best based on these data? The answer is no, for three reasons. First, individual behavior seems to be split between MR and *L1* predictions. In addition to the aggregate behavior explained above, in half of the 22 decisions in which the predictions are separated the prediction of one model is played more often than that of the other. Second, the incentives for a player following one of these rules are quantitatively weak: The minimal maximum regrets of the actions predicted by MR are close to the minimal maximum regrets of other actions, and the payoffs that a *L1* individual expects in choosing the *L1* pre-

TABLE 2.1: RELATIONSHIP BETWEEN THE PREDICTIONS OF MR AND *L1* IN STRATEGIC SITUATIONS FROM 17 EXPERIMENTAL STUDIES.

Research article	Different strategic situations	Size of Action space	Number of strategic situations in which $L1 \neq MR$	Number of strategic situations in which $L1 \cap MR = \emptyset$
Schotter et al. (1994)	6	3	2	1
Stahl and Wilson (1994)	10	3	1	0
Stahl and Wilson (1995)	12	3	1	1
Stahl (1999)	15	3	2	2
Costa-Gomes et al. (2001)	36	3	5	5
Goeree and Holt (2001)	8	2	0	0
	2	3	0	0
	1	4	0	0
	Traveler r=5	121	1	0
	Traveler r=180	121	0	0
Haruvy et al. (2001)	15	3	2	2
Di Guida and Devetag (2002)	30	3	9	5
Morgan (2002)	2	3	1	1
Becker et al. (2005)	Traveler r=2	99	1	0
Costa-Gomes and Crawford (2006)	16	200-800	13	13
Costa-Gomes and Weizscker (2008)	28	3	0	0
Selten and Chmura (2008)	24	2	0	0
Rey-Biel (2009)	40	3	8	3
Ivanov (2011)	9	3	2	2
	3	2	0	0
Arad and Rubinstein (2012)	11-20 game	10	1	0
Garcia-Pola et al. (2016)	32	4	1	0
Total	293		50	35

Notes: The first column lists the studies under consideration. The second column indicates the number of different strategic situations. If the games are symmetric this is equal to the number of games, but if they are asymmetric then it is equal to the number of games multiplied by the number of player roles in each game. Alternatively, this column shows the name of a game if the study has a game that is analyzed in the previous section of this chapter (Traveler = Traveler's Dilemma, 11-20 game = 11-20 money request game). The third column displays the number of actions available in the strategic situations indicated in the previous column. The fourth column shows the number of the strategic situations from the second column in which *L1* and MR predictions are not exactly the same. The last column indicates the number of strategic situations from the second column in which *L1* and MR predict entirely different actions.

dictions are close to those expected for other actions. Lastly, these data come from different subjects. The decisions by the same subjects in which the predictions are separated account for a proportion of the total choices that is too small for any analysis to be conducted on whether any subject follows one rule systematically in all games.

Despite these conclusions, Costa-Gomes and Crawford (2006; CGC, hereafter) is a noteworthy exception. 13 out of the 16 games from their paper are suitable for discriminating between *L1* and MR. Hence, to discriminate between these two models, the next section uses the data from CGC and propose a new experimental design to explicitly separate *L1* from MR.

2.4 EXPERIMENTAL ANALYSIS

Both MR and *L1* have been shown to predict behavior in a wide spectrum of games (see Halpern and Pass, 2012, for the former MR and Crawford et al., 2013, for the latter). This section analyzes two experiments which include two types of games which have very different characteristics but which are suitable for discriminating between these two models. First, we focus on guessing games with a large, continuous strategy space, then we propose a new set of normal-form games with a finite, relatively small strategy space.

2.4.1 TWO-PERSON GUESSING GAMES BY COSTA-GOMES AND CRAWFORD (2006)

CGC report an experiment in which 88 subjects play 16 different two-player guessing games with no feedback. In these games, each subject i has to make a guess between a lower limit a^i and an upper limit b^i. If subject i makes a guess x^i equal to the guess made by her opponent x^j times a target p^i, she obtains the maximum payoff. The closer the guess is to this optimum guess, the higher the payoff that the subject obtains. Specifically, the payoff in points is given by $s^i = max\{0, 200 - (|x^i - p^i * x^j|)\} +$

$max\{0, 100 - (|x^i - p^i * x^j|)/10\}$. Subjects were paid for 5 randomly chosen games, with an exchange ratio of 0.04 dollars per point. CGC vary the parameters of limits (a, b) and targets (p) across games to obtain a strong separability of the behavior predicted by different decision rules, and elicits initial responses to test which rule better explains the behavior of each subject.

TABLE 2.2: GUESSING GAMES, MODELS' GUESSES AND BEHAVIOR FOR PLAYER i

	Predictions			Compliance		
Game	NE	$L1$	MR	NE	$L1$	MR
(1)	(2)	(3)	(4)	(5)	(6)	(7)
1. $\alpha 1 \beta 2$	100	350	300	11	16	3
2. $\beta 1 \alpha 2$	100	150	175	14	16	17
3. $\beta 1 \gamma 2$	150	200	200	9	24	24
4. $\gamma 2 \beta 1$	300	350	400	39	17	9
5. $\gamma 4 \delta 3$	500	500	475	61	61	2
6. $\delta 3 \gamma 4$	650	520	520	23	23	23
7. $\delta 3 \delta 3$	900	780	645	21	23	3
8. $\delta 3 \delta 3$	900	780	645	18	25	3
9. $\beta 1 \alpha 4$	100	150	175	4	14	4
10. $\alpha 4 \beta 1$	150	500	325	3	27	2
11. $\delta 2 \beta 3$	300	350	465	4	14	1
12. $\beta 3 \delta 2$	390	780	645	1	14	2
13. $\gamma 2 \beta 4$	500	350	400	18	16	12
14. $\beta 4 \gamma 2$	750	600	600	7	25	25
15. $\alpha 2 \alpha 4$	350	210	225	10	17	2
16. $\alpha 4 \alpha 2$	500	450	325	5	25	1
$Total$				248	357	133

Notes: The name of each game in column (1) identifies the different parameters, the first two for player i and the last two for player j respectively. Limits: α for 100 and 500, β for 100 and 900, γ for 300 and 500, and δ for 300 and 900. Targets: 1 for 0.5, 2 for 0.7 3 for 1.3, 4 for 1.5. Columns (2) to (4) present the guess prediction of each model in each game, and columns (5) to (7) show the number of subjects (out of 88) that comply with each model in each game.

Column (1) in Table 2.2 enumerates the games, and columns (2) to (4) show the guesses predicted by NE, *L1* and MR in each game. Notice that in 12 out of 16 games the predictions of all three models are different. In contrast, in games 3, 6 and 14 *L1* and MR coincide while NE differs, whereas in game 5 NE and *L1* coincide while MR differs.

We now provide an intuition for why the guessing games provide a good separation between *L1* and MR. Formally, the *L1* ideal guess for player i for any parameter is $p^i * \left[a^j + b^j\right]/2$, and the *MR* ideal guess for player i is $max(a^i, p^i * a^j) + \left[min(b^i, p^i * b^j) - max(a^i, p^i * a^j)\right]/2$, which is usually different. In words, a *L1* subject calculates the middle value between the opponent's limits and applies her target to it. On the other hand, a MR subject first applies her target to the opponent's limits and then calculates the middle value. For example, consider a guessing game with $a = 100$, $b = 500$ and $p = 0.5$ for both players. A *L1* player thinks that her opponent randomizes between 100 and 500. She best responds by multiplying the target by the middle value between the limits of the opponent: $0.5 * 300 = 150$. In order to know what a MR player does, we multiply the target by the limits of the opponent: $0.5 * 100 = 50$ and $0.5 * 500 = 250$. A player cannot choose a guess below 100 so the guesses that generate the highest payoffs for a player are between 100 and 250. One of those exact guesses, 100 or 250, always generates the highest regret for any guess (because one of those two is the most distant from any guess and at the same time can potentially bring the highest payoff). Therefore, a player who minimizes maximum regret chooses the guess mid-way between those limits, minimizing the distance (in the value of the guess and, as a consequence, in payoff and regret) from both limits. Therefore she chooses: $(250 - 100)/2 + 100 = 175$.

Columns (5) to (7) report how many of the total of 88 comply with the prediction of each model in each game. 25.36% of the total of 1408 decisions comply with *L1* and only 9.45% with MR. 24.91% of the behavior the 13 games in which these two models are entirely separate comply with *L1* and 5.33% with MR. These figures suggest that

MR explains a considerably lower fraction of choices in the data. Nevertheless, these numbers provide no information regarding whether subjects systematically follow the predictions of each model across the different games.

To formally control for the consistency of subjects' behavior across games, we use mixture model analysis to estimate the distribution of the population across the three models. We adapt the maximum likelihood error-rate estimation from CGC to the population level analysis and restrict our analysis to the three models in the main analysis.[10] Let i denote the subjects ($i = \{1, ..., 88\}$), g the game ($G = \{1, 2, ..., 16\}$) and m the model considered (NE, *L1*, MR). Denote by a_g^i and b_g^i the lower and upper limits, and by x_g^i the guess of subject i in the game g.[11] Finally, let t_g^m be the guess predicted by model m.

Since the strategy space is continuous, we follow CGC and use a spike-logit error structure. In each game, each subject following model m makes a correct guess with probability $(1 - \varepsilon_k)$ and commits an error with probability ε_m, choosing a guess following a logistic distribution over the non-predicted options within the limits. We assume errors to be model-specific and identically and independently distributed across games and subjects.[12] The first assumption considers that it may be more cognitively demanding to follow some models than others, and may lead to larger error rates, while the second facilitates the statistical treatment of the data.

For player i in game g, let y be the guess of subject i's opponent and $S_g(x_g^i, y)$ be her expected monetary payoff, taking the expectation only over the selection of games that i is paid for. Let $f_g^m(y)$ be the density representing the beliefs that model m is best responding to. The expected payoff of player i in game g for model m's specific beliefs

[10]The estimation with all the models used in CGC can be found in the Appendix C.

[11]In the actual experiment, subjects were allowed to guess outside the limits. However, they were instructed that whenever they would guess outside these limits the computer would automatically adjust their guess to the nearest limit. For expositional purposes, we only focus on adjusted guesses and simply re-label them as guesses through this study.

[12]See Costa-Gomes et al. (2001), Iriberri and Rey-Biel (2013), Kovarik et al. (2018) or Garcia-Pola et al. (2016) for a similar approach.

is:

$$S_g^m(x_g^i) = \int_{100}^{900} S_g(x_g^i, y) f_g^m(y) dy. \tag{2.7}$$

To identify when a player makes a correct guess given that the strategy space is continuous, we consider guesses made by subjects that are within 0.5 points of the guess predicted by a model as a correct guess for that model. Let $U_g^{im} = \left[t_g^m - 0.5, t_g^m + 0.5\right] \cap \left[a_g^i, b_g^i\right]$ be the set of guesses that subject i can play in game g predicted by model m, and $V_g^{im} = \left[a_g^i, b_g^i\right] / U_g^{im}$ is the complement of U_g^{im} within the limits. The error density is then

$$d_g^m(x_g^i, \lambda_m) = \frac{exp\left[\lambda_m S_g^m(x_g^i)\right]}{\int_{V_g^{im}} exp\left[\lambda_m S_g^m(z)\right] dz} \tag{2.7}$$

for $x_g^i \in V_g^{im}$ and 1 elsewhere.

The value λ_m indicates the dispersion of erroneous guesses of a subject following m. If λ_m is 0, subjects following model m play randomly when making a mistake, while as λ_m tends to ∞ they play guesses closer to the ideal guess for model m with a higher probability and costlier guesses (further from the ideal guess) with a lower probability.

Let N^{im} be the set of games in which subject i makes a guess in V_g^{im}, and n^{im} the number of games in N^{im}. In each model m and game g, each subject has a probability $(1-\varepsilon_m)$ of making a guess $x_g^i \in U_g^{im}$, and a probability ε_m of making a guess $x_g^i \in V^i m_g$ with the corresponding density $d_g^m(x_g^i, \lambda_m)$. Therefore, the density of the guesses of a subject i following a particular model m is:

$$d^m(x^i, \varepsilon_m, \lambda_m) = (1-\varepsilon_m)^{(16-n^i m)} \varepsilon_m^{n^{im}} \prod_{g \in N^{im}} d_g^m(x_g^i, \lambda_m) \tag{2.7}$$

with an exception: when n^{im} equals to 0 or G, then $d^m(x^i, \varepsilon_m, \lambda_m) = 1$.

Denoting $p = (p_{NE}, p_{L1}, p_{MR})$ the probabilities corresponding to each model and adding up for all models and subjects yields the log-likelihood function of the whole sample:

$$L(p, \varepsilon_m, \lambda_m) = \sum_i ln\left[\sum_m p^m d^m(x^i, \varepsilon_m, \lambda_m)\right] \quad (2.7)$$

TABLE 2.3: CGC ESTIMATION RESULTS

	All 16 games		
Model	p_m	ε_m	λ_m
(1)	(2)	(3)	(4)
NE	0.33***	0.90***	0.30***
	(0.07)	(0.02)	(0.08)
L1	0.63***	0.88***	0.92***
	(0.07)	(0.03)	(0.14)
MR	0.04**	0.53**	0.00
	(0.02)	(0.23)	(0.28)

Notes: Columns (2), (3) and (4) contain the estimation results. Column (2) displays the estimated share of subjects who comply with each model $p = (p_{NE}, p_{L1}, p_{MR})$. Column (3) reports the corresponding estimated error parameter $\varepsilon = (\varepsilon_{NE}, \varepsilon_{L1}, \varepsilon_{MR})$. Column (4) reports the estimated dispersion of the error for each model $\lambda = (\lambda_{NE}, \lambda_{L1}, \lambda_{MR})$. Standard errors are shown below each estimated coefficient, in parenthesis. We report the significance levels (***$p < 0.01$, **$p < 0.05$, *$p < 0.10$) using bootstrapping with 500 replications (Efron and Tibshirani, 1994). Notice that the error rates are well behaved if they are close to zero and far from one (corresponding to random play), so we test whether each ε_m differs significantly from one (rather than zero)

Table 2.3 reports the estimation results. Observe that *L1* explains the behavior of 61% of the subjects, followed by NE which explains 33% and MR with only 4%. Therefore, only a small minority of subjects is best explained by MR. Moreover, the dispersion of the error of the MR model is the closest to random play, and that of *L1* gives the greatest probability of guesses closer to the model's predicted guess. Therefore MR is not only followed by only a few subjects but they are also the most imprecise when making mistakes.

For a better comparison with the original estimates in CGC, Appendix C presents two alternative models. Columns (2), (3) and (4) reproduces the estimates from Table 2.3 with all the types included in the original study by CGC (who disregard MR) while columns (5), (6) and (7) incorporate MR. In this last estimation, the proportion of NE is 19%, *L1* is the model that explains the highest proportion of the population and MR is no longer significantly different from 0%. This suggest that the 4% explained by MR in the previous estimation was just capturing behavior best explained by other models.[13]

In sum, these result show that, independently of the model specification, *L1* rationalizes a large part of the behavior of subjects in guessing games whereas MR explains the behavior of at best a negligible part of the population.

