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CHOOSING THE WRONG POND: SOCIAL COMPARISONS IN NEGOTIATIONS THAT REFLECT A SELF-SERVING BIAS*

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We explore the role that choice of comparison groups plays in explaining impasse in teacher contract negotiations. We hypothesize that the negotiators select "comparable" districts in a biased fashion such that teachers' salaries in districts that unions view as comparable are higher than teachers' salaries in districts that school boards view as comparable. We also predict that strike activity is positively related to the difference in the salary levels of the unions' and boards' lists of comparables. We test these predictions using a unique combination of subjective survey and field data on teacher contract negotiations in Pennsylvania. Both hypotheses are supported by the data.

I. INTRODUCTION

In his book, *Choosing the Right Pond,* Frank [1985] asserts that a "person's well-being depends in part on how large his income is relative to the incomes of others" and shows how such comparisons affect the wage structure of organizations. Frank's research builds on the large body of research in sociology and psychology that establishes the importance of social comparisons in determining individual satisfaction and behavior (see, for example, Festinger [1954] and Runciman [1966]).

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Although social comparisons play a major role in most salary negotiations, this is especially true in public sector negotiations, perhaps due in part to the more limited influence of market forces. In public sector contract negotiations, unions and employers frequently allude to salaries paid to workers in other municipalities and to other types of workers in the same municipality. Indeed, in states where disputes are resolved via arbitration, arbitrators are often statutorily *required* to take into account the salaries of other employees in similar and dissimilar positions [Brown and Medoff 1988].

When social comparisons enter into bargaining, the degree to which the two sides use the same set of "comparables" may affect their likelihood of reaching a voluntary agreement. When they agree on the relevant comparison group or groups, they will tend to advocate similar settlements and to settle relatively quickly. When there is substantial disagreement about the appropriate comparison groups, however, the two sides may have very different expectations about the outcome of the negotiation and be unable to reach agreement without an impasse.

In this paper we explore the role that choice of comparison groups plays in explaining impasse in contract negotiations. Based on our own prior research [Babcock et al. 1995; Loewenstein et al. 1993], we hypothesize, first, that negotiators' selections of comparables will be influenced by considerations of self-interest, and second, that the magnitude of the discrepancy between the two negotiators' comparables will help to predict impasse. We test these predictions with a unique data set that combines subjective survey data with field data on public school teacher contract negotiations in Pennsylvania. We surveyed the lead union and school board negotiators from all school districts in Pennsylvania and asked them for diverse information, including the list of districts that they viewed as comparable for purposes of salary negotiations. We then linked the data from this survey to field data that included, for each of the approximately 500 school districts, information about teachers' salaries within the district, teachers' salaries in adjoining districts, community salary levels, historical and current information about strikes, and other background information. The combination of survey and field data allow us to examine the relationship between strike activity and the subjective perceptions of the negotiators.

II. Background

It is customary in public sector contract negotiations to consider the salaries of comparable groups. In some situations the relevant group of comparables is well established. For example, in Wisconsin, where final offer arbitration is used to resolve teacher contract disputes, arbitrators rely heavily on comparisons with other districts in the same athletic conference (see Babcock and Olson [1992]). Districts in an athletic conference are similar in size and geographically proximate, so that they are likely to face similar financial characteristics and labor market conditions. The negotiators accept this set of comparables since they know arbitrators will use them if there is an impasse.[1]

Often, however, which districts are appropriate for comparison is not well established. In such cases, even though both sides have access to the same information concerning current and past contracts throughout the state, psychological factors can cause them to reach different conclusions about the appropriate set of comparables, and specifically to select comparison groups in a manner consistent with self-interest. In the context of public sector school negotiations, this would imply that the union representative's list of comparables will include higher salary districts than the board's list.

