CHOOSING JUSTICE IN EXPERIMENTAL DEMOCRACIES WITH PRODUCTION

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We examine in a laboratory setting how direct participation in choosing a principle of distributive justice and a tax system impinges on subjects' attitudes and subsequent productivity when they participate in a task, produce income, and then experience losses or gains according to the tax system. Experience with a redistributive principle and its associated taxation system in a production environment does not detract from overall acceptance of the distributive principle, particularly for subjects who participate in choosing the principle. Participation in discussion, choice, and production increases subjects' convictions regarding their preferences. For these subjects (especially recipients of transfers) productivity rises significantly over the course of the experiments. No such effect is evident for subjects who do not participate in setting the regime under which they are to labor. The results' implications for questions of democratic participation and the stability of income support programs are drawn.

One of the most important recent lines of inquiry in political philosophy deals with the question of distributive justice. An influential branch of this literature has used the game-theoretic notion of imperfect information to build on a tradition of "impartial reasoning." These studies have subsumed aspects of distributive justice under questions of fair division. John Rawls's *A Theory of Justice* (1971), the most celebrated of these works, develops an elaborate argument for a particular theory of distributive justice, and his conclusions are typical of this genre. (For an alternative formulation see Harsanyi 1953, 1975.) Rawls argues that under very specific hypothetical conditions (called an "original position") a set of individuals would unanimously choose, as the governing principle of distributive justice, to maximize the welfare[1] of the worst off individual in the society. Thus, Rawls's theory (and similar analyses) concentrates on the pattern of distribution and deemphasizes questions of entitlements.

By contrast, another set of studies underscores the role of entitlements in questions of distributive justice. A salient example (Nozick 1974) emphasizes fair procedures for maintaining entitlements to the rightful fruit of one's labor. A clear tension exists between these two approaches, in that the latter would regard any requirement to redistribute well-gotten gains as illegitimate, whereas the former would require a degree of redistribution as a minimal requirement of fairness.

In later writings Rawls (1985) shows that he is aware of this tension by setting up the problem of a political conception of justice. He identifies two personal aspects that must be taken into account in

any theory: the capacity for a conception of the good and the capacity for a sense of justice. Within the former is the notion of the *individual's* personal advantage—what one can gain from one's own efforts. The latter deals with moral sentiments regarding what is right from a societal point of view. He notes the potential conflict between the individual's desire for self-enhancement and the need for some form of distributive justice in the society. Redistribution to achieve the latter would be implemented by means of a taxation system.

By identifying that tension, Rawls raises the issue of the potential instability of any principle of distributive justice. Although a principle may appear fair from behind a veil of ignorance, it could begin to chafe in practice when individuals begin to feel its bite. Under taxation, individuals' conceptions of their own good may lead to dissatisfaction. This may lead affected individuals to adduce arguments of entitlements (in line with Nozick's arguments) in seeking to obtain more for themselves. They may begin to question the regime.

This strain poses major problems for modern theories of distributive justice for it raises the specter of the instability of a redistributional scheme. In practice, it has fallen to the democratic process to attempt to resolve these tensions, which manifest themselves operationally in the choice and exercise of the taxation system. Yet if justice in distribution is an important matter, and if it is to be supported in a free and democratic society, a number of political questions have to be addressed. Redistribution almost always involves some form of taxation, so it is necessary to consider how individuals react to an enforcement of redistribution via a taxation mechanism.

Taxation, after all, has played a central role in the evolutions (and revolutions) that have led to the democratization of states in the Western world. Taxation without representation was a prime motive force for the Magna Carta, the American Declaration of Independence, the French Revolution, and so on. The democratic principle that the population exercise ultimate control over the government's power to tax is considered a serious safeguard against tyranny. The underlying mythology of the democratic creed holds that the process will ultimately lead to a reasonably fair system of taxation.

But in large-scale, modern, representative democracies the single voter exercises that authority indirectly, at a great distance, with minimal information. Indeed, in many democracies, large portions of the franchised population fail to participate in the democratic process in any meaningful way. They take the taxation as it comes and only rarely, when a salient change in taxation rouses them from their "rational ignorance,"[2] do they react with political consequences for the political competitors in the system. Yet in their workaday lives average workers live intimately with the consequences of the taxation system. A portion of earnings is taxed via various taxes to pay for government services of all sorts. Standard economic reasoning argues that as income tax levels rise, incentives to earn additional income will be reduced and possible gains in productivity will not be realized. Economic growth will suffer. Ultimately, this may even affect the acceptability of governments. This means that any conceptions of justice in distribution are inherently political and raise empirical questions. Yet very little empirical work has examined the questions of the impact of particular rules of distributive justice on productivity and on the continued acceptability of a particular regime.