2.4.2 Normal-Form Games

This section introduces an experiment explicitly designed to separate the predictions of MR and *L1* in normal-form games with a discrete strategy space and presents the results.

Experimental Design and Procedures

We recruited 115 participants in three different sessions in May 2017, using the ORSEE recruiting system (Greiner, 2015).[14] The sessions were conducted using z-Tree software (Fischbacher, 2007) at the Laboratory of Experimental Analysis (Bilbao Labean; http://www.bilbaolabean.com) of the University of the Basque Country.

Subjects were given detailed instructions giving examples of games, which were different from those used in the experiment, of how they could make decisions and of

[13] We also perform the analysis at an individual level just as in CGC. Only 5 subjects are best explained by MR when including our main 3 models, and also when including all CGC types.

[14] The matching mechanism described below did not require an even number of participants.

the payment method. An English translation of the instructions can be found in Appendix E. The instructions were read aloud. Subjects were allowed to ask any question they might have during the instructions. Since the ability to anticipate regret is key to incorporating regret into the decision making process (Zeelenberg, 1999), we clearly stated in the instructions that feedback would be provided. Moreover, at the end of the instruction process, subjects had to answer several questions on the computer screen before they could proceed, and one of the questions referred to the feedback. By answering all the questions correctly, subjects guaranteed that they understood the instructions, including the fact that they would receive feedback about their behavior at the end of the experiment.

At the beginning of the experiment, the computer assigned subjects randomly to the roles of either a row player or a column player, and those roles were maintained throughout the experiment. Since we were mainly interested in the decision of row players, only two subjects were assigned to the role of column player in each session, and we focused on the behavior of row players (see e.g. Ivanov, 2010, for the same approach). Both roles were visualized from the row player perspective, so no player had information about the role to which they were assigned. Subjects were assured that they were playing against a real player. Then every subject participated in 16 one-shot normal-form games as shown in Figure 2.1. All numbers displayed in the games were expressed in Euros. Subjects played the games one by one in a random order, which was the same for all subjects. They had no possibility of leaving a game without making a decision, and they never knew which games they would face in later stages. No feedback was provided until all 16 choices had been made. Subjects had no time limits for making their decisions, and participants were not obliged to wait for others while making their choices in any game.

Subjects were shown the game in normal-form and made choices by clicking on a square box placed next to each action. Subjects could change their decision as many times as they wanted, but had to confirm their decision by clicking on the “OK" button in the corner of the screen before they were allowed to proceed to the next game. Subjects could never go back to previous games.

Once all players had submitted their choices in all the games, the computer randomly selected two different games for each subject for payment; that is, different participants could be paid for different games. For each player, the computer also randomly selected two opponents, one for each of those two selected games. That is, any row player could have served as an opponent for both column players, and since there were only two column players each of them served as an opponent for every row player in one of the two randomly determined games. However, being chosen as an opponent had no payoff consequence. Then the computer showed each subject on screen the two games selected for their payment, their own decisions in those games, and the decisions of the opponents selected for those games. Before actually being paid, the participants filled in a questionnaire eliciting their demographic data, cognitive ability and risk preferences. At the end of the experiment, each subject was privately paid the sum of the payoffs from the two selected games, plus a 2 Euro show-up fee. Subjects earned on average 12.37 Euros, with a standard deviation of 3.71.

EXPERIMENTAL GAMES

Subjects faced the series of 16 normal-form games displayed in Figure 2.1 sequentially: seven 3×3 games, seven 4×4 games, one 5×5, game and one 6×6 game. Since we are only interested in row players choices, in what follows we only discuss the row player perspective. The 16 games were designed to have the three following important characteristics.

First, different models of MR predict the same pattern of choices across all 16 games. In particular, actions marked with MR on the left of each game are the MR predictions and the IRM predictions.

Observe that, the games are designed such that there is no MR prediction in mixed strategies. They are designed such that MR mostly predicts pure strategies and the mixed strategies can be abstracted in the analysis below.[15] Similarly, the actions predicted by *L1* are marked with *L1* next to the corresponding row of each game. Notice that no game has any dominated strategy, so the predictions of the two approaches do not change even if Lk or MR are assumed models in which players have prior beliefs about their opponents being rational and only consider rationalizable strategies of others. Actions marked with NE on the left of each game are the Nash equilibrium predictions.

Second, the games are specifically designed to study different interactions between NE, *L1*, and MR. The seven 3×3 games and the seven 4×4 games share the same structure: In one game $NE = L1 = MR$ (G1 and G8), which should confirm whether at least one of the models considered is relevant in predicting most of the behavior in the experiment. Moreover, they are a benchmark for comparison with behavior in the other games. In one game $NE = L1 \neq MR$ (G2 and G9), in one game $NE \neq L1 = MR$ (G3 and G10) and in one game $NE = MR \neq L1$ (G4 and G11). These games enable the significance of the deviating model or the non-deviating models together are in explaining individual behavior to be determined. Finally, in the three remaining games (G5, G6, G7, G12, G13 and G14) and in the 5×5 and the 6×6 games (G15 and G16), $NE \neq L1 \neq MR$. These games are particularly useful for separation and for identifying of which model shows the highest descriptive power in explaining individual behavior.

Third, our games were designed to give strong incentives to individuals following

[15]There are two exceptions. In G4 and G11 the only MR in mixed strategies is to choose the action marked with MR with the highest probability, but not with probability one. In particular, for G4 $MR(S) = (1/9, 5/9, 1/3)$ and for G11 $MR(S) = (0, 3/29, 9/29, 17/29)$. As explained below, in these two games $L1$ predictions differ from the actions predicted by *MR* and by the and Nash equilibrium. In this situation, it is not possible to obtain such a strong a separation of *MR* and *L1* as in the other games, because the high payoff in the Nash equilibrium must be in the *MR* choice too, weakening the separation from $L1$.

MR or *L1* to behave according to their type. For example, an *L1* individual best responds to *L0* behavior that consists of randomizing among all possible choices. That is, an *L1* subject selects the action that maximizes the average payoff, assuming a uniformly random behavior of their opponent. In our games, the mean payoffs of the action predicted by *L1* is always at least 1 Euro greater than the second best option, given the belief that the opponent is L0.[16] In the case of actions predicted by MR, the minimal maximum regret is at least 2 Euros less than the action with the second lowest minimal maximum regret in all 16 games. For comparison, these differences are on average only 0.6 for *L1* and 0.7 for MR in the situations from Table 2.1 in which MR and *L1* are separated.

RESULTS

First, we provide basic descriptive statistics on overall behavior without requiring any within-subject consistency. In the second part we estimate a mixture-of-types model that shows which behavioral model best describes experimental data requiring certain individual consistency of subjects across different games.

Table 2.4 summarizes the behavior observed in our experiment. There are a total of 1,744 choices, as a result of the number of subjects playing as row players (109) multiplied by 16 games. The behavior observed in all games shows that subjects mostly comply with *L1*, but a significant proportion of their decisions also comply with MR and NE. 78% comply with NE, *L1* and MR when all three models coincide in their predictions. This indicates that at least one of the models that we consider is indeed relevant in describing subjects' behavior. When NE prediction differs from those of *L1* and MR, 51% of the choices comply with *L1* and MR, and 23% comply with NE. When MR prediction is different, 70% of the choices comply with NE and *L1*. Observe that this number is only 8% lower than the 78% of compliance when all three

[16] G4 and G11 are an exception. In them, the mean earnings of the predicted actions are 0.66 and 0.75 Euros greater respectively than the second best option. For an explanation, see footnote 9.

FIGURE 2.1: EXPERIMENTAL GAMES

G1

NE,L1,MR	6 , 5	3 , 1	4 , 2
	2 , 4	1 , 6	6 , 1
	2 , 5	5 , 3	1 , 7

G2

	1 ,2	6 , 7	1 , 10
MR	6 , 7	3 , 4	2 , 1
NE,L1	9 , 1	1 , 3	5 , 6

G3

L1,MR	5 , 8	7 , 10	3 , 1
	1 , 6	9 , 2	1 , 8
NE	3 , 4	2 , 3	5 , 6

G4

	7 , 3	2 , 2	3 , 8
NE,MR	2 , 3	9 , 10	1 , 3
L1	6 , 8	2 , 3	6 , 7

G5

MR	6 , 8	2 , 11	3 , 1
L1	9 , 1	5 , 3	1 , 4
NE	1 , 5	4 , 2	6 , 8

G6

NE	5 , 6	1 , 3	6 , 1
MR	2 , 1	7 , 2	3 , 4
L1	4 , 3	10 , 8	1 , 11

G7

L1	4 , 2	1 , 7	10 , 4
NE	2 , 3	6 , 5	2 , 2
MR	1 , 11	3 , 1	7 , 8

G8

	4 , 5	4 , 2	5 , 1	6 , 1
NE,L1,MR	7 , 3	6 , 8	3 , 5	8 , 4
	7 , 4	4 , 7	2 , 10	3 , 4
	9 , 7	1 , 6	1 , 2	7 , 9

G9

NE,L1	6 , 8	1 , 5	5 , 2	8 , 1
MR	3 , 1	4 , 4	2 , 3	6 , 7
	1 , 8	3 , 6	1 , 9	9 , 8
	1 , 4	7 , 1	1 , 2	3 , 11

G10

NE	3 , 7	1 , 1	1 , 5	7 , 2
	2 , 1	6 , 8	5 , 7	2 , 5
L1,MR	1 , 1	6 , 2	7 , 3	5 , 5
	1 , 5	8 , 5	2 , 7	4 , 4

G11

	5 , 2	3 , 4	3 , 3	3 , 1
	4 , 8	8 , 7	1 , 3	1 , 5
L1	5 , 3	7 , 5	4 , 4	1 , 2
NE,MR	2 , 1	5 , 2	1 , 4	6 , 7

G12

	9 , 8	2 , 9	2 , 6	2 , 5
MR	6 , 9	5 , 2	1 , 6	4 , 1
L1	8 , 4	8 , 2	4 , 1	1 , 3
NE	5 , 3	1 , 3	2 , 5	7 , 8

G13

L1	3 , 4	1 , 3	9 , 2	8 , 1
NE	4 , 1	7 , 6	3 , 1	1 , 3
MR	1 , 7	4 , 5	6 , 2	5 , 4
	2 , 9	2 , 1	9 , 8	3 , 6

G14

NE	1 , 4	7 , 2	8 , 7	1 , 3
MR	1 , 5	4 , 8	5 , 1	6 , 2
	3 , 6	3 , 6	3 , 4	7 , 9
L1	4 , 1	1 , 4	7 , 4	9 , 2

G15

L1	8 , 3	4 , 1	1 , 4	8 , 2	8 , 2
MR	5 , 1	1 , 4	4 , 5	6 , 7	5 , 1
NE	6 , 5	4 , 3	7 , 6	1 , 2	3 , 2
	3 , 9	3 , 6	1 , 4	9 , 2	5 , 5
	2 , 8	3 , 6	4 , 1	8 , 6	4 , 9

G16

L1	7 , 6	8 , 5	1 , 7	3 , 4	8 , 6	6 , 4
MR	5 , 7	6 , 5	4 , 6	1 , 5	7 , 4	4 , 1
NE	6 , 2	4 , 5	6 , 7	2 , 6	7 , 5	1 , 6
	4 , 3	7 , 1	3 , 2	3 , 2	3 , 2	6 , 2
	1 , 8	5 , 7	5 , 1	3 , 7	7 , 8	5 , 9
	5 , 8	1 , 6	4 , 3	2 , 2	9 , 1	5 , 5

models coincide in their predictions. However, 20% of the choices comply with MR. When only *L1* prediction differs from the others, we can observe that this is the one model that most alters the proportions from those observed in the second column. In fact, *L1* prediction is the only one that can be chosen in a higher proportion than the predictions of the other two combined. In the last column, where all three models give different predictions, *L1* prediction is selected most frequently (40%), followed by NE (21%) and MR (15%). Notice that the sum of these proportions (76%) is approximately equal to the compliance rate of the three models when they all coincide in their predictions (78%).

TABLE 2.4: COMPLIANCE RATES WITH EACH MODEL ACROSS DIFFERENT SUBSETS OF GAMES

	All 16 games	$NE = L1 = MR$ (G1, G8)	$NE \neq L1 = MR$ (G3, G10)	$NE = L1 \neq MR$ (G2, G9)	$NE = MR \neq L1$ (G4, G11)	$NE \neq L1 \neq MR$ (G5, G6, G7, G12, G13, G14, G15, G16)
NE	36% (631)	78% (171)	23% (49)	70% (152)	33% (72)	21% (187)
L1	49% (865)	78% (171)	51% (112)	70% (152)	38% (82)	40% (348)
MR	30% (526)	78% (171)	51% (112)	20% (43)	33% (72)	15% (128)
Other actions	23% (400)	22% (47)	26% (57)	10% (23)	29% (64)	24% (209)
Total	100% (1,744)	100% (218)	100% (218)	100% (218)	100% (218)	100% (872)

Notes: There are a total of 1,744 choices, as a result of the number of subjects playing as row players (109) multiplied by 16 games. The cells in this table show the compliance rates and the number of actions in parenthesis for each behavioral model given by the row in the games considered in each column. *Other actions* refers to choices that do not comply with NE, *L1* or MR. The *Total* row indicates the total number of choices collected for the corresponding subset of games. Notice that this number is not necessarily the sum of all the other rows.

L1 seems to rationalize more actions than NE and MR, but all three models seem to explain a significant part of the behavior. However, the above statistics disregard within-subject consistency of decisions. Our design enables us to analyze individual behavior across all games and check whether subjects are consistent in complying with the same model in most cases. Out of a total of 109 subjects, 15 comply with NE, 65 with *L1*and only 1 with MR in at least half of the games (8 out of 16). The behavior of 8 subjects does not comply with any of the three models in at least half of the games. In the following, we again use finite mixture-of-types models to estimate the distribution of the population among the three models, requiring consistency of behavior within subjects across games and making the models compete with each other.

To that end, let i denote the subject in the experiment ($i = \{1, ..., 109\}$), g the game ($G = \{1, 2, ..., 16\}$) and m the model considered ($NE, L1, MR$). Depending on the game, each subject has $c^g \in \{3, 4, 5, 6\}$ available choices. We assume that individuals follow a behavioral model but make errors with a probability of $\varepsilon_m \in [0, 1]$. In normal-form games with a finite and small number of strategies, we think it is more reasonable to assume errors to be uniformly distributed across the actions. Therefore, if a subject follows a model m, she chooses the model's predicted action with a probability of $(1 - \varepsilon_m)$, and with a probability of ε_m chooses any action with equal probability. We again assume errors to be model-specific and identically and independently distributed across games and subjects.

