

## Is financial dishonesty a rational decision?

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A field experiment was conducted to test the theory that financial dishonesty is a rational decision in which the likelihood of being dishonest increases with the utility of dishonesty and decreases with its cost. People in the streets of Cambridge, England, were given an opportunity to dishonestly accept a coin from an experimenter under two conditions of utility (low or high) and two conditions of cost (low or high). Utility was varied by the value of the coin, and cost was varied by the experimenter's statement. Thirty-one of the 84 subjects falsely claimed the coin. In agreement with the prediction, more dishonesty occurred in the low cost condition, but, contrary to expectations, the utility manipulation had no overall effect on dishonesty. Cost and sex of experimenter interacted significantly, in that the cost manipulation affected dishonesty only with the male experimenter. It was also found that high utility tended to produce greater dishonesty in the low cost condition than in the high cost condition.

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The decision to commit a crime may be rational, in the sense that it depends on the extent to which the expected utility of the crime exceeds its expected cost. The utility of an event or outcome is its positive subjective value or attractiveness, while the cost (or negative utility) of an outcome reflects its unpleasantness or unattractiveness. In theories of decision making (e.g. Edwards, 1961; Becker & McClintock, 1967), the expected utility of an outcome is the product of the subjective probability of its occurrence and its utility, while expected cost is the product of subjective probability and cost. Given the indeterminism of human behaviour, it is inadvisable to postulate a threshold theory in which people commit a crime if and only if its expected utility exceeds its expected cost. Instead, it is suggested here that the likelihood of committing a crime increases probabilistically with the difference between its expected utility and its expected cost. This is the meaning of a 'rational' decision in this paper. It is not necessary to suggest that subjective probabilities and utilities are combined at the conscious level, but only that people behave as if they are considering expected utilities and expected costs. In the simple case where there are only two possible outcomes of a crime, namely successful commission and being caught by the police, the likelihood of committing a crime should increase as the subjective probability and utility of successfully committing it increase, and should decrease as the subjective probability and cost of legal punishment increase. Although this formulation is not exactly equivalent to any other, the ideas can be traced back to Jeremy Bentham (Geis, 1955). More recently, Short & Stroudbeck (1965) and J. Cohen (1972) have emphasized the role of subjective probabilities and utilities in crime decision making, while Piliavin *et al.* (Piliavin, Hardyck & Vadum, 1968; Piliavin, Vadum & Hardyck, 1969) and Phillips & Votey (1972) suggested that the commission of delinquent acts depended on the relative sizes of the rewards and costs of them.

The assumption that crime is rational is central to the concept of deterrence. The major problem in trying to use deterrence research to test the above formulation is its frequent failure to separate changes in the severity of legal punishments from changes in their probability. The results of deterrence research become less inconsistent as the studies become more adequate methodologically. The most inconsistent results have been obtained in 'ecological' surveys in which crime rates, usually in different states of the USA, were related to indices of the severity and certainty of legal punishments in those states (e.g. Tittle, 1969; Chiricos & Waldo, 1970; Logan, 1972). The major difficulty with these surveys, apart from the inadequacy and non-comparability of official statistics, is to establish that any differences in crime rates really could be attributed to differences in legal punishments rather than to one or more of the many

other differences between areas. Correlational studies based on individuals are superior methodologically to ecological surveys, but it is still difficult to draw unambiguous conclusions from them. Both Willcock (1974) in England and Waldo & Chiricos (1972) in the USA found that youths who admitted committing large numbers of delinquent acts gave low estimates of the likelihood of being caught by the police. Contrary to these findings, Grupp, McCain & Schmitt (1971) showed that marijuana users gave higher estimates of the probability of detection for this crime than non-users. However, the users gave lower estimates of the likelihood of other unpleasant consequences, such as health damage and psychological dependence, so the overall results from this survey do not necessarily conflict with the rational theory of crime outlined here.

