

STATUS, EXCUSES, AND JUSTIFICATIONS

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ABSTRACT

We report the results of an experiment studying the interaction of status with type of account (excuses vs justifications) in determining the acceptance of accounts. Excuses and justifications were selected that had the same rank order of acceptability when rated by an independent sample drawn from the same population as the subjects of the experiment. Normative expectations were violated in a computerized tragedy of the commons task by an authority who offered either an excuse or a justification. We found that status was positively correlated with acceptance of justifications but negatively correlated with acceptance of excuses. In addition, unlike much recent research on accounts, justifications were more readily accepted than excuses. We conclude from this that it is a mistake to overgeneralize from previous findings that excuses are always more readily accepted than justifications. Concrete details of offense and setting appear to interact with type of account in determining their acceptance.

STATUS, EXCUSES, AND JUSTIFICATIONS

I. INTRODUCTION.

In theories of accounts there are good reasons for believing that acceptance of any kind of account, whether an excuse or a justification, is positively correlated with status. (See especially Blumstein, et al, 1974; Hunter, 1984; Scott and Lyman, 1968.) In attribution theories there are good reasons for believing that acceptance of excuses, but not justifications, is inversely correlated with status. (This can be shown to follow from either Jones and McGillis, 1976 or Kelley, 1967, but see especially Thibaut and Riecken, 1955 and Hamilton, 1978; 1980.)

Where theories of accounts predict that status has the same effect on acceptance of any kind of account, attribution theories imply a status by type-of-account interaction. In a previous paper we found support for the hypothesis that, unless a justification is unambiguously invalid, its acceptance increases with status (Massey, Freeman, and Zelditch, 1997). In the present paper we ask whether status has the same effect on excuses that it has on justifications.

II. HYPOTHESIS.

The idea of linking theories of accounts and attributions is not especially new. (E.g. see Crittenden, 1983; Snyder, Higgins, and Stucky, 1983.) Both are about how ordinary people explain behavior. In theories of accounts, an unexpected, untoward act is normalized—made warrantable and intelligible (from Garfinkel, 1964)—by an explanation that either accepts that the act is wrong but denies responsibility for it (excuses) or accepts responsibility for it but

denies that it is wrong (justifications) (from Austin, 1956). Attribution theories describe how events come to be attributed to one or more of their possible causes (Heider, 1958), hence how actors come to be held responsible for them (Hamilton, 1978). Causes are either internal (attributed to actors) or external (attributed to situations). In either case they are either stable or transient. Ability is a stable, fatigue a transient, internal cause. Task difficulty is a stable, luck or accident a transient, external cause. The theory of account's excuses are fashioned by appeal to attribution theory's external, or transient-internal, causes. Either diminishes responsibility for an unexpected, untoward act. Attribution theory complements the theory of accounts because whether an excuse is accepted or not is an important dependent variable in the theory of accounts; acceptance should depend in part on the credibility of the excuse; and whether an excuse is credible or not is something attribution theory should be able to explain.

Status plays a role in both theories. In the theory of accounts, status is factor that gives an actor the capacity to influence another to accept an account (Scott and Lyman, 1968). In attribution theory, category expectancies (such as status) are a factor from which knowledge, ability, and control, hence locus of causality, are inferred (Jones and McGillis, 1976; Thibaut Riecken, 1955). In the more sociologically oriented "intuitive lawyer" variant of attribution theory, role expectations play the same role in inferences of responsibility (Hamilton, 1978; 1980). But the part status plays in the two theories is different. In theories of accounts, status is an advantage whether the account is an excuse or a justification. But in attribution theories status is a liability. Attribution theories have nothing to say about justifications, but about

excuses they imply that external, or internal-transient, causes are more credible of low than high status.¹ The reason why attribution theory implies a negative effect of status on the acceptance of excuses is that, in both its forms (i.e. for both "intuitive psychologists" and "intuitive lawyers"), status is positively correlated with knowledge, ability, freedom and control. (See especially Jones and McGillis, 1976; Hamilton, 1978, 1980; Thibaut and Riecken, 1955.) Knowledge, ability, freedom and control are positively correlated with inferences of intentionality, stable-internal causes, and responsibility. But intentionality, stable-internal causes, and responsibility diminish the credibility of excuses.²