The likelihood of a particular individual following a certain model can be constructed as follows. Let $P_m^{g,a}$ be the predicted choice probability of model m for action a in game g. That is, $P_{NE}^{g,a}$, $P_{L1}^{g,a}$ and $P_{MR}^{g,a}$ are 1 if each model prescribes the choice and 0 otherwise. The probability of an individual i choosing a particular action a if she employs the model m is

$$(1 - \varepsilon_m)P_m^{g,a} + \frac{\varepsilon_m}{c^g}.$$

Let $x_i^{g,a}$ be 1 if action a is chosen by an individual i in game g and 0 otherwise. The likelihood of observing a certain sample $x_i = (x_i^{g,a})_{g,a}$ given type m and subject i is then

$$L_i^m(\varepsilon_m | x_i) = \prod_g \prod_a \left[(1 - \varepsilon_m)P_m^{g,a} + \frac{\varepsilon_m}{c^g}\right]^{x_i^{g,a}}. \qquad (2.7)$$

The log-likelihood function of the whole sample is obtained by adding up for all models m and subjects i, and assigning the corresponding probabilities $p = (p_{NE}, p_{L1}, p_{MR})$

to each model:

$$\ln L(p, \varepsilon_m | x) = \sum_i \ln \left[\sum_m p_m L_i^m(\varepsilon_m | x_i^{g,a}) \right]. \tag{2.7}$$

TABLE 2.5: ESTIMATION RESULTS

Model	All 16 games		Games where $NE \neq L1 \neq MR$	
	p_m	ε_m	p_m	ε_m
(1)	(2)	(3)	(4)	(5)
NE	0.32***	0.94*	0.31***	1.00
	(0.08)	(0.05)	(0.11)	(0.07)
L1	0.68***	0.57***	0.69***	0.73***
	(0.06)	(0.03)	(0.11)	(0.03)
MR	0.00	0.42***	0.00	0.41**
	(0.05)	(0.24)	(0.01)	(0.25)

Notes: Columns (2) and (3) contain the estimation results for all 16 games. Columns (4) and (5) contain the estimation results only for the games in which NE, *L1* and MR predict different behaviors (G5, G6, G7, G12, G13, G14, G15, G16). Columns (2) and (4) display the estimated share of subjects who comply with each model $p = (p_{NE}, p_{L1}, p_{MR})$. Columns (3) and (5) report the corresponding estimated error parameters $\varepsilon = (\varepsilon_{NE}, \varepsilon_{L1}, \varepsilon_{MR})$. Standard errors are shown below each estimated coefficient, in parenthesis. We report the significance levels for p_m and ε_m (***$p < 0.01$, **$p < 0.05$, *$p < 0.10$) using bootstrapping with 500 replications (Efron and Tibshirani, 1994). Notice that the error rates are well behaved if they are close to zero and far from one (corresponding to random play), so we test whether each ε_m differs significantly from one (rather than zero).

Table 2.5 reports the estimation results. There are two different estimations: One which contains all 16 games (columns (2) and (3)) and other which contains the estimates only for those in which NE, *L1* and MR predict different behaviors (columns (4) and (5)). The only model that seems to be relevant is *L1*, which explains the behavior of about 68% of the subjects in the estimates for all games and 69% if only games in which the three models predict different behaviors are considered. In both cases, the noise rate is significantly below 1. The frequency of NE is positive in both estimations, but the error rate is so high that it cannot be differentiated from 1 (pure noise), especially in the games in which all the behavioral models' give different predictions. This noisy play is capturing other types of behavior not included in this analysis, as explained below. Finally, no subject systematically follows MR, as the estimated frequency is never different from 0 in any estimation.

As a robustness test, we make two alternative estimations. Results are shown in Table E.1 in Appendix D. First, columns (4) and (5) report the estimations from Table 2.5 excluding games G4 and G11, in which there was a prediction of MR in mixed strategies (as explained in footnote 9). The conclusions remain the same. Second, columns (2) and (3) in Table E.1 show the results of the estimation with additional behavioral models. Even if *L1* reduces its frequency to 23%, is still the only behavioral model from Table 2.5 that remains relevant. Therefore, the conclusion that no one follows MR systematically holds.

In short, these results reinforce the above conclusions that MR has no relevance in explaining non-equilibrium behavior. When MR competes with *L1* in normal-form games and the experimental design is appropriate for separating the two, the results show that MR has no predictive power.

2.5 CONCLUSIONS

This chapter studies the relationships between minimax regret and *L1*, two alternative behavioral models for explaining deviations from Nash equilibrium in a wide spectrum of games. The comparison between these two types of models has been neglected by economists, probably due to the great distance between their underlying motivations. We point out the surprising degree to which their predictions coincide in a large number of games in the experimental literature. Then, we analyze two experiments that include games with very different characteristics that enable us to properly separate these models and identify whether both are actually relevant in explaining individual behavior or whether only one of them matters.

The experimental results show clear evidence in favor of the *L1* model over MR. Although many individual choices can be attributed to MR, very few subjects seem to follow these predictions systematically in the guessing games from Costa-Gomes and Crawford (2006) and no subject does so in our normal-form games specifically designed to separate these two models. Thus, our experimental results cast doubt on whether MR play any role as a relevant theory in describing individual behavior in strategic situations. Further research should test whether these results extend to other conditions or strategic situations.

2.6 Appendix A: Examples of applications of *MR* and *L1*

Here is an example to clarify how each model makes predictions. Consider the 3×3 game below.

Player i\Player j	L	C	R (NE)
T (NE)	6,1	1,6	5,6
M	4,6	4,6	3,1
B	1,3	7,1	1,7

Application of *MR*

First, we calculate $MR(A)$, the actions predicted by MR in pure strategies. The maximal payoffs that each player can obtain given each action of the other, defined in (2.1), are:

Player i\Player j	L	C	R	$u_j^*(a_i)$
T	6,1	1,6	5,6	$u_j^*(T)$=6
M	4,6	4,6	3,1	$u_j^*(M)$=6
B	1,3	7,1	1,7	$u_j^*(B)$=7
$u_i^*(a_j)$	$u_i^*(L)$=6	$u_i^*(C)$=7	$u_i^*(R)$=5	

The regrets of each action a given the actions played by the opponent, defined in (2.2), are:

Player i\Player j	$regret_j(L\|a_i)$	$regret_j(C\|a_i)$	$regret_j(R\|a_i)$
$regret_i(T\|a_j)$	0,5	6,0	0,0
$regret_i(M\|a_j)$	2,0	3,0	2,5
$regret_i(B\|a_j)$	5,4	0,6	4,0

The regret of each action a, defined in (2.3), is:

Player i\Player j	$regret_j(L\|a_i)$	$regret_j(C\|a_i)$	$regret_j(R\|a_i)$	$regret_i(a_i)$
$regret_i(T\|a_j)$	0,5	6,0	0,0	$regret_i(T)$=6
$regret_i(M\|a_j)$	2,0	3,0	2,5	$regret_i(M)$=3
$regret_i(B\|a_j)$	5,4	0,6	4,0	$regret_i(B)$=5
$regret_j(a_j)$	$regret_j(L)$=5	$regret_j(C)$=6	$regret_j(R)$=5	

Finally, the actions that bring the minimum regret are $MR_i(A) = \{M\}$ and $MR_j(A) = \{L, R\}$:

Player i\Player j	L(MR)	C	R(RM)
T	6,1	1,6	5,6
M (RM)	4,6	4,6	3,1
B	1,3	7,1	1,7

Then we calculate $MR(S)$, the actions predicted by MR in mixed strategies. The utilities of playing the mixed strategies that minimize regret, of playing each strategy with a probability of $(0, 5/6, 1/6)$ for player i, and of playing each strategy with a probability of (6/13,1/13,6/13) for player j for the example game are:

Player i\Player j	L	C	R	s_j
T	6,1	1,6	5,6	$\frac{67}{13},\frac{48}{13}$
M	4,6	4,6	3,1	$\frac{46}{13},\frac{48}{13}$
B	1,3	7,1	1,7	$\frac{19}{13},\frac{61}{13}$
s_i	$\frac{21}{6},\frac{33}{6}$	$\frac{27}{6},\frac{31}{6}$	$\frac{16}{6},\frac{12}{6}$	$\frac{249}{78},\frac{301}{78}$

Applying (2.2) gives the corresponding regret for the strategies given each strategy of the opponent. The mixed strategies yield the minimal maximum regret: 15/6 for player i and 30/13 for player j:

Player i\Player j	$regret_j(L\|a_i, s_i)$	$regret_j(C\|a_i, s_i)$	$regret_j(R\|a_i, s_i)$	$regret_j(s_j\|a_i, s_i)$
$regret_i(T\|a_j, s_j)$	0,5	6,0	0,0	$0, \frac{30}{13}$
$regret_i(M\|a_j, s_j)$	2,0	3,0	2,5	$\frac{126}{78}, \frac{30}{13}$
$regret_i(B\|a_j, s_j)$	5,4	0,6	4,0	$\frac{288}{78}, \frac{30}{13}$
$regret_i(s_i\|a_j, s_j)$	$\frac{15}{6}$,0	$\frac{15}{6}, \frac{26}{78}$	$\frac{14}{6}, \frac{279}{78}$	$\frac{153}{78}, \frac{128}{78}$

$MR(S)$ can also be calculated as in Renou and Schlag (2010), i.e. bycalculating the regrets of the mixed strategies as the corresponding convex combination of the regrets of the pure strategies:

Player i\Player j	$regret_j(L\|a_i, s_i)$	$regret_j(C\|a_i, s_i)$	$regret_j(R\|a_i, s_i)$	$regret_j(s_j\|a_i, s_i)$
$regret_i(T\|a_j, s_j)$	0,5	6,0	0,0	$\frac{6}{13}, \frac{30}{13}$
$regret_i(M\|a_j, s_j)$	2,0	3,0	2,5	$\frac{27}{13}, \frac{30}{13}$
$regret_i(B\|a_j, s_j)$	5,4	0,6	4,0	$\frac{54}{13}, \frac{30}{13}$
$regret_i(s_i\|a_j, s_j)$	$\frac{27}{6}, \frac{4}{6}$	$\frac{15}{6}, \frac{6}{6}$	$\frac{14}{6}, \frac{25}{6}$	$\frac{189}{78}, \frac{180}{78}$

Finally, we calculate the $IRM(A)$. The table below indicates the choices that survive one application of the deletion operator MR:

Player i\Player j	L(MR_j^1)	C	R(MR_j^1)
T	6,1	1,6	5,6
M ($MR_i^1 = MR_i^\infty$)	4,6	4,6	3,1
B	1,3	7,1	1,7

The following game remains if the actions that do not survive one application of the deletion operator MR are eliminated. Applying it again to the leftover game gives:

Player i\Player j	L($MR_j^2 = MR_j^\infty$)	R
M (MR_i^2)	4,6	3,1

Finally, $IRM_i(A) = M$ and $IRM_j(A) = L$:

Player i\Player j	L(IRM_j)	C	R
T	6,1	1,6	5,6
M (IRM_i)	4,6	4,6	3,1
B	1,3	7,1	1,7

APPLICATION OF *Lk*

Last, we show *L1(A)*, the choices that yield the maximum payoffs if the opponent chooses randomly among all actions:

Player i\Player j	L	C	R($L1_j$)
T ($L1_i$)	6,1	1,6	5,6
M	4,6	4,6	3,1
B	1,3	7,1	1,7

2.7 APPENDIX B: PROOFS OF THE PROPOSITIONS OF SECTION 2.3.1

Proposition 1. (i)

Proof for MR: A weakly dominant strategy always has the highest outcome possible for all strategies by the opponent: $u_i(WD_i|\vec{s}_{-i}) = max_{s_i \in S_i} u_i(s_i, \vec{s}_{-i}) = u_i^*(\vec{s}_{-i})$. As a result the regret of a weakly dominant strategy is 0: $regret_i(WD_i|\vec{s}_{-i}) = u^*(\vec{s}_{-i}) - u_i(WD_i, \vec{s}_{-i}) = u_i(WD_i, \vec{s}_{-i}) - u_i(WD_i, \vec{s}_{-i}) = 0$ and $regret_i(WD_i) = max_{\vec{s}_{-i} \in S_{-i}} regret_i(WD_i|\vec{s}_{-i}) = 0$. Since 0 is the minimum regret possible for a strategy: $min_{s_i \in S_i} regret_i(s_i) = regret_i(WD_i) = 0$ and $WD_i(S) = MR_i(S)$.

Proof for Lk: A dominant strategy $WD_i(S)$ is the best response to any strategy $s_{-i} \in S_{-i}$. That includes the uniform play of a *L0*, $r_{-i}(S_{-i})$, or any other k.

Proposition 1. (ii)

Proof for MR: A dominated strategy d_i has a lower utility for each strategy of the other players than the strategy that is dominated by, $u_i(d_i|\vec{s}_{-i}) < u_i(D_i|\vec{s}_{-i})$. As a result, the regret of d_i is always higher than the regret of D_i for every state: $regret_i(d_i|\vec{s}_{-i}) >$

$regret_i(D_i|\vec{s}_{-i})$. Therefore, the maximum regret is also higher:

$regret_i(d_i) = max_{\vec{s}_{-i} \in S_{-i}} regret_i(d_i|\vec{s}_{-i}) > max_{\vec{s}_{-i} \in S_{-i}} regret_i(D_i|\vec{s}_{-i}) = regret_i(D_i)$. Finally, the minimum regret possible is not that one of the dominated strategy, because that of the dominant strategy is always lower: $min_{s_i \in S_i} regret_i(s_i) \neq regret_i(d_i) > regret_i(D_i)$.

Proof for the Lk: A dominated strategy is by definition not the best response to any strategy of the other players. Therefore it is not part of the set of strategies that an Lk could ever play for any k.

Proposition 2.