Apart from questions about the validity of self-report measures of delinquency (e.g. Hood & Sparks, 1970; Farrington, 1973), it is difficult to establish causal order in these correlational studies. One wonders, for example, whether low estimates of the probability of detection precede or follow the commission of large numbers of delinquent acts. Quasi-experimental methodologies provide information about causal order. Ross, Campbell & Glass (1970) concluded that the introduction of the British breathalyser resulted in a decrease in drunken driving, although Schwartz (1968), using a less sophisticated method of analysis, found no effect on rape in Philadelphia of the introduction of more severe legal penalties. Both of these studies suffer from the difficulties of relying on official statistics, and their results are less conclusive than would have been the case in true experiments. Tornudd (1968), with police cooperation, was able to study the effect of systematically decreasing the likelihood of being prosecuted for drunkenness in three experimental towns in Finland, in comparison with three control towns. The decrease had no effect on the incidence of drunkenness, and Tornudd concluded that this was because the drinkers were not aware of any change in policy. Buikhuisen (1974) found that a special police campaign in Holland, backed up by newspaper publicity, was effective in reducing the incidence of driving with worn tyres in an experimental town in comparison with a control town. Both Tornudd and Buikhuisen had some degree of control over legal penalties, but their experimental units were towns. Schwartz & Orleans (1967) carried out what seems to be the only true experiment on deterrence involving random allocation of subjects to different conditions. They found that income tax evasion decreased slightly in a group reminded of the sanctions for it, although less than in a group given moral reasons against evasion. This experimental methodology is likely to produce the most conclusive results.

True experiments in which one or more independent variables are manipulated and criminal behaviour is the dependent variable are rare. This is surprising in view of the recent increase in the number of experiments with aspects of crime as an independent variable (e.g. Steffensmeier & Terry, 1973; Buckhout *et al.* 1974; Bickman, 1975). Almost all experiments on criminal behaviour as a dependent variable were designed to evaluate the relative effectiveness of different methods of treating convicted persons (e.g. Palmer, 1971; Empey & Erickson, 1972), despite practical difficulties such as getting the cooperation of institutional staff and theoretical difficulties such as the 'blunderbuss' nature of the treatment (e.g. Clarke & Cornish, 1972). Small-scale experiments in the related area of cheating have been carried out for many years, although it is more usual in them to have classes of children rather than individual subjects randomly allocated to different conditions. Some of the results obtained in cheating experiments seem directly relevant to the theory that crime is a rational decision. Mills (1958), Kanfer & Duerfeldt (1968), Hill & Kochendorfer (1969) and Johnson & Gormly (1972) all found that cheating decreased as the likelihood of detection increased. Mills (1958) and Dmitruk (1971) showed that cheating increased as more attractive prizes were offered, and Tittle & Rowe (1973) and Heisler (1974) reported that threats of sanctions decreased cheating. All these results suggest that decisions to cheat are influenced by rational considerations, in the sense used here.

The aim of this paper is to investigate whether decisions involving financial dishonesty are also

rational, in a field experiment involving random allocation of subjects to different conditions. There are a small number of attempts in the psychological literature to obtain behavioural measures of financial dishonesty or stealing under controlled conditions. In the pioneering work of Hartshorne & May (1928), and in the work of Brock & Del Giudice (1963), children were given opportunities to steal coins, while White (1972) gave children opportunities to steal gift certificates exchangeable at a local store. Merritt & Fowler (1948), in their influential research on the 'lost letter' technique, showed that letters left on the street and apparently containing coins were far less likely to be returned unopened than other letters. Hornstein, Fisch & Holmes (1968) did leave letters on the street containing money, but these researchers were interested in helping behaviour rather than stealing. Beigel (1962) and Coe, Kobayashi & Howard (1972) tried to manipulate stealing using hypnosis. Feldman (1968) described experiments in which people were approached in the streets of Paris, Athens and Boston and asked if they had just dropped some money. He found that the likelihood of dishonestly claiming the money decreased, slightly but not significantly, as the amount of money increased. Feldman also tried to overpay cashiers in shops in the three cities, and noted whether the cashiers dishonestly kept the overpayment. Bickman (1971) carried out an experiment in which coins were left in phone booths in New York and the people who went inside were asked whether they had found the coins. He showed that people were more likely to keep the coins dishonestly if asked by someone in lower class clothes than if asked by someone in upper class clothes. Finally, Diener, Fraser, Beaman & Kelem (1976) gave children opportunities to steal candies or coins from a house, and found that stealing increased when the children were in a group and when their names were not known to the householder.

