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Three Essays on Behavioral Economics

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Resumen

Esta tesis presenta tres ensayos enmarcados dentro de la economía conductual. El campo de la economía conductual se caracteriza por trascender algunos de los planteamientos del enfoque clásico en economía, que intenta explicar los fenómenos económicos mediante la caracterización del comportamiento de agentes perfectamente racionales y motivados exclusivamente por el propio interés, para incorporar limites en la racionalidad de los agentes y motivos distintos del interés propio. Para tal fin, los economistas conductuales estudian cómo variables tales como la cognición, la motivación, las emociones y los factores sociales y culturales influyen en la determinación de los resultados económicos.

En mi tesis adopto esta perspectiva y estudio tres temas de relevancia ecnonómica desde una perspectiva conductual. En el primer capítulo, estudio como el fenómeno denominado Disonancia Cognitiva, consistente en una sensación displacentera que se genera cuando un individuo alberga dos o más ideas mutuamente contradictorias, influye tanto en las preferencias como en la propensión a realizar esfuerzo de los individuos. El segundo capítulo se enmarca dentro del area denominada Voto Económico, la cual estudia cómo las condiciones económicas pueden influir en el voto electoral. En este capítulo estudio dos fenómos conductuales. El primero es cómo los individuos usan las condiciones económicas a nivel agregado cómo pista o señal para interpretar su situación económica personal, de modo que en diferentes contextos económicos los sujetos hacen atribuciones causales distintas sobre el desempleo, lo cual a su vez lleva a diferentes decisiones electorales. Segundo, como los indidivuos pueden tener distintas motivaciones para votar en función de su situación y perspectivas económicas. En el tercer capítulo, estudio como dos estilos cognitivos particulares, la impulsividad y la intuition, influyen en la cooperación.

A lo largo de mi disertoción realizo una exploración de algunas de las principales herramientas empleadas para hacer investigación en el campo de la economía conductural. Como consecuencia, en cada capítulo empleo una metodología distinta. El primer capítulo está compuesto por una primera parte consistente en un modelo teórico y una segunda parte en la que ofrezco apoyo empírico a las conclusiones de

este modelo con un estudio correlacional efectuado mediante el re-análisis de datos de un experimento previamente conducido. El segundo capítulo consiste en un estudio causal realizado mediante el análisis de datos de campo provenientes de un experimento natural. El capítulo tres consiste en un estudio causal realizado mediante un experimento aleatorio diseñado ad hoc.

A continuación se muestra un resumen pormenorizado de cada capítulo.

Capítulo 1: Preferencias Distributivas y Provisión de Esfuerzo: Una relación bidireccional

Este capítulo estudia la relación entre el esfuerzo y las preferencias distributivas. En primer lugar, presento un modelo teórico que establece una relación bidireccional entre ambos. El modelo se basa en dos simples ideas ampliamente aceptadas en la literura: el esfuerzo es costoso y los indidivuos sufren un conflicto entre el interés propio y el deseo de ser justos. En este modelo, los individuous obtienen utilidad del dinero y desutilidad del esfuerzo. Además de esto, experimentan disonancia cogntiva cuando la cantidad de dinero que se quedan es diferente de la cantidad que consideran justo quedarse. La disonancia cognitiva genera una desutilidad en una magnitud directamente proporcional a la diferencia entre la cantidad de dinero que el individuo se queda y la cantidad que cree que merece. Para maximizar su utilidad, los individuos escogen su nivel de esfuerzo y qué cantidad de dinero quedarse, pero también escogen sus creencias respecto a qué cantidad de dinero merecen. Como resultado, las preferencias distributivas de un individuo y su provisión de esfuerzo se determinan conjuntamente, de modo que las primera pueden determinar las segundas o viceversa.

Estudios anteriores han documentado cómo la provisión de esfuerzo puede determinar las preferencias distributivas. En la segunda parte del capítulo aportamos evidencia empírica a favor de la relación contraria (i.e. cómo las preferencias distributivas pueden determinar la provisión de esfuerzo) a través del re-analisis de datos de un experimento previamente conducido. El experimento consiste en una secuencia de dos fases. En la primera, los sujetos participan en una tarea de esfuerzo real. En la segunda, los sujetos son aleatoriamente asignados a grupos de cuatro sujetos. Dentro de cada grupo los sujetos son ordenados en una clasificación y asignados una dotación inicial de dinero acorde a esta clasificación, de modo que el primer clasificado obtiene 16 €, el segundo 12 €, el tercero 10€ y el cuarto 6 €. Los sujetos son aleatoriamente asignados a una de las siguientes dos condiciones: *Earned Treatment* y *Random Treatment*. En la condición *Earned Treatment* la clasificación de los sujetos en la segunda fase del experimento está determinada por su rendimiento en la tarea que hicieron en la primera fase, de modo que aquellos que más producieron en dicha tarea ocupan posiciones más elevadas en la clasificación. En la condición *Random Treatment* la posición de cada sujeto en la clasificación es asignada de forma aleatoria. Una vez que los sujetos han recibido su dotación inicial, participan en un juego del dictador de cuatro personas. La distribución de dotaciones del grupo es mostrada a cada miembro del mismo y este es libre de redistribuir el dinero como él desee entre los miembros del grupo. Después, un miembro de cada grupo es escogido aleatoriamente como dictador y el dinero de su grupo es redistribuido según su decisión. Los sujetos de nuestra muestra participaron dos veces en este experimento, habiendo transcurrido un año entre la primera y la segunda vez que participaron.

Los resultados de nuestro análisis muestran que las preferencias reveladas por los sujetos mediante las decisiones tomadas en el juego del dictador del primer año predicen el esfuerzo ejercido en la primera fase del experimento del segundo año, independientemente de la condicion (*Earned Treatment* o *Random Treatment).* Los sujetos que hacen una distribución igualitaria el primer año tienden a ejercer menos esfuerzo el segundo año que aquellos que hicieron una distribución egoísta.

Capítulo 2: Fluctuaciones a corto plazo en el desempleo y participación electoral

Este capítulo estudia la relación entre el desempleo y la participación electoral. Planteo la teoría de que el impacto del desempleo en la participación electoral depende de una interacción entre variables contextuales y dos motivación del voto clásicas dentro de la literatura: el voto psicotrópico y el voto sociotrópico. En primer lugar, formulo la hipótesis de que el contexto económico puede adectar la percepción de los votantes de las causas del desempleo y que, por tanto, existe una mediación del contexto en el impacto del desempleo sobre el voto. Cuando la tasa de desmpleo es baja, los votantes tienden a percibir la situación de desempleo como un fracaso personal y abandonar la participación en política. Cuando el desemplo es alto, por el contrario, los votantes perciben la situación de desempleo como un problema social, del cual los representantes electos son responsables, y aumentan su propensión a votar. En segundo lugar, planteo la hipótesis de que la importancia relativa de la motivación psicotrópica (i.e.basada en la situación personal del votante) y sociotrópica (i.e. basada en la situación general de la economía) del voto depende de los intereses de cada uno de los diferentes estratos de la población. En el ámbito partícular del desempleo, el peso relativo de los motivos psicotrópicos tendera a ser mayor en comparación con los sociotrópicos mientras más personal sea el efecto potencial del desempleo sobre el votante. Por tanto, el voto de los participantes del mercado laboral dependerá principalmente de su situación laboral personal y la preocupación por las magnitudes macroeconómicas será relativamente peequeña. Sin embargo, el voto de los individuos fuera del mercado laboral mostrará una mayor tendencia a descansar sobre motivos sociotrópicos. Esta tendencia será tanto mayor cuanto más desvinculado esté el individuo del mercado laboral. Por ejemplo, El voto de algunos estratos poblaciones como los estudiantes, los cuales tienen la perspectiva de incorporarse al mercado laboral una vez finalizados sus estudios, podrá esta motivado por factores sociotrópicos, pero en una medida menor que el de los pensionistas, cuyos ingresos no dependen del trabajo.

Para probar mis hipótesis, aprovecho la coyuntura ofrecida por el experimento natural generado por las elecciones generales de España de los años 2015 y 2016. El 20 de diciembre de 2015, España celebró las úndecimas elecciones generales desde su transición a la democracia. Ningún partido obtuvo un número de votos suficiente como para formar un gobierno estable en solitatio y los principales partidos no alcanzaron un acuerdo que permitiera formar un gobierno. Estó llevó a un bloqueo institucional que culimnó con la convocatoria de nuevas elección el 26 de junio de 2016. Como consecuencia, España tuvo dos elecciones generales en menos de siete meses. Esto me permite sobreponerme a un problema típico en estudios electorales. El intervalo normal entre dos elecciones consecutivas es de cuatro años. Este tiempo es lo suficientemente largo como para que durante el mismo se produzcan diversos cambios en las condiciones sociales y económicas del país, las cuales pueden actuar como variables de confusión impidiendonos hacer una evaluación limpia del impacto del desempleo en el voto. En el extraordinariamente breve intervalo de tiempo que separa las elecciones del 2015 de las del 2016, la economía española no sufrió cambios de gran envergadura y las perspectivas de crecimiento y creación de empleo no fueron sustancialmente modificadas. No obstante, hubo una importante variación interregional en el desempleo. En este estudio aprovecho la desigual distribución de las variaciones estacionales del desempleo entre las distintas áreas geográficas para medir el efecto de las fluctuaciones del desempleo a corto plazo sobre la participación electoral en un contexto en el que el estado general y las perspectiva a largo plazo de la economía permenecen estables.

Para efectuar mi análisis, empleo una combinación de datos agregados de registro y datos individuales provenientes de encuestas. Primero realizo un análisis de nivel agregado en el que estudio como las fluctuaciones en el desempleo afectan a la participación electoral mediante una serie de modelos en diferencias. Hallo que los incrementos en el desempleo llevan a incrementos en la participación electoral, pero solo en regiones en las que la tasa de desempleo era inicialmente alta. Por tanto, confirmo que la relación entre desempleo y participación electoral depende del contexto.

En la segunda parte, combino datos individuales de encuesta con los datos agregados usados en la primera parte. Estos datos me permiten, en primer lugar, estudiar como las decisiones individuales de voto son afectadas por el desempleo y, en segundo, investigar cómo varía la relación entre estras dos variables en diferentes estratos poblacionales. Para este propósito, me valgo de la técnica de inferencia causal conocida como diferencias en diferencias. En consonancia con los resultados hallados en el análisis de nivel agregado, encuentro una relación positiva entre el desempleo y la participación, pero solo en individuos residentes en regiones con una tasa de desempleo inicialmente alta. Sin embargo, esta relación no es uniforme para todos los estratos poblacionales. Para los participantes del mercado de trabajo, la decisión de votar está determinada por los cambios en la situación personal de empleo (i.e. pérdida u obtención de empleo) mientras que las fluctuaciones en la tasa de desempleo no tienen un efecto relevante. Solo las decisiones de los pensionistas se ven influenciadas por las fluctuaciones en la tasa de desempleo. Por tanto, los resultados sugieren que mientras más personalmente ligado está un individuo al mercado de trabajo mayor es la importancia de los motivos psicotrópicos en sus decisiones de voto y menor la de los motivos sociotrópicos.

Capítulo 3: Estilos cognitivos y cooperación: Intuition vs Impulsividad

Este capítulo estudia la relación entre la cooperación y dos estilos cognitivos diferentes: la impulsividad y la intuición. Existe una amplia literatura que estudia la relación entre la intuición y la cooperación. La mayor parte de esta literatura a abordado esta relación desde la perspectiva de la Hipótesis de Heuristicas Sociales (*Social Heuristics Hypothesis*). Según esta teoría, en las sociedades humanas, la cooperación acaba produciendo resultados positivos la mayoría de las veces. Como resultado, los individuos interiorizan la cooperación como estrategia por defecto. En situaciones en las que la cooperación es una respuesta subóptima, la respuesta más probable de un individuo que actue intuitivamente es cooperar. Investigadores de diversas áreas han intentado dilucidar la cuestión de la validez de la Hipótesis de Heuristicas Sociales a través de estudios meta-análiticos pero los resultados obtenidos han sido inconcluyentes. A mi juicio, esto puede deberse a dos motivos. El primero es el hecho de que algunas manipulaciones experimentales, consistentes en la reducción del autocontrol de los sujetos, las cuales se ha considerado que inducen un pensamiento intuitivo, en realidad inducen, no un pensamiento intuitivo, sino uno impulsivo. El segundo, una mediación del género en el efecto de la intuición.

Respecto al primero de estos problemas, mi hipótesis es que la impulsividad y la intuición son dos estilos cognitivos diferentes que deberían producir distintos tipos de comportamiento. La impulsividad es un estilo cognitivo caracterizado por la falta de autocontrol y que crea una tendencia a tomar aquellas decisiones que producen la mayor satisfacción inmediata, incluso cuando estás implican un menor beneficio a largo plazo. En el dominio de la cooperación, este estilo cognitivo debería llevar a los sujetos a maximizar su propio pago incluso cuando esto conlleve perdidas para otras personas. Por tanto, la impulsividad debería reducir la cooperación.

La intuición, en cambio, es un estilo cognitivo caracterizado por el uso de mecanismos de toma de decisiones rápidos y frugales en el uso de recursos cognitivos. Al contrario que los agentes impulsivos, los agentes intuitivos pueden considerar objetivos a largo plazo y metas complejas al tomar sus decisiones. Postulo que, tal y como plantea la Hipótesis de Heuristicas Sociales, los sujetos que actuan intuitivamente tienden a desplegar una respuesta por defecto adquirida mediante el apredizaje social. No obstante, argumento que esa respuesta será distinta para hombres y para mujeres, debido al hecho de que existen diferentes roles sociales para cada genero, cada uno prescribiendo un repertorio conductual distinto. Mientras que el rol femenino es comunmente asociado a comportamientos prosociales tales como el cuidado y la crianza, la masculinidad está asociada a comportamientos egoístas o incluso antisociales tales como la competición o la agresión. Como consecuencia, la intuición debería aumentar la cooperación solo en mujeres, mientras que debería aumentar el egoísmo en hombres.

Por tanto, formulo dos hipótesis principales. En primer lugar, que la impulsividad disminuye la propensión a cooperar. En segundso lugar, que la intuiticón aumenta la propensión a cooperar en mujeres y la disminuye en hombres. Para poner a prueba estas hipótesis, conduzco un experimento diseñado ad hoc para tal propósito. En el experimejnto los sujetos juegan a un Juego del Bienes Públicos. En este juego, los sujetos son clasificados en grupos de cuatro personas, a cada una de las cuales se asigna una dotación de dinero. Cada uno tiene que decidir qué parte de esa dotación invertir en un proyecto común. Las contribución a este proyecto benefician al grupo en su conjunto, pero suponen una perdida personal. Por ende, el tamaño de las contribuciones puede considerarse una medida del nivel de cooperación de cada sujeto, Los sujetos son asignados aleatoriamente a una de las siguientes tres condiciones; un grupo de control en el que los sujetos no se someten a ninguna manipulación y dos grupos de tratamiento en los cuales los sujetos se someten a una manipulación experimental destinada a inducir un pensamiento intuitivo e impulsivo respectivamente: Carga Cognitiva (*Cognitive Load*) y Vaciado del Ego (*Ego Depletion*). Los sujetos en la condición de Carga Cognitiva deben participar en una tarea de distracción, consistente en memorizar una secuencia de 7 dígitos, simulatáneamente a la toma de su decisiónen el Juego de Bienes Públicos. El objetivo de esta tarea es disminuir la capicadad de los sujetos de hacer deliberaciones largas y costosas a la hora de tomar sus decisiones en el Juego de Bienes Públicos. De este modo, los sujetos se ven obligados a tomar decisiones intuitivas. En el tratamiento de Vaciado del Ego los sujetos participan en un test de Stroop antes de comenzar el Juego de Bienes Públicos. El objetivo de esta tarea es vaciar a los sujetos de autocontrol. De este modo, tenderán a actuar de forma impulsiva en las decisiones posteriores.

Los resultados revelan que la condición de Vaciado del Ego aumenta la propensión de los sujetos a actuar como polizones (*free riders*; i.e. hacer una contribución nula al proyecto común) en el Juego de Bienes Públicos, confirmando así que la impulsividad genera un aumento del egoísmo. El efecto de la condición de Carga Cognitiva, en cambio, esta mediado por el género; aumenta la propensión a comportarse como un polizón en hombres, mientras que aumenta la cooperación en mujeres.

Contents

Chapter 1

Distributive Preferences and Effort Provision: A two-way link

1.1 Introduction

This paper studies two prominent topics in Economics: effort provision and distributive preferences. Effort provision naturally determines individual, group-level, and firm-level economic outcomes, and distributive preferences have important consequences in many socio-economic contexts. Importantly, the literature systematically documents considerable heterogeneity in both how much effort people provide (Alesina and Giuliano, 2011) and their distributive preferences (Cappelen et al., 2007). As a result, a large literature across several disciplines has explored the determinants of effort and preferences for redistribution. The former mostly focuses on the role of incentives (see Prendergast (1999) for a survey or e.g. Dohmen and Falk (2011) for more recent evidence; see also footnote 4 for other determinants). Regarding distributive preferences, the literature has widely analyzed their intergenerational transmission (Piketty, 1996; Alesina et al., 2018), the role of future income prospects (Alesina and Ferrara, 2005), or the effect of macroeconomic outcomes such as the income distribution (Kuzmienko et al. 2015); see Alesina and Giuliano (2011) for a review.

As for the connection between the two, many scholars have asked whether effort provision can determine distributive preferences. Some studies have detected a negative association between socio-economic status and the demand for redistribution (Alesina and Ferrara, 2005; Alesina and Giuliano, 2011; Barr et al., 2015; Barr et al., 2016). Since higher economic status might be partially linked to higher effort, this provides an indirect link from effort to preferences. Other scholars have sought more direct evidence and, in line with the above findings, they observe that people who make more effort claim larger parts of the "pie" while others call for more egalitarian distributions (Konow, 2000; Erkal et al., 2011; Rodriguez-Lara and Moreno-Garrido, 2012; Úbeda, 2014). This is known as *self-serving bias*. These papers document that effort provision determines human distributive preferences.

We are aware of two studies that suggest that the reverse relationship might also exist, i.e. distributive preferences may determine how much effort people exert.[1] First, Bandiera et al. (2005) compare the performance of workers under a piece-rate payment scheme and under an incentive scheme in which they are paid in relation to the average performance. In the latter, more effort increases workers' pay but imposes negative externalities on coworkers. Bandiera et al. (2005) find that subjects produce 50% more under the piece-rate payment. They argue that workers internalize the externalities imposed on others and interpret their findings as evidence for social preferences. This might suggest that social preferences determine effort. Nevertheless, they additionally report that the difference in productivity under the two incentive structures disappears when workers cannot monitor each other. Hence, it is not possible to rule out other explanations, such as peer effects as detected in Falk and Ichino (2006). Second, Erkal et al. (2011) propose a two-stage experiment. In the first stage, players participate in a real-effort task and are then ranked according to their performance, such that those who produce more receive higher payoffs. In the second stage, they can decide whether to donate part of their payoffs to other players (and how much), knowing the payoff of each individual within the relevant group. They observe that subjects who rank first are less likely to give to others than those ranked second. They hypothesize that selfish subjects self-select to the first rank by working harder than others, suggesting a possible link from preferences to effort. They then design a new set of treatments to test this hypothesis. Even though they find evidence for their conjecture, their experiment only provides an indirect test of a directional link from preferences to effort because effort and preferences are only elicited once in all their treatments and effort provision always precedes the elicitation of distributive preferences.

The objective of this paper is to provide an exhaustive analysis of the relationship between preferences for redistribution and effort provision in an environment, in which effort does not affect the total amount to be distributed in a society. We first propose a theoretical model, in which (i) people decide both how much effort to exert and what principle of justice to follow; (ii) there is a trade-off between self-interest and a principle of justice, typically present in models of social and distributive pref-

[1] As another example of a link from preferences to effort, Gill and Prowse (2012) explore how aversion to disappointment shapes effort provision.

erences (see e.g. Konow, 2000; Camerer, 2003; Cappelen et al., 2007; or Cooper and Kagel, 2016); and (iii) effort is costly. The model delivers the following messages. First, if how much one should work and what principle of justice to follow are determined jointly, people who work more prefer less redistribution and claim larger shares of the pie or, conversely, lower-effort individuals claim more egalitarian shares and request more redistribution. This is in line with prior evidence (Konow, 2000; Erkal et al., 2011; Rodriguez-Lara and Moreno-Garrido, 2012; Úbeda, 2014). In other words, the self-serving bias documented elsewhere arises naturally from utility maximization.[2] However, our model also generates the reverse hypothesis, suggested by Bandiera et al. (2005) and Erkal et al. (2011) but not directly tested in the literature: selfish individuals should make greater effort, whereas egalitarians should work less.

In the second part of the paper, we test the link running from distributive preferences to effort. We exploit a unique database of 275 subjects who participated in a real-effort experiment with distributional choices. The data set was originally designed to test the impact of real-life employment conditions on experimental behavior,[3] but it provides a great opportunity to test the link from preferences to effort. As opposed to standard experimental data, the subjects participated in an identical experiment twice, once in 2013 and then one year later, and the data are particularly rich in the pool composition and the information available. We exploit the temporal structure of the experiment, the presence of active labour-market participants contributes to the external validity of our results, and the available information enables to control for a large number of confounding factors.

Using a variety of methods and measures of distributive preferences, we show that the more unequal the distribution that an individual proposes the more effort she makes one year later. In fact, egalitarian individuals turn out to exhibit the lowest effort levels. This is true for both absolute effort and the change in effort between the two years. Additionally, we show that these results only hold if we consider the self-centered inequality (i.e. the bilateral payoff differences proposed by a subject between herself and the other group members), while there is no relation between effort and the inequality one proposes among the other members (non-self-centered inequality; Macro and Weesie, 2016). Our empirical strategy and robustness checks enable us to conclude that our results are not driven by changes in distributive

[2] An agent exhibits self-serving bias in our framework if she accepts a fairness principle that most benefits herself.

[3] These data were collected by Barr et al. (2016). Barr et al. (2016) and Demel et al. (2016) report complementary results using the same data set.

preferences or changes of economic status of subjects over time.[4]

The temporal structure of the experiment and the number of controls, robustness tests, and additional exercises notwithstanding, this study does not seek to identify a causal relationship. The main argument of the present study is rather the contrary: The link between distributive preferences and effort provision is bidirectional. Effort shapes human distributive preferences and distributive preferences predict how much effort people make. As a result, scholars should use causal claims regarding effort and preferences with caution and each phenomenon might be difficult to study in isolation. Our theory provides one explanation. If people adopt their fairness principles endogenously (and there is considerable evidence in support of this conjecture; see e.g. Barr et al., 2016, or Rodriguez-Lara and Moreno-Garrido, 2012), people align their preferences with the effort and the effort exerted with their preferences. That is, the bidirectional link between effort and preferences arises naturally from utility maximization. These findings provide further evidence that preferences might be endogenous (see Bowles (1998) or Fehr and Hoff (2011) for two seminal treatments of this topic). We thus contribute to the literature by providing a partial explanation of the pluralism of fairness ideals (Cappelen et al., 2007). Simultaneously, the results place distributive preferences among the determinants of effort and productivity.

The rest of the paper is organized as follows. Section 1.2 provides a theoretical link between distributive preferences and effort. Section 1.3 presents the experimental procedures. The results are reported in Section 3.3. Section 1.5 concludes. The Appendix contains further details regarding the results and numerous robustness checks.

1.2 Theoretical link between distributive preferences and effort

In this section, we study the link between effort and preferences for redistribution theoretically. The model proposed is based on two simple ideas widely accepted in the literature: People face a conflict between self-interest and fairness and effort is costly. It is well documented that people have conflicting motives between their self-interest and fairness principles. Their self-interest prescribes them to keep as much as possible for themselves, while fairness criteria tell them to treat others fairly.

[4] We also include a rich set of controls that contain other standard determinants of effort provision, such as gender (Gneezy et al., 2003; Niederle and Vesterlund 2011; Aspeteguia et al., 2012; Azmat et al., 2016; Bandiera et al., 2016), current economic status (Alesina and Giuliano, 2009), and experimental rank (Gilll et al., 2015) among other examples; see the Appendix.

Different people resolve this conflict in different ways. The standard approach is to predict behavior holding fairness principles constant, while our theory rather argues that the effort decision and the decision on how to treat others may both result endogenously from a decision process.

In our framework, people choose their effort level e and how much to keep for themselves y, but they also decide their beliefs as to how much they deserve, ϕ. This is in line with empirical evidence (see e.g. Konow, 2000, for early evidence or Gino et al., 2016, for a recent review). Assume $e \in [\underline{e}, \overline{e}]$ with $\underline{e} \geq 0$, which is thought of as the minimal effort that one is willing to exert.[5] Let $y \in [0, \overline{y}]$, where $\overline{y}$ is the total amount to be divided in a group of n subjects. The amount of $\overline{y}$ is fixed and independent of effort in our setting.[6]

The utility of an individual has three components. First, people have a utility from their material payoff $v(.)$, which we assume to be twice continuously differentiable, increasing, and concave. Second, people compare the amount that they keep for themselves to what they believe that they deserve. We assume that even selfish individuals are well aware of what might be fair.[7] For the sake of simplicity and in line with our experimental results, we assume that people can choose between two fairness rules. They either choose to believe in egalitarianism or they select a proportional rule according to which a fair allocation scales up with one's effort. That is, $\phi \in \{\frac{\overline{y}}{n}, \phi(e)\}$ with $\phi(.)$ being differentiable and continuous, satisfying $\phi_1(e) > 0$, and $\phi(\underline{e}) < \frac{\overline{y}}{n} < \phi(\overline{e})$. Therefore, there exists $\widehat{e}$, such that $\phi(\widehat{e}) = \frac{\overline{y}}{n}$. Lastly, effort is costly with an increasing and convex cost function $c(.)$. We normalize $c(\underline{e}) = 0$.

The decision problem of each individual is

$$\begin{gathered}\max_{\{e,y,\phi\}} \; U(e, y, \phi) = v(y) - f(y - \phi, \alpha) - c(e) \\ \text{s.t. } e \in [\underline{e}, \overline{e}],\; y \in [0, \overline{y}],\; \phi \in \left\{\tfrac{\overline{y}}{n}, \phi(e)\right\}\end{gathered}$$

where $f(.)$ reflects the (non-pecuniary) cost of deviating from what one considers fair for oneself and $\alpha \in [0, 1]$ denotes how sensitive an individual is to a fairness norm. The function $f(.) \geq 0$ is assumed to be twice continuously differentiable with $f_1(.), f_2(.) > 0$ and $f_{11}(.), f_{12}(.) > 0$. That is, the disutility from not adhering to a sharing rule increases in both arguments, raises at increasing rates in the first argument, and the increases are higher for individuals with higher α. Moreover, $f(0, \alpha) = 0$ for any α and $f(x, \alpha) \to \infty$ as $\alpha \to 1$.

[5] The parameter $\underline{e}$ may be set to zero and may differ from one person to another.

[6] In our experiment, $n = 4$ and $\overline{y} =$€44 .

[7] There is evidence that people who behave as selfish follow certain fairness criteria when allocating among strangers. See e.g. Konow (2000) or Gino et al. (2016).

Before we characterize the optimal behavior, observe that the proposed model can be viewed from two different–though related–perspectives. On the one hand, it embodies the idea of the conflict between selfish and non-selfish motives in the traditional models of distributive preferences (e.g. Cappelen et al., 2007).[8] Depending on the model parameters and the shape of $f(.)$, different fairness principles such as e.g. selfishness and egalitarianism are special cases of our general specification. The difference between these models and our approach is that we let people choose endogenously the belief ϕ and, consequently, their fairness type. This feature brings us closer to the second perspective. Cognitive dissonance is a term used in psychology referring to the discomfort of holding conflicting views or beliefs (Festinger, 1957), such as the discussed conflict between self-interest and sharing rules. Konow (2000) argues that people may "choose" their beliefs regarding what is their fair share and reports evidence in support of his claim (see Gino et al., 2016, for a recent survey). Our approach may be considered a variation of the cognitive-dissonance model in Konow (2000) with a cost of effort.

Proposition 1 *(i) The optimal effort $e^* = \underline{e}$ if and only if $\phi^* = \frac{\overline{y}}{n}$. In that case, $y^* \geq \frac{\overline{y}}{n}$, with y^* being decreasing in α and $y^* = \frac{\overline{y}}{n}$ as $\alpha \to 1$.*

(ii) $e^* > \underline{e}$ if and only if $\phi^* = \phi(e)$. In that case, $y^* > \frac{\overline{y}}{n}$, y^* increases in e^*, and $y^* = \phi(e^*) > \frac{\overline{y}}{n}$ as $\alpha \to 1$.

Proof. If $e^* = \underline{e}$, $c(\underline{e}) = 0$. Then, $U(\underline{e}, \frac{\overline{y}}{n}, \frac{\overline{y}}{n}) > U(\underline{e}, y, \phi)$ for any $y < \frac{\overline{y}}{n}$ and ϕ and, since $\frac{\overline{y}}{n} > \phi(\underline{e})$ by assumption, $U(\underline{e}, y, \frac{\overline{y}}{n}) > U(\underline{e}, y, \phi(\underline{e}))$ for any $y \geq \frac{\overline{y}}{n}$. This proves the "if" part of (i). Now, if $\phi = \frac{\overline{y}}{n}$, $U_1(e, y, \phi) < 0$, proving the "only-if" part. Lastly, $U(\underline{e}, \frac{\overline{y}}{n}, \frac{\overline{y}}{n}) > U(\underline{e}, x, \frac{\overline{y}}{n})$ for any $x < \frac{\overline{y}}{n}$, showing that $y^* \geq \frac{\overline{y}}{n}$.

As for (ii), if $e^* > \underline{e}$, $U_1(e, y, \frac{\overline{y}}{n}) < 0$ for any e, y. Thus, $e^* > \underline{e}$ cannot be optimal and $\phi^* = \phi(e)$. Conversely, $\phi^* = \phi(e)$ only if (y^*, e^*) satisfy $v(y^*) - f(y^* - \phi(e^*), \alpha) - c(e^*) \geq v(y^*) - f(y^* - \frac{\overline{y}}{n}, \alpha) - c(e^*)$. This implies that $\phi(e^*) \geq \frac{\overline{y}}{n}$ and this only holds if $e^* > \widehat{e} > \underline{e}$. Moreover, $\frac{\partial u(.)}{\partial y} = v_1(y^*) - f_1(y^* - \phi(e^*), \alpha) = 0$, leading to $\frac{dy^*}{de^*} = \frac{-f_{11}(.)\phi_1(.)}{v_{11}(.) - f_{11}(.)} > 0$ and $\frac{dy^*}{d\alpha} = \frac{f_{12}(.)}{v_{11}(.)} > 0$.