Let $a_i = \{x_i, y_i\}$ and $a_j = \{x_j, y_j\}$ be the possible actions for players i and j in a 2×2 game.As defined in Section 2.2.2, $y_i \subseteq L1(A)_i \iff u_i(y_i|\vec{r}_j(A_j)) \geq u_i(x_i|\vec{r}_j(A_j))$. In a 2×2 game $\vec{r}_j(A_j)$ is to play each action with a probability of 0.5. Substituting and multiplying by 2 gives the condition for an action y_i to be predicted by $L1$:

$$y_i \subseteq L1(A)_i \iff u_i(y_i, x_j) + u_i(y_i, y_j) \geq u_i(x_i, x_j) + u_i(x_i, y_j) \qquad (2.7)$$

In order analyze whether a strategy y_i is a subset of $MR(A)$, it is necessary to study 4 different cases:

Case 1: $u_i(y_i, x_j) \geq u_i(x_i, x_j)$ and $u_i(y_i, y_j) \geq u_i(x_i, y_j)$

Case 2: $u_i(y_i, x_j) \leq u_i(x_i, x_j)$ and $u_i(y_i, y_j) \geq u_i(x_i, y_j)$, not being both equal

Case 3: $u_i(y_i, x_j) \geq u_i(x_i, x_j)$ and $u_i(y_i, y_j) \leq u_i(x_i, y_j)$, not being both equal

Case 4: $u_i(y_i, x_j) \leq u_i(x_i, x_j)$ and $u_i(y_i, y_j) \leq u_i(x_i, y_j)$

For case 1, y_i is a weakly dominant strategy, and proposition 1 applies. In that case, y_i is predicted by MR and condition (2.7) is satisfied, so it is also predicted by *L1*. By symmetry, same goes for case 4. If case 2 applies, $u_i^*(x_j) = u_i(x_i, x_j)$ and $u_i^*(y_j) = u_i(y_i, y_j)$. As a result, $regret_i(y_i) = u_i(x_i, x_j) - u_i(y_i, x_j)$ and $regret_i(x_i) = u_i(y_i, y_j) - u_i(x_i, y_j)$. Which strategy has the minimum regret depends on the relation given by condition (2.7), specifically:

$$u_i(y_i, x_j) + u_i(y_i, y_j) \geq u_i(x_i, x_j) + u_i(x_i, y_j) \iff regret_i(y_i) \leq regret_i(x_i)$$

So in case 2, y_i has less regret than x_i if and only if the condition (2.7) is satisfied, and therefore y_i is also predicted by *L1*. By symmetry, the same goes for case 3.

2.8 Appendix C: CGC alternative estimations

We show the results of two alternative estimations for CGC in Table C.1. First, we incorporate the types originally considered in CGC into the estimation from Table 2.3. Second, we add MR to that estimation.

The estimation results in columns (2), (3) and (4) show the results for the population level estimation with the original types from CGC.[17] *D1* (Dominance 1) eliminates one round of dominated decisions and best responds to uniform-play over the opponent's remaining decisions. *D2* (Dominance 2) does the same but for two rounds of dominated decisions elimination. *Soph* is a type that knows the actual distribution of subjects decisions and best responds to it. The estimation results in columns (5), (6) and (7) incorporate MR into the previous estimation. The distribution of the types estimated is similar to the individual analysis in CGC. *L1* is the model that explains the largest part of the population and MR explains a fraction the population not statistically different from 0.

[17] *NE* is denoted Eq in CGC.

TABLE C.1: CGC ALTERNATIVE ESTIMATION RESULTS

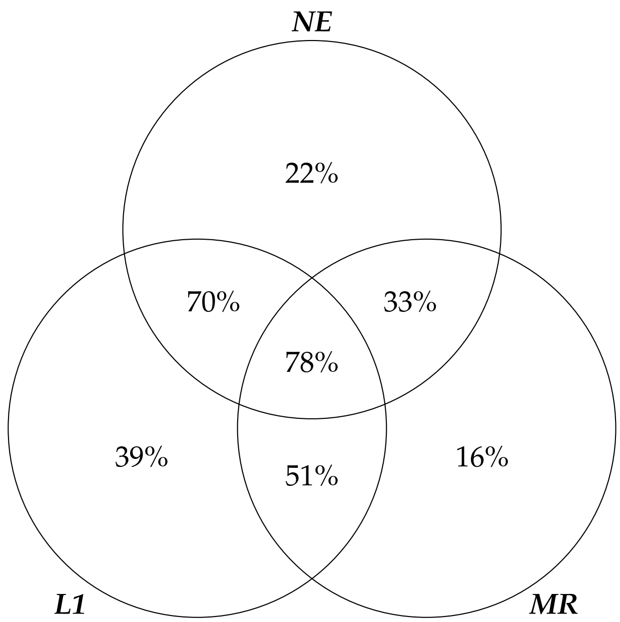
	CGC types			CGC types plus MR		
Model	p_m	ε_m	λ_m	p_m	ε_m	λ_m
(1)	(2)	(3)	(4)	(5)	(6)	(7)
NE	0.27***	0.88*	0.34**	0.19***	0.63***	0.07
	(0.07)	(0.08)	(0.19)	(0.05)	(0.07)	(0.13)
L1	0.45***	0.90***	1.02***	0.42***	0.52***	0.69***
	(0.08)	(0.02)	(0.15)	(0.09)	(0.09)	(0.18)
L2	0.23***	0.82***	1.14***	0.20***	0.41***	0.66***
	(0.07)	(0.03)	(0.19)	(0.05)	(0.09)	(0.16)
L3	0.01	0.01***	0.12	0.03*	0.53***	1.03***
	(0.07)	(0.37)	(0.48)	(0.02)	(0.11)	(0.12)
D1	0.00	0.03***	0.50	0.13**	0.95	0.74*
	(0.07)	(0.31)	(0.54)	(0.05)	(0.32)	(0.49)
D2	0.00	0.53*	1.17**	0.00	0.04***	0.97**
	(0.06)	(0.33)	(0.55)	(0.01)	(0.26)	(0.44)
Soph	0.00	0.01***	0.87**	0.00	0.29**	1.25***
	(0.02)	(0.40)	(0.41)	(0.06)	(0.37)	(0.38)
MR				0.04	0.52**	0.58
				(0.05)	(0.27)	(0.72)

Notes: The estimation results in columns (2), (3) and (4) show results for the estimation with the original types from CGC. The estimation results in columns (5), (6) and (7) incorporate MR to the previous estimation. Columns (2) and (5) display the estimated proportion of subjects complying with each model named in column (1). Columns (3) and (6) reports the corresponding estimated error parameter ε. Columns (4) and (7) report the corresponding estimated distributions of the errors, λ parameters. Standard errors are shown below each estimated coefficient, in parenthesis. We report the significance levels (***$p < 0.01$, **$p < 0.05$, *$p < 0.10$) using bootstrapping with 500 replications (Efron and Tibshirani, 1994). Notice that the error rates are well behaved if they are close to zero and far from one (corresponding to random play), so we test whether each ε_m differs significantly from one (rather than zero).

2.9 APPENDIX D: ALTERNATIVE PRESENTATION OF THE RESULTS OF OUR EXPERIMENT

Figure D.1 presents the results visually. 78% of the decisions comply with NE, *L1* and MR when the three models coincide in their predictions. This indicates that at least one of the models considered here is indeed relevant in describing subjects' behavior. When the predictions of one model do not coincide with either of the others, 39% of subjects' choices comply with *L1*, 22% with NE and 16% with MR. The combination of two models followed most when only two predict the same behavior are NE and *L1* with 70% compliance, followed by the combination of *L1* and MR with 51%, and the combination of NE and MR with 33%.

FIGURE D.1: VISUAL REPRESENTATION OF THE RESULTS OF OUR EXPERIMENT



Notes: Each circle corresponds to the model marked in bold next to it. The figures inside each circle indicate the percentage of subjects' choices that comply with the corresponding model in different sets of games depending on the overlapping of the circles. The percentage indicated in the area where each circle does not overlap with any other, indicates the compliance of the corresponding model in the games where the predictions of that model do not coincide with those of either of the other. The percentages shown in the area where two circles overlap indicate the compliance of the corresponding models in the games where those two models predict the same action, which is different from the action predicted by the remaining model. Finally the percentage indicated in the middle of the figure where all circles overlap refers to the compliance of the three models in the games where all three prescribe the same behavior.

2.10 APPENDIX E: ALTERNATIVE ESTIMATIONS FOR OUR EXPERIMENT

We show the results of two alternative estimations in Table E.1. First, we incorporate more behavioral models into the estimation. Second, we consider the same models as in Table 2.5 but remove those games in which the prediction of MR in mixed strategies is not the action marked as MR in our games (G4 and G11).

The estimation results in columns (2) and (3) show results for the estimation with the additional behavioral models. A represents an altruistic individual who chooses the actions that could lead to the result with the maximum sum of the payoffs of both players. *IA* represents an inequity averse player who chooses the actions that could yield the result with the minimum difference in payoffs between the two players. *MAXMAX* and *MAXMIN* describe individuals who apply the traditional maximax and minimax rules to the payoffs of the games. *PE* represents a player who chooses the actions that could lead to a Pareto efficient payoff profile. Table E.2 shows how far apart the predictions of all the models are separated from each other in the games. The estimation results reveal that *L1* continues to be a relevant model and the frequency of MR is still not different from 0 when more behavioral models are considered. It also confirms that NE is not a significant model, and that the high error rate indicated in Table 2.5 was attributable to other types of behavior. *L1*, *L2*, and *L3* are the most relevant models. An altruistic type who chooses the action that could bring the maximum sum of the payoffs of both players and as a type who applies the minimax rule to payoffs (and not regret), are also significant.

The estimation results in columns (4) and (5) confirm that the conclusions of Table 2.5 remain unchanged even if the games in which the prediction of MR in mixed strategies is not the action marked with MR in our games are removed.

TABLE E.1: ALTERNATIVE ESTIMATION RESULTS FOR OUR EXPERIMENT

	All games		All games except G4 and G11	
Model	p_m	ε_m	p_m	ε_m
(1)	(2)	(3)	(4)	(5)
NE	0.01	0.63***	0.29***	0.92
	(0.01)	(0.06)	(0.08)	(0.07)
L1	0.23***	0.52***	0.71***	0.56***
	(0.05)	(0.03)	(0.07)	(0.03)
MR	0.01	0.56***	0.00	0.23***
	(0.01)	(0.11)	(0.05)	(0.25)
L2	0.21***	0.51***		
	(0.06)	(0.03)		
L3	0.28***	0.64***		
	(0.05)	(0.14)		
A	0.14***	0.44***		
	(0.04)	(0.06)		
IA	0.00	0.85***		
	(0.00)	(0.14)		
MAXIMAX	0.02	0.41***		
	(0.02)	(0.14)		
MINIMAX	0.09**	0.61***		
	(0.04)	(0.07)		
PE	0.00	0.04**		
	(0.01)	(0.21)		

Notes: Columns (2) and (3) contain the estimates for several behavioral models for all 16 games. Columns (4) and (5) contain the estimates for NE, *L1* and MR only for the games in which the prediction of MR in mixed strategies is the action marked as MR in our games. Columns (2) and (4) display the estimated proportion of subjects complying with each model named in column (1). Columns (3) and (5) report the corresponding estimated error parameters. Standard errors are shown below each estimated coefficient, in parenthesis. We report the significance levels for p_m and ε_m (***$p < 0.01$, **$p < 0.05$, *$p < 0.10$) using bootstrapping with 500 replications (Efron and Tibshirani, 1994). Notice that the error rates are well behaved if they are close to zero and far from one (corresponding to random play), so for each ε_m we test whether it differs significantly from one (rather than zero).

TABLE E.2: SEPARATION BETWEEN THE PREDICTIONS OF DIFFERENT MODELS

	NE	*L1*	*MR*	*A*	*IA*	*MAXMAX*	*MINMAX*	*PA*	*L2*
NE	0.00								
L1	0.75	0.00							
MR	0.75	0.75	0.00						
A	0.82	0.85	0.73	0.00					
IA	0.69	0.79	0.76	0.67	0.00				
MAXMAX	0.84	0.56	0.84	0.54	0.66	0.00			
MINMAX	0.72	0.75	0.66	0.52	0.63	0.84	0.00		
PA	0.68	0.74	0.68	0.60	0.42	0.64	0.72	0.00	
L2	0.50	0.38	0.81	0.79	0.76	0.66	0.69	0.74	0.00
L3	0.19	0.75	0.88	0.76	0.63	0.78	0.63	0.74	0.38

Notes: Each cell of the table shows the proportion of the decisions across all 16 games in which two different behavioral models predict different strategies. Any given number in row i and column j is the rate of separation between the behavioral model listed in row i and the behavioral model listed in column j. The minimum value is 0 if both models prescribe the same behavior, and the maximum value is 1 if they predict different strategies in all games. The separation in each game is calculated by dividing the number of choices in which one model predicts an action with a positive probability and the other does not by the total number of strategies predicted with a positive probability by either of the two models.

2.11 APPENDIX F: TRANSLATION OF THE INSTRUCTIONS

INSTRUCTIONS

Welcome and thank you for taking part in our experiment! Please read these instructions carefully. The same instructions are given to for all participants. Please do not write on these instructions.

If you have any questions, please raise your hand and a member of the team conducting the experiment will answer them. From now on, communication with other participants in the experiment is not allowed. Please, turn off your phone now. If you do not agree to these rules, we are sorry, you will not be able to take part in the experiment.

The University of the Basque Country has provided the funds for this experiment. You will receive 2 Euros just for participating. However, you can earn more money during the experiment. How much you can earn depends on your decisions, on those of other participants, and on chance. Everything you earn will be paid privately in cash at the end of the experiment session. During the experiment, all numbers will represent Euros. All your decisions will be confidential.

THE EXPERIMENT

This experiment consists of making choices in 16 rounds. In each round you will be randomly and anonymously matched with another participant from this session, though not necessarily with the person physically next to you in this room. You will not know who any of these participants are, and they will not know who you are.

In each round, you and the participant with whom you are matched with, will make independent decisions, i.e., you will not know the decisions of the other participant. The two decisions –yours and that of the other participant– will jointly determine how much money each of you earns in the corresponding round.

Each round is independent from the others: the amount that you can earn in each round depends solely on the decisions made in that round. Once a round is over, neither you nor the participant with whom you are matched can change the decision made in that round. In addition, throughout the 16 rounds neither you nor anyone else will know what any other participant has done. You will only find out once the experiment is over.

In each round you will have to **choose an action**. Here is an example of the decision that must be made and how will be shounx in each round. **This example is only an illustration**. The situations that you will face in the 16 rounds will be different from this example and will change from round to round.

Each decision problem will be presented in the form of a table similar to the one below (but with different values). You will see the corresponding table each time you have to choose an action.

	The other participant chooses:			
	Action A	Action B	Action C	Action D
You choose: Action A	1 ; 3	4 ; 8	4 ; 9	3 ; 4
You choose: Action B	5 ; 2	1 ; 5	4 ; 2	8 ; 8
You choose: Action C	2 ; 3	5 ; 3	3 ; 5	1 ; 4
You choose: Action D	9 ; 6	7 ; 8	1 ; 7	5 ; 6

Each row of the table corresponds to an action that you can choose: A, B, C or D. The decision that you must make is which of them to choose. The participant you will be matched with, will have to choose among his/her actions too, as shown in the columns of the table: A, B, C or D. That is, you choose rows while the other person chooses columns. To simplify things, the experiment is programmed such that all participants –including the person with whom you are matched– see their decision as in our example. That is, each of you will be presented with your possible actions in the rows of the table. At the time of choosing, you will not know what action was chosen by the participant you have been matched with, and when the participant you have been matched with chooses his/her action, he/she will not know what action you have chosen.

Your decision and that of the other participant will determine the payments for each of you. In the table, your actions and your payments appear in red, while those of the other participant appear in blue.