### **Method**

The measure of financial dishonesty used in this research was similar to that used by Feldman (1968). People in the central shopping streets of Cambridge, England, were given an opportunity of dishonestly accepting a coin which an experimenter offered them. The experiment employed a 2×2 between-subjects factorial design, with two levels of utility (low or high) and two levels of cost (low or high). An attempt was made to vary the utility of dishonesty by offering either a 10p (low utility) or 50p (high utility) coin. An attempt was made to vary the cost of dishonesty by changing the experimenter's statement. In the low cost condition, the experimenter said 'Excuse me, I think you dropped this', while in the high cost condition he said 'Excuse me, did you drop this?' It can be argued that the cost of dishonesty was greater in the second condition, because it was more difficult and more unpleasant for the subject to have to tell a lie in order to claim the coin. In the first condition, it was easier for the subject to accept the coin passively. In the interests of simplicity, the design was intended to investigate the immediate utility and immediate cost of only one outcome, dishonestly claiming the coin. No attempt was made to make the subject aware of possible future outcomes, or to vary their subjective probabilities. On the basis of the above theory that crime is a rational decision, we would predict more dishonesty in the high utility condition, more in the low cost condition, and no interaction between utility and cost. One male and one female student, both neatly dressed and aged 20, served as experimenters. On an *a priori* basis, the theory would not predict that sex of experimenter would influence dishonesty.

The procedure was as follows. At the beginning of each trial the experimenter consulted a stack of index cards to determine the experimental condition to which the next subject would belong. Each card contained the name of one of the four conditions, and the cards were arranged in a random order. Once the experimenter had determined the experimental condition, the next person (other than a child) who was alone and had at least one arm free was designated as the subject. The experimenter then positioned himself so that he and the subject would walk past each other. When he was about 10 feet past the subject, the experimenter bent down as if picking something off the ground. He then turned and ran after the subject, calling 'Excuse me', and when he was parallel with the subject he offered one of the two coins and made one of the two statements. The experimenter then waited for the subject's reply in silence. If the subject refused the coin, the experimenter prompted him once by saying 'Are you sure?', and then turned and walked away. After the completion of each trial, the experimenter recorded the condition and the outcome on a data sheet, together with some information about the subject and the situation (sex, estimated age, dress, social class, place, day, time, and any statement made by the subject). One of us (R. F. K.) observed

some of the trials to ensure that the experimenters were behaving as we wanted. The experimenters were not blind to the experimental conditions, but they were not told the experimental hypotheses.

In all, 84 subjects were tested, with about ten in each of the cells of a  $2 \times 2 \times 2$  design comprising sex of experimenter, cost and utility. The dependent variable was whether or not the subject claimed the money. The subjects included 49 males and 35 females; 54 who were judged to be aged 40 or less and 30 older; 27 dressed casually (e.g. jeans, anorak) and 57 more smartly; and 21 judged to be lower class on the basis of speech, 45 judged to be middle or upper class, and 18 judged to be students.

## Results

Thirty-one of the 84 subjects (36.9 per cent) dishonestly claimed the money. An analysis of variance was carried out to compare the dependent variable of financial dishonesty with the independent variables of utility, cost and sex of experimenter (see McNemar, 1962; Kidd & Berkowitz, 1976, for a discussion of analysis of variance with dichotomous measures). Table 1 shows that the main effects of cost and sex of experimenter were statistically significant. Nearly twice as many of those in the low cost condition (46.5 per cent as opposed to 26.8 per cent in the high cost condition) falsely claimed the money, and nearly twice as many of those tested by a male experimenter (47.5 per cent as opposed to 27.3 per cent with a female experimenter). In contrast, the manipulation of utility had no effect on dishonesty (35.6 per cent of those in the low utility condition being dishonest, in comparison with 38.5 per cent of those in the high utility condition). There was also a significant interaction between cost and sex of experimenter and a near-significant interaction between cost and utility. The other interactions were not significant. Table 2 shows the significant interactions in more detail. The cost manipulation affected dishonesty only with the male experimenter, and not with the female experimenter. Similarly, the cost manipulation affected dishonesty only in the high utility condition, and not in the low utility condition. Alternatively, it could be said that high utility produced high dishonesty in the low cost condition, but that high utility produced low dishonesty in the high cost condition.