There have been a number of efforts devoted to examining various aspects of Rawls's arguments as they bear on distributive justice.[3] An early attempt to test one particular aspect of Rawls's assumptions

as it bears on these questions can be found in Brickman 1977. Essays by Greenberg and Cohen (1982) discuss many of the social-psychological theories, issues, and findings bearing on distributive justice, as do Bierhoff, Cohen, and Greenberg (1986) and Deutsch (1985). From different disciplinary perspectives Hochschild (1981) and Soltan (1982) present reviews of some empirical findings and tests that bear on other aspects of distributive justice. Jasso (1980, 1986), building on a tradition of equity theory in sociology, has attempted to represent sentiments regarding justice in mathematical form. More directly addressing points of our concern, Hoffman and Spitzer (1985) have done experiments directly testing subjects' sensitivity to entitlements as a basis for fair distribution. More recently Frohlich, Oppenheimer, and Eavey (1987a, 1987b), Lissowski, Okrasa, and Tyszka (1989) and Bond (1989) have conducted experimental tests of some of the central aspects of Rawls's argument regarding choices of principles of distributive justice.

But none of these analyses addresses the issues of productivity and the continuing viability of a distributive justice system operating in practice. We examine in a laboratory context how direct participation in choosing a principle of distributive justice (and an associated taxation system) from behind a simulated "veil of ignorance" impinges on subjects' attitudes toward their tax system. We also explore the impact on subjects' productivity when they experience the consequences of the tax system in a production environment.

In particular, we address three questions:

1. Is acceptance of the distributive justice principle and taxation system a function of economic experience under the regime imposed by the group?

2. Is productivity affected by the exercise of the taxation rule?

3. Are there differences in the answers to these questions when the principle and taxation scheme are imposed rather than agreed upon in a participative fashion?

Research Design

In his celebrated *A Theory of Justice* John Rawls (1971) argued strongly that a particular rule of distributive justice is most fair: maximizing the primary goods of the worst-off member of society (the difference principle). In that work, Rawls's justification for his conception of a principle of distributive justice is, at base, contractarian; that is, it is based on empirical assumptions and arguments regarding what representative individuals would do in a hypothetical, idealized, original position. A recent set of experiments (Frohlich, Oppenheimer, and Eavey 1987a, 1987b; Lissowski, Okrasa, and Tyszka 1989) has explicitly attempted to test the validity of some of Rawls's empirical assumptions and the plausibility of his conclusions. In those experiments we tested which principle of distributive justice would be chosen when Rawls's contractarian "original position" was approximated in a laboratory context.[4] Those experiments showed that it was generally possible to obtain consensus on a single principle of distributive justice as the "most fair" using the methods proposed by Rawls. And there was *one* principle that stood out as the runaway winner (chosen more than 75% of the time). But the principle chosen was not the one anticipated by Rawls. Rather, it was a "mixed principle" that in effect maximized the primary goods of the group as a whole subject to a constraint that the primary goods (or income) of the worst-off individual in the group not be allowed to fall below a predetermined floor. Subjects wanted the society to have a (legitimate) "safety net"; but given that, they wanted producers to enjoy the benefits of their labor.[5]

These experiments were designed to go

beyond an identification of the most desirable principle of distributive justice. Modifying and extending the basic design we used previously (Frohlich, Oppenheimer and Eavey 1987a, 1987b), we constructed the current experiments to test the stability of support for a principle of distributive justice and its associated taxation scheme in an ongoing setting of production. That setting, moreover, allows for the testing of productivity changes in response to experience with production and redistribution.

Subjects were recruited from large undergraduate classes at the University of Maryland, College Park—students who were generally not close friends. Three different treatment groups were established. In two of them individuals were familiarized with principles for redistributing income. They were told that they were to choose a redistributive principle to govern the redistribution of income that they would earn in a latter part of the experiment, but they were not told the nature of the task they would perform. After discussions among themselves, subjects decided collectively on a principle of distributive justice to govern their future taxation scheme. In one variant the choice was by unanimous decision, in the other by majority rule.[6] They did this in a simulated "original position." Since the nature of the task to be performed was not made known to the subjects prior to their decision, they were effectively unable to estimate their likely future productivity and economic status in the production economy.

A third treatment group was established as a control to determine the impact of discussion and decision on the acceptability of the principle and on subsequent productivity. In this treatment (referred to below as the "imposed" group) the group was given the same introduction to principles of distributive justice but was not allowed to choose the principle and taxation scheme. The principle was imposed by the experimenters. The mandated principle was maximizing the average with a floor constraint—the modal choice of the previous experiments.[7]

After the choice or imposition of a principle, all treatment groups were subject to the same regime. Subjects were assigned the task of correcting spelling mistakes in a text excerpted from the works of Talcott Parsons. Each text was arduous (as the reader can easily confirm by reading any random selection from Parsons) and contained spelling errors. Each individual did the same task, and each received wages for his or her individual production. The marginal pay rate had considerable returns to scale, as is apparent in Table 1.

Individuals' outputs were checked; their earnings, taxes, and take-home pay were calculated and reported to them along with the equivalent yearly income flows implied by the earnings. The principle either chosen by the group or imposed upon them was then applied to their earnings and redistribution was carried out in accordance with the principle. Their post-tax payments for that period were calculated and reported to each of them (see Table 1, col. 4). Taxes needed to raise individuals above the floor income were assessed proportionately against the earnings of those who earned more than the floor income. This process of work, pay, and redistribution was carried out three times in each experiment. Measurements of preferences for principles, satisfaction with the principles, and degree of certainty with the ranking of principles were administered at each stage of the experiments.[8] Repeating the task allowed for an examination of the relationship between experience with taxation on the one hand and (1) attitudes toward the principles and (2) productivity, on the other. An outline of the experimental structure and the points at which each of the relevant variables was measured is indicated in the Appendix.