The hypothesis that status is correlated with expectations of knowledge, ability, freedom and control has been consistently supported by empirical studies (Hamilton, Blumenfeld, and Kushler, 1988; Hamilton and Sanders, 1981; 1983; Nazareth and Kanekar, 1986; and, less robustly, Hegtvedt, Thompson, and Cook, 1993). Empirical studies have also consistently supported the hypothesis that expectations of knowledge, ability, freedom and control are positively correlated with internal causality (Thibaut and Riecken, 1955) and attributions of responsibility (Hamilton, Blumenfeld, and Kushler, 1988; Hamilton and Sanders, 1981; 1983).

But few studies have attempted to investigate a status by type-of-account interaction and where they have, evidence of a status by type-of-account interaction is at best inconclusive. Looking at all studies in which there is any inequality (status, power, authority), any account variable (demands, offers, acceptance), and any account (excuses, justifications, both): Interactions are sometimes found and sometimes not. Where an interaction is found, its pattern is not consistent from study to study (citations below).

Comparing studies of justifications with studies of excuses does suggest an interaction. Gonzales, 1992 and Gonzales, et al, 1990 found that justifications were used more often and more successfully by high than low status offenders. Massey, Freeman, and Zelditch, 1997 also found that, unless a justification was consensually invalid, its acceptance was positively correlated with status. On the other hand, neither offers of excuses (Wiley and Eskilson, 1981) nor their acceptance (Ungar, 1981) are much affected by status. This suggests a status-by-type of account interaction, but not the one suggested by attribution theory.

Studies in which excuses can be more directly compared with justifications seldom find any interaction at all. Four find no status effects on either (Hunter, 1993; Hunter and McClelland, 1991; McClelland and Hunter, 1992; Riordan, Marlin, and Gidwani, 1988). Five find positive effects of status but no status by type-of-account interaction (Blumstein, et al, 1974; Felson and Ribner, 1981; Much and Schweder, 1978; Walton and Sedlak, 1982; Weinstein, 1980).

Three do find a significant interaction (Gonzales, 1992; McLaughlin, Cody, and O'Hair, 1983; Shields, 1979), but the direction of the interaction is not consistent from study to study. And the direction found by Shields (1979), that excuses are positively but justifications negatively associated with status, is inconsistent with the hypothesis suggested by attribution theory. But even if the interactions were consistent, there would be a problem trying to draw conclusions about a status by type-of- account interaction from this body of research. It is difficult to compare the effect of status on the acceptance of excuses with its effect on the acceptance of justifications without some independent criterion equating acceptability of the

particular excuses used with that of the particular justifications used.

It is the fact that we think we have a reasonable method of comparing the two kinds of accounts that led us to the experiment we report in the present paper. In this experiment, we will look at a subject (B)'s, private acceptance of an excuse or a comparable justification by a confederate (A) of an unexpected, untoward act by A, where "private" acceptance means that B's response is not observable to A. The hypothesis we test is that

HYPOTHESIS 1. There is a status by type-of-account interaction such that

- a. B's private acceptance of a justification by A is directly proportional to the status of A relative to B;
- b. B's private acceptance of excuses by A is inversely proportional to the status of A relative to B.

HI. METHOD

A. Setting.³

The setting we used to test this hypothesis was a computerized version of the tragedy of the commons dilemma (Messick, et al, 1983), and the subjects understood the experiment to be a study of this dilemma. Two subjects, occupying separate rooms and working on separate computer terminals, were instructed by computer that each represented a community each of which possessed local water supplies fed from a single, finite, reserve. (The dilemma was given this particular form because of drought in California at the time.) The dilemma was to balance personal interests in providing as much water as possible for their community against the collective interest of both communities in sustaining the water supply for as long as possible.

Each subject earned one point for each unit of water used, and points earned money at the end of the experiment. But no one could continue to use water if anyone used up the total water supply.⁴ Subjects did not know how much water was used by the other community.