The table reads as follows:

- If you choose A and the other participant chooses A, you receive 1 and he / she receives 3

- If you choose A and the other participant chooses B, you receive 4 and he / she receives 8

- If you choose A and the other participant chooses C, you receive 4 and he / she receives 9
- If you choose A and the other participant chooses D, you receive 3 and he / she receives 4

- If you choose B and the other participant chooses A, you receive 5 and he / she receives 2
- If you choose B and the other participant chooses B, you receive 1 and he / she receives 5
- If you choose B and the other participant chooses C, you receive 4 and he / she receives 2
- If you choose B and the other participant chooses D, you receive 8 and he / she receives 8

- If you choose C and the other participant chooses A, you receive 2 and he / she receives 3
- If you choose C and the other participant chooses B, you receive 5 and he / she receives 3
- If you choose C and the other participant chooses C, you receive 3 and he / she receives 5
- If you choose C and the other participant chooses D, you receive 1 and he / she receives 4

- If you choose D and the other participant chooses A, you receive 9 and he / she receives 6
- If you choose D and the other participant chooses B, you receive 7 and he / she receives 8

- If you choose D and the other participant chooses C, you receive 1 and he / she receives 7
- If you choose D and the other participant chooses D, you receive 5 and he / she receives 6

The decision problem in each round will be shown as a table like the ones in these examples. The rounds will differ in two aspects. First, the amounts in the cells will differ from round to round. As in the example, how much you can earn in each round will depend on your decision and that of the other participant.

Second, the number of actions that you can choose from will also differ form round to round. In some rounds you will have to choose between four actions, as in the example, but in other rounds you will have to choose between 3, 4, 5 or 6 actions. The following table shows an example in which you have to choose between 3 actions:

	The other participant chooses:		
	Action A	Action B	Action C
You choose: Action A	3 ; 5	8 ; 6	5 ; 5
You choose: Action B	5 ; 7	6 ; 4	4 ; 1
You choose: Action C	7 ; 9	4 ; 2	8 ; 9

Again, this is just an illustrative example and the numbers that you will see in the experiment will differ from those shown here.

HOW TO USE THE COMPUTER

In each of the 16 rounds, you will see a table like the one shown in the examples on the screen, with the same colors and with the white boxes corresponding to your possible actions. You can choose an action by clicking on the corresponding box. For instance, say that in the first example with four actions you choose action C. When you click on the box for action C it will change color as shown in the following table:

	The other participant chooses:			
	Action A	Action B	Action C	Action D
You choose: Action A	1 ; 3	4 ; 8	4 ; 9	3 ; 4
You choose: Action B	5 ; 2	1 ; 5	4 ; 2	8 ; 8
You choose: Action C	2 ; 3	5 ; 3	3 ; 5	1 ; 4
You choose: Action D	9 ; 6	7 ; 8	1 ; 7	5 ; 6

OK

The choice is not final: you can change it as many times as you want by clicking on another box so long as you have not yet clicked on the "OK" button in the corner of the screen. Once you click on "OK" the option chosen will be final and you will go on to the next round. You cannot move on to the next round until you have chosen an option and clicked "OK".

PAYMENTS

After you have decided in all 16 rounds, your payments will be determined as follows. The computer will select 2 of the 16 rounds randomly for each participant. You will receive the total amount that you have earned in those 2 rounds, in line with your own decision and that of the person you have been matched with in each round. Your final payment will be the 2 Euros plus the amount in Euros that you have earned in those 2 rounds.

At the end of the experiment, the screen will show the complete table of decisions for each of the rounds selected: your decisions in each round, and those of the participant you were matched with.

SUMMARY

In this experiment you will make **decisions in 16 rounds**. In each round you will be matched randomly and anonymously with another participant.

In each round, you and the person you will be matched with, will have to **make independent decisions** that will determine how much money each of you earns in that corresponding round. Throughout the 16 rounds, neither you nor anyone else will know what the other participants decide. You will only know once the experiment is finished.

Each round will be a decision problem in the form of a table in which you have to **choose an action**. You choose from the rows and the other participant chooses from the columns. Your decision and that decision of the other participant will determine how much each of you will be paid. In the table, your actions and your payments appear in red, while the actions and payments of the other participant appear in blue.

After you have decided in all 16 rounds, the computer will choose **2 rounds at random**. You will again see the table for the decisions of those 2 rounds, and will be informed of what action you chose in that round and what action the other participant chose. You will receive the amount of money that you earned in those 2 rounds, according to your decision and that of the person you have been matched with in each round. Your final payment will be 2 Euros for participating plus the amount in Euros that you have earned in those 2 rounds.

CONTROL QUESTIONS

Before beginning the 16 rounds, please answer the following control questions about the situation represented by the table below. The rounds will not begin until everyone has answered the 4 questions below the table correctly.

	The other participant chooses: Action A	Action B	Action C
You choose: Action A	3 ; 5	8 ; 6	5 ; 5
You choose: Action B	5 ; 7	6 ; 4	4 ; 1
You choose: Action C	7 ; 9	4 ; 2	8 ; 9

1) Consider this table. If the participant you are matched with chooses action A and you choose action C, what will your payment be if this round is chosen for your payment?

2) If the other participant chooses action B and you choose action A, how much will the other participant be paid if this round is chosen for his/her payment?

3) True or False: We will pay you the sum of the payments of the 16 rounds.

4) True or False: At the end of the 16 rounds you will be able to see the complete table of the decisions for the rounds for which you will be paid, your actions and those of the participant you have been matched with.

Chapter 3

Hot versus Cold Behavior in Centipede Games

3.1 INTRODUCTION

Strategic environments involving sequential decision-making are commonly represented by games in extensive form. A strategy in extensive-form games involves a complete plan of actions, with one action for each possible information set. In a lab, the researcher can thus elicit behavior using either the direct-response or “hot" method as opposed to the strategy or “cold" method. Under the direct, hot elicitation procedure players observe the behavior of others in previous stages of the game before they take an action and can therefore only react to realized past actions. By contrast, under the strategy, cold elicitation procedure, players take actions in all the hypothetical situations in which they can find themselves throughout the game, without observing the actions of their opponents. Since both elicitation procedures are strategically equivalent, one should observe the same behavior independently of which is employed. However, the empirical validity of this equivalence has been debated for decades.

Does the elicitation method (hot vs. cold) yield different individual behavior? One traditional argument is that this would be the case because players are forced to think more about the different hypothetical situations in the cold method, which can in turn modify their behavior, while in the hot method players react to the actual behavior of others, potentially triggering emotional responses that may also affect subjects' behavior (Roth, 1995; Lowenstein, 1996). Brandts and Charness (2011) provide a comprehensive survey of 29 studies that explicitly compare hot vs. cold elicitation methods. They find no differences in behavior in 16 cases, 6 in which behavioral differences are detected between the two elicitation methods, and 4 in which the evidence is mixed. They conclude that their analysis should dispel the beliefs that the strategy method inevitably yields results different from those gathered using the direct-response method, but their study rather suggests that both elicitation procedures lead to the same behavior under certain conditions but not in others and provides suggestive evidence that certain features of the environment may play a role. They do not find evidence that the behavior elicited via the two methods generally diverges when emotions are involved–as suggested above–but they document that punishment levels are typically lower under the strategy method. Moreover, the behavior observed is more likely to differ from one method to the other if subjects make fewer contingent choices, suggesting that simpler, less complex strategic situations enhance the ability of subjects to reason similarly under both elicitation methods. Lastly, they find that any behavioral differences between the two procedures seem to diminish over time. In sum, whether the strategy method is behaviorally equivalent to direct-response elicitation remains an open question and more evidence is required to provide a conclusive answer. In particular, we are aware of no studies that target this issue using a unified experimental framework that enable a clean comparison between comparable strategic environments.

We contribute to this debate by testing this issue using Centipede Games (CGs, hereafter), introduced by Rosenthal (1981). In these games, two players decide alternately between two options: pass (and continue the game) or take (and stop it right away). The particular feature of CGs is their incentive structure. Both players have incentives to pass because stopping at any later decision node always leads to higher payoff. However, they also have incentives to take because if the other player takes in the subsequent decision node they will get a lower payoff. Due to that incentive structure, backward induction predicts a unique, symmetric behavior for both player roles: take in every decision node. If both players follow this prediction, the player that moves first stops the game in her first decision node, independently of other details of the incentive structure.

Using the hot method, McKelvey and Palfrey (1992) were to first to show that the unique subgame perfect Nash equilibrium fails to predict how people play this game. This divergence from subgame perfect equilibrium has been extensively replicated (e.g. Fey, McKelvey and Palfrey, 1996, Nagel and Tang, 1998, Palacios-Huerta and Volij, 2009, Levitt, List and Sadoff, 2011, Kawagoe and Takizawa, 2012, Garcia-Pola, Iriberri, and Kovářík, 2016). Following McKelvey and Palfrey (1992), hot elicitation has become the most commonly used method in CGs, though there are a few exceptions. Nagel and Tang (1998) used the strategy method to elicit behavior in a twelve-node CG. They conclude that the behavior in their experiment does not differ from that in McKelvey and Palfrey (1992), but they do not test this issue formally. The only paper that provides a direct experimental comparison of the two methods using CGs is Kawagoe and Takizawa, (2012) who compare behavior under both methods in the six-node increasing-sum CG from McKelvey and Palfrey (1992) and in the constant-sum CG from Fey, McKelvey and Palfrey (1996), using a within-subject design.[1] They find no differences between the two elicitation procedures. We argue that this might be due to the use of the within-subject design in their comparison. Subjects face one elicitation procedure right after the other using the same game, a feature that may explain why virtually the same behavior is observed in both cases.

[1] Kawagoe and Takizawa (2012) scale the payoffs in the constant CG up to balance the incentives across the two CGs.

Our study compares behavior under the hot and cold treatments in four variants of six-node CGs using a between-subject design. Two games are the canonical increasing- and constant-sum variations from McKelvey and Palfrey (1992) and Fey, McKelvey and Palfrey (1996), respectively, tested in Kawagoe and Takizawa (2012). The other two CGs, proposed in García-Pola, Iriberri, and Kovářík (2018), preserve the incentive structure of CGs but differ dramatically from the two canonical games in the evolution and/or (a)symmetry of payoffs. One is an increasing-sum CG with two different dimensions of asymmetry across player roles. First, the first player obtains a larger payoff than the second player independently of who takes first. Second, the second player has almost no incentives to pass, but for the first player that incentives are almost as strong as in the canonical CG proposed by McKelvey and Palfrey (1992). The last CG is a variable-sum CG, meaning that the sum of payoffs neither monotonically increases nor decreases over the decision nodes. This game also provides asymmetric incentives across the two players.

Importantly, given that all four games preserve the incentive structure of CGs, they provide no space for punishment (but they do provide space for positive emotional responses and thus reward passing behavior, a feature that might stimulate taking later under the hot method) and the number of choices available to each player is constant across all four CGs. Hence, the determinants detected by Brandts and Charness (2011) are held constant across our four CGs. On the other hand, an incentive structure that is asymmetric across the two players may be viewed as a source of complexity during the decision-making process. In that case, Brandts and Charness (2011) suggest that behavior is more likely to differ between the two elicitation procedures in the asymmetric games than the symmetric ones. Lastly, since Brandts and Charness (2011) conclude that differences due to the elicitation method tend to disappear over time, people play each CG for 10 different periods in our hot treatment so that we can tell whether their behavior converges toward the behavior elicited via the cold, strategy method.

Our data reveal significantly different behavior between the two elicitation methods in the two canonical CGs. In particular, in the hot method behavior shifts toward stopping earlier. However, the behavior observed does not differ between the hot and cold methods for the other two CGs. Since the main difference between the two pairs of CGs is that both players face the same incentives in the former but not in the latter and this result holds for both players, we attribute these effects to payoff (a)symmetry. People might find it easier to "put themselves in others' shoes" if others face comparable situations. This way, they are more likely to realize in symmetric games that the other player also has the same incentives to stop earlier. This seems to be reinforced under the direct, hot method but less so in the cold treatments, yielding behavior closer to subgame perfect equilibrium in the first case. On the other hand, if payoff asymmetry is considered as an element of game complexity, this finding may seem to contradict one conclusion of Brandts and Charness (2011). Lastly, again in contrast to Brandts and Charness (2011), we observe no convergence of the behavior elicited in the hot treatment towards that of the cold treatment. If anything, the behavior in one of our games rather diverges slightly with experience.

The chapter is organized as follows. Section 3.2 introduces the experimental procedures and design. Section 3.3 presents the results. Section 3.4 discusses the results and concludes.

3.2 Experimental Design

3.2.1 Experimental Procedures

The data presented in this study come from two sets of experiments. The data elicited using the strategy method is the same than the one from the first chapter, where 151 subjects participated in four sessions in May 2015.[2] In May and June 2018, we con-

[2]Given the matching protocol in the cold treatment, the number of participants did not have to be even.

ducted seven additional sessions with a further 218 subjects, employing the hot elicitation method, explicitly designed for the purpose of this chapter. For both treatments, recruitment was carried out with the ORSEE (Greiner, 2015) and we ensured that the subjects had participated in no any similar experiments in the past. All sessions were conducted at the Laboratory of Experimental Analysis (Bilbao Labean; http://www.bilbaolabean.com) at the University of the Basque Country using z-Tree software (Fischbacher, 2007).

The two treatments share some common features. The subjects were given identical instructions in both cases, except for the specific changes inherent in each elicitation procedure as described below. The instructions gave explanations of three examples of CGs (other than those used in the main experiment), how subjects could make their choices, the matching procedure, and the payment method. The instructions were read aloud to guarantee public knowledge. Subjects were allowed to ask any questions they might have throughout the instruction process. After the instruction process, all participants had to answer several control questions on the computer screen before they could proceed. An English translation of the instructions from the cold and hot treatments can be found in Appendices B (of the first chapter) B (of this chapter), respectively.

At the beginning of each treatment, each subject was randomly assigned the role of either Player 1 (who decides first then chooses in the odd decision nodes) or Player 2 (who decides in the even decision nodes). To avoid any possible associations from being first or second or number 1 vs. 2, subjects playing as Player 1 were labeled as *red* and those playing as Player 2 *blue* throughout the experiment. At the end of the experiment for both treatments, the participants were invited to fill in a questionnaire eliciting information in a non-incentivized way concerning their demographic data, cognitive ability, social and risk preferences.

Bellow, we describe in detail the design features of each treatment separately.

COLD TREATMENT

In the cold treatment, each subject participated in 16 different CGs one by one in a random order (which was the same for all subjects) with no feedback between the different games. These 16 CGs included the four CGs analyzed in this study (displayed in Figures 3.1 and 3.2). Subjects made their choices game by game. They were never allowed to leave a game without making a decision and get back to it later, and they were unaware in any stage of which games they would face in later stages. There was no time constraint and the participants were not obliged to wait for others while making their choices in the 16 games. Our design minimizes reputation concerns and learning as far as possible. Hence, the choice in each game reflects the initial play and each subject can be treated as an independent observation. The CGs were displayed in the extensive form on the screens, as shown in the instructions in Appendix B of the first chapter.

