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Abstract

Standard international economic theory suggests that people should embrace economic integration because it promises large gains. But recent events such as Brexit indicate a desire for economic *dis*integration. Here we report results of an experiment, based on a strategic analytical framework, of how size and distribution of potential gains from integration influence outcomes and individuals' inclination to embrace integration. We find that cross-country inequality in potential gains acts as a friction to realize those gains. This suggests that to better understand recent phenomena, international economic theory should account for distributional considerations and behavioral aspects it currently ignores.

Keywords: endogenous institutions, globalization, indefinitely repeated games, social dilemmas.
JEL codes: C70, C90, F02

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1 Introduction

International integration of markets promises large economic gains as resources can be allocated more efficiently throughout the world economy. Yet, after decades of increasing economic integration, there are signs of a desire to scale it back. The backlash in the support of regional and multilateral trade agreements is one indication. The choice of a majority of the British people in 2016 to exit the European Union is another. The question is why: what pushes individuals to limit or to scale back economic integration given that it is costly?[1]

Here, we use an experiment to study a possible contributing factor: the size and distribution of potential economic benefits. The experimental methodology is helpful because it allows us to establish causality while minimizing possible confounding factors—political, social and cultural, for instance. The starting point in our design is that economic integration expands both size and heterogeneity of the trading group. This is *potentially* beneficial because it raises prospective payoffs for everyone. But, it also induces frictions—trust and enforcement problems that may encourage short-sighted conduct and discourage cooperation (Rodrik, 2000).[2] In this setting, size and distribution of prospective benefits may influence choices for two reasons.

First, laboratory evidence from two-player cooperation tasks reveals that opportunistic behavior is elastic to the size of economic incentives (Dal Bó and Fréchette, 2018). It is an open question if this result extends to large groups of strangers, such as traders in a global market. This is an important angle to explore because it has been argued that traditional analysis significantly

[1]Brexit may cost up to 10 percent of UK per capita GDP (Sampson, 2017); Chakraborty et. al. (2017) estimate the cost of financial disintegration in Europe at half percent of GDP.

[2]Rodrik (2000) asserts that individuals must be willing—and not only able—to cooperate with each other but face additional frictions as compared to exchange within borders, due to different cultural, legal and political institutions.

underestimates the size of the possible gains from integration (Desmet et al., forth.; Ossa, 2015); if economic incentives affect conduct, then this underestimation might affect outcomes and the support for integration.

Second, even though countries tend to gain from integration, these benefits may be unevenly distributed or simply *perceived* to be so, due to cultural or political considerations. This sort of inequality of opportunity across countries is an unexplored angle in current theories in international economics. Studying it is meaningful because of mounting evidence that economic inequality distorts decision-making by inducing short-sighted conduct (Haushofer and Fehr, 2014), and hinders coordination on efficient norms (Camera et al., forth.).

The experiment consists of a cooperation task involving many rounds of play. Twenty-four participants are randomly assigned to one of three types (countries), each defined by a specific cooperation payoff, and then are thrust into two economic scenarios. In the "autarky" scenario, participants have a fixed counterpart of their same type; here, economies are small, homogeneous and with minimal trust and enforcement frictions. In the "economic integration" scenario, participants get a boost in their cooperation payoff but now find themselves in a 12-person group with an equal mix of types, where counterparts are anonymous and change at random in each round. Now, economies are large, heterogeneous, with more frictions and—importantly—entail inequality of opportunity since cooperation payoffs vary from type to type.

In each scenario, *actual* payoffs are endogenous, thus may fall short of *potential* depending on counterparts' behavior. After participants experience each kind of scenario, we form a single 24-person group and ask participants if they wish to leave it for autarky, or scale its size down to 16 by forcing one type into autarky. The majority preference determines the configuration of this last economy. Between-subjects treatments manipulate the distribution and size—relative and absolute—of the prospective gains from integration so that in one case integration ensures equal opportunity, while in another it pushes prospective payoffs further apart. We employ the theory of repeated games to identify a self-enforcing norm of conduct that supports efficiency, and to derive hypotheses which we then test using the data collected.

We report four main results. First, participants struggled to capture the benefits made possible by integration because trust and enforcement problems prevented cooperation on a wide scale. Second, this finding is robust to the size of prospective payoffs; cooperation did not improve when we boosted the potential gains from integration by almost 70%. As a result, integration created winners and losers, within and across player types. Third, inequality of opportunity acted as an obstacle to realizing the gains from integration; cooperation and welfare declined when prospective payoffs were unequal as compared to when they were not. Fourth, participants' attitudes toward economic integration were elastic to inequality of economic opportunity: all else equal, the support for integration sharply declined when it exhibited inequity in the distribution of *potential* gains across types.

These results suggest that to better understand recent phenomena, international economic theory should account for cross-country distributional considerations it currently ignores. The experiment also corroborates the notion that economic integration may produce outcomes that diverge from those predicted by standard models because frictions limit individuals' ability to realize the gains from trade—an angle of inquiry that is gaining traction in the international literature (Antràs and Costinot, 2010).

The study develops as follows. Section 2 discusses related experiments, and Section 3 the design. Theoretical predictions and testable hypotheses are in Section 4, results in Sections 5, and policy considerations in Section 6.

2 Related experimental literature

There is abundant laboratory evidence suggesting that when the group size varies exogenously, larger groups attain less efficient outcomes than smaller groups, both in pure coordination games (Van Huyck et al., 1990; Weber, 2006), and in social dilemmas where coordination problems are compounded by opportunistic motivations (Camera et al., 2013; Isaac and Walker, 1988). Our study replicates these baseline results by initially confronting subjects with small and large groups, before endogenizing group size.

The literature on endogenous group size focuses on the impact of group entry or exit rules on the choice of group size, in social dilemmas. A main finding is that cooperators will readily aggregate into large groups of like-minded individuals, as long as institutions exist that allow positive assortative matching (or self-selection) by facilitating the identification and isolation of free riders (Ahn et al., 2009; Cinyabuguma et al., 2005; Croson et al., 2015; Güth et al., 2007; Maier-Rigaud et. al., 2010). Our study differs from these earlier designs in three dimensions. First, the underlying task is not the usual VCM game with fixed partners, but a cognitively simpler decision problem involving counterparts who are strangers. Second, given our focus on international integration, the group size is determined by a collective decision process that aggregates the preferences of all participants while preventing ostracism of single individuals, self-isolation, and self-selection. Third, many outcomes, including the efficient one, are consistent with equilibrium, which is unlike earlier social dilemma designs where theory invariably predicts inefficient play, and more similar to the coalition-formation study in Nash et al. (2012).

To implement these features we use the frictional trading game developed in Camera et al. (2013), and build on the design in Bigoni et al. (2019), where players are homogeneous, identically benefit from cooperation in any group size (i.e., equal opportunity), and large groups offer a prospective surplus gain of 50% to everyone. A main finding is that individuals prefer small to large groups, and they are unable to realize most of the potential gains, leaving open the question of whether the choice to form large groups, and their economic performance, depends on the size and distribution of potential gains. The present study provides answers by, first, exploring much wider surplus gains and, second, by studying performance and choices under uneven distributions of those gains. A second important open question concerns the impact of inequality of opportunity – an aspect that is largely unexplored in the literature on cooperation in supergames, and group formation. We contribute to this line of research by studying the effect of spreading apart players' benefits from cooperation on group performance and group choice.

Finally, our study contributes to expand the use of experimental methods to the field of international economics, following in the footsteps of the pioneering market experiment in Noussair et al. (1995) that tested the competitive trade model in the lab, and the social dilemma experiment in Barrett and Dannenberg (2012) about the difficulties of international collective action in addressing climate change problems.

3 Experimental design

The design captures the idea that economic integration expands the efficiency frontier relative to domestic trade, but also weakens the contractual environment, creating trust and enforcement problems. Individuals must coordinate on new norms of conduct, and must rely to a greater extent on self-enforcing rules, which magnifies the incentives for opportunistic conduct (Rodrik, 2000).

To do so we adopt the trading game developed in Camera and Casari (2014), where trust and enforcement problems create temptations for short-sighted conduct, and endogenize the group size building on Bigoni et al. (2019). Below we describe the Baseline treatment.

A round of play. Twenty-four participants are randomly assigned to one of three types $i = 1, 2, 3$ for the entire session, 8 per type.[3] Think of a type as a country. In each round half of the players are consumers and half producers, and every producer meets a consumer. In each pair, interaction takes the form of a "helping game" where only the producer makes a choice; see Table 1.

Table 1: Payoffs in a Producer-Consumer Pair.

	Producer's choice	
	Defect	*Cooperate*
Consumer (type i):	3	$9 + 2i + g$
Producer (any type):	6	0

The game is interpreted as a situation where a producer and a consumer can engage in a transaction. It is up to the producer whether to pursue this possibility (cooperate) or not (defect). If the producer does not pursue it, then she obtains 6 points while the consumer earns 3 points (1 point= USD 0.18). Instead, if the producer chooses to trade, then she has an immediate cost implying that she earns nothing while the consumer of type i earns $9 + 2i + g$ points where $g \geq 0$. Hence, cooperation payoffs increase in i and we say that type 1 is "disadvantaged" while type 3 is "advantaged" relative to the "middle" type 2. Given the structure of this one-shot interaction, for a producer to be

[3]In the experiment types 1,2 and 3 correspond to the colors green, red, and blue.

willing to invest in cooperation, there must be some prospect of future trade opportunities, which is next discussed.