The experiment consisted of ten trials, or turns, at each of which each subject withdrew a variable amount of water from his/her community's local reserve for use by his/her community. At the end of each trial, the local community reserve was replenished from the general reserve by a "water monitor" (WM). WM's behavior was pre-programmed by the computer, hence WM functioned like a confederate of the experimenter. Subjects were told that they would complete three rounds of ten trials each, but in fact they completed only one round.

Before the first trial of the experiment, subjects voted on whether the WM should replenish local reserves after each turn at a fixed rate (of one-half of whatever was left in the local reserve) or, alternatively, at whatever rate the WM thought fit. The vote was manipulated so that it always turned out two-to-one in favor of the fixed rate.

Thus, the vote established the expectation that subjects would be replenished at a rate of one half the amount of water left in their local reserve at each turn, an expectation that WM violated on four of the ten turns. Subjects on these four critical trials were replenished with zero units of water. Nor was there any possible way to mistake what WM was doing or who was doing it.

Such an unambiguous offense is well outside the scope of existing evaluation-expectation theories of status, and threatens any attempt to manipulate the status conditions

of the accounts process, especially in a repeated measures design. But it is incongruence between expectations and actions that activates normalization processes, hence the veridical offense.

The ten trials of the experiment were arranged in a fixed order in such a way that in each of two blocks of five trials there were two replenishment (R) trials, on which WM behaved as expected; followed by one non-replenishment (NoR), offense trial, on which WM violated expectations without offering an account; followed by one replenishment trial on which expected behavior immediately followed an offense for which there had been no account; followed by one non-replenishment, account (NoR,A) trial on which an offense was immediately followed by an account. Accounts were introduced only at trials five and ten. But, because of the repeated measures design, the second block of trials differed from the first in that all the behavior of the WM in the second block of trials could be interpreted by subjects in the light of the first account at trial five.⁵

Accounts were offered whether or not subjects demanded them. It was possible for subjects to question the behavior of the WM on any trial, but they were instructed that, in order not to unduly disrupt the flow of the experiment, their questions would only be answered after selected turns. Whether or not a subject ever asked a question, at trial five s/he received an explanation responding to any questions asked by anyone during trials one to five and at trial ten s/he received an explanation responding to any questions asked by anyone during trials six to ten. The two accounts were the same except for small differences in wording. Following the first offer of an account at trial five, the second at trial ten was worded in a way

that referred to the first in order to maintain plausibility.

B. The Dependent Variable.

Following each trial, subjects completed a computerized questionnaire designed to assess their responses to the WM's behavior. They were instructed that in addition to examining the behavior of individuals in the commons dilemma, we were also interested in their impressions of it. Among other questions, they were asked to indicate on a five-point scale (where 5 was the highest score) the extent to which they regarded their own actions, the other participant's actions, and the WM's actions on the most recently completed turn to be appropriate, justified, legitimate, and fair. An index of the acceptability of the WM's account at each trial was made by summing responses to the WM on these four items, which measure the subjects' beliefs in the propriety of the WM's behavior on that trial (cf Massey, Freeman, and Zelditch, 1997).

Measured in an earlier experiment in the same setting (Massey, Freeman, and Zelditch, 1997), this index had an average alpha of .92 over the ten trials of the experiment and alpha decreased if any of the four items was deleted from the scale. There was some difference between alpha for the first trial of the experiment and all other trials. It was .85 on the first trial of the experiment. Because many subjects still treated this trial as a practice trial, we decided to omit it in the present paper. Alpha increased to .91 on the second trial and thereafter ranged from .90 to .96.

C. Comparability of Excuses and Justifications.

Excuses and justifications were generated and ranked by independent samples of

subjects drawn from the same population as those used in the present experiment. One sample of 20 female subjects was led through five trials of the commons dilemma task, after which they were asked to write down what they thought would be a good or bad excuse and a good or bad justification for the water monitor's failure to replenish local water reserves. A second sample of 22 male and 40 female subjects, drawn from the same population (Stanford undergraduates with no previous experience in deception experiments), was again led through five trials of the commons dilemma task and then asked to rate on a seven-point scale the acceptability of the excuses and justifications generated by the first sample of subjects.⁶

In the present experiment we equated the acceptability of excuses and justifications by using two accounts each of which was at the mid-rank of the mean ratings given by the second sample of subjects. Accounts at the mid-rank were comparatively ambiguous: They were at the mid-rank because there was less consensus about rating them. Nevertheless, we believe that our procedure makes the acceptability of the excuses we used comparable to that of the justifications we used for the population from which the subjects were sampled. (It would be unwise to generalize the comparability of the two beyond the population used for the experiment.)