Psychological research shows that people process information in a self-serving fashion, placing greater weight on information that is consistent with their preferences. In an early study documenting this "self-serving" bias, Hastorf and Cantril [1954] examine student perceptions of a contentious football game between Princeton and Dartmouth. Students from both schools watched a film of the game and rated the number of penalties committed by both teams. Princeton students saw the Dartmouth team commit twice as many flagrant penalties and three times as many mild penalties as their own team. Dartmouth students, on the other hand, recorded an approximately equal number of penalties by both teams. As the researchers concluded, it was as if the two groups of students "saw a different game."

More recent research finds evidence of a self-serving bias in many domains. For example, people who are told that extrover-

1. This argument stems from Mnookin and Kornhauser [1979] and Farber and Katz [1979], who argue that negotiators bargain "in the shadow of the law." That is, their reservation values in bargaining depends on what they expect to happen if there is an impasse.

sion is a desirable trait tend to recall episodes from their lives that portray themselves as extroverts, and those who are told that introversion is desirable tend to recall episodes that cast themselves as introverts [Sanitioso, Kunda, and Fong 1990]. People who are asked to rate their ability at various tasks tend to place greater weight on dimensions of the skill they excel at [Dunning, Meyerowitz, and Holzberg 1989]. For example, fast drivers believe that speed is an important dimension of "driving skill," while cautious drivers tend to emphasize safety, so that 90 percent of drivers believe that they are in the top 50 percent of drivers [Svenson 1981].

One type of self-serving bias that has received special attention in the psychology literature is a bias in perceptions of fairness. People tend to arrive at judgments of what is fair or right that are biased in favor of their own self-interests. Messick and Sentis [1979] demonstrated such a fairness bias in the context of compensation for work. Subjects were asked to specify the fair rate of pay for two people—the subject and another person—who had worked either ten or seven hours at a task. The person who worked seven hours was always paid $25. Subjects were asked how much the person who worked ten hours should be paid. When the subject had worked seven hours, they thought that the other person should be paid $30.29 for ten hours of work. But when the subject had worked ten hours, they thought they deserved $35.24. The difference between $30.29 and $35.24 was cited as evidence of a self-serving bias in perceptions of fairness.

Beyond documenting the self-serving bias in perceptions of fairness, findings from a number of studies suggest that the bias may constitute an important cause of inefficient bargaining outcomes. In one study [Roth and Murnighan 1982] subjects bargained over how to distribute a stack of 100 lottery tickets. If one player won the lottery, she received $20. If the other won, she received $5. There are two obvious ways to split the tickets: 50 tickets to each (equal chance of winning) or 20 tickets to the $20-prize player and 80 tickets to the $5-prize player (equal expected dollar value). When neither player knew who had which prize amount, subjects generally agreed to divide the tickets about equally, and only 12 percent of pairs failed to reach an agreement, ending up with no payoff. However, when both parties knew both prize amounts, subjects seemed to view as fair the distribution of chips that would benefit themselves. The $20-prize player was likely to hold out for half of the tickets, while the $5 player de-

manded 80 tickets to equalize expected values. In this condition 22 percent of the pairs failed to reach agreement.

In another study [Camerer and Loewenstein 1993] MBA students in a negotiations course bargained (for grade points) over the sale of a piece of land knowing only the value of the land, if they were buying it, or its cost, if they were selling it. All pairs agreed on a sale price. In a second phase the same pairs of students renegotiated after learning the value or cost of the land to their partner. Twenty percent of pairs failed to settle in this second round despite the fact that they had more information—specifically, about their partner's reservation price. Students who negotiated a good settlement in the first negotiation thought that selling the land again at the same price was fair. Their partners typically thought that they deserved a better price than in the first round, to be compensated for having lost out in the first round.

Experimental results from Weg, Rapoport, and Felsenthal [1990] also point to self-serving assessments of fairness. Subjects played a "shrinking pie" game in which they alternated making offers for how to divide an amount of money. Every time an offer was rejected, the value of the amount to be divided decreased. They ran three conditions that varied the rate at which the pie shrank for the two subjects. In two of the conditions the pie shrank at different rates for each subject, and in the third it shrank at the same rate. Disagreement rates were dramatically different across the conditions. In the equal-rate condition 88 percent of subjects made efficient agreements (the divider's initial allocation was accepted). In the two unequal-rate conditions only 43 percent of pairs achieved efficient agreements. Apparently, subjects in the unequal rate conditions came to different conclusions about how these asymmetries should influence the distribution of the money.