A supergame: A sequence of rounds defines a supergame, which is of uncertain duration. Participants know they will play 18 rounds at which point, at the end of each round, a random device determines whether or not a new round will be played, with probability $\beta = 0.75$. This ensures a minimal common experience across sessions. Consumer and producer roles are randomly assigned in round 1, and deterministically alternate thereafter.

Counterparts in the supergame depend on the kind of economy: *fixed pairs* or *mixed groups*. In a *fixed pairs* economy, players have a fixed partner of their same type. Here, the economy is homogeneous, small (size 2), and there is perfect monitoring of actions since the counterpart is stable. Instead, at the start of a *mixed groups* economy, players are randomly assigned to 12-player groups with an equal mix of types, and the computer randomly forms six consumer-producer pairs at the start of each round (uniform probability). Here, the economy is heterogeneous, large (size 12), and players are "strangers" who can neither establish a reputation nor identify counterparts or monitor their past actions as counterparts are unstable. The counterpart's type is also hidden from producers (but not from consumers). To promote coordination, in each round players are informed if the outcome in their pair was the same as in all other pairs of their mixed group.

The economy's layout is payoff-relevant: we set $g = 0$ in fixed pairs and $g = a = 3$ in mixed groups. A mixed group is our way to model *economic integration*: relative to fixed pairs (autarky) cooperation payoffs are larger for everyone—with a capturing the potential benefit of integration—but additional frictions emerge due to larger group size, unstable and heterogenous counterparts, and poor monitoring of past conduct. Importantly, economic integration entails *inequality of (economic) opportunity* because of the gap in cooperation payoffs within the mixed group.

A session. Each session includes five supergames, starting and ending simultaneously for every player. The economy's layout, fixed or mixed, is exogenous in supergames 1-4 – to give players a common experience with the two kinds of economies – and endogenous in supergame 5. Participants are informed that supergames 1-2 have fixed pairs, and supergames 3-4 have mixed groups; we also study the reverse order "mixed-to-fixed." Matching across supergames is pre-arranged to minimize contagion effects: players are informed that they cannot be matched to counterparts from previous supergames except in the last supergame if there is a mixed group.

Before supergame 5 starts, players are provisionally assigned to a mixed group with all 24 participants. Then, they must privately express a preference for either (i) staying in the mixed group; (ii) scaling back its size to 16 by excluding one type from it; or (iii) leaving the group for a fixed pair. After this choice, the computer randomly selects one type with equal probability; the majority choice among these 8 players determines the group configuration in supergame 5. Two outcomes are possible: (i) there is a mixed group of 24 players or, (ii) there is a mixed group of 16 players (two types) and 4 fixed pairs (of the type excluded or that left).[4]

The group selection procedure is simple to understand and gives equal weight (ex-ante) to the opinion of each individual and type. A player choosing

[4]The instructions inform participants that they will have an opportunity to alter size and composition of their interaction set in supergame 5, without providing specific details until the end of supergame 4. Doing so minimizes the chance that behavior in the initial four supergames is affected by the intent to avoid a future possible exclusion.

"leave," intends to give up potential gains to ensure reciprocal interaction with a stable partner. If "exclude" prevails, this scales back the group size from 24 to 16 (neither of which was experienced before) without altering potential payoffs or monitoring structure in the mixed group, while lowering potential payoffs among the excluded. Hence, all else equal, "stay" is the socially efficient choice but it is (weakly) dominated by "exclude" because it creates no costs to those selecting it while offering two benefits: less strategic uncertainty (less players to coordinate with) a chance to mitigate free-riding, or punish past free-riding, by excluding the least cooperative type. Hence, an increased preference for "stay" relative to "exclude" signals greater trust and cohesion among participants.

Treatments. There are four treatments; see Table 2. In Baseline, potential gains from integration are equitably distributed, $a = 3$ for everyone, so integration does not remove the 4-point payoff gap between advantaged and disadvantaged. Two main treatments manipulate this payoff gap through a mean-preserving spread of the Baseline integration benefit a.

Table 2: Benefit of Integration and Inequality of Opportunity.

	Player type $i = 1, 2, 3$						
Treatments	a			$9 + 2i + a$			Payoff Gap
Baseline	3	3	3	14	16	18	4
No-Gap	5	3	1	16	16	16	0
Wide-Gap	1	3	5	12	16	20	8
High	5	5	5	16	18	20	4

Notes: Consumers' integration benefit a and cooperation payoffs in mixed groups, ordered left to right by type $i = 1, 2, 3$. Last column: cooperation payoff difference for consumer of type 3 and 1. Cooperation payoffs in fixed pairs are $9 + 2i$ points in all treatments.

No-Gap redistributes potential gains from integration top-to-bottom, $a = 5, 3, 1$ for type $i = 1, 2, 3$, so integration entails equal opportunity as cooperation payoffs are 16 points for every consumer. Instead, Wide-Gap introduces *inequity* in the distribution of potential gains from integration, $a = 1, 3, 5$, so integration entails more severe inequality of opportunity as the advantaged-disadvantaged payoff gap doubles to 8 points. In a supplementary High treatment, we raise a from 3 to 5 points for everyone, so we have a generalized improvement in economic prospects that does not affect the baseline payoff gap. For the experimental procedures see Appendix A.1.

4 Theoretical benchmark and hypotheses

In this setup many outcomes are possible. We start by explaining how many points a player can earn in a supergame and then discuss the efficient outcome.

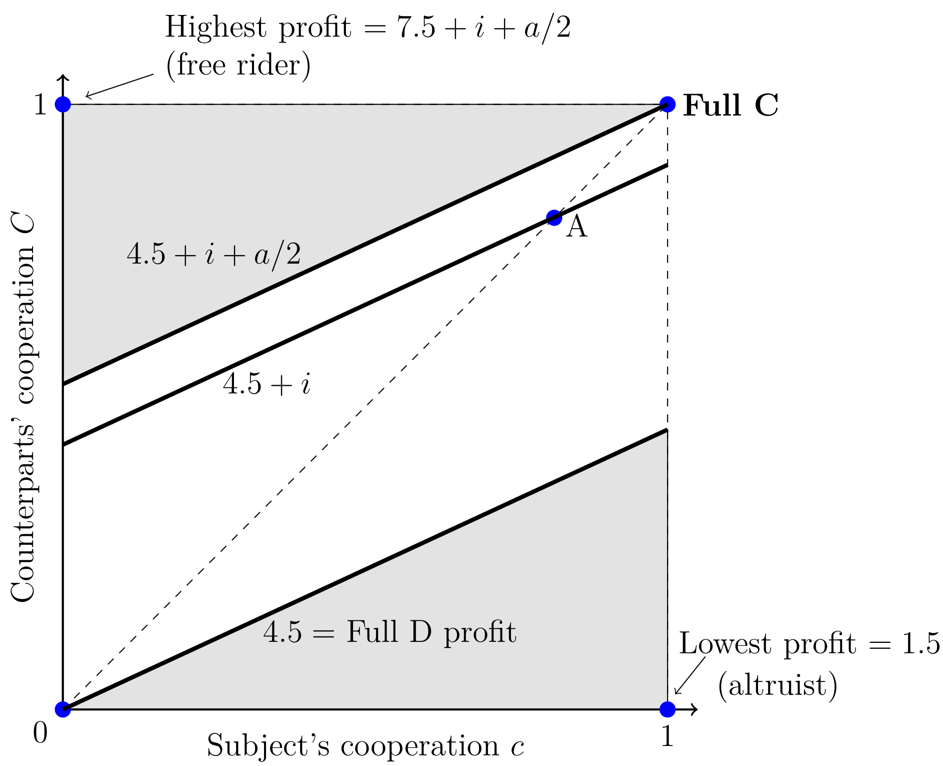
Let *profit* denote the points earned ex-post by a player in the average round of a supergame. For a player of type $i = 1, 2, 3$, profits range between 1.5 and $7.5 + i + a/2$ points. Indeed, each person alternates between consumer and producer roles, so the profit function is (approximately) the average payoff in two consecutive rounds; using Table 1, we define it as

$$\pi(c, C) = \frac{1}{2}\left[3 + (1 - c)6 + C\left(6 + 2i + a\mathbb{1}_{\text{mixed}}\right)\right].$$

Profits depend on the subject's *cooperation rate* $c \in [0, 1]$, i.e., the relative frequency of cooperation as a producer, the cooperation rate C of the average opponent, and the economy's layout (the indicator function $\mathbb{1}_{\text{mixed}} = 1$ in a mixed group and 0 otherwise). Defection gives 6 points to a producer and 3 to a consumer, which explains the term $3 + (1 - c)6$. Consumers earn additional $6 + 2i$ points if the counterpart cooperates, an amount that jumps by a points in a mixed group. This gives us the term $C\left(6 + 2i + a\mathbb{1}_{\text{mixed}}\right)$.

Fig. 1 traces iso-profit lines *in a mixed group*, and shows that subjects' choices determine if integration improves welfare, or not. Define two reference outcomes that entail symmetric and time-invariant choices: *Full Cooperation* ($c = C = 1$, the efficient outcome), and *Full Defection* ($c = C = 0$).