**Table 1.** Summary of analysis of variance

Variables	<i>F</i> (d.f. = 1, 76)	<i>P</i>
Main effects		
Utility	< 1	—
Cost	3.94	0.05
Experimenter	4.21	0.04
Interactions		
Utility × cost	3.50	0.06
Utility × experimenter	< 1	—
Cost × experimenter	9.77	0.003
Utility × cost × experimenter	< 1	—

**Table 2.** Percentage dishonest in different categories

		% (n)		% (n)	$\chi^2$	<i>P</i>
Male experimenter	Low cost	71.4 (21)	High cost	21.1 (19)	8.23	0.004
Female experimenter	Low cost	22.7 (22)	High cost	31.8 (22)	0.11	n.s.
Low utility	Low cost	37.5 (24)	High cost	33.3 (21)	0.00	n.s.
High utility	Low cost	57.9 (19)	High cost	20.0 (20)	4.42	0.04

The  $\chi^2$  statistic and *P* value show the significance of the difference between the two percentages in a row, based on the figures in a  $2 \times 2$  table.

Dishonesty showed no signs of being related to any characteristics of the subject. Males were slightly, but not significantly, below average in their dishonesty rate (32.7 per cent), as were those over 40 (33.3 per cent), those dressed casually (29.6 per cent) and those judged to be lower class (33.3 per cent). Furthermore, none of these characteristics was significantly related to any of the three independent variables of utility, cost and sex of experimenter. This shows that the random allocation was successful, and that the above results cannot be explained away on the basis of different subject populations in the different conditions.

These results can be compared with those obtained by Feldman (1968). He used only male subjects, and his two male experimenters comprised one compatriot and one foreigner in each of the cities of Paris, Athens and Boston. His procedure corresponded to the high cost condition in the present experiment, since his experimenters approached the subjects saying 'Excuse me, did you drop this money?' His lower monetary value (the equivalent of one dollar) corresponded approximately to our high utility condition, and his higher monetary value was between two and five times greater. The percentage of subjects falsely claiming the money in our high cost-high utility condition (20 per cent) was somewhat higher than Feldman found in Paris (8 per cent), Boston (10 per cent) or Athens (12 per cent). If the numbers had been sufficiently large to permit the investigation of only the male experimenter-male subject combination in this condition, it is likely that the dishonesty rate would have been at least as high. While female subjects were more likely to falsely claim the money from both experimenters, the male experimenter-male subject combination was associated with a slightly above average dishonesty rate (41.7 per cent). Our finding that the utility manipulation had no main effect on dishonesty is consistent with Feldman's results, even though his monetary values were larger than ours. This suggests that variations in utility over quite a wide range have no main effect on dishonesty.

## **Discussion**

The results of this experiment do not wholly support the theory that financial dishonesty involves a rational decision. In agreement with the theory, decreasing the cost of the act resulted in the expected increase in dishonesty, but only with the male experimenter. It would be interesting to establish whether this result was replicable with other male and female experimenters. If so, it might perhaps be suggested that the costs of falsely claiming money from the young female were higher than from the young male, and that, in the case of the young female, the experimental manipulation of costs did not succeed in reducing them significantly. Contrary to the theory, increasing the utility of the act did not result in the expected uniform increase in dishonesty. However, increased utility did seem to be associated with increased dishonesty at the low cost level, but associated with decreased dishonesty at the high cost level. Contrary to the theory, cost and utility did not have additive effects. It may be that cost and utility were not independent in this experiment, and that an increase in the value of the money which could be falsely claimed simultaneously resulted in a disproportionate increase in the cost of claiming it.

Objectively, the behaviour of the subjects in the present experiment was dishonest, in that they were claiming money which did not belong to them. Subjectively, however, it is possible that some of them really did believe that the money was theirs. In our opinion, the number of subjects who genuinely believed this was extremely small. According to the experimenters' notes, most of those who claimed the money did so with alacrity and with no hesitation, often appearing very pleased and thanking the experimenter. Some seemed insincere in saying that the coin was theirs, and some showed signs of guilt, being flustered or hurrying away afterwards. Some tried to justify their acceptance of the coin by saying 'I often drop money' or 'I must have a hole in my pocket', although one said more frankly 'You never know your luck'. In contrast, many of those who refused the coin hesitated, checked bags or pockets, nearly accepted the money, or accepted it and then gave it back. Although we had no measure of