The recording of subjects' rankings of

Table 1. Pay Rates ($)

Total Number of Errors Corrected	Total Earnings[a]	Equivalent Yearly Salary
1	.25	$ 2,080
2	.50	4,160
3	.75	6,240
4	1.00	8,320
5	2.50	20,800
6	4.00	33,280
7	5.50	45,760
8	7.00	58,240
9	10.00	83,200
10	13.00	108,160
11	16.00	133,120
12	19.00	158,080
13	22.00	183,040
14	25.00	208,000
15	28.00	232,960

[a]At .25 for the first to fourth errors corrected, 1.50 for the fifth to ninth, and 3.00 for the tenth and up. Note that in the first two experiments the rates were $.50, $1.00 and $2.00. We changed the rates thereafter to increase the number of recipients of low earned income.

principles of redistributive justice at each major stage of the experiments allowed for a measure of any possible change in preferences for principles as a result of experimental treatment both across treatments and within treatments. The measurements of subjects' confidence in their rankings permitted a comparable indication of any change in confidence in their preferences for principles. Similarly, elicitation of their degree of satisfaction with the principle at various stages charted their economic experience's impact on their preferences. The number of corrections they found was a direct measure of their status in the economy. Any changes in productivity as a result of experimental treatment or experience over the course of the experiments were directly measurable.

Experimental Findings

Before reporting the findings, it is instructive to get a preliminary sense of why so many groups favored one principle over all others and what sorts of concerns they expressed. To do this we excerpted from the conversations subjects had during the course of their deliberations. The discussions that took place in the experimental groups were diverse and wide-ranging. Subjects appeared to take the problem they were charged with seriously, and it seemed that they were engaged in thoughtful reflection about their decisions. The transcripts of their discussions are far too long to quote at length, but a sampling of their arguments may give the reader a feel for the considerations that came into play.

Virtually all groups were concerned that individuals not fall below a certain minimum level of support. The reasons (Frohlich and Oppenheimer 1988) were concern both for the fate of the individual and the welfare of society:

> I would like to see that everyone at least has the basic things. After that I don't really care. [If the floor is too low] . . . a lot of people are going to be starving and they will be without shelter and housing. (p. 99)

> If you have people that are really poor . . . they have a tendency to just stay there because you

know there isn't enough nutrition, they can't get an education and all these kinds of things. But if you put it on a certain minimum then they have a chance to get out of that situation. They have a chance. (p. 72)

Without a floor . . . you would probably have a lot of crime which would affect everyone's income in terms of insurance, health costs, etc. (p. 2)

They also raised questions of entitlement and of the illegitimacy of redistribution:

SUBJECT 1: So the people that make more money have to support the people that don't do anything.

SUBJECT 2: That's the way it is.

SUBJECT 1: That's not fair . . . I should get what I worked [for] and what I deserve. I don't think I should have to pay for some bum. (p. 147)

In the discussions subjects often cited the potentially corrosive effect on the incentives of redistribution: "But one other thing is here to maximize the productivity too, and you need to have some kind of incentive there to work hard" (p. 116). But most groups in the choice experiments chose a compromise principle that took into account both entitlements and concern for those at the bottom of the income scale: "Maximize the average with a floor constraint? That's the one that I kind of like too. I like it because you're guaranteed a certain amount of money so you're not going to walk away empty. But then if you have to do harder work or more work, you have a chance to maximize your profits" (p. 138). One concern regarding the experimental design was that subjects truly be behind a "reasonably" thick veil regarding their likely standing in the production economy they were to occupy. The verisimilitude of their deliberations, and the thickness of the veil regarding their ultimate position in society is reinforced by some of their comments:

The reason why I'm suggesting what I'm suggesting is that in this particular instance I would assume that our goal as a group is for each of us to get out of here with as much money as possible. Is that a rational assumption in order for all of us to get out of here with as much money as possible and since we don't know what the skills are? I mean out in society I am fairly confident that I could get a fairly good amount of money, but the skills out there are the skills that I am aware of that I have and can deliver. We don't know what the skills are here. So I might be able to make the maximum amount, then again my skills may be such that I won't be able to make a penny here under a normal system. So if I can't make any money with my skills, I would like to be able to make some money by insuring that there is some system that we all get out of here with something more than we came in with. (p. 129)[9]

Although these brief excerpts are a very small selection of the total conversations, from them it should be apparent that the subjects took the exercise seriously and that substantive concerns were voiced.[10]

The major treatment variable in the analysis under consideration here was the degree of participation by the subjects in their choice of a principle and taxation scheme. The results of the group choices are represented in Table 2. In the treatment groups in which discussion and choice took place, the most popular group choice was the principle of maximizing the average with a floor constraint. Of the 18 groups 11 chose that principle outright while 4 groups combined it with a range constraint. Only three groups chose the principle of maximizing the average with a range constraint. These results are roughly comparable to those obtained in previous experiments (Frohlich, Oppenheimer, and Eavey 1987a, 1987b) except for the absence of group choices of the principle of maximizing the average income. The principle imposed by the experimenters was maximizing the average with a floor constraint of 99 hundred dollars. The floor imposed was the mean of earlier choice experiments cited above and conformed closely to the mean of the

Table 2. Group Choice of Principle

Type of Experiment	Principle Chosen: Floor Constraint	Principle Chosen: Range Constraint	Imposed	Total
Choice	15	3	0	18
Imposed	0	0	10	10
Total	15	3	10	28

floors chosen by the subjects in the experiments reported here.