Figure 1: Iso-profit Lines for Type i Player in a Mixed Group.



Notes: Profit = payoff in the average round. Full defection gives 4.5 points. Full cooperation gives $4.5 + i$ points in fixed pairs and $4.5 + i + a/2$ in mixed groups. The lowest profit occurs if the subject always cooperates ($c = 1$) and every counterpart defects ($C = 0$). The highest profit occurs in mixed groups if the subject always defects and every counterpart cooperates ($c = 0, C = 1$). Point A corresponds to the cooperation rate $c = C \in (0, 1)$ that is needed to generate the profit under full cooperation in a fixed pair.

Suppose players act symmetrically. Full defection (axes origin) grants identical profits of (6+3)/2=4.5 points in any economy. As cooperation improves we move along the 45^0 line and profits in mixed groups grow proportionately, reaching a maximum of $4.5 + i + a/2$ (top-right corner) depending on the type;

the average full cooperation profit is 6.5 points in fixed pairs and 8 points in mixed groups of the main treatments. Point A, which identifies full cooperation profits in fixed pairs ($4.5 + i$ points), shows that mixed groups can attain larger profits than fixed pairs even with partial cooperation.

If cooperation is asymmetric, then profits have a wider range. Left of the 45^0 line we have profits of *free riders*. These players cooperate less than the average counterpart, thus raising their own earnings by disproportionately lowering others'. Right of the 45^0 line we have profits of *altruists*. Asymmetries in cooperation can generate profit heterogeneity within and across types and, in particular, profits that are larger in fixed pairs than mixed groups for some players but not others, as if economic integration created "winners" and "losers." Extreme cooperation asymmetries can push a free-rider's profit above the full cooperation level, and an altruist's profit below the full defection level; see the shaded areas in the figure. None of these outcomes are efficient.

Partial cooperation generates economic inefficiency. To calculate an efficiency measure, we first determine the *realized surplus* by subtracting the full defection profit from the average realized profit in the economy.[5] *Realized efficiency* is the ratio of realized surplus to its theoretical maximum, and is directly proportional to the average cooperation rate in the economy. It goes from 0% (full defection), to an average of 57% in fixed pairs (depending on the type), to 100% in mixed groups. Hence, economic integration can substantially improve welfare of each individual. Therefore, if mixed groups support efficient play, then choosing a mixed group with all 24-players is the Pareto-efficient choice. But is efficient play part of an equilibrium?

Proposition 1. *Full cooperation is a sequential equilibrium in each economy, and in every treatment.*

[5] The maximum surplus in fixed pairs varies between 1 and 3 points in fixed pairs (5.5-4.5 and 7.5-4.5), and it is 3.5 points (8-4.5) in mixed groups of the main treatments.

Proof. See Appendix A □

To prove the proposition we demonstrate the existence of a strategy that supports efficient play in fixed pairs as well as in mixed groups of any size. The proof is a version of Kandori (1992, Proposition 1), extended to the case of heterogeneous players. Consider the following trigger strategy: the player always cooperates as a producer, but will forever defect as soon as some producer defects. Since at the end of each round everyone sees if outcomes differed across meetings, defecting in cooperative equilibrium triggers an immediate and permanent collective sanction: full defection. Full defection is always an equilibrium in the continuation game since defection is always a best response to everyone else defecting. Hence, it is incentive-compatible for a player to follow this collective sanction. In the proof of Proposition 1 we derive a condition ensuring that the player has no incentive to deviate in equilibrium, i.e., the continuation probability $\beta \geq \beta^* := 6/(6 + 2i + a\mathbb{1}_{\text{mixed}})$. The threshold value β^* varies across player types and economy's layout (see Table 3).

Table 3: Threshold continuation probability β^*.

	Fixed Pair			Mixed Group		
Treatment	i =1	2	3	i =1	2	3
Baseline	.75	.60	.50	.55	.46	.40
No-Gap	.75	.60	.50	.46	.46	.46
Wide-Gap	.75	.60	.50	.67	.46	.35
High	.75	.60	.50	.46	.40	.35

If the cooperative strategy is a best response for disadvantaged players (who have the least incentive to cooperate), then this is so for everyone else. In fixed pairs $\beta^* = 0.75$ for the disadvantaged, a threshold that drops in mixed groups since cooperation payoffs increase for all. As the continuation probability is 0.75, full cooperation is part of a sequential equilibrium in all economies.

Corollary 1. *In all treatments, economic incentives to cooperate exist for all players and are the largest in mixed groups. The treatment manipulations do not alter the equilibrium set.*

It should be clear that there are many symmetric equilibria, including full defection and partial cooperation, but full cooperation is the Pareto-dominant one. If Pareto efficiency is the empirically relevant equilibrium selection criterion, then payoff-maximizing players should coordinate on full cooperation, and prefer economic integration (the 24-player group) over other group configurations. However, it is unclear what equilibrium (if any) players will select in the experiment because the theory does not generate a specific prediction regarding cooperation rates and group choices. Based on the arguments developed above, we thus put forward three hypotheses:

H 1. *Cooperation is unaffected by the size of prospective earnings.*

H 2. *Cooperation is unaffected by inequality in prospective earnings.*

H 3. *The decision to "integrate" is unaffected by treatments.*

On the other hand, experiments reveal that strangers struggle to cooperate, even if they are completely homogeneous (Camera et al., 2013), while, broadly speaking, cooperation tends to improve when the economic incentives become more favorable (Dal Bó and Fréchette, 2018). An increasing body of evidence also suggests that income distribution fundamentally influences human behavior (Cappelen et al., 2014); in particular, economic inequality hinders coordination on efficient play in finitely and indefinitely repeated social dilemmas (Camera et al., forth.; Tavoni et al., 2011), and fosters short-sighted conduct both in the lab and in the field (Haushofer and Fehr, 2014). These considerations suggest two alternative behavioral hypotheses: (i) larger prospective payoffs should foster cooperation, and (ii) inequality of opportunity should magnify opportunistic conduct and reduce cooperation.

5 Results

Here we discuss outcomes in supergames 1-4 and then show how they affected group choices, starting with the Baseline treatment.

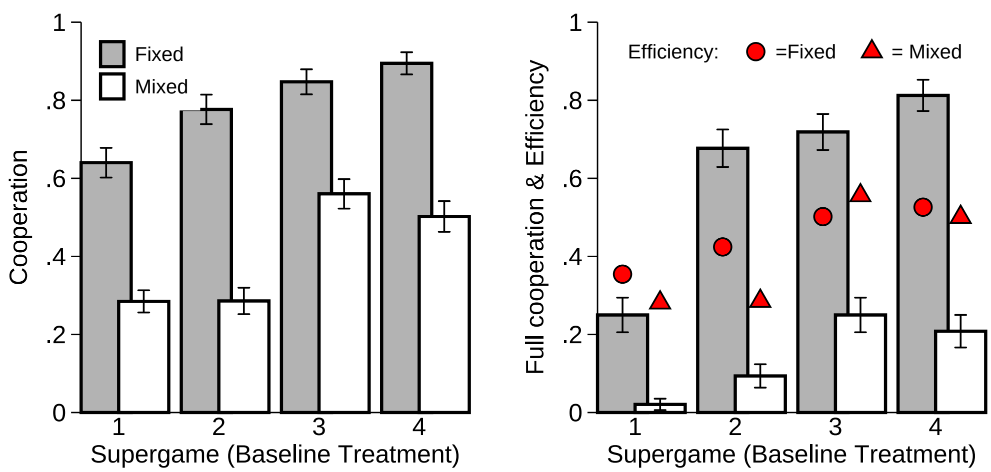
5.1 The baseline case

In this treatment there is a 4-points gap between prospective payoffs of advantaged and disadvantaged consumers, and the potential gains from economic integration are identical across types, $a = 3$.

Result 1. *Mixed groups cooperated less than fixed pairs.*

Fig. 2 and non-parametric tests provide evidence. In Fig. 2 the unit of observation is one subject in a supergame (N=192 per supergame 1-4).

Figure 2: Cooperation in the Baseline treatment



Notes: One obs.=one subject in a supergame 1-4, rounds 1-18 (N = 384 per economy, fixed pairs and mixed groups). *Cooperation:* average relative frequency of cooperation in a supergame. *Full Cooperation:* relative frequency of full cooperation in a supergame (=1 if the subject as a producer cooperated in every round, 0 otherwise). *Efficiency:* average realized efficiency in the supergame. The whiskers identify the standard error of the mean.

The left panel reports the average cooperation rate, the right panel the relative frequency of full cooperation, for fixed pairs and mixed groups.[6] Mixed groups are the least cooperative: the average cooperation rate is 0.79 in fixed pairs, and 0.41 in mixed groups (0.61 and 0.14, respectively, for full cooperation). These differences are statistically significant when we use a session as the independent unit of observation (two-sided Wilcoxon-Mann Whitney ranksum tests with exact statistics where, p-values < 0.001, N1=N2=8). Result 1 confirms the robustness of the findings in Bigoni et al. (2019); Camera et al. (2013), where players are homogeneous.

Fig. 2 also reveals that experience with the game matters. Subjects either experienced two consecutive supergames of economies as fixed pairs followed by economies as mixed groups (4 sessions), or the reverse ordering (4 sessions). For the group size, cooperation is greater in supergames 3-4 as compared to 1-2 according to non-parametric tests[7] and a GLM regression (Table B1, in Appendix B.1)[8] that also supports an additional result.