Since cognitive dissonance becomes prohibitively costly as $\alpha \to 1$, $y = \frac{\overline{y}}{n}$ in (i) and $y = \phi(e^*)$ in (ii) in the limit to avoid these costs. In (ii), $\phi(e^*) > \frac{\overline{y}}{n}$, since $e^* > \widehat{e}$. ■

Proposition 1 characterizes the optimal behavior of an individual who faces a conflict between self-interest and fairness (and, thus, "suffers" from cognitive dissonance). The main observation is the close link between the optimal effort and

[8]Such a conflict is also present in the standard models of social preferences (see Cooper and Kagel, 2016, for a survey).

the fairness principle selected in the optimum. If people are not willing to exert much effort–e.g. because their effort costs are high–it is optimal to exert minimal effort and follow the egalitarian fairness principle. However, if people decide to make an effort, they work hard enough and adopt meritocratic fairness rules so as to be able to take more than the egalitarian share of the pie. In such a case, there is a positive association between effort level and how much an agent claims for herself from the cash distributed. Hence, self-serving bias arises endogenously as part of the optimal decision in the model. Moreover, since no constraints are placed on the curvature of $\phi(.)$, high $\phi'(.)$ and $\phi''(.)$ in combination with high levels of effort may justify selfish-like decisions $y^* = \overline{y}$. Lastly, the optimal claims are associated with individual sensitivity to cognitive dissonance as expected.

The rest of the paper empirically tests the link running from preferences to effort.

1.3 Experimental procedures

1.3.1 Experimental design

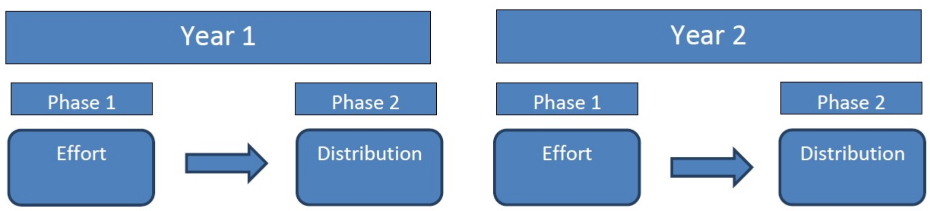

Figure 1.1: Temporal structure of the experiment.

We exploit an experimental data set that includes data on a two-wave experiment in the Spanish cities of Bilbao and Córdoba. In both cities, there were two waves of the experiment, one in 2013 (year 1) and another in 2014 (year 2). Figure 1.1 illustrates the temporal structure of the experiment. In 2013, 18 experimental sessions were conducted in Córdoba and 16 in Bilbao; 31 of these sessions involved 16 participants and 3 sessions involved 12 participants. A total of 532 participants participated in year 1. In 2014, a total of 16 sessions in Córdoba and 13 in Bilbao were conducted; 16 involved 16 participants and 13 involved 12, so a total of 412 participants participated in year 2. In this paper, we focus on the 275 subjects who participated in both waves. Table 3.1 summarizes their socio-demographic characteristics in the first year and their distribution across treatments (see below for treatment details). In year 1, 100 participants were unemployed, 85 were in work, and 90 were full-time students. 55.65% of participants were women. The ages ranged from 18 to 35, with an average of 26.11 years. Lastly, 54.18% of the participants (and a slightly higher percentage of those who were unemployed) were from Córdoba and the rest from Bilbao. Barr et al. (2016) provide further information regarding recruitment and anonymity preservation. Most importantly, Barr et al. (2016) provide an exhaustive analysis of whether those participating in both years differ in any respect from others. They find no differences between those who participated in year 1 and returned in year 2 and those who participated in year 1 but did not return in year 2. This makes us confident that our results are not driven by any particular characteristic of the 275 individuals in the sample under study. Naturally, no participant in 2013 was informed about the possiblity to participate in 2014.

In each year, subjects participated in the same two-stage experimental protocol. Participants were distributed randomly and anonymously into groups of four and participated sequentially in the following two phases:

	All	Students	Employed	Unempl.
Sample Size	275	90	85	100
Characteristics				
Female (%)	55.64	56.67	61.18	50
Age (mean)	26.11	22.33	27.83	28.05
Location				
Bilbao (%)	45.82	47.78	47.06	43
Córdoba (%)	54.18	52.22	52.94	57
Treatments*				
Earned (%)	60.36	60	63.53	58
Random (%)	39.64	40	36.47	42

*The two treatments, Earned and Random, are explained below.

Table 1.1: Overview of the participants (year 1).

Phase 1. In the first phase, players took part in a real-effort task. There were two types of task,: *filling* and *emptying*. In the former, each subject received a box of yellow and blue gravel and a tray of small plastic pots. The task consisted of filling as many small pots as possible with exactly seven yellow and seven blue pieces of gravel. In the emptying task, each participant received a tray of small pots filled with yellow and blue gravel. The task was to empty the pots and sort the gravel according to color; yellow gravel into one container and blue into another. Observe that the filling task prepares the material for the emptying task and vice-versa. See Barr et al. (2016) for the English translation of the instructions. The tasks were designed to be as simple as possible, such that any differences in production would be due to differences in effort rather than in abilities or inherent skills. Table 1.2 summarizes the effort variable separately for the two years and tasks. Even though production tended to be higher for the filling task, the effect of preferences on these variables was found to be the same for both tasks. We thus normalize the production in each task and pool the data from both tasks to obtain a single measure of effort for all participants.

	Both Years	Year 1	Year 2
Random Treatment			
Pots Filled	19.98	18.72	21.37
	(4.92)	(4.39)	(5.12)
Pots Emptied	39.30	39.66	39
	(8.65)	(7.15)	(9.81)
Earned Treatment			
Pots Filled	21.20	20.27	22.06
	(4.39)	(4.13)	(4.47)
Pots Emptied	41.09	40.91	41.32
	(8.36)	(6.54)	(10.25)

Table 1.2: Average performance (and standard deviation) in the real effort task.

Phase 2: In the second phase, the four members of each group were ranked[9] and everybody received an endowment in such a way that the higher rankings corresponded to higher endowments. The distribution of money among the four members of each group was the same for all groups in all the experimental sessions. The players ranked first obtained €16, those ranked second obtained €12, those ranked third €10, and those ranked fourth €6. This makes a total of €44 for each group ($\overline{y} = 44$). Since this number does not depend on effort, efficiency reasons behind effort provision can be ruled out. For the actual task, each player received a tray showing the whole distribution of endowments among the four members of her group. Her endowment in the tray was shown in blue, while the endowments of the other members in yellow. The task of each participant in this phase was to redistribute the four endowments among the four members subject to a single constraint: That the

[9]The ranking details are described below.

total amount among the four players (including herself) must be €44. Most importantly, there was no obligation whatsoever to respect the initial endowments. Since any proposed redistribution was possible in this task, each participant played the role of a Dictator in a four-person Dictator game. Once all participants had made their reallocation decisions, one of the four proposed distributions in each group was selected randomly for payment.[10]

There were two treatments: Earned and Random. In the Earned Treatment, production in the first phase determined the ranking of each subject in the second phase. Higher production led to higher ranking in the group. Ties were resolved randomly. In the Random Treatment, subjects were ranked randomly, so rank did not provide any information about efforts in the first phase.[11] All individuals participated in the same treatment in both years.Table 3.1 shows that 60.36% of the subjects participated in the Earned Treatment. In the main text, we pool the data from both treatments. In Appendix G, we show the results separately for each treatment.

1.4 Estimation results

Figure 1 shows the structure of our data. The data is a panel, with individuals being the cross-sectional variable ($i = 1, 2, ..., 275$) and the two years characterizing the temporal structure ($t = 1, 2$). We focus on the subjects who participated in both years, so our panel is balanced.

The main objective of the empirical part of this study is to test whether distributive choices determine effort provision. Since effort is elicited before the distributive task in both years, our main empirical strategy seeks to explain effort provision in year 2 on the basis of distributive choices in year 1, controlling and not controlling

[10]This feature of the experiment differs slightly from the assumptions in Section 1.2, since beliefs about others' types become relevant for the effort decision in the experiment. Incorporating such beliefs into the model would complicate the analysis but only generate straightforward predictions. An alternative reading of the theoretical prediction in Section 1.2 can be as follows: if two individuals hold the same beliefs regarding others' types, the more egalitarian individual will exert less effort and viceversa.

[11]Participants in the Earned Treatment were informed that their production would determine their endowments in the second phase, but they were not informed what the task in phase 2 would be. As a result of this design feature, it could be argued that behavior in years 1 and 2 is not comparable because in year 2 subjects knew the whole experimental procedure. We ran few sessions, in which people were informed about the whole procedure from the very beginning of the experiment in year 1. This increased effort but did not affect the link between distributive choices and efforts. Therefore, we only include an "information-treatment" dummy in our regressions below.

for effort in year 1. More precisely, we estimate variations of the following model:

$$e_{i2} = \beta_0 + \beta_1 d_{i1} + \beta_2 X_{i1} + \varepsilon_i, \tag{1.1}$$

where d_{i1} is a measure of distributive preferences in year 1 (specified in different ways below), and X_{i1} is a set of controls, including treatment dummies, first-year outcomes, and individual characteristics. The main interest lies in the estimate of β_1 that shows whether distributive choices in year 1 explain effort provision in year 2.

To analyze the robustness of our results, we estimate variations of our model that–most importantly–differ in the measure of distributive preferences d_{it} in the following subsections. We also estimate other variants in which we use different sets of controls, and a few models only use a subsample of our population. Section 1.4.4 reports additional results.

1.4.1 Pure preference types

As a starting point, we classify people into three classic types:

1. *Selfish* subjects keep all the group payoff for themselves in the distribution phase. Formally, individual i is selfish if

$$y_i = \overline{y} \text{ and } y_j = 0 \text{ for } j \neq i.$$

2. *Egalitarian* subjects distribute the payoff equally in the distribution phase; that is,

$$y_i = y_j = \frac{\overline{y}}{n} \text{ for each } j.$$

3. *Proportional* individuals respect the initial endowment and leave the distribution unchanged.

Table 1.3 summarizes the percentages of subjects according to each of these principles of justice in our subsample of 275 individuals. The table shows that each type is present in the population, but the numbers depend on the treatment and differ form one year to the other. Moreover, less than half the sample can be classified into one of these categories; i.e. more than 50% of subjects do not fit into these pure types. We start our analysis using these pure types because of their prominence in the literature. The next section proposes alternative measures of subjects' distributive preferences.

Year 1	Selfish	Egalitarian	Proportional	Other
All	7.27%	30.91%	8.73%	53.09%
Earned Treatment	9.64%	27.11%	13.86%	49.39%
Random Treatment	3.67%	36.70%	0.92%	58.71%
Year 2	Selfish	Egalitarian	Proportional	Other
All	10.18%	19.27%	5.09%	65.46%
Earned Treatment	13.25%	17.47%	7.23%	62.05%
Random Treatmen	5.50%	22.02%	1.83%	70.65%
Both Years	Selfish	Egalitarian	Proportional	Other
All	8.73%	25.09%	6.91%	59.27%
Earned Treatment	11.45%	22.29%	10.54%	55.72%
Random Treatment	4.59%	29.36%	1.38%	64.67%

Table 1.3: Classification of the 275 subjects who participated in both years according to each principle of justice

The main estimation results relating subjects' types in year 1 to their efforts in year 2 are shown in Table 1.4. The regressions reported differ in two aspects. Regressions $(1-2)$ do not control for effort in year 1, while models $(3-4)$ do. Past performance enables us to control–among other things–for potential skill differences. Additionally, regressions (2) and (4) include more controls than (1) and (3). The dummies *Proportional*, *Egalitarian* and *Other* are set to one if a subject behaves according to the corresponding principle of justice and 0 otherwise. The behavior of *Selfish* subjects thus represents the benchmark category. In all models, we also control for treatment (*Treatment=1* for the Earned treatment) and gender, since women consistently produce more than men.[12]

The estimates show that *Proportional* individuals produce less than selfish one, but the difference is not significant. *Egalitarians* produce on average 0.840 standard deviations less than selfish subjects in model (1). The coefficients decrease in absolute values if other controls are included as well as if we control for past performance. However, the coefficients are always highly significant. Quantitatively, model (4) in Table 1.4 suggests that, compared to the average production in each task, egalitarians produce 15.3% less than the average production in the filling task and 14.5% less in the emptying task than selfish individuals. These numbers show that the difference in performance between selfish and egalitarian individuals is economically significant. Appendices A and B show that the results in Table 1.4 are robust to other model

[12] Appendix A contains variants of the models and lists all the control variables and their estimates in models (2) and (4).

	Dependent variable: effort provision			
	(1)	(2)	(3)	(4)
Proportional	-0.439	-0.487	-0.282	-0.336
	(0.316)	(0.318)	(0.293)	(0.300)
Egalitarian	-0.840***	-0.825***	-0.617**	-0.686***
	(0.261)	(0.262)	(0.243)	(0.248)
Other	-0.443*	-0.494**	-0.375	-0.410*
	(0.250)	(0.245)	(0.231)	(0.231)
Treatment	0.195	0.0590	0.0667	-0.0289
	(0.133)	(0.142)	(0.125)	(0.135)
Female	0.388***	0.351***	0.254**	0.274**
	(0.127)	(0.125)	(0.119)	(0.119)
$\text{Production}_{t=1}$			0.462***	0.472***
			(0.0677)	(0.0794)
Constant	0.351	0.865**	0.455*	0.706**
	(0.266)	(0.336)	(0.246)	(0.317)
Other Controls	NO	YES	NO	YES
Observations	275	275	275	275
R-squared	0.086	0.178	0.222	0.277

Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table 1.4: Effort provision and pure preference types.

specifications and less extreme definitions of our types.

1.4.2 Selfishness and Inequality

The previous subsection shows that purely selfish individuals produce more than pure egalitarians. However, a large number of individuals cannot be classified into any of the pure types. Therefore, this section provides further evidence regarding the link running from preferences to effort by applying two alternative measures of distributive preferences. We first compute an indicator of selfishness, reflecting simply the proportion of the total payoff that a player allocates to herself in year 1. Formally,

$$Selfishness_i = \frac{y_i}{\overline{y}},$$

where y_i again denotes the amount allocated by player i to herself and $\overline{y}$ is the total amount to be distributed. This indicator takes a value of 1 for strictly selfish individuals and would be zero for those who propose to keep no money for themselves.[13]

Observe that selfishness abstracts from how an individual distributes the money to others. Our second alternative measure of distributive preferences takes into account the distribution proposed for the whole group. More precisely, we propose the following measure of inequality:

$$Inequality_i = \frac{\sum_{i \neq j} |y_i - y_j| + \sum_{j \neq k} |y_j - y_k|}{(n-1)\overline{y}}. \tag{1.2}$$

The numerator in (1.2) is the sum of bilateral payoff differences between all the group members proposed by individual i, while $(n-1)\overline{y}$ is the maximal value that the numerator can take. The denominator normalizes (1.2) to lie between zero (all the group members earn the same amount) and one (one member of the group– be it the Dictator or anybody else–receives all the group money). Since virtually all subjects proposed at least the egalitarian amount for themselves, the proposed indicator of inequality lies between zero for egalitarians and one in the case of selfish individuals in our data. The values range from zero to one for the remaining subjects.

Table 1.5 corroborates the findings from Table 1.4. Both indicators of distributive preferences are positively related to production. In particular, the more selfish an individual is and the more unequal the distribution that she proposes, the more effort she exerts. Table 1.5 (and Appendix A) again show that the results are highly robust

[13]No subject proposed zero for herself in the experiment.

	Dependent variable: effort provision			
	(1)	(2)	(3)	(4)
Selfishness	0.976***	0.825***		
	(0.293)	(0.277)		
Inequality			0.809***	0.696***
			(0.227)	(0.215)
Treatment	0.0956	0.00969	0.0634	-0.0183
	(0.138)	(0.131)	(0.139)	(0.131)
Female	0.367***	0.291**	0.372***	0.296**
	(0.125)	(0.118)	(0.125)	(0.118)
Production$_{t=1}$		0.472***		0.471***
		(0.0790)		(0.0787)
Constant	-0.0255	-0.0354	0.211	0.164
	(0.234)	(0.220)	(0.221)	(0.208)
Other Controls	YES	YES	YES	YES
Observations	275	275	275	275
R-squared	0.177	0.276	0.181	0.280

Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table 1.5: The effect of selfishness and inequality on effort.

to the inclusion of different controls and other model specifications.[14]

In sum, we observe a positive association between subjects' efforts and their selfishness or, conversely, a negative association between effort and adherence to the egalitarian fairness principle.

1.4.3 Self-Centered vs. Non-Self-Centered Inequality

To study the mechanisms behind the results reported above, this section focuses on our measure of inequality. Macro and Weesie (2016) distinguish between two different notions of inequality: *Self-centered inequality* and *non-self-centered inequality.* For each individual, self-centered inequality measures the pairwise payoff differences between her payoff and those of other members of her group, ignoring the differences between others. Non-self-centered inequality, in contrast, ignores how one compares with the others and only measures inequality between them. Since Macro and Weesie (2016) show that both inequalities can explain behavior in multiplayer dictator games, we ask whether our results from previous sections are due to self-centered inequality, non-self-centered inequality, or both.

To that end, we define self-centered inequality as

$$SCI_i = \frac{\sum_{i \neq j} |y_i - y_j|}{(n-1)\overline{y}}$$

and non-self-centered inequality

$$NCI_i = \frac{\sum_{j \neq k} |y_j - y_k|}{(n-1)\overline{y}}.$$

Both measures lie between zero and one; $SCI_i = 0$ if individual i proposes as much for herself as for anybody else, while $SCI_i = 1$ if i proposes to keep all the money and proposes nothing for the others. For the latter, observe that NCI_i does not depend on y_i. Thus, $NCI_i = 0$ if the distribution proposed by i assigns the same amount to all $j \neq i$, independently of the amount kept by i, whereas $NCI_i > 0$ if any inequality among others is proposed and $NCI_i = 1$ if one individual gets all the money and others (including i) get nothing.[15]

Table 1.6 reports separate regressions for the association between effort and the two types of inequality. There is a significant positive relationship between self-centered inequality in year 1 and production in year 2, but the estimated effect of

[14] We reestimated models (3) and (4) using the standard deviation of each distribution and the Gini Index in Appendix C and obtain the same results.

[15] $NCI_i = 1$ never happens in our data.

	Dependent variable: effort provision			
	(1)	(2)	(3)	(4)
SC Inequality	0.797***	0.688***		
	(0.225)	(0.212)		
NC Inequality			-0.249	-0.350
			(1.720)	(1.610)
Treatment	0.0800	-0.00422	0.129	0.0367
	(0.138)	(0.131)	(0.145)	(0.137)
Female	0.371***	0.296**	0.317**	0.246**
	(0.125)	(0.118)	(0.127)	(0.119)
$\text{Production}_{t=1}$		0.471***		0.493***
		(0.0787)		(0.0800)
Constant	0.225	0.176	0.236	0.186
	(0.221)	(0.208)	(0.228)	(0.214)
Other Controls	YES	YES	YES	YES
Observations	275	275	275	275
R-squared	0.181	0.280	0.142	0.251

Standard errors in parentheses***; p<0.01, ** p<0.05, * p<0.1

Table 1.6: Self-centered and non-self-centered inequality and effort.

non-self-centered inequality is never significant and actually negative. Moreover, note that the estimated coefficient for self-centered-inequality in model (2) in Table 1.6 is similar to the estimated effect of inequality in regression (4) in Table 1.5 (0.688 vs. 0.696) and both models actually have the same R^2. This suggests that the relationship between our measure of inequality from Section 1.4.2 and production is mainly driven by how one individual treats others compared to herself and does not seem to depend on the inequality generated between others.

1.4.4 Additional results

In this subsection, we provide further evidence that distributive preferences affect how much people work.[16]

Distributive preferences and change in effort. We ask whether distributive preferences in year 1 can also predict how people change their production from year 1 to year 2. The estimated model modifies model (1.1) as follows:

$$\triangle e_i = \beta_0 + \beta_1 d_{i1} + \beta_2 X_{i1} + \varepsilon_i, \tag{1.3}$$

where $\triangle e_i = e_{i2} - e_{i1}$. Again, the main interest lies in parameter β_1. We only report the complete regressions of this model for the five measures of distributive preferences in Table 1.7.

We observe that not only absolute effort levels but also the changes in productivity between year 1 and 2 are related to subjects' distributive choices and the association mirrors that of previous sections. Since most subjects increase their effort from year 1 to year 2, Table 1.7 reveals that egalitarian subjects increase effort less than selfish individuals, while the remaining subjects lie in between. The change in production is again positively related to the indicators of selfishness and inequality and, in the latter case, the effect is driven by self-centered inequality. Even though these results do not establish causality, Table 1.7 provides further evidence that distributive preferences affect effort.[17]

Subjects consistent in their distributive preferences. One important question for the interpretation of our results is whether the documented link from preferences to effort may not actually be driven by the change in preferences from one year to the other. To address this issue, we reestimate the models from Tables 1.3 - 1.6 with

[16] All the results in the main text control for economic status in year 1. Appendix F additionally shows that our results are robust to controlling for changes in status from one year to the other.

[17] Appendix D lists all the control variables and their estimates.

Dependent variable: change in effort between 2013 and 2014					
	(1)	(2)	(3)	(4)	(5)
Proportional	-0.168				
	(0.323)				
Egalitarian	-0.530**				
	(0.266)				
Other	-0.317				
	(0.249)				
Selfishness		0.656**			
		(0.298)			
Inequality			0.569**		
			(0.231)		
SC Inequality				0.566**	
				(0.229)	
NC Inequality					-0.453
					(1.726)
Treatment	-0.127	-0.0865	-0.110	-0.0987	-0.0584
	(0.145)	(0.140)	(0.141)	(0.140)	(0.145)
Female	0.189	0.206	0.211*	0.211*	0.172
	(0.127)	(0.127)	(0.127)	(0.127)	(0.127)
Constant	0.529	-0.0465	0.112	0.122	0.134
	(0.341)	(0.238)	(0.224)	(0.224)	(0.229)
Other Controls	YES	YES	YES	YES	YES
Observations	275	275	275	275	275
R-squared	0.129	0.128	0.132	0.132	0.112

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.7: Change in effort from year 1 to year 2.

individuals who exhibit consistent distributive choices between years 1 and 2, using two alternative measures of consistency. In the main text, we restrict our analysis to people who were classified in both years in the same preference type, excluding subjects categorized as *Others* in Table 1.3 because their behavior is largely unstable. Table 1.8 reports the estimates. We observe that, even though only 51 individuals are consistent according to this criterion, none of our results change: Selfish people produce more than egalitarians and production scales up with selfishness, inequality, and self-centered inequality, while non-self-centered inequality cannot explain individual production levels. Appendix E reports that the conclusions do not change if we restrict our attention to the 65 subjects who hold the same amount for themselves in both years.

1.5 Conclusions

This paper studies the link between distributive preferences and effort provision. Previous literature shows that individual productivity shapes subsequent distributive choices. We contribute to this evidence by analyzing the reverse link, reporting that distributive preferences predict subjects' future productivity. As a result, effort shapes preferences and preferences determine effort. We propose a model that shows that such a two-way link can be in line with utility maximization: an individual is better off believing in egalitarianism if–for some reason–she is not willing to exert effort and has no incentives to exert costly effort if she prefers equally distributed allocations that do not reflect differences in effort provision. The cognitive dissonance between payoff maximization and a principle of justice makes agents choose effort and preferences jointly, leading to *self-serving bias* as a result of a decision process. In addition, we show that the reported link is mostly driven by self-centered inequality, while the proposed inequality among others does not predict effort provision in our data.

Recall that this paper only analyzes contexts where efficiency plays no role. Participants cannot affect the total amount to be distributed by working harder and this might affect the effort of the different types analyzed here. The natural question that cannot be answered with the available data then is whether the reported associations still hold in a framework, in which greater efforts increase the aggregate money in each group.

The above results contribute to the understanding of two other pieces of evidence. First, Bartiling et al. (2009) show that egalitarians are less-likely to self-select to competitive environments. Our paper provides a mechanism that can explain their findings: egalitarians prefer non-competitive environments because they are not will-

Dependent variable: effort provision					
	(1)	(2)	(3)	(4)	(5)
Proportional	-0.562				
	(0.763)				
Egalitarian	-1.543***				
	(0.518)				
Selfishness		1.874**			
		(0.704)			
Inequality			1.582***		
			(0.516)		
SC Inequality				1.544***	
				(0.521)	
NCInequality					8.240
					(7.533)
Treatment	-0.579	-0.397	-0.490	-0.459	-0.238
	(0.441)	(0.438)	(0.432)	(0.432)	(0.469)
Female	0.436	0.458	0.468	0.473	0.342
	(0.347)	(0.353)	(0.345)	(0.347)	(0.379)
Constant	1.393**	-0.558	-0.152	-0.140	0.249
	(0.673)	(0.644)	(0.569)	(0.572)	(0.608)
Observations	51	51	51	51	51
R-squared	0.479	0.438	0.466	0.459	0.354

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.8: Subjects consistent in their distributive choices over the two years.

ing to exert effort. Whether this is indeed the case is an interesting idea for future research.

Second, Leibbrandt (2012) reports that sellers who are more prosocial in a laboratory experiment are also more successful in a naturally occurring market. This seems to be at odds with our findings. However, our setting differs from real-life markets in that greater efforts do *not* generate larger amounts of money in our experiment. Moreover, the success of business people in a market typically entails many factors which are confused one with another, preventing the effect of pure moral motives on effort. The subject of our study should thus be viewed as the interplay between distributive choices and the intrinsic incentives to provide effort, free of efficiency, reputation, and other related concerns.

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Appendix A: Benchmark models

Appendix A supplements the benchmark estimations in Section 3.3. First, in contrast to the main text, it lists all the control variables and the corresponding estimates for interested readers. Second, it complements the main results by including models without controls to show that the results are robust to the inclusion or exclusion of the control variables.

We control for demeaned age and gender (*Age* and *Female*). Moreover, Barr et al. (2015, 2016) show that employment status or being a student can have an influence on distributive preferences. Therefore, we control for employment status (*Student* and *Employed* dummies, making unemployed individuals the benchmark category). Additionally, Gill et al. (2015) show that past ranking can have motivational effects, inducing subjects to exert more or less effort. We thus introduce rank dummies from year 1 in some models (with the benchmark being ranked first). Similarly, we also include the payoff in year 1 ($Payoff_{t=1}$).[18] Naturally, our models also control for differences across treatments ($Treatment = 1$ for Earned treatment and $Information = 1$ for people who were informed about the whole experimental procedure from the very beginning of the experiment) and experimental location (*Location*). Tables 1.A.1 - 1.A.5 report the results. The tables show that all results are largely robust to the model specification.