Knez and Camerer [1995] examined the impact of outside options on the ultimatum game. In the standard ultimatum game one party (the proposer) offers a division of a fixed amount of money (e.g., $10) that the other party (the responder) can accept or reject. If the responder rejects the offer, then both persons get a payoff of $0. In this game the modal offer is typically $5, and agreement rates are high. They ran a variant of the ultimatum game in which, instead of getting zero if the offer was rejected, each side would get a positive payoff. This change led to a very substantial reduction in settlement rates relative to the standard

game, again, seemingly because players responded to the outside options in a self-serving fashion. For example, if the responder had an outside offer of $4 and the proposer had an outside offer of $3, the responder would think that a fair division involved dividing the surplus equally: i.e., $1.50 of surplus each, or $4.50 to the proposer and $5.50 to the responder. But proposers rarely offered splits that gave themselves a cash payment lower than $5. The result was that adding such "outside options" increased the rate of disagreement to about 50 percent. Repeating the game five times did not reduce the disagreement rate.

In our own prior experimental research on bargaining [Babcock et al. 1995], we found evidence of the self-serving bias even when subjects were paid for being unbiased. Furthermore, we found that the magnitude of the bias was a strong predictor of impasse. We presented participants with diverse materials from a lawsuit resulting from a collision between an automobile and motorcycle (depositions, police reports, doctors' reports, etc.). Participants were assigned the role of plaintiff or defendant, and attempted to negotiate a settlement. Delays in settlement resulted in substantial financial penalties, and if the negotiators ultimately failed to settle, the award to the plaintiff was determined by an impartial judge who had earlier read exactly the same case materials.

Before they negotiated, participants were asked to predict the judge's ruling, were told that this estimate would not be communicated to the other party, would not affect the judge's decision (that had already been made), and that they would be paid a bonus if their prediction was close to the actual ruling. Nevertheless, plaintiffs' predictions of the judge's award amount were substantially higher than those of defendants, and the degree of discrepancy between plaintiff and defendant was a strong predictor of whether they settled the case (as opposed to relying on the judge's decision).

In follow-up experiments we attempted to reduce the magnitude of the bias by having subjects write an essay arguing the other side's point of view, or by informing them of the bias. Neither of these interventions had a measurable effect. Subjects consistently believed that the judge's decision would be in their own material interest. Indeed, when subjects were informed about the bias, they became convinced that *their negotiating opponent* would be highly biased, but believed that they themselves would not succumb to the bias. The facts that subjects are unable to rid

themselves of the bias when rewarded for doing so, and that their belief that they are not subject to the bias, demonstrate clearly that the self-serving bias is not a deliberate strategy.

Other findings from the same series of experiments point to biased assimilation of information as the likely psychological mechanism underlying the self-serving bias. Subjects were presented with eight arguments favoring the side they had been assigned (plaintiff or defendant) and eight arguments favoring the other side. They were asked to rate the importance of these arguments as perceived "by a neutral third party." There was a strong tendency to view arguments supporting one's own position as more convincing than those supporting the other side, suggesting that the bias operates by distorting one's interpretation of evidence. Moreover, when we ran a version in which subjects were presented with their roles (plaintiff or defendant) only *after* reading the case materials, the magnitude of the bias was substantially reduced and almost all of the pairs reached rapid agreement on damages.