Stability of Preference over Production and Redistribution Periods

Is an individual's acceptance of a principle of distributive justice adversely affected by his or her economic experience of production and redistribution? Our experiments provide a number of measures of this possible effect. Here we report on changes in the subjects' rankings of distributive justice principles over time, subjects' confidence in their rankings, and their satisfaction with the principle under which they were laboring. In the following three tables we examine the results of these measures for all groups in the experiments.

Table 3 reports the overall changes in scores of the subjects' first-place rankings of principles over the period in which they were involved in production.[11] Notice that among the 129 subjects there are only 16 gross changes in first-place rankings (the off-diagonal elements), which results in only five net changes. *Maximizing the average* gains five first-place rankings, *floor constraint* loses three, and *maximizing the floor* loses two. While no baseline for the statistical significance of these changes is available, the small number of net changes (5 of 129, roughly 4%) is prima facie evidence of considerable stability. Certainly an erosion of 3 of 81 first-place rankings (3.7%) of floor constraint cannot be construed as a serious erosion of support or an indication of alienation.

Nor is this stability confined to one treatment group in the experiments. When the subjects who chose their principle are examined separately and are compared with those who had it imposed, both groups behaved similarly. For subjects in the choice experiments the number of changes in first-place rankings is 7 of 79 possible. With choice the floor constraint lost 2 of 56 first-place rankings (3.6%). In the imposed experiments there were 9 of a possible 50 changes in first-place rankings

Table 3. Changes in First-Place Rankings of Principles

Start of Production	End of Production Periods: Maximize Floor	Maximize Average	Floor Constraint	Range Constraint	Total
Maximize floor	3	0	2	1	6
Maximize average	0	21	2	0	23
Floor constraint	0	6	73	2	81
Range constraint	1	1	1	16	19
Total	4	28	78	19	129

Table 4. Impact of Production and Redistribution on Subjects' Confidence in Rankings by Experimental Treatment

Experiments	Mean Certainty before Tasks	Mean Certainty after Tasks	n	t-score	*p*
All	3.78	3.96	119	2.55	.012
Imposed	3.46	3.67	49	1.02	.315
Unanimity	3.95	4.19	46	3.00	.004
Majority rule	4.04	4.04	24	.00	1.000

and floor constraint lost 1 of 25 first-place rankings (4%).[12]

Security of Rankings of Principles As a Function of Economic Experience

In addition to actually recording the rankings of each of the principles at various stages by each of the subjects, we also asked how sure they were about their rankings. We can use changes in this measure to see whether their experience increased or decreased their convictions.[13] The scores for their answers directly after the group choice (or imposition) were compared with the scores at the end of all tasks and redistribution to identify the production period's impact on their convictions. Table 4 gives the results of that analysis.

It is clear from the table that for the group as a whole the production experience had a positive impact on the certainty of their convictions. They are more secure after production and redistribution than they were before that experience. But a breakdown of the data into treatment groups shows that the effect is not at all uniform.[14] In the unanimity experiments, where all subjects had to agree to the principle there is a strong positive impact on subjects' security regarding their ranking of principles. For subjects in the majority rule experiments however, this is not the case. They exhibit no change in their conviction. The subjects in the imposed rule experiments experienced an increase in the security of their rankings, but it was not statistically significant. Discussion and agreement via consensus does matter with regard to reinforcement of subjects' subsequent security of preference, but discussion and agreement without consensus does not. Thus, discussion and agreement alone do not appear to account for the effect. The requirement of a consensus decision rule has an impact. This difference between the two types of choice experiments should be borne in mind in light of the widespread finding in psychological and social science experiments of the salutary effect of group discussion on subjects.[15]

Satisfaction with the Principle As a Function of Economic Experience

What of our third indicator of stability, satisfaction? We specifically asked the subjects, before each production period and then after the last one, how satisfied they were with the principle under which they were operating.[16] Data from those responses can be used to test concerns (such as Nozick's) that a principle of distributive justice like Rawls's would suffer eroding support, making it ultimately unstable. In terms of our experiment, this is what might be termed an "alienation hypothesis": "The satisfaction with the group's decision, made behind the veil of ignorance, will go down after leaving it as subjects begin to experience redistribution."