[18] Our result is unaffected if we include a dummy for being dictator or both the payoff from year 1 and the dictator dummy. This is not surprising, because people were only informed about their payoffs but not whether their distribution was selected for payment. Since both variables are correlated, we only report regressions with the payoff variable in this paper.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Proportional	-0.343	-0.367	-0.439	-0.487	-0.222	-0.228	-0.282	-0.336
	(0.319)	(0.320)	(0.316)	(0.318)	(0.292)	(0.294)	(0.293)	(0.300)
Egalitarian	-0.859***	-0.818***	-0.840***	-0.825***	-0.599**	-0.592**	-0.617**	-0.686***
	(0.262)	(0.264)	(0.261)	(0.262)	(0.242)	(0.244)	(0.243)	(0.248)
Other	-0.479*	-0.443*	-0.443*	-0.494**	-0.379	-0.372	-0.375	-0.410*
	(0.251)	(0.253)	(0.250)	(0.245)	(0.230)	(0.232)	(0.231)	(0.231)
Treatment		0.150	0.195	0.0590		0.0317	0.0667	-0.0289
		(0.135)	(0.133)	(0.142)		(0.124)	(0.125)	(0.135)
Female			0.388***	0.351***			0.254**	0.274**
			(0.127)	(0.125)			(0.119)	(0.119)
$\text{Payoff}_{t=1}$				-0.0172*				-0.0165*
				(0.00893)				(0.00839)
Age				-0.0202				-0.0194
				(0.0180)				(0.0169)
Ranked 2^{nd}				-0.0468				0.158
				(0.169)				(0.163)
Ranked 3^{rd}				-0.564***				-0.209
				(0.179)				(0.179)
Ranked 4^{th}				-0.461***				0.156
				(0.176)				(0.196)
Student				0.0251				-0.0440
				(0.180)				(0.170)
Employed				0.0600				-0.00610
				(0.151)				(0.142)
Location				-0.0509				-0.0807
				(0.146)				(0.137)
Information				0.432**				0.367*
				(0.211)				(0.198)
$\text{Production}_{t=1}$					0.488***	0.486***	0.462***	0.472***
					(0.0665)	(0.0672)	(0.0677)	(0.0794)
Constant	0.701***	0.581**	0.351	0.865**	0.631***	0.606**	0.455*	0.706**
	(0.236)	(0.259)	(0.266)	(0.336)	(0.216)	(0.238)	(0.246)	(0.317)
Observations	275	275	275	275	275	275	275	275
R-squared	0.050	0.055	0.086	0.178	0.208	0.208	0.222	0.277

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 1.A.1: Effort provision and pure preference types.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Selfishness	0.843***	0.807***	0.863***	0.976***	0.539**	0.529**	0.579**	0.825***
	(0.280)	(0.281)	(0.277)	(0.293)	(0.259)	(0.260)	(0.259)	(0.277)
Treatment		0.189	0.225*	0.0956		0.0698	0.0994	0.00969
		(0.131)	(0.129)	(0.138)		(0.121)	(0.121)	(0.131)
Female			0.396***	0.367***			0.263**	0.291**
			(0.127)	(0.125)			(0.119)	(0.118)
$\text{Payoff}_{t=1}$				-0.0226**				-0.0214**
				(0.00931)				(0.00875)
Age				-0.0237				-0.0225
				(0.0179)				(0.0168)
Ranked 2^{nd}				-0.0428				0.156
				(0.168)				(0.161)
Ranked 3^{rd}				-0.594***				-0.240
				(0.176)				(0.176)
Ranked 4^{th}				-0.468***				0.151
				(0.174)				(0.194)
Student				0.0185				-0.0515
				(0.179)				(0.169)
Employed				0.0637				0.00146
				(0.149)				(0.141)
Location				-0.0653				-0.0868
				(0.144)				(0.135)
Information				0.387*				0.330*
				(0.208)				(0.196)
$\text{Production}_{t=1}$					0.498***	0.493***	0.469***	0.472***
					(0.0663)	(0.0669)	(0.0674)	(0.0790)
Constant	-0.180	-0.280*	-0.544***	-0.0255	0.0153	-0.0236	-0.211	-0.0354
	(0.127)	(0.145)	(0.166)	(0.234)	(0.119)	(0.137)	(0.160)	(0.220)
Observations	275	275	275	275	275	275	275	275
R-squared	0.032	0.039	0.073	0.177	0.198	0.199	0.214	0.276

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.A.2: Effect of selfishness on effort.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Inequality	0.691***	0.649***	0.698***	0.809***	0.481**	0.470**	0.511**	0.696***
	(0.218)	(0.221)	(0.218)	(0.227)	(0.201)	(0.203)	(0.202)	(0.215)
Treatment		0.164	0.198	0.0634		0.0487	0.0770	-0.0183
		(0.132)	(0.130)	(0.139)		(0.121)	(0.121)	(0.131)
Female			0.400***	0.372***			0.266**	0.296**
			(0.127)	(0.125)			(0.119)	(0.118)
Payoff$_{t=1}$				-0.0225**				-0.0215**
				(0.00919)				(0.00864)
Age				-0.0236				-0.0225
				(0.0178)				(0.0167)
Ranked 2^{nd}				-0.0439				0.154
				(0.167)				(0.161)
Ranked 3^{rd}				-0.596***				-0.242
				(0.176)				(0.175)
Ranked 4^{th}				-0.494***				0.128
				(0.173)				(0.193)
Student				0.00888				-0.0605
				(0.179)				(0.168)
Employed				0.0506				-0.0102
				(0.149)				(0.141)
Location				-0.0762				-0.0959
				(0.143)				(0.134)
Information				0.388*				0.331*
				(0.208)				(0.196)
Production$_{t=1}$					0.498***	0.494***	0.470***	0.471***
					(0.0659)	(0.0666)	(0.0669)	(0.0787)
Constant	-0.00524	-0.0945	-0.349***	0.211	0.118	0.0905	-0.0876	0.164
	(0.0809)	(0.108)	(0.134)	(0.221)	(0.0754)	(0.102)	(0.129)	(0.208)
Observations	275	275	275	275	275	275	275	275
R-squared	0.035	0.041	0.074	0.181	0.203	0.203	0.218	0.280

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.A.3: Effort of inequality on effort.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SC Inequality	0.664***	0.629***	0.676***	0.797***	0.470**	0.460**	0.500**	0.688***
	(0.217)	(0.218)	(0.215)	(0.225)	(0.199)	(0.200)	(0.199)	(0.212)
Treatment		0.177	0.213	0.0800		0.0581	0.0872	-0.00422
		(0.131)	(0.130)	(0.138)		(0.121)	(0.121)	(0.131)
Female			0.399***	0.371***			0.265**	0.296**
			(0.127)	(0.125)			(0.119)	(0.118)
$\text{Payoff}_{t=1}$				-0.0228**				-0.0218**
				(0.00924)				(0.00868)
Age				-0.0239				-0.0227
				(0.0178)				(0.0167)
Ranked 2^{nd}				-0.0448				0.154
				(0.167)				(0.161)
Ranked 3^{rd}				-0.599***				-0.244
				(0.176)				(0.175)
Ranked 4^{th}				-0.498***				0.125
				(0.173)				(0.193)
Student				0.00892				-0.0608
				(0.179)				(0.168)
Employed				0.0544				-0.00713
				(0.149)				(0.140)
Location				-0.0738				-0.0939
				(0.143)				(0.134)
Information				0.385*				0.329*
				(0.208)				(0.196)
$\text{Production}_{t=1}$					0.500***	0.495***	0.471***	0.471***
					(0.0658)	(0.0665)	(0.0669)	(0.0787)
Constant	0.0153	-0.0846	-0.338**	0.225	0.131*	0.0972	-0.0799	0.176
	(0.0779)	(0.107)	(0.133)	(0.221)	(0.0725)	(0.101)	(0.128)	(0.208)
Observations	275	275	275	275	275	275	275	275
R-squared	0.033	0.040	0.073	0.181	0.202	0.203	0.217	0.280

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.A.4: Self-centered inequality and effort.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
NC Inequality	0.883	0.166	0.145	-0.249	0.0556	-0.231	-0.229	-0.350
	(1.739)	(1.791)	(1.768)	(1.720)	(1.577)	(1.627)	(1.618)	(1.610)
Treatment		0.219	0.256*	0.129		0.0909	0.119	0.0367
		(0.137)	(0.136)	(0.145)		(0.125)	(0.125)	(0.137)
Female			0.371***	0.317**			0.239**	0.246**
			(0.129)	(0.127)			(0.120)	(0.119)
$\text{Payoff}_{t=1}$				-0.0108				-0.0115
				(0.00884)				(0.00828)
Age				-0.0239				-0.0227
				(0.0183)				(0.0171)
Ranked 2^{nd}				-0.0558				0.154
				(0.171)				(0.164)
Ranked 3^{rd}				-0.618***				-0.245
				(0.180)				(0.179)
Ranked 4^{th}				-0.526***				0.130
				(0.177)				(0.197)
Student				0.0811				-0.00238
				(0.182)				(0.171)
Employed				0.0966				0.0267
				(0.152)				(0.143)
Location				-0.0792				-0.0991
				(0.147)				(0.137)
Information				0.381*				0.322
				(0.213)				(0.199)
$\text{Production}_{t=1}$					0.519***	0.513***	0.493***	0.493***
					(0.0661)	(0.0668)	(0.0672)	(0.0800)
Constant	0.133*	0.0157	-0.212	0.236	0.229***	0.180*	0.0266	0.186
	(0.0751)	(0.104)	(0.130)	(0.228)	(0.0690)	(0.0972)	(0.123)	(0.214)
Observations	275	275	275	275	275	275	275	275
R-squared	0.001	0.010	0.039	0.142	0.186	0.187	0.199	0.251

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.A.5: Non-self-centered inequality and effort.

Appendix B: Quasi-Selfishness and Quasi-Egalitarianism

Table 1.B.1 contains variations of the estimations in Table 1.4 in which we relax the definitions of egalitarianism and selfishness. In particular, we classify an individual as quasi-selfish if she keeps for herself strictly more than €40 (out of the total €44 to be distributed). Analogously, an individuals is categorized as *quasi-egalitarian* if she keeps for herself between €11 and €14 (that is, such an individual holds at most €3 more than a strict egalitarian) and distribute the remaining money equally among the other members of the group. Table 1.B.1 shows that the results are unaffected by this modification.

	(1)	(2)	(3)	(4)
Proportional	-0.413	-0.575*	-0.274	-0.445
	(0.294)	(0.300)	(0.272)	(0.283)
Quasi-Egalitarian	-0.740***	-0.845***	-0.541**	-0.711***
	(0.227)	(0.234)	(0.211)	(0.221)
Others	-0.443**	-0.618***	-0.418**	-0.589***
	(0.224)	(0.225)	(0.207)	(0.212)
Treatment	0.166	0.0278	0.0493	-0.0516
	(0.135)	(0.143)	(0.125)	(0.135)
Female	0.390***	0.351***	0.248**	0.269**
	(0.128)	(0.125)	(0.120)	(0.119)
$\text{Payoff}_{t=1}$		-0.0213**		-0.0201**
		(0.00917)		(0.00863)
Age		-0.0237		-0.0226
		(0.0179)		(0.0168)
Ranked 2^{nd}		-0.0627		0.147
		(0.169)		(0.162)
Ranked 3^{rd}		-0.605***		-0.246
		(0.177)		(0.177)
Ranked 4^{th}		-0.487***		0.127
		(0.174)		(0.194)
Student		0.0171		-0.0397
		(0.180)		(0.170)
Employed		0.0572		-0.00782
		(0.150)		(0.142)
Location		-0.0478		-0.0823
		(0.145)		(0.137)
Information		0.426**		0.359*
		(0.209)		(0.197)
$\text{Production}_{t=1}$			0.470***	0.472***
			(0.0680)	(0.0796)
Constant	0.351	1.039***	0.469**	0.893***
	(0.239)	(0.325)	(0.221)	(0.307)
Observations	275	275	275	275
R-squared	0.080	0.184	0.219	0.281

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.B.1: Regression results with quasi-selfish and quasi-egalitarian individuals (rather than purely selfish and purely egalitarian types).

Appendix C: Alternative Measures of Inequality

In Section 3.3, we report a positive relationship between inequality in the distribution proposed in year 1 and production in year 2, using a specific indicator of inequality. We have repeated the analysis, applying two different measures of inequality–the standard deviation and the Gini coefficient of the distribution–in Tables 1.C.1 and 1.C.2 to prove that the results presented in Table 1.5 hold no matter which measure of inequality is used.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SD	0.0357***	0.0337***	0.0363***	0.0426***	0.0250**	0.0245**	0.0266**	0.0366***
	(0.0115)	(0.0116)	(0.0114)	(0.0119)	(0.0105)	(0.0106)	(0.0106)	(0.0113)
Treatment		0.171	0.205	0.0711		0.0535	0.0823	-0.0115
		(0.132)	(0.130)	(0.138)		(0.121)	(0.121)	(0.131)
Female			0.399***	0.372***			0.266**	0.296**
			(0.127)	(0.125)			(0.119)	(0.118)
$\text{Payoff}_{t=1}$				-0.0227**				-0.0217**
				(0.00921)				(0.00866)
Age				-0.0238				-0.0226
				(0.0178)				(0.0167)
Ranked 2^{nd}				-0.0467				0.152
				(0.167)				(0.161)
Ranked 3^{rd}				-0.600***				-0.246
				(0.175)				(0.175)
Ranked 4^{th}				-0.496***				0.126
				(0.173)				(0.193)
Student				0.00934				-0.0601
				(0.179)				(0.168)
Employed				0.0522				-0.00872
				(0.149)				(0.140)
Location				-0.0749				-0.0949
				(0.143)				(0.134)
Information				0.388*				0.331*
				(0.208)				(0.196)
$\text{Production}_{t=1}$					0.499***	0.495***	0.471***	0.471***
					(0.0659)	(0.0665)	(0.0669)	(0.0787)
Constant	0.00452	-0.0901	-0.344**	0.219	0.124*	0.0936	-0.0841	0.171
	(0.0794)	(0.108)	(0.133)	(0.221)	(0.0740)	(0.101)	(0.128)	(0.208)
Observations	275	275	275	275	275	275	275	275
R-squared	0.034	0.040	0.074	0.182	0.202	0.203	0.217	0.280

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.C.1: Standard deviation of the proposed distribution and effort.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gini Index	0.721***	0.676***	0.719***	0.824***	0.507**	0.495**	0.531**	0.704***
	(0.238)	(0.240)	(0.237)	(0.245)	(0.218)	(0.221)	(0.220)	(0.231)
Treatment		0.168	0.202	0.0696		0.0509	0.0793	-0.0129
		(0.132)	(0.130)	(0.139)		(0.121)	(0.121)	(0.131)
Female			0.393***	0.360***			0.261**	0.286**
			(0.127)	(0.125)			(0.119)	(0.118)
Payoff$_{t=1}$				-0.0214**				-0.0205**
				(0.00916)				(0.00861)
Age				-0.0231				-0.0220
				(0.0179)				(0.0168)
Ranked 2^{nd}				-0.0552				0.145
				(0.168)				(0.161)
Ranked 3^{rd}				-0.607***				-0.250
				(0.176)				(0.176)
Ranked 4^{th}				-0.506***				0.120
				(0.173)				(0.194)
Student				0.0289				-0.0432
				(0.179)				(0.168)
Employed				0.0616				-0.000732
				(0.149)				(0.141)
Location				-0.0768				-0.0965
				(0.143)				(0.135)
Information				0.382*				0.326*
				(0.208)				(0.196)
Production$_{t=1}$					0.500***	0.496***	0.473***	0.472***
					(0.0659)	(0.0665)	(0.0669)	(0.0789)
Constant	0.0167	-0.0763	-0.324**	0.230	0.133*	0.104	-0.0689	0.181
	(0.0779)	(0.107)	(0.132)	(0.221)	(0.0725)	(0.100)	(0.127)	(0.208)
Observations	275	275	275	275	275	275	275	275
R-squared	0.033	0.038	0.071	0.177	0.202	0.202	0.216	0.277

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.C.2: Gini coefficient of the proposed distribution and effort.

Appendix D: Changes in Production

Table 1.D.1 provides a more detailed view of the results from Table 1.7 in the main text.

	(1)	(2)	(3)	(4)	(5)
Proportional	-0.168				
	(0.323)				
Egalitarian	-0.530**				
	(0.266)				
Other	-0.317				
	(0.249)				
Selfishness		0.656**			
		(0.298)			
Inequality			0.569**		
			(0.231)		
SC Inequality				0.566**	
				(0.229)	
NC Inequality					-0.453
					(1.726)
Treatment	-0.127	-0.0865	-0.110	-0.0987	-0.0584
	(0.145)	(0.140)	(0.141)	(0.140)	(0.145)
Female	0.189	0.206	0.211*	0.211*	0.172
	(0.127)	(0.127)	(0.127)	(0.127)	(0.127)
$\text{Payoff}_{t=1}$	-0.0157*	-0.0201**	-0.0204**	-0.0207**	-0.0123
	(0.00906)	(0.00945)	(0.00934)	(0.00938)	(0.00887)
Age	-0.0184	-0.0212	-0.0212	-0.0213	-0.0214
	(0.0182)	(0.0181)	(0.0181)	(0.0181)	(0.0183)
Ranked 2^{nd}	0.386**	0.378**	0.377**	0.377**	0.369**
	(0.172)	(0.170)	(0.170)	(0.170)	(0.172)
Ranked 3^{rd}	0.189	0.157	0.156	0.154	0.139
	(0.182)	(0.179)	(0.178)	(0.178)	(0.180)
Ranked 4^{th}	0.848***	0.844***	0.828***	0.825***	0.804***
	(0.179)	(0.177)	(0.176)	(0.176)	(0.178)
Student	-0.121	-0.130	-0.139	-0.139	-0.0881
	(0.183)	(0.182)	(0.182)	(0.182)	(0.183)
Employed	-0.0801	-0.0683	-0.0785	-0.0761	-0.0451
	(0.153)	(0.152)	(0.152)	(0.151)	(0.153)
Location	-0.114	-0.111	-0.118	-0.116	-0.119
	(0.148)	(0.146)	(0.145)	(0.145)	(0.147)
Information	0.295	0.267	0.268	0.266	0.262
	(0.214)	(0.212)	(0.211)	(0.211)	(0.214)
Constant	0.529	-0.0465	0.112	0.122	0.134
	(0.341)	(0.238)	(0.224)	(0.224)	(0.229)
Observations	275	275	275	275	275
R-squared	0.129	0.128	0.132	0.132	0.112

Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table 1.D.1: Changes in effort from year 1 to year 2.

Appendix E: Consistency

In this section, we repeat the regressions from Tables 1.4 - 1.6, using a subsample of subjects. We focus on individuals who are consistent in their distributive choices between the two years. Tables 1.E.1 - 1.E.3 report the results for the 51 individuals classified in the same type in both years; Tables 1.E.4 - 1.E.6 show the estimates with the 65 individuals who proposed the same amount for themselves in both years. Even though this substantially decreases the number of observations, the relevant estimates are still highly significant, independently of the definition of consistency. These results provide strong evidence that our results are not driven by changes in subjects' preferences over the two years.

	(1)	(2)	(3)	(4)
Proportional	-0.299	-0.208	-0.334	-0.562
	(0.703)	(0.714)	(0.709)	(0.763)
Egalitarian	-1.017**	-1.065**	-1.069**	-1.543***
	(0.401)	(0.407)	(0.401)	(0.518)
Treatment		-0.295	-0.252	-0.579
		(0.368)	(0.364)	(0.441)
Female			0.534	0.436
			(0.347)	(0.347)
$\text{Payoff}_{t=1}$				-0.0315
				(0.0197)
Age				-0.0578
				(0.0524)
Ranked 2^{nd}				0.237
				(0.491)
Ranked 3^{rd}				-0.548
				(0.527)
Ranked 4^{th}				-0.507
				(0.464)
Student				-0.501
				(0.460)
Employed				-0.122
				(0.456)
Location				0.441
				(0.412)
Information				1.353**
				(0.608)
Constant	0.615*	0.819*	0.502	1.393**
	(0.341)	(0.427)	(0.468)	(0.673)
Observations	51	51	51	51
R-squared	0.126	0.138	0.180	0.479

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.E.1: Consistency 1: Preference types.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Selfishness	1.266**	1.292**	1.307**	1.874**				
	(0.524)	(0.531)	(0.522)	(0.704)				
Inequality					1.010**	1.044**	1.053**	1.582***
					(0.399)	(0.405)	(0.398)	(0.516)
Treatment		-0.176	-0.147	-0.397		-0.215	-0.187	-0.490
		(0.357)	(0.351)	(0.438)		(0.357)	(0.351)	(0.432)
Female			0.567	0.458			0.565	0.468
			(0.345)	(0.353)			(0.342)	(0.345)
$\text{Payoff}_{t=1}$				-0.0324				-0.0346*
				(0.0202)				(0.0195)
Age				-0.0507				-0.0491
				(0.0533)				(0.0516)
Ranked 2^{nd}				0.132				0.167
				(0.498)				(0.486)
Ranked 3^{rd}				-0.650				-0.590
				(0.537)				(0.525)
Ranked 4^{th}				-0.530				-0.513
				(0.482)				(0.464)
Student				-0.489				-0.480
				(0.470)				(0.459)
Employed				-0.137				-0.107
				(0.468)				(0.455)
Location				0.471				0.480
				(0.422)				(0.410)
Information				1.248*				1.352**
				(0.619)				(0.607)
Constant	-0.645**	-0.550	-0.896**	-0.558	-0.363*	-0.241	-0.582	-0.152
	(0.288)	(0.349)	(0.403)	(0.644)	(0.203)	(0.287)	(0.350)	(0.569)
Observations	51	51	51	51	51	51	51	51
R-squared	0.106	0.111	0.159	0.438	0.116	0.122	0.171	0.466

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.E.2: Consistency 1: Selfishness and Inequality.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SC Inequality	0.989**	1.017**	1.031**	1.544***				
	(0.399)	(0.405)	(0.397)	(0.521)				
NCInequality					4.804	5.520	4.174	8.240
					(7.421)	(7.700)	(7.674)	(7.533)
Treatment		-0.200	-0.172	-0.459		-0.153	-0.110	-0.238
		(0.357)	(0.351)	(0.432)		(0.385)	(0.382)	(0.469)
Female			0.571	0.473			0.528	0.342
			(0.343)	(0.347)			(0.369)	(0.379)
$\text{Payoff}_{t=1}$				-0.0344*				-0.00113
				(0.0197)				(0.0186)
Age				-0.0485				-0.107*
				(0.0521)				(0.0546)
Ranked 2^{nd}				0.148				0.146
				(0.489)				(0.539)
Ranked 3^{rd}				-0.611				-0.903
				(0.527)				(0.564)
Ranked 4^{th}				-0.531				-1.032**
				(0.466)				(0.472)
Student				-0.475				-0.529
				(0.462)				(0.505)
Employed				-0.101				-0.0447
				(0.458)				(0.500)
Location				0.480				0.157
				(0.413)				(0.440)
Information				1.333**				0.708
				(0.611)				(0.624)
Constant	-0.350*	-0.236	-0.581	-0.140	-0.120	-0.0326	-0.350	0.249
	(0.202)	(0.288)	(0.351)	(0.572)	(0.189)	(0.292)	(0.364)	(0.608)
Observations	51	51	51	51	51	51	51	51
R-squared	0.112	0.117	0.166	0.459	0.008	0.012	0.053	0.354

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.E.3: Consistency 1: Self-centered and non-self-centered inequality.

	(1)	(2)	(3)	(4)
Proportional	0.451	0.463	0.278	0.0344
	(0.888)	(0.914)	(0.916)	(0.908)
Egalitarian	-0.786**	-0.784**	-0.747**	-0.882**
	(0.307)	(0.312)	(0.310)	(0.332)
Treatment		-0.0203	0.0347	-0.0695
		(0.311)	(0.311)	(0.351)
Female			0.444	0.414
			(0.317)	(0.301)
$\text{Payoff}_{t=1}$				-0.0305**
				(0.0150)
Age				-0.0952**
				(0.0445)
Ranked 2^{nd}				0.127
				(0.427)
Ranked 3^{rd}				-0.412
				(0.415)
Ranked 4^{th}				-0.839**
				(0.397)
Student				-0.388
				(0.406)
Employed				0.168
				(0.379)
Location				-0.0388
				(0.349)
Information				0.668
				(0.525)
Constant	0.384*	0.393	0.0791	0.903
	(0.226)	(0.261)	(0.343)	(0.559)
Observations	65	65	65	65
R-squared	0.109	0.109	0.137	0.392

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.E.4: Consistency 2: Preference types.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Selfishness	0.991*	0.998*	1.010**	1.666***				
	(0.503)	(0.507)	(0.499)	(0.620)				
Inequality					0.785**	0.792**	0.796**	1.312***
					(0.378)	(0.382)	(0.375)	(0.462)
Treatment		-0.0837	-0.0261	-0.221		-0.0916	-0.0343	-0.243
		(0.310)	(0.307)	(0.348)		(0.309)	(0.306)	(0.346)
Female			0.557*	0.573*			0.552*	0.567*
			(0.315)	(0.297)			(0.314)	(0.294)
$\text{Payoff}_{t=1}$				-0.0440**				-0.0450**
				(0.0170)				(0.0168)
Age				-0.0801*				-0.0814*
				(0.0436)				(0.0431)
Ranked 2^{nd}				0.206				0.209
				(0.426)				(0.423)
Ranked 3^{rd}				-0.341				-0.330
				(0.415)				(0.412)
Ranked 4^{th}				-0.761*				-0.735*
				(0.402)				(0.400)
Student				-0.219				-0.218
				(0.405)				(0.402)
Employed				0.235				0.242
				(0.376)				(0.373)
Location				0.193				0.195
				(0.355)				(0.352)
Information				0.913				0.944*
				(0.546)				(0.543)
Constant	-0.465*	-0.427	-0.804**	-0.471	-0.234	-0.191	-0.560*	-0.0737
	(0.276)	(0.312)	(0.373)	(0.578)	(0.187)	(0.238)	(0.314)	(0.517)
Observations	65	65	65	65	65	65	65	65
R-squared	0.058	0.059	0.105	0.385	0.064	0.065	0.111	0.394

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.E.5: Consistency 2: Selfishness and inequality.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SC Inequality	0.747*	0.753*	0.760**	1.258***				
	(0.378)	(0.381)	(0.375)	(0.466)				
NC Inequality					9.363	9.640	8.649	10.26
					(6.563)	(6.659)	(6.610)	(6.404)
Treatment		-0.0853	-0.0278	-0.227		-0.108	-0.0493	-0.139
		(0.310)	(0.307)	(0.348)		(0.316)	(0.315)	(0.360)
Female			0.555*	0.571*			0.508	0.434
			(0.315)	(0.297)			(0.322)	(0.310)
$\text{Payoff}_{t=1}$				-0.0441**				-0.0151
				(0.0170)				(0.0145)
Age				-0.0804*				-0.127***
				(0.0435)				(0.0454)
Ranked 2^{nd}				0.203				0.142
				(0.425)				(0.442)
Ranked 3^{rd}				-0.341				-0.472
				(0.415)				(0.428)
Ranked 4^{th}				-0.759*				-0.991**
				(0.402)				(0.404)
Student				-0.218				-0.360
				(0.405)				(0.421)
Employed				0.238				0.325
				(0.375)				(0.392)
Location				0.194				-0.0568
				(0.355)				(0.364)
Information				0.922*				0.423
				(0.547)				(0.536)
Constant	-0.219	-0.179	-0.551*	-0.0560	-0.0696	-0.0182	-0.354	0.253
	(0.186)	(0.238)	(0.315)	(0.520)	(0.161)	(0.221)	(0.305)	(0.527)
Observations	65	65	65	65	65	65	65	65
R-squared	0.058	0.060	0.105	0.386	0.031	0.033	0.071	0.333

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.E.6: Consistency 2: Self-centered and non-self-centered inequality.

Appendix F: Changes in Employment Status

Observe that our main estimation results control for employment/student status of subjects in $t = 1$. However, Barr et al. (2016) document that changes in employment status between the two years affect the distributive preferences. Therefore, to make sure that our results are not driven by changes in employment/student status, we re-estimate the main models controlling for these changes (rather than the status in $t = 1$). More precisely, instead of including the *Student* and *Employed* dummies, we include the variables *Become Employed*, *Become Unemployed* and *Become Student* which take values of 1 if the subject was Employed, Unemployed, or Student, respectively, in $t = 2$ but belonged to a different category in $t = 1$. Table 1.F.1 reestimates the regressions from Tables 1.4 - 1.6 using these alternative controls for status changes, while Tables 1.F.2 and 1.F.3 reestimate the regressions from Tables 1.7 and 1.8, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Proportional	-0.469	-0.314								
	(0.317)	(0.300)								
Egalitarian	-0.796***	-0.649***								
	(0.263)	(0.248)								
Other	-0.448*	-0.366								
	(0.248)	(0.234)								
Selfishness			0.980***	0.810***						
			(0.292)	(0.277)						
Inequality					0.800***	0.672***				
					(0.226)	(0.214)				
SC Inequality							0.791***	0.665***		
							(0.224)	(0.212)		
NC Inequality									-0.342	-0.377
									(1.716)	(1.606)
Treatment	0.0636	-0.0336	0.0986	0.00354	0.0669	-0.0232	0.0837	-0.00923	0.132	0.0278
	(0.143)	(0.135)	(0.138)	(0.131)	(0.139)	(0.131)	(0.138)	(0.131)	(0.145)	(0.137)
Female	0.356***	0.282**	0.371***	0.298**	0.374***	0.301**	0.373***	0.301**	0.324**	0.256**
	(0.125)	(0.118)	(0.125)	(0.118)	(0.124)	(0.118)	(0.124)	(0.118)	(0.127)	(0.119)
$\text{Payoff}_{t=1}$	-0.0166*	-0.0168**	-0.0223**	-0.0218**	-0.0220**	-0.0217**	-0.0224**	-0.0221**	-0.0101	-0.0119
	(0.00897)	(0.00843)	(0.00938)	(0.00882)	(0.00925)	(0.00870)	(0.00929)	(0.00874)	(0.00887)	(0.00831)
Age	-0.0182	-0.0154	-0.0213	-0.0178	-0.0211	-0.0176	-0.0212	-0.0177	-0.0245*	-0.0203
	(0.0143)	(0.0135)	(0.0141)	(0.0133)	(0.0141)	(0.0133)	(0.0141)	(0.0133)	(0.0144)	(0.0135)
Ranked 2^{nd}	-0.0535	0.145	-0.0530	0.139	-0.0518	0.140	-0.0528	0.140	-0.0653	0.139
	(0.170)	(0.163)	(0.168)	(0.161)	(0.167)	(0.161)	(0.167)	(0.161)	(0.171)	(0.164)
Ranked 3^{rd}	-0.567***	-0.206	-0.596***	-0.236	-0.598***	-0.239	-0.602***	-0.242	-0.626***	-0.243
	(0.179)	(0.179)	(0.176)	(0.176)	(0.175)	(0.176)	(0.175)	(0.176)	(0.179)	(0.179)
Ranked 4^{th}	-0.454**	0.151	-0.469***	0.137	-0.493***	0.116	-0.498***	0.112	-0.519***	0.127
	(0.177)	(0.196)	(0.175)	(0.194)	(0.174)	(0.193)	(0.174)	(0.193)	(0.178)	(0.197)
Become Employed	-0.0720	0.0116	-0.100	-0.0136	-0.0954	-0.0102	-0.0995	-0.0137	-0.0612	0.0222
	(0.147)	(0.139)	(0.146)	(0.138)	(0.146)	(0.138)	(0.146)	(0.138)	(0.149)	(0.140)
Become Unemployed	0.176	0.231	0.196	0.248	0.171	0.228	0.171	0.227	0.200	0.255
	(0.205)	(0.193)	(0.204)	(0.192)	(0.203)	(0.191)	(0.203)	(0.191)	(0.208)	(0.195)
Become Student	0.391	0.289	0.351	0.260	0.344	0.254	0.348	0.257	0.433	0.323
	(0.321)	(0.303)	(0.317)	(0.299)	(0.316)	(0.298)	(0.316)	(0.298)	(0.323)	(0.303)
Location	-0.0625	-0.0724	-0.0805	-0.0817	-0.0914	-0.0907	-0.0899	-0.0894	-0.0945	-0.0930
	(0.147)	(0.138)	(0.145)	(0.136)	(0.144)	(0.136)	(0.144)	(0.136)	(0.148)	(0.138)
Information	0.456**	0.405**	0.408*	0.367*	0.407*	0.366*	0.404*	0.363*	0.402*	0.360*
	(0.212)	(0.200)	(0.209)	(0.197)	(0.209)	(0.197)	(0.209)	(0.197)	(0.214)	(0.200)
$\text{Production}_{t=1}$		0.469***		0.468***		0.467***		0.467***		0.492***
		(0.0796)		(0.0791)		(0.0789)		(0.0789)		(0.0800)
Constant	0.831**	0.609*	-0.0108	-0.0773	0.222	0.115	0.238	0.128	0.265	0.147
	(0.338)	(0.320)	(0.228)	(0.215)	(0.213)	(0.201)	(0.213)	(0.201)	(0.220)	(0.207)
Observations	275	275	275	275	275	275	275	275	275	275
R-squared	0.186	0.283	0.186	0.282	0.190	0.286	0.189	0.286	0.151	0.259

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.F.1: Changes in Employment Status: Main Models