Ninety-two subjects participated in the experiment. Forty-two of these heard excuses. The excuse was, "I didn't refill your local reserve because the records got mixed up--out of my control." Fifty heard justifications. The justification was, "I didn't refill your local reserve because we should wait for bad times to dig into reserves."

D. Status.

We crossed these two accounts with two levels of status. Forty-three of the ninety-two subjects interacted with a (fictitious) equal status WM, who they were told was randomly chosen from the same subject pool that they themselves were chosen from. Forty-nine interacted with a WM specially chosen for having completed a PhD dissertation on water resource management.

IV. RESULTS.

The basic design of the experiment crossed two between factors (status and type of account) with one repeated factor (trial). A preliminary analysis was made of a 2 (status) by 2 (type of account) by 9 (trials) repeated measures analysis of variance (RMANOVA), but the more important analysis is a 2 x 2 x 6 RMANOVA of trials 5-10, i.e. the trials including and following the first account. Because the within-subjects variable violated the assumption that the variances of all differences are equal, we report only the multi-variate RMANOVA which does not require this assumption (Girden, 1992; Stevens, 1986). (Significance levels are reported for the Pillais-Bartlett Trace.)

A. Preliminaries.

The main effect of trial was significant for trials 2-10 ($P=.000$), suggesting that each trial of the experiment had a unique effect. That the non-replenishment on trial 3 was unexpected and untoward is evident from the sharp decrease in acceptability of the WM's behavior between trials 2 and 3 (see table 1). Averaging

TABLE 1 ABOUT HERE

across between-subjects factors, WM lost 7.59 points from trial 2 to trial 3. ($P=.000$ in an

analysis of trial by trial differences.) Although we did not predict it (because the offense is unambiguous), there was also a significant effect of status on the number of points lost between the two trials ($P=.008$).

Finally, there was also a main effect of type of account in RMANOVA of trials 5-10 ($P=.000$). Cross-sectional ANOVA found this effect to be significant at trial 8 ($P=.000$), at which WM repeated the offense without explanation, as well as trials 5 ($P=.000$) and 10 ($P=.001$), suggesting a strong carry-over effect of the account on trial 5. Where the effect was significant, justifications were consistently more acceptable than excuses.

This result has some importance because the direction of the effect contradicts McLaughlin, Cody, and O'Hair's (1983) hypothesis that, because justifications threaten other's "face" more than excuses, they are less readily accepted. This hypothesis has been supported by several investigations (Hunter and McClelland, 1991; McClelland and Hunter, 1992; Riordan, Marlin, and Gidwani, 1988; Riordan, Marlin, and Kellog, 1983). But there is considerable other evidence that concrete circumstances of task, situation, and offense make a significant difference to the effectiveness of different kinds of accounts (Bell, Zahn, and Hopper, 1984; Blumstein, et al, 1974; Howard and Pike, 1986; Hunter, 1993; Much and Schweder, 1978; Tedeschi, et al, 1983). This suggests that it would be a mistake to generalize the effectiveness of justifications from the present experiment. At most, this finding tells us that it is equally a mistake to generalize from earlier investigations that excuses are always more readily accepted than justifications.

B. The Interaction of Status and Type of Account.

The interaction of status with type of account in RMANOVA of trials 5-10 falls short of the 5% level of significance ($P=.1$). Nevertheless, the results are sufficiently consistent that we believe it reasonable to conclude that status is an advantage when an offense is justified⁷ but a liability when it is excused. The behavior of high status WM's is consistently more acceptable than that of low status WM's when WM offers a justification of an offense, except for trial 7. (Trial 7, the WM's second repetition of expected behavior after the offense and account on trial 5, behaves differently from all other trials in block 2 not only in this but in three previous experiments in this setting. See Massey, Freeman, and Zelditch, 1997). Though the pattern is less consistent for excuses, the behavior of high status WM's is less acceptable than that of low status WM's when WM offers an excuse in five of the six trials in block 2. (There is no difference between the two on trial 6.) Averaging across trials 5, 6, 8, 9, and 10—omitting trial 7 because of our experience with it in earlier experiments—the behavior of high status WM's is 1.32 points more acceptable than that of low status WM's if WM offers a justification, while the behavior of high status WM's is .75 points less acceptable than that of low status WM's if WM offers an excuse. Though the significance levels are again soft, the status by account interaction is significant or almost significant on trials 5 ($P=.08$), 8 ($P=.09$), and 9 (.04).