Table 5 reports the overall changes in scores of the subjects' self-reported satis-

Table 5. Satisfaction after the Veil Was Lifted

Number of Cases and Mean	Before First Task	After First Task	After Second Task	After Third Task
N	94	93	97	125
Mean	3.80	3.84	3.85	3.85

faction with the principles over the periods in which they were involved in production.[17]

It is quite apparent that the data fail to support the alienation hypothesis. Indeed, the evidence bolsters the converse—increasing satisfaction and acceptability. Although the changes are not significant, average satisfaction increases over the production periods. Indeed, in a separate analysis of subjects in the choice versus the nonchoice experiments, the increasing support is much clearer for the choice experiments, but the alienation hypothesis doesn't find statistical support even in the cases where the group's principle was imposed. Nevertheless, some differences between these treatment groups deserve comment. Subjects in choice experiments expressed a higher level of satisfaction at the end of the experiments than did subjects in the imposed experiments. Their mean satisfaction was 3.95 (N = 87) while the others had a mean of 3.60 (N = 42) for a t-value of 1.95 significant at the .05 level.

Impact of Economic Experience on Productivity

Let us now address one other main question: Would productivity be affected by the exercise of the distribution-taxation rule, and are any such effects a function of participation in the taxation decision?

The basic measure of productivity was the actual number of mistakes found and corrected by subjects at each of the three production phases. Changes in productivity using these measures were then evaluated. Consider Table 6. It is clear from the first line of the table that for the full set of experiments, experience with production and redistribution *increased* productivity. Average production rose significantly between the first and last periods. This is in flat contradiction to the drop anticipated by an alienation effect. There is, however, at least one possible explanation that would save the hypothesis that redistribution, sapping incentives, should lead to reduced productivity. Since the tasks performed were repeated with different texts, subjects' experience with the task might be expected to increase their productivity as they progressed along a learning curve. After all, subjects might be expected to increase their efficiency as they became more familiar with the demands and mechanics of what they were doing.

However, the breakdown of the subjects into choice and imposed treatment

Table 6. Changes in Production by Experimental Treatment

	Mean Production				
Experiments	First Period	Last Period	F	*p*	n
All	5.78	6.65	9.87	.002	135
Choice	5.82	7.02	10.92	.001	85
Imposed rule	5.70	6.02	.58	.451	50

Table 7. Impact of Experimental Treatment on Production: Choice vs. Imposition

Production	Choice Experiment Mean	Imposed Experiment Mean	F	*p*	n
First	5.82	5.70	.035	.852	140
Last	7.02	6.02	3.241	.074	135

groups negates this explanation as a basis for the observed increase in productivity. The second line of the table shows that the increase in productivity found in the group as a whole is found in greater measure in the subjects who engaged in discussion and choice of a taxation scheme. The third line shows that the subjects who participated in the imposed rule experiments experienced only a marginal and statistically insignificant increase in productivity. Whatever effect experience may have on increasing productivity, it is vitiated by working under an imposed regime. Conversely, it is manifest and potentially augmented when the regime is a matter of participatory choice. The discussions and choice behind the simulated "veil of ignorance" appear to make a difference in subsequent performance.

A direct test of this experimental treatment can be obtained from an analysis of variance using the experimental treatment as a categorical variable. Are the differences in productivity significant between the subjects who discussed and chose a principle and those who simply had one imposed on them by the experimenter? Comparing the first and last period productivity for the two experimental treatments in Table 7 shows that the difference in average production between the two groups is eight times larger in the last period (7.02 versus 6.02) than in the first period (5.82 versus 5.7). Are these differences significant? Table 7 allows us to answer that question.

Clearly the experimental treatment has no effect on the *initial* performance of the subjects. At the beginning of the first production period, subjects in the choice experiments and those in the imposed rule experiments performed almost identically. Discussion and choice as opposed to the experimenters' imposition of a rule, seems to make no *initial* difference to their performance.

Subjects' performance in the *last* period tell a rather different story. Here the F-statistic for the experimental treatment begins to approach significance ($F = 3.24$, $p = .07$), although the effect appears not to be particularly strong. Thus, the experimental treatment appears to have had an impact on productivity *after the subjects experienced the consequences of working under the rule*, but the effect appears not to have been particularly strong.

Taxpayers and Recipients: The Impact of Different Experiences on Productivity

Given the indications that discussion and choice versus imposition has an impact on productivity, one is led to ask whether other variables help us understand differences in productivity. Are some subgroups of subjects more affected by the experimental treatment than others, since participants in the experiments experience the effects of redistribution differently? Some are taxpayers, some recipients of transfers. Does one's status as either taxpayer or transfer recipient have a differential impact on the subsequent production behavior? And is this relationship also affected by whether the individual had participated in the process

Table 8. Changes in Production: All Experiments by Tax Status

Tax Status	First Mean	Last Mean	F	*p*	n
Taxpayers					
Production	7.51	8.23	4.08	.047	71
Production ratio	.27	.25	1.45	.232	71
Recipients					
Production	3.49	4.58	5.35	.024	59
Production ratio	.12	.14	1.60	.211	59

of choosing the rule that governed redistribution of earnings?

Subjects were grouped by tax status as net taxpayers or net recipient's of transfers (over the whole experiment).[18] To test for changes in productivity relative to the group, each individual's ratio of the group's production in the first and last periods was calculated. This ratio serves as a measure of the relative burden borne by the two economic groups: taxpayer versus recipient. Changes in these ratios should reflect changes in the relative productivity of taxpayers and recipients over the course of the experiments.

