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Is Arbitration Addictive?

Evidence from the Laboratory and the Field

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ABSTRACT

We test for the presence of an addictive effect of arbitration (positive state dependence) using data both from a laboratory bargaining experiment and from the field. We find no evidence of state dependence in the experimental data, and we find weak evidence of positive state dependence in the field data on teachers in British Columbia. Hence, we reject the view that use of arbitration *per se* leads to state dependence either through reducing uncertainty about the arbitral process or through changing the bargaining parties perceptions about their opponents. The results further suggest that an explanation for any positive state dependence we find in the British Columbia field data must lie in an aspect of the arbitration process which is not captured by our simple experimental design.

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Compulsory arbitration has become an important method for resolving collective bargaining disputes in the public sector. However, many industrial relations scholars have expressed concern that the use of arbitration is associated with growing dependence on arbitral intervention and a breakdown in the ability of bargaining parties to negotiate a settlement. (See Anderson (1979) for a survey of this literature). This problem has been dubbed the "narcotic" or "addictive" effect of arbitration.

Attempts to measure this addictive effect have produced contradictory results. For example, Kochan and Badersneider (1978) find evidence of positive state dependence (an addictive effect of arbitration) in a sample of police and firefighters in New York state. However, Butler and Ehrenberg (1981) show using the same data that if permanent bargaining-pair-specific "fixed effects" are controlled for, bargaining pairs who used arbitration in the last round of negotiations were less likely to use arbitration in the current round (negative state dependence). Hence, bargaining pairs who had used arbitration in the past appeared to be "hung-over" rather than addicted. More recently, Currie (1989) uses 35 years of information about annual teachers' negotiations in the Canadian province of British Columbia to demonstrate that there can be an addictive effect of arbitration even when fixed effects are accounted for.

These studies beg the question of why one might expect to see positive or negative state dependence in arbitration data. Clearly, this question must be answered before any normative judgment can be made about whether the discovery of state dependence in arbitration data should be interpreted as evidence that arbitration is an undesirable way of resolving disputes.

In this paper we test for the presence of state dependence using data generated by a bargaining experiment. The value of running an experiment is

that we can focus on particular aspects of the arbitration process and abstract from others. Specifically, we abstract from many of the institutional aspects of interest arbitration and focus on the hypothesis that the use of arbitration *per se* causes state dependence.

There are at least three reasons why the use of interest arbitration might lead to state dependence: 1) The use of interest arbitration could reduce uncertainty about arbitration, thereby reducing the cost to the parties of using the process (Farber and Katz, 1979); 2) Failing to negotiate a settlement might create antagonism between the parties, leading to further disagreements in future; or alternatively, 3) Failing to negotiate a settlement might demonstrate to both parties that their opposition is "tough" and lead them to be more compromising in future negotiations. The first two reasons lead to positive state dependence while the third leads to negative state dependence.

The Data

The experiments consisted of a series of repeated pie-splitting games. Each subject negotiated with the same opponent for twenty rounds over the division of a fixed quantity. One-hundred-thirty-one subject pairs first bargained ten rounds without an arbitration system followed by ten rounds with an arbitration system that determined the split of the pie in the event of disagreement. The experimental treatments consisted of randomly assigning pairs into five groups, each with a different type of arbitration system.

Initial experiments with five bargaining pairs per treatment were conducted in 1984, and further experiments with approximately twenty pairs per treatment took place in 1988. All experiments were conducted using Plato software at the University of Arizona's laboratory for experimental economics using University of Arizona students. Upon arrival at the laboratory, a

subject was seated at a computer terminal and given the instructions necessary to send, receive, and accept offers. Subjects did not know the identity of their opponents, who were seated at different computer terminals some distance away. They did know that they would be bargaining with the same opponent for all twenty rounds.