	(1)	(2)	(3)	(4)	(5)
Proportional	-0.139				
	(0.323)				
Egalitarian	-0.482*				
	(0.267)				
Other	-0.271				
	(0.252)				
Selfishness		0.616**			
		(0.297)			
Inequality			0.524**		
			(0.230)		
SC Inequality				0.521**	
				(0.228)	
NC Inequality					-0.413
					(1.723)
Treatment	-0.144	-0.105	-0.126	-0.115	-0.0796
	(0.145)	(0.141)	(0.141)	(0.141)	(0.146)
Female	0.199	0.216*	0.219*	0.219*	0.186
	(0.127)	(0.127)	(0.127)	(0.127)	(0.127)
$\text{Payoff}_{t=1}$	-0.0170*	-0.0213**	-0.0214**	-0.0217**	-0.0138
	(0.00912)	(0.00954)	(0.00942)	(0.00946)	(0.00891)
Age	-0.0121	-0.0139	-0.0137	-0.0137	-0.0160
	(0.0145)	(0.0144)	(0.0143)	(0.0143)	(0.0145)
Ranked 2^{nd}	0.370**	0.358**	0.359**	0.359**	0.350**
	(0.173)	(0.171)	(0.170)	(0.170)	(0.172)
Ranked 3^{rd}	0.203	0.172	0.171	0.169	0.153
	(0.182)	(0.179)	(0.178)	(0.178)	(0.180)
Ranked 4^{th}	0.836***	0.826***	0.811***	0.809***	0.794***
	(0.180)	(0.178)	(0.177)	(0.177)	(0.179)
Become Employed	0.106	0.0849	0.0870	0.0841	0.108
	(0.149)	(0.149)	(0.148)	(0.148)	(0.150)
Become Unemployed	0.293	0.308	0.292	0.292	0.311
	(0.209)	(0.207)	(0.207)	(0.207)	(0.209)
Become Student	0.173	0.157	0.151	0.153	0.210
	(0.327)	(0.323)	(0.322)	(0.322)	(0.324)
Location	-0.0837	-0.0831	-0.0899	-0.0888	-0.0915
	(0.150)	(0.147)	(0.147)	(0.147)	(0.149)
Information	0.348	0.320	0.319	0.317	0.315
	(0.216)	(0.213)	(0.213)	(0.213)	(0.215)
Constant	0.357	-0.153	-0.00780	0.00274	0.0244
	(0.344)	(0.232)	(0.217)	(0.217)	(0.221)
Observations	275	275	275	275	275
R-squared	0.135	0.134	0.137	0.137	0.120

Standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 1.F.2: Changes in Employment Status: Changes in Production

	(1)	(2)	(3)	(4)	(5)
Proportional	-0.449				
	(0.756)				
Egalitarian	-1.371**				
	(0.522)				
Selfishness		1.603**			
		(0.707)			
Inequality			1.408**		
			(0.520)		
SC Inequality				1.369**	
				(0.524)	
NC Inequality					7.792
					(7.284)
Treatment	-0.306	-0.106	-0.216	-0.184	0.0720
	(0.465)	(0.460)	(0.455)	(0.455)	(0.476)
Female	0.504	0.488	0.516	0.515	0.397
	(0.375)	(0.384)	(0.374)	(0.377)	(0.402)
$\text{Payoff}_{t=1}$	-0.0329*	-0.0323	-0.0353*	-0.0348*	-0.00521
	(0.0191)	(0.0197)	(0.0189)	(0.0191)	(0.0172)
Age	-0.0381	-0.0324	-0.0302	-0.0297	-0.0728
	(0.0423)	(0.0429)	(0.0415)	(0.0419)	(0.0433)
Ranked 2^{nd}	0.379	0.298	0.309	0.292	0.316
	(0.460)	(0.465)	(0.453)	(0.456)	(0.494)
Ranked 3^{rd}	-0.570	-0.702	-0.630	-0.656	-0.940*
	(0.516)	(0.523)	(0.511)	(0.513)	(0.534)
Ranked 4^{th}	-0.736	-0.767	-0.744	-0.761	-1.207**
	(0.460)	(0.480)	(0.460)	(0.462)	(0.457)
Become Employed	-0.413	-0.371	-0.406	-0.405	-0.426
	(0.379)	(0.389)	(0.379)	(0.381)	(0.408)
Become Unemployed	-0.220	-0.306	-0.268	-0.280	-0.288
	(0.570)	(0.581)	(0.567)	(0.570)	(0.613)
Become Student	1.143	1.233	1.137	1.154	1.655*
	(0.892)	(0.912)	(0.891)	(0.896)	(0.937)
Location	0.127	0.133	0.160	0.156	-0.211
	(0.443)	(0.455)	(0.441)	(0.444)	(0.456)
Information	0.988	0.841	0.974	0.948	0.317
	(0.645)	(0.655)	(0.644)	(0.647)	(0.638)
Constant	1.271*	-0.425	-0.0744	-0.0555	0.293
	(0.649)	(0.639)	(0.551)	(0.553)	(0.572)
Observations	51	51	51	51	51
R-squared	0.510	0.471	0.497	0.491	0.416

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.F.3: Changes in Employment Status: Consistency

Appendix G: Treatments

Recall that the main analysis in the main text pools the data from the Earned and Random treatments. In this section, we re-estimate the regressions from the main text separately for each treatment. Tables 1.G.1 and 1.G.2 report the results.

The results in both treatments always preserve the signs from the pooled regressions, but we observe a general tendency for the results to be stronger in the Random treatment than the Earned one. All estimates regarding distributive preferences are significant in the Random treatment. In the Earned treatment, this is also true in most cases, but there are exceptions. The dummy for egalitarians does not predict effort provision systematically at 10% or less; the results are also weaker for selfishness. Nevertheless, the results are qualitatively unaffected in the cases of all the other variables.

We can only speculate about the reasons why the results are quantitatively weaker in the Earned treatment, but we attribute it to a *crowding out* of intrinsic incentives by incentives to rank higher in the distributive task in the Earned treatment. According to e.g. Deci (1971), providing explicit incentives which are external to a task (money or non-pecuniary prizes) can diminish the extent to which internal rewards (such as interest for the task, the will to learn or to fulfill a duty) provide motivation for the task (see e.g. Deci et al. (1999) for a meta-analysis and Benabou and Tirole (2003) for a formalization of such an interplay). In our case, there are no sources of extrinsic motivation in the *Random Treatment* and people must exert effort for moral, intrinsic reasons only. By contrast, greater production leads to higher rank and more arguments to justify non-egalitarian behavior in the Earned treatment. Such a source of extrinsic motivation may thus make moral motives less prominent, leading cognitive dissonance to take on a smaller role in this treatment.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Proportional	-0.143	-0.134								
	(0.328)	(0.323)								
Egalitarian	-0.451	-0.461								
	(0.290)	(0.285)								
Other	-0.325	-0.336								
	(0.265)	(0.261)								
Selfishness			0.531	0.569*						
			(0.327)	(0.322)						
Inequality					0.534**	0.557**				
					(0.255)	(0.251)				
SC Inequality							0.518**	0.541**		
							(0.252)	(0.249)		
NC Inequality									0.312	0.278
									(1.769)	(1.745)
Female	0.124	0.0884	0.156	0.124	0.166	0.134	0.167	0.134	0.116	0.0824
	(0.154)	(0.152)	(0.150)	(0.149)	(0.150)	(0.148)	(0.150)	(0.148)	(0.150)	(0.148)
$\text{Payoff}_{t=1}$	-0.0144	-0.0149	-0.0177*	-0.0186*	-0.0188*	-0.0197**	-0.0190**	-0.0199**	-0.0110	-0.0115
	(0.00943)	(0.00928)	(0.00972)	(0.00958)	(0.00954)	(0.00940)	(0.00962)	(0.00947)	(0.00908)	(0.00895)
Age	-0.00384	-0.00674	-0.00822	-0.0115	-0.00918	-0.0124	-0.00939	-0.0127	-0.00381	-0.00675
	(0.0217)	(0.0214)	(0.0217)	(0.0214)	(0.0216)	(0.0213)	(0.0216)	(0.0213)	(0.0218)	(0.0215)
Ranked 2^{nd}	-0.0616	0.174	-0.0569	0.180	-0.0605	0.176	-0.0584	0.178	-0.0628	0.166
	(0.200)	(0.220)	(0.197)	(0.217)	(0.196)	(0.216)	(0.196)	(0.216)	(0.199)	(0.219)
Ranked 3^{rd}	-0.916***	-0.511*	-0.925***	-0.516*	-0.920***	-0.512*	-0.923***	-0.515*	-0.951***	-0.559**
	(0.217)	(0.273)	(0.212)	(0.268)	(0.211)	(0.267)	(0.211)	(0.267)	(0.214)	(0.270)
Ranked 4^{th}	-0.881***	-0.145	-0.860***	-0.114	-0.881***	-0.137	-0.886***	-0.142	-0.885***	-0.168
	(0.215)	(0.373)	(0.209)	(0.370)	(0.207)	(0.367)	(0.208)	(0.367)	(0.211)	(0.372)
Student	0.0540	0.0387	0.0139	-0.00767	-0.0113	-0.0319	-0.0120	-0.0328	0.0894	0.0735
	(0.215)	(0.212)	(0.215)	(0.212)	(0.214)	(0.211)	(0.214)	(0.211)	(0.212)	(0.209)
Employed	-0.109	-0.0908	-0.0894	-0.0700	-0.102	-0.0832	-0.0977	-0.0786	-0.0873	-0.0680
	(0.181)	(0.178)	(0.178)	(0.176)	(0.178)	(0.175)	(0.178)	(0.175)	(0.180)	(0.178)
Location	-0.0182	-0.0441	0.0166	-0.00484	0.00923	-0.0130	0.0111	-0.0110	-0.000247	-0.0219
	(0.208)	(0.205)	(0.201)	(0.198)	(0.200)	(0.197)	(0.200)	(0.197)	(0.203)	(0.200)
Information	0.463**	0.404*	0.466**	0.409*	0.465**	0.408*	0.463**	0.406*	0.463**	0.407*
	(0.232)	(0.230)	(0.226)	(0.224)	(0.225)	(0.223)	(0.225)	(0.223)	(0.228)	(0.226)
product		0.374**		0.378**		0.377**		0.377**		0.364**
		(0.156)		(0.155)		(0.154)		(0.154)		(0.157)
Constant	0.975**	0.729*	0.458	0.188	0.565**	0.304	0.586**	0.326	0.609**	0.360
	(0.384)	(0.392)	(0.280)	(0.297)	(0.261)	(0.279)	(0.261)	(0.278)	(0.272)	(0.288)
Observations	166	166	166	166	166	166	166	166	166	166
R-squared	0.254	0.281	0.251	0.279	0.259	0.287	0.258	0.286	0.238	0.264

Standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 1.G.1: Effort and distributive preferences: Earned treatment.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Proportional	-1.665	-1.160								
	(1.232)	(1.164)								
Egalitarian	-1.790***	-1.131*								
	(0.589)	(0.580)								
Other	-1.392**	-0.801								
	(0.572)	(0.559)								
Selfishness			1.753***	1.163*						
			(0.623)	(0.601)						
Inequality					1.269***	0.811*				
					(0.470)	(0.454)				
SC Inequality							1.286***	0.829*		
							(0.469)	(0.453)		
NC Inequality									-1.611	-1.506
									(4.478)	(4.097)
Female	0.672***	0.562***	0.670***	0.568***	0.684***	0.577***	0.674***	0.570***	0.650***	0.539**
	(0.216)	(0.205)	(0.213)	(0.201)	(0.214)	(0.202)	(0.214)	(0.201)	(0.225)	(0.207)
$\text{Payoff}_{t=1}$	-0.0244	-0.0208	-0.0419*	-0.0312	-0.0407*	-0.0299	-0.0410*	-0.0302	-0.0208	-0.0167
	(0.0213)	(0.0200)	(0.0224)	(0.0211)	(0.0224)	(0.0211)	(0.0224)	(0.0211)	(0.0219)	(0.0201)
Age	-0.0201	-0.0206	-0.0229	-0.0211	-0.0254	-0.0231	-0.0247	-0.0225	-0.0417	-0.0323
	(0.0315)	(0.0295)	(0.0313)	(0.0292)	(0.0313)	(0.0292)	(0.0313)	(0.0292)	(0.0318)	(0.0292)
Ranked 2^{nd}	0.104	0.0889	0.00946	0.0298	0.0272	0.0417	0.0122	0.0320	0.00810	0.0256
	(0.299)	(0.281)	(0.294)	(0.275)	(0.295)	(0.276)	(0.295)	(0.275)	(0.310)	(0.284)
Ranked 3^{rd}	0.0340	0.0832	-0.121	-0.0128	-0.114	-0.00662	-0.124	-0.0132	-0.0836	0.0203
	(0.304)	(0.286)	(0.298)	(0.280)	(0.299)	(0.281)	(0.299)	(0.281)	(0.311)	(0.286)
Ranked 4^{th}	0.138	0.249	0.0668	0.210	0.0507	0.199	0.0493	0.198	-0.00471	0.182
	(0.304)	(0.287)	(0.297)	(0.280)	(0.298)	(0.280)	(0.297)	(0.280)	(0.308)	(0.285)
Student	0.107	-0.0119	0.119	0.00768	0.102	-0.00573	0.110	-0.000389	0.0510	-0.0460
	(0.325)	(0.306)	(0.319)	(0.299)	(0.319)	(0.299)	(0.319)	(0.299)	(0.331)	(0.303)
Employed	0.374	0.204	0.332	0.178	0.343	0.187	0.335	0.182	0.464*	0.238
	(0.262)	(0.250)	(0.265)	(0.250)	(0.265)	(0.250)	(0.265)	(0.251)	(0.271)	(0.253)
Location	-0.0320	-0.0963	-0.0922	-0.140	-0.103	-0.147	-0.0994	-0.145	-0.111	-0.156
	(0.209)	(0.197)	(0.206)	(0.193)	(0.207)	(0.193)	(0.207)	(0.193)	(0.215)	(0.197)
Information[1]	-	-	-	-	-	-	-	-	-	-
product		0.425***		0.438***		0.441***		0.440***		0.492***
		(0.114)		(0.111)		(0.112)		(0.112)		(0.110)
Constant	1.173*	0.823	-0.644	-0.313	-0.234	-0.0393	-0.210	-0.0242	-0.203	0.0122
	(0.627)	(0.596)	(0.416)	(0.398)	(0.391)	(0.368)	(0.391)	(0.368)	(0.414)	(0.382)
Observations	109	109	109	109	109	109	109	109	109	109
R-squared	0.221	0.321	0.201	0.311	0.197	0.308	0.198	0.309	0.138	0.286

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1
[1]Information is only available for Earned treatment.

Table 1.G.2: Effort and distributive preferences: Random treatment.

Chapter 2

Short-term unemployment fluctuations and voter turnout

2.1 Introduction

The relationship between economic conditions on the one hand and voting preferences and behavior on the other is one of the central questions in political science. However, although the particular link between unemployment and voter turnout has been extensively studied for decades and received considerable attention after the 2008 financial crises, there is no consensus regarding whether unemployment stimulates or discourages voting participation and the mechanisms driving this relationship are poorly understood (see Sockemer (2017) and Margalit (2019) for recent surveys). As a consequence, we do not know whether and, if so, under which conditions politicians are held accountable for poor economic performance and we cannot predict how diverse economic shocks affect political behavior. This is a particularly important issue for politics because the voting behavior of a small group might have dramatic consequences on electoral outcomes.

The classic literature on the topic has argued in favor of one of two competing hypothesis. The *Mobilization Hypothesis* predicts a positive relationship between unemployment and turnout because people experiencing economic hardship will blame the government for their situation and engage in political activity in order to express discontent (Schlozman & Verba, 1979). In contrast, the *Withdrawal Hypothesis* posits a negative relationship between unemployment and turnout: people undergoing economic hardship would suffer from problems caused by their financial situation, devoting all their resources to “hold body and soul together,” and withdraw from politics (Rosenstone, 1982). Which effect dominates is an empirical question, but the evidence is mixed: there exist studies documenting negative (Caldeira et al, 1985; Rosenstone, 1982; Southwell, 1988; Wolfinger & Rosenstone, 1980), null (Arcelus & Meltzer, 1975; Conway, 1981; Fiorina, 1978; Kinder & Kiewiet, 1979, 1981), as well as positive (Artés, 2014; Burden & Wichowsky, 2014; 2019; Martins & Veiga, 2013) link between the two phenomena. Why do different studies find such disparate results?

Incantalupo (2011) proposed an *Unemployment-In-Context* theory that argues that the effect of unemployment on turnout depends on the context: when unemployment is low, voters tend to perceive unemployment as a personal failure and withdraw from politics. In contrast, under high unemployment, voters perceive unemployment as a societal problem, for which elected representatives should be held accountable, and tend to turn out. Incantalupo (2011) relates the propensity to turn out in the U.S. to unemployment, finding support for his hypothesis.[1]

These results notwithstanding, analyzing the causal link between unemployment and turnout entails several empirical challenges. The main challenge stems from the fact that economic shocks are not assigned randomly. Another challenge is that elections are "rare" events, taking place in long time intervals, not necessarily aligned with economic shocks. There thus might be a considerable time gap between the shock (e.g. loss of a job) and the elections or, especially in case of aggregate shocks, people might realize that the economic conditions have changed after the elections have taken place. As a result of both challenges, the effects documented using typical voting data might be confounded with longer-term structural changes in the economy that might go hand in hand with other social, economic, and political changes in the society as a whole. This in turn prevents researchers from making general claims concerning whether any effect is causal or due to unobserved variables that impact both unemployment and voting. These issues are exacerbated by the observation that the impact of economic shocks on voting behavior is typically transient (Margalit, 2019) and by behavioral predispositions of people to exhibit numerous perception biases and short memories (Stevenson and Duch, 2013).

Previous literature mostly relies on cross-sectional analyses and has only recently started to use dynamic data. most studies suffer from omitted variable bias if they do not control for the local socio-economic "context" as illustrated by Incantalupo (2011), and we are aware of no study that would systematically target the misalignment in the timing of the shocks and the elections. Most importantly though, note

[1] Relatedly, Killian et al. (2008) show that voters are more prone to turn out when they perceive their personal economic situation as being worse than national average and Cebula (2017) shows that the relative unemployment rate of the state with respect to the national unemployment rate is positively associated to turnout rate in the U.S. presidential elections.

that the proposed mechanisms behind the link between unemployment and turnout heavily center on the economic hardship and therefore on the "unemployed." We overcome these issues by employing dynamic data to explore the *changes* in the dependent and independent variables, we control for the context as in Incantalupo (2011), and we exploit a natural-experimental nature of our data that mitigate the misalignment of unemployment fluctuations and the timing of elections (see below). Last, we combine macro- and individual-level data to be able to uncover the motivations of different population segments in function of their employment status and labor-market involvement.

From a theoretical perspective, we propose a *Dual Voting Hypothesis* that posits that the aggregate link between unemployment and turnout results from a complex interaction between the context a là Incantalupo (2011) and the conflict between pocketbook and sociotropic participation motives. Our theory is based on two observations. First, even population segments affected indirectly or even unaffected by economic shocks are likely to perceive the changing economic conditions and thus adapt their voting behavior. That is, the employed and people not in the labor force might condition their political participation to labor market conditions, but each segment reacts *differently* to unemployment shocks. Second, different types of voters have different stakes in the labor market, they face different contexts, and they are subjects to different economic shocks. More precisely, we argue that the more at stake an individual has in the labour market, the more her behavior will be driven by pocketbook as opposed to sociotropic voting motives. The labor force would represent one extreme with voting behavior mostly dominated by their individual interest. On the other extreme, pensioners with little at stake in the labour market would be the segment that "can afford" to behave in line with sociotropic motives.

We thus hypothesize that individuals who loose their job mobilize themselves but only if they perceive their worsened prospects to be a problem of the society for which to blame the government (i.e. under high unemployment). In contrast, people who find a job might actually disconnect from political involvement under such adverse conditions as they would attribute their new job to their abilities and effort, leading them view unemployment as an individual, rather than a societal problem. Under low unemployment, the whole population, including the labor force, would view unemployment as a personal issue and they would not be mobilized, and people loosing job might in fact withdraw from politics in line with the Withdrawal hypothesis. Although we expect differing impact depending on the overall unemployment, observe that our theory posits that the turnout of the labor force will be primarily driven by poctketbook voting motives, but the direction would depend on the context. In contrast, people with little or no stakes in the labor market behave according to sociotropic motivations, using local unemployment rates as a signal of how the economy functions. Among such out-of-labor agents, we focus on pensioners who do not rely–and are likely to never rely in the future–on work compensation. We thus expect them to be primarily driven by sociotropic reasons and be mobilized in response to negative unemployment shocks, but the strength of the effect should increase with the unemployment in line with blaming-the-government argument.

The aggregate link between unemployment and turnout then results from the interaction among these differing–and sometimes opposing–forces. If unemployment increases in a high-unemployment context, those loosing jobs would outnumber those finding work, leading to an overall mobilization among the labour force that would be reinforced by the mobilization of those out of the labor force. Hence, higher unemployment would unambiguously stimulate political participation if the initial unemployment is high. The prediction is more ambiguous under low unemployment: The labor force would withdraw from politics overally while pensioners might be mobilized in response to increasing unemployment. The aggregate impact thus depends on the relative numbers of each segment. Since there typically are more people in the labor force, compared to out-of-labor force (and this is also the case of Spain, the country under study), we predict an increase in unemployment to discourage voting participation under low unemployment areas but this effect is expected to be weaker (in absolute terms), in comparison with the effect under high unemployment.

To test our hypotheses, we provide a comprehensive analysis of the link between employment fluctuations and voter turnout using data from the 2015 and 2016 general elections in Spain. The 2015 elections led to the most fragmented Spanish parliament in the democratic history of the country and the parties failed to agree on a viable government coalition. As a result, the elections were repeated in June 2016, six month and six days later. We exploit the natural-experiment-like nature of this event: such a fast repetition of the election enables to study the clean impact of short-term economic fluctuations on voting behavior because such a short period prevents the country from undergoing any structural changes of the overall state of the economy and the socio-political environment. However, the data exhibit a sufficiently rich unemployment dynamics at the regional level that we exploit. We take advantage of local unemployment fluctuations to study the joint impact of the individual and regional changes in unemployment, in isolation from changes in the overall state of the economy, on changes in voter turnout. Methodologically, we combine register and survey data and perform dynamic multilevel analysis using different geographical levels to analyze how changing economic conditions affect political mobilization at the individual as well as aggregate level. We particularly focus on the contrast between the labor force–exploiting both both job loss and employment between the two elections–and pensioners. The former are directly affected by the unemployment dynamics, while the latter are not exposed to any individual shock but perceive the changing economic conditions as discussed above.

In line with the Unemployment-in-Context hypothesis, the official Spanish voting and unemployment registers show that the relationship between unemployment and turnout is mediated by the local unemployment context. When comparing regions with a high initial level of unemployment with those with a low initial level of unemployment, we observe that increases in unemployment lead to an increase in turnout in the former while fluctuations in employment have a negative–although non-significant–effect on turnout in the latter. This evidence and our approach provides a stronger causal argument for the Unemployment-in-context hypothesis.[2]

In order to look at the mechanisms underlying this result, we conduct a multi-level analysis combining official aggregate regional unemployment and voting data with individual labour and voting information from official surveys from representative samples of the Spanish population. We first corroborate the above aggregate finding at the individual level: unemployment stimulates the turnout of survey respondents living in high-unemployment regions, while the respondents living in low unemployment areas are somehow discouraged. Nevertheless, as hypothesized, the aggregate picture results from a complex process. Respondents from the labor force only change their voting behavior in response to changes in their personal employment status, whereas the aggregate unemployment dynamics does not predict the changes in their political participation. Therefore, the labour force is driven by pocketbook voting motivation. In contrast, pensioners are more likely to turnout if unemployment increases at the aggregate regional level and this effect increases with average unemployment, suggesting that sociotropic motivation and context drive their turnout decision.

These results provide a support for our *Dual Voting Hypothesis* according to which both pocketbook and sociropic voting motives matter depending of the interests of different population strata. We show that pocketbook motives dominate sociotropic arguments among the labor force, whereas the latter dominate among pensioners with no stakes in the labor market. This line of reasoning is parallel to the classic correlation between economic standing and the support for redistribution policies: richer people are against more redistribution while poorer people favor more redistribution and the former decrease the support as they become richer (Alesina and Giuliano 2010; Margalit 2013). Here, we argue that pocketbook voting motives dominate among those who have personal interest in the labor market, while those

[2]This additionally makes us confident that Spanish politics and the two specific elections under scrutiny do not exhibit an unusual link between unemployment and turnout.

who do not can "afford" to behave according to collective sociotropic interest. Our arguments and findings thus provide another step toward resolving the conflicting results in the literature regarding the link between unemployment and turnout and regarding the conflict between the Mobilization and Withdrawal hypotheses.

Another contribution of this study is to position different voting motives among the determinants of political participation. To the best of our knowledge, the literature resolving the conflict between the pocketbook and sociotropic motives studies the role of different voting motives on vote choice (Fiorina, 1981; Healy et al., 2017), rather than on whether people vote. However, since there is large evidence that different motives affect who people vote, it would not be surprising that they also matter for turnout. We provide evidence that it is the case.

2.2 Background

Spain celebrated its 11th general election since its transition to democracy on December 20, 2015. Although the country was undergoing a modest recovery after the 2008 crisis that hit the Spanish economy particularly hard as compared to other countries, most of the population was still dissatisfied with the high unemployment rates and low wages in the country. No party obtained enough votes in the 2015 election to be able to form a stable government and the four most voted parties (*PP*, *PSOE*, *Podemos*, and *Cuidadanos*) did not arrive to any agreement. As a result, the country was in a political deadlock and the Spanish King Felipe VI on May 3, 2016 decided to call new elections. The new elections were held on June 26, 2016.

This led Spain to run two elections in less than seven months, dramatically shorter time gap than the usual period of four years. In such a short time span, Spain economy underwent very little change and the prospects of growth and employment creation were not changed substantially. However, there was an important regional variation in the (un)employment dynamics, a substantial part of which can be attributed to the arrival of the summer that usually comes hand in hand with an increase of employment in Spain due to the increase of domestic and international tourism. The Spanish economy to a large extent relies on tourism. As a consequence, the arrival of the summer traditionally produces a positive shock on employment due to the creation of seasonal jobs that usually shut down in autumn. In the specific period under study ranging from December 2015 to June 2016, a total number of 357,271 jobs were created (see table 2.1), but longer-term growth and unemployment prospects were not modified.

We exploit the unequal variation of seasonal shocks in unemployment across different geographical areas in order to measure the effects of short-term unemployment fluctuations on electoral outcomes in a context were the long-term state of the economy remained stable. This provides a context enabling us to measure the effects of short-run changes in unemployment with little changes in the the overall state of the economy and the society as a whole.

Regarding the political outcomes, we focus on voter turnout for its dynamics. The overall participation rate decreases by 1,186,348 individuals in the June 2016 elections with respect to the December 2015 elections, but this aggregate figure masks important regional variation in the turnout evolution, which showed a great degree of heterogeneity at local lelves. We observe variations in participation ranging from a decrease of 21% to an increse of 53% depending on the municipality. This study relates the participation dynamics to the unemployment fluctuations.

2.3 Data

This article employs three data sources. We combine the official Spanish electoral data, collected by the Spanish National Statistics Institute (*Instituto Nacional de Estadística*; INE, hereafter), unemployment data collected by the Spanish National Employment System (*Servicio Pùblico de Empleo Estatal*; SEPE, hereafter) and survey data collected by the Spanish Center for Sociological Research (*Centro de Investigaciones Sociológicas*; CIS, henceforth).

2.3.1 Register data

To measure the aggregate regional statistics, we use official Spanish data downloaded from the INE (`https://www.ine.es`). To measure turnout, we employ data from the Spanish general elections run on December 20, 2015 and June 26, 2016. To measure unemployment, we use monthly collected register data corresponding to the number of people registered in the SEPE (`https://www.sepe.es/`); we particularly focus on December 2015 and June 2016. Importantly, these data include the whole Spanish population (rather than a sample). The employment of official register data has obvious advantages over survey data and other data-collection techniques relying on self-reports, including the absence of response bias and the eventual inaccuracies due to respondents' memory issues.

Since the above data are available at different geographical levels, we introduce the four levels to which Spain is administratively divided: *comunidades autónomas*, provinces, *comarcas*, and municipalities. *Comunidades autónomas* are the first-level political and administrative units in the country. There are 17 of them in Spain and they differ in many aspects. Nevertheless, since we cannot perform any meaningful statistical analysis with 17 observations, we mostly work with the other three aggregation levels described in more detail in what follows:

(A) *Municipalities.* Municipalities (*municipios* in Spanish) correspond the most basic administrative level in Spain and it is the lowest disaggregation level for which electoral and employment data are available. There is a total of 8,113 *municipios* in our data.[3] Although this disaggregation maximizes the number of observations, the municipalities are very heterogeneous in size and socio-economic characteristics. For example, the population of the municipalities in our dataset at working age range from 2 to 2,043,166 with a standard deviation of 30,179.62.

(B) *Comarcas.* These geographical units correspond to a set of territories that usually comprehend several municipalities within one province (see point (C) below). *Comarcas* usually form a natural region composed by territories sharing common physical, historic, and human characteristics. Despite the historical importance of the concept of *comarcas* in Spain, only five out of the 19 *comunidades autónomas* (*Aragón, Catañuña, País Vasco, Comunidad Valenciana*, and *Galicia*) recognize the *comarcas* as official administrative units. For these *comunidades autónomas*, we use the official delimitation. For the remaining *comunidades autónomas*, we consider *comarcas agrarias*, a delimitation established by the Spanish Ministry of Agriculture and Fishing, Feeding and Environment that divides the territories in units that are similar in nature to *comarcas*.[4] This classification results in 404 observations. The advantages of this aggregation level is that it enables to reduce significantly the degree of heterogeneity across the observational units while maintaining the number of observations high enough to preserve a sufficient power of our statistical analysis.

[3]In Spain, there are 8,124 *municipios*. Although we gathered the data on all of them, several *municipios* merged or were split into multiple ones during the period under study. We thus restict our analysis to the municipalities that did not overcome any such change.