Result 2. *Cooperation was stable in fixed pairs, but unstable in mixed groups.*

Fig. 3 reports the mean cooperation rate by round of play (all rounds in supergames 1-4, all subjects). In fixed pairs, cooperation is stable until round 18, when the random termination process starts. Instead, in mixed groups cooperation declines; see Table B1, in Appendix B.1 for additional

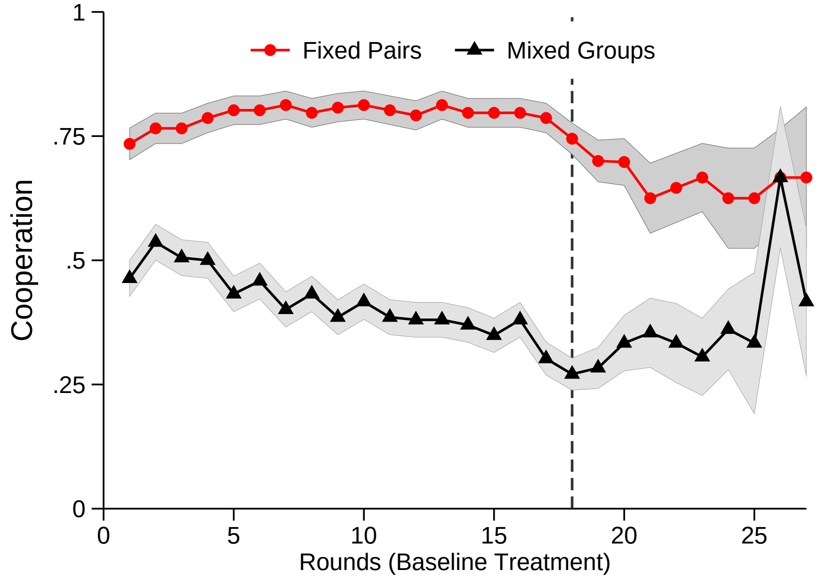
[6] Let $c_t = 1$ denote a cooperative action by a player who is a producer in period t (0, if defection). Let t_p be the number of periods in which this player was a producer in the supergame. The *cooperation rate* for this player is $\sum_{t=1}^{t_p} c_t/t_p \in [0, 1]$. If we let π_t denote the payoff to a player in round t and T be the total number of periods of the supergame, then the profit realized by the player in the average period of the supergame is $\sum_{t=1}^{T} \pi_t/T$. Since supergames' duration varies (between 18 and 36 rounds, expected 21 rounds, with s.d. 3.45), we focus on the first 18 rounds (approximately 86% of all observations).

[7] Two-sided Wilcoxon-Mann Whitney ranksum tests; cooperation p-values=0.029, 0.086 and full cooperation p-values=0.029, 0.114 for fixed pairs and mixed groups, N1=N2=4).

[8] Please contact the authors for supplementary material.

details. Taken together, results 1-2 reveal that participants faced additional frictions in realizing the possible gains from trading in an integrated economy as opposed to autarky. These frictions reduced the willingness to cooperate and, hence, did not allow society to benefit from economic integration.

Figure 3: Cooperation Dynamics in the Baseline treatment



Notes: One obs.=average subject in a round of supergames 1-4, all rounds. *Cooperation:* relative frequency of cooperation averaged across all subjects in all supergames. The shaded areas identify the standard error of the mean.

Result 3. *Mixed groups did not attain greater efficiency than fixed pairs.*

Fig. 2 and Table 4 provide evidence. Realized efficiency is proportional to average profits in an economy, as defined in Section 4. Full defection profits are 4.5 points (0% efficiency), while full cooperation profits are on average 6.5 points in fixed pairs (57% efficiency) and 8 points in mixed groups (100% efficiency). In Baseline, profits are statistically similar according to a linear regression (6 points in fixed pairs and 5.9 in mixed groups).[9] We regress average profits in a supergame on a *Mixed Group* dummy that is one for a mixed group (0, for a fixed pair, the base of the regression). We also add an *Experienced* dummy taking value 1 in supergames 3-4 (0, otherwise, the base of the regression), which we interact with the *Mixed Group* dummy in order to control for order effects. Standard controls are included.

Table 4: Realized Profits in Partnerships vs. Mixed groups in Baseline.

Dep. var.: realized profit	Coeff.	S.E.
Mixed Group	-0.090	(0.076)
Experienced	0.610***	(0.135)
Mixed Groups × Experienced	-0.036	(0.133)
Controls	Yes	
Constant	6.340***	(1.235)
N	32	
R^2	0.874	
adj R^2	0.822	

Notes: Linear regression on realized profit, with standard errors (S.E.) robust for clustering at the session level. One obs.= one supergame in a session, supergames 1-4, rounds 1-18. *Mixed Group*=1 if it was mixed groups (0, otherwise), *Experienced*=1 if the supergame is 3 or 4 (0, otherwise). *Controls* include duration of the current and previous supergame, self-reported sex, and three measures of understanding of instructions (response time and wrong answers in the quiz, and self-reported instruction clarity in the final survey). Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively.

We can reject the hypothesis that, when subjects are inexperienced (supergames 1-2) mixed groups attained greater efficiency than fixed pairs. The *Mixed Group* coefficient is negative, close to zero and insignificant. We can similarly reject this hypothesis for experienced subjects (supergames 3-4); the sum of the coefficients *Mixed Group*+*Mixed Group* × *Experienced* is also neg-

[9]This is confirmed if we use a session as the independent unit of observation (two-sided Wilcoxon-Mann Whitney ranksum tests with exact statistics, p-value=0.574, N1=N2=8).

ative and statistically close to zero (F-test, p-value=0.437). The results do not change if we include all rounds in a supergame.

In sum, economic integration failed to increase average payoffs due to a general decline in cooperation. The natural questions are whether conduct in mixed groups differed across player types and whether integration was economically beneficial for some but not others.

Result 4. *The advantaged tended to cooperate more than the disadvantaged.*

Tables 5-Table 6 provide support. Table 5 reports average cooperation rates by type, kind of economy, and order of play. The advantaged cooperated more than others in sessions that started with mixed groups (panel b) but this behavior is not systematic because it is not observed when sessions started with fixed pairs (panel a). We establish the significance of this observation through GLM regressions. We regress average cooperation rate in a supergame on player type dummies (type 2 is the base of the regression). Standard controls are included. Marginal effects are in Table 6.

Table 5: Cooperation by Player Type and Order of Play.

	(a) Fixed-to-Mixed			(b) Mixed-to-Fixed		
	Disadv.	Middle	Adv.	Disadv.	Middle	Adv.
Fixed Pair	0.81	0.66	0.65	0.80	0.84	0.97
Mixed Group	0.54	0.55	0.51	0.22	0.26	0.37

Notes: One obs.=one subject in a supergame 1-4, Baseline treatment, rounds 1-18.

Considering all data pooled together (col. 1 and 4), we find no significant difference in cooperation across types (Wald tests on coefficients on type dummies for columns 1 and 3, p-values=0.552,0.346, respectively). Once we separate the data based on the order of play, we only find differences in sessions starting with mixed groups, where the advantaged cooperated significantly more than the disadvantaged (Wald tests, p-values < 0.001, col. 3 and 6). Hence, H1 can be rejected only for sessions that started with mixed groups. We now discuss the consequences of these cooperation differentials.

Table 6: Cooperation by Player Type in Baseline–Marginal Effects.

	Fixed Pairs			Mixed Groups		
Dep. var.: coop. rate	All (1)	2-to-12 (2)	12-to-2 (3)	All (4)	2-to-12 (5)	12-to-2 (6)
Disad.	0.036	0.061	-0.048	-0.027	-0.010	-0.037
	(0.065)	(0.063)	(0.093)	(0.045)	(0.079)	(0.051)
Adv.	0.074	-0.030	0.138	0.031	-0.059	0.139***
	(0.061)	(0.044)	(0.111)	(0.049)	(0.092)	(0.029)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	192	96	96	96	48	48

Notes: GLM regressions on avg. cooperation rate; standard errors (in parentheses) robust for clustering at session level. One obs.=one supergame 1-4, Baseline, rounds 1-18. *Disad.* =1 one for a type 1 player (0, otherwise); *Adv.*=1 for a type 3 player (0, otherwise); type 2 is the base value. Col. 1 and 4: all sessions; Col. 2 and 5: sessions with 2-2-12-12 order; Col. 3 and 6: sessions with reversed order of economies. *Controls* include duration of current and previous supergame, self-reported sex, and three measures of understanding of instructions. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level.