The bargaining protocol in each round was unstructured, as is naturally the case in the field. Each party's last offer was posted on their screen and on their opponent's screen at all times and parties were free to revise their offers at any time. An offer consisted of a number between 100 and 500. A schedule translated these numbers into cash payoffs. Each round was limited to five-and-one-half minutes. At the end of that time, if the parties had not agreed on a division they were deemed to be in dispute.

In rounds without arbitration, disputing parties forfeited the entire pie. In rounds with arbitration, a division was imposed by the "arbitrator". In all cases, the arbitrator was modeled as a random draw from a normal distribution. For further details about the experimental design, see Ashenfelter, et al. (1991).

In order to compare the results obtained from our experiment to information about the operation of arbitration in the field, we reanalyze the British Columbia teacher data previously analyzed by Currie (1989). These data consist of information on 35 annual contract negotiations between 75 school boards in British Columbia and their teachers. The data span the years 1947 to 1981. For further information, see Currie (1989).

A Non-Parametric Test for State Dependence

A simple test for state dependence involves dividing bargaining pairs into categories defined by the number of disputes they had. Under the null hypothesis of no state dependence, all permutations of dispute histories that

sum to a given total number of disputes would be equally likely, and this is the basis of our test. For example, suppose that we observe three negotiations per pair. There are eight patterns that can occur: { (000), (100), (010), (001), (110), (101), (011), (111) }. It is, of course, true that these eight patterns are not equally likely even with no state dependence, but *no state dependence does imply that each pattern with the same total number of disputes would occur with the same frequency*.

It should be clear that we cannot learn anything about state dependence by looking at bargaining pairs with either zero or three disputes in three rounds, but we can learn about state dependence from pairs with one or two disputes. Consider bargaining pairs with two disputes in three rounds. Our test is based on the prediction that if there is no state dependence then each of the three possible patterns { (011), (110), (101) } is equally likely. On the other hand, if there is positive state dependence then the first pattern (011) should be most likely and the last pattern (101) least likely. Intuitively, this is because the first pattern has only disputes once a dispute occurs while the third pattern has disputes only following no dispute (implicitly assuming no dispute in the zeroth round). Analogously, if there is negative state dependence, the third pattern (101) should be most likely and the first pattern least likely (011).

While there is substantial inter-pair heterogeneity in how dispute-prone pairs are (Ashenfelter, et. al. 1991), it is important to recognize that this heterogeneity does not affect our test. The intuition for this can be illustrated using our example of three negotiations per pair. Suppose there are equal numbers of two types of bargaining pairs: highly dispute-prone (type A with a dispute probability of 2/3) and less dispute-prone (type B with a dispute probability of 1/3). Type A pairs will have a higher dispute rate than type B pairs so that the two-dispute category will be

disproportionately composed of type A pairs. If there is no state dependence and we looked only at the type A pairs, we would expect to find that each of the three patterns with two disputes in three rounds is equally likely (each occurs with probability 1/3). The same argument applies for the type B pairs. If we then pool all observations on types A and B pairs who have two disputes in three rounds, each of the three patterns is still expected to occur 1/3 of the time if there is no state dependence. The fact that we cannot identify which pairs are type A and which pairs are type B has no bearing on the test.

The first ten rounds of the experiment provide a benchmark where the entire pie is lost in the event of a dispute (similar to a strike). In contrast, the arbitration mechanisms in the last ten rounds provide an arbitrary division of the pie in the event of a dispute. None of the pie is lost directly, but part of the pie may be lost due to bargainers' risk aversion and the uncertainty about what the division will be. On balance, arbitration offers a lower cost of disputing than does the strike-like mechanism of the first ten rounds. Currie and McConnell (1991) present field evidence that disputes occur with higher frequency where arbitration is the dispute settlement mechanism than where strikes are used to settle disputes.