V. CONCLUSION

Despite the relaxed significance level of the last analysis, we believe the data support the argument that status does not have the same effect on excuses that it has on justifications. The data confirm Scott & Lyman's (1968) hypothesis that status, because of its influence effect,

positively affects acceptance of justifications, but not their assumption that excuses behave like justifications. Instead, the present experiment supports the implication of attribution theory that, in excuse-making, status is a liability. Theories of accounts and attribution theory share the assumption that status creates expectancies: The higher the status the greater the expected ability and control over the situation. They differ over the consequences of such expectations for the credibility of excuses. What the present data support is the argument by attribution theory that such expectations are more likely to undermine the credibility of excuses by high than by low status offenders.

We also found that, unlike much recent research on accounts, justifications were more readily accepted than excuses. But this is probably due to concrete details of offense and setting. All we can conclude from it is that it is a mistake to overgeneralize from previous findings that excuses are always more readily accepted than justifications.

FOOTNOTES

1. There is a great deal about attributions in an accounts context that will not be affected by status. For example, independent of status, unexpected, untoward acts are likely to give rise to attributions of stable, internal causes because they are non-common (Jones and Davis, 1965), non-consensual and non-normative (Kelley, 1967), and are likely to be hedonically relevant and personalized (Jones and McGillis, 1976).
2. Weiner, et al's (1972) theory of achievement attribution, in which the inferred cause of high status success will be stable and internal, but the inferred cause of high status failure transient

and external, does not contradict this argument because unexpected, untoward acts are only "failures" after the fact, the definition of which is what is at issue in an account.

3. This and the next section, including notes 4-6, are taken almost word for word from Massey, Freeman, and Zelditch, 1967.

4. The setting introduces mixed motives, which are typically beyond the scope of theories of accounts, but the only relation we study in the present paper is between the subject and a (fictitious) water monitor whose only function is to replenish local water supplies and whose only motive is to maintain the common supply of water.

5. A fixed order confounds within-subjects treatments with trial effects, but we did not randomize the trial at which offenses or accounts occurred because we do not study any within-subject effects in this experiment.

6. Phase 2 found no gender differences in rating or reacting to accounts. We balanced gender as far as possible but disregard it in the present paper. We also pretested our status manipulation (see below) in phase 2. It had no effect on how subjects rated the acceptability of either excuses or justifications.

7. Massey, Freeman, and Zelditch (1967) found that status was a liability if justifications were unambiguously unacceptable, hence the present finding is limited to justifications that are either unambiguously acceptable or else ambiguous.

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Table 1: Mean Acceptability of the Water Monitor by Status and Type of Account by Trial

Type of Account	Status Condition	n	Trial*										Trials 5 to 10
			2 R	3 NoR	4 R	Trials 2 to 4	5 NoR,A	6 R	7 R	8 NoR	9 R	10 NoR,A	
Justification	High	22	16.05	10.59	14.00	13.55	14.18	15.32	15.00	11.50	15.27	13.05	14.05
Justification	Equal	20	16.60	8.00	13.80	12.80	13.10	14.95	15.25	9.30	12.70	12.65	12.99
Excuse	High	27	14.81	7.96	14.38	12.38	8.89	13.78	13.44	7.26	12.89	9.37	10.94
Excuse	Equal	23	16.65	7.22	13.43	12.43	10.91	13.78	14.04	7.35	13.74	10.17	11.67
Means by Trial		92	15.96	8.41	13.93	12.76	11.58	14.40	14.36	8.74	13.63	11.16	12.31

*R = Replenishment; NoR = No replenishment; NoR,A = No replenishment with an account