[4]As explained above, most *comarcas* are formed by several adjacent municipalities within a single province. However, there are five *comarcas agrarias* composed by municipalities that are adjacent but belong to more than one province. In all such cases, the *comarca* is formed by a majority of municipalities belonging to one particular province and a minority belonging to an adjacent province. We thus include the *comarca* into the province, to which the majority of the municipalities belong. The cases are: (a) *La Jacetania* composed by 20 municipalities, from which 16 belong to the province of Huesca and 4 to the province of Zaragoza. We include it in Huesca. (b) *Bergueda* composed by 30 municipalities, 19 belonging to Barcelona and 1 to Lleida. We include it in Barcelona. (c) *Cerdanya* composed by 17 municipalities, 11 belonging to Girona and 6 to LLeida. We include it in Girona. (d) *Osona* composed by 50 municipalities, 47 belonging to Barcelona and 3 to Girona. We include it in Barcelona. (e) *Selva* composed 21 municipalities, 20 belonging to Girona and 1 to Barcelona. We include it in Girona.

The working-age population of our *comarcas* ranges from 897 to 3,139,275 with a standard deviation of 201,639.3.

(C) *Provinces.* Our highest aggregation level corresponds to provinces, the second highest order administrative unit in Spain (only bellow the *comunidades autónomas*). There exist 52 provinces, each composed of a number of municipalities that typically share a common history, culture and other relevant climate, territorial, and economic characteristics. Naturally, this administrative unit reduces the number of observations considerably, leading to issues with statistical power. As a result, we give more confidence to the results corresponding to municipalities and *comarcas* and include the province-level analysis as a robustness check.

Tables 2.1-2.3 show summary descriptives. Table 2.1 gives an overview of relevant variables at national level, while Table 2.1 shows a summary description of population sizes at the different aggregation levels. As we can see, at the lowest level of aggregation (Municipalities), we have a high number of observations (8113), but also great heterogeneity in terms of population size, which range from 2 inhabitants to 2043166. The contrary is observed at the highest level of aggregation, in which we observe a substantially higher level of heterogeneity in population size at a cost of a dramatic decrease in the number of observations, which are narrowed to 52. The level of Comarcas represents and intermediate case. In order to overcome these issues of each and make the most of our data, we combine these three aggregation levels in our analysis. Table 2.3 shows averages of our two main variables of interest; unemployment and turnout rates. They both decreased from December 2015 to June 2016 by the same percentage, but the aggregate figures do not allow to appreciate the regional differences and potential contextual variables.

	December 2015	June 2016	Δ
Unemployed	4112082	3754811	- 357271
Participation	25311568	24125220	- 1186348
Census	34578948	34540632	- 38316
Working Age Population	30542072	30542072	0

Table 2.1: Descriptives

2.3.2 Survey data

The CIS (`https://www.cis.es`) is a public scientific institute studying Spanish society and collecting data on numerous issues of socio-economic interest, including

		Working Age Population		
	N	Mean (SD)	Minimun	Maximum
Municipalities	8113	3764.584 (30179.62)	2	2043166
Comarcas	404	75599.19 (201639.3)	897	3139275
Provinces	52	587347.5 (762261.2)	55986	4284992

Table 2.2: Number of observation and working age population by aggregation level

	December 2015: Mean (SD)	June 2016: Mean (SD)	Δ
	Municipalities		
Unemployment Rate	0.1104734 (0.0583495)	0.0949463 (0.0547454)	-0.0155271 (0.0298808)
Participation Rate	0.7500952 (0.0690364)	0.7349863 (0.0753723)	-0.0151089 (0.0458901)
	Comarcas		
Unemployment Rate	0.1291841 (0.0387829)	0.1141425 (0.0382935)	-0.0150416 (0.0175663)
Participation Rate	0.7288295 (0.0425751)	0.7017524 (0.0476019)	-0.0270771 (0.0166789)
	Provinces		
Unemployment Rate	0.1396301 (0.0344017)	0.1255508 (0.0364726)	-0.0140793 0.0161853
Participation Rate	0.7251378 (0.0505709)	0.694432 (0.0521516)	-0.0307057 (0.0103501)

Table 2.3: Average Unemployment and Participation Rates

pre- and post-electoral surveys for each Spanish election. To these aims, they interview a representative sample of the Spanish population. Most importantly, the data are available at the individual level, enabling to study how personal unemployment shocks affect one's decision to vote.

The surveys typically only contain information about respondents' province of residence, the highest aggregation level discussed in Section 2.3.1.[5] Therefore, our multilevel analysis in Section 2.4.2 combining the register and individual survey data is based on only 52 unemployment rates corresponding to the 52 provinces in our data. Assigning these 52 values to each individual depending on the provide generates certain multicolineality issues, discussed in detail in Section 3.3.

Here, we focus on the pre- and/or post-election surveys from the general 2015 and 2016 elections:

(A) 2015 pre- and post-election surveys. Within the two months previous and next to the 2015 elections, the CIS interviewed the same respondents in what we call the 2015 pre- and post-electoral surveys, respectively. These data thus generate a two-period panel, in which the same individuals report their employment status and their (intended or recalled) voting behavior.[6] More precisely, for the pre-electoral survey, the respondents are asked whether they intend to vote in the 2015 elections; in the post-electoral survey, they are asked whether they indeed voted or not. Hence, the former measures the intention to vote, while the latter measures the recalled turnout. One can argue that the intention to vote and the recalled voting decision reflect different phenomena. Although this is a valid argument, we claim that both variables reflect participants' motivation to vote. Hence, they can be considered as valid approximations of the *willingness to turn out* of each respondent while participating in the survey. Most importantly though, the results generated using these data are but one piece of evidence in our overall empirical strategy and the results generated using these data are corroborated in other specifications.

The dependent variable from these data is constructed using binary indicators of intended abstention and recalled abstention, respectively. More specifically, $Abstention_{it=0-} = 1$ for respondents who answer either "No, for sure" or "Most likely not" to the question of weather they would vote in the elections in the pre-electoral survey; $Abstention_{it=0-} = 1$ otherwise. $Abstention_{it=0+} = 1$ for respondents who declared that they did not vote; $Abstention_{it=0+} = 1$ otherwise.

[5]More local information, such as the municipality or *comarca* of residence, is not available though.

[6]The data-collection before and after the elections from the same subjects was unique to 2015. Such data structure is not available for any other elections.

Concerning the employment status, each respondent reports whether she is employed, unemployed, or has a non-work-related occupation at the time of the survey in both the pre- and the post-electoral surveys. This way we can observe any change in the employment status, namely job loss of employed participants and employment of those unemployed before the elections.

Naturally, these data contain no information on the 2016 elections as the decision to run them was taken by the Spanish King Felipe VI on May 3, 2016. As a consequence, the 2015 surveys cannot be linked to the 2016 elections. We employ them in Section 3.3 to investigate the impact of changes in personal employment status on the changes between the intention to turn-out and the recalled turnout. This will allow us to make causal inference on the relationship between personal unemployment and turnout by applying a difference-in-differences approaches.

Since both surveys were run in December 2015 and official unemployment data are only available monthly, there are no data on the aggregate unemployment fluctuations between the pre- and post-electoral surveys. As a result, rather than including the change of the local unemployment context as in model (2.2), we simply study whether the impact of the two variables of interest is mediated by the overall level of unemployment in December 2015 in the province of the respondent's residence.

Our data for this analysis consist in a panel of 6,185 responders.[7] Tables 2.4 and 2.5 show summary statistics about the sample.[8]

(B) 2016 post-electoral survey. The CIS uses the 2016 post-electoral survey, conducted within the month that followed the elections, to elicit participants' turnout in both the 2015 and 2016 elections and certain information about their employment history. This first enables to construct an abstention dummy variables $Abstention_{it=0}$ and $Abstention_{it=1}$ for the 2015 and 2016 elections, respectively.

As for the working status, the data contains the employment status in June 2016. However, the survey only provides partial information about the employment status of the respondents in 2015. More specifically, all unemployed respondents were asked how long they have been unemployed. Such unemployed participants were provided the following possible answers: "Less than 6 months", "Between 6 moths and a year", and other options referring to periods longer than a year. Since the time gap between

[7]17,452 people were interviewed for the pre-electoral subject, of which 6,241 also participated in the post-electoral survey (In addition one subject who had not participated in the pre-electoral survey participated in the post-electoral). We focus our analysis on the panel formed by these responders. Of these, a total number 56 responders were excluded from the analysis because they deny to respond to questions concerning our main variables (employment status or abstention) in at least one of the surveys or had an unidentifiable employment status. This gives us a total number of 6,185 responders.

[8]We describe the variable $\Delta Abstention_i$, shown in Table 2.5 in Section 2.4.2.

	N of responders			Abstentions	
	Pre	Post		Pre	Post
Employed	2,588	2,644		161	291
Unemployed	1,222	1,126		90	186
Student	289	280		12	44
Housekeeper	442	449		30	57
Pensioner	1,644	1,686		78	178
N	6185		Total	371	756

Table 2.4: Descriptive statistics of 2015 Pre-Post Electoral survey panel. Number of responders and abstentions by employment status

Employment Status	
Lost Employment	172
Got Employed	216
Voting Decision	
$\Delta Abstention_i$=-1	272
$\Delta Abstention_i$=0	5,481
$\Delta Abstention_i$=1	432

Table 2.5: Changes in employment status and voting decisions from pre- to post-electoral survey

the two elections is six months, respondents who chose the first option are considered employed at the time of the 2015 elections. Unfortunately, we have no information on the 2015 employment status of subjects who were not unemployed in June 2016. We therefore construct a binary indicator distinguishing between those who lost a job in less than six months between the two elections and all the other respondents. This limitation notwithstanding, the available information allows us to perform an analysis similar to the difference-in-differences in reverse analysis proposed by Kim and Lee (2019) in order to study the effects of job loss.

Our sample for this analysis contains data from 5,264 respondents[9]. Tables 2.6 and 2.7 show summary statistics about the sample.

	N of Responders		Abstentions	
	2016		2015	2016
Employed	2,405		326	413
Unemployed	888		198	248
Student	247		44	51
Housekeeper	393		49	65
Pensioner	1,331		154	193
N	5,264	Total	771	970

Table 2.6: Descriptive statistics of 2016 Post-electoral survey. Number of responders and abstentions by employment status (at the time of the survey)

Employment Status	
Lost Job	167
Voting Decisions	
$\Delta Abstention_i$=-1	220
$\Delta Abstention_i$=0	4,625
$\Delta Abstention_i$=1	419

Table 2.7: Changes in employment status and voting decisions from the 2015 to the 2016 elections

[9]A total number 911 responders were excluded from the analysis because they denied to respond whether they voted or not in 2015 or in 2016 or had an unidentifiable employment status

2.4 Results

Our analysis is divided into two parts. Section 2.4.1 presents an aggregate, macro-level analysis of voters' turnout, while Section 2.4.2 conducts a multi-level analysis contrasting the individual vs. macro determinants of one's decision to vote.

In order to analyze causally the relationship between unemployment and turnout, the idealized scenario is to regress the changes in unemployment between the 2015 and 2016 elections on the corresponding differences in turnout. By taking the differences, we control for any influence of all time-invariant characteristics of the observations. As for any time-varying heterogeneity, we take advantage of the particularly short time gap between the two elections, arguing that any differences between two periods would be negligible. Hence, the combination of the models in differences and the quasi-experimental nature of our data consisting in a particularly short time gap between two election events deem unnecessary the inclusion of controls into our models and allows us to make causal claims regarding the relationship between the two variables using a simple model.

The macro-level analysis in Section 2.4.1 follows this approach.

However, the individual-level survey data described above present several limitations that prevent us from being able to follow the ideal approach verbatim. Moreover, we are interested in whether both the individual and aggregate (un)employment shocks influences one's decision to vote. Hence, we would ideally regress the change in one's employment status, the change in regional unemployment, and their interaction on the change of individual turnout, employing a panel of interviewees for the 2015 and 2016 elections. Nevertheless, such panel data are not available and only the province of residence of the survey respondents is known. This generates two issues. First, we cannot connect the people from the 2015 and 2016 surveys and estimate a model in differences at the individual level. Second, there are only 52 provinces leading to 52 different values of the unemployment rate. The problem is that assigning such a small number of values to each survey respondent generates extreme correlations between two of the "variables", the unemployment rate of the province an individual resides in and the interaction between this varaible and the individual unemployment shocks.[10]. Hence, we cannot include the two variables in the same model due to the multicollinearity issues. Notwithstanding this, we are able to overcome these issues and follow the ideal approach closely enough (see Section 2.4.2 for details).

[10]Correlation between individual employment status and their interactions with province level unemployment rates reach values of $\rho > 0.95$. As a result, models including this interaction term show variance inflation factors (VIF, hereafter) above 14

2.4.1 Macro-level Analysis

The objective of this section is to replicate the marco-level results from the previous literature that mostly employs data from the U.S. To that aim, we first ask whether there is a direct relationship between unemployment fluctuations and voter turnout. Then, we ask whether the relationship is context-dependent as suggested by Incantalupo (2011).

Direct (unmediated) relationship between unemployment and turnout. We start estimating the following model:

$$\Delta Turnout_i = \alpha_i + \beta * \Delta Unemployment_i + e_i, \tag{2.1}$$

where $t = 0$ corresponds to December 2015 and $t = 1$ to June 2016, and i labels the geographical areas. Then, $\Delta Turnout_i = Turnout_{it=1} - Turnout_{it=0}$ and $\Delta Unemployment_i = Unemployment_{it=1} - Unemployment_{it=0}$. We estimate model (2.1) for each of our three aggregation levels, municipalities, *comarcas*, and provinces.

The estimation results are summarized in Table 2.8; Figure 2.1 plots the data and the estimated relationships. Irrespective of the geographical level, there exists a significantly positive relationship between the regional changes in unemployment and the change in turnout. Quantitatively, we estimate that one percentage increase in unemployment raises the turnout rate between 2.86% and 21.8% depending on the specification, but due to the low number of provinces, we would give more confidence on the estimates using municipalities and *comarcas*. These results support the *Mobilization Hypothesis*: unemployment makes people more prone to vote in our data. Notwithstanding this, the following analysis shows that these results suffer from the omitted variable problem.

Unemployment In Context. Incantalupo (2011) suggests that model 2.1 suffer from the omitted variable issues if one does not control properly for the context. In particular, he argues that the increases in unemployment should have a positive effect on turnout in high unemployment regions, whereas the effect should switch the sign under low unemployment. Since Incantalupo (2011) only provides correlation evidence, our regressions in differences provide a more causal evidence of his theory.

To test his hypothesis in our data, we present two sets of models. First, we simply reestimate model (2.1) separately for municipalities and *comarcas* below and above the median unemployment rate in December 2015.[11] The result are presented

[11] This specification does not suffer from the multicolineality issues mentioned above.

VARIABLES	(1) Municipalities	(2) Comarcas	(3) Provinces
ΔUnemployment	0.0286*	0.121**	0.218**
	(0.0170)	(0.0470)	(0.0850)
Constant	-0.0147***	-0.0253***	-0.0276***
	(0.000574)	(0.00109)	(0.00181)
Observations	8,113	404	52
R-squared	0.000	0.016	0.116

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.8: OLS Regressions on ΔTurnout. Estimated coefficients. Standard errors in parentheses. Separate regressions for each aggregation level.

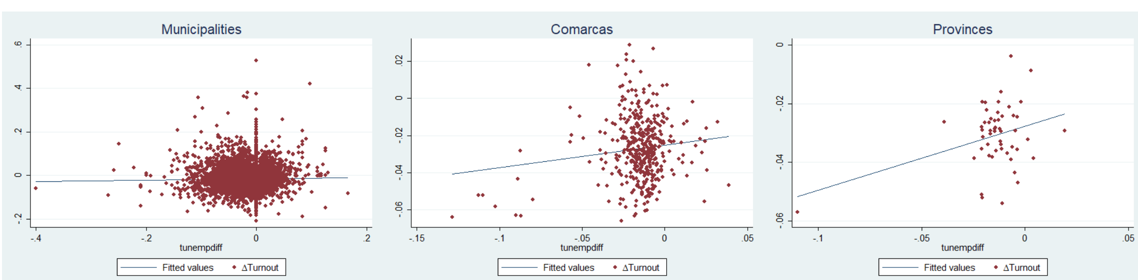

Figure 2.1: Effect of ΔUnemployment on ΔTurnout. Each point in the scatterplot represents a municipality, comarca or province respectively. The line represents the linear relationship between ΔUnemployment and ΔTurnout.

in Table 2.9; Figures2.2 plos the empirical and estimated relationship for the municipalities and *comarcas*, respectively.[12] The table reveals that, irrespective of the disaggregation, the impact is indeed mediated by the context: the estimated relationship between changes in unemployment and changes in turnout is positive territories with high unemployment, while the relationship is negative under low unemployment. That is, if unemployment is high, its increases mobilize people to vote whereas people are discouraged by raising unemployment under low unemployment. Note though that the negative relationship in the low-unemployment areas is not statistically significant.

As a second specification, we test the same hypothesis estimating the following extension of model 2.1:

$$\begin{aligned} \Delta Turnout_i = \alpha_i + \beta_1 \Delta Unemployment_i + \\ + \beta_2 \Delta Unemployment_i \times UnemploymentRate_{t=0} + e_{it} \end{aligned} \tag{2.2}$$

where $UnemploymentRate_{t=0}$ is the unemployment rate in either the municipality or *comarca* in t=0 (i.e. in December 2015). Model 2.2 enriches model 2.1 and the approach taken in Table 2.9 by asking whether the impact of the change of unemployment varies linearly with the initial unemployment conditions within each area and allows to test directly whether the effect of the unemployment rate differs in a low- vs high-unemployment context within one unique regression.[13]

Table 2.10 displays the estimates. The results at both geographical levels corroborate that the *impact* of unemployment interacts positively with the initial unemployment rate ($\beta_2 > 0$, $p < 0.05$). This confirms the hypothesis that the unemployment context mediates whether changing economic conditions mobilize or discourage voters. In addition, since the impact would be negative ($\beta_1 < 0$, $p < 0.01$) if the unemployment was theoretically zero but positive if the unemployment was one ($\beta_1 + \beta_2 > 0$, $p < 0.01$[14]), the results reinforce those in Table 2.9. However, as the unemployment rates are overally high in Spain as compared to other European countries or the U.S., the lower bound of unemployment rates in Spain is higher than in the data from the U.S. This could explain why the negative effect in Table

[12] The same models for provinces would generate unreliable estimates due to an extremely low number of observations.

[13] This approach additionally increases the statistical power of the models in comparison with Table 2.9 but can suffer from the discussed multicolineality issues. The VIF suggest that there is no multicolineality problem with regression (1) in Table 2.9, but model (2) indeed exhibits multicolineality ($VIF = 18.04$). Since the results in column (2) corroborate those in column (1), we maintain the estimates in Table 2.9 for interested readers.

[14] We test $\beta_1 + \beta_2$ is signifficantly different from 0, in both of the models shown in Table 2.10. We obtain p=0.0000 for model (1) and p=0.0011 for model (2)

2.9–significant in the U.S. data (Incantalupo, 2011)–does not reach significance at 5% for the range of unemployment rates in Spanish regions.[15]

In sum, our macro-level analysis replicates the existing findings, suggesting that the Spanish case does not differ qualitatively from the U.S. data typically employed in the previous literature[16]. In addition, our approach provides stronger evidence for the causal interpretation of the *Unemployment-in-context* hypothesis of Incatalupo (2011). The following section investigates the mechanisms behind these macro-level effects.

	(1)	(2)	(3)	(4)
	Mun. in Com.	Mun. in Com	Com. in Prov.	Com. in Prov
	Above	Bellow	Above	Bellow
ΔUnemployment	0.0468**	-0.0143	0.235***	-0.142
	(0.0198)	(0.0276)	(0.0535)	(0.0910)
Constant	-0.0177***	-0.0122***	-0.0212***	-0.0306***
	(0.000719)	(0.000861)	(0.00148)	(0.00163)
Observations	3,812	4,301	195	209
R-squared	0.001	0.000	0.091	0.012

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.9: OLS Regressions on ΔTurnout. Estimated coefficients. Standard errors in parentheses. Sample of models (1) and (2) consist in municipalities in comarcas with an unemployment rate above (1) and bellow (2) the median respectively. Sample of models (3) and (4) consist in comarcas in provinces with an unemployment rate above (3) and bellow (4) the median respectively.

2.4.2 Individual-Level Analysis

The previous section shows how changes in unemployment rates affect turnout at the macro-level. This section complements the previous one by studying the impact

[15]We additionally estimate the relationship proposed by Cebula (2019), employing the ratio between the unemployment rate and the national unemployment rate. This specification captures the effect of the *relative* unemployment. The results are in line with those reported here. See Appendix A

[16]The results shown in Tables 2.8-2.10 hold when we re-estimate the models excluding outliers.

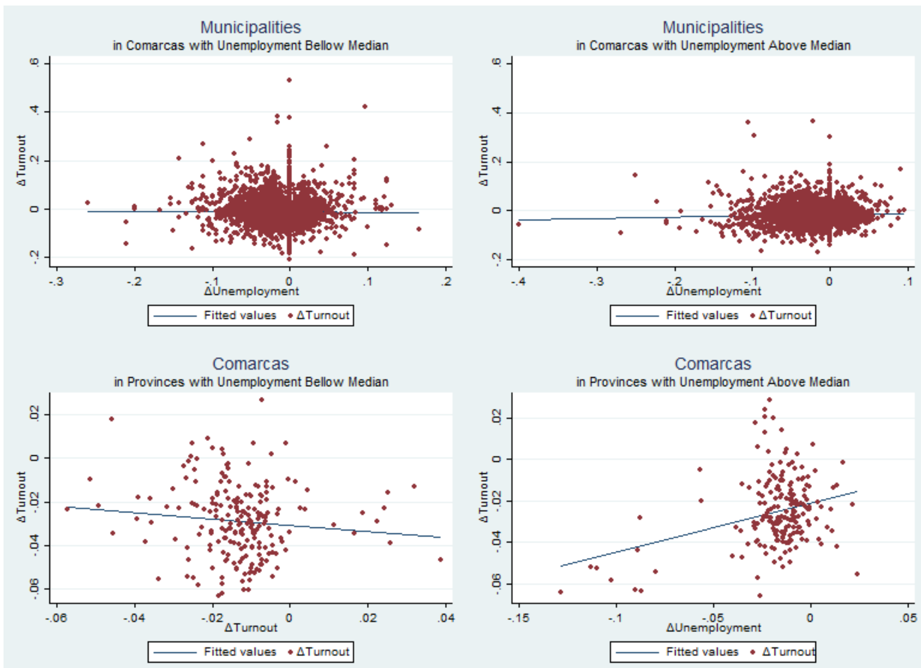

Figure 2.2: Effect of ΔUnemployment on ΔTurnout by Initial Level of Unemployment. Each point in the scatterplot represents a municipality, comarca or province respectively. The line represents the linear relationship between ΔUnemployment and ΔTurnout

	(1) Municipalities	(2) Comarcas
ΔUnemployment	-0.318***	-0.486**
	(0.0533)	(0.197)
ΔUnemployment$_i$* Unemployment Comarca$_{t=0}$	2.599***	
	(0.379)	
ΔUnemployment$_i$* Unemployment Province$_{t=0}$		3.580***
		(1.130)
Constant	-0.0145***	-0.0266***
	(0.000573)	(0.00116)
Observations	8,113	404
R-squared	0.006	0.040

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.10: OLS Regressions on ΔTurnout. Estimated coefficients. Standard errors in parentheses. Model (1) estimates the effects of changes in unemployment (at the municipality level) and the interaction between these changes and the initial employment rate (at the comarca level).Model (2) estimates the effects of changes in unemployment (at the comarca level) and the interaction between these changes and the initial employment rate (at the province level).

of unemployment shocks at the individual level and asks whether the impact differs across different sociological groups.

To address these issues, we report a multi-level analysis, combining the aggregate data described in Section 2.3.1 (and employed in the previous section) with the survey data from Section 2.3.2. Our approach is designed to study two questions. First, do the individual or the aggregate (un)employment shocks (or both) affect participation and, if so, how? Under an ideal scenario (similar to Section 2.4.1), we regress the change of individual turnout on the change of individual employment status, the change in regional unemployment, and their interaction, employing a panel of interviewees for both the 2015 and 2016 elections. Unfortunately, such data structure is not available at the individual level and the ideal approach leads to the multicolinearity issues discussed above. However, the available data enables two modelling strategies that approximate the ideal specification and corroborate each other. Second, we analyze these issues for different population strata to investigate how the impact of unemployment shocks differs according to the involvement in the labor market.

2015 pre- and post-electoral surveys. Recall that these two surveys constitute a panel of the same people interviewed right before and after the 2015 elections and we observe the working status of each respondens in both moments. On the other hand, as both surveys were conduceted in December 2015, we do not observe the variation of the regoinal unemployment rate between the two surveys. Hence, as a measure of context, we employ the unemployment rate, rather than its change. In addition, little variation of the unemployment rate between the two periods and the fact that we only observe the province for each respondent leads to the discussed correlation between the unemployment rate the respondents lives in and the product of the rate and the individual unemployment shock. This prevents us from being able to include the interaction between both variables into one model.[17] As a consequence, we estimate the following model:

$$\Delta Abstention_i = \beta_1 \Delta Unemployed_i + UnemploymentRate_{i=0} + e_{it}, \tag{2.3}$$

where the dependent variable $Abstention_i = Abstention_{it=0+} - Abstention_{it=0-}$, the two binary variables defined in Section 2.3.2. Hence,

- $\Delta Abstention_i = -1$ for respondents who did not intend to vote initially (in $t = 0-$) but ended up voting (in $t = 0+$),
- $\Delta Abstention_i = 1$ for those who intended to vote but did not,

[17]Each of our main models shows a VIF>14 if this interaction term is included

- $\Delta Abstention_i = 0$ for the subjects who did not change their decision to vote.

Since this dependent variable takes three possible ordered values, we employ the ordered-logit model to estimate model (2.3).

As for the regressors, $\Delta Unemployed_i = Unemployed_{it=0+} - Unemploymed_{it=0-}$, being $Unemployed$ a dummy indicating whether individual i was unemployed before and after the 2015 elections, respectively, and $UnemploymentRate_i$ controls for the unemployment rate in the i's province of residence.

We estimate several variants of model in Tables 2.11 and 2.12, depending on the sample and depending on how we control for the context. In both tables, we first estimate the effects of job loss (column (1)), restricting the analysis to subjects employed in the pre-electoral survey. Under such specification, the control group corresponds to respondents employed in both the 2015 pre- and post-electoral surveys. Second, we focuse on those unemployed in the pre-electoral survey (column (2)), assessing the effect of finding a job as opposed to those who remained unemployed. Third, we focus on the labor force, regressing the dummies for job loss as well as finding a job on the change of abstention (column (3)). Last, we repeat the same model for the whole sample (column (4)). The difference between Tables 2.11 and 2.12 is the way how we control for the context. Table 2.11 reports the estimates of model (2.3), in which we simply control for the province unemployment rate in December 2015, but in which we do not control for properly for how the context mediates the impact of the shocks. Hence, Table 2.12 estimates two models separately for provinces with an above-median and below-median unemployment rates.

Both tables corroborate each other in that the individual effects are restricted to those who find a job between the two surveys.[18] People who get employed are more likely to belong to a higher category of the change of our abstention variable. In addition, the contrast between columns (3) and (4) in both tables suggest that virtually all the impact is driven by the people in the labor force. Last, Table 2.12 confirm the role of context: the individual-level effect only survives under high unemployment. That is, people who find a job are less likely to show up at the polls (compared to their intentions) but only if the unemployment is high. Quantitatively speaking, people who get employed between the two surveys are roughly $e^{0.55} = 1.73$ times more likely not to change their decision (as compared to changing the decision from no intention to vote to voting) and change their turnout decision toward non-participation (as opposed to changing their decision) in high-unemployment provinces (columns (3) and (4) in Table 2.12, top). We do not find any effect of losing a job on turnout.

[18]The only exception is column (2) in Table 2.12, top, where the estimator is also positive but non-significant. We attribute the loss of significance to the loss of statistical power caused by the

VARIABLES	(1) Job Loss	(2) Get Employed	(3) LF	(4) Whole Sample
$GetUnemployed_i$	0.191		0.119	0.103
	(0.254)		(0.241)	(0.243)
$UnemploymentRate_{it=0}$	-0.192	2.920	1.283	1.599
	(2.028)	(2.547)	(1.571)	(1.268)
$GetEmployed_i$		0.364*	0.529***	0.526***
		(0.207)	(0.204)	(0.204)
Observations	2,588	1,222	3,810	6,185
Pseudo R-squared	0.0003	0.0035	0.0022	0.0016
Prob > chi2	0.755	0.114	0.060	0.040
LR chi2(2)	0.56	4.34	7.39	8.30

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.11: Ordered Logit Regressions on ΔAbstention. Estimated coefficients. Standard errors. Model (1) includes only respondents who were employed at the time of the pre-electoral survey. Model (2) includes only respondents who were unemployed at the time of the pre-electoral survey. Model (3) includes every respondent who was in the labor force at the time of the pre-electoral survey. Model (4) includes the whole sample.

(A) Provinces with an unemployment rate above the median

	(1) Job Loss	(2) Get Employed	(3) LF	(4) Whole Sample
$GetUnemployed_i$	0.369		0.221	0.204
	(0.343)		(0.323)	(0.321)
$GetEmployed_i$		0.325	0.574**	0.561**
		(0.270)	(0.265)	(0.261)
Observations	1,155	694	1,849	2,919
Pseudo R-squared	0.0012	0.0020	0.0028	0.0018
Prob > chi2	0.287	0.232	0.092	0.097
LR chi2(2)	1.13	1.43	4.77	4.67

(B) Provinces with an unemployment rate below the median

	(1) Job Loss	(2) Get Employed	(3) LF	(4) Whole Sample
$GetUnemployed_i$	0.00919		0.0110	-0.00565
	(0.378)		(0.361)	(0.369)
$GetEmployed_i$		0.413	0.487	0.498
		(0.323)	(0.320)	(0.323)
Observations	1,433	528	1,961	3,266
Pseudo R-squared	0.0000	0.0030	0.0013	0.0008
Prob > chi2	0.981	0.206	0.334	0.326
LR chi2(2)	0.00	1.60	2.20	2.24

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.12: Ordered Logit Regressions on ΔAbstention. Respondents in Provinces with Unemployment Rate Above and Bellow the Median. Estimated coefficients. Standard errors in parentheses. Model (1) includes only respondents who were employed at the time of the pre-electoral survey. Model (2) includes only respondents who were unemployed at the time of the pre-electoral survey. Model (3) includes every respondent who was in the labor force at the time of the pre-electoral survey. Model (4) includes the whole sample.