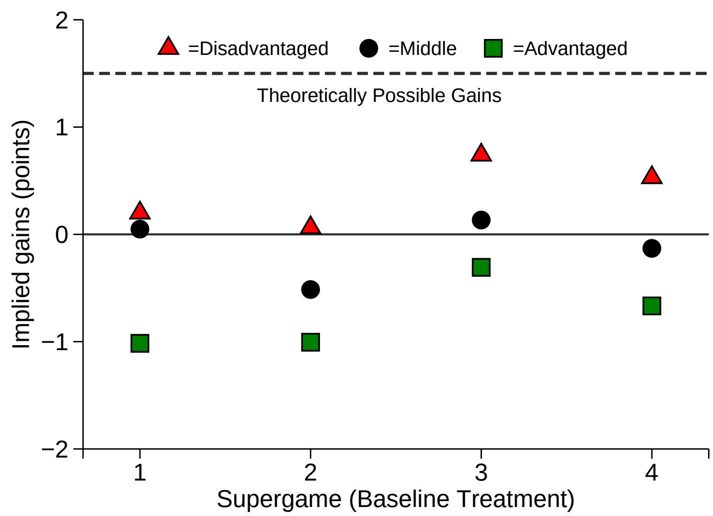
Result 5. *Economic integration created winners and losers, implying relative gains for the disadvantaged but relative losses for the advantaged.*

Fig. 4 reports the *implied* gains from integration. Fixing a supergame $n = 1, \dots, 4$, we ask: would the average type i player gain or lose points if she were in a mixed group instead of a fixed pair? Each marker reports the average difference in realized profits.[10] As all consumers could earn 3 extra points in mixed groups, the potential gain is 1.5 points for every type (dashed line). Empirically, implied gains are below this value, and inversely related to the type—negative for the advantaged (squares) and positive for the disadvantaged

[10]Hence, this profit difference is calculated between-subjects, and is not biased by the order of economies within a session, as a within-subjects measure would be.

(triangles). These differences are statistically significant according to a linear regression (see Table B2 in Appendix B). An interpretation is that economic integration in the experiment created winners and losers across types.[11]

Figure 4: Implied Gains from Economic Integration.



Notes: One obs.=one subject in a supergame 1-4, Baseline treatment, rounds 1-18. *Implied gains:* difference in realized profits between mixed groups and fixed pairs, by supergame.

We study how this experience affected subjects' choice of economic organization by pooling the two exclusion choices into a single one called "exclude." We thus have three possible group choices: "stay" in the 24-player mixed group (economic integration), scale it back to 16 players, or "leave" it (autarky).

Result 6. *"Leave" was the majority choice, while "stay" was the least frequent.*

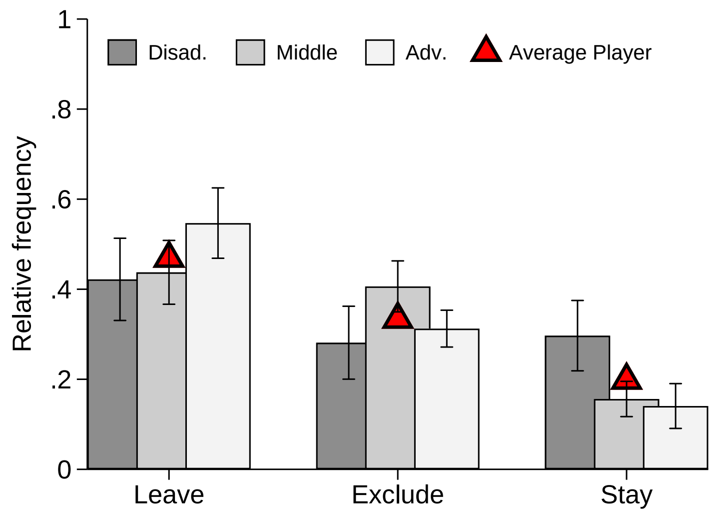
A related observation is that a subject's experience in supergames 1-4 explains variation in group choices, while differences in prospective payoffs (i.e.,

[11] There are also winners and losers within the same type. If we interpret a type as a country, then this observation would seem consistent with international trade theories. Yet, these differences within a type are due to unequal cooperation rates, with frequent cooperators being "losers" and frequent defectors "winners."

the player's type) do not; see Fig. 5, and Table 7.

Fig. 5 reports the distribution of group choices by type. The choice frequency is inversely related to the group size. Fixed pairs are the majority choice; we cannot reject the hypothesis that the frequency is equal to 50% (two-sided t-test, p-value=0.584, N=8). We can also reject the hypothesis that "leave" is as frequent as "exclude," and "exclude" is as frequent as "stay" (two-sided Wilcoxon-Mann Whitney ranksum test with exact statistics, p-values=0.046, 0.044, N1=N2=8). This evidence matches that in Bigoni et al. (2019) where, however, players are homogeneous and cannot exclude others.

Figure 5: Distribution of group choices by player type in Baseline.



Notes: One obs.= one session of Baseline ($N = 8$). Relative frequency of choice "leave", "exclude" and "stay." The whiskers identify the mean standard error.

The fact that players prefer "exclude" over "stay" is intriguing because scaling back the size of the mixed group dissipates potential surplus (while "stay" is socially optimal) and cannot improve the player's prospective payoffs. However, it can decrease strategic uncertainty and be used as a form of sanction. Hence, a natural question is what explains group choices and whether the advantaged chose differently than the disadvantaged.

Table 7: Group Choices in Baseline–Marginal Effects

Dep. variable= group choice	Leave (1)	Exclude (2)	Stay (3)
Disadvantaged	0.070	-0.122	0.052
	(0.142)	(0.133)	(0.048)
Advantaged	0.048	-0.057	0.008
	(0.090)	(0.085)	(0.038)
Δ Profit	-0.173***	0.105*	0.068**
	(0.061)	(0.056)	(0.029)
Cooperation Balance	-0.017	0.005	0.012
	(0.080)	(0.072)	(0.014)
100% C in Fixed Pairs	0.118	-0.084	-0.034
	(0.134)	(0.126)	(0.028)
100% C in Mixed Groups	-0.482***	0.327***	0.155
	(0.141)	(0.110)	(0.126)
Mixed-to-Fixed	-0.160**	-0.027	0.187***
	(0.067)	(0.085)	(0.067)
N	192	192	192

Notes: Multinomial logit regression on preferences for the three group choices. One obs.=one subject in a session of Baseline. Robust standard errors (in parentheses) adjusted for clustering at session level. The regressors Δ *Profit* and *Cooperation Bias* are standardized, and together with the categorical variables *100% C in Fixed Pairs* and *100% C in Mixed Groups* capture the subject's experience in supergames 1-4. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively.

A multinomial logit reveals that experience, not prospective gains, explains observed variation in group choices. We regress a player's group choice on the dummies *Advantaged* and *Disadvantaged* (type 2 is the base). Four explanatory variables capture the subject's experience in the first four supergames: the continuous regressor Δ*Profit* is the (standardized) difference in profits realized in mixed groups vs. fixed pairs; *Cooperation Bias* is the (standardized) difference between the player's cooperation rate in mixed groups and that of her opponents; the dummy variables *100% C in Fixed Pairs* and *100% C in Mixed Groups* capture the effect of experiencing efficient play. The dummy *Mixed-to-Fixed* soaks up the order effect (fixed pairs first, or mixed groups). Standard controls are included. Table 7 reports marginal effects.

The player type dummies have equal sign, are statistically similar and indistinguishable from zero (Wald tests). Hence, having different economic prospects does not *per se* affect group choices.[12] What matters is individual experience (supergames 1-4). A positive profit differential and experience of efficient play in a mixed group both significantly lower the probability of choosing "leave." However, they do so to different degrees and for different reasons. A one standard deviation increase in $\Delta Profit$ lowers the probability of "leave" by 17 ppts., raising the probability of "exclude" by 10 and of "stay" by 7 ppts. This captures the choice of free-riders, who can more easily extract rents in mixed groups than fixed pairs—where negative reciprocity is possible. Experiencing efficient play in a mixed group lowers the probability of "leave" by 49 ppts., while raising that of "exclude" by 33 ppts.; those who met cooperative strangers, tried to replicate this favorable outcome by choosing a 16-player group to mitigate strategic uncertainty.[13] This result is in line with Bigoni et al. (2019), who show that homogeneous players choose large or small groups based on their individual cooperation experience in those groups.

To sum up, when economic integration offered equal potential gains to all, the choice of economic organization was similar across types and primarily tied to past personal experience—not prospective payoffs. Did reshuffling the

[12] Using a session as an independent observation, we cannot reject the hypothesis that each group choice is equally frequent for any two player types (two-sided Wilcoxon-Mann Whitney ranksum test with exact statistics, p-values ranging from 0.170 to 0.625, for the nine different pairwise comparisons, N1=N2=8).

[13] *100% C in Mixed Groups*=1 in 8 of 192 obs.; *100% C in Fixed Pairs*=1 in 138 of 192 obs.

prospective gains from integration alter this result?

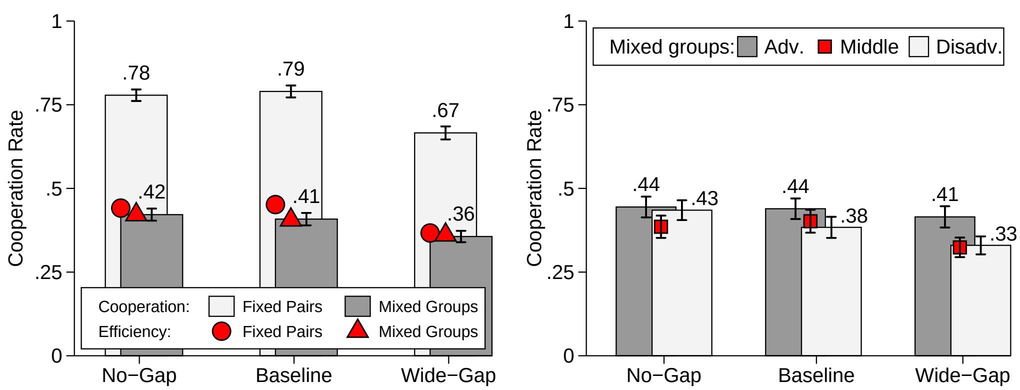
5.2 The influence of inequality of opportunity

In No-Gap economic integration no longer implies inequality of opportunity because prospective gains a are redistributed from the advantaged to the disadvantaged. Instead, inequality of opportunity is extreme in Wide-Gap because the distribution of prospective gains is iniquitous: the advantaged disproportionately benefit at the expense of the disadvantaged; see Table 2.[14]

Result 7. *Cooperation and profits are lower in Wide-Gap than in No-Gap.*

Evidence is in Fig. 6 and Table 8. The left panel in Fig. 6 reports average cooperation and realized efficiency by group size, pooling supergames 1-4.