reaction time, it seemed from the experimenters' notes that the decision to refuse the coin took longer than the decision to accept it. One fairly typical refuser nearly accepted the coin, then said it was unlikely to be hers since she did not see how it could have fallen, then said 'I'd take it if I thought it was mine, but why don't you have it?' A few were certain that the coin was not theirs, saying that they had not brought any money with them or that they had not had any 10p (or 50p) coins. Two refusers were annoyed or rude, and one engaged the experimenter in a discussion about the honesty of taking coins from people when you knew that they did not belong to you. As far as we can tell, no subject thought that the request was anything other than genuine, although one initially thought that it was a joke. Some others initially misunderstood what the experimenter was saying, thinking for example that he wanted them to give change. There was some evidence in the experimenters' notes that subjects had correctly perceived whether the coin was a 10p or a 50p piece, and no evidence of mistakes on this score. It would perhaps have been more satisfactory to debrief the subjects and to include a post-experimental questionnaire designed to investigate the validity of the measures of dishonesty, cost and utility. This was not done because of the dangers of unfavourable reactions from subjects and unfavourable publicity, and also because of the practical problems of detaining people on the street for any length of time without their prior consent. It is one thing to debrief an introductory psychology student participating in a social psychology experiment as a course requirement, and quite another to debrief a member of the general public who did not even realize that he was participating in an experiment.

Naturalistic field experiments of this kind raise ethical problems, partly because of the element of deception and the absence of debriefing, and partly because the dependent variable is unethical behaviour. Such experiments should only be carried out if it is believed that the benefits in terms of increased knowledge exceed the costs in terms of the effects on the subjects. It seems to us that this experiment had a negligible impact on the lives of the subjects, and, furthermore, that the impact would have been much greater if there had been any attempt at debriefing. It is true that the subjects were presented with an opportunity to be dishonest, but such opportunities are not unusual. The experimental paradigm is not too different from the real-life situation of being given too much change by a shop assistant, for example. It would not be realistic to argue that gaining money dishonestly in such a situation might encourage someone to commit serious crimes. In trying to resolve ethical issues, it would be helpful to carry out surveys of samples drawn from the subject population to establish what they think about an experiment, and also to try to investigate the long-term effects of experimental manipulations. Such evidence as we have at present suggests that even extremely stressful experiments such as those carried out by Milgram (1974) have negligible long-term effects. Furthermore, after the purpose of the experiment had been explained to them, 84 per cent of Milgram's subjects said that they were glad to have taken part, 15 per cent were neutral, and only 1 per cent expressed negative views about his experiments.

The great methodological advantage of field research over laboratory research, especially when the subjects are not aware that they are participating in an experiment, is its greater external validity, or the increased ability to generalize from the experiment to real life. This is particularly true when the subjects are drawn from the general population rather than from introductory psychology classes. How far it is possible to generalize from the dishonest behaviour studied in the present experiment to more serious criminal behaviour, however, is an empirical question. For example, it was noted above that males were slightly but not significantly less dishonest than females in the experiment, whereas the 1975 *Criminal Statistics* show that nearly four times as many males as females were found guilty of indictable offences of theft and dishonest handling. It may be that experiments in which persons of all ages, sexes and social classes are given opportunities to steal or to be dishonest might yield a more accurate picture of the reality of crime than the official statistics. Systematic observation and recording of criminal behaviour as it happens is rare (cf. Zimbardo, 1969).

Up to the present time, most theories about criminal behaviour have been process theories in which present behaviour is explained by reference to a sequence of past events. This is equally true of psychological approaches such as social learning theory (e.g. Trasler, 1973) and sociological perspectives such as labelling or social reaction (e.g. Taylor, Walton & Young, 1973). The major problem in evaluating such theories is to devise a methodology which permits unambiguous conclusions about their adequacy. Neither participant observation methods (e.g. S. Cohen, 1972), correlational research (e.g. Belson, 1975), longitudinal surveys (e.g. West & Farrington, 1977), nor quasi-experiments (e.g. Farrington, 1977) allow this. It seems to us that theories about immediate, situational influences on criminal behaviour are more susceptible to testing, and that naturalistic field experimentation in which subjects are randomly allocated to different conditions is the most satisfactory methodology for testing such theories. The present paper is intended to be one small step in this direction.

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