It seems highly plausible that a similar phenomenon could help to explain impasses in public sector contract negotiations. Comparison groups can be viewed as forms of arguments: high salary comparables favor a large salary increase and low salary comparables favor a small increase. If the two sides differentially weight arguments based on their own self-interest, as they did in the experiments just reviewed, then we would expect the union's set of comparable districts to pay a higher average salary than the board's. Moreover, since both sides are unaware that they are processing the information in a biased fashion, they will tend to view their comparables as the *right* ones. This does not mean that they will not, in addition, misrepresent the districts they view as comparable to the other side, increasing the salary discrepancy between the two sides' articulated comparables. But their own privately held view of comparables is more likely to affect their own bottom line or "reservation salary." When the comparables they privately view as relevant are too far apart, impasses will be likely to occur.

III. Data

Our data set consists of survey responses and field data from school districts in Pennsylvania. There are 500 school districts in Pennsylvania, all of which are unionized. Teachers are repre-

sented by one of two national teachers' unions, the American Federation of Teachers or the National Education Association. Management interests are represented by the elected school board in each district. Since 1971, the law that regulates teacher collective bargaining allows teachers to strike if there is a negotiation impasse.[2] Since that time, approximately 8 percent of all teacher contract negotiations have ended in a strike, with an average strike duration of 16.4 days.

A. Field Data

We collected data on school district and community characteristics in Pennsylvania for school years 1983–1984 to 1988–1989. We use these data to test for differences in survey respondents and nonrespondents, and also to use as control variables in equations for strike propensity. We collected information on district size (enrollment), district wealth (value of property per student), and strike activity (incidence of strikes and strike duration) from the Pennsylvania Department of Education. Local unemployment rates were collected from the Pennsylvania Department of Labor and Industry, and an alternative measure of district wealth (average resident income) was obtained from the 5 percent public use samples of the 1980 and 1990 U. S. Census. These variables are all measured at the district level, with the exception of the unemployment rate which is measured at the county level.

We also collected information about teacher salaries in each of six years from the Pennsylvania Department of Education. Teachers are paid according to a salary matrix: columns represent a teacher's level of education (BA, BA + 8 credits, etc.) and rows represent a teacher's level of experience in that district. Each element in the matrix is the level of salary for a teacher with that particular education/experience combination. To construct a measure of the salary level, it would not be appropriate to take the average teacher salary in each district since the distribution of teacher experience and education differs across districts. Our measure of salary in a district, therefore, consists of the salary paid to a teacher with a bachelor's degree and fifteen years of experience. This point represents the median characteristics of teachers in Pennsylvania. We also conducted all of the analyses that follow using four other points on the salary sched-

2. See Olson [1984] for a discussion of Pennsylvania's collective bargaining statute.

ule: master's degree and fifteen years of experience, bachelor's degree and five years of experience, master's degree and five years of experience, and average salary in the district. Every point gives qualitatively identical results. Indeed, most of the findings presented below would be strengthened by using these other points.

B. Survey Data

In March 1994 we sent a survey to the lead negotiator for each side in all school districts in the state. The lead negotiators are the union presidents and the school board presidents. Both are elected by their constituents (teachers or citizens of the school district). A cover letter requested their participation in our survey to learn about teacher contract negotiations. We assured them that their responses would remain confidential. The Pennsylvania School Board Association gave approval for the survey and provided us with their mailing list. The Pennsylvania Federation of Teachers and Pennsylvania State Education Association were unwilling to provide us with their mailing lists, but did agree to send out our surveys from their central offices. The response rate was 57 percent for the union presidents and 35 percent for the school board presidents.[3] The higher union response rate may have been due to the fact that the union surveys were addressed with the unions' mailing labels.

To test whether the returned surveys were representative of the larger populations, we tested for differences between respondents and nonrespondents with respect to the following variables: district size (measured by enrollment), salary for teachers, district wealth (measured by average resident income and the value of property per student), and previous strike activity (measured by percent of contract negotiations that resulted in a strike). We found no significant differences between respondents and nonrespondents for either the union or the school board for any of these variables.