Figure 6: Inequality of Opportunity Affects Cooperation



Notes: One obs.=one subject in a supergame 1-4, rounds 1-18 ($N = 384$ per type of economy, fixed pairs and mixed groups). Left panel: overall cooperation rate in mixed groups and fixed pairs. Right panel: average relative frequency of cooperation in a supergame of mixed groups, by subject type. See notes to Fig. 2 for additional details.

[14]Considering each treatment in isolation, we find that outcomes in supergames 1-4 are broadly consistent with Results 1-4 for BASELINE. Evidence is provided in Appendix 7.

Inequality of opportunity in mixed groups increases as we move across treatments, left to right; the mixed-group payoff gap between advantaged and disadvantaged players increases from 0 (No-Gap), to 4 points (Baseline), to 8 points (Wide-Gap); see Table 2. Three observations stand out. Cooperation is the lowest in Wide-Gap, not only in mixed groups but also in fixed pairs (even if treatments did not alter any parameter of fixed pairs, but only of mixed groups). This suggests that the overall structure of the session influenced the structure of incentives, not just the structure of the specific supergame. Second, the cooperation decline is nonlinear; we see no drop as we increase the gap from 0 to 4 points, but a sharp drop when we double the payoff gap in mixed groups to 8 points. Third, in relative terms, the drop in cooperation is similar in mixed groups and fixed pairs (about a 14% decline).

GLM regressions reveal a significant cooperation decline for the average subject. Panel A in Table 8 reports marginal effects. The dependent variable is the cooperation rate of a subject in supergames 1-4, which we regress on two treatment dummies (Baseline is the base value) each of which is interacted with an order dummy and an experience dummy taking value 1 it is the second supergame with the same economy size. Standard controls are included.

Cooperation increased relative to the baseline case when mixed groups had no inequality of opportunity (positive coefficient on the No-Gap dummy), but this is statistically significant only in fixed pairs. Conversely, cooperation declined relative to the baseline case when we doubled payoff gaps in mixed groups (negative coefficient on the Wide-Gap dummy). This effect is significant only in mixed groups. We can reject the hypothesis that the coefficients on these two dummy variables are similar (Wald tests, p-values=0.037, 0.033 for fixed pairs and mixed groups, respectively). Hence, we can reject H2.[15] The

[15] If we use a session as the independent unit of observation and two-sided tests, then we

cooperation decline that resulted when we doubled payoff gaps in mixed groups caused a decline in average profits. The natural question is: did all player types decreased their cooperation, or only some?

Table 8: Inequality of Opportunity and Cooperation–Marginal Effects

Dep. var.:	Fixed Pairs		Mixed Groups	
Coop. rate	Coeff.	S.E.	Coeff.	S.E.
Panel A: All Player Types				
No-Gap	0.123***	(0.032)	0.026	(0.070)
Wide-Gap	-0.055	(0.083)	-0.201*	(0.112)
Panel B: Disadvantaged Players				
No-Gap	0.004	(0.088)	0.001	(0.065)
Wide-Gap	-0.106	(0.085)	-0.279***	(0.098)
Panel C: Middle Players				
No-Gap	0.127*	(0.067)	0.003	(0.100)
Wide-Gap	-0.001	(0.118)	-0.247*	(0.132)
Panel D: Advantaged Players				
No-Gap	0.190***	(0.053)	0.047	(0.073)
Wide-Gap	-0.083	(0.113)	-0.103	(0.132)

Notes: GLM regression on avg. cooperation rate; standard errors (S.E.) robust for clustering at session level. Data from supergames 1-4 of the three main treatments, rounds 1-18. One obs.=one subject in a supergame (N=128 per player type, per treatment). No-Gap and Wide-Gap are treatment dummies (Baseline is the base). The treatment dummies are interacted with two dummy variables (not reported): an order dummy taking the value 1 if the session started with mixed groups (0, otherwise), and an experience dummy taking the value 1 if it is the second supergame with the same economy size (0, otherwise). We also include standard controls (not reported): duration of the previous supergame, self-reported sex (average in the economy), and two measures of understanding of instructions (average response time and average wrong answers in the quiz). Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively. Marginal effects are computed at the mean value of regressors of continuous variables.

Result 8. *Disadvantaged and middle types reduced cooperation as payoff gaps widened in mixed groups, while the advantaged did not.*

cannot reject the hypothesis that cooperation rates in mixed groups are similar across treatments (two-sided ranksum tests with exact statistics, p-values between 0.328 and 0.937 for each of the three treatment comparisons, N1=N2=8). The same holds true for fixed pairs (p-values between 0.185 and 0.900). Similar considerations can be made if we consider full cooperation and realized efficiency.

A related observation is that we observe treatment effects in fixed pairs for Advantaged and Middle players even if we anticipated none, because treatments only manipulated the payoffs gaps in mixed groups.

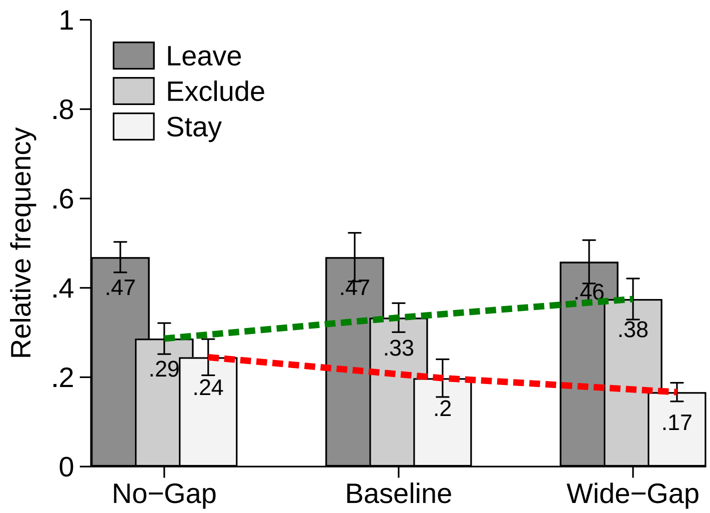
Evidence is in Fig. 6 and Table 8. The right panel in Fig. 6 reports average cooperation in mixed groups separately for each player type, pooling supergames 1-4 for each treatment. Moving from No-Gap to Wide-Gap, generates a marked decline in cooperation only the disadvantaged and middle players (10 and 7 percentage points, respectively). The regressions in panels B-D in Table 8 confirm that this decline is statistically significant, while it is insignificant for advantaged players (the treatment dummies in Panel D, for mixed groups are statistically similar, Wald test p-value=0.221). Hence, we can reject H1. What is surprising is that inducing inequality of opportunity in mixed groups induced a cooperation decline in fixed pairs, for some player types, even if the payoff matrix in fixed pairs never changed across treatments. This suggests that inequality of opportunity negatively affected cooperative attitudes in the entire session, not just in the specific supergame in which it was introduced. The next result provides additional support for this view.

Result 9. *The frequency of "leave" was unaffected as payoff gaps widened in mixed groups. By contrast, "exclude" crowded out "stay."*

Fig. 7, statistical tests and the regressions in Table 9 provide support. The figure reports relative frequencies of group preferences, by treatment. The choice "leave" is unaffected by treatments; we can neither reject the hypothesis that it is equal to 50% (two-sided t-test, p-values=0.390, 0.420 for No-Gap and Wide-Gap), nor that it is similar in pairwise treatment comparisons (two-sided Wilcoxon-Mann Whitney ranksum test with exact statistics, lowest p-value=0.775, N1=N2=8).

Second, removing payoff inequality from mixed groups shifts frequency mass from "exclude" to "stay." We cannot reject the hypothesis that "leave" is more frequent than "stay" (p-values < 0.001 for No-Gap and Wide-Gap, N1=N2=8). However, while in No-Gap "exclude" is as frequent as "stay" (p-value=0.589, N1=N2=8), in Wide-Gap it is more frequent than "stay" (p-value=0.005, N1=N2=8) and as frequent as "leave" (p-value=0.390, N1=N2=8).

Figure 7: Inequality of Opportunity Influenced Group Choices.



Notes: One obs.= one session ($N = 8$ per treatment). Relative frequency of choice "leave", "exclude" and "stay." The whiskers identify the mean standard error.