The behavior was elicited using the strategy method. That is, rather than making their choices node by node and observing the behavior of their opponents in the preceding nodes, each subject submitted the decision node in which she would take for the first time and thus end the game (if her opponent had not already taken). During the actual decision, the subjects faced the extensive-form representation of the game and decided as follows. The branches corresponding to different options in the game were generally displayed in black but the branches corresponding to each players' choice were displayed in red for Players 1 and in blue for Players 2. Depending on the player, they had to click on a square white box that stated either "Stop here" or "Never stop". To ensure that subjects thought enough about their choices, once they had made their decision whether to stop at a node or never stop by clicking on the corresponding box, they did not leave the screen immediately. Rather, the chosen option changed color to red or blue depending on the player and they were allowed to change their choice as many times as they wished, simply by clicking on a different box. If they did so, the option chosen previously would turn back to white and the newly chosen action would change color to either red or blue. To proceed to another game in the sequence, the subjects had to confirm their decision by clicking on an "OK" button in the bottom right corner of the screen. They were only allowed to proceed once they had confirmed. In terms of strategies, for each game and each player type, participants had faced four different options to click on: *Take the first time*, *Take the second time*, *Take the third time*, and *Always pass*, without knowing the strategy chosen by the other player. The instructions in Appendix B of the first chapter provide some examples of how the different stages were displayed to the subjects in this treatment.

When all subjects had submitted their choices in the 16 CGs, three games were randomly selected for payment for each subject. Hence, different participants were paid for different games. The procedure, which was carefully explained to the subjects in the instructions, was as follows. The computer randomly selected three games for each subject and three different random opponents from the whole session, one for each of those three games. This means that the same participant may have served as an opponent for more than one other participant. Nevertheless, being chosen as an opponent does not have any payoff consequence. To determine the payoff of a subject from each game selected, her behavior in each game was matched to the behavior of the randomly chosen opponent for this game. At the end of the experiment, the subjects were privately paid the sum of the payoffs from the three games selected at a conversion ratio of 1 Euro for each 10 experimental points, plus a 3 Euro show-up fee.

HOT TREATMENT

In the hot treatment, each subject played only one of the four CGs displayed in Figures 3.1 and 3.2. Since the direct method already provides feedback within each game, we also explored how behavior evolves over the repetitions of the same game. Therefore, each participant played the same game ten times against a randomly chosen opponent in each round. This design feature enables us to analyze the behavior in the first round, but also how behavior changes with experience and whether the behavior elicited via the hot method converges to or diverges from the behavior observed under the strategy method. Subjects made their choices round by round, with no the possibility of going back to the previous rounds. There was no time constraint, but subjects had to wait for their opponents to decide in each corresponding decision node and for all player pairs to finish the current round before the computer would generate new random pairs in each round. Our design minimizes reputation concerns, minimizing the number of times that each subject plays with the same opponent in each group as far as possible and ensuring that subjects never play against the same opponent in two consecutive rounds. Additionally, the matching was designed so that there were more than two and mostly three cohorts and so that subjects from one cohort never met a subject from any other cohort, leading to at least two independent observations for each CG. The CGs were displayed in extensive form on the screens, as shown in the instructions in Appendix B.

Since behavior was elicited using direct, hot method, the players decided in each node alternately one after the other. As in the cold treatment, the branches in the game were generally displayed in black but the branches corresponding to the players' two current choices were displayed in red for Players 1 and in blue for Players 2. In each corresponding decision node, each participant had to click on one of the two square white boxes corresponding to each optionm which stated either "Stop here" or "Continue". In the last decision node for each player, the boxes stated "Stop here" or "Never stop" instead. Once a player chose "Stop here" the round finished. To ensure that subjects thought enough about their choices, once they had made their decision whether to stop at a particular node or not by clicking on the corresponding box, they did not leave the screen immediately. Rather, the chosen option changed color to red or blue depending on the player and they were allowed to change their choice as many times as they wished, simply by clicking on a different box. In such a cases, the option chosen previously would turn back to white and the newly chosen action would change color to either red or blue. To proceed to the next decision, subjects had to confirm their decision by clicking on an "OK" button in the bottom right corner of the screen. They were only allowed to proceed once they had confirmed. These colors were maintained in the game tree when players were deciding in the next decision nodes as an indication of the decisions made for that round. The instructions in Appendix B provide some examples of how the different stages were displayed to the subjects in this treatment.

When all subjects had submitted their choices in all 10 rounds, two rounds were randomly selected for payment for each subject (or one round in case of the exponentially increasing-sum CG, labeled as CG1 below, to preserve equality of payoffs of subjects across different games). Hence, different participants were paid for different rounds, according to their decisions and the decisions of their corresponding opponents in each round. At the end of the experiment, the subjects were privately paid the sum of the payoffs from the two rounds (or one round) selected randomly at a conversion ratio of 1 Euro for each 10 experimental points, plus a 3 Euro show-up fee.

3.2.2 Experimental Games

In this study, we analyze four different variations of six-decision node CGs. Figure 3.1 displays the extensive-form representations of the four CGs. Figure 3.2 represents these games graphically, using players' payoffs. In the latter case, the y-axes correspond to the seven potential terminal nodes in each CG, while the x-axes plot both the payoffs of each player and the sum of the payoffs of both players at the corresponding decision nodes. This alternative depiction enables three important features to be observed. First, the figure reflects the incentives of each player role to take or pass in each decision node and their evolution over the nodes. Second, it reveals the (a)symmetry in both the size of payoffs and their evolution across the two roles. Lastly, Figure 3.2 reflects the evolution of the sum of payoffs across the two player roles over the different decision nodes.

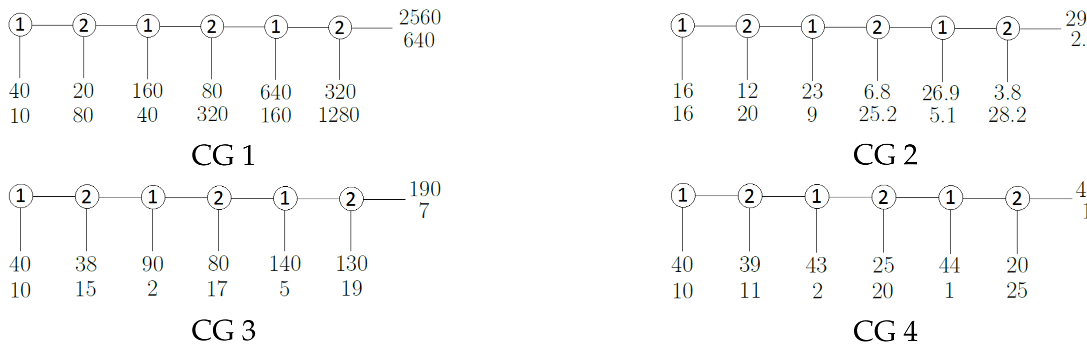


FIGURE 3.1: THE FOUR CENTIPEDE GAMES USED IN THE EXPERIMENT

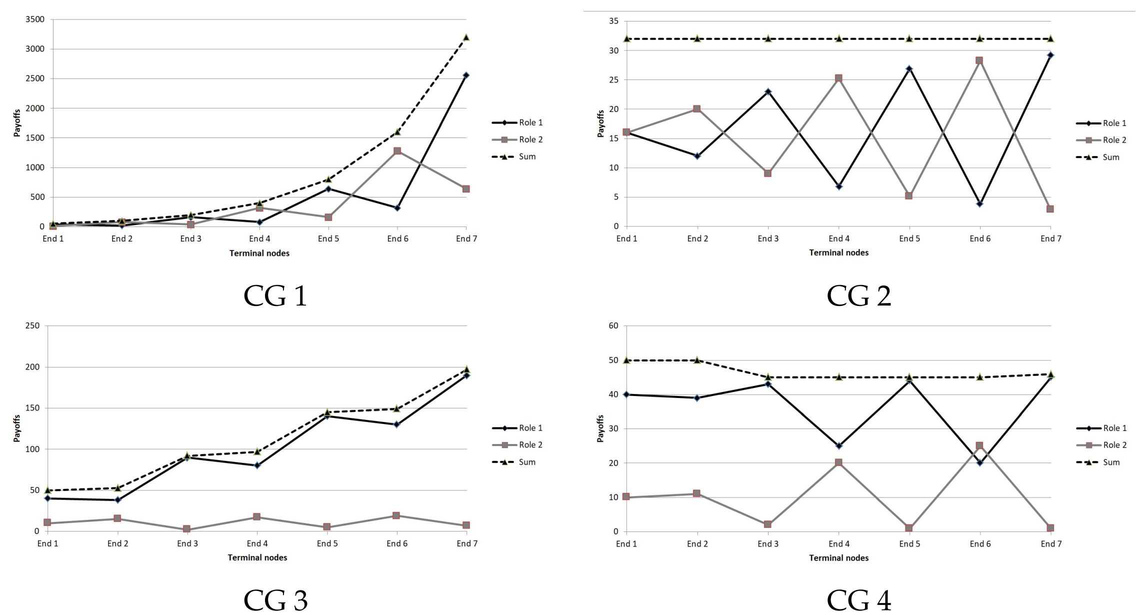


FIGURE 3.2: ALTERNATIVE REPRESENTATION OF THE FOUR CENTIPEDE GAMES USED IN THE EXPERIMENT

We selected these four CGs in order to provide considerable variation in the evolution of both the payoffs in each player role and the sum of payoffs of both players, while preserving the payoff structure of a CG. That is, in all four CGs, (i) each player is always better off passing and taking at the next decision node; but (ii) she always gets a higher payoff by taking than if she passes and her opponent takes in the subsequent decision node.

The first game, labeled as CG1, is the exponentially increasing-sum CG originally tested by McKelvey and Palfrey (1992). The sum of the payoffs of both players always doubles moving from one node to the next and the splitting rate of the sum between the players is always the same and is symmetric across the players. This game provides strong, symmetric incentives for both players to pass, at least in the initial stages of the game. CG2 is the constant-sum CG proposed by Fey, McKelvey, and Palfrey (1996). In this game, the sum of the payoffs remains constant throughout the game, so there is no social gain in moving forward. Again, the incentives of the two players are symmetric.

Unlike CG1 and CG2, CG3 and CG4 have not previously been tested in the literature. They differ from CG1 and CG2 mostly in that the payoff paths differ considerably from one player role to the other. Like CG1, CG3 is an increasing-sum game. Nevertheless, the payoffs of Player 1 are always higher than those of Player 2 and they increase considerably over the decision nodes, while the payoffs of Player 2 remain low and slightly decrease. As a result, Player 1 has incentives not to take while the contrary occurs for Player 2. Finally, CG4 is a variable-sum CG in which the sum of the payoffs decreases initially, stays constant in the middle nodes, and rises slightly at the end. Once again, the payoffs of Player 1 are higher overall than those of Player 2, but this time the evolution of the payoffs is similar.

In total, we collected data from 151/216 subjects in the cold/hot treatments. Out of the 216 in the hot treatment, 62 played CG1, 64 played CG2, 56 played CG3, and 34 played CG4.

3.3 RESULTS

Figure 3.3 summarizes the experimental results. Due to the difference in elicitation methods between the two treatments, behavior cannot be compared directly. In the hot treatment, Figure 3.3 reports the frequencies of the terminal nodes observed in each game, given the (random) matching of subjects in the actual experiment. On the figure, the three darker bars corresponding to each terminal node display the distribution of behavior in the first round, the average behavior over all ten rounds of the experiment, and the behavior in the last round, respectively. The figure reveals that there is little variation in behavior over rounds. The only exception seems to be CG4, where there is a tendency to take earlier as subjects gain experience. However, given that the comparisons between the hot and cold treatments generally generate the same results independently of whether the initial, average or final behavior in the hot treatment is used, the main text focuses on the average behavior over all the ten experimental rounds (see the results in Appendix A.1 for more details concerning the comparison of behavior across different rounds).

Regarding the behavior elicited in the cold treatment, people submit their strategies simultaneously and the distribution of terminal nodes reached is not observed directly. To enable the behavior in the two treatments to be compared, we proceed as follows. We generate 100,000 random sub-samples from the cold treatment to match the number of subjects in each role in each game in the corresponding hot treatment and randomly match into pairs the behavior of the sampled subjects from different roles. This generates 100,000 potential distributions of terminal nodes, each corresponding to one possible realized distribution of behavior if the subjects from the cold treatment participated in the hot treatments (respecting the sample sizes of the latter). The white bars in Figure 3.3 plot the average terminal-node frequency across these 100,000 simulations and the corresponding 95% confidence intervals.

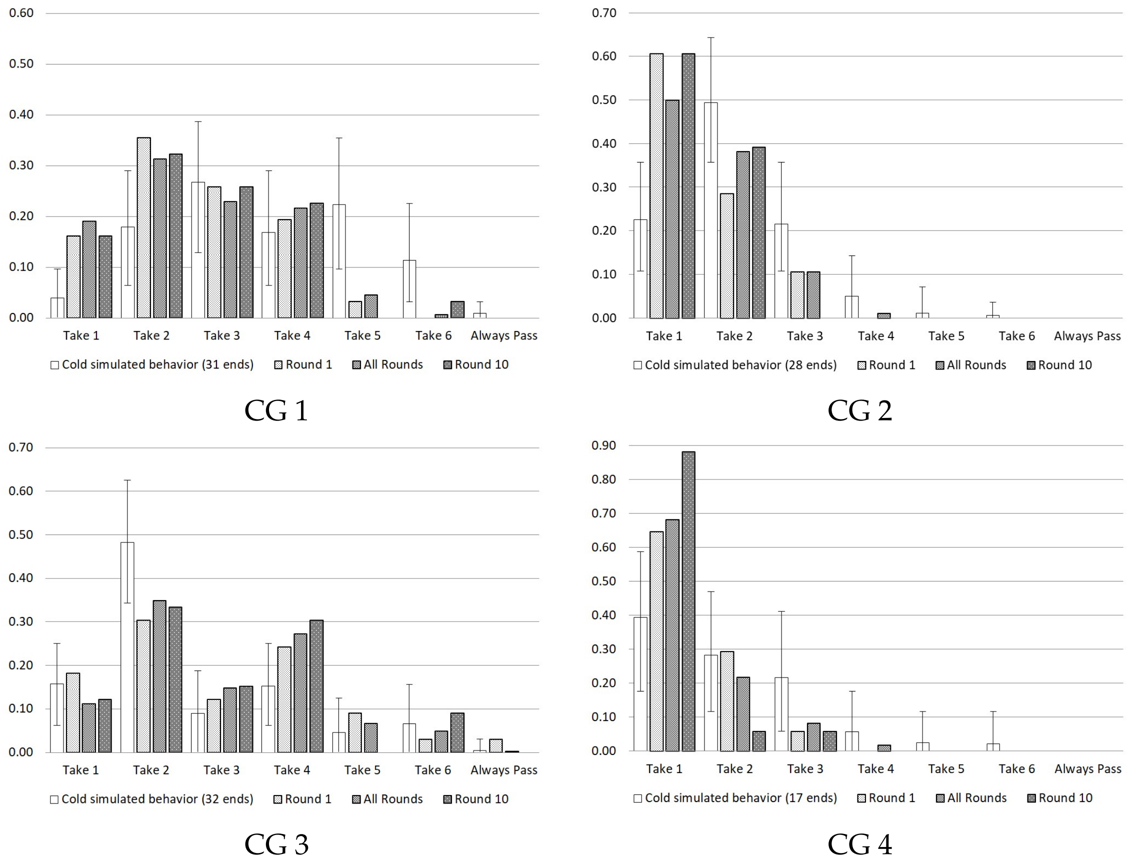


FIGURE 3.3: COMPARISON OF BEHAVIOR BETWEEN TREATMENTS.