The data in Table 8 indicate that in the experiment as a whole, whether a subject is a taxpayer or not, productivity appears to rise, although recipients' production appears to rise more sharply. Moreover, there is no apparent significant difference in the share of burdens borne by taxpayers and recipients over the course of the experiments. The ratios of their group's production appears relatively stable.

But a different picture emerges when the subjects are further subdivided into experimental treatment groups. Table 9 shows the analysis of changes in productivity for taxpayers and transfer recipients in the choice experiments as opposed to in the imposed experiments. The trends toward increasing productivity identified for all experiments is reflected in the choice experiments in both subsets of par-

Table 9. Changes in Production by Tax Status and Experimental Treatment

Tax Status and Experimental Treatment	First Mean	Last Mean	F	*p*	n
Choice Experiments					
Taxpayers					
Production	7.57	8.32	3.11	.084	47
Production ratio	.27	.23	4.86	.033	47
Recipients					
Production	3.66	5.42	8.17	.007	38
Production ratio	.12	.16	4.48	.041	38
Imposed Experiments					
Taxpayers					
Production	7.52	8.17	1.35	.255	29
Production ratio	.26	.27	.35	.558	29
Recipients					
Production	3.20	3.05	.05	.824	21
Production ratio	.13	.11	.79	.385	21

ticipants, although some differences emerge.

Note that in the choice experiments the increase in productivity is lower among taxpayers than among recipients. Hence, transfer recipients become relatively more productive members of their experimental group as time goes on. For taxpayers the increase in production of corrections is significant only at the .08 level. For recipients, however, the gain in production is highly significant both substantively and statistically. Gains in production of corrections are almost 50%. This gain is statistically significant at the .01 level. This differential increase in productivity between the two groups leads to an increased relative sharing of the burden of production by welfare recipients over the course of the production runs. Their *per capita share* of production rises significantly from 11.8% during the first period to 15.9% during the last.

These gains in productivity in the choice experiments can be sharply contrasted with an absence of the same effect in the imposed experiments. The picture in the imposed experiments is quite different. There taxpayers increase their production of corrections in a manner reminiscent of the previous result (but now statistically insignificant), while recipients show a very small and insignificant *drop* in productivity.[19] These small changes within groups lead to no significant difference in the ratio of the burden born by the two groups, although the recipients appear tending toward *diminishing* their relative performance.

It is important to note the effect of time on the behavior of the two groups. As time passes, subjects in the two treatment groups react differently to their experiences with the redistributional system. From Table 9 we know that recipients in the choice treatment increased their output significantly over the course of the experiment, while those in the imposed treatment did not. A direct measure of last-phase production differences between the treatment groups is given by the F-statistics for differences between the two treatments in the last round of production. They are highly significant ($F = 10.43$, $p = .002$). Similarly, the differences in the ratio of recipients' production to the total group production varied significantly across experimental treatments. Recipients in the choice treatment increased their productivity in absolute terms not only, but also relative to taxpayers in their treatment group.

The implicit differences in the behavior of participants in the choice versus imposed rule experiments can be tested directly by an analysis of variance using the experimental treatment as a dichotomous variable. The first line of data in Table 10 deals with the productivity of recipients in the last production period. It shows the significance of the experimental treatment: choice and discussion versus imposition of a principle. The impact of the treatment variable is clear and unequivocal. Recipients who discussed the principles and participated in a choice outperform their counterparts in the imposed experiments by a wide and significant margin. Moreover, they account for a larger share of their group's production. Their production ratio is 50% higher than recipients in the imposed experiments. The second half of the table deals with net taxpayers. There the effect of the treatment is not at all evident. Discussion and choice versus imposition seems not to make a difference in the performance of taxpayers. The only significant difference is that in the choice experiments the average taxpayer bears a lower proportion of the burden of production (23.7%) than his or her counterpart in the imposed experiments (26.9%). But that difference is attributable to the increase in productivity of recipients in the choice experiments.

It is important to recall that there was no significant difference in the *initial* measures of productivity for either recipients or taxpayers across treatment groups.[20] Thus, the improved perform-

Table 10. Impact of Experimental Treatment on Final Production by Tax Status

Tax Status	Choice	Imposed	F	*p*	n
Recipients					
Last production	5.42	3.05	10.434	.002	59
Last production ratio	.16	.11	7.039	.010	59
Taxpayers					
Last production	8.32	8.17	.219	.641	71
Last production ratio	.23	.27	7.676	.007	71

ance among recipients in the choice experiments cannot be attributed solely to a discussion effect. It emerges only after subjects have experienced production and redistribution.

Thus, the choice-imposition variable does not act uniformly on the classes created by the tax system. This is further evidence that the effect is not solely engendered by discussion. The content of the decision and the status of the subject in the environment after the veil is lifted makes a difference. Initial lack of difference and subsequent large differences only for transfer recipients give further force to the argument that the observed differences are not solely due to the treatment variable (participation in the discussion-and-decision phase) but are a product of an interaction of the treatment variable and the individual's status under the implementation of the redistributive principle.