Given the direction of the observed shifts, we can reject the hypothesis that "exclude" is greater than or equal in Wide-Gap as compared to No-Gap, and can reject the opposite hypothesis for "stay" (one-sided Wilcoxon-Mann Whitney ranksum tests with exact statistics, p-value=0.082, 0.053 for "exclude" and "stay" respectively, N1=N2=8). Overall, this is evidence that a 24-player group was more frequently selected when mixed groups removed inequality of opportunity (No-Gap) as compared to when they enhanced it (Wide-Gap). Hence, we can reject H3.

Table 9: Group Choices in the Main Treatments–Marginal Effects

Dep. variable= group choice	Leave (1)	Exclude (2)	Stay (3)
No-Gap	-0.017	-0.040	0.057
	(0.060)	(0.035)	(0.059)
Wide-Gap	0.007	0.033	-0.040
	(0.061)	(0.040)	(0.047)
Δ Profit	-0.247***	0.127***	0.119***
	(0.042)	(0.023)	(0.042)
Cooperation Bias	-0.048	0.050*	-0.003
	(0.039)	(0.030)	(0.023)
100% C in Fixed Pairs	0.095	-0.016	-0.079**
	(0.061)	(0.056)	(0.037)
100% C in Mixed Groups	-0.327***	0.075	0.252**
	(0.110)	(0.075)	(0.115)
Mixed-to-Fixed	-0.146***	0.029	0.117**
	(0.042)	(0.037)	(0.048)
N	576	576	576

Notes: Multinomial logit regression on preferences for the three group choices. Data for the main treatments. No-Gap and Wide-Gap are treatment dummies (Baseline is the base). For other details see notes to Table 7.

We use a multinomial logit regression to find out if this result is confirmed once we control for variation in subjects' experience across treatments. To do so, we adjust the specification of the regression in Table 7, by pooling the data from all main treatments, omitting the type covariates and instead adding treatment covariates (Baseline is the base of the regression). Marginal effects are in Table 9. The regression confirms that once we control for experience, there is no residual treatment effect on the frequency of "leave;" the coefficients on the treatment dummies are close to zero, insignificant, ad statistically similar (Wald test, p-value=0.667). Instead, there is a significant effect on the frequency of "stay," which increases by about 6 percentage points in No-Gap relative to Baseline, while it falls by 4 percentage points in Wide-Gap.

These differences in marginal effects are statistically significant (Wald test, p-value=0.061). The variation in "stay" is balanced by the opposite variation in "exclude," although we cannot reject the hypothesis that treatment coefficients are similar in this case (Wald test, p-value=0.124). An interpretation is that the support for economic integration increases when the process is known to be beneficial *and* to reduce extant differences in economic inequality across countries. The converse is true when integration widens economic gaps.

To understand if treatments differentially affect types's choices, we study the "stay" choice using a separate logit regression for each type, pooling data from all main treatments. The treatment coefficients in columns 1 and 3 are statistically similar (Wald tests), meaning that types 1 and 3 act similarly across treatments; instead, we do find that type 2 players more frequently select "exclude" in No-Gap than the other two treatments. Support for this is Table B7 and Fig. 9 of Appendix B.

Summing up, a majority of players preferred fixed pairs to any other group configuration. Removing inequality of opportunity from mixed groups did not change this result—although it improved cohesiveness and trust, as seen from the shift in preferences from "exclude" to "stay." The open question is whether group preferences are elastic to a generalized increase in *prospective* gains from integration. To answer this question we study the High treatment, where we raised a by 67% relative to the baseline setup.

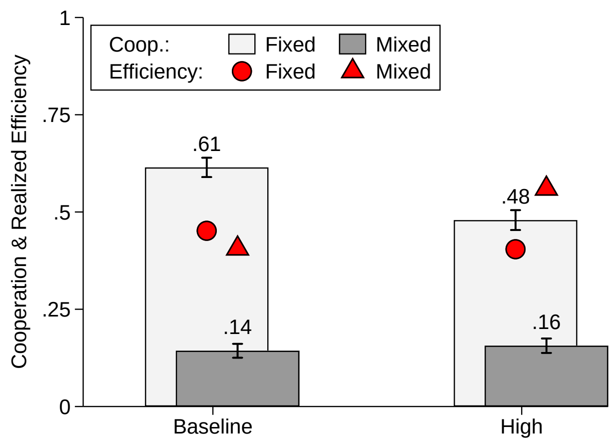
5.3 An increase in potential gains from integration

In High, every consumer can earn 5 more points in mixed groups than fixed pairs. This boosts the economic incentives for cooperation in mixed groups.

Result 10. *Cooperation did not increase in High as compared to Baseline.*

A related observation is that cooperation declined in fixed pairs. Evidence is provided in Fig. 8 and the GLM regression reported in Table B9.

Figure 8: Effect of an Increase in Prospective Payoffs on Cooperation



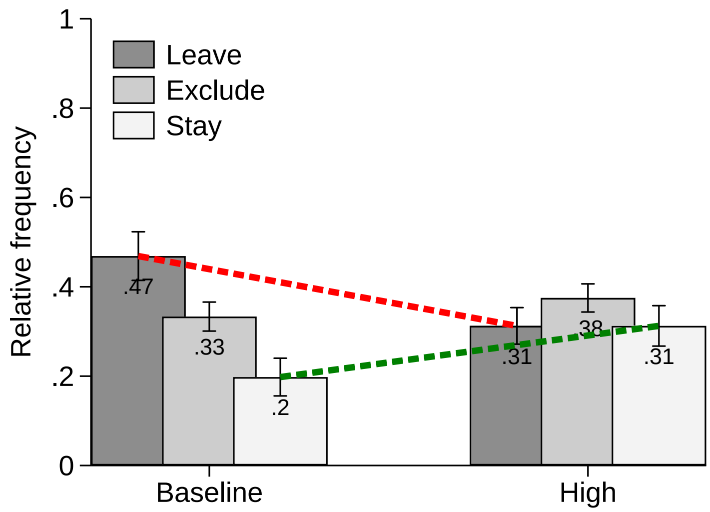
Notes: One obs.=one subject in a supergame 1-4, rounds 1-18 ($N = 384$ per economy kind, fixed pairs and mixed groups). See notes to Fig. 2 for additional details.

Using a session as an independent observation, we can reject the hypothesis of identical cooperation in Baseline and in High for fixed pairs, but not for mixed groups (two-sided Wilcoxon-Mann Whitney ranksum test with exact statistics, p-values=0.061,0.488, N1=N2=8). A GLM regression confirms this result; see Table B9 in Appendix B. Hence, we cannot reject H1 because cooperation did not change in mixed groups. The fact that there was a treatment effect in fixed pairs is surprising because the structure of payoffs was unaltered relative to the baseline setup. Though raising a did not improve cooperation rates, it did raise profits and hence realized efficiency in mixed groups relative to fixed pairs (see Fig. 8). As a result, integration created mostly winners (average implied gains are 0.97, 0.77, and -0.11 points for types 1,2,3, respectively), which affected group choices.

Result 11. *"Leave" was less frequently selected in* High *as compared* Baseline*, while the frequency of "stay" and "exclude" each increased.*

Evidence comes from Fig. 9 and the multinomial logit regression in Table 10, showing that group choices were affected by the High treatment.

Figure 9: Effect of an Increase in Prospective Payoffs on Group Choices



Notes: Distribution of choices for "leave" (left), "exclude" and "stay" (right). One obs. = one session (N=8 per treatment).

Fig. 9 shows that "leave" declined by 16 ppts in HIGH, while "stay" increased by 11 ppts.[16] As seen before, the shift in probability mass from "leave" towards "exclude" and "stay" is primarily influenced by the difference in realized profits in Mixed Groups vs. Fixed pairs. A one-standard deviation increase in the coefficient on the Δ *Profit* regressor reduces the probability to "leave" by 20 percentage points, while it similarly increases the probabilities to select "exclude" and "stay" (Wald test, p-value=0.703).

[16] Using a session as an independent observation, a two-sided test rejects the hypothesis of identical "leave" choices in Baseline and High, but not "stay" (two-sided Wilcoxon-Mann Whitney ranksum test with exact statistics, p-values=0.047,0.165, N1=N2=8).

Table 10: Group Choices in High vs. Baseline–Marginal Effects

Dep. variable= group choice	Leave (1)	Exclude (2)	Stay (3)
High	-0.103	0.048	0.055
	(0.071)	(0.055)	(0.041)
Δ Profit	-0.201***	0.090**	0.111***
	(0.044)	(0.037)	(0.034)
Cooperation Bias	-0.037	0.024	0.013
	(0.048)	(0.041)	(0.017)
100% C in Fixed Pairs	0.182***	-0.145**	-0.037
	(0.066)	(0.060)	(0.029)
100% C in Mixed Groups	-0.333***	0.242**	0.091
	(0.118)	(0.100)	(0.075)
Mixed-to-Fixed	-0.191***	-0.028	0.219***
	(0.054)	(0.045)	(0.046)
N	384	384	384

Notes: Multinomial logit regression on preferences for the three group choices. Data for Baseline and High. *High* is a treatment dummy. For other details, see notes to Table 7.

The *High* dummy offers an estimate of the impact of the 67% increase in *prospective* gains from integration, when we control for *realized* gains. The probability of choosing "leave" falls by 10 ppts., while "exclude" and "stay" increase by 5 ppts. each, but none of these coefficients is statistically insignificant. Hence, the increment in prospective gains itself reinforced the effect of realized gains, but it is of second order importance. Mixed groups became more attractive because they generated better earnings than in the baseline setup, not simply because they promised better earnings.