Figure 3.3 shows that the behavior differs across the four CGs in line with the incentives. People stop consistently later in CG1, which provides the largest incentives to pass. This tendency is observed for both roles. CG3 shows the second greatest tendency to take later, and reflects the asymmetry between the two players: subjects in the role of Player 1 (odd decision nodes) tend to stop later than subjects playing as Player 2 (even nodes). There is some passing in CG2, but few pairs of opponents reach the third decision node independently of the treatment. Lastly, in CG4 the vast majority of pairs end the game at the first decision node under both elicitation methods.

At first sight there are some differences between the two treatments, but they seem to show up exclusively in the first or second decision nodes and disappear in later stages of each game. Hence, it cannot be concluded from the figures whether the two methods generate the same behavior or not.

To test formally whether the distributions differ from one treatment to the other, we perform three different tests for each game. Remember that we perform between-subject comparisons, enabling us to use tests assuming the independence of the two samples. First, we test two different null hypotheses using the chi-squared tests of independence. One directly tests whether the behavior in both treatments comes from the same distribution, and the other asks whether the behavior from the hot treatment comes from the simulated distribution under the cold method. Third, we perform two-sample Kolmogorov-Smirnov tests.[3] Table 3.1 displays the *p*-values generated by the 3 (tests) $\times$ 4 (games) = 12 tests.

The three tests all conclude that behavior differs form one elicitation method to the other in CG2. In contrast, equal behavior under both elicitation methods can never be ruled out in CG3 and CG4.[4] For CG1, the chi-square tests indicate that the hypothesis of the same behavior in the two treatments cannot be accepted ($p < 0.02$), while the Kolmogorov-Smirnov test suggests otherwise ($p = 0.139$). The main difference between these tests is that the former two tests take into account the whole distribution of behavior, while the latter only looks at the largest difference between the two cumulative distribution functions of behavior. It is thus possible to have two distributions that do not differ much from one another at any particular point, leading the Kolmogorov-Smirnov test to accept the equality the two distributions, but which consistently differ sufficiently in the whole support of the distribution for a test that considers the whole distribution to reject it. This is actually the case here. A look at the tests of proportions show that they indeed suggest that significantly higher proportions of people

[3]We alternatively performed tests of proportions, which generally confirm the results of the tests reported in the main text. See Appendix A.2.

[4]As mentioned above, people tend to stop earlier with experience in CG4. If equality of distributions is tested under both treatments in CG4, all three tests conclude that behavior does not differ from one treatment to the other using the initial and average behavior in the hot treatment. However, all three tests reject equality of distributions using the last-round behavior ($p = 0.039, 0.001$, and 0.035, respectively; see Appendix A.2). This suggests that behavior under the hot method diverges from that elicited with the strategy method.

stop in the initial phases of the game under the hot method than the cold method (see Appendix A.2).

As a result, we conclude that our experimental subjects tend to stop earlier under the hot method in CG1 and CG2 than in the cold treatments. These results are in sharp contrast with those from the within-subject comparison in Kawagoe and Takizawa (2012).[5] For CG3 and CG4, the cold and hot methods seem to generate the same behavior. The following section provides a hypothesis for why the conclusions might differ from one game to another across the different games.

TABLE 3.1: TESTS THAT COMPARE BEHAVIOR IN HOT (AVERAGED OVER THE 10 ROUNDS) AND COLD ELICITATION METHODS

Game	Chi-squared test: Both come from the same population	Chi-squared test: Hot comes from cold	Kolmogorov-Smirnov test: Both distribution are equal
(1)	(2)	(3)	(4)
CG1	0.019**	0.000***	0.139
CG2	0.003***	0.000***	0.033**
CG3	0.749	0.278	0.681
CG4	0.238	0.071*	0.487

Notes: Column (1) identifies the game. Columns (2) and (3) report the *p*-values for the chi-squared test of two different null hypotheses: (2) shows the probability of observing those particular distributions if data from both treatments come from the same population, and (3) shows the probability if the data from the hot treatment comes from the cold treatment. Column (4) reports the *p*-values for the Kolmogorov-Smirnov test under the null that the two distributions are equal.

3.4 DISCUSSION

Employing the between-subject design, this chapter compares the behavior elicited by the direct, hot method and by the strategy or cold method in four six-node CGs, which differ in their incentive structures from one game and one player role to another. We observe that whether the two methods generate the same or different behavior

[5] Kawagoe and Takizawa (2012) do not provide any exhaustive analysis of this issue. They only perform the Kolmogorov-Smirnov test, which is designed for two independent samples, but their behavior under both elicitation methods come from the same population, violating this assumption. This might be one reason why our results contradict theirs.

depends on the game under scrutiny. More precisely, we find significant differences between the two elicitation methods in the two canonical CGs, in that people stop somehow earlier under the hot method, while both elicitation procedures generate the same behavior in two other CGs.

What determines whether the results differ from one game to another? As shown in Figure 3.2, the main difference between the games that generate different behavior and those that do not is the (a)symmetry of incentives between the two player roles. The incentives are similar in size and evole in the same way as the game evolves for both players in the games in which differences are observed between the hot and cold treatments. By contrast, the incentive structures are asymmetric in the other games: one of the games preserves the same evolution in payoffs but differs in size–and thus the incentives to take or pass–from one player role to the other. In the second asymmetric game size and evolution change, such that one player has greater incentives to pass and those incentives increase over time, while they are lower and actually decrease for the second player role. This suggests that payoff (a)symmetry across player roles might be a determinant of whether or not the elicitation procedure affects how people behave. This contributes to the debate initiated in Brandts and Charness (2011) regarding the conditions that determine when the direct-response elicitation generates different behavioral responses from the strategy method.

As for the mechanism behind our finding, payoff asymmetry naturally hinders the understanding of opponents' motivations by (boundedly rational) decision makers. As a result, asymmetric incentives may prevent decision makers from "putting themselves in the shoes of the opponent". By contrast, when the incentives are aligned it is easier to realize that others also have strong incentives to take at every decision node in CGs. This might reinforce the decision to take earlier in symmetric game than in comparable asymmetric games. Our results suggest that this effect might be reinforced more under the hot method than under the strategy method. Whether this is a general phenomenon is a matter for further research. However, if there is indeed an interaction between payoff (a)symmetry and decision making under “hot" conditions, payoff asymmetry across players should be carefully considered when designing experiments involving sequential reasoning.

3.5 APPENDIX A: ADDITIONAL RESULTS

APPENDIX A.1 EVOLUTION OF BEHAVIOR FROM ROUND TO ROUND IN THE HOT TREATMENT

Subjects played the same CG over 10 different rounds in our hot treatment. It is therefore possible to test whether the behavior of subjects in this treatment differs from one round to another due to learning. Figure 3.4 plots the average terminal node across all the player matches on the y-axes and the experimental rounds on the x-axes for the different CGs. The figure confirms the tendency to stop later in the increasing-sum CGs (between the second and third nodes) than in the constant-sum and variable-sum CGs, in which people stop on average between the first and the second decision nodes. Most interestingly though, we detect little tendency to stop earlier or later with experience. To test this formally, we ran four ordered-logit regressions at the level of matched pairs of subjects to test whether the stopping node depends systematically on the round for each game, taking into account potential correlations within cohorts. Table 3.2 reports the results. There is no trend in behavior over time for CG1, CG2 and CG3. The only game that shows some evolution is CG4, in which the stopping node decreases slightly from one round to another (0.04 per round). This effect is significant at 5%.

In summary, we find little learning with experience except in CG4 in which people–if anything–tend to stop earlier in later rounds. These observations contrast with those of both Mckelvey and Palfrey (1992) and Fey, Mckelvey and Palfrey (1996) who ran 10 repetitions of CG1 and CG2, respectively, and found that their subjects played more in line with the theoretical prediction (stopping earlier) in later rounds. In contrast, it coincides with Nagel and Tang (1998), who found no tendency to stop earlier or later over the 100 rounds of their CG.

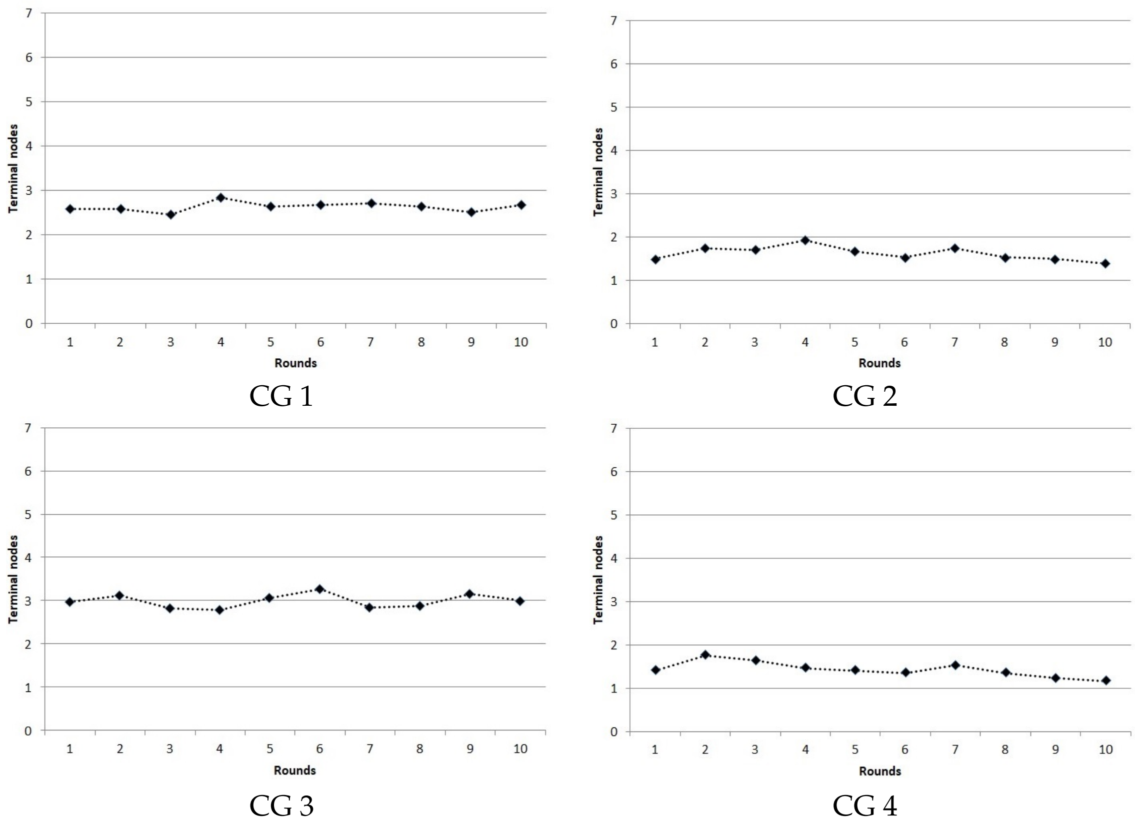


FIGURE 3.4: MEAN TERMINAL NODE OVER THE 10 ROUNDS IN THE HOT TREATMENTS.

TABLE 3.2: ORDERED-LOGIT ESTIMATIONS: EFFECT OF EXPERIMENTAL ROUNDS ON TERMINAL NODES REACHED BY EACH MATCHED PAIR FOR THE FOUR CGS; HOT TREATMENTS.

	CG1	CG2	CG3	CG4
	(1)	(2)	(3)	(4)
Round	0.0062561	-0.025974	0.0139131	-0.0434938**
	(0.022909)	(0.0218323)	(0.0302528)	(0.003155)
Constant	2.597849**	1.771429***	2.902011***	1.67451***
	(0.2732784)	(0.1027561)	(0.0858486)	(0.0083292)
Observations	310	280	329	170

Notes: Robust standard errors clustered at cohort level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix A.2 Hot vs. Cold Treatments: Alternative tests

Table 3.3: Tests that Compare Behavior in Hot (Round 1) and Cold Elicitation Methods

	Chi-squared test		Kolmogorov-Smirnov test
Game	Both come from the same population	Hot comes from cold	Both distribution are equal
(1)	(2)	(3)	(4)
CG1	0.026**	0.000***	0.096*
CG2	0.019**	0.000***	0.033**
CG3	0.698	0.349	0.900
CG4	0.297	0.137	0.608

Notes: Column (1) identifies the game. Columns (2) and (3) report the *p*-values for the chi-squared test of two different null hypotheses: (2) shows the probability of observing those particular distributions if data from both treatments come from the same population, and (3) shows the probability if the data from the hot treatment comes from the cold treatment. Column (4) reports the *p*-values for the Kolmogorov-Smirnov test under the null that the two distributions are equal.

Table 3.4: Tests that Compare Behavior in Hot (Round 10) and Cold Elicitation Methods

	Chi-squared test		Kolmogorov-Smirnov test
Game	Both come from the same population	Hot comes from cold	Both distribution are equal
(1)	(2)	(3)	(4)
CG1	0.025**	0.000***	0.096*
CG2	0.003***	0.000***	0.033**
CG3	0.369	0.060	0.635
CG4	0.039**	0.001***	0.035**

Notes: Column (1) identifies the game. Columns (2) and (3) report the *p*-values for the chi-squared test of two different null hypotheses: (2) shows the probability of observing those particular distributions if data from both treatments come from the same population, and (3) shows the probability if the data from the hot treatment comes from the cold treatment. Column (4) reports the *p*-values for the Kolmogorov-Smirnov test under the null that the two distributions are equal.

3.6 Appendix B: “Hot" treatment instructions in English (original in Spanish)

THANK YOU FOR PARTICIPATING IN OUR EXPERIMENT!

TABLE 3.5: TESTS OF PROPORTIONS THAT COMPARE BEHAVIOR IN HOT AND COLD ELICITATION METHODS

Game	End 1	End 2	End 3	End 4	End 5	End 6	End 7
CG1	0.063*	0.020**	0.047**	0.004***	0.063*	0.597	1
CG2	0.004***	0.002***	0.166	0.486	0.680	1	1
CG3	0.587	0.142	0.295	0.992	0.741	0.896	1
CG4	0.092*	0.112	0.287	0.363	0.538	1	1

Notes: The table reports the p-values of the test of proportions performed on the two different proportions of accumulated matches that ended before the end reported in each column in the hot and cold elicitation methods. For example, 0.002 for CG2 and End 2 indicates that the proportion of subjects stopping CG2 in either the first or second decision nodes differs statistically between the hot and cold treatments at 0.2%.