As a robustness check, we run several alternative specifications redefining our abstention indicator to account only for *voluntary* abstention.[19] Tables 2.5 and 2.B.2 in Appendix B summarize the results. These models reinforce the results reported in the main text.

2016 post-electoral survey. The previous analysis does not allow to assess how the change of individual participation relates with the *changes* of the aggregate unemployment. To fill this gap, we exploit the 2016 post-electoral survey that elicits information about interviewees' voting behavior in both elections, their employment status at the time the survey was administered, and the time of unemployment for the unemployed individuals. As explained in Section 2.3.2, the latter piece of information allows us to include a binary variable $GetUnemployed_i$, indicating those who were employed during the 2015 elections but unemployed during the 2016 elections. Nevertheless, we cannot identify people who found a job between the two elections with the available data. We complement the survey data with the province-level unemployment statistics in both December 2015 and June 2016.

Again, the ideal specification is the natural extension of model (2.1), but, due to the data limitation described above, we estimate the following variant:

$$\Delta Abstention_i = \beta_1 GetUnemployed_i + \beta_2 \Delta UnemploymentRate_i + e_{it}. \qquad (2.4)$$

Note first that, rather than the classic diff-and-diff approach, the variable $GetUnemployed_i$ and model (2.4) only allows to perform an alternative specification. In the classic diff-and-diff approach, we measure the effects of a treatment (job loss, in our case) by accounting for difference in the evolution of outcomes between two different groups of subjects; one in which these receive the treatment in t=1 and other in which they do not, both of them being untreated in t=0. This approach was taken in the analysis of the 2015 pre- and post-electoral survey. In this case, we have a sample in which every subjects is treated in t=1, but within these, some, we identify some that were already treated in t=0 and some that were untreated. We measure treatment effects by comparing the evolution of these two groups in an approach somewhat similar to diff-in-diff in reverse analysis (Kim & Lee, 2019). Second, we cannot again include the interaction between the two regressors in (2.4) due to the multicolineality. Again, we separate the regressions for the provinces with high and low unemployment.

sample size.

[19] In the analysis shown in the main text the variable $Abstention_{it=0+}$ takes value 0 only for subjects who declared that they voted and 1 otherwise. Thus, we count as abstention the behavior of every subject who did not vote. In Appendix B, we modify the dependent variable $Abstention_{it=0+}$ to take value 0 in cases in which the subject refrained from voting for motives other than his will to abstain. See Appendix B for more details

Table 2.13 presents the estimate of an ordered-logit regression of model (2.4) for the whole sample (column (1)) and separated for provinces with an above- and below-median unemployment rates (columns (2) and (3), respectively). Irrespective of the specification, loosing a job does not predict the change of abstention, a finding in line with the estimates in Tables 2.11 and 2.12. In contrast, the fluctuations in the province unemployment rates affect significantly the individual voting decisions but the effect is again restricted to provinces with above-median unemployment rates where higher unemployment enhances turnout (or decreases abstention). This corroborates the findings from the macro- and individual-level analysis above.

VARIABLES	(1) All	(2) Above	(3) Bellow
$GetUnemployed_i$	0.123	0.147	0.128
	(0.242)	(0.318)	(0.369)
$\Delta UnemploymentRate_i$	-6.502**	-7.323**	-5.198
	(3.011)	(3.357)	(6.595)
Observations	5,264	2,286	2,978
Pseudo R-squared	0.0010	0.0021	0.0003
Prob > chi2	0.101	0.102	0.698
LR chi2(2)	4.58	4.56	0.72

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.13: Ordered Logit Regressions on ΔAbstention. Whole Sample. Estimated coefficients. Standard errors in parentheses. Model (1) includes every responder in the sample, (2) only responders living in provinces with an unemployment rate above the median, (3) only responders living in provinces with an unemployment rate bellow the median

The information in the survey data additionally allows us to test whether different segments of the population react to the individual vs. province-level economic shocks differently. Our hypothesis posits that the more involved people are in the labour market the more important the individual shocks will be, while the impact of the aggregate shocks should matter relatively more for people with no stakes in the labor market. These impacts should additionally be influenced the overall unemployment context.

To that aim, we reestimate the models from Table 2.13 for subjects in the labor force and those out of the labor force. First, Table 2.14 focuses on the labor force. Due to the data limitations, we can only estimate the impact of job loss and the change of the aggregate unemployment. Panels (A), (B) and (C) in Table 2.14 indicate that people who become unemployed between the two elections do not change their turnout decision, compared to other unemployed individuals, and the three panels show that the labor-force participants do not change their decision as the province unemployment fluctuations. All these observations corroborate all the previous results; the latter result is in line with our hypothesis that posits that people involved in the labour force would be relatively more influenced by their individual unemployment shocks. In our data, the relevant impact does not seem to be the case of loosing a job but rather due to the disengagement of people who get employed in high-unemployment areas (in Tables 2.11 and 2.12).

(A) **Respondents in the LF in t=1 (Pooled)**

VARIABLES	(1) all	(2) above	(3) bellow
$GetUnemployed_i$	0.0740	0.0933	0.0741
	(0.235)	(0.309)	(0.358)
$\Delta UnemploymentRate_i$	-3.497	-5.315	1.790
	(3.729)	(4.100)	(7.920)
Observations	3,293	1,438	1,855
Pseudo R-squared	0.0003	0.0012	0.0001
Prob > chi2	0.627	0.436	0.952
LR chi2(2)	0.93	1.66	0.10

(B) **Respondents unemployed in t=1**

	(1) all	(2) above	(3) bellow
$GetUnemployed_i$	-0.0543	-0.171	0.110
	(0.242)	(0.318)	(0.373)
$\Delta UnemploymentRate_i$	-8.068	-4.243	-13.13
	(10.49)	(13.99)	(16.31)
Observations	888	481	407
Pseudo R-squared	0.0007	0.0007	0.0017
Prob > chi2	0.722	0.822	0.707
LR chi2(2)	0.65	0.39	0.69

(C) **Respondents employed in t=1**

	(1) all	(2) above	(3) bellow
$\Delta UnemploymentRate_i$	-3.200	-6.744	6.410
	(4.075)	(4.403)	(9.050)
Observations	2,405	957	1,448
Pseudo R-squared	0.0003	0.0024	0.0004
Prob > chi2	0.438	0.138	0.479
LR chi2(2)	0.60	2.20	0.50

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.14: Ordered Logit Regressions on ΔAbstention. Respondents in the Labor Force. Estimated coefficients. Standard errors in parentheses. In every panel Model (1) includes every responder in the indicated subsample, (2) only those living in provinces with an unemployment rate above the median, (3) only those living in provinces with an unemployment rate bellow the median.

Table 2.15 estimates the same models for those not in the labor force in aggregate (top panel (A)) and then separating these people into three groups, students, housekeepers, and pensioners (panels (B) - (D)). Naturally, these people do not participate in the labor force. As a result, we can only ask how their voting behavior reacts to aggregate unemployment fluctuations. Since the income of most respondents labeled as pensioners does not depend on work and they have little prospect of incorporating themselves in to the labor market in the near future, they represent the best category of the people with no stakes in the labor market. We thus focus on them below. As students at voting age are likely to join the labour force in near future and housekeepers' households typically depend financially on members of labor force, they are likely to a certain extent care about both individual and labor market shocks and thus have more complex trade-offs between pocketbook and sociotropic motivations.

The estimated results in Table 2.15 support our hypothesis and expectations: people not in the labor force are more likely to change the category of our abstention variable downwards. Informally speaking, they are more likely to change their decision from abstention to turnout. The effect is again driven by high-unemployment areas and, as hypothesized, by pensioners. Though the effect is weakly significant even for responders in low unemployment areas when studying all subjects our of the labor force together, this effect is substantially weaker than that of subjects in high-unemployment areas. When running separate analysis for each group out of the labor force, we find that the effect is only signifficant for pensioners in high unemployment areas. (Panel (A)) in low The estimates are never significant for students and housekeeper, who–as mentioned above–might have more complex motives.

(A) Out of the LF in t=1 (Pooled)	(1)	(2)	(3)
	all	above	bellow
$\Delta UnemploymentRate_i$	-12.69**	-11.67**	-20.93*
	(4.959)	(5.726)	(11.59)
Observations	1,971	848	1,123
Pseudo R-squared	0.0037	0.0051	0.0037
Prob > chi2	0.017	0.058	0.075
LR chi2(2)	5.68	3.60	3.17

(B) Students in t=1	(1)	(2)	(3)
	all	above	bellow
$\Delta UnemploymentRate_i$	-11.47	-8.588	-37.76
	(8.628)	(9.439)	(23.97)
Observations	247	111	136
Pseudo R-squared	0.0050	0.0048	0.0144
Prob > chi2	0.200	0.373	0.127
LR chi2(2)	1.64	0.79	2.33

(C) Housekeeper in t=1	(1)	(2)	(3)
	all	above	bellow
$\Delta UnemploymentRate_i$	-7.927	-1.578	-24.07
	(14.15)	(18.38)	(26.14)
Observations	393	198	195
Pseudo R-squared	0.0010	0.0000	0.0056
Prob > chi2	0.587	0.932	0.363
LR chi2(2)	0.30	0.01	0.83

(D) Pensioners in t=1	(1)	(2)	(3)
	all	above	bellow
$\Delta UnemploymentRate_i$	-13.33**	-14.83**	-12.80
	(6.623)	(7.479)	(15.42)
Observations	1,331	539	792
Pseudo R-squared	0.0039	0.0091	0.0013
Prob > chi2	0.064	0.074	0.409
LR chi2(2)	3.42	3.18	0.68

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 2.15: Ordered Logit Regressions on ΔAbstention. Respondents out of the Labor Force. Estimated coefficients. Standard errors in parentheses. In every panel Model (1) includes every responder in the indicated subsample, (2) only those living in provinces with an unemployment rate above the median, (3) only those living in provinces with an unemployment rate bellow the median.

2.5 Discussion

Voter participation is a capital issue within the discipline of political science. However, the literature studying the effects of unemployment in turnout is inconclusive.

In this study, we build on Incantalupo's (2011) *Unemployment-In Context* hypothesis that assumes a context-dependent relationship between unemployment in turnout such that, under high unemployment, unemployment mobilizes voters, while it decreases participation in low-unemployment contexts. We take advantage of the unusual repetition of Spanish General Elections in 2015 and 2016, which generated a political cycle of less than seven months, to study the effect of short-term unemployment fluctuations in turnout from both macro- and individual-level perspective.

We find that the relationship between mobilization and unemployment turns out to be more complex. As in Incantalupo (2011), increases in unemployment lead to an increase in turnout only in regions with a high initial level of unemployment, but the association reverses in regions with low unemployment. Once we look at the mechanisms behind this results though, we uncover that the aggregate effect emerges from a combination of the contextual variables and different individual voting motives across different population strata. Pensioners' voting decisions are determined by the macroeconomic conditions captured by aggregate level unemployment rates such increases in unemployment rate in a region whose initial unemployment level was already high increases the propensity to turn out of these voters. In contrast, people involved in the labor force are largely insensitive to unemployment fluctuations at the regional level, but their voting participation seems to be determined by their individual unemployment conditions.

These results provide an additional step toward the understanding of the determinants of political participation and toward resolving the conflicting evidence regarding how economic conditions shape turnout.

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Appendix A: Unemployment Ratio

In our main macro-level analysis we have studied the effects of unemployment in context on turnout via a specification akin to the one used in Incantalupo (2011). We study how the effects of fluctuations in unemployment on turnout differ depending on whether this fluctuations are produced in a territory within a region with a high or low initial level of unemployment. In this section we will re-analyze or data using a different the specification proposed by Cebula (2019).

We study the effects of relative unemployment on turnout, where relative unemployment is defined as the ratio resulting from dividing the state local unemployment rate by the national unemployment ratio. This ratio should reflect the relative well-being of local job markets respect to the national trend rather than the absolute well-being. That is, we would be measuring to what extent a particular territory is better-off or worse-off in terms of unemployment respect to the average situation of the country.

This idea is similar to the one that has motivated our analysis, so we consider this a fruitful alternative specification to provide further test of our hypothesis.

For a particular territory i at time $t = [0, 1]$ we define Unemployment Ratio as the division of the unemployment rate of i at time t by the national unemployment rate at time t:

$$UnemploymentRatio_{it} = Unemployment_{it}/NationalUnemployment_t \tag{5}$$

This implies that $UnemploymentRatio_{it} > 1$ for any i with an Unemployment rate above the national unemployment rate and <1 for any with i with an Unemployment rate bellow at time t the national unemployment rate.

We replicate the models used in our main aggregate level analysis (Tables 2.8-2.10 with this specification. Results are shown in Tables 2.A.1-2.A.3. First we measure the direct and unmediated relationship between unemployment and turnout through the following model:

$$\Delta Turnout_i = \alpha_i + \beta * \Delta UnemploymentRatio_i + e_{it} \tag{6}$$

where:

$$\Delta Turnout_i = Turnout_{it=1} - Turnout_{it=0} \tag{7}$$

$$\Delta Unemployment_i = UnemploymentRatio_{it=1} - UnemploymentRatio_{it=0} \quad (8)$$

We run the model for our three aggregation levels (Table 2.A.1). Though the coefficient fails to reach significance at the Muncipality level it is significant at 5% level for the other two levels and, as expected, has a positive sign; reflecting that increases in relative unemployment lead to an increase in turnout.

Next we study unemployment in context. WE regress a model using our original formulation (i.e. using differences in unemployment rate instead of differences in unemployment ratio as independent variable), but this time we divide Muncipalities in two groups according to whether they are in Comarcas with an unemployment ratio bigger or smaller than one (i.e. wether they have an unemployment rate bigger or smaller than the national average). We also conduct an analysis in the Comarcas level dividing them in two groups according to whether they are in a province with an unemployment ratio above or bellow 1. Results are shown in Table 2.A.2 As expected, we found that the effect of unemployment fluctuations is positively and significantly related to turnout in high unemployment regions, while in low unemployment regions the coefficient is negative fails to reach significance.

Finally we run a model with an interaction term similar to the one used in the original analysis, with the difference that we use the unemployment ratio of the region instead of the unemployment rate to account for contextual conditions:

$$\begin{aligned} \Delta Turnout_i = \alpha_i + \beta_1 * \Delta Unemployment_i + \\ + \beta 2 * \Delta Unemployment_i * UnemploymentRatioContext_{t=0+} e_{it} \end{aligned} \quad (9)$$

Results are show in Table 2.A.3. We obtain similar result to those found in our original analysis (Table 2.10), finding that increases in unemployment have *per se* a negative effect in turnout but tend to increase turnout when the context is of high unemployment.

Cebula (2019) includes an alternative specification of their relative unemployment measure consisting in subtraction of the national unemployment rate from local unemployment rates:

$$RelativeUnemployment_{it} = Unemployment_{it} - NationalUnemployment_t \quad (10)$$

VARIABLES	(1) Municipalities	(2) Comarcas	(3) Provinces
ΔUnemployment Ratio	-0.000792	0.0118**	0.0274**
	(0.00221)	(0.00598)	(0.0104)
Constant	-0.0151***	-0.0267***	-0.0303***
	(0.000521)	(0.000847)	(0.00137)
Observations	8,113	404	52
R-squared	0.000	0.010	0.121

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.A.1: OLS Regressions on ΔTurnout. Estimated coefficients. Standard errors in parentheses

VARIABLES	(1) Mun. in Com.	(2) Mun. in Com.	(3) Com. in Prov.	(4) Com. in Prov.
	Above	Bellow	Above	Bellow
ΔUnemployment	0.0680***	-0.0239	0.228***	-0.145
	(0.0218)	(0.0243)	(0.0520)	(0.0931)
Constant	-0.0186***	-0.0127***	-0.0225***	-0.0294***
	(0.000829)	(0.000754)	(0.00149)	(0.00165)
Observations	2,875	5,238	176	228
R-squared	0.003	0.000	0.099	0.011

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.A.2: OLS Regressions on ΔTurnout. Estimated coefficients. Standard errors in parentheses.

VARIABLES	(1) Municipalities	(2) Comarcas
ΔUnemployment	-0.318***	-0.486**
	(0.0533)	(0.197)
ΔUnemployment$_i$* Unemployment Ratio Comarca$_{t=0}$	0.350***	
	(0.0510)	
ΔUnemployment$_i$* Unemployment Ratio Province$_{t=0}$		0.482***
		(0.152)
Constant	-0.0145***	-0.0266***
	(0.000573)	(0.00116)
Observations	8,113	404
R-squared	0.006	0.040

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.A.3: OLS Regressions on ΔTurnout. Estimated coefficients. Standard errors in parentheses.

This specification reflects deviations from national unemployment rate in a linear way, such that $\mathrm{Re}\, lativeUnemployment_{it} > 0$ for any i with an Unemployment rate above the national unemployment rate and <1 for any with i with an Unemployment rate bellow at time t the national unemployment rate.

We run our models with this specifications. Results are shown in Tables 2.A.4 and 2.A.5 . In Table2.A.4 we run a model equivalent to the one shown in Table 2.A.1 using this new measure of relative unemployment instead of the Unemployment Ratio. Results for this specification are similar. We do not run a new version of the model in Table 2.A.2. In this model we divided the sample in two sub-samples according to whether the Unemployment Ratio of the region was above or bellow 1. We could divide the sample according to whether the our Relative Unemployment measure is above or bellow 0, but we would obtained identical results, as the division would be the same (any territory with an unemployment rate above the national unemployment rate would be above an any region with an unemployment rate bellow the national rate would be bellow).

Table 2.A.5 shows the results for a model with an interaction term analogous to that in Table . Results for this model are somewhat similar to those found in previous specifications of the model.Though the pure effect of changes in Unemployment (β_1) is non-significant in model (2) and has only a weak level of significance in model (1), were has an unexpected sign; the effect of the interaction term goes in line with what we have seen in previous anlyses.

VARIABLES	(1) Municipalities	(2) Comarcas	(3) Provinces
ΔRelative Unemployment	0.0286*	0.121**	0.218**
	(0.0170)	(0.0470)	(0.0850)
Constant	-0.0150***	-0.0267***	-0.0302***
	(0.000514)	(0.000839)	(0.00138)
Observations	8,113	404	52
R-squared	0.000	0.016	0.116

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.A.4: OLS Regressions on ΔTurnout. Estimated coefficients. Standard errors in parentheses.

VARIABLES	(1) partratediff	(2) partratediff
ΔUnemployment	0.0320*	-0.00400
	(0.0170)	(0.0610)
ΔUnemployment$_i$*Relative Unemployment Comarca$_{t=0}$	2.599***	
	(0.379)	
ΔUnemployment$_i$*Relative Unemployment Province$_{t=0}$		3.580***
		(1.130)
Constant	-0.0145***	-0.0266***
	(0.000573)	(0.00116)
Observations	8,113	404
R-squared	0.006	0.040

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.A.5: OLS Regressions on ΔTurnout. Estimated coefficients. Standard errors in parentheses.

Apendix B: Voluntary Abstention

One of the most plausible causal mechanism mediating the relationship between changes in unemployment and changes in turnout is a motivational shift, such that changes in employment situation can increase or decrease the will to participate in politics. With this in mind, we think it could be fruitful to analyze exclusively shifts from voting to abstention that are voluntary, not considering abstentions the case of voters that abstained due to reasons external to voters' will. For this reason, in this section we conduct a supplementary analysis in which we repeat the specification employed in Tables 2.11 and 2.12 with the only exception that we exclude from the category of non-voter those respondents that declared that wanted to vote but could not in the post-electoral survey from the category of non-voters. This way, voters who incurred in involuntary abstention take value 0 and only those who deliberately abstained from voting are classified as non-voters.

In our main analysis of the 2015 pre- and post-electoral surveys (see section 2.4.2) we coded the intention to vote stated in the pre-electoral survey (variable $Abstention_{it=0-}$) and the memory of vote stated in the post-electoral survey (variable $Abstention_{it=0+}$) as a binary variable that take value 1 for non-voters and value 0 for voters. This allowed us to analyze changes in the probability of turnout as a function of changes in personal employment status. We gave value 1 to every respondent that declared that she did not vote in the post-electoral survey. We built the variable $Abstention_{it=0+}$ upon the answers to a question in the post-electoral survey which elicited subjects' memory of vote. This question had five possible answers: 1. "Could not vote", 2. "Did not want to vote", 3. "Normally he/she votes, but this time he/she did not want to", 4. "Normally he/she votes, but this time he/she did not could not", 5. "Voted". In the analysis in the main text the variable $Abstention_{it=0+}$ takes value 1 for each subject giving any answer different from 5. Hence, behavior of subjects that wanted to vote but could not do it (answers 1 and 4) is considered an abstention. In this section, we repeat this analysis with the difference that the variable takes value 0 for subjects who provided answer 1, 4 or 5, and value 1 only for subjects giving answers 2 or 3. This way in the present analysis only the behavior subjects who did not want to vote is considered an abstention, while involuntary abstention is not.

As it can be seen in Tables 2.B.1-2.B.2, results under this new specification are similar to those found in our original specification (Tables 2.11 and 2.12).

VARIABLES	(1) Job Loss	(2) Get Employed	(3) LF	(4) Whole Sample
$GetUnemployed_i$	0.0528		0.0558	0.0895
	(0.287)		(0.272)	(0.274)
$UnemploymentRate_{it=0}$	-0.359	3.080	0.858	0.817
	(2.255)	(2.897)	(1.766)	(1.420)
$GetEmployed_i$		0.404*	0.439*	0.487**
		(0.240)	(0.241)	(0.242)
Observations	2,588	1,222	3,810	6,185
Pseudo R-squared	0.0000	0.0038	0.0013	0.0010
Prob > chi2	0.972	0.138	0.307	0.213
LR chi2(2)	0.06	3.96	3.61	4.49

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.B.1: Ordered Logit Regressions on ΔAbstention. Whole Sample. Estimated coefficients. Standard errors in parentheses.

(A) Provinces with an unemployment rate above the median

VARIABLES	(1) Job Loss	(2) Get Employed	(3) LF	(4) Whole Sample
$GetUnemployed_i$	0.340		0.310	0.334
	(0.385)		(0.363)	(0.365)
$GetEmployed_i$		0.538*	0.615**	0.649**
		(0.311)	(0.306)	(0.305)
Observations	1,155	694	1,849	2,919
Pseudo R-squared	0.0009	0.0050	0.0031	0.0023
Prob > chi2	0.376	0.084	0.107	0.079
LR chi2(2)	0.78	2.99	4.47	5.07
	(1)	(2)	(3)	(4)

(B) Provinces with an unemployment rate below the median

VARIABLES	(1) Job Loss	(2) Get Employed	(3) LF	(4) Whole Sample
$GetUnemployed_i$	-0.281		-0.243	-0.196
	(0.421)		(0.403)	(0.405)
$GetEmployed_i$		0.205	0.198	0.259
		(0.379)	(0.386)	(0.388)
Observations	1,433	528	1,961	3,266
Pseudo R-squared	0.0005	0.0007	0.0005	0.0003
Prob > chi2	0.508	0.588	0.723	0.708
LR chi2(2)	0.44	0.29	0.65	0.69

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.B.2: Ordered Logit Regressions on ΔAbstention. Respondents in Provinces with Unemployment Rate Above and Bellow the Median. Estimated coefficients. Standard errors in parentheses.

Chapter 3

Cognitive Styles and Cooperation: Intuition vs Impulsivity

3.1 Introduction

Cooperation is an essential human behavior that is crucial for a wide range of social domains. Due to its complexity and the importance of its consequences, scholars across many fields have devoted a large amount of attention to the study of its nature and determinants (Axelrod & Hamilton,1981; Fehr & Fischbacher, 2003; Rand & Nowak, 2013).

One of the issues that has been widely studied is the relationship between intuition and cooperation. The leading theory addressing this issue is the Social Heuristic Hypothesis (SHH, hereafter; Rand et al., 2012). According to this theory, in human societies, cooperation ends up leading to positive outcomes most of the times. As a result, people internalize cooperation as a default strategy. In situations in which cooperation is a suboptimal response, subjects acting on intuition are more likely to display a cooperative response. Deliberation, in turn, allows individuals to override this cooperative heuristic.

Scholars across many fields have tried to settle the question of the validity of the SHH through meta-scientific studies, which have produced mixed results, with two meta-analyses supporting the SHH (Rand, 2016; Rand, 2017) and one showing evidence against it (Kvarven et al., 2020).[1] These meta-analyses have been done by

[1]These meta-analyses include experiments employing four kind of games: Public Good Game, Prisoner´s Dilemma, Ultimatum Game and Trust Game. Similarly, we find two meta-anlysis of Dictator Game experiments, considered to measure altruism rather than cooperation. One of them finds a positive relationship between intuition and altruism (Rand et al. 2016), while the other

pooling together a large amount of experimental studies employing manipulations of diverse nature whose effects have been indistinctly labeled as intuition.

We identify two issues within this literature. First, we argue that certain manipulations, considered to promote intuition, in fact induce an impulsive cognitive style, that is substantially different and might have different behavioral consequences. Second, as documented by Rand et al. (2016), the effects of intuition on cooperation might be different for men and women. We hypothesize that the mixed results found in this literature may be partially explained by insufficient attention devoted to the differences between cognitive-style manipulations and its differing effects across genders.

Impulsivity is defined as "*a predisposition toward rapid, unplanned reactions to internal or external stimuli with diminished regard to the negative consequences of these reactions to the impulsive individual or others*" (Brewer & Potenza, 2008, p. 65). We can think of impulsivity as a state of mind which makes individuals unable to override urges and impulses. An impulsive decision maker will tend to "yield to temptation" and make decisions producing the largest immediate gratification even when this comes at the expense of better long-term outcomes. Impulsivity is tightly linked to self-control, defined as "*the ability to override or change one's inner responses, as well as to interrupt undesired behavioral tendencies and refrain from acting on them*" (Tagney et al., 2004, p. 274), and which can be understood as a self-regulatory resource having an antagonistic role respect to impulsivity. The more self-control an individual possesses, the less likely she behaves impulsively (see De Ridde et al., 2012 for a meta-analysis on the relationship between self-control an a wide range of impulsive behaviors).

Intuition, in contrast, is a decision making approach characterized by the reliance on fast and few cognitive resource consuming decision making mechanisms. Usually labeled as System 1 thinking, is described as a type of reasoning "*characterized as automatic, largely unconscious, and relatively undemanding of computational capacity*" (Stanovich & West, 2000, p.658; see also, Kahneman, 2011). Both impulsive and intuitive agents make decisions quickly and with little thinking. Intuitive decision makers will not engage in complex, time and cognitive resource consuming mechanism to achieve their goals, but rather use simple and fast decision making mechanism such as heuristics and rules of thumb. However, unlike impulsive decision makers, intuitive decision makers can consider long term interest and complex goals when making their decisions. In fact, there exist studies which show how intuitive decisions give favorable outcomes, sometimes even outperforming decisions taken through deliberation in complex and long-term goal oriented tasks such as

finds a null result (Fromell et al. 2020) managerial decisions (Agor, 1986; Burke & Miller, 1999; Khatri & Ng, 2000), investments decisions (Huang & Pearce, 2015) and chess playing (Chase & Simon, 1973a, 1973b). In a similar way, SHH suggest that the positive relationship between intuition and cooperation is the result of a cooperative heuristic internalized because of its tendency to produce positive long-term outcomes.

The aforementioned differences between impulsivity and intuition might be particularly relevant effect in the context of cooperation. While SHH posits that subjects on intuition display a default cooperative response, impulsive subjects would be unable to override selfish impulses, leading to an increase in proself or even antisocial behavior. In fact, low self-control and impulsivity have shown to be correlated with a wide range of antisocial behaviors, such as starting fights, shoplifting and perpetrating domestic violence (Chamorro et al., 2012; Eysenck, 1981; Pratt & Cullen, 2000; Wright, Caspi et al., 1999) and is at the core of personality disorders, such as antisocial personality disorder (Swann et al., 2009) and psychopathy (Cleckley, 1951; Hare, 2003). For these reasons, we argue that impulsivity, unlike intuition, should decrease the extent of cooperation.

Another relevant difference is the mediation of gender in the effect of intuition on cooperation. SHH posits that, when adopting an intuitive cognitive style, individuals display a default behavior learned through social interactions. However, social roles are different for men and woman, leading to different ways of socialization and gender specific social norms, which could, in turn, lead to the acquisition of different default responses. While femininity is commonly associated with prosocial behaviors, such as caring and nurturing, masculinity is associated with more proself or even antisocial behaviors such as aggression and competition (Frieze & Li, 2010). Empirical research indeed shows that men are more inclined than women to behave aggresively (Archer, 2004) and competitively (Niederle & Vesterlund, 2007). In the context of cooperation, Rand et al. (2016) already showed results pointing in this direction. Through a re-analysis of 22 Dictator Game (DG) studies, they found that, while manipulations inducing intuitive behavior have no effect on males' average giving , they make female participants more generous.[2] Espinosa & Kovářík (2015) showed, in a re-analysis of 6 different experimental studies (including Rand et al., 2012), that encouraging reflection decreases the prosociality of males but not females. Additionally, Croson & Gneezy (2009) analyzed gender differences in several economic games

[2]It should be noted that Rand et al. (2016) only studies the effect of manipulations inducing intuition on average giving in the DG. No other possible effects on the distribution are studied. As it can be seen in Section 3.3, similarly to Rand et al. (2016), we do not find a significant effect of intuition in males when studying averages, but we do find that intuition significantly increases free riding in males.

and found that, while male behavior is rather stable, female subjects' behavior is more likely to be influenced by subtle cues in the experimental context leading them to increase their concern for certain prosocial motives such as inequality aversion, reciprocity and cooperation. Thus, the mediation of gender effects proves to be an essential feature to take in consideration. We consider that the internalization of cooperation as a default strategy hypothesized by SHH may well describe the behavior of women but not men, for whom intuition may even increase selfish behavior.