For some of the analysis we use a set of 75 matched pairs: districts in which both the union and the board returned the survey and answered a particular question regarding the districts that they consider comparable to their own. Response probabilities of the two parties were independent. There is no relationship

3. Much of the analysis relies on the responses to one particular question. The joint response rate (for returning the survey and answering this question) is 54.5 percent for the union and 27.2 percent for the board.

between receiving a valid response from one party and receiving a valid response from the other party. We also tested whether the group of 75 districts differed from the rest of the districts. There were no significant differences between the two groups of districts with respect to district size, salary for teachers, and district wealth. However, there was a significant difference between the two groups for previous strike activity: the 75 districts were *less* strike prone than the other districts.

The survey included two sets of questions concerning social comparisons. First, respondents were asked to list the districts they felt were comparable to their own for the purpose of contract negotiations. A hypothetical example of these lists is shown in Table I. In this example both the union and the board list the districts, Valley View and Cedar Creek, as being comparable to their own.[4] The union's list contains three districts that the board does not list, and the board's list contains two districts that the union does not list.

Second, the respondents were asked questions concerning whether they tended to use teachers in other school districts or residents in the community for salary comparison purposes. The survey also included questions about the respondents' demographic characteristics (education, experience, and gender).

IV. Empirical Results

A. Selection of Comparable Districts

For the 75 districts in which both the union and the board returned surveys, both sides listed an average of approximately 4.5 districts as comparable (4.6 for the union and 4.4 for the board).[5] Out of these 4.5 districts the average number of districts in common on unions' and boards' lists was 1.88. The two sides were asked what made the districts they listed comparable. The most frequent response for both sides was that the districts listed neighbored their own (82 percent of unions and 84 percent of boards), followed by the fact that the districts had financial characteristics similar to their own (68 percent of unions and 68 percent of boards).

4. A hypothetical example is used to assure respondent anonymity.

5. For the unmatched sample the union listed 4.7 districts as comparable, and the board listed 4.3 districts as comparable. The differences between the matched and unmatched samples (for the union and board) are not statistically significant.

TABLE I
HYPOTHETICAL EXAMPLE: UNION'S AND BOARD'S LISTS OF COMPARABLE SCHOOL DISTRICTS

East Lake school district			
Union's list		Board's list	
Valley View	$27,008	Valley View	$27,008
Cedar Creek	$28,200	Cedar Creek	$28,200
Monrovia	$28,514	Steel City	$27,170
Suburban	$27,776	East Hills	$26,065
Beechwood	$28,800		—
	—	Average	$27,111
Average	$28,060		

To test for self-serving selection of comparable districts, we calculated the average salary in the districts listed by the union and board.[6] (In the hypothetical example of Table I, the average salary of the districts listed by the union is $28,060 and the average salary of the districts listed by the board is $27,111.) The average salary for union comparables was $27,633 (SE = 416), and the average salary for board comparables was $26,922 (SE = 340).[7] The mean difference of $711 is significantly different from zero (p = .003), and substantial: about 2.4 percent of average salary in these 75 districts. In recent teacher strikes, the last offers of the two sides typically differ by about 1 percent.[8] Therefore, the 2.4 percent discrepancy represents a significant gap. In 25 percent of the districts the average of the unions' lists was more than $1000 greater than the average of the boards' lists. These findings are consistent with self-serving selection of comparable districts: the unions' choices of comparable districts were higher paying than the boards'.

We also examined whether the average of each side's list was systematically related to the characteristics of the districts or to the negotiators themselves. As independent variables, we use the negotiators' years of experience, years of education, and the average and variance of teacher salaries in neighboring dis-

6. Throughout the paper, when we calculate the average salary of a set of districts, we average what districts pay a teacher with a bachelor's degree and fifteen years of experience, not what the average teacher in the district earns. As described earlier, we also conduct the analysis using four other points on the salary schedule and the results are qualitatively unchanged.