This is an experiment, so there is to be no talking, looking at what other participants are doing or walking around the room. Please, turn off your phone. If you have any questions or you need help, please raise your hand and one of the researchers will assist you. Please, do not write on these instructions. If you fail to follow these rules, YOU WILL BE ASKED TO LEAVE THE EXPERIMENT AND YOU WILL NOT BE PAID. Thank you.

The University of the Basque Country has provided the funds for this experiment. You will receive 3 Euros for arriving on time. Additionally, if you follow the instructions correctly you have the chance of earning more money. This is a group experiment. Different participants may earn different amounts. How much you can win depends on your own choices, on other participants choices, and on chance.

No participant can identify any other participant by his/her decisions or earnings in the experiment. The researchers can observe each participant earnings, but they will not associate your decisions with the name of participant name.

During the experiment you can win experimental points. At the end, these experimental points will be converted into cash at a rate of 1 experimental point = 0.10 euros. Everything you earn will be paid in cash, in a strictly private way at the end of the experimental session.

Your final earnings will be the sum of the 3 Euros that you get just for participating and the amount that you earn during the experiment.

Each experimental point earns you 10 Euro cents, so 10 experimental points make 1 euro (10 x 0,10 = 1 Euro).

For example, if you obtain a total of 80 experimental points you will earn a total of 11 Euros (3 for participating plus 8 from converting the 80 experimental points into cash).

For example, if you obtain a total of 45 experimental points you will earn a total of 7.5 Euros (45 x 0.10 = 4.5 + 3 = 7.5)

For example, if you obtain a total of 190 experimental points you will earn a total of 22 euros (190 x 0.10 = 19 + 3 = 22)

Groups:

All participants in this session will be randomly divided in two different groups, the RED group and the BLUE group. Before you start making decisions, you will be informed if you are RED or BLUE, and you will maintain that status throughout the experiment.

Game and options:

The experiment will consist of 10 rounds of the same game. In each round you will be matched randomly with a participant form other group which will always be a different from the one from the previous round. That means , if you are RED in each round you will be matched randomly again with another BLUE participant, and if you are BLUE in each round you will be matched randomly again with another RED participant. Nobody will know the identity of the participant with whom you are matched, nor will it be possible to identify him/her by his/her decisions during or after the experiment.

A description of the games follows. Every round has the same format, as represented in graphic form below. If you are a RED participant you can only make choices in the red circles. If you are a BLUE participant you can only make choices in the blue circles.

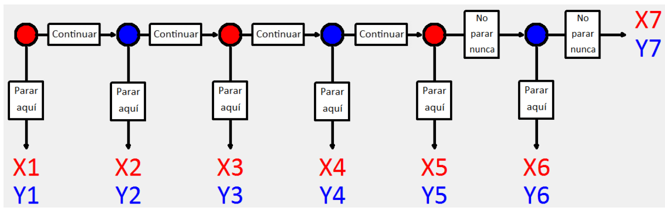


FIGURE 3.5: GAME

In each round, each participant, RED or BLUE, has three chances to determine the earnings of both participants, in which he/she can one of two actions: stop or continue. In the graphic representation, the circles colored, RED and BLUE, identify which participant chooses. As the direction of the arrows shows, the game should be read from left to right. The earnings of the two participants are represented by X and Y, which in each circle of each game will be different numbers, representing experimental points.

The RED participant has the first chance to choose: he/she can "Stop here" or continue.

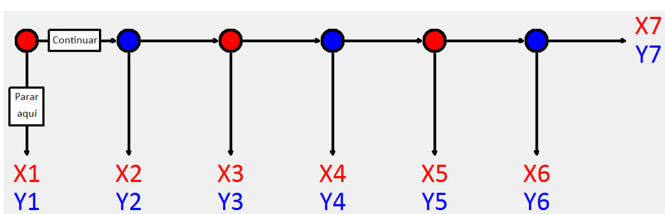


FIGURE 3.6: RED 1

In the graphic representation the downward arrow in the first RED circle represents "Stop" and the rightward arrow represents continue. If the RED participant chooses "Stop here", the RED participant receives X1 and the BLUE participant Y1, and the round ends. If the RED participant chooses "Continue", then the round continues and it is the BLUE participant who chooses in the first blue circle.

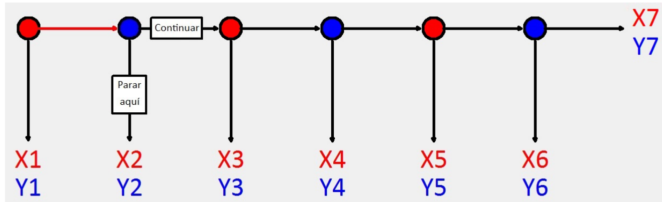


FIGURE 3.7: BLUE 1

The BLUE participant can choose "Stop" or continue. In the graphic representation, the downward arrow in the first BLUE circle represents "Stop here" and the rightward arrow represents continue. If the BLUE participant chooses "Stop here" the RED participant receives X2 and the BLUE participant Y2, and the round ends. If the BLUE participant chooses "Continue", then the game continues and it is the RED participant who chooses again in the second red circle.

This description is repeated in the second red and blue circles, until the last chance is reached by the RED and BLUE participants.

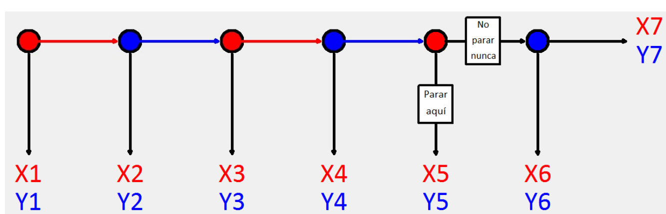


FIGURE 3.8: RED 3

In the last chance for the RED participant, represented by the third and last red circle, the RED participant can choose "Stop here" or "Never stop". If the RED participant chooses "Stop here" the RED participant receives X5 and the BLUE participant Y5, and the game ends. If the RED participant chooses "Never stop", then it is the BLUE participant who chooses for the last time.

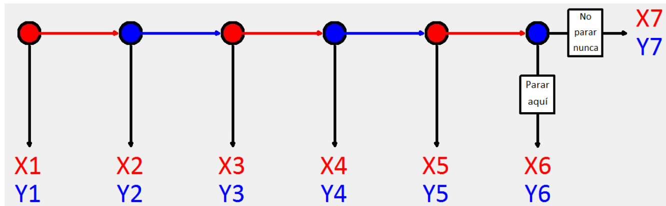


FIGURE 3.9: BLUE 3

In the last chance for the BLUE participant, represented by the third and last blue circle, the round ends. If the BLUE participant chooses "Stop here" each participant receives, X6 for the RED and Y6 for the BLUE, and the round ends. If the BLUE participant chooses “Never stop” the round ends and the quantities that the participants receive are X7 for the RED and Y7 for the BLUE.

In summary, in each round you have to choose where to stop or continue in the different circles of your color. That means that in each round you can choose between two different Options in each of the different circles of your color: stop here or continue in the first circle of your color, stop here or continue in the second circle of your color and stop here or “Never stop”in the third circle of your color You will play the same game 10 times with different participants and the participant who chooses “Stop here” before the other participant is the one who ends the game and determines the experimental points earned by both participants.

In order to make the game easier to understand, three examples are shown below. In the examples we show the choices by the RED participant (shaded in red) and ones by the BLUE (shaded in blue) for a hypothetical game, and we identify the earnings for each participant.

Example 1:

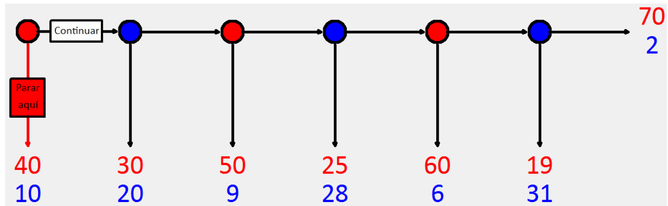


FIGURE 3.10: EXAMPLE 1

The RED participant has chosen "Stop"in the first red circle. Because the RED participant has stopped before the BLUE participant: The RED participant earns: 40 The BLUE participant earns: 10

Example 2:

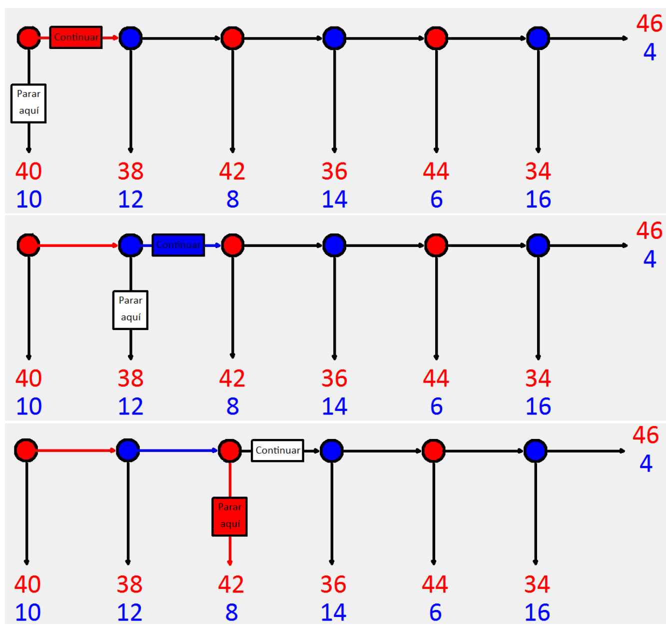


FIGURE 3.11: EXAMPLE 2

The RED participant chose "Continue"in the first red circle. Then the BLUE participant chose "Continue"in the first blue circle. Finally, The RED participant chose "Stop here"in the second red circle. A a result: The RED participant earns: 42 The BLUE participant earns: 8

Example 3:

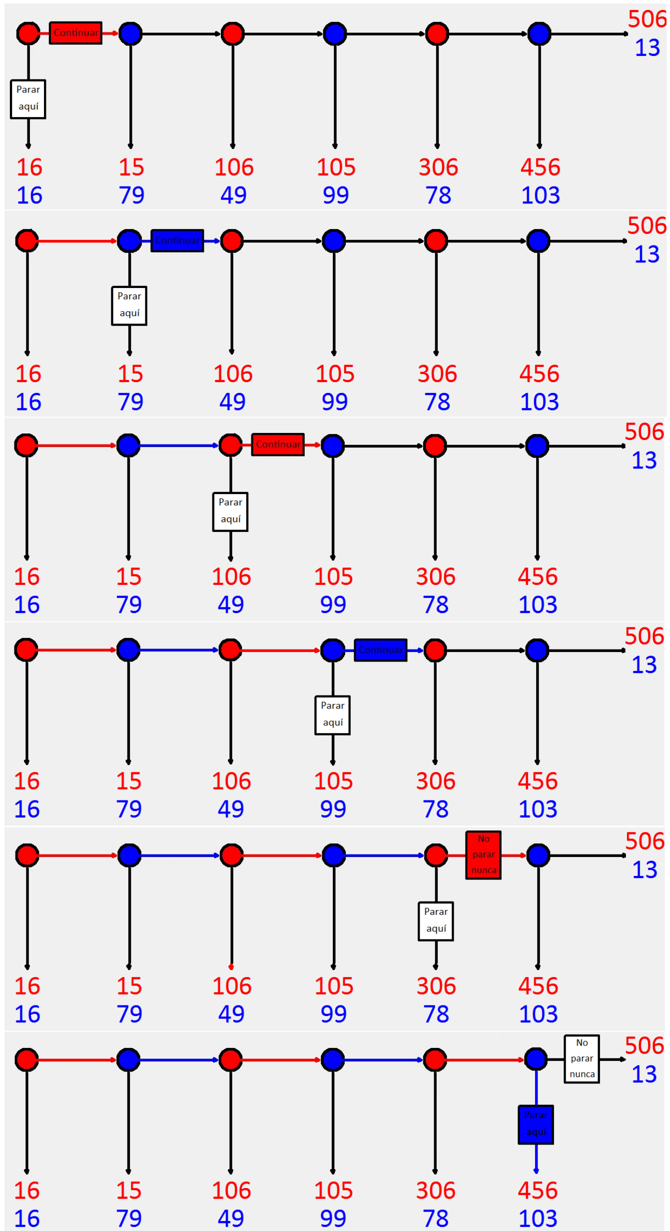


FIGURE 3.12: EXAMPLE 3

The RED participant chose “Continue”in the first red circle. Then the BLUE participant chose “Continue”in the first blue circle. In the second red circle, the RED participant chose “Continue”. In the second blue circle, the BLUE participant chose “Continue”. In the third red circle, the RED participant chose “Never stop”. Finally, the BLUE participant chose "Stop here"in the second blue circle. A a result: The RED participant earns: 456 The BLUE participant earns: 103

Note: These examples are just an illustration. The experimental points that appear are examples, i.e. they are not necessarily the ones that will appear in the 16 games. In addition, the examples ARE NOT intended to suggest how anyone should choose between the different options.

How the computer works: In each game, you will see 2 white boxes, one for each of your possible options. To choose an option, click on the corresponding box. When you have selected an option, the box will change color, as shown in the examples. This choice is not final: you can change it whenever you want by clicking on other box as long as you have not yet clicked the "OK" button that will appear in the bottom-left corner of each screen. Once you click "OK" your choice will be final and you will move on to the Next decision. You cannot pass on to the next decision until you have chosen an option and have clicked "OK".

At the end of each round you will see a summary of what happened in that round.

Earnings:

Once you have submitted your choices in the 16 games, the computer chooses Two rounds at random for each participant for payment. You will be paid depending on the actions that you chose and the ones that the participant you were matched with chose in each of those two rounds.

At the end of the experiment, you will be informed about which were the two rounds selected for payment, which were the decisions made by you and the ones of the corresponding participant you were matched in those rounds and what will be your final payment.

Summary:

- The computer will choose randomly whether you are a RED or BLUE participant for the whole experiment.

- You will participate in 10 rounds of the same game and in each of them you will be matched randomly with a participant of the other color.

- In each game, each participant can choose between two different options in three different circles: stop or continue in the first circle of his/her color, stop or continue in the second circle of his/her color, and stop or "Never stop"in the third circle of his/her color. The quantities are always the same in each round and the participant that chooses "Stop here" before the other participant is the one that ends the game and determines the experimental points for both participants.

At the end, the computer will randomly choose 2 of the 10 rounds for each player, and you will be paid depending on the actions chosen by you and by the participant you were matched to in each of those two rounds.

The experiment will start shortly. If you have any questions or you need help, please, raise your hand and one of the researchers will help you.

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