If the use of arbitration leads to state dependence relative to the no-arbitration case, then we expect that the extent of state dependence will differ between the first ten rounds of the experiment and the last ten. For example, if experience with arbitration reduces uncertainty about future awards thereby lowering the costs of disputes, then we will see positive state dependence in the last ten rounds but not in the first ten. If hostility toward a recalcitrant opponent leads to state dependence then we expect to see more positive state dependence in the last ten rounds than in the first ten since each party can "punish" their opponent at lower cost when

arbitration is provided then when it is not. Alternatively, parties might be less afraid of their "tough" opponent when disputes are less costly. In this case, we would expect more negative state dependence in the first ten rounds than in the last ten.

These considerations suggest that the most natural way to carry out our test for state dependence in our experimental data would be to first divide the data into the ten rounds negotiated with arbitration and the ten negotiated without arbitration. This is, in fact, the first test that we carried out. However, since there are many possible patterns of disputes in ten rounds, we have chosen to present results based on the pattern of disputes in groups of five rounds. Our conclusions are not affected by this decision, but the exposition is considerably simplified.

We divided the data into four groups: rounds 1 to 5, 6 to 10, 11 to 15, and 16 to 20. We then pooled the data from the first two groups and the data from the last two groups. To give an example, suppose a pair had two disputes in the first five rounds, three disputes in the next five rounds, three disputes in the next five rounds, and three disputes in the last five rounds. The pair would contribute one observation to the two-disputes without-arbitration category, one observation to the three-disputes without-arbitration group, and two observations to the three-disputes with-arbitration category. The thirty-five years of British Columbia data were also divided into seven groups of five negotiations each: 1947 to 1951, 1952 to 1956, and so on, and these groups were pooled.

Results

The frequency distributions of patterns of disputes for each category of total number of disputes (from one through four) are contained in table 1. If there was no state dependence, then one would expect to see an equal

number of observations of each pattern within each category: Thus the expected number of observations of each pattern is equal to the total number of observations in the number-of-disputes category divided by the number of possible patterns. By comparing the actual number of observations of each pattern with the expected number, we can test whether the null hypothesis of no state dependence fits the data.

Casual examination of Table 1 shows that, within number-of-dispute categories, the differences between the actual and expected number of each pattern are larger for the field data than for the experimental data. We cannot reject the null hypothesis of no state dependence in the experimental data. The chi-squared test statistic for the first ten rounds of the experiment is 24.8 with 26 degrees of freedom (p-value = 0.53), and the chi-squared statistic for the last ten rounds of the experiment is 17.8 also with 26 degrees of freedom (p-value = 0.88). In contrast, the Chi-squared test statistic for the field data is 79.5, again with 26 degrees of freedom (p-value = 2.5e-7) which indicates that the null hypothesis of no state dependence can be rejected with a high degree of confidence in the B.C. field data.

Table 1 also shows that in the experiment disputes became more frequent once arbitration was available. See Ashenfelter et al. (1991) for an extensive discussion of this phenomenon in these data.

Unfortunately, it is difficult to determine from Table 1 whether state dependence in the field is positive or negative. In order to get at this question, we compared the number of times that disputes occurred together (one dispute following another) in the field data to what would be expected if there were no state dependence. If disputes are contiguous more often than one would predict under the null hypothesis of no state dependence, then this is evidence of positive state dependence, and vice-versa. This

comparison is shown in Table 2. Overall, disputes occur contiguously at approximately the rate expected by independence (263 observed contiguous pairs vs. 250.8 expected). There are more than the expected number of observations with contiguous pairs in the two-dispute category, suggesting positive state dependence. However, there is no clear pattern in the three- or four-dispute categories. Thus, while we reject independence in the field data, we find only weak evidence of positive state dependence.

Discussion and Conclusions

We began by stating that the advantage of using experimental data was that we could abstract from some aspects of the arbitration process as it occurs in the field and focus on others. Our experiment captured the facts that employers and unions engage in repeated negotiations, that actual negotiations are unstructured, and that there are losses associated with breakdowns in the collective bargaining process. In addition, we believe that we have incorporated the uncertainty and the reduction in dispute costs associated with the institution of an arbitration system in a realistic way.