7. This is calculated using our most recent year of field data.

8. This figure is based on the authors' calculations from media coverage of teacher strikes.

tricts (those that share a boundary with the district). The rationale behind including this last measure is as follows: if there is large variation in the salaries of teachers in the neighboring districts, the board might focus on the very low-paying neighbors and the union might focus on the high-paying neighbors, causing the set of comparison districts that each side uses to have very different salary levels (that reflect a self-serving bias). If there is very little variation in the teacher salaries in the neighboring districts, the two sides would be unable to form comparison districts with very different salary levels (given that they both view proximity as very important for establishing a district as a potential comparable). Therefore, increases in variation would increase the average of the union's list but decrease the average of the board's list.

The regression results are presented in Table II. In the first two columns the dependent variable is the average salary of the union's list, and in the second two columns the dependent variable is the average salary of the board's list.[9] As one would expect, the average of each list is positively related to the average salary in neighboring districts. Moreover, consistent with the prediction just discussed, an increase in the variation of teacher salaries in neighboring districts increases the average salary of the union's list and decreases the average salary of the board's list. The only statistically significant effect for education or experience is that more highly educated school board presidents are associated with higher average board lists.

B. Relationship to Strike Activity

Having established a self-serving bias, the central question is the degree to which the magnitude of this bias predicts strike activity across districts. We use field data on contract negotiations in Pennsylvania from 1983–1989 to examine this issue. In Table III we estimate tobit models of strike propensity, where the dependent variable is the percent of contract negotiations in this time period that ended in strikes. The independent variables include the difference in the average salary of the comparable districts chosen by the union and board and a set of control variables listed in the table notes.

The first column of Table III presents tobit results based on

9. Again, these averages refer to the average of the salary for a teacher with a bachelor's degree and fifteen years of experience.

TABLE II
REGRESSION OF UNION AND BOARD LISTS OF COMPARABLES: SURVEY AND FIELD DATA

	Average of union's list		Average of board's list	
Average salary of neighboring teachers	0.9978 (19.141)	0.9880 (18.505)	0.9593 (19.235)	0.9399 (19.498)
Variance of salary of neighboring teachers	0.0123 (2.009)	0.0132 (2.143)	−0.0105 (−1.797)	−0.0104 (−1.857)
Experience of union president		−0.0114 (−0.673)		
Education of union president		0.3118 (1.221)		
Experience of board president				0.0210 (1.413)
Education of board president				0.2268 (3.421)
R^2	0.70	0.70	0.82	0.84
N	262	262	129	129

t-statistics are in parentheses. Field data are from school districts in Pennsylvania from 1988–1989. Regressions also include a constant term. Dependent variables are measured in $1000's. Average salary refers to the average of the salary for a teacher with a bachelor's degree and fifteen years of experience. Data from the first two columns contain districts in which the union returned the survey, and data from the third and fourth columns contain districts in which the board returned the survey.

the matched sample of districts in which both parties returned surveys and provided a list of comparable districts.[10] The first column indicates that increases in the difference between the average salaries of the two lists significantly increased the likelihood of a strike.[11] The point estimate suggests that a district in which the union's list is $1000 greater than the board's list will have a 49 percent higher strike rate than a district in which the average salaries of the union's and board's lists are the same.

The second and third columns of Table III report analyses that include all districts in which one or both parties provided a list of comparables. The second column presents an analysis in

10. Seventy districts are used because of missing strike data on five districts.
11. These results do not depend on the particular point on the salary level that we chose. They are also not sensitive to whether we measure the difference in the lists as the proportional difference (the difference as a share of the prenegotiation salary level) rather than the absolute difference.

TABLE III
TOBIT MODELS: PROPORTION OF CONTRACT NEGOTIATIONS THAT ENDED IN A STRIKE: SURVEY AND FIELD DATA

	Matched sample	Missing data replaced with neighbor mean	Missing data estimated
Difference in average	0.145	0.076	0.073
salaries of union's and	(1.85)	(2.10)	(2.14)
board's lists of comparables	[0.49]	[0.22]	[0.22]
Number of observations	70[a]	325[b]	325[b]
Log likelihood	−18.81	−147.83	−147.73
Log likelihood (0)	−23.36	−155.10	−155.10