In sum, we hypothesize, first, that intuition and impulsivity should have different effects on cooperation and, second, that the effects of intuition should depend on the gender of the decision maker. We predict that impulsive decision makers will be unable to override their selfish impulses and choose the option that gives more immediate gratification, which corresponds to maximizing gains for themselves even when this comes at the expense of others. Intuitive decision makers, in turn, will display a behavior previously internalized as a default strategy, as predicted by SHH. For females, this default strategy will be cooperation, as predicted by the classic formulation of the SHH. Males' default strategy may be different and more influenced by proself motives.[3]

To test these predictions, the present study investigates the differing effect of impulsivity and intuition on cooperation in a one-shot four-player Public Good Game (PGG), by employing an impulsivity-inducing manipulation Ego Depletion (ED, hereafter) and an intuition-inducing manipulation Cognitive Load (CL, hereafter). We choose a one-shot PGG because is the most widely studied game within the literature of intuition and because it genuinely present subjects a conflict between self-interest and cooperation. In this game, players are assigned to a group and have to choose how much of their payoff to invest in a common project. Contributions to the common project benefit the group considered at large but come at a personal loss. The contribution of an individual is therefore considered a measure of her tendency to cooperate.

The literature typically investigates the effect of quick and non-deliberative thinking by assigning subjects exogenously to a condition that induces this cognitive style. The cognitive styles induced by these manipulations have been treated indistinctly as part of a common category labeled with names such as intuition, automatic behavior/responses (Cornelissen et al., 2011; Rand et al., 2015) or system 1 thinking (Liu Hao; 2011; Grolleau et al. 2018; Cappelletti et al. 2011; Neo et al. 2013;

[3]We have pre-registered the hypothesis regarding the differences between impulsivity and intuition, originally predicting that ED should lead to an increase in selfish behavior and CL to an increase in prosocial behavior. We have not pre-registered the gender hypothesis, though the pre-registration includes an exploratory analysis of gender effects.

Fromell et al. 2020). Little attention has been devoted to differences between the nature of the manipulations.

The most frequent manipulations are Time Pressure (TP), Recall Induction, Cognitive Load (CL) and Ego Depletion (ED). TP and CL force subjects to make decisions quickly and with little or no deliberation by limiting the resources (time and cognitive resources, respectively) needed for reflective thinking. Recall Induction uses conceptual priming to induce subjects to make decisions intuitively. All these manipulations have in common the fact that they induce subjects to make decisions through mechanisms that are fast and require little thinking, but none of them aims to produce changes in self-control or self-regulatory resources of any kind. Therefore, in line with the literature, we consider that these manipulations will have the effect of turning subjects into intuitive decision makers and lead subjects to display a default response, as described in the gender-specific SHH, exposed above.

Nevertheless, we argue that the case of ED stands aside from the rest of the manipulations because, unlike the others, it aims to reduce subjects' self-control and thus induce impulsivity. This way, subjects under ED make decisions quickly and with little or no deliberation as a consequence of a shortage of self-control which causes them to act on impulse (Muraven et al., 1998; Baumeister, 2002). This state of mind is different from intuition, and could have different behavioral consequences. Subjects depleted of self-control will be unable to override the impulse to make the decisions which produce the largest immediate gratification. This impulsive responses should not be influenced by social roles and, therefore, should not differ across genders. Hence, we hypothesize that ED will make subjects more selfish, regardless of their gender.

We choose ED and CL as our main manipulations because of its the great degree of structural similarity, which makes them highly comparable. Both manipulations consist in making subjects undergo a secondary task in addition to the PGG. CL consists in a secondary task that is performed simultaneously to the main game in order to distract the subjects and prevent them from making long and effortful deliberations in their decisions. ED consist in a secondary task that is performed before the main game in order to deplete subjects' self-control, so that they will tend to act on impulse on subsequent tasks. Therefore, both treatments have a similar structure, consisting in a main (the PGG) and a secondary task, with the difference that in ED the secondary and the main task are taken sequentially and in CL they are taken simultaneously. The key difference is that, while both tasks impair subjects' ability to make long and careful deliberations, ED reduces self-control while CL does not. Thus, ED makes subjects impulsive and CL makes them intuitive.

We are aware of a set of issues concerning ED. Recently, several meta-analysis and replication studies have cast doubt on the validity of ED (Hager et al. 2018, Carter et al.2015; Hager et al. 2016; Dang, 2018; Vohs et al. 2021). We therefore employ an adaptation of the Stroop task by Dang et al. (2017), which consistently depleted subjects of self-control in the multi lab registered replication in Dang et al (2021).

In line with our first hypothesis, we find that ED leads to an increase in free riding regardless of subject's gender, confirming that impulsivity increases selfishness. The effect of CL, in turn, depends upon subjects' gender: it increases free riding in males, while it makes females more cooperative. Hence, as hypothesized the effect of intuition is mediated by gender and intuitive subjects display a default behavior determined by their social roles.

3.2 Methods

3.2.1 Design and Participants

The study follows a between-subjects design with three treatments (ED, CL, Control). Subjects were randomly assigned to one of the three conditions.

A total number of 208 subjects[4] participated in the experiment, 82 in the ED treatment, 66 in the CL treatment and 60 in the control condition. 84 participants were male and 125 female. Participants' age ranged between 18 and 58, with an average of 21.84 (SD=4.88).

Participants were recruited via ORSEE (Greiner, 2004). The experiments were conducted via computer using z-Tree (Fischbacher, 2007) and pavlovia.org. The experimental sessions took place in the Laboratory of Experimental Analysis (Bilbao Labean; http://www.bilbaolabean.com) at the University of the Basque Country.

3.2.2 Procedure

Each participant was randomly assigned a computer in the lab. Each computer was placed in an isolated workstation so that privacy and anonymity of subjects' decisions was ensured. Participants where given written instructions and the experimenters read the instructions aloud. Each participant received a show-up fee of €3.

The structure of the experiment can be divided in four stages.

First, subjects were asked to introduce their gender and age in the computer.

[4]One subject from the Ego Depletion treatment was excluded from the sample because, due to a software error, he could not participate in the ED task.

Second, subjects in the CL treatment were asked to memorize a set of numbers and subjects in the ED treatment undertook a Stroop task. These task are described in detail in below. Subjects in the control group did not engage in any of these task and skipped directly to the next phase.

Third, independently of the treatment, subjects were given the instructions for the PGG and were asked to make their contributions. The game consisted in a one-shot four person PGG. Each subject was assigned an endowment of 100 ECUs (each ECU being equivalent to €0.05 ; therefore the endowment was €5) and randomly match with other three subjects. They were free to invest as much of their endowment as they wanted in a common project. Each ECU invested in the common project was doubled and split equally between the four members of the group. Note that each ECU invested in the common project gives a personal return of 1/2 ECUs. Therefore, investing in the common project comes at a personal cost. However, if every subject invest their whole endowment in the project, each of them would get double the amount they would get if none invested anything. Hence, subjects face a trade-off between self-interest and cooperation when choosing their contribution. Thus, the size of this allows us to measure cooperation.

Right after choosing their contribution, subjects in the CL treatment were asked to write down the sequence of numbers they had memorized after they made their contributions.

Finally, subjects were ask to fill a set of questionnaires, including Cognitive Reflection Test (CRT, hereafter; Frederick, 2005), Social Value Orientation (SVO, hereafter; Messick & McClintock, 1968; McClintock, 1972; Crosetto et al., 2012) 13-Iten Brief Self-Control Scale (Tangney et al., 2004), Rational Experiential Inventory (REI-10, hereafter;Epstein et al.,1996) which is formed by two subscales (Need For Cognition (NFC, herafter) and Faith in Intuition (FI, hereafter)), a socio-demogrpahic questionnaire, and a short questionnaire on feelings and attitudes regarding the Covid-19 pandemy.

Cognitive Load Task

The CL task was implemented with the objective of keeping subjects' mind distracted while making their choice in the PGG. To that purpose, we apply the task most frequently used in the literature, consisting in asking subjects to memorize a sequence of numbers to keep their working memory busy while performing the main task.

Subjects were shown one of the following sequence of number during 15 seconds; 3242163 or 1509842. These numbers were chosen from a set of randomly generated sequences for having neither two equal nor two consecutive adjacent digits (e.g. 01, 45). The software did not allow to use the copy-paste function on them. Each subject's number was different from the number of subjects sitting next to him, so that they could not get any help from other participants in the memorization task. It was forbidden for subjects to talk with other subjects during the experiment and to use their mobile phones or electronic devices other than the computer they were assigned in the lab. Subjects were not allowed to use pencils or pens. These measures were taken in order to ensure that the only way for subjects to recall the numbers successfully was to retain them in their memory until the time they were asked to write them down.

In order to ensure that the decisions in the PGG were taken under cognitive load, subjects were not given any information about the game until their numbers were shown. Instructions for the PGG were given only once subjects have been shown their numbers and asked to memorize them.

Once subjects had made their contributions in the PGG, they were asked to write their numbers into the computer. They got an extra payment of €3 if they recalled their numbers correctly. The payoff for the CL task was independent of the rest of the experiment in order to avoid a potential entitlement effect confound (Hoffman & Spitzer, 1985; Hoffman et al., 1994) that could interfere with subjects decisions in the PGG.

Ego Depletion Task

A large number of ego depletion tasks have been employed in the literature (for a list, see Carter et al., 2015; Dang, 2018). However, recently a series of meta-analysis and replication studies have cast doubts on the validity of most of them (Hagger et al., 2010, Carter et al., 2015; Hagger et al., 2016; Dang, 2018; Vohs et al., 2021). Therefore, we employ a Stroop task adapted from Dang et al. (2017), which proved to consistently reduce of subjects' self-control in the multi-lab registered replication of Dang et al. (2021).

In this task subjects face a series of trials. In each trial, subjects are displayed one of the following words: "BLUE", "RED", GREEN" or "YELLOW". Each word was written in either blue, red, green or yellow font. In some trials the color of the font coincides with the color designated by the word (e.g. "YELLOW" written in a yellow font). We label these trials "congruent". In others the font was of a different color (e.g. "BLUE" written in a red font. We label these trials "incongruent"). For a smooth performance in the task, a number on the keyboard was assigned to each of the four colors (1=red, 2=blue, 3=green, 4=yellow). When a word was shown subjects had to pronounce the color of the font (NOT the color designated by the word) and press the corresponding key. Next, a blank screen was shown during 500 ms. Then followed a fixation (5 stars in a white font in the place where target words appear) of 200 ms. After this, the next trial begins and the process is repeated.

Subjects participated in a total of 256 trials, of which 75% were incongruent and the rest congruent. The program registered both the answers introduced through the keyboard and the response times. In addition, subjects were asked to pronounce the answers orally. In order to prevent subjects from being disturbed by the noise created by other subjects, each subject was provided with a pair of earplugs they could used if they wished.

In order to ensure that subjects performed the ED task as best as they could, it was incentivized. Subjects got €0.02 for each correct answer, but got a penalization of €0.004 for each second taken to give their response, so that they received no payoff for a correct trial if they took five seconds or more to answer. No negative payoff could be received for any trial. Subjects did not get any payoff for trials in which an incorrect answer was given. Subjects could earn up to €5.12 in the task, which is the amount they would received if they would answer correctly the 256 trials with a response time of 0.00 seconds in each of them.

3.3 Results

In this section we present the experimental results. First we check whether our manipulations have the desired effect. In the following subsections we study the effects of the manipulations on cooperation. In Section 3.3.2 we analyze how manipulations affect contributions in the PGG. In Section 3.3.3 we test how manipulations affect the probability of displaying certain kinds of behavior typically observed in the PGG. Finally, Section 3.3.4 investigates gender effects.

3.3.1 Manipulation Check

Before presenting our analysis of behavior in the PGG, we briefly analyze the manipulation tasks and their effectiveness in achieving their goal of inducing an intuitive/impulsive cognitive style.

Out of the 66 subjects in the CL task 63 recalled their numbers successfully (95.45%).

As for the ED task, the average number of correct answers in the Stroop task was 243.72 out of 256 (95,2 %). The smaller number of correct answers in the sample is 204 (79,69 %) and the largest 256 (100%). 90% of the sample gave a percentage of correct answers above 91%. The average response time per trial for the whole sample

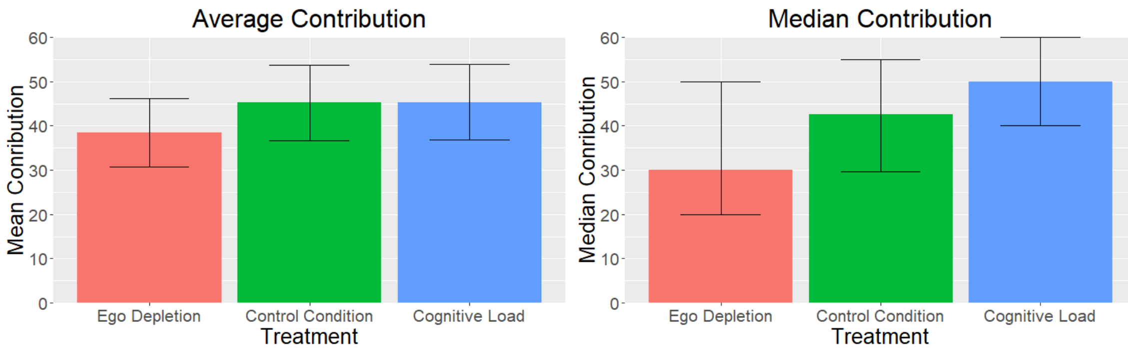

Figure 3.1: Average and Median Contribution in PGG. Error bars denote 95% confidence intervals

was of 0.87 seconds, with individual average response times per trial ranging from 0.57 to 1.62 seconds. Subjects won an average payoff of €4.02 in the task.

This indicates great involvement of subjects in both manipulation tasks; thus, ensuring that subjects effectively underwent the manipulations as intended by experimenters.

As expected, decisions in the PGG were faster in both treatments compared to the control group. This is in line with the purpose of inducing fast and non-deliberative decisions. The average response time for the contribution entry in the PGG was 23.68 (SD=40.91) in the control treatment, 19.01 (SD=33.31) in the ED treatment and 11.70 (SD=24.25) in the CL treatment. Non-parametric tests find significant differences between CL and control treatment, but not between ED and control treatment (Wilcoxon Ranksum: ED=Control: p=0.1384; CL=Control p=0.0024; ED=CL: p=0.2517).

3.3.2 Contribution

Treatment	N	Mean	SD	CI	Min	Max	Median	Mode
Ego Depletion	82	38.49	35.35	[30.72 ,46.25]	0	100	30	0
Control	60	45.2	33.06	[36.66,53.74]	0	100	42.5	100
Cognitive Load	66	45.35	34.73	[36.81, 53.89]	0	100	50	0

Table 3.1: Descriptives of Contribution

VARIABLES	(1) Contribution	(2) Contribution	(3) Contribution
Cognitive Load	0.148	1.270	2.583
	(6.155)	(6.021)	(6.057)
Ego Depletion	-6.712	-6.314	-5.459
	(5.862)	(5.724)	(5.753)
Age		1.443***	1.530***
		(0.486)	(0.484)
Female		9.452**	8.785*
		(4.777)	(4.971)
SC Score			0.317
			(0.354)
FI Score			-0.801*
			(0.471)
NFC Score			-0.517
			(0.481)
CRT Score			-2.976
			(2.922)
Constant	45.20***	7.565	30.70**
	(4.455)	(12.09)	(15.54)
Observations	208	208	208
R-squared	0.009	0.065	0.096
H0:CL=ED			
p	0.231	0.175	0.149

Estimated coefficients (Standard errors in parentheses)
*** p<0.01, ** p<0.05, * p<0.1

Table 3.2: Treatment effects on Contribution (OLS). p-values of a post-estimation test of equality of coefficients for Cognitive Load and Ego Depletion are shown at the bottom.

Table 3.1 summarize subjects contributions to the public good. Figure 3.1 plots average and median contributions. Contributions in the control condition is higher than in the ED treatment but lower than in the CL treatment (ED, M=38.02, SD=35.39; Control, M=45.2, SD=33.05; SD=35.39; CL, M=45.35, SD=34.73). However, the confidence intervals overlap and no significant difference are found neither in overall (p=0.3191, Kruskal-Wallis) nor in pairwise comparisons between treatments (Wilcoxon Ranksum: ED=Control p=0.1562; CL=Control p=0.8654, ED=CL p=0.2514).

In Table 3.2 we estimate treatment effects on contribution through an OLS model. In Model (1) we estimate treatment effects using a dummy for ED and CL respectively. Model (2) adds socio-demographic control and Model (3) adds personality traits as captured by the results of tests included in the post-experimental questionnaire (see Section 3.2.2). We find a positive effect of CL and a negative effect of ED as expected, but none of them is significant.

Since the sample size is small, we also report medians and k-sample tests, which are more adequate measure for our sample sizes. The treatment differences are larger using medians and the differences go in the expected directions (ED=30, Control=42.5, CL=50). Differences between CL and ED treatments are signifficant at α=10% (k-sample: ED=Control p=0.472, CL=Control p=0.768, ED=CL p=0.098).

Table 3.3 reports the effect sizes of our manipulations.[5].The effect size of ED is close to the average effect found in the literature.[6] We do not find big differences when considering males and females separately. Though the effect for males is bigger in magnitude we, the sign of the effect is the same for both genders. The effect size of CL is in turn very small in the pooled data. However, the effect sizes of CL for males and females separately are dramatically higher but have opposite signs. That is, as hypothesized above, CL increases cooperation in females but reduces it in males; these two effects cancel each other in the pooled data.

In sum, results point in the direction of our hypothesis. Although the average

[5]We present effect sizes calculated as the average fraction of the endowment invested in the treatment condition minus the average fraction of the endowment given in the condition respect to which we compare the treatment. This measure of effect size has been used in the meta-analysis on the effects of intuition on cooperation (Rand 2016, Kvarven et al. 2020). Hence, we chose it in order to make our results comparable with those analyzed in these studies. Other popular measures of effect size (Cohen's d and Hedge's g) are shown in Appendix B

[6]The meta-analyses studying the effects of manipulations similar to those employed by us in cooperation (Rand 2016, 2017; Kvarven et al. 2020) find similar effect sizes. The individual effect sizes of analyzed studies roughly stay in a range between -0.10 and 0.23 and the vast majority are bellow 0.10. The overall effect found through the meta-analysis of this studies ranges from -0.0044 to 6.14.

	ED - Control	CL - Control	ED - CL
	Whole Sample		
Effect Size	-6.71	0.15	-6.86
	Male Participants		
Effect Size	-9.23	-14.62	5.40
	Female Participants		
Effect Size	-5.02	9.76	-14.77

Table 3.3: Effect Sizes calculated as the average fraction of the endowment invested in the treatment condition minus the average fraction of the endowment given in the condition respect to which we are comparing the treatment (in percentage points).

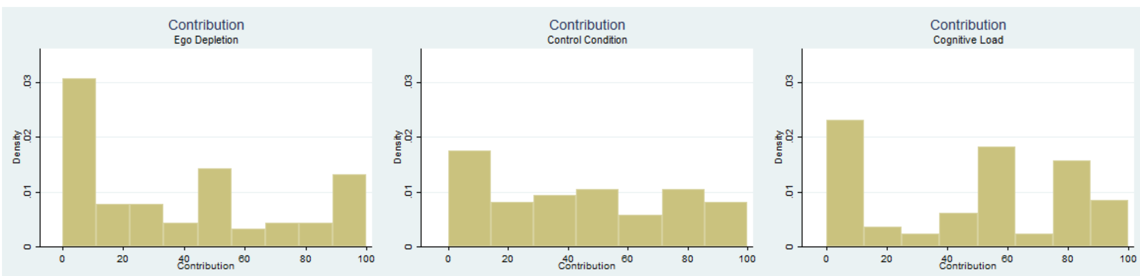

Figure 3.2: Distribution of contribution in PGG by treatment

contributions do not differ significantly across treatments, we find a weakly significant difference in medians between CL and ED conditions and effect sizes have values within our expectations. In the subsequent sections we find more conclusive evidence. In Section 3.3.3, we find that ED, despite not having a discernible effect on average contribution, significantly increases free riding. Thus, leading to an increase in selfishness. In Section 3.3.4 we will confirm that CL has opposite effects for males and females.

3.3.3 Behavioral Types

Figure 3.2 plots the distributions of the contributions in the PGG for each treatment. As we can observe, the contributions in the control treatment are somehow uniformly distributed between 0 and 100. In the treatment groups, in turn, we find a more irregular shape with three peaks in the leftomst, rightmost and center of the distribution respectively. Since both CL and ED aim to impair subjects ability for deliberation, subjects in treatment groups will most likely rely in simple and quick

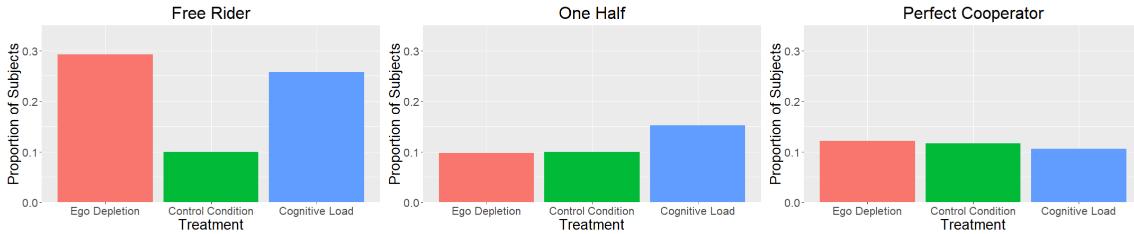

Figure 3.3: Proportion of subjects in each behavioral type by treatment

decision making mechanism, such as choosing salient options. In line with this, as reflected in Figure 3.2, most of the subjects in treatment groups make a contribution consistent with one of the following three behavioral types, which can be interpreted as focal points (i.e. a set of answers that people tend to choose by default due to its salience (Schelling, 1957, 1960)): free riding, making a contributing one-half or perfect cooperation (ED:51,23%, Control:31.67%, CL:51,52%). Hence, in this section, we analyze the impact of ED and CL on these particular behaviors.

The most frequent behavior in our sample is free riding in every condition except the control treatment (see Table 3.1). Free riding corresponds to the extreme response of not cooperating at all (i.e. making a contribution of 0) and it is a widely studied behavior, since it corresponds to the normative prediction for the PGG and is consistent with selfish preferences. Other frequent behaviors are contributing one-half and perfect cooperation. We label perfect cooperation to the kind of behavior in the opposite extreme of free riding; i.e making a contribution of 100% of the endowment and we label one-half the behavior consisting in contributing half of the endowment while keeping the other half.

Figure 3.3 shows the percentage of subjects behaving according to each of the three described behavioral types in each treatment. There are no significant differences between treatments neither in the proportion of perfect cooperators (ED:12.20%, CL:10.61%, Control:11.67%; test of Proportion: ED>Control p=0.4618, CL<Control p=0.4250, ED>CL p=0.3816) nor in the proportion of players contributing one-half (ED:9.76%, CL: 15.15%,Control:10%; test of Proportion: ED<Control p=0.4808, CL>Control p=0.1929 , ED<CL p=0.1591). However, both CL and ED have a positive effect on free riding. The tests of proportions reveal that the proportion of free riders is significantly larger than in the control condition in both treatments (ED:29.27%, CL:25.76%, Control:10%. Proportion test: ED>Control p=0.0027, CL>Control p=0.0111, ED>CL p=0.3176).

All these results are confirmed in a regression analysis in which we study the effect of each treatment on the probability of behaving according to each of the behavioral

	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
VARIABLES	FR	FR	FR	OH	OH	OH	PC	PC	PC
Cognitive Load	1.139**	1.170**	1.155**	0.474	0.597	0.601	-0.107	0.0653	0.175
	(0.514)	(0.524)	(0.535)	(0.550)	(0.570)	(0.580)	(0.567)	(0.591)	(0.604)
Ego Depletion	1.315***	1.381***	1.421***	-0.0274	0.0418	0.0657	0.0503	0.148	0.240
	(0.494)	(0.505)	(0.514)	(0.569)	(0.582)	(0.593)	(0.525)	(0.548)	(0.558)
Age		-0.0809	-0.0836		0.0596*	0.0576*		0.0796**	0.0840**
		(0.0629)	(0.0611)		(0.0333)	(0.0340)		(0.0328)	(0.0335)
Female		-0.883**	-0.904**		-0.0436	0.0444		-0.215	-0.233
		(0.347)	(0.373)		(0.447)	(0.474)		(0.447)	(0.467)
SC Score			-0.00394			-0.00488			0.0301
			(0.0268)			(0.0346)			(0.0340)
FI Score			0.0347			-0.00359			-0.0152
			(0.0370)			(0.0444)			(0.0440)
NFC Score			0.0834**			0.0217			0.0213
			(0.0389)			(0.0458)			(0.0460)
CRT Score			0.193			0.137			0.00729
			(0.216)			(0.270)			(0.277)
Constant	-2.197***	-0.0227	-1.270	-2.197***	-3.573***	-3.836***	-2.024***	-3.787***	-3.387***
	(0.430)	(1.389)	(1.562)	(0.430)	(0.988)	(1.374)	(0.402)	(0.954)	(1.303)
Observations	208	208	208	208	208	208	208	208	208
Pseudo R-squared	0.040	0.078	0.116	0.008	0.027	0.031	0.001	0.041	0.050
Prob > chi2	0.012	0.002	0.001	0.551	0.409	0.800	0.955	0.195	0.494
LR chi2(2)	8.83	17.29	25.88	1.19	3.98	4.60	0.09	6.05	7.40
H0:CL=ED									
p	0.635	0.579	0.499	0.322	0.276	0.295	0.763	0.876	0.903

Estimated coefficients (Standard errors in parentheses)
*** p<0.01, ** p<0.05, * p<0.1

Table 3.4: Treatment effects on behavioral types (Logit Regressions). Dependent variable=1 if subjects belongs to the specified behavioral type; 0 otherwise. FR= Free Rider, OH=One Half, PC=Perfect Cooperator.p-values of a post-estimation test of equality of coefficients for Cognitive Load and Ego Depletion are shown at the bottom.

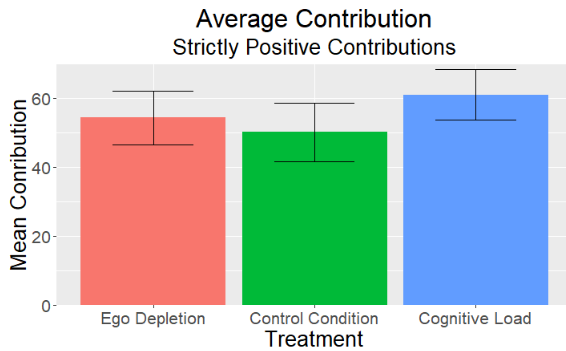

Figure 3.4: Average Contribution of subjects making a contribution>0. Error bars denote 95% confidence intervals

types (Table 3.4).[7] Therefore, we can conclude that both CL and ED have a positive effect in free riding.

Since our treatments stimulate free riding, we test whether there is any treatment difference among those who submit a strictly positive contributions. Hence, we estimate the effect of treatments in those subjects not free riding, i.e. making a positive contribution. This analysis reveals an interesting pattern.

Figure 3.4 shows the average contribution for subjects making a contribution larger than 0 in each treatment(ED: M=54.41, SD=29.90; Control: M=50.22, SD=30.97; CL: M=61.08, SD=25.54). We detect that contribution of people who contribute a strictly positive amount in the CL treatment is larger than in the control condition. This difference is significant at α=7% (Wilcoxon Ranksum: ED=Control p=0.4367, CL=Control p=0.0655, ED=CL p=0.2120). No significant differences between the rest of the conditions and no significant differences when comparing treatments altogether (p=0.1655, Kruskal-Wallis). This conclusion is reinforced using a regression analysis (Table 3.5). Model (1) shows a weakly significant positive effect of CL on contribution. This effect reaches α=5% level significance when including controls (models (2) and (3)).

Hence, results suggest a structural difference in the effects of CL and ED. ED seems to have an unambiguous effect consisting in increasing free riding, in line with our hypothesis. CL, in turn, seems to have a twofold effect: it increases free riding but increases cooperation among those subjects making a positive contribution. In the following section, we document that this difference is driven by gender.

[7]The structure of Table 3.4 is identical to Table 3.2

VARIABLES	(1) Contribution	(2) Contribution	(3) Contribution
Cognitive Load	10.86*	11.44**	13.74**
	(5.727)	(5.650)	(5.753)
Ego Depletion	4.192	4.510	6.490
	(5.489)	(5.405)	(5.494)
Age		1.157***	1.286***
		(0.436)	(0.441)
Female		1.881	1.172
		(4.751)	(4.986)
SC Score			0.414
			(0.339)
FI Score			-0.694
			(0.448)
NFC Score			0.205
			(0.481)
CRT Score			-2.315
			(2.919)
Constant	50.22***	23.27**	40.77***
	(3.950)	(11.19)	(14.25)
Observations	161	161	161
R-squared	0.023	0.065	0.093
H0: CL=ED			
p	0.238	0.214	0.194

Estimated coefficients (Standard errors in parentheses)
*** p<0.01, ** p<0.05, * p<0.1

Table 3.5: Treatment effects on subjects who make a strictly positive contribution (OLS Regressions). p-values of a post-estimation test of equality of coefficients for Cognitive Load and Ego Depletion are shown at the bottom.

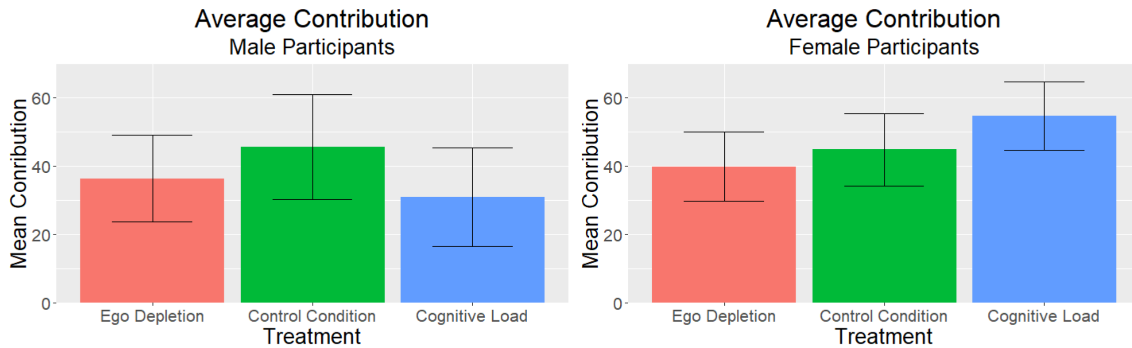

Figure 3.5: Average Contribution in each Treatment by Gender. Error bars denote 95% confidence intervals

3.3.4 Gender Effects

So far, we have only included gender as a control in our estimations and found that women contribute more (Table 3.2) and are less likely to free-ride (Table 3.4) than men.[8].