6 Discussion

From the perspective of international economics, phenomena such as Brexit or, more generally, the backlash in the support of regional and multilateral trade agreements present a puzzle. People should embrace economic integration, because they can economically benefit from it. The traditional explanation is that countries that open up their trade typically end up with *absolute* losers alongside winners (Stolper and Samuelson, 1941). This may make integration more difficult, ex ante, or promote disintegration, ex post. Our experiment reveals that the backlash may occur even if everyone is a *potential* winner in absolute terms. The analysis suggests an additional angle that may help us understand recent phenomena: inequality of opportunity *across countries*. In the experiment, unequal economic prospects reduced individuals' willingness to cooperate with each other, thus acting as a friction in realizing the benefits promised by integration. These results demonstrate the importance of enhancing standard international economic theory to make explicit not only the economic constraints, but also the human limitations that interfere with the process of trade. Our experiment reveals that there is much to learn from explicitly modeling the institutions and contractual solutions that are necessary to attain the gains from economic integration—a perspective that echoes the approach to studying globalization taken in a recent theoretical literature (Antràs and Costinot, 2010).

More concretely, the experiment provides three insights about factors that influenced the emergence and success of economic integration in the lab, and thus might similarly influence outcomes outside the lab. First, efficient trading norms do not easily scale up because trust does not easily scale up. As a result, integration is unlikely to generate the promised benefits—and, hence, to garner much support from the public—in the absence of institutions that can facilitate a trust-building process, and mitigate contractual enforcement frictions across countries (Results 1-6). To the extent that expanding trade beyond local boundaries requires a greater reliance on self-enforcing agreements, then these institutions are key to lower the incentives for short-sighted conduct.

Second, inequality of economic opportunity across countries might act as a friction in realizing the gains promised by economic integration. In the experiment, inequality had a negative effect on the performance of integrated economies because it inhibited cooperation and harmed cohesion (Results 7-9). Not only was the willingness to cooperate elastic to relative economic advantages, but when potential gains were distributed iniquitously, those who chose mixed groups more frequently sought to exclude others from their group—a choice that condemned the excluded to low-payoff interactions.

Third, the backlash against integration can hardly be ascribed to a communication failure of policymakers and economists, say, the potential benefits were not made transparent to the public. Individual experience primarily shaped subjects' actions and attitudes toward integration in our experiment, not so much the potential gain. In fact, when we boosted economic prospects across the board and clearly communicated this to subjects, this failed to improve outcomes (Results 10). We do see a lower desire to seek autarky (Result 11), but this is primarily due to larger realized earnings in mixed groups. Once this factor is accounted for, the boost in potential gains improved attitudes toward integration, but is of second order.[17]

Supposing that these laboratory results reflect a principle of behavior that also underlies external decision processes, what policy considerations can we make given the current disintegration tendencies?

First, policymakers should focus on strengthening *formal* institutions that foster mutual trust by enhancing transparency, and discourage countries from seeking purely immediate benefits by providing effective sanctions for short-

[17] This may help explain why Britons voted for Brexit even if, as noted in Sampson (2017, p. 164) "[L]eaving the EU is not in the economic interest of most of the left-behind voters."

sighted, uncooperative conduct. This and other experiments reveal that overcoming opportunistic temptations is inherently difficult if the contractual environment is weak—even if long-run cooperation can be highly profitable for all parties involved. This may explain why keeping together potentially very profitable economic unions and multilateral trade systems seems so challenging. The institutional requirements established by the EU for new members, and the WTO's sanctioning of uncooperative members may be ways to address these challenges and thus should be strengthened rather than weakened.

Second, it has been argued that standard models of trade in goods and services may underestimate the gains from trade by not taking into account the dynamics of innovation in integrated markets. If so, the experiment suggests that it may not matter if this determination is made transparent to policymakers and practitioners, but not to the public, because the mere prospect of larger gains did not seem to drive choices in the experiment. Individual experience is what mattered most.

Third, there is scope for cross-country redistributive policies, such as the "cohesion" policies in the EU, to foster a spirit of cohesion among member countries. Typically, countries with a high per-capita income are net contributors, so that stronger countries transfer some of their gains from integration to weaker countries. This is similar to our No-Gap treatment, where top-to-bottom redistribution improved the overall desire to scale up groups all the way to 24 players. It also boosted overall cooperativeness and profits relative to the opposite scenario (Wide-Gap treatment) in which integration was beneficial for all but disproportionately so for those who were already well-off. This is a new angle to explain the voting behavior of individuals—such as those voting for Brexit—who in principle would gain from integration but might feel left behind in relative terms.

This last aspect is especially important if we think of the benefits from integration in the experiment as embedding also a "political" component. If so, the loss in political sovereignty that is generally associated with economic integration might have greatly reduced the perceived benefits in some countries but not others, due to different cultural views. This may also contribute to explain the disintegration trends the world has been witnessing.

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A Appendix

A.1 Experimental procedures

The experiment was conducted at the Economic Science Institute's laboratory at Chapman University and involved 768 undergraduates recruited between 2/2017 and 03/2019. We ran 8 sessions per treatment, each with 24 participants none of whom had previous experience with this game. Treatments have variation in self-reported sex composition between 41 and 49 percent males (average is 44%). On average, participants were paid USD 32, including a show-up fee of USD 7 and the payoff from an incentivized quiz on the instructions that was taken before the start of the experiment. The average duration of a session was 1 hour and 40 minutes. Instructions were recorded in advance and played aloud at the beginning of a session, participants had the possibility to follow on individual copies. We used neutral language for the instructions (words like "cooperation" or "help" were never used). The instructions informed players that only one of the five supergames completed would be randomly selected for payment, with public random draw at the end of the experiment. The points earned in that supergame were converted into dollars according to a pre-announced conversion rate of USD 0.18. The experiment was programmed using the software z-Tree (Fischbacher, 2007). No eye contact was possible between participants. We collected demographic data in an anonymous survey at the end of each session.

A.2 Proof of proposition 1

Here we prove that full cooperation is a sequential equilibrium in every group and treatment. We say that a norm of cooperation is being followed in the group whenever all players adopt the trigger strategy discussed in Section 4. For convenience let the defection payoffs be, respectively, d and $d-l$ to a producer and a consumer. Let $k_i := 9+2i$ denote the cooperation payoff to a consumer of type $i=1,2,3$ under fixed pairs. Given this notation, a necessary and sufficient condition for full cooperation to be an equilibrium is reported in the following lemma:

Lemma 1. *Fix an economy. Let $k_i+a = 11+a$ denote the smallest cooperation payoff in that group. If the continuation probability*

$$\beta \geq \beta^* := \frac{d}{a+k-d+l} \in (0,1),$$

then full cooperation is a sequential equilibrium.

Study the payoff to a type i player, who alternates between producer and consumer roles, having the opportunity to earn $k_i + a$ every other round, in a cooperative equilibrium. Let $s = 0, 1$ denote the role of the player at the start of a round, where 0 is for a producer and 1 for a consumer. The type of counterparts does not affect the player's payoff—only the counterparts' cooperation rate. In mixed groups, producers cannot discriminate based on the consumer's type as this information is not observed. Hence, in cooperative equilibrium the player nets

$$v_0 := \frac{\beta(a + k_i)}{1 - \beta^2} \qquad \text{and} \qquad v_1 := \frac{a + k_i}{1 - \beta^2},$$

while off-equilibrium there is full defection so the payoff corresponds to the one associated to infinite repetition of the static Nash equilibrium, denoted

$$\hat{v}_0 := \frac{d + \beta(d - l)}{1 - \beta^2} \qquad \text{and} \qquad \hat{v}_1 := \frac{d - l + \beta d}{1 - \beta^2}.$$

Note that full defection payoffs are type invariant. It is immediate that off-equilibrium a producer has no incentive to deviate from the sanctioning rule, because defecting is the unique best response to every other producer defecting in every round. Hence, we only need to show that $v_0 \geq \hat{v}_0$, i.e., in equilibrium the player has no incentive to defect as a producer, by refusing to help some consumer.[18] This inequality can be rearranged as $\beta \geq \beta_i^* := 6/(6+2i+a\mathbb{1}_{\text{mixed}})$ for the case of fixed pairs and mixed groups and the Lemma automatically follows. Note that $\beta_i^* < 1$ because $a + k_i - (2d - l) > 0$ by assumption for all player types. The Lemma exploits the fact that the lowerbound probability β consistent with cooperation is a decreasing function of the player's return from cooperation $a + k_i$. Hence β_i^* decreases in i, so that players of higher type, who have higher returns from cooperation in the group, also have a greater incentive to cooperate. Proposition 1 follows from observing that in the experiment $\beta = 0.75$ and the most stringent requirement comes from fixed pairs composed of type 1 players, in which case $a = 0$ and $k_1 = 11$, so $\beta_1^* = 0.75$ represents the smallest lowerbound threshold.

[18] Though in the experiment discounting starts on round $T = 18$, the round in which the random termination rule started, one can demonstrates that the incentives to cooperate monotonically decline until round t. It follows that by studying the incentives to cooperate in equilibrium using payoffs associated with the beginning of round T ensures those incentives are satisfied in all $t < T$. In period $t = T$ payoffs correspond to v_s above. The details of this demonstration are provided in Bigoni et al. (2019).