Discussion

The results of these experiments offer tentative answers to the questions posed at the outset. Acceptance of the taxation principle is not adversely affected by the economic experience of subjects after the veil is lifted. There is no evidence for an undermining of the principle as subjects experience its effects. Guaranteeing a subsistence income to those who cannot earn that level of reward does not undermine the legitimacy of the system in the short run as measured by rankings of principles, security regarding one's rankings, or satisfaction. However, when subjects participate in choosing the principle, their satisfaction is higher than when it is imposed upon them. When they participate and are required to choose *unanimously*, their confidence in the choice increases. This is not the case when they choose via majority rule.

The floor constraint principle appears to be a robust choice when experienced in practice in a production environment. Its first-place ranking is unchallenged at the end. In summary, acceptance of the principle is, if anything, enhanced by the production and redistribution experience. But these effects appear to be stronger in the choice experiments. Participation in discussion and choice makes a difference in subsequent attitudes and changes in attitudes.

This short-run acceptability of the principle across all groups must be tempered by an underlying trend in productivity. Productivity shows a tendency to increase in taxpayers in all treatments and shows a substantial increase among recipients in the choice experiments. By contrast, recipients in the imposed experiments seem to decrease their productivity. At least for recipients of transfers, participation in a group discussion behind a veil of ignorance, participation in choosing a redistributive principle, *and experience with the economic consequences of the principle* seem to make a difference. Transfer recipients' productivity goes up signifi-

cantly over time. Rather than acting as a disincentive, the taxation and redistribution system that guarantees a floor *when agreed to in a participative fashion* seems to spur those at the bottom of the income distribution to increase their effort and output. Participatory democracy (at least in an experimental setting) aids productivity.

The discussions behind the simulated veil parallel and throw into clear relief the widespread political debates in Western democracies regarding the appropriate limits for the welfare state as opposed to a laissez faire economy. The results reaffirm that subjects can reach consensus on a principle behind a simulated veil of ignorance. Concern for the poor and weak, a desire to recognize entitlements, and sensitivity to the need for incentives to maintain productivity all enter into subjects' deliberations regarding a fair rule for implementing distributive justice. The choice of, and continued support for, the floor constraint principle demonstrates how it serves as a compromise incorporating these competing normative and empirical demands.

Moreover, the effect of the experimental treatment on productivity offers an insight into how a redistributive regime can be rendered stable. There are two themes that pervade the arguments of both politicians and citizens who attack income support programs: (1) An income support program will sap incentives among recipients and will make them dependent and unproductive; (2) It will create a moral hazard and attract the "undeserving"—the "welfare cheaters"—which is unfair to all those who work and ultimately pay for the programs. At least within small groups, explicit discussion of what constitutes fairness establishes the need for, and legitimacy of, *some* sort of support program. When those who ultimately are to be recipients of transfers participate actively in the decision, the dreaded result of increasing dependence and sloth do not materialize. Quite the contrary. It appears that recipients who actively participate in the decision recognize the entitlement claims of those who are paying for transfer payments and almost literally redouble their efforts to pull their own weight. By contrast, when recipients function under a (functionally identical) income support system that has been imposed upon them and in which they have had no substantial say, they appear to free ride. Their efforts appear to flag and their productivity shows a declining trend. They appear to view the transfers they receive as their due, and they do not make efforts to become self-sustaining. Genuine participation and debate in determining the content of programs appears necessary to prevent the undermining of income support programs.

Of course, there are enormous barriers to the implementation, writ large, of the kind of process that we have set up in the laboratory. But the data point to a need for more informed debate and discussion. For without that participation, the fears of income support critics might be well founded. This need for broader participation is all the more important, given the well-known propensity in democratic systems to decreased political activity with decreased socioeconomic status.

Apart from the experimental results, the possible efficacy of involving workers in decisions is demonstrated by the evidence emerging from the industrial relations literature. Profit-sharing schemes, participation in workplace quality of life issues, the use of quality circles, and a general openness of management to the suggestions from the shop floor are all beginning to show promise of increasing worker productivity. The necessity of participation in such schemes is reflected in Kanter 1987 (p. 32): "For gainsharing plans to work, a particular organizational structure and corporate culture are required—and these include an open discussion of the plan to gain employee accept-

ance, the establishment of cross-unit teams or task forces to develop the plan, and the adoption of suggestion systems."

A moment's reflection on the underlying dynamics of the production environment in the experiments and, indeed, on the production teams operating in industry reveals that they represent "social dilemmas." There are incentives for the least productive to free ride and accept their guaranteed payoffs or their share of the productivity gains of others' efforts. The extensive literature on cooperation in social dilemmas reveals that discussion (especially relevant discussion) increases cooperation in social dilemmas (Dawes 1980; Dawes, McTavish, and Shaklee 1977; Messick and Brewer 1983). From that perspective it should not be surprising that increased participation in determining the content of the reward structure should increase the efforts of those who otherwise would have an incentive to free ride. Indeed, any income redistribution system can be seen to incorporate elements of social dilemmas; so potential solutions that are effective in overcoming free rider tendencies there are of interest in insulating redistributive systems from deleterious effects.

One should not go too far in interpreting these results. The laboratory setting requires a compression of time, income, and stakes and involves small groups. And their subjects, after all, are university students. Nevertheless, the results provide an argument for meaningful participation in the democratic process which goes beyond the simple requirements of informed decision making and fairness. Meaningful participation on a broader scale might well have beneficial implications for the productivity of the economy. It is not all clear how such participation might be accomplished, but its potential impact appears to be worth examining.