t-statistics are in parentheses. In brackets is the percentage increase in the percent of negotiations that end in a strike when the independent variable is changed from $0 to $1000 (a change from the union and board having identical average lists to the union's list being $1000 greater than the board's list). Field data are from teacher contract negotiations in Pennsylvania, 1983–1989. Regressions include a set of control variables including district wealth, county unemployment rate, a dummy variable for any previous strike activity, the number of school days lost in a previous strike that were not rescheduled, and the variation in teacher salaries in neighboring districts. Regressions in columns 2 and 3 also include a dummy variable that equals one for observations in which missing values for the "self-serving bias" are replaced with predicted values. Average salary refers to the average of the salary for a teacher with a bachelor's degree and fifteen years of experience. Observations are weighted by number of contracts negotiated in the time period.

a. These data come from districts in which both the union and the board returned the survey.

b. These data come from districts in which either the union or board (or both) returned the survey.

which missing values for one side's list were replaced with the salary average of adjoining districts. For example, if the union in Pottsville gave a list of comparables but the board did not, we included the average salary of teachers in districts adjoining Pottsville as the average of board comparables. The third column presents an analysis in which missing values were estimated based on the mean and variance of salaries in adjoining districts (using the coefficients from columns 1 and 3 of Table II). As is evident from the regressions reported in the second and third columns of Table III, the impact of the independent variable is slightly smaller (see brackets in the table) when moving to the larger sample (with missing data replacement), although the coefficients remain significantly different from zero.[12]

Using the sample of strikes, we analyzed whether the size of the difference in the lists could predict strike duration. We find a positive, but not a significant effect of this variable on strike duration. This is similar to some previous research which finds that

12. The standard errors reported in columns 2 and 3 may be too small because we have not corrected them to account for the fact that for some of the observations one of the regressors (the difference in the lists) is an estimated variable itself which contains prediction error.

variables that predict strike incidence often do not predict strike duration (i.e., see Kennan [1980]).

C. Importance of Different Comparison Groups

In addition to testing for the self-serving bias by comparing salary means of comparable teachers, we examined whether each side's assessments of the importance of different comparison groups reflected such a bias. In teacher contract negotiations the two sides sometimes compare teacher salary, not only with teacher salaries in other districts but also with salaries of community members in the same district. We explore whether a side's assessment of the appropriate comparison group to use (teachers or community residents) is biased in a self-serving way: for example, if board presidents in poor communities believe that community salaries should be given substantial weight in the determination of teacher salaries while board presidents in wealthy communities do not, and vice versa for union presidents. The survey asked respondents to rate the importance of comparisons to the salaries of comparable teachers and the importance of comparisons to the salaries of residents in the community. Importance ratings are measured on a scale of 1–5: 1 indicates that the comparison group in question does not matter at all, and 5 indicates that it matters a great deal. We combine these responses with the data on the actual salaries of teachers in adjacent districts and community residents to test for the presence of self-serving biases.

Table IV presents regressions in which the dependent variables are the importance ratings for the two comparison groups.[13] The first two columns examine the two sides' ratings of comparisons to salaries of community members as a function of the actual level of income of the residents in the respondent's district. The coefficient in the first column indicates that unions in wealthy communities believe that comparisons to residents' salaries are more important than do unions in poor communities. This finding is consistent with self-serving selection of comparison groups. The second column indicates that boards' ratings of the importance of using community members as a comparison group do not depend on the income of those community members.

The last two columns examine how the two sides rate the

13. Due to potential questions raised by the discrete nature of the dependent variable, we also ran the same analysis as an ordered probit and obtained virtually identical results.