We find no evidence of state dependence in the experimental data. Hence our hypotheses that the use of arbitration *per se* leads to state dependence either through reducing uncertainty about the arbitral process or through changing the bargaining parties perceptions about their opponents, are rejected. These results suggest that an explanation for any positive state dependence in the British Columbia field data may lie in aspects of the arbitration process that are not captured by our experimental design.

It may be important to allow for face-to-face negotiations, or for possible principal-agent problems between the bargaining parties and the agents that represent them. If these factors were behind a finding of state dependence, policy makers might well be concerned about the use of

arbitration as a dispute-resolution procedure.

Alternatively, apparent state dependence might be caused by unobserved characteristics of bargaining units that change slowly over time and hence are not captured by our analysis. This sort of spurious state dependence should not be a cause for concern, and it could be the cause of the weak positive state dependence in the field data. While we used rather short runs (5 rounds), our analysis did not control for observable time-varying factors that could be captured in a more complex model that accounted for year effects and other time-varying observable characteristics of bargaining pairs in the field (Currie, 1989).

We believe that experiments along the lines of the one discussed here may prove useful in isolating the causes of state dependence in arbitration data. Further work could start with our "bare-bones" framework and add key institutional features. At the least we have demonstrated that bargaining experiments of this kind can be used to rule out hypotheses with a definiteness that is not usually possible using field data alone.

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Table 1

Actual vs. Expected Number of Observations of Each Possible Pattern of Five-Negotiation Outcomes by Total Number of Disputes

One Dispute Total	Exper 1-10	Exper 11-20	Field B.C.
Expected # in Each Pattern:	15.6	9.6	23.6
Actual # in Each Pattern:			
00001	16	12	24
00010	7	9	33
00100	19	7	31
01000	17	13	11
10000	19	7	19

Two Dispute Total	Exper 1-10	Exper 11-20	Field B.C.
Expected # in Each Pattern:	2.9	3.8	11.4
Actual # in Each Pattern:			
00011	3	5	14
00101	2	5	5
00110	4	2	24
01001	1	2	8
01010	4	3	12
01100	5	3	9
10001	3	4	16
10010	3	3	9
10100	2	6	16
11000	2	5	1

Three Dispute Total	Exper 1-10	Exper 11-20	Field B.C.
Expected # in Each Pattern:	.4	3.8	8.1
Actual # in Each Pattern:			
00111	0	4	14
01011	0	4	4
01101	0	1	9
01110	0	4	11
10011	0	4	2
10101	2	7	13
10110	0	3	6
11001	1	2	7
11010	0	5	9
11100	1	4	6

Four Dispute Total	Exper 1-10	Exper 11-20	Field B.C.
Expected # in Each Pattern:	.4	6.2	9.0
Actual # in Each Pattern:			
01111	1	9	7
10111	0	8	7
11011	0	3	3
11101	0	6	19
11110	1	5	9

Table 2

Observed and Expected Number of Contiguous Pairs of Disputes

British Columbia Data

		# of Contiguous Pairs				
		0	1	2	3	Total
# of Disputes Category						
2	Expected	68.4	45.6			45.6
	Observed	56	58			58
3	Expected	8.1	48.6	24.3		97.2
	Observed	13	37	31		99
4	Expected			27	18	108
	Observed			29	16	106
All	Expected	76.5	94.2	51.3	18	250.8
	Observed	69	95	60	16	263

Note: The expected number of contiguous pairs is computed within each category as the number of observations in the category multiplied by the expected fraction of times that number of contiguous pairs will show up in five trials. The expected fraction is computed conditional on the total number of disputes in the category assuming independence (no state dependence). For example, there are 10 possible configurations of two disputes in five rounds. Six of these ten configurations have no contiguous pairs while four of the ten have one contiguous pair. There are 114 cases with two disputes in the B.C. data, so that the expected number of cases with no contiguous disputes is $0.6 \cdot 114 = 68.4$ and the expected number of cases with one contiguous pair is $0.4 \cdot 114 = 48.6$.