Appendix: Sequence of Events In the Experiments

1. oral recruitment of five subjects in large university classes
2. orientation to the problem of choosing a principle of distributive justice
3. measurement of ranking of principles and degree of conviction regarding those rankings
4. reading about implications of the principles and testing (and retesting when needed) regarding the nature and implications of the principles
5. measurement of ranking, etc.
6. group discussion and choice (unanimous, or majority rule), or imposition
7. measurement of ranking, etc. and of satisfaction regarding principle chosen
8. first production period—gross production (TSK1YP), associated income, and net redistributed income reported
9. measurement of rankings, etc.
10. second production period—production (TSK2YP) and incomes reported
11. measurement of rankings, etc.
12. third production period—production (TSK3YP) and incomes reported
13. measurement of rankings, etc.
14. questionnaire

The variables TSK1YP–TSK3YP refer to the subject's production of corrections in TaSK1–TaSK3.

Notes

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1. Rawls introduces the notion of "primary goods" and discusses his principle in terms of maximizing

the primary goods available to the worst-off individual. We use the terms *welfare* and *income* to represent these goods.

2. The term *rational ignorance* was first coined by Anthony Downs (1957) in his ground-breaking book. It refers to the incentives individual voters have to limit their efforts in gathering political information, given the small differences they can (individually) make in the political process.

3. The analytic examination of Rawls's arguments is voluminous. Yearly citations of Rawls run to the hundreds. We, however, are primarily concerned with the application of empirical methods to questions raised by his arguments.

4. A fuller description of the rationale for this methodology is presented in Frohlich and Oppenheimer (1989).

5. That finding is consistent with an earlier theoretical result of Howe and Roemer (1981). They modeled the problem of a choice of a distributive principle behind the "veil of ignorance" as a game and concluded that the choice of distributive principle would be a function of the degree of risk aversion among the individuals. One implication of their analysis is that if the representative individuals who are charged with choosing a principle have a variety of degrees of risk aversion, the principle or principles in the core would be mixed. Thus, there is both empirical and theoretical reason to expect the choice of a mixed principle.

6. In most of the results reported below the two choice groups unanimity and majority rule are treated as a single group, since the particular decision rule was found to have insignificant impact on some variables. Where significant differences occur, they are reported separately.

7. The actual floor imposed was the average of the floors chosen in the experiments cited above.

8. Satisfaction with the "group's choice of principle" was measured only after the last production period in the imposed experiments.

9. A debriefing questionnaire was administered at the end of the last production period to gather data on characteristics of the subjects. No factors were found to have strong explanatory power regarding preferences for, or choices of, principles of distributive justice. Some, however, were able to explain variance in the *level* of the floor constraint chosen. All in all, however, the "thickness" of the induced veil is always a plausible source of experimental error.

10. Indeed, a reading of the full transcripts gives one an overwhelming sense of the seriousness and substantive nature of the deliberations. A full copy of the conversations, (as well as all other experimental data) either on paper or as a flat ASCII file is available for further analysis for the cost of electronic duplication plus postage.

11. Recall that the ranking of the principles just prior to the start of production is made immediately after the group choice (or imposition) of the principle, while the final ranking takes place at the conclusion of the experiment after three rounds of production and redistribution.

12. It should be noted, however, that the net number of changes in first-place rankings for all principles seemed to vary by whether the group was permitted to choose. In the choice groups there were four net changes while in the imposed rule groups there were eight net changes, indicating somewhat greater stability in the choice groups.

13. The actual question was, "How do you feel about your ranking of these principles?" And the response code was *Very unsure, unsure, no opinion, sure, very sure.* Weights of one to five were assigned to the categories with a higher value corresponding to greater security.

14. Here the responses of subjects in the unanimity experiments and in the majority rule experiments are presented separately rather than as one "choice experiment" group. The reason for doing so is that the two groups showed differences in the dependent variable in question here.

15. Below we will identify other results that point to the conclusion that discussion per se cannot account for many of the differences between treatment groups.

16. The exact question read, "How satisfied are you with the distributive principle selected by (for) the group?" The responses were *very unsatisfied, unsatisfied, neither satisfied nor unsatisfied, satisfied,* and *very satisfied.* Satisfaction was scored one to five, respectively. Unfortunately responses to this question were not elicited in 7 of the 10 groups in the imposed treatment.

17. Note that the first measure of satisfaction with the principle is taken immediately after the group choice (or imposition), while each subsequent measure is taken after one round of task completion.

18. It should be noted that other ways of implementing the concept of taxpayer status do not lead to more than marginal changes in the conclusions supported by the data analysis.

19. One possible explanation for the greater increase in productivity of recipients in the choice experiments is that the taxpayers were already producing at such a high rate that there was little room for improvement. That explanation is negated by the finding that in the imposed experiments the productivity of the *taxpayers increased* while that of the *recipients decreased.*

20. The F-statistics for experimental treatment effect on recipients' productivity is minuscule for the first round of production ($F = .39$, significant at the .53 level). For taxpayers it is vanishingly small.

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