TABLE IV
IMPORTANCE RATINGS REGRESSIONS: SURVEY AND FIELD DATA

	Dependent variable is			
	How important are salaries of community residents		How important are salaries of comparable teachers	
	Union response	Board response	Union response	Board response
Actual income of community residents	0.0225	−0.0044		
	(3.036)	(−0.400)		
Actual salaries of comparable teachers			0.014	−0.076
			(0.835)	(−2.367)
N	278	169	278	171
R^2	.032	.001	.003	.032

t-statistics are in parentheses. Field data are from the 1988–1989 school year. Regressions include a constant term. The dependent variable is measured on a scale of 1–5: 1 indicating the comparison matters not at all, and 5 being it matters a great deal. The independent variables are measured in 1000's.

importance of comparisons to teachers in neighboring districts. We use as an independent variable the average of salaries of teachers in bordering districts. These results show that the boards' ratings of the relevance of using teachers as a comparison group depends on the salaries of those teachers (column 4). In districts surrounded by neighboring districts paying their teachers high salaries, boards view these neighbors as being less important for comparison purposes than do boards in districts that are surrounded by districts that pay teachers low salaries. This is consistent with a self-serving bias by boards in evaluating the relevance of using teachers as a comparison group. The results in column 3 do not indicate a similar bias by the union.

V. DISCUSSION

The findings just presented support our two central hypotheses: that parties involved in labor negotiations select comparison groups in a self-serving fashion, and that the magnitude of this bias helps to predict impasse.

The methodology we employed in this study is, to the best of our knowledge, unique in bargaining research. Most research

falls under the heading of laboratory or classroom experiments, on the one hand, or econometric analysis of field data, on the other. The main disadvantage of laboratory experiments is that they tend to involve relatively small monetary incentives, and small and unrepresentative subject populations such as students. The main disadvantage of econometric analyses of field data, besides the lack of experimental control, is the absence of data concerning mediating mechanisms. Thus, for example, if one wanted to study the effect of the self-serving bias with field data alone, one might proceed by assuming that the magnitude of the bias is positively related to the variance of salaries in neighboring districts. One would then test for a relationship between impasse and variance.[14] Observation of such a relationship, however, would not constitute strong evidence for the hypothesized causal chain. Our data set, which combines survey and field data, avoids several of these pitfalls. As with field data, subjects are experienced bargaining practitioners bargaining in familiar settings. As with experimental data, however, we are able to examine subjective measures collected from the bargainers, and can thereby test our specific causal hypothesis much more directly than is possible without such data.

At the same time, it must be acknowledged, that our analysis suffers from the same limitations inherent in all nonexperimental research, which is that we cannot establish causality definitively. It is always possible that some third variable such as the aggressiveness of the parties causes both impasse and self-serving selection of comparables, or that a past history of strikes causes parties to select more extreme comparison groups, rather than the reverse. The latter would be true, for example, if the negotiators either needed to justify their decision to strike or used it to increase constituent commitment to a strike. In our previous research on the self-serving bias, however, we were able to establish a causal sequence running from the self-serving bias to impasse [Babcock et al. 1995]. We did this by introducing an experimental manipulation that attenuated the bias and that, as would be expected if the bias produced impasse, radically reduced impasse.

Another alternative account of our findings is that they reflect strategic misrepresentation on the part of negotiators rather

14. Babcock and Wang [1994] conducted such a test and found that increased variation in salaries in neighboring districts is associated with significant increases in strike duration.

than a cognitive bias. That is, the two sides may imitate self-serving beliefs as a part of positional bargaining. While we assured the survey respondents that their answers would remain confidential, respondents who were skeptical of our claim might record their strategic responses rather than their actual beliefs. Alternatively, in advocating a particular set of comparables to the other side during negotiations, the parties lose track of the fact that they have selected them strategically and come to view them as fair. However, our earlier experimental findings suggest that the self-serving bias was evident well before the two parties had begun negotiating together. What seems quite plausible, and consistent with our previous research, is that negotiators were never aware that they were acting strategically in the first place.

If the self-serving bias is a major cause of negotiation impasse as the current study and our past research suggest, then any policies or actions that restrict parties' leeway in selecting comparables may decrease impasse. Mediators, for example, might help parties to agree on comparables, or to agree on criteria for the selection of comparables. It is only human to compare oneself to others. The task for those who would like to reduce the deadweight loss of impasse is to produce a convergence in such comparisons between negotiators.

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