In this section we explore gender differences in treatment effects. Our initial hypothesis is that ED should increase impulsivity and thus increase selfishness in subjects, irrespective of their gender. In contrast, CL might have a different effect across genders. Since CL induces an intuitive cognitive style, causing subjects to behave according to social heuristics that might depend upon gender roles, it might make females cooperate and males defect.

When analyzing differences in behavior of males and females within each treatment, as expected, we find no differences in control and ED treatments but we do find that in the CL treatment females are significantly more cooperative (p=0.006) and less likely to free ride (p=0.017). See Appendix C for more details.

Figure 3.5 shows the average contribution in each treatment for male and female participants separately. Consistent with our hypothesis, males contribute less in both treatments, as compared to the control condition (ED: M=36.39, SD=35.82; Control: M=45.62, SD=36.18; CL: M=31, SD=35.63), whereas females contribute less in the ED treatment but more in the CL treatment (ED: M=39.90, SD=35.33; Control:M=44.91, SD=31.33, CL: M=54.67, SD=31.14.). Nonetheless, we do not

[8]Indeed, we find that, overall, average contributions of female participant are larger than those of male participants with difference being close to an α=5% (p=0.0523, Wilcoxon Ranksum) and that the proportion of Free Riders is significantly larger among males than among females (p=0.0071, Test of proportions).

find significant differences in males, neither in overall comparison of treatments (p=0.2766, Kruskal-Wallis) nor in pairwise comparisons (Wilcoxon Ranksum: ED=Control p=0.2769, CL=Control p=0.1034, CL=ED p=0.5716). In case of females there are no significant differences in treatments considered altogether (p=0.1184, Kruskal-Wallis). In pairwise comparisons though, the difference between ED and CL conditions is significant (Wilcoxon Ranksum: ED=Control p=0.3526, CL=Control p=0.2062, CL=ED p=0.0485).

In Table 3.6 we run the models presented in Table 3.2 separately for each gender. We find that ED has a negative effect on contributions both for males and females. The coefficient for the CL treatment, in turn, has different signs for male and female participants, such that is positively related to contributions for females and negatively for males. None of these coefficients is statistically significant in models (1) and (2), but the CL coefficient reaches a marginal level of significance for female subjects in model (3).

In Table 3.7, we pool the data and estimate gender effects through a model with interactions. Results of this estimation are similar to those shown in Table 3.6. We find that treatment effects for male participants, captured by the coefficient of ED and CL indicators respectively, is negative in the case of both treatments, though none of them reaches significance. The effect for females is captured by the sum of the coefficients of the treatment indicator, the female indicator and the female*treatment interaction term. The effect is negative for ED and positive for CL, though the coefficients do not reach joint significance. In addition, post estimation tests reveal that effects of CL and ED are significantly different for females but not for males. More importantly, a post-estimation test shows that the effect of CL is significantly different for male and female, while the effect of ED is not. Thus, confirming our hypothesis of a mediation of gender in the effect of intuition on cooperation but not in the effect of impulsivity.

All in all, these results point in the direction of our hypothesis, suggesting that ED decreases contributions regardless of the gender, while CL decreases males' contributions and increases that of females.

	Male Participants			Female Participants		
	(1)	(2)	(3)	(1)	(2)	(3)
VARIABLES	Contribution	Contribution	Contribution	Contribution	Contribution	contribfemale
Cognitive Load	-14.62	-12.27	-12.60	9.758	10.14	12.61*
	(10.15)	(10.01)	(10.18)	(7.560)	(7.477)	(7.511)
Ego Depletion	-9.231	-8.156	-8.156	-5.019	-5.092	-2.337
	(9.622)	(9.440)	(9.521)	(7.223)	(7.142)	(7.237)
Age		1.463**	1.678**		1.328*	1.212*
		(0.700)	(0.724)		(0.681)	(0.679)
SC Score			0.213			0.486
			(0.566)			(0.460)
FI Score			-0.572			-0.980*
			(0.810)			(0.586)
NFC Score			-1.216			-0.190
			(0.891)			(0.575)
CRT Score			0.524			-6.588
			(4.574)			(3.975)
Constant	45.62***	12.10	23.51	44.92***	16.18	51.13**
	(7.321)	(17.58)	(23.36)	(5.484)	(15.70)	(20.99)
Observations	83	83	83	125	125	125
R-squared	0.026	0.077	0.114	0.036	0.065	0.116
H0: CL=ED						
p	0.5679	0.6571	0.6374	0.0371**	0.0300**	0.0318**

Estimated coefficients (Standard errors in parentheses)
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3.6: Treatment effects on Contribution separated by gender (OLS Regressions on Contribution). p-values of a post-estimation test of equality of coefficients for Cognitive Load and Ego Depletion are shown at the bottom.

VARIABLES	(1) Contribution	(2) Contribution	(3) Contribution
Cognitive Load	-14.62	-12.37	-11.41
	(9.655)	(9.516)	(9.499)
Ego Depletion	-9.231	-8.203	-7.982
	(9.150)	(8.995)	(8.940)
Female	-0.708	1.079	0.0366
	(8.988)	(8.851)	(8.904)
Female*Cognitive Load	24.38*	22.54*	23.04*
	(12.43)	(12.23)	(12.16)
Female*Ego Depletion	4.212	3.108	4.127
	(11.82)	(11.62)	(11.58)
Age		1.399***	1.486***
		(0.484)	(0.482)
SC Score			0.321
			(0.352)
FI Score			-0.820*
			(0.469)
NFC Score			-0.547
			(0.479)
CRT Score			-2.570
			(2.916)
Constant	45.62***	13.57	36.88**
	(6.962)	(13.02)	(16.39)
Observations	208	208	208
R-squared	0.046	0.084	0.115

(a)	T. Effect of females	Ego Depletion			Cognitive Load		
		(1)	(2)	(3)	(1)	(2)	(3)
	Coefficient (Treatment+Female+Female*Teatment)	-5.727	-4.017	-3.818	9.050	11.241	11.665
	p	0.501	0.632	0.652	0.305	0.197	0.183

		(1)	(2)	(3)
(b.1)	H0: CL (Males) = CL (Females)			
	p	0.0064***	0.0056***	0.0080***
(b.2)	H0: ED (Males) = ED (Females)			
	p	0.6487	0.5798	0.5847

		Males			Females		
		(1)	(2)	(3)	(1)	(2)	(3)
(c)	Ho:ED=CL						
	p	0.547	0.636	0.696	0.0434	0.0338	0.0305

Estimated coefficients(Standard errors in parentheses)

*** p<0.01, ** p<0.05, * p<0.1

Table 3.7: Mediation of gender in treatment effects on Contribution (OLS Regressions). (a) shows, for each model, coefficients for the effects of ED and CL respectively in females (calculated as the sum of coefficients treatment+Female+Female*Treatment) and the p-values for the significance test of these coefficients. (b.1) and (b.2) Shows p-values for a post-estimation test of equality of the treatment effect for males and females, for CL and ED respectively. (c) Shows p-values for a post-estimation test of equality of the effects of CL and ED within each gender.

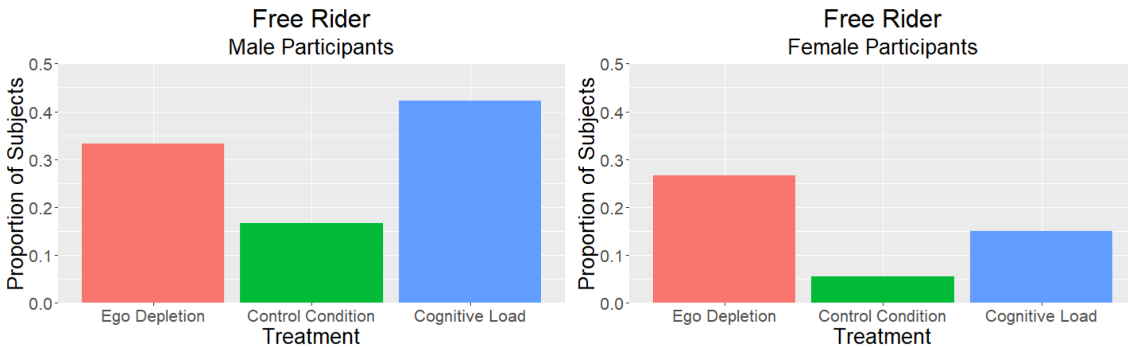

Figure 3.6: Proportion of Free Riders in each Treatmet by Gender

Next, we analyze free riding. Figure 3.6 shows the proportion of free riders in each treatment for male and female participants separately. As we can see, the proportion of free riders is larger in both treatment groups than in the control condition for both male and female participants, though the difference is more remarkable in the case of males (Male: ED:33,33%, Control:16.67%, CL:42.31%, Female: ED:26.53%, Control:5.56%, CL:15%). However, treatment effects on free riding seem not to be symmetrical across genders. For males, CL seems to lead to a significant increase in free riding, while ED does not and there are no significant differences between ED and CL treatments (Males Proportion test: ED>Control p=0.0791, CL>Control p=0.0240, ED<CL p=0.2396). The situation is somewhat reversed in the case of female participants, in which we detect a strong effect of ED through a test of proportion, whereas the difference between CL and Control condition is only weakly significant. So is also the difference between CL and ED treatments (Females Proportion test: ED>Control p=0.0061, CL>Control p=0.0902, ED>CL p=0.0933).

In line with these results, the logit model in Table 3.8 shows that ED leads to a significant increase in the probability of Free Riding for females, while CL has no discernible effect. The opposite happens for males, for which CL leads to a significant increase in the propensity to Free Ride, while CL has no effect.

In Table 3.9 we estimate jointly the effect for males and females in a model with interaction. Looking at the sign of the coefficients, we see that both ED and CL increase Free Riding for males. For females in turn, ED increases Free Riding, while CL decreases it. Nonetheless, only the effect of CL for male reaches a weak level of significance; the rest of the effects are bellow the threshold of 10% level significance. Post estimation tests reveal that effects of CL and ED are significantly different for females but not for males and the effect of CL is different for males and females, while the effect of ED is not.

	Male			Female		
	(1)	(2)	(3)	(1)	(2)	(3)
VARIABLES	Free Rider	Free Rider	Free Rider	Free Rider	Free Rider	Free Rider
Cognitive Load	1.299*	1.467**	1.429**	1.099	1.101	1.019
	(0.676)	(0.709)	(0.728)	(0.852)	(0.852)	(0.877)
Ego Depletion	0.916	1.137	1.147	1.815**	1.815**	1.843**
	(0.661)	(0.693)	(0.709)	(0.796)	(0.796)	(0.825)
Age		-0.273**	-0.256*		0.00595	0.0260
		(0.133)	(0.136)		(0.0600)	(0.0573)
SC Score			0.0312			-0.0446
			(0.0388)			(0.0406)
FI Score			0.0279			0.0313
			(0.0552)			(0.0527)
NFC Score			0.106*			0.0898*
			(0.0628)			(0.0511)
CRT Score			0.315			0.170
			(0.330)			(0.330)
Constant	-1.609***	4.022	2.652	-2.833***	-2.962**	-5.041**
	(0.548)	(2.713)	(3.026)	(0.728)	(1.498)	(2.025)
Observations	83	83	83	125	125	125
Pseudo R-Squared	0.040	0.101	0.161	0.064	0.064	0.110
Prob > chi2	0.126	0.015	0.020	0.027	0.065	0.088
LR chi2(2)	4.14	10.41	16.58	7.21	7.22	12.40
	Male Participants			Female Participants		
	(1)	(2)	(3)	(1)	(2)	(3)
H0:CL=ED						
p	0.4799	0.5579	0.6405	0.1917	0.1933	0.1474

Estimated coefficients (Standard errors in parentheses)
*** p<0.01, ** p<0.05, * p<0.1

Table 3.8: Treatment effects on free riding separated by gender (Logit Regressions). Dependent variable=1 if subjects is a Free Rider (Contribution=0).

VARIABLES	(1) freerider	(2) freerider	(3) freerider
Cognitive Load	1.299*	1.280*	1.307*
	(0.676)	(0.681)	(0.698)
Ego Depletion	0.916	0.944	0.982
	(0.661)	(0.668)	(0.679)
Female	-1.224	-1.266	-1.276
	(0.911)	(0.914)	(0.930)
Female*Cognitive Load	-0.201	-0.171	-0.238
	(1.088)	(1.091)	(1.112)
Female*Ego Depletion	0.898	0.902	0.911
	(1.035)	(1.039)	(1.057)
Age		-0.0785	-0.0786
		(0.0627)	(0.0605)
SC Score			-0.00286
			(0.0270)
FI Score			0.0336
			(0.0374)
NFC Score			0.0867**
			(0.0390)
CRT Score			0.171
			(0.220)
Constant	-1.609***	0.0806	-1.172
	(0.548)	(1.409)	(1.607)
Observations	208	208	208
Pseudo R-squared	0.078	0.088	0.127
Prob > chi2	0.004	0.003	0.002
LR chi2(2)	17.27	19.46	28.16

(a)	T. Effect of females	Ego Depletion			Cognitive Load		
		(1)	(2)	(3)	(1)	(2)	(3)
	Coefficient (Treatment+Female+Female*Treatment)	-0.125	-0.158	-0.207	0.591	0.580	0.617
	p	0.859	0.824	0.775	0.353	0.366	0.351

		(1)	(2)	(3)
(b.1)	H0: CL (Male)= CL (Female)			
	p	0.0166**	0.0160**	0.0165**
(b.2)	H0: ED (Male)= ED (Female)			
	p	0.507	0.462	0.4802

		Males			Females		
		(1)	(2)	(3)	(1)	(2)	(3)
(c)	Ho:ED=CL						
	p	0.480	0.538	0.567	0.192	0.180	0.146

Estimated coefficients (Standard errors in parentheses)

*** p<0.01, ** p<0.05, * p<0.1

Table 3.9: Mediation of gender in treatment effects on free riding (Logit Regressions). Dependent variable=1 if subjects is a Free Rider (Contribution=0). (a) shows, for each model, coefficients for the effects of ED and CL respectively in females (calculated as the sum of coefficients treatment+Female+Female*Treatment) and the p-values for the significance test of these coefficients. (b.1) and (b.2) Shows p-values for a post-estimation test of equality of the treatment effect for males and females, for CL and ED respectively. (c) Shows p-values for a post-estimation test of equality of the effects of CL and ED within each gender

Considered altogether, the results of this section point in the direction of our hypothesis. We confirm that the effects of CL are different for male and female subjects, while the effect of ED is similar for both genders. Analyses seems to suggest that ED makes subjects more selfish regardless of their gender, whereas CL makes males more selfish and females more cooperative. Nevertheless, these are not totally conclusive in statistical terms, which inform us of the need of enlarging the sample.

3.4 Discussion

The present study investigates the different impact of intuition and impulsivity on cooperation through a PGG experiment with an ED and a CL manipulation.

We build on two hypothesis. First, that impulsivity is a distinct cognitive style essentially different from intuition. The behavioral consequences of impulsivity, unlike those of intuition, might not be consistently explained by SHH. We predict that impulsivity should impair subjects' ability to overcome their selfish impulses and lead to a reduction in prosocial behavior regardless of the subjects' gender.

The second of out hypothesis concerns the mediation of gender in SHH. SHH hypothesizes that intuition leads subjects to display a default response acquired through repeated social interaction. While the original formulation of SHH assumes that this default answer is cooperation for subjects of both genders, we build in the results shown by Rand et al (2016) to propose a more complex formulation that posits that this answer differs from males to females in function of of gender roles. Female social roles are commonly associated with prosocial concerns, which makes females acquire cooperation as a default response. Nonetheless, male social roles are frequently associate with proself or even antisocial attitudes, such as aggression and competition. This makes us think that males' default response may be more selfish.

To test our hypotheses, we study the effects of impulsivity and intuition on cooperation through a PGG with three experimental conditions: an ED condition, aimed to induce impulsive behavior, prior to the game; a CL condition, aimed to induce impulsive behavior, and a control condition in which subjects play the game without undergoing any manipulation. Despite not finding significant differences in average contribution across treatments, we find that ED leads to a significant increase in free riding, in line with our first hypothesis. Respect to CL, we find a twofold effect, it increases propensity to free ride in some subjects while it increases cooperation in those making a positive contribution.

Regarding the second hypothesis, we find that the effect of CL on cooperation is significantly different for males and females while ED is not. Thus, confirming that there exist a mediation of gender in the effects of intuition but not in the effects of impulsivity. Results suggest that ED leads to an increase in free riding regardless of subjects' gender, whereas CL seems to make males more prone to free ride and females more prone to cooperate. However, this later result does not achieve the desired level of statistical significance in some of our analysis.

All in all, we find that results point in the direction of our hypotheses. We have found the expected signs in every analysis we have conducted. However, some failed to reach the desired level of significance significance. We reckon that this could be due to a lack of statistical power caused by the small size of our sample. Hence, we conclude that additional experimental sessions should be conducted in order to increase sample size. We plan to enlarge the sample size, the least, to double up the current number of observations.

Previous research within the literature has focused mainly in measuring the difference between decisions taken through deliberation and those taken under a quick and non-deliberative cognitive style that has been indistinctly labeled as intuition. By pointing out the differences between impulsivity and intuition, as well as differences in how males and females are affected by intuition, we propose a new lens through which look at published scholarly and uncover new topics to be studied in future investigations. On the one hand, we show that the effect of impulsivity as well as the effect of intuition in males bears a different sign from that of the effect of intuition in females. Previous meta-analysis about the effect of intuition on preferences have not taken in consideration the difference between impulsivity and intuition and only Rand et al (2016) and Rand (2017) have separate the effect of intuition for males and females. The small (and sometimes null) effect of intuition found may be a product of these opposing effects canceling each other. A new meta-analysis which considers these differences may find that previous studies have underestimated the effect of intuition.

On the other hand, during the last decade scholars within the fields of economics and psychology have devoted a great deal of attention to the effects of intuition and have study its effects on a wide range of economic games and on topics unrelated to preferences, such as honesty (see Köbis et al., 2019 or a meta-analysis). The effects of impulsivity on this topics, however, have been rarely addressed, since the difference of this cognitive style with respect to intuition has not receive much attention so far. Future research should investigate the effects of impulsivity on these issues and how they differ from those of intuition.

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Appendix A: ED task adaptation

A large number of tasks have been employed in the literature in order to deplete subjects (for a list of the most frequently used tasks, see Carter et al., 2015; Dang, 2018). However, recently a series of meta-analysis and replications have cast doubts on the validity of most of them (Hager et al. 2018, Carter et al.2015; Hager et al. 2016; Dang, 2018; Vohs et al. 2021). We choose a Stroop task adapted from the one in Dang et al. (2017), which proved to consistently lead to a reduction of subjects self-control in the multi lab registered Replication in Dang et al. (2021).

In the original task by Dang et al. (2017, 2021) subjects were presented each time one of the following words: "BLUE", "RED", GREEN" or "YELLOW". Each word was written in an either blue, red, green or yellow font. In some trials the color of the font was the color designated by the word (e.g. e.g. "YELLOW" written in a yellow font. We label these trials "congruent"), while in others the font was of a different color (e.g. "BLUE" written in a red font. We label these trials "incongruent"). When the word was shown subjects had to read the color of the font aloud and press the spacebar. After this was done, a blank screen was shown during 500 ms. Then followed a fixation (5 stars in a white font in the place where target words appear) of 200 ms. After this, the next word appear and the process is repeated.

Subjects in the treatment group repeated this process until they completed a total number of 256 trial, of which 75% were incongruent and the rest congruent.

The conditions of our experiment demanded that a large number of subjects take the task simultaneously at the same time. The nature of the PGG requires that a large number of subjects play the game simultaneously, so that random groups of four players could be formed and identity of each players' team mates remains anonymous. We did not have the means to check the accuracy of answers given orally by this large number of subjects simultaneously. For that reason, we made a slight modi

cation respect to the original task by Dang et al. (2017, 2021) in order to adapt it to our conditions.

A key was assigned to each of the four colors (1=red, 2=blue, 3=green, 4=yellow), subjects had to press the key corresponding to the color of the font in order to give their answers. The program registered their answer and the response time. In addition, subjects were asked to pronounce the answers orally, though correctness of oral answer could not be checked. In order to prevent subjects to be disturbed by the noise created by other subjects oral answers, each subject was given a pair of earplugs they could used if they wanted.

In order to ensure that subjects performed as best as they could, the task was incentivized. Subjects got €0.02 for each correct answer, but got a penalization of 0.004€0.02 for each second taken to give their response, so that they received no payoff for a correct trial if they took 5 seconds or more to give their answer. No negative payoff could be received for any trial. Subjects did not get any payoff for trials in which an incorrect answer was given. In sum, subjects could earn up to €5.12 in the task, which is the amount they would receive if they would give a correct answer correctly the 256 trials with a response time of 0.00 seconds in each of them.

Pictures of the user's interface are shown in Figures 3.A.1 to 3.A.4 , which taken together show an example of the sequence of screens displayed during the task. Figure 3.A.1 shows an incongruent trial (the word "AZUL", i.e. Blue in Spanish, written in a yellow font). After the subject gives his answer by pressing one of the keys assigned for response (1, 2, 3 or 4, shown in the Key Guide in the bottom of the screen), a blank screen is shown during 500 ms (Figure 3.A.2). Then a fixation screen is shown during 200 ms (Figure 3.A.3). After this, the next trial appears on screen and the process is repeated. Figure 3.A.4 shows an example of a congruent trial (the word "VERDE", i.e. Green in Spanish, written in a green font). This process is repeated until subjects complete 256 trials (192 congruent and 64 congruent) presented in random order. As it can be seen in the picture, a Key Guide is shown in the bottom of the screen at any time, including the blank and fixation screens. This Key Guide show which key of the keyboard corresponds to each color. It allows subjects to check which key corresponds to each answer at any time and allow them to make the task without having to memorize the keys.

Figure 3.A.1: Stroop task. Trial Screen. The screen shows an incongruent trial, the word "AZUL" (Blue in Spanish) is written in a yellow font.

Figure 3.A.2: Stroop Task. Blank Screen

Figure 3.A.3: Stroop Task. Fixation Screen

Figure 3.A.4: Stroop task. Trial Screen. The screen shows a congruent trial, the word "VERDE" (Green in Spanish) is written in a green font.

Appendix B: Effect Sizes

	ED - Control	CL - Control	ED - CL
		Whole Sample	
Cohen's d	-0.19 (-0.53,0.14)	0.004 (-0.34,0.35)	-0.20 (-0.52,0.13)
Hedges's g	-0.19 (-0.53,0.14)	0.004 (-0.34,0.35)	-0.19 (-0.52,0.13)
		Male Participants	
Cohen's d	-0.26 (-0.78,0.27)	-0.41 (-0.97,0.15)	0.15 (-0.36,0.66)
Hedges's g	-0.25 (-0.77,0.27)	-0.40 (-0.95,0.15)	0.15 (-0.36,0.66)
		Female Participants	
Cohen's d	-0.15 (-0.58,0.28)	0.31 (-0.14,0.76)	-0.44 (-0.86,0.02)
Hedges's g	-0.15 (-0.57,0.28)	0.31 (-0.14,0.76)	-0.44 (-0.85,0.02)

Table 3.B.1: Effect Sizes (CI in parentheses)

Appendix C: Within-Treatmen analysis of Gender Effects

In this section we analyze gender differences in subjects behavior within each treatment. Figures 3.C.1 and 3.C.2 differences in average contribution (ED: Males M=36.39 SD=35.82, Females M=39.90 SD=35.33, Control: Males M=45.62 SD=36.18, Females M=44.92 SD=31.33, CL: Males M=31 SD=35.63 Females M=54.67 SD=31.14) and proportion of free riders (ED: Males:33.33%, Females:26.53%; Control: Males:16.67%, Females:5.56%; CL: Males:42.31%, Females:15%) respectively between males and females. In both cases, we observe no significant differences between male and female subjects in ED treatment, whereas in the CL treatment we find that average contribution is significantly higher (Wilcoxon Ranksum: ED p=0.5911, Control p=0.9457, CL p=0.0087) and the proportion of free riders is significantly lower in females. In the control condition, we find no significant differences in average contribution. The proportion of free riders is lower among females, but this difference is only marginally significant (Test of Proportion: ED Males >Females p=0.2534, Control Males> Females p=0.0799, CL Males>Females p=0.0066).

We find similar results through a regression analysis. In Table 3.C.1 we estimate the effects of gender on contribution. For each treatment we run three models, (1) includes only a dummy which takes value 1 if the subject is female and 0 otherwise, in (2) we add age and in (3) we include personality traits. We find that female subjects are significantly more cooperative than males in the CL condition. Gender has no significant effect in the rest of the conditions. Similar results are shown in Table 3.C.2 were we estimate the effects of gender on the probability of free riding through separate logit models for each treatment. Female subjects are significantly less likely to free ride in CL treatment, but there are no significant effects of gender in the rest of the conditions. Therefore we confirm that ED has the same effect in

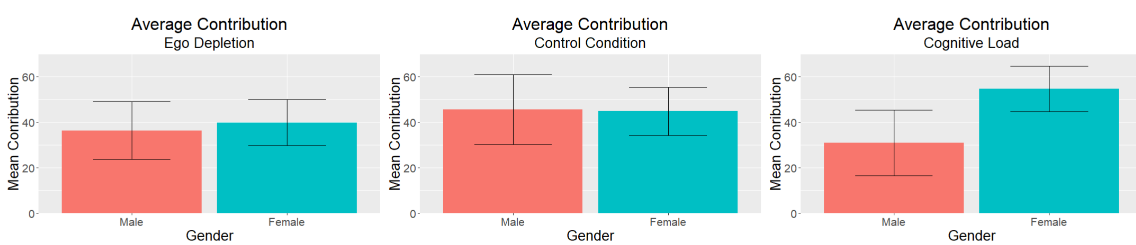

Figure 3.C.1: Average Contribution of Male and Female subjects within each treatment. Error bars denote 95% confidence intervals

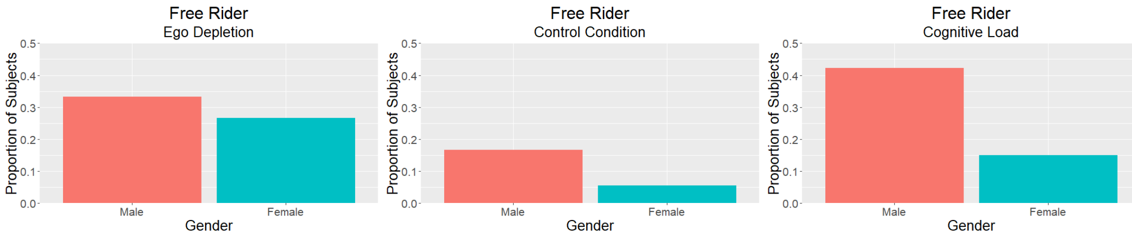

Figure 3.C.2: Proportion of Free Riders among Male and Female subjects within each Treatment

males and females, while CL has different effects for each gender.

Hence, we confirm that, as hypothesize, the effect of ED is similar in male and female subjects, while the effects of CL differ significantly depending on subjects' gender.

	Ego Depletion			Control Condition			Cognitive Load		
VARIABLES	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Female	3.504	4.096	6.238	-0.708	1.100	-0.805	23.67***	23.60***	20.23**
	(8.000)	(7.979)	(8.486)	(8.785)	(8.487)	(8.632)	(8.306)	(8.275)	(9.026)
Age		1.214	1.162		1.415**	1.782***		1.881	1.221
		(0.932)	(0.960)		(0.595)	(0.601)		(1.544)	(1.623)
SC Score			0.0128			0.430			0.462
			(0.612)			(0.582)			(0.700)
FI Score			-0.821			-1.691**			-0.322
			(0.841)			(0.834)			(0.838)
NFC Score			0.111			-0.928			-0.980
			(0.843)			(0.895)			(0.828)
CRT Score			1.999			-6.485			-5.659
			(4.938)			(5.394)			(5.312)
Constant	36.39***	9.475	23.40	45.63***	13.19	55.66**	31***	-9.086	29.96
	(6.185)	(21.57)	(28.78)	(6.805)	(15.13)	(22.68)	(6.466)	(33.52)	(42.11)
Observations	82	82	82	60	60	60	66	66	66
R-squared	0.002	0.023	0.037	0.000	0.090	0.209	0.113	0.133	0.188

Estimated coefficients (Standard errors in parentheses)
*** p<0.01, ** p<0.05, * p<0.1

Table 3.C.1: Gender effects by treatment (OLS Regressions).

	Ego Depletion			Control Condition			Cognitive Load		
VARIABLES	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Female	-0.325	-0.340	-0.421	-1.224	-1.260	-1.036	-1.424**	-1.693***	-1.746**
	(0.491)	(0.493)	(0.522)	(0.911)	(0.913)	(1.058)	(0.595)	(0.650)	(0.731)
Age		-0.0296	-0.0214		-0.0466	-0.116		-0.423**	-0.453**
		(0.0644)	(0.0654)		(0.0974)	(0.155)		(0.199)	(0.229)
SC Score			0.0124			-0.0323			0.0216
			(0.0372)			(0.0673)			(0.0543)
FI Score			0.0571			0.110			-0.0186
			(0.0557)			(0.110)			(0.0657)
NFC Score			0.0536			0.160			0.104
			(0.0540)			(0.113)			(0.0702)
CRT Score			-0.00345			0.647			0.0940
			(0.310)			(0.590)			(0.416)
Constant	-0.693*	-0.0391	-1.364	-1.609***	-0.587	-3.531	-0.310	8.639**	9.429*
	(0.369)	(1.461)	(1.886)	(0.548)	(2.137)	(3.995)	(0.397)	(4.205)	(5.256)
Observations	82	82	82	60	60	60	66	66	66

Estimated coefficients (Standard errors in parentheses)
*** p<0.01, ** p<0.05, * p<0.1

Table 3.C.2: Gender effects on free riding (Logit Regressions). Dependent variable=1 if subjects is a free rider (Contribution=0); 0 otherwise.